This paper, which deals with experimental economics published in the years 1930-1960, is adapted from

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A more comprehensive history, which also deals with the field after 1960, can be found in

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ON THE EARLY HISTORY OF EXPERIMENTAL ECONOMICS
BY

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I. INTRODUCTION

In the course of coediting the *Handbook of Experimental Economics* it became clear to me that contemporary experimental economists tend to carry around with them different and very partial accounts of the history of this still emerging field. This project began as an attempt to merge these "folk histories" of the origins of what I am confident will eventually be seen as an important chapter in the history and sociology of economics.

I won't try to pin down the *first* economic experiment, although I am partial to Bernoulli (1738) on the St. Petersburg paradox. The Bernoullis (Daniel and Nicholas) were not content to rely solely on their own intuitions, and resorted to the practice of asking other famous scholars for their opinions on that difficult choice problem. Allowing for their rather informal report, this is not so different from the practice of using hypothetical choice problems to generate hypotheses about individual choice behavior, which has been used to good effect in much more modern research on individual choice.

But I think that searching for scientific "firsts" is often less illuminating than it is sometimes thought to be. In connection with the history of an entirely different subject, I once had occasion to draw the following analogy (Roth and Sotomayor 1990, p. 170):

Columbus is viewed as the discoverer of America, even though every school child knows that the Americas were inhabited when he arrived, and that he was not even the first to have made a round trip, having been preceded by Vikings and perhaps by others. What is important about Columbus' discovery of America is not that it was the first, but that it was the *last*. After Columbus, America was never lost again...

That being the case, I will try to identify the historical context out of which contemporary experimental economics has grown, by identifying early experiments which have initiated streams of experimental investigation that continue to the present. For this purpose, I begin in the 1930's. Starting from a low level of activity, the literature of experimental economics has experienced exponential growth in every decade since, which has yet to level off. I will follow the story in detail only up until 1960; the more modern consequences of these early experiments can be followed in detail in the *Handbook.* I will concentrate on three strands of the early experimental literature, each of which have left both substantive and methodological trails in the modern literature.

The first strand concerns experiments designed to test theories of individual choice. I will focus on an experiment reported by Thurstone (1931), concerned with ordinal utility theory, on an influential critique of this experiment by Wallis and Friedman (1942), and on subsequent experiments taking account of this critique by Rousseas and Hart (1951) and Mosteller and Nogee (1951), as well as on the celebrated work of Allais (1953).

The second strand I will concentrate on concerns tests of game theoretic hypotheses. I will start with the experiment performed by Dresher and Flood in 1950, which formulated the now famous Prisoner's Dilemma game (Flood, 1952, 1958), and continue with the work of Kalisch, Milnor, Nash, and Nering (1954), and Schelling (1957).

The third strand I will concentrate on concerns early investigations in Industrial Organization. I will concentrate on the work of Chamberlin (1948) and Siegel and Fouraker (1960). 1.)

One of the methodological themes that can be traced in all three of these strands is how economists have come to rely today primarily on experiments in which subjects' behavior determines how much money they earn. This is one of the practices that has grown to distinguish experimental economics from experimental psychology, for example.

Finally, each of these strands of experimental economics was profoundly influenced by the publication in 1944 of von Neumann and Morgenstern's *Theory of Games and Economic Behavior*, and I shall try to follow this connection also.

II. EARLY EXPERIMENTS CONCERNING INDIVIDUAL CHOICE

An experiment on individual choice whose direct descendents in the economics literature are easy to follow, was reported by L.L. Thurstone (1931), who considered the problem of experimentally determining an individual's indifference curves. 2.) Thurstone was concerned with testing the indifference curve representation of preferences, and with the practicality of obtaining consistent choice data of the sort needed to estimate indifference curves. He reported an experiment in which each subject was asked to make a large number of hypothetical choices between commodity bundles consisting of hats and coats, hats and shoes, or shoes and coats. (E.g. the questions about hats and shoes would involve expressing a preference between a bundle consisting of 8 hats and 8 shoes or one consisting of 6 hats and 9 shoes, and so on for many such pairs of bundles.) He reported the detailed data for one subject, and found that after estimating from the data the relative tradeoffs the subject was prepared to make between hats and shoes and between hats and coats (under the assumption that the indifference curves were hyperbolic), it was possible to estimate a curve which fit fairly closely the data collected for choices involving shoes and coats. Thurstone concluded that choice data could be adequately represented by indifference curves, and that it was practical to estimate them this way.

A lengthy and critical review of Thurstone's experiment was given by W. Allen Wallis and Milton Friedman (1942, particularly pp. 177-183). One of their lines of criticism was that the experiment involved ill specified and hypothetical choices. They summarized their position as follows (Wallis and Friedman, pp. 179- 180): "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."

Furthermore, "For a satisfactory experiment it is essential that the subject give actual reactions to actual stimuli... Questionnaires or other devices based on conjectural responses to hypothetical stimuli do not satisfy this requirement. The responses are valueless because the subject cannot know how he would react." 3.)

