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THE ABILITY TO «OUTPERFORM THE MARKET»: LOGICAL FOUNDATIONS BASED ON THE THEORY OF RATIONAL BELIEFS

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ABSTRACT

Is it theoretically legitimate for an investor to attempt to outperform the market index? Or is the investor simply deluding himself? The purpose of this paper is to apply the new Theory of Rational Beliefs (RB) to demonstrate the following result: Even when all agents have symmetric information, there exist *three canonical strategies* in an RB environment that make it theoretically legitimate to try to outperform the market. More specifically, while the three strategies that we will identify are not directly implied by RB theory which is a positive theory, the strategies are logically compatible with RB theory when interpreted in a normative manner from the standpoint of an individual agent attempting to exploit the existence of structural change in the environment. This is what we mean by the claim that the proposed strategies are «theoretically legitimate». Moreover, we propose that these three strategies are canonical in the sense that *all* theoretically legitimate strategies for outperforming the market (including all successful hedge fund strategies that we know of) are combinations of these three basic strategies.

Key words: Portfolio Choice, Rational Beliefs, Diversity of Beliefs, Asset Pricing, Market Efficiency, Bounded Rationality.

JEL Classifications: Cl1, D5, G11, G12, G14.

I - INTRODUCTION

A central paradox of financial economics is the persistence of the belief that it is possible to outperform the market in a Bloomberg world where all investors possess the same economic data *and* obtain these data at the same time. To begin with, a subset of investors *do* indeed outperform the market over long periods, and end up accruing far more wealth than other agents who possessed essentially the same data. But how did they do this? Was it simply by luck? Or did these winners have access to private data – a claim infrequently heard in recent decades since it became illegal to trade on inside information? Or were they just «smarter» in some sense that has not been well defined?

At the more fundamental level of serious financial theory, the core question can be restated. Is it theoretically possible for an investment manager in an informationally efficient world to be able to outperform the market «on average»? More specifi-

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cally, does economic theory make clear the precise circumstances in which it is logically possible to do so, once the roles of inside information and good luck are ruled out? If the answers to these fundamental questions are «yes,» we shall claim that the implied strategies are «theoretically legitimate».

Going one step further, if no such strategies exist, is it then ethical for investment managers to charge fees for purporting to be able to outperform the market? And does the converse hold true if theoretically legitimate strategies do exist? In practice, managers justify their fees on the basis of track record (luck?), personal contacts with those who matter (inside information?), or vague assertions to the effect that «after years in the business, we intuitively understand the dynamics of markets better than most». Such answers are both too circular and too self-serving to be satisfactory.

From the standpoint of *classical* economic and financial theory, the answer to both of our fundamental questions is an unambiguous «no»: Given symmetric information, the twin theories of Rational Expectations and Efficient Markets demonstrate that it is impossible to outperform the market on any systematic basis. «Outperforming the market» in this context means that the returns from investing in some *subset* of risky assets exceed the returns from holding the market capitalization-weighted index of *all* risky assets – the index first identified in the Capital Asset Pricing Model (Cochrane, 2001).

By extension, classical theory implies that it is indeed unethical for an investment manager to charge investors a fee for purporting to be able to outperform the market index (to «add alpha», in today's parlance). Nonetheless, while most investors do not outperform the market on a long-term basis, some in fact do and have achieved enviable and well-documented track records. But how did they do this? And what in turn is wrong with classical theories that purport to demonstrate that superior performance is logically impossible, except by luck (or by behavior that is unethical or illegal)?

The purpose of this paper is to answer these and related questions from first principles. Specifically, we apply the new Theory of Rational Beliefs (RB) to demonstrate the following result: Even with symmetric information in an RB environment, there exist three canonical strategies that make it theoretically legitimate to attempt to outperform the market in the precise sense identified above. More specifically, although RB – viewed as a positive theory – does not directly imply the three strategies, each is logically *compatible* with and indeed *inspired* by RB theory when it is interpreted in the normative context of an individual agent trying to achieve above-market returns. This compatibility with RB is what we mean by the claim that the three proposed strategies are *theoretically legitimate*.

Moreover, we propose that these three strategies are canonical in the sense that *all* theoretically legitimate strategies for systematically outperforming the market (including all successful hedge fund strategies that we know of) are combinations of these three basic strategies. Finally, our results seem to bestow ethical legitimacy on the «active» investment management industry in which investment managers adopt the strategies proposed herein in their efforts to outperform the market. Consistent with elementary economic intuition, implementation of these strategies requires investments of time and money, and the acquisition of particular skills. There is no free lunch.

To sum up, we shall argue that RB proffers compelling logical and ethical foundations for the first time for the concept of «active» as opposed to «passive» investment management, (widely known as «indexing»).

Overview and Summary: Section II sets forth the basic theoretical structure we shall draw on. Section III identifies the three strategies for outperforming the market, and offers several concrete examples of each. Of particular interest is Example 4 of the third strategy. It sets forth an RB-based generalization of the portfolio problem that in turn permits a generalization of the «efficient frontier» concept of modern finance, as well as a new concept of an optimal dynamic asset allocation strategy. Section IV discusses the relationship between the strategies we have identified and the concept of «bounded rationality». This discussion was proposed by two reviewers uncomfortable with the way in which our Type-3 strategies sometimes violate the RB requirement of unbounded rationality. The issues that arise here are difficult and important, and may ultimately imply the need for a generalization of RB theory itself. Such a generalization is sketched in Section 4.1.

II - THE FRAMEWORK, AND THE INVESTOR'S TRUE FORECASTING PROBLEM

RB Theory: In his original paper on Rational Beliefs (RB), Kurz (1994) emphasized that the central idea of his new theory was that agents do not possess complete «structural knowledge» about the workings of the economy. By modeling such ignorance in a general equilibrium context, Kurz and his colleagues (e.g., Kurz, 1996) were soon able to model important new concepts such as belief structures, correlated mistakes, and endogenous risk. In classical Rational Expectations theory (RE), the assumption that all agents possess complete information about the economy's structure guarantees that agents cannot make mistakes, and that endogenous risk will therefore not exist.[1]

The RE Price Map: But what exactly *is* structural ignorance in Kurz's theory? Consider the following classic asset pricing equation characteristic of RE financial models:

[1] Kurz's generalization of classical equilibrium theory is very analogous to Harsanyi's generalization of the classical theory of non-cooperative games with complete information (Harsanyi, 1967-1968). In this latter case, players do not possess complete structural information about the underlying game – specifically, about the true «types» of their antagonists. They thus cannot know the true payoff matrix of the game. As a result, they make mistakes; and mistakes increase the volatility of real-world political and economic behavior, just as endogenous risk increases the level of financial market volatility in non-game situations. Moreover, Harsanyi's reliance upon the existence of an underlying «true distribution of types» known by all mirrors Kurz's reliance upon the existence of an underlying «stationary measure» of the law of motion of the economy that is known by all. In both theories, the postulated existence of these measures plays the role of a *symmetry argument* that permits the resulting theories to be internally consistent and logically «closed».

$$M : \{X\} \to \{P\} \quad (1)$$

where X is the *exogenous*-state space as in Arrow's classic model, P is the price of the security, brackets $\{\bullet\}$ denote a probability measure, and M is a pricing model (typically the equilibrium map) that maps news about the current state into the price space. This pricing model will of course operate in a dynamic setting where some underlying stationary stochastic process governs the evolution of states over time, and hence of price, and hence of returns (because returns are typically functions of changes in prices). For notational simplicity, we let the brackets $\{X\}$ around X denote this stochastic process and thus dispense with intertemporal notation. Classical RE models assume that all agents know the true map M for all assets (the property of perfect conditional foresight) *and* know the true stochastic process $\{X\}$ governing the evolution of states. It then follows that all agents will know the true process governing prices and hence returns.

The RB Price Map: RB models assume neither. Indeed, the theory assumes that the economic environment is weakly non-stationary or «statistically stable» in the precise sense of Kurz - Motolese (2006). Therefore, agents *cannot* know either the true stochastic process or the map M that maps news about the realization of states over time into the evolution of prices and thus returns. Specifically, the utilization of econometric inference based on the law of large numbers will not permit agents to learn these parameters. This is what is meant by «structural ignorance».

Instead, agents utilize the assumption that the stochastic process is only weakly non-stationary to learn the *stationary measure m* of the underlying statistically stable process. Indeed, not only do they learn this, but they also use it as a constraint when attempting to construct a «rational» forecast (belief) about the future. Here is exactly what this concept means: According to the Lebesgue decomposition theorem, each agent's forecast (belief) about the future can be decomposed into a weighted sum of two entities: (1) the probability measure B^m implied by stationary measure that summarizes the long-run average behavior of the economy as revealed by the data, plus (2) the probability measure B_i^s which captures how the agent personally believes the future of the economy will actually unfold. The *difference* between the two beliefs represents the agent's views as to how non-stationarities will skew system dynamics away from those implied by the stationary process m.

Additionally, when constructing the subjective belief B_i^s, the agent is assumed to be constrained by the Rationality Constraint whereby the degree to which he might be more optimistic than the stationary forecast over certain time intervals will exactly offset the degree to which he is more pessimistic over other intervals. This RB property ensures that the agent's forecast cannot be contradicted by the empirical data embodied by the stationary measure and known to all agents.

Finally, it is assumed that the agent will assign weights k and $1-k$ with $0 < k \leq 1$ to the two components of his rational belief. In the polar case where $k = 1$, the agent completely adopts the stationary forecast B^m as his own best forecast. Conversely, when k is very close to 0, the agent will assign virtually no weight to the stationary forecast, and bet almost entirely on his own subjective forecast B_i^s.[2]

To summarize more formally, agent i's rational belief B_i can always be represented as

$$B_i = k \cdot B^m + (1 - k) \cdot B_i^s \tag{2}$$

where $B_i^s \perp B^m,\ 0 < k \leq 1$, and B_i^s satisfies the Rationality Constraint.[3]

When agents possess forecasts of this kind, but have different forecasts, i.e., heterogeneous beliefs, then at least $n - 1$ agents make mistakes (because at most only one agent can possess a true forecast). Such mistakes, especially mistakes that are correlated, create endogenous risk (for example, «market overshoot»). When endogenous risk exists, the induced equilibrium will typically exhibit much greater volatility than in classical models which assume that endogenous risk does not exist because homogeneous rational expectations ensure that mistakes do not occur.

Because the distribution of beliefs plays such an important role in RB theory, and because everyone is assumed to know that this distribution matters (just as in Keynes' «beauty contest»), RB scholars have in the past few years reformulated the basic asset-pricing equation to make explicit its dependence on the market's state of belief. Equation (**1**) above thus becomes:

$$M^* : \{\{X_i^e\}\} \rightarrow \{P\} \tag{3}$$

where X^e is the *extended state vector* that consists of Arrow's exogenous states and a new observable variable, namely the «state of market belief about the future states», the subscript **i** denotes that this is agent **i**'s subjective belief – via equation (2) – about the evolution of the extended state space, and the double brackets indicate that what matters to future prices will be the *distribution* of all individual probabilistic beliefs about the extended state space, such as agent i's forecast $\{X_i^e\}$.

