

PAUL SAMUELSON AND FINANCIAL ECONOMICS

by Robert C. Merton*

Introduction

It has been well said that Paul A. Samuelson is the last great general economist—never again will any one person make such foundational contributions to so many distinct areas of economics. His profound theoretical contributions over nearly seven decades of published research have been ecumenical and his ramified influence on the whole of economics has led economists in just about every branch of economics to claim him as one of their own. I am delighted to take part in this celebration of his Ninetieth Birthday.

This Volume provides a special opportunity to honor this universal man of economics as he enters his 10th decade. On such Festschrift occasions, the common practice is to write a substantive piece building upon the honoree's work. However, here I try my hand at a different format: synthesizing Samuelson's work in financial economics itself. As everyone knows, Paul Samuelson is his own best synthesizer and critic, and so the format as executed will only be at best second-best.¹ Synthesis, we know, involves abstraction from the complex original. With Samuelson, we must be severely selective since even with confinement to a single branch of economics, the wide-ranging scope and unflagging volume of his researches allows only a few elements of the work to be examined. Within that brute reality, I limit my discussion to just three of his chief contributions to the field of financial economics: 1) The Efficient Market Hypothesis; 2) Warrant and option pricing; and 3) Investing for the long run.

Happily, I had the great good fortune to explore this same synthesizing theme in print nearly a quarter century ago [Merton, 1983], covering early major contributions of Samuelson a number of which are not discussed here, such as expected utility theory (from reconciling its axioms with non-

stochastic theories of choice to its reconciliation with the ubiquitous and practical mean-variance criterion of choice), the foundations of diversification and optimal portfolio selection when facing fat-tailed, infinite-variance return distributions.² As we shall see, however, it is remarkable how much of Samuelson's early research remains in the mainstream of current financial economic thought decades later, having gained even greater significance to the field with the passage of time.³ Samuelson's discoveries in finance theory, as in economic theory generally, constitute the manifest core of his multiform writings. His accomplishments in both the problem-finding and problem-solving domains of theory are legend. Another, latent but no less deep, theme of Samuelson's writings is trying to divert us away from the paths of error, whether in finance research, private-sector finance practice or public finance policy.

Samuelson's attacks on error are not limited to engagements in the economics arena. He has upon occasion used the life works of other economists to discredit the widely held myth in the history of science that scientific productivity declines after a certain chronological age. The strongest debunking of this ill-founded belief would, of course, have been the self-exemplifying one. While my brief search of the literature produced neither an exact cutoff age where productivity is purported to decline nor whether this decline is to be measured by the flow of research output per unit time or by its rate of change, the data provided by Paul Samuelson's lifetime pattern of contributions are robust in rejecting this proposed result on all counts. Representing twenty-seven years of scientific writing from 1937 to the middle of 1964, the first two volumes of his *Collected Scientific Papers* contain 129 articles and 1772 pages. These were followed by the publication in 1972 of the 897-page third volume, which registers the succeeding seven years' product of seventy-

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eight articles published when he was between the ages of 49 and 56. A mere five years later, at the age of 61, Samuelson had published another eighty-six papers, which fill the 944 pages of the fourth volume. A decade later the fifth volume appeared with 108 articles and 1064 pages. Simple extrapolation (along with a glance at his list of publications since 1986) assures us that the sixth and even a seventh volume cannot be far away.

Nearly a quarter century ago, I presented Paul with a list of his then thirty articles in financial economics and asked him to select his favorite ones, leaving the criteria for choice purposely vague. By the not-so-tacit demanding criterion that was evidently applied, he was drastically selective, choosing only six. I list these below. Four of the six articles appear in journals not on the beaten path of most economists, but happily they are reproduced in Samuelson's collected scientific papers.

Paul Samuelson's 1982 Selection of his favorite financial economics papers

1. "Probability, Utility, and the Independence Axiom," *Econometrica* 20, no. 4, October 1952, pp. 670–678; [1952b, I, Chap. 14].
2. "General Proof that Diversification Pays," *Journal of Financial and Quantitative Analysis* 2, no. 1, March 1967, pp. 1–13; [1967a, III, Chap. 201].
3. "The Fundamental Approximation Theorem of Portfolio Analysis in Terms of Means, Variances, and Higher Moments," *Review of Economic Studies* 37, no. 4, October 1970, pp. 537–542; [1970a, III, Chap. 203].
4. "Stochastic Speculative Price," *Proceedings of the National Academy of Sciences, U.S.A.* 68, no. 2, February 1971, pp. 335–337; [1971a, III, Chap. 206].
5. "Proof that Properly Anticipated Prices Fluctuate Randomly," *Industrial Management Review* 6, no. 2, Spring 1965, pp. 41–49; [1965a, III, Chap. 198].
6. "Rational Theory of Warrant Pricing," *Industrial Management Review* 6, no. 2, Spring 1965, p. 13–39; [1965b, III, Chap. 199].

Perhaps a bit selfishly, we in financial economics are especially thankful that Paul paid no heed to the myth of debilitating age in science. Five of the six

articles he selected in 1982 as his most important papers in our branch of economics and all but six of his more than three-score contributions to our field to date were published after he had reached the age of 50.

Along with his foundational research and important directives on avoiding the paths of error, there are the characteristic Samuelsonian observations in the history of economic science. Samuelson's writings on Smith, Ricardo, Marx and his many essays on the evolution of more contemporary economic thought provide much grist for the mill of the historian of science. But, to focus exclusively on those explicit undertakings in the history of economic science is to miss much. Part of an unmistakable stamp of a Paul Samuelson article is the interjections of anecdotes and stories around and between his substantive derivations, which serve to entertain and enlighten the reader on the developmental chain of thought underlying that substantive analysis.

One happy example in financial economics is Samuelson's brief description in the "Mathematics of Speculative Price" [1972a, IV, Chap. 240, p. 428] of the rediscovery of Bachelier's pioneering work on the pricing of options. In the text, he wrote:

In 1900 a French mathematician, Louis Bachelier, wrote a Sorbonne thesis on the *Theory of Speculation*. This was largely lost in the literature, even though Bachelier does receive occasional citation in standard works on probability. Twenty years ago a circular letter by L. J. Savage (now, sadly, lost to us), asking whether economists had any knowledge or interest in a 1914 popular exposition by Bachelier, led to his being rediscovered. Since the 1900 work deserves an honored place in the physics of Brownian motion as well as in the pioneering of stochastic processes, let me say a few words about the Bachelier Theory.

The footnote elaborates

Since illustrious French geometers almost never die, it is possible that Bachelier still survives in Paris supplementing his professional retirement pension by judicious arbitrage in puts and calls. But my widespread lecturing on him over the last 20 years has not elicited any information on the subject. How much Pioncare, to whom he dedicates the thesis, contributed to it, I have no knowledge.

Finally, as Bachelier's cited life works suggest, he seems to have had something of a one-track mind. But what a track! The rather supercilious references to him, as an unrigorous pioneer in stochastic processes and stimulator of work in that area by more rigorous mathematicians such as Kolmogorov, hardly does Bachelier justice. His methods can hold their own in rigor with the best scientific work of his time, and his fertility was outstanding. Einstein is properly revered for his basic, and independent, discovery of the theory of Brownian motion 5 years after Bachelier. But years ago when I compared the two texts, I formed the judgment (which I have not checked back on) that Bachelier's methods dominated Einstein's in every element of the vector. Thus, the Einstein-Fokker-Planck Fourier equation for diffusion of probabilities is already in Bachelier, along with subtle uses of the now-standard method of reflected images.

In addition to providing the facts on how Bachelier's seminal work found its way into the mainstream of financial economics after more than a half century of obscurity, Samuelson's compact description provides a prime example of multiple and independent discoveries across the fields of physics, mathematics, and economics.⁴ On the issue of allocating the credit due innovative scholars, he also provides an evaluation of the timing and relative quality of the independent discoveries. His mention of Poincaré provides a hint that there may be still more to the complete story. Furthermore, note his signature use of a chain of eponyms, the "Einstein-Fokker-Planck Fourier equation . . .", to compactly remind us of the sequence of scientists to whom we owe credit. And, of course, what economist wouldn't relish this revelation of the great debt owed to this early financial economist by the mathematical physicists and probabilists to be added to the well-known debt owed to Malthus by the Darwinian biologists?

Although most would agree that finance, micro investment theory and much of the economics of uncertainty are within the sphere of modern financial economics, the boundaries of this sphere, like those of other specialties, are both permeable and flexible. It is enough to say here that the core of the subject is the study of the individual behavior of households in the intertemporal allocation of their

resources in an environment of uncertainty and of the role of economic organizations in facilitating these allocations. It is the complexity of the interaction of time and uncertainty that provides intrinsic excitement to study of the subject, and, indeed, the mathematics of financial economics contains some of the most interesting applications of probability and optimization theory. Yet, for all its seemingly obtrusive mathematical complexity, the research has had a direct and significant influence on practice. The impact of efficient market theory, portfolio selection, risk analysis, and option pricing theory on asset management and capital budgeting procedures is evident from even a casual comparison of current practices with, for example, those of the early 1960s when Paul Samuelson was just publishing his early foundational papers in finance.

