FINANCIAL ECONOMICS

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PREFACE

In this collective tribute to Paul Samuelson, the editors wisely chose a path differing somewhat from the usual route of a Festschrift. Rather than asking the contributors to report some research of their own that derives from Samuelson’s work, they have asked us to try our hands at synthesizing his chief contributions to our respective spheres of economic theory. And since Paul Samuelson would probably confess that his discoveries in economic theory constitute the core of his multiform writings, there could scarcely be a better format for honoring this universal man of economics. His theoretical contributions 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. This volume provides a special opportunity to do so once again although presumably each contributor to it knows full well that the claims of all the rest are just as valid.

Paradoxically, this “best” format is in fact only second-best (or perhaps one should say, $n^{th}$ best). For, as everyone knows, Paul Samuelson is his own best synthesizer and critic. It therefore follows with inexorable logic that he should be writing his own Festschrift (which would then be truly critical). This rational, if unorthodox, alternative would have been especially apt for this chapter on his contributions to financial economics, a subject in which Rational Man reigns supreme. Nevertheless, that alternative would have been dubiously optimal since the volume is designed to do him honor rather than delegate him to perform still another feat of critical synthesis for the benefit of his fellow economists. Just another instance, I suppose, of the complexity of rationality in action.

After this proper rationalization—in both the psychological and economic senses of the term—of our collectively undertaking the task of synthesis rather than
imposing it on Paul Samuelson himself, there remains the problem of establishing a synthesizing design adequate to encompass his contributions. We know that synthesis involves abstraction from the complex original. Here, we must be severely selective in our abstractions since the wide-ranging scope and unflagging volume of his researches allows only a few dimensions of the work to be examined. To make matters worse, the understandable focus on his contributions to economic science leads us to neglect the noneconomic latent themes that are so distinctive of the Samuelsonian corpus. One such thematic interest pervading practically all his writings consists of observations in the history and sociology of science. The law of comparative disadvantage rules out my examining this matter in fitting detail. Besides, the effort would consume too much of the scarce space available for filing the financial economists' claim on Samuelson, and they, no less than the economists from the nine other branches of economic theory represented here, would not wish to receive anything less than their 10 percent pro rata share. Still, I cannot wholly resist the temptation to call attention to this theme in the hope that it will lead others, better qualified, to explore this matter elsewhere.

And so this preface to a miniaturized Samuelson Sampler will only touch upon some characteristic Samuelsonian observations in the history and sociology of science before turning to a more detailed, annotated Contents of his fundamental work in financial economic theory, this to be followed by a brief Afterword.

As is readily apparent from even a quick perusal of the four volumes of his collected scientific papers, Samuelson's writings on Smith, Ricardo, and 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, bountiful as they are, to focus exclusively upon these explicit undertakings in the history of economics is to miss much. Part of the unmistakable stamp of a Paul Samuelson article is his interjection of anecdotes and stories around and between his substantive derivations, which are nuggets to be mined by even the most mathematically timid historian of science who, by necessity, must bypass the bristling equations. These stories, of course, also serve as way stations of entertainment and enlightenment for those who are riding the locomotive of his 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 Poincaré, 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. 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. 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?

Along with the anecdotes and stories, there are the eponyms that would surely appear in everyone’s benevolent parody of a typical Samuelson paper. These highly synoptic histories of the subject1 serve to remind us all—from the economist well-practiced in the history of economic thought to the novice theorist, unencumbered by such knowledge, who benefits from such cram courses—that we owe credit to a sequence of scientists (as we have just seen in his preceding footnote with its allusion to the “Einstein-Fokker-Planck Fourier equation”). While such penchant for hyphenation is, of course, not unique, no one uses it more profitably and with greater skill than Paul Samuelson.

One example of such skillful use is his treatment of Benoit Mandelbrot and the stable distributions. As we all know, Mandelbrot is responsible for some of the most

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1As the sociologist R. K. Merton notes (1976, p. 130, footnote 53): “Every scientific discipline has some practitioners who take pleasure in keeping green the memory of developers of ideas though none, to my limited knowledge, more so than Paul Samuelson, master constructor of those freight trains of eponyms that instantly catch up main lines in a genealogy of ideas (‘an exact Hume-Ricardo-Marshall model of international trade’ can serve as the example of the hyphenated variety although a longer search would surely uncover as long a freight train as the adjacency type exemplified in ‘the economic theory of index numbers associated with the names Pigou, Kônus, Keynes, Stachel, Leontief, Frisch, Lerner, R. G. D. Allen, Wald, and my own theories of revealed preference’).”
fundamental research on infinite-variance stable distributions and was the first to introduce them into financial economics. However, as we all also know, the practice among economists (including Samuelson) is to refer to these distributions as “Pareto-Lévy distributions.” On the occasion of his paper “Limited Liability, Short-Selling, Bounded Utility, and Infinite-Variance Stable Distributions” [1976c, IV, Chap. 247], it appears that Samuelson chose to correct this “injustice.” In the opening footnote (p. 534), he acknowledges Mandelbrot's contributions with “Every economist working with stochastic processes has room to be grateful to Dr. Benoit Mandelbrot of IBM, for his pioneering work in extending finite-variance models to the infinite domain of Lévy and beyond; and the author feels a particular debt to his works of the last 15 years.” However, the key acknowledgement occurs three pages later in the section subtitle where he substitutes “Mandelbrot-Lévy Stable Distributions” for the standard “Pareto-Lévy.” To justify and to underscore the significance of this substitution, he adds in a footnote:

Mandelbrot uses “Pareto-Lévy distributions” to describe the stable-additive functions with finite mean but infinite variance. However, as he points out, Pareto only knew their tail's asymptotic property; so my label is more fitting than that which Mandelbrot too modestly originally suggested; moreover, it can also be used for the stable case where the mean is not defined, an area to which Mandelbrot has also made great contributions.

