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The Early Developments of Experimental Economics: the Influence of Game theory

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THE EARLY DEVELOPMENTS OF EXPERIMENTAL ECONOMICS: THE INFLUENCE OF GAME THEORY

by

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Abstract. An historical overview of experimental economics points out significant analogies between its origins and the contemporary emergence of game theory. In both cases, their effective introduction in economics was postponed until the 1960s. Such a delay cannot be ascribed to the supposed division between empirical and theoretical work but to the fact that in the 1950s experimental methodologies, such as game theory, were mainly developed by scholars of other social sciences which forced a re-examination of the prevailing postulates of economic theory. This interdisciplinary community gave rise to two different approaches: the socio-psychological one represented by experimental games, and the economic-managerial one represented by business games. Although both approaches were an outgrowth of game theory, the prevalence of the first had important consequences on subsequent developments.

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1. Introduction

Experimental economics has not been acknowledged as a constituent part of economic science until recently. Therefore, it is not at all surprising to find that its history has not yet been examined very thoroughly, although forty years have passed from the appearance of the first contributions. This delay can be partially attributed to the absence of a widespread agreement on the first principles of the discipline. But another cause is that economists have often viewed any kind of contamination with suspicion. The idea that human behaviour may be described by referring only to economic principles is still supported by many influential schools of economics. The consequences of this assumption that will not be discussed in the following pages are however crucial for the main purpose of this paper, that is to describe how the origins of experimental economics were characterised by an interdisciplinary approach. Moreover, another controversial relation will be discussed, that between theory and empirics. Even with respect to this point, economists seem to have drawn over sharp distinctions (like that, for example, between industrial economics and business science) with the aim of emphasising their status of mathematical and deductive scientists, rather than using empirical and inductive ones.

This clear-cut division between theoretical models and experimental tests did not seem to characterise other social sciences and this fact produced paradoxes such as that pointed out by Alvin E. Roth in an overview of experimental economics:

> 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).

Statements like this give evidence of the obstacles opposed to the introduction of experimental economics in the corpus of economics. In the same article, Roth puts off the overcoming of this ostracism until 1985, when the *Journal of Economic Literature* introduced the entry "Experimental Economic Methods" in its classification system. But to divide in periods the history of experimental economics we can refer to other sources. One of its leading protagonists, Vernon L. Smith, proposes to distinguish three phases:

1. The early years	1952-1962
2. The middle years	1963-1975
3. The maturity	1975-today.

In this way, Smith identifies the beginning of experimental economics in the 1952 Santa Monica Conference, and recognises two turning points: the first being by the publication of its own version (Smith 1962) of the "experimental imperfect market" proposed by Edward

Chamberlin (1948), the second by the fact that since 1975 "experimental economics experienced such an explosion of growth that a separate and larger-scale treatment is essential" (Smith 1992, p. 278).[1]

If there is a widespread consensus about the passage between the second and the third phase (Plott 1982, Roth 1987a, Roth 1993 and 1995, Dimand 1999), to fix the starting point of the story is a more controversial question. A different proposal may be the following:

1. The origins	1948-1959
2. The take-off	1960-1975
3. The maturity	1975-today.

The start of the first phase would correspond to the publication of Chamberlin's paper while the second phase would start with the publication of Sidney Siegel and Lawrence Fouraker's *Bargaining and Group Decision Making*. The historical importance of Chamberlin's experiment is still in debate in literature. Some authors are inclined to consider it as an isolated fact and therefore not determinant for the following developments,[2] while others assess it as the contribution of an initiator, that is an author that, even though he didn't assume the role of the forerunner, was the first to highlight a question or a method by proposing it to his successors.[3] The second turning point would be given by the book of Siegel and Fouraker (1960), that played the role of establishing the modern experimental methodology by making a synthesis of two approaches, the socio-psychological and the economic-managerial, carried out by two different scientific communities.

To argue about this explanation of the origins of experimental economics is the main purpose of this paper. Besides the merely chronological questions, the object of the analysis is to show that experimental economics originated as an outgrowth of game theory and that, therefore, its evolution can be described by making explicit reference to the introduction of game theory into economics.[4] An overview of the early years of experimental economics indeed shows that in

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1. In his article, Smith does not intend to make an account of the history of experimental economics, but his influence gives to the paper, that in his own words is made of "personal impressions", such a value.

