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Thought and Performed Experiments in Hayek and Morgenstern

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> ... 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.
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> Oskar Morgenstern 1972a, p. 710

> 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.
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> Friedrich A. Hayek 1968, p. 180

1. Introduction

It is generally recognised that the inception of experimental economics, as well as early developments in the field, are related to the works of Oskar Morgenstern (Schotter 1992). There are at least two main reasons why Morgenstern became involved, in a theoretical sense, in the advent of experiments in economics, though he did not participate in the actual conduct of experiments. First, in conjunction with John von Neumann, he developed the theory of games, whose way of modelling the process of decision making proved to be very well suited for experimental applications. Second, and more generally, Morgenstern was the kind of unconventional thinker who tended to engender "unexpected" turns in the development of the mainstream in science rather than contribute to its systematisation. Experimental economics can indeed be viewed as a profound novelty in the development of economics whose impact is still to be completely assessed (V. Smith 1989). However, Morgenstern's training as an economist and much of his early work in economics are by no means devoid of reference to the mainstream. In fact his Austrian origin pervades most of his contribution to economics, though he can be considered a proper representative neither of the Austrian tradition nor of neoclassical economics.[1] Despite the well-known differences which make it a distinct variant of the

[1] The position of Morgenstern in the history of economics is the subject of very different, sometimes opposite, assessments. For instance, Boettke (1994, p. 2), who considers Mises and Hayek the main representatives of the Austrian school as an alternative approach to mainstream economic theory, argues that "individuals like Schumpeter, Haberler, Machlup, Morgenstern and Robbins would carve out their one 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 species (Vaughn 1994), as regards the focus of this paper the Austrian approach shares one main methodological feature with the mainstream which is questioned from the experimental side - that is, its deductive character. That the Austrian approach to economics as a science is mainly deductive can be easily illustrated by reference to the very diffuse way of arguing among writers in the Austrian tradition represented by their construction of thought experiments (Moss 1997). In particular, Friedrich A. Hayek, following the tradition of Wieser and Mises, repeatedly referred to an imaginary centrally planned economy in order to highlight the beneficial effects of having decision decentralised among individual agents in actual economies. Moreover we shall see that Hayek's use of general equilibrium as a purely fictitious construct can be interpreted as functional to thought experiments as well.[2]

In the first part of this paper, we seek an understanding of why the two Austrian scholars Hayek and Morgenstern decided to use such different methods of economic investigations as thought and performed experiments. We argue that there is a specific episode in the middle of the 1930s, namely the debate on the role of the assumption of perfect foresight in economics, that can be seen as the origin of the dissatisfaction of the two writers with the contemporary state of the art in economics, with specific reference to the use of the notion of equilibrium. We show that, notwithstanding some important differences in the assessment of the usefulness of equilibrium analysis, the outgrowth of the debate is a call for empirical analysis shared by both Hayek (1937) and Morgenstern (1935). But, two very different ways forward were originated. On the one hand, Morgenstern (1954) turned towards an inductive approach and argued for the introduction of experiments in economics, and, on the other hand, Hayek (1946 and 1968), still on the deductive side, argued for the analysis of the market process and competition as a discovery procedure.

The second part of the paper takes stock of the different approaches of Hayek and Morgenstern as how to progress in empirical work and turns towards an analysis of the methodological differences implicit in the use of thought *versus* performed experiments. This part deals mainly with Morgenstern's proposal for the endorsement of experimental investigation in economics as an alternative to the canonical use of thought experiment. Firstly, we discuss the intellectual process which explains Morgenstern's position as stated in the passage quoted at the beginning of the paper. In the 1930s Morgenstern's method of analysis often recalled the technique of thought experiment inherited from the Austrian school. But in the 1950s and 1960s, after the publication of the *Theory of Games and Economic Behavior*, he proposed a view of the role of experimentation in economics which stressed the difference between thought and performed experiments. We argue that there are a number of elements, such as individuals' limited computational ability, the length of logical chains of deduction, and imperfect knowledge, which must be taken into account in order to convincingly assess Morgenstern's methodological preference for the experimental alternative. Secondly, we

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neoclassical economists." In the same volume, Schotter (1994, p. 556) opens his contribution on social institutions and game theory by observing that "even a casual reading of the introduction of their book indicates that von Newmann and Morgenstern viewed game theory as a unifying theory for the social sciences and not as a narrow replacement for neoclassical economic theory." On Morgenstern's role in the history of economics, see Innocenti 1995.

[2] On Hayek's notion of equilibrium, see Zappia 1996.

analyse the possible relationships between the introduction of game theory and the delineation of the experimental viewpoint. Finally, we sketch out the rationale of Hayek's reliance on the method of thought experiment in the 1960s.

Before moving on it is worth making a preliminary point. Throughout the paper we shall consider Morgenstern's 1954 definition of thought experiment as the reference point for analysis. As we shall see in greater detail in the third section, Morgenstern (1954, p. 484) defines as thought experiment 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." This is certainly clear cut, but also quite a restrictive definition of thought experiments. Morgenstern seems to be thinking especially of the qualitative results about 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. But the methodology of thought experiment has been applied by Austrian economists in general not only to comparative statics but also to the comparison of the significance of the competitive market with imaginary constructs representing institutional organisations alternative to it. This holds mainly when the role of one important element of a complex system is looked at, such as the role of alert entrepreneurs in the Austrian theory of the competitive market. Thus the use of thought experiments can be better seen as a method of avoiding contemplating true complexity in the first instances of analysis, in order to come to terms with the complexity of economic phenomena by contrast.[3] Nevertheless, for explanatory purposes we find it more convenient in this paper to embrace Morgenstern's more restrictive definition.

2. Perfect foresight and the use of the method of thought experiment in the 1930s

A main dissatisfaction among the economics profession about the state of contemporary economic theory in the 1920s and 1930s was originated by the inability of equilibrium theory to take time into account in a serious manner. In a stationary equilibrium time has a simple calendar function and the structure of relative prices does not change over time because neither the external data nor the actions by individuals change. As many writers such as Hayek, Hicks and Lindahl were to become aware in those years, a more interesting approach would be to consider equilibrium as a state in which individuals do not change their plans because they were able to correctly anticipate external changes, not simply because there were no changes in external data. The role of time in economic theory was a controversial issue especially in the Austrian circles of Ludwig von Mises and Hans Mayer, two distinct groups of young economists including, respectively, Hayek and Morgenstern.[4] The analysis developed in this section

[3] The "method of imaginary construction" or "thought experiment" typical of the Austrian tradition is best exemplified by Mises's "evenly rotating economy." In Mises's work (1963, pp. 244-250), the role of entrepreneurships emerges by contrast with the role of individuals operating in a time-less, static economy in equilibrium.