Although he is not a card-carrying sociologist of science, Paul Samuelson’s work is pervasive as we know from the testimony of the veteran sociologist of science, Robert K. Merton (1973). This sociological subtheme in Samuelson's writings can perhaps be traced to his early, soon-abandoned thought of possibly taking up life as a sociologist. From his papers devoted exclusively to the history of ideas to the cryptic-but-critical asides that pepper all his writings, Samuelson consistently demonstrates a deep insight into the social structure for the allocation of rewards in science. His papers also provide a wealth of examples and analyses of multiple discoveries and rediscoveries; waxing and waning fame in science; precocity in science; allocation of credit as a zero-sum game; oral publication; loss of credit from publication in obscure places and from obliteration of source by incorporation in canonical knowledge; and “near misses” in scientific discovery. As is evident from even this short inventory (drawn exclusively from Merton’s catalog of sociology of science problems), Samuelson’s excursions into the history and sociology of science cover much of the terrain in these fields.

As demonstrated, for example, in his “The Balanced-Budget Multiplier: A Case Study in the Sociology and Psychology of Scientific Discovery” [1975, IV, Chap. 274], Samuelson has the cognitive distance needed to evaluate his own work as well as that of others. His substantive contributions to financial economics provide a rich source for another such case study, and were Samuelson writing this chapter, he

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2 In his autobiographical piece, “Economics in a Golden Age: A Personal Memoir” [1972b, IV, Chap. 278, p. 885 and infra], Samuelson reveals that “there was a minute in my sophomore year when I toyed with the notion of becoming a sociologist. . . .” Citations of Samuelson’s Collected Scientific Papers give year of original publication, volume in roman numerals, chapter, and sometimes specific pages.
would surely discuss his work as exemplifying these varied sociological concepts. Just one more reason why this volume's best format is really only second-best.

Designed to whet the appetite for further exploration of the latent themes in his writings, this teaser on his observations in the history and sociology of science also underscores the losses that inevitably occur when one tries to synthesize Samuelson originals. There is the further difficulty in finding a synthesizing design that can capture the distinctive flow of his developing problem formulations and solutions. If, however, abstraction is a necessity (as it is here), then it is surely better to abstract by selecting and reproducing in their entirety a small subset of his articles rather than attempting to summarize them all. Hence the notion of a Samuelson Sampler. The choice of an anthology also neatly provides the opportunity for some tradeoff between the best synthesis (which requires that he undertake the task) and doing Paul Samuelson honor (which certainly requires that he not). And so the constrained solution is to ask Paul to select the subset of financial economic articles for inclusion and then to have me write the introduction to them. (Note that by having Samuelson himself choose the articles, this format quite fittingly has the additional feature of providing some interesting data to the historian of science.)

In asking Paul to make his selections, I presented him with a list of his thirty articles in financial economics and left the criteria for choice purposely vague. By the not-so-tactful demanding criterion that was evidently applied, he was drastically selective, choosing only six. These are now listed in the Contents, followed by that promised Introduction of mine. Four of the six articles appear in journals not on the beaten path of most economists, and at one time their reprinting would have made them far more accessible to a wider audience. They all now appear in Samuelson's collected scientific papers, and so it is happily unnecessary to reproduce them here.

CONTENTS


INTRODUCTION

Although most would agree that finance, microinvestment 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 money management and capital budgeting procedures is evident from even a casual comparison of current practices with, for example, those of the early 1960s. The effects of financial research have even 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 (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. Thirty 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.

In economic theory, it is traditional to take the existence of households and their tastes as exogenous to the theory. However, this tradition does not extend to economic organizations that are regarded as existing primarily because of the functions they serve and are, therefore, endogenous to the theory. Hence, to derive the functions of these economic organizations, the behavior of individuals must first be derived. It is, therefore, not surprising that the evolution of modern financial economics began with the study of individual choice under uncertainty and that Samuelson's early contributions to this evolution were in this area.

In financial economics, the basic problem of choice for an individual is to determine the optimal allocation of his or her wealth among the available investment opportunities. The solution to the general problem of choosing the best mix is called portfolio selection theory. Of course, for this problem to be well posed, it must be assumed that the individual has a preference ordering for ranking alternative choices. While it has long been the accepted standard to assume that
preference orderings satisfy the axioms of the von Neumann-Morgenstern expected utility maxim, this was not the general view among economists in the early 1950s, when Samuelson published a sequence of papers investigating the legitimacy of these axioms.

The essence of the expected utility maxim is that for each individual, there exists a unique (up to a positive affine transformation) nondecreasing function \( U(\cdot) \) whose expected value can be used to determine the preference orderings of that individual among stochastic alternatives. That is, if \( x_i \) denotes the random variable payoff to choosing the \( i^{th} \) alternative where the individual's assessment of the distribution for \( x_i \) is \( x_{i^*} = x_{i^*} \) with probability \( p_{i^*}, k = 1, \ldots, m \), then that individual will strictly prefer the \( i^{th} \) alternative to the \( j^{th} \) alternative if and only if \( E[U(x_i)] > E[U(x_j)] \) where "\( E \)" denotes the expectation operator [that is, \( E[U(x_i)] = \sum_{i=1}^{m} p_i U(x_{i^*}) \)]. It follows that in selecting among \( n \) mutually exclusive alternatives, the individual will choose the one that has the largest value for \( E[U(x_i)] \).

As is readily apparent, the orderings generated by this maxim are not invariant to a positive monotonic transformation of \( U \) and they are hence represented by a unique additive cardinal utility function. However, by the late 1940s, economic theorists were in general agreement that the nonstochastic theory of consumer choice required only that preference orderings be ordinal and that nothing of operational significance would be added by a cardinal ordering. (Another important commonplace that, as we all know, Samuelson did more than just help to establish.)

Thus, since von Neumann and Morgenstern claimed that their axioms imply a cardinal utility representation for preferences, the validity of the axioms would seem to create a fundamental discontinuity between the nonstochastic and stochastic theories of consumer choice. That is, when events are known with certainty, then only ordinal preferences matter, but when events are uncertain (even a "little bit") cardinality becomes essential. This perhaps explains why the early reaction to these axioms by many economists including Samuelson [1950, I, Chap. 12] was a composite of puzzlement and criticism.

