

Discussion of “Competing Theories of Financial Anomalies”

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In his classic 1970 paper, Eugene Fama wrote:

[Efficient markets and asset pricing] research . . . did not begin with the development of a theory of price formation which was then subjected to empirical tests. Rather, the impetus . . . came from the accumulation of evidence . . . that the behavior . . . of speculative prices could be well approximated by a random walk . . . Faced with the evidence, economists felt compelled to offer some rationalization In short, there existed a large body of empirical results in search of a rigorous theory.

Today, three decades later, the search for a rigorous theory continues. As before, stylized empirical facts constrain the various modeling efforts. What is different, Alon Brav and J. B. Heaton (2001) and the organizers of this conference acknowledge, is that behavioral finance determines much of the empirical and theoretical agenda.

Behavioral finance is new. It studies how financial decisions in households, organizations, and markets are truly made. Decision processes are often crucial to decision outcomes. This is the main reason why behavioral finance borrows ideas from psychology.¹ In contrast, modern finance is based on the classical notion of *homo economicus*, that is, the normative axioms that underlie expected utility theory, risk aversion, rational expectations, and Bayesian updating. Economics does not weigh the cost of thinking. It does not treat cognition as a scarce resource. Herbert Simon (1983) calls this approach “the Olympic model.” Regrettably, few people are olympians. In experiments, decision makers systematically and often willingly violate the axioms of rationality [Tversky and Kahneman (1986), Conlisk (1996), Rabin (1998)]. Studies of real-world financial decision making strengthen this conclusion. For instance, in prior work (1998) I have sketched “a portrait of

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¹ An additional reason is that since the 1960s economists have left decision processes largely unexamined. According to Miller (1986), “We abstract from all these [behavioral] stories . . . not because the stories are uninteresting but because they may be too interesting and thereby distract us from the pervasive market forces that should be our principal concern.”

the individual investor.” The portrait is unimpressive. I list four classes of anomalies relating to irregular perceptions of the dynamics of equity prices, perceptions of value, risk management, and trading practices. What is surprising is the failure of many people to infer basic investment principles from years of experience, for example, the benefits of diversification.

Besides decision anomalies (seen in experimental data) and anomalies in financial behavior (seen in survey and trading data), behavioral research has also recorded a series of new market anomalies, for example, excess volatility, long-run reversals in share prices, intermediate-term price momentum, the predictive power of the book-to-market value ratio in the cross section of asset returns, and the long-run underperformance of initial public offerings (IPOs) [for a survey, see De Bondt and Thaler (1995)]. The market anomalies have put modern finance on the defensive. The reasons are clear. The behavioral studies employ traditional data and methods. The tests are designed to differentiate between a rational and a behavioral hypothesis, as in the original studies of stock market overreaction.² Most important of all, the results lead one to believe that investor sentiment influences equity returns and that sophisticated investors may be able to benefit, in a methodical way, from other people’s cognitive and emotional shortcomings. Thus the human factor in asset pricing can no longer be ignored.

Modern finance has responded to the challenge in different ways. It either reinterprets the new facts as nonanomalous (e.g., the abnormal profits compensate for time-varying risk), it questions their pervasiveness and robustness [Fama (1998)] or it argues that markets may yet be “minimally rational,” in the sense that markets fail to supply opportunities for abnormal profits [Rubinstein (2001)].

Brav and Heaton’s article belongs to the first category. The authors discuss stock market under- and overreaction to news. What looks like poor judgment does not have to be poor judgment, they say. Brav and Heaton compare “a theory of rational structural uncertainty,” built on rational Bayesian learning but incomplete and uncertain information about the structure of the financial system, with the approach that first discovered under- and overreaction. They employ a simple model to demonstrate that the two theories can be hard to distinguish. In a narrow sense, this is correct since both theories generate comparable return patterns in reaction to news.³ When rational investors estimate a valuation parameter that is unstable, they give too much weight to recent data because they worry about structural change. On the other hand, once a shock has occurred, they do not give enough weight. From a behavioral perspective, investor overreaction is explained by the unwarranted use of

² This is not the case in every behavioral study. I agree with Fama that “market efficiency can only be replaced by a better specific model of price formation, itself potentially rejectable” (1998, p. 284).

³ The robustness of Brav and Heaton’s illustrations is an open question, however. The authors do not prove general theorems. For example, what happens if agents disagree about the structure of the economy?

stereotypes and a naïve emphasis on features of similarity (i.e., the "representativeness" heuristic). Underreaction occurs when new evidence runs counter to a firmly held view. People persevere in their beliefs ("conservatism"). They filter the data to confirm what they think they already know.

