

## Chapter 2

# The Very Beginnings

### Choosing a Dissertation Topic

Chris Starmer      Good afternoon and welcome. For the record, let me say today is May 28, 2010. My name is *Chris Starmer*, and I am the moderator of this Witness Seminar on the *Emergence and Evolution of Experimental Economics*. The event is organized by *Harro Maas* and *Andrej Svorenčík*, and funded by the *Dutch Science Foundation*. We are at the premises of the *Royal Dutch Academy*,<sup>1</sup> and together with me are, from my right, participants *Frans van Winden*, *John Ledyard*, *Jim Friedman*, *Charlie Holt*, *Vernon Smith*, *John Kagel*, *Betsy Hoffman*, *Reinhard Selten*, *Charlie Plott*, *Al Roth*, and *Stephen Rassenti*.<sup>2</sup> Welcome to all. During the event, my plan is, over a number of sessions, to explore with you four broad topics.

These are loosely defined topics relating to the community of experimental economists, issues relating to funding, skills and techniques of experimental economics, and issues related to laboratories of experimental economics. I am not going to

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<sup>1</sup> The official name of the Dutch Science Foundation is The Netherlands Organisation for Scientific Research, the full title of the KNAW is *The Royal Netherlands Academy of Arts and Sciences*, or Koninklijke Nederlandse Akademie van Wetenschappen. It is housed at the Trippenhuis, Kloveniersburgwal 29, in Amsterdam. The seminar took place in *The Old Meeting Room* located on the first floor of the Trippenhuis Building with views out on the canal. The adjacent *Rembrandt Room* (so named because Rembrandt's famous *Nightwatch* covered one of its walls) was used for breaks.

<sup>2</sup> A twelfth participant Reinhard Tietz, a German experimental economist, was unable to attend the Witness Seminar due to an illness.

dwell at this point upon those themes, and I think in many ways, it is for us to create the story for those themes as we go along. The first session deals with issues related to the growth of the community of experimental economists, and I plan to start by asking two or three questions initially directed at particular individuals just to break the ice, so to speak, to the extent that the ice needs breaking. But then to very quickly move to open questions to which everybody is invited to contribute.

I am going to put the first question to Jim Friedman. It is related to your dissertation from Yale, in 1963, on the theory of oligopoly.<sup>3</sup> To my knowledge, it constitutes the first thesis within experimental economics.<sup>4</sup> I wonder if you can tell us something about the circumstances that led to the work in that thesis.

Jim Friedman

Yes. My first exposure to anything experimental in economics was in the Fall semester of 1959 when I was taking a required microeconomic theory course from *William Fellner* as a beginning graduate student. In that course he had a couple of experimental papers. I think one of them was Vernon's JPE competitive markets paper,<sup>5</sup> and the other was *Mosteller* and *Nogee* who were not economists but did a utility experiment.<sup>6</sup> Following that, in my second year, I was taking a course during the whole academic year from a visiting professor who was *Martin Shubik*.

On toward the spring of that year, when I was floundering about coming up with a dissertation topic, *Martin* said, "Why don't you do an experiment?" He had talked a little about experimentation during the course that I had had with him. Following that, in September of '61, which was the beginning of my third year—that was the point when qualifying and comprehensive examinations were being done. In the oral part of those examinations, I was asked by somebody on the committee, probably *Koopmans*, what were my dissertation plans.

<sup>3</sup> Friedman's dissertation was titled *The Theory of Oligopoly*. An abridged version was published in: **Friedman, James W.** 1963. "Individual Behavior in Oligopolistic Markets: An Experimental Study." *Yale Economic Essays*, 3(2), 359–417.

<sup>4</sup> In the same year F. Trenery Dolbear, now at Brandeis University, also graduated from Yale. An abridged version was published in: **Dolbear, F. Trenery.** Ibid. "Individual Choice under Uncertainty: An Experimental Study." 419–69.

<sup>5</sup> **Smith, Vernon L.** 1962c. "An Experimental Study of Competitive Market Behavior." *The Journal of Political Economy*, 70(2), 111–37. And a correction \_\_\_\_, 1962a. "Errata: An Experimental Study of Competitive Market Behavior." *The Journal of Political Economy*, 70(3), 322–23.

<sup>6</sup> **Mosteller, Frederick and Philip Nogee.** 1951. "An Experimental Measurement of Utility." Ibid. 59(5), 371–404.

I said, “Oh, I’m going to do an experiment,” I think, but I had no concrete plans of any sort. *Tjalling [Koopmans]* says have I seen the work of *Siegel* and *Fouraker*. This was referring to their second book, the one called *Bargaining Behavior*,<sup>7</sup> which at that point was unpublished, but it was circulating as two fat working papers.<sup>8</sup> Of course, I had not seen it. I had not heard of them. He lent me copies, and it was the basis of what I did. That work taught me a lot about how to run an experiment, and it provided also a starting point from which I designed what I did.

Chris Starmer As you initially described it, it seemed like the only option you had come up with. But you sound like you got more excited in the idea as you understood the work that was emerging.

Jim Friedman Well, what I would say, Chris, is this. I had previously been thinking about various areas [of economics] in which I never really got my mind around something I could figure out how to do and that would seem to have some merit. Somehow, just instinctively, when *Martin* made the experimentation suggestion, I had this feeling—yes, I think I can do that. I can do something interesting. And I had that sense without having a precise plan about what to do. But the plan itself then followed on very readily once I absorbed the Siegel-Fouraker manuscript.

Chris Starmer When did you become aware of Reinhard Selten’s work on oligopolies?

Jim Friedman [pause] I’m not sure. I know when I met Reinhard; we were just talking about that yesterday.<sup>9</sup> [It was] the summer or late spring of 1967. Summer to early fall was the end of a year that I had spent at Berkeley in which I had spent a great deal of time with *John Harsanyi*. It was [also] the beginning of a year when Reinhard was going to go to Berkeley and do the same thing. I spent a lot of time with *John*. But I would say he spent

<sup>7</sup> **Fouraker, Lawrence E. and Sidney Siegel.** 1963. *Bargaining Behavior*. New York: McGraw-Hill.

<sup>8</sup> **Siegel, Sidney; Lawrence E. Fouraker and Donald I. Harnett.** 1961. *Bargaining Behavior. Volume 1. The Uses of Information and Threat by Bilateral Monopolists of Unequal Strength*. Pennsylvania State University. **Fouraker, Lawrence E. and Sidney Siegel.** 1961. *Bargaining Behavior. Volume 2. Experiments in Oligopoly*. Pennsylvania State University.

<sup>9</sup> Both Friedman and Selten attended a dinner the night before the seminar. The other attendees were Marcia Friedman, Charlie Holt with wife Martha Ann Talman, Betsy Hoffman, John and Harriette Kagel, John Ledyard, Harro Maas, Andreas Ortmann, Charlie and Marianna Plott, Stephen Rassenti, Vernon and Candace Smith, and Andrej Svorenčik.

time there more effectively than I did.<sup>10</sup> [some laughter] But in any case, Reinhard stopped in New Haven [where Yale University is located.] I knew at least of him from *John [Harsanyi]*'s mention.<sup>11</sup> I don't know if I knew of him through other channels prior to that time. And then we met in the summer or early fall of '67.

Chris Starmer

Okay. Well, thank you. Perhaps I could turn to Reinhard because you published in Germany in 1959 the first paper on experimental economics by anyone sitting around this table. And I wondered how did you come up with the idea of doing an experiment?

## Crossing Disciplinary Boundaries

Reinhard Selten

[pause] When I began to study mathematics [at the Johann-Wolfgang-Goethe-University in Frankfurt am Main], I also went to psychology courses, and I actually studied a lot of psychology. The psychology students had to take part as subjects in experiments, at least at that time in Frankfurt, they had to have a certain number of points together in order to get later into the proseminar.<sup>12</sup> I gathered enough points and went also to the proseminar. I found the psychological experiments quite interesting. Afterwards, it was also always explained to us what the experiments were about. And so I got familiar with the technique of running experiments with human subjects. Not that I ran any on my own. I was just a subject. But that made me familiar [with them].

Later, I was doing game theory. After I finished my masters [in mathematics] on game theory,<sup>13</sup> I was hired by Professor *Sauermann* for a research project, which was about

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<sup>10</sup> **Editor:** Is this a reference to Selten and Harsanyi's work that led to their joint Nobel Prize? **Jim Friedman:** "No, I only meant that Reinhard and John clicked professionally in a way that John and I did not. John and I talked regularly, but we didn't get into any collaborative work and I don't think either of us had any significant impact on the other."

