Kahneman and Tversky and the making of behavioral economics

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5. What to conclude from psychological experiments?

How Smith and Thaler incorporated behavioral deviations in economics

1. From psychology to economics

In the late 1970s economists who followed Vernon Smith’s (1927-) experimental work in economics, corroborated the experimental results of behavioral decision research and concluded that rational choice theory had not been successful in describing individual economic choice behavior [Grether and Plott (1979, 1982)]. Shortly thereafter, financial economist Richard Thaler (1945-) used these experimental findings to infer very different implications for economic theorizing [Thaler (1980, 1993)]. Thaler introduced the work of Daniel Kahneman and Amos Tversky, and argued it provided the solution to the empirical anomalies encountered in financial economics. Thaler’s efforts contributed to the development of behavioral finance in the 1980s, which led to the rise of behavioral economics in the 1990s. The rise of behavioral finance and behavioral economics forced Smith to clarify his position on individual economic behavior. In the 2000s, this produced a collaboration between Smith and Gerd Gigerenzer (1947-) and it was through Gigerenzer that Smith would align experimental economics with the work of Simon.

This chapter thus deals with a transition period in the 1980s in which new methods and insights from behavioral decision research altered both experimental and mainstream economics. The second section of this chapter retraces the initial corroboration of the experimental results of the psychologists by experimental economists in the late 1970s and early 1980s. The third section shows how Thaler and a number of other financial economists accepted the experimental corroboration but inferred a different conclusion. The fourth section sets out Thaler’s continued efforts to contribute to developing the field of behavioral finance in the 1980s. Section five discusses Smith’s response to the rise of behavioral finance. Section six serves as a conclusion.

2 Corroboration and incorporation of psychology’s behavioral deviations in Smith’s experimental economics

In the 1960s and 1970s Smith gradually came to the conclusion that economics needed to be altered [e.g. Lee (2004), Smith (1962, 1965, 1967, 1974)]. His experiments formed an important basis for this. Smith stressed that time was
necessary for the market to reach an equilibrium and argued that experiments should be used to investigate which factors in the real world determine to which equilibrium the market drives the economy to over time. According to Smith, economics was too theoretical and failed to look seriously at actual behavior in the real-world economy. Smith complained that the standard references, Paul Samuelson’s *Foundations of Economic Analysis* (1947) and Roger Allen’s *Mathematical Analysis for Economists* (1938), only discussed “the purely formal properties of the theory” [Smith (1959), p.65], and were of little direct use when applied to real-world problems. He complained that these authors talked about the “inputs” of the production function without giving them any interpretation. When one did so one immediately was forced to make a distinction between the different kinds of inputs, Smith argued, and as a result one ended up with quite different mathematical results. Smith insisted repeatedly that, as opposed to the standard theory, his position had implications “in a very real economic sense” [Smith (1959), p.67].

Smith’s experimental results and his growing dissatisfaction with modern economics led him midway through the 1970s to what is probably his strongest denunciation of this framework. “I believe that the microeconomic theory of the pre-1960’s is a dead end,” Smith wrote, and immediately added an alternative: “The new microtheory will, and should, deal with economic foundations of organization and institution, and this will require us to have an economics of information and a more sophisticated treatment of the technology of transacting” [Smith (1974), p.321]. However, Smith did not imply that pre-1960s microeconomics should be put aside, but argued instead for a serious revision and extension of the theoretical framework. Smith had his reservations with respect to the economics of Allen and particularly the economics of Samuelson and he developed an experimental method that many of his fellow economists were not yet ready to accept.

Smith’s link to behavioral decision researchers and other psychologists was close. Smith’s important early collaborator, Sidney Siegel (1916 – 1961), was a psychologist [e.g. Innocenti (2008)]. Experimental research in economics more generally arose from a brief postwar period of cooperation between operations researchers, computer scientists, mathematicians, economists and psychologists [Dimand (2005), Weintraub (1992), Lee (2004)]. The link was, for instance, close enough for Smith to participate in discussions on Leonard Savage’s and others’ decision theory. In “Measuring Nonmonetary Utilities in Uncertain Choices: The
Ellsberg Urn” (1969) Smith took a position in the ensuing debate on the violations of Savage’s normative theory as presented by Maurice Allais, Daniel Ellsberg and others. Smith’s response to the Ellsberg argument is important because it shows how Smith attempted to strike a balance between Savage’s theory and its opponents, an attempt in which he tried to bridge the opposing theoretical and experimental sides. “I stand with those, like Savage, Raiffa, and Schlaifer, who say they would not want to violate the axioms consciously,” Smith started his argument. Yet, he was unwilling to go all the way with Savage: “However, having stated this I am not prepared to assert that he who seriously and consciously violates the axioms, and in my judgment knows what he is doing,’ is thereby simply making a ‘mistake,’ and should be given a little more conditioning and ‘education’” [Smith (1969), pp.324-325].

