

CHAPTER 2 TRADITIONAL VS. BEHAVIORAL FINANCE

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ABSTRACT

This chapter examines the tension between traditional and behavioral finance, which differ only in that the latter incorporates behavioral forces into the otherwise-traditional assumption that people behave as expected utility maximizers. Behavioralists typically argue that their approach can account for market inefficiencies and other results that are inconsistent with traditional finance, while traditionalists reject this new paradigm on the grounds that it is too complex and not capable of refutation. A history of behavioral research in financial reporting shows the importance of sociological factors in building acceptance for behavioral finance. Behavioral researchers should redouble their efforts to demonstrate that the influence of behavioral factors is mediated by the ability of institutions (like competitive markets) to scrub aggregate results of human idiosyncrasies. Such research will establish common ground between traditionalists and behavioralists, while also identifying settings in which behavioral research is likely to have the most predictive power.

INTRODUCTION

The traditional finance researcher sees financial settings populated not by the error-prone and emotional *Homo sapiens*, but by the awesome *homo economicus*. The latter makes perfectly rational decisions, applies unlimited processing power to any available information, and holds preferences well-described by standard expected utility theory.

Anyone with a spouse, child, boss, or modicum of self-insight knows that the assumption of *homo economicus* is false. Behavioralists in finance seek to replace *homo economicus* with a more-realistic model of the financial actor. Richard Thaler, a founding father of behavioral

finance, captured the conflict in a memorable NBER conference remark to traditionalist Robert Barro: “The difference between us is that you assume people are as smart as you are, while I assume people are as dumb as I am.” Thaler’s tongue-in-cheek comparison aptly illustrates how the modest substantive differences in traditionalist and behavioralist viewpoints can be exaggerated by larger differences in framing and emphasis, bringing to mind the old quip about Britain and America being “two nations divided by a common tongue.” (For what it’s worth, when confirming this account of the exchange, Thaler reports that Barro agreed with his statement.)

The purpose of this chapter is to guide readers through this debate over fundamental assumptions about human behavior, and indicate some directions behavioralists might pursue. The next section provides a general map of research in finance and describes in greater detail the similarities and differences between behavioral and traditional finance. The ensuing section places the disagreements between the two camps in the context of the philosophy of science: behavioralists argue, a la Thomas Kuhn, that behavioral theories are necessary to explain anomalies that cannot be accommodated by traditional theory. In return, traditionalists use a philosophy of instrumental positivism to argue that the competitive institutions in finance make deviations from *homo economicus* unimportant, as long as simplifying assumption is sufficient to predict how observable variables are related to one another.

A brief history of behavioral research in behavioral financial reporting then shows that while these two philosophical perspectives are powerful, they are incomplete. The success of behavioral financial reporting also depends heavily on sociological factors, particularly the comingling of behavioral and traditional researchers within similar departments. Because most finance departments lack this form of informal interaction, behavioralists must redouble their efforts to pursue a research agenda that will persuade traditionalists. The last section proposes a research agenda that behavioralists can use to address both their substantive and sociological challenges: developing and testing models explaining how the influence of

behavioral factors is mediated by the ability of institutions (like competitive markets) to scrub aggregate results of human idiosyncrasies. Such research should establish common ground between traditionalists and behavioralists, while also identifying settings in which behavioral research is likely to have the most predictive power.

A THREE-DIMENSIONAL MODEL OF RESEARCH IN FINANCE

A helpful way to illuminate the similarities and differences between traditional and behavioral finance is to map finance research in a matrix with three dimensions: institution, method, and topic, as shown in Figure 2.1, below.

(Insert Figure 2.1 about here)

The institution can be thought of as the topic of study of a finance researcher. As described in Bloomfield and Rennekamp (2009, p. 143),

North (1990) emphasizes “the varying meanings and usage of the concept of *institution*. One of the oldest and most often-employed ideas in social thought, it has continued to take on new and diverse meanings over time, much like barnacles on a ship’s hull, without shedding the old.” We use the term *institution* to refer to laws, common practices and types of organizations that persist over long periods of time. Thus, institutions in accounting research would include the existence of capital markets and financial reporting, managerial reporting techniques, tax laws, and auditing. Note that specific organizations are not institutions, but the types of organizations are. For example, Bear Sterns and Lehman Brothers were never institutions, but “banks” are. Sociologists emphasize that institutions include norms and beliefs that impact social behavior (Scott, 2007). Thus, we also include as institutions practices like management forecasting behavior or the nature of conference calls, and common forms of commercial arrangements and ‘best practices,’ such as long-term contracts, relative performance evaluation, and debt covenants.