[4] 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 Kirzner 1994.

concentrates on how Hayek and Morgenstern dealt with the problem of introducing the time element in equilibrium theory. In a purely Austrian style, this step was deemed to be effective only in so far as it could induce a better understanding of the way individuals plan their actions in an intertemporal context. In Lachmann's words, after dealing with the subjectivity of values those economists working on the Austrian variant of the neoclassical approach in the 1930s were striving to incorporate into economic theory the subjectivity of expectations (Lachmann 1990).

Despite the common origin of their efforts, when Hayek and Morgenstern came to use the time dimension in order to find room for expectations in their economic theory, their investigations produced contradictory results. Hayek, who is credited to be the first economist to introduce the notion of intertemporal equilibrium in economics,[5] made his first attempt at explicitly incorporating expectations in equilibrium theory in the 1933 Copenhagen Lecture on "Price expectations, monetary disturbances and malinvestment." Hayek presented this essay as an answer to Myrdal's allegation that there was no role for expectations in his trade cycle theory. On the contrary, he argued, "how important a place do they [expectations] play was in fact one of the purposes of this lecture" (Hayek 1933b, p. 155). From the beginning of his essay he tried to make the point clear:

> The main difficulty of the traditional approach is its complete abstraction from time. A concept of equilibrium, which essentially was applicable only to an economic system conceived as timeless, could not be of great value. Fortunately in recent times there have been considerable changes on this very point. It has become clear that, instead of completely disregarding the time element, we must make very definitive assumption 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 (Hayek 1933b, pp. 139-40).[6]

This passage struck Morgenstern, in his essay on "Perfect foresight and economic equilibrium" (1935, p. 171), as an exemplar of the prevalent, erroneous opinion among those authors dealing with the time element in economic theory that "the theoretical perfection of equilibrium could not be obtained without the assumption of complete foresight by the economic subjects and the entrepreneurs."[7] In fact, Morgenstern argues,

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[5] See Milgate (1979), who refers to Hayek 1928.

[6] Hayek's main goal in the 1933b essay was to show how a theory of business cycle could be based on "expectations inevitably doomed to disappointment." This is why he did not elaborate further on the notion of "correct foresight." In fact, when the paper was translated into English (in 1939) Hayek remarked the reader in a footnote to the paragraph just quoted that "Economics and knowledge," published in 1937, contains a more elaborate and "partly revised" analysis of the relationship between equilibrium and foresight. As it will be clear in a moment, this footnote is originated by Morgenstern's attack on this point.

[7] 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 was released, there are of course minor differences between the passage we quote in the text from Hayek 1933b and the quotation of the same fragment given in Morgenstern 1935 .

despite being generally used as a pre-requisite for the notion of equilibrium over time, the meaning of "full," "perfect" or "correct" foresight is not obvious at all.[8] 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).[9]

Morgenstern's rebuttal of the notion of perfect foresight is part of a general discomfort with "the common logical carelessness in expression which characterises theoretical economics" (Morgenstern 1935, p. 169). This is utterly incomprehensible to Morgenstern, who seemed to consider that the adoption of the assumption of perfect foresight could stop the necessary development of economic theory towards a satisfying explanation of interpersonal decision problems over time.[10] In Morgenstern's view, the problem with the assumption of perfect foresight is not simply whether it is a realistic hypothesis or not, as it might appear from the quotation of the 1935 essay given above. Morgenstern (1935, p. 173) 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, apart from considerations about its realism, the hypothesis of perfect foresight can be a useful tool for analysing the behaviour of an individual taken in isolation. But once one leaves the "Robinson Crusoe economy" the situation dramatically changes:

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[8] 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 implied by the introduction of the expectation element in economic theory. In this connection, once more, Hayek's 1933b essay is quoted as a (negative) example.

[9] It might be interesting to recall that the definition of "demi-gods" is probably more suggestive but otherwise equivalent to those of "superoptimiser," such as in Winter 1985, and "homo-rationalis," such as in Aumann 1985, used to identify the knowledge abilities attributed to individual agents in current general equilibrium theory and game theory respectively.

[10] As it has been noted, 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 (Leonard 1995, p. 313). Morgenstern's explicit endorsement of the axiomatic method is to be found 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 dispensed with" (Morgenstern 1936, p. 396). It hardly needs stating that this shift of approach culminates in the introductory chapter of *The Theory of Games and Economic Behaviour*.

> 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 (Morgenstern 1935, p. 173).

Morgenstern (1935, pp. 173-74) thus reproduces the paradox of Holmes and Moriarity[11] he had used in a previous work of his in order to argue 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*" and concludes that "*unlimited foresight and economic equilibrium are thus irreconcilable with one another*." It is worth stressing that Morgenstern's use of the paradox is a clear application of the method of thought experiment. The mental chain of deductions attributed to Holmes is intended to show that the "empirical" information from a mental, not performed, experiment allows the theorist to rule out certain theoretical conjectures, which, in this instance, are how equilibrium is to be defined in an interpersonal decision problem over time. Actually, the paradox is essential for Morgenstern's argument that the attempt to solve the problem of strategic interaction over time among individuals by assuming perfect foresight is logically inconsistent.

Let us now see how Hayek reacted to Morgenstern's allegation. In the 1930s Hayek was looking for more solid foundations for his own theory of the trade cycle in different, but, in his view, strictly related directions: equilibrium theory, capital theory and monetary theory. Hayek presents "Economics and knowledge" as an attempt at making equilibrium analysis "in principle applicable to a progressive society and to those intertemporal price relationships which have given us so much trouble in recent times," which he sees as the preliminary step "to prove my contention that the tautological propositions of pure equilibrium analysis as such are not directly applicable to the explanation of social relations" (Hayek 1937, p. 41, 35). And after giving the celebrated definition of equilibrium as a state of coordination of individual plans,[12] 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 of course to Morgenstern's 1935 paper. We

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[11] As it is well-know the paradox concerns the paralysing effect on actual action of thinking strategically in a two-person game, which in the example is the attempt by Moriarity 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 behaviour of the other player. It must be noted, however, that Morgenstern's reasoning does not consider the possibility of using mixed strategies.

