The Sidney Siegel Tradition: The Divergence of Behavioral and Experimental Economics at the End of the 1980s

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Abstract: Over two days in February 1988, several key experimental economists and cognitive psychologists met to explore the possibilities of joint research promoted by the Sloan and Russell Sage Foundations under the rubric behavioral economics. The original vision that the meeting could open a line of inquiry on the growing body of behavioral "anomalies" and their robustness in a market setting proved naive. The divide between both camps was too big to bridge given the fundamentally different approaches to experimentation. The article traces how the work of Sidney Siegel, a psychologist briefly active in the 1950s, was recast by experimental economists as the basis of their experimental research, including the emphasis on performance-based payments of experimental subjects and avoiding deception. My reconstruction of this meeting and its aftermath sheds new light on the origin of the divergence of experimental and behavioral economics at the end of the 1980s.

Keywords: experimental economics, behavioral economics, Sidney Siegel, Behavioral Economics Program

JEL codes: B21, B31, C90, C91, D03

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Vernon Smith once asked Amos Tversky whatever happened to the tradition of Sidney Siegel in psychology. “You are it,” Tversky replied. “That was not a compliment,” Smith explained. “That was a touché. He was putting me down. ‘You are it. You continued a bad tradition’” (Svorenčík and Maas 2015, 88).[1]

It was during a two-day conference, a Witness Seminar held in 1988, when this brief and at first sight innocuous, if not jocular, exchange took place. Even if we have to rely solely on Smith’s memory—Tversky died two decades ago—this exchange is far from being merely a cute anecdote. Rather, it is symptomatic of an unease growing at that time between experimental economists, including Smith, and a group of economists and cognitive psychologists soon to be referred to as behavioral economists.

Indeed, the conference was plainly titled “Experimental Economics and Psychology”; in 1988, when it took place, economists following the work of Tversky and Daniel Kahneman, two cognitive psychologists, had not yet claimed the label behavioral economics. But the label, itself with a distinguished history (Sent 2004; Augier 2005; Heukelom 2010), was in the air again. The Russell Sage Foundation revived it through its Behavioral Economics Program that ran between 1986 and 1992, as did the Alfred P. Sloan Foundation with a similar program between 1984 and 1989. The link between these two programs was Eric Wanner, who served first as a program officer and vice president

[1] The recollection of this conversation appears several times in Smith’s writings, but in all instances the circumstances, such as when the encounter took place, are not preserved. It first appeared as a cryptic note in E. Roy Weintraub’s history of game theory volume in 1992. A decade later it can be found in a special issue on the relationship of psychology and economics. And it has also a prominent place in Smith’s autobiography. See Smith 2008, 200; Hertwig and Ortmann 2001, 442; and Smith 1992, 247.

at the latter, then as a trustee and president of the former institution. Unsurprisingly, Wanner attended this conference. In fact, the Sage Foundation funded the meeting.

It might have been the warm California weather that was the reason for selecting February as the time for this intimate gathering. Historical weather records confirm that for most of the one dozen participants, February 12–13, 1988, on the West Coast was a welcome respite from the harsh Midwest and East Coast weather.[2] But the location was certainly not accidental. The campus of the California Institute of Technology was chosen because of Caltech's position at the forefront of experimental economics research. Most important, unlike Smith's University of Arizona, the other leading center of experimental economics in the United States at the time, it housed experimentalists also doing research on individual choice—a topic near and dear to psychologists.

In a seminal paper on preference reversals, the Caltech economists David Grether and Charles Plott had attempted a decade earlier to remedy what they considered the flaws of psychologists' experimental design. Two of the thirteen reasons that they entertained as possible explanations for this phenomenon stand out. One was that the experimenters in the previous studies were psychologists. Having the reputation for

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[2] The attendees were Eric Wanner, Colin Camerer (Pennsylvania), Donald Coursey (Washington), Robyn Dawes (Carnegie-Mellon), John Kagel (Houston), Daniel Kahneman (UC Berkeley), Charles Plott (California Institute of Technology), Alvin Roth (Pittsburgh), Vernon Smith (Arizona), Shyam Sunder (Minnesota), Richard Thaler (Cornell), and Amos Tversky (Stanford**)**. The psychologists are Dawes, Kahneman, Tversky, and Wanner. The rest are economists. Camerer is a special case. He received a doctorate in decision theory. When asked about how he would describe himself in the 1980s, he answered, "I started as an experimental judgment researcher and quickly began to do experimental economics in my first faculty job. But, for example, my dissertation research was about the psychology of judgment, and it did have some experiments in it, but it wasn't closely tied to economics" (interview with Colin Camerer, September 6, 2011, California Institute of Technology).

deceiving subjects and subjects second-guessing psychologists' experiments, Grether and Plott (1979, 629) "felt that the experimental setting should be removed from psychology" in order to give the results additional credibility. Thus the critical point was not the involvement of particular psychologists per se but how deception and reputation for deceiving corrodes experimental control. The other reason that stands out is what Grether and Plott called misspecified incentives. Most prior studies focused on hypothetical, unmotivated choice and did not incorporate performance-based monetary payments for experimental subjects. Despite the great lengths that Grether and Plott took to prevent preference reversal from occurring, ultimately they failed in their attempt to disprove the psychologists' findings.[3]

The no-deception rule and insistence on paying subjects are the hallmarks of experimental economics research, and the latter is being traced back by experimental economists to none other than Sidney Siegel, a psychologist briefly active in the 1950s and early 1960s. These precepts only partly explain Tversky's rejoinder to Smith's question about Siegel's legacy in psychology. Tversky, a meticulous experimenter himself, was aware of the pitfalls of deception and proper motivation for maintaining experimental control. Yet during the 1970s, as Floris Heukelom (2011a) argues, the types of experiments run jointly by Tversky and Kahneman while developing their research

