Marketing Laboratory Experiments: The Resistible Rise of Laboratory Markets

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Thesis in Economics

Introduction

For the preponderance of undergraduate students of economics, the introduction to the epistemological foundations of the discipline begins and ends with the first several pages of their principles textbook. It consists of the perfunctory declaration that 'economics is an observational social science.' This might be followed by a discussion of the iterative relationship between theory and empirical work and the myriad of data sources on existing economics. It may even be followed by the casual observation that it would be 'unethical or cost prohibitive to perform any kind of experiment on an existing economy.' The outcome is that these students will join Jacob Viner's world in which "economics is what economists do," and unconsciously internalize the implicit epistemological assumptions of economics via osmosis.

It was into this state of affairs – mostly in the 1950s and 1960s – that 'experimental economics' in all its manifestations was born. It has subsequently been framed as a 'subdiscipline' consisting of three subsets: market experiments, individual choice/behavioral experiments, and game theoretic experiments.[1] Some framings would add public choice and institutional design experiments, while others would subsume these fields within one of the original three. Each of these subfields would initially have a great deal of difficulty gaining recognition within the broader economics discipline, and each has since enjoyed some degree of success.[2] This success would come to a head in 2002, when Vernon Smith – widely considered the founder of experimental economics – won the Nobel Prize in economics.

This paper will endeavor to develop a history of market experimentation. It will begin with a discussion of its earliest manifestations in the work of Edward Chamberlin, tracing its development through the dominance of Vernon Smith, Charles Plott and their students, and ending with its prospects for the future. It will pay particular attention to the iterative process by which market experimentation developed and gained presence in a changing disciplinary context. In the earlier period of market experimentation, spanning from Edward Chamberlin's work in the 1940s through the mid 1970s, the substantial research will lend itself to rather comprehensive analysis. However, from the mid 1970s on, the proliferation of market experimentation will require restricting the purview to only the most substantial developments.

This project will furthermore require abandoning the notion of experimental economics as a subdiscipline, which is motivated primarily by the uniqueness of laboratory methods in the economics discipline and not by any common research agenda, theoretical methodology, or epistemological grounding of the subfields. The concept of experimental economics as a 'subdiscipline' suffices for survey articles and general method textbooks, but it belies the substantial divide in origins, experimental method (particularly epistemology), research agenda, and significant results obtained that characterize

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[1] Davis, Douglas and Charles Holt. Experimental Economics. Princeton, NJ: Princeton University Press, (1992)
[2] Moscati, Ivan. "Early Experiments in Consumer Demand Theory: 1930-1970." *Economics Working Paper Archive EconWPA,* (2005) 20.

experimentation in economics. Moreover, it fails to capture the fundamentally different conflicts each form of experimentation has had with the economics discipline as a whole, and subsequently obfuscates this analysis. The concept of an experimental economics subdisicpline is therefore insufficient for the task of understanding the development of market experimentation and is subsequently refuted in the text of this paper.

Understanding the genesis of market experimentation in economics will require the selection of a model of disciplinary development. This model will need to pay particular attention to 'subdisciplines' or other discrete areas of common research interests and method. The most critical components of the theory will characterize the actual dynamics between the economics discipline as a whole and its research subdivisions. For this reason, the more grandiose formulations of disciplinary development put forward by Thomas Kuhn and Imre Lakatos will be instructive, predominantly in their failures to capture the experience of researchers working with experimental markets. More instructive formulations of disciplinary development will be found in recent work specifically tailored to economics, most notably David Colander, Richard Holt, and Barkley Rosser's The Changing Face of Economics.

This paper will argue that the development of market experimentation in economics followed a fundamentally different path than the development of other forms of experimentation in economics, most notably individual choice and behavioral experimentation. In addition, this paper will argue that market experimentation was motivated by a desire to better understand the basic principles that drive economic interactions, which can be more effectively isolated in a laboratory market than in a 'naturally occurring' market. This agenda grew from several key researchers' affinity for both economics and the methodology of the natural sciences, rather than any ideological take on the economics core or prevailing economic theory. This literature has furthermore developed into a rich body of research concerning the design of market institutions, motivated by a desire to employ economics to increase general welfare and to more generally increase the profile of market experimentation in economics.

This research faced resistance because of its incongruity with the core of economics. However, this incongruity can be understood to be epistemological rather than ideological or theoretical. This distinction is critical for two reasons. First, the epistemology and method of market experimentation – based on the natural sciences and a search for *universal* economic principles in a lab – are the most unique and defining characteristics of this form of experimentation. Second, the epistemology and apparent lack of utility of this research drove its difficulties with the economics discipline, not the significant conclusions of the research. This distinguishes the market experimentation research from, for example, the behavioral experimentation research that contradicts the rational actor model or expected utility theory. In short, to have Milton Friedman call the results of research "obvious"[3] poses a very different set of problems than if he were to call the conclusions, themselves, wrong.

[3] Plott, Charles. Interviewed by Paul Slattery on 09-21-07

The substantial contributions to economic understanding made by market experimentation will be noted throughout the paper. Particular attention will be paid to the way these contributions were motivated by their interaction with the broader economics discipline and to the way market experimentations' incorporation into the broader discipline was subsequently accelerated . Laboratory markets have contributed to economic understanding in two fundamentally different ways that can be associated with earlier and later market experimentation. This distinction is characterized by many exceptions, but it is instructive in understanding the intellectual genesis of the subfield. In its earlier period, market experimentation contributed to a better understanding of very basic, *universal* principles of economics – a claim that in and of itself poses questions about the nature of economics. These basic principles related primarily to the convergence of markets, the dynamics around market equilibrium, and the import of market institutions for both. In its more recent period, market experimentation has also applied its fundamental strengths in institution analysis to the often-lucrative task of institutional design.

These contributions have required a fundamental rethinking of the epistemology of market experimentation, which was necessary to understand both the import of results within the subfield and to render them communicable to the broader discipline. Early market experimentation was held back, in part, by the methodological prejudices of the economics discipline but also, in part, by the inability of the researchers to articulate the meaning and import of their results. Experimentation in economics was fundamentally new, and while this required the development of experimental techniques that would pass muster in the economics community, but more importantly it required an understanding of the meaning and applicability of results from experimental markets.

In response to a range of typical criticisms from the discipline as a whole, market experimenters have had to develop a substantial body of theory to explain the unique utility of their research. Rather than falsifying theories or attempting to simulate naturally occurring markets – two intuitive but flawed approaches for a discipline based on observation and statistical induction from field data – experimentalists have taken the tact that laboratory markets are real markets in the real world. The conclusion is that the universal principles operating in the very complex cases found in 'nature' – as is the rhetoric of experimental economics – should obtain in the controlled, simple markets created in a laboratory.

Experimental markets are therefore conceived of as a separate source of empirical data that may or may not support prevailing theories and may themselves drive the development of new theories. An experiment may be *informed* by general theory or naturally occurring markets, but its purpose is not to replicate the *conditions* of a theory or a naturally occurring market. Rather its purpose is to reveal, in a controlled setting, the fundamental economic principles in operation. It is rarely possible to use an experiment to get on the domain of abstract theory or to mimic the conditions of a naturally occurring market, but it is often possible to formulate an experiment that will – within the inherent strengths and admitted weaknesses of economic experimentation – indicate a great deal about the general economic principles operating within a theory or natural market.

This turn is out of sync with a discipline that interprets itself as a contextual social science rather than a natural science searching for universal principles. However, it has created the most appealing aspects of experimental work – particularly the ability to examine the relative efficiency and social impact of different market institutions in numerically comparable, replicable, and precise ways. It has facilitated the contributions of market experimentation to more general theories of economics, but more importantly, it has facilitated the contributions of market experimentation to regulation and institution design, which have raised the profile of and demonstrated the utility of laboratory markets.

This paper will conclude with an examination of the place of experimental markets in the discipline today and the future of market experimentation. It appears that institutional design and laboratory market experiments will likely become a permanent feature of the disciplinary landscape. In this process, experimentation will become simply another source of data, simply another empirical tool. This transformation is made possible by the fact that experimentation has epistemological rather than theoretical tensions with the economics discipline, and has therefore been able in the fullness of time to find its way into various applications by persuading the broader discipline of its analytical worth.

Outline

This paper will now turn to a brief explanation of the origins of experimentation in economics. It will proceed to the question of its own method, which is given to all the joys and problems of the contemporary history of science research. It will then turn to a set of theoretical formulations regarding the dynamics of the economics discipline, which will be necessary in organizing the progress of market experimentation though each will turn out to be insufficient in its own way. It will then trace the development of the subfield of market experimentation before finally turning to the question of its place in the present and future of the discipline.

The Origins of Economic Experimentation

‘Experimental economics’ can characterize any use of human subjects in a laboratory to perform research in economics. If experimental economics is conceived as a subdiscipline consisting of individual choice/behavioral, game theoretic, and market experiments, it is not at all clear where to begin the narrative. The origins of each of these forms of experimentation can be found in textbooks and some of the meager historical work on experimental economics. These descriptions will be provided first. However, as the interest of this paper is in subdisciplinary development, rather than exhaustive history, the logical point of departure could also be the research commonly perceived by modern practitioners as the earliest antecedent of their own work. This decision rule generates particularly instructive ambiguity.

One body of work, found in general textbooks, historical overviews, and behavioral writings, posits Louis Leon Thurstone’s 1931 paper in the *Journal of Social Psychology* as the beginning of experimental economics. Thurstone used surveys of women to determine the quantitative relationship between the utility of handbags and shoes. He attempted to develop indifference curves from this method and rejected the transitivity of preferences.[4] Apart from being thoroughly critiqued by Wallis and Friedman, as well as Stiglitz, the paper had a “negligible impact” on the development of demand theory.[5]

Charles Holt and Douglas Davis collaborated to produce comprehensive textbook entitled Experimental Economics in 1993. While Davis himself admits that the project would be nearly impossible now given the proliferation of experimentation in economics,[6] the description of the origins of experimentation is still instructive. The authors maintain that experimental economics can be divided into market, game-theoretic, and behavioral experiments. They subsequently divided the origins of economic experimentation into three categories.

The earliest manifestations of market experimentation are found in the work of Edward Chamberlin. Chamberlin, who would later teach Vernon Smith, became interested in economic experiments as a pedagogical tool in the classroom. However, he was intrigued by their results. In 1933, he published *The Theory of Monopolistic Competition (A Re-orientation of the Theory of Value)*, which was “motivated by the apparent failure of markets to perform adequately during the Depression.”[7] Chamberlin was persuaded that “certain predictions of his theories could be tested … in a simple market environment, using only graduate students as economic agents.”[8]

Chamberlin reported this first market experiment in 1948. It was methodologically problematic on a number of levels, and its methodological problems would form the basis for later research. Chamberlin induced values for buyers and sellers by dealing them out from a deck of cards. Sellers earned the difference between the face value of the dealt cards (cost) and their negotiated contract price. Buyers earned the difference between their dealt willingness to pay and their negotiated contract price. The essential feature of

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[4] Thurstone, Louis Leon. 1931. The Indifference Function. *Journal of Social Psychology* 2:139–67.
[5] Davis, Douglas and Charles Holt. Experimental Economics. Princeton, NJ: Princeton University Press, (1992) p.5.
[6] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[7] Davis, Douglas and Charles Holt. Experimental Economics. Princeton, NJ: Princeton University Press, (1992) 6
[8] ibid. 6

this market, it would turn out, was that it "was both unregulated and essentially unstructured."[9] This market design anticipated Vernon Smith's later research that uncovered the import of trading institutions in market convergence. It turned out that while this market was 'competitive,' it was completely inefficient. Chamberlin concluded from the market's failure to converge at an efficient price, that the market itself was inefficient. This conclusion was "initially ignored in the literature" and indeed "Chamberlin himself all but ignored" the result.[10]

Davis and Holt identify the origins of game-theoretic experimentation in the work of a collection of "psychologists, game-theorists, and business-school economists"[11] who performed experiments in the 1960s. These researchers were "inititally interested in behavior in the context of the well-known prisoners dilemma."[12] This work anticipated later work on oligopolistc competition by Heinz Sauerman and Reinhard Selten, as well as Sidney Siegel and Lawrence Fouraker, though its genesis is beyond the scope of this paper.[13]

The third set of origins for economic experimentation can be found in individual choice experiments in simple settings. These experiments were set up so that "strategic behavior is unnecessary and individuals need only to optimize."[14] The purpose of these experiments was to "evaluate [the] tenets of the basic theory of choice under uncertainty."[15] This worked generated substantial controversy as it often refuted the basic theory of choice under uncertainty that had been formulated by in the 1940s and 1950s.

The ambiguity in points of origin is instructive because it begins to reveal the divisions in the 'subdiscipline' of experimental economics. The behavioral and market researchers are pursuing fundamentally separate research agendas with fundamentally different antecedents. Robert Frank, a famous behavioral experimeter, recognizes Thurstone as a forerunner in interviews. Conversely, Vernon Smith, when prompted to discuss Thurstone, said "yes there were some choice experiments, but nobody was doing any market experiments until Chamberlin."[16] For Smith, Thurstone belongs to an entirely separate intellectual trajectory that is not pertinent to the development of his area of research.

Charles Plott, Smith's partner in the development of market experimentation, chooses the work of Frederick W. Taylor as his antecedent. In referring to his first contact with Vernon Smith's work, he says, "I saw that somehow he was dealing with the theory of Frederick W. Taylor."[17] Taylor, the father of scientific management, used experiments to examine pay rates and various other management decisions around the turn of the century. Plott added that he had spent some time studying Taylor, "so [he] knew that the

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[9] ibid 7
[10] ibid 7
[11] ibid 8
[12] ibid 8
[13] ibid 8
[14] ibid 8
[15] ibid 9
[16] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[17] Plott, Charles. Interviewed by Paul Slattery on 09-21-07

experimental methods were quite powerful in these areas, not based on psychology."[18] Plott, like Smith, has a significant amount of experience designing institutions for Auction houses and governments through the use of experiments, and he takes scientific management as the precursor to his form of experimentation. He, like Smith, is careful to rule out the individual choice experiments as intellectual antecedents to his work. In the case of Plott, there was nothing in the discussion to prompt the distinction.

In short, while general methodological surveys – including one written by Smith – recognize Thurstone, Smith and Plott choose their antecedents elsewhere. These divisions in the perception of intellectual history suggest the fundamental distinctions in research interests and preferred methodology that exist within the experimental economics 'subdiscipline.' The subfields that have been subsumed under this category are fundamentally not of the same intellectual trajectory, and this distinction is important to the practicing researchers.

Method

This paper addresses the development of market experimentation in economics from the 1950s until the present. This means that it is a relatively contemporary account of the history of science and is subject to all the attending advantages and problems. It is not grounded in any substantial preexistent literature and therefore stands to make a unique contribution to the history of economic thought, but as a result, it must be overly reliant on the meager extant literature and internalist histories.

This project is by no means a Whig history. The attribution of the Nobel Prize to Vernon Smith and Daniel Kahneman – to the exclusion of Charles Plott – has already generated several such contemporary accounts deeply invested in the allocation of credit for substantial research contributions to either Smith or Plott. Unfortunately, the difficulties attending the contemporaneous nature of this subject of study are compounded by this debate. Furthermore, the parsing of credit for the development of market experimentation between Smith and Plott is far less interesting for economics as a whole than the question of how Smith, Plott, and others were able to make space in the discipline for a relatively new and, at the time, methodologically suspect subfield.

This project will instead attempt to understand the substantial developments in market experimentation as they relate to the development of the subfield and its interactions with the larger discipline. This task will begin by examining the relevant literature on the history and sociology of economic thought, but the intent is for this to serve merely as a reference point. The purpose of this paper is not to evaluate the success or failure of models of disciplinary change, but instead to understand how a particularly interesting subfield was able to develop within the economics discipline by posing a series of epistemological questions. This change will be understood in part through the research produced by the subfield and in part by the recollections and characterizations of the researchers themselves. This method should be able to call upon the sociological and

[18] Plott, Charles. Interviewed by Paul Slattery on 09-21-07

informal knowledge of the imbedded researchers while keeping it in relation to the material the researchers produce.

This study presents a number of unique methodological opportunities and concerns. First, there is the problem of the small, internalist, and occasionally charged nature of the existent literature, which has already been noted. Second, and more pressingly, the development of much of the history of this subfield to date was driven by a remarkably small number of personalities, even given the relative novelty of the field. This study presents an opportunity to witness how a few charismatic leaders can champion the development of a subfield, but it may mean that the development of this subfield was as much characterized by the nature of these leaders as by a more general process of the development of subfields. The third related and crucial aspect of the methodology of this project is that the relatively few charismatic leaders that championed the development of market experimentation are still alive and available for interview.

This paper will make extensive use of this opportunity. Its primary motivating data is a set of interviews conducted by the author that are unique in their scope and duration. The interviewees include Vernon Smith, Charles Plott, and Douglas Davis. The interview of Smith took place in his (now former) office at George Mason University and lasted around two hours. The interview of Charles Plott took place at Caltech and spanned around four hours, including a tour of the experimental economics facilities at Caltech, which are depicted in the pictures in this document. The interview of Douglas Davis took place in his office at Virginia Commonwealth University and lasted around an hour and a half. The timeframe for each of these interviews was not stipulated beforehand, and in each case the date of the interview was freely selected by the interviewee to maximize the chance that it could be afforded their full attention. The interviews lasted as long as the interviewee wanted them to last, and in each case, the interview method was casual and dynamic.

The questions asked of the interviewees were not standardized but instead tailored to the particular experiences and perspective of each interviewee. The questions were furthermore only partially determined beforehand, and in many cases, were modified substantially throughout the discussion. The questions were not confined to the experiences of the individual but, in many cases, took advantage of the individual's perspective on the subfield as a whole. The interviewers hypotheses about the nature and history of market experimentation were often shared with interviewees to give them opportunity to comment. There was never an attempt on the part of the interviewer to truncate the response of an interviewee. In all instances, the tone and character of the interview were designed to ensure the maximum engagement and comfort of the interviewee to encourage the sharing of internalist knowledge of the subfield.

The interviewees were selected according to different criteria and contribute different perspectives to the study. Vernon Smith has become the figurehead of market experimentation and was selected for his intimate knowledge of both the earliest moments and foundational studies of the subfield. Charles Plott was selected for similar reasons, though he brings the unique perspective of having been the leader of Caltech's

experimental economics program – arguably the premiere experimental economics program – since its inception. Plott and Smith occupy, in some senses, very similar positions in the subfield. They are considered the two founders of the subfield. Much of their research has been collaborative. As the subfield developed, each began to publish surveys of the literature and methodological documents. They are, in essence, the charismatic leaders of the subfield and should not be taken as representative practitioners of market experimentation.

