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Financial integration, housing, and economic volatility ☆

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ABSTRACT

The Great Recession illustrates the sensitivity of the economy to housing. This paper shows that financial integration, fostered by securitization and nationwide branching, amplified the positive effect of housing price shocks on the economy during the 1994–2006 period. We exploit variation in credit supply subsidies across local markets from government-sponsored enterprises to measure housing price changes unrelated to fundamentals. Using this instrument, we find that house price shocks spur economic growth. The effect is larger in localities more financially integrated, through both secondary loan market and bank branch networks. Financial integration thus raised the effect of collateral shocks on local economies, increasing economic volatility.

1. Introduction

The recent Great Recession, many argue, had its origins in the boom and bust in housing and the knock-on effects of the resulting financial crisis (Brunnermeier, 2008). The length and depth of the recession is partly attributed to the slow recovery from the housing bust and the associated consumer-debt overhang (Mian and Sufi, 2014).

This paper shows that financial integration amplified the boom and bust housing cycle by strengthening the spillover from the housing market to the rest of the economy. As shown theoretically in Morgan, Rime, and Strahan (2004), financial integration can smooth credit supply disturbances, but it can amplify the effect of collateral booms (housing booms). Increases in collateral values alleviate contracting problems between borrowers and lenders (Holmstrom and Tirole, 1997). With financial integration, credit can flow into regions with collateral (housing) booms from areas without (or with smaller) booms. Hence, booming regions experience stronger increases in growth (fueled by credit), while nonbooming regions export capital, which slows down their growth. Thus, financial integration ought to amplify the effects of housing shocks on real economic activity and lead to divergence in economic growth across areas.

We test these implications using panel data at the Central Business Statistical Area (CBSA) level. We first show that financial integration strengthened the effect of housing shocks on several broad economic outcomes (income, employment, and total output). We then show that housing shocks in external markets connected financially reduce local economic outcomes, consistent with capital flowing toward areas with stronger housing

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markets and away from those with weaker housing markets.

In our first core test, we evaluate whether shocks to house prices during the 1994 to 2006 period stimulate the local economy and whether financial integration amplifies this stimulus. For identification, we exploit subsidies in financing from the government-sponsored enterprises (GSEs) Federal National Mortgage Association (Fannie Mae) and Federal Home Loan Mortgage Corporation (Freddie Mac). Fannie and Freddie subsidize mortgages, but only those below the jumbo-loan threshold. The threshold is exogenous to individual CBSA economic conditions, as it depends mechanically on past changes in nationwide housing prices. The jumbo-loan threshold matters because borrowers below the threshold enjoy more abundant and cheaper credit. Borrowers close to the threshold are funding-constrained and respond to a rise in the threshold by increasing their housing demand (Adelino, Schoar, and Severino, 2011).

While the jumbo-loan cutoff changes uniformly across all markets, its effects vary across CBSAs. For example, in Los Angeles (where about 5.3% of loan applicants fall within 5% of the jumbo-loan cutoff) the change in the cutoff would have a bigger impact than in Wichita, Kansas (where this fraction is only 0.5%). We build a set of instruments to trace out how an exogenous shock to financial constraints (a change in the jumbo-loan cutoff) affects local housing prices, incorporating interactions related to both demand- and supply-side frictions. CBSAs with more borrowers around the cutoff (more constrained borrowers) should experience slower housing price appreciation than other markets. As the constraint is relaxed (by increasing the cutoff), constrained borrowers enjoy wider access to GSE subsidies. Hence, demand for housing ought to increase more in markets with more borrowers near the cutoff. However, if housing supply is elastic, this increase in demand would have a smaller effect on prices. Following this intuition, we use three instruments: the share of applicants within 5% of the cutoff in year $t-1$, its interaction with the change in the jumbo-loan cutoff from year $t-1$ to t, and a triple interaction with the measure of housing supply elasticity (Saiz, 2010). We find our instruments do explain housing price growth within CBSAs, in line with this intuition.

Armed with exogenous variation stemming from the jumbo-loan cutoff movement, we show that housing prices have a strong causal impact on growth in local employment, personal income, and output. In our base model, a 1% increase in housing prices increases local growth by about 0.34% and increases nonconstruction, nonfinance employment growth by about 0.22%, suggesting that higher prices spill over to sectors not directly related to housing.

We then evaluate the core issue of this paper, which is whether the effect of house price shocks on real economic output strengthens with financial integration. We employ two measures of financial integration to address this issue. The first captures integration stemming from the growth of secondary market for mortgages; the second, the extension of bank branch networks across markets. In local areas 1 standard deviation above the mean of financial integration, a 1% housing price shock raises local growth by about 0.43%, but the effect is insignificant at 1 standard deviation below the mean of financial integration. Our results imply that financial integration increased local economic volatility by amplifying the effect of collateral values (house prices) on the overall economy.