The essence of the puzzlement was that, on the one hand, the von Neumann-Morgenstern axioms were quite "sensible" and on the other hand, their apparent implication of additive cardinal utility was not. There existed at that time many examples of nonstochastic preference orderings that could not be represented in an additive cardinal form, and those that did admit a unique additive cardinal representation required such special assumptions about the marginal rates of substitution among goods that they were regarded as empirically irrelevant. This puzzle was at least in part resolved by Samuelson and others when they recognized the failure of von Neumann and Morgenstern to make explicit the independence axiom (or as Samuelson also called it, their zero\(^{th} \) axiom), which is essential to their system. As Samuelson later described the situation:

Around 1950, Marschak, Dalkey, Nash, and others independently recognized the crucial importance of the independence axiom. Prior to this the Neumann-Morgenstern axioms had puzzled many economists, including myself. A number of interpretations of those
axioms, much like the Manne octane example, has seemed to fulfill the spirit of those axioms and yet to not lead to additive cardinal utility. Marschak, I, and no doubt others had come to suspect that the independence axiom had been implicitly assumed in the pre-axiom concepts of the Games discussion. At the Paris conference, M. Malinvaud presented me with a confirmation of this suspicion. [1952, I, Chap. 14, p. 140, fn. 3]

It was undoubtedly the recognition of this “missing” axiom together with some critical correspondence with L. J. Savage that led to Samuelson’s reconciliation of the expected utility maxim with the nonstochastic theory of consumer choice in his seminal paper, “Probability, Utility, and the Independence Axiom.” As Samuelson summarized it in the second paragraph of that paper (p. 137),

When we come to a theory of consumers’ preference in stochastic situations, the basic methodology is the same [as in nonstochastic situations]. We are alone interested in the “more or less” ordinal relations that determine how the consumer will choose between one uncertain prospect with its “probable prizes” and any other specified uncertain prospect. . . . Recently, Ramsey, de Finetti, and Savage have worked out an interesting theory of what I term “consistent ordinal reactions to uncertainty situations.” . . . If a person is to be “consistent” in dealing with “mutually exclusive” outcomes, we find ourselves postulating for him “strong independence conditions” in the uncertainty realm of the type that we regard as empirically absurd in the nonstochastic realm. It is these strong independence conditions that create the existence of certain special or canonical indices of utility and probability that are “additive.”

In his elaboration of this summary, Samuelson indicates that “mutually exclusive” were the “magic words” that convinced him of a legitimacy of the “strong independence axiom” in the stochastic realm that it does not have in the nonstochastic realm.

Having reconciled the expected utility maxim with the nonstochastic theory of consumer choice, Samuelson endorsed the theory (“Having blown cold concerning the theory, I should like to conclude by again blowing hot” [p. 144]). However, he did so only after expressing his legitimate concerns over being “caught between the Scylla and Charybdis of theoretical formulation and operational empirical hypothesis formation” with respect to the appropriate dimensional space in which the axioms are presumed to be operating. Pushing his point to the extreme, Samuelson noted “If every time you find my axiom falsified, I tell you to go to a space of still higher dimensions, you can legitimately regard my theories as irrefutable and meaningless.” Upon rereading this section of the paper, I was struck by the prophetic nature of the discussion (especially the part that focuses on the use of the axioms in a dynamic framework) because of its connection with the intertemporal consumption choice and portfolio selection problem, a problem that did not even begin to be analyzed until a decade later.

The early development of this problem by Phelps, Hakansson, Leland, Mossin, and, of course, Samuelson, used the method of stochastic dynamic programming

As Samuelson notes in the paper, he was not the only economist to reconcile the axioms with the nonstochastic theory of consumer preferences. True to form as his own most historical critic, in almost the same breath that he used to select this paper for the sampler, he remarked, “I think that Marschak’s formulation was the best.”
together with the assumption that the expected utility maxim applied to preferences for lifetime consumption. Because they all made some rather specialized additional assumptions (for example, intertemporally additive utility, with a single consumption good and serially independent and identically distributed random variables for the returns on investments), the only stochastic argument of the "derived" or indirect utility function was wealth. Therefore, at each point in time, the derived behavior of the intertemporal maximizers in these formulations is operationally indistinguishable from a "static" maximizer whose preferences with respect to "end-of-period" wealth satisfy the von Neumann-Morgenstern axioms. However, as these early analyses were generalized to include the empirically more realistic possibilities of multiple consumption goods with stochastic relative prices and distributions of investment returns that are neither serially independent nor identical, the resulting indirect utility functions have other stochastic arguments in addition to wealth. And, for these more general cases, it is, of course, possible to derive behavior that is consistent with the axioms of the expected utility maxim in the higher dimensional space of lifetime consumption but which is inconsistent with these axioms when applied to the single dimension of "end-of-period" wealth.4

Samuelson's premonitory remarks are thus as pertinent to current research in financial economics as they were in 1952. Indeed, they will undoubtedly become even more pertinent as research on this fundamental problem of choice progresses. Future refinements in the theoretical analysis of the consumption and portfolio selection problem will almost certainly require increasing the dimensional space in which the axioms are assumed to apply. Such progress will therefore, by necessity, make ever finer the distinction between increasing the dimensionality in an attempt to capture more empirically realistic detail and increasing it in an endless attempt to save the theory.

As exemplified by the advice in that ancient and familiar adage, "Don't put all your eggs in one basket," diversification is regarded as sound and prudent behavior by investment practitioner and lawmaker alike and was so regarded long before the advent of the modern theory of portfolio selection. One need hardly do more than contemplate the images associated with the words "gamblers," "plungers," "risklovers," "speculators" (all used to describe those who behave in an antipodal fashion) to note that diversification as normal behavior in investment matters is a widely shared belief. As a reflection of this belief, it is now almost the universal practice for analyses of the portfolio selection problem to exclude as "empirically irrelevant" those preference orderings that do not exhibit this normal behavior but that otherwise satisfy the axioms of the expected utility maxim.

