THE EXPERIMENTAL TURN IN ECONOMICS

A History of Experimental Economics

Assessment Committee:

Prof. Dr. K. Knorr

Prof. Dr. F.A.A.M. van Winden

Prof. Dr. S. Rosenkranz

Prof. Dr. L.T.G. Theunissen

Prof. S.G. Medema

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THE EXPERIMENTAL TURN IN ECONOMICS

A HISTORY OF EXPERIMENTAL ECONOMICS

De experimentele wending in de economie

(met een samenvatting in het Nederlands)

Experimentálny obrat v ekonómii

(so zhrnutím v slovenskom jazyku)

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ANDREJ SVORENČÍK

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Promotoren: Prof.dr. J. Plantenga

Prof.dr. M. Morgan

Copromotor: Dr. H.B.J.B. Maas

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1 The Experimental Turn of Economics

On May 28, 2010, a dozen leading experimental economists descended on Amsterdam from all cardinal directions to pause their research for two days and ponder about their past. Some had no doubt why they were invited. They were the pioneers of one of the most important transformations of economics in the 20th century. They had laid down the intellectual foundations of experimentation in economics, they had conducted landmark economic experiments, and they had initiated applied research with far-reaching consequences for both the discipline and the economy. Some of the attendees wondered why other equally accomplished researchers had perhaps not been selected. A few were even hesitant and somewhat perplexed, questioning their relevance to the history of experimental economics. Yet all were there, maybe for the last time in such a format, delighted to be together without the bustle of most scholarly meetings; and ready to take part in a witness seminar dedicated to the history of experimental economics.

In the recorded sessions, during many informal breaks, over food and plentiful wine, with wives or just with the organizers, memories of people, memories of events, and recollections of ideas and their origins were filling the rooms.[1]

The first one to speak was James ("Jim") Friedman. Chris Starmer, the moderator of the seminar, asked about his graduate studies in the early 1960s at Yale, where he defended in 1963 possibly the first experimental dissertation in economics (Friedman, 1963).[2] Then Reinhard Selten, the German 1994 Nobel Laureate for his path-breaking game theory contributions, was questioned about his path to experimental research which resulted in the first experimental publication among the participants and his very first journal article. In 1959 he published a paper with his adviser Heinz Sauermann, that investigated the

[1] The Witness Seminar on the Emergence and History of Experimental Economics took place on May 28 and 29, 2010, in Amsterdam. Its attendees were in alphabetical order – James (Jim) W. Friedman (University of North Carolina), Elizabeth (Betsy) Hoffman (Iowa State University), Charles (Charlie) A. Holt (University of Virginia), John H. Kagel (Ohio State University), John O. Ledyard (Caltech), Charles (Charlie) R. Plott (Caltech), Stephen J. Rassenti (Chapman University), Alvin (Al) E. Roth (Harvard), Reinhard Selten (University of Bonn), Vernon L. Smith (Chapman University), and Frans A.A.M. van Winden (University of Amsterdam). Reinhard Tietz (University of Frankfurt) had to cancel due to illness. The discussion moderator was Chris Starmer (University of East Anglia).

[2] Trenery Dolbear, a fellow graduate student of James Friedman at Yale, finished and defended another experimental thesis the same year. DOLBEAR, F. T. 1963. Individual Choice Under Uncertainty: An Experimental Study. *Yale Economic Essays,* 3**,** 419-469.

behavior of oligopolies, a theme similar to James Friedman's (Selten and Sauermann, 1959). During the 1960s the first community of economists systematically conducting experimental research was established around Sauermann and Selten in Frankfurt am Main. Selten and Friedman met through Austin Hoggatt (1929-2009), an economist at UC Berkeley, who from the mid-1960s operated the *Management Science Laboratory*. It was a computer-equipped facility with extraordinary malleability and cutting-edge technology but which, for reasons put forward later in this dissertation, failed to generate much subsequent research.

The only other person among the participants who had known Hoggatt personally in the 1960s was Vernon Smith.[3] He was next to speak. Smith's involvement with experimental economics is fairly well known and documented. He participated in Edward Chamberlin's classroom experiment at Harvard in 1952 and has been involved in experimental research since 1956 (Smith, 2008a). Smith is often claimed as the father of experimental economics. The 2002 Nobel Memorial Prize "for having established laboratory experiments as a tool in empirical economic analysis, especially in the study of alternative market mechanisms" has only boosted this reputation.[4] (Among many references see for instance Kohout, 1993, Lynch and Gillespie, 2002, Lee, 2004, Odean and Simkins, 2008). The initial questions at the seminar were, however, directed at his early interaction with Hoggatt, Selten, and also the experimental psychologist Sidney Siegel.

After these prepared questions, the seminar moved along four themes that I selected in advance as possible non-orthogonal bases[5] of the history of experimental economics: loosely defined topics related to the community of experimental economists; issues

[3] Charles Plott met Hoggatt only in the 1970s.

[4] In 1993 Smith addressed the issue of being the founding father by saying that he was not the only one: "(Experimental economics) occurred to many at the same time, "I'm still around, I've out-lived the other fathers, so I guess you could call me the father of experimental economics." KOHOUT, C. 1993. Smith's Work in Experimental Economics Honored. *Inside Tucson Business*, April 14 -April 20, 1993, p.3. Later in his autobiography Smith explicitly mentions Selten "I have never felt that his [Selten's] Nobel Prize in game theory should eliminate him as a contender in experimental economics. His early work paralleled mine, and he even published three years earlier than I did, if that counts for anything. He was unquestionably a father of experimental economics who was still living." SMITH, V. L. 2008a. *Discovery - A Memoir,* Bloomington, IN, AuthorHouse. Other economists who engaged early in experimental research and either spurred further research or provided key advances early on are Edward Chamberlin, Heinz Sauermann, Austin Hoggatt, Charles Plott, John Kagel, and Raymond Battalio.
Peter Bohm, a Swedish economist, is hailed as the father of field experiments. HARRISON, G. W. 2008b. Peter Bohm: Father of field experiments. *Experimental Economics,* 11**,** 213-220.

[5] In mathematics, the orthogonal bases of a Cartesian space allow us to describe any point as a linear combination of the bases. In a less precise setting, such bases describe non-overlapping, uncorrelated, or independent objects.

relating to funding; skills of experimental economists and techniques of experimental economics; and issues pertaining to the economic laboratory as the primary space of experimental research. By experimental economics I denote the field within economics that from its beginnings in the 1960s and early 1970s has continuously studied economic phenomena and theories through the application of the experimental method. However, the issue of the intellectual development of the discipline was not among the four axes. Still, it naturally permeated the seminar, being the issue closest to the participants' daily scientific practice and understanding of the history of economics; and it was not picked as one of the four organizing themes of the seminar precisely for this reason.[6]

The underlying question behind these four themes was 'How did economics become an experimental science?' Economics underwent several major transformations in the 20th century. Mathematical formalization, economic modeling, and econometrics transformed it to such a degree that two economists living a century apart would have had trouble to understand each other and practice economics in the same fashion. These transformations preceded the experimental turn and are much better understood now than they were one or two decades ago (Morgan, 1990, Weintraub, 2002, Boumans, 2011, Morgan, 2012, Düppe and Weintraub, 2014). Historians of economics have only recently begun to turn their attention to the history of experimentation in economics. One of the main contributions of this thesis is a better understanding of the transformative power of economics experiments, which I argue equals the other transformations in its importance and the profoundness of its impact on economics. All three were new methods introduced from the periphery of the profession, initially encountered resistance from it and won their advancement into the mainstream for other than methodological reasons. They resolved criticism through the application and improvement of the new method - better mathematical theory, better statistical techniques, and better experimental design[7] - and by demonstrating the practical value of the new methods.

All three aimed at introducing rigor to economics and wanted to make economics more scientific - more like the rigorous natural sciences. From the late 19th century an increasing number of economists believed that mathematics could introduce a superior

[6] The details of the many other seminar design decisions are discussed in SVORENČÍK, A. & MAAS, H. 2015. The Emergence of the Experiment in Economics: Experiences with a Witness Seminar. *In:* SVORENČÍK, A. & MAAS, H. (eds.) *The Making of Experimental Economics: A Witness Seminar.* Springer.

[7] The meaning of better in the context of economics experiments is explored throughout the thesis.

level of rigor through a compact, straightforward and tractable mathematical rendering of economic propositions, an effective way of proving them and, through mathematization, a method of drawing otherwise unattainable consequences from formal economic theories and models. The econometric ideal of the 1930s was supposed to instill rigor by combining economic theory, statistics and mathematics. Mathematics alone was deemed insufficient to guarantee "rigorous thinking similar to that which has come to dominate in the natural sciences," as stated in the Constitution of the *Econometric Society* (Frisch, 1933, p. 1). However, by the 1950s this ideal and idealized union collapsed, giving way to a division of labor between mathematical economists working on theory building and econometricians concerned with statistical work (Morgan, 1990, p.264). Both led to excess. Mathematical models on the one hand were often pursued for their elegance and tractability rather than their empirical content (Hahn, 1970, Blaug, 2003). For instance, Frank Hahn in his presidential address to the Econometric Society in 1968 stated: "The achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic states [economic equilibria] which they give no reason to suppose will ever, or have ever, come about" (Hahn, 1970, p.1).

Econometrics, on the other hand, was primarily reduced to data mining, downgraded to the subservient singular role of theory testing, and even statistical data were outsourced away from economists to statistical offices (for instance Leamer, 1983, Hendry, 1980, Leontief, 1971) To give one early example, Oskar Morgenstern complained that "there appears to be a tendency in the Econometric Society that purely abstract work, especially if it involves the use of some symbols, already constitutes qualification for office in the Society." In Morgenstern's view, Fellows "must have been in one way or another in actual contact with data they have explored or exploited for which purpose they may have even developed new methods. [8] Examples of similar complaints abound.[9]

[8] Letter to Dr. Alfred Cowles, June 9, 1953.Box 39, Oskar Morgenstern Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[9] James Heckman in his Principal Contribution section in Blaug's Who's Who stated that the subject of economics is "widely perceived to be discredited because it has so little empirical content and cares so little about developing it" BLAUG, M. 1986. *Who's who in economics: a biographical dictionary of major economists, 1700-1986,* Cambridge, Mass., MIT Press. P.324.

Econometrics was developed because of the ingrained belief that economics was debarred of the experimental method, that economists were not in a position to isolate, control and manipulate economic conditions (Haavelmo, 1944, Boumans, 2014, Epstein, 1987). Experimental economics revisited this belief, but it went further than simply introducing the practice of economic experiments. It was above all about redefining the relationship between economic theory and data. By creating data that were specifically produced to satisfy conditions set by theory in controlled environments that were capable of being reproduced and repeated, the generation of experimentalists gathered at the witness seminar sought – some explicitly, others less so – to turn experimental data into a trustworthy partner of economic theory. This was in no sense a surrender of data to the needs of theory. The goal was to elevate data from their denigrated position, acquired in postwar economics, and put them on the same footing as theory. Data gathered under the control of economists with the above desired qualities could not be avoided by theorists or explained away as irrelevant to economic theory. This attempt to reconceptualize the relationship between economic theory and rigorous experimental data lies at the heart of what I call the **experimental turn**; and the actual process of unfolding of the experimental turn with its twists and turns is the subject of this thesis. In the rest of this chapter I outline the notion of the experimental turn.

1.1 Driving Forces and Levels of the Experimental Turn

The personal experiences of the eleven witnesses and numerous other early experimentalists testify to the struggles to raise the standing of what they saw as rigorous data and make them a respectable partner for economic theory.[10] Their engagement with experiments started often quite accidentally, sometimes out of curiosity or by inspiration from previous experiments, often in psychology. Some of these episodes are described in detail in the following chapter, such as Vernon Smith's night epiphany to repeat Chamberlin's experiment to study several trading sessions and the subsequent

Another Nobel laureate, Ronald Coase, famously quipped in reference to econometric testing that "if you torture the data long enough, Nature will confess." COASE, R. H. 1982. *How Should Economists Choose?,* Washington. DC, The American Enterprise Institute for Public Policy Research.

[10] At this point I use the word experimentalist casually, but in the course of my dissertation I unpack the various elements of the way in which the identity of experimental economists emerged and evolved.

convergence to theoretical market equilibrium; Charles Plott's realization during a fishing trip with Smith that one can induce utility functions not merely over traded goods but also in the public sector, and the ensuing convergence to majority voting equilibrium in experiments with group decisions with Morris Fiorina; Raymond Battalio and John Kagel's accidental encounter with Skinnerian operant psychologists; Reinhard Selten's psychology experience at university and the meeting with Austin Hoggatt a few years later; John Ledyard's observation of Smith's ability to modify an impromptu experiment and how this change had dramatic implications for his own theoretical investigations; and many more similar examples. Such intimate experiences set in motion what I call the driving forces of the experimental turn. These four driving forces are generalizations of the motives that I encountered in interviewing over fifty early experimental economists, more than two thirds of all the experimentalists active by the early 1990s. None of my interviewees explicitly cast his or her path to experimentation in the language of driving forces.[11] The driving forces emerged as:

a) A conceptual reconfiguration of what counts as data and evidence in economics (integrity),

b) Knowing how and by whom such data are created (rigorousness),

c) A realization that experimental research is most potent when it goes in tandem with economic theory (a virtuous circle),

d) Elevating experimental data to the same level as theory (symmetry).

The driving forces resulted in an expansion of permissible empirical evidence by including data created in controlled experimental and later laboratory conditions under the personal supervision of the (experimental) economist, who did not have to rely on data gathered and processed externally, for example by statistical offices. This added an element of personal responsibility and even ownership of the experimental data that precipitated a serious view of the data and discarded the possibility that the data alone are flawed and not the theory to which they are related. The establishment of dedicated spaces for experimental research, economics laboratories, further nurtured the ability to replicate and amend experimental design. These features of experimental research

[11] So far I have interviewed only three female experimentalists – Elizabeth Hoffman, who participated in the Witness Seminar, Catherine Eckel and Rosemary Nagel.

countered objections about the reliability of experimental data, thereby strengthening the symmetry between theory and data as well as speeding up the interaction between theory/model building and empirical evidence.

Both theory and experimental data were envisioned to be equal parts of a virtuous circle. Economic theory had several functions in the experimental turn. First, the testing of theory as a starting point led to a style of continuous experimentation that encouraged the emergence of the identity of experimental economists. Second, in the 1980s various theories were examined experimentally for the first time in a process which can be aptly described as experimental imperialism. Third, and in one sense a response to questions of the relevance and novelty of experimental research, experimentalists aimed at the most recent and advanced theory with the goal of entering into conversation with theorists. Together with what the experimentalists perceived as the equal standing of experimental data and theory, this was needed to start up a virtuous circle of theory-data development. Last, but quite as important, the fact that Smith, Plott, Battalio and Kagel observed supporting evidence for economic theory - in certain experimental situations and often under much weaker assumptions than traditional theory presupposes – was a stimulus that helped them to persevere in their experimental research.

Thus far my brief discussion of the experimental turn has focused only on the individual experiences of particular early experimentalists. However, the impact of the driving forces was not felt on this individual level only. As soon as the experimentalists entered into a dialogue with theory and tried to convince other economists who had no direct experience with experiments about the merits of the experimental method, the novel type of data, and the intriguing results obtained through them, the experimental turn reverberated on both the local and experimental community levels and eventually percolated through the whole discipline of economics. The local level was constituted by the immediate environment of the emerging experimentalists - their colleagues, students, and co-authors - and soon centered on the experimental economics laboratories. As more economists were going through their personal transformations and became more geographically dispersed, the sense of a community and later an identity emerged. This also speeded up the stabilization of experimental practices and methods that in turn launched a continuous engagement with experimentation.

The communal level, in contrast to the previous two levels, can be characterized as a process of the passive reception of experimental economics. While active reception defined the community of experimental economists and influenced its internal growth, passive reception defined for the rest of the profession the boundaries, distance and image of experimental economics. The boundaries were negotiated on two fronts: publications in general economics journals and funding. Success in passive reception enabled experimental economics to join the economic mainstream, i.e. the experimental method became an accepted, if not widely used, method in economics.

In this respect, the second half of the 1980s is the crucial and hitherto unacknowledged period in the history of economics. In the course of my investigation I demonstrate that during this period experimentation stabilized as a new knowledge generating method and gained acceptance; an experimental community emerged and became institutionalized through the *Economics Science Association*; the computerized economic laboratory developed and rapidly proliferated; publications in leading journals soared; sources of funding were continuously secured; successful applied experimental projects were carried out; the experimental economists experienced a major internal dispute that consolidated their credibility in the eyes of the rest of the profession; and, not least in the line, separation and methodological tensions with the emergent behavioral economists were set in motion. In short, the experimental turn was complete on all the various levels – individual, local, communal and discipline wide – that it operated on.

1.2 Combination and Colligation

Experimental economics is part of a long, albeit so far undistinguished history of experimentation in economics. Despite the regular proclamations going back to the 19th century that economics was debarred of the experimental method, the tradition of experimentation in economics is a patchwork of numerous concurrent and concomitant developments, ranging from agricultural economics experiments in the 19th century, time and motion studies and managerial experiments in the interwar period, and measurement of utility, early game theory and experimental games in the 1950s. For instance, agricultural experiments emerged in the late 19th century (Hall et al., 1917, Venn, 1933) and had an impact on statistical experimental design (Fisher, 1935) and the subsequent probabilistic turn in social sciences in the 1930s (Krüger et al., 1987), social psychology, in particular that through the brief but influential work of Sidney Siegel and Laurence Fouraker at the turn of the 1950s entered into economics (Siegel and Fouraker, 1960, Fouraker and Siegel, 1963, Siegel et al., 1964). Edward Chamberlin's classroom demonstrations of the 1930s-1950s appear to be a singular episode, although one which directly influenced Vernon Smith (Chamberlin, 1948). Managerial experiments, time and motion studies in the interwar period (Holmes, 1938, Hart, 1943, Mayo, 1933, Taylor, 1911); experimental games and simulations in the 1940s -1960s (Cohen, 1964, Shubik, 1975b, Thrall et al., 1954);[12] the first generation of behavioral economics in the 1960s associated above all with Richard Cyert and Herbert Simon (Augier, 2005)[13] are other threads in the rich canvas of the history of experimentation in economics that predate experimental economics. The newer threads subsequent to experimental economics include the second generation of behavioral economics from the late 1980s onward (Heukelom, 2014) and an even more recent neuroeconomics movement from the turn of the millennium.[14]

What is important about these threads predating experimental economics is that in some cases, as exemplified by the interwar managerial economists, communities of researchers emerged which continuously engaged in experimental research. Furthermore, in some cases even dedicated spaces for conducting experiments – laboratories – were created. This for instance was the case for a network of experimental agricultural farms. What these two episodes lacked was a direct connection to economic theory at least in the way that economic theory was conceived at the time. Economists in the late 19th century were not interested in the dependence of various agricultural crops on the physical and biological aspects of farming. Similarly, despite pluralistic approaches in the interwar period, effects of illumination, rest breaks, length of the working day and working week, wages, food, humidity, and temperature on worker performance were not part of contemporary economic theorizing. While Chamberlin's experiments were aimed to

[12] Mary Ann and Robert Dimand in their volume on the history of game theory until 1945 discuss at length voting experiments conducted in the 1880s. DIMAND, M. A. & DIMAND, R. W. 1996. *A history of game theory Vol. 1. From the beginnings to 1945,* London [etc.], Routledge. See Chapter 5 "Lewis Carroll and the Game of Politics."

[13] Often referred to as the "Carnegie school of behavioral economics."

[14] The individual decision making experiments started by e.g. Frederick Mosteller and Ward Edwards were evolving within social psychology (Paul Slovic, Sara Lichtenstein, Daniel Kahneman, Amos Tversky) and eventually were critical in shaping the second generation of behavioral economics.

demonstrate the validity of his monopolistic competition theory, his activities were mainly solitary.

The case of experimental games and simulations in the 1940s-1960s is far more interesting. There was a community and a clear connection to game theory. However, this thread did not succeed in permanently introducing experimentation to economics due to the weak position, or, put differently, the insufficient passive reception, of game theory within economics at the time. Behavioral economics of the 1960s had a similar fate. For behavioral economists, experiments together with other approaches such as simulations were merely a proxy for their endeavor to replace and rebuild existing economic theory.

There is therefore something unique about experimental economics and the second half of the 1980s and early 1990s in particular. What was novel in the history of the experiment in economics is that all the various elements partially missing from the preceding episodes for the very first time came together in the form of experimental economics in the second half of the 1980s. A community of economists (broadly conceived) doing experiments; experiments on economic phenomena falling within the scope of economics; laboratories; and the relationship to economic theory were in various degrees and forms present in earlier episodes from the history of experimentation in economics. Experimental economics was the first to combine them all.

These elements were colligated by and through the vision of experimentation as a better source of data for economic theorizing. The *combination* and *colligation* constitute what I denote the experimental turn in economics. Experimental economics is hence the most consequential thread in the history of experimentation in economics. Subsequent threads such as contemporary behavioral economics, neuroeconomics and field experimentation benefitted from the groundwork laid down by the experimental turn precipitated by experimental economists.

Even if we accept that experimental economics is a watershed episode both in the history of experimentation in economics and the history of economics in general, would it not be more appropriate to refer to it, in Kuhnian terminology, as the experimental revolution? Kuhn in his *Structure of Scientific Revolutions* advocated the view that scientific fields, once established after a period of pre-science, evolve through cyclic phases of normal science of ‘puzzle solving’ within a current paradigm, a crisis phase instigated by an increased number of unsolved anomalies, and a revolutionary science phase in which a new paradigm replaces the old one. The obvious candidate for a normal science would be neoclassical microeconomics or rational choice theory. While experimental economics research contributed to the establishment of various choice anomalies such as the Allais paradox (Allais, 1953), Ellsberg paradox (Ellsberg, 1961), or preference reversal (Lichtenstein and Paul, 1971, Lindman, 1971, Grether and Plott, 1979), experimental economics was not the original source of these anomalies. Both paradoxes were introduced before experimental economics started slowly to emerge in the 1960s and were introduced through the study of hypothetical choices. The preference reversal phenomenon was first observed by psychologists in the early 1970s. Because they elicited choices and reservation prices either at low incentive levels or only hypothetically, the economists (and future experimentalists) Charles Plott and David Grether conducted a landmark experimental study that attempted to remedy what they considered the flaws of psychologists experimental design, but failed in their attempt to disprove the psychologists' findings. They even concluded that their failure "[suggested] that no optimization principles of any sort lie behind even the simplest of human choices and that the uniformities in human choice behavior may result from principles which are of a completely different sort from those generally accepted" (Grether and Plott, 1979, p. 623). However, neither the preference reversal nor any of the similar anomalies led to a paradigm shift in economics in the Kuhnian sense of incommensurable paradigms and disciplinary matrixes. Neoclassical microeconomics and rational choice theory still reign supreme in economics (Hausman, 1992, 230-236).[15]

Certainly experimental economics has not spearheaded any concentrated attempt at a paradigm shift in economics that would be akin to behavioral economics. In the context of this discussion it is important to note that experimental economics has two meanings. It refers to both a research method and a field within economics. The experimental method was developed, refined and perfected by a group of dedicated economists who through this process developed a shared identity of being experimental economists. But, while behavioral economics is a distinct field within economics, it has not introduced any new research method. Rather it has advocated a theoretical innovation and adjustment

[15] An excellent and thorough study by Ivan Moscati which reconstructed experiments in consumer demand theory in the period 1930 –1970 concluded that these experiments exerted only a slight influence on the developments of the time in neoclassical consumer theory. MOSCATI, I. 2007. Early Experiments in Consumer Demand Theory: 1930-1970. *History of political economy.,* 39**,** 359-402.

of neoclassical economics to account for empirical regularities often established in an experimental setting about the cognitive deficiencies of decision-making.

In this context, cliometrics calls for attention. It is associated with an econometric way of doing economic history and its emergence is often referred to as the cliometric revolution (Williamson et al., 2008). It capitalizes on the econometric ideal of connecting theory, statistics, and mathematics by creating or using historical data and has replaced the old qualitative approach to economic history which has taken a prominent part in economics from its establishment. Therefore the label 'revolution' seems appropriate.

Another candidate to consider before settling on the notion of the experimental turn is Lakatos' research program framework. Its theoretical hard core and protective belt are relevant for the history of experimental economics. Another quotation from Grether and Plott's paper on preference reversal certainly lends support to this approach: "The fact that preference theory and related theories of optimization are subject to exception does not mean that they should be discarded. No alternative theory currently available appears to be capable of covering the same extremely broad range of phenomena" (Grether and Plott, 1979, p. 634). However, Lakatos' framework, like Kuhn's, does not do justice to the various issues that the history of experimental economics was needed to account for and the term 'experimental turn' covers and organizes this ground. These are issues such as the epistemic value of experiments, the meaning and relevance of the experimental laboratory, and the significance of the passive reception by the rest of the economics profession of experimental economics, and experimental economics being viewed as both a method and a field. Using the term 'experimental turn' encompasses the combination and colligation of the experimental method, experimental economics as a research field, and also various other elements. Semantically, a turn is more fitting term to describe a transformation of economics or any science due to the introduction of a new research method, as opposed to the introduction of a new influential theory or school of thought. In Section 1.4 I discuss how my study of the experimental turn relates to our understanding of communities in the history of science. In this section I return to the issue of the transformation of sciences.

From a historiographic point of view, the notion of the experimental turn is by its nature a colligatory term.[16] Like 'Italian renaissance' or 'French revolution', the term 'experimental turn' is developed from the study of history and after having identified a particular pattern of change and examination of these patterns provides new information, insight and understanding of the constituent elements of the pattern and their interaction. The more general colligatory terms such as 'renaissance' or 'revolution' suggest that the notion of a scientific turn may be helpful in situations where Kuhnian and other frameworks are insufficient.

A group of historians recently advocated the term 'cold war rationality' that rested on the assumption that whatever rationality was, "it could be stated in algorithmic rules - whether these were strategies in game theory, the consistency specifications of personal utilities, linear programming code, actuarial formulas for clinical decisions, or cognitive representations" (Erickson et al., 2013, p. 30). More importantly, while the idea of algorithms was far from novel, "visions, theories, and devices that seem to foreshadow this or that element of Cold War rationality can be found in other times and places" (Erickson et al., 2013, p. 31). Similarly, Mary Morgan in her analysis of simulations research argued that the 1960s is an important, but unacknowledged period in the history of social sciences in America. Not because of the introduction of simulation to the repertoire of methods, but because the coalition, albeit short-lived, of experiments with models and real experiments "coalesced together as a new technology under the all-embracing single term: simulation" (Morgan, 2012, p. 302). Simulation referred to "a very

[16] Experimental turn belongs to a class of so-called colligatory terms. The nature and usage of colligatory terms or patterns is best explained in the following quotation from C. Behan McCullagh's *The Logic of History:*

> "Historians often find patterns in the past, or more strictly, in the information about the past they have acquired after studying available evidence. These are patterns formed by actions and events, and often represent changes of a certain kind. Descriptions of these patterns provide new information about the past, about the way in which actions and events related to one another. The concepts used to describe these patterns are not everyday concepts, but are concepts developed from the study of history, such as 'renaissance' and 'revolution'. They name types of pattern quite often found in history. Generally, as will be seen, they have not been defined very precisely, so the same term is sometimes used to refer to patterns of somewhat different kinds. The word 'colligation' is derived from the Latin word colligere, meaning to bring things together. Some colligatory words and phrases name unique patterns, in the same way as proper names refer to unique people or places; and some colligatory words name common patterns, just as common nouns can refer to many things (philosophers say they name classes of things). The phrase 'the French Revolution' refers to just one pattern of events, whereas the word 'revolution' is a common noun, which can be used to refer to a whole range of revolutions."

MCCULLAGH, C. B. 2004. *The logic of history: putting postmodernism in perspective,* New York, N.Y., Routledge. pp. 125-6.

broad range of practices: a variety of types of 'experiments' including people in role-playing experiments (known then as "gaming"), computation machines, probability setups, statistical data, mathematical models, and games of chance" (Morgan, 2012, p. 301). The structure of both examples is similar to the structure of the experimental turn. Various elements, often with long histories, are combined for the very first time and precisely this colligation characterizes and drives the ensuing emergence of a new field or the reconfiguration of existing ones. The case of the experimental turn was neither about replacing field or econometric research with experimental research nor about introducing the experimental method to economics. It was mainly about a claim that is deeply normative and at the same time empirical; namely, that rigorously controlled data collected in the laboratory or the field are a better foundation for economic theorizing than other types of data. It was not a 'battle' to establish experimentalism, but a struggle to (re-)establish symmetry and integrity of data and theory.

One can exploit the colligatory nature of the experimental turn and group its various elements according to shared characteristics. The following section introduces such a classification. Admittedly, it is a standard dichotomy in the history of science, but it proves particularly helpful in understanding the transformative effects of the experimental turn.

1.3 Sociological and Epistemic Strands of the Turn

To comprehend the experimental turn it may be useful to separate its two strands – one sociological and the other epistemic. Whereas the former is related to the way in which the experimental method became diffused within economics and was received by the profession, the latter deals with the way that the experimental method in economics evolved. Both strands remain connected, twisted together like the DNA's double helix, with the four driving forces of the turn – integrity, rigorousness, virtuous circle, and symmetry – as its four constituent elements. Conceptual reconfiguration of what count as data and evidence in economics (integrity) has besides the obvious epistemic dimension a sociological one also which lies in the dialogue between experimentalists and non-experimentalists as well as between experimentalists. Reconfiguration had to be accepted by the discipline, including those who had no direct experience with the experimental method; the greater part of the discipline. Similarly, asking how and by whom experimental data are created (rigorousness) is not just a matter of the proper way of experimental control and intervention, but also a matter of mutual trust and convention among experimentalists and the trustworthiness of the experimental community. The realization that experimental research is most potent when it goes in tandem with economic theory (the virtuous circle) and elevating experimental data to the same level as theory (symmetry), are not only epistemic and ontic beliefs respectively, but also reflect the ingrained hierarchical structure of postwar economics, with (general) theorists being located at the top.

In the remainder of this section I want briefly to outline how the two strands interacted, from the examples of the experimental community and its institutionalization through economics laboratories, the passive reception of experimental research, the institutionalization of the experimental community through the *Economic Science Association*, to the first-price auction controversy.

Initially, economic experiments were performed in ad-hoc places such as classrooms. Only later with a deepening continuous style of experimentation were dedicated spaces for such research – economics laboratories – created. Experimental economists' control in the laboratory has become inextricably linked to the spatial arrangements of the experimenter and the subjects, their separation, forms of communication, and flow of information between subjects and between them and the experimenter. Laboratory space thus acquired various epistemic functions (as well as sociological & ontological ones). The most important feature of economics laboratories, which advanced their proliferation, is the computer. Computerized experiments provided many advantages over pen and paper experiments. On an *operational level* they essentially substituted for the experimentalist's labor – speeding up research, allowing experiments that were too laborious to conduct without them, drawing subjects from a larger, more diverse and better-managed subject pool. Furthermore, by relying on an explicit routine or procedure in the form of computer code, the experimenters' experience-based and, often, tacit skills of running experiments were displaced. On a *conceptual level*, computerization provided experimentalists with the transformative power to design not only any existing institution but also completely new ones. Eventually many such institutions left the confines of the laboratory and were implemented beyond them.

The process of investigating the reliability and robustness of the experimental instruments (payments, instructions, subject pools, etc.), which turned them from an object of investigation into a provisionally trusted tool, had a side effect. Experiments engendered further experiments and thus started to transcend the theoretical frame in which they had been built, turning into complex question-generating machines, not simply tools for testing models. Public goods experiments with various contribution mechanisms, ultimatum and dictator games, private and common value auctions, and combinatorial auctions are among the best known examples.

The experimenter's control became inextricably linked to the special arrangement of subjects in relationship to each other (separation, communication, flow of information), to computers (screens) and to the experimenter in the laboratory. In particular, computerized laboratory experiments allowed experimenters to pursue larger and more complex experiments with a greater number of participating subjects drawn from larger and more diverse subject pools; to implement more complicated institutions; and to investigate their models. Laboratory space thus acquired various epistemic functions. However, the laboratory became also the center of a local experimental community around which it was organized. Without a continuous engagement with experimentation, there would have been no need to create laboratories, but the creation was justified since it lowered the cost of setting up a space for experiments. Thus, the continuous style of experimentation allowed the experimental method to mature and stabilize. Results and publication helped to generate funds for the establishment of more laboratories which in turn allowed experiments with computer technology to be conducted.

The most obvious example of the sociological strand are the strategies of argumentation that experimentalists employed in getting their papers published, their research funded and the practical aspect and real world applications of experimental research in economics demonstrated. Experimental economists developed and employed various technologies of argumentation in constructing acceptable scientific arguments. These were not limited to specific experimental methods and practices, but were also arguments used to convince non-experimental economists, in particular the editors and referees of top economics journals.

Once the "battle over journals" was won at the turn of the 1980s, experimentalists lost their fear of being marginalized and ghettoized by the rest of the profession and in 1998 launched the first journal dedicated to experimental research. From early on, experimental research found a strong ally in Daniel Newlon, the Economics Program Director at the *National Science Foundation*. He was able to sell experimental research as scientific and thus counter the frequent attacks on the economics program and its budget. Although the NSF significantly promoted experimental research, its funding did not usually cover all expenses, notably building laboratories and paying staff.[17] Funding for applied research proved to be crucial in this respect. Moreover, it killed two birds with one stone. Not only did applied research procure more funding for itself, it also allowed the practical value of such endeavors to be demonstrated.

The driving forces behind the experimental turn were most compellingly expressed in a pamphlet, *Economic Science Association – a Prologue,* by Vernon Smith in 1986, in which he called for the establishment of a new society of economists. The proposed association would unify experimentalists and non-experimentalists who shared and practiced a vision of economic science based on an intimate and self-reinforcing relationship between theory and the observational data generated under their control either in the field or laboratory.

This evolution culminated in the creation of the ESA. Its name bears the legacy of the aspirations of many, though not all, experimental economists in the second half of the 1980s, most compellingly characterized in Vernon Smith's *Prologue*. In this unpublished but, at the time of the ESA's inception, widely circulated document, Smith provided a very dense programmatic statement, unparalleled in its scope, about the role of experimentation in economics, the closeness of experiment and observation, the ensuing transformation of the discipline by the recovered responsibility of generating one's own data, and what experimentalists and like-minded economists should do to advance economics as an observational science.

The interaction of the epistemic and sociological strands of the turn is particularly clear in the case of the first-price auction controversy, to which I devote the whole penultimate

[17] The most important exceptions where the NSF covered the costs of infrastructure were Austin Hoggatt's Management Science Laboratory, and Raymond Battalio and John Kagel's animal laboratory. Their specificity is discussed in Section 3.1 above.

chapter of this dissertation. Looking at it through the epistemic strand, the controversy contains two overlapping debates – one about the proper way of experimental control through monetary payments and another about adjusting theory in light of countervailing evidence. Considering this episode through the lens of the sociological strand brings out the role of the Economic Science Association. It is not coincidental that the establishment of the ESA and initial phase of the first-price auction controversy took place in the second half of the 1980s at the same time. The establishment of the ESA forced experimental economists to crystallize their own experimental economics identity and also their views about their standing within the economics profession. A split ran along the lines of their being concerned with "just" a sub-field within economics or representing a transformative force of practicing economics. The latter view embodied by Vernon Smith in his *Prologue* prevailed at the time of the ESA's establishment and envisioned it as a broad gauged society of economists committed to the experimental turn and the research on first-price auctions by Cox, Smith and Walker an example of such an approach.

The controversy therefore needs to be understood also as a continuation of the internal struggles for intellectual and professional leadership and control, despite the apparent unity of the experimental economics community exuded by the ESA. In the long run the ESA eventually evolved into a market place for experimental economists. Instead of an experimental revolution, experimental economists delivered a highly influential and still growing mutation. The name of the association remains the most visible legacy of the ESA's founding spirit.

On a deeper level, the controversy contributed towards the acceptance of experimental economics in that it increased the trustworthiness of the experimental community and increased the trust in experimenters among the rest of the profession. This is slightly paradoxical when one considers that some of the motivation for and source of contention between the various camps of the controversy stemmed from not trusting what others were observing, their treatment and reporting of data, and the conclusions they were drawing. However not shying away from openly waging a "war" erased any possible suspicion of collusion among the experimental economists and thereby making experimental economics more transparent to those who did not have direct experience with economic experiments.

These brief outlines show that the epistemic and sociological strands are intertwined. The goal of the subsequent chapters is to disentangle them.

1.4 The Turn and the Scientific Communities

In dealing with the scientific communities that bring about a transformation of sciences such as that of the experimental economists, we need to look at the communities both as sociological entities with their structural operations, epistemic entities with their related ideas and practices, and as historic entities with an evolving notion of their identity, self-perception and standing within their native community. The experimental turn in economics is a case study that allows us to study from all three perspectives on scientific communities and the plethora of approaches that have been developed in line with these three notions.

Communities became focal to the history of science only in the second half of the twentieth century. Until then, perhaps first expounded to great effect by the Victorian polymath William Whewell (Whewell, 1857), the received view was to understand science as a cumulative and continuous enterprise. It was through the work of Thomas Kuhn on scientific revolutions and to a lesser degree through the work of Robert Merton on the sociology of scientists that recent generations of historians, philosophers, sociologists of science and historians of science abandoned the cumulative progress approach in favor of understanding the disruptive nature of scientific change (Merton, 1968, 1st ed. 1949, Kuhn, 1970, 1st ed. 1962).[18] With their contributions, communities or the parts that advance the change moved to the center of attention. Although Kuhn must be credited for highlighting the role of scientific communities, he was surprisingly silent on the operation of these communities - not only the way in which normal science unfolds, but also in showing how communities which develop new paradigms disseminate their ideas

[18] Kuhn in his *Structure of Scientific Revolutions* advocates the view that scientific fields, once established after a period of pre-science, evolve though cyclic phases of normal science, of "puzzle solving" within a current paradigm, a crisis phase instigated by an increased number of unsolved anomalies, and a revolutionary science phase in which a new paradigm replaces the old one.

and gain acceptance - presumably because this might differ from one community to another.[19] My dissertation attempts to address exactly this topic.

Peter Galison investigated the interaction of various paradigms and their native communities (Galison, 1997). He examined how physicists from different traditions or cultures with overlapping but not identical domains went about collaborating with each other and with engineers to develop particle detectors and radar. To describe this interaction he used the metaphor of a trading zone where the two communities create a pidgin language that affords exchanges across disciplinary boundaries. In respect to the community of experimental economists, Galison's framework appears to be suitable for investigating applied experimental research where experimentalists interact with clients from business or government who come from a different 'tradition'. However, the interaction of experimentalists with non-experimental academic economists poses a challenge to the trading zone concept. Experimental economists viewed themselves as native to the economics community and they wanted to gain acceptance for experimental research by the rest of the profession and not to develop a separate community with its own paradigm. For this reason, the interaction took place primarily in economics journals, the most important outlet for economists in the second half of the 20th century, where experimental research was submitted. This process is examined in detail in Chapter 5.

In Section 1.2, on the combinatory and colligatory nature of the experimental turn of economics, I argued why the Kuhnian framework does not fit the accompanying notion of an experimental revolution. However, a number of sociological frameworks have been proposed to study scientific communities, in particular their ascent from obscure but innovative research groups to mainstream defining groups. The first was the concept of a research school introduced by J. B. Morrell (Morrell, 1972) and popularized by Gerald L. Geison (Geison, 1981). Morrell's model grew out of his investigation of nineteenth-century chemical laboratories, focusing, in particular, on the role of charismatic leaders and their disciples. Since then it has been applied to non-laboratory communities, even to communities of economists (Cord, 2011). A major drawback of the research school approach is that it provides only a checklist of characteristics possessed by the research

[19] Instead, Kuhn focuses on the incommensurable nature of succeeding paradigms and the failure to rationally account for scientific progress that inspired the work of Paul Feyerabend and Imre Lakatos, among others. FEYERABEND, P. K. 1975. *Against method: outline of an anarchistic theory of knowledge,* London, LAKATOS, I. & MUSGRAVE, A. (eds.) 1970. *Criticism and the growth of knowledge*: Cambridge University Press.

schools in various degrees and does not look into the dynamics of identity formation, or the changing position of a particular research school within the whole discipline and among competing schools (Geison et al., 1993, p. 236).

A variety of sociological frameworks have been proposed to address the life cycle of academic groups. For instance, Mullins' four-stage theory of research groups was originally developed to explain the evolution of the so-called Phage Groups in molecular biology and was later used as a conceptual framework for studying the history of US sociology in the 20th century (Mullins and Mullins, 1973). More recently it was applied in the history of economics (Medema, 2011).[20] Other frameworks include the collaborative circles approach (Farrell, 2001) and that of scientific intellectual movements (Frickel and Gross, 2005).[21]

These various conceptual frameworks for scientific communities have the same shortcoming. They do not give much leverage on intragroup interaction and differentiation; they presuppose the existing identity of a new group. But these shifts are the reason why such groups differentiate themselves from the rest of their native communities and also why we actually talk about conceptual transformations of science, scientific revolutions, or turns. Therefore, as well as the notion of scientific communities as sociological groups, we can add that these communities may be defined by their

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[20] The more traditional line of inquiry focuses on the evolution of scientific thought; in the field of the history of economics various schools of thought have received detailed treatment. More recently several volumes on communities of economists have appeared. Sociological analysis was provided by Marion Fourcade in her book *Economists and Societies* (Princeton University Press 2009). Roger E. Backhouse and Philippe Fontaine edited the annual supplement of the History of Political Economy (2010) entitled *The Unsocial Social Science? Economics and Neighboring Disciplines since 1945*, in particular their *Conclusions: The Identity of Economics—Image and Reality*. pp. 343-351. In addition see the first issue of the same journal in 2011 on *Intellectual Communities in the History of Economics* edited by Evelyn L. Forget and Craufurd D. Goodwin, their introductory essay in particular.

[21] The experimental turn and the community of experimental economists blends various aspects of all three approaches, but is not uniquely described by any single one. Experimental Economics was not established around a programmatic statement. Smith's Prologue appeared only when a community was in place. The induced value theory or the seminal 1982 paper on experiments as microeconomic systems provided intellectual stimuli but was not the impetus for organizational efforts, or efforts to gain recognition. The scientific intellectual movement framework allows us to highlight the great lengths to which experimental economists went to gain resources. However, it fails to account for such stealth: mostly avoiding open confrontation with the rest of the profession, framing its discourse in terms of continuity and cooptation; and being folded into the economics mainstream. Frickel and Gross mention in their conclusions that their framework needs to explore forms of movement that do not easily fit as the case of "stealth SIMs [Scientific Intellectual Movements]," which pursue change while emphasizing continuity, and cooptation, in which the language of the movement is folded into mainstream discourse without affecting real change." FRICKEL, S. & GROSS, N. 2005. A General Theory of Scientific/Intellectual Movements. *American Sociological Review,* 70**,** 204-232. Another interesting aspect that SIMs and Moulin share is the emphasis on the finiteness or episodic nature of groups - not only when they fail, but also when they succeed.

shared ideas, practices, norms, and beliefs. Such an epistemic understanding of communities can be traced back (again) to Kuhn, together with Ludwig Fleck's notion of thought collectives that was rediscovered in the late 1970s (Fleck, 1979 [1935]). Kuhn's work engendered also the very influential research line of social constructivism in the history of science. It rests on the importance of human and social elements in the production of scientific knowledge, which has stimulated three broad lines of research.

The first line emulated Kuhn and in great detail studied paradigm shifts, broadly conceived, in fields as varied as geology, Darwinism, and particle physics (Rudwick, 1985, Secord, 1986, Pickering, 1984, Collins, 1985). As already explained, my thesis does not pursue the Kuhnian framework. Nevertheless, experimental economics provides a case study of a macrohistory depicting how economics made the transition from a non-experimental to an experimental discipline.

The second line of research, in contrast, has focused on individual interaction within various epistemic communities. These microhistories have advocated the view that science is a local phenomenon with scientists acting and interacting in their social and material environment.[22] Perhaps the most famous and influential example was that of Bruno Latour and Steve Woolgar at the end of the 1970s (Latour and Woolgar, 1986, 1st ed. 1979). In *Laboratory Life: The (Social) Construction of Scientific Facts* they introduced and reinterpreted the laboratory as a privileged space of knowledge production.[23] They problematized the different roles of individual members of a research team and showed how their activities represented and, through inscriptional devices, were involved in the production of scientific facts. While my study does not provide an ethnographic analysis of the workings of an experimental economics laboratory, the laboratory as a dedicated space for knowledge production is a key theme of Chapter 3.[24] My thesis adds to both micro- and macro-histories by analyzing the links from the adoption of an experimental method on the individual, local and communal levels to change on a macro, discipline-wide level.

[22] Kuhn in the second edition of the *Structure of Scientific Revolutions* elaborated that revolutions take place in rather small communities of scientists, often including no more than a few hundred.
[23] The second edition's subtitle omitted the word 'social' from the subtitle "social construction of scientific facts." This reflected Latour and Woolgar's shift from the perspective of social constructivism towards the actor-network theory, which was developed by Latour and Michel Callon in the 1980s.
[24] Hence with its emphasis on how contemporary scientific knowledge is produced through socially complex, collective activities it is more akin to a recent book on the emergence of nanotechnology. MODY, C. C. M. 2011. *Instrumental community: probe microscopy and the path to nanotechnology,* Cambridge, MA, The MIT Press.

The third line of research, apart from the influence of Kuhn, was stimulated by another work. Ian Hacking's *Representing and Intervening* illustrated how experimentation often has a life independent of theory and argued that a sound philosophy of experiment provides compelling grounds for scientific realism (Hacking, 1983). Its influence has been substantial. Theory ceased to dominate philosophical and historical thought about the sciences; social, philosophical, and historical studies of experimentation with special emphasis on the laboratory, experimental practice and instrumentation began to be conducted (Knorr Cetina, 1999, Kohler, 1994, Kohler, 2008, Livingstone, 2003, Shapin and Schaffer, 1985, Shapin, 2010). Because the literature has grown tremendously and yet has not addressed economics, my dissertation only scratches the surface in this respect. This is because I am more interested in triangulating the concept of scientific communities as epistemic, sociological and historical groups and the varying scope (individual, local groups, communities, the scientific field) that any transformation of science involves.

The transformation of economics to an experimental discipline does not completely fit the answers provided so far by historians, sociologists of science and STS scholars. Analogies to the histories of the experimental natural sciences or psychology are equally deficient. All these disciplines became experimental one or more centuries before economics did. However, this may actually be an advantage. The recentness of the experimental turn, the richness of the available or soon to be available resources are going to provide fertile ground for issues of great interest to scholars in a variety of domains. The following section explores how the recentness of the experimental turn affects the available sources for writing its history.

1.5 The History of the Experimental Turn

The recentness of the experimental turn compared to the other transformations frames the way in which its history can nowadays be written. For me, it was necessary to amass a vast trove of untapped sources ranging from archival collections, oral history interviews to the witness seminar that together would allow the history of experimental economics to be chronicled, synthesized, and contextualized.

In regard to archival sources, I had privileged access to the (almost) complete extant papers of Charles Plott, Elizabeth Hoffman, and Al Roth, had access to some of the files of Mark Isaac, John Ledyard, Tom Palfrey, Frans van Winden, Arthur Schram, and was the first to examine some of the private files of Edward Chamberlin and Sidney Siegel. Over four dozen experimental economists and related figures were extensively interviewed. The methodological issues stemming from the closeness to the participants is discussed in full detail in Appendix, 'Distance and Sources in Writing Contemporary History'. While preparing for the Witness Seminar, I conducted a series of interviews leading to a large scale oral history project aimed at capturing the memories of all of the experimental economists active before the 1990s. Over the past five years I have talked to over two thirds of my target group. A full list of interviews can be found in Section 9.2.

Apart from these primary sources, a number of internal and external historical accounts are available. Whereas none refers explicitly to the experimental turn, they are nevertheless helpful in understanding it both historically and historiographically. A compounding problem of writing a history of experimental economics is the lack of a landmark publication of the same rank as Keynes' *General Theory*, von Neumann and Morgenstern's *Theory of Games and Economic Behavior*, or equally agenda setting papers such as Kahneman and Tversky's P*rospect Theory*, or Lucas's *Econometric Policy Evaluation: A Critique*. Instead of a canonical history of before and after such a landmark publication, the experimental economics historiography has grappled with identifying and selecting various strands of experimental research and molding them together into a narrative of accumulation with typical recurring tropes of the precepts of economic experiment. Before delving into this matter more deeply, let me now first survey the available internal accounts and then turn to the research by historians of economics.

1.5.1 Internal Accounts

A number of noteworthy personal accounts exist, but those by Selten and Smith stand out. The first attempt at a partial and personal history of experimental economics which went beyond a mere summary of chronologically ordered research results or a literature review was penned by Reinhard Selten and Reinhard Tietz in 1980 (Selten and Tietz, 1980). The occasion was Heinz Sauermann's 75th birthday and the goal of the paper was to describe Sauermann's leading role in the emergence of experimental research in

Germany from the late 1950s. The paper was written in German and therefore, unfortunately, has not been cited in subsequent historical works. [25] A piece that in contract garnered a wide readership is Vernon Smith's recollection for the purposes of the 1991 History of Political Economy annual conference, *Toward a History of Game Theory*. In his paper *Game Theory and Experimental Economics: Beginnings and Early Influences,* Smith recounted his path to experimentation and also the developments of experimental economics until his move to the University of Arizona in 1975. The seminal moments of Smith's career were his attending one of Edward Chamberlin's class at Harvard in 1952; the preparation of a course at Purdue University when he decided to use Chamberlin's classroom market experiments and modify them; a brief, but consequential interaction with Sidney Siegel; and experimental research at Purdue in the 1960s and at Caltech in the 1970s. He also elaborated on the 1952 Santa Monica conference and the profound results of Siegel and Fouraker's work. The paper is also valuable for the personal reminiscences obtained by Smith of other pioneering experimentalists - Martin Shubik, Herbert Simon, James Friedman, and Reinhard Selten.[26] In his engaging and frank autobiography he provided further personal details of these events and extended his narrative to the late 2000s (Smith, 2008a). Smith elaborated on his decade-long period at Purdue in a separate piece that appeared unnoticed in a volume in honor of Emanuel Weiler (Smith, 1981). Moreover, a number of interviews with Smith have explored various facets of his life and research (Lynch and Gillespie, 2002).

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[25] An English account of the history of experimentation in Germany is provided first by Selten himself SELTEN, R. 2003. Emergence and Future of Experimental Economics. *In:* GALAVOTTI, M. C. (ed.) *Observation and Experiment in the Natural and Social Sciences.* Boston: Kluwer Academic Publishers. and then in a Festschrift in honor of Selten's 80th birthday, entitled *The Selten School of Behavioral Economics,* which includes personal accounts of early German experimental economists such as Otwin Becker, Reinhard Tietz, and Horst Todt, who were all members of the first experimental economics community in the world in Frankfurt in the 1960s. OCKENFELS, A. & SADRIEH, A. 2010. The Selten school of behavioral economics a collection of essays in honor of Reinhard Selten. Berlin; Heidelberg: Springer.
Another look into European developments is provided in Frans van Winden's account of his foundation of the Amsterdam experimental research group at the beginning of the 1990s. VAN WINDEN, F. 2007. 'Economie in beweging' Experimentele en politieke economie. *In:* POLAK, M., SEVINK, J. & NOORDA, S. (eds.) *Over de volle breedte Amsterdams universitair onderzoek na 1970.* Amsterdam: Amsterdam University Press.

[26] Smith wanted to add to his piece Plott's remarks on the early period before 1975. However, he received them only after the conference and because Smith's paper was already too long the editor E. Roy Weintraub did not let him include them. Box 82, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. Plott's personal account can be found in the introduction to his collected volumes. PLOTT, C. R. 2001. *Collected papers on the experimental foundations of economics and political science,* Cheltenham, UK; Northampton, MA, USA, Edward Elgar.

In addition to Smith's autobiography, Ross Miller's *Paving Wall Street: Experimental Economics and the Quest for the Perfect Market* is another book that gives insights into the growth and the active and passive reception of experimental economics. Miller attended Plott and Smith's course at Caltech in the 1970s and later co-authored papers with both (Miller, 2001). This book combines a history of both finance and experimental economics with insights into how these two fields work and have interacted. He contextualized theory and the applied research related to experimental work on bubbles and asset markets.

Around the same time as Smith was preparing his article for the volume *Toward a History of Game Theory,* Alvin Roth was editing the *Handbook of Experimental Economics* with John Kagel. He realized that "contemporary experimental economists tend to carry around with them different and very partial accounts of the history of this still emerging field." His 1993 paper in the *Journal of the History of Economic Thought* attempted "to merge these "folk histories" of the origins of what I am confident will eventually be seen as an important chapter in the history and sociology of economics." (Roth, 1993, p. 184). Unlike Smith, Roth did not provide his own "folk history" about the way in which his operations research and game theory background had led to experimental work. Roth captured his decade long initial collaboration with Keith Murnighan, a social psychologist, in a separate piece (Roth and Murnighan, 2004). Rather, he identified three experimental and one game theory thread that framed the development of experimental economics in the post-WWII period. These were experiments that tested theories on individual choice, game theory inspired experiments, and industrial organization experiments spurred on by Chamberlin.

Roth speculated why experimental economics had appeared only after WWII. Without acknowledging that there is a longer history of experimentation in economics, he implicitly suggested why its other strands failed to prompt an experimental turn. He saw the reason in the emergence of game theory. After all, many early experimenters were also game theorists. It "brought to economics a kind of theory that lent itself to experimental investigation, and in some cases demanded it. … This concern with the "rules of the game," the institutions and mechanisms by which transactions were made, together with precise assumptions about the behavior of individuals and the information available to them, gave rise to theories that could be tested in the laboratory" (Roth, 1993, p. 201). The importance of the relationship between experimental work and theory acknowledged by Roth is crucial for understanding the experimental turn. However, his story is devoid of the institutional developments of the experimental economics community or the twists and turns in the development of the experimental method. Therefore it is unsurprising that in Roth's assessment, experimental economics developed cumulatively. "The end of the 1950s thus left the experimental enterprise in economics on solid foundations. The 1960s were a decade of steady growth, and the first reviews of economics experiments began to appear" (Roth, 1993, p. 202). While his narrative ended with the 1960s, the abovementioned *Handbook* extended the history to the present. "The 1970s brought further growth, including growth of research support" mainly from the NSF and regular meetings that brought together various experimental groups both in Germany and the USA. The growth since then had been "explosive, and with this growth came the accompanying signs of having become a 'mainstream' subject." In fact, he was obliquely referring to a heated debate surrounding the first-price auction controversy that he was part of at the time of writing (discussed in Chapter 6 below).

Daniel Friedman and Shyam Sunder posed a number of deep historical questions about the history of experimental economics. "Why has the experimental tradition been so late to emerge in economics? … Why were innovators able to develop new techniques in the 1960s and 70s and not before? Why did mainstream economists begin to acknowledge the relevance of laboratory experiments in the 1980s and not even later?" (Friedman and Sunder, 1994, p. 121) While acknowledging that they were not professional historians, they saw the answers in that various microeconomic theories "matured to the point that they could provide serious alternatives to one another as a foundation for understanding economic phenomena. Microeconomists by the late 1960s often had to choose among alternative equilibrium concepts (e .g., competitive equilibrium, Nash equilibrium, and the core) before they could begin to interpret field data" (Friedman and Sunder, 1994, p. 123). Alternative theories are equally plausible a priori and thus empirical evidence must be marshaled and laboratory experiments seemed the most appealing. They also emphasized the role of early game theory experiments. "Experimentalists were able to seize the opportunities created by progress in economic theory because of earlier laboratory work especially in decision theory and game theory" (Friedman and Sunder, 1994, p. 124). They were in large part echoing what they learned from Charles Plott in private communication. Plott's development of his understanding of the experimental method appeared later, in the introduction to the three volumes of his collected papers (Plott, 2001).

In summary, we can see that the internal accounts contain not only personal reminisces of experimentalists, but also various elements such as the crucial role of the relationship between theory and data viewed through the lens of experiments; these all support my argument about the experimental turn in economics. However, the developments are viewed cumulatively: experimental economics was spun from a variety of threads and game theory appears to have had a particularly important role. The stories lack any comprehensive coverage of all the developments and underlying epistemic and sociological transformations – but this is not an indictment; rather it confirms the nature of internal accounts. Next I turn to external accounts that are more distant from the subject matter, but have different limitations.

1.5.2 External Accounts

Francesco Guala was probably the first and only writer to mention that economics underwent a "real 'laboratory revolution' " or a "laboratory turn" (Guala, 1999, pp. 7-9). His influential work has been more methodological than historical. The most important exception is his entry on the history of experimental economics in the *New Palgrave* encyclopedia, where he summarized the state of historiography on the history of experimental economics:

> "A proper history of experimental economics is yet to be written, and one challenge faced by historians of the discipline is its strikingly interdisciplinary character. The rise of experimental economics takes the form of several, partly independent and partly intertwined threads that can be brought under a single coherent narrative only with difficulty. It is partly for this reason that most of the existing historical literature consists of personal recollections or reconstructions of individual trajectories rather than of a collective enterprise" (Guala, 2008a, p. 152).

Guala believed that the rise of experimental economics in the 1970s and 1980s coincided "with the slow exhaustion of general equilibrium theory, the turmoil in macroeconomics, and an increasing disillusionment about econometrics," which allowed experimentalists to capitalize on the earlier experimental work and fully unfold "one of the most stunning methodological revolutions in the history of science" (Guala, 2008a, p. 156 and p. 152). While I concur with Guala's broad assessment, I hope that my dissertation delivers the missing rich history that he requests.

An excellent and thorough study by Ivan Moscati reconstructed experiments in consumer demand theory in the period 1930-1970. He ended his paper with an analysis of why these experiments exerted only a slight influence on the developments in neoclassical consumer theory in this particular period. Moscati asserted that it "was not due to unawareness on the part of the economics profession" since many key theorists at the time were involved in them and therefore "academic, social, political, or economic factors" should be taken into account (Moscati, 2007, pp. 392-3). However, Moscati rather favored a purely epistemological rationalization. Neoclassical economists, he conjectured, were skeptical of the reliability and epistemic value of experimental data, in light of the possible experimental failure of auxiliary assumptions, in particular, on the lines of the Duhem-Quine problem. This is naturally a very interesting hypothesis that I investigate in the context of my theory-data symmetry claim, but also through an account of the reception of experimental research by the leading journals.

Kyu Sang Lee is the only historian of economics who has appraised the contributions of Vernon Smith.[27] In his dissertation and subsequent work he has identified three themes - rationality, minds, and machines - to describe how they influenced Smith at different stages of his research (Lee and Mirowski, 2008). His recent work has moved to the history of the experimental economics community and its relationship to mechanism design, a topic that is not covered in my dissertation.

The only work that hints at the long tradition of experimentation in economics and its changing nature is a collected volume *The Experiment in the History of Economics* edited by Philippe Fontaine and Robert Leonard (Fontaine and Leonard, 2005). It contains several historically rich essays covering a broad spectrum of topics - socio-economic experiments proposed by Ernest Solvay and Knut Wicksell; thought - and performed -

[27] Bergstrom and Eckel, two economists - the latter a prominent experimental economist - provide the economists' perspective on Smith's work. BERGSTROM, T. C. 2003. Vernon Smith's Insomnia and the Dawn of Economics as Experimental Science. *The Scandinavian Journal of Economics,* 105**,** 181-205, ECKEL, C. C. 2004. Vernon Smith: economics as a laboratory science. *The Journal of Socio-Economics,* 33**,** 15-28. The best summary of Smith's career, in my opinion, not only spanning his experimental work but enriched with personal recollections is Mark Isaac's brief piece. ISAAC, M. R. 1996. Vemon L. Smith. *In:* SAMUELS, W. J. (ed.) *American economists of the late twentieth century.* Cheltenham, UK; Brookfield, Vt., US: Edward Elgar. Isaac was a graduate student of Plott at Caltech and later a long-standing colleague of Smith in Arizona.

experiments in Hayek and Morgenstern; experimental economic games and influences from game theory. Innocenti, one of the contributors to the volume, also separately investigated the game theory connection to experimental economics (Innocenti, 2000, Innocenti, 2004). Leonard, a decade earlier, looked at bargaining experiments in economics and psychology (Leonard, 1994).

Floris Heukelom's recent book examines how the research of two psychologists, Daniel Kahneman and Amos Tversky, led to the second generation of behavioral economics in the late 1980s and the 1990s (Heukelom, 2014). It is relevant for the history of experimental economics due to their intertwined trajectories, often employing experiments on the same topics but with different purposes and implications for economic theory.

Overall the current experimental economics historiography provides a stimulating foray into our understanding of the history of experimental economics. It is predominantly written in terms of economic theory and shows how theory development and theory testing advanced experimental economics. The central role is attributed to Vernon Smith. Through a contextualization and examination of the practice of various experimental economists, I challenge this literature not only through a more nuanced and richer account drawing on untapped sources, but also by shifting the emphasis on the data-theory relationship, the forces of the experimental turn in economics, and the image and identity of experimental economics community with a varied cast of characters.

1.6 Outline of the Dissertation

The thesis is divided into six further chapters. The next chapter traces how the experimental turn emerged on a personal level. The goal is not to trace the history of various prominent strands of experimental economics research, such as market experiments, oligopolies, individual choice, etc. – sometimes referred to as paradigmatic experiments (Guala, 2008b). Instead I want to present four case studies revealing in some detail the nature of the four driving forces of the experimental turn. Thereby a number of the main protagonists of the turn are introduced together with their individual paths to experimentation. The chapter concludes with an analysis of the way in which the individual trajectories intersected, resulting in an emerging community continuously pursuing experimental research.

The transition from isolated economists-cum-future-experimentalists operating in an emerging community of likeminded scholars became tethered to a specific, purpose-built location – the economic laboratory. Chapter 3 documents this evolution. I emphasize not only the linkage to community, but also how the materiality of the laboratory facilitated experimental control and the workings of the experimental engine, a continuous style of experimentation and experimental imperialism.

The foundation of the *Economic Science Association* in 1986 marks the culmination of efforts to organize and institutionalize experimental economics in North America. On a more fundamental level its founding document, the *Prologue*, is the most vocal statement of the ideas of the experimental turn – the desire to reconceptualize the relationship between economic theory and data gathered in controlled conditions in the laboratory or the field under the supervision of economists. Chapter 4 further follows the history of ESA and its internationalization. In particular, the foundation of the journal *Experimental Economics* resolved the longstanding issue of the fears of experimentalists that they would be ghettoized if an independent journal were pursued.

Indeed, the publication success of experimental research reflected the image and standing of experimental economics among the rest of the economics profession. Chapter 5 therefore zooms in on the issue of the passive reception of experimental research – that is, how it was viewed and evaluated by journal editors and referees who did not have any direct experience with the experimental method. For this purpose various publication strategies employed by experimentalists as documented through the corpus of Charles Plott's papers and correspondence are reconstructed.

The penultimate chapter, Chapter 6, examines a crucial part of the experimental turn – the first-price auction controversy. This episode entailed issues related to rigorous experimental control, what it means to test a theory, and how to adjust theory in light of countervailing evidence. Furthermore it must be assessed against the backdrop of the creation of the ESA and struggles for acceptance in leading journals. Beyond this nexus of the various issues related to the experimental turn lies the issue of trust – trust in experimental data and the interpretation of experiments, mutual trust among experimentalists, and trust of the rest of the profession in the scientific conduct of the experimental economics field. By openly waging a “war,“ experimentalists erased any possible suspicion of collusion among themselves and thereby made experimental economics more transparent to those who did not have direct experience with economic experiments and effectively completed the passive reception of experimentation in economics.

The final chapter, Chapter 7, revisits the arguments elaborated on throughout this dissertation about the experimental turn in economics. Furthermore, I outline how far the generic notion of turn applies to the history of economics in particular and the history of science in general.

2 From Personal to Communal Turn

In this chapter I return to the four driving forces of the experimental turn. They are derived from over fifty interviews with early experimental economists and non-experimental economists, and also with people who were active in funding the agencies, journals and publishing houses that dealt with the experimentalists.[28] The driving forces are: 1) integrity – the expansion of the permissible type of data in economics by introducing experimental data and advocating its advantages; 2) rigorousness – the personal collection of data under controlled conditions; 3) the virtuous circle – the realization that experimental research is most potent when it goes in tandem with economic theory; and 4) symmetry – placing experimental data on a par with economic theory. Each driving force is introduced through the story of an economist who eventually, in no small part through the described experience, embarked on a path of experimental life.

Although these four cases illustrate how closely the driving forces are meshed, each is chosen so that I can introduce a varied, but not a complete, cast of characters who make repeated appearances in the course of the rest of my dissertation. John Ledyard's case deals with the data-theory symmetry driving force. It is particularly suitable in that it shows the direct transfer from an experienced experimentalist, Vernon Smith, to the theorist Ledyard. The early experiences of Charles Plott share a common element – his desire to produce the experimental data himself – since he did not trust what others observed. His case is used to highlight the issue of how and who produces experimental data - the driving force of rigorousness. The case of James Cox and his experience with the imperfections of field data reveal the attractiveness of experimental data and how this helped to change what count as data and evidence in economics. Finally, the virtuous circle driving force is introduced through Reinhard Selten's story of how the seminal concept of sub-game perfect equilibrium came about through the interaction of experiments and theoretical considerations.

The last section of this chapter looks at the interaction of early experimentalists and the driving forces. In particular, a continuous style of experimentation, with experiments

[28] For details see the Interview List in Section 9.2.

turning into question generating machines, and experimental imperialism became manifestations of the communal turn and were taken up by an emerging community of experimental economists.

Before I delve into these cases, a caveat is in order. All of them are built primarily around personal recollections that were conveyed to me decades after the events I question. The typical problems that I discussed in the previous chapter and the Appendix "Distance and Sources in Writing Contemporary History" apply to them.

2.1 John Ledyard's Data-Theory Symmetry

At the time that James Friedman, the first person to speak at the Witness Seminar, was working on his Ph.D. (1959-1963) at Yale, John Ledyard was an undergraduate student at Wabash College, a small liberal arts college in Indiana. While majoring in mathematics he got interested in economics. Thomas Munch, a graduate student from nearby Purdue University, was hired to teach a course in mathematical economics, since no one at Wabash felt comfortable teaching it. Munch suggested that Ledyard should apply for graduate school and he was accepted by Purdue. Going to grad school was not a difficult decision for Ledyard, who at the time was facing the draft for Vietnam.

Ledyard attended several graduate courses taught by Vernon Smith: Decision Theory, Optimal Control, and Investment and Production, all based on Smith's 1955 dissertation from Harvard. However, experimental economics was never mentioned during his studies (1963-1967):

> "I never heard the word experimental economics, never saw an experiment. He [Smith] never ran an experiment in class. Charlie [Plott] got there [in 1965], Charlie hadn't seen experiments before.[29] So although I was around these guys, the concept of an experiment, much less actually seeing one, didn't arise in any of our credit [courses or] in any of the graduate students' [research] at Purdue."[30]

[29] This turns out not to be correct. Plott encountered some experiments during his graduate studies in Virginia and Tullock and Buchanan's class. Charles Plott Interview.
[30] John Ledyard Interview.