The most common research methods are economic modeling and econometric analysis of data archives, with experimentation a distant third, along with a smattering of field studies, surveys, and simulations. Almost every research study published in peer-reviewed finance journals is motivated or guided by a theory, even if not explicitly stated. By far the most predominant theories are drawn from economics. These include theories of efficient markets and no arbitrage (crucial for studies of asset pricing and market behavior), agency theory

(central to corporate governance), monetary theory (in banking), and stochastic processes (for financial engineering). A growing number of studies draw their theory at least partly from psychology. Psychological research has made considerable progress over the last three decades developing robust theories of how people behave, which have been summarized into the categories of drive (fundamental motivations as described by Maslow's hierarchies of needs), cognition (how humans analyze data and draw conclusions), and affect (emotional responses to environmental stimuli, and how those responses affect behavior).

The three-dimensional model of finance research clarifies the rather slight differences between traditional and behavioral finance. Both address largely the same institutions and use similar methods. The distinction between the approaches lies entirely in their theoretical underpinnings. Many studies use econometric techniques to test psychological theories and are therefore appropriately called behavioral. Others use experimental methods to test economic theory as discussed in Chapter 6 and are therefore appropriately called traditional.

Even the distinctions in theory should not be overstated. While traditional finance incorporates no element of human psychology, behavioral finance usually incorporates *almost* no element, relying primarily on economic theory. The reason is straightforward: finance institutions place people in complex settings that are best described in terms of information, incentives, and actions that can be taken—exactly the building blocks of economic theory. Thus, behavioral studies typically include only a small element of psychology, integrated into the economic theory needed to understand the institution itself. In this way, behavioral finance adds only a slight wrinkle to traditional finance, which is to alter some of one or more facets of an assumption at the very foundation of economic theory: how do individuals behave?

ARGUING ABOUT ASSUMPTIONS: A PRIMER IN PHILOSOPHY OF SCIENCE

Disagreements about fundamental assumptions lead to various philosophical debates. The following discussion provides a brief primer on the philosophies of science that behavioral and traditional researchers in finance rely on most heavily.

Behavioralists often defend their iconoclastic approach by referring to Thomas Kuhn's (1962) popular and influential book *The Structure of Scientific Revolution*. Kuhn argues that science progresses through *paradigm-shifting* and 'normal' science. A paradigm provides a theoretical framework for researchers to test and bolster (or modify) through what Kuhn calls 'normal science.' Normal science establishes the validity of the paradigm, but may also uncover anomalies – observations inconsistent with the paradigm. New paradigms become successful only if they can explain anomalies of sufficient quantity and importance in a sufficiently simple way.

Copernicus and Einstein represent archetypal examples of scientists who introduced new successful paradigms. In Copernicus's time, Tycho Brahe had provided exceptionally detailed observations showing that planetary motion was inconsistent with a simple geocentric model of the solar system. According to the geocentric theory, planets orbited the earth, but the data indicated that they must move backwards at certain points in their path. Copernicus demonstrated that a different paradigm, in which all planets (including the earth) orbited the sun, could allow a much more elegant explanation of Brahe's observations: all planets move in ellipses around the sun, resulting in apparent retrograde motion when seen from the earth.

Einstein also provided an entirely new paradigm that replaced Newtonian mechanics. To simplify a far more complex story, Einstein's special theory of relativity (Einstein, 1920) was inspired in part by experimental observations that the speed of light in a vacuum is the same in every direction, a result difficult to reconcile with Newtonian mechanics.

The appeal of Kuhn to behavioralists is obvious. Kuhn allows behavioralists to paint the traditionalist as a modern-day Ptolemy, papering over increasingly obvious anomalies, while painting themselves as Copernicus, or even better, Einstein.

Traditionalists often show a fondness for *instrumental positivism*, a variant of a closely related set of philosophies. All variants of positivism emphasize the importance of predictive power: science is a process of deriving refutable hypotheses from a theory, and then testing those hypotheses and discarding theories that are not supported. A particularly extreme variant is Popper's *strict logical positivism* (Newton-Smith, 1981, Chapter III), which claims that theories can never be supported by evidence, they can only be refuted. Strict logical positivism is not very popular among practicing scientists for two reasons. First, most find empirical support for the theory to be persuasive evidence in the theory's favor. Second, positivism provides no guidance on the origin of theories or how scientists should choose between two theories that are supported by some evidence, but also have some predictions that are empirically rejected. However, weaker forms of positivism are shared by most traditionalists in finance.

