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Comment

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Introduction

The housing boom and bust that occurred between 1996 and 2009 is an empirical event that shattered older theories of the housing market and left a gaping need for a richer model of housing dynamics. Simple theories that link housing prices with interest rates or other credit market variables cannot explain the magnitude of the boom or bust with standard assumptions about rationality (Glaeser, Gottlieb, and Gyourko 2010). If we assume enough irrationality of the right kind, then the most extreme cycle will not reject our theory, but such a freewheeling model does not really generate any testable hypotheses either. The goal is to find that solid, middle place with enough freedom to actually explain what happened, but enough clearly refutable predictions so that the model can be properly tested.

"House Price Booms and the Current Account," by Klaus Adam, Pei Kuang, and Albert Marcet, attempts to find that high ground. Their open economy model enables the interest rate to be formed endogenously, which is quite preferable to most housing models, but their largest contribution is their modeling of beliefs. They assume a belief structure that is "near rational," still tethered to conventional assumptions about rationality but with enough flexibility to generate more extreme price swings. While I am unsure as to whether their precise formulation of near-rationality will become standard, this basic approach seems correct—a reasonable balance of psychological realism and traditional modeling parsimony.

The model is able to generate the large price swings that we have recently experienced. It can also generate the one-year serial autocorrelation in price movements that is such a strong feature of the data

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(Case and Shiller 1989). To achieve this with a relatively simple model is a significant step forward, but to make further progress, their model needs to be subjected to a tougher range of empirical tests.

The Empirical Problems

While explaining the magnitude of price convulsions is not the only empirical challenge facing housing theorists, it is certainly the most extreme. Between 2001 and 2005, the Case–Shiller Index (20-city repeat sales increase) experienced a 46% increase in real terms. Between 2006 and 2009, the index fell by over one-third. The more nationally representative Federal Housing Finance Agency (FHFA) repeat sales index showed a less extreme boom-bust cycle, but even that index experienced a 53% real price gain between 1996 and 2006 and a substantial decline since then.

The recent housing convulsion is an extreme event, but it is symptomatic of a more general tendency of housing prices to exhibit high levels of variance. Glaeser and Gyourko (2006) write down a simple dynamic housing model and find that the variance of price movements over time and space is about far too high in many markets to be explained by "fundamentals" such as local income changes. This high level of variance is frequently identified with housing cycles, and America and the world have certainly experienced many earlier booms and busts. New York City during the 1920s was such an example, and the Japanese boom-bust cycle over 20 years ago is another.

Beyond high volatility of prices, a second empirical puzzle is the strong positive serial correlation of price changes at shorter time horizons, such as one year (Case and Shiller 1989). Predictability of housing price movements is not itself a challenge to conventional theory, since houses deliver far more than just financial returns, but the extent of this predictability is hard to explain. Glaeser and Gyourko (2006) report a one-year coefficient of price changes on lagged price changes of .71. This extremely high figure is hard to match with any obvious "fundamental" cause.

But while housing prices are positively serially correlated in the short run, they mean revert significantly in the longer run (Cutler, Poterba, and Summers 1991). Glaeser and Gyourko (2006) estimate a five-year coefficient of price changes on lagged price changes of –.32. This coefficient is somewhat easier to reconcile with benchmark models, if building new housing takes time and if fundamentals mean revert over time, as they appear to do.

While construction also shows significant volatility—positive serial correlation of prices at one-year horizons and mean reversion at five-year horizons—these facts are probably best seen as a natural consequence of the price movements rather than as independent puzzles. As long as prices are quite volatile, we should expect construction to also change substantially over time. Given standard housing supply elasticities (which are often estimated to be above one), the construction boom of 2003–2006 was, if anything, somewhat smaller than the price movements would suggest, at least for the nation as a whole, which may be explained by heterogeneity in housing supply elasticities across space.

The final empirical challenge may be hardest—explaining why some areas experienced housing price explosions, while others did not. Glaeser, Gyourko, and Saiz (2008) present evidence that price fluctuations were less extreme in areas with more elastic housing supply. Economic theory certainly predicts that any demand shock—whether caused by rational or irrational forces—should impact price less and quantity more in places where supply is more elastic.

But while supply elasticity appears to impact how bubbles play themselves out, there is plenty of variation that is not explained by supply elasticity. Both Phoenix and Dallas appear to have fairly elastic supplies of housing. Dallas experienced little price appreciation during the boom—it built a vast number of new homes. Phoenix managed to see both huge increases in construction and huge increases in prices. Indeed, a lasting puzzle from the boom is why home buyers in areas like Phoenix and Las Vegas (where land is relatively abundant, permitting is relatively easy, and prices have historically stayed extremely close to construction costs) were willing, for a very short time, to pay so much for housing.

The cross-national variation is also quite interesting. Many other nations experienced price booms similar to the United States, but others did not. Germany and Japan, for example, had extremely stable housing prices. Some speculate that the memory of their earlier boom-bust cycle dampened excessive optimism, but we really have little ability to explain the appearance of housing price convulsions across time and space.

