

My Life in Finance in 12 Questions*

Campbell R. Harvey

Duke University and National Bureau of Economic Research

ABSTRACT

In August 2025, Frank Fabozzi, the editor of the *Journal of Portfolio Management*, interviewed me about my career in finance. His questions focus on my path to academia, how I determine my research topics, and my contributions to academic and practitioner literature. This is the unabridged version of the interview. It is lightly edited for grammar, paragraphing, and style, with content preserved close to verbatim. An abridged version of the interview is forthcoming.

Keywords: Academic careers, asset management, corporate strategy, economic forecasting, bond market, stock prices, risk management, efficient frontier, skewness, performance evaluation, passive investing, active management, AI in finance, rebalancing policies, investment strategies.

JEL: G10, G11, G12, G14, G15, G23, M41

* Version: September 17, 2025.

1. Why did you choose to become an academic rather than a practitioner of finance?

It's an interesting story. I never imagined that I would become an academic. I am the first one in my family with a bachelor's degree. My father made it through grade 10.

After completing my undergraduate degree, I decided to do a master's in business. I thought I would be a practitioner. At the end of my first year of master's studies, I had two summer internship offers. Both were in Toronto, my home base. One was at a major media firm, in corporate strategy. The other was also in corporate strategy at what was then the largest copper mining firm in the world, Falconbridge. I took the job at the copper firm. I was given a singular task: to design a model to forecast U.S. real GDP. At the time, I didn't think it was a big deal. However, reflecting, it was a remarkable task to give to an intern.

The economics of copper are highly procyclical. Given the time that it takes for exploration and development, opening, reopening, and closing mines, etc., the single most important input for planning is a forecast for real GDP. That was my job as a student intern.

There was no way that working alone I could compete with the professional forecasting companies that had dozens of econometricians, great data, and computing power. But I had an idea. There was this researcher at the University of Chicago, Eugene Fama. He had the insight that the stock prices we observe today should contain information about future real GDP growth. It was very straightforward that the earnings of the firm should be influenced by the strength of economic growth and prices today should reflect the expected cash flows. Think of this as the numerator in a valuation. The discount rate for those expected cash flows is the denominator.

The problem was that the stock market was a very unreliable forecaster of economic growth, which Fama detailed in his research. Nevertheless, it was a good idea, and I decided to do something related to the insight that today's asset prices should contain information about future economic growth. I concluded that the reason stock prices were unreliable in forecasting GDP growth had to do with the nature of stocks. Stocks don't have a fixed expiration. The cash flows are very long dated and variable – dividends can change. Further, when we discount the expected cash flows, the denominator is the risk, and shifting risk could lead to false signals.

I decided to look at the bond market. The bond market has many advantages:

1. A fixed maturity.
2. The cash flows are known (coupon and principal).
3. The risk is relatively constant and low.

This led me to look at the difference between a longer-term yield and a short-term yield. The idea there was to subtract out expected inflation. It turned out that the yield curve worked quite well in forecasting real GDP growth and recessions. In my sample, inverted yield curves (short rates higher than long rates), preceded all recessions – without a false signal. I was scheduled to show this to senior management at Falconbridge. But one day, when I arrived at work, they told me I was terminated.

It was obviously a surprise, especially given I was an intern. It turned out my whole group was let go because we were in a recession. It wasn't declared yet, but it was a recession, and the company had to make tough choices.

I was out of work four weeks before classes would restart in the fall. It was impossible to find another internship at that late point. But I was excited about writing a paper based on my finding and worked on it for the remaining weeks of the summer. Then, when I returned for my second year of master's studies, I showed it to faculty, and they said, "You need to apply for a Ph.D."

This was also unexpected. Again, I had not considered that path and had no experience with academia. I didn't even know where to apply. But the one school that was an obvious choice was the University of Chicago, where Eugene Fama was, given he wrote the paper that inspired my research on the yield curve and economic growth.

Looking back, by luck, I had the ideal application to Chicago. I attached my paper, and said, "This is my research, and I want to build on it by pursuing a Ph.D." I was accepted at Chicago, and a number of other schools, but I really wanted to go to Chicago. That's how it started.

My dissertation, on how the inverted yield curve predicts recessions, went on to forecast almost every recession after publication. That's where I caught the research bug. It was during my master's studies that I realized that I could do something that was useful for both corporate planning and investment management. In addition, it was fun.

2. Who were the greatest influences on your career?

The list is long. I came into the finance profession at the ideal time. I've been so lucky to interact with all the key people who laid the foundations of finance. Look at my Ph.D. committee. Eugene Fama is probably the most influential person for me, but Merton Miller was also on the committee, as was Lars Hansen. In my career, I've had lunch with Franco Modigliani, interacted many times with Bill Sharpe. I had numerous conversations with Fischer Black, Bob Merton, Steve Ross, Dick Roll, Doug Breeden, Myron Scholes, Dick Thaler, Rob Engle, Bob Shiller and Harry Markowitz. I even had meetings with Jack Bogle and Myron Gordon. The list is much longer.

But let me tell you about the influence of Harry Markowitz. Obviously, his 1952 paper is foundational for finance, but very few people read it. They know the efficient frontier, but it's important to read that paper, which was written before I was born. It gave me several research ideas.

For example, on page 91, Markowitz makes assumptions on preferences. Those assumptions guarantee that investors only care about expected returns and variance. He explicitly rules out a preference over the third moment, which is skewness. He is very clear about this, saying his results only hold if there is no preference for skewness.

This led me to think about generalizing his model to incorporate a preference for skew. It was a natural extension. Think of it like this: There are many possible portfolios with identical expected returns and identical variances. The investor should prefer the one with the most positive skew. Investors like a big upside and they dislike a big downside. This led to my *Journal of Finance* paper in 2000 with Akhtar Siddique and several papers after that.

Another research idea that Markowitz's 1952 paper inspired was that distributions are stationary. Eugene Fama was also interested in generalizing this assumption. It makes sense that risk premia vary over the business cycle. To get people to invest in the stock market at a trough of a recession, you need to offer a high expected return. Similarly, in good times, the expected return should be lower as investors have extra cash to allocate to risky assets.

The dynamic behavior of risk is not in the original Markowitz paper but just seemed very intuitive to me. Much of my early research showed that we could allow for risk to change through time, to vary with the business cycle and also with expected returns. This dynamic modeling of risk dominated some of my early papers.

