

Market Efficiency or Behavioral Finance: The Nature of the Debate

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Academic finance (also known as financial economics) espouses a methodology that has been largely discredited (or, at the very least, challenged) in all other disciplines, not to mention the philosophy of science itself. And this methodology is so ingrained that it is rarely even addressed, let alone seriously debated. The term behavioral finance, which ought to be applied to a different, more experimental methodological approach to finance, is instead applied to a set of papers that make slightly different assumptions in their mathematics/statistics without even using different methods.

Then, not a formal debate, but a two-and-a-half-hour battle with Gene Fama. His closing comment to me after dinner that evening was “You have pushed me an epsilon toward the abyss.” Driving home that night, in a really hard rain, I had a long time to ponder that statement. Why is it an abyss? And why only an epsilon? I concluded that it is only an epsilon because they are afraid of the abyss. And what is in the abyss? The abyss is the realization that we have crossed the line where we can no longer explain most of the market’s behavior on the basis of rational economic paradigms.

—Robert Haugen in “Post Modern Finance”
(*The International Review of
Financial Analysis*, 1998, 6:159)

Although the catalyst for this paper is Fama’s (1998) latest salvo in defense of his efficient markets hypothesis and the capital asset pricing model of others [EMH and CAPM, subsequently], our objective is more ambitious than a simple refutation of Fama’s arguments. We intend to show that, beneath the surface of quasi-scientific debate or dispute, there are issues that go deeper, to the core of what knowledge is and how it is to be acquired in financial economics. In short, our concern is with ontology (what is to be known) and epistemology (how it is to be known) and how conversations about both in financial economics are conducted.

Fama’s recent essay is not the first attempt to salvage the EMH and the CAPM or to attack what is now being called “behavioral finance” since Fama’s own devastating critique of the SLB model in Fama and French (1992). The abbreviation SLB stand for Sharpe (1964), Lintner (1965), and Black (1972), and

it is used to leave the much more familiar and ubiquitous acronym CAPM untainted by controversy. Attempts at salvaging a deeply rooted and accepted paradigm, however indefensible it may be, and attacking behavioral finance have been made by Ball (1996), Shanken and Smith (1996), and now by Fama himself. In fact, the Shanken and Smith paper invokes principles of the philosophy of science in its defense of the status quo, via sorcery in the name of Karl Popper, unfortunately both misspelled and misquoted. This invocation is unusual in the realm of financial economics where, as Smith once advised one of the authors of this paper, researchers know and care very little about philosophy. Yet the most comprehensive and intensive attack on behavioral finance for the sake of guaranteeing the survival of the EMH is, by far, the recent paper by Fama.

In his opening there is a misuse of language by Fama, which relates to the underlying issues of his paper. He argues, “Reasonable changes in the approach used to measure abnormal returns typically suggest that apparent anomalies are *methodological* [our italics] illusions” (ibid., p. 285). With regards to the word *methodological*, Fritz Machlup made this observation while chairing a session on economic methodology at the 1963 annual meeting of the AEA: “Semiliterates adopt the word [methodology] when they are concerned neither with philosophy nor logic, but simply with methods” (Machlup, 1963, p. 204). Yet, the issues raised by Fama are exclusively issues of method and not methodology. His methodology, which is also that of almost everyone else doing research in financial economics, is the methodology of positive economics (*de facto*, instrumentalism), as defined by Friedman (1953) and modified by others (among them by Fama himself) to fit the needs in defense of their cherished theories. In quoting Machlup, we do not mean to be pejorative to Fama, who most certainly is not semiliterate. In fact, he is one of the most eloquent writers, if not *the* most eloquent writer in this subfield of eco-

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nomics. Why then does he call a method (a technique, measure, or approach) a methodology (a branch of philosophy or logic)? The distinction between the two and a possible reason for this misnomer are the subject of first section of this paper.

In the past, the many “effects” noted by researchers that could not be accounted for by the CAPM were casually tossed into a dustbin labeled “anomalies.” From Fama’s recent paper, one may get the impression that what has been considered an anomaly (so far, just an aberration and not a serious hazard to the validity of a theory) is something more serious and has to be reckoned with. Anomalies must be refuted in order to keep faith in the paradigm. The second section is about the word *anomaly* and how it is used in the current debate to support or refute a theory or paradigm.

In the third section, we turn to another, albeit less obvious, misnomer that causes wide confusion in the field: the term “behavioral finance” itself. Behavioral finance, as a part of *behavioral economics*, is that branch of finance that, with the help of theories from other behavioral sciences, particularly psychology and sociology, tries to discover and explain phenomena inconsistent with the paradigm of the expected utility of wealth and narrowly defined *rational* behavior.¹ Behavioral economics is mostly experimental, using research methods that are rarely applied in the traditional, mainstream finance literature. Fama’s paper discussed herein serves to deepen the confusion even further. He refers to behavioral finance as work that, although based on some theories developed in psychology (prospect theory and the over- and under-reaction hypotheses derived from prospect theory), is just a subset of the event studies literature. Specifically, Fama is concerned with those event studies that show evidence contrary to what one would expect to find if the EMH and CAPM were true.

