FINANCIAL ECONOMICS

Essays in Honor of Paul Cootner

William F. Sharpe
Cathryn M. Cootner

FINANCIAL ECONOMICS: *Essays in Honor of Paul Cootner*

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PREFACE

In 1950 the intersection of the fields of finance and economics was small indeed. Academic work in finance relied more often on rules of thumb and anecdotal evidence than on theory and adequate empirical studies. Economists showed only fleeting interest in financial institutions, speculation, and the host of other aspects of uncertainty that comprise much of the domain of the field of finance.

Three decades later the situation is radically different. There is now a rich body of theory relevant to problems in finance; and extensive empirical tests have been conducted to see how well the theoretical constructs accord with reality. It is not overstating the case to say that the field was revolutionized between 1950 and 1980.

Many of those who helped bring about the changes in finance were trained as economists. They approached finance problems with the attitudes and the standard tools of the economist. When they found the paradigms of traditional economics inadequate for the subject at hand, they invented new approaches. But throughout, their style was that of the economist. As a result, we now have a domain increasingly referred to as *Financial Economics.* As with many academic disciplines, an attempt to provide a careful definition would be foolhardy. But the term is widely used. Many recent holders of Ph.D.'s in finance and economics call themselves financial economists. There is a journal of financial economics, and several graduate programs offer degree programs in financial economics.

Paul Cootner was one of the first and one of the best financial economists. No one did more to create and develop the field, and in this respect he can truly be seen as a revolutionary.

This book is intended as a tribute to Paul and to his contributions to finance, economics, and financial economics. The authors include Paul's colleagues, students, and friends (and, in a real sense, all could be included in every one of the three categories). A key paper based on Paul's last work is also included because it is of great importance and because it represents some of his finest work. It also serves as a reminder of the great loss associated with Paul's death and the great contributions he made during his life.

Detailed descriptions of the papers are unnecessary. But a brief overview may prove desirable.

In the initial essay David Pyle provides a review of Paul's work. Since it was of such wide scope and appeared in diverse places, this was not an easy task. The paper is an important summary of Paul's work and shows the breadth of his interests and abilities.

The first part of the book contains two papers dealing with important aspects of major paradigms used in financial economics. In the first, Robert Merton analyzes both the mathematics and the economics of the now widely used continuous-time models. Paul Cootner was one of the first to use such a model and his final work relied heavily on the approach. In the second paper Stephen Ross provides a much more general basis for the now traditional mean-variance approach than is typically used in its derivation. Financial economics employs several paradigms at present—some discrete, others continuous, some based on utility functions, others on arbitrage relationships, some based on complete markets, others on incomplete markets. But, as these papers help illustrate, the approaches have many similarities.

The next writing in the book consists of the summary of Paul Cootner's major work on capital asset pricing in a general equilibrium framework. The paper was prepared by David Pyle using material from several manuscripts prepared by Paul. In the 1960s and 1970s far too many were willing to take future security prices as primitive values in theories of capital asset pricing. Paul insisted on starting on a more fundamental level—with uncertainty about production and consumer demand. As the paper shows, he made substantial progress on this important path.

The second part of the book includes six papers in financial economics. In the first, Paul Samuelson reminisces about Paul Cootner and returns to one of Paul Cootner's early ideas—that security prices may move randomly, but only within a range bounded by "reflecting barriers." The issue is not simple, as the paper shows. But it provides a detailed discussion of the issues and shows that Paul Cootner was right in questioning the easy and often-used assumption that security prices follow a truly random walk.

The paper by Joseph Stiglitz deals with the central issues associated with information and capital markets. It starts with the seeming paradox that if the market were perfectly efficient it would not pay any investor to get information, and thus security prices could easily depart from appropriate values, leading to market inefficiency. With information-gathering an endogenous variable, capital market models become both more complex and more realistic. As the paper shows, such an approach leads to a host of paradoxes, but it also provides sensible explanations for otherwise inexplicable phenomena.

