Charles R. Plott's

Collected papers on the experimental foundations of economic and political science

1. Public economics, political processes, and policy applications
2. Market institutions and price discovery
3. Information, finance and general equilibrium

Cheltenham, UK, Northampton, MA, USA:
Edward Elgar 2001

A review essay

by

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March 15, 2003

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Acknowledgment: I appreciate the comments of Ted Bergstrom, Colin Camerer, Peter Earl, Werner Gueth, Glenn Harrison, and Ralph Hertwig on earlier drafts of this article. None of them should be held responsible for the views expressed here.

As everyone reading this journal knows, the Nobel Prize in Economic Sciences 2002 went to Daniel Kahneman and Vernon L. Smith. In its "advanced information", the Economics Prize Committee of The Royal Swedish Academy of Sciences (EPC 2002) credits Kahneman as having been "a major source of inspiration behind the recent boom of research in behavioral economics and finance. His research has also had a substantial impact on other fields." (p. 20) The authors credit Smith as being "the most influential figure in launching experiments as an empirical methodology in economics." (p. 20); they continue, "Unlike Kahneman, [Smith] did not start out by challenging the traditional economic theory of rational decision-making. Rather, he tested alternative hypotheses regarding market performance, in particular the importance of different market institutions." (p. 20) Smith, the authors continue, focused on the interaction between agents in specific market environments, and was instrumental in "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. (...)." (p. 20; see also p. 3[1], 5, 9, and 10 for other brief references to Plott's work.)

Below I shall argue that these statements (as well as similar statements in various derivative documents such as the "press release" or the "information for the public") play fast and loose with the historical facts and sloppily and incorrectly assess the scope of Plott's work and his "pioneering influence" (as Vernon L. Smith put it graciously in his Nobel Banquet speech, Smith 2002). 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 in the influential movement that culminated in the economics award for 2002."

It is, of course, notoriously difficult to make assessments about the influence of academic researchers, especially if they are pioneers. As Alvin E. Roth noted, "many contemporary experimental economists carry around with them different and very partial accounts of the history of the field." (Roth 1995, p. 4). This statement prompted David Warsh to observe, "But then the great thing about the Nobel Award is that it settles on one of these stories and personalizes it." (Warsh 2002, p. 2) Indeed. And therein lies a danger, as I shall argue below. But first to the issue at hand.

The volumes to be reviewed here collect in one place papers that span Plott's career. Selected and annotated by Plott himself, they are a useful starting point for an assessment of his work. Volume One collects 23 articles on methodology, various aspects of public economics, and policy applications, volume Two collects 26 articles on market institutions and price discovery, while volume Three, in addition to his classic "Will Economics become an Experimental Science?" 1991 presidential address to the Southern Economic Association, collects 19 papers on issues of information and asset valuation, general equilibrium, and individual behavior in systems. Each volume is prefaced by the same Introduction which briefly sketches his career[2], then details the history of the research

collected in these volumes, and comments on the contents of all volumes.

It is the nature and merit of such collections that they make accessible in one place papers that appeared in more unusual outlets. So here too, e.g., a methodological chapter on the application of laboratory methods to public choice (Plott 1979[3]) or the chocolate pizza design chapter (Cohen, Levine, & Plott 1978[4].) Apart from these relatively inaccessible papers, the volumes reviewed here also contain about two dozen articles that have appeared in top economics and political science journals.

How influential a pioneer was Plott? Plott's introduction to these volumes and an "Autobiography: the Early Years to 1975" that Vernon L. Smith wrote (2002a) upon the request of the Nobel Foundation, agree on many aspects of their collaboration in the late 1960's and early 1970's.[5] It is undisputed, for example, that Smith, inspired by classroom experiments his teacher E. H. Chamberlin conducted at Harvard, developed what he called the theory of induced valuation sometime between 1963 - 1965 at Purdue[6], later published as Smith (1976)[7]. According to his own account, while he continued to think about experimental economics, and while he used it in his teaching, in the late sixties and early seventies (i.e. before he visited Cal Tech), Smith had focused his energies on the economics of uncertainty, corporate finance questions, and natural resource economics, with considerable success.[8] In contrast, it seems that Plott was drawn into experimental economics through work with one of his graduate students at Purdue (Harvey Reed, more about him below) and, while already at Cal Tech, through a *eureka* experience that made him realize how the methodology that Smith had developed could be applied to problems in public economics, public choice and political science. All the indications are that by the time Smith arrived at Cal Tech, a full-fledged and financed experimental program was underway, with many public economics experiments completed, in which Morris Fiorina (who had conducted coalition experiments with Bill Riker and was familiar with experimental methods from social psychology) and Plott were the main contributors.[9]

Not surprisingly, there are some differences in the various recollections on which I draw but I shall have to leave it to interested historians of thought to sort them out. 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. Indeed, there is agreement that the years Smith spent as a visiting scholar at the Center for Advanced Study in the Behavioral Sciences (1972 - 73) and Cal Tech (1973 - 75) were pivotal years for the development of experimental economics in the USA[10].