Ledyard's dissertation, supervised by Stanley Reiter, was completely theoretical and dealt with optimal resource allocation mechanisms and their information efficiency, a topic now considered part of mechanism design.[31] His first job was at Carnegie Mellon, where he was hired as a mathematical economist. He was not involved in any of the simulations research or experimental games that were advanced at Carnegie by Richard Cyert, Herbert Simon, John Marsh or Trenery Dolbear (Augier, 2005).

Ledyard spent most of the period 1970-1985 at Northwestern University. It was then at the forefront of the rise of game theory with asymmetric information, which took over the lead in economics from general equilibrium theory. However, only a few years earlier, when Ledyard was in the job market in 1966 as a freshly minted Ph.D., Northwestern would not even offer him an interview, because "mathematical economists were not deemed as real economists."[32] From 1968 under the leadership of Stanley Reiter, who arrived from the disintegrating Purdue, Northwestern's new *Managerial Economics and Decision Sciences Department* started building faculty with mathematical emphasis and from the 1970s specialized in game theory in particular. "Satterthwaite studied incentives for truthful voting … Kamien and Schwartz explored incentives to innovate in competitive markets. … Hugo Sonnenschein and John Roberts examined pricing and market competition. Others, such as Roger Myerson, focused on mechanism design, or, like Robert Weber, researched the effects of private information in competitive settings (Golosinski, 2008). All of them became influential figures in mechanism design.

Ledyard's office neighbor was Theodore Groves. Their proximity helped to launch a productive collaboration, which in the early 1970s aimed at solving the "free-rider" problem. The free-rider problem is a theoretical problem present in markets with public goods.[33] Unlike competitive markets, where all consumers pay the same price for private goods, markets with public goods, according to neoclassical theory, would require personalized prices reflecting their marginal willingness to pay for the public good. However consumers face an incentive to underreport their marginal willingness to pay (which no one can check) and this leads to inefficient provision of the public good; put

[31] It later appeared in Econometrica: LEDYARD, J. O. 1971. A Convergent Pareto-Satisfactory Non-Tatonnement Adjustment Process for a Class of Unselfish Exchange Environments. *Econometrica: Journal of the Econometric Society,* 39**,** 467-499.

[32] John Ledyard Interview.

[33] A (pure) public good is a good whose consumption by a consumer does not reduce its availability to other consumers (non-rivalry) and one that no consumer can be excluded from its consumption (non-excludability).

differently, they have a private incentive to free ride on other people's contributions to the good.

The free rider problem has a long history. Until the late 1960s economists devised conditions for Pareto optimal provision. But they did not deal with the issue of the truthful revelation of private information.[34] In this respect William Vickrey's study of various auction formats provided the crucial stimulus for subsequent research. Vickrey established that each bidder in the second-price auction has a dominant strategy to reveal her/his true valuation of the auctioned item (Vickrey, 1961). Capitalizing on this insight, Edward Clark and Theodore Groves independently and almost simultaneously launched at the end of 1960s the study of demand revealing or incentive-compatible mechanisms for which there is a dominant strategy to reveal one's true demand for the public good consumption (Clarke, 1971, Groves, 1973). Such mechanisms theoretically induce revelation by charging individual agents as a function of what other agents respond and not as a function of what they respond themselves.

This theoretical insight had direct implications for Ledyard's own research. Among the topics that Ledyard felt passionate about from the 1970s onwards was the design of such mechanisms: "I was interested in what you could and couldn't do. … How do you solve externality problems? How do you get the free rider problem fixed?"[35] Economics, at the time primarily identified as a study of the allocation of scarce resources (Backhouse and Medema), did not say much about ways of allocating these resources beyond noting that they happen at the intersection of demand and supply. Clark and Groves were providing theoretical answers to the question of how to allocate them in a specific public goods setting.

> "For example, when I wrote this paper with Groves on solving the free rider problem, it was a theoretical exercise. This had nothing to do with building [them in the real

[34] Knut Wicksell first identified the problem in 1896 and proposed a set of prices that leads to an efficient allocation by reflecting only the private benefit from the consumption of the public good. Erik Lindahl in 1919 suggested that Wicksell's prices could be generated through a tâtonnement mechanism. This was later developed by Samuelson in 1954, who proposed the Lindahl-Samuelson condition for the Pareto optimal provision of a public good. LINDAHL, E. 1919. *Die Gerechtigkeit der Besteuerung*, WICKSELL, K. 1896. *Finanztheoretische Untersuchungen : nebst Darstellung und Kritik des Steuerwesens Schwedens,* Jena, G. Fischer, SAMUELSON, P. A. 1954. The Pure Theory of Public Expenditures. *Review of Economics and Statistics,* 36**,** 387-389.
[35] John Ledyard Interview.

world] – it never occurred to me anybody would ever do this. Why? Partly because it required computation. We still didn't have PC's or anything like that."[36]

By the mid-1970s theorists had concluded that it is impossible to create a mechanism for the provision of public goods that always chooses a Pareto optimum, balances the budget, and also provides a dominant strategy for the participants to reveal truthfully (Groves, 1976). The joint work of Groves and Ledyard addressed this issue. They came up with a mechanism that meets these conditions except for the dominant strategy. Their mechanism had truthful revelation only as a weak Nash equilibrium. Each consumer is required to send a message to the government, specifying his or her proposed incremental contribution to the good. The government then sums those incremental provisions to find the total proposed contributions and then sets taxes by a complicated rule. Ledyard could not imagine that this could ever be implemented (Groves and Ledyard, 1977). Then Vernon Smith visited Northwestern.

During the academic year 1976/7 Smith gave a seminar at Northwestern and the issue of the Groves-Ledyard mechanism came up. It is worthwhile to quote at length from my interview with Ledyard:

> "We were describing this mechanism and Vernon says, "You know, I can test this whether it works or not." I said, "What do you mean?" And he says, "I'll run an experiment." I said, "What the heck are you talking about? What do you do?" And so he hauls seven or eight graduate students into a classroom. He ran the mechanism and it didn't work. It didn't converge to the equilibrium. It didn't produce the outcomes the theory said it would produce. And I thought, okay. So back to [doing] theory. I don't care; this doesn't bother me.
>
> It bothered Vernon a lot because we sat around that evening talking and he says, "Oh, I know what I did wrong." And he went back the next day and he hauled the students back in the room, changed the rules just a little bit in ways that the theory wouldn't notice the difference. From our theory point of view, it wouldn't have mattered. But he changed the rules a little bit and bang that thing zapped in and converged." [37]

[36] John Ledyard Interview.
[37] John Ledyard Interview.

The difference between the two experiments was the information shared with the test subjects. The first time around, the subjects wrote down their number on a piece of paper and then Smith wrote them up on the board. Then he asked the subjects to send another message and if the messages were the same twice in a row he would stop, since that stability would be interpreted as having reached equilibrium. But the messages did not stop the first time Smith had run the experiment a day earlier.

> "The second time Vernon did it, he didn't write the individual numbers up on the board. He wrote the total. From knowing your own and the total, you could compute what the sum of the others is, which is all you needed to know. That caused it to converge because you stopped having – there's some game theoretic reasons you might expect that to happen. It never occurred to me at the time. It was consistent with some of Vernon's earlier experiments where less information is sometimes better than more when you're getting people to react. But I found this just fascinating. I thought this is just cool the way he did that. This is neat, number one. Number two, all of a sudden I have a way of maybe how this mechanism could be used. So I said, "Experiments are something I have to watch. But I'm not going to dive in and become an experimentalist.""[38]

Suddenly a deeply theoretical issue that occupied pages of *Econometrica* and was an important stimulus of what is now labeled mechanism design became an empirical issue that could inform theory and perhaps even have policy implications.[39] The fact that the experiment did not converge at the first attempt, but did at the second with a change of only one rule (the information structure available to the participants) not required by theory to make its prediction made a lasting impact on Ledyard.

> "[T]he theory didn't distinguish between those two rules, but Vernon knew how to find a rule that would lead to an equilibrium. It meant he knew something that I didn't know and he had a way of demonstrating it that was really neat. I started out actually wanting to be a physicist or an engineer and it was kind of the closest I'd gotten to that way of – the way I used to express this was it was the first time I saw data that I was going to have trouble ignoring. As a theorist up until then, the

[38] John Ledyard Interview.

[39] Ledyard views this as an early example of test-bedding of a theoretical design in a laboratory that now a few decades later is done routinely "in much the same way an aeronautical engineer will test an airplane in a wind tunnel." John Ledyard Interview.

> fact that the elasticity of demand for electricity was two was: “So what?“ The data could be wrong, a measurement error, or you've got one observation, you don't have a replicable thing. All of a sudden as a theorist I'm confronted with a possibility of having replicable data scientifically generated in a way that could really challenge my theories. You could start to distinguish between theories on grounds other than just what looks pretty. Economists were always claiming theories were bad because they were not elegant. So that was really cool. That was an important time.“[40]

This passage reflects all the four forces of the experimental turn. The experimental data were for Ledyard rigorous, (“could not be ignored”) because they were controlled and replicable. Therefore they counted as valid data (integrity) and such experimental data were for him at the same level as theory (symmetry). This gives the experimentalists a central position in the theory-data nexus. The experimentalist comes to serve as a guardian, a warrant, as we will see in Plott’s case in the next section. Paradoxically, the perceived symmetry liberated the experimental method, because it gave it focus and guidance through theory. The passage also shows the direct responsibility of the experimentalist for his/her data. This responsibility is binding, not merely in the sense of performing experiments that can be replicated, but also in the sense that experimental data are respected in future theoretical endeavors (the virtuous circle).

The implications of all these issues were not entirely clear to Ledyard immediately after Smith’s demonstration.[41] However Ledyard spent a year at Caltech soon after as a Sherman Fairchild Distinguished Scholar and participated at the 2nd Experimental Workshop in Tucson in 1979. He has been involved in experimental research ever since, following a slow start, in part impeded by his administrative jobs at Northwestern

[40] John Ledyard Interview.

[41] Ledyard soon realized that the way that Smith implemented the Groves-Ledyard mechanism was a different mechanism than the one they proposed: “It turns out he didn't really test our mechanism because it was too complex to do because it required computations that you couldn't do off the top of your head, so he changed the rules a little bit to make it easy enough to compute and to generate rewards. It was our mechanism in equilibrium, but out of equilibrium was a different mechanism. So it's not clear whether it was a fair test or not. But at the time, I mean, who knows?” This so-called Smith auction mechanism for the provision of public goods, however, became widely used in laboratory experiments. SMITH, V. L. 1979a. An Experimental Comparison of Three Public Good Decision Mechanisms. *The Scandinavian Journal of Economics,* 81**,** 198-215, SMITH, V. L. 1979b. Incentive Compatibile Experimental Processes for the Provision of Public Goods. *In:* SMITH, V. L. (ed.) *Research in Experimental Economics.* Greenwich, Connecticut: JAI Press, SMITH, V. L. 1980. Experiments with a Decentralized Mechanism for Public Good Decisions. *The American Economic Review,* 70**,** 584-599.

(Associate Dean) and Caltech (Division Chair). Like John Ledyard, many economists experienced a similar process of being captivated by experimental results and experimental method, not being completely able to articulate this experience, and wanting to fuse theory and data together in a novel way.

2.2 Charles Plott's Rigorousness

The importance of Charles ("Charlie,") Raymond Plott for the experimental turn cannot be reduced to a mere illustration of the driving force of rigorousness - the personal collection of data under controlled conditions - that I want to present in this section. Throughout the rest of the thesis, we will encounter him as a pivotal figure in developing experimental methodology, community building at Caltech and beyond, creating economics laboratories, fighting with editors and referees to get experimental papers into the leading economics journals, pioneering applied experimental research and organizing support for the NSF economics research. His influence is ubiquitous. Not surprisingly, the late James Buchanan, his thesis advisor at the University of Virginia where he graduated in 1965, called him his most important and influential student.[42]

Plott's Virginia pedigree set him on a path of public choice and axiomatic social choice theory.[43] His first job was at Purdue University and since 1971 he has been based at the California Institute of Technology. During his six years in Indiana he met Vernon Smith, who left Purdue in 1967, and a host of students who became experimentalists later on. These included John Ledyard, whom we met in the previous section, but also other economics Ph.Ds.: John Kagel, Raymond Battalio and the social psychologist Keith Murnighan. Kagel and Battalio and their economic experiments on pigeons and rats are

[42] Private conversation with the author, 13th annual Summer Institute for the History of Economic Thought at the University of Richmond June 29 – July 2, 2012. Plott's other advisor was Gordon Tullock and Ronald Coase influenced Plott as well. Coase left the University of Virginia for the University of Chicago in Plott's last year and was influential in transforming Plott's master's thesis into an article in the Journal of Law and Economics. PLOTT, C. R. 1965. Occupational Self-Regulation: A Case Study of the Oklahoma Dry Cleaners. *Journal of Law and Economics,* 8**,** 195-222.

[43] For the history of public choice see MEDEMA, S. G. 2011. Public Choice and the Notion of Creative Communities. *History of Political Economy History of Political Economy,* 43**,** 225-246. For the interaction of public choice and experimental economics, see Plott's piece PLOTT, C. R. 2014. Public Choice and the Development of Modern Laboratory Experimental Methods in Economics and Political Science. *Social Science Working Paper, California Institute of Technology,* 1383.

treated separately in the next chapter; Murnighan became a close long-term collaborator of Alvin Roth in the second half of the 1970s and the 1980s when they both worked at the University of Illinois in Urbana-Champaign.[44] I cannot do full justice to the course of Plott's path to economic experimentation, though parts of the Smith-Plott interaction, including the pivotal fishing trips, are discussed for instance in the Witness Seminar and Smith's autobiography (Smith, 2008a).

Let me therefore begin in Fall, 1972. Plott and Smith went on a fishing trip to Lake Powell, where the subject of economic experiments came up again.[45]

> "Vernon was again telling me about this idea for inducing preferences for studying an exchange economy experiment when I realized that it was possible to do experiments in public economics and public choice. In such experiments the institutions were nothing like the markets that Vernon had studied and the variables would be public goods instead of the private goods implicit in the use of poker chips. In fact, it was immediately clear to me that all of the issues of public economics could be studied by the use of laboratory methodology" (Plott, 2001, p. xi).

Excited about this idea, on the way back to Pasadena he sketched out the idea for committee experiments. A committee consisting of a several members had to decide how much of two public goods to choose. Decisions were made by majority rule. Plott's major methodological innovation in this design was the introduction of circular indifference curves. In the two-dimensional space of the two public goods each committee member was given a point at which the utility of consuming the two public goods was maximal and decreased proportionally to the distance from this point. All market experiments until then had been restricted to quasi-linear preferences.

Plott showed this design idea to his colleague Morris ("Mo") Fiorina.[46] Although a political scientist, Fiorina had some experience with running experiments. He had helped his

[44] Murnighan attended Plott's graduate voting theory course at Purdue. Keith Murnighan Interview.

[45] After an earlier discussion with Smith about demand/supply experiments, Plott thought that Smith's "results were silly, and it was clearly not demand and supply, but it must have been a Bayesian game. I thought that I could build up priors in learning to get these systems to converge away from the competitive equilibrium and, therefore, show that it was not the law of supply and demand that was working but, in fact, it was a Bayesian game." Charles Plott at the Witness Seminar.

Plott asked a Purdue student Harvey Reed to investigate this matter, which led to Reed's dissertation. REED, H. J. 1973. *An experimental study of equilibrium in a competitive market.* Ph.D., Purdue University.

advisor Bill Riker[47] "with his classical experiments on coalition formation and was familiar with the social psychological literature and the experimental methodologies used in psychology" (Plott, 2001, p. xi).

> "And it was not 45 minutes later or maybe an hour late he came back with some data. And I told this story before. He said: "Charlie, you will not believe what I saw." And I did not. And the experiment converged to the predicted majority-rule equilibrium. In fact, I had to go do it myself. And saw the same thing he did. A complete surprise." [48]

In John Ledyard's case we already saw the combination of a positive result (convergence to the theoretical equilibrium) and the utter surprise. In Plott's case the element of not witnessing the experiment itself comes to the front. After all, the positive result could have been an artifact not only of the experimental design, but also of the experimenter's actions.

Plott's experience had two consequences for him. The first became in his career a discernible thread of skepticism of experimental results. The second was a novel methodology of theory discrimination. Let me start with examples of theory discrimination and go on to the issue of distrust and surprise, since both are part of what I call the driving force of rigorousness.

The success of the majority rule theory in experiments posed a challenge.[49] "If I reported it as a positive result, no one is going to believe me. How do I go about now

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[46] Morris P. Fiorina (1946-) obtained a Ph.D. in Political Science from the University of Rochester in 1972. Before moving to Harvard (1982-1996) and Stanford (1998-), he was a professor at Caltech (1972-1982).

[47] William Harrison Riker (1920-1993) was an influential political scientist who pioneered the application of mathematical reasoning to political science and founded the Rochester school of political science. He built up game theory and social choice theory to coin a positive political theory. His students include Kenneth Shepsle, Richard McKelvey, *Morris Fiorina* and Peter Ordeshook. Riker received his Ph.D. from Harvard in 1948 and remained at Lawrence University until 1962, spending the academic year 1960-61 at the *Center for Advanced Study in the Behavioral Sciences at Stanford*. From 1963 until his death he remained in Rochester. His most important works include RIKER, W. H. 1962. *The theory of political coalitions,* New Haven, Yale University Press.; a textbook with Peter Ordeshook RIKER, W. H. & ORDESHOOK, P. C. 1973. *An introduction to positive political theory,* Englewood Cliffs, N.J., Prentice-Hall.; RIKER, W. H. 1982. *Liberalism against populism: a confrontation between the theory of democracy and the theory of social choice,* San Francisco, W.H. Freeman, BUENO DE MESQUITA, B. & SHEPSLE, K. A. 2001. *William Harrison Riker: September 22, 1920-June 26, 1993,* Washington, D.C., National Academy Press.

[48] Charles Plott Interview.

[49] The word success requires some elucidation. Most theories, for example, the majority rule, are not stochastic models and thus provide point prediction(s) of the equilibrium or equilibria without any confidence intervals for a given set of preferences. The (average) experimental data are usually not exactly on the theoretical equilibrium. Hence, strictly speaking, the theory should be rejected.

understanding how to report a positive result?"[50] The solution was not to focus on one theory and its experimental testing[51], but on several and see how well they fared. Instead of taking the theoretical prediction as the benchmark, the observed data in the experiment were taken as the benchmark and distances of various models to this benchmark determined which model was deemed more accurate. Hence, as soon as Plott could trust the experiment because he was directly involved in the data production (experimental design, execution of the experiment), he could trust the collected data. Such data were rigorous. The first study using this methodology grew out of Fiorina and Plott's committee experiments (Fiorina and Plott, 1978). In this seminal paper sixteen different theories and models drawn from economics, sociology, political science and game theory were compared to the experimental results.

Plott's work with David Grether, another colleague at Caltech, on preference reversals used the same methodology of theory discrimination (Grether and Plott, 1979).[52] At the beginning of the 1970s, psychologists Sarah Lichtenstein and Paul Slovic were first to report preference reversal in laboratory experiments. Plott recalls: "I didn't think we would see preference reversals when we actually controlled for everything. Boy, we sure did."[53] What motivated their study was that the earlier papers either had not used financial incentives at all or had used financial incentives without an effort to control for income effects. For the alleged phenomenon of preference reversal they produced thirteen methodological, psychological and economic theory explanations as alternative explanations. One of them was that experimenters were psychologists with a reputation for deceiving subjects and leading them to speculate about the true purpose of the experiments thus affecting their behavior. Along with performance-based subject remuneration, the refusal to deceive subjects has become of the basic methodological tenets of experimentation in economics. Experiments using deception could 'pollute' the

[50] Charles Plott at the Witness Seminar.
[51] Testing a theory is a standard experimental approach that is often labeled theory rejection. Plott in the introduction to his Collected Papers recalls that: "We thought that we were going to use experiments to reject the idea of majority-rule equilibrium. My idea was to kill my model as being relevant to the real world and in so doing to kill the work of all those who had been working on the mathematics of the problem. Theory rejection is a rather destructive enterprise. Nevertheless, we thought that producing such a negative result would be interesting, and easy to achieve. We did not, however, have a negative result, and thus could not take the easy shot." PLOTT, C. R. 2001. *Collected papers on the experimental foundations of economics and political science,* Cheltenham, UK; Northampton, MA, USA, Edward Elgar. p. xii.
[52] Preference reversals occur when individuals change their preferences regarding a pair of alternatives when the associated choice has been formulated differently. In the context of the Grether Plott study, the pair of alternatives was a pair of lotteries.
[53] Charles Plott at the Witness Seminar.

pool of potential subjects and hence jeopardize experimental control. This was the reason for Plott's distrust of psychologists' results. It is not that they would lack personal integrity. Rather it is that the observations and data collected by experimenters, including economists or Plott himself, might lack rigor. He reflected on this at the Witness Seminar and it is worth quoting it in full as it touches on the question of subject payments.

> "Well, I don't know about the rest of these guys [addressing the Witness Seminar participants], but I never believe any of these things any one of these guys tells me about data or experiments. I have to do it myself. Vernon was telling me [about] paying his subjects, but of course I didn't really believe that it was necessary.[54] I had run a whole series of experiments [including] some with Vernon.[55] In markets, if you don't pay the subjects a commission, that last unit is not traded. It is easy to see it. If you don't pay them anything at all, you discover that sometimes it will converge, sometimes it won't.[56,] [Experiments in which subjects are not operating with controlled incentives] are not reliable. In committee experiments, if you don't pay them, it turns out fairness just takes over like crazy. You will see the system go from a core/equilibrium to something that is quite different and sometimes quite arbitrary. So, the thing we discovered was that if you don't pay them, all of a sudden you start getting a wide variety of outcomes that we thought are inexplicable. When you do pay them, all of a sudden the structure of the models tightens up, and the models work. So, in fact, we even pushed payments up to \$500.00 or \$1,000.00 to see if the levels of the payment made a difference, and it turns out no.[57] Getting it really high doesn't make much difference. This is

[54] Comment made by Charles Plott after the Witness Seminar: "Vernon's first experiment was without payment. After that I am not sure that he ever tested the need to pay but he certainly felt it was needed. It was not really questioned. It comes up explicitly in my experiments with Fiorina because we faced a problem raising money and because our research overlapped with people who used methods for social psychology and thus no payments."

[55] Expanded version by Charles Plott: "Well, I don't know about the rest of these guys, but I typically do not trust the experiments of other people and I am even skeptical of my own until I see a healthy pattern of replication and robustness. I have to do it myself. So Vernon reported that paying subjects was important (and reported the market experiment he did). I did not fully believe it was necessary so ran a series of experiments. I actually ran some with Vernon." Witness Seminar Transcript.

[56] MILLER, R. M., PLOTT, C. R. & SMITH, V. L. 1977. Intertemporal Competitive Equilibrium: An Empirical Study of Speculation. *The Quarterly Journal of Economics,* 91**,** 599-624.

[57] The issue of the size of payments has received considerable attention, especially in the context of the ultimatum game and other bargaining situation in low income countries see for instance: ROTH, A. E., PRASNIKAR, V., OKUNO-FUJIWARA, M. & ZAMIR, S. 1991. Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An Experimental Study. *The American Economic Review,* 81**,** 1068-1095, SLONIM, R. & ROTH, A. E. 1998. Learning in high stakes ultimatum games: an experiment in the slovak republic. *Econometrica (New Haven),* 66**,** 569-596. HOFFMAN, E., MCCABE, K. A. & SMITH, V. L. 1996. On

again in the very early ‘70s when I adopted this [methodology of paying subjects]. It wasn’t because somebody told me to do it. It was because the data came out that way.”

Many more examples of distrusting of what others observe could be furnished from Plott’s career – skepticism of the endowment effect, willingness to pay and willingness to accept (Plott and Zeiler, 2007, Plott and Zeiler, 2005) or framing (Cason and Plott, 2014), to name a few. But even without further elaboration, the examples clearly show how personal involvement in the collection of data in controlled conditions is a crucial source of rigor. True, paying subjects, not deceiving them, checking robustness, repetition and replication of experimental observations are equally important. Yet personal supervision, seeing it oneself, gaining intimate experience with experiments, dealing with surprises, building up the little nitty-gritty details, various skills for designing and executing experiments was at the heart of the personal conversion of economists into experimentalists.

Plott’s experience of the early 1970s had an impact on Vernon Smith as well. By the end of the 1960s Smith had withdrawn from active experimental research and pursued research, for instance, in resource economics. As his occasional discussions with Plott reveal, Smith remained interested in the topic, but without Plott’s impetus he would perhaps not have returned to it at all, and not as early as 1973. At Plott’s invitation Smith became the Sherman Fairchild Distinguished Scholar at Caltech in the academic year 1973/74, resulting in what are now classical experimental studies (Miller et al., 1977, Plott and Smith, 1978).[58] They jointly offered a seminar on experimental economics which Plott has taught ever since; it is where Caltech experimentalists are made.[59] Smith’s seminal paper on the induced value theory (Smith, 1976) received its form while Smith was in southern California. This paper develops the experimental economics methodology to show how the attachment of rewards (money in particular) to various actions in an experiment achieves or “induces” control in an experiment. It grew out of a working paper written during his Caltech stay that Plott commissioned because he needed

expectations and the monetary stakes in ultimatum games. *International Journal of Game Theory,* 25**,** 289-301.

[58] Smith remained in California for another academic year, working at USC a short drive away from Caltech.

[59] The workshop produced experimentalists in the 1970s such as Ross Miller, Betsy Hoffman, Mark Isaac, Mark Spitzer, Thomas Palfrey, and Brian Binger.
Subsequent two decades produced experimentalists such as Paul Brewer, Yan Chen, Charles Noussair, and Mark Olson. Charles Noussair Interview.

published references in a joint NSF grant application with Fiorina that would justify his need to pay experimental subjects (Smith, 1973).[60]

While rigorousness as a driving force deals with the person of the experimenter and her/his direct involvement in data production, the driving force of integrity deals with the quality of data; it is illustrated in the next section by James Cox's experience with non-experimental data.

2.3 James Cox's Data Integrity

In the course of the 20th century, the quantity and variety of data available to economists increased dramatically, but left economists increasingly dependent on data collected by someone else. While a natural consequence of the division of labor; statistical offices were dealing with the intricacies of data collection and processing furnishing authoritative data which economists could use without hesitation. As the John Ledyard example illustrated, the limitations on the quality and replicability of data had the side effect of separating theorists and theory from empirical evidence. Economic experiments provided a solution to this distrust of empirical data by creating data in controlled experimental and later laboratory conditions under the personal supervision of the (experimental) economist who did not have to rely on data gathered and processed externally. This conceptual reconfiguration of what count as data and evidence in economics (integrity) and the fact that experimental data are evidence that is relevant for economists are best illustrated by James Cox's path to experimentation.

James Cox met Vernon Smith at the University of Massachusetts in the early 1970s. For Cox this was the first academic appointment after finishing his Ph.D. dissertation at Harvard in 1970. While Massachusetts at this period is well-known for its rancorous disintegration (Mata, 2009), Cox remained there until 1977 when he joined Smith at the University of Arizona where he remained until 2006.

[60] The 1976 papers while referring to the 1974 working paper state that the "paper is an articulation of concepts originally developed in the course of several seminars in experimental economics taught at Purdue University, 1964-67." SMITH, V. L. 1976. Experimental Economics: Induced Value Theory. *The American Economic Review. Papers and Proceedings of the Eighty-eighth Annual Meeting of the American Economic Association* 66**,** 274-279. p. 274.

During his graduate studies, Cox took a course taught by Simon Kuznets on development economics. Each student before writing a paper had to present his research idea in front of the class. When a student suggested that he wanted to apply a theoretical model on a particular dataset, Kuznets frequently asked:

> " 'What makes you think that this variable in your theoretical model has anything to do with that data?' And the shell shocked, deer in the headlights graduate student might reply, 'Well, they have the same name, don't they?' And then Kuznets would pounce saying, 'If you don't know how that data was collected you have no idea what you're doing. In other words these are numbers to you. You need to know how they were collected and if you don't, you have no idea how to proceed'."[61]

Knowing how one's data are collected was the take home message that Kuznets tried to instill in his students and Cox soon learned the lesson the hard way. In the very same course, a fellow student suggested using data from the national income and products accounts of Pakistan. Cox recalls:

> "I was sitting in the middle of the classroom and I happen to look over and there's two Pakistani students, they were sitting there desperately trying to stay polite. Practically putting their fists in their mouths to not laugh. Just a desperate struggle to remain polite. After the class I walked out and approached them. I said, "It seemed to me that you were really having a problem here with this data that this classmate's gonna use in his paper. What's wrong?" And they said, "Well, we're on leave from the *office that makes up those numbers*." I was naive. I was shocked, just shocked. I asked: "How can you do this?" Everyone takes these numbers – that's real data, national income and products accounts of a country, a sovereign state. And they said, "Well, in order to quality for various UN aid programs, Pakistan has to produce national accounts. We have no way of collecting that data. So, in order to get the aid programs our assignment is to make up the numbers so that we can get the aid." [62]

These Pakistani students were part of a professional training program that brought mid-level bureaucrats to get a master's degree at Harvard in what was the predecessor of the

[61] James John Ledyard Interview.
[62] James Cox Interview. My emphasis.

Kennedy School of Government. The final experience that made Cox acutely aware of the limitations of empirical data gathered in the field by someone else was his work with American Petroleum Institute (API) data. API was the main source of data used by the private sector and the government on U.S. oil production, drilling costs, and proved reserves. Together with Arthur Wright they wrote an article on energy policy published in the AER (Cox and Wright, 1976). They presented a theoretical model that allowed them to study the effects of a special tax on the oil industry. The theoretical model was fitted with time series data from the API.

> "One critical distinction in cost data was the breakdown between what are called intangible drilling costs and tangible drilling costs. Intangible costs are things like what you pay the workers, they're called roustabouts actually, they drill – operate the drills. Tangible drilling costs would be something like the cost of steel well casings. The breakdown is very important because intangible costs can be expensed, written off as a cost in the year in which they're incurred, tangible costs have to be capitalized and depreciated over 20 years. The tax implications are vastly different. And our study of the effect of the policy was dependent on that distinction in the data."

The paper was well received and even facilitated much energy policy consulting for Cox and Wright. A few years later they wanted to update their study and the key was to get the latest data. However, separate data on intangible drilling costs were no longer available. Cox and Wright thought that the best way to proceed would be to

> "continue to impute the breakdown at the end of the series where we don't have it. We'll do it a regression on the years when we have it and then use that to impute the breakdown. We did that. Guess what we got. We got a perfect fit. Why? We discovered the rule that they had used to do the breakdown. They never were able to collect the breakdown data. We should have known that from the procedures we had to adopt to use the data in the original paper."[63]

These three experiences taught Cox "to thoroughly distrust published – what is so-called authoritative data that most economists would just use unquestionably, still do in fact. Most applied econometricians don't question published data."[64] Once Cox became aware

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[63] James Cox Interview.
[64] James Cox Interview.

of Smith's experimental research in Arizona and the circumstances and nature of the experimental data, given his prior negative experiences with field data collected in uncontrolled circumstances and by someone else, Cox was primed to doing experiments. The experimental method seemed like a viable solution to these data problems. It was natural for him to accept experiments as a valid source of data. When an offer to join Smith in Arizona came, he did not hesitate. In 1977 Cox moved there and apart from rejoining Smith, he had another colleague from Massachusetts, Ronald Oaxaca. Both, the latter to a lesser extent, soon thereafter embarked on experimental careers. Smith and Cox with several co-authors, most notably Mark Isaac,[65] a trained experimentalist and a Ph.D. student of Charles Plott, embarked in the late 1970s on a major research undertaking to study the bidding behavior in first-price auctions. Their research led to what became known as the first-price auction controversy, a major internal dispute among experimentalists in the second half of the 1980s and early 1990s which is the subject of Chapter 6, below.

The absence of the various problems that conventional data suffered from, through better control and fewer auxiliary assumptions about the underlying data-generating process, was the source of the lure and promise of experimental data. By discussing Cox's experience with non-experimental data, I wanted to show the role of data integrity in the personal conversion of early experimentalists.

2.4 Reinhard Selten's Virtuous Circle

The number of experimenters in Europe in the early 1990s was smaller than in North America. This was also a sign of the smaller population of economists and of the lagging behind of overseas developments. However, if the reference point had been the 1960s, Europe, Germany in particular, was at the forefront of experimental economics research. The first research community of experimental economists in the world was formed around Heinz Sauermann (1905-1981) in Frankfurt in the 1960s, more than a decade

[65] R. Mark Isaac (1954-) received his Ph.D. from Caltech in 1981. Until 2001 he worked at the University of Arizona and served as Chair of the Economics Department between 1991and1998. Since 2001 he has been at Florida State University. He served as the Secretary/Treasurer of the Economic Science Association from its inception in 1986 until 1997 and then as its Treasurer until 2013.

earlier than in the US.[66] The community included Reinhard Selten, the 1994 Nobel laureate for his game theory research, Reinhard Tietz,[67] Otwin Becker and a number of other researchers. In the period from the late 1960s up to the 1980s they organized a series of international conferences with a number of overseas participants, such as James Friedman, Charles Plott, Austin Hoggatt, Andrew Schotter[68] and Al Roth. These were in part funded by a foundation deed that Sauermann controlled. A number of conference proceedings and dedicated volumes were published as well.[69] In 1977 they formed the *Gesellschaft für experimentelle Wirtschaftsforschung*, GfeW, (*The Society for Experimental Economics Research*) which was the first association dedicated to experimental research in economics and which has become the umbrella organization for experimentalists from German-speaking countries.[70]

The last case study illustrating the driving forces of the experimental turn focuses on the experience of Reinhard Selten, the intellectual leader of the "Frankfurt school of

[66] It was Sauermann who actually coined the term 'experimental economics' in the invitation for the first conference in 1967. He was a good friend of Morgenstern, who supported Selten in his trips to the US in the 1960s where he met with game theorists Harsanyi and Nash. James Friedman and Austin Hoggatt were also involved in experimental research at this time.
Morgenstern (1902-1977) was an Austrian economist, who from 1938 until his death was based at Princeton. Together with John von Neumann, Morgenstern established the mathematical field of game theory in the seminal book MORGENSTERN, O. & NEUMANN, J. V. 1944. *Theory of games and economic behavior,* Princeton. Robert Leonard provides a detailed historical treatment of Morgenstern and the early game theory. LEONARD, R. J. 2010. *Von Neumann, Morgenstern, and the creation of game theory: from chess to social science, 1900-1960,* New York, Cambridge University Press.

[67] Tietz was a Ph.D. student in Frankfurt from 1963 to 1971 and wrote a dissertation entitled *The Aspiration Balance-oriented model of the business cycle* (KRESKO) in which he experimentally explored bargaining between employers and unions on macroeconomic issues. He was a professor of economics at Frankfurt until his retirement in 1993. He served as the Chairman of the German Society for Experimental Research from 1982 to 1995.

[68] Andrew Schotter graduated with a Ph.D. in economics from NYU in 1973, supervised by Oskar Morgenstern. Morgenstern knew Heinz Sauermann and he persuaded Schotter to attend an experimental conference in Germany in 1977, where he met Plott and the German experimentalists. As he described it, "The way I got into experiments was through my advisor. Oskar Morgenstern was invited to a conference in Germany run by Heinz Sauermann and Reinhard Selten, who was Sauermann's student actually, if I'm not mistaken. The invitation went to Morgenstern. He had never done experiments himself but he was interested in them. He just said to me, why don't you go? You'll get a trip to Germany, why don't you write a paper for this? I have no idea of what possessed me to do it, but actually, I tested a theory. The theory tested was in a paper that originated as a joint paper between Morgenstern and von Neumann, I think, which was called *Theory Absorption*." Andrew Schotter Interview.

[69] Eight volumes of *Contributions to Experimental Economics* were published in the 1970s. After Sauermann's death, Tietz edited a volume TIETZ, R. 1983. *Aspiration levels in bargaining and economic decision making: proceedings of the Third Conference on Experimental Economics, Winzenhohl, Germany, August 29-September 3, 1982,* Berlin; Heidelberg; New York; Tokyo Springer-Verlag..

[70] The GEW was founded on 16th June 1977 by Sauermann. There were 7 founding members: Otwin Becker, Rudolf Richter, Heinz Sauermann, Reinhard Selten, Reinhard Tietz, Horst Todt, Ulrike Vidmajer und Hans Jürgen Weber. It was kept intentionally small so that the money that he donated in his will to GfeW should be in trusted hands. Reinhard Selten Interview.

experimental economics." His early experimental research is the earliest example of a virtuous circle – where theory and experimental data interact and forge mutual development.

During his studies of mathematics at the Johann-Wolfgang-Goethe-University in Frankfurt am Main, Selten attended several psychology courses. The psychology students were required to participate as subjects in experiments. This experience made him more sensitive to the idea of running experiments, as he grew familiar with the technique of running experiments, though he did not run any on his own. After finishing his master's degree in mathematics with a game theory topic,[71] he was hired by Sauermann. His research project was an application of game theory to the theory of the firm. By then, he had read Herbert Simon's work and become convinced by his ideas about bounded rationality.[72] He also read a book published by the *American Management Association* about a computer management game (Ricciardi, 1957). In his doctoral thesis, Selten worked on cooperative games (Selten, 1961) and was interested in oligopoly theory. The computerized management game made him think that he could run oligopoly experiments without the need of a computer. "I was quite impressed by the fact that it was possible to approach the questions of cooperative game theory by experiments."[73] With Sauermann's encouragement, he designed an experiment based on the Cournot oligopoly situation, which gave him his first publication and also his first experimental paper (Selten and Sauermann, 1959).[74]

While this paper presented experiments with a clear connection to an existing theory, namely Cournot's oligopoly, later in the 1960s he ran oligopoly experiments that "did not

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[71] Selten wrote his master's thesis in 1957.

[72] Selten had read the following papers: SIMON, H. A. 1955. A Behavioral Model of Rational Choice. *The Quarterly Journal of Economics,* 69**,** 99-118.; SIMON, H. A. 1956. Rational Choice and the Structure of the Environment. *Psychological Review,* 63**,** 129-38., both reprinted in SIMON, H. A. 1957. *Models of man: social and rational; mathematical essays on rational human behavior in society setting,* New York, Wiley. Source: Witness Seminar.

[73] Reinhard Selten at the Witness Seminar. He had read the seminal paper by Kalish, Millnor, Nash and Nehring, the experimental paper from 1954 in the book edited by Thrall, Coombs and Davis, *Decision Processes*. KALISCH, G. K., MILLNOR, J. W., NASH, J. F. & NERING, E. D. 1954. Some Experimental n-Person Games. *In:* THRALL, R. M., COOMBS, C. H. & DAVIS, R. L. (eds.) *Decision Processes.* New York: Wiley.

[74] SELTEN, R. & SAUERMANN, H. 1959. Ein Oligopolexperiment. *Zeitschrift für die gesamte Staatswissenschaft,* 115**,** 427-471. Reprinted in SAUERMANN, H. 1967. *Beiträge zur experimentellen Wirtschaftsforschung,* Tübingen, Mohr., pp. 9-59.
English Translation SAUERMANN, H. & SELTEN, R. 1960. An Experiment in Oligopoly. *General Systems, Yearbook of the Society for General Systems Research.* Ann Arbor, MI: Society for General Systems.

involve clear theory."[75] For instance, he looked at oligopoly situations with demand inertia. Let me quote a lengthy extract from the witness seminar where Selten recounts how he arrived through the interaction of experiment and theory at the seminal concept of sub-game perfect equilibrium.

> "Demand inertia[76] means the quantity sold by an oligopolist in a period is decreasing in his/her price but it also is increasing as a function of the quantity sold in the previous period. *The total quantity sold in a period may depend on time expressed by the period number*, but apart from this, only on the current average price. We did experiments on three-firm oligopolies with price variations and demand inertia. In every period the quantity sold by an oligopolist is a linear function of a variable called demand potential and the price of this oligopolist.
>
> The three oligopolists had different average cost and faced a higher interest rate for negative account balances than for positive ones. The model had also some other features enhancing its complexity. It had the form of a dynamic game. My research associate Otwin Becker[77] and I tried to find some theoretical solution, but the problem was too hard and we gave up on it.
>
> Later, I simplified the game to a theoretically tractable form and then I could determine a unique solution of the dynamic game with finitely many periods by backward induction. But then I realized that the game has many other equilibria. Nevertheless the backward induction solution seemed to me very natural and thinking about the reason for this I came up with the concept of sub-game perfectness. Thus in an oblique way an experiment on a dynamic game motivated

[75] Reinhard Selten at the Witness Seminar.
[76] The demand potential of an oligopolist is the sum of three components, first the quantity sold in the previous period, second, a multiple of the amount by which the oligopolist's price was below the average price in the previous period, and third a time component depending on the period number.
[77] Otwin Becker (1932-) is an Emeritus Professor of Economics at the University of Heidelberg, Germany. He was a member of Heinz Sauermann's *Seminar für Mathematische Wirtschaftsforschung* (Seminar for Mathematical Economics) and his thesis concerns the economic decision making of households. His research interests include experimental economics, econometrics, and information systems (business informatics). His early experimental work appeared in the Beiträge zur experimentellen Wirtschaftsforschung. Becker was a founding member of the *Gesellschaft für experimentelle Wirtschaftsforschung* [Society for Experimental Economics Research]. His experience in Frankfurt and with Selten in particular is recast in BECKER, O. 2010. Encounters with Reinhard Selten: An Office Mate's Report. *In:* OCKENFELS, A. & SADRIEH, A. (eds.) *The Selten School of Behavioral Economics.* Berlin Heidelberg: Springer..

> a theoretical analysis of a highly simplified version of it and as a side effect of this analysis a basic game theoretic concept was introduced."[78]

Reinhard Tietz was Selten's close associate in Frankfurt in the 1960s. They spent much time travelling together and discussing their experimental and theoretical research. Tietz summarized the influence of experiments on Selten's career:

> "The basis of his Nobel prize for sub-game perfection is experimental economics, the confrontation between experimental reality and theory. I think it was together with game theory and theory of rationality one of the three main impulses, Triebkräfte [driving forces], for Reinhard Selten."[79]

Experiments like those conducted by Selten and others that I have discussed in this chapter so far illustrate the *question-generating nature* of experimental research. The ability to generate new data quickly, or at least faster than field data can be collected (e.g. consumer demand data) or generated (e.g. survey data), speeds up the process of answering the questions generated by empirical data This process is even faster when a theory gives guidance for what sorts of question to pose on the data. As in Selten's example, the answers are often not fully satisfactory and more data are needed. Repeating the same experiment is one way of proceeding. Changing the experimental design to control for variables of interest in a different way or including new variables is another, for instance, what type of information is shared as in Ledyard's example. Thus experiments are an engine that once kick-started can lead to a continuous style of experimentation. For the German experimentalists around Sauermann in the 1960s, this is exactly what experiments did. The fact that they also led to a continuous engagement in the US from the 1970s onward, but not in the 1960s, suggests that there were some conditions without which the engine could come to a halt.

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[78] Reinhard Selten at the Witness Seminar.
Vernon Smith commented on this fragment: "Would that all theorists could be influenced by this sort of experience! This is how theory should be done; much better than making up the facts about what is important and then cranking out paper and pencil models based on them." Witness Seminar Transcript.

[79] Reinhard Tietz Interview.

2.5 Intersecting Trajectories

The four driving forces can be split into two groups. Two relate primarily to theory and two to data. The symmetry driving force is about seeing that experimental data matter for theory, but not seeing how this impact goes on. This is shown in the virtuous circle of Selten where theory is not merely evaluated, but is a new element to be created. John Ledyard's example is particularly suitable for demonstrating the driving force of symmetry. The interaction between Smith and Ledyard comprises the accumulated knowledge in Smith's experience, by then two decades long, with the experimental method.

The other two driving forces relate to data. The rigorousness driving force deals with the presence of the experimenter and her direct involvement in data production. Being rigorous is also about questioning the beliefs of oneself and others. But it is wider than proper scientific conduct; it is about control. The integrity driving force illustrated by James Cox's experience with non-experimental data deals with the quality of data. Taking experimental data seriously meant discarding the possibility that it is only the data that are flawed and not the theory to which they are related.

All four driving forces present different ways of thinking about the triangle 'scientist-theory-data'. The replicability of and amenability of experimental design countered objections about the reliability of experimental data and speeded up the interaction between theory/model building and empirical evidence. The four examples of conversion show how experimentation allowed economists to rethink their understanding of what it means to be a good economist not simply in one's abstract commitment to epistemic tenets, but, more importantly, in practice as well.

All of the experimental economists whom I have discussed so far are theorists by training, more specifically mathematical economists, which was not typical in the 1950s and 1960s when many of them graduated. This may be seen to confirm the received idea that the early experiments in economics were predominantly theory driven; intended to test theory. But a close reading of four driving force cases reveals the relation between theory and experiment in a different light. These driving forces are equally about theory and data, and in particular about elevating data to the focus of an economist's work. Selten would not have reached the idea of sub-game perfect equilibrium had it not been for the experiments he performed. Experiments allowed Plott to discriminate between theories. Giving a theory its "best shot" is not about the theory, but about the data, the quality of the data. James Friedman left experimentation and turned into a theorist because of a lack of connection between his experiments and the theory that he wanted to test.

For experimentalists the question of what comes first – theory or data – became far less important once they experienced the four driving forces. The question of what comes next became far more important: how to modify theory, how to better control experiments, how to design relevant experiments, etc. In effect, experiments became question generating machines for economics research. Experiments transcended the theory or data that they were based on. The four driving forces helped to start the experimental engine. In the next chapter we see how important the community of experimentalists became in keeping the engine running and how computerization accelerated the engine. A particular example of this machine is discussed in Chapter 6, on the first-price auction controversy.

The four case studies clearly show how interconnected and closely related the American group of early experimentalists was. Smith, Ledyard, and Plott are all connected via Purdue. In the next chapter we encounter John Kagel and Raymond Battalio. When they came to Purdue as graduate students at the end of the 1960s, Smith had already left and there was no institutional memory of the experiments he had performed early in the decade. Even during Ledyard's studies at Purdue in the mid-1960s he encountered no experiments at all. In the first chapter I mentioned that James Friedman, Vernon Smith and Reinhard Selten are all connected through Austin Hoggatt, but whereas Friedman's experiences with Hoggatt made him more or less give up on experiments and move to game theory, Smith and Selten were inspired by Hoggatt's laboratory and once the opportunity to build their own laboratory presented itself, they did not hesitate.

Both Purdue and Caltech were important institutes for the emergence of experiments in economics in the 1960s and 1970s, respectively. In contrast to traditional economics departments, both fostered research that went beyond the traditional boundaries of well-defined economic subfields at the time.

The individual trajectories of the early experimentalists traversed academic space-time, intersected and from individual researchers built themselves into a community of economists who through their repeated interaction and continuous engagement with the experimental method were gradually grasping the full extent of the turn's implications. While the driving forces operated at a personal, individual level, without a community of like-minded scholars who actively practiced the experimental method, notably in close relationship to economic theory, a continuous style of experimentation could not emerge and the experimental engine would have to halt, as it did in the US in the 1960s. Despite a number of researchers such as James Friedman, Lester Lave, Trenery Dolbear, Austin Hoggatt, Martin Shubik, William Starbuck and Roger Sherman pursuing experimental work, Smith's attempts did not succeed, unlike those in Frankfurt where Selten and Sauermann managed to foster a community of experimentalists.[80]

2.5.1 Tucson Meetings

Let me move from accidental interactions and unexpected contingencies to organized efforts. In this respect the Tucson Experimental Workshop played a crucial role. When in 1977 Vernon Smith organized what became the first Tucson Experimental Workshop, it drew 21 participants – who more or less represented the entire nascent community of researchers involved in economic experiments or at least slightly interested in them. At the time, the largest groups where experimental research flourished were at Caltech and Arizona. From 1972 Charles Plott at the *California Institute of Technology* in Pasadena was involved in systematic experimental research with a number of collaborators and students. The research group at the University of Arizona in Tucson developed when Vernon Smith moved there in 1975 after a two- year sojourn in southern California (Caltech and USC).[81] While the first workshop focused on completed research, the second

[80] If Sidney Siegel had not prematurely died in 1961, he would have certainly been a driving figure in cultivating an experimental community in the US. For the context of the 1960s, the centrality of Carnegie Mellon must be noted. There was an openness to new ways of doing research not found in vested institutions, thus confirming studies in history of science which show that innovation very often comes from the fringes of a discipline. Friedman and Dolbear as experimentalists were offered a job there, Ledyard worked there after graduating from Purdue and Holt studied there too.

[81] The experimental community at *Caltech* has included a number of faculty members: Charles Plott, David Grether, Morris Fiorina, John Ferejohn, Roger Noll, John Ledyard, Tom Palfrey, David Porter, Colin Camerer (and more in the past fifteen years) and has produced several Ph.D. students who embarked on successful experimental careers - Elizabeth Hoffman, Matthew Spitzer, Mark Isaac, Tom Palfrey, Mark Olson, Charles Noussair, Yan Chen and Katerina Sherstyuk. In the broadest sense the *Arizona* experimental group eventually grew to include: in the *ESL research faculty*: Brian Binger, Kevin McCabe, Shawn LaMaster, David Porter, Stephen Rassenti, and Vernon Smith; *Accounting*: Fisher, Schatzberg, Waller; *Economics*: Bryson, David Conn, James Cox, Elizabeth Hoffman, Mark Isaac, Libecap, Oaxaca, David Pingry, Stan Reynolds, Wells,

in 1979 included also “embryonic and horizon areas of research“ and had 34 participants (with five from Arizona and nine from Caltech). The third workshop took place in 1984, with 54 listed participants.[82]

Typically four types of participant regularly attended the Tucson workshops. First, those who at the time were immersed in full time experimental research (the economists Plott, Smith, Kagel, and Battalio and psychologists Green, Rachlin, Kahneman). Second, their graduate students whose studies exposed them to experimentation (Hoffman, Isaac, Palfrey, Williams). Third, economic theorists (Cox, Ledyard, Roth, Grether, Holt) intrigued by experiments who in a consequence gradually integrated experimental observations into their theoretical work and eventually adopted experiments in their research activities. Fourth, scholars appreciative of experimental research (Noll, Newlon). Yet only a few of the participants considered themselves primarily experimentalists by the early 1980s.[83]

Experimental economics pioneers offered workshop style classes, a form that still remains the standard. The workshop style follows a format established by Smith in the 1960s and Plott and Smith in the early 1970s. Students read the classic experimental papers and a selection of papers on a variety of experimental topics, often following the specific interests of the instructor. The emphasis, however, lies in the application. Students are required to select a topic and design an experiment, prepare and run it, analyze the data and draw preliminary conclusions. This has frequently led to joint publications and

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Edward Zajaz; *Finance*: Dyl, Harlow, Suchanek. Unlike the community at Caltech the Arizona group disintegrated in 2001 when a large number of faculty members left, mostly for GMU.

[82] The first workshop took place at the Westward Look Resort, Tucson, March 18-20, 1977. The second workshop, also entitled the NSF Experimental Economics conference was held at the Arizona Inn in Tucson, October 19-21, 1979. The third Experimental Economics Workshop took place at the Westward Look Resort, March 27-9, 1984. The papers presented in the first workshop were refereed and some were accepted for the first volume of Research in Experimental Economics (1979). The 34 participants in 1979 were: Battalio (Texas A&M), Burns (Australia), Dawes (Oregon), Easley (Cornell), Ferejohn (Caltech), Green (Washington), Grether (Caltech), Groves (UC), Harstad (Illinois), Hoffman (NW), Isaac (Caltech), Kagel (Texas A&M), Ledyard (NW), Marrese (NW), Murningham (Illinois), Nelson (Caltech), Newlon (Associate director for Econ, NSF), Noll (Caltech), Palfrey (Caltech), Philips (Cornel), Plott (Caltech), Rachlin (STONY Brook), Roth (Illinois), Rotchschild (Wisconsin), Stadon (Duke), Thaler (Cornell), Wilde (Caltech), Williams (Indiana). From Arizona – Alger, Auster, Cox, Smith, Taylor, Walker. Jerry Green, Jack Hirschleifer, Michael Spence, and George Stigler were invited but declined.

[83] In the late 1960s and early 1970s both Plott and Smith were pursuing successful and respected careers in economic theory – social choice and resource economics, respectively. Even in the late 1950s and 1960s experimental research was a small part of Smith’s interests.

For a long time Alvin Roth would reply to the question whether he is an experimenter – “No, I’m a theorist. He doesn’t do that anymore. He says I’m a theorist as well. But that’s a big difference. … By hiring me [Kagel in 1988], it was a real commitment on his part to have a strong hand in experiments.” John Kagel Interview.

dissertations with an experimental component.[84] Experimentalists such as Plott and Smith provide small sums of money for students to run their experiments, carefully following their progress and imparting practical skills and tacit knowledge. Under their supervision, students can experience the driving forces of the turn. They can see how rigorous data are generated, accept the integrity of experimental observation, enter a virtuous circle and learn to appreciate the equal standing of theory and experimental, or more generally controlled, data.

2.5.2 Experimental Imperialism

Unlike public choice or law and economics, the case of experimental economics is not an example of economic imperialism. These economists were not employing an established tool of economic science; rather, they introduced and adapted a method that is the hallmark of the natural sciences, the controlled experiment.[85] The professionalization of this community has often been misunderstood by non-experimental economists as the creation of a distinct sub-field, thereby obscuring the distinction between a new field and a novel method within economics, an issue that will make repeated appearances in the course of this paper.[86]

Experimental economists are not defined through the subject matter of their inquiry. Unlike fields such as industrial organization, labor economics or international economics that have extended the boundaries of their subject matter, experimental economics is pervaded by the gradual application of its method to new, previously experimentally unexamined, and often very well-known issues within economics. Early experiments to do with markets and individual and group decisions published in the 1960s and 1970s,

[84] Arlington Williams is a case in point. He was one of the first students to attend Smith's experimental economics class at Arizona."About half way through that course, I was hooked. We had to do a final paper in that class and I decided to run some oral double auctions and shift the supply and demand curves around a little bit. Watching the oral double auction market converge to the competitive equilibrium and not having any idea what the formal process was that was actually generating that convergence was a source of considerable astonishment to me. After that experience I knew that I wanted to focus on experimental economics."Arlington Williams Interview. Williams in his Ph.D. dissertation (1978) was the first to program double auction experiments.

[85] This is not to say that there was a conscious borrowing from the natural sciences.

[86] Experimental economics is thus in the sense of a method much closer to mathematical economics and econometrics than to behavioral economics (at least in its second incarnation from the 1980s). But unlike these two methods, experimental economics is directly involved in production of data-generating processes.

finance (1982), law and economics (1982), public goods (1984, 1985), industrial organization (1986), marketing (late 1980s), general equilibrium (1998), and many others[87] are manifestations of what I call *experimental imperialism*.[88]

This was reinforced by the way that graduate students with experimental work in their theses were marketed. Until the mid-1990s these graduates did not enter the academic job market as experimental economists but as having standard economics specializations. Once these students found a job, their experimental research was published and funded; their research portfolio became predominantly experimental.[89] This went hand in hand with the increased acceptance of experimentation in economics.

The four driving forces of the experimental turn led to a continuous style of experimentation with experiments expanding to experimentally unexplored territory and serving as engines or question generating machines in experimental research. Experimentation provided a novel way of observing and data gathering in economics and concurrently a new location for such activities. This new site with its complex structure embedded in the experimental community is taken up in the next chapter.

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87 The publication date often says little about the time when the actual research was done. For instance, the experiments reported in Lian and Plott's paper on general equilibrium were conducted in 1990, eight years before being published.
Law and Economics: HOFFMAN, E. & SPITZER, M. L. 1982. The Coase Theorem: Some Experimental Tests. *Journal of Law and Economics,* 25**,** 73-98.;
Finance: FORSYTHE, R., PALFREY, T. R. & PLOTT, C. R. 1982. Asset Valuation in an Experimental Market. *Econometrica,* 50**,** 537-567.;
Public goods: Isaac, Walker, and Thomas (1984) ISAAC, R. M., WALKER, J. M. & THOMAS, S. H. 1984. Divergent Evidence on Free Riding: An Experimental Examination of Possible Explanations. *Public Choice,* 43**,** 113-149, ISAAC, M. R., MCCUE, K. F. & PLOTT, C. R. 1985. Public Goods Provision in an Experimental Environment. *Journal of Public Economics,* 26**,** 51-74.
Industrial Organization: HOLT, C. A., LANGAN, L. W. & VILLAMIL, S. A. P. 1986. MARKET POWER IN ORAL DOUBLE AUCTIONS. *Economic Inquiry,* 24**,** 107-123.
Marketing: HOFFMAN, E., MENKHAUS, D. J., CHAKRAVARTI, D., FIELD, R. A. & WHIPPLE, G. D. 1993. Using Laboratory Experimental Auctions in Marketing Research: A Case Study of New Packaging for Fresh Beef. *Marketing Science,* 12**,** 318-338.;
General Equilibrium Theory: LIAN, P. & PLOTT, C. R. 1998. General Equilibrium, Markets, Macroeconomics and Money in a Laboratory Experimental Environment. *Economic Theory,* 12**,** 21-75.

88 Imperialism implies continued research and thus the development of a community. For instance Peter Bohm, an early experimentalist in Sweden, investigated the public goods problem in the early 1970s, yet his work did not spur any subsequent research for almost a decade. BOHM, P. 1972. Estimating demand for public goods: An experiment. *European Economic Review,* 3**,** 111-130.

89 See for instance the case of Elizabeth Hoffman at the Witness Seminar.

3 The Place and Space of Experimental Economics – The New Site of Economic Research

> Dean to the physics department chair: "Why do I always have to give you guys so much money, for laboratories and expensive equipment? Why couldn't you be like the mathematics department? All they need is money for pencils, paper and waste-paper baskets. Or even better like the economics department. All they need are pencils and paper."[90]

If one could travel three decades back in time to examine the experimental economics landscape, one would see no more than a handful of research groups and even fewer laboratories.[91] By the early 1990s the landscape had changed dramatically. In 1992 almost forty groups were in operation, each with a laboratory. In 1994 Daniel Friedman listed 84 laboratories, though this figure may rather suggest the number of active research groups (Friedman and Sunder, 1994, pp. 207-10). Currently, there are at least 175 active laboratories worldwide.[92]

The second half of the 1980s was the period when the economics laboratory as it is now known emerged and rapidly proliferated. The laboratory gradually became the focal point of experimental practice, communities and their identity. To be an experimental economist changed in this period from being an economist who conducts experiments to an economist who conducts them with computers in a laboratory. Yet as we will see, computers are not sufficient to turn either an economist into an experimentalist, or a place into a dedicated laboratory.

The distinction between place and space originates from Michael de Certeau's book *The practice of everyday life*. Whereas the former is identified by objects and their physical location, the latter is determined "through operations which, when they are attributed to

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[90] This joke motivated the author during his undergraduate studies of physics and economics to explore experimentation in economics.

[91] By 1985 the following labs that used computers existed: Bonn, Arizona, Caltech, Indiana and Houston.

[92] A list composed by Dimitri Dubois and Marc Willinger at the LAMETA laboratory at the Université de Montpellier I. Their directory excludes neuroeconomics laboratories. DUBOIS, D. & WILLINGER, M. 2009-2014. *The experimental labs in the world* [Online]. LAMETA laboratory at the Université de Montpellier I. Available: http://leem.lameta.univ-montp1.fr/index.php?page=liste_labos&lang=eng [Accessed August 22, 2014].

‘some objects,’ specify ‘spaces’ by the actions of historical subjects” (Certeau, 1984, p. 118). In the case of experimental economics, the places were initially classrooms and nowadays are primarily laboratories equipped with networked computers, partitions, and software. These objects are credited with the functions of experimental control and intervention by economists, programmers, administrative personnel, student researchers and other members of an experimental community and the invited test subjects.[93] All these elements together create the space of the economics laboratory that I seek to disentangle in the course of this chapter.[94]

My investigation takes me on a trail that starts in various opportune locations, classrooms foremost, and then follows the vagaries of six laboratories - four in the US and one each in Germany and the Netherlands. These laboratories stand out from other labs because of their technological innovations and impact on experimental research.

In the first section of this chapter I describe the typical transition that most experimentalists went through: from using classrooms to operating in dedicated laboratories. Two exceptions – Austin Hoggatt’s *Management Science Lab* at Berkeley established in 1964 and the animal laboratory of Raymond Battalio and John Kagel at Texas A&M in the 1970s and 1980s – provide a crucial insight in understanding the laboratory place.

Hoggatt’s laboratory plays a key role in the history of experimental economics. It was the first ever economics laboratory designed for experimental research and, what is even

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[93] The opposition between space and place was successfully exploited in the history of science. See for instance the book *The heavens on Earth* where David Aubin, Charlotte Bigg and Hans Otto Sibum examined observatories and astronomy in the 19th century. AUBIN, D., BIGG, C. & SIBUM, H. O. 2010. *The heavens on earth: observatories and astronomy in nineteenth-century science and culture,* Durham [NC], Duke University Press.

[94] There are currently no experimental economics studies within the science studies literature. These have traditionally focused on laboratory sites of research in the natural sciences. A number of experimentalists provided brief descriptions of what a laboratory looks like and how it operates. Martin Shubik provided a review of behavioral science laboratories including those that were used for experimental economics and experimental gaming research in the 1970s. SHUBIK, M. 1975b. *The uses and methods of gaming,* New York, Elsevier. Experimental economists have provided a few practical descriptions of doing experiments with an accompanying account of what a laboratory should look like. John Hey, who established the second laboratory in Europe after Bonn at the University of York, UK, in the late 1980s, wrote one of the earliest books on experimental economics, which reflects the dominance of mainframes HEY, J. D. 1991. *Experiments in economics,* Oxford, UK; Cambridge, USA, B. Blackwell.. Stephen Rassenti elaborated on computer technology RASSENTI, S. J. 1990. Computers in Experimental Economics. *Social Science Computer Review Social Science Computer Review,* 8**,** 520-523.. Daniel Friedman and Shyam Sunder’s account provides an excellent snapshot of the mid-1990s. FRIEDMAN, D. & SUNDER, S. 1994. *Experimental methods: a primer for economists,* Cambridge [England]; New York, Cambridge University Press. The ESA mailing list occasionally has discussions about setting up a laboratory.

more striking, it was equipped with computers, thought nowadays to be inseparable from economics laboratories. Its centrality lies both in its failure and its success. Hoggatt's laboratory on the one hand swallowed the same amount of resources as Smith's and Plott's labs two decades later in the 1980s - over 2 million current dollars, yet it produced hardly any research. Selten and Smith's experience with Hoggatt's laboratory was important in their decision to pursue their own computerized laboratories, including some of the design features of Hoggatt's laboratory.

We will see that Hoggatt uniquely focused on infrastructure instead of building a community of experimentalists. But infrastructure, the hardware and software, does not perform experiments by itself. In the 1960s there were hardly any experimentalist groups, because they were far too distant from one another. In contrast with Hoggatt, the early experimenters - Smith, Plott, Selten, and others in the 1980s - focused equally on creating a community around their labs and on facilities. In between, in the 1970s emerged a laboratory run by Raymond Battalio and John Kagel which, inspired by behaviorist operant psychology, used animals such as rats and pigeons to study economic theory. Unlike Hoggatt's laboratory, the animal laboratory was immensely productive. Yet it was unique and never replicated – providing further insights into the place and space of economic experimentation.

The second section follows the laboratories of two leading figures in experimental economics - Vernon Smith's *Economic Science Laboratory* (ESL) at the University of Arizona and Charles Plott's *Laboratory for Experimental and Political Science* (EEPS) at Caltech, founded in 1985 and 1986 respectively. Both are predated by more than a decade of continuous experimental operations; in the case of Arizona with the computer system called PLATO.[95] My focus turns to the computer technology used in these labs.

Initially, ESL and EEPS pursued different technologies – terminals connected to a time-sharing mainframe and locally networked personal computers, respectively. Eventually, Ethernet local networks became the standard and EEPS became the main model for subsequent laboratories. The growth in the number of laboratories follows the general trend of exponential increases in processing power and transistor density, a standardization of networking technology accompanied by accelerating falls in the cost of

[95] The emphasis will be mostly on ESL and EEPS where I had access to detailed archival records. For information about Bonn I rely mostly on my interview with Selten and the Witness Seminar.

hardware. While such developments explain the spread of computers at universities, they do not satisfactorily explain why computers begun to be used in economic experiments.

To answer this question, one needs to go beyond a mere investigation of the technological innovations that computers brought to economic experimentation. The purpose of this chapter is to do so. In the fourth section I focus on the social infrastructure and the division of labor that emerged in economics laboratories. Running computerized experiments required new skills, programming in particular. With some exceptions, most experimental economists outsourced programming. They used ad-hoc programmers at first, but eventually established positions for full time programmers. Graduate students with experimental topics continue to program their own experiments, but many laboratories have an additional level of support stuff.

Whereas social infrastructure deals with one particular laboratory, the nature of interaction among experimentalists was another important factor in the rapid proliferation of labs at the turn of the 1980s. Experimental economists exhibit high levels of inclusion and mutual support. The experimental community was marked by the sustained efforts of experimentalists to attract economists to experimental research, provide hands-on expertise and experience in designing and conducting experiments, often with start-up funding, and make software for running computerized experiments available to everyone – thus further lowering the barriers of entry to experimental research. One of the consequences has become increased cooperation between experimentalists and theorists.

Altogether, the social infrastructure, division of labor, and the inclusive nature of the experimental economics community were decisive in turning a laboratory place into a functioning space for generating observations.

The Center for Research in Experimental Economics and political Decision-making (CREED) and its laboratory in Amsterdam were established in 1990. Its history, outlined in section four, illustrates what happens when all the elements examined in the previous two sections come together. Thanks to a deliberate focus on building a community, sufficient funding for building physical infrastructure as well as a social one through the transfer of best practices through regular workshops, the CREED laboratory has become one of the most successful in Europe.

Economics laboratories together with computer technology redefined the notion of experimental control and intervention. Laboratory space gained various epistemic functions. Partitions and computer screens controlled the communication of subjects among themselves and with the experimenter. Computers recorded faster and more reliably what was taking place during an experiment. These and other examples are discussed in the penultimate section. Computerized experimentation transformed experimentalists' understanding of the powers of economic experiments. With the advent of computerized experiments, experimental economists became interested in research questions that could not be conducted as hand-run experiments lest experimental control were compromised. Various types of auctions formats, including ones never conceived before, utility markets and others with allocation algorithms requiring substantial computational capacities were studied. The key to these experiments was the ability to almost instantaneously feed information back to subjects and thus provide the type of control and intervention that was beyond any experimental research done before the introduction of computerized experiments. This type of experiment proved to have far reaching practical consequences (Alexandrova, 2006, Guala, 2005, Nik-Khah, 2008, MacKenzie et al., 2007).