A third aspect of Markowitz was influential for me: the idea that the inputs for his quadratic programming of the efficient frontier are assumed to be exactly known. There is no uncertainty. We know precisely the expected returns and the covariances and the variances.

But we know that's just not the case. We see what happens when we vary the assumption on the expected return on one asset. Even if the new expected return is close to the old one, we often get radically different portfolio weights. The Markowitz frontier is very sensitive to the inputs which, again, are assumed to be exactly known.

Many people in the literature proposed ways to deal with that. Certainly, practitioners don't use the straight Markowitz 1952 model. One way to inject uncertainty in the inputs is resampling or bootstrapping. I had another approach. I am a Bayesian at heart, and it seemed that the Bayesian technique had lots of advantages over other methods. I generalized the Markowitz approach to allow for higher moments and for uncertainty in the inputs. I published a technical paper in *Quantitative Finance* in 2010 based on those insights.

There are three research streams that fell out of Markowitz's 1952 paper, and, importantly, Markowitz realized the implications of the assumptions he was making. All these assumptions are clearly spelled out in his paradigm setting paper. I joke that my research epithet might read, "He was a careful reader of Markowitz's footnotes."

3. What motivates your choice of research topics?

It's a difficult question. When you start out in the profession, you struggle to generate important ideas. I remember going to my Ph.D. advisor with some ideas, not for my dissertation but for other papers. I remember his reaction to the first one. He just shook his head. He barely looked up from his desk. I went on to Number 2, and he looked up and said he didn't think it was a big idea. Then the third idea I pitched to him, he was umming and ahing, and he said, "Well, maybe you could get a publication out of this in a lesser-ranked journal."

So, I left. It was not a long meeting, maybe 15 minutes. I went back to talk to my fellow Ph.D. students, and told them what my advisor had said, that all three ideas were no good. "Oh, that's devastating," one said. "You must be super upset that all three of your ideas got shot down."

But I didn't view it like that, and I didn't feel bad at all. Indeed, I had the opposite reaction. My advisor had saved me a huge amount of time and kept me from

pursuing ideas that were not big ideas. More importantly, it was an indirect signal that he thought I could do better. It was energizing to me.

For me, that lesson when I was a student was very impactful. Yes, I could write a paper and get it published in a lesser journal. But publication is not the goal. The goal is to change how people think in the profession and change it in a way that contributes something new and useful. That is very hard to do. What I am looking for is not just to publish a paper. I am looking for big ideas to improve investor outcomes.

Yes, it's true, I have published plenty of small ideas. Often, they are building on a bigger idea and trying to reach a new audience. An example is my work on time-varying risk exposures, which I published in a very top journal. Then I had a number of follow-on papers covering both developed and emerging markets that further pushed the main insights and the technology I developed.

Often, I take a fresh look at a highly researched subject, for example, my work on performance evaluation. This topic has been researched since about 1969. There are hundreds of papers on performance evaluation. I came to the area knowing that it was going to be challenging to offer something new. But I did and published it in the *Review of Financial Studies* with Yan Liu.

The idea is remarkably simple. We hear practitioners say all the time that past performance is not indicative of future performance. We have seen that warning so many times and it is standard compliance practice. But to me, I thought there should be some relation between the past performance and the future performance if the manager is skilled – or unskilled. But if we calculate the risk-adjusted performance of many funds (alphas), we see that there is no predictability for the alphas in the next period. The R^2 is essentially zero. This means that in the cross-section we have no way of identifying future winners and losers based on past performance. But why is that? My idea was that there is considerable noise, and the noise might be impacting the predictability. Is there a way to reduce the noise in the alpha estimation? If we go back to the basic principles and think about estimating alpha for a particular fund, we have the fund's historical returns, and then we choose whatever factor model that will serve as a benchmark. It could be just the market, or it could be the three-factor or five-factor model. It doesn't really matter. A regression model is used to estimate the alphas and betas. The error term, by construction, must average out to exactly zero, and that means that the alpha contains not just the excess performance but also the noise. One hundred percent of the noise goes into the alpha.

Another way of looking at this is that we are choosing the alpha and the beta in this regression so that we maximize the time-series R² for a particular fund. But that is not what we are really interested in. We should be interested in the cross section of funds and figuring out which will outperform and which will underperform in the next period. What is important is the cross-sectional R². The idea in the paper is simple but powerful. We adjust the alpha for noise, and any time we adjust the alpha, we destroy the time series R². But by adjusting this alpha for noise, it increases the predictability for the cross section.

A simple example could be a fund that has an incredible year with a 60% return that is far beyond anything it has ever had. The fund managers explain it: They had one very important bet that worked, and there is no chance that they are going to have 60% return the next year. But looking at the past number, it is 60%. The idea is to take out the noise and adjust that alpha downwards to something more indicative of what could happen in the future. That is exactly what I do in that paper. Once the noise in the measured excess performance is reduced, the past cross section of noise-reduced alphas predicts the next period's cross section, the realized alphas. What is also interesting in that regression, I showed there is very significant predictability: The past does help forecast the future if past performance is noise-adjusted.

But it is also the case – this is research unpursued – that this regression has more than 2,000 observations, which is quite a few. We could include other predictor variables in this cross-sectional regression, for example, fees, the experience of the manager, whether the manager has a private jet or drives a Ferrari, etc. There are various possibilities that I have not pursued yet. To answer the question, I am willing to challenge conventional thinking.

Another paper, “Investment Base Pairs,” with Christian Goulding, questions how we put portfolios together. Often, we sort. We take the top third long, the bottom third short, and ignore the middle or have an alternative linear style rule. Goulding and I reconstruct from the bottom up, looking at all possible pairs of stocks, and propose a method for ranking the attractiveness of every single pair. Researchers often justify their choices by following what was done in the past. For example, they use the Fama-French sorting method and justify it by saying that other researchers use that method. Why not consider other methods?

Part of my motivation is to look at very conventional problems and come up with an unconventional approach. Of course, research is very hard, and we fail more often than we succeed. That’s the nature of all research. But I have always tried to tackle hard problems, knowing that there is a substantial chance that I will fail.

The easy problems – those are the smaller ideas – and I am always interested in bigger ideas.

4. Which of your academic papers has had the greatest practical impact, and why?

I will give you two answers here, even though you are asking for one. I want to mention my most-cited paper, because it has had very substantial impact on the practice. Most people in academic finance don't know about it because it is published in a top accounting journal. The paper has to do with earnings management. This is important for those looking at company fundamentals and coming up with recommendations as to what stocks to include in a portfolio.

