The rational–behavioral debate in financial economics

Alon Brav, J.B. Heaton and Alexander Rosenberg

Abstract The contest between rational and behavioral finance is poorly understood as a contest over 'testability' and 'predictive success.' In fact, neither rational nor behavioral finance offer much in the way of testable predictions of improving precision. Researchers in the rational paradigm seem to have abandoned testability and prediction in favor of a scheme of *ex post* 'rationalizations' of observed price behavior. These rationalizations, however, have an unemphasized relevance for behavioral finance. While behavioral finance advocates may justly criticize rationalizations as unlikely to lead to a science of financial economics with improving predictive power, rational finance's explanatory power plays a key role supporting the limits of arbitrage arguments that make behavioral finance possible.

Keywords: market efficiency, event study, behavioral finance, Friedman
JEL codes: B41, G12, G14

1 INTRODUCTION

In defending their own approach and in attacking behavioral finance, researchers in the rational finance paradigm argue that they hold the high ground of testability and predictive power (see, for example, Fama 1998b and Constantinides 2002). On closer inspection, however, researchers in the rational paradigm seem to have abandoned testability and prediction in favor of a scheme of *ex post* 'rationalizations' of observed price behavior. Since the rational model has enormous flexibility to generate such rationalizations, it is nearly always possible for rational finance to 'explain' seemingly anomalous results even when behavioral explanations enjoy at least equal plausibility (see Brav and Heaton 2002). The *ex post* explanatory power of rational economics – rather than predictive success or testability – remains the method of choice for most rationalists in the rational–behavioral debate in financial economics. At the same time, however, behavioral finance advocates often reject outright such attempts to 'rationalize' price behavior. In doing so, they deny the constructive role that rational counter-explanations play in the limits of arbitrage arguments that make behavioral finance possible. Rationalizations have an unemphasized relevance for behavioral finance. While behavioral finance advocates may

justly criticize rationalizations as unlikely to lead to a science of financial economics with improving predictive power, rational finance's explanatory power plays a key role supporting the limits of arbitrage arguments that make behavioral finance possible.

In clinging to purely rational explanations without significant predictive content, researchers in the rational paradigm largely ignore the fundamental dictates (though not his practice) of Milton Friedman's 'positive economics' (see Friedman 1953a). Friedman rejected concern with assumptions in the interest of a singular focus on prediction. Thus, a serious practitioner of Friedman's methodology would not care whether assumptions are 'rational' or 'behavioral.' The primacy of predictive power should crowd out any affinity for particular portions of the assumption space. Rational finance's devotion to rationality requires something that is hard to square with Friedman's dictate: a highly restricted 'assumption space' that excludes all assumptions inconsistent with rational behavior. Put another way, rational finance's adherents implicitly assert that 'assumptions really do matter' though this position is inconsistent with the methodological view that justified 'unrealistic' assumptions of pure rationality in the first place. The persistent focus on *rationality assumptions* rather than *predictions* may help explain the lack of predictive success in rational finance. With the exception of advances in derivative pricing, few of the top achievements in rational finance demonstrate significant predictive advances.

Despite its limited success in terms of testability and prediction, rational finance enjoys a hidden relevance to behavioral finance. Rational finance has long responded to behavioral challenges by positing rationality-based counter-explanations to explain price behavior that is arguably irrationality-induced.[1] By producing purely rational explanations that can explain observed asset prices – 'rationalizing' those prices – rational finance actually nurtures the growth of behavioral finance. If investors, like researchers, cannot be sure that anomalies are caused by irrational behavior, then they may be unwilling to commit capital to betting against them (see Shleifer and Vishny 1997). Being wrong about an anomaly's causal factors means bearing potentially large amounts of unwanted systematic and idiosyncratic risk. The steady supply of rational explanations provided by rational finance lends plausibility to behavioral finance's counter-explanations by supporting 'limits of arbitrage' arguments.

Our view of the rational–behavioral debate is both critical and hopeful. It is critical in the sense that the taunts traded by each side in the rational–behavioral debate are often both inconsistent and unconstructive. Rational finance advocates have long criticized behavioral finance for lacking novel and quantifiable predictions of financial market behavior. But rational finance itself has few achievements of that sort. At the same time, behavioral finance advocates criticize the effort to 'rationalize' behavior by factoring information sets, utility functions, and transactions costs into the

rational choice model (see De Bondt 2002). Behavioral finance advocates are perhaps right to criticize the flexibility of the rational apparatus. But they go too far when they deny the role that 'rationalization' plays in supporting the limits of arbitrage.

More hopefully, our examination of the rational–behavioral debate may help identify present and future lines of productive research that combine the insights of rational and behavioral finance. Rational finance, for example, has begun to take more seriously the problem of investor uncertainty about the fundamental structure of the economy. Several recent papers explore the role that such uncertainty can play in generating financial anomalies (see, for example, Lewellen and Shanken 2002, Cagetti *et al.* 2002). The line between rationality and irrationality often blurs in such models (see Brav and Heaton 2002). Some behavioral finance papers have begun to take limits of arbitrage arguments more seriously, exploring the *empirical* applicability or inapplicability of these assertions with respect to certain financial anomalies (see, for example, Lamont and Thaler 2003, Mitchell *et al.* 2002) rather than treating the "limits of arbitrage" as a defense of behavioral finance theory that can simply be invoked without serious analysis. It remains to be seen whether recognition of links between rational and behavioral finance can create a science of financial economics with greater predictive power.

