



The arrival of behavioral economics: from Michigan, or the Carnegie School in the 1950s and the early 1960s?[☆]

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Abstract

The essay discusses the rise of (modern) behavioral economics during the last few decades. In contrast to Louis Uchitell's assertion, in his Feb. 11, 2001, NY Times essay, that behavioral economics began in 1994, I would try to argue that it began during the 1950s and early 1960s, although some aspects of it had even emerged in the works of Marshall, Wesley Mitchell, J.M. Clark and others, before WWII.

Although contributions of many writers have helped the rise of behavioral economics including psychologists Kahneman and Tversky, I regard the works of George Katona and Herbert Simon instrumental in its rise. While the works of Katona and his colleagues at Michigan University led to the use of survey method in economics and its utilization in measuring the impact of consumer expectations on macroeconomic activity, the work of Simon at Carnegie Tech. (a tremendously stimulating intellectual environment for economic theorizing then) resulted in the important theoretical foundations of behavioral economics, such as the concept of bounded rationality. Interestingly enough, that stimulating environment also led to the many contributions of Franco Modigliani, M. Miller, and others, and the start of the rational expectations hypothesis by John Muth—a student of Simon and Modigliani.

The essay also provides the characteristics of behavioral economics, which include the utilization of the theoretical findings of psychology and other social sciences; its concentration on real observed behavior of economic agents; its rejection of the simplistic model of rational maximizing agents and its replacement with Simon's bounded rationality; and its emphasis on the utilization of an interdisciplinary approach in economics.

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1. Introduction—the New York Times acknowledges the arrival of behavioral economics

“In the histories of Economics still to be written, the Spring of 1994 will almost certainly be flagged as momentous,” writes Louis Uchitelle of the *New York Times* (February 11, 2001, section 3, p. 1). For, he proclaims, “Behavioral economics had finally arrived.” (Ibid.).

Louis Uchitelle’s essay, entitled “Following the Money, But Also the Mind: Some Economists Call Behavior a Key,” in addition to proclaiming the rise of behavioral economics in American universities, also provides examples of the kinds of questions asked and the types of research conducted by behavioral economists, and introduces some of the pioneers of this approach to economics (albeit in a non-systematic and imprecise way). During that “momentous” Spring, he observes, the Department of Economics at Harvard University had decided to hire a behavioral economist (i.e. David Laibson), whose Ph.D. dissertation “drew as much on psychology and quirky behavior as on standard economics.” (Ibid.). This decision by Harvard was followed by Economics Departments at MIT and other leading Ph.D. granting institutions. Among the arguments made by Louis Uchitelle is that students too have become interested in behavioral economics. As he reports, a great number of Ph.D. students in economics at Harvard, MIT, Yale, Stanford, Chicago, Princeton, Berkeley, and other prestigious American universities can now be considered behavioralist, since they apply psychological principles to the study of economics. According to Uchitelle, close to 20% of the graduate students in economics are now behavioralist. The significance of this percentage would be appreciated more once we realize that this is up from almost zero in 1990, when the behavioralist economist hired by Harvard in 1994 had begun his graduate studies at MIT. Furthermore, he maintains, the behavioral economists’ reputations have been on the rise ever since (section 3, p. 11).

Uchitelle correctly observes that behavioral economists are not and should not be viewed as rebels; the ranks of behavioral economists include various established economists. An example is Richard Thaler, “now 55, the University of Chicago economist who teamed up with two psychologists in the 1970s; out of their collaboration came the early findings of behavioral economics.” (Ibid.). The essay observes that behavioral economics combines economic reasoning with more than psychological principles. For, “Lately, a few behaviorists have ventured beyond psychology, wading into sociology and anthropology to help explain the dynamics of the economy.” (Ibid.).

The rise of behavioral economics is more complicated than what is suggested by the February 2001 essay in the *New York Times*. (For example, the Spring of 1994 might be viewed as much less significant in the history of behavioral economics than the 1950s or the year 1984, as I will demonstrate below.) To demonstrate this complex development, an attempt will be made to define behavioral economics and explain its historical evolution. I will explore the reasons behind its evolution, and the contributions of its pioneers. George Akerlof, Richard Thaler, and Robert Frank, names of economists suggested in the *New York Times* essay, have made substantial contributions to the development of behavioral economics, and can be viewed as pioneers. However, the rise of modern (i.e. post-1950s) behavioral economics has benefited from the works of various writers, in particular two giants of behavioral economics (who are not mentioned in the *New York Times* essay): George Katona and Herbert Simon, whose roles in the emergence of this body of thought

were instrumental. I will demonstrate that aspects of behavioral economics existed before the 1950s. The efforts of Simon and Katona were instrumental in the rise of behavioral economics as an identifiable body of economic thought. In discussing the contributions of George Katona, the efforts of his colleagues at the Michigan University's Institute for Social Science Research should be acknowledged. For example, according to Simon's third Mattioli Lecture, Katona and those colleagues were the first economists to use the techniques for gathering empirical data often called "survey methods" (Simon, 1997, p. 88). Those methods, instead of being experimental in structure, are aimed at measurement of important parameters that appear in economic models, and, in the words of Simon, "were particularly focused on expectations and intentions: the estimates that economic actors themselves made about future events." (Ibid.). In discussing the contributions of Simon, I explore the contributions of his colleagues at Carnegie Tech. (now Carnegie-Mellon University) during the 1950s and the early 1960s, and the productive intellectual climate of that institution that gave rise to various debates relevant to behavioral economics. It was this productive intellectual climate, and the preoccupation with the theme of rationality, that was conducive to the rise of Simon's notion of bounded rationality, the start of rational expectation hypothesis by John Muth, and the related contributions of Franco Modigliani and other writers at Carnegie Mellon University. The simultaneous presence of these writers and other productive colleagues, their joint authorship (with Charles Holt) of *Planning Production, Inventories and Work Force* (Holt et al., 1960), and the focus of the faculty on a common theme, contributed to that remarkable intellectual climate, and very productive results. In the words of Sheffrin, "Although the development of the diverse doctrines of bounded rationality and rational expectations by collaborators could be viewed as just a historical coincidence, it is more likely that an intense preoccupation with a set of problems led to two researchers on different paths in search of a solution" (Sheffrin, 1996, p. 1). Interestingly enough, not only Simon, Modigliani, and Muth, but also the populizers of rational expectations, i.e. Lucas, Sargent, and Rapping, were all at Carnegie-Mellon University at some point during the 1960s (Sent, 1997, p. 326). Sargent too eventually embraced the notion of bounded rationality, although for a different purpose and with a different interpretation than that of Simon.