This is a bottom-up issue and a very critical one. How earnings management was measured in the past largely focused on fundamental data like accruals. The goal of a company's management is to hit the earnings target, and managers could make some accounting assumptions in order to smooth the earnings so that they hit the target.

I was always skeptical of this story. Indeed, in my board of director's experience, I never saw anything like it. I would ask questions about the accruals, and it just seemed like this was not a first-order issue. This is another feature of my research: I decided to go ask the CFOs. Instead of doing an academic exercise or a practitioner-oriented fundamental analysis exercise, why not just ask CFOs and CEOs how they manage the earnings? I embarked with John Graham, and Shiva Rajgopal on a large-scale project where we interviewed top CFOs and conducted a comprehensive survey.

It turns out that CFOs will not mess with accounting assumptions. If they do and it is detected, they lose their jobs. Instead, they do what is now known as "real" earnings management. If at the end of the quarter, it looks like they are going to miss the consensus forecast, they take "real" actions. For example, they slash advertising in the last week of the quarter, or they delay hiring people or a new capital project, so that they hit the earnings. This is real earnings management. The advantage is that it cannot be questioned. It is a business decision. There is no accounting magic or anything like that.

We asked these CFOs the following question in several different ways, because it is controversial: Do you – and we use this hot word – "destroy" value by making decisions at the end of the quarter to cut back on advertising, hiring, or capital projects to hit your earnings target?

We believed this was a biased question and anticipated a biased (downward) response. Yet 78% of the CFOs admitted destroying value. That was a shocking finding, and it fundamentally changed how people viewed earnings management. The first-order mechanism to hit the target is real earnings management, while accruals have distant relevance. This had very substantial impact, both in academia and with practitioners.

The other paper I mentioned earlier. I generalize the Markowitz frontier to allow for higher moments and, in particular, skewness in portfolio selection. People obviously have a preference for skewness. They like the big upside and dislike the downside. Asset returns are often skewed, so the input is skewed, and the preferences of any portfolio selection model should allow for skewness. It makes sense to put all that together, and that is what I did.

This idea I also pursued in my book, *Strategic Risk Management*, with Sandy Rattray and Otto van Hemert. Risk management should not be an afterthought or delegated to a separate team. Risk management should be holistic with the portfolio formation process. You set your portfolio up so that it has built into it some downside protection. The extreme downside is the negative skewness.

The textbooks in finance – and, by the way, I've never used a textbook in all of my years of teaching – are still in the world of Markowitz's mean and variance optimization. But the practice of finance has moved beyond that. We have many different ways to incorporate downside risk management in portfolio management. There is still a long way to go. It is a journey that's taken me 25 years.

Let me give you a simple example of where we need to go in the practice of finance that is linked to my paper. Many portfolio managers are evaluated in terms of a benchmark, and risk is measured by so-called tracking error. Tracking error is the standard deviation of the difference between the manager's return and the benchmark return. The manager is given some tracking error budget and makes decisions based on that. But if you think about it deeply, it does not make any sense. Since we are looking at the standard deviation of the portfolio return minus the benchmark, it doesn't matter if we are above the benchmark or below it because the square of a negative number is identical to the square of its positive counterpart. That is, in this risk framework, underperformance and outperformance are treated identically. A manager may have a tracking error budget that they exceed, but every single month, they beat the benchmark. To me, again, this is where the skewness idea comes in. What we should do is look at the downside tracking error and the upside tracking error. We want to maximize the upside tracking error as well as outperformance of the benchmark and

minimize the downside tracking error. It is the downside tracking error that is the risk. This is all consistent with my work looking at not just variance but skewness. It is an area where we need to do some extra work.

5. Are there aspects of financial theory that consistently fail to translate into practice?

Any theory is a simplification of the world. We know, given the simplifications, that these theories are incorrect. However, even though a theory might be rejected, it is still potentially useful.

Take Sharpe's capital asset pricing model (CAPM) from 1964. This is a very elegant model that has very strong assumptions and has been incredibly impactful, both in academia and the practice of finance. It has a number of assumptions that we know are not accurate, but again, the outcome of the model is very useful. This model has one major implication, that investors should hold the market portfolio levered up or down depending on their risk preference.

But think about how this model is derived. One assumption is that the prices are always the true prices. This is the same as the efficient market hypothesis in a very, very strong way. While people differ based on risk aversion, they have the same expectations for returns and variances and covariances. There is no disagreement. In economics, we do this all the time with the so-called representative agent. Like a central planner, we model something as if the economy is one person.

Consider the influence of Sharpe's idea, for example, the amount of passive investing in index funds. It is now greater than active investing, as I pointed out in my new paper with Chris Brightman, "Passive Aggressive." If we think about this in a little more detail, it is extremely unlikely that stocks are priced at every point in time at the true price. The true price, of course, we cannot observe. But it just seems extraordinarily unlikely that the market is always exactly right.

Now, I am talking about the U.S. market, which is the most efficient market in the world. What if there is a small deviation from market efficiency? Maybe – and this would be impressive – some stocks are up to 1% overvalued and some stocks up to 1% undervalued. That would be a good outcome but let us assume that that is the case.

Given what a passive index fund does, by construction, it will overweight the overvalued stocks and underweight the undervalued stocks. Rob Arnott has championed this key insight many times. The index fund does not care about the

relative valuation or the fundamentals of a company. It only cares about one thing, the price, because the portfolio weight just depends on market capitalization.

This is where theory translates into practice, but in my opinion, it translates in a problematic way. Index funds and passive investing have so many advantages – very low fees, for one. But I am concerned about the future. I have made calculations based on the trend of passive versus active. If we continue along the path that we have been on for the last 25 years, in 10 years, 80% of investment will be passive. I worry about that because there is a crucial role for active investment. Active investors look at the fundamentals and try to determine if a stock is overvalued or undervalued. If the stock is undervalued, they buy, and in buying, drive the price up to something closer to the true and *unobserved* value.

Active investment is critical because it helps with price discovery and makes the market more efficient. The problem, of course, is that the track record of active investors is not great and that the fees are higher. It is a trade-off. This is an example of a theory (the CAPM) that translates into practice (buy a passive index fund), but people do not question the theory's assumptions, which is always important to do.