Rousseas and Hart (1951) reported a subsequent experiment on indifference curves designed in reply to Wallis and Friedman and as a followup to Thurstone. They constructed what they viewed as a more concrete and realistic choice situation by having subjects choose from different possible breakfast menus, with each potential breakfast consisting of a specified number of eggs and strips of bacon. For added concreteness they specified that "each individual was obliged to eat all of what he chose�i.e. he could not save any part of the offerings for a future time."(p. 291) 4.)

In this experiment individual subjects made only a single choice (repeated subsequently a month later) and also were asked to state their ideal combination of bacon and eggs. While this had the advantage of avoiding the artificiality of having subjects make many choices of the same type, it left Rousseas and Hart with the problem of trying to combine individual choice data collected from multiple individuals. They adopted the approach of seeing whether choices made by individuals with similar ideal combinations could be pieced together to form consistent indifference curves. Although they pronounced themselves satisfied with the results, we will see that the practice of testing theories of *individual* choice by combining data from *groups* of subjects was regarded as questionable by subsequent experimenters. 5.)

To put subsequent experiments in perspective, however, it is important to note that 1944 was the year in which von Neumann and Morgenstern's *Theory of Games and Economic Behavior* appeared. This presented and brought to wide attention both a more powerful theory of individual choice and a new theory of interactive behavior, and both had a profound influence not only on economic theory but also on experimental economics. The predictions of expected utility theory give a new focus to experiments concerned with individual choice, and the predictions of game theory�and its concerns with precisely specified "rules of the game"�sparked a new wave of experimental tests of interactive behavior. 6.)

Starting with the individual choice experiments, various aspects of expected utility theory were soon subjected to experimental investigation�see e.g. Preston and Baratta (1948), Mosteller and Nogee (1951), Allais (1953), Edwards (1953a,b), May (1954), Davidson, Suppes, and Siegel (1957), and Davidson and Marschak (1959) to name only a few. 7.) Of these, the most closely connected to that of Thurstone (1931) is the experiment of Mosteller and Nogee (1951), who essentially sought to test expected utility theory in much the same spirit that Thurstone had examined ordinal utility theory. (Mosteller and Nogee were also well aware of the Friedman- Wallis critique of Thurstone's experiment. 8.))

Mosteller and Nogee (1951) began their paper thus (p.371):

The purpose of this paper is to report a laboratory experiment that measured in a restricted manner the value to individuals of additional money income. 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. (emphasis in original)
Their point was that von Neumann-Morgenstern expected utility functions are derived from assumptions about *individual* choice behavior, and that laboratory experimentation provides an opportunity to look at this behavior unconfounded by other considerations. Their general plan of attack had four main steps (pp. 372-73):
(a) to have subjects participate in a game with opportunities to take or refuse certain gambles or risks entailing use of *real money*; (b) from behavior in the game to construct a utility curve for each subject; (c) to make predictions from the utility curves about future individual behavior toward other and more complicated risks; and (d) to test the predictions by examining subsequent behavior toward more complex risks. (emphasis in original)
The method underlying their construction of the utility curves involved observing whether subjects would accept lotteries with given stakes as the probabilities varied. (They also devoted some attention to arguing that the size of the payoffs could be regarded as significant in terms of alternative employment opportunities available to their subjects.)
Their general conclusions (p. 399) were that it was possible to construct subjects' utility functions experimentally, and that the predictions derived from these utility functions "are not so good as might be hoped, but their general direction is correct." And with differences of emphasis, I think that this is a conclusion with which many experimental economists would still agree.

However, much more is now known about various systematic violations of expected utility theory that can be observed in the laboratory. Perhaps the most famous is the "Allais paradox." (Allais suggested, incidentally, that experiments could be used not only to test the predictions of particular theories of rational choice, but also to *define* rational behavior. 9.)) Allais asked subjects to make two hypothetical choices. The first choice was between alternatives A and B defined (Allais 1953, p. 527) as

A:

Certainty of receiving 100 million (francs)
and

B:

Probability .1 of receiving 500 million
Probability .89 of receiving 100 million
Probability .01 of receiving zero

and the second choice was between alternatives C and D defined as

C:

Probability .11 of earning 100 million
Probability .89 of earning zero
and

D:

Probability .1 of earning 500 million
Probability .9 of earning zero.

It is not difficult to show that an expected utility maximizer who prefers A to B must also prefer C to D. However Allais reported that a common pattern of preference was that A was preferred to B but D was preferred to C. Note that although Allais' choices were hypothetical, the phenomenon he reported has subsequently been reproduced with real choices (involving much smaller amounts of money). 10.)

