

Early Development of Experimental Economics at the Interdisciplinary Crossroads[†]

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Abstract

The present paper aims to elucidate the interdisciplinary interactions in which a father of experimental economics, Vernon Smith, engaged in the early 1960s. In particular, the paper calls attention to a thread of research which evolved around the psychological concept, level of aspiration, and suggests that the local interaction of Smith with William Starbuck, the latter of whom once criticized Smith's early experimental endeavor, set Smith on the quest for a methodological means whereby experimental economists could design laboratory experiments whose outcomes could be meaningfully interpreted with the help of the law of supply and demand.

Keywords

Vernon Smith, William Starbuck, level of aspiration, satisficing, induced value theory

1. Introduction

... we admit that the idea [that the endowment effect, status quo bias, and the aversion to losses are both robust and important] is now part of our endowment, and we are naturally keener to retain it than others might be to acquire it. (Kahneman, Knetsch and Thaler 1991: 205)

The present paper aims to elucidate the interdisciplinary interactions with behavioral scientists in which a father of experimental economics, Vernon Smith, engaged in the early 1960s. In a previous paper (Lee and Mirowski forthcoming), I argued that Smith's induced value theory—his trademark methodological innovation—as it had been put to work in practice, was tightly linked to what Smith thought was an appropriate way to conceptualize the relationship between preferences and demand—according to which the law of demand would never be violated. As Smith himself repeatedly indicated, he set about to build the induced value theory in 1963 (Smith 2002: 14; 1992: 275)—or in 1964 (Smith 1976: 247, n. *)—almost right after his interdisciplinary interactions. In the present paper, I call attention to them with a view to lending support to the conjecture that they did set Smith on the quest for a methodological means whereby experimental economists could design laboratory experiments whose outcomes could be meaningfully interpreted with the help of the law of supply and demand. Furthermore, the present paper suggests it may have been the case that in the second half of the twentieth century, cognitive and/or psychological models in behavioral sciences had 'additional' difficulty infiltrating the economics discipline especially when they were advertently or inadvertently mobilized to bring into question a certain, well-entrenched idea that economists were

“naturally keener to retain ... than others might [have] be[en] to acquire” (see the epigraph), such as the law of supply and demand.

At the outset, it is worth stressing that Smith (1992: 247-8, 260-75) himself explicitly acknowledged the influence of a psychologist, Sidney Siegel, upon the initiation of the former’s methodological endeavor (later to culminate in the birth of the induced value theory). The present paper does *not* cast doubt on Siegel’s influence on Smith in connection with the formulation of the induced value theory that Smith has emphasized in his account of the early period of his version of experimental economics. Rather, it brings into focus another interdisciplinary interaction on which no light has yet been shed, viz., Smith’s intellectual interaction with a behavioral scientist, William Starbuck, in the early 1960s. By so doing, the present paper aspires to make more complete the portrayal of the early development of Smith’s version of experimental economics at the interdisciplinary crossroads.

To this end, I bring into the foreground the context and content of Starbuck (1963b) and how the central concept therein, the level of aspiration (LOA henceforth), was used by Starbuck to criticize the law of supply and demand. Interestingly enough, both Herbert Simon and Sidney Siegel paid special attention to the LOA in the 1950s and 1960s, reflecting the influence of a prominent social psychologist of the twentieth century, Kurt Lewin; and Starbuck’s criticism (1963b) of Smith’s landmark paper (1962) had recourse to the LOA, reflecting, in turn, the influence of Simon and Siegel. After providing a précis of Smith’s early classroom experiments and clarifying what kind of psychological problem was lurking in the background of his experiments in section 2, I briefly summarize Simon’s use of the LOA in his satisficing hypothesis in section 3. Not only because the LOA was first studied intensively by a pioneer of twentieth-century (social) psychology, Kurt Lewin, but also because his work on the

LOA exerted significant influence upon Starbuck and Siegel, section 4 is devoted to reviewing Lewin's work on the LOA. Subsequently, Siegel's research on the LOA is discussed in section 5. Sections 3 to 5 are intended to characterize the context relevant to understanding Starbuck (1963b). In section 6, I sum up Starbuck's paper (1963b), which was directly aimed to criticize Smith's early experimental endeavor, and put forward a plausible interpretation of what Smith learned (not) to do from his interdisciplinary interactions. And section 7 concludes.

2. The humble beginning of Smith's experimental economics at Purdue

Most intellectual traditions have an iconic account of their origins, and Smith's version of experimental economics is not different. As Smith (1981: 369-70; 1992: 241-2; 2002: 10-11; see also Lynch and Gillespie 2002: 39) himself recounted repeatedly, his first encounter with an (economic) experiment took place in the fall of 1952—his first year at Harvard University. When Smith entered Harvard, it had been four years since the publication of Edward Chamberlin's paper (1948) on "pedagogical experiments" (95). As one might suspect, Chamberlin (1948) had no opinion of laboratory experimentation as a 'serious' research method for economics; he merely found the outcomes of his pedagogical experiments instrumental in adding minor strength to his condemnation of standard competitive economic theory (102).