New financial product and market designs, improved computer and telecommunications technology and advances in the science of finance during the past four decades have led to dramatic and rapid changes in the structure of global financial markets and institutions. The scientific breakthroughs in financial economics in this period both shaped and were shaped by the extraordinary flow of financial innovation, which coincided with those changes. The cumulative impact has significantly affected all of us—as users, producers, or overseers of the financial system.

The extraordinary growth in size and scope of financial markets and financial institutions including the creation of the enormous national mortgage market in the United States were significantly influenced by the models developed in financial economic research. The effects of that research have also been observed in legal proceedings such as appraisal cases, rate of return hearings for regulated industries, and revisions of the "prudent person" laws governing behavior for fiduciaries. Evidence that this influence on practice will continue can be found in the curricula of the best-known schools of management where the fundamental financial research papers (often with their mathematics included) are routinely assigned to MBA students. Although not unique, this conjoining of intrinsic intellectual interest with extrinsic application is a prevailing theme of research in financial economics. Samuelson, once again, did much to establish this theme as a commonplace and to exemplify it in his substantive writings.

It was not always thus. Fifty years ago, before the birth of the economics of uncertainty and before the rediscovery of Bachelier, finance was essentially a collection of anecdotes, rules of thumb, and manipulations of accounting data with an almost exclusive focus on corporate financial management. The most sophisticated technique was discounted value and the central intellectual controversy centered on whether to use present value or internal rate of return to rank corporate investment projects. The subsequent evolution from this conceptual potpourri to a rigorous economic theory subjected to systematic empirical examination was the work of many and, of course, the many included Paul Samuelson.

After this brief overview of Samuelson's multifaceted influence on the ethos of financial economic research, I turn now to that promised discussion of three of his chief contributions to the field.

II. The Efficient Market Hypothesis

A question repeatedly arises in both financial economic theory and practice: When are the market prices of securities traded in capital markets equal to the best estimate of their values? I need hardly point out that if value is defined as "that price at which one can either buy or sell in the market," then the answer is trivially "always." But, of course, the question is rarely, if ever, asked in this tautological sense, although the distinction between value and price is often subtle. Moreover, as the following examples suggest, the answer to this question has important implications for a wide range of financial economic behavior.

In the fundamentalist approach of Graham and Dodd to security analysis, the distinction between value and price is made in terms of the (somewhat vague) notion of intrinsic value. Indeed, the belief that the market price of a security need not always equal its intrinsic value is essential to this approach because it is disparities such as these that provide meaningful content to the classic prescription for successful portfolio management: buy low (when intrinsic value is larger than market price) and sell high (when intrinsic value is smaller than market price).

In appraisal law, the question is phrased in terms of how much weight to give to market price in relation to other non-market measures of value in arriv-

ing at a fair value assessment to compensate those whose property has been involuntarily expropriated. In corporation finance, the answer to that question determines the extent to which corporate managers should rely upon capital market prices as the correct signals for the firm's production and financing decisions.

Characteristically, Samuelson's version provides both a clear distinction between value and price and a focus on the broadest and most important issue raised by this question: When are prices in a decentralized capital market system the best estimate of the corresponding shadow values of an idealized central planner who efficiently allocates society's resources? Thus, in "Mathematics of Speculative Price" [1972a, IV, Chap. 240, p. 425], he wrote:

A question, for theoretical and empirical research and not ideological polemics, is whether real life markets—the Chicago Board of Trade with its grain futures, the London Cocoa market, the New York Stock Exchange, and the less-formally organized markets (as for staple cotton goods), to say nothing of the large Galbraithian corporations possessed of some measure of unilateral economic power—do or do not achieve some degree of dynamic approximation to the idealized "scarcity" or shadow prices. In a well-known passage, Keynes has regarded speculative markets as mere casinos for transferring wealth between the lucky and unlucky. On the other hand, Holbrook Working has produced evidence over a lifetime that futures prices do vibrate randomly around paths that a technocrat might prescribe as optimal. (Thus, years of good crop were followed by heavier carry-over than were years of bad, and this before government intervened in agricultural pricing.)

As we know, such theoretical shadow prices are "prices never seen on land or sea outside of economics libraries." However, testable hypotheses can be derived about the properties that real-life market prices must have if they are to be the best estimate of these idealized values. Because it is intertemporally different rather than spatially different prices that are of central interest in financial economics, most of Samuelson's analyses in this area are developed within the context of a futures market. In his 1957 "Intertemporal Price Equilibrium: A Prologue

to the Theory of Speculation" [1957, II, Chap. 73], however, he does use spatial conditions of competitive pricing as tools to deduce the corresponding conditions on intertemporal prices in a certainty environment. From these local "no-arbitrage conditions," he proves that the current futures price must be equal to the future spot price for that date. In completing his analysis of the price behavior over time, he shows that the dynamics of "allocation-efficient" spot prices can be determined as the formal solution to a particular optimal control problem.⁵

Samuelson underscores his use of the word *Prologue* in the title by pointing out that "A theory of speculative markets under ideal conditions of certainty is Hamlet without the Prince," (p. 970). Indeed, his later papers, "Stochastic Speculative Price" [1971a, III, Chap. 206], "Proof That Properly Anticipated Prices Fluctuate Randomly" [1965a, III, Chap. 198], and "Rational Theory of Warrant Pricing" [1965b, III, Chap. 199], have in common their deriving the stochastic dynamic behavior of prices in properly functioning speculative markets. They also share the distinction of being important papers published in obscure places, which nevertheless found their way into the mainstream. Such occurrences suggest that high visibility of scientific authors may tend to offset low visibility of publication outlets.

Published in the same issue of the *Industrial Management Review*, "Proof That Properly Anticipated Prices Fluctuate Randomly" and "Rational Theory of Warrant Pricing" are perhaps the two most influential Samuelson papers for the field. During the decade before their printed publication in 1965, Samuelson had set down, in an unpublished manuscript, many of the results in these papers and had communicated them in lectures at MIT, Yale, Carnegie, the American Philosophical Society, and elsewhere. The sociologist or historian of science would undoubtedly be able to develop a rich case study of alternative paths for circulating scientific ideas by exploring the impact of this oral publication on research in rational expectations, efficient markets, geometric Brownian motion, and warrant pricing in the period between 1956 and 1965.

In "Proof That Properly Anticipated Prices Fluctuate Randomly," Samuelson provides the foundation of the efficient market theory that Eugene Fama independently and others have further developed

into one of the most important concepts in modern financial economics. As indicated by its title, the principal conclusion of the paper is that in well-informed and competitive speculative markets, the intertemporal changes in prices will be essentially random. In a conversation with Samuelson, he described the reaction (presumably his own as well as that of others) to this conclusion as one of "initial shock-and then, upon reflection, that it is obvious." The time series of changes in most economic variables (GNP, inflation, unemployment, earnings, and even the weather) exhibit cyclical or serial dependencies. Furthermore, in a rational and well-informed capital market, it is reasonable to presume that the prices of common stocks, bonds, and commodity futures depend upon such economic variables. Thus, the shock comes from the seemingly inconsistent conclusion that in such well-functioning markets, the changes in speculative prices should exhibit no serial dependencies. However, once the problem is viewed from the perspective offered in the paper, this seeming inconsistency disappears and all becomes obvious.

Starting from the consideration that in a competitive market, if everyone *knew* that a speculative security was expected to rise in price by more (less) than the required or fair expected rate of return, it would *already* be bid up (down) to negate that possibility. Samuelson postulates that securities will be priced at each point in time so as to yield this fair expected rate of return. Using a backwards-in-time induction argument, he proves that the changes in speculative prices around that fair return will form a martingale. And this follows no matter how much serial dependency there is in the underlying economic variables upon which such speculative prices are formed. Thus,

We would expect people in the market place, in pursuit of avid and intelligent self-interest, to take account of those elements of future events that in a probability sense may be discerned to be casting their shadows before them. (Because past events cast "their" shadows after them, future events can be said to cast their shadows before them.) [1965a, III, Ch. 198, p. 785].

In an informed market, therefore, current speculative prices will already reflect anticipated or forecastable future changes in the underlying economic variables that are relevant to the formation of prices,

and this leaves only the unanticipated or unforecastable changes in these variables as the sole source of fluctuations in speculative prices.

Samuelson is careful to warn the reader against interpreting his conclusions about markets as empirical statements:

You never get something for nothing. From a nonempirical base of axioms, you never get empirical results. Deductive analysis cannot determine whether the empirical properties of the stochastic model I posit come close to resembling the empirical determinants of today's real-world markets. [1965a, III, Ch. 198, p. 783].

Nevertheless, his model is important to the understanding and interpretation of the empirical results observed in real-world markets.

Suppose that one observes that successive price changes are random (as empirically seems to be the case for many speculative markets). Without the benefit of Samuelson's theoretical analysis, one could easily interpret the fact that these prices wander like a drunken sailor as strong evidence in favor of the previously noted Keynes view of speculative markets. Whereas had it been observed that speculative markets were orderly with smooth and systematic intertemporal changes in prices, the corresponding interpretation (again, without Samuelson's analysis) could easily be that such sensible price behavior is (at least) consistent with that of the shadow prices of the idealized rational technocratic planner.

