6-2011

Using Monte Carlo Simulations to Establish a New House Price Stress Test

James R. Follain
Rockefeller Institute of Government, Albany NY, follainj@rockinst.org

Seth H. Giertz
University of Nebraska-Lincoln, sgiertz2@unl.edu

Published in *Journal of Housing Economics* 20 (2011), pp. 101–119; doi:10.1016/j.jhe.2011.04.003

Submitted June 16, 2010; published online May 4, 2011

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James R. Follain

The Rockefeller Institute of Government, 411 State Street,
Albany, NY 12203; follainj@rockinst.org

Seth H. Giertz

Department of Economics, CBA 368, P.O. Box 880489, University of Nebraska–Lincoln,
Lincoln, NE 68588-0404; sgiertz2@unl.edu (Corresponding author)

Abstract

The focus of this paper is on the house price stress test (termed ALMO) that was designed to assess the fiscal strength of Fannie Mae and Freddie Mac and, if necessary, to trigger remedial action in order to avert a crisis. We assess whether the ALMO stress test was an adequate representation of an extremely weak housing market, given the best available information leading up to the Great Recession. A Monte Carlo simulation model is developed to estimate the severity of low probability events (i.e., severe house price declines). We illustrate the complexity and subjective nature of the process used to generate a plausible house price stress test scenarios. A major finding is that the ALMO stress test scenario severely understated (possibly by 50% or more) what an updated statistical process would have suggested. Part of this stems from idiosyncrasies related to the construction and implementation of ALMO, while other factors include a fundamental shift in the relationship between housing price appreciation and key explanatory variables—especially over the past 10–15 years, which shows a heightened role of momentum in explaining changes in housing prices. We offer several suggestions for a new stress test that include: continual updates and testing; variation across markets; and, like the recent FRB stress test, the scenario should be based on real (rather than nominal) price patterns.

Keywords: housing, financial crisis, stress test

1. Introduction

We begin this paper with an analogy to the issues to be addressed. Consider the catastrophic hurricane of 2005—Katrina. The city of New Orleans did have protection against the ravages of flooding associated with hurricanes. Like all insurance plans, it was based upon a sense of the distribution of the severity of hurricanes and the cost curve relating levee cost to levels of protection. The system in place was inadequate to protect New Orleans so a series of studies were conducted to address two central questions. First, should the probability distribution of flood severity be redefined and new levees built based on the new information, holding constant the degree of protection thought to be provided by the previous system? For example, perhaps the severity of a 100 year flood was previously underestimated and current data provide what is believed to be a better estimate. If so and if protection from a 100 year flood is still the desire of the people of Louisiana, then new levees to protect against such an event should be built stronger and higher. Second, if the previous definitions of these stressful events were on target, should the degree of protection provided by the levees be increased to withstand something more severe than a 100-year flood?[1] In addressing these questions, one study suggests that the previous definitions were accurate, but that a new and higher degree of protection ought to be provided. "A 100-year level of levee protection from hurricane storm surge is inadequate for a major city like New Orleans, and officials should consider relocating residents out of the most vulnerable areas" (National Research Council, 2009).

1. A third possibility is that both the desired level of protection and the severity of a 100 year flood did not change, but the levees were flawed and did not provide the level of protection advertised.

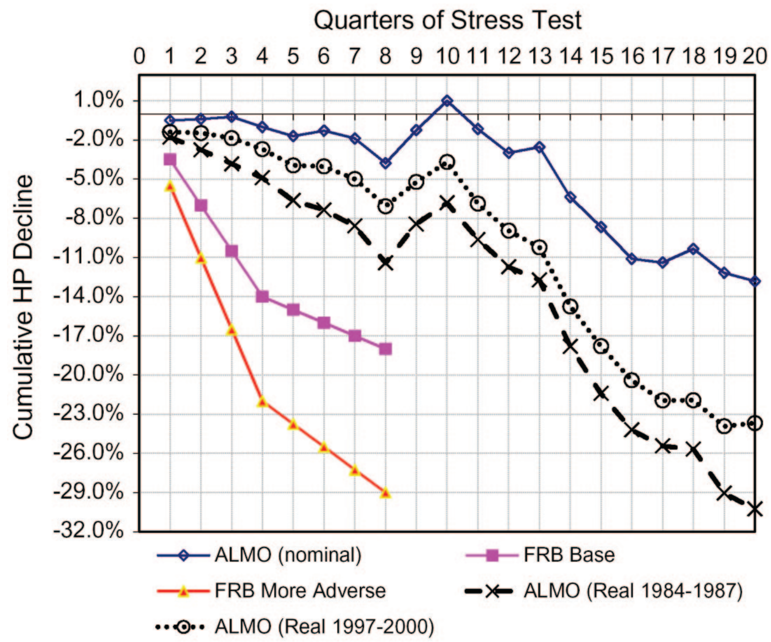

Figure 1. Cumulative HP Decline under ALMO and FRB stress tests. *Source*: Authors' calculations. ALMO nominal is the nominal house price decline used by FHFA (formerly OFHEO). ALMO (Real 1984–1987) expresses the ALMO scenario in real terms based on the actual rate of inflation during the ALMO experience. ALMO (Real 1997–2000) expresses the ALMO scenario in real terms during a period with lower inflation.

1.1. Economic capital as a buffer

This paper undertakes a similar exercise, focusing not on hurricanes and levees, but rather on the ongoing depression in many housing markets and economic capital, which US Banks and other financial institutions such as Freddie Mac and Fannie Mae were required to hold to protect against such severe economic events. As with New Orleans and Katrina, the protection in place for much of the US financial sector failed, so the same type of questions can be asked. First, did the policies and risk management systems in place prior to the Great Recession understate the likelihood and extent of a serious decline in house prices?[2] This is akin to Katrina being caused by an underestimation of the frequency of a 100 year flood. Second, and aside from the first question, should the desired level of protection built into risk management systems and sought by government regulators be amended to withstand events of greater severity? This is akin to saying that policymakers had a good grasp of the frequency of a 100 year flood, but the latest flood was, say, a 200 year event. In the first case, risk management systems should be recalibrated, based upon a more accurate and up-to-date view of a severe house price shock. In the second case, analysts and regulators should reexamine evidence to determine whether the benefits from increasing the level of protection exceed the costs.

1.2. Motivation

The motivation for this paper is the collapse of US mortgage giants Fannie Mae and Freddie Mac and, in particular, the house price stress test scenario applied to these two government-sponsored enterprises (GSEs). The test was designed to assess the economic strength of these institutions and, if necessary, to trigger remedial action in order to avert a crisis. The stress scenario is based upon congressional legislation that outlines the degree of protection congress expected the GSEs to have in place—a scenario sometimes referred to as the ALMO stress test.[3] Credit losses for the GSEs' portfolios are projected using this scenario and a host of other assumptions, including the probabilities of default on particular pools of mortgages and the severity of losses in a stressful environment. These two home-mortgage securitizers were required to hold enough capital to withstand the ALMO stress test.[4] The implementation of the stress test and the monitoring of the institutions' adherence to its implications falls under the purview of the Federal Housing Finance Agency (FHFA).[5]

2. In addition to understating the likelihood of a sharp drop in housing prices, the economic implications of such a drop may too have been underestimated. For example, the interconnectedness of the mortgage industry throughout the financial sector may not have been fully appreciated.
3. ALMO stands for states Arkansas, Louisiana, Mississippi and Oklahoma. The ALMO stress test is based on the experiences of these states during the 1980s.
4. A stress test scenario for housing refers to a clearly-defined and sustained path of substantial declines in house prices. This low probability scenario is then used to predict the implications of such an event for financial institutions and for the broader economy.
5. FHFA was created by legislation signed in 2008 that merged the Federal Housing Finance Board (FHFB) and the Office of Federal Housing Enterprise Oversight (OFHEO). OFHEO officially went out of existence July 30, 2009—one year after the legislation was signed. Because most of the period of our study pre-dates the creation of FHFB, we generally refer to OFHEO as the regulatory body governing the GSEs and attribute data and models now housed by FHFA to OFHEO.

Before proceeding, consider four important characteristics of OFHEO's ALMO house price stress scenario:

1. Most importantly, the house price pattern was constructed to simulate a weak housing market. The ALMO stress scenario is a ten year path of house prices in which prices are relatively flat for the first two years and then decline by about 13% over the next three years. Beyond year five, house prices rise such that the net change after 10 years is about zero (see Figure 1).

2. The ALMO stress test is measured in nominal terms; there is no adjustment for variation in inflation across time-periods. Because ALMO was measured in nominal terms, it implies a more (less) severe stress scenario for periods where inflation is higher (lower) than during the 1984–1993 ALMO period. During the run-up in US housing prices from 1997 to 2006, low inflation resulted in an ALMO stress scenario that was 20% weaker in real terms than the actual ALMO experience (from 1984 to 1993).[6] The dotted line in Figure 1 shows the actual ALMO scenario converted to real terms and beginning in 1997. By contrast, the dashed line shows what the ALMO path would have been, had it been defined in real terms (and thus adjusted for the inflation rate beginning in 1984). The difference between the two lines is driven by the difference in the inflation rate between the two periods. Because ALMO was specified in nominal terms, it substantially lessens the severity of the stress test for this low-inflation period. However, had ALMO been defined in real terms from the beginning, it would (at least for the first eight quarters) still represent a much less severe scenario than the 2009 FRB base scenario (FRB, 2009). Differences in inflation across the two periods accounts for about 40% of the difference between the ALMO and FRB base scenarios.[7]

3. The scenario was not updated or amended as new information became available and economic circumstances changed; that is, the same specific scenario was applied each year until the GSEs went into conservatorship in September 2008.

4. The scenario is applied to all mortgages without consideration for the geographic location of the property underlying the mortgage. For example, the same scenario was applied to loans secured by houses in Los Angeles and those in rural North Dakota. Likewise, the scenario did not change as lending standards became more lax, higher-risk borrowers entered the market en masse, and the complexity and breadth of mortgage securitization increased.

1.3. Goals and approach

The primary goal of this paper is to examine whether the ALMO stress test was an adequate representation of an extremely weak housing market using the best available information up until the Great Recession. We pursue this goal by estimating a variety of regression specifications for real house price growth and then develop a Monte Carlo simulation model to estimate low probability events. Extreme scenarios are defined as those paths over a threeyear period that represents the worst 1% and 5% outcomes. The specifications are estimated using quarterly data on house prices from OFHEO for all fifty states and for years 1975–2009. We pay particular attention to variations in the definitions of stressful scenarios based upon the time-periods and groups of states included in the estimation. We also conduct a variety of sensitivity analyses to learn more about the nature and robustness of a more up-to-date stress test scenario.