In any case, Chamberlin considered his experiments, at least, "stimulating and instructive to students" (95); accordingly, he performed another classroom experiment on the very first day of his graduate monopolistic competition course in the fall of 1952, with a later-to-be father of experimental economics, Smith, in the student/subject pool. In his classroom experiment, Chamberlin distributed cards with maximum buying prices to half of the students/subjects, and those with minimum

selling prices to the other half; thus, the first half of the students/subjects assumed the role of buyers, and the other half the role of sellers. Buyers (sellers) were instructed to move around inside the classroom to meet and haggle with sellers (buyers) until both parties settled their negotiations. Once a deal was struck, the buyer and the seller involved were to report their negotiated price and quantity to Chamberlin. When all the students/subjects finished their negotiations, or when no more agreement was forthcoming, Chamberlin announced conclusion of the experiment, and subsequently showed his students/subjects the underlying, “implicit supply and demand (without-income effects) schedules” (Smith 1981: 370).¹ Chamberlin demonstrated the discrepancy between the (predicted) price at the intersection of supply and demand, and the actual trading prices; of course, he also illustrated the incongruity between the (predicted) quantity at the intersection of supply and demand, and the actual trading volume. The conclusion to be drawn was simply and clear: “supply and demand theory was worthless in explaining what had happened; namely, that prices were not near the equilibrium, and neither was the quantity exchanged” (370).

According to Smith (1981: 369-70), contrary to Chamberlin’s expectation, most Harvard economics graduate students in the early 1950s were far from enthusiastic about Chamberlin’s classroom experiments, and Smith was no exception. Three years or so later, however, it occurred to Smith that Chamberlin’s classroom experiments might help him teach introductory economics to hard-nosed engineering students at Purdue University (1981: 370; 1992: 242). He was determined not to reiterate Chamberlin’s procedure though. Instead, he substituted a double auction procedure for Chamberlin’s less structured one, so as to increase the likelihood of corroboration of the supply-and-demand-based competitive theory (Smith 1981: 370; 1992: 243; 2002: 11).

In a simple oral double auction experiment, the subject/student pool is divided into two sub-groups. Students/subjects in one group take the buyer roles; those in the other group, the seller roles. Each student/subject is given a card with a reservation price. Depending on the role assigned to a subject, his/her reservation price represents either a maximum buying price or a minimum selling price. During the experimental 'period,' each subject is allowed to submit a verbal offer to buy ('bid') or sell ('ask') to an auctioneer, subject to his/her role and reservation price. A new bidding price should be greater than the standing bidding price; symmetrically, a new asking price should be less than the standing asking price. Whenever a buyer (seller) accepts the standing asking (bidding) price, a trade is made at the very standing asking (or bidding) price just accepted. This procedure is repeated within a pre-determined time limit of a 'period.' When the time limit is reached, or no more bidding or asking is forthcoming, the period is closed, and a new period is commenced. A 'run' consists of several distinct periods, the number of which should be determined beforehand. An 'experiment' is composed of either several 'runs' or simply one 'run.'

During the five-year period (1956-1960), Smith performed a series of classroom experiments at Purdue using the basic double auction procedure coupled with various market supply-and-demand configurations; his first publication in experimental economics (Smith 1962) was solely based upon those classroom experiments at Purdue. Although it is fair to say that Smith's experimental designs in that period were far from sophisticated, his experimental 'outcomes' were quite notable. In most of his supply-and-demand experiments, Smith observed rapid convergence of trading prices and quantities toward the price and quantity at the intersection of supply and demand curves.

As I have so far explained, Smith (1962) replaced Chamberlin's trading procedure with an oral double auction procedure; however, still the same was the procedure of deriving the market supply and demand curves from the minimum selling prices and maximum buying prices assigned to seller students/subjects and buyer students/subjects. Within a period, these prices were not to be altered, that is, they were *imposed* as the immutable bounds upon subjects' behavior. Smith assumed that they could directly be linked with the individual supply-and-demand schedules (or more accurately Smith took those prices as individual supply-and-demand schedules); market supply (demand) schedules were simply the horizontal sum of individual supply (demand) schedules.

This drastically simple logic of supply and demand is rarely discussed beyond the first chapters of undergraduate introductory textbooks, and Smith had no problem with this rough-and-ready approach partly because he was performing his classroom experiments in his undergraduate introductory classes. Given the popularity of Marshall in introductory and intermediate courses, it comes as no surprise that Smith had in mind Alfred Marshall's *Principles of Economics* and, as Smith himself pointed out, "Marshall's celebrated description [therein] of a corn exchange" (1992: 243). In Marshall (1920, Book V, Chapter II, Section 2), where the corn exchange example was presented, Marshall indeed used the minimum selling prices and maximum buying prices as bounds upon behavior without evoking anything remotely close to the utility maximization subject to budget constraints. The point is that in both Chamberlin's experimental design and Smith's (1962), supply-and-demand schedules were imposed without much justification of what might lie beneath them as their causes or determinants. In other words, they were taking a phenomenological stance as to supply-and-demand theory. Smith changed his position as to whether one should

go beneath supply and demand sometime between 1960—when he submit the first draft of Smith (1962) to *Journal of Political Economy* (see Smith 1981: 372; 1992: 245)—and 1963 or 1964—when he initiated his first attempt at the induced value theory. Then, the question is: why did he feel obliged to go beneath supply and demand?