In the light of Samuelson's analysis, we all know that the correct interpretations of these cases are quite the reverse. For speculative market prices to correspond to their theoretical shadow values, they must reflect anticipated future changes in relevant economic variables. Thus, it is at least consistent with equality between these two sets of prices that changes in market prices be random. On the other hand, if changes in speculative prices are smooth and forecastable, then speculators who are quick to react to this known serial dependency and investors who are lucky to be transacting in the right direction will receive wealth transfers from those who are slow to react or who are unlucky enough to be transacting in the wrong direction. More important, under these conditions, current market prices are not the best estimate of values for the purposes of

signaling the optimal intertemporal allocation of resources.

In studying the corpus of his contributions to the efficient market theory, one can only conclude that Paul Samuelson takes great care in what he writes. As is evident throughout his Proof paper and in his later discussion of the topic in "Mathematics of Speculative Price," [1972a, IV, Chap. 240] he is keenly aware of the ever present danger of banalization by those who fail to see the subtle character of the theory. Thus, having proved the general martingale theorem for speculative prices, he concludes

The Theorem is so general that I must confess to having oscillated over the years in my own mind between regarding it as trivially obvious (and almost trivially vacuous) and regarding it as remarkably sweeping. Such perhaps is characteristic of basic results. [1965a, III, Chap. 198, p. 786].

Without Samuelson's careful exposition, the martingale property could easily be seen as either a simple deduction (whose truth follows from the very definition of competitive markets) or as a mere tautology. That is, subtract from any random variable, Y_t , its conditional expectation as of $t-1$, $E_{t-1}[Y_t]$ and as a truism, the sum of the $\{Y_t - E_{t-1}[Y_t]\}$ will form a martingale. Indeed, in discussing the fair expected returns $\{\lambda_t\}$ around which speculative prices should exhibit the martingale property, Samuelson points out that

Unless something useful can be said in advance about the $\{\lambda_{t-1}\}$ – as for example, $\lambda_t - 1$ small, or λ_t a diminishing sequence in function of the diminishing variance to be expected of a futures contract as its horizon shrinks, subject to perhaps a terminal jump in λ_t as closing-date becomes crucial – the whole exercise, becomes an empty tautology. [1972a, IV, Chap. 240, p. 443].

But, of course, such restrictions can be reasonably imposed (using for example, the capital asset pricing model and the term structure of interest rates), and it is these restrictions that form the basis for testing the theory.

Many less precise discussions of the efficient market theory equate the theory with the property that speculative price changes exhibit a random walk around the fair expected return. However, Samuelson clearly distinguishes his derived martin-

gale property from this much stronger one by showing that such changes need not be either independently or identically distributed for the theory to obtain. He is also careful to make the distinction between *speculative* prices that will satisfy the martingale property and *nonspeculative* prices (as well as other economic variables) that need not exhibit this property in a well-functioning market economy. In his "Stochastic Speculative Price" analysis, for example, the optimal stochastic path for the spot price of a commodity is shown not to satisfy the martingale condition for a speculative price. Indeed, only in periods of positive storage when the spot price also serves the function of a speculative price will the expected changes in the spot price provide a fair expected rate of return (including storage costs). "Thus," Samuelson remarks, "Maurice Kendall almost proves too much when he finds negligible serial correlation in spot grain prices" [1965a, III, Chap. 198, p. 783]. I only allude to the import of this message for those in other areas of economics who posit and test models of rational expectations.

In preparing this piece, I found in my files a 25-year-old unpublished manuscript of Samuelson's, "Nonlinear Predictability Though the Spectrum is White," which he had given to me with a kind invitation to once again become his co-author and "revise as seems best." As is clear from the title, Samuelson's intent was to provide a specific and empirically plausible model to underscore his point that "white noise" lack of (linear) serial correlation in stock returns is not sufficient to ensure the non-predictability of those returns. As he describes it

The "efficient markets hypothesis" is sometimes overdramatized by the description that "speculative price behaves like a random walk." More exactly the correct hypothesis is that the speculative price is a *martingale* and therefore, has a *zero* autocorrelation or "white spectrum" with a zero Pearsonian correlation coefficient between price changes in non-overlapping time periods.

Samuelson elaborates on the implications:

It follows from a zero autocorrelation that any "technical" or "chartist" method of prediction that depends on *linear* multiple correlation is doomed to failure. Econometricians commonly test, and often verify, the white-spectrum necessary condition for the efficient-market

hypothesis. This *necessary* condition is *not*, however, *sufficient*. Zero autocorrelation would be equivalent to probabilistic independence (of "excess" returns) if the data were assuredly drawn from multivariate Gaussian distributions. However, for non-Gaussian distributions as with curvilinear functions of Gaussian variates, higher than second-moment tests must also be confirmed. Thus, the whiteness of spectrum with its guarantee of the *impotence of linear multiple regression prediction* is not at all a guarantee that *nonlinear* chartism will fail.

Although the file also contains some mathematical modeling of mine, apparently in anticipated acceptance of his invitation, the paper was neither completed nor circulated. I harbor the hope that with this rediscovery Paul will consider publishing it in full. In the meantime, I sketch out here a simplified version of his central thesis in an example from that modeling.

Let X_t denote the realized return on a stock minus its "fair" expected return between time $t - 1$ and t . If the stock price satisfies the efficient market hypothesis, then the expected excess return on the stock will satisfy the martingale property that

$$E[X_t | X_{t-k}] = 0, \text{ for } k = 1, 2, 3, \dots$$

Suppose however that the process for X_t is given by

$$X_t = a\varepsilon_{t-1}(\varepsilon_{t-1}^2 - b) + \varepsilon_t$$

where the $\{\varepsilon_t\}$ are independently and identically distributed Gaussian random variables with zero mean and variance σ^2 and $a > 0$. Consider the linear serial correlation between the excess return from $t - 1$ to t and the excess return from $t - k - 1$ and $t - k$, given by

$$\begin{aligned} E[X_t X_{t-k}] &= 0 \text{ for } k \geq 2 \text{ and all } a \text{ and } b \\ &= a \left[E(\varepsilon_{t-1}^4) - bE(\varepsilon_{t-1}^2) \right] \text{ for } k = 1 \\ &= a\sigma^2 \left[3\sigma^2 - b \right] \text{ for } k = 1 \end{aligned}$$

If the stock price is efficient with respect to linear combinations of past returns, then we have that $E[X_t X_{t-k}] = 0$ for $k = 1, 2, 3, \dots$ and therefore, $b = 3\sigma^2$. Under that white-spectrum condition, we have that the conditional expected excess return is given by

$$E[X_t | \varepsilon_{t-1}] = a\varepsilon_{t-1} (\varepsilon_{t-1}^2 - 3\sigma^2)$$

By inspection, $E[X_t | \varepsilon_{t-1}] > 0$ and one will earn a greater than fair expected return on the stock, i.e., it is “undervalued” when either $\varepsilon_{t-1} > \sqrt{3}\sigma$ or $-\sqrt{3}\sigma < \varepsilon_{t-1} < 0$ and $E[X_t | \varepsilon_{t-1}] < 0$ and one will earn a less than fair expected return on the stock, i.e., it is “overvalued” when either $0 < \varepsilon_{t-1} < \sqrt{3}\sigma$ or $\varepsilon_{t-1} < -\sqrt{3}\sigma$. Put in terms of the directly observable excess returns, we have that

$$E[X_t | X_{t-1}] = aX_{t-1} [X_{t-1}^2 + 18a^2\sigma^6 - 3\sigma^2],$$

which will not equal 0 in general and thus, the martingale test condition for the Efficient Market Hypothesis fails.⁷

Thus, Samuelson concludes, “Despite the resulting impotence of *linear* prediction, the experienced eye will soon recognize that the example’s white-spectrum series is anything but a random walk, instead being the archetype of a stationary time series that does lend itself to profitable *nonlinear* filtering.” In a characteristically careful clarification, he goes on, “The point of this dramatic example is *not* to deny that numerous people in the marketplace may learn to recognize the predictability structure present in this time series—and, in so learning, may subsequently act to wipe out that structure. The point of the example is to illustrate how weak is the power of a test of mere *unautocorrelation* to appraise the efficiency and predictability of market prices.”⁸

Samuelson not only exercises great theoretical care himself, but he also tries to induce such in his readers. On his derivation of the Efficient Market Hypothesis, he warns, for example, against reading “too much into the established theorem”

It does not prove that actual competitive markets work well. It does not say that speculation is a good thing or that randomness of price changes would be a good thing. It does not prove that anyone who makes money in speculation is *ipso facto* deserving of the gain or even that he has accomplished something good for society or for anyone but himself. All or none of these may be true, but that would require a different investigation [1965a, III, Chap. 198, p. 789].

Samuelson later undertook that investigation (1972b) and demonstrated that uninformed speculators (in later literature, also known as “noise traders”) confer less benefit to society than their losses. In an extension of “Proof” (1973), he

showed that the dynamics of properly discounted present values of assets must also exhibit the same martingale property.

