



**Collected Papers on the Experimental Foundations of Economic and Political Science:**

**I. Public Economics, Political Processes, and Policy Applications;**

**II. Market Institutions and Price Discovery;**

**III. Information, Finance and General Equilibrium**

by Charles R. Plott  
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As everyone reading [the *JEP*] knows, the 2002 Nobel Prize in Economic Sciences went to Daniel Kahneman and Vernon Smith [BS '49]. The Economics Prize Committee of the Royal Swedish Academy of Sciences credits Smith as being “the most influential figure in launching experiments as an empirical methodology in economics” and “establishing standards for what constitutes a good experiment. Other researchers have furthered this tradition. Charles Plott, in particular, has written several important papers, further developed the experimental methodology, and spearheaded experimental research in new areas.”

I argue that these statements play fast and loose with the historical facts and sloppily and incorrectly assess the scope of Plott's work and his “pioneering influence” (as Smith put it graciously in his Nobel Banquet speech). Specifically, Smith, after acknowledging Kahneman's contributions, noted “I wish to celebrate . . . the pioneering influence of Sidney Siegel, Amos Tversky, Martin Shubik, and Charles Plott on the intellectual movement that culminated in the economics award for 2002.”

How influential a pioneer

was Plott? Plott's introduction to these volumes, and Smith's “Autobiography: the Early Years to 1975,” which he wrote upon the request of the Nobel Foundation, agree on many aspects of their collaboration in the late 1960s and early 1970s. It is undisputed, for example, that Smith, inspired by classroom experiments his teacher Edward Chamberlin conducted at Harvard, developed what he called the theory of induced valuation, sometime between 1963 and 1965 at Purdue. (Purdue was Smith's first teaching post and a place where he spent the overwhelming part of the years 1955–67, partially overlapping with Plott who, after having graduated from the University of Virginia, started his career at Purdue in 1965. After a one-year stint as visiting professor at Stanford during the academic year 1968–69, Plott moved in 1971 to the California Institute of Technology.)

As Plott describes it, “My introduction to the use of laboratory experimental methods in economics was in the late 1960s, resulting from the fact that Vernon Smith and I both enjoy fishing. We frequently fished together and while fishing we always talked about economics.

**From right: Miller, Plott, and Stefan Feuerabendt (BS '85) in 1983. At the time, Miller and Plott were working on a paper on information in markets. Then a professor of economics, Plott is now the Harkness Professor of Economics and Political Science.**



From time to time, Vernon would tell me about some of his experimental work. He had actively conducted experiments in the late 1950s and early 1960s, but by the late 1960s he was doing other things. Professional acceptance of his work had not been overwhelming. In fact, it had been substantially ignored and his research interests had long since turned elsewhere. That is not to say that his enthusiasm and capacity to talk about the topic had diminished.”

According to Smith’s account, while he continued to think about experimental economics, and while he used it in his teaching, in the late ’60s and early ’70s he had focused his energies on the economics of uncertainty, corporate finance, and natural resource economics, with considerable success. Smith then spent two years at Caltech, as a Sherman Fairchild Distinguished Scholar in 1973–74 and a visiting professor of economics in 1974–75. All the indications are that by the time Smith arrived at Caltech, a full-fledged and fully financed experimental program was under way, with many public-economics experiments completed, in which [Assistant Professor of Political

Science] Morris Fiorina and Plott were the main contributors.

Like the question of whether John Lennon or Paul McCartney was the main musician and songwriter for the Beatles, the question of the relative contributions of Vernon L. Smith and Charles R. Plott to experimental economics strikes me as irrelevant. The Beatles were, probably in most people’s view, the joint effort of Lennon and McCartney, and similarly a good case can be made for the symbiotic interaction of Plott and Smith, if only for a limited amount of time.

There is, in particular, agreement that Plott and Smith “talked experiment . . . on many bass fishing trips” during those years and that the two ended up teaching a seminar for student credit that was attended by “three paying customers (including an undergraduate, Ross Miller [BS ’75]), and several faculty.” (Both quotes are from Smith’s “Autobiography.”) There was no looking back for either of them, nor for experimental economics, from that spring semester in 1974 on. While efforts to keep Smith in Southern California failed—he chose instead to make the Univer-

sity of Arizona into one of the premier centers of experimental economics in the world—Plott continued to build the experimental program at Caltech into a center whose influence can hardly be overestimated. As David Warsh, editor of *economicprincipals.com* and formerly of *The Boston Globe* and *Forbes* magazine, put it in his online essay “The Vital Many,” “Caltech, despite the tiny coterie of barely fifteen social scientists on its faculty remains the discipline’s spiritual home.”

Apart from being the catalyst for Smith’s renewed attention to matters experimental, and apart from being the driving force behind the Caltech experimental program, Plott made a number of significant contributions, some of them with Smith and some of them with the students and faculty who attended that seminar in the spring of 1974. (Miller recently published an insightful and deservedly acclaimed book, *Paving Wall Street*, which traces modern finance and modern institutions such as frequency auctions and derivatives back to those simple experiments at Caltech.) I list some of Plott’s contributions below.

