AN INTERVIEW WITH ROBERT J. SHILLER

INTERVIEWED BY

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MD Interview

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A recent article in The Economist magazine divided economists into "poets" and "plumbers," the former articulating radical new visions of the field and the latter patiently installing the infrastructure needed to implement those visions. Bob Shiller is the rare economist who is both poet and plumber. Not only that, he is also entrepreneur and pundit. His work has fundamentally changed the theory, econometrics, practice, and popular understanding of finance.

Starting in the late 1970's, Bob boldly challenged the prevailing orthodoxy of financial economics. He showed that financial asset prices often deviate substantially from the levels predicted by simple efficient-markets models, and he developed new empirical methods to measure these price deviations. In the early 1980's, Bob went on to argue that economists need a much more detailed understanding of investor psychology if they are to understand asset price movements. He pioneered the emerging field of behavioral economics and its most successful branch, behavioral finance. At the end of the century, Bob articulated his vision of finance in a wildly successful popular book, *Irrational Exuberance*. He became so well known that TIAA-CREF asked him to appear in a series of full-page advertisements in the popular press.

Although Bob does not believe that investors use financial markets in a perfectly rational manner, he does believe that these markets offer great possibilities to improve the human condition. His recent work asks how existing financial markets can be used, and new financial markets can be designed, to improve the sharing of risks across groups of people in different regions, countries, and occupations. He has explored risk-sharing possibilities not only in journal articles, but also in business ventures and a 2003 book, *The New Financial Order: Risk in the 21st Century*.

It was a great privilege for me to interview Bob Shiller. Bob's arrival at Yale when I was a Ph.D. student there set the course of my career as an economist. Bob reinvigorated the Yale tradition of macroeconomics, with its emphasis on the central role of financial markets in the macroeconomy and its idealism about

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FIGURE 1. Robert J. Shiller.

the possibility of improving macroeconomic outcomes. First as a thesis adviser, then as a coauthor, mentor, and friend, Bob showed me how to contribute to this tradition.

The interview took place at the 2003 annual meetings of the Allied Social Science Associations in Washington, D.C. We met in a hotel suite, ate a room service meal, and had the enjoyable conversation that is reproduced below.

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Campbell: Bob, I'd like to start right at the beginning by asking you when you first encountered economics. How did you get started with it?

Shiller: The earliest memory that I have is Samuelson's *Economics*, his introductory textbook. My brother is four years older than I. He took an introductory economics course in college and was assigned this book. He brought it home on a college recess when I was a freshman in high school; I happened to find it and read some of it. I discovered that elementary economic analysis has great power when applied to the major problems of the world and this discovery was an unexpected pleasure.

It may seem odd that I would read my older brother's textbook. I have always been a voracious reader, and whatever is around . . . .

Campbell: You would just pick up whatever is there?

Shiller: My mother used to tell the story about when I was in the early years of elementary school. She had a minor foot problem, and went to the library to get a book on care of the feet. She left it on the table and I read it.

Campbell: I'm surprised you didn't become a podiatrist.

Shiller: I read just about everything, though not all things made such an impression on me as economics.

Campbell: Did you think then that you might become an economics major in college?

Shiller: No, not until much later. The formative experience for me was more with mathematics. When I was 14 years old I had a geometry teacher, Roger Souci, who inspired me in mathematics. And he gave me the idea of doing research. We had just learned formulas for the circumference of a circle and the volume of a sphere. From his inspiration, I wrote a paper for him in which I derived the formula for the length of a spiral. I hadn't really had calculus yet, but I thought of a limiting argument. From this teacher I became excited about doing quantitative research, but the idea that it would be in the realm of economics did not crystallize until I was in college.

Campbell: So when you applied to college, you put down mathematics?

Shiller: I put down physics, though I didn't feel committed to that. An interest in physics is apparently very common among economists. I didn't decide on economics until well into college. I tried to keep all my options open for as long as possible.

What really struck me at that age was the incredible range of career choice that our free modern society allows young people. No longer are we expected to follow in our father's career, or anything like it, or even anything like what we have seen as children. This freedom to choose seemed such a precious gift, an opportunity to involve one's inner purposes in a life's work, a gift that must not be squandered. I did not want to let chance or inertia dictate where I went.

I was very impressed by the gravity of the career decision, and the schedule that imposed on me that decision as part of choosing a major and a field for graduate school. In my junior year in college at the University of Michigan I went on long walks thinking about what I wanted to do with my life. I became a wanderer on campus. I remember getting a sore foot, and so I went to the doctor. He told me then that I had a stress fracture in a metatarsal, the kind of thing that happens to soldiers on long forced marches.

Campbell: So then how did you end up with economics?

Shiller: I could perfectly well have chosen another field, but economics came in because of a conviction that grew in me of an alignment of my personality with this field. Maybe it has to do with the emotional weights I place on quantitative reasoning, and an appreciation of the impact of economic outcomes on our lives, the importance of economic forces in our lives, and the possibility of taking control of them. I could feel a real sense of purpose in working in economics.

Campbell: Were there people in college, economics professors, who inspired you to go to graduate school or otherwise influenced you?

Shiller: At the University of Michigan, I remember getting the Stolper–Samuelson theorem from Wolfgang Stolper. I remember trying to understand the timing of devaluations with Robert Stern. Shorey Peterson was a guiding influence who helped me with my writing and with economic reasoning. George Katona gave me my first exposure to behavioral economics.

I should say I was also influenced by fellow undergraduates, who were heading to business careers. I probably learned just as much from talks with Bruce Wasserstein, then but a teenager, but later to found the investment bank Wasserstein Perella.

Campbell: But you decided to go to graduate school straightaway from college.

Shiller: Yes, well we didn't have a choice. We would either go there or go to Vietnam. I was adamantly opposed to our war in Vietnam, but also I wanted to get on with my immersion in economics. I had a National Science Foundation fellowship and so I didn't see any reason not to go to it directly.

Campbell: So, how did you decide on MIT for graduate school?

Shiller: I didn't put the same amount of thought into that as I did of going into economics. It seemed to be the place to go at the time. I was certainly impressed with the faculty there. I already mentioned Paul Samuelson. I had also acquired as an undergraduate an admiration for Franco Modigliani and Robert Solow.

Campbell: When you got to MIT what was it like? Was it what you expected? Also, what were the hot topics?

FIGURE 2. Conference on Monetary Mechanisms in Open Economies, Helskinki, 1975. Speaker is Stanley Black. In foreground with pipe is Franco Modigliani. Row in foreground, left to right: Robert Shiller, Benjamin Friedman, Jacob Frenkel, and Edmund Phelps. In back row, Harry G. Johnson (with dark beard), diagonally opposite him, Karl Brunner (with white beard).