Smith took a position that in a crucial way differed from the position taken by decision theorists and behavioral decision researchers. Smith did not want to violate Savage’s axioms, but he did not accept the conclusion that people who violate the axioms were making mistakes either. People may have very good reasons for deviating from the axioms, Smith argued. For instance, this could be because they take into account what other people, such as friends and colleagues, think of their decisions. Smith did not accept the conclusion that deviations from the axioms are to be understood as mistakes implying the need for education. For Smith, deviations from Savage’s axioms, even when they were systematic, were not problematic because over time the market would correct those mistakes. Whereas decision theorists, such as Savage and behavioral decision researchers such as Ward Edwards and Amos Tversky believed that systematic violations immediately raised questions about the normative theory, for Smith this link was much less direct. For Savage, Edwards and Tversky, a decision was either normatively correct (rational) or normatively false (irrational). To Smith, rationality was a matter of content and degree. People might have reasons for initially deviating from the norms, and in a market context the institution of the market would ensure that in due time they would adjust their behavior towards the rational behavior. Smith was of the opinion that “even if [Savage’s] axioms are to be regarded as basically a normative theory, the theory can also do valuable service in helping us to understand actual behavior” [Smith (1969), pp.324-325]. The normative theory shows where and when people deviate from the norm, and in that sense guides the description of observed decision
behavior. But it also serves as a description of human behavior in market equilibrium, and thus helps us to understand how decision behavior adjusts over time in markets.

Another way in which Smith’s experimental work differed from that conducted by behavioral decision researchers is that his decision makers were not individual subjects but economic units. In one of his theoretical papers on investment and production planning, Smith, for instance, started as follows: “We imagine individual decision making units, which we call ‘firms’” [Smith (1960), p.198]. The individual decision making units of Smith’s theories and experiments were not individual human beings, as in the decision theory and the experiments of the psychologists, but they were individual economic units such as firms, consumers, and producers. Smith was not interested in the individual as an individual, but was interested in the individual in its role as a particular economic decision making unit.

As matter of fact, Smith was not even interested in the individual as an economic decision making unit, but instead in how the market institution influenced the unit’s behavior over time. In “Experimental Studies of Discrimination Versus Competition in Sealed-Bid Auction Markets” [Smith (1967)] Smith stated that the “primary purpose” of his experiments was “to study individual bidding behavior and price determination under two alternative forms of market organization: (1) price discrimination, [...] and (2) pure competition” [Smith (1967), p.56]. It was not the individual unit’s behavior that should be investigated, but the market environment that affected its behavior. In mathematical psychology and behavioral decision research the individual functioned as a measurement instrument for (average) individual psychological characteristics, as set out in Chapters two and three. In Smith’s experiments, the individual functioned as a measurement instrument for characteristics of the market mechanism.

Because Smith’s experiments are historically and methodologically connected to the experiments of the psychologists of behavioral decision research, and because Smith at different points actively engaged in discussions in decision theory, it might appear that experimental economics, decision theory, and behavioral decision research developed in tandem in the 1960s and 1970s. But Smith’s experimental economics differed in at least two crucial ways from the experiments conducted by the psychologists. First, Smith did not investigate the individual human being, but instead he investigated economic decision making units. Smith was only interested in individual human beings in their role as an economic decision making unit. Second,
Smith did not assume the static point of view in which it is believed that the individual that deviates from the normative theory has made a mistake. In contrast, Smith was interested in how decision behavior changes over time, and in the environments that induce these changes. Smith took a stance that he at least once labeled “a crude macrobiological approach,” in which the system, when not exogenously altered, tends towards a “stable equilibrium” [Smith (1968), p.410].

But Smith did not entirely dismiss the experimental results of the psychologists either. The stream of experimental results obtained by behavioral decision research in the 1970s which showed that individuals violate rational choice theory, required experimental economists assume a stance. David Grether (1939- ) and Charles Plott (1938- ), who had joined Smith’s experimental economics program in the 1970s, decided to subject the experimental results of the psychologists to a test. The reason that they could do so was that all the material and expertise were already available. The rise of experimental economics had produced an environment in which the results of the psychologists could be tested without requiring economists to learn new methods or techniques. Checking as many possible explanations as they could think of for the results obtained by the psychologists, Grether and Plott (1979, 1982) sought to falsify the findings of the psychologists. Moreover, they set out to test the experimental findings based on the presumably much more rigorous standards of (experimental) economics. The rise of the experimental method in economics had made experimental economists confident they could beat psychologists at their own game, or at least critically assess their work by using their own experimental method.

Grether and Plott (1979, 1982) focused on the alleged phenomena of “preference reversals,” the phenomenon that occurs when individuals change their preferences regarding the same choice when it has been differently formulated; and “intransitivity,” the related phenomenon showing that actual individual preferences are not always transitive. 35 Grether and Plott (1979) were very suspicious of the empirical evidence produced by the psychologists, and aware that economists and

35 Their references to behavioral decision research consisted of only a few articles [e.g. Slovic and Lichtenstein (1971, 1973), Tversky (1969, 1971)]. Paul Slovic and Sarah Lichtenstein were, however, quick to remark in the American Economic Review that “there is a substantial body of research on preference reversals within the psychological literature that is being neglected here. Moreover, reversals should be seen not as an isolated phenomenon, but as one of a broad class of findings that demonstrates violations of preference models” [Slovic and Lichtenstein (1983), p.597]. From that moment on, investigations of empirical falsifications of rational choice theory in experimental economics increased.
psychologists did not always use a rational choice theory for the same purpose. They emphasized that “[t]here is little doubt that psychologists have uncovered a systematic and interesting aspect of human choice behavior” [Grether and Plott (1979), p.624], but wondered whether 1) the phenomenon also held in more typical economic situations, and 2) whether it could be explained by means of economic theory.