Although the debate over finance theory is supposedly being conducted in good Popperian terms, with demands for empirical testing of hypotheses and with an eye toward their falsification, the polemic over the tests themselves is carried out in the *ad hoc* terms that learned academics are supposed to deplore. Should not the tests themselves be subjected to rigorous tests, to make sure which tests, if any, are best at detecting what it is we are trying to detect? This is rarely undertaken, with Brown and Warner (1980 and 1985), Barber and Lyon (1996), and Loughran and Ritter (2000) being among the few exceptions. What we seem to ignore is that every empirical test is a joint test of the method (not just the model) and of what is being tested. The pitfalls and problems associated with the disparity being the handling of hypotheses and tests are discussed in the fourth section.

The fifth section is a brief summary of the state of affairs of financial economics and some suggestions to move the field toward a more progressive and possibly more fruitful research program.

Method and Methodology

Method

Although Fama (1998) argues, “Reasonable changes in the approach used to measure abnormal returns typically suggest that apparent anomalies are methodological illusions” (*ibid.*, p. 385), there is nothing “abnormal” nor methodological about them. The term *abnormal return* evolved from the *event study*, for which Fama now claims patent rights:

Event studies, introduced by Fama et al. (1969) produce useful evidence how stock prices respond to information. (*ibid.*, p. 283)

Irrespective of Fama’s claim of creation, the event study is a *method of analysis*, used as a part or as a building block of a *methodology*. We discuss the logic of event studies and the apparent statistical and other problems associated with all of them elsewhere (Frankfurter and McGoun, 1993 and 1995). Still, a brief summary of what they are and how the “abnormal return” metaphor became part of the financial economics vernacular is perhaps in order.

The core metaphor in event studies is “abnormal return.” The term seems to have drifted into common usage rather than having been explicitly created. Though Fama et al. (1969) claim to have performed the first event study, they cannot be unequivocally credited with (nor blamed for) the metaphor. These authors do not expressly refer to “abnormal returns” *per se*, but they do lay the foundation for others to create the metaphor. They write, “The average residual can be interpreted as the average deviation... of the returns of split stocks from their normal relationships with the market” (*ibid.*, p. 8). And they note, “... there are a few months for which the residuals are abnormally large and positive” (*ibid.*, p. 20). Generally, however, they refer to Sharpe’s (1963) market model parameters as “... reflect(ing) the ‘normal’ relation between that stock and the market.”

A bit earlier, Ball and Brown (1968) refer to an “abnormal performance index” that resembles, but is not exactly, “cumulative abnormal returns.” By 1972, however, the metaphor is in wholesale use. Scholes (1972) prefers “prediction errors” to “residuals” (*ibid.*, p. 189), but specifically defines his cumulative measure as an “Abnormal Performance Index” and refers to it as a measure of “abnormal return” (*ibid.*, p. 190).

What difference does it make that “CARs” stood for “cumulative average residuals” in the earliest event studies and now stand for “cumulative abnormal returns”? Consider the structure of the bivariate ordinary least squares (OLS) model as described by Johnston (1963). In figure 1-2 of the chapter titled “The Two-Variable Linear Model,” he labels the line that

meets the OLS requirements as $\alpha + \beta X$. Then he explains:

The practical situation then is that we have n pairs of sample observations on X , Y , which can be represented on a scatter diagram as in Fig. 1-2 above. The essential difference from that figure is that in practice the line $\alpha + \beta X$ is unknown. Instead we have a set of *hypotheses* [our emphasis]

$$Y_i = \alpha + \beta X_i + \mu_i \quad i = 1, 2, \dots, n$$

$$E(\mu_i) = 0 \quad \text{for all } i$$

$$E(\mu_i \mu_j) = \begin{cases} 0 & \text{for } i \neq j; i, j = 1, 2, \dots, n \\ \sigma^2 \mu & \text{for } i = j; i, j = 1, 2, \dots, n \end{cases} \quad (1-5)^2$$

In (1-5) μ_i is called *disturbance term* (or stochastic term) in the true but unobservable relation $\alpha + \beta X_i$ which is based on all the assumptions of (1-5).

The shift from *disturbance term* to *residuals* is not simple. Residuals exist only in sample space, if and only if all the assumptions made in (1-5) hold and if the unknown parameters, α and β , in (1-5) are estimable. For this to occur, the principles of OLS are invoked, and as a consequence a residual term e_i is defined. This residual term is the vertical difference between the observed value of the dependent variable and its expected value as prescribed by the corresponding value of the independent variable and the parameter estimates.

The innocuous technical term *residual* has nothing to do with abnormality. On the contrary, it is a very normal part of the two layers of assumptions that are necessary for its conceptualization. The first layer is the estimate of the “true” relationship; the second layer is the use of this estimated relationship to estimate the value of interest. This is the parlance used in Fama et al. (1969). Concurrently, however, the authors of that article imply that by aggregating these terms in a certain way for a certain subset of economic units, one can infer something about the *economic relationship* “... of the returns of split stocks from their normal relationships with the market,” all this happening in equilibrium. By judiciously mixing method with methodology, they create a concept that by some magic becomes superior to an error in a simple regression. One also must not forget that Myers (1972) shows rather convincingly that there is no such thing as an *equilibrium-restricted* regression.