Shiller: The rational expectations revolution was emerging, long before the Lucas Critique, and that was exciting.[1] I well remember Richard Sutch, who overlapped one year with me at MIT, and who wrote a dissertation on the term structure of interest rates that involved testing whether the term structure is consistent with rational expectations and autoregressive forecasting equations for interest rates.[2] My own dissertation grew out of his, through the intermediation of my advisor, Franco Modigliani.[3] Later, Franco and I wrote a joint paper with some of these results.[4]

But, in truth, I didn't have a complete affection for this rational expectations theory. It was something to work on at that time. I didn't fully believe that people could be so calculating in their expectations. There were already elements of behavioral economics in my thinking, if not in my published research.

I met Jeremy Siegel practically on the day I arrived at MIT in 1967. I got to talking with him since MIT had all the incoming students scheduled alphabetically for our chest X-rays and Siegel comes right after Shiller. Jeremy impressed me immediately. And he always had a marvelous way of putting quantitative analysis

FIGURE 3. Poconos vacation, 1989. Left to right, front row, Jeffrey Siegel, Benjamin Shiller, and Andrew Siegel. Back row, Ellen Schwartz Siegel, Virginia Shiller holding Derek Shiller, Jeremy Siegel, and Robert Shiller.

into a real-world perspective, something I have enjoyed in his company ever since. I have long since used him to test the reality of theoretical notions.

Campbell: What was your first year like?

Shiller: I guess you could say that I spent that time orienting myself to the role of quantitative economics. It took me a while to consolidate in my mind what role it, and the assumption of rational economic agents, should play.

Campbell: For the first year, you probably had star faculty.

Shiller: I actually relished Samuelson's lectures, which some other students found too much of a stream-of-consciousness. To me, they were a delight. There was no disagreement that Solow's growth theory lectures were gems. I also remember M.M. Postan, the historian of Europe, who was visiting from Cambridge University at the time. He was anything but a technical economist, but he inspired and supported me. Later, Robert Hall arrived from Berkeley, and rewarded us with his original, unconventional thinking.

Campbell: I am wondering whether people or topics or people that you came across in graduate school influenced your subsequent direction. You started to work with Franco Modigliani.

Shiller: I was attracted to Franco because he was building a really ambitious model of the economy. There was something real and tangible about his approach to economics that I found appealing. Today, I still believe that there is something solid and important about these ambitious large-scale econometric models, despite the profession's largely turning against them. Not so long ago I did some work with Ray Fair that showed that such large-scale model forecasts do indeed contain information not seen in vector autoregressive forecasts or judgmental forecasts either.[5]

Anyway, Franco also impressed me because he made known his moral judgments. The war in Vietnam was going on then and we talked about that. There was also some turmoil about the relevance of abstract economic theory among other students I associated with. What I tried to do with rational expectations theory was to make it relevant and believable. I thought that it had some implausible assumptions about people calculating too much. So, one of my solutions was that if people have a greater information set than economists, then the projection of their expectations onto the common information set must be the same as the projection of the actual onto the information set. Obviously, people do not do elaborate calculations, but it seemed plausible that they would do even better than that.

Campbell: It would look as if they had estimated autoregressions.

Shiller: Right.

Campbell: But that is not a response to this criticism that they do too many calculations.

Shiller: Well, it is in the sense that it relieves us of assuming that they actually do the calculations. The smart money do not run autoregressions, but they do think a lot about the data, and so it seemed plausible that they could do even better than the autoregressions with their own different methods. That was the thought, anyway. I was just trying to make rational expectations theory somehow believable to myself and to understand how it could be even approximately valid. I attempted in my dissertation to make the case that patterns of change in the term structure of interest rates matched up somewhat well with what we would expect if investors had such abilities.

The other thing I did for my dissertation was to develop a distributed-lag estimator based on smoothness priors.[6] I thought that distributed lags were an important topic, since practically nothing in economic relationships happens instantaneously, and lags are likely to be distributed over time. The effect of lagged variables may even grow with lag, and then decline asymptotically to nil, but the available econometric theory for lag estimation was just very arbitrary, and people were estimating relationships that I just could not believe. At their worst, they would impose a single one-quarter lag when using quarterly data, and a single one-year lag when using yearly data, letting the period of data collection dictate the length of the lag. Robert Solow had improved this practice with his rational lags, and Shirley Almon with

her polynomial lags, but even these were really arbitrary parameterizations. They often produced funny lag patterns that were artifacts of the procedure.

I was wondering what we really wanted to assume about distributed lags. I was thinking that the existing approaches didn't capture our information. Their restrictions didn't correspond to our true priors. We should base our analysis on a good representation of our prior information about the lag structure, mostly just that the lag structure should be fairly smooth through time. So, I started reading Bayesian econometrics, and launched off on nonparametric estimation, using what I called smoothness priors. I later found out that Grace Wahba, in the Statistics Department at Wisconsin, was onto a similar smoothness idea, and her approach was more thoroughgoing than mine, though not applied to the estimation of distributed lags. Also, later, the same idea of smoothness was embodied in what is now called the Hodrick–Prescott filter. Anyway, the distributed-lag estimator I developed at the time had a good application in my dissertation to my study of the term structure of interest rates.

Since then, a lot of others have developed nonparametric estimation into a significant field, and there has been a lot of activity in Bayesian econometrics as well. Unfortunately, even today, the economics profession at large has not adopted any such methods on a substantial scale for applied work.

As far as I recall, no one had ever mentioned Bayesian methods at MIT, though I found Ed Leamer, an assistant professor at Harvard, who was deeply involved in using Bayesian foundations to adapt the scientific method to economics.[7]

Campbell: Who taught econometrics?

Shiller: Franklin Fisher had written a book on the identification problem in econometrics. We went through that whole book, an elegant treatise, but perhaps too much on that topic.[8] The econometrics course I had with Edwin Kuh doesn't stand out in my memory, but I can say that he impressed me about the importance of regression diagnostics and of isolating influential observations, practices that not enough people implement even today.[9] I learned the essential lesson to be skeptical of econometric results. I remember when Leonall Anderson and Jerry Jordan came to MIT in 1968 to present the "St. Louis Model" of the U.S. economy.[10] The results were impressive, but not really received well at MIT. Later, our skepticism was borne out. Ben Friedman reestimated the same model in 1985 and found that the new data provided by the mere passage of time had destroyed their results.[11] There are lots of ways econometric analyses can go wrong.

I then started reading time-series analysis on my own. I never took a course in that.

Campbell: Did you read Box and Jenkins?

Shiller: I certainly did, but I wanted to combine it with Bayesian methods. Arnold Zellner at the University of Chicago somehow discovered me; he invited me starting as a graduate student to a series of Bayesian econometrics conferences. It was at one of these conferences in 1972 that I first met Sandy Grossman, then only 19 years old, and already a dazzling intellect. I was fortunate to have the opportunity to work with him later on several papers.

I tend to attribute my interest in Bayesian statistics and time-series analysis to my physical-science orientation, which had been with me since childhood. I have long admired scientists. I thought that the Bayesian methods would help adapt the scientific method to economics, help us to base our analysis on what we do know, and let the data speak for what we do not know. The kind of science that appealed to me was the kind that was based on careful observation followed by induction that allowed you to discover a general principle. It was that discovery process that excited me. Bayesian econometrics appealed to me then as a good approach since it didn't impose some arbitrary model. In fact, the prior was supposed to come from some previous analysis; your prior was your earlier posterior.

