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Campbell R. Harvey, PhD: Examining  
Quantitative Investment Strategies



**INVESTMENTS & WEALTH INSTITUTE®**

# Campbell R. Harvey, PhD

## EXAMINING QUANTITATIVE INVESTMENT STRATEGIES

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Campbell R. Harvey, PhD

*Harvey received the 2016 and 2015 Best Paper Awards from the Journal of Portfolio Management for his research on distinguishing luck from skill. He has received eight Graham and Dodd Awards/Scrolls for excellence in financial writing from the CFA Institute. He has published more than 150 scholarly articles on topics spanning investment finance, corporate finance, behavioral finance, financial econometrics, and computer science. He edited the Journal of Finance—the leading scientific journal in his field and one of the premier journals in the economics profession—during 2006–2012.*

*Harvey is a founding director of the Duke CFO Survey. This widely watched quarterly survey polls more than 1,500 chief financial officers worldwide. He is a partner and senior advisor at Research Affiliates, LLC, which oversees more than \$180 billion in client funds, and he serves as investment strategy advisor to the Man Group PLC, the world's largest publicly listed global hedge fund provider.*

*In March 2021, Campbell Harvey spoke with members of the Journal of Investment Consulting editorial advisory board about quantitative investment strategies. Taking part in the discussion were Inna Okounkova, Columbia University and editor-in-chief of the Journal; Mark J. P. Anson, The Commonfund; Edward Baker, Mesirow Financial; Ludwig Chincarini, University of San Francisco and United States Commodity Funds; Philip Fazio, Merrill Lynch; Geoffrey Gerber, TWIN Capital Management; and Tony Kao, SECOR Asset Management.*

**Inna Okounkova:** What major forces helped to shape your career and bring you where you are today?

**Campbell Harvey:** When I was an undergraduate, I was not thinking that I would be an academic. I did a liberal arts degree that included mathematics, history, English, and subjects like that. During my MBA studies, I started working as a research assistant on a few academic papers, and I realized I could actually contribute at the level at which I was working. A

significant event happened to me in the summer of my first year. I took a job in corporate strategy at the then-largest copper-mining firm in the world, based in Toronto. I was tasked with building a model to predict future gross domestic product (GDP) growth—and for copper that is very important information.

Because copper moves with the business cycle, any ability to forecast GDP helped the company make decisions about opening and closing mines around the world. I knew a lot of companies did this type of forecasting with complex econometric models. I considered those models unnecessarily complex and extremely rigid—so I had the idea of trying to extract information from financial markets. I recalled some research at the University of Chicago had analyzed stock prices for information about future economic growth, and the evidence was mixed. At the time, the joke was that the stock market forecasted nine of the past four recessions.

I started by trying to discern why this information was so unreliable. The stock market seemed like an ideal indicator because we would expect future economic growth to drive anticipated cash flows for companies. But in the case of equities, a lot of other things are going on. As a result, there are many false signals. So I looked at the bond market. Bond yields are also forward-looking, but there are a number of differences between, let's say, Treasury bonds and the stock market. First, bonds have a fixed maturity, stocks do not. Second, Treasury bonds have a fixed coupon, which means investors know exactly how much they will get at the bond's maturity, so essentially the cash flows are fixed.

Another aspect of this project that intrigued me was the risk. Stocks are risky. That shifting risk may or may not be related to the business cycle, whereas Treasury bonds are relatively risk free. Both investment vehicles should contain information about the future. Basic theory tells us that a bond's yield is a combination of the expected real rate, expected inflation, and a risk premium. The real rate is linked by economic theory to real GDP growth. I looked at the slope of the term structure of interest rates, or the yield curve, and I found it to be a reliable forecaster of economic growth.

I was preparing a presentation to the highest-level officials in the company, but a week before I was scheduled to present, the entire corporate strategy group was laid off. I was out of a job as a summer student. Nevertheless, I continued working on the idea.

In the fall, when I went back for the second year of my MBA program, I showed the paper to a few faculty members and they said: "Oh, this is important. This is a big idea." They said there are three courses you are required to take to get your degree. We will combine those into one course, and all you are going to do is work on this paper. When they looked at the next version, they suggested that I apply to a PhD program. My application said: "I have an idea, and here is a draft of the paper I am working on. I know it is early, but I am really interested in this research, and please admit me to your program." I was admitted to multiple programs, but I decided to go to the University of Chicago. I showed up with the paper, although the usual approach is to spend a couple of years doing course work and then begin thinking about your dissertation topic.

For me, it was completely different. The first day at the University of Chicago, I was working on my idea. Eventually, my dissertation committee looked at the paper. They were skeptical, as they should have been. The committee included future Nobel laureates: Eugene Fama,<sup>1</sup> who was my chair; Merton Miller;<sup>2</sup> and Lars Hansen;<sup>3</sup> and of course others that I learned a lot from. They were skeptical because my dissertation showed that an inverted yield curve—when long-term rates go below short-term rates—predicted recessions. Data were limited, so the inverted yield curve worked four times out of four. The committee members said I might just be lucky. I said maybe it is a lucky finding, but the economic foundation is rock solid; my finding is not data mining. We can all agree the economic foundation should work, and it actually does work.

They were impressed that I predicted the double-dip recession in the early 1980s when the major econometric services did not. Of course, they also were impressed that the cost of delivering this forecast was much lower than paying thousands of dollars for a forecast from one of the econometric services. At the time, the cost of a copy of the *Wall Street Journal* was 25 cents. And

here was a forecast that was as good as one that cost thousands of dollars.

