

How Can a Psychologist Inform Economics? The Strange Case of Sidney Siegel

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ABSTRACT. Before Kahneman and Tversky showed how behavioural economics could bring psychology and economics into a unified framework, in the 1950s a social psychologist, Sidney Siegel, entered the realm of economics and laid the foundation of experimental economics. This paper gives an assessment of Siegel's overall contribution and claims that Siegel was not only a pioneer of experimental economics but also of behavioural economics. Had his view on the integration of psychology and economics been more promptly received, it might have triggered a different and more successfully path to the injection of greater realism in economics. When Siegel died, his approach to integrate psychology and economics lost its main advocate. Although his legacy was paramount in the work of the Nobel Prize Vernon Smith, Siegel endorsed a quite different approach to how make interdisciplinary research effective.

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1. Introduction

Toasting his 2002 Nobel Prize during the Royal Banquet in Stockholm, Vernon Smith acknowledged the status of pioneers of the “intellectual movement” leading to the foundation of experimental economics to four scholars. The first of the list, and probably the less known,¹ was a psychologist passed away in 1961, Sydney Siegel, known by Vernon Smith just six weeks before his untimely death. In Smith’s words, in this narrow span of time Siegel had “strongly influenced me in becoming committed to experimental economics” (Smith 2002). This tribute might seem surprising since Siegel had conducted his first experiment in 1954 and then published just two books and a handful of papers in psychological journals. Nonetheless, his brief acquaintance with economic experimentation had been significant enough to make him as one of scholars to whom Smith was more indebted.

It is not straightforward to explain the reasons of such a tribute. Ten years before, Smith had attributed Siegel’s influence on his work to the insight that experimental subjects could deviate by utility maximization because they can get bored by repeating the same decision over and over. This intuition led Siegel to introduce some sources of variability in the experimental procedure. The consequence of this adjustment was to restore maximizing behaviour among most subjects. Smith claimed that this, seemingly minor, issue was “of fundamental methodological importance, and I think it is unfortunate that it was not more widely known among experimentalists in both economics and psychology.” (Smith 1991, p. 5) By making reference to the integration between economics and psychology, Smith raised the very issue on which Siegel’s role could have been more influential than it had been during his short life. Thus, Smith added “Perhaps it was not possible for this work to be widely known in either of these two cultures, if economists were willing to accept the premise of the paper without evidence and if psychologists were unwilling to accept the premise with evidence.” (Smith 1991, p. 6)

Smith’s point was that, before Kahneman and Tversky showed how behavioural economics could bring psychology and economics into a unified theoretical framework, Siegel had already proved that laboratory experiments could improve economics by incorporating evidence from psychology. Moreover, he had offered an instructive example of how psychologists and economics could successfully collaborate by sharing not only methods but also theories and principles. As Smith claimed, this contribution was not appreciated for a long time. In this perspective, Siegel’s brief and atypical scientific career can be considered, on one hand, a lost opportunity and, on

¹ The others were, in order, Amos Tversky, Martin Shubik and Charles Plott (Smith 2002).

another, the first step of an underneath path only coming to surface in the recent emergence of behavioural economics.

The objective of this paper is to shed light on Siegel's effort to meld psychology and economics. It also tries to clarify why his approach remained largely unexplored since the 1980s. In the next section, a portrait of Siegel's life and research is drawn. Section 3 gives an in depth assessment of his contribution to experimental economics, which was not confined to laboratory methodology only. Finally, Section 4 discusses what all this implies for the recent trend towards integrating psychology into economics.

2. The strange case of Sydney Siegel

The facts leading Siegel to become an initiator of experimental economics were quite peculiar. It is quite evident that his unusual formation process played a crucial role in instilling in him the openness of mind so important for being a pioneer in the field.

Siegel's parents emigrated to the United States from Romania at the turn of the century and settled at New York to run first a bakery and then a restaurant.² Sidney was born on 1916 as the last of five children. His childhood was spent by helping his parents in their activities and by attending primary schools. With the Great Depression, his family's financial conditions deteriorated and, in the early thirties, Sydney lived on the road by roaming back and forth across the United States, finding occasional jobs in the summer and coming back to New York during the winter. In this period he became a sort of juvenile delinquent, who occasionally transgressed law by playing pool for cash, stealing objects and extorting money. In Siegel's second wife words, this behaviour was tempered by an innate aversion to any kind of violence: "Throughout his life, Sid was strongly sympathetic with 'underprivileged' and 'delinquent' kids, but he felt no kinship with the hoodlums. His own delinquencies had been strictly a way of getting by in the life of a city which offered no jobs and few channels to success to its slum children." (Engvall Siegel 1964, p. 4)

This period came to an end when he moved to Los Angeles in 1939, after his first marriage and his father's death. There he attended a school for repairing radio and then was hired as a technician in a radio shop. Two years later, in 1941, his first son Jay was born. To earn a living for the family, Siegel asked to be enrolled in the Army, but his attempt was frustrated by the report of having been affected by tuberculosis some years before. So he started working as a civilian

² These biographical notes mainly rely on the memoir written by Sydney's second wife, Alberta Engvall Siegel (1964), who was a professor at psychology at Stanford University and played an active role in Siegel's professional life.

employee in the Army Signal Corps, where he specialized as electronic engineer. In 1942 he entered for the first time an academic environment by attending courses on engineering and mathematics at Stanford University. In 1943, he also began to teach science and mathematics in a secondary school in San Jose, although he was not a college graduate and therefore not eligible for teaching in a high school. To settle this problem, he enrolled at San Jose State College in 1945, while continuing school teaching. He obtained a bachelor degree in 1951, at the later age of thirty-five. Then he enrolled graduate school at Stanford University, where began his research career that had to last less than a decade.