The paper by Stephen Schaefer deals with a subject that greatly interested Paul Cootner—the effects of taxes on investor behavior and, ultimately, on equilibrium prices and holdings in security markets. The paper shows that those hoping to understand economies with the type of tax structures typical of most Western countries may have to abandon the luxury of using the traditional and highly convenient assumptions of complete and frictionless markets.

Possible impacts of taxes are also the focus of the paper by Merton Miller and Myron Scholes. They examine several popular executive compensation schemes to see which ones might be advantageous to all parties involved with the exception of the tax collector.

The goal is to identify procedures that do not appear to be tax motivated, since such arrangements are the clearest candidates for tests of the newer models dealing with contractual arrangements designed to provide incentives for agents (managers) to act in a manner consistent with the welfare of the principals (owners).

The paper by Michael Brennan and Eduardo Schwartz deals with the valuation of a particular type of bond and with the optimal strategy to be followed by the holder of such a bond concerning its redemption. Their approach is quite general and offers a practical and promising method for valuation of many other instruments.

In the last paper in this section, William Sharpe offers a possible reconciliation of a widespread practice of security research firms with the tenets of the part of financial economics known as portfolio theory. While the approach may be an excessively charitable interpretation of the behavior of many such firms, the paper does provide some suggestions for those who wish to continue the practice while operating in a more rigorous manner.

The final part of the book contains three papers that deal more with economics per se than with financial economics.

In the first paper, Yoram Barzel considers the need to separate income and substitution effects when testing the law of demand. He concludes that the most desirable setting for such a test is provided when the consumer's consumption mix is the same as his or her endowment. Given price uncertainty, consumers may choose investments which provide "endowments" of just this sort, since they will then be better off if relative consumer good prices change in any manner. The implication is that those who choose to hedge in this way are likely subjects for a revealing test of the law of demand—a clear case of financial economics providing assistance to traditional economics.

The second paper in this section concerns a subject which interested Paul Cootner from the start of his career. Robert Solow provides an assessment of the theory of economic growth and concludes that the approach is "just about played out." He ends by suggesting that what is needed is a theory in which disequilibria can arise endogenously. The comparison may be forced, but the desire to produce a model in which fundamental uncertainty about, for example, consumer demand, can lead to business cycles is one that Paul Cootner shared; his work on capital asset prices in a general equilibrium framework was designed to lead toward that goal.

The book concludes with a paper that Paul greatly enjoyed. Here, George Stigler (at his best) deals humorously with issues about which one may presume he feels very strongly. (Students of the Stiglerian wit will note that he has provided a slightly altered ending in this version.) It is fitting that the book ends with this paper, for Paul also felt strongly about these issues. Indeed, many of Paul's opinions were very strongly held. But he too had wit and humor. Once, after being introduced to an audience by a successful businessman who asked rhetorically, "If you're so smart, why aren't you rich?", Paul responded, "If you're so rich, why aren't you smart?"

Paul Cootner was a pioneer who helped create and define an important discipline. He was an academic's academic with a complete sense of the practical as well. This book represents a small token of appreciation by some of his friends for all that he did for us and for our profession.

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ISSUES IN ECONOMICS

THE WORKS OF PAUL H. COOTNER: A REVIEW ESSAY

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The essays in this book were written to honor Paul Cootner, but it is Paul's scholarship and the scholarship that he encouraged that will be his best memorial. Paul's interests in economics and finance were broad. In his varied professional contributions one finds a consistent pattern: the insistence on a sound theoretical basis for the analysis and a careful consideration of all of the important evidence on the subject, including both historical data and the details of informed practice.