At this juncture, one has good reason to pay attention to, with a little imagination, the parallels between Smith's odyssey and the attempts made by such thinkers as Fleeming Jenkin, W. Stanley Jevons, and Alfred Marshall to resolve the problematic aspects of the law of supply and demand—see Mirowski (2004: chaps. 13, 14). In particular, it is useful to pay some heed to what concerned one of the first inscribers of the (geometric) supply and demand curves, Fleeming Jenkin:

The first law of demand and supply may now be stated as follows:—Prop. 1. *In a given market, at a given time, the market price of the commodity will be that at which the supply and demand curves cut.* ... The law thus stated assumes that each man knows his own mind, that is to say, how much of his commodity he will then and there sell or buy at each price, and that the condition of his mind shall not vary. ... But, in practice, men's minds do not remain constant for five minutes together... (Jenkin 1887: 77-9, emphasis in original)

This extract clearly indicates that to derive Chamberlin's and Smith's supply and demand curves, one should posit the fixity of mind. Of course, they operationalized this desideratum by assigning fixed reservation prices to subjects. Yet, could this experimental practice be justified by some empirical evidence testifying that market participants come equipped with fixed mindsets? Probably one way to circumvent answering 'difficult' questions of this sort might be to augment the

economics a bit with a view to maintaining that ‘maximization’—be it unconscious or not—is going on beneath the surface of supply and demand, and thus to transform the issue of the fixity of market participants’ mindsets into the problem of maximization and the less problematic issue of the profit motive in the market—the more the better, as far as money concerns (cf., Mirowski 2004: 349-52). Indeed, one may well argue it was exactly this ‘transformation’ that Smith intended to achieve with his induced value theory.

3. Herbert Simon and the reservation price as the level of aspiration

As opposed to most contemporaneous economists, Herbert Simon (1955) did not posit the fixity of mind in considering market exchanges, nor did he opt for maximizing as a relevant cognitive process. According to him, the reservation price would have an adjustment mechanism of its own—instead of being fixed by a certain external force—and would play a crucial role in the workings of the cognitive process of relevance to economics and behavioral sciences, that is, satisficing. The LOA was crucial in Simon’s satisficing because, he thought, the reservation price was nothing other than the LOA, a concept frequently deployed in psychology. For instance, Simon considered how an economic agent desiring to sell his/her house would simplify his/her decision-making process:

As an example of (a) [simplification of the continuous utility function into the two discrete ranges (satisfactory or unsatisfactory)] let S represent possible prices for a house an individual is selling. He may regard \$15,000 as an “acceptable” price, anything over this amount as “satisfactory,” anything less as “unsatisfactory.” In psychological theory we would fix the boundary as the “aspiration level”; in

economic theory we would fix the boundary at the price which evokes indifference between selling and not selling. (Simon 1955: 104-5)²

From Simon's perspective, the reservation price is the LOA, and the LOA is the barometer that guides economic behavior. The LOA-based satisficing behavior could be compared to the use of a combination heating-cooling thermostat. In summer, the temperature to which the (cooling) thermostat is set is the maximum allowable indoor temperature; in winter, the temperature to which the (heating) thermostat is set is the minimum allowable indoor temperature. Therefore, externally fixing the reservation price for a buyer (the LOA for a buyer) is analogous to living in an apartment equipped with an excellent air conditioner, yet with a strange thermostat whose temperature setting could be altered only on, say, May 15 and August 15. From Simon's viewpoint, this system is inexcusably rigid, so much so that the heating-cooling thermostat which does not allow a resident to change the temperature setting as frequently as possible at his/her will could readily be thought of as exemplifying utter irrationality. Hence, it comes as no surprise that Simon later discussed (March and Simon 1958: 48-9) the feedback link between search outcomes and the LOA adjustment mechanism.

And yet, from the perspective of most contemporaneous economists, to consider the reservation price to be identical with the LOA—the latter of which was equipped with an adjustment mechanism of its own—was to muddle the analysis since the equilibrium outcomes could hardly be pinned down. It is plausible that the supply and demand curves conceived as the LOA schedules do not stay stationary, even when no trade has yet been made. A crucial component of Simon's satisficing

hypothesis, the LOA, appeared to most contemporaneous economists, in all probability, too treacherous a concept to accommodate.

4. The level of aspiration as it was formalized by Kurt Lewin and his collaborators

In Simon's own words, the LOA plays a crucial role of "provid[ing] a computational mechanism for satisficing" (Simon 1996: 30); and one might consider the LOA to be inherently antithetical to maximizing. In fact, that is far from the truth, and the LOA as it was conceptualized by one of the pioneers of twentieth-century (social) psychology, Kurt Lewin, was, by definition, tightly linked to maximizing as a cognitive process.

As two renowned social psychologists, Morton Deutsch and Robert Krauss (1965), once remarked, "[p]erhaps no other area of research ... has been the subject of so many studies as that of the level of aspiration" (51) among a number of different threads of theoretical and experimental research conducted by Lewin—an intellectual giant, whose life and scholarly achievement have attracted huge attention from historians of postwar American (social) psychology.³ Since the early 1930s, when the very first experimental inquiry of the LOA was conducted in Berlin (see Lewin et al. 1944: 333), the LOA attracted so widespread—albeit unorganized—attention from experimental psychologists that Lewin (and his students) could not help portraying the experimental studies of the LOA conducted between the early 1930s and 1944 as "a bit chaotic" (Lewin et al. 1944: 356). In the first half of the 1940s, Lewin and his collaborators were determined to put forward a formal, theoretic treatment of the LOA so as to "give orientation to further experimentation" (1944: 356).

