What Fama and French's Latest Research Doesn't Tell Us

By Michael Edesess
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With the high name recognition and respect that the team of Eugene Fama and Kenneth French enjoys in the world of finance, anything they publish warrants attention. Their latest offering, Size, Value, and Momentum in International Stock Returns, offers some interesting data on global equity performance. But they fail to offer any insights that explain the reasons behind their findings.

The duo of Fama and French is most famous for their 1992 and 1993 papers documenting strong historical value and size effects. (Fama is also famous – or infamous, depending on your perspective – for his association with the efficient market hypothesis.) The core observation of Fama and French's seminal papers was that the returns on small-company and value stocks – those with high book-to-market value ratios – have historically outperformed the market to a greater degree than can be explained by the capital asset pricing model (CAPM).

I'll go into this and their new findings in more detail a little later. First, I'll offer a primer on their methodology. Then I'll describe how they constructed their study, and the results they obtained. Finally, I'll put on my curmudgeon hat and tell you what I think is wrong with the whole thing.

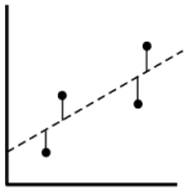
Regression

Fama and French's latest paper is an exercise in running regressions. Most readers of *Advisor Perspectives* will be familiar with the term regression and to a degree with the method, but it's worth a quick review to ensure that we all have the basic tools to assess their work.

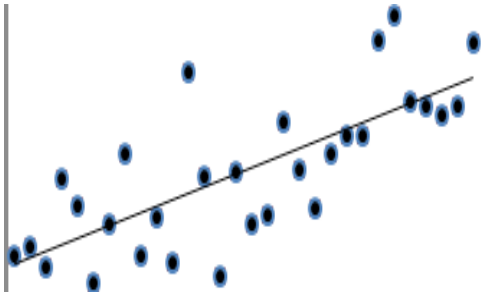
Regression analysis is the most used mathematical modeling technique in all of social science, finance included – and the most overused. Regression is usually explained to students in about the second year of high school – at least it was in my high school. There, it was called least-squares fit.

You have data points that graph something like this:

You try drawing a straight line through them and measure its distance from each point, like this:



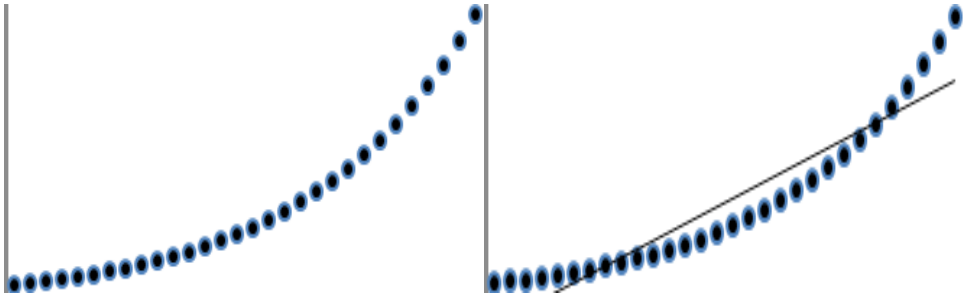
With a little math, you can find the line that minimizes the sum of the squares of those distances. This gives you the *least-squares fit*:



This exercise is also called “running a regression.” The line is called the regression line – it represents an inferred underlying linear dependency of the vertical-axis variable upon the horizontal-axis variable.

You could visualize these points in three dimensions instead, with a plane drawn through them using the same method. Then the plane would express a dependence of one of the variables on the other two. The same thing can be done in higher dimensions with more variables, but you can’t visualize it.

The method gets dicier if the underlying dependency isn't linear. For example a graph of the growth of an asset at compound interest over time looks like this:



Obviously, the line on the right isn't a good fit to the points on the left. Since this is well known, time-series regressions for asset growth generally modify the asset values by taking their logarithms. But if you get the underlying relationship wrong – if there even is one – you can get wacky results.