The main worry of Grether and Plott was that the experimental results were mere artifacts produced by the experimental setup of the psychologists. They produced thirteen (!) methodological and theoretical economic explanations for the falsifications: 1) no real money was used and incentives may therefore have been misspecified; 2) different incomes of the subjects may have influenced some experiments; 3) in most of the psychological experiments indifference between two options was not possible; 4) perhaps subjects did not give their true selling or bidding price but acted strategically; 5) subjectively perceived probabilities from the lotteries used may not be equal to actual objective probabilities; 6) perhaps subjects chose lexicographically, as in Tversky’s elimination-by-aspects theory [Tversky (1971)], which would account for a moderate form of preference reversals; 7) perhaps the magnitudes of the choices were too close, leading to apparent intransitivity, as in Tversky (1969); 8) the cost of decision making could be too high compared to the expected pay-off, leading subjects to not make an effort; 9) perhaps the choices subjects faced contained too much information for the subject to process within the time available; 10) subjects could have been confused or might have misunderstood the experiment; 11) perhaps the phenomena reported occurred only in a few subjects; 12) the subjects were relatively unsophisticated psychological undergraduates, whereas more sophisticated subjects might make more rational choices; and 13) the experimenters were psychologists, leading subjects to speculate about the true purpose of the experiments, and hence perhaps to change their behavior.

This last explanation for the findings particularly illustrates that Grether and Plott went to great lengths to show that the findings of the psychologists had been mere artifacts. Grether and Plott’s message was that every possible explanation for the psychologists’ findings needed to be controlled for, even the argument that the results should not be taken seriously for the sole reason that the experimenters had been psychologists. Grether and Plott set up two experiments in which they controlled for all thirteen possible explanations. They specified incentives, they made the experiments very simple and they made certain that all subjects understood the
choices they could make. Furthermore, they used undergraduates as well as graduates, making it clear that they were economists and not psychologists, and they took the two possible explanations of Tversky into consideration. But, much to their surprise, they obtained results that were similar to those of the psychologists. Consequently, they remained “as perplexed as the reader who has just been introduced to the problem” [Grether and Plott (1979), p.624].

The first Grether and Plott article was published in the same year as Kahneman and Tversky’s prospect theory, and in Grether and Plott (1982) they recognized prospect theory as a prominent example of a rational choice theory which adjusted a number of assumptions in order to account for the empirical findings. But Grether and Plott (1982) stressed that prospect theory could not account for their experimental results; “We need to emphasize that the phenomenon causes problems for preference theory in general, and not for just the expected utility theory. Prospect theory as a special type of preference theory cannot account for the results” [Grether and Plott (1982), p.575].

The conclusions Grether and Plott derived from these results are important because they set the standard for experimental economists’ responses to these and similar findings for the following quarter of a century. According to Grether and Plott (1979, 1982), the experimental results pointed to an inconsistency between actual behavior and rational choice theory that was “deeper than the mere lack of transitivity or even stochastic transitivity.” The empirical results suggested “that no optimization principles of any sort lie behind even the simplest of human choices” [Grether and Plott (1979), p.623, emphasis added]. Grether and Plott did not believe that the empirical results could be addressed by making a relatively minor adjustment of rational choice theory, but drew the radical conclusion that utility maximization and rational choice should be completely abandoned as a description of and explanation for the decision making behavior of individuals.

However, and this was equally crucial for the experimental economic approach, Grether and Plott did not imply that utility maximization and rational choice as a description of market behavior were invalidated. With respect to market behavior the experimental results only showed that the economic subjects, who in the final market equilibrium behave according to rational choice and utility maximization, initially behave according to a to-be-developed theory that is completely unlike utility maximization and rational choice. Because the disciplining, rationalizing institution
of the market operates between individual behavior and market behavior, a falsification of individual rational optimization did not falsify rational choice as a description of equilibrium market behavior. Quite the contrary, the experimental results only emphasized the role of the market as the mechanism that rationalizes individual behavior. Smith, Grether and Plott assumed that the market drives the behavior of the economy to a rational, utility maximizing equilibrium over time. Therefore, the fact that initially individual behavior systematically deviated from rational utility maximization only showed how important the market mechanism was in driving individuals to rational behavior in market equilibrium.

3. Thaler’s financial economic anomalies and the creation of behavioral finance

In postwar neoclassical economics, the market had largely become an empty concept, mainly due to Samuelson’s work. In Samuelson’s framework [Samuelson (1947)], it was assumed that all the decision makers in the economy always maximize utility. Samuelson needed to make this assumption for his operationalist approach. If individuals were assumed not to always maximize utility it could not be supposed that their observed behavior was on individual demand and supply curves and it would then be impossible to operationalize demand and supply curves through the operation of measuring individual choices. But assuming that individual subjects in the economy always maximize utility implied that the economy was always in equilibrium [Weintraub (1991)]. In contrast to the view later developed by Smith and in experimental economics, time was not an element in the Samuelsonian neoclassical economic world. Samuelson only considered static equilibria. There was a direct link between the behavior of individuals and the market; the market was nothing more than the sum of all the individual behaviors. The adjustment of individual behavior to market equilibrium did not come into play.

In the 1960s and 1970s, a new field in economics appeared that used the Samuelsonian neoclassical theory as a theoretical foundation for its empirical investigation of stock market behavior. Based on research conducted during the 1950s and 1960s by Franco Modigliani (1918 – 2003), Merton Miller (1923 – 2000), and Harry Markowitz (1927- ), financial economics, as the new field came to be called, gradually appeared as an accepted genuine sub-branch of neoclassical economics in the second half of the 1960s and 1970s [Jovanovic (2008), Poitras and Jovanovic
(2007), MacKenzie (2006)). The empirical study of stock markets was linked to neoclassical economics through what came to be referred to as the efficient market hypothesis. The efficient market hypothesis specified the theoretical position of neoclassical economics in the case of the stock market, and any market for that matter [Jovanovic (2008)]. “A market in which prices always ‘fully reflect’ available information is called ‘efficient’” [Fama (1970), p.383].

