HOUSEHOLD REACTION TO CHANGES IN HOUSING WEALTH

By

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It is widely claimed that home prices, as well as stock prices, have an impact on consumption

and hence on aggregate economic activity. Notably, the declining stock market since March

2000 is widely described as a contributing factor to the recession of 2001 and economic

weakness thereafter since it worked to weaken consumption growth, and the relative mildness of

the recession is often described as related to the rise of home prices, since the rise of real estate

prices since 2000 has been seen as having an effect on consumption that offsets that of declining

stock prices. But, it has been hard to quantify the separate effects of these different measures of

wealth and the impact of the wealth changes on consumption, and hence hard to quantify the

risks to the present economic expansion that might be created by falling housing prices.

In his paper for this meeting, Christopher Carroll presents a detailed list of issues in the

estimation of the separate wealth effects of stock-wealth and non-stock wealth on consumption

for the United States, a survey of the literature, some estimates of the two wealth effects, as well

as a simulation of the effects of these wealth effects in the recession of 2001. He finds a number

of complex issues regarding the estimation of wealth effects, and hence, as one would expect

given the variety of issues, a wide range of estimates. He shows that there is substantial

momentum through time in expected changes in consumption, setting aside the random-walk

argument of Hall (1978) that would have suggested that there is little point in estimating wealth

effects. Carroll's own estimates of the wealth effect show a somewhat larger wealth effect for

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consumption than for the stock market, but the difference is not statistically significant. Taking the point estimates for the wealth effects, Carroll concludes that the rise of housing wealth since the first quarter of 2000 added 2.2% to aggregate consumption by the third quarter of 2003.

Carroll's estimation method begins from an expected-utility model with habit formation. I have been skeptical of any expected utility models for consumption ever since I remember reading Shefrin and Thaler's 1988 paper on the "behavioral life cycle hypothesis," because, as they pointed out, for most people the difference between consumption and income—that is, saving—is mediated by institutions, not individual decisions. Most people save mostly in ways dictated by their home mortgage repayment schedule, their 401(k) plans, and their IRAs, and have little other savings. Moreover, Benartzi and Thaler have recently shown that a change in institutions—their Save More Tomorrow Plan—can have a powerful impact on actual saving. By analogy, recent changes in institutions, such as the spread of credit cards, the expansion of the home equity loan industry, and the industry standard of a higher loan-to-value ratio for home mortgages, must be part of the explanation for changes in saving behavior.

But Carroll is not tied to his expected utility maximization framework; his method departs from that theory in its final implementation, in that he does not present an expected-utility-theory justification for differing wealth effects of the stock market and real estate. He refers to the separate "mental accounts" (which originated in this context with Hersh Shefrin and Richard Thaler in their "behavioral life cycle theory") whereby people have a different psychological reaction to changes in different kinds of wealth. This is certainly a realistic adaptation of the theory, and there is ample micro evidence that people react differently to different kinds of wealth.

I think that it is useful to look at the US data for insights as to the impact of these wealth effects. Looking at plots of the data is always an essential thing as a check on the reality of a model. I assembled my data set separately from his, as a further check (see Appendix for sources).

I decided to plot the personal saving rate (personal saving as a percent of disposable personal income), rather than the level of consumption, which I thought would be more revealing of some basic facts. Carroll says in his paper that there are problems with basing modeling on saving rates, since the saving rate would be affected by changes in "the structure of the retirement system, the tax system, expected productivity growth rates, and many other features of the economy that have changed profoundly over time." But, Carroll presents no argument why changes in such factors would not also affect the relation that he estimates; indeed with a "mental accounts" theory it is hard to imagine how one could rule out such effects.

I personally like to view the saving rate because the percent saved out of income is a metric that many people use to describe their consumption patterns, and so we are viewing the same construct that our subjects think about. Changes in consumption are explained quite well by changes in income, while Carroll's Table 1 regressions of the change in consumption on variables excluding income yield low R squares; in contrast, a regression of annual percentage changes in consumption on annual percentage changes in disposable personal income 1948-2003 yields an R square of 0.84. It would seem natural to look at consumption relative to income and hence at saving rates.