<sup>11</sup> Selten spent the academic year 1967–68 as a Visiting Full Professor at School of Business Administration, University of California at Berkeley. He met Harsanyi in 1961 when visiting Oskar Morgenstern, a good friend of his advisor Heinz Sauermann, in Princeton.

<sup>12</sup> A course of study for graduate and advanced undergraduate students, conducted in a manner of a seminar.

<sup>13</sup> Selten wrote his master's thesis in 1957.

application of decision theory to the theory of the firm. At this time, I had several things. First, I read *Herbert Simon* and got convinced of his ideas about bounded rationality.<sup>14</sup> Second, I saw a paper by—I don't remember the name of the author—the American Management Association edited a book about a computer management game.<sup>15</sup> I think it was the first, which was in the literature.

Also, I have read the seminal paper by Kalish, Millnor, Nash and Nehring,<sup>16</sup> the experimental paper from 1954 in the book edited by Thrall, Coombs and Davis *Decision Processes*. Of course, in my [doctoral] thesis, I have worked on cooperative games.<sup>17</sup> And I was quite impressed by the fact that it was possible to approach the questions of cooperative game theory by experiments.

Then I was very much interested in oligopoly theory. I read a lot about oligopoly theory. [After I had become aware of the computerized management game,] I thought that we can run oligopoly experiments and we do not need a computer for this purpose. What they can do with this computer, I also can do without it. I designed an oligopoly experiment with Cournot oligopoly [model] basically. After I had run this, my Professor *Sauermann* asked me to write it down. And I said, "Well, I don't know how to write an economics article [laughs], I have no feel for it." So we wrote it together.<sup>18</sup>

Chris Starmer

Did you feel like you were doing something very unusual at the time?

<sup>14</sup> **Editor:** Do you remember which papers did you read? **Reinhard Selten:** "I have read the seminal papers:" **Simon, Herbert A.** 1955. "A Behavioral Model of Rational Choice." *The Quarterly Journal of Economics*, 69(1), 99–118. and \_\_\_\_\_. 1956. "Rational Choice and the Structure of the Environment." *Psychological review*, 63(2), 129–38., both reprinted in \_\_\_\_\_. 1957. *Models of Man: Social and Rational; Mathematical Essays on Rational Human Behavior in Society Setting*. New York: Wiley.

<sup>15</sup> **Ricciardi, Franc M.** 1957. *Top Management Decision Simulation: The A.M.A. Approach*. New York: American management association.

<sup>16</sup> **Kalisch, Gerhard K.; J. W. Millnor; John F. Nash and E. D. Nering.** 1954. "Some Experimental N-Person Games," R. M. Thrall, C. H. Coombs and R. L. Davis, *Decision Processes*. New York: Wiley, 513–18.

<sup>17</sup> Selten's dissertation: **Selten, Reinhard.** 1961. "Bewertung Von N-Personenspielen," Frankfurt: Johann Wolfgang Goethe-Universität Frankfurt am Main.

<sup>18</sup> **Selten, Reinhard and Heinz Sauermann.** 1959. "Ein Oligopolexperiment." *Zeitschrift für die gesamte Staatswissenschaft*, 115, 427–71. Reprinted in **Sauermann, Heinz.** 1967. *Beiträge zur Experimentellen Wirtschaftsforschung*. Tübingen: Mohr., pp. 9–59.

English Translation **Sauermann, Heinz and Reinhard Selten.** 1960. "An Experiment in Oligopoly," *General Systems, Yearbook of the Society for General Systems Research*. Ann Arbor, MI: Society for General Systems, 85–114, 206.

- Reinhard Selten      Yeah. It was unusual, of course, because experimental economics did not exist as a field. We made a search of papers before our paper appeared. There were twenty papers in the literature, mostly in the psychological literature beginning with *Thurstone*.<sup>19</sup> But in the '50s, it began to get more frequent but also there are very few before that. I think that experimental economics as a field emerged only in the '60s.<sup>20</sup> I don't know when. And though it was something very unusual, my fellow assistants would laugh at me and call me Doctor Mabuse, the gambler.<sup>21</sup> [some laughter]
- And when there was a[n election] campaign of [the German Chancellor Konrad] Adenauer, which said "no experiments,"<sup>22</sup> they also showed it to me and said, "No experiments." So this were [unfinished]
- Chris Starmer      But it didn't worry you about crossing these boundaries?
- Reinhard Selten      It didn't worry me at all. We always sent, in our early experiments, discussion papers around. I must say that some people had doubts about the external validity of that,<sup>23</sup> but usually people did not oppose this. They found it quite interesting. It is not that these things got strong opposition. Of course, they were not dangerous for anybody, they were just a peculiarity.
- Chris Starmer      That is a subject I would like to come back to later on how results were perceived by and received by the relevant communities. I will definitely come back to that. I wonder now if I could just move to Vernon and ask you about your

<sup>19</sup> **Thurstone, Leon L.** 1931. "The Indifference Function." *Journal of Social Psychology*, 2, 139–67.

<sup>20</sup> **Editor:** Were you aware of agricultural experiments or time and motion studies? **Reinhard Selten:** "I knew of agricultural studies and time studies but I did not think of them as part of experimental economics. I understood experimental economics as aiming at the advancement of economic theory by the observation and analysis of the behavior of economically motivated subjects in laboratory or field experiments. I still think that this is a reasonable definition. It seemed to me, that the agricultural experiments and time studies, I knew of, did not contribute to the advancement of economic theory."

<sup>21</sup> The character Dr. Mabuse, a supervillain and a master of hypnosis, was introduced first in a novel *Dr. Mabuse, the Gambler* by Norbert Jacques in 1919 and featured in several popular movies.

<sup>22</sup> The slogan of 1961 campaign of the incumbent German government party was "Auch heute keine Experimente, CDU" [No experiments today either, the Christian Democratic Union].

<sup>23</sup> **Editor:** Were the doubts about experiments you mention explicitly phrased in terms of external validity at that time? **Reinhard Selten:** "We got letters from people to whom we sent discussion papers which expressed such doubts. However, they did not use the term "external validity." I do not remember when I first encountered this term but it was certainly only much later."

- initial meetings with other experimental economists. I think that you met with *Sidney Siegel* in Stanford in the early '60s, perhaps in 1961. When did you learn about the research that was going on in Germany by Selten and others?
- Vernon Smith I don't recall. I do remember, I think it was in the '60s, Reinhard was running some seminars in Germany. And I was invited to one or two of those, but I never made it.<sup>24</sup> And I don't recall when I first met Reinhard.
- Chris Starmer Did you know other experimentalists before meeting Sidney Siegel?
- Vernon Smith Well, by the early '60s, and well before that actually, I knew of the Mosteller and Noguee experiment.<sup>25</sup> I don't know when. I would have read that pretty early. And I knew of the work that was summarized by *Andreas [Ortmann]*<sup>26</sup> like the Thurstone experiments. There was something on social indifference curves by Hart<sup>27</sup> in 1930s. I never read Morgenstern's book.<sup>28</sup> I had a copy of the *Decision Processes* book since it originally came out, so I was familiar with that. In fact, when I first started teaching experimental economics, a graduate seminar in 1963, we didn't have enough literature of our own to really make a course that we were developing at Purdue where I used the

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<sup>24</sup> Smith was invited to the first, second, and third International Conference on Experimental Economics that took place in 1971, 1977, and 1982 respectively as evidenced in **Smith, Vernon L.** "Smith Papers," *Vernon Lomax Smith Papers*. David M. Rubenstein Rare Book and Manuscript Library, Duke University, Durham, North Carolina.

<sup>25</sup> **Editor:** Were you aware of agricultural experiments, field experiments such as the negative income tax? **Vernon Smith:** "I knew of the agricultural production experiments designed to measure input substitution in the production of crops, milk, and so on, and it always had seemed to me the natural way to approach supply as well as demand issues. An important paper that I read and assigned in class was **Heady, Earl O.** 1957. "An Econometric Investigation of the Technology of Agricultural Production Functions." *Econometrica*, 25(2), 249–68. This paper would have appeared a year and a half after I started doing experiments in January 1956. Iowa State University had been an important center for this development."