Finally, note the *absence* of brackets around the equilibrium map M^*. This conforms to more recent formulations of RB, which assume that agents *do* know this map. Specifically, agents would know the correct probability distribution of the sequence of future prices and thus returns if they could learn in advance the true values of the future-extended state vector. This is a strong assumption, but a very useful one because it has facilitated construction of RB models that are econometrically testable (e.g., Kurz - Motolese, 2006).

Nonetheless, for purposes of the normative theory of superior forecasting that we are about to introduce, we shall retain the assumption that agents do *not* know the map M^*. Rather, each agent will have his own subjective probability distribution of the map, denoted by $\{M_i\}$ for agent i. The *distribution of these distributions* across

[2] For technical reasons, k cannot actually equal 0, as the stationary forecast must be given a weight of at least ε.

[3] The orthogonality condition reflects the fact that the two right-hand measures are mutually singular, and follows from the Lebesgue decomposition theorem of measure theory.

agents $\{\{M_i\}\}$ will affect price just as will the distribution of the forecasts of the extended state space across agents. Importantly, we will show below that, via two types of strategies, agents seeking to make superior forecasts will be able to exploit other agents' inability to know the true stochastic process governing X^e and the true model M^*. For these reasons, the pricing model underlying our own *normative* theory of forecasting must take the fully general form of the functional

$$\{\{M_i\}\} : \{\{X_i^e\}\} \rightarrow \{P\} \qquad all\ i = 1, ..., n \tag{4}$$

This makes explicit the *two* types of uncertainty that agents can exploit in seeking to construct superior forecasts: state space uncertainty and pricing model uncertainty. Of course, no individual agent is aware of the content of (4). Rather, he will express his own views about these two kinds of uncertainty in a muddled form via his subjective belief about the future values of all relevant exogenous *and* endogenous variables such as X^e and P. The only restriction on an agent's views is that they be «rational» in the sense that they satisfy equation (2).

III - THE THREE CANONICAL STRATEGIES FOR EARNING SUPERIOR RETURNS

We now outline a theory of how an agent can achieve superior forecasts of returns in a manner consistent with the positive theory summarized by (4) above. This is an avowedly normative theory, based on the following assumptions:

1. Earning excess returns in a legitimate manner can be achieved only by a superior *forecast* of future returns. The assumption is a simple one, but the crucial role of forecasting per se is rarely stressed. Indeed, the author knows of no course within the Institute of Certified Financial Analysts or in any business school that is dedicated to teaching how to arrive at superior forecasts of returns. Given the dominance of the RE paradigm, this is perhaps not surprising.

2. Superior returns forecasting reduces to superior asset price forecasting because returns are functions of prices. While this observation may seem straightforward, investment analysts will often assert: «Fortunately, we are in the arbitrage business, and so we do not have to forecast future prices». This claim is clearly false. Arbitrage bets *in practice* are bets on the uncertain rate of convergence during a specified period of two or more future *prices*. Recall the poor implicit price forecasts that underlay the bets by Long Term Capital Management hedge fund. These forecasts (compounded by high leverage) caused the firm to go bankrupt in 1998.

In the special case where returns are defined to be the returns from *portfolios*, not merely individual securities, then superior returns forecasting will involve not only superior price forecasting, but also the utilization of a superior *logic* of portfolio optimization. Such a logic will be outlined below. Utilization of this logic can demonstrably shift the entire efficient frontier outwards.

3. RB theory can guide us as to *how* to arrive at superior forecasts of prices and returns. Specifically, provided that agents make the effort to understand the meaning and implications of the functional (4), they can learn how to construct superior forecasts. Forecasts purporting to be superior and are based on fundamental economic theory such as (4) will be called *legitimate forecasts*. In contrast to the instructive role of RB theory in this regard, classical RE theory makes clear from the start why *no* legitimate forecasts can exist in a stationary environment where prices evolve in accord with equation (1). In sum, exploitation of the added generality of RB (compare equations (4) and (1)) makes it *theoretically* possible and thus legitimate to try to outperform the market index.

4. Most important, whereas we postulate symmetric information in the sense that all agents learn all knowable facts about the economy simultaneously, we also postulate that agents with symmetric information will inevitably *interpret* the information differently thereby arriving at differing forecasts. That they do so has been a central tenet of RB theory from the start. This being true, however, some interpretations will be *superior* to others. Given our normative orientation, we wish to identify the intrinsic nature of these superior interpretations. More ambitiously, we wish to develop replicable general rules that make it possible *systematically* to turn superior inferences into superior forecasts and thus superior returns. We could then state that *systematic active management* (at least the sort based on superior inferences) is a legitimate enterprise. When this is the case, it is ethical for investment managers to charge fees for their services.

To the best of our knowledge, none of this has been attempted yet within RB theory. For example, in modeling structural change, RB models permit agents to receive «private signals» that in some black-box manner help them form their subjective views about the impact of a given structural change. We are interested in deepening our understanding of *how* this happens in a normative context where agents are interested in arriving at superior forecasts based on whatever quantitative or qualitative signals they receive. Our central focus will thus be on the concept of *superior inferences*. Importantly, such inferences will always be quantitative in nature because they take the form of conditional probability assessments. Whether the underlying conditioning variables (such as «signals») are qualitative or quantitative therefore does not matter.

We now introduce the three basic strategies to achieve superior forecasts of returns:

- *Strategy 1*: Produce superior forecasts of the evolution of the exogenous states via a superior understanding of structural changes that stem from the environment's non-stationarity. Stated more simply, this strategy aims to make an investor less surprised by «the news» than most other investors are by equipping him with a better understanding of the environment.
- *Strategy 2*: Exploit the existence and nature of endogenous risk – regardless of whether the context is the short run when «price overshoot» can occur, or the long run when «long cycles» play out. In contrast to the first strategy where the goal is to do a better job forecasting the news about the extended state vector, this strategy aims to help an investor better forecast the reaction of asset prices to such news. In doing so, the strategy will make possible superior risk assessment of asset prices and returns.

- *Strategy 3*: Exploit widespread errors of inference. Many of the laws of motion governing the economy and asset returns are intrinsically *counter-intuitive*, which leads many agents to make erroneous inferences and thus problematic forecasts. Agents who avoid such errors will possess superior forecasts. Interestingly, this third strategy applies to both stationary and non-stationary environments, but more so in the latter cases.

  The issues that arise here are subtle, and an example will prove helpful. Consider the true implications for changes in interest rates and currencies when there is a shift in global investor preferences for US assets. For example, if foreigners become disenchanted with prospective returns on US assets and attempt to «pull out their money,» would this simultaneously raise US interest rates and lower the value of the dollar, as is universally assumed? No. By virtue of Tobin's «No Net Selling» accounting identity, foreigners as a whole cannot withdraw their money at all. Accordingly, the impact will fall almost exclusively on the value of the dollar-not on interest rates. The chain of logic involved is highly counter-intuitive, as is explicitly made clear by the title of Tobin's own classical paper that discusses the true role of accounting identities, «Pitfalls in Financial Model Building» (Tobin – Brainard, 1968). Thus, an investor who *does* understand such logic will arrive at superior inferences about the behavior of the dollar and interest rates, as will be noted in a more detailed discussion of this example below.

  For the moment, the key point is that our argument about superior inferences in this case does not depend on any assumptions of non-stationarity or of structural change. The same situation arises within physics whose laws of motion are stationary. For more than two millennia, most intelligent physicists failed to understand that the sun goes around the earth, and not vice versa. In both economics and physics, superior inferences can be arrived at without exploiting non-stationarity. And while a true law of motion does indeed exist in both examples cited—so that «everyone» can eventually learn the truth—such learning may be drawn out over a *very* long period. Thus, those who invest time and money to learn the truth sooner than others will make better forecasts.

  Importantly, classical RE theory *implicitly* assumes that such errors do not exist at all. This implicit assumption is a corollary to the requirement that all agents know the true dynamics of the system and that markets are complete, and thus do not make mistakes of any kind.

We now discuss each of the three strategies in detail. In doing so, the author will often illustrate the strategies involved by drawing on some of his own research over the past two decades. Interestingly, much of this research was aimed at implementing precisely the three types of strategies identified above. In cases where the underlying research was not published publicly, we cite in the bibliography the appropriate research papers published privately by Strategic Economic Decisions, Inc.

STRATEGY 1: *Exploit Structural Changes in the Environment*

The first strategy for arriving at superior forecasts is to invest in identifying and understanding structural changes *before* the market does, or *better* than the market does. In terms of RB theory, we exploit the fact that the stochastic process generating the evolution of the states $\{X^e\}$ in equation (4) is non-stationary. In Strategy 1, this is the *only* term in the RB model that we draw on. That is, we are not concerned with uncertainty about the equilibrium map *M*, or with future prices or returns, or with the magnitude of endogenous risk. We are interested only in the underlying dynamics of the state variables, «news» that affects future prices.

What does systematically superior forecasting mean in this case? The null strategy, of course, is to adopt the stationary measure as a forecast, that is, simply to assume that we can obtain the conditional probability of future news given today's state by econometrically analyzing past data. In effect, we pretend that the world is stationary. An alternative null strategy is to adopt the consensus forecast which may well differ from the stationary forecast.

The ostensibly superior strategy would be to acknowledge what history has shown: irregular structural changes occur over time, indicating that the environment is non-stationary. By apprehending such changes and assessing their impact on future prices, an agent can possess a forecast that should be superior to a stationary forecast that ignores such developments. But how is the agent to apprehend such changes? One way is to invoke a host of ancillary theories and models drawn from different disciplines. These ancillary models can offer compelling evidence of the existence and nature of structural changes – evidence that in turn lead to superior forecasts. We are about to offer two case studies along these lines.

Nonetheless, there can be no law-of-large-numbers verification of the «correctness» of such models of structural change as is possible in a stationary environment. For this reason, the adoption of such models amounts to a subjective forecast *ex ante*. But it need not be subjective *ex post,* as we are about to see. This is a critical distinction because it underlies the concept of a superior forecast being a forecast that was right for the right reason, *ex post.*

We can witness this process of formulating hypotheses about the explicit nature of structural change by applying ancillary models in contemporary discussions about the topic of global warming. Extensive research has caused the consensus view of climatologists about this threat to evolve over the past decade. Those clinging to the notion that «nothing has changed» are becoming fewer and fewer. The process of convergence towards today's posterior consensus is aptly described in the January 2007 Intergovernmental Panel on Climate Change of the United Nations.

Let us now present two case studies from contemporary economics where theories of structural change were developed that generated superior inferences and forecasts. Thereafter, we comment on the possibility of developing a *general* logic along these same lines.