In the context of the whole dissertation, this chapter continues the investigation of the experimental turn at the individual, local and communal level. Towards the end of the previous chapter I arrived at the proto-stage of forming an experimental economics community that gravitated around regular meetings and a number of research groups. In this chapter, I want to complete my investigation of the parallel development of experimental economics communities and to show how this development is inextricable from the emergence of the experimental laboratory. Only in the next chapter do I move to the next level, the interaction of experimental economists with the rest of the profession and the institutionalization of the experimental economics community.

3.1 From a Classroom to a Computerized Lab

The history of experimentation in economics shows that experimental research has used a wide variety of sites. The earliest experiments were conducted both in the field and laboratories. However, no academic economist did any of them.[96]

The first experiment on an economic topic by an academic economist was conducted by Edward Chamberlin. He ran them from the late 1930s until the 1960s in the first class of his various undergraduate and graduate economics courses at Harvard. He used them to demonstrate that, unlike his theory of monopolistic competition, the neoclassical theory of competitive markets does not adequately capture market processes (Chamberlin, 1948).

It is typical of early experimental work that the experimental data collection was performed in fortuitous locations, mostly classrooms that were not purpose-designed and purpose-equipped for experimental research. The first dedicated space for experimental economics research was Austin Hoggatt's laboratory at Berkeley. The next subsection discusses his laboratory and sets in motion the exploring of various other laboratories that document the multiple iterations from a classroom to a functioning contemporary laboratory.

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[96] Early agricultural experiments by John Bennet Lawes and Joseph Henry Gilbert were conducted at the longest continuously running agricultural experimental station. The Rothamsted Experimental Station was founded in 1843. It studied the effects of organic and inorganic fertilizers on various crop yields for 57 years and thereby laid the foundations of modern scientific agriculture. Its so-called classical field experiments were conducted on patches of land on its farm. HALL, D., WARINGTON, R., RUSSELL, E. J. & LAWES AGRICULTURAL TRUST, C. 1917. *The book of the Rothamsted experiments,* London, J. Murray. Some of its original experiments are still ongoing and the data can be retrieved from ROTHAMSTED RESEARCH. 2014. *Classical Experiments* [Online]. Available: http://www.rothamsted.ac.uk/long-term-experiments-national-capability/classical-experiments [Accessed September 27, 2014]
It is worth noting that R.A. Fisher was hired in 1919 to perform statistical analyses of the collected experimental data. That triggered his interest in experimental design, which had profound consequences for the development of statistics, econometrics, and psychology. FISHER, R. A. 1935. *Design of experiments,* Edinburgh; London, Oliver and Boyd.
The economic aspects of farming were marginal to these experiments. The first experiment involving people as experimental subjects dealing with a topic related to economics - as it was viewed by modern economists and also economists at the time - were conducted in the second half of the 1920s. The table lists the 'Hawthorne effect' experiments and the psychometric study by L.L. Thurstone. HOLMES, W. G. 1938. *Applied time and motion study,* New York, Ronald Press Co, HART, C. W. M. 1943. The Hawthorne Experiments. *Canadian Journal of Economics and Political Science,* 9**,** 150-163, MAYO, E. 1933. *The human problems of an industrial civilization,* New York, Macmillan Co, THURSTONE, L. L. 1931. The Indifference Function. *Journal of Social Psychology,* 2**,** 139–67.

3.1.1 Austin C. Hoggatt and his Visionary Lab

When Austin C. Hoggatt died on April 29, 2009, at the age of seventy-nine the experimental economics community lost a low profile yet very influential figure. Hoggatt was the first to build a computerized laboratory for controlled experimentation in economics or, more broadly, in the social, behavioral, and decision sciences - *The Management Science Laboratory* at the Center for Research in Management Science at UC Berkeley in 1964.

Hoggatt developed the idea of a laboratory during his one-year research fellowship at the Miller Institute for Basic Research in Science (1960-1961), a prestigious appointment at Berkeley. Frederick E. Balderston (1923-2004), his longtime colleague at the School of Business Administration, was closely involved in planning and building the laboratory. The laboratory was located in the basement of a building and contained 262 m^2 of floor area, “including the computer and equipment and control roams, two briefing rooms, four fixed experimental cubicles, and an open experimental area that can be rearranged quickly for different uses by moving sound-proofed partitions into place on overhead tracks” (see Figure 1). Hoggatt’s laboratory provided easy opportunities for space configuration, control of the interaction of subjects, and the recording and storage of subjects’ behavior. The lab's wiring was connected “to twenty-four locations for computer terminals, TV display, TV camera, and audio communication.”[97] The first generation hardware included, for instance, a time-shared PDP-5 with teletypewriters and a memory-to-memory link to a PDP-8 computer, which controlled other equipment in the experiments – both expensive state-of-the-art items of equipment.

For the design of the laboratory professional architects were consulted. The design was based on a questionnaire about current and future demand sent to potential users of the laboratory. Unfortunately, it remains unclear who these users were and what their experience with experimentation was. “Experimenters were asked to describe experiments they had done, were doing, and wanted to do. They were also asked to describe experiments they thought someone else ought to do, including ‘far out’ experiments that should be done ‘sometime by someone.’” Their collected responses

[97] Final report of the NSF development grant SOC75 – 0817 and NSF-GS-32128 December 1976. Box 48, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University

“showed that infinite flexibility was not required, there being far more stability in the requirements than had been suspected” (Hoggatt et al., 1969, pp. 203-205).[98]

Hoggatt ’s laboratory played a key role in the history of experimental economics neither because it was the first economic laboratory ever, nor, which is more striking, that it was equipped with computers, which are nowadays inseparable from economics laboratories. This centrality lies both in its failure and its success.

James Friedman recalls his cooperation with Austin Hoggatt in the computerized laboratory in the late 1960s:

> “[Y]ou could run experiments based on models that were much too complicated to use in a hand run operation. And, in fact, the experiment that Austin and I ran there had such a model. It was a model you couldn’t dream of doing in a hand built operation, and, as a matter of fact, I think we somewhat overcomplicated the model.”[99]

As we saw earlier, little came of this effort: it was a dead end (Friedman and Hoggatt, 1980). The laboratory failed because it hardly produced any research, even though it swallowed the same amount of resources as Smith’s *Economic Science Laboratory* and Plott’s *Experimental Economics and Political Science Laboratory* two decades later - more than a million 1985 US dollars or over two million dollars in current terms. Both are discussed in Section 3.2. In contrast, its success lay in the desire of others to emulate “Auggie.” Hoggatt showed to others that computerization goes hand in hand with better experimental control, new types of intervention, and speeding up the execution of experiments and the collection, storage and processing of data. This was decades before the benefits of computerization would have become apparent without Hoggatt’s laboratory.

In the late 1960s, Martin Shubik operated a far less ambitious facility. In 1967 he reported to the head of the Office of Naval Research that at Yale “in the building of the Department of Industrial Administration, we currently have a small gaming laboratory with six rooms for experimental subjects. There is an IBM 1620 computer adjacent to these rooms. However, there is essentially no specialized equipment in each room.” His

[98] It is unclear who the consulted experimenters were.
[99] James Friedman at the Witness Seminar.

experiments required “carrying material back and forth between the experimental subjects and the computer. After an experiment has been run, it is necessary to take the punched cards and the printed outputs and to do a fair amount of separate analysis by hand or by computer.”[100] In contrast, Hoggatt could “run experiments in the morning and discuss results over the print-out at lunch on the same day,” and could also communicate with his subjects through TV (Hoggatt et al., 1969, p. 207). Shubik hoped, but unsuccessfully, to upgrade his laboratory into a “gaming facility” similar to Hoggatt’s. Given the “crude level of time-sharing in computer systems to date” he wanted to run a “sufficiently simple” game with subjects entering their decisions immediately on consoles and waiting for the results.

For Shubik, going from a computerized game experiment to a computerized system for gaming experimentation was “a natural step.” Such a system contains “not merely a computerized game but a means for building and modifying many different games, but also storage, organization and analysis of experimental data” and he said **“**it should be designed so that the experimental subjects can enter their information easily into the computer system“ (Shubik, 1975a, p. 235). Shubik did not succeed in building such a facility. Hoggatt, however, did. He upgraded the time-sharing microcomputer, terminals, video displays and increased storage capacity with the aid of another NSF grant (SOC75 - 08177) for the period 1971-1975.[101]

It was precisely Hoggatt’s focus on infrastructure, instead of building a community of researchers who would actually run experiments, that his laboratory failed. Far more successful were Selten and Smith. Their experience with Hoggatt’s laboratory was important in their decision to pursue their own laboratories. In the 1960s there were few experimentalists,; they were a geographically dispersed group. The difference between Smith, Plott, Selten in the 1980s and Hoggatt two decades before was that the first three

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[100] Letter by Martin Shubik to Dr. Robert J. Lundegard Head, Logistics & Mathematical Statistics Branch Office of Naval Research from February 1 1967. Box 12, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
A detailed description of the Yale gaming system is in SHUBIK, M. 1975a. *Games for society, business, and war : towards a theory of gaming,* New York, Elsevier. pp. 236-243.
[101] A report from 1976 describes the newly acquired equipment: “time-sharing system based on two microprogrammable META4 computer, with a shared core memory of 120,000 32-bit words, two disks having storage capacity totaling 24 million words, a magnetic-tape unit for backup and data preparation, a link to the Berkeley Campus CDC 6400 computer; modems for linking users and other systems outside the laboratory; eighteen hard-copy terminals, and a CRT display system consisting of twelve CRT screens with keyboard and light-pen and RAMTEK support unit.” Box 48, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

focused equally on creating a community around their labs and the facilities. I return in the next section to their laboratory-building efforts, which to some extent included the emulation of Hoggatt's laboratory. Before I can address the issue of community and the social infrastructure of a laboratory, however, the case of Battalio and Kagel's animal laboratory is worth considering.

3.1.2 Battalio and Kagel's Animal Laboratory

> "When I look back on it, I think it was a little bit crazy thing to do, because it was animals. Economics with animals? Come on, give me a break."[102]

John Kagel and Raymond Battalio were graduate students at Purdue from 1967 until 1970 and wrote there a joint dissertation under the supervision of Robert Basmann, an econometrician.[103] "[T]here was no history of Vernon's work since Vernon had left by the time I arrived. There was no history of that work there or any of that being taught."[104] Basmann was interested in finding what one could call well-controlled data. He was not interested in the typical field data that suffer from all the problems discussed in James Cox's case of data integrity as a driving force. He was looking for data that would allow economic theory to be tested without worrying about the auxiliary assumptions that standard field data require. Both Battalio and Kagel were tasked with "finding data sources that would really match up to what Basmann would call the primitives of the economic theory" such as consumer data in the theory of consumer choice. No part of the dissertation was ever published, nor was it well received, and at last Kagel after graduating, for lack of other job opportunities, followed Basmann to the University Texas A&M University. Battalio joined them there.

It was just happenstance that Kagel and Battalio learnt in the academic year 1971-72 how Skinnerian operant psychologists used token economies for therapeutic purposes. Kagel's brother in law was an animal operant psychologist. Upon hearing about Battalio and

[102] Interview with John Kagel.
[103] Robert L. Basmann (1926-) received his Ph.D. in Econometrics from Iowa State University in 1955. Currently he is an emeritus professor at the Department of Economics at Binghamton University where he moved in 1988. He has pioneered the research of econometric estimation and testing methods for simultaneous economic equation systems.
[104] John Kagel at the Witness Seminar.

Kagel's problems with acquiring suitable data about consumer choice, one of the brother in law's colleagues suggested that token economies could be a source of such data. These economies were implemented by clinical psychologists who applied Skinnerian psychology in their work with mental ward patients as a way of motivating them to conduct various activities. For performing various tasks, to do with personal hygiene, for example, patients received tokens which they could use for purchasing goods, such as cigarettes, coffee, candy, cookies, milk and soda. The standard reference work in the early 1970s that Battalio and Kagel used was the book by Ayllon and Azrin (Ayllon and Azrin, 1968). Together with Robin Winkler and his student Edwin Fisher who were running the token economy with Leonard Krasner at Central Islip State Hospital in New York they used 38 female chronic psychotics. Their cooperation led to a number of publications in economics, psychology and biology journals.[105]

In the early/mid-1970s Kagel was giving a talk about his token economies research at a seminar in the psychology department at the State University, New York at Stony Brook. After the seminar he was approached by an operant psychologist, Howard Rachlin, who with his graduate student Leonard Green had done similar experiments with rats and pigeons.[106] Soon they started a fruitful collaboration to which Rachlin and Green brought their expertise in running experiments with animals in operant chambers and Battalio, together with Kagel, their economics expertise, to interpret the collected data. Animals were not only cheaper than mental ward patients, but experiments on animals primarily allowed them far better experimental control. The first animals they used were at Rachlin's laboratory in Stony Brook, although Battalio and Kagel were based in Texas. Soon they applied for an NSF grant that would allow them to build an animal laboratory at Texas A&M.[107] Battalio had been an electrician in the Navy and one of his neighbors was a veterinarian. To run their animal experiments they used snap lead electronics that conducted electric current. Battalio was able to operate them and the veterinarian

[105] See for instance the following articles in economics journals KAGEL, J. H. 1972. Token Economies and Experimental Economics. *Journal of Political Economy,* 80**,** 779-785. BATTALIO, R. C., KAGEL, J. H., WINKLER, R. C., FISHER, E. B., BASMANN, R. L. & KRASNER, L. 1973. A Test of Consumer Demand Theory Using Observations of Individual Consumer Purchases. *Economic Inquiry,* 11**,** 411-428, BATTALIO, R. C., FISHER, E. B., KAGEL, J. H., BASMANN, R. L., WINKLER, R. C. & KRASNER, L. 1974. An Experimental Investigation of Consumer Behavior in a Controlled Environment. *Journal of Consumer Research,* 1. KAGEL, J. H., BATTALIO, R. C., WINKLER, R. C. & FISHER, E. B. 1977. Job Choice and Total Labor Supply: An Experimental Analysis. *Southern Economic Journal,* 44**,** 13-24, KAGEL, J. H. & BATTALIO, R. C. 1980. Marihuana and work performance: results from an experiment. *Journal of Human Resources,* 15.

[106] John Kagel Interview.

[107] This was the NSF grant "Experimental Studies of Consumer Demand and Labor Supply Behavior: Comparative Statics and Dynamical Adjustment Processes" that ran from 1978 to 1982.

neighbor could tell whether the rats were sick. Kagel was not involved with the programming. Green advised Battalio what equipment to buy and how to do the programming and he programmed the first experiment. They had two or three boxes, so-called operant chambers. Each had levers to push on. They pioneered the idea of keeping the rats in the box for 24 hours a day, in no small part because they did not at first have any holding facilities. Later on they acquired cages for their animals.

They began with one rat, but eventually scaled up to 22 (Bragg, 1986). Putting into action the idea of generating controlled data suitable for testing economic theory, they studied the effect of a substantial, twenty-five percent drop in income, which for rats relates to the amount of food and wealth in proportion to their weight. They also studied low income rats, that is, rats which are underfed, and high income rats, which were normally fed. Each rat was placed in a computer-controlled cage with two food dispensers. The first was connected to a lever that held one pellet of food for six seconds before giving it to the rat: the second gave it four pellets after holding them for twenty seconds.[108] The rich rats preferred not to wait and pressed the first lever, while the poor undernourished rats eagerly waited for more food. (Kagel et al., 1995, pp. 182-188)

They also demonstrated the existence of (subject-specific) Giffen goods in such a setting; they studied the income and substation effect; the backward bending labor supply curve which related the behaviors of rats to a similar trend in those of humans in regard to the hours worked and wage rate;, risk aversion with rats exhibiting risk averse behavior throughout all consumption levels; and the fanning out of indifference curves, to name a few.

Experimenting on animals provided the much-needed high quality controlled data that Kagel and Battalio wanted. While the idea and methodology of using animals were imported from psychology, issues from economic theory motivated their experimental designs. With time, both became more experienced and eventually were able to publish research on their own; often testing both economic and psychological theories of the same phenomena. Yet animal experiments never took off within economics. There were several reasons for this. First, one needed specific skills to program and operate the animal chambers with their snap shot electronics. Second, commitment to animal experiments required setting up an animal laboratory and maintaining it. This was not easy to do and, with time, laboratory requirements became more and more stringent,

[108] The times and quantities varied across treatments.

along with the limitations on what could be done with rats and pigeons. Third, the very topic and its relevance to economics were too esoteric for most people to get into. In addition, in the 1970s they were the only experimenters working on individual decision and classic preference theory. There was also a fourth reason – that with animals one cannot do experiments that one can do with humans – in particular, in game theory, which became increasingly important within economics as did experimental economics in the 1980s. For these reasons both felt compelled to move away from the very narrow niche of animal experiments to more 'mainstream' type experiments.

When Battalio and Kagel started their work on animal experiments, they believed that it would be welcomed by the profession as "real science." However, among economists the reception was varied. True, Battalio and Kagel's results were welcomed by those in favor of economic imperialism: that "as if" or subconscious rationality could even be found in rats and pigeons. Two of their papers were published by the Chicago-based Journal of Political Economy. Yet some economists viewed animal experiments as "crazy."[109]

Kagel left Texas A&M for the University of Houston in 1982. They continued working together, having a joint NSF grant running until 1991.[110] Their last paper on animal experiments appeared in the AER in 1990 (Kagel et al., 1990). Battalio remained at College Station until his premature death in 2004 (Kagel and van Huyck, 2007). The animal laboratory stayed with him since he had the necessary electrical skills to keep it running, while Kagel wanted to move to more 'mainstream' experimental work on humans. By around 1988, Battalio had closed the laboratory because it failed to comply with updated environmental standards and he moved the equipment to a psychology laboratory on campus. About two years later he sent the equipment to Green.[111] In 1995 Battalio, Kagel and Green published a comprehensive treatment of their methodology and findings, as a coda to their decade-long collaboration (Kagel et al., 1995).

In the academic year 1983/84 with funding by the NSF and the local Energy Laboratory, Kagel created a computer laboratory at the University of Houston to study human

[109] John Kagel Interview and the Witness Seminar. Two papers using animals in the Journal of Political Economy were: BATTALIO, R. C., KAGEL, J. H., RACHLIN, H. & GREEN, L. 1981. Commodity-Choice Behavior with Pigeons as Subjects. *Journal of Political Economy,* 89**,** 67-91, BATTALIO, R. C., KAGEL, J. H. & REYNOLDS, M. O. 1977. Income Distributions in Two Experimental Economies. Ibid.85**,** 1259-1271.

[110] National Science Foundation: "Collaborative Research in Experimental Studies of Choice Behavior in Certain and Uncertain Contexts" 8/85-1/88, $40,000. Renewal 7/87 -6/91, $151,000, with J. H. Kagel.

[111] John Van Huyck Interview.

behavior.[112] He began a long-term collaboration with a young MIT- educated theorist Dan Levin, mainly on auction theory and experiments (Dyer et al., 1989, Kagel and Levin, 1985, Kagel and Levin, 1986). Ronald Harstad, who stayed at Houston from 1983 to 1987, collaborated with them both and was primarily involved in programming experiments and managing the laboratory (Kagel et al., 1987, Harstad et al., 1990).[113] In 1988 Kagel was hired by Pittsburg with the promise of being able to build a computer lab there. He joined Al Roth who in the second half of the 1980s made a definite commitment to including experimental economics in his research portfolio.[114]

3.2 A Tale of Two Laboratories

In this section I want to complete the trajectory from opportune locations to dedicated spaces for experimental economics research. When Reinhard Selten was hired by Bonn University in 1984, he was wondering what to request and thought, "I should have a real laboratory."[115] The idea of having a computer laboratory was not novel for him, as he knew Hoggatt's laboratory from his research trips to the Bay Area in the 1960s. Selten was fully aware of the advantages of computer-assisted experiments and took Hoggatt's laboratory as a model for the first economic laboratory in Europe.[116] Selten's laboratory began with 6 computers and very limited space. It was used as workspace that had to be vacated during experiments. Gradually the number of computer stations increased to 12, 18, and finally 24 and the laboratory became fully dedicated to experimental research.[117] Selten's laboratory was the first in Europe. Its operation was somewhat secluded from

[112] John Kagel, Ronald Harstad interviews.
[113] Ronald M. Harstad is a game theorist and experimental economist who received a Ph.D. in Economics from the University of Pennsylvania in 1977. He was at the University of Illinois at Urbana-Champaign 1977-1981, where he collaborated with Keith Murnighan, Al Roth and Francoise Schoumaker on NSF funded Interactive Behavior Experimentation from 1979-1983. Later he moved to Texas A & M University (1981-1983) where he primarily worked with Raymond Battalio. Kagel was spending the academic year 1981/82 at the Hoover Institute and then left for the University of Houston where Harstad joined him (1983-1987). Together he collaborated with John Kagel and Dan Levin on the NSF funded "The Role of Information and Information Processing in Auctions: Theory and Experimentation" from 1984-87.
[114] Already in 1983 Roth was writing to Martin Shubik: "I share some of your skepticism about pure game-theoretic extensions to problems of bargaining and reputation. This is what led me into experimental work." Letter from May 18, 1983. Box 22, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[115] Reinhard Selten Interview.
[116] The second European laboratory seems to be the one at the University of York established by John Hey in the late 1980s. Source: ENABLE grant van Winden Papers.
[117] Reinhard Selten Interview.

the developments in North America to which I want now to return. In the following two subsections I discuss the history of two laboratories that are of particular historical significance. Vernon Smith's *Economic Science Laboratory* (ESL) at the University of Arizona was founded in 1985 and was predated by the so-called PLATO laboratory. Charles Plott's *Laboratory for Experimental and Political Science* (EEPS) at Caltech was founded in 1986. Both labs pursued initially different technologies for connecting experimental subjects through either terminals connected to a PLATO mainframe computer or locally networked computers. I follow their development including the demise of PLATO.

3.2.1 Smith's ESL in Arizona

Vernon Smith met Hoggatt through Lester Lave and the Ford Foundation Summer Workshops for Experimental Economics at Carnegie Mellon in the early 1960s. Smith did not get into computerized experiments until he moved to the University of Arizona.[118] In the spring of 1976 during his experimental economics class Mike Vannoni, an undergraduate student, told him about the PLATO system. PLATO stands for *Programmed Logic for Automated Teaching Operations* and was the first computer assisted instructional system for programmed learning. It was built and maintained by the University of Illinois in Urbana-Champaign's Computer-Based Education Research Laboratory (CERL) and Minnesota's Control Data Corporation (CDC) in the 1960s. In the

[118] A computerized laboratory was built at Purdue in the late 1960s. Bill Starbuck, who also attended the Ford Summer Workshop, spearheaded the efforts. FROMKIN, H. L. 1969. The Behavioral Science Laboratories at Purdue's Krannert School. *Administrative Science Quarterly,* 14**,** 171-177. This paper reports that Smith was also involved in the laboratory's design. Smith has no recollection of his involvement, but it was apparently built by the time when Smith had left Purdue in 1967. Vernon Smith Interview & Witness Seminar. Keith Murnighan recalls using the lab in the early 1970s for running his experiments (Keith Murnighan Interview).
Dennis Weidenaar served as the dean of the Krannert School of Management in the 1990s. He had attended Vernon Smith's experimental economics class in the 1960s. During his tenure as Dean, Charles Noussair, an experimentalist with a Ph.D. from Caltech, was hired and he restarted experimental economics at Purdue. When Noussair came to Purdue in 1994 he was the only experimentalist there. The lab built in the 1960s "was used only for classes. It had an observation deck and had the physical layout of an experimental lab although it was used for teaching classes. It did not have computers, [but] it was well laid out for pen and paper experiments" (Charles Noussair Interview). At first Noussair and later, after Timothy Cason joined Purdue in 1998, both used other classrooms that would support computerized experiments. Eventually they managed to equip the room with computers and return it to its original purpose. In 2002 the laboratory was named "Vernon Smith Experimental Economics Laboratory."

1980s the non-profit research unit became a profit oriented enterprise named NovaNET. [119]

By the 1970s a number of PLATO equipped facilities were being set up across the US, all equipped with “dumb“ terminals that were basically remote monitors connected by a telephone line to the “intelligent“ mainframe computer in Illinois[120] which processed everything and stored all data. The terminals were computers with plasma touch screens, which combined easy-to-follow display capabilities and easy-to-understand user responses with an ability to allow persons at several terminals to interact in a “common“ process. Stephen Rassenti recalls that “everybody was just enthralled with the idea that we could be communicating with each other using these machines.”[121]

The laboratory in Arizona was initially used for courses in physics and languages. It was housed on the third floor of the Science Library and equipped with sixteen terminals on line with the PLATO mainframe. The machines were rather large, a third of a meter wide, 2/3 of a meter tall and just as deep with a separate keyboard in front (see Figure 2). The keyboard was noisy and so were the machines. Cardboard screens hid the terminals of the subjects so that none of them could see what the others were doing.

TUTOR was the programming language of the PLATO system. The whole system was ahead of its time “in terms of enabling individual and group interactions all to be computerized.”[122] Its main use at the time was to write educational exercises (for physics – showing equations, graphs, illustrations of rolling balls, asking review questions; for language lessons – filling in blanks, showing correct answers). Stephen Rassenti, Arlington Williams and other collaborators of Smith used this interactivity as part of their trading program (Rassenti, 1990, Williams, 1980). They saw PLATO’s possibilities for economics experiments. In order to develop these possibilities they had to address unique problems such as message processing conflicts. When they were using the mainframe in Illinois, there were some 60 other groups using it at the same time. One of the computational

[119] For a history of PLATO see WORTHY, J. C. 1987. *William C. Norris: portrait of a maverick,* Cambridge, Mass., Ballinger Pub. Co.; HART, D. M. 2005. From "Ward of the State" to "Revolutionary Without a Movement": The Political Development of William C. Norris and Control Data Corporation, 1957-1986. *Enterprise and Society,* 6**,** 197-223. MCDONALD, C. F. 2011. *Building the information society: a history of computing as a mass medium.* Ph.D, doctoral dissertation, Princeton University. (contains a chapter on PLATO).

[120] The average current laptop has as much computing power as the mainframe in Illinois had. So most contemporary experiments could be done with the PLATO system of three decades ago.

[121] Stephen Rassenti Interview.

[122] Vernon Smith Interview.

features of the mainframe was timesharing, say 1/60 of a second of processing time devoted to each group. But from time to time subjects could observe congestion - delays in responses or temporary freeze ups of their displays, even if the phone line was working properly. The phone line communication was made over long distances on copper wire, which introduces noise, or so-called line errors. They could also pause the terminals, but also introduce outright errors in the data. Another type of problem was the intermittent unannounced taking down of the mainframe for maintenance. Cox recalls that one could be in the middle of an experiment and "all of a sudden, the computer would go down for maintenance."[123] Often, electrical storms in the Mid-West led to a message on the screen 'service temporarily suspended.'[124]

A particular case of a programming difficulty presents the importance of delivering messages to subjects at the very same time so that no one gets a time advantage in decision-making, for this can affect the outcome of an experiment. For instance, in double auction experiments the price shown needs to be updated whenever a new bid or offer is submitted, so that people do not act on old prices. When any bid or offer was being submitted, other players had to be disabled from submitting theirs. The programmers had to come up with technology that would reserve central memory for one individual at a time - a queuing system was needed and this was a programming challenge.

The idea of creating the Economic Science Laboratory (ESL) was circulated among the Arizona economics faculty and university administration in the summer of 1984. David Pingry penned the first draft. Isaac and Smith's comments were incorporated in the second. A year later, the ESL was established as a research center with Smith as its research director and the laboratory in the Science library was closed. When the ESL laboratory was completed, all the equipment was transferred to the Economics Department.[125] Many of the 16 terminals were old and did not function properly, while

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[123] James Cox Interview.

[124] Andy Schotter did not use PLATO, but had a computer terminal in his office that was connected to the NYU mainframe via a telephone line. His second experiment ever was computerized and appeared as SCHOTTER, A. & BRAUNSTEIN, Y. M. 1981. Economic Search: An Experimental Study. *Economic Inquiry,* 19**,** 1-25. It poses a one-person decision problem. Schotter recalls: "I would bring people in one at a time, they'd sit down, and then I'd have to call up the mainframe and connect through the phone line. Sometimes the phone line would go down and you'd lose all of the data for that session. But I would get people one at a time, they'd come in, and the program was such that after we gave the instructions, they say your first wage is this, do you want to continue searching or not, and they'd say yes or no. Then, if they said no, that would be it. If they said yes, then they would offer them another wage, and then they'd have to decide when to stop, so the theory predicts stopping behavior." Andrew Schotter Interview.

[125] The 1985 inventory lists all the PLATO equipment at University of Arizona: "13 Plato IV terminal Magnavox (all in Science Library); 8 Plato IST1 Terminal CDC (4 econ dept); 14 Plato IST Terminal CDC (5 in

excess demand for PLATO during peak hours impeded Smith group's research efforts. In late 1985 a three-year plan was drafted. It included installing "at least a two-system computer-markets capability (PLATO and non-PLATO) funded by a combination of internal and external funds."[126]

In the academic year 1987-8, ESL continued to prepare for the adoption of the new generation of "multisite interactive capability in the form of PLATO-NOVA. Far from being a dead horse, the University of Illinois [was] expending considerable resources to bring PLATO NOVA to the frontiers of interactive computing. Their NOVA system will uniquely fill the needs of experimental economists around the country."[127] The NovaNET system delivered PLATO from a new mainframe via a satellite communications system. It was designed to support up to 4000 users. In Tucson, a third party, CompuSat, which sent the signal to ESL over a local phone line, received the satellite signal. Reverse traffic went back to Urbana over a dedicated landline. The IBM-type microcomputers in the ESL were connected through a Local Area Network (LAN). The 1987-8 ESL report concluded,

> "it is clear that the highest standard for experimental economics laboratories is evolving to exactly the dual system [LAN and PLATO] which we now have."

A few years later, however, they were proved wrong.[128] The technology used in experimental labs moved exclusively to local networks. Mainframe computing, in which ESL had invested so heavily, was not a viable alternative, as the next subsection explains. The coincidental presence of PLATO in Arizona gave Smith and his associates an early head start. But its high cost and the developing networking computer technology pushed other experimentalists in a different direction.

Science Library); 12 CDC Micro Plato Disk Drives; 2 Versatec Printer and Interface CDC Printers in the Science laboratory, 18 microcomputers." Box 56, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[126] Box 55, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[127] 1992 NSF instrumentation grant, Box 59, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[128] Out of thirty-one North American labs in 1992, only 4 used PLATO/NovaNET and another 2 mainframes; 11 token ring local networks (e.g. Caltech), 7 Ethernet and one Arcnet local network mostly with Novel or the IBM Operating System, and three unspecified local area networks. For three of the labs no information is available.

3.2.2 Plott's EEPS at Caltech

In 1987 Charles Plott completed his multiyear effort to establish his own laboratory at Caltech. Hitherto he had used a variety of spaces for his experiments. There was a language laboratory in the basement of the Dabney Hall located near Plott's office, a classroom at Caltech or some other local university. These locations were chosen to fit the needs of the experiments. The language laboratory was good for controlling and monitoring subjects and was used in 'prisoner's dilemma' experiments. Likewise technology was developed and adapted according to current needs. When subjects needed to be isolated from each other yet able to exchange messages, they were placed in offices and linked through phone lines. In such cases Plott and his research assistant would communicate via walkie-talkies [129] (Hong and Plott, 1982). Despite coming up with these ingenious ways of conducting their experiments, the available technology had a number of shortcomings. Telephone lines were often busy during the experiment and seeking information by phone was costly in the sense of forgoing trading opportunities. Brian Binger, a Caltech Ph.D. student in the 1970s, recalls his participation in the Hong Plott experiments:[130]

> "Each trading session was signaled by a blast of an air horn, and then we were to dial other participants and try to make trades, and one of the things that I recall specifically that we soon learned that it was very difficult to get through on the telephone system. So, I think most of us adopted the strategy of dialing the first four digits of a trading partner and when the air horn was blown, we dialed the last digit and were able to get through and made a trade with that person without doing any particular searching. And I always wondered whether that technology impeded the ability to search for better alternatives."[131]

When market experiments were conducted, assistants were hired to record bids or collect bids and while walking to the principal experimentalist they arranged the bids in descending order. This saved time and speeded up experiments. The more complex the experiments became – e.g. with many subjects; not being paired, but operated simultaneously; multiple markets; real time interactions, etc. – the more the limitations

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[129] Other, less frequently used instruments in economic laboratories include bingo cages, dice, or walkie-talkies.

[130] Another participant in this experiment was Mark Isaac, a graduate colleague of Binger, Mark Spitzer and Elizabeth Hoffman. His participation made him want to attend Plott's experimental economics class which set him on the path to becoming an experimentalist.

[131] Brian Binger Interview.

of the experimental control became apparent. Loud subjects could monopolize the floor; subjects with a foreign background might be limited by their command of English; there were delays in displaying prices on the board, due to the speed of trading; the maintenance of records became problematic[132] (see for instance Plott and Sunder, 1982, Forsythe et al., 1982, Forsythe et al., 1984). These considerations led Plott to want a computerized laboratory.

The first good opportunity appeared in 1984. Since October 1984, Plott in collaboration with the Computer Science Department of Southern University in Alabama, had been studying "the choice, modeling, analysis, simulation, and demonstration of mechanisms for pricing" NASA Space Station services. This project motivated Plott to seek additional support from companies such as GM and IBM and private foundations and public funding agencies such as the NSF to create his own laboratory. All the funding was approved during summer of 1987 and the physical equipment (computers, desks, etc.) was installed in the fall of 1987.

EEPS was situated in the basement of Baxter Hall, where Plott and other economists have their offices, and replaced parts of the Caltech Art Gallery.[133] The initial participating faculty included Mahmoud El-Gamal, Peter Ordeshook, David Grether, Thomas Palfrey, John Ledyard, Richard McKelvey and David Porter. According to a report from September 1988, EEPS consisted of a large room in which experiments were normally conducted and two adjacent rooms which were used by programmers, monitors and for storage.

> "Twenty IBM PS/2 model 50's have been installed and networked in the large room [see Figure 4]. Each is on a table with rollers so the room can be reconfigured to meet the special needs of experiments. Other computers located in the adjacent rooms are used as monitors and also have the capability of processing data, running printers and a plotter, etc., during an experiment. Near the laboratory is a large classroom and several soundproof rooms used as practice rooms for musical instruments. All of this space can be made available and

[132] Charles Plott Interview.

[133] The art gallery's closure, which was supported by David Grether, then the Humanities and Social Sciences division chairman, stirred much bad feeling and even led to a small student protest – as recalled by Kim Border, who was then a Caltech undergraduate. Nowadays, he is a professor of economics at Caltech and still has the "Save the Art Gallery" t-shirt.

computers can be rolled into them should complete isolation of subjects be desired."[134]

Through the JPL project Plott met Hsing-Yang Lee, a graduate student of Southern University in Baton Rouge, Louisiana. He was subsequently hired as a laboratory technician and was responsible for creating the local area network of computers. Later he developed the Multiple Unit Double Auction, MUDA, an IBM DOS based software that was able to create the first network for twenty markets and up to twenty people at any personal computer (Plott, 1991, Plott and Gray, 1990).

In the early days of EEPS it was assumed that IBM's PC-net would become the industry standard for local networks, but this choice was abandoned in favor of Ethernet.[135] They had to reconfigure the software to it, which was not straightforward. In this regard a major difference between EEPS and ESL became apparent. ESL had developed over 40 PLATO software applications and spent over 3,000 hours of development time (Rassenti, 1990, p. 520). This locked them into the PLATO technology. EEPS, in contrast, had invested much less time and effort in PC-net, and thus moving to Ethernet was easier. One of the reasons for abandoning PLATO was its cost. At this time a connection to mainframe cost a couple of thousand dollars per month. With the proliferation of LAN technology and freely available packages such as MUDA it became standard practice in the experimental economics community to pay a programmer instead.

Before the development of MUDA, PLATO's advantage was its ability to run multiple markets on a network of computers, such as auctions or asset markets. MUDA was a major step forward in creating such environments on a system not based on PLATO. MUDA was a software package containing "a combination of a variety of different types of market organization." Each of the types can be "recovered from the composite."[136] Thus the electronic market could take many different forms accommodating up to 20

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[134] 1988 Annual Report to The Lynde and Harry Bradley Foundation on the Experimental Economics and Political Science Laboratory, Folder "Bradley Foundation 1987-1992 Original Proposal and Progress Reports." Plott Papers.

[135] Researchers were also engaged with a computerized laboratory at the very same time at Carnegie Mellon. Tom Palfrey recalls his colleague Sanje Srivastava, who built a ring type network of computers and programmed communication protocols linking individual computers. When Palfrey moved to Caltech in the fall of 1986, Plott already had his laboratory open, its computers networked with a more standardized protocol. "It wasn't a kludged network like what we did at Carnegie. But he let me use my method because all my programs were written so I got all these com ports and it was just this mess of wires all over there." Thomas Palfrey Interview.

[136] 1991 Annual Report to The Lynde and Harry Bradley Foundation on the Experimental Economics and Political Science Laboratory, Folder "Bradley Foundation 1987-1992 Original Proposal and Progress Reports." Plott Papers.

people and 20 markets with the option of written communication among subjects and multifactor and multiproduct production.[137] Plott has used MUDA for experiments on general equilibrium, international trade and other interdependent systems (e.g. Johnson and Plott, 1989, Plott and Gray, 1990, Plott, 1991, Güler et al., 1994, Noussair et al., 1995).

While the issue of reducing the cost of computer technology and local networks was crucial in the decline of PLATO and nurtured the rapid growth of experimental economics labs in the early 1990s, the story is incomplete if it considers the technological developments alone. The case of MUDA software suggests that the role of the experimental community is paramount in this development. The following section tries to disentangle how the laboratory place turned into a space.

3.3 From Place to Space: Computers &Community

So far my narrative has focused primarily on technology. To show that there is more to it than that, let me first go back to Austin Hoggatt and his laboratory and then traverse the various laboratories of the last four decades, often not in chronological order, to distill my argument about the laboratory as a space and its close ties with experimental communities.

We saw that several experimentalists such as Reinhard Selten, Vernon Smith and also James Friedman visited Hoggatt's laboratory in the late 1960s. Hoggatt tried to interest other people in using his "facility," but with little success. Garry Brewer at RAND wrote in 1972 to Martin Shubik about his meeting with Hoggatt: "The purpose of the meeting was never well defined, but my guess (corroborated later by Hoggatt) was that he is trying to appeal to interested garners outside of the Berkeley environment to use the laboratory and do PR work among professional colleagues."[138]

Hoggatt's state-of-the-art facility never established a permanent community of researchers, or, put in economics terms, it lacked a sufficient concentration of human capital. In sociological terms, it lacked a community. This was reflected in its budget. A

[137] Ibid.
[138] Letter of Garry Brewer to Shubik November 15, 1972. Box 16, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

closer look at Hoggatt's estimate of the money needed to replicate his laboratory suggests that the labor costs for programmers (salaries for experimentalists were excluded) constituted only 10% of the fixed costs of building the laboratory. The budgets for the first five years of operation of ESL and EEPS included the salaries of researchers and, together with all other salaries, constituted 80% of the whole.

Moreover, Hoggatt was more interested in perfecting his facility than in experimentation per se. For James Friedman, Hoggatt was "a great dreamer" who in a good and positive sense could foresee the computer-human interaction in an experimental laboratory. Hoggatt earned his Ph.D. at the University of Minnesota in 1959, writing apparently the first thesis in the United States that used simulations with a human-to computer interface.[139] In 1961, Hoggatt confided "I have been working rather hard on the problems of laboratory instrumentation for experimental gaming and I am sure I would have some results to present." He signed his letter: "A. Hoggatt 'Drone'."[140] Shubik characterized him as very bright, but "probably a little too much engineering oriented.[141] It sort of stopped him from having some of the theory insights he might have had."[142] When in early 1973 Daniel Friedman was visiting Berkeley as a doctoral student recruit, he recalls Hoggatt's proudly showing him around the laboratory (Friedman and Sunder 1994, p. 127) .Hoggatt proved more of a salesman than an experimentalist. He spent tremendous amounts on perfecting his laboratory and promoting the idea of computerized experimentation. Although his laboratory did not flourish, his idea of computerized experimentation took off a decade later.

Yet the story of investing in equipment instead of people is not restricted to Berkeley. Mark Isaac had a similar experience at Florida State University where he moved in 2001 after two decades in Arizona.

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[139] Hoggatt's Berkeley Obituary. FREY, U. 2009. *Austin Hoggatt, professor emeritus at the Haas School, dies at age 79* [Online]. Haas School of Business. Available: http://berkeley.edu/news/media/releases/2009/05/07_hoggattobit.shtml [Accessed October 6, 2012]. See also BALDERSTON, F. E. & HOGGATT, A. C. 1962. *Simulation of market processes,* Berkeley, Institute of Business and Economic Research, University of California.

[140] Letter from Austin Hoggatt to Martin Shubik February 20, 1961. Box 8, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[141] In his 1975 review of the facilities built to study various types of gaming, Martin Shubik summarized his views on Hoggatt's laboratory: "I have felt that behavioral scientists in general could be encouraged to do far more experimentation if a central facility were made available to them. This facility would have its own dedicated computer and system support to provide for a broad range of aids in building, running and analyzing different experiments in various behavioral sciences." SHUBIK, M. 1975a. *Games for society, business, and war : towards a theory of gaming,* New York, Elsevier.

[142] Martin Shubik Interview.

> "Tim [Salmon] and I had been given the offer at the same time. We found out that there was a lab that had been built in another building with a speculative idea that eventually people might learn how to use it. When Tim came, he actually took the plastic covers off the machines, so there was physically a lab in place."[143]

The hiring of an experienced experimenter such as Isaac was enough to turn FSU into a lively space for experimental research.

3.3.1 Social Infrastructure of an Economic Laboratory

When I talked to Catherine Eckel, a second-generation experimental economist, at the ESA conference in Tucson in 2009, she brought out precisely the different elements that take a laboratory clearly beyond matters of computer technology. She had spent the academic year 1994-5 as a visitor at the ESL. Although she had experience with running experiments since the early 1990s, it was her first experience with a laboratory. She soon learnt that there is more to a laboratory than a room full of computers. It is worth quoting her in full.

> "It's an environment where people can [1] *work together* to investigate different things, and you need various bits of support in order to make that work. You need [2] *programmers* who can help the experimentalists translate their ideas into screen designs. That's really important to have programmers who have a good sense of what a [3] *good interface* is and can help you refine your design so that it [4] *communicates easily with subjects*. And it's important to have a [5] *workshop*, so a situation where people can present ideas in kind of a [6] *safe environment*. So you can present new ideas and get your designs refined by feedback from other people. This is a really important part of a lab, is having that kind of [7] *mutual mentoring* almost environment. And also you need [8] *people to help run things*, so students, undergraduates and graduate students who help conduct the session: include the subjects, conduct the sessions, that kind of a thing."[144]

The various elements of a well-tempered laboratory in her description are numbered. The last, item 8, relates to support staff, items 1 and 5 through 7 to the operations of the laboratory community and the remaining items 2 through 4 to technical staff. Let me deal

[143] Marc Isaac Interview.
[144] Catherine Eckel Interview. My emphasis.

with the first two separately; the issue of technical staff is treated in an independent sub-section. One thing is clear already - these elements presupposed by laboratory experiments have to be seen as a community endeavor.

The support staff seem the least important element of a laboratory. Yet experimentalists agree that they are indispensable although they add with the same breath that they are the most difficult to procure and keep. Their salaries are usually not covered by grants and institutional support is hard to gain. Take for instance Stephen Rassenti, the current director of the various labs at Chapman University where Smith and his associates moved in 2007.

> "A lab is not just a physical facility, they don't work well without personnel to accompany the physical facility. By that I do not only mean the lab director, I mean a computer programmer and Jennifer. Jennifer organizes and runs all our experiments for us, schedules people and without her this laboratory would not work the way it works. It took us a lot of time to figure this out that we needed a Jennifer. We have a full time programmer also, and we are hiring another full time programmer. Basically, you need technical people operating the laboratory for you in addition to research faculty. Otherwise it just doesn't work."[145]

Though Rassenti emphasizes the technical personnel, it is also the administration - the "Jennifer" - who makes things run smoothly.

Creating an active experimental research community is a deeply ingrained idea that experimentalists follow whenever they change places. For instance, James Cox transferred from Arizona to GSU in 2005. A physical laboratory was already present, but the community had to be cultivated.[146]

> "What I needed to do when I came here was to create a center. I didn't have to create a physical lab; I needed to create a center because experimental economics did not have a presence here. There was no undergraduate course in experimental economics. There was no Ph.D. field in it."[147]

Cox proceeded in several directions, as Mark Isaac had done at FSU and many other experimentalists coming to new departments in the late 1990s and later. He started to

[145] Stephen Rassenti Interview, description of the laboratories at Chapman University.
[146] Some faculty members were interested or involved in doing experiments: Ron Cummings himself and Paul Ferraro and Laura Taylor were environmental economists who also did some experiments.
[147] James Cox Interview.

teach a graduate course on experimental methods. The class follows the format developed by Smith at Purdue in the 1960s and Plott and Smith at Caltech in the early 1970s. Beside reading the classic papers and an assortment of recent papers on various experimental topics, students get primarily hands-on experience of designing and running their own experiments. This helps them discover the driving forces of the turn. Experimental data are a natural part of economics (integrity), experimentation part of an economist's toolkit with a particular emphasis on theory testing and development (symmetry and the virtuous circle), and they get directly involved in producing the data through programming, execution and data analysis (rigorousness). A seminar series or a workshop with visitors and local students presenting, teaching undergraduate courses, using students as research assistants – they all contribute to the development of a community centered on the laboratory. Furthermore, these elements have various roles which are determined by the size of the laboratory community and the resulting division of labor. Let me turn to this aspect now.

3.3.2 Division of Labor

With Charles Holt as the most notable exception, most first generation experimentalists in the 1980s and 1990s did not learn how to program and had to rely on programmers. These have always been mostly professional programmers. With the dissemination of software packages, programming is increasingly done by graduate students, in particular when working on their dissertation research.[148]

Learning how to program is a significant investment. Those, for instance, who learnt the TUTOR language for PLATO in the 1970s and 1980s found it "very facilitative" for what they were doing. According to Smith, "it took quite a while when that technology changed. We had such a great investment in it that it was difficult to switch. We continued to use it even though we began to use the DOS operating system that IBM had developed, which was more portable."[149] A quarter century later, Rassenti is "still partial" to TUTOR.[150] All of the PLATO based software had to be either reprogrammed or abandoned. Smith had a whole cadre of assistants (e.g. Rassenti, LaMaster), students (Williams), and external programmers. Plott has his laboratory technicians and a mix of

[148] The programming of experiments for contract research is primarily done by professional programmers.
[149] Vernon Smith Interview. My emphasis.
[150] Stephen Rassenti Interview.

undergraduate and graduate students. Even in the case of the animal experiments of Kagel and Battalio that we encountered in the previous chapter, only Battalio, who had been an electrical engineer in the Navy, knew how to program the snap shot electronics of the operant chambers.

Programmers were essential, but good ones were hard to retain. Already in the Budgetary Plan for Fiscal 1985-1986, ESL's first year of operation, Smith argued that retaining Stephen Rassenti and Shawn LaMaster[151] was essential:

> "they are key systems engineering and programming personnel who are at the core of ESL, essential to a smooth transition and to its prospering. … Dr. Rassenti is a unique blend of systems engineer, computer scientist, and economist. He is essential to the continuation of the ESL's on-going software development work, the development of new ESL expert software, and in determining ESL's long term hardware and software needs."[152]

In later years, Smith pushed hard to keep increasing their salaries to remain competitive with IT industry standards. ESL's initial budget from 1985 supported nine full time positions. The five half-time distinguished researchers included the ESL director, a theorist committed to research in the development of theory disciplined by laboratory or field experimental data, an econometrician working on relevant econometric issues pertaining to the relationship of theory and experimental data, an experimental economist, and a scholar with an interest in applied experimental research. Furthermore appointments for technical staff research assistants, visiting scholars, postdoctoral and pre-doctoral students, and a half-time secretary would satisfy all the ESL (laboratory and faculty) secretarial needs.[153]

This look at ESL suggests a far more detailed description of the composition of the laboratory community at a large and well-funded laboratory. Plott identified similar elements of a functioning laboratory and adds some - resident and visiting - theorists as one of the ingredients of his laboratory.

> "Competing theories constitute the heart of an experiment. The process of distillation takes time and the theorists must work closely with the

[151] LaMaster graduated from University of Arizona in 1985 and worked in various capacities on ESL experimental economics projects.
[152] Box 55, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[153] ESL proposal Box 55, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

experimentalists. … Successful interaction among scholars will require face-to-face communication for short periods varying from a week to two or three months. The interaction should take place before an initial design, again after pilot experiments, and finally during the reporting phases."[154]

The division of labor between experimentalists and theorists participating on a joint research project has also been noted in other disciplines such as physics (Galison, 1987). It has evidently emerged in economics as well. Examples include Kagel & Levin in the late 1980s; Roth and Murnighan, Cox and Smith in the early 1980s, Hoffman & Spitzer. Kagel puts it: "I tease Dan that he doesn't know where the lab is. But that's a perfectly great division of labor."[155] Al Roth replied similarly to the question about how often he goes to the laboratory:

> "[N]ot very often. I'm often there when we're testing what the software looks like, but I am no longer [involved]. I haven't run subjects in a while. No, almost all of my experiments are collaborations with someone. Normally, it's my coauthor who's running the subjects."[156]

At the same time, people such as Charles Plott, Charles Holt and many others still do their experiments and are actively involved with training their student assistants.

We see that the division of labor in experimental economics laboratories takes place on several levels such as theory-experiment and theorist-experimentalist, experimental design and its computerized implementation with experimentalists – programmers, the running of experiments with experimentalists and (graduate student) research assistants, administering laboratory activities with experimentalists and support staff. The various specialized skills make experimentation a group endeavor. This poses the question of maintaining control when various people are involved and not a single person can fully follow each step. There are internal checks for control – running pilots to make sure that all programming bugs are eliminated, training students to run experiments by experienced experimentalists or senior graduate students; experimentalists being competent in theory and econometrics. One particular way of maintaining control has

[154] A Proposal to The Lynde and Harry Bradley Foundation for the Laboratory for Experiments in Economics and Political Science, June 1987, Folder "Bradley Foundation 1987-1992 Original Proposal and Progress Reports." Plott Papers.
[155] John Kagel Interview. For instance Levin has carried out experiments without collaborating with Kagel.
[156] Alvin Roth at the Witness Seminar.

been the introduction and sharing of various software packages for economic experimentation.

3.3.3 Portability

Back in 1994 Daniel Friedman and Shyam Sunder emphasized that much software is tailor-made to a specific research agenda and thus lacks “the generality, transportability, and documentation of commercial software” that would make it portable to other laboratories, “though Plott's MUDA comes close“ (Friedman and Sunder, 1994, pp. 66-7). And easy transportability was an important motivation for Plott’s development of MUDA:

> “At Caltech the policy has been to develop tools and package them so they can be used with hardware that exists in most universities to create a modern functioning laboratory almost instantaneously. By simply asking for it, researchers can receive a developed and tested core of hardware specifications and configurations; software that supports many different types of experiments; subject training software; data viewing and graphics software; organizational technology, procedures, etc. Copies of this package have been distributed to over ninety different universities within the last two or three years and within this time, over twenty newly created laboratories have been established from it and many more are on line. The e-mail and phone requests for help and advice arrive constantly (All help is given freely).”[157]

Apart from portability, MUDA contributed to the lowering of costs of entry to experimental economics. Plott’s policy was to develop software instruments that “must be portable to other laboratories and researchers” or “can be used with hardware that exists in most universities to create a modem functioning laboratory almost instantaneously.”[158] A further software requirement was to be inexpensively adjustable to local needs. Distribution of these packages has been free and was developed and tested on a core of hardware specifications and configurations. The software was

[157] Proposal for Instrumentation for Institutional Process Design and Laboratory Testing in Economics and Political Science, Folder “Bradley Foundation 1987-1992 Original Proposal and Progress Reports.” Plott Papers.

[158] 1991 Annual Report to The Lynde and Harry Bradley Foundation on the Experimental Economics and Political Science Laboratory, Folder “Bradley Foundation 1987-1992 Original Proposal and Progress Reports.” Plott Papers.

designed so that it could support various types of experiment[159] and contained subject training software, data viewing and graphics software, organizational technology and procedures (discussed further in the section on control). Within a year of EEPS' operation MUDA was adopted by fifteen other universities in which laboratories were being developed or considered. In 1991, it was actively used at eighteen universities worldwide and 80 copies were distributed to people who were exploring the establishment of laboratories.[160]

Almost two decades later the variety of available software, including open-source, has continued to grow. At the turn of the century Plott and his programmers created *Marketscape*, an open-source "web server and software system for creating and deploying laboratory research experiments studying the economics of market behavior" (Brewer, 2009).[161] Charles Holt has "written about 60 interactive web-based programs that are available for general use, in particular for teaching" on his website (Holt, 1999-2014). With substantial NSF support and James Cox as principal investigator, *EconPort*, "an economics digital library specializing in content that emphasizes the use of experiments in teaching and research," was developed (Cox, 2006). A number of recruitment software programs have been developed. The *Online Recruitment System for Economic Experiments* (ORSEE) created by Ben Greiner and used by almost seventy labs is the most popular. The CASSEL-SSEL laboratory at UCLA & Caltech has a number of open-source, Java-based programs.[162]

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[159] The 1988 report mentions programs for two different tâtonnement processes; a program for multiple unit auctions; a general purpose program for matrix games with asymmetric information; and programs for voting and elections experiments.

[160] "These include England, Germany and the USSR. Laboratories are being developed with the software in Spain (two locations) and Holland. Other countries in varying stages of development are France, Canada, Australia, Japan, Austria, and Sweden. The software has been translated into both Spanish and Russian." 1991 Annual Report to The Lynde and Harry Bradley Foundation on the Experimental Economics and Political Science Laboratory, Folder "Bradley Foundation 1987-1992 Original Proposal and Progress Reports." Plott Papers

[161] "Marketscape is a portable web server and software system for creating and deploying laboratory research experiments studying the economics of market behavior. We keep the system on portable media (a CD and a USB pen drive) with a reasonable hope that you can go into any computer laboratory, anywhere, and just plug it in and run it, or just run it from your laptop or desktop that is plugged in to your university's network. Obviously, that won't always work for several reasons. But we believe it can work for enough users to be quite useful and powerful. Human subjects can access the experiments from almost any web browser. Experiments can include: single or multiple markets; multiple goods and currencies; multiple types of subjects; varying economic conditions or incentives." BREWER, P. J. 2009. *Experimental Economics with Marketscape* [Online]. Available: http://marketscape.caltech.edu/wiki [Accessed September 26, 2014].

[162] For subject recruitment (MooreRecruiting), a broad class of multi-stage games (Multistage), and large-scale experiments on interdependent systems (jMarkets) *Software Resources: Social Science Experimental*

For more than a decade the most popular, possibly the most frequently used, software package has been *z-Tree* which stands for the Zurich Toolbox for Readymade Economic Experiments (Greiner, 2004, Greiner, 2004-2014). It was developed in the late 1990s by Urs Fischbacher with impetus from Ernst Fehr and provides a stable, flexible, and easy to use software, even for those with few programming skills (Fischbacher, 2007, Fischbacher, 2007-2014). It is not restricted to market experiments as MUDA is and has been an easy programming entry point for graduate students.

And the list of software could go on.[163] What these efforts of developing and sharing software show is not standardization in the sense of using the same software over and over again for the sake of economy, but an attempt to lower the costs of entry and at the same time to guarantee experimental control. The "access to the tools of the trade" has been an important aspect of the "social economy" of the experimental economists (Kohler, 1994, Biagioli, 1999). Although it has never reached the levels of the fruit flies community, the importance of portable software should not be underestimated.

With Selten being somewhat isolated in Germany and Smith locked in the prohibitively expensive PLATO, it was Plott's EEPS that at the turn of the 1980s served as a standard of organization and technology for economics laboratories.[164] The reason for the rapid rise in the number of groups and laboratories does not lie altogether in the decreasing costs of computer technology, but rather in the culture of sharing software, the existing demand for experiments for which computer technology was relevant, experience with this technology, and other means of lowering the barriers to entry to the field.

We need to see these different ingredients - portable software, support staff, programmers, researchers - in order to understand how economists could engage in experiments that transcended the boundaries of hand-run experiments without compromising experimental control.

Laboratory (SSEL) [Online]. Available: http://www.ssel.caltech.edu/info/index.php?option=com_content&view=article&id=4&Itemid=6 [Accessed March 26, 2010].

[163] An unrepresentative online survey among ESA members from 2009 with 30 respondents listed the following software: 16 - z-Tree; 6- Visual Basic (.NET and VBA pooled); 3 – MatLab; 3 - Java and Javascript; 2 – Mathematica; 2 – Python; 2 – php; 1 - C#; 1 – MySQL; 1 - html and css; Others: REGATE software, online survey software such as Qualtrics, Psychphysics toolbox, Rat Image, Delphi toolbox, Delphi 7, Veconlab, and EconPort
Source: [ESA-discuss] What software are you using to program experiments? Dec 5, 2009.

[164] It remains unclear how far the laboratory in Pittsburgh had a similar role. Plott and his programmer Lee helped to established labs in Pompeu Fabra and Grenoble in 1991/2. Similar work was done by Smith and Rassenti throughout the 1990s.

3.4 Experimental Economics in Amsterdam

The establishment of the experimental economics community at the University of Amsterdam with its laboratory can be used as a case study that illustrates how the elements discussed in the previous section – community, laboratory, computer technology, funding and culture sharing among experimental economists – meshed together. The case of one of the most successful labs and experimental communities depicts the double helix nature of the epistemic and sociological strands of the experimental turn.

Every September from 1992 until 1997 *Workshops on Experimental Economics* took place in Amsterdam. Far beyond ESA meetings in their international scope, they had a major impact on both the European and American experimental economics communities. A generous Pioneer Project of the Dutch Science Foundation funded them. As part of establishing a new research program at the University of Amsterdam, an economics laboratory and a community of local scholars were funded, with a view to applying experimental methods to economic and political issues. *The Center for Research in Experimental Economics and political Decision-making*, CREED, was the direct consequence of the Pioneer grant. It was led by Frans van Winden and his doctoral student, Arthur Schram. The aim of the workshops was to promote the transfer of best practice in conducting experimental research and over the years it brought to Amsterdam essentially everyone involved from North America and everyone from the fledgling community in Europe.[165] It boosted the academic reputation of CREED, propelling it to the focal point of the European experimental community[166] and provided space for networking with other experimentalists; the importance of it was felt "especially for people in Europe."[167]

[165] The Amsterdam workshops had 40 to 50 participants. From the participants of the Witness Seminar only James Friedman and Steven Rassenti did not participate. The former was not active as an experimentalist at the time; the latter specializes in applied contract research in collaboration with Vernon Smith. Notable experimentalists who attended the workshops include Albers, Andreoni, Bohm, Bohnet, Bolton, Camerer, Cox, Fehr, Forsythe, Daniel Friedman, Gächter, Güth, Holt Harrison, Harstad, Hey, van Damme, Grether, Hoffman, Kagel, Levin, Loomes, McKelvey, Nagel, Olson, Palfrey, Plott, Porter, Potters, Roth, Selten, Smith, Starmer, Sugden, Tietz, Weber. The regular local attendees included, for instance, Arthur Schram, Frans van Winden, Theo Offerman and Joep Soenemans.

[166] This was also perceived by Americans – e.g. Harrison, Cox, and Palfrey interviews. In the first half of the 1990s, CREED's reputation was not based on their research output, which experienced a significant dip, once CREED scholars involved moved into experimental research.

[167] Frans van Winden Interview.

Before I focus on the CREED group I should need to examine the career trajectory of Frans van Winden, the group's senior figure and leader.

Van Winden became a professor at the University of Amsterdam in 1983.[168] His interest in experimental economics grew with his exposure to it. Coming from a public choice background, he learned of Charles Plott and his experimental work on voting and coalition experiments. In 1986 he spent a semester at Caltech but with the intention of working on topics in political economy. He happened to have an office next to Plott's and they discussed also market experiments and other things as "they turned up. It was very nice. In addition for me it was very important to learn the tricks. I was an absolute beginner. I made many notes, perhaps about trivial things, just how to run the protocol, the procedures of an experiment. If you have never done it, it is all unknown territory." Plott at the time was receiving grants to create a computer laboratory at Caltech. Van Winden attended some of Plott's experiments, but no joint research came of it.

In 1987-8, the next academic year, van Winden was the President of the *European Public Choice Society* and attended the game theory research year at the *Center for Interdisciplinary Research* of the University of Bielefeld.[169] Apart from Reinhard Selten, the organizer of the event, a number of researchers interested in experimental economics were present. The research conducted was however theoretical. Discussions with both Selten and Güth precipitated van Winden's decision to launch his experimental work as they encouraged him "not to wait too long, just start, it is not that difficult, just start."[170] The contacts van Winden made in Bielefeld helped to set the stage for the Amsterdam workshops.

The Dutch Science Foundation developed a new scheme around 1988 – a personal development subsidy in support of big projects for younger researchers. It provided substantial funding and later it was renamed the Pioneer Subsidy. Van Winden after years

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[168] After obtaining a masters' degree in Amsterdam, van Winden was an assistant professor in Leiden 1973-1980 and associate professor in Utrecht 1980-1983. He obtained his Ph.D. (with the highest honors: cum laude) in 1981 from Leiden University, the Netherlands.

[169] In academic year 1987/8 Selten organized a research year Game Theory in the Behavioral Sciences [not sure about this formal title] at the Institute for Interdisciplinary Research. There were many people from various fields (econ, biology, sociology, psychology, political science, behavioral sciences) including experimenters done by James Lang, Albers (computer-aided spatial games), Harstad (A Framing Effect observed in a Market Game); Walker, Gardner, Ostrom (common pool resources). Most of the research was non-experimental, mostly theoretical. It was mostly directed towards non-cooperative game theory. Four volumes Game Equilibrium Models were published in 1991. SELTEN, R. 1991. *Game equilibrium models,* Berlin; New York, Springer Verlag.

[170] Frans van Winden Interview.

of toying with the idea of doing experimental research himself applied with the intention of creating a laboratory. The application was declined despite being favorably evaluated, because the reviewers thought that he needed to attract psychologists to his team. They did not believe that van Winden could start a laboratory on his own without any significant laboratory experience. Moreover, van Winden was considered to be a senior scholar and thus not eligible for the award. He was advised to join forces with a younger researcher and submit a joint proposal. Arthur Schram, van Winden's student, finished his Ph.D. in 1989. Their proposal was successful, Schram being the formal 'pioneer' of the project, on condition that van Winden would become the director of CREED and would play a central role in the elaboration and monitoring of the research program. In their proposal they not only promised to create an experimental research group with a computerized laboratory that would integrate experimental economics with political decision-making, but also to disseminate experimental research across the Netherlands and help to start new labs. The full name of the laboratory became the *Center for Research in Experimental Economics and political Decision-making* (CREED). The Pioneer grant provided full support for six years – salaries, experimental expenses, the cost of setting up the laboratory, workshop series. Computerized experiments were already quite established as a tool and the cost of setting up such a laboratory was substantially lower than five years before, when Plott was opening his laboratory. They also hired Mark Olson, a Ph.D. student of Charles Plott, thus importing a fully trained experimentalist. He stayed until 1996 and then moved back to the US.

Designing the laboratory was taken very seriously. In November 1990 van Winden and Schram visited several labs in the US: Caltech (Plott), Arizona (Smith), Virginia (Ron Harstad, Doug Davis), Pittsburgh (Roth), Carnegie Mellon (Sunder), but also the one in Bonn (Selten). These trips were enough for them to figure out what they wanted. They realized that they did not want to get simply a room with 30 workstations; they wanted also a reception room and also an observation room. They thought this was an ideal setting, since psychologists also use observation rooms where the experimenter stays during the experiment. Initially the experimenters remained there, but later they remained in the main room so that they could be closer to their subjects when problems arose (e.g. answering questions, computer malfunctions, etc.]. They also talked to psychologists for instance about the colors of the room. A color specialist was involved to make it "not too heavy, not too boring." They thought immediately of installing an audio system, but in the end decided not to. They also installed a computer network that was independent from the university's because this s it gave them the freedom to manage the network without having to follow university rules and restrictions on managing computer networks.

The first experiments (1991) were carried out at the Saskia House, near de Waag (the old Customs building from the 17th century) in the Jodenbreestraat. However, in 1992 the economics faculty moved to another location at the Roeterseiland complex. Despite the tedium of having to set up everything twice, the laboratory at Roeterseiland was much better; for instance, the laboratory in the Saskia House was smaller and had a slanting floor which was not always ideal for the comfort of the subjects and modifying what they saw, nor for the positioning of desks for computers.

When the laboratory re-opened in 1992, van Winden said in a speech that "this is a sacred ground from the experimentalists' point of view." He was referring to the van den Waals-Zeeman laboratory, named after two Nobel Prize winners; the Van't Hoff laboratory in chemistry, named after another Nobel laureate, and the psychologists in the nearby university building, who also had a very good reputation. "We felt modest and had to build up a reputation." When CREED is pronounced in Dutch – [kreet] – it means 'cry' or 'shout'. Van Winden asserted that building reputation required establishing social links. For this they adjusted the Iowa Political Stock Market to Dutch conditions. It was the opening experiment at CREED and it attracted much attention from the media and also the faculty.

The annual workshops and the visiting experimentalists apprised researchers in Amsterdam of the latest developments, which helped to establish CREED's academic reputation, but also fostered the integration of the experimental community. When van Winden was giving seminars in later years he was often told that the Amsterdam workshops were instrumental in starting up European experimentation outside of Germany. CREED was the third laboratory in Europe after Bonn and John Hey's laboratory in the UK. Labs in Barcelona and Grenoble opened soon after (1992). Some very important research was presented in its early stages in Amsterdam – the trust games by McCabe, Berg, and Dickhaut (Berg et al., 1995); the Quantal Response Equilibrium theory by McKelvey and Palfrey (McKelvey and Palfrey, 1995); or the gift exchange game presented by Ernst Fehr, Kirchsteiger, and Riedl (Fehr et al., 1993). According to those who attended the early Tucson workshops the atmosphere in Amsterdam was very similar.[171] Frans van Winden, one of the co-organizers, described the gathering as "a small, very enthusiastic group."[172] The program had only single sessions, which afforded a joint experience of presentations and comments, and sometimes disagreement about how to do experiments in particular between the Germans and North Americans.

This section completes the history with its twists and dead ends of the emergence of modern experimental economics laboratories. We have seen the complex interplay of technology, local communities with the division of labor, the role of funding, but also the operation of the experimental community as a whole. However, in order to understand how laboratories turned into a space for creating new knowledge, we need to examine the processes inside of the laboratory.