Emphasizing the contributions of Katona, Simon, and their coworkers at Michigan and Carnegie in the rise of modern behavioral economics does not imply that I do not view the contributions of others as significant. The works of the many economists indicated above, and those of psychologists Kahneman and Tversky, have been instrumental in bringing behavioral economics to the attention of the economics profession. (Although behavioral economics had existed before 1994, to a large extent it had been ignored by the economic profession.) The works of psychologists Daniel Kahneman and Amos Tversky were particularly helpful in bringing behavioral economics to the attention of economists. Their 1973 essay pioneered psychological experiments which proved useful for the testing of the postulate of rationality in economics (Daniel and Tversky, 1973).

2. What is behavioral economics?

Behavioral economics is seen by its advocates as a reaction to the deficiencies of conventional economics. In his Preface to the *Handbook of Behavioral Economics* (edited by Gilad

and Kaish), Herbert Simon maintains that “We need to augment and amend the existing body of classical and neoclassical economic theory to achieve a more realistic picture of economic process” (1986, p. xvi). For Simon, economists, as social scientists, must be prepared to name the key attributes of human actors (Simon, 1985, p. 303). “Behavioral economics is the name we give to the research enterprise that seeks to meet these needs,” states Simon (1986, p. xvi).

Gilad, Kaish, and Loeb credit George Katona for forging the term behavioral economics, to imply the introduction of insights drawn from social psychology into (macro) economic behavior. (1984, p. 1). In his 1980 book, Katona summarizes modern developments in behavioral economics as follows: “During the past three decades numerous empirical studies of economic behavior have been carried out and their theoretical foundation has been clarified. There was a rapid development and articulation of data, theory, and methodology. A new discipline of behavioral economics was emerging.” (1980, p. 3). To Katona, the starting point of behavioral economics “consists of the empirical investigations of the behavior of businessmen and consumers in one country in one time. Generalizations about economic behavior emerge gradually by comparing behavior observed under different circumstances.” (Ibid.). Psychology is an important component of behavioral economics for, “behavioral economics measures and analyzes such psychological antecedents of economic activities as the motives, attitudes, and expectations that influence decisions in economic matters.” (Ibid.). The importance of psychology to economics is acknowledged by various advocates of modern behavioral economics including Simon who acknowledges the contributions of psychologists Kahneman and Tversky (1979, p. 506). The contributions of these two is acknowledged in the *Handbook of Behavioral Economics* (p. xviii).

As the above indicates, behavioral economics differs greatly from the procedures of conventional economics, “which deduces principles of economic behavior from features of human nature assumed to be valid at all times and in all cultures.” (Yang and Lester, 1995, p. 436). Behavioral economics is also different from econometric studies, which commonly begin by setting up general models and then testing deviations from the models by confronting them with statistical data. Simon’s third Mattioli Lecture paid a great deal of attention to the inadequacies of econometrics. Among the points he made was that: “In few cases of interest do econometric techniques enable us to specify reliably the structural equations of the system we are studying. If we are to arrive at such specifications, we must find additional sources of data that can give us realistic priors.” (1997, p. 74).

Gilad, Kaish, and Loeb, in their 1984 *Journal of Behavioral Economics* essay, summarizing the stated views of the participants of the 1984 SABE (i.e. Society for the Advancement of Behavioral Economics) conference, define behavioral economics in terms of (at least) four objections to mainstream economics. These include: (1) a rejection of positivism as the methodological foundation for economic research, (2) a refusal to accept the use of deductive reasoning as a sufficient basis for a (social) science, and (3) a marked dislike of static analysis of equilibrium outcomes rather than disequilibrium processes. But their most important criticism of the mainstream theory is (4) an objection to the simplistic economic model of rational agents exhibiting optimizing behavior (1984, p. 1). As an alternative to the notion of rationality as optimization, Herbert Simon introduced (and coined the term) bounded rationality during the 1950s. Since the 1950s, various interpretations of the concept have emerged. In the words of Gigerenzer and Selton: “bounded rationality has

become a fashionable label for every model of human behavior.” (2001, p. 4). We may find other objectives emphasized by behavioral economists. Many (if not most or all) behavioral economists view the interdisciplinary approach to the study of economics as necessary. Explaining the behaviorist approach of the Carnegie School of the 1950s and the 1960s, Oliver Williamson (a graduate student there) sees the need for an interdisciplinary approach by arguing that: “Even if economics is the queen of social sciences (opinions differ), no single social science is able by itself, to explain the range of complex organizational phenomena with which Carnegie was concerned.” (2000, p. 7). The following provide other features of behavioral economics. Many writers (Simon, 1955, 1979, 1959; Kahneman and Tversky, 1982, 1984; Nelson and Winter, 1982; Heiner, 1983) have utilized substitutes for conscious utility maximization, such as satisficing, heuristics, and routines. (Gilad and Kaish, 1986, p. xniii). Leibenstein (1976) and his followers have sought the difference between optimizing behavior that individual members of an organization may exhibit for their own good and the less than optimal decisions this causes for the organization (i.e. economic unit) they belong to. Akerlof and Dickens (1982), Gilad, Kaish and Loeb, Cohen and Axelrod (1984) and others, while accepting the (conventional) utility maximizing assumption, advocate a behaviorally modified objective function that reflects dissonance and framing biases found in the laboratory (Gilad and Kaish, 1986, p. xviii). As stated by Gilad and Kaish, all behavioral economists agree that: “The neoclassical model of perfect information availability, optimal information processing, and the utility maximization that results is in severe need of overhaul.” (Gilad et al., 1984, p. 3).