Positivism is closely tied to *instrumentalism*, which views science as a method of identifying associations among observable variables, but does not argue that the variables themselves, or the theories that describe the relationships between these variables, necessarily describe reality. (A philosophy that does so would be called 'realism.')

Rather, variables and theories are merely tools or instruments that allow for theories to be tested. Instrumentalist positivism has a natural appeal to traditionalists because the assumption of *homo economicus* is patently unrealistic. Still, as Milton Friedman (1953) argues in his classic book *Essays in Positive Economics*, economic theory has great predictive power, and the realism of its assumptions are irrelevant. All that matters is whether economic variables behave as *if* all decisions are being made by *homo economicus*. Even in physical sciences, researchers often make assumptions they know are false, such as assuming that atoms have no volume or that velocities are linearly additive. Neither is true, but data indicate that the world behaves as *if* they

are, except at very small sizes or high velocities. Positivism also offers traditionalists another argument against behavioralists: until positivism offers a single explicit alternative to *homo economicus*, behavioral finance is irrefutable. Any apparent anomaly can be explained by offering up another post hoc psychological tendency. While few traditionalists are strict positivists (who would never place value on results that support a theory), support clearly has less value if refutation is impossible.

Kuhn's (1962) perspective is not in direct opposition to instrumental positivism. Yet, behavioralists tend to argue Kuhn against traditionalists, who reply with instrumental positivism. While both arguments have substance, they also contain a rather contentious personal element. By adopting a Kuhnian perspective, behavioralists implicitly brand their opponents as old, fading Luddites. (Kuhn famously claimed that individual scientists never change their minds; instead, fields change because the old scientists die or retire, and are replaced by a new generation of scientists who hold to the new paradigm). By emphasizing instrumental positivism, the traditionalists imply that behavioralists are arguing their case on the basis of realism rather than predictive power, and suggest that behavioralists are not even real scientists because they proffer an irrefutable theory that can be adapted ex post to accommodate almost any observation.

Here are some key paragraphs from one of the most pointed criticisms of behavioral finance, written by Eugene Fama, a founder of modern (traditional) finance. The paper was a response to two modeling papers by Barberis, Shleifer, and Vishny (1998) and Hong and Stein (1999) that used different behavioral assumptions to generate both price underreactions and overreactions, as observed in econometric studies. Fama poses himself the question of whether the empirical evidence, along with these ex post models, should convince him to "discard market efficiency." Fama (1998, p. 284) answers no, reasoning as follows:

First, an efficient market generates categories of events that individually suggest that prices over-react to information. But in an efficient market, apparent underreaction will be about as frequent as overreaction. If anomalies split

randomly between underreaction and overreaction, they are consistent with market efficiency. We shall see that a roughly even split between apparent overreaction and underreaction is a good description of the menu of existing anomalies.

Second, and more important, if the long-term return anomalies are so large they cannot be attributed to chance, then an even split between over- and underreaction is a pyrrhic victory for market efficiency. We shall find, however, that the long-term return anomalies are sensitive to methodology. They tend to become marginal or disappear when exposed to different models for expected (normal) returns or when different statistical approaches are used to measure them. Thus, even viewed one-by-one, most long-term return anomalies can reasonably be attributed to chance.

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.

Any alternative model has a daunting task. It must specify biases in information processing that cause the same investors to under-react to some types of events and over-react to others. The alternative must also explain the range of observed results better than the simple market efficiency story; that is, the expected value of abnormal returns is zero, but chance generates deviations from zero (anomalies) in both directions.

Fama's (1998) first two points question the robustness and reliability of the supposed anomalies. His last two points are that one must discard a reasonably successful theory such as market efficiency only if provided with one that not only explains what existing theory explains, but also goes further without being too complex, and while still being refutable.

While these arguments are largely what one would expect from an instrumental positivist, Fama's style of argument suggests an antipathy to behavioral work that goes beyond the data. No serious researcher in finance, behavioral or otherwise, is likely to 'discard market efficiency.' Instead, they will relax particular assumptions about individual behavior that might create modest but important deviations from market efficiency. Moreover, Fama (1998) misstates what it means for a market to be efficient. If researchers can reliably predict overreactions to ten types of events and reliably predict underreactions to another ten types of

events, the fact that the market may react appropriately on average (without conditioning on which type of event occurs) hardly counts as market efficiency. Arbitragers can simply bet on overreaction to the first ten and bet on underreaction to the second ten and earn abnormal returns. This is like saying that post-earnings-announcement drift does not exist, because even though returns predictably rise after good news and fall after bad news, there is no abnormal return if we don't distinguish whether the news was good or bad.

