THE PRESCRIPTIVE TURN IN BEHAVIORAL FINANCE

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ABSTRACT

As business finance transformed itself into modern finance and then into financial economics, the extreme simplification called the homo economicus underwent a character transformation. The first stage was from "a model of" to "the model of." Accordingly, for most of its roughly fifty-year life, the task of finance research has been to discover how the finance practice we observe is a consequence of rational choices. The second, more recent stage of the transformation has been from "the model of" to "the model for," using the word "model" in a slightly different sense as something to be emulated. The so-called "behavioral finance" literature, which could have been a basis for a new paradigm, has taken a marked prescriptive turn. Its implicit purpose is to discover and remedy deviations from rational choices, presuming that if people do not behave according to the prescription of theory, then something is wrong with people and not with the theory. This paper charts this transformation and the implications of the fatal fixation of finance upon rational choice.
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Introduction

Mankind have had, at all times, a strong propensity to realize their own abstractions.--Adam Smith

At the birth of modern finance, the expected utility [EU, subsequently] axioms of Von Neumann and Morgenstern (1947) specified how the *homo economicus* made rational investment choices under conditions of risk. It did not matter that these axioms were such extreme simplifications of human behavior that undergraduate students encountering them for the first time still find them a puzzling foundation for further study. Consistent with Friedman’s (1953) doctrine of positive economic methodology, “wildly unrealistic” assumptions were necessary for casting complex economic phenomena in manageable terms for theory construction. Should the theories built upon such simplifications fail empirical testing then the assumptions would be replaced by others (possibly, but not necessarily, more realistic) until the resulting theories provided “. . . sufficiently good ‘approximation’ for the purpose at hand” (ibid., p. 15). No one ever argued, however, that there either was or should be such a person as the *homo economicus*. He was a methodological convenience and a tolerated myth.

As finance as an academic discipline transformed itself into modern finance and then into financial economics, as it is often called, Von Neumann and Morgenstern’s species of the *homo economicus*, in Friedman’s new interpretation, underwent a character transformation. The first stage of this transformation was from “a model of” to “the model of.” For most of its roughly fifty-year life, financial economics has been a search for explanations grounded in the EU axioms, as if the financial system were the outcome of a teleological evolutionary process that has necessarily led to emergence of the *homo economicus*, and as if the task of finance research were to discover how the finance practice we observe is a consequence of rational choices based upon EU. Thus, the *homo economicus* is no longer just a methodological convenience for modeling the economic behavior of individuals and of personified markets and institutions. He is someone who must exist within us and whom financial economists must find.

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1 From “The History of the Ancient Logics and Metaphysics”
2 Although axioms sound as if they express deep meanings, in fact they are just convenient assumptions that gain increased currency through extensive use.
The second stage of the transformation has been from “the model of” to “the model for,” using the word “model” in a slightly different sense as something to be emulated. Within the past 10-15 years, numerous empirical anomalies have arisen in finance that have defied extensive efforts to explain financial decision making by individuals (often referred to as agents) as economically rational choices. One response to this failure has been the advent of behavioral finance (BF, subsequently), recently labeled the “anomalies literature,” (Fama, 1998) that attempts to build theories on the results of psychological research concerning how people do, in fact, make choices under uncertain conditions.

At first glance, this is perfectly consistent with the original concept of the behavior of the *homo economicus* as a methodological convenience to be supplanted by more realistic behavioral assumptions. But the BF literature has taken a marked prescriptive turn, its purpose being to discover and remedy deviations from the EU axioms of rational choice. The thinking appears to be that if people do not behave according to the prescriptions of theory, then something is wrong with people and not with the theory. Thus, “irrational” human behavior must be modified.

The following five sections of this paper chart the transformation of the *homo economicus* in finance. First it was a useful simplification with a long history (Section I) but clearly a fiction (Section II). Then it became the foundation of modern finance as a necessity that must be there (Section III). Now it is becoming an ideal that ought to be there (Section IV).

There is a massive literature of commentary on and criticism of the *homo economicus*. All the same, few have addressed the question why not just the orthodox practitioners of financial economics, but even its most profound and prominent critics, have insisted upon this single-minded artificial construct of human nature as the sole foundation for scientific inquiry. And none have addressed the question why, when the limitations of the model have become too obvious to ignore, finance is insisting upon changing humanity to conform to it. In Section V we discuss the meaning of this firm and fatal attraction of financial economics to a fiction.
I. A Simplification

On November 10, 1619, René Descartes had a curious dream, which has had a profound impact on science (Davis and Hersh, 1986). Descartes woke up from his sleep, but science, including finance, which clearly didn’t exist at the time, continued to slumber, dreaming Descartes’ dream for nearly four hundred years.

The essence of this now famous dream was a unified method of scientific inquiry. Descartes in his Discourse on the Method of Rightly Conducting the Reason and Seeking Truth in the Field of Science later summarized its principles.

The first rule was never to accept anything as true unless I recognized it to be certainly and evidently such: that is, carefully to avoid all precipitation and prejudgment, and to include nothing in my conclusions unless it presented itself so clearly and distinctly to my mind that there was no reason or occasion to doubt it. The second was to divide each of the difficulties which I encountered into as many parts as possible, and as might be required for an easier solution. The third was to think in an orderly fashion when concerned with the search for truth, beginning with the things which were simplest and easiest to understand, and gradually and by degrees reaching toward more complex knowledge, even treating, as though ordered, materials which were not necessarily so (Descartes, 1960, p. 15).

At first blush, this all seems unexceptionable. Nevertheless, the beliefs underlying the method cannot be taken for granted. Perhaps the most striking attribute of Descartes’ method is its reliance on what might be termed a "mechanical" metaphor. Just as the performance of a machine is a simple sum of the functions of its individual subassemblies, nothing is either gained or lost from dividing something (a problem, the universe) into pieces or assembling something from pieces. This is explicit in his second rule that problems might be divided into parts, and the solution to the whole problem can be assembled from the solution of each of its component sub-problems. The mechanical metaphor is implicit in his third rule that the solution to a complex problem can be achieved by beginning with a simpler (hypothetical) problem and proceeding through a series of increasingly more complex problems until reaching a solution, and it is also implicit in his first rule in which he feels free to divide the universe into a subject (himself) and an object (everything else).