Gilad, Kaish, and Loeb argue that behavioral economics is not a field in economics as much as it is: “a way of looking at the traditional fields in economics.” (Ibid.). This explains why proponents of behavioral economics have applied it to various fields in economics. Some economists at Michigan have engaged in behavioral macroeconomics; the works of Harvey Leibenstein, John Tomer, Randall Filer (and some of this author’s) can be classified as behavioral microeconomics; various individuals have proposed a behavioral theory of the firm (such as Cyert and March, 1963; Nelson and Winter, 1982); and the works of some can be classified as behavioral labor economics, others as behavioral public finance; and this author has applied behavioral economics to the study of foreign direct investment (Hosseini, 1994, 2002) and economic development (Hosseini, 2001). Several writers (like Shleifer, 2000, and others) have engaged in behavioral finance. In their studies of how investors interpret and act on information to make investment decisions, these writers have argued that investors do not always behave rationally and in predictable and unbiased manner as assumed in traditional financial studies. To them, investors make mistakes and, like money managers and financial advisors, often are overconfident in their ability to outperform the market.

The authors of the *Handbook of Behavioral Economics* prefer to call behavioral economics “an approach in doing economic research.” (1986, p. xix). These authors propose the following three postulates in assessing what behavioral economics is.

1. Following Herbert Simon (1978, 1979), they argue that economic theory must be consistent with the accumulated body of knowledge in the behavioral disciplines, including psychology, sociology, anthropology, organization theory, and decision sciences. This requirement is at the root of the behavioral economic studies attempting to improve the

assumptive realism of economic theory (Gilad and Kaish, 1986, p. xix). (Of course, as suggested by Gigerenzer and Selton, “The lack of information flow between disciplines can hardly be understated.” 2001, p. 10).

2. Economic theory should concentrate on and be able to explain real observed behavior. “This shift in emphasis to what actually happens rather than the logical conditions necessary for things to happen unites behavioral economists in a quest for a stronger descriptive base to economics. The survey-based research of Katona (1980) and his successors is a manifestation of this postulate.” (Gilad and Kaish, p. xix).
3. As emphasized by Gilad and Kaish, economic theory should be empirically verifiable with field, laboratory, survey, and other microdata-generating techniques being acceptable means of verification. The recent rise in popularity of experimental economics is certainly consistent with the “behaviorification” of economics (Ibid.).

3. Did history begin in 1994?

According to the February 2001 *New York Times* essay by Louis Uchitelle, behavioral economics arrived in 1994, when Harvard University’s Department of Economics hired David Laibson. However, Carnegie and Michigan had already witnessed the arrival of modern behavioral economics during the 1950s and the 1960s, and Harvard’s Economics Department was not unfamiliar with (modern) behavioral economics. Harvard’s Harvey Leibenstein had, prior to 1994, utilized behavioral economics ideas in several of his published works: including those about X-efficiency (Leibenstein, 1966, 1976, 1978, 1980, 1983), in his *Beyond Economic Man: A New Foundation for Microeconomics* in 1976, and in his essay “On Relaxing The Maximization Postulate.” Neither was the economic profession unaware of the behavioral approach to economics. Simon’s *Models of Man* (Simon, 1951) and Katona’s *Psychological Analysis of Economic Behavior* (Katona, 1951) both appeared in 1951. Although behavioral economics was, for the most part, ignored by mainstream economists and economic journals, it was not totally dismissed. This explains why Simon’s works, for example, were published by leading mainstream journals, why he was invited, in 1978, to present the prestigious Richard T. Ely Lecture before the American Economic Association, why he was awarded the Nobel Prize in economics in 1978, and why, as Oliver Williamson argues, there is “a growing appreciation for the need to find ways to model bounded rationality.” (p. 2). Attempts by authors such as Ariel Rubinstein (1998) to model bounded rationality are indicative of this fact (See his 1998 book). (It seems to me that mainstream economic journals only published heterodox papers which adhered to more formal methods of presentation.) Although Bruce Caldwell too acknowledges the double standard used (for publishing) by mainstream economics journals, he writes “This is not to say that the work of men like Simon, Scitovsky, Cyert, March, Leibenstein, Katona, Nelson, Winter, and Williamson were unnoticed by the profession: unlike traditional heterodoxy, these people eventually got published. But for the most part, these researchers were mentioned only in passing in undergraduate texts . . .” (in the Handbook of B. E., p. 10).

Before 1994 (but after World War II), the economic profession had witnessed other research/publication by advocates of behavioral economics. Kaish’s doctoral dissertation

“Levels of Aspirations and the Theory of Economic Behavior” was defended in 1964, and Kahneman and Tversky’s essay on the “Psychology of the Prediction,” which demonstrated that human beings are poor Bayesians, was published in 1973. Other significant contributions were Akerlof and Dicken’s “The Economic Consequence of Cognitive Dissonance” (*AER*), and Kahneman and Tversky’s “The Psychology of Preferences” which appeared in 1982, Ronald Heiner’s “The Origins of Predictive Behavior” (*AER*) was published in 1983, Cohen and Axelrod’s “Copying with Complexity: The Adaptive Value of Changing Utility” (*AER*) in 1984, Gilad and Kaish’s “Cognitive Dissonance and Utility Maximization” in 1987, Warren Gramm’s “Rise and Decline of the Maximization Principle” in 1988, to name but a few. Furthermore, many years before 1994, various journals committed to a behavioral approach to economics had emerged. Among these, we can include the *Journal of Behavioral Economics* (now called *Journal of Socio-Economics*), the *Journal of Economic Behavior and Organization*, and the *Journal of Economic Psychology*. These three journals deal with different aspects of behavioral economics, and have different approaches. The *Journal of Socio-Economics* deals with broadly defined behavioral economics, is most open to alternative theoretical approaches to behavioral economics, and encourages papers in behavioral economics that have social implications. The *Journal of Economic Behavior and Organization* focuses on both behavioral and organizational questions, is more technical, and puts more emphasis on the use of mathematics and formal proofs. The *Journal of Economic Psychology*’s focus is economic psychology, and from the perspective of both the economic psychologists and economists. In practical terms, this journal publishes papers that deal with both experiments and surveys in economic psychology.