In our final set of tests, we provide micro-foundations for the effects of financial integration. We show that individual lenders reallocate funds across their markets toward those with stronger housing price appreciation and away from weaker areas (controlling for economic fundamentals). We alleviate reverse causality by controlling for lender-year effects and local economic conditions, and we show that the results are similar in magnitude when we limit the sample to lenders whose market share is too small to plausibly move local housing prices. The estimated elasticity of credit growth with respect to housing price growth of about 1.7 is large enough to generate spillovers and, thus, fuel a general economic boom. We then show a negative relation between local outcomes and external housing shocks in financially connected markets, suggesting that capital outflows lead to growth divergence between integrated markets.

Our paper contributes to three strands of literature. First, many argue that the Great Recession has its root in the crash of housing prices beginning in the middle of 2006. Our results support this explanation but also suggest that the pre-crash economic boom was itself fueled by house price appreciation. The findings extend Mian and Sufi (2011), who show that households financed consumption with housing wealth during the boom. Moreover, large-firm investment (Chaney, Sraer, and Thesmar, 2011) and self-employment and employment in small firms (Corradin and Popov, 2012; and Adelino, Schoar, and Severino, 2014) increase with housing wealth. Chakraporty, Goldstein, and Macinlay (2013) find that local business lending declines when banks reallocate capital toward areas with housing booms. Unlike these studies, however, we go a step further and estimate the total effect of housing price shocks on the economy and we condition this estimate on aspects of the financial system. We document that shocks to housing have had a large effect on the overall economy, especially in markets that are well integrated nationally.

Second, the effect of financial integration on economic volatility and business cycle synchronization has been explored both across US states and in the context of liberalization of international capital markets (e.g., Peek and Rosengren, 2000; Morgan, Rime, and Strahan, 2004; Demyanyk, Ostergaard, and Sorenson, 2007; Kalemli-Ozcan, Papaioannou, and Peydró, 2013; Imai and Takarabe, 2011; Cetorelli and Goldberg, 2012). Most of these studies explore settings where capital supply shocks dominate and, hence, document that integration can increase synchronization and smooth business cycles. We find that integration can amplify shocks and de-sychronize asset markets in an environment of strong credit demand and a stable, profitable financial sector.

Third, conventional explanations for the US housing boom blame loose lending practices as the key driver of price appreciation (e.g., Mian and Sufi, 2009; Demyanyk and Van Hemert, 2009; Keys, Mukherjee, Seru, and Vig, 2010; Loutskina and Strahan, 2011). Yet these studies do little to explain why booms were concentrated in places such as Arizona, California, and Florida. Financial integration can help rationalize large regional booms by allowing capital to flow into areas with strong credit demand.

The paper proceeds as follows. In Section 2, we develop our key hypotheses and then briefly review the forces leading to more financial integration. In Section 3, we describe our integration measures and estimate the relation between shocks to housing prices and local growth. Here, we establish a first-stage model that relates changes in credit supply subsidies from the GSEs to house price appreciation. We then estimate the causal impact of housing price growth on the economy as a whole and test how that impact varies with financial integration. To support our interpretation of the main result, we show in Section 4 that banks do reallocate funds to booming markets and that, as a result, local economic growth becomes negatively correlated with external housing booms (that is, in markets connected through financial ties). Section 5 concludes the paper.

2. Financial integration

This section describes how financial integration affects capital flows across local markets and briefly describes forces leading to greater integration over time.

2.1. How financial integration affects local economies

Morgan, Rime, and Strahan (2004) show theoretically that financial integration can amplify or dampen local cycles, depending on the source of shocks. Integration dampens the effect of credit supply shocks by allowing such shocks to be shared across local areas.[1] During the 1980s and early 1990s, credit shocks were a major source of business cycle instability (Bernanke and Lown, 1991). The number of bank and savings and loan (S&L) failures during the 1980s, for example, averaged more than 150 per year, and the collapse of the S&L industry amplified downturns in areas such as California and Texas. Integration makes local economies less sensitive to these supply-side financial disturbances because capital can flow in from external sources and, thus, allow investment to continue, even if local lenders are distressed. Empirically, state-level banking integration fostered by deregulation during the 1970s and 1980s lowered volatility of state economies in these years.

Conversely, by allowing credit to flow between markets, financial integration can amplify the effect of local booms. For example, if collateral values rise in a locality, borrower debt capacity and demand for credit increase. Integration helps bring financial resources from abroad to satisfy the higher credit demand. The influx of credit raises growth above what would have been possible in a stand-alone, or dis-integrated, financial system.