In the last paragraph of “Proof,” Samuelson concludes by raising a number of questions, all of which focus on an issue central to making operational his concept of properly anticipated prices. Namely, where are the basic probability distributions (for which the martingale property of speculative prices applies) to come from? Although he makes no pronouncements on this issue, by identifying it he opened gates to its resolution in the important later work by Fama (1970). Fama defines market efficiency in terms of a hierarchy of information sets that are the basis for forming the probability distributions. He shows that if changes in speculative prices (around their fair expected returns) form a martingale based upon the probability distribution generated by information set Φ , then these price changes will also satisfy the martingale property for the distribution generated by any information set Φ' that is a subset of Φ . It therefore follows that if these prices do not satisfy the martingale property for information set Φ' , they will not satisfy this property for any information set Φ that contains Φ' as a subset. Thus, Fama makes operational Samuelson’s martingale requirement for properly anticipated prices by showing that it is possible to reject the martingale property (and hence, market efficiency) by using only a subset of the information available to any (or for that matter, all) investors. As Fama makes clear in his development of the strong, semi-strong, and weak versions of the efficient market theory, it is also possible for speculative prices to satisfy the martingale conditions for one information set but not to satisfy it for another.

The martingale property of speculative prices is the key element in Fama’s development of procedures for testing market efficiency. Indeed, as Fama points out, virtually all empirical studies of speculative price returns (both pre- and post-“Proof”) can be viewed as tests of this property and that remains the case to this day, which underscores further the significance of Samuelson’s having established it as the crucial one for price behavior in an efficient market.

The early empirical studies focused on tests for serial correlation and comparisons of return performance between buy-and-hold and various simple filter-type trading strategies. While their results

were on the whole consistent with market efficiency, these studies were, by necessity, limited to investigations of small numbers of securities and relatively short observation periods. This perhaps explains why the practicing financial community paid little attention to the results of those studies. However, with the development in the late 1960s of large-scale stock return data bases (principally at the University of Chicago Center for Research in Security Prices) and the availability of high-speed computers, there came an avalanche of tests of the efficient market theory, which were neither limited to a few securities nor to short observation periods.

Using return data on thousands of securities over more than forty years of history, some of the studies extended the earlier work comparing buy-and-hold with various mechanical trading strategies. Others, such as the Jensen (1968) study of mutual fund performance, broke new ground and analyzed the performance of real-life portfolio managers. In collectively echoing the findings of the earlier limited examinations, these large-scale studies put to final rest the myth that professional money managers can beat the market by miles, and indeed, cast doubt on whether they could even beat it by inches.

As the evidence in support of the efficient market theory mounted, the results and their implications for optimal strategy were widely disseminated to both the investing professional and the investing public in popular and semi-popular articles written by a number of academics. Included in this number is Paul Samuelson. With the widespread dissemination of this mountain of accumulated evidence, the practicing financial community could no longer ignore the efficient market theory although, as is perhaps not surprising, few (at least among the money managers in that community) accepted it. Here again, Samuelson exercises great care in his writings on this controversial issue by always keeping clear the distinction between “not rejecting” and “accepting” the efficient market theory. In discussing the controversy between practicing investment managers and academics in “Challenge to Judgment” [1974b, IV, Ch. 243, pp. 479–480], for example, he writes:

Indeed, to reveal my bias, the ball is in the court of the practical men: it is the turn of the Mountain to take a first step toward the theoretical Mohammed, . . . If you oversimplify the debate, it can be put in the form of the question,

Resolved, that the best of money managers cannot be demonstrated to be able to deliver the goods of superior portfolio-selection performance. Any jury that reviews the evidence, and there is a great deal of relevant evidence, must at least come out with the Scottish verdict: Superior investment performance is unproved.

With characteristic clarity, Samuelson provides a constructive perspective on the controversy by pointing out that while the existing evidence does not prove the validity of the efficient market theory, the burden of proof belongs to those who believe it to be invalid. In his final paragraph of “Challenge to Judgment,” (1974b, p. 485), he summarizes the point:

What is interesting is the empirical fact that it is virtually impossible for academic researchers with access to the published records to identify any members of the subset with flair. This fact, although not an inevitable law, is a brute fact. The ball, as I have already noted, is in the court of those who doubt the random walk hypothesis. They can dispose of the uncomfortable brute fact in the only way that any fact is disposed of—by producing brute evidence to the contrary.

Later in the same journal, Samuelson revisits the question of market efficiency in real-world markets measured in terms of possible superior investment performance:

Fifteen years have passed since my “challenge to judgment.” What has been the further testimony of the 1970s and 1980s? What, in sum, is the judgment of 1989 economic science on the challenge to judgment? Broadly speaking, the case for efficient markets is a bit stronger in 1989 than it was in 1974, or in 1953 when Holbrook Working and Maurice Kendall were hypothesizing that stock and commodity price changes are pretty much a random walk (or a white-noise martingale) [1989b, p. 5].

Five years later, he reconfirms his position:

To commemorate this Journal’s fifteen years of success, I reviewed the cogency and accruing empirical verisimilitude of that agnostic questioning of activist judgmental investing.

By and large, the ball that was put in the court of the would-be judgment-mongers never did get returned with point-winning velocity. The jury of history did not find systematic inefficiency that exercisers of judgment could use to achieve excess risk-corrected returns.

We can expect the debate to go on. And that tells you something about the approximate microinefficiency of the organized markets where widely owned securities are traded. [1994, p. 15]

However, Samuelson is discriminating in his assessment of the Efficient Market Hypothesis as it relates to real-world markets. He notes a list of the "... few not-very-significant apparent exceptions" to micro-efficient markets [1989b, p. 5]. He also expresses belief that there are exceptionally talented people who can probably garner superior risk-corrected returns ... and names a few. He does not see them as offering a practical broad alternative investment prescription for active management since such talents are few and hard to identify. As Samuelson believes strongly in micro efficiency of the markets, so he expresses doubt about *macro* market efficiency, supporting the views of Franco Modigliani and Robert Shiller.

There is no doubt that the mainstream of the professional investment community has moved significantly in the direction of Paul Samuelson's position during the 30 years since he issued his challenge. Indexing as either a core investment strategy or a significant component of institutional portfolios is ubiquitous and even among those institutional investors who believe they can deliver superior performance, performance is typically measured incrementally relative to an index benchmark and the expected performance increment to the benchmark is generally small compared to the expected return on the benchmark itself. It is therefore with no little irony that as investment practice has moved in this direction these last 15 years, academic research has moved in the opposite direction, strongly questioning even the micro efficiency case for the Efficient Market Hypothesis. The conceptual basis of these challenges come from theories of asymmetric information and institutional rigidities that limit the arbitrage mechanisms which enforce micro efficiency and of cognitive dissonance and other systematic behavioral dysfunctions among individual investors that purport to distort market prices away from rationally determined asset prices in identified

ways. A substantial quantity of empirical evidence has been assembled, but there is considerable controversy over whether it does indeed make a strong case to reject market micro efficiency in the Samuelsonian sense.⁹ What is not controversial at all is that Paul Samuelson's Efficient Market Hypothesis has had a deep and profound influence on finance research and practice for the past 40 years and all indications are that it will continue to do so well into the future.

III. Warrant and Option Pricing

If one were to describe the important research gains in financial economics during the 1960s as "the decade of capital asset pricing and market efficiency," then surely one would describe the corresponding research gains in the 1970s as "the decade of option and derivative security pricing." Once again, Samuelson was ahead of the field in recognizing the arcane topic of option pricing as a rich area for problem choice and solution. His research interest in options can be traced back at least to the early 1950s when he directed Richard Krueger's thesis on puts and calls (1956). As is evident from that thesis, Samuelson had already shown that the assumption of an absolute random walk for stock prices leads to absurd prices for long-lived options, and this before the rediscovery of Bachelier's work in which this very assumption is made. Although Samuelson lectured on option pricing at M.I.T. and elsewhere throughout the 1950s and early 1960s, his first published paper on the subject, "Rational Theory of Warrant Pricing," appeared in 1965 [III, Chap. 199]. In this paper, he resolves a number of apparent paradoxes that had plagued the existing theory of option pricing from the time of Bachelier. In the process (with the aid of a mathematical appendix provided by H. P. McKean, Jr.), Samuelson also derives much of what has become the basic mathematical structure of option pricing theory today.¹⁰

Bachelier postulates that stock prices follow a random walk so that the expected change in the stock price over any interval of time is zero. The limit of this stochastic process in continuous time in modern terms is called a Wiener process or a Brownian motion. Bachelier also postulated that the price of a call option (or warrant) that gives its owner the right to buy the stock at time T in the

future for an exercise price of \$a must be such that the expected change in the option price is also zero. From these postulates, Bachelier deduced that the option price, $W(X; T, a)$ must satisfy the partial differential equation

$$1/2\sigma^2 W_{xx}(X; T, a) - W_r(X; T, a) = 0$$

subject to the boundary condition $W(X; 0, a) = \text{Max}[0, X - a]$ where X is the price of the stock and σ^2 is the variance rate on the stock. The solution of this equation is given by

$$W(X; T, a) = (X - a)\Phi\left(\frac{X - a}{\sigma\sqrt{T}}\right) + \frac{1}{\sqrt{2\pi}} \exp\left[-\frac{(X - a)^2}{2\sigma^2 T}\right] \sigma\sqrt{T}$$

where $\Phi(\cdot)$ is the standard normal cumulative density function. For an at-the-money option (that is, $X = a$) and relatively short times to expiration T , the Bachelier rule that the value of option grows as \sqrt{T} is a reasonable approximation to observed option prices. However, as Samuelson points out, for long-lived options the formula implies that the option will sell for more than the stock itself, and indeed, for perpetual options ($T = \infty$), the value of the option is unbounded.