After you publish a research finding, two things usually happen. First, if your finding was due to luck, the effect gets weaker. I was showing four accurate forecasts out of four, and the effect could weaken. What often happens is the effect completely fades away. The out-of-sample validation of my dissertation turned out well. We have had four recessions since I graduated, and each was preceded by an inverted yield curve. So far, no false signals. You could argue that the last forecast was lucky because the inverted yield curve in June 2019 obviously was not predicting the COVID-19 pandemic. But consider the counterfactual analysis. In late 2019, according to the Duke CFO Survey,<sup>4</sup> 70 percent of chief financial officers (CFOs) thought a recession was going to occur in the next twelve months.

That is how I got started with my idea that some degree of predictability is inherent in the business cycle and also in risk premiums. I have published a number of papers on dynamic risk premiums. Each of the papers stems from my initial insight of analyzing the term structure of interest rates and economic growth in the business cycle.

**Inna Okounkova:** So the low interest rates did not have any impact on that signal?

**Campbell Harvey:** My model is pretty simple. It has one variable: the difference between long-term and short-term interest rates. It has successfully predicted eight out of the past eight recessions. The model is also good at predicting the duration of the recessions. Although some may say the model is not working because of the Fed's quantitative easing (QE) policy and low interest rates, my response is no model is perfect, there is a lot of noise. Indeed, the Fed's influence was far greater in the early part of my sample. In the 1960s and 1970s, Fed policy could affect the entire yield curve. Today, it is difficult for the Fed to control the yield curve because persistent government deficits and off-balance-sheet items have caused the bond market to grow very large.

The Fed does have some control at the very short end, though some think that degree of control is overstated. I do not buy the argument that because of QE, my model no longer works. I have heard that argument multiple times. I heard it before the Global Financial Crisis when the yield curve inverted. I certainly heard it in 2019. Actually, I believe the Fed induces a lot of the noise, but the track record of the indicator speaks for itself.

**Ludwig Chincarini:** When these models are used, the signal starts maybe in month T minus 10. If you actually start trading when the signal arrives, you lose money overall. Is that the case for your model, or do you always make money?

**Campbell Harvey:** You are asking a completely different question. You are asking about using the signal in a trading strategy. I am talking about the ability of the slope of the yield curve to predict economic growth. Those are related but different questions.

Given that my model has a good track record as an indicator of expected economic growth, we should be able to design some sort of strategy to trade on that signal. It is a challenge, however, because the stock market does not behave exactly like the business cycle.

In some recessions, the stock market actually goes up. Timing is important, so you are correct in positing that an inversion might happen a year before the recession starts. So would you wait to initiate the strategy? I have not published a paper linking this idea to any trading strategy, but I know some asset managers use this information for their quantitative trading strategies. Investors need to be careful in trading on this signal because we have only had eight recessions. That means only eight observations, so it is easy to overfit a strategy. In addition, this information is just a single variable. An asset manager certainly should consider other variables in developing a successful trading strategy.

**Inna Okounkova:** What do you regard as your major achievements?

**Campbell Harvey:** One of my favorite papers is my 2000 *Journal of Finance* article on conditional skewness and pricing (Harvey and Siddique 2000), which has had a significant impact on the profession. Essentially, the concept of the paper is that we seem to be stuck in a world of expected returns and variances. Even today, investors tend to compare the Sharpe ratios of different strategies and ignore other dimensions of risk.

In Markowitz's 1952 Nobel prize-winning paper, he acknowledges the assumptions he is making. One of the assumptions he clearly spells out is that the model does not work if there is a preference for higher-order moments—for example, a skew. We know that investors dislike downside risk, and we also know that asset returns are not distributed symmetrically. So it is important for asset managers to explicitly integrate the downside risk or skew in portfolio design. A lot of portfolio designs do not take skew into account; consequently, the portfolio manager has to rely on risk management as a second process. I have long advocated that risk management and portfolio design should be integrated. This point is forcefully made in one of my recent books (Harvey et al. 2021). One way to accomplish that is to explicitly take into account the downside in the optimization.

I also believe my 2017 Presidential Address published in the *Journal of Finance* has had an impact in the industry.

In the address, I point out that more than 400 factors (an implausible number) have been published in top journals, likely a result of data mining.

One of the topics I discuss in the address is incentives. Journal editors want their journal to publish papers that will get as many citations as possible. They know that research with negative results—the researcher tests a signal that does not work—garners very few citations. Authors also know the objective function of the editor so they do not pursue papers that show negative results. They may put them in a file drawer and continue to try to find a factor, signal, or methodology that actually works. This practice leads to data mining and publication bias. My guess is that at more than 90 percent of the finance schools in the world, the author of a single publication in the *Journal of Finance* has a job for life. The incentive to find a factor that works is extreme. In Harvey et al. (2016), I called out the profession, arguing it is likely that more than half of our empirical findings are false, which is pretty strong language. I said that goes for my research as well.

I think members of the profession have thought about this and have come to believe we need a higher hurdle than the usual two standard errors, or two-sigma rule, which is appropriate only if the researcher does a single test. That is basic statistics. If 400+ factors can clear this hurdle, we need a higher hurdle.

Another part of my Presidential Address concerns the need to be careful about how we make inference; we need to incorporate our prior beliefs. I give an example of a series of three experiments.

The first experiment involves a musicologist who claims she can determine by looking at any two pages of a Haydn or Mozart score whether the composer is Haydn or Mozart. I play piano. I play Haydn and Mozart, but there is no way I can do that. So an experiment is designed in which ten different pairs of scores are used, and the musicologist gets ten out of ten correct.