I also thought that science is, at its core, really intuitive. Charles Darwin didn't follow a research program that was outlined for him. He was trying to think how this whole thing works and observed everything he could. Leamer referred to "Sherlock Holmes inference," in response to that fictional detective's attention to all the details, but I would prefer to call the ideal "Charles Darwin inference."

Campbell: You started to mention the Lucas Critique.

Shiller: When I first read Lucas's paper in 1975, I thought that there was nothing new in it. The idea of rational expectations was already prominent at MIT, through Modigliani and Sutch.

Campbell: But they didn't actually cause you to change your mind about econometric modeling. Their papers assume a fixed structure.

Shiller: If you were to take Franco aside then, and ask him, "isn't there a risk that if policy changes, the expectations structure might change," he would say, "obviously." But, Lucas presented this in a very forceful way . . . . Lucas is a great writer.

Campbell: Another idea that was floating around at the time was the efficient-markets hypothesis. Did you come across that in graduate school?

Shiller: Well, that was already well established.

Campbell: I am just wondering if that was a big part of the discussion in graduate school at the time?

Shiller: My dissertation was about the expectations theory of the term structure, which was an efficient-markets model. We talked a lot about efficient markets.

Campbell: Did you at the time already have seeds of the critiques that you later mounted so effectively?

Shiller: Well, as I just said, it didn't seem to me that ordinary people were estimating autoregressions as was represented in those models. There were already seeds of my later views of excess volatility in my mind. I noticed that when I estimated autoregressions, if I constrained the sum of coefficients to be one in the short-rate autoregression, that is, to have a unit root, I could come close to explaining the volatility of long rates. It bothered me that the difference between this sum and one wasn't well estimated, in other words, there seemed to be great uncertainty about whether there was a unit root. As you know, this has turned out to be a very contentious issue.

Campbell: This was before Dickey–Fuller and any of the other unit root literature in econometrics.[12]

Shiller: The issues of unit roots were very much bothering me then. I thought that maybe there was excess volatility. In the case of the term structure, if there is not a unit root in the short rate process, then there would appear to be excess volatility in long rates. That unit-root–excess-volatility issue is not in my dissertation, but I was wrestling with that as I wrote it.

Campbell: Let's move forward then for now. You left graduate school. Your first job was at Minnesota. What happened there?

Shiller: I had some wonderful colleagues there, such as Tom Sargent and Chris Sims. But, for me that was the slowest period of my life in terms of academics. I didn't publish for several years. I felt that I had to get on with my personal life. The biggest thing then was that I met my future wife Ginny. Now we have been happily married for almost 27 years.

Campbell: Then you picked up the theme of excess volatility after a few years.

Shiller: I had written about a rational expectations model of the term structure, but, after thinking about it intuitively, wondering what is causing the big movements in long rates, I cast about for other interpretations.

Campbell: The first paper was on long-term interest rates.[13]

Shiller: It seemed tangible and real to me that the long rates were not moving only for rational reasons.

Campbell: How then did you carry the analysis to the stock market?

Shiller: That was a very simple transition. As you know, the expectations theory of the term structure is a present-value model, and the efficient-markets theory of the stock market is also a present-value model. I thought that the stock market might be an even better example of excess volatility. Another advantage to the stock market was that one could get a lot of data. I found the Cowles data, and created from it time series of price, dividends, and earnings back to 1871. That was what I needed, since the present-value relation extends over so many years, as you know.

Campbell: As I learned from you! That is an interesting point, that you were doing work on historical financial data, very early on. Also, Jeremy Siegel has become known for that. I wonder if the two of you discussed that.

Shiller: Well, we did. Using only a short recent sample period *seems* scientific to many people, because they think that *the best* data, which are collected with greatest accuracy, should always be used. So, people thought that you should rely not on long historical time spans, but rather on high frequency of sampling. You can get daily data more recently, while if you sought long historical time series the best you could get further back was monthly, or annual. So, people wanted to stay with these recent data, and perhaps they thought that doing so was being very "scientific." But, I had a different concept of what "scientific" means, and I thought, from my own reading in science, that scientists have to look at discrepant data, at things that are not so well measured.

Campbell: You also were aware that with the long span of the present-value relation, the testing required a long sample.

Shiller: Well, that seemed very intuitive to me. My student Pierre Perron and I wrote a paper presenting a Monte Carlo study on power of tests as frequency of observation goes to infinity, holding the sample length, measured in years, fixed.[14] In the cases we studied, power does not appear to go to infinity as the number of observations does. Later, Pierre teamed up with Peter Phillips and developed a real theory confirming this.[15] Also, I should mention the work that my student Andrea Beltratti and I did to extend the excess volatility framework to consider excess covariance between assets' prices.

Campbell: So as you look at it now, some 20 years since the excess volatility work came out, how do you think it has affected the field of finance?

Shiller: I thought that excess volatility was an especially important anomaly regarding the efficient-markets theory. It certainly pointed to a possible failure of efficient markets.

Behavioral finance has emerged since then, but what exactly caused that I do not know. Excess volatility is an anomaly that is very different from other anomalies. The other anomalies, such as the Monday effect, or the January effect, do not seem fundamental. If you read Fama's "Efficient Capital Markets," he talks about anomalies, but the ones he talks about sound like the result of a little bit of friction disrupting the otherwise precise predictions of the model.[16] But, you wouldn't think that friction would cause excess volatility. Friction ought to slow things down or smooth them out, and the volatility seemed excess by a wide margin. It also accorded with intuitive feelings that people who look at the market have, and I wanted to try to show that there might be a scientific basis for those feelings.

Campbell: You mentioned behavioral finance, which is probably the big theme of your career. So, let us move beyond the excess-volatility work. But, then what you really did do was develop an alternative perspective. If the excess volatility grew out of your thesis with Franco, how did this alternative view develop?

Shiller: Well, I met Ginny in 1974. Soon after we married, she enrolled in a Ph.D. program in psychology at the University of Delaware, not far from the University of Pennsylvania where I taught at the time. We lived in Newark, Delaware, and I commuted to Philadelphia. So, by day I was an economics professor, but by night I was living amongst a whole community of young psychologists. Some of their thinking made an impression on me.

Since our days together in Delaware, Ginny has accompanied me, first to Cambridge, Massachusetts, where I visited the National Bureau of Economic Research, and then to MIT, and then finally, in 1982, to Yale, where she got an appointment at the Yale Child Study Center. Over all these years, I have talked a lot with Ginny about my work, and her work. She was a big influence on me. She still is.

The next event was that I met Dick Thaler in 1982. I was invited to give a talk then at Cornell, where he was associate professor, and we immediately hit it off. He has a remarkable ability to put economics in a broader perspective than we are accustomed to seeing, a perspective informed by psychology. And over the years, his stature has grown and grown. I believe he was already connected then with Daniel Kahneman and Amos Tversky.

Campbell: So you learned about them through him?