[3] A similar case is Grether's replication of Kahneman and Tversky's experiments from the 1970s on the violation of Bayes' rule that led them to the "representativeness heuristic." For details and references for Kahneman and Tversky's papers, see Grether 1980.

program of heuristics and biases shifted toward questionnaires with hypothetical choices.[4]

There was also something other than the contrast between hypothetical choices and subject payments that motivated Tversky's response. Siegel, as Smith has frequently argued (see footnote 2 for details), was seen by Tversky

> as part of the Skinner animal behaviorist tradition in psychology, a tradition that approached decision behavior as an objectivist "black box" study of the choices made by animals and people under various controlled experimental conditions. It eschewed the idea of studying decision in humans as part of cognitive processes using introspection, surveys, and subject oral and written reports, which are then interpreted by the scientist in terms of models of cognition. Skinner had rejected this methodology as unreliably subjectivist. Cognitive psychologists in turn rejected Skinner's behaviorism as devoid of all attempts to understand mental thought processes. This is typical academic maneuvering: They are both right (and wrong). (Smith 2008, 200)

Thus the brief conversation between Smith and Tversky is a window into a complex relationship between experimental economists and contemporary behavioral economists. Francesco Guala (2008, 156) refers to this relationship as marked by "a persistent low-intensity conflict at the methodological and theoretical level." In this article I place the historical beginning of this "low-intensity" conflict at the 1988 Caltech meeting.

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[4] Heukelom (2011a, 820) referred to this as a shift to "more relaxed standards of the experimental method."

The historical understanding of the emergence of contemporary behavioral economics has been greatly enhanced by the groundbreaking work of Heukelom (2014). In particular, the patronage of the Sloan and Sage Foundations played a crucial role in the rapid ascent of behavioral economics (Heukelom 2012). Experimental economists were involved in the Behavioral Economics Program from early on, as well. There was even a working group on decision making and experimental economics created. Its first meeting was the conference at Caltech, where the exchange between Tversky and Smith took place. More important, the meeting was also the last gathering of the working group, and what emerged from it sheds light on the separation of experimental and behavioral economics at the end of the 1980s.

The remainder of the article is divided into four sections. The first one focuses on the meaning and emergence of the "Siegel tradition." In the second section, I focus on the Caltech meeting and its significance within Sage Behavioral Economics Program. The penultimate section deals with the divergence between market experimental economists and behavioral economists. The last section ties the threads of this article together and relates them to the rest of this volume.

1. Sidney Siegel and "His" Tradition

The thing that got me really, deeply interested in heavy experimental gaming was Sidney Siegel. And to me, it was a true tragedy that he died so young.

—Interview with Martin Shubik, June 30, 2010

For Shubik, Siegel's premature death was not just a missed research opportunity; "Sid's" death was also a personal calamity. Their friendship started under the most unlikely

circumstances. While spending the academic year 1955–56 at the Center for Advanced Study in the Behavioral Sciences (hereafter CASBS), a recently established institution for the study of human behavior overlooking Stanford University's campus, Shubik went camping with Martin J. Beckmann, a mathematical economist at the Cowles Foundation at Yale, to Yosemite National Park. In the evening at a public campground fireplace at the Tuolumne Meadows surrounded by other nature explorers, instead of reflecting on the day's trek and the beauty around them, the two men explored a different type of beauty, namely, the axiomatization of utility theory. There was somebody on the other side of the fireplace who was obviously listening to their conversation. "Finally," Shubik recalls, "he could not stand it any longer, and so he came around the fireplace to where Beckmann and I were seated and sort of immediately joined the discussion and introduced himself." It was Siegel, an assistant professor of psychology at Pennsylvania State University.[5]

By 1955 Siegel had had by any measure an unconventional academic career. In 1951, at the mature age of thirty-five, he obtained his bachelor's degree from San Jose State College. By 1954 he had obtained a PhD in social psychology from Stanford University and embarked on a mainly experimental project "to measure subjective probability *behavioristically* on the basis of empirically determined utilities" (Davidson, Suppes, and Siegel 1957, 25; emphasis mine).[6] Siegel's interest in both utility theory and experimental measurement remained unabated after he joined Penn State in the summer of 1954.

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[5] Interview with Martin Shubik, Shubik's home, New Haven, Conn. June 30, 2010; See also Smith 1992, 250.

[6] The coauthors were two young, Stanford-based philosophers, Donald Davidson and Patrick Suppes. A preliminary report of their work appeared already in 1955 as Davidson, Suppes, and Siegel 1955.

Being on different coasts was not ideal for collaboration even though Shubik and Siegel "immediately took a complete liking and complete understanding to each other."[7] Siegel helped Shubik get an adjunct appointment at Pennsylvania State University from 1957 to 1959, although Siegel spent the academic year 1957–58 at the CASBS.[8] The last hurdle preventing their collaboration was Siegel's prior commitment to a joint project with Lawrence E. Fouraker, an economist at Penn State who later became dean of the Harvard Business School. During the winter of 1958, Siegel and Fouraker conducted experiments in bilateral monopoly leading to a book, now a classic in experimental economics (Siegel and Fouraker 1960). Finally in 1959, a few years after their fortuitous meeting, Shubik, together with Siegel and Fouraker, ran additional oligopoly experiments exploring quantity adjustment models (Fouraker, Shubik, and Siegel 1961). Siegel and Shubik planned to embark on a major experimental research project.[9] Their correspondence reveals Shubik as the driving force behind the plans, who was often urging Siegel to devote more time to their ambitious endeavor: "Siegel, we are growing old and there is work to be done."[10]

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[7] Interview with Shubik; referring to the camp fire conversation.

