My Life in Finance

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Foreword

I was invited by the editors to contribute a professional autobiography for the *Annual Review of Financial Economics*. I focus on what I think is my best stuff. Readers interested in the rest can download my vita from the website of the University of Chicago, Booth School of Business. I only briefly discuss ideas and their origins, to give the flavor of context and motivation. I do not attempt to review the contributions of others, which is likely to raise feathers. *Mea culpa* in advance.

Finance is the most successful branch of economics in terms of theory and empirical work, the interplay between the two, and the penetration of financial research into other areas of economics and real-world applications. I have been doing research in finance almost since its start, when Markowitz (1952, 1959) and Modigliani and Miller (1958) set the field on the path to become a serious scientific discipline. It has been fun to see it all, to contribute, and to be a friend and colleague to the giants who created the field.

Origins

My grandparents emigrated to the U.S. from Sicily in the early 1900s, so I am a third generation Italian-American. I was the first in the lineage to go to university.

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My passion in high school was sports. I played basketball (poorly), ran track (second in the state meet in the high jump – not bad for a 5'8" kid), played football (class B state champions), and baseball (state semi-finals two years). I claim to be the inventor of the split end position in football, an innovation prompted by the beatings I took trying to block much bigger defensive tackles. I am in my high school's (Malden Catholic) athletic hall of fame.

I went on to Tufts University in 1956, intending to become a high school teacher and sports coach. At the end of my second year, I married my high school sweetheart, Sallyann Dimeco, now my wife of more than 50 years. We have four adult children and ten delightful grandchildren. Sally's family contributions dwarf mine.

At Tufts I started in romance languages but after two years became bored with rehashing Voltaire and took an economics course. I was enthralled by the subject matter and by the prospect of escaping lifetime starvation on the wages of a high school teacher. In my last two years at Tufts, I went heavy on economics. The professors, as teachers, were as inspiring as the research stars I later profited from at the University of Chicago.

My professors at Tufts encouraged me to go to graduate school. I leaned toward a business school Ph.D. My Tufts professors (mostly Harvard economics Ph.D.s) pushed Chicago as the business school with a bent toward serious economics. I was accepted at other schools, but April 1960 came along and I didn't hear from Chicago. I called and the dean of students, Jeff Metcalf, answered. (The school was much smaller then.) They had no record of my application. But Jeff and I hit it off, and he asked about my grades. He said Chicago had a scholarship reserved for a qualified Tufts graduate. He asked if I wanted it. I accepted and, except for two great years teaching in Belgium, I have been at the University of Chicago since

1960. I wonder what path my professional life would have taken if Jeff didn't answer the phone that day. Serendipity!

During my last year at Tufts, I worked for Harry Ernst, an economics professor who also ran a stock market forecasting service. Part of my job was to invent schemes to forecast the market. The schemes always worked on the data used to design them. But Harry was a good statistician, and he insisted on out-of-sample tests. My schemes invariably failed those tests. I didn't fully appreciate the lesson in this at the time, but it came to me later.

During my second year at Chicago, with an end to course work and prelims in sight, I started to attend the Econometrics Workshop, at that time the hotbed for research in finance. Merton Miller had recently joined the Chicago faculty and was a regular participant, along with Harry Roberts and Lester Telser. Benoit Mandelbrot was an occasional visitor. Benoit presented in the workshop several times, and in leisurely strolls around campus, I learned lots from him about fat-tailed stable distributions and their apparent relevance in a wide range of economic and physical phenomena. Merton Miller became my mentor in finance and economics (and remained so throughout his lifetime). Harry Roberts, a statistician, instilled a philosophy for empirical work that has been my north star throughout my career.

Efficient Markets

Miller, Roberts, Telser, and Mandelbrot were intensely involved in the burgeoning work on the behavior of stock prices (facilitated by the arrival of the first reasonably powerful computers). The other focal point was MIT, with Sydney Alexander, Paul Cootner, Franco Modigliani, and Paul Samuelson. Because his co-author, Merton Miller, was now at Chicago, Franco was a frequent visitor. Like Merton, Franco was unselfish and tireless in helping people think through research ideas. Franco and Mert provided an open conduit for cross-fertilization of market research at the two universities.