The paper proceeds as follows. Section 2 explores the nature of rational finance, arguing that it is mostly aimed at "explanation" rather than testability and prediction. Section 3 focuses on the relevance of rational explanations for the behavioral finance approach. Section 4 concludes.

2 THE NATURE OF RATIONAL FINANCE

Rational finance's mantra of testability and prediction traces back to Milton Friedman's famous essay, 'The methodology of positive economics.' According to Friedman (1953a: 7):

> The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed.

In the jargon of philosophers of science, Friedman's methodological approach falls into a broad category of 'instrumentalism,' the view that theories are tools for prediction which merely organize descriptions of phenomena and enable us to draw inferences from past to future (see Nagel 1961). By contrast to 'realists' about scientific theory, instrumentalists are indifferent to questions of whether theories and laws are literal assertions about the world, or whether the explanatory entities and processes they describe actually exist. Questions about the truth of theories are distinct from questions about their predictive power. The most controversial

consequence of Friedman's approach – the idea that the realism of 'assumptions' is irrelevant to the assessment of a theory – purportedly follows from the primacy of predictive success.[2] Put simply, those who worry about the realism of assumptions unnecessarily constrain the 'proper' objective function of a predictive science. Since that constraint may eliminate highly 'unrealistic' assumptions that nevertheless maximize predictive power, the constraint hinders the 'ultimate goal of a positive science' and is therefore ill-advised. This view justifies both highly unrealistic '*as if*' assumptions of investor rationality and a purposeful lack of concern with institutional features and 'real' causes of individual behaviors.[3]

If taken seriously, Friedman's instrumentalism offers a Faustian bargain: Give up hope for *explanation* – the identification of the causes and effects in economic phenomena – and secure in exchange the possibility of greater predictive success. Freed from the constraint that assumptions be causally 'realistic,' the researcher may pluck assumptions from anywhere in the assumption space. Assumptions can be rational, behavioral, make-believe, or just plain silly, so long as they are internally consistent and combine to generate models that make testable predictions whose predictive power improves in precision with further testing and refinement. But assumptions from a wholly unconstrained assumption space – even when internally consistent in any given model – are likely to imply 'causal' explanations that are unconvincing because the assumptions are too much at odds with our beliefs about the world. Further, the assumptions that work for one type of prediction might imply a causal explanation that is at odds with the causal explanation implied by a different model that predicts well for a different type of prediction. This would leave researchers with models that are good for testability and prediction, but bad for causal understanding and explanation. If causal explanations are to be consistent, then assumption spaces may have to be constrained. The Faustian bargain of instrumentalism gives up the guarantee of explanatory consistency in return for freedom from the bonds of constrained assumption spaces. The net reward should be greater predictive success.

On inspection, however, it is clear that rational finance does not accept the Faustian bargain of Friedman's instrumentalism. Rather, rational finance seeks to 'explain' price behavior with coherent stories based on a constrained portion of the assumption space – that portion that includes only assumptions consistent with complete rationality. This preference is revealed throughout rational finance research, but nowhere more explicitly than in the recent paper by Rubinstein (2001).[4] Perhaps with tongue in cheek, Mark Rubinstein nevertheless writes (emphasis in original):

> When I went to financial economist training school I was taught "the Prime Directive": *explain asset pricing by rational models. Only if all attempts fail, resort to irrational behavior.* That is, as a trained financial

economist, with the special knowledge about financial markets and statistics that I had gained and aided by the new high tech computers, databases and software, I must be careful how I used my power. Whatever else I did, I should follow the Prime Directive.

It soon becomes clear that 'all attempts' to 'explain asset pricing by rational models' does not necessarily mean 'all *plausible* attempts' or 'all *reasonable* attempts' or 'all those attempts *grounded on empirical evidence*.' Instead, Rubinstein seeks to enforce an injunction against behavioral finance *in toto*, unless no possible rational explanation is available. He goes on:

> The burgeoning behavioralist literature indicates that many behavioral finance economists have lost all the constraints of this directive–that whatever anomalies are discovered, illusory or not, behavioralists will come up with an explanation grounded in systematical irrational behavior.

Rubinstein's view is that '[w]ith patience, we will find that the anomalies that appear puzzling today will either be shown to be empirical illusions or be explained by further model generalization in the context of rationality.' He traces the 'Prime Directive' back to the ancient Greek attachment to reason, forward to Descartes' conviction that all men are equally rational, and ultimately to Darwin's claim that rationality might have been selected for. Recognizing that what was selected for was only being smarter than one's competitors, but not necessarily being rational, Rubinstein turns to a normative justification of his assumption: Adam Smith's insight that self-interestedly rational conduct will, through the invisible hand, attain a social benefit to rational agents. On that basis, Rubinstein infers that the allocative efficiency of the American economy over two centuries proves that economic agents had to have been pervasively rational.