Example 1: *Resolving the Solow Productivity Paradox*: In 1987, economist Robert Solow famously quipped: «Evidence of the importance of the computer is everywhere except in the US productivity data». Solow was understandably perplexed by the fact that, despite the great Information Technology revolution of 1975-1994, US productivity growth had declined markedly since its high-water mark in the 1960s, and had languished at about 1.5% for two decades. By the mid-1990s, investment analysts focused on two issues: (1) Why had the IT revolution not increased productivity, especially because the Personal Computer and the Internet had been around for at least 15 years? (2) Would anything happen to change this story in the near future?

At the invitation of the Domestic Economic Council at the White House in 1996, the author undertook a study (Brock, 1997) to try to explain why productivity growth had stagnated despite the technology boom. In the study, he offered an explanation for this paradox – an explanation that led to a prediction that productivity was about to accelerate dramatically in an S-curve manner. Moreover, he proposed that this acceleration would not depend on high levels of capital spending. At the time, this pair of predictions was strongly anti-consensus, yet both proved correct. From the vantage point of the present essay, what is more important is that they proved correct basically for the right reasons in the sense discussed above: The specific hypotheses implying the forecast were largely vindicated (Jorgenson, 2006; Atkinson - McKay, 2007).

What exactly was the nature of the theory of structural change underlying the predictions? There were four building blocks to Brock's theory:

1. The theoretical concept of «learning by doing» (Arrow, 1962). Arrow was perhaps the first economist to stress how the accumulation of on-the-job skills is an important variable in explaining productivity growth independently of technological innovation and capital spending.
2. The empirical work of Brynjolfsson and Hitt (1997) on the impact that capital spending on IT had on corporate productivity and profitability. Brynjolfsson and his colleagues identified the critical role of «optimal organizational architectures» in explaining the growth of productivity and profitability within firms. The principal finding was that innovation and capital spending alone does not permit firms to reap excess returns unless they also adopt new incentive structures and organizational architectures that are optimally compatible with new technology. At a macro level, this theory implied that *nations* with flexible workplace rules and appropriate incentive structures would reap the gains from new technologies much more rapidly than nations with inflexible structures.
3. The theoretical work of Paul Milgrom and John Roberts (1990) on «supermodular production functions». Their work formalized the concept of *jointly* increasing returns across technologies – a theory ideally suited to predicting and indeed explaining the synergistic cross-effects of general purpose technologies that the author of the study had anticipated.
4. The advent of the Netscape's Internet browser technology in 1995. This last development was instantly identified by Bill Gates as *the* definitive advance in the IT revolution since the invention of the chip. Gates understood that the browser would finally «domesticate» the IT revolution very rapidly, permitting tens of millions of US workers to utilize the Internet effectively for the first time.

The Brock study predicted that, with this new browser technology in hand, Arrow's learning-by-doing arguments would come into play fully, as would those of Brynjolfsson and of Milgrom - Roberts, and the Third Industrial Revolution would take off. The prediction of S-curve productivity growth flowed immediately from Milgrom - Roberts. This was the basis of the structural change theory that the author presented in his 1997 paper. The events of the next eight years would seem to vindicate the analysis. To the astonishment of most analysts, productivity growth exhibited a strong S-curve; it rose from under 1% in 1995, peaked at 4.1% in 2002, and fell back to 2% at this writing. Profit growth for its part (allowing for the recession in 2001) was the best in half a century, notwithstanding a collapse in capital spending during 2002-2003.

The fundamental point is that, by investing in appropriate research involving both induction (e.g., Brynjolfsson - Hitt, 1997) and deduction (Milgrom - Roberts, 1990), it was possible to synthesize diverse ancillary theories into an *ex ante* theory of a particular structural change that led to superior inferences and forecasts. Moreover, the theory was testable *ex post* and successful to the degree that the principal explanatory hypotheses underlying the theory have been largely substantiated.

Example 2: *Increased Stability of the Real Economy*: The US recession of 1991 was caused by the anxiety generated by the Gulf War, by a severe real estate slump, and by the extremely serious credit crunch associated with the US savings and loan crisis. The downturn of 2001 was due to the collapse of capital spending and of the stock market. More recently, US economic growth slowed significantly due to the 2005-2007 recession in the housing sector.

In all three cases, Brock (1996) applied a theory of structural change dating back to 1987 to predict that the impact of these adverse developments on the economy would be significantly less than was expected by the consensus. In all three cases, this proved to be the case. During both the 1991 and 2001 recessions, it was not even clear that recessions occurred at all, given the National Bureau of Economic Research's definition of a recession as two or more successive quarters of negative growth. The «remarkable stability» of the economy became a theme of Federal Reserve Board Chairman Alan Greenspan in his economic commentaries, of Australia Reserve Bank Governor Ian MacFarlane, and of pundits at the Davos World Economic Forum and elsewhere.

Brock's theory of structural change advanced seven reasons why the US economy would prove very stable. In doing so, it helped explain one of the most remarkable economic developments of the 20th century: an arresting reduction of about 75% in the decade-to-decade standard deviation (volatility) of GDP growth, of consumption growth, and of household income growth between 1900 and 1990. In short, the real economy became very much less risky than anyone had ever imagined that it would. (This was not true of the financial markets, however).

Why did this happen? The answer lies in the following set of seven structural changes:

1. A 70% reduction in the value of the average correlation coefficient of output levels of the principal sector pairs in the US economy – a reduction in the industrial «domino effect»;
2. The advent in the 1930s of unemployment insurance, Social Security, and other «income stabilizers»;
3. The discovery and utilization of the role of fiscal and monetary policy in stabilizing the economy;
4. The rise of two-income families, permitting a significant diversification of family incomes;
5. The shift of employment from the manufacturing sector, which is prone to inventory cycles, to a more stable service sector largely devoid of inventory cycles;
6. The revolution in finance that has permitted securitization of loans and many other forms of risk spreading and risk reduction; and
7. The advent of credit cards and other credit facilities, which permit households to maintain consumption levels during periods of unemployment.

The resulting stability of household income in turn explained another extraordinary structural change of the century: the soaring growth of the household debt-to-income ratio, a phenomenon most often attributed to household greed or consumer irrationality. Once the increased stability of income is taken into account, the ambiguous concepts of greed and irrationality need not enter the analysis. The Expected Utility Theorem implies that, if the riskiness of household income falls by 75%, it would be *rational* for borrowers to increase their debt-to-income ratios significantly over time, as well as for lenders to extend much more credit. This is apparently what happened as *both* the supply and the demand functions in the credit market shifted far out to the right during the past 50 years.

Hopefully, the two examples presented above help substantiate the rationale for our first strategy for outperforming the market. Specifically, they illustrate how an investment of time and money directed towards a superior understanding of structural changes can lead to superior forecasts of «the news» – news about productivity growth in the first instance, and about GDP stability in the second. There is doubtless nothing new about the ability to exploit structural changes in this manner so as to improve price forecasting. What is new is that such activities are compatible with a general equilibrium theory in which agents possess symmetric information in a non-stationary environment, namely RB theory.

Conditioning, Causality, and Theories of Structural Change

In both case studies analyzed, each forecast implied by its respective theory of structural change proved superior to the consensus forecast for the *right reasons.* Specifically, the hypotheses underlying the postulated theories of structural change proved correct *ex post.* In such cases, a theory of structural change can be deemed «legitimate» because it passed an *objective* test of its validity.

To express this point more abstractly, we propose to view a *theory* of structural change as a set of hypotheses implying that the conditional probability $\{x|y\}$ under the stationary measure is being transformed into a different conditional probability $\{x|y\}^*$ for reasons that ideally (but not necessarily) can be tested *ex post*. This transformation can occur in two ways. First, in certain contexts, new variables will join *y* as conditioning variables that alter the conditional probability of *x*. For example, if *x* is productivity growth, and *y* is the rate of capital deepening, then the new variable «optimal organizational architecture» has now joined «capital deepening» *y* as a new variable useful in forecasting *x*.

Second, in other contexts, the *way* or the *magnitude* that *y* impacts *x* may have changed for reasons unrelated to the emergence of any new conditioning variables. For example, a comparably large increase in oil prices affected US GDP growth much less in 2004 than in 1973 or 1979. This was true mainly because the utilization of oil per unit of GDP growth fell by about 50% during this period. Finally, ongoing changes in the US labor market reduced the impact of a given unemployment reduction during the late 1990s to such an extent that the Phillips curve inverted.

Note that while the «conditioning» involved in all these examples is *inferential* in nature, it is not necessarily *causal*, to utilize a distinction made in decision theory. That is, the postulated theories of structural change amount to statements of the form:

> Given that I learn the true value of *y* at time *t*, I will now assign a probability to *x* different than I would have assigned at time $t-j$ in the past.

The reason why this is so may be causal («I believe *y* causes *x,* and I now believe that a given change in *y* will cause a lesser impact on *x* than before»), but it may also be merely associative («Knowing the value of *y* informs me about the value of *x* for reasons that I do not believe to be causal»). As an example of the latter case, an agent's discovery that a randomly selected person is Norwegian will change his conditional probability that the person is blond.

Nonetheless, we conjecture that the relationships involved in structural change analysis will be causal in nature. Adopting a theory of structural change inherently seems to imply a belief that new developments are *causing y*'s impact on *x* to be different from what it used to be. Causal relationships by nature are more testable than merely associative ones. Accordingly, whenever a theory of structural change cites a causal relationship, it should ideally be possible to test whether the theory was right for the right reason *ex post.* This distinction matters because, when an agent is right for the right reason, it can then be claimed that a *superior inference* led to a *superior forecast* – a forecast that will in principle be responsible for garnering excess returns. When this is the case, we can credit skill, not luck, for the superior forecast.[4]

Strategy 2: *Exploiting Endogenous Risk*

Robert Shiller (1981) demonstrated empirically that traditional models of equity market valuation could explain only about 25% of observed equity market volatility over many decades. In his Presidential Address at the American Finance Association

[4] Very subtle issues arise when interrelating the concepts of structural change, causality, and testability. Section 5 of Dawid (2004) includes a very good analysis of these issues.

two years later, he asked if anyone in the audience could explain the source of the other 75% of risk – the «unexplainable» risk he had unearthed. No one could. Perhaps the greatest contribution of RB theory has been to provide a compelling answer to this challenge – an answer that flows from foundational economic theory rather than from trendy yet problematic concepts such as «noise trading» and «investor irrationality».

Specifically, RB reveals that the behavior of rational agents who inevitably make forecast mistakes about the likelihood of future states $\{X^e\}$ generates the extra volatility. These mistakes stem from the environment's non-stationarity and thus its unknowability. When such mistakes are *correlated*, agents tend to buy or sell in tandem, thus generating price swings that would not occur in an RE environment, which are mistake-free. This extra volatility is called «endogenous risk» in RB theory, and total (observed) volatility decomposes into the sum of exogenous volatility and endogenous volatility.

Our second canonical strategy for outperforming the index is to exploit the existence and nature of endogenous risk. But what exactly does this mean? And how is it possible? The answers are quite subtle. To begin, the existence of extra volatility *per se* cannot be exploited in any normative sense. This is because the magnitude of *true* volatility is an observable and so will be known to all. Therefore, the stationary measure captures it. Thus, knowledge that *part* of observed risk is «endogenous» in nature bestows no competitive advantage in the quest for superior performance.