The Black-Scholes option pricing model is another case in point. Again, the model's assumptions are highly specialized. For example, volatility is constant. But we know that volatility is not constant. Again, this model had a massive impact, both on academia and the practice of finance, but only later did we introduce models that allowed for time-varying volatility. Derivatives research has moved to relax the assumptions, whereas equity research has yet to arrive there.

Sometimes we are a bit naïve. We take academic ideas and just assume that they are true. Another example of this, which is right along the lines of my current research, has to do with factor exchange-traded funds (ETFs). Somebody publishes a paper detailing a new risk premia or anomaly, and then the intellectual property is available to anybody. An ETF launches a product based on something published in a top scientific journal. The pitch is, “This has gone through peer review. It’s a high hurdle. We’re going to package a product, and it will likely perform the same as the published version.”

That’s problematic in many different dimensions. Whatever was published in the academic journal may be overfit. Choices were made to get the best possible fit, and we know from published research that the out-of-sample performance of academic papers is subject to a very substantial haircut, which is consistent with the overfitting.

Recent research looks at ETF launches, which must provide the SEC with a backtest. These backtests look very good. But excess performance after launch is flat on average. Again, this is consistent with the theme of grabbing academic research and applying it without critically evaluating the assumptions or the research methods.

Some basic assumptions academics make simply do not work in the real world. One of those is no transaction costs – a point emphasized by Cliff Asness and Andrea Frazzini in the *Journal of Portfolio Management*. Almost all the factor papers that have been published assume zero transaction costs. Transaction costs have come down, but the one transaction cost that is hard to mitigate is the impact cost.

A large fund will impact prices by its trading. That is the transaction cost. Popular factors have a long leg and a short leg. Sometimes that short leg includes smaller capitalization stocks. Such shorting is expensive, yet academic papers assume zero transaction costs.

That is a substantial disconnect and one of the reasons why some of these factors, when implemented, fail to outperform after real-world frictions are accounted for.

A provocative paper recently published in the *Journal of Finance* by Theis Ingerslev Jensen, Bryan T. Kelly, and Lasse Heje Pedersen looks at marginally significant factors and their out-of-sample performance. The performance is about the same, which is consistent with no overfitting. But is there a different explanation? It seems obvious to me. Since these factors were marginally significant, they were probably not significant once we take reasonable transaction costs into account. Adding transaction costs will decrease performance. That means the alphas go down. What is marginally significant becomes insignificant. After adding real-world costs, these factors are insignificant in and out-of-sample. We need to pay special attention to the assumptions.

6. What kind of roles have you held outside of academia, in consulting, advisory, or investment committees, for example?

I have been very fortunate and have had many engagements with the practice of finance, both in investment finance as well as corporate finance. I have over 35 years of public and private board experience. This has been very valuable for me

in seeing how companies operate from the inside and the practical problems that we academics often ignore.

Some examples on the corporate finance side: We spend so much time teaching our students how to estimate the cost of capital for a company. We estimate betas, risk premia, and then determine the weighted average cost of capital. Yet in the practice of finance, often other methods are used. In particular, the comparable method is not something we go into in detail in our teaching. Seeing the variety of methods used both by corporations and the very top consultants was a great learning experience for me. Another example is to go through a merger or an acquisition. I have experience like that. It is one thing to be an academic writing about important corporate events; it is another thing to live through it.

On the investment side, again, I have been so lucky to have had such high-quality engagements. I will talk about the two main engagements I have right now. For the past 20 years, I have been an Investment Strategy Advisor at Man Group. Man Group is the largest publicly listed hedge fund group in the world and was run for many years by Emmanuel Roman. It is based in London. Man Group taught me about research culture.

First, let me describe a bad research culture. Suppose two researchers propose two different ideas and both ideas are deemed to be high quality. Both researchers are equally capable, and they do the research and present the results. The first researcher did rigorous work, and everybody agrees the idea was good, but in the end, there was no alpha. The second researcher also had a high-quality idea. The second researcher's idea led to a significant alpha. Ex ante, people could not tell if one idea was better than the other. Both were high-quality researchers, the quantitative work was rigorous, but one idea fails and the other works.

Think of the toxic scenario where management rewards the second researcher and punishes the first. That is very, very bad. If we let that person go or slash their bonus, for the next research task, both researchers will data mine and p-hack like crazy because they know they have to get a result. If they don't, they will suffer the consequences.

Once a week at Man AHL, we have lunchtime seminars in which two researchers have a half hour each to present their ideas and their findings. One of these I recollect very distinctly: Two very high-quality researchers with two very different but both high-quality ideas. The first person presents, goes through the results, and the idea fails to produce alpha. The second person goes through their presentation, also a super interesting idea. In the end, their idea also fails. We sat

there for an hour. After the second researcher finishes, 50 attendees give a strong round of applause. Their managers are nodding, “Good work.”

That is a culture I admire. Research is hard. The expected outcome is a null result. These two researchers did a great job and people realized that their ideas did not work, but they will be encouraged to try again. One of those tries will result in something important.

I have also learned about the difference between how we conduct trading strategy research as academics and as practitioners. Academics can data mine, p-hack, and publish a paper. The paper does not replicate out of sample. So what? Many academics just check the publication box. They have the publication in the top journal, and they move on to something else. They don’t even do the replication. Somebody else does it.

Practitioners have a very different set of incentives. Reputations are on the line. If a new product is data mined, it will not work out of sample. If it does not work out of sample, clients are going to be angry. Then they might redeem funds invested. Further, performance fees are zero. As practitioners, there is strong incentive not to data mine, to be very disciplined at the research stage.

The second engagement I have does not overlap with the first. I have been Director of Research at Research Affiliates in Newport Beach, California, for almost eight years. I am attracted to outside work with a strong research culture that is very careful about how to construct models.

Research Affiliates finds ways to do active investing at very low cost – some people call this smart beta; I prefer to call it “active-lite” – and make small changes that are designed to do better than standard index products. This is something they have done for the last 20 years. Their culture is remarkable. I published a paper with the founder, Rob Arnott, and the late Harry Markowitz. It described how to put the best practices together for research methods in the era of machine learning. The idea is that considerable discipline is needed to avoid overfitting historical tests. If we overfit, then we know the live-trading performance will be poor, which means investors will be disappointed.

This is not the only paper on this theme from Research Affiliates. The goal is repeatable performance. If we see something in the backtest, we want it to be repeated in live trading. Both companies I am associated with have a strong culture of doing everything possible to have repeatable performance to meet investor expectations.