Of course, it is too easy to question the bases of Rubinstein's faith in the Prime Directive, and there is a good chance that Rubinstein is just poking a bit of fun at himself and his colleagues. But even if Rubinstein himself is not as zealous a believer as his words might suggest, we venture that many rational finance advocates would defend these views with fury. That is a tough task. Though, as Rubinstein correctly notes, economists have long had faith that the price-mechanism is the most effective way to aggregate information, this is true even when individuals employ information imperfectly, when prices reflect irrational choices, and when imperfections in the market prevent agents – rational or not – from attaining optimal outcomes. Rubinstein writes, 'the securities market today probably does a better job than ever before of aggregating the wisdom of those that trade in it'. True enough, but it may do just as good a job of aggregating the unwisdom (see Shiller 2000).

Like many rationalists, Rubinstein employs two old saws in defense of the Prime Directive that might support rationality, but only when harnessed together with further controversial assumptions. The first is that a trading strategy that exploits irrationality to earn profits will attract imitators and counteracting strategies. The second is the old argument from Alchian and Friedman that, echoing Darwin, irrationality will be selected against, or has been selected against, for long enough to be or to have already been driven to extinction. So far as the first consideration is concerned, the literature on limits to arbitrage, examined below, provides sufficient caution against the self-limiting nature of irrational asset pricing. As far as the second, natural selection famously satisfies – selects the best available quick and dirty solution to a survival problem, not the elegant and slowly perfected one. Whether that solution is 'rational' is an empirical question tautologically assumed away by the Prime Directive.

But criticizing Rubinstein's representation of the 'Prime Directive' is surely less interesting than understanding the admissions that lie beneath his statements. Despite their stated commitment to the goals of testability and prediction, researchers in the rational paradigm show little advancement in those terms and Rubinstein's article suggests why. True prediction and testability are perhaps not what rational finance is trying to do.[5]

Since testability and predictive success are by definition an empirical matter, it is important that we now turn attention to what followers of rational finance, like Rubinstein, characterize as their most significant achievements. Do these achievements evidence predictive success? Our list of top achievements (admittedly subjective) includes the principle of 'no arbitrage,' market efficiency, the net present value decision rule, derivatives valuation techniques, Markowitz's (1952) mean-variance framework, event-studies, multifactor models such as the APT, ICAPM, and the Consumption-CAPM. Without delay, we consider the nature of each of these in turn.

Consider first the principle of 'no arbitrage.' As a theoretical assumption, lack of arbitrage says that in perfect markets that are free of transactions costs and any other impediments, two assets providing a claim to identical future cashflows ought to sell for the same price in equilibrium. The generality of the idea has allowed financial economists to derive powerful restrictions and important insights on the relation between the prices of various assets and, to a large extent, many of the achievements listed below are different incarnations of this principle. The generality has a high cost, however, since the usefulness of 'no arbitrage' hinges on additional assumptions such as the specific market structure, investors' beliefs and preferences. These auxiliary assumptions limit its empirical applicability and thus its testability. The concept provides little to work with except in the most trivial of circumstances (the price of an identical loaf of bread in the same supermarket) or in the world's best developed and standardized derivatives markets (see below).

Second, consider the idea of 'market efficiency' as a top achievement (see Fama 1970). Much like 'no arbitrage,' we cannot map market efficiency to observables without additional predictions regarding the nature of the equilibrium asset-pricing model (the 'joint hypothesis problem').[6] Market efficiency cannot generate predictions of increasing precision since precision must come from the missing ingredients – namely, the asset-pricing model used by investors and the prevailing market structure that they face. Even as a theoretical matter, Grossman and Stiglitz (1980) have argued that equilibrium asset prices cannot reflect all available information when information is costly. Empirical testability of this modified form of market efficiency hinges itself on the specification of quantities such as information costs, risk aversion, degree of noisiness, and how these quantities vary over time (see also Lee 2001). With so many unobservables underdetermined by the data, market efficiency remains an explanation without serious predictive ability.

Third on the achievement list is the criterion of net present value ('NPV'). Managers are instructed to calculate the present value of an investment opportunity's expected cashflows using a cost of capital that is appropriate given the cashflows' systematic risk. The calculation of an NPV, however, is simply another manifestation of the principle of no arbitrage and consequently suffers from the same deficiencies. Expected cashflows and their discount rates are not observable, and tests that have been conducted of the NPV criterion are not encouraging (see Gilson *et al.* 2000).[7]

As a fourth achievement, consider derivatives valuation techniques, which, undoubtedly, have proven to be the closest to predictive success. If transactions costs and other imperfections that prevent arbitrage can be disregarded then the price of the derivative asset can indeed be predicted. Three caveats are due, however. First, derivative contracts can be mispriced without the opportunity for arbitrage. For example, consider put contracts in states of the world in which investors cannot short the underlying stock (e.g., Evens *et al.* 2002, and Lamont and Thaler 2003). In this case market makers price the puts to reflect the higher costs of shorting. As a result, the puts trade at a higher implied volatility, violating put-call parity. The degree of this violation is difficult to predict. Second, as the collapse of Long Term Capital Management demonstrated, derivative pricing models are susceptible to misspecification of the process governing the value of the underlying asset. This can cause arbitrageurs to use bad models that seem to 'work' for several years but which ultimately result in large losses. Finally, option prices were apparently incorrect prior to the introduction of the Black and Scholes option-pricing model, deviating substantially from the new model's 'no arbitrage' predictions and suggesting the possibility of large profits to be earned at the expense of 'irrational' market makers (see MacKenzie and Millo 2001).

