RESISTANCE IS FUTILE: THE ASSIMILATION OF BEHAVIORAL FINANCE

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Abstract

In this paper we compare and contrast modern finance (the de facto ruling paradigm of financial economics) with what is being called (most of the time) behavioral finance, and some time "the anomalies literature." The faithful of the ruling paradigm have marginalized behavioral finance by making it the "anomalies literature." But even the supposed proponents of behavioral finance are marginalizing themselves by clinging to the underlying tenets, forms, and methods of what is now called modern finance. They have allowed it to set the terms of the debate and made it the benchmark against all finance is not only judged, but also labeled "finance." But finance research is subject to the same "mistakes" that behavioral finance attributes to practitioners, and it is these same "mistakes," perhaps more than the fierce attacks of the supporters of the ruling doctrine that are preventing behavioral finance from emerging as a new paradigm. In effect, the mere failure of behavioral finance is proof of its veracity and legitimacy.

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I. Introduction

The purpose of this paper is to review and evaluate the contribution of what many today call "behavioral finance" (BF) to the financial economics literature. Interest in the subject has been growing recently. On the one hand, we have seen "attacks" on BF by Ball (1996), Fama (1998), and others. On the other hand, we have seen an anthology of articles by the most frequent contributors to the BF literature published by the mainstream *Financial Analysts Journal* in 1999. Interestingly, the lead article of this collection is titled "The End of BF." This is quite a paradox, because how can something have an end before it has really had much of a beginning? Although something with the name "behavioral finance" sounds as if it would be a new methodology or even a significant new paradigm for research in financial economics, BF has never been, and looks as if it may never be, either. In fact it may really have ended before it began, and our intention in this paper is to describe how this happened.

In the science-fiction television and film series *Star Trek: The Next Generation*, there is a species called the Borg, a collective of techno-organic drones acting in concert as a single organism. In their pursuit of perfection, they roam the galaxy in search of other species, whose capabilities they acquire through a process of assimilation -- turning their captives into Borg and effectively absorbing their knowledge into the hive mind. The first line of every encounter with the Borg is familiar to *Star Trek* aficionados: "Resistance is futile. You *will* be assimilated." We do not intend this paper to be an *ad hominem* attack on modern finance, but we cannot help but be struck by parallels with these characters from *Star Trek*. Although modern finance's *homo economicus* is an independent individual quite unlike a Borg drone, which will de-activate itself if detached from the collective, it is certainly a species in pursuit of perfection (in its information and markets), making precise cost-benefit analyses of every decision. Like the Borg, *homo economicus*' favorite epithet is likely to be "Inefficient!," the ultimate depravity.

Modern finance[1] itself appears to be something of a techno-organic life form, with inspired theoretical and empirical work combined with, and both augmented and circumscribed by, complex mathematics, massive data sets, and esoteric statistical tests. And the current encounter between modern finance and BF is not unlike the encounters between the Borg and the Starship Enterprise. Modern finance is attempting to assimilate BF, adding its capabilities, including a smattering of experimental methods, but without experiencing any fundamental changes in its own methodology, just as the Borg assimilate humans, acquiring their knowledge but destroying their humanity and turning them into drones.

Elsewhere we have discussed how modern finance has marginalized BF by making it the "anomalies literature" (Frankfurter and McGoun, forthcoming). But recent surveys of BF by its pioneers, who one would expect to be its champions, suggest that more is happening (Thaler, 1999; Statman, 1999; Shleifer, 1999; Shefrin, 2000). BF is not being exiled to a remote planet; it is indeed being assimilated. And unlike the crew of the Enterprise, BF is not resisting the process. In Section II we review how the efficient markets hypothesis (EMH) and the capital asset pricing model (CAPM), the cornerstones of modern finance, met with a crisis. And in Section III we discuss how BF, which some have suggested as a way out of the crisis, has been labeled "the anomalies literature," thusly preserving not only the hegemony of modern finance, but also precluding any possibility for its refutation and/or the emergence of an alternative. Then in Section IV we describe the strange phenomenon of how even the supposed proponents of BF cling to the underlying tenets, forms, and methods of modern finance, necessarily resulting in the assimilation of BF into modern finance. In closing in Section V we show how finance research itself is subject to the same judgmental "mistake" that BF has discovered in finance practice. The findings of BF may simultaneously explain its assimilation and prove its point.