Modern (i.e. post-1950s) behavioral economics can be viewed as a reaction to the oversimplification of the economic process that occurred in the mid 20th century. Many writers argue that (at least aspects of) behavioralism had appeared in the works of pre-1950s writers: including Alfred Marshall, John Commons, Thorstein Veblen, Joseph Schumpeter, George Katona, and many other distinguished economists of past and recent generations. (Gilad and Kaish, 1986, p. xvi). Marshall’s behavioralism is obvious in the following (Marshall, 1885) statement, “They regarded man as, so to speak, a constant quantity and gave themselves little trouble to study his variations. They took for granted that other Englishmen were very much like those in the city. They were unaware that inhabitants of other nations had peculiarities of their own; but they regarded such differences, when they thought of them at all, as superficial and sure to be removed as soon as other nations had got to know that better way which Englishmen were ready to teach them. The same bent of mind, that led our lawyers to impose English civil laws on Hindoos, led our economists to work out their theories on the tacit supposition that the world was made up by city men.” (in Pigou, Ed., 1966; quoted by Hosseini, 1991).

Gilad and Kaish maintain that Adam Smith used psychology in his studies of the economy, having a broad view of the economy absent in modern conventional economics (1986, p. xvii). However, “from Ricardo on, the mainstream has gradually moved away from Smith’s broad view of the full human experience to its present ascetic state where the bare bones of rationalism dominate and very little human flesh is to be seen covering them.” (Ibid.). (Of course, members of the German historical school too questioned the universality of economic science, as did the likes of Veblen, Mitchell, J.M. Clark and other writers.) To Gilad and Kaish, during the past one hundred years, the likes of

institutionalists Veblen, Mitchell, and J. M. Clark, although called iconoclast (Veblen), accused of empiricism without theory (Mitchell), and better known for his work on the accelerator theory than behavioralism (J.M. Clark), “have been warning other economists that they had better make provision to include psychology in their field or it would be in trouble.” (Ibid.). French social psychologist Gabriel Tarde seems to be the first writer to use the term economic psychology, as suggested by psychologist Stephan Lea (in *The International Encyclopedia of the Social Sciences*). *The Journal of Economic Psychology*, in its (1992, p. 192) Editorial, traces the roots of economic psychology, which is viewed as behavioral economics or at least a branch of it, to Gabriel Tarde in 1881 when he attempted to apply psychological methods to economic behavior. It is no wonder that Tarde entitled his 1902 two volume book, *Economic Psychology*. Many of his arguments in this book (about leisure, etc.) influenced American and British opponents of conventional economics (Tarde, 1902).

Wesley Michell and J. M. Clark went further than simply talking about the inclusion of psychology in economic analysis by criticizing the narrow confines of conventional economics. Wesley Mitchell was critical of the theorizing done by conventional economists of his time. In a 1929 essay, Mitchell wrote: “Economic theory of the speculative kind is as cheap and easy to produce as high mathematics or poetry—provided one has the gift. And it has the problematical relation to reality as do these products of imagination.” (In Landreth and Colander, 1994, p. 342).

Moreover, Mitchell (1914) differed significantly from orthodox neoclassical economics on the issues which concern empirical work. Like Veblen, he was critical of the hedonistic psychological assumptions of orthodox economic theory, although he did not accept Veblen’s instinct theories. To Mitchell, economics, as a social science, could develop a better explanation of the activities of humans by basing it upon empirically grounded psychology. Wesley Mitchell expressed his dissatisfaction with the separation of economics from psychology, i.e. deploring of what he called economics’ nonintercourse with psychology, in his 1914 *Quarterly Journal of Economics* essay “Human Behavior and Economics.” Clark (1918) seems to agree with Mitchell; that unhappiness is discussed in his 1918 *Journal of Political Economy* paper: “The economist may attempt to ignore psychology, but it is sheer impossibility for him to ignore human nature If the economist borrows his conception of man from the psychologist, his constructive work may have some chance of remaining purely economic in character, But if he does not, he will not thereby avoid psychology. Rather, he will force himself to make his own, and it will be bad psychology.” (vol. 26, p. 4).

4. George Katona and the rise of behavioral economics

Some of the more psychologically inclined economists, as well as those emphasizing consumer behavior, view George Katona as the father of “modern” behavioral economics. In his 1980 work, Katona made the claim that: “A new discipline of behavioral economics was emerging.” (p. 3). His own research was instrumental in the development of this ‘new discipline.’ Katona’s contributions to the start of behavioral economics are obvious in his 1980 *Essays on Behavioral Economics*, as they are in his other publications published

prior to 1994. Years before 1994, George Katona discussed the importance, in economic theorizing, of “the empirical investigations of the behavior of businessmen and consumers” as human actors, and thus the importance of psychology to economics. This, is clear in his 1951 book *Psychological Analysis of Economic Behavior*. Criticizing the economic profession for neglecting the behavioral foundations of economics, Katona writes: “This book is based on the thesis that economic processes stem directly from human behavior and that this simple but important fact has not received its due in modern economic analysis. This author has set for himself the task of describing a psychological approach to economic analysis and the current research in the field of economic behavior.” (Preface, p. 111). Katona’s above-mentioned publications are sufficient to merit him a significant position among the pioneers of modern behavioral economics. To the authors of the essays in Honor of George Katona (Strumpel et al., 1972), it was the efforts of George Katona at the Survey Research Center that started this new body of thought. Rensis Likert recalls his conversation during the 1948 convention of the American Economic and Statistical Associations with a “competent” (conventional) economist about Katona and the Survey Research Center which reflects the “recognition of the initial phase of George Katona’s pioneering work in the development of a new body of knowledge bridging a gap between economics and psychology.” (Strumpel et al., 1972, p. 3). To Likert, “George Katona has had a major role in creating a new field of knowledge: psychological or behavioral economics.” (Ibid., p. 8). According to Likert, Katona’s contributions began when he became involved in the consumer studies done for the Board of Governors of the Federal Reserve during the mid-1940s (Likert, 1972, pp. 3–8). Before this study, the prevailing view among economists was that, as far as the macroeconomy and its major changes were concerned, “consumers were felt to have no influence, since their rate of expenditures was determined by their income, which in turn was controlled by the decisions of business and government.” (Ibid., pp. 3–4). Toward the end of World War II, Rolf Nugent and some fellow economists became concerned about the possible impact of consumer behavior in the immediate post-war period: if consumers decided at the end of the war to buy consumer goods, using the billions of dollars of cash, Series E Bonds, and the time and demand deposit they owned, there would be a serious inflation, since the supply of consumer goods would be limited. Nugent urged the University of Michigan’s Division of Program Surveys to expand their War Bonds studies by interviewing samples of consumers in order to assess the amount they intended to spend at the close of the war. An advisor to the Division of Program Surveys, Princeton University economist Walter Steward urged that study and suggested the Fed’s Board as the most likely source of support, since the Fed was very concerned about inflation.