The crux of the theoretical framework deployed by Lewin et al. (1944) can be summarized as follows:⁴ let us assume that an individual is confronting a decision-making problem in a goal-striving situation. She should make up her mind over which level of difficulty of a given task (A^n)— n denotes a level of difficulty; A stands for the given task—to take as her next goal to pursue. She will either ‘succeed’ in attaining a goal (Suc) or ‘fail’ to accomplish a goal (Fai). Different levels of ‘valence’ (V_a)⁵ are associated with ‘success,’ and ‘failure.’ She takes into account the valence of future success in accomplishing the selected level of difficulty of the task, $V_a(SucA^n)$, and also the valence of future failure, $V_a(FaiA^n)$. In addition, she considers the probability of success and the probability of failure as she perceived them, i.e., the “subjective probability of succeeding and failing” (Lewin et al. 1944: 361): $Prob(SucA^n)$ and $Prob(FaiA^n)$. Then, she configures two different “weighted valences” (364) out of the two valences and the two probabilities. First, she calculates the weighted valence of future success, ${}^{\circ}V_a(SucA^n) = V_a(SucA^n) \cdot Prob(SucA^n)$. Second, she calculates the weighted valence of future failure, ${}^{\circ}V_a(FaiA^n) = V_a(FaiA^n) \cdot Prob(FaiA^n)$. And then, she puts together two different ‘weighted valences’ to form the “resultant weighted valence” (364). In fact, she simply adds the weighted valence of future success and the weighted valence of future failure to form the resultant weighted valence, ${}^{\circ}V_a(A^n) = {}^{\circ}V_a(SucA^n) + {}^{\circ}V_a(FaiA^n)$. Finally, she maximizes the resultant weighted valence (with respect to n , level of difficulty). In the expression of Lewin and his collaborators (364), the LOA is defined as follows: level of aspiration = n when ${}^{\circ}V_a(A^n) =$ maximum.

From today’s perspective, Lewin’s notations might look clumsy. However, one cannot miss that the discussion of the LOA by Lewin and his collaborators bore a

close resemblance to subjective expected utility theory; and indeed this striking resemblance was soon to be noticed by Siegel.

5. The level of aspiration as it was discussed by Sidney Siegel

In recent years the level of aspiration concept has enjoyed a revival as a basic element in cognitive theory. One reason for interest in the concept is that it forms a meeting ground between modern utility theorists and Lewinian field theorists. (Starbuck 1963a: 51)

In his comparative study of experimental economics and experimental social psychology, Robert Leonard (1994) made an interesting observation concerning Siegel's experimental studies of economic models in collaboration with an economist, Lawrence Fouraker (Siegel and Fouraker 1960; Fouraker and Siegel 1963). According to Leonard, Siegel and Fouraker's experiments—the ones held in high esteem by Smith (1992: 268-75)—were “intended to explicitly reveal the shortcomings of such models [economic models of bilateral monopoly or two-person bargaining] and suggest where recourse might usefully be made to psychology: hence the incorporation of the ‘aspiration level’” (Leonard 1994: 350). Indeed, Siegel and Fouraker (1960: 69-70) stressed the lack of explanatory power of traditional economic models of bilateral monopoly or two-person bargaining, and claimed that the LOA would go some way toward elucidating the process and outcome of bilateral monopoly or two-person bargaining.

In fact, Siegel did not simply made use of the LOA to criticize some economic models. He also made an effort to spell out how the LOA itself would come into being through the cognitive workings of a decision maker. After receiving his Ph.D.

in psychology from Stanford University in 1954, Siegel was drawn toward decision theory through his involvement in the Stanford Value Theory Project directed by two analytical philosophers, Donald Davidson and Patrick Suppes (see Davidson, Suppes and Siegel 1957). It appears that after the involvement, Siegel was disillusioned at the old style of social psychology, converted into a steadfast advocate of subjective expected utility theory (see Messick and Brayfield 1964). Thus, it comes as no surprise that he—who had held in high esteem Lewin’s work (see A. Siegel 1964: 8)—highlighted the similarity between the theoretical framework employed by Lewin et al. (1944) and subjective expected utility theory, and called attention to the potential gains from trade between (traditional) psychology and subjective expected utility theory:

It is a remarkable fact that, by a simple change in nomenclature, the theoretical model used by Lewin et al. ... in the prediction of the choices (decisions) of individuals in a goal-striving situation ... may be rendered fundamentally equivalent to the theoretical model employed by decision and game theorists. (Siegel 1957: 253)

The benefit to traditional psychologists, accustomed to dealing with terms like “success” and “failure,” will accrue because of the much greater formality and rigor characteristic of decision theory, a rigor unknown to many psychologists. The benefit to decision theory will come because the synthesis will render available to decision theorists a large amount of existing experimental evidence with relevance to their work which has not yet been recognized by them. (254)

Fusing the influence of Lewin and the enthusiasm toward subjective expected utility theory, Siegel tackled the problem of “ascertaining a person’s level of

aspiration” (Siegel 1957: 261) in 1955 to 1957 (A. Siegel 1964: 10-11). For instance, Siegel (1957) made an attempt to fill the lacuna in Lewin et al. (1944) by bringing into discussion the problem of measuring the degree of valence (utility), a central variable decision makers were presumed to act on. In particular, he proposed that one should give heed to the following question: what kind of utility function can best capture the decision-making behavior of an individual with a certain LOA?