A third school of philosophy would suggest that Fama's (1998) position is colored more than a little by sociological forces within the scientific community itself. Sociological philosophers like Feyerabend and Lakatos (and Thomas Kuhn, at times) often cast their arguments in radical terms: that objective successes and the ability to predict the real-world are *entirely* irrelevant to their success in being adopted by other scientists, scientific "progress" is an illusion, and the path of science is *entirely* political and social. While few practicing scientists would accept such extreme claims, even fewer would doubt the influence of social and political factors in guiding research in finance, ranging from the explicit impact of financial support (from the Federal Reserve Bank, for example) to the prestige conferred by affiliation with premier institutions.

The sociological perspective suggests that behavioralists will face significant challenges in getting the much larger traditionalist community to adopt their perspective. Few faculty members at the highest ranked institutions are behavioralists, and finance departments are nearly devoid of faculty trained in the fundamental disciplines of the behavioral sciences, such as psychology and experimental methods. These facts explain why behavioral perspectives on finance appeared only recently within finance departments. For those who might think the tradition is longer, two key facts need emphasis. First, Richard Thaler, often called the father of behavioral finance, was an economist during his years at Cornell and is Professor of Behavioral Science and Economics at Chicago, not in finance. Second, much of Thaler's work *in finance* (rather than in economics or decision theory) is almost entirely devoid of behavioral content. Papers such as DeBondt and Thaler (1985, 1987) provided hotly-contested evidence of market

inefficiency. However, while the authors might have asserted that the causes for inefficiencies are behavioral, psychological explanations took a back seat to demonstrations of mispricing.

How pessimistic should behavioralists be about their future in finance? The next section provides some answers by looking at a subfield of applied finance that has debated traditionalist and behavioralist views for many decades, and one that faces different sociological forces: financial reporting.

THE RISE AND FALL AND RISE OF BEHAVIORAL RESEARCH IN FINANCIAL ACCOUNTING

The Rise of Behavioral Research in Accounting

One look at the possible future of behavioral finance is provided by the history of behavioral research in financial reporting. Financial reporting can be viewed as a subfield of finance focusing on the role of accounting data and other financial disclosures in market behavior, management decisions, executive compensation, related institutions, and the effects of those institutions on reporting decisions. Empirical research in financial accounting is typically dated to a paper by Ray Ball and Phil Brown, then of the University of Chicago's Accounting Department. Ball and Brown (1968) show that stock prices rose (fell) when firms reported earnings that were higher (lower) than expected by a simple time-series model. Their results surprised finance professors because accounting earnings are both considerably-delayed reports of financial performances and include accruals, which can be viewed as a noisy measure of the cash flows that provide the foundation of most valuation models in finance.

Ball and Brown's (1968) paper contained another surprise. Not only did the market respond sharply to the earnings announcement, but also the response continued for many months. This post-earnings-announcement drift was in direct contradiction to Fama's Efficient Markets Hypothesis, formalized in Fama, (1970), and ultimately drove what Fama (1998, p. 304) referred to as the only market anomaly that was "above suspicion". However, researchers in accounting using stock price data archives were steeped in traditional views of homo

economicus until well into the 1980s, and paid little attention to a price drift that they deemed most likely to be an artifact of a flawed model of expected returns or flawed statistical technique.

While archival researchers were solid traditionalists, they worked side-by-side with people steeped in behavioral methods even at the University of Chicago. In particular, researchers like Robert Libby drew from a rich literature examining the decision making of doctors and jurors and others in professional situations, and applied those techniques to auditors (see Libby [1981] for an excellent review and introduction). This has led to decades of research examining how auditors weigh evidence as they attest to the accuracy of account balances, and how their judgments might be affected by the order in which information is presented or whether the information includes irrelevant details.

Research on auditor behavior led to a wealth of 'decision aids': simple techniques that can be used to improve audit outcomes by limiting deviations from optimal decision making. Behavioral research also led to an early form of behavioral finance for sociological reasons: behavioral auditing researchers and traditionalist archival researchers worked together on teaching, hiring, workshops, and other departmental matters. These behaviorists began conducting experiments in financial reporting as it became clear to them that (1) decisions by individual investors drove market reactions to accounting information, and (2) decisions of individual investors were likely to be driven by the same behavioral forces that drive those of jurors, doctors, and auditors.

