

Experimental economics, history of

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Experimental economics has been the protagonist of one of the most stunning methodological revolutions in the history of science. In just a few decades, economics has been transformed from a discipline where the experimental method was considered impractical, ineffective, and largely irrelevant, to one where some of the most exciting advancements are driven by laboratory data.

Like many other new developments in the social sciences during the second half of the twentieth century, experimental economics is largely a by-product of the combination of massive investments in science, a fertile intellectual culture and socio-political conditions in the 1940s and 50s in the United States. Although it is possible in principle to identify earlier experimental or proto-experimental work being done in economics and psychology (see Roth, 1995), there is hardly any direct intellectual, personal or institutional continuity between these isolated episodes and today's fully institutionalized experimental program. The most striking example is perhaps the series of attempts to estimate demand functions experimentally, that involved several psychologists and economists starting from the early 1930's (cf. Moscati, 2006). Despite overlapping in time with the work of the recognized pioneers of the discipline, these studies failed to kick off a large-scale experimental revolution such as the one that took place from the 1980s onwards.

A proper history of experimental economics is yet to be written, and one challenge faced by historians of the discipline is its strikingly interdisciplinary character. The rise of experimental economics takes the form of several, partly independent and partly intertwined threads that can be brought under a single coherent narrative only with difficulty. It is partly for this reason that most of the existing historical literature consists of personal recollections or reconstructions of individual trajectories rather than of a collective enterprise. It is possible, however, to identify some key moments and achievements that have helped to establish experimentation as a legitimate method of investigation in economics.

The traditional view of economics as a primarily non-experimental science was outlined in the methodological writings of nineteenth century economists. John Stuart Mill for example (1836, p. 124) identifies several practical obstacles towards the use of the

experimental method, in particular the impossibility of controlling key economic variables, and of keeping background conditions fixed so as to check the effect of manipulating each cause in isolation. This was Mill's main justification for adopting the so-called "a priori deductive" method, a mix of introspection and theoretical reasoning, to determine what an idealized *homo oeconomicus* would do in given circumstances. Despite various changes in economists' methodological rhetoric and practice, it took a century and a half for philosophical skepticism towards experimentation to fade away.

Like many methodological revolutions in science, the experimental turn in economics was primarily made possible not by a change in philosophical perspective, but by a number of innovations at the level of scientific practice and theoretical commitment. At a very general level, in the middle of the twentieth century economics was in the process of becoming a "tool-based" science (Morgan, 2003): from the old, discursive "moral science" of political economy, it was changing into a discipline where models, statistics, and mathematics played the role both of instruments and, crucially, of *objects* of investigation. During this conceptual revolution economists came to accept that the path towards the understanding of a real-world economy might have to go through the detailed analysis of several tools that have apparently only a vague resemblance to the final target of investigation. Theoretical models and computer simulations entered the economists' basic toolkit first, with laboratory experiments following shortly after.

It is generally agreed now that the birth of experimental economics owes a lot to the publication of von Neumann and Morgenstern's *Theory of Games and Economic Behavior* (1944) and to the subsequent developments of game and decision theory. Although game theory is often seen as a contribution primarily to the theoretical corpus of contemporary economics, this was not how it was perceived at the time. Von Neumann and Morgenstern's work initially found fertile ground in a community of scientists, devoted to the simultaneous development of a great variety of approaches and research methods, and interested in their application to solve scientific, policy, and management problems across the disciplinary boundaries – from conflict resolution in international relations, to group psychology, cybernetics, and the organization of the firm, to name just a few.

"Gaming" – playing game-theoretic problems for real – was common practice in the mathematical community at Princeton in the 1940s and 50s, and quickly spread elsewhere as game theory increased in popularity. This practice did not involve sophisticated experimental design, but was conceived mainly as a useful way of illustrating game theoretic puzzles, as well as a check on abstract speculation and a guide to the theoretician's intuitions. Traces of this attitude can be found in the writings of some pioneers in game theory in the 1950s, who explicitly advocated a combination of formal theorizing with empirical evidence of various kind, and engaged in (mostly casual) forms of experimenting to back up their theoretical claims. A chapter of Thomas Schelling's *The Strategy of Conflict* (1960), for example, is revealingly called "Game Theory and Experimental Research"; and Martin Shubik's explorations of simulation and "gaming" in the same years also had a distinctively experimental flavor (e.g. Shubik, 1960).