[12] "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). It has been convincingly argued that Hahn's 1973 definition of equilibrium, which still sets the standard to start from in current equilibrium theory, is nothing but a stochastic version of Hayek's definition (Littlechild 1982).

must understand, than, that the discussion of 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 which must exist in order that equilibrium may be arrived at. It is rather the defining character of a state of equilibrium. Nor 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)

If one looks for the partial revision announced by Hayek in the 1939 re-print of the 1933 lecture mentioned before, two clarification points can be highlighted. On the one hand, correct foresight is now precisely identified neither as an assumption nor as a pre-condition for equilibrium, but as the defining characteristic of the equilibrium itself. On the other hand, correct foresight is not equivalent to perfect foresight, for the requirement of correct foresight is deemed to be, in some sense, lighter. Both these clarification points implicitly refer to the questions raised by Morgenstern. In particular, the second point hints at the cognitive and computational capabilities of individuals so as to stress that "demi-gods" are not necessarily involved. Furthermore, Hayek's conclusion that "relevant foresight" will be defined later, encourages the reader to wait for more detailed analysis, as we shall see in what follows. However, it is worth noting that Hayek did not endorse Morgenstern's point of the logical inconsistency of equilibrium with perfect foresight. His contention seems to be that the revision of the notion of equilibrium he proposes can show how the "tautological propositions of pure equilibrium analysis" about the action of a single individual are to be applied to the explanation of social relations.

To conclude on the "chronology" of the Hayek-Morgenstern exchange on this issue[13] we need only mention Hayek's 1946 essay on "The meaning of competition" where, once more, Hayek refers to Morgenstern's paper. In this critical essay on the state of the contemporary theory of competition, Hayek points out that "the modern theory of competitive equilibrium *assumes* the situation to exist which a true explanation ought to account for as the effect of the competitive process" and goes on to recall that "according to the generally accepted view, perfect competition presupposes ... [amongst other conditions] ... complete knowledge of the relevant factors on the part of all participants in the market" and that this condition is, amongst the others, "the critical and

[13] Though we are not going to deal with them in this paper, it is surely possible to find other subjects of common interest and, sometimes, common assessments by the two authors. See, for instance, Morgenstern (1936, p. 402) harsh comments on the state of the theory of capital and the use of the notion of natural rate of interest in the Bohm-Bawerkian and Wicksellian tradition and compare it with Hayek's attempt at re-designing it in the *Pure theory of capital*, including the explicit rebuttal of the notion of natural rate of which he himself made wide use in *Prices and Production* (Hayek 1941, p. 35n.).

obscure one" because "the standard can evidently not be perfect knowledge of everything affecting the market on the part of every person taking part in it."[14]

To sum up on this part two very important questions remain open. As for the first question, though Hayek referred to Morgenstern's main point, he continued using equilibrium with "correct" foresight as a useful logical instrument. In fact, after delineating in "Economics and knowledge" the research project which will substantially drive him away from pure economic theory – and with respect to which the 1946 essay is a crucial step forward – Hayek made a last attempt at reformulating the model of the real economy on which his theory of the trade cycle was based in *The Pure Theory of Capital*. In the introductory chapters of that volume, Hayek clarifies that for a coherent assessment of the theory of capital it is necessary to start with intertemporal equilibrium analysis, even if only as a preliminary step towards "the causal analysis of the process in time" to be developed in a second volume (see in particular Hayek 1941, ch. 2).[15] Moreover the celebrated 1945 essay on "The use of knowledge in society" contains the notable reference to equilibrium prices in a decentralised market system as aggregators and conveyors of disperse information, that is to their role of reducing the informational requirements about individuals' capability and knowledge usually made in economic theory.[16]

As regards the second question to be addressed, it must be noted that the main outgrowth of the discussion about perfect foresight is a quest for a closer attention to the empirical content of economic theory that was explicitly endorsed by both authors. Although with different emphasis, in their essays of the mid 1930s both Hayek and Morgenstern outlined a two step procedure to develop economics both as an exact and as an empirical science. As for the first step of the procedure, aimed at making economics an exact science, the typical neoclassical methodological flaw 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 allows exact recognition of all the implications of a given set of propositions and exact derivation of theorems from propositions. Though contrary to Morgenstern he never personally contributed to this development, Hayek (1937, p. 35) points to the same issue when he explains 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

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[14] Hayek concludes on this point with a concession towards Morgenstern's argument 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," and refers the reader, in a related footnote, to see Morgenstern's 1935 paper. However, it seems as if Hayek was 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.

[15] Hayek (1941, p. 26) makes it clear that "the use of the equilibrium method here means constructing an imaginary state in which the plans of different people (entrepreneurs and consumers generally) are so adjust to one another that each individual will be able to sell or buy exactly those quantities of commodities which he has been planning to sell or buy." It is worth recalling that while almost the whole of *The Pure Theory of Capital* is an exercise in intertemporal equilibrium theory, Hayek never wrote the announced second volume.

[16] As it is well-known, the evolution of Hayek's thought is an intricate matter which has been the subject of diverging interpretations, with specific regard to the role of equilibrium theorising (see, for instance, Caldwell 1988 and Moss 1994).

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 aim of the second step of the procedure, on the other hand, is to specify the empirical content of economics. To whatever extent equilibrium theory can be developed, for instance by means of the axiomatic method, the process through which equilibrium can be obtained in actual economies remains to be investigated; and this necessarily entails an examination of what foresight and knowledge mean for individuals acting in actual economies. On this issue one can compare the following statements by Morgenstern (1935, p. 178) and Hayek (1937, p. 46) respectively:[17]

> Taken for granted that the theory of equilibrium already exist in final, complete form (that is, of such a kind that there no longer remains any problem to be solved) then we could construct a case in such a way that this completed science would be recognised uniformly by all economic subjects and understood equally well by all. ... To this end, it would be necessary to trace still wider relationships between degree of foresight and equilibrium.

> The statement that, if people know everything, they are in equilibrium is true simply because that is how we define equilibrium. ... It is clear that, if we want to make the assertion that, under certain conditions, people will approach that state, we must explain by what process they will acquire the necessary knowledge. ... The significant point here is that it is these apparently subsidiary hypothesis or assumption that people do learn from experience, and about they acquire knowledge, which constitute the empirical content of our proposition about what happens in the real world.

But in the 1930s Hayek and Morgenstern hinted at the way in which the empirical content was to be investigated in two different manners. To be specific, it is true that both authors were still thinking in deductive terms. In particular they were trying to cope with the complexity of the assumption of incomplete knowledge in actual economies by using mental chains of deductions and conjectures. In other words, they both were thinking in terms of *thought experiments*, by changing one relevant variable at a time and then contemplating the results on the economy. But they used two different starting points for their thought experiments. On the one hand, Hayek relied on his own definition of equilibrium as a state of plans co-ordination over time, which made the exercise sensible, such as in comparative static analysis. On the other hand, Morgenstern was not convinced at all that a plausible notion of equilibrium with foresight could be

[17] In order to value the relevance of these statements for current economic theory compare also Arrow's remarks (1996, pp. xiii-iv), introducing a recent volume on the status of the rationality hypothesis in economics: "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?"

built and then showed limited confidence in the effective meaning of the mental deductions he was drawing from the assumption of incomplete knowledge.[18]

In order to try to make sense of Hayek's and Morgenstern's diverse viewpoints on empirical analysis we find it useful, first, to examine the constructive part of the "debate" on perfect foresight. It is of course true that the two authors' approaches are better defined in their ensuing works, as we shall see in the next section. But some interesting insights can be inferred from the 1930s articles as well.