A second goal of this paper is to illustrate the complexity and subjective nature of the process used to generate a plausible house price stress test scenario.[8] To that end, this paper highlights the numerous assumptions, sensitivity analyses, and other judgments necessary to define an extreme house price event. This issue is raised because of the emphasis on transparency in many ongoing discussions about regulatory reform. For example, the importance of transparency was raised by the congressional oversight panel in its review and critique of the Federal Reserve Board (FRB) stress test, which we discuss in the next section. While the report praised the FRB efforts, it also said that "additional transparency would be helpful both to assess the strength of the banks and to restore confidence in the banking system." While we agree that more information about the details of the stress test would be helpful,

6. From 1997 to 2006, the CPI increased by roughly 25%, compared to 40% for years 1984–1993.

7. We are not positing that real price changes are the main driver of mortgage default. Indeed, nominal price appreciation is the critical driver; however, we do advocate basing the specific nominal stress test (applied in any given environment) on a severe real housing price scenario. The real scenario should be adjusted to account for expected inflation in the new environment, thus keeping the real value of the stress test constant across inflationary environments. This was not the case with the ALMO stress test. It held the nominal stress test constant while allowing the real value to vary among inflationary environments. While the real impact of ALMO's severity is understated for periods with low inflation rates, nominal price changes are important to monitor. Nominal housing price changes are likely more important than real price changes in determining default. Absent nominal price changes, inflation lowers house prices by the rate of inflation, however, inflation also lowers outstanding mortgage debt by the exact same percentage (in real terms). In the presence of deflation (along with nominal drops in house prices), it is possible for mortgages to become "under water" even with real house price appreciation, since, again in real terms, mortgage debt is increasing more rapidly than house prices. Of course, in modeling default, it also important to examine the relationship between inflation and mortgage interest rates, since many times refinancing may be a better alternative than default.

8. Because of the recent crisis, even the notion of a stress test has fundamentally changed. Until recently, the most serious risk faced by financial institutions invested in the mortgage market was believed to result from rising interest rates—which could in turn result in falling house values. The notion of plummeting house prices along with stable (or low) interest rates was often not on the radar screen. For example, see Stiglitz et al. (2002), who, in examining the risks that Fannie Mae and Freddie Mac posed to the public, conclude that the probability of default by the GSEs is extremely small. In fact, they report that the GSEs could withstand a stress scenario that they estimate that has a less than one if 500,000 likelihood of occurring. Stiglitz et al. (2002) also note that the office of management and budget (OMB) had looked at the GSEs' ability to withstand a ten-year great depression-like scenario. In describing OMB's analysis, they report that, assuming 1990s levels of capital, "the probability of either Fannie Mae or Freddie Mac defaulting would be 'close to zero."

our conclusion is that transparency will not be sufficient to bring about widespread agreement about the appropriate stress test because, at its core, predicting extreme events is very difficult and evidence to define these events will not be equally compelling to all.

1.4. Findings

This paper finds that OFHEO's ALMO stress test scenario, in place at the beginning of the Great Recession severely understated what a more robust and updated statistical process would have suggested. As shown in subsequent sections, such a process would have implied a scenario with more than double the price declines of ALMO. Part of this stems from a fundamental shift in the relationship between housing price appreciation and key explanatory variables—especially over the past 10–15 years, which shows a heightened role of momentum in explaining changes in housing prices. A variation of this paper's preferred scenario incorporates the potential for a substantial drop in employment growth, which in many cases results in an additional 20–30 percentage point drop in housing prices (over a three year period).

Several broad policy implications stem from our analysis. First and foremost, a regulatory stress test should be subject to ongoing scrutiny. A legislatively mandated stress test scenario without a process to provide scrutiny and updating seems inadequate to capture the dynamics of the housing and mortgage markets. Second, regulators cannot be expected to produce evidence of an extreme event that is both compelling to everyone and transparent. At some point, their independent and informed judgments must play a critical role. Third, a stress scenario should be constructed in real terms; otherwise, the effective severity of the stress test is driven by inflation (or expected inflation). Fourth, housing markets are influenced by both local and national (and international) factors. Heterogeneity across local housing markets should be acknowledged and taken into account when constructing and implementing stress tests.

1.5. Caveats

It is important to keep in mind several important caveats when drawing conclusions and policy implications from this work. First and most importantly, this paper eschews a full-scale autopsy of the reasons for the recent collapse in house prices and the unprecedented spike in mortgage foreclosures. The causes are numerous, the debate and evaluation ongoing and it is still too early to estimate anything remotely close to a fully specified structural model that captures the myriad of factors and players underlying the crisis. Additionally, with so many factors at play, at the margin, any one of them could have pushed the financial sector over the edge. Nonetheless, and as demonstrated in this paper, the estimated reduced form econometric models do highlight patterns and changes occurring in recent years, providing insights into improving the specification of a stress test, our primary goal. In particular, some of the patterns confirm and even enhance long-standing views among many analysts about the heterogeneity of housing markets and, especially the widely disparate impacts of interest rate shocks and sharp distinctions in the house price process among high price variance versus other states. Another striking pattern is a sharp increase in the relative importance of lagged house price growth versus lagged employment growth, which seems consistent with the views of Robert Shiller and others who emphasize the role of overblown and unsustainable expectations about house price appreciation in the early 2000s. More generally, plausible versions of a basic reduced form equation seem to have changed in important ways, which leads us to make the case for new and more severe stress tests for the mortgage and home finance markets.

Second, the focus here is upon the stress test scenario fed into the larger model used by OFHEO to compute regulatory risk-based capital for the GSEs. No attention is given to the many other features of the process, which will undoubtedly be found to have their own systematic shortcomings upon careful scrutiny and updating. In particular, the size and implications of a severe stress scenario may be very different in a different institutional environment. Obviously, getting the institutions right is a tremendously important and difficult task—and again, one that is not addressed here.

Third, a technical note, our focus is on a stress scenario for a representative state (or a composite state). Another alternative is to pick one based upon a representative sample of states. This becomes important if geographic diversification is thought to be critical. Indeed, earlier work on Basel II (see Calem and Follain (2003)) found that a nationally diversified portfolio would be expected to have 40% of the capital associated with a regionally concentrated portfolio. That conclusion may also need to be reexamined in light of more recent history. Rather than pursue the issue in this paper, which is a major and complex effort in itself, we choose to state it as a caveat that warrants additional research. However, such considerations would only alter the specific sizes of the stress test scenario, but not the relative rankings or the broader qualitative issues that we raise and identify.

Fourth, we are silent about the role of the GSEs themselves. We have in mind their ongoing responsibility to monitor what they (and their shareholders, which for the time being is primarily the federal government itself) consider to be prudent amounts of economic capital above and beyond what is required by regulators.[9] In fact, one of us (Follain) was part of a group at Freddie Mac from 2000 to 2002 with the mission of looking into this issue. A particularly interesting and intriguing question is what the internal monitors of credit risk capital for mortgages were finding and saying to senior management and the Board of Directors in the months and years leading up to the collapse. It appears that they also underestimated events such as Great Recession. It would seem appropriate to release more information on their thinking.

9. However, given that these institutions are already insolvent (save for government rescue), it is unlikely that they have the means to increase their economic capital, absent additional assistance.

2. Recent literature and background

This paper is part of a large and burgeoning literature addressing the current mortgage crisis, its causes, and potential remedies. Two aspects of this vast literature are of intense focus here. The first is the research on bank capital and the GSEs. This portion of the literature places the development and use of stress tests in the context of the broader issue of setting regulatory capital guidelines for large financial institutions. The second (area of focus) is the large body of work on the drivers of house prices. This has been a research focus, especially in the field of real estate and urban economics, for many years. A recent surge is underway in this literature to explore the reasons for the collapse in house prices and the accuracy of the previous structural models of house prices to explain the collapse. The goal of this survey section is not to provide a comprehensive overview of this literature, but rather to motivate and contextualize our decision to focus upon a relatively simple model of house price growth.

2.1. The role of a stress test scenario for economic capital

The function of regulatory capital is to provide financial institutions with sufficient (liquid) assets to withstand a serious deterioration or elimination of its net worth. The capital is usually expressed as a percent of the institution's assets. This is how leverage requirement rules are defined. Risk-based capital rules, like those proposed for Basel II, allow the final percentage to vary with the riskiness of the bank's portfolio.[10] Various methods have been proposed to help institutions and regulators determine and maintain optimal economic capital to assets ratio. Some, like Basel II, are rulesbased, but are also risk-adjusted. Calem and Follain (2003) present an example that illustrates how the proposed Basel II rules would be applied for mortgages. The paper's focus rests upon one formula embedded in the Basel II framework. A parameter in that formula (central to the process) is the correlation between mortgage default and a single state variable intended to capture the major economic forces that drive this process. Calem and Follain (2003) examine a variety of benchmarks in estimating the assetcorrelation parameter for mortgages that offer the protection proposed in Basel II—which would require portfolios to be no riskier than BBB+ bonds (which are sometimes termed "medium safe"). Given the rule and Basel II's assumed asset-correlation parameter of 15%, banks would compute their required regulatory capital by inserting into formulas other information about their mortgage portfolio—e.g., estimates of the current probability of default (pd) and loss given default (LGD).

In contrast to Basel II, Congress created OFHEO (in 1992) and charged them to carry out a different approach for estimating risk-based capital requirements for mortgages backed by the two GSEs. This approach is built around a large and complex model used to measure the credit losses of the portfolios held by the GSEs. Specific characteristics of the mortgages are entered into a set of equations that outputs estimated default probabilities for various categories of mortgages. The model also estimates losses given default in a stress scenario, the potential value of existing credit enhancements, and many other factors. A critical part of this system is the OFHEO (also referred to as ALMO) stress test, which is a projected path of national house prices during a ten year stress period. The GSEs were required to hold enough capital to withstand this stress test scenario. The specifics of this stress scenario were presented in the introduction.

