

Tracing the Divergence of Behavioral and Experimental Economics in the Rockefeller Archive Center's Collections

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I once asked Amos Tversky whatever happened to the tradition of Sidney Siegel in psychology, and he said, “You are it.” That was not a compliment. [group laughter] That was a touché. [group laughter] He was putting me down. ... “You are it. You continued a bad tradition.”²

Vernon Smith at the Witness Seminar

This brief, and at first sight innocuous, if not jocular, exchange took place during a short two-day conference. Even if we must rely solely on Smith’s memory (Tversky died over two decades ago), this exchange is far from being so insignificant as to be cast aside as merely a cute anecdote. Rather, it is symptomatic of an unease growing at that time between experimental economists, including Smith, on the one hand, and a group of economists and cognitive psychologists – soon to be referred to as behavioral economists – on the other.

Indeed, the conference was plainly titled “Experimental Economics and Psychology”; in 1988, when it took place, economists following the work of Daniel Kahneman and Amos Tversky, two cognitive psychologists, had not claimed the label “behavioral economics” yet. But the label, itself with a distinguished history (Heukelom, 2010; Sent, 2004; Augier, 2005), 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 at the latter, then as a trustee and the president of the former institution. Unsurprisingly, Wanner attended this conference. In fact, the Russell Sage Foundation funded the meeting.

The warm California weather might have been the reason for selecting February as the time for this intimate gathering. Historical weather records confirm that

for most of the just one dozen participants, February 12-13, 1988, was a welcome respite from the harsh Midwest and East Coast weather.³ 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 importantly, unlike Smith's University of Arizona, the other leading center of experimental economics in the US 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, 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 deceiving subjects and subjects second-guessing psychologists' experiments, Grether and Plott "felt that the experimental setting should be removed from psychology," in order to give the results additional credibility (Grether and Plott, 1979, p. 629). Thus, the critical point was not the involvement of particular psychologists per se, but rather how deception and a reputation for deceiving corrode 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 went in order to prevent preference reversal from occurring, ultimately, they failed in their attempt to disprove the psychologists' findings.⁴

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

shifted towards “more relaxed standards of the experimental method” with an emphasis on questionnaires with hypothetical choices (Heukelom, 2011, p. 820).

There was also something other than the contrast between hypothetical choices and subject payments that motivated Tversky’s response. Siegel, as Smith had 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 (Smith, 2008, p. 200).

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

The historical understanding of the emergence of contemporary behavioral economics has been greatly enhanced by the groundbreaking work of Floris Heukelom (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 importantly, 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.

Russell Sage Foundation'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 Russell Sage Foundations (Heukelom, 2014). This program, the brainchild of Eric Wanner, was run by Sloan from 1984 to 1989. After being promoted to the position of 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 Russell 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 focusing on particular topics. Besides a group on intertemporal choice headed by George Loewenstein and Jon Elster, 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 was "experiments with simulated markets designed to examine the market consequences of individual and social psychological processes."⁵ 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 Donald Coursey, who graduated in 1982 from Arizona under Vernon Smith's supervision and recipient of a grant from the Sloan Foundation, took the lead and submitted a proposal that was motivated by some of the very same considerations that we encountered 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.⁶

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 psychologist bring to the laboratory. To stimulate and guide the discussion, two papers were circulated. One was an early draft of Vernon Smith's paper "Theory, Experiment and Economics" that was to appear in a 1989 issue of the *Journal of Economic Perspectives* (Smith, 1989). The other paper comprised notes by Daniel Kahneman written for the meeting and titled "Experimental Economics from a Psychologist's Perspective." The afternoon included a visit to Plott's recently established experimental economics laboratory at Caltech and a demonstration of market experiments using the multiple double auction software.⁷

The second day was left for discussion of current research projects of the participants (for their list, see footnote 3). Thaler talked about endowment effect experiments and preference reversal; Roth talked about sequential bargaining such as in ultimatum games; Plott presented prospect theory tests; Kagel spoke about his latest behaviorist animal experimental research on Giffen goods and the violation of the matching law; Camerer talked on the hindsight bias; Coursey discussed the prisoner's dilemma and public goods; and Sunder spoke on salient payments.⁸ It 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.⁹

Two years after the meeting, Wanner reflected on the gathering that it "resulted in something of a *donnybrook* between the experimental economist and psychologists present."¹⁰ The heated argument focused on three issues covered in Smith's and Kahneman's accompanying papers: (i) inclusion of data gained from hypothetical questions for which there is no economic incentive to give any

particular answer; (ii) focus on (market) equilibrium as a result of repeated experience versus focus on one-shot experiments without the opportunity for learning; and (iii) 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.¹¹ Regarding the second issue, as Heukelom has succinctly put it,

Smith and Plott wanted to concentrate on the question how the market eventually steers individual behavior towards rational equilibrium, and what the 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 (Heukelom, 2012, p. 279).