3.5 Experimental Control and other Epistemic Functions of Laboratory Space

> "The term 'experimentation' refers to a design and not to a location. The essential features of experimental design are control and comparison, with randomization an essential part of control. Where randomization can be achieved in the field, experiments can be conducted away from any laboratory" (Siegel, 1964, p. 20).

Sidney Siegel's quotation highlights an important issue of laboratories in any discipline. One can conduct controlled experiments not only inside of them, but also in other locations such as classrooms, casinos, field locations, essentially anywhere, if one can properly control the relevant factors.

For instance, with the advent of portable computers, wireless networks and broadband Internet, it has become much easier to transform various spaces into an experimental economics laboratory.[173] It is even possible to engage subjects at various locations and connect them through the experimental software with the experimenter. Figure 10 depicts an experimental session conducted at the SSEL in March 2012. A new type of call

[171] Interviews with multiple participants such as Charles Holt, James Cox, Mark Isaac and others.
[172] Frans van Winden Interview.
[173] The first mobile laboratory was probably established in 1998 in Wyoming by Jason Shogren. WEEEL-*Wyoming Environmental Experimental Economics Mobile Lab* had ten laptops and one master laptop. Another mobile laboratory was founded at the turn of the century by Andreas Ortmann in Prague. Since then the number has steadily grown.

market was studied using the *Marketscape* software. Two experimentalists can be seen, Plott and an undergraduate student enrolled in his class on experimental methods in economics and political science; a graduate student working on a related theoretical issue and a programmer who resolves any technical issues that may arise during the experiment. No subjects were physically present in the laboratory and none actually came into physical contact with the research team.[174]

While this example poses the question 'What and where exactly is the laboratory?' it reinforces Siegel's original claim. Furthermore, this example is quite unique and demonstrates some of the most recent and still infrequent ways of running experiments. Nevertheless, it allows me to consider the connection between experimental control and the laboratory space.

Plott's early experiments on majority rule four decades before (1972/3) were undertaken in a classroom in the Baxter Hall, a newly opened building for the Division of Humanities and Social Sciences at Caltech. For the first applied experimental research study in 1977 with James Hong, various offices in Baxter were used. The need was to separate subjects in order to allow them to communicate with other subjects via telephone calls. Plott and Hong were personally collecting the data from the subjects. A horn was used to announce a new period (Hong and Plott, 1982). One of the subjects, an economics graduate student called Brian Binger, recalled that once a new period was announced it was difficult to get connected to other subjects. Therefore subjects devised a strategy of dialing the first three digits of the four and dialing the fourth immediately after the horn sound. They did not shop around for better deals, but agreed a deal with the first person connected. If the experiment were to be conducted in a laboratory with subjects sitting behind networked computers in the laboratory, separated by partitions and communicating by chatting on the screen of their computer stations, such issues with compromised experimental control could easily be obviated.

The design of EEPS was based on Plott's experience of using classrooms. It was supposed to address a range of needs such as public goods, voting mechanisms, and market experiments. The laboratory had been designed in a way that allows it to be reconfigured for a variety of experiments, including bizarre ones. Tables are on wheels, partitions are movable, the walls are painted with flowers by Plott's wife, who is an artist, and paintings are placed to create a relaxing atmosphere, for Plott believes that this is an important

[174] E.g. subjects were paid by mailing checks/online.

factor[175] (see Figure 6). The layout has remained essentially the same for the past 25 years. Plott was and still is not in favor of fixed partitions. He prefers to have a white-board all around the room so that instructions or follow up questions can be explained from any angle. The laboratory is well positioned within the building to draw subjects in and isolate them: "Since it is close to a parking lot and has its own entrance, restroom facilities, water, etc., the entire laboratory area can be isolated from the rest of the building."[176]

Reinhard Selten, in contrast, prefers fixed partitions with curtains (see Figure 5). He first encountered cubicles at Hoggatt's laboratory in Berkeley. Those cubicles were completely isolated, which is what made them so expensive. Because Stöpper, a colleague psychologist in Bielefeld where Selten worked before moving to Bonn, used to leave the cubicle doors ajar, since he claimed that some people get claustrophobic in closed cubicles, Selten decided to have curtains instead. The issue is not simply the subjects' well-being; it is a trade-off between the high cost of setting up isolated cubicles with an appropriate monitoring of subjects when the doors are closed and the requirements of isolating subjects from each other. Curtains are not sound-proof, but they are sufficient to achieve Selten's purpose of isolating subjects. Unlike Hoggatt, Selten did not demand the isolation of the experimentalist from the subjects. Curtains remain a unique way of isolating subjects, most likely used only in Bonn.[177]

The laboratory at Georgia State University led by James Cox has two different types of partition. Figure 7 shows computers in the front sunk into desks with low black barriers around them. At the back are large movable white partitions for surrounding each computer. The barriers let the subjects both see each other and the experimentalist and thus they can observe whether someone is doing or not doing something, but they cannot see easily what is displayed on the neighboring monitors. With movable partitions, the subjects' isolation is stricter. The presence of permanent (see Figure 8 and Figure 9) or removable partitions in part reflects the perennial space constraints, but also the fact that many labs were created through a gradual transformation of classrooms into

[175] Keith Murnighan, a social psychologist, in our interview stated that the color of the walls in social psychology labs is taken seriously.
[176] 1988 Annual Report to The Lynde and Harry Bradley Foundation on the Experimental Economics and Political Science Laboratory, Folder "Bradley Foundation 1987-1992 Original Proposal and Progress Reports." Plott Papers.
[177] Reinhard Selten Interview.

computer rooms and eventually experimental economics laboratories that were not deliberately built as labs.

Next to EEPS there is another experimental economics laboratory at Caltech, the SSEL. It was opened in the summer of 1999 and is run by a group of Caltech experimentalists – Tom Palfrey, John Ledyard, Colin Camerer, and formerly Richard McKelvey and Jacob Goeree. Palfrey and McKelvey jointly designed the laboratory. It has permanent partitions and a quite different layout than EEPS (see Figure 9). The reason for this was Tom Palfrey's primary interest in game theory. In such experiments "you really have to have much more control over [the] ability of the subjects to communicate, and eye contact, things like that. You want more privacy, I think, in game theory experience. Market experiments, it doesn't matter. They're all so busy trying to trade they're staring at their screen. Other kinds of experiments, I worry about that." Computers are organized in several rows all directed towards a podium with the experimentalist station, white-board and projector screen.

When the new economics department building in Arizona (the McClelland Hall) was being designed, including a new ESL equipped with 40 stations, the main issues of concern were also privacy and visibility, two aspects of experimental control and intervention. The main person involved in designing the laboratory was Stephen Rassenti. He wanted to see the subjects and also installed microphones in the ceilings to hear whether the subjects chatted. On the technical aspect of the laboratory specification, Rassenti asked for the best possible system that could be installed. They had limited space and Rassenti thought that they used it efficiently. It used to be occupied all day almost every day. When most of the ESL members moved in 2002 to GMU, the new laboratory built there was not ideal. In an effort to save money in building the laboratory, the stations were a little too small for comfort. There were half partitions, which did not go far enough. When someone leaned back in her chair she could see her neighbor's screen. These are elements of experimental knowledge that only an experimentalist with extensive experience with experiments appreciates, because (s)he has learnt how such material aspects of a laboratory influence what happens during an experiment in terms of control, intervention and what is being observed.[178]

[178] Smith and his associates decided to leave GMU in 2007. At Chapman University they built a unique facility reflecting three decades of building and using laboratories. They now have three labs and a fourth is under construction. They have at least six researchers doing experiments, so there is some demand for laboratory space. All three labs were built from scratch under Stephen Rassenti's supervision and input

Experiments such as the one just described stimulate the development of tacit knowledge. Laboratories with their stable layouts and regular use focus reinforce the development of the experimenters' skills and tacit knowledge. Think of the earlier discussion of the symmetry driving force and the deep impression made on Ledyard by Smith's subtle alteration of the experimental design. Unlike an armchair theorist, an experimental economist must think about the flow of information and communication or what kind of information when, where and how to display it. Because one particular theory on some of these issues may not be specific enough, the experimentalist makes decisions which are in no small part influenced by his experience in the laboratory. This experience capitalizes on the interaction between technology, the means of control and the laboratory space, including physical location within the laboratory.

In the remainder of this section I discuss seven points that come out from this interaction: 1) subject pool control, 2) instruction delivery, 3) randomization, 4) uniformity, 5) recording, 6) malleability, and 7) speed.

First, computerization provides easier subject management and a pre-experimental selection of subjects from the subject pool with any choice of experience, background or specific features (gender, participation in previous or similar experiments). Before Plott and his team developed the Subject Pool Program, experimentalists used campus fliers and let students sign up in classes or at student employment offices.[179] They had little control over demographic features or exposure to previous experiments and it was time-consuming to recruit subjects. The software package they devised was the first to solve a number of technological, procedural and managerial problems in recruiting subjects, records and bookkeeping. Better subject pool control meant more reliable experimental data. By 1991, Plott's subject pool contained about 1,000 non-Caltech people. With the spread of emails, online recruiting for experiments has become even easier and a number

from other researchers. The first laboratory seats 24 people with six pairs of computers to allow work in pairs. Normally it is used by 12 subjects, because the designers realized that 12-14 is enough for most experiments. The second laboratory has 14 seats and everyone can see the two screens at the front (see Figure 8). The room is raked so that subjects cannot see the screens of the subjects in front of them. The third laboratory is larger, with 24 seats and everyone can see the large screen at the front. In some of their previous labs the researchers had computers around the perimeter or sometimes on flat floors (ESL). Over time they have learnt that if space and funds allow, it is better to orient everybody in the same direction, preferably with a slope. This helps subjects to read and instructions to be projected, while the experimentalist is more visible. People in the same row do not see their neighbors' screens. Stephen Rassenti Interview.

[179] Subject Instruction Package - contained templates for a number of experiments.

of other subject pool management programs have been developed and become more popular.[180]

Second, computers can be used for giving instructions and checking comprehension, but so can handouts and transparencies. Experimentalists differ on this point. PLATO users used to put all instructions on screen with a comprehension questionnaire at the end. Plott, in contrast, never had much use for overhead projectors, since they cause subjects to look up and down: “Their minds lag, are all over the place. I don’t use them if I don’t have to.”[181] Kagel has a similar concern: “I always use transparencies anyway because I read everything out loud, with them having a copy to go along with. I don’t like them to just read the instructions on their own.”[182]

Experimentalists were not the only ones who had to get used to computers. One of the conclusions of Shubik’s 1969-70 report on his one year NSF grant with Gerrit Wolf, entitled *Experimental Economic and Psychological Modeling in an Automated Laboratory* was that “the next experimentation we would like to do in relation to the grant necessitates the acquisition and development of economically literate and computer comfortable persons.”[183] Computer literacy remained a problem even two decades later, as Cox recalls in speaking of his experience with PLATO in the late 1970s and the 1980s:

> “In those days, since it was long before PCs, subjects didn't know anything about interacting with a computer. Furthermore, it's not even clear that they would necessarily know how to use a keyboard. They didn't necessarily have typing skills. For a typewriter, it would be, yeah. The touch sensitive panels were very supportive of this because you could create box areas where people could touch them. So, you want to bid, you could touch the screen to indicate you want to bid or if it's a double auction, you want to accept an offer or you want to submit an offer. And just putting numbers on the keyboard, that's easy to learn, and then, touch the screen, confirm.”[184]

[180] ORSEE, for instance, was discussed in the previous section. Alvin Roth at the Witness Seminar: “These days we have a computerized subject pool [organized through ORSEE], our subjects come into our lab most often but they get recruited on the web and there’s lots of database management of subjects. You can have an experiment where you say, ‘I want subjects who are all college students and haven’t participated in an experiment before,’ or you can say, ‘I want subjects who haven’t participated in a public goods experiment,’ or you can say, ‘I want to recruit sessions that will be only men and only women.’ ”
[181] Charles Plott Interview.
[182] John Kagel Interview.
[183] Box 47, Martin Shubik Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[184] James Cox Interview.

Third, while the delivery of instructions and their comprehension are crucial, the experimentalists have much better control over the randomization of subjects (in order to achieve independence of the treatment variables) or control over the arrival of external information provided by the experimentalist. Fourth, computerization allows better control over experimental conditions that must be common across all subjects or uniform enforcement of the rules and procedures established in the experiment. For example, Charles Holt recounts problems with the lack of uniform control. In the early 1980s he was still at the University of Minnesota:

> "We did a double oral auction, you write it [contract prices] on the board. And Laura [Cohen] and Anne [Villamil], they were great. We didn't want the students to talk outside of the bids and asks. And they'd just stand up like school teachers and glare at them, and of course, I would, too. But you had to impose a little bit of discipline if you don't have computer terminals separate visually then. And in those days, there weren't such things."[185]

Fifth, besides better and more uniform control of subject interaction, computers provided a more accurate recording of the social interaction and decisions of subjects and subsequent data management and analysis. In hand-run experiments the data of interest are recorded by the experimenter but this is less reliable than computers and the data then need to be transcribed, which opens the door to transcription error.

Sixth, the experimentalist has the ability to change instructions, remove program bugs whenever they occur – fewer subjects show up, unexpected software errors, etc. In other words, computers allow flexible control and intervention.

This flexibility came at a price. In the first two years of having a laboratory, Selten recalls, "we were struggling with the technique." Only in the third year was Selten able to start conducting experiments. The programming of an experiment was very time-consuming – sometimes it took many months. The reasons were the quality of the part time programmers, programming challenges, and communication issues with programmers. Selten eventually hired a full time programmer and later his Ph.D. students programmed as part of their research. A great advance came with two students, Klaus Abbink and Abdolkarim Sadrieh, who developed the software toolkit *RatImage*[186] (Abbink and

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[185] Charles Holt Interview.
[186] Reinhard Selten Interview.

Abdolkarim, 1995). Both ESL and EEPS experienced similar initial problems in mastering new tools, whether hardware or software.

At first, it would seem that the time saved on not running slow hand and paper experiments was used, perhaps in disproportionate amounts, for preparing computerized experiments. Although the starting costs of mastering a new tool were high, the potential benefits which were eventually realized were substantial – better control during the experiment. This better control meant, for instance, that subjects spent less time in the laboratory, i.e. did not get so easily bored or tired. More observations could be collected in the laboratory (treatment repetitions, etc.) Better control strengthened the experimenters' trust in the collected data and made them rigorous. Furthermore, such an investment in programming was based on the commitment to experimental research and expectations of continuous involvement.

Seventh, computer use delivers to subjects and experimentalists a significantly faster and real-time data analysis and feedback. Furthermore, the experimentalists can see preliminary data analyses and graphical depictions in real time on their monitors. It introduced a new type of control - continuous intervention throughout the experiment that responds to the behavior of participants. Smith recalls in this context that at first they thought only that computerization would make their pen and paper experiments less laborious ("no paper shuffling"); that parts of experiments which did not include subjects' decision making could all be done more efficiently; that data would be recorded and stored more accurately and securely.

> "But when we started doing them, we gradually realized that after a while we were thinking about experiments in a different way. Now we could do far more. We could do experiments that would process messages. Our capacity for processing messages was greatly increased. That started to influence our experimental designs. In almost no time, we were thinking about doing experiments in natural gas pipeline networks, electric power. Those were experiments that required a lot of optimalization and computer assistance. It never occurred to us, until we got into it, that now you could do, what is now known as smart computer systems markets where you could apply allocation algorithms to the decision of the subjects."[187]

[187] Vernon Smith Interview. See also SMITH, V. L. 2008b. *Rationality in economics: constructivist and ecological forms,* Cambridge, Cambridge University Press. pp. 309-10.

With their decisions as inputs they could compute optimal allocations and feed them back to the subjects for another round and later even in real time. Later such market experiments were labeled by Smith “smart markets.”[188] Smith’s quotation, however, is a shortcut to a more complex process. It was not simply the computer at the beginning that everything then followed; it was, as we saw in the previous section, the social infrastructure that enabled the computer to become so important.

3.6 Conclusions

Economics laboratories had become the primary locations of experimental economics research by the early 1990s. They were the result of a decade long development from ad-hoc opportune places to dedicated, purpose designed spaces equipped with networked computers. Networked computers, their hardware and software, became the distinctive feature of the economics laboratory and the core instruments of experimental economics research. The advances in computer technology, the transition from mainframe to personal computers, improved networking capabilities both local and worldwide, and the salient presence of decreasing costs contributed to the dramatic growth of the economics laboratories.

As this chapter has extensively demonstrated, there is more to this history than technology. I employed Michel de Certeau’s distinction between the place and the space of economics laboratories. While the former captures the physical laboratory with its features, the latter encompasses the laboratory’s local, technological and social infrastructure. It is precisely the infrastructure, in the broad sense of the word, that turned the laboratory place into a functioning laboratory. These various elements that laboratory experiments presuppose have to be seen as a community endeavor. The laboratory community is not restricted to the local community of researchers engaged in experimental research. Austin Hoggatt’s Berkeley laboratory of the 1960s shows the indispensability of building a local community and not merely focusing on the technology. From the late 1970s a division of labor in local laboratory communities has emerged. Not

[188] The earliest of these “smart market“ experiments were carried out for the Arizona Corporation Commission in 1984-5. But Smith conducted his earliest computerized experiments in 1977; the foundation for smart markets were laid down a few years later RASSENTI, S. J., SMITH, V. L. & BULFIN, R. L. 1982. A Combinatorial Auction Mechanism for Airport Time Slot Allocation. *The Bell Journal of Economics,* 13**,** 402-417.

only the experimentalists, but also the programmers, graduate students, administrative staff, visiting scholars, theorists from the local economics department engaging in collaboration with the local experimentalists.

Laboratories helped with acquiring a style of continual experimentation and became a focal point for each local experimental community. Laboratories became not only the place where rigorous experimental data were collected, but a site that needed to be experienced. Laboratory communities are an integral part of the larger experimental community where its various members pursue experiments on similar topics and allow the experimental engine to run. Experimentalists developed a culture of sharing such resources, which allowed them to lower barriers to computerized experimentation and of entry to their field in general.

The computerization of the laboratory or place was closely connected to the control of experimental research. On the epistemic level, computerized experiments advanced the stabilization of experimental practice. They provided many advantages over pen-and-paper (hand-run or manual) experiments and affected the experimental turn on two levels. On an operational level, they replaced the experimenter's labor by speeding up experiments and improving collection and storage of data. More importantly, on a conceptual level, computerization brought about an unexpected transformation in experimenters' ability to exert control. The confluence of space and computer technology inside the economic laboratory dramatically transformed the way in which experimental economists thought about what could be studied in experiments and the enforcing of experimental control. Experimentalists' control became inextricably linked to special arrangements (separation, communication, flow of information, randomization) of subjects in relation to each other, to computers (screens) and experimentalists inside the laboratory.

The economic laboratory embodies the driving forces of the experimental turn. Its complex technological and social infrastructure is there to make data rigorous, allow theory to be tested and new theory developed on the basis of experimental data – the theory symmetry and virtuous circle driving forces. As a new site of economic research, it brings the economist back to the data collection process and is built on the assumption of data integrity.

3.7 Photos of Laboratories

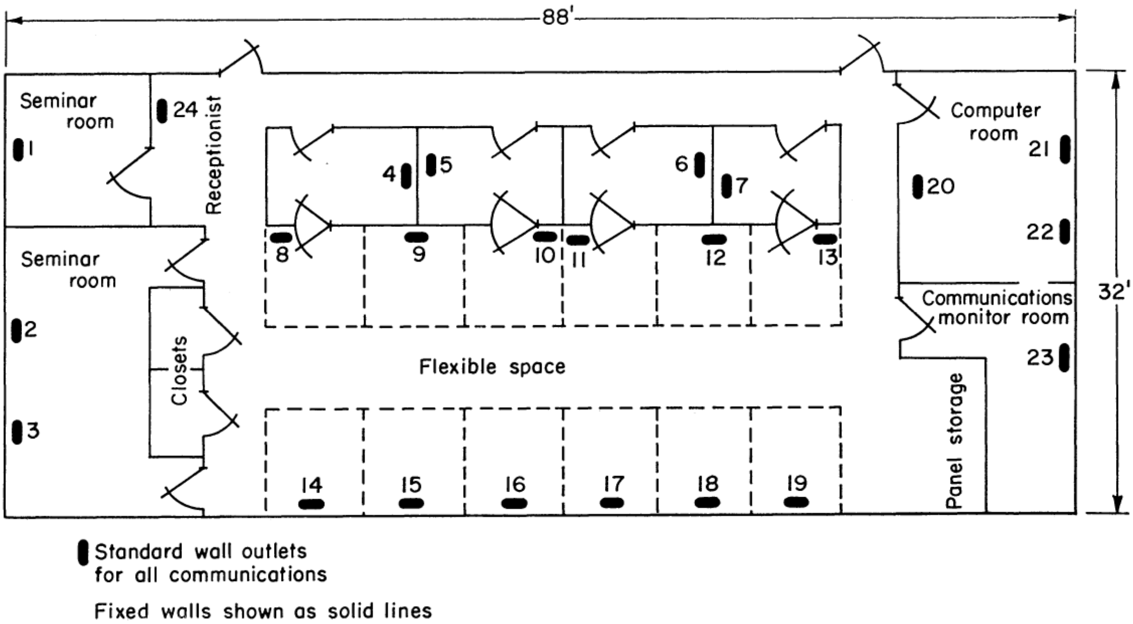
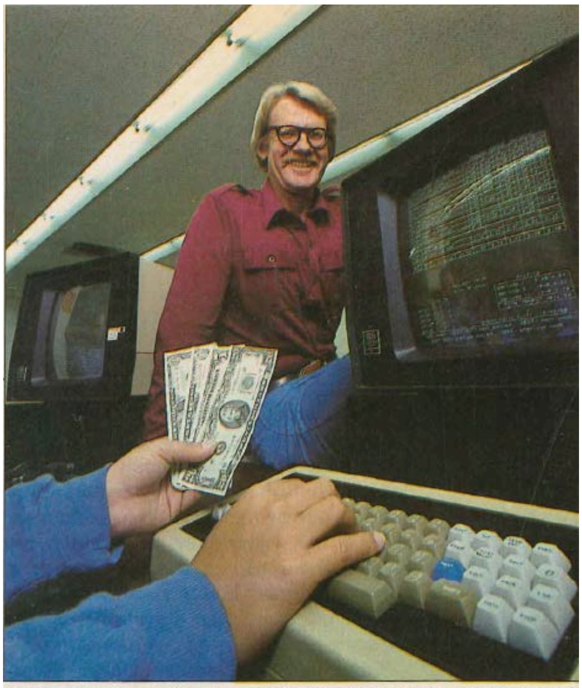


Figure 1 Floor plan of Hoggatt's Management Science laboratory

Source: (Hoggatt et al., 1969, p. 202)



Economist Vernon Smith of the University of Arizona developed some of the laboratory techniques that make it possible for a few subjects to simulate market behavior in the real world. In most of Smith's experiments, subjects buying and selling for real money trade with one another through computer networks.

Figure 2 PLATO terminals in Arizona

Source: Forbes, December 1, 1980

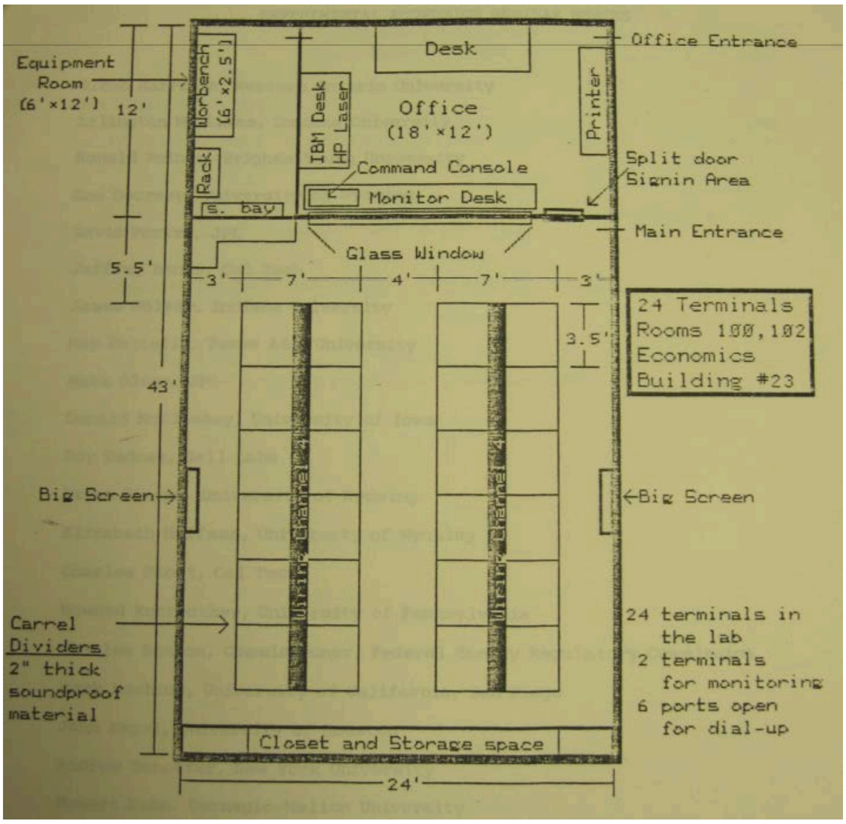


Figure 3 First Economic Science Laboratory at the University of Arizona

Source: The 1985-6 ESL annual report. Box 56 Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

Figure 4 IBM computer model 50 used in EEPS (Caltech)

These machines were the first computers installed in the EEPS in 1985. They were used until 1996, when Pentium personal computers were installed. Source: Personal Archive

Figure 5 Bonn laboratory

Source: BONNECONLAB. 2012. Laboratory for Experimental Economics at the University of Bonn—BonnEconLab [Online]. Available: http://www.bonneconlab.uni-bonn.de/ [Accessed March 17, 2012].

Figure 6 Plott's EEPS laboratory at Caltech

Source: personal archive

Figure 7 The EXperimental Economics CENter (ExCEN) at Georgia State University
Source: personal archive

Figure 8 One of the labs at Chapman University
[Stephen Rassenti in the back] Source: personal archive

Figure 9 SSEL laboratory at Caltech

Source: personal archive

Figure 10 Experimentalists in the EEPS laboratory [March 2012]

Source: personal archive. From left to right – Travis Maron (programmer), a graduate student, Plott, and an undergraduate student.

4 Institutionalization of Experimental Economics

“I hereby declare that we function under the name *Economic Science Association*,“ wrote Vernon Smith in a memo to fourteen members of the Executive Committee on June 27, 1986.[189] This performative statement concluded the first, albeit crucial part of a year-long pursuit of a new professional society that Smith set out to create. Of particular importance in this pursuit was the part played by Smith’s *Prologue*, a dense programmatic statement that argued for the establishment of the *Economic Science Association* (ESA). While it contained the basic organizational issues of a future society, the *Prologue* preeminently tried to make sense and give direction to the transformation of experience of conducting experiments. In other words, the *Prologue* is of central importance for understanding the experimental turn - how the symmetry of theory, the data with their virtuous circle and integrity and the personal responsibility and control of the creation of new data led from personal transformations to the communal level of the experimental turn.

Smith aimed at a society that should be broad-gauged, not limited to experimentalists alone, and should merit the name “science.” Although the society was eventually named the *Economic Science Association*, agreement on its name was preceded by a pronounced dispute that pertained to the nature of economics, the significance of experimentation in particular, and what should be the relationship and boundaries between economists who conduct experiments and the vast majority of other economists. The perceived dominance of Smith and his Arizona group had repercussions that spilled over to the first-price controversy, discussed in Chapter 6. Hence the inception of the ESA provides a critical insight into the economists who adopted the experimental method in their research transformed themselves into a community and developed an identity as experimental economists, and also into their struggles for intellectual and organizational leadership within the community.

The lingering negotiations about the recognition and perception of experimental economics by the rest of the economics profession were above all manifested in the issue of whether to create a dedicated experimental journal, which was marked by the fear of being ostracized from the rest of the profession. By the mid-1990s this persistent matter

[189] Folder “Economic Science Association – Establishment, Information, Sections.” Plott Papers.

became intertwined with the issue of coalescing the worldwide experimental community. The solution to this quandary eventually required a restructuring of the ESA that would explicitly acknowledge and integrate non-American experimentalists within it. In particular, experimentalists in Germany led by Reinhard Selten had already instituted a first experimental research group under the auspices of Heinz Sauermann in the early 1960s; the first society of experimentalists in 1977; the first computerized economics laboratory in Europe in 1984; and the second outside the US. This separate German tradition also included a different perspective on experiments - less tethered to neoclassical economics, more open to bounded rationality, admitting more complex experiments, and stricter on the notion of independent observation. These differences in observational practice were already weakening in the 1990s.

This chapter charts the dramatic period in which ESA was established in 1986, the combined events leading to its internationalization in 1997 and the launch of the association's official journal Experimental Economics in 1998. The chapter is divided into the following sections. The first section examines Smith's Prologue and its importance for the experimental turn. In the second section, I reconstruct the chronology of events leading to the establishment of the Economic Science Association (ESA), in particular the discussions and dissent around its name. Then, in the third section I examine the early reception of the ESA which suggests that the initial support of like-minded economists including many non-experimentalists gradually evolved into an association of experimental economists. The fourth section moves the narrative to the years 1996-98 when ESA underwent a major transformation; it became a truly international society with its own journal Experimental Economics. The final section revisits the issue of the name of the Economics Science Association from the perspective of a quarter century after its inception.

4.1 The Prologue

The Westward Look Resort is set at the base of the Santa Catalina Mountains and provides panoramic views of Tucson. The local warm fall weather is cherished by many ESA members who have been gathering in this resort since the late 1970s for the Experimental Workshops that later changed to annual ESA Fall meetings and after 1998 became the annual North American Regional ESA meetings.[190] The idea of establishing a new scholarly organization was discussed for the first time during the third Tucson workshop in 1984. Already it was envisioned as something that would not focus "simply upon experimental economics narrowly defined but rather upon the process of the development and testing of microeconomic theory from the methodological perspective which has evolved from experimental economics."[191]

The impetus for Smith to carry out these informal plans was furnished by Colin Day, the Editorial Director of Cambridge University Press, who inquired whether there was any interest in a journal for experimental economics. "It seems to us," Smith, Mark Isaac and David Conn wrote in a memo asking the University of Arizona for financial support to cover the administrative costs of setting up an association, "that such a journal would be successful if associated with a professional society as we had been considering. Therefore, we believe that now is the time for the University of Arizona to give birth to an Economic Science Association (ESA) with our 1984 conference participants as the core prospective membership."[192] Most of the actual organizing was done by Mark Isaac while Smith focused on the programmatic aspect.[193]

The idea of creating the ESA was formally presented by Smith at the Fourth Experimental Economics workshop which took place at Westward Look from February 27 to March 1, 1986:

> "Many of us feel that it may be time to create a new professional organization whose primary concern is with the advancement of economics as an observational science; that would emphasize rigorous observation in which the process of data collection, whether in the laboratory or field, is under the control of the scientist."[194]

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[190] James Cox, Charles Holt and Vernon Smith Interviews. In recent years there has been a break in this tradition; the North American regional ESA conferences took place in Santa Cruz (2013) and Fort Lauderdale (2014).

[191] Memo from David Conn, Mark Isaac and Vernon Smith to Richard Imwalle, Director of the Development Office, University of Arizona. April 11, 1985. Folder "Economic Science Association – Establishment, Information, Sections." Plott papers.

[192] Ibid.

[193] Marc Isaac graduated from Caltech in 1981 and belongs to the first generation of economists with training in experimental methods. He was hired by Smith and stayed in Tucson until 2001. He was the Secretary-Treasurer of the ESA from 1986 to 1996, and since then he has been the Treasurer.

[194] Prologue, p.9. All quotations from the Prologue are from a revised version June 1986. Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

During a luncheon address, Smith outlined his idea that economics ought to be based on rigorous observation and pointed out how it translates into the organizational structure of the proposed organization. The written adaptation which was widely circulated was entitled 'Economic Science Association: Prologue', but it also gained the label of manifesto.[195] The Prologue is a dense programmatic document which begins by sketching the development of economics from the mid-20th century to the 1980s. During this period, Smith elaborated, economics by becoming mathematical had asserted the perception that it was "the hardest of the soft sciences" and reaffirmed its practiced and inevitably non-experimental nature, which he called "a congenital defect."[196] Koopmans in his controversial 1947 paper *Measurement without Theory* exposed the deficiencies of the prevalent mode of empirical economics in the pre-war period that led to a new methodology of measurement developed at the Cowles Commission.[197] The new methodology of measurement that Koopmans and Cowles called for led in Smith's view to three developments: "a logical and computational solution to the nonexperimental nature of economic observations," institution-free theory, and a closer relationship between theory and measurement. However, the high hopes and expectations invested in mathematical economics and the developments that it spurred had not been achieved, "not because of any weakness of our [sic] diligence, determination, or imagination, but because they are unfulfillable with the data and methods of the 1950s."[198] Though identifying with the post-war advancements in economics to which he had contributed, Smith sensed a growing discontent with the state of economics among his fellow economists.[199]

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[195] Herbert Simon to Smith. February 27, 1986. Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[196] Prologue, p.1. Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[197]Tjalling C. Koopmans (1947) *Measurement without Theory.* The Review of Economics and Statistics. 29(3), pp. 161-172. For an exposition, see Malcolm Rutherford (2001) *Institutional Economics: Then and Now*. The Journal of Economic Perspectives. 15 (3), pp. 173-194. Historians of economics while acknowledging the importance of institutional economics in the interwar period in the US have argued that this period should be considered a period of pluralism of approach in economics which after WWII gave way to what is now call the dominant economic mainstream. MORGAN, M. S. & RUTHERFORD, M. 1998. *From interwar pluralism to postwar neoclassicism,* Durham, N.C., Duke University Press.

[198] Prologue, p.2. Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[199] For instance a few years earlier in a letter to Gardner Ackley, then the President of AEA and department chair at the University of Michigan, Smith raised his concern: "It was a remarkable session [at the AEA meetings] in terms of some of the verbalized admissions to the effect that the scientific content of economics is poorly served by professional economics. Although I thought it was appropriate at the time, I now think it is unfortunate that the session exchange was not published. I keep encountering private

Throughout the Prologue, Smith invoked Lakatos and his research program framework.[200] Outside of the hard core of mathematical economics, he saw the advancements of institution-specific theory and experimental market economics. While the "unit of study" in postwar mathematical economics was "the individual and the technology that constrained his choice set", the two developments outside the hardcore concentrated on exchange contract or transaction and explicitly emphasized "observation as the centerpiece of economic analysis."[201] According to Smith's interpretation exchange contracts and transactions had already served Adam Smith as the starting point of his analysis. In this tradition, exchange contracts consist not only of individuals; they also include the "information and allocation rule properties of the exchange institution that define the contract"[202] By the 1980s standard (advanced) microeconomics textbooks had abandoned this view and turned to individuals, showing how technology constraints over choice set lead to decisions which disregard the role of institutions.

The emphasis on observation in the institution-specific theory and developments in experimental market economics mark a departure from what became of measurement without theory, namely, theory without measurement, where the most important bridge between theory and observation became model specification.

> "Institution-specific theory generates hypotheses that can be stated directly in terms of the observable actions chosen by agents, as well as in terms of the resulting indirect allocations and cost imputations. This makes possible a much

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expressions in line with the pronouncements of members of the panel. Many seem to feel that there is a collective professional reluctance to go public with any frank discussion of such deep, if disturbing, concerns." Letter from January 11, 1983, Box 9, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[200] Smith has been an avid reader of the philosophy of science literature – Popper, Lakatos, Feyerabend, and more recently Polanyi and Mayo. Smith and Kuhn served together in the mid-1960s at an NSF Panel for graduate student support. The influence of Lakatos is most visible in MCCABE, K. A., RASSENTI, S. J. & SMITH, V. L. 1991. Lakatos and Experimental Economics. *In:* DE MARCHI, N. & BLAUG, M. (eds.) *Appraising Economic Theories: Studies in the Methodology of Research Programs.* Edward Elgar.. But the influence permeates much of Smith's work in the 1980s. For instance, Smith viewed much of economics as a degenerating research program: "In economics the tendency of theory to lag behind observation seems to be endemic, and, as theorists, few of us consider this to be a "terrible state." But, as noted by Lakatos (1978, p. 6), "where theory lags behind the facts, we are dealing with miserable degenerating research programmes." SMITH, V. L. 1989. Theory, Experiment and Economics. *The Journal of Economic Perspectives,* 3**,** 151-169. See also Chapter 6.3, below.

[201] Prologue, p. 2-3. Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[202] Acknowledgment of this has had implications for experimental methodology and research, but also theoretical research of such economists as William Vickrey (1914-1996) on auction theory, Leonid Hurwicz (1917-2008) on mechanism design, Ronald Coase (1910-2013) on externalities, and Oliver Williamson (1932-) on contract theory.

> more rigorous study of individual behavior, and of how institutions perform through their effect on agent responses."[203]

One of the means for such an assessment is provided by controlled experiments.[204] Notwithstanding these peripheral developments, much of the economic profession was "still hard-wired to the traditional way of thinking" leading to regularly occurring claims against experimental research. Smith lists a number of them in the *Prologue*, but two will illustrate his point.

> "We continue to hear the naive proposition that if the logic is correct, and the assumptions well specified, then the conclusions must follow, so that there is nothing in a theory to test. Or that when the observations are consistent with a theory, this is uninteresting because it simply confirms what we already knew."[205]

To counter such claims, Smith urged, "we have to find new and more compelling responses that address the pattern of thinking that leads to" them. Smith did not directly elaborate in the *Prologue* on these answers. Some of them, however, appeared in an article of his that was published in the *Journal of Economic Perspectives* written around the same time as the establishment of the ESA. He argued that:

> "Experimentalists in economics frequently encounter an argument that proceeds roughly as follows: (a) If a theory is well articulated with clearly stated assumptions, and if there are no errors in the logic and the mathematics; then, (b) certain correct conclusions follow from the theory. So (c), what is there in a theory to test? The punch-line (c) often comes out in other forms without the conditionals (a) and (b) being stated. For example, when the data are consistent with the predictions of a theory, it is sometimes said that the results are not interesting because they merely confirm what economists already knew (or

[203] Prologue, p.4, Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[204] "Thus competitive theory was examined experimentally using such field institutions as the continuous double oral auction, posted offer pricing, posted bid pricing, and sealed bid-offer auctions with and without long-term multilateral contracting features. The results suggested that these institutions were neither equivalent nor benign, at least under the conditions studied. Some of these empirical results, such as those based on the double auction have been called "surprising," which indeed they are given the conventional institution-free wisdom." Prologue, p.6, Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[205] Prologue, p.7, Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

teach?), which seems to suggest that "truly" authoritative theory cannot be doubted seriously. When the data are inconsistent with the predictions of theory it is not uncommon to assert that there must be "something" wrong with the experiments. … Similarly, my experience has been that questions about experimental procedure are more likely to be raised when the results appear to disconfirm accepted theory than when they appear to confirm such theory. However, if one wants to gain a greater understanding of economic phenomena, the most productive knowledge-building attitude is to be skeptical of both the theory and the evidence. This is likely to cause you to seek improvements in both the theory and the methods of testing" (Smith, 1989, pp. 167-8).

The reason for a new society is not the agenda of promoting experimental research, or as Smith put it pugnaciously: the "battle is not for experimentalism"; rather, "the battle is for a way of thinking that emphasizes the *integrity and primacy of observation."*[206] Therefore, Smith advocated that the *Economic Science Association* must be a broad-gauged society. It should be based on a new understanding of what economics is or should be about - the intimate relationship with rigorous observation. The remarkable point of this assertion, from a historical point of view, is the lightness with which Smith moves between experiments and (non) experimental observation. There seems to be no opposition between observation and experiment, rather the generic label observation subsumes types of observation that take place in different locations – field or laboratory:

"The laboratory and field data generating methods must be part of the core [sic] of economic science. Since each has its unique strengths and weakness, neither can survive as an island. The goal of each is the same - observational coherence - and this also requires integration between experimental and field observations."[207]

This normative statement with a Lakatosian reference also has implications for theoretical research, because the "theorist is needed both before and after observation to help do the hard work." Both theorists and experimentalists should engage in a perpetual virtuous circle where rigorous observation of whatever provenance as long as it

[206] Prologue, p.8, Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. My emphasis.
[207] Prologue, p. 12, Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

is rigorous and an active pursuit of further observations discipline their research and practice.

Although widely distributed in the years 1986 and 1987, twenty-five years later the Prologue has largely been forgotten by its readers, leaving only faded traces within the institution that it helped to establish.[208] The *Prologue* differs from anything else ever written by any experimental economist. It is not a methodological piece or a summary of recent research, like so much that was written in the 1980s or 1990s. Nor is it an attempt to present the experimental method and the namesake field to a non-experimental audience. It is, rather, a very dense programmatic statement, unparalleled in its scope, of the role of experimentation in economics, the closeness of experiment and observation, the equal standing of theory and rigorous empirical evidence "regardless of its provenance as long as it is rigorous", the ensuing transformation of the discipline by the recovered responsibility of generating one's own data, and what experimentalists and like-minded economists should do to advance economics as an observation science – all the driving forces of the experimental turn.

Choosing the title ESA reflected in Smith's recollection "what we thought we were doing." Some non-experimental economists "did not like it, and I can understand that, because there is a tradition of economists being very pretentious ... I didn't really think it was, and the group of others, at that time, [who thought it was] was small."[209] What were Smith and his colleagues in Arizona doing? They had just established a computerized laboratory and were engaged in an influential research program. More specifically, Smith and his Arizona associates were involved in creating a premier experimental laboratory facility, the Economic Science Laboratory (ESL), which was established in 1985. The resemblance of the names ESA and ESL is not accidental.[210] The introduction of a laboratory profoundly changed what could be done and observed in experiments – it offered a much broader range of institutional settings including some completely new ones. Furthermore,

[208] The *Prologue* was printed in an obscure journal published by the University of British Columbia under a different title: SMITH, V. L. 1988. New directions in economics. *Journal of Business Administration.*
[209] Smith Interview.
[210] Smith, in the postscript to a letter written in 1984 to the Provost of the University of Arizona regarding the establishment of ESL. "I am not really happy about the title, tentatively Economic Science Laboratory, because it is much more than a 'laboratory' in the narrow sense. Program for Experimental and Applied Economic Analysis, or something similar may be better." It was a reluctant start of Smith's long involvement with giving institutions names that included the word ' Science'. The name ESL was eventually adopted; it paved the way for the ESA, and later the *Interdisciplinary Center for Economic Science* at GMU in 2002, and the *Economic Science Institute* at Chapman in 2008. The only exception is IFRRE - International Foundation for Research in Experimental Economics – which Smith founded in 1997.

together with James Cox and James Walker, Smith investigated auctions, first-price single unit auctions in particular. This became a premier experimental economics research program in 1980s which in a series of articles made theoretical and experimental research coalesce in exactly the same fashion as Smith described in his Prologue. The details and relation to the foundation of the ESA are reserved for Chapter 6 of this dissertation.

4.2 Selecting a Name – the Birth of the Economic Science Association

In the course of 1985 the ideas expressed in the *Prologue* germinated. On October 21, 1985, the "Pre-organization Committee on ESA" consisting of Smith (chair), James Cox, David Conn, and Mark Isaac circulated an invitation for the creation of an organization "whose principal initial activity would be to sponsor regular conferences" such as the 1986 Fourth Experimental Workshop and would be organized around several sections.[211]

Responses soon started to arrive indicating two points – general support for an organization, but disagreement about its mission.[212] Both were expressed in the earliest surviving example of the letters that he received, one from James Friedman. In his letter of December 14, 1985, he remarked: "the Economic Science Association seems to me a good idea if it is kept centered about experimentation in economics, broadly conceived." If the ESA is to "include everything that's economics … [that] might be a bad idea. It doesn't look to me like the discipline needs another general purpose association, but it could use a focal point for experimental research, and that focal point should be broad enough to interest people who are not running experiments, but who are doing things, or might [be] doing things, relevant to experimenters."[213]

By early 1986 the *Prologue* had been penned and distributed. Herbert Simon received the ESA manifesto in a letter dated February 21, 1986, and sent a reply six days later. Like Friedman, Simon welcomed the idea of a new association. Bypassing the experimental

[211] Strangely, it was, even at this early date, sent under the letterhead of the Economic Science Association!
[212] The Smith Papers are surprisingly incomplete in this regard. I suspect that most of the correspondence was transferred to Charles Plott, who succeeded Smith as the ESA President. Plott's files have a more complete coverage of correspondence by Smith in which Plott was not involved than the Smith Papers themselves.
[213] Friedman did not receive a copy of the *Prologue*. Later he joined the ESA (Section Game Theory and Applications), but was not an active member. Letter of James Friedman to Smith, December 14, 1985, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

method altogether, he saw the new organization's emphasis on empirical work as just one important part of it.

> "I hope that there would be a strong effort to bring into the association, if one is created, people who are trying to develop a tradition of field work at the micro level (not only survey work, though that's important) but also institutional research at the intensive 'case-study' level. I hope it would include also people in the cognitive science community working on the processes of human decision making at a micro level."[214]

Simon's reply, his last sentence in particular, is very much in line with his long-term interests and interdisciplinary research activities (Simon, 1991, Augier, 2005). His main interest was in substantial revision of the core assumptions of microeconomic theory, something not on Smith's agenda at all.

Smith presented the *Prologue* during lunch on Friday February 28, 1986, and in the evening a membership meeting chaired by James Cox was held to discuss the formation of the ESA. The ensuing debate unraveled what letters from Simon and Friedman already had suggested and the issue about the purpose of the ESA shifted to encompass the name of the association**.** Should the organization be an experimental economics association only or one that followed Smith's broader view outlined in the *Prologue*? Moreover, should it be named ESA or something else? These two questions could not be separated. They encapsulated different normative stances to the questions of what economics should be about, how descriptive the title should be, and how the outside perception of the method and the field should be weighted in these deliberations.

Those who opposed centered their arguments on the perception of the rest of the economics profession of the move. It was argued that it might appear hostile or worsen the position of experimentalists now in the process of integration[215] and would have an impact on economics as a whole.[216] Moreover, the title 'ESA' was more normative than descriptive; they would prefer a title highlighting the experimental aspect that defines the mutual interest of the membership, such as the Experimental Economics Association. For Smith and his Arizona associates, however, in the context of the debates, the title was more descriptive than normative, because it reflected the way they thought and practiced

[214] Letter of Herbert Simon to Smith, February 21, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[215] Alvin Roth Interview & Alvin Roth at the Witness Seminar.
[216] Charles Plott Interview & Charles Plott at the Witness Seminar.

economics rather than suggesting that other experimentalists and economists should follow suit. In addition, they simply ignored outside perceptions of the association's name. The sensitivity to what other colleagues would think of the name can be also looked at through the prism of who attended the meetings – not economists from top traditional departments, but from Arizona, Caltech, Pittsburg, Texas A&M. Yet by 1986 all of the participants had a respectable publication record in leading journals.[217]

Reinhard Selten was invited to join the ESA, but he did not want to be associated with such a name, because it suggested that economics without experiments was not science. In his broad view of science, humanities are included, and experimentation is not a distinguishing mark.[218] Roth did not attend the Tucson meeting,[219] but joined the ESA as he did not object to an institution for economists conducting experiments. Nevertheless, the word 'science' in the proposed title, in his recollection, suggested that experimentalists thought "that some of our colleagues were not scientists, which was not my opinion," and believed this could cut off the integration with the profession."[220] The ESA was in Kagel's view a natural consequence of the growing field and the natural desire to have an association.

When Plott was the president of the *Public Choice Society* (PCS) in the years 1976-78, he used PCS meetings to bring together people doing experiments. This continued for many years and public choice was used as a vehicle for experimentalists. These scholars were from a public choice background and knew each other quite well, since the PCS community was small.[221] For instance, the *Fourth Experimental Economics Workshop* in 1986 took place at the end of February and March and not in the fall when previous workshops had been held, because the PCS was holding its annual meeting in nearby Phoenix. Given that many ESA members were active PCS members, merging the two

[217] Chapter 5 provides more details on the penetration of experimental research in top economics journals.
[218] Selten was invited by Smith to join the game theory section in September 1986. There is no reply in the Smith or Plott papers. According to Selten, science is any systematic and rigorous study and includes humanities or non-experimentally based economics. Reinhard Selten Interview.
[219] Smith wrote a letter to Roth on March 13, 1986 that "the idea of an organization devoted to the advancement of economics as an empirical science – measurement with theory – was adopted unanimously. We are still struggling to settle on a name." It was also suggested that Roth be nominated as the section head of Game Theory, Bargaining and Other Applications, and that he be sent him a copy of the Prologue. Roth accepted the nomination. Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[220] Alvin Roth at the Witness Seminar.
[221] I lack the conference programs to judge the extent of this venue and how it compares to the Tucson meetings.

events seemed a prudent measure.[222] When the ESA was being discussed, one issue was whether or not it should continue to collaborate with the PCS. Plott thought that public choice was a way of getting integrated with the profession, and getting jobs for students, and therefore he thought it would be risky to pull away from a community that was supportive and had connections at some universities. Plott considered the ESA name to be "a little bit presumptuous." According to Plott, "if a discipline has to call itself a science, it's not."[223] Although not enthusiastic, he was not against the creation of the society. But "I thought that we need to go very, very slowly."[224]

However, there was unanimous agreement from the workshop in Tucson about forming a new organization. A proposal for its organizational structure and a ballot with several suggestions and the possibility of suggesting a different name was distributed with a memo that Smith wrote on March 27, 1986.[225] The society would be led by four officers - President, President elect, and Vice President, all three for h a one year term and a treasurer-secretary for an indefinite term. The ten section heads would each have a three year term with initial "staggered terms of one, two and three years." [226] Several possibilities for a name were offered - Economic Science Association (ESA), Economic and Social Science Association (ESSA); Association for the Advancement of Economic and Social Science (AAESS); Association for the Advancement of Economic Science (AAES); Society for the Advancement of Economic and Social Science (SAESS); Society for the Advancement of Economic Science (SAES). Smith sent this memo to the fourteen members of the "Executive Committee of a New Organization:" – Smith (President), Ledyard (Past President), Plott (President Elect), Isaac (Secretary & Treasurer), and the

[222] Several PCS Presidents have an experimental track record – Charles Plott was the PCS president from 1978-1980, Elinor Ostrom (1982-84), John Ledyard (1980-82), Smith (1988-1990) and John Ferejohn (1990-92).
Actually the 1986 PCS meeting was originally scheduled to be in Tucson. However, not enough hotel rooms could be arranged and at the last minute, therefore, it was moved to Phoenix. Mark Isaac Interview.
[223] Charles Plott Interview.
[224] Charles Plott Interview.
[225] Other issues were Hoffman's discussion on a joint meeting with PCS and a proposed conference in Illinois. The list of members of the Executive Committee is not available in the Smith papers. However, one can infer it from the list of nominees for the created positions of Officers and Section heads. See the next footnote.
[226] President Smith, VP Ledyard, President Elect Plott and Secretary Treasurer Isaac. Nominees for the Executive Committee were (with duration of term in brackets) Raymond C. Battalio (1), David A. Conn (1), Don L. Coursey (3), James C. Cox (1), David M. Grether (2), Elizabeth Hoffman (3), Mark Isaac (indefinite), John H. Kagel (1), John O. Ledyard (1), Charles R. Plott (3), Alvin E. Roth (2), Vernon L. Smith (2), Shyam Sunder (3), Arlington W. Williams (2).

proposed section heads Battalio, Conn, Coursey, Cox, Grether, Hoffman, Kagel, Roth, Sunder and Williams.

The news was spreading among economists. On April 7, 1986, Deidre McCloskey, a co-founder of the *Cliometric Society* in 1983 (Williamson et al., 2008, p. xi), who was originally not informed about the intent to create the ESA, wrote a letter to Smith. Submitting expenses for a talk given in Arizona the previous December, she noted with reference to the cliometric revolution: "I heard from various people about your recent conference and the revolution you plan. I favor revolutions: been in couple myself, and liked the experience." Then she continued to express her worry about the rhetoric and "a sort of separatism" that accompanied the proposed society and urged him "to stay in the union."[227] Smith replied a week later on April 15 reiterating the main purpose of the association: "As to the revolution, to me the idea would be to have an association that advances, enhances, promotes… economics as an empirical (field and empirical) science. That should be the substance, but this broad base is supported also by the pure experimentalists (if there are any) because they fear being ghettoized." This suggests that fear of being excluded was used not only as an argument for bundling the forces of the experimentalists, but also as a reason for opposing a broadly conceived association. The issue of the association's name had yet to be settled by the Executive Committee, but Smith shared his concerns with McCloskey: "something like 'Association for the Advancement of Economic Science' sounds less like a group that believes others don't do it [science]. I think the name is important because people think it is."[228]

On April 22, 1986, Charles Plott wrote a short one-page letter to all members of the Executive Committee: "I think some disagreement about the name and purpose of the new organization exists among us." He suggested an alternative title – the "Experimental Political Economy Association," which had not been included in Smith's memo of March 27. "I'm not crazy about the name but it brings into focus what I take to be the issue that needs to be discussed." He thought that including the words 'experimental' and 'political' and removing 'science' was important. "I suspect that an interest in experimental method is what brings the group together. I think it should be a specialized organization. … if we are going to promote and explore something we should be able to agree about the 'thing'

[227] Letter from Deidre McCloskey to Smith, April 7, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[228] Letter from Vernon Smith to McCloskey, April 15, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

which I think is experimental and not necessarily science." He continued: "I fear that 'economic science' in the context of a new organization looks pretentious and perhaps offensive. Are we trying to say that a new organization is needed to promote science because the other associations are not doing that?" He wanted to include the word 'political', because "there is much closely related experimental work in political science."[229]

Smith wrote a new memo on May 9, 1986, in which he summarized the results from the returned ballots and responded to Plott. The proposed organization structure was viewed positively and the replies stressed that it should be kept flexible, in particular by creating and dropping sections. 'ESA' received 6 1/3 votes, 'AAES' 1 1/6;' SAESS' 1/3; 'SAES' 2 2/3; and 'EPEA' 2 ½ votes.[230] On three pages Smith went over Plott's letter in detail. Alluding to the *Prologue*, he repeated that: "some of us feel that creating a narrower organization built around the use of experimental method is too confining. It better expresses what we were yesterday than where many of us are today and the direction we are going, invoked examples of recent research, and shared "direct responsibility for our data" with the empirical economists. As to the issue of the word 'science' in the title and the possible "separatism with main stream economics" similar to that of economic history, Smith rejoined: "We should not and are not, I think, saying that a new organization is needed to promote science because the other associations are not doing that. … Our purpose should rather be to advance economics as an observational science, of value in itself, but increasingly linked with appropriate theory and measurement methods. … this effort should complement and stimulate, not substitute for, other organizations." In conclusion, Smith suggested having a new section on Political Economy, which had hitherto been part of Law and Political Economy (section 8). In addition a new ballot that included Plott's suggestion on the name was distributed.[231]

By June 27, 1986, all the members had replied and 'ESA' tied with 'AAES' (Association for the Advancement of Economic Science). Each got eight votes - each Executive Committee member could select as many options as s/he wanted. The run-off between the two ended in favor of 'ESA' (nine votes to two). If one considers that the committee consisted

[229] Emphasis in original. Letter from Charles Plott to Smith, April 22, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[230] Some of the voters expressed indifference between two or three options, so Smith decided to count those as l/3 and l/2 votes.

[231] Vernon Smith memo, May 9, 1986. Folder "Economic Science Association – Establishment, Information, Sections." Plott Papers

of six University of Arizona members and one former graduate student of Smith (Arlington Williams), three from Caltech and the remaining four from various institutions, the selection of the name 'Economic Science Association' is not at all surprising.

On the same day Roger Noll,[232] then at Stanford, was invited to become the head of the Political Economy section for a two year period. The offer was accepted, for Noll both "personally admire[d]" Smith's work and concurred with his "perspective [on] what economics is all about. … It ought to be an observational science, and neither applied mathematics, nor philosophy." Noll also made a number of detailed comments about the proposed section titles and under the weight of his arguments the eleventh section was adopted and the titles of other sections were changed.[233] Noll suggested attracting such people as econometricians Edward Leamer, Daniel McFadden, and theorists Kenneth Arrow, Leonid Hurwicz and Robert Wilson. He noted and Smith concurred in his reply of July 17 that the section names "reflect the interests of [the] founding group, rather than a coherent way of dividing the discipline." Moreover, they agreed that *experimental methods are not a field, but a method – the opposite being " 'wrong' intellectually*."[234] Yet the growing publication success of experimental economics research fraught with the label of "a ghettoized 'field'." Having a section purely on experimental methods is not completely in alignment with the purpose of the association and experimental methods should count among the empirical methods. Yet given the sizable support for defining the new association in narrow, experimental terms, Smith confided, it had to be recognized in such a way. "I don't see any support, now, to merge it with 'empirical methods'" from Charles Plott in particular. "I believe this [narrow scope] is wrong intellectually, and a potential disaster."[235]

In September 1986, the section heads together with Smith approached potential members of their sections. One of the more revealing exchanges in this respect again includes Deidre McCloskey. She was asked to join the section for History, Methodology

[232] Roger Noll, received his Ph.D. in economics from Harvard, joined Caltech in 1965. Except for two leaves of absence (Council of Economic Advisers 1967-68 and Brookings 1970-1973), he stayed there until 1984 when he joined Stanford's economics department.

[233] Letter of July 10, 1986. Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. The 'Applications and Economic Policy' section name was changed at Noll's suggestion to "Policy Implementation and Evaluation."

[234] Smith letter to Noll July 17, 1986, my emphasis; Noll originally wrote "I wonder if continuing to make experimental economics a field is not only wrong intellectually but prejudices its future role in the discipline." Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[235] Smith letter to Noll July 17, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

and Philosophy. However, she refused to join "an organization entitled the Economic Science Association. … the name of the new Association makes *Fatal Error #6*."[236] Despite agreeing with "more observational seriousness" and being "sick of theory spinning in economics, and the phony empirical work that goes with it," she claimed that such a title is not only impolite and impolitic, but more importantly attaches the association "to the setting star" of scientism. For McCloskey this implied that introspection is not scientific and suggests that the "psychology of c. 1960 serves as a good model for economics." Surprised by McCloskey's unexpected rejection, Smith's rebuttal started with a surprising claim and concluded with a defense of parts of economics. "I personally have no strong convictions concerning the name. The problem is that we have not succeeded in finding an alternative."[237] He rejected McCloskey's proposal for a different name such as The Association for Experimental Economics, for the very reason "that our task should not be limited to experimental data." For Smith the word 'experimental' meant "any fact finding exercise conducted under the direct control and responsibility of the researcher." Moreover, he did not feel haunted by the scientism connotations of the word 'science', because the "meaning of words changes to accommodate new understanding." He objected that the *Prologue* excludes introspection and asserts that McCloskey is wrong about too much theory spinning in economics, for the task is to put "spinning under the constraints of data." The claims about "phony empirical work" Smith considered to be unfounded and disparaging. "What I think is needed in empirical work are extensions that deal with the issue of incoherence across widely different data sources" as was already happening in experimental work (as defined above). "I am doing it myself; i.e. using such introspective data, and comparing it with other observations." Here Smith referred to other kinds of observation such as survey data collected from subjects in experiments and data collected in experiments. Again, this illustrates the ease with which Smith treats various sources of observation and the way in which they can complement each other.

The Economic Science Association was incorporated in October 1986. The month before was spent reaching out to prospective members. Section heads in coordination with Smith sent out dozens of letters to scholars of various backgrounds. According to the

[236] Letter from Deidre McCloskey to Vernon Smith, November 1, 1986. Box 12, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. Emphasis in original.
[237] Letter from Vernon Smith to McCloskey, November 12, 1986 Box 12, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

available records, only Deidre McCloskey, Paul Joskow,[238] and Reinhard Selten explicitly declined. Roger Meyerson and Leo Hurwicz were contacted, but no evidence is available whether they rejected the invitation or simply did not respond. Robert Wilson, the auction theorist, and Kenneth Arrow, whom Smith knew from his year at Stanford's Center for Advanced Study in the Behavioral Sciences in 1961, were supposed to be contacted, but copies of invitation letters were not preserved.[239] Reinhard Selten and Peter Bohm were the only European based experimentalists who were invited to join a section. Moreover, those who were approached were either well known to Smith or at least to the section heads, reflecting realistic expectations about their commitment to ESA. The final bylaws state the purposes of the Association in two points:

> "I. To advance, enhance, or further economics as an observational science through use of laboratory and field methods of observation and data collection under the control and responsibility of the research investigator, and the development of economic theory and statistical or econometric methods based on such direct observations and data. The Association seeks to foster replicable, clearly documented, empirical work in all subdisciplines of economics, and recognition of the important tasks of data creation, data quality evaluation and empirical description, as well as theory development and testing.
>
> II. To sponsor annual meetings, small workshop conferences, joint meetings with other societies, and to engage in such other activities that will promote the objectives stated in I."[240]

[238] Paul Joskow met Smith for the first time during his graduate studies at Yale in the early 1970s. They later interacted through paper submissions when Joskow served in various editorial positions at the Bell Journal of Economics (1976-1983). Finally they both served at Market Efficiency Working Group of Leading Edges in Social and Behavioral Science volume discussed in Section 5.3. Joskow wrote: "I have read through the Prologue and agree with much of what you have to say. However, it is not clear to me that another organization (or perhaps eventually yet another journal?) is the best response. As a general matter, I think that there are too many organizations and too many journals in the economics profession. People end up talking too much to "believers" in what they are doing and not talking enough to "outsiders." Letter from Paul Joskow to Vernon Smith, September 23, 1986. Box 12, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[239] Surprisingly, people from the *Public Choice Society* such as Gordon Tullock and James Buchanan were not members, either because of the close ties of both associations or for strategic reasons (not wanting to send the wrong signal to PCS members). In addition, one would have expected Hugo Sonnenschein (Smith's student at Purdue), other game theorists (e.g. Harsanyi), William Vickrey (auction theory) and perhaps Ronald Coase to join.

[240] Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

To facilitate this multi-faceted enterprise Smith suggested that the ESA be organized first in ten sections, but in discussions after the March workshop eleven sections were agreed upon, "each with essential connections to the observational foundations of economics:" 1) Computer Science and Economics, 2) Econometrics, 3) Economic Theory, 4) Experimental and Field Empirical Methods, 5) Game Theory and Applications, 6) History, Methodology and Philosophy, 7) Law and Economics, 8) Organization, Accounting and Finance, 9) Policy Implementation and Evaluation, 10) Political Economy, 11) Psychology and Economics. The *Prologue* describes these sections in detail, each with an agenda for ongoing research. Let me however move to the more important element in the history of the ESA.

4.3 Early Reception of ESA

The first annual ESA meeting took place in March 1987 jointly with the *Public Choice Society*. The general membership meeting had 32 attendees. A new President Elect, Raymond Battalio, and four new section heads were chosen: Daniel Kahneman for Economics and Psychology;[241] Douglas North for History Methodology and Philosophy;[242] John Kagel for Experimental and Field Methods; and John Ledyard for Economic Theory. The ESA Bylaws did not specify membership requirements and, in fact, there are no membership records for the period 1987 -1997, when membership was formally introduced. Membership at first was informal; a person was considered to be a member if she had attended several past conferences. There is a membership list of 93 names from April 1, 1987 only. Therefore it provides a unique vantage point into ESA's reception. From today's perspective the list is impressive. Apart from the new section heads listed above, it included Orley Ashenfelter (then editor of AER, 1985-1997), Joseph Stiglitz (2001 Nobel Laureate), Mark Machina (decision theorist), Martin Shubik (game theorist, experimental games pioneer), Robert Basmann (econometrician, advisor of Kagel and Battalio), Robert Clower (microfoundation theorist and AER editor 1981-1985), Lance Davis (cliometrics), Oliver Williamson (2009 Nobel Laureate), Gary Becker (1992 Nobel

[241] Co-recipient of the Nobel Memorial Prize in Economic Sciences with Vernon Smith in 2002.

[242] Recipient of the Nobel Memorial Prize in Economic Sciences in 1994 "for having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change." Smith collaborated with North through the Center for Political Economy that North created at Washington University in Saint Louis in 1983.

Laureate), Hal Varian (microeconomist), Richard McKelvey and William Riker (both political scientists and Riker founder of the Rochester school of political science that introduced game theory and mathematics to political science), Herbert Simon (1986 Nobel laureate), Paul Slovic (economic psychologist), Richard Thaler (behavioral economist), Clive Granger (2003 Nobel laureate), James Heckman (2000 Nobel laureate), Edward Leamer (econometrician), Mancur Olson (economist), and Russel Hardin (political scientist).

It is not clear, in fact, how many of these prominent members who were not pursuing experimental research ever attended any of the ESA's meetings, or were in any way involved with it.[243] Enlisting support from them was probably an expression of their affinity with the declared goals of ESA summarized in the *Prologue*. Moreover, the list reflects the acceptance of experimental economics and Smith's high standing and good connections within the economics discipline and to a lesser extent also the ability of other members of the Executive Committee to attract such a group of economists.[244]

While affinity does not imply active involvement with the ESA, the ability to attract at least in the early stages a respected and influential network of people beyond the strictly defined group of experimentalists, highlights a unique aspect of the experimental community. Many decisions were conscious and strategically conceived by its leaders.[245] As recalled by Andrew Schotter, ESA's 9th president 2001-2003:

> "Labor economics moves on, right? There's no central organization that's trying to determine should we go and do structural labor or natural experiments and having a debate about that. But experimental economics I found interesting because it was a field that was actually engineering its own path and its own success. There were conscious decisions being made in rooms, you know, not back rooms, but rooms with experimentalists attending ESA conferences. And I think that that's somewhat rare."[246]

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[243] A notable exception is Hal Varian who served on the Executive Board in the 1990s. Kahneman and Thaler attended at least one of the ESA conferences in Tucson.

[244] James Friedman recalled in our interview that Smith worked very hard at the beginning to get people such as Roger Noll involved. Noll had done some experiments but was primarily known as a theorist, not an experimentalist. A number of people like him, including Friedman, participated on panels, but were not actively involved. James Friedman Interview.
The section on Game theory and Applications had the fewest members.

[245] An issue highlighted in interviews with both Andrew Schotter and Glenn Harrison.

[246] Andrew Schotter Interview.

Game theory in comparison did not become so dominant through conscious design, Schotter continued: “there was no game theory society until years and years later, and there was no meeting where they said, ‘Can’t we get acceptance this way, should we start a game theory journal?’”[247] The issue of a specialized journal is a major theme of the second half of this chapter.

