Preface*

Introduction

It is surely a coming of age of financial economics that the field is now covered by Annual Reviews, a not-for-profit organization that has been providing the scientific community with authoritative critical reviews of the most important scholarly advances since 1932, starting with the *Annual Review of Biochemistry*. Annual Reviews now covers 40 disciplines ranging from analytical chemistry to sociology, and it has been our pleasure and privilege to serve as editors for this inaugural volume of the *Annual Review of Financial Economics*.

One of the most exciting aspects of this discipline is the constant interplay between theory and practice that is unique among the social sciences. Despite its use of rather arcane mathematics, and unlike other mathematically sophisticated branches of economics, financial economics has found its way into the mainstream of financial practice. It was not always thus. The scientific breakthroughs in financial modeling both shaped and were shaped by the extraordinary flow of financial innovation that coincided with revolutionary changes in the structure of world financial markets and institutions during the past four decades.

In this preface, we provide some personal reflections—meant neither to be authoritative nor exhaustive—on the origins of financial economics, where the field stands today, and what the future might hold.

Origins

The core of financial economics is the study of the behavior of economic agents in allocating and deploying their resources, both spatially and across time, in an uncertain environment. Time and uncertainty are the central elements that influence financial behavior. The complexity of their interaction brings intrinsic excitement to the study of finance because it often requires sophisticated analytical tools to capture the effects of this interaction. Indeed, the mathematical models of modern finance contain some of the most beautiful applications of probability and optimization theory. But, of course, all that is beautiful in science need not also be practical. And surely, not all that is practical in science is beautiful. Here we have both. With all their seemingly abstruse mathematical complexity, the models of financial economics have nevertheless had a direct and significant influence on both academia and the financial industry, as evidenced by the number of Nobel prizes awarded to financial economists[1] and the many trillions of dollars that comprise the businesses built on academic models and methods (passive

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[1]There have been eight, so far: James Tobin (1981), Franco Modigliani (1985), Harry Markowitz (1990), Merton Miller (1990), William Sharpe (1990), Robert Merton (1997), Myron Scholes (1997), and Robert Engle (2003). Paul Samuelson (1970) and Kenneth Arrow (1972) made foundational contributions to financial economics but their Nobel prizes were awarded primarily for other contributions.

index funds, risk models and portfolio optimization software, exchange-traded options, and OTC derivatives). This conjoining of intrinsic intellectual interest with extrinsic application is a prevailing theme of research in modern finance.

The origins of modern financial economics can be traced to Louis Bachelier's magnificent dissertation, completed at the Sorbonne in 1900, on the theory of speculation.[2] This work marks the twin births of the continuous-time mathematics of stochastic processes and the continuous-time economics of option pricing. In analyzing the problem of option pricing, Bachelier provides two different derivations of the Fourier partial differential equation as the equation for the probability density of what is now known as a Wiener process/Brownian motion. In one of the derivations, he writes down what is now commonly called the Chapman-Kolmogorov convolution probability integral, which is surely among the earlier appearances of that integral in print. In the other derivation, he takes the limit of a discrete-time binomial process to derive the continuous-time transition probabilities.[3] Along the way, Bachelier also developed essentially the method of images (reflection) to solve for the probability function of a diffusion process with an absorbing barrier. This all took place five years before Einstein's discovery of these same equations in his famous mathematical theory of Brownian motion. Not a bad performance for a thesis on which the first reader, Henri Poincaré, gave less than a top mark.[4]

Sophisticated mathematical models and a strong influence on practice were not always hallmarks of financial economics. Indeed, Bachelier's work was unknown in the finance literature for more than a half century.[5] During most of this period, finance was almost entirely a descriptive discipline with a focus on institutional and legal matters. Financial theory was little more than a collection of anecdotes, rules of thumb, and shufflings of accounting data. Mathematical models of finance were focused on the time value of money and the most sophisticated tool of analysis was present value.[6] Application of these models by practitioners in non-financial firms was largely confined to staff who set guidelines for capital-budgeting decisions. Although the mathematical modeling of bond-price sensitivity to interest rates (duration) had been developed by Frederick Macaulay in 1938, there was little evidence of its use in practice more than 20 years later, even by issuer and trader specialists in the debt markets.

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[2]For English translations, see A.J. Boness in Cootner (1964) and Davis & Etheridge (2006). Mandelbrot (1989) provides a brief biography.

[3]This same approach using the binomial process is now applied as a numerical-approximation method to solve complicated derivative-security pricing problems and was the foundation for the Cox-Ross-Rubinstein (1979) binomial option pricing model ; see Merton (1992b) for additional references.

[4]Bernstein (1992, p.19) reports that the thesis was awarded "mention honorable" instead of "mention trés honorable." See Dimand (1993) for further discussion.