Let us start with Morgenstern first. Although Morgenstern's 1935 essay is almost unanimously recalled for its critical part, it aimed at being constructive too. On the one hand, if equilibrium theory is to be preserved then an alternative formal structure must be devised in order to deal with the intricate relationships of economic phenomena involving interpersonal decisions: "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" (Morgenstern 1935, p. 174). This is clearly the objective of the *Theory of Games and Economic Behavior*. But the 1935 essay contains no substantive hint which could suggest the formal representation emerging from the following developments. However, Morgenstern (1935, pp. 175-79) offers the reader an informal model of possible learning by individual agents which is of some interest.[19] Given that "individual's insight into [economic] relationship" is a necessary starting point even after dispensing with the hypothesis of perfect foresight, it could be imagined that, among individuals striving to understand the relationship between their behaviour and that of others, there can be types endowed with different degrees of knowledge who are interested in subsets of the environment and who will adjust their own "opinion" about the environment until "there is no longer any improvement in the sense of constant welfare." One can also imagine that there are highly knowledgeable individuals who are able to evaluate the "overall" consequences of their own behaviour, not because they are endowed with perfect foresight, but because they have "purely theoretical knowledge of [economic] relationships." Useful versions of Walrasian equilibrium, Morgenstern argues, are based on this kind of knowledge, which is not perfect knowledge,[20] thus

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[18] It might be worth noting at this stage of our analysis that in these years Morgenstern and Hayek were to become also political adversaries. In particular, as Robert Leonard argued in his discussion of this paper during the ECHE Meeting in Paris, Morgenstern was beginning to distinguish himself from his Austrian teachers because of their radical liberalism. As a result, there might also be a political component of the debate which we do not deal with.

[19] Morgenstern presents it as the economic analogous of Russell's theory of types in logistics.

[20] 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. In fact, Morgenstern (1935, p. 177) perceptively notices that if one maintains 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" this is incorrect because "it is also posited by the theory that individual acts *rationally*. The 'rationality' posits, however, 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 to be unaware that the requirement of "rationality" attributed to Walrasian (and Paretian) equilibrium, which he understands to be something more than individual rationality, is the equivalent of the perfect foresight assumption in an uncertain context. If the actions of individual agents

concluding that "obviously, one has to start from below in order to build up; one cannot begin at once with the highest type." On the other hand, Morgenstern (1935, p. 175) clearly states that he wants to investigate the importance of foresight and knowledge for actual economies: "Next to the assumption of complete, unlimited foresight, there must be rejected, too ... the assumption that there exists no foresight at all ... some positive degree of 'knowledge' as to future behaviour, that is one with more or less established assumptions about the future, is absolutely necessary for the economy."[21] But Morgenstern provides no more than some vague suggestions concerning the direction in which research is to proceed in the concluding remarks of the essay. Individuals endowed with a certain "degree of foresight," which is based on a certain minimum amount of insight into economic relationships, usually revise expectations in response to environmental changes. This means that expectations must be included in the data of the economy and that, as for other data, comparative statics analysis is necessary to correctly understand their role in actual economies.

> The problem could be put in the following manner: exactly which division of different degrees of expectations and foresight corresponds to the conditions of equilibrium described by the Walrasian equations? It is a long road until a satisfactory answer may be obtained for this question. Who knows whether the idea of equilibrium can be retained at all? The variations of expectations which one can make quite unequivocally indicate that this factor turns up in the same manner as all the other factors – for example, the changes in supply of the means of production – all of which if varied influence the final result (Morgenstern 1935, p. 183).

The problem with Morgenstern's viewpoint is that, as he himself suggests in the above quotation, he is delineating a research strategy which resembles an exercise in comparative statics without any consistently defined equilibrium state to start from.[22]

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vary continuously with foresight and the future realisation of relevant variables are a continuous function of actions, it is possible to show that there exists a foresight that would cause itself to be true, such as in a rational expectations equilibrium. Apart from the information requirement that the rationality of agents is common knowledge, this is analogous to Morgenstern's argument on rationality (Radner 1989). As a result, the logical impossibility of a rational expectations equilibrium cannot be argued on this ground.

[21] In an, admittedly provisional, attempt at defining which relationship holds between the "degree of foresight" and equilibrium, Morgenstern proceeds by distinguishing between the 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 in the 1935 essay Morgenstern did not have a probabilistic notion of perfect foresight, which in the end he considered as a synonym for effective foresight. It must be admitted that a clear probabilistic view of the introduction of time into equilibrium theory cannot be found in other contemporary authors, such as Hayek himself, Hicks and Myrdal, to limit ourselves to the authors quoted by Morgenstern. Nevertheless this has deeper implication for the soundness of Morgenstern's argument, 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. If this reconstruction is correct, a better understanding of what are the necessary requirements for equilibrium over time to hold can be found in Hayek's (1937, p. 42) definition of "correct foresight" which we have already quoted.

[22] The point is implicitly recognised by Morgenstern himself (1935, p. 180): "If, ..., it is meant that the theory of equilibrium describes only an absolutely static situation, then one can, of course,

Moreover even after the elaboration of a "cogent examination" of interpersonal decision problems given in the *Theory of Games and Economic Behavior*, which can be considered as the realisation of the first step in the two step procedure we mentioned before, Morgenstern seemed to be no longer interested in defining and, what is more important, using a correct notion of general equilibrium over time for the analysis of the empirical content of economic theory. He moved directly to the second step of the procedure. Some insights into this shift of interest can probably be found also in the 1935 essay which closes with a precise reference to the need of a more inductive approach:

> 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 can be seen as the methodological option he favoured in order to solve the problem of what the empirical content of economic theory is made of.

As far as Hayek's approach is concerned, his viewpoint is clearly stated from 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). As we have mentioned above, Hayek (1937, pp. 40, 41) holds that a state in which "[individual] plans are mutually compatible and ... there is consequently a conceivable set of external events which will allow people to carry out their plans and not cause any disappointment" is the necessary reference point for analysis because "we can speak of a change in data only if ... expectations coincide. If they conflicted, any development of the external facts might bear out somebody's expectations and disappoint those of others, and there would be no possibility of deciding what was a change in the objective data." However economics becomes an empirical science only in so far as it can be proved that "under certain conditions, the knowledge and the intentions of the different members of society are supposed to come more and more in agreement." The second step of the two step procedure we have emphasised is thus clearly defined by Hayek (1937, p. 45) as an investigation into "the *process* by which individual knowledge is changed."