The Adam, Kuang, and Marcet Framework

Adam, Kuang, and Marcet begin their paper by empirically documenting the negative time series relationship between the current account surplus and housing prices over the 1996–2006 period. This evidence motivates paying close attention to credit markets in their theoretical work, and surely these played some significant role in facilitating the housing price convulsion.

They also show that real interest rates were relatively low during the 2000–2005 period when prices were going up. Their choice of measure for the real rate, the one-year adjustable mortgage rate adjusted by inflation expectations provided by expert survey respondents, presents a more volatile series than more standard measures, such as the ten-year rate adjusted for similarly survey-based inflation expectations. Their choice is not necessarily wrong, but it does present more rate volatility.

More interest rate volatility makes it potentially easier for credit market variables to explain the housing market price shifts, but only if the rate shifts line up with the price shifts. The low rates of the 2000–2005 period seems to go well with the rising prices during that period, but prices were also rising between 1996 and 2000 when their measure of real rates were high. Over a longer time horizon that includes the 1980s, the correlation between real rates and housing is quite modest. Glaeser, Gyourko, and Gottlieb (2010) find that a 100 basis point swing in real rates is typically associated with a 7% swing in housing prices, and that coefficient falls when we include even the most basic of controls, such as a time trend.

But any analysis of the connection between interest rates and housing prices is compromised by the endogeneity of real rates. This endogeneity is typically ignored by housing economists, while Adam, Kuang, and Marcet quite thoughtfully connect interest rates to broader movements in the current account. This is potentially important because rates may certainly reflect demand for housing credit, as well as the supply of credit. One reason why the measured relationship between interest rates and housing prices is relatively modest is that high housing demand may push interest rates up.

This reverse causality is particularly likely when using measured mortgage rates, which may reflect assessment of default risks, instead of ten-year treasury bond rates, which is more standard in the literature. If assessed default risk is low during boom periods, this could create a spurious negative correlation between prices and interest rates. The Adam, Kuang, and Marcet model, however, does treat the real interest rate as an exogenous variable, determined by an international lender. Perhaps, given the more than 15 trillion dollar size of the American mortgage market, it would be sensible for future work to allow some feedback where housing borrowing impacts the real rate.

Their core model assumes a representative consumer that receives shocks to the demand for housing. This representative consumer approach is standard in macroeconomics, but is somewhat less common in urban economics, where housing prices typically reflect the value associated with one place versus another. Consumers may be identical, but housing is not. If Adam, Kuang, and Marcet were more interested in explaining the heterogeneous experiences of the boom, then they should probably move to a dynamic Rosen-Roback spatial equilibrium model, perhaps along the lines of Glaeser and Gyourko (2006).

Their homogeneous place, homogeneous consumer model does enable them to incorporate important aspects of the housing, such as collateral constraints. The borrowing constraint is itself realistic and important, although one can question their decision to allow future prices to influence the constraint. Their assumption means that higher rates of expected price growth make it easier to borrow. I suspect that this is actually true—banks discounted default risk during the boom because they expected plenty of price appreciation—but the assumption is somewhat nonstandard in the housing literature. It would be more normal to assume that current prices alone influence the constraint. At this point, I think their assumption is best seen as shorthand for a more complex model of the banking structure that could be fleshed out in further work.

Adam, Kuang, and Marcet deserve considerable credit for incorporating housing supply into the model. Often supply is treated as exogenous, or even more problematically, rents are treated as exogenous and prices are derived as a function of the expected rent series. Yet the ability to deliver new homes is certainly important in mediating price changes, which is exactly what Glaeser, Gyourko, and Saiz (2008) find.

Their specific assumptions about housing supply are well in line with the literature. Their iso-elastic functional form assumption, for example, is not unusual. Perhaps the most important departure from realism is the assumption of a common national supply elasticity. Actual supply elasticities differ significantly from place to place. Building in greater Houston is just very different from building in greater San Francisco and that would matter as they attempt to think more seriously about differences within the United States.

Using this model, they calculate a deterministic steady state and then use linear, and linear quadratic, approximations to calculate the impact of shocks in a stochastic environment. Their appendix makes it clear that the somewhat more general linear quadratic assumption does not change the primary implications of their simulation.

The key result of the simulation is the interest rate changes that we see in the data are too small to generate large increases in housing prices when expectations are rational. If buyers do not anticipate that low rates will become high again, then the observed interest rate change predicts a price jump of approximately 4%. If buyers anticipate the real rate increase experienced in 2006, then the price jump is only 2%.

Moreover, the timing of the price jump is quite unlike the price growth that America actually experienced. Their model predicts that prices will immediately jump and then either stay at the higher level, if no real rate increase is anticipated, or gradually fall, if a rate increase is expected. In reality, prices rose steadily over this period.

Their conclusion is exactly the same as reached by Glaeser, Gyourko, and Gottlieb (2010) using a very different methodology: the observed change in real rates is far too small to explain the observed price boom. Their calculation is that the predicted price increase is about 10% of the actual price growth. We have a slightly higher number, but one that is still very far below the actual price growth. Their work supports the view that if credit markets matter, they must matter because of some interaction between credit availability and less than perfectly rational consumers.