Plott recognized that the use of laboratory methodol-

ogy—back then applied exclusively to markets (by economists) or games (by political scientists)—could be applied to public economics, public choice, and indeed political science, i.e., on topics such as voting on which he had worked theoretically. His papers on committee decisions under majority rule and on the impact of agenda-setting on committee decisions—“a stunningly powerful tool to use if one is interested in manipulating voting groups,” as he called it—had tremendous impact and remain highly readable and entertaining pieces for everyone who wonders about the usefulness of experimental economics, or those who wonder about the ways faculty meetings transpire.

In the April 2003 issue of the *Southern Economic Journal*, Charles Holt of the University of Virginia tells the following story about the real effects of one early agenda-setting study. “[Luce Professor of Law and Social Change in the Technological Society Michael] Levine and Plott had been members of a flying club that was to meet and decide how to spend a large sum of money on a collection of airplanes to be used by the membership. After being appointed to serve on the Agenda Committee, they distributed a survey of members’ preferences to assist in structuring the discussion at the meeting. The survey results were used to design an agenda that the authors believed would yield a fleet of new aircraft that they [the authors] preferred. The president of the club had different preferences and repeatedly tried to deviate from the agenda during the meeting, but was ruled out of order in each case. The authors were asked to resign from the club after an account of the agenda strategy was published in

the *Virginia Law Review*.”

[Plott, however, says, “The Holt story is incorrect. I was not a member of the flying club. Levine was, and he came to me because he knew that I was an expert in committee decisions and he was responsible for a very large committee that had a very difficult and controversial decision to make. He wanted to know how to find the “best” decision, and I told him there was no such thing as a “best” or “fair” decision, and that the outcome was a function of the agenda’s design. Interested in the application of known science, I then talked him into letting me design a set of procedures that would be “fair” but also lead to the decision that he liked best. The survey was distributed afterward, and allowed us to test the agenda’s influence by revealing the preferences that were actually in place during the meeting. The club generally liked the agenda, congratulating Levine on a job well done, and there was certainly no asking for resignations—I have no idea where that came from.”—ed.]

Plott appears to have been the first experimentalist to

intervene in a regulatory dispute. Plott and James Hong [BS ’76] (reading 5, volume II), reported experiments conducted for the U.S. Department of Transportation to “shift the burden of proof in a policy debate.” The relevant debate involved the railroads and the dry-bulk barge industry. The railroad lobby wanted the barges to post their prices with the Interstate Commerce Commission, and claimed to want so for altruistic reasons (e.g., that it would lower prices, increase efficiency, and help the independent barge-owners). Apart from the alleged concern for consumer welfare on the part of the railroad lobby not being particularly credible in the first place, its claims clearly contradicted the so-called posted-price effect Plott and Smith had earlier identified: In a market where prices cannot be changed once they have been posted, prices get pushed up if sellers post them and pushed down if buyers post them. Sure enough, the general theory used by the railroads failed to predict what was observed in a simple and scaled-down version of the industry.

Plott, more than other experimentalists in the ’70s, seems to have realized the potential for what is now becoming known as “design economics.” This insight is likely to have been the result of Plott’s work in public choice (agenda setting) and of his work on regulatory issues, both of which invited thinking about counterfactual scenarios. The problem, as Plott says, was “to design institutions that perform a particular task, as was the case in the study of agendas. . . . The experiments are used as ‘test beds’ in which the performance qualities of the institutions are assessed and the reliability of the model that led to the design in the first place is ascertained.” Besides the barge study already mentioned, Plott and various collaborators did experiments that informed policy-making on, to name a few reprinted in these volumes, the allocation of airport landing slots; the right to use railroad tracks; price-setting policies for the use of space-station resources; FCC auctions; and the EPA’s new emissions-trading mechanism. Plott appreciated the importance of institutions—a theme acknowledged in his “fundamental equation” that related outcomes to various ways in which preferences, physical possibilities, and institutions could interact. It needs stressing that this insight today is second nature to all experimentalists, and even the better theorists, but back then in the dark ages of economic theorizing the importance of institutions was mysterious to most economists.

Plott pioneered with Smith and Miller a methodology for the study of assets, bubbles, futures markets, and other aspects of financial markets. As Smith comments, “this must have been the first scientific paper in economics

with an undergraduate coauthor.” That paper initiated numerous experimental studies on rational expectations and the ability of markets to aggregate information. Plott himself was involved in several influential papers that constitute a convincing exercise in persuasion about the astonishing ability of markets to, as he wrote in the *Southern Economic Journal* in 2000, “collect information that is dispersed across the economy, aggregate it like a statistician, and publish the findings in the form of prices.” And all that without publication delay, I cannot resist adding!