As indicated by its title, Samuelson's "General Proof That Diversification Pays" [1967a, III, Chap. 20] derives the important general conditions under which individual optimal portfolio selection behavior is to diversify. When Samuelson's paper was published in 1967, the Markowitz-Tobin mean-variance theory of portfolio selection was already well developed. This theory does provide specific content to the rule of diversification. From earlier classic insurance examples, it was

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4See Merton, (1982, pp. 654-655) for a discussion of such inconsistencies.
well known that for independently and identically distributed investments, the expected return on the portfolio is the same for all "mixes," and that an equal investment in each will minimize the variance of the return on the portfolio. By restricting investor preferences to "risk averters" (for whom variance in the portfolio’s return is a "bad"), the mean-variance theory demonstrates that such "equal" diversification behavior is indeed optimal.

However, as was well known to Samuelson and the others before 1967, choosing a portfolio on the basis of its mean and variance alone will be consistent with the axioms of the expected utility maxim only under some rather restrictive and unsatisfactory assumptions about both preference orderings and the joint probability distribution of available investments. Despite the intuitive appeal and analytical tractability of the mean-variance theory, many economists were, therefore, understandably skeptical of results derived in this special framework. It is this valid skepticism that makes Samuelson’s general proof of diversification behavior so important to the field even though such behavior had been clearly demonstrated in the mean-variance context. That Samuelson saw this as the central purpose of his general analysis is clear from his statement of intent: "The whole point of this paper is to free the analysis from dependence on means, variances, and covariances" (p. 854).

As we know, it is often difficult to define an intuitive concept such as diversification behavior; to give it meaningful content is not always easy. A seemingly sensible general definition, for example, might be that the investor chooses to take some position in (almost) all available investments. In the absence of restrictions on borrowing and short-selling, a contradiction of such behavior would require that the investor's optimal demand for at least some investments be exactly zero. However, because the conditions under which the investor's demand for an investment will equal zero are so singular, this proposed definition imposes virtually no restrictions on either preferences or the distribution of investment returns. Were such a general definition of diversification behavior adopted, therefore, it would have practically no operational meaning.

In the general proof, Samuelson characteristically deals simply and effectively with this problem of definition. Rather than becoming bogged down in an explicit and necessarily tedious discussion of an appropriate general definition of diversification behavior, he does so implicitly by proceeding through a sequence of assumptions and theorems in an almost inductive manner. Throughout the analysis, moreover, the reader never has cause to lose sight of the central theme of the paper: namely, that the essential behavioral characteristics of portfolio selection can be derived without using the mean-variance model.

Samuelson begins his analysis in the general proof with the limited but agreeable concept of equal diversification and derives the restrictions on preferences and investment distributions required for such behavior. In Theorem I, he proves that if preferences satisfy the axioms of the expected utility maxim with the associated utility function $U$ strictly concave, and if the available investments are independently and identically distributed, then the investor’s optimal behavior will be to invest an equal fraction of his wealth in each investment. This “almost obvious”
theorem (as Samuelson describes it—p. 848) demonstrates the identical "equal" diversification behavior derived in the corresponding mean-variance framework while still avoiding the objectionable assumptions of that framework.

The theorem is almost obvious in the sense that if diversification behavior (as intuitively understood) is ever to be realized, then it surely would be realized for this posited distribution of investment returns. Yet, without the assumption of strict concavity for \( U \), the conclusions of the theorem are, of course, false. Thus, by making the natural but very specific assumption of independently and identically distributed investment returns, in his first case Samuelson establishes concavity (or equivalently, global positive-risk-aversion in the Arrow-Pratt sense) as the essential watershed for preference orderings if diversification behavior is posited to be the norm. Because this concavity restriction permits preference orderings that are not permissible under the risk-aversion restriction required in the comparable mean-variance analysis, Samuelson also establishes that this stronger requirement of the mean-variance model is not essential for diversification behavior.

Having established the watershed restrictions on preferences, Samuelson focuses the analysis on determining conditions for the distribution of investment returns that will ensure diversification behavior. In Theorem II, he shows that equal diversification will still be the optimal rule if the assumption of independence used in Theorem I is replaced by the less restrictive assumption of a symmetric joint distribution that is defined by the cumulative joint density function, \( P(x_1, x_2, \ldots, x_n) \), being a symmetric function of its arguments. However, as Samuelson quickly verifies, "if equal diversification is to be mandatory, symmetry or some assumption like it is of course needed" (p. 851).

Having shown the severe restrictions imposed on the distribution of investment returns by this narrow definition of diversification behavior, Samuelson broadens the definition to positive diversification where investments need only be held in positive (but not necessarily equal) amounts. (As my earlier comments indicate, I believe this to be about as general a meaningful definition of diversification behavior as one is likely to find.) In Theorem III, he shows that if all investments have a common mean and one investment is distributed independently of the rest, then the optimal investor behavior will be to put some, but not all, his wealth in this investment. He also points out two corollaries (p. 853):

*Corollary I.* If any investment has a mean at least as good as any other investment and is independently distributed from all other investments, it must enter positively in the optimal portfolio.

*Corollary II.* If all investments have a common mean and are independently distributed, all must enter positively in the optimum portfolio.

Noting the role of independence in the theorem and its corollaries, Samuelson explores the conditions under which positive diversification becomes mandatory when the strict independence assumption is dropped. By a simple counterexample drawn from mean-variance analysis, he demonstrates that in general, every investment in a group with identical means need not enter with positive weight in the optimum portfolio. Specifically, Samuelson shows that a necessary condition for
positive diversification in his two-investment example is that \( \text{Var}[x_i] \geq \text{Cov}[x_i, x_j] \) and \( \text{Var}[x_j] \geq \text{Cov}[x_i, x_j] \) where \( (x_i, x_j) \) are the respective returns on the two investments posited to have the same means. For sufficiently large positive correlation, this condition will be violated unless the variances of both returns are the same.