Descartes’ method presupposes a design of nature in the same way as there is a design of a machine, and the moral philosophers who preceded Adam Smith believed that since man is a part of nature, there must be a design of man. Thomas Hobbes applied Descartes’ method to the nature of man in a manner strikingly familiar to Friedman’s (1953) “The Methodology of Positive Economics,” seeking to understand man in his natural state, free of the complications caused by the institutions and customs of the societies in which man lived. Hobbes concluded that the nature of man was motion, and the consequences of this motion—
competition, distrust, and the pursuit of glory—we recognize in the self-interest that is the soul of the *homo economicus* (Myers, 1983). To Hobbes, man’s inherently self-interested nature was decidedly pernicious and had to be curbed by external authority, which is quite a contrast to the subsequent view of self-interest (shared by modern finance) as an admirable trait. The parallel between Hobbes’ motions of men and the motions of the physical bodies in the cosmos was of course not coincidental. It made sense at the time that if there were one grand design, both man and the universe he inhabited had similar natures.

Such an appeal to a natural order that bound man and the universe together served to reconcile self-interest with social-interest, thereby turning what for Hobbes had been a harmful force into a beneficial one. Just as the universe was designed so that all its parts worked together in harmony, so also must men have been designed to work together in society.

Alexander Pope captured this idea in a poem:

On their own Axis as the Planets run,
Yet make at once their circle round the Sun;
So two consistent motions act the Soul;
And one regards itself, and one the Whole.
Thus God and nature link’d the gen’ral frame,
And bade Self-love and Social be the same (Pope, 1896, p. 218).

Quite obviously, there was a strong element of imagination and faith, religious or otherwise, in this conclusion. In his familiar work on the division of labor in *The Wealth of Nations*, Adam Smith dealt with the reconciliation of self-interest and social welfare, but in a more realistic way, showing how the structures of societies and economies could result from the commonplace daily activities of the “economic man” as he (the economic man) went about satisfying his needs. But Smith’s “economic man” was a real, multi-dimensional person--he was not yet the *homo economicus* of today (Oakley, 1994).

The reconciliation of public and private interests with which we are concerned in this section was addressed by Jeremy Bentham, whose utilitarianism was a system of “mathematical morality” analogous to mathematical physics (Pribram, 1983). James Mill was probably the first to adopt Bentham’s *felicific calculus* and attempt to reduce political economy to deductive logic (Oakley, 1994). David Ricardo, however, has generally been given more credit (or blame) than Mill for purging political economy of history, philosophy, theology, and moral values (Minowitz, 1993), and treating man as an essentially dehumanized *homo economicus*, although not in such precise terms as Von Neumann and Morgenstern did later.
Ricardo sought to elucidate a natural state of being and not one applicable to individuals in a real economy. From the behavior of his *homo economicus*, he would be able to deduce the laws of the economy if it were free to operate according to a (not necessarily beneficent) order and equilibrium as nature intended (Wilk, 1996). Ricardo would have expected individuals to behave this way if they were free to act in accordance with their inherently self-interested nature to “truck, barter, and exchange one thing for another,” as Adam Smith had put it. Accordingly, if Ricardo had a normative task at all, it would have been to guide public policy and not individual behavior.

J.S. Mill was probably the first to explicitly label the *homo economicus*, and to define him:

> as an individual who is “determined by the necessity of his nature to prefer a greater portion of wealth to a smaller,” and moreover, is “able of judging of the comparative efficacy of means of obtaining the possession of wealth” (Pribram, 1983, p. 173).

Mill was well aware that this was a simplification to facilitate economic analysis, which position is perfectly consistent with Friedman’s a century later. And for Friedman, Von Neumann and Morgenstern’s more elaborate, but still fundamentally the same, EU axioms of choice under uncertainty were no more than convenient simplifications for developing theory (and setting social policy).

There is still considerable controversy surrounding the methodologies of Smith, James Mill, Ricardo, and J.S. Mill, including who was responsible for the *homo economicus* and what they thought could be done with such a simplified model of human nature (Hollander, 1985; Aksoy, 1991). What we have tried to present in this section is a temperate consensus view. The specifics however, are not so important as the general conclusion that the *homo economicus* was a product of the classical economists and had its antecedents in the moral philosophers who preceded them. But there is a missing piece to this story that has enormous implications for the methodology of finance today. How is it that the moral philosophers’ “nature of man” so closely resembles the economists’ “convenient simplification of man” that it is not really possible to tell when the first became the second?

As we have outlined it in this section, the *homo economicus* has a long history, rooted in the faith and naïve understanding of the physical sciences and of society of the 17th, 18th, and early 19th centuries, which leaves us in a very puzzling position with regard to finance in the 21st century. The intellectual underpinnings of modern finance’s dominant model are at least two hundred years old. The research methods in economics (and finance) that are often criticized today as poor imitations of physics arose from an earlier (and long since
discredited) belief that the nature of man and of the universe were the same and demanded the same methods for their scientific study. A reasonable interpretation of this might be that the moral philosophers, the classical economists, and the neo-classical financial economists all sought simplifications of similar observations of human behavior. And it should surprise no one that although their needs differed, their conclusions were the same.

A deeper interpretation of this is that it is often possible to find whatever one is looking for, and the moral philosophers, the classical economists, and the neo-classical financial economists were all implicitly looking for the same thing--a Cartesian man whose rational behavior made sense. If this is true, then there have always been strong normative underpinnings of the \textit{homo economicus}, an issue to which we will turn later in the paper.

In the following section we shall see that there has been no shortage of challenges to the meaningfulness of the \textit{homo economicus} in other disciplines. And in general, there has been no shortage of commentary on the limitations of a research methodology relying solely upon the \textit{homo economicus}. How is it then that finance remains addicted to what, perhaps erroneously and unfairly, has become known as the “Ricardian vice”?