Note that not all of the individual choice experiments motivated by von Neumann-Morgenstern expected utility theory depended upon the novel parts of that theory. For example, May (1954) reported that it was possible to elicit intransitive preferences in choices involving no uncertainty. His results thus show a violation of even ordinal utility theory, and his experiment could in principle have been conducted as a test of the earlier theory. However (as has often seemed to be the case since), the further development of the theory may have clarified the role that experiments could play.

As mentioned earlier, following von Neumann and Morgenstern (1944), there also began to be considerable attention paid to experiments involving interactive behavior. We turn next to some of these.

III EARLY EXPERIMENTS CONCERNING INTERACTIVE BEHAVIOR

In January of 1950, Melvin Dresher and Merrill Flood conducted at the Rand Corporation an experiment which has had an enormous if indirect influence, since it introduced the game that has subsequently come to be known as the Prisoner's Dilemma. 11.) The game they studied was the hundred-fold repetition of the matrix game given below, between a fixed pair of subjects who communicated only their choices of row (1 or 2) or column (1 or 2).

(-1, 2)	**(1/2, 1)**

(0, 1/2)	**(1, -1)**

Payoffs were in pennies, with each player receiving the sum, over the one hundred plays of the game, of his payoffs in each play. The unique Nash equilibrium prediction is that the players should choose (2,1)--the second row and the first column--at each of the hundred repetitions. Thus the predicted earnings of the players are 0 for the row player (henceforth "Row") and $0.50 for the column player (henceforth "Column"). 12. Of course this is inefficient, since if the players instead played (1,2) at every period, for example, their earnings would be $0.50 for Row and $1.00 for Column�i.e. they would both earn more. But this is not equilibrium behavior. That equilibrium play is substantially less profitable than cooperative play made Dresher and Flood anticipate�correctly�that this game would present a demanding test of the equilibrium predictions.

The observed payoffs, for a pair of players whose play was reported in detail in Flood (1952, 1958) were $0.40 for Row and $0.65 for Column. This outcome is far from the equilibrium outcome, although it also falls considerably short of perfect cooperation. (This observation has since been replicated many times.) Dresher and Flood interpreted this as evidence against the general hypothesis that players tend to choose Nash equilibrium strategies, and in favor of the hypothesis that a cooperative "split the difference" principle would be more powerful in organizing the data from games of this kind.

Dresher and Flood included in their report of the experiment the following passage, describing an alternative interpretation given by John Nash (Flood 1958, p. 16):

Dr. Nash makes the following comment (private communication) on this experiment:

'The flaw in this experiment as a test of equilibrium point theory is that the experiment really amounts to having the players play one large multimove game. One cannot just as well think of the thing as a sequence of independent games as one can in zero-sum cases. There is much too much interaction, which is obvious in the results of the experiment.

'Viewing it as a multimove game a strategy is a complete program of action, including reactions to what the other player had done. In this view it is still true the only real absolute equilibrium point is for [Row] always to play 2, [Column] always 1.

'However, the strategies: [Row] plays 1 'til [Column] plays 1, then 2 ever after,
[Column] plays 2 'til [Row] plays 2, then 1 ever after,
are very nearly at equilibrium and in a game with an indeterminate stop point or an infinite game with interest on utility it *is* an equilibrium point.

'Since 100 trials are so long that the Hangman's Paradox cannot possibly be well reasoned through on it, it's fairly clear that one should expect an approximation to this behavior which is most appropriate for indeterminate end games with a little flurry of aggressiveness at the end and perhaps a few sallies, to test the opponent's mettle during the game.

'It is really striking, however, how inefficient [Row] and [Column] were in obtaining the rewards. One would have thought them more rational.

'If this experiment were conducted with various different players rotating the competition and with *no information given to a player of what choices the others have been making until the end* of all the trials, then the experimental results would have been quite different, for this modification of procedure would remove the interaction between the trials.'

Dr. Dresher and I were glad to receive these comments, and to include them here, even though we would not change our interpretation of the experiment along the lines indicated by Dr. Nash. (emphasis in original)

Despite the limitations of this very exploratory, preliminary experiment, 13 it foreshadows some of the best of experimental economics. It tests the clear predictions of a general theory, on a difficult test case. And the results allow alternative hypotheses to be developed. When they are as clearly stated as Nash's comments, they suggest further experiments. 14 And as the quoted passage makes clear, some of the most interesting outcomes of an experiment may be the manner in which its results pit alternative interpretations against each other.

In choosing a difficult test case, Dresher and Flood formulated a game which has since engaged both theorists and experimenters in a number of disciplines, as a large literature has developed around the prisoner's dilemma, which has been used as a metaphor for problems from arms races to the provision of public goods. 15 This too is one of the indirect virtues of experimentation. The design of an experiment to test a particular theory often forces the experimenter to focus on specific aspects of the theory other than those which naturally come to the fore in the theoretical literature. The insights gained from designing an experiment are, as in this case, often of value even apart from the actual conduct of the experiment. Thus there is an interplay, on many levels, between theory and experiment.