The objective of event studies is to measure the difference between the actual returns and the returns that would have been observed had the event not occurred. This measurement requires a set of three assumptions paralleling the two layers; first, that it is possible to estimate the “true” relationship between stock returns and the returns on the market; second, that the difference between the actual returns and the returns predicted by the “true” relationship measures the “true” impact of the event (the *disturbance term* or *abnormal return*), and third, that it is possible to estimate the

“true” impact (the *disturbance term* or the *abnormal return*) using the difference between the actual return and the return predicted by the estimated relationship (the *residual term*).

There is nothing “abnormal” about a *disturbance term*. In fact, the *disturbance term* can be quite large even when the event has no impact. Furthermore, it is well known that the probability of an observed value in OLS to be exactly equal to the expected value is zero, with the exception of the trivial case when the number of observations equals the number of parameters to be estimated. There is nothing abnormal about *residual terms* either. Why, then, has there been such a hasty shift from *residuals* to *abnormal returns*? Because when the metaphor is used thus, it implies that the theory upon which it builds is true and that there is something that is indeed different or peculiar or *abnormal* about returns as a result of the event.

Methodology

The methodology of financial economics in general, and research in market efficiency in particular, is unmistakably the methodology of positive economics as invented by Friedman (1953), and later corrupted by Sharpe (1964) and, at the present, by Fama. Friedmanian positivism—in essence, instrumentalism (the model is an instrument of prediction upon which social policy can be formulated)—is built on the following three tenets:

1. The primary requisite of a theory is to produce acceptable forecasts.
2. The secondary requisite of a theory is to be simple and fruitful.
3. The assumptions of the theory *must be* unrealistic to satisfy requisites 1 and 2.

In the second requisite, the word *fruitful* means that the model is useful as a foundation for other, better forecasting theories or models. In the third requisite the unreality of the assumptions is a necessary, but not sufficient condition. Thus, unrealistic assumptions alone do not create fruitful theories. In addition, Friedmanian instrumentalism is deeply infused with the philosophy of radical individualism as interpreted by Friedman. Although he vehemently denies it, Friedman’s methodology is value-laden.

Yet, Friedman makes it clear that there are a myriad hypotheses out there. Under most conditions, an hypothesis cannot be *proved* true or valid. It remains on the table so long as it *cannot be rejected*. Until and as long as it is a good predictor (a subjective term) in the sense that there is no better predictor out there (also a subjective assessment) that also cannot be rejected, it should be embraced.

Sharpe (1964), in proposing his CAPM, corrupts Friedman a bit. Although clearly the CAPM is a positive model, Sharpe declares it to be immune to tests of predictive ability:

... since 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)

Accordingly, a positive model such as his CAPM does not have to have especially good predictive ability as long as it is either internally consistent or a part of accepted economic doctrine.

This brief introduction to methodology brings us to Fama (1998), who has no qualms about the requisite of prediction. His modification of the methodology is more basic, however, and goes to the heart of scientific discourse.

A problem in developing an overall perspective on long-term return studies is that they rarely test a specific alternative to market efficiency. Instead, the alternative hypothesis is vague, market inefficiency. This is unacceptable. Like all models, market efficiency (the hypothesis that prices fully reflect available information) is a faulty description of price formation. Following the standard scientific rule, however, market efficiency can only be replaced by a better specific model of price formation, itself potentially rejectable by empirical tests. (ibid., p. 284)

We submit that there never has been, nor ever can be, a direct test of the EMH in any of its forms. Such a test must involve the survey of a “reasonable” (number of) market participants/economic agents, showing them a list of all available information at a certain point regarding a capital asset and asking them to check-mark the information they were aware of and incorporated in their decision making. Of course, such a test is a utopian ideal; thus, all tests of market efficiency are tests of some model (such as the CAPM) that predicts what the price of a capital asset should be if all information were taken into account. The wealth of “effects” discovered in an untold number of studies, published or presented before learned societies, show that the CAPM (or “market model,” if we are being cautious) cannot account for any of numerous observed “effects” that are branded anomalies. These include the *small firm/large firm*, *end-of-the-year*, *end-of-the-month*, *Monday*, *Yom Kippur* and *neglect* effects, just to mention the better-known ones. Fama is well aware of that and openly admits that “... the hypothesis that prices fully reflect available information is a faulty description of price formation” (parentheses omitted). Then

Fama completely disregards Friedman, and of course all philosophers of science. He argues that, in order to dispose of the EMH, an alternative model that better explains “... price information, itself potentially rejectable by empirical tests,” should be formulated, before one is to entertain the disposal of the current hypothesis.

Recall for a moment, if you will, the creation of the world as described in the first chapter of Genesis. Having created the world in six days, God prepares to rest. He retires (*de facto* creating the Sabbath). He looks around and sees that an hypothesis explaining the mechanism of markets is missing. He creates with great haste the EMH. Because the EMH got there first, by Fama’s logic, that part of the creation story of the world can be replaced only by another model that must show more convincingly with much stronger tests than those extant tests of the EMH that the alternative hypothesis holds. Now, to make certain that such an era will never come, the EMH and CAPM are elevated to the level of methodology thereby setting the standard by which all alternative hypotheses must be judged and inevitably found wanting. Because all empirical tests (including those that support the CAPM) can and will be subject to methodological criticism, especially those with results we do not like, the God-created paradigm will hold forever. De Long and Lang (1992) convincingly show that, for a sample of economic papers none of the unrejected null hypotheses were true, and that journals tend to publish papers that do not reject a null hypothesis.