2. Among the others, Martin Shubik reports: "As far as I can see his class exercise was an isolated event and influenced no one I know. Bill Parker (economic historian) actually played in Chamberlin's game. He notes that it was used only as a class exercise, not with any thought of experiment" (in Smith 1992, p. 248). Even more caustic judgements are attributed to the same subjects of the experiment: "The scuttlebutt among the Harvard graduate students was that the whole exercise was a sort of silly" (Smith 1981, pp. 154-155).

3. The only contribution before Chamberlin's - to my knowledge - is the paper by L. L. Thurston (1931) concerning an experimental test of the existence of indifference curves, deserving better the definition of an isolated fact.

4. In his introduction to a collection of papers on experimental economics, Vernon L. Smith describes the origins of experimental economics in the following way: "It is a curious fact that during the

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. This heterodox orientation of experimental economics and game theory can explain because their definitive introduction in economics was postponed until the 1960s, when the emerging of new paradigms of economic behavior left room for a variety of interpretations.

The paper is structured in the following way. Section 2 outlines the characteristics of this latter process and points out the analogies with the former. Such a parallelism is considered a fact so relevant as to explain by itself the influence of game theory on experimental economics, but Section 3 discusses other arguments, particularly the role played by business and experimental games, which further justify the importance of this relation. The other thesis of the paper - analysed in Section 4 - is that in the 1950s, similarly to what happened for other economic applications of game theory, the progress of experimental economics was delayed by the scepticism of economists about the new method founded by John von Neumann and Oskar Morgenstern. The diffusion and application of game theory was indeed carried out by a scientific community pursuing a strongly interdisciplinary approach, which kept it separate from the leading economic schools.

2. The interaction between game theory and experimental economics

Today, the interplay between game theory and experimental economics is generally considered as one of the most fruitful area of research. To explain the close relation existing between these two methods, it is useful to go over the process of introduction of game theory into economics.

In the authoritative entry "Game Theory" of *New Palgrave of Economics*, Robert J. Aumann writes:

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years 1956-1960 when I was conducting the experiments that would be published in 1962, several others were independently starting to make use of the techniques that would ultimately become known as the methodology of experimental economics". One of the purposes of this paper is to enlighten the reader about the meaning of such a curious fact, that Smith seems to describe further on as a "multiple discovery" à la Merton: "Here, indeed, seems to be an example in which the time was ripe for an idea, and more than one person responded. It is my hypothesis that there were a number of us at this time, about whom it can be said that we were (1) dissatisfied with the state of our empirical knowledge of the credibility of economic theory and (2) harboured considerable curiosity about how economic processes actually worked» (Smith ed. 1989, pp. 1-2).

> The 1950's were a period of excitement in game theory. The discipline had broken out of its cocoon, and was testing its wings. Giants walked the earth.(...) And 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).

According to Aumann, the incubation period of game theory ended in 1959, when its introduction in economics was definitively established. This judgement is also confirmed by other historical references (Schmidt 1990, Dimand and Dimand 1992, Mirowski 1992, Leonard 1992 and 1995, Innocenti 1995): from the publication in 1944 of *Theory of Games and Economic Behavior*, fifteen years went by before its most important scientific applications could be definitively founded. In the meantime, even if some precursory contributions were published, it was mainly the mathematical language of game theory which was to be improved at an unparalleled rate. At the end of the 1950s, most of the tools that had to be applied in economics were already defined, but such a mathematical advance occurred with extensive application only in social sciences different from economics[5]. Exceptions were very few. A first example was the analysis of oligopolistic markets developed by Martin Shubik (1953, 1959a). In bargaining theory, Harsanyi (1956) applied solution criteria proposed by Nash to Zeuthen's model. For competitive markets, both the proof of the existence of competitive equilibrium (Arrow and Debreu 1954) and the proof of the identity between *core* and contract curve (Shubik 1959b) employed game theory. Finally, decision theory introduced game theory in its toolbox both for models in conditions of certainty, by founding the mathematical apparatus of linear programming, and for models in conditions of uncertainty, through games against Nature.

But these contributions don't permit one to consider the 1950s as the period in which economics and game theory established a fruitful interaction for at least two reasons. The first is that most applications applied only the mathematical tools created or used by game theorists (i.e., fixed point theorem, minimax theorem or Nash equilibrium). The second is that these applications - including those contained in *Theory of Games and Economic Behavior* - didn't exploit the originality of the new approach and merely confirmed neo-classical principles by proving results already obtained through differential mathematics (Innocenti 1995).