Many of these studies provided subjects with financial reports that contain similar information that is presented under different accounting methods. In a typical experiment (e.g., Dyckman 1964), some subjects might see financial statements reporting high income, but reporting in a footnote that inventory was accounted for under the first-in-first-out (FIFO) method, while the remaining subjects see financial statements reporting low income, but reporting in a footnote that inventory was accounting for under the last-in-first-out (LIFO) method. Analysis of the footnote would indicate that performance was identical in the two

versions, but that in a period of rising prices, LIFO accounting results in lower income and smaller ending inventory than FIFO. However, limitations to information processing and a 'functional fixation' on reported earnings might lead individual investors to assess performance more favorably for the FIFO firm.

The Fall of Behavioral Research in Accounting

Traditionalists in finance soon put a stop to this research program. In a highly influential paper, Gonedes and Dopuch (1974) proffer two arguments against applying behavioral perspectives to investor and market behavior. The first argument is that investors might well devote additional resources to understanding information they felt was more important, and that the experiments did not allow such a choice of resource allocation. The second argument spoke directly to the institutions in finance that make investors' individual limitations uninteresting. As Gonedes and Dupoch (1974, p. 106) comment:

Even if these studies were based upon an explicit theory of resource allocation by individuals, it still is not apparent that their results would be pertinent to issues of reporting to capital market agents. To see this, consider the implications of capital market efficiency and competition in the market for information.

Recall that the kind of efficient market considered here is simply a competitive market, a market within which each individual is a price-taker. Given this type of market, any generalizations made about the aggregate behavior of capital market agents on the basis of results from lab/field studies are extremely tenuous. Specifically, given an efficient capital market, studies of the behavior of particular types of investors (e.g., "average" investors or "financial analysts") are not likely to lead to reliable generalizations about the relationship between the production of accounting information and capital market equilibrium. To see this, recall that, within a competitive market, market behavior is a function of the interactions among rivalrous price-takers. The attainment of equilibrium in such a market is induced by the workings of the system as a whole, or *aggregate* market behavior, and not by the actions of particular individuals. Since the lab/field studies concentrated on individual behavior rather than competitive market phenomena, their relevance to the issues at hand seems nonexistent.

Note also that available lab/field studies fail to simulate competition among sources of information. Indeed, the information available to subjects is usually deliberately limited to accounting information. This limitation makes the settings of these studies even further removed from the setting within which the equilibrium prices of firms' ownership shares appear to be established.... To be sure, the indicated deficiencies of lab/field studies can, in principle, be overcome.

But to our knowledge, few (if any) attempts to do so have been completed or are even underway.

Gonedes and Dopuch (1974) hardly spelled the end of behavioral research in accounting. As Gonedes and Dopuch (p.106, footnote 32) indicated in a footnote to the above quotation, “This statement does not imply that lab/field approaches are irrelevant to all accounting issues. Indeed, these approaches may be particularly helpful in resolving some issues of managerial accounting.” Because Gonedes and Dopuch’s argument resonated well within the traditionalist research community, researchers trained in behavioral methods turned their attention away from financial reporting topics, reasonably assessing little chance such work would be published in top journals. (Dopuch was the editor of *Journal of Accounting Research* at the time the paper was published). Instead, they focused their efforts on the behaviors of individual managers and particularly of individual auditors because research on the latter began to receive funding from public accounting firms.

The Rise (again) of Behavioral Research in Accounting

Top journals in accounting shied away from publishing behavioral papers in financial reporting until the mid-1990s. Libby, Bloomfield and Nelson (2002) argue that two key forces led to a resurgence of such research. The first was mounting evidence that financial markets were not, in fact, informationally efficient. Bernard and Thomas (1990) and Abarbanell and Bernard (1992) provide particularly persuasive evidence in accounting that strongly supported views that the post-earnings-announcement drift, as identified by Ball and Brown (1968), was very likely to reflect inefficiency.

The second force leading to a resurgence of behavioral financial accounting research was the technological advances that allowed experimental researchers to address the deficiencies Gonedes and Dopuch (1974) indicated at the end of their quotation above: to establish equilibrium prices within a competitive market. As discussed in Chapter 6

Experimental Finance of this volume, these studies show little evidence that markets debias pricing.