There is in particular agreement that Plott and Smith "talked experiment on many bass fishing trips" (Smith 2002a, p. 13) 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) and several faculty" (Smith 2002a, p. 13) 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 University of Arizona into one of the premier centers of experimental economics in the world --, Plott continued to build the experimental program at Cal Tech into a center whose influence can hardly be overestimated. As Warsh puts it, "Cal Tech despite the tiny coterie of barely fifteen social scientists on its faculty remains the discipline's spiritual home." (Warsh 2002, p. 7)

Apart from being the catalyst to Smith's renewed attention to matters experimental, and apart from being the driving force behind the Cal Tech experimental program, Plott made a number of significant contributions, some of them with Smith and some of them with the students and faculty that attended that seminar in the Spring of 1974[11], and many by himself or in collaboration with later students and other collaborators.[12] Below I enumerate what I consider his significant accomplishments.

First, Plott recognized that the use of laboratory methodology – back then applied exclusively to markets 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. Indeed, Plott identifies his papers with Fiorina (Fiorina & Plott 1978) on committee decisions under majority rule and Levine (Plott & Levine, 1977; Levine & Plott 1978) on the impact of agenda setting on committee decisions – "a stunningly powerful tool to use if one is interested in manipulating voting groups" (Plott 2001, xiii) – as three of the four papers from which all the papers in these three volumes flow. Plott's insight, made possible by using public rather than private goods and by dropping the quasi-linearity of inducements that had been used up to that point, had tremendous impact beyond experimental research in economics[13]; these papers 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. All three papers are reprinted in part 2 of Volume One, as are various related papers such as Kormendi & Plott (1982) which, building on Isaac & Plott (1978), explored experimentally committee decisions under alternative procedural rules and a new non-monetary method of preference inducement (=points towards a course grade). Related is also Grether, Isaac, & Plott (1981) which is reprinted here (Volume One, reading 16), and which is a brief summary of Grether, Isaac, & Plott (1989), a book-length study of the allocation of airport landing slots, published as a report that garnered national level attention about a decade earlier.[14]

Second, and this brings us to the fourth paper Plott considers seminal (Plott & Smith 1978; like the path-breaking papers with Fiorina and Levine essentially written in 1974), he compared, in conjunction with Smith, the performance of one-sided oral auctions (earlier studied by Smith) and a new institution (earlier studied by F. Williams) that had two key distinguishing features: subjects could trade several units rather than one (as in Smith) but

they could not change the price once they had posted it (in contrast to Smith's auction institution). In effect, Williams (motivated by real-world retail markets and other price-posting markets in which there is a price commitment over some time) had stumbled upon a new laboratory market institution – the posted price institution that had rather interesting efficiency properties[15] and turned out to have significant policy relevance. Plott and Smith showed that it was in fact the second feature of the Williams experiment that accounted for the results that he got. Plott subsequently continued to explore the implications of this new institution and what became known as "posted price effect": prices get pushed up if sellers post them and prices get pushed down if buyers post them. Needless to say, the discovery of this effect had a major impact on experimental industrial organization research as evidenced in his early and widely cited *Journal of Economic Literature* article (Plott 1982) as well as his later Handbook article (Plott 1989) which updated and expanded the *JEL* article; all three articles are reprinted in parts 1 and 2 of Volume Two, as are many related articles such as Plott (1986) – a *Science* article that explained the implications of the posted price phenomenon and at the same time successfully conveyed the usefulness of experimental economics to a wider audience (reading 1, Volume Two).

Third, Plott appears to have been the first experimentalist to intervene in a regulatory dispute.[16] Plott and Hong (1982), also reprinted in these volumes (reading 5, Volume Two), reported experiments that Plott conducted for the US 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 railroads 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 small barge owners). Apart from the alleged concern for consumer welfare on the part of the railroads lobby not being particularly credible in the first place, its claims clearly contradicted the posted price effect Plott and Smith (1978) had identified. 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: "[T]his evidence presumably shifts the burden of proof to the price posting advocates, who must now identify the specific features of the barge industry which, if incorporated in the experiment, would reverse the conclusion." (Plott & Hong 1982, p. 18) Plott (1987), in another paper not reprinted in this collection, summarized a number of other studies and introduced to economists the "parallelism" concept which today is often used in less differentiated ways than originally used by Plott.[17] Other papers related to research in policy issues such as Isaac & Plott (1981) on the curious effects of (non-binding) price ceilings and floors, or Isaac & Plott (1981a) on the possibility of "conspiracy" (= collusion) in oligopolistic markets, or Grether & Plott (1984) on the effects of certain market practices that facilitate the maintenance of non-competitive prices, however, are reprinted in part 2, Volume Two.