At the end of my second year at Chicago, it came time to write a thesis, and I went to Miller with five topics. Mert always had uncanny insight about research ideas likely to succeed. He gently stomped on four of my topics, but was excited by the fifth. From my work for Harry Ernst at Tufts, I had daily data on the 30 Dow-Jones Industrial Stocks. I proposed to produce detailed evidence on (1) Mandelbrot's hypothesis that stock returns conform to non-normal (fat-tailed) stable distributions and (2) the time-series properties of returns. There was existing work on both topics, but I promised a unifying perspective and a leap in the range of data brought to bear.

Vindicating Mandelbrot, my thesis (Fama 1965a) shows (in nauseating detail) that distributions of stock returns are fat-tailed: there are far more outliers than would be expected from normal distributions – a fact reconfirmed in subsequent market episodes, including the most recent. Given the accusations of ignorance on this score recently thrown our way in the popular media, it is worth emphasizing that academics in finance have been aware of the fat tails phenomenon in asset returns for about 50 years.

My thesis and the earlier work of others on the time-series properties of returns falls under what came to be called tests of market efficiency. I coined the terms "market efficiency" and "efficient markets," but they do not appear in my thesis. They first appear in "Random Walks in Stock Market Prices," paper number 16 in the series of *Selected Papers of the Graduate School of Business*, *University of Chicago*, reprinted in the *Financial Analysts Journal* (Fama 1965b).

From the inception of research on the time-series properties of stock returns, economists speculated about how prices and returns behave if markets work, that is, if prices fully reflect all available information. The initial theory was the random walk model. In two important papers, Samuelson (1965) and Mandelbrot (1966) show that the random walk prediction (price changes are iid) is too strong. The proposition that prices fully reflect available information implies only that prices are sub-martingales. Formally, the deviations of price changes or returns from the values required to compensate investors for time and risk-bearing have expected value equal to zero conditional on past information.

During the early years, in addition to my thesis, I wrote several papers on market efficiency (Fama 1963, 1965c, Fama and Blume 1966), now mostly forgotten. My main contribution to the theory of efficient markets is the 1970 review (Fama 1970). The paper emphasizes the joint hypothesis problem hidden in the sub-martingales of Mandelbrot (1966) and Samuelson (1965). Specifically, market efficiency can only be tested in the context of an asset pricing model that specifies equilibrium expected returns. In other words, to test whether prices fully reflect available information, we must specify how the market is trying to compensate investors when it sets prices. My cleanest statement of the theory of efficient markets is in chapter 5 of Fama (1976b), reiterated in my second review "Efficient Markets II" (Fama 1991a).

The joint hypothesis problem is obvious, but only on hindsight. For example, much of the early work on market efficiency focuses on the autocorrelations of stock returns. It was not recognized that market efficiency implies zero autocorrelation only if the expected returns that investors require to hold stocks are constant through time or at least serially uncorrelated, and both conditions are unlikely.

The joint hypothesis problem is generally acknowledged in work on market efficiency after Fama (1970), and it is understood that, as a result, market efficiency per se is not testable. The flip side of the joint hypothesis problem is less often acknowledged. Specifically, almost all asset pricing models assume asset markets are efficient, so tests of these models are joint tests of the models and market efficiency. Asset pricing and market efficiency are forever joined at the hip.

Event Studies

My Ph.D. thesis and other early work on market efficiency do not use the CRSP files, which were not yet available. When the files became available (thanks to years of painstaking work by Larry Fisher), Jim Lorie, the founder of CRSP, came to me worried that no one would use the data and CRSP would lose its funding. He suggested a paper on stock splits, to advertise the data. The result is Fama, Fisher, Jensen, and Roll (1969). This is the first study of the adjustment of stock prices to a specific kind of information event. Such "event studies" quickly became a research industry, vibrant to this day, and the main form of tests of market efficiency. Event studies have also found a practical application -- calculating damages in legal cases.

The refereeing process for the split study was a unique experience. When more than a year passed without word from the journal, we assumed the paper would be rejected. Then a short letter arrived. The referee (Franco Modigliani) basically said: it's great, publish it. Never again would this happen!