Siegel tackled this question by considering simple decision-making situations, which can be recapitulated as follows (see Siegel 1957: 256-7): suppose a student is taking a class. She desires to receive at least a B—in other words, her LOA is a B. She is told that her final grade will depend on outcomes of the choices she will make, and that she will be faced with two different decision-making tasks. In addition, she is informed that her final grade will also depend on coin flipping.

In the first decision-making task, she can choose either 1 or 2. If she chooses 1, her grade will be either an A or a C depending on the outcome of coin flipping. If the head occurs, her grade will be an A; otherwise, a C. If she chooses 2, her grade will be a B no matter what happens in coin flipping. The first decision-making situation is summarized in table 1; and the second decision-making situation analogous to the first one in table 2.

[table 1 here]

[table 2 here]

According to Siegel (1957), the student will surely choose 2 in the first decision-making task and 3 in the second decision-making task “since [her] level of aspiration is to achieve a grade of B” (256) and “in terms of [her] level of aspiration

any grade less than B is a “failure” (257). Therefore, one arrives at the following conclusion: $P(\text{head}) \cdot u(B) + P(\text{tail}) \cdot u(B) > P(\text{head}) \cdot u(A) + P(\text{tail}) \cdot u(C)$, and, therefore, $u(B) - u(C) > u(A) - u(B)$. $P(\text{head}) \cdot u(B) + P(\text{tail}) \cdot u(D) > P(\text{head}) \cdot u(C) + P(\text{tail}) \cdot u(C)$, and, therefore, $u(B) - u(C) > u(C) - u(D)$.⁶ In other words, one can conclude that the distance between B and C is the greatest on the utility scale (see Siegel 1957: 257).

Based on this conclusion, Siegel detailed three characteristics of the LOA (257). Furthermore, on the basis of the characteristics, he put forward his “formal and more rigorous definition” (257) of the LOA: “The level of aspiration of an individual is a point in the positive region of his utility scale of an achievement variable; it is at the least upper bound of that chord (connecting two goals) which has maximum slope; i.e., the level of aspiration is associated with the higher of the two goals between which the rate of change of the utility function is a maximum” (257, emphasis removed).

Note that the LOA is determined through the (subjective expected utility) maximizing process in Lewin et al. (1944); on the other hand, Siegel (1957) did not spell out the way that the LOA would be established in the first place. The crux of Siegel’s (1957) demonstration was that the LOA would be “associated with the upper bound of the largest distance on the utility scale” (257). Even if the utility (or subjective satisfaction) of an individual could be represented in the way described by Siegel (1957)—and even if it is admitted that the LOA is determined in the first place through the (subjective expected utility) maximizing process—one can never be sure that the LOA-guided behavior (as opposed to the LOA-setting behavior) could best be explained by subjective expected utility theory.

It appears that Siegel was well aware of this problem. In the penultimate section of Siegel (1957), he highlighted that Simon's (1955) satisficing model was concerned with sequential decision-making situations—in Siegel's own words, “realistic human situations requiring decision” (260)—where exhaustive search is impossible. And Siegel's added:

... when the individual does not know what variables determine the order of the offers (and therefore does not know whether the future alternatives are likely to be “better” or “worse” than those with which he has already been confronted), he may well choose the first satisfactory alternative which is offered. In the terms of the present paper, the individual may choose the first alternative containing an outcome at or above his level of aspiration. The techniques suggested in this paper could be used to provide an empirical test of this hypothesis. (Siegel 1957: 260)

So, Siegel did not bring into question Simon's use of the LOA in the context of sequential decision making, the exemplary decision-making situation Simon had in mind in putting forward his satisficing hypothesis. Be that as it may, what is especially intriguing is the idea Siegel (1957) introduced in the final section of the paper: “... a useful behavioral model of decision making ... 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 (*LOA*)⁷ and reinforcement effects (*R*)” (261). What is the ‘reinforcement’ that Siegel introduced out of blue? Siegel left no indication whatsoever in his paper. And yet, today's experimental economists can immediately recognize what Siegel had in mind: the use of monetary payments to experimental subjects—see Siegel (1959, 1961). The final move Siegel (1957) made

in his paper was to shift attention to non-sequential choice situations and present his “extended model” (261):

In terms of such an extended model, it may be said that if various alternatives are available to an individual [if the decision-making situation is not sequential], he will choose from among these alternatives, toward each of which he has a subjective probability of attainment and a utility, so as to maximize subjectively expected utility *SEU*. That is, the individual will choose so as to maximize $SEU = \sum_i p_i u_i$, where $u = f(LOA, R)$. (Siegel 1957: 261)⁸

It is not easy to pinpoint what Siegel intended to do with his ‘extended model.’ Yet, at this juncture, one should be reminded that Smith learned the significance of the use of the monetary payments—the ‘reinforcement’—to experimental subjects directly from Siegel (Smith 1992: 247-8, 260-75), and also that Smith has never paid attention to the LOA in his published papers. Interestingly enough, in the early 1960s, a behavioral scientist, William Starbuck, left an interesting comment on the relationship between the LOA and the monetary payments to experimental subjects:

