SAMUELSON INTERVIEW
(edited for clarity and readability)

Merton: I am delighted to be on this side of the desk for the first time in almost 40 odd years. I think we have an obligatory question that everyone has been asked. So you are being asked this. The first question is, what was your response to receiving the Nobel Prize and being the first American to receive it for economics?

Samuelson: My response was the common one. At 5:30 in the morning the phone rang. My wife (was) in bed next to me and said, which child has had an accident? I listened and a Swedish accented voice said how does it feel to win the Nobel Prize? I wasn't sure that it wasn't a hoax, but I said to my wife, it's okay, no child is involved. Then the announcement came that I had been named. It seemed genuine and not a hoax, and I was surprised. I think one of my reactions was, and my second daughter criticized me later when I told her about it, I said well it's nice to have a lot of hard work rewarded. She said that was a very stuck up answer (laugh).

Merton: It was clear from the citation that despite the tradition of the prize being for a specific contribution, the committee clearly could not avoid saying you have made specific contributions, each which would be more than Nobel Prize quality, to almost every field in economics. Then of course, there is your textbook. They made it clear, and I think everyone would agree, that you are perhaps the last truly general economist in making contributions to every field. If we think of contributions to the special field of mine -

Samuelson: You are talking about finance now?

Merton: Yes, finance now. You may recall that 20 years ago, I took note that all of your most significant finance papers had been produced

after the age of 50, therefore negating the purported iron rule that scientific productivity declines with age beyond a certain point. I have since noted that you have added 26 more publications since that time, just in finance. So I think you are continuing to provide additional data along those lines.

Samuelson: Are you saying that when I once said in finance I am only a Sunday painter? That isn't quite true. (laugh)

Merton: Yes. That's exactly right. Or, we all wish we could be such Sunday painters (laugh). We will come back to that element later, I hope, in these questions. But turning to finance, I have my favorites of your papers and your contributions. Can you tell us what your favorite contributions are?

Samuelson: Well, all brain children are equal. Some are more equal than others. Almost every one of those papers was directed toward a prior formulated problem. For example, about the time you came on the scene, I wrote about what the time profile of risk taking should be, depending upon whether you were going to retire and die one year from now, or retire and needed income 30 years from now. I wrote the paper to work out what was rational, because, without argument, everybody assumed that as you got older you should be more conservative in your equity tolerance. They seemed to believe, because all economists have a smattering of classical statistics, that with large numbers comes greater security, lower variability. So, if you have 30 years to retire, you are going to have a sample of 30 quasi independent items. Therefore risk erodes as the horizon ahead grows.

Well, there is an obvious exception. By that time, we all knew about constant relative risk aversion. When you use that utility model for your postulated hypothesis, a rational investor facing truly white noise would do the same thing when you have one year to go as if you had 100,000 years to go. So, this was a misunderstanding of limit laws in mathematical statistics as being applicable to the finance problem.

Every one of the problems I wrote about was already in the air. Your father would have understood that. The doubletons. The tripletons. I have never been a believer in the single great man theory of either heroic generals, the Napoleons, or heroic scientists, the Einstein's and Newton's. We are all different, but the scientific quasi self-correcting process is a group process. That particular paper came out with a rather short delay time, simply for the reason that Seymour Harris at Harvard was a pal of mine. He was editor of the journal, and he gave it fast treatment. Two or three other people at about the same time did much the same thing. They got credit for it, and I got credit for it. But to me, getting credit for something isn't the goal. It is answering the question. In that circumstance, I felt that the first pass at the problem was a failure. I had not proved that you should so something different over time.

That has been a recurring problem, of course, and some very considerable progress has been made. I am glad to see that the name Samuelson is connected with that process, but I am not referring to Paul A. Samuelson. (laugh) I am referring to William Samuelson who, with Zvi Bodie and your humble servant, (laugh) developed a very important point. It is not the statistical law of large numbers that makes some sense of the customary dogma. It is the fact that you have the option of working harder or working less hard if your optimistic guess goes wrong. In that model, when you add the variability of your supply effort over your life cycle, there is some truth to it.

But if you ask, which is my favorite in the finance area, it is the paper I wrote for the retirement session of Jim Tobin at Yale. Jim Tobin was a beloved friend and really my role model. I tried to be as righteous as he was and as judicious as he was. In the paper I buried the usual assumption in finance, that you have a random walk. By the way, I have never believed in the true random walk. When I proved that anticipated prices will fluctuate randomly, it was not that if you wait long enough this pea that I have in my hand will sell for as much as that Cadillac that you are driving. That is what happens with a real random walk. It was the Martingale process that I was elucidating. That is still not well understood in the field of finance, and I have actually written a trilogy of papers on that particular topic.