He soon decided to focus his attention on social psychology. His first project was entitled “Cognitive Ambiguity and Ethnocentrism,” which gave birth to his Ph.D. dissertation thesis on “Certain Determinants and Correlates of Authoritarianism.” The thesis included Siegel’s first experiment. To detect a measure of authoritarianism, some students were asked to match some photographs of faces with an equal number of generic sentences randomly chosen from different and unrelated sources. The number of associations arbitrarily made by the students was considered by Siegel as an indicator of overconfidence and, more specifically, of an “intolerance for cognitive ambiguity.” (Siegel 1954, p. 251)

Siegel discussed his Ph.D. thesis in the fall of 1953. In the same days, he came in contact at Stanford with two young philosophers, Donald Davidson and Patrick Suppes. Davidson had received his education at Harvard, where he studied mathematical logic with Willard Quine, and moved to Stanford in 1951. He was bound to become one of the most distinguished analytical philosophers of the past century, whose discussions of concepts of action, truth and communicative interaction have generated considerable debate in philosophical circles around the world. Patrick Suppes had obtained his Ph.D. from Columbia University in 1950 and soon became acquainted with the logician McKinsey, who was writing an introductory book to game theory (McKinsey 1952). His influence pushed Suppes to study utility theory and to write a paper on the foundations of probability (Suppes 1951), the first of a series leading him to become a key contributor to the field. Davidson also became involved in this topic when Mc Kinsey moved from RAND Corporation in Santa Monica to Stanford in 1952.³ The result of their common interest was the project of giving an experimental measurement of utility. To perform this task, it was necessary to adapt the laboratory

³ In the interview in which Davidson acknowledged this debt, he also reminded the dramatic circumstances of McKinsey’s moving to Stanford: “But in fact McKinsey was the guy who was teaching both of us [Davidson and Suppes]. He was one of the inventors of quantified modal logic, though he didn’t publish much of his stuff. We hired him because he was with the RAND Corporation in Santa Monica, and there was all this stuff about his being a bad security risk because he was a homosexual. So they took away his security clearance and Stanford hired him. Then McKinsey committed suicide.” (Lepore 2004, p. 236)

procedures used in psychology. The promising, albeit not exactly young, student Sidney Siegel was judged the right person to join the project.

The experiments were carried out in the spring of 1954 and the results reported in a Stanford technical paper in the August of 1955 (Davidson, Siegel, and Suppes 1955). This preliminary version was revised and enlarged in the book “Decision Making: An Experimental Approach” (Davidson, Suppes, and Siegel 1957).

Suppes’ memory of the period deserves to be quoted: “This was my first experimental work and consequently in a genuine sense my first real introduction to psychology. The earlier papers on the foundations of decision theory concerned with formal problems of measurement were a natural and simple extension of my work in the axiomatic foundations of physics. Undertaking experimental work was quite another matter. I can still remember our many quandaries in deciding how to begin, and seeking the advice of several people, especially our colleagues in the Department of Psychology at Stanford.” (Suppes 1979, p. 8) The determinant role played by Siegel in this preliminary work is clearly pointed out by the experimental design, which anticipated most of his later work. The main motivation of the study was to replicate Mosteller and Noguee’s (1951) test of utility maximization, which was affected by some methodological limitations.⁴ In their experiment, a series of choices between playing and not playing were submitted to 14 students. When they exhibited indifference between two options, a number was associated to the bets until the underlying utility function was traced. Davidson, Suppes and Siegel claimed that Mosteller and Noguee’s method did not allow inferring any consequence on the interval scales separating the points of the utility function, unless it was proved that the numbers assigned were unique up to a linear transformation. Moreover, their experimental method could be criticized for other two features: first, that every choice was expressed as the acceptance or the reject of a gamble, and therefore unbalanced in favour of the second option for risk prone subjects; secondly, that objective probabilities were implicitly assumed as equal to subjective probabilities.⁵ By amending these flaws, Davidson, Suppes and Siegel’s experiment showed that 15 subjects over 19 chose as if they were maximizing expected utility and, more importantly, their preferences could be represented by a utility curve unique up to a linear transformation. On this evidence, the authors proudly claimed that their experiment was the first “to measure subjective probability behavioristically on the basis of empirically determined utilities.” (Davidson, Suppes, and Siegel 1957, p. 25)

In summer 1954 Siegel moved to Pennsylvania State University to enter the faculty in the Department of Psychology. The next step in his research career was twofold. On one hand, he

⁴ The drawbacks of Mosteller and Noguee’s design are discussed in Camerer (1995, p. 620).

⁵ In support of this point, Davidson, Suppes, and Siegel (1957) quoted Edwards (1954), who had showed that, independently of utility considerations, people could prefer some probabilities to others.

corroborated his abilities as a statistician by teaching a graduate course in statistics and, on the other hand, he further improved the experimental techniques to measure the utility function. The first activity led him to write *Nonparametric Statistics for the Behavioral Sciences* (Siegel 1956), which rapidly became the standard non-technical handbook for researchers of all social sciences. The second effort was specifically aimed at providing an estimation of the intervals of the utility function. In two papers published in psychological journals (Siegel 1956a, Hurst and Siegel 1956), Siegel devised an ingenious procedure to determine in the laboratory an ordered metric scale of preferences, which improved the method previously proposed by Coombs (1950).

These results strengthened Siegel's confidence in experimentation and led him to deal with more challenging issues, such as learning processes. This project originated in Siegel's mind by two different inspirations. The first was Estes' model on learning theory; the second was Herbert Simon's 1955 paper on bounded rationality.

Estes' model (1950), which triggered off a wide debate on the informative and computational capacities of economic agents, aimed to show that learning could be represented as a converging stochastic process. To support his theory, Estes (1954) surveyed some experimental evidence, which also attracted Siegel's interest. The experimental task consisted of betting on two different events, to which two largely different probability values were associated. The finding supporting Estes' model was that choice repetition induced subjects to match their predictions to the actual proportions of occurrence of the two events, rather than to bet rationally only on the most probable one. Siegel replicated the same experiment and showed that the convergence to the more probable outcome could be improved by rewarding subjects with real money and by diversifying the laboratory procedure (Siegel 1959, Siegel and Goldstein 1959, Siegel, Siegel and Andrews 1964). Siegel's design included three different treatments (no payoff, reward, and risk), whose comparison provided unambiguous evidence of the importance of monetary incentives to subjects' behaviour, and a set of ingenious techniques to relieve the tediousness of long sequences of choices.