\section*{II. A Fiction}

There is a vast literature from a variety of disciplines that addresses the differences between the assumptions of economic theory and the way choices are made in the real world. March (1978) has described this chronic discrepancy between rational choice models and observed choice behavior as “theoretical puzzlement with respect to the simplicity of decision behavior.”

Choice experience, as depicted by rational choice theory, can be broken into two basic steps that March (1978) labels guesses. In the first guess, a chooser must anticipate future consequences associated with an array of current choice alternatives. Second, the chooser must guess or imagine what his or her future preferences will be for the anticipated consequences associated with the current choice alternatives.

Fundamental discrepancies between normative choice theory and actual choice experience have been noted in both steps. Simon (1955) critiques the first step of rational choice experience that posits a chooser estimating future consequences based on an omniscient possession of information about all possible alternatives. Simon argues that any attempt to understand how people actually make choices has to recognize the information
processing limitations (in terms of computational capability, organization and utilization of memory, etc.) of humans. Simon’s work suggests that the limited cognitive capacity of a decision maker combines with characteristics of the decision environment to result in choices where choosers are less than omniscient; such conditions are termed “bounded rationality” and contrast with rational choice models assuming possession of full information by the chooser (“omniscient rationality”).

With respect to the second step of rational choice theory, March (1978) has critiqued influentially the assumption of instrumental rationality, or what he terms “calculated rationality.” Based on empirical findings, March argues that people and organizations make choices under conditions “where goals are unstable, ill-defined, or apparently irrelevant” and goes on to question the validity of rational choice researchers’ assumption of the “role of clear prior purpose in the ordering of human affairs” (Ibid., p. 595). March points out the philosophical conundrum posed by suspending the assumption of calculated rationality that presumes choices are made in light of discursively entertained preferences. To wit, how is it that a person can choose among alternatives in the absence of a priori goals or preferences? This paradox is similar to that faced by the Cheshire cat who, when asked by Alice which path to take, replies that any path will suffice since Alice has no particular final destination in mind. By pointing out that people make choices in conditions where they have vague or nonexistent preferences, March fundamentally questions the bedrock logic of calculated rationality that is the sine qua non of rational choice theory.

March explicitly notes that bounded rationality and goal ambiguity should not be considered as evidence of stupidity or errors that should be corrected by transforming people to fit rational choice theory. Moreover, he calls into the question the efficacy of tinkering with the assumptions of rational choice theory and suggests that the most fruitful response may entail a paradigm shift drawing on theory from ethics, criticism, and aesthetics. Nonetheless, after all of the reservations he has about rational choice theory, he offers modifications for rehabilitating it because he is unaware of promising alternatives to rational choice.

Mitchell and Beach (1990) echo the skepticism over rational choice and expected utility models. They broadly characterize two primary responses to this growing disenchantment. First, they point out efforts, such as Kahneman and Tversky’s Prospect Theory (1979) (PT, subsequently), to make the theory fit the data by modifying definitions or assumptions. Second, they point out a fundamentally different approach to decision making
that focuses on psychological mechanisms involved in choice. For example, the model of Constructive Consumer Choice Processes (Bettman, Luce, and Payne 1998) posits the psychological construct of heuristics (rules of thumb) to incorporate the notion of bounded rationality. To address the issue of goal ambiguity, this model posits the existence of information processing metagoals that subconsciously govern heuristic application and choice. In short, the model of Constructive Consumer Choice Processes focuses on how people choose among heuristics, which then are used to choose among choice alternatives. Hence, although consumers may not show overt signs of processing large amounts of information or of possessing clear goals, internal psychological mechanisms are responsible for helping consumers choose how to choose.

While Mitchell and Beach consider the psychological approach as an entirely new paradigm for conceptualizing decision making, Taylor (1993) points out that traditional rational choice theory and rival psychological models actually have more in common than is generally recognized. These commonalities stem from Cartesian influence that privileges disengagement (or objectivity) and mechanism over the ordinary ways engaged people experience the world. Rational choice theory is premised on the primary Cartesian position asserting that how we commonly experience the world is not really how things work. And models of choice premised on psychological constructs such as heuristic processing view choice as the functioning of a mechanism in a body. Furthermore, the objectivist and mechanistic positions work in cahoots to prop up rational choice theory. The mutually reinforcing relationship between rational choice theory and psychological models that have evolved to account for discrepancies between rational choice theory and observations of actual choice experience is captured well by Taylor:

> If the picture can be made to seem implausible on the phenomenological level (and this is not hard to do), one can be reassured by the reflection that it all has to be explained mechanistically on a more basic level anyway and at that level that picture must be right. Reciprocally, the force of otherwise powerful arguments against mechanism is neutralized by the thought that in some sense we “know” that thinking is all information processing anyway, so surely some computer-based explanation must hold in the last analysis (Ibid., pp. 324-235).

Thus, March’s (1978) inability to transcend the rational choice paradigm is understandable in light of the mutually reinforcing powers of disengaged and mechanistic views. His suspicion, however, that other paradigms might offer a viable alternative theory is warranted. As outlined by Dreyfus (1992), Taylor (1993), and Etzioni (1988) and preliminarily implemented by Mitchell and Beach (1990), philosophy of perception and the mind provided by thinkers such as Heidegger (1962), Merleau-Ponty (1962), and
Wittgenstein (1968) provide a basis for transcending dualistic and mechanistic thinking. A notable framework that synthesizes many elements of this philosophy is Bourdieu’s oeuvre on practical experience (e.g., Bourdieu 1977, 1990). Some implications of this alternative understanding of choice are:

1. Choice is experienced by embodied agents located in concrete landscapes, as opposed to the unsituated and disembodied observer presumed in rational choice theory.

2. Choice plays out while agents are in the temporal flow of living, in contrast with the posture of the rational decision-maker possessing a time-elongated relationship to choice and continuously entertaining both the present and the future.