There is a little appreciated fact about the split paper. It contains no formal tests (standard errors, t-statistics, etc.) The results were apparently so convincing as confirmation of market efficiency that formal tests seemed irrelevant. But this was before the joint hypothesis problem was recognized, and only much later did we come to appreciate that results in event studies can be sensitive to methodology, in particular, what is assumed about equilibrium expected returns -- a point emphasized in Fama (1998).

Michael Jensen and Richard Roll are members of a once-in-a-lifetime cohort of Ph.D. students that came to Chicago soon after I joined the faculty in 1963. Also in this rough cohort are (among others) Ray Ball, Marshall Blume, James MacBeth, Myron Scholes, and Ross Watts. I think I was chairman of all their thesis committees, but Merton Miller and Harry Roberts were deeply involved. Any investment in these and about 100 other Ph.D. students I have supervised has been repaid many times by what I learn from them during their careers.

Forecasting Regressions

In 1975 I published a little empirical paper, "Short-Term Interest Rates as Predictors of Inflation" (Fama 1975). The topic wasn't new, but my approach was novel. Earlier work uses regressions of the interest rate on the inflation rate for the period covered by the interest rate. The idea is that the expected inflation rate (along with the expected real return) determines the interest rate, so the interest rate should be the dependent variable and the expected inflation rate should be the independent variable. The observed inflation rate is, of course, a noisy proxy for its expected value, so there is a measurement error problem in the regression of the ex ante interest rate on the ex post inflation rate.

My simple insight is that a regression estimates the conditional expected value of the left-hand-side variable as a function of the right-hand-side variables. Thus, to extract the forecast of inflation in the interest rate (the expected value of inflation priced into the interest rate) one regresses the ex post inflation rate on the ex ante interest rate. On hindsight, this is the obvious way to run the forecasting regression, but again it wasn't obvious at the time.

There is a potential measurement error problem in the regression of the ex post inflation rate on the ex ante (T-bill) interest rate, caused by variation through time in the expected real return on the bill. The model of market equilibrium in "Short-Term Interest Rates as Predictors of Inflation" assumes that the expected real return is constant, and this seems to be a reasonable approximation for the 1953-1971 period of the tests. (It doesn't work for any later period.) This result raised a furor among Keynesian macroeconomists who postulated that the expected real return was a policy variable that played a central role in controlling investment and business cycles. There was a full day seminar on my paper at MIT, where my simple result was heatedly attacked. I argued that I didn't know what the fuss was about, since the risk premium component of the cost of capital is surely more important than the riskfree real rate, and it seems unlikely that monetary and fiscal actions can fine tune the risk premium. I don't know if I won the debate, but it was followed by a tennis tournament, and I think I did win that.

The simple idea about forecasting regressions in Fama (1975) has served me well, many times. (When I have an idea, I beat it to death.) I have many papers that use the technique to extract the forecasts of future spot rates, returns, default premiums, etc., in the term structure of interests rates, for example Fama (1976a,c, 1984b, 1986, 1990b, 2005), Fama and Schwert (1979), Fama and Bliss (1987). In a blatant example of intellectual arbitrage, I apply the technique to study forward foreign exchange rates as predictors of future spot rates, in a paper (Fama 1984a) highly cited in that literature. The same technique is used in my work with Kenneth R. French and G. William Schwert on the predictions of stock returns in dividend yields and other variables (Fama and Schwert 1977, Fama and French 1988, 1989). And regressions of ex post variables on ex ante variables are now standard in forecasting studies, academic and applied.

Agency Problems and the Theory of Organizations

In 1976 Michael Jensen and William Meckling published their groundbreaking paper on agency problems in investment and financing decisions (Jensen and Meckling 1976). According to Kim, Morse, and Zingales (2006), this is the second most highly cited theory paper in economics published in the 1970-2005 period. It fathered an enormous literature.

When Mike came to present the paper at Chicago, he began by claiming it would destroy the corporate finance material in what he called the "white bible" (Fama and Miller, *The Theory of Finance* 1972). Mert and I replied that his analysis is deeper and more insightful, but in fact there is a discussion of stockholder-bondholder agency problems in chapter 4 of our book. Another example that new ideas are almost never completely new!