[1] What most finance academics, including us, call "modern finance" is the paradigm that evolved around the efficiency markets hypothesis, the capital asset pricing model, and whatever else is based on expected utility maximization.

II. The EMH/CAPM "Crisis"

In 1970 Eugene Fama (1970) published a paper titled " Efficient Capital Markets: A Review of Theory and Empirical Work." This paper redefined his earlier allusion to the efficiency of capital markets (Fama, 1965) and simply argued that in an efficient market prices reflect all what there is to know about a capital asset. Later, Fama's efficient market hypothesis (EMH) was "endowed" with three distinct forms of "informational efficiency," namely, the weak, the semi-strong, and the strong form.[2] Although the exact origins of these three forms are not explicitly known or traceable, it is generally held that: the weak form implies a random walk of some form (part of Fama's 1965 definition of efficiency) and that one cannot take advantage of the knowledge of historical price movements to earn superior returns on investments; the semi-strong form implies that prices at any given time incorporate all publicly available information; and the strong form implies that prices at any given time incorporate *all* information, whether public or private.

The EMH revolutionized beliefs about the pricing and the operation of capital markets, because it was consistent with an ideology that endorsed markets, not only capital markets, but other markets as well, as a near-perfect allocational device. Accordingly, one must conclude from this that as a social policy, the best government can do is not to interfere with market operations, because it would turn something which is *efficient* (good) into something which is *inefficient* (not good). And anything inefficient is to be avoided.

Sharpe (1964), Lintner (1965), and Black (1972) constructed a statistically testable capital asset pricing model (CAPM) that describes the pricing mechanism of capital assets and asserts that "beta," the relationship between firm returns and market returns, is the sole determinant of risk for which investors must be paid a premium.[3] The EMH and CAPM are internally consistent and connected in the sense that the latter provides a means for testing the former. This synthesis opened a door for empirical

[2] There are also two other dimensions of efficiency, namely, allocational efficiency and liquidity. In the interest of the discussion here these other forms can be disregarded.

[3] Although both the EMH of Fama and the CAPM of Sharpe et al. describe the operations and the characteristics of capital markets in general; that is, markets not just for stocks but for all capital assets, all subsequent tests of both the model and the hypothesis have been done on a sample, sometimes even a quite limited sample, of common stocks.

validation of both the hypothesis and the pricing model, and through this door, or rather floodgate, thousands of research papers streamed to accept or reject the validity of either or both the hypothesis and the model. A very large number of these empirical studies found that the theory couldn't be rejected, based on the data that were available at the time. After a while, however, many studies began to find results that showed the existence of "effects" that the CAPM could not explain or were inconsistent with the EMH in that all relevant information did not appear to be reflected in the price. The list of the "effects" is rather large, but some at least must be mentioned here to give a feeling for the scope and complexity of the problem.

Banz (1981) and Reinganum (1981) found evidence that the CAPM understates cross-sectional average returns of NYSE and AMEX-listed firms with low market values of equity, and it overstates those of firms with high market values of equity. This well-known phenomenon is now generally referred to in the literature as the small firm effect (SFE). Lamoureux and Sanger (1989) find the SFE in NASDAQ-traded firms and conclude that the SFE cannot be attributed to market structure differences between the NYSE/AMEX and the NASDAQ. As a consequence, they conclude that these markets are no different in this respect. Nevertheless, evidence contrary to the SFE also exists. Keim (1983) and Brown, Kleidon and Marsh (1983) find instability and reversals in the firm size anomaly for NYSE/AMEX listed firms. Their results indicate that factors other than the relation between a stock's return and overall market returns need to be incorporated in modeling a firm's expected returns.

Other empirical evidence supports the view that the relation between risk and stock returns is captured by some combination of firm specific and market specific information. Basu (1983) finds E/P (earning/price) and firm size both to be constituents of average returns for NYSE/AMEX listed firms. And he also finds beta, as extracted from the CAPM, to be positively related to returns, indicating that an overall market factor is a component of expected returns In addition to firm size and beta, Bhandari (1988) documents that leverage, as measured by the total debt-to-equity ratio, is instrumental in explaining the expected stock returns of NYSE/AMEX firms. Both of these studies, however, discredit the preeminence of the CAPMs beta in the explanation of returns.