3. Choices occur in sociohistorically-situated space, as opposed to the sociohistorical vacuum in which rational choice theory depicts choice taking place.

In short, there is an extensive literature, spanning the same half-century as modern finance and financial economics that has acknowledged and studied the clear differences between rational choice and actual choice. Some of this literature, though intended to be a radical departure from rational choice, is actually complementary to rational choice and rooted in the same philosophical tradition. While such alternatives may represent incremental improvements in capturing some semblance of choice, its terminology nonetheless privileges the simplification. More radically, there exist post-Cartesian philosophies of the social world that have flourished and been refined for over a half century. Yet this literature has not only been ignored, but also denied by financial economics. For most of its life, finance has looked only to the *homo economicus* as an explanation of financial phenomena.

### III. A Foundation

In a recent paper Kalay and Michaely (2000) “re-examine” three separate and similar theories of dividend policy posited during the 1970’s that connect dividends to the corporate and/or individual tax code. Without going into a critique of the particular statistical models they chose and especially into the critical issue of the relevant time-horizon for investment decision-making, here is their conclusion after 21 pages of elaborate analysis of vast quantities of data:

Our empirical evidence—time-series return variations and no cross-sectional return variations—is not explained by known tax models. It could very well be that these empirical
findings are somehow related to a more complex [our italics] theory of tax effects, yet to be developed (ibid., p.73).

This is one among many papers we could have selected as an illustration of yet another study, built on the tenets of the methodology of modern finance, failing to link the changes in tax laws to changes in the dividend policies of firms. The tax effect of dividends is one of the most obvious economically rational hypotheses in corporate finance, but once again it could not be shown to be a significant determinant of corporate dividend policy. A sensible conclusion to be drawn from this and other similar studies would not be for one to ratchet up theory complexity several more notches, but that one should consider possible reasons why dividend policies do not follow the rationality of the _homo economicus_. Yet surprisingly, the advocates of the dominant “rational” paradigm continue to argue that no empirically-supportable “rational” model having been found in forty years to explain the “rationality” of dividends is still not proof that such model cannot or will not be eventually found.

The dividend puzzle in general and the problematic effects of the tax code on dividends in particular testify to the dominance of the _homo economicus_ in finance. Why is it that Kalay and Michaely and many others persist in using an approach that has failed them so often? Why do they continue to believe that a more complex variation of an already complex formulation might be the way to make progress? Why is it that so many financial economists resist any new alternative paradigm such as BF? And why is it that even the proponents of this very alternative paradigm are willing to accept the _homo economicus_ both as a yardstick and as a goal, when he is a non-existent mythical construct that was invented not to describe reality but to facilitate model-building?

Before we attempt to answer these questions, and we must admit that we are not certain we can, it is useful to continue our story of the development of this _homo economicus_ and his emergence as the only form of behavior in academic finance. This stage of his development is what we have referred to in the introduction as the transformation from “a model of” to “the model of.”

Historically, the EU axioms describing the investment behavior of an individual under conditions of risk or uncertainty³ were the basis for what is called today modern portfolio theory, a normative algorithm invented by Harry Markowitz (1952, 1959). In his first paper
Markowitz (1952) argued that the investment in the asset having the highest expected return is not supportable either theoretically or empirically. Then he proceeded to develop an algorithm based on the EU axioms by an insightful combination of quadratic programming and classical statistics of expected returns and the variance-covariance matrix of expected returns. He called the resulting solution set, consisting of an infinite number of portfolios in the expected return-variance space not dominated by any other possible portfolio (also called second degree stochastic dominance), the E-V (for Expected Return – Variance) efficient set. In private correspondence responding to an inquiry by the first author, Markowitz remarked that:

> I was taking Tjalling Koopmans course on "Activity Analysis" a.k.a. linear programming. Koopmans spoke of efficient (i.e., undominated) and inefficient (i.e., dominated) allocations of resources. Having this terminology in my head, I distinguished efficient versus inefficient portfolios and EV combinations.

This introduced the term “efficiency” into finance that later under Fama (1965, 1970), in the form of the efficient markets hypothesis (EMH, subsequently), gained extraordinary significance along with the capital asset pricing model (CAPM, subsequently) as one of the two pillars of modern finance. Because members of the efficient set were diversified portfolios (with one exception—the maximum expected return portfolio that, by definition, can have just one asset), Markowitz concluded that diversification both as a serious prescription and as folksy wisdom (“Do not put all your eggs in one basket”) makes sense. In no time, the efficient set was commonly referred to as the “frontier of efficiency” or “the efficient frontier.”

What must be noted, however, is the fact that the Markowitz algorithm is a normative model that requires relatively few assumptions and makes no direct reference to the behavior of capital markets. A normative model can be useful for one who is content to apply it without making any assumptions or having any illusions regarding the behavior of all economic agents whose activities determine what is observable in financial markets in terms of prices and quantities (volume). Also, at the end of the day, the only test of a normative model is its performance, which is the benchmark for its use by a rational decision maker.

This distinction of a normative model vis-à-vis a positive representation of every agent’s behavior is very important, because finance academics personify markets as if somehow markets “behave”. This is done, of course, for the sake of exempting their models.

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3 There is a statistical distinction between risk and uncertainty and even between uncertainty and uncertain uncertainty. Nevertheless, for the sake of simplifying the discussion here, we use risk and uncertainty.
from considering the behavior of agents, both individually and collectively, and, consequently, to replace complex sociological and psychological intricacies with simple price-volume data.

Then two things happened almost simultaneously. First, Sharpe (1964), Lintner (1965) and Black (1972), by making a set of additional and markedly unrealistic assumptions, created the now ubiquitous CAPM. Second, Fama introduced and defined informational market efficiency. Later Fama (1998) was to make it unmistakably clear that the CAPM and the EMH go hand-in-hand. But before we discuss the substance of this transformation moving modern finance’s *homo economicus* from *a* to *the* model of behavior, both the methodological and ideological implications of this move need to be clarified.