The central question for financial economists was whether stock markets indeed are efficient, as theory predicted, or inefficient, for which an explanation then would have to be found. In the second half of the 1960s and 1970s two opposing views developed. At MIT, Paul Cootner (1930 – 1978), Hendrik Houthakker (1924 – 2008) and others developed and defended the idea that the stock market was not efficient. “[P]rice changes are not purely random but follow certain longer run trends,” Houthakker argued [Houthakker, quoted in Jovanovic (2008), p.228], and Cootner more bluntly stated that “[t]he stock market is not a random walk.” [Cootner, quoted in Jovanovic (2008), p.225]. The Chicago Graduate School of Business held and fiercely defended the opposite view, that the stock market is efficient and that the stock prices over time will appear to be a random walk [Jovanovic (2008)]. In Chicago, Eugene Fama (1939- ), a student of Miller, arose as the main protagonist, defending the efficient market as an empirical and theoretical fact [e.g. Fama (1970)]. Inspired indirectly by then Chicago mathematician Savage, Fama distinguished between “sophisticated traders” who were experienced enough to determine the intrinsic value of securities and act accordingly, and “other participants” who did not (yet) posses this skill and who produced the random noise around the intrinsic value. Sophisticated traders ensured that the market prices remained or returned quickly to the underlying value of the stocks [Jovanovic (2008), Fama (1970).

On the basis of the empirical corroboration of the psychological results by Grether and Plott, financial economists began to look seriously at the results of the psychologists. One illustrative example is Arrow (1982), which discussed a number of phenomena in the (stock) market that contradicted the “rationality hypothesis,” such as individuals’ unwillingness to accept government subsidized insurance below its actuarial value and observed irrationality in financial markets. Arrow suggested “that these failures of the rationality hypothesis are in fact compatible with some of the

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36 See e.g. Markowitz (1952, 1959, 1965), Modigliani and Miller (1958), Miller and Modigliani (1963) and the collected papers in Modigliani, Abel, and Johnson (1980).
specific observations of cognitive psychologists” [Arrow (1982), p.5]. Arrow, in other words, drew a direct line from observations in the laboratories of the psychologists to contradictions observed in the market. Experimental results from psychology, that in themselves had nothing to do with the economy or with markets, were linked to economics and used as an explanation for the unsolved financial economic puzzles. In one sentence Arrow linked two very different phenomena:

an important class of intertemporal markets shows systematic deviations from individual rational behavior and [...] these deviations are consonant with evidence from very different sources collected by psychologists. [Arrow (1982), p.8]

Systematic deviations from rational behavior by individuals in the laboratory could be an explanation for observed market deviations only when one understood the relation between individual and market behavior to be direct, as was the case in Samuelsonian neoclassical economics.

However, deviations from the theory of efficient markets had been discussed before. For instance, market deviations could be explained as resulting from market imperfections such as transaction costs and limited information. The deviations could furthermore be explained as short-run phenomena that would quickly disappear through arbitrage. But Arrow considered these explanations to be insufficient and argued that the results of the psychologists and experimental economists implied that market imperfections should be understood as genuine phenomena, and not as having resulted from temporary distortions of the market. Thus, Arrow recalled that “[a]ny argument seeking to establish the presence of irrational economic behavior always meets a standard counterargument: if most agents are irrational, then a rational individual can make a lot of money; eventually, therefore, the rational individual will take over all the wealth” [Arrow (1982), p.7]. But, Arrow argued, arbitrage and related arguments could easily be countered: “(1) Not all arbitrage possibilities exist. [...] (2) More important, if everyone else is “irrational,” it by no means follows that one can make money by being rational, at least in the short run” [Arrow (1982), p.7].

Attempts to incorporate the corroborated findings of the psychologists in financial economics appear scattered through the literature from 1980 onwards. But
Thaler was the first economist to draw economic implications from behavioral decision research findings explicitly. This central focus of his work made him a great promoter of Kahneman and Tversky’s work in economics. Extensive references to the work of Kahneman and Tversky occurred in almost every publication by Thaler. Thaler was an economist from the Chicago Research School of Business and a colleague of Fama. In the 1980s he worked predominantly in financial economics, advancing the experimental results and the theoretical approach of Kahneman and Tversky as an explanation for the observed falsifications of the efficient market hypothesis, and thus disagreeing with the prevalent financial economic view in Chicago. In the 1980s the exploration of systematic deviations from the efficient market hypothesis in financial markets by Thaler and others became known as behavioral finance.

Thaler’s first behavioral finance paper, “Toward a Positive Theory of Consumer Choice,” appeared in 1980 in the Journal of Economic Behavior and Organization. By 1991 Thaler had collected enough material to publish a book, entitled Quasi Rational Economics and consisting of sixteen of his papers that tested the traditional neoclassical economic models and offered alternatives. In 1993 Thaler edited another book entitled Advances in Behavioral Finance for the Russell Sage Foundation (RSF), consisting mainly of papers from the latter half of the 1980s, which was followed by a second volume in 2005, with the same title. Kahneman and Tversky were behavioral finance’s theoretical founding fathers, but Thaler was its earliest and strongest advocate.