Paul's view of the proper relationship between economic theory and business practice was captured in the introduction to one of his Stanford course descriptions:

> This course is aimed at what I would call innovative managers: the people who . . . want to know how things might be done optimally so that they might devise innovative policies—profitable to them and to their firms—that would change the way things are done. What I plan to teach is not what is often derogatorily known as "pure theory"—i.e., stuff too abstract to be

**Willis H. Booth Professor of Banking and Finance. I am grateful to James Hoag and William Sharpe for help in preparing this review. The final draft of this paper was written while I was visiting at the Division of Banking Research and Economic Analysis, Comptroller of the Currency, Washington, D.C.; I am grateful for their support. Patricia Murphy prepared the typescript.*

> useful—but instead a useful picture of how things would be done if stodginess and inappropriate wisdom could be overcome.
> Theory is not the problem in unrealistic courses: it is *bad* theory which causes difficulties.[1]

This comment to M.B.A. students came rather late in Paul's lamentably shortened career. The attitude it reflects is apparent in his earliest professional work.

ECONOMIC HISTORY

Paul Cootner's unpublished thesis (1953) and two published papers flowing from that thesis (1963d, 1965b) dealt with innovation and economic development based on a detailed analysis of railroad investment in the United States from 1826 through 1886. This work was in the spirit of the "new economic history" in developing an explicit economic model—one founded in neoclassical economic theory—with which to confront the historical record. Paul's virtually encyclopedic knowledge of the history of U.S. railroads suggested to him that U.S. railroad investment in the nineteenth century could not reasonably be viewed as a continuous process of expansion. Rather the economic model had to be capable of explaining the repetitive swings between passenger and general freight traffic investment in the East and bulk traffic and trunk-line investment in the West and South.

By incorporating adjustment lags (for reasons of technology and uncertainty) into a neoclassical investment model and by considering worldwide shifts in the demand for those final goods and services that could be produced efficiently in this country, Paul developed a coherent and effective challenge to the prevailing, Schumpeterian view that railroad innovation had played a critical role in initiating economic cycles:

> It is clear, I think, from what has gone before that I find *no* evidence anywhere in the process of railroad development of anything that would constitute a technical jump great enough to generate an exogenous railroad boom, the reverberations of which might cause a business cycle to develop . . . .
> Partly because railroads were steadily improved and partly because American economic development gradually filled up the land-masses which could be reached by cheap water routes, successive business cycles were marked by increasing railroad investment. The process is not, however, one in which railroads are continually breaking into new uses from which they had been barred by either ignorance or conservatism. Instead, we see them built in the East . . . then neglected in that role when interest shifts West, only to be revived again. . . . It is demand rather than supply which seems to dominate railroad history, and the demands . . . [were] international in scope.[2]

[1]Course description for B.S. 329, Financial Intermediation, Graduate School of Business, Stanford University, undated.

[2]Paul Cootner, "The Role of Railroads in United States Economic Growth," *Journal of Economic History*, 23 (December 1963), 504–5.

Paul's views on the role of economic theory in understanding economic data are further illustrated in the context of economic history by his reviews of two books on railroad history by Fogel (1961a, 1965c) and his review of the classic study of U.S. monetary history by Friedman and Schwartz (1966b). Although he found areas of disagreement with these authors (indeed, quite a lot to disagree with regarding the selection of tests and the standards of testing in the Friedman-Schwartz study), Paul was unstinting in his praise of "good analysis, not papier maché formalism"[3] and "the end to the artificial distinction between the principles taught in economic theory courses and the data-gathering or idle speculation that used to be called economic history."[4] It is a hallmark of Paul Cootner's subsequent work that the underlying economic principles were stated and used.

