PART I

The Formative Years
Introduction

Experimental market economics began with a burst of papers and books from 1959 to 1963: Hoggatt (1959), Sauer and Selten (1959, 1960), Siegel and Fouraker (1960), Fouraker, Shubik, and Siegel (1961), Smith (1962), Fouraker and Siegel (1963), Suppes and Carlsmith (1962), and Friedman (1963). Many of us were unaware of the parallel research being conducted almost simultaneously by others. E. H. Chamberlin’s (1948) precursory study of an informal exchange market had directly influenced the first experiments I conducted in the period 1956–60. This constitutes the published background to the nine papers appearing in the first part of this collection.

The unpublished background includes my significant encounter with Sidney Siegel, reported in my essay “Experimental Economics at Purdue” (1981). One can only speculate as to the course of experimental economics in the last quarter century had it not been for Sid Siegel’s untimely death in the autumn of 1961. My opinion is that his energy and towering intellectual competence and technique as an experimental scientist would have accelerated greatly the development of experimental economics. Had he lived there would have been a sustained effort in experimental economics at another institution besides Purdue University. It appears that he has no intellectual descendants in psychology, but many in economics, although few of the latter may be fully aware of their heritage. Also part of the unpublished background is my experience (discussed in paper 9) in the revision and final acceptance for publication of my first experimental paper. My referees did not understand (nor did I at the time) the significance of what I was attempting to do in that paper, and their questions forced me to begin thinking about how we come to know what we think we know, and why one might do laboratory experiments. This experience strengthened my resolve to do several follow-up studies (papers 2–5) and, as it developed, to work out a methodology of experiment for economics. In the meantime I had to make a living and during these formative years I was writing and publishing on other topics – capital and investment theory, corporate finance theory and the economics of uncertainty, and natural resource economics. Compared with the experimental work, this other re-
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search was much easier to do and easier to publish. The ballistics of investment and bioeconomics was just undergraduate physics with a flair for modeling in a particular economic context. The profession was hungry for new theory in these areas, indeed, any area that employed recognizable methods; it was not hungry for evidence, and certainly not laboratory evidence, because theory provided all the requisite understanding. One had to justify one’s interest in experimental inquiry with each new effort. This had a disciplinary and salubrious effect on the development of my thinking as to what economic inquiry was about. I would have been insulated from that influence if I had adhered to more familiar professional paths, comfortably supported by cultures that did not reason why. Other experimentalists have had similar experiences. Experimentation in economics owes much of its development to the challenge to reexamine continually everything we do — our procedures, our empirical interpretation of theories, the replicability and robustness of results, and the implications of experiment for new and better theory. But other areas of empirical research, notably economic history, have evolved under a somewhat similar discipline.

I began teaching a graduate seminar, annually, in experimental economics at Purdue in the spring of 1963. My students that year were Robert Brennen, Jerry Dake, Carter Franklin, Clarke Johnson, Thomas Muench, James Murphy, John Powers, Donald Rice, Hugo Sonnenschein, James Stremo, Peter Stroth, Norman Weldon, and John Wertz. My paper with Don Rice, “Nature, the Experimental Laboratory, and the Credibility of Hypotheses” (1963), grew out of that seminar. It was our first methodological effort, and dealt exclusively with the issue I later would call parallelism, a term I got from reading some work of the astronomer Harlow Shapley. His use of the term was with reference to biological parallelism, but I thought it was a term worth generalizing. I never liked the phrase external validity as used in psychology because the problem of the comparability of data sets applies to any two environments whether they are both naturally occurring, both experimental, or one is experimental and the other naturally occurring. All environments are relevant sources of data on some aspect of human behavior, perceptions, or thought processes. I think the conceptual approach that Don Rice and I used in this paper to analyze models, data from nature, and data from experiment is still useful. The particular Bayesian formalism we develop has never, to my knowledge, been implemented. But informally, I think it captures the essential elements of the scientist’s prior and posterior beliefs about theory and the relevance of different kinds of evidence to testing theory. That framework includes the case in which one’s a priori belief is that a particular kind of experimental data is of no relevance to theory or field observations. An example, perhaps, for some economists is the Kahneman-Tversky hypothetical assessment of attitudes toward risk. For those who distrust hypothetical
assessments no number of replications will change such a person’s posterior beliefs about the credibility of the theory. A less subjective example is the following: the fact that a stone falls when dropped does not verify Newton no matter how many times the experiment is replicated. The observation is consistent with all inverse laws of attraction and is irrelevant to testing the inverse square law.

