

# General Discussion: Housing, Credit and Consumer Expenditure

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*Chair: Martin Feldstein*

**Mr. Lazear:** I understand, John, the mechanism that you are talking about. It is certainly a very interesting one. It is a little surprising, though, in the context of a life-cycle model or permanent-income hypothesis, that we think as credit markets get better and our ability to smooth consumption over time gets better that these effects are actually going to become more pronounced, so you are going to see more volatility from the housing wealth effect.

I guess the question I would have in mind is, If I think about this in terms of total consumption, as Sydney Ludvigson was thinking about it as well, how do those effects offset this? For example, labor earnings would be the obvious thing that would come to mind in this context. You see a downturn in housing. That would be correlated probably with downturns in labor earnings. The question is, If I am looking at this in the aggregate and trying to understand how these things are correlated over the cycle, do I have to think about more or less smoothing over time?

**Mr. Visco:** I liked the paper very much. Perhaps the only shortcoming of the paper is that it is based on time series data, and therefore you are unable to distinguish households according to whether they are homeowners or renters. This is, however, a very important characteristic of your model. In fact, you state that the net result

might be zero exactly because the renters may compensate any possible effect of homeowners.

You also said that in Italy there are large differences compared to countries where financial markets are much more developed. I think, however, that recent changes in financial markets have substantially reduced these differences.

We also have evidence that the distinction between homeowners and renters is extremely important in Italy. In a recent Bank of Italy study, we examined the behavior of more than 50,000 households over a period of more than 10 years. We obtained a relatively large effect for homeowners, with a propensity to consume out of housing wealth more or less around the figure you have—3.5 percent. However, there is a negative effect, exactly as you were suggesting, for renters.

My question is whether there is any possibility to use large cross-section or panel data for other countries. Certainly in the United States, there must be data that allow you to measure the different impacts of changes in house prices on these two different, very important sets of consumers.

**Mr. Makin:** A question for Sydney Ludvigson: You just finished your presentation by talking about housing wealth effects and consumption, then conditioned it by saying, “Unless there are spillover effects.”

Could you expand on that a little bit? And, secondly, a little bit more detail on the covariance terms in the nonhousing consumer durables table would be interesting.

**Mr. Blinder:** One question for each person, and these are real questions. I don't have answers in mind for each.

For John: As I understand the construction of the credit conditions index in the United Kingdom, and even more so in the United States where it comes from the Senior Loan Officer Opinion Survey, the question arises whether the index is actually reflecting mostly institutional legal changes, which you emphasized a lot in the narrative, or market behavior for a given institutional legal structure.

The question for Sydney relates back to an earlier discussion, especially of Bob Shiller's paper. Is there a way we can distinguish between the hypothesis that it is risk premia that are shrinking versus expectations that are soaring, which was the Shiller hypothesis?

**Mr. Muellbauer:** To Ed Lazear: His question is a very interesting question, which is quite similar, in some ways, to a brief discussion I had with Bill White. Bill White said, "There is going to be a payback. Increased access to credit means you borrow more. If you borrow more, you have to pay it back eventually."

In a way, part of the answer to that you can see in Chart 3 of the paper. In Chart 3 of the paper, we plot the long-run effect on the consumption-to-income ratio of the two financial assets. The long line that has a declining trend is net liquid assets. That is, net liquid assets minus debt divided by non-property income. The chart shows the contribution of that to the long-run consumption-to-income ratio. So, you can see there is a very big offset from financial liberalization because people have acquired a lot more debt. So, net liquid assets are now negative, compared with the early days. That is offset, to a large extent, by the financial market-credit market liberalization. That answers both Bill White and you, to some degree at least.

Turning to Ignazio Visco's question. I am very much in favor, of course, of using good panel data where those data exist. We are planning ourselves to return to the U.K. micro evidence to see whether we can produce more robust estimates than the two studies that are listed so far.

As far as Italy is concerned, my worry is whether the omission of income changes may not have biased the collateral effects or the wealth effects of the estimates, but I very much support that style of work.