Chan and Chen (1991) attribute the SFE to the fact that portfolios of small NYSE firms contain a large proportion of marginal, financially distressed firms. They argue that high leverage and reduced

dividends explain abnormal returns[4] associated with portfolios of small firms. Fama and French (1992) analyze both NYSE/AMEX and NASDAQ-traded firms and find that market capitalization and the ratio of the book value of equity to the market value of equity better explain cross-sectional average stock returns than beta, leverage and E/P. They suggest that firm size and the book-to-market equity ratio are useful for extracting market information about risk and expected returns, and they are better proxies of risk than beta. In fact, they found beta to be insignificant.

Amihud and Mendelson (1986) develop and empirically verify a "liquidity hypothesis" under which asset returns are positively related to the relative bid-ask spread, which in turn is negatively related to investor liquidity needs. Furthermore, they find the SFE to be a consequence of the spread effect, with firm size functioning as a proxy for liquidity. The significance of the bid-ask spread and the inconsequence of size reported by Amihud and Mendelson (1989) gives rise to the postulate that the excess returns of small firms is an illiquidity premium caused by either the lack of investors' interest and/or paucity of publicly available information. Merton (1987) develops a Multi-period CAPM that rests upon the assumption that market participants require a premium for investing in firms for which little public information is available. This notion was later parlayed into the neglect effect, according to which the returns of firms that are not followed at all, or at most by a small number of analysts, are inferior to those of firms that are followed by a large number of analysts

Investor interest and publicly available information may also vary according to the market in which a firm's stock is traded. Studies in the early 1990s document significantly higher returns for NYSE/AMEX-listed firms than NASDAQ firms, in contrast to Lamoureux and Sanger (1989) who find no difference between the two markets.[5] Reinganum (1991) finds the NASDAQ to be more liquid than the NYSE for small firms and ascribes the higher return on NYSE small firms to a liquidity premium. Loughran (1993) attributes the difference between firm's returns on the NYSE and the NASDAQ to the poor performance of recent Initial Public Offerings (IPOs) on the NASDAQ, while Fama, French, Booth and Sinquefield (1993) attribute the difference between NYSE/AMEX and

[4] The term “abnormal return” comes from the voluminous literature of event studies. *De facto*, abnormal returns are nothing more than the arbitrary compilation of error terms from a simple linear regression often called the “market model.” This market model is similar, but not mathematically equivalent to the CAPM.

[5] One must wonder, based on 1999’s record-breaking performance of the NASDAQ, how well this, as well as all other, empirical findings, stands up to the test of time.

NASDAQ-traded stocks to higher financial distress costs for NYSE firms. Overall, these results indicate that there might be an exchange effect that has to be controlled when studying firm specific returns.

The findings of these effects, as well as other more exotic effects such as the end-of-the-month, end-of-the-year, January, weekend, Yom Kippur, Value Line, etc. effects, cast serious doubts on the EMH/CAPM combination, potentially precipitating a crisis for modern finance theory. But rather than admitting the seriousness of the problems they posed, these effects were christened "anomalies," a diminutive term that implied a tolerable aberration from the dominant belief system instead of a serious challenge to it. Some feel that there is no need to alter the reigning paradigm at all (Fama, 1998); that with greater diligence in seeking out better data and subjecting it to more sophisticated statistical tests, the anomalies will disappear. But others are beginning to think that at the very least, the foundations of finance theory need buttressing with a somewhat different way of doing things. This new approach has been christened BF.

III. The BF "Solution"

Serious questioning of modern finance as a paradigm started when Prospect Theory of Kahneman and Tversky (1979) and Tversky and Kahneman (1990) was imported into studies of asset pricing. Prospect Theory, that has been founded on the outcome of numerous experimental psychological studies, is just one alternative to the expected utility maxim of Von Neuman and Morgenstern (1967) upon which modern finance has been based. [6] Since the reason for turning to BF was the inexplicable market responses encountered while testing the EMH/CAPM, psychological findings were mustered as explanations of either "overreaction" or "underreaction" to information.

The overreaction hypothesis of DeBondt and Thaler (1985, 1987) was one of the path-breaking applications of BF to the solution of the EMH/CAPM crisis. But it did have its precedents. Basu (1978) reports superior returns of low P/E stocks and inferior returns of high P/E stocks and

[6] BF has embraced a selection of psychological phenomena along with prospect theory, as we shall discuss in section IV. Other literature critical of expected utility maximization starting with Allias Paradox (1952) and continuing

interprets this finding as an inappropriate response to information inconsistent with the EMH that is later corrected. Dreman (1979) builds his argument on psychological factors, proposing that investors react to events in a fashion that consistently overvalues the prospects of the "best" investments and undervalues those they consider the "worst." Earlier others, (Hickman; 1958, and Atkinson; 1967) had found similar reactions to disappointing reports.