Douglas Davis was selected according to different criteria. As his quotes suggest, he is a member of a younger generation of experimental economists who came to the subfield when it was relatively established. In contradistinction to Smith and Plott, he refers to himself as being "in the trenches."[19] In addition to his distinct perspective on the field in relation to Smith and Plott, Davis is the coauthor of the aforementioned Experimental Economics, along with Charles Holt. It was felt that, having participated in the ambitious project of incorporating much of experimentation in economics into one textbook, Davis would be able to comment on the makeup and unity or disunity of the 'subdiscipline' today, and indeed, he did.[20]

The position of each interviewee within the field poses unique questions for the interpretation of their statements. Plott and Smith, in characterizing the most significant moments in the subfield's development were often speaking about their own work. Furthermore, all of the interviewees were familiar with each other. The sequence of the interviews was: Smith, Plott, and Davis. As a result, Plott had the opportunity to and did ask about Smith's characterizations of events. Davis had the opportunity to and did ask about both Smith and Plott's characterizations of events.

For the reader's reference, the transcripts of each of these interviews have been provided in the appendix to this document. The interviews were recorded in real-time with the knowledge of the interviewee, and the recordings were subsequently transcribed. transcribed.

[19] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[20] ibid.

The History of Science

This history, in attempting to understand the development of market experimentation as a subfield within economics, will require both a theory of disciplinary development and a theory of the dynamics of subfields within a discipline. The purpose of this history is not to evaluate the validity of each of these models of the history of science or economics. Instead, each theory will be examined in turn for the degree to which it is useful or not useful in understanding the particular development of market experimentation. The more grandiose theories of disciplinary progress espoused by Thomas Kuhn and Imre Lakatos are not nearly as helpful as the more micro level analysis provided by Joseph Remenyi and, in particular, Colander, Holt, and Rosser. However, it is instructive for an understanding of the nature of economic experimentation to quickly develop the failings of Kuhn and Lakatos' models.

Kuhn is not particularly helpful for the project of this paper, and it is useful to understand why. Kuhn claims that disciplines do not change by the process of linear accumulation of knowledge. Instead, he claims that disciplines undergo scientific revolutions or "paradigm shifts." A discipline is initiated by a period of prescience, during which there is not a central paradigm for research. As a central paradigm develops, the discipline moves into a period of "normal science." This period is interrupted when a new paradigm capable of incorporating encountered anomalies into a new central paradigm causes a crisis and subsumes the old paradigm. A key component of the theory is that paradigms are incommensurable. Paradigms are not combined but instead battle for dominance of the discipline until one wins out.[21]

This framework is not particularly useful for understanding the development of experimentation in economics because it takes place on far too grand of a scale. It furthermore presents an inappropriate binary choice between paradigms. An argument could be made that attempts by behavioral experimenters to reject expected utility theory and the rational actor model might eventually lead to an alternative paradigm. The methodology of behavioral experimenters and economists does not seem to be commensurate. When asked about the relationship of behavioral experimentation to market experimentation, Douglas Davis noted that economists and social psychologists "speak a different language" and have difficultly even communicating on a basic level.[22] This would seem in line with the incommensurable nature of paradigms.

However, even in the case of behavioral experiments, Kuhn's model of a paradigm shift is not particularly helpful. The level of analysis taking place in experimental economics is simply not of the scope and potency to pose a real threat to the existing research paradigm. Falsification of descriptive theories under controlled circumstances is not sufficient to found an entirely new scientific program. When asked about behavioral experiments, Plott conceded that "the representative actor model probably isn't going to work if you test it." The issue for Plott, and really the issue for any application of Kuhn to behavioral experiments, is the lack of an alternative: "right now, I guess I cannot give

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[22] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007

you a principle that holds up out of that body of material."[23] Even when the behavioral experiments move beyond theory falsification – such as those involving prospect theory and preferences for fairness – the modifications do not seem to constitute a paradigm that first, compelling competes with the economics paradigm and second, is sufficiently disparate from the economics paradigm to fail to communicate with it. In fact, Matt Rabin, a prominent behavioral economist, characterized his view of the role of behavioral economics far differently: "I want to create better models of people and plug those better models into economics."[24]

Kuhn's model seems even less appropriate when applied to market experimentation. Far from a different paradigm incommensurate with the economics paradigm, market experiments seek to better understand the theoretical principles that are often already described by neoclassical economics. In fact, as will be seen, most of the results of market experimentation confirm neoclassical postulates. A misunderstanding of the intent of experiments might lead one to argue that experimentation is an alternative paradigm because of its methodology. However, as Plott articulates, "experimentation is a method that is sometimes useful and sometimes not. The fact that experiments cannot answer all questions does not mean that there are no questions at all that experiments can answer."[25] Experiments are not meant to replace other empirical methods but, in fact, to supplement them. In Plott's mind, "one can just as easily turn the discussion around by posing questions that can only be answered by application of experimental methods."[26] Market experimentation is meant to fill a gap in empirical technique left by the analysis of field data. It is an empirical tool just as econometrics is an empirical tool, and is not intended to replace any existing method.

In fact, the only way in which Kuhn is instructive is that both Douglas Davis – a market experimenter who focuses on posted-price institutions – and Kenneth Binmore – an experimental game theorist who has done work with institutional design – offer up that they are practicing "normal science" without prompting.[27] Experimentation aspires to be a functional methodology within the economics discipline, particularly in the case of game theoretic or market experimentation. It is meant to be Kuhn's normal science rather than his paradigm shift.

Imre Lakatos provides a more useful framework, though it still requires heavy modification for the purposes of this project as it is without a theory of subfields. In an interesting turn, Vernon Smith actually offered up that he felt Lakatos much more clearly articulated his experience than Kuhn had: "Lakatos is one. I think he's far better than Kuhn. I think for operating scientists to be able to relate to what's being talked about Lakatos is very good."[28]

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[23] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[24] Colander, David, Richard Holt and Barkely Rosser. The Changing Face of Economics. Ann Arbor: University of Michigan Press (2006) 151
[25] Plott, Charles. Interviewed by Paul Slattery on 09-21-07 by parkin
[26] ibid.
[27] Davis and binmore Colander, David, Richard Holt and Barkely Rosser. The Changing Face of Economics. Ann Arbor: University of Michigan Press (2006) 64
[28] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007

Lakatos believed each discipline had a Scientific Research Program characterized by a 'hard core.' Within this hard core could be found irrefutable articles of faith. He argued that these "irrefutable articles of faith are buttressed in the [Scientific Research Program] by methodological rules, the positive and the negative heuristics, the sum of which forms the *hard core* of the SRP."[29] The positive and negative heuristics function as 'dos and don'ts' for the practitioners of a field, both laying out the research methodology for the field and setting some hard boundaries for that research. For change to occur, a competing SRP must arise outside of the discipline and overcome the extant SRP based on its capacity to explain phenomena.[30]

There are a number of appealing aspects of Lakatos' approach to disciplinary organization and progress. First, it anticipates the distinction between behavioral experiments and market experiments. Behavioral experiments emerged outside of the discipline in social psychology and psychology. They were performed according to the methodological heuristics of psychology, and the Wallis and Friedman paper, as well as the Stiglitz paper, attacked them using references to the methodological heuristics of the economics discipline. As will be discussed later, Plott and Grether wrote an important paper confirming preferences reversals that actually takes the same form: criticizing the behavioral experiments based on the use of methodology from psychology that is inconsistent with the methodology of economics.

Second, the attention to the place of methodology in Lakotos is helpful for this paper. As noted above, the claim is that market experiments actually buttressed the 'irrefutable articles of faith.' While the focus on institutions and their importance in producing convergence was not previously well specified in the core, the institutional emphasis constitutes an addition to rather than conflict with the core articles of faith. However, the market researchers violated methodological heuristics in two senses. First, they performed research in the hard core in the sense that they examined the convergence of markets, which is 'obvious' – to quote Milton Friedman again – from a neoclassical perspective. Second, more basically, they performed experiments in a discipline that did not use experiments, which posed an epistemological question jeopardizing many of the positive and negative heuristics.

There are, however, difficulties with Lakatos. Behavioral economics might constitute an SRP that is somewhat in competition with the prevailing economics SRP. It does come from outside of the discipline. However, just as in the case of a 'paradigm' for Kuhn, it seems neither to be a fully functional alternative to the economics SRP nor in sufficient conflict with the SRP to bring about a 'switch.' The arguments that market experimentation does not constitute a fully separate and competing SRP parallel the arguments that it does not constitute a Kuhnian 'paradigm.' Both Kuhn and Lakatos' theories operate on far too grand of a scale to really capture the process of exchange between market experimentation and the broader economics discipline. However, a modification of Lakatos theory can make it more relevant.

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[29] Remenyi, Joseph. "Core demi-core interaction: toward a general theory of disciplinary and subdisciplinary growth." *History of Political Economy*, (1979) pp.31-32.
[30] Remenyi, Joseph. "Core demi-core interaction: toward a general theory of disciplinary and subdisciplinary growth." *History of Political Economy*, (1979) pp.31-32.

Subdisciplines

Joseph Remenyi, in an article entitled "Core demi-core interaction: toward a general theory of disciplinary and subdisciplinary growth," provides extensions of Lakatos that are meant to extend his framework to subdisciplines. Remenyi divides the discipline into a core, a theory protective belt, and a applied theory protective belt. He argues that subdisciplines can be similarly divided into a demi-core and protective belt. By analogy, "*the demi-core is to the subdiscipline what the hard core is to the SRP*."[31] The contents of the demi-core are not specified much beyond this statement.

Subdisciplines, for Remenyi, can be mapped onto the discipline based upon the content of their demi-core and research agenda. Subdisciplines with demi-cores from outside of the discipline that do research within the discipline are mapped with the demi-core outside the discipline and the protective belt crossing into the discipline. Other subdisciplines are mapped within the theory or applied theory protective belts.

This modification of Lakatos is partially motivated by the complaint that Lakatos' model "not provide sufficient flexibility, for it denies the possibility of progress through internal conflict; anomalies and hard-core-rejecting or –conflicting theories are simply ignored; a successful (and revolutionary) competing SRP must come from outside the profession."[32] This approach is far more helpful than the more grandiose approaches. While it still retains the SRP framework, it allows for contributions to and modifications of the dominant SRP that arise naturally in subdisciplines. Moreover, it suggests how the behavioral, game theoretic, and market experimental work might be envisioned.

Remenyi, however, is incomplete without a definition of the demi-core. If the objective is to understand the development of a subdiscipline, demi-cores cannot be understood purely by analogy to the core. Remenyi believed that a demi-core did need not be as strict as the core of a discipline. It could accept some or all of the disciplinary core, or it could potentially reject some of the core's postulates while accepting others. This notion of a demi-core is a useful framework within which to understand the market research as its heterodox methodology did not and does not preclude its researchers from claiming allegiance to portions of the neoclassical core.

The demi-core had some specific content for Remenyi. It was to contain within a subdisciplinary research agenda comprised of similar 'articles of faith' to those found in the actual core. These articles of faith may be partially determined by the discipline's articles of faith, but they may be also be unique to the subdiscipline. As will be seen later, the belief in market convergence espoused by many market experimenters requires far fewer preconditions than would be suggested by neoclassical theory.

The demi-core of the subdiscipline is also meant to contain its research methodology. This is crucial for the development of market experimentation as its unity is essentially

[31] Ibid, 33.
[32] Ibid, 40.

only to be found in its odd methodological program. Unlike many subdisciplines, its methodology was not by and large borrowed from the core but was essentially unique and in conflict with the methodology of the core. As the discipline developed, it clearly developed a set of methodological assumptions and an epistemological grounding that would seem to belong in the demi-core. Moreover, it developed best research practices that were essentially technical in nature. The concept of a demi-core provides a place in which to locate this unique epistemology and the body of practical knowledge developed in the practice of experimentation.

Subdisciplinary Dynamics

Understanding the position of market experimentation within the broader framework of the discipline is insufficient. The relationship between the subfield and the discipline was clearly dynamic and motivated alternations in both, though most notably the subfield. Remenyi provides some theory to deal with this question.

Remenyi claimed that the dynamic interchange between the core of a discipline and a subdiscipline took place within a particular disciplinary context. This disciplinary context is defined by the nature of the field, the field's technical and institutional heuristics, the core-threatening anomalies uncovered by the subfield, and the exogeniety or endogeneity of the demi-core.

The actual dynamics between a subfield and the discipline occur when the subfield uncovers a core-threatening anomaly. This anomaly could be responded to with one of two defensive processes. These defensive processes were set in motion because of the oversight principle, which demands that practitioners from a given subfield are "aware of (i) all developments within their own specialized areas(s) and (ii) developments in at least one other specialty within the discipline."[33] When reports of a core-threatening anomaly are uncovered by a practitioner of the Scientific Research Program, this practitioner responds to try to neutralize the claim. This occurs through either progressive or degenerative problem shifts.

The sent of intellectual responses to a core-threatening anomaly are referred to as 'errant hypothesis' responses. These responses are of two forms. Progressive problem shifts are legitimate changes in theory or critiques of method that lead to the improvement of the SRP. Degenerative problem shifts are purely ad hoc defenses that fail to actually address the core-threatening anomaly. They are indicative of the stagnation of an SRP and its impending failure.

A discipline may also respond to core-threatening anomalies with an institutional response. Institutional responses are essentially sociological in nature. For the researcher positing a core threatening anomaly, the include difficulties getting hired, getting funding, or publishing papers.

[33] Ibid, 35.

Remenyi's approach here is till rather myopic, despite the improvements he has made on Lakatos for the purposes of this paper. The discipline is itself dynamic, and the subdiscipline itself is dynamic. There is no reason to suspect that an interaction between the two entities cannot produce a mutual reformulation of both over time, and indeed that is exactly what seems to have happened in the instance of market experimentations.

Sociological Approach

Colander, Holt, and Rosser, in a book entitled The Changing Face of Economics, interviewed a number of researchers who they defined as being on the 'edge of economics.' The introductory essay provides an approach to disciplinary history that is not based on model formulation – as the previously discussed theories are – but instead on a sociological understanding of the discipline as a reflection of disciplinary culture and the basic tenets of human nature. This model permits both the discipline and the subdiscipline to change over time without any grand conflict.

Their interpretation of the makeup of the discipline and its resistance to change seems to more in line with the way in which individuals interrelate within a discipline and the experiences described by the market experimenters. Rather than an elaborate system of practitioners defending a scientific research paradigm through institutional and intellectual defense mechanisms, they posit that the mainstream individuals in a discipline ""become fixed in their ways of looking at things and often reject alternative views without giving them serious consideration." This phenomenon is simply "a part of human nature."[34] Moreover, their concept of the views of the mainstream members of the profession is not nearly as constricted as Remnyi's: "at any point in time, and especially by the time the term becomes generally used, a large pat of the mainstream profession disagrees with important dimensions of what is then thought of as orthodoxy."[35] Finally, it is often the case that researchers will find particular arguments interesting or worth reading without incorporating them into their work or their view of the discipline. [36]

They base their dynamic analysis on the divides between those who are mainstream and nonmainstream and those who are heterodox and orthodox.[37] The individuals with the most control over the discipline and those would be likely to offer up something like an institutional defense mechanism are the elites. These elites, for the authors, are the individuals in a discipline who have made substantial contributions to the research in the past and therefore have substantial control over institutions and leading journals.[38] Disciplinary progress occurs as the elites change and accept new theories and practices.[39] This process leads to small changes over time that are not recognized immediately. These changes come from within the discipline, and in fact, most of the force driving change for the authors comes from within the discipline.[40]

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[34] Colander, David, Richard Holt and Barkely Rosser. The Changing Face of Economics. Ann Arbor: University of Michigan Press (2006) 11
[35] ibid. 8
[36] ibid. 3
[37] ibid. 6
[38] ibid. 10
[39] ibid. 4
[40] ibid. 5

Colander, Holt and Rosser's view of subdisciplines is subsumed under the framework of mainstream vs. nonmainstream and orthodox vs. heterodox. They believe that there is a category of economists operating on the edge of economics but still pertinent to it. In their discussion, it becomes clear that "the edge is where the action is in the profession."[41] The question of whether an invidual on the edge falls into the heterodox or mainstream category is "primarily a matter of the individual's proclivity to fit within the existing mainstream and the degree to which he or she directly attacks, rather than softly criticizes, the work of the elite."[42]

Those who are distant enough from the elite become heterodox. The most common way to do this is through methodology. The authors claim that "it is because of their method, not their ideas, that most heterodox find themselves defined outside the field by the elite."[43] Heterodox researchers are typified by a hostility to orthodoxy. In act, "often the fundamental intellectual content of a heterodox school is rejection of orthodoxy, or at least major elements of orthodoxy."[44]

Being genuinely heterodox has serious consequences for the researcher in question. Working on the edge can be problematic, "especially for those whose proclivity is toward attacking, rather than working within, the existing field and hence finding themselves in heterodoxy."[45] This individual will likely have trouble getting funded by the NSF and receiving other benefits given to more mainstream and even nonmainstream members of the profession.[46] Oftentimes, they will have trouble "gaining funding for their work, and they will likely be squeezed out of the desion-making process at their universities."[47]

Being nonmainstream, however, is a different experience from being heterodox. Nonmainstream critiques of orthodoxy are typically much more accepted by the orthodox. In fact, their critiques can often be worked into the existing theory. Nonmainstream groups at the edge are typified by the fact that they generate their own theory and ideas as well as attacking the orhodox. They often are given to highly complex model building or other methodological convergences with the orthodox because it assists in their acceptance by the orthodox.[48]

Dynamics Within the Subdiscipline

It would also seem to be the case, however, that the subdsicipline itself must be capable of changing, and this is not articulated by any of the theorists. As soon as the subdiscipline comes into conflict with the broader discipline, it is conceived of as a

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[41] ibid. 14
[42] ibid. 14
[43] ibid. 11
[44] ibid. 9
[45] ibid. 14
[46] ibid. 9
[47] ibid. 14
[48] ibid. 17

homogenous and stable entity, but as the history of market experiments will demonstrate, this need not be the case at all.

Remenyi describes a kind of ‘bravado impulse’ that encourages the practitioners within a discipline to come to bold new conclusions provided they do so within the confines of the methodological heuristics, but this impulse would seem to obtain equally well for individuals outside of the orthodox or mainstream within the confines of the methodological prejudices of the subfield.

Moreover, if the heuristics of the discipline are capable of changing over time, it would seem that the heuristics of the subfield must be as well. Throughout the course of experimental economics’ development, it has been subject to a broad range of criticisms from the outside. These criticisms have motivated a great deal of change within the subfield.

These changes have furthermore taken place on a number of grounds. Market experimentation has changed its method in the course of its own methodological development and in response to outside criticism. It has furthermore changed its framing of the value of its research to more clearly explain the ways in which it can be useful. It has, in fact, developed an entirely new epistemological foundation upon which to ground its research.

Before taking on the significant historical developments in market experimentation, it will be useful to have a sense of what the subfield has become today. Importantly, in understanding the nature of experimental markets work today.

The creation of experimental markets has gone through a significant intellectual genesis. It originally faced an epistemological problem in that it had to define the nature of what exactly it was that it was doing. This is not a retrospective construction, but in fact the way Charles Plott characterized the original problem.[49] Experimentation was an entirely new methodology in economics. It constituted a new form of data collection that creates an entirely new relationship between theory and data. In fact, the fact that textbooks always provide a sort of handwaiving argument that economics is not an experimental science means, in a way, that the dilemma of forging this new epistemology was a deeper confrontation with the core because it confronted a basically submerged set of assumptions within economics. It challenges fundamental assumptions about the nature of the core postulates by making them universal and essentially existing principles.