Rather, what bestows an advantage is familiarity with the *theory* of RB, which permits an understanding of *when and why* different varieties of endogenous risk arise in the first place. It is one thing to know along with everyone else that the real-world standard deviation of equity returns is approximately four times greater than predicted by classical RE models, but not know why. It is an altogether different matter, and a much greater advantage, to have a better understanding than others do regarding when and why market overshoot will occur.

In fleshing out this second strategy for outperforming the market, we shall distinguish between *five* sources of endogenous risk. These imply a wide-ranging host of strategies to outperform the market – strategies that are nonetheless unified because they all stem from the ability to understand and exploit endogenous risk.

Five Sources of Endogenous Risk

1.A. Agent uncertainty and thus agent mistakes about the evolution of the extended states that is about the stochastic process $\{X^e\}$;

1.B. The variance and covariance of $\{\{X_i^e\}\}$ appearing in equation (4) (and hence the distribution and correlation of X^e- mistakes *ex post*);

2.A. Agent uncertainty and thus agent mistakes about the true model M that maps realizations of the extended state space into price (recall equation (4));

2.B. The variance and covariance of $\{\{M_i\}\}$ appearing in equation (4) (and hence the distribution and correlation of $M-$ mistakes *ex post*);

3. The incompleteness of hedging markets;
4. The extent and distribution of investor leverage; and
5. The «persistence» of agents' beliefs and long-cycle endogenous risk.

What follows is merely a sketch of how each of the above factors causes endogenous risk, as well as the strategies for superior risk assessment and hence performance that such causal links suggest.

1. *State Uncertainty* («*Classical*» *Endogenous Risk*): RB scholars have analyzed in some detail sources #1.A and #1.B (e.g., Kurz, 1997). The basic insight is that the greater the correlation of beliefs about the future state (including the state of market beliefs), then the greater the likely price movement when investors learn that their forecast was wrong. Normatively, this implies that an agent should not attempt to assess future price risk without taking into account the *ex ante* distribution of market expectations about X^e.

More specifically, when he calculates the returns from a prospective investment predicated on his *own* expectations $\{X_i^e\}$, the investor must take into account and compare his own beliefs with the stationary measure *and* with the current market consensus. This is a central empirical and theoretical result of Kurz and Motolese (2006). An investor who consistently takes all three of these variables into account in the correct manner will possess a clear competitive advantage in forecasting returns relative to investors who do not do so. Most investors will *not* do so because the discovery that they should is both new and quite counter-intuitive. This is just one of many examples as to how an understanding of classical endogenous risk can give an investor a competitive advantage.

2. *Pricing Model Uncertainty*: In attempting to explain the problematic behavior of currency markets, Brock (1995) developed several hypotheses about the impact of endogenous risk sources #2.A and #2.B. He called this source of endogenous risk «Pricing Model Uncertainty» (PMU). It focuses on the extra volatility caused by agents' inability to know the true pricing map M^* – and from agent **i** knowing that agent j also does not know the map, and vice versa. We will now discuss this form of endogenous risk on its own terms, independent of classical endogenous risk – but both coexist in the real world. That is why we have written the pricing map (4) in a fully general form that allows P to depend on uncertainty about both X^e and M^*. This contrasts with more standard RB models predicated on (3), where all agents are assumed to know the correct price P once they learn the realization of the extended state X^e. But as Kurz originally asked (Kurz, 1994): How could the agents ever know the true state-to-price map M^* in a non-stationary environment?

We now introduce two case studies that help explain how an understanding of the role of PMU in generating endogenous risk can help investors earn superior returns. The first study focuses on the problematic behavior of foreign exchange rates. The second focuses on the relative riskiness of different asset classes.

Currency Market Misbehavior: Currency values are notoriously difficult to explain, much less to forecast (Frankel and Froot, 1987, 1990). Brock (1995) attempted to explain some aspects of currency market misbehavior by identifying PMU's role in generating endogenous risk. His principal finding was that PMU can give rise to two very different kinds of trading regimes, both of which are observed in currency markets. In «overshoot regimes», currencies values trend up or down to a surprising degree. In «undershoot regimes,» by contrast, currencies drift randomly to a surprising degree – ostensibly oblivious to news about fundamentals. How does each regime arise?

Suppose a representative agent i does not know M, and adopts a distribution $\{M_i\}$ representing his beliefs about how prices will respond to a given realization of the extended state vector. Suppose also that this is true of other agents. Then when the news about the state vector is announced, agents will be uncertain as to how prices will respond. Assuming realistically that agents are «benchmarked» so that what matters is their performance *relative* to the performance of average agents, how *should* they respond?

To answer this question, we will consider two polar cases.

- First, suppose that, for whatever reason, the market reacts to the news by selling off. Once the sell-off begins and serial correlation is observed, a strategy of trend-following will be optimal for most agents. Such behavior will reap excess returns as long as the trend lasts; in addition, agents who chose to ignore the trend will know that they will underperform their benchmark. They thus have an incentive to join the bandwagon.

Once the trend has begun, however, an agent will wish to predict *how far* the trend will extend in order to help him know when to exit the market. Is there a logic that can help the agent assess trend duration better than others who do not utilize such logic? We conjecture that there is, and PMU is a critical component.

The *more* that most agents lack any idea what the «right» price is given the realization of the state vector – where such agnosticism is represented by a large value of $VAR\ [\{\{M_i\}\}]-$ then the *less* will they be able to sense when «the trend has gone too far». Thus, they face less downside risk in continuing to ride the trend, *and* they will continue reaping excess returns from doing so by exploiting ongoing serial correlation. As a result, the trend will last longer than it otherwise would. In sum, the greater the magnitude of PMU, the larger the overshoot. It is as if investors are sailors lost at sea with no Magnetic North on their compass to tell them where they are going. Each sailor simply keeps up with the others, none knowing whether there are rocks ahead and thus whether they should reverse course.

At the other extreme, if the level of PMU is very small then most everyone will know when prices have fallen too far, for «too far» will now possess a clear meaning. Agents will rapidly reap their profits and exit the market, thus bringing the minitrend to an early end. In the case of currencies, we conjecture that the level of PMU is great, and that this is one reason why currencies have often trended far more than most anyone expects. The comments of traders interviewed both by Frankel and Froot (1987, 1990) and Brock (1995) make clear that most currency traders abandoned fundamentals-based models during the 1980s. They learned from real-world experience that knowing the news did *not* help them very much in forecasting the behavior of currency values. Exchanges markets were thus frequently described as being chaotic and prone to bubbles.

- Second, consider the opposite polar case where, for whatever reasons, the market does not begin to sell off when delivered adverse news. That is, there is no incipient reaction to the news. Then no serial correlation will exist for traders to exploit. In this circumstance, the greater the magnitude of PMU, the more it would be rational for agents to ignore the news altogether. Bemoaning the «craziness» of currency markets, they should head off to the beach. The result will be a «drift regime,» the opposite of a trend regime.

As Brock (op. cit) demonstrates, currencies have indeed exhibited a mixture over time of trend regimes and drift regimes. In the former case, the induced endogenous risk is *positive*, the observed volatility of currencies is greater than it would be in an RE world. Yet in the latter case, it is *negative*, the market under-reacts to news – the opposite of what Shiller (op. cit.) found in his celebrated study of US equity markets. Investors who understand this dualistic behavior of foreign exchange markets should possess an advantage over those who do not.

Relative Riskiness of Different Asset Markets: We turn now to a second example of the role of PMU in generating endogenous risk. Consider the following ordering of all asset classes: T-bills, government bonds, corporate bonds, equities, growth stocks, technology stocks, emerging markets, and finally currencies. When investors are confronted with this ordering, and are asked to explain what it represents, they typically respond: «You are progressing from the least to the most risky asset class: T-bills possess stable and predictable prices most of the time, whereas emerging markets and currencies exhibit highly risky and unpredictable prices».

But care is needed here. For what kind of risk is being referred to? The amount of *exogenous* news driving each asset class is *not* monotonically increasing across the given ordering. If it were, then even in an RE environment, you would expect the markets to be increasingly volatile as we progress along the continuum – as they indeed are. But this is not the case, as there is no evidence that the amount of news affecting T-bills is any greater than that affecting technology stocks or currencies. What is really going on is that the ordering of asset classes reflects an increasing amount of PMU, and we are probably witnessing an increasing amount of PMU-induced *endogenous* risk as we move out along the continuum.

For example, most investors believe they know the «right» price of T-bills given the levels of the Fed funds rate and bond yields. Moreover, they act on this belief and thus *cause* the right price to prevail. For this reason, there is rarely any discussion about T-bill overshoot. Emerging markets and currencies lie at the other end of the spectrum where overshoot is often observed and much discussed. As a case in point, the Morgan Stanley Emerging Market Index fell by 23% between May 10 and June 25 of 2006. Most observers agreed that there was no news that justified this overshoot other than some gossip about prospects for a tiny increase in Japanese interest rates.

Commentators also noted that this was a classical example of market overshoot resulting from (1) the inability of investors to know the «true value» of such markets *given* the news, and (2) from the incentive of benchmarked investors to herd together. It is also noteworthy that the Morgan Stanley Index fully recovered from this sell-off within six months. This recovery would not have occurred had the reason for the spring sell-off been exogenous news of a fundamental nature.

To conclude, PMU represents a separate and additional source of endogenous risk. An understanding of PMU-induced endogenous risk proffers new strategies for investors attempting to achieve superior returns. It enhances their ability to know the specific conditions under which various kinds of overshoots and undershoots will occur, and whether the market might recover from such bouts. Our conclusions here are somewhat novel and tentative, and significant theoretical and empirical research will be needed to substantiate the hypotheses we have advanced. The most important of these is that RB theory *without* PMU will probably be unable to account for the total level of empirically observed volatility in most markets. Specifically, we hypothesize that version (3) of RB theory with its extended state vector will not sufficiently account for total volatility, and that the generalized version (4) will be needed.

3. *Amplification of Endogenous Risk Due to Market Incompleteness and Leverage*: The role of endogenous risk sources #3 and #4 (hedging market incompleteness and the extent of leverage) is rather straightforward. In an RE world, it is usually assumed that markets are complete and that agents know enough to avoid mistakes. Thus, bankruptcies do not occur, market meltdowns do not occur, investors can infinitely short their positions (Cochrane, 2001), and the debt/equity irrelevance theorem of Modigliani - Miller (1958) holds true. When markets are incomplete, however, as they must be in an RB world (where contracts cannot be written against market belief states, and agents are jointly wrong in many of their forecasts), then panics and bankruptcies emerge naturally and are to be expected. The only hypothesis we would make here is that, the greater the market incompleteness or the greater the leverage, then the greater will be the *amplification* of the endogenous risk generated by sources #1 and #2 above. This statement remains a conjecture awaiting a full proof within RB theory.

