In sum, Bill Griffeth has produced an interesting book on mutual fund management. The book gives the reader the opportunity to view the investment strategies of a number of respected fund managers. It also provides readers with varying perspectives on where the mutual fund industry is heading. One change that would have made the book more interesting is a chapter devoted to comparing the investment styles of the different managers.

This book is well-suited as supplemental reading for students in Investments and Portfolio Management courses because it provides an intuitive perspective on investing that is not presented in standard investment theory texts. The range of investment styles and strategies should serve to elicit discussion and to motivate standard topics in investment theory. In addition, the book is excellent background reading for individuals interested in the field of money management because in addition to the investment intuition provided, many of the questions deal with how the mutual fund masters got their start in the field.

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The New Finance: The Case Against Efficient Markets. By ROBERT A. HAUGEN. Englewood Cliffs, N.J.: Prentice Hall, 1995. Pp. xiii + 146.

The Efficient Markets Hypothesis (EMH) has been very good to academics in finance, regardless of whether one actually believes it. Believers (described as "Zealots" in the book) have been able to use the EMH as a benchmark against which they conduct empirical tests of asset pricing models. Nonbelievers ("Heretics") have also made use of it; the hunt for efficient market "anomalies" has become an industry, not just for practitioners seeking profits, but for academics seeking publications. These anomalies, in turn, have provided material for "Zealots" seeking to provide rational explanations which do not rely solely on behavioral tenets. And (to borrow a phrase from Linda Ellerbee) so it goes.

In this book, the author attempts to collect some of the anomalies in the field and provide a single behavioral, as opposed to rational or "efficients markets," explanation for them. The explanation begins by building a case that the stock market consistently overreacts, as suggested by evidence in DeBondt and Thaler (1985) and others. One problem that immediately arises with a simple overreaction hypothesis is the additional evidence, e.g., Jegadeesh and Titman (1994), that prices tend to *trend* over the intermediate term, and not reverse as the overreaction theory suggests. The author cites this evidence as well, and then proposes that the stock market still overreacts . . . *but slowly*. That is, following good news, it takes the market a while to overreact and find a (too high) price, after which the market corrects or reverses itself (also slowly) and finds the "true" value. Therefore, not only does the stock market misprice, but it does so slowly, deliberately, in both directions and over long horizons.

Much of this book dwells on the implications for the individual investor of this pattern in stock prices. Clearly, if investors can identify those stocks which have performed poorly and, due to the overreaction to bad news, are therefore undervalued, they should simply buy these securities and hold them long enough for the market to recognize its error and re-price the security at its true value. The author cites many new, and some old, results in order to call undervalued stocks *value stocks*, and overvalued stocks *growth stocks*. Value stocks are also associated with high book-to-market ratios, high dividend yields, and small market capitalization; growth stocks are the mirror image in these characteristics. With these definitions, the author can bolster his case by citing the results of Fama and French (1992) and Lakonishok, Shleifer and Vishny (1993), among others, by calling their long-horizon results a by-product of the market's initial (shorter-horizon) overreaction to news.

Another large portion of the book attempts to demonstrate that, as the author puts it, "value stocks always win" in returns. In addition, the author, citing other evidence, claims that value stocks are also less risky. Of course, the claim that low risk stocks will *always* yield higher returns is discomfiting to many financial economists (even those who aren't card-carrying Zealots). To deal with this issue, early in the book the author begins a discussion of Modern Portfolio Theory (the "*Tool*"), the Capital Asset Pricing Model (CAPM) (the "*Theory*") and the EMH (the "*Fantasy*") in order to persuade the reader that the notion of "higher risk, higher returns" is a misguided one in choosing between equity securities. While I found this discussion very entertaining, I believe that it is primarily misleading. For a few (arguable) reasons given, the author states as incontrovertible fact that the CAPM is dead. Despite this statement, however, there may be hope for the Zealots of the world. First, while the CAPM has failed in some recent tests, there still exists a lively debate about whether the CAPM is in fact "dead" (see, e.g., Jagannathan and Wang (1994) or Kothari, Shanken, and Sloan (1995)). Second, when any model fails, it is *always* valid to ask whether a "rational" model can be developed (with other independently testable predictions) to explain the puzzles.

Although I believe the jury is very much still "out" on new asset pricing models, here too the author tells us that the search for such developments is misguided and will (must) ultimately prove futile. As support, he points out that much of this value "premium" is earned in January and that a valid risk premium should be constant through time (despite the fact that this is not a requirement of any model of which I am aware). With this "fact," however, we have the whole story: the stock market, believing that the past is a better predictor of the future than it really is, slowly overreacts to new information, eventually and inevitably reversing as truth is discovered, leading to large and low-risk investment opportunities for individual investors willing to seek out undervalued stocks. Moreover, these profit opportunities will, he suggests, defy attempts to explain them in a rational framework.

Taken as a whole, does this single explanation hold together? It may do a fairly good job of fitting some of the facts ex post, but it faces some formidable challenges. First, while Fama (1991) (the "Pope") begins his discussion of the EMH with the statement that "the extreme version of the market efficiency hypothesis is surely false," perhaps the reason that the notion continues to be debated is that it is easy to believe a less radical corollary that people will, if they can, make money in the markets. If one believes this, a natural question to ask of the author's hypothesis is, why doesn't the "value premium" go away? That is, why don't institutions and individual investors trade in order to take advantage of these excess profits? The author attempts to answer this question in two ways with, I believe, only limited success.