<sup>26</sup> This is the paper that was sent to the participants in advance of the seminar to stimulate their memories.

<sup>27</sup> **Rousseas, Stephen W. and Albert G. Hart.** 1951. "Experimental Verification of a Composite Indifference Map." *The Journal of Political Economy*, 59(4), 288–318.

<sup>28</sup> Morgenstern's book was mentioned in Ortmann's paper. **Morgenstern, Oskar.** 1963. *On the Accuracy of Economic Observations*. Princeton, N.J.: Princeton University Press. (revision of the first edition from 1950).

Fouraker and Siegel, the working paper volumes or copies of those.<sup>29</sup>

[Besides the work outlined by Andreas] there was work in experimental games by social psychologists. Oh, and Ward Edwards' 1954 paper.<sup>30</sup> In fact, I met *Ward Edwards* early.<sup>31</sup> It would have been certainly by the early '60s. In the '60s at Purdue we regularly had visiting speakers in, and Ward Edwards would have come at least once. And also *Anatol Rapoport* who was at Michigan. In fact, I used Ward's work in decision-making under uncertainty, many of his papers in the early classes that I taught.

Now, as the literature began to develop in experimental economics, I replaced some of that early work. It became less of a course on decision-making and more of a course on markets. And I started to use some of the papers or books. Of course, *Bargaining Behavior* was published in 1963. Oh, and then there is the 1960 book. [inaudible] What's that?

Jim Friedman  
Vernon Smith

*Bargaining and Group Decision Making*.<sup>32</sup>

Yes, I used that. But this was after I had met Sid Siegel.

<sup>29</sup> **Editor:** What other literature did you use? **Vernon Smith:** "The working papers included other authors, prominently *Martin Shubik* who knew and worked with *Sid Siegel*, and Sid's student Don Harnett. The final Fouraker Siegel book (1963) was published after Sid's untimely death at age 45 in 1962; it incorporated the work of Martin and Don from the working papers. Martin had worked with Sid and Larry *Fouraker* on the Cournot oligopoly experiments. *Siegel* and Harnett on the experiments using GE executives as subjects in their replication of the bargaining experiments. All are credited in the preface by Larry, but none survived as co-authors which I thought was ungenerous having used all the material in classes before it was published. Martin and Sid had a large agenda for further work that would have been path breaking at the time and established experimental economics much more prominently in the 1960s and 70s, but ended with Sid's death. People today have no idea of Sid's energy and depth."

<sup>30</sup> **Edwards, Ward.** 1954. "Variance Preferences in Gambling." *The American Journal of Psychology*, 67(3), 441–52.

<sup>31</sup> **Editor:** I wonder whether you interacted with *Ward Edwards* during your stay at USC in the academic year 1974/75. **Vernon Smith:** "Yes, I did. We got together sometime in that period, and I attended probably 2–3 of his Behavior Decision Theory conferences over in the valley in the 1970s, probably then, but also after I went to Arizona. I have long thought that the important early contributions of *Edwards* (whose father was a known economist) and *Anatol Rapoport* deserved more recognition. They were the pioneers that trained and created the generation of psychologists who (e.g. Slovic, Lichtenstein) did what was to be called Behavioral Economics."

<sup>32</sup> **Siegel, Sidney and Lawrence E. Fouraker.** 1960. *Bargaining and Group Decision Making; Experiments in Bilateral Monopoly*. New York: McGraw-Hill.



## Early Meetings and Seminars

- Chris Starmer And I think in the early '60s, perhaps '63, '64, there were meetings at Carnegie Institute that you participated in. Is that right? You met other experimentalists there?
- Vernon Smith Oh, are you talking about the Ford Foundation?
- Chris Starmer: Yes, Ford Foundation Research.
- Vernon Smith *Lester Lave*<sup>33</sup> was at Carnegie Institute of Technology before they changed the name,<sup>34</sup> and Lester had done a couple or three papers on prisoner's dilemma games.<sup>35</sup> And we had been in communication.<sup>36</sup> *Dick Cyert*<sup>1</sup> at Carnegie Tech, also *Herb Simon* I'm sure would have been a factor in encouraging this, led us to make a proposal to the Ford Foundation to do summer faculty research workshops. The first one we did was in the summer of 1963. Then we did one in the summer of 1964. And as I recall, [Roger] *Sherman* was there. Jim, were you at either of those?
- Jim Friedman No.
- Vernon Smith Okay.
- Jim Friedman: I was at something—no that was later. I was at something at Berkeley, but that was [in] '68.<sup>37</sup>
- Vernon Smith Yes. And I don't know who else might have been at that. Oh, *Bill Starbuck* probably would have been at one of those. *Bill Starbuck* came to Purdue from Carnegie Tech sometime in the '60s.
- Chris Starmer Was this meeting focused on experimental economics?
- Vernon Smith *Bill Starbuck's* focus was on everything. [laughter] He had wide-ranging interests. And if you were doing experiments, he was interested in that. He clearly had some exposure to

<sup>33</sup> **Frank Trenery Dolbear Jr.** received his Ph.D. in Economics from Yale in 1963. He spent the next three years at Carnegie Institute of Technology. Since 1968 he has been at Brandeis University. He was active in experimental research only in the 1960s.

<sup>34</sup> Carnegie Institute of Technology became Carnegie Mellon University in 1967.

<sup>35</sup> **Dolbear, F. Trenery and Lester B. Lave.** 1966. "Risk Orientation as a Predictor in the Prisoner's Dilemma." *The Journal of Conflict Resolution*, 10(4), 506–15. **Lave, Lester B.** 1962. "An Empirical Approach to the Prisoners' Dilemma Game." *The Quarterly Journal of Economics*, 76(3), 424–36. \_\_\_\_\_. 1960. *An Empirical Description of the Prisoner's Dilemma Game*. Santa Monica, Calif.: Rand Corporation, \_\_\_\_\_. 1965. "Factors Affecting Co-Operation in the Prisoner's Dilemma." *Syst. Res. Behavioral Science*, 10(1), 26–38.

<sup>36</sup> See for instance correspondence from 1965 and Smith evaluation of Lave's experimental work from 1969. **Smith, Vernon L.** "Smith Papers," *Vernon Lomax Smith Papers*. David M. Rubenstein Rare Book and Manuscript Library, Duke University, Durham, North Carolina.

<sup>37</sup> In July 1968 *Austin Hoggatt* together with John T. Wheeler directed the Workshop in Experimentation in Management Science at University of California, Berkeley. Friedman attended for one month. See for instance, **Hughes, G. David and Philippe A. Naert.** 1970. "A Computer-Controlled Experiment in Consumer Behavior." *The Journal of Business*, 43(3), 354–72.

- experiments and certainly, the idea of experimentation by the time he came to Purdue. He had been influenced by *Herb Simon* and the crew at Carnegie Tech. I am pretty sure that he probably was at that seminar. Later, he was instrumental in designing the first lab at Purdue.<sup>38</sup>
- Chris Starmer And would you think of that as a gathering of experimental economists? I suppose what I am getting at is was this one of the first meetings of experimental economics? Would that be a characterization?
- Vernon Smith Yes. And we went through the literature that was available at the time in 1963. And the participants were encouraged to work on projects and to do some experimental work.

## Chance Encounters and Conversions

- Chris Starmer Okay. Thank you. I think what I would really like to do now is to open things up to you to choose when you want to contribute and what you want to say. When I introduce a topic, if you want to speak to it, can I remind you of the convention to raise your hand. If there are many hands, I will try and keep a record of the sequence. I will try and acknowledge you when you have raised your hand, so you won't have to keep it in the air. You will hopefully know you have made my list. And it is one hand for a new theme and two if you have got some interjection, which is very closely related to what is being discussed as the present topic.
- I should also say that if there are particularly popular topics that many people want to contribute to across the course of these sessions, it may not be possible to practically let everybody speak to every topic that they might have something to contribute to. But hopefully, over the course of the two days, there will be good opportunities for all to speak. But apologies if I, at times, move from topics before you have

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<sup>38</sup> In an autobiographical essay *Starbuck* details his experience with experimental work in economics and psychology, influence of *Simon* and *Cyert* and his involvement in designing computerized social science laboratories (1967): **Starbuck, William H.** 1993. "Watch Where You Step!" *Or Indiana Starbuck Amid the Perils of Academe (Rated PG)*, *Management Laureates: A Collection of Autobiographical Essays*. Greenwich, Connecticut; London, England: JAI Press, 63–110. See also **Cyert, R. M.; J. G. March and W. H. Starbuck.** 1961. "Two Experiments on Bias and Conflict in Organizational Estimation." *Management Science*, 7(3), 254–64. **Starbuck, William H. and Frank M. Bass.** 1967. "An Experimental Study of Risk-Taking and the Value of Information in a New Product Context." *The Journal of Business*, 40(2), 155–65. For the joint work with Vernon Smith on a laboratory at Purdue see Chapter 6, Footnote 8.

an opportunity to speak. I think it is inevitable that people will have more to say than we have a chance to hear.

