The following originally appeared in *Investment Gurus* by Peter J. Tanous, New York Institute of Finance, 1997. Reprinted with permission.

An Interview with Eugene Fama

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Investment Gurus
February 1997

Eugene Fama grew up in Boston, a third generation Italian-American. While an undergraduate at Tufts University, he excelled in athletics and majored in French—an inauspicious beginning for a future giant in the field of economics. But he also worked for a professor who was trying to develop "buy" and "sell" signals based on price momentum. Although the theories the professor devised worked well when applied to the past, they worked poorly when Fama tested them in real time. That puzzle, plus the skills that he acquired evaluating stock market data, drew Gene Fama to business school. After earning his doctorate at the University of Chicago, he joined the faculty there in 1963.

A simplified version of his dissertation, "Random Walks in Stock Market Prices," was published in *Institutional Investor* magazine, provoking a stir. It was Gene's article that introduced the still-controversial efficient market theory to the investment community. (There are many variations of the efficient market theory, but they all postulate that stock prices promptly and fully reflect all public information.) Very few academics specializing in investment research have any audience in the investment community, but that article made Gene Fama very well-known on Wall Street. But he is an academic and technical terms are used in this interview. We covered some of these terms earlier in the book, but here's a quick, but non-scientific, refresher course on some of the lingo:

Efficient market theory: The theory that holds that stocks are always correctly priced since everything that is publicly known about the stock is reflected in its market price.

Random walk theory: One element of the efficient market theory. The thesis that stock price variations are not predictable.

Active management: The practice of picking individual stocks based on fundamental research and analysis in the expectation that a portfolio of selected stocks can consistently outperform market averages.

Passive management: The practice of buying a portfolio that is a proxy for the market as a whole on the theory that it is so difficult to outperform the market that it is cheaper and less risky to just buy the market.

Outliers and fat tails: In a normal, bell-shaped distribution of returns on investment portfolios, the majority of the returns, or data, can be found in the "bell," or bulge, which centers around the weighted average return for the entire market. At the ends, both right and left, we find what are known at "outliers," those returns which are either very bad (left side) or very good (right side). Of course, few managers are either very good or very bad. Those returns on the right and left tails are known as outliers since they live on the outlying fringes of the curve. Similarly, "fat tails" refers to larger than normal tails of the curve, meaning that there are more data on the extremes than you might expect.

Peter J. Tanous: How did you first get interested in stocks?

Fama: As an undergraduate, I worked for a professor at Tufts University. He had a "Beat the Market" service. He figured out trading rules to beat the market, and they always did!

I beg your pardon?

They always did, in the old data. They never did in the new data [laughter].

I see. Are you saying that when you back-tested the trading rules on the historic data, the rules always worked, but once you applied them to a real trading program, they stopped working?

Right. That's when I became an efficient markets person.

Okay. Let's get into it. You're known for your work on efficient capital markets. In fact, on Wall Street, the phrase "efficient market" is often attributed to you. I believe you and Ken French made the point that stock market returns are, in fact, predictable over time. How does that jibe with the random walk theory?

The efficient market theory and the random walk theory aren't the same thing. The efficient market theory is much more powerful than the random walk theory, which merely postulates that the future price movements can't be predicted from past price movements alone. One extreme version of the efficient market theory says, not only is the market continually adjusting all prices to reflect new information but, for whatever reason, the expected returns—the returns investors require to hold stocks—are constant through time. [For example, we know that, since the '20s, returns on the New York Stock Exchange common stocks have averaged a little over 10% per year.] I don't believe that. Economically, there is no reason why the expected return on the stock market has to be the same through time. It could be higher in bad times if people become more risk-averse; it could be lower in good times when people become less risk-averse.

So risk is the component that leads to how much you get paid?

It could be just taste, too, you know. People's taste for holding stocks can change with time. None of that is inconsistent with market efficiency and it can give rise to some predictability in returns. The predictability is simply based on the returns people require to hold securities.

But, in one of your papers, you did refer to the predictability of returns over time. Is that just the investor getting paid for the risk he was willing to take? Is that the point?

It could be that or it could be that people are simply more risk-averse in bad times.