The next step in Siegel's approach to learning theory was triggered by Simon's celebrated paper on behavioural choice, whose aim was "to construct definitions of 'rational choice' that are modeled more closely upon the actual decision processes in the behavior of organisms than definitions heretofore proposed." (Simon 1955, p. 114) Simon's definition of satisficing behaviour relied upon the psychological concept of aspiration level, which defined an alternative as satisfactory. Siegel was immediately supportive of this proposal and took it into account in devising an experiment to prove that a behavioural model of decision making should include an explicit assumption on how individuals define their level of aspiration. (Siegel 1957, Becker and Siegel 1958)

In 1959 Siegel was named Professor of Psychology at Penn State University, where he planned what was to be the last project of his scientific career. In his continuous effort to integrate psychology with economics, he started to collaborate with Lawrence E. Fouraker, who taught economics at Penn State.⁶ Their joint work resulted in two books: *Bargaining and Group Decision Making: Experiments in Bilateral Monopoly* (Siegel and Fouraker 1960) and the posthumous *Bargaining Behavior* (Fouraker and Siegel 1963).

The main finding of the 1960 book was that in bilateral monopoly bargainers were inclined to maximize payoffs by selecting the Pareto optimal solution and by dividing the surplus equally. The convergence became more likely when greater amounts of relevant information were available to the bargainers. Siegel and Fouraker stressed how this outcome contrasted with Schelling's (1957) argument that in a bargaining situation less informed bargainers are in a more advantageous position with respect to the more informed ones. The 1963 book extended experimental analysis to oligopoly by providing further confirmation of the role of complete information in implementing a Pareto optimal market solution.

As well known, in the same years the faith in the virtue of the perfect competition paradigm was corroborated by Vernon Smith's (1962) version of Chamberlin's experimental imperfect market, which left an indelible mark in the history of experimental economics. In this history next-to-come Siegel did not play an active role. He died suddenly of heart attack on November 29, 1961, while he was working at the Center for Advanced Study in the Behavioral Sciences of Stanford University. The legacy he left behind is impressive for a man that was graduated only ten years before at the age of 35. But more striking is probably what he could have made for changing that history: "One can only speculate as to the course of experimental economics in the last quarter century had it not been for Sid Siegel's untimely death in the autumn of 1961. My opinion is that his energy and towering intellectual competence and technique as an experimental scientist would have accelerated greatly the development of experimental economics. Had he lived there would have been a sustained effort in experimental economics at another institution besides Purdue University. It appears that he has no intellectual descendants in psychology, but many in economics, although few of the latter may be fully aware of their heritage." (Smith 1991, p. 3)

What the rest of the paper intends to make clear is precisely because Siegel had so many descendants among experimental economists but they were so unaware of his legacy.

⁶ Lawrence Edward Fouraker was born in 1923 and obtained a Ph.D. in economics from the University of Colorado in 1951. In the same year he accepted a teaching position at Penn State, where he was also nominated assistant dean for research at the College of Business Administration. After Siegel's death he joined the Harvard Business School (HBS) in 1961, where he was promoted full professor in 1963. Until his death in 1998 he served in many executives roles, including over 10 years as dean of the HBS.

3. Why experimental economists are so indebted to Siegel?

Experimental economics has not been acknowledged as a constituent part of economic science until recently. Therefore, it is not at all surprising that its history has not yet been thoroughly examined, although more than fifty years have passed from Chamberlin's (1948) experiment. Much of this delay can be attributed to the fact that the formalization of economics had created such a sharp division between theoretical models and empirical analysis that many assumptions made by economists about human nature are considered simply wrong by behavioural and psychological research (Rabin 2002, p. 661). As a consequence, experimental economists have often found easier to address this audience rather than their own colleagues: "When I began my own experimental work about a dozen years ago, it was most convenient to publish the results in journals of psychology and business." (Roth 1987, p. 1) In the same article, Alvin E. Roth sets the overcoming of this barrier in 1985, when *Journal of Economic Literature* introduced the entry "Experimental Economic Methods" in its classification system. More recent contributions on the historical evolution of the discipline⁷ outline three historical phases: the early years, dating from 1948 to the early 1960s, the middle years, almost the whole of the 1980s, and the following maturity. Chamberlin's first attempt to test an imperfect experimental market took the lead, while the next breakthrough was achieved by Siegel and Fouraker's (1960) work on bargaining behaviour and Vernon Smith's (1962) reprise of Chamberlin's experiment. This historical assessment focuses almost exclusively on the methodological achievements of Siegel and Fouraker (1960). Specifically, book's contribution was crucial in establishing, first, that the conversion of subjects' payoffs into cash rewards is a necessary requirement for experimental findings' validity and, secondly, that subjects' information conditions are key variables in the laboratory. If it is quite clear that these accomplishments were attributable only to Siegel, it has to be pointed out that he also provided relevant theoretical contributions to the economic issues he dealt with. This appraisal can be extended to include bargaining theory, on which Fouraker provided just an introductory theoretical framework. In fact, Siegel's continuous effort to pursue interdisciplinary collaborations had already driven him to search for help in other directions.⁸ If these planned collaborations would

⁷ Smith (1992), Roth (1993, 1995), Leonard (1994) are the first attempts to present a systematic history of experimental economics. The recent collection of essays edited by Fontaine and Leonard (2005) offers an informative and pluralistic account of it.

⁸ Martin Shubik, who met Siegel at Stanford in 1959, gives a personal account of one of these collaborations: "Siegel needed an economist involved in game theory to plan new experiments and he thought that Fouraker

have improved his later theoretical reputation, it can hardly be said. But this historical bias was certainly due to the emphasis placed on methodological issues by Vernon Smith, who confined Siegel's theoretical contribution in the dark. In order to reassess Siegel's legacy, it is useful to discuss first his methodological achievements and then the substantial ones.

What Siegel did in nearly ten years of research was to create ex-novo a scientific method to conduct experiments in economics by providing a set of structural guidelines. Later, Smith systematized these procedures but, as acknowledged by himself, he relied heavily on Siegel's insights. That Siegel was aware of his pioneering role is shown by the fact that he did not lose any opportunity to make explicit his methodological view. Since the book on utility maximization co-authored with Davidson and Suppes, he developed and constantly updated the list of requisites that economic experiments had to fulfil. Current view, as summarized in recent handbooks,⁹ identifies four guidelines to which economic experiments must adhere: procedural regularity, motivation, unbiasedness and calibration. On all these requisites, Siegel's contribution was fundamental.

