portfolio construction forum

The takeaways from the latest Fama-French research

Michael Edesess | Fair Advisors | 25 March 2014

Eugene Fama and Kenneth French's research has gained considerable attention in the world of investment finance since their articles on size and value effects in 1992[1] and 1993[2]. Their latest work, A Five-Factor Asset Pricing Model[3], covers five factors – size (i.e. capitalisation), value (i.e. book-to-market ratio), broad market factor, profitability (profits-to-assets ratio) and investment (how much of a company's earnings it invests in new or expanded ventures).

My question is whether this work provides any information that can be of practical value to advisers or investors. After careful evaluation of their paper, I conclude that the answer is no. The work is too opaque to allow thorough independent analysis or confirmation, and it provides no explanation or motivation to believe that its findings based on historical data have any implications for future investments.

AN ARCANE AND ALMOST IMPENETRABLE PRACTICE OF EXPOSITION

The first problem with the recent Fama-French paper is that it is extremely difficult for a layperson to understand and glean details of how the work was performed or how the models were constructed. Many academic fields suffer from this problem – indeed, in some fields it is simply unavoidable. But here, details that could have been provided are apparently either assumed to be already known to the reader or else are simply overlooked.

The problem with this pattern of academic behavior is that it renders the field insular and unable to take on critical analysis from outside the group of thoroughly indoctrinated participants. It also makes it difficult for analysts who are not within that sphere to evaluate anything for potential practical applications. Those who do so anyway often draw the wrong implications from the work or simply use it to back up claims that cannot be checked.

One result of this opacity is that some of the most crucially important distinctions that must be made, in order to determine whether any implications can be drawn from the analysis, cannot be made by the reader.

A THUNDEROUSLY CASCADING SERIES OF TENUOUS ASSUMPTIONS

Particularly notable is how many awkward and questionable assumptions the paper makes on its way to performing analyses and reaching conclusions. Its opening set of assumptions continue to be incredible to me, as I have already written[3]. The most surprising thing is that

the assumptions contradict the views that Fama has stated.

Fama won the 2013 Nobel Prize in economics, and his early work on defining and arguing for efficient markets was seminal in producing the rich debates of market efficiency that have taken place since. Fama gave an interview with George Mason University economics professor Russ Roberts on Roberts's invaluable website econtalk.org[4] in which Fama reiterated firmly, "I believe in efficient markets."

However, the assumptions in Fama and French's paper make the error that investors who are unaware of – or do not believe – the efficient-market hypothesis make. These investors say something like, "This company will have very high profits, so it's a great investment!"

What those investors don't understand is what Fama, as much as anyone else, taught us. If a company is expected to have high profits in the future, then its price will already be high enough to cause its expected return to equilibrate with that of other companies with comparable risk.

The Fama-French article makes exactly the mistake that those naive investors make. It says that the higher a stock's future profitability, the higher its expected return. But of course, you can't fix a stock's market price while varying its future profitability. The more profitable it will be, the higher its price will be – and, according to efficient-market theory, its expected return will remain fixed.

This astonishingly flawed argument is used to motivate the search for an empirical relationship between profitability and return. Why is theoretical motivation needed? Because as Fama and French ought to know, if you don't mine financial data in search of something in particular, then you will mine it in an undisciplined way and eventually discover an "anomaly" that is nothing but a statistical accident, a product of noise or randomness.

But, even if we were to accept this argument as motivation – which we certainly shouldn't – the next step further strains intellectual acceptability. For the entire future sequence of profits of a company, Fama and French (as well as Robert Novy-Marx, a finance professor at the University of Rochester to whose paper[5] on profitability they refer) substitute as a "proxy" the company's current, one-year profitability. Since this does not, of course, take into account profit growth, it is a poor proxy. If this year's profitability were a proxy for future profitability, WhatsApp would not have sold for $16 billion!

So, the theoretical justification for the Fama-French investigation is bereft of meaning. That leaves the investigation as no more than what it actually claims to be – an "empirical asset pricing model". Compared to standard asset pricing models, which "work forward" say Fama and French, empirical asset pricing models "take as given the [historical] patterns in average returns, and propose models to capture them".

In other words, they mine the data to see what mathematical description comes closest to fitting them, without seeking to create a theoretical model – an asset pricing model that "works forward" – to explain them. That is another reason why it is so mysterious that Fama-

French invoke a theoretical justification to motivate their data study.

THE CRUCIAL DETAILS THEY DON'T EXPLAIN

I went to a talk the other day in which someone said his company had run "millions of regressions" to find out which variables predicted alpha. The company found lots of variables that predicted alphas. I noticed and remarked that what he probably meant was not that these variables predicted alphas, but that they were contemporaneous with alpha – a big difference. He said he would have to ask whoever ran the regressions and get back to me.

Timing matters. For example, it matters exactly at what point in time Fama and French measured company size, book-to-market ratio, profitability and investment (the latter two of which are supposed to be proxies for the entire future stream of profits and corporate investments), and over what time period they measured the return on investment with which they correlated these variables.