ECONOMIC DEVELOPMENT

The insights that Paul gained from the study of innovation and economic growth in nineteenth century America supported his analysis of economic development in a broader context (1963c). In "Social Overhead Capital and Economic Growth," which was first presented in 1960, Paul considered the existing theories of the role of social overhead capital in the process of economic growth and found them wanting. In the existing theory it was suggested that "pecuniary external economies" would lead to underinvestment in important types of capital (such as transportation and electric power generation) and hence to the need for governmental intervention to insure the appropriate investment in social overhead capital. Paul was able to show that the existing theory depended crucially on imperfect foresight. He proposed an alternative model in which it was assumed that ". . . entrepreneurs forecast future prices and their *mean* expectations about the future are always correct . . . that these forecasts are subject to some variance, and that entrepreneurs are risk averse."[5] In short, in 1960 Paul used what would now be called a rational expectations viewpoint to model a problem in economic development. In doing so, he was able to show that the prevailing view of pecuniary external economies was equivalent to no risk taking by entrepreneurs while the standard neoclassical view, in which social overhead capital poses no problem, was equivalent to risk neutrality. Using this richer model of entrepreneurial behavior, he developed the view that is now generally accepted that ". . . the concept of pecuniary external economies includes all situations in which imperfect knowledge of the future reduces the scale of investment and the responsiveness of the economic process to the

[3]Paul Cootner, "A Monetary History of the United States 1867–1960 (by Milton Friedman and Anna Jacobson Schwartz): A Review Essay," *History and Theory,* 5 (1966), 100–108.

[4]Paul Cootner, "Railroads and American Economic Growth: Essays in Economic History (by Robert W. Fogel): A Book Review," *National Banking Review,* 2 (June), 612.

[5]Paul Cootner, "Social Overhead Capital and Economic Growth," in *The Economics of TakeOff into Sustained Growth,* ed. Walt W. Rostow (London: Macmillan), p. 266.

signals of a price system. . . ."[6] This implied a concommitant reduction in the special role assigned to social overhead capital in economic development.

SPECULATION AND HEDGING

Paul's work on railroad investment caused him to consider the shifting demand for U.S. products—and in particular agricultural products—in the nineteenth century. Knowledge of commodity price behavior gained in this manner and his strong interest in the economics of uncertainty led to an abiding interest in commodity price behavior and in speculation and hedging. His published work on this topic began with the famous exchange with Lester Telser (1960a, 1960b) on the nature of returns to speculators. The original analysis presented in this note, along with interpretations of the major contributions by other scholars, were incorporated in a major paper that he read at a Food Research Institute Symposium at Stanford (1967a) and in a succinct, but highly informative, article for the *International Encyclopedia of the Social Sciences* (1968a).

Paul's work on speculation and hedging is one of his major contributions to the economic literature. No one who reads the Food Research Institute study can fail to be impressed by his masterful summary of a difficult literature. He also developed two major ideas that all serious students of speculation and hedging have had to consider.

An appreciation of the role of hedging in managing the risk faced by the holders of commodity inventories, plus careful observation of informed practice, led Paul to conclude that optimal hedging strategies need not result in hedgers being short commodity futures over the entire harvest cycle. After the harvest, when inventories are large, holders of inventories can reduce the per unit risk of holding those inventories by going short in the futures market for that commodity (or a related commodity). However, if inventories become sufficiently small before an uncertain harvest, producers and merchants can reduce their risk by going long in the futures market. Paul argued that failure to take this potential shift in hedgers' strategies (and therefore in the risk premium available to speculators) into account would result in smaller perceived profits than those that would actually be obtainable by informed speculators. He presented considerable empirical evidence in support of this view. In the *International Encyclopedia* this result and its implications for futures prices were summed up as follows:

> If speculators do make money, futures prices must rise during the period [hedgers] are short futures. Since hedgers are usually short and speculators usually long, Keynes argued that futures prices will normally rise over the lifetime of each contract. More recently Cootner has shown that in agricultural commodities hedgers are frequently long (and speculators are short) in the period prior to harvest when inventories are low. In cases where that pattern

[6] Ibid., 281.

