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Symposium in Honor of Vernon Smith

Tribute to Sidney Siegel (1916-1961): A Founder of Experimental Economics

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Flying in to Washington, and knowing that I was allotted some time for this session celebrating my approaching 90th birthday, I wondered what I could possibly say in a panel where others are speaking about me. Then it occurred to me that it was a perfect occasion to speak about Sidney Siegel, who had a particularly strong influence on me. Many of you know something about Sid, but there are some things you may not know. So that will be my topic.

During the academic year, 1961-1962, I was a visiting associate professor at Stanford. At the beginning of the autumn quarter I had the truly significant experience of meeting Sidney Siegel and discovering that we had both been doing "experimental economics." Unknown to both of us at the time, Reinhard Selten had also been pioneering economics experiments in Germany. Sid, a social psychologist, was a truly powerful experimental scientist who strongly influenced me in becoming committed to experimental economics. He died unexpectedly at age forty-five, within a few weeks of our meeting. Eventually I read all of his publications, including his classic, *Nonparametric Statistics*, and his two books co-authored with Lawrence Fouraker. Sid was a master experimentalist, but much more; he also used theory and statistics with great skill in the design and analysis of experiments. I am persuaded that if Sid had lived he would not only have been a deserving Nobel Laureate, well out in front of the rest of us, but also the timetable for the recognition of experimental economics would have been expedited, perhaps by several years.

It is important to be long-lived in addition to making an important contribution if you are to obtain such recognition. But if not that does not mean you will be without influence or undeserving of credit thanks to the integrity of the citation tradition in academia.

It was the autumn of 1961 and the event was a dinner party at Marc Nerlove's house on the Stanford campus. Marc had invited a number of guests at this event in celebration of several visitors at Stanford and the Center for Advanced Study in the Behavioral Sciences. Sid was attending with his wife, Alberta Siegel, a child development research scholar, who was visiting at the Center that year. Sid was returning as a Fellow for the second time. Jack Hirshleifer was also a visitor; as I recall he was in the engineering economics program at Stanford, and attending the dinner party with his wife, Phyllis. Many regulars were there from the Economics Department. In the course of informal discussion Sid and I discovered that we were both doing "experimental economics," although I have no memory of whether we used that term. In fact, I do not recall when the term

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was first used, although we did use it to describe the first Ford Foundation Faculty Workshop at Carnegie Tech in the summer of 1964. The sub-field of experimental economics was not yet recognized.

Sid and I were mutually very excited to learn that we were each doing experiments motivated by hypotheses derived from economic theory. Somehow, at Marc's event, Sid got into talking about his origins and childhood—growing up poor on the streets of New York. His family—father, mother, and one brother—lived in a two-bedroom apartment in New York City. Because his brother "was a genius who played the violin," he needed to have every advantage, the best that the struggling family could provide. So his brother slept in the second bedroom, and Sid was relegated to sleeping on the living room couch. He recounted that he was essentially loose on the streets of New York, got into trouble with the police for minor teen-age infractions. He failed to finish high school (until years later), hung out in pool halls, and supported himself as a pool shark.

Sid was later inducted into the Army, which "saved him," as he described it, from a wasted existence. He signed up for the U.S. Army Signal Corps, learned the principles of electricity, communication, and its associated physics. (See his wife Alberta's memoir in *Decision and Choice*, McGraw-Hill, 1964). His mastery of the material enabled him to teach in the Signal Core program. This had qualified him for a temporary special California teaching credential. After his discharge from the Army, he taught in a San Jose secondary school. But his credential expired and the school principal suggested that he enroll in some college courses, thereby justifying continuance of his teaching credential. Sid noted that this plan had a slight problem: He had never graduated from high school, having dropped out as a freshman! They found a way around this bureaucratic hurdle, enabling him to get a high school degree by examination.

Sid finished his B.A. at San Jose State College in 1951, when he was 35, and completed his Ph.D. at Stanford three years later. His book *Nonparametric Statistics,* a classic still in print, was published in 1956; Sid died five very productive years later on November 29, 1961. What a short, volatile, and distinguished career.

Some 30 odd years later I attended a small Psychology and Economics conference at Caltech, also attended by the distinguished cognitive psychologists Danny Kahneman and Amos Tversky. In the context of one of the discussions, wherein the rigor of some experimental psychology study came up, it was natural for me to blurt out, "Whatever became of the tradition of Sidney Siegel in psychology?" In reply, Amos Tversky quipped, "You're it!" This was intended as a put down, a *touché*. Siegel was seen by the emerging cognitive psychology school 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-making in humans as part of cognitive processes, and using introspection, surveys, and subject oral and written reports, which are then interpreted by the scientist in terms of models of cognition. Skinner had rejected this methodology as unreliably subjectivist.

