# PERSPECTIVES

# Past, Present, and Future Financial Thinking

## William F. Sharpe and Robert Litterman

*At the 67th CFA Institute Annual Conference, held 4–7 May 2014 in Seattle, Robert Litterman interviewed William F. Sharpe to elicit his perspective on a number of investment issues, including the capital asset pricing model, asset allocation, behavioral finance, and retirement income.*

**Litterman:** One of the best opportunities for me in my role working with CFA Institute is the opportunity to participate at conferences. This is the third year in a row that I've had the distinct pleasure of interviewing a Nobel Laureate. Joining me today is Bill Sharpe. Bill, perhaps you could start by discussing the intellectual environment that caused you to do the research that led to the capital asset pricing model (CAPM).

**Sharpe:** I was working at the RAND Corporation, which was an absolutely fabulous place, intellectually, and I was also working full-time on my PhD at the University of California, Los Angeles (UCLA), which RAND made very easy to do. At UCLA, I was influenced primarily by two key people: Armen Alchian of the economics department and Fred Weston of the business school. RAND was also a major influence because it was a pioneer in linear programming and operations research and had serious computing power.

**Litterman:** Was Harry Markowitz at RAND at the same time?

**Sharpe:** Harry came after I had been there a while. I won't bore you with the story, but I had started another dissertation, and I thought I was halfway finished when the chap at UCLA whose work I was extending said he didn't believe there was a dissertation there. Then Fred Weston said, "Well, Harry Markowitz just came to RAND. You like his work. Why don't you go talk to Harry?"

**Litterman:** Correct me if I'm wrong, but many groundbreaking papers were rejected when first submitted to journals, and I believe that was also true with the CAPM paper (Sharpe 1964).

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**Sharpe:** It certainly was. I submitted it in early 1962, and it was promptly rejected. The referee said that the CAPM makes "unrealistic assumptions," but I was taught that all models make unrealistic assumptions. It did not seem fair, so I appealed the decision. It was finally published in 1964, when there were some different referees and a different editor. Later, I learned the identity of the referee who had suggested rejection—one of the nicest people ever. He was at UCLA, and we remained good friends after all this was revealed.

**Litterman:** Did you have any clue at the time how important, how seminal, this work was?

**Sharpe:** Yes and no. I didn't know how good it was, but I did know—and I believe time has proven me correct—that it was undoubtedly the best paper I was ever going to write.

And I remember sitting by the phone, waiting for a phone call, and nothing happened. I later discovered that in that era, people didn't read finance journals right away; they often waited a year or two. So, it was quite a while before the paper began to attract attention.

**Litterman:** Throughout your career, your development has focused on very practical issues associated with asset allocation, style attribution, the Sharpe ratio, and so on. Could you talk about that?

**Sharpe:** The phrase I like to employ is "useful theory." I have always worked with that goal, and I believe it came in large part from Armen Alchian and Fred Weston, as I mentioned, and, of course, from what was going on at RAND early in my career.

**Litterman:** Your research has been very practical. I would also say that it seems to focus very much on using computers, even in the earliest days. I saw in your curriculum vitae something about having written a book on BASIC (beginner's all-purpose symbolic instruction code) programming around the time when you were developing

the CAPM. You also wrote something about airport transport or something like that.

**Sharpe:** Yes, the airport cargo loading paper came from a RAND project. Those were the early days of OR (operations research), linear programming, computers, and computer programming. I love to write computer programs. My wife will tell you that I probably do so at least every other day—because I enjoy it. It's a consumer good for me. But in that era, I was in the midst of people who were designing computer programming languages and writing compilers and interpreters.

When I got to the University of Washington, I believed that all people should be programming and that students should learn about programming. John Kemeny had just developed a language at Dartmouth called "BASIC," and I thought it was perfect. However, there was neither a compiler nor a book available. So, not knowing any better, I decided to write a book (Sharpe 1967), teach a course, and write a compiler so that the students could actually run programs. I wrote an interpretive compiler for BASIC, and since then, I've done a lot of programming.