Fourth, and closely related to the preceding point, Plott, more than other experimentalists in the seventies, seems to have realized the potential for what is now becoming known as "design economics" (Roth 2002). 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 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." (Plott 2001, xx) Understanding that experiments could be used as 'test beds' and being involved in a number of consequential attempts in using experimental research in actual policy making such as the barge study already mentioned (Plott & Hong 1982), the allocation of airport landing slots (Grether, Isaac, & Plott 1981; see also Gueler, Plott, & Vuong 1994) and the rights to use railroad tracks (Brewer & Plott 1996), space station pricing policies (Banks, Plott, & Porter 1988, Plott & Porter 1996), FCC auctions (Plott 1997), and the EPA's new emissions trading mechanism (Cason & Plott 1996), to name a few reprinted here, made Plott further appreciate the importance of institutions - a theme that was prominent already in Plott & Levine (1977) and Levine & Plott (1978)[18] as well as in Plott & Smith (1978), and in his own ruminations on the application of laboratory experimental methods to public choice (Plott 1979). In fact, the theme was acknowledged in the "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 (or disparaged, when it was mentioned by Austrians or institutional economists). It is therefore noteworthy that Plott, in several of his articles (e.g., Plott 1994, 1994a, reprinted here in part I, Volume One), already speaks of "designer markets" -- as in "designer markets are becoming a reality" – stressing the engineering aspect of such an enterprise. It is also noteworthy that Plott (1979, p. 155/6) had already suggested in his first major methodology piece – a companion piece, and counterpart of sorts, to Smith (1976) – that a major purpose of laboratory methods was the exploration of "new modes of organization" and of "'synthetic' processes" through "institutional engineering" (lit. cit.) The title of Plott's talk at the 2001 Nobel Symposium on Behavioral and Experimental Economics in Stockholm -- "Equilibrium, Equilibration, Information and Multiple Markets: From Basic Science to Institutional Design" (Plott 2001a) – reflected therefore well what in a different context he described as drift from "curiosity-driven" to "mission-driven" research, research for that matter that had real consequences.

Fifth, Plott contributed papers (e.g., Plott 1983; Isaac, McCue, & Plott 1985; Banks, Plott, & Porter 1988, all reprinted here) on non-classical economic environments, i.e. environments with externalities or public goods. The first two articles, in particular, have become classics of sorts. The first demonstrated that the market behaved as if there were no externalities at all and demonstrated that trading activities were not inhibited by the knowledge that one's trades would hurt others. The second article was a reaction to the work of Marwell & Ames (1979, 1980, 1981) whose results suggested a much more favorable impression of the human condition. Since Ledyard (1995, pp. 134 - 144) features a detailed and excellent discussion of the merits of the Isaac, McCue, & Plott experiment, and its implications for

the Marwell & Ames results, I shall leave it at that. (I ought to note though that this was one of the papers that was published with a large delay; as Ledyard notes correctly, the Isaac, McCue, & Plott results were well-known by 1980/81, and significantly influenced research efforts in the years leading up to publication.)

Sixth, Plott pioneered with Smith and that undergraduate seminar participant in the Spring of 1974, a methodology for the study of assets, bubbles, futures markets and other aspects of financial markets (Miller, Plott, & Smith 1977). As Smith comments, "this must have been the first scientific paper in economics with an undergraduate coauthor" (Smith 2002a, p. 14). It was, in any case, rather influential and initiated numerous experimental studies on rational expectations and the ability of markets to aggregate information. Plott himself was involved in influential papers such as those of Forsythe, Palfrey, & Plott (1982, 1984), or Plott & Sunder (1982; 1988), which made economists appreciate financial markets as efficient information gathering tools under various conditions (insiders). All these papers are reprinted in part 2 of Volume Three, as is Plott (2000) - a convincing exercise in persuasion about the astonishing ability of markets to "collect information that is dispersed across the economy, aggregate it like a statistician, and publish the findings in the form of prices." (2) And all that without publication delay, I can't resist adding! Other closely related papers are Plott's work on information cascades (Hung & Plott 2001; not reprinted here) which shows how public information and the actions of others are integrated with private information, his work on the winner's curse (Lind & Plott 1991; reprinted in Volume Two, reading 13) which demonstrates that the ability of markets to aggregate information is sensitive to organization and incentives, and his work on non-speculative bubbles in experimental asset markets (Lei, Noussair, & Plott 2001; not reprinted here) which identifies the relative contributions of lack of common knowledge of rationality and plain irrationality to their emergence. More recently, Plott was involved in various papers that explore the basic principles of asset pricing theory and the validity of the CAPM in both thick and thin financial markets (e.g., Bossaerts, Kleiman, & Plott 2000, reprinted as reading 14 in Volume Three; see also Bossaerts & Plott 1999 and Bossaerts & Plott 2002, both not reprinted).

Seventh, Plott pioneered with Louis Wilde issues of asymmetric information and quality assurance, an issue that 2001 Nobel prize laureates Akerlof, Spence, and Stiglitz had explored theoretically in the seventies. Miller & Plott (1985) and Lynch, Miller, Plott, & Porter (1991), which like Plott & Wilde (1982) are reprinted in part 2, Volume Three, seem to have been the first papers trying to experimentally understand the possibility of the "lemons" problem, and the potential of warranties and reputations as means that could overcome them. One of the surprising results of the Lynch, Miller, Plott, & Porter study was the finding that reputations might not necessarily do the trick, contra the message communicated by the path-breaking Klein & Leffler (1981). As the current rush to study reputation mechanisms on the internet demonstrates, these papers were well ahead of their time.