These characteristics would seem to be shared by the same work that, in Aumann's opinion, was permitted to escape this impasse. Shubik (1959b) proved the correspondence between a nineteenth-century concept, Edgeworth's contract curve, and the core proposed by

5. Although in the 1950s the main field of application of game theory was represented by war games, much work concerned psychology, sociology and politics (See Luce and Raiffa 1959, Mirowski 1991, O'Rand 1992, Rider 1992, Riker 1992, Leonard 1995).

Shapley and Gillies in 1953 as a solution concept for cooperative games.[6] In 1980, Schotter and Schwödiauer, making an assessment of the historical meaning of this paper, ascribe it the role of a "vehicle" giving renewed attention to the economic applications of game theory. At the same time, they point out that the equivalence with Edgeworth's model had revived a standpoint already expressed by the reviewers of *Theory of Games and Economic Behavior:*[7]

> 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 intuitively appeal. Since it yielded no new results, little could be gained through its use (Schotter and Schwödiauer 1980, p. 480).

On such a view, Schotter and Schwödiauer consider the process of introduction of game theory into economics had come to a standstill that lasted until the early 1970s. However, this judgement neglects an important characteristic of Shubik's *Edgeworth market games*: although the author applied the tools defined by von Neumann and Morgenstern to the Walrasian equilibrium model, his outcome differed from Arrow and Debreu's proof of competitive equilibrium as Edgeworth's model differs from Walras'. While Walras and Arrow - Debreu based their contributions on an exogenous price system, Edgeworth and Shubik founded their models on an explicit process of bargaining among the economic agents. This last assumption, besides being more realistic, created a more fertile ground for the application of game theory.

A comparison between this story and the beginnings of experimental economics points out that they have a lot in common. The acknowledgement of Chamberlin (1948) as a precursory paper allows us to consider the 1950s as the "heroic" period of experimental economics. In that decade - how it is argued in the following section - the application of game theory decisively improved the methodologies for testing economic models, even if such progress was not as quick as in other social sciences. Another proof of the parallelism was given by the publication of Siegel and Fouraker (1960). The importance of this book is widely recognised in literature (Friedman 1969, Shubik 1988, Smith 1992, Roth 1993 and 1995). For what concerns the object of this paper, it is sufficient to remember that Siegel and Fouraker tested some bargaining models (Zeuthen/Nash/Harsanyi, Bowley, Edgeworth/Shubik) translated in new versions through game theory. The recognition of this new language as a *trait d'union* between an established discipline, theoretical economics, and a new research area, experimental economics, is one of the main reasons of the historical importance of Siegel and Fouraker's work, but some others can be

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6. The first definition of *core* is attributable to two contemporary works: D. B. Gillies, *Some Theorems on N-Person Games*, Ph.D. Dissertation, Department of Mathematics, Princeton University, 1953 and L. S. Shapley, "Open Questions", included in *Report of an Informal Conference on the Theory of N-Person Games*, mimeo, Princeton University, 1953.

7. For an account of the reviews of *Theory of Games*, see Innocenti (1993).

added. First of all, the explicit introduction of information among the variables considered, as a consequence of the distinction between games with complete and incomplete information.[8] Secondly, the detailed description of instructions and rules adopted in the experiments. Finally, the recognition of the monetary incentives as a crucial factor to the validity of experimental findings. [9]

But the main element explaining the value of that book is that it represented the first successful collaboration between a psychologist and an economist in testing social models of behaviour. Before their contribution, two distinct approaches to experiments were separately pursued: the socio-psychological, which readily absorbed the originality, not only formal, of game theory, and the economic-managerial, that represented the first effective way of verifying economic behaviour in controlled experiments. However, a link connecting these distinct paths to founding experimental methodology existed, specifically that both were an outgrowth of game theory.

3. The two paths to the foundation of experimental methodologies

In the experimental imperfect market analysed by Chamberlin (1948), any reference to game theory was absent. Although it is reasonable to suppose that Chamberlin had seen von Neumann and Morgenstern's *Theory of Games*, his experiment aimed to confute one of the main neoclassical postulates: the tendency to a market clearing vector of prices in perfectly competitive markets.

Apart from the experimental outcomes, replicated with opposite results by Smith (1962), Chamberlin's plan was, in his own words, modest: he wanted to make "a very tiny breach" in the belief that economics did not need to verify its theories in controlled experiments.[10] The main reason of his failure is that the realisation of his project was largely imperfect. It tested a

> random meetings economy: more-or-less simultaneous bilateral bargaining with no opportunity for the complete multilateral

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8. Siegel and Fouraker studied the effect of increasing the amount of relevant information in games of bargaining and concluded that "bargainers with complete information have more realistic expectations with respect to their own profit than less informed bargainers, and are under a sort of moralistic pressure for a fifty-fifty split of the joint payoff" (Siegel and Fouraker 1960, p.70).