Spurred by Jensen and Meckling (1976), my research took a turn into agency theory. The early papers on agency theory emphasized agency problems. I was interested in studying how competitive forces lead to the evolution of mechanisms to mitigate agency problems. The first paper, "Agency Problems and the Theory of the Firm" (Fama 1980a) argues that managerial labor markets, inside and outside of firms, act to control managers faced with the temptations created by diffuse residual claims that reduce the incentives of individual residual claimants to monitor managers.

I then collaborated with Mike on three papers (Fama and Jensen 1983a,b, 1985) that study more generally how different mechanisms arise to mitigate the agency problems associated with "separation of ownership and control" and how an organization's activities and the special agency problems they pose, affect the nature of its residual claims and control mechanisms. For example, we argue that the redeemable residual claims of a financial mutual (for example, an open end mutual fund) provide strong discipline for its managers, but redeemability is cost effective only when the assets of the organization can be sold quickly with low transactions costs. We also argue that the nonprofit format, in which no agents have explicit residual claims to net cashflows, is a response to the agency problem associated with activities in which there is a potential supply of donations that might be expropriated by residual claimants. Two additional papers (Fama 1990a, 1991b) spell out some of the implications of Fama (1980a) and Fama and Jensen (1983a,b) for financing decisions and the nature of labor contracts.

Kim, Morse, and Zingales (2006) list the 146 papers published during 1970-2005 that have more than 500 cites in the major journals of economics. I'm blatantly bragging, but Fama (1980a) and Fama and Jensen (1983a) are among my six papers on the list. (The others are Fama 1970, Fama and MacBeth 1973, Fama and French 1992, 1993. If the list extended back to ancient times, Fama 1965a and Fama, Fisher, Jensen, and Roll 1969 would also make it.) I think of myself as an empiricist (and a simple-minded one at that), so I like my work in agency theory since it suggests that occasionally theoretical ideas get sprinkled into the mix.

Macroeconomics

Toward the end of the 1970s, around the time of the agency theory research, my work took a second turn into macroeconomics and international finance. Fischer Black had similar interests, and I profited from many long discussions with him on this and other issues during the years he spent at Chicago in the office next to mine.

Since they typically assume away transactions costs, asset pricing models in finance do not have a natural role for money. Fama and Farber (1979) model a world in which financial markets are indeed frictionless, but there are transactions costs in consumption that are reduced by holding money. Money then becomes a portfolio asset, and we investigate how nominal bonds (borrowing and lending) allow consumer-investors to split decisions about how much money to hold for transactions purposes from decisions about how much of the purchasing power risk of their money holdings they will bear. We also investigate the pricing of the purchasing power risk of the money supply in the context of the CAPM.

Extending the analysis to an international setting, Fama and Farber (1979) show that exchange rate uncertainty is not an additional risk in international investing when purchasing power parity (PPP) holds, because PPP implies that the real return on any asset is the same to the residents of all countries. The point is obvious, on hindsight, but previous papers in the international asset pricing literature assume that exchange rate uncertainty is an additional risk, without saying anything about PPP, or saying something incorrect.

Three subsequent papers (Fama 1980b, 1983, 1985) examine what the theory of finance says about the role of banks. The first two (Fama 1980b, 1983) argue that in the absence of reserve requirements, banks are just financial intermediaries, much like mutual funds, that manage asset portfolios on behalf of depositors. And like mutual fund holdings, the quantity of deposits has no role in price level determination (inflation). Bank deposits also provide access to an accounting system of exchange (via checks and electronic transfers) that is just an efficient mechanism for moving claims on assets from some consumer-investors to others, without the intervention of a hand-to-hand medium of exchange like currency. Because it pays less than full interest, currency has an important role in price level determination. The role of deposits in price level determination is, however, artificial, induced by the requirement to hold “reserves” with the central bank that pay less than full interest and are exchangeable for currency on demand.

Corporate Finance

As finance matured, it became more specialized. The teaching and research of new people tends to focus entirely on asset pricing or corporate finance. It wasn’t always so. Until several years ago, I taught both. More of my research is in asset-pricing-market-efficiency (66 papers and 1.5 books), but as a result of longtime exposure to Merton Miller, I have always been into corporate finance (15 papers and half a book).