As a first theme, I'm interested in exploring how people got started in doing experimental research. Perhaps I might ask you to think about two dimensions of this. One is how did you first become aware of experimental research? And secondly, what drew you into doing it? And so if I could invite people who would perhaps like to speak on that topic? Betsy.

Betsy Hoffman: Actually, I might cede to Charlie [Plott]. Charlie, did you have your hand up?

Charlie Plott: Yeah. I was going to say something about that.

Betsy Hoffman: Why don't you start, and then I will follow you because what I have to say follows from what Charlie is going to say.

Charlie Plott: At least my experiments, my exposure came through chatting with Vernon. I was at Purdue in the late '60s.<sup>39</sup> And Vernon continued to tell me about the convergence in his demand and supply experiments. I thought that his results were silly, and it was clearly not demand and supply, but it must have been a Bayesian game. So I had been touched by *Harsanyi*. 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, which I considered to be quite different principles. I commandeered a graduate student named *Harvey Reed* who did this [experiment] and immediately he demonstrated that my beliefs were correct.<sup>40</sup> II Later, I found out,<sup>41</sup> however, that *Harvey* was a terrible experimentalist, and the procedures he was using were really embarrassing.<sup>42</sup>

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<sup>39</sup> Plott was at Purdue from 1965 until 1971.

<sup>40</sup> **Reed, Harvey Jay.** 1973. "An Experimental Study of Equilibrium in a Competitive Market," Purdue University, Plott and Carl H. Castore were on Reed's committee.

<sup>41</sup> Paper presented "An Unsuccessful Attempt to Experimentally Discredit the Law of Supply and Demand" (with H. Reed), Western Economics Association Meeting, Las Vegas, 1974.

**Editor:** When did you find this out? **Charlie Plott:** "I found that out in 1973 when I had a student attempt to replicate Harvey's experiments. Much later I published a paper that discussed the matter and introduced the concept of "reparameterization" as a way to understand and interpret the data as actually supporting the theory that the experiments were originally intended to explore."

<sup>42</sup> **Editor:** Why were Reed's procedures embarrassing? **Charlie Plott:** "The procedures he used are now known to create market inefficiencies. He was conducting a market experiment following Vernon's work when I was still at Purdue, probably somewhere near 1970. Harvey had an environment with two units and the appropriate way to induce the incentives was not developed until my work with Fiorina and the proper way to conduct the double auction when individuals could trade multiple units was not developed until my much later work with Vernon."

The next encounter [with experiments] was again shadowed with Vernon. This is around 1969, 1970,<sup>43</sup> and I was interested in the mathematics of axiomatic social choice theory and voting. These are public goods environments. And I realized that one could take Vernon's idea about induced preferences and induce them in a much broader economic environment. In that sense I began to test things that were evolving out of voting theory and out of cooperative game theory without side payments, which is much different from the bargaining problem.<sup>III</sup>

We were studying the core, bargaining sets and [were] doing so within institutions that could be precisely defined. Features of Robert's Rules<sup>44</sup> then led us into discovering that there was a host of principles coming out of game theory, not cooperative game theory, not with games with side payments, but out of dominance relation and treating these abstractly that were extremely powerful in demonstrating and predicting. And I became associated with *Morris Fiorina* who had worked with *Bill Riker*, who was essentially doing the same thing in political science.<sup>45, IV</sup> Bill and Mo had studied many, many procedures.<sup>V</sup>

That is how I got started. Then Vernon came to Caltech where he began to get more focused on markets and market institutions.<sup>46</sup> That set that stage<sup>VI</sup> and, of course, there is many, many, many results and discoveries that evolved from there.<sup>VII</sup>

Chris Starmer  
Betsy Hoffman

Thank you. Betsy, do you want to follow?

Yes, I was pretty sure what Charlie was going to say, and I knew that I would follow directly on from what he was saying. I came to Caltech in 1975 as a graduate student. Having been a historian, I have a Ph.D. in history as well,<sup>47</sup> I came to Caltech recruited by *Lance Davis* for the sole purpose of improving my quantitative and theory skills so that I could go back to economic history with a new set of tools. I came to Caltech with a very

<sup>43</sup> The exact dates remain unclear, but dating to 1970–1971 seems as more precise.

<sup>44</sup> Robert's Rules of Order is a set of rules for running meetings and conferences.

<sup>45</sup> After moving to Caltech in 1971.

<sup>46</sup> Smith visited Caltech, where he received his undergraduate degree in 1949, as a Fairchild Scholar in the academic year 1973–74. Bill Riker was also in residence.

<sup>47</sup> Hoffman earned her Ph.D. in history from University of Pennsylvania in 1972. The dissertation was entitled "The Sources of Mortality Changes in Italy since Unification." It later appeared in book form **Hoffman, Elizabeth**. 1981. *The Sources of Mortality Changes in Italy since Unification*. New York: Arno Press.

clear purpose. In fact, I didn't even intend to finish a second Ph.D. I really came just to get the tools. It was like a post-doc.

But after my first year at Caltech, I got talking to an economist who told me that the jobs were much better in economics, and this was somebody outside of Caltech.<sup>viii</sup> This was somebody who didn't have a particular personal interest in my continuing in economics and said that I would really, as long as I had invested as much time as I had and I had taken all the classes and I passed prelims, that there was huge benefit to my finishing the Ph.D. in economics, even if I went into economics as an economic historian. I continued [with the doctoral program], and I actually ended up writing a dissertation that is a quantitative economic history dissertation on the Colorado River compact.<sup>48</sup>

I wrote a dissertation that is totally unrelated to experimental economics and much more related to the purpose for which I went to Caltech. But my last quarter there,<sup>49</sup> I had to take some classes, and I had taken all the classes that I felt were important for the purpose for which I had come. But as long as I was going to finish a Ph.D., I had one more quarter of classes I had to take. *John Ferejohn* was my mentor, my advisor, and I went to him and I said, "So John, what's left that I should take?" And he said, "Well, you can't leave Caltech without taking Charlie's seminar in experimental economics." And I said, "Why? What good is it going to be to me as an economic historian?"

And he said, "It's just that this is the most important thing that is happening at Caltech right now," and "you can't leave Caltech without taking the seminar." I said, "Fine." I mean, why not. I had three courses I had to take to check off the last three boxes. And it changed my life. I took this seminar, and it was the most fun. I had lots of fun as an economic historian. I had done lots of fun things. But this was the most fun I had ever had.

Chris Starmer      Okay. I'm going to make a mental note of the fact that it changed your life, and I'm going to return to that when we come to the section on skills and ask you about that again.

Betsy Hoffman      That is fine.

Chris Starmer      Al, you signaled a little while ago.

<sup>48</sup> **Hoffman, Elizabeth.** 1979. "Essays in Optimal Resource Allocation under Uncertainty with Capacity Constraints," Pasadena: California Institute of Technology.

<sup>49</sup> **Editor:** The chronology does not seem correct. **Betsy Hoffman:** "It was the third quarter of my second year. I wrote my dissertation the next year and graduated in 3 years."

Al Roth

I came from a different tradition. I studied game theory, and I took a game theory course when I was a graduate student at Stanford<sup>50</sup> from *Michael Maschler* who was visiting from the Hebrew University. He didn't talk much about experiments, but he talked a little bit about the work of *Amnon Rapoport* who was interested in the bargaining set, which Michael was interested in. There were lots of, not lots, but there were experiments in game theory going back a way starting with the prisoner's dilemma maybe.<sup>51</sup> When I came to the University of Illinois,<sup>52</sup> having written a theoretical thesis, one of the other fellows who came just as I did was *Keith Murnighan*, a social psychologist from Purdue.