In 1952 the Ford Foundation and the University of Michigan sponsored a conference on "The Design of Experiments in Decision Processes," which was held in Santa Monica (in order to accommodate the game theorists and experimenters associated with the Rand Corporation). Some of the experimental papers from this conference appear in Thrall, Coombs, and Davis (1954). 16 The paper by Kalisch, Milnor, Nash, and Nering, which reported a small-scale experiment involving several different n-person games, anticipates some issues of experimental design that have played important roles in the subsequent literature. Some of the games they looked at were constructed to allow particular theoretical predictions to be tested on a domain on which the theories in question would make unambiguous predictions. They write

...the negotiation procedures were formalized (e.g., the identities of a player's opponents were concealed from him and he was allowed to bid, accept, decline, or counter-bid in a very limited number of ways...) The construction of a theory to deal with an unlimited or very large number of negotiation possibilities is as yet so difficult that it seems desirable to

restrict and severely formalize the negotiation procedure to the point where a meaningful theory can be constructed. (p. 302)
The choices the players made were not hypothetical, rather the profits they would take home from the experimental session were proportional to their payoffs in the experimental games. And (after finding mixed support for various game-theoretic hypotheses) the authors concluded with a discussion of design features that might make it easier to interpret future experiments, saying (p. 326): "The same set of players should not be together repeatedly since there is too much of a tendency to regard a run of plays as a single play of a more complicated game" and "It would be better to play an unsymmetrical game so that there would be no obviously fair method of arbitrating the game and avoiding competition."

These two bits of advice are very different from one another, but are each representative of what have proved to be important aspects of the design of economic experiments.

The first bit of advice is solidly grounded in theory. If the same players play a game more than once, their behavior even the first time they play may be different than if they were going to play only once, since in the repeated case they can anticipate that actions in the first period may affect the outcome in future periods. 17.

The second bit of advice was grounded not in theory, but in a clearly observed experimental regularity: in symmetric situations players often agreed on equal divisions. By suggesting that this is because equal division in symmetric games is a "fair" method of division, and that experimenters should seek to avoid such situations, the authors seem to have been suggesting that subjects are sometimes motivated by considerations that the experimenter can only imperfectly control. In this view, the demands of fairness in situations that seem to subjects to call for fair behavior may sometimes overwhelm the motivations that the experimenters are trying to induce (via the monetary rewards), so that the game being played is different from the one intended by the experimenter.

Another hypothesis about why equal divisions are so often observed in symmetric situations was offered by Thomas Schelling. He proposed that in many situations the problem facing economic agents is predominantly one of coordination, and that by focusing on outcomes that might be "prominent," some of the costs of coordination failure could be avoided. Schelling (1957) reported an experiment in which he confronted "an unscientific sample of respondents" with a variety of (hypothetical) problems. The following are two examples: 18.

You and your partner (rival) are to be given $100 if you can agree on how do divide it without communicating. Each of you is to write the amount of his claim on a sheet of paper; and if the two claims add to no more than $100, each gets exactly what he claimed. If the two claims exceed $100, neither of you gets anything.
and
You and your two partners (or rivals) each have one of the letters A, B, and C. Each of you is to write these three letters, A, B, C, in any order. If the order is the same on all three of your lists, you get prizes totaling $6, of which $3 goes to the one whose letter is first on all three lists, $2 to the one whose letter is second, and $1 to the person whose letter is third. If the letters are not in identical order on all three lists, none of you gets anything."

Schelling reports that in the first of these problems, 36 out of 40 subjects chose $50. Of course, since this yields an equal division, it could have been caused by a desire to be fair, instead of because it is a "prominent" outcome. 19. But it is harder to explain the results of the next problem as a result of anything but the prominence of alphabetical order: 9 out of 12 A's, 10 out of 12 B's, and 14 out of 16 C's chose the order ABC. This illustrates the power of experiments to test a hypothesis in different ways, the better to distinguish it from alternative hypotheses that might yield similar predictions on some domains. 20.

Schelling's point was that a wide variety of cues could serve to make an outcome prominent, and facilitate coordination. His comments were directed primarily at game theorists, the point being that highly abstract models might exclude factors that play an essential role in facilitating coordination. But there is a lesson for experimenters too, which is that details of how experiments are conducted may be of considerable importance, even if they concern features of the environment not addressed by existing theories. Sometimes these details will be worth study in their own right, and sometimes the experimenter will wish to avoid constructing the environment in a way that introduces unwanted influences (e.g. think how the results for the second problem would differ if the players were identified by colors instead of letters). The considerable influence of Schelling's experiments was for many years felt mostly indirectly, through the ways in which various kinds of phenomena were interpreted by subsequent authors. 21 Recently, however, there has been a renewed interest in coordination experiments, motivated in part by macroeconomic questions (see Ochs 1995).