By the way, I believe that about the only way you can get your arms around a lot of difficult issues, including many with which financial analysts deal, is by using Monte Carlo analysis programs—in many cases, generating 100,000 or 1 million scenarios.

My theoretical abilities, in terms of mathematics, are actually remarkably limited. I stopped my formal math education after one course in calculus, so I rely on computers more than many of my colleagues do, in part because I can only do so much analytically. But using computers is a very powerful and useful way to approach a number of problems.

**Litterman:** Let's talk about the lessons from the capital asset pricing model. I would think that the core message is something about how covariance matters more than, say, risk or variance. And how have those lessons been useful throughout the years?

**Sharpe:** That covariance matters more than variance comes from Markowitz's (1952) portfolio theory. Harry developed an analysis that showed how you should use probabilistic predictions about the future to build useful, efficient portfolios. He also showed how the type of efficient portfolio that you should create depends on the risk tolerance of your client. I did what any microeconomist would do and asked, What if everybody did what Harry said and then came to market and traded with one another? What could you say about prices in that kind of equilibrium setting? That's where the capital asset pricing model came from.

I think the big message is very simple, perhaps embarrassingly so: The market portfolio is a really cool thing. A market portfolio is sort of your "piece of everything." If you have $x$% of the money, you buy $x$% of the shares of every company and $x$% of all the bonds that are issued. This portfolio has very attractive properties. For the average investor, the market portfolio is, in some sense, the best portfolio.

The second conclusion has to do with covariance. The general version is that securities and portfolios that are going to do badly in bad times should offer a higher expected return than securities and portfolios that are not going to do as badly in bad times. In the capital asset pricing model, which makes a particular set of assumptions, the measure of doing badly in bad times is beta, which is a scaled version of the covariance of the return on the security (or the portfolio) with the market portfolio. With more general models, such as pricing kernel models—which, by the way, I now advocate—it is a slightly different measure but is still related to the relationship with the market in the same qualitative way.

Another argument, which I made in an *FAJ* article (Sharpe 1972), was to forget equilibrium theory and understand that the arithmetic reveals that the average investor has to hold a market portfolio and that after costs, the average dollar invested in low-cost market index funds must outperform the average dollar invested in actively managed funds.

**Litterman:** You mentioned that early on, one referee believed you were making unrealistic assumptions, and over the years, a lot of these assumptions have been relaxed. In fact, you wrote an *FAJ* article (Sharpe 2007a) about more general utility and asset allocation in that context. What do you believe are the important developments since the original capital asset pricing model?

**Sharpe:** On the theoretical side, the first development, which was consistent with the referee's comments, was the realization that the assumption that everybody agrees on the probability distribution of all future cash flows for every security is too strong an assumption. Obviously, people do not agree; otherwise, there would be very little trading and many investment practitioners would be out of work. Over the years, using Monte Carlo simulation and other analyses, we've discovered that people can disagree quite radically and that as long as there isn't a bias allowing the majority of people to overestimate or underestimate prospects, the results are very similar to those of the CAPM or broader equilibrium models.

Fischer Black (1972) examined the case where people cannot borrow at the riskless rate, relaxing another assumption that I made in the early model. And then a whole raft of new asset pricing models emerged, of which there are now many.

So, there's the whole theoretical progression, but there's also the view that the real world does not seem to be very kind to most of our theories, including asset pricing theories. So, what do you do?

I did some empirical work in my youth that I recently reviewed when I was asked to compile a book of my selected works (Sharpe 2012). This meant I had to go through the pain of reading my previous work, including one paper that examined 34 mutual funds using only annual returns. It was published in a reputable journal. Can you believe that? That's what it was like early in my career; it was a very different time.

Part of the problem with empirical work is this: In a market in which everyone is trying to make money and beat the other participants, if some strategy were to achieve this, everybody would be searching for it. When it was discovered, many investors would try to exploit it, thereby changing prices such that it would not work in the future. As I wrote long ago, a successful system to beat the market carries the seeds of its own destruction. It's a moving target. When testing a theory with empirical data, you should be delighted if there's even a shred of suggestion that something in the theory might be right and you should not, I would argue, become too fixated on the fact that something could have allowed you to beat the market—unless you can tell a convincing story of why it should continue to work even after its historic performance is known.

I think it is important to always consider "macroconsistency," a term I believe only I use. As I have said, simple arithmetic shows that after costs, the average actively managed dollar must underperform the average low-cost index fund dollar. That's just arithmetic. So beware of claims that a non-market index strategy is superior for any and all investors.

To sum up, although empirical work is central to academic research and to helping us evaluate theories, one must be cautious when evaluating research that assumes that historical frequencies of events reflect investors' estimates of probabilities before the fact.

**Litterman:** Has behavioral finance research changed your thinking in any significant way?

**Sharpe:** Yes. It may surprise some people, but I have been a fan of behavioral research for a long time. Paul Slovic did some absolutely fascinating experiments before behavioral economics, let alone

behavioral finance, existed. I think we ignore all that work at our peril.

One of the things I tried to do in the last book I published (Sharpe 2007b) was to use computer simulation to look at equilibria, where people had utility functions that aligned with Kahneman and Tversky's (1979) prospect theory—distinctive utility functions in which, for example, a dollar lost was twice as bad as a dollar gained was good. I was pleased to see that if there aren't too many of those people—or even if there are, if their "kinks" aren't at the same point—you can get equilibria that are not too much unlike those of the capital asset pricing model.

Today, I worry about people trying to figure out how to finance their lives after they have retired. One problem that bothers me is that I don't really trust the old, simple assumption about people's preferences. To be technical, the assumption is that they have smooth utility functions—one for each future year—and you can use those to decide whether a retirement income strategy is efficient or inefficient. I am perplexed about how to really focus on this issue and to provide appropriate advice to clients. Consequently, I believe it is important to pay a lot of attention to behavioral research.

The other side of the argument is that, as someone once said, behavioral economics is the study of the preferences of undergraduates in psychology courses who are playing for small stakes. Although I believe there's a lot of foolish material in the behavioral literature, there is also some really fine research, which we need to take into consideration.

**Litterman:** You bring up the retirement issue, which you've done a lot of writing and thinking about. What are the main issues facing pension funds today—particularly, public pension funds that are underfunded? Do you have any advice or thoughts on these issues?

**Sharpe:** Unfortunately, I thought I was done with that research. I wrote papers and worked with some pension funds in the 1980s. After ERISA came out, I taught a student seminar class at Stanford University. I believed I was finished with it, but it keeps coming back to haunt me.

I won't bore you with the details, but I was asked to lead a group to help advise the City of Carmel-by-the-Sea, California, on its pension situation with CalPERS, the state pension plan. I am also involved in a group at Stanford that is looking into the problem.

Are public pensions a problem? You bet. Is this a disaster? You bet. The true liabilities of the public pensions in the United States—by which I mean governmental pensions—are, according to the actuaries, much larger than the assets. Using

any sensible economic view of the value of those liabilities, the difference in value is astronomical. It's a crisis of epic proportions. Let me describe this more clearly.

If the state has promised a worker certain payments in the future for having worked at least up to this date—so-called accrued benefits—and it is certain that those payments are going to be made, anybody, any economist, and probably most of you in this room would ask, how do you value that? It's simple. You find US Treasury securities that would provide cash flows to match those payments. That is how you should value the liability.

As most of you know, that is not what the Governmental Accounting Standards Board and the state and local systems do. They value those liabilities at 7.5% or 8% on the grounds that they are pretty sure they'll earn that in the long run. This is crazy. It gets even worse. Because they want to minimize the reported value of the liabilities, they want to use a high discount rate, and in order to justify it, they have to build really risky portfolios. Consequently, they believe that one of the great things to do is put money in private equity, or maybe a hedge fund, because then they can assume an extra 300 or 400 bps of expected return for an illiquidity premium (or just because hedge fund managers are so smart).

So, the tail wags the dog. Idiotic accounting drives even worse investment decisions. This is the classic case of an organization that borrowed money while issuing purportedly guaranteed payments and then used the money to invest in risky securities. Where have we recently heard that this is not a good thing?

Of course, you can point to the politics to see why politicians might give benefits that are very large to employees, especially those who may be able to influence elections in various ways. By making sure the benefits are mostly in the future, politicians can pretend that they cost a lot less than they're going to cost. It's a very bad situation.

In the private sector, we have a shift from defined benefit plans to defined contribution plans, where the problem doesn't exist, although there are other problems with defined contribution plans. The accounting for the remaining defined benefit plans in the private sector is somewhat better than that for the governmental plans, but it has recently gotten a little worse. The federal government decided it was acceptable to value liabilities at a long-term average of corporate rates rather than at the most current rate. So that's also a problem, but it's nowhere near as serious as the mis-valuation of state and local pension liabilities.

**Litterman:** You alluded to the financial crisis. Would you like to comment on the implications of it—what caused it and what we should think about in terms of our portfolios going forward?

**Sharpe:** I think those are questions for experts on banking and investment banking, which I'm not. But I can tell you that I'm inclined to agree with my colleague Anat Admati. You may have read about some of her work, heard her testimony, or seen her in action. For decades, she and many others have advocated much higher capital reserves and higher capital requirements: not 5% or 8%, but 15%–20% or more. There is a whole set of economic arguments as to why such requirements would at least reduce, if not eliminate, the dangers of runs and crises of the sort that we experienced from 2007 to 2009. Further, if such requirements would be a cost to the banks, it's mainly because they would take away the "free put" banks get from the government when things go bad. Admati argues—and I certainly agree—that we shouldn't be providing that put to the banks for free.

**Litterman:** One of the consequences of the financial crisis has been very low interest rates and quantitative easing, which now seems to be tapering off. Many investors view the current environment as one in which it's difficult to achieve significant returns. How would you characterize the environment, and what are the implications for investors?

**Sharpe:** I worry about that. In my research, I've turned to the problem of what people do when they reach retirement. How do you provide retirement income for somebody with Social Security and some savings, perhaps from an IRA or a 401(k) plan? This problem was dire to begin with, but it's much direr with low real rates.

Low nominal rates are one thing, but we continue to have real rates that, unless you go far out on the maturity spectrum, are negative for Treasury Inflation-Protected Securities (TIPS). That is a real shift of wealth to borrowers from lenders—in particular, from investors who are investing for their retirement, both in the accumulation and decumulation phases.

It can be infuriating how seldom the press brings up this issue. They say, "Aren't low interest rates great? You can get a cheap mortgage." But if you're on the other side, trying to save money, low rates are not great.

**Litterman:** You have written a lot about asset allocation: integrated asset allocation, dynamic asset allocation, and, most recently, adaptive asset allocation (Sharpe 2010). Can you talk about asset allocation and the implications for investors today?

**Sharpe:** As many of you know, I always approach any investment decision as follows: If *x* is a good thing for almost everyone, then what happens if everybody does it? Or what happens if the people for whom it's a good thing do it? Will markets clear? Is a strategy consistent with what everybody else wants to do—that is, macroconsistent? The adaptive asset allocation idea comes from that mindset.

Here is the simple version. Let's say you believe that you're an average person and that you want to take as much risk and get as much expected return as the average investor. The theory says to hold a market portfolio. To keep it simple, assume that the current market portfolio consists of stocks and bonds with weights of 60% and 40%, respectively—the fabled 60/40 portfolio.

Now, imagine that down the road, you look at the values of stocks and bonds in the market as a whole and, for whatever reason, it's 70% weighted in stocks and 30% in bonds. If you want to be like the market because you're an average investor, then you should be 70/30 as well. However, if you look at the investment policy statement of, say, a typical defined benefit pension fund, it will state that the policy is to be 60/40 or some other combination of relative values. Should the pension fund follow that policy? Most of them don't, but imagine if they were to. If stocks were to go up and thereby make the proportions 70/30, pension funds would have to sell stocks and buy bonds.

Almost all policy statements that I've seen are stated in terms of percentage by value for each asset class. Those policies are all contrarian. They all recommend a decision to sell winners and buy losers, and not everybody can do that.

In any event, for the investor who wants to be average in risk and return, the adaptive asset allocation formula says that if the market is 70/30, you're 70/30, too, and that's how you write your policy statement. It goes on to say that if you want to be *x*% riskier than the market, then you use a formula that tells you how to adjust your desired proportions when the market moves in a particular way, in terms of stocks and bonds. Even then, you don't do a lot of trading, which is a good thing.

**Litterman:** Why did the market go from 60/40 to 70/30?

**Sharpe:** That's not in my range of expertise. Predicting things and making probability assessments of what the future outcomes may be is your job, not the job of theoreticians.

**Litterman:** Presumably, either the expected returns have gone up to justify this or the risk aversion has changed.

**Sharpe:** Or risk has changed.

**Litterman:** It has been said that "smart beta" is the new wave. Do you view smart beta as in-sample testing?

**Sharpe:** I will try to be as diplomatic as I can and focus on a definitional matter. When I created the capital asset pricing model, I emphasized a concept that I called "market sensitivity," which is a measure of how sensitive the price or the return of an instrument—a stock or a portfolio—is to changes in the value of the overall market portfolio. Others believed "beta" was a good term for this, so we used that. For decades, beta in financial circles was defined as a measure of the scaled covariance of a return with the market return. Then, factor models emerged that dealt with the covariance of a return with, say, a portfolio of value stocks or a portfolio of big stocks, as in my returns-based analysis (Sharpe 1992) or the Fama–French (1992, 1993) work. Within the factor models, there were measures of how returns moved with the factors. I even wrote a paper early on to try to clarify that beta was more than just the sensitivity to a single factor (Sharpe 1984).

So, when I hear "smart beta," it makes me sick. By definition, if you're talking about doing better than an appropriate benchmark, then it's important that you specify what the benchmark is. If the benchmark is not the market portfolio, that's fine. You can say, "I'm going to beat the market more often than not." We used to call that "alpha." Or if you say, "I'm going to beat the market by tilting toward small stocks, away from the market proportions," then that's fine as well. That is a factor bet or a factor tilt.

Then, what I want to hear is your story. Are you telling me that this is good for everyone because you and your friends are really smart, the people who hold the market—the indexers—are dumb, and the people who underweight your favorite stocks are really dumb? If you and your friends are going to overweight value stocks and my friends and I are going to hold index funds, somebody somewhere is underweighting the value stocks. If you are smart and I am dumb, then the people on the other side of your positions must be *really* stupid.

If your story is that you have found a way to exploit stupidity and if you're right, I would suggest that before too long, the really dumb investors will begin to choose index funds or move in your direction, as may some of the index fund people. If so, your edge will diminish and eventually disappear.

I believe there is a lot of confusion in the current discussion. As academics, we don't patent or copyright anything, so we can't say that the

academic community copyrighted the term "beta" in this context. But I believe that, either intentionally or unintentionally, the current use of the term by active managers adds to confusion and doesn't help clarify matters. Also, I doubt that many of these active management strategies will be winners in the future.

**Litterman:** Expanding on that point, you've written a lot about how investors should focus on how much they're paying for products and on minimizing expenses (Sharpe 2013). I don't believe anyone would disagree with that. Does that mean we should not try to find alpha in the market? Or is there alpha—if you know how to find it?

**Sharpe:** By definition, not all active managers can beat the market before costs, and after costs, only a minority can. We know that. That's just arithmetic. But you've identified one of the great conundrums that we used to worry about in the earlier days. If everybody starts buying index funds, then I'm going to issue shares at very high prices and have everyone buy them. We absolutely need people to do research, searching for mispriced securities. That's central to the argument. The arithmetic still holds, but there is indeed a social service in finding mispriced securities.

One of the great ironies is that it's hard to capture many of the benefits of that social service, but if you think about it, to perform that social service well is no small task. We shouldn't expect to get paid much for an approach that is simple. The old-fashioned activity we used to call "security analysis" is difficult and should be valuable. Again, the problem is how the market adjusts to information. Security analysts would like to at least recover the cost of the research and hopefully make a profit for a while.

We used to worry about there being too much indexing, but I believe we've learned that human nature is such that there will still be plenty of people looking for under- and overpriced stocks. Therefore, I've stopped worrying about there being too much indexing. But at some point, it could become a concern.

**Litterman:** Is it your view that markets have become more efficient over the decades?

**Sharpe:** My instinct tells me yes but probably because it's so easy to do complex empirical work now. The standards for good empirical work are so much higher than they were in my youth. I really don't do such analysis anymore. But I would say that I've been around so long that I've seen spectacular empirical results that don't stand up after somebody does the same analysis in a different country, for a different time period, or after waiting

a year or two to see what happens. So, it's hard for me to comment.

We certainly know there is more indexing and—I believe without question—more closet indexing, involving funds with high $R^2$ values. One could argue that that has made markets a little less efficient, but I doubt that a careful reading of all the empirical results would suggest any strong evidence one way or the other.

**Litterman:** Do you have any advice for the audience?

**Sharpe:** I would like to talk about my work on retirement income. I was concerned about defined benefit plans for a number of years. I then turned my attention to the accumulation phase of defined contribution plans, and now I am concentrating on the decumulation phase for individuals.

Consider this scenario: You're retired, you've got Social Security, and you've got some money from your 401(k) plan and maybe your personal savings. What do you do? Do you buy an annuity? If not, what investment and spending strategy do you follow? As many of you know, the entire investment world—broadly construed, including insurance companies—is after you. You can't get away from those television ads from insurance companies, financial advisers, and institutional investors.

I am focusing my attention on this issue. And it's a really hard problem—the hardest problem I've ever considered—because it's multidimensional: You have two people in a household, the kids, the charities, and so on; you have huge uncertainties in terms of medical issues and longevity; and you have many future years to consider. Fortunately, there is a burgeoning literature in this area.

I have been writing Monte Carlo analysis programs to allow me and others to investigate the properties of alternative strategies. If I really knew how to choose relevant multiperiod utility functions for individuals, I would try to optimize, but absent this, I can at least look at the properties. A simple version of my Monte Carlo software is available at no cost at retirementincomescenarios.blogspot.com. It's open source, so you can get the code and do anything you want with it.

Finally, I'd like to talk about Social Security. We all know Social Security is a problem. The latest Social Security Trustees Report says that the unfunded liability of the Social Security system, projected to the infinite future, is somewhere around 150% of GDP. It's a big number (although nowhere near as large as the number for Medicare). But if you look closely at the Trustees Report, you'll find a really interesting number—albeit one that I don't get too excited about because it's not quite

what it appears to be. Using the same economic assumptions, the trustees take people who are not now in the system—people who will be entering Social Security from now until forever—and find the present value of the payments and the contributions. Here is the fascinating part: It turns out that they are equal. So, Social Security, according to that calculation, is in balance for future entrants.

But we still have this huge unfunded liability. Why? The problem is that those of us now on Social Security did not put enough in while we were working and those who are still working are not going to put enough in to take care of freeloaders like me and perhaps some of you. If you accept the trustees' calculations, Social Security is not broken going forward. But what we did in the past was a really stupid thing, and somebody's got to come up with about $18 trillion to $20 trillion to rectify it.

The qualification, however, is that all the calculations were made using the real rate of interest of approximately 2.9%. The last time I looked at the TIPS curve, even at the longest maturities, the real rate of interest was at least 100 bps lower. If you were to use a more realistic real rate of return, then the numbers for both current and future participants could look considerably worse.

My personal opinion is that people often make stupid financial decisions, and I would hate, from a personal value standpoint and not speaking as an economist, to see Social Security diminished. If anything, I'd like to see it increased, perhaps just by allowing people to buy in, but I don't believe they should be able to buy in at a 2.9% real rate.

*This article qualifies for 0.5 CE credit, inclusive of 0.5 SER credit.*

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