In this paper, we study 1994 to 2006, a time when the financial sector was profitable in the US. Mian and Sufi (2011) document that local housing booms stimulated consumption by allowing homeowners to borrow against rising home equity values during these years. We argue that financial integration strengthened this effect by allowing credit to flow in from other markets. Because bank failures were all but nonexistent during these years, financial integration both amplified (instead of dampened) local cycles during this sample period and led to negative spillovers across markets. For example, consider two CBSA markets—A and B—that are well integrated financially. A shock to housing prices in market A draws financial resources away from B, thus accommodating the credit demand and raising output in A. All else equal, the positive shock in A lowers output in B, because capital there becomes scarce. Thus, the economy in A ought to be positively stimulated by its own housing price shocks (by increasing debt capacity and drawing in finance from abroad). Conversely, B is negatively affected by A's boom in housing. Thus, capital flows both raise the effect of collateral shocks (i.e. housing booms) on the local economy and create negative spillover effects in connected markets.[2]

Based on these ideas, we test the following three implications of financial integration. First, financial integration ought to amplify the positive effect of local collateral shocks (housing shocks) on credit demand and growth. Second, integration ought to increase the extent to which capital flows into booming markets and away from busting (or less booming) ones. Third, integration ought to generate a negative association between growth and collateral shocks in other markets connected financially, because external booms draw capital away and thus reduce growth. We test the first hypothesis in Section 3, and the second and third ones in Section 4.

2.2. Increasing financial integration

Two forces have contributed to increasing financial integration over time: the advent of securitization and deregulation of branch banking. The move toward integration in mortgage lending began in the 1970s with the activities of the GSEs, Fannie Mae and Freddie Mac. Together they have played the dominant role in fostering securitization and the mortgage secondary market. As shown by Frame and White (2005), the GSEs combined market share has grown rapidly since the early 1980s. By 1990, about 25% of the \$2.9 trillion in outstanding

[1] Ashcraft (2005), Becker (2007), and Gilje (2012), for example, show that the supply of local bank finance affects investment.

[2] Beyond its effects on capital flows, integration is associated with lower investment by lenders in private information about local business conditions, borrower credit quality, and housing price fundamentals. With financial integration, residential mortgage credit supply becomes more responsive to changes in the market value of collateral than in the past. Securitization forces lenders to condition their credit decisions more on public signals (e.g., borrower FICO scores and loan-to-value ratios) that they can pass to the secondary market and less on private or soft information (Rajan, Seru, and Vig, forthcoming). The geographic diversification of bank operations lowers the value of soft information collection and leads loan officers to condition their lending decisions more on hard information (Loutskina and Strahan, 2011). We argue that these two forces (more flighty capital and more reliance on public information) lead to increased collateral volatility and raise the sensitivity of local cycles to variation in collateral values.

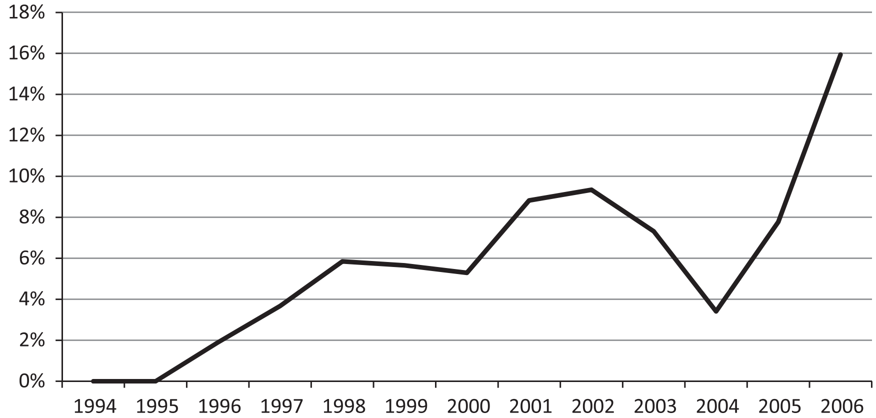

Fig. 1. Year-by-year percentage change in the jumbo-loan cutoff over time. This figure reports the annual percentage change in the size of a single-family mortgage that can be sold to the Federal National Mortgage Association (Fannie Mae) or the Federal Home Loan Mortgage Corporation (Freddie Mac) from 1994 to 2006.

mortgages were either purchased and held or purchased and securitized by the two major GSEs. By 2003, this market share had increased to 47%. GSE access to implicit (now explicit) government support allows them to borrow at rates below those available to private banks and to offer credit guarantees on better terms than competitors without such implicit support.

Loutskina and Strahan (2009) further show that the GSEs enhance mortgage liquidity, reduce the cost of borrowing, and increase mortgage acceptance rates conditional on borrower credit quality. The GSEs operate under a special charter, however, that limits the size and risk of mortgages that they may purchase or securitize. These limitations were designed to ensure that the GSEs meet the legislative goal of promoting access to mortgage credit for low- and moderate-income households. In 2006, for example, the GSEs could purchase only mortgages below $417,000. The loan limit increases each year by the percentage change in the national average of single-family median housing prices during the prior year, based on a survey of major lenders by the Federal Housing Finance Board (Fig. 1).[3] Because the loan limit changes mechanically as a function of past national housing prices, local housing supply or demand conditions are fully exogenous to the jumbo loan cutoff. We exploit this fact in developing our instrument for housing price growth.