The proposal had some supporters among the various Board members, but faced opposition among the Research Staff of the Board. The Fed approved the pilot study in 1944, to be undertaken in 1945, with George Katona as one of its directors. The first nationwide survey of the ownership of liquid assets by consumer was conducted by Katona in 1946. The Research Staff, who opposed the collection of data by measuring consumer intentions because they felt they could derive more accurate data from the distribution of the ownership of liquid assets, were finally convinced by George Katona. In fact, Katona’s efforts at the Survey Research Center, which was assisted by his colleagues, led to his conclusion in 1949 that, contrary to general expectations, the USA was not moving toward a recession, since consumers were planning to spend about as much in 1949 for consumer durables as in 1948

(Ibid., p. 6). According to Likert, “when Katona started his nationwide consumer surveys, the prevailing view was that the rate of consumer expenditures was not an independent factor affecting the level of economic activity. Increased or decreased rates of expenditures by business or government were viewed as the factors which determined whether we had good times or bad times. Consumers were felt to have little independent influence.” (Ibid., p. 8). It was Katona who insisted that consumers are important as an independent factor affecting the United States economy and its level of activity. “The data he collected, and the analysis that he and his colleagues made, gradually demonstrated even to the most skeptical that consumer perceptions, expectations, and motivations can exercise a significant, independent impact upon our economy.” (Ibid.).

James Morgan, a colleague of Katona at the University of Michigan, acknowledges the pioneering role of Katona at the Survey Research Center (Morgan, 1972, p. 15). To James Morgan, what gave coherence and stability to the research at that (Katona-led) center was a focus on: an interdisciplinary approach to the study of economics; the use of personal interviews with a national sample as the basic data sources; a focus on salient, important economic decisions where some genuine decision making is likely to be taking place, and where mass shifts in the decisions can have major social impact; repeated studies using sufficiently stable methodology so that changes and trends could be discovered; where possible, re-interviews to study stability and changes in individual household behavior; and, when possible, a focus on mass changes, rather than cross-section differences (Ibid., pp. 16–17).

Nobel Laureate James Tobin gives a great deal of credit to George Katona. According to Tobin, “economists concerned with consumer and saving behavior owe Katona a debt for insights and ideas as well as for data,” and that “Katona is right that once consumption is not liquidity constrained, it is a highly psychological variable. Wealth itself is highly psychological . . . when Katona measures confidence, he is also measuring an important dimension of wealth.” (1972, pp. 37–38).

Burkhard Strumpel viewed Katona as the father of behavioral economics: “Behavioral economics, a new brand of empirically-oriented social science first developed by George Katona, . . . to explain changes in the economic system by analyzing actions and predispositions to actions on the individual level. Katona established the connection link between individuals and the system, between the micro-and the macro-level of economic analysis.” (1972, p. 83).

The 1982 paper by Gilad and Kaish recognizes three categories of research in behavioral economics: methodology (defined as the transfer of psychological methodology to the study of economics), counterpart analysis (i.e. the inclusion of psychological variables in the statistical analysis of economic data), and theoretical (defined as the bringing of psychological theory to bear directly on the traditional postulates and assumptions of economic theory) (Gilad and Kaish, 1982). The third (i.e. the theoretical) category is viewed to be more significant than the first two; in the theoretical realm, psychology is used to question the basic axioms of economic theory (the winter 1982 issue of the *Journal of Behavioral Economics*). To the authors, Katona’s contributions were more in the methodological and counterpart analysis fronts, making only limited contributions in the theoretical category. According to Gilad and Kaish, while Katona criticizes economic generalizations, he offers no new theory and mainly points to the limitations of simplified methods in

predicting complex phenomenon (Ibid.). These two historians of behavioral economics view Simon's contributions as more significant (i.e. more in the realm of what they call theoretical).

5. Behavioral economics, Simon, and the Carnegie School during the 1950s and early 1960s

Herbert Simon, having made significant contributions, is also viewed as a giant in the realm of behavioral economics. Gilad and Kaish regard his contributions to behavioral economics as relatively more significant because he uses psychology to question the basic axioms of economic theory, and proposes an alternative to what he critiques (Ibid.). Using psychology, Simon questions the viability of the rationality assumption in conventional economics, introduces bounded rationality, and replaces the maximization assumption of conventional economics with satisficing. Simon's contributions are significant because he has been concerned with the development of a model of man, to be cast against the more traditional model of economic man (person), and has offered a viable strategy of research aimed at replacing the traditional maximizing calculus (Ibid.). To Gilad and Kaish, Simon's theoretical arguments are applicable to a broad range of economic issues and are as general as, but more realistic than, traditional principles in economics. Simon's views of what economic theory ought to be is best represented by what he wrote not too long before his death, in his Raffaele Mattioli Lectures (#3). To him, for economic theory to guide the operation of the economy, it: "requires building an adequate, empirically based, theory of bounded rationality The knowledge that economic actors possess and do not possess, the computations that economic actors can make and cannot make must not enter as ad hoc assumptions . . . but must be shaped and tested by the sharpest empirical methods that we can devise." (1997, p. 63). Obviously, Simon's contributions had begun much earlier. As early as 1947, Simon subscribed to the proposition that human behavior is intendedly rational but only limitedly so (Simon, 1947, p. xxiv). In his *Model of Man*, Simon spoke of replacing the goal of maximizing with that of satisficing as an essential step toward the principle of bounded rationality (1957, p. 204–205). This view had been discussed in his 1955 *Q.J.E.* article Behavioral Model of Man, and also appeared in the 1958 book *Organizations* by March and Simon (1958), leading to the 1963 book by March and Richard Cyert: *A Behavioral Theory of The Firm*.