From a normative standpoint, an investor who understands this relationship will have an advantage in risk assessment and therefore in price forecasting that others will not possess.

4. «Persistence» and Long-Cycle Endogenous Risk: One of the great enigmas of financial economics is the existence of «long cycles» in the *valuation* of equities, and arguably of commercial real estate. While the existence of such cycles is widely acknowledged, most investors do not act on them for three reasons:

First, cycles of this kind are long-term in nature, and relatively few investors possess sufficiently long-term time horizons to exploit them.

Second, classical economic theory cannot account for such cycles. In particular, the cycles cannot be explained by the variables of traditional valuation models (for example, dividend growth, earnings growth, and discount rates). Thus, by what textbook logic could long-term investors have forecast the great US bull stock market of 1981-2000? Indeed, equity analysts all but gave up attempting to explain how the price/earnings ratio of the Dow Jones average could have risen from 8 to 32.

Third, the difficulty of calling the turning point of long-term valuation cycles has caused «market timing» strategies to be viewed as very risky.

One of RB theory's greatest contributions is to make possible for the first time a coherent account of the nature and rationale of long cycles (Kurz - Beltratti, 1997; Kurz - Motolese, 2001). It does this first by modeling the market's «belief structure» as an independent variable in its own right, and second by identifying a particular class of belief structures: persistent bouts of market optimism and pessimism. Persistence here refers to the fact that once agents come to hold a correlated belief that is optimistic about returns (for example, most agents expect above-average returns), they stick to this belief for a good while – and they behave similarly when they turn pessimistic.

In an extension of this research, Brock (2004) has argued that two conditions are necessary and sufficient for the endogenous generation of long cycles. To see this, consider a simple four-state return process in which the four possible states are all four combinations of «High» and «Low» returns for bonds and stocks in a given year, e.g. the joint outcome $H^B \cdot L^S$ denoting high bond and low stock returns. Assume that the process is Markovian, and not Independent and identically distributed random variables (i.i.d.) Assume also that the following two conditions characterize this process governing returns:

1. Stock and bond returns are negatively correlated; and
2. The two states $H^B \cdot L^S$ and $L^B \cdot H^S$ exhibit persistence in that there is a *high* transition probability that, once the system is in either of these two states, it will remain there.

Then under these conditions, the process will generate long-run bull and bear markets.

What are the normative implications of this result? How can an understanding of long cycles bestow a competitive advantage on a long-horizon investor? To take the simplest case, suppose an investor adopts as his best forecast the stationary measure of a returns process, and suppose that the joint distribution of stock and bond returns satisfies the two foregoing conditions. Then to do so, assuming that the investor wishes to maximize utility, he will apply the logic of stochastic dynamic programming for Markov chains (Lucas - Stokey, 1989). The result will be a state-dependent strategy or «optimal policy» that will cause him to stay heavily invested in the asset where high returns are being garnered *and* largely disinvested in the other asset class for fairly long periods, on average. This strategy will reap the excess returns associated with the serial correlation present in both bull and bear market regimes.

Interestingly, it will *appear* as if the investor is actively managing his investments by engaging in the dangerous strategy of market timing. But this is not in fact the case. His policy function will result from purely *passive* considerations given his adoption of the stationary measure as his best forecast, and given the logic of dynamic portfolio optimization. The investor need make no judgments about market timing and turning points. Moreover, a failure to identify and utilize the optimal policy function would be irrational and would cause the investor to suboptimize relative to his own belief structure.

This completes our discussion of the second canonical class of strategies whereby investors can reap excess returns in a theoretically legitimate manner: the exploitation of several different manifestations of endogenous risk. In an RE environment, none of these strategies is available because endogenous risk does not exist.

STRATEGY 3: *Exploiting Logical Errors of Inference*

The third and final strategy for enhanced returns stems from an investor's ability to avoid widespread errors of inference that cause many investors to make poorer forecasts and, thus, lower returns. Here, we are proposing neither to exploit superior inferences about structural change (Strategy 1) nor to exploit the different types of endogenous risk (Strategy 2). Rather, we are exploiting the fact that the *logic* (both inductive and deductive) involved in economic and asset price forecasting is demanding and often counter-intuitive, even in contexts where non-stationarities do not arise.

This last point is important, for it means that we now broaden the domain of exploitable inferences beyond those occasioned by non-stationarities. From a normative perspective, investors who chose to educate themselves about the counter-intuitive aspects of economic and market behavior end up less wrong in their forecasts than those who do not.

Reviewers of this paper questioned the extent to which this third strategy is consistent with RB theory, as the other two strategies clearly are. In particular, they suggested that the utilization by any given agent of the third type of strategy amounts to the exploitation of the «bounded rationality» of others. If so, do not such strategies violate the RB assumption of unbounded rationality on the part of all agents? This difficult matter will be discussed at the point 1 of the Conclusion.

EXAMPLE 1: *Canonical Example of the Birthday Paradox*: This first example is deliberately non-economic in nature, yet it so clearly illustrates an aspect of the logic involved. Consider the following textbook problem: «What is the probability that, out of 50 randomly selected people, some pair have the same birthday?». The only datum needed to arrive at the right answer is that the probability of a randomly drawn person having a given birthday is 1 in 365. This information is known by everyone. The correct answer to the question is that it is slightly over 98% probable that some pair have the same birthday. Yet in posing this question to several hundred randomly selected people over the past decade, the author has heard widely varying answers with a mean of about 7%.

People's inferences are erroneous for two reasons – and lack of correct data is not one of them. First, it is counter-intuitive that the right way to solve the problem is to invert it and to ask what the probability is that some pair does *not* share a common birthday. Second, it is very difficult for normal people to fathom the compound conditional probability calculations needed to arrive at the right answer to this inverse question, in particular to utilize Stirling's formula from combinatorial analysis. Most of the examples that follow will resemble the birthday paradox in that the generic problem is not one of inadequate information, but of faulty inferences from available information due to the difficulty of the logic involved. Sometimes the required logic is inductive and sometimes, deductive. We discuss the relationship between the concept of erroneous inferences and that of «bounded rationality» in the essay's Conclusion below.

EXAMPLE 2: *The Paradox of Interest Rates and Foreign Capital Flows*: It is all but axiomatic throughout the investment management industry that one of the greatest threats to the US economy and financial markets lies in the prospect that «foreign investors who have been funding US trade deficits will tire of acquiring more US assets, will stop financing the US current account, and will pull their money out». The result would be much higher US bond yields and a lower dollar. The former would weaken the US economy, and the latter would drive up inflation.

Along these lines, it was widely believed that the Japanese would pull their funds in the latter 1980s. But this never happened. In more recent years, it has frequently been rumored that the Chinese would do the same. But this too has not yet happened. Nonetheless, foreign asset preferences have shifted significantly over time, with alternating regimes in which dollar assets fell from favor (for example, 1972-1980 and 1985-1988) followed by regimes in which dollar assets rose in favor (for example, 1980-1985, and 1995-2000). In the former cases, the price of the trade-weighted dollar fell precipitously, and in the latter, it climbed. Yet such shifts in asset preferences failed to affect interest rates materially – to the surprise of most investors. Indeed, the failure of interest rates to correlate negatively with the dollar has long been a paradox that has led to many wrong forecasts.

Indeed, since the end of the Bretton Woods era, US interest rates have *positively* correlated with the dollar, as was documented in a recent Federal Reserve Bank study (Gagnon, 2005). So surprising and counter-intuitive were this study's findings that they were written up in an article in *The Economist* (September 10, 2005). Both the Federal Reserve Board study and the release of its findings treated the matter empirically. That is, the relationship between the dollar and interest rates was established after the fact on the basis of real-world data. Could this counter-intuitive behavior have been deduced decades in advance from first principles – leading to superior forecasts and risk assessment?

Yes, and Brock (1985, 1986) deduced this relationship by exploiting the particular subset of national income accounting identities that made clear *why* foreign asset preference shifts would have little impact on the level of US bond yields. In particular, the algebraic identity of the capital and current account of every nation ensured that (1) foreigners could never stop funding the US current account deficit as long as it persisted, (2) foreign holders of US assets *as a whole* could never «pull out their money» from the US (the «no net selling» condition of central banking), and (3) the dollar and not the interest rate would bear the brunt of adjustment when US assets lost favor in the eyes of foreign investors.

Investors who understood these deductions not only made superior forecasts of interest rates and of the dollar, but also of the US economy. For when the dollar and not the interest rates bear the brunt of the adjustment process, the real economy is *stimulated* due to a larger volume of exports – and not depressed by higher rates.

EXAMPLE 3: *Fallacies about Expected Returns from Simple Trades*: When a naive investor contemplates a trade, he may well assess his expected return by taking into account only his own forecast of future price. A more sophisticated investor will compare his own forecast with the market consensus, understanding that what matters is the extent to which the market has not anticipated his view. But a truly sophisticated investor will contrast his own forecast with that of the consensus *and* with that of the stationary measure revealed by empirical analysis. This is a central empirical and theoretical result of Kurz - Motolese (2006). They reveal both theoretically and empirically that the most accurate forecast of the risk premium results from a proper comparison of all three variables.

Clearly, an investor who consistently and correctly takes all three of these variables into account will possess a competitive advantage in forecasting returns. Most investors will *not* do so because the logic dictating that they should is both new and counter-intuitive – even to the authors of the paper cited.

EXAMPLE 4: *Fallacies about Optimal Portfolios and Asset Allocation*: This example of how to add value through superior inference stems from widespread confusion about the nature of an optimal portfolio in a dynamic setting and about the meaning of the «efficient frontier.» We attempt to clarify matters by demonstrating the existence of *three* successively general efficient frontiers that *three* successively general statements of the portfolio problem imply. From a normative standpoint, an active manager seeking to outperform his market benchmark should restrict his attention to the «highest» of these frontiers where the expected return per unit of portfolio risk is the largest. Interestingly, to achieve a return on this highest frontier, the investor will have to apply RB theory in a novel manner.

The first of the three portfolio logics we review was originally set forth by Markowitz (1952). In this highly specialized case, the environment is implicitly assumed to be stationary and the stochastic process governing asset returns is assumed to be i.i.d. At an abstract level, the relevant optimization logic can be summarized as:

$$L : R \times U \to P \tag{5}$$

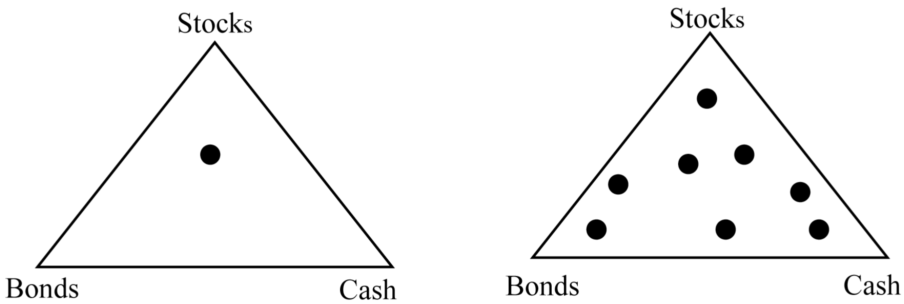
where L denotes the logic of static non-linear programming, R is the unconditional joint distribution of asset returns, U is the agent's utility function, and P is the set of all feasible portfolios. Whether the optimization problem is conceived of as static or dynamic does not matter because, given a fixed level of risk aversion, an investor's optimal portfolio never changes.

