The End of Behavioral Finance

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In 1985, Werner De Bondt and I published an article that asked the question: "Does the stock market overreact?" The article was controversial because it gave evidence to support the hypothesis that a cognitive bias (investor overreaction to a long series of bad news) could produce predictable mispricing of stocks traded on the NYSE. Although this idea was hardly shocking to practitioners, the conventional wisdom among finance academics was that we must have made a mistake somewhere.

The academic community considered several possibilities to explain our results: We made a programming error; the results were correctly measured but explainable by chance variation (data mining); the results were correct and robust (no data mining), but rather than discovering mispricing caused by cognitive errors, we discovered some new risk factor. The possibility that we had both the facts and the explanation right was thought by many academics to be a logical impossibility, and the demise of behavioral finance was considered a sure bet.

Fifteen years later, many respectable financial economists work in the field called behavioral finance.[1] I believe the area no longer merits the adjective "controversial." Indeed, behavioral finance is simply a moderate, agnostic approach to studying financial markets. Nevertheless, I too predict the end of the behavioral finance field, although not for the reasons originally proposed.

To understand what behavioral finance is and why it was originally thought to be a fleeting heresy, one must first understand the standard approach to financial economics and why those who used this approach believed, on theoretical grounds, that cognitive biases could not affect asset prices.

Why Behavioral Finance Cannot Be Dismissed

Modern financial economic theory is based on the assumption that the "representative agent" in the economy is rational in two ways: The representative agent (1) makes decisions according to the axioms of expected utility theory and (2) makes unbiased forecasts about the future. An extreme version of this theory assumes that every agent behaves in accordance with these assumptions. Most economists recognize this extreme version as unrealistic; they concede that many of their relatives and acquaintances—spouses, students, deans, government leaders, and so on—are hopeless decision makers. Still, defenders of the traditional model argue that it is not a problem for some agents in the economy to make suboptimal decisions as long as the "marginal investor," that is, the investor who is making the specific investment decision at hand, is rational.

The argument that asset prices are set by rational investors is part of the grand oral tradition in economics and is often attributed to Milton Friedman, one of the greatest economists of the century and one of the greatest debaters of all time. But the argument has two fundamental problems. First, even if asset prices were set only by rational investors in the aggregate, knowing what individual investors are doing might still be of interest. Second, although the argument is intuitively appealing and reassuring, its adherents have rarely spelled it out carefully.

Suppose a market has two kinds of investors: rational investors (rationals), who behave like agents in economics textbooks, and quasi-rational investors (quasi's), people who are trying as hard as they can to make good investment decisions but make predictable mistakes. Suppose also that two assets in this market, X and Y, are objectively worth the same amount but cannot be transformed from one into the other. Finally, assume that the quasi's think X is worth more than Y, an opinion that could change (quasi's often change their minds) while the rationals know that X and Y are worth the same. What conditions are necessary to assure that the prices of X and Y will be the same, as they would be in a world with only rational investors?

This question is complex, but some of the essential conditions are the following. First, in dollar-weighted terms, such a market cannot have too many quasi's (in order for the rational investors

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to be marginal). Second, the market must allow costless short selling (so that if prices get too high, the rationals can drive them down). Third, only rational investors can sell short; otherwise, the quasi's will short Y when the two prices are the same because they believe X is worth more than Y. The result would be no equilibrium. Fourth, at some date *T*, the true relationship between X and Y must become clear to all investors. Fifth, the rationals must have long horizons, long enough to include date *T*. These conditions are tough to meet.

Consider the example of the Royal Dutch/Shell Group, as documented in Rosenthal and Young (1990) and Froot and Dabora (1999). Royal Dutch Petroleum and Shell Transport are independently incorporated in, respectively, the Netherlands and England. The current company emerged from a 1907 alliance between Royal Dutch and Shell Transport in which the two companies agreed to merge their interests on a 60/40 basis. Royal Dutch trades primarily in the United States and the Netherlands and is part of the S&P 500 Index; Shell trades primarily in London and is part of the Financial Times Stock Exchange Index. According to any rational model, the shares of these two components (after adjusting for foreign exchange) should trade in a 60–40 ratio. They do not; the actual price ratio has deviated from the expected one by more than 35 percent. Simple explanations, such as taxes and transaction costs, cannot explain the disparity.[2]

Why don't rational investors intervene to force the shares of Royal Dutch/Shell back to their rational 60–40 ratio? The answer is that hedge funds do make investments based on this disparity: They buy the cheaper stock and short the more expensive one. Indeed, Royal Dutch/Shell is one of many such investments Long-Term Capital Management had in place in the summer of 1998. In August 1998, when things started to unravel for LTCM, the Royal Dutch/Shell disparity was relatively large, so at a time when LTCM might have chosen to increase the money it was willing to bet on this anomaly, it had to cut back instead. Shleifer and Vishny (1997) envisioned this scenario in their article explaining the "Limits of Arbitrage."