[8] At the CASBS, Siegel was attracted to the work of the economists who were spending a year at the center, namely, Kenneth J. Arrow, Milton Friedman, Robert M. Solow, and George Stigler.

[9] See, for instance, a letter to Sidney Siegel from Martin Shubik, dated July 5, 1960, which elaborates on sixteen open issues that could be addressed experimentally (box 8, Martin Shubik Papers).

[10] Martin Shubik to Sidney Siegel, February 15, 1960, box 8, Martin Shubik Papers.

1.1. Probability Matching Experiments

Siegel's approach is well represented by his work on so-called probability matching. William Estes (1950), an eminent experimental psychologist, conducted experiments in the early 1950s that studied his own learning theory, in which learning was represented as a converging stochastic process.[11] In his experiments, Estes let two lights flash on and off repeatedly, with one light being more likely to be turned on. Then he asked participants to predict which light would go on next in a series of 120 trials, with no rewards for correct or penalties for incorrect guesses. He concluded that subjects simulated the frequencies of the lights turning on—hence probability matching. This result appeared in the proceedings of the 1952 Santa Monica conference and drew a lot of attention from the conference participants, as it contradicted the simple game-theoretic prediction that subjects should always choose the more frequent light (Estes 1954; Flood 1954; see also Smith 1992, 260–70 for an insightful discussion).

Siegel got interested as well and hypothesized that since subjects had to perform so many repeated guesses, the observed matching behavior was caused by the absence of any incentives to guess correctly as well as boredom. He then modeled both factors within the expected utility framework, with utilities associated with correct predictions and variability of the decision situation. The lack of financial incentives made guessing costless. In his experiments therefore Siegel introduced two new variations. In addition to providing no monetary incentives for correct guesses, in one new treatment he paid subjects for the number of times they were correct and in another included both rewards

[11] Estes was a graduate student under Skinner's direction, but in the 1950s, to Skinner's disappointment, he drifted more toward mathematical modeling of mental processes. For more on Estes, see Association for Psychological Science 2011.

and a penalty for being incorrect. He also devised a set of ingenious techniques to relieve the tediousness of long sequences of binary choices. In the original Estes design, subjects adhering to the game-theoretic rational prediction would be required to choose one light—either the one on their left or on their right side. To prevent what Siegel called kinesthetic monotony, subjects were seated in a swivel chair and randomly asked to turn right or left. On one side there was a panel with the two electric lights, and on the other side there was a mirror reflecting those two lights. Hence if a subject wanted to choose the same light again and again, he did not have to choose "right" repeatedly, but depending which side he was randomly asked to turn, he would vary between "right" and "left." The net result of monetary incentives and the swivel chair was that the students did not simulate the frequency but picked the optimal pure strategy against the flashing lights (Siegel and Goldstein 1959; Siegel 1961).

1.2. Birth of a Tradition

Siegel died suddenly on November 29, 1961, from a fatal heart attack in his office at the CASBS. During his second residence at the center, he had planned to complete a book that would combine experimental tests and mathematical modeling of repeated choice such as those just described (Siegel 1964, vii).

The incomplete elements of the joint work with Shubik became part of the posthumously written book by Fouraker, a sequel to their volume of 1960 (Fouraker and Siegel 1963). Both volumes have become classics in experimental economics. That happened not because of the lasting significance of the experimental investigation of bilateral monopolies and oligopoly. On the contrary, that topic had become something of

a dead end by the early 1970s.[12] Rather, its lasting importance lies in the way Siegel employed the experimental method as well as the way these two volumes were integrated and interpreted by subsequent experimentalists, Vernon Smith and his associates in particular, as the origin of proper methodology for experimenting in economics.[13]

Shubik was not the only one whose research plans were thwarted by Siegel's premature death. So too were those of Smith. He was spending the academic year 1961–62 at Stanford as a visiting associate. At a dinner organized by Marc Nerlove, an econometrician and agricultural economist as well as the 1969 Clark medalist,[14] Smith and Siegel met only to discover that they had both been doing experimental research for some years (Smith 2008, 198). Within a few weeks of their meeting, Siegel died. Nevertheless, the brief encounter had a profound impact on Smith. Even four decades later, when receiving the 2002 Nobel Prize in Economics, he expressly thanked Siegel for influencing his experimental career.

None of Siegel's psychology doctoral students continued his experimental economics research, and Fouraker soon became a full-time administrator. Shubik, in part lacking Siegel's experimental guidance, immersed himself in simulations research and experimental gaming—the type of open-ended experimental work that Siegel did not pursue (Engvall Siegel 1964, 17). In contrast, Smith clearly is an intellectual disciple of

[12] Charles Plott and James Friedman at the Witness Seminar, quoted in Svorenčík and Maas 2015.

[13] It was Siegel, not Fouraker or Shubik, who brought in the experimental know-how to their collaboration.

[14] Nerlove was also Grether's adviser. During Smith's stay at Stanford he taught for one term the graduate microeconomics sequence, and he presented some of his market experiments. Grether was one of the attendees. Interview with David Grether, November 29, 2009, California Institute of Technology.

Siegel. The first of his experimental papers was published in the spring of 1962. In it, Smith referenced Siegel in only one footnote, and that was a late addition, since they had become aware of each other's work just prior to Siegel's death (Smith 1962, 111n2). The reported experiments were done without monetary payments, though another footnote reported Siegel's completed and future experiments with monetary incentives (Smith 1962, 121n9).