[5]The rediscovery of his work by economists in the early 1950s is generally credited to Paul Samuelson via the statistician Leonard J. Savage (see Samuelson 1965a). A discussion of Samuelson's own considerable contributions to finance theory can be found in Merton (1983, 2006).

[6]Unlike in finance at this time, the mathematical models of the actuarial discipline addressed both the time value of money and the evaluation of uncertainty. The term "actuary" appears to have been used first as the title for the chief executive of the Equitable Life Assurance Society at its founding in 1762. Over the succeeding 200 years, applications of actuarial modeling involve almost exclusively evaluating mortality risks for life insurance and annuities and casualty risks for nonfinancial assets. For an overview of the use of actuarial methods in finance, see O'Brien (1992).

Modern financial economics begins only in the late 1950s and 1960s. The Modigliani & Miller (1958) propositions for the determinants of corporate capital structure was a breakthrough not only because they provided a rational framework for thinking about debt and equity but also because of the no-arbitrage principle on which they were based. The Markowitz (1952, 1959) mean-variance theory of portfolio selection, with an important extension by Tobin (1958), provided a tractable model for quantifying the risk-return tradeoff for general assets with correlated returns. Building on the Markowitz-Tobin fundamental work, Sharpe (1964) and Lintner (1965) investigated the equilibrium structure of asset prices, and their Capital Asset Pricing Model (CAPM) became the foundational quantitative model for measuring the risk of a security. The CAPM would later give rise to the multi-trillion-dollar business of passive investing via long-only, market-capitalization-weighted index funds, as well as the foundation of performance attribution for professional investment managers.

Another milestone of the 1960s research on investment practice was the Samuelson (1965b) and Fama (1965) Efficient Markets Hypothesis that holds that, in a well-functioning and informed capital market, asset-price dynamics are described by a (sub)martingale in which the best estimate of an asset's future price is the current price, adjusted for a "fair" expected rate of return. Under this hypothesis, attempts to use past price data or publicly available forecasts about future economic fundamentals to predict future security prices are doomed to failure. Earlier, Maurice Kendall (1953) of Royal Statistical Society fame had presented empirical evidence that stock and other speculative price changes were not forecastable. Indeed, Bachelier (1900) had formulated his theoretical option-pricing model assuming a martingale-like behavior for security prices. It was, however, the Fisher & Lorie (1965) study of historical stock returns that probably first drew practitioner attention to the Efficient Markets Hypothesis. Using the newly created database of the Chicago Center for Research in Security Prices, Fisher & Lorie showed that a randomly selected stock held from the mid-1920s to the mid-1960s would have earned, on average, a 9.4% annual compound return. Returns of this magnitude were believed to be considerably larger than those most professional managers had earned for their clients during that period. Rigorous scientific confirmation of this belief was provided by a host of empirical performance studies along lines set by Jensen (1968) who used the CAPM as a benchmark to test for superior performance among United States mutual funds in the post-war period.

In the late 1960s and early 1970s, financial models became considerably more sophisticated, involving both the intertemporal and uncertain aspects of valuation and optimal financial decisionmaking. Dynamic portfolio theory extended and enriched the static Markowitz mean-variance model. Intertemporal and international capital asset pricing models expanded the single risk measure, beta, of the Sharpe-Lintner CAPM to multidimensional measures of a security's risk.[7] The mathematical tools used in these models—stochastic differential and integral equations, stochastic dynamic programming, and partial differential equations—were a quantum level more complex than had been used previously.

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[7]For references and analysis on dynamic portfolio theory and intertemporal capital asset pricing, see Merton (1992b). Solnik (1974) was the first to adopt these models into an international framework. Although static in its formal development, the Arbitrage Pricing Theory of Ross (1976) also provides for multiple dimensions in the measure of a security's risk.

The watershed development in terms of impact on practice was the Black-Scholes (1973) and Merton (1973) model for option pricing.[8] Virtually from the day the papers were published, this work brought the field to closure on the subject. The Chicago Board Options Exchange (CBOE) began trading the first listed options in the United States in April 1973, a month before the official publication of the Black-Scholes paper. By 1975, traders on the CBOE were using the model to both price and hedge their options positions. Indeed, Texas Instruments created a hand-held calculator that was specially programmed to produce Black-Scholes/Merton option prices and hedge ratios. Such a complete and rapid adoption of finance theory into finance practice was unprecedented, especially for a mathematical model developed entirely in theory. That rapid adoption was all the more surprising because the mathematics used in the model were not part of the standard mathematical training of economists, either academic or practitioner.[9]

Black and Scholes, Merton, and others quickly recognized that their replicating-portfolio approach could be applied to the pricing of general derivative securities with arbitrary nonlinear payoffs contingent on one or more traded-security prices. Hence, at the same time that their work was closing gates on fundamental research on options, it was simultaneously opening new gates by setting the foundation for a new branch of finance called contingent-claims analysis (CCA). The applications of CCA range from the pricing of complex financial securities to the evaluation of corporate capital budgeting and strategic decisions and include, for instance, a unified theory for pricing corporate liabilities and the evaluation of loan guarantees and deposit insurance. Indeed, the theory and mathematical modeling of CCA for these applications have become even more important to finance practice than the original options applications.