I was so early in the field that I was able to do a lot of coinages. For example, the European option, or what we call European option in contrast to what we call an American option. That's my nomenclature. You know the reason for that.

Merton: I do, but why don't you tell us.

Samuelson: Out of idle curiosity, I developed an early interest in option pricing, I subscribed to the Freed RHL Service. I think I paid $150 a year and bi-weekly I got a report. It would say RKO warrants produced, on our recommendation, a profit of a thousand percent. Then they would give you a list of hot items. And what was really a hot item was one when the stock went down by 50 percent, you would go down by only 10 percent. And when the stock went up by 50 percent, you would get a result which was 250 percent. That's a bargain you don't want to pass up.

I used his recommendations, and I used newspapers and compared results. One of my colleagues, Dick Eckaus, would borrow this survey from me. One day he said, why do you take that survey? It's almost always wrong. I said, well, it costs me $150 a year, but if I get one good idea from it, that is very worthwhile. I thought about it for 24 hours, and when I saw him the next time I said, I want to amend what I said. When you get one good idea ***net***, because it's recommendations I have had from optimistic brokers. I've got a new IBM for you. By the way, in the old days "an IBM" was considered something which couldn't go down and could only go up. (laugh) Those new IBMs recommended to you, that's the way you really lose money (laugh).

But to go into the substantive result, I took the simplest process I could imagine, or what I called red noise. White noise is truly a random walk. The future is independent of the past. Knowing that the stock rose yesterday has no influence on the probability distribution of what it will do percentage wise between today and tomorrow. That's white noise. Zero serial correlation coefficient in statistical parlance. Red noise (is what) I called regression towards a mean, where there is a negative serial correlation through time. So that if things (went) up a lot yesterday, today you should bet that they won't go up as much as they

would normally do between today and tomorrow. That is red noise. Blue noise is the opposite. If things went up yesterday, they will go up, in a probability sense, more often tomorrow. The baseball announcer has it, of course, conveniently all wrong. Ted Williams is due to hit because he hasn't hit for seven days. That's red noise (laugh). Ted Williams is hot. He's sure to hit tomorrow because he's hit for seven days. That's blue noise. The better description of an efficient market with random movements, which is white noise, is in between.

Of course, in my 19 - what was it 69 - article? - where I showed there is no rational dependence with the length of the horizon; that was based on white noise. So I said, well, what will happen if we have red noise of if we have blue noise. There are three different cases. To my pleasant, but not complete surprise, it turned out there was a systematic effect with red noise. You did one thing, the common thing, if you were a certain kind of individual. I won't go into the intricacies of it, but when you surprise yourself, you please yourself (laugh).

Merton: You mentioned the story of the European and American options. I seem to recall you telling me, as you have counseled many times, that if you are going be in an area you should do theory, but you should still go out into the field and find out what the real world is like in that context. I seem to recall that you did some investigation earlier on with put and call dealers.

Samuelson: Yes. I would fly down to New York, and in those days the state of Illinois regarded options as illegitimate gambling. So the Wall Street Journal of Chicago had no option prices. The only option prices were in the New York edition. The put and call dealers, who had a little trade association, were mostly European refugees in dingy offices outside of the high rent areas. I would go there and ask my innocent questions. A typical question was, Mr. Godkin, is it true that if I buy a put because I think a stock is going to go down, and I am correct in my inference, and the stock does go down, and I make a profit on my put, the profit which came from the drop in a stock can be treated as a long term capital gain if I've held that position for over six months or over one year? He said absolutely. I said well, you know if I sell short that's not true. He said, I

know that, but this is different. I said are you sure? He said absolutely. I said are you telling me that if I wrote a letter to the IRS and asked for a private letter, they would confirm that is the case? He said why would you want to do that? (laugh)

That's just one tale. But talking to still another one of those guys, he said, I don't understand. Why are you here? What are you up to? I said I am trying to study the science of option pricing. He said, that's hopeless. You will never succeed. I said why not? He said it takes a European kind of mind to understand this mystery. (laugh) So in revenge, I gave the name, European option, to the simpler option (laugh) and reverse for the American option.

Merton: We have talked about the price -

Samuelson: Excuse me. Can I also say that I have an interest in an article which, to my surprise, is not included in my collected papers? The general rule on my collected papers was at the beginning I thought I will do it selectively and just collect the best of them. But the particular paper on a statistical topic I thought, I am going to omit that because it was only written to correct somebody, and in a way, to show that I was smarter than he was. I told Ed Kuh, my late colleague, and he said, hey Paul, I think that's one of the most interesting papers you ever wrote. So I thought well, who am I to judge? I will publish them all. But the review I wrote of Beat the Market, by Thorpe & Kassouf is not in the collected papers. I don't know whether I should blame you…

Merton: I think you need to blame me.