Samuelson traces this result to the absolute Brownian motion assumption which for T large implies the possibility of large negative values for the stock prices with nontrivial probability. Noting that most financial instruments have limited liability and, therefore, cannot have a negative price, Samuelson introduces the idea of "geometric Brownian motion" to describe stock price returns. By postulating that the logarithmic price changes, $\log[X_{t+\tau} / X_t]$, follow a Brownian motion (with possibly a drift), he shows that prices themselves will have a lognormal distribution and, therefore, this ensures that they will always be nonnegative. Moreover, because lognormal distributions preserve themselves under multiplication, stock returns will have a lognormal distribution over any time interval. Indeed, this geometric Brownian motion has become the prototype stochastic process for stock returns in virtually all parts of financial economics.

Using much the same procedure of Bachelier, but modifying his postulates to include the geometric Brownian motion and the possibility of a nonzero expected rate of return on the stock, α , Samuelson derives a partial differential equation for the option price given by

$$1/2\sigma^2 X^2 W_{xx}(X; T, a) + \alpha X W_x(X; T, a) - \beta W(X; T, a) - W_r(X; T, a) = 0$$

subject to $W(X; 0, a) = \text{Max}[0, X - a]$ where β is the required expected return on the option. For the case corresponding to Bachelier's where the required expected return on the option is the same as on the stock (that is, $\beta = \alpha$), the solution can be written as

$$W(X; T, a) = X\Phi(h_1) - ae^{-\alpha T}\Phi(h_2)$$

where $h_1 \equiv \left[\log(X/a) + (\alpha + 1/2\sigma^2)T\right] / \sigma\sqrt{T}$ and $h_2 \equiv h_1 - \sigma\sqrt{T}$.

Even when $a = 0$, Samuelson's solution satisfies $W(X; T, a) \leq X$ for all X and T . Hence, the substitution of the geometric Brownian motion for the arithmetic one eliminates the Bachelier paradox. However, as the reader can readily verify for $X = a$ and T small, $W(X; T, a) \sim \sigma\sqrt{T}$ as in the Bachelier case.

Bachelier considered options that could only be exercised on the expiration date. In modern times, the standard terms for options and warrants permit the option holder to exercise on or before the expiration date. Samuelson coined the terms "European" option to refer to the former and "American" option to refer to the latter.¹¹ Although real-world options are almost always of the American type, published analyses of option pricing prior to his "Rational Theory" paper focused exclusively on the evaluation of European options and therefore did not include the extra value to the option from the right to exercise early.

Because he only requires that the option price be equal to $\text{Max}[0, X - a]$ at the expiration date, Samuelson's (" $\beta = \alpha$ ") analysis formally applies only to a European type of option. However, he also proves that his solution satisfies the strict inequality $W(X; T, a) > \text{Max}[0, X - a]$ for $T > 0$ and $\beta = \alpha \geq 0$. Thus, under the posited conditions, it would never pay to exercise a call option prior to expiration, and the value of an American call option is equal to its European counterpart. In consequence, he views the special " $\beta = \alpha$ " case of this theory as incomplete and unsatisfactory. It is incomplete because it provides no explanation of early exercise of options or warrants. Although it resolves the Bachelier paradox, the theory is unsatisfactory because it creates a new one; namely, the value of a perpetual call or warrant, $W(X; \infty, a)$, is equal to the

stock price, X , independently of the exercise price. That is, according to the theory, the right to buy the stock at any finite price a (where this right can never be exercised in finite time) is equal to the price of the stock (which in effect is an option to buy the stock at a zero exercise price where the right can be exercised at any time).

Although he rejects the special case of his theory when $\beta = \alpha$, Samuelson resolves both its incompleteness and its paradox within the context of his general theory by simply requiring that $\beta > \alpha$. He does so by first formally solving his differential equation for the value of a European warrant. He then shows that for $\beta > \alpha \geq 0$ and any $T > 0$, there exists a number $C_r < \infty$ such that $W(X; T, a) < X - a$ for all $X \geq C_r$. Thus, for $\beta > \alpha$, there is always a finite price for the stock at which it pays to exercise prior to the expiration date, and hence, the American feature of an option has positive value. He also shows that $\beta > \alpha$, $W(X; T, a) < X$ for $a > 0$ and the value of a European perpetual call option, $W(X; \infty, a)$, is zero.

Having established that the early exercise provision has value when $\beta > \alpha$, Samuelson then proves that the correct formula for an American call option or warrant will satisfy his partial differential equation subject to the boundary conditions: (i) $W(0; T, a) = 0$; (ii) $W(X; 0, a) = \text{Max} [0, X - a]$; (iii) $W(C_r; T, a) = C_r - a$; (iv) $W_x(C_r; T, a) = 1$, which he calls the "high-contact" condition.¹² For those familiar with parabolic partial differential equations of this type, it may appear that the boundary conditions are overspecified. However, C_r , which is the time boundary of stock prices where the option should be exercised, is not known, and it is precisely the over-specification that permits the simultaneous determination of the option price and the time boundary. Of course, closed-form solutions to such boundary value problems are not easy to derive although Samuelson does solve the perpetual call option case. He also develops a recursive integral technique that is a precursor to the numerical approximation methods used to solve these equations to this day.

While Samuelson mentions the greater riskiness of a warrant over the stock and different tax treatment, his principal argument for the $\beta > \alpha$ case and possible early exercise is that the stock is paying or may pay dividends during the life of the warrant. As formulated in his differential equation, α is the expected rate of price appreciation in the stock and,

therefore, will be equal to the expected rate of return on the stock only if there are no cash dividends. In the example he discusses at length, where the dividend rate is a constant fraction, δ , of the stock price, he shows that for the expected rate of return on the warrant to just equal that of the stock $\beta = \alpha + \delta$, and therefore, $\beta > \alpha$. This analysis also makes it clear why a perpetual warrant on a currently nondividend-paying stock will not have a price equal to the stock price (as predicted by the $\beta = \alpha$ theory): namely, it could only do so if it were believed that the stock would never pay a dividend.

As Samuelson would be the first to say, his 1965 warrant pricing theory is incomplete in the sense that it simply postulates the first-moment relations between the warrant and stock. Yet, the basic intuitions provided by his theory have been sustained by later, more complete, analyses. For example, his focus on dividends as the principal reason for early exercise of call options and warrants was later justified in his 1969 "A Complete Model of Warrant Pricing That Maximizes Utility" [III, Chap. 200] (He brought me along as his junior coauthor), where it was shown that dividends are the only reason for such early exercise. Still later, an arbitrage argument presented in Merton (1973) proves that this result holds in general. Earlier warrant pricing theories uniformly neglected the possibility of early exercise in the development of their evaluation formulas. Samuelson, in addition to proving that early exercise was a possibility, shows that the effect of this possibility on value can be quite significant especially for long-lived options and warrants. Furthermore, his demonstration that the schedule of stock prices at which the warrant should be exercised can be endogenously determined as part of a simultaneous solution for the warrant price provides one of the cornerstones of modern option pricing theory and its application to the evaluation of more complex securities.¹³

In a subsequent conversation with me, Samuelson contrasted the "Rational Theory" with its companion piece "Proof That Properly Anticipated Prices Fluctuate Randomly" by noting that "the results of the paper were not obvious," and that he "was not sure how they would come out until the work was done." Despite his obvious delight with the paper (I do not doubt that this is his favorite among his contributions to financial economics) and despite the many important contributions it contains, discussion of the paper led Paul to remark

that "Too little is written about the 'near misses' in science." While far from unique in the history of science, Samuelson's "Rational Theory" is surely a prime example of such a near miss by an eminent scientist.

Open the financial section of a major newspaper almost anywhere in the world and you will find pages devoted to reporting the prices of exchange-traded derivative securities, futures, warrants and options. Along with the vast over-the-counter derivatives market, these exchange markets trade options and futures on individual stocks, stock index and mutual-fund portfolios, on bonds and other fixed-income securities of every maturity, on currencies, and on commodities including agricultural products, metals, crude oil and refined products, natural gas, and even, electricity. The volume of transactions in these markets is often multiple times larger than the volume in the underlying cash-market assets.¹⁴ Options have traditionally been used in the purchase of real estate and the acquisition of publishing and movie rights. Employee stock options have long been granted to key employees.

In all these markets, the same option-pricing methodology is used both to price and to measure the risk exposure from these derivatives. However, financial options represent only one of several categories of applications for the option-pricing technology. "Option-like" structures are lurking everywhere.¹⁵

Virtually everyone would agree that the Black-Scholes option pricing model published in 1973 was the breakthrough that led to an explosion in option and derivative security pricing research in the 1970s that has had widespread impact on finance research and practice to the current time. I focus here only upon the development of the Black-Scholes option pricing formula and its relation to Samuelson's "Rational Theory" formula.

The foundation of the Black-Scholes model is that, at least in principle, a dynamic hedging strategy can be derived to form a riskless portfolio of the option, the stock, and riskless bonds. Moreover, if such a portfolio can be created, then to avoid the opportunity for arbitrage, it must yield a return exactly equal to that earned on a riskless bond. From this condition, it follows that there must be a unique relation among the option price, the stock price, and the riskless interest rate.