Siegel was not the first to note the importance of monetary payments in economic experiments as a way to exercise better control over subjects' motivations.[15] But he was the first to systematically apply it in a series of experiments, notably in those that experimental economists include in their bibliographies. In the mid-1980s Caltech with Plott and Arizona with Smith were the largest and most influential centers of experimental economics research in North America (Svorenčík 2015). Both Plott and Smith maintained a comprehensive library of all works relevant for experimental economics, which were the go-to repositories for local experimentalists at Caltech and Arizona until papers could be easily retrieved online. The library catalogs from the time shortly after the 1988 Caltech conference reveal a specific picture of reception of Siegel's work. What is included is a small fraction of the twenty-eight publications that Siegel wrote during his brief career (Engvall Siegel 1964, 22–23). The "U of Arizona Bibliography" from January 22, 1990, lists only the two books by Fouraker and Siegel. Plott's "Inventory Experimental Lab Bibliography" from October 9, 1989, has in addition the two papers mentioned in the context of the probability matching experiments—the

[15] It seems that Wallis and Friedman (1942) were the first to point out the significance of experimental payments. Mosteller and Nogee (1951) used payments as well. For details, see Moscati, this volume.

one coauthored with Donald Goldstein and the 1961 paper on decision making and learning, as well as Siegel's last book, *Choice, Strategy, and Utility*, from 1964. Most of Siegel's work in psychology journals is absent from both bibliographies. That is not because the experiment economists were avoiding such outlets. The Arizona bibliography has over one hundred articles in psychology journals, and the Caltech one has just over fifty articles in psychology journals. Similarly, a bibliography of experimental research compiled by Elinor Ostrom from 1984 lists only the two books by Fouraker and Siegel. A comparison of these inventories with an online bibliography made a decade later by Charles Holt at the University of Virginia at the end of the 1990s reveals an almost identical picture with the Caltech inventory.[16]

To understand Siegel's position on monetary payments, it is instructive to read a longer quote from the book that Siegel was working on at the time of his death, and which was later completed by Siegel's wife assisted by one of his graduate students:

> Siegel was quite convinced that hypothetical choices were unreliable and that experimental subjects had to be rewarded in order to be adequately motivated. On this matter, Siegel departed from psychologists' standard approach: "Because of our belief in the central importance of employing payoffs which are meaningful to subjects, rewards which in fact they covet, we have little confidence in experiments in which the 'payoffs' are points, credits, or tokens. Or perhaps it

[16] This *does not* mean that experimental economists were ignorant of other work by Siegel. This is evidenced, for instance, in Smith's extensive discussion of Siegel's work in his reflections on game theory and experimental economics in the 1992 *History of Political Economy* supplemental issue on game theory. Rather, Siegel's other work was considered less relevant for experimental economics. NB: All three inventories are in the Charles Plott Papers at the California Institute of Technology.

> would be more accurate to say that we have little confidence in the use of the term payoff to label such trivia. The relevance of such experiments to any theoretical notions about reward, payoff, or utility seems to be dubious." (Siegel 1964, 148)

Siegel, unlike many of his contemporaries, was against the use of deception in experimental research. In general, deception in experiments occurs when the actual purpose of an experiment differs from the purpose announced to the test subjects. In a discussion about the timing of subject payments, he warned that "college students are by now so familiar with the notion of an experimental 'hoax,' with experimenters who employ deception in the service of their experiments, that they might not believe an experimenter who told them he was going to pay them at the conclusion of their trials" in probability matching experiments (Siegel 1964, 152). Put differently, deception in Siegel's view undermined the relationship between the experimenter and the subjects, who, in consequence, are often too preoccupied with the true agenda of the experiment or doubt the announced relation between actions and rewards, which therefore ultimately jeopardizes experimental control.[17]

Both opposition to deception and performance-based payments have been stressed in Smith's work ever since he met Siegel and were adopted as standard precepts by experimental economists (Smith 1976, 1982; Plott 1982; Wilde 1981; Guala 2005).[18] When Don Coursey applied to the Sage Foundation for a grant to organize a working group on experimental economics that eventually led to the Caltech meeting, he wrote

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[17] According to Siegel's wife, for him it was primarily an unethical malpractice. See Engvall Siegel 1964.

[18] The above-mentioned memoir by Siegel's wife also lists clear procedures: no deception, closeness to data, and closeness to theory as other core themes of Siegel's research (Engvall Siegel 1964, 16–21).

that "although the exact origin of experimental economics techniques is hard to pinpoint, most economists working in the area would attribute a large share of the development credit to Sidney Siegel and Lawrence Fouraker's work in the late 1950s."[19] Indeed, Siegel's premature death allowed experimental economists who have continued to work on market experiments to appropriate him as the intellectual and historical source of procedural standards of experimental economics research, thereby creating a tradition that Smith in his question to Tversky concisely labeled as the Siegel tradition.

2. Russell Sage's Experimental Economics Working Group

The major catalyst of Kahneman and Tversky's work in economics that led to contemporary behavioral economics was the Behavioral Economics Program of the Sloan and Sage Foundations (Heukelom 2014). This program, the brainchild of Wanner, was run by Sloan from 1984 to 1989. After being promoted to vice president of the Sloan Foundation in the summer of 1985, Wanner was appointed as a trustee and president of the Russell Sage Foundation in the summer of 1986. The Behavioral Economics Program at the Sage Foundation ran from then until 1992. Under Wanner's leadership both foundations provided the necessary support for behavioral economics and created "a sense of mission" (Heukelom 2012).

