Theoretical Foundations: Discussion

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Bradford Cornell
William Goetzmann
Roger Ibbotson
Martin Leibowitz
Rajnish Mehra
Thomas Philips
Robert Shiller
Richard Thaler

STEPHEN ROSS (Moderator)

I have a few brief comments. They will be brief for two reasons. First, I am confused. Second, even in my confusion, I am in the uncommon position of not having a lot to say. Let me turn first to Cliff Asness's presentation.

What is puzzling to me about Cliff's presentation is that the discussions about P/Es and other broad descriptors of the market seem to me to be discussions that we could have held 100 years ago. The vocabulary would have been a little different, but in fact, not only could we have held the discussion, I suspect these discussions *were* held 100 years ago. So, I don't think we are saying many things differently now than we said back then.

What is troubling to me is that we are supposed to be making progress in the theory. To the contrary, the theory seems to me to be in a wasteland, not just regarding the risk premium but, more generally, in much of finance. We are in a period of time, a phase, in which data and empirical results are just outrunning our ability to explain them from a theoretical perspective. This position is a very tough one for a theorist who used to dine high on the hog when we had derivatives pricing, where theory worked wonderfully. Now, we are interested in theory to explain the problems, which is not working quite so wonderfully.

It seems to me that the issues involving P/Es are issues involving whether or not these processes are mean reverting. Obviously, something like the P/E has to revert to the mean; it is only a yield. Jonathan Ingersoll made a wonderful comment about interest rates and whether interest rates revert or not. He noted that interest rates existed 4,000 years ago in Egypt and if interest rates didn't mean-revert, they would be 11,000 percent today. So, they have to revert.

We know P/Es revert, but they seem to revert very slowly, and we are able to measure the reversion only with great difficulty. Our efforts to measure, for example, stock returns—not actual returns but expected returns—have basically been futile.

I also have some comments about Richard Thaler's presentation. I am often characterized as a defender of the neoclassical faith. I know I am because often I am asked to debate Richard. Sometimes, however, I am characterized as a shill of the neoclassical school. So, it is not clear to me which position I am supposed to represent in the minds of market pundits. But I will say that I feel a bit like one of those physicians with a gravely ill patient to whom I would like to suggest the possible benefits of herbs and acupuncture—alternative medicine. I call for "alternative finance," not behavioral finance as the alternative approach, but an alternative that may offer a little bit of hope.

What I actually think is that our prey, called the equity risk premium, is extremely elusive. We cannot observe the expected return on stocks even with stationarity in time-series data because volatility and the short periods of time we are able to analyze give us little hope of actually pinning down a result. The best hope, from the empirical perspective, seems to lie in cross-sectional analysis, which is not what we are talking about here; we are talking mostly about *time series*, for which we do not have many observations. Cross-sectional analysis says that the excess returns should be the risk premium times the beta. If we could find some way to spread excess returns, maybe through P/Es of individual stocks, then we'd have a better chance of measuring expected return at each point in time—no matter what theory we decide to pin our hopes on.

The theory itself is a myth, and in this case, Richard and I are in complete agreement. Any hope of tickling, or torturing, some reasonable measure of the risk premium out of consumption data is forlorn. It resides in the hope that somehow people are rational.

I love old studies. For example, in one study on consumption data that was done mostly in Holland, the researchers observed shoppers in supermarkets to see what happened when the price of soap was higher than the price of bread. These shoppers did not adjust their marginal rates of substitution to the prices of consumer goods at a single point in time, let alone in the presence of uncertainty and over time. But consumption theory has always said that people would adjust their marginal rates of substitution for prices that evolve over time in a stochastic world.

I am not at all surprised, nor am I troubled, by the fact that we do not find any meaningful correlations between something that we may or may not be able to measure, such as expected return and consumption, and the interplay between them. So, I applaud Richard's view that we ought to consider other reasons to explain why people do what they do.

The real puzzle may be: Why do investors behave the way they do based on what the premiums actually are? And here too, I have to say that even though neoclassical theory is not up to the task of explaining this behavior, and it is not doing a good job, I am not sure that behavioral theory has much more to say to us.

