Paradoxes versus formalism in economics. Evidence from the early years of game theory and experimental economics.

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Abstract. This paper argues that the acceptance of two recent methodological advances in economics, namely game theory and laboratory experimentation, was affected by the history dependence constraining the formalization of economics. After an early period in which the two methods were coolly received by economists because their applications challenged some basic hypotheses of mainstream economics, their subsequent acceptance was the result of the corroboration of those same hypotheses. However, the recent emergence of some paradoxes has finally revealed that the effectiveness of game theory and experimental techniques in economics is improved when descriptively implausible and normatively unsatisfactory assumptions such as the centrality of individual maximization in decision theory and the definition of rationality as consistency in preferences are revised.

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1. INTRODUCTION

A common view in modern controversies over mathematical formalism in economics is that mathematics is not a neutral tool but a language with a peculiar interpretative framework quite distinct from economics. The introduction of formal assumptions and analytical methods into economic models may lead to unintended theoretical outcomes. This lack of awareness may influence the interpretation of the model, which can become self-referential and lacking in empirical meaning.

This issue could be dealt with by making the rhetoric of economics explicit. By revealing the implicit assumptions hidden by the model's mathematical symbols or by dissecting the literary translation of deductive reasoning, economists could fully recognize the concrete implications of their theories.

Another way of dealing with the issue is to learn from historical reconstruction how economics was affected by this methodological feature. If this approach were applied to recent developments, historical analysis could redirect the frontiers of the discipline towards more consciously chosen targets. Although the privileged perspective of historians may be biased, it gives historians an advantage over theorists for evaluating the real incidence of the drawbacks of formalization.

By the middle of the twentieth century, the formalization of economics had reached such a level that it is hardly surprising that historians of economics were becoming increasingly concerned that formal mathematical reasoning could be detrimental to economic analysis.[1] Nevertheless, the consequences of this lack of commensurability between economics and mathematics have not been fully assessed. Specifically, the presumptive separation between models and empirical analysis due to axiomatics induces mathematical economists to underrate or even evade the empirical plausibility of their theories. They preserve their assumptions against factual counterarguments by inculcating in their methodology a sort of rigidity that is reminiscent of Lakatos' hard core but that can be better identified as a sort of history dependence. When the mainstream community is generally agreed on the effectiveness of a mathematical procedure or formal assumption, this is placed in the black box of accepted postulates and treated as irrefutable. Exhibiting similarities with the effect of path dependence in biological and social processes, the formalist revolution in

[1] The claim that mathematics is a language that needs only to be translated to effectuate proper economic applications has been criticized on the basis of historical arguments by, among others, Ingrao and Israel (1987), Clower

economics has been affected since its inception by a "sensitive dependence on initial conditions" (Liebowitz and Margolis 1995b, p. 210), which has hindered rather than promoted methodological innovations. This interpretation would justify, for example, the charge of "innocuous falsificationism" made by Blaug (1980, p. 259) about the methodology of economics or the disregard shown by economists for the use of spatial models (Krugman 1995, pp. 64–65).

This paper intends to provide historical evidence to support this view. Specifically, the introduction of two methodological advances in economics, namely game theory and experimental techniques, was affected by the history dependence constraining the evolution of mathematical economics. After an early period in which the two innovative methods were coolly received by economists because their applications challenged some basic assumptions of conventional economics, their subsequent acceptance was the result of the corroboration of those same hypotheses. However, the recent emergence of some paradoxes, i.e., deep-seated contradictions between theoretical predictions and empirical inferences, has finally revealed that the effectiveness of game theory and laboratory activity in economics is improved when descriptively implausible and normatively unsatisfactory assumptions such as the maximization of individual choice and the consistency in preferences as a requisite of economic rationality are revised.

After preliminary discussion of a recent definition of the methodology of economics that sets the framework for the ensuing historical interpretation, this paper describes the changing features of game theory and experimental economics as a reaction to the core beliefs of mainstream economics. An overview of the latest developments shows how the recent emergence of paradoxes has led to the questioning of the validity of the same long-standing hypotheses of economic theory.

2. SOME PRELIMINARY NOTES ON METHODOLOGY

Methodological issues in economics are rarely clear-cut. When one goes through the definitions of the many approaches to economic theory, one's first impression is that boundaries dividing the various schools of thought are often uncertain or confused. Consequently, a preliminary question is how dividing lines can be drawn. The same distinction between orthodox and heterodox methodologies is difficult to make. To provide a framework for analysis, I address this problem by

(1995), Backhouse (1998), Blaug (1999), Mirowski (2002), Weintraub (2002), Blaug (2003), Giocoli (2003), and Rosser (2003).

citing a definition that can be considered as an updated version of the methodology of mainstream economics, for reasons I discuss subsequently.

In two papers published in leading journals of mathematical economics, Ariel Rubinstein, a major contributor to theoretical game theory, attempted to revive the definition of the axiomatic approach to economic modeling. In 1991, he focused on the methodology of game theory and 10 years later on laboratory experimentation, surprisingly using similar arguments.[2] In the first paper, Rubinstein proposed a "perceptive" interpretation of game theory:

> If we adopt the view that a game is not a rigid description of the physical rules of the world, then a game-theoretic model should include only those factors which are perceived *by the players* to be *relevant*. Modeling requires intuition, common sense, and empirical data in order to determine the relevant factors entering into the players' strategic considerations and should thus be included in the model. (Rubinstein 1991, p. 919).

By freeing game theory from the task of describing the physical world, Rubinstein's definition transfers to the theorist's subjective perception the function of choosing the factors that players perceive as relevant. In this way, the game theorist establishes a link with empirical knowledge virtually unconstrained because his or her choice can be made without outside interference. The acceptability of the model to readers indicates how successfully the theorist represents players' perceptions of real life. However, as these readers are typically other theorists, the fact that real players have those perceptions cannot be proven.

Once this definition is accepted, a fully abstract characterization of game theory is attained, as Rubinstein makes clear in these conclusive remarks:

> There exists a widespread myth in game theory, which is possible to achieve a miraculous prediction regarding the outcome of interaction among human beings using only data on the order of events, combined with a description of the players' preferences over the feasible outcomes of the situation. For forty years, game theory has searched for the grand solution which would accomplish this task. The mystical and vague word 'rationality' is used to fuel our hopes of achieving this goal. I fail to see any possibility of this being accomplished. Overall, game theory accomplishes only two tasks: it builds models based on intuition and uses deductive arguments based on

[2] Thus, it is surprising to read in the later paper the following admission: "If you had asked me 10 years ago about experimental economics I probably would have said that I do not see the point of conducting experiments in economics." (Rubinstein 2001, p. 616).

> mathematical knowledge. Deductive arguments cannot by themselves be used to discover truths about the world. (Rubinstein 1991, p. 923).