Securitization helped integrate markets for low-risk, prime mortgages but has had less impact on markets for small business loans and high-risk mortgages or jumbo mortgages, which are much more likely to remain in the portfolio of local lenders (Loutskina and Strahan, 2011). The gradual development of nationwide branch banking worked in parallel with securitization to enhance and complete the process of financial integration for more information-sensitive forms of credit. Much of the branch expansion occurred as states relaxed restrictions on branching within states during the 1970s and 1980s and in response to interstate banking and branching fostered by passage of the Interstate Banking and Branching Efficiency Act of 1994 (IBBEA). With these legal changes, banks now may operate across many states and localities. Such cross-ownership ties allow deposits collected in one market to finance information-intensive (i.e., nonsecuritizable) loans in another. Expanding banks' internal capital markets via interstate banking fosters integration and capital flows because such funds are less costly than relying on external finance (Houston, James, and Marcus, 1997). Gilje, Loutskina, and Strahan (2013) show that branch networks remain important in allowing funds collected in one locality to finance lending in another. They find that an exogenous positive liquidity shock in some branches propagates to other markets, expanding credit for hard-to-securitize mortgages but only in markets connected by branch networks.

Even with interstate banking reform (IBBEA), states continue to exercise some authority to restrict or limit interstate entry. For example, states that oppose entry by out-of-state banks could use provisions of IBBEA to erect barriers to some forms of out-of-state entry, to raise the cost of entry, and to distort the means of entry. IBBEA allows states to employ various means to erect these barriers. States may set regulations on interstate branching with regard to four important provisions: (1) the minimum age of the target institution, (2) whether or not to permit de novo interstate branching, (3) whether or not to permit acquisition of individual branches instead of whole banks, and (4) how tightly to control the percentage of deposits in insured depository institutions controlled by any single bank or bank holding company. We exploit the heterogeneity of branch restrictions across states to build a policy instrument for our deposit-based measure of financial integration.

3. Housing prices and economic growth

This section addresses two questions. First, did housing price shocks stimulate the local economy during the boom? Second, did financial integration strengthen the stimulus from housing price shocks to overall economic performance? To answer these questions, we need to trace out the causal impact of shocks to local housing prices on

[3] The limit is 50% higher in Alaska and Hawaii.

overall economic output by CBSA-year ($Y_{i,t}$). Specifically, we estimate instrumental variable (IV) panel regressions with the following structure:

$$Y_{i,t} = \alpha_t + \gamma_i + \beta_1 \text{ House Price Growth}_{i,t} + \text{Control Variables} + \varepsilon_{i,t} \quad (1)$$

and

$$\begin{aligned} Y_{i,t} = {} & \alpha_t + \gamma_i + \beta_1 \textit{ House Price Growth}_{i,t} \\ & + \beta_2 \textit{ Financial Integration}_{i,t} \\ & + \beta_3 \textit{ Financial Integration}_{i,t} \times \textit{House Price Growth}_{i,t} \\ & + \textit{Control Variables} + \varepsilon_{i,t}. \end{aligned} \quad (2)$$

If changes in housing prices raise borrower debt capacity and, in turn, raise consumer demand and firm investment, then $\beta_1 > 0$ in Eq. (1). If financial integration, by allowing capital to flow in from external markets, strengthens this effect, then $\beta_3 > 0$ in Eq. (2).

We use the annual percentage change in the Federal Housing Finance Association's CBSA-level housing price index to measure house price growth. We evaluate four measures of local real economic growth: (1) personal income growth as reported by the Bureau of Economic Analysis (BEA), (2) employment growth and (3) employment growth without sectors directly affected by housing (construction and finance) as reported by the Bureau of Labor Statistics (BLS), and (4) gross domestic product (GDP) growth reported by Moody's Analytics.[4] We estimate Eqs. (1) and (2) for a CBSA-year panel data set with both year and CBSA fixed effects.

Table 1 reports summary statistics for housing price growth and for personal income, employment, and GDP growth for our sample. The sample begins in 1994 because the deposit-based measure of financial integration becomes available post-1994. We end the analysis in 2006 for two reasons. First, we do not want our estimates to be driven by the financial crisis and the ensuing Great Recession. Second, our identification strategy relies on the consistent and mechanical increase in the jumbo-loan cutoff over time. After the onset of the financial crisis, however, this cutoff was raised aggressively in high-priced markets to support housing prices. The level of the cutoff has been maintained across other markets even as housing prices have dropped sharply. The instrumental variables are thus both less powerful after 2006 and becoming potentially endogenously related to local fundamentals.