Specifically, Thaler built on two lines of Kahneman and Tversky’s research. Thaler systematically connected Kahneman and Tversky’s biases of rational choice in experiments to the anomalies of rational choice theory found in financial economics, and he made this connection the cornerstone of a new research program. He collected phenomena that were anomalous in financial economics and were compatible with the biases found by Kahneman and Tversky. Sometimes explanations were offered on the basis of prospect theory or by means of some other theory. Usually, however, these violations of standard economic theory were presented without any explanation to account for them, and he simply stressed what they implied, that neoclassical theory had been violated.
For the *Journal of Economic Perspectives*, Thaler published two series of “anomalies” columns that had the sole purpose of proclaiming that economics had serious problems. The first series contained fourteen anomalies articles and appeared between 1987 and 1991. The second series contained four publications and appeared between 1995 and 2001. The first anomaly article in 1987 documented “the January effect.” When the market for stocks is in efficient equilibrium, in the neoclassical world the average monthly return should be equal for each month. There is no reason to expect that stocks would perform better just because it happens to be a certain month. However, this was exactly what was observed in the case of January. Especially for smaller firms stock returns were substantially higher in January compared with other months. How could this January effect be possible given the theory of efficient markets? The answer was that it was not possible and that one needed a theory such as Kahneman and Tversky’s prospect theory to account for the findings.

Loewenstein and Thaler (1989) showed that many similar anomalies existed in and outside the economy that have to do with intertemporal choice. For example, people prefer to pay too much tax in advance and to receive some back when the year is over instead of the reverse, even when the first option is subject to costs in terms of lost interest. Schoolteachers who can choose between being paid in nine months (September-June) or in twelve (September-August), choose the second option although from an economic perspective the first is more rational. But Loewenstein and Thaler also cited the dermatologist who lamented that her patients were unwilling to avoid the sun when she told them about the risks of sun cancer, but who were quick to stay out of the sun when she told them about the risk of getting “large pores and blackheads.” This example, Loewenstein and Thaler argued, was also a violation of economic theory because it showed myopia in patients they should not have if they acted rationally. The implicit reasoning was that economic theory could be applied to every aspect of our lives and that therefore also violations of economic theory could be drawn from every corner of life. The recurring message of the anomalies articles was that there are serious problems with economic theory which cannot be easily dismissed, and which need to be taken seriously.

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37 The anomalies of the first series have been collected in *The Winners Curse* (1992).
In his anomalies column Thaler cited examples from finance that were clearly economic. The structure of the anomalies was often similar to the biases produced by Kahneman and Tversky. One anomaly that Thaler frequently investigated and that became one of the principal anomalies of behavioral finance was the “endowment effect.” The endowment effect was an application of the framing effect of Kahneman and Tversky that showed that individuals’ preferences are subject to an initial framing process. In other words, individuals’ preferences depend on the quantity of the means they are endowed with. The experiment is as follows. Divide a group of subjects randomly into two sub-groups and give one of the two sub-groups a standard coffee mug. Subsequently, ask the sub-group with the mug what price they would minimally want to sell the mug for. Also ask subjects of the sub-group without mugs what price they would maximally want to pay for the mug. Typically, the willingness to accept (WTA) is about twice the willingness to pay (WTP). Apparently, people reframe their preferences after receiving the mug. In economics, this endowment effect could serve as an explanation for the often observed fallacy of taking into account sunk costs [see e.g. Thaler (1980, 1987), Tversky and Kahneman (1981)]. The endowment effect further falsified the Coase theorem, which says that in order to attain the efficient market allocation, the initial endowment of the goods should be irrelevant. The Coase theorem depends on the assumption that for every individual WTA equals WTP, so that trading will continue until the goods are in the hands of those with the highest WTP. But given the demonstrated systematic difference between WTA and WTP, the Coase theorem no longer held true [Kahneman, Knetsch, and Thaler (1990)].

A defining characteristic of Thaler’s behavioral finance was that it adopted Kahneman and Tversky’s understanding of normative and descriptive. In addition, Thaler accepted their understanding of the positive realm of economics as covering both the normative and the descriptive domain. This understanding diverged from the postwar economic understanding in which a value-free positive domain was contrasted with the application of positive economic theories for specific value-laden goals of the policy maker in the normative realm. As a result, Thaler’s introduction of Kahneman and Tversky’s meaning of normative and positive could not but lead to confusion. Thaler equated Kahneman and Tversky’s descriptive domain with the
economists’ positive and used normative both in Kahneman and Tversky’s meaning and in the economists’ meaning. Thaler (1980) expressed it as follows:

Economists rarely draw the distinction between normative models of consumer choice and descriptive or positive models. Although the theory is normatively based (it describes what rational consumers should do), economists argue that it also serves well as a descriptive theory (it predicts what consumers in fact do). This paper argues that exclusive reliance on the normative theory leads economists to make systematic, predictable errors in describing or forecasting consumer choices. [Thaler (1980), p.39]

The conceptual re-organization of economics that Thaler took over from Kahneman and Tversky played an important role in behavioral economics in the 1990s and 2000s and it would determine how behavioral economists started to think about policy advice in the 2000s. This will be discussed in more detail in Chapter six. In line with Kahneman and Tversky, Thaler argued that further theoretical advancement of the normative theory was perfectly fine, but that because economists had ignored the fact that real-world behavior of individuals does not agree with this theory so long, they should now also pay more attention to building a descriptive theory of economic behavior.