Of course, hedge strategies using a warrant or other convertible securities and the stock were not

uncommon undertakings by practitioners long before 1973. Thorp and Kasouff's *Beat the Market* (1967) is devoted entirely to such hedging strategies. In his "Rational Theory" paper, Samuelson discusses at length (including numerical examples) the use of hedge positions between the warrant and the stock as a means for deriving bounds on the discrepancies between β and α . These bounds translate through his warrant pricing equation into bounds on the range of rational warrant prices. In this discussion, he goes on to mention that the opportunity cost or carrying charges for the hedge should be included and therefore, the riskless rate of interest would enter into the bounds. Thus, Samuelson had in his paper the hedging idea for restricting prices and the possibility that the interest rate would enter into the evaluation, both of them key elements in the Black and Scholes analysis. Yet, neither he nor the others pushed their ideas in this area the extra distance required to arrive at what became the Black-Scholes formula. As Samuelson later wrote in "Mathematics of Speculative Price" [1972a, IV, Chap. 240, p. 438],

My 1965 paper had noted that the possibility of hedging, by buying the warrant and selling the common stock short, should give you low variance and high mean return in the $\beta > \alpha$ case. Hence, for dividendless stocks, I argued that the $\beta - \alpha$ divergence is unlikely to be great. I should have explored this further!

The most striking comparison to make between the Black-Scholes analysis and Samuelson's "Rational Theory" is the formula for the option price. In their derivation, Black and Scholes assume a nondividend-paying stock whose price dynamics are described by a geometric Brownian motion with a resulting lognormal distribution for stock returns.¹⁶

This is, of course, the identical assumption about stock returns that Samuelson made. Under these conditions, the Black-Scholes no-arbitrage price for a European call option, $F(X; T, a)$, is shown to be the solution to the partial differential equation

$$\begin{aligned} 1/2\sigma^2X^2F_{xx}(X; T, a) + rXF_x(X; T, a) \\ - rF(X; T, a) - F_t(X; T, a) = 0 \end{aligned}$$

subject to the boundary condition $F(X; 0, a) = \text{Max}[0, X - a]$ and where r is the (instantaneous)

riskless rate of interest that is assumed to be constant over the life of the option. By inspection, this equation is formally identical to the one derived in the "Rational Theory" for the special " $\beta = \alpha$ " case if one substitutes for the value of " α " the interest rate " r ." It follows, therefore, that the Black-Scholes option pricing formula, $F(X; T, a)$, is formally identical to the Samuelson option pricing formula, $W(X; T, a)$, if one sets $\beta = \alpha = r$ in the latter formula.

It should be underscored that the mathematical equivalence between the two formulas (with the redefinition of the parameter α) is purely a formal one. That is, the Black-Scholes analysis shows that the option price can be determined without specifying either the expected return on the stock, α , or the required expected return on the option, β . Therefore, the fact that the Black-Scholes option price satisfies the Samuelson formula with $\beta = \alpha = r$ implies neither that the expected returns on the stock and option are equal nor that they are equal to the riskless rate of interest. Indeed, Samuelson notes in his "Mathematics of Speculative Price" (1972a) that even if α is known and constant, β will not be for finite-level options priced according to the Black and Scholes methodology. It should also be noted that Black-Scholes pricing of options does not require knowledge of investors' preferences and endowments as is required, for example, in the Samuelson-Merton 1969 warrant pricing paper. The "Rational Theory" is clearly a "miss" with respect to the Black-Scholes analysis. However, as this analysis shows, it is just as clearly a "near miss."

This said, it may seem somewhat paradoxical to suggest that the Black-Scholes breakthrough actually added to the significance of Samuelson's "Rational Theory" for the field, yet I believe it did. Before Black-Scholes, there were a number of competing theories of warrant and convertible security pricing. Some, of course, were little more than rules of thumb based on empirical analyses with limited data. Others, however, like the "Rational Theory," were quite sophisticated. The Black-Scholes analysis provides a degree of closure for the field on this issue, and thus renders these earlier theories obsolete. However, as noted here and as shown in detail in the Appendix to "Mathematics of Speculative Price" [Merton, 1972], virtually all the mathematical analysis in the "Rational Theory" (including its formidable McKean appendix) can be used (with little more than a redefinition of parameters) to

determine the prices of many types of options within the Black-Scholes methodology. As one example, consider options where early exercise can occur. As is shown in Merton (1973), one can solve for the Black-Scholes price of either a European or an American call option on a proportional-dividend-paying stock simply by substituting $\beta = r$ and $\alpha = r - \delta$ into the "Rational Theory" analysis of the " $\beta > \alpha$ " case. Similar results obtain for the evaluation of put options.

As a second example, there is the solution in the McKean appendix for the price of an option on a stock whose return is a Poisson-directed process that is discussed in Cox and Ross (1976) and Merton (1976). As still a third example, there is the Samuelson development in the "Rational Theory" of the partial differential equation for option pricing and its solution that uses a limiting process of discrete-time recursive difference equations and a local binomial process for stock price returns. This development is formally quite similar to the simplified procedure for Black-Scholes option pricing presented in Cox-Ross-Rubinstein (1979) and Sharpe (1978) as well as to the numerical evaluation procedure for options in Parkinson (1977). In light of these consequences, Samuelson's "Rational Theory of Warrant Pricing" is some near miss!

IV. Investing for the Long Run

In so many branches of economics, Paul Samuelson is a kind of gate-keeper. When he is not busy opening gates to new research problems for himself and an army of other economists to attack, he is busy closing gates with his definitive solutions. And in between, he somehow finds the time to convey to both the professional practitioner and the general public those important research findings that have survived the rigors of both careful analytical and empirical examination.

Samuelson's new discoveries in finance are foundational. However, his diligence in trying to subvert error is also deeply important to the field. Just as in investing where the most gold goes to those who show us how to make money, so the most academic gold (or credit) goes to new discoveries. But in investments, as Samuelson's work in efficient markets and portfolio theory amply demonstrates, there is also considerable value to being shown how not to lose money by avoiding financial

errors. Just so, there is also considerable value to those who divert us away from the paths of error in research.

By defanging the St. Petersburg Paradox, Samuelson (1960, 1977) has taught us not to unduly fear unbounded utility and, thereby, he has left intact the important body of research into the economics of uncertainty that is based upon the HARA family of utility functions, most of whose members are unbounded functions. While defending the legitimacy of the HARA family, he has also kept us from becoming enthralled with the enticing geometric mean maximization hypothesis where log utility, a particular member of the family, is proclaimed to be the criterion function for “super” rational choice.¹⁷ Samuelson discriminates among brain children, and his success in saving the profession from being drawn further along these paths of error has been due in no small part to his willingness to reaffirm basic beliefs whenever, like the phoenix, some new version of an old error arises. Disposing of one version in his “The ‘Fallacy’ of Maximizing the Geometric Mean in Long Sequences of Investing or Gambling” [1971b, III, Chap. 207], Samuelson returned to battle a second one (this time taking me along as coauthor) in “Fallacy of the Lognormal Approximation to Optimal Portfolio Decision Making Over Many Periods” [1974, IV, Chap. 245]. Still later in 1979, he countered a third with his paper of the monosyllabled title, “Why We Should Not Make Mean Log of Wealth Big Though Years to Act Are Long.”

Beginning sometime in the early 1980s a new fallacy, also associated with long-horizon investing, arose that over the next two decades would have a far greater impact on real-world practice than the fallacy of investing so as to maximize the expected log return on one’s portfolio. This new fallacy prescription is that “Stocks are not risky in the long run.” That is, over a long enough investment horizon, stocks will out perform risk-free long-maturity bonds and so investors with long-term investment goals such as saving for retirement should invest their retirement savings in equities.

This prescription, like the max expected log strategy, is driven formally by an assessment that one investment strategy will outperform another (or all others) with increasingly greater probability the longer the investment horizon, until in the limit of an infinite horizon, the probability of superior performance approaches 100 percent. As a matter of

mere mathematics, it can indeed be shown that under relatively mild assumptions about the expected return on the stock market and its volatility, the probability that stocks will *underperform* bonds goes to zero as the horizon becomes infinite and that indeed over a 25–35 year horizon the estimated probability of such a “shortfall” is in low single digits. The apparent (asymptotic long-run) dominance of stocks over bonds permits nearly universal and uniform advocacy for this investment policy, independently of individual economic status. Hence, it is argued that investors with a long-horizon goal should invest in stocks over bonds, *without regard to their risk-tolerance preferences*. Further “practical” support for this prescription was provided by observing that historical returns on the U.S. stock market outperformed bonds over every (or nearly every) 15- or 20-year time period in the last century. Nearly every advice engine on the Internet offers this same age-dependent strategy as a fundamental principle of retirement saving. The same principle is central to asset allocation advice to corporate pension funds.

Characteristically, Samuelson recognized early on that the question of the effect of age on risk-taking and optimal portfolio selection was an important issue, worthy of careful scientific analysis. And so, in 1969, Samuelson published a paper on the optimal intertemporal portfolio selection and consumption problem, which applied the method of stochastic dynamic programming together with the Expected Utility criterion. Although others studied the problem,¹⁸ it was Samuelson who focused his analytical modeling on the substantive issue of age-dependent influences on portfolio allocations and often-discussed-but-not-well-defined related concepts such as “businessman’s risk.” He shows that risk-averse investors with constant-relative-risk-aversion (“CRRA”) utility functions (which includes the heralded log function) and facing the same investment opportunities each period of their investment life, would allocate the same fractions of their optimal portfolio between risky equities and safe short-term debt, *independently of their age*. And while this surely does not rule out age-dependent portfolio behavior for some preferences, it just as surely demonstrates that growing investment conservatism with age is not a robust optimization principle which obtains universally. And in particular, the Samuelson finding provides absolute counter-evidence against the claimed absolute dom-

inance of investing in equities over bonds when the investment horizon becomes very long. And this is so even though the temporally independently and identically distributed returns for equities posited in the Samuelson model also satisfy the probability condition that as investment horizon increases, the probability that equities under-perform bonds decreases, asymptotically approaching zero.