The FRB employs another alternative in conducting its 2009 stress test. Like the OFHEO stress test, the FRB stress test specified the severity of house price declines. Unlike the OFHEO stress test, the process the FRB followed not set by legislation. While many of the details have not been made public, the FRB test appears to have been based upon expert opinions, recent historical trends and internal judgments. The FRB stress test was then applied using internal and proprietary models of the financial institutions affected—which were also not made public. While many of the details remain secret, it seems clear that the FRB felt that the OFHEO stress scenario was not severe enough (again see the Figure 1).

A recent paper by Löffler (2009) is very much in the spirit of our work. In addressing a similar question, Löffler employs an AR(1) model, using national house price data, to test whether the actual decline in house prices is consistent with his simulation results. He answers affirmatively; that is, his AR(1) model applied to the OFHEO data series for the entire US from 1975 to 2005 could produce extreme outcomes that include what has actually happened. However, applying the same approach to Case-Shiller data for 20 large cities yields a different conclusion. We will offer similar insights—the answer depends upon which data and which model one uses to generate distributions for future changes to house prices.

2.2. Reduced form versus structural model of house prices

The literature exploring house price movements is immense and growing rapidly.[11] It includes traditional multiple equation structural models with a wide variety of exogenous variables, as well as time-series and VAR approaches. (See Malpezzi (1999) for a good example of the latter approach.) These models are estimated with national, state and metropolitan-level data. Through the course of this research project, we have explored a wide array of models; however, the analysis in the following sections is based upon the estimation of a single-equation time-series model with lagged values of several economic variables as well grouped-state dummies and year fixed effects. Though we recognize that such a model is unable (and does not even attempt) to sort out the numerous and evolving factors that have contributed to the massive declines in house prices, we do feel it is an appropriate approach for our purposes for several reasons.

10. For the a recent statement from the FRB regarding the status of Basel II, see http://edocket.access.gpo.gov/2008/pdf/E8-17555.pdf
11. For example, note the recent spate of articles published in the *Journal of Housing Economics*: http://www.citeulike.org/journal/els-10511377

One particular variable, the user cost of capital, is omitted, even though it is quite common in house price model; thus, an explanation for omitting it is in order. The user cost of capital can be expressed such that $uc = (i - \pi)P$, where i is an interest rate, π is the expected rate of future price appreciation of housing, and P is the price of housing stock. The role of user cost is firmly ingrained in standard theories of investment and housing economists have written scores of articles investigating its role in a wide variety of situations.[12] We omit user cost because it rests upon an important assumption that, in our view, cannot be adequately defended given the experiences of the 2000s. That assumption is that we can estimate with confidence the expected rate of housing price appreciation. However, it appears that expectations about future of house prices have diverted in fundamental ways from views previously held by mainstream housing economists. The simplest explanation is that expectations were "irrational"—an idea long championed by Robert Shiller and a result found throughout the experimental literature on asset bubbles (Caginalp et al., 2000). This view holds that expectations can be heavily influenced by recent price changes and that the five to ten years leading up to the 2006 peak were dominated by positive momentum. Thus, prices became much less rooted to long-term benchmarks. So this critical input to user cost seems very difficult to measure with any confidence. Follain (2008) elaborates on this point, with specific references to the ideas of Shiller and his longtime collaborator, Karl Case.

This view is not universally held. In fact, several recent papers have estimated house price models with a user cost component. These include Goodman and Thibodeau (2008), Case and Quigley (2009) and Gabriel et al. (2008). These papers necessarily rely upon a rule or benchmark to specify user cost and by using these previous benchmarks can identify a widely held view—house prices were destined to decline and the bubble to burst. We surely agree with this prediction, but it is based upon some measurable and exogenous benchmark of expected house price inflation. Our approach circumvents this step, allowing us to remain agnostic with respect to expected housing price appreciation. Instead, we include two key interest rate measures and lagged values of house price growth. As is demonstrated in the next two sections, this specification shows substantial increases in the relative importance of lagged inflation as a predictor of changes to house prices in the latter years. This is consistent with the basic views of Case and Shiller and runs counter to an approach that rests upon steady measures of expected inflation. Something dramatic happened to these expectations in the last 10 years that seems outside of any previous benchmarks. Until more is learned about how to model these expectations, a less specific and less structural approach may have substantial advantages.

The model we estimate includes lagged values of the dependent variable as well as lags of all of the other state-specific covariates, such as employment. Some papers focus attention on a more precise and statistically driven specification and interpretation of the lagged terms on the endogenous variables. One example is Capozza et al. (2004). They develop a dynamic difference equation with autoregressive error terms. This approach includes a parameters that encompass a wide range of possible outcomes from mean reversion to the other extreme of divergent oscillation. They also include economic variables intended to proxy for information costs, supply costs, and expectations. Their empirical analysis uses data for 62 large metro areas for the years 1979–1995. Based on estimates from various lag structures, they calculate specific measures of mean reversion versus momentum. They find wide variations in the estimated coefficients and the implied dynamic behavior among the metropolitan areas and conclude that the dynamic properties of housing markets are specific to time and location.

In another paper, Lai and Order (2010) also develop and estimate a dynamic difference equation with the potential to measure mean reversion and momentum. They use data for 44 MSAs from 1980 to 2005 and rely on an equilibrium relationship between rents and values. They report evidence of momentum throughout the period; however, momentum increased substantially after 1999 resulting in what could be characterized as a bubble after 2003.

Our results also shed light on these issues, but our approach is simpler and, again, imposes less structure on the coefficients. As mentioned above, our approach also includes several lags of the key variables. We chose four quarterly lags as a plausable starting specification but did not pursue or test for the possibility of a unique or optimal lag structure for all groups and time periods examined. Instead, we highlight the sum of the lagged coefficients of the lagged house price terms as an indicator of the potential of the momentum effect. All else equal, a positive sum of these coefficients is interpreted as momentum; the larger the sum the stronger the momentum. A negative sum is interpreted as mean reversion.

3. Data and estimation

3.1. Data

Several sources are used to create quarterly time-series datasets for each of the 50 US states and D.C. The data extend from the beginning of 1975 to the third quarter of 2009. Housing price data are from OFHEO's state housing price index (HPI).[13] HPI is a repeat-sales index that measures state-level price growth for single family housing and includes housing purchased using conventional mortgages that is subsequently purchased or securitized by Fannie Mae or Freddie Mac. Interest rate data rates for Treasury notes and bills are from US Treasury (and are available from numerous sources). Quarterly data on nonfarm employment are from the Bureau of Labor Statistics (BLS).

12. One of us (Follain) has written many articles that highlight the potential role of user cost as it relates to housing, including one that was written over 25 years ago (Follain, 1982).

13. As noted earlier, in 2009 OFHEO was absorbed into the Federal Housing Finance Agency (FHFA).

Data on each state (along with the interest rate variables) are pooled into one large dataset comprising 7089 observations (139 observations for each of the 50 states and D.C.).

States are separated into three groups in order to compare the experiences of states with different characteristics. The state groupings are constructed by ranking states by the variance of their HPI from 1991 to 2009. States with more housing price variability are likely to respond differently to exogenous changes than are states with lower variances. For example, housing supply may be more inelastic in these states. (See Goodman and Thibodeau, 2008, and Harter-Dreiman, 2004, for recent empirical findings on housing supply elasticities.) The state groupings are:

1. Delaware, Massachusetts, California, Rhode Island, New York, New Jersey, Maryland, New Hampshire, Hawaii, Florida, Maine, Virginia, Oregon, Washington, Arizona, D.C. and Nevada. These are the high-variance states. Also, note that these states are primarily "coastal" and are often characterized by large MSAs where land (or housing) is relatively constrained.
2. Connecticut, Vermont, Minnesota, Colorado, Montana, Utah, Pennsylvania, Idaho, Wyoming, Illinois, Michigan, Wisconsin, New Mexico, Georgia, Alaska, South Carolina and Missouri. These are the medium-variance states.
3. South Dakota, North Carolina, Louisiana, Tennessee, North Dakota, Kentucky, Alabama, Kansas, Iowa, Mississippi, Nebraska, Arkansas, West Virginia, Ohio, Texas, Oklahoma and Indiana. These are the low-variance states. In general, the population centers of these states are not characterized by binding land (or housing) constraints. Traffic congestion is also less severe in the population centers of these states, and thus, the non-pecuniary costs of living farther from the central business district, for example, are relatively low.

Table 1 compares summary statistics for each of these three groups for all years and for two sub-periods, 1975–1990 and 1991–2009. Real HPI growth (weighted by state employment) varies greatly over time and across state groups. By definition, HPI variability falls as one moves from group 1 to group 3. However, it is also true that states with higher price variances also experienced the greatest increases in real housing prices from 1975 to 2009. When including all years, real annual HPI appreciation for group 1 is four times that for group 2—the HPI for group 3 states is the same (in real terms) after quarter 3 of 2009 as it was when the dataset began in quarter 1 of 1975. Breaking these numbers down by time-period shows even greater disparity across groups from 1975 to 1990. Post-1991, the picture is quite different. Here, HPI growth is stable across all three groups of states (ranging from a low HPI increase of 1.2% to a high of 1.6%).

Table 1. Annual growth rates.

	All	Group 1	Group 2	Group 3
1975–2009				
RealHPI[a]	1.2	2.4	0.7	0.0
Employment[a]	2.1	2.3	1.7	2.2
*TB*10[b]	–0.14			
*TB*10 – TB1[b]	0.10			
1975–1990				
RealHPI[a]	0.7	2.7	–0.1	–1.2
Employment[a]	2.5	3.0	2.0	2.4
*TB*10[b]	–0.01			
*TB*10 – TB1[b]	0.09			
1991–2009				
RealHPI[a]	1.5	1.6	1.5	1.2
Employment[a]	1.2	1.2	1.0	1.4
*TB*10[b]	–0.25			
*TB*10 –TB1[b]	0.11			

Calculations are based on data from FHFA (formerly OFHEO), BLS, and U.S. Treasury.

a. Measured in percent change.

b. Measured in percentage point change.

Comparing the two time-periods, overall real HPI growth is over twice as large post-1991 than pre-1990 (1.5% versus 0.7%)—even after accounting for the substantial drop in housing prices beginning in 2006. For group 1 states, average annual HPI growth drops from 2.7% pre 1991 to 1.6% post 1990. For the other two groups, the reverse is true: negative growth pre 1990 is followed by average annual growth that ranges from 1.2 to 1.5% post 1991.