Plott, when asked about the mission or purpose of economics, was emphatic that it is a science as opposed to a social science. It's purpose is the search for universal principles. He cited Alfred Marshall's contention that "that economics is about the way people go about the ordinary business of life"[50] and proceeded to disagree with it. He claims that economics is about "he principles that operate, the laws that operate with people." This search for principles takes economics "out of the study of history and the study of economies as they are found growing in the wild, and puts it on the level of trying to decide what principles are operating when you watch these things."

This is not simply a perspective on the nature of economics. This is the necessary perspective to justify experimentation, and it has been conceived of over the course of justifying experimentation in economics. To Plott, "that's profoundly important because if you are supposed to study economies in the wild, there's no place for an experiment."[51] On the other hand, "if you're studying the principles, then it could be the best place to study the principles are in economies that look absolutely nothing like the ones that nature created."[52] He goes on to assert that "whole schools have it wrong, like the University of Chicago and other places, when they say you're supposed and study these field [?]. I couldn't care less about that."[53]

This is a challenge to the core of economics that is not operating on the level of core postulates but instead reformulating what core postulates might mean. Practitioners within a discipline may take certain core principles on faith that then are challenged outside of the discipline. Plott is insisting on something fundamentally different, which is that there are universal laws of economics. This is a fundamental question about the epistemology of the field, about the nature of its core postulates. This is the kind of

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[50] Plott, Charles. Interviewed by Paul Slattery on 09-21-07

[51] ibid.

[52] ibid.

[53] ibid.

challenge that could only come from within the discipline as it calls for a much deeper revision of the discipline than a challenge to a core postulate might.

This challenge to the foundations of economics has been one of the most important components of experimental economics and one of its most important contributions it has made to the field of economics. It has come to economics and said "here's my methodology; it has these obvious warts right on its face. Let's talk methodology, practical methodology. Look at the assumptions associated with the data, with your results, your empirical analysis."[54] This has had any number of positive effects on the discipline as a whole, but it has "clearly has lead to better methods of collecting data … We see much better data collected. People are much more inventive about collecting data in the lab and out of the lab. As a consequence, I think we get better information."[55] It has forced economics to methodologically reconsider itself.

This epistemology had to be established against the background of all of the assumptions practitioners of economics had brought to the table about experimental economics. There is a natural inclination to assume that the utility of an experiment is simulating the contingent phenomena found in nature and learning things that can be directly applied to natural markets. Plott makes clear that this is not the case. He claims that "simulating markets is a dead end, completely dead end."[56] It is simply the "wrong way of thinking about experiments, and it's the wrong way of thinking about science."[57] He contrasts this approach with the search for principles which can then be incorporated into theory:

> I think the concept of simulation is wrong epistemologically. That's not the way we learn. People who follow a kind of simulation think "I want to do an experiment and make it mirror the real world as much as possible because then I want to take the results and apply it there." That's not the way it works. What happens is, you do the experiment, you learn about the principles, you develop a theory, and then you apply the theory. It's the theory that takes you from the data to these more complex things, not the experiment. I think that people don't really appreciate that indirect way in which science works.[58]

There was similarly a natural inclination to presume that the fundamental value of experimental economics was the testing of theories of human behavior. This approach was rejected by all three of the economists interviewed. Plott bluntly declared "a lot of the behavioral research is just negative. It just says, 'do people optimize?' Well, the answer is no."[59] Davis was yet more blunt. He shared the sense "that theory falsification is not helpful."[60] It does not, for Davis, have a great deal of utility. If it is necessary for models to "start from specific assumptions about the way everybody behaves in every

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[54] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[55] ibid.
[56] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[57] ibid.
[58] ibid.
[59] ibid.
[60] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007

circumstance, how do we abstract from it?" [61] In fact, Davis characterized this approach as "doing bad psychology."

Theory falsification is particular difficult because its nearly impossible to truly get on the domain of a theory and it can almost always be falsified. Plott asserts: "I don't like the theory falsification, I think that's another dead end. All the theories we use are false. Falsifying a theory is child's play. There's nothing to that."[62] Davis asserts that "It's fine. It's just not very much fun. It's not very informative to shoot down theories all the time. The question for theory is can you organize some behaviors. Theories are…I don't know what it was like in your first principles class, but theories are always – well they're not wrong – they're oversimplifications."[63] The competitive model serves as Plott's example: "the competitive market is untestable" because "it assumes that there is an infinite number of people and everybody is a price taker."[64] The interesting element for Plott is not to be found in the theory falsification but in the fact that even without all of the assumptions imposed by theory, a laboratory market will tend to converge. Plott is less interested in theory falsification in the lab and instead focuses on a competing system of models between which experimentation can help you differentiate.

The role of the laboratory is also differently understood by experimental economists than it previously was. It has been conceptualized as a part of the real world and therefore a place in which the real world universal principles of markets obtain. An experimental market is meant to be a real market; it is simply one that can be more thoroughly controlled than another market. What this formulation has done "is contributed really basic discoveries about the way things work."[65] Once inside the laboratory, "we're actually able to see things operating and understand them in ways we were never able to in history, see phenomena that no one ever saw before and understood before."[66]

This fundamental capability of the lab requires a whole new method that is distinct from theory falsification and from market simulation and is based on experimental knowledge. There is an entirely new relationship between theory and data mediate through the experiment. In a lab, for Davis, "you try and test a theory and it's never going to be so clean that you can say this theory is rejected and you should discard it or you can come up with another theory that's going to explain the behavior, but is often the case that your thought for how could I address, that dialogue between the theorist and the person who collects the data…experimentalists can suggest alternative ways to do the theory or better tools. The tools may end up allowing you insights into other areas all together."[67] Smith shares this perspective on laboratory methods. Laboratory technique for him is a value in of itself. It is the "body of experimental knowledge – human capital – that is connected with every science that is in the experimentation side of it that has a life of it's own separate from the theory and what the theorists do."[68]

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[61] ibid.
[62] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[63] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[64] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[65] ibid.
[66] ibid.
[67] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[68] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007

This is certainly a unique methodological approach, but it is, to the experimenters at least, not at all incommensurate with the neoclassical school of thought. Plott argues that experimental economics is well within a long tradition of the analysis of the convergence of markets:

> The models we are using are neoclassical. The law of supply and demand is pretty old. Nash equilibrium comes basically from Cournot in 1825. The relationship between institutions and economics is really old. General equilibrium comes from the physiocrats before Adam Smith. So there's this long history of asking, at an abstract level, … certainly Walras tells us about general equilibrium, gives us a notion of general equilibrium.[69]

Davis refers to himself as "a mainstream industrial organization economist who uses experiments."[70] He feels compelled to recognize that "other people are different but I see myself more in the mainstream."[71]

Smith characterizes the relationship to the neoclassical school slightly differently in that he feels that the analysis of institutions actually does make experimental markets work unique from the neoclassical school, but he claims that it is merely a return to a much older commitment of economics. He says "there's nothing in economics about – they don't treat these trading institutions"[72] not addressing these trading institutions, however, is to "not deal with the problem of the connection between equilibrium theory and these institutions that people trade by around the world."[73] This imperative of economics, which Smith traces back to classical economics, has somehow been lost.

In Market experiments, however, economics recovers this commitment to equilibrium analysis, institutions, and the search for principles. Plott marvels that "the accuracy of the demand and supply model is an amazing fact of life."[74] He believes that "through the application of experimental methods we can now understand features of its operation and how its operation can be influenced by institutions."[75] He goes on to claim that these principles of economics "lead to an understanding of the complex world around us better than any other branch of science or philosophy."[76]

Experiments furthermore get us closer to things that we genuinely don't understand, particularly the convergence of markets. It has uncovered that for market convergence to obtain, people "didn't have to have to have complete information; they didn't have to be sophisticated."[77] In fact, it is the institutions that matter for market convergence, as has been demonstrated over and over by experimentation. In Smith's first market experiments

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[69] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[70] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[71] ibid.
[72] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[73] ibid.
[74] parkin interview of plot in 03
[75] ibid.
[76] ibid.
[77] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007

"it turned out that these trading institutions, like the oral outcry double auction, were incredibly fast at discovering the equilibrium prices and people didn't have to be sophisticated or know anything about economics."[78] The market experiments demonstrated that the neoclassical assumptions for equilibrium were not at all necessary but instead a focus on institutions.

Disciplinary Context

All of the characteristics of experimental markets work described above did not come into being on their own. They came into being within a particular disciplinary context. This context was the neoclassical school of economics that considers "the central core of economics to be the theory of equilibrium, based on the optimizing behavior of fully rational and well-informed individuals in a static context."[79] Moreover, "perhaps the most important characteristic of the neoclassical orthodoxy is that axiomatic deduction is the preferred methodological approach." [80] This approach is certainly incommensurate with laboratory methods, but by the time of the rise of experimental markets, the neoclassical school had started to wane in significance.

Sill, the use of experimental methods in economics faced a lot of resistance, and it was, in fact, from this resistance that the epistemological foundation of market experiments discussed above was born. These arguments against market experimentation took several generic forms. The typical claims included: First, that the use of college students as subjects rendered the experiments irrelevant because college students were not representative subjects. Second, the experiment doesn't mirror the real world and therefore cannot tell us anything helpful. Third, the laboratory is so contrived that it can never teach us anything useful about the real world. Fourth, that there are always methodological problems with experiments.

Most of these criticisms, however, are not directed toward experimentation in general. They are typically addressed to "particular experiments conducted."[81] They furthermore typically "suggest that the experiment did not answer a question that the critic wanted answered."[82] In fact, these are "not criticisms of the use of experiments"[83] but instead "a call for additional experiments."[84] These kinds of criticism arise:

> not because they know what's happening, it's not because they've thought about it, it's not because they can't imagine how or they imagine in silly ways how one might apply an experiment to what they are interested in, which is some kind of field economy. And the answer is…nothing. It has

[78] ibid.
[79] Colander, David, Richard Holt and Barkely Rosser. The Changing Face of Economics. Ann Arbor: University of Michigan Press (2006) 7
[80] ibid.7
[81] parkin interview of plot in 03
[82] ibid.
[83] ibid.
[84] ibid.

> nothing to do with that, but we can study the principles that are operating in both of them.[85]

The Development of Market Experimentation

After Chamberlin ceased doing market experiments, there was no movement until Smith found himself in the very particular context of Purdue. He was with a group of young economists who would eventually make Purdue famous for its program in mathematical economics. In this group of young economists, "there was one thing we all agreed on…we were all pretty dissatisfied with our graduate educations."[86] This group of researchers were given free reign, and the university leadership was "very accepting"[87] of Smith's interest in experiments. He did knot know, however, "how unusual that was."[88] As it turned out, "what was going on there was not anything that could have been reproduced anywhere else. So it was a great incubator."[89] It was in this context that Smith did the experiments that paved the way for his famous "An Experimental Study of Competitive Market Behavior" in the *Journal of Political Economy* in 1962.

Smith 62

This paper turned out to be a crucial step in the development of experimental methods. In Smith's words, "Chamberlin showed that certain markets didn't work, but I thought it would be a more powerful demonstration if you gave markets a better chance than he was giving them by repeating them and also using these trading rules."[90] Smith had been getting different results than Chamberlin, and through the process of running multiple experiments he was able to figure out why. It was through this process that he "we learned a lot about the importance of repeat interaction, which he didn't have and the rules of trade, the institutions." The economy Chamberlin had established was "basically what theorists later would call a random meetings economy – people meeting at random, except they would just meet once … there was no opportunity to learn over time."[91] Smith then did two things to improve efficiency: "I repeated it, and I also used an oral outcry two sided auction."[92]

The body of the paper offers some interesting insights as well. Smith sets up the project of the paper by saying, "this article reports on a series of games designed to study some of the hypothesis of neoclassical competitive market theory."[93] Smith is clearly framing the work as hypothesis testing, a process he would later reject in the full development of

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[85] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[86] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[87] ibid.
[88] ibid.
[89] ibid.
[90] ibid.
[91] ibid.
[92] ibid.
[93] Smith, Vernon. "An Experimental Study of Competitive Market Behavior." The Journal of Political Economy, Vol. 70, No. 2. (Apr., 1962), pp. 111-137. 111

the epistemology and method of market experimentation. He is aware of the difficulty that he might be taken for simulating a natural market and hedges with the statement "I would emphasize, however, that they are intended as simulations of certain key features of the organized markets and of competitive markets generally, rather than as direct, exhaustive simulations of any particular organized exchange."[94] More importantly, Smith demonstrates that "We do not require an indefinitely large number of marketers, which is usually supposed necessary for the existence of 'pure' competition."[95] He determined that "even where numbers are 'small," there are strong tendencies for a supply and demand competitive equilibrium to be attained as long as one is able to prohibit collusion and to maintain absolute publicity of all bids, offers, and transactions." This is the discovery, more fully developed throughout the course of market experiments, that the Walrasian hypothesis and all of its attendant stipulations for convergent is unnecessary.[96]

The paper, while published in the *Journal of Political Economy*, was paid a completely minimal amount of attention.[97] Plott believes this is because the paper was sold wrong. He says, "by the way, I think that's also the reason that Vernon Smith's early stuff didn't take off. His early experiments? Pfft…nothing, because he sold it wrong."[98] This paper was actually "pure discovery on his part. He didn't design those experiments to test anything because none of the assumptions of the model were met."[99] Smith is not actually "thinking about theory testing when he's doing that. He's really doing a data-driven experiment."[100] Smith's paper was perhaps framed wrong, but he had stumbled on some critical methodological advances in the use of experiments.

Smith also shared a story about his experience submitting the paper to the JPE. He said, "I sort of naively thought that would be a good place to send it." He thought, "the University Chicago, they appreciate markets .. they believe that markets work, and I was showing that that had validity." He learned instead that "they already knew that markets work so they didn't need any evidence."[101] While the paper was eventually published in the JPE, it turned out to be a fight for Smith because of the methodological prejudices of the JPE.

Developing Literature

While Smith was still at Purdue he also began developing a course using "quite a bit of that stuff in psychology" meaning the work being done by Anatol Rapaport and Ward Ewards.[102] In fact, Smith would have Ward and Rapaport come to Purdue and give talks. The students in his courses were also doing work with market experiments, and the experimentalists "started to generate our own literature."[103]

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[94] ibid.111
[95] ibid.111
[96] ibid.134
[97] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[98] ibid.
[99] ibid.
[100] ibid.
[101] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[102] ibid.
[103] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007

At that time, Plott also came to Purdue from the University of Virginia. He had been there because of the "serious openness to almost any question you asked" and he found Purdue to be "essentially the same way."[104] It is at this point that the famous bass fishing trips between Plott and Smith began. They would chat about "fishing, economics, and the stock market." Smith would "go back and talk about these experiments because he was preoccupied with them" and "he kept saying about how this converged and what he did to get it to converge and how he had some problems he couldn't solve."[105]

Plott was skeptical of experimentation at first. He recalls thinking "this is silly. This guy thinks that supply and demand works."[106] Plott set out to demonstrate that it was not, in fact, supply and demand but a Bayesian game that drove convergence. He and a graduate student named Harvey Reed set up an experiment, and Plott continued to experiment from that point forward.

Smith 65

This paper, entitled *Experimental Auction Markets and the Walrasian Hypothesis*, returned in many ways to the themes of his 1962 paper. This experiment was meant to test the Walrasian Hypothesis against the Excess Rent Hypothesis. It attempted "(1) the severest test yet attempted of the equilibrating forces operating in competitive auction markets and (2) a more rigorously controlled test of the Walrasian hypothesis."[107] It was "the experimental design was determined by the objective of providing good discrimination between the competing hypotheses."[108] This is still hypothesis testing. However, it was able to demonstrate once again that "again concludes that competitive equilibrium works…even under extreme conditions."[109]

This paper, moreover, initiated the extraordinarily careful selection of participants that would remain consistent throughout Smith's thought, as well as the careful articulation of his methodology:

> No subject participated in more than one of the sessions. The sessions were run separately in each of two series separated by several months. Subjects were given no advance warning that an experiment was going to be performed in their class, and the experimental sessions discussed in this paper were intermingled with sessions for entirely different experiments. This procedure was used to minimize information transfer between subject groups.[110]

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[104] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[105] ibid.
[106] ibid.
[107] Smith, Vernon. 'Experimental Auction Markets and the Walrasian Hypothesis" *The Journal of Political Economy*, Vol. 73, No. 4. (Aug., 1965), pp. 387-393.
[108] ibid.
[109] ibid.
[110] Smith, Vernon. 'Experimental Auction Markets and the Walrasian Hypothesis" *The Journal of Political Economy*, Vol. 73, No. 4. (Aug., 1965), pp. 387-394.

Smith 67 paper

Smith's "Experimental Studies of Discrimination Versus Competition in Sealed-Bid Auction Markets" was published in the *Journal of Business* in 1967. It maintains some of the methodological innovations of the past, like the rule that no subject can be involved in more than one experiment.

It furthermore begins the engagement with the design of institutions:

> For institutional background we discuss briefly some mechanics of the Treasure-bill auction. This auction is an ideal example because its organization and functioning are know in considerable detail, and it has been proposed that the discriminative practice of the Treasure be replaced by a competitive procedure. However, the bill auction as such, has no dominating interest for this study. Our interest is in the general characteristics of the sealed-bid auction and the effect of price discrimination and pure competition on behavior, whether the commodity be securities or potatoes.[111]

This is the beginnings of the methodological foundation for the design of institutions through the use of experiments. It articulates how the conversation might become relevant to policy. However, it makes clear that this relevance will not have to do with the simulation of the actual conditions of the market. Instead, it comes from the principles in operation in similar economic situations.

This paper also begins to demonstrate the utility of experimental economics for the study of auctions. It demonstrated a (1.) "higher variance in competitive bids" [112] and (2.) that the (2.) "total receipts of a monopolistic seller are higher in competitive auction when there are not many rejected bids."[113]

Fishing Again

In either 1969 or 1970, Vernon Smith and Charles Plott went on another fishing trip. Smith was tell a story and "drawing in the sand, drawing an Edgeworth box, and when he was drawing this Edgeworth box, I realized that you could do the same thing or do a generalization about [this game theory model] and study these political models."[114] Plott "worked out the parameters on the plane back from Lake Powell" and gave them to Mo Fiorina, a colleague, when he got back. In his words, "that's how that got started,"[115] meaning on political modeling through experimentation.

Smith at Caltech

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[111] ibid.57
[112] ibid.57
[113] ibid.57
[114] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[115] Plott, Charles. Interviewed by Paul Slattery on 09-21-07

In 1971, Vernon Smith traveled to Caltech for a year to teach a course with Plott. It was around this time that Plott started doing experiments at Caltech. Caltech, like Purdue was a "really a very open intellectual atmosphere."[116] Plott described the attitude of his colleagues as "if you think it's an interesting question, go for it." He arrived at Caltech interested in axiomatic social choice theory, but after a year he began doing experiments again. This was the beginning of Caltech's experimentation proram.