The period 1969–1982 just after the publication of Samuelson’s paper shows no widespread adoption of this prescription for long-horizon investors to allocate a large fraction of their portfolios to equities, perhaps because it was a very poor one for stock performance in the United States. The creation of ERISA and with it, the corporate pension fund industry in 1974 thus did not cause equities to become a significant part of pension fund portfolios immediately. However, by the late 1980s after some strong performing years, institutional pension-fund investors had moved their allocation to equities increasingly to the point of their dominating the typical portfolio. The large shift to equities was encouraged by the actuarial treatment of pension expenses that applied the traditional Law of Large Numbers approach to argue that expected returns on the pension asset portfolio could be treated as virtually sure-thing returns over the long horizon of pension liabilities and so projected pension expense would be reduced by holding larger expected return (and larger risk) equities instead of bonds. This “institutionalization” of the principle that “Stocks are not risky in the long run” was completed when the pension accounting rules were adopted that called for firms to use the *projected* pension expense in computing accounting earnings for the firm instead of the *realized* pension expense based on the actual performance of the pension fund portfolio, with any reconciliation of deviations between the two smoothly amortized over a ten-year period.

In a series of papers, Samuelson (1989b,c, 1990, 1994, 1997a,b) was quick and clear to define the issue: Investing in equities may well be part of an optimal investing strategy for pensions as elsewhere, so long as the risk that goes with the higher expected return on equities is properly accounted for in the decision. What is fallacious . . . and therefore dangerous . . . reasoning is the misapplication of the Law of Large Numbers to argue that these higher expected returns will turn into higher realized returns almost certainly, if one has a long enough horizon, and thus with a long horizon one

need pay no attention to the risk component. Samuelson presented his position both in intuitive fashion and in very formal mathematical terms why the exclusive focus on diminishing probability of a shortfall from equities as horizon lengthens is not sufficient for dominance because it does not take into account the growing magnitude of the expected shortfall that occurs as horizon increases. That is, what matters is the product of the probability of stocks underperforming times the conditional expected shortfall when they do, and while the probability is declining, the size of that expected shortfall is growing and so one needs to consider the net growth or decline of their product with horizon. Furthermore, it turns out that the product grows with longer horizon and thus, the shortfall risk in that sense is not declining at all but increasing.¹⁹

Samuelson along with others also highlighted the fallacy in simply taking the realized stock returns in the United States for the last century or more as statistically significant empirical proof of the dominance principle by pointing out that from a statistical perspective that long history is only a single sample. He then goes on constructively to specify the proper representation which uses the historical data in what is formally a “bootstrap” process to generate by Monte Carlo techniques the prospective distributions from the past. These distributions demonstrate that a significant shortfall risk does exist, even with a long horizon. Samuelson and others also noted that the data themselves are subject to selection bias in that the United States stock market performance over the 20th Century may not be an unbiased estimate of the future for it or any other country’s. Had the focus instead been on the investment history over the same period in other countries, Argentina, Russia, and Japan for instances, the “obvious” empirical evidence for nearly sure-thing outperformance of stocks over bonds in the long run would hardly be so obvious.

Despite the cautioning writings of Samuelson and others on the subject, the influence of the “Stocks are not risky in the long run” principle actually expanded and grew enormously with the creation of Define-Contribution 401k pension plans in the beginning of the 1990s, in which individuals are directly responsible for allocating their retirement savings. Every advice engine, whether on the Internet or at a mutual fund complex, had this as one of its foundational principles. The extraordinary

expost performance of the U.S. stock market in the 1990s only served to confirm the validity of the principle, even in the not so long run.²⁰ The related argument for age-dependent growing conservatism exemplified by the rule of thumb "Invest fraction 100 minus your age in stocks," was institutionalized by the mutual fund industry which offers Lifecycle funds which adjust the stock-bond mix towards more bonds as one gets older. Having correctly educated investors on the power of diversification among assets as an efficient means for managing risk, intuitive explanations by analogy were put forward claiming that diversification across time works in a similar way to justify the principle of more stock allocation the longer the horizon. Indeed, the principle that one can earn high equity expected returns with virtually no risk if one has a long horizon is tailored-made for arguing that Social Security should consider funding with investments in equities whether in private accounts or the government-controlled fund.²¹

Throughout this period, Samuelson was steadfast in making the case that there are no shortcuts to taking into account risk. Because the performance of 401k plans go directly to individuals, he reiterated the points made by his 1969 paper that sensible preference functions for evaluating the risk-return tradeoff do not necessarily lead to ever increasing allocations to stock as one has longer time until retirement. He demonstrated formally and in simple illustrations the fallacy of time diversification (1997b).

Characteristically, Samuelson having made the strong multi-dimensional case against universal age-dependent arguments for holding a larger fraction in stocks the longer the horizon until retirement, then goes on to investigate what characteristics of the return distribution would cause those counter-example CRRA-utility investors of his 1969 paper to hold more equities the longer the time until retirement. In Samuelson (1989a, 1991, 1997b), he shows that such age-dependent behavior will obtain if one replaces intertemporally independently- and identically-distributed stock returns (a "white noise" process) posited in his 1969 paper with stock returns that exhibit mean-reversion or negative serial correlation (what he calls a "red noise" process). However, he also shows that the age-dependent behavior can go in the opposite direction with a larger fraction of the portfolio allocated to risky equities the *shorter* the time left

before retirement, if stock returns exhibit momentum or positive serial correlation (what he calls a "blue noise" process). Having made these affirmative cases when age-dependent portfolio allocation is optimal, he points out that the evidence for either mean-reversion or momentum in stock returns is hardly overwhelming. He concludes by reaffirming his position that stocks are risky in the short, intermediate and long runs and that arguments for holding stocks based on a contrary belief are fundamentally flawed.

After the large three-year decline in equity markets and interest rates between 2000 and 2002, there were widespread, deep losses in corporate pension fund portfolios and in individual retirement accounts. Together with the fall in interest rates which caused pension liabilities to increase at the same time, the effect was to cause enormous shifts toward large under-funding of corporate pension plans, which in weakened industries such as steels, airlines and automobiles has caused, or at least accelerated, bankruptcies. These failures in turn have caused the government insurer of corporate pensions, Pension Benefit Guarantee Corporation, to incur enormous losses, going from a large reserve surplus to a huge negative shortfall on its balance sheet, raising the specter of another taxpayer-bailout as was experienced with deposit insurance and the thrift institutions in the 1980s. The ceiling on PBGC insurance coverage has in turn led to large losses in accrued pension benefits by higher-paid workers in these industries.

With these events, corporate plan sponsors, pension regulators and other overseers have taken notice: Rating agencies are already taking into account pension underfunding on setting credit ratings and it is a safe prediction that they will move from there to recognizing that the risk as well as the expected return of pension fund assets, like any other risky asset of the corporation, needs to be taken into account in assessing the creditworthiness of the firm. The Financial Accounting Standards Board in the United States is currently studying widespread pension accounting reforms with focus on the use of projected pension expenses instead actual expenses for determining earnings of the firm. Similar reforms are already further underway in the United Kingdom and in the setting of international accounting standards.

Today 36 years after the publication of Samuelson's paper identifying and analyzing age-depen-

dent optimal rules for long-horizon investing, we thus find that work at the center of some of the most important private- and public-sector finance-related policy issues around the world.

V. Afterword

As noted at the outset of my remarks, a prevailing theme of research in financial economics is the conjoining of intrinsic intellectual interest with extrinsic practical application. This research has significantly influenced the practice of finance whether it be on Wall Street, LaSalle Street, or in corporate headquarters throughout the world. In this regard, Paul Samuelson provides a sterling counterexample to the well-known dictum of Keynes that “practical men, who believe themselves to be quite exempt from intellectual influences, are usually the slaves of some defunct economist.” Any attempt to trace *all* the paths of influence which Samuelson has had on finance practice is, of course, doomed to failure—we need only remember the seemingly countless editions of his basic textbook on which so many practitioners were reared.²²

As in all fields where the research is closely connected with practical application, in financial economics, conflicts in problem choice are not uncommon between those that have the most immediate consequences for practice and those that are more basic. As is evident from the following excerpt from his Foreword to *Investment Portfolio Decision-Making* [1974c, IV, Chap. 244, p. 488], there is surely no doubt how Paul Samuelson resolves such conflicts in his own research.