The 1970s and 1980s also witnessed considerable progress in corporate finance with the explicit recognition that the frictionless world of Modigliani & Miller's propositions did not hold in practice, and that optimal capital structure and dividend policy were, in fact, possible in the presence of certain capital market imperfections. Kraus & Litzenberger (1973) observed that an optimal capital structure could be derived as the result of the trade-off between the marginal tax benefits from the deductibility of interest payments and the costs of debt financing due to financial distress. Ross (1973) and Jensen & Meckling (1976) identified another source of friction associated with debt financing: agency costs, i.e., perverse managerial incentives that are counter to the interest of shareholders. Miller (1977) incorporated personal taxes into the analysis and derived an optimal aggregate debt/equity ratio, but claimed that firm-specific capital structure was still indeterminate. And Myers (1984) and Myers & Majluf (1984) proposed the Pecking Order Hypothesis for capital structure in which corporations are hypothesized to have a specific preference ordering for financing choices, with internal funds as the most preferred, debt financing second, and equity financing last. This pecking order may be viewed as an outcome of behavioral motives, or as a reflection of the different transactions costs associated with the three sources of financing.

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[8] For the story of how they developed their model, see Bernstein (1992, chapter 11) and Scholes (1998); for extensive references and extensions of the model and its wide-ranging applications beyond option pricing, see Merton (1992b, 1998) and Lim et al. (2006).

[9] The Itô stochastic calculus used in the dynamic replication method has direct parallel links from the mathematical side of Bachelier's thesis. Kiyoshi Itô told one of us (Merton) that it was Bachelier's approach—and not Norbert Wiener's later work—that inspired Itô's development of the stochastic calculus.

A breakthrough at the intersection of corporate finance and capital markets also occurred in the 1970s with the idea of "real options," a term coined by Stewart Myers in 1977 that refers to the application of option-pricing theory to analyzing real investment opportunities and making capital-budgeting decisions involving drug discovery, oil fields leases, mineral rights, alternative production processes including modularity, multiple-fuel powerplants, patents, and the option to commence, delay, or abandon a project [see Trigeorgis (1996) for additional references]. Although Black, Scholes, and Merton were certainly aware of the broader applications of their framework, even they did not fully appreciate the breadth with which option-pricing theory could and would be applied in the 35 years since the publication of their papers.

The industry applications of financial economics in the 1970s had been primarily in equity markets and equity derivative securities. The big, new applications of the 1980s were in the fixed-income arena. The models incorporated major multivariate extensions of the CCA methodology to price and hedge virtually every kind of derivative instrument, whether contingent on equities, fixed-income securities, currencies, or commodities. Dynamic models of interest rates were combined with CCA models to price both cash-market and derivative securities simultaneously. The enormous U.S. national mortgage market could not have functioned effectively without mathematical models for pricing and hedging mortgages and mortgage-backed securities whose valuations were especially complex because of prepayment options. By the late 1980s, the time-lag for adoption of finance-theory models into practice was essentially nil. Indeed, the mathematical models used in finance practice became as sophisticated as any found in academic financial research.

The industrial uses of financial models in the 1970s were primarily by U.S. institutional equity investors, market makers and brokers trading U.S. equity options, currency traders, and a few fixed-income traders. During the 1980s, the industry audience for these models expanded greatly, becoming global in scope and including commercial and investment banks and institutional investors of all types (see Grundfest, 1990). Practitioners in financial institutions actually took on a major role in applied research, including the creation of proprietary databases, the development of new numerical methods for solving partial differential equations, and the implementation of sophisticated estimation techniques for measuring model parameters.

During the 1970s, derivative-security exchanges were created to trade listed options on stocks, futures on major currencies, and futures on U.S. Treasury bills and bonds. The success of these markets, measured in terms of trading volume, can be attributed in good part to the increased demand for managing risks in the volatile economic environment, and this success strongly affected the speed of adoption of quantitative financial models. For example, experienced traders in the preceding over-the-counter (OTC) dealer market had achieved a degree of success by using heuristic rules for valuing options and judging risk exposures. However, these rules of thumb were soon found to be inadequate for trading in the fast-paced exchange-listed options market with its smaller price spreads, larger trading volume, and requirements for rapid trading decisions while monitoring prices in the stock, interest rate, and option markets. In contrast, formal mathematical models along the lines of the Black-Scholes/Merton model were ideally suited for application in this new trading environment.[10]

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[10]The OTC dealer market traded almost exclusively at-the-money options with standardized expiration maturities. The experiential-based heuristic valuations so developed were not at all adequate for pricing in-the-money and out-

The growth in modeling sophistication and practical application of financial economics during the 1980s far exceeded the 1970s, and paralleled the extraordinary growth of financial innovation (see Finnerty 1992, and Miller 1986, 1992). Huge government budget deficits, especially in the United States, increased several times the amount of sovereign debt worldwide that required intermediation and placement, and a wave of deregulation in the financial sector were important factors driving innovation.[11] They were not, however, the only ones.