Shiller: No, actually, Kahneman and Tversky's prospect theory appeared in 1979, and I had heard a lot about that paper before I met Dick.[17] But, I think that Dick jumped onto their inspiration much faster than I, and had a lot of insights to convey to me.

Thaler's dissertation at Rochester in 1974 had been about measuring the value of a life for economic purposes, assuming that everyone was rational.[18] It was somewhat later that he met Kahneman and Tversky, and that set the course of his career. He turned fundamentally against his earlier work. I did not have any such epiphany. I never came as close to psychology proper in my research as he did. My research remained more quantitative and centered more on conventional economics.

Dick Thaler and I have been working together since 1991 to organize a series of NBER conferences on behavioral finance, sponsored by the Russell Sage Foundation. Also, out of this grew a series of NBER conferences on behavioral macroeconomics that George Akerlof and I have been organizing since 1994.

Campbell: Well, there was this other strand in your work, the consumption-based asset pricing, the material with Sandy Grossman. Looking back on it, it was innovative.

Shiller: That research was very exciting to me, though I had doubts about that too. I thought there was some truth to the consumption-based asset pricing model, but again I didn't fully believe it. I had long talks with Sandy, and discussed models, and I remember saying that I just don't believe this model.

Campbell: And he said that he did believe it?

Shiller: Well, I can't summarize his thinking. He is a pretty practical guy, too. It becomes a subtle question of the philosophy of science, how far to pursue a model. This is an interesting model, and it seems to explain some things, as some of my work with Sandy revealed.[19] But there were also substantial problems with the consumption-based asset pricing model.

For example, I had derived an inequality that showed a lower bound on the variance of the intertemporal marginal rate of substitution, and found that this appeared to be widely violated by the data.[20] Lars Hansen and Ravi Jagannathan later did a splendid job of establishing the scope and significance of such a violation.[21]

Beyond this, I just wanted to move on to something else. And Sandy has moved on to something else, too.

Campbell: He certainly has. In the early years when you were doing behavioral finance it was extraordinarily controversial, and there were big fights. So, do you have any stories from that early time?

Shiller: This excess volatility got quite a hostile response. Well, I should say that a lot of people were quite friendly about it, but I think it was costly to me to do this. People didn't really receive it well. It was politically incorrect somehow.

Campbell: I remember you had a Brookings paper in 1984 where you laid out what has become the standard paradigm of behavioral finance, where you laid out the importance of social contagion and looked at the interaction of noise traders and rational arbitrageurs.[22] Did that get a hostile reception at Brookings?

Shiller: No, not at Brookings, I don't think. The hostile reception that there was, was subtle. It was not that people started shouting at me, or, as you can testify, later, at you and me. Instead, some tried to marginalize or ignore what we were saying. They tended to try to dismiss the theory without even looking at it. They often described it as if we had made some egregious error, a stupid error. On the other hand, I didn't think it was a totally bad reception, even from the beginning. Our profession indeed includes a lot of open-minded people, who really look at the evidence, even though their own published work may not make obvious to readers the breadth of their understanding and personal desire to pursue the truth.

I remember from that 1984 Brookings paper that I had a paragraph that highlighted an important error in economists' thinking. If markets are perfectly efficient and expected returns exactly constant, it implies that price is exactly the expected value of the present value of expected future dividends. That is true. The widespread error is to assume that, from the assumption that expected returns are *approximately* constant, it follows that price is *approximately* equal to the present value of expected future dividends, approximated equally well. That error has inclined people to think that, given that short-term stock market returns are hard to forecast, the level of the stock market itself must be equally hard to distinguish from its fundamental value, the present value of expected future dividends. They conclude that every movement in the stock market must have a rational foundation. In that Brookings paper, I said that this error is "one of the greatest errors in the history of economic thought." Someone at the Brookings conference where I presented the paper said afterward that I should take that provocative line out of the paper. I asked Bill Nordhaus, who was also at the conference, for advice: Should I really delete that line? Bill said "No no, don't take it out!"

Thinking about approximation error led me further to examine how inadequate appreciation of the low power of some tests of market efficiency had misled researchers. It led them into widely accepting a theory, the expected present-value model for aggregate stock prices, that is egregiously wrong.

Campbell: That is an interesting story because I remember seeing that line about the greatest error in the history of thought in the paper and thinking that most people are more aggressive in person than they are on paper, and thinking that perhaps you are the exception that proves the rule.

Shiller: Well, maybe I am a more aggressive person on paper. I think I become a different person when I am writing. That is, in part, why I have kept a diary all my life, continually since I turned 12 years old. Writing just stimulates my mind. I believe that people are stimulated by conversation: that is the way the brain works. Keeping a diary and talking in it to oneself creates a more idiosyncratic view.

Campbell: So, having a social influence on yourself.

Shiller: It seems to work that way. Do you keep a diary?

Campbell: I don't, but maybe I should.

Shiller: It seems to elevate my thinking, in the sense that writing down my thinking makes it come to fruition. If I am not writing it down, my mind would just drop it. It helps me to think things through systematically and adopt resolve to take action, and it reminds me of my own past thinking.

Campbell: Another thing that you started in the late 1980's was using survey methods. I remember you did surveys around the time of the 1987 stock market crash. And, many economists had been skeptical about surveys. Was that an influence from Ginny? How did you get started in this direction?

Shiller: It probably was in part an influence from Ginny. She gave me support in pursuing a line of research that made little sense from a career standpoint, but that I (or, should I say, we) really believed in.

I remember reading Milton Friedman's *Essays in Positive Economics*, where he argues against relying on what people say when they explain their motives for their economic actions.[23] There is even a tradition among psychologists against doing that. There is obviously a tradition there against asking people "Why did you do that?" and taking what they say at face value. That is not economics and it is not psychology.

But, on the other hand, economists, such as Milton Friedman, seemed to assume that is the only thing one could do with surveys, and to advise instead that economists should rely exclusively on formal optimizing models, and test them statistically using price and quantity data. But it seemed to me that economists often seemed to live in a rarified world. Often, there are very simple explanations of why people do what they do, and economists ignore them. We should ask people about what they do, at least find out the focus of their attention and the assumptions they were making, though still not take their answers at face value.

Economists often impute thoughts to people, implicitly in their optimizing models, that are not really in people's minds at all, it seemed to me. So, I thought we should find out what people say they were thinking, that this is interesting research. I viewed this as not career-optimizing research for me. But, I already had tenure when I began this research, and so I thought, this is what tenure is for. I do not have to do the same things others are doing.

There was a big stock market drop on September 11th and 12th of 1986, and I immediately thereafter did a little postcard questionnaire asking investors what they were thinking on those days. I learned from the reaction that I got from this and subsequent research that probably nobody else in the world was doing such research on what people think during crashes. Merton Miller later pointed me to a Securities and Exchange Commission interview study of participants in a stock market crash in 1946, but apparently no one else had ever done such a thing since.

I was thinking that science involves a lot of herd behavior: Too many scientists do the same thing. There are career reasons why they do, but scientists are often most effective for the long term when they move independently. From this perspective, I was noticing the volatility, and thinking about it.