> usually obtains, if speculators are to profit, futures prices must fall prior to harvest and rise thereafter. In short, payment of risk premiums would imply a seasonal pattern for futures prices.[7]

Paul's work played a major part in developing a second important concept in the theory of speculation: the role of related futures contracts in limiting the risk premium that has to be paid on any given contract. That is to say, he brought the concept of nondiversifiable risk to bear upon the question of speculators' risk. Paul noted that when a speculator could obtain a futures contract that was positively correlated with another futures contract, a spread position (long one contract and short the other) would have less risk than either contract alone. The empirical evidence that he brought to bear on this question suggested that risk premiums were only paid for the nondiversifiable risks of commodity speculation.

Paul also wrote a series of papers drawing analogies between commodity speculation and hedging and these operations in financial markets (1961b, 1963a, 1972a). When he died, Paul left drafts of work in progress that extend his ideas about the determinants of commodity price behavior and offer insights into the relationship between real and financial markets. The economics literature is poorer because these manuscripts are incomplete.

FINANCIAL INSTITUTIONS

Paul Cootner's interest in the economics of financial institutions developed along with his interest in financial speculation and hedging. The explicit model used in "Speculation in the Government Securities Market" (1963a) is a model of bank liquidity. A study of commercial banking, "The Structure of Competition in Commercial Banking in the United States" (1963b), carried out with Deane Carson, combines a short review of U.S. banking history with an extensive policy analysis of commercial banking structure and competition as they found it in 1960.

The paper, "The Liquidity of the Savings and Loan Industry" (1969), was part of a larger study that was undertaken after the 1966 liquidity crisis in this industry. In this paper Paul discussed the initial attempts by the Federal Home Loan Bank to use its advances policies to induce savings and loan associations to limit the rates they paid on savings deposits. This ill-fated policy which was started in early 1965 began to fail later that year. In December 1965, the Federal Reserve Board permitted an increase on the rate payable on certificates of deposit at member banks. As most readers know, this was just the beginning of the attempts at savings interest rate controls and the concommitant strains on the liquidity of savings institutions. Paul would surely be pleased that the Congress

[7]Paul Cootner, "Speculation, Hedging, and Arbitrage," in *International Encyclopedia of the Social Sciences*, ed. David L. Sills, 15 (New York: Macmillan), 119.

in 1980 has finally started to dismantle the controls that he argued against more than 10 years earlier:

> Whatever merits one may find in rate regulation, it cannot be accomplished by one sector of a highly competitive market. It is true that when the regulation was first attempted, neither the commercial bank sector nor the debt securities markets were as competitive as they gradually became, and so the prospects for success were more sanguine at the outset than they were in retrospect—but that is an outstanding characteristic of partial price regulation. . . . There is some serious question about the merits of attempting to control soundness by . . . rate controls because of its tremendous impact on liquidity problems. Only an a priori unlikely combination of [events] could have caused the strain on associations that occurred [in 1966]. The occurrence of this strain, however, indicates how sensitive a tool rate controls are, and how important it would be to find other means of solving the problems of association soundness.[8]

The paper on savings and loan liquidity includes a discussion of the liquidity problem of financial institutions which carefully distinguishes this problem from the problems of association soundness and the stability of the supply of intermediation services. Many current discussions of financial institutions would benefit if the spokesmen and writers on the topic had read Paul's paper.

The book that Paul Cootner wrote with William Baxter and Kenneth Scott on *Retail Banking in the Electronic Age* (1977b) contains a model of retail banking that should provide the basis for much of the future analysis of this sector of financial markets. A major insight in this study was the realization that the production of retail banking services requires substantial inputs by the user of those services, and that the neoclassical theory of the firm would have to be modified to take this fact into account in order to provide a suitable model of the retail banking firm. The simple model the authors developed using this insight goes a long way toward meeting this goal. In the process the model helps explain some existing empirical anomolies in bank cost studies and in the pricing of deposit balances.