Having shown that positive diversification is not mandatory when returns are positively correlated, Samuelson examines the suggestion (which he assigns to Robert Solow) that "abandoning independence in favor of negative correlation ought to improve the case for diversification" (p. 854). As Samuelson points out, it is easy to prove positive diversification in the mean-variance framework for any number of investments with common mean among which all the pair-wise linear correlations are negative. But, to do so in his framework requires a more general and stronger concept of negative interdependence. As an appropriate generalization of pair-wise negative correlation, Samuelson chose the requirement that the conditional cumulative density function of the returns for each of the \( n \) available investments satisfy

\[
\frac{\partial P(x_i | \mathbf{x}_j)}{\partial x_j} > 0, \quad i, j = 1, \ldots, n
\]

where \( P(x_i | \mathbf{x}_j) \) is the conditional probability that the return on investment \( i \) is less than or equal to \( x_i \) when the returns on all other investments are given by the vector of values, \( \mathbf{x}_j \equiv (x_{j,1}, \ldots, x_{j,i-1}, x_{j,i+1}, \ldots, x_{j,n}) \). This definition of negative interdependence leads to Theorem IV, which states that positive diversification is mandatory if all available investments have the same expected return and have this property of negative interdependence. Thus, Samuelson closes his analysis of diversification: "Having now shown that quite general conclusions can be rigorously proved for models that are free of the restrictive assumption that only two moments count, I ought to say a few words about how objectionably special the 2-moment theories are (except for textbook illustration and simple proofs)" (p. 855).

Before discussing these few words of Samuelson's and their significance for the theory of portfolio selection, I digress to point out a characteristic of his style of thought that is handsomely illustrated by the preceding analysis.

That Samuelson closed his study of diversification as he did is typical of a pattern found in many of his most important contributions to financial economics as well as other fields. He opens the gate to a new area of (demonstrated) fruitful research and then leaves it sufficiently open so that others can enter, to build upon his work and determine the detailed outer boundaries of the insight he has provided. In this case of the general proof, he provides the fundamental insight (that the interesting behavioral results derived in the mean-variance context can, in essence, be replicated in the general context of the expected utility maxim) and then presents sufficient analysis to demonstrate its validity using the most straightforward model or example.

That this opening of gates is no idle claim can be exemplified by the following case study. Upon rereading the general proof (this time as carefully as I should have
done in the past) and upon some reflection, I found myself able to make the following extensions to Samuelson's Corollary I and Corollary II (which, to my knowledge, have not been written down elsewhere).

Define the conditional expected return on investment $i$ by $E(x_i | x_i) = \int x_i \cdot P(x_i | x_i) \, dx_i dx_i$, where $P(x_i | x_i)$ is the conditional probability function defined by Samuelson. The extensions to Corollary I and Corollary II respectively can be written as

**Corollary I':** If investment $i$ has a mean at least as good as any other investment, and satisfies

$$\frac{\partial E[x_i | x_i]}{\partial x_j} < 1, \quad \text{for all } j \neq i,$$

then investment $i$ must enter positively in the optimal portfolio.

**Corollary II':** If all investments have a common mean and if, for each $i$, $i = 1, \ldots, n,$

$$\frac{\partial E[x_i | x_i]}{\partial x_j} < 1, \quad \text{for all } j \neq i,$$

then all investments must enter positively in the optimal portfolio.

These corollaries include the cases explicitly analyzed by Samuelson in the general proof. That is, if investment $i$ is independently distributed from all other investments, then $\partial E[x_i | x_i] / \partial x_j = 0$ for all $j \neq i$, and if investment $i$ has negative interdependence (that is, $\partial P(x_i | x_i) / \partial x_j > 0$) with all other investments, then $\partial E[x_i | x_i] / \partial x_j < 0$ for all $j \neq i$. Moreover, Corollary I' shows that, provided that $\partial E[x_i | x_i] / \partial x_j < 1$, positive diversification is mandatory among a group of investments with identical mean, even though there may be generalized positive interdependence among all investments. As Samuelson demonstrated in his mean-variance counterexample, too much positive interdependence will, of course, negate positive diversification. In particular, if $\partial E[x_i | x_i] / \partial x_j > 1$ for all $j \neq i$, then among a group of investments with identical mean, investment $i$ will not be held in a positive amount in the optimal portfolio.

These two corollaries are merely extensions of the analysis in the general proof. They are proved by using the identical procedure used by Samuelson to prove Theorem III. Moreover, in his discussion of why positive diversification need not be mandatory once the independence assumption is dropped, Samuelson pointed out that "Only if, so to speak, the component of an investment that is orthogonal to the rest has an attractive mean can we be sure of wanting it." (p. 853). It was this explanation, along with his definition of negative interdependence, that led me to posit the restriction on the joint probability distribution of returns in terms of the derivatives of the conditional mean.

To see the connection between Samuelson's orthogonality statement and the
conditional mean function most readily, consider the special \((n = 2)\) example of two investments where the mean return to investment \(i\) is denoted by \(\mu_i\) and where it is assumed that \(\frac{\partial E[x_i | x_j]}{\partial x_i} = b_{ij}\), a constant. Define the random variable \(\epsilon_i \equiv x_i - b_{ij}x_j\). Then, by construction, \(x_i = b_{ij}x_j + \epsilon_i\) where \(E(\epsilon_i | x_j) = E(\epsilon_i) = \mu_i - b_{ij} \mu_j\). Because \(\frac{\partial E[\epsilon_i | x_j]}{\partial x_i} = 0\), \(\epsilon_i\) represents the component of investment \(i\) that is orthogonal to investment \(j\). Under the posited conditions of Corollary I, \(\mu_i \geq \mu_j\) and \(b_{ij} < 1\), and therefore, \(E(\epsilon_i) > 0\). Hence, a sufficient condition for the orthogonal component to have an attractive mean is that this mean be positive. Thus, Corollary I' simply confirms Samuelson's discerning statement that it is the mean of the orthogonal component of an investment's return that determines whether or not that investment enters positively in the optimal portfolio.

It is indeed illustrative of the Samuelson style of thought that fourteen years after its first publication, one can still be led by his paper to further extensions of the theory. I need hardly say that the two corollaries presented here by no means exhaust the possibilities for fruitful research along the lines of the analysis laid out in the general proof. And although this is not the occasion to pursue such a development further, I end this digression with the following theorem as an indication of just one direction that such further research might take.