7. How do you manage potential conflicts between academic independence and industry partnerships?

There are always conflicts of interest. The question is: How binding are the conflicts of interest?

Many academics do outside work as expert witnesses. I have the impression sometimes that their view on a particular issue could be substantially influenced by the side that is paying them. That is not how I operate. When somebody approaches me to be an expert witness for a particular side, I need to be convinced that that side is in the right, or I do not accept the work.

This probably explains why I've not done much outside litigation work, because I make this clear up front! Conflicts of interest, I think, in expert witness work, are severe. My reputation is far more important than any money being paid to me.

I have never been in a situation where I was under pressure to deliver a certain result because it was good for the company. Never. If that occurred in the future, then one of two things would happen. Either I would refuse or I would terminate my relationship with that firm. That is the way I operate.

I have worked hard to build a reputation as somebody you can trust to give an unbiased opinion. Let me give you an example. Some people might say this sounds like a conflict of interest, but it is not. We talked about this earlier. The passive index fund, the S&P 500 fund, for example, overweights stocks that are overvalued and underweights stocks that are undervalued. A company I am associated with has a product that tries to undo that. Is that somehow a conflict of interest, because I am pointing out something that I believe is true and there is a product out there trying to give the investor a better experience, and the success of that product obviously influences the profitability of the company?

I come from the school of efficient markets, the University of Chicago, and as a student of Gene Fama, I think I know how this works. We can never exactly measure efficiency, because we don't know what the true price is. It is extremely unlikely that markets are 100% efficient, but it is likely that the U.S. market is relatively more efficient than those of other countries. Efficiency, from the view of many at the University of Chicago, is a relative concept and very useful for testing models and ideas.

If we design a product to capitalize on something that we have written about, that we have research on, and that we believe in, is that a binding conflict of interest? I don't think so.

I will not say anything that I do not believe in scientifically. It is just not worth it, after spending all these years in this field, to build a reputation, to let it go, to put it at risk, by saying something that I don't believe. I cannot speak for other people, but my opinion is probably consistent with others who work in active management, that it is always best to be scientific. We pursue the truth knowing that we can never find the truth, that the goal should be to improve the investor experience.

That doesn't always happen – there are lots of failures – but that's the goal.

8. What is your view on the rise of artificial intelligence (AI) and machine learning in asset management? Is it evolution or hype?

It is interesting that you asked this. In my teaching, one of my courses is on decentralized technologies, and AI is a central focus. I published a paper that used deep learning techniques 25 years ago and had a so-called null result, which means it didn't work.

Why didn't it work? There were two issues. The first is somewhat mitigated today: The neural network didn't have enough layers. We have had significant advances in computing technology and software so that we can do a much better job of deep learning today than 25 years ago.

The second issue was a lack of data. That has not changed. As someone involved in this research for a long time, I believe there will be substantial deployment of AI capabilities in asset management. Much of this will be AIs that help sift through reams of data.

Asset management has systematic and discretionary approaches. But the discretionary is usually quantitative. There will be no going back to the old days of the discretionary manager manually updating a spreadsheet they maintain for a particular company. That is long gone. The discretionary manager today looks at hundreds of different databases simultaneously and tries to come up with the best and worst possible stocks. They need quantitative tools for that. AI is ideal at gathering data for that in a quantitative, systematic way.

Most of what we use in finance and academic finance is monthly data that goes back to 1926 or 1963. That is just not enough to train some of these AI models. Again, I think AI's primary use will be to sort through different types of data and come up with the most salient information for both systematic and discretionary managers. There are some amazing AI applications looking at alternative types of

data, for example, satellite data. There is a massive amount, which is pretty good for the AI and a potentially useful input for trading strategies.

Some might ask whether we can get around the data limitations by looking at high frequency data. There are two problems with that. First, at least today, to run something through an AI model takes time, and with high frequency, we need to move quickly, so parametric models are still preferable. Second, yes, there are more observations if we sample every 10 seconds versus every month, but the signal-to-noise ratio is much lower for high frequency data. It is a trade-off.

Another issue applies to both parametric and AI-based models. We might train or fit the model on one regime but then flip into another regime. This is terrible for a parametric model because our parameters change, and if we don't adjust them, our forecasts will be way off. AI might have a better chance of adapting to a different environment, a new regime, but it is still very imperfect at this time and plagued by a lack of data.

The routine tasks, like coding, are already performed or assisted by AI. Many traditional financial analyst jobs will be usurped by AI. AI will be used on big data applications in finance, for example, as I mentioned, satellite data. The number of cars in the parking lots of certain retail outlets is a good example of that. But the application of AI to traditional trading strategies, that is more of a stretch.

Again, we need the data. A promising database is one that, like earnings calls, people have been mining data for a while. That requires a very large amount of data.

Say we take all the earnings calls for all firms, put them into a large language model (LLM), and train the LLM on everything to do with a company. In the next earnings call, certain unscripted questions are asked, and the CEO answers them. We then have the LLM answer the same questions and look at the variance between the human and the LLM responses. That could be a trading signal. This is an example of what can be done.

I saw an interesting paper recently that used an LLM to replicate the Survey of Professional Forecasters. The LLM does the forecasting by taking on the characteristics of the people who are doing the forecasting. For the Survey of Professional Forecasters, we know the identity of the group of forecasters. This is very interesting and will be a routine part of investment management over the next 10 years.

Do I see it replacing what we do? No, but it will make what we do much more efficient. We will be able to perform many more tasks with the time that we have

and potentially improve models. This is important. But I believe AI is overhyped, that what I call “tech washing” is occurring. This is similar to “greenwashing.” Certain asset managers will tout their machine learning and AI capabilities because, say, their summer intern took a course in machine learning.

That’s not good enough. If we are going to do AI and machine learning at the highest level, we need to have a team that is trained in AI and machine learning. A master’s course in quantitative finance is just not good enough.

9. How does your industry experience shape how you teach finance?

I teach an advanced elective course called “Global Asset Allocation and Stock Selection” that is heavily biased towards quantitative investment management research – including many of my contributions. Much of the course is about challenging the students to unlearn the concepts that they have been taught in previous finance courses.

For example, my students learn how to use mean variance or Markowitz optimization. I show them that straight Markowitz optimization is an exercise in overfitting. That is just not the way it is done in industry. I give them the tools to do that. We also revisit the traditional asset pricing models, CAPM, the Fama-French three-factor model. I challenge students to think about what these models mean and how to use them in practice. To use a model in practice, we need to understand what the model can and cannot do.