9. Their experimental design employed intermediaries to exchange offers and subjects were paid in cash.

10. "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).

> dissemination of information, and no opportunity to learn by repeated exchange through successive market trading periods (Smith 1992, p. 243).

He defined an experimental imperfect market by using rough rules: it did not provide for either any protocol for the bargaining process or devices for motivating subjects. Therefore, his attempt created more problems than it solved for those who wanted to venture into the experimentation of economic models.

But in the 1950s, better conditions for the process of refinement of experimental methodologies had to be created by game theory, whose influence followed two distinct paths: the first, corresponding to the social-psychological approach, was expressed by experimental games, the second, defined as the economic-managerial approach, by business games.

In order to outline the main characteristics of the evolution of experimental gaming, one can start from an analogy with econometrics. While the latter was originated by the work of Fisher, Frisch and Tinbergen and by the ensuing foundation of the Econometric Society, the former stemmed from the Rand Corporation that, in the summer of 1952, promoted at Santa Monica, California a conference on "The design of experiments in decision processes".[11]

Among the papers discussed in that occasion, two had a noticeable effect on the following developments. The first paper was presented by Estes (1954) - and discussed by Flood (1954a and 1954b) - and tested a stochastic learning model, the other was proposed by Kalisch, Milnor, Nash and Nering (1954) and concerned the theory of n-person games.

The experiment made by Estes was aimed at showing that learning could be represented as a stochastic process converging in an exact prediction of the probability distribution associated with the real event. The test of the model was intended as corroborating the definition of rationality peculiar to game theory, which was based upon such a probabilistic approach.

But Estes' paper was so relevant for the debate it gave rise to. A first reply was given by Flood (1954a) at the same conference. In his paper, Flood criticised Estes' experiment similarly to how economists attacked game theorists, particularly for having made too strong assumptions about the informative and computational capacities of the subjects and for the unrealistic use of mathematical concepts like mixed strategies. The same arguments were recalled and developed by Herbert Simon in his 1957 book, *Models of Man*, which proposed to distinguish between the subjective rationality of the experimental subject and the objective rationality of the experimenter. The first would be founded on a perception of the external world which doesn't necessarily coincide with the second, expressed by the characteristics of experimental design. The main consequence would be that the learning process was aimed more at identifying the model for decision with the best fit among those perceived by each subject than at defining a behaviour that maximises the final outcome of the game. Moreover, Estes' paper also influenced

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11. Thrall, Coombs and Davis (1954) edited the conference proceedings.

the work of Sidney Siegel.[12] In the early 1960s, starting from the experiment of Estes, Siegel offered a proof of the importance of monetary incentives to experimental subjects' behaviour, whose influence on the following research is discussed in Smith (1992, pp. 261-263).

The other experiment presented at Santa Monica by Kalisch, Milnor, Nash and Nering (1954) tested the validity of some solution concepts proposed by game theorists.[13] Although the experimental results were far from conclusive, both because of the difficulty in determining a "neutral" technique[14] and because of the indeterminacy of the tested concepts, the paper represented a substantial step in the foundation of experimental gaming because it defined a first tool box. The subjects were firstly instructed in the main principles of game theory and then submitted to the experiment with predetermined time. The payments to subjects consisted of tokens that were converted to dollars at the end of the experiment. The discussion of the results took environmental conditions into account. The different personalities of subjects were taken into account in explaining their performances.[15]

But the main finding of the experiment was to show how games in normal or extensive form could be simply submitted to experiments. The improvement made possible by the use of game theory as opposed to the inaccuracy of Chamberlin's experiments explains why the attempts to improve the technique proposed by Kalisch, Milnor, Nash and Nering multiplied very rapidly.

The list of the literature published in the 1950s clearly shows how it remained unrelated to economics. Deutsch (1958), Flood (1958), Loomis (1959), Scodel, Minas, Ratoosh and Lipetz (1959) tested the prisoner's dilemma; Vinacke and Arkoff (1957) verified the theory of coalition proposed in *Theory of Games*; Stone (1958) and (Schelling 1958, 1959) verified the Nash bargaining theory and the theory of focal points; Mosteller and Nogee (1951), Allais (1953), Edwards (1953), Flood (1955), Davidson, Suppes and Siegel (1957) made some experiments testing the expected utility function proposed by von Neumann and Morgenstern.