Williams 73

In 1973, Fred Williams published "The Effect of Market Organization on Competitive Equilibrium: the Multi-unit Case" in the *Review of Economic Studies*. This "reports experiments designed to extend Smith's work to the case in which traders may trade multiple units of the commodity in any market period."[117] It was the first extension of Smith's experiments in the case of multi-unit auctions. It anticipated the far more complex markets that would develop with the advent of computers.

This study also employed many of the important advancements in technique that Smith had made. Each subject only participated in one session as a buyer and one session as seller. It employed induced value theory and noted the debt to Vernon. This paper had to employ symmetric demand and supply curves, as it would have become to complex to attempt the accounting for a multi-unit auction. The advent of the PLATO computer system would, of course, resolve this issue.

This paper also made an important contribution in its own right. Williams chose to freeze the prices after the first period to simulate the effect of stickiness in prices. Interestingly, in this context, price leadership by sellers raised prices above the equilibrium role and price leadership by buyers lowered prices below the equilibrium level. This result is the exact opposite of the result obtained by Smith with continuous bidding. This result suggested the importance of price stickiness as a market institution.

Smith 76

In 1976, Vernon Smith published a method paper entitled "Experimental economics: Induced Value theory" in the *American Economic Review*. This paper outlined induced value theory, essentially a method of payment of subjects that could induce utility curves in the subjects through different payment rates for different kinds of consumption. By

[116] ibid.
[117] Williams, Fred. The Effect of Market Organization on Competitive Equilibrium: the Multi-unit Case *The Review of Economic Studies*, Vol. 40, No. 1. (Jan., 1973), pp. 97-113.

inducing indifference curves, this paper made it possible to study differentiated goods markets or intertemporal markets.

The more interesting aspect of this paper for the purposes of this project, though, is the time Smith spends making an argument for the utility of the laboratory setting in hypothesis testing. He believes the state of economic hypothesis testing "based on casual observation … one develops a model, which is then tested with the only body of field data that exists. The results of the test turn out to be ambiguous or call for improvements, and one is tempted to now modify the model in ways suggested by the data "to improve the fit."[118]

Obviously, the meaning of significance testing in this framework is dubious. Smith believes that the laboratory is an ideal setting in which to perform significance testing provided the question lends itself to laboratory investigation. In the lab, the "fact that one can always run a new experiment means that it is never tautological to modify the model in ways suggested by the results of the last experiment."[119]

Smith furthermore develops the argument that the laboratory is a real market. He claims that "the characteristics of 'real world' behavior that we consider to be of primitive importance—such as self-interest motivation … —arise naturally, indeed inevitably in experimental settings."[120] This is because the lab "becomes a place where real people earn real money for making real decisions about abstract claims that are just as 'real' s a share of General Motors."[121]

PLATO

1976 saw a major methodological breakthrough in market experimentation. This was the advent of computer assisted markets. This began in the lat 70s when smith went to the University of Arizona and had Arly Williams as his graduate student.

Originally, the purpose of PLATO was to run experiments previously run by hand for longer periods of time, but "That turned out not to be near as important as the fact that we could handle a whole lot larger message spaces, and we could do far more complex experiments.[122] For Smith, "this is what gave birth to the notion of a smart computer assisted market."[123]

The assistance of computers furthermore made it possible to attempt far more complex policy questions. Smith worked on the liberalization of electric power in Australia for a

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[118] Smith, Vernon L, "Experimental Economics: Induced Value Theory," American Economic Review, American Economic Association, vol. 66(2), 1976. pages 274-79, May. 274
[119] ibid.274
[120] ibid.274
[121] ibid.274
[122] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[123] ibid.

time, and this required "essentially a five-node radial network with all the constraints and everything that govern the change of claims on energy."[124] The capacity to even begin to participate in this kind of policy discussion was the direct afflux, for Smith of the event of the computer.

Computers furthermore created the possibility of a separated market. This made it possible to extend the experiments to include "general equilibrium – two and more commodities that people trade." It turned out that in these markets "converge also in very little more time than it takes an isolated market.[125]

The advent of computers also made it possible for the computer to make calculations for the subject of the experiment. This meant "You could bid on the components of a good and it would assemble the packages. That was just mechanism design."[126]

76 Committees

By this time, Plott had turned definitively toward policy questions. He began to do experiments with Mo Fiorina to simulate committee settings. This was, for Plott, "where modern experimental economics starts."[127] The committee work was part of a "whole series of experiments in the early 70s dealing with earning, social choice, committees, and agendas.'[128] Much of it did not get "published until later because people kept rejecting it. It was really hard to get stuff published." [129]

The committee stuff involved substantial methodological developments. This is actually the first occurrence of the use of the language 'naturally occurring.' Moreover, Plott and Fiorina "started out with a phenomenon [they] wanted to study."[130] This was crucial because it gave them a "well-formulated alternative space."[131] This essentially permitted them to turn the committee work into a competition between models. Plott notes "if you go back, you'll see that there's something like 10 or 15 models there. That had never been done before."[132]

Making Experimental Economics Useful

With the advent of the committees work and all of the other applications previously listed, market experimentation had reached an impressive level of methodological rigor and contributed some substantial results to the discipline. However, the subfield still did not attract a great deal of attention within the discipline of economics. Experimentaiotn

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[124] ibid.
[125] ibid.
[126] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[127] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[128] ibid.
[129] ibid.
[130] ibid.
[131] ibid.
[132] ibid.

was still subject to skepticism concerning its utility. It was still subject the question "'what's this telling me about the US stock market?'" For Plott, the "answer is nothing … I think those comments probably come from people who don't know what you're supposed to see."

However, the problem remained of finding a niche in which the subfield could establish itself against this skepticism. For Plott this was a deliberate process. The response to "that type of atttude that is exactly the reason I did the barge." In fact, "That's the reason we did the first antitrust with the Ethel case. That's the reason we looked at information aggregation and started looking at options and futures markets."[133] Smith and Plott inaugurated a deliberate campaign to communicate the worth of experimental economics through policy applications.

This begins "with this old paper that Vernon Smith and I did on the posted price effect."[134] Milton Friedman had argued "that they should use a one-price auction rather than a discriminative auction for the sale of treasury bills." [135] Smith set out to run experiments on the proposition. Smith and Plott then conducted a series of experiments on posted price institutions. This move brought the focus "classical industrial organization and gives us a policy focus that we could then start pushing."[136]

The significance of this moment cannot be overstated for Plott. When asked "Where does the posted price effect discovery fit in in gaining acceptance for experimental economics?" He responded "I think that's the spot."[137]

Smith and Plot 78

This paper begins by making an appeal for the use of laboratory methods in investaging price theories. Rather than theory falsificaiotn, Plott and Smith envision laboratory economics serving as theory's proving ground:

> If well-formulated theories consistently fail to predict simple laboratory behaviour, then one would be hesitant to trust their predictions in richer environments. Furthermore, when replicable laboratory behaviour can be demonstrated, one should seek those extensions of accepted theory which explain why it occurs.[138]

The paper itself compares a one-sided oral auction to the posted-price institution. The conclude that while the "posted-bid and the oral-bid institutions are remarkably efficient … but the oral bid auction is more efficient."[139] This concept of efficiency is crucial. It is

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[133] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[134] ibid.
[135] ibid.
[136] ibid.
[137] ibid.
[138] Plott, Charles R & Smith, Vernon L, "An Experimental Examination of Two Exchange Institutions," Review of Economic Studies, Blackwell Publishing, vol. 45(1), 1978. pages 133-53 133
[139] Plott, Charles R & Smith, Vernon L, "An Experimental Examination of Two Exchange Institutions," Review of Economic Studies, Blackwell Publishing, vol. 45(1), 1978. pages 133-53 133

essentially the percent of potential realized gains from trade achieved by a particular market institution. In Plott's words:

> The role of efficiency says that I can make institutional comparisons even though I theoretically don't understand them. That allows us to make measurements and make statements about things we don't thoroughly understand. And that is extremely important because what we don't understand is almost everything. That was important.[140]

Efficiency gave market experimenters the conceptual tool to begin making comparative judgments about the institutions they created in their labs. To whatever extent these institutions were relevant to the institutions in the real word via their relationship to universal economic principles, these experiments could then serve to make important and quantifiable recommendations to policy makers about which institution to install.

The concept of the posted-price effect – that the posting of prices by the producer will raise prices – lead to Plott and Smith's engagement in the dispute of inland water freight's posting of prices. Plott was in Disneyland with his friend John Snow, both a Virginia classmate and at the time the chief council for the Department of Transportation. The railroad were attempting to force the barges to post their prices. However, according to the posted-price effect, that would have actually had a detrimental effect. Snow offered Plott the opportunity to help, and with that Plott and Smith had their:

> first real live regulatory experiment or application of experiments. Which was very, very important. I wanted to do that because I thought that, in order for people to gain acceptance of looking at the science the way I saw it, you had to show them that it was useful. And what that does is show that it's useful. [141]

The barges study, for Plott, was a way of reinforcing his epistemological concept of economics. This was again the case when Plott, Marc Isaac, and David Grether took on the question of changing the way in which airports allocate landing spots. It was also the case when plot agreed to take on the antitrust case, and it precisely so he could deliver this line in his letter to *The Economist* concerning experimental economics:

> Have the principles been put to valuable use? Experiments have played a central role in several major instances. These include the allocation of the rights to land at major U.S. airports, regulations governing pricing in natural gas pipelines, the Ethyl case in antitrust, the design of the auction mechanism used by the Federal Communications Commission, the architecture of the Regional Clean Air Markets in Southern California, the electric power markets in operation in Southern California, decisions regarding access to public railroad tracks, methods of allocating resources on Space Station Freedom, etc. Many other applications are underway.

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[140] Plott, Charles. Interviewed by Paul Slattery on 09-21-07
[141] ibid.

> The editorial carried the implication that laboratory experimental work has no applications and that impression is seriously wrong.[142]

For Plott, the extensive participation in regulatory skirmishes was an ideal way to raise the profile of market experimentation and deliver its immediately obvious worth. This was a deliberate strategy executed in response to skepticism about the subfield. This policy focus was picked up on by government regulatory agencies. These regulatory bodies who were "very interested in people who did experiments" because "if you did experiement you had to think critically about what the assumptions in a theory implied."[143]

The Discipline Today

The economics discipline looks radically different today than it did when experimental economics first came onto the scene in the 1950s. Colander, et. Al. have stressed that "we emphasize complexity as the defining factor of the new work at the edge of economics because it appears to us to be the vision behind this work."[144] Moreover, as Binmore argues, "it is probably true that my belief that one should approach the social sciences in the same way that we approach the physical sciences used to be unusual. But my impression is that this attitude is quite common nowadays."[145]

The discipline itself, defined as modern mainstream economics, is now willing to be open to new approaches "as long as they demonstrate a careful understanding of the strengths of the recent othodox approach and are pursued with a methodology acceptable to the mainstream." [146] The elite too, have changed. They are now fundamentally more open, thought they are still resistant to new methodologies. [147] Moreover, Colander, et. all argue that "sometimes it is simply just a process of time before the ideas are accepted, as was the case with the experimental economics that started in the 1950s but has taken several decades to be accepted into the mainstream.[148]

Experimental Econ today

Experimental economics today is far more fractious than it has been previously. Davis responded to the notion of updating the comprehensive textbook he composed with Holt by writing, "the fields are so enormous now … they've changed in relative

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[142] Plott, Charles. Charlie Plotts Letter to the Economist. 2006.
[143] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[144] Colander, David, Richard Holt and Barkely Rosser. The Changing Face of Economics. Ann Arbor: University of Michigan Press (2006) 18
[145] ibid. 59
[146] ibid. 10
[147] ibid. 10
[148] ibid. 19

importance."[149] Markets research now occupies a less central space, but the mechanism design that it evolved into is one of the most prominent parts of the subfield.[150]

Moreover, elite universities have started to build experimental economics programs. Davis notes that "Harvard decided a couple years ago that they were going big-time into the experimental business, and Princeton did to." Conversely, "there isn't any major development at MIT."

Smaller universities have started to create facilities as well: "you know Appalachian State has a lot of experimentalists and they do a lot of work. They have neither location nor…Tennessee has a lab. New Mexico.[151] Experimental economics is no longer the purview of a particularly forward thinking or permissive departments of economics.

Davis actually expressed the opinion that there "is some perhaps justified resentment to this kind of overpublication of experimental work."[152] In fact, he seems to think it has lost some of the value of its former skepticism and methodological rigor. Smith too believes that "It's pretty hard to know what economists think they are doing. Basically, they are doing whatever is next, and that's coming out of – mostly out of – the immediate literature. Well…and I think there's a lot of that in experimental economics." [153] Smith locates the blame for this phenomena in the fact that experimental economics has 'exploded' as a discipline.[154] He, in fact, belives:

Well, it's pretty hard to ignore now. There are too many people doing it. There are journals now that regularly publish experimental work that do not specialize in experimental work. The AER, the Economic Journal, and then some of the industrial organization journals. They accept those manuscripts pretty even-handedly.[155]

The Subdiscipline of the Future

In the future, it seems likely that experimental economics will become an alternative methodological tool not linked to any research agenda. Indeed, Davis strongly believes this was the original intent of experimental economics. In his words, "there are stupid experiments and there are good ones and they are at best a compliment to the things you get from other data sources."[156]

Moreover, it seems unlikely that social psychologists and behavioral economists will try to stay within econ departments and within the experimental economics subfield. Davis believes that they "will become their own subfield as they drift away from other kinds of

[149] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[150] ibid.
[151] ibid.
[152] Smith, Vernon. Interviewed by Paul Slattery on July 3rd, 2007
[153] ibid.
[154] ibid.
[155] ibid.
[156] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007

psychology and these economists kind of drift away from the standard allocation questions."[157]

It may in fact be the case that 'experimental economist' will mean something very different in the future. Much like econometricians, experimental economists may be methodologists who do research in the theory of experimentation. In fact, Davis said "I know guys like Vernon and Charlie…more Vernon in his recent writings talks more about the philosophy of data collection and method of thought."[158] It would seem that we may already have the first generation of experimental economists in the methodological sense.

[157] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007
[158] Davis, Douglas. Interviewed by Paul Slattery on Oct. 1st, 2007

Conclusion

In sum, market experimentation has made its way into a kind of general acceptance within the economics discipline. It has done so against a variety of criticisms – obviously predominantly methodological – from outside members of the discipline. These criticisms, however, impelled transformations in the subfield of market experimentation. It may be partly the byproduct of the fact that this subfield is defined by Charles Plott and Vernon Smith being such strong forces in the subfield, but the subfield itself has been responsive to external criticism and external misunderstanding.

Most interestingly, market experimentation has never been excluded from the mainstream because of one of its research conclusions. It has not been expelled because it refuses to accept a core postulate like the rational actor model or expected utility theory. Instead, experimental economics had a fundamental epistemological disagreement with the discipline. The neoclassical school of thought is characterized by a grand processes of deduction from phenomena. Smith and Plott, however, insist that is not what they do. Instead, they work with natural, universal phenomena in a controlled context to more fully understand them.

Once market experimentation had resolved its epistemological problem and determined how to understand its results and practices, it still had another problem. It had to convince the discipline at large to recognize not an objective fact, not a theory, but an epistemology of science. It was not necessary to get the discipline to capitulate and accept its epistemology of science, but it had to attempt to get other researchers to understand it or its results would never seem valid. Plott and Smith were both compelled to write a number of transdisciplinary survey articles and popular articles to attempt to create this understanding of the subfield itself.

Ironically, it was not the successful communication of an epistemology, but instead its successful application to a number of rather mundane economic problems fixed that gained market experimentation its name. The design of auctions and resolution of barge pricing would hardly seem the purview of a subfield that was called into being with something so rich as its own epistemology as its first question. It is in these mundane fixes, though, that the subfield finds its reward for establishing firmly and resolutely its epistemological foundation before attempting to fix any part of human society.

Interview With Charles Plott
Conducted by Paul Slattery
09-21-07

Slattery: The first question I've asked everybody is – and you can call B.S. on this question if you think it's not appropriately framed, but what for you is the mission or purpose of economics?

Plott: It's a science. What's the mission or purpose of any science? You want to see how…Actually, in a sense, that could be a little deeper question than you might suspect because different people have different ideas about what economics is about in terms of the phenomena we study, and a lot of people would pick out the idea of Alfred Marshall who says that economics is about the way people go about the ordinary business of life. I disagree with that. I think it's about the principles that operate, the laws that operate with people. That means it takes it out of the study of history and the study of economies as they are found growing in the wild, and puts it on the level of trying to decide what principles are operating when you watch these things. That's profoundly important because if you are supposed to study economies in the wild, there's no place for an experiment. So, if you're studying the principles, then it could be the best place to study the principles are in economies that look absolutely nothing like the ones that nature created.

Slattery: You need some sort of controlled experimental environment.

Plott: Yeah. Some of your questions early were directed at what were the controversies or what seemed to be the controversies, and I think that that issue there partitions them all. That kind of explains where you find many critics of experiments. It's not because they know what's happening. It's not because they've thought about it. It's because they can't imagine how or they imagine in silly ways how one might apply an experiment to what they are interested in, which is some kind of field economy. And the answer is…nothing. It has nothing to do with that, but we can study the principles that are operating in both of them. And there's a logic for that and the way we understand it and a way you go about it. That probably is something that is quite deep. What's the mission? Economics I think is about the principles that operate when people go about the ordinary business of life. I think Marshall got it wrong. I think whole schools have it wrong, like the University of Chicago and other places, when they say you're supposed and study these field **[?]**. I couldn't care less about that.

Slattery: That seems to be a difference between your characterizations of economics and other peoples'…that you're looking for more universal, fundamental principles that are applicable everywhere – that are natural.

Plott: Economics is filled with universal principles, absolutely filled with them, but somehow, people don't understand. They haven't looked at it that way.

Slattery: It's very contextual or relativist right now.

Plott: By the way, I think that's also the reason that Vernon Smith's early stuff didn't take off. His early experiments? Pfft…nothing, because he sold it wrong.

Slattery: How should he have sold it?

Plott: Well he sold them like he was trying to simulate a stock market. That's not what he was doing. What he was doing was studying the principles of the way markets operate. And so, when people looked at it, they said, "this is not the way the US stock market works, no stock market works like this. This is completely irrelevant." I think he was pretty well dismissed across the board because his way of articulating that…

Slattery: Didn't take.

Plott: Was just wrong. It wasn't very thoughtful.

Slattery: Bradley Batemen said something about this, but also Holt and Davis' textbook and some interviews with Herb Gintis – they say that experimental economics has contributed "humility" or the word "humble" comes up…

Plott: Oh, I think that's junk. What it's done is contributed really basic discoveries of the way things work. We're actually able to see things operating and understand them in ways that we were never able to in history. See phenomena that no one ever saw before and understood before.

Slattery: Well the interesting thing is that everything that I've ever seen about or read about you associates you with economic engineering or design economics. So how does that fit in with the sort of basic science, simple principles?

