# The Fallacies of Financial Economics: An Essay in Honor of James R. Thompson

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## Introduction

Over the past 50 years, the late Professor M.C. Findlay (1944-2008) and I provided a critique of the intellectual path taken by financial economists. Professor J.R. Thompson joined us in the task more recently. Some of the fruits of these collaborations are provide in this paper.

Prior to the 1950's, research in finance was essentially taxonomic and descriptive. There was only one really academic journal at the time (the *Journal of Finance*) although occasionally the economics journals (*American Economic Review, Journal of Political Economy,* etc.) and the *Journal of Business* carried articles that dealt with financial topics. The only quasi-academic practitioner journal was the *Financial Analysts Journal*. The leading textbooks were heavily institutional and dealt with subjects such as bond indenture agreements, legal claims in the event of bankruptcy, etc.

It was assumed that research in finance was supposed to describe practical phenomenon (stocks, bonds, credit arrangements and the like). It never occurred to most financial writers (the term "theorist" would have been out of place) to propose elaborate theoretical constructs. How finance fit into the grand scheme of the overall functioning of an economy was left to … economists! The finance function (the nature of corporations and how they raise money) was merely described by finance practitioners and even finance professors. Why corporations exist and any sort of notion about an equilibrating mechanism was just not the purview of finance, although as we shall see some rudimentary propositions were put forth even early on.

Finance as an area of formal study was initially defined as the financing of corporations through the issuance of debt and equity securities. The subject was divided into two sub-areas: corporate finance (the study of the issuance of securities) and investments (the study of the purchase of securities). The general reasoning was that markets are best understood as institutional arrangements. The pricing of securities was accepted as inefficient by definition. A list of the earliest texts would include (in order of date published): Ripley, *Railroads: Finance and Organizations,* 1915; Dewing, *The Financial Policy of Corporations,* 1919; Dewing, *Corporation Finance*, 1922. Mead, *Corporation Finance,* 1923; Gerstenberg, *Financial Organizations,* 1933; Graham and Dodd, *Security Analysis*, 1934; and Shaffner, *The Problem of Investment,* 1936. Of these, the Graham and Dodd book became an investment classic and was carried into editions well past 1960. It was truly the "bible" for securities analysts for generations, and its principal author (Ben Graham, a finance professor at Columbia University) was a teacher and mentor of the most successful of all modern investors, Warren Buffett.

Predicates of this literature were books arguing that investment decisions were often based on irrationality. Among the more well-known was Charles Mackay's 1869 classic *Extraordinary Popular Delusions and the Madness of Crowds*. In 1931, F. A. Allen wrote *Only Yesterday*, a great little book about speculation in the 1920s. Keynes' (1936) *General Theory,* particularly chapter 12, fits into this category as well although his endeavor had a very different overall purpose. Excellent quotations from these volumes are found in Thompson, Williams, and Findlay, *Models for Investors in Real World Markets* (2003) and will not be repeated here.

Methodologically, even most economists (say, prior to the mid-20th century) wrote in prose. To be sure, there were those like Walras, Pareto, and even Marshall who used diagrams and equations, but the economists who were interested in financial subjects (the Austrians, for example) did their analysis in English (or German, as the case might be). Even economists like Frank Knight and Keynes rarely used more than simple equations and diagrams. Keynes was a very adept statistician, but he felt much of the phenomena economists dealt with could not be even grossly quantified. This was the case *because* the real world of economics is essentially uncertain (in the sense used by Knight and Keynes as discussed below) and not because, as is assumed by econometricians today, that Keynes lacked the computing facility to analyze large data sets.

By the 1950s, papers began appearing in the finance literature that were no longer essentially descriptive and taxonomic, and the subject matter became broader. The initial Modigliani and Miller paper (1958), which appeared not in a finance journal but the *American Economic Review*, dealt with capital structure issues. The subject was long considered in finance, and even Professor Ezra Solomon's subsequent book (1963) used the term "optimal capital structure" as it had been known in the finance literature for some years. J. B. Williams reviewed the "Law of Conservation of Investment Value" in 1938, discussed its implications and discarded it in a paragraph. But Modigliani and Miller (MM) introduced an "arbitrage proof" and suddenly the subject rose to scientific heights unimagined by Williams, Solomon, and others. The MM "proof" consisted of showing that, if two firms had the same cash flow streams but different values due to capital structure differences, further profitable incentives to trade would exist until the values "equilibrated" (were the same). They even offered "empirical evidence" to support this "hypothesis."

This is not the place to rehash that debate, but let it be said that on both theoretical and methodological grounds most professors and almost all practitioners at the time thought MM were wrong due to the lack of consideration of different treatments of interest and dividend streams in corporate income tax law. MM admitted such in a subsequent *mea culpa* article which revealed how little the two economists really knew about such institutional arrangements as taxation and transaction costs. Somehow the fact that their "empirical evidence" was improperly analyzed was ignored by all but "nitpickers" like Professor Solomon of the Stanford Business School (1963). Nevertheless, the stage was set for a major transformation in the way finance was studied.

