Financial Economics

Financial economics is at once both an ancient subject, with origins in business practice, and also one of the newest, with origins in applied mathematics. From the former perspective, it seems closest to accounting, and to money and banking, both of which arise from the attempt by practitioners to codify their own experience (Poitras 2000, Goetzmann and Rouwenhorst 2005). From the latter perspective, it seems closest to forms of engineering, where tools of dynamic programming and modern probability theory, for example, are deployed to solve practical problems facing businesses and consumers (Cont 2010).

In both of these guises, financial economics has always maintained a somewhat uneasy relationship with economics proper, for which the implied audience is typically some state actor with instrumental goals of its own. Until very recently, public finance and monetary economics were considered finance enough for economics, these subjects being attempts to provide the conceptual framework needed to guide government policy, fiscal and monetary policy respectively. (See, for example, Musgrave 1959 and Gurley and Shaw 1960). Corporate finance and investment management were left to the schools of business, and it was therefore in those schools of business that modern finance first grew up. The initial impetus for that growth came from developments in business practice, specifically the revival of private capital markets in the decades after World War II. This revival happened first in the United States, which helps to explain why the field of modern finance was initially largely an American invention.

The Capital Asset Pricing Model

On the supply of funds side, burgeoning pools of private investment capital inside pension funds and insurance companies cried out for scientific management, and portfolio theory developed to meet that demand. By treating the returns on individual securities as random variables, Markowitz (1952) was able to provide a framework for thinking about efficient portfolio diversification, and for actually calculating the portfolio weights that would achieve minimum risk for any given expected return target. (See also Roy 1952, writing outside the U.S.)

On the demand for funds side, increased scarcity of investment funds inside corporations forced new attention to capital allocation decisions, as well as to decisions about external funding in capital markets; corporate finance theory developed to guide these decisions. By abstracting from taxation and bankruptcy, Modigliani and Miller (1958) were able to provide a framework for thinking systematically about the problem of the appropriate capital structure of a firm, a framework that clarified not only the choice between debt and equity finance, but also the choice between internal and external funding.

These two different material origins of modern finance help to explain the different versions of the Capital Asset Pricing Model (CAPM) that emerged at more or less the same time, as well as the mutual incomprehension with which they initially received one another. Bill Sharpe's CAPM (1964) built on the portfolio theory pioneered by Harry Markowitz, as did the CAPM of Jan Mossin (1966). By contrast, the CAPMs of Jack Treynor (1962) and John Lintner (1965) both built on the corporate finance theory pioneered by Franco Modigliani and Merton Miller. The eventual 1990 Nobel (the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel) that recognized these achievements was shared by Markowitz, Sharpe, and Miller, thus recognizing both versions, and hence both origins.

Nevertheless, for teaching purposes, the canonical CAPM became that of Sharpe, which made its way into textbooks as the following formula:

$$ER_i = R_f + \beta_i(ER_m - R_f).$$

This says that the expected return on asset i is equal to the riskfree rate of interest R_f plus the amount of risk in the asset β_i times the price of risk. According to CAPM, portfolio diversification implies that risk is properly measured by the covariance of an asset with the diversified market portfolio (*not* by the asset's own variance), and that the price of risk is the excess expected return on that diversified market portfolio, $(ER_m - R_f)$.

The two origins shared the CAPM Nobel, but that prize did not come for twenty-five years, a delay that in retrospect can be understood as reflecting the time it took for the new finance to be recognized as a contribution to economics proper. To most economists, CAPM just didn't seem that important; it was about private finance not public finance, and also about finance not money. At the beginning, the new finance entered economics only to the extent that it seemed to bear on the traditional fiscal and monetary policy concerns of economics.

Thus, Sharpe was not initially seen as the main road leading from Markowitz; first there was Tobin (1969) whose contributions to portfolio theory fed directly into the economists' project of building large-scale econometric models of the economy, the better to calibrate fiscal and monetary policy interventions. Similarly, Lintner and Treynor were not the main road leading from Modigliani and Miller; first there was Ando and Modigliani (1969) whose "Econometric Analysis of Stabilization Policies" actually implemented the necessary parameterization. Thus Tobin and Modigliani got their Nobels first, in 1981 and 1985, respectively. Initially, the work of Tobin and Modigliani was considered finance enough for economics.