Plott pioneered the multiple-unit double auction, which permits multiple-unit or “block” trades and thus allows within the classic double-auction framework the study of markets with large volumes and many traders. This, preparing as it did the study of more complicated general-equilibrium and international-trade experiments, marked not only another important conceptual step—a major generalization of the double auction—but also yet another technological innovation. [It allowed as many as 20 markets to operate simultaneously in real time, allowing complex, interdependent systems to be studied. Even more importantly, it was designed to run over a Local Area Network, or LAN, rather than requiring specialized hardware, meaning that it could be (and was) set up by almost anyone almost anywhere. Its descendants are still in use today.—ed.] I should note that the multiple-unit double auction had perplexed both Plott and Smith for a long time.

While Plott rarely wrote explicit pieces about methodology, his oeuvre is pervaded by important methodological ruminations. In fact, pretty



**The very first multinational market experiment, run in December 1995 over the Web with participants in Australia, Canada, France, Germany, Japan, the Netherlands, Spain, Sweden, and elsewhere in the United States. From left are Hsing Yang Lee, the lab’s technical manager; Paul Brewer (BS ’89, MS ’92, PhD ’95); and Plott.**





**Plott's fishing trips continue—last summer found him with this beauty of a barramundi that he caught and released on the Bullo River some 400 miles southwest of Darwin, Australia.**

much every paper in these volumes contains them—clearly a reflection of the considerable hostility that pioneers such as Plott and Smith encountered. Writing in the mid-'90s, after Plott rightly claimed that experimental economics had become an experimental science, Lola Lopes of the University of Iowa still observed that “[a]lthough every major economic journal now regularly publishes experimental work, the field is still not mainstream and experimental economists have their work cut out to convince theoretical economists about the feasibility and value of subjecting economic ideas to empirical tests.” Even now, as Harvard’s Alvin Roth put it so memorably, “we’ve won the battle for the journals, but not yet the battle of departments.”

The Economics Prize Committee of the Royal Swedish Academy of Sciences identified Smith’s major successes as his contributions to market mechanisms, tests of auction theory, the use of the laboratory as a “wind tunnel,” and experimental methodology. Clearly, Plott has contributed his fair share to all of these, and then some. The present volumes are a most impressive testament to his accom-

plishments. How influential was he? Highly, if the measuring rod is the number of ISI citations, or the number of influential students, or his impact on issues of regulation, deregulation, or anti-trust, or the allocation of airport slots, or resources on space stations, etc. As Warsh observed, “Had the Swedes chosen not to combine the honors for experimental and behavioral economics [Kahneman’s field] in a single award, presumably Plott would have shared the prize with Smith.”

Kahneman is a follower of the heuristics-and-biases school of thought, which, in contrast to earlier schools, has argued that the “apparently universal quirks in human judgment that routinely affect economic behavior” (Warsh) demonstrate that ordinary people, and even experts, are cognitive misers whose reasoning, judgment, and decision-making abilities are an embarrassment to the picture of human beings as rational actors. Indeed, throughout the Nobel documents runs an undercurrent that suggests behavioral and experimental economics have jointly put to rest “homo oeconomicus”—that self-interested and rational beast with which neo-classical

economists have been so enamored.

The heuristics-and-biases program has been the dominant paradigm in research on human reasoning, judgment, and decision-making over the past few decades. In light of its rapidly growing acceptance among economists and other scholars, it is therefore interesting to note that it has been under attack for some time by a large number of psychologists. Drawing on notions of bounded rationality, these critics argue that humans have evolved surprisingly effective simple decision rules that in many contexts serve them well, and that this redefines what constitutes rationality by taking into account constraints on resources such as time, knowledge, and cognitive processing ability. I am convinced that future researchers in the philosophy and sociology of science will have a feast in tracing how an entrenched program such as heuristics-and-biases was able to take over another market while it was losing slowly but surely on its own turf. It has been fascinating to watch how economic theorists, often blissfully unaware of the disputed status of the heuristics-and-biases program, have taken the results

of its advocates at face value.

It is notoriously difficult to make assessments about the influence of academic researchers, especially if they are pioneers. For that reason, the Royal Swedish Academy of Sciences is an important, if not the most important, current writer of the history of thought. While every reconstruction is to some extent a rationalization that smoothes the course of history to construct a compelling narrative, the selection of what *Business Week* online called “the odd couple” of Kahneman and Smith not only has slighted the pioneering influence and accomplishments of Charles R. Plott, it has brushed away—and therein lies the real danger for all of us—the controversial conceptual and methodological issues that have made, and continue to make, for some of the most promising recent advances in both economics and psychology. □

*Plott, however, is philosophical about not having been given a share of that prize. “Of course, I was disappointed. However, the real disappointment was that the institutions that supported the research when no one else would, Caltech being the case in point, did not get the recognition.”*