A. Industrial organization

Turning now to the organization of markets, one early experiment which has exerted a major, if delayed influence on modern experimentation, was reported in 1948 by Edward Hastings Chamberlin. Chamberlin prefaced his article with an explanation of what he thought laboratory experiments might bring to economics, beginning with a description of what he took to be the conventional wisdom on the subject. He wrote as follows:
It is a commonplace that, in its choice of method, economics is limited by the fact that resort cannot be had to the laboratory techniques of the natural sciences. On the one hand, the data of real life are necessarily the product of many influences other than those which it is desired to isolate�a difficulty which the most refined statistical methods can overcome only in small part. On the other hand, the unwanted variables cannot be held constant or eliminated in an economic "laboratory" because the real world of human beings, firms, markets, and governments cannot be reproduced artificially and controlled. The social scientist who would like to study in isolation and under known conditions the effects 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. (emphasis in original)

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.)

Chamberlin went on to describe the hypothesis motivating his experiment, which was that, contrary to the prevailing orthodoxy, market outcomes would often differ from competitive equilibrium "under conditions (as in real life) in which the actual prices ... are not subject to 'recontract' (thus perfecting the market), but remain final."

Chamberlin created an experimental market by informing each buyer and seller of their reservation prices for a single unit of an indivisible commodity (i.e. for each buyer the price below which he could profitably buy, and for each seller the price above which he could profitably sell), and he reported the transactions that resulted when buyers and sellers were then free to negotiate with one another. He noted that the reservation prices of the buyers, in aggregate, determined the market's demand curve, while the reservation prices of the sellers determined the supply curve, so that the competitive equilibrium (price and volume) could be established unambiguously (and was under the control of the experimenter).

The experiment he reported involved forty-six markets created in this way, with slightly varying equilibrium prices. He observed that the number of units transacted was greater than the competitive volume in forty-two of these markets, and equal to the competitive volume in the remaining four markets, while the average price was below the competitive price in thirty-nine of these markets and higher in the rest. Chamberlin interpreted the systematic differences he observed between actual transaction prices and volumes and those predicted by the competitive equilibrium as supporting his hypothesis. At the same time, he noted that the results he observed caused him to correct an erroneous assertion he had made in Chamberlin (1933, p. 27) that none of the "normally included buyers and sellers" (i.e. those who would transact at equilibrium) could fail to transact even when the market did not achieve equilibrium. In fact, what he observed was that sometimes a buyer, for example, might find that all of those sellers from whom he could afford to buy had already sold their unit to some other buyer, at a price below the equilibrium price. Chamberlin closed by cautioning that his results should be regarded as preliminary, and noted that some of the regularities he observed might be sensitive to the shape of the supply and demand curves.

In the years since Chamberlin's experiment, his technique for constructing experimental markets with known supply and demand curves has been widely employed (see e.g. the chapters by Holt, Kagel, and Sunder in the *Handbook*). More generally, Chamberlin's experiment illustrates the empirical power that comes from being able to create an environment in which the predictions of a theory (in this case competitive equilibrium) can be precisely known.

Like May's experiment on intransitivities in individual choice, Chamberlin's is an experiment that could have been done before von Neumann and Morgenstern, since it tested hypotheses that predated their work. Also, it should be noted that Chamberlin's experiment employed only hypothetical payoffs.

The end of the decade of the 1950's, and the beginning of the next, was marked by experiments concerning duopoly and oligopoly behavior, in the work of Hogatt (1959), Sauermann and Selten (1959, 1960) and Siegel and Fouraker (1960) (which won the 1959 Monograph Prize of the American Academy of Arts and Sciences). The work of Siegel and

Fouraker was perhaps the most extended experimental economics study reported up until that time. 22

Siegel and Fouraker (1960) reported a series of experiments in which subjects bargained in pairs until they reached agreement over a price and quantity, which served to determine their profits (each subject was given a payoff table which informed him of his own profits for each possible price and quantity). They designed a series of careful experiments to distinguish among a large variety of hypotheses concerning bilateral monopoly, hypotheses drawn from diverse sources in classical economic theory, game theory, psychology, and from the earlier experimental work of Schelling (1957). (They concluded [p. 69] that "Consideration of traditional economic forces cannot be depended on to yield an adequate explanation of the prices arrived at in bilateral monopoly bargaining.") One of the notable features of their experiments was the attention they paid to the information available to the subjects about each other's payoffs. They compared the case in which each subject knew only his own payoff table with the case in which one subject knew both payoff tables and the case in which both subjects knew both payoff tables. They found that, as the information increased in this way, the frequency with which subjects chose the Pareto optimal quantity increased, as did the frequency with which they chose a price that gave them equal payoffs.