Figure 1 shows a plot of the US personal saving rate, along with the real S&P 500 stock price index and an index of home prices, all relative to *per capita* disposable personal income (and rescaled by multiplying by a constant). All data begin in 1950 except the home prices,

which begin in 1975. The "wealth effect" may be said to refer to a supposed negative relation between the saving rate and either of the other two series plotted.

Figure 1. Saving, Stock Prices and Home Prices Relative to Per CapitaDisposable Personal Income, End of Year, 1950-2003
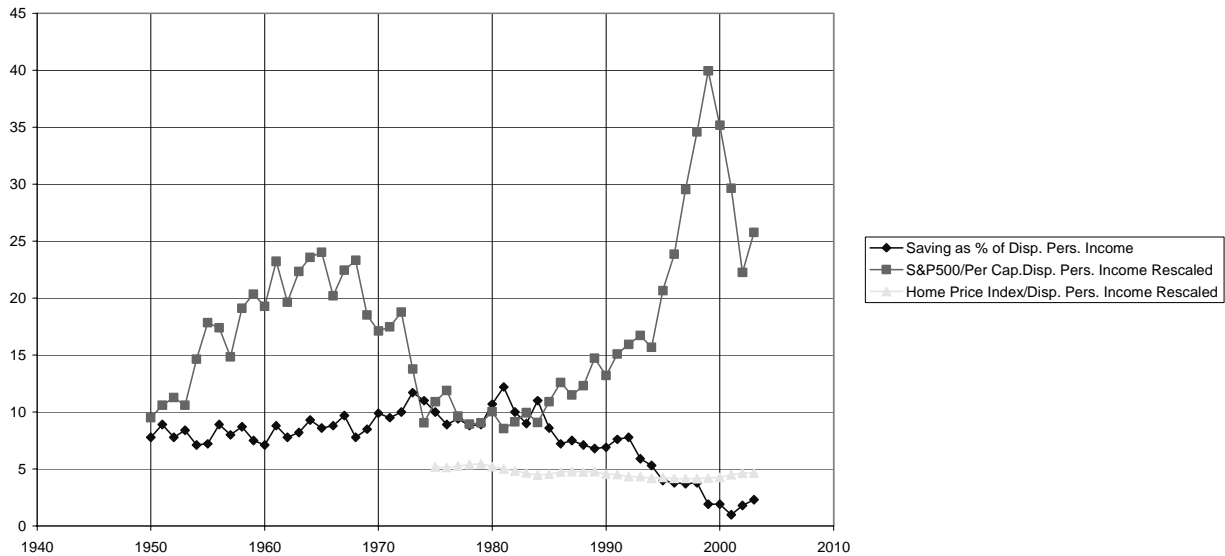
Sources of Data: See Appendix

It is plain from this figure that the personal saving rate is dominated by very low frequency movement. The personal saving rate rose from the beginning of the period to 1981, when it reached 12 percent, then declined, to very low levels today. With such a simple pattern through time, it is going to be hard to tell what the relation really is between consumption and other slow-moving variables.

From this figure, it would seem to be hopeless to disentangle the "true relation" between saving and these two wealth variables with these aggregate data. That is why, in my work with Karl Case and John Quigley, we despaired of estimating the wealth effects with aggregate data, turning instead to cross-sectional-time series data. Of course, the weakness of this approach is that cross-sectional consumption patterns may not be directly applicable in interpreting national consumption patterns.

Home prices relative to per capita disposable personal income have been virtually steady since 1975, with a slight downtrend, which happens to match more or less the downward trend of saving over the same period. No convincing evidence can be distilled from a correlation of two trend lines. We have a longer history of stock market data, and there clearly is no strong relation between stock prices and personal saving, unlikely, it would seem, to be significant unless we had centuries' more such data.

Since the stock market has made much larger swings, the market capitalization of the stock market is on the same order of magnitude of that of the housing market, it might seem plausible to suppose that the stock market has had a bigger impact on saving than has the housing market. The housing market is held more widely across the general population, but the time path of national home prices has been so tame that the housing market does not seem to be a likely candidate to be established as the major explanation of the change in the saving rate.

How then does Carroll get statistically significant estimates of the two wealth effects using only aggregate data? His model imposes substantial structure that allows him to infer from the short-run wiggles in these series some substantial long-run effects. He admits that when he left only one lag in wealth, then he found that "estimates are implausibly sensitive to the exact specification. But, I wonder how much we can really believe the results that require the

assumption of a peculiar functional form.