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 et al., 1986a; Kahneman et al., 1986b; Knetsch, 1989; Kahneman et al., 1990). The last paper, in particular, was a result of discussion between Smith with Kahneman and Thaler at a conference organized by Alvin Roth in May 1985 (Roth, 1987). Smith's position there "was that these results were suspect until they were demonstrated in a market context with monetary incentives and opportunities for learning."¹² 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; Plott and Zeiler, 2007; Cason and Plott, 2014; Plott and Zeiler, 2011; Isoni et al.,

2011), though Kahneman, Knetsch and Thaler are not active in this line of inquiry anymore.

Behavioral Game Theory Working Group

After the conference meeting at Caltech, the working group on experimental economics never convened again.¹³ Instead, the remainder of its appropriated funds was later spent on a conference organized by Colin Camerer on behavioral game theory at the University of Pennsylvania in 1990. Given its success, a new working group with the same name was subsequently established.¹⁴ The goal was to bring experimenters and theorists “to share experimental evidence of bounded rationality and speculate about how such evidence might be used to build new theory.”¹⁵ The issues involved consisted of 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.

Camerer’s friendly takeover of the moribund experimental economics working group and refocusing it on experimental game theory along the goals mentioned above was exactly within the goals of the Behavioral Economics Program. This move allowed for bypassing the issue of how ‘anomalies’ fare in reward-driven markets and experimentation of a different type than market experiments gained foothold in the Program.¹⁶ Although the Program closed soon after establishing the working group, it provided the seed for Camerer’s acclaimed book on this topic (Camerer, 2003).

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. A lengthy quote is worth citing:

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, p. 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 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.¹⁷

Conclusions

Over two days in February 1988, a number of key experimental economists and cognitive psychologists met to explore possibilities of joint research promoted by the Sloan and Russell Sage Foundations under the rubric of behavioral economics. Wanner's original vision that the meeting could open a line of inquiry that would study the growing body of behavioral "anomalies" and their robustness in market setting proved naïve. The divide between both camps was too big to bridge, given the fundamentally different approaches to experimentation. The one promoted by Kahneman, Tversky and Thaler had had its origins in cognitive psychology; the other by Smith, Plott, and other market experimental economists had drawn its foundations in Sidney Siegel's experimental practice. Regarding tools and techniques, the former group advocated allowing deception and hypothetical choices in economic experiments, while 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. The goal was not a radical vision of abandoning neoclassical economics, but rather an adjustment, using various observations, data as well as insights from cognitive

psychology. Experimental economists, on the other hand, at that time wanted to concentrate on the question how the market eventually steers individual behavior towards rational equilibrium, and what the equilibrium exactly looks like. Furthermore, they had spent considerable resources – laboratories with latest computer technology and custom-made software – to study these research questions. The goal was not to protect neoclassical economics, but rather to produce rigorous data that theory and theorists need to take seriously.

I have argued elsewhere that the period of 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, and an important internal dispute was played out openly in front of the economics profession, ultimately increasing the trustworthiness of experimental economics, which 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, as well as on the conceptual level of how to modify theory in light of countervailing evidence and what is considered as such evidence - became common knowledge, thanks to the meeting at Caltech. In consequence, the Behavioral Economic Program moved away from resolving these differences that the experimental economics working group was supposed to examine to being primarily focused on supporting projects of developing economic models on the basis of “behavioral principles.” 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. In the course of 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 economists declined, the experimental economics community has retained the basic procedural standards that it has claimed and cultivated from Sidney Siegel.

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2. David Grether Interview - one session at California Institute of Technology on November 29, 2009.
3. John Kagel Interview - three sessions at Ohio State University on September 22, 2009;
4. John Ledyard Interview - one session at California Institute of Technology on January 12, 2015.
5. Charles Plott Interview - two sessions at California Institute of Technology on November 25, 2009.
6. Charles Plott Papers - personal archive located at the California Institute of Technology.
7. Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.
8. Martin Shubik Interview - two sessions at his home near New Haven, CT, on June 30, 2010.
9. Martin Shubik Papers - David M. Rubenstein Rare Book & Manuscript Library, Duke University.
10. Vernon Smith Interview - two sessions at Chapman University on November 17, 2009.
11. Vernon Smith Papers - Vernon L. Smith Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University, Durham, North Carolina.
12. Richard Thaler Interview - one session at University of Chicago on June 16, 2014;
13. Eric Wanner Interview - one session in New York City on April 15, 2015.