Many financial economists favor rational models of asset prices on an a priori basis. Only if all other attempts fail are they willing to consider behavioral explanations.⁴ The title of their article, and the way in which Brav and Heaton develop their ideas, plays into this prejudice. The two theories are in direct competition, it is said, and the rational structural uncertainty (RSU) model is a formidable opponent, not easily dismissed. In my view this assessment understates the fundamental power of the behavioral approach, and it overstates the still indeterminate contribution of rational learning. I have several specific concerns. These are as follows.

Brav and Heaton's article, I am afraid, diminishes behavioral finance. For instance, some readers may be left with the impression that it is primarily about the psychology of judgment and belief formation. But when investors develop bad habits, when they procrastinate or, just the opposite, when they engage in hyperactive (addictive) trading, these are definitely problems of rationality. The psychology of choice is important to behavioral research. The RSU model is silent on these matters.

Brav and Heaton also create a false contradiction when they state that, in behavioral finance, individuals have "considerable knowledge about the fundamental structure of the economy." (Only the RSU model, they assume, treats this presumption as invalid.) To the contrary, the issue of financial literacy, and of naïve *mental frames*, is at the core of behavioral research. A mental frame refers to the decision-maker's perspective on a particular question, for example, how the problem is formulated, and how actions and outcomes are experienced. In practice, when a financial or other problem arises, there is often no unitary model of truth, even though there are degrees of knowledge. The tacit models that people use are fluid, they can be misleading, and they are not internally consistent [see, e.g., Salter (1983)].⁵ Yet because frames simplify a complex underlying reality, and because they affect

⁴ Mark Rubinstein (2001) calls this rule the "prime directive" of asset pricing research. He traces the principle to the philosophers of the French Enlightenment. Friedrich von Hayek (1948) discusses the matter at length. He defends the English antirationalistic individualism and calls it a misconception that "Adam Smith invented the bogey of the 'economic man.'" "Human reason, with a capital R, does not exist in the singular . . . but must be conceived as an interpersonal process in which anyone's contribution is tested and corrected by others," he says. In his review of tests of the rational expectations hypothesis, Michael Lovell (1986) is also critical of the prime directive. He asks facetiously: Should the facts be allowed to spoil a good story?

⁵ Even when there is consistency, the inner logic is not Aristotelian logic of the type that says "one plus one is two." Rather the logic is psychologic. For example, in surveys, investors who tilt their portfolios toward equity are more likely to see themselves as "leaders," they "worry" less about the future, and they believe more firmly that "entrepreneurial values benefit society" [De Bondt (2001)]. Configurations of values and beliefs characterize clusters of people and cultures. The configurations are not random. Yet it is hard to derive them from axiomatic principles.

values, beliefs, and decisions, we want to understand them. For any mental frame, the critical questions are: What is in? What is out?

A related point is that people do not have many insights that are uniquely their own. Much of what we know, we accept on faith. (What happened at Pearl Harbor? Is Pluto a planet? Does eating spinach make us healthier? Are stocks the “best” investment for the long run?) Every child that is born cannot possibly re-create from scratch, in Bayesian fashion, all of our collective knowledge about the universe. Instead, mental frames are socially and culturally shared.

These shared beliefs, fabricated to a degree by opinion leaders and men in advertising, have great influence.⁶ What is troubling, however, is that many frames are both mistaken and resistant to change. The RSU model says nothing about the sources of illusions or about the mind processes that sustain them. Indeed, all the pseudo-science, myth, and superstition that envelop us (say, on the benefits of herbal medicines) seem to disprove the descriptive relevance of that model. The issue is not easily reduced to a debate about rational versus irrational learning. As I already hinted, economic self-interest matters too. Pressure groups in society spend large sums of money to shape the content of news reports, for to create a world dominated by particular problems is to create support for particular solutions.⁷

One reason why Brav and Heaton’s manuscript is enjoyable to read is that it reminds us of a grand philosophical question, asked by Thomas Kuhn: When do, and when should, scientists abandon one paradigm for another? In the present context the question becomes: Why do we need behavioral research if rational learning may yet explain troubling market anomalies?

