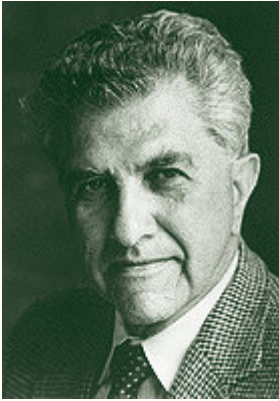


An Interview with Merton Miller

By Peter Tanous
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Merton Miller's illustrious academic career started at Harvard, from which he graduated in 1943. He spent the next few years in Washington, D.C., working at the US Treasury and the Federal Reserve. He earned his Ph.D. from Johns Hopkins in 1952. The following year, he joined Carnegie Tech, in Pittsburgh, where he taught economic history. At Carnegie Tech, Merton Miller first encountered another, somewhat older, economist, Franco Modigliani. Their subsequent collaboration was destined to become part of economic history. Modigliani won the Nobel Prize in Economic Sciences in 1985. In turn, Merton Miller won his in 1990. The product of their collaboration, which was quickly dubbed the "M&M theorem," is still widely discussed and argued among economists and corporate finance types.

If you thought economists were dull, Merton Miller will change your mind. He has a well-known sense of humor, and we'll put it to the test. While the M&M theorem is not directly about investing in stocks, it does have some very real application to valuing a company. By the time we're finished, I think you'll agree that everyone interested in the field should know something about it. We ask about his views on market efficiency and investing generally, and we get into areas few people have ever explored with Professor Miller. Here we go.

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

Merton Miller: Well, I don't know, because it was so long ago! They are part of the atmosphere. I was in economics even as an undergraduate. Stocks were part of the environment. How did you get interested in stocks?

I was an economics major at Georgetown. In my first economics class as a freshman, our professor, Dr. Gunther Ruff, asked the students why they were taking the course. I said, because I thought I might learn how to make money. He said, "My dear fellow, I have a Ph.D. in economics, and if I knew how to make money, I wouldn't be here."

When I started worrying about stocks, it was the late 1930s and early 1940s and it didn't seem like a good way to make money then, either. Stocks were in bad repute after 1929. A variety of questions were being raised everywhere about the role of the stock market crash in bringing on the depression. There were also congressional hearings and investigations, not only into the crash, but on the role of the corporation in American economic life. The subject of stocks was very much in the news. As an economics undergraduate, I also worked on a part-time basis in Cambridge, Massachusetts, for a company that was advising customers about portfolio decisions, writing reports. So I was constantly exposed to stocks, if only by reading through Moody's and transcribing numbers for the customer reports.

As far as personal investing was concerned, I was more concerned with my savings account than with stocks.

I guess that was appropriate to the '30s.

Yes, it was. You could get an interest-paying savings account in Harvard Square, providing there wasn't too much activity in your account. I would get my monthly allowance and put it in one of the local banks, making small withdrawals every day to pay expenses. After awhile, I would get a notice from the bank saying that there was too much activity in my account and they were closing it out. So, I would walk my money across the street to one of the other banks. There were four of them, one on each corner. I just put the money in the next bank. That way, I managed to have a checking account without paying transaction fees. I didn't feel guilty, because I knew that the banks had gotten the government to ban interest on checking accounts. I was just doing to them what they were doing to me.

I see the beginning of an economic theory here. As you know, Professor, our book focuses on interviews with great investment managers, but I also wanted to get some top academic points-of-view on markets. I thought it might be interesting to begin our conversation by talking about your celebrated work with Franco Modigliani in the area of corporate capital structure. I am referring, of course, to your combined work, amusingly known as "the M&M theorem." As I recall, instead of asking investors how they might determine which of a corporation's securities they might want to buy, you looked at it from the opposite perspective. You asked, how should corporations decide what securities to sell.

Yes. That was certainly part of it. Early on, I had to teach a course on corporate finance. I had never had a course in finance, or at least a business school variety course. My expertise was in public finance, particularly corporate taxation, since I had worked at the US Treasury. At first, I worked in the corporate tax unit of the Division of Tax Research at Treasury, later in the government finance unit at the Federal Reserve. So, I knew the tax side of corporate finance, and the economics of public finance, but not the standard finance stuff.

In 1954 or so, before they let me teach a business school finance course, at Carnegie Tech [*now Carnegie Mellon*], they said, you must sit in on the class of someone who is teaching it the proper Harvard Business School way. So, I sat in the class. When we took up case number one in the case book, I remember being struck that the solution was not obvious to me. After the instructor explained it, however, I said, Yeah. That's right; that makes sense. Then we came to case two, and I said, Okay, I remember how we solved case one, so the answer must be this. And, of course, it was different. I couldn't sense any connection from one case to the next. Everything was, as they say on railway tickets, good for this train and this day only. For

me, as an economist, it was frustrating to have no sense of a theory of corporate finance to tie all this material together.

Do I sense the origins of M&M theory here? I think you are saying that there wasn't just one right solution to the cases you studied. Likewise in M&M, you were seeking the optimal capital structure for a corporation; in other words, how much debt, and how much equity a company should have. Then you found out it didn't matter. There wasn't just one right answer.

That's down the road a bit. First, the problem was to figure out what determines these choices. There are various analogous models in economics that could have been applied in this area, but none of them seemed to work very well. Franco and I were both working on the problem, but from somewhat different perspectives—he from macroeconomics and me from corporate finance. I had some of the students in my finance class actually do some empirical work on capital structures, to see if we could find any obvious patterns in the data, but we couldn't see any. We couldn't find any consistent patterns and certainly no evidence of an optimal structure. We said, you know something, maybe there isn't any optimum! [*For example, in the proportions of debt and equity.*] Franco and I then tried to prove our suspicion that there is no optimal capital structure.

People often ask: Can you summarize your theory quickly? Well, I say, you understand the M&M theorem, if you know why this is a joke: The pizza delivery man comes to Yogi Berra after the game and says, Yogi, how do you want this pizza cut, into quarters or eighths? And Yogi says, cut it in eight pieces. I'm feeling hungry tonight.

Everyone recognizes that's a joke because obviously the number and shape of the pieces doesn't affect the size of the pizza. And similarly, the stocks, bonds, warrants, etc., issued don't affect the aggregate value of the firm. They just slice up the underlying earnings in different ways.

I recall a story that, after word got out that you had won the Nobel Prize in Economics, the media tracked you down and asked you to explain your theorem in a way their audience might understand. Like in ten seconds.

The pizza story is one I often use. Another is, if you take money out of your left pocket and put it in your right pocket, you're no richer. Reporters would say, you mean they gave you guys a Nobel Prize for something as obvious as that? [*Lots of laughter.*] And I'd add, Yes, but remember, we proved it rigorously. [*More laughter.*] Actually, we did use a new form of rigorous proof known as "arbitrage" proof. Arbitrage proof has since been widely used throughout finance and economics.

If I'm summarizing the M&M theorem correctly, the market value of any firm is independent of its capital structure, so the proportions of stock [*equity*] and bonds [*debt*] doesn't affect the value of the corporation. Now if that's the case, are all these highly paid corporate chief financial officers wasting their time trying to figure out how much preferred stock to issue, or how many bonds, or how much common stock?

To some extent. But remember, the M&M proposition is the beginning of wisdom; it's not the end of it. To really utilize it best, you have to tip the proposition on its head. You say, look, in

order to make this proposition true, you must make the following 15 or so assumptions. So if people out there say, aha, the M&M theorem doesn't hold true in the real world, then we say, it must be because one or more of the 15 assumptions must be failing. And that has provided the research agenda for the profession.

What happened after publication of our paper was that, for the next 40 years, people said, all right, we now know the answer to the capital structure question under ideal conditions. Let's now drop, or relax, some of these assumptions and see how it affects some of the conclusions. That's not the kind of undisciplined Harvard Business School, each case on its own, approach. It's systematic. You can say, for example, as we did even in our first paper, suppose there's a big corporate income tax with a 50% rate? That's going to affect the optimal choice between debt and equity. In fact, it's going to make issuing debt, rather than equity, extremely desirable [since interest is deductible for tax purposes]. Next, you go on from there and say, yeah, but firms don't have 100% debt. Then you have to start to explain why and think up additional reasons, such as agency costs or offsetting taxes, that will keep them from going to extremes. That's what the profession has been doing for 40 years.

It occurs to me that the great junk bond revolution might have had the effect of confirming or disproving the M&M theorem since so many companies opted to go heavily in debt. Did the popularity of junk bonds affect corporate values?

The junk bond revolution fits right in with M&M. Junk bonds prove there's nothing magical in a Aaa bond rating. Don't pass up big profit opportunities, or tax savings, just because of your credit rating. What counts is what you do with your money, not where it came from.

I also want to mention the one example where the original M&M theorem can actually be seen holding in the real world. It comes from the field of options, where it is known as the put-call parity. It holds to three decimal places. Options, of course, bring Myron Scholes, one of my former students, to mind as well as my good friend Fischer Black.

Their reputations are well established. These fellows developed the famous Black-Scholes model. Could you explain it briefly?

I don't have a pizza story, but I do want to go on the record saying that I regard their Black-Scholes formula as one of the major intellectual breakthroughs of the latter part of the 20th century in this field. It was not only an intellectual achievement, but it spawned a whole new industry. Their model was an amazing development because it is one of the few cases in finance where you can actually compute what a security is worth, not just in abstract terms, but in actual dollars.

Black and Scholes developed a formula which priced options as a function of observables. By observables, I mean that the warranted option price is a function of the strike price, the price of the underlying security, the interest rate, the time to maturity, and the volatility of the underlying security. The only thing that isn't directly observable is the volatility, but that can be very closely approximated. Much better to approximate the volatility of something than the mean expected return, which is what stock pickers have to do. You can always get a pretty good fix on the volatility, even though it's not perfect. It's still a lot easier than estimating the expected rate of return on shares. Incidentally, if you read the original Black-Scholes paper [*"The Pricing of Options and Corporate Liabilities,"* Journal of Political Economy, vol.81, May-

June, pages 637-659], you would note that they generously acknowledge the influence of the arbitrage proof from the M&M capital structure paper, which was earlier.

Since Fischer Black and Myron Scholes were able to determine option pricing by using all of the surrounding variables, might it be possible to do the same thing for stocks?

No, you can't really, except, perhaps, in some extreme cases. If a share is super highly leveraged, so that you just got this little thin sliver of equity over the debt, then Fischer and Myron pointed out that it's basically a call option, not a share. And you can, to some extent, price it that way. You can also do that with some kinds of bonds. But, by and large, options are the only case in finance where you can successfully price something as a function of observables.

That's very interesting. Now let's turn to the subject that is a focal point of this book: active versus passive management. Let me ask you right off the bat, do you believe in active management in any form?

Not really. That's based on my study of finance and my belief that markets know much more as markets than an individual does as an individual. This is, of course, the subject we talked about a couple of weeks ago. I should mention that I am a member of the board of directors of Dimensional Fund Advisors.

I had a long talk with Rex Sinquefeld.

Rex is one of my students, too. Almost everybody is because I've been around so long!

I spoke to another one of your students, Gene Fama.

Of course. I favor passive investing for most investors, because markets are amazingly successful devices for incorporating information into stock prices. I believe, along with Friedrich Hayek [*also a Nobel laureate, and a contemporary of John Maynard Keynes*] and others, that information is not some big thing that's locked in a safe somewhere. It exists in bits and pieces scattered all over the world.

Everybody has a little piece of the total information. Even the dentist from Peoria, I always say, at least he knows whether or not his patients are paying on time. So everybody has some information. The function of the markets is to aggregate that information, evaluate it, and get it incorporated into prices. But if information, as I insist, is widely scattered and diffuse, most individuals are not going to have much information relative to the total. Most people might just as well buy a share of the whole market, which pools all the information, than delude themselves into thinking they know something the market doesn't. They can't be hurt by doing that, because the price they pay will indeed reflect society's best current information.

I've tried to approach this as open-mindedly as possible and I've talked to top-tier academics, you among them. I've also talked to people in the business, like Rex Sinquefeld, who is dogmatic on this subject. Yet, when I talk to the active managers, especially those who have a fairly long performance history—what the academics call "persistence"—I keep running into anecdotes.

That's all they are . . .

But you keep running into these stories about information, seemingly previously unknown, that gets uncovered, with a certain amount of research. Isn't it true that, until somebody does that research, it really wasn't widely known?

Here's an example: Michael Price, who runs Mutual Shares, had a wonderful story about a metal, tantalum, that was going up in price. He did some research to find out which companies were involved in tantalum, and, in fact, managed to discover them before the effect of the price rise was generally reflected in the prices of those stocks. I expect there are many other stories like this.

Let me back up and say one thing more clearly, I hope. There are really two different groups of investors. One group, the overwhelming majority, and the group I've been talking about, has no significant private information not already in prices, and they should invest passively. They aren't going to make above-normal returns, except by accident. But there's another group that can hope to make money by careful research in the market. How much money can they expect to make? Taking the group as a whole, they make just enough, on average, to cover the cost of their research.

This distinction I've been making, between traders with significant non-public information and those without it—which includes most investors, including pension fund and mutual fund managers—is known as the Grossman/Stiglitz theorem. Sandy Grossman is a brilliant young economist at Wharton (and a former student of mine, needless to say). He was here at Chicago, and then went on to Stanford, Princeton, and now Wharton. Joe Stiglitz went from Yale to Stanford, and is now the President's chairman of the Council of Economic Advisors. They wrote a famous paper on rational expectations and prices [*On the Impossibility of Informationally Efficient Markets*, "American Economic Review, Vol. 70, 1980, pp 393-408]. Their proof that both the informed, and the uninformed, investors can expect to make the same return, on average, is neat.

The essence of the efficient market thing is, after all, as we in economics have always held: There's no free lunch. You can't just sit back in your office scanning the newspapers, reading research reports, and listening to "Wall Street Week," and hope to earn above-normal rates of return. To beat the market you'll have to invest serious bucks to dig up information no one else has yet. Because it looks easy, many people may be tempted to try it. But there's no automatic reward from investing in trying to dig up important non-public information. It's like gold mining. A few lucky ones may strike it rich, but most "active" investors are just wasting their time and money. Once they realize that average returns on investment in information are zero or less, if the industry becomes overcrowded, the smart ones will stop trying and will leave the search industry. They become indexers.

Isn't the research and the hard work you do the price you pay for the reward you achieve?

Yes, but it just compensates you for the expenses. Of course, I don't mean you, personally. I mean you, on the average. Remember, as economists, not psychologists, we deal with behavior on the average. This is just my view, of course. It's not the opinion of everybody in the finance or economics profession, needless to say.

I sensed that even Gene Fama and Bill Sharpe believe that a very few managers, like Peter Lynch at Fidelity Magellan, have persistently outperformed the market, and that is borne out by the data.

Well, we've heard many of these tales. We used to hear, for example, that Value Line had some kind of an edge. These tales come and go. They don't usually stand up forever, although sometimes they seem to last for many years. You can make a huge living in the investment field, moreover, if you can once get the reputation of being a winner. It's going to take a long time to reverse it.

I always use an example that dates back to the '30s. The big name then was Bernard Baruch. A genius. He was everybody's favorite pundit. There wasn't any economic issue where the press didn't go to see Barney. When you study his fabulous record, however, I think he was right once. But, he was right in a big way. If you make a big score way out on the right hand tail of the distribution, then the probabilities you face from then on are mostly the little moves to the left and to the right in the center of the distribution. You're not going to get that first big gain removed. You only need to make one big score in finance to be a hero forever.

I see your point. That one score will keep your average gain high for years. But take the whole outlier theory—the right tail of the distribution curve where you find the Peter Lynches and Warren Buffetts. What separates the men from the boys, so to speak, is persistence, isn't it?

Perhaps it would be, if we could measure persistence accurately. But in practice, it often comes down to not suffering a loss as big as the huge gain you made a while ago. Thus a fellow like George Soros may be skating on thin ice. You see, he made a big killing and if he would now just do modest investments, he would never lose it. He'd be a winner on balance over any time horizon. But if he insists on plunging again, he's just as likely to take a bigger loss. He may wind up giving it all back.

It's funny. One of the managers I'm interviewing, Richard Driehaus, said about Soros, "He had a hunch and bet a bunch!"

Right. And he'll have another hunch, and he'll bet another bunch, and this time he'll lose. But if he doesn't do it that way, if he has a hunch and bets a bunch and wins, and thereafter plays the conservative game, he'll go down in history as the genius of all time. The gains and losses average out, but only in the very, very long run.

To me, the name of the game is finding the people who show persistence at beating the market.

Well, let me tell you one of my favorite stories. I once asked a pension fund manager, why don't you just index your funds instead of doing all this churning you're doing there? And he said, I can't index the fund because then I wouldn't be worth \$400,000 a year! If you ask people in the trade, how come you make so much money? What do you want them to say? Oh, it was just dumb luck, Professor. I don't think you'll get that response very often.

The more typical answer is that it was our brilliant deductive analysis that got you that great performance.

Yeah. There are people like Bill Sharpe and Gene Fama who are working all the time to test various hypotheses about it, but to me the sample is way too small to judge "persistence," that is, to be able to tell luck from skill. There's another story I love to tell: The bursar of a British college, at Oxford, had members who were pounding on him that they weren't earning enough. He answered by saying, I admit our returns have been down recently but you must remember that the last two hundred years have been very unusual!

Big consolation!

I don't know how long is long enough to get rid of the influence of sample flukes.

I have no doubt that Bill Sharpe and Gene Fama's work all supports the efficient market theory.

I can't speak for them, of course, but I believe that most economists would accept the view that, while you sometimes can make a score by sheer luck, you can't do it constantly, unless you're willing to put the resources in. One way or another, you have to get significant non-public information, which most fund managers don't have.

In fact, I thought the most convincing of Gene Fama's points was that he took ten years of mutual fund data from The Top 20 Morningstar funds and looked at their performance for the following ten years.

And there was no correlation. It has all the earmarks of a random process. One amusing thing that the SEC once did was, they said you can't bring out a new commodity fund unless you've got five years of experience. So what do you do? You run your fund on the small until you manage to hit five good years. Then you've got a track record, and you say we've done it five years in a row! And then you go public, of course. All the studies have found that there is no correlation between the results of the previous five years and the subsequent five years. Virtually no correlation. But that's a mass statistical test. There may be one fund that was high in both periods. But remember, in economics, we work with statistical aggregates, not individuals, so that is bound to happen sometimes. Individuals, quite naturally, resent our pointing that out. They say, don't treat me as a statistical aggregate. I'm an individual!

I've got to tell you, I spoke to Peter Lynch, who was absolutely wonderful. I said, Peter, you've got to realize that to the great academicians, and we're talking Nobel Prize winners, you are the millionth monkey, the lucky orangutan at the typewriter who wrote Romeo and Juliet. And Peter is not the only one with a great record.

That's why they're where they are and I'm where I am. It's a tough argument to counter. He did have success. Anything we say sounds like sour grapes. If we're so smart, why aren't we rich?

No. They don't talk like that. That would be very inelegant. They wouldn't do that. The point they do make is, wait a minute, let me tell you how I did it! I mean, this is the process that I use, and continue to use, and guess what? It's not magic. It's just common sense, and it works.

Here's the way to look at it. There's a famous trader in the bond market at the board of trade—I'm getting so old, I can't remember his name—but he made huge amounts of money trading bonds and bond futures. He said, I've got a foolproof trading system here. But here's the acid test of whether I really have a winning system. I will accept a few hand-picked students and teach it to them. The test is whether they make money. Can you explain it to a third person, and if that third person trades, does he make money? He set up a little school and he trained these people. You know what happened? He's now out of the business and so are the students. Maybe Peter Lynch can do it, but can he teach another person to do it? If he could, we'd have some evidence that it's more than just luck.

Well, he says he can in his books. The way I put it to Peter Lynch was, if I read both of your books, which I have several times, I'd find the answer to getting rich is to hang out at the mall and see what's selling.

I don't read the books. But that's the thing that makes us academics so skeptical. If it's a teachable skill, then perhaps you can teach it to many others. That may generate enough data to tell skill from luck. After all, when a 15 handicap golfer breaks par, which can happen, you know it's just dumb luck. But to be considered a real champ, you have to break par in hundreds of matches. My point is that you can't tell skill from luck unless you have large samples. We just don't have them for testing skill in stock picking.

I have to ask you a favor.

Okay.

You know that you're noted for having a wonderful sense of humor. There's a story, and I don't know if its true or not, but if it is I'd love you to tell it. It's about a speech you were supposed to give in Hamburg.

Whether it's true or not, here is the story. I was traveling in Germany many years ago and a friend of mine, a German professor, arranged for me to give a talk to the finance faculty at the university in each city. I wanted to see all of the big cities in Germany, including Hamburg, but my friend said, I can't send you to the University of Hamburg because they're all communists there. There is, however, one school in Hamburg where the communists haven't taken over, and that's the high command staff school of the German army, the Hochschule der Bundeswehr. I'll set it up for you, he said.

So I went from Cologne to Hamburg on a military pass. I get into the Hochschule der Bundeswehr and, like he said, it's a military school. The students, all in uniform, went everywhere running at a trot, not only in the corridors, but up the stairs. Now the only talk I had for this trip was on a fairly technical subject of interest only to finance professors. So I looked down from the lectern at the rows of young uniformed faces sitting politely at attention in the high-tech auditorium, and the only thing I could think of to say was: "Gentlemen: Tomorrow we invade Poland!" *[Gales of laughter.]*

We're nearing the end of our talk, professor. I wonder if I might ask you, based on your experience, how do you think people should invest for the future, be it their retirement, or college education, or what have you? Should they buy index funds?

Absolutely. I have often said, and I know this will get some of your readers mad, that any pension fund manager who doesn't have the vast majority—and I mean 70% or 80% of his or her portfolio—in passive investments is guilty of malfeasance, nonfeasance or some other kind of bad feasant! There's just no sense for most of them to have anything but a passive investment policy. And I know people will say, yeah, but if everybody invested passively, who would discipline the corporations? Well, as I explained earlier, the few people who are willing to spend the money to do it. And they will get enough extra returns to compensate for their costs. But that's about it. Most pension fund managers cannot even reasonably hope to do any better than a passive fund. And, on a risk adjusted basis, they don't! I believe that data are quite strong on this.

In fact, Bill Sharpe thinks only "mad money" should be actively managed.

That's based on the principle that, as long as you keep the amounts of active money reasonably small, the active managers won't do too much damage.

I'll tell you another story that will irritate your audience. The first time I made this point was in the '50s, when there was a guy at a pension fund who was explaining to me that he had five separate managers. At the end of each year, he'd see which manager did the best and which did the worst. He fired the worst and he brought in another one.

A fairly common tactic and theory.

A common theory. Well, I always say that's like having a passive fund, all right. Only it's the most expensive way to do it. Because if you have five separate managers, you're going to wind up pretty much with the market average. So why not just go there in the beginning and stop all this style analysis nonsense. Some people, I'm sure, make a handsome living tracking styles and so forth. I'm very skeptical. If I were in charge of a pension fund, I would put it in passive management.

But the style thing does have relevance. The academics have demonstrated that styles of stock vary together. In other words, the growth stocks tend to perform similarly but, for example, the growth and the value style don't perform the same way.

Well, you know, I suppose if you take 50/50 growth and value, you get back to the market. How are you going to tell which one is due to take off?

You can't. The idea is that you allocate assets by style. One thing I find interesting is that the data show that value stocks outperform growth stocks.

They show that they have over some period of time. As I said, I'm always worried that the last two hundred years, or whatever your sample period is, have been somewhat unusual. I take a very long view and I'm not convinced, yet, that simple passive investing isn't the best way to go for the vast bulk of all investors. Unless you can explain to me why some strategy that everybody could follow is superior.

Oh, they explain it all right. They explain it by risk. They say you get rewarded for the risk you take. Value stocks are riskier. Ergo, you get more reward with value stocks. Now, that's controversial.

Yeah. But if it is risk that accounts for the differential, and it has to be if the differential is not just some random sampling fluke, then some day the risk will happen. And when it does, you give it all back. After all, our Dimensional Fund Advisors small-cap portfolio under performed the S&P 500 for 6 or 7 years in a row. It's back up again now, but who knows when it will tank again? All you can say is that small stocks are part of total wealth. I should hold my share of them, not just the S&P.

Do you practice what you preach in your own investments, or do you secretly have an active manager on the side?

No. I do read the papers. Sometimes I get intrigued by the idea of a drug company that has a drug for obesity, or something like that. I may take a flyer on some of those things.

Boy, am I glad to hear that!

Yeah. But that's strictly recreational. It's not serious investing.

But for serious investing, I presume you invest in the market.

For the equity portion of my portfolio, yes. But I made a mistake, probably along with many others in my generation who lived through the '70s. I had too balanced a portfolio—too much bonds, relative to stocks. Had I put more in stocks, I'd be wealthier today!

Many thanks, professor. That is no doubt worthy advice.

Indeed it is! Stockbrokers rejoice! Here is the noted economist, Merton Miller, telling us he wished he had put more of his own money in stocks. And once again, we are exposed to the prevailing view among academics that those of us who try to beat the market are just wasting our time. Sure, some of us will succeed, just like a few of us will win the lottery or hit a slot machine jackpot.

I found it interesting that Miller's view of the efficient market hypothesis is not extreme. He allows that some people may be able to get information before others, a la Michael Price, and profit from that information. However, he believes that in the aggregate, the extra profits will only amount to the money spent doing the research. But, among those making those extra profits, there will be some who do very, very well. Our challenge is to identify these winners and observe how they do it.

As I reread Miller's comments, I was impressed by the elegance of his points, and the compelling explanations of his views on market efficiency. Miller is an historic figure in the field of economics. We asked him to stray from his normal field, the classic Miller and Modigliani theorem. But the journey was worth it. This wise man—I dare not call him old—not only shared insights and wisdom with us, he did so with humor.

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