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establish perfect foresight, for nothing can be changed *ex definitione*, since everything is given as static and unchangeable. If even a single variation should result, then one can no longer speak of equilibrium nor even of a tendency toward equilibrium, but, rather, the paradox described would result. It is clear that for a theory of equilibrium which 'explains' only a *static situation, which is given as unalterable* and which, because of this basic assumption, is completely unable to say anything about the economy when a variation occurs, is utterly unimportant from a scientific point of view. It would hardly deserve the names of theory and science."

In the second part of his 1937 essay Hayek delineates the empirical element of his research project as an inquiry into the "division of knowledge" in competitive markets. Indeed, in this essay one can find Hayek's first claim about the importance of *personal* knowledge, which is to become a recurrent main theme in his future work:

> price expectations and even the knowledge of current prices are only a very small section of the problem of knowledge as I see it. The wider aspect of the problem of knowledge with which I am concerned is the knowledge of the basic fact of how the different commodities can be obtained and used, and under what conditions they are actually obtained and used, that is, the general question of why the subjective data to the different persons correspond to the objective facts (Hayek 1937, p. 51).

But although he makes continuous reference to the empirical aspect of his own analysis, Hayek never seems to think of actually verifying (or falsifying by means of an empirical counterexample) the propositions he puts forward. In fact he explicitly claims (1937, p. 55) that he intends to proceed in a strictly deductive fashion:

> 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.

Hayek's much more durable commitment to the principles of the Austrian school as compared to Morgenstern's "heterodoxy" finds here a notable example.[23] As we have briefly recalled in the introductory section, deductivism is one methodological feature which is shared by almost all of the scholars of the Austrian school, starting from Carl Menger's attack of the German historical school. The reader will not be surprised, then, to find in what follow that the Austrian Hayek committed itself to a deductive approach even in his search for the empirical content of economic theory. However two connected points must be stressed before moving on to Morgenstern's turn towards induction. On the one hand, Hayek's deductivism is substantially different from Mises's a priorism. While Mises based his aversion to empirical testing on the methodological viewpoint that the fundamental postulates of economic behaviour are to be considered true independently of real experience – that is they are Kantian synthetic a priori proposition – Hayek does not intend to deny that a priori propositions mainly reflects structures and connections among objects of economic reality, though he maintains that they can be discovered only as a result of a mostly deductive theoretical effort.[24] On the other hand,

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[23] It is worth recalling that Hayek's (1925, p. 20) first work on monetary aggregates and the business cycle, his study on monetary policy in the United States in the early 1920s, contains an harsh critique of Mitchell's statistical approach because of its "lack of interest in theory." See also Hayek 1933a, Ch. 1.

[24] On the methodological nuances of Austrian deductivism see B. Smith 1994. That it can be found a shift towards a realist perspective in Hayek's later works has been argued in Lawson 1994.

as we shall see in the following section, Hayek is not in principle averse to prediction of "patterns" and to empirical recollection of economic regularities thus opening room for the comparison between the implications of the theory and the observation of these regularities.

3. Thought *versus* performed experiment: the contribution of Oskar Morgenstern after the *Theory of Games*

In his talk at the 1952 Santa Monica Conference, Morgenstern (1954, p. 484) argued that performed experiment was a serious candidate for replacing thought experiment in the toolbox of economists:

> 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 search for the missing empirical stage of the two step procedure outlined in the previous section, which had begun in the 1930s, appeared to eventually draw to an end twenty years later. The fact is that in the two decades spent in the United States, Morgenstern's professional knowledge had been greatly enriched by the collaboration with John von Neumann in the co-writing of *Theory of Games and Economic Behavior*, which was by far his most important contribution to economic science. Among other effects, that joint work gave a decisive push to the foundation of experimental economics.

The Santa Monica conference played a crucial part in this story, because it may be considered the occasion in which experimental gaming originated.[25] The conference, where game theorists met experimenters associated with the Rand Corporation, was promoted by the Ford Foundation and the Michigan University with the title of "The Design of Experiments in Decision Processes," and featured some very influential contributions. Two papers in particular had a noticeable influence on subsequent developments. The first was presented by Estes (1954) and tested a stochastic model of learning; the second paper was given by Kalisch, Milnor, Nash and Nering (1954) and concerned the theory of n-person games.

The experiment proposed by Estes was aimed at showing that learning could be represented as a converging stochastic process. Estes interpreted the experimental results as corroborating the definition of rationality peculiar to game theory, one essentially based on a probabilistic approach. But the historical importance of Estes's contribution rests above all on its influence on experimental methodology. A first consequence was already implicit in Flood's discussion (1954) of Estes's paper, which criticised the experiment in a way which resembles the way economists attacked game theorists. Flood

[25] For the history of experimental economics, see V. Smith (1992), Roth (1993) and (1995).

maintained that the assumptions about the informative and computational capacities of the players were too strong, and the use of mathematical concepts like mixed strategies quite unrealistic. The same points were developed some years later by Herbert Simon in his 1957 book, *Models of Man*. Simon considered wrong to test the learning model proposed by Estes in controlled conditions and objected to the modelling of maximising behaviour made in game theory. He proposed a distinction between the subjective rationality of the experimental subject and the objective rationality of the experimenter. The former would be founded on a perception of the external world which does not necessarily coincide with the latter, expressed by the characteristics of experimental design. The main consequence would be that the learning process was aimed more at identifying the model for individual decision with the best fit among those perceived by each subject than at defining a behaviour that maximises the individual outcome of the game.[26]

The paper presented at Santa Monica by Kalisch, Milnor, Nash and Nering was reported an experiment verifying some solution concepts for n-person games.[27] Although the experimental results were far from conclusive, both because of the difficulty in determining a "neutral technique" and because of the indeterminacy of the tested concepts,[28] the paper represented a crucial step in the development of experimental gaming. Methodologically the main finding of the experiment was to show how game theory could allow the translation of the hypotheses to be tested into simple and precise models. The subjects were firstly instructed in the main principles of game theory and then submitted to the experiment with predetermined time. The payments to the subjects consisted of tokens that were converted into dollars at the end of the experiment. The discussion of the results took into account both the environmental conditions and the different personalities of the subjects.[29]

The significance of these contributions was appreciated by an authoritative conference participant, Oskar Morgenstern, who also contributed actively to the discussion. The passage quoted above from the 1954 paper was undoubtedly influenced by the experiments presented at the 1952 conference, but the awareness that there might be a fruitful relation between economic theory and experimentation had been clear in Morgenstern's mind at least since 1950.