Samuelson: Is that Volume 3? No, I blame myself. Maybe I thought it was too ephemeral. Actually it's got a lot of verve, and in a certain sense, writing that paper may have kept me from discovering the Black-Scholes formula. Also, some content of the paper may have a remote bearing on some important events in history, such as Long Term Capital. (laugh) I don't want to touch any nerve, but the leit motif of my review was there is no such thing as a perfect hedge. The premise of the book was it is very easy to make money. For mergers and stuff,

just do this and that, and it's money for jam. Well, it's never money for jam. There is always an eventuality that has relevance, where you can always lose money in the real tangible world.

Dr. Samuel Johnson was asked, how do you know this table exists? He said, I know it this way, and he kicked it. I refuse to go the limit to zero time, which is the essence of the Bachelier-Weiner-Ito-Merton (laugh) instantaneous stochastic calculus. I don't think that Myron Scholes or Fischer Black knew that calculus, but by brilliant inference they guessed at it. We are all sleep walkers when we are doing original work. Later, everything is cut and dried. So, I should have been more eclectic.

Merton: On the issue of what you could call a near miss in science, it doesn't escape anyone looking at your 1965 warrant paper, that the Black-Scholes formula and your formula are exactly the same except your beta is equal to the interest rate.

Samuelson: Well, not everybody sees that. Young Robert did. (laugh) And actually, there is a sentence where I have a certain hedge, and I say it creates a very low variance, but it didn't create a zero variance because it was a finite time thing. I don't mind, I prefer not to fail in solving a problem, but I don't mind in failing. I don't even mind if, in my past, I have a conjecture which is wrong. What I do mind is staying wrong. And that is work in itself.

Merton: You mentioned your horizon work, and in terms of the impact of the finance science on practice, you wrote several papers. The first was in 1969. So that is 30, 36 years ago. We have topically, in recent times, Social Security, private accounts, and 401Ks. Those seem to be mainstream big issues which were addressed then and are still right on the front page today.

Samuelson: Actually, my first published paper is on life cycle income and maximizing utility for a single consumer. I did it for the purpose of showing that if you had independent utilities, you could - by diddling the interest rate and

observing the revealed preference of the life cycle planner - work out what had to be their utility. You know where I got the idea? I got the problem in Senator Paul Douglas' class in intermediate economics at the University of Chicago when I was still in my teens. I got the insight on how you would be able to do this from what I took to be the well known understanding that, from the way people react to bigger or smaller bets, you could determine what their, Arrow-Pratt measure of risk tolerance and risk aversion was. In that first paper I don't even mention the probability aspect of it. But it was the probability aspect of it that gave me the lever to handle the non-stochastic treatment there.

I think that some of my most interesting papers in this field were before there was much of a literature in this field of finance. For example, A Fallacy in Interpretation of Risk was published in a very obscure continental publication. It probably has hundreds of citations, and it still comes in where I get back to the story. Stan Ulam is a famous mathematical topologist. He is an immigrant to America from Poland, and he is the person who discovered for Teller the hydrogen bomb at Los Alamos. He's a pal of mine in the Society of Fellows, and he said to me one day, I'll tell you the definition of a coward. I said, what's that? He said, if you give him a two to one bet, but charge him only one to one on the game, he won't bet. He's a coward. I said Stan, you're not quite right. It depends on the size of the bet. Because if you are a risk averse person, the dollars you win are not as meaningful and worthwhile to you as the dollars you lose. Years later I told that story to the head of my department, whom I didn't name, and I proved the theorem that you should not take the bet. In fact, I offered him the bet, and he said, no because that is too much money. It's a good bet in money terms, but I would feel it too much. He said - and this is where he corrected me you might say - if you let me make that bet a thousand times, then I'll do it. Now, he was depending upon the legitimate Law of Large Numbers. I said no, if you won't take it one time, I can prove a theorem that you shouldn't take it any number of times. It doesn't work that way. That was the first article.

There is great misconception about the Law of Large Numbers. Insurance does not work by taking on more risks. Insurance works by taking on more independent risks and then dividing the involvement among more and more people. If you double the number of ships you insure and, let's say you

quadruple the number of insurers, then you will have actual risk reduction. It's both of those elements. An article I wrote about that wasn't particularly in economics, but it has had a long history of discovery, and rediscovery, and variations, and so forth.

Merton: I recall you published both your warrant pricing paper and your paper showing that properly anticipated prices fluctuate randomly in the same issue.

Samuelson: By the way, it was a very obscure local journal, which doesn't even exist any more. (laugh)

Merton: I think it was the Industrial Management Review.

Samuelson: That's right.