Missing from Figure 1 are a lot of other variables that must influence the saving rate. Carroll mentions some of these other variables. Yet other variables include the public's concerns about the increasing longevity brought on by advances in medical care, the public's changing attitudes toward investment in human capital, rather than financial investments, attitudes that may change because of widespread concerns about globalization, the impact of computers on our livelihoods, and so on, the public's changing ideas about the social status importance of owning a large home, the public's declining demand for children, or for early marriage. With all these other variables, many of them hard to measure, one might think that it is hopeless to disentangle the relations.

Of course, the original Modigliani and Brumberg life-cycle model of saving would suggest that we look at changes in the number of people in young and old generations to explain the changing pattern of saving through time. But, research shows that such demographics do not very well explain the changes in saving rates (see Deaton and Paxson). Indeed, the high number of baby boomers approaching retirement today in the United States would imply that the saving rate should be high now, not low as it actually is.

Looking at the data reminds us that part of this pattern of behavior of saving might have something fundamental to do with inflation. Figure 2 shows the personal saving rate and the inflation rate (consumer price index or CPI) for the United States. There is a much more suggestive similarity between the two series here than there is for either wealth effect seen in Figure 1. It is striking that the peak of the saving rate corresponds fairly closely to the peak of the inflation rate, that is, around 1980, just before Paul Volcker's new economic policy took effect. One sees that the two spikes in inflation, in 1974 and 1979, representing peaks in inflation

corresponding to the two oil crises, were matched by very high saving rates. Moreover, the very low saving rate today corresponds to unusually low inflation rates. However, other factors must be at work besides inflation, since when inflation was extremely low in the early 1950s the saving rate was high.

**Figure 2: Personal Saving Rate and Inflation Rate, 1950-2003**
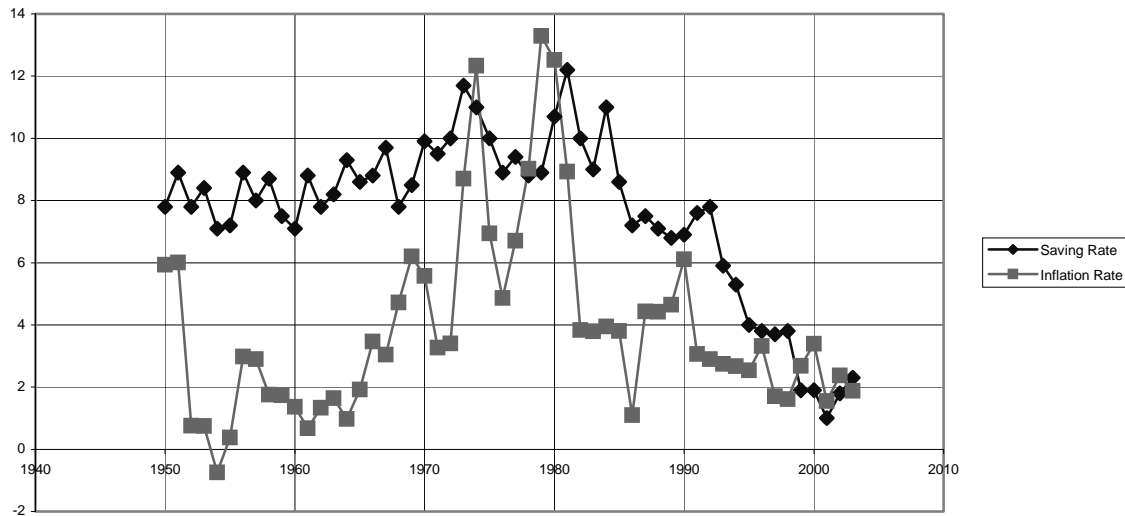
That saving rates are correlated positively with inflation rates was also noted in a cross-sectional study of saving in 150 countries over 1966-1995 (Loayza *et al.* 2000). They interpreted this correlation by interpreting inflation as a measure of "macroeconomic volatility" and thought that high inflation induces a high precautionary motive for saving. I have found in some survey work of attitudes towards inflation in a number of countries that people see inflation not just as a measure of volatility but rather more interpret it as a thief that systematically takes away their

real incomes, since they do not well understand that inflation also raises wages and salaries. Such a feeling that inflation is gnawing away at their livelihoods might well produce a higher demand for saving.