He and I got along well, and we thought that we would try to do some experiments on the games that I had written my dissertation about. For him, it was natural to go into the lab, and there were lots of experiments by psychologists on bargaining and on group interactions and things like that. That was the literature I was initially aware of. I think probably the first experiments by someone around this table that I became aware of were Charlie's committee experiments having to do with the core,<sup>53, IX</sup> because, again, those were in the game theoretic tradition.

Chris Starmer

John and Reinhard?

Reinhard Selten

I wanted you to add that *Mike Maschler* was actually one of the first experimenters. He did experiments maybe around 1960 with cooperative characteristic function games. Before he got a university position, he was a high school teacher. He had his high school class play characteristic function games with 3 or 4 players. They bargained face to face about coalitions to be formed and the division of payoffs among the members. There was no time restriction. At the end of a game they had to turn in a card showing what they had agreed

<sup>50</sup> Roth studied at Stanford between 1971 and 1974.

<sup>51</sup> **Kalisch, Gerhard K.; J. W. Millnor; John F. Nash and E. D. Nering.** 1954. "Some Experimental N-Person Games," R. M. Thrall, C. H. Coombs and R. L. Davis, *Decision Processes*. New York: Wiley, 513–18.

<sup>52</sup> Roth moved to the University of Illinois in 1974 and remained until 1982.

<sup>53</sup> This is the already mentioned seminal paper **Fiorina, Morris P. and Charles R. Plott.** 1978. "Committee Decisions under Majority Rule: An Experimental Study." *The American Political Science Review*, 72(2), 575–98. and **Isaac, Mark R. and Charles R. Plott.** 1978. "Cooperative Game Models of the Influence of the Closed Rule in Three Person, Majority-Rule Committees: Theory and Experiment," P. Ordeshook, *Game Theory and Political Science*. New York: NUY Press, NA.

upon and for each player separately the reasons for her or his conduct.

I met him at Princeton in '61 at a game theory conference<sup>54</sup> organized by *Oscar Morgenstern* who had invited me and made it possible for me to be there. At this conference Mike Maschler already had a list of all his experimental results with him. He must have run his experiments in 1960 or even before. Later he sent me his research report.<sup>55</sup> He had submitted it for publication to the *Journal of Conflict Resolution* and they asked him to shorten it, but he never did this.

At one of the conferences in Germany on *Bargaining* and *Coalition Formation* I told him that we would in our volume print his research paper completely as it was.<sup>X</sup> He wouldn't have to change anything. We will just print it. So almost 18 years later, it was published.<sup>56</sup>

*Maschler's* research report had a great influence on my own work. I very carefully looked at his experimental results and the reasons given by the players. Finally I came up with something called "equal share analysis," a behavioral theory about n-person games in characteristic function games. His work was very important for me.

Chris Starmer

I will take perhaps three more on this point. John, Charlie, and Jim. John.

John Kagel

I came to this in a totally different way. I was at Purdue when Charlie was on my comprehensive exam committee; I remember distinctly failing his question. But the interesting or perhaps surprising thing is that there was no history of Vernon's work since Vernon had left by the time I arrived.<sup>57</sup> There was no history of that work there or any of that being

<sup>54</sup> Published privately in 1962 as **Princeton University, Conference**. 1961. "Recent Advances in Game Theory; Papers Delivered at a Meeting of the Princeton University Conference, October 4–6, 1961," *Recent advances in game theory*. Princeton, N.J.: Princeton University Press. The introduction was written by Maschler. Contributions were made by Morgenstern, Vickrey, Fouraker, Suppes, Afriat, Aumann, Shapley and many others.

<sup>55</sup> **Maschler, Michael**. 1972. "Equal Share Analysis of Characteristic Function Experiments," H. Sauermann, *Contributions to Experimental Economics = Beiträge zur Experimentellen Wirtschaftsforschung*. Tübingen: Mohr, 130–65.

It was based on an 1965 working paper \_\_\_\_\_. 1965. *Playing an N-Person Game, an Experiment*. Princeton, N.J.: Princeton University, Econometric Research Program Research Memorandum No. 73.

<sup>56</sup> \_\_\_\_\_. 1978. "Playing an N-Person Game: An Experiment," H. Sauermann, *Bargaining Behavior. Contributions to Experimental Economics = Beiträge zur Experimentellen Wirtschaftsforschung*. Tübingen: Mohr, 231–328.

<sup>57</sup> Kagel started at Purdue in 1967 and he received his doctorate in 1970.

taught.<sup>XI</sup> I got into doing experiments because I was a graduate student of *Bob Basmann*. And *Basmann* was an econometrician who was very concerned with that the data correspond to the primitives in our economic models. And he was also interested in individual choice.<sup>XII</sup>

The standard field data that is available for that kind of thing just didn't fit. In terms of looking at that I had come across the experiments by *Thurstone* and *May*<sup>58</sup> and those people. *Ray Battalio* and I were just looking for a place where we could collect individual consumer choice data. It was just by happenstance that we learned about token economies.<sup>59</sup> And by happenstance there was a gentleman [*Robin C. Winkler*] at *Stony Brook* at the time who was a psychologist and was also interested in economics of token economies. There was a natural connection there. We hooked up and started to do our first experiment in a backwater of a mental institution, which is to say the least somewhat unusual.<sup>60</sup>

At one point, you raised the issue of how were results received by different communities. Well, you can imagine how some of these results were received by certain communities. One of the interesting elements of how it was received by one community though was at the time that we were doing the experiment, I gave a talk to the psychology department at *Stony Brook*. I don't think anyone in economics bothered to show up. They wouldn't have advertised it there. We were talking about revealed preference theory and things like that.

One guy got up in the middle of the talk and said that if economics is so primitive, I have got nothing to learn here and walked out of the room. [some laughter] At the end of the talk, *Howard Rachlin* and *Leonard Green* came up to me and said, "We've already done this sort of experiment." I said,

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<sup>58</sup> **May, Kenneth O.** 1954. "Intransitivity, Utility, and the Aggregation of Preference Patterns." *Econometrica*, 22(1), 1–13.

<sup>59</sup> **Ayllon, Teodoro and Nathan H. Azrin.** 1968. *The Token Economy; a Motivational System for Therapy and Rehabilitation*. New York: Appleton-Century-Crofts, **Kagel, John H.** 1972. "Token Economies and Experimental Economics." *Journal of Political Economy*, 80(4), 779–85.

<sup>60</sup> **Battalio, Raymond C.; Edwin B. Fisher; John H. Kagel; Robert L. Basmann; Robin C. Winkler and Leonard Krasner.** 1974. "An Experimental Investigation of Consumer Behavior in a Controlled Environment." *Journal of Consumer Research*, 1(2), **Battalio, Raymond C.; John H. Kagel; Robin C. Winkler; Edwin B. Fisher; Robert L. Basmann and Leonard Krasner.** 1973. "A Test of Consumer Demand Theory Using Observations of Individual Consumer Purchases." *Economic Inquiry*, 11(4), 411–28.



“Yeah? Okay, show me.” And that led to our designing our own experiments.<sup>61</sup> I learned about these other people when [Martin] Shubik put a session together at the AEA meetings. I don’t know what year that was.<sup>62</sup> So just my ignorance about what other people were doing. And we found out about Vernon and Charlie and other people.

We immediately wrote to them and told them what we were doing.<sup>63</sup> There wasn’t email back then, so it took a little while. But that is how I became aware of what other people were doing.

Chris Starmer  
Vernon Smith

Thank you. Is that a one hand up Vernon? It’s a two hand up. I think it is great that John started on this [experimental work] uncontaminated by anyone else because this was, I think, very exciting work that he and Ray [Battalio] did. In fact, it was the only game in town at the individual decision and classic preference theory work. There is one thing I wanted to mention that is in the Ortmann paper. He refers to the Radford paper on economics of a prisoner of war camp.<sup>64</sup> I learned of that work very early. In fact, it was pretty widely known, and a lot of economists were interested in that paper. It was just a really wonderful contribution.<sup>XIII</sup> It helped to influence me on the possibility of experimental [work]—although that was not a controlled experiment.

