

Campbell Harvey

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Finance has surely existed in one form or another since the earliest days of civilization. But finance as we know it, as a mathematical subfield of economics, is relatively young; many date its genesis to 1952, when economist Harry Markowitz published an article on the use of modern statistical methods to analyze investment portfolios. Today, the discipline seems as ubiquitous as the financial services sector itself, which grew from 2.8 percent of U.S. GDP in 1950 to a pre-recession peak of 8.3 percent of GDP in 2006.

Among the leading thinkers in academic finance is Duke University's Campbell Harvey. As a doctoral student at the University of Chicago in the 1980s, he turned out to be at the right place at the right time: Half of his dissertation committee was made up of future Nobel laureates — Eugene Fama, Lars Hansen, and Merton Miller. In the years since, his research interests have spanned such topics as the modeling of risk, the yield curve as a source of information about expectations of economic growth, equity and bond returns in emerging economies, and changes in the risk premium in financial markets. He has long been interested in bitcoin, a type of digital currency; this interest led him to offer a class at Duke this spring on creating new ventures based on bitcoin technology.

In addition to his appointment at Duke's Fuqua School of Business, Harvey is a research associate of the National Bureau of Economic Research. He has been a visiting professor at the Stockholm School of Economics and the Helsinki School of Economics and a visiting scholar at the Fed's Board of Governors. He is president-elect of the American Finance Association and a former editor of the *Journal of Finance*. He is also the investment strategy advisor to Man Group, one of the world's largest hedge funds. David A. Price interviewed Harvey at his office at Duke in April 2015.

EF: How did you become interested in economics in general and finance in particular?

Harvey: In a summer internship during business school, I was working on a fascinating problem for a company that, at the time, was the largest copper mining company in the world. The price of copper correlates closely with the economy. They wanted me to figure out if there was a better way to forecast what was going to happen in the economy than



what was commercially available. I had an idea that turned out to be, I guess, a pretty good idea; I was using information in financial instruments — in particular, the term structure of interest rates — to extract information about what people in the market at least think is going to happen in the economy. That really got me interested. After that point, I had the research bug, so to speak, and I never looked back.

EF: Financial economists, of course, have a large practitioner community in addition to those in academia. How do the incentives of finance economists in the private sector differ from those of academics, and how does this affect how they approach their work?

Harvey: There's a lot of similarity in that you are interested in discovering something. To be published in academic finance or economics, the idea must be unique; it's the same in the practice of finance — you're looking to do something that your competitors haven't thought of.

There are differences, though. The actual problems that are worked on by practitioners are more applied than the general problems we work on in financial economics.

The second difference is that in academic financial economics, you have the luxury of presenting your paper to colleagues from all over the world. You get feedback, which is really useful. And then you send it in for review and you get even more feedback. In business, it's different; you cannot share trade secrets. You really have to lean on your company colleagues for feedback.

The third thing that's different is access to data for empirical finance. When I was a doctoral student, academia had the best data. For years after that, the pioneering academic research in empirical finance relied on having

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this leading-edge data. That is no longer the case. The best data available today is unaffordable for any academic institution. It is incredibly expensive and that's a serious limitation in terms of what we can do in our research.

Sometimes you see collaborations with companies that allow the academic researchers to access to data that they can't afford to buy. Of course, this induces other issues such as conflicts of interest.

The fourth difference is the assistance that's available. Somebody in academia might work on a paper for months with a research assistant who might be able to offer five to 10 hours per week. In the practice of management, you give the task to a junior researcher and he or she will work around the clock until the task is completed. What takes months in academic research could be just a few days.

The fifth difference is computing power. Academics once had the best computing power. We have access to supercomputing arrays, but those resources are difficult to access. In the practice of management, companies have massive computer power available at their fingertips. For certain types of studies, those using higher frequency data, companies have a considerable advantage.

EF: You've argued that more than half of the papers published in empirical finance are probably false because they have a mistake in common. Can you explain what that mistake is?

Harvey: It's a mistake that is made in the application of statistics. Think of testing for an effect. You try to see if there is a significant correlation between what you're trying to explain, let's call it Y, and the candidate variable, X. If you do that, we have well-established procedures and statistics; we look for a correlation that is a couple of standard deviations from zero — the so-called two-sigma rule, or 95 percent confidence level.