So, for example, if we apply a multi-factor model for performance evaluation, we are effectively constructing a benchmark portfolio, a portfolio with fixed factor weights determined by the estimated betas. That is the benchmark portfolio to evaluate a manager. Compare that to the manager’s portfolio, and the difference is the alpha. But that benchmark portfolio needs to be fair, because we are looking at the manager, and the manager is providing returns that are after all trading costs and all fees. Our benchmark needs to do the same thing.

I talk about many different frictions that academic finance assumes away and then try to give my students a real-world experience. Earlier in the year, I asked a chief investment officer (CIO) to come in and pitch the class on a new product that was not yet launched and had not yet been pitched to anyone outside the CIO’s company.

My students received the pitch deck 24 hours before class and came up with some due diligence questions. They sent their questions to me shortly before class, and I selected a subset among them and recognized the students during the pitch.

That is how the class works. The CIO is making the pitch, I interrupt and say a student has a specific question on this slide.

It was a magical experience for the students. I said to them, “This is as close as it gets. When you graduate, you’re going to be sitting listening to some other CIO or portfolio manager making a pitch, and you’re going to be asking the same kind of questions that you were asking today.”

My students were very grateful for it. I did not quite expect the high quality of the questions. I had one student with a physics background who asked incredibly challenging questions about one of the models used by the CIO. I did not anticipate the CIO’s reaction. The CIO got something out of the exercise – this was far more than the CIO doing educational service. Many of the student questions could have been asked by potential investors. It led the CIO to revise and improve the presentation. It was good for the company and the CIO, and it was good for the students.

My course is practically oriented, and I bring practitioners into the class. I brought in the CIO, a CEO of a hedge fund who challenged the students to think about geopolitical risk, and a managing director from a leading firm to talk about the high valuation of the stock market, including the mechanics and the implicit assumptions. They were amazing learning experiences for my class. All three were great. In other parts of my course, I take the opportunity to deliver some recent keynote addresses from conferences I have been invited to.

Of course, I am always talking about research. I like to focus on material that is not even published, new ideas that will be published in the next five years, so when my students graduate, they have an edge over students from other schools. Indeed, many students from other schools are taught by following a textbook. That is all old, very old. I want my students to have something their competitors do not have – the latest ideas.

There is also a research project in my course, so the students have the chance to do something new. All projects need to be approved by me, but I am very lenient in what I allow the students to pursue. This gives them a hands-on experience.

10. What do you hope your students take away when you discuss real-world cases?

To be honest, I am not a big fan of the case method, but I have a very large library of cases, many of which are published, mainly focused on emerging markets finance. That was a course I taught for many years.

In contrast to most case approaches, we avoid pre-packaged cases. When my students prepare a case, they create a new case themselves under my supervision and develop the solution as well as the presentation materials.

What I want my students to take away from a real-world case – and we often do live cases, or business situations that are unfolding in real time – has two dimensions. First, there are certain principles of finance that we can apply generally. But there are limitations in applying our models. The model might not be appropriate for the particular situation described in the case. That is the top insight, knowing when or when not to apply a model. The second is the realization that this time is always different.

There may be similarities, but we should not overemphasize them. Examine the situation and realize that it is different. I am critical of the traditional case method in which students learn cases without having the analytical models to interpret the case setting. Students are taught to apply the lessons of one case to another case. Since the situations are rarely the same, that could lead to bad decisions.

I do prefer doing live cases in my class when there is a decision that could be made. One example of a live case from my emerging markets corporate finance course.

One group of students wrote a case on the potential expansion of the Panama Canal. At the time, this was not on anyone's agenda. It was a hypothetical idea. They did a detailed analysis, the project evaluation, and in the end advocated for expanding the canal's capacity. It was great because it was a situation that had not occurred, but they made a credible case as if they were pitching the Panama Canal Authority (ACP) rather than the class. They did a very nice job.

I take all my cases and I publish them on the internet or in books. Everything is public. Many years after that case, I received a call from an unusual area code. I picked up, and it was the head of the ACP. "We saw your case on the expansion of the canal," he said. "We were impressed by the analysis, and it turns out that this is something that we are considering in real time."

My students were prescient, and the head of the ACP asked, "Can you come down to Panama and talk to us and go through our analysis, which closely follows your students'?"

I thought, "Wow, that is great." I would not usually do a trip like this, but I could not resist the experience. This was an academia having an influence over important public policy decisions.

I went down to Panama and met with various people involved in the analysis and the decision making. The expansion turned out to be politically controversial – something that was not detailed in the case. But I was able to navigate through and offer some help in terms of their analysis. In the end, they went ahead with the expansion project.

11. How do you prepare students for a world where passive investing is dominant?

As I mentioned earlier, most equity investing – 53% – is passive today. If we extrapolate over the last 25 years, passive's growth is very steady. This is not controversial. If it continues on its current track, 80% of investment will be passive in 10 years.

How do I prepare my students for this? It is very important for them to understand the costs of passive investing. We already talked about one cost – the lack of price discovery. Passive only cares about market capitalization and ignores all fundamental information. I gave the example of a potential 1% misvaluation and the drag that would induce on a cap-weighted portfolio. However, the misvaluation does not necessarily need to be small.

Strategy (MSTR) is a case in point. Strategy's main asset is over 600,000 bitcoins, so we can immediately determine the company's fundamental value or net asset value (NAV) directly by marking those holdings to market and subtracting liabilities. It is a straightforward calculation. Strategy stock, at the time of this conversation, is 63% more expensive than its NAV. That is not a 1% misvaluation. It is a 63% misvaluation. Indeed, the misvaluation is featured on Strategy's website dashboard under the name "mNAV," or multiple of net asset value. Strategy is no microcap. With a nearly \$100-billion market capitalization, Strategy would be a prime candidate for inclusion on the S&P 500. While Strategy was recently excluded from the addition list, 80% of S&P 500 stocks have less than \$100 billion in market cap and 5% have less than \$10 billion.

Does the passive investor care? No, what they care about is the stock price. That stock price could be 63% overvalued, it could be exactly at its NAV, or it could be undervalued. The passive investor just buys at the stock price. This is an example of a serious misvaluation, and passive totally misses it.

Passive has other issues. In addition to price discovery, passive is also associated with decreased liquidity, according to researchers. Decreased liquidity is bad and makes trading the stock more expensive.