Plott: Well once you understand the principles you understand how to use those principles to get systems to do things they weren't able to do before.

Slattery: So the idea is to get a hold of the principles and then apply them, rather than to say…simulate a market.

Plott: I think simulating markets is a dead end. Completely a dead end. I think it's the wrong way of thinking about experiments, and I think it's the wrong way of thinking about science. Simulations, basically, are good tools, but they should be looked at as

more mathematical problems. It's a way of solving problems that you can't solve otherwise because they're just too technically hard. So you get in, you simulate to get a feeling for it. Sometimes you'll simulate in economics and you'll see features of institutions and processes that are mysterious. You don't know what role their playing. So you put those in an experiment and watch, and after watching a while, you begin to understand what role they might play. So it actually can lead to a really deeper understanding of what it is you're observing. There's no doubt about that. But the purpose of the experiment is wrong and the concept of simulation is wrong epistemologically. That's not the way we learn and apply it. People who follow simulation think, "I want to do the experiment and make it mirror the real world as much as possible because then I want to take the results and apply it there." That's not the way it works. What happens is you take an experiment, you learn about the principles, you develop a theory, and then you apply the theory. It's the theory that takes you from the data to these more complex things, not the experiment. I think that people don't really appreciate that indirect way in which science works.

Slattery: So experimental economics – when people pull out in textbooks just generally what it is – they'll say something like "it's an effective tool for theory falsification," but you see it as being generative to, as producing theories?

Plott: Well, no. It doesn't produce theories. It's toying with the data that you see that produces theories. Also, I don't like the theory falsification. I think that's another dead end. All the theories we use are false. Falsifying a theory is child's play. There's nothing to that.

Slattery: It's about competitive models?

Plott: It's competitive models. This philosophical idea that you need to design an experiment to test a theory – we see that in places in economics where the theory is really quite well developed. We do see instances of that working. But as a broad call or guiding principle about how to do it. Uh-uh.

Slattery: Doesn't work out.

Plott: These are really deep issues by the way.

Slattery: Somewhat through the Nobel Committee, somewhat through the historians of economic thought, somewhat through the textbook writers – they've come up with this line that there are three subdivisions of experimental economics as a subdiscipline (they call it a "subdiscipline"). There are individual choice experiments linked to social psychology and the behavioral school; there are game theory experiments; and there are people who do market experiments. As far as I can tell, that line is misleading or doesn't describe much because a lot of the people who do market experiments or the game theorists aren't particularly fond of or don't have the same research agenda as the behaviorists. I don't think that it holds together. There's no common research agenda.

Plott: Well, there are several bodies of phenomena. It's interesting that you didn't mention committees or group choice or voting which basically is a part of public economics. And we do lots of experiments on those things as well.

Slattery: I don't know why these three are the standard line.

Plott: Well, it kind of reflects where theories develop. There are theories of individual choice that are quite well defined, and they're really easy to identify. The question is how a person chooses. What are the cognitive processes? Is he an optimizer? How do his beliefs form? I mean, those are all kind of principles that govern an individual. And presumably, if you're going to study individual behavior, you're going to need at least one individual. It's not a computer problem. So need to study an individual, and usually when you think about that it requires very special tools. There are lots of subtheories, like perception – Did I notice? Did I see it? Is thought part of your patterns or is it a reaction like catching a ball? So there's a whole series of subtheories or subparts. Then when you get into more than one person…typically it's two because in game theory, anything detailed your not going to be able to solve. So two or three people in something really simple like an auction, or a matrix game, or a coordination game – there you're seeing people focus on principles that are alive in more complex models, like the idea of coordinating across an equilibrium. That occurs in really big, complex models. What is happening there is they're pulling out a feature of that model or basically a principle and looking at it operating in a small manageable area. Now, they may have lost everything of interest in doing that. It might not operate that way in a more complex environment. But they pull it out in a way that it's manageable, and usually that means no more than two or three. Then the jump from there to a committee or market is a pretty big jump because we've been unsuccessful in taking the principles that we observe at the individual level or at a two person level and developing models of markets. For example, there is no hope as I know and certainly no example of someone starting from behavioral economics and being able to explain what would happen in a market model or market experiment. That ability to go from quote behavior to a market, even a small market of five or six people – they can't do that. It's not done. In fact, you can't do that well with two person games either. In some sense, when you move up to a market we find that things really work well, the suggestions from game theory are quite powerful, but you can't turn the crank on game theory and get out a law of supply and demand. We don't know how to do it.

Slattery: It seems particularly that the insights from behavioral research – I don't know how you would aggregate it and formalize it up to a market level at all.

Plott: A lot of the behavioral research is just negative. It just says, "do people optimize?" Well, the answer is no.

Slattery: What is your take on the biases and heuristics school?

Plott: One of the first serious biases as it was turned up in economics was Dave Grether and I on preference reversals. And I think that was really important because it says, "if

you take something like a market model and push it in a limited case, in the special case of a single individual, you can reject it." You can see that when you specialize the market behavior as we imagine an individual behaves down to the individual, it's false. In a simple case, we reject the model. It tells you that in the limit in some sense, these models that we are dealing with are really false.

Slattery: You can't just take a representative actor?

Plott: Yeah, the representative actor model probably isn't going to work if you test it. It means that, to study it, you have to go about it a little different way. Oh, I think that heuristics and biases – I read these things. I look at them. Frequently, they'll tell us about perception. They suggest things we might look for in more complex environments. Right now, I guess I cannot give you a principle that holds up out of that body of material.

Slattery: You studied at UVA – where Holt now has a program of experimentation – but as you pointed out, that wasn't the case when you were there. Were there certain characteristics of the UVA program when you were there that made you more receptive to experimental economics?

Plott: When I was there, Jim Buchanan, Ronald Coase, and Gordon Tullock were all there. At that time, public choice was just beginning. As a matter of fact, I was Buchanan's research assistant. So for the first public choice meeting, Buchanan asked me to do a survey and list anything that might have a public choice flavor to it. So I published what's called a provisional bibliography that was then used to organize the first meetings on public choice. I was deeply involved in that, but nothing was experimental when I was there at all. But public choice basically is quite scientific in the sense that they were interested in principles of behavior to see how those principles might carry over to political environments. In that sense, it was very structured. Public choice was also focused on design: given the way we think that people might behave, can we construct institutions that make the system behave quote better in some sense? So the whole idea of design – it wasn't talked about in those terms – was inherent. But, of course, design has been inherent in economics since the beginning. I mean, that's what economics is about. Adam Smith thought they ought to do away with certain types of legislation, rules, and taxes.

Slattery: So [Vernon] Smith talked about this. There's a focus in experimental economics – particularly in the market experiments – on institutions (trading institutions, etc.). And those seem to be somewhat poorly theorized or not thoroughly theorized in the neoclassical school, like trading institutions or auctions. Why do you think that's the case? Do you think there was a dearth of research there before experimental economics?

Plott: I think that probably what's happened is that the concept of an institution has become more crystallized as something that's recognized. But you know, there's always been this focus in economics on institutions and the rules. In fact, there are whole schools called institutional economics. They basically wanted to look in great detail at rules. They had no amazing theory about the implications of those rules.

Slattery: American institutionalism seems a bit disorganized when you read it.

Plott: And the idea of law in economics. Why is there this interaction between law and economics? Well, it's because the lawyers are the guys who are experts on defining and identifying institutions. So there's always this kind of symbiosis between institutions and economics. Even in the early experiments – if you look back at the early experiments that Sydney Siegel did – you'll notice that he did some treatments where there was information feedback. He actually said, "what happens if I know what you did?" That's an institution. He didn't realize that it was an institution, but he knew from a systems/behavior point of view that it was important. So it's always been there. It's only really recently that the word institutions began to be used widely. I think in experimental economics it starts with this old paper that Vernon Smith and I did on the posted price. It might start a little before that. It starts before that with Milton Friedman's argument that they should use a one-price auction rather than a discriminative auction for the sale of treasury bills. It was picked up by Vernon, who actually did some experiments on it. So that was really a serious institutional issue right there. Which institution generates more? Vernon did some experiments there. Then the next real serious step was where Vernon and I discovered the posted price institution. That brings it more into classical industrial organization and gives us a policy focus that we could then start pushing. Then the next big stages came from finance and asymmetric information – the stuff we did on information aggregation. At the same time, the game theorists started to come in and could solve certain kinds of auction process.

Slattery: Was this before the committee work you did in the 80s?

Plott: Oh, no. Committee work is the first stuff. That's where modern experimental economics starts. Vernon did the stuff in the 50s, but then he drops out. Then I get interested talking with him. Notice that you can generally take that old stuff that he had done in the 50s and generalize it to public choice issues. Then there's a whole series of experiments in the early 70s dealing with earning, social choice, committees, and agendas. Now, a lot of that wasn't published until later because people kept rejecting it. It was really hard to get stuff published. That's really where it starts. We had a full-on experimental program here at Caltech, and that's really where it started. That was around '71 or '72, and then we got the money to invite Vernon here in '73 or '74. That's really where it starts. Vernon was out until then, from the '50s.

Slattery: Why do you think Vernon got out of it…was it just that people weren't picking it up?

Plott: Well, I think that people weren't receptive. No one was excited about it. He tried to sell it and couldn't sell it, and no one was paying attention to it. He got interested in environmental things. He was doing these amazingly great things on both risk preferences and environmental types of predator-prey models and these stock flow problems that called on his early engineering background, these differential equations

that he would screw around with. He never lost interest – ever lost interest; he was always interested in it, but he wasn't doing it.

Slattery: Why do you think people weren't receptive or it was difficult to get published early on?

Plott: In the '50s?

Slattery: Yeah.

Plott: Because he sold it wrong. I think that what you learned wasn't well articulated. They were looking for what this tells me about the US economy and not what it tells me about economics.

Slattery: Was the committee stuff well received early on – you said it took a while to publish it?

Plott: Yeah, it was well received. The first one was in the American Political Science Review. That work is really important, and probably not many people notice it because a lot of first things were done there that were inherited later. For example, even the language, the use of "naturally occurring." That was used a little bit in the 50s, but to say that "we have experiments and there's naturally occurring things, we're not really that interested in these naturally occurring things, we're interested in the principles operating." Another thing that we did which was extremely important was that we started out with a phenomenon we wanted to study. We said, "we're not starting out with a theory; we're starting out with a definition of what we want to study. It's going to be committees where they've made up their minds and there's a well-formulated alternative space. And we want to know if we can predict." Then, it's really a contest over models, and if you go back, you'll see that there's something like 10 or 15 models there. That had never been done before.

Slattery: It seems a lot more along the lines of the scientific method.

Plott: I don't think philosophy of science was guiding us at all – I really don't. I think that the philosophers of science, as far as I'm concerned, had not been in the discussion. That was more trying to think through what the arguments were and what it is that we thought we were learning. That was really important also – when I said I didn't like rejection – the committee stuff actually came out of a rejectionist philosophy because the theory of committees – this equilibrium concept – I invented. I showed first what it looked like – mathematically what it looked like – and I'd published that in the AER in the mid 60s. Well, all my buddies had been generalizing that to continuums of people and Hilbert spaces of alternatives and this mathematically general relationship. Blooee. I was interested in something else. And I realized in conversations with Vernon that you could generalize the methods he was using and change it a lot to study this kind of committee phenomena. I thought, "wouldn't it be cute to run an experiment which would show all that is just dead wrong, and I can show it's wrong because I'm the guy who invented the

theory and I could testify that it's wrong." It would kill what I did and also what all these other guys were doing. I thought, "wouldn't that be cute." So that was the objective…what the hell? So I came back and there was a – what I would say was a serious discovery, you know, kind of an "aha what?!" I came back, and Mo Fiorina who was a political scientist had worked experimentally with Bill Riker. So a lot of this early experimental work was influenced by Bill kind of indirectly. So I sketched out the experiment that we would run and Mo, who had done Bill's experiments, went off and did it. I wouldn't even have expected him to do it. He just left and came back and said "Charlie, you would not believe what happened." "What happened?" He says, "it converged right down to the equilibrium; the model worked."

Slattery: Well, that didn't turn out for you.

Plott: That didn't turn out at all. It was an "aha." How could this model work? I could not believe it was going to work – neither of us did. I didn't believe him, and I accused Mo of screwing up the experiment. We immediately went out and did a handful more. Now we have a problem. The reigning philosophy was falsification. We were getting a positive result. How do you tell somebody you're getting a positive result that supports your own theory? They're going to say, "oh, you guys just did it." Now we had to think through how do you learn from a positive result? We knew for sure that the theory was false because by that time, as we were going on, we knew enough about the agendas to know that I could design another experiment using the agenda and changing the institution so this very general model I had would just go up in flames. Now that we know that we're dealing with a theory that is generally false but has a lot of power in some contexts, how do you study it and how do you report what it is you've learned? So all of that first paper was trying to think through that issue. What are we learning from the experiment? What is it teaching us? So the lessons that come out of that exercise are still with us, in fact, almost all of the major lessons you'll find in that first paper. It branches off.

Slattery: Was that widely cited or did people sort of distill the lessons?

Plott: Oh yeah, because from that becomes the agendas. The agendas basically set the stage for a whole lot of things. From that came the studies of elections and election processes, and we began to see that this body of mathematics actually had implications that we could see. Part of that too – at that time, I was an axiomatic social choice theorist, I was just interested in the axioms. I could see that by slight changes in the axioms I could generate new theories of games, as many as you wanted, just by changing a little axiom here or there. And we needed some way – because there were so many theories, all very similar – we needed some way of giving us an intuition about which ones were probably most fruitful to follow. The laboratory experimental methods we developed then and since could take us that direction.

Slattery: And test axioms?

Plott: No, you're not really testing the axioms because axioms are axioms. Classes of axioms you can say compute predictions of the world that a class of axioms will give

you. By changing one or two of those axioms a little bit I could get a completely different prediction in the same environment. In some sense, it wasn't testing the axioms because there were no random variables – testing seems to say is it true or false. We know it's false because none of these things have random variables. So the moment you see something that doesn't happen with certainty, the theory is gone. Because we're dealing with theories that are inaccurate, we don't have a concept of randomness. We don't have a concept of a random variable, so the concept of testing becomes a little vague. You'll see me being very careful here because all during this time being extremely careful about what it is you were arguing was super important.

Slattery: That would make sense. You're generating a new subdiscipline or subfield.

Plott: We were out there all alone.

Slattery: Could you talk a little about the Purdue department in the late '50s?

Plott: '60s, I went there in '65.

Slattery: Smith was still there then right?

Plott: Smith was there. We overlapped slightly.

Slattery: He characterized it as sot of an incubator.

Plott: I saw that you said that. In a way, University of Virginia was very open also about what it was. There was a serious openness to almost any question you asked. You could ask anything that was a legitimate question. It was pretty much unconstrained – what you wanted to study. Purdue was essentially the same way. I went to Purdue though because, at that time, Purdue was specializing in mathematical economics, and I was really interested in mathematical economics. I went there because they had a collection of technical mathematical economists. And by the way, that department split apart a little later and created mathematical economics groups all over the United States. It was arguably – in terms of mathematical economics – the most influential group in the country, at least in the early '60s.

Slattery: Smith seems to move around a lot looking – his characterization to me was "wherever there was an opportunity where they would let me do my work" – but you've been at Caltech since…

Plott: Caltech is a great place. You can do anything you want to.

Slattery: Just haven't ever had any reason to move around?

Plott: Caltech is really a very open intellectual atmosphere. I came here in '71. Of course, at that time now there was only Lance Davis, who was an economic historian, and Jim Quirk, who was a mathematical economist. Both of those guys came from Purdue. So

Purdue began to break apart. Lance Davis and Jim Quirk came here. They then got me to come here. This was in ‘70 or ‘71. So when I came here I was interested in axiomatic social choice theory and mathematical economics. It wasn’t until that first year that I became interested in doing some experiments. There weren’t any experiments done anywhere – not just here. There wasn’t any…you know, it was “guys, you ask the question. Go for it. If you think it’s an interesting question, go for it.” There was no preconception about what’s a good question or how you go about it other than being precise, and if you’re going to say something, be sure it’s right.

Slattery: It’s been interesting telling my professors that I’m doing a thesis in the history of experimental economics. They do this chair shift thing and ask “oh…you thinking of going into that?” It’s cool that they were very receptive here. Smith talked a lot about submitting a paper to the JPE and hearing “oh, we already know that.”

Plott: Oh, that happens all the time. I remember a conversation with Milton Friedman once, and he said, “what have you learned?” I said, “you get this convergence.” And he said, “oh that’s obvious.” Well, no, that’s not obvious. No one had ever seen that in a really technical way, and no one can explain it. Still, no one can explain it. Although, we’re getting really close now.

Slattery: You think so? Explaining the process of convergence?

Plott: Yeah, it looks like it’s related to Newton’s method of solving systems of equations. You can actually see the derivatives; you can see the gradients. We’re getting really, really, really close.

Slattery: What would be the most recent articulation of that – a paper I could turn to?

Plott: Oh, Vernon and I have a book that’s going to be published in June. There’s a paper in there by Peter Bossaerts and me. If you want something simple…if you want something complex, that’s a little different. Are you a mathematician?

Slattery: I’ve done enough to support my econ.

Plott: Before we’re over, I can show you a little something of how that works.

Slattery: Where does the posted price effect discovery fit in in gaining acceptance for experimental economics?

Plott: I think that’s the spot.

Slattery: That’s the breakthrough.

Plott: Mhm. For several reasons. First is, from an intellectual point of view, with that thing we also invented efficiency, which you asked me or were going to ask me. The role of efficiency says that I can make institutional comparisons even though I theoretically

don’t understand them. That allows us to make measurements and make statements about things we don’t thoroughly understand. And that is extremely important because what we don’t understand is almost everything. That was important. It’s also important because it’s an institution which one sees in industrial organization.

Slattery: It’s more immediately obvious how it’s applicable.

Plott: After we discovered that – it’s an interesting story – I was in Disneyland with a friend of mine who I graduated from the university of Virginia with. Turns out to be John Snow, former secretary of the treasury. We were classmates at the University of Virginia getting our economics degrees. At that point, he was chief council for the Department of Transportation. He said, “you know, I have these railroad guys who want to force the barges to post their prices.” I said “John, as far as I can see, it’s going to hurt. I can understand why they would do it, let me tell me what we’ve scene experimentally.” He said, “geeze, want to address that problem.” Well that gave us the first real live regulatory experiment or application of experiments. Which was very, very important. I wanted to do that because I thought that, in order for people to gain acceptance of looking at the science the way I saw it, you had to show them that it was useful. And what that does is show that it’s useful. It also established another really key thing. The structure of that argument is basically a shift in the burden of proof. It says the railroads are saying this, and we found that it’s exactly the opposite of what they want. Why is that Mr. Railroad? So what’s happened is it’s shifted the burden of proof to them. So it’s a strategy for using experiments to keep an argument moving along. You shift the burden of proof. They say this is true. And we say, no you can’t use that argument; better use another one.

Slattery: Back in their court.