Figure 2 plots the real HPI for each of the three groups of states. By construction, all groups start at a value of 100 in 1975. The trend for each group is based on the weighted average HPI for states within the group—where the averages are weighted by state employment. The direction of changes is similar for groups 1 and 2, but the pattern is greatly amplified for group 1—with both much more precipitous increases and decreases. The pattern for group 3 is a muted version of that for group 2, except during the second half of the 1980s, where the two trends diverge. In contrast to HPI growth, employment growth is much more robust pre-1990 than post-1991. This is due primarily to the dismal rate of US employment growth during the first decade of the 2000s (see Irwin, 2010). The pattern is similar for each of the groups; however, the drop in employment growth is more pronounced for group 1 states, where the annual growth rate fell by 1.8 percentage points (versus 1 percentage point for group 2 and 3 states).

3.2. Estimation approach

Equation 1 relates housing price appreciation to fundamentals that are believed to influence housing prices. It includes time and fixed effects for state groups in order to absorb unobserved factors and can be expressed such that:

$$\log\left(\frac{HP_{it}}{HP_{it-1}}\right) = \alpha_t + \alpha_{group} + \alpha_{season} + \sum_{j=1}^{4}\left(\frac{HP_{it-j}}{HP_{it-1-j}}\right) + \sum_{j=1}^{4}\left(\frac{Emp_{it-j}}{Emp_{it-1-j}}\right) + \delta_1(TB10_{t-4} - TB1_{t-4}) + \delta_2 TB10_t + \varepsilon_{it} \quad (1)$$

The dependent variable, $log(HP_{it}/HP_{it-1})$, measures the quarterly growth rate in real housing prices (HP) for state i in time t. The explanatory variables include lags of housing

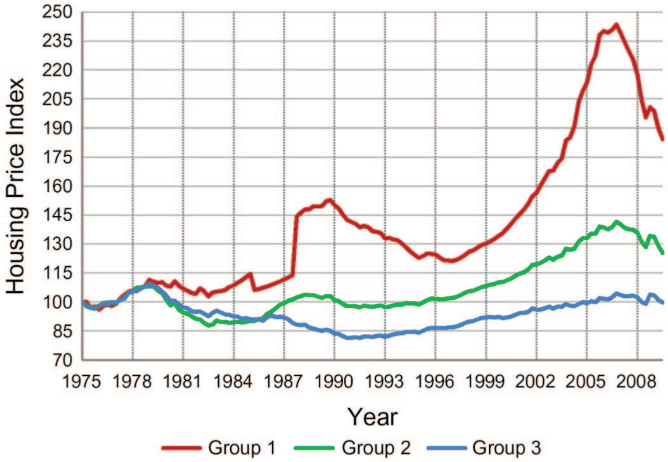

Figure 2. Real housing price appreciation by state group. Calculations are based on FHFA's (formerly OFHEO) state housing price index. The housing price index is normalized to 100 for all states in 1975. Group averages are weighted by state employment. See Section 3 for state groupings.

price growth, $log(HP_{it-j}/HP_{it-1-j})$, and lags of employment growth, $log(Emp_{it-j}/Emp_{it-1-j})$, for the preceding four quarters, where j represents lagged quarters one to four. Housing price lags are included to help distinguish between markets characterized by price momentum and those where mean reversion is more prominent. Momentum (or self-reinforcing) effects would include bubbles, where behavior such as speculation can lead to periods of rapid price increases and, after peaking, rapid price decreases. Lags in employment growth are intended to pick up more traditional changes in housing demand. The ten-year treasury rate ($TB10$) is included because it is correlated with mortgage interest rates and is also exogenous (i.e., not influenced by changing lending practices, etc.). Additionally, a one-year lag of the yield spread ($TB10_{t-4} - TB1_{t-4}$) is also included because it has been shown to be a good predictor of real economic activity (e.g., see Estrella and Hardouvelis, 1991; Estrella and Mishkin, 1998). Additionally, dummies for state-group (α_{group}), time (α_t) and seasonal (α_{season}) are included in the equation.

As an alternative to group dummies, full state fixed effects could be included in the model. This would impose stronger controls for heterogeneity across states, but would reduce the degrees of freedom and wash away within-group cross-sectional variation. Results in the following sections are not sensitive to several alternative criteria for grouping states. For example, results from employing groupings used by Abraham and Hendershott (1994) OFHEO has only a nominal effect on the results.

By imposing a variety of sample restrictions, we test whether the relationships described in Equation 1 have changed over time and whether they vary by region. To this end, Equation 1 is estimated for the following samples:

1. All observations (from 1975 to quarter 3 of 2009).
2. High price variance states.
3. Medium and low price variance states.
4. Observations from 1975 to quarter 4 of 1990.
5. High price variance states for years 1975 to quarter 4 of 1990.
6. Medium and low price variance states for years 1975 to quarter 4 of 1990.
7. Observations from 1991 to quarter 3 of 2009.
8. High price variance states for years 1991 to quarter 3 of 2009.
9. Medium and low price variance for years 1990 to quarter 3 of 2009.
10. Observations from 1975 to quarter 4 of 2000.

By comparing estimated coefficients when employing these different sample restrictions, we test whether the relationship between the explanatory variables and housing price growth varies across states. For example, housing prices in states where housing supply is more inelastic should be more responsive to the explanatory variables. Furthermore, changes to political and economic institutions could alter the relationship between the explanatory variables and housing price growth. And, it is possible that these changes had a heterogeneous impact across the country. Results from the regression analysis can be used to examine these questions.

The fixed effects can be thought of as measuring the influence of an array of factors that are not explicitly included in the estimation—and which often are not available in sufficient detail over a long period of time. Additionally, if, over time, there are major shifts in the relationship between explanatory variables and housing price growth, then our insight into the factors that led to the collapse of major components of the US financial sector may be limited. As addressed in a later section, a heavy reliance on fixed effects makes it difficult to assign a relative importance to the array of factors that may have contributed to the sharp decline in the housing market and the broader turmoil that spread through the economy more generally, however, we show that our analysis can be used to assign probabilities to various severe economic events—even if the proximate cause of the economic stress remains opaque.

4. Evaluation of model estimates and implications

In this section estimates are presented for the model developed in the previous section. The factors correlated with housing price growth are examined for the full sample and then compared to estimates for the various subsamples in order to highlight possible heterogeneity in this relationship over time and across states. Additionally, the regression results are important because they lay the foundation for the simulation and stress test conducted later in this section.

4.1. Review of regression estimates

Table 2 presents regression estimates for Equation (1) under the ten different sample restrictions. Estimated coefficients for the four employment growth lags vary greatly by specification, but consistent with theory, are always positive. As a caveat, while employment is a core fundamental in determining housing prices, the theoretical foundations

Table 2. Estimation results for 10 combinations of time period and states dependent variable: $ln(HP_t/HP_{t-1})$.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years:	1975–2009			1975–1990			1991–2009			1975–2000
States (Group):	All	1	2 & 3	All	1	2 & 3	All	1	2 & 3	All
$ln\left(\frac{Emp_{t-1}}{Emp_{t-2}}\right)$	0.332	0.301	0.344	0.328	0.320	0.351	0.111	0.123	0.114	0.335
	(0.066)	(0.155)	(0.048)	(0.090)	(0.214)	(0.079)	(0.022)	(0.036)	(0.036)	(0.079)
$ln\left(\frac{Emp_{t-2}}{Emp_{t-1}}\right)$	0.358	0.352	0.360	0.410	0.450	0.407	0.092	0.104	0.106	0.383
	(0.048)	(0.107)	(0.041)	(0.058)	(0.163)	(0.049)	(0.022)	(0.038)	(0.031)	(0.055)
$ln\left(\frac{Emp_{t-3}}{Emp_{t-2}}\right)$	0.320	0.199	0.360	0.343	0.169	0.418	0.096	0.125	0.098	0.34
	(0.063)	(0.113)	(0.053)	(0.093)	(0.141)	(0.099)	(0.019)	(0.022)	(0.030)	(0.076)
$ln\left(\frac{Emp_{t-4}}{Emp_{t-3}}\right)$	0.346	0.221	0.378	0.358	0.187	0.406	0.117	0.102	0.129	0.350
	(0.058)	(0.086)	(0.064)	(0.071)	(0.110)	(0.095)	(0.020)	(0.033)	(0.031)	(0.065)
$ln\left(\frac{HP_{t-1}}{HP_{t-2}}\right)$	–0.339	–0.312	–0.426	–0.432	–0.386	–0.482	0.235	0.247	0.099	–0.394
	(0.061)	(0.082)	(0.085)	(0.062)	(0.081)	(0.089)	(0.033)	(0.046)	(0.034)	(0.060)
$ln\left(\frac{HP_{t-2}}{HP_{-1}}\right)$	–0.087	–0.083	–0.172	–0.170	–0.124	–0.216	–0.045	–0.119	–0.077	–0.125
	(0.045)	(0.073)	(0.058)	(0.051)	(0.087)	(0.064)	(0.019)	(0.032)	(0.022)	(0.049)
$ln\left(\frac{HP_{t-3}}{HP_{-2}}\right)$	0.064	0.094	–0.029	–0.012	0.056	–0.068	0.257	0.254	0.209	0.030
	(0.057)	(0.106)	(0.071)	(0.064)	(0.121)	(0.074)	(0.016)	(0.036)	(0.018)	(0.062)
$ln\left(\frac{HP_{t-4}}{HP_{-3}}\right)$	0.102	0.149	0.019	0.042	0.114	–0.020	0.173	0.090	0.234	0.070
	(0.033)	(0.044)	(0.041)	(0.035)	(0.053)	(0.044)	(0.029)	(0.043)	(0.037)	(0.033)
TB10	–0.006	–0.002	–0.008	–0.004	0.002	–0.006	–0.009	–0.007	–0.010	–0.004
	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.000)	(0.000)	(0.000)	(0.001)
$TB10_{t-4}$ – $TB1_{t-4}$	–0.001	0.001	–0.002	–0.002	0.000	–0.003	0.002	0.004	0.001	–0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)	(0.001)
Group 1 dummy	0.004			0.011			0.00002			0.005
	(0.001)			(0.003)			0.0004			(0.002)
Group 2 dummy	0.002		0.002	0.003		0.004	0.0001		0.0003	0.002
	(0.001)		(0.001)	(0.002)		(0.002)	0.0002		0.0002	(0.001)
Seasonal dummies										
Quarter 1	–0.004	–0.002	–0.003	0.003	0.006	0.003	–0.011	–0.011	–0.009	–0.0004
	(0.003)	(0.006)	(0.004)	(0.006)	(0.011)	(0.009)	(0.001)	(0.002)	(0.001)	(0.004)
Quarter 2	–0.005	–0.004	–0.004	0.004	0.007	0.003	–0.007	–0.008	–0.006	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.000)	(0.001)	(0.002)
Quarter 3	–0.006	–0.009	–0.005	–0.001	–0.005	0.001	–0.009	–0.008	–0.009	–0.002
	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)	(0.009)	(0.001)	(0.002)	(0.001)	(0.004)
Constant	0.006	–0.001	0.019	0.015	0.007	0.057	0.036	0.065	0.041	0.023
	(0.006)	(0.046)	(0.005)	(0.028)	(0.027)	(0.024)	(0.002)	(0.004)	(0.001)	(0.008)
R-squared	0.210	0.270	0.230	0.220	0.220	0.240	0.570	0.690	0.490	0.200
Observations	6,834	2,278	4,556	3,009	1,003	2,006	3,825	1,275	2,550	5,049