3.1. GSE housing-finance subsidies as instruments for housing price growth

Shocks to the overall economy both affect and are affected by the value of real estate and collateral, in general, and the value of housing, in particular. Our aim is to trace out the causal impact of shocks to housing on the overall economy. Hence, we need instruments that move housing prices (are sufficiently powerful) but otherwise remain unrelated to fundamental drivers of local economic growth (meet the exclusion restriction for valid instruments). We use subsidies in housing finance from the GSEs to build such instruments.[5] Potential home buyers receive a financing subsidy through the activities of the GSEs, which stand ready to buy mortgages that fall below the jumbo-loan cutoff and meet a set of credit-worthiness underwriting criteria. The cutoff is binding on borrowers, as is evident from the histogram of loan applications and loan approval rates presented in Fig. 2 (adapted from Loutskina and Strahan, 2009). The large spike in loan applications and approval rates just below the jumbo cutoff indicates that the funding is both more abundant and cheaper below the jumbo-loan cutoff. The cutoff is the same everywhere (except Alaska and Hawaii), and it increases annually based on a mechanical formula linked to past changes in national housing prices. The increase in the jumbo-loan cutoff thus raises the subsidy to some potential home buyers, but the increase, crucially, is not dependent on conditions in a local area (CBSA).

In our identification strategy, we exploit the idea that the effect of this increased subsidy varies across local housing markets. Markets with substantial demand near the jumbo-loan cutoff should see homebuyers benefiting greatly when the cutoff rises. In contrast, in markets where most home prices fall below the jumbo-loan cutoff in $t-1$, home buyers would receive no incremental benefit from an increase, as all potential homebuyers are already subsidized. We capture this local market sensitivity to jumbo cutoff movements using the fraction of borrowers with constrained access to GSE subsidies.

In building our instruments, we utilize the detailed data on mortgage applications collected annually under the Home Mortgage Disclosure Act (HMDA). Whether a lender is covered depends on its size, the extent of its activity in a CBSA, and the weight of residential mortgage lending in its portfolio. Our sample covers a broad set of loans originated in the US economy from 1994 to 2006. The HMDA data include loan size, whether or not a loan was accepted, some information on borrower credit characteristics, and the location of the property down to the ZIP code level.[6]

To measure credit constraints, we build an instrument equal to the fraction of applications in a CBSA within 5% of the jumbo-loan cutoff. Because changes in home prices or credit demand could affect this fraction, we use last year's mortgage applications distribution, not the contemporaneous distribution of approved loans. By including borrowers just below the cutoff, we capture the idea that home buyers often split their borrowing into a senior (nonjumbo) mortgage to gain the subsidy and finance the remainder with a second-lien mortgage from a portfolio lender (i.e., a lender who holds the mortgage) plus equity. Thus, many mortgage applicants below, but not too far below, the jumbo-loan cutoff would also benefit from its increase via gaining access to cheaper credit.

[4] The CBSA-year-level GDP estimates are also available from BEA, but only starting in 2001. We cross-reference the Moody's Analytics data with BEA and find the correlation of 98.7% between two data series.

[5] Adelino, Schoar, and Severino (2011) use a similar strategy at the transaction level to trace out how GSE subsidies affect the price per square foot of housing.

[6] See the Internet Appendix for a detailed description of the sample selection procedure.

Table 1
Summary statistics for economic outcomes, financial integration and instruments for housing price growth.

This table reports summary statistics for housing price growth, four measures of local economic growth, and two instruments built reflecting the distribution of mortgage credit around the jumbo-mortgage cutoff. We also report summary statistics for our two measures of financial integration. *Deposit-Based Integration* equals the fraction of deposits in a Central Business Statistical Area (CBSA) year owned by banking companies with deposits in other markets. *Market-Based Integration* equals the fraction of loan applications taken by all lenders that fully rely on the external sources of funds (non-depository lenders plus depositories lending in CBSAs without a branch presence). GDP=Gross Domestic Product.

Variables	Mean	Standard deviation
CBSA-level Economic Outcomes		
Housing Price Growth	5.41%	4.63%
Personal Income Growth	5.21%	2.55%
Employment Growth	1.46%	2.39%
Employment Growth, without Construction and Finance	1.14%	2.62%
GDP Growth	5.39%	3.04%
Instruments		
Share of New NJ Borrowers	0.357%	0.788%
Share Around Cutoff	1.650%	2.042%
Share Around Cutoff × *Change in Cutoff*	0.092%	0.145%
Saiz Elasticity	2.595	1.422
Financial Integration Measures		
Deposit-Based Integration	81.4%	15.3%
Market-Based Integration	59.7%	15.2%