The second experiment involves a tea drinker who claims to be able to tell if the milk was put into the teacup before or after the tea was poured. This seems a little implausible. So the experiment uses ten pairs of cups, and the tea drinker gets ten out of ten correct.

The final experiment involves a bar patron who has had too much to drink and claims that the more he drinks, the better he can see the future. In this experiment, we flip a coin ten times, and the tipsy patron calls every single one right.

So, we have three different experiments—ten in a row—and the  $p$ -value is less than 0.001. What's your inference? What do you take away from that? Classical statistics tells us that all of

these results are highly significant. Experiment one involves a musicologist, and a person in that profession should be able to tell the difference between Haydn and Mozart. Perhaps you are skeptical about the tea drinker in experiment two, but maybe there is a bit of truth in the claim, and we can learn from the  $p$ -value that this person actually can discern the difference between the milk going in before or after the tea. The third experiment is preposterous. I don't care if the person gets twenty in a row. It does not make sense that the person can foresee the future. We all need to consider our prior beliefs.

Classical statistics just looks at the  $p$ -value, and too often we rely only on classical statistics. To make things worse, there are many fundamental misunderstandings about what that  $p$ -value actually means. My Presidential Address encourages members of our profession to include their prior beliefs in making inferences. This approach is not exclusive to an academic setting; it can be used in most practical asset-management decisions. Let's say you develop a trading strategy based on a solid economic foundation—for example, my yield curve model. It appears to work. Then you are presented with a purely data-mined strategy, developed perhaps after thousands of different variables were tried. The strategy works better than the strategy with the solid economic foundation. So what do you do? You should severely down-weight the inference for the data-mined strategy—because it is data mined—and lean more toward the strategy that has a solid economic foundation. That is how prior beliefs should be used.

**Philip Fazio:** Now that you've exposed all of this in several good papers, where are we in the academic world with data mining?

**Campbell Harvey:** I spent a year reading articles outside the field of finance, and only about 10 percent of the references in my Presidential Address are related to finance and economics. I learned how other fields minimize the chance of false positives, but some of the solutions are difficult to apply to finance. For example, in an experiment in biology or the medical field, the researcher can pre-register the protocol: what will be tested, the sample size, the statistical procedure, the cutoff, the outlier exclusion rule. All parameters are specified in advance. During the experiment, the researcher follows the protocol, which minimizes the chance of severe data mining.

However, in finance, the data are easily available—data on stock returns, data on company fundamentals—so the researcher could data mine and register the experiment, even though the researcher already knows the results. Much of the problem results from what we reward in the finance profession; for example, some schools just count a faculty member's publications and base promotions and raises on that number. Top schools, however, do not do that. They look at the quality of the papers,

some of which might not even be published in a top journal. They are looking for papers that will have a major impact. A paper that is a data-mining expedition is not going to have any impact, because it is not going to perform well out of sample. In real time, however, an editor cannot discern the potential impact easily.

The data-mining problem is less severe in the practice of asset management. The reason is simple: When a researcher who data mines moves the strategy into live trading, the strategy likely is going to fail. That means that the company loses reputation, loses the assets under management, and does not earn a performance fee. For asset managers doing quantitative research, incentives are aligned. Having the right research culture is crucial. The penalty is severe for having a culture that encourages data mining. Academia has no penalties like that, other than in the top schools, so there is no easy fix.

**Philip Fazio:** Can't you just require an out-of-the-sample period as part of the test framework?

**Campbell Harvey:** That is hard to do. Consider submitting a paper to the *Journal of Finance*, and the editor's decision letter says: "This looks interesting. Resubmit the paper with the out-of-sample evidence in twenty years." We know that stock returns are quite volatile, and the signal-to-noise ratio is really low. Twenty years might not even be enough. Again, researchers need to consider prior beliefs in the context of their being based on economic fundamentals. We simply do not have enough signal. The researcher has to be disciplined by economic theory. That is a way to minimize the effects of data mining.

**Ludwig Chincarini:** Another thing you mentioned in your Presidential Address, which I thought was important, is that we don't do a lot of replication studies in finance or economics. How do you think the profession can move forward in this area? Would it be a good idea to have graduate students replicate someone else's work?

**Campbell Harvey:** My 2019 paper called "Replication in Financial Economics," which was published in *Critical Finance Review*, shows that I am an advocate of replication studies, and the mechanism you suggest is exactly what this paper proposes. The idea is that replicating a paper should be part of PhD programs for a first-year or second-year summer project. Replication has many dimensions. One goal would be just to get the result in the sample that was used, but there could be other avenues. The student could start with that, then maybe take the result out of sample to test its viability, maybe take it to a different country. Many possibilities exist, but the replication task would be assigned to a student and then—this is important—the results would be posted to a public database.

One of the costs of replication is that students, often master's program students, email to say: "I tried to replicate your finding. It must be wrong because I can't replicate it." Then you waste time working with the student, who probably has made basic coding errors. Let's say we replicate a study one time and place the results in a public repository. When we run out of papers to replicate, new students in the second round of replication can deal with the students that did the first round of replication, rather than dealing with the original author. The new students might find mistakes in the old replications. That is a way to deal with this problem. Most of the journals have code-sharing policies now, and that's helpful.