Eighth, Plott pioneered the multiple unit double auction(MUDA)(Plott & Gray 1990; see also Plott 1991, both reprinted in these volumes) which permits multiple unit or 'block' trades and thus allows within the classic DA framework the study of markets with large volumes and many traders. This article, preparing as it was 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. In follow-up work with J. C. Jamison, Plott studied the equilibration properties of the multiple unit double auction under conditions of unpredictable shifts of demand and supply (Jamison & Plott 1997; Vol Two, reading 16). I should note that the multiple unit double auction had perplexed both Plott and Smith for a long time. As a matter of fact, following a conversation on Lake Monroe, Indiana, in either fall 1969 or spring 1970, Plott persuaded a Ph.D. student of his – Harvey Reed – to write his dissertation on a related topic. This dissertation work was reported in Plott (1986a) in the section on reparameterization, another intriguing methodological idea whose basic rationale consists of understanding what subjects in fact do, and looking at whether a duly reparameterized model explains the data. It did for the Reed data, even though from today's perspective the experimental procedures were not up to snuff - an early and important example, thus, of the astonishing robustness of certain laboratory market environments.

Ninth, Plott pioneered experiments in general competitive equilibrium – classical circular flow – models, with fiat money and bonds as well as one input and one output, in a path-breaking paper (Lian & Plott 1998; see also Goodfellow & Plott 1990 which investigated the simultaneous determination of input and output prices; both papers are reprinted in part 3,Volume Three; see also Plott 2000, reprinted as reading 9 in Volume Three for a highly readable description). Once again, creating such a model was both a conceptual and technological masterpiece (especially taking into account that the Lian & Plott sessions were done in 1990!). Surprisingly, the data from all sectors of the economy were converging to the magnitudes predicted by the static general competitive equilibrium. These two papers laid the foundation for the experimental investigation of patterns of international trade, and the working of the law of comparative advantage, in Noussair, Plott, & Riezman (1995), and the principles of exchange rate determination, and working of the law of one price and purchasing power parity (both rejected!), in Noussair, Plott, & Riezman (1997). Both these papers are also reprinted in part 3, Volume Three. Needless to say, they are yet another convincing illustration of the ability of markets to aggregate and disseminate information. They are also, and the issue deserves special mention (and possibly a separate acknowledgment all by itself), yet another example of an important technological innovation. Arguably, no other experimental laboratory has, over time, produced the same number of innovations (e.g., the application of MUDA software on a local area network).

Tenth, whether exploring asset markets, general equilibrium, or international trade or finance, Plott faced the very fundamental question of how markets find solutions to the very complex systems for which they aggregate (and disseminate) information. Nevermind

finding one solution when there are several. This topic (which is clearly, and interestingly, related to the question of how exactly it is that markets aggregated information) led to various experimental attempts to understand the process of convergence, or price discovery (e.g., Johnson & Plott 1989; Plott & George 1992; Jamison & Plott 1997; Plott & Smith 1999; Plott 2000, all reprinted in Volume Two, part III). In sum, this work shows that "First, market prices can exhibit the type of instability predicted by classical dynamic models. Second, the conditions under which instability is observed are ... captured by models of the form developed by Marshall and Walras ... . Third, the appropriate model, Marshall or Walras, depends on the properties of the underlying demand and supply." (Plott, *forthcoming)*. Specifically, markets will not find, or will avoid, equilibria that are unstable. For an important recent extension of this work see Brewer et al. (2002).

Eleventh, while Plott rarely wrote explicit pieces about methodology (notable exceptions being Plott 1979, 1986a, 1987, 1991, 1996, 1996a, 1999), his oeuvre is pervaded by important methodological ruminations.[19] Indeed, three major themes run like a thread through many of the early papers. First, experiments are simple and special cases of the real world. General theories, then, ought to work in these cases. If they don't, they have failed. That the testing was done in a simple (but very real) world, is not an argument against experiments; rather it should lead to more experiments to understand the failure of the theory in the simplified reality. Second, to the extent that theories might not exist, experiments help to illuminate such terra incognita. Plott has labeled these two modus operandi the "theory-first" and "data-first" approach to experimental research. The data-first approach is of obvious importance in areas where the choice of the wrong institution (or institutional detail) could have dramatic consequences. The third major theme that shows up throughout Plott's oeuvre, and sometimes very bluntly so, is the argument that, if a theory fails an experimental test, it should not automatically be abandoned. Rather the questions then ought to become whether viable alternatives to the failed theory exist. If not, the failed theory should be maintained for the time being. This stance is the major thrust of Plott (1986a)[20] and also of a letter that he sent to *The Economist* in response to an ill-informed editorial about laboratory methods in experimental economics (Plott 1999a). Says Plott, arguing clearly against the growing influence of people parading alleged anomalies left and right, "Critics of the theories, scholars who relish in pointing out that the theories have been falsified, have produced absolutely nothing that will do a similar job. Much like the editorial, these scholars suppose the sole purpose of economics is to study individual choice. They ignore the fact that much of economics is about markets."