Chris Starmer  
Charlie Plott

Thank you. Charlie.  
Just in terms of a couple of names. Keith Murnighan, as it turns out, was at Purdue. Keith was a student of Carl Castore who was a psychologist, and Keith and Carl sat in my social choice course.<sup>65</sup> We actually looked at voters’ paradoxes. Now, the interesting thing about that was that Carl listened to this, and he saw the cycle in voting,<sup>66</sup> and he says we can test that. He decided to get [LP long play vinyl] records, [and follow] the

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<sup>61</sup> Kagel, John H.; Raymond C. Battalio; Howard Rachlin; Leonard Green; Robert L. Basmann and W. R. Klemm. 1975. “Experimental Studies of Consumer Demand Behavior Using Laboratory Animals.” *Ibid.* 13(1), 22–38.

<sup>62</sup> This could be the 1975 AEA session which was, however, not organized by Shubik but by Gary Becker.

<sup>63</sup> The earliest letter from Charlie Plott to John Kagel is from January 1977, and Battalio from June 1974. Plott, Charles R. “Plott Papers,” *personal archive of Charles Plott*. California Institute of Technology,

<sup>64</sup> Radford, R. A. 1945. “The Economic Organization of P.O.W. Camps.” *Economica*, 12, 189–201.

<sup>65</sup> That was in the academic year 1970–71, just before Plott left for Caltech.

<sup>66</sup> The voting paradox or Condorcet’s paradox is a situation in which collective preferences can be cyclic (i.e. not transitive), even if the preferences of individual voters are not.

typical psychologist approach. He went around and asked people their preferences for these big records. And so then he was going to have a group choose a record that everyone could get and structured them so there was intransitivity.

The interesting thing about it was that at that time, it wasn't clear what he was going to learn. We didn't realize that procedures [were overwhelmingly important]. We didn't have the dominance relation looking at it. We really hadn't separated the idea of games without side payments [from games in characteristic function form], which we did later.<sup>67</sup> And so he did this [experiment], and it seemed he didn't learn anything. But that was a clear step towards the problem of trying to control preferences in these very complex areas where everything is of public goods [nature].<sup>XIV</sup> *Carl Castore* in some sense was an instrumental step. He even got some money to study this. I think that nothing finally came of it, but he is a very interesting name.<sup>68</sup>

And another name that is interesting with respect to Purdue was a guy named *Cliff Lloyd*. *Cliff Lloyd* was absolutely fascinated with the problem of testing preference theory. He thought preference theory was testable. And *Cliff* had an influence; you could see a lot of heads shaking here.<sup>XV</sup> *Cliff* had an influence on a lot of us because he looked to the second order conditions and the symmetry of the substitution matrix, and he said that substitution matrix is just loaded with testable propositions. He spent much of his time trying to understand how one would set up an experiment. He even tried to contract for a little village in Alaska so he could actually control the incomes to test for the symmetry of the substitution matrix.<sup>69</sup>

<sup>67</sup> Reference to non-cooperative game theory. See earlier statement by Plott. For a discussion of the dominance relation see **Plott, Charles R.** 1976. "Axiomatic Social Choice Theory: An Overview and Interpretation." *American Journal of Political Science*, 20(3), 511–96.

<sup>68</sup> In fact, Castore and Murnighan published a study based on their LP experiments. However, they did not investigate intransitivities to any extent. **Castore, Carl H. and J. Keith Murnighan.** 1978. "Determinants of Support for Group Decisions." *Organizational Behavior and Human Performance*, 22(1), 75–92.

Earlier versions appeared in 1972–3 as working papers **Castore, Carl H.** 1972. *Intragroup Concordance and the Effectiveness of Majority Rule Decisions*. Ft. Belvoir: Defense Technical Information Center. Purdue Univ Lafayette, **Castore, Carl H. and J. Keith Murnighan.** 1973. *Decision Rule and Intragroup Goal Concordance as Determinants of Individual Reactions to and Group Decisions*. Ft. Belvoir: Defense Technical Information Center. Purdue Univ Lafayette.

<sup>69</sup> The location of Lloyd's experiment was Postville, Newfoundland, in Canada, but he considered other locations as well. This research was published only posthumously under the title *Northern Store Project*. **Lloyd, Cliff.** 1980. *The Collected Works of Cliff L. Lloyd*. Burnaby, B.C.: School of Business Administration and Economics, Simon Fraser University.

Anyway, that was *Cliff's* major thing. He was also a student of *John Hicks*. There is a real continuity of this kind of asking what are the principles of individual decision-making that might lead to testable propositions. Anyway, what influenced all of us at Purdue was *Cliff's* worrying about this particular problem [of empirical support of theories]. He was leading all of us to ask such questions about these complex theories. What features of models might lead to a testable proposition? The idea of an experiment was never in question. The idea of a testable proposition was never in question. All this was just a second nature for most of us who were in that environment.

Now, in terms of receptiveness, let me make one comment. In the late '60s I moved away from individuals into groups [decision making]. I think that was a major transition.<sup>XVI</sup> When we started then going to the [professional] meetings and submitting our papers, they organized us in economics of education.<sup>70</sup> [some laughter] Namely, they looked at all of our work as nothing but pedagogical devices. There was no science there [for the organizers] at all. <sup>XVII</sup>

## The JEL-Code and Closeted Experimentalists on the Job Market

Chris Starmer	How do you think you should have been classified if you had been able to choose?
Charlie Plott	How should we have been?
Chris Starmer	Yes.
Charlie Plott	Well, I think that we were dealing with the [empirical] foundations of economics [but] economics does not have [such] a classification. If the experiment was a committee experiment, I would have put it having to do with something with public choice. If it was a market experiment, I would have had it in microeconomics. I wouldn't have separated it out as anything special. It is data about phenomena [and the

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<sup>70</sup> **Editor:** I have not found any evidence for this so far. **Charlie Plott:** "The first experimental paper I gave at a professional meeting was placed in a section of education. It might be noted that in the 1960's and perhaps today, there was a healthy use of hands on methods to demonstrate economics but it was not viewed as experiments. Some of the economists who were focused on economics education were at Purdue."

- empirical relationships the data present]. But that is the way it was treated—just education.
- Chris Starmer Thanks, Charlie. Betsy.
- Betsy Hoffman I wanted to build on that, and this is 10 years later. I'm on the job market in 1978 and was basically counseled, even by Charlie, not to talk about this really exciting work I was doing in experimental economics,<sup>xviii</sup> but to focus in my job talks completely on my work on the Colorado River compact and to never mention experimental economics. John [Ledyard] attended one.
- Chris Starmer And why was that?
- Betsy Hoffman Because it was considered that I would be a pariah on the job market if I sold myself as an experimental economist. But the junior faculty, at every university where I went to give a talk, the junior faculty would whisk me into their offices and close the door and say okay, I want to hear about experimental economics.<sup>71</sup>
- Chris Starmer And were you starting to think of yourself as an experimental economist at that point?
- Betsy Hoffman I was in both. I had my feet in both camps [economic history and experimental economics]. John [Ledyard] really helped me get the interview and get the job at Northwestern.<sup>72</sup> I know that even though he never said anything about it. But it was the economic historians who took me under their wing and really probably made me get hired. Now, John greased the skids, but I think without *John Hughes* and *Joel Mokyr*, it would have been hard to persuade the rest of the department to hire me. The economic historians adopted me. I taught economic history. I was part of the economic history seminar. I was writing and published a book in economic history.<sup>73</sup>
- But I was starting to do experiments, and I started my work with *Matt Spitzer* who had been at Caltech with me and was in the law school [at Northwestern]. I started my work with *Ed Packel* who was and still is at Lake Forest. Matthew and I put in the first NSF grant, and I know we are going to be talking about

<sup>71</sup> **Editor:** Where did you have your interviews? **Betsy Hoffman:** “I interviewed at Arizona, Swarthmore, Washington, Iowa, Ohio State, Northwestern, and Boston College. I had more scheduled, but cancelled the rest when I got offers from Northwestern and Swarthmore, my top choices.”

<sup>72</sup> Ledyard was a Fairchild scholar at Caltech in the academic year 1977/78.

<sup>73</sup> **Hoffman, Elizabeth.** 1981. *The Sources of Mortality Changes in Italy since Unification*. New York: Arno Press.