We do not have much replication in financial economics, mainly because most data is widely available. Those data are so easy to get that the cost of replication is pretty low, but the main point I made in my paper is that we need to reduce the cost of replication, we need to make it easier. *The Journal of Finance* now has a section for replication studies, which is a step forward; in fact, a replication paper I submitted, Harvey and Liu (2021), is forthcoming. (The paper is now posted on Social Science Research Network, or SSRN.) If senior people in the profession begin to do replications, it could change the culture. We need to change the culture, not just the attitude of researchers, but also the attitudes of people in the dean's office and others at the university who might regard a replication study as somehow inferior to another type of paper. In other sciences, replications are treated equally.

**Inna Okounkova:** Maybe the industry can contribute as well—because quant shops do retest signals on different data, first, before deploying them. In many cases, in my experience, the signal does not work but it's kept silent.

**Campbell Harvey:** You are correct that every day industry professionals pour over what is posted on SSRN. If they see an interesting finding, they send the paper out for replication; often the research works, but the academic ignores real-world frictions, such as trading costs. With reasonable trading costs, the effect may go away. Even with the research on 400 factors, no study (that I know of) includes trading costs. For example, a cross-sectional momentum strategy with 800-percent turnover is going to be costly and will likely wipe out whatever alpha is reported.

Replication is going on within the industry, but no one is interested in sharing it because they want their competitors to waste resources doing the replication too. The same problem occurs in the pharmaceutical industry with ten firms conducting the same experiment, but nobody knows what anybody else is doing. A company might try fifty different signals and find that forty-eight do not work. That result would be useful to post, but the company does not post it because its decision-makers want their competitors to waste resources pursuing the

forty-eight other signals that they know do not work. A similar problem exists in other sciences. There is no easy fix.

**Edward Baker:** I can't help thinking that this excessive fascination with factors is making the markets less efficient.

**Campbell Harvey:** Numerous forces make the markets less efficient. Quantitative asset managers are typically careful about the robustness of the factors in their strategies, because they do not want to disappoint clients. Some exchange-traded funds, however, are thrown together on the basis of an academic paper. The manager pitches the fund to clients and explains that the strategy is based on peer-reviewed research. Some managers will promote hundreds of these types of funds, knowing that more than half the strategies are probably not going to perform well, but collect the fees anyway. So, you are correct that the current focus on factors can lead to inefficiencies.

Many different forces drive the balance between inefficiency and efficiency. While we have great data and the latest machine learning tools, there is still endemic overfitting. So, in deploying these strategies, they drive prices away from fundamentals. We saw that happen this year with the rise of retail investors' participation growing from about 15 percent of trading volume to 20 percent, and maybe it will even rise to 25 percent next year. These are part-time investors, and given the complexity of the information involved, we have seen them drive prices far from reasonable fundamentals. In my opinion, the market will become less efficient in the short term before it becomes more efficient in the long term.

**Philip Fazio:** Why isn't the academic world more rigorous with regard to economic or practical motivations for the tests you're doing? You'd think you could nip a lot of these problems in the bud with strong motivations.

**Campbell Harvey:** I think my research has had some impact in that it is nearly impossible for a job candidate to seek an assistant professorship in finance or economics on the basis of an empirical paper showing that a trading strategy or factor "works." That route is effectively gone. At the profession's top journals, almost always an economic foundation motivates the empirical exercise described in the article. Today, it is extremely difficult to get a paper published by simply doing some data analysis. I think we are moving in the right direction.

**Edward Baker:** You founded the Duke CFO Global Business Outlook Survey. Surveys in general have quite good predictive power. How would you evaluate the efficacy of your survey, and what are your plans for future developments in that area?

**Campbell Harvey:** I am a co-founder of the Duke CFO Survey. Part of my motive in starting the survey was that I thought we

were too siloed as academics. We were writing papers about how people act without talking to them. My idea was to get a group of chief executive officers and CFOs engaged and interact with them to learn about the practice of finance—how they make decisions. Even if they are taking actions that are theoretically incorrect, that knowledge helps us think about that decision point in a different way. Ultimately, my motivation was twofold: to interact with practitioners directly and to identify a tangible measure of expectations that could be used in other applications.

In finance, our models are about expectations. The capital asset pricing model says that the expected excess return on a stock is proportional to the beta, which is multiplied by the expected risk premium. The problem is that we cannot directly measure the expected risk premium and the expected return on a stock. The Duke CFO Survey has the ability to measure expectations. I was pleased that Andrei Shleifer used our data in an important paper (Gennaioli et al. 2016) because his influence in economics is substantial. We have seen a shift in economics that actually considers expectations as a first-order item.

Think of the survey results as new data we did not have before. We have collected these data over twenty-five years, and a number of papers are based on them. Most people in finance do not realize that the paper of mine that has had the most impact is not in finance; it is in accounting. The paper (Graham et al. 2015) is about earnings management. We asked a question related to how earnings were manipulated. The question was, “Would you be willing to destroy firm value to smooth out your earnings and hit the target?” That question purposely included a hot word—“destroy.”

Including that word in a question is unusual. Number one, we would expect some inherent bias, such as “Oh, we would never do that.” But using the word “destroy” increases the bias. Our results were shocking. Seventy-eight percent of the CFOs agreed that they routinely destroyed value to smooth their earnings. This discovery changed the way we thought about earnings, smoothing, and manipulation. In accounting, earnings management was thought to be done with accruals, smoothing them by recognizing an amount early or late, but the CFOs said, no, we are not doing that because it is observable and we do not want to be called out for anything that looks like accounting shenanigans. We showed that they take other actions, such as slashing research and development spending, delaying or canceling investment projects, reducing advertising, and delaying hiring. All these actions were originally planned because they yield positive net present values. A project is approved because it has a positive net present value. If canceled or delayed to manage earnings, the CFO is destroying value. Nothing about this is illegal, however; it is just a business decision. We were able to gain these types of insights by asking the CFOs directly about their practices.