Twelfth, and closely related to the preceding point, Plott early on also explored the impact of the sharply diverging experimental practices of economists and psychologists (Grether & Plott 1979; 1982; see also Grether 1978). Specifically, together with David Grether he studied whether preference reversals that psychologists had observed — people indicating a preference for bets with a high probability of winning a relatively low prize, and placing a higher value on alternative bets with low probabilities of winning a relatively large prize were artifacts of psychologists' very different experimental practices such as their

(lack of) use of financial incentives and their (lack of an appreciation for the damaging effects of the) use of deception. To wit, "in order to give the results additional credibility, we felt that the experimental setting had to be removed from psychology." (Grether & Plott 1979, 629) To their own surprise, the preference reversal phenomenon survived their attempts to expurgate it. (More than a decade later Chu & Chu 1990 succeeded; see also Cox & Grether 1996 for another important study in this area.) Needless to say, Grether & Plott (1979) became highly influential and, quite ironically, seems to have legitimized the work of Kahneman and Tversky to economists. In Myagkov and Plott (1997; reading 20, Volume Three), two decades later, Plott took on the related issue of loss aversion and the prospect theory propagated by Kahneman and Tversky as an alternative to expected utility theory.[21] Myagkov and Plott confirmed key features of prospect theory (e.g., the asymmetry of gains and losses, the importance of reference points, and the occurrence of risk seeking in the loss domain) but they also suggested that these results, and implications of prospect theory such as "the endowment effect", may not survive experience (repetition). Indeed, in an intriguing recent working paper with Kathy Zeiler, Plott (Plott & Zeiler 2002) investigates the "The Willingness to Pay/Willingness to Accept Gap, the 'Endowment Effect' and Experimental Procedures for Eliciting Valuations" and manages to deconstruct the WTP/WTA gap -- one of the last biases or cognitive illusions that was not successfully questioned. The results of this paper, and of Myagkov & Plott (1997), provide evidence for the "discovered preference hypothesis" (Plott 1996a; reprinted as reading 19, Volume Three) which states that, while individuals have consistent preferences over states, they only become known to individuals with thought and experience. In a sense, this hypothesis rationalizes economists' typical practice of running subjects through a situation repeatedly because it is not clear what exactly it is that experimental subjects react to in an initial round (see also Hertwig & Ortmann 2001, 2001a, for a discussion of this and related methodological issues). It is also an implicit attack on the practices of those close to the heuristics-and-biases school about which more presently. Note the superb irony: someone thinking highly of the rational actor paradigm and economic theory, in his experimental work tries carefully to make sure that he does not pick up transient behavior stemming from subjects' neither fixed nor stable preferences when thrown into a laboratory situation entirely novel to them. In contrast, those who attack economists' alleged traditional treatment of decision makers' preferences as fixed and given, in their experimental work typically use one-shot games. With the exception of the recent Plott-Zeiler working paper, all other papers can be found in part 4 of Volume Three.

In sum, then, we see in the volumes reviewed here a body of work that in its breadth and depth, and in its impact both in economics and political science, as well as on the real world, is most impressive. The Economics Prize Committee of the Royal Swedish Academy of Sciences (2002) identified Smith's major successes as his contributions to market mechanisms (1.1.), tests of auction theory (1.2.), the use of the laboratory as a "wind tunnel" (1.3.), and experimental methodology (1.4.) Clearly, Plott has contributed his fair share to all of these, and then some. [22] The Economics Prize Committee of the Royal Swedish Academy of Sciences (2002) identified Kahneman 's major successes as

several regularities in choice under uncertainty, and argues that prospect theory goes a long way towards explaining these regularities (2.1. -2.3.) Clearly, Plott has done his fair share to debunk some of the results of the heuristics-and-biases program and he has asked important questions about the value of prospect theory. Roth (1995, p. 22) identifies as the various functions of experimental research the testing of theories (Plott's "theory-first" approach) and the provision of empirical facts that theory has not yet explained or even addressed (Plott's "data-first" approach). Experimental research may also "whisper into the ears of princes" (Plott's "design and testbed approach"); that is, help policy makers make good decisions. Plott clearly has contributed to all of these areas, and he has done so across a variety of domains. While his research, like that of Selten and Smith, has been "opportunistic in character" (Plott) in that it applied laboratory methodology "when an opportunity presented itself" (Plott), it has unfailingly, and dramatically, advanced our understanding not just of the science and art of experimentation in economics but about the real world. In fact, he has in various ways affected the real world outside of the laboratory, with tools originally created in the laboratory. The present volumes are a most impressive testament to his accomplishments.