But suppose we tried 20 different versions of X, 20 different things, to try to explain Y. Then suppose that one of them, just one, satisfied this rule where the correlation is two standard deviations from zero. It's possible that this one "worked" purely by chance. That two-sigma rule is valid only for a single test, meaning there is one Y and one X. As soon as you go to one Y and 20 X's, then you need to change the rules because something is going to appear significant by luck. And when you go to even more than 20, there is almost a 100 percent probability something is going to show up by a fluke.

It turns out — and this is not just in empirical finance, it's also in economics more generally — that if you just open almost any scientific journal, and if there is an empirical paper, you will see a table with different variables tried. That is what we call multiple testing. With multiple testing, the standard sorts of cutoffs are not appropriate. So this has a wide ranging application in terms of how we do research in economics.

In medicine, there is a famous study that concluded that over half of the medical research that was published was likely false. The conclusion in economics is no different than in medicine; it's the same idea, that people do

not properly account for multiple testing.

My paper tries to go beyond previous findings in other fields like medicine. I develop a framework to check for tests that we do not observe. A person says that X explains Y and is significant — well, what happens if that person tried 20 things but just didn't report it? My research also incorporates something that is important in finance — the tests can be correlated. In my example, with the 20 different X variables, it makes a big difference if the Xs are correlated or uncorrelated.

You might think that people might be upset at me for doing something like this, but that is not the reaction I have experienced. I think it helps that I made the mistake, too. I'm on the list of people who failed to properly account for multiple tests, so some of the things that I thought I had discovered in the past are below the bar. I'm pointing a finger at myself, also.

EF: You've written extensively about bitcoin and other so-called crypto currencies. How do you think their role will evolve and does the rest of the payments industry have anything to be worried about?

Harvey: This is a significant innovation that is poorly understood by the general public and poorly understood by the companies that are about to be disrupted. It is a method that makes transactions more efficient. When you swipe your credit card at the gas pump, most people don't realize that the credit card fee is 7 percent. It's very inefficient when you are faced with transaction fees like that. The lowest-hanging fruit that is going to be disrupted is money transfer done by companies like Western Union, where it's routine to charge 10 percent or more on a transfer. A similar transfer fee in bitcoin is about 0.05 percent. The worldwide money transfer market is \$500 billion per year and potentially \$50 billion a year can be saved. In a broader sense, bitcoin is not just about money transfers — it establishes a new way to exchange property.

The foundation of bitcoin the currency is the technology behind it, called the block chain. This is a ledger containing the transaction record of every bitcoin over the history of bitcoin. It is a ledger that is available to anybody who is on the network. It is fully transparent. The advantage is that if I go pay for something with a bitcoin, then the vendor checks this historical transaction record to see if I actually have the coin to pay. So there is no counterfeiting, there is no double-spending, there is no bouncing of a check. On top of that, this ledger is protected by cryptographic barriers generated by historically unprecedented computing power.

Campbell Harvey

► Present Positions

J. Paul Sticht Professor of International Business, Fuqua School of Business, Duke University
Research Associate, National Bureau of Economic Research

► Education

Ph.D. (1986), University of Chicago
M.B.A. (1983), York University
B.A. (1981), Trinity College, University of Toronto

► Selected Publications

“... and the Cross-Section of Expected Returns,” NBER Working Paper, October 2014 (with Yan Liu and Heqing Zhu); “Managerial Attitudes and Corporate Actions,” *Journal of Financial Economics*, 2013 (with John Graham and Manju Puri); “The Theory and Practice of Corporate Finance: Evidence From the Field,” *Journal of Financial Economics*, 2001 (with John Graham); numerous other articles in such journals as the *Journal of Finance*, *Journal of Political Economy*, and *Quarterly Journal of Economics*.

This technology is offside to the run-of-the-mill hacker because the entrance fee is about \$500 million of hardware — and even with that, you cannot change the historical transaction records. So it is not going to be hacked. It’s secure. There’s plenty of talk about bitcoin being stolen and things like that, but that is all the result of incompetent third parties. It has nothing to do with the actual technology behind bitcoin.