**Edward Baker:** You’ve used this survey for a long time to estimate the equity risk premium. What are the current surveys telling you about the equity risk premium? What estimates are you seeing, and what is your personal estimate, especially in the interest-rate environment we’re in now?

**Campbell Harvey:** The survey asks respondents to estimate the equity risk premium over one year and ten years. The ten-year premium is probably most relevant because it is used for longer-term asset allocation and for calculating the cost of capital. When we look at the yield spread between, let’s say, the S&P 500 Index and a ten-year bond, historically that premium has floated between 2.5 percent and 5 percent. Because we have a long sample, it is fairly smooth. Currently, the premium is around 3 percent, which suggests the market is not expecting an abnormal stock-bond relationship over the next ten years. The premium is a forecasted measure of what is expected to happen in the stock market relative to the bond market.

**Edward Baker:** I’d like to go back to your comment about the impact of retail investors on the markets these days and think about cryptocurrencies. I know you’re in the process of publishing or have recently published a paper on that topic. What is your view about whether institutional investors should be including cryptocurrencies as part of their asset mix?

**Campbell Harvey:** I have a full teaching deck on that question, but I will try to give a brief answer to your question. The crypto complex is currently worth \$1.8 trillion. Apple is worth \$2 trillion, about the same. A reasonable question is, would a globally diversified portfolio hold any Apple stock? Yes, of course. Should it hold some cryptocurrency? It is hard to make the case it should be excluded. People use the term “crypto” broadly, but that’s misleading because of the many types of cryptocurrency. Bitcoin, the oldest cryptocurrency, makes up about 50 percent of crypto’s total capitalization. The original vision was that bitcoin would be a transaction mechanism, but it turns out that is not practical. The current model makes bitcoin transactions too expensive and too slow to use widely, but it is useful for large transactions. Ethereum is a distributed computational platform that has spawned the world of decentralized finance, which I consider really exciting.

In February 2021, someone moved \$5.2 billion in bitcoin from one address to another, and the cost for that transaction was \$17. That is amazing. Right now, however, bitcoin falls more in the category of a store of value. But the arguments I hear about why bitcoin should have a very high value do not make a lot of sense. The claim that bitcoin equals new gold is a logical fallacy. Gold has a value of \$9 trillion. That means, with 21 million units of bitcoin, each bitcoin should be worth \$400,000. The fallacy is that the result is arrived at by saying that bitcoin equals gold.

In my recently released book, *DeFi and the Future of Finance*, I take a deep dive into what is happening in the decentralized finance (DeFi) space. Essentially, we have been operating in the world of centralized finance for 100 to 150 years. Even with the advent of computers, not much has changed. We have the same banking structure—central banks, brokerages, insurance companies—and the same business model we have had for years. Decentralized finance, however, totally changes the game. Saving and lending occurs between peers. DeFi has no overhead, no brick-and-mortar structures, so in DeFi savings rates go up, and borrowing rates go down.

In this new world, the spread enforced by commercial banks basically disappears, which creates a substantial disruption. Exchanges are decentralized and trading occurs peer to peer with an algorithm. Much is going on under the radar screen. Those who simply focus on bitcoin and whether it should be in their portfolios are not seeing the scaffolding of a new city that is being built and that will completely change the way we conduct finance, a city where everybody is banked. Right now, 1.7 billion people in the world are unbanked, and the same number are probably underbanked. What do I mean by underbanked? Suppose I am an entrepreneur. I have a great idea that I think can yield a 20-percent return on investment. I go to the bank for a loan and they say my idea is great but they would prefer to have a large customer rather than 100 customers like me. But the bank increases the credit limit on my credit card to cover the loan. However, the rate on the credit card is 20 percent. Consequently, I will never pursue that project.

With this new technology, finance is a lot more democratic. Many people in the middle are eliminated. Anybody can pursue projects that have high rates of return—the types of projects that can deliver substantial economic growth. Right now we are essentially stuck in 2-percent, maybe 3-percent, real GDP growth. DeFi gives us the possibility of reducing the financial constraints that exist today, opening up the possibility of much higher growth. Ideally, no one should be prevented from pursuing an opportunity that is a good idea. The door should be open for someone to finance it.

We are heading toward a world where everything is tokenized, and the mainstream media are finally covering more than just bitcoin—for example, nonfungible tokens, or NFTs, which tokenize unique items. We sometimes call them deeds. The item could be a piece of art, a contract, a loan—any unique asset.

We take basic economics courses as undergraduates or even as PhD students. We study the history of markets. My book begins with the statement that we have come full circle. What I mean by that is the original market exchange was peer to peer, it was a barter system. Bartering in that context was inefficient, and we needed some medium of exchange to mitigate that inefficiency. This new technology promises a means

to barter in a different way. It is efficient, and everything is tokenized.

Investing in crypto might mean investing in a store of value like bitcoin, or it might mean investing in a stablecoin. But why do that if the crypto is linked to the dollar? Because it allows deposits in many of the protocols that offer a much higher savings rate than is available from a traditional bank. Or it might mean investing in a governance token, which is almost like buying a piece of a startup in a different way; instead of investing with equities, invest with tokens. In the future, diversified portfolios can be far more diversified because investors will be able to add assets to their portfolios they could not have added in the past. Illiquid assets, which are difficult to invest in, will be newly accessible.