How influential was Plott? Highly, if the number of ISI citations would be the measuring rod, 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 in a single award, presumably Plott would have shared the prize with Smith." (2002, p. 6)[23]

So, what went wrong? How could the Nobel Prize in Economic Sciences end up in the hands of Kahneman and Smith only? Warsh suggests, and he seems well-informed, that "this year's Nobel Award is almost certainly a compromise, designed to get both specialities [behavioral and experimental economics, AO] on the map." (Warsh 2002, 2)

What is clear is that the Nobel Prize committee thought that experimental research in economics and research on the psychology of economic decision making, while originally coming from rather different traditions, are approaching each other and are currently merging in a wave of research on behavioral and experimental economics.[24] This was the stated rationale of the 2001 Nobel Symposium, which brought together "some of the world's leading economists and psychologists for three days to discuss past, present and future research".

It is interesting to take a closer look at the list of participants (namely, presenters and discussants[25]) since its balance seems heavily tilted toward "behavioral economics", or more precisely followers of the heuristics-and-biases school of thought which, in contrast to earlier schools, has argued that the "apparently universal quirks in human decision judgement that routinely affect economic behavior" (Warsh 2002, p. 1) demonstrate that ordinary people, and even experts, are cognitive misers whose reasoning, judgement, and

decision making abilities are an embarrassment to the picture of human beings as rational actors. Indeed, throughout the advanced information, and also in derivative documents such as the "information for the public"[26] or the "presentation speech"[27] runs an undercurrent which suggests that behavioral and experimental economics have put to rest successfully "homo oeconomicus" - that self-interested and rational beast with which neo-classical economists have been so enamored. Proponents of what has become known as the heuristics-and-biases program (e.g., Tversky & Kahneman 1974; Kahneman & Tversky 1996) have suggested that mental short-cuts, or "heuristics," such as availability and representativeness (which are used to explain, among other things, base rate fallacy, conjunction fallacy, and belief in the law of small numbers) or anchoring and adjustment (which are used to explain the hindsight bias) were responsible for the alleged systematic departures from norms.

The heuristics-and-biases program has been the dominant paradigm in research on human reasoning, judgment, and decision making over the past few decades (Lopes 1991). In light of its rapidly growing acceptance among economists and other scholars, it is therefore interesting to note that the heuristics-and-biases program has been under attack for some time among psychologists (e.g., Christensen-Szalanski & Beach 1984; Lopes 1991; Gigerenzer 1991a, 1996; Cosmides & Tooby 1996; see for an eminently readable summary of this critique Gigerenzer, Hertwig, Hoffrage, & Sedlmair, *forthcoming*). These days, important parts of the heuristics-and-biases program such as the base-rate fallacy (e.g., Koehler 1996) or the overconfidence bias (e.g., Juslin, Winman, & Olsson 2000), or more generally the alleged importance of intrinsic motivations (e.g., Eisenberger & Cameron 1996; see also the discussion in Hertwig & Ortmann 2001, p. 396) are considered myths, at least in some quarters.

The critique has systematically challenged the notion of human decision makers as systematically flawed bumblers so successfully propagated by the heuristics-and-biases school. Drawing on notions of bounded rationality, the proponents of this view argue that humans have evolved surprisingly effective simple decision rules that in many contexts serve them well, and redefines what constitutes rationality by taking into account constraints on resources such as time, knowledge, and cognitive processing ability (e.g., Gigerenzer, Todd, & the ABC Research Group, 1999; see also Gigerenzer & Selten 2000; and Smith 2002c). In short, the reality of cognitive illusions (biases) and the usefulness of heuristics (and the explanatory power of concepts such as availability and representativeness or anchoring and adjustment) are very much contested. This is clearly shown by a very public debate between advocates of the heuristics-and-biases program and those in the ecological rationality program (e.g., Kahneman & Tversky 1996; Gigerenzer 1996; or the various contributions of Gigerenzer, Hertwig, and Hoffrage in Plott & Smith *forthcoming*); see also Smith (1991) whose assessment of the rational choice critique of the heuristics-and-biases school mirrors Plott (1986a) but is even more blunt in its dismissal of it.

I am, in any case, convinced that future researchers in the philosophy and sociology of science will have a feast in tracing how an entrenched program such as the heuristics-and-biases program 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, and often blissfully unaware of the Duhem-Quine thesis (Smith 2002; Soberg 2002), have taken the results of the advocates of the heuristics-and-biases program at face value. A good example is Rabin (1998) who, in his explicit dismissal of the Duhem-Quine hypothesis[28] and in his deliberate neglect of the controversies surrounding the heuristics-and-biases program, stands out.[29]

What has all this to do with Charles Plott? The volumes that I reviewed are a testament to someone who, while not married to one particular approach of experimentation, is guided, wherever possible, by theories of (rational) individual choice and markets. This modus operandi, far from being ideologically motivated, is the results of decades of outstanding experimental work. Starting with his early work on voting experiments all the way to his experiments on international trade and finance, Plott has found – over and over again – that economic theory organizes the data better than one has reason to expect. Better for sure than the various one-trick ponies that get continuously paraded in front of our very eyes in response to a continuing stream of (alleged) anomalies. Plott (1986) is a summary of sorts of this stance and also, in a sense, the closest that he ever came to formulating an explicit methodological program: "When tested directly, the theory (of rational individual choice) can be rejected. It is retained because neither an alternative theory nor an alternative general principle accomplishes so much." (S301) His work on individual choice persuasively illustrates the usefulness of this approach (as does his work in every other area that he has researched), an approach that seems to have become rather unfashionable in certain quarters.