At the next level of abstraction, the underlying stochastic process of returns is allowed to be *non*-i.i.d., for example Markov. This renders the portfolio problem essentially dynamic in nature. An implicit assumption of stationarity is still maintained. This is the problem analyzed by Samuelson (1969), and its logic can be summarized as:

$$L^D : R^C \times U \to S \tag{6}$$

where L^D denotes the logic of stochastic dynamic programming, R^C is the *conditional* joint distribution of asset returns – a stationary measure adopted by all investors, U denotes the utility function, and S is the set of feasible policy functions. (A policy function in dynamic programming specifies which portfolio is the utility-maximizing portfolio for a given «state» or «history».) An investor will thus rotate state-dependent optimal portfolios as the state changes, just as a farmer mechanically rotates his crop mix with the seasons. Figure 1 diagrams the difference between the solutions to (5) and (6).

FIGURE 1



A. Classical Case: One Optimal Portfolio

B. General Case: One Optimal Strategy
(designating which portfolio is optimal in each possible «state»)

In the third and most general representation of the portfolio problem, the underlying process governing returns is non-i.i.d., as in the second case, but the restrictive assumption of stationarity is replaced by the more general assumption of stochastic stability. The logic here can be summarized as:

$$L^D : RB_i \times U \to S \tag{7}$$

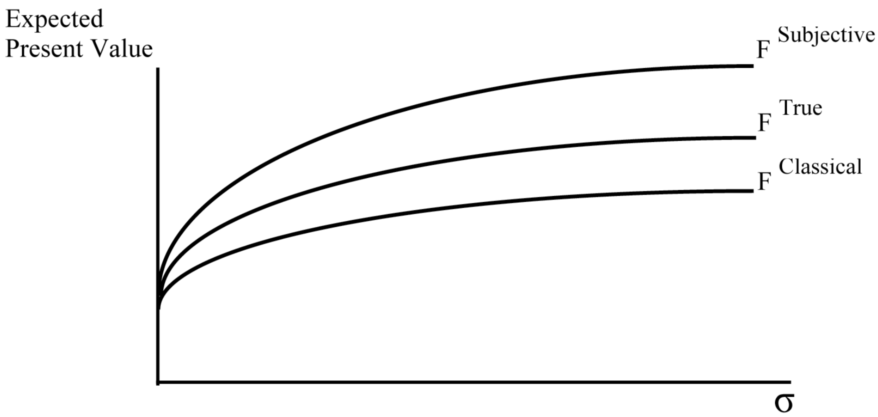
where L^D remains the same as in (6), RB_i denotes the investor i's *subjective* rational belief about the sequence of future states and prices (it is essentially equation (2) above), U remains agent i's utility function, and S remains the set of feasible policy functions. The notation in equations (5) – (7) makes clear the precise way in which the logic of (5) is a special case of the logic of (6), and the logic of (6) is a special case of the logic of (7).

We now note that these three representations of the portfolio problem give rise to three successively «higher» efficient frontiers, as illustrated in Figure 2. The gap between the two lowest frontiers represents the gains in risk-adjusted returns that can be made from exploiting the non-i.i.d. nature of the joint conditional distribution of returns, that is, the memory structure of the underlying stochastic process. More specifically, an investor who troubles to learn that the distribution is non-i.i.d. and exploits this (via dynamic programming) to arrive at an optimal policy function rather than an optimal fixed portfolio will achieve a *higher* payoff, one that lies on the «True» frontier.

It is important to note that, contrary to what many practitioners might infer, this gain does not result from «active» investment management. The investor accepts the stationary measure R^C and optimizes over it in a strictly passive and indeed robotic manner. Our discussion of «passive market timing» above included an example of such a strategy.

The intermediate frontier is designated «True» because it is objective in nature. It is calculated from optimizations over the stationary measure R^C carried out for all degrees of risk aversion. Constructing this frontier requires no subjective judgments about the future, just as is the case in the classical Markowitz frontier applicable to i.i.d. environments.

FIGURE 2 - The Three Efficient Frontiers of the Dynamic Portfolio Problem



This is not the case with the highest possible frontier, the «Subjective Frontier». This frontier is generated by optimizing according to agent **i**'s personal, subjective beliefs about the future – equation (**2**). The reason that this frontier will always dominate the others is subtle. The Subjective Frontier incorporates the payoffs that come from agent's own beliefs that his forecast of the *timing* of future events is superior to that implied by the stationary process. Recall that the «rationality constraint» of RB compels him to accept the stationary forecast of the mean market return. His sole advantage in seeking to outperform his benchmark lies in the postulated superiority of his forecast of the timing of future market returns.

He will thus solve for a different and (in his eyes) superior policy function than an agent who accepts the stationary measure as his forecast would pick. Moreover, his expected utility payoff from adopting this policy function will be *higher* than that associated with any other possible policy function given *his* beliefs about the future. As a result, the top-most efficient frontier will always dominate the intermediate one *in his eyes*. Hence, we have labeled the top-most frontier the «Subjective Frontier». Kurz himself (2008) notes that the frontier relevant to a RB world is indeed subjective.

Brock (2004) has shown that this generalization of portfolio theory renders problematic such classical concepts as (1) the cap-weighted market portfolio as a normative performance benchmark, (2) the alpha versus beta distinction, (3) the active and passive management dichotomy, (4) the distinction between tactical and strategic asset allocation, and (5) the concept of rebalancing towards an optimal portfolio. Regarding the last two concepts, Figure 1 makes clear why an optimal policy function is *automatically* optimal tactically as well as strategically. This is a simple corollary of the application of Bellman's Principle of Optimality (backwards induction) in dynamic programming. Moreover, the classical notion of tactical asset allocation as «optimal rebalancing to a fixed optimal portfolio» has no meaning in this more general context, just as it has no meaning in an agricultural context where a farmer completely rotates his crops with the seasons. In both cases, there is nothing towards which to rebalance.

EXAMPLE 5: *A Logic for Creating Superior Models of Structural Change*: In the first class of strategies we discussed for outperforming the market (the exploitation of structural changes), we characterized a theory of structural change as an exercise in the revision of conditional probability: «Given the structural changes that are occurring, I believe I now need to revise my former conditional probability $\{x|y\}$». But exactly how might such revisions be accomplished in a non-stationary world where historical data may be irrelevant or non-existent, where the inferences to be made are necessarily subjective, and where the nature of the relevant conditioning information may well be qualitative?

To help answer this difficult question, consider the example of an investor obliged to construct an optimal portfolio based on incomplete and subjective information. Pretend that it is the year 1992, that Russian markets are opening up, and that the investor is striving to construct a truly global portfolio including Russian assets. No historical data on Russian returns yet exist to help. Still, according to the logic of portfolio optimization, he needs *some* estimates of the relevant means, variances, and covariances in order to construct an optimal portfolio. At a normative level, how should he process the limited and indeed subjective information that he possesses to arrive at credible estimates of these parameters?

Bayesian decision theory provides important guidance here. A key general rule for investors is to exploit the Principle of Asymmetric Inference (Raiffa, 1968 and Howard-Matheson 1984): When confronted with an assessment problem in a non-stationary environment or where desirable data is non-existent (for example, Russian returns), deduce one's subjective probabilities by conditioning one's inferences in that particular logical order that best corresponds to one's understanding of the world.

For example, while an analyst might find it difficult if not meaningless to assess US returns *given* Russian returns, he will be quite comfortable assessing Russian returns *given* US returns. This could be true because US markets help drive emerging markets. Let us denote this particular problem as one of assessing the conditional distribution $\{x|y\}$ where x denotes Russian returns and y denotes US returns. Using elementary operations of probabilistic expansion, the *unconditional* mean of Russian returns that will be needed in portfolio optimization is easily computed from this distribution. How can the second moment of the joint distribution be computed, however, to yield the required VAR and COV parameters? Few analysts can assess such variables intuitively – especially in the case of COV.

Brock and O'Leary (2007) have drawn upon the Principle of Asymmetric Inference to resolve this problem. They show that the classical definition of the correlation between x and y

$$\rho[X, Y] \equiv \frac{Cov[X, Y]}{\sqrt{V[X]}\sqrt{V[Y]}} \tag{8}$$

can be *re-expressed* as

$$\rho[X, Y] = \frac{E[(Y - E[Y]) \cdot E[X|Y]]}{\sqrt{V[E[X|Y]] + E[V[X|Y]]}\sqrt{V[Y]}} \tag{9}$$

Note in (9) that *every* term in the classical definition (8) has been replaced by a term in which the required distribution (or expected value) of Russian returns x is *explicitly* conditioned on US returns y, thus facilitating the required assessments. This formalism can clearly be extended to «Bayesian updating» in a dynamic context. Moreover, Bayes' theorem can be invoked to yield the inverse distribution $\{y|x\}$ if the assessment problem demands it. In deriving this, however, the analyst need never think in terms of the required reverse conditioning with which he is (by assumption) completely uncomfortable. The mathematics of probability theory achieves all this for him.

What does all this say from a normative standpoint? The benefits from assessing statistical moments conditionally in the manner of (9) are analogous to the benefits that accrue from learning probability theory to make superior bets on paradoxical problems such as the birthday paradox. In the present case, the information needed to construct an optimal portfolio in a world of inadequate data can be extracted in a logically consistent manner by properly applying the laws of conditional probability. Investors who utilize such modes of inferences should outperform those who do not because the latter will end up utilizing forecasts that are demonstrably *inconsistent* with their own best beliefs, and thus inferior.

EXAMPLE 6: *Fallacies about Asset Market Pricing*: In his celebrated theory of Asset Market Equilibrium, James Tobin (1969) first pointed out that changes in flow-of-funds variables (e.g., fiscal deficits, savings rates) are much less important in explaining changes in asset prices than are changes in stock-of-wealth preferences. The latter refers to the preferences of investors as to how they wish to allocate their entire *stock* of wealth. Yet most investors do not understand the stock/flow distinction introduced here. Not only is the distinction not taught to them during their training, but the news appearing daily on their Bloomberg screens centers almost exclusively on flow variables such as the current rate of Chinese central bank purchases of US Treasuries, the current household savings rate, the current growth rate of GDP, etc. Investors therefore understandably believe that the behavior of such flow variables determines the levels of asset prices and yields, and will do so in accord with the law of supply and demand.

But this is not the case. While flows can matter, stock readjustments typically matter much more (Friedman, 1977). Consider the low US bond yields of recent years (the celebrated «Greenspan Conundrum») and what caused them. Those believing in flow explanations have been perplexed because credit demands in the past four years have been very strong and the savings rate has reached an all-time low. Supply and demand in a flow sense would thus predict high yields.