Fortunately, statistics derived from the sum of the squared distances tell you whether your fit is good or not. They do not, however, tell you why.

The 1992-1993 Fama-French studies

The CAPM says that a portion of a stock's return is a linear function of the return on the whole market – specifically, the stock's beta times the market return. Hence, part of the stock's variability is due to its correlation with the market. The "systematic" risk of this market-related portion is compensated with an expected return.

The remainder of the stock's return variability is "idiosyncratic." The risk of this residual non-market-correlated variability is not rewarded, because in theory it can be eliminated through diversification.

However, the classic Fama-French study found evidence of return variability in value stocks and small stocks that was not correlated with the market, but was nevertheless richly rewarded over a long period. They posited that stock returns should depend on these two new fundamental factors as well as beta.

When they added "smallness" and "value-ness" to "marketness" as stock characteristics, they got a better regression line fit. This introduced the idea that you should regress a portfolio's returns against three variables: the whole market, the small stock market portfolio, and the value stock portfolio, to see how much of its performance is "explained" by its statistical dependency on each of these factors.

It should be noted that Fama and French ended both papers with similar caveats, saying in the 1992 paper that their results "are not economically satisfying" and asking, "What is the economic explanation for the roles of size and book-to-market equity in average returns?"

The 2011 paper

Fama and French's 2011 paper, rather disappointingly, does not attempt to explain why the returns of any stock should depend on its degree of "smallness" and/or "value-ness." Instead, it is a continuation of the exercise of running data regressions.

Fama and French have access to plenty of data. They use monthly returns data for 23 countries from November 1990 to January 2011, grouping the country data into four major regions – North America, Japan, Asia Pacific, and Europe – and aggregating it to form a global region. They have company size and book-to-market value ratios for all the stocks in their database too, allowing them to divide the stocks for each region into size and value/growth categories.

Having divided the stocks into value and size categories for each region, they construct monthly returns series intended to represent the "size" and "value" components of regional returns. This construction involves a series of rather arbitrary operations. Like the regressions in their 1992 and 1993 papers, these are, say Fama and French, "examples of empirical asset-pricing models; that is, they try to capture the cross-section of expected returns without specifying the underlying economic model that governs asset pricing." In other words, it's an exercise in running regressions, not in explaining their results.

Size/value returns

One of the objectives of this paper is to see how size/value characteristics affect stock returns within each region, and another is to see how much each region's returns can be related to *global* size/value returns.

To explore this question, Fama and French constructed 25 stock portfolios in each region, and regressed them against the three factors: market returns, value stock returns, and small stock returns. If the resulting regression line passes through (or near) the origin of the graph, that means the three factors explain all of the returns, with no residual. In Fama and French's language, "If we find a set of explanatory portfolios that spans the MV [mean-variance] tangency portfolio, we capture the cross-section of expected returns, whatever the underlying model generating asset prices." Note the final clause.

The result was that the intercept got pretty close to zero for each region (though not at a high level of statistical significance), especially if you left out microcaps. Microcaps exhibited an exaggerated value effect – their returns increased substantially with value-ness even after the value factor and other factors common to all stocks had been removed. Fama and French did not find as good a fit when they regressed regional returns

against global market, size and value factors – a result they interpret as meaning that the regional markets are not globally integrated.

The momentum effect

Fama and French added to this paper a factor that does not appear in their earlier papers: the momentum factor. They made this addition because of the relatively recent observation that stocks have tended to perform better in the subsequent month or year if their previous year's return outperformed the market.

Fama and French evidently think their work on the momentum factor is important, because in the working paper version I downloaded they included a comment appealing to professional readers of the draft version: "***This is important. Is there a way to give it more emphasis?"

Fama and French's data exhibit the momentum effect, as have other studies, except in Japan. Adding momentum as a fourth factor creates a better fit to returns, in some regions.