In 1986 Wanner wanted to create working groups focused on particular topics. Besides a group on intertemporal choice headed by George Loewenstein and Jon Elster

[19] Don Coursey to Eric Wanner, February 26, 1987, subgroup 2, box 193, folder 1424, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York. More than two decades later, the participants of the Witness Seminar on the history of the experiment in economics also singled out Siegel in the very same fashion (Svorenčík and Maas 2015).

and a behavioral approaches to financial markets group headed by Richard Thaler and Robert Shiller, the Advisory Committee of the Behavioral Economics Program approved a third working group, one on experimental economics. From the very start, one of the catchment areas of the Sloan program "experiments with simulated markets designed to examine the market consequences of individual and social psychological processes."[20] Hence it was natural to start a working group on experimental economics. The initial idea was that a senior experimentalist such as Plott, together with one senior cognitive psychologist such as Kahneman, should be in charge. Eventually Coursey, who graduated in 1982 from Arizona under Smith's supervision, and recipient of a grant from the Sloan Foundation, took the lead and submitted a proposal motivated by some of the very same considerations I discussed in the previous section:

> It has become clear that certain experimental economics environments produce behavior which can only be fully understood through the use of cognitive psychology. In particular, economists must often reconcile the role of pecuniary motives with the role of non-pecuniary motives in describing individual behavior. Additionally, standard economic theory has often been observed to do a better job explaining the behavior of individuals in latter, rather than initial, segments of experiments. Individuals often converge to the predictions of economic theory, but the process through which they learn or adapt to this end is a puzzle.[21]

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[20] Russell Sage Foundation Board of Trustees Docket, June 1988, subgroup 2, box 194, folder 1433, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

[21] Don Coursey to Eric Wanner, February 26, 1987, subgroup 2, box 193, folder 1424, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

The 1988 meeting of the working group at Caltech did not have a fixed agenda. The first day was dedicated to the different approaches and priors that economists and cognitive psychologists bring to the laboratory. To stimulate and guide the discussion, two papers were circulated. One was an early draft of Smith's paper "Theory, Experiment, and Economics" that appeared in 1989 in the *Journal of Economic Perspectives*. The other paper comprised notes by Kahneman written for the meeting, with the title "Experimental Economics from a Psychologist's Perspective." A visit to Plott's recently established experimental economics laboratory at Caltech and a demonstration of market experiments using the multiple double auction software took place in the afternoon.[22]

The second day was left for discussion of current research projects of the participants (for their list, see footnote 2). Thaler talked about endowment effect experiments and preference reversal; Alvin Roth discussed sequential bargaining such as in ultimatum games; Plott discussed prospect theory tests; John Kagel outlined his latest behaviorist animal experimental research on Giffen goods and the violation of the matching law; Colin Camerer talked about the hindsight bias; Coursey addressed the prisoner's dilemma and public goods; and Shyam Sunder discussed salient payments.[23] It

[22] For details about the history of Plott's laboratory and the MUDA software, see Svorenčík 2015.

[23] Wanner's handwritten notes from the meeting, subgroup 2, box 193, folder 1424, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

is a shared recollection of the participants that the meeting was lively and revealing of the different nature and role of experimentation in economics and cognitive psychology.[24]

Two years after the meeting, Wanner reflected that it "resulted in something of a *donnybrook* between the experimental economist and psychologists present."[25] The heated argument focused on three issues covered in Smith's and Kahneman's accompanying papers: (1) inclusion of data gained from hypothetical questions for which there is no economic incentive to give any particular answer; (2) focus on (market) equilibrium as a result of repeated experience versus focus on one-shot experiments without the opportunity for learning; and (3) experiments with deception. In regard to allowing deception and hypothetical choices in economic experiments, experimental economists wanted to maintain Siegel's dictum of always avoiding deception and always paying subjects based on their performance, while cognitive psychologists and Thaler, unsurprisingly, did not.[26] In regard to the second issue, as Heukelom (2012, 279) succinctly puts it, "Smith and Plott wanted to concentrate on the question how the market eventually steers individual behavior towards rational equilibrium, and what the

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[24] Interviews with Colin Camerer, September 6, 2011, California Institute of Technology; David Grether, November 29, 2009, California Institute of Technology; John Kagel, September 22, 2009, Ohio State University; John Ledyard, January 12, 2015, California Institute of Technology; Charles Plott, November 25, 2009, California Institute of Technology; Vernon Smith, November 17, 2009, Chapman University; Richard Thaler, June 16, 2014, University of Chicago; and Wanner. Interview with Wanner took place in New York on April 15, 2015; Both Grether and Ledyard attended the meeting, as they were based at Caltech, but are not listed as official attendees.

[25] Board Meeting memo, February 27, 1990, subgroup 2, box 7, folders 69, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York; my emphasis.

[26] When asked about the meeting, Thaler recalled: "It's clear that psychologists and economists had very different views about what was a proper experiment. One difference still exists today which is economists have this taboo about deception" (interview with Richard Thaler, June 16, 2014, University of Chicago).

equilibrium exactly looks like. Wanner, Kahneman, Thaler, and the advisory committee, on the other hand, were more interested in how initial individual behavior deviates from the theoretically defined equilibrium, irrespective of whether it exists or not. In addition, Wanner, Kahneman, and Thaler questioned how often economic markets are allowed the time to mature towards."

These different approaches were not discussed on an abstract level only; they framed, for instance, the discussion about the empirical validity of the theoretical claim that the willingness to pay for an object equals the willingness to accept. In line with the prospect theory, it was proposed that the observed difference, first established through answers to hypothetical questions, between the two quantities can be explained by the so-called endowment effect—once you own something, you value it more. In a series of articles in the second half of the 1980s, it was discussed whether real incentives, repeated interaction, and involvement of markets reveal both quantities to be equal (Knetsch and Sinden 1984; Kahneman, Knetsch, and Thaler 1986a, 1986b, 1990; Knetsch 1989). Kahneman et al. 1990 in particular was a result of discussion between Smith and Kahneman and Thaler at a conference organized by Roth (1987) in May 1985. Smith's position was that "these results were suspect until they were demonstrated in a market context with monetary incentives and opportunities for learning."[27] The discussion has not been settled and continues to fill the pages of leading journals such as the *American Economic Review* (Plott and Zeiler 2005, 2007, 2011; Isoni et al. 2011; Cason and Plott

[27] Thaler's Proposal to the Behavioral Economics Program February 1988, subgroup 2, box 172, folder 1222, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

2014), though Kahneman, Knetsch, and Thaler are not active in this line of inquiry anymore.[28]

When Smith asked Tversky about the fate of the Siegel tradition in psychology, he did not have only subject payments versus hypothetical choices and the no-deception rule in mind. There are two other things that can be reconstructed from Smith's writings evaluating psychologists and behavioral economists from the time shortly after the Caltech conference.