To repeat, the sheer productivity of behavioral finance, especially in collecting empirical facts, is what got us to the question in the first place. But Brav and Heaton may also have emphasized that behavioral finance is worthwhile because it is relevant and pragmatic. It studies (and proposes to adjust) decision processes so that finance practitioners may reach better decision outcomes. Paul Slovic (1972) states it well when he writes that “a full understanding of human limitations will ultimately benefit the decision maker more than will naïve faith in the infallibility of his intellect.” Today, the list of people who want to learn about the psychology of money is nearly endless: individual investors, financial planners, money managers, investment bankers, executives who manage earnings with an eye toward the firm’s stock price. It even includes Alan Greenspan. Which rational economist would have thought

⁶ Indeed, when people interpret their lives or look for guidance, impressions of this kind may well trump experience and logic. In many cases, there is no other choice. Major personal decisions, say, relating to career choices, are not repeated so often that people can learn much from their own experience.

⁷ Marcia Angell (1996) analyzes the clash between law and junk science in the breast implant case and the economic interests that motivate it. Elaine Showalter (1997) studies chronic fatigue, Gulf War, and other syndromes as imaginary epidemic illnesses of the 1990s.

that, in the arsenal of monetary policy tools, Greenspan's spoken words are perhaps his most powerful weapon?⁸

In contrast, the rational approach has no comparable practical agenda.⁹ The RSU model starts from the premise that investors already know what is best for them. The model is specifically designed to rationalize anomalous price patterns, *ex post*. In that sense, something more is lost: discipline in methods. Brav and Heaton note with satisfaction that the RSU model is "clearly flexible enough" to explain well-documented biases like the excessive certainty or overconfidence effect. But flexibility is not a feature that we necessarily wish for in a model. If we explain everything, chances are that we explain nothing. My reading of other articles in the rational learning literature is that they justify robust decision rules. Contrary to what is observed, and also contrary to the RSU model, the rules insinuate a lack of self-confidence.

These reflections touch upon a more general complaint about the rational deductive approach. It puts a great premium on mathematical logic, but it delivers few testable predictions.¹⁰ Theory is not used in a creative way, to sail into uncharted waters. More commonly, empirical facts, no matter how perplexing, are rationalized *ex post* and the student is sent home with the suggestion that "if it still can be rational, it must be." With its requirement of "realism in assumptions," behavioral finance brings at last some discipline to financial modeling. Consider again the case of stock market overreaction. The arguments in favor are at three distinct levels. First, in experiments, subjects behave as predicted by the representativeness heuristic. Second, survey and trading data confirm that most investors love past winner stocks and hate past losers. Third, at the market level, systematic price reversals occur.¹¹

My discussion has labeled behavioral finance a research program that is productive, pragmatic, and disciplined. Before concluding, I want to add one more adjective: intuitive. It is only natural to extrapolate from investor psychology to market behavior. (It fits with what we hear in the news media.) Because the future is uncertain, asset valuation *must* depend on the quality of judgment, it seems. The counterargument, much investigated, is rational arbitrage. Brav and Heaton carefully acknowledge the theoretical limits of arbitrage once we allow structural uncertainty (or mental framing) effects.

⁸ It is fitting that the American psychologist and philosopher, William James, defined *truth* in exactly such pragmatic terms. He writes: "'Grant an idea or belief to be true,' [pragmatism says], 'what concrete difference will its being true make in any one's life? . . . What is the truth's cash-value in 'experiential terms'?' The moment pragmatism asks this question, it sees the answer" (1909, p. v).

⁹ Yet there may be some practical consequences. For example, Brav and Heaton state that the RSU model "may" imply a government role to improve information disclosure by firms.

¹⁰ Brav and Heaton do not list any new predictions that are derived from the RSU model.

¹¹ Fama (1998) and Rubinstein (2001) worry that researchers will arbitrarily pick and choose from a list of psychological biases in order to "behavioralize" market anomalies. Depending on the anomaly, at least one bias will inevitably fit the facts. I share their concern. If carefully defined, however, psychological biases are not as slippery a concept as rationality. In principle, the data can falsify the hypothesis at hand.

Modern finance, built on the logic of rational choice, helps our understanding of market behavior when the forces of arbitrage are strong. (Consider, e.g., the Black–Scholes option pricing formula.) Yet while we take pride in the progress of financial economics as a science, we should never forget that it is a social science. Much is learned, I believe, by studying how people process data and solve problems.

In a decade or two we will know more about the relative success of behavioral finance and other viewpoints, as the profession searches for “a rigorous theory” of asset prices. In an interview with *The Economist* in April 1994, Merton Miller predicted that “the blending of psychology and economics will lead nowhere.” (Miller also conceded, I might add, that the mix “is becoming popular simply because conventional economics has failed to explain how asset prices are set.”) Others, like Richard Thaler (1999), forecast the end of behavioral finance. “[It] is no longer as controversial a subject as it once was . . . In the not-too-distant future, the term . . . will be correctly viewed as a redundant phrase. What other kind of finance is there?” he asks.

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