The level of aspiration is most likely to be a significant concept in situations where the problem solvers’ subjective definitions of success play important roles in their decisions. In many circumstances (particularly circumstances of the type often created for an experiment), subjectively defined success is important simply because the net utilities ... are relatively unimportant ... little effort required and small monetary or social rewards [*sic*]. (Starbuck 1963a: 54)

According to Starbuck (1963a), the LOA loses its significance in the laboratory experiment only if the workings of experimental subjects' 'subjective definitions of success' are repressed; and one way to repress them is to employ a significant amount of monetary rewards. Now the question is: was there any significant reason that Smith wanted to prevent the LOA from intruding into his laboratory? It is notoriously difficult to make definitive statements about intentions of scholars. Yet, it is sometimes possible to present plausible and informed conjectures about them. Section 6 is intended to lend support to the conjecture that one of the goals Smith wanted to achieve with his induced value theory was to keep the LOA from intruding into his laboratory.

6. Mobilizing the level of aspiration to bring into question the law of supply and demand: Starbuck's case

The year 1962 was a milestone in the development of Smith's experimental economics for two different reasons: first, his first paper in experimental economics (Smith 1962) was published in *Journal of Political Economy*. Second, the support of his research program from the National Science Foundation (NSF) kicked off in 1962 (see Newlon 1989: 211, table 3). Two years later, the NSF provided research funding to the project—titled “Studies of Bargaining and Decision Behavior” (see also Newlon 1989: 211, table 3)—that Smith was conducting in collaboration with William Starbuck. In the first half of the 1960s, Smith and Starbuck were colleagues at Purdue, designing together the Behavioral Science Laboratories at the same institute (see Fromkin 1969: 171, n. 1; Starbuck 1993: 77-8). Starbuck took part in the Carnegie summer faculty research workshops in experimental economics organized by Smith and Lester Lave (see Smith 1992: 276); and, according to Starbuck's

autobiography (1993: 77), he was “invited [by Smith] to share a research grant for experimental studies of economic behavior.” In Smith (1964: 181, n. *), he thanked Starbuck for “valuable suggestions and comments”; Starbuck (1965: 451, n. 1) also expressed his gratitude for Smith’s “helpful criticism and comment.” Outwardly, they maintained a mutually supportive intellectual relationship until 1967 when both of them left Purdue.⁹

However, it is not that difficult to spot some ‘indications’ of the tension which I believe existed between Smith and Starbuck in the 1960s. First, Smith and Starbuck never wrote a paper together, although they shared the NSF research grant. Second, circa 1966 and 1967, Smith found Starbuck’s research performance disappointing, opting not to continue his collaboration with the latter.¹⁰ Third, Starbuck became firmly determined to abandon laboratory experimentation after his collaboration with Smith because of, among other things, his skeptical appraisal of the use of monetary payments to experimental subjects—the core component of the induced value theory Smith began building in 1963 or 1964 (see section 1):

... designing an experiment is much like writing a computer program. Just as a computer does only what it is told, almost all subjects strive to follow instructions and respond to offered rewards. Thus, subjects’ behaviors are direct results of the instructions and reward systems. My experiments were revealing a lot about my own beliefs and very little about my subjects’ properties other than obedience. I might better run computer simulations. Although complex simulations are very difficult to understand, even very complex simulations are much simpler than people. (Starbuck 1993: 76)

Starbuck believed that the use of monetary payments to experimental subject would prevent, among other things, the workings of the LOA—one of “subjects’ properties” (see the quote above)—as pointed out at the closing of the previous section. One may take the induced value theory—or the practice of controlling subjective preferences with the help of monetary payments to experimental subjects—as an outcome of Smith’s attempt to keep the LOA from intruding into his laboratory. In other words, it is a plausible conjecture that the LOA was at the heart of bifurcation of Smith and Starbuck. Indeed, Starbuck’s paper I discuss below (1963b) lends support to this conjecture.

In Starbuck (1963b)—which he originally wrote at Carnegie Institute of Technology in 1959 when he was involved in the Behavioral Theory of the Firm Project headed by Richard Cyert and James March¹¹—he aimed to depict “a first step toward a model of economic man which is more realistic from the viewpoint of the behavioral scientist” (128). To achieve this goal, Starbuck made use of the LOA and Simon’s satisficing hypothesis, and concentrated on a bargaining (bilateral monopoly) situation. He endeavored to elucidate the image of market emerging from his two-agent microcosm where both agents were to display the LOA-guided satisficing behavior. The resulting image of the market, succinctly recapitulated in the very last paragraph of the paper, was not to be welcomed by contemporaneous economists: “The market which a satisficing problem-solver creates is almost continuously in flux, and momentary equilibria are highly dependent upon the time path by which they are reached. The bounded rational decision process is based on the premise that equilibria are and should be attained rarely, if ever. The rationality of satisficing man rests on his ability to, and need for, change” (136).