Observe that at the heart of modern finance/financial economics is Friedman’s instrumentalism, the most binding aspect of which is the usefulness of a model as long as its forecasts are a “sufficiently good ‘approximation’ for the purpose at hand,” where the purpose at hand is public policy. Sharpe (1964) justified explicitly (and all the others who followed him, implicitly) his mechanism of capital asset pricing by the model’s internal consistency and by claiming that:

> . . . the proper test of theory is not the realism of its assumptions but the acceptability of its implications, and since these assumptions imply equilibrium conditions which form a major part of classical financial doctrine, it is far from clear that this formulation should be rejected (ibid., p. 434)

In effect, this contention relaxes still further the token Friedmanian requirement of sufficiently good approximations, i.e. forecasts.

The final ontological makeover came from Fama’s (1965, 1970) assertion of capital markets’ informational efficiency, which necessitates the behavior of the *homo economicus*. Recall that Markowitz’s use of the term “efficiency” came from the application of the mathematics of operations research and in a technical sense implied second-order stochastic dominance. Following Fama, the term acquired a moral, quasi-religious, connotation. If markets were efficient, then tinkering with them would make them inefficient, which would be an unforgivable sin in a society that strives for efficiency (the synonyms of which are interchangeably.

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4 One of these, the assumption of homogeneous expectations by all market participants as to the expected returns and the variance-covariance matrix of expected returns of all capital assets (stocks, bonds, baseball cards, *objet d’art*, moon rocks, etc.), seemed so totally unrealistic to the first reviewer of Sharpe’s (1964) now classical paper that he termed it preposterous. The editor of the *Journal of Finance* concurred, and the paper was summarily rejected. With the change of editorship, Franco Modigliani hand-carried the paper to the newly appointed editor, who published it without hesitation.
operative, businesslike, useful, industrious, skillful, expert, and successful). So, the next three decades were directed toward research that addressed whether markets were indeed efficient, without ever having reached a universally accepted conclusion. The end result, however, was to cement together market efficiency and the *homo economicus*. Both were so desirable that they had to exist, and if somehow someone discovered that they didn’t, it was necessary to see that they did. And of course only a single methodology, employing and implying both market efficiency and the *homo economicus*, was permitted. The question of efficiency became the ontological boundary beyond which no researchers were supposed to go, boldly or otherwise.

The transformation from “the model of” to “the model for” was less than elegant, however. In the course of roughly 20 years convincing statistical evidence has been surfacing that essentially refutes the validity of the paradigm, and indirectly, the existence of the *homo economicus*. Jensen, one of the ardent promoters of the idea of market efficiency, is interestingly among the first to cast serious doubts in the academic finance literature about market efficiency and the existence of the *homo economicus*. As the editor of the *Journal of Financial Economics*, he dedicated an entire issue to “anomalies.” In the introduction of this special issue, among other things, this is what Jensen (1978) had to say about market efficiency.

Yet, in a manner remarkable similar to that described by Thomas Kuhn in his book, *The Structure of Scientific Revolutions*, we seem to be entering a stage where widely scattered and as yet incohesive evidence is arising which seems to be inconsistent with the theory. As better data become available (e.g. daily stock price data) and as our econometric sophistication increases, we are beginning to find inconsistencies that our cruder data and techniques missed in the past. It is evidence which we will not be able to ignore (ibid., p.95).

In spite of all this, and other evidence included in the volume Jensen concludes:

Unlike much of the ‘inefficiency literature’ of the past, each and every one of these studies is a carefully done scientific piece. Each of the authors displays in varying degrees a commonly held allegiance to the Efficient Market Hypothesis witness the general reluctance to reject the notion of market efficiency.

Viewed as a whole, however, the studies provide a powerful stimulus and serve to highlight the fact that there are inadequacies in our current state of knowledge. My reaction to this is one of excitement and enthusiasm. I have little doubt that in the next several years we will document further anomalies and begin to sort out and understand their causes. The result will not be an abandonment of the ‘efficiency’ concept, nor of asset pricing models. Five years from now, however, we will as a profession have a much better understanding of these concepts than we now possess, and we will have a much more fundamental understanding of the world around us (ibid., p. 100).

These do not read like unbiased comments. According to Jensen, previous studies challenging market efficiency were not “carefully done scientific pieces,” but those (articles)
in the volume, done by those (researchers) who have an “allegiance” to the EMH and a “general reluctance” to reject it, are. Also, the fact that the supporters of the EMH found inefficiencies is a testimony of their academic honesty and that their work must be trusted more than the work of the EMH critics. The implication is that only EMH supporters are capable of doing top-notch scientific work, and only inferior researchers would not have a prior belief in the EMH. In other words, contrary to what Jensen promises at the outset, there is no danger that a “Kuhnian revolution” is in the offing.

Three significant developments have followed the “discovery” (or official recognition) of anomalies. One (but not necessarily the first) is that all evidence pertaining to the questioning of market efficiency is termed an anomaly, thereby reducing such evidence to a lower level than the pedestal on which the efficiency paradigm stands (Frankfurter and McGoun, 2001).

Second, a deluge of papers, starting with Banz (1981), appeared that show the existence of “effects” that are inconsistent with either the CAPM or the EMH. If such effects systematically can be proven, then the CAPM is incomplete at best and probably even incorrect. The ultimate rejection of the CAPM eventually came from Fama, who in Fama and French (1992) concluded that the CAPM’s beta has no predictive power and that an accounting ratio (something that can be at least partially manipulated by management) is a better indicator of risk than beta.

The third development is the importation of the PT of Kahneman and Tversky (1979), and Tversky and Kahneman (1990) to finance. This last development is perhaps the most significant, because it is both a refutation of the existence of the homo economicus as we know him as well as a new research program, over- and/or under-reaction, that evolves from it (DeBondt and Thaler, 1985, 1987). This is, in effect, the birth of BF. This development also creates a real-world investment strategy, called “contrarian”, meaning contrary to the tenets of the CAPM/EMH (Dreman, 1998).