With bitcoin, you also don’t need to worry about your private information being hacked. In usual transactions, we routinely give up private information such as bank account numbers, debit cards, or even Social Security numbers. Of course, vendors accepting bitcoin might actually require some private information to verify your identity, which is fine. But bitcoin’s much more secure.

And that’s just the tip of the iceberg. What’s really interesting is that there is other stuff that can be done with this technology that is almost completely under the radar screen.

For example, in this ledger, you could also put what we call conditional contracts: very simple contracts like stocks, bonds, options, forwards, futures, or swaps. So it provides a different way to exchange at very low transaction fees and it is very fast.

EF: Apart from the potential just for disruption, these currencies seem to generate strong reactions, pro and con. Why is that?

Harvey: It’s hard to think about the value of bitcoin because it isn’t backed by anything, so it is only valuable if people believe it’s valuable. Now that is also true with fiat currency. But the U.S. dollar, a fiat currency, is legal tender in the United States, which means you are obligated by law to accept dollars for payment. The government enforces taxation and can incarcerate you if you fail to pay your taxes. So there is more to the dollar than “it has value because people believe it has value.”

On top of that, if you look at the currency of the United States and another country, the so-called foreign exchange rate, people are generally comfortable thinking about movements in those currencies in terms of the monetary policies and economic growth of the two countries. You put those together and you get an idea of what is driving variation in the exchange rate. With bitcoin, it’s not so simple because there are no economic fundamentals. It is a world currency, so it is not tied to any particular country. The only monetary

policy is an algorithm that says that bitcoins will be created at a decreasing rate and cap out at 21 million in about 2140. So it is much more difficult to think of the fundamentals behind bitcoin.

I think all this leads to considerable volatility in terms of the value of bitcoin. But there is a way to do transactions that bypasses the volatility issue. Given that there is a fully regulated exchange in the United States for bitcoin, I can have a wallet with U.S. dollars in it, and when I need to transact in bitcoin, I can move some dollars into bitcoin, I buy what I need to buy, the vendor accepts the bitcoin and immediately translates it back into U.S. dollars. All you see is a U.S. dollar price on both sides. Indeed, in the software, you see the pricing in U.S. dollars, you hit send, and the vendor gets what was promised in U.S. dollars.

So the volatility doesn’t bite you for transacting, but it does bite you in terms of the store of value. Right now, bitcoin is not a reliable store of

value because it is too volatile. Volatility will likely decrease when the market is more liquid and when bitcoin is better understood by the general public, but right now the people who hold bitcoin are mostly speculators. Given that it is eight times more volatile than the S&P 500, it is hard to recommend as a store of value at this point. Nevertheless, I believe this technology has considerable promise.

What will happen in the future will be something digital. Whether it’s the bitcoin model, I’m not sure, but it will be something *like* bitcoin. And indeed, I have lot of confidence that the block chain technology is definitely here to stay.

EF: Speaking of the S&P 500 volatility, you showed in the late 1980s that the risk premium in the United States is countercyclical, but there isn’t a consensus on why that is the case, is there? What do you think is the best explanation?

Harvey: What we’re talking about when we’re talking about the risk premium is that when you invest in the stock market, you expect to get a higher return on average than if you invest in Treasury bills. It is the same for investing in a corporate bond: You expect to get the higher rate of return for a risky corporate bond than for the equivalent maturity of a U.S. Treasury bond.

The risk premium changes over time. There are many different explanations as to why it would change, but the most intuitive one for me is that when you go into a recession,

there is much more uncertainty than when you're not in one. People are worried about what is going to happen in terms of their job stability or even their bonuses. It makes it less likely that you are going to take a chunk of money and invest it in the stock market. During these periods, stock prices fall. So think of the risk premium as the extra expected return you need to offer to get somebody into the stock market. It will be quite high in the depths of a recession.

On the other hand, when we are in good economic times, people are very calm, they do not want to miss out on financial opportunities, and the stock prices are driven up. When the stock price is high, almost by construction, the expected returns are lower. So you get a countercyclical pattern in risk premium. This pattern is found in many different types of markets.