Plott: Back in their court. Now that was a conscious decision. That wasn’t an accident. That argument was there because we were trying to think through what the argument was going to be, how it was going to be made. All through these kinds of applications you think about what are going to be the arguments. The first thing they’re going to say is “oh, their college students.” We made sure they weren’t. They were engineers; they were JPO employees. They couldn’t be dismissed.

Slattery: Do you see a difference in the performance of markets that you run with college students as opposed to the general population?

Plott: No, I don’t see any difference with 8-year-old children, 10-year-olds in Russia. You have to make sure that they understand the rules. You have to get their attention long enough and provide some kind of incentives.

Slattery: Can you talk about theory-driven vs. data-driven experimental research?

Plott: Data-driven research is kind of my concept. You notice that that’s where the committees started. It seems to me necessary. In fact, I think Vernon’s experiments were data driven. He really didn’t have the law of supply and demand or…Well, lets take the

competitive model. The competitive model is untestable. It assumes that there is an infinite number of people and everybody is a price taker.

Slattery: I really don't know how you do that.

Plott: What's that? How you going to do that? What is that? But yet you see that in an "as-if" way, it works really well. You use those assumptions, do the mathematics, and crank out the prediction. Man, it'll converge down there.

Slattery: Yeah, Vernon actually expressed a lot of annoyance that his first one worked out so well. He didn't expect that at all.

Plott: In fact, that was pure discovery on his part. He didn't design those experiments to test anything because none of the assumptions of the model were met.

Slattery: Well, you can't meet them.

Plott: No, it's untestable. So he's not thinking about theory testing when he's doing that. He's really doing a data-driven experiment. And you see that almost every major result that we have in economics – except one or two – are data first, are data-driven. The only ones that aren't are a few that come out of auction theory, which is really well defined.

Slattery: Seems very manageable.

Plott: People talk about theory testing all the time, but when they do, they start getting into trouble. Their arguments become a little incoherent. I think that most people really have not thought through what this means. By the way, they call it the empirical approach in biology. Biology is kind of interesting in that sense because there's no theory, just experiments. They call it the empirical approach. They look at the phenomena in the data and say this causes that this causes that. Psychology is the same way.

Slattery: What do you think the relationship should be between economics and psychology? What do you see on that boundary? There's seems to be a difficulty with a lot of the behavioral stuff in that the way psychology approaches the problem or the way they design experiments doesn't obtain in economics.

Plott: Well I think that psychologists have made some really good contributions in bringing our attention to certain kinds of things that are really useful. For example, preference reversal comes right out of early psychology – Slovich and Lichtenstein in the early 70s – which basically says, "hey guys, there's a limit to the extent to which you better take that theory seriously." It also shows that one of the things that economists through was untestable, namely preference theory, is testable and wrong. So it's really important in the sense of bringing something to us and showing it's testable. So there are contributions along those lines. What psychology hasn't brought to us is…psychology in some sense is kind of atheoretical. They think that things should be normally distributed. So most psychology experiments are based upon lots of data. You look at the mean, you

change a variable, and you do some **analysis variance**, and you say, "did this variable have an effect?" Then you tell a story. Oh, it was the long hair effect. Notice that they never tell us what the mean is going to be. Economists don't do that. We say, "we're predicting the mean, and whether or not it's sensitive to something it shouldn't be might be an item of curiosity. But did it effect it in a way such that another model that predicted another mean should take over?" So economics always has this background of principles and theory behind it, and it's always kind of a contest, a tension between theories. Psychology by contrast tends to be more of a rejectionist philosophy. Namely, this theory says that this variable shouldn't have an effect, it has an effect, so the theory is wrong.

Slattery: Which, when you're building predictive economic models, doesn't get you very far.

Plott: Doesn't get you very far. Right. There's a lot of good stuff in there. I love this FMRI stuff for example. It's really powerful.

Slattery: For some reason historians of thought are obsessed with interesting anecdotes. If I read about you and Smith bass fishing one more time on this subject, I'll go insane, but I feel like I should get a little deeper into it. So the bass fishing anecdote is a reference to your time at Purdue?

Plott: Well, when I got to Purdue, I had always enjoyed fishing. I was fishing since I was a kid. I'd get out in the water and putter around. Growing up in Oklahoma, I was always out there fishing. In fact, we go over here and you'll see all kinds of stuff dealing with **[?],** and there's probably some pictures from South Dakota over there. In fact, I see some feathers over there from South Dakota.

Slattery: Right, the state bird, which we invite people to our state to shoot and which is nonindigenous.

Plott: So when I got to Purdue and talked to Vernon, of course, he'd been there for a long time, I found out that he had been fishing in these little creeks for smallmouth bass, which I had never fished for. Vernon and I would go out – and of course, Vernon was delighted because he was the only fisherman around. So if we had some time, we would go out, and he had a little tin boat…
Slattery: Little Alumacraft thing?

Plott: Yeah. He had that hanging around his house. We'd go put that in the Tippecanoe River and go floating around or take it to one of the lakes. We were always out. It was a way of getting out. It was great.

Slattery: Did that carry over to Caltech or is there nowhere to go around here?

Plott: Well, we fished around there a lot, but then we also caught flights and went to Lake Powell. Even when he was somewhere else, we could meet on the flight and go to Lake Powell. We had a stockbroker – we used the same stockbroker – both of us are interested

in the stock market and always have been. So even our stockbroker would pile up and go to Lake Powell with us. So we would be chatting about fishing, economics, and the stock market. Those were the three topics of conversation the whole time. We weren't just chatting about experimentation because he was interested in these predator-prey models and these differential equations. He was also interested in some individual choice, risk preferences. And I was interested in certain axiomatic things. It was just an intellectual…But he would also go back and talk about these experiments because he was preoccupied with them. So, he kept saying about how this converged and what he did to get it to converge and how he had some problems he couldn't solve. And I looked at this as a theorist and thought, "this is silly. This guy thinks that supply and demand works." As a game theorist, I could look at it and say, "I don't believe that because you shouldn't have all these disequilibrium trades if the theory works. So what one should be able to do is go back and show that what's going on here is a Bayesian game, and wouldn't that be neat to show that the fundamental principles driving economics were not law of supply and demand as we thought for centuries but are in fact principles of Bayesian games?" So that drew my interest to experimental economics. I had a graduate student named Harvey Reed, and I said, "hey Harvey, why don't you take a look at this." I sketched out what I thought the theory should look like and what an experiment might look like, and Harvey went off and wrote a dissertation with this. Couldn't get a job. Finally went to law school and made zillions of dollars. But a little later…I had done enough experiments or watched enough experiments – Vernon by the way was not at Purdue, he'd left. Then Vernon and I were fishing again around '69 or '70. We met at Lake Powell. He was telling me the same story he'd told many a times and drawing in the sand, drawing an Edgeworth box, and when he was drawing this Edgeworth box, I realized that you could do the same thing or do a generalization about that and study these political models. That was an "aha" moment. I got really excited, and Vernon had *no* idea what I as excited about.

Slattery: It reminds me of punctuated equilibrium theories of evolution, where the punctuations are all fishing trips.

Plott: Yeah, Yeah. Those two things were definitely punctuated by fishing trips, and probably some others too. I worked out the parameters on the plane back from Lake Powell. It was those parameters that I handed to Mo Fiorina, who went off and did them that day, as soon as I got back. That's how that got started. Again, I saw that we had a positive result when we thought we were going to get a negative result, which caused us to slow down and rethink the whole thing. I have a huge NSF proposal based on that stuff in the early 70s. I should have published that, even the proposal. About how the trees that we should go out, all the variables that we might look at, what it is we seem to be seeing, how you design experiments to pit these theories against each other

Slattery: All by fishing trips

Plott: It all came out of fishing trips. That's literally how that worked. A lot of people thought I was a student of Vernon. No, that's just not true at all. I was just a theorist. I happened to see that…What I think that probably I had, somewhere along the lines, I had

an appreciation for what one can learn from a simple case. I don't know where I got that appreciation. It might have been as an undergraduate coming out of management. You were asking who did the first experiments. As far as I know it was Frederick W. Taylor, father of scientific management, around the turn of the century, and he was doing time and motion studies. He was doing field experiments. He had these guys who were loading pig iron on train carts. He had a guy who would jog in 5 or 10 miles, load pig iron all day, and then jog back home. And he says, "I'm not getting enough work out of this guy," and he started paying him per ingot loaded. He increased the pay and watched this guy's productivity go up until he wasn't jogging home anymore. That was where the piece rate began to be invented. And he has a whole series of things on the optimum sized shovel. And I saw that somehow he was dealing with the theory of Frederick W. Taylor.

Slattery: In a simple case.

Plott: He was looking at the simple case and then generalizing from that. So I knew that the experimental methods were quite powerful in these areas, not based on psychology. Notice also that we're not doing theory testing; we're not doing preference testing; we're doing something else here. So I had a proclivity to do that anyway. And I was dealing with simple cases because I was a theorist and naturally looked at the simple cases.

Slattery: So do you see your research – and again, you can all BS on the terms in question if you want – as mainstream economics, as orthodox economics, what would you say your relationship with the neoclassical school is?

Plott: Well, it's just a different source of data than most people look to. You know, I don't see…the models we are using are neoclassical. The law of supply and demand is pretty old. Nash equilibrium comes basically from Cournot in 1825. The relationship between institutions and economics is really old. General equilibrium comes from the physiocrats before Adam Smith. So there's this long history of asking, at an abstract level, … certainly Walras tells us about general equilibrium, gives us a notion of general equilibrium. And Marshall, Marshall's book is called Principles, which is pretty interesting. Somehow we lost the idea that there are principles. Principles all of a sudden turn out to be elementary. So rather than talk about the principles and laws of economics, as we should, what are the axioms and the principles, we've lost that concept, which has always been part of me.

Slattery: It seems like a lot of the research as it goes forward is small slices of empirical questions and doesn't really have a referent back to any center principles.

Plott: But to me the principles we are studying are that, and they are the principles of economics. They've been around a long time. And they work pretty good.

Slattery: There are a number of explanations that have been given for experimentation's low impact early on. I'll run through them, and see what you think. The first is that they have low explanatory power.

Plott: I have no idea where you got that.

Slattery: This is a characterization of experimentation in general in economics, so I guess that would go back to some of the psych experiments.

Plott: Again, I don't have any idea what anybody would have in mind when they say that. Maybe they mean applications to field experiments. Maybe they mean…

Slattery: It's the "this is a lab and doesn't apply to the real world" argument.

Plott: Well, you have to be really interesting there. That comes from this roundabout understanding of thinking about it. The lab teaches about theory. Theory teaches you about things, more complex things. What people tend to not realize is that that's what's happening. So they look at the lab and say, "what's this telling me about the US stock market?" The answer is nothing. They look at the lab and say, "what's this telling me about this antitrust case?" The answer is nothing. It's not supposed to. You shouldn't even look to that. I think those comments probably come from people who don't know what you're supposed to see. And the answer to that, by the way…it's that type of attitude that is exactly the reason that I did the barge. That's the reason we did the first antitrust with the Ethel case. That's the reason we looked at information aggregation and started looking at options and futures markets.

[brief recording break…artifact of the recording device…first portion of my question retrieved from prepared document of questions]

Slattery: So the focus in the key moments in experimental economics gaining acceptance would be…

Plott: I think those were all key moments in helping people see that it has power. I think another key moment in the growth would be this review I did for the Journal of Economic Literature where I came out with one and Vernon came out with one. I gave a series of what one learns from an experiment, how one goes about it. I also gave this argument that experiments are the real world. That's an argument. Experiments aren't modifications of the real world. There is not an experimental world and a real world.

Slattery: Yeah. They happen in this world.

Plott: They happen in this world. It's a simple case. So general theory should have to work in the simple case. That little argument right there was very powerful, and I think that turned the heads of a lot of guys like Charlie Holt. Holt wasn't doing an experiments until he read that argument. That's really where Holt gets started. He says, "oh, yeah…now I can see." I think that article had a really big impact in pushing things along. In terms of the science, there's so much.

Slattery: What about the impact of computers?

Plott: That didn't happen until the late '80s.

Slattery: So that came after…

Plott: The '80s and '90s. In fact, we're just now seeing the impact of computers. I ought to take you down and show you my lab…show you what a modern smart market looks like where we're really doing something complex. Um…where were we? Gosh. There's so many things in terms of the science. I think that the preference reversal – showing that you could reject preference theory. Really, I think that's one of the reasons you see behavioral economics being so successful because Kahneman and Tversky used that in their first publications. I think that was really important. There's another one that's really important that Dave Grether did showing Bayes' rule works. That puts subjective probabilities in there. The airport study shows that you can test these auctions, and it also shows that the core is a really interesting powerful tool in group decisions. So we had the core, we had demand and supply, we had Bayes' law, we know that preferences can be tested and rejected, we know that the institutions had big time impact both in the committee and in market stuff, we know that there's a difference in auction rules…now this is all before '78. This is all known. That's before anything happened.

Slattery: I thought committees was '82.

Plott: The committee stuff was published in '76…so that's all out. Then Al Roth starts getting involved around '79 or '80. That's a little different story. He was worried about bargaining. Vernon does a couple things – by this time Vernon's back – he does a really interesting thing on the public goods problem. He pulls together a lot of old experiments he did and called them boundaries, which had to do with monopoly and some standard stuff. So Vernon gets back, and he starts putting stuff out, so he has some old stuff he puts out in the mid '70s. Then he leaves Caltech and goes to USC and starts doing public goods experiments. That was the first time that they had tried to test [**Groves legend**?] type things and these solutions to public goods. So what that does is say, "you know, we can use these experiments in this public goods way that you guys hadn't thought about and so it's kind of a watershed." The next thing happens around '79. I'm at the university of Chicago giving a lecture, and in that lecture my students are Colin Cameron who was a graduate student at the university of Chicago then and a guy sitting in the corner – I was lecturing in the library – a guy sitting in the corner reading the newspaper drinking coffee was sham cinder. **Sham cinder** then listens to these lectures a little bit and you find him sitting at the table with the rest of us attending a class. So **sham cinder**, Colin Cameron, and Mike Johnson who was in marketing – those were the three serious students. Hillel Einhorn, who was a psychologist, but he died unfortunately, was there. Jay Rousseau, also a psychologist, was there. So those were my students. **Sham cinder** says, "I'm an accountant, and these finance guys at the University of Chicago really piss me off." "Why's that?" "Well, they say because of rational expectations, because information is automatically and immediately incorporated into the system, you don't need accounting." I knew nothing about these theories at that time. So he wanted to do an experiment to see whether that would work or not. So we worked and worked and worked and finally figured out how one could study this asymmetric information in finance, and that started

all the stuff about asymmetric information, information aggregation, prediction markets – all that whole thing comes out of there. So that's a big watershed. At about the same time, we start looking at futures markets. So Vernon and I did another paper that was quite important with a guy named **Ross**. The idea was: could markets carry over inventory from one period to another? Is it possible that the markets can carry over summer into winter? Does that kind of forward speculation work? Turns out it does. But that's the first two-period model that's run. That's also in the early '70s coming out of the class that Vernon and I taught.

Slattery: That was here though.

Plott: Oh yeah, this is '70s, this is mid '70s. Then I looked at that and said "wait a minute. What about futures markets and other kinds of things?" I'd already studied it. So I got together with Forsythe and Tom Palfrey, and we did the first serious exchange experiment with the Edgeworth box type thing with a futures market and showed that the futures market had something we called a swingback. The function of a futures market is to take information about the future and bring it to the present so the present can act as if it knows what's going to happen in the future. That means that futures markets aren't random bullshit. They aren't just gambling. They actually have a function. That's in the late '70s early '80s. All that's done here. So now we have allocation over time, asymmetric information, we have industrial organization and Nash equilibrium coming out of Ethel, we have committee works coming in the core, and Vernon has public goods. Now Vernon is roaring in by this time. This is late '70s. He goes to Arizona and gets hooked up with Arly Williams, whose interested in the Plato system.

Slattery: And that's where the computers come in.

Plott: That's where the computers come in. That was the first computerized thing beginning to look at a computerized layout. That all comes out of the university of Arizona and Vernon and Williams. They worked and worked and worked on this. Very creative. That early stuff they were doing was just so creative. So they have the first computerized electronic market showing that demand and supply works and all that stuff.

Slattery: Which allowed much greater complexity, right?

Plott: Well they had to get the rules down so precise. They had to figure out how to do that. They had a lot of problems with computers, just the technology. Dealing with the network was all new. What do screens look like? What does the feedback look like? What are the rules? Do you have an electronic book? So all this stuff was unknown until Arly Williams and Vernon started pounding it out. So that's where that comes from. About four or fives years later we get a contract to study space stations. We get the money to develop an electronic trading system, and I developed a system called MUDA double-auction, which we put on a disk and circulated. This made the computerized possibilities everywhere. I mean this little disk we put out was just used everywhere, but it allowed people to do computerized markets and multiple markets, which they weren't

able to do before. They could collect the data. That was used by hundreds and hundreds of people in the very early ‘90s. Then we went to the web.

Slattery: So computers and the web would be two key steps?

Plott: Very key steps. Local area networks and then the web. We were doing lots and lots of stuff with local area networks before we went to the web.

Slattery: And if you get the parameters right, you can simulate much more complicated markets.

Plott: Yeah, you can create a lot more complex markets.

Slattery: I guess then I’ll ask my questions about the future. Do you think it will eventually be possible to nearly replicate a market and test institutions within that or are we still going to be abstracting to the same degree with general principles?

Plott: Well…simulate a market…well the software we develop now, they are markets. We implement them and do things on them in the field. So there’s no simulation at all. It is. After a while, you learn that the experiments get bigger and more complex, and after a while, the experiment becomes the thing. We just finished auctioning off fishing rights off the coast of Australia. For the United Nations, we auctioned off natural rubber in Thailand.

Slattery: You have a pretty good idea of what happens.

Plott: We do it. We have business relationships with one of the guys who owns one of the largest auction houses in the world, and he’s out finding business and we do it. I work with Cantor Fitzgerald, biggest bond house in the world; in fact, some of their rules come straight out of what we learn in experiment. We know they work. I just say do this this this, and that goes into their software, not mine.

Slattery: So at some point, there’s no point in trying to continuously generate bigger and bigger experiments?

Plott: We do generate bigger and bigger experiments where there’s an issue. Let me just show you one…

[After lunch and tour of the lab.]

Plott: Hoggett and Friedman set up an experiment that lasts months. Very complex thing with advertising and public production and all kinds of stuff. They basically are – it’s really complex – they try to control for everything. It’s so complex that they don’t get any real results except they get one out of there that’s important but never made an impact. They show that, over time in these two person interactions, there is an evolution of strategies toward the Cafferty strategies. They see that very clearly in their data, but

it’s not in a way that would be convincing to anybody except someone like me who went back and got their data and looked at every time series. What that did for me is that it told me that I don’t want to study two person games. Between Anatol Rapoport and the sociologists and the Hoggett Friedman experiments, I can see that I don’t think that’s going to be very productive, so I never ever went that direction. But at the same time, there’s a third guy in here named Bill Riker, who is the mathematical political scientist from the University of Rochester, who starts studying von Neumann and Morgenstern’s solutions in voting things. He trained Mo Fiorinia. Now, his results never go anywhere, but he trained Mo Fiorina, who is my coauthor. Now, Mo’s instincts are fantastic. He is really good. Much of the stuff he thought about he had thought his way through because he was working with Riker. So there are several kind of mushrooms popping up here. Now all this time, Vernon’s gone, but that was where I came in to it. It was kind of a vacuum. Hoggett and Friedman had finished their experiments. Vernon was gone. Here I was all excited because I thought I could kill all my buddies’ theories with this new idea, and it turned out I couldn’t do it.

