

# The baby boom, the baby bust, and the housing market

A reply to our critics

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## 1. Introduction

What drives booms and busts in the housing market? This is one of the central questions of housing economics, and it is the one we took up in our 1989 paper. In one part of that paper, we showed that in post-war U.S. time series, demographic trends are closely associated with trends in real housing prices. In times when the adult population was growing quickly, housing prices have tended to rise; in times when the adult population was growing slowly, housing prices have tended to fall. We noted that if this empirical link continues to hold in the future, the falling birth rate in 1970s will lead to falling real housing prices in the 1990s.

Much has happened since we wrote that paper. Most important, since 1987 (the end of our sample), real housing prices have fallen. The residential investment deflator divided by the GNP deflator – the measure we used previously – declined by 2.7 percent from 1987 to 1990. Over the same period, housing starts fell 24 percent.

Because of these developments, our paper generated far more interest and controversy than we had ever anticipated. We were especially surprised by the attention that we received from the media. Some of this interest arose because the weak housing market was hard to explain: the downturn in housing from 1987 to 1990 was out of proportion with the slowdown affecting the rest of the economy. Some of this interest arose because of the

inevitable confusions that arise when laymen try to interpret articles written for professional economists.<sup>1</sup>

One of the most persistent confusions is the interpretation of our (in Hendershott's word) 'infamous' forecast of a 47 percent decline in housing prices. In fact, a careful and honest reader of our paper will find that we never endorsed any particular point forecast of how much housing prices will fall as a result of the baby bust. The '47 percent' forecast came from extrapolating a particularly simple summary of the time series. We wrote,

Every good student of econometrics can recite the perils of forecasting beyond the experience of the data. The predicted growth of our housing demand variable is lower than has been experienced over the past forty years, and the period of low growth is protracted. Hence, we cannot be confident about precisely what effects this slow growth will have. Yet experience does tell us that slow growth in demand is associated with falling prices. Even if the fall in housing prices is only one-half what our equation predicts, it will likely be one of the major economic events of the next two decades.

In the following section of our paper, we turned to a model of housing-market dynamics with more desirable theoretical properties than the simple forecasting equation, and we considered the impact of a baby boom and baby bust. For plausible parameter values, that model generated fluctuations in housing prices no larger than 22 percent.

In the end, we concluded that demographics are a central driving force behind the housing market. This conclusion suggests that housing demand is likely to remain low during the 1990s. We were agnostic – as we remain today – regarding exactly how large a fall in housing prices will be caused by the fall in demand.

Responding to these four commentaries on our paper is difficult for two reasons. First, although the commentators all doubt our conclusions, they have little in common beyond that. Not only would they have taken a different approach than we took, they all would have taken a different approach from each other. Second, none of these commentators proposes an alternative hypothesis about what drives housing prices. There is little doubt that housing prices have fluctuated substantially over the past forty years. We believe that one can explain much of these fluctuations with changing demographic trends. Although the four commentaries take exception with our conclusion, none of them proposes an alternative explanation of these large changes in housing prices.

<sup>1</sup>The distinction between real and nominal is especially problematic. One reporter asked, 'When you say *real* prices are going to fall, do you mean that prices are *really* going to fall?'

Despite these two difficulties, we proceed to discuss the detailed points raised in each of the papers.

## 2. Hamilton

Hamilton begins by calling into question the inclusion of a time trend in our basic forecasting equation. He points out that a negative time trend in the equation implies that, if population were stationary, prices would fall indefinitely. He writes: 'Until we know where the negative 8.1% time trend comes from, and until we have some reason to believe that it will persist for the next twenty years, the MW prediction cannot be taken seriously'. Hendershott makes the same point, adding: 'This long-run property seems most implausible, and it alone would discourage most people from using the equation to forecast far into the future'.

The simplest defense of the equation that we use is to consider the opposite: what if we did not include a time trend? In such an equation, a positive coefficient on demand would imply that if the population grew at a constant rate indefinitely, housing prices would rise to infinity. This result would be just as subject to Hendershott and Hamilton's critique as the one we present. The way out of this quandary is to remember a lesson from basic econometrics: don't take the functional form too literally.

Our specification is meant to approximate the relationship between housing prices and demand growth in the range of demand growth observed in the data. Locally, we do much better fitting the data including a time trend than excluding it. As we warn in the paper, extending a model to make predictions outside the range of data in which it is estimated is always dangerous. There may be some non-linearity in the relationship between demand growth and price, or a more complex dynamic relationship, which we do not pick up in our specification. But we can still be confident that the reduction in demand growth will lead to a fall in prices, even if we cannot be confident of the magnitude of the change.

As we pointed out ourselves, the regression that we presented is not derived from a fully consistent model of housing-market dynamics. In the model we presented in section 6, zero demand growth does lead to a constant price – and, indeed, for any constant level of demand growth, the price eventually remains constant. If the correct model is the 'naive expectations' model, then a reduction in demand growth will lead to a fall in price over some period, after which the price will be constant. If such a model is correct, then the regression specification that we use will be approximately valid for medium-term forecasts.

Hamilton's second and third points are that the CPI for rent fell during the 1970s, and that over the same period housing consumption rose by more than can be explained by growth in our demand series. He argues that the

first of these facts explains the second: housing consumption rose in the 1970s because the price went down.

In pointing out that any model of the housing market has to take both price and rent into account, Hamilton is certainly correct. In our original work, however, we avoided using the CPI for rent because of the measurement problems with this series. The decline in measured rents over a period when all price indices rose dramatically is a mystery that needs to be explained.