Calculations based on data from FHFA (formerly OFHEO), BLS, and US Treasury. Robust standard errors are in parentheses. Regressions are unweighted (i.e., not weighted by employment). All specifications also include year fixed effects.

for the relationship between *changes* in employment and *changes* in house prices is less clear. To the extent that changes in employment (which influence housing demand) are expected, they should already be factored into housing prices, preventing opportunities for arbitrage. Thus, changes in housing prices should only affect housing prices to the extent that these changes are unexpected. This is a parallel to the random walk hypothesis for stock prices. However, the extent to which housing markets are efficient in this respect is not a settled issue (see, e.g., Case and Shiller, 1989; Shiller, 2005).

Turning to lagged housing price appreciation, estimated coefficients are negative (at least for lags from the two previous quarters) for all samples that include pre-1991 data (columns 1, 3, 4, 5, 6, 8, and 10 of Table 2). This suggests mean reversion: price increases (decreases) in one quarter are followed by price decreases (increases). This is also inconsistent with pricing bubbles. By contrast, the corresponding lagged coefficients are generally positive for all of the post-1990 samples (columns 2, 7, and 9 of Table 2). And, among the specifications including only post-1990 data, estimated coefficients on the housing price lags are much larger when group 1 (i.e., high variance) states are included in the sample. Taken together, this suggests that a change occurred not just in the magnitude of the relationship between housing price growth and the lags, but also in the direction of that relationship. The pattern in this later period is consistent with bubble phenomena (although other explanations also exist), where price changes in one quarter are self-reinforcing in subsequent quarters.[14]

4.1.1. Insights from the full sample

Table 3 presents a summary of the key results from the regressions. Estimated coefficients on the lagged variables are summed to assess the cumulative impact of lagged housing price and employment growth, respectively. Column 1 of Table 2 shows a strong relationship between lagged employment and housing prices; a 1% increase in employment over each of the previous 4 quarters is associated with a 1.36% increase in real housing prices. The relationship between lagged housing price growth and current housing price growth is negative; a 1% increase in real housing prices over each of the previous 4 quarters is associated with a –0.26% change in current housing prices (again adjusted for inflation). This pattern is inconsistent with pricing bubbles, since price increases, ceteris paribus, are generally followed by decreases and vice-versa.

4.1.2. Insights from comparing results from the subsamples

Recall that the US experience over the past decade has been unique, not only in the magnitude of price appreciation (and subsequent price depreciation), but also in scope. Never before (or at least since the advent of modern house-price indexes) has housing price appreciation been so

14. Another explanation for what we characterize as mean-reverting phenomena, is simply data noise. Under this scenario, which cannot be ruled out, an outlier (due to measurement error) in one period is corrected in the next period.

Table 3. Summarizing key coefficients.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years:	1975–2009			1975–1990			1991–2009			1975–2000
States (Group):	All	1	2 & 3	All	1	2 & 3	All	1	2 & 3	All
Unweighted regressions										
Sum of *Emp* lags [a]	1.355	1.072	1.443	1.438	1.127	1.582	0.415	0.453	0.446	1.408
Sum of *HP* lags [a]	-0.259	-0.153	-0.608	-0.572	-0.340	-0.786	0.621	0.472	0.465	-0.418
T10	-0.006	-0.002	-0.008	-0.004	0.002	-0.006	-0.009	-0.007	-0.010	-0.004
$TB10_{t-4} - TB1_{t-4}$	-0.001	0.001	-0.002	-0.002	0.000	-0.003	0.002	0.004	0.001	-0.002
Constant + Average F.E. [b]	0.043	0.018	0.056	0.022	-0.023	0.045	0.052	0.041	0.059	0.031
Employment-weighted regressions										
Sum of *Emp* lags [a]	0.829	0.698	0.945	0.850	0.602	1.098	0.455	0.636	0.350	0.835
Sum of *HP* lags [a]	0.349	0.336	-0.062	0.109	0.309	-0.219	0.655	0.424	0.493	0.224
T10	-0.005	-0.004	-0.007	-0.002	0.001	-0.004	-0.008	-0.007	-0.009	-0.003
$TB10_{t-4} - TB1_{t-4}$	-0.001	0.001	-0.001	-0.003	-0.002	-0.003	0.003	0.005	0.001	-0.003
Constant + Average F.E. [b]	0.043	0.040	0.051	0.017	-0.001	0.035	0.048	0.041	0.055	0.025

Calculations based on data from FHFA (formerly OFHEO), BLS, and US Treasury.
a. This is the aggregate of the estimated coefficients on the four lagged variables.
b. This is the estimated constant term plus the average of the estimated coefficients on the years dummies.

widespread, with similar phenomena observed across much of the country (with only a few exceptions). However, while the trends have been more homogeneous than in the past, the degree to which prices changed did vary greatly across the country. The high price variance states have seen sharper price increases followed by sharper decreases over the past two decades than have the primarily medium and low price variance states (again, see Figure 2). A comparison of the sum of lagged coefficients for the high price variance and medium and low price variance regressions buttresses the patterns observed in the data.

Regression results (presented in Table 3) are suggestive of a structural change in the relationship between housing price appreciation and the explanatory variables. The pre-1991 regression (Table 3, column 4) yields cumulative effects from lagged employment and lagged housing prices that are similar to those from the full sample. However, the relationship between both sets of lags and house price appreciation is amplified. The sum of both the employment and housing price lags are larger in absolute magnitude, suggesting greater reliance on employment growth (a core fundamental) and also that a change in housing prices will be followed by a more rapid (or larger) reversion in the opposite direction (which again is inconsistent with selfreinforcing price bubbles).[15] This is in stark contrast to the post-1990 regression (Table 3, column 7), where the sum of the employment lags is 0.42, or 71% smaller than for the pre-1991 period. Turning to the housing price lags, the difference in the cumulative measure is even more startling. Post-1990, the cumulative housing price lags equal 0.62—i.e., very close in absolute magnitude to the measure for the pre-1991 period (–0.57), but the direction of the relationship has reversed (from negative to positive)! This suggests a shift away from fundamentals and towards a market characterized by momentum and pricing bubbles. Post-1990 period, lagged housing price growth does not act to offset current growth as the momentum effect dominates, leading to ever larger price appreciation; likewise, price declines are self-reinforcing, leading to even sharper price declines.

Weighting the regressions by employment (so that larger states are given more importance than smaller ones) paints a somewhat different picture. Again, see Table 3. When including all years, momentum now tends to dominate. For group 2 and 3 states, mean reversion is still present, but is very modest. For the group 1 states, momentum dominates. It may be that momentum is more pronounced in some of the larger states within group 1. For example, California, New York and Florida are in group 1 and all saw sharp runups in house prices. These three states constitute less than 18% of the group 1 sample (i.e., three of 17 states), but roughly 55% of group 1 employment. Thus, for the group 1 regressions, employment weighting increases the importance of these three states by more than threefold. In any case, with employment weighting, what we characterize as momentum increases sharply for all groups when moving from the pre 1990 to post 1991 specifications (consistent with the unweighted results).

For each of the ten (unweighted) specifications, *F*-tests find that the cumulative impact of the employment growth lags are statistically different from 0 at nearly any level of significance. The one exception is the pre-1991 specification for the high variance states, where the lagged employment variables have a *p*-value of 0.051. Turning to the housing price lags, *F*-tests find that, for seven of the specifications, the cumulative effect of the lags are statistically different from 0 at nearly any level of significance. However, for the three of the five specifications that include pre-1991 data for high price variance states (specifications shown in columns 1, 4, and 6 of Table 2), the cumulative effect of the housing price lags is not statistically different from 0—a result consistent with the efficient markets hypothesis.[16]

Next, a χ^2 test is employed to assess the statistical importance of the apparent structural break shown in Table 3. In comparing the pre-1991 and post-1990 regressions (columns 4 and 7 from Table 2), the χ^2 test results are consistent with a structural break for both employment and housing prices; i.e., the null that the sum of the coefficients on the lagged housing price variables are equal in the two regressions is rejected at virtually any significance level; and, the same is true when testing for the equality of the sum of the coefficients on the lagged employment terms. This same result is found again when comparing pre-1991with post-1990 regressions that include only the states with low and moderate house price variances (columns 6 and 9 of Table 2). Looking only at the high price variance states (columns 5 and 8 of Table 2), the χ^2 test results are again consistent with a structural break for the housing price lags (at the 1% level), but not for the employment lags (even though the sum of the lagged employment coefficients pre-1991 is about 1.5 times larger than the equivalent post-1990 measure). Finally, when comparing high price variance states to medium and low price variance states, the null that the sum of the coefficients are the same for the two groups can never be rejected for both employment and housing prices.