This call for pure axiomatics to identify the purposes of game theory relieves modeling from the quest for a definition of rationality. In essence, Rubinstein's approach seems reminiscent of the Austrian method of thought experiment, which purported to simplify the intricate reasoning needed to translate the complexity of reality into a simpler set of assumptions. This method proved useful mainly for investigating the role of a single important element in a complex system. For instance, Moss (1997) defined general equilibrium theory as "the grandest thought experiment in all of economics" because the functioning of the whole economic system was subsumed under the concept of market equilibrium. In any thought experiment, theories are tested only in the theorist's mind, which plays the role of a virtual laboratory. By definition, there is neither empirical confirmation nor refutation of the results of such tests. To quote Rubinstein again from the commemoration of Nash's Nobel Prize: "An economic theoretical model is not required to be tested except in our own brain. The art of economic modeling requires the avoidance of issues which are certainly connected to the main topic but whose inclusion in the analysis would prevent clear-cut results." (Rubinstein 1995, p. 13).

In 2001, Rubinstein returned to the methodological issue by discussing the use of experimentation in economics. His view on the mathematization of economics is apparently unchanged:

> The concepts which we attach to the mathematical symbols are the targets of economic theory. These are not the formal models that rather the concepts which appear in the interpretation. Economic theory is about the real world in the sense that these concepts are taken from our deliberations on the world. A good model is realistic in the sense that it orders our perception of real life social phenomena. It is realistic if it describes a situation as it is perceived by decision makers rather than as a presentation of the physical world. According to this approach models are not meant to be isomorphic with respect to reality but rather to the way in which the world is perceived by its inhabitants. And as economic theorists our goal is to clarify the connections between different types of concepts and arguments and patterns of reasoning. We attempt to 'draw links' (a phrase often used by Aumann) and 'understand', rather than 'predict'. (Rubinstein 2001, p. 618).

One might wonder how this passage relates to experiments. The following excerpt is helpful:

> (…) when an economic model is based on intuitions about how people reason, experimental economics can verify that these intuitions are not extrinsic. Experiments serve as a test of the plausibility of assumptions and not conclusions. When experimental economics feeds economic models it can suggest new ideas about human reasoning in economic situations. (Rubinstein 2001, p. 619).

A slightly disingenuous outline of Rubinstein's view might be as follows. The only way to build consistent economic models is to describe how people's minds simplify real experiences. Consequently, there is no escape from the complexity of the real world other than to resort to the theorist's intuition, which aims to identify the true perceptions of decision makers. This corresponds to the identification of economic modeling as a thought experiment, in which the experiments can eventually refute the 'extrinsicity' of initial assumptions but which are ineffective in testing the conclusions of the models. However, if assumptions represent not facts but intuition, and thus cannot be verified, model results are also virtually irrefutable**.**

This suggests a need to maintain the division between formalized and empirical arguments that has characterized mathematical economics since its inception. Whether axiomatization involves making precise deductions from given premises and verifying the logical adequacy of those premises, empirical evidence cannot refute an axiomatized economic model. Thought experiments do not admit contradictions because the theorist's intuition fixes axioms and draws implications using a process in which internal consistency is the only check. Using this approach, as Rubinstein's perceptive interpretation suggests, mainstream economics preserves its theories by removing the possibility of their empirical refutation.

However, Rubinstein suggests a possible weakness of mathematical economics: "An economic model differs from a purely mathematical model in that it is a *combination* of mathematical structures and interpretation. The names of the mathematical objects are an *integral* part of an economic model." (Rubinstein 2001, p. 617). To be explicit, the mathematization of economics cannot avoid the issue of its empirical content just because economics is a social science in which the use of mathematics is never completely abstract. Any mathematical symbol or theoretical assumption always possesses an implicit or explicit empirical meaning that cannot always be translated into words. Economic models are therefore a mixture of formal reasoning and pseudo-empirical arguments that cannot be disentangled. In this framework, casual empiricism continuously produces scientific paradoxes, i.e., contradictions between theoretical predictions and empirical inferences based on common sense that can undermine economists' confidence in the validity of their theories.[3]

This perspective raises two issues. First, if axiomatics has been the prevailing approach in economics since the 1930s, to what degree have formal assumptions benefited from the lack of a means to demonstrate its external (in)effectiveness? Second, does the emergence of paradoxes imply a weakening of this protective belt and a restoration of the empirical content of current economic theory?

Rubinstein's papers develop a framework within which to answer these questions not only because they outline the methodology of mainstream economics but also because they concern two formal tools, the introduction of which into economics were affected by these features. In their early periods, both game theory and experimental economics challenged some core beliefs of economic theory by revealing weaknesses that undermined their descriptive and normative validity. The implications of those criticisms were generally underrated primarily because economists were dazzled by the chimera of the absence of contradictions: "The twentieth-century yearning for an ironclad guarantee of the complete absence of contradiction in the marketplace (or perhaps more threatening: between the ears of the rational individual) was a mirage, little better than the nineteenth-century yearning for a perpetual motion machine. It was a machine dream; and dreams can sometimes be salutary, so long as they are never confused with conscious reality. As von Neumann had insisted, after Gödel everything would have to be different. There was only one proverb to broadcast to those fearing internal contradictions in their systems: provided there were no contradictions, then absence of contradiction would, of necessity, be undecidable." (Mirowski 2002, pp. 414–15).

The aim of the next two sections is to examine in detail those historical processes and to assess the extent to which they were influenced by this methodological feature.

[3] The popularity of this view among economists has been perceptively questioned by Robert J. Clower: "So unless axiomatics can be made to play a critical as contrasted with a constructive role, it is likely to be as little use to an empirical scientist as a broken saw to a carpenter. I suspect most economists regard paradoxes as foibles of our discipline; but because scientific paradoxes emerge only from theories that are not entirely devoid of empirical content, the thing to be regretted in contemporary economics is not the plenty but the paucity of paradoxes."(Clower 1995, p. 310).

3. STRATEGIC INTERACTION VERSUS INDIVIDUAL MAXIMIZATION

Since its inception, game theory was considered a tool whose applications asked too much of human rationality. An authority on this subject, Herbert Simon, turned the early enthusiasm for *Theory of Games and Economic Behavior* (Simon 1945) to disillusionment following his pioneering analysis of bounded rationality. According to Simon (1957), the approach of game theory was "wrongheaded" because it made the same mistake as neoclassical theory, which was "to erect a theory of human choice on the unrealistic assumptions of virtual omniscience and unlimited computational power." (Simon 1957, p. 202). At the same time, he ascribed to game theory the following legacy: "the main product of the very elegant apparatus of game theory has been to demonstrate quite clearly that it is virtually impossible to define an unambiguous criterion of rationality for this class of situations." (Simon 1957, p. 487–488).

To acknowledge that game theory revealed that too many requirements must be met for economic agents to be maximizers was not exactly a criticism if it was addressed to the promulgators of the theory, von Neumann and Morgenstern. The first chapter of *Theory of Games and Economic Behavior* contains an instructive passage, often quoted but not always fully appreciated:

> Consider now a participant in a social exchange economy. His problem has, of course, many elements in common with a maximum problem. But it also contains some, very essential, elements of an entirely different nature. He too tries to obtain an optimum result. But in order to achieve this, he must enter into relations of exchange with others. 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 problem, 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. This kind of problem is nowhere dealt with in classical mathematics. We emphasize at the risk of being pedantic that this is no conditional maximum problem, no problem of the calculus of variations, of functional analysis, etc. (von Neumann and Morgenstern 1947, p. 11).