Dreman and Berry (1995) summarize the six predictions of the overreaction hypothesis:

1. For long periods "best" stocks underperform while "worst" stocks outperform the market.

2. Positive surprises boost "worst" stock prices significantly more than they do for "best" stocks.

3. Negative surprises depress "best" stock prices much more than they do for "worst" stocks.

4. There are two distinct categories of surprises: *event triggers* (positive surprises on "worst" stocks, and negative surprises on "best" stocks), and *reinforcing events* (negative surprises on "worst" stocks and positive surprises on "best"). *Event-triggers* result in much larger price movements than do *reinforcing events.*

5. The differences will be significant only in the extreme quintiles, with a minimal impact on the 60% of stocks in the middle.

6. Overreaction occurs before the announcement of earnings or other surprises. A correction of the previous overreaction occurs after the surprise. "Best" stocks move lower relative to the market, while "worst" stocks move higher, for a relatively long time following a surprise.

Dreman and Berry (1995) claim that all six predictions of *overreaction* show statistical significance. Other overreaction evidence is found in Fama and French (1992), Lakonishok, Shleifer and Vishny (1994), and Loughran and Ritter (1996). Much research of overreaction is in the IPO literature, including Loughran and Ritter (1995). In a nutshell, several studies have shown that the long term performance of IPOs is below what the market expects it to be at the time of the initial offering.

On the other hand, several event studies have shown evidence of underreaction in which the market response to new information appears to be too little or too late. Bernard and Thomas (1990)

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with Rubinstein's (1988) "Similarity" and several other alternatives has never been taken as seriously and is practically unknown to most researchers of financial economics.

and Abarbanell and Bernard (1992) show that financial analysts underreact to earnings announcements, either overestimating or underestimating quarterly earnings after positive or negative surprises. Michaely, Thaler and Womack (1995), find price responses to dividend cuts and/or initiations to continue for an excessively and irrationally long time. Ikenberry, Lakonishok and Vermaelen (1995) contend that investors underreact to firms' share repurchases.

The empirical findings of the BF literature have given rise to an investment strategy that systematically exploits the fact that the market is not as efficient as EMH would have it and takes positions contrary to what efficiency would recommend. Perhaps the best-qualified spokesperson of the contrarians is David Dreman, who has not only written two books on the subject (Dreman, 1979, 1998), but also actively manages $8 billion in assets. Dreman and others believe that they can systematically outperform the market by taking advantage of psychological factors that many of the studies mentioned so far claim to have found.

Although certainly suggestive, and perhaps even remunerative, BF has its limitations. It is a concoction of numerous psychological effects, only a few of which we've mentioned here, and as with overreaction and underreaction, often contradictory. No matter what happens in the market, there is a psychological effect that can be mustered to explain it. The empirical evidence in support of most behavioral theories is almost exclusively event studies, which are seriously flawed (Frankfurter and McGoun, 1995). And so far, having been designated the "anomalies literature', the sole purpose of BF has been to discredit the EMH/CAPM. This gives modern finance the home court advantage and virtually immunizes it from refutation, because terminal flaws can always be found in every empirical test.

First Ball (1996) and then Fama (1998) attacked BF, the former with vehemence, the latter with finesse. Ball (1996) argues that one has to stick with the EMH because (1) we don't have anything better, (2) it has sufficed in the past, and (3) it has become a strongly held conviction. Ball can think of only one alternative to the EMH, which he does call BF referring principally to the works of DeBondt and Thaler (1985, 1987). He dismisses it on the grounds that the investors' myopia implied by the DeBondt and Thaler work would be "grossly inconsistent" with the notion of competitive markets, and BF is also replete with its own anomalies. Ironically, in a co-authored work with Brown (Ball and Brown, 1968) Ball himself discovered a "post-earnings-announcement 'drift' in prices."