First, in 1991 he published an article, "Rational Choice: The Contrast between Economics and Psychology," in the Chicago-based *Journal of Political Economy*. It was a review and fierce criticism of *The Behavioral Foundations of Economic Theory*, which was based on a conference that took place in 1986 at the University of Chicago (Hogarth 1987). This conference is often viewed as the first real test of the new behavioral economics program, since it met such resistance from many proponents of rational expectations theory (Heukelom 2014, 158).[29] Smith (1991, 893) argued that "psychologists since Siegel have not attempted to apply their perspective and questions to market experiments of the kind studied by experimental economists." Smith not only defended experimental economics from the various criticisms raised against it in the volume but was highly critical of the work of Kahneman, Tversky, and Thaler because it had turned away from Siegel.

Second, this turning away from Siegel was not just on the level of experimental technique but also on a conceptual level: how to modify economic theory in light of

[28] Their last involvement was Knetsch, Tang, and Thaler 2001.

[29] Interview with Richard Thaler among others.

rigorous behavioral-experimental evidence not matching it. For Smith, Siegel's probability matching experiments discussed in detail above are "exemplary in showing what psychology can contribute to economics by modifying decision theory in the light of rigorous experiment. In contrast, "many psychologists today who study decision making emphasize the predictive failures of 'rational' theory" (Smith 1992, 268). In experiments without sufficient motivation and excessive tedium, it was rational for subjects not to be "rational." Once these factors were accounted for theoretically, Siegel studied them experimentally. Smith argued, and many experimentalists at the meeting such as Plott and Sunder shared his view, that research on anomalies including the one on endowment effect and the difference between willingness to pay and willingness to accept should follow in Siegel's footsteps. Yet embracing the Siegel tradition does not exclude cognitive psychology. Reflecting on the exchange with Tversky, Smith summarized this position: "Obviously you use all the instruments at your disposal, recognizing the hazards of subjectivism and the dead-end extreme of the behaviorist's unwillingness to delve into that 'black box' called the brain" (Smith 2008, 200).

2.1. Behavioral Game Theory Working Group

After the conference meeting at Caltech, the working group on experimental economics never convened again.[30] Instead, the remainder of its appropriated funds was later spent on a conference organized by Camerer on behavioral game theory, at the University of

[30] Coursey, Plott, and Tversky apparently formed a committee, but no evidence of output has been found (Coursey to Eric Wanner, February 17, 1988, subgroup 2, box 198, folder 1463, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York).

Pennsylvania in 1990. Given its success, later a new working group with the same name was established.[31] The goal was to bring experimenters and theorists together "to share experimental evidence of bounded rationality and speculate about how such evidence might be used to build new theory."[32] Three issues were involved—how much rationality is necessary for players to determine equilibrium strategies; the role of fairness in bargaining outcomes; and whether learning from experience leads to equilibrium choices.[33]

Camerer's friendly takeover of the moribund experimental economics working group, refocusing it on experimental game theory along the goals mentioned above, was exactly within the purview of the Behavioral Economics Program. This move bypassed the issue of how "anomalies" fare in reward-driven markets, and experimentation of a

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[31] Richard Thaler to Eric Wanner: "I think this meeting is an example of the behavioral economics program at its finest" (May 27, 1992; Participant Evaluations 1992, subgroup 2, box 196, folder 1442 Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York).

[32] Russell Sage Foundation Meeting of the Board of Trustees, Appendix D, February 27, 1990, subgroup 2, box 7, folder 67, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

[33] The attendees included twenty-four experimenters and theorists: James Andreoni, University of Wisconsin; Robert Aumann, Hebrew University; Kenneth Binmore, University of Michigan; Russell Cooper, University of Iowa; Robyn Dawes, Carnegie-Mellon University; Robert Forsythe, University of Iowa; Charles Holt, University of Virginia; Eric Johnson, University of Pennsylvania; Ehud Kalai, Northwestern University; Dave Kreps, Stanford University; George Mailath, University of Pennsylvania; Paul Milgrom, Stanford University; Andy Postlewaite, University of Pennsylvania; Amnon Rapoport, University of Arizona; Al Roth, University of Pittsburgh; Ariel Rubinstein, Tel-Aviv University; Andy Schotter, New York University; Reinhard Selten, University of Bonn; Hugo Sonnenschein, University of Pennsylvania; Richard Thaler, Cornell University; Amos Tversky, Stanford University; John van Huyck, Texas A&M; Keith Weigelt, University of Pennsylvania.

different type than market experiments gained a foothold in the program.[34] Although the program closed soon after establishing the working group, it provided the seed for Camerer's (2003) acclaimed book on this topic.

In a paper published in 1990, Camerer elaborated on the bones of contention at the Caltech meeting, namely, whether markets correct biases, and probability judgment in particular.