Also by the 1950s, the difference between theory and method became blurred. As we observed above, the earliest finance articles and books were taxonomic and descriptive, but they also contained some theoretical propositions. Books with titles like *Principles of Corporation Finance* made some pretense to have a theoretical basis. This is certainly true if we define "theoretical" as propositions about the way corporations (and financial markets) behave. These propositions were often not testable in a purely scientific way, but they were analytical statements nonetheless. The proof was usually casual (not using statistical data testing) empirical observation. Most writers at the time did not think "scientific" testing of these propositions could be accomplished (as perhaps one might do in the real sciences like physics or chemistry). These were still analytical, theoretical propositions nonetheless. After all, Keynes wrote a "general theory" which was quite analytical and which depended almost entirely on casual empiricism. Moreover, he wrote it almost completely in English!

Perhaps the real manifestation of the mid-life crisis was Markowitz' original paper (1952a). This effort was laden with diagrams and equations and looked really out of place along with the other articles in that issue of the *Journal of Finance*. Even Milton Friedman thought Markowitz' dissertation was really mathematical programming (operations research was the term used then) and not economics (and certainly not finance). Nevertheless, students of finance at the "better" universities were required to read the paper, although most professors had no idea how to teach it. The general instruction was, "Read it and we will discuss it later." For most of us (students at the time) later was well into Ph.D. programs where our professors still didn't know what to make of it.

By this time it was increasingly assumed that the methods employed by the physical sciences were appropriate for the social "sciences" (the term "social studies" being replaced by "social sciences" in most universities). Also, the use of another "more rigorous" language, that of diagrams and equations, rather than English, became the hallmark of "scientific" papers. The use of ever more complex mathematical formula (many borrowed from the physical sciences and disguised as financial economics) was defined eventually *de facto* as the only real theoretical analysis. The usual procedure (even today) was to start out with a literature search (going back no more than a few years generally), propose an elegant (read: mathematical) theoretical structure, do "rigorous" empirical analysis, and then conclude with "stories" which purport to tie the arguments together.

The Gordon/Howell Report and Pearson Report on the lack of intellectual content of business school higher education (deemed "trade schools" by many in the 1950s) played a major role in this transformation. Thompson, Williams, and Findlay, (2003, pp. 112-113) provide a more complete discussion of this topic. It should be noted at this juncture (1950s) that the purview of finance expanded to the use, as well as the raising, of capital (capital budgeting, working capital management, etc.). Textbooks dropped the titles of "Corporation Finance" or some such equivalent in favor of names like "Financial Management" or "Managerial Finance." The general reasoning was that financial markets may be analyzed using crude models and equilibrating techniques. The pricing of financial assets was implicitly deemed to be efficient.

## Let's Take a Random Walk

The next major development in our short history was the appearance of the "random walk" hypothesis. Over a five year period beginning in 1959, a number of articles appeared. All of this started with a modest paper by Harry Roberts who was a statistician at the University of Chicago at the time. Roberts assumed stock price changes conformed to a normal distribution, made random selections from such a distribution, added the results to an arbitrary starting price and noted the results looked a lot like the DJIA over time. From this he cautioned that stock price "patterns" (assumed by "chartists" to predict stock prices) might be nothing more than the analysis of noise. The process is described at length in Thompson, Williams, and Findlay, (2003) pp. 120-121. Note that the line of reasoning actually goes back beyond Roberts to Bachalier's 1900 doctoral dissertation and Holbrook Working's research on commodity prices in the 1920s.

At this point, (at least) two problems arose: The first had to do with the conversion of price patterns to returns (changes in prices plus dividends). Return distributions appeared to have tails "too fat" to be normal (e.g., Stable Paretian, with characteristic exponent $\approx 1.7$ rather than 2). The second had to do with the economic fact of earnings retentions. Unless corporate managers were complete idiots and consistently earned nothing from re-invested (in the business) earnings, one would expect stock prices to rise over time. These technical problems were solved by weakening the returns assumption to a martingale with a constant (but not necessarily zero) mean and a symmetric (but not necessarily normal) distribution. Price was assumed to follow a sub-martingale (with a constant or rising mean).

The random walk literature was targeted at discrediting what is still called "technical analysis." The latter assumes one can "beat the market" by charting movements in stock prices. It is short term in nature and depends far more on variances and serial correlation than whether the underlying mean of stock price distributions is zero or modestly positive. The real importance of the random walk literature, however, was to set the stage for the emergence of "financial economics" and the efficient markets hypothesis (EMH).

The forms of the efficient market hypothesis were originally outlined by Williams and Findlay (1974) and later cataloged by Thompson, Williams, and Findlay (2003, pp. 120-131). The forms were divided into three categories: 1) Weak form efficiency (basically the random walk hypothesis that stock price movements cannot be used to predict future price movements), 2) Semi-strong efficiency (new information is rapidly impounded into prices), and 3) Strong form efficiency (no one can "beat the market" consistently and over long periods of time). By the time the geometric Brownian random walk hypothesis appeared as the weak form of the EMH, the game was basically over as far as financial researchers were concerned. After 1970, most academics had convinced themselves that there were no exploitable patterns in stock price time series, and little further work was done. This is unfortunate since "it is not clear that the pessimism of the EMH school concerning the ability of enlightened technician to beat random strategies is justified" (Thompson, Williams, and Findlay (2003), p.123).

Semi-strong efficiency advocates produced papers that appeared to offer empirical proof that "the U.S. securities markets are highly efficient, impounding relevant information about investment values into security prices quickly and accurately" (from Sharp's investments textbook circa 2000). This conclusion was based on numerous "events studies" the elements of which are described in Thompson, Williams, and Findlay (2003, pp. 123-124). Semi-strong efficiency formed the core of the EMH since weak form efficiency was almost universally accepted, and strong form efficiency was never overwhelmingly popular.