Campbell: So you were ready when the crash happened.

Shiller: Yes, and when this big crash came in 1987, I thought that this might be the chance of a lifetime for research on speculative bubbles. I first worried that maybe somebody else would do such a survey about what people were thinking on the day of the biggest one-day stock market crash (and still today, biggest to date), making mine unnecessary. But, on further thought, I thought maybe not. I had learned that there was no organization that was set up to do this very fast. I concluded that there was a chance that no one else would do it. Months later, President Reagan's Brady Commission did do a survey of investment professionals as part of its report on the crash, but not only was it late, after people were possibly in a different frame of mind, but also it did not really ask what they had been thinking on the day of the crash.

To arrange this survey, I stayed up practically all night on both October 19th and 20th, 1987. I was exhausted, but happy to see the survey in motion within days of the crash: 2,000 questionnaires were mailed out to individual investors and 1,000 to institutions. Between the two, I got almost 1,000 responses.

I didn't even try sending this to a scholarly journal. I thought it would be rejected. I put it in my book, *Market Volatility*.[24]

Campbell: Now, I don't know a whole lot about psychology, but I am impressed that there is a trend away from strict behaviorism, toward studying what goes through people's minds. You seem to be saying that you are pursuing that same line of thought within economics, that if we have models that ascribe certain purposes to economic agents, we should look to what they say they are trying to do. So, maybe there is a parallel with psychology.

Shiller: I suppose.... Yes, there was a trend within the social sciences of studying intentions. It has been called interpretive or hermeneutic social science. Of course, intentions are part of classical economics. An optimizing model is a representation of intentions, but there is traditionally no attempt to collect data on what intentions, or associated world views and popular models, are. Economists try to observe actions rather than intentions to test these models.

In 1987, I thought I should do my survey since otherwise the chance would forever be lost. Even though it wasn't being used by practically anyone else in economics proper, I thought it would someday be useful.

Campbell: And then you found other applications of it.

Shiller: And then I found Chip Case. He has been a great colleague. A year after the stock market crash, in 1988, we did a questionnaire survey of recent homebuyers to study a housing bubble in California, and the end of a bubble in Boston.[25] We compared across cities, boom, postboom, and nonboom, in Milwaukee. We learned some very basic things. For example, we learned that Milwaukeeans are very uninterested in real estate. Fascination with and attention to speculative markets is something that varies geographically, presumably because of different market experiences.

Campbell: So, that is something that leads to my next question. How did you get interested in real estate? Was it just a natural idea, "Oh, there is volatility in real estate too so let us look at that too?"

FIGURE 4. Red Square, Moscow, 1989, left to right, Alan Auerbach, Robert Shiller, Lawrence Katz, David Wise, and Lawrence Summers.

Shiller: Well, partly my sense of herd behavior influences a lot of my thinking. Economists themselves are herdlike in their research directions, and so there is a lot to be gained by staying away from these common topics. Well, maybe not career opportunities, but intellectual opportunities, to go off onto topics that no one is studying. So, I did a survey of the literature to see what was known about the efficiency of real estate prices. Are they a random walk? In my survey of the literature, there was almost nothing about the efficiency of home prices. And, when you think of it, real estate is just about as important as the stock market, in terms of total market value. Why was there all this study about the stock market and not of real estate?

Campbell: So, did you look for a housing economist? Was that how you found Chip?

Shiller: He was connected with Ray Fair, who was writing a textbook with him. Also, Chip is a kindred spirit: he had written an article in 1986 about the Boston housing market, looking at all the fundamentals, and concluding that there was nothing there that would justify the nearly 40% increase in housing prices in one year in the mid 1980's.[26] He was a great collaborator, and I think we learned a lot about what was going on in people's minds during this bubble.

FIGURE 5. Founders of Case Shiller Weiss, Inc., 1991. Front row, Robert Shiller and Charles Longfield; back row, Karl Case and Allan Weiss.

Campbell: So, then, in the real estate context, you went beyond academic work and started a company.

Shiller: Well, my student at Yale, Allan Weiss, after he graduated in 1989, wanted to work with me on the producing the indexes Chip and I had developed, to produce these on an ongoing basis as a commercial enterprise. Also, Allan had been thinking about how to manage real estate risk, and he thought that getting into the index business might somehow be a way to make take these thoughts into action. We set up Case Shiller Weiss, Inc., in 1991, and Allan was president; Chip and I were board members. We initially hoped to make the indexes the basis for futures contracts, but that still hasn't happened. We became forecasters of housing prices. We expanded the company to be a provider of an automated valuation model, an econometric model that provides instant online valuations of homes. We were lucky in our timing, for our first efforts here coincided with a rapid transition to online lending in the mortgage and home equity loan industry, and so these lenders became our customers. Allan made this company a big success. In 2002, we sold the company to Fiserv, Inc., but it continues to function independently as Fiserv CSW, Inc.

All of this happened because, in the late 1980's, Chip and I had to create real estate price indexes for our purpose of testing real estate market efficiency. At that time, there were really no available real estate price indexes that could be used to test market efficiency. The available median price was extremely choppy through time, and we thought that was due to the changing mix of houses sold. Chip, in his

FIGURE 6. Robert Shiller in his office at Yale University, 2003, with colleagues, left to right, William Goetzmann, William Brainard, Stefano Athanasoulis, and Carol Copeland.

article in 1986, had created a repeat-sales price index for Boston. I discovered that while Chip had independently discovered that method, there had been a treatise on the repeat-sales price index in the early 1960's by Martin Baily, Richard Muth, and Hugh Nourse.[27] But, they never seriously implemented it. Twenty-five years had gone by and there were still no repeat-sales home price indexes produced on a continuing basis or for any substantial geographic areas. So, we had to develop them. We improved the repeat-sales method, found the data, and started producing indexes. My student Will Goetzmann wrote his dissertation here at Yale on repeat-sales indexes, which he applied to the market for paintings, and he is now back at Yale as my colleague, and head of the International Center for Finance here.

Chip and I hadn't expected to get into the index number business, but we published an article on real estate price indexes, for four cities. Then, we tested (and soundly rejected) the efficient-markets hypothesis for single-family home prices using these indexes.[28] We found some very substantial momentum in home prices. It seems as if my excitement then was not entirely unlike that which Leeuwenhoek must have experienced when he looked through the microscope for the first time. We *saw* from our plots what real estate prices were doing, that they behaved very smoothly through time, unlike stock prices. People must have intuitively been assuming that there was price inertia, but they had never actually seen it. One cannot clearly see home price movements without some

careful econometrics, because of the noisiness of individual home prices and the incommensurability of dates of sale of houses.

My student Allan Weiss thought there was a business in providing real estate price indexes. He started the business, and pursued all the difficulties of creating a kind of business where there was no prior model to copy. I was an advisor. Well, I was more than an advisor. I did all the econometrics initially and wrote the computer program to construct the indexes, the same program that our company still uses today. I worked with Allan and Chip on developing applications for our indexes. The three of us made a tour of futures exchanges and other risk management companies to try to get markets for real estate risk started.