**Theorem**: Let \(x^*\) denote the return on any optimal portfolio with mean \(\mu^*\) and positive variance. Let \(\mu_i\) denote the mean return on investment \(i\) and \(R\) denote the mean return on an investment whose return is orthogonal to \(x^*\).

\[
\begin{align*}
(i) & \quad \text{If } \frac{\partial E[x_i | x^*]}{\partial x_i} > 1, \text{ then } \mu_i > \mu^* \\
(ii) & \quad \text{If } 0 < \frac{\partial E[x_i | x^*]}{\partial x_i} < 1, \text{ then } R < \mu_i < \mu^* \\
(iii) & \quad \text{If } \frac{\partial E[x_i | x^*]}{\partial x_i} < 0, \text{ then } \mu_i < R \\
(iv) & \quad \text{If } \frac{\partial E[x_i | x^*]}{\partial x_i} = b_n \text{ a constant, then } \mu_i - R = b_n(\mu^* - R)
\end{align*}
\]

Obviously, this theorem provides essentially the same type of restrictions on the expected returns of investments as the classic Sharpe-Lintner-Mossin Capital Asset Pricing Model, but does so without that model's dependence on the mean-variance theory of portfolio selection. It is thus just one more example of Samuelson's fundamental insight that all the interesting restrictions implied by the mean-variance model can be derived in the general context of the expected utility maxim.

As noted just before that digression, Samuelson ends the general proof with a critical review of the conditions under which the mean-variance theory of portfolio selection is consistent with the expected utility maxim. He shows that if the mean-variance criterion is to be consistent with (virtually) any joint distribution of investment returns, then preferences must be represented by a strictly concave, quadratic utility function (that is, \(U(x) = x - bx^2, b > 0\)). However, as Samuelson
goes on to note, even this restriction on preferences is not sufficient if the range of possible values of \( x \) can exceed \( 1/2b \). He also shows that consistency with (virtually) any concave utility function requires that the joint distribution of investment returns be Gaussian. Here, too, Samuelson observes that this restriction on investment returns is not sufficient if preferences are permitted that exhibit infinite marginal utility at \( x = 0 \) or at some other subsistence level. Moreover, using the rectangular distribution as a counterexample, Samuelson also dispels the mistaken belief of some that the mean-variance theory is consistent with the expected utility maxim if each investment's return simply belongs to the same 2-parameter (but not necessarily Gaussian) family of probability distributions.

Having demonstrated just how specialized and empirically objectionable the conditions are for the mean-variance theory to be rigorously applicable, Samuelson explores the possibilities of mean-variance as an approximation theory for investor behavior. He casts doubt on the assumption that the Gaussian distribution will, in general, be a reasonable approximation for investment returns by noting that this distribution is wholly inconsistent with limited liability, an important characteristic shared by most financial investments. This doubt is extended even to the asymptotic case of large numbers of investments by Samuelson's demonstration of the treachery involved in the improper interchange of limits that can easily occur in naive applications of the central limit theorem. However, he does point out that the quadratic approximation to \( U(x) \) and its associated mean-variance solution provides asymptotic approximation to optimal investor behavior as the amount of dispersion around each investment's expected return becomes smaller and smaller. Indeed, as we now know from a later Samuelson paper and from the analyses by others of the continuous-time portfolio selection model, this valid asymptotic approximation result provides the primary justification for the use of the mean-variance theory in situations other than textbook examples and illustrations.

That the points made in Samuelson's critical review of the mean-variance theory were important to the field is evident from a collection of three papers on the same topic which appeared in the January 1969 issue of the Review of Economic Studies. Two of these papers, one by Karl Borch and the other by Martin Feldstein, criticize the use of the mean-variance analysis, and the third is a reply to these criticisms by James Tobin. This collection of papers also provides for the historian of science an unfortunate, but dramatic, example of the costs in the velocity of circulation of ideas and in the lost recognition accruing to their author associated with publishing a paper like the general proof in a narrowly circulated journal (which the Journal of Financial and Quantitative Analysis was in 1967). Although all the essential issues raised in the three Review of Economic Studies papers are covered in the general proof, none of these papers cite the coverage. Apparently, even a Samuelson paper can encounter difficulties in making its way into the mainstream when published in a thoroughly obscure journal.

There is little doubt that the Borch-Feldstein-Tobin interchange as well as the development of the continuous-time model with its "mean-variance-like" simplicities provided the stimulus for Samuelson's "The Fundamental Approximation Theorem of Portfolio Analysis in Terms of Means, Variances, and Higher
Moments." [1970a, III, Chap. 203]. In this expansion on his discussion in
the general proof, the focus is on the usefulness of the quadratic approximation when
the dispersion of investment returns is small. As Samuelson put it, "In a sense, therefore, it provides a defence of mean-variance analysis—in my judgment the
most weighty defence yet given" (p. 877). Indeed, this paper remains the definitive
statement on the valid use of mean-variance analysis, and for that matter, higher-
moment analyses, as approximation theories for optimal investor behavior.

The essential requirement of Samuelson’s "Fundamental Approximation
Theorem" is that the distribution of investment returns has (what he calls)
"compact" probabilities. As a reasonably general example, suppose that the random
variable return per dollar on investment $i, x_i$, can be written in terms of a parameter
$\sigma$ as

$$ x_i = \mu + \sigma^2 a_i + \sigma \epsilon_i $$

where $\mu$ and $a_i$ are nonstochastic and as a function of $\sigma$, are $O(1)$; and where the
random variable $\epsilon_i$ has the properties that (i) $E(\epsilon_i) = 0$; (ii) $E(\epsilon_i^2) = \sigma^2 > 0$; (iii)
$E[\epsilon_i^{4+k}] < \infty$, for $k = 1, 2, \ldots, r$. Then, in the asymptotic sense as $\sigma \to 0$, $x_i$ will
have a compact distribution.