Behavioral anecdotes and observations are intriguing. Behavioral survey work is empirically fortified. But behavioral theory does not seem to have a lot of content yet. In interpreting the study that Richard mentioned about the incompatibility of two gambles, one has to be very careful. Those gambles are incompatible if they are assumed to hold over the entire range of the preference structure. But there is no reason to believe that the gamble holds over the entire range of the preference structure. We do not believe that if the guy wins $20 million he won't take the 110 to 100 gamble. The uniformity requirements in that assumption bend the question. A lot of curious things are going on in those kinds of analyses of behavioral assumptions. And even the richer models, such as those of DeLong and Shleifer (1990), have their own problems.

In summary, I am a theorist and I am confused. I would like theory to make progress, and I would like for us to be able to address some of these issues successfully. I do not really care whether we do so from a neoclassical or another perspective, but I find myself facing an enormous, complicated array of phenomena that come under the heading of "the equity risk premium puzzle" and I'm completely unable to explain any of it.

RAJNISH MEHRA: One thing that Richard Thaler missed was that most of these models do not incorporate labor income. Constantinides, Donaldson, and I (1998) have been doing work in this area for the last couple of years. We have been analyzing the implications of the changes in the characteristics of labor income over the life cycle for asset pricing. The idea is simple: The attractiveness of equity as an asset depends on the correlation between consumption and equity income, and as the correlation of equity income with consumption *changes* over the life cycle of an individual, so does the attractiveness of equity as an asset. Consumption can be decomposed into the sum of wages and equity income. A young person looking forward in his or her life has uncertain future wage *and* equity income; furthermore, the correlation of equity income with consumption will not be particularly high as long as stock income and wage income are not highly correlated. This is empirically the case. Equity will thus be a hedge against fluctuations in wages and a "desirable" asset to hold as far as the young are concerned.

Equity has a very different characteristic for the middle-aged. Their wage uncertainty has largely been resolved. Their future retirement wage income is either zero or fixed, and the fluctuations in their consumption occur from fluctuations in equity income. At this stage of the life cycle, equity income is highly correlated with consumption. Consumption is high when equity income is high, and equity is no longer a hedge against fluctuations in consumption; hence, for this group, equity requires a higher rate of return. The way Constantinides, Donaldson, and I approach this issue is as follows: We model an economy as consisting of three overlapping generations—the young, the middle-aged, and the old—where each cohort, by the members' consumption and investment decisions, affect the demand for, and thus the prices of, assets in the economy. We argue that the young, who should be holding equity, are effectively shut out of this market because of borrowing constraints. In the presence of borrowing constraints, equity is thus exclusively priced by the middle-aged investors, and we observe a high equity premium. We show that if there were no constraints on young people participating in the equity markets, the equity premium would be small.

So, I feel that life-cycle issues are crucial to any discussion of the equity premium.

JOHN CAMPBELL: I want to follow up on the point Rajnish Mehra made because one part of Richard Thaler's talk was normative analysis—the claim that if the equity risk premium is as much as 4–5 percent, long-term investors should obviously hold their money in stocks or even leverage a position to hold their money in stocks. I think that, as a normative statement, that prescription is simply wrong.

I am going to take as a benchmark a model with constant relative risk aversion at some reasonable, traditional low number. The simple formula for the share you should put into stocks if you are living off your financial wealth alone and if returns are distributed identically every period is as follows: the risk premium divided by risk aversion times variance. Suppose the risk premium is 4 percent and the standard deviation of stocks is 20 percent; square that and you get 4 percent. Now, you have 4 percent divided by risk aversion times 4 percent. So, if your risk aversion is anything above 1—say, 3 or 4—you should be putting a third of your money in stocks or a quarter of your money in stocks. It is just not true that with low risk aversion and a risk premium of 4–5 percent you should put all your money in stocks.

So, what's happened to the puzzle? Why don't I get an equity risk premium puzzle when I look at it from this point of view? Well, the key assumption I made is that *you are living off your financial wealth entirely.* It follows then that your consumption is going to be volatile because it will be driven by the returns on your financial wealth. The only way to get an equity risk premium puzzle is that when you look at the smoothness of consumption, you see that it is much smoother than the returns on the wealth portfolio. Why is that?