Conceptual breakthroughs in finance theory in the 1980s were perhaps fewer and less fundamental than in the 1960s and 1970s. But the research resources devoted to the development and empirical implementation of mathematical models and financial databases to support them were considerably larger. Moreover, the opportunities and feasibility of implementing these models in practice were also much greater.[12] The subdisciplines of empirical finance and financial econometrics came into their own as the growing sophistication of financial models required equally sophisticated methods for their empirical implementation, both within the halls of academia and in industry.[13] This empirical renaissance was undoubtedly fueled by the parallel set of breakthroughs in computer technology, including vastly more powerful hardware and software, and data collection and organization. The advent of affordable, fast desktop computers, machine-readable real-time and historical data, and electronic connectivity between market participants and exchanges have breathed life into financial econometrics, irrevocably changing the way finance is practiced and taught. Concepts like alpha, beta, R^2, correlations, volatilities, and cumulative average residuals have become concrete objects to be estimated and actively used in making financial decisions.

These innovations in computing, telecommunications, and empirical implementation also made possible the formation of many new financial markets and substantial expansions in the size of existing ones. Those same technologies made feasible the numerical solution of new complex CCA models with multivariate partial differential equations (see Wilmott et al. 1993). They also sped up the solution of existing CCA models to allow virtually real-time calculations of prices and hedge ratios. CCA and related topics were widely incorporated in top business-school curriculums during the late 1970s and early 1980s. As a result, by the mid-1980s, there was a significant pool of MBA and Ph.D. professionals trained in modern finance theory who were available to put the theory into practice. The pool was further augmented by mathematicians and scientists with advanced degrees attracted to the financial-services industry by high salaries and challenging problems.

Success of the new trading markets and intermediated products can lead to further success through a process called the financial-innovation spiral (Merton 1992a,b, 1993) that proceeds as follows: The proliferation of new trading markets in standardized securities, such as futures,

of-the money options of varying expiration maturities, as required for valuing listed options on the traded exchanges.

[11]For discussion of the interplay between financial regulation and innovation, see Miller (1986, 1992) and Merton (1992a, 1995).

[12]See Eckl et al. 1990, Marshall & Bansal 1992, and Smith & Smithson 1990 on the development of financial engineering during this period.

[13]See the extensive list of references in Campbell et al. (1997) and Lo (2007) for an historical perspective on empirical finance and financial econometrics.

options, and swaps, makes possible the creation of a wide range of new financial products, many custom-designed and sold OTC by financial intermediaries to meet selected needs of investors and corporate issuers. Next, volume in the new markets expands further as the intermediaries themselves trade simply to hedge their own exposures from the products they sold. Such increased volume in turn reduces marginal transactions costs and thereby makes possible the further implementation of new products and trading strategies, and this, in turn, leads to still more volume. New markets also evolve as some successful products become standardized and their source of distribution moves from intermediaries to markets. Success of these trading markets and custom products then encourages investment in creating additional markets and products, and so on it goes, spiraling toward the theoretically limiting case of complete markets and zero marginal transactions costs.

The reduction in transactions costs for financial institutions was substantial during the 1980s. The costs of implementing financial strategies for institutions using derivative securities, such as futures or swaps, can be one-tenth to one-twentieth of the cost of using the underlying cash-market securities.[14] This is especially the case for investments by foreign institutions that are often subject to withholding taxes on either interest or dividends.

The decline in costs does not derive only from reductions in bid-ask spreads and commissions. There are also cost savings from movement down the learning curve. With the cumulative experience of having built several new markets, innovators become increasingly more efficient and the marginal cost of creating additional markets falls. While some may view the extraordinary growth in derivative securities over the past five years as only a fad, more likely explanations are the gains from more efficient risk transfer and the vast savings in transactions costs from their use. Looking to the future, with such cost savings, we are not going back: Derivatives are an essential and permanent part of the mainstream global financial system.

In the 1990s, another significant development in financial economics was the integration of corporate financial policy with capital market dynamics, as in Leland (1994, 1998), in which a corporation's optimal capital structure is determined endogenously as a function of the corporate tax rate, bond covenants and bankruptcy costs, and a given set of price dynamics for the firm's assets. Building on Merton (1974), Black & Cox (1976), and Brennan & Schwartz (1978), Leland derived closed-form solutions in a dynamic setting that allowed for default to be determined endogenously as the result of an optimal decision policy carried out by equity holders. Since then, Leland's framework has been extended in many directions, and we now have a much richer understanding of the connections between corporate financial policy and capital markets from this literature.