Let $\omega_i(\sigma)$ denote the optimal fraction of an investor's portfolio allocated to
investment $i$, which is derived as the solution to

$$ \max_{\omega} \left\{ E \left[ U \left( \sum_1^n \omega_i(\sigma) x_i \right) \right] \right\} $$

subject, of course, to the constraint that

$$ \sum_1^n \omega_i(\sigma) = 1. $$

Let $\omega^{**}(\sigma)$ denote the optimal solution to the "r-moment problem"

$$ \max_{\omega^{**}} \left\{ \sum_1^n \left[ U(\sigma)(\mu) \right] E \left[ \sum_1^n \omega^{**}_i(\sigma) (x_i - \mu) \right] \right\} $$

Then, for $[x_i]$ with compact distributions, Samuelson's fundamental theorem
(p. 880) states that:

The solution to the general problem $[\omega_i(\sigma)]$ is related asymptotically to that of the
$r$-moment problem $[\omega^{**}_i(\sigma)]$ by the high-contact equivalences $\omega_i(0) = \omega_i^{**}(0)$, $\omega'_i(0) = \omega_i'^{**}(0)$, $\ldots$, $\omega^{(r-2)}_i(0) = \omega^{(r-2)}_i(0)$. Not only does this theorem show that the true optimal solution and the solution to
the associated $r$-moment problem converge in the limit, but it also provides bounds
for the errors in using this approximating solution for small, but finite, values of $\sigma$. That is, $|\omega^* - \omega| = o(\sigma^{-2})$ where $o(\cdot)$ is the usual asymptotic order symbol.\(^5\)

The special case of the fundamental theorem when $r = 2$ is, of course, the quadratic approximation associated with the mean-variance solution. This special case (highlighted by Samuelson as a separate theorem) shows that, indeed, the mean-variance solution and the true optimal solution will converge and the error in using the mean-variance solution will be $o(1)$.

As noted by Samuelson, a prototype example of his compact distributions is a Wiener process or Brownian motion with a drift where the parameter $\sigma$ is identified as the square root of time. Thus, his analysis “does throw light on the reasons why enormous ‘quadratic’ simplicities occur in continuous-time models” (p. 879) where the processes are posited to be continuous sample-path diffusions and the length of time between transactions is the infinitesimal $dt = \sigma^2$.

I need hardly do more than mention the “Sharpe-Lintner-Mossin Capital Asset Pricing Model” to underscore the significance of Samuelson’s approximation theorem to financial economic theory. Moreover, it is, if anything, even more important to empirical financial research and financial practice, both of which have focused almost exclusively on the mean-variance type of analyses. Of course, Samuelson is not alone in having pointed out the perils associated with this mode of analysis. However, unlike the blanket condemnation implicit in the simple counterexamples used by purists to make their point, the error bounds of Samuelson’s theorem provide selective condemnation and thereby permit the empirical economist and practitioner alike to judge the merits of their analyses in each individual case. If, for example, the time between observations and the time interval between (potential) revisions of a portfolio are “sufficiently short,” then the Samuelson theorem provides a justification for using the mean-variance specification as a reasonable approximation. As Samuelson summarizes it in the closing sentences of his paper, “But it also needs emphasizing that near to $\sigma = 0$ when ‘risk is quite limited,’ the mean-variance result is a very good approximation. When the heat of the controversy dissipates, that I think will be generally agreed on” (p. 882).

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

\(^5\)That is, if $g(\sigma) = o(h(\sigma))$, then $\lim[g(\sigma)/h(\sigma)] = 0$ as $\sigma \to 0$. If $g(\sigma) = 0(h(\sigma))$, then $\lim[g(\sigma)/h(\sigma)]$ is bounded.
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 nonmarket measures of value in arriving 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 carryover 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.

As Samuelson notes, without the transversality or other terminal boundary condition, these local arbitrage conditions are necessary but not sufficient to ensure an optimal path.
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.

As these papers illustrate, the analysis of such price dynamics in an uncertain environment is considerably more difficult, both conceptually and analytically, than in the certainty case. And given the substantive importance of such analyses to the field, it is not surprising that Samuelson chose the mathematics of speculative price as his subject for the twelfth John von Neumann Lecture at the 1971 fall meeting of the Society for Industrial and Applied Mathematics. As part of this lecture (published as "Mathematics of Speculative Price") he presents a molecular model of the firm that he uses to connect optimal portfolio behavior in the financial markets with "real" investment characteristics of firms. With this model (that has the same characteristic simplicity of, for example, his exact consumption-loan model), Samuelson once again opens a gate to a new research area. However, on this occasion, his central purpose is the closing of gates to earlier research through a critical synthesis of a wide range of topics in capital market theory. As it happens, Samuelson's synthesis reviews the three papers that remain in our miniaturized Samuelson Sampler, thus making it happily unnecessary for this celebrative essay to do more than make a few passing observations on these seminal pieces.

In defending his Prologue model of speculative price against his own criticism of excluding uncertainty, Samuelson expressed the belief "that conquering the easier problem, in which future conditions are for simplicity's sake assumed to be foreseeable and foreseen, will provide a useful springboard from which to attack the realistic speculative market problems" (p. 947). Fourteen years later in "Stochastic Speculative Price," he returned to that analysis and demonstrated the validity of this defense. In this elegant paper requiring little more than two pages in print, Samuelson shows that under the axiom that the expected rather than the known-for-certain prices can be substituted in the local arbitrage conditions, the stochastic dynamic behavior of the spot price is determined as the solution to a stochastic dynamic-programming problem. That is, if the spot price $p_t$ for each $t$ satisfies $q_t(E(p_{t+1})/p_t - (1 + r)/a) = 0$ where the carryover quantity $q_t$ is positive only if the expected return from such storage is adequate to cover both capital and shrinkage costs, $1 + r$ and $1 - a$, then the dynamic path for $p_t$ is given by the solution to

$$\max E \left\{ \sum_{i} (1 + r)^t U[H_t + aq_{t+1} - q_t] \right\}$$

where $H_t$ is the uncertain harvest at time $t$ and the function $U$ is defined by
$U'[c] = P[c]$ where $P[c]$ is the function relating consumption demand at time $t, c_t$, to price.

This relation reduces to exactly the one derived in Samuelson's prologue model when the harvests are nonstochastic and the expectation operator, $E$, becomes vacuous. Moreover, even if demand functions are stationary and harvests are independent and identically distributed over time, the resulting changes in the spot price will be neither independently nor identically distributed because of the intertemporal linkages caused by the optimal carryover decisions. However, the general properties of this optimal carryover rule are consistent with those that a technocratic planner would prescribe. The derived time series characteristics of spot prices are also broadly consistent with those observed for real life organized commodity markets and as $T \to \infty$, the spot price does have an ergodic distribution.