Despite these weaknesses, however, the relative success of predictive models in derivative pricing is undeniable. Tellingly, derivative pricing theory may be the only theoretical endeavor in which assumptions regarding market structure are reasonably 'realistic'. Most derivative markets where pricing models work *are* liquid with low transactions costs and fewer market imperfections than other financial markets. In addition, the Black and Scholes assumption regarding the stochastic process of the underlying asset has proven to be robust to various generalizations.[8] The important lesson is that the success of derivative pricing may be due to the fact that successful derivatives pricing models are hardly 'as if' models at all. Instead, these models may be better characterized as 'as is' models in that their important assumptions tend to mirror those we observe in reality.

Consider next Markowitz's (1952) mean-variance framework and the capital asset pricing model (CAPM). Unquestionably, these important contributions have reinforced the *normative* importance of diversification and covariance risk. Yet, are they of the kind that generate refutable *predictions*? Consider first mean-variance analysis. The key inputs into the computation of optimal portfolio weights are expected returns, variances and covariances which must, in turn, be determined by an unspecified asset pricing model. Does the CAPM fair any better? Roll's (1977) insightful article questioned the very testability of the CAPM and has never been refuted. Admittedly, the CAPM and its early tests were geared towards the kind of bold predictions that Friedman has advocated, but CAPM has been refuted to the extent it has been tested (see Fama and French 1992).

Our list contains additional achievements. Among them are event-studies, multifactor models such as the APT, ICAPM, and the Consumption-CAPM. But accumulated event study evidence proves only that prices typically react *quickly* to new information, not that prices react *correctly* (see Brav and Heaton 2003). As for multifactor models, they specify neither the number nor the names of their factors (see Fama 1998a). This means that empirical investigators have no *ex ante* way to map these models to observables, and few constraints on their ability to label *ex post* covariance as 'risk' (see, for example, Fama and French 1996). Finally, as we pointed out with respect to the CAPM, early versions of the Consumption-CAPM that did indeed make refutable predictions (see Breeden 1979) have been rejected as well (see, for example, Hansen and Singleton 1982 and Mehra and Prescott 1985).

The focus on *ex post* explanation and the absence of significant examples of *ex ante* predictive success in rational finance's 'top achievements' seems far from Friedman's asserted ideal. Rational finance's self-imposed constraint to certain portions of the assumption space cannot have its sources in their predictive success, or in any instrumentalism about theorizing. As all students of constrained maximization could point out, the self-imposed 'rationality' constraints may have limited rational finance's predictive

success. For all their normative power and mathematical beauty, the tools of rational finance have not given settled answers to some of the most basic questions of financial economics, e.g., What is the cost of capital for this firm? What is its optimal capital structure? Is rational finance trying to answer these questions? Is *ex post* "explanation" more rewarding? Is it the only available option?[9]

3 THE RELEVANCE OF RATIONAL FINANCE

Despite its general lack of testability and predictive success, rational finance is not irrelevant to behavioral finance. The explanatory power of the rational framework – if not its testability and predictive success – plays an important but unemphasized role supporting the plausibility of behavioral explanations. Rather than incompatible, the rational and behavioral approaches often intertwine, a fact their advocates nonetheless seem loathe to admit. [10]

The role of rational finance in behavioral finance starts with the standard objection to behavioral finance: that competitive arbitrage will drive to zero any mispricing caused by behavioral traders' bad investment strategies. That idea has a considerable pedigree in modern financial economics (see Friedman 1953b; Fama 1965) and remains the quintessential objection to behavioral finance. Behavioral finance's answer – the 'limits of arbitrage' argument – is used to defend the persistence of irrationality-induced mispricing. The basic argument is easily summarized. In Shleifer and Vishny (1997), the most important if not seminal contribution to the literature, the authors argue that arbitrageurs typically speculate with other people's money. Because those people tend to withdraw funds after poor performance, arbitrageurs may be unable to keep funds fully committed against episodes of mispricing. Why do the arbitrageur's rational investors leave money on the table by withdrawing funds when large profits are within grasp with the exercise of a little patience? Shleifer and Vishny (1997) assume that while arbitrageurs can identify irrationality-induced mispricing, the incompleteness of their evidence (the same incompleteness that daunts scientific theory) prevents them from completely convincing their (rational) investors of its existence.[11] The results are short investment horizons and and unwillingness by rational investors to bet fully against alleged mispricing (see also, De Long *et al.* 1990). Short horizons and a lack of committed capital prevent the arbitrageur from acting to squeeze out the mispricing, thereby allowing irrationality to survive.

The problem of keeping funds committed occurs because although the irrationality-induced mispricing is identifiable by the arbitrageur, the arbitrageur's investors find it difficult to distinguish rational from irrational price behavior. Consider the assumption of Shleifer and Vishny (1997: 38, 40 (emphasis added)) on the key factor limiting investor commitment to arbitrage despite the assumed rationality of both arbitrageurs and investors:

> Both arbitrageurs *and their investors* are fully rational. . . . We assume that investors have no information about the structure of the model determining asset prices. . . . Implicitly *we are assuming that the underlying structural model is sufficiently nonstationary and high dimensional that investors* [who provide arbitrageurs with funds] *are unable to infer the underlying structure of the model* from past returns data. . . . Under these informational assumptions, individual arbitrageurs who experience relatively poor returns in a given period lose market share to those with better returns.