Two methodological aspects of Siegel and Fouraker's work are especially notable. First, they took pains to insure that their subjects interacted anonymously, in order to avoid introducing into their experiment uncontrolled "social" phenomena. Second, not only did they follow the increasingly common practice of motivating subjects with monetary payoffs, but they investigated the effect of changing the incentives by changing the size of the payoff differences that resulted from different decisions. That is, they were not content to observe that subjects could make substantial profits from choosing, e.g. the Pareto optimal quantity. They also considered how much of a difference it made if the quantity chosen was only a little more or a little less than the Pareto optimum. They noted that in the first of the payoff tables they used this difference was small, and conjectured that the variance they observed around the Pareto optimal quantity might be due to the fact that the subjects felt that the potential payoff difference was not worth the hazards of continued bargaining. They say (p. 34): "If this reasoning is correct, then increasing the difference in payoff to each bargainer between contracts at the Paretian optima and contracts at quantities adjacent to the optima should lead to the negotiation of a higher percentage of contracts on the optima."

They then went on to present results obtained from payoff tables which increased the size of these differences, and reported that they did indeed observe much less variance around the Pareto optimal quantity.

Siegel and Fouraker used their results to motivate a theory based on the "level of aspiration" of the subjects, which they proposed was the variable effected by the differing amounts of information. They went on to explore this hypothesis in oligopoly models as well (Fouraker and Siegel 1963). 23.

Independently, Sauermann and Selten (1959, 1960) formed related hypotheses on the basis of rather different oligopoly experiments. 24

I think Siegel and Fouraker's views on the place of experimentation in economics have stood the test of time. They said (1960, pp. 72-73):

Our data have been observations made specifically to meet the purposes of this research. We have not turned to preexisting data. In the specific case of bilateral monopoly, it would be extremely unlikely that appropriate naturalistic data could be collected to test the theoretical models.... Although exchanges under bilateral monopoly conditions are common, such... descriptions as may be available will not generally be in an appropriate form for testing theoretical models...

We have made our observations under controlled conditions. We have not only collected observations especially for this research, but we have also done so under conditions which make the observations relevant to the research purposes. In using the laboratory rather than the field, we have been able to isolate the phenomena of interest to the research....

We have used the experimental method. That is, we have manipulated certain variables and observed the effects of variations in these upon certain other variables. By so doing, we have demonstrated that the amount of information available to a bargainer and his level of aspiration are significant determinants of the price-quantity contracts which will be reached. We aver that only the experimental method could have demonstrated the influence and importance of these determinants.

B. Concluding remarks

It is striking to note the distinguished game theorists among the earliest experimenters (Nash, Schelling, Selten, and Shubik, for example, set a high standard of distinction by any measure). I have already indicated that I think this is no accident. Rather, game theory brought to economics a kind of theory that lent itself to experimental investigation, and in some cases demanded it. The reason is that it seeks to provide precise models of both individual behavior (in vN-M utility functions) and of economic environments. This concern with the "rules of the game," the institutions and mechanisms by which transactions were made, together with precise assumptions about the behavior of individuals and the information available to them, gave rise to theories that could be tested in the laboratory.

By the end of the 1950's two of the features that have come to distinguish experimental economics were already clearly in evidence. First is the concern for testing theories of great potential generality (such as theories of equilibrium) on specific, controlled environments, and the consequent attention to rules of play. Second, many of the experiments attempted to gain control of subjects' motivations by paying the subjects based on their performance, so that subjects' performance could be analyzed under the assumption that they were seeking to maximize the utility (or sometimes simply the expected value) of the money earned. That is, by this time the reaction of experimental economists to the Wallis- Friedman critique of hypothetical choices was already beginning to take shape in a tendency to rely primarily on experiments in which subjects' behavior determined their *monetary* payoffs. 25

And although the end of the 1950's is still quite early in the development of experimental economics, a number of experiments completed well before then have continued to exert a powerful influence on modern research. Individual choice experiments in the spirit of Allais (1953) have inspired the search for other systematic violations of expected utility theory (see Camerer, 1995, for a survey of the modern literature). Prisoner's dilemma experiments in the spirit of Dresher and Flood (Flood, 1952, 1958) not only became a small cottage industry by themselves, they also influenced game theory in ways that make their full effect hard to grasp. They are, for example, very close kin to the public goods experiments described by Ledyard (1995). And the basic design of Chamberlin (1948) for inducing individual reservation prices and aggregate supply and demand curves has become one of the most widely used techniques in experimental economics, and plays a role in much of contemporary experimental economics, as do the methodological considerations raised by Siegel and Fouraker (1960). Finally, the theoretical work of the early game theorists, and especially of von Neumann and Morgenstern, and of Nash, have had such profound effects on both modern economic theory and on experimental economics that it is fair to say that their influence is pervasive in every aspect of modern economic experimentation.

The end of the 1950's thus left the experimental enterprise in economics on solid foundations. The 1960's were a decade of steady growth, and the first *reviews* of economics experiments began to appear (see Rapoport and Orwant, 1962; Cyert and Lave, 1965; and Friedman, 1969 26). Rapoport and Chammah (1965) compiled a considerable body of work associated with the prisoner's dilemma, and a set of German experiments is reported in Sauermann (1967) (who may have coined the term "experimental economics"). 27 Well over a hundred experimental economics papers were published in the 1960's. 28 By the end of the decade a good deal of thought had begun to be given to questions of experimental methodology as such: see for example the description of a computerized laboratory by Hogatt, Esherich, and Wheeler (1969). The *Handbook* attempts to bring the story up to date.