The burning issue in corporate finance in the early 1960s was whether the propositions of Modigliani and Miller (MM 1958) and Miller and Modigliani (MM 1961) about the value irrelevance of financing decisions hold outside the confines of their highly restrictive risk classes (where a risk class includes firms with perfectly correlated net cashflows). With the perspective provided by asset pricing models, which were unavailable to MM, it became clear that their propositions do not require their risk classes. Fama (1978) tries to provide a capstone. The paper argues that the MM propositions hold in any asset pricing model that shares the basic MM assumptions (perfect capital market, including no taxes, no transactions costs, and no information asymmetries or agency problems), as long as either (i) investors and firms have equal access to the capital market (so investors can undo the financing decisions of firms), or (ii) there are perfect substitutes for the securities issued by any firm (with perfect substitute defined by whatever happens to be the right asset pricing model).

The CRSP files opened the gates for empirical asset pricing research (including work on efficient markets). Compustat similarly provides the raw material for empirical work in corporate finance. Fama and Babiak (1968) leap on the new Compustat files to test Lintner's (1956) hypothesis that firms have target dividend payouts but annual dividends only partially adjust to their targets. Lintner estimates his model on aggregate data. We examine how the model works for the individual firms whose dividend decisions it is meant to explain. It works well in our tests, and it continues to work in subsequent trials (e.g., Fama 1974). But the speed-of-adjustment of dividends to their targets has slowed considerably, that is, dividends have become more "sticky" (Fama and French 2002). The more interesting fact, however, is the gradual disappearance of dividends. In 1978 almost 80% of NYSE-Amex-Nasdaq listed firms paid dividends, falling to about 20% in 1999 (Fama and French 2001).

Post-MM corporate finance has two main theories, the pecking order model of Myers (1984) and Myers and Majluf (1984) and the tradeoff model (which has many authors). These theories make predictions about financing decisions when different pieces of the perfect capital markets assumption of MM do not hold. The pecking order model does reasonably well, until the early 1980s when new issues of common stock (which the model predicts are rare) become commonplace (Fama and French 2005). There is some empirical support for the leverage targets that are the centerpiece of the tradeoff model, but the speed-of-adjustment of leverage to its targets is so slow that the existence of targets becomes questionable. (This is the conclusion of Fama and French 2002 and other recent work.) In the end, it's not clear that the capital structure irrelevance propositions of Modigliani and Miller are less realistic as rough approximations than the popular alternatives. (This is the conclusion of Fama and French 2002.)

In my view, the big open challenge in corporate finance is to produce evidence on how taxes affect market values and thus optimal financing decisions. Modigliani and Miller (1963) suggest that debt has large tax benefits, and taxation disadvantages dividends. To this day, this is the position commonly advanced in corporate finance courses. Miller (1977), however, presents a scenario in which the tax benefits of debt due to the tax deductibility of interest payments at the corporate level are offset by taxation of interest receipts at the personal level, and leverage has no effect on a firm's market value. Miller and Scholes (1978) present a scenario in which dividend and debt choices have no effect on the market values of firms. Miller (1977) and Miller and Scholes (1978) recognize that that there are scenarios in which taxes do affect optimal dividend and debt decisions. In the end, the challenge is empirical measurement of tax effects (the marginal tax rates implicit) in the pricing of dividends and interest. So far the challenge goes unmet.

Fama and French (1998) take a crack at this first order issue, without success. The problem is that dividend and debt decisions are related to expected net cashflows – the main determinant of the market value of a firm's securities. Because proxies for expected net cashflows are far from perfect, the cross-section regressions of Fama and French (1998) do not produce clean estimates of how the taxation of dividends and interest affects the market values of a firm's stocks and bonds. There are also papers that just assume debt has tax benefits that can be measured from tax rate schedules. Without evidence on the tax effects in the pricing of interest, such exercises are empty.