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¹ I would like to thank the participants of the HES Session "Histories of Behavioral Economics" at the 2015 AEA/ASSA meetings in Boston, Colin Camerer, in particular, as well as the 2015 HOPE conference attendees, Ivan Moscati, in particular, and the anonymous referee for their helpful comments. My gratitude goes equally to my interview subjects listed in references. All errors remain mine.

² SVORENČÍK, A. & MAAS, H. 2015. *The Making of Experimental Economics: Witness Seminar on the Emergence of a Field*, Springer. The recollection of this conversation has a number of occurrences in Smith's writings, but in all instances the circumstances such when the encounter took place are not preserved. It first appeared as a cryptic note in 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. SMITH, V. L. 2008. *Discovery - A Memoir*, Bloomington, IN, AuthorHouse, HERTWIG, R. & ORTMANN, A. 2001. Experimental practices in economics: a methodological challenge for psychologists? *The Behavioral and Brain*

Sciences, 24, 383-403, SMITH, V. L. 1992. Game Theory and Experimental Economics: Beginnings and Early Influences. *History of Political Economy*, 24, 241-282.

³ The attendees were Eric Wanner, Colin Camerer (Pennsylvania), Donald Coursey (Washington), Robyn Dawes (Carnegie-Mellon), John Kagel (Houston), Daniel Kahneman (California 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 were 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” (Camerer Interview).

⁴ A similar case is Grether’s replication of Kahneman and Tversky’s experiments from the 1970s on the violation of the Bayes’ rule that led them to the “representativeness heuristic.” For details and references for Kahneman and Tversky’s papers, see: GRETHER, D. M. 1980. Bayes Rule as a Descriptive Model: The Representativeness Heuristic. *The Quarterly Journal of Economics*, 95, 537-557.

⁵ From the Russell Sage Foundation Board of Trustees Docket, June 1988, Subgroup 2, Box 194, Folder 1433, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

⁶ Letter from Don Coursey to Eric Wanner, 26 February 1987, Subgroup 2, Box 193, Folder 1424, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

⁷ For details about the history of Plott’s laboratory and the MUDA software, see SVORENČÍK, A. 2015. *The Experimental Turn: A History of Experimental Economics*. Ph.D. thesis. University of Utrecht.

⁸ Eric Wanner’s handwritten notes from the meeting. Subgroup 2, Box 193, Folder 1424, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

⁹ Camerer, Grether, Ledyard, Kagel, Plott, Smith, Thaler, and Wanner interviews. Both Grether and Ledyard attended the meeting as they were based at Caltech, but are not listed as official attendees.

¹⁰ My emphasis, Board Meeting memo, February 27, 1990. Subgroup 2, Box 7, Folders 69, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

¹¹ Thaler when asked about the meeting recalled that: “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.” Thaler interview.

¹² Thaler’s Proposal to the Behavioral Economics Program, February 1988. Subgroup 2, Box 172, Folder 1222, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

¹³ Coursey, Plott, and Tversky apparently formed a committee, but no evidence of output has been found. Coursey letter, 17 February 1988, Subgroup 2, Box 198, Folder 1463, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

¹⁴ Letter from 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 Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

¹⁵ Russell Sage Foundation Meeting of the Board of Trustees, Appendix D, February 27, 1990, Subgroup 2, Box 7, Folder 67, Russell Sage Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York.

¹⁶ The transition was facilitated by Camerer's high regard in both communities. He obtained a PhD in behavioral decision science from the University of Chicago, supervised by experimental psychologists. He has been involved in the Behavioral Economics Program from early on. And his first encounter with economics experiments was through Charles Plott. Plott spent the 1980 winter term in Chicago and gave a graduate course on experimental methods. Only a handful of students attended and Plott, as he always did in such a course, required them to devise an individual experimental project. Camerer and Shyam Sunder were two attendees of the Caltech conference who credit the beginning of their experimental career to Plott's Chicago course.

¹⁷ This gives a new quality to the review of Siegel's last book written by Ward Edwards: "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?" EDWARDS, W. 1967. Review: Decision and Choice: Contributions of Sidney Siegel. *Journal of the American Statistical Association*, 62, 291-293.