- funding later. But it was very clearly, even in 1978 and 1979, not a respected thing to do.
- Chris Starmer  
Charlie Plott  
Charlie.  
I have a comment on that. It is true, these students that came out in political science and economics, even though they were fire-breathing experimentalists, we counseled them to actually sell themselves as something else. “Don’t sell yourself as an experimental economist. There are no jobs for experimental economists. There are jobs for traditional people.” Say, “I do the traditional stuff, and oh by the way, I also have an interest over here.” But, approach experiments strictly as an aside thing.
- But then once the camel has his nose under the tent, then it can expose itself as something else, and that seemed to work quite well. But when getting in the door, there was a solid block.
- Betsy Hoffman  
But when I came up for my third year review,<sup>74</sup> I got the explicit recommendation of—stop doing experimental economics. You are never “going to become famous fast enough.” I will never forget that quote. Go back to doing economic history.
- John Ledyard  
Who was it?  
Frans v. Winden  
That is a good question  
Charlie Holt  
For the record.  
Betsy Hoffman  
Start naming names and I will –  
John Ledyard  
*Leon Moses?*  
Betsy Hoffman  
Who?  
John Ledyard  
*Moses* wasn’t an economic historian.  
Betsy Hoffman  
No. He was a theorist. He was chair of the department at the time.
- John Ledyard  
Was that *Dale Mortensen*?  
Betsy Hoffman  
No, it wasn’t *Mortensen*.<sup>75</sup>  
John Ledyard  
I don’t remember.  
Betsy Hoffman  
Anyway, I was actually recruited to Purdue to be an experimental economist, so that was how I launched a career as an experimentalist after being told that I was not going to get famous fast enough, so that would have been in 1981.<sup>XIX</sup> I was recruited to Purdue, and I haven’t done an economic history paper since.
- Chris Starmer  
John and then Steve.

<sup>74</sup> Hoffman came up for a third year review in the fall of 1981.

<sup>75</sup> **Editor:** Who was it then? **Betsy Hoffman:** “Actually it was *Dale Mortensen*.”

- John Kagel I just wanted to make a quick follow up to what Charlie said about how these things should have been classified. I agree 100 percent that they should be classified by the topic, by the subject matter of whether you are dealing with, say, auctions or you are dealing with voting and this sort of thing, because it is a tool. It is not like econometrics. It is very far from econometrics where there are real high-powered techniques that are being developed all the time. I think it is more an approach to looking at questions. And it should be in the context of those questions. We should not be talking just to ourselves in terms of our professional work.
- Stephen Rassenti Well, my introduction to experimental economics was entirely different. I was an engineering graduate student at Arizona, and I remember hearing experimental economics or what Vernon was doing there as referred to as southwest economics disparagingly. I don't know where the expression came from. [some laughter] But Vernon introduced me to a couple of topics that I eventually used in my dissertation as an engineer. And I went off to work with Bell Labs after that and came back to experiments later, but it was not directly a part of my dissertation.<sup>76, XX</sup>
- Chris Starmer Jim, do you want to follow?
- Jim Friedman Actually, there are two things I would like to comment on. One of them popped up after I put my hand up before. The first harks back to your mentioning my dissertation perhaps being the first experimental one. There was another graduate student at Yale at the same time that I was there who was finishing an experimental dissertation just when I was. His name was *Trenery Dolbear*. [Smith: eh]<sup>XXI</sup> And you [John Ledyard] probably knew him at Carnegie.<sup>77</sup> *Tren* was in a cohort that came to graduate school a couple of years before I did, so I was acquainted with him, but I didn't know him very well. I wasn't aware that he was doing an experimental dissertation until we were both on the job market.
- Dick Cyert* who was the Dean of the GSIA at Carnegie had us both come out for job interviews. He did the unusual thing of bringing us out simultaneously, so we flew out and back together, stayed together, and then we both had job offers.

<sup>76</sup> **Rassenti, Stephen.** 1982. "Zero/One Decision Problems with Multiple Resource Constraints Algorithms and Applications," *Systems and Industrial Engineering*. University of Arizona.

<sup>77</sup> **Editor:** Did you know him, John? **John Ledyard:** "I was aware of Dolbear, but did not know him personally."

And I would like to comment on what you were saying, Betsy and you Charlie, about the admonitions about going forth as an experimentalist. In 1963, which I think was before 1981, [Hoffman: oh yes, considerably] the only arrow in my quiver was oligopoly experiments. I did not apologize for that and say I did this junk for my dissertation, but I will do something real later. When I was asked in an interview what my plans were, at that point, I had plans for further experiments.

I did have some good job offers at the time, including Carnegie where they were seriously interested in my experiments. Penn where they were not seriously interested in my experiments, I think, but they made me a job offer. And Yale where there was really nobody with any serious interest in experiments, where I stayed and continued in an environment in which nobody cared a lot about what I was doing but was very supportive that I should do it. So I have a little different take. Also, the first article I sent off to a journal was one of the easiest acceptances I ever had. I know I don't have to speak to this group of people about how editors and referees treat one's brilliant papers.

I have had my share of rough treatment, sometimes fair and sometimes unfair, but that experimental paper following my dissertation sailed into *Econometrica* in a way that gave me delusions about how easy it was.<sup>78</sup> [some laughter]

Chris Starmer

Vernon.

## Institutional Settings

Vernon Smith

Well, Purdue and *Carnegie Institute of Technology* were pretty different [from other places], [Hoffman: Yeah] and I think this tells you why it is that the first Ford Foundation summer fellowship was sponsored with Purdue and Carnegie. As a faculty member at Purdue,<sup>79</sup> I never felt the least bit [different.] In fact, I was encouraged to do what I was doing.

<sup>78</sup> Friedman, James W. 1967. "An Experimental Study of Cooperative Duopoly." *Econometrica*, 35(3/4), 379–97.

<sup>79</sup> Smith stayed at Purdue from 1955 until 1967 with a hiatus at Stanford during the academic year 1961/2.

It is just when I got outside of Purdue, I found the world much different. The Purdue program really—the faculty there, which consisted of *Stan Reiter*, *John Hughes*, later *Nathan Rosenberg*, *Lance Davis*, [Plott: *Jim Quirk*] *Jim Quirk*, *Saposnik*. For example, when we created the *School of Industrial Administration* in 1957, not all of those people were there yet. We decided not to have departments. And what you are hearing around this table is why we didn't want to have departments.

The Purdue program was built on basically three things: economic theory, quantitative methods, and economic history. The quantitative methods included econometrics and experimental economics. Basically, what was in the program was whatever the faculty was doing. It was a faculty that was geared to developing “knowledge-how” more than “knowledge-that.” I think that this had a lot of similarities with what was going on in Pittsburgh. In fact, John [Ledyard], you went to Pittsburgh.

John Ledyard  
Vernon Smith

I got lucky and went to both places.

Yeah. I think that really does a lot to explain that tolerance for doing unusual things [which] was an important part of the early development of experimental economics.

Jim Friedman

But that tolerance did exist at some of the more standard places as well.

Vernon Smith  
Betsy Hoffman  
Chris Starmer  
Charlie Holt

Ehm.

In very small measures.

Charlie [Holt].

I was a graduate student at Carnegie Mellon.<sup>80</sup> The great thing about Carnegie was they encouraged the faculty to write papers with the graduate students. I worked on two papers with *Dick Cyert* who was president of the university then, and *Morris DeGroot* was a statistician and student of [*Leonard, Jimmie*] *Savage*.<sup>81, XXII</sup> Every Saturday, we would go into the president's office and we would sit down, and *Dick Cyert* would say—it was the behavioral economics tradition—this is the way decisions are really made in the world. For example, when we make investment decisions, we don't look at rates of return very carefully. We look at what are

<sup>80</sup> Charlie Holt studied at Carnegie Mellon from 1970 until 1977. During 1971–1973 he was stationed in Japan as a Navy Reserve.

<sup>81</sup> **Cyert, Richard Michael; Maurice H. DeGroot and Charles A. Holt.** 1979. “Capital Allocation within a Firm.” *Syst. Res. Behavioral Science*, 24(5), 287–95, \_\_\_\_\_. 1978. “Sequential Investment Decisions with Bayesian Learning.” *Management Science*, 24(7), 712–18.



the retained earnings? What do we have to work with? We went back and redid some of Jorgenson's work adding retained earnings.<sup>82</sup> I remember it was the feeling of here is the world, this is the way the business world works. And I got a lot out of that.