It is Fama (1998) again who changes course and from his condemnation of the CAPM of just six short years before turns against BF. In this rather puzzling paper Fama in a dramatic fashion declares a method, a methodology, and the EMH/CAPM as one and inseparable, and that unless and until a new and better methodology is found, BF is neither an alternative nor a potential substitute for the EMH.

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5 Small firm, large firm, Monday, end-of-the-week, end-of-the-month, end-of-the-year, January, Yom Kippur, liquidity, neglect, etc., too numerous to mention.
IV. A Necessity and An Ideal

Now that there is more-or-less officially such a thing as BF, it has become necessary to be more specific about what “non-behavioral” or “traditional” finance has been. Following are three themes of behavioral finance, identified by Shefrin (2000) contrasting BF to what had been finance before:

1. . . . I assign the label *heuristic-driven bias* to the first behavioral theme. In contrast, traditional finance assumes that when processing data, practitioners use statistical tools appropriately and correctly.

2. . . . I assign the label *frame dependence* to the second behavioral theme. In contrast, traditional finance assumes *frame independence*, meaning that practitioners view all decisions through the transparent, objective lens of risk and return.

3. . . I assign the label *inefficient markets* to the third theme. In contrast, traditional finance assumes that markets are efficient. Efficiency means that the price of each security coincides with fundamental value, even if some practitioners suffer from heuristic-driven bias or frame dependence (Shefrin, 1999, pp. 4-5).

Although Shefrin might be considered a proponent of BF, there is a marked difference here in the words used to describe traditional finance (appropriate, correct, transparent, objective, fundamental) and behavioral finance (biased). This certainly echoes the dialectical relationship between “normative” and “descriptive” choice experience noted by March (1978) and discussed in Section II, in which the former was consciously deliberative, sophisticated, and important, and the latter was subconscious, crude, and unimportant. Later, we will consider the normative implications of such descriptions, but for now we will concentrate on the researchers’ expectation that “rational choices” must be there, which notion we introduced in the preceding section.

This dialectic is all too common in the rhetoric of the opponents of BF. Fama marginalizes BF by referring to it as the “anomalies literature”:

Since the anomalies literature has not settled on a specific alternative to market efficiency, to get the ball rolling, I assume reasonable alternatives must choose between overreaction and underreaction. . . . Two recent papers . . . present behavioral models that accommodate overreaction and underreaction (Fama, 1998, p. 284).

And associating behavior and models of behavior with over-reaction and under-reaction; that is, inappropriate, abnormal, unexpected, etc., reaction, makes sure that behavior according to

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6 The reference is clearly to “modern” finance. The irony of Shefrin calling modern finance “traditional” is in that that pre-MM finance (not financial economics) was called traditional after the publication of the two indifference propositions of Modigliani and Miller (1958) and Miller and Modigliani (1961). Clearly, labels can be attached to diametrically opposite concepts from one era to another.
modern finance is considered the opposite reaction (appropriate, normal, expected, etc.). The very term “behavioral finance”, accepted by proponents and opponents alike, however, implies that it concerns how people really do “behave” and that the rational behavior of traditional finance is a fiction.

This entrenchment of the *homo economicus* as the behavioral model upon which to build finance theory began long before finance theory in the classical period of economics. Oakley (1994) and others have attributed this to an attempt to emulate the physical sciences, thereby placing political economy on a higher plane (more objective and more quantitative, hence more accurate) than the other moral sciences. But the *homo economicus* was more than a means by which economics could imitate physics. As outlined in Section I, there was a long intellectual tradition in which economics and the physical sciences were both regarded as part of a natural order to the universe, and it would have made perfect sense for the methodologies of both to be the same. Why then was the *homo economicus* retained in the late 19th century by which time this view from the 17th, 18th, and early 19th centuries had been abandoned?

We find a telling contradiction within the masterwork of the noted economic historian Joseph Schumpeter (1954). Consider first his discussion of David Ricardo, in which he introduces the term “Ricardian Vice” that we already mentioned.

[Ricardo’s] interest was in the clear-cut result of direct, practical significance. In order to get this he cut that general system to pieces, bundled up as large parts of it as possible, and put them in cold storage—so that as many things as possible should be frozen and “given.” He then piled one simplifying assumption upon another until, having really settled everything by these assumptions, he was left with only a few aggregative variables between which, given these assumptions, he set up simple one-way relations so that, in the end, the desired results emerged almost as tautologies. . . The habit of applying results of this character to the solution of practical problems we shall call the Ricardian Vice (Schumpeter, 1954, page 473).

Now consider his later discussion of equilibrium analysis following the marginalist revolution.

It was during the period under survey [1870-1914] that the inevitable happened: mathematical methods of reasoning began to play a significant and indeed decisive role in the pure theory of our science. . . . Did space permit, it could be shown instructively that a great part of the controversies of that period consisted simply of controversies between people who lacked a powerful tool of thought and people who possessed it (Schumpeter, 1954, page 954).

The obvious connection between these two passages, which Schumpeter doesn’t seem to have made, is that in order to reduce economic phenomena to a form in which it is possible to apply mathematical reasoning, it is necessary to pile one simplifying assumption upon another. What we appear to have been left with after the marginalist revolution is the same *homo economicus* as we had before it. Whereas it had been a simplification by which we
might discover the natural laws of the workings of the economy, it was now the mathematical logic the economy had to follow.

The key question, of course, is whether the same simple model was optimal for both purposes or whether the classical model was responsible for the neo-classical one. While Friedman’s positivism might provide us with a defensible rational for the *homo economicus*, would it have taken the same form without the long-discredited intellectual tradition out of which it was born? This shift in justification can perhaps explain why the *homo economicus* is still with us today in finance. As long as we demand mathematical reasoning, we are stuck with the simplifications that mathematics can deal with. But why do we demand mathematical reasoning?