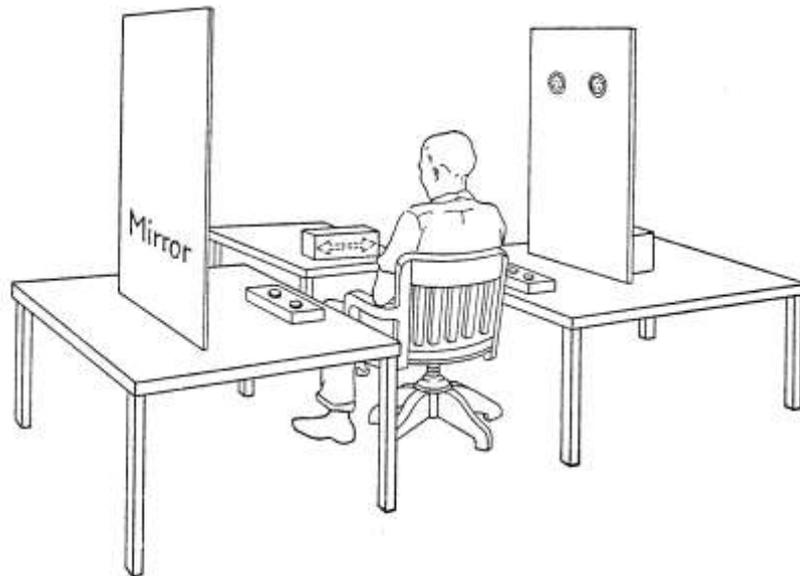
The principle of procedural regularity is met when experimental design permits replications that would accept as being valid by other researchers. It requires that instructions are fully detailed, methods of recruiting subjects are unbiased and all the features of the laboratory environment are under the control of the experimenter as much as possible. Siegel was perfectly aware that laboratory is not a socially neutral context, but it is itself an institution with its own formal or informal, explicit or tacit, rules. For this reason he did not disdain to perform experiment in the field. His first experiment on the measure of the intervals of the utility function was conducted in a penitentiary, where inmates served as subjects and cigarettes as rewards (Hurst and Siegel 1956). But when conducting experiments in the laboratory, he took care of any detail with exceptional rigour. A few examples can illustrate this point better than an exhaustive review.

For his first experiments on utility theory, Siegel devised a special dice on which numbers were substituted with nonsense syllable, such as "ZOJ", "WUH" or "QUJ", which subjects could not associate to any meaning. This expedient aimed at avoiding that subjects' betting depended on "prejudices and superstitions which many people hold concerning familiar events, e.g., heads on coins, evens on dice, etc." (Siegel 1956a, p. 66) This notwithstanding, Siegel used to conduct some preliminary trials to test if subjects preferred any nonsense syllable to another.

was not expert enough. He was really interested at collaborating with me. When Siegel came back to Penn State University, I used to fly there once each two or three weeks to work with him, but we did not have time enough to obtain concrete results." (Shubik 2008) Their collaboration is documented in a series of letter, dating from July 5, 1960 to December 14, 1960. (Shubik 1960)

⁹ Davis-Holt (1992), Friedman and Sunder (1994), and Friedman and Cassar (2004).

In the 1959 test of Estes' learning model (Siegel, Siegel and Andrews 1964), to relieve the boredom of 240 trials of the two-choice uncertain-outcome decision, subjects were seated in a swivel chair facing a signal light with two arrows pointing right or left, as illustrated below. (Siegel, Siegel and Andrews 1964, p. 68)



On the right of the chair, there was a table with a platform with two push buttons and a vertical panel with two electric lamps. On the left side, there was another table on which another platform with two buttons and a large mirror reflecting the wooden board were mounted. The procedure started when one of the arrows on the signal light illuminated. Depending on what was the pointed direction, which was randomly selected, subjects had to rotate the swivel chair and to face alternatively the panel or the mirror to predict what lamp illuminated by pressing the appropriate button. This arrangement imposed to subjects willing to adopt a pure strategy by predicting the same light in all trials to change push button. In this way, “The boredom of repeatedly making the same cognitive response (e.g., right, right, right, right, right, right, . . . , right) is relieved, as is the kinesthetic boredom of remaining seated in the same position for an extended period and pressing the same button over and over.” (Siegel, Siegel and Andrews 1964, p. 68)

Siegel's carefulness might sometimes appear pedantic, if not fussy. By repeating the same experiment with four and five year old boys, Siegel changed the setting as below. (Siegel, Siegel and Andrews 1964, p. 55)



Boys were asked to choose one of the two opaque bottles, each containing an object, for 200 times, of which 100 times without any reward and 100 times with rewards, represented by a variety of dolls, toy cars, sweets, and trinkets.¹⁰ In order to avoid order effects, for half of the boys the choice sequence was reversed. Unsurprisingly, this treatment was bound to fail: “Among the children who were observed initially in 100 trials of the payoff condition, however, it would not have been possible to maintain cooperation through subsequent trials under the no-payoff condition. Not unexpectedly, these children, who had been playing the game for a prize, were reluctant to continue with the game once the possibility of receiving a prize was eliminated.” (Siegel, Siegel and Andrews 1964, p. 57)

Despite some redundancies, Siegel’s attention to the details also led him to focus on the motivation issue. Induced value theory, as codified later by Smith (1976), imposes the use of a reward medium to induce pre-specified features in experimental subjects and makes their innate characteristics largely irrelevant. Siegel was quite convinced that hypothetical choices were unreliable and that experimental subjects had to be rewarded in order to be adequately motivated. On this matter, Siegel departed from psychologists’ standard approach: “Because of our belief in the central importance of employing payoffs which are meaningful to subjects, rewards which in

¹⁰ Siegel did not neglect either the chance of boys’ satiation: “It is to be expected that a variety of such prizes is more appropriate for youngsters than a collection of a single kind of item. That a 4-year-old might be quickly satiated if more and more of the same kind of reward began to pile up before him is suggested by the usual notions about diminishing marginal utility and by the fact that young children – and perhaps some other types of subjects as well – are not likely to make discriminations implied by counting; they may observe that they acquire, say, one, two, three, and then “many” toy cars, but once they have many, the acquisition of more may become meaningless. The use of diversity of rewards circumvents this difficulty.” (Siegel, Siegel and Andrews 1964, p. 148)

fact they covet, we have little confidence in experiments in which the ‘payoffs’ are points, credits, or tokens. Or perhaps it would be more accurate to say that we have little confidence in the use of the term payoff to label such trivia. The relevance of such experiments to any theoretical notions about reward, payoff, or utility seems to be dubious.” (Siegel, Siegel and Andrews 1964, p. 148) For Siegel, real payoffs - being them cash, students’ grades, cigarettes or trinkets - made subjects’ choices responsive to the variables under investigation, which were generally underrated in the “no payoff” condition.