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[14]See Perold (1992) for an excellent description of the comparative costs of various alternatives for executing basic investment strategies. Although institutions have experienced the greatest reduction in the cost of direct market transactions, retail investors have in places had similar experiences: Discount retail brokers in the United States now charge as little as 3 to 5 cents a share in commissions on stock transactions.

The Current State of Financial Economics

Having presented our perspectives on the origins of modern finance—which, in keeping with the tradition of Annual Reviews, is not meant to be historically complete—we now turn to the current state of financial economics, which, of course, is the subject of this inaugural volume of the *Annual Review of Financial Economics*. But before doing so, we must say a few words about the current context in which we have taken on this endeavor.

It is now widely acknowledged that the scientific breakthroughs in finance and the extraordinary innovations in finance practice of the past decades transformed the financial system over the past 40 years as they expanded opportunities for risk sharing, lowering transaction costs, and reducing information and agency costs. In 2009, no major financial institution in the world— including central banks—can function without the computer-based mathematical models of modern financial science and the myriad of derivative contracts and markets used to extract price- and risk-discovery information as well as to execute risk-transfer transactions. But, in 2009, we also find ourselves only now beginning to emerge from a two-year global financial crisis of a magnitude and scope not seen in nearly 80 years. Indeed there are some that attribute a major cause of the crisis to the cumulative changes in the financial system brought about by financial innovation, derivatives, and the quantitative models of modern finance science.

We believe that the forensic data-gathering and analysis of the Financial Crisis of 2007–2008 is far from complete, let alone any conclusive report on what triggered the extraordinary events we are experiencing and what were the foundational vulnerabilities that made the losses and dislocations so broad and deep. Many plausible hypotheses have been offered from just about every source, but at this time, that is all we have—hypotheses. When the results are finally in, the causes will surely be multidimensional, with no single element close to being a sufficient explanation.[15] There will, of course, be many mistakes uncovered and elements of bad and incompetent behavior found among financial-service providers, their customers, and those in government charged with their oversight and protection, fools and knaves aplenty, and they need to be addressed. However, we should also identify and analyze the important structural causes of the crisis, those that can occur even if all participants in the financial system are well-informed and well-behaved, and these require at least as much attention if we are going to learn and improve from this devastating experience (see, for example, Khandani et al. 2009). We see no shortcut to the careful collection of data, rigorous evaluation of competing hypotheses, and further scientific analysis to find out what happened, and to formulate policies and actions to prevent this from happening again in the future. This very dysfunctional crisis thus has a functional element in the many important lessons that will be learned and the new theories, models, and data sources developed as a consequence of its having happened. We are confident that the application of the existing tools of finance to analyzing the crisis at hand will also serve

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[15]In contrast to the tragic explosion of the space shuttle *Challenger* in 1986, for which Richard Feynman gave a riveting demonstration that the disaster was caused by the failure of an O-ring that had become brittle in sub-zero-degree weather, there will be no dramatic and singular cause of the Financial Crisis of 2007–2008. Note that even in the case of the *Challenger* explosion, the failure of the O-ring was only part of the answer; management failures apparently also contributed to the launch decision despite earlier warnings of potential problems with cold-weather conditions.

to dispel any concerns that modern financial economic science is "broken" and in immediate need of a wholly new paradigm.

But academic disciplines are constantly evolving and, we hope, improving, and so a new paradigm may very well emerge as we continue our process of self-examination and innovation. If Zen Buddhists are correct in their observation that one can never step into the same river twice, this volume is a unique snapshot of financial economics as it stands today. In the 15 articles that comprise this inaugural volume, we have an idiosyncratic but nonrandom sampling of the various strands of the financial economics literature that Bachelier unwittingly initiated more than a century ago.

It is truly fitting that we begin in the beginning, with Paul Samuelson's personal reflections on his foundational and multifarious contributions to the field. Middle-aged financial economists have drawn some comfort from the fact that Samuelson did not turn his attention to finance until his late forties, but such comfort is short-lived when they realize that Samuelson's productive years continued for half a century more, and this remarkable icon of modern financial economics still publishes regularly. We are deeply grateful to Samuelson for sharing with us his personal thoughts about the field he did so much to create, and readers are in for a real treat.

Robert Jarrow takes on the twin topics of credit risk and the term structure of interest rates, both of which are critical analytical inputs to the spectacular growth in credit derivatives and securitized debt over the past decade, and which played an important role in the Financial Crisis of 2007–2008. Specifically, in his article on credit risk, Jarrow covers structural and reduced-form models, incomplete information, credit derivatives, and default contagion, and argues that reduced-form models, not structural models, are appropriate for the pricing and hedging of credit-risky securities. And in reviewing the vast literature on the term structure of interest rates, Jarrow also includes a discussion of the arbitrage-free pricing and hedging of interest rate derivatives, the Heath-Jarrow-Morton model, forward and futures contracts, the expectations hypothesis, and the pricing of caps and floors.