According to this statement, the founding fathers considered game theory as a device for demonstrating that the principle of individual maximization when applied to interacting agents could produce an outcome intended by no-one. Given this definition of optimal choice dependent on the assumptions made about other players' choices, there was no straightforward principle of social rationality that could deal with the solution of games as constrained optimization did for individual choice. However, if the centrality of the assumption of self-seeking maximization were to be removed, the metaphysics of neoclassical economics would be seriously undermined.[4]

The person responsible for this critical perspective was Oskar Morgenstern, who acted as a translator of von Neumann's insights from mathematics to economics (Leonard 1995, Mirowski 1992). He spent his scientific life critically assessing the methodology of neoclassical economics (Innocenti 1995). In 1941, shortly before *Theory of Games*, Morgenstern wrote a paper entitled *Maxims of behavior* that remained unpublished, probably because it represented the embryonic formulation of the remarks co-authored with von Neumann quoted above.[5] Even if this short treatise did not deal directly with the maximization problem, its main arguments aimed to weaken the normative validity of this assumption. Morgenstern's starting point was the distinction between unrestricted and restricted maxims. The first is "one where its pursuit does not have to take into account whether other individuals do or do not accept and simultaneously follow the same maxim of behaviour" and consequently they are "purely formalistic" criteria (OMP, Box 49, p. 3). On the contrary, the restricted maxim states "will lead to the desired result […] provided that not too many persons act according to this same maxim at the same time." (p. 4). By referring to the maxim of withdrawing money when economic crisis looms, "it will retain this objective rationality only if the number of people applying it remains sufficiently small. There exists a point where the maxim becomes invalid because of the fact that the withdrawal of funds by more persons will lead precisely to the event against which it is supposed to protect." The implications were clear-cut: "the influence of great numbers upon the success or lack of success of the maxim is not quite obvious

[4] "The research program of neoclassical economics is the challenge of finding a neoclassical explanation for any given phenomenon—that is, whether it is possible to show that the phenomenon can be seen as a logical consequence of maximizing behaviour—thus, maximization is beyond question for the purpose of accepting the challenge. […] Whether maximization should be part of anyone's metaphysics is a methodological problem. Since maximization is part of the metaphysics, neoclassical theorists too often employ *ad hoc* methodology in order to deflect possible criticism; thus any criticism or defense of neoclassical maximization must deal with neoclassical methodology rather than the truth of the assumption." (Boland 1981, p. 1035).

[5] Urs Rellstab's detailed account of Morgenstern's diaries confirms this point: "On 17 May 1941, von Neumann asked Morgenstern to write a paper on 'maxims', referring to 'maxims of behavior', a topic that had been Morgenstern's main interest since the beginning of their discussions." (see 5 April 1940, OMPD). On the very next day, happy about von Neumann's request, Morgenstern began work on the paper: "I have a mountain of notes (about the subject) and I will write a short paper, maybe 5000 words." (17 May 1941, OMPD). "This paper, 'the maxims', had a direct impact on von Neumann's decision to collaborate with Morgenstern. […] Although it was never published, Morgenstern wanted to publish it later on several occasions, and both von Neumann, and later Gödel, urged him to do so (28 January 1948, OMPD)." (Rellstab 1992, p. 84).

and would disclose itself only to a more careful study. Thus subjective and objective rationality may fall wide apart which probably constitutes the majority of cases." (p. 5).

Morgenstern pointed out another implication of his interpretation: "In order to point out to economists the nature of the questions treated in this paper and to show their relevance for some of the topics with which they are largely concerned, it might be suggested to replace the word "maxim" by "plan". It will then become apparent that, e.g., the consistency of plans which is often being postulated as a condition of equilibrium is nothing but an extension of the points raised here; it would probably be advisable to conceive of a plan as a much more complex entity than they are understood to be in general." (p. 21).

Morgenstern's remarks concern a problem that was significant for contrasting earlier game theory with the methodological status of contemporary economics: the inadequacy of the concept of equilibrium.[6] It is hardly surprisingly that the word equilibrium was practically eliminated from *Theory of Games*. The postulate of equilibrium analysis is that economic agents choose the optimal action while assuming that other agents are rational, or, much the same, are maximizers. In von Neumann and Morgenstern's project, to solve a game did not mean to determine how opposing maximizing choices could be balanced by means of static assumptions about the behavior of others. Rather, they conceived of solutions as multiple and often indeterminate because in cooperative *n*-person games the characteristic function was the outcome of the dynamic process of coalition formation. Rational behavior was consequently defined in terms of a process in which agents learn to calculate the benefits accruing to each player from joining every possible coalition. The ensuing concept of stable sets was defined as any subset of payoff vectors enjoying some desirable properties. However, within this set, players were not always able to determine the best alternative. von Neumann and Morgenstern approached this indeterminacy with a positive attitude because they were searching for the theoretical foundations of social science, in which indeterminacy was common.[7]

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[6] Mark Blaug (2003) attributes to increasing concentration on the end state of the equilibrium by orthodox economists in the 1950s the contemporary fall of game theory in economics.

[7] "All these considerations illustrate once more what a complexity of theoretical forms must be expected in social theory. Our static analysis alone necessitated the creation of a conceptual and formal mechanism which is very different from anything used, for instance, in mathematical physics. Thus the conventional view of a solution as a uniquely defined number or aggregate of numbers was seen to be too narrow for our purposes, in spite of its success in other fields. The emphasis on mathematical methods seems to be shifted more towards combinatorics and set theory—and away from the algorithm of differential equations which dominate mathematical physics." (von Neumann and Morgenstern 1947, p. 45). Even the reliance of *Theory of Games* on minimax was largely superseded by the opponents of *Theory of Games* (for example, Ellsberg 1956). This method was thought to be limited to zero-sum two-person

Awareness of the inadequacy of the individual maximization hypothesis for the social sciences permeated the scientific community following *Theory of Games*. A contemporary source of evidence was Luce and Raiffa's *Games and Decisions*, which influenced the reception of social scientists to game theory.[8] Its introduction contained skeptical remarks about the maximization criteria: "Though is not apparent from some writings, the term 'rational' is far from precise, and it certainly means different things in the different theories that have been developed. Loosely, it seems to include any assumption one makes about the players maximizing something, and any about complete knowledge on the part of the player in a very complex situation, where experience indicates that a human being would be far more restricted in his perceptions. The immediate reaction of the empiricist tends to be that, since such assumptions are so at variance with known fact, there is little point to the theory, except possibly as a mathematical exercise." (Luce and Raiffa 1957, p. 5).

On these grounds, the question of how to define a choice as optimal had to be addressed cautiously: "The problem for each player is: what choice should he make in order that his partial influence over the outcome benefits him most? He is to assume that each of the other players is similarly motivated. This characterization we shall come to know as the normalized form of an *n*-person game." (p. 6).