These contributions shared two features:

1. The object of verification was represented, or could be represented, by games in normal form;

2. Their theoretical background related to economics only indirectly, being set mainly within other social sciences.

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12. Other works developing Estes's insights by employing game theory were Schelling (1957) and Atkinson and Suppes (1958).

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

14. 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).

15. For example, the table position of the players assumed an unexpected importance. For a detailed discussion of the experiment, see Luce (1959).

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

On Simon's account, in the 1950s experimental games did not fill the gap between experimental methods and economics. To find an economic way to experimentation before the 1960s, it is necessary to make reference to the other outgrowth of game theory, which was given by business gaming.

The first *business game* was performed by a group of economists and managers directed by Richard Bellman and was published in 1957. At the same time, R. Andlinger and J. R. 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).[16]

Bellman's game simulated an oligopolistic market where five firms competed in an economy growing at a constant rate. Each player received some information about the activity of his firm and about the market conditions of the previous period. On these bases, he had to determine selling prices, expenditure on marketing and R&D, the quantity of product and a plan of investment for the following period. The rules of the game also allowed firms to buy further information about the market characteristics. The game was repeated for a predetermined number of periods and after each period the outcome of the decisions taken by the five players was made public, by calculating the respective market share, the average costs and the estimated productivity for the following periods.

What made these kinds of games different from experimental games was the diversity of a theoretical approach. Business games, especially in the first phase, didn't have the purpose of verifying the empirical validity of specific theories, but only that of being a tool for training and selecting the managers of big firms. Economic theories were rather an essential requirement to

16. The *business game* by Bellman was the most significant of those mentioned because it was the first to be made. The only relevant difference between Bellman's and Andlinger and Greene's experiment is that the former considers a market for consumption goods while the latter a market for capital goods.

design the experimental environment, which as an a priori was exempt from verification.[17] This fact had important consequences. By not assuming an explicit objective function or a model to be communicated to the subjects, the difficulties in playing the game were greatly reduced. Moreover, subjects' instructions could avoid any theoretical assumption and make reference only to simple rules drawn by business practice.

The simplicity of the structure allowed the experimenters to pay much more attention to the improvement of experimental techniques, which as a result improved rapidly. Such progress reached its height in 1959 with the contribution of Austin C. Hoggatt, who can be considered a forerunner of Siegel and Fouraker's work.[18] His business game did not represent anymore a device for training managers, but rather a tool to gain new knowledge about human behaviour:

> We focus on using game situations as a research tool for studying the behavior of human beings in conflict situations. (...) To observe how the subject's actual behavior compares with various types of maximizing behavior as these are visualized in economic theory (Hoggatt 1959, pp. 192-195).

The purpose of the experiment was to test the validity of Cournot equilibrium. Three firms producing goods that were differentiable and infinitely divisible played the game. Each manager-subject knew his cost function, the market demand, the initial situation and a rough estimate of the cost function of the other firms. The game was divided in three moves: the first was made by an arbiter who fixed a number t of periods, that subjects didn't know until the period t+1 in which the game ended; the second move was made by each manager, who decided at the start of the period his level of production, only knowing the total production of the

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17. In 1957, Franc Ricciardi, one of the authors of Bellman's experiment, wrote: "The problem, then was to build into the model the interrelationships of these factors. Should share of the market, for example, be based on decisions in the areas of marketing, research and development, price, and plant capacity? Assuming these are the determining factors (actually, they are not the ones that were ultimately chosen), what should be the relationships among them? This example illustrates the two-fold problem facing the research group at that point: (1) selecting the factors that should interact to create the various result in playing the game, and (2) selecting the value relationships among the same factors". To solve this problem, an easy way would have been represented by an empirical verification, but this last was judged complicated and of uncertain success: "The alternative to conducting such an empirical study was to make use of the general assumptions accepted by most economists. Obviously, such assumptions were available. It is generally accepted, for example, that quantity sold varies inversely with price, and marketing expenditures tend to increase the attractiveness of a product, at least up to a certain point. The relationships among the other factors chosen to be built into the game are also known in a general way, as are their relationships to the over-all business operation. Since the game, after all, was to be a training device, not an analytical tool, the idea of special studies was discarded in favor of using the accepted generalizations" (Ricciardi 1957, pp. 54-55).

18. Another paper that anticipated Siegel and Fouraker's work was Sauermann and Selten (1959), that shared many characteristics with Hoggat (1959) but was published in United States only in the 1960. So it is reasonable to argue that it did not play a minor role in the process described here.

previous period but not the quantity produced by the other firms; the third move was made by the arbiter who selected the clearing market price.