In the wake of this crisis, financial economists have begun to sift through the wreckage and we are fortunate to have three insightful perspectives on this topic. Franklin Allen, Ana Babus, & Elena Carletti observe that financial crises have occurred over the course of centuries, and are often preceded by a credit boom and a rise in real-estate and other asset prices, as in the current crisis. They provide a game-theoretic framework for financial crises and survey the corresponding empirical literature. Dale Gray's review begins with an overview of the key features of the Financial Crisis of 2007–2008 and then develops a CCA framework for modeling financial crises and sovereign risks. And Randall Morck & Bernard Yeung present an historical perspective on comparative corporate governance to illustrate the potential changes that crisis can bring about. Comparative financial histories show that corporate governance regimes are largely stable through time, but are capable of occasional dramatic change in response to a severe crisis. Legal origin, language, culture, religion, accidents of history (path dependence), and other factors affect these changes because they affect how people and societies solve problems.

Changes to corporate governance and financial structure are also motivated by factors affecting the supply of equity and credit, and Malcolm Baker describes the channels through which these

effects manifest themselves in corporate finance. In particular, supply effects can arise from a combination of three ingredients: investor tastes, limited intermediation, and corporate opportunism. This framework helps to organize empirical approaches that more precisely identify and quantify supply effects through variation in one of these three ingredients, and the empirical evidence shows that shifting equity and credit market conditions play an important role in dictating corporate finance and investment. Michael Roberts & Amir Sufi present complementary empirical evidence regarding financial contracting, which clarifies the role of creditor control rights in and out of bankruptcy, collateral, and the prospect of ex-post renegotiation in determining corporate contractual terms.

The reach of financial economics goes well beyond corporations, impacting individuals directly through basic consumer finance—the subject of Peter Tufano's contribution—through life-cycle considerations and pension plans, which Zvi Bodie, Jérôme Detemple, & Marcel Rindisbacher cover, and through its effects on income distribution and social welfare, which Asli Demirgüç‑Kunt & Ross Levine discuss. Although consumer finance is a significant component of the U.S. economy, it has had a smaller footprint within the scientific literature of financial economics. Tufano's article focuses on four key functions of consumer finance: payments, risk management, moving funds from today to tomorrow (saving/investing), and from tomorrow to today (borrowing). He provides data showing the economic importance of consumer finance in the American economy, and considers these findings in contexts spanning economics, marketing, psychology, sociology, technology, and public policy. Bodie, Detemple, & Rindisbacher review the recent literature on consumer financial decisions over the life cycle, outlining its implications for the rational design of pension plans. They also summarize the recent empirical literature on the actual behavior of households regarding saving, investing, and insuring their consumption in old age, and discuss some practical implications for the design of pension systems. And Demirgüç‑Kunt & Levine provide a review of the literature on financial economics and inequality, highlighting substantive gaps in the extant literature. Finance plays a crucial role in the preponderance of theories of persistent inequality, and although subject to qualification, the bulk of empirical research suggests that improvements in financial contracts, markets, and intermediaries expand economic opportunities and reduce inequality. Together, these three articles illustrate the ubiquity of financial economics in our lives, and the importance of financial models in managing our wealth and, more importantly, our financial risks.

One aspect of financial risk that 2008 brought into sharp focus is the variability of risk measures like volatility. The VIX index, a forward-looking measure of S&P 500 volatility, skyrocketed from a low of 16% in May 2008 to a high of 80% in October 2008, and is currently hovering around 25%. Not surprisingly, volatility-based financial products have become fashionable. Peter Carr & Roger Lee provide a review of volatility derivatives, a class of derivative securities where the payoff explicitly depends on some measure of the volatility of an underlying asset. These contractual-agreement products allow the purchaser to either insure or hedge against changes in risk as well as value in a wide array of asset classes. In addition to expanded opportunities for risk transfer, these markets provide important risk-discovery information not previously available and thus greater financial-market transparency. Prominent examples of these derivatives include variance swaps and VIX futures and options. They provide an overview of the current market for these derivatives, survey the early literature on the subject,

and present relatively simple proofs of some fundamental results related to variance and volatility swaps.

Financial econometrics has also progressed since the early tests of the Random Walk Hypothesis, and Yacine Aït-Sahalia's article provides a review of one important strand of this literature: the parameter estimation of continuous-time models in financial economics. Much of the CCA literature is based on continuous-time stochastic processes satisfying Itô stochastic differential equations, and the standard approach to estimating the parameters of these processes relies on their transition densities. However, closed-form analytical expressions for these densities are available only in a few special cases, and confining our attention to only these cases would severely limit the practical relevance of CCA models. Fortunately, computationally intensive non-parametric methods have been developed to estimate a much broader class of continuous-time stochastic processes, and Aït-Sahalia describes these innovations as well as traditional parametric estimation methods.