The lesson from this example is that even when the relationship between two prices is easy to calculate and fixed by charter, prices can diverge and arbitrageurs are limited in their ability to restore the prices to parity. What, then, are the prospects for prices to be rational in more-complex settings?

Take the case of Internet stocks. Many, if not most, professional analysts believe that the valuations of Internet stocks are too high. In surveys of professional investors that I conducted in the spring of 1999, the median respondent thought that the intrinsic value of a portfolio of five Internet stocks (America Online, Amazon.com, eBay, Priceline.com, and Yahoo!) was 50 percent of the market price. Suppose the "professionals" are right and these multibillion dollar companies are worth only half of their current prices. Suppose further that this valuation is the consensus of Wall Street experts. How can such a situation exist? The answer is that it may be an equilibrium (although not a "rational equilibrium") as long as the Wall Street experts are not the marginal investors in these stocks. If Internet stocks are primarily owned by individual investors, Wall Street pessimism will not drive the price down because the supply of short sellers will then be too limited. Although some hedge funds are willing to bet on convergence for the Royal Dutch/Shell disparity, few are willing to bet on the demise of the Internet frenzy, or at least too few to cause it to happen.

The analysis of Internet stocks applies with even greater force to the current level of the U.S. stock market. The consensus on Wall Street (and on similar streets around the world) is that the U.S. stock market is 20–30 percent overvalued; yet, prices can continue to increase because the investors who are willing to bet on a decline have too few dollars to prevail. First, in the U.S. market, the largest investors—pension funds, endowments, and wealthy individuals—typically use some rule of thumb for asset allocation, such as 60 percent in equities, and are thus relatively insensitive to the level of asset prices. Second, such insensitivity is even more characteristic of individual investors in 401(k) plans, who rarely rebalance their portfolios.

Evidence That Should Worry Efficient Market Advocates

The previous section showed that the premise of behavioral finance—that cognitive biases may influence asset prices—is at least theoretically possible. But is it worth the trouble? What is the evidence that existing models cannot do the job? Surely the Royal Dutch/Shell example, although striking, is not by itself enough to undermine the rational efficient market paradigm that has served the field well for so long. I will briefly discuss five areas in which behavior in the real world seems most at odds with the theories in textbooks.

Volume. Standard models of asset markets predict that participants will trade very little. The reason is that in a world where everyone knows that traders are rational (I know that you are rational, you know that I am rational, and I know that you know that I am rational), if I am offering to buy

some shares of IBM Corporation and you are offering to sell them, I have to wonder what information you have that I do not. Of course, pinning down exactly how little volume should be expected in this world is difficult, because in the real world people have liquidity and rebalancing needs, but it seems safe to say that 700 million shares a day on the NYSE is much more trading than standard market models would expect. Similarly, the standard approach would not expect mutual fund managers to turn over their portfolios once a year.

Volatility. In a rational world, prices change only when news arrives. Since Robert Shiller's early work was published in 1981, economists have realized that aggregate stock prices appear to move much more than can be justified by changes in intrinsic value (as measured by, say, the present value of future dividends). Although Shiller's work generated long and complex controversy, his conclusion is generally thought to be correct: Stock and bond prices are more volatile than advocates of rational efficient market theory would predict.

Dividends. Modigliani and Miller (1958) showed that in an efficient market with no taxes, dividend policy is irrelevant. Under the U.S. tax system, however, dividends are taxed at a higher rate than capital gains and companies can make their taxpaying shareholders better off by repurchasing shares rather than paying dividends.[3] This logic leaves us with two major puzzles, one about company behavior and the other about asset prices. Why do most large companies pay cash dividends? And why do stock prices rise when dividends are initiated or increased? Neither question has any satisfactory rational answer.[4]

The Equity Premium Puzzle. Historically, the equity premium in the United States and elsewhere has been huge. For example, a dollar invested in U.S. T-bills on January 1, 1926, would now be worth about $14; a dollar invested in large-cap U.S. stocks on the same date would now be worth more than $2,000. Although one would expect returns on equities to be higher, because they are riskier than T-bills, the return differential of 7 percent a year is much too great to be explained by risk alone (Mehra and Prescott 1985).