One manifest problem with the Friedman-Sharpe-Fama instrumentalist *methodology* is the unreality of assumptions. Although this is necessary for the sake of building mathematically tractable models, any resemblance of the results to reality is accidental. This is, in fact, the reason why the financial economists Fama now calls *behavioral* imported theories from the field of psychology that are more consistent with individuals’ behavior. Unfortunately, in many cases their methods of analysis have remained the same as the methods of EMH/CAPM research. Accordingly, if the EMH is often called *modern finance*, what Fama calls behavioral finance might be called *late-modern finance*. It is not truly behavioral, a point to which we will return in more detail in the third section.

Anomalies

Synonyms of the word *anomaly* according to our thesaurus, include: *aberration*, *deviation*, *digression*, *exception*, *irregularity*. These many synonyms and definitions are not unique to the philosophy of science. This is an important point, as we shall illustrate.

In financial economics *anomaly* is commonly used to describe a temporarily inexplicable observation. The implication frequently is that, with a little thought, the unexplained result could be accommodated within one's functionalist world-view. The term, when applied this way, suggests that the user has not even considered the possibility that the underlying paradigm that produced the anomalous result is faulty. The paradigm, in effect, is loftier than the anomaly. Within the confines of the higher truth the paradigm represents, the anomaly is just an aberration, a digression that should be noticed, then simply dismissed or, if need be, tolerated.

This is how Ball (1996), defending the EMH/CAPM in the face of the behavioral finance onslaught, and others in financial economics use *anomaly*, to describe an "exception" to a well-established rule or belief. But Ball does not stop here. He tries to convince his readers to accept not only that he was the first to find evidence of an anomaly as far as the EMH goes (Ball and Brown, 1968), but also that he introduced the concept "... to the financial economics literature in a paper [I] published in 1978 ..." (ibid., p. 11).

In footnote 33, Ball (1996) indicates that his use of the word *anomaly* in Ball (1978), was "borrowed" from Kuhn's (1970) "famous book ... which introduced the term to the philosophy of science and scientific method" (ibid., p. 13). This may be a bit presumptuous. The word *anomaly* without a doubt appeared in the literature even earlier, and we distinctly remember seeing it in a work published in 1977 in the *Journal of Finance*. The word was used there to report an anomalous finding regarding the Modigliani and Miller (1958) and Miller and Modigliani (1966) works (Boness and Frankfurter, 1977, p. 775 and p. 786).

All that said, one still must take a closer look at Kuhn (1970). Following is an early paragraph in which Kuhn first uses the word *anomaly*. Note that he does not put the word in quotation marks as he does for new nomenclature elsewhere in his book. He seems to be using *anomaly* just as the appropriate word to describe the phenomenon.

Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. (ibid., page 52)

Another important point is that anomalies, according to Kuhn, eventually cause the modification (adjustment) of the paradigm. No such thing can be observed regarding either the EMH or the CAPM.

Scientific thought does not exist in a vacuum. To the contrary, it evolves and develops within an existing belief system. Scholars of the Middle Ages never thought of electromagnetic waves because they could never even imagine, let alone believe in, their technological reality, yet today electromagnetic waves are important instruments in the discovery of new physical phenomena.

The existence of a belief system is even more important in the social sciences, where there are no tangible technological discoveries that result in concrete inventions such as CD-players, camcorders, or digital cameras. What we have in the social sciences are models that may or may not describe human behavior. Positive economic models in particular are suspect because of the "unreality" of their assumptions. These models, of course, are direct consequences of a system of beliefs, which, as in the case of the CAPM, for instance, are now recognized as a matter of an idiosyncratic view of the world; that is, what segment of market data one looks at.

It is not our intention to critique Kuhn (1970), as more qualified people have done that already.³ Our aim is to point out that Kuhn serves well the interests of scientific elite in the social sciences because these disciplines are not capable of technical discoveries. There is no economic equivalent of the camcorder or CD-player. A reviewer can easily suppress articles postulating a new paradigm, or venturing to critique an old one. All he or she has to do is to argue that the paper is "poorly written." On the other hand, for an established model, as long as the "right" people accept it for whatever reason, others wielding data and statistical tests will find empirical support for its existence. And there are always data and statistical tests to reject inconvenient findings as weak or non-existent. This is precisely what Fama (1998) does on a massive scale. In attacking the inconvenient results of tests based on more realistic assumptions than those of the EMH, Fama aims to discredit the *methods of analysis* on one ground or another. As a result, the anomalies are either made to disappear or declared "weak," and the *methodology* is rejected. Thus, such behavior ensures the eternal existence of a paradigm that cannot possibly be true because its assumptions are totally at variance with almost everything we know about human nature and behavior.