When flows come in, passive funds just buy the market. This increases correlation between stocks. It works the same way on the downside. If there are redemptions, everything is dumped simultaneously. Again, this increases correlations. Passive leads to higher correlations among stocks. This decreases diversification for investors – which is bad.

There are many other issues. Firms that are competing against each other have common owners. These common owners might not want Company 1 doing something that might hurt Company 2, their rival. This undermines the competitive process.

I detail the downsides of passive investing in my recent paper. I also make it very clear that active has not done a particularly good job of outperforming. The goal of my course is to prepare students to do active investing in a way that maximizes their chances of doing well as an active investor. On average active investing has underperformed. I hope my students are better than average.

12. What are you currently working on that you are most excited about?

I am excited about all the projects I am working on. Let me give you the top four that are particularly energizing.

I have an unreleased paper that deals with an issue that is very controversial in finance: the thresholds that we use for declaring something significant. Think of trading strategies – in statistics, we use a two-sigma rule that the t-stat needs to be above 2. However, if we try many different strategies, just by random chance, something will work. It is important to control for multiple testing.

This is based on two papers that I published in the *Journal of Portfolio Management* – “Backtesting” and “Evaluating Trading Strategies” with Yan Liu – as well as my *Review of Financial Studies* paper, “... and the Cross Section of Expected Returns,” with Liu and Heqing Zhu. In those papers, we give rule-of-thumb advice. The new paper focuses on setting a threshold and goes into much more detail. It is co-authored with two statisticians, Alessio Sancetta and Yuqian Zhao, and makes the case that we need to substantially increase the thresholds used in conventional practice. It is called “What Threshold Should be Applied to Statistical Tests in Financial Economics?” If we do not make adjustments, too many false discoveries will likely negatively impact investor outcomes.

This is controversial in finance right now because many researchers are pushing the idea that we can use the two-sigma rule, that we do not need to worry about

hundreds of trading strategies, anomalies, and factors that have been tried. That just contradicts my intuition, and this paper explores this question in a statistically rigorous way.

The second paper is co-authored with Christian Goulding and Hrvoje Kurtović and is called “The Disagreement of Disagreement.” The idea is that many papers look at disagreement and relate disagreement to various phenomena. However, there are many different measures of disagreement. Disagreement as a standard deviation of earnings forecasts is one measure, short interest is another, and idiosyncratic volatility yet another. But what is the correlation of these different measures?

That is the first step in our paper. It turns out the correlation is small, about 0.1. What does that mean? A paper with one measure of disagreement that says disagreement is associated with some finding, well, if we drop in another measure of disagreement, the finding might not be robust.

The paper is entitled “The Disagreement of Disagreement” because disagreement measures just do not correlate that much. We also provide a theoretical contribution that creates a composite measure of disagreement using the components of disagreement, for example, the dispersion of analyst forecasts. The dispersion of analyst forecasts cannot be the whole story because we observe those forecasts. Participants in the market take them into account. That does not necessarily reflect their disagreement – it is just one piece of information that market participants use. We put it all together and come up with this composite that looks extremely interesting. The paper has been heavily downloaded, and it seems like a contribution on disagreement, which is something that people don’t focus on that much.

We often talk about forecasts. For example, many follow the Survey of Professional Forecasters. But we rarely talk about their disagreement. I think that is something that makes some real contributions.

I have another paper with Christian Goulding called “Investment Base Pairs.” The title is based on genetics. Those who study genetics know that there are many pairs in our DNA, and 90% of the pairs are what they call “junk.” Junk means these pairs have no known usefulness in human quality of life and survival: 10% are important and 90% are useless.

We challenge the way that portfolios, trading strategies, and factors are constructed. With the usual factor, if we follow the Fama-French approach, we are long the top 30%; short the bottom 30%; and stuff in the middle is ignored. We look at all possible pairs. The analogy is to a Fama-French portfolio. Let’s say

we have deciles, and we would be long the top three deciles and short the bottom three deciles. We can always represent that as a series of pairs. For example, long 1, short 10; long 2, short 9; long 3, short 8. That portfolio is identical to the Fama-French portfolio.

We can do much more. At the top and the bottom 30%, we identify the best pairs. We have a signal. Think of the pair as Asset A and Asset B. It's just a pair. It could be gold and silver, and we determine whether gold is long or short, depending on circumstances. There is a signal for gold and a signal for silver. One attractive feature is that the signal for gold predicts the gold return, and the signal for silver predicts the silver return.

This is obvious, and we call this the own-asset predictability that makes the pair attractive. But there's more. We want the signals for gold and silver to be uncorrelated.

Suppose a signal for gold and a signal for silver are highly correlated. That means if there is a strong signal for gold leading to a positive return, then there will likely be a very strong signal for silver leading to a positive return. The spread between the two – because one is long and one is short – is going to be tiny. We want the opposite, a negative correlation. The lower the correlation of the signals, the more attractive it is. I am describing just two of the five components of what a good pair looks like according to our model. At every point in time, we look at the various pairs and figure out what the best ones are.

That is how we reconstruct portfolios from the bottom up. I recommend people read the paper. The results are striking. We use four standard asset classes and three standard signals – standard value, carry, momentum – nothing special.

We show the traditional way of doing long-short produces mediocre performance for almost everything in our sample. The assets that we look at are, not surprisingly, equity, fixed income, FX, and commodities. But when we use our base pairs method, throw out 90% of the junk pairs, and just focus on the top 10%, these traditional signals look pretty good.

This is a methodological contribution and is independent of the assets used, the signals, and the investment time horizon. It is a method through which we can take the signals and develop a trading strategy. I am very excited about it.

The last paper is a little different, but there are some linkages. It is called “The Unintended Consequences of Rebalancing,” with Michele Mazzoleni and Alessandro Melone. Many large pensions and target date funds (TDFs) regularly rebalance, and we know what their rebalancing rules are. Look at the U.S. We

know the U.S. is the most efficient market in the world. But these funds are giant, and even though the U.S. is the most liquid and efficient market, there could be market impact when pensions conduct massive, complex rebalancing trades. We detail that impact.

Think of it this way: A giant pension has a very public rebalancing strategy. The pension drifts out of sync with its target allocations and needs to rebalance. Maybe it has fallen below the equity target. That means it must buy, and everybody knows that it has to buy. What happens then? Some investors will front run the trade because they know it will have market impact. To get ahead of the pension rebalancing, they buy first. Then the pension comes in and buys. Then they sell. It is front-running but not illegal front-running. They are using public information.