Finance and General Equilibrium

To understand better the resistance of economics to modern finance, it is necessary to appreciate the role played by general equilibrium theory in orienting economists' sense of their intellectual project in the years after World War II, and to appreciate also the place of finance in that theory.

Famously, the canonical Arrow-Debreu model (1954) extended general equilibrium from one period to many, and from the case of certainty to the case of risk, by imagining markets in state-contingent commodities, each one with its own price. In the real world, of course, such markets are noticeably absent, except in a very few cases of generic raw commodity inputs such as wheat, cotton, and oil. But, crucially, Arrow (1953) also showed how financial instruments that promise state-contingent payoffs (so-called Arrow-Debreu securities) could substitute for those missing commodity markets, and how the price of those financial instruments could substitute for state-contingent commodity prices. In effect, he argued that a world of complete financial markets would be equivalent to the idealized Arrow-Debreu world of complete state-contingent commodity markets, and either world would provide the institutional basis for full general economic equilibrium.

In this way of thinking, the role for policy intervention, both fiscal and monetary, arises from institutional imperfections of one kind or another that prevent the economy from achieving full general economic equilibrium. If the institutions were perfect, the economy would already be at that equilibrium, and there would be no need for policy intervention. But the institutions are not perfect, so policy intervention is needed to push the economy closer to the ideal. Finance enters the picture because proper calibration of policy intervention requires understanding how any particular intervention is transmitted through financial markets into the broader economy. Public finance and monetary economics were finance enough for economics, but as real world financial markets developed, public finance and monetary economics had to adapt to those developments by bringing in relevant bits of modern financial theory. That's what Tobin and Modigliani, among others, thought they were doing.

For both men, the key link between the ideal world of Arrow-Debreu and the large-scale econometric modeling project they were conducting was the theory of "monetary Walrasianism". And the key figure who set the agenda for monetary Walrasianism was Jacob Marschak, initially in his pre-war "Money and the Theory of Assets" (1938). The story of intellectual transmission from these origins has only recently begun to surface, but a central chapter clearly involves Marschak's role, during 1943-1948, as Research Director of the Cowles Commission. (See Mehrling 2010; also Hagemann 2006, and Cherrier 2010.)

Crucially, it was Marschak, before anyone else, who took the pre-WWII work of Irving Fisher as his starting point, especially Fisher's 1930 *The Theory of Interest* *as Determined by Impatience to Spend Income and Opportunity to Invest It*. Subsequently, post-war macroeconomics became a project of bringing the sensibility of Irving Fisher to bear on post-war problems, using updated analytical tools that had been developed in the meanwhile, many of them under the pressure of war. James Tobin, himself Research Director of the reconfigured Cowles Foundation from 1955-1961, and 1964-65, can be viewed as the standard bearer of this neo-Fisherian American-style Keynesianism (Tobin 1985, Dimand 1997).

Marschak-Tobin was the first road leading from Irving Fisher, but CAPM and modern finance was the next, and this second road led in a much more radical direction. The Marschak-Tobin monetary Walrasian project was about providing a scientific basis for government policy intervention to steer the economy closer to the full general equilibrium ideal. By contrast, the finance project was about changing financial institutions themselves to eliminate the imperfections and frictions that were the putative source of policy leverage. The goal was nothing less than to make the world ever more like the complete-markets ideal, and so also presumably ever less in need of expert policy guidance. If finance were to be successful, economics would become irrelevant.

This second road leading from Irving Fisher to modern finance is clearest in the life and work of Fischer Black, who from the first interpreted CAPM as the broader macroeconomic construct that arises when we retrace the steps of Irving Fisher but with risk, time, and equilibrium built in from the beginning. (See Mehrling 2005: 93-98.) Black's particular view has not (so far) prevailed, but the larger transformative finance project that he anticipated certainly has. In my view, future historians will see developments after about 1970 in these more fundamental terms, as a struggle between old economics versus new finance, and the contemporary quarreling between Keynesianism and monetarism *within* economics will come to seem much less important. To the generation of Tobin and Modigliani, and also Milton Friedman, "financial economics" meant applications of economics to financial topics. To the generation of today, it means applications of modern finance to economic topics.