On a related subject, I think you also said that fundamental analysis is of value only when the analyst has new information, which was not fully considered in forming current market prices. When I hear that I say: Hey Gene, that's the point! The analyst believes he knows something, or infers something, that other analysts don't see. He sees an evolution taking place or he believes this company is doing better than people think, and that's why he gets paid millions of dollars on Wall Street to pick stocks. What's wrong with this thesis?

Well, not everybody can have that talent. In fact, as far as I can tell, not many do. The system is designed to make that very difficult. By that, I mean that under US accounting [and regulatory] systems, if you reveal anything, you have to reveal it to everybody.

Fair enough, but what if the analyst is making a judgment on the future prospects of the company. For example, the analyst might say, "The Street says this company is going to earn $0.82 per share and I say it's going to earn $1.10 because I'm seeing order flow, consumer demand, customers' tastes for the product and what have you." Now, if the analyst is right, he's worth the millions he gets paid. My question is: in your thesis, if he's right, is he right because he's so smart or just

because he's lucky?

For the most part, I think it is luck. The evidence is pretty strong that active management doesn't really do better than passive management.

Except, of course, when we start talking about the so-called outliers, those managers, like the Gurus in this book, who have persistently outperformed the market. That, in turn, leads to the other great exercise in our business, particularly with mutual funds, which is the predictability of future investment success based on past success. I know you've done some work on that, too.

One of my students just finished his thesis on that subject, actually. What he found was that performance does repeat when it's on the negative end! In other works, funds that do poorly, tend to do poorly persistently.

Why couldn't one postulate that the same would be true at the other end of the spectrum?

One could postulate it, but it doesn't seem to be true. On the negative end of the spectrum, you have things like turnover and fees and all that kind of stuff, which can explain why you have negative persistence in poor returns.

Yes, but good managers trade and charge fees, too. They might even deserve them more!

Poorly performing funds tend to be higher fee and higher expense funds. In fact, when my student adjusted for fees and expenses he could explain most of the persistent under-performance.

One thing I did a couple of years back was take all the funds that survived from the beginning of the Morningstar tapes, which is 1976. Now, funds that survive that long will have survivor bias built into the test, because only the successful funds survive. So I split the sample period in half and took the 20 biggest winners of the first 10 years, or the first half of the period, and I asked how did they do in the second half of the period. Well, in the second half of the period, half of them were up and half of them were down.

Wow. Half were up and half were down? [That indicates that there was no predictive value in the fact that these managers all finished in the top half in the first ten year period.]

Exactly half, relative to a risk-corrected model.

How did you adjust for risk?

I used the three-factor model.

The three-factor model takes into account market risk; value versus growth styles; and also size, which is the large-cap stocks versus small-cap distinction, right?

Yes. But since most retail funds have a bias toward growth stocks, the adjustment helped them.

So even risk tested, the data came out 50/50, which means that the mutual funds that did the best for ten years only had a 50/50 chance of repeating their success. I'm curious to know who the biggest winner was in both periods?

Fidelity Magellan.

What's the reason for that?

Obviously, the performance of that fund has been really good.

It has, to Peter Lynch's credit. Another issue you have addressed: that old subject, value stocks versus growth stocks. Are stocks of good companies good stocks to invest in?

They're good stocks, they just don't have high expected returns.

Then growth stocks are stocks of good companies, not good stocks, right?

To me stock prices are just the prices that produce the expected returns that people require to hold them. If they are growth companies, people are willing to hold them at a lower expected return.

As we get into this, I think our readers are going to be surprised to read that value stocks are riskier than growth stocks. That is counterintuitive.

I don't know why it's counterintuitive.

Well, we used to think of value stocks as stocks that may have already had a decline, that are languishing. We believe we're buying value stocks at the bottom and waiting for them to go back up again.

Value stocks may continue to take their knocks. Their prices reflect the fact that they are in poor times. As a result, because people don't want to hold them—in our view because they are riskier—they have higher expected returns. The way we define risk, it has to be associated with something that can't be diversified away. Everybody relates to a market risk. If you hold stocks, you bear stock market risk. But the stock market is more complicated than that. There are multiple sources of risk.

In our business, we usually associate growth stocks with high earnings multiples, and value stocks with low earnings multiples. Multiples are themselves usually an element of risk. So, if a growth stock falters on its anticipated growth path, it declines precipitously because it no longer deserves the multiple that had previously been awarded to it when its prospects were better. Therefore, a lot of people think that growth stocks, in fact, are riskier. What's wrong with that thesis?