The first event devoted specifically to “The Design of Experiments in Decision Processes” was a 1952 two-month seminar sponsored by the Ford Foundation, organized in Santa Monica by a group of researchers at the University of Michigan. The seminar’s location was intended to facilitate the participation of members of the RAND Corporation, a think-tank sponsored by the US Air Force, where among others Merrill Flood was conducting various game-theoretic experiments (including famously the first Prisoner’s Dilemma experiments). It is difficult to assess at all precisely the role of the Santa Monica seminar in the birth of experimental economics, because apart from an important minority, most of the papers eventually published in the proceedings (Thrall, Coombs and Davis, 1954) are theoretical rather than experimental in character. Several later protagonists, however, first became familiar with the idea of experiments in economics thanks to the Santa Monica seminar, which therefore functioned as a catalyst in various indirect ways (cf. Smith, 1992).

The most extensive experimental projects of the 1950s were pursued at Penn State, Michigan, and Stanford. In collaboration with Lawrence Fouraker, the psychologist Sidney Siegel conducted a systematic investigation of bargaining behavior at Pennsylvania State University, trying to combine what he took to be the most advanced aspects of economics (the theory) and psychology (the experimental method). The project came to an abrupt end with Siegel’s death in 1961, but the resulting book (Siegel and Fouraker, 1960) was well received and won the American Academy of Arts and Sciences best monograph prize. Siegel and Fouraker’s experiments focused on several aspects of bargaining behavior, but are particularly significant for the systematic study of variations in the monetary payoffs and in the information made available to the subjects. Interestingly, this research project was rather disjoint from current developments in axiomatic bargaining theory, focusing instead on testing various hypotheses from the psychological literature. “Level of aspiration theory” emerged eventually as the best predictor of bargaining behavior.

From the point of view of experimental design, Siegel is often credited with being the first experimenter to highlight the importance of using real incentives to motivate subjects but, with hindsight, his experiments with Fouraker are also remarkable for the implementation of strict between-subjects anonymity. The latter practice would become very common in later experimental economics, usually as an attempt to implement economic theory’s standard atomistic assumptions (especially the ban on other-regarding preferences). Fouraker and Siegel, in contrast, recognized that interpersonal reactions do matter (contrary to the standard economic theory), but left a systematic investigation of their effects for later research.

More or less simultaneously Ward Edwards at Michigan pioneered the experimental study of Expected Utility Theory, as axiomatized in the second edition of *The Theory of Games* (1947). Amos Tversky, a student of Edwards and Coombs, would play a major role in the institutionalization of behavioral economics two decades later, as we shall see. In the mid-fifties an interdisciplinary group was also at work on the new theory of individual decision making, under the heading of the “Stanford Value Project”. Donald

Davidson and Pat Suppes (both to become famous later for their contributions to philosophy) published with Siegel one of the first monographs of experimental decision theory (Davidson et al., 1957). At the centre of their research were measurement issues, in particular the implementation of learning theory and Frank Ramsey's method for measuring utilities and subjective probabilities.

Another major centre of interdisciplinary research in those years was the Carnegie group working on the psychology of organizations. Herbert Simon – working at Carnegie and the RAND Corporation, himself a participant in the Santa Monica seminar – is usually credited as a pivotal player in this connection, although his influence on experimental economics is not so straightforward. The Carnegie group made use of a variety of methodologies, among which experimental “role playing”, “business games”, and simulations were central. In their larger projects, like the “Carnegie Tech Management Game”, human decision makers took managerial decisions in an environment simulated by a computer. Although primarily devised for pedagogic and illustrative purposes, such games were also used to shed light on the “boundedly rational” processes of decision making that guide behavior in big organizations. Simon played an important role more as a source of moral support and intellectual inspiration than for his direct input into the experimental literature. As a matter of fact, later research in behavioral and experimental economics would depart substantially from the spirit of the bounded rationality approach, devoting more effort to emphasize deviations from the normative model of rationality rather than to the investigation of the routines and decision algorithms used by decision makers (Lopes, 1992).