Merton: So that was quite a journal to have both papers in it. You know, of course, that work framed what became the efficient markets hypothesis, and it has had a profound impact on practice. In this very office as your assistant I saw all the piled yellowed pages from seminars you had given. So I know that you really were involved both with that topic and with the warrant pricing topic from about the mid-50s, or nearly a decade before the papers were published.

Samuelson: By the way, I published in recent years a paper with co-authors that clears up an open problem that I couldn't solve in the first paper I ever wrote. It is part of the mathematical literature now. But the computer helped because usually mathematica is very much connected with finance theory. What does it mean to say who maximizes the expected value of a utility of wealth outcomes? That is the problem of an associative mean. My first paper was grappling with that problem. I knew there was some partial differential equation, which had to be satisfied by the observable partial differential equation between the gain and the loss. It ended up a third order of partial differential equation with about 300

coefficients in it. I am still pretty sure that can be simplified down beyond that problem. So a problem isn't something that you have in the past, it's like a child, or a perpetual care giver. You've got it with you all the time. (laugh)

Merton: I have some questions for you regarding the history of science, and I know all your freight trains of…

Samuelson: By the way that freight train is your father's usage.

Merton: I see.

Samuelson: Yeah. When I say the Ramsey-Solow-Smith-Jones theorem. Yeah.

Merton: Well, that gives us a quick path of your thinking of the history. For those reasons, I would like to ask you a couple of questions about your early work. We were talking about the efficient markets, or martingale paper. I know you gave that paper at a seminar at Carnegie, among a number of places, in the 1950s I think, because I saw the yellowed sheets. I wondered whether perhaps in the audience was a graduate student named Jack Muth. Do you think that was possible?

Samuelson: Well, yes. That's possible. This is part of the genesis of the rational expectations paper, right?

Merton: Yes.

Samuelson: Muth and Lucas, and so forth. And the answer is yes. But I think that it could also have been inferred from an earlier paper that is called Stochastic Speculative Price. I think that paper is in the third volume of my collected papers. It is also in the huge page format of the Proceedings of the National Academy. Since these papers are reproduced by photo offset, it is only two or three pages. It has to do with what rational theory of pricing would be for a crop which is subject to weather variations and to carry over. It was part of a

generalization of an AER paper I wrote on spatial arbitrage paper. This probably would be in the 1950s, or maybe even earlier. I deduced an ergodic probability distribution, (by assuming that) the time series of weather was stationary. But I failed to deduce what was believed in the literature. I don't mean that it was wrong in the literature, that you can carry a very small amount inventory at a negative price, because there is some convenience in the carrying of the inventory. But in that paper you certainly had the notion of a warranted price pattern such that, if it prevailed, if you reran the experiment, it would not differ the second time in its probability aspects.

Now that is the kind of rational expectations I believe in. I do not believe in the more common version, which I associate, maybe unfairly, with Lucas, Sargent and others, that we should assume in macroeconomics that there is a wisdom of the crowds which is working out a Darwinian model of rational expectations. To me, the truth in that doctrine is that you can't perpetrate the same predatory mistake against somebody endlessly. You fool me once, shame on you. You fool me twice, shame on me. People do learn not to be tricked by the same trick again and again. But I do not think that we get out of a pre-Roosevelt-Hoover depression by activism in fiscal policy and/or in Federal Reserve policy only because people have been cheated and surprised. I think it happens for very rational reasons. That is what I believe, but that is a far cry from what, at least early on, Lucas and Sergeant derive from Muth. They could not legitimately derive that from my stochastic speculative price for this commodity.

I may say that I was always influenced by earlier writers. And I always thought that Holbrook Working at the Stanford Food Research Institute, who did for 30 years important papers on wheat prices, futures prices, and so forth, that he is an unsung hero who still gets too little credit.

Merton: It's been four decades since those papers, and they are still in the lead. I've got one other topic you wrote about that has been in and out, and now it is back in again. That is the work on what you originally called the Pareto distribution, and then you renamed it.

Samuelson: No. Wait a minute. The main renaming, (laugh) that's been done by ?? He has reproduced some of his early papers. He has a book, you know. Did you see it?

Merton: Yeah.

Samuelson: It's a recent book. He's kind of cut out from it. (laugh) But I was interested, as was the young Chicagoan. Yeah. He was interested in that problem, and I was interested in it. I was able to take the portfolio problem for log normal distributions, and show that if you don't have finite variances, you have a similar problem that is solvable. Is that what you're talking about?

Merton: Yes. I think your work was probably the closest to doing anything in terms of getting some decision rules with those distributions.

Samuelson: I went on a family vacation to a Japanese restaurant in Cambridge, Massachusetts, and there was a physicist from Lincoln Labs who had an interested in that topic. He claimed that he had by graphical numerical methods, solved the optimal, the Black-Scholes formula, for those distributions. Since I've never heard anything about it. Do you believe that? Do you think it is solvable?