The role of translator of the potentiality of new tools for the development of economics was not an unusual one for Morgenstern. It is generally acknowledged that he played a fundamental role in qualifying the usefulness of the application of game theory to economics.[30] Starting from a severe criticism of neoclassical theory, Morgenstern

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[26] Moreover, Estes also influenced the work of Sidney Siegel. In the early 1960s, just starting from Estes's experiment, Siegel carried out a proof of the importance of monetary incentives to the behaviour of experimental subjects. The influence of Siegel on experimental economics is discussed in V. Smith (1992).

[27] The solution concepts were the Nash equilibrium, the Shapley value, the von Neumann and Morgenstern solution, the core defined by Gillies and an arbitrage scheme proposed by John Milnor.

[28] In drawing the conclusions, the authors wrote: "Although it is clear that the results do not coincide exactly with any present theory, it is a question how much the outcome was influenced by the experimental technique" (Kalisch, Milnor, Nash and Nering 1954, p. 268).

[29] For example, the table position of the players assumed an unexpected importance. See Luce (1959) for a detailed comment of the experiment.

[30] See Mirowski 1992, Rellstab 1992, Schotter 1992, and Leonard 1995.

claimed that game theory could radically improve the state of economic science by changing not only its mathematical but also its theoretical foundations, in particular by removing the main simplifications of the Walrasian system. But in the 1950s Morgenstern's contribution was relatively neglected with respect to von Neumann's. Indeed the application of game theory to economics was characterised by an emphasis on its formal aspects, at least until the 1970s, when the identification of game theory with the theoretical analysis of competitive markets was weakened. Only in the last two decades, have the heterodox potentialities of the application of game theory to economics been developed, especially with the proposal of a certain number of game theoretical models that have tried to give up the main neoclassical postulate, namely the coincidence between rational choice and the solution of a well-defined maximisation problem. This issue was submitted to critical examination in the first chapter of *Theory of Games and Economic Behavior*, where it is easy to see Morgenstern's influence.

> 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 maximise 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 which affect his interest (von Neumann and Morgenstern 1944, p. 11).

The project in which Morgenstern was interested departed from the statement of lack of realism and logical inconsistency of the assumption of perfect foresight. After dispensing with this essential hypothesis of neoclassical theory, in Morgenstern's view economists had to turn their attention to models of learning that were not much different from those made explicit later by the debate about Estes's experiment. In conditions of incomplete knowledge and strategic interdependence, only the introduction of some kind of subjective rationality, in Simon's sense, could provide a basis for describing how agents determine their own rational behaviour. This research program was very closely bound up with the subsequent foundation of experimental economics. Hence, the reasons that induced Morgenstern to argue for the usefulness of game theory were almost the same as those which induced him to support experimental economics. In particular, in his opinion, orthodox economic theory was wrong because it assumed that agents could perform the complex activities necessary to reach a competitive equilibrium. By offering an environment for empirical analysis and by providing counterexamples, experimentation, like game theory, could force a re-examination of the basic postulates of the neoclassical approach.

In order to throw light on Morgenstern's view, we can make reference to three different issues: his criticism of contemporary economic methodology, his dissatisfaction with thought experiment, and his awareness of the crucial role played by game theory in creating a better abstract environment for experimental investigation.

Morgenstern's view about methodological questions was expounded in the papers of the 1930s we have discussed in the previous section.[31] A few years later, when von

[31] But see also Morgenstern 1941.

Neumann and Morgenstern began their period of collaboration in Princeton, they shared the view that neoclassical theory was not rigorous enough, especially because it made improper use of mathematics. In particular, the formulation of problems was too inaccurate to prevent their translation in axiomatic terms, the statements were often treated as proofs, and the empirical references were confusing and offered no clue as to the relation between theory and empirical evidence. As we have mentioned above, in order to correct these methodological flaws Morgenstern (1936) proposed the introduction of Hilbertian logic into economics, as a formal tool able to identify exactly all the implications of any proposition. The *Theory of Games and Economic Behavior*, notwithstanding its reference to a "sense of modesty,"[32] presented what Morgenstern meant to be his research project for dealing with the first step of the procedure outlined in the previous section, concerning economics as an exact science. But the same methodological modesty, and the preponderant presence of von Neumann's mathematics, postponed any progress in the analysis of the empirical content of economic theory.

Morgenstern returned to this problem in his 1950 book "On the accuracy of economic observations." In the second chapter of this volume, a number of sources of errors in economic statistics were listed and the first one was the lack of designed experiments.[33] This deficiency was considered the main cause of an essential difference between natural and social sciences: while in natural science the theorist could establish the circumstances in which data were collected with confident accuracy, in social science this was not so, because the producer of data was usually a person different from the user.[34] The consequence was that to make the interplay between theory, measurement and data collection as close in economics as it was in physics, it was necessary to add performed experiments to the tools commonly employed by economists to empirically verify their models.

It was exactly this parallelism between economics and natural sciences that also led Morgenstern to consider thought experiment method as unsatisfactory. The criticism contained in his contribution to the Santa Monica Conference deserves full quotation:

> Its methods involve imagining conditions that differ from the known conditions and then attempting to identify the proper factor to which the imagined variations could be ascribed. This procedure consists in the drawing of implications and like other experiments may lead to the discovery of new facts. It is legitimate but exceedingly difficult to handle, hence the numerous times when it has given rise to poor results. It is

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[32] "The sound procedure is to obtain first utmost precision and mastery in a limited field, and then to proceed to another, somewhat wider one, and so on ... The field covered in this book is very limited, and we approach it in this sense of modesty." (von Neumann and Morgenstern 1944, p. 7).

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

[34] "In general, economic statistics are merely by-products or results of business and government activities had have to be taken as these determine. Therefore, they often measure, describe, or simply record something that is not exactly the phenomenon in which the economist would be interested." (Morgenstern 1950, p. 14). Further on, "Thus the development of economics is dependent to a very high degree upon an agglomeration of statistics which in the main is rather accidental from the point of view of economic theory" (Morgenstern 1950, p. 17).

> 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 it is of "great" length. (Morgenstern 1954, p. 484)

It is worth noting that while in the 1930s, as we have seen in the previous section, Morgenstern attacked the logical consistency of the Walrasian system by means of a typical thought experiment, in the 1950s he questions exactly the efficacy of the method of thought experiment by making use of a very similar argument. The length of chains of reciprocally conjectural reactions and counter-reactions carried *ad infinitum* was employed to show earlier that perfect foresight prevented any definitive equilibrium resolution and later that thought experiment encountered serious logical problems. If in the 1930s Morgenstern's solution to the indeterminacy of equilibrium was an arbitrary decision breaking the chain of conjectures, in the 1950s his proposal for interrupting the indefinite deductive procedure of a thought experiment is the performing of a laboratory experiment.