Efficient Markets

Historically, the origin of the audacious finance project can be traced to the much more modest idea that stock market prices follow a random walk. Initially little more than an empirical characterization of the data, the “random walk theory” developed into a powerfully evocative theoretical construct called the Efficient Markets Hypothesis (Fama 1965, 1970, 1991), and then even farther into a theory of the economy as a whole. Arrow’s general equilibrium framework had envisioned the economy fundamentally as a system of real commodity exchange, with financial markets serving as a kind of subordinate mechanism to facilitate that exchange. By contrast, modern finance came to see the economy fundamentally as a system of asset pricing, with commodity exchange conceived as a kind of special case. As one measure of the intellectual distance travelled since Arrow (1953), consider the changing fortunes of the lowly option. The apparently esoteric state-contingent security of Arrow and Debreu is today seen as the fundamental building block of all of finance, and the problem of option pricing as the key to understanding asset pricing more generally.

The Efficient Markets Hypothesis originated as an attempt to explain a startling empirical fact. If you draw a chart showing how the price of a stock (or stock index) has moved over some period of time, it will typically bear a striking resemblance to similar charts showing the movement of variables that we know to be random, such as coin flips. If you keep track of a series of coin flips, counting heads as +1 and tails as -1, the cumulative sum will go up and down in a way that is hard to distinguish from the movement of a stock price. Harry Roberts (1959), a statistician at the Graduate School of Business at the University of Chicago, convinced himself of this “random walk theory” by showing charts of stock prices and cumulative coin flips to stock market analysts, who could not tell the difference. In succeeding years Roberts’ students, most notably Eugene Fama, applying more sophisticated statistical techniques to the data, came to more sophisticated versions of more or less the same conclusion.

This new statistical way of thinking about the data focused attention on two questions: the independence of successive price changes over time, and the shape of the probability distribution of price changes at a point in time. For most economists, the independence question seemed the more important of the two, since statistical independence would imply zero expected speculative profit, which economists recognized as a characterization of equilibrium in competitive markets. Thus the random walk theory, initially only a statistical characterization of the data, came to be endowed with economic content, got renamed as the Efficient Markets Hypothesis, and then adopted as the centerpiece of a distinctive Chicago-style approach to finance (Fama and Miller 1972).

The random walk formulation of EMH was soon shown to be not entirely correct (Samuelson 1965, Mandelbrot 1966); the correct statistical consequence of assuming zero expected speculative profit was a "martingale" not a random walk. What is important is not that successive increments are statistically independent, but only that current price is the conditional expectation of future price. As Samuelson stated it (under the simplifying assumption of a zero discount rate):

$$P_t = E(P_{t+1}|P_t).$$

Notwithstanding this correction — and notwithstanding also the branding effort of Cootner's *Random Character of Stock Prices* (1964) which sought to establish a distinguished ancestry for this approach in the person of Louis Bachelier (1900; Davis and Etheridge 2006) — the EMH became the brand of Chicago rather than of MIT, if only because Chicago wanted the brand while MIT did not. At MIT, efficiency was merely an idealization, unlikely to hold reliably in the real world, hence not an attractive null hypothesis and certainly not a useful maintained hypothesis (Mandelbrot 1971). Chicago went the other way.

This intellectual split, right at the origin of the modern field, would have continuing reverberations throughout the subsequent decades, as fledgling finance grew to be an enormous field in its own right. One immediate consequence showed itself in dispute over the proper interpretation of statistical tests of asset pricing theories such as CAPM. Such tests were always joint tests of both the theory and the efficient markets hypothesis, so negative results could be met either by rejecting CAPM or by rejecting efficiency (Roll 1977). Chicago reliably chose the former, while MIT reliably chose the latter (Shiller 2000).

From this point of view, the importance of the Samuelson-Mandelbrot results was not so much to correct a technicality in the mathematical formulation of EMH, but rather to endow the formulation with a deep economic interpretation, namely No Arbitrage. Once No Arbitrage was placed at the center of finance, the new field could grow essentially unhindered by any further intellectual inheritance from economics, no utility curves or production functions, no supply and demand. And so it did, for more than a decade.

No Arbitrage

The reason that finance could make progress from such minimal economic foundations is that for finance the empirical relevance of No Arbitrage is high, for two simple reasons: there are many actors (speculators) actively searching for arbitrage opportunities, and it is relatively costless for them to exploit any opportunities they find. It follows that, for finance, the principle of No Arbitrage is not merely a convenient assumption that makes it possible to derive clean theoretical results, but even more an idealization of observable empirical reality, and a characterization of the deep and simple structure underlying multifarious surface phenomena.