Thus, Thaler not only accepted the empirical evidence presented by Kahneman and Tversky, but he also accepted their accompanying methodological distinction. Essentially, Thaler accepted Kahneman and Tversky’s attempt to recreate economics in the image of behavioral decision research. As set out in Chapter four, prospect theory made a unificatory claim. It claimed that behavioral decision research and economics were part of the same program, and that the approach of behavioral decision research was better than that of the economists. Therefore, the economists should adopt prospect theory and its methodological distinctions. Thaler accepted this reasoning entirely. He provided Kahneman and Tversky’s approach with more economic content, but he left the theoretical structure intact. From the early 1980s to the early 1990s, Thaler’s promotion of Kahneman and Tversky’s prospect theory
acted as the main catalyst for establishing an economic program based on the work of Kahneman and Tversky.

Another catalyst for developing behavioral finance in the 1980s was the support of the Alfred Sloan Foundation (ASF), the Russell Sage Foundation (RSF) and the National Bureau of Economic Research (NBER). The best way to conceive of the role of the NBER, the ASF and the RSF is in contributing to the attachment of a small group of researchers to a large and influential research program. Their financial resources explain how a few financial economists interested in research from a particular branch of psychology could develop a more or less coherent research program built largely around the work of two psychologists. From 1986 onwards, the ASF and later the RSF were consistent sponsors of behavioral economic research. In the mid-1990s the RSF set up a series of books in behavioral economics, set up a “Behavioral Economic Roundtable” that regularly brought and still brings behavioral economists together, and organized and still organizes a series of workshops in collaboration with the NBER. The financial support from ASF, RSF and NBER is a relevant characteristic of the rise of behavioral finance and behavioral economics, but is not further discussed in this dissertation.

4. Distinguishing experimental economics from the rising star of behavioral finance

The difference between Thaler’s behavioral finance and Smith’s experimental economics is that behavioral finance investigates individual behavior and that experimental economics investigates markets. The growing number of behavioral financial publications in the 1980s and the influence of Kahneman and Tversky’s work more generally pressed Smith to distinguish his experimental economics more clearly from these psychologists and their economic off-spring. In 1989, ten years after the first Grether and Plott article, Smith asked:

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38 From a different starting point, Ana Santos’ dissertation The Social Epistemology of Experimental Economics (2006) arrives at a taxonomy that is similar to, yet differs somewhat from the history described here. Within what she broadly labels “experimental economics” she distinguishes three types of experiments: “market experiments” such as those of Smith, and “non-market experiments,” the latter including “individual behaviour and decision-making experiments” such as those of Allais (1953) and “game theory experiments,” such as Kalish, Milnor, Nash, and Nering (1954) and Schelling (1957).
How do we close the [...] gap, between the psychology of choice and agents’ economic behavior in experimental exchange markets? [...] I think we economists need to accept these replicable empirical results [of behavioral decision research] as providing meaningful measures of how people think about economic questions. For their part, psychologists need to accept the dominating message in experimental research on the performance of a wide variety of bidding, auctioning and customer (posted price) markets: markets quite often “work” in the sense that over time they converge to the predictions of the economists’ paradigm. [Smith (1989), p.165, emphasis in original]

The conclusions drawn from the experimental results of Grether and Plott (1979, 1982) have been held by Smith, Grether, Plott and other experimental economists from the late 1970s until today. Over the years experimental economists have struggled over how to formulate their approach and how to distinguish their ideas from Thaler’s behavioral finance and more broadly from those of the behavioral economists. Part of the difficulty was (and still is) that experimental economists and Kahneman and Tversky’s prospect theory are seemingly very close. Experimental economists agree that the psychologists’ experimental findings indeed disprove rational choice of individual decision behavior, which easily led to the conclusion that they also agreed with the theoretical implications that were drawn by behavioral decision researchers and behavioral finance economists.

Another difficulty was that experimental economists conducted the same kind of experiments as the behavioral decision researchers and behavioral finance economists, but with a different purpose. Behavioralists conducted experiments with individual human subjects to investigate the decision making characteristics of the individual. Experimental economists conducted experiments with individual human subjects to investigate the market. The two sides conducted the same experiments, but with a different question in mind. Chapters two to four showed that experimental psychology used human subjects not because it was interested in any particular individual, but as a measurement instrument for measuring the characteristics of the individual. Experimental economics went a step further. It was not interested in the particular individuals in experiments, nor in the individual or his or her characteristics in general. It needed the individuals to experiment on a phenomenon that was altogether different from the individual subjects of the experiment. Like the biologist
who investigates a virus through its effect on laboratory mice, so experimental economics investigated the market but needed individual subjects as it were, to investigate the market. Nevertheless, experimental economists were easily understood as investigating human behavior, as will be set out in more detail in the following chapter. Frequently found statements in experimental economics of the sort “[i]n laboratory market experiments, we test the theory’s assumptions about agent behavior” [Smith (1989), p.154] could understandably be misunderstood as statements about the psychology of human beings. Moreover, as a result of this subtle distinction, experimental economists were pressed to distinguish themselves more clearly from Thaler’s behavioral finance and later from behavioral economics.