The decline in saving after 1980 may also be due to the effects of inflation on our institutions that mediate saving. The extremely high inflation around 1980, coupled with the institution of a conventional mortgage with very high mortgage payments, also forced homeowners with large mortgages to save a lot then, in the form of paying off the real mortgage very fast at the beginning. Unless nonhomeowners who were indirectly receiving these mortgage payments offset the higher saving of the homeowners, there will be a rise in aggregate saving. According to the behavioral life cycle theory of Shefrin and Thaler, the faster repayment of mortgage balances caused by inflation would not be expected to be consumed by mortgage owners, who are likely to put that income into a different "mental account." (Analogously, there are also inflation-induced distortions in the measure of the personal saving rate.)

Figure 3 shows the personal saving rate along with the constant maturity10-year treasury yield, which is a proxy for the 30-year conventional mortgage rate. The similarity between the two series is very striking. Indeed, the two series peak in the same year, 1981, and have overall a similar pattern. If one regresses, using this annual data, the personal saving rate on the ten-year Treasury yield and a linear time trend one finds an R squared of 0.90. Both the ten-year Treasury and the time trend are highly significant.

**Figure 3 Saving Rate and 10-Year Treasury Yield**
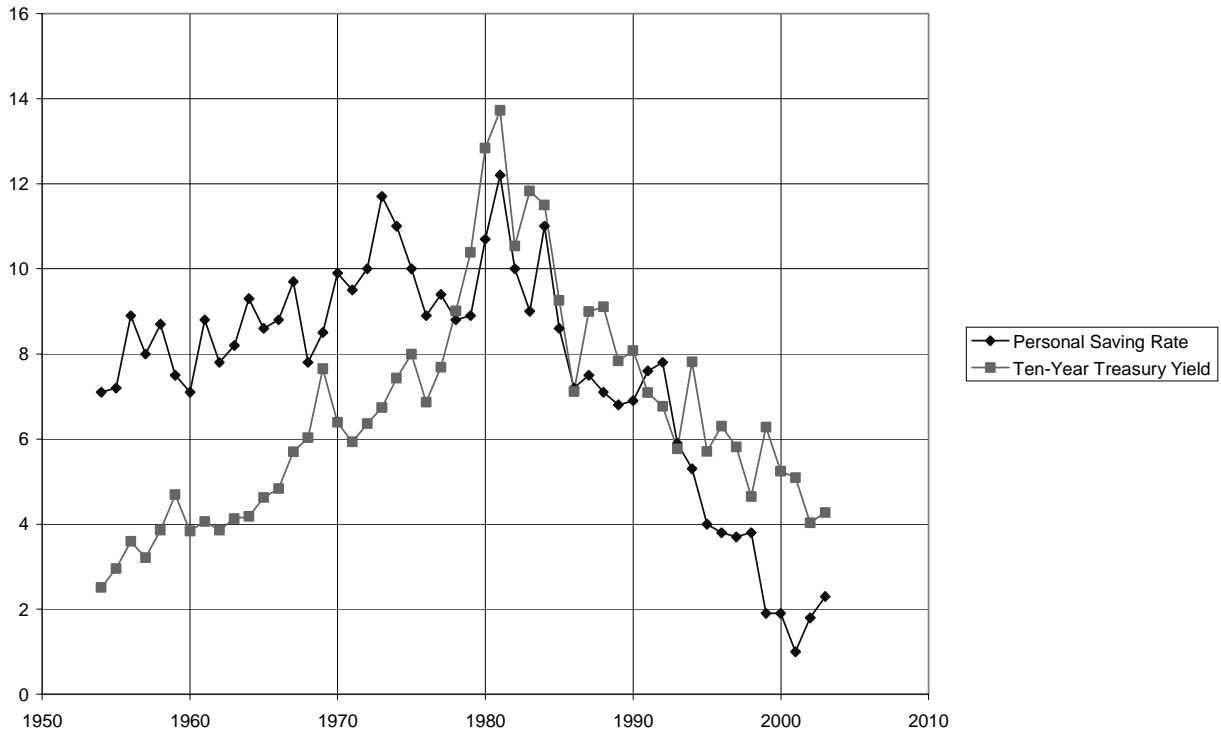
Note that the slight decline in home prices relative to national income that we saw in Figure 1 is not the same as a decline in housing *wealth* relative to national income. The repeat-sales home price index is effectively the price of a standard home, not the median home, which has been growing larger in size over the years. Over the period of 1975-2003, national home prices have been rising at a rate of 1.2% a year relative to the CPI, falling at a rate of 1.3% a year relative to disposable personal income, and virtually constant relative to *per capita* disposable personal income. We might expect housing prices to outpace the consumer price index (as Carroll notes), if the productivity growth in the relatively traditional construction sector is slower than in the rest of the economy, and because of increasing scarcity of land, but the home prices have not been growing much faster than the CPI.