What Vernon Smith did some years later was just to develop this insight: “In thinking through the implications of ‘other things in the utility function,’ I found Sid Siegel’s paper on two-choice uncertain outcome situation particularly helpful [Siegel 1961]. In this binary choice situation, the interpretation of over twenty years of psychology literature had been that people were not rational; specifically, they failed to maximize. Since monetary payoffs had not been used, Siegel hypothesized that subjects did not maximize because there was nothing of value worth maximizing, and that the observed matching behavior of subjects was due to ‘monotony, both kinesthetic and cognitive’ [Siegel 1961, p.768]. Accordingly, he developed an additive model of utility with two terms: the first was the utility of reward, the second the utility of variability, diversification, or monotony relief. The model predicted that subjects would be drawn away from matching toward maximizing by introducing monetary payoffs, and the greater the payoff levels the nearer would be the response to the maximizing response. The data confirmed the prediction. Then Siegel’s ingenuity was turned to a procedure for raising the utility of variability as a treatment.” (Smith 1991, p. 5)

Siegel’s emphasis on the motivation issue did not impair his receptivity to the third experimental requisite of unbiasedness. According to it, experiments should be conducted in a way that does not lead participants to perceive any behaviour as being expected or correct. It is well known that this argument is so compelling as to require the systematic use of placebo and treatment groups in medical and psychological experiments. In some cases, this procedure involves deceiving subjects about what the experimenter is investigating. On the contrary, in experimental economics it is currently assumed that cheating should be banned.¹¹ Siegel endorsed this rule in all his experiments. He believed that deceptive experiments would have created an atmosphere of suspect and scepticism towards laboratory methods: “Anyone who has worked with the repetitive-choice situation under study here knows that at least some subjects formulate the suspicion that the experimenter is altering the sequence of events as the experiment proceeds, and some think that he

¹¹ “The question of trust is an important one: it is an unfortunate fact that experiments in psychology are tainted by distrust. We do not want the same taint to be attached to experiments in economics.” (Hey 1991, p. 21) See Bonetti (1998) for a survey and a criticism of this view.

is altering it in response to the specific patterning of their choices. Moreover, many subjects doubt that the sequence of events is random, or they do not understand randomness; they watch for a patterning and sometimes make their choices under the assumption that a patterning exists. Our procedures were generally designed to demonstrate to the subject that the experimenter does not alter the event sequence as the experiment goes along.” (Siegel, Siegel, Andrews 1964, pp. 149-150)

The last principle embodied by Siegel in the toolbox of experimental economics was calibration. It requires that the correspondence between laboratory findings and theoretical predictions is fully and unambiguously specified. To serve this purpose, Siegel’s experiments were as simple as possible and the design did not manipulate ever more than one treatment variable, just for being able to discern accurately the implications for the model under investigation.¹² In reminding Siegel’s statistical expertise, his wife summed up well his view on calibration: "He averred that the best-designed experiment is one requiring no statistical analysis at all. Where statistics are needed, the simpler the better. A major argument for nonparametric tests is their simplicity: their basis is easily grasped, the computations are straightforward, and no distorting transformations are imposed on the raw scores. Preferring clean and simple designs, Sid had little use for the analysis of variance and typically voiced his suspicion by proclaiming his inability to understand the meaning of any interaction. A simple two-group experiment usually sufficed to test hypothesis of interest." (Engvall Siegel 1964, pp. 18-9)

This set of methodological achievements were applied by Siegel to well defined theoretical targets. His confidence in experimental economics was based on the belief that economics’ formal language makes it a perfect field for laboratory testing. It is not casual the Siegel’s first contact with economics was expected utility as axiomatized by von Neumann and Morgenstern (1947). If the collaboration with Davidson and Suppes was triggered by the attempt to improve Mosteller and Noguee’s (1951) design, Siegel’s later work made clear his view on utility theory. He was strongly committed to the idea that maximization principle had to be further specified in order to be empirically relevant.¹³ As a matter of fact, individuals could be considered maximizing agents only

¹² “In planning the experiments reported in this book, our effort was to employ the simplest possible experimental design. We have deliberately restricted our studies to the use of a single-variable design. In any given experiment, only a single independent variable was manipulated, and our interest centered on a single dependent variable.” (Siegel, Siegel and Andrews 1964, p. 153)

¹³ It is revealing that Vernon Smith gave a different assessment of Siegel’s conception of rationality: “Siegel interpreted Simon’s argument as suggesting that the rational model is essentially correct, but more or less incomplete. To make it complete, it was necessary to examine decision problems carefully from the utilitarian point of view of the decision maker (not just from the point of view of the experiment/theorist). Note that this interpretation differs from the ‘satisficing’ and ‘bounded rationality’ constructions that were later put on Simon’s original idea, construction that were critical of the very foundation of rational behavior