The results showed that firms' production fluctuated around the short run Cournot equilibrium, but didn't allow being able to determine if the equilibrium was stable in the long run. However, Hoggatt's discussion emphasised the disparity in the profits obtained by the firms, that was considered surprising for the supposed homogeneity of the subjects. Moreover, he was aware of the complexities of the interpretation of results. Among the many 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). In this way, Hoggat showed that it was possible to use a business game to test an economic theory. This evidence gradually extended to economic experiments all the work and the connected methodological improvements reached within business gaming. This influence, besides proving crucial to make headway, can also explain why the introduction of controlled experiments in the economists' toolbox was delayed until at least the 1960s.

4. The causes of the delay

To summarise, in the 1950s two different approaches to the introduction of experiments in social sciences were simultaneously followed: one was represented by experimental gaming and was adopted more by sociologists, psychologist, philosophers and decision theorists than by economists, the other by business games, that were more within the scope of management science than of economics. Moreover, both approaches were an outgrowth of game theory, especially because this method permitted one to translate into simple models the theories to be tested. On this account, it may be argued that the introduction of experimental techniques in economics was delayed by the same causes that postponed the introduction of game theory into economics.

A major factor was the division existing between economists and game theorists. It is generally acknowledged that the initial development and the diffusion of game theory was promoted by a community of mathematicians, who presented their results thoroughly without making any concession in terms of language. This emphasis placed on formal aspects created a gap between this community and economists, which prevented the rapid application of game theory in economics.

A revealing example of the difficulties faced by economists in absorbing game theory was documented by the report of the 1948 annual conference of the American Economic Association. Discussing a presentation by Oskar Morgenstern, William Jaffé criticised game theory with arguments clearly testifying to his lack of understanding. Therefore, he deferred any judgement:

> Actually no theory of duopoly or oligopoly has been worked out along the lines he suggests. We may grant that his speculation are far from idle and that they are eminently desirable, but for those of us to whom the mathematics of games is still unfamiliar, it is too early to tell (Jaffé 1948, p. 20).

In the following comment, Martin Bronfenbrenner, after having pointed out some weaknesses in Chamberlin and Robinson's oligopoly theories, turned to the authors of *Theory of Games*, who claimed to have solved them, with a disingenuous request for explanation:

> to disprove as far as they can the obvious charges of formalism and pyrotechnics which may be levelled against their work. (...) They should formulate some substantial body of their results in a form susceptible to testing against the received doctrine, and then carry out the tests which are indicated. In any case, Professor Morgenstern would do well to reformulate as large as possible a segment of his contribution in simple arithmetical or diagrammatic examples (Bronfenbrenner 1948, pp. 25-26).

The charges of formalism and pyrotechnics represented more a signal of the embarrassment created in Bronfenbrenner and in his colleagues by the language employed by von Neumann and Morgenstern than the product of a close examination of their book.[19]

More intrinsic elements also explained the obstacles that von Neumann and Morgenstern had to overcome in order to circulate the content of *Theory of Games*. First, their book not only implied the abandonment of differential analysis but also its pages lacked any reference to economic literature. The only authors to be mentioned were Walras, Böhm-Bawerk and the two Mengers. The absence of any comparison with economic schools, especially the neoclassical one, represented a cause of puzzled reactions like those quoted before.[20] Second, *Theory of Games* contained proposals that were objectively unsuitable for the application to economics. First, the best mathematically developed part of the book concerned zero-sum two-person games and minimax theorem, which fit more parlour games and military issues than economic behavior. Second, the transformation of non-zero-sum into zero-sum games through the device of the fictitious player appeared an artificious technicality having little correspondence with the prevaling way of modelling social interaction in economics. Finally, the reliance on cooperative

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19. Other participants also gave similar judgement (Mc Cracken 1948, Mc Cord Wright 1948).

20. This prejudice was also reinforced by the initial scepticism about a discipline raising the techniques of poker players to the rank of science: "In some ways the name Game Theory is unfortunate, for it suggests that the theory deals with only the socially unimportant conflict of interest found in parlor games, whereas it is for more general than that. Indeed, von Neumann and Morgenstern entitled their now classical book *Theory of Games and Economic Behavior*, presumably to forestall that interpretation, although this does not emphasize the even wider applicability of the theory" (Luce and Raiffa 1957, p. 2).

games neglected the fact that enforceable agreements with other players are difficult to find in the real world.