The state and status of the EMH are perhaps best described by the following paragraph (reproduced here with his permission) taken from our private correspondence with Myron Gordon, responding to an earlier draft of Frankfurter and McGoun (1998):

Roughly one third of the literature [of the EMH] is devoted to the more elegant elaboration of the theory, much like the paintings of more and more representations of Christ in the Middle Ages. One third is devoted

to exploring the theoretical implications of one or another imperfection—viewed in isolation. The balance is devoted to tests that demonstrate the presence of anomalies, i.e. Fama and French. Of course, as you note, the possibility of an alternative paradigm is not ever considered, and it is stomped on wherever it rears its ugly head. Hence, ideologues remain secure in the belief that with all its imperfections there is no alternative to the theory of PCCM [perfectly competitive capital markets].

Behavioral Finance: What Is It and What Is It Good for?

There are several different ways one might interpret the term behavioral finance in order to distinguish it from more traditional finance. One is as a body of knowledge. There are indeed only a relative handful of familiar papers that have been labeled behavioral finance and are acknowledged as such in the finance discipline.⁴ And although there are a few institutions that are known for their work in behavioral finance,⁵ there have been until recently no regular conferences nor journals dedicated to it. So, from this standpoint, behavioral finance consists of an assortment of findings concerning the heuristics, to use the phrase of Miller (1977),⁶ and heuristics and biases to use the later language of Tversky and Kahneman (1990), describing the reality of individual decision making in financial situations. Another way to interpret behavioral finance is as a *method*. Given its roots in cognitive psychology, as found in Kahneman et al. (1990), for example, its methods are those of experimental psychology.

Yet another, and perhaps the most important, interpretation of behavioral finance is as a *methodology*. Although it is not yet possible to state unequivocally what the methodology of behavioral finance is, it is not likely to adhere like orthodox finance to the three tenets of Friedman's instrumentalism as presented in the first section of this paper.

In his famous essay, Friedman (1953) argues the pre-eminence of *prediction* over *explanation*—a term he uses rarely and then mostly in quotations. It is not clear whether he means this to indicate that there is no such thing as *explanation*, as the word is commonly used outside economics to imply understanding, or whether *explanation* and *prediction* are symmetrical as in Hempel and Oppenheim's deductive-nomological model,⁷ with *prediction* being the superior term for his purposes. *Instrumentalism*, on the other hand, accommodates either interpretation.

But explanation is far more than just prediction. It creates understanding, either in a causal-mechanical sense—revealing the mechanisms, causal or otherwise, underlying the way the world works (Kenny, 1979 and Hicks, 1979)—or it creates understanding in

a unifying sense, reducing the number of patterns that must be used to describe the way the world works (Kitcher, 1989, and Salmon, 1989). The primary purpose of a behavioral finance theory is more likely to produce acceptable explanations rather than predictions; that is, to help us to understand what might occur or what has occurred in finance, relegating prediction to secondary consequence.

Nor does the *methodology* of behavioral finance require that a theory be simple, as in Friedman's second tenet. The social and economic world is a complex place, and there is no evidence that its underlying structures, if there are such things, can be expressed in simple mathematical terms any more than can those of art or history. And it would be definitely detrimental to a behavioral finance theory if its assumptions were unrealistic as required by Friedman's third tenet, because then we would learn little about why a theory that works does so, which is the essence of explanation in a causal-mechanical sense. And it would not tell us what to do if the theory does not work. In fact, without a doubt, it is the unreality of the assumptions of the orthodox finance theory that have provided the motivation and reason for behavioral finance.

So, then, what is Fama's behavioral finance? In his terms, it is easy to summarize:

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)

Finally, given the demonstrated ingenuity of the theory branch of finance, and given the long litany of apparent judgment biases unearthed by cognitive psychologists, it is sage to predict that we will soon see a menu of behavioral models that can be mixed and matched to explain specific anomalies. (ibid., p. 291)

In short, behavioral finance, according to Fama, seeks to explain apparent biases in the market (as revealed by abnormal returns) with theoretical models incorporating the individual biases observed by cognitive psychology. In the words of Miller (1977), anyone who ever talked to corporate finance officers would not believe that "... the firm's value had indeed been maximized by some specific financial decision or strategy" (ibid., p. 272).

To state this version of behavioral finance somewhat differently, it is work that refers to the body of knowledge of cognitive psychology as support for what might be called *quasi-rational* assumptions, to use Thaler's (1992) phrase, but employs the same *methods* and the same *methodology* as traditional finance.⁸ Of course, one might argue that these new as-

sumptions are more *realistic* than those of the implicitly self-proclaimed *rational* finance and reflect a concern with *realism*, at odds with Friedman's instrumentalism. But they themselves are certainly unrealistic, and arguably even more unrealistic than the current assumptions, because there is no reason to believe that individuals with biases might aggregate in such ways as this work assumes.

The only reason to regard this behavioral finance as sufficiently different from traditional finance, thus deserving a different label, is that it challenges market efficiency. Market efficiency in its vernacular form states that there are no unexploited profit opportunities lying around out there that the astute rational investor can consistently grab. How is it conceivable that rational investors have not eliminated such opportunities through their own actions in competition with each other, and in the process driven quasi-rational investors to rationality or bankruptcy? The answer: anomalies that suggest it must not be real. They (the anomalies) are there because of the illusion created by substandard methods of research.