Libby et al. (2002) downplayed what might have been the most important aspect of the resurgence: many accounting departments included researchers who were actively conducting behavioral research in other areas. While many of the most prestigious departments had purged their ranks of behavioralists, including the University of Chicago, Stanford University, University of Pennsylvania (Wharton), and the University of Michigan, behavioral research was active at many of the top state institutions, especially the University of Illinois, University of Texas, and University of Washington. This activity had two positive effects on the resurgence of behavioral financial reporting research. First, it meant that trained behavioralists were able to quickly shift topics back to financial reporting once they believed such research might be published in top journals, which now occurs with regularity. Second, it meant that many traditionalist researchers had been exposed to behavioral research ideas and had developed amicable working relationships with behavioral researchers in their departments.

What does this history of financial reporting research augur for behavioralists in finance? On the optimistic side, this history shows that evidence can overcome Fama's (1998) objections, just as it overcame those of Gonedes and Dopuch (1974). However, finance departments lack a faction of researchers who are applying behavioral theories to areas of finance in arguments about the discipline of market institutions are less compelling. The next section proposes a research program that can address this sociological challenge, while also addressing Fama's substantive objections.

A RESEARCH PROGRAM FOR BEHAVIORAL FINANCE

Behavioralists in finance are working hard to address Fama's 1998 critique. The bulk of behavioral finance work still consists of empirical studies demonstrating that markets or firms behave in ways that are anomalous with respect to traditional models, but are consistent with

one of the many individual behavioral tendencies identified by psychological research. The best of this research uses psychological research to predict and demonstrate an anomaly that has not yet been previously demonstrated. Traditionalists naturally rebut individual studies, leading to a back-and-forth debate over empirical methods and interpretation that is yielding a research literature in the best tradition of Kuhnian normal science. The collected mass of evidence makes headway in convincing new finance researchers that behavioral perspectives can improve predictive power, but still fails to address Fama's demand for a simple, unified and refutable theory.

Modelers have made some progress toward simplicity and refutability by demonstrating that behavioral forces can be incorporated into otherwise traditional models. Some, such as Barberis et al. (1998) and Hong and Stein (1998), try to create simple models that yield apparently incompatible outcomes (those papers seek to reconcile short-term underreactions and long-term overreactions). Others seek to identify counterintuitive results of known behavioral forces such as the Barberis and Huang's (2008) model incorporating loss aversion and framing into asset pricing to understand the equity premium puzzle. To the extent subsequent evidence supports these predictions, behavioralists can counter criticisms that their alternative is entirely post hoc.

Whether modelers will ever be able to address Fama's (1998) demand for simplicity and refutability is doubtful. Individual behavior is inherently complex and the deviations from homo economicus are so numerous that traditionalists will always be able to point to a profusion of models as evidence that behavioral finance is not simple or refutable.

What should behavioralists do? One answer is for behavioralists in finance to strive to demonstrate *interactions* between behavioral forces and institutional features. The areas of the most strident tension are those in which disciplinary institutions seem the strongest: competitive and liquid securities markets. However, traditionalists rarely argue that individuals who are not disciplined by market institutions still act like *homo economicus*. Many traditionalists are even

willing to accept that behavioral forces acting on individual managers can influence the behavior of large firms, even in the absence of labor markets, compensation schemes and corporate governance institutions. This suggests a possible common ground among behavioralists and traditionalists. Researchers in both camps are likely to agree with the following statement: Behavioral forces have a greater impact on market and firm behavior when institutions have weaker disciplinary power. This statement can be tested through a research design like that presented in Figure 2.2 below:

(Insert Figure 2.2 about here)

For simplicity, consider a hypothetical laboratory study using a market setting similar to that in Bloomfield, O'Hara, and Saar (2009), but in which some traders are given information about asset values and others are not. The uninformed traders are given injections of testosterone in cells 3 and 4, but are injected with a placebo in Cells 1 and 2. Behavioral research suggests that testosterone will make the uninformed traders more aggressive, lose more money, and drive excess price volatility. However, assume that the market used in Cells 2 and 4 allow informed traders to borrow on margin, and discipline market prices, while Cells 1 and 3 do not permit this.

The statement above predicts that, as shown in Figure 2.3, the slope of line B will be flatter than the slope of line A, because disciplinary forces would limit the effect of testosterone on market prices (as informed investors use their extra access to capital to drive price closer toward fundamental values). The most hard-core traditionalists might argue that even the weakest institutions, such as labor markets, are still adequate to eliminate individual behavioral forces, so that observed excess volatility is 0 in all four cells. The market hard-core behavioralists might argue that even the strongest institutions, such as global equity markets for Fortune 100 companies, are insufficient to discipline bias, so that observed bias is above 0 in all

four cells—but they would probably still agree that slope of line B will be flatter than that of line A.