Crucially, it is the principle of No Arbitrage that lies behind the Fundamental Theorem of Asset Pricing which asserts the existence of a positive linear pricing rule (Ross 1976b, 1978). The most important practical implication of this Theorem is that one can price assets that are not yet traded simply by reference to the price of assets that are already traded, and without the need to invoke any particular theory of asset pricing. This feature opened the possibility of creating new assets, such as options but also other financial derivatives, that would in practical terms “complete” markets, and so help move the economy closer to the ideal characterized by Arrow (1953). This possibility subsequently served as the legitimation for the entire field of financial engineering (Ross 1976a). But the first use of the principle of No Arbitrage, and to this day the most resoundingly successful use, was to solve the previously intractable problem of pricing options. The story of how that happened is instructive.

A call option, the right to purchase a given security at a given exercise price on or before a given maturity date, is clearly a risky security, and so in principle should be amenable to pricing by means of the Capital Asset Pricing Model. Such anyway was the idea of Fischer Black when he first started to work on the problem, having been initially introduced to CAPM through the work of his colleague Jack Treynor. What made the problem hard was that the amount of risk in the option changed over time, and in a non-linear way, as the price of the underlying referenced security changed. Nevertheless, by characterizing these price dynamics, Black was able to obtain a differential equation and then, working with Myron Scholes, solve it for the famous formula

$$W(x,t) = xN(d_1) - ce^{-r(t^*-t)}N(d_2)$$

where x is the stock price, N(d) the cumulative normal density function that describes the distribution of the stock price, c the exercise price, r the rate of interest, t* the expiration date, and d_1 and d_2 are functions of these data and the variance of the stock return (Black and Scholes 1973).

Black and Scholes came to the correct formula using CAPM, but in the end it turned out that the formula did not depend on CAPM at all, or indeed on any other theory of asset pricing either. Robert Merton (1973) came to the same formula in a different way, by thinking about the option as a portfolio of stock and riskless debt, with portfolio proportions that changed over time as the price of the stock changed. (That proportion is called the hedge ratio; it is a number indicating how many shares of the referenced security should be held to hedge the risk in the option.) Assuming continuous and costless trading, the portfolio could exactly replicate the payoff from the option. Since we know the price of the replicating portfolio, it follows that we also know the price of the option. One problem with Merton's formulation was the crucial but implausible assumption of continuous and costless trading; in practice exact replication was simply impossible. To the rescue came the principle of No Arbitrage, since it allows us to price the option even if replication is not possible in practice.

The solution of the option pricing problem was just the beginning. An entire field of financial engineering was the result. Notwithstanding Merton (1990), instead of calling that field *Continuous Time Finance* and so conceptualizing it as a field of applied mathematics, as an economist I would rather emphasize the principle of No Arbitrage as the essential foundation for the field, and the source of its continuing link to economics. Crucially, it was the principle of No Arbitrage that made it possible for developments in finance to re-enter economics in the 1980s, through the unlikely mechanism of the martingale equivalence theorem (Harrison and Kreps 1979). The story of how that happened is also instructive.

In Samuelson's original work on the connection between No Arbitrage and the martingale property, he assumed an exogenously fixed discount rate, and limited his attention to asset prices. As a consequence, it was not at all clear how general his result was, since in a more general economic model the discount rate should be endogenous and should fluctuate with the economy as a whole. That means, as LeRoy (1973) and Lucas (1978) pointed out, that there is no reason to suppose that the martingale property will necessarily be a general feature of efficient asset market prices; in other words, Samuelson's results were a special case. From this point of view, Harrison and Kreps (1979) was crucial, rehabilitating the foundational place of the martingale property, by shifting attention to risk neutral pricing under a martingale equivalent probability measure. In doing so, they also made it possible to treat No Arbitrage and the martingale property as foundational for economics, as well as finance.

Finance and General Equilibrium, Redux

Just so, we can understand the work of Cox, Ingersoll, and Ross (1981, 1985a, 1985b) as an attempt to connect the insights of No Arbitrage back to economic "fundamentals". "In work on contingent claims analysis, such as option pricing, it is common, and to a first approximation reasonable, to insist only on a partial equilibrium between the prices of the primary and derivative assets. For something as fundamental as the rate of interest, however, a general equilibrium model is to be preferred." (CIR 1981, 773). To do this, the authors built on Ross's own Arbitrage Pricing theory (Ross 1976b) to produce a general equilibrium model driven by a k-dimensional vector of state variables. In the end they were forced to specialize the model considerably in order to achieve definite results for the dynamics of interest rates and the term structure, and as a consequence economics did not pick it up.