Simon's contributions, and those of his coworkers, began to appear at Carnegie during the 1950s. During that period, Carnegie Mellon University, then called Carnegie Institute of Technology, had given rise to an intellectually stimulating environment for economic theorizing. The Graduate School of Industrial Administration (GSIA) at Carnegie had attracted an extraordinary group of economists—Herbert Simon, Franco Modigliani, Merton Miller, James March, Richard Cyert, Emile Grumberg, Charles Holt, Hohn, Robert Lucas, Thomas Sargent, and many more. According to Oliver Williamson, a student of these writers, "Of the many distinguished faculty at Carnegie, none cast a larger shadow than Herbert Simon. Everything seemed to be within his purview . . ." (Ibid., p. 1). Carnegie attracted many brilliant graduate students- the likes of John Muth and Oliver Williamson, some of whom, like John Muth, joined the faculty of Carnegie for several years.

The intellectual climate at Carnegie during those years made the GSIA a truly productive research environment. It is no wonder that Nobel prizes in Economic Science were awarded to four of the above-mentioned economists: [Herbert Simon \(1978\)](#), [Franco Modigliani \(1985\)](#), [Merton Miller \(1990\)](#), and [Robert Lucas \(1985\)](#), for the work they started while at Carnegie. ([Williamson, 2000](#), footnote #2). And, that some of its graduate students—the likes of [John Muth](#) (who, as some believe, at least should have shared the 1995 prize with Lucas) and [Oliver Williamson](#) (a likely future Nobel recipient) went to make some of the greatest contributions to economic theory, from the work they started while at Carnegie.

The theoretically fertile atmosphere of Carnegie Tech. during the 1950s and the early 1960s gave rise to the works—in behavioral economics—of Simon, [Richard Cyert](#), and [James March](#), and of the beginning of [Williamson's Transaction Cost Economics](#) (which uses Simon's bounded rationality). Carnegie Mellon University was the birthplace of much of the contributions of [Franco Modigliani](#) and [Miller](#). Of particular interest and significance is the simultaneous emergence of rational expectations and bounded rationality at that institution in its golden days. As stated in the Introduction, this simultaneous development should not be viewed as a historical coincidence, or an oddity. For, I find several interconnected reasons for the simultaneous development of these two different (but related) concepts. These are as follows:

1. The preoccupation of various writers at Carnegie Tech. with the general theme of rationality (and thus expectation formation) had to be helpful. I agree with [Esther-Mirjam Sent](#), [Sheffrin](#), and [Hartley](#) that the general theme of rationality was in the Carnegie air as early as 1954. In fact, the works of the professors of [Muth—Simon](#), [Modigliani](#), [Holt](#) and [Grunberg](#)—demonstrate this. In that environment, as correctly suggested by [Sent](#), [Muth's](#) rational expectations hypothesis thesis was regarded as an illustration of the rationality implicit in the contributions of [Grunburg](#), [Holt](#), [Modigliani](#), and [Simon \(2000, p. 17\)](#). On the basis of the arguments made by [Simon](#), [Sent](#) even tries to find a psychological connection between those theories: “[Muth](#) may have intended the rational expectations hypothesis as a subtle suggestion to [Grundbeg](#), [Holt](#), [Modigliani](#), and [Simon](#) that they themselves were rational expectationists, but just had not figured it out. Hence, for [Muth](#), the hypothesis may have just been part of a project to undermine the authority of his collaborators and his advisor.” ([Sent, 2000, p. 17](#)).

Psychological motives aside, the fact is that both themes reflected the preoccupation of various writers at Carnegie Mellon with the general theme of rationality. According to [Modigliani](#), even the [Lucas](#) version of REH is connected to that general Carnegie theme. “Before [Lucas](#) the model was incomplete because expectations were outside the model. Then with the help of my paper with [Emile Grunberg \(1954\)](#) he has formalized the notion of rational expectations. Remember that [Lucas](#) was originally at Carnegie Mellon University and his work was preceded by two other important contributions which came out of Carnegie Mellon.” ([Snowdon and Vane, 1994, p. 248](#)).

2. That preoccupation would not have emerged had it not been for the remarkable sense of collaboration at Carnegie during the 1950s and the early 1960s. For, it gave rise to many joint projects, papers, and monographs, all focusing on the same general theme. Examples include the now famous 1954 [Grunberg–Modigliani J.P.E.](#) paper “The Predictability of Social Events.” Or, the collaboration of [Simon](#), [Muth](#), [Modigliani](#), and [Holt](#)

in a study of production smoothing that eventuated in their jointly written book on the subject. (Young and Darity, 2001, p. 782). In that project, “Muth, then a graduate student was assigned the task of looking at the problems of production of future sales.” (Young and Darity, quoting Simon, p. 782). According to Muth, “I also participated as a graduate student in a project sponsored by the Office of Naval Research on aggregate scheduling. The faculty members involved in that were Charles Holt, Franco Modigliani, Herbert Simon, and initially Robert Schlaifer.” (quoted by Young and Darity, 2001, p. 789). James Hartley mentioned several other joint projects and publications involving various Carnegie faculty members. And according to Sent, Simon recalls that he (Simon), Modigliani, Holt, and Muth: “Worked closely and amicably together for several years.” (p. 13). Muth had mentioned that two of his landmark rational expectations papers were part of the “Planning and Control of Industrial Operations” project that involved Simon, Holt, Modigliani, and Muth (p. 13). Unfortunately, that sense of cooperation came to an end. Franco Modigliani left Carnegie for MIT in 1960, and Muth for Indiana in 1964. However, before Muth’s departure, the cooperative atmosphere at GSIA had ended; it slowly began to turn sour. It was this change of atmosphere, and the ensuing conflicts that caused Herbert Simon to move to the Department of Psychology. The conflict started when the coalition of neoclassical economists and operations research specialists (i.e. those emphasizing formalism more than the likes of Simon and fellow behavioralists) at the GSIA came to dominate the GSIA senior policy committee. According to Simon, it all began with (his former student) John Muth, i.e. it was really Muth that drove him away. (Sent, 1997, p. 324).