The rational for strong form efficiency lied in a combination of the semi-strong tests and the fact that a great many supposedly smart people are engaged in the securities business. It was argued that with so many people and so much information available, there should be few "bargains" (or "overpriced" securities) to buy (sell short). Numerous studies of mutual funds demonstrated that their average performance was, if anything, below that of the market as a whole. It also seemed to be true that past performance was no guide to future performance. It was concluded that investors could do as well picking securities at random than buying a mutual fund. Since managers of funds were deemed to have as much analytical skill and information as anyone, it was concluded that no one could "beat the market" over the long-run. By this time in history, it should be noted, the purview of finance expanded yet further to include analysis of stock prices movements and returns. The general reasoning was that markets may be analyzed using statistical techniques. Also, the pricing of securities was implicitly assumed to be efficient.

## The Capital Asset Pricing Model or CAPM

While the EMH was being developed, other writers (e.g., Treynor (1961), Sharpe (1964), Lintner (1965), and Mossin (1966)) carried Markowitz' work further. They assumed all investors to be essentially the same. Investors were deemed to possess the same information, have access to the same securities, and be able to borrow or lend at the same riskless rate. [Note: Criticism of these assumptions was generally met with a citation to Professor Friedman's (1953) essay]. It then followed that everyone would perceive the same efficient frontier and (by virtue of a separation mechanism) hold the same portfolio on it. It was then demonstrated that, in equilibrium, the only portfolio everyone could hold would be a microcosm of the market (called the "market portfolio" and often reflected by a broad index such as the S&P 500). The market portfolio would then be at the point of tangency with the efficient frontier of a ray emanating from the riskless rate, creating the Capital Market Line (CML).

This line of reasoning became the basis for the Sharpe Ratio (1966) of portfolio performance which is still used by many practitioners (although virtually none of them know the assumptions underlying the model or its limitations, which are numerous). Individual securities were assumed to be priced in terms of their non-diversifiable (also called systematic) risk on the Capital Asset Pricing Line (CAPL). These procedures also formed the basis for the Jensen (1969) and Treynor (1965) measures of performance (less accepted among practitioners).

Skeptics raised the following question: How is the conclusion that all the shares fall on the CAPL enforced? "If everybody is in all essential ways the same (often called the homogeneity assumption) and knows this, the result will be obtained. Otherwise, to enforce the result by 'arbitrage,' we not only need a perfect short sale mechanism but also the agreement that a long (short) position in an underpriced (overpriced) stock and a short (long) position in a portfolio with the same beta is, in fact, riskless.[1] Recognizing the non-systematic risk of the former, even advocates quickly talk about many such positions, each small, to invoke the Law of Large Numbers (similar to the tales told for option pricing models if share prices are allowed to have large jumps and discontinuities).[2]" (Findlay, Williams, and Thompson (2003), p. 94).

It was further observed, "With 'arbitrage' defined as simultaneous long and short holdings of 'equivalent' positions, 'equivalent' has come to be viewed in statistical terms (mean, variance and correlation). If historical data exist, they are often employed (as in stocks' beta coefficients). If no data exist, subjective estimates of return distributions and correlations (e.g., as in Bayesian analysis) are made. Future events are assumed to be subject to probability distributions that, in turn, are assumed to be subject to estimation.

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[1] Although the single factor CAPM has apparently been rejected in the academic literature, the remaining candidates are multiple factor models where these same conditions apply to multiple betas.

[2] Otherwise, derivative pricing models do envisage transactions closer to true arbitrage and allow a return (albeit riskless) on net investment.

More complex models provide distributions of distributions, and the real world has essentially been assumed to be one of risk where the forces of ignorance and darkness are overcome by the assumption of probability distributions." (Findlay and Williams (2000-2001), pp. 186-187). It was also noted that, "In such a world, 'making the right decision' involves picking the alternative with the most favorable probability distribution of outcomes. Hence, not only can people know they 'made the right decision,' but they can know this *ex-ante*. To the extent the actual outcome exceeds (falls short of) the expected return of the alternative selected, one becomes "lucky" ("unlucky")." (Findlay, Williams, and Thompson (2003), p. 99).

Long Term Capital Management provides an interesting example of the above. As we observed, "When, after only a couple of years of operation, almost all LTCM's equity was lost in August 1998, LTCM's managers blamed their troubles on a 'six sigma' event. This would be six standard deviations from the mean of a (presumably) normal distribution, an event with a probability of occurrence of about one in a billion. That is, the managers made the 'right' decisions but were 'unlucky.' Were they really that unlucky, or are there more things than are dreamt of in their probability distributions – for example, that 29 year Treasuries do not constitute good delivery against a short position in 30 year Treasuries)?" (Findlay, Williams, and Thompson (2003), p. 95).