Games and Decisions also contains an extended discussion of the maximization of expected utility defined as the "postulate of rational behavior". Luce and Raiffa defined it as "ix. Of two alternatives which give rise to outcomes, a player will chose the one which yields the more preferred outcome, or, more precisely, in terms of the utility function he will attempt to maximize expected utility" (p. 50). However, the meaning of this definition is intended to be merely **tautological**: "We shall take it to be entirely tautological in character in the sense that the postulate does not describe behavior but it describes the word 'preference'." The only alternative was "to accept certain experimental operations as defining 'preferences' and then to attempt to verify postulate (ix). This is basically much simpler for the experimentalist, but experience indicates that it is not always successful." (p. 50).

games and its value was merely methodological. RAND's emphasis on it cannot be ascribed to von Neumann and Morgenstern. See, for example, Rapoport and Orwant (1962, pp. 2–3) and Mirowski (1992, p. 140).

[8] "A citation analysis of the literature in the social sciences between 1956 and 1965 suggests that R. Duncan Luce's collaboration with Howard Raiffa in the publication of *Games and Decisions* (1957) was probably the most influential event in the literature of that period for the spread of game-theoretic thinking by the early 1960s." (O'Rand 1992, p. 188). See also Riker (1992, p. 216).

If the skepticism surrounding the descriptive validity of the individual maximization hypothesis grew out of the first laboratory experiments, its normative meaning was rediscovered in the same period by Nash. Today, game theorists generally agree that equilibrium points restore the concept of a balance of forces, i.e., equilibrium, as does the main device for predicting the outcome of a game, and in this way, it promotes the criteria of individual maximization as the key assumption of economic behavior.[9] For example, Robert Aumann, in *What is Game Theory Trying to Accomplish?*, stated: "The Nash equilibrium is the embodiment of the idea that economic agents are rational; that they simultaneously act to maximize their utility. If there is any idea that can be considered *the* driving force of economic theory, that is it. Thus in a sense, the Nash equilibrium embodied the most important and fundamental idea of economics, that people act in accordance with their incentives." (Aumann 1985, p. 43). Hence, the essence of mainstream economics coincides with the concept of Nash equilibrium because it assumes that each player maximizes individually, given the assumption that others also maximize.

The viewpoint of historians is not very different. For instance, Mirowski argues: "There is no doubt that at this juncture Nash regarded himself as codifying a general principle of rationality from within the neoclassical tradition, as he then understood it; it bore no game-theoretic content or implications." (Mirowski 2002, p. 336). Subsequently, he defined Nash's equilibrium as "an attempt to extend constrained optimization of something which behaves very much like expected utility to contexts of interdependent payoffs." (p. 339).

This interpretation fits the opening lines of Nash's paper on non-cooperative games: "[*Theory of* Games] contains a theory of *n*-person games of a type which we could call cooperative. This theory is based on analysis of the interrelationships of the various coalitions which can be formed by the players of the game. Our theory, in contradistinction, is based on the absence of coalitions in that it is assumed that each participant acts independently, without collaboration or communication with any others. The notion of equilibrium point is the basic ingredient in our theory. This notion yields a generalization of the concept of the solution of a two-person zero-sum game." (Nash 1951, p. 286).

The equilibrium point was intended by Nash to replace von Neumann and Morgenstern's minimax criterion with a solution concept based on the maximization principle and on the idea that

[9] Myerson's article on the historical influence of the Nash equilibrium contains the following definition: "every member of society will act, within their domain of control, to maximize welfare as they evaluate it, given the predicted behavior of others. The concept of Nash equilibrium is, in its essence, the general formulation of this assumption." (Myerson 1999, p. 1069).

all players maximize. However, an analysis based on the hypothesis that players know how their opponents will act effectively removes the impact of social interaction on decision theory. This feature, *per se*, is a sufficient explanation because the community developing game theory in the 1950s criticized the equilibrium point as a means of dealing with strategic choice.

Luce and Raiffa (1957) can again be quoted as authoritative witnesses. Their discussion of the Nash equilibrium identified two major shortcomings. The first concerned the solution of the prisoner's dilemma, in which "although a 'rational' player can do no better than play his undominated strategy (assuming a single-shot game without preplay communication), two 'irrational' players always do better than two 'rational' ones." Another example is the repeated version, in which "The unique overall equilibrium behavior demands that each player employ his undominated strategy on each trial, which seems contrary to ordinary wisdom." The consequence is that the Nash equilibrium, besides being not universally applicable to non-zero-sum games, "is rejected as the basic tool for a normative theory", even if "it may be of pragmatic importance in descriptive studies." (pp. 111–112).

The second problem is the possibility of multiple equilibrium points: "[…] suppose that (σ_1, σ_2,…, σ_n) and (λ_1, λ_2,…, λ_n) are both equilibrium points of a general game, then, first, there is no assurance that an intermixture of strategy choices, such as (λ_1, σ_2, σ_3,…, λ_i,…, σ_j,..., λ_n) is also an equilibrium point; and second, there is no assurance that the payoff to a player is the same for two different equilibrium points, i.e., in general: $M_i(\sigma_1, \sigma_2,\dots, \sigma_n) \neq M_i(\lambda_1, \lambda_2,\dots, \lambda_n)$." (p. 172).

On this account, Luce and Raiffa's conclusions are discouraging: "The failure of the general equilibrium notion to have these two properties raises much more serious questions as to its merits than could be raised against the minimax concept. […] First, if each player were to confine his strategy choice to those which are a part of some equilibrium *n*-tuple, the resulting problem faced by each player is again a game. It is a contraction of the old game, but it may be just as difficult to analyze conceptually as the original game. Indeed, in some sense it may be more difficult for a player to analyze it because it crystallizes the difficulties involved. Thus, the equilibrium notion does not serve in general as a guide to action." (p. 172).

The validity of the equilibrium point was questioned by Luce and Raiffa on normative grounds. In fact, in the following decade, Nash's concept was rarely applied and was largely criticized using arguments similar to those quoted above.[10]

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[10] In the first textbook on game theory, McKinsey wrote: "It must be remarked that Nash's theory—although it represents a considerable advance—has some serious inadequacies and certainly cannot be regarded as a definitive

To summarize, in the 1950s, some features that placed game theory in opposition to the toolbox of mainstream economists were already manifest. In particular, the assumption of maximizing behavior as the normative and positive foundations of the theory of individual choice was considered to be inadequate by those game theorists who were attempting to develop a social theory of strategic interaction. The focus on minimax as the criterion of rationality that characterized most game theorists at RAND was restricted to two-person zero-sum games. Beyond this specific case, there was no unifying principle for selecting the best choice. A multiplicity of solution criteria was proposed but none was seen as superior to any other. Nevertheless, in subsequent years, the utilitarian model was not revised. A widely held view was that game theory had little effect on economics because the results of its first applications were already known to economists. The original contribution to the progress of theory was irrelevant.[11] What this position fails to recognize is that a call for revision of the postulate of individual maximization in earlier game theory led economists to ignore the new method.[12] The formal apparatus of mainstream economics relied on this assumption to such a degree that its validity was regarded as unquestionable. Moreover, the argument overlooks the fact that subsequently game theorists consciously pursued the replication of the same results that mathematical economists had already proved using other methods.