The list is interesting for one more reason. It contains the names of Richard Thaler, Paul Slovic and Daniel Kahneman. However, it does not mention other economists (Loewenstein, Prelec, Camerer) and social psychologists (Lichtenstein, Tversky) who are *nowadays* typically associated with behavioral economics and experimental psychology (related to decision making). Only Camerer has been involved with the ESA and served as its president (2001-2003).[248]

4.4 ESA Structural Changes

When James Cox, ESA’s eighth President between 1997-1999, sat down to write the introduction for the first issue of ESA’s official journal *Experimental Economics* that was scheduled to appear in June 1998, he was closing more than a decade’s contention over establishing a specialized journal outlet for experimental research and also looking back at the frantic year of 1996, when the decision to do so was made. Since the subscription to the journal came with ESA membership, which for the first time was tied to an annual fee, his concise introduction which carries a conciliatory and upbeat tone reached the whole community. It deserves to be quoted in full:

> “From its founding in October 1986, the Economic Science Association (ESA) has sought to advance economics as an observational science. ESA has always viewed itself as an international organization and has sought to be inclusive of theoretical and empirical research based on several disciplines. In practice, the distinguishing feature of most papers presented at ESA meetings to date has been their use of

[247] Andrew Schotter Interview.
[248] The ESA has had the following presidents: *Vernon Smith* (1986-1987), *Charles Plott* (1987-1988), Raymond Battalio (1988-1989), *Elizabeth Hoffman* (1989-1991), *Charles Holt* (1991-1993), Robert Forsythe (1993-1995), *Thomas Palfrey* (1995-1997), *James Cox* (1997-1999) *Andrew Schotter* (1999-2001), *Colin Camerer* (2001-2003), *Ernst Fehr* (2003-2005), *John Kagel* (2005-2007), *James Andreoni* (2007-2009), *Tim Cason* (2009-2011), *Alvin Roth* (2011-2013), and Jacob Goeree (2013-2015). I have interviewed all those whose names are in italics.

> controlled laboratory experiments and the dominant theoretical foundation of the research papers has been economics. The simultaneous launching of the journal, Experimental Economics, and amendment of the ESA bylaws to create the office of European Secretary and provide for European meetings, establish the organizational structure for ESA to more effectively promote its central objective. Thus we should now be better able to build upon the strengths of both the American and European traditions in experimental methods in economics, psychology, and behavioral science in furthering the development of economics as an observational science. Experimental Economics will provide a central forum for communication of research by scholars who share this objective" (Cox, 1998, p. 7).

The message is very dense and for ESA members, who were privy to the deliberations behind the most important change of ESA's bylaws, it abounds with allusions that I try to disentangle in the following two subsections. The main protagonists of these changes on the part of the ESA are its two consecutive presidents Thomas Palfrey (1995-1997) [249] and James Cox (1997-1999); and on the other the trio Charles Holt, Arthur Schram and Zac Rolnik representing the impetus behind a journal and internationalization.

As mentioned in Chapter 2, James Cox like Vernon Smith graduated with a Ph.D. in economics from Harvard. His first position was at University of Massachusetts at Amherst, where the two were colleagues from 1970 to 1972. Remaining at Amherst even after the infamous implosion of its economics department (Mata, 2009), Cox joined Smith once more at the University of Arizona in 1977. They became close associates working on single and multiple unit auctions, a premier research program in 1980s which in a series of articles coalesced theoretical and experimental research in exactly the fashion that Smith later described in his *Prologue*.

Therefore, given Cox's Arizona imprint, it is not surprising that he wove the formation of *Experimental Economics* and formal internationalization into the tenet infused into the ESA that economics was an observational science. Nonetheless, he conceded the practical difficulties of maintaining a broad platform for researchers from various disciplines interested in the experimental investigation of economic problems, who were supposed

[249] Thomas Palfrey graduated from Caltech's then new Social Science Ph.D. program in 1981 and attended classes taught by Charles Plott on experimental research methods, together with Marc Isaac and Elizabeth Hoffman. In 1986 he joined Caltech as a tenured professor and in 1998 opened a second experimental laboratory at Caltech, the Social Science Experimental Laboratory (SSEL).

to be united by ESA's original concept. Instead, economics and economic experiments gained primacy.

The issue of a dedicated journal for experimental research is older than the desire to make the ESA an international organization. By the mid-1990s, the community of experimentalists grew both in the US and also in Europe, and experimenters continued to be successful in publishing their papers. Their earlier fear of ghettoization waned. The issues of the journal and the need for the formal integration of non-US experimenters became intertwined. Below, I trace how both matters were resolved.

4.4.1 Against a Dedicated Journal

From a historical point of view the most striking omission from Cox's inaugural statement is that he does not acknowledge the longstanding resistance of experimentalists to the prospect of a specialized journal. But precisely this resistance explains why he avoided even alluding to it. This issue can be traced back to the late 1970s, when experimental research started to gain momentum and a community began to coalesce around Caltech and Arizona, and later in other places. Instead of delineating its vagaries from the very beginning, let me advance to two instances – in periods shortly before and shortly after ESA was founded.

In the first section I mentioned a letter from an editor of the Cambridge University Press, Colin Day, in early 1985, inquiring about the demand for an experimental journal that precipitated Smith's decision, jointly with Cox and Conn, to pursue the creation of the ESA. They believed that the journal could be a success "if associated with a professional society as we had been considering." Smith's reply to the editor has not been preserved in Smith's papers, but it described the "relationship between ESA and our proposed journal, *Economic Science*." [250] A year later when he tried to persuade Paul Joskow at MIT to join ESA's Section 9 (Policy Implementation and Evaluation), Smith argued against a journal:

> "I, and all the experimental people I know, are opposed to an experimental economics journal, which we consider to be neither necessary nor desirable. It could easily have a ghettoizing effect which I think we should avoid. Incidentally,

[250] My emphasis. Memo from David Conn, Mark Isaac and Vernon Smith to Richard Imwalle, Director, Development Office, University of Arizona. April 11, 1985. Folder "Economic Science Association – Establishment, Information, Sections." Plott Papers.

experimentalists have been under some pressure from journal editors to start an experimental journal, which they see as a means of reducing their manuscript crunch. An important concern of ESA founders."[251]

A few years later on October 11, 1990, Zachary ("Zac") Rolnik, a senior editor at Kluwer Academic Publishers, addressed a letter to Charles Plott thanking him for their meeting in LA in June that year: "I continue to follow the elusive trail of a journal in experimental economics. By now I have spoken with many of the important people in the field and I get the sense that there may be a movement towards such a journal." Acknowledging that he is an outsider to the community of experimentalists, he voiced his impression of their shifting preference for a journal: "The Economic Science Association is interested in starting a journal but have no confirmed plans."[252]

As it turned out, Rolnik was wrong at the time and the journal had to wait until 1996. Yet he was right in asserting that "economists outside the experimentalists see a niche and interest in such a journal while experimentalists range from the excited (Reinhard Selten) to the negative (Alvin Roth)."[253] Indeed, for Selten launching a specialized journal was an easy decision. And if he had the support of other experimentalists, he would take the initiative: "I did not want to do it without them. I could have done it before that [*Experimental Economics* in 1998]. I could have done it in Germany, but I really did not want to do it without them."[254] At some point either in 1986 or in the first half of the 1990s,[255] he even "got calls from five publishing houses who all wanted to establish a journal on experimental economics at the same time. I told all of them that there are five publishing houses trying to do this, and then nobody did it because it would have been a catastrophe if five such things would happen at the same time."[256]

For Selten, the reason for the lack of support was the concern that "in America there was a strong feeling that these papers should get into the ordinary journals."[257] Experimentalists across the pond objected that a specialized journal would give editors of

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[251] Letter from Vernon Smith to Paul, L. Joskow. September 29, 1986. Folder "Economic Science Association – Establishment, Information, Sections." Plott Papers.

[252] Letter of Zac Rolnik to Charles Plott on October 11, 1990. Plott Papers. NB: Rolnik refers to experimental economics as a field.

[253] Letter of Zac Rolnik to Charles Plott on October 11, 1990. Plott Papers.

[254] Reinhard Selten Interview.

[255] This information comes from the Reinhard Selten Interview and Witness Seminar respectively.

[256] Reinhard Selten at the Witness Seminar.

[257] Ibid. This may be explained by Selten's track record of submitting papers to non-refereeing journals. Frey, Bruno (2003) *Publishing as prostitution? – Choosing between one's own ideas and academic success*. Public Choice 116, pp. 205–223.

top journals the excuse for rejecting papers as being too specific,[258] and thus effectively quench the conversation with editors and non-experimental referees, that they were seeking. The specter of ghettoization that was haunting the nascent community of experimentalists was the main argument against establishing a journal. Plott put it aptly: “the problem I felt was that the major journals, the audience of economics need to be exposed and might not be exposed if it [experimental research] was in its own little specialized journal.”[259] The next chapter is devoted to the issue of getting experimental research published in (leading) economic journals.

Apart from the generally shared fear of ghettoization, there had been a bad experience with other specialized journals in emerging fields. For the Arizona and Caltech people the premature establishment of *Public Choice*[260] and [Review of] *Economic Design*[261] were, according to Plott, instances that marred the attempts “to get those kinds of messages out to the general population.”[262] For Al Roth, his strong opposition to a journal was partially formed by the parallel experience of game theory.[263] The *International Journal of Game Theory* was, according to him, formed too early.[264] It was not that this journal would be receiving too many papers, quite the opposite. Game theory papers could be published in Econometrica and hence IJGT did not have enough good papers. Their quality varied from “wonderful papers” to “papers with false theorems."[265] The incentives for economists were to send a paper first to Econometrica or another top established journal, where they usually succeeded. “And as game theory expanded so that there started to be lots of excellent papers that couldn’t be published in the top economics journals, then the Journal of Games and Economic Behavior was formed, and I think it

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[258] Essentially raised by everyone in my interviews.
[259] Plott at the Witness Seminar.
[260] James Cox Interview, Plott at the Witness Seminar. Public Choice appeared first in December 1966.
[261] Charles Plott at the Witness Seminar .
The first issue of the Review of Economics Design appeared in December 1994, the second volume in December 1996. It began quarterly publication only in 1998.
[262] Charles Plott at the Witness Seminar.
[263] When Roth was polled about the issue of an experimental journal in the early 1990s, he was strictly opposed. He even insisted that he would not send papers, nor referee any articles. Roth Interview.
[264] The *International Journal of Game Theory* was founded by Oskar Morgenstern in 1971 (volume 1 appeared in 1971 but was actually completed in 1972) The first Managing Editor was Gerhard Schwodiauer who served until 1980 and was followed by *Anatol Rapoport* until 1984. The Editors were successively William Lucas, Cornell University volumes 9–12 (1980–1983); Reinhard Selten, University of Bielefeld, volumes 13–17 (1984–1988); Joachim Rosenmuller, IMW, Bielefeld, volumes 18–23 (1989–1994); Dov Samet, Tel Aviv University, volumes 24–29 (1995–2000); For details see: 2002. Preface. International Journal of Game Theory, 1995-2001. *International Journal of Game Theory,* 31**,** 151–153.
There was a section on Games and Experiments directed by Selten from 1992 to 1995 and then by Ron Harstad until 2000.
[265] Interview with Roth, Roth at the Witness Seminar.

actually leap-frogged over the *International Journal of Game Theory* [IJGT] because of the bad history that it was still suffering."[266] Selten, an editor of IJGT in the 1990s, does not share Roth's perception. The early years of IJGT in his view, reflected more of the editor's personality than the low quality of papers received.[267]

Rolnik was also scheduled to speak with Elizabeth Hoffman, the President Elect of ESA, in Tucson after the annual ESA meeting in October 1990. He had clear requirements for the new journal to fulfill: "it is important to have the journal well-rounded, and *international*."[268] By 'well rounded' he meant that it should cover not only applications of experiments in economics, but also finance, accounting, psychology, political science, and decision theory. And by 'international' he had the European audience in mind., in particular in Germany (Selten, Güth and others) and the UK (Hey, Loomes, Sugden).

The issue of a specialized journal continued to resurface regularly in the early 1990s reflecting sustained, if not increasing, willingness from publishers to consider it. In January 1991 Elizabeth Hoffman formed an Experimental Journal Task-Force to investigate this issue.[269] Under Charles Plott's chairmanship a report was prepared that concluded "there does not exist a sufficient backlog of unpublished manuscripts to justify the publication of a new journal." A list of requirements for a future arrangement was prepared and the issue was scheduled to be discussed again. Moreover, another counterargument was used: "For the moment experimentalists seem to enjoy a healthy relationship with the journals."[270]

Despite the persistent quarrels with editors and referees, in the early 1990s experimental research papers from a wider group of practitioners started to appear in top journals at an accelerated rate, which in the view of the experimentalists signaled a growing acceptance of their work.[271] The quip at the time was that AER stands for Alvin Elliot Roth.[272] By the mid-1990s, Roth had concluded that the battle for journals was over,

[266] Roth at the Witness Seminar.
[267] Selten at the Witness Seminar.
[268] Letter of Zac Rolnik to Charles Plott, October 11, 1990, Plott Papers. My emphasis.
[269] The members of the task force were: Raymond Battalio, James Cox, Robert Forsythe, Roy Gardner, Ron Harstad, Charles Holt, John Kagel, John Ledyard, Charles Plott (Chairman), and Vernon Smith.
[270] Report of the Experimental Journal Task Force, Fall 1991. Plott Papers.
[271] Taking AER, JPE, QJE and Econometrica – 1940s 2x, 1950s – 10x, 1960s 13x, 1970s 19x, 1980-84 28x, 1985-89 42x, 1990-95 Source Y2K bibliography compiled by Charles Holt, calculations made by the author.
[272] In fact in this period he had one paper in each of the years 1988, 1989, 1992 and 1994, and three papers in 1993. In 2005 he published five, albeit in the *Papers and Proceedings*.

unlike the battle over (top) departments and the emergence of a journal was only a matter of time. [273]

During Charles Holt's presidency (1993-95), two proposals to start a journal were circulated. One was developed by Kluwer and the other was informal. In a letter dating from November 16, 1993, Rolnik approached Tom Palfrey, the President Elect of the ESA, and advocated Kluwer as "an ideal candidate," who had "started a number of excellent specialized journals in economics" and provided the sample prospectus and survey questions used for the launch of the *Review of Accounting Studies.*[274] The ESA President Robert Forsythe introduced the proposal to the Executive Committee in the Fall 1993 meeting and a decision was made to establish a committee headed by Holt "to draft what would be the ideal journal proposal, for subsequent discussions with ESA & with the publisher."[275]

In mid-1994 Kluwer had entertained another proposal for an experimental journal. Rolnik in his proactive drive already wanted "to begin by setting up a slate of editors and getting a home." He consulted Martin Weber in Mannheim, Germany, who was very positive about the idea and "Kevin McCabe and John Dickhaut at the University of Arizona suggested a name that would be broader than experimental economics and was echoing Smith's suggestion in 1985: *Economic Science: Theory, Experiments, and Methods*.

A flurry of emails was exchanged that bounced around ideas on the need and usefulness of surveying members (using print/electronic formats) and suggesting exploration with other publishers before committing to Kluwer. They mentioned worry about a possible lack of submissions and exchanged details of existing electronic journals. Both Holt and Palfrey were leaning "more towards an electronic journal, which could hold data, instruction, procedural details in appendices, with a short-paper format for the results,"[276] and having an ESA newsletter.[277] The benefits of a journal were weighed

[273] Alvin Roth Interview.
[274] Letter of Zac Rolnik to Tom Palfrey, November 16, 1993. Palfrey Papers
[275] Notes of Secretary-Treasurer Mark Isaac in an email to President Tom Palfrey March 20, 1996. Palfrey Papers.
[276] Email by Tom Palfrey, May 10, 1994. Palfrey Papers.
[277] At the end of 1996, once Palfrey had taken up the presidency of the ESA, Andreas Ortmann came up with the idea of a digest intended had the purpose "to enhance communication among and dissemination of information to members of the Economic Science Association and, more generally, those interested in experimental economics. The digest has been approved by Tom Palfrey who, as a matter of fact, suggested its name." Palfrey Papers. It eventually evolved into a listserv at the University of Arizona and then into two Google Groups groups - ESA Experimental Methods Discussion (esa-discuss@googlegroups.com) and ESA

against a working paper series - should it be open to non-member submissions? should they be refereed? how should they be distributed? The emails reveal how novel the options of the World Wide Web were at the time.

Palfrey contributed to the discussion only once, but with a detailed summary making two interesting points. First, the issue of starting a journal had become imminent, despite the repeated arguments against: "Survey of ESA members? I am unconvinced that this will yield useful information to us. The point is that sooner or later we will have to make a decision about the journal. Ideas have been informally solicited from the membership for several years now, and all that happens is that we are cycling through all the old arguments again and again."[278] Second, who would do the editorial work and who should nominate this "dictator"?[279] Palfrey suggested:

> "My instincts are that there needs to be one accountable person who can spearhead the operation (the editor/dictator). That person can then pick a couple of editorial board members, if he/she wishes, to help with screening if necessary, and hire at least one office staff for archiving."[280]

With the exception of archiving, this was what eventually happened. In Fall 1994 at the ESA meeting, President Holt reported on the journal to the Executive Committee and then to the General Meeting, listing the options that were discussed. The decision was "to gather together a proposal to put before the membership."[281]

4.4.2 Internationalization & the ESA journal – two sides of the same coin

In September 1993 and 1995 Charles Holt attended the *Amsterdam Workshops on Experimental Economics* that we encountered in Chapter 3.[282] At the latter, he agreed with Arthur Schram, a local professor and co-founder of the CREED experimental group, that they should "go ahead and start a journal. Just do it."[283] The suggestion must have come from Holt, at the time already a very well established experimentalist, who had

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Announcements (esa-announce@googlegroups.com) which started on September 16 and November 19, 2006 respectively. They currently have 1927 and 1839 subscribers respectively.

278 Palfrey's memo, undated. Palfrey Papers.

279 Palfrey's memo, undated. Palfrey Papers.

280 Palfrey's memo, undated. Palfrey Papers.

281 However, this did not happen. Notes of Secretary-Treasurer Mark Isaac in an email to President Tom Palfrey March 20, 1996.

282 Van Winden Papers.

283 Witness Seminar.

favored the journal during his ESA presidency. Holt thought that with the rising numbers of European experimenters, it would be good to have an American and a European co-authoring the new proposal. They approached Kluwer, in the person of Zac Rolnik, who was caught "out of the blue this time."[284] Unsurprisingly, he was instantly on board. Their strategy was to build first a group of influential and respected advisors. Holt approached Vernon Smith and Schram, while Rolnik approached Reinhard Selten, then a very recent Nobel laureate. Selten was obviously sympathetic, but it is not clear why Smith changed his view and gave his support.[285] Then Al Roth followed, persuaded by the presence of Selten and Smith. Plott, the envisioned third advisor, proved to be the most difficult to sway, because he wanted a certain number of "pages reserved for color illustrations." Plott thought it would be "innovative and would make a splash" and "many subtle patterns in the data become more obvious when presented graphically with color enhancement,"[286] but for cost reasons Kluwer did not agree.[287] Holt pressured Plott, telling him that he must decide, as Holt and Schram wanted to write to the ESA officials, and eventually he agreed.[288] Enlisting Plott, Roth, Selten, and Smith ensured backing from the most respected experimentalists and was a strong signal to the community. Moreover, having both Schram and Selten on the Advisory Board, Holt argued, "will ensure a close tie to experimentalists in Europe."[289]

On March 5, 1996, Charles Holt dispatched a personal letter to a selected group of ESA members giving them the details of the joint proposal with Arthur Schram in collaboration with Kluwer: "Why a journal? The prospectus makes a case for a journal *that will serve the experimental economics community*." He praised the results of bargaining with the publisher, for low subscription rates, a "significant electronic component for the dissemination of data, instructions, software, and electronically enhanced displays (with dynamics, color, etc.),"[290] and limited success to color printing.

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[284] Charles Holt Interview.
[285] March 22, 1996, Smith confirmed to Palfrey his support of the proposal. Palfrey Papers
[286] Charles Plott Interview.
[287] Plott's vision of colored graphs came to reality in a massive tome co-edited with Vernon Smith, The Handbook of Experimental Results. Elsevier. 2008.
[288] This seems to be a typical example of the sort of information cascade that Holt was studying (with Lisa Anderson, whose dissertation explores them) at the time. ANDERSON, L. R. & HOLT, C. A. 1997. Information Cascades in the Laboratory. *The American Economic Review,* 87**,** 847-862.
[289] Charles Holt Interview.
[290] Emphasis in original. Letter from Charles Holt to a selected group of ESA members, March 5, 1996. Thomas Palfrey Papers.

Tom Palfrey was spending the academic year 1995/96 in Paris on sabbatical and learned about the proposal only on March 18. However, on March 14, Rolnik had already sent out a questionnaire for the proposed journal.[291] Finally after Holt returned from a trip in Italy, Holt and Palfrey managed to talk together over the phone on March 20. They agreed that the nature of the partnership between the journal and ESA needed to be formalized.

In addition, the issue of ESA's not being international enough came up in Holt and Palfrey's conversation.[292] No scholar outside North America had attended the Fourth Experimental Economics Workshop in Tucson in 1986.[293] And only two Europeans, Peter Bohm and Reinhard Selten, had been invited to join one of ESA's sections. The 1990 annual meeting had no more than 47 registered participants, only one Martin Weber from Europe. The 1995 annual meeting already had 141 registered participants, three parallel sessions and each paper had a commentator, a practice now abolished, but still only 16 scholars from Europe were present. Extrapolating from the attendance numbers one could assume that there was hardly any community of experimentalists in Europe. But this is hardly the case. First, the weak attendance of Europeans at the ESA meetings is in part explained by the expensive transatlantic flight tickets and limited travel budgets at the time.[294] Second, although the number of experimenters in Europe was smaller, the first research community of experimental economists in the world had been formed around Professor Heinz Sauermann (1905-1981)[295] in Frankfurt in the 1960s, more than a decade earlier than in the US. For more details on the German community, see Chapter 2. In the early 1990s the Amsterdam *Workshops on Experimental Economics* had an important impact on both the European and American experimental economics communities due to their unparalleled international scope compared to the ESA meetings. By 1996 the number of experimentalists in Europe had risen and Selten's Nobel Prize for his game theory research was also a boost for experimentalists.

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[291] Questions included: "Do you think the proposed journal would fill a need? Do you think that enough material of excellent quality could be obtained to fill such a quarterly journal? Would you be willing to submit papers to this journal? Do you see the Web site adding value to this journal? Would you be willing to act as a referee? Which are your current journal(s) of first choice when publishing papers within the Aims and Scope of this journal? Who would you expect to see on the editorial board?" Schram Papers
Letter from Rolnik to Palfrey March 7, 1997. Palfrey Papers.

[292] Palfrey's notes from the phone conversation with Holt on March 20, 1996, made March 21. Palfrey Papers.

[293] The first two workshops had no any European attendees; only Selten attended the third workshop.

[294] Frans van Winden Interview; Palfrey letter to ESA officers April 2, 1996. Palfrey Papers.

[295] For more information on Heinz Sauermann see Section 2.4.

The issue of ESA's not being truly international, though not perceived by the Americans, was clear to the Europeans. When Palfrey had polled previous ESA Presidents about the 1996 Kluwer proposal, Smith in his reply noted that the ESA "from the beginning has been international in its scope."[296] However there was only one European member of the ESA's Executive Committee – Frans van Winden.[297] Primarily there was a desire for a greater acknowledgement of the European community that would manifest itself in ESA conferences in Europe also and not primarily in Tucson. This stance was most clearly taken by the CREED group in Amsterdam. The market survey conducted by Kluwer indicated that "internationalization is crucial for the success of the journal."[298]

Palfrey then decided to poll the officers and ex-presidents of ESA "about the best form for this affiliation to take and how to go about implementing it. Among others issues, this involves ESA participation on the Editorial Board of the journal, the formal role of the "Advisory Board" vis-à-vis the editors and the publisher - and ESA representation on it, reduced dues for ESA members, any official endorsement of the journal by ESA, and so forth." [299]

Essentially all the replies in Palfrey's files suggest that the journal should be an official publication of the ESA.[300] Plott expanded on his previous email and suggested, for instance, that the publication of referee reports in controversial cases might be encouraged; he stressed the electronic component of publication and linking subscriptions to membership of the ESA.[301] In expressing support for the journal Smith remarked that "things are going great guns."[302] Some members of the ESA Executive Committee thought that things were perhaps going too fast; it was not clear that enough high quality papers were available, given the members' various experiences on editorial boards of second tier journals publishing experimental papers. James Cox raised the issues of the ownership of the title and the enforceability of Kluwer's willingness to keep

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[296] Smith email to Palfrey March 22, 1996, Palfrey Papers. Box 20, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[297] Van Winden served as a member of the Executive Committee from 1994 to 2001.
[298] Schram papers.
[299] Undated email, but sent before March 22, 1996; Only Raymond Battalio did not respond. Palfrey Papers
[300] Hoffman email to Palfrey March 24, Smith email to Palfrey March 22, Forsythe in an email predating the poll March 19, 1996. Palfrey Papers.
[301] Plott email to Palfrey March 22, 1996. Palfrey Papers
[302] Smith email to Palfrey March 22, 1996. The phrase 'going great guns' means that something is proceeding very successfully and quickly. Palfrey Papers.

subscription rates low that had been agreed between Rolnik and Holt. For instance, the PCS allowed the publisher of their journal *Public Choice* to own the title. This became a major error restricting the ability of the PCS members to steer their journal themselves. [303] In summary, Cox wanted the following questions to be addressed:

> "(1) Why would the ESA want to endorse a journal that it does not own the title to?
>
> (2) Why would ESA want to let someone else capture the rights to the journal title, Experimental Economics?
>
> (3) Why would ESA not want to retain the authority to choose both the editors and the editorial board of an experimental economics journal?" [304]

The results of the Kluwer questionnaire, with a response rate of 65%, were overwhelmingly favorable. Almost 90% of respondents saw the need for a journal and were willing to submit papers and serve on its board. A third of the 62 replies came from outside of North America and a number of comments referred to the need for an international presence: "Your main problem is to ensure that the journal is not seen 'belonging' to some factional group ... Even though ESA thinks of itself as international, I do not think it is perceived as such by non-Americans. [305] The journal not being factional is a very important point, as the opposite would reduce credibility of the journal and of the whole experimental community."

Palfrey with permanent feedback from Cox set out to write a concentrated strategy for the ESA that would contain the ESA Journal Guidelines, advice how to react to Kluwer's proposal, and would go on to ask what changes of ESA bylaws were needed to reflect the extent of the journal's adoption and internationalization. [306] In regard to internationalization, Palfrey summarized the debate in April 1996: "There have been

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[303] Similar problems occurred for the European Economic Association (EEA). The EEA partnered with Elsevier in 1986 and designated Elsevier's journal European Economic Review as its official journal, which remained the property of Elsevier. "Dissatisfaction at Elsevier's pricing policies also persisted, and was highlighted by the adverse publicity arising from Ted Bergstrom's study." James Cox Interview The library journal subscription was then $1225 per year. Since 2003, the EEA's journal has been the *Journal of the European Economics Association* published by MIT press. For details see BERGSTROM, T. *Breaking away: Success Stories* [Online]. Available: http://www.econ.ucsb.edu/~tedb/Journals/alternatives.html [Accessed 2011, February 20].

[304] James Cox's email to Palfrey March 25, 1996. Palfrey Papers

[305] May 31, 1996 memo containing results from Zac Rolnik. Schram Papers

[306] Palfrey memos on April 2, 9 and 10, 1996, Palfrey Papers.

concerns voiced, particularly from European members ("member" is currently defined loosely as participation in recent ESA conferences) that the current structure of ESA executive committee has led (apparently unintentionally) to insufficient non-US representation in the governing body of ESA."[307] Therefore he suggested creating the position of a European Secretary.

In regard to the journal, Palfrey and Cox advised the ESA to "have an official journal, [with] a discounted subscription which will be included in an annual ESA membership fee. The membership fee should be structured to encourage membership in ESA and to help offset publishing costs and ESA operating costs." ESA should have the right to determine the "members of the editorial board, particularly editors or co-editors," and should have the right to "keep the number of articles sufficiently limited to ensure high quality." Another requirement that became a bone of contention with Kluwer was that the title of the journal was to be owned by ESA. These general guidelines were written in the light of the Kluwer proposal, but the ESA officers were willing to pursue other publishers if Kluwer was not willing to commit to the ESA requirement. In regard to the particular Kluwer proposal, they agreed that Charles Holt and Arthur Schram should be co-editors and also ex-officio members of the Executive Committee of the ESA. This was viewed as the best reaction to counter Kluwer's "first mover advantage."[308] The proposed titles were either *Journal of Experimental Economics*, or *Experimental Economic*s.

Kluwer, represented by Rolnik, was willing to accept most of the terms, but wanted to keep the right to choose the editors through a mutual agreement with the ESA, leaving the latter with only nominating rights. They feared that if an editor turned out to be an outstanding researcher, but an organizational disaster, they would lose control. Kluwer also wanted either to own the title or enter into a co-ownership arrangement, sharing losses and profit.[309] A long term contract was requested, suggesting that Kluwer was worried that after the initial contract expired if the journal was successful the ESA would shop around and leave Kluwer with the initial expenses. This would also provide an incentive for Kluwer to do a good job that would persuade the ESA not to leave them after the initial period. Finally, Kluwer wanted to announce the new journal in the

[307] Palfrey memos on April 2, 9 and 10, 1996, Palfrey Papers.
[308] James Cox to Tom Palfrey, Palfrey Papers.
[309] Rolnik email to Palfrey June 4, 1996. Palfrey Papers.

summer of 1996 with the first issue in spring 1998. Palfrey wanted to hold it back until the bylaws were approved in October 1996.[310]

The basic details were agreed between Rolnik and Palfrey during a meeting in Café Flora (or perhaps Les Deux Magots) on Boulevard St. Germain in Paris on June 28, 1996. September and October were spent on many exchanges about the legal details of the contract, each party negotiating for its maximum benefit, in particular on the issue of ownership and editorial succession.[311]

Finally, on October 17-20, 1996, ESA convened again in the Westward Look in Tucson, Arizona. Palfrey submitted the three main ESA Bylaw changes and the final contract with Kluwer. First, the ESA officers were reorganized - a new European Membership Secretary and North American Membership Secretary were created replacing the position of secretary that in the past had merged in the joint function of the Secretary-Treasurer (Mark Isaac). The new position of treasurer was restricted to a 6-year, renewable term. The annual meeting of the ESA would in future be held in Europe once every 3 years, and regional ESA conferences could be organized annually.[312]

Second, the subscription to the official ESA journal was to be included in the annual ESA membership fee, and the two new co-editors, Schram and Holt,[313] became ex officio non-voting members of the executive committee. The editor or co-editors would be selected by a Board of Editors and appointed by Kluwer.[314] Besides the Board of Editors, the journal would also have an Advisory Board to give guidance to the editors.[315] The contract with Kluwer was set for 15-years with the option to renew. The frequency of the journal was set at three issues per year and rising to four by volume 5. Kluwer guaranteed low rates for members and also student rates. Most importantly, the ESA retained the full title

[310] Email from Tom Palfrey to ESA Officers June 5, 1996. Palfrey Papers.

[311] Palfrey received help from Michael Keller, a lawyer at Caltech's Patent Office.

[312] As James Cox recalls, it was a spoken agreement that the regional conference would not be organized on the continent that hosted the annual meeting, but this provision was not included, which caused some consternation during the Andrew Schotter's Presidency.

[313] Holt stepped down on July 1, 2004, replaced by Tim Cason, and becoming a member of the Advisory Editors. Schram stepped down on January 1, 2007, and also became one of the Advisory Editors.

[314] "The editorial board will be responsible for appointments and reappointments of the coeditors, approval of the coeditors' nominations of associate editors, general editorial policy, and making recommendations to the publisher regarding changes in publication policy including but not restricted to subscription rates, special issues, number of issues per year, number of articles per issue, journal format (articles vs. comments, replications), data archiving, exploitation of new publishing technology and so forth."
The first Editorial Board consisted of 31 members spanning a variety of disciplines and countries (USA – 22; Spain 1, UK 3, Germany 2, Switzerland 1, Japan 1, the Netherlands 1).

[315] The Advisory Board consisted of James Cox (ESA President), Thomas Palfrey (Past President), Charles R. Plott, Alvin E. Roth, Andrew Schotter (President Elect), Reinhard Selten, and Vernon L. Smith

to and copyright of the journal, published articles and would receive royalties from sales. Kluwer would also maintain the journal website. And the editors received a budget to cover the placing of colored graphs on the cover.[316]

Formal internationalization did not immediately lead to the impression that the ESA represented all experimentalists the world over. This was compounded by the fact that the first dedicated experimental journal came with membership of the ESA. The only other subscription option was an expensive institutional subscription. Hence members of the GfeW who wanted to subscribe to the journal had to become members of the ESA. Many, senior GfeW members in particular, considered this an attempt of the Americans to dominate, "American imperialism,"[317] not least in view of the fact that the GfeW was an older organization. In addition the question was raised "Why don't ESA members join the GfeW – a marriage-like arrangement?"[318] There was a strong desire on the German side to have a professional organization "because of this special thing – you have to keep a German identity, or German language identity, because we have Austrians and Swiss."[319] The issue thus became "how to fold in their separate identity with"[320] the ESA.

The issue was discussed at the workshop Theories of Bounded Rationality in Bonn (May 6-10, 1997), where Selten has held a chair in economics since 1984; however, no agreement was reached. But the younger generation of German experimentalists did not mind being members of the ESA. Martin Weber in 1996 became the first ESA European Secretary. He organized the first international ESA conference in 1998 in Mannheim. Even the GfeW Chairman Werner Güth supported it. The breakthrough was achieved in Mannheim the following year, during a dinner with representatives of ESA and GfeW. Cox, Palfrey, Holt, van Winden, Schram, Weber, Selten, Tietz and Güth, among others, were present. The meeting was tense, as recalled by the non-German participants.[321] The solution to the impasse came through a free subscription to *Experimental Economics*. Zac Rolnik from Kluwer recognized that publishers earn most from selling journals not to individuals, but

[316] Actually this never happened. The front cover of the journal depicts a step function. Holt describes its colorfulness as being inspired by Amsterdam. Personal communication at the Witness Seminar. The cover changed slightly for Volume 12 (2009) issue 1, with Springer as a new publisher.
[317] James Cox Interview; van Winden drew an analogy with similar feelings in the European Public Choice movement (he was its President from 1986/7; Schram was co-President in 1997/8). Frans van Winden Interview. "And there also we felt that it was important for Europe to also develop itself and not to get too much dominated by the people from the United States, and that played a little bit of a role."
[318] Quote from Charles Holt Interview, repeated in James Cox Interview.
[319] Reinhard Selten Interview.
[320] Charles Holt Interview.
[321] James Cox, Thomas Palfrey, Martin Weber, Frans van Winden Interviews.

to libraries. Members of the GfeW would receive free subscriptions to the journal for a two year period, hence effectively becoming members of ESA without having to pay any dues. After this they would have to decide whether they wanted to continue the journal through ESA membership. The journal was distributed by the GfeW, and thus Kluwer did not have to pay the postage costs.[322]

The publication date of the first issue of *Experimental Economics* overlapped with the first annual meeting of the ESA outside of the US, the one in Mannheim from June 11-13, 1998. The final agreement between Palfrey and Rolnik and the ESA and German experimentalists addressed all concerns – the journal and society being truly international and not appearing factional. Through the ownership of the journal title the ESA remained in charge of its image, while the journal exuded credibility through the scientific reputation and credit of its board members. All these considerations were aptly summarized in the editorial to the first issue:

> "The international nature of this research is apparent from the list of authors for the first issue, and from the list of associate editors who have graciously agreed to assist us. We are pleased to have the support of seven advisory editors, who have done pioneering work in experimental economics. These editors include past and future Nobel Prize winners and members of the board of the Economic Science Association. Although experimental papers have been and will continue to be published in leading general journals, the number of high-quality experimental papers has grown to an extent that a specialized journal can provide a useful platform for the exchange of ideas and results."

Experimental Economics was included in the Social Science Citation Index from volume 7 in 2004 and by 2009 it had become a journal with the ninth largest impact factor in economics (Economic science Association, 2009).[323]

[322] James Cox Interview.

[323] The impact factor was embellished by the publication of Urs Fischbacher's z-Tree manual which has drawn close to 400 citations. FISCHBACHER, U. 2007. z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics,* 10**,** 171-178.

4.5 Conclusions – a Congenital Design Deficiency?

The name of the ESA, even a quarter century after its foundation, remains the most visible legacy of Smith's *Prologue*, which itself has lain on dusty shelves and the boxes of those who read and forgot about it. For a non-experimental economist and for a new member of the experimental community, the name *Economic Science Association* and its relationship to its official journal *Experimental Economics* must be perplexing, in particular if not accompanied by an explanation of the origin of the name ESA, which nowadays has a historical flavor. From this viewpoint it is not surprising that ESA's website goes to great lengths to emphasize experiments as its core activity while a quarter of a century ago the idea behind the ESA was that it should be about connecting theory to observable data in a new and innovative way, sparked by the experimental turn.[324]

In the foundational debates of 1986, the use of the word 'science' appeared pretentious to senior scholars such as John Kagel, Charles Plott, Al Roth, Reinhard Selten and many junior scholars alike, for various reasons described in this chapter. The opposition to the name *Economic Science Association* was obviated by a majority vote and it reflected the strong position of Smith and his Arizona group. But while the question of launching a dedicated experimental journal has been resolved, the name issue is latent and continues to resurface. At the last Executive Committee chaired by Andrew Schotter in 2001, somebody raised the prospect of changing the name of the *Economics Science Association*, because he thought it was pretentious to say that "we are the only scientists."[325] When John Kagel was the president (2005-07), he proposed to rename the ESA the *Society for Experimental Economics*, or SEE with the subtitle 'we look for and SEE

[324] Consider the ESA website. Its top frame, visible from every subsection of the website, has not only the full title Economic Science Association, but also a subheading "Using controlled experiments to learn about economic behavior." The section About ESA provides this description: "The Economic Science Association (ESA) is a professional organization devoted to economics as an observational science, using controlled experiments to learn about economic behavior. The ESA welcomes participation by economists interested in the results of such experiments, as well as scholars in psychology, business, political science, and other related fields." Only the Bylaws section states the purpose of ESA that was agreed in 1986 "to advance, enhance, or further economics as an observational science through use of laboratory and field methods of observation and data collection under the control and responsibility of the research investigator." Even though the first two descriptions are more accurate, clearly, much of the original ideas behind the ESA and the desire to make an impact on the economic profession beyond promoting the experimental methods have been forgotten. ECONOMIC SCIENCE ASSOCIATION. 2010-12. *ESA - Experimental Economics* [Online]. Available: https://www.economicscience.org/esa/index.html [Accessed September 17, 2012].

[325] Andrew Schotter Interview. Schotter deferred the issue to the next president.

what other people conjecture,' because he thought that "it would be kind of neat."[326] Al Roth, the current president (2011-13), does not object to a name change: "I wouldn't mind seeing the name of the society have the word experimental in it, just because that would be descriptive. And because there are other ways of doing science."[327] Such uneasiness and ambivalence over the name are also reflected in the way that some economists list their ESA membership in their CVs. Tim Cason, ESA president 2009-2011 and editor of *Experimental Economics* (2004-09), and John Kagel explain that *Economic Science Association* stands for "the international association of experimental economists" and "a.k.a. Experimental Economics Association" respectively.[328] Al Roth started to include his membership of the ESA only once he had become its President Elect in 2009, although he had been a member since its inception and had been declaring his membership of half a dozen other societies.[329]

However, Tom Palfrey, president 1995-1997, thinks that the name will not be changed, because the ESA is already established and people are used to it.[330] Elizabeth Hoffman, president 1989-1991, believes that opening this issue may quickly make it personal,[331] and according to others, for the very same reasons they would prefer to abstain from such a move or even discussion. Vernon Smith still believes that the name ESA is a suitable label for the experimental enterprise. However, "I understand the moves to change the name here. If people don't feel they are doing science, they shouldn't carry that name."[332] In 2012, John Kagel predicted at a session commemorating the ESA's quarter century that within the next twenty-five years the society would change its name.[333]

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[326] John Kagel at the Witness Seminar.
[327] Alvin Roth at the Witness Seminar.
[328] Timothy Cason's online CV from March 2011 retrieved April 2011; John Kagel's online CVs from March 2006 retrieved April 2009.
[329] Alvin Roth's online CV compared February 2009 and June 2011.
[330] Thomas Palfrey Interview.
[331] Elizabeth Hoffman at the Witness Seminar.
[332] Smith Interview. This is an inverted argument of the one that Charles Plott made: that if a discipline needs to call itself a science, it is not. See Footnote 223.
[333] Harro Maas and I organized a joint session of the *History of Economics Society* and the ESA in January 2012 at the ASSA annual meetings in Chicago with the title *Reflecting on Twenty-Five Years of the Economic Science Association*. Presiding: Harro Maas (University of Utrecht); Papers presented: *Historical Perspective on ESA's First Quarter Century* by Andrej Svorencik (University of Utrecht); *The Prologue to ESA from Today's Perspective* by John Kagel (Ohio State University); *Structural Changes to ESA in 1995-1997: The Journal and International Meetings* by Thomas Palfrey (California Institute of Technology); *Making ESA International* by Martin Weber (University of Mannheim); the Discussant was Steven Medema (University of Colorado-Denver). Vernon Smith (Chapman University) could not attend due to illness.

Why does the issue of ESA's name remain so contentious? To answer this question one must look at the fragmentation of the experimental community at the time of the ESA's inception. Compared, for instance, to the *Public Choice Society*, one key difference is that the ESA's formation came at a time when several research groups were already in existence, with one of them, Arizona, playing a leading organizational role and also dominant in size.[334] The fact that in the 1990s other research groups existed in Europe, with different practices and norms, added a further complication. This presence of several such groups, with the associated cast of dominant personalities, contributed to the fits and starts that we saw around the formation of the ESA and the decision to establish a journal. While the founding members were committed to the forces of the experimental turn, they had somewhat different visions of where experimental economics is and how it should evolve, in particular what its image should be among the rest of the profession. There were two additional sources of fragmentation in the late 1980s and early 1990s: the first-price auction controversy and the growing differences between experimental and behavioral economists. The former is examined in the penultimate chapter, the latter touched upon in the concluding one. (Heukelom, 2014)

[334] Until its demise in the early 2000s, the relative standing of the Arizona research group continued to decline as other groups continued to grow or churn out Ph.D. graduates thereby reinforcing their standing. Smith and a host of others moved to George Mason University in 2001. Mark Isaac left for Florida State University in 2001. James Cox left for Georgia State University in 2005. For some of the details, see Smith's Autobiography SMITH, V. L. 2008a. *Discovery - A Memoir,* Bloomington, IN, AuthorHouse.

5 Rejecting Rejections – the Passive Reception of Experimental Economics

"I read the enclosed [referee reports] as an invitation to argue."

Charles Plott to Mark Isaac, November 4, 1982, regarding their paper on public goods that was eventually published after two rejections (Isaac et al., 1985).

In this chapter I concentrate on the interaction of experimentalists with the rest of the economics profession in order to better understand how experimental research gradually gained acceptance in the wider economics community. I describe a mechanism of passive reception of experimental economics, in which experimentalists were dealing with several audiences simultaneously. Experimenters had to convince editors and referees of journals that their research should be published. To continue experimental research it was necessary to secure funding from the *National Science Foundation* (NSF) in particular. Funding was used both for building laboratories and for running experiments. Further research generated new research questions (the experimental engine) and additional demand by experimentalists to publish their new results. Other audiences, which are discussed in this chapter only indirectly, included economists at research seminars, non-experimenters within economics departments where experimentalists were located, and departments hiring experimentalists for the first time. There were also economists and non-economists at companies and public institutions which commissioned applied experimental research.

I refer to this process as passive reception. The various audiences that experimentalists sought to convince of the merits of the experimental method shared a lack of direct experience with economic experiments. Their knowledge of the experimental method was mediated by reports of such research and not directed to gaining experiential knowledge about the intricacies of the experimental method and research. My study of the active reception began in Chapter 2 where in the case studies for the four driving forces of the turn I also traced the individual trajectories of several early experimentalists. Then, in Chapter 3 the progress of the experimental turn from the individual and communal level through the creation of small local communities, such as those at Caltech or Arizona, was linked to the emergence of economics laboratories.

Passive reception is an important element of the experimental turn because it decided whether experimental economics would join the economics mainstream, i.e. whether the experimental method would become an accepted, if not widely used, method in economics. Thus, while the active reception defined the community of experimental economists and influenced its internal growth, the passive reception defined the boundaries, the distance and image of experimental economics to the rest of the profession. The boundaries were negotiated on two fronts – publications in general economics journals and funding mainly from the National Science Foundation (NSF). These two fronts were closely connected and remain so, because publication success has increasingly been a significant element in grant evaluation and success in gaining grants provides external affirmation that the supported research agenda is worthwhile to pursue and shields it in part from skeptics.

The aspect of passive reception by journal editors and referees is examined in the first section of this chapter. Whereas there were no public detractors of experimentation in economics, the early experimenters such as Charles Plott and Vernon Smith encountered skeptics and systematic rejections of their submitted papers. Getting them published required tenacity on the writers' part to go through several rounds of often heated discussions with editors and referees. These iterations present a unique perspective on the arguments raised against experiments in economics and the strategies developed by experimental economists to counter them. Specifically, I use the research corpus of Charles Plott covering the period from the mid-1970s to the mid-1990s to analyze these strategies. This chapter requires patience from the reader – not only because that the corpus is so extensive, but primarily because these strategies are varied and nuanced.

The second section analyzes how experimentalists were able to secure funding in the same period. I study both the objections raised by the evaluating panels and the role of the NSF program officers, Daniel Newlon in particular, who often rejected the unfavorable recommendations and was able to steer funds to experimentalists for reasons that are also scrutinized here.

The third section addresses book publications about experimental economics since they presented a vehicle for the dissemination of experimental results and methodology in a comprehensive and pedagogic way. The book publications influenced both the active and passive reception of experimental economics and they reflect the stage of maturity that experimental economics had reached.

In the remainder of this chapter I will refer frequently to the following papers by invoking their authors:

Isaac Plott's paper deals with the effect of price ceilings and price floors, published in AER (Isaac and Plott, 1981)

Isaac McCue Plott's paper on public goods provision, published in the Journal of Public Economics (Isaac et al., 1985)

Palfrey Forsythe Plott's paper on rational expectation and asset valuation in experimental markets, published in Econometrica (Forsythe et al., 1982)

Plott Sunder's paper on information aggregation, published in Econometrica (Plott and Sunder, 1988)

5.1 Publication Strategies

> "I realize that it is rather unusual for an author to be so aggressive with a submission and might even be bad politics. It sure makes me feel better."
>
> Charles Plott to Robert W. Clower, February 29, 1984.
>
> "P.S. How come I don't feel better when you let off steam?"
>
> Robert W. Clower to Charles Plott, August 14, 1984.

Referee reports and editorial decisions provide an unfiltered view of the reception of experimental work by non-experimentalists. Correspondence most often took the form of an initial rejection to which the experimentalists forcefully responded with subsequent negotiations about the form and content of the paper reflecting a deeper discussion about the nature of economics, the role of the experimental method, and the position of experimental economics.

Before I can address the various strategies that the experimentalists developed, I should address the question 'Why were experimentalists so keen on getting their research published in leading economics journals?' When we look for instance at the history of econometrics, we see a different strategy than theirs. Econometricians just a few years after founding the Econometric Society launched their own journal Econometrica. Why did experimental economists not do the same? An important part of the answer to this question lies in the changed landscape of economics. By 1980s economics had undergone a process of both unification and differentiation. While the interwar period is best described as the age of pluralism in economics, (Morgan and Rutherford, 1998) the post war period saw the emergence of mainstream - first neoclassical synthesis and later the dominance of general equilibrium. Having this overarching dominant theoretical framework from the 1960s onwards, impelled a process of differentiation. Many new subfields, mostly applied economics in nature emerged, fueled by the growth of the economics discipline. [335] These new subfields gained institutional structure, often supported by dedicated journals, and soon started to live their separate lives.

The fear of being shunned by other economists was most visible in the experimentalists' efforts not to be excluded from leading economics journals. This anxiety had both a social and an epistemic element. Experimental economists viewed themselves as economists and wanted to be taken seriously by traditional theorists, the most esteemed group in economics. After all, the theorists were originally the peers of such early experimentalists as Smith, Plott, Roth, Selten, Kagel and Battalio. Starting a dedicated experimental journal would mean a smaller influence on the discipline as a whole which would counter the belief of experimentalists in the equal and symmetric standing of rigorous (experimental) data and theory, with its consequence that the experimental method should be part of the core of economics.

Furthermore, papers in the top outlets served several purposes that fed into each other. Besides personal career advancement, publications served as evidence to funding agencies, the NSF above all, furnishing further support. The details of the funding of experimental economics may be found in the following section.

Instead of approaching the publications chronologically, I would rather look at the style of operation of Charles Plott and his co-authors.[336] I do not trace how individual strategies

[335] Adam Smith's 'the size of the market determines the size of division of labor' seems to apply.

[336] In the period 1965, when Plott graduated from Virginia, until 1995, he had published 60 papers in refereed economics journals and political science journals. Nine were in AER, five in Econometrica, one JPE, and three in QJE.

To give an impression of the growth of experimental papers, consider the following data points: Leontief reported that the share of papers in the AER with experimental data in the periods 1972-76 and 1977-1981 were 0.5% and 1.9% respectively. A follow up study by Morgan reported on the period 1982-1986 and observed that 6% of all articles in the AER were experimental. LEONTIEF, W. 1982. Academic Economics. *Science,* 217**,** 104-107. MORGAN, T. 1988. Theory versus Empiricism in Academic Economics: Update and Comparisons. *The Journal of Economic Perspectives,* 2**,** 159-164.

emerged, developed and capitalized on other strategies. Most strategies were practiced from early on and by the second half of the 1980s they were honed and perfected to suit the particular circumstances of individual articles under consideration. Moreover, experimentalists shared these strategies - not in correspondence or private conversations alone,[337] but also publicly at meetings such as those in Tucson. Until at least 1990, the Tucson meetings and subsequently the ESA meetings had only plenary sessions. This allowed common knowledge to be built among experimentalists and was an important factor in fostering the experimental community and its identity. Glenn Harrison attended the 1984 Tucson meeting and he, like a number of other attendees,[338] recalls one particular session where Charles Plott used one of his papers that had had a tortuous path to publication as a case study of publication strategies:

> "[Plott] said, "I've gone through all the journals. Here, let me just take you through some of the criticisms and how we dealt with them and what the editor [replied.]" He would literally just read out what the editor would say, what the referee would say, and how he responded. People were literally learning the craft of getting papers past editors and referees. … There was a clear focus on how do you get published, how do you keep things from being regarded as cheap behaviorism, untethered theory, how important it is to have rigorous theory, etc., etc. The reason was, otherwise, the top journals won't be interested in it, so that was a very clear focus then. At that stage, everybody in my first generation was lapping that up, because we had been writing papers. We were simply being taught by some masters on how to [publish experimental papers.] That is one of the contributions of Vernon [Smith] and Charlie [Plott], which Charlie is very happy to remind Lisa [Rutström] and I. Over wine and with a smile, he said: "You know, there are a lot of early scars that I incurred, you reaped the benefit of." My generation reaped the benefit of – I am an old man – but even in the 1980s and earlier, they had been incurring a lot of scars that made it easier for us just to be able to publish stuff."[339]

[337] For instance Ernst Fehr recalls that: "Charlie Plott once told me "You have to educate the editors." And I think there's a lot of truth in that. When you do something new you always have to educate the people. And the early experimental economists deserve the credit, all the behavioral economists, that they fought the battles upon which the next generation could build." Ernst Fehr Interview.
[338] Many interviewees such as Holt, Schotter, and Isaac recall this session where attendees discussed how to counter negative referee reports.
[339] Glenn Harrison Interview.

The paper that Plott was referring to appeared in print a year later. It was the public goods paper by Isaac, McCue and Plott (Isaac et al, 1985), which will be the starting point of my analysis in the next section. Harrison's quotation also justifies the selection of Charles Plott and his correspondence with editors for the study of the passive reception of experimental economics. Furthermore, the quotation touches upon the tactics in the communities for countering criticism.

Before a paper reached an editor's desk, it was usually circulated through a departmental working paper series and presented at various conferences and seminars. While such a practice was commonplace across the whole discipline, both Smith and Plott sent some of their papers to various prominent theorists with expertise in the subject of the particular experimental paper, sometimes resulting in a conversation about the particular results and experimentation in general. Plott and Smith knew many of these recipients personally from previous academic appointments, sabbaticals or profession related work.[340] For instance, Plott sent the first asset market experiment (Forsythe et al., 1982) to Jerry Green, Eugene Fama, Robert Wilson, and other theorists in Yale, Stanford, UCLA, but also to the Federal Trade Commission.[341] Plott's first general equilibrium experiment (Lian and Plott, 1998) was sent to Solow, Samuelson, Debreu, Friedman, McKenzie, and Arrow.[342] Plott knew editors such as Peter Fishburn, Franklin, Hugo Sonnenschein from his (mathematical) social choice research period of the 1960s and 1970s.

5.1.1 A Representative Letter

Typically, Plott used a number of strategies simultaneously. Therefore in this subsection I provide an example of a representative letter to an editor and only afterwards do I tease these strategies out one by one and add further examples of their use. The letter below is also one of the earliest instances of an elaborate rebuttal of the dismissal of a submitted paper. Plott wrote this particular letter on October 17, 1979, to George H. Borts, the AER editor from 1969 until 1980. The letter is concerned with the paper that Plott wrote with one of his students Mark Isaac, entitled *Price Controls and the Behavior of Auction*

[340] For instance, Smith while at the Center for Advanced Study in the Behavioral Sciences at Stanford in 1961-2 met Ken Arrow and one of his students at Purdue was Hugo Sonnenschein; Plott at the University of Chicago in 1980 got to know Gary Becker and Milton Friedman and through work for the NSF they both met Robert Solow.

[341] Folder "1982 Forsythe Palfrey Plott Folder 4 Correspondence." Plott's Papers.

[342] Only Solow replied. Folder "Plott Correspondence 1993 I." Plott Papers.

Markets: An Experimental Examination. (Isaac and Plott, 1981) The paper deals with one of the most basic economic situations discussed in all introductory economics courses – the effect of price ceilings and price floors (i.e. price controls) on market price and quantity. Since the days of Alfred Marshall price controls had been modeled within the framework of competitive markets and partial equilibrium analysis. Such modeling presupposed strong theoretical assumptions for prices to converge to equilibrium. By the late 1970s, a number of papers on experimental markets were published that observed that the predicted equilibrium is reached under less strict theoretical assumptions. These papers showed that the particular institutional setting governing the interaction between buyers and sellers affects whether convergence will take place (Smith, 1962, Smith, 1965, Miller et al., 1977, Plott and Smith, 1978). From a methodological perspective the Isaac-Plott paper is a straightforward extension of these previous market experiments. Isaac and Plott added to previous double auction market experiments only by restricting which prices could be attained in the experiment.[343] This makes the provided example even more telling since the editorial process did not have to assess any novel experimental design.

The paper was first submitted to the AER in February 1979. The authors sent a corrected version in March 1979. By October of the same year the paper had been reviewed by one referee who did not recommend it for publication in the AER. Although this letter is not preserved in Plott's files, Borts in subsequent correspondence referred to the referee as not taking the experiment by Isaac and Plott seriously. In the letter from October 17, 1979, Plott asked for a reconsideration of the paper. Borts obliged and on May 6, 1980, replied that the new referee viewed the paper as important for market theorists. If the paper could be shortened, first by removing the details of experimental procedure that he did not find essential, then he would be willing to publish it. [344]

I have identified nine different strategies, which for convenience purposes I denote S1 to S9:

[343] The Isaac-Plott paper observed that in case of strictly binding price controls, the experiments are well described by the partial-equilibrium model. However, in the case of non-binding price controls the convergence to the static competitive equilibrium price does not occur; the prices fall below (above) the equilibrium in the case of a price ceiling (floor). Around the same time a number of related experiments on price controls were performed and published. SMITH, V. L. & WILLIAMS, A. W. 1981. On Nonbinding Price Controls in a Competitive Market. *The American Economic Review,* 71**,** 467-474, DON, L. C. & VERNON, L. S. 1983. Price Controls in a Posted Offer Market. Ibid.73**,** 218-221.
[344] Ibid.

- S1 Asking for knowledgeable referees because previous referees were ignorant of experimental economics
- S2 Claiming that results are interesting, relevant for theory and have applications
- S3 Claiming that the experiments present real situations
- S4 Claiming that the theory applies to simple cases
- S5 Citing basic research
- S6 Conducting more experiments
- S7 Shifting of the burden of proof
- S8 Steering clear of a specialized journal
- S9 Claiming that field has been confused with method

Plott's letter to Borts from October 17, 1979, is rather long and contains the first seven strategies. Below I quote the letter in full and for each strategy I indicate when it starts and when it ends. Please note that in one case one strategy is nested within another – S2 is nested within S1 – which means that S1 was interrupted by S2 and subsequently came to an end. Text in italics shows my emphasis, but any underlined text was marked by Plott. The original paragraph layout is preserved.

> "Dear George:
>
> We request that you reconsider the Isaac-Plott paper. [**S1**]The referee has based the decision on an amazingly sweeping generalization about experimental economics.[345] It reflects neither systematic thought about the generalization itself nor an awareness of the literature which has specifically addressed that point of view. In fact, it was the discovery that such an attitude can be dismissed that recently attracted so many economists to the laboratory field. The literature is so filled with discussions about it that Isaac and I did not even include a methodological section [in the submitted paper].
>
> [**S2**] Before continuing we should demonstrate that you are getting and sending inconsistent messages and therefore should at least give serious consideration to our arguments. Your decision on the original Levine-Plott agenda paper (AER 1978) was to require that the nonlaboratory application be cut from the paper on

[345] As the referee report has not been preserved in Plott's personal archive it is unclear what the "sweeping generalization about experimental economics" was. Based on Borts' suggestions quoted earlier I conjecture that Plott refers to the claim that experimental economics is at best a teaching method and has nothing to say to theory nor any practical application.

the grounds that it was uninteresting. We were disturbed by this decision because the entire paper represented a (perhaps the first) good example of how one can use laboratory methods to learn things about the behavior of more complicated processes. Even though the portion you had us remove was subsequently published in the Virginia Law Review, the separation was unfortunate. The continuity among theory, laboratory, and application was lost, and we now do not have it as a self-contained document which can be used to demonstrate that any claim about the general irrelevance of laboratory processes, like that of the current referee, is simply false. You may also find it informative to reread the referee report on the Grether-Plott paper (AER 1979). [**end of S2**] The methodological justification for that study is not substantially different from that for the Isaac-Plott paper but the difference of opinion of the referees is dramatic. The fact that such obvious inconsistencies exist suggests that the subject should at least be approached with an open mind.

The referee's opening remark reveals amateur status. [**end of S1**] [**S3**]The laboratory processes are not “games.” Those participating often did so at some personal expense because the possible return on their time was considerable (over $30 per hour for some and an average of $8-$10 per hour over all participants). The laboratory processes are simple and very special markets [**end of S3**] [**S4**] but they are nevertheless real markets which should be governed by the same principles that are supposed to govern *all markets*. The justification for studying them is the same as the justification for studying the simple special cases and special types of any complicated phenomenon.

In order to see why these markets are real, one need only apply directly the theory of derived demand. It works as follows. Let $R^i(x^i)$ be the revenue received by individual i from some source expressed as a function of the number of units (xi) he has to sell. Standard derived demand theory tells us that $\partial R_i/\partial x_i$ is limit price (inverse demand) function for this individual. It is important to note that *the theory places no restriction upon the source of the revenue* so when the source is an experimenter the derived limit price function for this individual is just as real as when the source is a business. Furthermore, the *theory places no restriction on what x is called* (unless the individual gets consumption pleasures from it) so the theory applies equally as x_i becomes baseball cards, shirts, food, or "commodities" created especially for the purposes of an experiment. There are no "side payments" or incidental sources of enjoyment so as long as the individual prefers more money to less we can be assured the preferences for units of x have been induced. The individual is indeed a "demander."

The supply side of the market is handled similarly. Each supplier, j, faces an individualized cost function $C^j(x_j)$ which indicates what j must pay the experimenter as a function of units purchased for resale. Profits to j, which are j’s to keep, are simply the revenues received by j over costs $C^j(x_j)$. Clearly that $\partial C^j(x_j)/\partial x_j$ is a real marginal cost function. The fact that it was constructed by the experimenter makes the concept no less *relevant* because the concept is intended to apply universally.

We have then a valued and scarce resource. Almost any textbook will say that those conditions are sufficient for the existence of an economic problem. The laboratory markets are thus real markets and the principles of economics should apply to them as readily as they are supposed to apply to any other market [**end of S4**]

[**S5**]Even though the markets are real enough, it is true that such markets are very simple compared to naturally occurring markets. Perhaps the referee does not believe in studying simple cases, but that is simply a rejection of basic research. [**end of S5**] [**S6**]Perhaps the referee thinks that the market institutions or the subjects should be different, but that is an argument for <u>additional</u> experiments as opposed to <u>no</u> experiments. [**end of S6**] [**S7**] Perhaps the referee simply believes that the phenomenon we discovered is characteristic of <u>no</u> other types of markets, but certainly his belief, especially in the absence of any investigation, cannot be taken seriously as evidence. Furthermore, how would such a hypothesis have been generated without the laboratory methods to start with (certainly the phenomenon, if it generally exists, would not have been discovered by the application of standard econometric techniques)? The truth is that the referee, like many people, has not really thought carefully about the problem. You might find the enclosed (p. 592 ff.)[346] of interest where we speculate about what the problem might be.

Had the referee read the paper thoughtfully, he would have found the lesson spelled out clearly enough. *Markets are sensitive to institutions in ways which the accepted general theory of markets does not anticipate. It is therefore not as general as has been presupposed*. What modifications to theory can systematically deal with the facts are open theoretical questions and therefore stand as a challenge to theorists. What institutional features of markets induce the phenomenon or prevent it are open empirical questions which we are willing to pursue whenever those who claim to really know how "prices are actually set in a trading market" identify the prominent features of those markets which are supposed to make the difference. [**end of S7**]

[**S1**] We are willing to provide a more detailed methodological argument. Clearly we have thought about the problem a lot. We confront it almost daily and with every experimental design. We sent you the paper because the discovery is really striking and does demonstrate some aspects of the power of experimental methods. We are asking for a thoughtful response. [**end of S1**]

Sincerely,

Charles R. Plott

Professor of Economics"[347]

It is worthwhile to note that compared to other letters to editors it is very unusual and rare that Plott added at the end of the letter that he is a professor of economics. I interpret it as a way of reaffirming that experimental economists want to be taken seriously by the rest of the profession and that they are part of the profession.

[346] Unclear reference.

[347] Letter from Charles Plott to George Borts, October 17, 1979. Folder "SSWP 253 Isaac, Plott 6-81 Price Controls and the Behavior of Auction Markets An Experimental Examination." Plott Papers.

5.1.2 A Survey and Interpretation of Strategies

This subsection elaborates on the strategies used in the typical letter and provides further examples of their use. In general, these strategies were used jointly and repeatedly.

5.1.2.1 S1 Knowledgeable Referees

The first strategy employed in Plott's representative letter was the request for a referee who is familiar with the experimental literature, if not an experimentalist himself. Usually this request came only after an initial rejection, not with the first submission. Notice that S1 is has two distinct parts in the representative letter. The first at the beginning of the letter conjectures why the referee did a poor job. The arguments that the referee raised were not about poor experimental design, data analysis nor significance of the findings which would be about the substance of the paper. Plott complained about the flat rejection the paper received, which showed in his eyes insufficient consideration of its substantive issues. In the second part of S1, which is the last paragraph of the letter, he highlighted the amount of work invested and requested a commensurate "thoughtful" reconsideration.

Requesting that a paper be judged by someone familiar with its topic and methods is a standard strategy not restricted to experimental economics. However experimental methods were novel and only a handful of economists were qualified. We can see examples of this strategy from the mid-1970s until very recently. Experimentalists did not ask for sympathetic or biased referees, but rather knowledgeable ones:

> "The point I wish to make is that experimental methods involves a special expertise and that the opinion of at least one specialist (not someone who is simply sympathetic) should be involved in the refereeing process."[348]

Sometimes Plott would suggest specific referees who would be able to appreciate the contribution and innovation of the paper:

[348] This letter referred to this paper LYNCH, M., MILLER, R. M., PLOTT, C. R. & PORTER, R. 1991. Product Quality, Informational Efficiency, and Regulations in Experimental Markets. *In:* ISAAC, M. R. (ed.) *Research in Experimental Economics.* Greenwich, Connecticut: JAI Press. Letter from Charles Plott to Robert Clower, January 11, 1985. Correspondence Drawers, Plott Papers.

> "Novices who are asked to be referees are frequently incapable of recognizing when important progress has been made. They also tend to incompetently judge the work from a methodological perspective. Several good young experimentalists exist now - Bob Forsythe (Iowa), Mark Isaac (Arizona), Tom Palfrey (CMU), Betsy Hoffman (Purdue), Arlie Williams (Indiana)."[349]

Sometimes matters escalated. A paper on information asymmetries that experimentally tested markets similar to those suggested in Akerlof's seminal paper on the market for lemons (Akerlof, 1970) took almost eight years from its first submission to get published (Lynch et al., 1991, hereafter, Lynch at al paper) It was originally submitted to the AER when Robert Clower was the editor and it was resubmitted when Ashenfelter became editor in 1985. Plott complained to him about the poor quality of the refereeing process so far, providing another example of S1:

> "The correspondence makes the nature of the problem clear. We used a sequential decision process design and did not elaborate upon the many considerations that led to each step. The referees either did not notice or did not accept the legitimacy of that methodology. The referees were also poorly informed about what is known experimentally. What we are doing might be wrong, but I don't see anyone else making more progress on applied problems. I have not seen any criticisms that were not fully anticipated by the design. Yet, the same issues appear and are answered again and again. I think publication of this paper in the AER will be important especially if it generates a public discussion of the criticisms that are now appearing in private."[350]

Ashenfelter was not hostile to experimental papers or Plott, per se. By the time of this letter to Plott, Ashenfelter had had some direct experience with experimentation. He published two experimental papers. The first contained a policy field experiment (Ashenfelter, 1978). The other was a laboratory experiment which was conducted at Smith's *Economic Science Laboratory* in the summer of 1984 and winter of 1988 (Ashenfelter et al., 1992).[351]

[349] Letter from Charles Plott to Robert Clower, February 29, 1984. Correspondence Drawers, Plott Papers.
[350] Letter from Charles Plott to Orley Ashenfelter. August 23, 1985. Correspondence Drawers, Plott Papers.
[351] Ashenfelter "was a very important figure in helping field experiments get a foothold" in economics journals, the AER, which he was still editing, in particular. John List Interview. LIST, J. A. & LUCKING-REILEY, D. 2000. Demand Reduction in Multiunit Auctions: Evidence from a Sportscard Field Experiment. *American*

When the final AER rejection of the Lynch at al paper came, Ashenfelter reflected on the whole refereeing process and defended his predecessor:

> "Both the external referee and I have come to the conclusion that your paper was treated fairly, especially in view of its receiving a second hearing by Clower, which I think it deserved. (I typically use two referees on a paper, while my predecessor rejected one-third of all papers without any refereeing.)"[352]

The Lynch et al paper was eventually published in 1991, not in a journal, but in the book series *Research in Experimental Economics*. It was launched by Smith in 1979, because he wanted an outlet which would allow unabridged accounts of experimental research to be published and guaranteed peer review that drew on referees with direct or indirect experience with the experimental method (Lynch et al., 1991). Strategy S1 was a natural response to reports that revealed what were to experimentalists superficial reviews. Otherwise, other strategies needed to be called on.