The CAPM and its successors (arbitrage pricing, APT, etc.) are described in detail in Thompson, Williams, and Findlay (2003) chapter 4. Although the formal CAPM presentation involves a host of fairly unrealistic assumption (see above), the theory makes sense intuitively in that the market probably does price risk in some sort of reasonable way. Even the old institutionalists believed T-bills carried the lowest risk (and hence the lowest returns), while increasingly risky securities (longer term government bonds, large capitalization corporate bonds, large cap stocks, small cap stocks, etc.) should have higher *ex-ante* yields. Building a "theory" to prove this turned out to be much harder than intuition would have assumed, however. At this point, notice that the finance purview expanded yet again to include an equilibrium theory of stock prices and microeconomic notions of market efficiency. The general reasoning was that markets may be analyzed using standard microeconomic techniques. Pricing was (is) explicitly assumed (proven) to be efficient.

## The Joint Hypothesis and Anomalies

During the 1970s and 1980s, M.C. Findlay and I began to sense that finance had gone down the wrong path in its attempt to become a "science." We began to publish a series of papers and two books that argued such. Our textbook [Williams and Findlay (1974)] adopted a Graham and Dodd approach to investments while castigating the EMH for being unrealistic, overly formal, and wrong-headed. It gave analytical treatment to modern portfolio theory *a la* Markowitz, but we began the chapter on "Capital Market Theory" with a quotation from Aesop: "We can easily represent things as we wish them to be." We accepted the weak form of the EMH but vehemently rejected the semi-strong and strong forms (to the chagrin of our publisher, Prentice-Hall, who felt our positions would harm the sale of the book – they were right, of course).

By 1974, the "true believers" dominated academic circles, and not accepting the EMH was a death warrant for papers sent to the "respectable" journals. Literally hundreds of papers "not disproving" the EHM were published in these journals, and almost none was accepted adopting the contrary view. Indeed, it was a commentary on how "unscientific" the review process had become that papers inverted the "scientific method" to get published, as we pointed out again more recently. (Thompson, Williams, and Findlay (2003), pp. 125-126).

Findlay and I coined the term "The New Finance" in an article published in 1980. The arguments in this piece and many more (see references) with several other co-authors attacked the very structure of research methodology in finance. In the present endeavor, I am summarizing some of these arguments, but an exhaustive discussion would make it appear that our contributions to the literature were greater than they were. Most of these efforts appeared in heterodox journals considered secondary by most unbiased sources. However, our views set the stage for the so-called "anomalies" literature and a further critique (see below). The 1980 paper was widely read (among the twenty-five most cited through 1995 by the editors of the publishing journal), and it appeared in several readings books for awhile. Alas, it was much ado about nothing, and even books began using our term ("new finance") and our arguments without even citation [cf. Haugen (1999)].

At the time, our 1980 article was only speculating about how corrupt the research agenda had become. As it turns out, the so-called "Climategate" scandal that developed at the University of East Anglia Climate research Unit a few years ago is mild by comparison. We now have knowledge about some of the behind-the-scenes chicanery going on at the University of Chicago while we were preparing our 1980 paper. A great quotation is found in Thompson et al. (2003, p. 119), but a teaser includes the following statement from a young assistant professor who was at Chicago at the time: "I could hardly believe my ears. Here were six scientists openly hoping to find no departure from ignorance. I couldn't hold my tongue, and blurted out, 'I sure am glad you are all keeping an open mind about your research.'" The conclusions Professor Findlay and I reached after three decades of research: Perhaps the purview of finance had expanded too far. Our general reasoning: Markets cannot be analyzed using standard microeconomic techniques, and asset pricing is explicitly inefficient.

Twenty years ago, problems could be identified with the "joint hypothesis" of efficiency (i.e., the semi-strong efficient markets hypothesis, or EMH) and equilibrium (e.g., the capital asset pricing model, or CAPM). Even back then, the growing anomalies literature created problems for the CAPM, and the consensus was that the arbitrage pricing model (APM) would soon replace it. Subsequently, the CAPM was abandoned by practitioners (even those with MBAs from Chicago) and seriously questioned in the academic literature. The APM never achieved its expected lofty aims either, and Fama-French (1992, 1996) took two major anomalies (i.e., the small firm and price to book value phenomena), declared them to be risk, and inserted them into what they claim to be a new equilibrium model.

EMH testing has gone on to short interval events studies, which are not as sensitive to a benchmark (at least for measuring statistical significance). Hence, generally accepted theory still implies that an investor without private information (or indeed, any analysis) who buys stocks can expect (in the expected value, or on average, sense) to pay the "right price." That is, the price which will provide the equilibrium expected rate of return is still the ruling price. However, deciding what model might describe this phenomenon remains undetermined at present. "In retrospect, we were all taken for a ride by the joint hypothesis. What actually happened was that the anomalies began to appear; the advocates asserted that a joint equilibrium-efficiency hypothesis was being tested and one could not tell which one was failing (rarely was it noted that both could be failing); and the specific equilibrium model employed was picked as the fall guy." (Findlay, Williams, and Thompson (2003), p. 92).

From this we arrive at the current state of affairs where theorists maintain the efficiency of markets subject to an equilibrium model to be conceived eventually. Put another way, securities prices are still assumed to adjust to news quickly and continue to offer the fair expected return, "with nobody to know what it is." (For those experiencing déjà vu, that phrase was used in one of the promotions in the South Seas Bubble.) Many financial economists today argue that financial markets are NOT efficient and no one really thought they were. My collection of papers rejected solely on the basis of lack of adherence to the EMH offers empirical evidence to the contrary, however, and a recent quotation from an eminent finance professor really lets the cat out of the bag:

> "But is the Efficient Market Hypothesis (EMH) really responsible for the current crisis? The answer is no. The EMH, originally put forth by Eugene Fama of the University of Chicago in the 1960s, states that the price of securities reflect all known information that impacts their value. The hypothesis does not claim that the market price is always right. On the contrary, it implies that the prices in the market are mostly wrong, but at any given moment it is not at all easy to say whether they are too high or too low." (Siegel, 2009, editorial page)

Boy, this is sure useful information, isn't it? Markets are efficient because they take all information into account but they tell us nothing about actual value. Contrast this statement with the textbook observations of Sharpe et al. of only a decade ago where one is led to believe that all this "efficiency" has something to do with real values!