The pattern of home prices in recent years is confirmed by other data that were collected and analyzed by Fiserv CSW, Inc. (formerly Case Shiller Weiss, Inc.). Figure 4 compares the annual percentage changes (fourth quarter to fourth quarter) since 1987 in both the OFHEO USA Home Price Index and the Case Shiller National Home Price Index. (In both cases the indexes are repeat-sales indexes; the data end in the third quarter of 2003, but this third quarter value was used in place of the fourth quarter after a correction was made for the usual seasonal pattern between third and fourth quarters.) The two series correspond fairly closely, even though they depend on separate data sources, and both series show a decline in prices nationally in 1990, then a steady uptrend in price increases from the mid 1990s, and most recently a fall in the rate of price increase. Incidentally, this fall in the rate of price increase is further confirmed by the Commerce Department's data showing sales of new homes down in December 2003 for the fourth straight month, and in a decline in December in the median price of a new home. It should be noted, however, that the latest data shown in Figure 4 are subject to revision, and recently the revisions of initial OFHEO data have often been upward.

Our analysis of home price data (Case and Shiller 1989) showed that home prices are anything but a random walk, and rates of increase show substantial inertia. The buildup since 1990 of national home price increases has been gradual, extending over the late 1990s, and the weakening of this trend has been in evidence for several years now. It would be reasonable to extrapolate this decline into a prediction of further declines in the home price rate of increase, possibly even leading to negative rates of change in national home prices nationally in just less than a few years.

Figure 4. Annual Percentage Change in National Home Prices, CSW and OFHEO, 1988-2003

Year (Showing End of 4-quarter period)
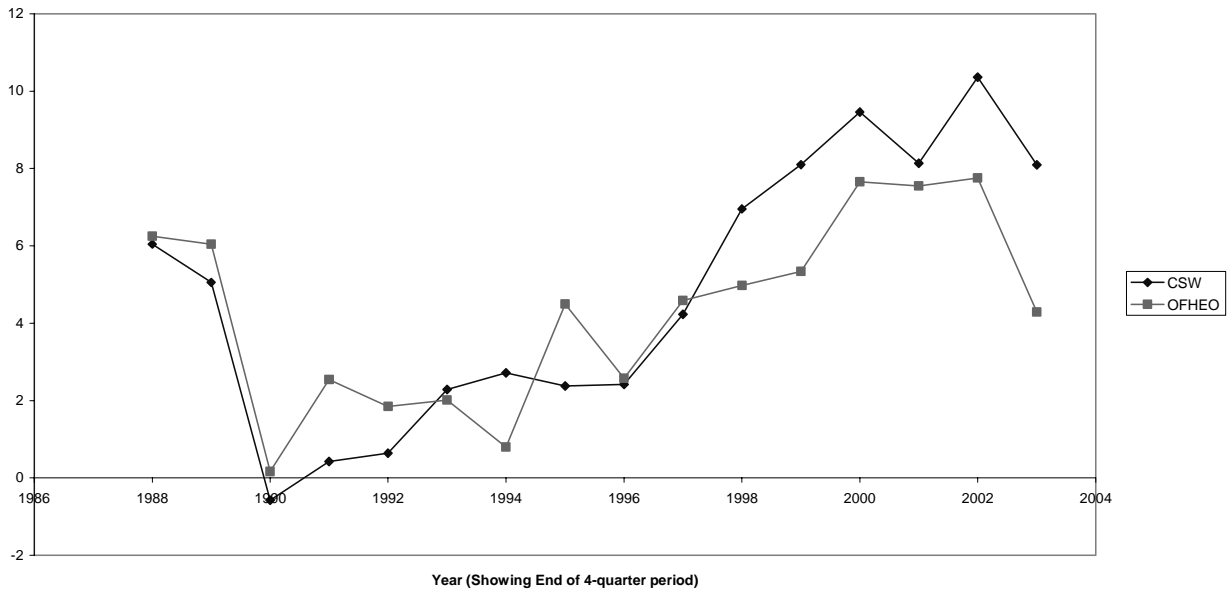
This analysis of national data masks some important differences across cities. Karl Case and I, in our recent Brookings Paper, found that volatile home prices and high and rapidly rising multiples of home prices recently relative to income have been mostly confined to just eight states, on the two coasts. We concluded that there was evidence of a speculative bubble today in those states, and that there is concern for a bursting of the bubble there and falling home prices.