It is fairly obvious that stocks will be tokenized and we will trade the tokens, but the market for tokenizing private equity is really huge. Companies not yet public—pre-IPO [initial public offering] firms—represent an illiquid market, but this technology offers the average investor the possibility of investing in these types of firms very early. The diversification possibilities are enormous, which is good for all investors and good for companies that are seeking financing. In the past, companies basically had to deal with a bank to get financing, and many could not. DeFi offers a different method of financing.

**Edward Baker:** You published an interesting paper, Bekaert et al. (2016), in which you showed that emerging markets still behave quite differently from developed markets and should be considered as a separate asset class. Why has the convergence process taken so long? Will emerging markets ever converge, and is environmental, social, and governance (ESG) investing part of that process? If emerging markets actually embraced global ESG standards, would that facilitate this process?

**Campbell Harvey:** Some traditional emerging markets have become developed. The United States was an emerging market at some point, and now it is a developed market. This progression from emerging to developed will continue. Eventually emerging markets will disappear (at least on this planet), and that is a good thing. But in the meantime, certain countries are just at the start of their financial development, and they offer special challenges, special risks, and special opportunities for investors. These markets do not move in lockstep with markets in the United States, but as a group they are more correlated today than they were, say, twenty years ago. The emerging markets provide diversification potential and the possibility of higher expected returns. Investors who are willing to take these risks will pursue those investments.

Will there be crises? Yes. Will there be crashes? Yes. But with diversified portfolios, investors can hope to ride through those challenges. Robust equity markets provide much-needed

capital for emerging markets so they can move more rapidly toward becoming developed markets. I feel quite positive about emerging markets in general, because with technological change the growth of these markets will accelerate. Many people in these countries cannot access schools or even the internet, but that is changing. Eventually, everybody in the world will have free internet. Everybody will have some sort of smartphone. That means they can become part of the “internet of value,” and those who are ambitious can realize their dreams. I think we will see substantial growth—much higher growth in emerging markets than in developed markets—and historically we have learned that what is good for emerging markets is also good for developed markets.

Regarding the second part of your question, I am nervous about a company selling an ESG investment product that promises “ESG alpha.” Asset managers point to 2020 and say, “Look, these ESG-friendly stocks did really well.” True, prices went up in 2020, appearing to validate the ESG investment thesis, but what does that mean? We know that often as prices go up, expected return goes down. I think many investors will be disappointed. Plenty of greenwashing is going on.<sup>5</sup> Some managers are more concerned about getting investors to allocate capital to their funds than the quality of the products they are offering.

E, S, and G are difficult to define. The most precise component to define in terms of carbon is E, and a low-carbon portfolio is the easiest to implement. The G component has extremely diverse interpretations, and the same is true for S. These themes are difficult for quantitative finance to measure. We might be able to quantify some level of carbon impact, so I think most of the investing will be in the E component. Still, I worry that many ESG investments are oversold. To get ESG alpha, a backtest is concocted, perhaps with data mining and overfitting. The backtest looks good, but investors likely will be disappointed by the performance out of sample.

**Edward Baker:** I was alluding more to government policy and government’s ability to destroy value, especially on the G side, by implementing policies that hurt foreign investors. I think emerging markets have a long history of experiencing problems of this sort. Turkey is a recent example.

**Campbell Harvey:** Government policy that destroys value is not just a problem in emerging markets. If regulation is too harsh, companies just go offshore. If regulation is too weak, consumers are exploited. The regulators who formulate these policies should be making a big investment to get the right balance. This regulatory dilemma is apparent not only in emerging markets.

A situation could arise in an emerging market involving some sort of expropriation. Perhaps a company had a deal with the

government for a lower tax rate, but the tax rate still went up. The next time the firm is considering investing—or possibly another company is considering investing for the first time in that country—they remember the country’s track record; the risk premium associated with that country goes up and consequently less money comes in. There is an equilibrating process, but the problem is that politicians are not necessarily in office for the long term. Often, they make decisions that look good in the short term but cost the country in the long term. This is a problem in the United States, where the regulatory horizon is typically between two and six years.

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*We could call this crowding. Essentially, the asset manager is not establishing boundaries, not imposing limits. The asset manager takes more capacity than is feasible for a particular strategy in order to get more assets under management.*

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**Edward Baker:** It makes politicians sound like CFOs.

**Campbell Harvey:** Yes, that is exactly the case. Short-termism is a fundamental problem with our political system and with the way businesses are run.

**Ludwig Chincarini:** May I turn us to the topic of crowding? You recently wrote a paper, Arnott et al. (2019), with Rob Arnott on the crowding of factors. Then you wrote a very good paper, Harvey et al. (2020), about fund structure. What’s your primary takeaway from your research into crowding?

**Campbell Harvey:** I am increasingly sympathetic to the Berk and Green (2004) idea that we should not look at fund manager alpha after all expenses. They argue that we need to consider the gross return of funds, before fees. Even if fund investors earn zero alpha, managers are being paid. Consider the incentives in this situation if we assume a fixed dollar amount of alpha. An asset manager who has a limited amount of assets under management could distribute some of that alpha to her investors, but with a much larger amount of assets under management, distributing that fixed amount of alpha would cause it to eventually go to zero. That scenario is relatively consistent with what we observe in mutual funds, for example, when the alpha (after fees) is essentially zero across the complex.