On Alan Blinder's question on the endogeneity of the credit conditions index: In the Bank of England paper, if you look at it (it is rather a long paper), we really do everything possible to prevent that credit conditions index from being endogenous. We controlled for many variables. We have examined sample selection effects. It has been very careful work to exclude that possibility, with a lot of attention to various measures of risk to try to eliminate the correlation.

For the U.S. credit conditions index that we have, it is pretty provisional so far. This is work with John Duca. I think your point is absolutely right. There is a bit of endogeneity in there still, which we soon have to eliminate. If we use the HP-filtered version of it, it gets rid of a lot of the wiggles. We get very robust results, very similar results. I don't think the overall shape of the thing is going to be very different. But you are right. One has to be very careful not to confuse short-run endogenous variations due to other things with the evolution of the supply function for credit.

*Ms. Ludvigson:* John Makin asked about the spillover effects. What I was referring to there was that it looks like from the empirical analyses that I have seen—including John Muellbauer's own work—that other fundamental determinants of consumer spending are more important than are fluctuations in housing wealth. Variables such as the unemployment rate and income seem to be very important in determining contemporaneous consumption fluctuations. The empirical relationships we have been discussing today are marginal relationships; that is, they show the effect on consumer spending of housing wealth or credit market conditions, controlling for income and income expectations. In thinking about the overall change, we want to take into account the possibility that changes in housing wealth might influence these other fundamentals, which in turn influence spending.

The covariance terms you asked about combine several other aspects of the model we investigated. If you look at your footnote, you see an expression, which is an approximate equilibrium condition in the model. These covariance terms refer to covariance between surprises in consumer spending and some of the other terms on the right-hand side of that expression. We can talk about that more at length, if you like, afterward.

Alan Blinder had a question. How do we distinguish a hypothesis of risk premia versus expectations soaring? If you don't mind, I'll give a rather roundabout answer to this question. This is not an easy question to answer. In the discussion, I had said if we accept for the moment that housing price-dividend ratios will continue to fluctuate

within their old historical ranges, then one of two things must happen. However, it is possible that these ratios could remain above their pre-1998 historical norms for a very long period of time.

I've done some work with Jessica Wachter of Wharton (not to be confused with Susan Wachter, who is her mother and who presented a paper earlier) and Martin Lettau, where we analyzed in a rational asset pricing model what the effects of the so-called Great Moderation were on risk premia in the equity market. We calibrated the model, fed in estimates of the changing volatility of fundamentals—mainly consumption—and found you could explain a large fraction of the sustained run-up in equity values relative to earnings and dividends. They still remain above their historical norms prior to the mid-1990s via this decline in macroeconomic risk essentially.

That is a model-based way of thinking about whether changes in asset market valuation ratios may be attributable to risk premia. With housing wealth, it strikes me that the situation is more complicated because houses effectually pay a dividend equal to the service flow from the durable consumption stock.

It is possible that fluctuations in housing dividends provide simply a hedge against rent risk, which is a point made by Todd Sinai and Nicholas Souleles. So, if you think housing consumption has become less volatile, as it surely has with the Great Moderation, this might have dampened volatility in house prices and might have increased the demand for renting relative to buying. At the same time, housing consumption capitalizes the value of the living conditions in the general environment in which a home is purchased. You can imagine that a decline in the volatility of those services might instead lead to an increase in the demand for housing assets.

So, you can look at calibrated models to see whether rational models, which attribute fluctuations in valuation ratios to movements in risk premia, fit the data.

If you want to go model-free, then maybe surveys would be helpful. However, surveys have their own limitations. For example, in this work with Jessica and Martin, there is a transition period, as individuals learn

about the shift to a new lower-volatility regime. If you ask people in a survey about their expectations for prices going forward, you have to be careful about where you are in that transition period.

Are you at the beginning of the transition to a new persistently higher valuation ratio? In that case, you would rationally expect higher prices, and that would be perfectly consistent with a decline in risk premia.