But note the tautology. By definition, rational investors are those who neither over- nor underreact, and by definition, over- and underreaction is what rational investors do not do. These terms coexist within the world of orthodox finance and what Fama refers to as behavioral finance. All models of this world must incorporate rational investors with or without quasi-rational investors, since without rational investors (either in fact or in the *persona* of the model builder), there would be no such thing as over- or underreaction.

We have already suggested a bit about what the method and methodology of a true behavioral finance might be. What we might add at this point is the necessity of distinguishing between *logical* and *rational* (Devlin, 1997). What traditional finance calls *rational* is in fact *logical*, and what it pejoratively calls *quasi-rationality* legitimately deserves the more reputable term *rationality*, given the current theories of evolutionary psychology (Pinker, 1997). We actually do observe the *logical* behavior assumed in orthodox finance—in subjects who have suffered certain forms of brain damage (Damasio, 1994). Fama's behavioral finance may be quite rational, and his market efficiency, though quite logical, may also be quite irrational. With further research, we might find that *logical* over- and underreaction, if it is even possible to reliably detect it (as we argue against elsewhere in this paper), might still not be *rational* over- and underreaction.

Statistical Tests: Method or Madness?

Perhaps the greatest irony of the Fama article and of financial economics in general is the terms in which the debates over the EMH and CAPM, and all other

models, theories, and hypotheses for that matter, are being conducted. As good Popperians, we are supposed to subject our models, theories, and hypotheses to rigorous empirical tests and abandon those that are falsified. We are to expose them to the harsh test of reality and let the facts speak (or *scream*, as a reviewer once put it to us) for themselves. But, as we have pointed out in previous sections, little or nothing in finance, most prominently the EMH and CAPM, ever seems to be falsified and abandoned. Rather, we pile up study upon study and anomaly upon anomaly and never seem to be able to make any progress on the "dividend puzzle," the "capital structure puzzle," or any number of other "puzzles" that practitioners would really like to be solved.

The reason is that as much as we would like to believe in them, we are not especially successful at finding the so-called "facts" against which to test our models, theories, and hypotheses. While we pay lip service to Popperian testing of these things, our debates over what are and aren't "facts" are conducted in the *ad hoc* terms we are taught to deplore. The following statements summarize some key portions of the argument in Fama's paper, which are expressions in neither Friedmanian nor Popperian methodology:

1. *Anomalies are overreported*: "I doubt that the literature presents a random sample of events. Splashy results get more attention, and this creates an incentive to find them" (Fama, 1998, p. 287).

2. *They are probably illusions*: "Long-term return anomalies are sensitive to methodology" (*ibid.*, p. 284). "They tend to disappear when reasonable alternative approaches are used to measure them" (*ibid.*, p. 288). "If a reasonable change in the method of estimating abnormal returns causes an anomaly to disappear, the anomaly is on shaky footing, and it is reasonable to suggest that it is an illusion" (*ibid.*, p. 303).

3. *When they aren't illusions, as evidence they are irrelevant, insignificant or tentative*: "Some apparent anomalies may be generated by rational asset pricing" (*ibid.*, p. 287). "Which anomalies are above suspicion? The post-earnings-announcement drift first reported by Ball and Brown (1968) has survived robustness checks, including extension to more recent data ... Again, though, the anomaly is stronger for small stocks" (*ibid.*, p. 304). ("Small stocks always pose problems in tests of asset pricing models, so they are prime candidates for bad-model problems in tests of market efficiency on long-term returns") (*ibid.*, p. 304). "The short-term continuation of returns documented by Jegadeesh and Titman (1993) is also an open puzzle, but it is still rather new and further tests are in order" (*ibid.*, p. 304).

4. *And the anomalies cancel each other out*: "A roughly even split between apparent overreaction and underreaction is a good description of the many of ex-

isting anomalies” (ibid., p. 284). “I shall not do a count of underreaction versus overreaction studies. The important point is that the literature does not lean cleanly toward either as the behavioral alternative to market efficiency” (ibid., p. 287).

5. *Therefore, markets are efficient*: “If anomalies split randomly between underreaction and overreaction, they are consistent with market efficiency” (ibid., p. 284). “On the whole, the long-term return literature seems more consistent with the market efficiency prediction that long-term return continuation and long-term return reversal are equally likely chance results” (ibid., p. 289). “The bottom line is that the evidence against market efficiency from the long-term return studies is fragile” (ibid., p. 285).

Fama makes quite a point in his paper that any alternative model to the EMH must be falsifiable. “Does the new model produce rejectable predictions that capture the menu of anomalies better than market efficiency?” (ibid., p. 291). And he has certainly done a capable job assembling the evidence falsifying the so-called “behavioral” models. Falsification of behavioral finance is indeed failure to falsify EMH, and Fama is not unjustified in looking at the evidence he has accumulated as consistent with the EMH. But a close look at his argument forces us to question whether the EMH is itself falsifiable, which, if it is not, would be quite a serious blow to its scientific respectability.

Consider the steps in his argument one by one. Anomalies may indeed be overrepresented in the current literature, but before market efficiency had secured the high ground as the theory against which all others must be judged, it was likely to have been overrepresented in the literature as well. What appears in public in one form or another is what dissertation committees and tenure committees want people to write, what program chairs of conferences want people to hear, and what journal editors want people to read. And it is not surprising that all these should be much the same. We never see the papers that didn’t make the cut at a conference or journal or that went into the file cabinet without being submitted or the results that went into the trash without ever having been written up.