Just look at the data. It's true that growth stocks vary together, and it's true that value stocks vary together. In other words, their returns tend to vary together, which means that there is a common element of risk there. Now, for growth stocks that seems to be a risk that people are willing to bear at a lesser return than the return they require for the market as a whole. Whereas, if I look at the value stocks, which we also call distressed stocks, their returns vary together, but people aren't willing to hold those except at a premium to market returns.

So you're saying that I expect to make more money when I buy value stocks than I do when I buy growth stocks.

Right. On average. Of course, sometimes you get clobbered.

We've always associated the risk of getting clobbered more with growth stocks than with value stocks that have already taken their lumps.

The data don't support that.

The other dimension, of course, is size. Now the size effect is very easy for those of us in the investment community to accept. The notion that small companies are riskier than large companies seems obvious.

That's not the reason the community accepts it. What they think is that small companies pay higher returns because they're unknown, or something like that. It's not because they're more risky. The risk, in my terms,

can't be explained by the market. It means that, because they move together, there is something about these small stocks that creates an undiversifiable risk. That undiversifiable risk is why you get paid for holding them.

What causes that risk?

You know, that's an embarrassing question because I don't know.

Fascinating. I would assume that the risk is that small companies have a lower survival rate than large companies.

No. That's not it at all. The good news and the bad news about that is that the reason small companies don't survive is because some of them fail, others get merged; that's bad news and good news. Here's a fact I always use. First I say I don't know, but then I say it's fair. Here's my example. The 1980s were, supposedly, the longest period of continuous growth the country's seen since the second world war. Yet, in that decade, small stocks were in a depression. Small stock earnings never recovered from the '80-'81 recession. They were low the whole decade. The market was fooled every year by that, because in every previous recession, the small companies came back. Why did that happen in the '80s? I don't know. But it happened. And it tells you there is something about small stocks that makes them more risky.

Another question that comes up frequently is if markets are correctly priced, how do you explain crashes when they go down twenty percent in one day?

Take your example of growth stocks. If their prospects don't go as well as expected, then there will be a big decline. The same thing can happen for the market as a whole. It can also be a mistake. I think the crash in '87 was a mistake.

But if '87 was a mistake, doesn't that suggest that there are moments in time when markets are not efficiently priced?

Well, no. Take the previous crash in 1929. That one wasn't big enough. So you have two crashes. One was too big [1987] and one was too small [1929]!

But in an efficient market context, how are these crashes accounted for in terms of "correct pricing"? I mean, if the market was correctly priced on Friday, why did we need a crash on Monday?

That's why I gave the example of two crashes. Half the time, the crashes should be too little, and half the time they should be too big.

That's not doing it for me. What am I missing?

Think of a distribution of errors. Unpredictable economic outcomes generate price changes. The distribution is around a mean—the expected return that people require to hold stocks. Now that distribution, in fact, has fat tails. That means that big pluses and big minuses are much more frequent than they are under a normal distribution. So we observe crashes way too frequently, but as long as they are half the time under-reactions and half the time over-reactions, there is nothing inefficient about it.

Let's go back to value stocks versus growth, and large versus small stocks. Tell us why the three-factor model contributes to our knowledge of risk in investments.

The three factors are the market factor, the size factor, and the distress [value] factor. We distinguish between distress and growth. What we find is that, in addition to the market factor in returns, in other words the fact that stocks move together, it's also true that small stocks move together, and big stocks move together, but not in the same way. The value stocks move together and the growth stocks move together but the two

groups are different from each other. There are at least three dimensions of risk: market risk, small stock versus big stock risk, and distress stock versus growth stock risk. When I say risk, I mean that these groups move together. We could have found that they didn't move together, and then it would have been market inefficiency.

What would that have told us?

It would have told you that you could get a diversified portfolio of small stocks, and a diversified portfolio of big stocks, short the big stocks and buy the small stocks, and get a positive return with no risk.

Why would that be true?

It would only be true if there weren't a common factor in the return on small stocks that caused them to have randomness that wasn't shared with big stocks.

I'm not sure I follow.

If there's no small stock risk, and I take a diversified small stock portfolio, I would be able to explain its return entirely in terms of the market risk. So there's nothing left over.

I see. We're comparing small stock returns to the market as a whole. What you're saying is that small stock returns have risk that's not explained by the market. And this higher risk is the size risk you talk about in the three-factor model? Is that correct?