Fama's (1998) dismissal of BF is far more clever. With the careful screening of 20 or so papers, mostly from the domain of "post-event studies," he first attempts to discredit the empirical evidence in support of BF. Then, he argues that because BF's evidence is random and conflicting, it proves the existence of the EMH. But his conclusion is the most important part of the paper in which he makes BF synonymous with the anomalies encountered in event studies. In short, BF is nothing more than an aggregation of so-far inexplicable phenomena encountered in testing the EMH/CAPM. It has no independent existence; it is not a methodology in its own right; it has been assimilated.[7]

It is not surprising that the proponents (in fact, the very founders) of the EMH/CAPM ought to attempt to discredit and/or assimilate BF in the way in which Ball, Fama, and others have. But what is astonishing is how the proponents (once again in fact, the very founders) of BF have acquiesced in this. The BF explorers that could have founded a new species of finance have not found a region of research space to call their own. They have strayed too close to the EMH/CAPM fleet, and being unable to escape from its tractor beams, are being pulled in to their doom.

IV. Behavior Finance's Fatal Attraction

Let us begin with a description of the distinctiveness of BF given by on of its proponents in the recent special issue of the *Financial Analysts Journal* devoted to the topic.

> People are "rational" in standard finance; they are "normal" in behavioral finance. Rational people care about utilitarian characteristics but not value-expressive ones, are never confused by cognitive errors, have perfect self-control, are always averse to risk, and are never averse to regret. Normal people do not obediently follow that pattern. (Statman, 1999, page 12)

There are several questions posed by this definition:

1. What is a "utilitarian characteristic" and what is a "value-expressive characteristic"?

2. What does it mean to be "confused" by a "cognitive error"?

[7] Here we should remind the reader that Fama and French (1992) conclude that "[the CAPMs] beta has no predictive power," the ultimate failure of a positive model. But a few years later Fama (1998) makes the point that the CAPM and the EMH are inseparably intertwined.

3. What is the "perfect self-control" it is rational to have?

4. What is the "risk" to which it is rational to be averse?

5. What is the "regret" to which it is rational not to be averse?

6. Why are "rational" and "normal" antonyms, rather than "rational" and "irrational" or "abnormal" and "normal"?

Let us search first for the answers in Statman's article. Since the EMH lies at the heart of modern finance theory, being the current research tradition, it is not surprising that Statman believes the controversy between modern finance and BF to center on the definition of market efficiency.[8] He offers two definitions, and argues that we ought to accept the first and reject the second.

> To some, market efficiency means that there is no systematic way to beat the market. To others, it means that security prices are rational--that is, reflect only "fundamental" or "utilitarian" characteristics, such as risk, but not "psychological" or "value-expressive" charactics, such as sentiment. (Statman, 1999, page 18)

So "risk" is a "fundamental" or "utilitarian" characteristic and "sentiment" is a "psychological" or "value-expressive" one, and we can conclude from this that "fundamental" means "not psychological" and "utilitarian" means "not value-expressive." Both of these pairs of antonyms presume that there is something about investments that has nothing to do with investors; that is, that there is something fundamental that has nothing to do with individual psychology and something utilitarian that has nothing to do with individual values.

Platonism in mathematics is the view that there are mathematical objects (i.e. numbers, functions, sets, etc.) which exist outside of space-time. They are non-physical, non-mental, and acausal; that is, they exist independent of us, our theories concerning them, and our use of them (Balaguer, 1998). What we have in Statman's definition with "something fundamental" and "something utilitarian" is platonism in finance, a belief in the existence of something independent of us (it is not psychological or value-expressive), of our theories concerning it (the purpose of which theories is to discover it, not

create it), and our use of it (our knowledge of finance has no effect upon it). That a security has a platonic price or value independent of the market on which it is bought and sold is a curious idea today that harkens back to the classical economic concept of value of the 18th century. And even if there were such a thing, being non-physical, non-mental, and acausal, it would be impossible for us to know it (Balaguer, 1998). These are deep philosophical matters that have been and ought to continue to be discussed at great length, but for this paper let us consider just two things: "risk" and "wealth."

> In the investment context, risk is a utilitarian characteristic, and those who restrict their attention to it are considered rational. The rubric of "rationality" is not so easily extended to other characteristics, such as social responsibility, the display of wealth, the excitement of an initial public offering, or the camaraderie of Internet chat rooms (Statman, 1999, p. 21).