Of course, if the parameters of our financial models are unknown and must be estimated, then presumably this challenge is also faced by the economic agents that populate our financial models. Therefore, the process by which those agents learn about the parameters of the model should be accounted for as well. Lubos Pastor & Pietro Veronesi survey the recent literature on learning in financial markets, and show that many financial regularities and anomalies—the volatility and predictability of asset returns, stock price bubbles, portfolio choice, mutual fund flows, trading volume, and firm profitability, for example—are easier to understand once we acknowledge that parameters in financial models are uncertain and must be inferred over time.

The fact that individuals learn over time suggests that behavior may be more subtle and complex than our models can capture. *Homo economicus* is an idealization that simply may not be consistent with *Homo sapiens*, and Peter Bossaerts' review of the recent neuroscientific literature on decisionmaking provides a detailed exposition of some of the inconsistencies. Financial decision making is the outcome of complex neurophysiological processes involving, among others, constant reevaluation of the statistics of the problem at hand, balancing of the various emotional aspects, and computation of the very value signals that are at the core of modern economic thinking. The evidence suggests that emotions play a crucial supporting role in the mathematical computations needed for reasoned choice, rather than interfering with it, even if emotions (and their mathematical counterparts) may not always be balanced appropriately. The emerging field of decision neuroscience may provide a number of new tools for improving financial decision-making.

The Future of Finance and the Finance of the Future

It may be difficult to believe that the pace of general financial innovation during the past decades can sustain itself into the future. However, there are reasons to believe that it will. The decision to implement a new innovation involves a tradeoff between its benefit and the cost. With secularly lower transactions and learning-curve costs, the threshold benefit required to warrant implementation declines secularly. Hence, holding fixed the same pace of change in the

underlying economic fundamentals as in the past, the implementation of financial innovation is likely to be more rapid because the threshold for change is lower.

With much lower costs of change, it becomes profitable not only to introduce new products and create new markets, but also to change entire institutional arrangements (including geographical and political locations) in response to much smaller shifts in customer tastes or operating costs than in the past. Lower transactions costs, together with the prospect of greater global competition in financial services, form the basis for forecasting substantial increases in both the frequency and the magnitude of institutional changes for private-sector and government financial intermediaries, and for regulatory bodies. In the past, mathematical models played a key role in supporting the creation of new products and markets. In the future, that role will expand to include supporting the creation of entire new institutions.

A successful conceptual framework for analyzing issues involving the global financial system in the future must address, endogenously, differences in institutional structure across geopolitical boundaries and in the dynamics of institutional change. The neoclassical-economics perspective addresses the dynamics of prices and quantities. But it is largely an "institution-free" perspective in which only functions "matter" (see Merton 1992a, 1993, 1995 and Crane et al. 1995 for a more detailed discussion of the functional perspective). It thus has nothing to say directly about cross-sectional or intertemporal differences in the institutions that serve those functions.

In contrast, there is the institutional perspective in which institutions not only matter but are the conceptual "anchor." This perspective takes as given the existing institutional structure and views the objective of public policy as helping the institutions currently in place to survive and flourish. Framed in terms of the banks or the insurance companies, managerial objectives are similarly posed in terms of what can be done to make those institutions perform their particular financial services more efficiently and profitably. The institutional perspective addresses cross-sectional differences across borders but is static in focus. Because institutions are the anchor, institutional change is exogenous within this perspective.

Drawing on both of these perspectives, the functional perspective takes as given the economic functions performed by financial institutions and seeks to discover what is the best institutional structure for performing those functions at a given time and place. It does not posit that existing institutions, whether private sector or governmental, operating or regulatory, will be preserved. Functions thus serve as the conceptual anchor here. Because institutions "matter" but are not the anchors, institutional changes are endogenous within this perspective.

The increasing flexibility and global mobility of financial institutions, together with the technology for creating custom financial contracts at low cost, have far-reaching implications not only for the regulation of financial services, but for national monetary and fiscal policies as well. Thus, policymakers are effectively speculating against a long-run trend of declining transactions costs if they assume that "traditional" frictions within their individual financial systems will continue to allow national governments to pursue monetary and related financial policies with the same degree of control as in the past.[16] Much the same point applies to a nation's fiscal

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[16] If the principal mechanism of central bank influence on macro-investment policy comes from controlling banks that ration credit to their customers (Stiglitz 1988 and Stiglitz & Weiss 1981), then what would be the impact on that

policy, which will surely be further constrained not just with respect to taxes "targeted" at financial services and transactions but even with respect to general personal and corporate income taxes.[17]

Given the Financial Crisis of 2007–2008, risk management is perhaps the central topic for the management of financial institutions over the next decade. Of course, risk management has always been important to such institutions. However, the focus in the past has been on capital adequacy to ensure performance. Equity capital is a robust "all-purpose" cushion against unanticipated losses. However, it is, at times, a quite expensive and inefficient means for doing so. With the vast array of financial instruments and quantitative models for estimating exposures to risk, there is now a greater opportunity to eliminate risk exposures of the firm on a more targeted and efficient basis by either hedging or insuring specific, non-value-enhancing risks (see Merton & Bodie 1992 and Merton 2005). The cost is that the user of hedging techniques must have a more precise, quantitative assessment of the firm's business risks than the user of equity capital. In turn, the greater need for precision places greater demands on the use and accuracy of mathematical models that measure exposures. As discussed in the Group of Thirty (1993) and GAO (1994) reports, financial institutions that use hedging as the principal means of covering their exposures need specially trained staff and sophisticated risk-management systems as well as senior management and board members who can provide informed oversight of those systems.