Morgenstern's confidence in the usefulness of experimentation in social sciences was strengthened by the ensuing flourishing experimental activity, that could be interpreted as an outgrowth of game theory in two senses. On the one hand, in the 1950s there were two different approaches to experimentation and both relied on game theory. On the other hand, from the beginning their outcomes seemed to challenge some of the most basic assumptions of economic theory.

The influence of game theory on the origins of experimental economics followed two distinct paths: the first, corresponding to the socio-psychological approach, was expressed by experimental games, the second, defined as the economic-managerial approach, by business games.

The first approach was mainly developed by social scientists (psychologists, sociologists, mathematicians, philosophers, and decision theorists) belonging to a deeply interwoven community mainly financed by military funds. Deutsch (1958), Flood (1958), Loomis (1959), Scodel, Minas, Ratoosh and Lipetz (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), Davidson, Suppes and Siegel (1957) made an experimental testing of the utility function proposed by von Neumann and Morgenstern in *Theory of Games*; Stone (1958) and Schelling (1958, 1959) verified Nash bargaining theory and the theory of focal points. These contributions shared two main features: firstly, the object of verification was represented, or could be represented, by games in normal form; secondly, their theoretical background related to economics only indirectly, being set mainly within other social sciences.[35]

[35] As regards the first point, it was confirmed by an exhaustive review of the 1950s experimental gaming (Rapoport and Orwant 1962), that presented and discussed over forty experiments showing how each of them could be represented as the verification of a game in normal form. As to the second point, it was corroborated by an authoritative witness, Herbert Simon, who judged that period in the following way: "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

While in the 1950s experimental games did not fill the gap between experimentation and economics, the other approach, developed by business-school economists, represented the proper economic way to experimentation. The first *business game* was performed by a group of economists and managers directed by Richard Bellman and was published in 1957. Later, Andlinger and Greene set up a *Business Management Game* (Andlinger 1958), and a group of IBM researchers organised a laboratory to make experiments in decision analysis (International Business Machines 1958a and 1958b). Finally, Hoggatt (1959) employed a business game to test Cournot model. What made business games different from the previous group of experimental games was that they did not aim at verifying the empirical validity of specific theories, but only at being a tool for training and selecting the managers of big firms. Economic theories were an essential requirement in designing the experimental environment, but no explicit model was communicated to the subjects, a factor which greatly reduced the difficulties in the playing of the game. Moreover, the instructions to subjects could avoid any theoretical assumption and make reference only to standard business practice, as it could be captured by a simulated game. Just the simplicity of the game structure allowed the experimenters to pay closer attention to the experimental techniques, which as a result improved rapidly. This progress was particularly evident in the experiment performed by Hoggatt. Even though its purpose was simply to test the validity of Cournot's equilibrium, what Hoggatt made clear was how complex the interpretation of the results was. Among the various determinant factors, he mentioned the subjects' intellectual capacity, the different patterns of learning, the single personalities' attributes (distinct in dominance and submission), and the sociological referents (culture, sex, age and occupation). This multiplicity of factors matched the variety of the types of economic behaviour to be considered "rational" or "maximising:" as Hoggatt (1959, pp. 192-195) stated: "we focus on using game situations as a research tool for studying the behaviour of human beings in conflict situations ... [our aim is] to observe how the subject's actual behaviour compares with various types of maximising behaviour as these are visualised in economic theory."

Hoggatt's business game was intended to point out the complexity of human behaviour, which did not conform simply to neoclassical maximising behaviour but was rather the outcome of a mixture of conflicting maximum problems. This closely resembles the point made by von Neumann and Morgenstern in the quotation from the first chapter of *Theory of Games* mentioned above. It is therefore not at all surprising that in 1962 Morgenstern, in evaluating the meaning of the interplay between game theory and business, recognised 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 the opening of this essay, where Morgenstern acknowledges that experiments were basic to the foundation and application of new economic theories, was an explicit reference to this issue.

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mathematical learning theory), statisticians, and interdisciplinary types close to cybernetics and management science" (quoted in V. Smith 1992, pp. 253-254).

To summarise, then, in Morgenstern's view the solution to the problems left over unresolved from the 1930s lay in the new attitude stemming from experimentation, which would ultimately succeed in establishing economics as an empirical science. Morgenstern considered experiment as an essential tool for the economist to implement the empirical analysis invoked, before the foundation of game theory, but never carried out. The more the first step of the two step procedure was grounded in axiomatics, the more the second step could take different paths to the analysis of the process through which equilibrium could be obtained. But the process of scientific discovery was not intended to stop at this point: in order to thoroughly implement the second step one had to go back and look for revision of the theoretical models from which analysis departed.

Once this method was accepted, it could also reveal that the conventional conception of economic behaviour was clearly distinct from that of game theoretical models. Morgenstern believed that game theory was capable of deriving through direct experimentation a conception of rationality alternative to the neoclassical one. At that time, a concrete proof of how this process could be effected was given by Allais' experiment, which inspired the following experimental search for systematic violations of expected utility theory and which was reviewed by Morgenstern only some years later. Even if, in Morgenstern's words, Allais had only showed counterexamples in "outlandish situations" (Morgenstern 1972c, p. 67) which did not conclusively demonstrate the invalidity of von Neumann-Morgenstern utility index, he had showed how experimental proofs could allow the revision of a theory by establishing new axioms to be fitted in its proper domain.[36] This example confirmed Morgenstern's belief that even all of the basic axioms and concepts of game theory could be tested by observing real behavior involving simple choices. The collected evidence would have, almost surely, permitted the appearence of new theoretical explanations by disclosing "some properties of a nature hitherto unknown" (Morgenstern 1954, p. 496).

While the need to point out the critical points in neoclassical theory and to provide fresh empirical content to economics pushed Morgenstern towards experimental methods and a more inductive approach, Hayek, still on the deductive side, based the analysis of the market process and competition as a discovery procedure on an approach which still relies mostly on the methodology of thought experiment.