Hamilton's method for calculating the change in age-specific housing quantity is interesting, but hard to interpret. He concludes that the quantity of housing consumed by a young adult rose by more than can be explained by income growth because he uses 15% as the real price increase for housing in the 1970s. Some housing price indices, however, rose between 20 and 30% over this period. If one applied a price rise of 25% to his calculation, one would conclude that the change in income could almost exactly explain the change in housing quantity. Telling the story this way does imply a small price elasticity, which some may find implausible. As an alternative, we are perfectly willing to believe that high prices in fact discouraged consumption, and that some other factors, such as the low after-tax real interest rates in the 1970s, are part of the story.

### 3. Hendershott

Hendershott's first point – the time trend in our basic specification – is discussed above. His second point is that our results are not stable when the sample is divided into two sub-periods: the estimated coefficient on the demand term seems to differ between the two periods, and forecasting changes in price over the second period using the estimated equation from the first period underpredicts the change in price.

We believe this criticism gets the problem backwards. Our sample is fairly short to begin with. Dividing it in half leaves one with very little data, especially for estimating a low-frequency relationship such as the impact of demographic change. We would have rather seen the extension of our work to more data – either to more countries, as in Engelhardt and Poterba, or to longer time periods.

Hendershott's last point is that we should take into account a measure of the user cost of housing (*RAT*) that includes price appreciation. This is exactly the approach we take in the intertemporal model of section 6 of our paper. But we do not implement it empirically for reasons that are well illustrated by Hendershott's attempt to do so. Hendershott models the real user cost as depending on current and lagged house price changes – in effect assuming that house buyers have extrapolative expectations about housing prices. In a proper intertemporal model, extrapolative expectations will lead

to even more pronounced price swings than in our models with 'naive' and rational expectations.

To forecast using Hendershott's approach, one must assume a value for *RAT*. But once one has made a forecast for house prices, one should be able to come up with a better estimate of *RAT*. It seems that, at a minimum, the proper approach to forecasting with Hendershott's model would be to look for a fixed-point path of prices – that is, a path of prices that produced a *RAT* that in turn implied the same prices. Such a path might not exist at all – and if it did, it could well include wild swings in price in response to small (demographic?) shocks.

#### 4. Holland

Holland is the only one of our critics to take up what we admitted was a problem in our original paper: our failure to find any relationship between our demographic demand variable and the quantity of housing. He finds that, once one accounts for non-stationarity, changes in demand do explain changes in residential investment. This finding is certainly good news, in that it shows that the demographically-derived variable that we created is indeed relevant to the quantity of housing.

Holland interprets his other finding – that the real price of housing is not cointegrated with the growth rate of demand – as evidence that there is no relation between housing prices and demand. We find this conclusion difficult to accept. The empirical work in our paper showed that there was a close relation between the growth rate of demand and the growth rate of prices. Holland is correct in pointing out that in the model we presented in section 6 of our paper, changes in the growth rate of housing demand affect the steady-state level (and not the steady-state growth rate) of housing prices. Unfortunately, it seems clear from inspecting the data that the rational-expectations model that we presented does not fit the data. We do not know *why* this model does not fit, but clearly it does not. We are therefore suspicious of Holland's argument that one should use a restriction implied by that model in forecasting future house prices.

Holland suggests that the relationship we report between housing prices and demographic variables may be spurious. That is, our *t*-statistic of 9 may be overstated because of a failure to take into account the subtle econometric issues that arise in the presence of unit roots. We believe that this is a valid question to raise, but we are skeptical of Holland's approach to addressing it. Holland takes the approach of changing our regression specification to an alternative that is less likely to have the potential for spurious regression. One cannot know, however, whether Holland's conclusion that the housing market is efficient is due to more accurate standard errors or to a less

powerful specification. A better approach would be to maintain our specification and attempt to compute standard errors that are valid in the presence of unit roots. Nothing in Holland's paper gives us reason to believe that the relationship between population and housing prices that we report is indeed spurious.

## **5. Engelhardt and Poterba**

Of all of the papers discussed here, only Engelhardt and Poterba's presents new data. This is exactly the kind of work that is necessary to push forward our understanding of the dynamics of housing markets.

Their first result is supportive of our work: in cross-section data from Canada, the relation between age and housing demand looks almost exactly the same as that which we found in the United States. Interestingly, although it is not necessary to make their work comparable to ours, the pattern of births in Canada, and thus the time series of demographically-driven housing demand, is also the same as that for the United States. But the two countries have strikingly different histories of housing prices, and although the growth of the demographic demand series closely matches growth in prices in U.S. data, the two series seem completely unrelated in Canadian data.

Although this last result is certainly a disappointment, there appear to be some important differences in the mechanisms generating housing prices in the two countries. The most dramatic price movement in the United States is during the 1970s, when prices rose between 20 and 30 percent, depending on the index used. In Canada, by contrast, there are three price movements of larger magnitude: between 1970 and 1975, prices rose approximately 50%; over the next ten years they fell back to the level from which they had started; and from 1985 to 1989 they rose approximately 25%. It is clear that slowly-changing demographic factors could not have produced such large and sudden swings. We note with satisfaction that the Canadian data do disprove one of the commonly heard arguments against our forecast: 'It's just impossible for prices to fall by that much'. Yet we would also like to see a more complete explanation of what was driving Canadian housing prices before concluding that demographics had no role.

## **6. Conclusion**

All four of these commentaries concentrate on the empirical relationship between real housing prices and demographic trends. None of them has given us reason to believe that the relationship we reported is, in fact, not

present in the data. We were disappointed that none of the commentaries took up the questions that our paper left open. In particular, why doesn't the price of housing anticipate predictable changes in demand? Are the supply and demand elasticities as low as is necessary to explain the large changes in price that we have observed historically? And, finally, what explains the booms and busts that occur regularly in local housing markets? These questions should prove to be fruitful topics for future research.