That was an interesting experience. For an academic economist, it is a good experience to run a business. Allan would discuss with me everything about the business. In the early days, Allan and I even had to loan money to the company so that we could meet the payroll, so we really experienced the anxieties of the business world, and I believe this has affected my thinking about economics.

Campbell: I would like to talk now about the stock market overvaluation of the late 1990's, and the fact that you were watching the market in light of your earlier work and a concern that the market was becoming overvalued.

Shiller: Well, my book, *Irrational Exuberance*, is a case in point.[29] Well, before that, of course, in 1996, you and I were invited to testify before the Federal Reserve Board.

Campbell: My impression, for what it is worth, is that Greenspan had already been formulating his opinion about irrational exuberance before that meeting.

Shiller: You are no doubt right. How could he really have been suddenly swayed at that meeting, there were so many different opinions expressed there. So, ...

Yes, this reminds me, we have skipped over *our* collaborations. I have written over a dozen papers with you, more than with anyone else. You were a very big influence in my life. You made my analysis rigorous. We developed vector autoregressive, and cointegrated, models, and you helped deal very much with various criticisms, notably the unit root criticisms. It was your idea, I believe, to have a cointegrated vector autoregression, involving the dividend-price or earnings-price variables as an information variable.

Campbell: Yes, I remember when I was on the job market going to San Diego and learning about cointegration. Robert Engle and Clive Granger were just doing this stuff on cointegration. They were thinking about it in terms of disequilibrium adjustments, or partial adjustments, to an equilibrium that relates to the long run and not the short run, and I remember thinking it needn't be that way. The same economic model that generates the long-run equilibrium might also determine the short-run adjustment to that equilibrium. It fit very nicely with the issues that were being raised by critics of your excess volatility.

Shiller: That was in a sense the final step. My work on testing for excess volatility never progressed further after that. That led to your decomposition of returns into a component relating to new information about future dividends and a component relating to information about future returns.

FIGURE 7. Robert J. Shiller and John Y. Campbell, 2003, at Littauer Center overlooking Harvard Square.

Campbell: Well, that was another thing in our joint work. The loglinearization we developed together allowed us to think of a present-value model with time-varying interest rates.[30] And let us now give credit to you, you came up with that extension that allows a loglinearization in terms of interest rates, and I ran with it in different directions, in terms of consumption and all that. But, the idea that you could loglinearize the equation was fundamental. That was an eye-opening moment for me, an epiphany for me.

Shiller: That was a beginning of a number of papers.

Campbell: Well, let's go back though to the late 1990's. After going public that we thought the stock market might be too high, there were several years when the market kept going higher. By 1998, we published a paper saying that the market was perilously high; we published that in the *Journal of Portfolio Management*.[31]

Shiller: That was our joint testimony that we prepared for publication. That was when we first really went public with it. That was when there weren't so many caveats as in our earlier statements.

Campbell: I guess I'm just asking whether it was personally hard for you to stick to your guns during this period. I certainly found it hard. You stuck to your guns with a vengeance, and you wrote *Irrational Exuberance*.

Shiller: I felt propelled by the market, and the collective delusions about the economy we were experiencing then, to write something against it. The book came out in March 2000, the very top of the market. That timing was luck. Well,

it wasn't entirely luck; I had a sense this market had to come to an end soon, and so I rushed to write that book.

Campbell: With a feeling that it was now or never?

Shiller: I wrote that book at breakneck speed.

Campbell: And I believe that Jeremy Siegel encouraged you to do that.

Shiller: That's right. I had been thinking of coming out with another edition of my collection of papers *Market Volatility*, and Jeremy said I should just write a whole new book, and this was the time.

Campbell: Did he say this was the time because he too thought that the market was overvalued?

Shiller: I think, . . . interesting question . . . he did say it was the time for me to write this book. I think that he did share some of my concerns. Nine months later, in March 2000, he wrote a *Wall Street Journal* piece about the overpricing of technology stocks.[32] He sounded very much like me then, except that he was confining his attention to a certain class of stocks—technology stocks—rather than to the whole market.

Often, a lot has been made of our differences, that he is the bull and I the bear, but as a matter of fact we were very much on the same wavelength in many ways. I think that what he was saying was "I don't know if you're right Bob, but this is an interesting argument and this is the time to get a book out." Maybe he thought with my book I would produce something that focused only on technology stocks. Note, too, that the latest edition of his book *Stocks for the Long Run*, contains a chapter on behavioral finance.[33]

I have a philosophy that one must start big projects immediately on inspiration; otherwise, one will never start them. So, after the phone call with Jeremy, I started writing the book immediately.

Campbell: Like the survey after the stock market crash.

Shiller: Yes, in a way, it was impulsive. Fortunately I didn't have any appointments that afternoon. I immediately went on a long walk, thinking about this idea, and I started writing before I lost the inspiration, so that it would be framed in my mind as a going project. Then I started calling publishers, including Peter Dougherty at Princeton, who became an important formative influence on this book.

I was writing a different book at the time, and I abruptly dropped it. That is an emotional thing to do: when one is writing a book, one doesn't want to stop it, and one fears that it will never be done.

Campbell: What other book was that?

Shiller: That is the book that was finally entitled *The New Financial Order: Risk in the 21st Century*, and appeared in 2003.[34] I started that book in 1997, so I had been working a year and a half on that book when I had to set it aside. Fortunately, I was later able to rekindle my enthusiasm for that book, with Peter Dougherty's encouragement and help. Dougherty was a terrific editor for *Irrational Exuberance*, and I am very lucky to have him again with *New Financial Order*. A really good editor can offer subtle guidance that makes all the difference in the

final product. I should add that my wife, Ginny, also read and marked up the entire manuscript, and helped me organize my thinking for that book.

Campbell: Do you think that *Irrational Exuberance* has affected people's understanding of the stock market?

Shiller: Well, at least it affected mine, in the sense it was writing up, consolidating, my thinking. I thought about all the different things that I had studied over the years, and tried to state their relevance to the current situation. I don't view it as a popular book. Some people would say that it was a popularization. But it was a popularization only in the sense that I left the math out. It was a bit like the discussion or conclusion to one of our joint papers, John. There are actually two equations in *Irrational Exuberance*, though they are buried in the endnotes.

In a sense I was writing this book for myself. It was just exactly my thinking. It reflected the inner thoughts I have when I try to put financial research in a broader perspective. So, I was surprised that the book ended up on the *New York Times* nonfiction bestseller list. This is a very unusual event for a university press book. The only significant thing that I did to appeal to a broader audience was just to try to make it interesting, interesting to me.

There is something to be said for a very broad focus in economics. Economics is different from a lot of other fields. One thing is that it is harder to compartmentalize and be useful. In chemistry, one can take some particular compound and do an analysis of it, but, to be useful in economics, one has to have a broader perspective. There seems to be a greater risk in economics than in chemistry of doing something useless.

Campbell: Yes, I think that is right. There are some disciplines where there are many little bricks that have to be assembled to make the wall, but perhaps less so in economics.