> The point of experiments like these is to establish empirically what kinds of irrationality persist under the experience, incentives, institutional structure, and learning opportunities that are present in markets. Representativeness does seem to persist in one set of experiments (though it is eroded by experience); other irrationalities may vanish quickly. For instance, it is easy to extinguish "probability matching" (e.g., Estes, 1976) and induce maximizing in subjects. An empirical understanding of what irrationalities persist, and under what conditions, could lay the foundation for an economic theory that uses evidence of systematic irrationality to make better predictions, rather than invoking the tired argument that markets always correct irrationality. (Camerer 1990, 169)

The reference to probability matching is important. Imagine Siegel had not turned his attention to probability matching but to another "irrationality" that is harder, if not

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[34] The transition was facilitated by Camerer's high regards in both communities. He obtained a PhD in behavioral decision science from Chicago, supervised by experimental psychologists. He had been involved in the Behavioral Economics Program from early on. And his first encounter with economics experiments was through Plott. Plott spend the winter term 1980 in Chicago and gave a graduate course on experimental methods. Only a handful of students attended, and Plott, as he always does in such a course, required them to devise an individual experimental project. Camerer and Sunder are two attendees of the Caltech conference who credit the beginning of their experimental career to Plott's Chicago course.

impossible, to eliminate with the introduction of performance-based payments and proper motivation control. The magnitude of that decision for the future of experimental economics only equals the perennial thought of what would have happened if Siegel had not died at such a young age.[35]

3. The Aftermath

In the spring of 1992, when the decision to discontinue the Behavioral Economics Program was pending, Wanner approached everyone previously involved with the program.[36] In a lengthy response to Wanner, Vernon Smith strongly criticized both the organization of the program and the research on which it had focused. He was the only critical voice out of the thirty-eight letters received. In particular, he was "still puzzled by these rejections, and the similar experience, I understand, of other experimental economists."[37] Indeed, several experimental proposals were rejected: Mark Isaac and James Walker's on public goods provision; Shyam Sunder's on bounded rationality modeling of individual behavior in market setting; Raymond Battalio's on duopoly markets; Ronald Harstad's on a behavioral approach to normative economic analysis; John van Huyck's on coordination failures; Amnon Rapoport's on voluntary provision of step-level public goods; Camerer's on bubbles and fads in asset prices as well as a

[35] This gives a new quality to the review of Siegel's last book written by Ward Edwards (1967, 293): "We cannot know what Siegel might have done. But this book is a deeply impressive record of what he did do. Even with 20 more years than Siegel had, how many of us can aspire to do so much?"

[36] In fact, the Behavioral Economics Program was closed in the summer of 1992.

[37] Smith to Eric Wanner, May 15, 1992, box 16, Vernon Smith Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

renewal grant for Smith and Isaac.[38] With the exception of Camerer's proposal, which was rejected in the fall of 1987, all these projects were declined a few months after the Caltech meeting in May 1988.[39] Smith's subsequent two resubmissions were rejected as well.

If the program were to be continued, Smith intimated, "you need to address the issue of why experimental economics was not more prominent in the program."[40] The fact that the significance of experimentation for behavioral economics decreased is an important historical question (Heukelom 2011a, 2011b). In the late 1970s, once Thaler acquainted himself with the work of Kahneman and Tversky as well as experimentalists like Smith and Plott, he believed that behavioral economics would become primarily an experimental enterprise.[41] However, once it became apparent that experimental market economists in their proposals were unable, at least in the eyes of the Advisory Committee of the Behavioral Economics Program, to make a "sufficiently clear connection between

[38] Smith and Isaac held a Sloan grant for a project to conduct research in experimental economics on market anomalies, computerized matching markets, and public goods provisions. In 198687 a renewal proposal was rejected.

[39] Advisory Committee Meeting, May 17, 1988, subgroup 2, box 194, folder 1433, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York. The archival records containing the assessment of individual projects are restricted indefinitely, preventing the historian from reconstructing the particular reasons why these proposals were not funded.

[40] "In [Wanner's] list of Grantees there are only about 7 in 45, and I believe these were largely funded under the Sloan round before Sage took over" (Smith to Eric Wanner, May 15, 1992, box 16, Vernon Smith Papers).

[41] Interview with Richard Thaler.

behavioral principles and experimental market predictions to impress the Committee favorably," their projects were not supported.[42]

It did not imply at all that any proposal with an experimental component was automatically rejected. Alvin Roth received a small grant for a new edition of his book on axiomatic models of bargaining that explicitly aimed to incorporate experimental evidence.[43] Even more interestingly, John Kagel's work on the winner's curse was repeatedly funded by both foundations in the years 1986–91. For both Thaler and Wanner, Kagel's combination of theoretical, field, and experimental research is a prime example of behavioral economics.[44]

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[42] In full, the Advisory Committee concluded at its meeting in May 1988 that "although there were a number of competent proposals for *market experimentation*, one involving cooperative behavior in duopolies, another on public goods provision, and a third on aftermarkets, none of these projects developed a sufficiently clear connection between behavioral principles and experimental market predictions to impress the Committee favorably. . . . The lean yield from this exercise provides additional evidence (whether needed or not) that the interdisciplinary territory we are attempting to occupy in the behavioral economics program is thinly populated. Theorists confine themselves to theory, *experimentalists to market experiments*, and ethnographers to descriptive work. It is difficult to find individuals willing to cross traditional lines and *look at naturally occurring decision problems in a way that is guided by, and relevant to, behaviorally grounded theories of economic decision making*" (Advisory Committee Meeting, May 17, 1988, subgroup 2, box 194, folder 1433, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York; my emphasis.

[43] The second edition did not appear, because when Roth "began to prepare the material I discovered that the experiments on strategic models weren't yet comprehensive enough to reliably draw conclusions addressed to the theory, and so I set out to fill some of the gaps, including gaps in the theory." Instead, one theoretical and two experimental papers were published (Alvin Roth to Eric Wanner, May 22, 1992, subgroup 2, box 155, folder 1114, Russell Sage Archives, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York).

[44] Interviews with Richard Thaler and Eric Wanner Interview with Wanner took place in New York on April 15, 2015.