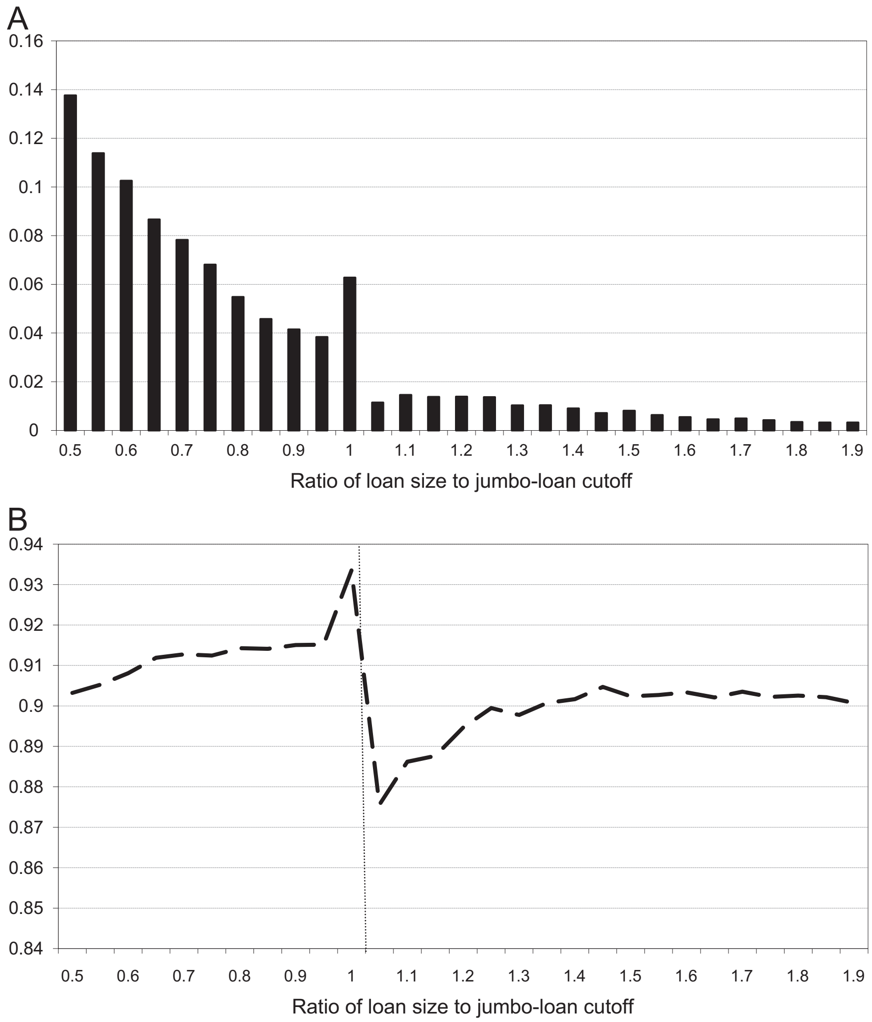

Fig. 2. Loan applications and share of approved loan applications, 1994–2006. (A) Histogram of loan applications and (B) share of approved loan applications.