Why is this important? It is important because if it were easy for rational investors to identify the mispricing perceived by the rational arbitrageur, then the asserted funds commitment problem is implausible. If a rational arbitrageur can convince his rational investors that current price patterns reflect cognitive biases of non-rational agents, he will have access to capital that will allow him to bet against that mispricing. Recall that short horizons and limited resource commitments among arbitrageurs result from the inability to *persuade investors* to keep those funds committed. If a rational arbitrageur can identify a clear instance of mispricing, and can communicate this to those with capital to invest with the arbitrageur, then that capital will stay committed to the arbitrageur's strategy. If it is easy to identify irrationality-induced mispricing, then it is easy to keep capital committed to the exploitation of the anomaly. If it is easy to keep capital committed to the exploitation of mispricing, then there are no limits to arbitrage, and no long run non-rational mispricing is likely to survive.

It is here that rational finance enjoys an unemphasized relevance for behavioral finance. The key to the limits of arbitrage in Shleifer and Vishny (1997) is the existence of uncertainty on the part of rational investors, not cognitive biases. It is simply too easy to "rationalize" most financial anomalies in one way or another and this plainly impacts arbitrage activity. One possibility is that anomalies compensate for risk (see Fama and French 1996). Another is that anomalies occur by chance or data mining (see Fama 1998b). But in allowing that anomalies are consistent with rationality through the possibility of both risk and chance, the rational theorist concedes just what the behavioral financial theorist requires. In such an environment, rational arbitrageurs may be unable to convince their rational investors to invest capital into an arbitrage opportunity that could be driven by chance or expose them to unwanted systematic and idiosyncratic risk. It is the pervasive inability to distinguish rational and non-rational behavior – a real world rational-behavioral debate – that lends plausibility to behavioral explanations *ab initio*.[12,13]

In evaluating our claim that 'rationalization' is an important limit of arbitrage, consider two examples: (1) the build-up and blow-up of the Internet bubble, and (2) the superiority of value equity strategies. During

much of time the Internet bubble existed, many investors believed that Internet stocks were overvalued and that this overvaluation was related to the naïve strategies of individual investors and to the ultimately unsustainable momentum trading of some institutional investors. Yet many of these arguably rational investors were unwilling to make large bets against the perceived mispricing. Several of the best known hedge fund investors took large losses betting both with and against such stocks and some exited the business entirely.[14] The Internet bubble is also interesting because many market analysts offered 'stories' and new valuation methods meant to rationalize the large market capitalizations attached to certain technology companies.[15] Skeptical investors who wanted to bet against perceived mispricing had to confront these rationalizations. Their inability to be certain that Internet and technology stocks were mispriced probably played a role in limiting arbitrage against this (now widely agreed) mispricing during the bubble period.

The superiority of value strategies is well-documented in both the behavioral and rational literatures (see, for example, Lakonishok *et al.* 1994, Fama and French 1992). Behavioral finance advocates attribute the superiority of value strategies to the tendency of investors to extrapolate past results too far into the future (De Bondt and Thaler 1985, Lakonishok *et al.* 1994). As with other behavioral stories, the superiority of value stocks requires some limit to arbitrage that allows mispricing to persist. Rationalizations provide one such limit, and probably an important one. The asserted possibility that the superiority of value strategies manifests the greater riskiness of such stocks (see Fama and French 1993) may be sufficient to limit arbitrage against possible mispricing. If it was otherwise and there was no plausible rational story at all for the superiority of value stocks, it would be difficult or impossible for behavioral advocates to justify the extrapolation explanation. This is especially true since there are few other plausible limits to bets on the relative pricing of value and growth stocks.[16] More likely, the difficulty of detecting whether prices are rational or not plays a crucial role in the ongoing debate about this and other financial anomalies (see Summers 1986).

All this suggests that behavioral finance should embrace the role of 'rationalization' in supporting the limits of arbitrage. A more thoughtful search for the foundations of limits of arbitrage may reveal just how necessary the rational explanation is to the limits of arbitrage and thus to the survival of irrationality-induced mispricing. This is not to say that 'rationalization' is good science, or that rational finance advocates should be content with low predictive power. Rather, we suggest that it is the very flexibility of rational modeling that may help support the existence of irrationality-induced anomalies in financial markets (see Brav and Heaton 2002). At the extreme, perhaps *every* equilibrium prediction that assumes the survival of irrationality may require a (shadow) prediction from some rational model.

This further suggests that our hope in rational arbitrage may be misplaced. Rational arbitrage is likely to bring prices back not to the point where they are obviously rational, but only to the point where they are no longer obviously irrational.

Of course, rationalization is not the only limit to arbitrage. Behavioral finance advocates point to other limiting mechanisms as well, including slow information diffusion and short-sales constraints. We think that these mechanisms do play some role in limiting arbitrage, but they may be uninteresting to behavioral finance by comparison to rational uncertainty because they would vastly limit the scope of behavioral finance.