Slattery: It starts affirming things.

Plott: Yeah. And that was really important because it made me think about now that I’ve got a positive result, what do I do with it?

Slattery: that’s funny because Vernon had the same reaction. Oh yeah, I’ll demonstrate that this thing doesn’t converge, that it doesn’t work, and then it worked, and it was “oh, what do I do now?”

Plott: Yeah. Vernon never really…if you read his early writing really carefully…he never picks up this thesis that I was pushing, namely that general theories apply in general if they apply in special cases. I’m seeing that it seems to work in this simple case. Maybe it doesn’t work in complex cases. Maybe it doesn’t work in other cases. Tell me why not, and we’ll start testing that.

Slattery: So it kind of reverses the burden of proof.

Plott: So it’s the kind of reversing the burden of proof that engages the discussion. Otherwise, you’re just rejected: “that’s not the stock market. You’re studying the wrong thing.” So it’s really slippery in there. If you watch the way I write, this kind of junction is protected very jealously and really very rigorously, usually several times in a paper. Sometimes in the title. Sometimes in the middle. Always at the end…do a little preaching.

Slattery: That seems just the product of being on the frontier that if you don’t position yourself well epistemologically…

Plott: Science isn’t science unless you can tell somebody else what you’ve learned. It’s a vehicle for letting you know what I’ve learned and letting you convince yourself that you see what I see.

Slattery: “I learned that the coefficient is big on this regressor” doesn’t count.

Plott: That’s right. If I can’t communicate with you so you can check for yourself whether our experiences are the same, there’s no point to the enterprise. So this discussion is super important. It’s kind of a part of the whole thing. Somehow I think I probably have a fairly deep understanding of that. I’m not sure that all my colleagues have thought about that the same.

Slattery: That’s what I was saying earlier with the development: “oh we found a new data set, let’s run a multiple regression, talk about the coefficients for five minutes, and then we’ll do some poor job of connecting it to theory later in the conclusion.”

Plott: Yeah. I have trouble with students trying to avoid that, and they get pissed off at me.

Slattery: Yeah, I’m finding it frustrating right now because I’m trying to work on disembodied technology and openness to ideas and if that has any impact in development, and it’s very hard to get a hold of in the first place. All the literature thus far is pretty bad because what people will do is say, “this has something to do with disembodied technology and openness to ideas,” and then they’ll take international telephone call traffic and say that’s a proxy for openness to ideas and this certain kind of trade is a proxy for openness to embodied technology.

Plott: So the problem is openness and embodied have no operational meaning.

Slattery: Nothing, and then they proxy them. They figure out that the proxies are endogenous. Then they instrument the proxies. At some point you’re just looking at area and population as proxies for nothing.

Plott: Yeah, that’s something that’s very hard. You’ll watch this experimental stuff and a lot of the work is in making these things operational. What I’m theorizing about has to be operational. It’s not something I define; it’s something I point to. It’s not an abstract concept that we’re watching. It can be abstract in some theoretical sense, but the evidence is there in front of you.

Slattery: I think that the idea of working on development work – not that I think of development work as a cohesive subdiscipline – but the idea of that is appealing to me.

Plott: But there’s a lot of stuff there that is interesting and begs for – a lot of development has to do with institutional change – as far as I know, there are no principles governing how institutions will evolve, but the development guys are in there making these broad statements about “this is happening because…” I think that getting in there and trying to figure out if there are principles involved in the institutional evolution – one thing people think, certainly at Chicago, they think the principle is that institutions will always evolve

to increase efficiency. If you can change the institution to capture a few more gains from exchange, then there are resources to pay people off who are hurt.

Slattery: So you're honing it continuously.

Plott: That's true unless I want my institution to be monopolist – I want it to cut you out. So you have to be very careful in postulating these types of hypothesis. They're running around out there.

Slattery: The trouble I've had the whole time is that the theory is so – I mean, what is disembodied technology?

Plott: It's something that's a property of a mathematical model.

Slattery: Yeah, it's not embodied technology. It's defined in the negative. It's not embodied technology, which is basically defined as far as I can tell as something we could have data on.

Plott: I thought that that really was a feature of a model. Like, if it's defined mathematically, then I don't need all this language. I can look at the mathematics and define what it is in terms of relationships inside the model. It's kind of like the physicists. After a while, you only theorize about the mathematics, you don't theorize about the thing. And so you have to take that step I think, at least I do all the time.

Slattery: Get back to the thing?

Plott: You have to get it in the model, and you're theorizing inside the model. Then after a while, you start checking how good that model is as opposed to another model. So…used to be this word called conceptual realism – I think it was conceptual realism. You define something and because it's attached to a word you think it's a real thing. Kind of Wittgensteinian.

Slattery: A discursive object, something you talk into being.

Plott: Is that right? You've been around the literature.

Slattery: Yeah, through discourse you construct something and all of a sudden you have an object, you've made this thing just from talking about it.

Plott: Well you do that in mathematics. You have mathematical objects – you can. You're talking about it; you write it down. It is a mathematical object. It has mathematical properties. That doesn't mean it's a model of any thing. That doesn't mean it captures anything.

Slattery: Right, doesn't mean it's going anywhere.

[end]

Interview of Douglas Davis
Performed by Paul Slattery
Oct. 1st, 2007

Slattery:
Davis: You got your. You're trying to explain market behavior based on a []. You can't say much with dynamic models. They're too complicated. So people start using these procedures where you try to implement the theory, as they would say. So they provide full information or more recently, they would remix people after every decision. If your interested really in testing the theory. You know, if static equilibrium has people mixing according to a mixing distribution, then some kind of remixing of market participants across periods would seem appropriate. But if you want to look at those results and say from this we have surprising support of our theory in this repeated context. Well that's I'm afraid is the temptation. People do this procedure because it implements the theory, but their really talking about a static theory used to explain a dynamic context. This is the second revision of the paper. I think they'll probably take it. I'm less happy with it at the end because really what I want to say is something I could say informally.

Slattery: Were the comments on it encouraging?

Davis: Second revision usually is. What I said last time was – trying to be real political about it – results are sensitive to the procedure that you use. He wanted something more definitive than that. So I'm trying to be delicate without irritating people, and there are methodological implications of these kinds of considerations, which are waters in which I don't have a license to wade. I get pretty uncomfortable trying to make strong methodological statements.

Slattery: Why is that?

Davis: I think in this particular case – there are certain instances in which I feel better about it – but here – I don't mind saying that we need to be careful. I could point out instances in which people use these very narrow static predictions without qualification to talk about dynamic results. But I don't want to say that it's best to test IO theories in a repeated game context because it's not necessarily true. A critic could say you can never really implement a theory in a way. You don't know that people understand instructions. You don't know what they're thinking. But I don't have a clear sense – I haven't thought it through enough to know... I don't have the list of when a theory should be specified in a particular way. So I'd rather dodge the bullet this time.

Slattery: Part of why the textbook is really interesting for my project is that textbooks sort of declare that a subdiscipline exists. Writing a textbook called "experimental economics" means that everything in there is experimental economics and can be taught. Was part of the mission to sort of normalize the language or methodology? Was that difficult? You list individual choice experiments, game theory experiments, and market experiments – those three strands. Was it difficult to write a textbook that sort of spoke to all of those?

Davis: It's more difficult then it was. We talk about revising it. I think Charlie Holt probably has a better sense. The fields are so enormous now. Although, they've changed in relative importance. It wasn't too bad at the time. We probably didn't – looking at the world in 1991 when we were working on it, then market experiments were really big. Individual choice experiments were small. Behavioral economics was in its incipiency. Clearly it was there. Game theory was still a separate field apart from market experiments. Now, if we revised it, market experiments occupy a much less prominent role than they used to. Although institutional design is now probably one of the least questionable sort of "what can you do with experiments that you couldn't do before?" There's little doubt that…

Slattery: Institutional design works?

Davis: Institutional design is really useful. There's been big mistakes, but still, the stuff that Al Roth, Vernon, and Rassenti and those guys have done designing these new auctions using computers – that's unquestionably a big contribution. But then again, experiments about double-auction markets for example – this is what I do, I do posted-offer markets for my research – certainly occupies a less central place. In fact, it looked like game theory was kind of played out for a while because there's an embarrassment of predictions there. It seemed like you could change – you know, theoretically equivalent normal forms with the same game theory properties would get different results. It seemed kind of stylized. Now my sense is that developments in behavioral economics – behavioral game theory – have sort of brought that back.

Slattery: Smith – when I asked him about behavioral economics – his reaction was "I read it because it's out there, but that has nothing to do with my research agenda." Plott seemed a little hostile in a way. What is your take on behavioral economics? Plott called using the rational actor model and then falsifying that theory "fighting demons" or something like that. What is your sense?

Davis: Well I share that sense. Talk to [asem] next door. We just hired a behavioral economist. He's a really smart guy. You could talk to Dan Levine or one of these guys. There are lots of really productive guys. I just don't think we're able to build models that proceed from – if our models have to start from specific assumptions about the way everybody behaves in every circumstance, how do we abstract from it? We lose tractability. When you start doing bad psychology…but then I can't be too generic. To say that's what I think of behavioral economics is not true because there are guys like Colin Cameron or Dick Thaler who find these persistent anomalies that are strong enough

that they affect market outcomes in predictable ways. That's more interesting. But just to go and find – it's just to easy to reject rationality for example.

Slattery: All theories are false?

Davis: Yeah. It's fine. It's just not very much fun. It's not very informative to shoot down theories all the time. The question for theory is can you organize some behaviors. Theories are…I don't know what it was like in your first principles class, but theories are always – well they're not wrong – they're oversimplifications. We have to assume when we construct theories … Even if you identify some characteristics of individual behavior like adjustment processes in Cournot models, the fact of the matter is that there isn't an adjustment process, there are three or four. We could make some progress if we could incorporate heterogeneity in our models. To say I'm testing fictional play and I reject fictional play or best response is not all that useful.

Slattery: Is theory falsification one of the sort of primary utilities of experimental economics or is its something different? What do you think the field can contribute?

Davis: um…

Slattery: Or the question could be framed wrong.

Davis: No. It's a good question. I do think about it a lot. I don't think I have a good answer. The problem with the standard notions of theory falsification are the ones that I was talking about when you first came in with those papers. I think it's pretty difficult for anybody to ever get on the domain of the theory. Now I think that in a strict sense, we're not going to succeed, but then I think that it certainly can lead to a useful debate. For example, Vernon always talks about – he's got this paper in experimental economics…

[knock…talks to person at door]

Davis: What were we talking about?

Slattery: Whether theory falsification was one of the primary utilities of experimental economics or if there was something else.

Davis: I guess the short answer is probably yes. It's a complicated question. One of the things you can learn…You know, in physics for example, these debates that Vernon will elaborate on with great wit and even know the proper references. No one's ever going to accept except that you've got the right tool to test that particular theory. Clearly we can get better tools to look at things. There's an extra complication in economics in that there's this notion of individual behavior that underlies it all. I did this antitrust logit model…are you familiar with this?

Slattery: Not the particular paper.

Davis: Well no, but this is a model that the department of justice uses to anticipate the effects of mergers. It's got a logit demand curb. You assume firms are strictly in equilibrium pre-merger. With just two pieces of information – if you look at the shares, the prices of the firms, if you assume that they are strictly in equilibrium – if you just know essentially the own-price elasticity and the cross-price elasticity of the firms – it's dressed up a little differently when you use a logit model – basically with those two pieces of information or reasonable guesses about those and the assumption of strict equilibrium behavior pre-merger, you can predict the anticompetitive consequences of a merger. This looks like a sitting duck for experimental investigation.

Slattery: Particularly the strict equilibrium pre-merger.

Davis: Yeah. So I tried to look at those with Bart Wilson and we found that it didn't organize behavior well at all. It did in simple contexts – it would sort of predict outcomes, but for the wrong reasons. The reasons was that markets with a lot of what they call own elasticity, it's really inside elasticity, markets where you had real inelastic responses to your own prices or a lot of sensitivity to prices for the outside goods – these were differentiated products – you'd see a lot of price variability in those markets because the incentives for adjusting were not all that strong – unilaterally adjusting. Whereas, things tighten down and you get these really elastic responses to your own price increases. In those markets, say before a merger, if they swing way below the equilibrium, those are the markets most likely to shoot up post merger following the equilibrium. But we didn't see any evidence that people recognized any change in the strategic nature of the game, but these were markets that were conducted with 25 or 30 periods pre-merger and followed by 25 to 50 periods post-merger. Then, if we went to slightly tougher environments like where there was some simple asymmetry, it just didn't organize things at all, even for the wrong reasons.

Slattery: It just came apart.

Davis: Yeah. It just came apart. Well, what are the critics going to say about that? My prior was that I think that we're seeing – I'm not an advocate of these kinds of models because I think they're imposing more structure on the world than exists. They lead to an artificial sense of understanding. The prior that really seemed to drive behavior was this inside elasticity, you know, the general response of the market to changes in the prices of all. So if that's what predicting price swings, then we should just pay attention to that rather than pay attention to the equilibrium adjustment. Because we don't seem to observe the kind of convergence that's necessary – you know, the Department of Justice is interested in 5% price adjustments, really very small. But you know, what would the critics say about that? Well, they intensely disliked it for a variety of reasons. One it just said this model is a bad idea. But what it did lead to was that people would say "what can you say about college kids playing a game for an hour. They make 30 decisions or 50 decisions. That's not enough." Well, that lead me to say, you know I could construct a better tool here. I can't address everything, but what if I reduced the length of the trading periods to just a couple seconds – 7 or 8 seconds. Then they could play hundreds of periods. I haven't gotten back to looking at this differentiated product competition yet,

but that turned out be extremely interesting because you solve – some of the problems with posted offer markets you see with markets of short duration with repetition you fix lots of those problems. Now in some sense you can look at dynamic price adjustment processes in ways you never could before. So there is clearly this process of you try and test a theory and it's never going to be so clean that you can say this theory is rejected and you should discard it or you can come up with another theory that's going to explain the behavior, but is often the case that your thought for how could I address, that dialogue between the theorist and the person who collects the data…experimentalists can suggest alternative ways to do the theory or better tools. The tools may end up allowing you insights into other areas all together.

Slattery: Right, the actual experimental development process is educational in itself.

Davis: That's right.

Slattery: So is experimental economics a set of methodologies that's applied in several different, distinct subfields of economics or is experimental economics a subfield? One thing that struck me is that all of the textbooks and all of the history sort of put markets and game theory and individual choice together, but it seems like their motivated to do so just because experimentation is an odd methodology in economics so they lump them together. It seems like they are different research agendas entirely.

Davis: I think the perception of the people who first started doing experiments was that it was an alternative empirical tool with its limitations and strengths. And I strongly believe that its ultimate use will be as an alternative empirical tool, not as a separate subfield. There are stupid experiments and there are good ones and they are at best a compliment to the things you get from other data sources. So that's a straight answer for you there. I don't view it as progressing as a subfield of its own, in the end.

Slattery: I spoke with Vernon smith about some of its early papers. He told this really funny story about attempting to publish in the JPE a paper that confirmed that markets converged, and he was very frustrated that he had found that markets converged. His quote was something along the lines of "I naively thought that I should send it to Chicago because they like markets there, but what I found out was that they already knew that markets work. They didn't need me to tell them that." Can you talk a little bit about why you think there was resistance to experimental work early on and why the early results didn't obtain that well? I know Vernon had a lot of trouble publishing early on and sort of got turned off. For a lot of the 70s, it was only Charlie Plott doing work.

Davis: Well, I think it was such a radical proposition to have college students make decisions in two hours that sophisticated business people make over much longer time periods. I think that it was difficult at first because it was so strange. I come from the generation where this was sort of novel but it was reaching some increased consensus. What was nice about it in the mid 80s and the early 90s was the idea that "here's my methodology; it has these obvious warts right on its face. Let's talk methodology, practical methodology. Look at the assumptions associated with the data, with your

results, your empirical analysis." It clearly has lead to better methods of collecting data in all these ways. The boom field experiments for example. Vernon said, in his early papers, like the one in the AER in the '80s, clearly there are better ways to collect data. There are rewards in economics to collecting better data like there are in any other field. There are now. We see much better data collected. People are much more inventive about collecting data in the lab and out of the lab. As a consequence, I think we get better information.

Slattery: I was just talking about the sort of resistance.

Davis: I think as happens with a lot of new fields. I think it went maybe too far in the other direction. People are sort of thinking "Well it's an experiment, that seems to be accepted. We should consider this. We should publish this." It's important to have a healthy sense of skepticism. That's one of the reasons that attracted me to experimental economics, and that got a little lost. Actually, my personal perception – you know there's this big disagreement in the field now: fights over field experiments as opposed to laboratory experiments. You know, Levitt and… is it Levitt and List…that new paper that's coming out?

Slattery: I don't know.

Davis: You've heard about it, right?

Slattery: Yeah. Plott talked about field experiments vs. lab experiments – sort of, the control in a lab is the whole point.

Davis: That's right. The stuff that John List is talking about is a lot of the stuff that Vernon and Charlie talked about in the late 80s. There are many of the same conditions and you can have just as profound problems in the field as you do in the laboratory. But I sense politically there is some perhaps justified resentment to this kind of overpublication of experimental work.

Slattery: You think it's actually overpublished now?

Davis: Well I don't think it's overpublished. I think there are areas where there has been a theory that's guiding what's going on. They're looking for behavioral anomalies and it's very easy to come up with behavioral anomalies. And you find some people who seem to have very little in the way of standard economic training that are talking about phenomena that don't fit very well into the standard things that economists do. I have to be careful here because there are a lot of good behavioral economists, but there is certainly a subset of people who…I think there is some concern in the profession about where are we going here and what is this work really saying. Part of what motivates or catapults this forward is not so much a debate about field experiments vs. lab experiments but the idea that we need to revisit experiments in general. We're not being careful enough or skeptical enough about what we're doing. So the interest may not be entirely directed at…

Slattery: Herb Gintis had a humorous quote that what worried him about teaching behavioral economics was that he would attract people that hate economics. So…Plott had a lot to say about the process by which experimentation contributes to economics, and he actually said that the way that Vernon had sold his early papers was wrong. He wasn't actually simulating markets or trying to get his experiments to look as much like a market as possible. What you're supposed to do is create an experiment with very simple conditions, get the data, and from that data construct a theory and then the theory gets you from the data to the real world. Do you see experiments as generating theory, as generating sort of – he called them universal principles of economic behavior? That was sort of his answer to theory falsification. It was no, what we really ought to be doing is designing experiments that will give us data that we can build theories out of.