3. The issues raised by the 1954 Grunberg and Modigliani paper, and by Simon’s 1954 paper: “Bandwagon and Underdog Effects and the Possibility of Election Predictions,” too had relevance to rational expectations.

These two papers raised issues relevant to rational expectations. Modigliani was also interested in the rationality/expectations issue. According to James Hartley: “But, often overlooked is the fact that Modigliani was simultaneously carving out a third method of incorporating expectations into models.” (Hartley, 2000, p. 3). This is obvious in the 1954 Grunberg–Modigliani paper, as well as in the 1954 paper by Simon. The starting point of both papers was the claim that: “in reacting to the published prediction of future events, individuals influence the course of events and thereby falsify the prediction.” (Grunberg and Modigliani, 1954, p. 465). In both of these papers, public prediction is possible. Speaking of Grunberg and Modigliani, Hartley argues, “They reach this conclusion by noting that there is a fixed point at which the prediction of an event causes people to behave in a manner which induces the predicted outcome to be the same as the actual outcome.” (2000, p. 4). Hartley views the Grunberg–Modigliani paper as: “an interesting antecedent to Muth’s work on rational expectations.” (2000, p. 5).

Simon too comments on the role of his 1954 paper, and that of Grunberg and Modigliani, his role and their significance to rational expectations. He writes: “. . . a little earlier, Modigliani, together with Emile Grunberg, wrote their paper on the possibility of economic predictions (would they be falsified by reactions if they were published?), and I was closely involved in that project, providing (as indicated in a footnote) a proof of their central theorem using Brouwer’s fixed-point theorem. I did not co-author that paper, however, but published a parallel one on election predictions, based on the same

theorem. The fixed-point here is very closely related to rational expectations equilibrium, and some (e.g. Chipman) have thought that in these papers Grunberg, Modigliani, and I have laid the groundwork for REH. I will let history judge that, but if true, it is ironic.” (quoted by *Young and Darity, 2001*, p. 782). *Hand’s (1990)* paper, as suggested by Hartley, sees the Grunberg and Modigliani paper as a precursor of Muth’s rational expectations. This claim seems to be justified when one reads the following footnote in that paper: “The argument of this paper suggests that the agents’ reaction may create difficulties for the formulations and execution of policy.” As Hartley reminds us, “this footnote came 20 years before Sargent and Wallace (1976) showed exactly this result.” (p.5).

4. Muth being influenced by his professor/advisor Herbert Simon.

Muth admits that Simon, as his professor in several courses and as his advisor, had “very important general influence” on him (*Young and Darity, 2001*, p. 789). However, he does not admit to any direct influence. Given that Simon not only collaborated with Muth in various projects but also served as his professor and advisor, it comes as somewhat surprising that Muth does not admit a direct influence, and, specifically, did not refer to Simon’s related contributions on the possibility of making correct prediction. (Sent, p. 12). (We can make a similar point about Grunberg–Modigliani’s contributions.) This is particularly true since Muth has acknowledged that two of his landmark rational expectations papers were connected to the above-mentioned project that included Simon. (Sent, p. 13).

Given above, Muth’s concept of rational expectations had to be influenced by Simon (and Modigliani and Grunberg) in his views of expectation formation. This argument is made by Simon; it is also made by others. For example, in a December 1991 letter to Young and Darity, Simon wrote that John Muth “undoubtedly felt challenged by my views on bounded rationality.” (In *Young and Darity, 2001*, p. 782). In reference to the afore mentioned 1954 papers, Simon, in one of his papers, also states that: “In any event, those two papers had some influence on Jack’s thinking about forecasting. What clearly did have influence on his thinking was, without any challenge, his disagreement with bounded rationality, and his belief that people are more rational than Simon thinks they are.” (*Young and Darity, 2001*, p. 783).

Richard Cyert (Carnegie’s GSIA Dean at the time) makes a similar point. To him, at the time, Muth’s rational expectations hypothesis: “was viewed as an answer to discussions and arguments that we had on the faculty, stimulated by Herbert Simon’s attack on the concept of rationality in economics. Jack wrote the paper in a spirit of rebellion, Jack always being a rebel. He wanted to show that Herb was not only wrong, but that economists should emphasize rationality even more.” (in a 1991 letter to Young and Darity, in *Young and Darity, 2001*, p. 783).

In another December 1991 letter to Young and Darity, Marc Nerlove makes the same point: “The story I recall is that the REH originated at Carnegie in 1950s in the form of a dare: Herbert Simon challenged Muth to come up with a theory of information as rational as the theory economists use to explain the allocation of resources.” (*Young and Darity, 2001*, p. 783).

Muth disagrees with the exact account of this challenge. According to Muth, the assertion that “Herbert Simon challenged me to come up with a theory of information as rational as

the theory economists use to explain the allocation of other resources is definitely not true. There was never such a challenge. The only thing even remotely resembling that is when Franco Modigliani assigned a problem in class to explain executive salaries. Herb Simon presented a model to explain that phenomenon. As a member of Modigliani's class, I tried to develop one too, but it wasn't very good." (Ibid., p. 783).

Rational expectations hypothesis was not popularized by Muth, but by other economists connected to Carnegie's GSIA, in particular Robert Lucas and Thomas Sargent. However, in contrast to Lucas, Sargent is also associated with bounded rationality. Perhaps because of the difficulties that appeared during the 1970s and 1980s in his work on rational expectations (listed in Sent, 1997), rational expectations turned out to be a dead end for Sargent in many ways. As a result of these difficulties, Sargent turned to bounded rationality. This explains why "Sargent (1993) called his latest venture *Bounded Rationality in Macroeconomics* and tried to make connections to Simon's program of bounded rationality . . ." (Sent, 1997, p. 325). The result was a vastly different interpretation of bounded rationality. After all, Sargent's utilization of the concept of bounded rationality, in contrast with that of Simon's, was to strengthen neoclassical economics.