Although the literature on risk is voluminous, we can say with certainty that never in the history of finance or economics has anyone shown that risk is fundamental (not psychological) or utilitarian (not-value-expressive). We have assumed that it is and that we can measure it as the dispersion of a historical relative frequency distribution. But early articles in economics journals on risk very clearly state over and over again that we've made such assumptions for computational convenience, and they express serious reservations about the assumptions' efficacy (McGoun, 1995). What has happened is that we've used these assumptions for so long that we've forgotten that we've merely made assumptions, and we've come to believe that the world is necessarily this way. In truth, risk is no different than any other "psychological" or "value-expressive" attribute we might care to imagine.

Nothing seems to be as fundamental or utilitarian as the "wealth" that rational investors are supposed to maximize, but recall that in the transition from economic theory to finance theory, "wealth" is assumed to be an appropriate substitution for "utility." No one believes that wealth and utility are equivalent, but what makes the replacement acceptable is that unlike the old saying, money (wealth) *can* often buy happiness (utility). Of course, money does not actually buy happiness. Rather, it buys *things*, and in a modern market economy in which more and more *things* have a price, everyone is free to exchange their money for those *things* that give them the most happiness. As long as finance is willing to ignore the happiness that money *can't* buy, then wealth maximization is indeed utility maximization.

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[8] The first sentence in the article abstract is: "Market efficiency is at the center of the battle of standard finance

Now, consider Statman's "other characteristics, such as social responsibility, the display of wealth, the "excitement" of an initial public offering, or the camaraderie of Internet chat rooms" that are not "utilitarian" characteristics. Rational investors are not supposed to pay attention to these in their decision-making, but at the same time they are allowed to spend their wealth on them. They can contribute to socially responsible causes; they can purchase things for ostentatious display; they can buy their way into an exciting initial public offering or a collegial Internet chatroom. They can even hire someone to control their behavior or take out an insurance policy against regret if they want to. But their intended use of their wealth for these things can have nothing to do with the means of obtaining that wealth.

Statman's objection to the second definition of market efficiency in the above quotation is that normal people do not behave as if they were "rational," and our finance must reflect the way that people really behave. Thaler concurs:

> I predict that in the not-too-distant future, the term "behavioral finance" will be correctly viewed as a redundant phrase. What other kind of finance is there? (Thaler, 1999, p. 16).

And we concur, too.

But the real problem is different. Not only do normal people not behave as if they were "rational," but there is no non-platonic thing as "rational." And even if there were such a thing as "rational," there would be no way to distinguish it from "not-rational." Whatever people do is going to be "psychological" and "value-expressive" because they are people, and as such it is impossible for them to do anything that isn't. Statman calls "risk" a "fundamental" or "untilitarian" characteristic, but the history of risk in economics and finance leaves no doubt that this statement itself is value-expressive -- of the values of modern finance. "Wealth maximization" is hardly a "fundamental" or "utilitarian" characteristic, since it is impossible to segregate the capital markets (on which one "invests") from the markets for such things as social consciousness, ostentatious display, excitement, collegiality, personal control, peace-of-mind, etc. (on which one "consumes").

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versus BF versus investment professionals" (Statman, 1999, p. 18).

Yet modern finance theory has assumed that there is such a thing (as "rational behavior"), has based its methodology and its methods on the existence of such a thing, and is forcing BF to be structured as if there were such a thing. And BF is going right along with it. Thaler does not even try to conceal this subservience:

> Indeed, behavioral finance is simply a moderate, agnostic approach to studying financial markets. (Thaler, 1999, p. 12),

as if BF was a kinder, gentler strain of modern finance. It's impossible to be an agnostic without accepting the existence of the underlying religion, allowing it to set the terms of the debate, and making it the benchmark against which everything is judged. The opponents of BF have tried to assimilate it by labeling it the "anomalies literature." And the proponents of BF, by themselves labeling it the "not-rational literature" (albeit under the less pejorative adjective "normal") are a (perhaps unintentional) party to their own assimilation.

So far in this section we have addressed some of the questions posed by Statman's description of the distinctiveness of BF that we quoted at the beginning. There is no such thing as a "utilitarian' ("not-value-expressive") characteristic, and if there were, there wouldn't be any way to know it. That risk is "utilitarian" is by itself "value-expressive" -- a belief that finance has adopted and forgotten that it is a belief. And by using the word "normal" as the opposite of "rational," we make it sound a little more value neutral and hide our making "rational" the standard we use to define "normal."