In early 2003 Karl Case and I did a questionnaire survey (virtually identical to one that we did in 1988) of recent homebuyers in four US cities: Boston, Milwaukee, San Francisco and Los Angeles. Three of these cities were experiencing booms in 2003, while Milwaukee was not. These two surveys allow us to compare attitudes across cities with very different real estate price experience and across time. We found that people in some of these cities in 2003 (as also in 1988) had quite high expectations for future home price movements, even though home prices are quite high compared to historical norms for price relative to income. We asked our respondents "On average over the next ten years, how much do you expect the value of your

property to change each year?" In Los Angeles, a city that has shown extraordinary price increases, the mean answer in our 2003 survey was +13.1% and the median answer was +8.1.% If one expects home prices to go up at such a rate for such a long time, as well as pay a substantial dividend in terms of housing services, then one is expecting quite extraordinary long-term returns to owning a home, and this should produce quite a demand for housing. Unfortunately, such a demand is not likely to persist once home price increases slow down, and a decline in demand may thus produce declining prices; that is how speculative bubbles eventually burst.

Case and I tried to find out, using our 2003 questionnaire, why the housing market in some of these cities went through such an extraordinary boom right after the stock market collapsed. For example, home prices started increasing even faster in Los Angeles and Boston then they did before 2000. Our approach was very direct. We first asked our respondents whether the recent behavior of the stock market (which had of course been a huge decline) encouraged or discouraged them to buy their house, or had no effect on their decision to buy a house. Those who said that the stock market encouraged them outnumbered those who said it discouraged them by over thirty to one in both Los Angeles and Boston. The next question on the questionnaire was open-ended: "Please explain your thinking here."

Reading answers to open-ended questions requires some judgment, as certainly the answers varied widely. But, there was a common theme: the vivid experience with declining prices in the stock market, as well as the various accounting and corporate management scandals, soured them on stocks as an investment, and these people turned to investments that were "hard assets" or "solid investments" that were not likely to decline as the stock market had.

Reading these answers suggests that there were some very important attitudinal differences through time, and that these attitudinal differences that had such an impact on the stock and housing markets would plausibly also have effects on consumption itself. There is reason therefore to doubt that the relation of consumption to these wealth variables is stable through time.

The presence of such speculative booms in some cities raises some concerns, even if the US real estate market itself today looks unexceptional. The speculative booms going on in some cities could come to a sharp end, when people no longer expect such high price increases in those cities, and thus no longer have reason to want to hold so much housing.

In our cross sectional study of the wealth effect, Karl Case, John Quigley and I looked at the effect of housing wealth and stock market wealth on consumption across the states of the United States and across countries. We found evidence for a strong regional wealth effect from real estate prices.

So there are potentially serious regional problems if a real estate bubble in some cities comes to an end. A possible decline in real estate in these regions could weaken the regional economies.

Perhaps even more important, a decline in home prices in some cities could trigger problems in our financial institutions. A decline in home prices after the peak of the home price market in 1925, and accelerating down after 1929, did so. Widespread mortgage defaults contributed to the biggest banking crisis in US history in the early 1930s. The decline in urban land prices in Japan after the peak of their market in 1990 led to the kind of banking crises that have been part of the Japanese malaise ever since. A decline in home prices in the US starting in Texas in the 1980s led to the savings and loan crisis. The decline in home prices on both coasts

starting in 1990 led to a recession that had negative effects on the economy for years, and a recession that had particularly severe effects on some cities (such as Los Angeles and Hartford) that had some of the sharpest declines.