My “Experimental Studies of Discrimination versus Competition in Sealed-Bid Auction Markets” (paper 5) became part of a series of developments related to the design of market institutions. This development is reported in the introduction to Part V.

The idea that laboratory methods in economics are driven by what I called “induced value theory” was developed in lecture-discussion notes for my experimental seminar at Purdue in the period 1963–67. This theory was originally designed to explicate the role of rewards in defining the private characteristics (“circumstances of time and place”) of each subject and thus the market supply and demand environment (known by the experimenter, not the subjects) in my early experiments. It also sought to explain, in terms of “other things in the utility function” (that is, other motivations than money) why some experiments might be consistent with some theory in the absence of monetary rewards, and why some experiments might fail because of inadequate motivation. The objective was to provide some structured guidelines for designing and interpreting the results of experiments. In thinking through the implications of “other things in the utility function,” I found Sid Siegel’s paper on the two-choice uncertain outcome situation particularly helpful (Siegel 1961). In this binary choice situation, the interpretation of over twenty years of psychology literature had been that people were not rational; specifically, they failed to maximize. Since monetary payoffs had not been used, Siegel hypothesized that subjects did not maximize because there was nothing of value worth maximizing, and that the observed matching behavior of subjects was due to “monotony, both kinesthetic and cognitive” (Siegel 1961, p. 768). Accordingly, he developed an additive model of utility with two terms: the first was the utility of reward, the second the utility of variability, diversification, or monotony relief. The model predicted that subjects would be drawn away from matching toward maximizing by introducing monetary payoffs, and that the greater the payoff levels the nearer would be the response to the maximizing response. The data confirmed the prediction. Then Siegel’s ingenuity was turned to a procedure for raising the utility of variability as a treatment. I leave it to the reader to find out how this was accomplished. The data confirmed the prediction. This work was, in my view, of fundamental methodological importance, and I think it is unfortunate that it was not more widely known among experimentalists in both economics and psychology. Perhaps it was not possible for this work to be widely known in
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either of these two cultures, if economists were willing to accept the premise of the paper without evidence and if psychologists were unwilling to accept the premise with evidence.

In my experimental classes induced value theory was soon generalized to choice in “commodity space,” and by 1965 it had provided the basis for a general equilibrium “Edgeworth box” experiment for a student’s (George Hill’s) project in my seminar class. The results of that experiment turned out to be inexplicable, not because of problems with induced valuation but because none of us was yet fully sensitive to the institution as a variable also driving behavior. We had used a uniform price sealed bid-offer auction for the first time and this, as it became clear to me much later, was the source of the inexplicable behavior.

I discussed these ideas in the late 1960s and early 1970s with Charles Plott, who prevailed upon me to get “something written up” that could be cited. This led to the two 1976 papers on induced valuation and bidding and auctioning institutions. This also led to two joint papers with Plott and with Miller and Plott that were priceless first collaborations for me. The second is reprinted here, the first in Schools of Economic Thought: Experimental Economics, cited in the Preface. During this period M. Fiorina and Plott were using the concept to induce individual preferences on a two-commodity public good space \((X_1, X_2)\). The institution they used was Robert’s Rules of Order, including majority rule, while the message space consisted of amendments and votes. M. Levine and Plott collaborated to add the agenda to this institution and to manipulate it as a treatment variable. In these papers, Fiorina, Levine, and Plott were instrumental in creating the complementary field of experimental political economy.