Merton: I don't know how to do it, and it's rare to say that you can. I think it has some very deep complexities in it. And of course, by using those distributions, you throw out most of estimation theory and optimization theory. So that's a pretty big price. I guess I was looking to ask what you think about that challenge between—

Samuelson: I'll tell you what I think. I think that most experimental lab work is leptokurtic. It's not Gaussian, and the fourth moment is large relative to the second moment. This is technical jargon. But I don't believe that Mandelbrot or anybody else has shown with any persuasiveness that we have to go the whole way to infinite variances. For example, when I gave a proof that with symmetric independent probabilities, you should put 1/N of your wealth into each of the

securities, do you know what the answer to that problem is with the Cauchy distribution? A sample of 20 Cauchy independently distributed variables has exactly the same distribution as a single one. To me, that means that the expected utility of any concave function blows up to infinity. There is no concave utility function which could handle the Cauchy distribution. It's an important name, a French name. (laugh). I am just blocking out of it at the moment.

So what I believe is that the actual processes in different commodity markets, different futures markets, and different stock markets - you have written on this - these processes that are cascaded on each other. But they still have finite variances. I can remember a Chinese trader telling me that Black-Scholes doesn't do too badly on stocks, but it does badly on certain commodity prices. Of course, Black-Scholes in the familiar form is just the lognormal case. I don't know whether they worked out any extensions. Isn't there a smile or something?

Merton: Oh, yes. Black-Scholes, other than the formula, it is really an approach, an attempt-to-replicate approach. Yes, absolutely. There's been every kind of variance, stochastic volatilities and jumps, and all the rest. It has gone a long way. I want to ask you another history of science question.

Samuelson: Yeah.

Merton: Can you share the whole Bachelier story with which you came to your warrant pricing?

Samuelson: Well, of course, before Bachelier there wash the RH (laugh) Jimmy Savage. He was an eminent mathematical statistician, then at Yale, and he sent a number of economists a purple printed postcard, asking who is this guy Bachelier that seems to have written in 1914, a French book on the theory of speculation? Do you know anything about him? I thought, well yes, I know something about him because I was talking to Stan Ulam years ago about that, but I will look (it) up. I did not find the 1914 popular book in the MIT library, but I did find his Ph.D. thesis written at the Sorbonne, University of Paris, under the

direction of a very great French mathematician. I'm blocking out his name. Poincare. It appears that Poincare did not participate much in it. So this was really a clever thing by him. I persuaded Paul Cootner, I think, to translate it and reproduce it in the collection of articles he was doing. So this was a very important discovery. There are a few other still not very well known earlier French people. Also you know that in the Amsterdam 18th century handbooks of finance - I think a lot of them may have been refugee Jews from Spain - there are a lot of the square root approximations and deltas that are old hat now in financial engineering.

Merton: You have mentioned again the importance of understanding practice. Of course, finance has had the great good fortune that finance science has really had a huge impact on practice.

Samuelson: You know what helped in this?

Merton: What?

Samuelson: A market to be viable has to build up volume. Tax straddling started the currency market. If there hadn't been that, I think it would have been a much slower process of accumulating the necessary critical mass, because these are not constant returns to scale processes. The markets themselves are increasing return processes. It was a shibboleth that no new market will ever fail, but when they tried to have a market where you insured against changes in macro variables, you couldn't get two sides. It takes two to tango.

Merton: And you have certainly been involved. Are there any of those, the developments, I think of commodities… and the various students you have over the years have contributed, any of those that—

Samuelson: Well, I'll just tell one story. I was asked to supervise a senior's honors thesis of a Sloan School student. I didn't know him from Adam. His

name was Hillenbrand. I did supervise him, and he got a good grade. I think he got a job at Smith Barney or—

Merton: Salomon Brothers?

Samuelson: Yeah, or Salomon Brothers. Years later there was a leak from Salomon Brothers on salaries, and Hillenbrand was top man with a $22 million bonus that year. The treasurer of MIT, or somebody, said to me, hey didn't you supervise his thesis. I said yes. He said, don't you think you ought to write him a letter of congratulations and remind him of the great good old days at MIT (laugh). Well I took that to be a hint. So I did. But I sent for his thesis and looked up what its contents were. It hadn't made a big impression on me. And I did write a letter. I said, I see from newspapers that you are famous now, and I have to tell you it's not everybody who can disprove his own thesis, because in your thesis you show that you couldn't make money using bond futures. (laugh) But I never got an answer from him at all.

Merton: Oh really?

Samuelson: I mentioned it later to the treasurer. He said, well, that's all right. He made a good contribution to the professorship.