Cognitive psychologists, in turn, rejected Skinner's behaviorism as devoid of all attempts to understand mental thought processes. This is typical academic maneuvering: They are both right (and both wrong). Obviously you use all the instruments at your disposal, recognizing the hazards of subjectivism and the dead-end extreme of the behaviorist's unwillingness to delve into that "black box" called the brain.

For me, the quip by Amos was a compliment in the extreme. I am happy, indeed, honored, to be thought by someone as "it" in the Siegel tradition, and also to embrace the learning from

cognitive psychology while recognizing its subjectivist hazards and its many weaknesses in depending heavily on people's conscious cognition. The self-aware mind has little appreciation of the brain's ability to function effectively outside our control. We now have behavioral economics, brain-imaging technologies, and neuroscience, which are in the process of transforming traditional psychology—while reinterpreting and building upon the earlier traditions of psychology. Recently included is the by-passed social psychology gem *The Theory of Moral Sentiments* (1759), by Adam Smith.

Experimental economics began almost simultaneously, and independently, in three different independent traditions. I did my first market experiment in January 1956 and continued throughout the 1950s, but I did not get up the courage to publish my first experimental article until 1962. Simultaneously, Reinhard Selten in Germany was conducting the first oligopoly experiments, which would be published in 1959. Siegel, in conjunction with Larry Fouraker, did their first bargaining experiments in the 1950s and published them in 1960. Siegel, Fouraker, Shubik, and Harnett, working together at Pennsylvania State (Shubik was at Yale), subsequently conducted many bargaining and oligopoly experiments that would be published in 1963 after Sidney died. Larry Fouraker finished their 1963 book, which had already appeared in the form of three thick Pennsylvania State working papers. My original copies did not survive one of my many moves, but my longtime friend Martin Shubik, cast in concrete at Yale and an inveterate packrat who can be counted on to save everything, tells me he still has all of these wonderful treasures. Other economists in this early period who made contributions include Austin Hoggatt, Lester Lave, and Roger Sherman. In 1963, James Friedman, under the influence of Siegel, Fouraker, and Shubik, would publish his thesis on oligopoly competition.

Few behavioral or experimental economists realize how much of their methodological tradition came from Sid Siegel. Sid was adamant in his opposition to using deception in the treatment of subjects in the laboratory. Otherwise, how can we expect subjects to believe anything we tell them in the instructions? I used the same arguments, but my real reason was that it is immoral, whether or not it became known. I believe it was the same for Sid. Moreover, Sid was equally adamant that subjects should be paid in cash for the payoffs resulting from their decisions, contrary to the tradition in psychology happy to compel participation in experiments as a condition for majoring in psychology. He did not, however, recruit from classes. Rather, he recruited from the student population registered for part time employment through the university, whom he thought would be more uniformly motivated to earn dollars. Often he varied payoff levels as a treatment to verify its effect on decision. Other psychologists in that tradition included Ward Edwards and Anatol Rappaport at the University of Michigan.

If you read his experimental protocols, you learn that if he is doing a battery of experiments with treatment variations that will be compared, he recruits enough subjects to do all the treatments simultaneously. For example, if it's the Bertrand price competition model under duopoly and triopoly, single versus repeat play, all sessions are run simultaneously with the entire population of subjects randomized into the 2x2 treatments. He was justifiably concerned about time-of-day and sequential effects and controlled for them.

After my visit at Stanford, I started teaching a Graduate Seminar in experimental economics at Purdue in 1963. In the seminar, from 1963 to 1965, I developed and taught what I called the "theory of induced valuation" and its extension to multiple-unit purchases or sales by an individual participant. Induced valuation was simply a technique that enabled the experimenter to use monetary rewards to control economic incentives, and motivate choice in the laboratory study of markets and in all other group-decision and management problems. That idea would have been

importantly influenced by Sid Siegel's relentless concern for adequate payoffs together with the problem I faced in explaining and defending why I was doing experimental economics, what it was, and why it should be part of economics. The need to answer critics drew me into articulating the methodological foundations of using experiments to study economics. The idea of "induced valuation" was the beginning of that methodological effort.

Thank you for this 90th birthday opportunity for me to discuss the 65th anniversary of my significant encounter with Sid Siegel. I have attached a few references to his works, the record of a short but spectacular life.

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