The most famous experimental discovery of this period is due to a scholar who was to have little to do with later developments in experimental economics. Maurice Allais had been developing in France his own version of utility theory as a cardinal measurable quantity, well before the publication of the *Theory of Games*. His interest in violations of Expected Utility was therefore driven by the conviction that von Neumann and Morgenstern's theory was unable to capture the utility and probability functions of real decision makers. At a conference he organized in Paris in 1952, during a lunch-break, Allais presented Leonard Savage with a “questionnaire” that was to become famous as the “Allais paradox” experiment. When Savage gave answers that were inconsistent with the expected utility model he himself supported, Allais was encouraged to extend his questionnaire and to circulate it more widely.

Interestingly Allais took the violations of expected utility not only as entirely predictable, but as proof of the theory's descriptive *and* normative inadequacy. The results were partially published in French in *Econometrica* (1953) but received little attention in the short term. The main immediate result of the Allais experiment was Savage's switch to a purely normative defense of expected utility (Jallais and Pradier, 2005). Milton Friedman at the time was developing his methodology of positive economics which accorded no importance to the accuracy of the models of individual decision used to predict aggregate phenomena; and Allais's chauvinistic polemic against the “American School” probably did little to attract sympathy. For about two decades Allais did not pursue research in this area any further.

The only large-scale experimental research project in Europe during this period was led by Reinhard Selten in Frankfurt, under the auspices of Heinz Sauermann. Selten had been trained in mathematics and economics, but had also attended courses in experimental psychology. He was therefore particularly well positioned to grasp the potential of von Neumann and Morgenstern's work, but also to appreciate the central message of Simon's bounded rationality program. Like other early game theorists, Selten had immediately realized that the theory could contribute to the solution of important social science problems only if used in conjunction with empirical evidence. Indeed, even his most celebrated theoretical achievement (the concept of subgame perfection) was conceived in the context of a larger experimental project (Selten, 1995).

The last piece of the puzzle of experimental economics in the 1950s is at the same time the most important and the most idiosyncratic. Vernon Smith had been experimenting at Purdue, since 1956, focusing on the properties of different market institutions and their effects on the convergence towards equilibrium (cf. Smith, 1981). Smith had an engineering background and, unlike most experimenters at the time, did not approach experiments from a game theoretic perspective. In the 1940s and 1950s Edward Chamberlin at Harvard had been performing little classroom experiments for illustrative purposes, to show his graduate students the falsity of the competitive theory of markets. Although the results of such experiments had been published in the *Journal of Political Economy* (1948) nobody at the time, including Chamberlin himself, attributed particular scientific value to them. Smith was the exception: a few years after leaving graduate school he came to question the design used by Chamberlin, and to test the robustness of the "no convergence" results to variations in the exchange institution and repetition of the task.

Overcoming several obstacles, Smith managed to publish his counter-experiments to Chamberlin (Smith, 1962). In the early sixties Smith's experimental work received funding from the National Science Foundation, but apart from a brief attempt to collaborate with the Carnegie group (see Lee, 2004), his work in this phase was mostly carried out in isolation. One important exception is Smith's brief but important encounter with Sidney Siegel at Stanford in 1961. Smith perceived Siegel as much more advanced in methodological matters, and took from him several insights in experimental design that were to become the hallmark of economic experimentation (Smith, 1981; 1992).

Like other innovations of the previous two decades experimental economics went through a period of slow, quiet growth in the 1960s. Some early contributors, like Allais, disappeared from the scene; others, like Smith, quit experimenting for some time (1967-74) and generally struggled to find an audience. Some areas, like social dilemmas and bargaining experiments, were booming in psychology, but had little impact on the economics literature (cf. Leonard 1994). In the 1970s however the landscape of experimental economics changed considerably, partly thanks to the formation of a few key partnerships. During 1968-69 Amos Tversky began collaborating with Daniel Kahneman at the Hebrew University, initially on judgment, then on decision making. A visit to Oregon in 1971-72 cemented the link between the Jerusalem duo and

psychologists such as Robyn Dawes, Sarah Lichtenstein and Paul Slovic who were working on similar topics.