Shiller: Yes, of course we have data collection, like that done by the Census Bureau, lots of very little bricks put together for a foundation. Then we economists build on these foundations some very flimsy superstructures, some tenuous economic models.

Campbell: You talk about economists being useful. Some of the things you have done advocating financial innovation are certainly potentially very useful. And maybe your work on inflation indexation . . . .

Shiller: Some of that is joint with you.

Campbell: Some of it is. You also did some work on inflation with Jeremy Siegel very early in your career. And you have solo work on inflation. So, how did you first get the idea this was an important topic?

Shiller: That is another question that I can't answer exactly. Well, I keep getting back to things that I was taught about science. Getting back to physics, one of the most important things that I learned there was the importance of getting your units right. And, in economics, some of the most important fallacies in the history of thought have had to do with problems of units. The nineteenth century wage fund theory is an example, where economists confused a fund with a flow. My

instructor, Shorey Peterson, at Michigan, stressed this, but it was also my physics professors that stressed to me the importance of units of measurement. We write most of our long-term contracts in terms of dollars, a unit of measurement that changes through time, and that is just a changing yardstick. It is odd that we in the twenty-first century would be using such archaic measures.

Now, the history of thought on this is interesting: The first person to propose the compensated dollar was Simon Newcomb, an astronomer, in the 1870's.[35] He was an expert on systems of measurement.

And then Irving Fisher, another formative influence on me (though I never met him), also emphasized human foibles in designing monetary policy.[36] And this is the real beginning of behavioral economics.

It strikes me that, so often, economists build models that portray people as effectively paying attention to certain quantities that I suspect they are not even looking at. People are not even thinking in those terms, but are using an entirely different system of coordinates.

So much of the theory of the term structure of interest rates is a theory of *real* interest rates. When you point out to theorists that we have not had, until very recently, a term structure of real interest rates to observe, they sometimes say that a theory of the nominal term structure would be messy, inelegant. Nominal interest rates involve an inflation component that is not elegant to model.

Given the difficulties people have in behaving as economists assert they ought to behave, it has just seemed to me that the world should be more indexed, indexed in a way that is very easy for them. We should define an inflation-indexed unit of measurement, like the *unidad de fomento* in Chile. That way, we change people's psychological frame of reference. And, in fact, we should at the same time establish units of measurement for many such things. This is in my new book, *The New Financial Order*. As well as an inflation unit, there should be a wage unit that reflects the average wage. For this, we need better wage indexes. Wage indexes today are not repeated-measure indexes, and so they do not accurately reflect changes in individuals' compensation. Then, we would have also a productivity measure, different market baskets for the elderly, etc. And you should be able to write a check measured in any of these measures, not just in terms of currency, use your credit card with these units, and so on.

I wanted to call the unit that is indexed to inflation "baskets," referring to the market basket that underlies the consumer price index, and so ideally one could write a check in terms of baskets instead of dollars. Writing such a check for 10 baskets, say, would be like handing over 10 baskets of an array of real goods, and so, the name would help people to understand that in writing such a check they were in fact doing just this.

Campbell: Well, that is sort of like going back to the Middle Ages where feudal dues were specified in hogsheads of agricultural products.

Shiller: And yet, it draws on economic theory. Index number theory is an important area of progress in economic theory that I think ought to be applied so

that it can yield more tangible benefits to society. The indexed units of account may in some sense seem like going back to the middle ages, but in fact it would be going forward with some very sophisticated theory.

Campbell: Back to the future.

Shiller: Yes indeed. It also relates to new electronic technology. Irving Fisher, when he proposed his compensated dollar, assumed that we needed to base our transactions on a hand-to-hand currency.[37] It was difficult to conceive of a way to make the real value of currency absolutely stable. Irving Fisher was worried about the calculations required by indexation: One cannot expect people to do complex calculations every time they buy a newspaper. His way to solve this problem was the compensated dollar. Today, it is much easier to achieve that with electronic money, where the real value of the unit can be defined in terms of an index that is computed automatically and continually by computers. Now, with credit cards, smart cards, and the like, we really ought to have sophisticated units of measurements that are taught to children and established in our economy, so that it is easy to make sensible contracts. These are themes in *New Financial Order*.

Campbell: So, you mentioned the book, but that is just part of the whole research agenda, and the mission to promote new financial instruments to enhance risk sharing. And you had your earlier book, your Clarendon Lectures book on macro markets.[38] Some people would say: Isn't it surprising that the same Bob Shiller who argues that markets are excessively volatile is also promoting the further extension of financial markets. How would you respond to that?

Shiller: Yes, well it would be oversimplifying in the real world to say that just because there is excess volatility, we should not have markets. You know, no one has proposed, and I never proposed, that in response to excess volatility we should shut down the stock market.

Campbell: Well, Alan Blinder once told me that he thought that the stock market should be open one day a year.

Shiller: Well, that is a bit of a "sand in the wheels" theory. Jim Tobin might have gone along with that, but even Blinder is not advocating shutting the markets down. Excess volatility is just one example of how the inconsistency of human behavior is a potent force. The human mind is incredibly powerful; it is capable of computations that can dazzle you, but can also be very blundering and foolish at times. So, we have to design things so that they work well for real people.

For example, airplanes are designed so that they are very stable, do not go wildly off course when there is a minor pilot error. We have to design our financial institutions in the same way. It is a difficult problem, how to achieve this. One has to engineer around human limitations, and make it possible for people to do what they can do very well. Most people are quite capable of managing their lives and their own risks, and so we want to create the vehicles that enable them to do this, but also to have default options set up so that if they do nothing they will still fare fairly well.

So, excess volatility is a manifestation of a certain inconsistency in human behavior, and that same inconsistency has other manifestations, even things like wars, rebellions, things that have nothing to do with markets. So, I think that, overall, expanding markets is the right thing to do.

Campbell: You have also sought to expand markets, create new markets, partly through your company, and through efforts at persuasion.

Shiller: A lot of things are happening now. I get phone calls from many people who are thinking of creating some new derivative market or some new way of achieving social goals through incentives. That would go on whether I had been here or not, but one will never know what influence I had on that, if any.

Campbell: So, do you feel you understand the obstacles, from talking to people at futures exchanges, for example, or talking with people through the company you and Allan Weiss recently created, Macro Securities Research? You've become perhaps more savvy about the obstacles, the inertia that prevents these markets from becoming successful.

Shiller: Yes, Allan and I, and now Sam Masucci, who is Chief Operating Officer of Macro Securities Research, have been working for years to try to make better risk management happen. And we have learned very much about institutional inertia.

One reason why I wanted to create a more sensible system of indexation is precisely because of such institutional inertia. We were thinking of creating vehicles for people to hedge the value of their houses. When we went to futures exchanges to propose that, it occurred to us that a simple risk management product for homeowners should be one that protects the real value of their home, not its nominal value. For if people hedged the nominal value in a time of uncertain inflation, they could be creating bigger fluctuations in the real value than they would have had if they had not hedged. I asked people at these exchanges if we could create a hedging vehicle that was defined in terms of real values, and they just looked blankly at me. "What are you talking about?" So, it seemed that one could not do anything sensible if we have to make all of our economic contracts in terms of some crazy unit (money), so that people cannot manage such a simple construct as indexation. That is an example of what I observed from trying to get these things started. So, we need to set up an economic infrastructure that will make these things more possible.