For example, 'information' about most controversial assertions of mispricing is almost universally and quickly available (for example, 'twin shares' like Royal Dutch and Shell Transport, internet carve-out mispricing, and index inclusions). The same is true for broad market price levels and the demonstrated predictive relevance of P/E ratios.[17] That leaves short-sales constraints to carry a heavy load. Of course, short sale constraints do nothing for behavioral explanations of prices that are too low. Even where prices are too high, the cost of shorting simply puts an arbitrage bound around possible mispricing. It does not explain the reluctance of investors to short against mispricing that exceeds those bounds.

Most of behavioral finance is not focused on mispricing that can be explained by information diffusion or short-sale constraints alone. We believe that if behavioral finance advocates (including those in the real world markets) start matching the anomalies they care about to the limits of arbitrage created by information diffusion and short-sale constraints, they will be unsatisfied with the result. Behavioral finance – both in the academy and the markets – does not aspire to be just a science of those anomalies that can be explained by such limits. The new paradigm must generate predictions across all firms and across time. Institutional limits are neither necessary nor sufficient for this paradigm.

4 CONCLUSION

The most important philosophical concern facing financial economists today is the contest between rational and behavioral finance. Researchers in the rational paradigm typically assert that behavioral models employ ambiguous assumptions of irrationality undisciplined by rigorous mathematics, resulting in models that lack novel testable predictions of financial market behavior. Researchers in the behavioral paradigm criticize the failure of rational finance to generate meaningful predictive successes and dismiss as illusory the rigor of the rational approach, since its mathematically sophisticated models fail to identify measurable economic variables. Each accuse the other of owning so much modeling flexibility that anything can be explained, in behavioral models through employment of selected

psychological biases, and in rational models through assumptions about information sets, utility functions, and transactions costs. Consistent with their self-images as empirical scientists, participants in the contest between rational and behavioral finance assert their own commitment to 'testability' and 'prediction' while accusing the other of failing to commit to those goals. At the end of the day, however, the pretended debate over 'testability' and 'prediction' often hides the real successes and failures of both sides, and masks the interesting but unexplored links between the two approaches.

Alon Brav and Alexander Rosenberg
Duke University
brav@duke.edu
and alexrose@duke.edu
J.B. Heaton
Bartlit Beck Herman Palenchar & Scott
j.b.heaton@bartlit-beck.com

ACKNOWLEDGEMENT

NOTES

1 See, for example, the debate regarding the excess volatility of stock prices initiated by Shiller (1981) and the response by Kleidon (1986) and Marsh and Merton (1986).
2 While Friedman's methodology has long been criticized (mostly by philosophers, but also by behavioral economists like Herbert Simon), his work has been, by far, the most influential methodological statement in economics in this century. A complete treatment of Friedman's views is far beyond the scope of this paper. For extensive analysis, see Rosenberg (1976: 155–70, and 1992: 57–62) and Hausman (1992: 164–8). Hausman (2001) argues that contrary to Friedman's approach economists should be interested in explanation and in the diagnosis of cause and effect.
3 Friedman (1953a: 31) notoriously wrote that 'answers given by businessmen to questions about the factors affecting their decisions [is] a procedure for testing

economic theories that is about on a par with testing theories of longevity by asking octogenarians how they account for their long life...'.

4　See Mark Rubinstein (2001) titled 'Rational markets: yes or no? The affirmative case'. The article was motivated by a debate between Richard Thaler and Mark Rubinstein in November 1999 sponsored by the Berkeley program in finance.

5　Criticism of other areas of economics (game theory and choice theory) is voiced by Ariel Rubinstein (2001: 617). He states: 'I doubt that economic theory delivers the goods. We do not make predictions anything like those made in the natural sciences. The link between economic theory and practical advice is tenuous, if it exists at all. Academic economists like to emphasize the usefulness and applicability of what they are doing. This might be a result of guilt, or because we honestly want to save the world, or that we have a vested interest in this position or perhaps because it is indeed useful. However, let me say that after so many years in the profession I have yet to see a case in which a game theory or choice theory model (including of course my own...) contributed an insight that clearly should have influenced the real world. I cannot think of a case that I can use to convince the skeptics. Even if they do exist I doubt their benefits "justify" the investment societies make in our profession.'

6　Philosophers of science have long recognized that the verification or falsification of scientific laws cannot be achieved with a finite body of evidence. At most theories can be confirmed. Moreover, when we test hypotheses we do not test one part of a theory at a time but rather the body of central hypothesis along with other auxiliary propositions and stated *ceteris paribus* conditions are tested against observations. Since each of these components of a theory may be varied in response to disconfirming evidence, empirical data always underdetermines theory, as Duhem and Quine have argued. (see Cross 1982).

7　We cannot observe expected cash flows or discount rates, and we observe only one realization per period from the overall distribution of payoffs. Hence, it is nearly impossible to 'test' whether discounted expectations of these payoffs equal market values. Despite these limitations, Gilson *et al.* (2000) test the NPV criterion and find very large valuation errors in their study of discounted cash flow methods in bankruptcy. Kaplan and Ruback (1995) claim greater success, but their tests are controversial. They compare transaction prices of highly leveraged transactions to valuations performed on the projections developed in such deals. Because transaction prices may be hard-wired to projections, there are endogeneity problems in the Kaplan and Ruback (1995) study that may call its results into question.