It is notoriously difficult to make assessments about the influence of academic researchers, especially if they are pioneers. For that reason, the Economics Prize Committee of The Royal Swedish Academy of Sciences is an important, if not the most important, current writer of the history of thought. After sorting through the evidence, I conclude that the Committee has reconstructed the history of both "behavioral economics" and experimental economics sloppily and incorrectly. While every reconstruction is to some extent a rationalization that smoothes the course of history to construct a compelling narrative, the selection of "the odd couple" (BusinessWeek online October 11, 2002) 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, and empirical evidence, that I have sketched above and that have made, and continue to make, for some of the most promising recent advances in both economics and psychology.

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1. “Without any doubt, the main researcher in the experimental tradition is Vernon Smith. Smith not only made the most important early contributions, but also remained a key figure in the field to data. He has educated and collaborated with a larger number of younger researchers in experimental economics. The most prominent of these is Charles Plott, who has made important contributions to the field.” As I will argue below, this statement reflects inaccurately how things developed and is, without doubt, an assessment that can be disputed.

2. While the introduction is the same in all three volumes, the page numbering differs slightly. All references below are to the numbering of pages in Volume 1.

3. This paper was Plott's first methodology paper. It built on a paper delivered at a Resources for the Future conference in 1978, and Smith (1976).

4. This paper followed up on earlier work on the influence of agendas on final outcomes. It is remarkable for introducing communication into the experimental study (the effects of which were not predicted by any theory) and a design -- labeling of alternatives in a humorous manner -- that was designed to encourage verbal exchange but ended up inducing participants to ignore the payoffs. The result of this experiment led Plott to be suspicious to non-abstract settings for economics experiments (Plott 1986a, S304/5, especially footnote 3). This issue has recently become a topic of dispute (e.g., Ortmann & Gigerenzer 1997; Harrison & Rutstroem 2001).

5. As Plott describes it, "My introduction to the use of laboratory experimental methods in economics was in the late 1960's, 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 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." (Plott 2001, x)

6. Purdue was Smith's first teaching post and a place where he spent the overwhelming part of the years 1955 - 1967, partially overlapping with Charlie Plott who, after having graduated from the University of Virginia, started his career in Purdue in 1965. After a one-year stint as visiting professor at Stanford during the academic year 1968 - 1969, Plott moved in 1971 to the California Institute of Technology. It seems that Plott and Smith did not talk much, or with any consequence, experiment while being at Purdue. That was to come later. Smith (1991a) is a brief, and amusing, memoir of his early years at Purdue.

7. Incidentally, this is not the published version of Smith (1973) as the "advanced information" of the Nobel Prize Committee (EPC 2002) claims. That Cal Tech working paper was a review of the literature on how incentives affect behavior.

8. His curriculum documents Smith's impressive non-experimental output during these years: he had more than two dozen articles in top journals before he arrived at Stanford in 1972. Of those, only about half a dozen (including his 1962 and 1965 *JPE,* and 1964 and 1969*QJE* papers) were of an experimental nature and they all were finished well before they were published. Smith's next major work related to experiments was his now classic

paper on induced value theory (Smith 1976).

9. To judge from his various writings, Smith does not seem to have been aware of these activities.

10. As acknowledged by Plott, Smith, and also Roth (1995, pp. 20 - 21), Sauermann, Selten, and Tietz in Germany had explored independently, with considerable success and somewhat parallel to Smith's early attempts, the use of experiments in economics (Selten 1993, pp. 117-8) The acknowledgements, however, are perfunctory, hardly reflecting the considerable advances that the Frankfurt School of experimental economics made. But that's a topic for another occasion.

11. That undergraduate not only became co-author of important and influential articles (e.g., Miller, Plott, & Smith 1979, Miller & Plott 1985); he also recently published an insightful and deservedly acclaimed book that traces modern finance and modern institutions such as frequency auctions and derivatives back to those simple experiments at Cal Tech (Miller 2001).

12.As a matter of fact, many an article of Plott was published with undergraduate and graduate co-authors, as the acknowledgments to various articles, sometimes sadly (Hong), make clear.

13. Plott himself was an accomplished theorist in public economics who wrote classic papers such as "a notion of equilibrium and its possibility under majority rule" (Plott, *AER* 1967) – by far his most cited paper according to an ISI search –, "some organizational influences on on urban renewal decisions" (Plott, *AER* 1968), "the probability of a cyclical majority" (DeMeyer & Plott, *Econometrica* 1970), "a welfare function using 'relative intensity' of preference" (DeMeyer & Plott, *QJE* 1971), "path dependence, rationality and social choice" (Plott, *Econometrica* 1973), and "axiomatic social choice theory: an overview and interpretation" (Plott, *American Journal of Political Science* 1976)

14. The book is also of scientific interest because it provides the first experimental evidence that the bidding functions that are observed evolving in the excluded bid ("second price") auction constitute a Nash equilibrium and tend to be demand revealing.