EF: You co-founded the Duke University/CFO Magazine Global Business Outlook Survey, the poll of chief financial officers. Analyzing your survey results, you found, among other things, that CEOs and CFOs were overconfident and this affected their businesses.

Harvey: One of the questions we have been running for almost the entire survey, almost two decades, is that we ask the CEOs and CFOs to forecast the stock market return — the S&P 500 over the next year and the next 10 years. Why do we do that? We want them to provide a forecast of something that is common. We ask them about their firm also, but we are interested in the market as a whole. And on top of that, they are very knowledgeable of the S&P in general because they are often asked to explain why their company's stock price has changed — and you need to understand what is happening in the overall stock market to answer that question.

The unique thing in our question is we also ask for a confidence interval. We ask them for their assessment of a 1 in 10 chance the S&P 500 will be above X and a 1 in 10 chance it will fall below Y. So what we get is an 80 percent confidence interval for the forecast. We do not care that much about the accuracy of their forecast because it is very hard to forecast the S&P 500. We are more interested in the strength of their confidence in their forecast, and it turns out that the confidence bounds that they provide us are unreasonable by almost any metric. They are far too narrow. That was a surprising result.

We found that there was a correlation between this overconfidence and some of the investment policies within the individual firms. You see that this overconfidence affects the way that they choose investment projects and organize their capital structure.

EF: Financial economists have been saying for some time that index funds consistently outperform active management over time when fees are taken into account. Do you agree that this comports with what we see in the world, and if so, how do we account for the persistence of active management?

Harvey: I have thought a lot about this. The research on mutual funds basically concludes that the performance, once you have benchmarked to a passive investment, is negative. I have a new paper that uses the multiple testing techniques we talked about earlier and applies them to fund evaluation. Of course, if you have 7,000 mutual funds, some of them are going to look very good year after year after year, just by chance. In my research, we looked at mutual funds from 1976 onward and could not find one, not one, that significantly outperformed a passive benchmark.

For hedge funds, my colleague David Hsieh has concluded that the outperformance on average for hedge funds is essentially zero. This is better than mutual funds, but on average, it's zero.

So then the question is, well, why are there mutual funds? And why are there hedge funds? The key for hedge funds is that the excess performance is *zero on average* — that is different than every single hedge fund having a zero excess return. It means that there are many hedge funds out there that significantly underperform and many hedge funds that outperform. If you have a scientific process to try to separate the skillful from the lucky, then hedge funds become more attractive. So I believe that active management is something that, if it is done properly, can lead to positive excess returns.

The next category is maybe the most complex. There might be, let's say, a hedge fund that I know has zero excess return to benchmark, but I still might want to invest in it. How does that make sense? The key is that when we talk about the excess return, we think about adjusting for risk. If we take all of the risks that the hedge fund takes into account, and strip out the expected returns that are due to that risk, then whatever is left over is the so-called outperformance. Even with zero outperformance, I still might invest in that hedge fund because, as an investor, I do not have access to the types of risks that the hedge fund is actually taking. It is not as simple as buying an S&P 500 index fund. There are many other types of risks out there, and some of them are exotic. Maybe I would like to take some of those risks, and it is hard for me to actually do that on my own. For example, I might not be able to easily invest in an emerging market currency carry trade, where I buy the currencies of countries with high interest rates and sell the currencies of countries with low interest rates. I am just not equipped to do that, but this hedge fund is an expert at it.

The other thing to consider is behavioral biases on the side of the people selecting the investment managers. Even though the average return might be zero, people believe they are better than the average at selecting a mutual fund or a hedge fund. It is a classic behavioral bias: 85 percent of the people believe they are better than average.

EF: Researchers looking at the data on cash reserves of U.S. corporations have found that those reserves have been increasing, with 50 firms holding over \$1 trillion in total. Why have corporations been holding so much cash?

Harvey: There are two main reasons and perhaps a third. The first one that people talk about is that a lot of this cash is offshore and it is not repatriated because of the punitive tax rates in the United States. The second has to do with thinking of this cash holding as a so-called real option. And what I mean by that is that you always want to be able to move quickly if you see a really good opportunity. You cannot wait around four months to get bank financing or float a secondary equity offering or something like that. You need to move with speed, and cash gives you the flexibility to move with speed. It might be that a firm is available for sale and you can use that cash to do the deal instantly.