Martin Shubik's work was a key contribution to this interpretation. He deserves recognition for being the first economist to apply game theory systematically to economics. His 1953 PhD thesis, *Competition and the Theory of Games*, published with few changes as *Strategy and Market Structure* in 1959, provided an economic framework for game theory that incorporated virtually every subsection of contemporary handbooks of industrial economics. By quoting Morgenstern's premise to his book, Shubik "goes baldly after pivotal problems, some time-honored and associated with the finest names in economics. They are freshly formulated in a game theoretical manner, and the new solutions are carefully compared with earlier answers." (Shubik 1959b, p. IX).

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solution of the conceptual problem of this domain." (McKinsey 1952, p. 359). The same celebratory history of the Nash equilibrium written by Myerson (1999) admits that: "The impact of Nash's reconstruction of game theory spread slowly." (p. 1075). In the 1950s, exceptions to this lack of attention included the mathematical extensions of Nash's equilibrium made by Glicksberg (1952) and the first economic applications by Shubik and Thompson (1959).

[11] The unsympathetic judgment of Dorfman et al. in *Linear Programming and Economic Analysis* exemplifies this view: "the 13 years that have elapsed since the publication of *The Theory of Games* have seen no important application of game theory to concrete economic problems. The theory of games has had a profound impact on statistics and on military sciences; in economics it is still merely a promising and suggestive approach." (Dorfman et al. 1958, p. 445).

[12] A slightly different view emphasizes the idea that game theory was unsuccessful because its promulgators, von Neumann and Morgenstern: "were alienated from economics and were in many ways quite hostile to what was considered to be acceptable economics." (Weintraub 2002, p. 144). These two reasons must be kept separate. While the latter coincides with this paper's interpretation, the former does not fit with the arguments herein.

Notwithstanding his role as precursor, Shubik was the economist who proposed the first systematic adaptation of game theory to neoclassical economics. His 1959 paper on the identity between the core and Edgeworth's contract curve induced Aumann almost 30 years later to write in *The New Palgrave of Economics* that "in 1959 came Shubik's spectacular rediscovery of the core of a market in the writings of F. Y. Edgeworth (1881). From that time on, economics has remained by far the largest area of application of game theory." (Aumann 1987, p. 467).

In 1980, Schotter and Schwödiauer, by assessing the historical legacy of Shubik's paper, ascribed to it the role of a vehicle for reawakening economists' interests in the applications of game theory. At the same time, they argued that equivalence with Edgeworth's model had revived a view that had already been expressed by many reviewers of *Theory of Games*: "While this result was quite elegant, it spelled the end of the first renaissance in game theory: It seemed that the game theoretical analysis (which employed strictly cooperative game theoretical concepts) was too demanding informationally to be of any intuitive appeal. Since it yielded no new results, little could be gained through its use." (Schotter and Schwödiauer 1980, p. 480).[13]

Based on this view, Schotter and Schwödiauer considered that the process of introduction of game theory into economics had come to a standstill that lasted until the late 1960s. Aumann (1987) attributed to Harsanyi's 1967 Bayesian systematization of games of incomplete information, and to the final settlement of the so-called Nash program, the definitive acceptance of game theory. It is revealing that both these theoretical advances had nothing in common with the original inclination of game theorists towards the dismissal of the individual maximization principle. On the contrary, they revived this axiom by extending its applicability within the framework of game theory. Harsanyi's contribution dealt with games of incomplete information using the same formal apparatus applied to complete-information games. This made the Nash equilibrium point applicable to games in which differences in players' beliefs were not admitted by treating these as differences in players' information. The development of the Nash program further extended the applicability of the Nash equilibrium, and consequently, of the hypothesis of constrained individual maximization, by providing a non-cooperative equilibrium foundation for axiomatically defined solutions of cooperative games. Hence, both motivations for the establishment of game theory as a founding part of economics were deeply intertwined with the criteria of self-seeking maximization.

[13] The 1948 review of *Theory of Games* published in the *Economic Journal* deserves quotation: "[the economic applications of game theory] are in line with those given by the ordinary theory, and in some cases may be a little more general although the comparisons do not take account of the work on imperfect competition over the last twenty years. In view of this, it is to be regretted that the authors are in places less than generous to their colleagues who have tackled, not without success, these difficult problems with far less powerful tools." (Stone 1948, p. 200).

Normative economic theory extensively applied that part of game theory that did not conflict with its main assumption. The history dependence characterizing the status of mathematical economics also overcame the problem of the empirical validity of this hypothesis, which was already being questioned by the other subject of this paper, namely the coeval first attempts to introduce laboratory methods into economics.

4. LABORATORY TESTS VERSUS PREFERENCE CONSISTENCY

A comparison between the introduction of game theory into economics and the early years of experimental economics reveals many common features. Although the interplay between the two methods and economics is now considered a fruitful area of research, until recently, experimentalists experienced a surreptitious division between theoretical models and laboratory tests, some implications of which are explained by Alvin E. Roth: "When I began my own experimental work about a dozen years ago, it was most convenient to publish the results in journals of psychology and business." (Roth 1987, p. 1). The acceptance of experimental methods into economics was indeed anything but immediate. In the same paper, Roth delayed overcoming this ostracism until 1985, when the entry "Experimental Economic Methods" was added to the Journal of Economic Literature's classification system.

It is therefore not surprising that a detailed history of experimental economics has yet to be written. The first attempts to outline the evolution of the discipline (Plott 1982, Roth 1987a, Smith 1992, Roth 1993, and Dimand, forthcoming) recognize three phases: the early years, dating from 1948 to the early 1960s; the middle years, almost the whole of the 1980s; followed by a period of maturity. Chamberlin's first attempt to test an imperfect market took the lead,[14] while the next breakthroughs were Sidney Siegel and Lawrence Fouraker's work on bargaining behavior and Vernon Smith's reprise of Chamberlin's experiment. During this period, game theory made a major contribution to improving techniques for testing economic models. Siegel and Fouraker (1960) and Fouraker and Siegel (1963) made key contributions to this advance. Their influence has been widely recognized in the literature (Friedman 1969, Smith 1992, and Roth 1995). The acknowledgment of game theory as the *trait d'union* between a well-established research field,

[14] The only predecessor of Chamberlin was Thurstone's (1931) paper, which performed an experimental test of the existence of indifference curves, deservedly defined as an isolated fact.

market behavior, and the new analytical method of experimental economics is the main reason for this judgment, but there are other reasons. The first relates to the explicit consideration of information as a crucial experimental variable on the basis of the distinction between complete- and incomplete-information games. The second relates to the attention to detail in defining the rules of the games tested in experiments. The third involves the conversion of payoffs into salient cash rewards as a requirement for the validity of experimental findings.