Now consider the stock-of-wealth adjustments that have occurred, but which are never in the news. There has been a significant *reallocation* of US wealth from equities into fixed income assets and real estate. Consistent with this view, the valuation of the broad stock market as measured by the price/earnings ratio of the broad US stock market has fallen by nearly 50% between 2001 and 2007. Conversely, bonds prices have soared given the increasingly desperate quests for yields by pension funds and individual investors confronting a new era of retirement without defined benefit pension plans. Meanwhile, real estate valuations have also risen significantly.

From a normative standpoint, the forecasts of investors who understand the relevant stock-versus-flow logic will be based on the right variables and the right logic. Such investors should reap higher returns than those who do not.

EXAMPLE 7: *Confusion about Inflation and Monetary Policy*: Commentary in today's financial press reflects abundant confusion about the meaning of inflation, the sources of inflation, and the role of the monetary authority in the economy. Given the impact of inflation on assets prices and monetary policy, and given the critical role of the Fed, a superior understanding of inflation and monetary policy will give any investor a significant advantage. For brevity, we simply cite a single widespread misunderstanding that can be exploited, although many others exist. The relevant inferences in this case are primarily deductive rather than inductive in nature, as is true of most of the examples given above.

Assertion: «US national net worth as computed by the Federal Reserve Board has soared from $10.3 trillion in 1980 to $55 trillion today in 2007. The dramatic inflation of asset prices underlying this explosion of wealth is the result of an irresponsible creation of fiat money by the Federal Reserve Board. I am becoming sympathetic to the views of gold bugs that rampant inflation lies ahead and that gold really could rise to $10,000 an ounce».

Errors of Inference: The principal error here is to believe that it is necessary for the monetary authority to have created or «printed» money at a very rapid rate in order for asset prices to rise rapidly, as indeed they have. This is not the case. Indeed, the growth rate of the one kind of money directly under the control of the Fed (the domestic monetary base consisting of bank reserves and domestically held currency) has been at about 5.5% for the past two decades, a rate significantly lower than the 7.5% growth in household assets and net worth.

When money is *not* being printed at an irresponsible rate, the following factors can still combine to cause asset prices to rise rapidly: (1) rising investor optimism about returns (a shift in the belief structure), and (2) a rapid increase in credit availability. The latter permits optimists to acquire assets (for example, houses) at ever-higher prices. Have we witnessed rapid credit growth? Yes. During the past two decades, the 9.1% average growth rate of household credit has far exceeded the 5.5% growth rate of Fed-controlled money. This expansion is due to financial innovation and deregulation, and, in particular, the rise of non-banks. (See Brock, 2006 on all these points.)

A correlated error of inference that arises here helps explain the frustration of gold bugs whose expectation that the price of gold would soar into the thousands has long been disappointed. (Indeed, since 1980, the real price of gold has not exploded but declined greatly.) This is the erroneous belief that there is a *single* type of inflation that governs both asset prices *and* goods and services inflation. In fact, however, there are two quite distinct types of inflation. Goods and services inflation has been running at a moderate 2%-3% for well over a decade, reflecting both the above cited moderate growth in the monetary base and well-behaved unit labor costs. Asset price inflation, by contrast, has been running much higher on average, reflecting both investor optimism about returns and the unprecedented availability of credit.

Goods and services inflation, in theory, is directly linked with gold prices. This is because very high inflation in the goods and services markets typically signifies gross mismanagement of the economy. This in turn precipitates a loss of confidence in the government (for example, the case of Zimbabwe today where goods and services inflation is running at 3100%). With US good and services inflation having been moderate, it is thus not surprising that gold prices have remained stable. The dramatic rise of many asset prices is an independent matter, for reasons sketched above.

IV - CONCLUSION

In this concluding note, we address two outstanding issues: the problem of bounded rationality in the context of RB theory, and the degree to which our three proposed strategies are canonical.

1. *The Problem of Bounded Rationality*: RB is a theory of *unbounded* rationality in the sense that all agents are assumed to maximize their expected utility with respect to beliefs which are assumed to be rational. In particular, agents posses the requisite computational power, storage, and memory needed to carry out their optimizations. In commenting on the present essay, two reviewers questioned whether the strategies proposed herein – Type-3 strategies in particular – are compatible with the assumption of unbounded rationality. Further, if they are not, then does such incompatibility raise a problem?

At first glance, a normative theory positing that some subset of agents can draw inferences superior to those drawn by others from the same information would seem to assume that some agents are more rational than others, or equivalently that some agents are «boundedly rational». If this is the case, then would the proposed normative theory be deemed incompatible with RB theory and thus unsatisfactory – because RB assumes unbounded rationality? While the answer might seem to be yes, we will show that this is not the case. In clarifying these issues, we shall also attempt to demonstrate that the concept of unbounded rationality is highly problematic in the context of inference generation in a non-stationary environment, and even in a stationary environment such as that of the laws of physics.

To begin, all agents in our normative theory can be assumed to be exactly as rational as agents are assumed to be in positive RB theory. This is so because in both theories (i) all agents are unboundedly rational, in that they maximize lifelong expected utility with respect to their beliefs (forecasts), and (ii) all agents possess beliefs that are rational in that they are non-contradictable by the data. (However, this second proviso may be violated by use of our Type-3 strategies as will be seen below.)

Our normative theory differs from RB theory only in the sense that it addresses a question not (previously) posed within RB theory: In a non-stationary environment that generates difficult-to-understand structural changes, can agents who are willing to spend the requisite time and money identify a *subset* of RB-rational beliefs that are *superior* (in the sense that, based on symmetric information, they draw superior inferences that generate better forecasts)? Our answer is yes, and we have proposed specific strategies for identifying such subsets. Nothing in RB theory proper would seem to contradict the claim that such superior subsets of beliefs can be identified. Indeed, in certain respects, the proposed research strategy parallels the effort by Nielsen (1996) to identify a subset of rational beliefs that are «more plausible» than others. Our effort, however, is arguably more normative in intent than is Nielsen's.

Beyond merely being compatible with RB, the proposed theory should be viewed as being inspired by the very perspective on economics that RB has made possible. This is an economic landscape in which agents can exploit the «wiggle room» around the stationary measure that non-stationarities afford. In doing so, an agent will interpret commonly available data in the subjective manner that *he* deems best. RB theory does not in any way restrict his efforts to do so, and does not pass judgment on what a «better» or a «best» interpretation might mean. The only restriction is that the resulting belief must be consistent with the empirical measure set forth in equation (2).

This indeterminacy at the heart of RB is, in our view, part of its strength as a positive theory. Some might argue that RB could or even should have impose additional criteria of rationality designed to restrict the way agents should arrive at their rational beliefs – e.g., criteria of the kind we have proposed. Yet we would not agree. As a positive theory, RB does – and indeed should – focus on explaining real world data, and recent RB research (Kurz - Motolese, 2006) indicates how successful it has been in this regard. Since the variability of real-world outcomes that the theory sets out to explain reflects the beliefs of agents who do understand the implications of national income accounting identities for interest rates and currencies as well as those who do not, the theory as a positive theory must accommodate all such agents. In particular, it should not ignore subsets of agents even if their inferences can be shown *ex post* to have been inferior to those of others.

Similarly, RB accepts that in a world where agents are rational-yet-wrong, there will be subsets of agents who are «less wrong than others» (Kurz, 1994). An underlying reason could be the ability and/or incentives of such agents to draw superior inferences. From an RB perspective, the existence of such inferences can be viewed as one particular manifestation of heterogeneous beliefs arising from structural ignorance.

Suppose, however, that economists were to attempt to impose specific criteria of unbounded rationality upon the efforts of agents to arrive at beliefs that are RB-rational in a non-stationary environment. Waving aside the fact that they should not do so in the context of positive theory construction, *could they do so?* Would we really know what criteria to impose? In particular, does the concept of unbounded rationality even make sense in this context? We believe that it does not, and thus any such programme will not be viable. To see why this so, consider the following two difficulties that would arise in attempting to mandate the generation of unboundedly rational inferences in a non-stationary world. Note that the second set of limitations applies to stationary as well as non-stationary environments.

DIFFICULTY 1 - *Challenges to Inductive Logic*: Even if one assumes perfect data-processing and storage capabilities, the concept of unbounded *inductive* rationality has never been meaningfully defined for non-stationary yet stable environments, to the best of our knowledge. Kurz (1994) appreciated the heart of the problem, pointing out that the existence of unpredictable and initially unrecognizable regime shifts dooms the role of most forms of inductive logic in arriving at «the asymptotic truth». There is simply no truth to converge to except for that of the stationary measure. The problem here runs much deeper than traditional problems of convergence that arise even in stationary contexts, e.g., the failure of convergence for particular subsets of priors in Bayesian inference. For a good overview of the difficulties involved, see Dawid (2004).

DIFFICULTY 2 - *Challenges to Deductive Logic*: The problems posed by defining unbounded *deductive* rationality in inference generation are perhaps even greater than in the inductive case, and extend to stationary as well as non-stationary environments. We focus in particular on the process of constructing a theory from which meaningful deductions can be drawn and used in forecasting, e.g. the laws of physics or of biological evolution. The reason why the concept of unbounded deductive rationality is inherently problematic here is that the process of scientific discovery underlying theory construction is *itself* non-stationary. Thus, the evolution of physics (wherein the underlying laws of space-time are assumed to be stationary by virtue of their being laws about space-time) is studded with conceptual advances that arrive according to some non-stationary process. Each alters the nature of subsequent efforts to deduce the truth about the *same* stationary phenomena such as the orbits of the planets, or the behavior of matter.

In such a context, the concept of unbounded deductive rationality borders on the preposterous. To illustrate this point, a footnote discusses an example in physics that amounts to a *reductio ad absurdum*. This example demonstrates the bounded rationality of Einstein in his failure to think correctly according to the canons of deductive logic about the true nature of the time-invariant laws of general relativity. The reason for Einstein's deficiency in this regard was a non-stationarity in the evolution of the history of science: Specifically, he worked at a time *before* the discovery of the logic of field variables that obey the algebra of quaternions mapped in a Riemannian space-time manifold. And because of this, he «irrationally» failed to discover the generalization of general relativity he sought for so many decades. Moreover, he realized his fundamental mistake in this regard by the end of his life.[5]

[5] Einstein arrived at his 1916 general theory by appropriately imposing an invariance requirement on the yet-to-be-discovered laws of space-time, and by then exploiting this symmetry argument mathematically. He required that such laws be covariant under a particular group of transformations, namely the continuous transformations of relative motion *as well as* the discrete transformations of time reversal and «handedness». The result was his 1916 theory consisting of ten independent metrical field equations that were covariant under both the continuous and the discrete transformations. Unfortunately, it was discovered in experiments of the 1950s and 1960s that the laws of motion do not satisfy the discrete transformations. In particular, the laws of motion are not in fact time reversible.