Right. Take a diversified portfolio of value stocks. Those stocks will move together. That portfolio's return will not be perfectly explained by the market even if it has a few thousand stocks in it.

If that's the case, wouldn't growth stocks mirror the market as a whole?

Growth stocks do come closer to mirroring the market as a whole.

So once you've decided to take the market risk, creating your portfolio seems to come down to deciding what your overall risk level is, and then you allocate by size, and between growth and value, to achieve your risk/reward goals.

Have there been any studies that have ever impressed you about active management in any capacity? I mean, has there been any evidence that would suggest to you that all of the Wall Street analysts, gurus, salesmen, and research departments are anything but a complete waste of time?

You used the key word: salesmen. I might be willing to say that the people who get pointed at consistently, who have shown consistent performance even after they have been pointed at, really do have something. These are always the same people, Warren Buffet, Peter Lynch, and then who?

Okay. You talk to Rex Sinquefield, and he'll tell you that in any normal distribution you're going to get those outlying orangutans.

I put it carefully. I said if you identify them, and in the future they continue to do well, then I'm starting to believe it. This sounds like the frustrations of my college days when I found that the system that worked on the old data didn't work with the new data!

So, in fact, there may be a Lynch and a Buffett effect out there somewhere?

There may be, but the non sequiturs that people jump to after that is to say, Aha! Active management pays!

No, it means that Peter Lynch and Warren Buffett pay! And what is it about them that we can clone? Where's the next one?

Yeah. I don't think that's something you can teach anybody or anything like that. The Magellan Fund [once managed by Peter Lynch] by any risk-adjusted model, is off the map. But there are only one or two like that.

Isn't it interesting that the last three years' performance at Magellan Fund isn't Peter Lynch's? Jeff Vinik's performance was also good. I presume because he made a big bet on technology stocks and won.

Another thing I found when I looked at Magellan was that it had a greater small stock bias when it was a smaller fund.

Are you working on anything now that you could share with us?

We're trying to extend the three-factor model internationally. The scientific approach is always to say: does it work out of sample? In other works, does it work on new data, in this case, foreign stocks? So, what we are doing is trying to use international data to see if we can come up with a global view of risk and return.

How does it look so far?

The problem is that the international data stink. You can't get the kind of data we can get here in the US going back to 1926. We also have good accounting data going back to the early '60s. Internationally, you don't really have returns before 1988. And you only have a sub-sample of stocks.

How much data do you need to get a valid sample?

You never know until you do it, because it's a function of how variable the returns are. The problem with stock returns is the variability is so high. It takes long samples to really document anything. But, so far, the new data turn up the same kind of risk factors.

I guess we still haven't found a way to predict the future.

That kind of reasoning will get you closer to my way of thinking!

The trouble with you academic guys is that you all approach this with such religious zeal that I feel like a heretic if I disagree with any of you. Like I'm going to be excommunicated any second.

No. We'll just throw you out of the scientific community. You get to stay in the active management community.

Gene, you're very well known in our business for your work on returns. Do you do much work in the private sector?

Not a lot. I'm a little lazy! Most of the outside work I do is in a forum framework. I mean how am I going to manage to do all that if I go windsurfing every afternoon?

How's your windsurfing coming along?

I'm probably the best in the world over age fifty!

Who knows, Gene, maybe you're the millionth orangutan on the surfboard, the fifty-year-old outlier who wins the world championship.

A couple of things struck me about this discussion. You might or might not agree, but I thought I sensed a much

more open attitude from Gene about market efficiency, the concept he developed. I felt that his was not the extreme version of the efficient market theory that some others adopt, but rather an open-minded attitude which says that, yes, market efficiency is there and chances are you will never do better that the markets, and as a rule, active management just doesn't pay.

On the other hand, the door seemed open a crack to the reality that there are the occasional Peter Lynches and others who achieve truly great performance records over extended periods of time. The term the academics like to use for this is "persistence." Yes, these guys exist, but there aren't that many. Still, the sobering example Fama used that throws cold water on the performance expectations is the study he did on mutual fund performance over a ten year period since 1976. He then took the top performing funds in the group and analyzed them for the following ten years. The result: the top performing group only had a 50/50 chance of staying in the top half in the second ten year period. What are you going to do? I think it's time we talked to another active manager.