However, Siegel and Fouraker's work is also noteworthy because it paved the way for the successive assimilation of laboratory experiments into economics. Their contribution corroborated some theoretical predictions of mainstream economics**.** In particular, Siegel's research project was rooted in the social psychology of Kurt Lewin, which strongly influenced him. In the 1930s and 1940s, Lewin also developed a theory that the functioning of the human mind can be explained by means of assumptions similar to those of expected utility theory.[15] Siegel built on Lewin's contribution by extending his dynamic model based on the concept of levels of aspiration to the area of subjective utility theory. Consequently, his main purpose was to define the experimental conditions that constituted a valid environment for testing neoclassical decision theory. Siegel and Fouraker (1960) showed a clear tendency for bargainers to maximize payoffs by selecting the Pareto optimal solution and dividing the surplus equally. This outcome became more likely when greater amounts of relevant information were available to the bargainers. Fouraker and Siegel (1963) confirmed the validity of the Nash equilibrium prediction in bilateral monopoly and oligopoly in different treatments. The interpretation given to the emergence of equilibrium points had a particular neoclassical flavor: "Lack of information favors an equilibrium solution, where each participant seeks the best individual strategy, given the behavior of the other participants. This condition tends to dampen communication, although there is a tendency towards rivalistic behavior as the number of participants increases. Even if appropriate Pareto optimal goals exist in such a situation, there is no efficient way of reaching them because they cannot be mutually recognized." (Fouraker and Siegel 1963, p. 209).

The trend for experimentalists to embrace the faith in mainstream economics was reinforced by Smith's version of Chamberlin's experimental imperfect market, which closed the early period of experimental economics.[16]

[15] A detailed account of Lewin's contribution is given by Mandler and Mandler (1969).

[16] It is noteworthy that Vernon Smith's (1992) historical reconstruction of the early years of experimental economics devoted a whole section to Siegel's contribution in which this orthodox interpretation was supported (pp. 265–275).

Smith's reference point was, by Chamberlin's own admission, very modest: "The social scientist who would like to study in isolation and under known conditions the effect of particular forces is, for the most part, obliged to conduct his experiment by the application of general reasoning to abstract models. He cannot observe the actual operation of a real model under controlled conditions. The purpose of this article is to make a very tiny breach in this position: to describe an actual experiment with a market under laboratory conditions and to set forth some of the conclusions indicated by it." (Chamberlin 1948 p. 95).

Indeed, the realization of Chamberlin's project was largely imperfect. It tested a "random meetings economy: more-or-less simultaneous bilateral bargaining with no opportunity for the complete multilateral dissemination of information, and no opportunity to learn by repeated exchange through successive market trading periods." (Smith 1992, p. 243). He created an experimental imperfect market by using rough or largely unspecified rules: the market did not provide for either a protocol for the bargaining process or devices for motivating subjects. Therefore, the test was bound to raise more methodological problems than it solved for those who wanted to venture into experimentation with economic models. Nevertheless, Chamberlin's exploratory attempt provided some evidence that the hypothesis of perfect competition may have been less empirically relevant than an alternative one. Moreover, its design introduced the technique for inducing individual reservation prices and aggregate supply and demand curves that was later applied by most experimentalists.

By taking into account these considerations and the author's prominence, it is surprising that "according to the Social Science Citation Index, Chamberlin's paper was cited by other authors only four times between its publication in 1948 and its revival in 1962 by Vernon Smith." (Bergstrom 2001, p. 183). As often happens, the historical recognition of Chamberlin's experiment was due to its refutation. By introducing a sequence of trading periods instead of an uninterrupted series of exchanges and by using the double oral-auction procedure, Smith obtained a robust convergence toward competitive equilibrium in different versions of the experimental design.

As in Siegel and Fouraker's experiments on models of bilateral monopoly and oligopoly, Smith's outcome corroborated the established theories in a way that appealed to mainstream economists. The community of interests based on the work of Sidney Siegel and Vernon Smith was documented in a letter written by Smith to the editor of the Journal of Political Economy on October 26, 1961: "Incidentally, I have just today met Sidney Siegel for the first time and I have his to-be-published material on experiments in oligopoly. The results are terribly interesting. Duopolists, who only know their own profit outcomes (incomplete information), go to the Bertrand competitive price solution. Triopolists do the same but faster. This suggests that my competitive price results might be achieved in still thinner markets. But the real shocker is the effect of complete information in which duopolists know each other's profit outcomes. As the amount of information increases, duopolies decrease their tendency to the Bertrand competitive price. The invisible hand only works when it is invisible?!" (VSP, Box 14). Smith's confidence in the robustness of his own results was further strengthened by Siegel's work.

Although there is no doubt that those contributions accelerated the methodological improvement of the discipline, the presumption they created represented a constraint for subsequent developments. On the one hand, a new methodology confirming what had already been shown or proved with other methods should have been more readily accepted, but on the other, the absence of immediate new results reduced the usefulness of making efforts to absorb the new technique. The prevalence of the latter argument probably had the effect of hindering the adoption of experimental techniques by economists, which instead was promoted by later developments, as I argue in the next section. However, before discussing this point, it is worth showing that the rejection of the conservative attitude was already present in the early years of experimental economics.

In the 1950s, some experiments questioned some basic hypotheses of economic theory by using arguments that were to become more influential later. The two main topics of research were experimental gaming and individual choice. With regard to testing the predictions of game theory, the most common conclusion of experimental activity was that the observed results did not generally corroborate theoretical predictions. If game theorists themselves could not find optimal strategies in real laboratories, then von Neumann and Morgenstern's stable sets, i.e., the Nash equilibrium and Shapley value, would have to be revised to take into account experimental evidence.[17]

However, the major challenge to conventional economics came from the experimental investigation of individual choice. Debate about this issue was characterized by an effective criticism of the idealized behavior attributed to expected utility maximizers, and generally, to the idea that rationality in economics could be defined in terms of preference consistency. In the same period in which expected utility theory had become dominant, experimental work was producing robust evidence that contradicted its main assumptions.[18] A common thread links the most notable

[17] The history of experimental gaming is outlined in Roth (1993) and Dimand (forthcoming). Rapoport and Orwant (1962) were the first authors to survey this literature.

[18] The coeval foundation of expected utility theory also had its experimental supporters, who are briefly reviewed below.

contributions supporting this criticism, including contributions by Allais (1953), May (1954), and Edwards (1953, 1954).