4.1.3. Other factors to consider

Estimated coefficients for the other two economic variables—*TB*10 and the yield spread lagged four quarters—also show variability across both state grouping and time. The estimated coefficient for *TB*10 is consistently negative and statistically significant, implying that lower long-term interest rates are associated with higher housing prices. As noted earlier, changes to *TB*10 are a proxy for changes in mortgage rates, but are likely exogenous, since they are not influenced by changes to the characteristics of borrowers (such as their probabilities of default). Also, recall that *TB*10 is not in log form (and is not differenced), since this has important implications for interpreting the estimated coefficients. In contrast to employment growth, the estimated coefficients on *TB*10 are much larger (in absolute magnitude) post-1990 than pre-1991. The difference across time-periods is, at least partly, due to the much higher inflation pre-1991 and to the fact that *TB*10 is a nominal interest rate (whereas housing price growth is measured in real terms). High real

15. This result is consistent with arguments made by Shiller (2005).

16. When including all of the data, the housing price lags are statistically significant at the 10% level. For the other two specifications (columns 4 and 6 from Table 2), these lags have *p*-values in excess of 0.3.

interest rates discourage home-buying, whereas high nominal rates (along with expectations for high inflation) should not. Across states, the relationship is stronger for the lower price variation states than for the high variation states—possibly suggesting a stronger connection to fundamentals in the former group. χ^2 tests also support the claim that the importance of *TB*10 differs both across time periods and across states. When comparing regressions for high price variance states to those for medium and low price-variance states, the null that the coefficient for *TB*10 is the same across states is always rejected. And, for both sets of states, results from χ^2 tests confirm a structural break in the relationship between *TB*10 and growth in housing prices. All of this supports the notion that housing markets are local, that housing supply elasticities are heterogeneous across communities, and state and local characteristics are important factors to consider when analyzing housing markets.

The yield spread has a statistically significant and positive impact for all of the post-1990 specifications.[17] The effect is about 4.8 times larger from the high price variance states than for median and low price variance states (0.0037 versus 0.0008, respectively). The effect of the slope of the yield curve is sometimes negative and always statistically insignificant for pre-1991 specifications and when including all years. The null that the effect of yield spread is the same across specifications generally cannot be rejected (by a χ^2 test).[18] The lag of the yield spread has been shown to be a good predictor of recessions (Estrella and Mishkin, 1998) and of economic activity more generally (Estrella and Hardouvelis, 1991), however, its relationship with house prices appears to be much weaker. This may be because the correlation between housing prices and economic activity is not nearly as strong as the relationship between some other variables, such as residential investment, and economic growth (Leamer, 2007). And, the past couple of years aside, negative demand shocks generally manifest themselves in changes to investment, rather than falling prices.

Another important difference seen when comparing the regression estimates is the year fixed effects, which allow for year-specific constant terms. Adding the constant term to the average of the year fixed effects yields an average constant term for the post-1990 specification of 0.05, or well over twice as large as the pre-1991 measure (0.02). At first glance, it appears that fixed effects explain much more of the growth in the later period. However, it more likely reflects the fact that average real housing price growth for the US was much higher from 1991 to 2009 than for years 1975–1990, the past few years notwithstanding. In fact, as measured by OFHEO housing price index, average annual real housing price growth was 3.8 times greater in the latter period (1.72% versus 0.45%). Comparing the same measure (i.e., the constant term plus the average of the year fixed effects) across states (instead of across time periods) yields an estimate for the states with low to medium house price variance that is roughly three times the corresponding measured for the high variance states (0.06 versus 0.02, respectively). Again, the states with high variances in housing prices also had much greater real house price appreciation over years 1975–2009, even after factoring in the recent decline, which likely goes a long way in explaining the larger fixed effects for the regressions that include only these states.

In addition to these differences, the estimated root mean square error (RMSE) also differs greatly across time-periods, with the pre-1991 specifications yielding RMSEs three to four times as large as those for the post-1990 specifications. For the pre-1991 specifications, the RMSE always exceeds 0.04; for the three post-1990 specifications, the RMSE is 0.01 (or slightly larger). When including all years, the RMSE is about 0.03. (Interestingly, for a given time-period, the RMSE varies little across state groupings.) These differences have important implications for the forthcoming Monte Carlo exercise and the resulting stress test scenarios.

In sum and in broad terms, the estimation suggests dramatic differences in the underlying statistically based characterizations of the pre- and post-1990 periods. The more recent regime and the high price variance states show house price growth being much less sensitive to what are typically thought to be the "real" drivers of house price growth—employment growth and interest rates—less mean reversion, much more sensitivity to recent momentum in house price growth, and a lower constant growth rate. This relationship is more pronounced in the post-1990 era and especially among the high price variance states in that period.

4.2. The distribution of changes in house prices

Here, the estimated coefficients lay the foundation for a Monte Carlo simulation model. The simulation serves two broad purposes. First, it provides insights into the comparative statics implied by all aspects of the model estimates, including the implications regarding the mean square error and the role of momentum. Second, it generates estimates of specific stress test scenarios associated with extreme outcomes, which is a key goal of this paper.

4.2.1. Monte Carlo method

Estimated coefficients from the various regressions are used to project house price growth rates over three years (12 quarters). In addition to the estimated coefficients, inputs into the simulation include lagged values of employment growth, house price growth and interest rates in the year prior to the projection, as well as employment growth and interest rate scenarios during the three years of the projection. Prior to the Monte Carlo simulation, the regression estimates are used to generate predicted housing price paths over 12 quarters, where the predicted values are imputed by combining out-sample-data with the estimated regression coefficients.[19] In place of generating separate

17. Given the rate for *TB*10, an increase in the spread implies a reduction in the short-term interest rate.
18. The one exception, where the null is rejected, is when comparing the pre-1991 and post-1990 specification that includes all states (columns 4 and 7 of Table 2).
19. The constant term for these imputations is equal to the estimated constant term plus the (weighted) average of the estimated coefficients on the year dummies.

price paths for each state, we begin with a stylized dataset for our projection—which represents a hypothetical state (or composite of several states).[20] For this process, housing price growth is treated as an endogenous variable and generated iteratively. That is, because lags of the dependent variable are included as explanatory variables, out-of-sample predicted values must be generated iteratively, one quarter at a time. After each iteration, the newly imputed value for housing price growth then becomes the one quarter lag for housing price growth used in the next iteration.

The process begins with a benign set of assumptions regarding starting lagged values and inputted values for employment and interest rates for the 12 quarters; thus, we refer to the predicted values as a "projected" price path as opposed to a "forecast" of what will actually occur given current conditions. Less benign assumptions regarding the inputted data are also explored in the sensitivity analysis, however, an important part of our analysis is to test whether, even from a benign starting point, the estimated relationships presented earlier are capable of producing substantial drops in housing prices with probabilities, that while very low, are large enough to have important economic consequences.[21] For the different estimated equations, the model is then used to generate 1000 separate 12 quarter paths for housing prices. The Monte Carlo method is similar to generating out-of-sample predicted values with a couple of twists:

1. For each year of the three-year projection, the year fixed-effect is chosen at random (based on the actual share of observations for which each dummy is included in the relevant regression). Which year fixed effects are used is tremendously important. Assumptions regarding the year fixed effects could be circumvented by excluding year dummies from the regressions. However, as shown earlier in this section, the year fixed effects are extremely important and appear to control for substantial unobserved heterogeneity. Excluding the year fixed effects would almost surely bias the estimated coefficients.
2. For each quarter of the projection, a stochastic term is added to the prediction. The stochastic term is normally distributed with mean 0 and standard deviation equal to the RMSE (i.e., the standard deviation of the error term from the regression).

From the 1000 paths generated from each specification, 1000 cumulative three-year housing price changes can be calculated. These cumulative price changes represent the distribution of projected outcomes. Year fixed effects capture unobserved factors unique to a particular year—such as general economic conditions etc. Randomly choosing which fixed effect to include in the projection assumes that these unobserved factors are likely to occur again with similar probabilities as observed in the data. With this approach, unobserved factors can be used to better formulate the distribution of possible outcomes without the understanding the complexity underlying these factors—or to what degree specific unobserved factors are playing. Note that while this approach is used to gain insight into the severity of low probability events, it is not done by simply employing a (log-) linear model and extrapolating it out to extremes (that may not be present in the estimating data). For example, low probability events are not simulated by inputting extreme values for lagged employment or housing price growth, or by inputting extreme interest rate scenarios into a linear model. This approach would assume that estimated linear relationships hold over a much longer range than what is reasonable. On the contrary, here extreme outcomes are generated from uncertainty that is observed in the data and measured by the RMSE and the influence of unobserved factors captured in the year fixed effects.

To reiterate and expound on some issues raised earlier, this approach is built on a highly simplified model of the housing market, however, despite its simplicity, we believe that the approach is insightful and the costs associated with a richer model likely exceed the benefits. First, given the number and immense complexities of events occurring in recent years, identifying structural parameters, at this point in time, is likely to produce results that are either misleading or ones that contain so much uncertainty so as to have little practical value. While maintaining a reduced form approach, the model could still be made more complex. For example, additional explanatory variables could be added or a vector autoregression (VAR) framework could be adopted that would treat employment (and potentially other variables) as endogenous. The downside of adding additional variables is that this requires additional assumptions for the out-of-sample observations needed to generate the projected housing price paths. This adds another layer of uncertainty to the process and makes assessing the importance of a few core fundamental variables less transparent. Additionally, many of the available variables that could potentially be added to the model are highly correlated with either housing price growth or employment growth, which limits their value added. The downside of a VAR approach is that it also hampers transparency. On the plus side, a VAR recognizes that variables such as employment and housing price growth do not move independently, however, expecting to correctly identifying this relationship over the past decade (with any degree of precision) is likely quixotic.

With our approach the implications from alternative employment or interest rate scenarios can easily be explored. (For example, see the sensitivity analysis later in this section.) While this does not allow for a dynamic relationship between the housing and employment markets, for example, analyzing the impact of a shock to one of these sectors is again quite transparent. To the extent that one believes that these markets play off each other, via feedback loops, projected paths can be viewed as lower (or upper) bounds.

20. The stylized dataset uses average values, by quarter, for each of the economic variables. These values are used as the initial lags and are repeated for each year of the projection (the exception being housing price growth).
21. Later, out-of-sample state forecasts that start with observed state-level data are also explored.