Merton: Oh. Well that's great. What do you think going forward?

Samuelson: Well I'll tell you. I have questions. The whole existence of these elaborate derivatives is a marvelous opportunity to spread risks and share risks and so forth. In fact it's also a marvelous temptation to take on risk if 2-to-1 leverage is what you get by following the letter of the law on margin borrowing. By getting a good financial engineer to help, you can achieve leverages of, not 2-to-1, but 10-to-1, maybe 50-to-1. And most people in the industry don't really know what their risks are. I knew because I was a trustee of TIAA-CREF. A lot of the heads of firms in the city were fellow members. And they thought they had new profit centers which, without risk, were bringing them in marvelous amounts.

That is a profound misunderstanding of what was going on. So it certainly would take lots of difficult monitoring to be sure that we end up with a more stable situation rather than a less stable situation in times of crises. I should also say that in my own investing - which I never discuss with anybody and which is a very unimportant part of my life and which involves very little application of energy and time by me - I have not been able to find a remarkable congruence between making money on things where society benefits, and losing money on things where society is going to be hurt. On the contrary, if you take, let's say, the new Warren Buffett, or Warren Buffett-Munger methodology, where you buy a good company with a franchise. Apparently anybody can recognize what is a good company with a franchise. When I sat at Schumpeter's knee at Harvard, I learned there are no franchises under capitalism (laugh). The top rooms in the capitalism's hotel are always filled, but they are filled with different people. (laugh) This is a quasi belief. If there is such a thing as a permanent oligopoly that is able to coin money if it just keeps its nose clean. Well, I don't think that is so.

To take a different case, it is good to get the incentives of the decision makers in a corporation in alignment with the interests of the corporate stockholders. I won't go on and say to also be in alignment with the interests of the employees of the company, or with the sovereign democracy of societies. But at least get those involved. Well we've had an experiment, which has increased enormously the incentives of top CEOs. We are sitting here on the third floor of what was Lever House, the American headquarters of Uniliver, Leaver Brothers. Shoot a bullet just above us, and you'll hit the dining room too of our faculty club where the CEO who had the highest income in America sat. He has a nice office with a beautiful marble fireplace, and that fireplace could be used with fire every day of the year, because it had air conditioning that took away (laugh) the heat that was created by the fire. Well Phil Countway, who is a bachelor, received a thousand dollars a day, which is $365,000 a year for a top American CEO. I don't know the exact numbers, but I think it would be fairly accurate to say that that was 40 times the median annual income of a Leaver Brothers employee in America. Do you know what that number is today? More than 400 times. Now it could be that by virtue of a Darwinian freak, the new

generation of CEOs are ten times as smart, are ten times as conscientious, and work ten times as hard for the interest of shareholders, but I doubt that. I don't know whether I am correctly quoting Michael Jensen, who is your colleague at -

Merton: Well he's retired but—

Samuelson: Yeah. Now I believe it was his published opinion that we were moving earlier toward a greater congruence between this shareholders interests and management. I believe - now this is all third person hearsay, and he probably has written on this subject - that he has changed his mind. That is a very rare thing in an economist. It is even rarer in an economist who is over 21. (laugh) I think he has changed his mind because he has come to believe that under, what I'll call the Pitts - what was Pitts first name?

Merton: Walter Pitts.

Samuelson: The head of the SEC. The first designated head of President Bush. In a speech as the new head of the SEC, he said, I am going to run a kinder and gentler SEC. He got his job because he had been counsel lawyer to the four biggest accounting firms. He is regarded as an ignominious failure because of Enron, and what is it Gencom?

Merton: World Com.

Samuelson: World Com, and so forth. That loosening of the remuneration of people at the top, instead of making the interests more congruent with that of the shareholders is absolutely 180 degrees wrong. The smart thing to do is to lie about your true earnings within the perimeters of the new regulators and law, and sell out while the confidence game is still running, and then laugh all the way to the bank. You can't imagine anything more destructive of true long run interest. So that is, to me, a sad empirical finding. I have always known that a good trader - and there are very few. My definition of good trader is somebody who over a long period of time controls the down side and then comes up with the upside. I

suppose I know a few hundred people, and I would say with little time I might come up with ten names of people whom I consider to be pretty good traders. But there is no relationship between what their gains are and their contribution to the increase in total factor productivity of the society they live in. They are people who are a little quicker than others, and that quickness is where the money comes in. They also are a little bit shrewder. Now there may be one or two people who have hidden methods, which by definition I will never know about, who coin money repeatedly. There is something called the Medallion Fund Renaissance—

Merton: Yes. That's silent.