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 important 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 Fama 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 recent 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. Further, 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.) (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. (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 signalling 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, Chapt. 240] he is keenly aware of the omnipresent 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 \(\lambda\), a diminishing sequence in function of the diminishing variance to be expected of a futures contract at its horizon shrinks, subject to perhaps a terminal jump in \(\lambda_t\) 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 the 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 martingale 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.

Samuelson not only exercises great theoretical care himself, but he also tries to induce such care in his readers. 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].

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 \( \Phi \), then these price changes will also satisfy the martingale property for the distribution generated by any information set \( \Phi' \) that is a subset of \( \Phi \). It therefore follows that if these prices do not satisfy the martingale property for information set \( \Phi' \), they will not satisfy this property for any information set \( \Phi \) that contains \( \Phi' \) 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. Moreover, as he points out, virtually all empirical studies of speculative price returns (both pre- and post-“Proof”) can be viewed as tests of this property that serve to underscore 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 these 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 semipopular 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" [1974d, IV, Chap. 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," (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.

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 intertemporal analysis and option pricing." Once again, Samuelson was ahead of the field in recognizing 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 Kruizenga'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.

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 $S$ 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

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

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

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

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 warrants ($T = \infty$), 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, $\log[X_{t+T}/X_t]$, 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, $\alpha$, Samuelson derives a partial differential equation for the option price given by

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

subject to $W(X; 0, a) = \text{Max}[0, X - a]$ where $\beta$ 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 $h_1 = [\log(X/a) + (\alpha + 1/2\sigma^2)T]/\sigma\sqrt{T}$ and $h_2 = h_1 - \sigma^2 T$.

Even when $\alpha = 0$, 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. 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 $C_T < \infty$ such that $W(X; T, a) < X - a$ for all $X \geq C_T$. Thus, for $\beta > \alpha$, there is always a finite price for the stock where it pays to exercise prior to the expiration date, and hence, the American feature of an option has positive value. He also shows that for $\beta > \alpha$, $W(X; T, a) < X$ 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) $W(C_T, T, a) = C_T - a$; (iv) $W_X(C_T; T, a) = 1$. For those familiar with parabolic partial differential equations of this type, it may appear that the boundary conditions are overspecified. However, $C_T$, which is the time boundary of stock prices where the option should be exercised, is not known, and it is precisely the overspecification 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 today.

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, $\alpha$ 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, $\delta$, 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] (I was the 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
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

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. 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

$$\frac{1}{2} \sigma^2 X^2 F_{xx}(X; T; a) + r X F_x(X; T; a) - r F(X; T; a) - F_t(X; T; a) = 0$$

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 $\alpha$ 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 $\alpha$) 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, $\alpha$, or the required expected return
on the option, $\beta$. 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 $\alpha$ is known and constant, $\beta$ will not be for finite-level options priced
according to the Black and Scholes methodology. It should also be noted that

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7In 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 in a
recent conversation with me, Paul commented on his 1968 review as one in which "I won farthings and
lost pounds."
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 has actually added to the significance of Samuelson's "Rational Theory" for the field, yet I believe it has. 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," 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) 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!

As is readily apparent from even the small subset of his writings selected for this sampler in financial economics, as 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.

As noted at the outset of this Introduction, 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 eleven editions of his basic textbook on which so many
practitioners were reared. Nevertheless, to emphasize the significance of just the one
path explored here, I note only that in their widely adopted textbook for MBA
students in financial management, Principles of Corporate Finance, Richard
Brealey and Stewart Myers conclude with their list of the five most important ideas
in finance. Three on the list are: the mean-variance capital asset pricing model,
efficient market theory, and option theory.
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 [1974e, 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.

AFTERWORD
Space, not source material, is the scarce resource in this chapter. While the six
articles reviewed in this sampler are among Paul Samuelson’s most important
contributions to financial economics, they surely do not exhaust the set. Picking a
cutoff under such conditions is no easy task. I am indeed indebted to Paul for having taken on this burden and for being so selective. Had the choices been mine, I would have violated the budget constraint by including at least half a dozen more. One of these is “Efficient Portfolio Selection for Pareto-Lévy Investments” [1967b, III, Chap. 202], where Samuelson was the first to solve the optimal portfolio choice problem when investments have stable distributions with infinite variances. Another is his important “Optimality of Sluggish Predictors under Ergodic Probabilities” [1976a, IV, Chap. 249], which could be claimed to be within the permeable and flexible boundaries of financial economics.

Although the quite proper focus here is on Samuelson’s many new discoveries, 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 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. 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” [1974b, 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.”

This diligence and discrimination along with the strong opinions and decisive language so characteristic of Samuelson’s writings might be mistaken by some as dogmatism and inflexibility. He has written much about occasional dogmatism among economists and Paul Samuelson practices what he preaches. It is indeed his willingness to change his own views and admit to errors that makes his steadfastness on some issues so credible. No better example of this willingness can be found than in his early work on the von Neumann-Morgenstern Expected Utility Theorem. Samuelson’s first paper on their theorem, “Probability and the Attempts to Measure Utility” [1950, I, Chap. 12], is sharply critical of the required cardinality property of the preference orderings. In this paper, he sowed the seeds for what was
to be called the “zeroth axiom” in his seminal 1952 paper included in this sampler, but his early criticism went too far. As he indicates in the later paper (and in a still later 1965 postscript in his collected scientific papers), he reversed his view and was converted. Samuelson’s record here as elsewhere in his writings would provide a favorable data point for establishing the empirical validity of the astrophysicist John A. Wheeler’s injunction for prime researchers: “Make yours mistakes fast, and you will more often than not be the first to find your errors.”

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-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 fourth volume with 944 pages. Simple extrapolation (along with a glance at his list of publications since 1976) assures us the the fifth volume cannot be far away.

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 in this sampler and all but six of his thirty contributions to our branch of economics were published after he had reached the age of 50.

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 this afterword with a few observations (drawn as his student, colleague, and coresearcher) 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. Finally, although often masked by the apparent ease with which he produces, there is his diligence. Paul works 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 I.E.A.) 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 to 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 and still often does). 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.

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