## Arbitrage and Equilibrium

As we have seen, neoclassical financial arbitrage and equilibrium theory go back 50 years. Friedman (1953) argued that rational speculators (who buy low and sell high) are always present. According to him, these speculators drive the market so other behavioral patterns (such as buying high in order to sell at yet higher prices - the old "bigger fool" theory) are ultimately unprofitable and doomed to failure. He concluded that rational speculation is stabilizing and thus drives divergent prices to their equilibrium level. Friedman also provided us with the proposition of "positivism." Since all models are abstractions from reality, he maintained that their validity should be judged by their predictions rather than their descriptive accuracy (the assumptions made). This notion has perhaps done more damage than any other in the history of financial thought as we are discovering year by year (as unexplainable financial bubbles rise, collapse, and rise again).

As we discussed above, Professor Friedman did not think much of Harry Markowitz' work (1952, 1959), yet the two of them (without collusion, I might add) brewed up a stew that set finance on the wrong track (with initial help from MM and ultimately almost the entire Chicago business school faculty). As the providers of the seminal works on the subject, Friedman and Markowitz were able to make choices about definitions and assumptions. Most of these choices have been employed by those who followed and have remained unchanged for half a century (and perhaps unchallenged for probably four decades). In Markowitz' case, he assumed "that security returns could be depicted by an inter-temporally independent normal distribution. A normal distribution can be fully described by its mean (as a measure of central tendency) and variance (as a measure of dispersion). It is further assumed that the mean (as a measure of expected value) is good but that the variance (as a measure of risk) is bad, so that the investment problem involved a trade-off of the one against the other. This is basically the point at which variance of return became the measure of risk in the finance literature." [Findlay, Williams, and Thompson (2003), p. 93].

We continue, "The real insight of Markowitz is that return aggregated but that risk does not. In other words, the expected return (mean) of the portfolio will be a dollar weighted average of the returns (means) of the individual securities but that the risk (variance) of the portfolio will only be such an average in the limiting case where all of the securities' returns are perfectly correlated with each other. In all other cases, it will be lower. Thus it could be said that Markowitz provided a statistical definition of the benefits of diversification. If the securities are uncorrelated with each other, a large number of small investments will produce a near riskless portfolio of investments (even though the investments are individually risky). Markowitz then defines another concept of *efficiency* as that portfolio that has the highest return for a given level of risk, or, equivalently, the lowest risk for a given return. The set of efficient portfolios was called the efficient frontier, and it was argued that all rational, risk averse investors would select a portfolio from that set. A more explicit assumption of a world of risk (as Markowitz would be the first to grant) would be hard to imagine." [Findlay, Williams, and Thompson (2003), p. 93].

For many years, arbitrage had a clear meaning: the simultaneous purchase and sale of the same commodity at different prices so as to earn an instantaneous profit making no investment and taking no risk. Without time, investment, or risk, arbitrage could be conducted on a massive scale such that a single individual's trading could eliminate any price discrepancies. A key feature of arbitrage (which made it riskless) is that the commodity bought constituted good delivery against the simultaneous sale. Both positive investment and time have been added back to the meaning of arbitrage in the new world of modern financial economics. If the correlation between the positions is less than perfect, we have also added back risk. In sum, MM (see above) began the process of gutting meaning from the term "arbitrage," so that now Wall Street discusses “risk arbitrage” (a concept which would have been an oxymoron 50 years ago).

We pointed out that “The positive investment problem with arbitrage proofs of this sort has, over the years, generally been met by another assumption: a perfect short sale mechanism, which assumes that proceeds from the short sale are available for investment by the seller. Hence, any security expected to underperform (even if the expected return were positive) could be profitably sold short when the proceeds were placed in an identical security of average performance. In real life, of course, the short sale mechanism is far from perfect and identical firms are hard to find.” (Findlay, Williams, and Thompson (2003), p. 94.)

In 1961, MM proposed that, absent taxes, dividend policy was also irrelevant. Using similar proofs and constructs of their 1958 paper, they demonstrated that the ex-dividend price decline once a declared dividend had been paid would offset the value of the dividend (holding investment policy constant). In this demonstration, MM introduced a new concept to the finance literature: Symmetric market rationality (SMR). This assumes not only that all market participants are Friedman’s rational speculator but further that everyone in the stock market assumes everyone else is rational. Or, not only do bigger fools not exist but also nobody believes they do. Hence, nobody would buy when the price is “too high” or on momentum, and bubbles cannot exist.