(Insert Figure 2.3 about here)

Focusing on the interaction between institutional and behavioral factors has three key advantages for the behavioralists. First, it converts the distinction between traditionalists and behavioralists from a qualitative one to a quantitative one: the question is not whether behavioral forces always or never matter, but which institutions do a more effective job at disciplining those forces. Traditionalists have difficulty maintaining an absolutist position (effects of behavioral forces are always completely eliminated by all institutions) when the question is posed this way.

Second, focusing on disciplinary forces helps behavioralists respond to demands for simplicity. Human behavior will never be explained by simple theories. However, simple and traditional theories may determine what types of finance institutions will scrub aggregate behavior of the idiosyncrasies of individual human beings. To use a physics analogy, Newtonian physics has excellent predictive power when describing behavior of objects with low velocities and moderate sizes; otherwise, much more complex relativistic and quantum theories are required. Similarly, traditional finance will have good predictive power when institutions are highly competitive, and checks and balances scrub aggregate behavior of human idiosyncrasies. Otherwise, much more complex behavior theories are required.

The third benefit is sociological. As discussed earlier, accounting departments had a continuing presence of behavioral researchers who studied settings with weak disciplinary institutions. These researchers were able to develop behavioral theory without the added hurdle of convincing traditional researchers to accept a new paradigm in its most challenging setting (highly competitive financial markets). As evidence began to support behavioral hypotheses in those markets, these researchers were poised to address the topic. In the same way, the overall prospects of behavioral finance, particularly for those who want to address the most competitive

institutions, will be strengthened by a corps of researchers applying behavioral theory to the behavior of corporate managers and others operating in institutions of relatively weak disciplinary power.

These efforts will require behavioral researchers to think carefully about the nature of finance institutions and to characterize finance institutions in ways that emphasize the roles of both human decision making and institutional discipline. Most financial market markets do a rather poor job of this by ignoring most of the decision points and institutions. For example, the model of Barberis et al. (1998) focuses on a single representative investor subject to behavioral forces, but makes little mention of institutions, (such as competition) that discipline those forces. At the other extreme, models like DeLong, Shleifer, Summers, and Waldman (1991) show how market institutions can fail to discipline pricing errors, but the errors are generic, rather than the result of behavioral forces.

Substantial progress will come from applying behavioral perspectives to models of market microstructure, which explicate specific decisions in a clear institutional context. For example, the seminal models of Glosten and Milgrom (1985) and Kyle (1985) differ significantly in the decisions made by traders and market makers. Glosten and Milgrom assume that market makers first quote competitive prices and then investors decide whether to buy or sell at those prices. Prices change after every trade, ultimately allowing complete revelation of traders' information. Kyle assumes that investors first enter their orders to buy and sell, and the market makers compete to fill the orders at competitive prices.

Kyle and Wang (1997) show that in a Kyle-type model, overconfident traders can bias prices and survive in the long run because their aggressiveness makes others order less aggressively, allowing overconfident traders to create 'elbow room' from which they can profit, despite their bias. Such a result would not be obtained in a Glosten-Milgrom (1985) model, because overconfident traders would simply lose money on their unwise trades, to the benefit of the informed and unbiased traders. These models provide a clear identification of both the

biased decision makers and the disciplinary institutions, as well as the reason that one institution (the Glosten-Milgrom market) provides more discipline.

SUMMARY AND CONCLUSIONS

What will finance departments look like in 20 years? Richard Thaler, in an article titled “The End of Behavioral Finance,” makes this prediction:

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. (Thaler, 1999, p.17)

Thaler’s view is likely to prove optimistic. Many (or most) finance researchers are likely to be studying large, highly competitive asset markets and largely ignore behavioral modifications to traditional theory. Traditional theory will work well for these researchers, as long as they are focusing on the first-order effects of changes in finance institutions that are likely to diminish behavioral forces. Even absent these benefits, research trends simply do not allow for much more rapid change from the status quo.