There has been no dearth of commentary on the use of mathematics in finance. A recent book (Szostak, 1999) incorporates most of the arguments concerning the use of mathematics in economics, and the references in Szostak’s book can direct the reader to the earlier supporting literature. Of course the most obvious argument, and the one most likely to be used by its proponents, is:

1. Mathematics works better than other methods. If it didn’t, we wouldn’t use it, and no one would pay for it.7

But our example in the previous section showed that with regard to dividends, one of the most important and most puzzling problems in finance, it doesn’t work. And there are countless other problems in finance which have resisted mathematical methods. Even for those problems which mathematics appears to have successfully solved, most notably the Black&Scholes/Merton option pricing model, there is no way to tell whether it works or whether it is a self-fulfilling prophecy. It may only work because practitioners can be convinced that it works.

This leads to five more arguments for the use of mathematics which relate to the sociology of the profession of financial economics research:

2. Mathematics as a logical system is the highest form of reasoning. Mathematical skills are considered superior to verbal or other skills, and mathematical virtuosity is a way to assert of intellectual dominance.

3. Mathematics as a formal system has very precise rules. It is (deceptively) simple to identify who is right and wrong and to decide who has talent and deserves the rewards of the profession.

7 This argument is similar to the proverbial $20 bill that cannot be on the ground, because if it were there someone would have picked it up already.
4. Mathematics is a specialized language that empowers those who speak it to control those who don’t. The “experts” can sell their knowledge to those who are impressed by it, yet do not understand it, and the customers are not equipped to evaluate the quality of the product.

5. There is positive feedback between the use of mathematics in the practice of a profession and the importance of mathematics in the preparation for that profession.

6. Mathematics enhances the stature of a profession as a whole as well as that of its individual practitioners.

There is undoubtedly a measure of truth in these arguments, but on the whole, they are perhaps a little too cynical. They imply a not only unacknowledged but also unnoticed “conspiracy” among financial economists to deceive not only the public but also themselves. We have some colleagues who certainly fit this model, but we have many other colleagues who we believe to be intellectually honest and sincere in their desire to expand our knowledge and understanding of finance. A wholly self-serving system may still be difficult to change, but it would be hard to keep it secret.

A somewhat less cynical reason for the use of mathematics by financial economists is:

7. Mathematics as a Platonic object has often been described as beautiful. One can find aesthetic pleasure working in such a domain.

This is how Merton (1995) (the fils) describes the derivatives literature, which he might even argue is what en toto finance is. Although this may explain why financial economists do what they do, it does not explain why society not only allows them to do it, but also rewards them very handsomely for doing it. Mathematics must benefit its consumers as well as its producers in ways that are not so mercenary as the explanations in the preceding paragraph. Two of these are:

8. Mathematics is a universal language that transcends cultural barriers. Managerial prescriptions from mathematical theories appear free of ideology and look to be universally applicable.

9. Mathematical assumptions are conservative and reinforce the status quo.

With these two explanations, we have come close to the core reason for why mathematics, and by extension the homo economicus, have such a firm hold on the methodology (inclusive of methods) of finance research. But there is one remaining reason that incorporates these two that we believe is the best:

10. Mathematics supplies simple, certain answers in a chaotic, uncertain world. This is comforting.

With this reason, we have come full-circle to Descartes, with whom we began the discussion of homo economicus. The “Cartesian Anxiety” is the fear of chaos – the fear that not only does no one know how the world works, but that there are no rules by which the world works.
for anyone to know. In narrower terms, we have to believe that the financial system makes sense to someone and that someone is “manning the controls” while we sleep to ensure that nothing catastrophic occurs. A mathematics that is perfectly logical, has precise rules, and is free from ideology is the ideal antidote to the Cartesian Anxiety. Paradoxically, it also serves as a cloak for the ideological base on which modern finance’s methodology is erected.

Of course there can’t be hard, statistical “evidence” (in today’s political lingo “a smoking gun”) to our contention. We can point to nothing more solid than the comments numerous colleagues have made to us to the effect that “once we let go of the rationality assumption, where are we?” Yet, naturally, there is an abundance of indirect evidence such as the crucial importance of the word “efficiency” that we described in the preceding section as reassurance that the system works. But perhaps the best evidence is that where finance finds behavior that does not conform to our rationality assumptions, it has taken upon itself the task of correcting such behavioral mistakes.

To illustrate how rational choice has become an ideal, consider a recent survey of BF, written by a prominent researcher and supposed proponent of BF to explain it to finance practitioners. The intent of Shefrin’s (2000) *Beyond Greed and Fear: Understanding Behavioral Finance and the Psychology of Investing* is unmistakable from the very beginning.

Behavioral finance is the application of psychology to financial behavior--the behavior of practitioners. I have written this book about practitioners, for practitioners. Practitioners need to know that because of human nature, they make particular types of mistakes. Mistakes can be very costly. By reading this book, practitioners will learn to

• recognize their own mistakes and those of others;
• understand the reasons for mistakes; and
• avoid mistakes (Shefrin, 1999, p. 3).

Popular business books are intended less to explore the nuances of an issue than to instruct the purchaser, who must feel as if he or she has received value for his or her time and money invested in the book. So it is not surprising that this book promises up front that the reader will be able to do something better as a result of reading the book. But that something need not be a prescription of “rational choice.”

Why should all of this matter? What does it matter that the assumption of the *homo economicus* has become so much a part of mathematical finance that there are no recognized alternatives? What does it matter that BF, which ought to eschew the *homo economicus*, preserves him as a goal instead of an assumption and continues to rely as heavily on the use
of mathematics as traditional finance? So far in the paper we tried to explain how and why this all has happened. In the final section we aim to explain the implications.

V. An Ideology

A subtle but critical distinction can be made between markets as a cultural phenomenon and markets as an economic phenomenon. This distinction is the basis of an edited volume by the anthropologist James G. Carrier (1997), in which he refers to the former as the “market model”, a somewhat confusing label for us here given its common use in finance to describe a simple linear forecasting model for asset prices. In Carrier’s words:

[T]his collection is concerned not with economy itself, but with popular representations of it. . . [T]hose representations affect how people in the West understand their world, affect the symbols people invoke to persuade each other, affect how people act in the world, and those representations have these effects because people adopt, espouse and respond to them, not because they are true (Carrier, 1997, page xi).