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ENDNOTES

[Click on note numbers to return to the text at the note.]

1.Of course there is something artificial about dividing up the work in this way, and there are other ways in which it could be connected. For example, Siegel and Fouraker tested a hypothesis put forth by Schelling, and their work influenced not only subsequent experiments in industrial organization but also in bargaining.

2.Thurstone (1931, p. 139) remarks

"The formulation of this problem is due to numerous conversations about psychophysics with my friend Professor Henry Schultz of the University of Chicago. It was at his suggestion that experimental methods were applied to this problem in economic theory. According to Professor Schultz, it has probably never before been subjected to experimental study."

3.While this line of criticism is by no means uncontroversial, the question of actual versus hypothetical choices has become one of the fault lines that have come to distinguish experiments published in economics journals from those in psychology journals. Of course laboratory animals in psychology experiments face very well motivated choices, and Wallis and Friedman expressed some optimism about economic experiments using animal subjects as well, and cite Wolfe (1936) and Cowles (1937) as interesting examples. And a modern body of economic experiments has developed using laboratory animals, see e.g. Kagel (1987).

4.However this stipulation may have been addressed more at the first of Wallis and Friedman's criticisms than at the second�although Rousseas and Hart's description of their experiment is somewhat ambiguous on this score (a situation that would not be seen in a contemporary report of an economic experiment) it appears to me that the choices were still hypothetical, and that no breakfasts were in fact cooked and consumed in response to the choices made (although for additional concreteness it was nevertheless specified that all eggs would be scrambled...). However MacCrimmon and Toda (1969, p. 435) read Rousseas and Hart's account differently and conclude that their subjects were indeed required to eat their most preferred choice. MacCrimmon and Toda conducted an experiment themselves in which subjects did eat their choices�see the next footnote also.

5.See for example MacCrimmon and Toda (1969) who follow up on Thurstone and Rousseas and Hart with a similarly designed experiment which addresses the previous criticisms by using well motivated individual choice data. In one part of MacCrimmon and Toda's experiment, the choices were among bundles involving combinations of cash and French pastries, "with the stipulation that the pastries had to be consumed in the laboratory, before the subject received any other payoff." (p. 441). MacCrimmon and Toda argue that this procedure squarely addresses the Wallis-Friedman critique of Thurstone's experiment.

6.Expected utility theory had its predecessors in the work of Bernoulli and Ramsey, and there were predecessors of parts of game theory as well (see Weintraub, 1992), but recall my earlier comments about scientific "firsts."

7.I make no attempt to include a full listing, particularly since many early utility theorists employed an informal, but nevertheless revealing style of casual experimentation, casually reported. For example Markowitz (1952) gives a qualitative account of the responses "of my middle- income acquaintances" to hypothetical questions about lotteries involving gains and losses. More formal reports of experiments involving hypothetical choices still play an important role in this literature.

8.They report (p. 372) that "Plans for this experiment grew directly out of discussions with [Milton] Friedman and [L.J.] Savage at the time they were writing their [1948] paper. W. Allen Wallis also contributed to the discussions."

9."[R]ationality can [also] be defined experimentally by observing the actions of people who can be regarded as acting in a rational manner," Allais (1953, p. 504). A similar view is expressed by Schelling (1958, p. 257).

10.Camerer (1995) discusses the modern descendants of this work.

11.The famous story of the two prisoners each of whose dominant strategy is to confess, even though both do better if neither confesses, is due to Tucker (1950). Straffin (1980) recounts how Tucker came across the game on Dresher's blackboard and composed the story that has given the game its name. Apparently Howard Raiffa independently conducted experiments with a prisoner's dilemma game in 1950, but did not publish them (see Raiffa 1992).

12.If the game were played only once, it would in fact be a dominant strategy for Row to play row 2, and for Column to play column 1. In the repeated game these are no longer dominant strategies. That (nevertheless) no other actions occur at any period of the equilibrium of the repeated game follows by backward induction from the now familiar observation that on the *last* play of the game no player can do better than to play his one-period equilibrium strategy, and so for the purpose of calculating the equilibrium we can now treat the game as a 99-period repeated game, and repeat the argument.

13.Limitations of which the very first paragraph of Flood (1958) makes clear the investigators were aware.

14.Flood's 1952 report led quickly to followup experiments. Two were reported by Scodel, Minas, Ratoosh, and Lipetz (1959) and Minas, Scodel, Marlowe, and Rawson (1960). Like Dresher and Flood's experiment, these used monetary payoffs to avoid hypothetical choices. However some of the phenomena the authors observed made them question whether very small payoffs were significantly different from hypothetical payoffs.