Rajnish's point is that other components of wealth, such as human capital, are smoother, which is keeping down the total risk of one's position. If you have these other, much smoother human assets, then of course, stocks look very attractive. But I think it's important not to assert that a risk premium of 4 percent should induce aggressive equity investment.

I am reminded of Paul Samuelson's crusade over many years to get people to use utility theory seriously, as a normative concept. He was always trying to combat the view that you should just maximize the expected growth rate of wealth. He got so frustrated by his inability to convince people of this that he finally wrote an article called, "Why We Should Not Make Mean Log of Wealth Big Though Years to Act Are Long" (1979). It is a wonderful article, and the last paragraph says, "No need to say more, I've made my point and but for the last word, I've done so in words of but one syllable." And every word in the article is a one-syllable word except for the last word. It is almost impossible to read, of course, but the point is important: We may not want to use standard utility theory as a positive theory, but we should try to use it as a normative theory, in my view.

ROSS: If you are going to use it as a normative theory, though, you do not have to place your attention entirely on the constant relative-risk-aversion utility function. The broader class of linear risk-tolerance models has exactly the same function (with the addition of deterministic parts to the income stream), except they work in the opposite direction. So, if someone has a linear risk tolerance with a high threshold for that risk tolerance, then the equity risk premium puzzle reappears because the desire to invest is huge even when the risk premium is relatively low.

RICHARD THALER: Let me respond briefly. You have all these models that are based on consumption, and it is true (and I appreciate John Campbell's clarification) that to really understand this puzzle, you need to emphasize consumption smoothing. Otherwise, you get precisely the result that John suggested.

But the puzzle I was informally identifying before refers to other investors that I think have been neglected in much of this theoretical research. Those simulations that Marty Leibowitz was doing were mostly for defined-benefit pension funds, and I did some similar simulations for a foundation that I've been associated with over the years. Foundations have 5 percent mandatory spending rules. Now, if you crunch the numbers and you are investing in bonds, basically you are certain to be out of business in the near future unless you can find some bonds providing a 5 percent real rate of return. With TIPS we were getting close for a while.[1] But if the real interest rate is 2 percent and you have to spend 5 percent, you are soon going to be out of business. One question I have for the theorists, of which I am not one, is: What's the normative model we want to apply for those investors and what does it tell us about the kind of risk premium we should expect?

BRADFORD CORNELL: I have one question: Most of you are involved in one way or another with investment firms, and it is almost a mystery to me that you read academic papers where you see things like "consumption process," "labor income," "risk aversion," and so on, and then you attend an actual investment meeting—where none of these concepts are even remotely talked about. So, how do you bridge the gap between the supposed driving factors of the models and equilibrium returns and the way people who are actually making decisions make them? Is there a way to tie all of it together?

ROSS: There does seem to be a disconnect between the two areas and the two literatures. It is, actually, a fundamental theoretical disconnect. In these markets, with their many institutional players, the institutions are typically run by managers under some type of agency structure. So, there must be some sort of agency model for the people who run the pension funds and other institutions. They are the ones who

[1] Treasury Inflation-Protected Securities; these securities are now called Treasury Inflation-Indexed Securities.

make investment decisions. In the theoretical structures we build that include consumption, we seem to have the view, or maybe just the wishful thinking, that whatever the underlying forces in the economy are, these institutions will simply be transparent intermediaries of those forces, so the agents who are representing these institutions will simply be players in people's desire to allocate consumption across time or will be dealing with the life-cycle problems of people. Some take a Modigliani view that the *people* will adjust their actions around whatever the agents do. The net result is that the actions of the agents and the people coincide, which seems to me overly hopeful. I don't believe it is the case.

CLIFFORD ASNESS: Is it more complicated than saying the description Richard Thaler presented works better for what actually happens in a boardroom than any of the theory? Behavior like myopic loss aversion is true. Many of us have behaved that way. The fact that people make choices in the ways that they do does not have to be proven by a survey. As a manager who has gotten way too much money after a good year and too many redemptions after a bad year, I can tell you people focus on the short term.