The national situation could be more severe this time than it was after 1990, since this time the loan to value ratio is much higher, and so the effects of the decline on defaults would be bigger. Our financial institutions did nothing to hedge their exposure to home price declines in 1990, and they are not doing so now. Of course, one might not blame them too strongly, since hedging instruments for real estate price risk do not exist. But, if these institutions had shown any initiative, they could have put the impetus in place to create such hedging vehicles. In fact, Fannie Mae and other institutions who face risks associated with home prices appear to be unconcerned about home price risk, even though problems that they could encounter if home prices fall could have systemic effects and might require a government bailout.

Mortgage insurers are another institution that is very vulnerable to real estate price risk and that do nothing to hedge this risk directly. If there are substantial price declines in real estate, even regionally, municipal governments might find the credit ratings on their debt reduced. A real estate price decline could also put state governments in a difficult budget situation, just as the strong effects of the stock market decline did after 2000.

I conclude that although the "wealth effect" of national home prices on national consumption may be hard to prove, there is a serious risk of the consequences of home price declines at least regionally. The regional housing bubbles that appear to be going on in the United States ought to be concerns of the Federal Reserve Board.

Appendix: Sources of Data

All Data are for the end of each year, the last month of the year for the stock price index and the last quarter of the year for the saving rate, disposable personal income, and home price index, except that for 2003 the observation is the third quarter.

The home price index is the OFHEO USA Home Price Index (quarterly, 1980-I=100).

The stock price index is the monthly average of daily closing prices for December of the Standard & Poors 500 Stock Price Index

The disposable personal income and the saving rate (quarterly, in current dollars) are from the US National Income and Product Accounts, Table 2.1, Personal Income and Its Disposition, Lines 26 and 34 respectively.

References

Benartzi, Shlomo, and Richard H. Thaler, "Save More Tomorrow[TM]: Using Behavioral Economics to Increase Employee Saving" forthcoming, *Journal of Political Economy*, 2004.

Carroll, Christopher, Housing Wealth and Consumption Expenditure, Paper Prepared for the Academic Consultants' Meeting of the Board of Governors of the Federal Reserve, January 30, 2004.

Case, Karl E., John Quigley and Robert J. Shiller, "Comparing Wealth Effects: The Stock Market vs. the Housing Market," NBER Working Paper No. 8606, November 2001.

Case, Karl E. and Robert J. Shiller, "The Efficiency of the Market for Single-Family Homes," *American Economic Review*, 79:1, 125-37, March, 1989

Case, Karl E., and Robert J. Shiller, "Is There a Bubble in the Housing Market?" *Brookings Papers on Economic Activity*, 2004-I pp. 299-362.

Deaton, Angus, and Christina Paxson, "The Effects of Economic and Population Growth on National Saving and Inequality," *Demography*, 34(1):97-114, 1997.

Hall, Robert E., "Stochastic Implications of the Life-Cycle Permanent Income Hypothesis," *Journal of Political Economy*, 86:971-97, December 1978.

Loayza, Norman, Klaus Schmidt-Hebbel, and Luis Servén, "What Drives Private Saving across the World?" *The Review of Economics and Statistics*, 82(2):165-181.

Modigliani, Franco, and Richard Brumberg, "Utility Analysis and Consumption: An Interpretation of Cross-Section Data," in Kenneth K. Kurihara, ed., *Post-Keynesian Economics*, 338-436, New Brunswick, NJ.: Rutgers University Press, 1954.

Shefrin, Hersh, and Richard Thaler, "The Behavioral Life Cycle Hypothesis," *Economic Inquiry*, 26:609-43, October 1988, reprinted in Richard Thaler *Quasi-Rational Economics*, Sage 1991, 91-126.

Shiller, Robert J., "Why Do People Dislike Inflation?" in *Reducing Inflation: Motivation and Strategy*, ed. C. Romer and D. Romer, National Bureau of Economic Research and University of Chicago Press, 1997. (NBER Working Paper No. 5539, 1996).