It is hard to believe anyone took this argument seriously, let alone learned professors of finance. Of course, we have more history to go on today than MM had in 1961, but bubbles existed long before then and continue to do so, as the 2008-2009 collapse has demonstrated. MM themselves granted almost as much when they stated, “Symmetric market rationality cannot be deduced from individual rational behavior in the usual sense since that sense does not imply imputing rationality to others. It may, in fact, imply a choice behavior inconsistent with imputed rationality unless the individual actually believes the market to be symmetrically rational. For if an ordinarily rational investor had good reason to believe that other investors would not behave rationally, then it might well be rational for him to adopt a strategy he would otherwise have rejected as irrational.” (1961, p. 428). In one form or another, this assumption has persisted in models of the capital markets for more than 50 years. Most recently it has been embedded in “transversality conditions” which basically do not admit behavior that would cause the models to blowup. If the choice is between preserving the models or depicting reality, the prevalent philosophy has been to protect the models.

## What is the Real Nature of Risk and Uncertainty?

As we previously maintained, "In an uncertain world, future returns of available investment alternatives cannot be described by probability distributions, much less parameters of such distributions (e.g., mean, variance). Hence there would be no need for investor choice criteria expressed in such terms. With the substitution of risk for uncertainty and the push for equilibrium models, such a need arose and was rather quickly satisfied. Underlying all of this discussion is the notion that there is a desirability of income or wealth (from the days of Bentham generally called 'utility') which may not be proportional to value. It is generally assumed to rise with wealth, but not as rapidly. Hence, winning $100 will not provide as much pleasure as losing $100 will provide pain. It is this feature (i.e., diminishing marginal utility) which produces risk aversion. While diminishing marginal utility may indeed apply to most goods individually at some time (and gives rise to downward-sloping demand curves), it is not as clear that it might apply to the demand for all goods together over time ( or to wealth) or that consumption opportunities are the only desirable attributes of wealth.

"The organized study of risk preferences in the economics literature appears to date from von Neumann and Morgenstern (1944), which prompted the rediscovery of Bernoulli's analysis of the St. Petersburg paradox in 1738. Von Neumann and Morgenstern formalized the axioms of expected utility analysis, leading to the proposition that the utility of a risky event is not the utility of the expected value of the event but rather the expected value of the utility of each outcome of the event. These points are made more accessible to the less technical reader by Friedman and Savage (1948). In an effort to explain people who bought both insurance and lottery tickets, they also proposed a utility function with multiple inflection points. A function with even more inflection points is proposed by Markowitz (1952b). These non-monotonic functions came and went rather quickly a half century ago, so that subsequently such functions have been invariably assumed to be monotonic.

"Markowitz (1959) devotes extensive space to utility-maximizing portfolio selection (including a discussion of the dynamic multi-period selection problem). The first actual graph we can find that graphs alternative portfolios are against indifference curves (all points having the same expected utility), where the optimal solution is the point of tangency of the opportunity set with the highest indifference curve, is in Tobin (1958).[3]

"Several points are worth noting here. To derive an individual's utility function (from which indifference curves can then be extracted) it is necessary to ask lottery-type questions where all the probabilities and payoffs are specified (so that the expected value of the utility of each outcome can be computed). Hence, this procedure can only apply to a world of risk. Further, the monotonic functions do not explain gambling, one of the few types of pure risk situations people encounter. The earlier functions which did account for gambling generate indifference curves which may not provide unique tangency solutions with efficient frontiers. It should not be surprising that researchers came to simply assume that people had utility functions with specific mathematical properties." [Findlay, Williams, and Thompson (2003), p. 95].

Continuing along these lines, we wrote the following in an attempt to see how market equilibrium might be enforced: "The most elegant explanation, which also encompassed the real capital markets, is still the one provided by Fama and Miller (1972). Using all of the assumptions discussed under the CAPM (specifically unlimited borrowing and lending at the riskless rate), they observe that any set of monotonic, risk averse indifference curves will achieve the highest point of tangency with the CML rather than the efficient frontier itself. People who are more risk-averse will have the tangency between the riskless rate and the market portfolio, and will hold a combination of the latter and riskless securities (i.e., they will be lenders). Those who are less risk-averse will have the tangency beyond the market portfolio and thus will buy it on margin. Those with the tangency at the market portfolio will buy it and neither borrow or lend. At this point, some investors might wish to borrow and others might wish to lend. The simpler models treated the riskless rate as exogenous, and hence the intercept of the CML as constant (although, in principle, it could also be adjustable as, for example, a zero beta portfolio). Share prices, the efficient frontier, and the resulting market portfolio adjust until supply equals demand in the loan market, which produces equilibrium.

"The indifference curves of all investors are tangent to the CML at this equilibrium point, so that the slope of the CML provides everyone's marginal rate of substitution of risk for return (and is

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[3] In that uncertainty was at the center of Keynes' views on liquidity preference, it was less than satisfactory to some that Tobin sought to deal with the issue as a portfolio allocation problem in a world of risk.

thus the market price of risk). The difference between the riskless rate and the expected return on the market portfolio is the premium for holding equities.

"This is an elegant theory, even though virtually nobody (not to mention everybody) holds the portfolios it implies (Ed. Comment: "Positivism" injected again at this point). By placing the historical excess returns of equity over debt (the empirical equity premium) in risk-return space, it is implied that the excess is expected (in both the ongoing and unbiased estimator sense) and (is) a risk premium. By connecting it to the riskless rate with a line (CML), the risk premium is implied to be linear to risk over the entire market price of risk.