5.1.2.2 S2 Results are relevant for theory and have applications

The second strategy attempted to respond to the criticism that the experimental results in the context of the theory it purported to test are not informative or that the experimental research has no practical applications, by highlighting the close relationship between theory and experiment and showing how their interaction unfolds.

The use of S2 in the representative letter focuses on the practical application aspect of an earlier project by Plott and his Caltech colleague Michael Levin. This project was unique in the sense that it simultaneously explored theory (group decision making theory, in particular, agenda setting), its experimental testing, and also application of both theory and experiment on a specific real-world decision problem – how members of a flying club decide which airplanes to purchase (Plott and Levine, 1978, Levine and Plott, 1977). Although initially submitted to the AER as one paper, two publications came out that

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Economic Review, 90**,** 961-972, LIST, J. A. 2001. Do explicit warnings eliminate the hypothetical bias in elicitation procedures? : evidence from field auctions for sportscards. *The American Economic Review,* 91**,** 1498-1507, LIST, J. A. 2002. Preference reversals of a different kind : the "more is less" phenomenon. *The American Economic Review,* 92**,** 1636-1643.

[352] Underlined word in the original. Letter from Orley Ashenfelter to Charles Plott, May 23, 1986. Correspondence Drawers, Plott Papers. Robert Clower's relation to experimental economics is discussed again in section 5.1.2.8 on strategy S9.

separated the applied part from the rest. One of them appeared in a law journal, the other in the AER. As this was the very first example of experimental research with an applied component, it was natural that Plott should voice his unhappiness with Clower's decision to split the paper because it meant that "the continuity between theory and application was lost."

A more clear-cut statement of S2 provides the following extract from the refereeing process of the Plott Sunder paper on information aggregation in experimental asset markets (Plott and Sunder, 1988). This paper had a long journey to its final venue, Econometrica. It was first quickly rejected by the AER (1983) with the advice to submit it to a more specialized journal. Then it was rejected by the Journal of Political Economy (1984). In Econometrica it had one revise and resubmit (1985) and another revision (1987). The following extract is from a letter by Plott after receiving a rejection from the AER where the submission spent no more than five weeks:

> "Let me turn the analysis to the report. The report itself has two problems. After having looked at the data the referee thinks that the results are 'unsurprising.' I have given this paper to several audiences of major theorists (e.g., Stanford, Minnesota, Harvard) and I always demand a prediction before they see the data. Never has anyone been able to predict the results beforehand. The reason is simple. All of our models predict that all three series should produce identical results. Accordingly, theorists typically think that the model will either work in all series or in none of them. Reactions like this referee's are not unusual. Having seen the data any theorist will have an intuition about what is going on. The problem is that the resulting '*Monday quarterback*' then evaluates the experiment in terms of his/her own unarticulated theory. In this case the referee thinks that the two settings (series B and C) in which information aggregation occurred were characterized by 'better channels of information transmission.' Now this referee seems to be a competent sort and I'll bet that had one of his/her students put a meaningless concept like that in a term paper the student would have failed. Had the results gone the opposite way, the referee probably would have said that that the complete markets in which three markets were simultaneously operative, caused 'information overload.' The theory the referee is willing to accept as the 'obvious' explanation of these data does not meet the standards of rigor that the profession demands. Furthermore even in its squishy form the referee's theory is

> probably rejected by the data in the paper itself because in series A and C the 'channels of information' were identical but the results were opposite."[353]

A 'Monday quarterback' is a colloquial expression which in this context describes hindsight or ex-post explanations provided by referees after they saw what happened in experiments and how the experimental data confronted the tested theory. The goal of such explanations was to render the experiments unreliable and thus irrelevant to the theory that they attempted to test. While in this extract the shifting of the burden of proof is not made explicit, it will become apparent once strategy S7 is examined. To get there, however, it is necessary to analyze the following two strategies.

5.1.2.3 S3 Experiments present real situations & S4 Theory applies to them as well

Let me first state both strategies and their implications in general terms before and illustrating them. It will become clear why the third and fourth strategy rarely, if ever, appeared alone.[354] What the experimentalists often encountered was the claim that because experimental environments and the data obtained in them (E) are for various reasons different from naturally occurring situations (W), although both E and W are related to a particular theory or set of theories or models (T), therefore E has no relevance for T. While S3 focused on the antecedent of this implication, S4 focused on its consequent. Both strategies were necessary. Applying only S3, i.e. focusing only on the antecedent of the underlying criticism is not enough to invalidate the implication, the so-called fallacy of the inverse.

Strategy S3 addressed the alleged difference between E and W. To understand S3, it is important to note how the criticism it was addressing exploited the relationship between W and T at E's expense. W is chosen on the basis of what is believed the abstract T applies to in the real world or of which real world situations have motivated the development of T. And the critics use various features of W to highlight its differences from E so as to compromise its epistemic applicability to T. First, the undergraduate

[353] Letter from Charles Plott to John Riley, Associate Editor of the AER, February 17, 1984. Folder "SSWP 463 Plott Sunder 9-88 Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets." Plott Papers.

[354] See for instance Plott's defense of laboratory market procedures along the lines of S3 and S4 in his paper Rational Choice in Experimental Markets HOGARTH, R. M. R. M. W. Rational choice: the contrast between economics and psychology. 1987 1987 Chicago. University of Chicago Press.pp. 118-121

students as subjects of the experiments did not match in skills and experience of the population that would be involved when W is used to assess T (companies, stock brokers, etc.). Second and similar to the previous point, the stakes or rewards received by the subjects in E are too low compared to W. Third, E represents artificial situations that do not naturally occur with, for instance, subjects engaging in tasks that either have no counterpart in W or are highly contrived. Fourth, subjects in E are aware of the artificiality and try to second-guess what is being expected from them; in today's language what are called the experimenter's demand effects. These points are well illustrated by this extract from a referee report that Plott received on his public goods experiment with McCue and Isaac. After a rejection by the AER, the paper moved to the Journal of Public Economics, where it was eventually published. After its first rejection there, Plott sent a lengthy reply that was forwarded to the referee who anonymously responded:

> "There is only one point I would like to respond to in his letter (p. 4), namely that their data "'were generated from <u>real public goods problems</u> in the sense that real people faced real incentives and had a real, common interest in the level of the public good' and that my alleged failure to realize this reflects 'a deep misunderstanding of the use of experimental methods', etc.
>
> What Plott does not accept here [in his experiment] is that (a) a completely artificial construct which meets the formal definition of a real public good, (b) a set of real students of economics and (c) a set of real incentives that make these real people realize that they are guinea-pigs with some duty to perform, that all this may not be enough to convince critical readers that an experimental situation has been created from which there is <u>something</u> to learn.
>
> …
>
> The public goods in the experiments are completely artificial. Thus, for example, the costs for 'producing' the good are arbitrarily chosen and without any real meaning. What does experiments [sic] with such goods tell us about the real issues? The authors implicitly [sic] assume that their results are valid also for the latter case."[355]

[355] Underlined words in the original. Referee report from September 29, 1983. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.

The referee's points (b) and (c) can be addressed by further experiments that can check how far they are valid (using different subject pools, paying more, controlling for demand effects, etc.). The first point - that experiments are artificial or not 'real' - cannot be resolved on purely epistemic grounds and this is how S3 is connected with S4. S3 stated that while E is not a naturally occurring phenomenon, it is still real and "governed by the same principles [T] that are supposed to govern <u>all</u> markets." Plott goes to great lengths in the representative letter to explain why the experiments with price controls are real, e.g. why paying subjects based on their performance creates real incentives and hence produces decisions that are real.

Strategy S4 argued that E is not relevant for T alone but also, vice versa, that T is relevant for E. This is because T does not distinguish between E and W, since T claims to apply to both. Furthermore, it was argued by the experimentalists that E rather than W is epistemically privileged in regard to T, because experimental control and intervention allows a better implementation of T's assumptions and therefore E allows better testing even though it is not T only that is being tested in E (i.e. ancillary assumptions). In the words of Plott, E gives T its 'best-shot' at being confirmed. This is the meaning of *simple cases* or simple situations that Plott has in mind when he claims that theory applies to simple cases.[356] If T is not confirmed, E allows us to ask why that happened through the mechanism of the experimental engine.

The representative letter does not state the best-shot argument. To see it in action, let's use again the McCue Isaac Plott paper (Isaac et al., 1985). When it was first rejected by the AER, Plott replied to the editors Ashenfelter and Riley:

> "These are not isolated examples of problems with 'theory testing' methodologies. Certainly we need not convince you that the models we have in economics are remarkably incomplete. By virtue of their incompleteness they can be easily rejected. Model rejection (disconfirmation) is really trivial and if that is all that is accomplished by a paper most experimentalists will reject it. Given that fact, a good approach is to first attempt to reject a model under its most favorable circumstances. Of course if the model is not rejected, then the research is open to criticisms like yours that 'the experiments were designed to confirm … someone's

[356] For a discussion of "simple cases": see also PLOTT, C. R. 2014. Public Choice and the Development of Modern Laboratory Experimental Methods in Economics and Political Science. *Social Science Working Paper, California Institute of Technology,* 1383.

> theory.' If the model is disconfirmed under anything but· the most favorable circumstances, then theorists with a vested interest will claim that we got the experimental conditions wrong. You can see the catch 22 that must be avoided. Sometimes it is avoided as we did by using other methodologies."[357]

Through the strategies S3 and S4, experimentalists attempted to convince the editors, referees and their readers about the proper relationship between W, E, and T. Clearly an ontological argument lies behind these considerations, that channels to one of the tenets of the experimental turn – placing experimental data on a par with theory. On the one hand, critics raised the argument that E has nothing to say about W because of E's artificiality (subject pool, etc.). On the other hand, experimentalists argued that the way to relate E and W is through T. That is, the mutual dialogue or virtuous circle between E & T allows a better understanding and development of T which may be applied to W. This line of argument was used in the 1990s and later to counter the external validity criticism.

Strategies S3 and S4 explained why experimental situations are "real" from the perspective of investigated theory, albeit "simple" in regard to the naturally occurring situation that the theory purports to explain. Simplicity and being real, experimentalists argued, were actually advantageous for economics research as a whole. The following strategy S5 is thus a natural consequence of S3 and S4.

5.1.2.4 S5 It's Basic Research

The fifth strategy capitalizes on the previous two strategies in that it uses them to assert that experimental research which investigates economic theory presents basic research in economics. The term basic research' is not defined, but Plott used it in the standard sense in the context of economics, that is, research which deals with the fundamental issues of economic science without any specific applications in mind. Obviously, not all experiments contribute to basic research. However the very idea that some experimental research could or should be viewed as basic was novel and provocative. The application of the two previous strategies was meant to show that experimental tests of economic theory allow it to be given a best-shot because theory as it is stated applies to laboratory

[357] Letter from Charles Plott to John Riley and Robert Clower, September 18, 1984. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.

environments and, thanks to the ability to control and intervene, experimental environments allow a far better implementation of a theory's assumptions than a naturally occurring situation can. Since post-war economics evolved into a discipline of general theories and specific models, the argument of strategy S5 was that the natural counterpart of these things are experiments. Given that economic theory is considered to be the basic research in economics, then the related experiments must be so as well. As with other strategies, S5 is a restatement of two driving forces - symmetry and the virtuous circle - of the experimental turn.

To some referees this position felt pretentious. Take for instance the Isaac-McCue-Plott paper on public goods introduced in the previous subsection. After Plott criticized the negative review they had received, one of the referees retorted:

> "I get the impression (from their response to me and from the way they present their experiment) that the *authors feel they are conducting some kind of 'basic' research in Experimental Economics*. Not only do I find this position overly pretentious, but I also think - which is more important -- that their experimental design is not relevant for the issue they want to deal with."[358]

In this case it was the referee who sensed that Plott and his co-authors believed that they were conducting basic research. Plott's reply applied S5.

Sometimes S5 attempted to augment the legitimacy of experiments in economics by associating them with the standards and practices of other successful experimental sciences. This can be seen in a letter from Plott to Andrei Shleifer, then the editor of Quarterly Journal of Economics, regarding experiments with multiple unit auctions:

> "Experimental methods deal with behavior in very simple and special cases. That is a characteristic of experiments in all branches of science, and especially basic science."[359]

This paper was eventually published by the Journal of Economic Behavior and Organization (Jamison and Plott, 1997).

[358] My Emphasis. Referee Report from 29 September 1983. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.
[359] Letter from Charles Plott to Andrei Shleifer from April 14, 1995. Folder "Plott Correspondence 1995 Jan-Jun." Plott Papers.

5.1.2.5 S6 More experiments are needed

Whenever experimentalists faced the criticism that particular results were uninteresting, counterintuitive, or flawed because of the selection of parameters or the way in which they implemented theory, they tried to turn the tables on their critics. That was done in two closely related ways. The first was to ask for further experiments that would explore the alleged flaws of the submitted paper. Strategy S6 is for instance concisely stated in Plott's representative letter. If the flaw was a failure to control for a particular effect or variable, this could be investigated in new experiments. If the flaw was on the level of the interpretation of experimental data in light of the tested theory, the other strategy, called shifting the burden of proof to the critical referee, was applied. This discussion is relegated to the following subsection as S7, which uses an example based on the Isaac McCue Plott paper on public goods provision. The example shows how S6 and S7 work in tandem. In the rest of this subsection I want to focus on a different aspect of these strategies.

In the mid-1990s, Plott was trying to get published a paper which he had written together with one of his graduate students Kay-Yut Chen. It dealt with non-linear bidding in first-price auctions and was submitted in 1994 to *Games and Economic Behavior* which at the time was edited by the game theorists Ehud Kalai and John Ledyard. After two referee reports arrived, the editors suggested 'revise and resubmit'. The resubmission contained a letter by Plott in which he addresses the two referees.

> "Any conclusions, such as those we listed, must be understood as being acceptable only in the presence of a long list of maintained hypotheses implicit in the experimental procedures and the models themselves. The models are, almost by definition, always "underidentified" with respect to this list of maintained hypotheses. A statement of the form "you did not control for X" is not especially informative or powerful as a criticism of some particular paper, and is really only a *call for additional experimental papers* structured to determine if X is important in explaining what has been reported. Indeed, it is the beliefs that motivate such statements that are the *engines that take the science forward, paper by paper*."[360]

[360] Letter from Charles Plott to Ehud Kalai and John Ledyard from September 15, 1995. Folder "Plott Corr 1995 July-Dec." Plott Papers. The paper was eventually accepted and appeared in 1998. CHEN, K.-Y. & PLOTT, C. 1998. Nonlinear Behavior in Sealed Bid First Price Auctions. *Games and Economic Behavior,* 25**,** 34.

This quotation neatly illustrates how the request for further experiments along with the shifting of the burden of proof strategy is naturally connected to the notion of the experimental engine and continuous style of experimentation introduced in the previous two chapters. Both are different sides of the same coin; they serve to increase the trust in experimental observations. Strategy S6 arose from the belief that experimental research as an empirical activity often generates not only theoretical, but also new empirical questions related to the robustness of the existing experimental results, in the sense that it asks how far these results may be artifacts of the experimental procedures, the chosen parameters, and any details that the experimenters had to add to theory in order to make it operationalizable in the experimental setting. Any doubts about robustness could thus be resolved on empirical, i.e. experimental grounds, not other considerations. If referees raised concerns that did not fit inside such an empirical/experimental framework, the experimentalists would reject such criticism and demand that it be sufficiently specified to be subjected to new experimental investigation.

5.1.2.6 S7 Shifting of the Burden of Proof

The seventh strategy, as can already be seen in the representative letter, is an alternative and extension of the request for more experiments since it attempts to turn criticisms of the experimental method or experimental data into requests for input from the referee that could be used in developing theory or models related to the experiments in hand. In general, if a referee suggested that the experiment and its data are flawed and that some existing evidence or intuition should be considered valid, despite the negative evidence from the experiment, this strategy implied that the referee should provide a reason why the experiment should be seen as flawed and where the referee's preferred model broke down, to allow the experimenter to test the referee's allegations. Hence the label of this strategy – shifting the burden of proof. This strategy is a direct consequence of the symmetric standing of theory and experimental data as viewed by experimentalists. Therefore any objections raised by referees that results do not meet intuition were either rejected as unfounded because the experimenters trusted their experimental observation or turned into specifying these objections and turning them into testable propositions.

Another example of S7 can be found in the Isaac-McCue-Plott paper on public goods. Here is an extract from the letter that Plott sent after the first rejection by the Journal of Public Economics:

> [Referee] "objects to our choice of control variables and parameters.
>
> p. 1(10) ten students;
>
> p. 1(10) mainly economics students;
>
> p. 1(12) the cost of the public good was at the wrong level;
>
> p. 2(10) the subjects might have been confused in all experiments;
>
> p. 2(15) the instructions should be changed to make more explicit the fact that it was possible to lose money;
>
> After one sees the data, it is easy to generate theories that might be responsible for the results and to simultaneously suggest alternative experiments that would test those theories. All experiments in all sciences are open to such criticisms. In fact, one of the major features of the experimental method is that those who suspect that a result hangs on some particular variable or procedure are free to conduct the experiment to see if the hunch is correct. … In our case, however, the referee has provided no theory, no alternative data, and no uniformities in our data that lend support to the hypothesis that changes in any of the above parameters would alter any of our conclusions in an interesting way. In fact both existing data and theory suggest that none of the above variables will influence our conclusions."[361]

After Plott appealed against the first rejection, one of the editors of the Journal of Public Economics, N.H. Stern, rejected the revised paper, because one of the original referees was still not satisfied, though the other was somewhat sympathetic. We have encountered this particular referee in the discussion of S5, where s/he complained about the pretentiousness of experimentalists' claiming that they were performing "basic research." Then the referee went on to address Plott's attempt to shift the burden of proof:

> "Charles Plott responds by putting several serious points of criticism … and proclaims that 'the referee has provided no theory, no alternative data, and no uniformities in our data that lend support to the hypothesis that changes in any of

[361] Letter from Charles Plott to N.N. Stern from March 5, 1984. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.

> the above parameters would alter any of our conclusions in an interesting way'. My reply would be: What about some rudimentary common sense?"[362]

A paragraph later the referee added a claim that experimental subjects are aware of being "guinea-pigs with some duty to perform." This was already quoted in the section where strategies S3 and S4 were examined. Plott replied to these charges 6 months later. He stated that they were very "dissatisfied by the treatment of our paper" and went on to dispute all charges by the referees. The one about shifting the burden of proof summarized his position:

> "Our experiment conforms with the standard and widely-accepted standards of experimental economics. The degree of generality of such experiments is by now well-accepted (see ....). Why it is in those cases and not in other cases is for the referee to explain. … The referee claims that the experimenters contaminated the experimental environment by suggesting to the subjects that they had some 'duty to perform.' What aspect of our procedures caused this? Why did we get the same results with different experimenters? Why didn't the subjects feel a 'duty' to 'cooperate' rather than 'defect'? Why didn't the group go to the Lindahl equilibrium as an economist might have hoped? Why does the group behavior change when the treatment variables change? Why does behavior change with time and replication if subjects are only performing a duty? Does the referee claim that this contamination is a property characteristic of all research in which there is a direct interaction between the researcher and the economic agents such as field studies, questionnaires, or field experiments? Is the defect a property of all laboratory economic studies? Or, was it a defect of only this particular study? The referee answers none of these questions. The charge itself, when made as it is without explicit documentation, is unfounded."[363]

In this quotation we can again see how Plott probes the charge against the epistemic value of experimental data and thereby renders it empirically hollow. The paper was eventually accepted for publication by the Journal of Public Economics. Plott was able to

[362] Referee report from September 29, 1983. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.

[363] Letter from Charles Plott to N.N. Stern from March 5, 1984. Folder "SSWP 428 Isaac McCue Plott 1985 Public Goods Provision in an Experimental Environment." Plott papers.

convince the editor and referees that the previous objections were either irrelevant or that he had been able to address them in a revised version of the paper.

5.1.2.7 S8 Specialized journal

Often experimentalists received a report where the referees or editors suggested that the paper should be published in a specialized experimental journal. Experimentalists replied with a mixture of strategies – S2 and S5 in particular – but also with a claim about the general appeal of experimental research. What follows is an extract from a reply by Mark Isaac and James Walker to a referee report from the Journal of Political Economy that suggested that they should submit their paper to a specialized journal for experimental economics, a journal which did not exist at the time.

> "Can the referee provide us with a reference to a journal of experimental economics? We think not, because those in the forefront of developing experimental methods (especially Plott and Smith) have long insisted that experimentation is a methodology of economics and its results should be of interest to all economists. Even the three volumes of Smith's monograph series follow this rule in that the articles are first and foremost economics articles utilizing a variety of techniques: laboratory experiments, theory, econometrics and field experiments."[364]

In the next section when I discuss strategy S9 there is another example of such a suggestion made by an editor in 1983, namely, Robert Clower, then the editor of the AER. The cited monograph series was called Research in Experimental Economics. Its first four volumes appeared in 1979, 1982, 1985, and 1991. The irregularity of the publication highlighted the point that the series was not a journal. The Isaac-Walker paper was eventually published in Public Choice and presents one of the early experiments on free riding in a public goods setting (Isaac et al., 1984).

From the late 1970s experimentalists regularly discussed the issue of creating a journal dedicated to experimental economics. The consensus not to pursue such a journal lasted until the mid-1990s (see Section 4.4.1 above for details). Possibly the most convincing argument that experimentalists developed against the editors' and referees'

[364] Undated letter from Mark Isaac, early 1980s. Correspondence Series. Plott Papers.

recommendation to target a specialized journal had the form that general journals such as the JPE, QJE or AER should published the best articles in specialized fields. This can be illustrated from Plott's letter to the editor of JPE in regard to the Plott Sunder Rational Expectations paper (Plott and Sunder, 1988):

> "Are the results only of interest to specialists and will not be of interest to a broad set of readers and therefore should not be published in the JPE? This is a wrong criterion. *Everything in economics is specialized. The whole field of economics is a collection of specialists.* In such a research environment the function of a leading journal is not only one of publishing nonspecialized work, for to do so would transform the JPE into the Journal of Economic Literature. The function of the JPE is to *publish the best work in specialized fields*. In our opinion the Plott and Sunder is one of the two best papers utilizing laboratory techniques that have circulated in economics in recent years."[365]

Even more specialized journals were skeptical early on of experimental research. Only once Reinhard Selten became the editor of the International Journal of Game Theory in 1984 did the situation described in the following letter change.

> "As I suggested to you in my original letter, a majority of the IJGT editorial board favors no papers on experiments and almost all of this board would exclude all but those without really major new insight. So the IJGT should not be viewed anymore (despite past quality) as a place to publish experimental work, especially if it is only partial or preliminary in nature (e.g., one in a series of related papers), or relates to theoretical developments that are very 'ongoing' and are highly likely to either 'die' or finally appear in a much altered form."[366]

Selten was the editor of IJGT from 1984 until 1988 and edited also the section on "Games and experiments" from 1992 to 1995. This section was taken over by Ron Harstad, who led it until 2000.

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[365] Letter from Charles Plott to Jose A. Scheinkman from October 5, 1983. Folder "SSWP 463 Plott Sunder 9-88 Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets." Plott Papers.

[366] Letter from William F. Lucas to Charles Plott from December 24, 1980. Folder "SSWP 280 Plott, Rogerson 9-79 Committee Decisions under Majority Rule An Experimental Study." Plott Papers.
William F. Lucas (1933-2010) was an influential early game theorist who before joining Cornell worked for the RAND Corporation. He was the editor of IJGT from 1980 to 1983.

5.1.2.8 S9 A Method or a Field?

The dual meaning of experimental economics – both a novel research method and a field within economics - and the tension between these meanings have been important in developing the identity of experimental economists. This was also reflected in the debates with editors who primarily viewed experimental economics as a new field or method within economics and wanted to treat it as such. While examples of this tension abound well into the 2000s, the following example sheds more light on the AER in the first half of the 1980s. The paper being reviewed was written by Shyam Sunder and Charles Plott and entitled *Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets* (Plott Sunder paper). This title aptly sums up what the paper was about. It took almost five years to get it published. First it was submitted to the AER in January 1983 and rejected after a referee report in February 1983. Clower, then AER's editor, suggested that it should be published in a "Journal of Experimental Economics," as there was not "enough in the way of new results in this experiment to justify publication in the Review." Furthermore, Clower opined that:

> "On a more general note, I have leaned over (and I think Abramowitz has also) to give people working in the experimental area a platform. We did not concert our efforts, but one way or another both you and Vernon Smith were singing out loud and clear in the December issues of the AER and the JEL. I think you should now look elsewhere for an outlet for most of your writings."[367]

Plott's reply was penned very quickly:

> "Your letter suggests that I should make one further point. *Experimentation is a method; it is not a subject*. If those that supply the methods have anything to say at all, it is to people other than themselves. As the methods evolve in the future there might be a place for journals that specialize in examination of purely

[367] Letter from Robert Clower to Charles Plott, February 14, 1983. Folder "SSWP 463 Plott Sunder 9-88 Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets." Plott Papers. In fact the idea to get the paper published in a specialized journal was also implied in the referee report which claimed that:

> "These results strike me as interesting but unsurprising; they may surprise some who have taken extreme positions on 'Rational Expectations.' The research technique seems careful and contains a few novelties. Hence I believe that this work does deserve some outlet."

methodological issues. Right now no such journal exists (in spite of your claim to the contrary) and furthermore one should not exist."[368]

Clower's reply to Plott came on August 14, 1984, in which he reiterated his opposition to the publication of the Plott Sunder paper in the AER and claimed that it would take a few years for experimental methods to evolve to warrant another methodological paper such as the seminal one by Smith that Clower commissioned (Smith, 1982).

> "My own feeling is that there is nothing significant left to be said about the experimental method, per se. Perhaps in three or four years some additional methodological pieces will be in order; but those would surely be more than suitable for the leading general-purpose journals. Let me leave it at that. I don't think the present paper is going to make it at the AER, but if you want me to go on with further reviews I will be glad to obtain them."[369]

After the rejection of the Plott Sunder paper by the AER it was submitted to the JPE where Scheinkman finally rejected it in November 1984. Then it moved to Econometrica where after two rounds of revisions it was accepted by David Kreps, as we will see in the next subsection (Plott and Sunder, 1988).

The Lynch et al paper was introduced when discussing S1, the strategy that asked for qualified referees once the first reports came, suggesting careless reviews by some referees. Once Orley Ashenfelter had replaced Clower in 1985 as the AER editor, the former reflected on the policy of his predecessor in regard to the Lynch et al paper. The following extract is relevant for the discussion of S9:

> "I want you to know that I also do not agree with some of the early correspondence by Robert Clower to you in which he says "I have leaned over… to give people working in the experimental area a platform." It is precisely the idea that space should be "allocated to groups" and that we should "publish to stimulate discussion" that I do not think is appropriate. I believe we will be publishing good scientific research using the experimental methods in the next

[368] My emphasis. Letter from Charles Plott to Robert Clower, February 29, 1984. Folder "SSWP 463 Plott Sunder 9-88 Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets." Plott Papers.

[369] Letter from Robert Clower to Charles Plott, August 14, 1984. Folder "SSWP 463 Plott Sunder 9-88 Rational Expectation and the Aggregation of Diverse Information in Laboratory Securities Markets." Plott Papers.

> few years but we will be publishing it because it is good, substantive research in economics and not because experimentalists 'need a platform'."[370]

The confusion about the status of experimental economics as both a method and a field is still potent. Take for instance this extract from Plott's letter to Andrei Shleifer, then the QJE editor, regarding experiments on multiple unit auctions. The Journal of Economic Behavior and Organization eventually published this paper. (Jamison and Plott, 1997):

> "Experimental economics is a method, as opposed to a specialized field, that provides economics with types of data that never before existed. It permits economists to ask questions that could never before be posed because the data did not exist, including an enlarged role for questions of a basic scientific nature. Consequently, in the case of experimental papers, the first question can only be answered by someone who is knowledgeable about what is known, and the special techniques that have been developed for learning it. In other words, the inputs of experimentalists are necessary."[371]

Although Plott here denies the status of experimental economics as a field, he does it to emphasize that experimental data, when properly generated, have something to say to theory, and experiments form a valid research method.

5.1.3 Space Limitations

The Plott Sunder paper that we encountered in discussing strategy S9 also precisely exemplifies the issue of the length of experimental papers, which were in part bloated by the need to describe the experimental instructions, experimental design, methodology and theoretical background of the experiment. The paper when sent to the third journal was left intentionally long because it contained all the necessary information to replicate the research, in particular because it was the first experimental study to investigate information aggregation "when different traders have diverse information about an underlying state of nature" (Plott and Sunder, 1988, p. 1085). A similar case was the earlier submission of the Forsythe Palfrey Plott paper on the rational expectation and asset valuation in experimental markets which demonstrated that markets can disseminate information efficiently (Forsythe et al., 1982).

[370] Letter from Orley Ashenfelter to Charles Plott, May 23, 1986. Correspondence Drawers, Plott Papers

[371] Letter from Charles Plott to Andrei Shleifer, April 14, 1995. Folder "Plott Correspondence 1995 Jan-Jun." Plott Papers.

The Forsythe Palfrey Plott paper was submitted first to the Journal of Economic Theory (JET) in 1980. The JET editors were worried that in publishing experimental work with all the essentials they would be opening the journal to the possibility of endless notes and comments on some of these specialized details. [372] Then the paper moved to Econometrica where it was published after two rounds of revisions.

The answers to these demands to shorten papers rested on the explanations why instructions to the experimental subjects were needed. For instance, when Robert Forsythe was asked to shorten another paper by Forsythe, Palfrey, and Plott (Forsythe et al., 1984), he replied that omitting the instructions:

> "would lead to an incomplete presentation of the analysis. Further, including them greatly enhances the opportunity for their replication by other researchers in the field. It would be convenient if we could simply reference another source but these particular experiments are extremely complex relative to others presented in the literature and thus are unlike those reported elsewhere."[373]

Journals only gradually adopted standards for the publication of experimental papers. In 1985, the Journal of Economic Literature introduced the category of "*experimental economics methods*" in its JEL classification scheme (1985). Econometrica was the first journal to provide guidelines for this in 1991, in no small part because Tom Palfrey was then an Associate Editor (Palfrey and Porter, 1991). These guidelines included the requirement to include "an appendix which adequately explains the details of the experimental procedures" such as sample subject instructions. The body of each submitted paper should contain a section that would explain experimental procedures, such as:

> "1. The subject pool and any special recruiting procedures. 2. The experimental technology (e.g., manual or computer, or which computer net-work). 3. Any procedures to test for comprehension before running the experiment. 4. Matching procedures (particularly in game theory experiments). 5. Subject payments (use of artificial currency, average earnings, lotteries, grades, etc.). 6. Number of subjects used in a session. 7. Any use of experienced subjects. 8. Any use of practice trials. 9.

[372] Folder "1982 Forsythe Palfrey Plott 304," Plott Papers.

[373] Letter from Robert Forsythe to Martin J. Gruber, editor of the Journal of Finance, October 25, 1983, Folder "1984 Forsythe Palfrey Plott --84 Futures Market and Informational Efficiency A Laboratory Examination" Plott Papers.

> Timing (how long a typical experimental session lasted, and how much of that time was instructional). 10. Where and when the experiments were conducted. 11. Any use of intentional deception, or presence of instructional inaccuracies" (Palfrey and Porter, 1991, p. 1197).

The guidelines distinguished between what the submission should contain and what would eventually appear if the paper were accepted. The submission evaluated by its referees was supposed to be more extensive. In particular "sufficiently detailed" experimental data that would allow the "computation of the statistics, figures and tables reported in the paper" to be included (Palfrey and Porter, 1991, p. 1198). The published version sought to save space – for instance if the same instructions were published somewhere else. In addition, authors were always asked to make available to others on request anything that had been included in a submission that was accepted for publication. The final editorial decisions were made on a case-by-case basis.

5.2 Lowering the Barriers to Entry

This section explores the book publications about experimental economics research from the perspective of the interaction between the experimentalists and the rest of the profession. These books served a variety of purposes. They collected, synthesized, and explained in an accessible way the body of knowledge established by experimental research. They targeted not only existing experimentalists, but were directed to an audience of economists with little or no experience with experimental research and also for courses aiming to introduce the experimental methods to a new generation of economics students. Al Roth aptly described this as ways of lowering the barriers to the entry of experimental economics.[374] In no small part these publications, once they appeared, fostered the growth of the experimental community and its reputation among the rest of the profession. Experimentalists again, through these publications, consciously expended time and resources on improving their standing within the profession and its perception of them.

[374] Witness Seminar and also Alvin Roth Interview.

From the early 1980s editors of various publishing houses, aware of the growth of experimental economics and sensing an unoccupied niche and commercial success, were insistently contacting experimental economists with queries whether someone intended to write a book on a certain subject or to start a specialized journal.[375] The most persistent editors were Jack Repcheck at Princeton University Press (PUP), Zachary Rolnik at Kluwer, and Colin Day, first at the New York office of Cambridge University Press (CUP) and later at Michigan University Press. Whereas in the late 1960s Vernon Smith and James Friedman could not find a publisher for collected papers on this topic, editors in the 1980s, ironically, could not find an experimental economist who would write a book covering anything in the emerging field.[376] The experimental economists whom they approached were too preoccupied with their research and the few who entertained the thought of authorship lacked the stamina and time to carry out such an undertaking.[377]

For instance, Colin Day contacted Charles Plott in 1980 about writing "a book distilling the experience so far gained in experimentation [that] might be widely welcomed amongst academics contemplating research of this type."[378] Plott replied that he had a manuscript based on his class on experimental methods "which covers both experimental markets and experimental political processes" – one that he continued to work on for the rest of the decade. Although he never completed it, it circulated among experimentalists.[379]

By the late 1980s experimentalists themselves generally acknowledged the need for a comprehensive treatment of experimental economics. Jack Repcheck from Princeton University Press informed Elizabeth Hoffman:

> "[C]onversations with Vernon Smith, Charles Holt, and Andy Schotter lead me to believe that there would be strong demand for a book that attempted to synthesize the results of thirty years of work in this field. A book that spelled out

[375] It is important to note that editors of publishing houses are not included in the various audiences that the experimentalists dealt with in the process of passive reception. First, editors are not part of the academic economists population. Second, unlike editors of economics journals, they do not have any gate-keeping role over content. True, in this section or in my discussion of the inception of the journal Experimental Economics, editors are depicted as active agents promoting experimental economics, but they have always done so through the lens of the balance sheet.
[376] Evidence suggesting that editors viewed experimental economics rather as a field than a method is provided later in this chapter.
[377] Plott was contacted by Colin Day; Hoffman by Repcheck and Tryneski (Chicago University Press); Kagel & Roth by Repcheck, Day & Rolnik and other publishers (e.g. Elsevier, Basil Blackwell); Holt by Repcheck and Rolnik
[378] Letter from Colin Day to Plott, February 27, 1980. Plott Papers. In addition note that it was Colin Day who contacted Vernon Smith in 1985 regarding starting an experimental economics journal.
[379] Various drafts are available in Plott Papers.

the foundations, assumptions, and the accepted body of knowledge, and then presented the implications of these accepted findings for the rest of economics would fill a void in the literature."[380]

A few months later Repcheck wrote to Al Roth summarizing his recent discussions with Hal Varian, then a coeditor of the AER, and Orley Ashenfelter, then its editor. [381] Both told Repcheck "with the same sense of priority" that the first topic that came to mind "in need of book- length treatment" was experimental economics. "I find it significant," Repcheck concluded, "when the main editor and one of the chief subeditors of the discipline's flagship journal state the same concern."[382]

Two years later Repcheck finally succeeded in finding an author. When Charles Holt contacted him about a book manuscript surveying experimental economics co-written with Doug Davis,[383] Repcheck did not waste time. He visited Holt in Virginia the day immediately after their phone call and offered a contract that by far exceeded what Kluwer had previously offered.

By 1995 the acute book lacuna was closed. In rapid succession between 1991 and 1995 six books were published. The first one was a surveying monograph by the English experimental economist John Hey (Hey, 1991, Basil Blackwell). Two years later came the book by Holt and Davis discussed above. It had a much wider scope and a deeper analysis of topics in experimental economics (Davis and Holt, 1993, Princeton University Press). In 1994 Daniel Friedman and Shyam Sunder published a textbook-like introduction to experimental methods for non-experimental economists (Friedman and Sunder, 1994, Cambridge University Press). And finally, in 1995, there appeared the much-awaited *Handbook of Experimental Economics* edited by John Kagel and Al Roth. It contained eight chapters, each providing an authoritative account of a topic of intensive research in experimental economics written by leading figures in their own subfields (Kagel and Roth,

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[380] Letter from Jack Repcheck to Elizabeth Hoffman February 3, 1988. Hoffman Papers.
At the time Elizabeth Hoffman and Matthew Spitzer were considering the publication of two books – one entitled *Experimental Tests of the Coase Theorem*, a compilation of their research on the Coase theorem, and the other *An Introduction to Experimental Economics*. Neither of these projects materialized.

[381] Hal Varian served as a co-editor of the AER from 1987 until 1990. He also served on the Editorial Board of the *Journal of Economic Perspectives* between 1986 and 1996.The AER introduced co-editors to assist the editor in the mid-1980s.

[382] Letter from Jack Repcheck to Alvin Roth November 16, 1988. Box 18, Alvin Roth Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[383] It was James Hamilton, the author of the well-known textbook on time series econometrics who suggested to his colleague Holt that Princeton University Press be contacted. HAMILTON, J. D. 1993. *Time series analysis,* Princeton, NJ, Princeton University Press.

1995, Princeton University Press). In addition, four volumes of collected papers were published. The first covered the work of Vernon Smith until about 1990 (Smith, 1991, Cambridge University Press) and the second covered the joint experimental research of Raymond Battalio, Leonard Green, and John Kagel on individual choice experiments with animals (Kagel et al., 1995, Cambridge University Press). A more varied, albeit less significant, coverage of experimental economics articles included a volume edited by Smith (Smith, 1990, Elgar Publishing); and there were two volumes of *Recent Developments in Experimental Economics* (Hey and Loomes, 1993a, Hey and Loomes, 1993b, both Elgar Publishing).[384]

It was again Jack Repcheck's persistence that paved the way for the most influential of these books, the *Handbook of Experimental Economics*. The idea to have a handbook instead of a collection of papers came from Hal Varian. Once Repcheck had told him about Kagel and Roth's plans to edit a collection of papers on experimental economics, Varian "was glad, but somewhat disappointed because such a book (in his opinion) would only go halfway toward providing the information that the discipline needs." Varian had in mind the well-known handbook series covering various fields of economics published by North Holland: "a collection of original essays that seek to make clear to the entire discipline the relevance of a field," experimental economics in this case.[385]

With funding from Roth's Mellon Chair, Kagel and Roth organized a conference in 1990 that brought prospective chapter authors together. "We could offer a small inducement to people. The real inducement to people to write was you could shape where your sub field of experiments was going."[386]

It took almost five years to complete, but from the outset it was immediately recognized as a landmark publication, not least because 500 complimentary copies were sent at Roth and Kagel's choice to the most influential people in economics and because a price was negotiated with Princeton University Press that would be low enough for any graduate student to buy the book. At some point they were considering other publishers such as Elsevier, Michigan University Press, Kluwer, and Basil Blackwell; but none could offer

[384] In my overview I am not including edited volumes with original papers – such as PALFREY, T. R. 1991. *Laboratory research in political economy,* Ann Arbor, University of Michigan Press. or the much earlier ROTH, A. E. (ed.) 1987. *Laboratory experimentation in economics: six points of view,* Cambridge; New York: Cambridge University Press.

[385] Letter from Jack Repcheck to Alvin Roth November 16, 1988 Box 18, Alvin Roth Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[386] John Kagel Interview.

complimentary copies, pricing that would be accessible not only to libraries but to individuals, or comparable royalties.[387] With Princeton University Press they gained thus maximum visibility and reached their goal of lowering the barriers to entry. Elsevier [previously North Holland], the publisher of the established series of handbooks in various fields of economics, eventually printed the *Handbook of experimental economics results* edited by Plott and Smith[388] (Plott and Smith, 2008).

5.3 NSF and the Funding of Experimental Economics

The first section of this chapter has extensively demonstrated the difficulties that experimentalists faced in getting their research published in academic journals. However, to produce these papers they required funding to run experiments and in particular to pay their subjects. Publications also served as a proof for funding agencies, above all the *National Science Foundation*, to prove that the grants were bearing fruit; they allowed grants to be extended or awarded for new experimental research projects. Moreover, NSF funding served their recipients as a signaling tool. They could argue to their

[387] John Kagel, Alvin Roth Interviews and the Witness Seminar. A tentative marketing plan from 1990 suggested that "In order to make it clear that we have this expectation, we will make available 500 complimentary copies for those who demonstrate a desire to use the book in a course setting." Essentially the handbook followed the successful Princeton University Press policy that had operated with David Kreps' textbook *A course in microeconomic theory*. Nov 5 1990, Box 18, Alvin Roth Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
It is not clear how this plan changed and how the idea to distribute the 500 complimentary copies came about. Probably it was an offer from Repcheck who was worried that Roth and Kagel might defect to another publisher.

[388] The major driving force in this project was Plott, who had already envisioned it in early 1990s: "Authors should write very short reviews of very narrow topics. For example, 'industrial organization' is too broad, as is 'finance.' The topics for individual authors should be 'collusive behavior' or 'call market structure and behavior.'" Source: Letter to Commissioning Editor at Harwood Academic Publishers - July 20, 1993, Plott papers.
In 1996 potential authors, for instance Elizabeth Hoffman, were first approached with the following description of the state of the book literature: "The product will be quite distinct from Davis/Holt, Friedman/Sunder, and Kagel/Roth but also broadly supplement and complement what each of these books do. Davis/Holt is a comprehensive textbook and guide to experimentation. Friedman/Sunder is a Why we do it; How we do it; things to watch for; etc.- methodology from the point of view of practicing scientists. Kagel/Roth is a long in-depth treatment of certain key areas developing the interplay of different research streams who said what, when, why the controversies, the learning, how things are related. … Instead of long survey articles we invite very short papers that focus on specific experimental results or patterns of results. The short papers will rely heavily on tables, charts and graphs that are self-contained; i.e. they can be read and understood independently of the text. The reader is thus able to survey the essence of the results without reading the text." Source: Letter from Charles Plott and Vernon Smith, November 1996. Hoffman Papers. The long delay in publishing this handbook is partly accounted by the large number of authors involved.

colleagues and deans that their research was externally approved and funded. Thus getting research published had the counterpart of getting such research funded. But unlike other types of research in economics, there were the specific costs of experimental research, which had to be justified.

While in the first section, the process of rejecting rejections was steered by the experimenters themselves, through sending replies to editors and referees, in the case of funding experimental research, rejections of early experimental economics applications, in particular in the late 1970s and 1980s, were rejected by the administrators, most importantly Daniel Newlon at the NSF. Often when the NSF Economics Panel failed to recommend a particular experimental project for funding, Newlon rejected its opinion and funded such projects anyway. This section explains both how experimentalists benefited from the support of NSF and also how the NSF benefited from its support of experimental economics.

Interviewing any American experimentalist inevitably leads to the name Daniel Newlon. He was the director of the *National Science Foundation's* Economics Program, where in his own words "he … developed innovative ways of making grants in order to cushion the impact of federal budget cuts on the economics research community" and helped to fund two generations of experimental economists.[389] Before joining NSF in 1974, Newlon earned his Ph.D. from the University of Virginia in 1970 where he studied under Gordon Tullock.[390] First he served as Associate Director for the Economics Program and became its Director in 1980 and, except for a six month sabbatical at the World Bank in 1989, he managed the program until 2009.[391]

Many researchers before becoming experimentalists had had NSF funding. This was also the case of Charles Plott, who at the turn of the 1960s was pursuing research in social choice theory.[392] When he moved to Caltech in 1971 he became interested in the

[389] Daniel Newlon CV, shared with the author.

[390] Plott graduated from Virginia in 1965, Newlon in 1970. The University of Virginia, with James Buchanan and Gordon Tullock on the faculty, was a center of public choice research.

[391] After retiring from NSF, Newlon continued his involvement in the funding of economics. He became the Director of Government Relations at the American Economic Association and Board member of the Consortium of Social Science Associations (COSSA), an advocacy organization that encourages attention to and Federal funding for the social and behavioral sciences.

[392] Plott's first NSF grant, GS-36214, for a project entitled Political Economic Decision Processes with $59,800 awarded, covered the period 11/72-11/73. The two grants were: 1) 74-08685 - Experimental Examination of Group Decision Processes (with M. P. Fiorina) and $63,100 (Total: $88,100) for the period:

decision making of committees and after working with Mo Fiorina (see Chapter 2) he started to use some of this money for voting agenda experiments. After one such experiment that Plott conducted at UCLA, while he was paying his subjects in a classroom, James Blackman walked in. Blackman was the NSF officer in charge of the Economics Program and Newlon's predecessor.[393] He happened to be at UCLA talking to somebody else and planned to pay an unexpected visit to Plott at Caltech. But once he learnt from Plott's secretary that Plott was currently on the UCLA campus, he met Plott there. Blackman found Plott in a classroom after an experiment had just finished, dispensing money for something that the NSF had not agreed to pay for.

> "We sat down, and I explained to him in detail what I thought the advantages of laboratory experiments were, why this experiment bore on that. In fact, we spent much of the afternoon talking about what that meant and how you carried on the conversation with the other sciences. Apparently, Blackman was quite supportive after that."[394]

While the administrators of the NSF Economics program were supportive of experimental economics, assessment panels that evaluated the submitted proposals resisted. John Ledyard sat on one of these panels during 1978-1980 and he recalls that "there was sizeable resistance to experimental payments among most of the people I served with on the NSF panel, many of whose names have gone on to become very famous" including future Nobel Memorial Prize Laureates Michael Spence and James Heckman, but also the econometrician Arthur W. Goldberger and macroeconomist Robert J. Barro. The opponents, according to Ledyard, argued that:

> "This was budget money being spent on frivolous things as opposed to serious panel data gathering and the like. It looked like it was going to be costly and it was going to take away from other things. There was a lot of resistance and it was actually one of the things that made it harder to get them to approve

4/75-4/76 and 2) SOC-7814508 - A Laboratory Experimental Investigation of Institutional Influences on Political Economic Processes $95,083 for the period: 9/78-9/79

393 James Horton Blackman (1919-2003) worked for the National Science Foundation from 1967 until retiring in 1993 as acting deputy director of its Social, Behavioral and Economic Division.

394 Charles Plott at the Witness Seminar.

> experimental work. … Dan swam against that and ignored it. But it was intellectually a difficult fight."[395]

Three issues should be highlighted in the above quotation – the type of person who opposed the funding of experimental research; Newlon's decision to fund such research regardless; and the closely related issue of his and NSF's motivation for this support of experimental economics.

First, Ledyard's recollection of those who opposed funding experiments, including subject payments, overlaps with Newlon's memories that "the most skeptical were people who do regular empirical econometric research." Econometricians were used to working with panel data and had developed techniques for working with them. However, protecting their guild did not only motivate the opposition of econometricians to experimentalists' production of their own data in experiments. The post-war period establishing the distribution of labor to a large extent separated economic theorists from data gathering and empirical and econometric work and established a hierarchy with theory as the pinnacle of the profession. (Cherrier, 2014, Backhouse and Cherrier, 2014). But as panel data gathered by statistical bureaus was burdened with various measurement errors and deficiencies that were not controlled by the economists who used them, such data never became the equal partner of theory. Theorists, however, were more open to experimental data, as Newlon recalls:

> "The theorists tend to be more sympathetic to experimental research because experimental economics is rooted and motivated more as a way of testing theory than as a way of replicating actual real world situations."[396]

Second, Newlon's decision to "ignore" panel recommendation not to fund experimental projects is another example of rejecting rejections – the overarching theme in the passive reception of experimental economics. This was apparent most of all in the late 1970s and 1980s. One of the beneficiaries was Elizabeth Hoffman. She was hired by Northwestern University after she graduated from Caltech in 1979. With her former Caltech classmate Matt Spitzer, she embarked on an experimental research project to examine the Coase theorem. They received negative reports from the reviewing panel. Nevertheless, Newlon gave them $25,000 which they used to pay subjects in their experiments. This investment

[395] John Ledyard at the Witness Seminar.
[396] Daniel Newlon Interview.

paid off; a number of high profile articles led to the award of the first *Ronald H. Coase Prize* for excellence in the study of law and economics to Hoffman and Spitzer in 1986.

Finally, there were two related reasons why the NSF supported early experimental research. On the one hand, it was considered to be an investment for which the NSF could take credit, that might yield large returns and transform economics,. Recognizing trends and supporting emerging fields are the objectives of the program officers at the NSF. The funding decisions are not based simply on having “an adding machine there or a calculator” to add up the points awarded by a review panel. Newlon sees his role differently: “you can make judgment calls and make bets on emerging areas, and sometimes they emerge, and sometimes they don’t.”[397] Over the years, however, the process became more formalized, with less discretionary power for the NSF officers.

The other reason for supporting experimental research was the opportunity to use experimental economics as leverage within the NSF. Economics experiments could easily relate to experiments in other disciplines, the dominant natural sciences in particular, and capitalize on the association with the hallmark of scientific research. Garnering support within the NSF also allowed for the protection and even expansion of the economics program and its budget.[398] Again, as Newlon put it:

> “NSF is led primarily by physical and biological scientists, some of whom do not consider economics a science. By showing them that economists use the same laboratory methods that they are familiar with, we were able to strengthen support for continued NSF funding for economics. Also experimental economics provided compelling examples of useful counter-intuitive scientific results that non-economists could grasp. I

[397] Daniel Newlon Interview. Newlon continued: “So you do have policy decisions like the broadband communications spectrum, auction design generally, that motivate experimental research, but usually, at the end of the day, what the experimentalist is trying to do is to take a theoretical model that is motivating and being used for empirical work elsewhere in economics, isolate the key aspects of that model, and see if it tests out. And if it doesn’t test out, then that raises questions about the validity of the model and the empirical field work based on that model. And if it does test out, you sometimes get real interesting results that suggest new lines of theoretical research and a richer understanding of the observations that you make.”

[398] I do not focus on the travails of funding the social sciences, including economics; the budget cuts described by Newlon in the late 1980s, in particular, are well documented by Tiago Mata. MATA, T. & SCHEIDING, T. 2012. National Science Foundation Patronage of Social Science, 1970s and 1980s: Congressional Scrutiny, Advocacy Network, and the Prestige of Economics. *Minerva Minerva,* 50**,** 423-449, NEWLON, D. 1989. The Role of the NSF in the Spread of Economic Ideas. *In:* COLANDER, D. C. & COATS, A. W. (eds.) *The Spread of Economic Ideas.* New York: Cambridge University Press. The exact role of experimental economics remains to be explored.

ran a seminar series at NSF for years to showcase social science research results. The talk by Charlie Plott on his research on agenda setting experiments with applications to real world decisions was probably the most successful presentation in that series. I remember one of my bosses telling me a visit to Charlie Plott's laboratory at Caltech knocked his socks off."[399]

In fact both Plott's and Smith's experimental auction research and Plott's applied experimental barge study were featured in the first annual NSF program report in 1978 (Blackmann, 1978).

An understanding of experiments as a scientific activity was perceived also in university-wide grant applications. When Charles Holt moved to the University of Minnesota in 1982, he submitted a grant proposal for experimental research on auctions. The grant was approved. Later Holt learnt from his colleague Leonid Hurwitz, the 2007 Nobel Memorial Prize Laureate, who sat on the decision-making committee, that its members "really liked that proposal. The people in other departments could understand it. They could see why you would do an experiment. They thought it was important."[400] Holt recalls that this news made him think that:

> "[E]xperimental economics is going to do well if they do well in funding. And I think the National Science Foundation, NSF, has always funded experiments more heavily than you would think based on, say, pages published in major journals. And I think part of it is it could have been just DeGroot's [one of Holt's advisor at Carnegie Mellon] 'let's do something scientific so the physical scientists don't bother us.' But I think part of it is an interdisciplinary panel or a panel even across

[399] Newlon Interview. Newlon continued: "the 'music of market' is another example of how the results of experimental economics could be used to dramatize the value of economic research. Charlie Plott had a display of a computer screen showing bids and offers for bonds changing over time in an experimental financial market that crashed spectacularly. The bids and offers were set to music. Plott called this the music of markets. At NSF program reviews we would begin our presentations by asking audiences to think of their retirement accounts and then run the music of markets display.
There are a number of interesting stories I could tell about NSF support of experimental research. For example, Robert Byrd, when he was majority leader of the US Senate, demanded NSF not fund Vernon Smith's research on experimental tests of auction theory because there were higher priority uses for government monies in tight budgetary times. Vernon Smith later won the Nobel prize and his work helped shape the first broad band communication auction that generated over $20 billion in revenue for the government."
[400] Holt's recollection of what Hurwicz told him. Charles Holt Interview.

> different sub fields in economics that they can understand what this experiment is going to do."[401]

The relevance of experimental research and its strategic importance for the development of economics as a science was recognized in the 1980s in various science policy memoranda. For instance, the National Science Foundation presented four themes of the Economics Program long-range plan in 1983:

> "1) a new generation of macroeconomic models; 2) general conceptual framework for noncompetitive markets and nonmarket situations; 3) laboratory tested laws in the social sciences [which] will compare favorably with the rigor of laws in the physical and biological sciences; 4) new theories and better econometric technique for panel data of labor dynamics."[402]

One of the six members of the panel which drafted these recommendations was Charles Plott. Another instance of highlighting the future potential of experimental research was the *Leading Edges in Social and Behavioral Science* volume (Luce et al., 1989). In the section pertaining above all to economics entitled 'Choice and Allocation' four out of six chapters discussed the contribution of experimental economics.[403] Many of the authors of these chapters were experimentalists – Plott, Ledyard, Ferejohn, Roth and supporters such as Lance Davis, Roy Radner, William Riker, Stanley Reiter, Theodore Groves, or Leonid Hurwicz (Lee, 2014).

The ties between the NSF and the experimental economics community continued to be strong in the 1990s and beyond. At least eleven practicing experimentalists sat on the Economics Review Panel allowing experimental economics to be continuously represented at the evaluation process of grant applications.[404] Catharine Eckel in the years 1996-1998 and Rachel Croson in the years 2002-2004 served as visiting Program Directors of the Economics Program. Rachel Croson also served as the Division Director for Social and Economic Sciences at the NSF during the years 2010-12. Elizabeth Hoffman even sat on the National Science Board, the highest governing body of the NSF, from

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401 Charles Holt Interview.

402 Subpanel for Economics Summary of Minutes from April 8, 1983. Correspondence Series. Plott Papers.

403 The remaining two chapters dealt with macroeconomics and labor markets.

404 Information collected from online CVs. Only Economic Program considered, e.g. Decision Risk and Management Science: Alvin Roth (1985-1987), Elizabeth Hoffman (1989-1991), Ron Oaxaca (1991), Charles Holt, David Levine (1995-1997), James Andreoni (1996-1998), Dan Levin (2001-2003), Doug Davis (2003-2005, 2009), Alessandra Cassela (2004-06), Jacob Goeree (2007 – 2008), and Tim Cason (2005, 2009 - 2011).

2000-2008. Experimentalists, Charles Plott in particular, were involved in the restructuring of the NSF and establishing of a new directorate - the Social Behavioral and Economics Directorate.[405]

All this evidence shows the close cooperation between experimental economists and the NSF, with Newlon in particular, that started early on and has steadily continued. At first personal ties and educational background helped it to start. But it was primarily driven by the aligned and self-reinforcing interests of both groups – experimentalists needed funding and the stamp of approval while the Economics Program at the NSF needed to defend itself and increase its budget.

> "NSF featured some experimental work in order to build up a constituency within the NSF. And I remember one year, our grant was featured. I think Charlie [Plott] had that happen once. [Daniel Newlon] rotated it and he used that to basically help to build support within NSF, not only just for experimental economics, but for the Economics Division, [showing] that they are doing something useful."[406]

The opposition to funding experimental research within the profession came from some economists without direct experience of the experimental method. While theorists were attracted by the idea of symmetry (between rigorous data and theory), applied economists and econometricians who were used to field data and working around their quality issues were less enthusiastic. [407]

[405] "Charlie and I are co-conspirators. Charlie is probably one of the most, if not the most, politically astute economists that I've dealt with. There's a set of problems or issues associated with the *National Science Foundation* that we've tried to raise in various forums and venues. We're using this panel [sc. that of 2010] to raise some of those issues again. The fundamental problem or concern that Charlie and I share is that the economics program is caught in an institutional straightjacket at the *National Science Foundation*. That it cannot grow rapidly in the social sciences because the sister programs in sociology and political science are politically unpopular." Daniel Newlon Interview.

[406] Vernon Smith at the Witness Seminar

[407] For opposition to funding social science research in general and economic research in particular, see MATA, T. & SCHEIDING, T. 2012. National Science Foundation Patronage of Social Science, 1970s and 1980s: Congressional Scrutiny, Advocacy Network, and the Prestige of Economics. *Minerva Minerva,* 50**,** 423-449. US Senator William E. Proxmire issued the Golden Fleece Award from 1975 until 1988. It identified what he considered wasteful government spending. NSF was one of the awardees and animal experiments by John Kagel and Raymond Battalio were highlighted in his award report.

5.4 Conclusions

This chapter described and analyzed the process of passive reception of experimental economics – a process of the reception of experimental research by economists such as editors and journal referees who did not have direct experience with the experimental method, but had to be convinced about its merits if experimentalists in the profession were not to be marginalized. The role of funding, in particular funding from the NSF, proved crucial in supporting experimental research and keeping the experimental engine going. It has been a mutually beneficial alliance. For NSF, experimental economics was not only a risky investment with a large potential return; it was also a means of increasing scientific credibility vis-à-vis other more mature sciences operating with the NSF.

Book publications about experimental economics catered to two audiences. On the one hand, it lowered the barrier to entry into experimental research for non-experimentalists who now had accessible primers. On the other hand, it invigorated the community by giving it a sense of reaching a more mature stage and confirmed the standing of the authors of these publications.

My dominant focus in this chapter was a number of strategies developed by Charles Plott and other early experimentalists when attempting to get their papers published in leading journals. Experiments confirming economic theory were often viewed as unsurprising or uninteresting, offering little to be learnt. Experiments with disconfirming evidence, in contrast, were accused of faulty experimental procedures, for instance, or an unrepresentative student-based subject pool, or of applying theory in artificial situations. Experimenters designed strategies such as theory competition, giving theory a best shot, shifting the burden of proof. They turned criticism into a request for more experiments, or sometimes demanded referees with actual experience or at least knowledge of experimentation. These disputes served not only to persuade editors and referees, but were also instrumental in deepening experimental economists' own understanding of experimental methods, as described in the previous chapters. These strategies operated on the micro level of disputes over particular papers, but overall had consequences on the macro level, the acceptance of experimental economics by the rest of the profession. The various strategies are consequences of the four driving forces of the experimental turn – integrity of data, symmetric relation of theory with data, and data that are collected under rigorous conditions and have the potential to productively interact with theory in a virtuous circle.

Apart from epistemic functions, these strategies had a sociological component. They reflected experimentalists' attempts to gain more control of their destiny through influencing the publication process. However, the vigorous approach of the early experimentalists also stirred concerns by some non-experimentalists, including editors, that experimental economists are too lenient with each other and their community cannot handle self-regulation. This issue becomes central in the penultimate chapter of my dissertation, which treats the first-price auction controversy.

6 The First-Price Auction Controversy

Abbreviations frequently used in this chapter:

CRRAM - **C**onstant **R**elative **R**isk-**A**verse **M**odel

CRS - a 1982 paper by James C. **C**ox, Bruce **R**oberson, and Vernon L. **S**mith entitled Theory and behavior of single object auctions (Cox et al., 1982a)

CSW - used to refer to the joint work of James C. **C**ox, Vernon L. **S**mith and James M. **W**alker

CS - is used to describe both CRS and CSW

KR - John H. **K**agel and Alvin E. **R**oth, used in particular in the context of their 1992 comment (Kagel and Roth, 1992)

Any American economists who had not heard of economics experiments must have been enlightened when the December 1992 issue of the *American Economic Review* (AER) landed on their desks. Nine out of twenty two articles or 40% of the issue's pages dealt directly with experimental research. To a non-experimentalist, the least obvious experimental papers, judging from their titles, was a series of four comments on Glenn Harrison's 1989 AER paper *Theory and Misbehavior of First-Price Auctions* and his reply. Readers were drawn into an exchange that twenty years later remains the most prominent controversy among experimentalists conspicuously displayed on the pages of AER and marked by intense turmoil among its main protagonists.

In his 1989 paper Harrison delivered a "methodological critique" of the experimental evidence of overbidding in first-price auctions, i.e. bidding above the predicted Nash equilibrium bids of risk neutral bidders (Harrison, 1989). Overbidding was first observed by Vernon Smith in the late 1970s (Coppinger et al., 1980) and subsequently replicated in better-controlled laboratory conditions by Cox, Roberson and Smith (hereafter **CRS**)[408] (Cox et al., 1982a). It became the empirical edifice of a research agenda that Smith together with James Cox and James Walker (hereafter **CSW**) vigorously pursued during

[408] Bruce Roberson, like Smith's earlier co-authors Vicki Coppinger and John Titus, was an economics undergraduate at the University of Arizona who had attended Vernon Smith's experimental economics class.

the 1980s. CSW spent considerable effort on experimentally examining and theoretically modeling first-price auctions, in particular their *Constant Relative Risk-Averse Model* (hereafter **CRRAM**), which was based on the assumption of risk-aversion rather than risk neutrality (Cox et al., 1982a, Cox et al., 1982b, Cox et al., 1983a, Cox et al., 1983b, Cox et al., 1984, Cox et al., 1985, Cox et al., 1988, Walker et al., 1987). Harrison argued that the apparent overbidding was an artifact of the flawed control of the subjects' preferences in CRS's experiments. In consequence, if CRS lost control, the foundation of CSW's almost decade of work would crumble and their conclusions would scatter like a card house.

Harrison's 1989 paper did not appear as a comment on CRS or CSW, but as a stand-alone paper in the AER. Yet at this point, only one of CSW's many papers had been was published in the AER; in the annual *Papers and Proceedings* which has a different editorial process than the regular issues. With one of the most prominent research programs in the experimental community and Smith's intellectual reputation and professional leadership at stake, CSW requested to react. However, Orley Ashenfelter, the AER editor, allowed CSW to publish their reaction only as a comment, thereby inviting other experimentalists to submit their views of Harrison's critique and Harrison to reply to them all in the same issue.

Of the three other comments, the one by John Kagel and Alvin Roth (hereafter **KR**) is of particular significance since it was not merely a commentary on Harrison (Kagel and Roth, 1992). It was above all a ferocious rebuttal of the CSW research, in particular of the evidence for overbidding and the implications of the binary lottery procedure for CRRAM. KR's motivation originated in their own research. The lottery procedure was extensively studied by Roth in bargaining experiments and was designed to ensure that subjects' preferences were risk neutral. Kagel experimentally examined common value auctions which persistently exhibited the winner's curse (see Section 6.4 below) and suggested that anything other than risk neutral behavior does not fully explain the observed deviations from risk-neutral predictions and that learning and misunderstanding are relevant as well. Thus, the December 1992 issue of AER contained in fact two debates – one about the merit of Harrison's criticism and the other about the significance of CSW's research agenda.

Thus, the controversy is obviously convoluted and may usefully be structured along two axes – at the epistemic and the sociological level. On the epistemic level there are two overarching themes. The first one is the issue of sufficient experimental control to guarantee the establishment of empirical regularities. This was the case with Harrison's critique of overbidding. The second is the issue of how one modifies theory in light of countervailing empirical evidence that one believes to be rigorous. In the course of the 1980s CSW and KR developed different prior beliefs - about overbidding and the efficacy of the lottery procedure - which guided their interpretation of new experimental data. This in turn allowed different methodological commitments to mature. While CSW explicitly framed their research in the Lakatosian terms of an empirically progressive research program, KR were dismissing CSW's program as parameter fitting which unjustifiably abandoned some core economic assumptions to which they preferred to adhere.

The sociological dimension of this controversy is also multilayered and I want briefly to describe the various layers in outline before examining them in detail in the course of the chapter. First, there is the issue of publishing Harrison's flat maximum critique as a stand-alone paper in the AER, although it was above all a critique of CRS and by extension of CSW research that had not appeared in the AER. This brings out the role of AER's editor Orley Ashenfelter, his editorial board, and the referees of Harrison's paper.

Second, KR grew increasingly suspicious of CSW's handling of data and presentation of their experimental design specifications. An early major CSW paper reported only in the instruction appendix that subjects were not allowed to bid above their private values (Cox et al., 1982a). Later on, the issue of which data points should be discarded from the data analysis of CRRAM became a bone of contention.

The final sociological aspect is related to the establishment of the *Economic Science Association* at the end of the 1980s. As we have seen, both CSW and KR (and many other experimentalists) took opposite sides regarding the name of this association of experimental economists. All these aspects contributed to CSW's perception that they, and by extension experimental economics also, were under attack. In retrospect, the period shortly after the ESA's establishment marks the greatest influence of Smith and his Arizona group on the experimental community. Not only the subsequent robust growth of experimental economics and the separation of behavioral economists from experimental economics, but also the first-price auction controversy contributed to this relative decline.

This chapter will neither adjudicate who was right or wrong nor do full justice to all the intricacies of the dispute, in particular to all the counterarguments and counter-

counterarguments that were exchanged by everyone involved. Rather, through examining the epistemic and sociological levels of the controversy I want to convey a sense of the full force that this dispute exercised on the acceptance of experimental economics within the economics discipline. The issue of rigorous experimental control and influence over the experimental community, including its image within the profession presents two sides of the same coin – trust in the data and interpretation of experiments, that is, mutual trust among experimentalists; and trust from the rest of the profession in the scientific conduct of the experimental economics field. This fully captures the reason for the importance of the controversy.

Although the issue of bidding in first-price auctions touches on core problems of economics and experimental methods, nevertheless from the perspective of the entire science of economics it remains a narrow and particular one. The importance of this episode lies in enabling the experimental economics community, by presenting its internal dissent, to gain acceptance from the rest of the profession and thereby to conclude the process of the passive reception of experimental economics, which had started when the editors and referees of economics journals started to continuously and strenuously deal with experimental papers submitted in the 1970s, as examined in Chapter 5.