I have one comment about Steve Ross's initial response. I don't think anyone would argue about the fact that P/Es are mean reverting. But that is not the exciting part of the puzzle. The exciting part, which is incredibly challenging, is that if we all accept that P/Es are mean reverting to an unconditional mean, what we are disagreeing about is what that unconditional mean either should be, in theory, or is. Mean reversion is a pull toward something, and the open issue is not mean reversion but whether the "right" (meaning unconditional mean) P/E is 15. If it is and we are in the high 20s, then mean reversion is not going to work as a good model for the next year. But the pull was downward for a long time, so I do not think my comments were trying to be insightful about P/Es being mean reverting. They have to be, or else they are unbounded in some direction.

MARTIN LEIBOWITZ: This is just strictly an observational comment, not a theoretical one, and it has to do with the comment about myopic loss aversion or myopic return attraction, which is the other side of the coin. As Cliff Asness said, there's clearly some pain in the short term and also some joy in the short term, depending on your outcomes. But I think what actually happens is that people incorporate a kind of Bayesian revision, that the prospects for the future are based on what have been the most immediate short-term returns.[2] We see it in terms of the flow of funds into, for example, TIPS—a wonderful instrument with a great yield, a +4 percent real rate. We couldn't get anyone to invest in them until, suddenly, we had a 12.76 percent return year in the equity market, at which point, of course, the real return on equities was a lot lower than it had been and money started flowing into TIPS big time. Short-term return is a very powerful force.

THALER: Aren't you too Bayesian, then, to be sarcastic?

LEIBOWITZ: Yes, Bayes would recoil because in the fixed-income area, this short-term focus is clearly, you know, a kind of nuttiness, although there's something to it. It does show that real rates can decline. I think some people were thinking: Why were we stuck with real rates in the area of +4 percent? So, myopic loss aversion is not totally irrational, even in the fixed-income area. In the equity area, where the risk premium is so elusive and unmeasurable, I think that investors do place a lot of weight on these myopic results, and not just in the short term; they are interested in what the data say about the long term.

ASNESS: Can we call it Bayesian without priors?

LEIBOWITZ: I think there are priors. I think there really is a Bayesian division going on.

THALER: I want to explain that in the study by Marty Leibowitz, which I so meanly presented, one of the conclusions he reached is that those 20-year numbers look really, really good but that the plan sponsors, the target audience of Marty's study, were going to have to answer some difficult questions over the next two or three years. This problem is an agency problem. The investment committee or whoever is making the investment decisions will get a lot of heat if lots of losses occur on their watch. Typically, the manager running the pension plan is going to be in that job for only two or three years and will then rotate into another job.

ROSS: That agency problem exacerbates this issue even further. With the distinction between the real economy (represented by Rajnish Mehra and John Campbell) and the financial markets, the transmission

[2] Bayes' Law determines a conditional probability (for example, the probability that a person is in a certain occupation conditional on some information about that person's personality) in terms of other probabilities, including the base-rate (prior) probabilities (for example, the unconditional probability that a person is in an occupation and the unconditional probability that the person has a certain personality).

mechanism through institutions becomes even more difficult to explain. Are those who run institutions subject to a variety of psychological vagaries of this sort? Why, if this is an agency problem, has it been so poorly solved to date? It seems to throw up even more theoretical puzzles for us.

LEIBOWITZ: Just a real quick response. That research of mine that Dick Thaler mentioned actually spurred a whole series of papers in which we looked at all kinds of reasons why people would not be 100 percent in stocks. We looked at it from all kinds of different angles—both theoretical and empirical—and we always kept getting this kind of lognormal type of distribution with nice, beautiful tails; it was pretty weird never to see underperformance over long periods of time.

The only conclusion we could finally come to was that, basically, as people peer into the future, they see risk. They are not talking about something with volatility characteristics. They are not talking about return that behaves in a linear fashion. But they see something out there that, basically, fundamentally, scares them. They can't articulate it, but it keeps them from being 100 percent in stocks.