15.See Poundstone (1992) for a popular biography of von Neumann and his times, which focuses on the prisoner's dilemma. Poundstone devotes a good deal of attention to the early prisoner's dilemma experiments. And see Ledyard (1995) for a discussion of the large literature on public goods experiments, which are an important branch of contemporary experimentation that follow these early experiments.

16.Specifically Coombs and Beardslee (1954), Estes (1954), Flood (1954a,b), Hoffman, Festinger, and Lawrence (1954), and Kalisch, Milnor, Nash, and Nering (1954). And see Simon (1956) for some reinterpretation of the results of Estes. Oskar Morgenstern gave a talk at that conference, later published as Morgenstern (1954), in which he applauded the appearance of "strictly planned experiments," and anticipated a large future role for economic experiments of various kinds.

17.Recall Nash's comments about the prisoner's dilemma experiment.

18.Schelling's paper, and these examples, were also reprinted in his influential 1960 book *The Strategy of Conflict*.

19.See Stone (1958) for a related bargaining experiment.

20.It might still be that the equal division in the first problem is prominent *because* it is fair, so Schelling's prominence hypothesis does not necessarily contradict the fairness hypothesis implicit in the advice of Nash and his colleagues. Hypotheses about fairness play a lively role in the contemporary exchange among experimenters, see Roth (1995) for a review of how experiments have served to advance and to focus the debate.

21.This is not to say that there were not contemporary followups to his experiments: see e.g. Willis and Joseph (1959).

22.Especially when viewed in the light of their immediate followups to that work, in Fouraker, Shubik, and Siegel (1961), Fouraker, Siegel, and Harnett (1962), and Fouraker and Siegel (1963).

23.Thus, in analyzing their experimental results, Siegel and Fouraker sought to develop game theory in new directions. And although these directions involved 'non-standard' game theoretic considerations such as aspirations, Siegel and Fouraker saw both the origins of their experiments and their outcome as squarely in the game theoretic tradition. Speaking of von Neumann and Morgenstern's *Theory of Games and Economic Behavior*, Fouraker and Siegel say (1963, p. 6) "The reinforcement of economic theory with the mathematics and general methodology of that magnificent work has provided the impetus for a broad front of new research; we hope this book is a proper element of that movement."

24.American experimenters and theorists have subsequently come to regard aspirations as at most an intermediate variable, rather than as a primary explanatory variable. Our German counterparts have been more inclined to regard aspirations as a primary explanatory variable (although the two sides don't divide up quite so neatly: see e.g. the edited volume on "Aspiration Levels in Bargaining and Economic Decision Making," (Tietz, 1983). For other early thoughts on aspirations and expectations, see Simon (1959). The work of Sauermann and Selten in these early papers, and subsequently, has other things in common with the work of Simon, such as their common interests in decision making process.

25.It has today become extremely rare to see an experiment that does not use real payments published in an economics journal. However the relative efficacy (and cost efficiency) of the two kinds of experiments remains a subject of lively debate (particularly when the real payments may be small, or not very sensitive to players' behavior), and the debate is fueled by the fact that a number of individual choice phenomena which have first been identified with (inexpensive) experiments using hypothetical rewards have subsequently been robustly reproduced with real payments. (See e.g. Thaler, 1987, who after reviewing a number of studies in which the difference between real and hypothetical payments did not yield important differences in results notes that (p. 120): "Asking purely hypothetical questions is inexpensive, fast, and convenient. This means that many more experiments can be run with much larger samples than is possible in a monetary-incentives methodology.")

26.Rapoport and Orwant (1962) began their review by observing that "Such [experimental] research is rapidly becoming voluminous, but an overview of it can still be crammed into one article."(p1). Friedman (1969) appeared in a special "Symposium on Experimental Economics" that also included reports of experiments by Carlson and O'Keefe (1969), Cummings and Harnett (1969), Hogatt (1969), MacCrimmon and Toda (1969), and Sherman (1969).

27.The experimental papers included in Sauermann (1967) are Sauermann and Selten (1967a,b), Selten (1967a,b,c), Tietz (1967), and Becker (1967).

28.See the bibliographies in Sauermann (1967) and Shubik (1975). Other notable experimenters and experiments from this period are Becker, DeGroot, and Marschak (1963a,b,1964); Bower (1965); Contini (1968); Dolbear, Lave, Bowman, Lieberman, Prescott, Rueter, and Sherman (1968); Ellsberg (1961); Friedman (1963); Lave (1962); Lieberman (1960); Maschler (1965); Rapoport and Cole (1968); Shubik (1962); Smith (1962, 1964); and Yaari (1965). Rapoport, Guyer, and Gordon (1976, p. 423) present a graph of the "Number of articles, books, memoranda, etc., published from 1952 to 1971, on various aspects of game experiments" which shows a fairly steady rise from about thirty papers published in 1960 to between ninety and a hundred papers in each of 1967, 68, and 69, many by social psychologists.