Davis: That's pretty clever. He might be right. You know, I'm in a field of uncertainty because I realize you can't falsify anything. I haven't thought it through. There's some potential there. If I can identify factors with reasonably parsimonious models that can reasonably organize behavior in robust ways, it seems like – to call them universal behavioral principles seems a little grandiose – but you might have something to stand on anyways. He might be right. He might be right. What does Vernon say?

Slattery: I asked him "what is the purpose of experimentation?" and he talked mostly about designer markets type stuff, market design, institutional design.

Davis: For example, he's got this stuff now with Bart Wilson on spontaneous exchange and markets? Did he talk about that?

Slattery: No. He wanted to spend a lot of time talking particularly about how he got into experimentation. His big thing was that this gave us the opportunity to focus on institutions, which he thought were really poorly theorized in neoclassical thought. And he went along and explained the progression from when he first replicated Ed Chamberlin's experiments and said well I better do it in a couple periods. This random meetings economy is really inefficient and that's probably why were not converging. He talked through that process. But he didn't seem to want to talk very much about his later work, sort of what he's doing now. He wanted to recant the history. And bass fishing trips…

Davis: With Plott? Yeah.

Slattery: Actually, I told Plott that a large portion of the history of this field could be modeled if you imported from evolutionary biology the idea of punctuated equilibrium and you took from about '62 'til about '75…all the major methodological advances are just equilibriums punctuated by bass fishing trips. He thought that was pretty funny. So, what do you think has brought about the greater acceptance of experimental work? Was it methodological breakthroughs or was it sort of force of will over time?

Davis: I think there was a perception in the mid to late 80s I think that that economics had hit a pinnacle of technical complexity. So people were modeling…a lot of these you complicated dynamic analysis of tax collections, you know. I think there was a sense that the field needed to go somewhere else. There was an opening there not so much because the field was falling at its knees from unacceptable results, but it was time to move to some alternative ideas and I think that was the best of the available candidates. There was a fair amount of luck involved. It was also the case that you could show – there were these things you couldn't answer before. There was a big wave of decentralization in markets, politically. So you had these things that previously had been regulated markets and now they could be deregulated markets or some portion of them could be operated as deregulated markets. So there was this need to talk about new institutions. Some of it's an opening. In other words, there were some questions people really wanted to know. I think with experiments you could regard institutions that didn't exist.

Slattery: That seems to be one of the clear areas where it has contributed. Neoclassical thought has failed to address institutions that well. If you can sort of create that institution in a controlled environment…

Davis: At least you can get some rough sense…where the big mistakes are…some big mistakes are.

Slattery: It's the building the theory off the data, which goes to the real world? So what should the relationship between experimental economics and experimentation in psychology be as we move forward. My sense is that the behavioral stuff was not well thought of early on because of the fact that it was done by psychologists or social psychologists whose methodology did not obtain in economics. So what should the relationship between experimental economics and experiments in psychology be as we move forward? Do you think that these methods will be imported into economists work? A lot of the papers that seem to have really obtained seem to have come from economists who have stolen portions of the methodology, not from psychologists making forays into economics?

Davis: Vernon was talking about the brain scanning that psychology was dead right. I hear rumors that the results of the MRI stuff are oversold, that we don't have quite the insight into the brain we'd like to have. Whatever insights we get come at a very, very high price. I kind of see some give and take there. I think the psychologists recognize that some of the – you know, economics tends to be very pragmatic. I've published some papers in social psychology journals, and we just talk different languages. It's much easier to follow the discourse when it's in reference to a particular optimization analysis. I just got an email from the psychology department inviting me to their seminars this fall, and I sent one to them as well. My guess is that increasingly you will see these kinds of economists; behavioral economists and social psychologists will begin to form their own departments. They'll become their own subfield as they drift away from other kinds of psychology and these economists kind of drift away from the standard allocated questions that are typically the thrust of economics.

Slattery: So, you think the markets work is solidly within the discipline as the behavioral stuff is drifting outside?

Davis: Clearly the institutional design stuff is.

Slattery: This is why it's difficult for me to take – all of the preceding work in textbooks and history says there's this subfield called experimental economics and we're not going to describe a common research program but just methodology. I see this sort of research program of behavioral starting far outside economics and drifting in and collecting some economists and bringing them back out with it.

Davis: Right. I see it filtering out. I think it's going to just be viewed as a tool. Just as there are econometricians. Clearly you will have people who do experiments among other things. I think the sensible use of experiments will be among people who stay close enough to economic problems that they won't lose sight of – you know, I'm interested in studying economic questions. There are instances of idiosyncrasies in human decision-making processes. That sounds to me like psychology. An interesting question you might want to consider if you look at other empirical tools, there are theoretical econometricians. So the question would be will the field evolve to the point where many people do experiments but will the experimentalists occupy a role increasingly as methodologists. There is a journal, *Experimental Economics*. It was designed to address…they do consider these methodological issues. It's certainly not an exclusive focus of attention. I know guys like Vernon and Charlie…more Vernon in his recent writings talks more about the philosophy of data collection and method of thought. It would be quite interesting to see whether…there are these important questions that will need to be addressed. You know, there's so much to read now that I'm probably missing something I haven't seen. I bet that there's not a huge amount of attention…Does Al Roth spend a lot of time doing this?

Slattery: I think Plott spent a lot of time thinking about this. Plott surprised me by saying "simulating markets is epistemologically wrong." He'd spent a lot of time thinking about the philosophy of science. That may be one of the issues the field has to address. Clearly we can't replicate a market, replicate the conditions, so where do we draw the line? How do we make experimentation applicable to the "real world?" Although, he didn't like that either. He said experiments happen in the real world.

Davis: Yeah. That's funny. My wife is a professor of Spanish literature here, and we went to dinner with Charlie Plott. He talked at great length about literary theory.

Slattery: So are markets a natural phenomenon? Is supply and demand a discovery rather than a social system in a particular context? Smith and Plott both seem to be on an economics as a science kick. Plott refers to studying markets "in nature."

Davis: I ignore it. I guess that would be Vernon's most recent research with Wilson on spontaneous exchange. If you just give people goods and some basic mechanism of communication, does exchange emerge? How does it emerge? It seems to me its

interesting to look at. We're talking not about the comparison between college sophomores and testing very precise theories. We're talking about the evolution of human activity that took place over thousands of years. I'm concerned as to what we could learn. I found it interesting, for example, that even in that context there are these leaders who really seem to organize markets. It's not just a spontaneous condition. There really are people who invest in the group. I haven't decided whether that's just reason by analogy or whether that's a demonstration of something. The problem of the research will be illustrated with the reticence to give any answer about your question at all. We in developed economies are ingrained with a whole set of assumptions about interactions and what's meant by words and what's meant by interaction. Outside of that context, I can't say much of anything.

Slattery: It seems an odd argument to come out. Vernon was telling me people have to learn institutions and that's why he needed several periods so people become familiar with institutions and become more effective and it converges and at the same time to say it's universal principles.

Davis: Yeah.

Slattery: Do you see your research as mainstream economics…orthodox or nonorthodox or neoclassical?

Davis: Yeah I'm a mainstream industrial organization economist who uses experiments to evaluate theories. That's mostly the people I've worked with, I think. Take Charlie Holt, he's a guy who takes models really seriously. You find ways you can modify game theory like the logit equilibrium concept he and Jakob Goeree developed does a lot to resolve the problems you have with imputing outcomes in game theory circumstances, and you can use that to build models that can still talk about human behavior. Other people are different but I see myself more in the mainstream. I want to talk about real problems in the real world.

Slattery: Early on, you mentioned that one of the things that got you into experimental economics was the skepticism. It's been interesting trying to figure that out in the markets crowd. It seems like there are very particular characteristics of somebody who got into experimental economics – particularly in early experimenters. I think the first four researchers I read about had fired something over a .50 caliber machine gun. Vernon's speech for the Nobel Prize discusses how much fun it was to test the canon on the bombers in WWII. Maybe that's because of the skepticism or because it's bold to go into a new subfield like that. Early on did you not think of yourself as a neoclassical economist and you've come to that?

Davis: No. I thought of myself as just using a different tool set. The problem is there is the skepticism in the audience…

Slattery: The skepticism is in the audience, not the character of the subfield.

Davis: Yeah. There were new insights you could potentially get but you had to think about the idea of what you were doing in general. I think that's a reason why some new areas were becoming popular because I think it's good to think about your methodology. These things in very practical ways make people consider and defend the methodological assumptions that they use. And one of my frustrations giving a paper now is I'm prepared to talk about look what's silly about what I'm doing; let's compare it. Let's weigh it compared to the assumptions I have to make with other tools. In a very practical sense I want to talk about the reasonableness of the assumptions that we use for different kinds of analysis. And I will go give papers among people who are familiar with experiments, and they'll present and I'll say "why would you do an experiment for that?" and they'll say "I'm surprised you would ask that question, you who have done experiments for so long." I'm an old man now, but this is what it was when I started and now no one wants to talk about it.

Slattery: That seems like when a methodology or subfield has truly arrived: when it becomes possible to become a normal science person in that subfield and just have a list of practices that people aren't questioning.

Davis: I always thought that was, as I explained, I thought that was what you asked with experiments. There are things it might do and it might not do, but it certainly makes us think about the assumptions we use.

Slattery: Maybe this is because you're coming from a younger generation generation. In graduate school, were people supportive of you exploring experimentation or did that come later in your career?

Davis: I kind of stumbled into it because I was doing a thesis on statistical discrimination – non-antagonistic reasons you might perceive the majority population to be of superior quality to the minority population.

Slattery: This was a thesis or dissertation?

Davis: My dissertation. One possibility was if you are sampling from populations and there both the same, but you draw your inferences about the mean from the max observation and you're looking for superior quality you see ten times more whites than blacks. You're going to hire two people or three people, and they're both going to be white, if you really are able to identify the max. If your perception of the mean comes you're your observations of the max your looking at the quality of a limited population. But, I mean, how are you going to test that? So there was this guy – a student of Vernon's – Arly Williams. He said 'why don't you have people play a game to see if they form their perceptions about the mean from the max?' I started doing that and thought it was a lot more interesting from a lot of the other things I was doing. I went from there and worked for the federal government for a while. I worked of the Federal Trade Commission. I just became more interested in questions of industrial organization and corporations.

Slattery: It would seem an ideal place to be an experimentalist.

Davis: They were very...you see at the beginning places like the Federal Trade Commission and the Department of Justice, they were very interested in people who did experiments. The reason was if you did experiments you had to think critically about what the assumptions in a theory implied. It was just that focus on 'what are the empirical consequences of what we assume and what are we really saying about how people behave?'

Slattery: The institutional focus would seem very helpful in that context.

Davis: Yeah. That's right. That's right. I don't know if they still maintain interest.

Slattery: I think they do. One of the professors that I'm doing work with was the Chief Economist at the FTC for this last year and I mentioned that I was coming up to George Mason interview Vernon Smith, and she said "that's funny, I hired him for something." Apparently that's still fertile ground.

Davis: They're still auctioning off spectrum.

Slattery: Yeah. He's not designing the new spectrum auctions. That's someone else. Plott is working on aquaculture auctioning.

Davis: It's a pity; you didn't get to talk to Colin Camerer while you were down there?

Slattery: No. It was very helicopter drop, run back out.

Davis: Colin's a really smart guy and a fun guy to talk to. Have you interviewed some behavioral economists?

Slattery: I think my focus is going to be on the markets stuff.

Davis: I see.

Slattery: because think in a lot of ways it's been sort of lumped into experimental economics and I'm trying to articulate that a lot of the markets guys see themselves as neoclassical economists who focus on institutions and see this as a methodology. I think, somewhere between the 2002 noble prize and some of the history that's been written, people have a perception that not only individual choice and market experiments but also Kahneman and Tversky are sort of part of the same…

Davis: Well, that's the Nobel Prize. Half of it went to market experiments and the other half went to Kahneman, which is another side of the story.

Slattery: To my mind it's just totally separate research agendas.

Davis: Yeah.

Slattery: So the places that have labs are like UVA, Arizona, Caltech. How do you explain where experimental labs have landed…where programs have developed?

Davis: Well. They were a function of the people who were there to start out with. I mean labs are cheap right. The space is a bigger deal than a lab. Now there's just a gazillion of them. You know places tried to buy…they were at George Mason now they're going out to wherever in California…

Slattery: Chapman.

Davis: Chapman University. I mean Harvard decided a couple years ago that they were going big-time into the experimental business, and Princeton did to. As they trends come east people say they mature. I mean there are a lot of big schools that have labs, there are lots of little schools that have labs as well. Some of them built labs for educational labs primarily for educational purpose in the first instance. Other schools…you know Appalachian State has a lot of experimentalists and they do a lot of work. They have neither location nor…Tennessee has a lab. New Mexico. Environmental stuff gets a lot of attention cause they pay attentions to unusual valuations, contingent valuations. And mechanism design is huge. Water rights are becoming an increasingly important issue. Problems of the commons. Developing new mechanism for emissions control. All these issues environmental economists are interested in. They tend to have labs.

Slattery: What did the design of computer assisted experiments mean for experimentation. Arly Williams designed one of the first ones on the PLATO system in Arizona, but when I ask people what were the methodological breakthroughs that allowed this to make significant contributions they usually point to the advent of the computer. Do you have a sample before and after the computer or were you immediately doing experiments on the computer?

Davis: Well Arly came you know cause he decided he was going to program a double auction and I thought he was crazy because they tools at the time were so rudimentary. When I first came in the '80s here I some network programming and I knew very little about programming. You know I made it work. Ended up with a posted offer market with discounting opportunities. I was relying on delays in the mechanism to keep the thing from crashing. Prior to the TCP/IP protocol that they use on the internet, you had this problem of trying to keep the machines that were trying to listen from interfering with the people who were sending the messages because we didn't have protocol at the time. Every time you would call, if it was busy, you would hang up and wait an amount of time that was an exponent of the first call. If everybody was trying to call at the same time, it was okay for standard exchanges or printing documents, but if it was just continual calls, it would just bring the whole thing to its knees. I think programming…you know what Arly did with the double auction and also John Ketchum did a posted offer certainly made data collection easier, but as Vernon points, there were things you could see you could do with these things. Some calculations were made for participants so you could

have auctions for components of a good. You could bid on the components of a good and it would assemble the packages. That was just mechanism design. It was a new field that was created. You could agglomerate bids for parts or you could disassemble bids for gropus. That's just a part of what you could do. Now...like the stuff I'm doing with markets you're able to collect hundreds of periods of decisions. A single 10-second period is not the same as a single minute period, it's not very much different. So just the speed…I'm able to collect a lot more data than I was before. I'm able to collect data for example, suppose you take a posted offer market and you're interested in price setting procedures but you want to look at markets where…one of the projects I'm interested in doing this fall where production is not in advance but is made to order. What are the consequences? Macroeconomists talk about this stuff. What if that inventory lasts for some time. Analysis of those markets gets quickly complicated. There are a couple people who tried to look at that stuff by hand records. But there's an immense amount of accounting associated with saying, "well you've got a unit that's going to depreciate this much this period; you've got to figure all that out and go back and update your records figure out your profits be aware of the fact that you've got these units that are decaying here's your marginal cost schedule for new units." To get them enough experience with that kind of mechanism to try to understand what the impact is for adjustment… Computerization was great for just creating new institutions, but also allows us to get insights into phenomena we couldn't see before just because the record keeping wouldn't allow us.

[artifact of recording device]

Davis: Programming its much less expensive to design institutions than it used to be. Z-Tree I think makes things...it's a big development in that respect. There are pitfalls to that, but still in all to have people around who can design and adapt these institutions is much more expensive than any room full of computers. That's where I think you'll see experimental research gravitate toward bigger labs because only in bigger labs are they going to have the scale to support the continual appliation of these programs. Here I have kids to write code for me and to write good code it takes some time. You have a lot of people you're working with. When I actually get around to doing an experiment maybe there's some changes because I've thought about he problem more. It becomes kind of a slow process.

Slattery: Do you think IO, institutional design stuff is the future of experimental economics? Is there no future for experimental economics because behavioral economics will move this way and…will people still be writing experimental economics text books 10 years from now or will there be different methods textbooks for different fields?

Davis: I don't know. The view from the trenches here…Vernon and Charlie are up there, can see the broad field. I see experiments get incorporated more into classroom notes and textbooks, updates of books like that. I think it's time to see some effort to codify. The attitude that I get from people…I don't use experiments a lot in the class, maybe I should, but I think the time has come to say look at what fits in the core of economics, the principles. What's an important enough lesson that we should try to get people to change

their lectures to try to incorporate this and what not? And why is it important. I don't think the question has been addressed. I think that there will be a permanent area of economics that will be interested in these notions of mechanism design that will be a specialty area that will continue to exist.

Slattery: Do you get this phenomenon where – you know, everybody is an applied econometrician when they write a CV – will there be applied experimentalists and then experimental theorists?

Davis: Yeah I think. Those people will be more like engineers. Of course, people like Yan Chen for example, they're engineers. This area…what exactly I do…there's clearly stuff to do there. It's not center of the field anymore, that's for sure. Behavioral stuff, institutional design, are separate questions. I don't know what the future is going to be. Well there's behavioral game theory. There the interplay between theorist and the experimentalist has been most successful. I don't know if it will become the case that we'll get models that are increasingly well founded in human behavior, which is certainly possible. I can certainly see a place where…if what some people do as experiments is design new institutions. Other institutions naturally evolve. The question is the competitiveness of these institutions and the competitive consequences of these institutions are certainly of interest…and that's what I do. I don't have a sense of where that will be.

Slattery: Do you think the technical experimenters who essentially work on methodology will crop up where the biggest labs are now or will they crop up at standard elite American institutions?

Davis: I don't know.

Slattery: impossible question to answer?

Davis: Well, I don't' know. The guys who do history of thought seem to be scattered. You'll see them at liberal arts colleges because it doesn't take many resources to do this stuff. Whereas the guys who do experiments is increasingly going to become capital intensive and it has to be done probably mostly at big research institutions. I can think of arguments either way. I see legendary fights in departments with really smart guys that are resistant to this stuff. But the important thing is if you can still talk to each other. The legendary fights are important…not the ones that kill discourse. The idea that we're open to ideas, that we're attracted to the field, I think what we do becomes very uninteresting if we're not.

Slattery: Not open to new ideas?

Davis: yeah. It's the questions where you have the time to think about it and the most intense debates. It could go either direction. I'm a little place in the middle of nowhere and I can conduct enough of this debate with the occasionally attendance at a conference

and I don’t need to generate this consulting report very quickly. On the other hand, active disputes between important department members certainly stimulate debate.

Slattery: I’m sort of curious to find a place where that’s happened because you have Plott at Caltech where they don’t even have departments. Smith has taught at Amherst and Brown and GMU too, they’re all a little different as far as the departments go.

Davis: Right.

Slattery: I’m sort of waiting to find a place where there’s been one of these epic battles.

Davis: I bet Al Roth at Harvard. Paulfrey was at Princeton for a while but he went back to Caltech.