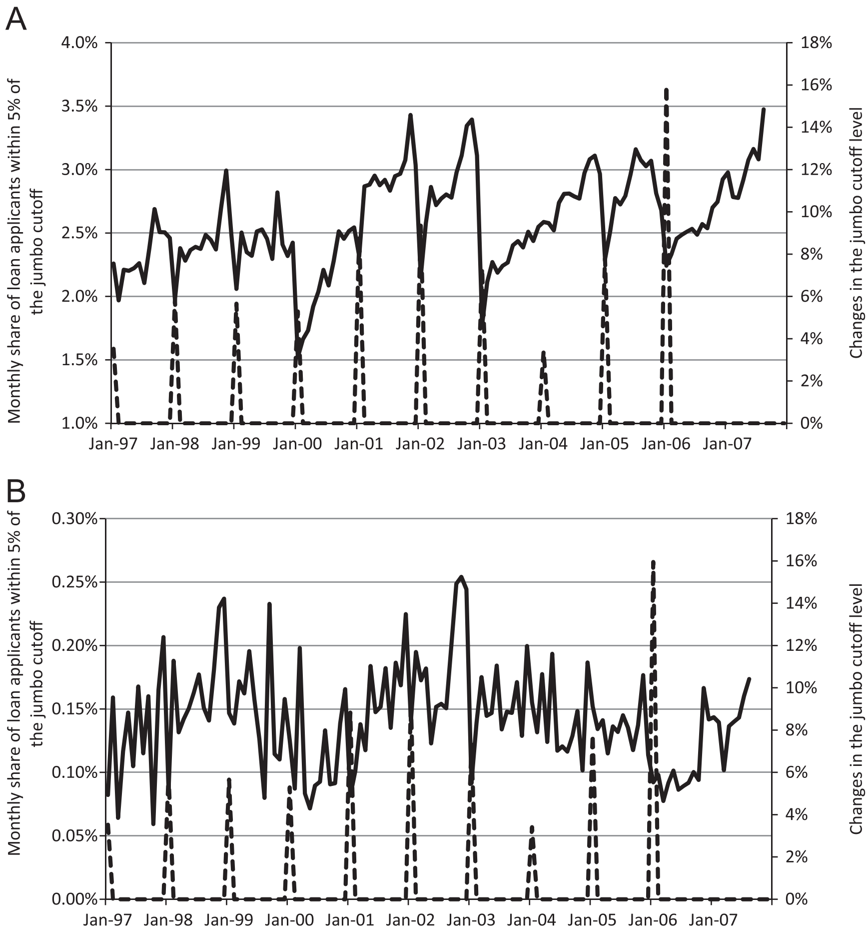

Fig. 3. Monthly share of borrowers within 5% of the jumbo-loan cutoff and change in jumbo cutoff. The solid line (left-hand-side scale) depicts the average monthly share of loan applicants within 5% of the jumbo-loan cutoff for Central Business Statistical Areas (CBSAs) in top (Panel A) and bottom (Panel B) quartile of average *Share Around Cutoff*. The dashed line illustrates the changes in the jumbo cutoff level (right-hand-side scale). (A) CBSAs in top quartile of average Share Around Cutoff. (B) CBSAs in bottom quartile of average Share Around Cutoff.

Fig. 3 illustrates the sensitivity of areas with high and low fractions of borrowers around the cutoff to movements in the jumbo cutoff. The figure presents monthly variation in the fraction of borrowers within 5% of the jumbo-loan cutoff.[7] In high-priced markets [such as Los Angeles (LA), California], the share of borrowers around the cutoff rises during the year as housing prices rise, then falls sharply when the cutoff increases in January. Bigger increases in the cutoff lead to bigger drops in the share of applicants around the cutoff, suggesting a large drop in credit constraints and wider access to GSE subsidies. In low-priced markets (such as Wichita, Kansas), there is no obvious link with changes in the cutoff. This evidence supports the premise underlying our instruments: borrowers with applications around the cutoff are credit-constrained and relaxation of the constraint affects a significant mass of borrowers in some markets (such as LA) but not in others (such as Wichita).

So, we use three variables in our instrumentation strategy. First, to capture the effect of credit constraints, we use the share of borrowers around the jumbo cutoff in year $t-1$. Second, we interact this variable with the percentage change in the cutoff between $t-1$ and t to capture the idea that exogenous shocks to the cutoff matter more where constraints are tighter. Third, we capture housing supply-side frictions with a time-invariant measure of physical impediments to expansion in the housing stock, such as waterways, mountains, etc. (Saiz, 2010).[8] Housing demand shocks ought to affect

[7] The aggregated proprietary monthly HMDA data were generously provided to us by the Federal Reserve Bank of Cleveland. In the confidential release of HMDA, mortgage applications are time stamped, whereas in the public release only the application year is disclosed. Hence, we are not able to use the variation within the year for our main tests.

[8] Saiz shows that cities with high supply elasticity have both slower increases in housing prices over time and faster population growth, compared with low-elasticity cities. These results make sense because low barriers to the expansion of housing implies that increased demand from population growth can be accommodated without increasing the cost of housing (e.g., land is not scarce in these areas). We use the elasticity estimates available online at http://real.wharton.upenn.edu/~saiz/ and then convert them to the new definitions of CBSA using the ZIP code overlap. This measure is available for 253 CBSA markets.

prices more where supply elasticities are low, so we interact the elasticity measure with our instrument for shocks to credit demand.

The first-stage model thus takes the following forms:

$$\begin{aligned}
&\textit{House Price Growth}_{i,t} = \alpha_t^{HP} + \gamma_i^{HP} + \textit{Other control variables}\\
&\quad + \beta_1^{HP}\ \textit{Share Around Cutoff}_{i,t-1}\\
&\quad + \beta_2^{HP}\ \textit{Share Around Cutoff}_{i,t-1} \times \textit{Change in Cutoff}_t\\
&\quad + \beta_3^{HP}\ \textit{Share Around Cutoff}_{i,t-1} \times \textit{Change in Cutoff}_t\\
&\quad \times \textit{Saiz Elasticity}_i + \varepsilon_{i,t},
\end{aligned} \tag{3}$$

where *Share Around Cutoff*$_{i,t-1}$ equals the share of applications within $\pm 5\%$ of the cutoff in year $t-1$, and *Change in Cutoff*$_t$ equals the percentage change in the cutoff between t and $t-1$. The direct effect of the *Saiz Elasticity*$_i$ (*Change in Cutoff*$_t$) is absorbed by the CBSA (year) effects.[9]

Because *Share Around Cutoff* captures the tightness of credit constraints, markets with more borrowers around the cutoff ought to experience slower housing price appreciation. So, we expect $\beta_1^{HP} < 0$. Markets with more constrained borrowers, in turn, ought to respond more to an increase in the jumbo loan cutoff, so $\beta_2^{HP} > 0$. However, if housing supply is elastic, the increase in demand from previously constrained borrowers would lead to only more houses being built, not housing price appreciation, so $\beta_3^{HP} < 0$.