CAMPBELL: I want to defend the relevance of consumption, even in a world with both behavioral biases and agency problems. It would be ludicrous to deny the importance of those phenomena, but even in a world with those phenomena playing a major role, consumption should have a central role in our thinking about risk in financial markets. In the long run, consumption drives the standard of living, which matters to people. So, consumption is a very influential force in investors' decisions.

Can consumption models be applied to endowments, to long-term institutions? I argue that they can, and I have some knowledge of this issue from talking to the managers of the Harvard endowment. Harvard's new president, Lawrence Summers, is trying to make sense of Harvard's spending decisions, which have always been made on an *ad hoc* basis. The endowment maintains very stable spending for a number of years, and then spending rises periodically. Now, in many universities, endowments generally have a smoothed spending rule, so spending levels are linked to past spending levels and the recent performance of the endowment. This rule makes perfect sense if you think that universities get utility from spending but also have some sort of habit formation. It is internal as related to their own history: They hate to cut the budget because it is really painful, the faculty are up in arms, and the students are screaming. And it is related to external situations: They hate to fall behind their competitors. I know that the Harvard endowment managers look very carefully at the management of the Yale endowment, because there's nothing worse than having Yale outperform Harvard. So, habit formation and consumption spending are extremely relevant to endowments. The relationship may be a little more complicated than just saying, "Oh, they have power utility," but you can make sense of the way they think by reference to spending, not only at the micro level but also in terms of the aggregate consumption in the economy.

In the long term, the correlation between consumption growth and the stock market has been quite strong—in the United States and in other countries. And it makes sense. We know that when the economy does well, the stock market does well, and vice versa. There is a link, a correlation, and it represents a form of risk over the longer run.

Aggregate consumption is also an amazingly accurate measure of the sustainable long-term position of the economy. We know that consumption, financial wealth, and labor income are all held together by budget constraints. You can't let your consumption grow indefinitely without some reference to the resources that are available to support it. So, no matter what the behavioral influence is, there is still a budget constraint that is bound to hold consumption, wealth, and income together. You can ask the empirical question when you look at the data: What adjusts to what? If you have a behaviorist's view, you might think that consumption would adjust to the harsh realities of the budget constraint over time. Instead, what seems to happen is that consumption follows a random walk—as if it is set to the level that is sustainable at each point in time. When wealth gets out of line or income gets out of line, they adjust to consumption. So, there's short-term volatility in the financial markets, but when financial wealth is very high relative to consumption, what tends to happen is financial wealth falls. That is just a fact, it does not suggest a particular model, but I think it does suggest the relevance of consumption—together with agency problems and very interesting and important behavioral phenomena—in thinking about the markets.

CORNELL: If consumption is relevant, what type of information would you expect to see flowing through the pipeline of an organization such as TIAA-CREF? How would you expect to see information flowing from the ultimate clients, who are the consumers, into the organization so that the organization can act as the agent on their behalf?

CAMPBELL: Well, TIAA-CREF is running a defined-contribution pension plan. So that, in a sense, information does not have to flow into it. But it seems to me the way to think about defined-benefit pension plans is that they have evolved over a long period of time to reflect the conservatism of the ultimate clients. For example, labor unions negotiate pension arrangements to give their members very stable income in retirement. And even if we accept that agency problems introduce imperfections, it seems to me that the liabilities defined-benefit pension plans have are very stable because of an expressed preference for stable consumption streams.

THALER: The residual claimant to those plans is the company, and the company is supposed to be virtually risk neutral. So, I think the model John Campbell described, which is sort of a habit-formation model, has some plausibility to it as applied to endowments. What is more difficult is to try to use that model in explaining the behavior of the typical plan sponsor of a defined-benefit pension plan.

ROBERT SHILLER: The general public of investors does not, of course, have an economic model like those produced by economists. They do, however, know the definition of stocks and bonds. They know that bondholders get paid first and stockholders are the residual claimants after the bondholders are paid. They know that. The original idea for a stock market was that stockholders are the people who can bear risk and that buying stocks is designed to be a risky contract—which, I think, is very much on investors' minds. So, if we tell them, "Well, in this last century, we were really lucky. Nothing really went wrong. We had five consecutive 20-year periods in which stockholders did really well," I believe that investors then think, rationally, that what we are telling them about low risk for stocks is pretty unconvincing. Investing in stocks is still investing in an asset that was designed for people who can take a lot of risk. There are no promises, and the government isn't going to bail you out if the stock market collapses. The government is perfectly free to throw on a big corporate profits tax; they've moved it up and down. And the shareholder gets no sympathy when the government does so. So, people are rational to be wary, to require a high expected return to take that risk.