The Allais paradox is probably the most celebrated experiment in the history of economics. It started a new field of study that was to model modern decision theory. In 1953, the paradox identified some regularities of choice behavior that were later to give rise to the new discipline of cognitive economics. Nevertheless, the results of Allais' experiments were interpreted by Allais himself as representing a paradox in his 1979 book, in which he proposed a "neo-Bernoullian" approach to economic choice to accommodate the violations of expected utility documented in 1953. In the 25 years that elapsed between these two contributions, the reaction of decision theorists was merely a reassertion of the normative validity of expected utility theory over the descriptive one.[19] However, Allais' view, stated in the English summary to the article, was quite different:

> (5) If rationality is to be defined as adherence to one of the systems of axioms which leads to a Bernoulli type formulation, then obviously no discussion is possible. Such a definition, therefore, has no interest *per se*. That is to say that rationality, to be interesting from a scientific point of view, must be defined, in our opinion, in either of two ways. First, it may be defined in the abstract by referring to a general criterion of internal consistency employed in the social sciences, that is, a criterion implying the coherence of desired ends and the use of appropriate means for attaining them. Secondly, rationality can be defined experimentally by observing the actions of people who can be regarded as acting in a rational manner.
>
> (6) The principle of internal consistency implies only: (a) the use of objective probabilities when they exist, and (b) the axiom of absolute preference which states that out of two situations, one is certainly preferable if, for all possible outcomes, it yields a greater gain. Together these two conditions are less restrictive than the formulation of Bernoulli. Consequently, there are rational types of behavior (in the sense of rationality defined above) which do not obey the Bernoulli formulation. It cannot be said, therefore, that a rational man must behave according to the Bernoulli principle.
>
> (7) The experimental observation of the behavior of men who are considered rational by public opinion, invalidates Bernoulli's principle. (Allais 1953, pp. 504–505).[20]

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[19] A detailed account of Savage's change of attitude on this issue is given in Jallais and Pradier (forthcoming).

[20] The conclusions of the paper are even more resolute: "Il résulte de tout ce qui précède que l'erreur fondamentale de toute l'école américaine, c'est de négliger indirectement et inconsciemment, la dispersion des valeurs psychologiques. [….] La négligence implicite par la formulation de Bernoulli de la dispersion des valeurs

Allais' main purpose was to show that the definition of rationality in terms of consistency had to be dismissed as a normative explanation of economic choice. Thus, although the assimilation of his experiment as a counterargument limited to the descriptive validity of expected utility theory misses the point, it has essentially been dominant, at least since the 1970s. The first replication of Allais' experiment was made 25 years later (Morrison 1967), but only Slovic and Tversky (1974) provided clear experimental evidence confirming the original paradox. A brief history of the ensuing "grudging acceptance" is provided by Camerer (1995) who postponed such acceptance to the 1980s for the following reasons: "These contributions got relatively little attention in the United States and England, both for sociological and scientific reasons. Their articles are bluntly critical of expected utility (and of some other alternative theories); I suspect many American readers are put off by the critical tone. Most of the work is published in book chapters or in journals like *Theory and Decision* and *Journal of Economic Psychology*, which are more widely read in Europe than in United States." (Camerer 1995, p. 627). In fact, economists dealt with Allais' view on rational choice by practically ignoring it.

Another decision theorist who criticized expected utility theory by using experimental tests was May (1954). Specifically, he showed that it was possible to elicit intransitive preferences in choices involving no uncertainty. He conducted an experiment in which he asked students to express their preferences on three characteristics of potential mates, namely intelligence, wealth, and beauty. The choices were to be made between pairs of potential mates endowed with these qualities to various degrees. The results showed that students' preferences were often intransitive. In the same paper, this finding was employed to reformulate Arrow's impossibility theorem. May proposed a function of social preference with the properties of completeness, unanimity, and absence of dictatorship as in Arrow's formulation but he replaced the property of independence of irrelevant alternatives with that of positive responsiveness, according to which a single individual can change his or her preference on a paired comparison without affecting the former social ordering. With this modification, May proved that simple majority rule was the only rule satisfying these four properties. May's proof was mentioned in Luce and Raiffa's *Games and Decisions* (1957, pp. 357–359) as the starting point for subsequent work on the failings of majority rule that did not acknowledge its experimental basis. Evidence against transitivity had to incorporate the Nobel-

psychologiques a pour conséquence que cette formulation ne saurait être valable, comme l'a successivement prétendu l'école américaine, ni pour représenter le comportement de l'homme réel, ni pour déterminer sa satisfaction absolue (cardinal utility), ni même pour donner 'une règle raisonnable de conduite à un home raisonnable'." (Allais 1952, p. 544).

prize-winning work of Kahneman and Tversky to constitute a different theoretical approach to rational choice.

In the mid 1950s, Ward Edwards proved experimentally that individuals have preference orderings over probabilities: they prefer some probability values to others. For example, in some experimental trials, subjects overweighted 0.5. Moreover, Edwards found that both risk preferences and preferences over probabilities were relevant in determining choices. In bets with the same expected value, some subjects preferred losing large amounts of money with a low probability to losing small amounts of money with a high probability. Edwards stated clearly that preferences over probabilities implied that expected utility theory was useless, not only descriptively, but also normatively, because it did not generate adequate utility curves to explain the experimental behavior. In addition to this contribution to experimental economics, Edwards (1954) provided a comprehensive review of the economic theory of decision-making of the early 1950s that scrutinized virtually all variations of expected utility theory. Having reviewed the theoretical inconsistencies of this approach, Edwards pointed out that, to become meaningful, these criticisms had to be tested experimentally. Edwards believed that Allais had successfully showed how the hypothesis of expected utility maximization was violated when bets involved significant amounts of money, but Mosteller and Nogee's (1951) experiment had supported the same theory by using small monetary values and by ignoring the different effects of winning and losing bets. Even this argument was lost in subsequent appreciation of experimental work by leading decision theorists.

To explain this lack of reaction, it is worth quoting Wallis and Friedman's (1942) review of the first experiment on indifference curves made by Thurstone in 1931: "It is questionable whether a subject in so artificial an experimental situation could know what choices he would make in an economic situation; not knowing, it is almost inevitable that he would, in entire good faith, systematize his answers in such a way as to produce plausible but spurious results." (Wallis and Friedman 1942, p. 180). This critical assessment was justified by the inadequacy of Thurstone's design to motivate subjects.[21] However, it also revealed that decision theorists were actively searching for a tool with which to test their models. Preston and Baratta (1948) made the first experiment to respond to Wallis and Friedman's critique. The most cited experiment among those supporting expected utility theory, i.e., Mosteller and Nogee (1951), grew out of the celebrated discussion of utility analysis by Friedman and Savage (1948): "Plans for this experiment grew directly out of discussions with Friedman and Savage at the time they were writing their paper. W.

[21] This point is extensively discussed by Roth (1995, pp. 5–6).