Gigerenzer and Selten, whose version of bounded rationality "is an elaboration of Simon's original concept," find Sargent's version of bounded rationality much more limited than the original version introduced by Simon. "Herbert Simon, who coined the term bounded rationality, used the metaphor of a pair of scissors, where one blade is the cognitive limitations of actual humans and the other the structure of the environment." (2001, p. 4).

Gigerenzer and Selten, echoing Simon's views, argue that: "Studying only one blade is not enough; it takes both for the scissors to cut." (Ibid.). Criticizing the improper use of bounded rationality, they write: "Bounded rationality is neither optimization nor irrationality. Nevertheless, a class of models known as optimization under constraints is referred to in the literature as "bounded rationality, and a class of empirical demonstrations of so called errors and fallacies in judgment and decision making has also been labeled bounded rationality. The fact that these two classes of models have little if anything in common reveals the distortion the concept of bounded rationality has suffered." (Ibid.). In several essays in the 2001 book (edited by these two authors,) Gigerenzer and Selten view Sargent's version of bounded rationality as an example of optimization under constraint (and thus not bounded rationality). *Rethinking Rationality*, (p. 5) by these two authors is one example (Ibid.). In another essay, "What is Bounded Rationality?", Selten writes: "The paper by AuMann and Sorin is a remarkable piece of work, but it is not a contribution to the theory of bounded rationality. The same must be said about the recent book on bounded rationality macroeconomics (Sargent, 1993). There, the assumption of rational expectation is replaced by least square learning, but otherwise an optimization approach is taken without any regard to cognitive bounds of rationality. Here, too, we see a highly interesting theoretical exercise that is, however, far from adequate as theory of boundedly rational behavior." (Ibid., p. 15). In another essay, "The Adaptive Toolbox," after praising Simon's contributions to bounded rationality, Gigerenzer writes: "Today, bounded rationality has become a diluted, fashionable term, used by the proponents of quite disparate visions of reasonableness: from optimization under constraints (e.g. Sargent 1993) to . . ." (Ibid., p. 37).

6. Concluding remarks

In their 1982 *Journal of Behavioral Economics* paper, Gilad and Kaish discuss, among other things, the difficulty behavioral or psychological economics faced in being accepted by the economics profession. Neither the surge of enthusiasm among some economists that followed the publication of Katona's *Psychological Analysis and Economic Behavior* and the establishment of the Survey Research Center in the 1950s, nor the 1970s enthusiasm among some economists that resulted from the initiation of a series of controlled laboratory experiments at Texas A and M University in studying the economic behavior of animals, was able to result in a change of the concept of economic man.

The pessimistic tone of that 1982 essay had completely disappeared in 1984, in another *Journal of Behavioral Economics* essay by Gilad, Kaish, and Loeb, entitled "From Economic Behavior to Behavioral Economics: The Behavioral Uprising in Economics." According to the authors, the many new developments in behavioral economics between 1982 and 1984, made the 1982 essay by Gilad and Kaish outdated. As reasons, they cited the birth of the Society for The Advancement of Behavioral Economics (SABE), its first annual conference in 1984, and the many important papers and contributions of that conference. To these behavioral economists, it was the winter of 1984 that should "be flagged as momentous" in the history of economics, and not the Spring of 1994 which was suggested by Louis Uchitelle's New York Times essay. While to Uchitelle that history began at Harvard in 1994 when a behavioral economist was hired, we can, however, argue that Harvard's Economics faculty members had not been totally unfamiliar with behavioral economics—Leibenstein had published works classified as behavioral economics as early as the 1966. In his 1985 (*Journal of Behavioral Economics*) essay, Leibenstein (1985) follows Herbert Simon in making a distinction between a substantive theory of rationality and a procedural one. Following Simon, he argues that real economic choices are procedural (and not necessarily substantive and optimal), for they involve intermediate steps in which particular procedures (i.e. human activities) are employed. These decision procedures will not usually lead to optimal decisions and choices, and they may be calculated or non-calculated procedures—faulty or incomplete ones. Leibenstein provides a list of non-calculating procedures (which lead to less than optimal decisions) that include: habit, emulating others, or ethical and moral imperatives, following rule of thumb, or standard procedures.

As suggested in previous sections, various authors made contributions to behavioral economics prior to 1994. Of course, the likes of George Akerlof, Robert Frank, Tversky, Kahaneman (authors mentioned in the New York Times essay) have made substantial contributions to behavioral economics. However, the works of Katona and in particular those of Simon and colleagues at Carnegie have been instrumental to the rise of behavioral economics in Post-WW II United States. I am surprised that these pioneers of modern behavioral economics are not mentioned in that essay.

The contributions to behavioral economics have had various directions. The empirical studies by Kahaneman and Tversky uncovered numerous violations of the principles of dominance and invariance concerning the axioms of ordinary utility, by demonstrating systematic biases in risky situations, thus, a rejection of the standard model of subjective expected utility, and proposing prospect theory as its replacement. These were followed in the works by Dickens and Kunreuther (Gilad et al., 1984). Studies by Dickens and others

applied new behavioralism to the issues of consumer and job safety that individual's errors of judgement affect their safety choices, and that these errors will not be eliminated in a competitive market setting. According to these studies, even attentive individuals can be inconsistent because they can be influenced by unique experiences. Werner Debondt, Richard Thaler, Schacter, and Hood applied behavioral economics to the stock market. Liebenstein and Winter emphasized that all decisions involve procedures (i.e. routines) which are sub-optimal. Some writers have focused on the implications of behavioral research in terms of normative economics and economic policy, as a result of which some suggest a new rationale for government intervention in the economy, due to individual judgement biases, failures of the market would also cause a failure of optimized behavior. As stated by Gilad et al. (1984), to writers such as Nelson and Winter, the normative criteria of conventional economics will be in doubt. The works of Dickens, Juster, Thaler (1983, 1982, 1978, 1977), and others have demonstrated that the effectiveness of public policy will be enhanced by introducing behavioral considerations (Ibid.). As argued by Nelson and Winter, "Policy analysis is one area in which behavioral economics can be useful." (1982, p. 379).

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