After the Sloan Foundation closed its Behavioral Economics Program in 1989, the trustees of the Sage Foundation placed their program on hold, resulting in halving the available budget (Heukelom 2012). This funding restriction translated into fewer grants, and those who were funded were more attuned to the program's goals; the Advisory Committee was more risk averse in its decision making, and its preference as to what behavioral economics should look like became more recognizable. In 1990, when it was not clear that the program would close as soon as 1992, Kahneman joined the Advisory Committee, a sign for market experimental economists of revealed preference about the future of behavioral economics and its single most important patron.

Last but not least, the Caltech conference made the depth of divide between both camps clear. As Kahneman, Tversky, and Thaler's approach to experimentation diverged from that of Smith, Plott, and other experimental economists invested in market experiments, the emerging behavioral economics became less and less reliant on experimentation and was equally embracing other empirical as well as modeling approaches. This move to a less experimentally driven behavioral economics was facilitated by the absence of a formal institutional umbrella with explicit membership rules; there was no flag hoisted and no territory clearly demarcated. Unlike experimental economists and the Economic Science Association established in 1986, there was only the Behavioral Economics Program and no society of behavioral economics that would group economists involved in the program.[45] Consideration for financial support was

[45] This is not to say that behavioral economists are a homogeneous group. From the early 1980s, there has been the Society for the Advancement of Behavioral Economics, whose membership does not overlap with members of the Behavioral Economics Roundtable. From the early 1990s Gerd Gigerenzer has been a vocal opponent of Kahneman and Tversky's research on heuristics and biases.

initially by invitation only, and a group of carefully selected open-minded economists and psychologists was approached.[46] The program's executive body was the Advisory Committee, with four to six members equally split between economists and psychologists, and it served as a gatekeeper. In 1992 when the program closed, it was replaced by the Behavioral Economics Roundtable, which has kept the gatekeeper role. Former recipients of behavioral economics grants were consulted, and ten of them with the most votes were installed. Only one of them, Camerer, can be classified as an experimental economist.[47] On the one hand, by not formally separating behavioral economics from the rest of the profession, it allowed behavioral economics to easily fuse with the rest of the profession and thus avoided the marginalization that befell Simon's behavioral economics two decades earlier. On the other hand, by maintaining the program semiclosed and seeking out young talents and high-profile scholars, it fostered a behavioral economics identity and guaranteed it self-replication. It afforded behavioral economics with increased focus, giving the group "a sense of mission" as well as allowing behavioral economics to gain a foothold in top economics departments.

4. Conclusions

Over two days in February 1988 several key experimental economists and cognitive psychologists met to explore the possibilities of joint research promoted by the Sloan and

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[46] In no small part this was due to the size of available resources. An open call would impose too much of a burden on the Advisory Committee and would likely lead to increased disappointment, as only a small fraction of submitted grants could be supported.

[47] The initial ten were George Akerlof, Alan Blinder, Camerer, Elster, Kahneman, Loewenstein, Thomas Schelling, Shiller, Thaler, and Tversky.

Russell Sage Foundations under the rubric behavioral economics. Wanner's original vision that the meeting could open a line of inquiry to study the growing body of behavioral "anomalies" and their robustness in market setting proved naive. The divide between both camps was too big to bridge given the fundamentally different approaches to experimentation. One promoted by Kahneman, Tversky, and Thaler had its origins in cognitive psychology; the other, by Smith, Plott, and other market experimental economists, had its foundations in Sidney Siegel's experimental practice. In terms of tools and techniques, the former group advocated allowing deception and hypothetical choices in economic experiments; the latter avoided such experiments. In terms of conceptual frames, economic equilibrium in particular, the former group was more interested in how initial individual behavior deviates from the theoretically defined equilibrium, irrespective of whether it exists or not. Instead of a radical vision of abandoning neoclassical economics, the goal was to adjust it using various observations, data, and insights from cognitive psychology. Experimental economists, on the other hand, at that time wanted to concentrate on how the market steers individual behavior toward rational equilibrium and what the equilibrium exactly looks like. Furthermore, they had spent considerable resources—laboratories with the latest computer technology and custom-made software—to study these research questions. Their aim was not to protect neoclassical economics but to produce rigorous data that theory and theorists need to take seriously.

I have argued elsewhere that the second half of the 1980s and early 1990s was pivotal for what I label the experimental turn in economics—turning economics into an experimental discipline. During this period the experimental economics community

institutionalized itself through the Economic Science Association established in 1986, the number of computerized laboratories exploded, leading journals were steadily publishing experimental research, an important internal dispute was played out openly in front of the economics profession, and ultimately increasing trustworthiness of experimental economics broke out shortly after the 1988 Caltech meeting (Svorenčík 2015). The emerging divide between nascent behavioral economists and experimentalists is an integral part of the experimental turn not only because the divergence coincides with these changes but also because it relates to the turn's underlying issue—the reconceptualization of the relationship between economic theory and rigorous experimental data. The differences between both groups—both on the methodological level of how to conduct experiments and what their role is and on the conceptual level of how to modify theory in light of countervailing evidence and what is considered such evidence—became common knowledge thanks to the meeting at Caltech. Thus the Behavioral Economic Program moved away from resolving these differences that the experimental economics working group was supposed to examine to being focused on supporting projects of developing economic models on the basis of "behavioral principles."[48] The end of Sloan's support of the program in 1989 only reinforced this move. While experimental market economists such as Smith and Plott were effectively excluded and felt so, other experimental projects such as Kagel's on the winner's curse, Roth's on bargaining, and Camerer's on behavioral game theory were supported. During the 1990s, experimental economists grew as a community in its size and in the diversity of its members' research interests. Although the proportion of experimental market

[48] See footnote 42 for details and Staddon, this volume, for a criticism of behavioral economics.

economists declined, the experimental economics community has retained the basic procedural standards that it claimed and cultivated from Sidney Siegel.

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