This latter point is critical. While the proponents of BF see it in positive terms, they reject it in normative ones. Consider the introduction to Shefrin"s (1999) popularization of BF:

> 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).

So one of the promises of BF is that we will never be "confused by cognitive errors," we will always have "perfect self-control," and we will never concern ourselves with "regret." We will be more

"rational" investors, which of course sounds much better than "abnormal." Admittedly, we evolved our "normality" to survive and reproduce in a world of hostile predators and not to maximize our wealth in a world of (hostile?, fiercely competitive?) financial markets, so there may certainly be behavioral characteristics we do not now have that we might "profitably" adopt. But that these are what we have been calling "rational" is again decidedly value-expressive. Even if such things exist and we were able to recognize "confusion by cognitive errors," "perfect self-control," and "regret," with any certainty, it is uncertain that this isn't/is what we ought to be doing in real financial markets.

BF has been fatally attracted to the myth that science, its methodology, its methods, its definitions, etc. are just what modern finance says they are and that "rational" behavior is what practitioners ought to be doing even if it isn't what they are doing now. But such assertions are a result of precisely the same "confusion by cognitive error" that finance is supposed to be exposing and correcting. It is commonplaces in sociological studies of scientists that the sociologists conducting the studies are not just the observers, but as scientists themselves are also the observed.

Let us consider how much of the BF that is being applied to practitioners can be also be applied to its academic proponents and opponents.

V. Hoisted by Its Own Petard

There have indeed been tongue-in-cheek suggestions that academic finance is not conducted with the same "rationality" that it prescribes for others.

> . . . behavioral finance will be dominated by young scholars who are not burdened with large investments in the old paradigm (even economists have trouble ignoring sunk costs) (Thaler, 1999, p. 16).

But there is much more. Following are three themes of BF, identified by Shefrin (1999) that we might also search for in finance research itself:

> 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).

Recall that on the preceding page, Shefrin referred to these themes as mistakes, which makes modern finance the standard to aspire to.[9]

There are a vast number of examples of each of these themes in finance research itself, but let us consider only one of each. Regarding the appropriate and correct use of statistical tools, we have already criticized this in detail for event studies, the most common method of empirical testing in finance (Frankfurter and McGoun, 1993). Yet, it would be useful to summarize a few of our major points here. Tests of statistical significance are meaningful only if there is some sort of chance process involved by which a random sample of independent elements is drawn from a universe. But event studies regularly include all events that occurred within a limited period of time to firms having characteristics that caused information regarding them to be a part of a select automated database. Every event study implicitly makes the doubtful assumption that these events are representative of all of the same category of event that have ever occurred or which will ever occur.

Certain fundamental assumptions of statistical inference are often violated in event studies. One is that event studies measure the reactions of markets during periods when certain events are believed to occur. Since firms themselves determine the occurrence of most events, they select themselves into the event study. So the sample is not random, and the impact of the event is a consequence of the event itself and what it was that caused the event to occur when it did. Event studies which question the timing of events are rare, and the possibility that the market may be reacting to the same cause that the firm was reacting to when it initiated the event is never considered in sufficient depth. And not only is the sample not random, but the events in the sample are not independent. The reaction of the market to

[9] Note too, the prevalence of such terms as "fundamental," "objective," and "transparent" in descriptions of modern finance. This would not be so damning if they applied only to the platonic world of finance models, but finance believes that there really are such things, and with better theories, better data, and better statistical tests, we will discover them.

later events must be strongly affected by knowledge of how the market reacted to earlier occurrences of the same events and of course also to knowledge of any previous event studies.

Statistical significance in event studies is compromised in other ways. Usually the data being used to test the hypothesis is not only used to suggest the hypothesis being tested, but it is also used to determine when the event with which the hypothesis is concerned actually occurred. And there is the problem of missing information. Usually, negative results are either never reported, never written up, never submitted to a journal, or never published in a journal. So there is a disproportionate preponderance of verified hypotheses in the literature. Clearly, there are marked biases in finance's heuristics concerning how to do an event study. Although there may be more sophisticated errors being made in the study of finance than in its practice, it is doubtful that there are fewer of them or that they are less serious.

The event studies critique also provides us with an example of how finance is susceptible to frame dependence, the second theme of BF. The traditional event study tests a hypothesis of the form:

$H_{1(Traditional)}$: Event X causes an unexpected change in the firm's value.