We could call this crowding. Essentially, the asset manager is not establishing boundaries, not imposing limits. The asset manager takes more capacity than is feasible for a particular strategy in order to get more assets under management. The average alpha is lower as a result. Again, it is a matter of

incentives. Although crowding is important, measuring it precisely is difficult. Many years ago, I was a scientific advisor for a major U.S. pension plan. When I did due diligence on alternative asset managers, such as hedge funds, one of the things I looked for was how many of a manager's funds were not allowing any new investment. That discipline is necessary for an investor to make any excess return.

The crowding paper you mentioned is in the review process. It has an interesting angle—that as the money comes in, an individual manager has a limited capacity for generating new ideas. We show that the so-called decreasing returns of scale—which means lower alpha with more assets under management—are mitigated when we move from a single asset manager to a team of asset managers, where more than one person is coming up with new ideas. We also show that diversity is important in terms of the background and expertise of that team. If the team is just a replica of the original solo manager, adding managers is not much help. Although these are characteristics investors can consider, crowding is definitely a phenomenon that negatively affects performance.

**Mark Anson:** I'd like to go back to your comment about mutual fund returns. Another of your great papers was "Luck versus Skill in the Cross-Section of Mutual Fund Returns," in which you re-examined the evidence and reconciled the economic results presented by Kosowski et al. (2006) and the results presented by Fama and French (2010). I think you found that Fama and French under-rejected the null hypothesis as zero alpha and Kosowski over-rejected it. Then you did a reconciliation of their sampling methods and explained why that would be the case. But coming back to the comment you just made, your paper remains silent on your belief regarding the ability of mutual funds to generate alpha. Can you offer some insight?

**Campbell Harvey:** Great question. You are referring to the replication paper that is forthcoming in the *Journal of Finance*. Fama and French essentially said there is no alpha, and Kosowski et al. said there is. Both papers used bootstrapping techniques—so what was going on? It turns out this replication exercise was a computational nightmare. It was the first time I did a large-scale deployment to a cloud computing platform. Someone reading that paper would have no idea of the computational power behind it. We implemented both sets of authors' bootstrapping techniques and found that both have issues. I think our paper will be useful going forward, because both papers are cited frequently by authors who just pull them off the shelf to make their inferences.

You asked my opinion about the ability of mutual funds to generate alpha. My opinion is that likely a small proportion of mutual funds are able to outperform benchmarks after fees—probably in the range of 3 percent to 5 percent. It is a really low number, and it is hard to find those funds. The way I see it, a

highly skilled fund manager delivering good performance with a mutual fund likely will jump to a hedge fund whose rewards are a lot greater. The proportion of hedge funds capable of delivering good performance is far larger. Even though thousands of hedge funds have little or no ability to deliver good performance, some certainly can.

**Geoffrey Gerber:** I'd like to discuss asset allocation related to your research on negative convexity. Compared to a buy-and-hold portfolio containing a 60/40 ratio of stocks and bonds, you induce negative convexity and give up return by selling to rebalance. We know that from a statistical standpoint, you reduce risk by rebalancing; you reduce risk by going back to your target portfolio. So from the standpoint of a Sharpe ratio, is there a sweet spot? What advice would you give someone doing asset allocation relative to a 60/40 target? When do you move back, or do you just not move back?

**Campbell Harvey:** Investment finance has two pillars: diversification and rebalancing. These concepts are so basic that almost everyone in finance thinks they understand them. In reality, diversification is poorly understood because many investors think of diversification as reducing variance, but risk involves more than variance. My 2000 *Journal of Finance* paper argued that, at a minimum, we need three dimensions. The downside needs to be taken into account. Consider two investments: one with a high Sharpe ratio and one with a lower Sharpe ratio. The higher Sharpe ratio investment may not be the better choice because it might have a giant downside tail.

Rebalancing seems really simple. For example, start with a 60/40 portfolio and do not deviate from the 60/40 ratio by periodic rebalancing. The idea of course is that rebalancing allows for greater diversification. If the portfolio just runs with no rebalancing, eventually the equity portion would be 99 percent, and the portfolio would simply be an equity bet with no meaningful bond exposure. The 60/40 capitalization-weighted portfolio does not really have a 60/40 ratio in terms of risk or variance; that ratio is closer to 90/10. Nevertheless, let's go with 60/40.

The first result is that mechanical rebalancing to 60/40 induces bigger drawdowns. The reason is simple. In a persistently falling market, buying to rebalance increases the magnitude of the drawdown and introduces a negative convexity. Buying in a falling market is akin to dynamically replicating a short put option. Conversely, selling in a rising market replicates a short call option. Put the two actions together and the outcome is a short straddle, which has, by definition, negative convexity. So, rebalancing is like adding a short straddle to a 60/40 portfolio and that short straddle induces extra risk.

I have been interested in rebalancing for quite a while. In a paper called "Strategic Rebalancing" (Ratray et al. 2019) as well as my new book, *Strategic Risk Management* (Harvey et al.

2021), we show both theoretically and empirically that rebalancing causes additional risk. People were surprised at this finding. They said, “I had no idea my rebalancing actually increases risk.” We advocate in the paper to be strategic about rebalancing. Why not just fit a simple trend-following model? If the trend-following model suggests the market is continuing to trend down, delay the rebalance. Use that information to strategically time the rebalancing. Historically, this strategy has been very effective.

Importantly, waiting to rebalance does not cost anything. The models we present are incredibly simple—models for three-month momentum or twelve-month momentum—and are straightforward to implement. The payoff is substantial in that the size of the drawdown is reduced when compared with the drawdown from using a mechanical strategy. This paper is quite simple, but investors still can learn much from it. They may have thought they understood rebalancing but realize perhaps they do not. My paper about commodity investment with Claude Erb (Erb and Harvey 2006)—along with a couple of earlier papers, in particular, Booth and Fama (1992)—also made this point. I believe the paper on strategic rebalancing already has had an impact, because I have talked to some major fund managers who are interested in changing their rebalancing process.