15. It was, incidentally, Plott who came up with a measure of efficiency for experimental markets – the ratio of actual total surplus as reflected in the data to the total surplus predicted by the induced values for buyers and agents, or the percentage of realized gains from trade – that was first reported in Plott and Smith (1978) and is now the measuring rod of choice for economists trying to understand and evaluate institutional choice. As Plott says correctly, "efficiency is one of the pillars on which modern experimental methods in economics rests." (Plott 2001, xiv)

16. In fact, the *JEL* survey article (Plott 1982)was an outgrowth of a paper that Plott presented in 1980 at a FTC conference to which he had been invited because governmental economists were wondering "if laboratory methods could be usefully applied in the area of anti-trust." (Plott 2001, xxi)

17. Plott (1987) introduces dimensions of "parallelism" as follows: "experiments that provide the best insights about the nature of upcoming options might include faithful reproductions of the anticipated situation, but there is no reason to believe that good experiments necessarily take that form." (p. 217) Drawing on 10 attempts (in all of which he was involved) to use experimental research in actual policy decision making, he argued that experiments could be used for a for a variety of purposes such as shifts in the burden of proof, or demonstrations of the workability of a mechanism, or exploration of potential designs ("pre-policy research"). Some of this benefited from a faithful scaling down of naturally occurring environments, others didn't. External validity, where possible at all, was not always necessary.

18. Holt (2003) tells the following story about the real effects of one early agenda setting study. "Levine and Plott (1977) 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 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*." Levine and Plott (1978) developed through a series of laboratory experiments a theory of agenda influence on committee decisions.

19. In fact, pretty much every paper in these volumes contains methodological ruminations - clearly that is a reflection of the considerable hostility that pioneers such as Plott and Smith encountered in the profession (see Hertwig & Ortmann 2001, and Roth 1995 for similar arguments). Writing in the mid-nineties, after Plott rightly claimed that experimental economics had become an experimental science, Lopes (1994, p. 212) 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 Roth put it so memorably, "we've won the battle for the journals [but we have] not yet won the battle for the departments." (Warsh 2002, p. 7) Be that as it may, many of the experimental articles of giants like Plott, Smith, and Selten were published years after they were written (if at all) because editors and referees gave the pioneers of experimental economics a hard time. Both Plott (in his introduction to these

volumes footnotes 2 and 7) and Smith (in his "Autobiography: the Early Years to 1995" pp. 11 and 13) provide far-out stories about the difficulties of publishing the results of experiments back then. Needless to say, this state of the world has complicated the construction of my narrative.

20. Smith (1991) makes explicit reference to this paper and this argument.

21. See also his short but succinct dismissal of Kahneman's claims of the domain of applicability of prospect theory and the value of economic theorizing (Plott 1996, unfortunately not reprinted).

22. I note in passing that the Economics Prize Committee's assessment of Smith's work is wanting, too. For example, the major interest of Smith over the past few years – as clearly reflected in his Nobel Prize speech – in constructivist and ecological rationality in economics (that is, his work on dictator, ultimatum, and trust games, as well as his attempts to connect his work to insights from evolutionary psychology and neuroscience) gets short shrift in EPC (2002). Bergstrom (2002) provides a somewhat partial but clearly superior assessment of Smith's work.

23. My prediction for the Nobel Prize in Economic Sciences 2002 were Daniel Kahneman, Charles Plott, and Vernon Smith. Smith to me was an obvious choice. Plott, to me, was an obvious choice too; Kahneman less so. This may to some extent reflect my academic upbringing. I learned experimental economics at Texas A&M University in the late eighties from the "famously careful Ray Battalio" (C. Camerer 2003). Later I spent two years (1995/96 and 1999/2000) with the Center for Adaptive Behavior and Cognition (formerly of the Max Planck Institute for Psychological Research in Munich, and since 1999 of the Max Planck Institute for Human Development in Berlin). Its director, Gerd Gigerenzer, has been a prominent critic of the heuristics-and-biases program initiated by Kahneman and Tversky.

24. It's not at all clear to me where the Committee got this idea. I certainly don't see it except in certain areas such as reciprocity theories and experiments.

25. Camerer, Fehr, Kahneman, Laibson, Loewenstein, Plott, Rabin, Rachlin, Roth, Slovic, Smith, Thaler, and Akerlof, Benabou, Gilbert, Laffont, Manski, Mirrlees, Mullainathan, Palfrey, Rapoport, Rubinstein, Selten, Shiller, respectively.

26. See http://www.nobel.se/economics/laureates/2002/public.html

27.See http://www.nobel.se/economics/laureates/2002/presentation-speech.html

28. "I emphasize what psychologists and experimental economists have learned about

people, rather than *how* they have learned it. Consequently, the focus of this essay is not at all on experimental methods per se." (Rabin 1998, p. 12) Statements like that make every experimental economist worth her or his money shudder, at best.

29. Unfortunately, the Economics Prize Committee of the Royal Swedish Academy of Sciences has chosen to let itself be informed significantly by his work. See the citation patterns in EPC (2002).