The use of time was crucial in Smith’s experimental economics. In experimental economics the market required time to drive the economy to equilibrium. Because of their use of time experimental economists could maintain that individual behavior initially deviated from the norms of rational choice theory and utility theory. At the same time, they could also maintain that the emerging market equilibrium was in line with rational choice and utility theory. Smith’s use of time distinguished him sharply from Samuelsonian neoclassical economics and behavioral economics and this was the main reason why he had difficulties explaining experimental economics’ position to behavioral finance economists. “People have their own homegrown beliefs about how markets work, or should work,” Smith carefully explained, and “questionnaire responses reflect these beliefs, which are often couched in terms of ‘fairness’ criteria.” As a consequence, “[people’s] initial behavior in a market may reflect these beliefs.” However, when these individuals operate in a market over time their behavior “adapts to the incentive properties of markets” [Smith (1989), p.166, emphasis in the original].

Smith went on to conclude that in economics there were “two experimental research programs,” both of which, he added, required considerable development. First there was the “economist’s maximizing paradigm,” which “often performs well in predicting the equilibrium reached over time in experimental markets.” However, the economist’s maximizing paradigm “is not generally able to account for short run dynamic behavior, such as the contract price paths from initial states to final steady states.” Second, there was “the psychologist’s ‘reference frame’ descriptive paradigm,” which is Kahneman and Tversky’s prospect theory and the behavioral finance that emerged from it. This psychological program did well “in explaining
subjects’ introspective responses, and their short-run or initial decision behavior, but it provides no predictive theory of reference frame adjustment over time.” Smith was quick to point out that a well-known paper from the psychological program agreed with this analysis. “In fact, the statement (Kahneman, Knetsch and Thaler, 1986, p.731) ‘that they (people) adapt their views of fairness to the norms of actual behavior’ can be interpreted as a description of what is observed in experimental markets” [Smith (1989), p.166].

Another way in which Smith tried to distinguish experimental economics more clearly from behavioral decision research, behavioral finance and behavioral economics was by abandoning the label “experimental game” as a description of his experiments. In the 1950s, 1960s and early 1970s game theory, the application of rational choice theory to situations of human interaction, had been an important source of inspiration for Smith’s experiments [Lee (2004), Weintraub (1992), Dimand (2005)]. But game theory as a description and explanation of the interaction of rationally acting self-interested individuals started from a description of individuals as optimizers of utility. The experimental results showed that this had been a wrong assumption. Therefore, game theory became inappropriate for experimental economists as a description of individual behavior. The fact that Smith during the 1970s explicitly discarded his use of the term “experimental games” to describe his experiments seems an unsolved puzzle [Lee (2004)]. But in the light of the corroborations produced by Grether and Plott (1979, 1982) Smith’s reasons can be illuminated. Smith considered game theory no longer a good description of individual behavior. Game theory still describes and explains the behavior of the individuals in the eventual market equilibrium, but cannot explain individuals’ behavior when they are first presented with an economic decision. It can neither explain the process of adjustment to equilibrium. As a result, the term “experimental game” became inappropriate as a description of experiments that investigated the adjustment behavior of the individual agents in a market setting. The experiments were still considered a game in the sense that they mimicked the crucial aspects of the market, but they were no longer a game in the sense of describing fully rational interacting individuals.

Pressed to distinguish experimental economics more clearly from behavioral finance, Smith was led to cooperate with psychologist Gigerenzer in the 2000s. In 2001 Smith contributed a chapter to Gigerenzer and Reinhard Selten’s edited volume
Bounded Rationality, The Adaptive Toolbox [McCabe and Smith (2001)]. In 2008 Smith published a monograph entitled Rationality in Economics, Constructivist and Ecological Forms in which he drew an explicit link between his own and Gigerenzer’s work. In the Handbook of Experimental Economic Results, volume 1 (2008), edited by Plott and Smith, Gigerenzer participates by making no fewer than six contributions, and the third, forthcoming volume in Gigerenzer’s Adaptive Behavior and Cognition (ABC)’s research group on bounded rationality in its title emphasizes the link with Smith: Ecological Rationality: Intelligence in the Real World. Finally, in 2008 Smith agreed to become co-director of Gigerenzer’s ABC group at the Max Planck Institute for Human Development in Berlin [email Gigerenzer to author, July 12, 2008].

5. Economics, behavioral decision research, and Kahneman and Tversky
In the 1960s and 1970s behavioral decision research became relevant to economics, as set out in Chapters three and four. Behavioral decision researchers produced experimental results that seemed to falsify rational utility maximizing behavior, the basis of all modern economics. The role of Kahneman and Tversky has been emphasized: Kahneman and Tversky’s prospect theory (1979) was a conscious attempt to influence economics and to alter economists’ reasoning.

The first to pick up on behavioral decision research’s experimental results were experimental economists such as Grether, Plott and Smith. Grether and Plott (1979, 1982) corroborated the experimental findings of psychology and drew the conclusion that rational choice as a description of individual human behavior should be entirely abandoned. However, experimental economists at the same time concluded that rational choice as a description of efficient markets in equilibrium could be maintained and that the experimental results only emphasized the rationalizing forces of the market. Furthermore, experimental economics did not accept behavioral decision research’s alternative accounts, and explicitly denounced the most visible theory among them, Kahneman and Tversky’s prospect theory.

An unexpected result of experimental economists’ corroboration of behavioral decision research’s experimental results was that it paved the way for behavioral decision researchers to enter financial economics. Thaler understood the psychological findings to show the irrationality of individual choices, and drew a direct link from the irrationality of individual choices to irrational features of the
behavior of markets. He immediately recognized Kahneman and Tversky’s research, and especially Kahneman and Tversky (1979), as an important new and improved theory of individual decision behavior. The different responses of Smith’s experimental economics and Thaler’s financial economics to the experimental results and to prospect theory’s alternative can be explained in terms of the different notion of the market in experimental economics and financial economics. To Smith the market was a rationalizing mechanism that requires time to drive the economy towards equilibrium. For financial economists such as Thaler, time was not an element of the market.