My exposition of these intertwined levels is structured in the following way. First, I focus on their common denominator, that is, the experimenter's ability to reliably induce attitudes to preference and risk. Second, I discuss Roth's research using the binary lottery procedure; the establishment of overbidding by CSW and their decision to pursue risk aversion in theorizing and experimenting; and Kagel's auction research. These constitute the elements of the subsequent controversy. However, without Harrison acting as a catalyst, the debate would not have occurred as it did. Third, Harrison's 'flat maximum' critique will be presented with the reactions that it stirred. Next, the direct exchange between CSW and KR is used to uncover two different experimental methodologies. Finally, I situate the controversy within the context of the emerging experimental community during the 1980s, in particular, the creation of the *Economic Science Association* and the reception of experimental economics in journals because it framed the debate and sheds light on the underlying issue of self-regulation by the experimental economics community and the corresponding issue of trust from the rest of the profession.

6.1 Inducing (Risk) Preferences

Preferences are the core concept of microeconomics and control over a subject's preferences is the key element of experimental research. The *Induced Value Theory* formulated by Smith (Smith, 1976, Smith, 1973) provided a general method and justification of imputing preferences in subjects through attaching monetary rewards to possible outcomes in an experiment. Smith stipulated and later formalized four principles - "precepts" of saliency, dominance, non-satiation, and privacy – that are sufficient conditions for a well-controlled microeconomic experiment (Smith, 1982). These conditions have been discussed extensively in the methodological literature (Guala, 2005, Santos, 2009), but only dominance will play a prominent role in this chapter. [409] Its effect is that the monetary reward provided in an experiment shall be the only significant motivation for each subject and the reward is the determinant of his or her actions.

Inducing preferences in experimental situations where decisions are made under risk requires a brief consideration of the von Neumann-Morgenstern *Expected Utility Theory* (Morgenstern and Neumann, 1944). Building on a set of axioms over people's preferences – the completeness, transitivity, continuity, monotonicity, independence, and reduction axiom – was defined by von Neumann and Morgenstern as an expected utility function over lotteries, or gambles, which has the form of a sum of the utilities of all possible outcomes weighted by their respective probabilities.[410] They proved that under these axioms a utility function over lotteries exists and an individual will choose one lottery over another if and only if the expected utility from it is higher (i.e. expected utility maximization). Thus, in risky decisions there are two utility functions – one for money (associated with specific outcomes) and one for lotteries (associated with the distribution of outcomes). The former is often called the Bernoulli utility function and the latter the

[409] The other precepts refer to the following. *Saliency* is the property of laboratory rewards given to subjects that makes the rewards a known function of the experimental actions and events. *Non-satiation* or monotonicity is the property of the laboratory rewards given to subjects that makes more of the reward always preferable to the subject. *Privacy* is the practice of keeping each subject's endowments and rewards (and the experimenter's goals) as private information not available to other subjects. Smith also discussed the *parallelism* precept which is the extent of the similarities between laboratory and field environments that permit generalizing laboratory findings to field environments.

[410] A (simple) lottery (gamble) is a probability distribution over a known, finite set of outcomes. If the outcome of a lottery might itself be another lottery, then we talk about *compound lotteries*. The reduction axiom allows the reducing of compound lotteries into simple ones. According to the independence axiom, when two lotteries are mixed with a third one, the same preference order is maintained as when the two lotteries are presented independently of the third one.

von Neumann-Morgenstern utility function or the *expected utility function*. The curvature of the utility function was later interpreted as the attitude towards risk. Pratt and Arrow introduced the most prominent measures of risk-aversion (Arrow, 1965, Pratt, 1964)[411]

Although there are models of risky choices that make predictions independent of agents' risk attitudes, most experiments cannot avoid risk preferences since the predictions of economic theories critically depend on them, risk neutrality in particular.[412] Two main approaches had been developed to control for risk attitudes by the time the first price auction controversy erupted. One focused on their measurement,[413] the other on their inducement.

According to the latter approach, which later became relevant for CSW, the experimenter induces specific preferences among experimental lotteries by using the *binary lottery procedure*. This procedure has two stages. In the first stage, an experiment is conducted with payoffs not in money or experimental currency but in lottery tickets. The number of lottery tickets earned affects the probability of winning a prize in the lottery that is played in the second stage. The way that the share of lottery tickets is transformed into the probability of winning determines the type of risk preference. If the share of tickets equals the probability of winning a prize, then the subjects who maximize expected utility should have a risk neutral preference over the outcome of the lottery. Berg and her co-authors expanded this procedure to allow a variety of risk attitudes to be induced (Berg et al., 1986).

The lottery procedure was originally suggested by a statistician Cedric Smith (Smith, 1961) and later independently developed and used extensively in bargaining[414] experiments by Al Roth and his co-authors (Roth and Malouf, 1979). In the first stage of their binary lottery game, two parties bargained over the division of lottery tickets. In the second

[411] Measures of risk aversion are associated with the curvature of the utility function. A linear utility function corresponds to a risk neutral subject, and a concave (convex) utility function corresponds to a risk averse (risk preferring) subject. The function $A(x) = - u''(x)/u'(x)$ is the Arrow-Pratt measure of absolute risk-aversion while $R(x) = x\,A(x)$ is their measure of relative risk aversion.
[412] For instance, the probability matching effect with a classic experiment by Sidney Siegel and his subsequent research. SIEGEL, S. & GOLDSTEIN, D. A. 1959. Decision-making behavior in a two-choice uncertain outcome situation. *Journal of Experimental Psychology Journal of Experimental Psychology,* 57**,** 37-42. Vernon Smith discusses how this research influenced his in SMITH, V. L. 1992. Game Theory and Experimental Economics: Beginnings and Early Influences. *History of Political Economy,* 24**,** 241-282.
[413] The Becker, DeGroot, and Marschak mechanism is the best known such procedure. BECKER, G. M., DEGROOT, M. H. & MARSCHAK, J. 1964. Measuring utility by a single-response sequential method. *Behavioral science,* 9**,** 226-32.
[414] Bargaining is an economic institution in which two agents decide how to divide an amount of money, points or lottery tickets.

stage, each subject was given a chance to win a money prize, where the probability of winning the prize equaled the share of lottery tickets obtained. If a subject obtained 60% of tickets, she had a 60% change of winning her prize. The other subject had a 40% chance of winning her prize. The two prizes typically differed for each of the two bargainers.

The reasons why this procedure theoretically induces risk neutral preferences is simple. The utility that subjects have over lotteries has the form of the von Neumann-Morgenstern expected utility. It is linear in the probability which is represented by the share of lottery tickets obtained through bargaining. So regardless of the curvature of the utility of the monetary reward of the lottery prize, all the expected utility maximizing subjects must submit risk-neutral responses in the first stage of the procedure. The whole binary lottery procedure is a compound lottery and success crucially depends upon the subjects' ability to reduce it to a simple lottery according to the compound lottery axiom, and order it in preference according to the independence axiom (see Footnote 410).

True, much experimental evidence had accumulated against the expected utility theory, such as the Allais and Ellsberg paradoxes (Ellsberg, 1961, Allais, 1953). Yet Roth insisted that the binary lottery procedure allowed an assumption of the theory to be implemented. In fact, prior experiments testing the predictions of Nash's theory of cooperative bargaining examined "bargaining under conditions they [game theorists] believed more closely approximated natural situations, and all had assumed, for the purpose of obtaining predictions from Nash's theory, that the preferences of all bargainers were identical and risk neutral" (Roth, 1987, pp. 14-5). When the predictions made by Nash's bargaining theory were disconfirmed, game theorists, according to Roth (Roth, 1988, Roth, 1995a, Roth, 1987), argued that these experiments did not control for risk neutrality or perfect information (i.e. knowledge of each bargainer's preferences). Hence, the binary lottery procedure provided a better test of Nash's theory by controlling the risk neutrality assumption and thereby previously raised criticism was allowed to counter it.

> "What binary lottery games do allow us to know is the utility of utility maximizers who are concerned with their own payoffs. Since this is the kind of data required by Nash's theory,[415] experiments using binary lottery games allow us to use the

[415] Roth essentially claims here that the binary lottery procedure provides a best shot for Nash's theory. The concept of best shot was discussed in Section 5.1.2.3 on publication strategies.

theory to make precise predictions. It is this which was missing from earlier experiments and from efforts to analyze bargaining data by *inferring ex post* what the utility of the bargainers might have been" (Roth, 1995b, p. 42, my emphasis).

Roth has been careful to mention that the procedure is not "a magic wand," but considered it an important tool of experimental control in experiments intended to test theory by implementing its assumption of risk neutrality. The added emphasis in his quotation refers in part to CSW's research, as will become apparent in Section 6.3.

Roth's game theory background and his path towards experimentation in the 1970s were discussed in Chapter 2. His experimental investigation of bargaining started in the late 1970s while he was preparing a monograph on game theory models of bargaining which appeared in 1979 (Roth, 1979). Roth and his co-authors studied in particular the effect of information provided to bargainers and the size of their lottery prizes on the agreed shares of tickets and also the rates of bargaining failure (Roth and Malouf, 1979, Malouf and Roth, 1981, Roth et al., 1981, Roth and Malouf, 1982, Roth and Schoumaker, 1983, Murnighan et al., 1988). [416] Roth also published in this period a series of game theory papers on bargaining looking specifically at the impact on risk aversion (Roth, 1979, Roth and Rothblum, 1982, Roth, 1985b, Roth, 1985a).

When Roth et al. varied the amount of information provided to subjects, which is of no consequence to Nash's theory, they found that the outcomes were sensitive to these changes. In one treatment, subjects knew only their own money prize amounts. The shares tended to be equal and coincided with the theoretical Nash prediction. In the other treatment, they knew both their own and the other's money prizes.[417] The shares tended to be at a level, providing an equal expected payoff from the lottery.(Roth and Malouf, 1979). Subsequently, they examined the case of only one of the participants knowing her opponent's prize, while the other did not. Their results were confirmed by observing a shift towards equal expected monetary rewards mainly caused by the fact that the player with the smaller prize was informed about both prizes. To reconcile the apparent problems with game theory assumptions, Roth and Murnighan (Roth and Murnighan, 1982) suggested "that certain agreements became 'focal' for reasons that

[416] Bargaining failure refers to not finding a mutually agreeable split of the tickets within an allotted time interval.

[417] The bargaining took place over PLATO terminals and subjects were always provided with the expected payoffs from the lottery for any proposed split.

might not be captured by the game theoretic models … but that the existence of these focal agreements was recognized by the bargainers, who incorporated them into their behavior in a strategic, game theoretic manner“ (Roth, 1995a, p. 255). Roth and Schoumaker saw an indirect confirmation of their 'focal point' approach in their experiments that showed that the choice of equilibrium could be influenced by manipulating subjects' expectations (Roth and Schoumaker, 1983).

However, not everyone agreed with Roth's inclination to interpret the data only in terms of game theory, allowing Nash's bargaining theory to be maintained. Werner Güth suggested that fairness is a driving force which leads subjects to reach deals that they perceive as fair distributions:

> "It seems justified to say that the behavioral theory of distributive justice offers an intuitively convincing and straightforward explanation for the experimental results of Roth and Malouf contradicting the most fundamental game theoretic axioms. In our view this explanation is more convincing than the approach of Roth and Schoumaker (1983) . . . What Roth (1985) calls the focal point phenomenon is in our view just the problem of deciding between two reward standards differing in their prerequisites. One can only wonder why Roth and his coauthors do not even consider the explanation offered by the behavioral theory of distributive justice (the first version of this paper, finished in 1983, was strongly influenced by discussions with Alvin E. Roth). Probably the main reason is that this would mean to finally give up the illusion that people can meet the requirements of normative decision theory" (Güth, 1988, p. 709-10, the text in parentheses is by Güth).

I cannot pursue here the details of the subsequent debate that led to the investigation of fairness, other-regarding or social preferences, and sub-game perfect equilibria (Güth and Kocher, 2013). But this quotation illustrates Roth's inclination to adhere to a broad theoretical principle - here, expected utility maximization - that may account for a variety of experimental situations rather than adopting a theory specific to a particular situation.

6.2 Establishing Overbidding in First-Price Private Value Auctions

Vernon Smith and his colleagues faced a similar problem in the early 1980s. They observed overbidding in first-price auctions in respect to a Nash equilibrium prediction based on risk neutrality. They could either take it as evidence against the assumption of risk neutrality; while maintaining expected utility maximization and equilibrium bidding; or they could opt for bidding errors and out-of-equilibrium bidding. They chose the former, but let me first explain how CRS established overbidding since that is central to Harrison's flat-maximum critique.

To understand CSW's subsequent auction research, we need a brief exposition of auction theory as it developed in the 1960s. Early in the 1960s, Vickrey (1914-1996) was the first to model auctions as a non-cooperative game with incomplete information, where each bidder knows with certainty his private value of the auctioned item, but only the probability distribution from which the other bidders' values are independently drawn (Vickrey, 1961). Vickrey started by examining the first-price sealed bid auction, that is, a market institution in which the bidder who submits the highest bid wins and pays the price equal to her bid. By assuming that bidders are risk neutral and expected utility maximizers, Vickrey derived the (Bayesian) Nash equilibrium bidding function, where individual (equilibrium) bids depend on the number of bidders in the auction.[418] Vickrey also studied three other private value auction forms – the second-price, the English and the Dutch auctions[419] - and derived important relationships between them. He proved that under the same assumptions all four types of auction yield equal expected revenue for the seller, i.e. the auction format does not matter for a risk neutral seller.[420] Only relying on the expected utility hypothesis and regardless of bidders' risk attitudes, Vickrey also showed that the first-price auction is isomorphic to the Dutch auction and the second-price to the English auction. This means that the bidders in one of the auction formats behave in the same way as in the other. For instance, in both second-price and English auctions each bidder has a weakly dominant strategy to submit a bid equal to her private value v_i.[421] These results were quite surprising, in particular if one considers that first and second-price auctions have a simultaneous bidding procedure, while in the other

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[418] More formally, if all N bidders have a private value v_i drawn from a uniform distribution on the interval $[0, v_h]$, then for each bidder i the equilibrium bid is $b_i = (N - 1)\, v_i / N$. Bidders in the first price auction lower their bid downward from their private value based on the number of bidders in the auction.

[419] In the second-price auction, the bidder with the highest bid pays a price equal to the second highest bid. In the English (or ascending price) auction the price of the auctioned item increases until only one bidder is left; while in the Dutch (or descending price) auction the price falls until some bidder accepts the price.

[420] This is now a special case of the revenue equivalence theorem.

[421] In other words, the second-price and English auctions elicit bidders' private valuations truthfully and are incentive compatible. The weakly dominant strategy implies Nash equilibrium. However, the first-price and Dutch auction do not have dominant strategy equilibria and thus the assumption of Nash equilibrium behavior is crucial.

two bids are submitted sequentially, so that bidders can observe the behavior of others and thus can act explicitly strategically. However, Vickrey's paper remained largely unnoticed by economists until the late 1970s when it became the theoretical benchmark for all theoretical and experimental research on auctions (Maskin, 2004).

Vernon Smith started in the years 1964-7 to do sealed bid auction experiments, Treasury bill auctions in particular.[422] There is no reference to William Vickrey's (1961) paper in Smith's publications at this time (Smith, 1966, Smith, 1967). Smith, to his own regret, remained unaware of Vickrey's research until around 1976.[423] The first laboratory examination of Vickrey was conducted by Smith and two economics undergraduates at the University of Arizona in 1977. They ran pen and paper experiments comparing the four auction formats. They found support for the second-price and English auction isomorphism, provided one allows learning in the case of second price auctions.[424] By contrast, they did not find support for the other isomorphism, because the average and variance of observed auction prices (i.e. the winning bids) in sessions of first-price auction were significantly above the Nash predictions, while those of Dutch auctions were not. The analysis of individual bidding data suggested the same.[425] The authors did not speculate about possible explanations of overbidding, but they concluded that in order to achieve "closer control over procedures, experimenter effects, information conditions and technical considerations in the different auction mechanism," each mechanism would have to be examined in a computerized environment (Coppinger et al., 1980, p.22).

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[422] As stated in Chapter 2, Smith's involvement with experimentation started with double auctions, a type of auction not considered by Vickrey. The background of Smith's Treasury auction research is described in his autobiography SMITH, V. L. 2008a. *Discovery - A Memoir,* Bloomington, IN, AuthorHouse. Pp. 291-4.

[423] Smith's 1976 summary of experimental results on bidding and auctioning institutions does not mention Vickrey (Vernon L. Smith, 1976a), but in this year he corresponded with Marschak. During a visit to Harvard in 1953, Marschak discussed "his procedure for inducing full-value bidding" with Harvard graduate students including Smith, which anticipated "Vickrey's ingenious 'second price' procedure" COPPINGER, V. M., SMITH, V. L. & TITUS, J. A. 1980. Incentives and Behavior in English, Dutch and Sealed-Bid Auctions. *Economic Inquiry,* 18**,** 1-22. p.11.

[424] Learning allowed the initial tendency to bid below value to be eliminated in the second-price auction.

[425] Regressing bids on private values should yield a coefficient coming from a distribution with a zero mean. However, the bids of nine out of fifteen subjects exceeded the Nash predicted bid at a 1% significance level.

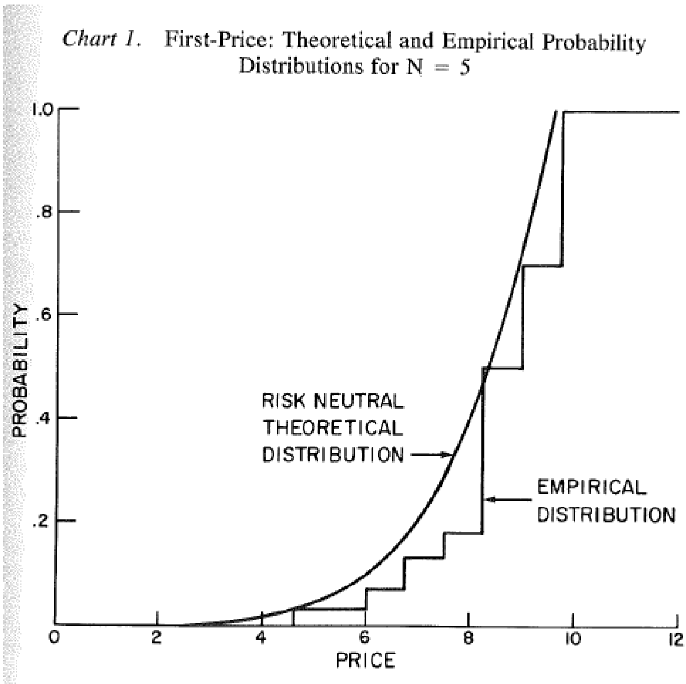

Figure 11 Cumulative probability distribution of market price in first-price auction.
The theoretical and empirical distributions are significantly different. Both are drawn from [0, v_h=10]
Source: CRS paper (Cox et al., 1982a, p. 25)

Based on this new research and Smith's earlier work on the Treasury bill auctions Smith was awarded an NSF grant for an experimental investigation of auctions that received several renewals and spanned most of the 1980s. James Cox became in this period a close collaborator. Shortly after his arrival at Arizona in 1977 he became interested and the first experimental paper with Roberson and Smith appeared in 1982. The lag reflected the long time that it took to go from initial design, to programming, collecting and analyzing data, and then thinking about new theory that could explain the overbidding in the first price auction. Smith was in charge of the experimental design. Cox's primary capacity was the theory, but the two interacted on the theory in the experimental design. James Walker came to the University of Arizona after finishing his Ph.D. at Texas A & M under the supervision of Battalio and Kagel. He took over the role of developing software, recruiting subjects, and running the experiments.

The next published[426] step was a paper by Cox, Roberson, and Smith (Cox et al., 1982a), which had all experiments computerized on the PLATO system (see Chapter 3). Given the previous evidence against the theoretical isomorphism of the first-price and Dutch

[426] It's important to stress that by 1981 CSW conducted numerous experiments, circulated them as working papers and continued to publish the results up until 1987 while working on additional experiments.

auctions, these two auction formats were the main focus of the 1982 paper. A major design feature of the experiments reported in this paper was that the subjects were not allowed to bid above their private value. Moreover, this was clear only from the instructions published at the very end of the paper (Cox et al., 1982a, p.39, 42). This became important later on and I return to it in Section 6.4. The auction format and the number of bidders N were the treatment variables. I will refer to the two auction formats - first-price and Dutch auction - as A and B respectively.

To control for treatment effects (saying that the sequence of A and B matter) and learning effects (that it takes time for subjects to learn how to bid), CRS employed a crossover design that had already been used by Smith (Coppinger et al., 1980). They matched ABA with BAB and each A or B consisted of ten rounds. At first, they focused on 3, 6, and 9 bidders. CRS conducted 160 auctions in total. They compared the distribution of prices in first-price auctions across all sessions for a given N with Vickrey's theoretical distribution of prices by applying the Kolmogorov-Smirnov test.[427] CRS concluded that for both six and nine bidders these distributions are not the same, while for three bidders CRS could not reject the null hypothesis that they have the same distribution. Similar results were obtained by binomial tests comparing observed and risk-neutral theoretical prices.

To understand what happens in the intermediary values of four and five bidders, CRS decided to run an additional 120 first-price and 120 Dutch auctions. They conducted the same statistical tests and the only graph in the paper depicts how the two price distributions differ for five bidders (Figure 11) An analysis of individual bids was not provided, because the main focus was on the isomorphism and auction efficiency. Using tabulated mean prices and price variances, they rejected the equivalence of the Dutch and first-price auctions and observed the greater allocative efficiency of second-price auctions. Cox recalls how the initial data informed CRS's choices:

> "Once we got into those experiments it started to become clear from the data that virtually all of the subjects were bidding as if they were risk averse, that virtually all of the data was on the one side of Vickrey's risk neutral bid function

[427] "The Kolmogorov-Smirnov one-sample test is a test of goodness of fit. That is, it is concerned with the degree of agreement between the distribution of a set of sample values (observed scores) and some specified theoretical distribution. It determines whether the scores in the sample can reasonably be thought to have come from a population having the theoretical distribution." SIEGEL, S. 1956. *Nonparametric statistics for the behavioral sciences,* New York, McGraw-Hill.

line. And it wasn't that the data was all over the place. It was tracking the changing predictions of the theory pretty well in one sense that as you increase the auction market size, what we'd call the non-falsification cone, which is the cone created by Vickrey's risk neutral bid line and the bid equals value line, since bidding above value is irrational. The size of that cone shrinks as you increase the market size."[428]

With these patterns in experimental data, CRS focused on their theoretical modeling. From a suggestion of the mathematical economist and future experimentalist John Ledyard, they developed the first Nash bidding model that allowed for bidders with heterogeneous utilities (i.e. different constant relative risk aversion parameters), which they called the Ledyard model and subsequently renamed the Constant Relative Risk-Averse Model (CRRAM). It generalized the Vickrey risk-neutral model of first-price auctions. In theory, for a risk neutral, risk-averse, or risk-seeking person, her utility is linear, strictly concave, or strictly convex in the pay-off, which is the difference between her personal value and the submitted bid. CRS showed that this implies that for all private values the bid of a risk-averse bidder is higher than the bid of a risk neutral bidder, which in turn is higher than the bid of a risk seeking bidder.[429] This is because risk-averse bidders tend to raise their bids in order to reduce the risk of losing the auction (Cox et al., 1988p. 65).

The utility function that CRRAM postulated, which has the functional form $(v_i-b_i)^{r}{}_{i}$, leads to a Nash equilibrium bid function which has two parts. For values below a certain threshold, the bid is a linear function of the private value. The remainder of the bid function cannot be expressed as an explicit function of private value, i.e. it does not have a closed-form solution.[430] The equilibrium bids in this model are above Vickrey's

[428] James Cox Interview.

[429] If bidder i's utility is no longer linear in pay-off (i.e. risk neutral), but strictly log-concave of the form $(v_i-b_i)^{r}{}_{i}$ with r_i taken from a 0-1 interval, then $1-r_i$ equals the Arrow-Pratt measure of relative risk-aversion of this utility function and is constant.

[430] The linear part of the bid function is defined only up to the highest bid that will ever be submitted by the least risk averse bidder. Let the CRRA parameter for this bidder be r_{max}. Let the highest possible private value be value be $\bar{v}$. Then the highest bid for the least risk averse bidder is $(n-1)\bar{v}/(n-1+r_{\max})$. The value that would elicit this same bid amount from a more risk averse bidder, with CRRA parameter r_i, is $v_i^* = (n-1+r_{\max})\bar{v}/(n-1+r_i)$. The linear part of the CRRAM bid function is $b_i = (n-1)v_i/(n-1+r_i)$. For values above v_i^* there is no closed form solution but the properties of this segment of the bid function have been derived by COX, J. C., SMITH, V. L. & WALKER, J. M. 1988. Theory and individual behavior of first-price auctions. *Journal of Risk and Uncertainty,* 1**,** 61-99. COX, J. C. & OAXACA, R. L. 1996. Is bidding behavior consistent with bidding theory for private value auctions? *In:* ISAAC, R. M. (ed.) *Research in Experimental Economics.* Greenwich: JAI Press.

benchmark and therefore CRRAM was used as its alternative hypothesis in statistical tests. In subsequent publications CRRAM was generalized to the class of log-concave models and further tested (Cox et al., 1988, Cox and Oaxaca, 1996).[431]

The most comprehensive evidence of overbidding and a summary of CSW's research on first-price auctions was published in an article (Cox et al., 1988). They reported that in 690 first-price auctions in 47 experiments, they had found a t-statistic for the null hypothesis that the mean deviation is zero, which was rejected in seven of the eight subsamples in favor of the alternative that price exceeds the Vickrey prediction, a result consistent with risk-averse bidding.

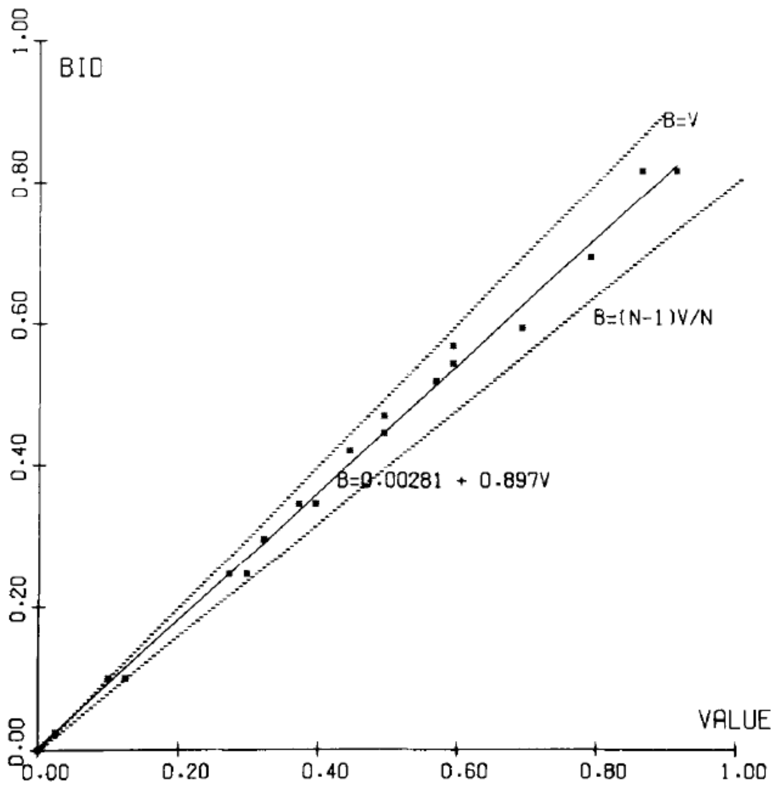

Fig. 5. Bidding behavior: Series 5, Exp. 2, $n = 5$, Subject 4

Figure 12 Normalized bid-value plots for an individual bidder.

Normalized bid values on an 0-1 interval The lowest line is the Vickrey prediction; should the bid be a winning one, bidding above private value (B=V) would lead to a loss. The line between the two is the linear fit (Cox et al., 1988, p. 80).

[431] These two papers contained a further generalization of the log-concave model. CRRAM is a parametric special case of this model.

The t-tests were based on pooling prices from various subjects within given subsamples. Looking at individual bidding data allowed CSW to pursue the risk-averse bidding hypothesis. Comparing each subject's bids with the Vickrey prediction, they rejected risk-neutral bidding in favor of risk-averse bidding for 94% of subjects. Graphs of bidding behavior for individual subjects such as Figure 12 suggested that subjects have various degrees of risk aversion. This was then confirmed by a regression of bids on a constant and on the private value for each subject. CRRAM predicts that the constant is distributed with a mean zero, but this does not happen for 21% of subjects. For 92% of subjects the slope coefficient is above the Vickrey value. CSW reported the exclusion of only a small number of bids since they belonged to the section of the CRRAM bid function that is not linear, but they provided no exact figures.

Other tests supported the view that behavior differed significantly across subjects and CRRAM was overall consistent with the experimental results for N larger than three. Establishing overbidding and the heterogeneity of risk aversion among subjects was an empirical basis for further theory development and experimental tests. The next section will briefly describe them.

6.3 CRRAM and the Lottery Procedure

With CRRAM explaining much but not all data, CSW conducted new experiments to test this new model further. The results were giving overwhelming additional support for CRRAM, but further inconsistencies were observed. Those were used for subsequent modification of the risk-averse model.

First, tripling of the subject payoffs had no significant effect on the estimated bid function parameters.[432] However, quadratic and square root transformations of subjects' profits led to bidding behavior inconsistent with that predicted by CRRAM.[433] Second, although

[432] The CRRAM utility function has various mathematical properties that provide predictions that can be tested. For instance, multiplying the profit of a winning bid by any positive factor has no effect on the equilibrium, but they are sensitive to additive wealth changes. More formally, according to CRRAM $U(x) = x^{1-r}$, hence multiplying the profit of a winning bid by any positive factor λ has no effect on the equilibrium bid because it gets $U(\lambda x) = \lambda^{1-r} x^{1-r} = \lambda^{1-r} U(x)$. However, that is not the case when wealth is added $U(x+w)$ cannot be factored by a constant and $U(x)$, as $U(x+w)= (x+w)^{1-r}$.

[433] When a power function transformation of money payoff to the winning bidder is implemented as a treatment, it is predicted to cause subjects to bid as if they were either less or more risk averse depending on whether the power is larger or smaller than 1.

individual subjects' bids were well fitted by the linear function of the private value, approximately 22% of estimated intercepts were significantly different from the predicted value of zero. Third, to explore the role of subjects' expectations of rivals' behavior, CSW used computer simulated bidders, who bid according to CRRAM.[434] The results were again consistent with CRRAM, but the significant non-zero intercept remained for the same share of subjects (around 20%) leading the writers to believe that it could be explained as a function of individual characteristics but not related to expectations of the rivals' characteristics (Walker et al., 1987).

Fourth, to account for the non-zero intercepts of estimated bid functions, in another iteration of theory and experiment, CSW constructed a modified version of their risk-averse model, CRRAM*. It modified the CRRAM utility function by adding two parameters. One reflecting subjective value attached to the act of winning the auction and the other a threshold level of monetary surplus.[435] Although both parameters cannot be directly observed, they can be manipulated by lump sum payments or charges and resulting changes compared to predicted changes of the intercept. CRRAM* was able to correctly predict 35 out of 45 paired comparisons (Cox et al., 1988, pp. 94-6). Fifth, Cox was able to extend the derivation of the bid function to multiple units - one for each agent in discriminative auctions with a uniform distribution of values - and came up with a derivation of equilibrium bid functions for the general class of log concave utility functions. The generalized CRRAM predicted that bidding would nonlinearly depend on private values, which they observed (Cox et al., 1984).

Finally, and most importantly for the controversy, CSW instead of paying subjects money for winning the first-price auction, decided to pay them in lottery tickets, i.e. applied the binary lottery procedure (Cox et al., 1985, Walker et al., 1990). The procedure as described earlier should have had the effect of risk neutralizing the subject in lottery tickets, and for risk neutral bidders the prediction of CRRAM or CRRAM* coincides with Vickrey's original prediction. However, the subjects continued to overbid, which, in CSW's

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[434] Human subjects were given these instructions: "Each computerized bidder is programmed to bid some fixed fraction of the resale value it receives in each auction. For example, one computerized bidder might always bid 0.8 times the value drawn, another might always bid 0.9 times its value. However, none of the computerized bidders bids less than 75% of resale value and all bid less than full resale value. In other words, the fractions used in generating the computerized bids are always between 0.75 and 1.0 (but not including 1.0)." This restriction follows from CRRAM.

[435] $u_i = (v_i - b_i + w_i - t_i)^{r}{}_{i}$ where w_i is the utility of winning; when increased, it pushes the intercept up. t_i is the income threshold which when increased shifts the intercept down.

view, was in violation of the conjunction of CRRAM and the lottery procedure, but was consistent with CRRAM with risk aversion.

Given the prior support for CRRAM that CSW observed, they conjectured that the reason for this failure is the compound lottery axiom of expected utility theory, because it added the compounding of probabilities as the only new element in the new set of experiments. Indeed, the cost of using the lottery procedure is generally incurred by adding a layer of transforming the experimental points into probability, and a layer of uncertainty in converting the probability into the realization of the fixed dollar prize.

In the conclusion of their paper, CSW voiced their opinion of Al Roth's research agenda:

> "Furthermore, these results may have implications for other research programs that must postulate the behavioral validity of the lottery procedure as a conditional in experimental tests of models that require risk attitude of agents to be controlled" (Cox et al., 1985, p. 165).

An interesting aspect of this quotation is the use of the term 'research program'. It is not coincidental. In fact, Smith and his co-authors often invoked Lakatos at the time (Cox et al., 1984, Cox et al., 1988, Cox et al., 1992, McCabe et al., 1989, Smith, 1989, McCabe et al., 1991).[436] For instance, CSW quoted Lakatos' position "written large in the history of science that 'there is no falsification before the emergence of a better theory'" (Cox et al., 1988, p. 89). Therefore the observed overbidding could not falsify Vickrey's theory without having CRRAM as an alternative theory to Vickrey's. Moreover, the alternative theory must do "at least as well as the incumbent theory in accounting for the non-falsifying observations, while at the same time accounting for some of the falsifying evidence. This perspective is perhaps the dominant characteristic of the practice of science as it has been interpreted by Lakatos" (Cox et al., 1984, pp. 999-1001).

CSW viewed their research as an example of a progressive research program that coalesced theoretical and experimental research, in which experimental observations informed theory, disciplined each other, and spurred further experimental and theoretical research.[437] When CRRAM was developed, CSW looked for ways of

[436] Compare also Footnote 200.

[437] Therefore it's not surprising that Smith opened his section on first-price auction research with a quotation from Robert Milikan, a physics Nobel Prize winner: "Science walks forward on two feet, namely theory and experiment. Sometimes it is one foot which is put forward first, sometimes the other, but continuous progress is only made by the use of both." MCCABE, K. A., RASSENTI, S. J. & SMITH, V. L. 1989. Lakatos and Experimental Economics. *University of Arizona Economics Department Discussion Paper,* 89-24.

challenging it and examining whether it had any “excess empirical content“ over Vickrey’s theory. Tripling payoffs and some of the new experiments described at the beginning of this section were the results of these efforts. The development of CRRAM* reflected an “ex-post hoc explanation” that attempted to theorize an empirical regularity that CRRAM could not account for (the non-zero intercepts of bidding functions). “In fact without CRRAM* no one would dream of doing such experiments. In this sense, CRRAM* makes “novel“ new predictions, and conforms to one of the key elements in Lakatos's concept of a progressive programme“ (McCabe et al., 1989, p. 41).

By mid-1986, CSW had written a second paper on the lottery procedure that they called *Inducing risk-neutral preferences: An examination in a controlled market environment*. It was a thorough examination of the lottery procedure in the context of first-price auctions. With additional experiments they confirmed all their earlier conclusions. In CSW’s view, principal innovation in Roth’s paper testing the Nash bargaining theory did not work and they questioned whether Roth’s experiments provided valid tests as he claimed. The paper had a long journey towards publication reflecting protracted arguments with referees. In 1986 and 1987 it was rejected by QJE, and in 1988 by the Journal of Economic Theory and eventually appeared in the *Journal of Risk and Uncertainty* in 1990.[438]

The application of the lottery procedure in the context of the first-price auctions was one of the sources of the controversy. Another emerged gradually from John Kagel’s research which I turn to in the following section.

6.4 Learning and Error in Common Value Auctions

Having spent most of the 1970s working on economics experiments that used laboratory animals, once he had arrived at the University of Houston in 1982, John Kagel wanted to move to human subjects and market related experiments. He teamed up there with a new assistant professor and theorist, Dan Levin.[439] They decided to launch an agenda on common value auctions since they had been intrigued by Robert Wilson’s applied

[438] Box 122, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[439] See Section 3.1.2 for more details on Kagel’s experimental research using animals, his interaction with Dan Levin and Ronald Harstad in Houston.

research on oil tracts auctions, the most typical example of common value auctions at the time. Wilson's research concentrated on a comprehensive theoretical treatment by Milgrom and Weber which invited experimental testing (Wilson, 1977, Milgrom and Weber, 1982). Houston being the oil capital of the US, it was easy for Kagel and Levin to obtain research funds from the local private Energy Institute to run their experiments.

Unlike private value auctions, common value auctions give the auctioned unit a specified value which is revealed only after the unit is sold. Each subject has an estimate (a signal) and an interval of what the value might be. So there are no private values, only estimates of the common value which are affiliated.[440] The price is determined by the highest bid (i.e. first-price auction) and its difference from the common value determines the profit. Under the assumption of risk neutrality, everything in the auction setup is symmetric across individuals except for the value estimates. Therefore, the Nash equilibrium bidding strategies will specify a person's bid as a function of the value estimate. Bidders in common-value auctions are susceptible to a "winner's curse." Since bids are based on imperfect estimates of value, the bidder who has the highest estimate of the value is likely to win the auction. The highest estimate is likely to be an overestimate, and therefore the highest bidder may be "cursed" by winning the auction and paying more for the item than it is worth (Davis and Holt, 1993, p. 288).

The initial experiments by Max Bazerman and William Samuelson confirmed the presence of the winner's curse, but this could have been attributed to the subjects' inexperience and the obvious errors they made (Bazerman and Samuelson, 1983, p. 629). Kagel and Levin's experiment was designed to address this issue and they introduced a nowadays standard way of implementing the common value auction in experiments (Kagel and Levin, 1986). The bidders not only had an opportunity to learn from their own experience, but also from the experience of others. In particular, all bidders could observe the actual earnings of the winning bidder. In addition, all the subjects in this experiment had had some prior experience in experimental auctions. Despite all this effort to educate subjects, a winner's curse was still clearly observed, although learning helped to mitigate it. Small groups of bidders earned positive profits, and larger groups made negative

[440] Individual estimates (or signals) are independently drawn from a distribution centered on the realization of the common value. However, bidders' private signals are positively correlated relative to the set of possible valuations. This is called affiliation.

profits; this, in their view, supported the hypothesis that the winner's curse is primarily caused by errors in judging the value of the object.[441]

Kagel, Harstad and Levin (1987) investigated first-price, second-price and English private value auctions with affiliated values. They observed that in the second-price auctions with affiliated values, bidders showed a persistent tendency to bid somewhat *above* their private values, and that the bids did not exhibit any tendency to converge to the true values over time. This was in striking contrast to what CRS observed in second-price auctions - that the bids were *below* the private values (Cox et al., 1982a). Although CRS and CSW studied only independent private value auctions, Kagel et al. argued that positive affiliation is unlikely to cause the difference (Kagel et al., 1987, p. 1298 footnote 22). Kagel et al. argued that “the key institutional feature responsible for these different outcomes“ was a design feature of the experiments reported by CRS and Coppinger et al. They imposed a binding ceiling on bids: that bids above private values were not allowed (Kagel et al., 1987, p. 1286, p. 1298).[442] This experimental feature was mentioned only in the experimental instructions at the end of the CRS paper, but not in the text of the 1982 paper itself. In this respect Al Roth’s contemporary account of this experimental feature is worth quoting in full:

> “Notice what this [the difference between the observations of CRS and Kagel et al.] illustrates about the power of experimental methods. As economists, we have become accustomed to the fact that, because field data are noisy and incomplete, apparently similar data sets may yield different conclusions. With experimental

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[441]The following extract from the Witness Seminar illustrates how Kagel and his co-authors arrived at this important insight that the winner’s curse is caused mainly by errors and lack of learning:
John Kagel: Let me give you a little anecdote here though because we were doing this at the *University of Houston*, and we were –
Chris Starmer: When was that?
John Kagel: This would be in ’84, ’83/’84. We are doing this at the *University of Houston* with MBAs. And one woman comes in to our experiment, and she says: “Is this the winner’s curse experiment?“ [group laughter] She went bankrupt in the sixth period or so [group laughter].
Chris Starmer: You saw to that.
Betsy Hoffman: So she knew the theory and wouldn’t do her any good.
John Kagel: She couldn’t apply it. But then the real puzzle for us, at least for me, was that we brought back experienced subjects. In small groups, they had overcome the winner’s curse. They weren’t anywhere near the Nash equilibrium, but then take the same subjects and you throw them into a larger group. They just go and, in fact, move in the wrong direction and commit winner’s curse all over again. You start to have to think about how can this be? How can they get it in the one case but not in the other? And then it just crystallizes. This has to be context specific learning. They have not absorbed the theory at all. And that was a big insight. That was just a tremendous insight because it really shows up in a lot of different areas.
[442] See also Roth’s discussion of this feature. ROTH, A. E. 1988. Laboratory Experimentation in Economics: A Methodological Overview. *The Economic Journal,* 98**,** 974-1031.

data, however, since the *collection of the data is fully under the control of the researchers*, we can hope to be able to identify the causes of such differences. In this case, by inquiring of the authors, *Kagel et al. were able to learn that an inadvertently unreported procedure of the earlier experiments* had been that bids in excess of a bidder's private value were not allowed. Once this point had been clarified, the differences between the two data sets also became rather clear" (Roth, 1988, p. 1008; my emphasis).

Bidding one's private value is the dominant strategy in the second-price auction. Hence the observed bidding above the private value was in Kagel's view a clear mistake on the part of the subjects. This strengthened Kagel and Levin's belief that errors and misunderstanding are frequent in auctions. They concluded:

> "[T]hat as expected profit conditional on winning the auction increased, bidders increased their bids proportionately more, earning a smaller share of profits compared to the RNE [risk neutral equilibrium] prediction, a result that is inconsistent with constant relative risk aversion, but which is consistent with *increasing* relative risk aversion" (Kagel, 1995, p. 526).

By 1985 there were two pieces of evidence against CRRAM. One was provided by the power transformation of payoffs (see section 6.3, above). A quadratic modification of payoffs led CRRAM to fail in explaining individual behavior (Cox et al., 1985). This became relevant for both Kagel and Roth, in that the lottery procedure, too, can be viewed as a modification to the payoff scheme. CSW reported that subjects' normalized bids shifted in the predicted manner eight out of twelve times. This was, according to KR, a weak test and should be compared to the null hypothesis of no effect which predicts that the subject's bids will shift downward in six out of twelve cases.

The other piece of evidence against CRRAM was delivered by Kagel and Levin (Kagel and Levin, 1985). Their paper reported that CRRAM did not explain individual bidding behavior when the size of the interval from which individual values were drawn was varied.[443] Estimating the predicted linear bidding function, they rejected CRRAM for 43% of subjects. The rejection rate for CSW was only 22% (Cox et al., 1988). CSW argued that

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[443] Private values were drawn from a uniform distribution centered around v_0 and bounded by v_0 - k and v_0 + k and k was varied across experiments. This is a different, way of drawing values, but equivalent to that in CSW.

Kagel and Levin's test is flawed, but I will not address this issue (Cox et al., 1992, pp. 1407-8).

In summary, in Kagel's view the data on private value auctions were suggesting that deviations from risk neutral predictions in private value auctions cannot be fully accounted for by constant relative risk aversion, and that learning or errors were appearing as well. The tensions were becoming increasingly acute, as can be seen in several instances where KR and CSW indirectly questioned each other's results and also from referee reports (Cox et al., 1985, Roth, 1988, Kagel and Levin, 1985). However, the looming debate took a decisive turn with Glenn Harrison's critique.

6.5 Harrison's Critique: from Message Space to Pay-off Space

By the beginning of 1987 several bones of contention had emerged. Although not all the research was published, it circulated among experimentalists. CSW vigorously pursued their research agenda on first-price auctions based on risk aversion to unseen depths, leading them, as a result, to conclude that the binary lottery procedure, designed to risk neutralize preferences - a key element of Roth's research - did not work. However, Kagel's research on common value auctions suggested that risk-aversion cannot account for overbidding. Furthermore, it questioned some of the experimental procedures of CRS and CSW (hereafter, **CS**) and Kagel et al. were unable to replicate all CS's results.

Harrison's criticism aimed at the core of CSW, namely, the overbidding which CRS had established. The gist of Harrison's argument was simple. CRS's testing of Vickrey's risk neutral bidding model of first-price auctions consisted of comparing Vickrey's prediction with either all the bids of each subject (within subjects analysis) or winning bids across auctions (between subjects analysis). In the terms of the language introduced by Smith in his seminal paper *Microeconomic Systems as an Experimental Science*, CS's tests relied on the message space of the auction, as all economics experiments did. Harrison argued that this was not the appropriate space to evaluate the behavior of experimental subjects. From the subjects' perspective it was "more natural to evaluate subject behavior in expected payoff space" (Harrison, 1989, p. 749).

Within Smith's language describing economics experiments (see Section 6.1), the environment consists of agents with their private values drawn from a distribution. The institution consists of messages (i.e. bids) and the allocation mechanism assigning the auctioned unit to the highest bidder. Agents choose messages: they do not directly choose commodity allocations. It is the institution which determines "allocations via the rules that carry messages into allocation" (Smith, 1982, p. 926). The final allocation of the auctioned unit in an experiment determines the payoffs of all subjects.

Harrison argued that one should move the analysis to these payoffs. More precisely, if one was looking at individual bids (the within subject analysis), one should take into consideration how much a unilateral deviation from the optimal Vickrey bid would affect the expected individual payoff. He referred to unilateral deviations bidding as misbehavior, and provided several alternative metrics of its costs to an individual. The choice of words worked like acid on CSW.

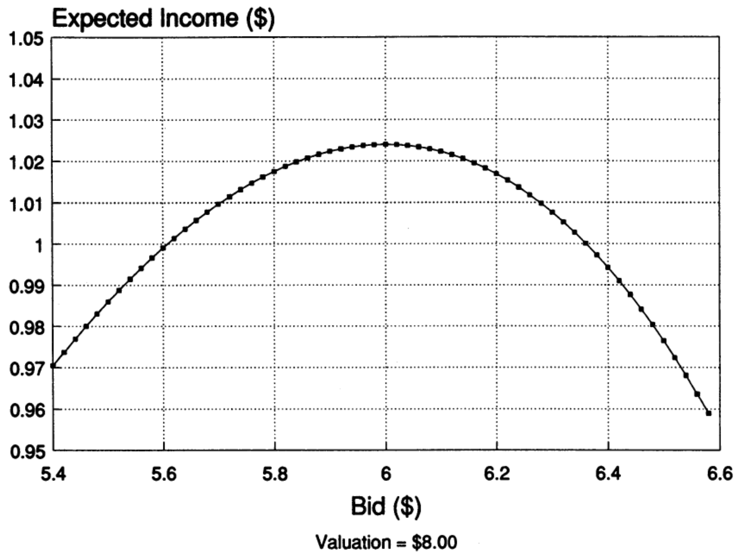

Figure 13 Foregone Expected Income for Alternative Bids
Source: (Harrison, 1989, p. 752)

Figure 13 illustrates Harrison's first metric. For N =4 and v_h = 10 (values taken from CRS) and a private value v_i = 8, the Vickrey risk neutral bid is $b^{NE}_{(Vickrey)i}$ = 6. Assuming that all agents follow Vickrey's model, one can derive the expected profit given v_i. If the agent deviates from the optimal bid, her expected payoff decreases, as shown in Figure 13. However, the shape is not symmetric around the optimal bid. An equal increase in bid decreases the expected payoff more than an equal decrease of the optimal bid would. If it were the other way, that would be a clear incentive to overbid. Subtracting the expected income from the optimal bid and doing the same for a variety of private values, Harrison arrived at Figure 14, which highlights his first conclusion. Bid deviations from Vickrey optimal behavior that appear large in the message space are negligible in the payoff space. Any bid deviation below one dollar results in a loss of expected payoff of no more than 16 cents. Since the curves are very flat around the optimal bid, this led to the label "the flat-maximum critique." Harrison provided two other metrics, which led him to the same conclusion.[444]

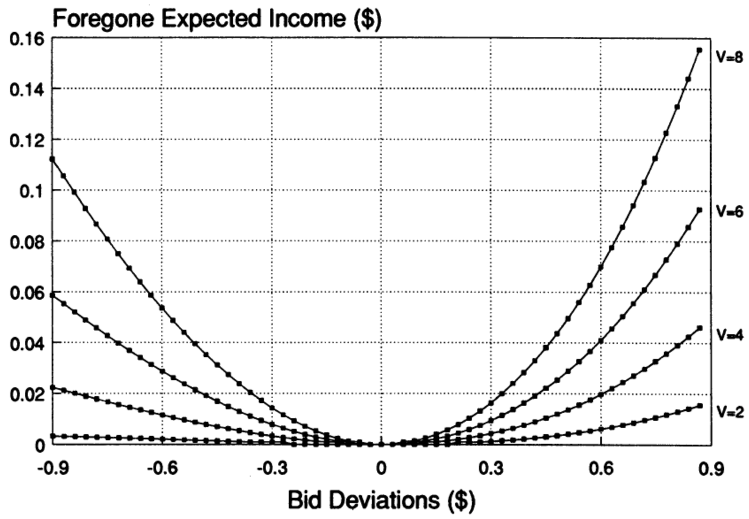

Figure 14 Foregone Expected Income for Alternative Valuations and Alternative Bids
Source: (Harrison, 1989, p. 753)

In addition, Harrison conducted a series of experiments with three treatments – subject experience with first-price auctions, reward type (money or lottery tickets), and bidding against computer simulated opponents which bid Vickrey's optimal bids. All three metrics were used to evaluate the new bidding data, but statistical tests used previously were also applied. From the data, Harrison constructed bid deviations from the various equilibrium predictions conditional on the assumed values of bidders' relative risk aversion. Because their distribution was "severely non-Gaussian," he preferred as "a more reliable measure of location," to take the median bid and not the means and variances of bids as CRS did (Harrison, 1989, p. 757). He concluded that, using either metric, the median measure of foregone expected payoff is less than 6 cents per bid. So, while in the message space, the observed bidding significantly differs from Vickrey's theory, in the pay-off space the deviations are insignificant and it cannot be rejected. Therefore, Smith's dominance precept is not satisfied:

[444] The first metric hinges upon the assumption that the deviating agent expects that everyone else will bid according to Vickrey. Harrison wondered how other types of expectation affect his argument. In metric 2, he assumed that all agents have r = 0.7 and the expected value E(r) = 1 (which is part of the calculation of the expected payoff). In the third metric, all agents have r = 0.7 and E (r) =0.7 as well. Thus, metric 1 tends "to overstate the opportunity costs of suboptimal bids" (Glenn W. Harrison, 1989, p. 755).

> "Frankly, our results pose a serious dilemma for the interpretation of these [CRS] experiments. In terms of the precepts proposed by Smith (1982), these experiments do not satisfy the requirement that rewards *Dominate* the subjective costs of the activity. As such, one has lost "control" over the experimental microeconomy under study. It is the purpose of an experimental design to make the monetary payoffs salient enough to be used as a surrogate for subjects' utilities. If we cannot conclude that this is the case for observed deviations from predicted behavior then we are unable to reject the theory generating those predictions" (Harrison, 1989, p. 761).

The flat maximum critique was not the first criticism that Harrison hurled into the waters of experimental economics. Through his distinguished career a distinct thread of a series of methodological appraisals can be identified. The common feature of these criticisms was that they raised issues with insufficient experimental control and poor operationalization of the assumptions of tested theories.[445] For instance, Harrison together with McKee criticized the experimental work on the Coase theorem done by Hoffman and Spitzer (Harrison and McKee, 1985a, Hoffman and Spitzer, 1982). Hoffman and Spitzer claimed support for the Coase theorem despite the equal splitting of payoffs among the subjects. Although such an outcome maximized the joint payoff of the subjects involved, it did not maximize their individual payoffs. Harrison and McKee showed that it stems from the subjects' misunderstanding of their unilateral property right, but also from offering subjects a too small payoff and payoff differences among alternatives. Once the issue of payoffs and payoff differences had been adjusted, the data supported the Coase theorem and sensitized Harrison to the issue of sufficient payoff control of subjects' preferences.[446] However, the major motivation for the flat maximum critique in the 1989 AER paper came when he conducted a Bayesian analysis of risk attitudes in first-price auctions, which suggested the calculation of foregone expected profit (Harrison, 1990).

[445] For more recent publications that can be added in experimental research to this thread of exposing a potential loss of control there are papers on issues related to elicitation of subjective values HARRISON, G. W., HARSTAD, R. M. & RUTSTRÖM, E. E. 2004. Experimental Methods and Elicitation of Values. *Experimental Economics,* 7**,** 123-140. An appraisal of neuroeconomics HARRISON, G. W. 2008a. Neuroeconomics: A critical reconsideration. *Econ. Philos. Economics and Philosophy,* 24**,** 303-344..

[446] Harrison and McKee also criticized Coursey, Isaac and Smith for failing to properly implement the contestable market hypothesis. HARRISON, G. W. & MCKEE, M. 1985b. Monopoly Behavior, Decentralized Regulation, and Contestable Markets: An Experimental Evaluation. *The Rand Journal of Economics,* 16**,** 51-69, COURSEY, D., ISAAC, R. M. & SMITH, V. L. 1984. Natural Monopoly and Contested Markets: Some Experimental Results. *Journal of Law and Economics,* 27**,** 91-113.

In Harrison's recollection, he wrote the 1989 paper "deliberately attacking the analysis in their [CRS's] study because their research was the Everest at the time to me, and still is. If they have this [payoff dominance] problem, the rest of us do." [447] However, the paper did not explicitly state that it was aimed at CRS, for Harrison believed that his criticism applied generally to most experiments. Yet the way that Harrison was presenting his arguments managed to upset some people.

Among the "upset people", of course, were CSW. In April 1986, before Harrison's submission to the AER, CSW submitted a paper entitled *Theory and Individual Behavior of First Price Auctions* which was rejected five months later. They were advised to publish it in a more specialized journal. Ashenfelter wrote defensively, "I enclose two reports on your paper by economists familiar with its topic and who have no obvious ax to grind."[448] A resubmitted and expanded version was again rejected by the AER in 1987 and it finally got published a year later in the first issue of a new journal the *Journal of Risk and Uncertainty* (Cox et al., 1988). On the other side, in 1987, Harrison's paper *Theory and Misbehavior of First Price Auctions*, a clear reference to CRS, was accepted by the AER. It was written in 1986, submitted to the AER in January 1987 and published in September 1989.

Until the 1980s no economists with a track record of experimental research sat on the AER's editorial board. The first one was Al Roth, who served from 1986 until 1988, which was precisely the period when he finalized the decision to pursue a career as an experimental economist in addition to being an already well-established game theorist.[449] John Kagel, who served from 1989 until 1995, then followed him. Ever since at least one and since the 2000s more than two experimentalists have been members of the AER's editorial board. While this does not imply that either Kagel or Roth was one of the referees of CSW or Harrison, it allowed CSW to interpret Harrison's critique as an indirect extension of KR's criticism of their research.

[447] Harrison Interview.
[448] Box 124, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.
[449] See Footnote 114 for more information.

6.6 Comments on Harrison and his Reply

What appeared as comments in the AER in 1992 were toned-down and more dispassionate versions of original submissions, though still heated. Instead of looking at all four comments and elaborating on all the points raised, I will focus primarily on Daniel Friedman's comment. Everyone agreed that the subjects' motivation and the costs of decision-making are important issues in experimental research, but they also agreed that Harrison pushed his case for preferring pay-off space to message space too far.[450] Harrison "pleaded guilty of youthful grammatical excess" for insisting on the payoff space being a more natural aspect to evaluate than the message space (Harrison, 1992, p. 1435).

Harrison commenced his graduate studies at UCLA in 1978 and Daniel Friedman was hired as an assistant professor in 1979. At Harrison's suggestion, Jon Salmon and Friedman started experiments on asset market price formation.[451] This was both for Harrison and Friedman their first experimental publication. Later on, when Harrison was writing what became the 1989 AER paper, Friedman was originally co-author. Friedman in an interview recalled that in part he considered Harrison's choice of words "intemperate" and also disagreed with some of his statistical analysis. There is, for instance, a statistical flaw in Harrison's argument which is seldom acknowledged. Suppose there are n big deviations. Then add n + 1 small deviations. The fact that the median deviation is then small is uninformative. Friedman recalls that "I thought [it] was misleading or sort of making a case without sort of going down the middle on what the evidence was really saying. So we got a divorce on that and he took it his own way."[452] Because this initial involvement of Friedman was a well-known fact to other experimentalists at the time, he decided to present his views in a comment.

Friedman's main point was that instead of basing the statistical analysis of individual bid deviations on their means (CS) or medians (Harrison), one should, given the non-Gaussian distribution of deviations, consider all bids.[453] This would allow constructing another

[450] The fact that all the comments disagree with Harrison should not be surprising. Comments typically voice disagreement and hence suffer from selection bias.

[451] See my previous discussion of the 1982 paper by Forsythe, Palfrey and Plott at nearby Caltech; they were also working on the topic of experimental tests of asset markets.

[452] Daniel Friedman Interview.

[453] This would also address the abovementioned statistical flaw which had prevented their writing the 1989 AER paper together with Harrison.

payoff metric or a loss function which would transform the data from the message-space by weighting each message (bid) by its payoff consequence. However, Friedman did not apply his loss function to CS's or Harrison's data. While CSW recommended measures of forgone payoffs as "useful heuristics to aid in experimental design," they dismissed Harrison's metrics as "uninformative." However, they agreed that Friedman's loss function could be "a useful heuristics" (Cox et al., 1992, p. 1392 & p. 1394). KR, for their part, believed that Harrison had provided a useful "diagnostic tool for determining when experimenters are likely to have lost control over subjects' incentives" (Kagel and Roth, 1992, p. 1379).

Friedman finished his comment with a reflection on how to adjust theory in light of countervailing evidence:

> "I suspect that Harrison's (1989) criticism springs largely from an uneasiness with the specific direction CSW have taken in modifying the theory, an uneasiness I share. Preferences of course are unobservable, so modifying the theory by generalizing the preference specification permits only relatively weak tests. Indeed, arbitrary special utility effects would "explain" virtually any sort of data, in the same sense that epicycles explained planetary motion before Copernicus. CSW's special effects are not completely arbitrary but they still make me uneasy" (Friedman, 1992, p. 1376).

He then went on to suggest that learning could be an alternative explanation of the observed data and overbidding could be "a transitory (albeit long-lived) disequilibrium" (Friedman, 1992, p. 1377).

CSW countered Harrison's (and also KR's) claim that their research disregarded the motivational issues for the subjects. Payoff transformations, the introduction of models with nonstandard motivation, and preference hypotheses such as CRRAM* were designed to investigate the relationship between subjects' behavior and monetary payoffs. Even in 1982, with the earliest experiments that used computers, CRS were attempting to control the subjective costs of "thinking, calculating, deciding, and transacting" by holding the expected payoff constant across experiments (Cox et al., 1982a, p. 15).[441] But in the same breath they acknowledged that because "neither utility nor the nonmonetary factors are observable," holding the ratio of the utility of the monetary rewards and that of nonmonetary factors invariant cannot be guaranteed (Cox et al., 1982a, p. 18).

Harrison spent the first half of his reply to his critics on restating his criticism and demonstrating that the problem of insufficient dominance of the experimental reward over subjective costs made itself felt in a variety of experiments, such as eliciting certainty equivalents with the Becker, DeGroot and Marschak mechanism (see Footnote 413), Kahneman and Tversky's prospect theory, endowment effect experiments, or predatory pricing experiments. He cast his criticism as a defense of "many of the fundamental tenets of economic theory" that, clearly referring to the rising tide of behavioral economics research in the 1980s, were "under severe attack in recent years" from the observation "of apparently robust behavioral anomalies in decisions that experimental subjects make in controlled environments" (Harrison, 1992, p. 1426). However, Harrison was not targeting behavioral economics alone; his point was more general. Like overbidding in first-price auctions, he argued, all these anomalies could be a consequence of the small costs of deviating from the theoretical predictions. Harrison also specifically discussed choice under risk and its systematic violations, such as the Allais paradox, the preference reversal phenomenon, and Bayes' rule (Harrison, 1994). Exposing these experiments as satisfying neither salience nor dominance was Harrison's broader goal (see Footnote 409).[454] Therefore Harrison strongly disagreed with KR's general assessment that experimental research was doing well in economics.

One of the problems with Harrison's critique is that he did not provide an alternative theory as to why subjects fail to bid optimally. In particular, if they can reach the flat area in the neighborhood of the optimum, why can they not choose the optimum and why do they miss it systematically? All commentators agreed that they knew little about the "costs and processes of decision making and learning" (Harrison, 1992, p. 1440). Twenty years later, Colin Camerer remarked:

> "Unfortunately, no one knows quite what to do about it to make the maximum steeper. You could do it by paying a lot of extra money or by some kind of nonlinear transformation, but often then you're not testing the theory quite as straightforwardly. And there are a number of cases like mixed strategy equilibrium, where if people are at equilibrium there's a zero cost in deviating."[455]

[454] Kagel in his assessment of auction research in the Handbook of Experimental economics opined that Harrison's stance seems like an excessively strong protection for the standard economic theory, which might have many inherently flat maxima. KAGEL, J. H. 1995. Auctions: A survey of experimental research. *In:* KAGEL, J. H. & ROTH, A. E. (eds.) *Handbook of Experimental Economics.* NJ: Princeton Univ. Press.
[455] Colin Camerer Interview.

Indeed, experimenters were aware of this issue from early on. Siegel, for instance, was the first to note the importance of monetary payments in economics experiments as a way of exercising better control over the subjects' motivation (Siegel et al., 1964). Even the issue underlying the flatness around the maxima of decision strategies had already been pointed out in a working paper by Detlof von Winterfeldt and Ward Edwards, which was never published. They concluded that flat maxima are present in "virtually all decision theoretic paradigms" (Winterfeldt et al., 1973, p. 40). Harrison became aware of this research only after his 1989 AER paper.

6.7 The Showdown

During the 1988 and 1989 annual meetings of the newly established *Economic Science Association* in Tucson, Arizona, the protagonists of the controversy met and openly exchanged their opinions. After one such session, Raymond Battalio told James Cox that "he was going to hang a sign over the meeting room calling it the O.K. Corral" in reference to the most famous gunfight in the history of the American Old West, which also took place in Arizona.[456]

The controversy swept the whole experimental community. The five comments in the AER pitted against each other nine prominent experimentalists, including ESA's first President, Smith, and four future Presidents. In the early days of the ESA almost all experimentalists made the annual trip to Tucson and the meetings had only one parallel session. So the flat maximum critique and the dispute between CSW and KR were unavoidable and everyone had to ponder about the intellectual issues at stake.

The personal stakes were also considerable and were mingled with the intellectual ones. Roth's major innovation, the binary lottery procedure, was deemed by CSW potentially unreliable, thereby questioning his research on testing Nash's theory in bargaining. Roth finally made commitment his commitment to pursue experiments and establish a major center in Pittsburg (alongside Caltech and Arizona). John Kagel joined him in 1988. Kagel's auction research made him suspicious and uneasy about CSW's pursuit of risk aversion and what seemed to Kagel their disregard of learning and the errors that abounded in the common value auctions that he specialized in. In late 1989/early 1990 Kagel was drafting

[456] James Cox Interview.

his survey chapter on auctions which appeared in the *Handbook of Experimental Economics* being edited by Roth and himself. He remarked, “a number of experimenters have taken the flat maximum critique as a frontal attack on auction experiments (and in some cases on the entire field of experimental economics)” (Kagel, 1995, p. 535).

Indeed, the flat-maximum controversy coincided with the persistent problems in getting CSW’s auction research published in the very top journals after the CRS paper. KR’s rebuttal of CSW, disguised as a comment on Harrison, gave CSW the impression of being under attack. This was magnified even more by the fact that they were very proud of their research. Smith, for instance, preferred to publish the CRS paper in the second *Research in Experimental Economics* volume, a book series that he launched in 1979, because it allowed unabridged accounts of experimental research to be published. For Cox this was his first experimental publication which he was keen to have had in a top journal. But he followed the advice of Smith, who wanted this paper “to be an example of how people ought to do experimental economics” and it was published in full.[457]

For Smith, their first-price auction research was an example of a virtuous circle of theory and evidence mutually challenging and disciplining each other. He hailed such virtuous circles in his 1986 *Prologue to Economics Science Association* which paved the way for the foundation of the ESA.[458] The CSW research agenda represented in Smith’s eyes the way that science should be done. They followed Lakatos’ concept of a progressive research program.[459]As I have argued in Chapter 4, the establishment of the ESA as a society dedicated to “economics as an observational science” was marked by a protracted debate about the society’s name, the significance of experimentation in economics and the relationship between experimentalists and the vast majority of economics researchers. Kagel and Roth were on the side which opposed the use of the word ‘science’ in the title of the society and rather preferred something more descriptive such as the *Experimental Economics Association*. The first-price controversy therefore should be viewed in this context as part of these debates.

[457] James Cox Interview.

[458] “Better theory is demanded by almost every empirical study, and new experiments should be definable from the new empirical implications of every advance in testable theories.” Prologue, p. 6. Box 38, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

[459] In a letter exchange between Roger Noll and Vernon Smith during the preparation of ESA in July 1986, they both agreed that: “there is a need to develop the implications of the work of [the Lakatos and Feyerabend type] for economic science. Lakatos was interested in such a project before he died, but he really didn't know how to involve *the right economists* in this exercise.” Smith to Noll, July 17, 1986, Box 11, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. My emphasis.

On the epistemic level, below all the intricacies of the data analysis, the validity of experimental procedures and control, stands the central issue of what counts as rigorous empirical evidence. This issue has an immediate influence on how one modifies theory in light of countervailing evidence and what counts as countervailing evidence. The empirical issue of overbidding invited a discussion of the relative importance of risk aversion (an obvious suggestion following on from Vickrey's theory) and other factors. The answers provided by CSW and KR depended on whether the experimental bidding data reflected either equilibrium or learning (moving from off-equilibrium to equilibrium). KR drew on learning because it had been suggested by the persistence of the winner's curse. CSW took the equilibrium path because it had been their theoretical starting point, underlying Vickrey's model. This suggested a focus on equilibrium bidding with risk aversion. Subsequent observations motivated by CRRAM were capable of explaining the data. Moreover, Smith's experience with double auctions and their convergence to equilibrium under much weaker conditions than the partial equilibrium theory required, informed his beliefs about pursuing risk aversion. Once the equilibrium assumption collided with expected utility theory through the lottery procedure, with all the evidence for (including some anomalies undermining) risk aversion CSW reasoned that the culprit was the lottery procedure and its crucial dependence on the reduction axiom (of compound lotteries).