First, he asserts that academics are to blame. Both in the form of (very) influential but misguided books (he mentions two in particular, one which he suggests led to, and whose effects he believes were only corrected by, the Great Depression of 1929, and the other published in 1954 and whose effects, the author notes in a dire statement, "are still on track") and in classes, they perpetuate the notion that cash flows (and hence values) are predictable, that markets are efficient and that the CAPM is correct. Personally, I find it hard to believe that I have persuaded even one eager young potential Wall Street trader that markets are efficient and few, if any, of the corporate financial analysts with whom I have spoken use the CAPM. Moreover, many investment advisors appear to be at least as skeptical and statistically sophisticated as any "heretic" I know. In short, I find it difficult to believe that, faced with persistent evidence that academics are wrong (and, by the author's description, horribly so), practitioners will continue to believe academics rather than the data.

Second, the author lays out a complicated set of reasons why institutional investors don't capture the value premium. Unfortunately, the stories behind these reasons appear to be confusing (at best) and/or contradictory. For example, in hiring portfolio managers, boards seek out bottom fishers, that is, boards appear to endorse the concept of undervalued "value" stocks. However, once hired and even though they were hired as bottom-fishers, these managers don't actually look for undervalued stocks. Instead, they stay away from bad *looking* stocks (which also happen to be value stocks due to the market's overreaction to the initial bad news) because they don't want to look like they're underperforming. Unfortunately, as the author points out, these managers *do* underperform on average because, to keep from *looking* like they're underperforming, they buy overvalued "growth" stocks and therefore, inevitably, underperform. However, elsewhere the author also suggests that these "growth stocks" will perform better in the short-term. Therefore, if managers are *always* making this short-term decision, how can they *consistently lose* on average? In addition, this degree of myopia (less than one year) doesn't appear to hold in the data for managerial turnover (see, e.g., Khorana (1995)).

At another point in the book, it is asserted that stock managers are not interested in risks, but only maximum returns. However, in explaining the January effect (which, if one believes this hypothesis, must be caused by a correction in that month of most of the markets' previous overreaction), he seems to argue that managers care about (at least the perception of) risk at the end of the year, but less so at the beginning of the year when they load up on value stocks (but doesn't that mean they would be interested in capturing at least some of the value premium? And if this is the month when they are placing "big bets," doesn't this imply that value stocks are riskier, contrary to what he suggests elsewhere?).

In these and other ways, the author's explanation occasionally appears rather tortured in its attempt to explain some of the patterns in the data. In addition, researchers have reported results, such as short-run reversals (Jegadeesh (1990)) whose incorporation into the story would make it even more complex, or results which suggest that some of the patterns described here are statistical artifacts of the data or the methods used (see, e.g., Bernard, Thomas, and Wahlen (1994) who refute the reversal pattern, although not the initial trending, seen in Figure 2.4). These results would require a modification of the behavioral explanations provided in the book. Of course, there is *always* an alternative behavioral explanation for *any* pattern in the data; some of them are undoubtedly true, but it is not unreasonable to hold these behavioral models to the same standards to which we hold "rational" models, i.e., they should be the simplest (and hopefully consistent) abstraction possible that is able to predict behavior, not just provide ex post explanations for observed features in the data.

Does the author's explanation pass this hurdle? I'm doubtful. Consider a simpler explanation here that much of the value "anomaly" is just the January anomaly in disguise. Let's also assume that the January effect is a genuine anomaly, which is to say a profit opportunity (although even here and in contrast with what the author implies, there are serious attempts to explain it in a "rational framework"). Then, to the extent they can, (smart) institutional investors may well try to take advantage of it, but they (and others) are hampered by market impacts which can be severe, particularly in the small-cap "value" stocks. In fact, market impact may negate many of the patterns in the data with which the author concerns himself; as an example, Ball, Kothari, and Shanken (1994) report that market impacts of as small as 1/8 renders the long-term overreaction profits, an important component of the author's story, unattainable!

The disadvantage of this simpler explanation, of course, is that it is not as provocative as the explanation in this book. Indeed, it seems that the January effect (about which the author has written a separate book) has been rediscovered many times, perhaps by those "finance professors in business schools throughout the world" who are "tenaciously [sifting] through computerized data files." (In fact, one concern which this phrase on page 1 of the book captures rather well is the fear that some of these "anomalies" are in fact the result of these same finance professors snooping the data (see Lo and

MacKinlay (1993)). With the exception of the January effect, many of the more recent puzzles which the author uses to justify his theory have had challenges raised to them. To put it another way (and to use the author's phrases in the Preface), some of these "stones and arrows flung at the paradigm by the nonbelievers" have already been thrown back. The author has attempted to rebundle several "stones and arrows" into one package to lob at the Zealots, but he has a tendency to state "facts" which are in fact "debates" (in other words, the target has moved in the meantime). Misguided as the Zealots' assumptions may appear to the author, I suspect that they will continue in their attempts to develop better asset pricing models to explain the anomalies, or better statistical models to assess their significance. Of course, the author likely approves of these activities, since it provides material for the next (equally entertaining) entry in his series.

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Financial Management and Analysis, by Pamela Peterson, is designed as a text for a student's first course in corporate finance. It covers the standard