as a first rough approximation. In order to explain experimental evidence, it was necessary to take account of congeries of other factors, which Siegel gradually brought into focus. In Davidson, Suppes, and Siegel (1957), this list included risk attitude and subjective probabilities; in the experiments on utility intervals (Siegel 1956a, Hurst and Siegel 1956, Siegel and Sheperd 1959, Guthrie, Becker and Siegel 1961), the socio-economic features of the individuals, being them inmates in a prison, men or women, socially close or distant people; in two-choice uncertain-outcome decisions (Siegel 1959, Siegel and Goldstein 1959, Siegel 1961, Siegel and Andrews 1962, Siegel, Siegel and Andrews 1964), the saliency of rewards and the removal of boredom; in tests on learning (Siegel 1957, Becker and Siegel 1958, 1962), the level of aspiration. In this way, utility function was used as a heuristic device: “The notion of utility is useful in providing a basis for experimental operations. By conceptualizing the experimental situation already described in terms of utility and by specifying relevant components of a utility function, we may identify those aspects of the experimental situation which are related to behaviour in that setting. From this, we may prescribe changes in the situation which will lead to predictable changes in choice behaviour. The construct of utility is useful here, then, in the degree to which it provides a rationale for determining the systematic environmental changes which lead to systematic and thus predictable, changes in behaviour. The overall utility of any possible outcome may depend on the subjective value of each of several conceptually distinct aspects of that outcome. To predict choices, one must identify the various aspects (or components, or factors) of the situation to which positive or negative utility is associated. With information about the utility of each component of each outcome, it is possible to assess the utility associated with any particular strategy. Such an analysis provides a rationale for experimental operations and yields hypotheses concerning the results of such operations – the effects of these operations on choice behavior.” (Siegel, Siegel and Andrews 1964, pp. 9-10)

A revealing example of Siegel’s behaviourism is the way he adapted the psychological concept of aspiration level to economic decision-making, by pioneering the concept of reinforcement learning (Erev and Roth 1998, Camerer and Teck-Hua 1999). His first step was to claim that psychological concepts like success and failure were respectively equivalent to positive and negative values of the utility function. Then, he assumed that the level of aspiration could be identified by the utility point associated to the greatest interval between that point and the next lower one. In this

as conventionally defined. In Siegel’s implementation, actions differ from the predictions of the standard model because of the decision cost. Since the latter is necessarily part of the problem of realizing rational outcomes, the result is not just a better descriptive/predictive model. It is a better normative model of action as experienced by the individual. Thus, the distinction between the descriptive and the normative model of behavior becomes clouded; neither is cast in objective reality independent of experience. I believe this is the right way, although certainly not the easiest way, to approach the problem of modeling rational behavior.” (Smith 1991a, p. 807)

way, decisions giving utilities below this point caused a psychological feeling of dissatisfaction and repeated experiences of success (or failure) led to a cumulative process of increasing (or decreasing) the aspiration level. Finally, Siegel claimed that this mechanism collided with the application of the maximization postulate of neoclassical economics.

As pointed out before, Siegel drew inspiration for his model by Simon's definition of satisficing behaviour, of which he provided the first experimental proof. Siegel's (1957) experiment showed that, being decisions taken by means of sequential procedures, revealed preferences depended on the presentation order. Individuals were inclined to choose the first available alternative above his level of aspiration, but this decision was not necessarily confirmed if choices were repeated because the level of aspiration constantly shifted upward or downward. These findings had harmful implications for standard utility theory: "In conclusion, it would seem that a useful behavioral model of decision making should include not only the concepts of utility and subjective probability, as do the present models, but should also include a formulation of the effects of level of aspiration and reinforcement on utility. That is, the model should include recognition that utility has a model in its own right, in which the main concepts are level of aspiration (LA) and reinforcement effects (R)." (Siegel 1957, p. 124)

With these premises in mind, Siegel approached bargaining theory, itself a particularly suitable area for testing the effects of aspiration level. Lawrence Fouraker (1957) was consonant with Siegel's idea that bargaining theory would have benefited by making explicit the behavioural and psychological determinants of agreements. In the laboratory, this asked for choosing what variables might be more significant for bargainers' behaviour. Siegel and Fouraker's choice fell on information conditions. The debate raised by Nash (1950, 1953) bargaining model, in which Harsanyi (1956) and Schelling (1960) were key players, led in this direction (Innocenti 2008). Nash and Harsanyi had proved, on an axiomatic basis, that with complete information it was rational to agree on the outcome maximizing the product of bargainers' utility functions and to distribute it equally in relation to the disagreement point. Schelling had contended that the process of finding an agreement in a bargaining process had a component that was inherently empirical, because it concerned a process of intellectual coordination among heterogeneous agent in which the context was decisive. Thus, Nash and Harsanyi's equilibrium solution had failed to solve the bargaining problem because they neglected the learning process through which bargainers defines their expectations on others' behaviour. By taking it into account, Schelling had claimed that more information could be better than less information in order to obtain better outcomes. The more informed bargainer, knowing that the other player knew only his own payoff, was more forgiving when his opponent made large demands.

Siegel and Fouraker's (1960) experimental findings were generally supportive of Nash-Harsanyi model. Bargainers were inclined to maximize joint payoffs by negotiating the Pareto

optimal agreement on the contract curve. But this tendency was shown being strictly related to the amount of information available to bargainers. More information on other's payoffs reinforced the convergence to the efficient agreement. Still, prices exhibited the tendency to take the value of the 50-50 split of the joint payoff, but this finding was proved to be dependent on subjects' level of aspirations, as measured by the recorded successions of bids and asks. In contrast, evidence on Schelling's curse of knowledge was "equivocal": half of the cases supported it, while the others opposed or failed to support it. The authors concluded that on Schelling's theory "further research, perhaps with larger sample sizes, will be necessary before any strong conclusions may be drawn." (Siegel and Fouraker 1960, pp. 58)

Fouraker and Siegel (1963) did not implement such a research program. The book contained a set of experiments extending the same design of the 1960 book to the case of oligopoly. The convergence to Pareto optimal agreements was generally confirmed, but clear evidence was provided that an appropriate manipulation of the environmental conditions (regime of complete or incomplete information, details of bargaining protocols, contract locations) could implement individual, rather than joint, profit maximization. Outcomes were so varied that they were classified, on the basis of a disaggregate analysis, according to a list of different subject's types, ranging from rivalistic to cooperative one. For each group, a psychologically-based justification was also given. For example, "A rivalistic decision could reflect two distinct motivations: (1) that the player derives satisfaction from reducing the profits of his opponent; typically, he wants to beat his rival, and (2) that the player wishes to send a punitive bid, as a signal that he is dissatisfied with the past responses of his opponent and wants him to alter his decisions in a manner that benefits the signaler." (Fouraker and Siegel 1963, p. 204-5)