The bottom line

The study's upshot, ultimately, is something of a dog's breakfast of conclusions and non-conclusions. I invite anyone with enough interest in this topic to have read this far to view the complete set in the Summary and Conclusions section on pages 21-23 of Fama and French's paper itself. Perhaps most notable – notwithstanding the extra-strength microcap value effect already mentioned – is a tendency they identify for both the value and momentum effects to exhibit themselves most strongly in smaller stocks, and least strongly in large stocks (with exceptions for Japan). This is perhaps not surprising, since larger stocks are subjected to more market scrutiny and analysis.

What's wrong with all of this?

Long-lived phenomena are often the by-product of a single accidental moment in history.

Social studies began in about 1957 to call themselves "sciences" and to assume an exaggerated posture of math-based objectivity in order to lump themselves with science. At the time, the Soviet Union's Sputnik launch had just siphoned all research funding to the physical sciences to enable the U.S. to catch up in space.

In Fama and French's paper, their posture of objectivity even extends to not trying to interpret or explain their results. One reason, of course, could be that they can't explain them. Another is that any explanation would be sheer speculation, and therefore doesn't belong in a scientific paper. That too, is another way of saying they don't have a good explanation.

There's nothing wrong with presenting results without explaining them, and leaving it to others to try to explain them. That's the idea of seminal research; if your findings provoke discussion and get others to interpret them, eventually leading to a new and improved theory, that's great.

Regression formulas alone, however – even if they fit the data well – are not a theory. They are merely here patterns perceived in data, barren of explanation – and possibly accidents of randomness. Unfortunately, the results of these regressions are being treated as if they were a theory in and of themselves. Without theory, regressions have no lasting or practical implications. Without an explanation of those results, there is no reason to apply results mined from historical data to predict the future, or even to evaluate historical performance that relies on making accurate predictions.

Some people, realizing that the historical outperformance of value stocks is not enough to sell the future performance of a value stock investment approach, have tried to concoct ersatz theories to explain it; hence, for example, the "fundamental indexing" approach to value investing.

So far, there are only two potentially viable explanations for the value effect. One is the behavioral finance explanation – the "anchoring" or the "availability" heuristic, for example. All you know is that it's a lousy stock because it's got a low price, so you predict it will be a lousy stock and don't buy it – keeping its price low even though its future rise will be as good as or better than that of a growth stock.

The other explanation is that small and value stocks carry risks that aren't captured by the usual risk models. But if that is the case, why apply band-aids to the models by adding small and value factors? How enlightening is that? If something is missing from our risk models – and there is – that won't fix it.

Exhibit A: Fama and French's handling of the momentum effect

In fact, I think this is an example of what David Colander et al. mean in their article in the book "What Caused the Financial Crisis" (reviewed in my *Advisor Perspectives* article of May 24), in which they speak of a "*systemic failure of the economics profession*" (emphasis in original). Colander et al. fault academic economic models because they "fail to account for the actual evolution of the real-world economy" and largely crowded out research on the inherent causes of financial crises.

Does this really apply to Fama and French's paper? Yes, I think it does. Let's take their handling, for example, of the so-called momentum effect.

The momentum effect has been documented over short time periods, but "reversion to the mean" – the opposite of the momentum effect – has been observed. In fact, it *must* be observed over longer periods if there is a momentum effect over short periods.

The momentum effect says that what goes up will keep going up. Reversion to the mean says what goes up will come down. That means that *over some short period* momentum will not continue but will reverse. Observation of financial crises suggests that momentum is gradual and long-lived, but reversal is sharp and sudden. You won't get good long-run performance by relying on momentum.

Where, you may wonder, do Fama and French suggest anything about the inevitable long-term failure of momentum in their paper? They merely woodenly note the momentum effect is discernable through the weak lens of linear regression – in fact a relatively unsophisticated methodology, in spite of all the babble about sophistication in the financial industry.

In my view, the Fama-French paper represents little more than a continuation of the economics profession's "systemic failure."

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