There is no *Salon des Refusées* in finance. The whole process has much more to do with intellectual fashion and less to do with the pursuit of knowledge concerning significant problems. We truly are drunks looking for our keys under a street light rather than where we lost them—and only under a street light in the right neighborhood. For this reason, any count of papers with anomalies versus without anomalies or with overreaction versus with underreaction is meaningless.

The second step is an especially weak one. If the anomalies *disappear* when we make a *reasonable* change in our method (whatever the word *reasonable*

might mean), it is equally true that they *appear* using a variety of reasonable methods. The inconclusiveness of the process is equally detrimental to the EMH as it is to behavioral finance.

The third step is an obvious immunization of the EMH against falsification, but is certainly not presented as such. Essentially, Fama is saying that, if our methods throw up anomalies that we cannot eliminate through *reasonable* changes in method, then we can do one of three things: find a rational explanation for them consistent with the EMH, relegate them to a backwater of the market in which the EMH’s applicability is of little or no interest (i.e., small stocks), or try a little harder and eventually penetrate the illusion. Essentially, then, nothing will ever falsify the EMH. Such immunization is quite common in science, and is, in fact, the reason Kuhn and others began to take a sociological approach to science, since having observed it in operation they concluded that science wasn’t following its own precepts.

The fourth step is literally true; that is, the existence of both overreaction and underreaction is consistent with the EMH. But this is not an especially strong statement. There being both overreaction and underreaction is also consistent with behavioral finance, which certainly does not preclude the existence of both. We reemphasize here that considering the profession’s various filters, presence or absence of a phenomenon in the literature tells us much less about the phenomenon than about the social structure of the profession; therefore, the impression that overreaction and underreaction are roughly equally represented means nothing. We might also point out that the *ad hoc* listing of studies in Fama’s Table 1 is empiricism at its most casual and would be severely excoriated if it had come from anyone having neither Fama’s reputation nor his standing at the *Journal of Financial Economics*. Imagine an equivalent “event study” in which the author made a very short and admittedly incomplete list of the specific occurrences of the event with which he/she happened to be personally familiar or which otherwise caught his or her fancy and used the list to conclude that the event was irrelevant.

Fama is indeed correct that what he has presented is consistent with market efficiency and that the evidence in the behavioral finance studies against market efficiency is still fragile. But the methods being wielded by Fama’s opponents in defense of behavioral finance quite similar to those Fama wields in defense of the EMH. To say that fragile BEHAVIORAL FINANCE evidence makes the case for the EMH stronger is also to admit that fragile evidence in support of the EMH makes the case for BEHAVIORAL FINANCE stronger. In this regard, Fama hoists himself by his own petard.

Returning to the issue addressed in the first part of this section, we think the problem is clear. Much as

we may want to believe that we can do so, we simply cannot use statistics to generate the so-called “facts” against which we are trying to test our theories. We can always find a period of time or a set of firms or a creative way to slice the data or a different test to run it through to produce the numbers we want to see to serve our interests.⁹ This is not to say that doing so has no epistemological value, but we are kidding ourselves that the debate over behavioral finance is being conducted in anything but largely ideological terms. Fama’s paper is an exemplar of the genre, and by no stretch of the imagination is it a dispassionate appeal to the facts. There are no facts that speak for themselves; rather, we find the facts that say whatever it is that we want to say. The debate or dispute concerning behavioral finance as presented by Fama is in no way “scientific” in any sense that we have come to associate with that term. Consequently, one cannot conclude that, therefore, markets are efficient, as Fama’s step 5 does.

Conclusions

Reading Fama (1998), one can rest assured that there is no need to worry. The EMH is alive and well! Anomalies, if more than a mere illusion, are chance results, something perfectly consistent with the EMH. Post-event continuation of pre-event results are about as common as reversals. But perhaps the most important contention is that anomalies are fragile and they disappear with the right manipulation of the data or model.

Fama’s message is certainly most welcomed among the three dozen or so textbook writers and finance professors testifying in rate hearing cases. If the EMH/CAPM is irreplaceable, several chapters of basic and intermediate textbooks can be devoted to the paradigm and to the survey of its tests and to its use in capital budgeting and regulatory proceedings and in portfolio decision making. One can even devote a new chapter to behavioral finance, necessitating a new edition of the textbook. But there is no need to junk the textbook.

But this is just one view of the state of affairs, and the nature of the debate about not only the EMH, but also financial economics. Another view is that what we present here may be read as an *ad hominem* attack on Eugene Fama. This is a serious concern of ours, because this has certainly not been our intention. His original work on market efficiency was a bold challenge to what was then the status quo in finance, but his challenge was so strong that he and his EMH have become the status quo. All his recent papers, including the 1998 article in the *Journal of Financial Economics* taken up here, are in effect quasi-official statements of what finance is. In standing as exemplars for the discipline and profession in this way, they are inevitably the

specific targets for broader criticisms directed not just at Fama, but at what academic finance, in general, has become. Finance’s problems run much deeper than Fama’s specific ideological defense of his EMH.