The CAPM

Without being there one can't imagine what finance was like before formal asset pricing models. For example, at Chicago and elsewhere, investments courses were about security analysis: how to pick undervalued stocks. In 1963 I taught the first course at Chicago devoted to Markowitz' (1959) portfolio model and its famous offspring, the asset pricing model (CAPM) of Sharpe (1964) and Lintner (1965).

The CAPM provides the first precise definition of risk and how it drives expected return, until then vague and sloppy concepts. The absence of formal models of risk and expected return placed serious limitations on research that even grazed the topic. For example, the path breaking paper of Modigliani and Miller (1958) uses arbitrage within risk classes to show that (given their assumptions) financing decisions do not affect a firm's market value. They define a risk class as firms with perfectly correlated net cash flows. This is restrictive and it led to years of bickering about the applicability of their analysis and conclusions. The problem was due to the absence of formal asset pricing models that define risk and how it relates to expected return.

The arrival of the CAPM was like the time after a thunderstorm, when the air suddenly clears. Extensions soon appeared, but the quantum leaps are the intertemporal model (ICAPM) of Merton (1973a), which generalizes the CAPM to a multiperiod world with possibly multiple dimensions of risk, and the consumption CAPM of Lucas (1978), Breeden (1979), and others.

Though not about risk and expected return, any history of the excitement in finance in the 1960s and 1970s must mention the options pricing work of Black and Scholes (1973) and Merton (1973b). These are the most successful papers in economics – ever – in terms of academic and applied impact. Every Ph.D. student in economics is exposed to this work, and the papers are the foundation of a massive industry in financial derivatives.

There are many early tests of the CAPM, but the main survivors are Black, Jensen, and Scholes (BJS 1972) and Fama and MacBeth (1973). Prior to these papers, the typical test of the CAPM was a cross-section regression of the average returns on a set of assets on estimates of their market βs and other variables. (The CAPM predicts, of course, that the β premium is positive, and β suffices to describe the cross-section of expected asset returns.) BJS were suspicious that the slopes in these cross-section regressions seemed too precise (the reported standard errors seemed too small). They guessed rightly that the problem was the OLS assumption that there is no cross-correlation in the regression residuals.

Fama and MacBeth (1973) provide a simple solution to the cross-correlation problem. Instead of a regression of average asset returns on their βs and other variables, one does the regression month-by-month. The slopes are then monthly portfolio returns whose average values can be used to test the CAPM predictions that the β premium is positive and other variables add nothing to the explanation of the cross-section of expected returns. (The point is explained best in chapter 8 of Fama 1976b.) The month-by-month variation in the regression slopes captures all effects of the cross-correlation of the regression residuals, and these effects are automatically embedded in the time-series standard errors of the average slopes. The approach thus captures residual covariances without requiring an estimate of the residual covariance matrix.

The Fama-MacBeth approach is standard in tests of asset pricing models that use cross-section regressions, but the benefits of the approach carry over to panels (time series of cross-sections) of all sorts. Kenneth French and I emphasize this point (advertise is more accurate) in our corporate finance empirical work (e.g., Fama and French 1998, 2002). Outside of finance, research in economics that uses panel regressions has only recently begun to acknowledge that residual covariance is a pervasive problem. Various new robust regression techniques are available, but the Fama-MacBeth approach remains a simple option.

Given the way my recent empirical work with Kenneth French dumps on the CAPM, it is only fair to acknowledge that the CAPM gets lots of credit for forcing money managers to take more seriously the challenges posed by the work on efficient markets. Before the CAPM, money management was entirely active, and performance reporting was shoddy. The CAPM gave us a clean story about risk and expected return (i.e., a model of market equilibrium) that allowed us to judge the performance of active managers. Using the CAPM, Jensen (1968) rang the bell on the mutual fund industry. Performance evaluation via the CAPM quickly became standard both among academics and practitioners, passive management got a foothold, and active managers became aware that their feet would forever be put to the fire.