**Tony Kao:** Current peacetime stimulus policies may trigger inflationary pressures not seen in a generation. If restrictive policies are enacted, they are likely to shrink equity risk premiums and significantly increase the cost of servicing government debt, which would not be market friendly. Are the markets taking sufficient account of the potential risks associated with rising levels of government debt, particularly in the United States? And given the expectation of rising inflation and real yield, how do you think quantitative investment strategies will handle the potential shift in repricing the market relationship?

**Campbell Harvey:** That is a current-events question, but let me answer it like this. I have a new paper called “The Best Strategies for Inflationary Times” (Neville et al. 2021). In this paper, we examine ninety-five years of data in the United States, the United Kingdom, and Japan, and analyze a range of different assets: equities, fixed income, and commodities; collectibles such as art, wine, and stamps; and a variety of dynamic approaches such as factor strategies and trend-following strategies. In the United States, we analyze eight inflationary episodes. The paper’s basic message is that equity performs poorly in inflationary times, thus we need to be careful about how we define inflationary times. Our definition is that inflation starts at around 2 percent and then crosses the 5-percent threshold.

If inflation is negative and becomes positive, that is likely good news because the economy moves from a depression or severe

recession to normal times. Our real interest is in those periods when the inflation rate goes from 2 percent to over 5 percent. Across these episodes over the past ninety-five years, the real U.S. equity return (which takes inflation into account) is -7 percent on an annualized basis.

We really care about an inflation surprise. Expected inflation is incorporated in bond yields, so our focus is on unexpected inflation because asset prices respond to unexpected inflation or changes in expected inflation.

In comparing the various assets, we observe that in rising inflation fixed income investments get hammered like equities, and long-duration fixed income instruments do the worst. But other dynamic strategies are interesting to examine.

Given that inflation risk has increased, I think it is incumbent on asset managers to re-examine the positioning of their portfolios. In June 2019 I declared that we had experienced one quarter of yield-curve inversion, and that my indicator had correctly predicted seven out of seven recessions. Because we know recessions are often associated with negative equity returns, I recommended then that investors re-examine their portfolios’ positioning and take appropriate steps.

The investors who took that advice did well. Today, the risk of inflation has increased substantially—the 2020 deficit was \$3.2 trillion; the actual size of the debt grew by more than \$4 trillion because of so-called off-balance-sheet debt. The deficit for 2021 will be more than \$2 trillion. The size of the Fed’s balance sheet is dramatically larger today than it was a year and a month ago (i.e., larger today than it was in February 2020). When we put all this together, it suggests risk. Perhaps the counterargument is that QE and deficits during the Global Financial Crisis were not really inflationary. In the year-over-year break in 2009, inflation did go above 5 percent briefly, but quickly moderated. That is a single observation. The size of the stimulus, the size of the QE, are much larger today. Even if the inflation forecast is 2 to 2.2 percent and is unchanged from a year ago, the risk is higher. Given the probability of higher inflation, it is time to reposition portfolios. ●

## ENDNOTES

1. Eugene F. Fama (1939–) shared the 2013 Nobel Memorial Prize in Economic Sciences with Lars Peter Hansen and Robert Shiller “for their empirical analysis of asset prices.” Fama is the Robert R. McCormick Distinguished Service Professor of Finance at the University of Chicago.
2. Merton Miller (1923–2000), along with two other U.S. economists, shared the 1990 Nobel Memorial Prize in Economic Sciences “for their pioneering work in the theory of financial economics.” Among Miller’s many achievements is the Modigliani and Miller theorem, which he developed with Franco Modigliani while both were professors at the Carnegie Institute of Technology.
3. Lars Peter Hansen (1952–) shared the 2013 Nobel Memorial Prize in Economic Sciences with Eugene F. Fama and Robert Shiller “for their empirical analysis of asset prices.” A leading expert in economic dynamics, Hansen has made fundamental advances in our

understanding of how economic agents cope with changing and risky environments. He is the David Rockefeller Distinguished Service Professor in Economics, Statistics at the University of Chicago's Booth School of Business and is director of the Macro Financial Research Initiative.

4. The Duke CFO Survey offers insights from business leaders on the financial outlook for their firms, the challenges they face, and their expectations for the economy. The Duke CFO Survey panel includes representatives of firms ranging from small operations to Fortune 500 companies across all major industries. Respondents include chief financial officers, owner-operators, vice presidents and directors of finance, accountants, controllers, treasurers, and others with financial decision-making roles. The CFO Survey is one of the most comprehensive and longest-running surveys of financial decision-makers. Started in 1996 by Duke University's Fuqua School of Business, the quarterly survey is now conducted in partnership with the Federal Reserve Banks of Richmond and Atlanta. The partnership, which began in the second quarter of 2020, enables Duke and the Richmond and Atlanta Feds to leverage their collective expertise in survey design and data analysis. See [https://www.richmondfed.org/research/national\\_economy/cfo\\_survey/about\\_the\\_survey](https://www.richmondfed.org/research/national_economy/cfo_survey/about_the_survey).
5. Greenwashing is the process of conveying a false impression or providing misleading information about how a company's products are more environmentally sound. Greenwashing is considered an unsubstantiated claim to deceive consumers into believing that a company's products are environmentally friendly.

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