Allen Wallis also contributed to the discussions." (Mosteller and Nogee 1951, p. 372). In their paper, Friedman and Savage stated their aim in the following unpretentious way: "This paper attempts to provide a crude empirical test by bringing together a few broad observations about the behaviour of individuals in choosing among alternatives involving risk. […] At the outset it should be confessed that we have conducted no extensive empirical investigation of either class of phenomena. For the present, we are content to use what is already available in the literature, or obvious from casual observations, to provide a first test of the hypothesis and to impose significant substantive restrictions on it." (Friedman and Savage 1948, pp. 282–283). The opening lines of Mosteller and Nogee's paper seemed to offer a solution to this lack of empirical relevance: "Although the notion of utility has long been incorporated in the thinking of economic theoreticians in the form of a hypothetical construct, efforts to test the validity of the construct have mostly—and in many cases necessarily—been limited to observations of the behavior of *groups* of people in situations where utility was but one of many variables […] The basic assumptions outlined by Friedman and Savage for constructing utility curves are adopted for the analysis of the present experiment." (Mosteller and Nogee 1951, p. 371–372). Mosteller and Nogee's experiment found that subjects adhered to expected utility in 70% of the cases. Notwithstanding some methodological weaknesses,[22] the paper gave rise to a series of laboratory tests that provided evidence confirming the same finding.[23] The quantity and quality of this scientific production proves that experimental methods were judged as anything but useless. However, the proponents of expected utility theory considered this source of evidence positively insofar as it corroborated what they had proved by using thought experiments. However, by the early 1970s, experimental refutations of the expected utility paradigm multiplied. The effect was that theoretical work began to produce various generalizations of expected utility and alternative models to systematize such refutations. The impact of these developments is well known: it deserves recognition for the 'official' acceptance of experimental economics. The emergence of scientific paradoxes has had the beneficial effects of revealing the anomalies contained in the generally agreed definition of economic rationality and of suggesting different ways of reviving the empirical content of economic theory. However, this is no longer a historical issue; rather, it represents food for thought on the present status of economic methodology.

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[22] Camerer (1995, p. 620) discusses extensively the drawbacks of Mosteller and Nogee's design.

[23] Rousseas and Hart (1951), Attneave (1953), Hurst and Siegel (1956), Davidson et al. (1957), Coombs and Komorita (1958), and Davidson and Marschak (1959). This literature is surveyed in Camerer (1995) and Roth (1995).

5. PARADOXES VERSUS FORMALISM

Today, it is widely acknowledged that game theory and experimental methods have enhanced the effectiveness of economics by revising its theoretical models and by extending its applications. However, this does not seem due to the corroboration of the hypotheses of conventional economics. On the contrary, it is attributable to the recent emergence of other paradoxes that have strengthened the arguments for considering the assumptions of maximizing behavior and of preference consistency as being descriptively implausible and normatively unsatisfactory.

Game theorists have increasingly adhered to the view that strategic interaction cannot be dealt with merely by using the principle of self-seeking maximization. Today, the need to differentiate between individual and collective rationality is a growing concern: "Game-theoretic methods provide an intriguing alternative to treating a group as though it were a sentient individual: we can cast the members of the group as players in an internal organizational subgame, vying for control of the group's actions in the larger game. It may be neither profitable nor necessary to regard the group's corporate behavior as though it were governed by any kind of individualistic preference structure." (Shubik 1982, p. 109).

This change has been facilitated by the emergence of two strands of research that have reinforced the unconventional attitude of game theorists. The first is laboratory work on the Prisoner's dilemma; and the second is evolutionary game theory.

The great influence exerted by the paradox of the Prisoner's dilemma is illustrated by its status as the key example in nearly all microeconomics texts used to introduce game theory to neophytes. This game represents the incarnation of the idea that the concept of individual rationality does not generalize in any unique or natural way to group or social rationality. By pointing out the inefficiency of the Nash equilibrium by means of similar arguments to those discussed in Luce and Raiffa (1957), the continuous, uninterrupted activity of laboratory tests of the Prisoner's dilemma has seriously weakened the appropriateness of individual maximization as a normative principle of economic choice. The impact of this contradiction is significant to the extent that variations on the Prisoner's dilemma have motivated the most influential applications of combining experimentation and computer simulation (Axelrod 1984).

The other paradox is an intrinsic component of evolutionary game theory. In this approach, which was first developed by John Maynard Smith (1982), individual choice plays no role in determining the final outcome. The players of the game are two interacting populations. The main assumption is that there is no decision making at all. Any outcome is the result of a process in which individual maximization is eliminated and strategies themselves are modeled as opposing plays. The sole criterion of effectiveness is the increase in the expected number of the offspring of a certain population. In this way, the pivot of conventional economics is discarded. While mainstream economics deals with rational maximizing agents, evolutionary game theory analyzes how patterns of behavior emerge, diffuse, and stabilize. The dynamics chosen to elicit behavior are the population dynamics that replace individual choice as an analytical tool. The paradox is that in laboratory experiments, boundedly rational players in evolutionary models often outperform maximizing agents.[24]

It is no accident that both approaches are closely connected with laboratory activity, which is the other source of paradoxes studied in this paper. The more recent history of experimental economics is characterized by the agreement of experimentalists on empirical findings that contradict the foundations of mainstream economics. The first is that preferences are highly context dependent. Second, experimental subjects prefer fair payoffs to maximized ones.[25] On this basis, experimental research aims to identify more precisely the motivational forces behind this evidence. Theoretical economists have taken into account this evidence by starting to question the definition of rationality in terms of a system of consistent axioms. Expected utility theory has been revised to assume not only normative but also descriptive value. The assumption of egoistic economic agents has been attenuated in various ways. Although this does not yet represent a general call for a revision of conventional decision theory, it has produced changes in economists' ways of thinking that explain, for example, the incorporation of cognitive and learning processes as variables in economic models.

Even if it is not the purpose of this paper to assess the methodological status of the discipline, some general implications of the paper's arguments can be summarized. The advantages of formal mathematical reasoning in economics are listed in numerous papers. A recent survey by Backhouse mentions the following: "It enables researchers to build on the work of their predecessors in a way that would otherwise be impossible. Formal arguments are explicit and can, therefore, be understood by subsequent researches. The way is open, therefore, for economic knowledge to become cumulative." (Backhouse 1998, p. 1852).

This paper has attempted to argue that this cumulative process can become detrimental if the breaking down of complex chains of reasoning in explicit and easy series of steps referred to by Backhouse as the common characteristics of formalization, axiomatization, and mathematization in

[24] See, for example, Binmore and Samuelson (1992) and Friedman (1996).

economics induces a crystallization of the theoretical assumptions to the point that they are not questioned.[26] In this way, modeling is bound by past methodology. Once this process has ascribed to a formal assumption or to a set of methods the status of a core belief, it ceases to be questioned. The nature of the thought experiment, which characterize mainstream economics according to Rubinstein's 'perceptive' interpretation, corroborates this crystallization by invalidating empirical evidence as a means of refutation.

This analysis of the early years of game theory and experimental economics has two main implications. First, on the one hand, it led economists to neglect the criticisms that game theorists and experimentalists made of some fundamental hypotheses of mainstream economists, and on the other hand, led them to overvalue applications that corroborated those assumptions. Second, the current growing diffusion of game theory and experimental methods is due to the emergence of other paradoxes that are even more manifest than those overlooked in the past. This historical evidence supports the main argument of this paper, namely that paradoxes are a useful tool with which to challenge the absence of contradictions that has characterized the use of formal mathematical reasoning in economics since the 1940s.

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[25] Ledyard (1995) and Schram (2000) present extensive surveys documenting that, in a variety of experimental situations, subjects deviate from own-payoff maximizing behavior.

[26] "The mathematisation of economics, has in practice involved breaking down arguments into smaller steps, so that mathematical techniques, over which there is no disagreement, can be used for as many steps as possible." (Backhouse 1998, p. 1849).

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