It is never wise to pre-announce a large trade to the public – but this happens all the time. We looked and the results were surprisingly strong. Consistent with the idea of working with practitioners of finance in my research, we convened a roundtable of 16 senior people from prominent pensions, and showed them the preliminary results and said, “There is market impact from your rebalancing. It seems like you are being front run.” I did not know what to expect. They could have said, “Well, we don’t think that’s possible. This is news to us. Maybe there’s a problem with your results.”

But that was not the response. The response was, “We know about this, and we know it’s a problem.” That validated our research findings. We found something that they know about but that had not been documented. I said, “Well, why don’t you change your rebalancing policy to avoid this front-running?” They said, “Well, it is easier said than done.”

Formal rebalancing policies were the big impediment. Going through the investment committees is a long bureaucratic task, and they were not sure they wanted to take that risk. One of the pension representatives said, “I agree with that, and that’s why we send the signal to our alpha desk.” I paused and then responded, “Let me just make sure I heard what you said. Instead of changing the rebalancing policy, your alpha desk is using the signal and front-running your own rebalancing as well as your colleagues’ rebalancing?”

The CIO said, “That is correct.” It seems highly dysfunctional to me. The size of the effect is large, conservatively estimated in our paper at \$16 billion a year. For the average pensioner, that adds up to a lost year of contributions, which is economically important.

I hope this research spurs pensions to re-examine their rebalancing policies and improve them. This is money right out of the pockets of pensioners. This is a good place to end, because it brings us full circle to the beginning of the conversation.

We have known about rebalancing for as long as we have had finance. Without rebalancing, a 60/40 portfolio becomes 70/30, 80/20, and soon 95/5. We need to rebalance for diversification. How to do it is such a basic issue that is not addressed. That is consistent with my research philosophy. I choose topics that sometimes challenge the practice, that are fairly simple. This is not complex. There is no machine learning, there is no AI, but there are important economic implications and a large amount of money being lost every year. We can do better.

References

- Asness, Clifford, and Andrea Frazzini. 2013. “The Devil in HML’s Details.” *Journal of Portfolio Management* 39: 49–68.
- Arnott, Robert D., Jason Hsu, and Philip Moore. 2005. “Fundamental Indexation.” 61 (2): 83–99. <https://doi.org/10.2469/faj.v61.n2.2718>
- Arnott, Robert D., Campbell R. Harvey, and Harry Markowitz. 2019. “A Backtesting Protocol in the Era of Machine Learning.” *The Journal of Financial Data Science* 1 (1): 64–74, Winter. <https://www.pm-research.com/content/ijjfds/1/1/64>
- Black, Fischer, and Myron Scholes. 1973. “The Pricing of Options and Corporate Liabilities.” *Journal of Political Economy* 81 (3): 637–654. <https://www.jstor.org/stable/1831029>
- Brightman, Chris, and Campbell R. Harvey. 2025. “Passive Aggressive: The Risks of Passive Investing Dominance.” SSRN, July 8. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5259427
- Fama, Eugene F., and Kenneth R. French. 1993. “Common Risk Factors in the Returns on Stocks and Bonds.” *Journal of Financial Economics* 33 (1): 3–56. <https://www.sciencedirect.com/science/article/abs/pii/0304405X93900235>
- Goulding, Christian L., and Campbell R. Harvey. 2025. “Investment Base Pairs.” Working Paper. SSRN, March 25. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5193565
- Goulding, Christian L., Campbell R. Harvey, and Hrvoje Kurtović. 2024. “The Disagreement of Disagreement.” Working Paper. SSRN, September 26. xal. 2005. “The Economic Implications of Corporate Financial Reporting.” *Journal of Accounting and Economics* 40 (1–3): 3–73. <https://www.sciencedirect.com/science/article/abs/pii/S0165410105000571>
- Hansen, Anne Lungaard, John J. Horton, Sophia Kazinnik, Daniela Puzzelo, and Ali Zarifhonarvar. 2024. “Simulating the Survey of Professional Forecasters.” SSRN, December 21. https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5066286
- Harvey, Campbell R. and Yan Liu. 2014. “Evaluating Trading Strategies.” *Journal of Portfolio Management* 40 (5): 108–118. <https://www.pm-research.com/content/ijpormgmt/40/5/108>

Harvey, Campbell R., and Yan Liu. 2015. "Backtesting." *Journal of Portfolio Management* 42 (1): Fall, 13–28. <https://www.pm-research.com/content/ijpormgmt/42/1/13>

Harvey, Campbell R. and Yan Liu, 2018. "Detecting Repeatable Performance". *Review of Financial Studies*, 31 (7): 2499–2552. <https://doi.org/10.1093/rfs/hhy014>

Harvey, Campbell R., Michele G. Mazzoleni, and Alessandro Melone. 2025. "The Unintended Consequences of Rebalancing." Working Paper. SSRN, February 3.
https://papers.ssrn.com/sol3/papers.cfm?abstract_id=5122748

Harvey, Campbell R., Sandy Rattray, and Otto van Hemert. *Strategic Risk Management: Designing Portfolios and Managing Risk*. Hoboken, N.J.: Wiley, 2021.
<https://www.wiley.com/en-us/Strategic+Risk+Management%3A+Designing+Portfolios+and+Managing+Risk-p-9781119773917>

Harvey, Campbell R., and Akhtar Siddique. 2000. "Conditional Skewness in Asset Pricing Tests." *Journal of Finance* 55 (3): 1263–1295. <https://onlinelibrary.wiley.com/doi/10.1111/0022-1082.00247>

Jensen, Theis Ingerslev, Bryan Kelly, and Lasse Heje Pedersen. 2023. "Is There a Replication Crisis in Finance?" *Journal of Finance* 78: 2465–2518.
<https://onlinelibrary.wiley.com/doi/full/10.1111/jofi.13249>

Markowitz, Harry. 1952. "Portfolio Selection." *Journal of Finance* 7 (1): 77–91.
<https://www.jstor.org/stable/2975974>

Sharpe, William F. 1964. "Capital Asset Prices: A Theory of Market Equilibrium under Conditions of Risk." *Journal of Finance* 19 (33): 425–442.
<https://onlinelibrary.wiley.com/doi/10.1111/j.1540-6261.1964.tb02865.x>