All they say in this paper is that the study covers July 1963 to December 2012. But we don't know when they measured their independent variables and over what time period they measured the rates of return that they attempt to relate to those independent variables. Did they measure corporate size, book-to-market, profitability and investment in 1963 and then run regressions of rates of return over the entire 1963 to 2012 period against them? Surely not, but they don't say what points in time, time intervals and sequences of events they are talking about. Without that information, the paper is incomplete.

Speaking of sequences of events that develop over time – a subject on which most of modern portfolio theory (MPT) seems to be almost mute – Fama and French also make no attempt to study at least two evolutionary-time processes about which their work invites study.

WHAT IS RISK?

Fama is not a believer in anomalies. In his Econtalk interview, he says:

> "There are two types of behavioral economists. There are [those] who are solidly based in psychology, reasoned economics. ... There are other finance people who are basically what I call anomaly chasers. What they are doing is scouring the data for things that look like market inefficiency, and they classify that as behavioral finance."

But what Fama and French are doing looks suspiciously like anomaly chasing. They're scouring the data for factors that produce higher returns.

That is why their work needs to be cleansed of implications of market inefficiencies and classified as the pursuit of risk factors. If the relationship of a stock to certain factors means

the stock has higher risk, then it would be understandable in terms of efficient markets that the stock would reap higher returns on average.

So why don't Fama and French pursue the obvious lines of investigation to see if those companies that correlate with the Fama-French "risk" factors – and thus have higher returns – actually do have higher risk? For example, why don't they check to see whether companies with higher profitability (or higher book-to-market ratios, etc) also go bankrupt more often? I can only speculate that the reason is because they think, as I have observed of most of the school of MPT, that they don't need to look at lifecycle events because everything of importance is captured in a small group of instantaneous (or time-unspecific) variables – expected return, variability, and correlation (none of which can actually be measured prospectively or even estimated accurately).

Along the same lines, wouldn't the fact – if it is a fact – that small stocks have higher expected returns prompt an investigator to try to relate this to the corporate lifecycle? Corporations do appear to have a life cycle[6], which has been written about in the literature of management science. Almost none of the thriving companies of the early-to-mid 20th century are still around or thriving now. Whatever happened to Sears Roebuck, Howard Johnson's and Kresge? Blackberry (i.e. Research In Motion) had a high expected return in its early and later life but then lost the monopoly advantage it had to newer entrants. Fama and French make reference to this life cycle possibility in their article by referring to a "term structure of expected returns" but they do not explore that concept.

PROCEDURAL PROBLEMS

Fama and French's paper, like nearly every other body of financial regression studies with which I am familiar – and even mathematically sophisticated textbooks like The Econometrics of Financial Markets[7] – do not give a passing nod to the assumption of mathematical regression theory that the underlying probability distribution of the dependent variable is at least symmetric, and for purposes of statistical analysis such as the use of t-tests, normal (that is, Gaussian). I learned in my freshman courses in college, and have taught in statistics courses myself, that when the probability distribution of a random variable used in a regression is closer to lognormal than normal, you should transform it by taking its logarithm before using it in the regression.

Since the log-return (the logarithm of one plus the ordinary rate of return on a security or a portfolio) is a perfectly fine measure of rate of return – in fact, it is the continuously compounded rate of return – you would think that analysts applying regression analysis would use that rate of return. It tends to have a symmetric and more-or-less close to normal probability distribution. The results can come out very substantially different when one uses continuously compounded returns. This stokes concern about the possible spuriousness of results.

If academic finance is as sophisticated as it is reputed to be, why doesn't it ever address this issue?

FAMA-FRENCH'S BOTTOM LINE

What Fama and French claim to do in their article is identify factors that span the historical rates of returns. That is, when one runs regressions of rates of return against these factors, one gets no alpha. The factors explain all the variation, so no additional factor, including skill, is needed. However, Fama-French also make the correct mathematical observation that these are not necessarily the only factors that explain performance.

These regressions are nothing more than attempts to find some set of – not necessarily unique – regression factors that span the historical data, as Fama-French themselves make clear by saying that these are "empirical asset pricing models" that "work backward". They make no claim that these factors will account for future performance, although they don't explicitly discourage that implication either, as perhaps they should.

This study does not, in my opinion, provide any useful information for practical application by investors or investment advisers. Maybe the study will motivate future research efforts that are free of the flaws that I find to exist in this Fama-French study.

ENDNOTES

1 & 2. Roberts also coauthored the lyrics of two must-see Keynes vs. Hayek rap debate videos.

3. Edessess, M., Why DFA's New Research is Flawed", *Advisor Perspectives*, 10 September 2013
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4. http://econtalk.org/
5. Novy-Marx, R., "The other side of value: The gross profitability premium", NBER Working Paper No. 15940. Issued in April 2010. http://www.nber.org/papers/w15940
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7. Campbell, J.Y., Lo, A.W., & MacKinlay, A.C., The Econometrics of Financial Markets, Princeton University Press, 1996.

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