"Estimates of the equity premium are not low. Depending on the proxies and the period employed, they range from 600-900 basis points. This has puzzled researchers, and the claim that it should be no more than 100 basis points (and possibly negative) was the basis for the Dow Jones at 30,000 or even 40,000 articles. See, for example, Glassman and Hassett (1999) who argued that 'Our calculations show that a perfectly reasonable level for the Dow would be 36,000 – tomorrow, not 10 or 20 years from now.' Such a comment justified the ever higher prices paid by investors in the late 1990s. When interviewed by the press, investors thought they were participating in a continuation of the 25% returns. As we ultimately learned, the resulting 'equilibrium' proved to be temporary (with the crash of 2000).

"Further, all stocks are not equally risky. If there is the great premium between stocks and debt, one might expect to see some sort of premium across stocks. Yet, for at least 30 years and using either return variance or beta, no compelling evidence has been found. As discussed above, Fama and French (1992) threw in the towel on these measures a decade ago. Haugen (1999) argues for an *inverse* relationship of risk and return across stocks. Basically, the market price of risk between debt and stock has eluded detection within the stock market itself." [Findlay, Williams, and Thompson (2003), p. 96].

Continuing our argument, "Given the perfect integration of the one-period debt market and the infinite-horizon stock market assumed by the CML, one may observe that it is still intellectually acceptable to treat the debt market itself as segmented by maturity (see, for example, Bodie, Kane, and Marcus, 1999). Indeed, all of the term structure theories except pure expectations assume some form of segmentation. These range from a complete segmentation (where each maturity level has its own suppliers and demanders, who will not move), to where borrowers and lenders have a preferred habitat and require a premium to move in either direction, to liquidity preference (where lenders prefer short maturities, while borrowers prefer long).

"A rising yield curve could imply anything from expected interest rate increases (under pure expectations) to a term premium (under liquidity preference) to differing supply-demand conditions in the sub-markets (under the others). Without further information, it would be rash to call this a risk premium for lending long. If the equity premium is compensation, it may not (all) be for bearing risk.

"If there is this much ambiguity interpreting a yield curve of Treasuries (*sic*), the true puzzle would appear to be what the equity premium purports to be measuring. Assuming a Treasury of the appropriate maturity to be as close to (nominal) riskless as we can get, consider a corresponding high grade corporate bond. After adjusting for the differing options and the expected value of default loss, any remaining return differential to serve as a risk premium must be close to two orders of magnitude less than the equity premium.

"One response might be that it is easier to estimate the ability to pay a finite, stated interest and principal than to estimate the cash flow to shareholders over an infinite horizon, so a lower premium is appropriate. It is here that the battle is joined. Is it merely that the unbiased estimators are quite imprecise (a variance, or *risk*, issue, such that risk aversion would demand a premium) or that such estimations cannot be made (an expected value, or *uncertainty*, issue)?

"The observation that everyone appears to extrapolate the present and recent past is again met with appeals to positivism (e.g., the market behaves as though it forecasts and performs recursions). The observation that the experience of America in general, and especially since WWII, has been unusually good is the subject of research in the emerging markets literature. The equity premium is only compensation at all if it is expected (i.e., demanded in the pricing as opposed to merely hoped for).

"The traditional neoclassical defense of the equity premium as compensation for risk (rather than, say, windfall) requires more assumptions to get us to the 'return generating process.' Not only does the market behave as though it forecasts and computes unbiased prices, but these only change on new information. Hence, it is 'nature' pulling balls from an urn to generate stock prices. 'Price discovery' is the market fumbling about to see which ball was pulled today. If different balls appear, we conclude that nature has switched urns, which, under the theory, means new information has appeared. The persistence of the premium implies it is compensation for something, and the only thing that the theory allows compensation for in equilibrium (where we are assumed to be) is risk. What could be more obvious?" [Findlay, Williams, and Thompson (2003), p. 97].

A more recent argument was advanced in the literature which is not risk-based. We continue, "After observing that the growth in share prices over the last 50 years exceeds the growth that can be explained by dividends or earnings growth, Fama and French (2002) conclude that much of the measured equity premium over that period is pure windfall. Indeed, they state: 'Our main conclusion is that the average stock return over the last half century is a lot higher than expected' (p. 637). Of course, if the equity premium incorporates unanticipated windfalls, the market price of risk does not measure what it purports to.

"How can a market in equilibrium produce 50 years of windfalls? We are told, 'the high return for 1951-2000 seems to be the result of low expected future returns' (p. 658). Or, we appear to have arrived at the Dow Jones 36000 result with the Dow only at 10000. Thus, the past price appreciation was the result of a market willing to accept ever lower future returns." [Findlay, Williams, and Thompson (2003), p. 97]. Note: As the Dow approached 6500 in 2009, this view seemed even more ridiculous.

Fama-French conclude "that the large spread of capital gains for 1951 to 2000 over dividend and earnings growth is largely due to a decline in the expected stock return" (p. 651). [4] We asked, and I continue to ask: Would everyone who has owned equities over the last 50 years, while consistently (yet unexpectedly) reducing his required rate of return for doing so, please stand and be recognized? Sadly, this *reductio ad adsurdum* is the logical result of the unquestioned adherence to the neoclassical paradigm. [5] It most

[4] This would have the further effect of making the parameters of the (already unobservable) purported equilibrium risk-return relationship time varying.