Hayek's deductive method consisted of obtaining by means of imagination what empirical analysis could not provide by itself. In this sense, thought experiments certainly had a purpose that was "purely pedagogic," as Moss (1997, p. 157) points out, but it also represented a tool to fill the gap between theory and reality according to an approach that Hayek would eventually more clearly define in the 1950s and 1960s. Indeed the full development of this methodological conception had to wait Popper's contribution. Hayek (1955) clearly explains that the aim of a model is limited to defining

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[36] "What has been done instead by Allais and others can be characterised as an attempt to show *counterexamples* which would conclusively demonstrate that individuals have a "utility of gambling" such that these examples would destroy the universal claim of the theory. Regarding counterexamples it is easy to falsify the statement "all swans are white" by showing one black swan. But it is not as easy to contradict an axiomatic theory which fulfils all the requirements of such a theory, as ours does. Instead of generalities one would expect that a new axiom be established to be fitted into the existing system, however modified. Then a proof should be given for the type and kind of utility that the modified system defines" (Morgenstern 1972c, p. 66).

a certain range of phenomena which can be produced by the type of situation which it represents. Empirically, it indicates only the range of phenomena to be expected. On 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 an artistic-like description of scientific work placed Hayek well apart from the inductive method based on experiments, especially in social sciences, where theories were particularly difficult to prove or disprove. In this field, the multiplicity of factors determining any situation prevents the validity of deductive reasoning from being established by direct observation. Therefore, deduction remains the only way to define the range of phenomena to expect. To do this, it relies upon the combined effects of our existing knowledge, which however represents new knowledge because "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).

Later, Hayek (1964) coined a specific word for this kind of theorising. He argued that the complexity of social phenomena requires that analysis is 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, p. 28-29).

From the viewpoint advocated here, if the range of what is permitted by theory is so wide as to prevent meaningful predictions, the only viable alternative is to rely upon the limits of imagination in order to distinguish between what is possible or not.

This approach is likely to be appropriate for a science like economics which has to deal with the extreme complexity of human behaviour, as could be argued by making reference to the example of the Walrasian equilibrium model. In the 1930s Hayek pointed out its conceptual and methodological flaws for the analysis of the competitive process when knowledge is disperse. In this new perspective a Walrasian system of equations is not fitted to the role of describing a kind of pattern which emerges when certain conditions are satisfied. In fact, when complex phenomena are investigated, it is only "explanations of the principle" that can be sensibly formulated. The notion of spontaneous order (Hayek 1968) is, as a result, offered as an alternative for the analysis of pattern coordination through the market process. Its purpose is to elucidate the very general conditions under which coordination holds. But this application would necessarily continue to ignore the more particular circumstances which determine that pattern. In this abstract environment thought experiments might still play their distinctive

role, that of discovering empirical information from experiments that can only been imagined.

The question which remains open was how to recognise what general conditions had to be taken into account to define a certain kind of pattern. This selection process, which was more difficult by far in economics than in natural sciences, can be dealt with through the similarity between the viewpoint of the observer - the economist - and the viewpoint of the observed subject - the economic agent.[37] In Hayek's view, as expressed in the quotation that opens this paper, the corollary was that the usefulness of performed experiment was invalidated by the fact that it could be employed only for facts that "are already known to the observer" (Hayek 1968, p. 180). On the other hand, Morgenstern's view proposed performed experiments for the very reason that, by claiming a separation between the rationality of the experimental setting, designed by the experimenter, and the rationality of experimental subjects, it could reveal different approaches to rational decision-making that are not available in the realm of thought experiment. It was precisely in terms of conceptions of rationality that a dichotomy of this sort acquired a defined meaning.

4. Conclusions

Economic science has acknowledged the usefulness of experimental methods only recently. Hence, it is not at all surprising that historians of economic thought have not yet made a close examination of its evolution. This delay can be attributed to the continuing absence of general agreement on the first principles of the discipline. But another cause is that economists have always had controversial views about the links between theory and empirics. This paper has attempted to develop insight into this problem by analysing how it was dealt with by the two Austrian economists, Hayek and Morgenstern, and our original standpoint lies in our consideration of the way they perceived the role of experimentation in economics. In developing our approach, we drew on a recent paper by Moss (1997). Moss highlighted the method of imaginary construction - or thought experiment - as a defining characteristic of the Austrian tradition, but at the same time he made no attempt to discuss the relationships between thought and performed experiment, and consequently that between deductive and inductive approaches to economics. In our paper, however, while we started with an acknowledgement of the role played by thought experiment in the methodological views of Hayek and Morgenstern before the 1940s, we have also discussed how they interpreted the interplay between abstract and empirical content of economics. We have argued that they shared the view that economics could improve its methodological status by developing according to a two step procedure. The first step was to become an exact

[37] "This is perhaps the place to mention that what we are discussing here is of course not the only difference between the physical and the social sciences, but rather a peculiarity which the latter share with those natural sciences which deal with comparatively complex phenomena. Another and perhaps more important peculiarity of the social sciences is due to the fact that here the *recognition* of the different kinds of facts rests largely on a similarity between the observer and the observed persons" (Hayek 1955, p. 18).

science, by introducing axiomatic method and analytical power, while the second step had to carefully and precisely define its empirical content.

In seeking to understand why Hayek and Morgenstern agreed on the necessity of the first step, we have analysed Hayek's acknowledgement that Morgenstern's critique of the notion of perfect foresight was definitive. In this way, Hayek came to realise that the equilibrium notion of plan coordination he introduced in the late 1920s and 1930s could only perform the role of an imaginary construction. This could be considered the starting point for Hayek's turn towards qualitative description of how order can be obtained. But it can also be considered as the watershed between the Austrian method of imaginary construction and Morgenstern's turn toward the appreciation of performed experiments.

It is indeed clear that after the 1940s and in particular after the introduction of game theory in the tool box of economists, Hayek and Morgenstern hinted at the way in which the second step, namely the specification of empirical content of economics, had to be interpreted in two 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 is changed, without actually verifying the propositions he puts forward. On the other hand, Morgenstern was deeply influenced by his joint work with von Neumann in the writing of *Theory of Games and Economic Behavior*. While in the 1930s Morgenstern's method of analysis often recalled the technique of thought experiment inherited from the Austrian school, which was used in his article on perfect foresight or in the famous paradox of Holmes and Moriarty, since the 1950s he explicitly supported the possibility of introducing experimental methods into economics by adopting laboratory procedures transposed from natural sciences. This change of perspective was a consequence of the flourishing of experimental activities originated by game theory. A brief historical overview of the early years of experimental economics has indeed shown that in the 1950s experimental methodologies were mainly developed by an interdisciplinary research community, which gave rise to two distinct approaches, experimental gaming and business gaming. Both approaches were an outgrowth of game theory for two reasons: first because game theory allowed to translate into verifiable hypotheses the models to be tested and therefore to observe real behaviour involving simple choices, and second, because experimental findings, as game theory, could disclose unknown properties of human behaviour challenging some of the most basic assumptions of economic theorising. These two consideration together explains why Morgenstern supported performed experiment as a useful tool to conduct the empirical step of economic inquiry.

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