The whole of these elements caused a separation not only between game theorists and economists but also between economists and experimenters. Such a division implied that a new scientific community, in which researchers of various disciplines could share and discuss their work, pursued the progress of game theory beyond the foundations given by Von Neumann and Morgenstern (1944).

Although the origins of this community dated back to the 1930s, when the Econometric Society and the Cowles Commission were established, the main actor of this story was the RAND Corporation, whose foundation was financed by the US military budget. The project for equipping the US Army Air Force, and its successor, the US Air Force, with a research staff took shape at the end of 1944, when Gen. Henry Harley Arnold wrote a letter to Theodor von Kármán, a Hungarian refugee, asking him to prepare a plan for creating a military research group in Santa Monica. Thirteen months later, von Kármán and the Army Air Force Scientific Advisory Board presented the programme "Towards New Horizons", which represented the official act of birth of Research ANd Development (Kaplan 1983, pp. 50-52).

The military character of the new organisation was partially changed at the beginning of 1947. Two members of the RAND Mathematical Division, Olaf Helmer and John Davis Williams, who were already interested in game theory, promoted the foundation of two new divisions, one dedicated to economics, the other to social sciences. Later Williams contacted John von Neumann and offered him to join the project. In December of 1947, the co-author of *Theory of Games* entered RAND Corporation as part-time consultant, starting his work on military research which was to last until his death.

As pointed out earlier, the 1952 Santa Monica Conference was promoted by the RAND Corporation, which most participants belonged to. The list of contributors clearly showed how the conference was characterised by an interdisciplinary approach.[21] The RAND also published most of the papers on experimental games written in the 1950s. These contributions dealt mainly with two-person zero-sum games, which represented the kind of game more appropriate for military simulations but not for economics.

It has been argued (McKenzie and Spinardi 1981, Schweber 1988, Mirowski 1991) that this fact conditioned heavily the following developments. The reliance on military funds would have shifted research activity far from its extension to economics. It was evident that in the period following the publication of von Neumann and Morgenstern's book, the state of US foreign policy pushed a more antagonistic reading of the models proposed in their book. In

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[21]. The list of participants to the Santa Monica Conference included the psychologists R. Bush, C. Coombs, W. Estes, L. Festinger, H. Simon, the mathematicians S. Karlin, J. Nash, L. Shapley and J. von Neumann, the economists G. Debreu, C. Hildreth, T. Koopmans, J. Marschak, O. Morgenstern and R. Radner and the decision theorists F. Mosteller and H. Raiffa (Thrall, Coombs and Davis, editors, 1954, Appendix A).

particular, two-person zero-sum games were a straightforward means of representing the Cold War and the relationship between the United States and Soviet Union.[22]

Such a view is in contrast with the point - repeatedly stated in *Theory of Games* - that the "standard of modesty" required by the foundation of a new mathematical method as game theory imposed the adoption of drastic simplifications as the zero-sum games or the minimax theorem, that only successive research would remove. This methodological approach, rather than the influence of military organisations, seems to be the main cause of the importance assigned to these tools.

The role played by the RAND Corporation can be evaluated differently if another element is taken into account. The most fruitful approach to experimental economics, which began at the Santa Monica Conference, was promoted by the RAND Corporation and other organisations financed by US military funds.[23] In the 1940s and 1950s these funds represented the only source of resources available for research work. The share of the US Federal budget devoted to R&D and assigned to military forces increased from 25% in 1935 to 90% in 1943 (Forman 1987, p. 153) and it remained very high after 1945. In an economy still suffering from the consequence of the depression of the twenties and thirties, military funds represented an essential means of survival for social researchers.[24] Thus, military associations became a centre of attraction for the scholars of different social sciences, who could carry on their work in an interdisciplinary environment.[25] It wouldn't be rash to affirm that the same integration of different scientists couldn't occur without this point of reference which, by permitting a continuous interaction between game theorists and experimenters, increased at the same time the distance between economists and experimenters.

This fact had another consequence in the 1960s when experimental economics, like game theory, increased its diffusion among economists precisely by confirming existing theories rather than by disproving them. Vernon Smith's repetition of Chamberlin's experimental imperfect market or Siegel and Fouraker's experiments on models of bilateral monopoly and oligopoly

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22. Later, such a parallelism was weakened by the same authors supporting it before. See for example Schelling (1959, pp. 258-261).

23. Besides the RAND Corporation, in the post-war period, the US Army counted on five research offices, all of them connected in some way to game theory: the Applied Mathematics Panel (AMP), including the Statistical Research Group of Columbia University to which Kenneth J Arrow collaborated in the first years of his career, the Office of Naval Research (ONR) at Stanford, that financed many publications on game theory, among which the book *Games and Decisions* by Luce and Raiffa, the Anti-Submarine Warfare Operations Research Group (ASWORG), which gave origin to the Operations Research Group and the Army's Eisenhower Advanced Study Group (Mirowski 1991).