Apart from its marked behavioural flavour, Siegel and Fouraker's main theoretical achievement was to highlight the critical role of information conditions on market efficiency. It was exactly this finding to puzzle the young Vernon Smith on his first meeting with Siegel, as confirmed by the letter he wrote to the editor of the *Journal of Political Economy* on October 26, 1961: "Incidentally, I have just today met Sidney Siegel for the first time and I have his to-be-published material on experiments in oligopoly. The results are terribly interesting. Duopolists, who only know their own profit outcomes (incomplete information), go to the Bertrand competitive price solution. Triopolists do the same but faster. This suggests that my competitive price results might be achieved in still thinner markets. But the real shocker is the effect of complete information in which duopolists know each other's profit outcomes. As the amount of information increases, duopolies decrease their tendency to the Bertrand competitive price. The invisible hand only works when it is invisible?!" (Smith 1961)

Smith's confidence in the Hayek hypothesis (Smith 1982) was hence initially corroborated by Siegel's findings. But when they met again six weeks later, Siegel was supportive of Smith's results less than expected: "A really important event at Stanford was my meeting Sydney Siegel, who was a fellow that year at the Center for Advanced Study in the Behavioral Sciences. I knew Syd only six weeks before, very inconveniently, he died. (I have never forgiven him. What a great experimental scientist!) I showed him my work. He was skeptical, too, but it was different; his was the skepticism of a scientist, not a wise guy. He had ideas, suggestions, and challenges for me that emanated from a deep commitment to the science of behavior. Through his cutting criticism came excitement and implicit encouragement." (Smith 1991a, p. 156)

What the final section of this paper intends to elucidate is that Smith and Siegel's conflicting views on the integration of psychology and economics provided even ampler ground for their disagreement.

4. Why economics and psychology have given so different accounts of individual behaviour?

Today, the relations between economics and psychology have become a popular topic under the heading of behavioural economics (Camerer 1999, Rabin 1998). The history leading to this development has not been straightforward. In the same years in which Siegel gave his contribution, others scholars forcefully claimed that economics would have benefitted from incorporating findings and theories from psychology. It is a fact, though, that these efforts remained out of the mainstream for a long period after. The work of Tibor Scitovsky, Harvey Leibenstein, George Katona, and Herbert Simon as well, had to wait a few decades for being fully appreciated and developed.¹⁴ Only recently, the so-called cognitive revolution has made clear that focusing on the psychological determinants of behaviour and, specifically, on brain functioning as an information-processing device is quintessential to the analysis of economic decision making. An explanation for this evolution is that the most common approach uses the standard economic definition of rationality as a benchmark to which psychological insights are to be tailored. The main consequence is that standard economics would preserve its integrity: "We predict that mainstream economics will ultimately meet the behavioral challenge by developing a new quasi-rational synthesis. Such a synthesis, for example, will identify when and where the framing of choices dramatically affects what choices are made and it will study how framing is conducted and countered in the real world.

¹⁴ See Lewin (1996) and Sent (2004) for detailed accounts of this history.

Better predictions, say of consumer choices, will be the result, but the standard framework of economic maximization will be for the most part preserved.” (Laibson and Zeckhauser 1989, p. 32)

Assessments like these take for granted that psychology and economics can successfully complement each other without changing their respective assumptions and premises. Psychology would inject greater realism into economic models without modifying its inductive orientation and, at the same time, economics would benefit from psychological insights without dismissing its deductively based approach and the rationality principle.

The problem with this sort of wishful thinking is that a long-standing debate compellingly points out that economics and psychology adopt dualistic and polarized ways to scientific knowledge. The list of features supporting this claim is a rich and multifaceted one.

A first element is related to the very object of analysis. While economics mostly adheres to the hypothesis of representative agent to build its abstract models, psychology and, more generally, cognitive sciences analyze individual behaviour in all its wide and disparate variety. Indeed, most of achievements in these fields are due to the investigation of exceptional and pathological cases, which highlight by contrast stable and persistent patterns of behaviour.

By adopting a heterogeneous conception of individuals, psychologists are prone to think of people as being motivated by different and often conflicting driving forces. This pluralistic view clashes with economists’ common assumption that money is the main motivation of people’s effort.

The same self-regarding assumption is taken by economists at face value. Not only individuals would be interested only at their own utility but they would be indifferent on relative differences in interpersonal utilities. On the contrary, psychologists hardly deal with strategic situations like bargaining without making assumption on how each individual takes into account other people’s needs and conditions to determine her or his choices.

This feature contradicts the commonly taken view that economists are interested in institutions and psychologists in individuals. It is rather the psychological approach to social interactions to assume that behaviour is, at a great extent, environment dependent. In contrast, even when economists acknowledge that framing matters, they usually ignore the possibility that individual’s choices might depend on guessing others’ decisional frames.

On the methodological level, psychology and economics are even more distant. They use quite different languages. Psychologists fail to assimilate terms like equilibrium or externality, but their vocabulary, expanded by more frequent interdisciplinary connections, is richer and articulated than economist’s, which has been purified from ambiguities by becoming increasingly formal and mathematical.

The axiomatic turn in economics has also produced the effect that its theoretical models are basically untestable without further manipulation. If axiomatics means to infer logical implications from given abstract premises, economic models cannot be directly refuted by empirical evidence but only in terms of internal inconsistency. On the other hand, psychological findings are mostly empirically determined and thus testable not only in the laboratory but also in the field. Such a surreptitious division of labour, according to which economists would be entitled to make theory and psychologist to perform experiments, has further increased the difficulties in the interaction between them. (Mourningham and Roth 2006)

This overall picture gives reasons enough to explain why psychology and economics remained so long apart in their efforts to understand and predict human behaviour. From an historical point of view, further insight can be gained by looking at the early history of experimental economics, of which Siegel and Smith were the main actors.

As well known, Smith's first experimental paper refuted Chamberlin's (1948) imperfect market experimental findings. By introducing a sequence of trading periods instead of Chamberlin's uninterrupted series of exchanges and by using the double oral-auction procedure, he obtained a robust convergence toward the competitive equilibrium.