The Three-Factor Model

The evidence in Black, Jensen, and Scholes (1972) and Fama and MacBeth (1973) is generally favorable to the CAPM, or at least to Black's (1972) version of the CAPM. Subsequently, violations of the model, labeled anomalies, begin to surface. Banz (1981) finds that β does not fully explain the higher average returns of small (low market capitalization) stocks. Basu (1983) finds that the positive relation between the earning-price ratio (E/P) and average return is left unexplained by market β. Rosenberg, Reid, and Lanstein (1985) find a positive relation between average stock return and the book-to-market ratio (B/M) that is missed by the CAPM. Bhandari (1988) documents a similar result for market leverage (the ratio of debt to the market value of equity, D/M). Ball (1978) and Keim (1988) argue that variables like size, E/P, B/M, and D/M are natural candidates to expose the failures of asset pricing models as explanations of expected returns since all these variables use the stock price, which, given expected dividends, is inversely related to the expected stock return.

The individual papers on CAPM anomalies did not seem to threaten the dominance of the model. My guess is that viewed one at a time, the anomalies seemed like curiosity items that show that the CAPM is just a model, an approximation that can't be expected to explain the entire cross-section of expected stock returns. I see no other way to explain the impact of Fama and French (1992), "The Cross-Section of Expected Stock Returns," which contains nothing new. The CAPM anomalies in the paper are those listed above, and the evidence that there is no reliable relation between average return and market β was available in Reinganum (1981) and Lakonishok and Shapiro (1986). Apparently, seeing all the negative evidence in one place led readers to accept our conclusion that the CAPM just doesn't work. The model is an elegantly simple and intuitively appealing *tour de force* that laid the foundations of asset pricing theory, but its major predictions seem to be violated systematically in the data.

An asset pricing model can only be dethroned by a model that provides a better description of average returns. The three-factor model (Fama and French 1993) is our shot. The model proposes that along with market β, sensitivities to returns on two additional portfolios, SMB and HML, explain the cross-section of expected stock returns. The size factor, SMB, is the difference between the returns on diversified portfolios of small and big stocks, and the value/growth factor, HML, is the difference between the returns on diversified portfolios of high and low B/M (i.e., value and growth) stocks. The SMB and HML returns are, of course, brute force constructs designed to capture the patterns in average returns related to size and value versus growth stocks that are left unexplained by the CAPM.

Ken French and I have many papers that address questions about the three-factor model and the size and value/growth patterns in average returns the model is meant to explain. For example, to examine whether the size and value/growth patterns in average returns observed by Fama and French (1992) for the post 1962 period are the chance result of data dredging, Davis, Fama, and French (2000) extend the tests back to 1927, and Fama and French (1998) examine international data. The results are similar to those in Fama and French (1992). Fama and French (1996, 2008) examine whether the three-factor model can explain the anomalies that cause problems for the CAPM. The three-factor model does well on the anomalies associated with variants of price ratios, but it is just a model and it fails to absorb some other anomalies. The most prominent is the momentum in short-term returns documented by Jegadeesh and Titman (1993), which is a problem for all asset pricing models that do not add exposure to momentum as an explanatory factor. After 1993, work, both academic and applied, directed at measuring the performance of managed portfolios routinely use the benchmarks provided by the three-factor model, often augmented with a momentum factor (for example, Carhart 1997, and more recently Kosowski et al. 2006 or Fama and French 2009).

From its beginnings there has been controversy about how to interpret the size and especially the value/growth premiums in average returns captured by the three-factor model. Fama and French (1993, 1996) propose a multifactor version of Merton's (1973a) ICAPM. The weakness of this position is the question it leaves open. What are the state variables that drive the size and value premiums, and why do they lead to variation in expected returns missed by market β? There is a literature that proposes answers to this question, but in my view the evidence so far is unconvincing.

The chief competitor to our ICAPM risk story for the value premium is the overreaction hypothesis of DeBondt and Thaler (1987) and Lakonishok, Shleifer, and Vishny (1994). They postulate that market prices overreact to the recent good times of growth stocks and the bad times of value stocks. Subsequent price corrections then produce the value premium (high average returns of value stocks relative to growth stocks). The weakness of this position is the presumption that investors never learn about their behavioral biases, which is necessary to explain the persistence of the value premium.