ROBERT ARNOTT: I think in this whole discussion of risk premiums we have to be very careful of definitions. In terms of expected returns on stock, there is the huge gap between rational expectation based on a rational evaluation of the sources of return, current market levels, and so forth, versus hope. The investors out there are not investing because they expect to earn TIPS plus 1 percentage point.

And we have a semantic or definitional problem in terms of past *observed* risk premiums, exemplified by the Ibbotson data, between a *normal or unconditional* risk premium, which a lot of the discussion so far seems to have centered on, and the *conditional* risk premium based on current prospects. So, one of the things that we have to be very careful of is that we clarify what we're talking about—past observed risk premiums, normal (unconditional) risk premiums, or conditional premiums based on current prospects.

ROGER IBBOTSON: We have talked mostly about either the behavioral perspective or the classical (or neoclassical) perspective. The classical approach can be interpreted or reinterpreted in many ways as we get more and more sophisticated in our understanding of what the risk aversion might be for the predominant people in the market. And we can put behavioral overlays on classical theory. Ultimately, I think this topic is a rich land for research, and I encourage it, but we are not very close now to getting a fix on an estimate for the risk premium. At first, it appeared that theory suggested low risk premiums, as per Mehra and Prescott (1985), but I think at this stage of the game, using classical theory with behavioral overlays, we can't pinpoint the answer.

THOMAS PHILIPS: An idea that ties together many of the discussions associated with the risk premium is the notion of how to estimate something if you don't have a model or if you're not sure what you are doing. The typical answer is to take the historical average or the sample mean. If we stop to consider why investors buy TIPS at certain times and pull out of hedge funds at other times, we find, more often than not, that the answer is grounded in their use (and abuse!) of the sample mean of the historical returns of that asset class. The trouble is that the sample mean is a terrible estimator. It is easy to show that the sample mean can have huge biases; you just have to vary the risk premium a little bit, for example, or have slightly different economic assumptions, and the estimate and reality diverge sharply. But the sample mean does seem to be the driving force behind most people's behavior. What you observe at cocktail parties or working with clients is this enormous drive toward investing in the asset class with the highest historical return. And I believe it is a fundamentally bad way to think about the problem.

MEHRA: I want to say a couple of things in defense of neoclassical economics. First, for psychological vagaries and other behavioral phenomena to affect prices, the effect has to be systematic. Unless these phenomena occur in a systematic way, the behavior will not show up in prices. So, one has to be very careful about saying, "This is how I behave so I should model market behavior that way." Many of our idiosyncrasies may well cancel out in the aggregate.

Second, most of our economic intuition is actually based on neoclassical models. Ideally, new paradigms must meet the criteria of cross-model verification. Not only must the model be more useful for organizing and interpreting observations under consideration, but it must not be grossly inconsistent with other observations in growth theory, business cycle theory, labor market behavior, and so on. So, I think we should guard against this tendency of model proliferation in which one postulates a new model to explain each phenomenon without regard to cross-model verification. A model that is going to explain one part of reality but then is completely inconsistent with everything else does not make much progress. That is my biggest concern.

ROSS: It seems to me also that there is a vocabulary issue at work here. We have heard the phrase "habit formation" used by many people to mean many different things. On the one hand, the term is used by the behavioralists as though it is some kind of psychological phenomenon. On the other hand, John Campbell uses it as a description of the way universities behave. In either case, it is difficult to tell the difference between whether some fundamental underlying costs that universities face produce a behavioral pattern that looks like habit formation on the preference side but might have nothing to do with it or whether the universities' preferences are perfectly independent across time, are intertemporally independent, but the basic cost structure induces a net behavior that looks like they're concerned about what they did in the past or they are concerned about preserving what they did in the past.