Smith's leading role made his view on the relations between psychology and economics enormously influential. He addressed it directly in a paper published in 1991, whose concluding section started by questioning: "Why is it that human subjects in the laboratory frequently violate the canons of rational choice when tested as isolated individuals, but in the social context of exchange institutions serve up decisions that are consistent (as though by magic) with predictive models based on individual rationality? Experimental economists have no good answers to this question, although adaptive learning models such as those of Lucas (1987) are suggestive. We need the help of psychologists, undeflected by battles with straw men." (Smith 1991b, p. 894)

To illustrate how this support could be provided, Smith came up with a revealing metaphor: "Language learning in children occurs in a social context. Without contact with people, children do not learn to speak. If they have such contact, they learn to speak in the total absence of formal instruction. But the same can be said of decision making: I could substitute "make market decisions" for "speak" in the last two sentences and they would apply to what we have learned in the laboratory about adults. On the basis of cognition alone, without the language of the market and ongoing social interaction with other agents, rational decision is frustratingly illusive." (Smith 1991b, p. 894)

This amounted to say that if experimental evidence on individual behaviour was contradictory with that on competitive markets, psychology could be helpful to reconcile the latter

with the former by devising what tacit learning process enhanced optimality when moving from the individual to the collective level. Smith was so consonant with what Lee and Mirowski (2006) call his “commitment to a special version of neoclassical demand theory” that psychologists were asked not to elucidate the reasons of the contradiction, but to provide empirical elements supporting the reference model.

Such a view, which places psychology in an ancillary position with respect to economics, also informed Smith’s 1962 paper. Although there is no doubt that it accelerated the methodological improvement of the discipline, but the presumption it created had a contradictory effect on subsequent developments. On the one hand, a new tool confirming what had already been proved theoretically should have been more readily accepted, but on the other hand Smith’s outcome made less attractive to integrate those psychological findings contradicting the rational approach to economic behaviour.¹⁵

On the theoretical side, this pattern of evolution was the consequence of the presumptive separation between models and empirical analysis characterizing mathematical economics since its inception, which has led economists to underrate or even evade the issue of the empirical plausibility of their theories. They have typically preserved their assumptions against factual counterarguments by inculcating in their methodology a sort of rigidity that is reminiscent of the concept of Lakatos’ hard core but that can be better defined as a sort of “history dependence”. When the mainstream community agrees on the effectiveness of a formal assumption, this is placed in the black box of accepted postulates and treated as irrefutable. Exhibiting similarities with the effect of path dependence in biological and social processes, the formalist revolution in economics has been affected since its inception by a “sensitive dependence on initial conditions” (Liebowitz and Margolis 1995, p. 210), which has hindered rather than promoted methodological innovations. This interpretation would justify, for example, the charge of “innocuous falsificationism” made by Blaug (1980, p. 259) about the methodology of economics or the disregard shown by economists for the use of spatial models (Krugman 1995, pp. 65).

As shown in this paper, Siegel’s view of interdisciplinary research was quite different. He was constantly in search for new empirical regularities. His approach to experimentation was deeply heuristic, in that he aimed at discover new stylized facts with the intent of increasing the empirical significance of economic models. Such an orientation did not conflict with the deductive foundation of economics. As a matter of fact, Siegel considered the ambiguities of the psychological language

¹⁵ It is well known that in the 1950s other experiments (Allais 1953, May 1954, Edwards 1953, 1954) had provided wide evidence contradicting the hypothesis of utility maximization and, more specifically, of preference consistency, but these works had to wait at least two decades to be fully appreciated. (see Moscati 2007)

as its main deficiency, whose removal had improved its effectiveness. But this formal upgrading should have not necessarily made psychology auxiliary of mainstream economics. Siegel was a genuine behaviourist in that he intended “to demonstrate that experimental operations based on a consideration of a psychological construct, utility, lead to predictable choice behavior. The reader is asked to note also the appearance of the word *behaves* in the core hypothesis. Analyses are directed to the ways, in which people actually behave, not to how they say they behave or would behave, nor to how they might expect others to behave. In our judgment, the hypothesis of maximization of expected utility can be given a fair test only by research in the behaviorist tradition. Choice behavior must be observed in realistic and significant choice situations.” (Siegel, Siegel and Andrews 1964, p. 19)

This attitude led him to adapt theoretical models to experimental results and not the reverse. When, for example, he firstly approached von Neumann and Morgenstern’s utility theory, he was clearly sympathetic with the rational hypothesis of individual maximization. Laboratory findings convinced him that the validity of this hypothesis strictly depended on psychological factors such as risk attitudes, motivation, boredom and level of aspiration. Thus, he openly integrated these elements in his models by discussing their theoretical consequences.

What moved Siegel on a different path with respect to Smith was his psychologically grounded sensibility. He had in mind that subjects in the laboratory were different each other and he always offered an interpretation of patently deviant behaviours. Notwithstanding his emphasis on the motivation issue, he constantly took into account that subjects could be motivated by a variety of reasons. He firmly supported the belief that economic choices were environment dependent and this implied that experiments did not simply aimed at building behavioural regularities but also to investigate how the manipulation of external conditions could make maximization behaviour more or less probable. Only in this perspective can his immediate consonance with Simon’s behavioural approach be adequately appreciated. In this way, Siegel claimed that an alternative to rational choice or mechanic adaptation in learning theory existed. This argument was promptly received by another future Prize Nobel, Reinhard Selten, who just few years later conducted a path-breaking experiment on aspirations and adaptation in the theory of firm (Sauermann and Selten 1962). It is not surprising that this line of research developed autonomously and recently re-emerged in the realm of behavioural economics. (Camerer 2003)

Finally, what this historical assessment shows is that Siegel was not only a pioneer of experimental economics but also of behavioural economics. Had his view on the integration of psychology and economics been more promptly received, it might have triggered a different and more successfully path to the injection of greater realism in economics. When Siegel died, his

approach to integrate psychology and economics lost his main advocate. Although his legacy was paramount in the methodological contribution of Vernon Smith's, Siegel endorsed a quite different approach to how make interdisciplinary research effective. There are many reasons to think that only another psychologist could have taken forward Siegel's insights and one might wonder because this did not happen. The answer is probably that what he made in nearly 10 years needed much more time to be further developed: "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 1967, p. 293)

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