4.2.2. Results from the Monte Carlo simulation

Results from the simulation are summarized in Table 4. The first row of Table 4 reports the RMSE for each of the regressions. The remainder of the table shows the cumulative percent change in projected real house prices at the end of the 12 quarter projection for each of the different regressions and at different points in the distribution. The top set of projections are for "group 1" states in the sense that the group 1 (i.e., high price variance) dummy is set to 1. The lower set of projections are for states in group 3 (i.e., states with lowest housing price variances) in the sense that all state group dummies are set to 0 (and thus, the constant term pertains to group 3, the dummy that was dropped in the regression). For columns 2, 5 and 8, projections labeled as group 3 are the same as those for group 1, since the corresponding regressions include only data on group 1 states. For columns 3, 6 and 9, projections for group 1 are the same as those for group 3, since here, the corresponding regressions include only data on groups 1 and 2.[22]

Based on the regression results when including all of the data, a severe stress event, associated with a one-percent probability, yields a cumulative price decline of between 19% and 23% after 12 quarters. The price decline associated with a one-percent stress scenario is also within this same range when relying on the regressions that include all quarters, but subsets of states (columns 2 and 3). Corresponding estimates for the severity of a five-percent stress scenario are between 5.2 and 6.6 percentage points lower. Turning to the results based on regressions for the other subsamples yields very different results—suggesting that the structural breaks found in the regression analysis have important implications for low probability events. Using the pre-1991 regression results, one-percent scenarios are associated with price declines of between 22% and 29%. The results based on the group 1 regression yield an over 27% price decline. For the post-1990 regressions, simulation results are even more dire, with one-percent scenarios associated with price declines of between 33% and 40%. The 40% decline results from the post-1990 regression and including all states. It is this scenario that stands out. When including all data or only pre-1991 data, the projected severity of one-percent scenarios are 25–50% smaller.

The past few years aside, the one-percent scenarios presented in Table 4 are much more severe than any threeyear experience for the nation or for any of the three groups of states examined here. For example, the sharpest real (employment-weighted) 12-quarter price decline ranges from 10.6 (for group 1 states) to 15.6% (for group 2 states). (Refer to Figure 2.) However, many states have experienced 12 quarter price declines similar to the one-percent simulation results. For example, 26 states have experienced 12-quarter real price declines of more than 20%, while 7 states have experienced real price declines

22. Also, note that for the post-1990 regression that includes all states, the estimated coefficients on the state group dummies equal 0 (at least when rounding to three decimal points); thus, projections based on this regression do not vary by state group.

Table 4. Core simulation results cumulative percent change in housing prices after 3 years (with stochastic shocks and random year fixed effects).

Years:	1975–2009			1975–1990			1991–2009		
States in regression:	(1) All	(2) Group 1	(3) Groups 2 & 3	(4) All	(5) Group 1	(6) Groups 2 & 3	(7) All	(8) Group 1	(9) Groups 2 & 3
RMSE [a]	0.031	0.032	0.029	0.043	0.045	0.041	0.011	0.013	0.010
Group 1									
Median	4.3	−11.8		14.5	9.3		−31.8	−21.9	
5th percentile [b]	−12.8	−16.3		−10.4	−15.9		−38.1	−29.4	
1st percentile [c]	−19.4	−21.5		−22.4	−27.2		−40.3	−33.0	
Group 3									
Median	0.0		−2.8	4.9		5.7	−31.9		−27.0
5th percentile [b]	−16.5		−18.3	−18.1		−15.9	−38.1		−33.4
1st percentile [c]	−22.8		−22.2	−29.1		−23.7	−40.3		−36.3

Calculations based on data from FHFA (formerly OFHEO), BLS, and U.S. Treasury. Estimated regression coefficients, a key component of this simulation, are presented in Table 2. Note, projections are not made for greyed-out areas. Such projections, if they were made, would apply to states that were excluded from the underlying regression.
a. The standard (or root mean square) error of the regression.
b. Based on the model and underlying assumptions, a drop of this magnitude is projected to occur once every 20 years.
c. Based on the model and underlying assumptions, a drop of this magnitude is projected to occur once every 100 years.

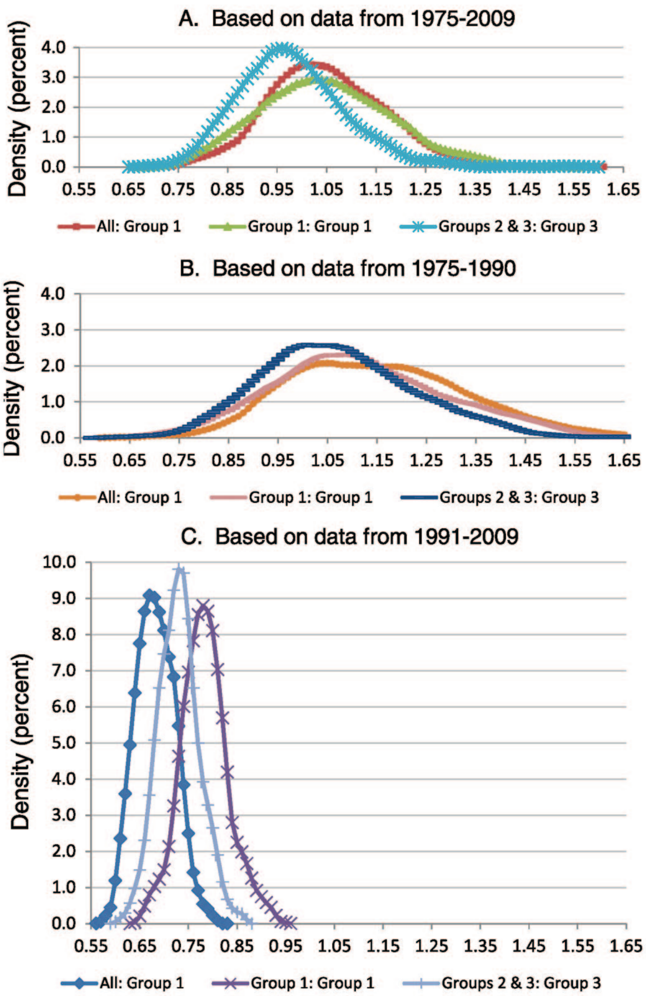

Figure 3. Simulated housing price distributions. (A) Based on data from 1975 to 2009. (B) Based on data from 1975 to 1990. (C) Based on data from 1991 to 2009. *Source*: Authors' calculations simulated housing price distributions in this figure. For each legend entry, the group(s) listed before the colon refers to the states included in the regression used for the simulation. After the colon, the group of states that the projections apply to is listed. Three-year cumulative house price changes are normalized so that 1.00 implies zero real growth in house prices.

exceeding 30%.[23] While the magnitude of the decline in house prices in recent years (at the state level) is unusual, what is more unusual appears to be the correlation in housing price changes across states. For example, the 12 quarter price decline for group 1 states is more than double anything this group experienced prior to the Great Recession. Prior to 2006, large price drops in one state (or region) were partially offset by stronger housing markets in other parts of the country—this time that was not true.

While the severity of a one-percent scenario has increased substantially in the latter period, the projected housing price distribution does not widen. In fact, density plots in Figure 3 show that the reverse is true—i.e., the price distribution is much tighter post-1990. This is driven to a large degree by the much smaller RMSE for the post-1990 period. As noted earlier, the RMSE increases by roughly a factor of four when moving from post-1991 to pre-1990. (Recall that the standard deviation of the stochastic term used in the simulation equals the regression RMSE.) The difference between the median and onepercent scenario was between 34 and 37 percentage points when using pre-1991 regressions. Post-1990, this difference falls to between 8 and 11 percentage points. Thus, despite a tighter price distribution, the severity of a one percent scenario increases substantially post-1991. In fact, the median price change from the post-1990 projections is a price *decline* of between 22% and 32%! Pre-1991, the median of the projected price changes ranges from increases of 5–15%. The tightening of the post-1990 price distribution is also observed when comparing the oneand five-percent scenarios. Pre-1991, one-percent scenarios are projected to be 8–12 percentage points worse than five-percent scenarios. Post-1990, this difference drops to between two and four percentage points.

The evidence suggests a structural break and the simulations based on post-1990 data look very different from those for the earlier period. This suggests that a new housing price stress test may be in order. However, the simulation results based on post-1990 data are peculiar—and do not seem sensible. For example, it does not seem reasonable expect housing prices to fall substantially (i.e., the median scenario) and for the uncertainty about future price changes to be much smaller than in the past.[24] One alternative would be to construct a stress scenario using all of the years of data. But, at the cost of simplicity, adjust the path based on several factors, such as the location of the housing, creditworthiness of borrowers and short-term trends designed to capture momentum within the market. What is clear from the simulations is that OFHEO's ALMO stress test appears quite weak compared to any of our alternatives.

4.3. Additional sensitivity checks

Table 5 presents a number of alternative 12-quarter price declines for one- and five-percent scenarios. These alternatives test the robustness of our core results. Recall that the simulation results presented in Table 4 were generated assuming a benign employment and interest rate regime. In some instances these checks support substantially more severe stress scenario.

One of the sensitivity checks replaces the benign employment scenario with a severe one. The severe employment scenario is based on the worst 16 quarter experience of any state present in the HPI data. In this case, the employment path is that experienced by Michigan between

23. These numbers are based on the HPI (beginning in 1975) and exclude post-2001 data. Extending the data through quarter 3 of 2009 increases the number of states that have experienced declines of 20% or more to 32; and, the number that have experienced declines of 30% or more to 11.

24. Given that these simulated distributions are generated using a benign economic starting scenario, the model certainly does not seem to perform well here. It may be that the regression specification, while reduced form and not restrictive in many of its assumptions, is not flexible enough to accurately models this later period. Alternative regression specifications could allow for nonlinear relationships or allow estimated coefficients to vary depending on whether house prices are rising or falling.

Table 5. Sensitivity checks to simulation results cumulative percent change in housing prices after 3 years (with stochastic shocks and random year fixed effects).