A more successful attempt to do essentially the same thing was the Real Business Cycle literature which started from the other end, with its foundations in economics, and then imported No Arbitrage under the disarming moniker of rational expectations. The connection to finance was made through the Euler equation of consumption that lies at the heart of both modern finance and modern macroeconomics. Economists like to write the equation like this:

$$U'(C_{it}) = E_t[\delta U'(C_{it+1})R_{jt+1}].$$

In this equation C_{it} is the consumption of consumer i at time t, U is a function that translates consumption into utility terms, δ is the subjective discount rate, and R_{jt+1} is the gross return on asset j in the period between t and t+1.

For finance, this equation is about how asset prices depend on time and risk preference, the equation is called the "consumption CAPM", and the asset in question is typically equity or long term bonds (Breeden 1979). But the same equation can be used to talk about the intertemporal fluctuation of income, and as such is at the core of both Real Business Cycle theory (Kydland and Prescott 1982) and its New Keynesian variants (Woodford 2003) that see the economy through the lens of the so-called Dynamic Stochastic General Equilibrium (DSGE) model. In this application, the asset is typically capital, or a rate of interest.

The link to finance, and the foundational role of No Arbitrage and the martingale, comes clear when we write the Euler equation instead as

$$1 = E_t[M_{it+1}R_{jt+1}]$$

where M is a stochastic discount factor treated as a free variable that must fit the cross section pattern of asset returns R. (Note how the characteristic utility foundations of economics disappear in this finance formulation.) Under the martingale equivalent measure, this equation can equivalently be written as

$$1 = E^M{}_t[R_{t+1}] \text{ or } P_t = E^M{}_t[P_{t+1}],$$

the latter formulation being exactly analogous to Samuelson (1965).

Thus it is that modern finance, having grown up outside of economics and independent from almost all of the intellectual inheritance of the field, re-entered economics and in doing so quite completely transformed it. The Monetary Walrasianism of Tobin and Modigliani survives in elementary textbooks, and in policy discussion circles, but hardly a trace can be found in the advanced academic literature. Originally conceived as an account of how government can make use of imperfections in markets as leverage for moving the economy closer to the complete markets ideal, Monetary Walrasianism has in the last forty years had its institutional and intellectual foundations cut out from under it by a combination of financial innovation and de-regulation, both domestically and internationally. The audacious finance project has come to pass; is economics irrelevant?

The Future of Finance

The consequence of financial innovation and de-regulation has quite clearly *not* been the achievement of the full general equilibrium ideal posited in Arrow (1953). Indeed, the global financial crisis of 2007-2009 has brought with it widespread questioning of both the DSGE model (in economics) and the EMH (in finance). Arrow himself was prescient about the limitations of both, insisting repeatedly in print that the problems of risk and time are not really addressed within the intertemporal general equilibrium model (Arrow 1978, 159; see also Arrow 1981). Indeed, as Frank Hahn, among others, has repeatedly emphasized, the model has no place in it for money (Hahn 1965).

That is the fundamental reason that academic economics and finance had so little to say when the crisis struck. The crisis was essentially about money and liquidity, matters from which both economics and finance had resolutely abstracted (Mehrling 2011). Before the crisis, most people thought that efficient markets would be liquid markets, because there would always be a buyer willing to step forward when price fell even a little bit below fundamental value. "Not so", is the verdict of history, and "Why not so?" is the question now confronting the field of financial economics.

This reading of history suggests that the road forward, both for finance and for economics, and hence *a fortiori* for financial economics, will involve a shift of focus to matters of money and liquidity. The crisis has made clear that the modern real world is in important ways not like the ideal world posited long ago by intertemporal general equilibrium theory. As a consequence, public finance and monetary economics that are oriented around that ideal world are simply not finance enough for modern economics. During the crisis, states were called upon to act and they did so, but without much help from economics. Public entities, just as much as private entities, confronted the problem of managing their affairs in the face of the "dark forces of time and ignorance". As in the past, we can expect the future fertility of financial economics to arise from attempts to respond to practical problems, both public and private, in the real world.

PERRY MEHRLING

See also:

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