The key point of this is that the “market” is not something that is outside us; rather, it is us. How we perceive it, how we want it to be, and how we study it are inextricably intertwined. There is no such thing, therefore, as value-free financial economics. Indeed, individual choice itself, the existence of which we have taken for granted thus far throughout this paper, is value-laden. It presupposes an answer to the long-standing debate between structure and agency. Individual choice means that agents are independent and free to act or to choose, limited only by their capacity (wealth) and scarcity (reflected in prices). Further, it means that individuals are not dependent upon each other and are unconstrained by culture or values, by social structures or institutions, or by history. Choice itself is a moral good as well as an economic good. Had we not seen ourselves, and admired ourselves, as individuals who were free to choose, we would never have chosen to investigate the economy as if it were driven by the individual choices of the homo economicus. The homo economicus would not have been conceived had we not both expected and wanted to discover that we were somewhat like him.

On an economic level, the Von Neumann and Morgenstern EU axioms for rational choices under conditions of uncertainty were methodological simplifications. Just how they simplify and how much they simplify has not been so obvious to finance researchers, but it has been obvious to others for a long time.

. . . Weber then lists four types of action, only two of which are labeled rational, and adds that most actions are of more than one type. The four are the instrumentally rational, the expressively rational, the traditional, and the affective. Of these, the idea of [instrumentally
rational] is readily grasped, being the familiar economists’ notion of action which embodies the choice of the likeliest means to a given end. . . Action is [expressively rational] when the goal is so dominant for the actor that it drives out calculation of means and consequences. . . Traditional action is defined as “the expression of settled custom” and glossed as often “simply a dull reaction to accustomed stimuli”. . . [Affective action is] where the agent is prompted by a simple, unreflective desire (Hollis, 1987, page 8).

As we have argued, finance goes farther than Von Neumann and Morgenstern by not only making methodological simplifications, but also turning them into a normative prescription. In light of Weber’s classification, for finance to be normative, it must exclude actions that are in whole or in part expressively rational, traditional, and affective. Then for instrumental actions, it must identify the ends (by assuming preferences/values) and specify the calculation (by assuming information/expectations). With its objective function that the goal of any financial decision is to maximize wealth, that it is a purely instrumental means for acquiring whatever else one desires, this can be done. How is it possible to deny that wealth is a suitable preference? Since wealth can presumably be used for moral purposes, it is in the use of wealth where morality ought to enter the calculation.

Yet, the so-called “rational choice” process by which wealth is acquired is not morally neutral. Saul (1992) points out the problems with “reason” and the structures that build on reason.

The development and control of intricate systems, for example, has become the key to power. The judgmental side of Descartes has come to the fore. It is answers we want—simple, absolute answers where, in reality, there is great complexity. An obsession with the true versus the false leads us to artificial solutions as reassuring as the old certainty that the world was flat. An obsession with efficiency as a value in itself has driven large parts of our economies into chaos (ibid., p. 20).

Finance in general and finance research in particular are such reasonable structures, and the purpose of both is to ensure the efficient functioning of the system independent of real matters. Simple truths are preferred to the puzzling complexities of the real world in which truth is more elusive if it exists at all. The system functions much better with something that can masquerade as an absolute truth and without common sense and morality. What Saul says about technocrats is applicable to both financial managers and finance researchers, and echoes the quotation from Adam Smith with which this paper began:

They seem actually to believe that their definitions of the world will become both real and permanent simply because they are the result of applied logic. When these formulae refuse to stick, the technocratic mind, rather than deal with failure, simply wipes the slate clean and writes a new definition. They are, in that sense, slaves of dogma (Ibid., p. 108).

The essence of management education in general and academic finance education in particular is not knowledge as the term would have been interpreted in the context of a
classical or liberal arts education, but knowledge as a rational method \((de \ facto, \ not \ education \ but \ training)\). Management education is about how to get something done, whatever it may be, and presumably this can be taught. Although it may be much more sophisticated and much finer in nuance, the idea underlying management education is the same idea underlying Taylor’s scientific management at the turn of the century, and one way of looking at the behavior of the \textit{homo economicus} is as a form of Taylorism – to behave in this way will rationalize the system.

The student who succeeds best at this game [of management education via the case method] invariably has an aptitude for abstract structures combined with an aggressive personality. Intelligence in this situation consists of a combination of analytical skills, untutored ambition and banal materialism. Creativity—which leads to new products—is not rewarded. Imagination—which enables the businessman to develop markets and sell—is also absent. And there is no hint that the values of society might be taken into consideration. . . .

Imagination, creativity, moral balance, knowledge, common sense, a social view—these things wither. Competitiveness, having an ever-ready answer, a talent for manipulating situations—all these things are encouraged to grow. As a result amorality also grows, as does a confusion between the nature of good versus having a ready answer to all questions. Above all, what is encouraged is the growth of an undisciplined form of self-interest, in which winning is what counts (Ibid., p. 121).

This is the educational background of finance professors, and it is also the education that they provide to their students. Thus, there is no surprise that these same qualities manifest themselves in the research and will go on until a scientific revolution surely will not come. This state of affairs will not change until the education of finance academics changes and a new research program will appear.

It is one thing for finance research to engage in discourse on the gaps between how people actually experience the world and the constructed, ideal, exceptional \textit{homo economicus}. It is quite another to insert the \textit{homo economicus} into all of us and conclude that any actions falling short of this ideal are errors or anomalies. The latter approach replaces a human form of life with a scientifically constructed form of life. Taylor (1993) notes how objective theories of how people mistakenly experience the world have the potential to “regestalt” experience. Thus, the \textit{homo economicus} is not just an instrumental fiction, but he possesses the potential to create himself.
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