Near Rationality and Price Movements

The primary contribution of this paper lies in its modeling of near rationality. The authors wisely attempt to balance more psychological realism and the modeling degrees of freedom that come with it against the advantages of staying near rationality. Limited departures from rationality are attractive both because they keep closer ties with the standard economics and because they impose more discipline on new models.

Adam, Kuang, and Mercet specifically assume that individuals forecast price growth by assuming that the log of price growth is the sum of a transitory component and a permanent component. In each period, there are two shocks to price growth—one becomes permanent and the other immediately fades away. Home buyers then attempt to forecast the model with the available data. Specifically, they assume that the expected value of price growth between periods t and $t + 1$ is equal to a weighted average of their expectation of growth between periods $t - 1$ and t and the actual price growth between $t - 2$ and $t - 1$. They use the one period lag of price growth to avoid simultaneity.

Adam, Kuang, and Marcet work to build the case that this is not profound irrationality, but something quite near actual rationality. If information is sufficiently limited, this is not a completely insane learning rule. Beliefs can converge to the rational expectations beliefs if the variance of the shocks to the growth rate converge to zero.

While there are ways in which this model resembles a standard learning model, no one should think that this is not a fairly large leap from standard rationality. Assuming that beliefs about price growth evolve so that future price growth is a weighted sum of lagged beliefs and lagged growth is very different than what I would typically think is using all of the available data to forecast a price. They micro-found their learning model by assuming that agents believe that growth rates follow a particular stochastic process, which is a permanent and transitory component that is not particularly linked to the forces that actually drive price changes. Individual readers can decide whether they share the authors' belief that this is "near rationality."

Their assumptions produce results that resemble the old adaptive expectations models that were discarded decades ago, but perhaps those models were discarded too rapidly. There is something very appealing about complete rationality, especially from a modeling perspective. There is only one way to get things exactly right and an uncountable number of ways to get things wrong, which makes assuming any particular error prone prediction assumption seem somewhat arbitrary. Yet adaptive expectations seem as reasonable as any other erroneous way of thinking, and survey evidence on home buyers suggests that there is some truth to the assumption. The key test is whether this model does well at explaining the data.

The first, particularly attractive, feature of the model comes out of the theory even before simulations—price changes are positively serially correlated. Price growth during one period creates the expectation of further price growth and that pushes price up further during the next period. Around the rational expectations equilibrium, the implied coefficient when the logarithm of price changes is regressed on the lagged logarithm of price changes equals their g parameter, which is the ratio of variance of the expected log of the growth rate in prices divided by the variance of the shocks to the log growth rate times the parameter ρ, which if housing depreciates very slowly, is a weighted average of the market discount factor and the private discount factor, as such is some number less than but close to one. As we do not currently know much about the variance of these beliefs about price growth rates, theory does not predict much about how big the serial correlation in price growth is going to be, but it certainly can be quite large. Adaptive expectations deliver that result.

Their simulations then show that a positive shock to real interest rates pushes prices up immediately and then that has feedback effects on future price growth. One period of fundamentals drives growth in the next period, which then fuels further growth. Eventually, growth comes in below expectations and the bubble starts unwinding.

They then use this model to make sense of the international experience. In the United States, the interest rate shock produces a boom-bust cycle that is in line with what America actually experienced. They obtain similar results for France, Italy, and the United Kingdom. Some countries have different predicted price experiences because of different levels of pre-2000 price changes. Those places, like Germany and Japan, that had less price growth before 2000 are predicted to have less of a boom and that is what the data says. They predict too little of a boom for Canada.

The next place to take the model is to more rigorous testing. The general implication that there will be positives serial correlation will surely be borne out in the data. But the model does seem to predict that past interest rate shocks should also have large effects, at least if the pattern of preshock price movements is right. I am more skeptical that this will be borne out in the data.

More generally, it would be good to have a version of the model that is more focused on subnational data, since there is so much more ability to run tests at a subnational level. Elasticity differences are significant, but so will be the track record of preshock price movements. These will provide added opportunities to test their model, or some similar adaptive expectations-based model.

One way to think about their model is that the adaptive expectations framework creates a multiplier effect, where exogenous shocks create a far larger price increase than would be predicted by a more standard model. This logic should work for other shocks beyond real rates. As such, a natural test of the model would be to look at price responses to other local demand shocks, such as oil price movements for Texas and so forth.

Conclusion

This paper is an important contribution to the growing literature about housing price fluctuations. They make two important contributions.

First, they show that the changes in real rates cannot explain the magnitude of the boom given standard assumptions about rationality, which supports Glaeser, Gottlieb, and Gyourko's (2010) quite similar claim. Second, they show that one model of near rationality can generate price dynamics that look far more realistic.

The first result helps eliminate past theories of housing price change that assume extreme levels of buyer knowledge. The second result points toward the future, where hopefully richer models that respect the limits of human cognition will give us a better ability to understand and predict large housing price movements. I like their model and think that it is an important contribution to the literature. Only future work can tell us whether it is the right framework for incorporating near rationality into housing and other asset models.

Endnote

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