24. In his autobiography, Richard Bellman offers an explicit testimony to that period: "I was horrified to see the genteel poverty in which many faculty people lived. Here were people who had devoted over twenty years to training and they made less than a checker in a supermarket" (Bellman 1984, p. 184).

25. For an authoritative account of the atmosphere that characterised the RAND Corporation in those years, see Arrow (1987a) and (1987b).

belonged to this tendency: in both cases the main purpose was more to reaffirm the established theories rather than to define their empirical meaning.[26] Although their experiments accelerated the process of methodological improvement, the presumption they created represented a constraint for the following developments. To clarify matters, it is useful again to employ the analogy with game theory. Most economic models discussed by von Neumann and Morgenstern in *Theory of Games and Economic Behavior* and the economic applications of game theory proposed in the 1950s belonged to orthodox economics. This fact may produce contrasting consequences, as pointed out by Schotter and Schwödiauer (1980) in the passage quoted above. On the one hand a new methodology which confirms what has already been shown or proved with other methods should be accepted more quickly, on the other the absence of immediate new results would decrease the utility of the effort to be made for absorbing the new technique. The same contradiction influenced the history of experimental economics, whose recent growth seems to depend more from having refuted some basic economic principles than having supported them. Even in the early years the acceptance of experimental methods was facilitated by its conforming to the current ideas, it was just the abandonment of this tendency to give them a specific advantage. But at the same time such an orthodox orientation was another cause of the delayed introduction of experimental methods in economics.

5. Conclusions

The historical overview of the early years of experimental economics and game theory shows that their definitive introduction in economics was postponed until the 1960s. This delay cannot be ascribed to a surreptitious division between empirical and theoretical work but to the fact that in the 1950s experimental methodologies, like game theory, were mainly developed by scholars of other sciences (psychologists, sociologists, mathematicians, philosophers, decision theorists and business-school economists) belonging to a deeply interwoven community, financed mainly by military funds. This interdisciplinary group gave rise to two distinct approaches: the socio-psychological one represented by experimental games, and the economic-managerial one represented by business games. Both approaches were an outgrowth of game theory, especially because this tool permitted the translation into simple and precise models of the theories to be tested.

The consequences of this evolution can be better evaluated by considering that in the 1950s the socio-psychological approach was the dominant one in the academic world. The other approach, although crucial to methodological developments, didn't bridge the gap between economists and experimenters for two reasons: the separation existing between theoretical

26. There were important exceptions (for example, Allais 1953 and Ellsberg 1956) but they did not have an immediate impact.

economics and management science and the fact that business games were not initially thought as being tools to verify theories but to prefigure the subjects' behaviour in real world.

This unbalance had important consequences. Firstly, the introduction of experimental techniques in economics was delayed by the same causes that postponed the introduction of game theory into economics. In particular, the emphasis placed on the mathematical principles of game theory created a gap not only between economists and game theorists but even between economists and experimenters. Secondly, it had important consequences for methodological developments. Four points, briefly, can be mentioned here: the detail of instructions, the importance of monetary incentives, the deadline effect and the role of information. Concerning the first point, more recent literature shows at first glance how the clarity and the meticulousness of the subjects' instructions are generally considered an essential requisite for experimental work. This was an inheritance more from business games and Siegel and Fouraker's work than from experimental gaming, whose distinctive approach placed less emphasis on the repeatability of experiments. Even the necessity of monetary rewards to motivate subjects was clearly established only in the 1960s, while in the fifties many experimental games practically neglected it.[27] Concerning the deadline effect, in the 1950s to define the length of an experiment was simply not on the agenda of experimenters and this was probably a consequence of the almost exclusive insistence on experimental games in normal form. Finally, the acknowledgement of the crucial role of information conditions was also a heritage of Siegel and Fouraker (1960). They pioneered the concept of common knowledge introduced by the philosopher Lewis (1969) by considering the difference between a situation in which an experimental subject is or is not informed that all other subjects have complete information. On the contrary, the work of the early game theorists shared the approach of von Neumann and Morgenstern (1944), who disregarded the treatment of information conditions to concentrate on the mathematical foundations of games in normal form.

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27. See Roth (1995) for an account of the reactions in the 1950s to the Wallis-Friedman critique of hypothetical choices.

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