In Europe, by 1972, Selten had moved to Bielefeld and started a collaboration with Werner Guth, later author of the first experiments on the Ultimatum Game (Guth et al., 1982). Allais in the meantime returned to expected utility in 1974, and was persuaded to publish a full report in English of his 1952 results (in Allais and Hagen, eds., 1979). This renewed interest would lead to a series of conferences and to the formation of an interdisciplinary community of theorists and experimenters in the Foundations of Utility and Risk Society. Allais's legacy would also begin to bear some fruits on the theoretical front. The late 1970s and early 1980s were characterized by a proliferation of alternative models to expected utility, mostly inspired by the experimental evidence that had been accumulated up until then.

After the happy anarchy of the earlier period, the seventies were marked by the beginning of some controversies and the partial separation of the experimental community into sub-disciplines. In 1974 an article by Tversky and Kahneman in *Science* was widely read as a challenge to the view that human beings were rational agents, and although it made experiments on judgment and decision making enter the intellectual debate at large, it also fed some deep cross-disciplinary misconceptions. A few years later Lichtenstein and Slovic's (1971) seminal experiments on preference reversals were introduced into the economics literature by Grether and Plott (1979), kicking off a series of theoretical and experimental papers that would fill the pages of the *American Economic Review* for years.

Charles Plott had been in close contact with Vernon Smith since the early sixties, and started to run experiments after his move to Caltech in the seventies. Their collaboration that not only led to important experimental papers, but also to the creation of the Caltech laboratory and the training of the second and third generations of experimental economists. An important outcome of this period was also the attempt to systematize the methodology of experimental economics around a set of rules or "precepts" of experimental design (Smith, 1976; 1982). Smith in these papers highlighted the importance of monetary incentives to control subjects' preferences, a practice that he had borrowed from Siegel – a psychologist – but that ironically was to become the main distinguishing feature of the "economic" way of experimenting, as opposed to the more liberal "psychological" way. With hindsight these methodological papers are also striking for their effective use of the language and conceptual framework of mechanism design theory. In this sense they reflected Smith's (and Plott's) attitude towards the use of experiments to tackle real-world problems of institutional design and policy-making, which Smith cultivated since the nineteen-sixties (e.g. Smith, 1967) and which informed much of their subsequent work.

With the slow exhaustion of general equilibrium theory (Rizvi, 2005), the turmoil in macroeconomics, and an increasing disillusionment about econometrics, the seventies created the conditions for the seeds of the 1940s and 50s to finally blossom. Experimental economists were in a position to take advantage of this situation. By the early 1980s most

of the “paradigmatic” experiments that would inform subsequent research had already been published (Smith and Plott’s experiments on auctions and markets, Lichtenstein and Slovic on preference reversals, Plott and others on public goods, Guth on the ultimatum game, Alvin Roth and others on bargaining). Consolidation meant also differentiation. A persistent low-intensity conflict at the methodological and theoretical level led to the creation of so-called “behavioral economics”. Whereas experimental economics refers primarily to a method of investigation, the work of behavioral economists is unified by a substantial project of revision of economic theory. In particular, the behavioral approach aims at developing more realistic psychological models to replace rational *homo oeconomicus*, with experimentation constituting a major but by no means exclusive source of evidence.

The history of experimental economics in the 1980s and 1990s is the story of a booming research program, increasingly influential within the discipline and in the social sciences at large, expanding in new directions – neuroscience for example – and attracting some of the most talented graduate students. Together with game theorists, experimenters have also been increasingly involved in policy making, notably by contributing to the design of new market institutions for the allocation of sensitive goods – from telecommunication licences to space stations, airport slots, and physicians and surgeons (Roth, 2002). In 2002 the Nobel Memorial Prize in economics awarded to Vernon Smith and Daniel Kahneman provided official acknowledgement of this remarkable revolution.

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