The same is true on the behavioral side. It could well be that there is some fundamental psychological underpinning that we can argue for in terms of habit formation. All you are really saying is that, on the preference side, people don't have adequately separable preferences all the time, that there is some induced link between preferences at one point in time and consumption at one point in time and consumption at another time. There may be some substitutability that we are not capturing in the additive case. So, I think that all of these phenomena have the funny and interesting property that both the neoclassical economist and a purely psychological economist, or behavioral economist (I don't know what the proper phrase is anymore), could wind up saying that the reduced form could be the same for both of them. They just have different ways of getting there.

SHILLER: I think the difference between behavioral economics and classical economics is totally a difference of emphasis. The behaviorists are more willing to look at experimental evidence, a broad array of evidence. Indeed, expected utility is a behavioral model; psychologists also talk about expected utility. So, I think the difference is somewhat methodological; it is not a subject matter difference. It is a question of how willing you are to experiment with different variations.

THALER: Well, habit formation is obviously to some extent a description of preferences. Nothing says it's irrational. The simple additive (and separable) model is the easiest to use, so we naturally started with that model. But you could add completely hypo-rational agents who have preferences that change from one period to another, and you could, of course, have agents who are making the so-called Bayesian forecasts that Marty Leibowitz referred to with those same preferences.

ROSS: There are some exceptions, though, like framing or path dependence. Those tend to be time inconsistent, and time consistency is required in what we typically think of as rational models.

WILLIAM GOETZMANN: A lot of interesting theoretical work is going on, but I want to put in a plug for empirics. Theorists have looked at the price behavior of markets and of individual securities, but a lot of the models have this behavioral component, rational or otherwise, at their heart—whether in identifying the marginal investor or what have you. Yet, we have almost no information about how actual investors behave. Organizations have a lot of that information, but it may never see the light of day for our research purposes. We're beginning to see a little bit of this information cropping up here and there (and sometimes companies that allow us to have it are sorry they did). But imagine the ability to take hundreds of thousands of accounts, time series of accounts, identify the people who seem to exhibit myopic loss aversion, and then test to see whether their behavior has any influence on prices. That work would provide a way to identify whether pathologically behaved people have a short-term or a long-term influence on price behavior. In the long run, empirical study is how we are going to be able to answer some of these questions.

RAVI BANSAL: There is a lot of discussion about preferences, and many of the implementations of this theory lead to the result that asset price fluctuations are a result of cost-of-capital fluctuations. The models do not have much room for expected growth rates. The models build on a long-held belief in economics that consumption growth rates and dividend growth rates are very close to being identically and independently distributed (i.i.d.). It is the notion that most people have. I think we need to rethink that idea. A lot of hidden persistent components are in these growth processes; the realized growth process looks like an i.i.d. process, but if these growth rates have a small persistent component, the ramifications are huge. Small persistent components of any of these growth rates would have dramatic implications for how we think about what is causing asset prices to fluctuate. Statistically, there is actually some evidence to support the view that there are some persistent components in both consumption and growth rates. If such components are put into a model, the unforeseen components can explain equity premiums because consumption goes up at the same time dividends go up. News about consumption and dividend growth rates continuously affects perceptions about long-run expected growth rates, which leads to a lot of asset volatility. This channel is important for interpreting what goes on in asset markets.

Behavior is important, clearly, but understanding the dynamics of cash flows, of consumption, is equally, if not more, important. So, in a paper that Amir Yaron and I wrote (Bansal and Yaron 2000), we allowed for that possibility. And we actually show that when you rely on the Epstein–Zin (1989) preference structure and allow for intertemporal elasticity of substitution to be more than 1.0 (which makes intuitive sense to me), then you can actually get the result that during periods of high anticipated consumption growth rates, the wealth-to-consumption ratio rises. So, in terms of the asset markets, asset valuations will rise simply because of higher expected growth rates. When you require the intertemporal elasticity of substitution to be more than 1.0, then when people expect good times, they want to buy assets. I find this quite intuitive. When you allow for this possibility, you can explain through these neoclassical paradigms a lot of the equity premium and volatility in the market. So, focusing on aggregate output growth is a pretty important dimension.