But by that time, Einstein (1945) realized he has made a mistake – a logical mistake at odds with the deductive logic of the theory of normative theory construction. Specifically, he had not required that the equations he was looking for in creating his theory of general relativity be covariant under what he had come to realize was the *right* symmetry group, namely the broader symmetry group associated with the concept of continuous relative motion alone. Had he, he would have known that he would have needed not ten but sixteen independent equations, since the underlying irreducible symmetry group is a sixteen parameter Lie group. Normative theory requires that the number of independent equations equal the number of independent parameters of the *irreducible* symmetry group of the theory (Sachs, 1970).

But had he gotten this far by having been unboundedly rational, he would also have known something even more profound: Just as Einstein had correctly realized the need to generalize the *geometric* logic of space-time from a Euclidean to a Riemannian logic, he would also have realized the need to generalize the *algebraic* logic of field theory to that of the non-commutative algebra of quaternions. For the relevant field variables would transform as spinors in accord with the algebra of quaternions (Sachs, 1982). This followed immediately from the nature of the irreducible representation group of the 16 parameter Lie group. But Einstein's bounded deductive rationality prevented his apprehending all this, and as a result he never succeeded in creating his unified field theory.

To conclude, we believe that in a non-stationary environment, fundamental problems arise in defining much less applying the concept of unbounded rationality within the context of inference generation and belief selection. In attempting to resolve these problems, RB strikes an appropriate compromise. It postulates that each agent is unboundedly rational in that he maximizes expected utility with respect to his own subjective yet rational beliefs – and does so in a demonstrably correct manner. Specifically, he must possess an extraordinary ability to understand the intertemporal optimization process, to compute, and to store information. However, RB is silent about the degree much less the nature of such «inferential rationality» as may be needed for the agent to select what he deems to be an optimal belief within the set of admissible beliefs. As a positive theory, RB does not need to take this extra step. In contrast, a normative theory consistent with RB should take it. But the limitations of both inductive and deductive logic that arise in the belief selection process render this a very difficult problem. In the next section, we propose a *generalization* of RB theory that might emerge as a result of addressing this formidably difficult problem. Interestingly, this extension encompasses environments that are both non-stationary as in RB theory and also stationary as in physics.

Type-3 *Strategies, Bounded Rationality, and a Proposed Extension of RB Theory*: In motivating our Type-1 and Type-2 strategies, we took pains to show how both are consistent with and indeed inspired by RB theory. In particular, both exploit the «wiggle room» provided by non-stationarity as it arises in the context of a stochastically stable process.

Our Type-3 strategies are fundamentally different. Some of these exploit biases in agents' understanding of how to optimize a portfolio and how to compute certain conditional probabilities. Such strategies are clearly inconsistent with unbounded rationality since the requisite laws of optimization and computation exist and could in principle be known by all agents. Other Type-3 strategies exploit invalid biases in the market's estimation of the *mean* of the price-to-state map M^*, and/or in the mean of the process $\{X^e\}$. For example, if the market believes that the dollar will fall and

This more ambitious and deductively-rational level of theory construction would be successfully carried out several decades later by Sachs (1982), who not only discovered the appropriate set of sixteen equations, but also discovered that the extra six equations constitute a generalization of Maxwell's equations. Additionally, the linear eigenfunction formalism of quantum-mechanics can be derived from these extra six equations as an appropriate asymptotic limit – but with no problematic probabilistic interpretation. This, of course, vindicated Einstein's fervent belief that there exists a single set of gravitational-electromagnetic equations that would unify much if not all of physics. Do we seriously claim that Einstein failed to find these because his «bounded rationality» led him to utilize the wrong symmetry group and hence the wrong algebraic language of physics? No. Einstein did the best he could *given* the state of development of mathematical physics in the 1910-1940 era of his own research. This example illustrates how the concept of unbounded (deductive) rationality is highly problematic in the philosophy of science. Brock (1992) discusses this point at length in the context of physics, game theory, information theory, aesthetics, and econometrics.

interest rate will rise when foreigners lose interest in US assets and sell them (a bias in estimating M^*), then agents who discern that the underlying correlation is *positive* rather than negative can profit by betting against the market. This particular strategy is clearly inconsistent with RB theory – as the theory stands at present. For unboundedly rational agents would know from the stationary measure that the two prices involved are *not* negatively correlated.

Yet is this all that can be said about the matter? No. Could not cases arise in which the stationary measure itself is both *biased and inadequate* in revealing the truth about the «mean law of motion» of the economy? If so, the appeal of defining rationality in terms of non-contradictability by the stationary measure will be much diminished. Is this not the case in economic applications where problems of observability and/or identifiability arise that bias statistical findings and constrain the ability of induction to reveal the truth? And is it not the case in physics where for many centuries the stationary measure implied that the sun went around the earth, that there were no black holes, and that dark matter did not exist? Our Type-3 strategies mainly target problems like this – problems in which errors of inference about the true law of motion exist and can be exploited.

In such cases, might not the inductive search for truth be *augmented* by the use of deductive logic just as has happened during the past three centuries where the interplay between deductive and inductive logic over time has accounted for most all scientific progress? More specifically, can deductive logic not complement inductive logic (i) by cautioning agents about the inadequacy of existing data when appropriate, and (ii) by implying altogether new experiments that generate new data that *ex post* will generate an altogether different stationary measure on a new and augmented set of state variables? And will this new stationary measure not reveal the previous one to have been both biased and inadequate? Finally, will not certain agents apprehend such deductive advances in our understanding of reality ahead of others?

If so, can such agents be deemed rational if they *ignore* these developments and adopt a belief that is consistent with the *old* stationary measure based upon old and inadequate theory? Most probably not. Indeed, from a Savage decision theoretical standpoint, they will be deemed irrational for doing so. This is because they would be ignoring information relevant to decision-making in order to comply with an arbitrary statistical straightjacket. But from an RB perspective they *will* be deemed rational for ignoring such deductive inferences.

In the case of economics, if an agent needs to predict how a particular labor/management dispute will be resolved, or how tensions between the Middle East and the US will be resolved and will thus impact oil prices, he might well invoke the purely deductive Nash-Harsanyi theory of bargaining to arrive at an estimate of the most likely outcome. But doing so renders his forecast a function of two variables that, while central to the Nash-Harsanyi theory, are often unobservable: the relative degree of risk aversion of the two parties, and their relative threat power. It is doubtful that the stationary measure on observables would provide sufficient guidance for decision-making here. We emphasize that this difficulty will arise in both stationary *and* non-stationary environments.

Can RB theory be generalized to cope with the family of problems we have cited? We believe it can be. Acknowledging the role of the non-stationarity of the process of scientific discovery itself, one could identify *a time-dated sequence of stationary measures.* This sequence would be indexed by «markers» designating the advent over time of important new theories (usually deductive in nature) – theories that call for an augmented set of observables needed to better understand the true law of motion of the economy, and to better estimate its stationary measure. A value of unity for this index would correspond to perfect theoretical knowledge.

For example, *subsequent* to the Nash-Harsanyi marker, researchers interested in analyzing the role of «power» in a political economy began their search for observables that might indirectly reveal the relative risk aversion of the players. It was the advent of new theory that made such a search for new observables appropriate. Moreover, a stationary measure incorporating such considerations would presumably be *superior* to one that omitted them. But if this is true in general, than the proposed sequence of stationary measures should converge monotonically to a better approximation of «the truth» as time goes on, and as deductive theories evolve and improve.

Regrettably, there is a snag in all this. Suppose that a new and superior theory of a deductive nature raises new problems of observability and/or identifiability in inductive logic. What can be done in this case? The proposed sequence of stationary measures could then be sub-indexed by an «empirical adequacy index» measuring the degree of adequacy of empirical analysis alone for arriving at a stationary measure *sufficient* for optimal forecasting by a decision theoretically rational agent. A score of unity for this index would imply that empirical analysis alone suffices for optimal forecasting, and that no augmentation of induction by deductive logic is called for. A necessary but not sufficient condition for a perfect score would presumably be the absence of any observability and/or identifiability problems. Only in this case will the stationary measure of RB Theory be an *unbiased* measure at time t in no need of emendation by the implications of deductive logic.

This score will in principle fluctuate over time, and we see no a priori reason to expect it to converge to anything. Thus, suppose the advent of the Nash-Harsanyi theory in the 1950s calls for the incorporation of variables such as relative risk aversion that are *empirically* unobservable yet *conceptually* indispensable. If so, our first index score would have been higher, but our second index will have a lower score than before the advent of the new theory, other things being equal. Of course this score could then rise back up as a result of future *theoretical* progress.

To conclude, we have proposed a generalization of the concept of Belief Rationality that would encompass deductive as well as inductive considerations. Such a programme would seem completely consistent with the permission that classical RB theory grants each agent to select some belief within the set of all Rational Beliefs as his own belief. Yet unlike previous RB theorists, we are interested in the process of belief selection pertinent to an agent attempting to identify a *best* belief from the set of all admissible beliefs. In doing so, we are led to a generalization of the classical criterion of Belief Rationality that equated «the best information available for decision-making» with «the stationary measure on observables to date». This generalized criterion of Belief Rationality will generate a *different* set of rational and thus defensible beliefs from which a best belief must ultimately be selected. Moreover, this set will evolve over time, as will the stationary measure on all observables central to classical RB theory.

2. *The Canonical Nature of Our Three Strategies*: There remains one last issue. We must justify the claim made in the Introduction that all theoretically legitimate strategies for systematically outperforming the market will reduce to some combination of our three canonical strategies. This claim stems from the fact that returns are ultimately functions of future prices, and that asset prices in RB are determined in accord with the generalized pricing map of equation (4). An inspection of this map shows that any competitive advantage that an agent might possess regarding the prediction of future prices will flow from (i) a better ability to assess the news – that is, the stochastic process $\{X^e\}$ governing the evolution of the extended state vector, (ii) a better ability to assess the implications for the behavior of prices (including «volatility») due to correlated mistakes about $\{X^e\}$ *and* uncertainty about the pricing map M^*, and (iii) an ability to exploit biases in the market's beliefs about the true map M^*, and/or to improve the map M^* itself via the inferences of deductive logic.

These are the only classes of superior inferences that are possible in a theory of a stochastically stable economy where (4) governs asset prices, and these are exactly the classes of inferences that our three strategies target. It thus seems plausible that any performance-enhancing strategy consistent with the logic summarized in (4) must factor into some combination of these three strategies. Moreover, all strategies we have analyzed satisfy this property. Of course, some new theory may well emerge in the future that either generalizes or dispenses with (4). In that case, some or all of our three strategies may be deemed theoretically illegitimate in the context of the new theory.

To conclude, we propose that the idealized investment firm of the future will be a four-story edifice. On the first three floors, three teams of specialists will apply the three respective strategies we have identified for exploiting opportunities for enhanced performance. On the top floor, management will piece all their insights together into a strategic whole. Moreover, the firm *will* be justified in charging a fee for attempting to outperform the market along these lines, since its research strategies are demonstrably consistent with the best of economic theory.

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