Traditional researchers are likely to be joined by three groups of behavioralists. Some, who will attract the bulk of controversy, will be demonstrating that behavioral modifications can provide useful insights and incremental predictive power in even the most competitive and disciplinary institutions. Others will be demonstrating that some institutions are less effective than others at disciplining individual deviations from the *homo economicus* assumption. These researchers will be providing the fundamental groundwork needed to identify the settings in which behavioral finance is most useful, and equally important, will be stating arguments that are difficult for traditionalists to refute: behavioral approaches are more useful in some finance

settings than others. The final group will be those who identify the finance settings in which behavioral forces are widely viewed to be only weakly disciplined such as decisions by individual managers in poorly functioning labor markets. These researchers will generate little controversy, as they will engage least directly with the traditionalists. However, they will be able to provide finance departments with a continuing presence of researchers who are well-trained in behavioral finance. They can turn their attention to other fields as traditionalists lose their resistance to behavioral techniques as they are convinced by new theory and evidence, or (as Kuhn would suggest) simply retire and are replaced by others who are willing to embrace the behavioral paradigm.

DISCUSSION QUESTIONS

- How can a scientific discipline succeed if it is based on an assumption that is demonstrably false (like the assumption that humans always rationally maximize expected utility)?
- How can sociological factors influence the path of scientific fields that are supposed to be based on the predictive power of theories?
- Will behavioralists in finance ever 'win over' traditionalists, will the two groups simply co-exist side by side, or will behavioral finance die out?
- How can behavioralists ever achieve simplicity in their field when human behavior is inherently complex?

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Figure 2.1. Three-Dimensional Matrix of Finance Research

Every research study in finance can be placed in a three dimensional matrix describing the institution being studied, the theory from which hypotheses are described, and the methods used to demonstrate results.

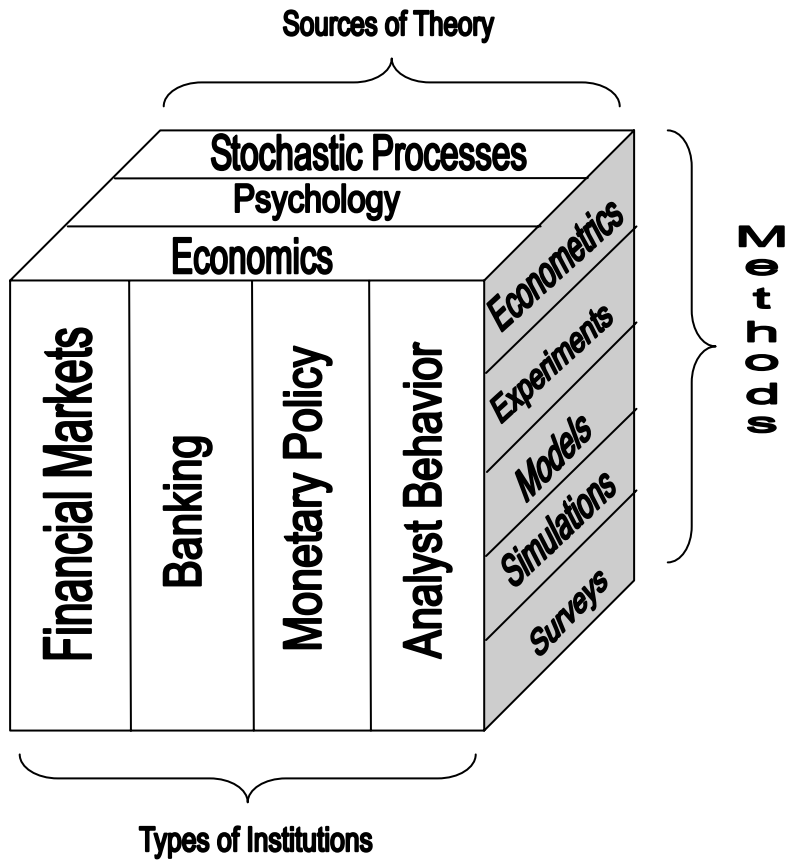


Figure 2.2. A Research Design for Behavioral Finance Studies

This research design clarifies the interaction between the strength of behavioral forces on individual decision-making and the ability of the finance institution in which individuals make decisions to eliminate behavioral forces in aggregate phenomena.

		Weak Behavioral Forces	Strong Behavioral Forces
Institutions	Strong Disciplinary Institutions	<i>(Cell 1)</i>	<i>(Cell 2)</i>
	Weak Disciplinary Institutions	<i>(Cell 3)</i>	<i>(Cell 4)</i>

Figure 2.3. Hypothesized Results of a Study Crossing Behavioral Forces with The Disciplinary Strength of Institutions.

Strengthening a behavioral force that induces biases in individual decisions should have a greater effect on aggregate phenomena in an institution with weak disciplinary forces than in an institution with strong disciplinary forces.