This different response to the findings of behavioral decision research was a reflection of a more fundamental difference between experimental economists and behavioral decision researchers such as Kahneman and Tversky. Behavioral decision researchers located the explanation for the deviations from rational behavior in the nature of human beings. For Kahneman and Tversky the reason that human beings often deviate in their behavior from what is rationally optimal is because they are made that way. As set out in Chapters two, three, and four, Kahneman and Tversky stood in an experimental psychology tradition in which the fixed characteristics of the individual were investigated. Kahneman and Tversky investigated the human being in the way that the physicist investigates the atom: on the assumption that there is one universal way in which the individual/atom can respond to a particular stimulus. This was a fundamentally different understanding from that of experimental economists who started from the assumption that individual behavior is constantly subject to change and who investigated how the market causes individual behavior to change. Experimental economists investigated behavior like biologists in that they assumed that individual behavior constantly adapts to an external selection force.

This also explains why prospect theory was so quickly adopted by many economists, whereas Simon had largely failed to influence economists. Kahneman and Tversky, neoclassical economists like Samuelson, and financial economists like Thaler, all conceived of individual behavior as a stable phenomenon that, like the physicist’s atom, could be isolated to investigate its fixed and universal properties. Simon and Smith on the other hand were social scientists who understood individual behavior as adaptive and hence as unfixed. In their view, therefore, it did not make sense to isolate the individual decision maker from its environment. The universal properties Kahneman and Tversky, Thaler, and Samuelson were looking for simply
did not exist in this view of economic behavior. The two sides had a different understanding of individual behavior and a different understanding of collective decision making in markets, administrative organizations and institutions. Simon and Smith clashed with mainstream neoclassical economists on a very basic level while Kahneman and Tversky agreed with financial economists on the fundamentals.

Financial economists had an approach not unlike behavioral decision research and therefore were not as fundamentally opposed to behavioral decision research as were experimental economists. But not being opposed is not the same as favoring. At least two further reasons explain why Kahneman and Tversky specifically were successful in influencing many mainstream economists. First, Heuristics and Biases and prospect theory explained the violations of the efficient market hypothesis as argued by financial economists in the 1960s and 1970s. Yet, as rational choice theory was a cornerstone of financial economics, abandoning it completely, as Simon for instance had proposed, would be too radical a step. Kahneman and Tversky’s prospect theory was successful because it offered rational choice theory an honorable way out. Kahneman and Tversky’s message to economists was that there was nothing wrong with their theory of rational choice, but that economists should recognize that their label of positive actually covered a normative and a descriptive domain. Kahneman and Tversky explained that economists had been using rational choice theory for both the normative and the descriptive domain, whereas it should only be used for the normative realm. Kahneman and Tversky thus offered financial economists one straightforward way out of their problems.

Second, the alternative Kahneman and Tversky offered in the form of prospect theory was close to rational choice theory and in fact essentially was rational choice theory as seen through psychophysical spectacles. Being close to the traditional rational choice theory had the advantage that it was the same framework for individual behavior that financial economists had been using and were familiar with. The conceptual step from traditional rational choice theory to prospect theory was a small one. Furthermore, prospect theory did not require serious alterations or that the theories and models economists used be abandoned. The introduction of a few extra parameters sufficed to allow financial economics proceed as before.

An unfortunate element that has blurred the understanding of this episode in the history of economics and psychology is that both the alternative proposed by Simon in the 1950s-1970s and the group of Kahneman and Tversky-inspired
economists that arose in the 1980s and 1990s have been labeled behavioral economics, a label first used by George Katona in 1946 [Juster (2004)]. This has led to the misunderstanding that the behavioral finance and behavioral economics of Thaler was the continuation or a reappearance of the earlier project of Simon. But this is not true. Simon’s behavioral economics and Thaler’s behavioral economics were two very different projects, and the idea that they share the same label, and thus must be somehow related is a red herring. 39

The late 1970s and early 1980s constitute a transition period in the recent history of economics. Smith and experimental economists gained prominence and used their new method to critically test the experimental results of behavioral decision research. They found that behavioral decision researchers had been right in important respects and concluded that rational choice theory was not successful as a complete description of human behavior. Financial economist Thaler, by contrast, drew a very different conclusion and reconceptualised financial economics on the basis of Kahneman and Tversky’s behavioral decision research. This unified financial economics with a specific branch of psychology, since in behavioral finance, economics became part of behavioral decision research. We will see in Chapter six that, from the late 1990s onwards, behavioral economists would again distinguish themselves from psychology and from behavioral decision research in particular. But in the 1980s, Kahneman and Tversky’s claim that behavioral decision research and economics were essentially about the same thing had been wholly accepted and applied in Thaler’s behavioral finance.

39 A classification such as ‘old’ and ‘new’ behavioral economics of Sent (2004) only provides easy ammunition to those eager to show that the two behavioral economics’ are related. Thus, it is common in contemporary behavioral economics to refer to Sent’s distinctions to quickly show that contemporary behavioral economics incorporates Simon’s behavioral economics [e.g. Angner and Loewenstein (forthcoming a,b)].