How did Starbuck arrive at this conclusion? As one can see from a working-paper version of Starbuck (1963b), he drove a wedge between Lewin et al. (1944) and Siegel (1957) by stressing that in the latter, the LOA-guided behavior had good reason to conform to the satisficing hypothesis:

The fundamental differences between these two definitions lie in the assumptions about the way the individual views his behavior alternatives. The definition proposed by Lewin, Dembo, Festinger, and Sears implies that the individual makes comparatively subtle distinctions among behavior alternatives. The level of aspiration is not crucial to the process of making these distinctions; it is simply a significant point on the continuum of evaluated alternatives. The definition proposed by Siegel implies that the individual makes comparatively crude distinctions among behavior alternatives. The level of aspiration plays a central role in the process of making these distinctions, since the primary differentiation among alternatives depends upon whether or not the alternative is above the level of aspiration. (3)¹²

In addition, Starbuck (1963b) stressed the “introspective appeal” (130) that the satisficing hypothesis (as opposed to the maximizing hypothesis) had “as a characterization of the usual and normal economics decision” (130). Subsequently, he put forward his satisficing model designed to tackle a one-buyer-one-seller situation. In his model, Starbuck imposed two sets of constraints on each market participant, one specifying his/her LOAs (levels of aspiration), and the other specifying the stability condition that portrayed the “tolerance for differences” between his/her LOAs and the alternatives available to him/her.¹³ According to this simple model, the market would be in equilibrium only when all the four sets of constraints were satisfied—i.e., only when each market participant did ‘satisfice’ and his/her LOA was

stabilized. What is of particular note in Starbuck's model is that the intersection of supply and demand curves would completely lose its bite. Starbuck's discussion of the implications of his simple model for economic theory is worth quoting at some length:

... if the demand sector of a purely competitive market is assumed to be composed of "identical" consumers, as is usual, (1) the equilibrium market state is essentially a random variable within a fairly large set of potential equilibria, and (2) the market can be in equilibrium at a point other than the intersection of the demand and supply curves. In one sense, of course, the usual demand and supply curves are ephemeral concepts in a model where goals are changing continuously. The only long-run equilibrium which would have significance would be one imposed by factors exogenous to the particular market. However, if the demand curve is defined as the maximum quantity which will be bought at a given price, and if the supply curve is defined as the minimum price for which a given quantity will be sold, the intersection of supply and demand may not be an equilibrium state. [n. 9: A number of economists have recognized the need to treat the supply and demand curves as bounds on behavior. Marshall made this approach explicit in his characterization of a corn market (1948, Book V, Ch. II, Sec. 2), and it is a central issue in Friedman's treatment of demand and supply (1951, pp. 8-17 of 300A). Recently this approach has played important roles in contributions by Clower (1960) and Smith (1962).¹⁴ One could also define the demand curve as the maximum price at which a given quantity will be bought, and the supply curve as the minimum quantity which will be offered at a given price.] (Starbuck 1963b: 135-6)

Starbuck opened his critical remarks by pointing out the indeterminacy issue in the bilateral monopoly models. Since this indeterminacy issue of bilateral

monopoly models is well known, one may not find Starbuck's argument very interesting. Yet, the point Starbuck made with his simple model was that should the fixity of mind not be guaranteed (or should the fixity of market participants' subjective definitions of success not be assumed), the intersection of supply and demand curves would lose its bite and the law of supply and demand would also be jeopardized. Given that in Smith (1962), the intersection of supply and demand curves represented the equilibrium state against which to evaluate the workings of laboratory markets, one can easily see that Starbuck (1963b) did not see eye to eye with Smith (1962) concerning such fundamental issues as the workings of mind and the validity of the law of supply and demand.

Smith was familiar with Starbuck (1963b) even before its publication,¹⁵ and, therefore, well aware that his colleague, Starbuck, called into question Smith's early attempt at experimental study of the workings of the market. It seems that in the early 1960s, Smith was very much ready to talk to Starbuck, probably because many different options were open to Smith. He had just published one paper in experimental economics on the basis of his classroom experiments, and was, in all probability, free to develop experiments in many different directions. Indeed, he initiated, so to speak, adversarial collaboration with Starbuck, a behavioral scientist experienced in laboratory experimentation (see Cyert, March and Starbuck 1961). In any case, it seems that Smith learned, at the very least, one thing useful from Starbuck. As I have discussed above, Siegel considered the LOA and the reinforcement to be two significant arguments within the utility function of an individual; yet, he did not mention anything very explicit about the interaction (effect) between the LOA and the reinforcement (or simply money). It was Starbuck (1963a) who made a very explicit, albeit conjectural, statement about the interaction (effect) and about the case

where the workings of the LOA could be ignored—as indicated at the closing of the previous section. In so doing, he may have indirectly (and perhaps inadvertently) given to Smith a valuable methodological suggestion.

7. Conclusion

As Leonard stressed, Siegel and Fouraker (1960) sought to incorporate the LOA so as to make “a marriage of economics and psychology ... in the laboratory” (Leonard 1994: 350)—see also Roth (1993: 200). In the first half of the 1960s, the LOA was not entirely ignored by social scientists in the United States—for instance, Martin Shubik, who collaborated with Siegel in the early 1960s,¹⁶ considered the LOA-based solution concept a significant one for gaming and game theory purposes (Shubik 1964: 456, table 1). Also, Starbuck’s paper (1963b), in addition to Siegel and Fouraker’s LOA-centered research, appears to have been attended to in the context of consumer choice theory in the United States—see Aoki et al. (1971)—despite its critical tone. However, it was mainly German experimental economists, such as Reinhard Selten, Heinz Sauermann, Reinhard Tietz (see Selten 1998; Tietz 1983, 1990, 1997; Tietz et al. 1978), who paid sustained attention to the LOA and the LOA-based experiments. In Roth’s (1993: 200, n. 24) words: “American experimenters and theorists have subsequently come to regard aspirations as at most an intermediate variable, rather than as a primary explanatory variable. Our German counterparts [sympathetic to Simon’s research program] have been more inclined to regard aspirations as a primary explanatory variable.” Needless to say, Smith also put aside the LOA in his experimental endeavor.