[5] The Fama-French paper also illustrates the confusion of states of the world we discussed above. "The behavior of dividends and earnings provides little evidence that rationally assessed (i.e., true) long run expected growth is high at the end of the sample period" (p. 646). Observe that an uncertain quantity (average post 2000 growth of dividends and earnings) is reduced to a world of risk ("rationally assessed") and then to certainty ("true") in a single sentence!

likely also explains the 2000 market crash and the collapse of 2007-2009.

How could intelligent people take this seriously? Ask the climatologists at the University of East Anglia. When your entire life has been devoted to a religion and all of your intellectual capital (not to mention your large salary and research grants) depends on perpetuating a myth (blindness, to be kind), how would you react?

## Conclusions

From the above, certain obvious conclusions can be drawn. First, it can be argued that the belief that we live in a near-certain world can lead to numerous errors and that this is the result of (1) substituting risk for uncertainty, (2) making the risk benign (i.e., a mildly-perturbed certainty) in the name of analytical tractability and (3) making acceptance of the model a test of faith. The result is a massive edifice built on a pile of assumptions, presumptions, and ignored evidence. The practical outcomes range from LTCM, to the dot.com bubble to Enron, to the recent market collapse, and heaven knows what else.

Second, we must be concerned with the concept of just what rationality means. As I observed at the beginning of this paper, many early writers grounded their arguments in an assumption that people and markets can be very irrational. Kogan, et. al. (2006, p. 196) have argued: "Most neoclassical asset pricing models rely on the assumption that market participants (traders) are rational in the sense that they behave in ways that are consistent with the objective probabilities of the states of the economy [e.g., Radner (1972) and Lucas (1978)]. In particular, they maximize expected utilities using the true probabilities of uncertain economic states." Rationality has now been defined as behavior consistent with the true probabilities of uncertain states!

Third, empirically, the financial economists have purported to fit statistical (i.e., probabilistic) models to uncertainty. Campbell, et. al. (1997, Ch. 1, p. 3) state: "What distinguishes financial economics is the central role that uncertainty plays in both financial theory and its empirical implementation. The starting point for every financial model is the uncertainty facing investors, and the substance of every financial model involves the impact of uncertainty on the behavior of investors and, ultimately, on market prices…The random fluctuations that require the use of statistical theory to estimate and test financial models are intimately related to the uncertainty on which those models are based." Observe that the word "risk" is then employed to depict deviations from the expectation of the probability distributions, and "risk premium" describes the excess return demanded to hold assets with such deviations. The "premium" reflects the return in excess of that offered by a certainty (in an "uncertain" world) or demanded by a risk neutral investor.

Fourth, the more mathematical approaches to financial economic theory have often employed a state-preference (e.g., Arrow-Debreu) approach which simply assumes equilibrium. Campbell (2000, p. 1516) argued: "For roughly the last 20 years, theoretical and empirical developments in asset pricing have taken place within a well-established paradigm. This paradigm emphasizes the structure placed on financial asset returns by the assumption that asset markets do not permit the presence of arbitrage opportunities—loosely, opportunities to make riskless profits on an arbitrarily large scale. In the absence of arbitrage opportunities, there exists a 'stochastic discount factor' that relates payoffs to market prices for all assets in the economy. This can be understood as an application of the Arrow-Debreu model of general equilibrium to financial markets. A state price exists for each state of nature at each date, and the market price of any financial asset is just the sum of its possible future payoffs, weighted by the appropriate state prices." He concluded, "…there seem to be no really appealing choices for processes to describe infinitely-lived asset price levels under rational expectations. Convergence to a fixed price has never been witnessed but non-stationarity seems equally bizarre and implausible. If neither condition is deemed acceptable, the alternative is irrational expectations. In that case, anything is possible." I would agree: Anything is possible in the real world, the one financial economists simply do not inhabit.

Fifth, the behavioral finance approach, which is simply the neoclassical view with biases (e.g., overshoot, undershoot, framing, etc.) added, fares no better [see Shiller (2005)]. If the neoclassical model is correct, then (as Fama has argued) systematic biases should be exploited and eliminated over time and unsystematic biases merely add to noise. If the contentions above are correct, then the behavioral jump-off and convergence points with the (non-existent) neoclassical equilibrium do not exist.

This leaves us with a rather uncomfortable conclusion: Most of the work of the past 50 years has been going up a blind alley. We really do not know much more than we did then. True, work has been provided for financial economists (who earn north of $200K a year in many universities and millions on Wall Street), but it is not clear that all the money spent on research has produced much of a return. How much should one pay for the information that markets are generally in equilibrium (except when they are not) and that we really cannot predict the future? (Or worse, that the future has already been determined forever and is impounded in asset prices.)

Findlay and I concluded over the years as we watched this misadventure unravel that perhaps finance should have been content to study bond indentures and the like and not have ventured elsewhere. At least students would have some practical information to draw upon. Perhaps finance with a more modest agenda (and less arrogance and hubris) might have produced more solid thinking over the past five decades. Unfortunately, many people who knew a lot about finance were not able to participate in the discussion because of this misplaced arrogance and hubris. People like M.C. Findlay had a lot to say, but they simply could not shout over all the noise created by people who really knew very little about the subject. Having a good background in physics and trying to replace real-world finance with make-believe notions about how the world might look given unrealistic assumptions made many a reputation. Sadly, it wasted much time and resources and gave us conditions that caused panics and unnecessary financial catastrophe.

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