Samuelson: - with a very long term record. There are some heavy hitters in the pre-hedge fund area, but they were hedge funds from the beginning, Bruce Kovner, Paul Tutor Jones, and so forth. But it's not a simple thing that laissez-faire necessarily creates for the good society. The most optimal thing, I believe - I'm centrist, I'm an ecclecticist. - is that with reasonable regulation, not zero regulation, the performance of a mixed economy can beat that of any planned dictatorial utopia or of any laissez-faire economy. I grew up for the first 15 years of my life, in essentially a pure capitalistic economy, with very few unions, and no onerous federal or state legislation. There was a nightclub singer, Sophie Tucker, who in her act would say, I've been rich, and I've been poor. Believe me, rich is better. Well, I was in the pre-mixed society and in the mixed society, and believe me, the mixed society is better. (laugh) However, and here I blame no politicians, I blame us, the electorate, the farther we get away from the Great Depression, the farther we get away from "the good necessary war, World War II", - which was run in a very efficient fashion by the U.S. Government, in comparison with say the, either the Russian or the German collectivist governments.

Altruism seems to be eroding away, and there is much more of the libertarian, me, me, me philosophy. It shows itself, of course, in savings over consuming patterns, and so forth. But you can't dissolve the electorate and get a new electorate. (laugh) The paradox to me is that I get, in my position of modest

affluence, hand out after hand out in the new order that we're in. And the people who work for me, and a nurse who helps me if I need a nurse, a gardener, and so forth, they are the ones who are giving me the handout. Now it's not as if we are getting them a more efficient mixed capitalism. It is a distinct possibility that estate taxation will be eroded away to zero. I think that is not a sociologically good thing for the future society, but I've only got one bullet (laugh).

Samuelson: We haven't discussed the innumerable wasted time against maximizing growth. (laugh)

Merton: Well, that was under my remarks, and I would like to bring it up because you touched on a number of cases in which I think that was one of the subtexts. Of course, the main text is your work, particularly in finance, because it works so well in terms of avoiding paths of error. And that is certainly a prime one.

Samuelson: Yeah. And in that connection I should mention my article with only one syllable word.

Merton: Yes. Mono. We have talked about your various contributions. I have noticed that one subtext has followed all the way through. You alluded to it with respect to the issue of people misunderstanding the role of insurance and that the subdivision of risk is the only way to really effect the Law of Large Numbers. But in general, you have had this goal of helping us to avoid paths of error. While that is not usually the highest reward in science, it is often one of the most important. It is one I grew up on as a student here in your office. And it seems to still be going on. It is that the only rational thing for the long run is to maximize the geometric mean. Maybe you might tell us a little of that story.

Samuelson: Well, this is a belief which became popular, and it spread. But it is peculiar because it came from the mathematical fraternity, the non-economists experts in statistics in probability. I guess some names would be Kelly, Breiman,

and Latane. Their argument was that over any horizon you should maximize the mathematically expected growth rate of the portfolio. That is very much a first moment approach that does not take account of variability. It boils down to using as your Laplacian utility, log of wealth, as your test of risk aversion. Now it is for a Laplacian who does not have log utility, which maximizes geometric means, there are Laplacians, perfectly dues paying, who are more risk averse than that and who maximize the harmonic mean of the certainty equivalent. For them it is folly to follow this particular practice, because if you work out every outcome of the game, they will find that perverting their true preference for the harmonic mean and accommodating to the log rhythmic mean, will mean every loss and every gain that they are behind in their ultimate harmonic mean, which is their own stipulated guide.

Well I suppose I have had to write at least half a dozen articles, as a new guy comes up and becomes converted to this. The last article I wrote on the subject, was written in words of one syllable only, with the exception of the last sentence, which read something like, and now I am done, and I have done what I did with words of one syllable only, but for syllable (laugh).

Merton: A self-negating illustration?

Samuelson: Yeah.

Merton: Well, those elements are there. You were referring to all the changes that have taken place. And you have been an observer, both as a scientist and as a practitioner in the markets. What would you say today to the investor, to the household, to the person on the street? There has been so much back and forth. Would you have any guideline prescriptions?

Samuelson: Well I think I could nominate for discussion social security reform. It was known from the beginning by experts like me - I was in from the beginning - that the original set of rates could not be written in stone, because at the beginning of any non-voluntary scheme you are swimming with money. Then