In order to make more complete the portrayal of the early development of Smith’s version of experimental economics at the interdisciplinary crossroads, the

present paper has shed light on a path followed by the LOA-related research. The resulting picture of the interdisciplinary influence is more complicated than believed in the past (Smith 1992: 247-8, 260-75). Also the present paper suggests that cognitive models in behavioral sciences, such as the LOA-based models, had difficulty entering the American economics discipline not simply because they were foreign to economists, but also because they could easily be mobilized to bring into question a certain, well-entrenched idea in economics.

Notes

¹ The market supply and demand curves were derived from the reservation prices distributed to students/subjects.

² See also Simon (1992 [1966]: 69, emphasis in original): “Heuristics that proceed on this basis [searching for a good-enough solution by a certain criterion] are sometimes called ‘satisficing’ heuristics. Suppose, for example, that someone wishes to sell a house. He does not know exactly how much he can get for it, but he can make an estimate. If bids come in close to this figure, he holds to it; if they do not, he gradually reduces it. When a bid comes in that meets the revised figure, he accepts it. The criterion of ‘good enough’ that adjusts in this way is called an *aspiration level*. Satisficing behavior that makes use of aspiration levels is prominent in the selection of chess moves...”

³ See Murphy (1949: chap. 21); Boring (1950: 723-8); Patnoe (1988); Danziger (1990: 173-8; 1992; 2000); Farr (1996: chaps. 6, 8, 9); Korn (1997: chaps. 4-6); Capshew (1999: 189-93, 225-6); Greenwood (2004: chaps. 8, 9). His best-known biography is Alfred Marrow’s (1969).

⁴ Here I stick to the notations used by Lewin et al. (1944).

⁵ ‘Valence’ could safely be interpreted as ‘utility.’

⁶ $P(x)$ stands for the probability of the occurrence of x ; $u(y)$, the utility of y .

⁷ Siegel (1957: 261) used ‘LA’ instead of ‘LOA.’ For the sake of consistency, I substituted ‘LOA’ for ‘LA.’

⁸ See the previous note.

⁹ Smith left for Brown University in 1967; Starbuck for Cornell University in the same year.

¹⁰ See Smith's letter to Richard C. Snyder [Dean, Graduate School of Administration, University of California, Irvine], (January 18, 1966), [ff: 1965-1966 Outgoing Mail, Box 14, Accession 96-106, Vernon L. Smith Papers, Rare Book, Manuscript, and Special Collections Library, Duke University (VSPD)], and his letter to James Deese [Department of Psychology, Johns Hopkins University], (April 4, 1967), [ff: 1966-1967 Outgoing Mail, Box 14, Accession 96-106, VSPD]).

¹¹ A version of this paper dated January 25, 1959 is available in Herbert Simon Collection at <http://diva.library.cmu.edu/Simon/>. The title page of this paper was marked, probably by Simon, "BTOF W.P. #7" (handwriting)—Behavioral Theory Of the Firm (Project) Working Paper #7.

¹² The page number refers to that in the working-paper version of Starbuck (1963b) mentioned in the previous note. In the ensuing paragraph, Starbuck argued: "What is sought in the level of aspiration concept is a mechanism for simplifying the organism-environment relation. ... The level of aspiration concept implies that the individual uses a very simple frame of reference with one dominant reference point. All past experience is summarized in the level of aspiration; all new experiences have immediate impact on the level of aspiration and then are forgotten" (4).

¹³ In detail, Starbuck (1963b: 130-1) deployed four inequalities to specify the LOAs for a buyer; two inequalities, the stability condition for a buyer; three inequalities, the LOAs for a seller; and three inequalities, the stability condition for a seller.

¹⁴ Here, Starbuck referred to the eighth editions of Alfred Marshall's *Principles of Economics*, Milton Friedman's "Notes on Lectures in Price Theory" (1951, mimeo), Robert Clower's "Keynes and Classics: A Dynamical Perspective"

(*Quarterly Journal of Economics*, 74: 318-23) and, of course, Smith's landmark paper in experimental economics (1962).

¹⁵ "The author is indebted to Richard M. Cyert, Charles W. Howe, James G. March, Jacob Marschak, and Vernon L. Smith for comments and suggestions" (Starbuck 1963b: 128, n. 1).

¹⁶ See his "Oligopoly Bargaining: The Quantity Adjuster Models" with Lawrence Fouraker and Sidney Siegel [Box 1, Accession 96-116, Martin Shubik Papers, Rare Book, Manuscript, and Special Collections Library, Duke University].

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Table 1. The First Decision-Making Situation

	<i>1</i>	<i>2</i>
Head	A	B
Tail	C	B

SOURCE: Siegel (1957, p. 256, slightly modified by the author)

Table 2. The Second Decision-Making Situation

	<i>3</i>	<i>4</i>
Head	B	C
Tail	D	C

SOURCE: Siegel (1957, p. 257, slightly modified by the author)