8　By robust we mean that more complicated models usually generate prices that are within the bid-ask spread about the Black–Scholes price. Furthermore, consider the evidence in Dumas *et al.* (1998) who study the performance of option pricing models in which volatility is a deterministic function of price and time. They show that the predictive and hedging performance of these option models is no better than a simple implementation of the Black–Scholes model.

9　Similar questions have been raised about the law and economics movement. (See Schroeder 2001).

10　We focus here on the relevance of rational finance for behavioral finance, but the dependence goes in the other direction as well. Rational finance often masks its dependence on irrationality by labeling it 'noise' but some sort of less than rational behavior is often needed in rational finance, for example, to generate trade in financial markets. (See Kyle 1985).

11　As Bertrand Russell has said: 'The whole problem with the world is that fools and fanatics are always so certain of themselves, but wiser people so full of doubts.'

12 Of course, the rational 'stories' that float around in financial markets are often different than the rational 'stories' that float around in academia. But whether the 'rationalizations' provided by market participants map one-for-one to academic theory is less important than the fact that many real world anomalies are protected by 'rational' explanations for their occurrence (e.g., 'New Economy' thinking). In many cases, even these real world rationalizations are supported by a real world nod to the Efficient Markets Hypothesis in a Candide-inspired assertion that the prices we see must be the best possible prices in any case (see Shiller 2000).

13 We have emphasized in this section the relevance of rational finance to behavioral finance. This dependency, however, cuts both ways. Rational finance frequently employs 'noise traders' that enable trade in financial markets (e.g., Kyle 1985). Consequently, if rational finance seeks to increase its predictive success, interest should be in improvements in predictions about the behavior of these 'noise traders'.

14 See, for example, Gregory Zuckerman, 'Hedged out: how the soros funds lost game of chicken against tech stocks', *The Wall Street Journal*, May 22, 2000: A1.

15 See, for example, Shawn Tully, 'Has The Market Gone Mad?' *Fortune Magazine*, January 24, 2000.

16 Other explanations for the persistence of value stock superiority include agency problems in the money management industry (see Lakonishok *et al.* 1992) and the distorting effects of prudent man laws (see Del Guercio 1996). Both of these explanations support our argument. When it is hard to identify mispricing and distinguish it from other rational stories, investments that turn out badly *ex post* may be difficult for arbitrageurs to defend, either to their bosses or to courts. If mispricing was easy to identify, there is no obvious reason why either agency problems or prudent man laws should limit investment in value stocks.

17 Information diffusion is likely to be very important in limiting some forms of day to day arbitrage. Consider the case where a hedge fund acquires inside information about company fraud. That hedge fund may need to limit its bets until it can be confident that a sufficiently large number of other investors have learned about the fraud as well. Otherwise, prices may move against the hedge fund for a long period of time. While such examples are important, they are not the subject of most behavioral finance interest in limits of arbitrage.

REFERENCES

Brav, A. and Heaton, J.B. (2002) 'Competing theories of financial anomalies', *Review of Financial Studies* 15(2): 575–606.

Brav, A. and Heaton, J.B. (2003) 'Market Indeterminacy', *The Journal of Corporation Law* 28(4): 517–39.

Breeden, D.T. (1979) 'An intertemporal asset pricing model with stochastic consumption and investment opportunities', *Journal of Financial Economics* 7: 265–96.

Cagetti, M., Hansen, L.P., Sargent, T. and Williams, N. (2002) 'Robustness and Pricing with uncertain growth', *Review of Financial Studies* 15(2): 363–404.

Constantinides, G. (2002) 'Presidential address: rational asset prices', *Journal of Finance* 57: 1567–91.

Cross, R. (1982) 'The Duhem–Quine thesis, Lakatos and the appraisal of theories in macroeconomics', *The Economic Journal* 92: 320–40.

De Bondt, W. (2002) 'Discussion of "competing theories of financial anomalies"', *Review of Financial Studies* 15(2): 607–13.

De Bondt, W.F.M. and Thaler, R.H. (1985) 'Does the stock market overreact', *Journal of Finance* 40, 793–807.

Del Guercio, D. (1996) 'The distorting effect of the prudent-man laws on institutional equity investments', *Journal of Financial Economics* 40(1): 31–62.

De Long, J.B., Shleifer, A., Summers, L.H. and Waldmann, R.J. (1990) 'Noise trader risk in financial markets', *Journal of Political Economy* 98(4): 703–38.

Dumas, B., Fleming, J. and Whaley, R.E. (1998) 'Implied volatility functions: empirical tests', *Journal of Finance* 53(6): 2059–106.

Evens, R., Geczy, C., Musto, D., Reed, A. (2002) 'Impediments to short selling and option prices', working paper Wharton School of Business.

Fama, E. (1965) 'The behavior of stock market prices', *Journal of Business* 38: 34–105.

Fama, E. (1970) 'Efficient capital markets: a review of theory and empirical work', *Journal of Finance* 25(2): 383–417.

Fama, E.F. (1998a) 'Determining the number of priced state variables in the ICAPM', *Journal of Financial and Quantitative Analysis* 33(2): 217–31.