Hedging or insuring by nonfinancial firms is a targeted form of risk control that can be an effective substitute for equity capital. For example, an international airline can use futures, forwards, and swaps to hedge against unanticipated changes in jet fuel prices, or option contracts to insure. Other examples are protecting against exposures to general commodity prices, interest rates, and currency exchange rates. As in financial firms, managers of these firms must understand much more detail about their business structures if they substitute hedging or insuring for equity capital. Developing the necessary understanding for effective hedging is likely to require retraining of managers not only in risk-management techniques but also in the ways they think about their businesses. Although hedging by nonfinanical firms is just beginning, its inevitable spread in the future will cause further growth in the application of financial models for risk management.

These innovations imply that major revisions in the accounting conventions used in contract enforcement and implementation of regulations will be required. Although it is too early to know what form these revisions will take, we conjecture that the "new" categories will be defined in terms of "equivalent" exposures, very much like the "deltas" of contingent-claim securities in CCA. This will surely be necessary if regulations and regulators become more functionally defined and principles-based. If this is the case, mathematical models of finance will have a new and enormous area of application.

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mechanism from the creation of a national midmarket lending market similar to the mortgage market (Cushman 1993)?

[17] See Lindgren & Westlund (1990) and Umlauf (1993) on the recent Swedish experience with a transaction tax. See Scholes & Wolfson (1992) for a general development of the theory and application of financial instruments and alternative institutional designs to respond to differing tax and regulatory structures. These techniques have a greatly magnified influence in a low-transaction-cost and global environment.

Over the vast expanse of history, financial economics has had a limited and ancillary impact on financial practice. But during the past four decades, financial economics has become central to practitioners in financial institutions and markets around the world. In the future, financial economics is likely to have an even larger role in the functioning of the global financial system, including regulatory and accounting activities. It therefore follows that future effective education in financial management for both private-sector managers and public policy makers will develop skills in financial economics. The educational challenge is to find ways to make those models accessible to the general management population. In particular, along with the many benefits that financial technology has brought to individual investors, there are also some attendant costs. For the first time in the history of modern civilization, it is now possible to risk a significant fraction of one's retirement assets with a click of a button. There is no doubt that investors today have at their disposal a powerful array of tools with which to invest their wealth, but do they have the necessary expertise to wield those tools responsibly?

The challenge for the financial economists and investment professionals over the next decade will be to reintegrate the statistical and mathematical analysis of security prices with a deeper understanding of investors' preferences and behavior, and how financial institutions evolve to suit them. The events of the past two years have demonstrated that markets are neither perfectly efficient nor completely irrational—they are adaptive (see Lo 2004, 2005). By acknowledging this more complex reality, we have a much better chance of preparing for future crises and reducing their aftershocks.

Any virtue can become a vice if taken to an extreme—and just so with practical applications of financial economics. We therefore close with a few words of caution about their uses and abuses. In the introduction to his Ph.D. thesis, Paul Samuelson warned against the tendency to fall in love with our own models (Samuelson 1947, p. 3):

> …[O]nly the smallest fraction of economic writings, theoretical and applied, has been concerned with the derivation of *operationally meaningful* theorems. In part at least this has been the result of the bad methodological preconceptions that economic laws deduced from *a priori* assumptions possessed rigor and validity independently of any empirical human behavior. But only a very few economists have gone so far as this. The majority would have been glad to enunciate meaningful theorems if any had occurred to them. In fact, the literature abounds with false generalization.
>
> We do not have to dig deep to find examples. Literally hundreds of learned papers have been written on the subject of utility. Take a little bad psychology, add a dash of bad philosophy and ethics, and liberal quantities of bad logic, and any economist can prove that the demand curve for a commodity is negatively inclined.

At times, our models may become too "interesting," and we lose sight of their ultimate purpose. The mathematics of these models are often precise, but the models themselves are not, being only approximations to a much more complex reality. Their accuracy varies considerably across

time and place. The practitioner should therefore apply such models only tentatively, assessing their limitations carefully within each application.

It is at once humbling and exhilarating that this challenge was taken up over a century ago in a French mathematician's doctoral dissertation. Even after 100 years, there is still so much left to be discovered.

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