Years:	1975–2009			1975–1990			1991–2009		
States in regression:	(1) All	(2) Group 1	(3) Groups 2 & 3	(4) All	(5) Group 1	(6) Groups 2 & 3	(7) All	(8) Group 1	(9) Groups 2 & 3
Group 1 – severe employment scenario									
5th percentile [a]	–27.8	–28.9		–21.7	–38.1		–38.4	–53.2	
1st percentile [b]	–33.5	–33.4		–34.5	–46.6		–40.3	–57.5	
Group 1 – 1 standard deviation shock to T10									
5th percentile [a]	–28.3	–19.7		–21.6	–30.3		–67.5	–54.3	
1st percentile [b]	–33.6	–26.9		–27.6	–35.8		–69.1	–57.0	
Group 1 – employment weighted									
5th percentile [a]	–8.9	–10.1		–7.7	–24.7		–39.8	–32.4	
1st percentile [b]	–15.0	–17.4		–15.0	–30.8		–41.4	–36.1	
Group 1 – constant RMSE									
5th percentile [a]	–12.8	–15.6		–2.0	–24.1		–33.9	–52.7	
1st percentile [b]	–19.4	–20.6		–13.7	–31.4		–38.1	–57.6	
Groups 2 and 3 – severe employment scenario									
5th percentile [a]	–30.9		–30.8	–28.4		–31.9	–38.5		–33.4
1st percentile [b]	–36.3		–34.2	–40.2		–39.1	–40.4		–35.9
Groups 2 and 3 – 1 standard deviation shock to T10									
5th percentile [a]	–30.6		–32.2	–28.3		–23.0	–60.8		–59.5
1st percentile [b]	–35.2		–37.6	–37.4		–30.9	–62.5		–61.2
Group 3 – employment weighted									
5th percentile [a]	–16.5		–19.1	–10.3		–14.4	–33.9		–28.2
1st percentile [b]	–22.8		–23.5	–21.1		–21.3	–38.2		–35.0
Groups 2 and 3 – constant RMSE									
5th percentile [a]	–16.5		–19.1	–10.3		–14.4	–33.9		–28.2
1st percentile [b]	–22.8		–23.5	–21.1		–21.3	–38.2		–35.0

Calculations based on data from FHFA (formerly OFHEO), BLS, and U.S. Treasury. Estimated regression coefficients, a key component of this simulation, are presented in Table 2. Note, projections are not made for greyed-out areas. Such projections, if they were made, would apply to states that were excluded from the underlying regression.

a. Based on the model and underlying assumptions, a drop of this magnitude is projected to occur once every 20 years.

b. Based on the model and underlying assumptions, a drop of this magnitude is projected to occur once every 100 years.

1978 and 1982, when employment fell by a total of 14%. Replacing the benign employment scenario with this severe one yields much greater price declines. For specifications using all years of data or pre-1991 data, this results in one-percent house price drops that are almost 40 to over 70% larger than the analogous results (presented in Table 4). Thus, instead of the 19–23% decline in house prices under the benign employment scenario (and including all years of data), one-percent price declines are now nearly one-third, or greater. Again, see Table 5.

A second sensitivity check raises the ten-year Treasury rate by one standard deviation (2.77 percentage points) for the entire 12 quarter projection, but leaves the yield spread unchanged. This is intended to examine the implication of a jump in mortgage interest rates on severe stress scenarios. For the specifications including all years of data, this interest rate shock results in one-percent price declines that are 25–73% larger—i.e., price declines of between 27% and 38%.

A third check performs the same Monte Carlo exercise used for Table 4, but replaces the estimated regression coefficients with ones that are employment-weighted. See Table 3, which compares lagged coefficients from the employment-weighted specification to those from the unweighted approach. Employment-weighting adjusts for the fact that states differ tremendously in size. Without employment-weighting, the experience of Wyoming, for example, is given equal weight as that of California or Texas. For the post-1990 period, income-weighting has little affect on the simulation results. For the other timeperiods, however, house price declines during one-percent scenarios are roughly 30% smaller than without employment weighting. Thus, while the adverse employment scenario suggest a more severe stress scenario than those in Table 4, employment weighting lends support for a less severe scenario.

Finally, the simulation is performed assuming a constant RMSE. Recall that for each quarterly projection a random component is added with mean 0 and a standard deviation equal to that from the respective regression. As noted earlier, the RMSE is much smaller for the post-1990 period. Here, the RMSE from the specification that includes all of the data is always used for the standard deviation on the stochastic term. For the full period, this has little impact, since these specification all have RMSEs that are (equal to or) very close to the new constant value. For the two sub-periods, the results are mixed. In some instances, one-percent scenarios are similar to the analogous cases from Table 4. In one instance (projections for group 1 states based on post-1990 data), the estimate is 74% larger. In this instance, the RMSE used is 2.5 times the size of the one associated with the estimated coefficients used in the simulation. As noted earlier, many of the post-1990 projections are peculiar. In this instance, even the 99th percentile suggests a substantial price decline. At the other extreme, the specification that includes all states and pre-1991 data finds the price decline at the first percentile that is much more modest—ranging from 14% to 21%. In this case, the constant RMSE assumption reduces the standard deviation of the stochastic term by more than 40%.

5. Conclusions: Putting it all together

We investigate whether statistical models estimated at different time periods and for different groups of states would lead to a clear conclusion about whether a new housing price stress test—replacing OFHEO's ALMO scenario—seems appropriate. Regression analysis finds that estimated parameters are quite sensitive to the period and groups of states used to estimate the model. There seem to be two offsetting effects at work. The models based upon earlier data show greater sensitivity to employment growth and the level of interest rates and, also, suggest a higher constant rate of growth in real house prices than models estimated using data from later years. These all tend to lead to lower stress tests and lower credit costs, all else equal. Offsetting these patterns is the fact that the RMSE for the earlier models is larger than that estimated in later years. Also, the parameters from the model estimated using more recent data suggest substantial momentum from recent house price trends, which is consistent with bubble phenomena and greater potential for extreme price volatility. More momentum implies less mean reversion. This is true throughout the post-1990 period, but is more pronounced for group 1 (high price volatility) states.

Thus, whatever conclusions we draw rest upon some judgments about the particular scenarios and assumptions used in the simulation analysis that generates stress scenarios (and which could then be used to estimate credit costs). The models estimated since the 1990s generate more stressful scenarios than ones based upon earlier data. That is, the 5th or 1st percentile outcomes are much worse than previous models would have suggested. In terms of the Katrina example, the more recent estimates suggest that the 100 year flood is worse than previously thought. However, the simulated housing price distributions using only post-1990 data are peculiar: the distribution has a much smaller variance than for the other time-periods and the distributions suggest almost no prospect for real price growth. For example, these results sometimes suggest that even 99th percentile scenario (i.e., an extremely strong housing market) sometimes shows price declines. While insights (with respect to momentum etc.) can be gleaned from the post-1990 analysis, simulations based on this period alone likely do not represent sensible scenarios.

However, when using all years of data (or even only pre-1991 data), it still appears that a new housing price stress test is desired. Even with benign assumptions with respect to employment growth and interest rates, simulations suggest that the ALMO test is too weak. Incorporating more severe employment or interest rate environments makes the case for a new, more severe, stress test even stronger. On balance, the sensitivity checks point toward a more severe stress scenario than what use for our core scenario. Although, the employment-weighted regression results weaken this conclusion.

From the numerous results generated via a relatively simple and single equation model of house prices, several additional conclusions can be drawn:

1. During normal economic circumstances, the results based upon the full sample seem plausible. That is, a 3 year decline in real prices of about 20% (Table 4 column 1) seems appropriate for a 1% degree of security and about 15% for a 5% degree of security.
2. As concerns about a recession rise, which was surely the case at the end of 2006, our sensitivity analysis suggests a stress test scenario with decline of more than 40% (see Table 5 and the 2 standard deviation shock to employment). This is quite similar to the recent FRB stress test when one accounts for 2006 and 2007 growth rates leading up to the numbers they project for the following two years, 2009 and 2010.
3. Our results are based upon real price changes. Nominal shocks would be larger during inflationary times and should be adjusted accordingly. See Figure 1, which shows that the ALMO scenario was a weaker test in the years leading up to the Great Recession (than it was during the 1980s) because it was defined in nominal terms. According to the recent FRB minutes, the stress scenario ought to be increased by 3%, which is the midpoint of their forecasts of inflation over the next three years.[25]
4. The correlation between house prices across states increased in recent years. This contributed to the financial crisis, but whether this pattern will continue is not known. The distribution of house prices for Group 1 states is wider with a higher expected value than that for Group 2 and 3 states. This was not the case pre-1991 when the distribution of prices for Group 1 states was much more skewed to the left. Part of this ambiguity probably stems from the use of state data. Our guess is that variations among markets will be more clear-cut if MSA data are used. Absent stronger evidence, we suggest that the regulators give much more attention to this issue going forward and prepare stronger stress tests for different parts of the country as circumstances suggest.

The results also suggest two other changes to the ALMO process. First, a stress test ought to be continually updated to incorporate new information. The resulting stress test will vary as new data and market conditions arise, as the parameterization of the house price process change, and as sensitivities to changes other economic variables evolve. Second, a stress test should be stated in real and not nominal terms. If ALMO had been applied in real terms, perhaps additional capital would have been held and the consequences of the Great Recession (of 2007–2009) would have been less onerous. Stress testing is an ongoing process that adjusts to changing market conditions including inflation. Legislating a stress test scenario stated in nominal terms that holds for an extended period seems to lack justification.

Evidence supporting substantial variations in the stress test scenario across groups of states is ambiguous. In fact, there is some evidence of increased correlation among states. This is issue is ripe for additional research to ascertain whether this increased correlation is an aberration or something expected to continue. If it continue, then the added value from geographical variations to the stress test may be small; however, such increased correlation would surely imply a reduction in the benefits from geographically diversifying portfolios, and would substantially lower the benefits from geographic diversification cited by Calem and Follain (2003).

One topic of ongoing debate is whether regulatory capital levels and, in essence, the stress test ought to vary over the business cycle (BIS, 2010). For example, more stressful scenarios may be appropriate during the boom part of a cycle in order to provide above average amounts of capital. Similarly, a less stringent stress test might be more appropriate during a recessionary period in order not to diminish the time to recovery. The Basel III plan is an example of a countercylical capital policy. Our results do not address this issue since the basic set of initial economic conditions for our simulations are the same. An extension of our work could be developed that would generate simulations for different sets of starting conditions.

More generally, we readily acknowledge that these judgments are based upon our reading of these results and many others that we have considered. The simulation exercise is complex (even with a relatively simple underlying econometric model) and there is little doubt that some analysts might reach different judgments; however, it is surely the case that the process and resulting picture could be made much more complex. For example, consider the thorny issue of regional diversification, not addressed here. Further consider potential variations in the stress test scenarios based upon initial conditions prior to the stress; or variations in interest rate conditions—which until the recent crisis was considered by many experts the most serious threat to the mortgage market. All of these would increase complexity, further cloud transparency and heighten the need for subjective judgments by those responsible for defining a stress test scenario. On the other hand, this paper underscores the contention that stress test scenarios are inherently complex and should not be written in stone by legislation. Rather, a team of independent analysts ought to be constantly in search of whether a change in the status quo is needed. They should be expected to make their best case, but the ultimate decision-makers should harbor no illusions that the evidence underlying the final recommendations of an extreme event will be crystal clear. We are confident of this much.

25. See Table 1 in http://www.federalreserve.gov/monetarypolicy/files/fomcminutes20100127.pdf .

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