EF: Do you think the importance of this has increased over time?

Harvey: We saw this flexibility in action during the financial crisis. Think of people like Warren Buffett, who had a lot of cash, just cleaning up, buying incredibly high-quality assets like Goldman Sachs at rock-bottom prices. So you could deploy that cash at a time when the expected returns were the highest. That is part of the flexibility. It is not just, “This firm is for sale, but we have to close it within a week or we’re not going to get it”; it is also that through time, you can be strategic and pick and choose when you do the investment. During the financial crisis, it was not easy to borrow for an investment.

The third aspect is also related to time. This is just my opinion — I don’t have any research paper on this — but I believe that with the exponential growth in technology, the rate of disruption has increased through time. It used to be product life was much longer than it is today. I think that some firms are thinking of the cash as insurance regarding this disruption. If a new technology arises, it gives them a cushion with which to try at least to attempt a counter-attack. To adapt to the situation, to maybe disrupt the disruptor. I think if you put those three things together, it pretty well explains why cash holdings have increased. There are other technical reasons that are less important.

EF: You were editor of the *Journal of Finance* for six years, from 2006 to 2012. What are the main lessons for authors that you took away from that experience?

Harvey: For authors, the advice is probably no different for the *Journal of Finance* than for any top economics journal, namely, that the editors are looking for disruption. Indeed, it is not much different than the world of business; there is a status quo and we are looking for somebody to challenge that and to come up with a fresh approach. We are not as interested in ideas that tweak the status quo.

The ideal is that when people look at the abstract, they’ll say, “Well, that can’t be right.” And then they read the paper and they are convinced. Within finance, it is also useful that your idea not only changes the way academics think, but it

also changes the practice of management. Just like if you’re doing macroeconomics research, you hope not just to publish in, say, the *Journal of Political Economy*, but you hope that the policymakers will read it and that it changes the way they think about policy.

EF: What do you think are the most important open questions in finance?

Harvey: One is how you measure the cost of capital. We had the capital asset pricing model in 1964, but the research showed very weak support for it. We have many new models, but we are still not sure. That’s on the investment side. On the corporate finance side, it would certainly be nice to know what the optimal leverage for a firm should be. We still do not know that. In banking, is it appropriate that banks have vastly more leverage than regular corporations? Again, we need a model for that. Hopefully these research advances are forthcoming. Some people have made progress, but we just don’t know.

EF: Who were your main influences in your development as an economist?

Harvey: I was very lucky to be at the University of Chicago in the early to mid-1980s because there were all of these people who we knew were going to win Nobels. As students, we talked about it all the time. I remember seeing Gary Becker out jogging — he was always exercising — and we joked that he was jogging so that he’d be in good shape to stay alive to win the Nobel Prize. You were sitting in his class, in Robert Lucas’ class, in Lars Hansen’s class, and you knew they were going to win. And in the business school you had Merton Miller and Eugene Fama, an incredible environment for thinking and research.

The seminars were electric. Unlike the experience that often you see today where somebody goes through some PowerPoint slides, it was totally different. The audience members had thought about the research paper, and they were ready to go at it. And there were no hard feelings.

One thing that was pretty important for me in my development was an office visit with Eugene Fama, my dissertation adviser, where I had a couple of ideas to pitch for a dissertation. I pitched the first idea, and he barely looked up from whatever paper he was reading and shook his head, saying, “That’s a small idea. I wouldn’t pursue it.” Then I hit him with the second idea, which I thought was way better than the first one. And he kind of looked up and said, “Ehh, it’s okay. It’s an OK idea.” He added, “Maybe you can get a publication out of it, but not in a top journal.” He indicated I should come back when I had another.

Even though he had shot down both of my ideas, I left feeling energized. The message from him was that I had a chance of hitting a big idea. That interaction, which I am sure he doesn’t remember, was very influential — it pushed me to search for big ideas and not settle on the small ones. **EF**