My pitch in this Foreword is not exclusively or even primarily aimed at practical men. Let them take care of themselves. The less of them who become sophisticated the better for us happy few! It is to the economist, the statistician, the philosopher, and to the general reader that I commend the analysis contained herein. Not all of science is beautiful. Only a zoologist could enjoy some parts of that subject; only a mathematician could enjoy vast areas of that terrain. But mathematics as applied to classical thermodynamics is beautiful: if you can't see that, you were born color-blind and are to be pitied. Similarly, in all the branches of pure and applied mathematics, the subject of probability is undoubtedly one

of the most fascinating. As my colleague Professor Robert Solow once put it when he was a young man just appointed to the MIT staff: “Either you think that probability is the most exciting subject in the world, or you don't. And if you don't, I feel sorry for you.”

Well, here in the mathematics of investment under uncertainty, some of the most interesting applications of probability occur. Elsewhere, in my 1971 von Neumann Lecture before the Society for Industrial and Applied Mathematics, I have referred to the 1900 work on the economic Brownian motion by an unknown French professor, Louis Bachelier. Five years before the similar work by Albert Einstein, we see growing out of economic observations all that Einstein was able to deduce and more. Here, we see the birth of the theory of stochastic processes. Here we see, if you can picture it, radiation of probabilities according to Fourier's partial differential equations. And finally, as an anticlimax, here we see a way of making money from warrants and options or, better still, a way of understanding how they must be priced so that no easy pickings remain.

In short, *first things first*.

There is no need to dwell on the prolific and profound accomplishments of Paul Samuelson, which have become legend—especially when the legend is a brute fact. Rather I close with a few observations (drawn as his student, colleague, and coresearcher over nearly four decades) on some of Paul's modes of thought that perhaps make such super achievement possible. First, there is his seemingly infinite capacity for problem finding and his supersaturated knowledge of just about every special sphere of economics. Second, there is his speed of problem solving together with the ability to put the solution quickly to paper with great skill, great verve, and lack of hesitation. Third, strong opinions and decisive language are characteristic of Samuelson writings, and yet it is his willingness to change his views and admit errors that makes his steadfastness on some issues so credible. Finally, although often masked by the apparent ease with which he produces, there is his diligence. Paul has always worked hard.

On the matter of sustained hard work of this particular kind, Paul is fond of a story (and so, he

repeated it in his presidential address to the International Economics Association.) about the University of Chicago mathematician Leonard Dickson, who was to be found playing bridge all afternoon every afternoon. When a colleague asked how he could afford to spend so much of his time playing, Dickson is said to have replied: "If you worked as hard at mathematics as I do from 8–12, you too could play bridge in the afternoon." As Paul also notes in that address, much the same story holds for the mathematician G. H. Hardy, in his case watching cricket rather than playing bridge. I can improve on these yarns with one about Paul from the glorious days when as his research assistant I lived in his office. I was working (not very successfully) on the solution to an equation in warrant pricing that was needed for some research Paul was doing when he left for the tennis courts (as he often did). Sometime later, the phone rang. It was Paul calling from the courts (presumably between sets) to tell me exactly how that equation could be solved Dickson and Hardy segregated creative work and well-earned play, and so it appears does Paul, but with a finite and significant difference. Even at play, he is at work.²³

Notes

1. Samuelson offers us some brief synthesizing observations on foundational developments in the field in his recent "Modern Finance Theory Within One Lifetime (2002)," but characteristically he confines his remarks only to the contributions of others.
2. See Samuelson (1950, 1952a,b, 1967a,b, 1970a).
3. The explicit content of Samuelson's early work reviewed here, of course, has not changed but its subsequent application and impact on the field, both in breadth and depth, surely has. Hence, even when overlaps with my past writings occur, the substance of Samuelson's work warrants repeating here, especially when the originals appear in obscure places. Thus, when applicable, the text draws heavily on my 1983 essay.
4. See Taqqu (2001) for more on the Bachelier story.
5. As Samuelson notes with his typical great care, without the transversality or other terminal boundary condition, these local arbitrage conditions are necessary but not sufficient to ensure an optimal path
6. Samuelson's draft is not dated but I would estimate 1980. The acknowledgement helped pin it down: "We owe thanks to the National Science Foundation for financial aid and to Aase Hugins for editorial assistance. Hal Stern, an MIT senior, kindly tested the data to verify its conformity with theoretical expectations." Hal Stern graduated from MIT in 1981.
7. Note that within this model, excess returns exhibit both mean-reversion and momentum, depending on their size: mean-reversion behavior for small-in-magnitude excess returns and momentum behavior for large-in-magnitude returns. Thus, we have in this early Samuelson work a conditional combination of both his "red noise" and "blue noise" processes for stock returns that he introduces in later work (1989a, 1991, 1997b) to demonstrate possible properties of age-dependent optimal portfolio selection rules.
8. It can be shown that if investor learning is sufficient to wipe out the profitable trading structure, the resulting new excess return process for the model of the example will be $X_t = \varepsilon_t[1 + a(\varepsilon_t^2 - b)]$ for which the martingale property obtains.
9. See Lo and MacKinlay (1999). Merton and Bodie (2005, especially p. 4, footnotes 8 and 9) provide extensive references on both sides of the controversy.
10. Samuelson uses warrants instead of call options as the specific instrument examined in his paper, perhaps because at that time, warrants were listed and traded on exchanges and so price data were available whereas options were only traded through dealers with opaque pricing. Indeed, I tested the Samuelson pricing model in the late 1960s using prices of listed perpetual warrants [Merton 1969]. Although there is a slight difference between the two in terms of dilution effects depending on whether the company is the issuer or not, the pricing models for warrants and call options are essentially the same and the terms are used interchangeably for purposes of the discussion here.
11. Samuelson started to formulate his theory of warrant and option pricing in the mid-late 1950s. As he often did, and still does, with a

- new area of research, he began then by talking to those in practice to get a sense of how it all works institutionally before proceeding with the formal model specification and theory development. So he went to New York to see a well-known put and call dealer (there were no traded options exchanges until 1973) who happened to be Swiss. After identifying himself and explaining what he had in mind, Samuelson was quickly told, “You are wasting your time—it takes a *European* mind to understand options.” Later on, Samuelson understandably chose the term “European” for the relatively simple(-minded)-to-value option contract that can only be exercised at expiration and “American” for the considerably more-complex-to-value option contract that could be exercised early, any time on or before its expiration date.
12. Later authors refer to this as the “smooth-pasting” condition.
 13. Merton (1972, 1973) proves that the Samuelson-positied “high-contact” condition is implied by the unique value-maximizing early-exercise strategy that rules out arbitrage possibilities.
 14. A recent Federal Reserve estimate is that \$270 trillion notational amount of derivatives are outstanding world-wide.
 15. Examples are insurance contracts including deposit and pension insurance, loan guarantees, privatization of Social Security, prepayment of mortgages, farm price supports, oil-drilling and automobile leases, quotas on taxis and fishing, patents, tax and market timing, tenure, labor-force training, health plans, pay-per-view television, retail store shelf space, modularity and flexibility in production processes, drug discovery phasing and movie sequel timing. See Merton (1992, 1998) for references. Jin, Kogan, Lin, Taylor and Lo (1997) provide a live website with an extensive and growing listing of applications.
 16. In a 1968 critique of the Thorp-Kasouff book, Samuelson quite correctly warns the reader that their reverse-hedge techniques in expiring warrants are no “sure-thing” arbitrage. Later [1972a, IV, Chap. 240, p. 438, n. 6], he reiterates a similar valid warning in his discussion of the Black-Scholes arbitrage argument. If, however, Samuelson had not discovered this overstatement in the Thorp-Kasouff analysis so quickly, then he might have used the occasion to pursue further his own earlier work in using hedge strategies to restrict the range of rational warrant prices. Perhaps this thought was in his mind when Paul commented to me on his 1968 review as one in which “I won farthings and lost pounds.”
 17. Cf. Kelly (1956), Latane (1959), Markowitz (1976) and Thorp (2004).
 18. Other early developers of this problem include Edmund Phelps, Nils Hakansson, Hayne Leland, and Jan Mossin. I developed a continuous-time version.
 19. Bodie (1995) provides an elegant demonstration of this point when he shows that the cost of buying “shortfall insurance” which is structurally a put option on equities with strike price equal to the forward price of the current value of the portfolio, is an increasing function of the investment horizon. Through his foundational work on options, Samuelson contributed, albeit indirectly, to the Bodie demonstration as well.
 20. Unfortunately the experience in Japan, the second largest economy in the world, during this period was quite the opposite: In 1989, the Japanese stock market hit a peak of over 39,000 and today, 16 years later, it is 14,700.
 21. It has been noted by a number of observers including Paul Samuelson that the government has an even longer horizon than any pension plan and furthermore, with a central bank it has no short-term liquidity problems, and so if the no-long-term-risk-to-stock-returns principle applies validly to retirement savings, why not apply it to funding *all* government expenditures?
 22. Bernstein (1992) nevertheless provides a rich description of the many paths of Samuelson’s influence on modern Wall Street.
 23. Happily, some things do not change. A few days ago, Paul called me (this time I was on a cell phone in a taxi cab) to discuss a certainty-equivalent calculation he was doing to demonstrate in still another enlightening way why the Kelly Criterion is not even near-optimal for those with non-log preferences that also do not risk ruin. After he painstakingly described the detail calculations he was performing in the mere two-period case, he asked whether they were correct. I responded that perhaps I could check them with pencil and paper after reaching my destination. Paul then reminded me that

Student was (reputed to be) able to compute Pearsonian correlation coefficients in his head. The message was clear. After my later checking, Paul's calculations were indeed correct.

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