Predictability. In an efficient market, future returns cannot be predicted on the basis of existing information. Thirty years ago, financial economists thought this most basic assumption of the efficient market hypothesis was true (Fama 1970). Now, everyone agrees that stock prices are at least partly predictable (see, for example, Fama 1991) on the basis of past returns, such measures of value as price-to-earnings or price-to-book ratios, company announcements of earnings, dividend changes, and share repurchases and seasoned equity offerings.[5] Although considerable controversy remains about whether the observed predictability is best explained by mispricing or risk, no one has been able to specify an observable, as opposed to theoretical or metaphysical, risk measure that can explain the existing data pattern (see, for example, Lakonishok, Shleifer, and Vishny 1994). Furthermore, the charge that these studies are the inevitable result of data mining is belied by the fact that the authors have covered every important corporate announcement that a company can make. Academics have not selectively studied a few obscure situations and published only those results. Rather, it seems closer to the truth to say that virtually every possible trigger produces apparent excess returns.

What should one conclude from these and other empirical facts? On one side of the coin is my own conclusion: In many important ways, real financial markets do not resemble the ones we would imagine if we only read finance textbooks. On the other side of the coin is the compelling evidence that markets are efficient: the performance of active fund managers. Many studies have documented the underperformance of mutual fund managers and pension fund managers relative to passive investment strategies (see, for example, Malkiel 1995). Furthermore, although there are always some good performers, good performance this year fails to predict good performance the following year, on average (see, for example, Carhart 1997). These cold facts should be kept firmly in mind when evaluating market efficiency. Regardless of the results of academic studies reporting apparently successful trading rules, real-world portfolio managers apparently have no easy time beating the market.

This brief discussion of some of the empirical literature should leave the reader with a mixed impression. Market behavior often diverges from what we would expect in a rational efficient market, but these anomalies do not create such large profit opportunities that active fund managers as a group earn abnormal returns. No inherent contradiction exists in this combination of facts, although economists have often been confused on this point. A drunk walking through a field can create a random walk, despite the fact that no one would call his choice of direction rational. Still, if asset prices depended on the path the drunk adopted, it would be a good idea to study how drunks navigate.

What We Have Learned

So far, I have been considering whether behavioral finance is a worthy endeavor on a priori grounds. My conclusion, unsurprising given the source, is that we can enrich our understanding of financial markets by adding a human element. Some researchers have been at this task for quite a while, however, so it is reasonable to ask whether any real progress has been made.

Perhaps the most important contribution of behavioral finance on the theory side is the careful investigation of the role of markets in aggregating a variety of behaviors. The second generation of this kind of theorizing has recently begun. Three teams of authors (Barberis, Shleifer, and Vishny 1998; Daniel, Hirshleifer, and Subrahmanyam 1998; Hong and Stein forthcoming) have undertaken the task of generating asset-pricing models to explain the puzzling pattern of empirical results from the last decade—in particular, returns that exhibit underreaction in the short run and overreaction in the long run.[6] All three studies draw on results from psychology to motivate the behavior of the agents in their models. At the very least, these works serve as "existence proofs" for behavioral finance theorizing. That is, they show that it is possible to create a coherent theoretical model, one grounded in solid psychology *and* economics, that can explain a complex pattern of empirical results. At the moment, no rival nonbehavioral model can say the same.

Progress has also been made in understanding the equity premium puzzle by using psychological concepts. Benartzi and I (1995) argued that the equity premium can be explained by a combination of behaviors called "myopic loss aversion." Loss aversion refers to the observed tendency for decision makers to weigh losses more heavily than gains; losses hurt roughly twice as much as gains feel good. We added the adjective "myopic" because even investors with long-term horizons appear to care about short-term gains and losses. We found that if investors evaluate their portfolios once a year, loss aversion can explain much of the equity premium.

Barberis, Huang, and Santos (1999) extended this idea in an ambitious new approach. They tried to explain the equity premium within a full equilibrium model that incorporates consumption as well as returns. They could do so only by adding another behavioral factor: the "house money effect." The house money effect captures the intuition that when gamblers are ahead (playing with what they refer to as the "house's money"), they become less loss averse and more willing to take risks. Similarly, investors who have recently earned high returns will be less risk averse.

On the empirical side, much of the effort of behavioral researchers has been in uncovering new anomalies that cause us to think hard about market efficiency. Of course, these studies also create controversy because the implications of the results are subject to interpretation.