Academic finance (also known as financial economics) espouses a methodology that has been largely discredited (or, at the very least, challenged) in all other disciplines,¹⁰ not to mention the philosophy of science itself. And this methodology is so engrained that it is never seriously addressed, let alone debated. The term *behavioral finance*, which ought to be applied to a different, more experimental methodological approach to finance, is instead applied to a set of papers that make slightly different assumptions in their mathematics/statistics without even using different methods.

The Fama paper leaves no doubt that, at present, methods are at the heart of every controversy in finance, but without methodology, there is no way to resolve them. Even if we could agree on what the “facts” might say about an hypothesis, theory, or model, without methodology, as the controversy over so-called “anomalies” shows, we cannot even agree about what is a “fact.” Asterisks indicating “significance” are not enough. It is extraordinary that one should fail to see that, regardless of the detail of the data and sophistication of the statistics, empirical research as it is currently undertaken can never resolve anything. In fact, it is likely that the more detailed the data and the more sophisticated the statistics, the more elusive their meaning and the less epistemological value they have.

Financial economics is not unique in the use of ideological arguments to defend hypotheses, theories, and models and immunize them against refutation. But it is unique in being such a young discipline without methodological controversy, and methodological controversy is ideological. As Kuhn (1970) argues, paradigms supplant each other for sociological and not scientific reasons. This is so because part of a paradigm is its definition of what is and isn’t evidence, and evidentiary rules are incommensurate. In order to discredit behavioral finance, Fama must incorporate it within the paradigm within which his methodological rules apply. It cannot be acknowledged as something different. But within his paradigm and by his methodological rules, markets must be efficient. To refer to the familiar joke, money simply cannot be lying around on the ground without someone already having picked it up.

The most serious problem with finance is that the lack of alternatives is clear evidence of lack of vitality. Paradigms define what questions are important, and financial economics is not able to answer, or in some cases even to ask, the questions that concern finance practitioners. It has become a glass bead game, intricate and elegant and *efficient* in its artificial world, but irrelevant to the real world—unless it can induce the real world to imitate it.

Notes

1. For an extensive review of this literature, see Rabin, 1998.
2. Johnston's notation.
3. For an excellent critique of Kuhn, see Hussein Sarkar, "The Task of Group Rationality: The Subjectivist View," unpublished manuscript, Louisiana State University, Baton Rouge.
4. The best known are papers by DeBondt and Thaler, some of which have been collected in Thaler (1991) and (1992). Many researchers in finance are also aware of the work of Kahneman and Tversky, cognitive scientists concerned with decision making. Several of their articles have been reprinted in Kahneman et al. (1990).
5. These include the University of Arizona and Virginia Commonwealth University.
6. Miller refers to observed phenomena inconsistent with the notion of "rational behavior" and market equilibrium as "heuristics."

If the phrase "don't behave that way" is taken to mean that firms and individuals don't literally perform the maximizing calculations . . . then it is most certainly correct. No corporate treasurer's office, controller's staff, or investment banker's research team that I have ever encountered had, or could remotely expected to have, enough reliable information about the future course of prices for a firm's securities to convince even a moderately skeptical outside academic observer that the firm's value had indeed been maximized by some specific financial decision or strategy. Given the complexities of the real-world setting, actual decision procedures are inevitably heuristic, judgmental, imitative and groping even where, as with so much of what passes for capital budgeting, they wear the superficial trappings of hard-nosed maximization (ibid., p. 272).

Still just a few lines after this statement Miller expresses his firm conviction that models that assume "rational behavior" "...generally lead to better predictions and descriptions . . . than any alternative available to them." Of course, we have no idea what the "losing" alternatives are. Miller invents his own metaphor "neutral mutations" (possibly, his freshly coined term for polymorphism), to convince his fellow members of the AFA that heuristics showing economic irrationality are harmless and cannot survive forever.

7. According to this model (Hempel and Oppenheim, 1953), an explanation is a deductive argument in which general laws and statements of conditions combine to explain an empirical phenomenon. For example, the general law "for all x , if x has the property P , then x has the property Q " and the condition that " a has the property P " provide an explanation for the observation that " a has the property Q ." In this model, explanation and prediction are symmetrical. If we have made the observation, then the law and the condition provide an explanation of it; if we have not made the observation, then the law and the condition predict we will.
8. The two papers that Fama uses as examples of behavioral finance are Barberis et al. (1998) in which "investors falsely perceive that there are two earnings regimes" (Fama, 1998, p. 288), and Daniel et al. (1997) in which "there are informed and uninformed investors" with the former "subject to two biases, overconfidence and biased self-attribution" (Fama, 1998, p. 289).
9. And we can always find a different way to weight the data (equally weighted versus value weighted), a different compounding period for returns (daily versus monthly), a different expected returns model (CAPM versus market model), a different event horizon (long versus short), or a different measure of overreaction and underreaction (ARs versus

AARs versus CARs versus BHARs)—all of which Fama addresses.

10. This includes the closely related disciplines of economics and accounting, both of which use vital and officially recognized alternative methodologies in addition to the standard one shared with finance. These alternative approaches to inquiry not only have their own conferences and journals, but also have sections within mainstream organizations and representation in the mainstream journals.

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