At the time, as graduate students, we were much more excited about *Lucas* and *Prescott* and rational expectations. And so I remember once, later when I was at Minnesota, and we hired *Ed Prescott* to come there. He told me, he said, "Charlie, you shouldn't do experimental economics. It was a dead end in the '60s and this could be a dead end in the '80s." I didn't listen to him.<sup>XXIII</sup>

Chris Starmer  
John Ledyard

John.

Up until very recently, my life was as a theorist and not as an experimentalist. But I remember similar things happening to me when I went on the job market as a theoretical economist, as a mathematical economist. People didn't much like them either. Northwestern wouldn't interview, so it wasn't just anti experiments. Economics was growing up in this time, and there were a lot of different branches, many of which weren't particularly cherished by traditionalists. I want to second the thing Vernon said about Purdue and Carnegie and sort of Caltech follows in this model of no departments, and you are just working on ideas.

*John Hughes* once told me when I was a graduate student, he said one of my fellow graduate students complained that they were having to learn mathematics, and they complained to *John Hughes* who was an economic historian who they thought would lend a gentle ear to the statements. John looked at them and said economics is what economists do, and Purdue economics is what Purdue economists do, and you have to learn what we are doing and that's it.

Chris Starmer  
Al Roth

Quick two hander from Al.

This question of resistance to experiments. In the '70s, *Keith [Murnighan]* and I mostly sent our papers to psychology journals.<sup>83</sup> But the game theory community was interested in

<sup>82</sup> **Jorgenson, Dale W. and Calvin D. Siebert.** 1968. "A Comparison of Alternative Theories of Corporate Investment Behavior." *The American Economic Review*, 58(4), 681–712.

<sup>83</sup> From the eleven joint publications in the period 1977 to 1988 (eight until 1983) only two were in psychology journals excluding two in the *Journal of Conflict Resolution*, which, according to **Al Roth**, "was in the late 1970s and early 1980s an interdisciplinary journal with political science flavor."

**Murnighan, J. Keith and Alvin E. Roth.** 1980. "Effects of Group Size and Communication Availability on Coalition Bargaining in a Veto Game." *Journal of Personality and Social*

experiments. There wasn't a lot of opposition to it. I'm reminded by Jim's early experience with referees, when I first started sending experiments to economics journals, it looked like life was going to be easy. *Keith* and I sent a paper to *Econometrica*, and I think that maybe they didn't even ask us to revise it.<sup>84</sup> The game theorists who reviewed it liked the idea that they were experiments. The referees' reports in those days didn't look like they do today where experimentally literate people look at it and talk about the experiment. Rather the referee's reports would say something like this paper reports an experiment. That's a nice idea. Let's publish it.

Chris Starmer

Frans v. Winden

Very last word, Frans.

I think many of these things escaped the minds of many people in Europe, except for the German speaking countries where there was already pretty soon an association in the mid '70s.<sup>85</sup> There was the very important early work by *Allais*.<sup>86</sup> But apart from that little was going on. Personally, I traveled a long road before I got to my own economic experiments and their design in the late '80s. I studied economics in my undergraduate studies and I did a minor in social psychology and a major in economic sociology. There I picked up some interesting work. I remember two textbooks that were very influential for me and really very interesting.

One was a very general introductory textbook in social psychology by *Kretch*, *Crutchfield*, and *Ballachey* *Individual in Society*.<sup>87</sup> The other one, which I liked even better, was by *Cartwright* and *Zander*.<sup>88</sup> It was on group dynamics and I was very much intrigued by that. Among the authors were

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*Psychology*, 39(1), 92–103, **Roth, Alvin E. and J. Keith Murnighan**. 1978. "Equilibrium Behavior and Repeated Play of the Prisoner's Dilemma Games." *Journal of Mathematical Psychology*, 17(2), 189–98.

<sup>84</sup> Murnighan and Roth published together only one paper in *Econometrica*, which was submitted in May 1981 and revisions were received in November 1981. \_\_\_\_\_. 1982. "The Role of Information in Bargaining: An Experimental Study." *Econometrica: Journal of the Econometric Society*, 50(5).

<sup>85</sup> *Gesellschaft für experimentelle Wirtschaftsforschung* [Society for Experimental Economics Research] was founded by Heinz Sauermann in 1977.

<sup>86</sup> **Allais, M.** 1953. "Le Comportement De L'homme Rationnel Devant Le Risque: Critique Des Postulats Et Axiomes De L'école Americaine." *Econometrica: Journal of the Econometric Society*, 21(4), 503–46.

<sup>87</sup> **Krech, David; Richard S. Crutchfield and Egerton L. Ballachey**. 1962. *Individual in Society: A Textbook of Social Psychology*. Tokyo [etc.]: McGraw-Hill Kogokusha.

<sup>88</sup> **Cartwright, Dorwin and Alvin Frederick Zander**. 1960. *Group Dynamics: Research and Theory*. Evanston, Ill: Row, Peterson. 2nd edition appeared in 1960 and the last in 1968.

Cartwright and Zander, Festinger, French and Lippitt. They formed a group around *Kurt Lewin*, who developed a field theory,<sup>89</sup> and they all worked at the University of Michigan. They tried to combine experimentation with formal mathematical modeling.

I don't think they were very successful in that respect, but I can remember that there were formalizations, and it was very interesting. I also must have heard about *Siegel* and *Fouraker*, and *Simon* in the context of economic sociology [and social psychology]. Later I got interested in political economy, [more specifically] the endogenization of political behavior in economic models. There I picked up an interest also in simulations from a more macroeconomic perspective. I heard about these simulation games or management games. Some of these were written for teaching actually. I learned about running macroeconomic models [on a computer] where you could steer variables like the interest rate and stuff like that.

I got interested in having a macroeconomic orientation with some micro underpinning. And in my Ph.D. thesis I did some simulations myself—numerical experiments, as I called them.<sup>90</sup> What really struck me at the time was the huge influence of econometricians, like *Tinbergen* and *Theil*, and the little interest they took in fundamental aspects of behavior, that is, what actually explains behavior; political behavior [in particular]. Through my background in social psychology and economic sociology, I had a big interest in this.

I'm pretty sure that I tracked what was going on. I came across the work by Charlie [Plott] on political decision-making, [like] voting and committee decision-making and things like that. I attended the public choice conference in San Francisco in 1980, and I met *Axelrod* there, for instance. I came [also] across experimental work. Then the key events for me were first that I visited Charlie [Plott] at Caltech where I was interested in the political economics that was developed

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<sup>89</sup> Lewin proposed in his field theory that human behavior is a function of both the person and the environment.

<sup>90</sup> A slightly adjusted version of 1981 Ph.D.-thesis appeared **van Winden, Frans**. 1983. *On the Interaction between State and Private Sector : A Study in Political Economics*. Amsterdam; New York: North-Holland Pub. Co. ; Sole distributors for the U.S.A. and Canada, Elsevier Science Pub. Co.

there by people like Rod Kiewiet and *David Grether* and got also interested in, of course, experimental economics.<sup>91</sup>

I can remember that Charlie [Plott] and I talked about an experiment with overlapping generations. I was interested, for instance, in social security. How a pay-as-you-go system might be sustainable over rounds. We discussed it a little bit, but it never materialized. But I got the opportunity to participate in experiments and learned how to write instructions, do the designs, etc. That was '86, and then in '87/'88 [a second key event took place] when I visited the ZiF: *Zentrum für interdisziplinäre Forschung*, in Bielefeld, where Reinhard [Selten] was organizing a research year on the project *Game Theory in the Behavioral Sciences*.<sup>92</sup> I had a lot of exposure to experiments and theory and met many people, like Werner Güth. I started to really think about establishing a lab and to get into laboratory experiments myself. These were key events for me.

Chris Starmer

Thank you very much for your contributions. We must stop here. I let things run a little late. But I must say, it has been very interesting to me and enjoyable. We will take a short break now and reconvene at half past.

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<sup>91</sup> Van Winden visited Caltech from July until September 1986.

<sup>92</sup> Four volumes titled *Game Equilibrium Models* were published in 1991 that cover a variety of topics in economics, biology, sociology, psychology, political science, and behavioral sciences. Most of the contributions were non-experimental, mostly theoretical (traditional non-cooperative game theory). Among the contributors are also researchers who have conducted experimental research such Wulf Albers, *Ron Harstad*, James Walker, Roy Gardner, and *Elinor Ostrom*. **Selten, Reinhard**. 1991. *Game Equilibrium Models*. Berlin; New York: Springer Verlag.

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