As a conservative scientist ought to be more concerned with Type I errors (rejecting a true hypothesis) than Type II errors (failing to reject a false hypothesis)[10], the standard approach is to recast the empirical test of H_1 in the form of a test of the null hypothesis H_0 of the form:

$H_{0(Traditional)}$: Event X does not cause an unexpected change in the firm's value.

It is simpler to compute the probability of the test results given that H_0 is true (committing a Type I error with regard to H_0) than to compute the probability of the test results given that H_1 is false (committing a Type II error with regard to H_1). Of course, this standard approach requires that H_0 be the negation of H_1, which is true of the hypotheses as stated for this traditional event study. The traditional event study is not concerned with strong evidence in support of $H_{1(Traditional)}$ of the form X implies Y; rather, it is

concerned with the absence of strong evidence in support of $H_{0(Traditional)}$ of the form X implies not-Y. In the traditional event study, there is a marked difference between what epistemology demands and the logic of statistical inference supplies.

This difference, however, is not unavoidable. A skeptical scientist might view the test quite differently and take an alternate approach -- denying that the event has any effect. Now, the hypothesis to be tested is:

$H_{1(Skeptical)}$: The event X does not cause an unexpected increase in the firm's value.

And the null hypothesis is:

$H_{0(Skeptical)}$: The event X causes an unexpected increase in the firm's value.

In this skeptical form of the event study, strong evidence in support of the $H_{1(Traditional)}$, which is the statement of epistemological interest, is the same as strong evidence against the $H_{0(Skeptical)}$, which is the formulation consistent with the logic of statistical inference.

It is simple to show that these two logically equivalent empirical tests can yield diametrically opposite epistemological results (Frankfurter and McGoun, 1993). Using the same data, the traditional event study can "confirm" that an event X *does* cause an unexpected change in the value of the firm, and the skeptical event study can "confirm" (even a little more strongly) that the event X *does not* cause an unexpected in the value of the firm. It is not entirely true that the same test on the same data with the same results "confirms" conflicting conclusions. The difference is that the believer using the traditional event study will settle for a theory that does not predict too badly, while the skeptic using the alternate event study demands a theory that predicts very well. It is possible to have statistically significant results that satisfy the criterion of one but not the other. What matters is how the test is framed.

Finance certainly pays lip service to the myth of a scientific method, which in effect means an efficient market for knowledge. Theories are created, empirically tested, and the observed “facts”

[10] As a single falsification is logically sufficient for the abandonment of a theory and true theories are more difficult to come by than false ones, it is better for an empirical test to fail to reject a false hypotheses than to reject a true one.

"speak for themselves" as to whether the theory is true or not. A vast body of literature in the sociology of science and the philosophy of science since Kuhn (1970) has shown beyond any doubt that science simply does not work this way, and in all likelihood cannot work this way. Although finance has not yet been subject to the same sociological and philosophical scrutiny as some other sciences, McCloskey (1985) provides one very provocative description of how the closely related discipline of economics may actually work, as opposed to how it claims that it works.

There may, however, be an interesting parallel here with Statman's two definitions of market efficiency quoted at the beginning of Section III. The market for knowledge may not be efficient in the sense that accepted theories reflect a fundamental truth and rejected theories a fundamental falsehood. A paradigm is a way of framing all research, and finance is unable to accept research that is framed in any other way. BF is allowing itself to be assimilated into the modern finance paradigm, because that is the only possible way research can be done today and still be called finance. And a paradigm also has built in biases regarding how results are to be interpreted, of which there is no better example that Fama's (1998) critique of BF. In this section we have shown that finance research is subject to the same "mistakes" that BF attributes to practitioners. What is more interesting is that it is these same "mistakes" that are preventing BF from emerging as a new paradigm. So in effect, the failure of BF is, paradoxically, proof of its justification.

On a more hopeful note, just because the market for knowledge is inefficient in this way does not mean that it is inefficient in that it can be beaten in the long term. In a market for knowledge, this means false theories are allowed to masquerade as truths indefinitely. Now that BF has finally earned special sessions at finance conferences, special issues in modern finance journals, publication of popular books by respected authors, two new dedicated journals, and even the enmity of very distinguished critics, it may be able to avoid assimilation by modern finance, preserve an original identity, stake out its own territory, and build a new civilization. Perhaps resistance is not futile.

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