References

Introduction


AN EXPERIMENTAL STUDY OF COMPETITIVE
MARKET BEHAVIOR

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I. INTRODUCTION

Recent years have witnessed a growing interest in experimental games such as management decision-making games and games designed to simulate oligopolistic market phenomena. This article reports on a series of experimental games designed to study some of the hypotheses of neoclassical competitive market theory. Since the organized stock, bond, and commodity exchanges would seem to have the best chance of fulfilling the conditions of an operational theory of supply and demand, most of these experiments have been designed to simulate, on a modest scale, the multilateral auction-trading process characteristic of these organized markets. I would emphasize, however, that they are intended as simulations of certain key features of the organized markets and of competitive markets generally, rather than as direct, exhaustive simulations of any particular organized exchange. The experimental conditions of supply and demand in force in these markets are modeled closely upon the supply and demand curves generated by the limit price orders in the hands of stock and commodity market brokers at the opening of a trading day in any one stock or commodity, though I would consider them to be good general models of received short-run supply and demand theory. A similar experimental supply and demand model was first used by E. H. Chamberlin in an interesting set of experiments that pre-date contemporaneous interest in experimental games.\(^1\)


1. Experimental Study of Competitive Market Behavior

Chamberlin’s paper was highly suggestive in demonstrating the potentialities of experimental techniques in the study of applied market theory.

Parts II and III of this paper are devoted to a descriptive discussion of the experiments and some of their detailed results. Parts IV and V present an empirical analysis of various equilibrating hypotheses and a rationalization of the hypothesis found to be most successful in these experiments.

Part VI provides a brief summary which the reader may wish to consult before reading the main body of the paper.

II. EXPERIMENTAL PROCEDURE

The experiments discussed in Parts III and IV have followed the same general design pattern. The group of subjects is divided at random into two subgroups, a group of buyers and a group of sellers. Each buyer receives a card containing a number, known only to that buyer, which represents the maximum price he is willing to pay for one unit of the fictitious commodity. It is explained that the buyers are not to buy a unit of the commodity at a price exceeding that appearing on their buyer’s card; they would be quite happy to purchase a unit at any price below this number—the lower the better; but, they would be entirely willing to pay just this price for the commodity rather than have their wants go unsatisfied. It is further explained that each buyer should think of himself as making a pure profit equal to the difference between his actual contract price and the maximum reservation price on his card. These reservation prices generate a demand curve such as $DD$ in the diagram on the left in Chart 1. At each price the corresponding quantity represents the maximum amount that could be purchased at that price. Thus, in Chart 1, the highest price buyer is willing to pay as much as $3.25 for one unit. At a price above $3.25 the demand quantity is zero, and at $3.25 it cannot exceed one unit. The next highest price buyer is willing to pay $3.00. Thus, at $3.00 the demand quantity cannot exceed two units. The phrase “cannot exceed” rather than “is” will be seen to be of no small importance. How much is actually taken at any price depends upon such important things as how the market is organized, and various mechanical and bargaining considerations associated with the offer-acceptance process. The demand curve, therefore, defines the set (all points on or to the left of $DD$) of possible demand quantities at each, strictly hypothetical, ruling price.

Each seller receives a card containing a number, known only to that seller, which represents the minimum price at which he is willing to relinquish one unit of the commodity. It is explained that the sellers should be willing to sell at their minimum supply price rather than fail to make a sale, but they make a pure profit determined by the excess of their contract price over their minimum reservation price. Under no condition should they sell below this minimum. These minimum seller prices generate a supply curve such as $SS$ in Chart 1. At each hypothetical price the corresponding quantity represents the maximum amount that could be sold at that price. The supply curve, therefore, defines the set of possible supply quantities at each hypothetical ruling price.