KR equally naturally took the opposite view. In particular, abandoning the reduction axiom would jeopardize a central tenet of game theory.

> "However, for people who prefer theories that organize a broad range of experimental data, no matter how well they might fit a small subset of the data, and who do not hold to the implicit assumption that all experimental data correspond to some equilibrium point prediction, these data are inconsistent with the risk-aversion hypothesis. The reason is that for expected-utility maximizers the binary lottery technique must be capable of controlling risk preferences. Of course, one explanation that preserves expected-utility theory and is consistent with overbidding and the binary lottery data is that subjects are expected-utility maximizers who overbid for reasons other than risk aversion, for example from bidding errors or out-of-equilibrium behavior similar to that observed in second-price auctions" (Kagel and Roth, 1992, p. 1383).

Even though empirical counterevidence is well known (Allais paradox, etc.), in Roth's opinion it still remained a useful approximation.[460]

CSW and KR also differed in the goals that their experiments were pursuing. CSW saw risk-aversion as a rigorous empirical foundation for their research – both theoretical and experimental. Further development of theories relying on the risk neutrality assumption was in their eyes perhaps an interesting exercise, but not a productive one.

> "If most people are naturally not risk-neutral, then it is necessary for the economist to develop non-risk-neutral models. Although it is possible that testing risk-neutral models by inducing risk-neutral behavior can contribute to this theory development process, it is more likely to provide a deceptive empirical justification for further and more extended dependence on the risk-neutral hypothesis" (Walker et al., 1990, p. 7).

When CSW rejected the lottery procedure, KR accused CSW of having too strong priors in favor of risk aversion and CRRAM. Where CSW saw an empirically progressive research program, CSW, in which evidence against CRRAM spurred on their modifications and additional tests, KR saw data fitting:

> "the superior fit of the CRRAM model in first-price auctions results from its larger number of free parameters than the RNNE [risk-neutral Nash equilibrium] or homogeneous risk-aversion alternatives and thus does not directly test the core hypotheses of risk aversion and equilibrium bidding" (Kagel and Roth, 1992, p. 1389).

Moreover, the issue of reporting practice and data analysis of CSW also made KR weary of CSW. Though without any direct reference, Roth addressed these issues publicly a few years later (Roth, 1994).

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[460] When Roth asked why these demonstrated anomalies had not swept away utility theory in the introduction to the *Handbook of Experimental economics* that he wrote at the turn of the 1980s, he noted: "In summary, there exists a substantial body of experimental evidence that shows that individuals are not ideally rational utility maximizers; on the contrary, there are a growing number of systematic violations of utility theory that can be robustly demonstrated and reliably replicated in the laboratory. Nevertheless, even a brief review of the contemporary economic literature reveals that economists remain by and large quite content to model individuals as utility maximizers. What can account for this? I've argued here that, to the extent that utility maximization is viewed [as a] useful approximation of behavior, it can't easily be displaced by counterexamples, since approximations always admit counterexamples. The experimental evidence is nevertheless very valuable - it is of the utmost importance to know where approximations break down" ROTH, A. E. 1995b. Introduction to Experimental Economics. *In:* KAGEL, J. H. & ROTH, A. E. (eds.) *Handbook of Experimental Economics.* Princeton University Press. P.78.

A discussion of the showdown would be incomplete without considering the role of Orley Ashenfelter, the editor of the AER. First, he made the decision to publish Harrison's piece, which triggered the whole 1992 series of comments. As an editor, he acted as a sort of cooler and mediator who helped to tone down the papers. Ashenfelter has had the longest tenure as editor of the AER in the post WWII period (1985-2001). Interestingly, Ashenfelter had some direct experience with experimentation. He had published two experimental papers. The first contained a policy field experiment. (Ashenfelter, 1978) The other was a laboratory experiment which was conducted at Smith's *Economic Science Laboratory* in the summer of 1984 and winter of 1988 (Ashenfelter et al., 1992). Ashenfelter et al. referenced the flat maximum critique of Harrison, Roth's research on bargaining among others, suggesting that despite being a labor economist and applied econometrician he had a command of the experimental literature. The exact nature of his involvement and of the referees must remain confidential until 2042 at the latest, when the fifty-year moving wall of AER editorial considerations will no longer apply.

As experimental research was gaining momentum in economics journals, concern about its merit was growing. For instance, in a letter to Charles Plott, John G. Riley, the AER's co-editor (1983-84) and later associate editor (1985-87), stated referring to the Lynch et al paper: "I have talked to a lot of economists about the upsurge in experimental research and find that they are generally more dubious than I am."[461] His concern at that time was "about the relevance of experimental results as predictors of behavior in market environment when the stakes were much higher. For example estimates of risk aversion in laboratories (e.g. asset choices) tend to be far higher than in markets."[462] However, doubts were not just about substantial issues related to experimental research (e.g. differences between behavior inside and outside of labs, insufficient stakes in experiments) as we saw in the previous chapter. There was also suspicion among non-experimentalists that experimentalists were lenient with each other. Such sentiment was voiced by other editors. In the context of the first-price auction controversy, Colin Camerer recounts:

> "I remember a conversation with an editor of a journal, a dinnertime conversation, with somebody saying, 'A lot of us have wondered whether the

[461] Letter from John G. Riley to Charles Plott, October 4, 1984. Plott Papers.
[462] Email communication with the author. October 13, 2014.

> experimentalists all accept each other's papers and they're not very tough in criticizing one another. And this guy Harrison seems to be like a whistle-blower who's now shown that a lot of what the experimentalists do is really suspect.' So there was suspicion from people outside experimental economics. Remember, this is also a time in the mid-'80s when very few major economics departments had experimentalists; grad students weren't doing as much as they are now at lots of schools. So there was a feeling that there's this strange process that goes on at a few schools: at Caltech and Texas A&M, Pittsburgh and Arizona. And maybe they're all being too easy on one another, but Harrison was exposing this corrupt practice."[463]

Therefore, the controversy precipitated by Harrison's paper allowed critical light to be shed on the experimental economics community. But the fierce mutual criticism of the experimenters involved demonstrated that the concern about possible leniency, if not tacit collusion, was unfounded and the internal control of the experimental community was strong enough.

The controversy also portrays experimental research as a messy and dynamic activity. It features a flurry of working papers, the replication of other researchers' results, new experiments designed to address outstanding or new questions, with a permanent crossing of the roles of the experimentalist and the theorist. The interesting aspect of the debate is that KR and CSW agree that when inconsistencies between theory and evidence arise, the cause can be inadequacies in the theory or inadequacies in the experimental test procedures. They even agree that the established theory must not be thrown overboard until an alternative is ready. But they disagree where and why the inconsistencies arise. Their different epistemic convictions about equilibrium and learning, the nature of economic theory in relation to evidence could be found at the heart of the dispute.

[463] Colin Camerer Interview.

6.8 Impact of the Controversy

At the 1992 January AEA meeting, which took place a month after the comments on Harrison had appeared in the AER (and roughly a year after they were approved for publication), a joint session of ESA and AEA took place with Plott, Kagel, and Smith presenting. They had moved on with their research to issues not yet experimentally explored. John Kagel discussed English common value auctions, Vernon Smith his recent research on fairness in ultimatum and dictator games, and Charles Plott talked about his overlapping generations experiments.[464] The personal heat that marked the controversy was gradually dissipating, people once again started talking to each other, but still some of the intellectual issues have persisted.

For instance, CSW were throwing away a small portion of the data, above a certain level, and they estimated only the linear part of the CRRAM bid function. Although KR in their comment incorrectly argued that up 25% of the data might have been discarded, they pointed to an important data analysis issue. CSW did not know how to deal with the non-linear part and acknowledged that the individual bid data (not pooled ones, as KR suggested) should be analyzed differently. The paper by Cox and Oaxaca did this. It was the first to make use of the loss function suggested by Friedman which accounts for the whole distribution of bids (Friedman, 1992, Cox and Oaxaca, 1996). They also developed a computational spline function method which could deal with all the bid data. They concluded that "Tests with individual subjects' bids and values indicate that data for about 48% of the subjects are consistent with the constant relative risk averse model and data for almost all subjects are consistent with the log-concave model" of first-price auctions. Only zero to 10% (depending on the statistical test) of the subjects conformed with the risk neutral model (Cox, 2008, p. 96).

Second, all experimentalists agree that having a reliable method for controlling risk preferences would be most useful for testing the decision models under risk. The lottery procedure was tested repeatedly. A recent summary concludes that in first-price sealed bid auctions:

> "the experiences of subjects affect how the inducing technique performs. Experience with monetary payoffs appears to dampen the effect of the induction

[464] It should be noted that Charles Plott was not directly involved in the controversy. There is no evidence whether he acted as a referee at some point.

technique so much that results differ little from those observed under monetary payoffs. This appears to be a hysteresis effect resulting from the prior monetary payoff auctions because the results come significantly closer to the risk neutral prediction when subjects have no previous auction experience" (Berg et al., 2008, p. 1095).

Additional experience with second-price auctions accelerates the convergence. In the case of choice between paired gambles, there is a "strong support for the performance of inducing" risk preference (Berg et al., 2008, p. 1095). This suggests that both positions have been accommodated. CSW were right that the lottery procedure does not work with first-price auctions, but the result does not transfer to Roth's research on bargaining or Kagel's on second price or common value auctions.

Third, KR and Friedman advocated a model of first price auctions that would take learning into account. Until recently CRRAM was the only alternative to Vickrey's theory; there were no theoretical models that incorporated learning. Reinhard Selten and his co-authors have developed a learning direction theory which capitalizes on the possibility of learning and uses a bounded rationality approach to bidding in first-price auctions (Neugebauer and Selten, 2006). Finally, as a result of the controversy, Harrison withdrew himself for a while from the experimental community, but has so far successfully continued with experimental research.[465]

6.9 Conclusions

This chapter dealt with the most important internal contention of the experimental economics community to date – the controversy about the nature of bidding in first-price auctions. What should we make of the first-price auction controversy? Was it a "Metric War," "a tempest in a tea cup," "a product of outsized personalities," a "collision of major research programs," or "the loudest debate amongst experimentalists ever heard"? I have argued that it functioned at a more fundamental level than all these descriptions of this multilayered episode would suggest.

[465] For instance a paper co-authored with Elisabet Rutström won the 2010 Editor's Award for Experimental Economics HARRISON, G. W. & RUTSTRÖM, E. E. 2009. Expected utility theory and prospect theory: one wedding and a decent funeral. *Experimental Economics,* 12**,** 133-158.

The controversy contributed towards the acceptance of experimental economics because it strengthened the trustworthiness of the experimental community and increased the trust of the rest of the profession in experimenters. This is slightly paradoxical when one considers that some of the motivation and source of contention between the various camps of the controversy stemmed from not trusting what others were observing, what they considered rigorous evidence and rigorous control, and the reporting and analysis of data. However, not shying away from openly waging a “war” erased any possible suspicion of collusion among experimental economists and thereby made their work more transparent to those who did not have direct experience with economics experiments. This demonstrates that the issues of rigorous experimental control and influence over the experimental community including its image among the rest of the profession present two sides of the same coin - trust in the data and in the interpretation of experiments, mutual trust among experimentalists, and trust of the rest of the profession in the scientific conduct of the experimental economics field.

7 The Experimental Turn of Economics Revisited

On December 4, 2001, a dozen leading experimental economists descended on Stockholm from all cardinal directions to extol their research for two days and ponder about their future. Some had no doubts why they were invited. They were the pioneers of one of the most important transformations of economics in the 20th century. They had laid down the intellectual foundations of experimentation in economics, they conducted landmark economics experiments, and they had initiated applied research with far-reaching consequences on both the discipline and the economy. Some of the attendees wondered why perhaps other equally accomplished researchers had not been selected. A few were somewhat perplexed that a dozen of behavioral economists had been invited as well. Yet all were there, the first time in such a format, anxious to be together and knowing that a much larger prize was at stake; ready to take part in the Nobel Symposium on Behavioral and Experimental Economics. [466]

The year after a Nobel Symposium on Game Theory was held in 1993,[467] three game theory pioneers, including Reinhard Selten, were awarded the Nobel Memorial Prize.[468]

[466] Twelve economists and psychologists were invited to deliver a talk: Colin Camerer, Ernst Fehr, Daniel Kahneman, David Laibson, George Loewenstein, Charles Plott, Matt Rabin, Howard Rachlin, Alvin Roth, Paul Slovic, Vernon Smith and Richard Thaler.
Discussants at the Symposium were: George Akerlof (Berkeley); Roland Benabou (Princeton); Dan Gilbert (Harvard); Jean-Jacques Laffont (Toulouse); Charles F. Manski (Northwestern); James Mirrlees, Trinity College, Cambridge; Sendhil Mullainathan (MIT); Thomas Palfrey (Caltech); Amnon Rapoport (Arizona): Ariel Rubinstein (Tel-Aviv & Princeton); Reinhard Selten (Bonn); Robert Shiller (Yale);
Other participants included: William Sharpe, Graduate School of Business, Stanford University; Robert Solow, Department of Economics, MIT; Jonathan Baron, Department of Psychology, University of Pennsylvania; Charles Holt, Department of Economics, University of Virginia; Werner Gueth, Humboldt University; Drazen Prelec, Department of Psychology, MIT; Kalle Moene, Department of Economics, University of Oslo; Peter Bohm, Department of Economics, Stockholm University; Martin Dufwenberg, Department of Economics, Stockholm University; Tore Ellingsen, Department of Economics, Stockholm School of Economics; Peter Juslin, Department of Psychology, University of Umea; Per Krusell, University of Rochester and IIES, Stockholm University; Assar Lindbeck, IIES, Stockholm University; Henry Montgomery, Department of Psychology, Stockholm University; Lennart Sjoberg, Department of Economic Psychology, Stockholm School of Economics; and Members of the Prize Committee
The Organizing Committee of the Symposium consisted of: Colin Camerer, Tommy Gärling, Lars-Göran Nilsson, Torsten Persson, Jean Tirole, and Jörgen Weilbull Source: Plott Papers.
[467] Nobel Symposium on Game Theory: Rationality and Equilibrium in Strategic Interaction. Bjorkborn Manor, Sweden, June 18-20, 1993. Speakers included: *John C. Harsanyi*, *Reinhard Selten*, Cristina Bicchieri, David Kreps, *Alvin Roth*, Paul Milgrom, *Robert Aumann*, *Eric Maskin*, Ken Binmore, Larry Samuelson, Ehud Kalai, Ingold Ståhl, Ariel Rubinstein, and John H. Holland. Source: Box 1, Alvin Roth Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

Hence the expectations in 2001 were clear. The time was ripe for a prize for experimental economics, behavioral economics, or both – depending on whom you asked at the time. With several outstanding contenders present, the symposium had the taste of a beauty contest.

Indeed, a year later, in the autumn 2002 two symposium participants were awoken to the news of being admitted to the exclusive club of Nobel laureates. The Prize was shared equally between the cognitive psychologist Daniel Kahneman "for having integrated insights from psychological research into economic science, in particular concerning human judgment and decision-making under uncertainty" and the experimental economist Vernon Smith "for having established laboratory experiments as a tool in empirical economic analysis, in particular in the study of alternative market mechanisms" (Nobelprize.org, 2014).

This award marks a symbolic end to a long tradition of defining economics as a non-experimental discipline – a discipline which cannot systematically capitalize on data created through experimental control and intervention. Starting shortly after its inception by the English classical political economists of the mid-19th century such as John Stuart Mill or John Elliot Cairnes[469] and extending to the mainstream economists of the late 20th

[468] As Economics was not included in the original list of fields listed in Alfred Nobel's will, the Economics Prize, introduced in 1969, is officially titled the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel. The other two 1994 laureates were John Harsanyi and John Nash.

[469] Perhaps the first to exclude experimentation from the domain of economics was John Stuart Mill. In both On the Definition of Political Economy and his highly influential A System of Logic MILL, J. S. 1843. *A system of logic, ratiocinative and inductive: being a connected view of the principles of evidence, and methods of scientific investigation,* London, J.W. Parker., he asserted the distinct and incompatible nature of the social ("moral and psychological") sciences and the natural sciences. Whereas the latter operate inductively through experiment and observation, the latter, including economics, proceed deductively from a priori principles and "it is seldom in our power to make experiments in them" Political economists lack the experimental "power," or, in modern parlance, experimental control. MILL, J. S. 1844. *Essays on some unsettled questions of political economy, by John Stuart Mill,* London, J. W. Parker.

> "How, for example, can we obtain a crucial experiment on the effect of a restrictive commercial policy upon national wealth? We must find two nations alike in every other respect, or at least possessed, in a degree exactly equal, of everything which conduces to national opulence, and adopting exactly the same policy in all their other affairs, but differing in this only, that one of' them adopts a system of commercial restrictions, and the other the adopts free trade."

Capitalizing on Mill, John Elliot Cairnes in his *Character and Logical Method of Political Economy* asserted that in economics observational control, and more specifically experimental control, cannot be achieved and used as a source of inductive generalizations:

> "[T]he utter inadequacy of the inductive method … as a means of solving the class of problems with which Political Economy has to deal, arising from the impossibility of employing experiment in economic inquiries under those rigorous conditions which are indispensable to give cogency to our inductions." CAIRNES, J. E. 1875. *The character and logical method of political economy,* London, Macmillan and Co.

century, including such luminaries as Milton Friedman and Paul A. Samuelson,[470] the idea of experimentation was dismissed. Take for instance John Neville Keynes, the father of the famous John Maynard Keynes. His *The Scope and Method of Political Economy* (1891) provided the most authoritative statement of the appropriate methods of neoclassical economics in the late 19th century and served as a companion volume for Marshall's Principles. The room for experimentation that Keynes could envision was fairly limited. The division of labor, its relations with workers' dexterity, or the law of diminishing returns could be examined experimentally. There was however a major problem with such experiments which practically debarred economics from the experimental method.

> "[T]hese are problems that lie only on the threshold of economics. Indeed, some of the laws thus determined by experiment may be regarded as data given to economics by other sciences, rather than as conclusions obtained by it. … [E]ven when some kind of experiment is possible, our power of controlling and varying the concomitant circumstances is very limited; nor can the experiment be freely repeated" (Keynes, 1891, pp.171-2).

Keynes could not imagine how to conduct experiments – both what could be studied experimentally and how to implement control so that the resulting data have the required qualities. A century later the pioneers of experimental economists revisited Keynes' judgment. Imagining or simply trying out experiments in economics was, however, not enough to change economics into an experimental discipline. It required a considerable reconfiguration of what counts as valid evidence and the way in which such evidence is generated. Furthermore, with the postwar rise of formal modeling and theorizing, early experimentalists had to recast the relationship between theory and experimental evidence.

The 2002 Nobel Prize summary statements and the press release clearly make an interesting distinction between the two recipients. Kahneman and by extension behavioral economics were spoken of in such words as "theory" and "insights" but not with reference to 'methods' and 'tools.' Smith and by extension experimental economics were, in contrast, spoken of in terms of "methods" and "tools," but not 'theory' and 'insights' (Nobelprize.org, 2014). What both shared in the press release is the reference to observation. Although it is generally agreed that the 2002 Prize was awarded for

[470] FRIEDMAN, M. 1953. *Essays in positive economics,* Chicago, Ill., University of Chicago Press, SAMUELSON, P. A. & NORDHAUS, W. D. 1985. *Economics,* New York, McGraw-Hill.

developing new methods in economics, it would be better viewed as a prize for observation and observing in economics.

In many different guises, my dissertation is indeed about observing and observation. The case of the experimental turn is neither about replacing field and econometric research with experimental research nor about introducing the experimental method to economics. It is mainly about a claim that is deeply normative and at the same time empirical; namely, that rigorously controlled data collected in the laboratory or the field is a better foundation for economic theorizing than other types of data, evidence or observation. It was not a “battle” to establish experimentalism in economics, but a struggle to (re-)establish integrity of observation and the symmetry with theory.[471]

Earlier methodological or historical accounts of experimental economics share a unique emphasis on theory first. My dissertation shows that, it was rather the converse, and had been so from the very start. “Giving a theory its best shot” is not about the theory; it is about the data and the quality of the data. Experiments finally produced data that were fit to test theory, data that were rigorous enough. These were not haphazard data, but data that had been geared to theory, so that theory could not escape. The methodologist’s reflex counterargument is to say that it is still theory after all. But this is exactly what it is not. The emphasis on rigorous observation was aimed by experimentalists to produce data that could turn the development of theory in economics into something it had not been until then – science.

The first-price auction controversy, i.e. the contention among experimentalists about the nature of bidding in first-price auctions in the late 1980s and early 1990s, is an outstanding case in point since it comprehensively amalgamated these issues of theory, rigorous observation and control. To understand this complicated and multilayered episode, it is useful to look at it through the various layers of the question “Do you see

[471] Observation and experiment have often been contrasted with one another as if they were distinct methods of obtaining knowledge. The volume produced by the research group on the history of observation in the sciences suggests that such a contradistinction is historically inaccurate and the scientific practice far more diverse. DASTON, L. & LUNBECK, E. 2011. *Histories of scientific observation,* Chicago, University of Chicago Press.. J.N. Keynes dispelled this false contrast most eloquently:

> “But this is of course not the case. Experiment is nothing more than the process of deliberately producing phenomena for ourselves, so that we may be enabled to observe them under the most advantageous circumstances. In experiment we have a control over the phenomena under investigation, and generally a far more precise knowledge of the conditions under which they occur, than is possible in cases, where they are brought about independently of our own action.“ KEYNES, J. N. 1891. *The scope and method of political economy,* London; New York, Macmillan.

what I see?"[472] The first meaning of this question is "Do you observe what I observe?" This is the question about the ability to replicate experimental observations. It also contains the issue of sufficient experimental control, including the flat maximum critique. The second meaning is "Do you interpret what we observe as I interpret it?" In the context of the controversy this boiled down to two options. According to one, subjects are bidding above the Nash equilibrium prediction made on the basis of risk neutrality, thereby questioning the assumption of risk neutrality while maintaining expected utility maximization and equilibrium bidding assumptions. According to the other, the observed behavior is evidence of bidding errors and out-of-equilibrium bidding. The third meaning is "Do you conclude what I conclude from our interpretation?" This relates to the decision to pursue the Constant Relative Risk Averse Model discussed in Chapter 6 or to question the validity of the lottery procedure. This makes clear the stakes which magnified rival notions of experimental design and data analysis, the way in which they informed overlapping research programs and the fundamental concepts of experimental research such as control, rigor and intervention.

The fourth meaning entails the question "Does someone with little or no direct experience with the experimental method see what an experimentalist sees?" The first three meanings of the controversy were wrapped in the broader issue, the experimentalists' trustworthiness, as perceived by those journal gatekeepers who lacked direct experience with experimentation, but who had had to evaluate experimental papers. This entailed their determining the epistemic value of experimental observations in the face of doubts about them – such as differences between behavior inside and outside of laboratories, insufficient stakes in experiments, and students as primary experimental subjects. By presenting disagreement through the first-price auction controversy, the experimentalists showed that they were committed to resolving the doubts about the rigorousness of experimental data and research. Put differently, by openly waging a "war," as prominently displayed on the pages of the AER, experimentalists gained the trust of the rest of the economics profession, leading to their joining the economics mainstream.

[472] This discussion is motivated by Smith's unpublished manuscript entitled *On Replicability in Economic Research* from 1981. The first price controversy was years away and Smith posed only the first three out of the four questions mentioned below. The manuscript was presented at the Symposium on Reliability and Replication in Applied Economic Research, at the 1981 AEA Meeting. Box 139, Vernon L. Smith Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University.

The major synthesis of my dissertation lies in the introduction of the colligatory term 'the experimental turn in economics' that I use to organize and interpret a set of changes which economists and the practice of economics underwent as it became an experimental science. My thesis brings the period of the second half of the 1980s to the forefront of historical interest. A community of economists (broadly conceived) doing experiments on economic phenomena that fell within the scope of economics, laboratories, and the relationship to economic theory had in various degrees and forms been present in earlier episodes from the history of experimentation in economics – such as the agricultural economics experiments of the 19th century, managerial experiments, time and motion studies in the interwar period and experimental gaming shortly after WWII. Experimental economics was the first to combine all these elements. They have been wrought upon by what I have called the four driving forces of the turn: 1) integrity - expansion of the permissible type of data in economics by introducing experimental data and advocating its advantages; 2) rigorousness - personal collection of data under controlled conditions; 3) the virtuous circle - the realization that experimental research is most potent when it goes in tandem with economic theory; and 4) symmetry - placing experimental data on a par with economic theory.

What I attempted in this thesis was to show how these driving forces operated and interacted. I started my analysis with individual economists and their conversion by experiencing these forces. By making use of the turn's epistemic and sociological strands and their double helix like relationship, I first followed the reverberation of the driving forces on both the local and experimental community levels through the process of active reception and the emergence of laboratories respectively. Eventually, I traced the percolation of the driving forces through the whole discipline of economics through the establishment of the ESA and passive reception of experimental research in journals and funding agencies. This stacking up of various levels, cutting through them and moving them across time reflected the colligatory nature of the experimental turn.

My thesis and its approach improve our understanding of the nature, roles and operations of scientific communities. Focusing on the way in which the individual trajectories of early experimentalists traversed the academic space-time, intersected and was built up from individual researchers to an emerging community of economists who through their repeated interaction and continuous engagement with the experimental method were gradually grasping the full extent of the driving forces and their implications, shows us experimental economists as an epistemic community with its related ideas and practices. What makes this epistemic community unique is the non-existence of a common unifying subject matter that could provide an outline for a historical narrative. Instead the experimental community is centered on a research method which cuts across an ever-increasing number of economics subfields. These various subfields meet in the economics laboratory.

The experimental economics community is also a sociological entity with its structural operations, identity, delineation of boundaries and internal struggles for leadership. A noteworthy feature of this community is its unwavering determination to engineer its own future. This was perhaps most apparent in the considerations leading to the establishment of the Economic Science Association and the ensuing struggles with its name. But it also included for instance holding conference sessions in which the whole small community discussed strategies for getting papers published in leading economics journals, including specific arguments to use against rejections received; senior experimentalists passing out money for running experiments to prospective experimentalists; ongoing discussion whether to launch a specialized experimental economics journal that would not impede the publication success elsewhere; detailed negotiations about the ownership and publication conditions of such a journal; close ties with the NSF; the multilayered first-price auction controversy and many more. This self-consciousness about its image and standing within economics led experimental economics from the periphery of the profession's interest to its core and from a handful of second-tier departments to the economics mainstream.

The epistemic and sociological notions of a scientific community are not static, but have evolved over time. Their development is inextricably linked to what has become the heart of any local experimental economics community – its laboratory. This purpose designed and equipped space embodies the driving forces of the experimental turn. It became a shorthand for controlled rigorous observation and data collection under the supervision of the experimental economist - a new knowledge-generating site. The experimental control register contains a number of historically contingent elements - most notably the performance based subject payments, the no-deception rule and the use of computerized experiments.

Computerized laboratories became not merely the place where rigorous experimental data were collected, but a space, in de Certeau's sense, that needed to be experienced. It is a space with a complex social infrastructure based on division of labor with researchers, programmers, support stuff, and students and the practice of sharing resources. Laboratories helped in acquiring a style of continual experimentation and speeded up the question-generating capacity of experiments, engines of the virtuous circle between data and theory.

History of science as a discipline has changed altogether in the 20th century - from a new discipline almost exclusively focusing on internal accounts of scientific progress to a plethora of approaches intertwined with the philosophy of science, sociology of science, anthropology, or the history of culture and technology. In the 1970s and 1980s historians of economics influenced by these developments became enamored of the Kuhnian, Popperian, and Lakatosian frameworks and attempted to recast the history of economics along the lines that these frameworks provided (Blaug, 1985, De Marchi, 1991).

The assertion of an experimental turn in economics raises the question whether a more general framework could be envisaged. The colligatory nature of the experimental turn certainly invites such an interpretation. In fact, three different and increasingly general types of framework could be constructed: first, an experimental turn in discipline X, Y, or Z; second, there could be an experimental, mathematical, or statistical and econometric turn in economics – all redefining what counts as evidence, rigor and the role of economists in economics. Even far less ambitious transformations could be targeted. After all, the history of experimentation in economics is far from over. Its newer episodes, such as behavioral economics, field experimentation,[473] or neuroeconomics, could be scrutinized according to the template of the experimental turn.

Third, there might be some patterns of these various turns that are independent of the particular disciplines or that are shared by them. These could include aspects of passive reception, technologies of argumentation in particular;[474] but also the universal

[473] Early instances of field experiments include BOHM, P. 1972. Estimating demand for public goods: An experiment. *European Economic Review,* 3**,** 111-130, LEVINE, M. E. & PLOTT, C. R. 1977. Agenda Influence and Its Implications. *Virginia Law Review,* 63**,** 561-604, KAGEL, J. H., BATTALIO, R. C. & WALKER, J. M. 1979. Volunteer Artifacts in Experiments in Economics: Specification of the Problem and Some Initial Data from a Small Scale Field Experiment. *In:* SMITH, V. L. (ed.) *Research in Experimental Economics.* For an insightful classification of experimental research, see HARRISON, G. W. & LIST, J. A. 2004. Field Experiments. *Journal of Economic Literature,* 42**,** 1009-1055. John List's website http://www.fieldexperiments.com provides a comprehensive bibliography of field experiments in economics.

[474] Peter Galison defines technologies of argumentation as "the concepts, tools, and procedures needed at a given time to construct an acceptable scientific argument." He identified them as one of ten core areas where one needs to join the forces of the history and philosophy of science. GALISON, P. 2008. Ten problems in history and philosophy of science. *ISIS,* 99**,** 111-124.

machinery of the epistemic and sociological strands that are combined through the colligation of the various elements of a turn. Perhaps the way that modern science is organized prevents paradigmatic revolutions from taking place. Instead of such disciplinary phase transitions, scientific transformations such as the one brought about by experimental economists are turning into growing mutations. Such an understanding of scientific change entails “moving away from the stack periodization typical of conceptual schemes, radical translations, gestalt switches, and paradigm shifts[. It] comes at a price: we lose the vivid metaphorical imagery of totalistic transformations” (Galison, 1997, p. 830). Instead of yearning after the totalistic transformations, we might finally be able to weave narratives that effortlessly move across the various scales on which science operates from the local and personal to communal and discipline-wide.

8 Appendix: Distance and Sources in Writing Contemporary History

> "How does research in contemporary history differ from research into past history? My own opinion is, that apart from the obvious physical difference of interviewing live people instead of reading their published memoirs or unpublished papers, the difference in research is not very great. The real difference is in the stance and the intent of the historian. Where does he stand in relation to the events? Is he writing from inside or out, as participant or as observer? Is his intent basically apologia, or an attempt to collect the whole story and stand back from it so that he can see it in the round? The answer determines the research, or rather what is done with it, for what finally counts is not the research per se but what you do with it after you've got it" (Tuchman, 1996, p. 95).

In this methodological appendix I want to address the issue of the nature of the experiment in economics from the perspective of sources of which I as a historian can avail myself. While researching on this dissertation I had to continuously address a major limitation in the current accounts of the history of experimental economics: their reliance on the scant published secondary sources. One remedy for this limitation was the employment of three types of primary source - archival collections, interviews, and a witness seminar.

The historian's role gains a completely new dimension when the events he seeks to describe, interpret, and explain occurred in the not so distant past, as in the case of experimental economics. This recasts the fundamental historiographic issue of the relationship between the historian and his sources.

It was Edward H. Carr who famously asserted the need to study the historian before one studies his historical claims. "To re-enact what goes on in the mind of the historian," allows us to understand how he selects and construes the past (Carr, 1961, p.26). However, a historian of contemporary events like me, who faces the ineluctable issue of interacting with his dramatis personae is at risk of becoming an actor in the history. It's not that he alone re-enacts the past; his sources also respond to such probing. Let me illustrate these points using my own research.[475]

A historical inquiry on contemporary economics or any recent topic cannot avoid dealing with the living economists, who in part may not share the same sensitivity to such an endeavor or who may indeed have a vested interest in shaping their legacy. Most participants of the experimental turn are still active in their late 60s, 70s, and 80s. These actors are furthermore in possession of primary sources informing the subject matter - few have made them accessible to scholars - and they are also privy to episodes that typically are not captured in published research records; and unless they are reported in various biographical and semi-biographical pieces, they will inevitably be lost due to natural attrition.

Gaining full access to Charles Plott's files, which are located in his laboratory and offices, was a time-consuming process requiring a certain amount of mutual trust, which, in turn, generated more trust and a closer relationship. The same occurred with the experimentalists whom I approached through a direct request for an interview. Each contact, informal discussion, or recorded interview with the living is a social interaction with its internal dynamics, issues of social hierarchy, and implications for maintaining an objectifying distance from the interviewees (see for instance Teichova et al., 2005, p. 10) and also entails a choice how to write history.

As a consequence of my interviews, I gained access to the research files of experimental economists whose collections have not been deposited in archives yet. However, I did not conduct interviews simply as a substitute for consulting archival collections or gaining access to them. Even if such collections were available, there are numerous limitations in what they contain. Interviews allow one to capture unrecorded and otherwise unpreserved recollections and impressions of events. Some of the interviewees became aware that their past might be of interest to historians, stopped discarding their materials and are now making their files available. For instance, Al Roth's papers are now deposited at Duke and with other scholars I am lobbying to finalize this move.

My interviews thus present in many senses a partial oral history of over sixty years of scholarly activity broadly conceived, and I am aware of and have encountered the

[475] See the recent work of E. Roy Weintraub on the history of general equilibrium analysis with Till Düppe DÜPPE, T. & WEINTRAUB, E. R. 2014. *Finding equilibrium: Arrow, Debreu, McKenzie and the problem of scientific credit*, Princeton University Press.

problems afflicting memory - forgetfulness, self-delusion, myopia, the inaccuracy of human recollection, reticence of narrators; the biases of interviewers discussed extensively in the oral history literature (Dunaway and Baum, 1996, Cutler, 1996, p. 99) appears also in the history of economics literature (Weintraub, 2005, Weintraub, 2007, Weintraub and Forget, 2007, Weintraub, 2010, Weintraub, 2012, Mata and Lee, 2007).[476] The approach I chose for handling these concerns is not new. However, the execution is tailored to the unique circumstances - it is based on the triangulation of sources, their cultivation and critical assessment. For instance, interviews are used cautiously and preference is usually given to contemporary materials whenever available. All information gained from interviews which could not be verified in archives and contemporary documents was cross-checked in interviews with other participants or through the transcript of the Witness Seminar. Such information was also used to indicate to the interviewees that I was privy to information that they did not anticipate I would be aware of, which in turn helped to go deeper in the interviews. Most interviews were followed up with enquiries about unclear recollections, requests for additional information and access to materials, without indicating specific reasons for needing particular documents. Over the years I have come to appreciate gaining answers to my questions in interviews without posing the questions themselves and letting interviewees talk longer than in a casual conversation. Thus the whole thesis is permeated with a sustained effort to confront the problem of distance between the sources and their historian.

8.1 Oral History and Archival Collections

Over the past five years, I have conducted over fifty recorded interviews totaling eighty-eight hours, numerous informal debates and also dozens of follow-ups. I estimate that I have talked to more than two thirds of the experimentalists who were active by the early 1990s. A full list of interviews can be found in Section 9.2. A notable gap in my coverage is that of the behavioral economists with both psychology and economics training. These

[476] An issue rather omitted from the cited references is the interviewers' possible interference with the interviewee's recollection by framing questions, and the various demand effects (i.e. second-guessing what the historians are looking for).

are not traditional oral interviews in the sense of deeply personal accounts.[477] Nor does their sole focus lie in the scientific achievements of the interviewees. Rather, they have a mixed focus: on the social history of experimental economics, the interviewee's perspective and participation in it, and their intellectual trajectories. In retrospect, seeking this broader focus was an important early decision, since the dual nature of experimental economics requires that developments in both the method and of the field should be pursued. Themes covered in the interviews were also affected by the choice to conduct a witness seminar and its four main topics.

As a consequence of my interviews I benefited from gaining privileged access to the research files of other experimental economists such as Elizabeth Hoffman, Mark Isaac, Tom Palfrey, Al Roth, Arthur Schram, and Frans van Winden.

Some of the experimentalists whom I interviewed decided to make their papers available through public archives. The largest collection of scientific papers of economists in the world is located at the Economists' Paper Project (EPP) of Duke University, USA. It contains several collections that cover experimental economics – the papers of Vernon Smith, Martin Shubik, James Friedman, and Al Roth. From the history of experimental economics perspective, Smith's papers are the most extensive and the most important at EPP. They contain an almost complete record of his scientific correspondence and research files. Friedman's papers consist of a single box containing his experiments related to his thesis at Yale in the 1960s. Friedman neither kept materials related to his subsequent experiments nor kept his correspondence. Shubik's papers, significant as they are for a variety of domains of economics, provide mostly insights into the history of experimental economics in the 1960s and 1970s. Shubik was not a central figure during the 1980s and later, which I argue is the crucial period for the experimental turn in economics. Al Roth claims that he kept records for only a small portion of his published papers and even for those hardly any referee reports or complete successions of various drafts are preserved. His correspondences files are incomplete, at least when they are cross checked with other collections (not only at EPP). They contain very interesting materials pertaining to game theory, but from the experimental economics perspective

[477] A rare example of research informed by traditional oral history is Till Düppe 's portrayal of Gerard Debreu. DÜPPE, T. 2012. Gerard Debreu's Secrecy: His Life in Order and Silence. *History of Political Economy,* 44**,** 413-449.

they are quite disappointing. Fortunately, Roth's move from Harvard to Stanford unearthed some misplaced files. [478]

Another major primary source is the scientific files of Charles Plott, who has spent the past four decades at the *California Institute of Technology* (Caltech). I have been fortunate enough to have privileged access to his files and make use of his invaluable personal library with a comprehensive collection of publications and unpublished works related to experimental economics until the year 2000. Plott's papers differ from Smith's in at least two respects. First, they are less complete than Smith's. Second, while Smith has kept all correspondence separate from his research files, making it challenging to match them up, Plott collated the correspondence pertaining to a particular research project with the project file. Other correspondence was kept chronologically, but numerous gaps exist.

Researchers active before the 1960s whose work is identified by experimental economists as having lasting influence were also investigated. I was able to locate the descendants of Sidney Siegel and Edward Chamberlin and inspect the preserved effects.[479] Milton Friedman's papers at the Hoover Institute and Frederick Mosteller's paper at Harvard were also consulted (Wallis and Friedman, 1942, Mosteller and Nogee, 1951).

8.2 The Witness Seminar

To remedy the paucity of accessible primary sources but also the limitations of the one-on-one interview, the **witness seminar method** was applied. This method was developed by the *Institute of Contemporary British History* and successfully applied in a variety of issues such as medical history, political history, diplomatic and defense history, or the history of IT (Tansey, 2006, Wellcome Trust, 1997-2012, Institute of Contemporary British History). In contrast to oral histories with typically one interviewee, the witness seminar

[478] I consulted a variety of archival collections at EPP. Some experimentalists corresponded with Robert Solow, Leonid Hurwicz, Robert Clower, Robert Lucas, and Lionel McKenzie and in my research on their papers I tried to locate or complete these exchanges. My pursuit of Edward Chamberlin's experimental research led to (largely unsuccessful) investigations of the Lloyd Metzler, American Economic Association, and Earl Hamilton Papers. The Oskar Morgenstern Papers were investigated primarily because of his connection to the German experimentalists Sauermann and Selten.

[479] There is a small archival collection of Chamberlin located at Harvard, but it contains only four boxes of post-WWII correspondence. The materials relevant to Chamberlin's experiments are still with his daughter. I located the wife of the late Austin Hoggatt, but have not been able to visit her.

brings together a group of selected participants in a specific event and provides a form of open peer review where all remarks are immediately susceptible to agreement or dispute by other participants. Moreover, a witness seminar captures remembrances of events and other details that typically are lost once witnesses pass away, thereby supplementing existing records and creating new resources (Tansey, 2009).

Similarly, by creating a discussion between the actors of the experimental turn, the goal of the seminar was to purposefully create an archival document that would reflect a collective memory. However, it does not provide a comprehensive treatment of the history of experimental economics. Rather it provides elements of a story from a specific perspective and with a specific purpose.

Since there was only a one chance to conduct the seminar, a great deal of preparation was done to gain and maintain control over its progress and outcome. The details of such deliberations, a report on the execution of the seminar, and a first assessment of the transcript can be found in a working paper co-authored with my advisor and co-organizer (Svorenčík and Maas, 2015). From the multitude of design points, the extent of active intervention by the moderator on the one hand and letting participants recount their group history with little guidance on the other was probably the most salient. All these design aspects are debatable, but it is left to the reader to judge the success of the seminar.

The very preparation of the *Witness Seminar on the Emergence and History of Experimental Economics* entailed extensive interviewing of the invited participants. Later the preparation of the seminar transcript afforded continued interaction with the experimentalists and greatly facilitated the access to personal archives.

The transcript of the seminar (soon to be) fully edited, annotated, and approved by its participants, constructs the history of experimental economics as the experimentalists themselves remember it. Such an internal account furnishes a valuable primary source for other scholars and is an integral part of my dissertation. Moreover, it marks the first application of the witness seminar method in the history of economics. It is to be deposited at the Economists' Paper Project at Duke University.

9 Primary Sources

9.1 Archives

Elizabeth Hoffman Papers - personal archive located at her home in Golden, Colorado.

Thomas Palfrey Papers - personal archive located at the California Institute of Technology.

Charles Plott Papers - personal archive located at the California Institute of Technology.

Alvin Roth Papers - personal archive located at Harvard University which was later transferred to Duke University and is now accessible as the Alvin Roth Papers, David M. Rubenstein Rare Book & Manuscript Library, Duke University. NB: I inspected the Roth papers before they were deposited at Duke. I am quoting box numbers according to the current order, though there may be some errors in matching the old inventory with the new.

Arthur Schram Papers - personal archive located at the University of Amsterdam.

Martin Shubik Papers - David M. Rubenstein Rare Book & Manuscript Library, Duke University.

Vernon Smith Papers - Vernon L. Smith Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, Durham, North Carolina. NB: I inspected the Smith papers first in October 2009 (and again every year since then) and they were not thoroughly processed until the following year. I am quoting box numbers according to the current order, though there may be some errors in matching the old inventory with the new.

Frans Van Winden Papers - personal archive located at the University of Amsterdam.

Witness Seminar - Witness Seminar on the Emergence and History of Experimental Economics took place on May 28 and 29, 2010, in Amsterdam

9.2 Interview List

1. James Andreoni Interview - one session at the AEA/ASSA Meetings in Philadelphia on January 5, 2014;

2. Orley Ashenfelter Interview - one session at the AEA/ASSA Meetings in Philadelphia on January 3, 2014;
3. Otwin Becker Interview - two sessions at his home in Walldorf, Germany, on November 22, 2010;
4. Brian Binger Interview - one session at his home in Golden, C0, on January 6, 2011;
5. Jordi Brandts Interview - one session at the ESA International Conference in Chicago on July 7, 2011;
6. Colin Camerer Interview - two sessions at California Institute of Technology on September 6, 2011;
7. Timothy Cason Interview - one session at the ESA International Conference in Zürich, July 12, 2013;
8. James Cox Interview - three sessions at Georgia State University on February 10, 2011;
9. André Daniere Interview - one session at his home Hadley, MA, June 29, 2010;
10. Trenery Dolbear Interview - one session at Brandeis University on May 19, 2011;
11. John Duffy Interview - one session at the ESA International Conference in Chicago on July 8, 2011;
12. Catherine Eckel Interview - one session at the ESA North American Conference in Tucson, AZ, on November 14, 2009;
13. Ernst Fehr Interview - one session at the ESA International Conference in Zürich, July 13, 2013;
14. Daniel Friedman Interview - one session at the ESA International Conference in Chicago on July 8, 2011;
15. James Friedman Interview - one session at Duke University on October 19, 2009;
16. David Grether Interview - one session at California Institute of Technology on November 29, 2009;
17. Glenn Harrison Interview - two sessions at Georgia State University on February 10, 2011;
18. Ronald Harstad Interview - one session at the ESA North American Conference in Tucson, AZ, on November 17, 2012;
19. John Hey Interview - one session at the ESA International Conference in Zürich, July 12, 2013;
20. Elizabeth Hofmann Interview - one session at the Cosmos Club Washington, DC, on October 8, 2009;

21. Charles Holt Interview - one session at the ESA North American Conference in Tucson, AZ, on November 14, 2009;
22. Mark Isaac Interview - one session at Florida State University on June 24, 2010,
23. John Kagel Interview - three sessions at Ohio State University on September 22, 2009;
24. John Ledyard Interview - one session at California Institute of Technology on September 10, 2009;
25. Hsing-Yang Lee Interview - one session at California Institute of Technology, March 27, 2012;
26. John List Interview - one session at the ESA International Conference in Chicago on July 8, 2011;
27. Keith Murnighan Interview - one session at Kellogg School of Business on June 18, 2014;
28. Rosemary Nagel Interview - one session at the ESA North American conference in Tucson, AZ, on November 18, 2012;
29. Daniel Newlon Interview - one session in Washington, DC, on October 7, 2009;
30. Charles Noussair Interview - one session at the ESA International Conference in Zürich, July 12, 2013;
31. Roland Oaxaca Interview - one session at the AEA/ASSA Meetings in Philadelphia on January 4, 2014;
32. Thomas Palfrey Interview - two sessions at California Institute of Technology on January 27, 2011 and March 26, 2012;
33. Charles Plott Interview - three sessions at California Institute of Technology on November 25, 2009 and August 18, 2011;
34. Amnon Rapoport Interview - one session at the ESA North American conference in Tucson, AZ, on November 18, 2012;
35. Stephen Rassenti Interview - two sessions at Chapman University on November 17, 2009;
36. Alvin Roth Interview - one session at Harvard University on September 25, 2009;
37. Zac Rolnik Interview - one session at University of Amsterdam on June 20, 2011;
38. Andrew Schotter Interview - one session at the ESA North American conference in Tucson, AZ, on November 14, 2009;
39. Arthur Schram Interview - two sessions at University of Amsterdam on June 20, 2011;
40. Reinhard Selten Interview - two sessions at Bonn University, February 24, 2010;

41. Martin Shubik Interview - two sessions at his home near New Haven, CT, on June 30, 2010;
42. Vernon Smith Interview - two sessions at Chapman University on November 17, 2009;
43. Hugo Sonnenschein Interview - one session at University of Chicago on June 19, 2014;
44. Matthew Spitzer Interview - one session at California Institute of Technology on August 28, 2009;
45. Shyam Sunder Interview - one session at the ESA North American conference in Tucson, AZ, on November 17, 2012;
46. Reinhard Tietz Interview - three sessions at his home in Frankfurt, February 22, 2010;
47. Richard Thaler Interview - one session at University of Chicago on June 16, 2014;
48. Mark Van Boening Interview - one session at the ESA International Conference in Zürich, July 12, 2013;
49. John Van Huyck Interview - one session at the ESA International Conference in Zürich, July 12, 2013;
50. James Walker Interview - one session at the ESA International Conference in Chicago July 9, 2011;
51. Martin Weber Interview - one session at the AEA/ASSA Meetings in Chicago on January 6, 2012;
52. Arlington Williams Interview - one session at University of Indiana on June 27, 2012;
53. Frans van Winden Interview - one session at University of Amsterdam, February 11, 2010.

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De experimentele wending in de economie

Een samenvatting in het Nederlands

De opkomst van de experimentele economie in de afgelopen decennia heeft de lang vigerende overtuiging dat economie een niet-experimentele discipline is, fundamenteel ter discussie gesteld. Met het begrip experimentele economie verwijs ik naar het veld van onderzoek dat in de jaren zestig en zeventig van de vorige eeuw is ontstaan, en waarin met behulp van experimentele methoden van onderzoek economische verschijnselen en theorieën worden onderzocht. Maar experimentele economie is meer dan een simpele toepassing van experimentele methoden in de economische wetenschap. Zij behelst bovenal een herdefiniëring van de relatie tussen theorie en data. In de gecontroleerde omgeving van het experiment kunnen data worden gecreëerd die voldoen aan de specifieke eisen van theorie. Dit betekent niet dat data ondergeschikt worden gemaakt aan de eisen die theorie aan ze stelt. Integendeel, de introductie van de experimentele methode in de economie leidde tot een bevrijding van de positie van data uit de ondergeschikte positie waarin zij zich traditioneel bevonden. Met kwalitief hoogwaardige data die beantwoorden aan de eisen van theorie is het economisch theoretici niet langer mogelijk data te negeren of als irrelevant terzijde te schuiven. Deze herdefinitie van de relatie tussen theorie en data, vormt de kern van wat ik in dit proefschrift aanduid als de "experimentele wending" in de economie.

Met het begrip "de experimentele wending in de economie" organiseer en interpreer ik het geheel van veranderingen die de persona van de econoom en diens onderzoekspraktijk ondergingen in de transformatie van de economische wetenschap tot een experimentele discipline. Dit begrip vat daarmee de belangrijkste bijdrage van mijn proefschrift aan de geschiedschrijving van de economische wetenschap samen. Met name de tweede helft van de jaren tachtig van de twintigste eeuw was voor deze transformatie belangrijk. Voor die tijd werden economische vraagstukken al wel incidenteel met behulp van experimentele methoden onderzocht. Ingrediënten van wat ik de experimentele wending in de economie noem, waren dan ook wel binnen de discipline aanwezig. Maar zij kwamen nooit samen waardoor zij geen duurzaam effect op de economische wetenschap hadden. Er waren, hier en daar, economische onderzoekslaboratoria, er was aandacht voor de verhouding van theorie en data. Te denken valt aan de experimentele landbouw in de negentiende eeuw, vroege managementgames, zogenaamde “time and motion studies” in het Interbellum, of bedrijfssimulatiespelen na de Tweede Wereldoorlog. Desondanks leidden deze vroege verschijningen van het experiment niet tot een blijvende acceptatie van de experimentele methode in de economische wetenschap. Dat veranderde definitief in de tweede helft van de jaren tachtig. In deze periode vloeiden verschillende ingrediënten van de experimentele methode zodanig samen dat ook de economische professie meer in de breedte overtuigd raakte van haar mogelijkheden. Deze ingrediënten duid ik ook wel aan als de drijvende krachten van de experimentele wending. Zij zijn: 1) integriteit – een conceptuele heroverweging van wat geldt als valide data in de economische wetenschap; 2) strengheid –data verzameling onder gecontroleerde omstandigheden en onder toezicht van de onderzoeker; 3) deugdzame cirkel – de wisselwerking en wederzijdse versterking van experimenteel en theoretisch onderzoek en 4) symmetrie – gelijkwaardigheid van economische data en theorie.

In mijn proefschrift onderzoek ik hoe deze ingrediënten elkaar op verschillende momenten versterkten en anderszins op elkaar inwerkten. Mijn startpunt is daarbij de individuele econoom. Ik laat vervolgens zien hoe de verschillende ingrediënten niet alleen van belang waren voor individuele onderzoekers, maar ook een organiserende functie hadden voor lokale onderzoeksgroepen en vervolgens voor een groeiende gemeenschap van experimentele economen. Ik duid dit proces aan als een proces van actieve receptie van de methode van het experiment, dat is een proces waarin individuele economen en economische onderzoeksgroepen zich de experimentele methode eigen maken en verder ontwikkelen. Tenslotte traceer ik hoe deze krachten doorwerken in het hele economische vakgebied en leiden tot de vorming van een nieuwe economische werkgemeenschap, de Economic Science Association (ESA) en tot een passieve ontvankelijkheid voor economisch experimeneel onderzoek bij toonaangevende economische tijdschriften en subsidiërende instanties. De successvolle wijze waarop de verschillende ingrediënten van de experimentele methode samenkwamen vat ik samen als de experimentele wending in de economie.

Het proefschrift is verdeeld in zeven hoofdstukken. In het eerste hoofdstuk introduceer ik de notie van de experimentele wending in de economie door een nadere begripsbegripsbepaling van wat ik als haar drijvende krachten zie. Hoe deze krachten op individueel niveau werken beschrijf ik in het tweede hoofdstuk. In plaats van paradigmatische lijnen in experimenteel economisch onderzoek te traceren, toon ik de aard van de vier drijvende krachten, of ingrediënten, van de experimentele wending door middel van vier case studies van individuele onderzoekers. Deze case studies stellen me tevens in staat een paar van de belangrijkste protagonisten van dit proefschrift te introduceren via hun 'conversie' tot het experiment. Ik laat vervolgens zien hoe deze individuele onderzoekers elkaar vonden in wat zich voorzichtig ontluikende locale gemeenschappen die zich organiseerden rond de experimentele methode.

Deze overgang van individueel naar collectief was sterk gebonden aan de ontwikkeling van een speciaal voor het experiment toegeruste locatie: het laboratorium. Ik documenteer deze complexe ontwikkeling in het derde hoofdstuk. Ik laat niet alleen zien hoe deze ontwikkeling nauw verbonden is met een zich eveneens ontwikkelende arbeidsdeling binnen een onderzoeksgroep, maar ook hoe de beschikbare technologie en inrichting van een laboratorium de mogelijkheden voor experimentele controle vergrootten en een soort 'experimentele locomotief' creëerden; een nieuwe stijl van onderzoek met welhaast imperialistische trekken.

Met de oprichting van de Economic Science Association (ESA) in 1986, die ik in hoofdstuk 4 bespreek, werd de experimentele economische gemeenschap in Noord Amerika op grote schaal geïnstitutionaliseerd. Een constituerend document van deze associatie, de tekst van een lunch-seminar van Vernon Smith dat onder experimenteel economen bekend stond als 'The Prologue', geeft een indrukwekkende samenvatting van de grondleggende gedachte achter de experimentele wending; het biedt een herbezinning op de relatie tussen theorie en data, met de productie van data onder gecontroleerde omstandigheden als ijkpunt. Het hoofdstuk vervolgt met de geschiedenis van de internationalisering van deze jonge organisatie, waarvan de oprichting van het tijdschrift Experimental Economics, tegenwoordig een van de leidende tijdschriften van de economische discipline, belangrijk deel uitmaakt. Uit de oprichting van dit tijdschrift sprak, zoals we zullen zien, een sterk verminderde angst van experimenteel economen dat een eigen tijdschrift zou leiden tot gettovorming voor hun onderzoek.

Inderdaad was door succesvolle publicaties van experimenteel onderzoek in toonaangevende economische tijdschriften het imago van experimenteel onderzoek binnen de economische professie sterk verbeterd. Maar aan deze gestegen reputatie was door pioniers van het eerste uur hard en bewust gewerkt. Hoofdstuk vijf onderwerpt de publicatiestrategieën van experimenteel onderzoekers en détail aan een nader onderzoek vanuit het gezichtspunt van wat ik aanduid als 'passieve receptie'; dat wil zeggen, vanuit het perspectief van redacteuren en referenten die geen ervaring hadden met experimenteel onderzoek. Ik baseer mij hierbij met name op de correspondentie van Charles Plott met vooraanstaande redacteuren naar aanleiding van zijn ter publicatie ingestuurde artikelen.

Het voorlaatste hoofdstuk onderzoekt het moment waarop de experimentele wending zichtbaar wordt. Dit moment situeert zich rond de controverse over experimenteel onderzoek naar gesloten veilingen. Verschillen van mening over de betekenis van rigoureuze experimentele controle, wat het betekent om een theorie te testen, en hoe en of een theorie kan worden aangepast in het licht van aanvullend bewijs liepen in deze episode hoog op, en dat nog wel in het belangrijkste economische tijdschrift, The American Economic Review. Een bespreking van deze controversiële periode voert terug naar de oprichting van ESA en de acceptatiestrijd van experimentele publicaties in toonaangevende economische tijdschriften. De achterliggende kwestie is die van vertrouwen – vertrouwen in experimentele data, in de interpretatie die experimenteel economen daaraan geven, het wederzijdse vertrouwen tussen experimenteel economen van verschillende theoretische snit, en vertrouwen van de rest van de economische professie dat experimenteel economen elkaar per saldo niet de hand boven het hoofd houden, maar bereid zijn verschillen in theoretisch inzicht en in interpretatie van de data publiekelijk uit te vechten. Kwaliteitscriteria voor experimenteel economisch onderzoek verkregen daardoor een grote mate van transparantie voor economen zonder ervaring met experimenteel onderzoek. Deze episode voltooide de acceptatie van het experiment als methode van onderzoek binnen de economische professie als geheel.

In het laatste hoofdstuk herneem ik het thema van de experimentele wending in termen van de epistemische en sociale kenmerken die daarmee verbonden zijn. Beide kenmerken, de karakteristieken van wetenschappelijk aanvaardbare kennis en van de sociale context waarin dergelijke kennis wordt geproduceerd, zijn als het ware opgebouwd uit de vier ingrediënten, of krachten, die in de experimentele wending samenkomen, zoals ik met name in het voorlaatste hoofdstuk heb laten zien. Naast een terugblik, bied ik in dit hoofdstuk ook een schets van de wijze waarop de notie van een 'wending' in een wetenschapsgebied gebruikt kan worden om ook andere gebieden van wetenschap dan de economie te analyseren.

Experimentálny obrat v ekonómii

Zhrnutie v slovenskom jazyku

Vzostup experimentálnej ekonómie v posledných desaťročiach prehodnotil dlhotrvajúce presvedčenie, že ekonómia je neexperimentálna disciplína. Pod experimentálnou ekonómiou chápeme poddisciplínu v rámci ekonómie, ktorá od svojich začiatkov v 60-tych a raných 70-tych rokoch minulého storočia nepretržite skúma ekonomické javy a teórie prostredníctvom experimentálnej metódy. Experimentálna ekonómia išla ale oveľa ďalej, než len za zavedenie experimentov do ekonómie. Bola predovšetkým o redefinovaní vzťahu medzi ekonomickou teóriou a dátami. Replikovateľné dáta vytvorené v kontrolovanom prostredí tak, aby spĺňali podmienky stanovené teóriou, sa mohli stať dôveryhodným partnerom ekonomickej teórie. To nebola v žiadnom zmysle kapitulácia dát pre potreby teórie. Práve naopak, cieľom bolo oslobodenie dát z ich podriadenej pozície nadobudnutej v povojnovom období a ich pozdvihnutie na rovnakú úroveň, akú má ekonomická teória. Dáta zhromaždené pod kontrolou ekonómov z vyššie uvedenými požadovanými vlastnosťami nemohli byť naďalej prehliadané teoretikmi alebo bagatelizované ako irelevantné pre potreby ekonomických teórií. Tento pokus o rekonceptualizáciu vzťahu medzi ekonomickou teóriou a rigoróznymi experimentálnymi dátami je jadrom toho, čo nazývame experimentálny obrat v ekonómii.

Pojmom „experimentálny obrat v ekonómii" usporadúvame a interpretujeme rad zmien, ktorými ekonómovia a spôsob ich ekonomického výskumu prešli pri zmene ekonómie na experimentálnu disciplínu. Tento pojem je hlavným prínosom našej dizertačnej práce k dejinám ekonomického myslenia. Naša práca prináša obdobie druhej polovice 80-tych rokov minulého storočia do popredia historického záujmu. Ekonomické problémy boli dovtedy len príležitostne vyšetrované pomocou experimentálnej metódy. Prísady experimentálneho obratu v ekonómii boli tiež prítomné predtým v ekonómii. Ale nikdy sa spolu nezišli a nemali trvalý vplyv na ekonomickú vedu. Z času načas existovali ekonomické laboratóriá, pozornosť bola venovaná vzťahu medzi teóriou a dátami. Príklady zahŕňajú experimenty poľnohospodárskej ekonómie v 19. storočí, skoré manažérske experimenty a takzvané časové a pohybové štúdie v medzivojnovom období, ale i experimentálne hry krátko po druhej svetovej vojne. Tieto epizódy experimentovania v ekonómii neviedli k trvalému prijatiu experimentálnej metódy v ekonómii. To sa natrvalo zmenilo v druhej polovici 80-tych rokov 20. storočia. V tomto období sa rôzne prísady experimentálnej metódy spojili dohromady takým spôsobom, že ekonomická profesia sa stala presvedčená o jej potenciáli. Tieto prísady označujeme ako hnacie sily experimentálneho obratu. Tými sú: 1) integrita - konceptuálna rekonfigurácia toho, čo sa chápe ako dáta a demonštrácia v ekonómii; 2) rigoróznosť - zber dát za kontrolovaných podmienok pod osobným dohľadom ekonóma; 3) účinný cyklus - poznanie, že experimentálny výskum je najúčinnejší, keď ide ruka v ruke s ekonomickou teóriou a 4) symetria - umiestnenie experimentálnych dát na úroveň s ekonomickou teóriou.

Cieľom tejto práce bolo ukázať, ako tieto hnacie sily pôsobili a integrovali sa. Naším východiskovým bodom boli individuálni ekonómovia. Potom sme ukázali, ako rôzne prísady boli nielen dôležité pre jednotlivých výskumníkov, ale tiež mali organizačnú funkciu pre miestny výskum a následne na vznikajúce spoločenstvo experimentálnych ekonómov. Tento proces označujeme ako proces aktívnej recepcie experimentálnej metódy, čo je proces, v ktorom si jednotliví ekonómovia a skupiny ekonómov prijali a začali rozvíjať experimentálnu metódu. Nakoniec sme venovali pozornosť prenikaniu hybných síl do celej disciplíny ekonómie prostredníctvom vzniku Economic Science Association (Združenie ekonomickej vedy) a pasívnej recepcie experimentálneho výskumu vo vedúcich časopisoch a grantových agentúrach. Úspešný spôsob, akým sa spojili jednotlivé zložky experimentálnej metódy, odrážajú povahu experimentálneho obratu.

Práca je rozdelená do siedmich kapitol. Prvá kapitola popisuje a analyzuje termín experimentálneho obratu a jeho hnacej sily. Druhá kapitola sleduje, ako sa experimentálny obrat prejavil na individuálnej úrovni. Namiesto sledovania histórie jednotlivých paradigmatických smerov experimentálneho výskumu, sme predstavili štyri prípadové štúdie odhaľujúce povahu štyroch hnacích síl experimentálneho obratu. Tým sme predstavili viacerých hlavných protagonistov experimentálneho obratu a predstavili ich osobné cesty k experimentálnemu výskumu. Kapitolu uzatvára analýza spôsobu, akým sa jednotlivé osobné trajektórie pretínali, čo malo za následok vznik komunity ekonómov, nepretržite sa venujúcich experimentálnemu výskumu.

Prechod od jednotlivých ekonómov, budúcich experimentátorov, pôsobiacich v novo vznikajúcej komunite podobne zmýšľajúcich výskumníkov, sa pripútal k špecifickému, účelovo zariadenému miestu - ekonomickému laboratóriu. Tretia kapitola popisuje tento vývoj. Zdôrazňuje nielen väzbu na komunitu a deľbu práce, ale aj ako fyzické vlastnosti laboratórií a dostupná technológia uľahčili experimentálnu kontrolu a fungovanie

„experimentálneho motora“, čím viedli k spusteniu kontinuálneho štýlu experimentovania a šírenia experimentálneho imperializmu.

Založenie Združenia vedeckej ekonómie (Economic Science Association) v roku 1986, ktorému sa venujeme vo štvrtej kapitole, bolo vyvrcholením úsilia organizovať a inštitucionalizovať experimentálnu ekonómiu v Severnej Amerike. Na hlbšej úrovni jej zakladajúci dokument, The Prologue, napísaný Vernonom Smithom, je najvýznamnejším prehlásením myšlienok experimentálneho obratu – snáh rekonceptualizovať vzťah medzi ekonomickou teóriou a dátami zhromaždenými v kontrolovaných podmienkach v laboratóriu alebo v teréne pod dohľadom ekonómov. Táto kapitola ďalej sleduje internacionalizáciu tohto mladého združenia a založenie časopisu Experimental Economics, v súčasnosti veľmi vplyvného ekonomického časopisu. Jeho vznik predstavovalo prekonanie dlhodobých obáv experimentátorov, že by prípadný časopis venujúci sa experimentálnej ekonómii mal za následok ich marginalizáciu.

Publikačným úspechom experimentálneho výskumu v hlavných ekonomických časopisoch sa zlepšoval imidž a postavenie experimentálnej ekonómie v rámci ekonomickej profesie. Za zvýšenou reputáciou experimentálneho výskumu stála cieľavedomá a sústavná práca prvej generácie experimentálnych ekonómov. Piata kapitola analyzuje publikačné stratégie experimentátorov a približuje ich z pohľadu pasívnej recepcie experimentálneho výskumu - čiže ako bol vnímaný a hodnotený editormi a recenzentmi časopisov, ktorí nemali žiadnu priamu skúsenosť s experimentálnou metódou. Primárnym zdrojom tejto kapitoly je korešpondencia Charlesa Plotta s významnými editormi v reakcii na jeho články predložené k publikácii.

Predposledná kapitola sa venuje obdobiu, keď sa experimentálny obrat stal viditeľným pre zvyšok ekonomickej profesie. Toto obdobie súviselo s kontroverziou ohľadom prvocenových aukcií. Rozdiely v názoroch týkajúcich sa rigoróznej experimentálnej kontroly, čo to znamená testovať teóriu a ako a či ju upraviť s ohľadom na nové dáta boli natoľko pálčivé, že si našli miesto vo významnejšom ekonomickom časopise The American Economic Review. Táto kontroverzia musí byť posudzovaná v kontexte súbežného vzniku Združenia vedeckej ekonómie (Economic Science Association) a úsilia publikovať v popredných časopisoch. Za týmto klbkom rôznych aspektov experimentálneho obratu sa nachádza otázka dôvery - dôvery v experimentálne dáta a ich interpretáciu experimentálnymi ekonómami, vzájomnú dôveru medzi nimi a dôveru zvyšku profesie, že experimentátori nedržia nad sebou ruku a sú pripravení verejne obhájiť a rozhodnúť spor ohľadom teoretického vnímania a interpretácie experimentálnych dát. Kritériá hodnotenia kvality experimentálneho výskumu sa tým stali transparentnejšími pre ekonómov bez priamej skúsenosti s ekonomickými experimentmi. Táto epizóda efektívne ukončila pasívnu recepciu experimentálnej metódy v ekonómii.

V záverečnej siedmej kapitole sa vraciame k experimentálnemu obratu v ekonómii prostredníctvom jeho epistemického a sociologického vlákna. Obe vlákna, povaha platných vedeckých poznatkov a sociálny kontext, v ktorom tieto poznatky vznikli, sú vytvorené štyrmi prísadami, hnacími silami, ktoré skonvergovali v experimentálnom obrate, ako to bolo predovšetkým vidno v predposlednej kapitole. Po tomto spätnom pohľade ďalej načrtávame ako zovšeobecnený pojem „obrat“ vedeckej disciplíny môže byť aplikovaný v dejinách iných vied než v ekonómii.

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