The book begins with a review of banking history. After developing a model of the retail banking firm, the authors use it to analyze electronic funds transfer as in innovation in the provision of deposit services. This discussion includes the same skepticism about the "EFTS Revolution" that Paul had developed earlier regarding the revolutionary nature of innovation. His insistence in his thesis that canals and turnpikes were important in limiting the scope and pace of railroad innovation is surely echoed here in the discussion of competitive means of payment (checks, credit cards) as limits on the pace and scope of innovation in the provision of deposit services: "Nothing in history suggests that any of these innovations [in means of payment], once adopted, immediately

[8]Paul Cootner, "The Liquidity of the Savings and Loan Industry," *Study of the Savings and Loan Industry* (Washington, D.C.: Federal Home Loan Bank Board), p. 289.

displaced its predecessor methods."[9] There is little doubt that this small book is more important than its narrow scope might imply, both as a model of how to study a problem in banking and for the theory of the banking firm that it contains.

INDUSTRY STUDIES

Paul's research also included studies of other nonfinancial industries. The published record contains a study with George Lof on the demand for and supply of thermal efficiency in steam electric generation (1965a), a major study with Daniel Holland of risk and rate of return as it relates to utility regulation (1963f, 1970), a comprehensive econometric model with Franklin Fisher and Martin Neil Baily of the world copper industry (1972b), and a study of inefficiency in the modern university (1974). Perhaps of equal importance, Paul made major contributions to legal and regulatory deliberations on security regulation, utility regulation, drug pricing (1968b), and other topics. This included influential testimony that helped eliminate cartels in commodity and security brokerage. As a former M.I.T. colleague has noted, Paul also had considerable influence on other scholars concerned with markets and their regulation:

> In the tradition of George Stigler at Chicago or John Meyer at Harvard, Paul Cootner used his analytical skills on all kinds of market phenomena—indeed he was the first to do so at MIT with sustained rigor on both the theoretical and empirical sides. His work on gas pipeline, pharmaceutical, and copper prices set governmental policies in those industries. His abilities to move from finance to economic theory, between MIT and the Harvard Business School, while stirring the pot at both places, has not been equalled.[10]

INVESTMENTS

For many readers of this book, Paul Cootner will be best known for his work on investments even though his bibliography is not dominated by works on this topic (1962a, 1964a, 1964b, 1966a, 1967b, 1972c, 1977a, 1978). Paul's interest in investments followed naturally from his early work on commodity price speculation and hedging. As a result of that work, he was one of the first economists to recognize the importance of what was then largely statistical analyses of stock price movements and to begin to provide a theoretical foundation for the apparently random behavior of stock price changes. In his first paper on stock market prices (1962a) Paul provided an operational definition of market efficiency:

[9]Paul Cootner, William F. Baxter, and Kenneth E. Scott, *Retail Banking in the Electronic Age: The Law and Economics of Electronic Funds Transfer* (Montclair, N.J.: Allanheld, Osmun & Co.), p. 6.
[10]Paul W. MacAvoy, private correspondence.

> The stock market is not a random walk . . . and I think I have enough evidence to demonstrate the nature of its imperfections. But, I do not believe that the market is grossly imperfect. In fact, I do not know why the process . . . is not worthy of the name perfection. It strays from perfection only to the extent that it defines . . . profitless speculation.[11]

Paul's initial contribution to the investments literature was followed by the collection of essays that he edited and in many cases inspired—*The Random Character of Stock Market Prices* (1964a). It would be hard to overstate the impact that this book had on the field for academics and professionals alike. This book includes the basis for the efficient markets approach to security prices and much of the early scientific work on option pricing. For many professionals the ideas in the book were initially an anathema although Paul lived to see them accepted and applied by "innovative managers." *The Random Character of Stock Market Prices* established Paul Cootner as one of the pioneers of modern finance, a somewhat unusual outcome for an editor. A valedictory comment made by Robert Merton helps explain why:

> Paul was present at the creation of what we now routinely call modern finance theory. He made major contributions to it through his own research and encouraged and guided basic research by graduate students, colleagues, and academic visitors, who came to MIT because Paul was there. . . . *The Random Character of Stock Market Prices,* the book which Paul put together, is full of papers by students whose research he directed. That this volume is a classic is a tribute to Paul's early recognition of the importance of this research and the high standard he applied to the research performed under his direction.[12]

These seminal contributions to the literature on stock price behavior are one example of Paul's pioneering efforts in finance. He was also an innovator in the theory of bond prices, although this work is less well known because Paul was never sufficiently satisfied with the work to publish it. In December 1966, he released a working paper entitled "The Stochastic Theory of Bond Prices." In this study Paul recognized the need for a theory of bond price behavior to accompany the emerging efficient market theories of stock and option prices. In the 1966 working paper bond yields were modelled as a Weiner process—an approach to bond yield analysis that has become increasingly important since the mid-1970s. Paul did not carry his analysis through to an equilibrium yield structure relationship. However, he did use his results to place upper and lower bounds on the value of a finite term call on a bond (using Samuelson's rational theory of warrant pricing) to estimate liquidity risk on bonds and to examine the implications for hedging in bond portfolios. Although this paper was not published, it was clearly a pioneering effort and was so recognized by the students and colleagues with whom he discussed the ideas it contains.

[11]Paul Cootner, "Stock Prices: Random vs. Systematic Changes," *Industrial Management Review,* 3 (Spring), 25–26.

[12]Robert C. Merton, private correspondence.

The Cootner-Holland study of risk and return (1963f, 1970) provides another example of the innovative nature of Paul's work in finance. This study was begun in 1961 and is one of the first quantitative studies of the relationship between risk and return. The authors set out to study "business risk" in the context of utility regulation and using accounting data in the estimations. Their basic hypothesis for company rates of return was that the average rate of return for a firm would vary with the standard deviation of the firm's rate of return around the mean of the industry to which it belonged and the standard deviation of the firm's rate of return around its own average rate of return. They found both of these risk measures to be positively and significantly related to the realized average rate of return. Finance has shifted away from the "business risk" concepts employed by Cootner and Holland in their study to take up the "financial risk" concepts developed by Sharpe and others. The profession clearly considered the Cootner-Holland risk–return study to be important; its circulation and quotation as an unpublished monograph were so extensive that the editors of the *Bell Journal* asked the authors in 1970 to publish a report on the 1963 monograph.

Paul's last publications were on capital market equilibrium. In this work he began to report on ideas that he had been considering and discussing for a number of years with his students and colleagues. From conversations with Paul, it is clear that he expected this work to be his most important contribution to economics and finance. The invited paper presented at the 12th Annual Conference of the Western Finance Association (1977a) was a preview of a more comprehensive work on equilibrium asset pricing. The paper entitled "Capital Asset Pricing in a General Equilibrium Framework" (1978) that appears in this volume was read as part of a memorial to Paul at the 1978 Western Finance Association Conference. This paper is a posthumous attempt to bring some of his work on capital asset pricing together in one publication. Since this involved combining and condensing incomplete drafts of three papers, it is clear that the 1978 paper does not do justice to the ideas that Paul tried to develop.

The central idea in Paul's work on asset pricing is the need to move beyond the partial equilibrium results of the existing capital asset pricing model. The existing model fell short in Paul's view, since risk was exogenous to the model and since real quantities did not appear anywhere in the model. In the more comprehensive model that he was developing, equilibrium is studied by examining the optimal consumption problem. Uncertainty is introduced through exogenous supply forces and price uncertainty is endogenous. In this framework, Paul assumed a Markov process for the supply of real goods and obtained an equilibrium price function with one-to-one correspondence between a given state of the world and an equilibrium price. This insures intertemporal consistency since each time a given state of the world occurs the same equilibrium price will be observed. The importance of Paul's work on asset pricing was noted by Stephen Ross: