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JOIM CONFERENCE SERIES DR. HARRY M. MARKOWITZ INTERVIEW WITH DR. RICHARD O. MICHAUD SAN DIEGO, MARCH 6, 2011

CONFERENCE SUMMARIES

H. Markowitz: I'll start by thanking Richard, Gifford, Sanjiv, and all my friends and well-wishers over the years.

R. Michaud: I have just a couple of things to say before we start on the interview.

We are here to honor Dr. Harry Markowitz, a true giant of modern finance and economics. As everyone knows, his major awards for his contributions to science include the 1989 John Von Neumann Prize in *Operations Research Theory* given by the Operations Research Society of America and The Institute of Management Sciences and the 1990 Nobel Prize in Economic Sciences jointly with Merton H. Miller and William F. Sharpe.

All of us here know Harry for his many contributions to finance and economics. Many of us have been privileged to know him as a friend. It is therefore fitting that here at the *Journal Of Investment Management Conference*, he is deservedly honored as our intellectual father. He was a pioneer in laying down the foundations of modern finance and asset management. He has been an unerring guide throughout its long history. His many articles and books continue to inspire us. But Harry takes no prisoners. If he thinks you are wrong, he will tell you and demand your best to prove him wrong. However, he is truly a generous man and tells his story with great respect for the truth.

In our time together, I hope to elicit some of the motivations and ideas behind the headlines of his work. Also, there is no one better to bring us back to the beginning of modern finance and to tell us something of the verbal history of the field, including the tremendous set of ideas that reached its amazing climax in his 1959 book on portfolio diversification. Finally, I hope he will share his thoughts on his most recent work and its implications for the future of finance.

A useful scientific model is a way to think about things. The Markowitz efficient frontier provides a wonderfully rich economic model because it allows us to think about a wide range of issues in asset management and modern finance. An especially interesting recurring theme that

I hope to highlight in our time together is how his mathematics and finance have always been informed by actual investment practice.

So let's start at the beginning, Harry.

1 Portfolio Selection 1952: The Birth of a Theoretical Model for Finance

R. Michaud: This paper is based on an epiphany. You rejected the expected returns rule then current in financial theory because it ignored the importance of diversification. The paper follows from your observations of how actual professional investors behave. Can you tell us about your epiphany and how the Markowitz frontier rationalizes investor behavior?

H. Markowitz: The epiphany happened sometime one afternoon in the business school library of the University of Chicago, while I was reading John Burr Williams' *Theory of Investment Value*. Let's start from the beginning. Why was I in the business school library? I was trying to get a PhD in economics, but I was there in the business school library reading John Burr Williams' *Theory of Investment Value*. How did that happen? I was at the stage where I had to choose a dissertation topic. I went to my advisor, Jacob Marshak. He was busy, so I waited in his ante-room. There was another man there. We started chatting: I told him why I was there. He told me that he was a broker and that he was waiting to see Professor Marshak. He suggested that I do a dissertation on the stock market. When I got into Professor Marshak's office, I said, "the guy out there says I should do a dissertation on the stock market. What do you think?" Professor Marschak said that Alfred Cowles (who endowed the Cowles Commission) hoped that people would do that. But Marshak did not know the finance literature. He sent me over to Marshall Ketchum, a professor at the Business School who gave me a reading list. By the time of the epiphany, I had read Graham and Dodd, I had read Weisenberger's *Investment Companies and Their Portfolios*, and now I was reading John Burr Williams' *Theory of Investment Value*.

Williams says that the value of the stock should be the present value of its future dividends. I said to myself, dividends are uncertain, he must mean the expected value of future dividends. But if you're only interested in the expected value of a security, you must be only interested in the expected value of a portfolio; and if you're only interested in the expected value of a portfolio, the way you maximize that is to put all your eggs in one basket, to put all your money in one security. Now I knew that is not right: everybody knows you're not supposed to put all your eggs in one basket. Also, I had read Weisenberger's *Investment Companies and Their Portfolios*, and had seen many well-diversified portfolios. I thought of the returns on securities as random variables, and the return on the portfolio as a weighted sum of these random variables. I knew offhand what the expected value of a weighted sum was, but I did not know offhand what the variance of a weighted sum was. So I got a book off the library shelf: Uspensky's *Introduction to Mathematical Probability*. I cannot remember the name of anybody who has been introduced to me today, but I still remember Uspensky's 1937 Introduction to *Mathematical Probability*; I found where it discussed the variance of the weighted sum of correlated random variables; I saw all those covariances—and that was my moment of epiphany! Obviously, the variance of the portfolio depends not only upon the volatility of the individual securities that comprise it, but also to what extent they go up and down together. I was a budding young economist interested in two quantities: risk and return; so I drew a graph with risk on one axis and return on the other. At the time, I was taking a course from Tjalling Koopmans on asset allocation. He distinguished between the efficient and the inefficient allocations of resources. So I distinguished between the efficient and the inefficient portfolios. This was the first efficient frontier. Many of the ideas that are basic to portfolio theory happened in that afternoon.

R. Michaud: Your paper uses inequality constraints and geometric arguments rather than relying solely on analytical formulas to describe the Markowitz frontier. To me that is just amazing. Were you concerned about limited borrowing and the need for computational methods in finance for modeling investor behavior even then?

H. Markowitz: The model I had of investment was the investment company, and investment companies did not have negative holdings. They had positive holdings, so I modeled that. I do not remember whether I knew the critical line algorithm already by 1952, but my article has a rather large footnote about how you move from one critical line to the next. I must have had the formula, because I drew these lines as straight lines in the three security and four security figures. My best guess is that I had solved the computational problem for the case where the only constraint is that the sum of the Xs equals 1, and the Xs were non-negative. It was by 1956, after being exposed to George Dantzig and learning a bit more about computation, that I published the general solution.

R. Michaud: But you could have used only analytical methods to solve.

H. Markowitz: Well.

R. Michaud: Like Lagrange multipliers.

H. Markowitz: They are analytic. You set up a Lagrange multiplier. There's something called the Kuhn–Tucker Theorem. If you do not have any inequality constraints: you set the partials equal to zero, throw in the original equation, and solve the system of simultaneous equations. But if you have inequalities, variables which are against the boundaries won't typically have zero partials. These partials, called etas, will typically be positive. Securities in the portfolio have their partials equal to zero; the ones that are out of the portfolio must have zero or positive partials. When their partials want to turn negative that determines when they get into the portfolio.

R. Michaud: It's amazing to me that you were talking about computational methods back in 1952 when few had any interest.

2 Markowitz's "Utility of Wealth" (1952): Behavior in the Face of Risk

R. Michaud: 1952 was a great year as well for other things. You published *The Utility of Wealth*. This paper modifies Friedman–Savage utility functions to materially increase the extent to which it is consistent with observed investor behavior. Your analysis also includes the notion of loss aversion and refers to psychological experiments to support plausibility. Are you the first behaviorist in finance?

H. Markowitz: I like to say that I'm the father of portfolio theory and the grandfather of behavioral finance. I still dabble in it. The most interesting thing in Markowitz 1952b, *The Utility of Wealth*, for me at this time is the last footnote, which points out that if you are an expected utility maximizer and you can design your own fair lottery, you would never choose a multi-prize lottery. But, on the other hand, it seems that all real-world lotteries are multi-prize. So that was the big puzzle: how do we explain that? There is a handbook on financial engineering that Andrew and Bob Merton are editing. They invited me to do a paper which I call *Portfolio Theory as I See It Still*, I have a new hypothesis there, to explain the existence of multiprize lotteries.

R. Michaud: In both 1952 papers, you use actual investor behavior as a guide for your mathematics and models. Does your work support or contradict the behaviorists who often claim that investor behavior is irrational?

H. Markowitz: The 1952 paper said that it offered mean–variance both as normative advice and as a positive theory. My 1959 book said nothing about mean–variance as a positive theory. I didn't say it was true, I didn't say it was false. I did not take a position on it as a positive theory, only as a normative theory. I recently re-read Markowitz 1952 and he is very cautious. He says, *perhaps* the actual computation of efficient frontiers may be of practical use. It will be so under two conditions: first, people want to use mean and variance; and second, they can come up with reasonable estimates, which I believed would involve a combination of statistics and security analyst views. As to whether someone would really like to use mean and variance, I distinguished between what I called the investor and the speculator. I said that the speculator may want to also consider the third moment. In particular, the 1952 paper says that people who run money but who don't want to gamble with that money—other people's money—may want to use mean–variance. So there is no contradiction. In Markowitz 1952b, *The Utility of Wealth*, I tried to explain gambling and insurance while avoiding certain bizarre implications of the Friedman–Savage article, but the last footnote says this does not work either.

R. Michaud: Well, let's skip the '56 paper on critical line algorithm to go to the '59 book.

H. Markowitz: OK.

3 Portfolio Selection 59: An Explosion of Ideas

R. Michaud: Your 1959 book is an amazing explosion, a nuclear explosion, of ideas and innovations. I do not know what to choose from your book—the law of average covariance foreshadowing CAPM; computational methods such as the critical line algorithm which everybody uses today, or defining the max risk point on the efficient frontier for long-term investors. What I got from your book, as I remember, is that by the time I got to the end, you were talking about rationalizing the Markowitz frontier for multi-period investors in terms of implied utility functions. There are other things as well. So what would you like to talk about?

H. Markowitz: Let me just give the broad context. What is in Markowitz 1959? Some things I said in 1952 that I still agreed with in 1959. That's just fine. But my opinions about mean and variance of *what* and *why*—these evolved from 1952 to 1959. They have remained about the same since then. I'm still a Bayesian for example. You can think of the 1959 book as having three purposes. The first was expository—it explains what's a mean, what's a variance and so on and so forth, including what's a mean–variance efficient set. This raises the question of how do you compute these things? The actual algorithm in general I wrote out in Markowitz 1956. Markowitz 1956 is an ugly paper. (Sanjiv, whatever you do, do not read Markowitz 1956.) The reason it is an ugly paper is that I distinguished cases in which there's linear equality constraints versus linear inequality constraints, and cases in which there are variables which must be non-negative and variables that are permitted to be negative. I hadn't yet learned from Dantzig—from whom I got all my computing education—how to reduce the complicated case to a simple basic case. In "1959" I do not assume there is no shorting. Between "56" and "59," I realized you could reduce the complex general case to the standardized case. In particular, slack variables are the standard way that linear programming converts inequalities into equalities. The sum of positive and negative parts is the way you convert variables which can be negative into ones which must be nonnegative. There are people in here who produce results that assume that the only constraint is that the sum of the Xs equal one. They speak of negative variables as if they are short positions. I wonder if they realize that this is not a realistic representation of short positions. For example, with this constraint set you can short a million dollars worth of X1, and go long a million and one dollars worth of X2. The sum would equal one, but that is not the way Reg-T works. The sum of the longs and shorts must be less than or equal to 2. This business of modeling with inequality constraints—maybe I had a hint of it in 52, but it would develop more fully around Dantzig. So between the 1956 article and the 1959 book, I realized that generality could be achieved more elegantly. In addition I was able to prove that the algorithm works even if the co-variance matrix is singular.

So one thing that had evolved between 1952 and 1959 was computation. Another was the rationale for using mean–variance analysis. I was a student of Leonard J. Savage. I say I became a Bayesian from Leonard J. Savage at point-blank range. He was an intellectual giant. So I had to reconcile portfolio theory with the theory of rational behavior under uncertainty. The theory of rational behavior under uncertainty, as Leonard J. Savage convinced many, says that you should maximize expected utility using probability beliefs where you do not have objective probabilities. Also, the single period mean–variance analysis sits in a many period world, and one of the things that's a big difference between 1952 and 1959 is the relationship between single-period analysis and the many period world. Not only was I exposed to George Dantzig at RAND, I was also exposed to Richard Bellman, and dynamic-programming. I had talked about mean–variance approximations to expected utility, and from Bellman's work it was clear that the utility function I was approximating was the dynamic programming "derived utility" function. These things are all in the 1959 book.

Chapter 3 of the book talks about the relationship between the expected return of a portfolio and that of securities; Chapter 4 talks about the variance of return of a portfolio. Chapter 5 tries to simplify these formulas by seeing what happens if you equal weight all securities. If you set each x value to 1 over n, and let n go to infinity, you find out that as you diversify more and more, the variance of the portfolio generally does not go to zero, but goes to the average co-variance. I cannot believe I'm the first one to observe that. It's such an easy result to get—it just falls out, but I guess people were not interested in correlated risks. Everybody assumed independence. The book also discusses why you should not go up the frontier beyond the point where you were starting to reduce expected log, which is the log of the 1 plus the geometric mean: if you go up the frontier too far, you start to reduce growth in the long run while you continue to increase volatility. I was not the first one to observe this—that would be Kelly and Latané.

R. Michaud: But you were the first to identify the max growth point for the efficient frontier.

H. Markowitz: The notion of expected log as growth in the long run was first investigated by John Kelly (I believe from Bell Labs) but introduced into finance by Henry Latané. Geometric mean was commonly equated with return on the long run, until Samuelson said we were wrong. One of the wonderful things about this profession—as Richard said, "Harry takes no prisoners"—is this dialectic process of arguing back and forth: Samuelson and me about investment for the long run; Bill [Sharpe] and me about CAPM; Richard and me about the resampled frontier. Now in Richard's case, I did an experiment and it came out in his favor. "You win a few, you lose a few."

Back to my 1959 book. It covered a variety of topics: in particular, the expected return and variance of portfolios versus those of securities, the law of the average covariance; mean–variance approximations to the geometric mean; how to compute mean–variance efficient frontiers; and why to compute them.

4 Markowitz–Samuelson Debate, 59, 76: Investment in the Long Run

R. Michaud: The law of average co-variance was in important ways a foreshadowing of the market line and some of the CAPM. The approximation to implied utility was a way of thinking about the mean–variance efficient frontier, which is a single-period model, having important implications for multi-period investing. In the maximum growth portfolio, you began the great Markowitz/Samuelson debate. You showed that long-term investors should not choose MV efficient portfolios higher than a point with approximately max expected log $(1 + r)$. This result sparked a disagreement between you and Paul. Harry had Latané, Breiman, Kelly, and Hakansson on his side. Paul had Bob Merton on his side. I wrote a paper on this subject published in 1981 because I was doing work on long-term investment policy and needing to understand the geometric mean I found myself between these two titans.

Samuelson's view is that many plays of an investment game should not matter in deciding your choice of utility function. As far as I can tell, the two of you were talking about two different things. You were talking about the long-term growth rate if you are a mean–variance investor, and Samuelson was talking about how repeated investing should not matter in terms of one's choice of utility. Samuelson wrote a paper called *Why We Should Not Make Mean Log of Wealth Big Though Years to Act Are Long*. And if you have not read this, you have to read it. It is all in single syllables because he wanted to be sure that you understood what he was saying.

H. Markowitz: Samuelson's paper is all in words of one syllable, except for the last word which is "syllable"—in which he calls attention to what he has done.

R. Michaud: He says he agrees with Harry that if you have many plays of the game, on average you will be able to beat someone else in terms of the growth rate of the portfolio, but "what we do doubt is that it should make us change our views on gains and losses should taint our risks. When you lose, and you sure can lose with N large, you can lose real big."

H. Markowitz: My response to that was in his memorial volume. He did not convince me: he went to his grave thinking he's right, and I'll go to my grave thinking I'm right. In my response I said that it is very difficult to debate an opponent who combines the attributes of Albert Einstein and Dr. Seuss. I now view the debate as a semantic matter. Each of us cites a correct theorem. Nobody disputes the theorems. The question is whether you can use the respective theorem to justify the statement "this is how to invest for the long run." Theorem 1—which is the strong form of the "Law of Large Numbers—goes like this: if there are two players, Harry and Paul, and Harry always maximizes expected log, and Paul always does something distinctly different. (For example, Paul does not deviate just once and then follow the max E log rule), then with probability one, there will come a time when Harry is richer than Paul and stays richer forever. I use that as justification for maximizing expected log for investment for the long run. Eventually you will get wealthier and stay wealthier than the other guy. Paul, on the other hand, cites a class of utility functions that he and Mossin wrote about called myopic utility functions. They have the property that if you play a game where you cash in your chips at the end, and then you score yourself according to a logarithmic or power function utility, then no matter the length of the game, your action right now will be to maximize the expected value of the same power or logarithmic function. This is Paul's basis for saying that any myopic utility function is consistent with investment for the long run.

R. Michaud: I get the feeling that if you had said geometric mean rather than logarithm he would have been happier.

H. Markowitz: Everybody knows that the log of one plus the geometric mean is the expected log of one plus return.

R. Michaud: What I mean is that the geometric mean is a statistic whereas the log looks like a utility function.

H. Markowitz: Could be. It's too late to use the argument on Paul.

5 Markowitz as a Computer Scientist: von Neumann Prize 1989

R. Michaud: We talked a bit about your work as a computer scientist. Did you want to add anything to that?

H. Markowitz: No.

R. Michaud: No? OK, so let's go

H. Markowitz: Wait a minute, I want to plug one idea. There is an algorithm called the Markowitz/Van Dijk algorithm. The problem it addresses is that if you have more than three or four state variables, it is extremely hard to compute the solution to a dynamic programming problem. Previously I had investigated quadratic approximations to utility functions. In dynamic programming, if we knew what the derived utility function was, we could make a quadratic approximation to it. But we do not know what the derived utility function is. So let us just find the quadratic that works best. Mark Kritzman and Sebastien Page, in the audience, applied the Markowitz–van Dijk algorithm to a practical rebalancing problem and found that it works just great. If the person who listed my accomplishments for the Von Neumann Award were listing them now, I would suggest including the Markowitz–van Dijk algorithm.

6 Markowitz and Limited Borrowing CAPM: 87, 96, 00, 05

R. Michaud: And so let us come to the present. Now you wrote a book in 1987, republished with some new material in 2000, and wrote papers in 1996 and 2005 from some of the research. The assumption that an investor can borrow without limit is crucial to the CAPM as well as to many papers and formulas on portfolio optimization and asset management. You showed that the implications of removing this unrealistic assumption on the CAPM are dramatic. These implications include the fact that the market portfolio is unlikely to be mean–variance efficient, no linear relationship between beta and return exists, and no mutual fund separation.

H. Markowitz: No representative investor.

R. Michaud: So what is left of the CAPM? You have done some serious damage to the CAPM, everyone's favorite finance theory. Given all this damage, how else is CAPM useful besides teaching MBAs?

H. Markowitz: The genius of CAPM was that it had neat, verifiable conclusions. If you go back to Bill Sharpe's paper, he admits that there is an assumption or two that are not fully realistic. But let us not judge the theory by its assumptions. Let us look at its implications and see if they are verified.

As to the two-fund separation theorem, if you assume that everybody seeks mean–variance efficiency, everybody has the same beliefs, and everybody either can borrow all they want at the risk-free rate or they can short and use the proceeds, then all efficient portfolios lay on a single straight line in portfolio space. But, no matter the dimension of the space, two points determine a line, hence the two fund separation theorem. CAPM is a neat theory. But data suggests that maybe there is not a linear relationship between expected return and beta. One should not be surprised if there is not such a linear relationship. One does not have to ascribe this to a failure to include all financial assets in the market portfolio. It maybe just because you cannot borrow all you want at the risk-free rate.

R. Michaud: These recent results of yours concerning restrained borrowing are some of the most important I have seen for current practice. I remember how Robert reacted when he first heard you speak. He had to tell anyone who wanted to listen about your proofs. These are revolutionary results. Harry is again pushing forward the profession in a dramatic way. I wanted to bring up a topic. I used to teach some time ago, and some of the early books I used to teach with talked about something called the interior decorator fallacy of portfolio management. The interior decorator fallacy implied that few investment managers knew much about modern finance. As a result they would naively recommend low volatility stocks for orphans and widows, more normal volatility stocks for people near retirement, more risky stocks for sophisticated speculative investors, and so on. Well, the implications of Harry's wonderful new research lead us back to this world in some serious ways. So Harry, how am I going to make recommendations to my friends? What advice should I give them in terms of how to invest? I cannot think in terms of the market portfolio anymore, can I?

H. Markowitz: If your friends do not have an investment advisor, my recommendation would be to get a well-diversified stock portfolio, also get a well-diversified bond portfolio, or if that's too complicated, use a savings account instead of bonds. The most important thing is to get the right mixture of stocks and bonds. 2008 was not an outlier; it was not the worst year ever. It was tied for the second-worst year ever. It was a downward move of almost two-and-a half-standard deviations. Two or more standard deviations should happen two-and-a-half percent of the time, which is one year in 40. So investors have to get a feeling for what volatility is. They have to choose a mixture of stocks and bonds with which they will be comfortable if there is another two-and-a-half-standard deviation move. The usual mistake of the small investor is to buy when the market has gone up and they think it is going to go up further, and then sell when the market goes down and they think it is going to go down further, so they chicken out in March 2009. You want a portfolio you can stick with.

7 Markowitz and Resampling

R. Michaud: I cannot help but ask you one final question about your work with Nilufer Usmen.

H. Markowitz: I did two things with Nilufer Usmen. I wonder which one you were interested in.

R. Michaud: The one that shows how the resampled frontier Bob and I invented (the Michaud Resampled Efficient Frontier) outperforms MV optimization in simulation tests even in the presence of a diffuse Bayes prior. Harry, I have heard you say you do not know why it works better. How do you feel about these results now?

H. Markowitz: I'm going to tell you quickly the two things I did with Nilufer. Nilufer and I did a Bayesian analysis of the probability distribution of stock market returns. We came to the conclusion that daily moves in the S&P 500 (of the log of ratio) were distributed like a Student-*t* distribution with four-and-a-half degrees of freedom. This has fat tails, but if you compound 250 training days worth of these things, you get less fat tails for annual returns. The other thing Nilufer and I did together was a simulated contest between a diffuse Bayes investor and a resampling investor. The resampling investor won. I now think resampling is really a sort of empirical Bayes in which you use history to decide what your priors are. The difference between resampling and empirical Bayes has to do with reversing two integrals, which is OK given certain assumptions. So I now think I understand why resampling works.

8 The Future of Finance in the 21st Century

R. Michaud: And to conclude what about the future of finance in the twenty-first century? Where do you think we were going and what would you like to tell us?

H. Markowitz: As far as mean variance analysis, I'm still hanging in there and helping lots of people, advising them how to do it and learning from my colleagues. As far as analysis goes, one of the things that is underutilized is asynchronous discrete-time simulation. So far I have been beating that drum and almost no one has been following. This is a natural for the folks in behavioral finance, because it gives you the opportunity to build models of individual behavior, either mean–variance or whatever you want, and then let the simulation show whether the macroeconomic implications of these micro factors give you an economy that looks like the real economy. There is a long-standing dispute in many fields about the analytic versus the simulation. Sure, analysis gives you results that are general and beautiful and so on, but it is very delicate as to what assumptions you can make. If you really want to solve the problem and see what the answer is, then you ought to use simulation.

R. Michaud: At the end of Markowitz FAJ 2005, you implore the world of finance to "get on with it." At the end of our book Michaud and Michaud 2008 Bob and I complain about the large chasm that often exists between academia and practice. How do we get good new ideas into the mainstream of academic finance and professional education? What do we have to do to shake things up and make financial education more relevant?

H. Markowitz: You have meetings with smart people with divergent views who sit around and argue with each other. I'll tell you one little story. There's an article in my selected works called *CAPM Investors Do Not Get Paid for Bearing Risk*. My wife, Barbara, who learned statistics from her education in psychology, read the paper and said, "you can't give this paper at Rob Arnott's seminar without showing it to Bill Sharpe." So I sent the paper off to Bill Sharpe. Bill called back in a week or so later and said, "tell Barbara its OK, we're still friends. Besides, I'm working on something else now."