Asset pricing theory typically assumes that portfolio decisions depend only on the properties of the return distributions of assets and portfolios. Another possibility, suggested by Fama and French (2007) and related to the stories in Daniel and Titman (1997) and Barberis and Shleifer (2003), is that tastes for other characteristics of assets, unrelated to properties of returns, also play a role. ("Socially responsible investing" is an example.) Perhaps many investors simply get utility from holding growth stocks, which tend to be profitable fast-growing firms, and they are averse to value stocks, which tend to be relatively unprofitable with few growth opportunities. If such tastes persist, they can have persistent effects on asset prices and expected returns, as long as they don't lead to arbitrage opportunities.

To what extent is the value premium in expected stock returns due to ICAPM state variable risks, investor overreaction, or tastes for assets as consumption goods? We may never know. Moreover, given the blatant empirical motivation of the three-factor model (and the four-factor offspring of Carhart 1997), perhaps we should just view the model as an attempt to find a set of portfolios that span the mean-variance-efficient set and so can be used to describe expected returns on all assets and portfolios (Huberman and Kandel 1987).

The academic research on the size and value premiums in average stock returns has transformed the investment management industry, both on the supply side and on the demand side. Whatever their views about the origins of the premiums, institutional investors commonly frame their asset allocation decisions in two dimensions, size and value versus growth, and the portfolio menus offered by money managers are typically framed in the same way. And it is testimony to the credibility of research in finance that all this happened in a very short period of time.

Conclusions

The first 50 years of research in finance has been a great ride. I'm confident finance will continue to be a great ride into the indefinite future.

Addendum – Provided by the tenured finance faculty of Chicago Booth

When my paper was posted on the Forum of the website of the Chicago Booth Initiative on Global Markets, the tenured finance faculty introduced it with the following comments. EFF

This post makes available an autobiographical note by Gene Fama that was commissioned by the *Annual Review of Financial Economics*. Gene's remarkable career and vision, to say nothing of his engaging writing style, make this short piece a must read for anyone interested in finance. However, as his colleagues, we believe his modesty led him to omit three crucial aspects of his contributions.

First, Gene was (and still is) essential to shaping the nature of the finance group at Chicago. As he explains in a somewhat understated fashion, he and Merton Miller transformed the finance group turning it into a research oriented unit. For the last 47 years he has held court on Tuesday afternoons in the finance workshop, in a room that now bears his name. Through the workshop, generations of students, colleagues, and visitors have been and continue to be exposed to his research style of developing and rigorously testing theories with real world data that has become the hallmark of Chicago finance.

Second, and equally important, is his leadership. Rather than rest on his laurels or impose his own views on the group, Gene has always sought the truth, even when it appeared at odds with his own views. He has promoted a contest of ideas and outlooks, all subject to his exceptional standards of quality. The makeup of the group has shifted as the world and what we know about it has changed. The current finance group at Chicago includes a diverse set of people who specialize in all areas of modern finance including, behavioral economics, pure theory, and emerging, non-traditional areas such as entrepreneurship and development that were unheard of when Gene arrived at Chicago. Contrary to the caricatured descriptions, there is no single Chicago view of finance, except that the path to truth comes from the rigorous development and confrontation of theories with data.

Finally, each of us has our own personal examples of Gene's generosity, kindness and mentorship. He is an impeccable role model. He is in his office every day, and his door is always open. By personal example, he sets the standards for the values and ethics by which we do research and run our school. All of us have learned enormously from Gene's generous willingness to discuss his and our work, and gently and patiently to explain and debate that work with generations of faculty. Gene likely enjoys as high a ranking in the "thanks for comments" footnotes of published papers as he does in citations. He has made the finance group an exciting, collegial, and welcoming place to work. He has greatly enhanced all of our research careers and accomplishments. He is a great friend, and we can only begin to express our gratitude.

We hope you enjoy reading Gene's description of his career that might just as well be described as the story of how modern finance evolved at Chicago.

Gene's Tenured Finance Faculty Colleagues at Chicago Booth

John H. Cochrane, George M. Constantinides, Douglas W. Diamond, Milton Harris, John C. Heaton, Steven Neil Kaplan, Anil K Kashyap, Richard Leftwich, Tobias J. Moskowitz, Lubos Pastor, Raghuram G. Rajan, Richard Thaler, Pietro Veronesi, Robert W. Vishny, and Luigi Zingales

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