Fama, E.F. (1998b) 'Market efficiency, long-term returns, and behavioral finance', *Journal of Financial Economics* 49(3): 283–306.

Fama, E.F. and French, K.R. (1992) 'The cross-section of expected stock returns', *Journal of Finance* 47: 427–65.

Fama, E.F. and French, K.R. (1993) 'Common risk factors in the returns on stocks and bonds', *Journal of Financial Economics* 33: 3–56.

Fama, E.F. and French, K.R. (1996) 'Multifactor explanations of asset pricing anomalies', *Journal of Finance* 51(1): 55–84.

Friedman, M. (1953a) *The Methodology of Positive Economics, in Essays in Positive Economics*, Chicago: The University of Chicago Press.

Friedman, M. (1953b) 'The case for flexible exchange rates', in *Essays* in *Positive Economics*, Chicago: *University of Chicago Press.*

Gilson, S.C., Hotchkiss, E.S. and Ruback, R.S. (2000) 'Valuation of bankrupt firms', *Review of Financial Studies* 13: 43–74.

Grossman, S.J. and Stiglitz, J.E. (1980) 'On the impossibility of informationally efficient markets', *The American Economic Review* 70(3): 393–408.

Hansen, L.P. and Singleton, K.J. (1982) 'Generalized instrumental variables estimation of nonlinear rational expectations models'. *Econometrica* 50: 1269–88.

Hausman, D.M. (1992) *The Inexact and Separate Science of Economics*, Cambridge: Cambridge University Press.

Hausman, D.M. (2001) 'Explanation and diagnosis in economics', *Revue Internationale De Philosophie* 55: 311–26.

Kaplan, S. and Ruback, R. (1995) 'The valuation of cash flow forecasts: an empirical analysis', *Journal of Finance* 50: 1059–93.

Kleidon, A.W. (1986) 'Variance bounds tests and stock price valuation models', *Journal of Political Economy* 94(5): 953–1001.

Kyle, A.S. (1985) 'Continuous auctions and insider trading', *Econometrica* 53: 1335–55.

Lakonishok, J., Shleifer, A. and Vishny, R.W. (1992) 'The structure and performance of the money management industry', *Brookings Papers on Economic Activity: Microeconomics*, pp. 339–91.

Lakonishok, J., Shleifer, A. and Vishny, R.W. (1994) 'Contrarian investment, extrapolation, and risk', *Journal of Finance* 49: 1541–78.

Lamont, O. and Thaler, R.H. (2003) 'Can the market add and subtract? Mispricing in tech stock carve-outs', *Journal of Political Economy* 111(2).

Lee, C.M. (2001) 'Market efficiency and accounting research: a discussion of capital market research in accounting by S.P. Kothari', *Journal of Accounting & Economics* 31: 233–53.

Lewellen, J. and Shanken, J. (2002) 'Learning, asset-pricing tests, and market efficiency', *Journal of Finance* 57(3): 1113–45.

Markowitz, H.M. (1952) 'Portfolio Selection', *Journal of Finance* 7: 77–91.

Marsh, T.A. and Merton, R.C. (1986) 'Dividend variability and variance bounds tests for the rationality of stock market prices', *The American Economic Review* 76(3): 483–98.

MacKenzie, D. and Millo, Y. (2001) 'Negotiating a market, performing theory: the historical sociology of a financial derivatives exchange', working paper, University of Edinburgh.

Mehra, R. and Prescott, E.C. (1985) 'The equity premium: a puzzle', *Journal of Monetary Economics* 15: 145–61.

Mitchell, M.L., Pulvino, T.C. and Stafford, E. (2002) 'Limited arbitrage in equity markets', *Journal of Finance* 57(2): 551–84.

Nagel, E. (1961) 'The cognitive status of theories', in Yuri Balashov and Alex Rosenberg (eds), *Philosophy of Science: Contemporary Readings*, London: Routledge: pp. 197–210.

Roll, R. (1977) 'A critique of the asset pricing theory's tests; part I: on past and potential testability of the theory', *Journal of Financial Economics* 4(2): 129–76.

Rosenberg, A. (1976) *Microeconomic Laws*, Pittsburgh: University of Pittsburgh Press.

Rosenberg, A. (1992) Economics – Mathematical Politics or Science of Diminishing Returns?, Chicago: The University of Chicago Press.

Rubinstein, A. (2001) 'A theorist's view of experiments, Joseph Schumpeter Lecture', *European Economic Review* 45: 615–28.

Rubinstein, M. (2001) 'Rational markets: yes or no? The affirmative case', *Financial Analysts Journal* 57(3): 15–29.

Shiller, R. (1981) 'Do stock prices move too much to be justified by subsequent changes in dividends?' *The American Economic Review* 71(3): 421–36.

Shiller, R. (2000) *Irrational Exuberance*, Princeton, NY: Princeton University Press.

Shleifer, A. and Vishny, R.W. (1997) 'The limits of arbitrage', *Journal of Finance* 52(1): 35–55.

Schroeder, J.L. (2001) 'Just So Stories: Posnerian Economic Methodology', *Cardozo Law Review* 22: 351–423.

Summers, L.H. (1986) 'Does the stock market rationally reflect fundamental values?' *Journal of Finance* 41(3): 591–601.