later, when everybody is being covered in a steady state, it is a different situation. When the 50th anniversary facsimile volume of my elementary textbook came out, that we printed in 1948, I had to write an introduction for it. In the introduction I said, well of course I had to reread it, and I was really surprised at how good it was. (laugh) I was particularly interested to see sentences like, of course later on there will have to be an adjustment to these rates in social security. The modern debate is, should there be private accounts, should equities be included? A good case can be made that equities ought to be part of any prudent person's portfolio for life cycle investing. If that is true, why should that not be true of the nest egg which is being organized and run for all of us in proportion to our proportionate shares? I do not take the view that equity investing is so risky that nobody's life cycle savings should rest upon the risk of their going down. If it is true that there is an unexploited opportunity here for society, then the U.S. should issue bonds and should take the proceeds of that, as custodian for the, nation's citizenry and put it into equities. People who have had lower total life earnings will get lower prorated returns. But part of that will be a little bit larger because of the equity part of it. There is a further argument for that because only the government - which was here before I was here, when my parents were here, and will be here when I'm gone and my grandchildren are here - it can do a certain amount of intergenerational risk sharing. So that is the way the good system, say a la Professor Modigliani, my late colleague, should be run. Now you have to have certain guarantees - which have actually been realized in societies like Sweden - that when the government puts its finger in the pot, it doesn't start saying well, we'll give this to faith-related organizations. (laugh) Or, we in the government think that anything that involves alcohol or big gas guzzling cars, should not be in the portfolio because we disapprove of those functions. There has to be some way of insulating. Union-run pension plans have not, notoriously, been wisely run. By the way, a lot of firm-run pension plans have not been so wisely run either (laugh). So there are inevitable risks as part of production, as part of being in a solar system, that should be accommodated and adjusted to. And I think that equities have an important role, both for the life-cycle saver, and also for the development of the society. And I think most of the economic problems are very manageable, if you come into

court with clean hands. This isn't true of abusive drug problems, or rebels without a cause in the suburbs of terrorism geopolitics. I'm not smart enough to prescribe for them. (laugh) But there are a lot of simple things that can be done in economics. For most people it is diversification and low turn-over in your portfolio. You don't hit very many homeruns that way, but you don't have lots of strike outs either. I have been at MIT for, well 60 years, and in the course of that time a number of colleagues have died and left a spouse, usually a widow. And so to my door have come a few widows over years asking for my wise advice. It's not business that I like to have. (laugh) But I give disinterested accurate advice. And I'm actually almost at a loss ever to say what a simple defensible strategy would be. Have the S&P 500. Have more index funds outside of the core of the S&P 500. Have European shares, and so forth. But it's very hard to get advice, and I know that people who don't have the simple knowledge and beliefs that are quasi optimal have to pay for it. If you haven't got a friend who will talk you out of your depression, you've got to pay a psychoanalyst and a pharmacist to do it. (laugh) But in a lucky country like America, it's really hard for things economically to go seriously wrong, unless you do crazy things. Mussolini could do some pretty crazy things. There are plenty of examples. I don't believe that economics is the dismal science. That may have been true when people were reproducing in caveman times and the Darwinian process was red in blood and tooth. But we are lucky now. I can't say the same thing about a few places in Africa and the Mid-East and so forth. But it has been an amazing epoch since 1950.

I think that's enough, huh?

Merton: Absolutely. Well, I thank you so much.

Samuelson: No. I thank you.

Merton: Sitting here in your office - if you will permit me - it reminded me when I was here as your research assistant, and you were fond of a

story you would tell about, I guess it was Dickinson, the mathematician at Chicago. As I recall the story, and I know you'll correct me -

Samuelson: Yeah.

Merton: - it was that he used to play bridge in the afternoon.

Samuelson: At the Quadrangle's Club…

Merton: At the Quadrangle's Club.

Samuelson: Yeah.

Merton: And then a colleague said—

Samuelson: No a student said.

Merton: A student said.

Samuelson: Professor Dickson, how can you waste your time playing bridge every afternoon at the Quadrangle's Club? And he, said well young man, if you thought as intensely and carefully between 8:00 and 12:00 as I do, you could play bridge. (laugh)

Merton: So much for the story of hard work and well earned play.

Samuelson: And tennis.

Merton: And tennis.

Samuelson: Yeah.

Merton: And tennis is, of course, for you. But you see, you too have done that and when I was here as your research assistant, we were working on a problem in warrant pricing. You took off, as you often did in the afternoon, to play tennis. I thought that was perfectly appropriate. I kept here working. But I may remind you that as I was struggling away on one of those partial differential equations, I got a phone call from you. You had come off the court, I guess between sets, to say you had thought about it, and here's the way you think we ought a do solution. So I just thought about that sitting here, and I said, well, you too, like Dickson and much the same story for Hardy with cricket, you work very hard. You well earned your play, except you do it with your own twist. It appears that even when you are at play you are at work.

Samuelson: Right. Now did I not play with your brother-in-law?

Merton: Yes you did. He tells that story to this day.

Samuelson: Right. Yeah. Well, it's been a good life. I've always been over-paid and under-worked because my work isn't work.

Merton: Yeah.

Samuelson: Yeah.

Merton: Well, thank you. This has been great.

Samuelson: Yeah.