One branch of empirical behavioral research should be uncontroversial: the investigation of what individual investors do with their money. Even if individuals' actions have no effect on prices, understanding how well individuals manage their portfolios is certainly useful to investors and investment professionals. Because data about individual behavior are hard to come by, such research is less common than the usual tape-spinning exercises with CRSP and Compustat, but some data are starting to emerge. Terrance Odean has managed to get a data set of trades made by some customers of one large discount brokerage firm. His research so far has shown that important behavior documented by psychologists in the lab, such as overconfidence and loss aversion, is also displayed by individuals managing their portfolios. Odean found that individuals trade too much (overconfidently thinking that they can pick winners, whereas the stocks they buy do worse than the stocks they sell) and are reluctant to sell losers (and mentally "declare" the loss), even though tax considerations should make them prefer selling a loser to selling a winner (Odean 1998).

Another important set of individual investors, in addition to those studied by Odean, is those who invest in 401(k) plans where they work. A large and rapidly growing pot of money is being managed by individuals who, for the most part, have little or no knowledge about investing. Benartzi and I (forthcoming) have recently studied one aspect of this group's decision making—diversification strategies. We found that many 401(k) investors appear to use simple rules of thumb to invest their money, including what we refer to as the "1/*n* heuristic": If a plan contains *n* funds, allocate contributions evenly among the *n* funds. We found that when plans add a stock fund, allocation to equities rises. As the public debates the pros and cons of privatizing some or all of the U.S. Social Security system, we will need to know more about how participants will take on the task of investing their retirement savings.

What's Next: A Wish List

Forecasting the future is always difficult, and the only prediction in which I have complete confi-

dence is that 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). So, instead of predicting what kinds of research will appear in the next decade, I offer a wish list of topics that I would like to see studied.

First, I would like to see the theory papers discussed previously come to grips with institutions. Most of the anomalies that receive attention in the academic literature are stronger for small- and mid-cap stocks than for large-cap stocks. For large-cap stocks, there seem to be more anomalies on the short side than on the long side. Why? I believe that the answer depends on limits-of-arbitrage arguments, but some of the institutional barriers, such as those regarding short selling, may also have behavioral explanations. Bringing institutions more directly into the behavioral model and applying the behavioral model to institutions will be hard but worth doing.

Second, I would like to see more behavioral finance research in the field of corporate finance. Most of the research so far has been in the field of asset pricing; much less has been done on corporate finance—at least recently. My favorite corporate finance paper is John Lintner's 1956 study of dividend policy. Lintner took an unusual tack for an academic—talking to executives about how they set dividend policy. After listening, he composed a very simple model in which companies move their dividends toward a desired payout ratio while being careful to avoid the necessity of ever cutting the dividend. To this day, his model remains an accurate description of dividend policy. One example of the kind of research that it might be possible to do in the realm of behavioral corporate finance is Jeremy Stein's (1996) article "Rational Capital Budgeting in an Irrational World." Stein ponders how companies should make investment decisions if asset prices are not set rationally. Many other papers, both theoretical and empirical, are waiting to be written in this important area.

Finally, I wish for more data on individual investors to become available. I hope someday soon a scholar will acquire a data set for online traders and day traders. Until such data become available, we will never fully understand what I think will become known as the Great Internet Stock Bubble. Similarly, tracking the behavior of investors in 401(k)-type pension plans is of growing importance. Benartzi and I have been hampered in our studies by the absence of longitudinal data for plan participants. For both cases, the data exist in the files of private firms. I am hopeful that some firms will see the benefit of sharing such data with researchers; for sharing to become a reality, confidentiality will have to be adequately protected—confidentiality of the source of the data and of the identities of the individual investors.

The End of Behavioral Finance

Behavioral finance is no longer as controversial a subject as it once was. As financial economists become accustomed to thinking about the role of human behavior in driving stock prices, people will look back at the articles published in the past 15 years and wonder what the fuss was about. 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? In their enlightenment, economists will routinely incorporate as much "behavior" into their models as they observe in the real world. After all, to do otherwise would be irrational.

Notes

1. For surveys of behavioral finance, see De Bondt and Thaler (1995), Shefrin (1999), and Shleifer (1999).
2. See Froot and Dabora, who also studied the similar cases of Unilever N.V./PLC and SmithKline Beecham.
3. See Miller (1986) for a convincing summary of this argument.
4. The argument is sometimes made that prices increase when dividends increase because companies are using a change in dividend to signal something. Benartzi, Michaely, and Thaler (1997) found no evidence, however, that increases in dividends provide any information about future changes in earnings.
5. For a sampling of the empirical literature, see De Bondt and Thaler (1987), Lakonishok, Shleifer, and Vishny (1994), Bernard (1992), Michaely, Thaler, and Womack (1995), and Ikenberry, Lakonishok, and Vermaelen (1995). For an alternative interpretation of this literature, see Fama (1998).
6. That is, short-run positive serial correlation and long-term mean reversion. See the three papers cited in the text for summaries of the empirical facts and see Fama (1998) for another interpretation.

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