Campbell: You have this new book coming out, *The New Financial Order*. So, how did you come to write that book? You started it back in 1997, and then dropped it.

Shiller: Well, I wrote *Macro Markets* in 1993. That book had a very technical side, where I talked about repeated-measures indexes, for example, and sources of volatility. But it also got into a side that is broader, about how institutional change will transform our markets and our lives. It was reviewed in the *New York Times*, and the reviewer, Peter Passell, called me up and said that I should really write a version of this book that is accessible to a broader audience. He said, all that math is intimidating and not necessary for the basic ideas. But, I told him that I didn't

know how to write a book for a broader audience on this topic, which seemed to be an inherently technical topic. So, I put that idea on the back burner. I didn't think I could do it.

Some years went by. My student Stefano Athanasoulis and I advanced the mathematical theory of fundamental risk management in the context of institutional design in several papers.[39] Working with Stefano really was very productive; we have a much clearer idea of the theory of these new risk markets now. We have written four major papers on this topic, and continue to work together. He has been a very important force in my work in this area, and has given me many ideas.

At the same time I began to think that there is in fact a lot to say without reference to these technical issues. Again, I started to write the book for myself, to try to understand the issues better myself.

Part of my motivation is that, after I wrote the book *Macro Markets*, people for the most part just did not react to it. They didn't see that it contained a good idea, an idea that was workable. So, I needed myself to come up with better arguments, and that meant integrating the risk management notions into a bigger picture.

Handling the real obstacles to risk management was the basic motivation. I wanted readers to see how analogous innovation has fitted into history, to see that radically new financial innovation is not unprecedented, and that it is not implausible that we would find ways to deal with barriers in human psychology, especially if new institutions are designed right to work around human foibles. I wanted it to be a serious book, and I thought I could put the vector autoregressions somewhere else. Anyone who wants to read about that can read some of our papers, for example.

I really got very excited about this book, but it has been very hard to write. In contrast, I wrote *Irrational Exuberance* basically in nine, months. Though there were some notes I incorporated that I wrote in 1987, the basic project really took only nine months. Writing *The New Financial Order* was very hard, and took a long time.

Campbell: One of the things that you say in the preface to your new book is that your 20 years at Yale have had a certain influence on your thinking.

Shiller: Well, I came to Yale because I admired a number of people there: Jim Tobin, Bill Brainard, Bill Nordhaus, and others. There was the Brainard–Dolbear article that talked about hedging risks to livelihoods.[40] And that is the earliest reference I could find to that idea. Tobin is implicit in his work about hedging idiosyncratic risks. He was concerned a lot about institutional change. He was the inventor of the Yale Tuition Payment Option, the institution that allowed student loan contracts to work toward managing lifetime income risks. And he made it happen, albeit only on a small scale. He got Yale University to implement it.

Campbell: But this has backfired for the University because people who owed big payments under the program are people who would otherwise give big donations and they are claiming to be so annoyed at the bills they get that they refuse to give money.

Shiller: Well, so they ended the program, and finally forgave the rest of the loans. The original plan had some shortcomings. Another problem is that the payments were defined by a line on the Federal Income Tax form. Unfortunately, when people married later, they wound up taxing the spouse. People hadn't anticipated this, and it seemed unjust. Now, there is a big question about how to handle that.

Campbell: You could be married and file separately.

Shiller: Well, but that is costly under current tax law. It will be easier once the income tax is more computerized, and you can push a button and the forms can come out in different ways. The reason that the Yale TPO had to be defined in terms of a line on the tax form is that they didn't have this technology. Now, one of the points of my new book is that technology is advancing so fast that the complexity of our institutions can go very far forward now.

Campbell: Well, if you look at the tax code, it has been doing that. The tax code has become more complicated because it is possible to do the calculations now.

Shiller: Well, obviously, for good risk management purposes, complexity may be necessary. But, anyway, as far as the Yale tradition . . . going back to Irving Fisher, a lot of people admire him. He was an innovator. He tried to get indexed bonds started. He had a kind of practical approach to economics. And he even did survey work.

Campbell: Really? I didn't know that.

Shiller: In his book *Money Illusion*, he reported an informal survey of shopkeepers in Germany during the hyperinflation, and asked them why they raised their prices, and concluded that they didn't really understand the role of inflation in their decision. So, there is a sort of tradition at Yale for down-to-earth policy-focused research.

Incidentally, there is another article in *Macroeconomic Dynamics* about the Yale Tradition. David Colander interviewed Jim Tobin and me about the Yale Tradition in economics.[41]

Campbell: I have a last question. Looking forward, are there any research topics that are going to be especially productive for you or for the profession in the next 10 years? If a graduate student comes to you and says I am looking for a field to work in or an idea to work on, what would you say?

Shiller: Well, it is hard to predict very far out what we will be doing. I have never known very far out what my own research will be. I think that there are a number of things to say.

One of the things I would say that perhaps most economists would not say in answer to such a question is that the information revolution is lowering the costs of doing things, and we have to think about what we as economists can do with that new opportunity. And so, economic researchers should be thinking more about what they can do constructively to develop more complex economic institutions.

It is starting to happen. We have a lot more research on financial engineering and mechanism design. It is an emerging thing.

I think that there is a lot of fundamental work to do on the integration of the theory of risk management into broader economic theory. It is very important to

try to advance this fundamental theory. Unfortunately, the day-to-day life in the profession has a tendency to distract one from such basic research with a million little diversions. I have to thank my administrative assistant for many years, Carol Copeland, for helping me to manage my time and to steer clear of distractions. People need to listen both to others' advice as well as to their own conscience to keep their research from becoming diluted.

A lot of what economists do is, though, very abstract. I would also encourage more economists to do practical things, like write patents, rather than just NSF proposals. There is a lot to be gained from the application of our theory.

I imagine myself in the coming decade celebrating and supporting others' efforts to do such things. It is hard to be an innovator because it is not a traditional course in the profession. It tends to look nutty. Well, the Wright brothers were thought of as nutty by many when they tried to develop the airplane. Today, new designers of airplanes are fully respected. But, today, in mechanism design or other constructive aspects of economics, we are in the Wright Flyer stage.

So, one of my missions in the years ahead is to help support innovation in finance. I get a lot of proposals now that cross my desk or my e-mail. The typical story is that "I have this idea that nobody will listen to." Sometimes the idea *is* nutty, but often not. Sometimes it just seems to be an idea that there is no current momentum for. I believe that eventually many of these ideas will be heard. There will be a lot of fundamental financial and social insurance innovations in coming years, coming from people all over the world who are inspired by financial theory and the rapid advance of information technology.

NOTES

1. Lucas (1975).
2. Sutch (1968).
3. Shiller (1972).
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