**Mr. Mayer:** I have two comments for Sydney. The first is about the first couple of graphs, which show the price-dividend ratio is affected a lot by long-term interest rates. That comparison converging back to its long-term average would depend on real interest rates moving back up again. The role of interest rates in the price-dividend process matters a lot. It is hard to think about the price-dividend ratio without putting in the real interest rate term, which is part of why I am a fan of thinking about the user cost.

The second is that measuring dividends on owner-occupied properties is enormously difficult to do because you don't really know how homeowners value that dividend. Some people just multiply the interest rate times the price. Again, the user cost model provides a better framework. Similarly, within the user cost model, Alan Blinder's comment is exactly right. In principle, there is no way of differentiating between risk and expected appreciation. Those literally are exactly the same effect on the price-dividend ratio. That is a difficult problem.

In terms of estimating the risk premium, it is enormously important to use cross-sectional data because the volatility of house prices across different markets varies enormously. You could look at markets like Houston, where there is very little volatility, and markets like New York, where there is a lot. That could help in thinking about how to incorporate volatility in these models.

**Mr. Sinai:** This is a quick technical question for John Muellbauer. I didn't see it, and maybe I missed the method of estimation for the time series consumption regressions in the United Kingdom and the United States and the issue of simultaneity and whether you have a lag-dependent variable in each of those. Answering the question of

how you estimated the equations will help answer for me how I take these numbers.

**Mr. Smets:** I would like to refer to some work of two colleagues of mine, Alessandro Calza and Livio Stracca, together with Tommaso Monacelli of Bocconi University. They document that the correlation between consumption and house prices differs across countries and that those differences are correlated with the flexibility of the mortgage market and associated features like loan-to-value ratios. They then show that one can replicate such a correlation in a general equilibrium model with constrained borrowers and lenders. So, in economies with higher loan-to-value ratios, you get a higher correlation between consumption and house prices. The crucial factor is that, although the loan-to-value ratios are higher, borrowers are still credit-constrained. This is an issue for the microdata. In reality, you also have borrowers who are not credit-constrained, and for those, naturally, you would think that Lazear's intuition would hold and the correlation would be less. In order to replicate a positive correlation between consumption and house prices, it seems that most borrowers—or a large fraction of borrowers—are actually credit-constrained. So, when there are shocks that move house prices, they do respond by changing their consumption.

**Mr. Muellbauer:** I forgot to reply to Sydney earlier, which was a mistake. Let me take up a couple of points that Sydney made. She says that Chart 2 results in the United States suggest that the U.S. credit conditions index has relatively low explanatory power.

That's not correct. The reason is that, in Chart 2 in these U.S. results, we include a stochastic trend. That stochastic trend incorporates the intercept shift in credit conditions, as well as other long-term factors like the rise in income and equality, changes in the Social Security system, and demographic changes and things of that sort.

In other words, when she says that the difference in fits between columns 1 and 2 is not very large, that doesn't mean very much because the intercept shift that is due to credit conditions is all there in the stochastic trend. We cannot identify which bit of the stochastic trend

is due to the intercept shift in the credit conditions index. That's the first point.

The second point really is that any empirical work from 1952 to 1998 that doesn't incorporate that very important institutional changes were taking place in the United States in that period seems to me pretty suspect. I am really not at all sure what these fitted relationships mean.

*Ms. Ludvigson:* Yes, Chris Mayer made a comment about the housing price-dividend ratio being affected by long-term interest rates. I totally agree with that. In fact, that is an important part of why the risk premium is so forecastable, I believe, because when a high-priced dividend ratio is forecasting lower housing returns, at the same time, it is forecasting higher short-term rates, and the premium itself is even more forecastable. That may be one reason we find relatively modest effects of movements in these forecastable changes in housing returns on consumption—because there are offsetting effects there.

In terms of measuring dividends, it is hard to do. I would welcome your suggestions on how to do that better and also on using the cross-sectional data to think about movements in risk premia.