In experiments 1–8 each buyer and seller is allowed to make a contract for the exchange of only a single unit of the commodity during any one trading or market period. This rule was for the sake of simplicity and was relaxed in
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Each experiment was conducted over a sequence of trading periods five to ten minutes long depending upon the number of participants in the test group. Since the experiments were conducted within a class period, the number of trading periods was not uniform among the various experiments. In the typical experiment, the market opens for trading period 1. This means that any buyer (or seller) is free at any time to raise his hand and make a verbal offer to buy (or sell) at any price which does not violate his maximum (or minimum) reservation price. Thus, in Chart 1, the buyer holding the $2.50 card might raise his hand and shout, “Buy at $1.00.” The seller with the $1.50 card might then shout, “Sell at $3.60.” Any seller (or buyer) is free to accept a bid (or offer), in which case a binding contract and the maximum demand price of the buyer involved in the transaction. These observations represent the recorded data of the experiment. Within the time limit

8 All purchases are for final consumption. There are no speculative purchases for resale in the same or later periods. There is nothing, however, to prevent one from designing an experiment in which purchases for resale are permitted if the objective is to study the role of speculation in the equilibrating process. One could, for example, permit the carry-over of stocks from one period to the next.

8 Owing to limitations of manpower and equipment in experiments 1-6, bids and offers which did not lead to transactions could not be recorded. In subsequent experiments a tape recorder was used for this purpose.
of a trading period, this procedure is continued until bids and offers are no longer leading to contracts. One or two calls are made for final bids or offers and the market is officially closed. This ends period 1. The market is then immediately reopened for the second "day" of trading. All buyers, including those who did and those who did not make contracts in the preceding trading period, now (as explained previously to the subjects) have a renewed urge to buy one unit of the commodity. For each buyer, the same maximum buying price holds in the second period as prevailed in the first period. In this way the experimental demand curve represents a demand per unit time or per trading period. Similarly, each seller, we may imagine, has "overnight" acquired a fresh unit of the commodity which he desires to sell in period 2 under the same minimum price conditions as prevailed in period 1. The experimental supply curve thereby represents a willingness to supply per unit time. Trading period 2 is allowed to run its course, and then period 3, and so on. By this means we construct a prototype market in which there is a flow of a commodity onto and off the market. The stage is thereby set to study price behavior under given conditions of normal supply and demand. Some buyers and sellers, it should be noted, may be unable to make contracts in any trading period, or perhaps only in certain periods. Insofar as these traders are submarginal buyers or sellers, this is to be expected. Indeed, the ability of these experimental markets to ration out submarginal buyers and sellers will be one measure of the effectiveness or competitive performance of the market.

The above design considerations define a rejection set of offers (and bids) for each buyer (and seller), which in turn defines a demand and a supply schedule for the market in question. These schedules do nothing beyond setting extreme limits to the observable price-quantity behavior in that market. All we can say is that the area above the supply curve is a region in which sales are feasible, while the area below the demand curve is a region in which purchases are feasible. Competitive price theory asserts that there will be a tendency for price-quantity equilibrium to occur at the extreme quantity point of the intersection of these two areas. For example, in Chart 1 the shaded triangular area $APB$ represents the intersection of these feasible sales and purchase sets, with $P$ the extreme point of this set. We have no guarantee that the equilibrium defined by the intersection of these sets will prevail, even approximately, in the experimental market (or any real counterpart of it). The mere fact that, by any definition, supply and demand schedules exist in the background of a market does not guarantee that any meaningful relationship exists.

*The design of my experiments differs from that of Chamberlin (ibid.) in several ways. In Chamberlin's experiment the buyers and sellers simply circulate and engage in bilateral haggling and bargaining until they make a contract or the trading period ends. As contracts are made the transaction price is recorded on the blackboard. Consequently, there is very little, if any, multilateral bidding. Each trader's attention is directed to the one person with whom he is bargaining, whereas in my experiments each trader's quotation is addressed to the entire trading group one quotation at a time. Also Chamberlin's experiment constitutes a pure exchange market operated for a single trading period. There is, therefore, less opportunity for traders to gain experience and to modify their subsequent behavior in the light of such experience. It is only through some learning mechanism of this kind that I can imagine the possibility of equilibrium being approached in any real market. Finally, in the present experiments I have varied the design from one experiment to another in a conscious attempt to study the effect of different conditions of supply and demand, changes in supply or demand, and changes in the rules of market organization on market-price behavior.