Fig. 4 illustrates our identification strategy for two extreme cases: a local market where most of the demand for housing is already subsidized by the GSEs and with very high supply elasticity (e.g., Wichita, where supply elasticity equals 5.5 and only 0.5% of total mortgage applications lie within 5 percentage points of the jumbo-loan cutoff) versus a market with a large mass of demand near the jumbo-loan cutoff and with low supply elasticity (e.g., Los Angeles, where supply elasticity equals 0.63 and about 5.4% of total mortgage applications lie within 5 percentage points of the jumbo-loan cutoff). An increase in the GSE jumbo-loan cutoff shifts housing demand only slightly in Wichita but substantially in Los Angeles. Because supply responds elastically in Wichita, prices barely rise. In LA, however, prices rise sharply, both because demand shifts further from the increased subsidy and because supply responds very little. Thus, we trace a shock in a supply of funding to the housing price changes accounting for both CBSA-specific demand shifts and the CBSA-specific supply conditions.

For robustness, we implement a second approach to estimating the sensitivity of local housing demand to changes in the jumbo cutoff. We compute the percentage

[9] Chaney, Sraer, and Thesmar (2011) use a somewhat different instrument to identify housing demand shocks based on the interaction of interest rates with the Saiz elasticity. This approach would not pass the exclusion restriction in our setting because we look at local outcomes (as opposed to investment by public firms, which do not depend solely on local factors). Interest rates affect investment demand across all types of investment, so they affect local outcomes both by affecting housing demand and prices and by affecting the returns to other types of investment. In contrast, we argue that changes in the jumbo cutoff affect local outcomes only by stimulating demand for housing and raising house prices (collateral). See the Internet Appendix for more discussion of this issue.

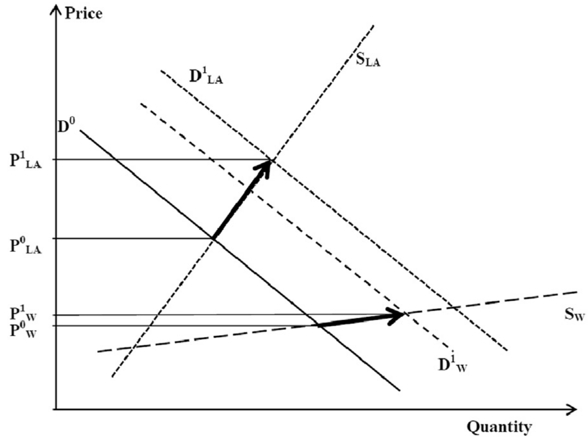

Fig. 4. Responses of different markets to changes in government-sponsored enterprise (GSE) loan cutoff. The graph illustrates the responses of two hypothetical markets to changes in the GSE loan cutoff. The subscript LA represents Los Angeles, California, and subscript W represents Wichita, Kansas. Two markets are characterized by different elasticity of housing supply (S_{LA} and S_W) as well as different shifts in the demand curves caused by the same change in the loan cutoff (D^1_{LA} and D^1_W). The graph illustrates the corresponding changes in the housing prices.

of loan applicants in year $t-1$ that would directly benefit from the change in the cutoff in year t by getting access to more readily available or cheaper credit, or both. Specifically, we estimate the fraction of loan applications in CBSA i that are jumbo (above the cutoff) in year $t-1$, but would fall below that cutoff in year t as a consequence of the increase in the cutoff between the two years (*Share of New NJ Borrowers*$_{i,t}$). This measure depends on borrower distribution in year $t-1$ and the cutoff changes in year t and is conceptually similar to the interaction term between *Share Around Cutoff*$_{i,t-1}$ × *Change in Cutoff*$_t$. *Share of New NJ Borrowers*$_{i,t}$ captures both the exogenous shock to the cutoff and sensitivity of local housing demand to that cutoff.

In these models, the first-stage regression is as follows:

$$\begin{aligned}
&\textit{House Price Growth}_{i,t} = \alpha_t^{HP} + \gamma_i^{HP} + \textit{Other control variables}\\
&\quad + \mu_1^{HP}\ \textit{Share of New NJ Borrowers}_{i,t}\\
&\quad + \mu_1^{HP}\ \textit{Share of New NJ Borrowers}_{i,t} \times \textit{Saiz Elasticity}_i + \varepsilon_{i,t}.
\end{aligned} \tag{4}$$

Similar to Eq. (3), we expect prices to rise faster in markets with a large mass of borrowers to become non-jumbo (so $\mu_1^{HP} > 0$), but less so in high elasticity markets ($\mu_2^{HP} < 0$).

Table 2 reports the first-stage equations [Eqs. (3) and (4)] linking the two sets of instruments to house price appreciation. To help make our identification strategy as transparent as possible, we also report simpler versions of Eqs. (3) and (4) that omit the interaction effects. In addition to the instruments, we control for local industry structure, captured by the share of nine different industries in local employment as reported by the BLS. We also control for the strength and health of the local banking sector: the average (deposit-weighted) capital-asset ratio, the log asset size of banks operating in the CBSA, and the average growth rate of assets of local

Table 2
First-stage regressions: *Housing Price Growth*.

This table reports regressions of housing price growth by Central Business Statistical Area (CBSA) year on two sets of instruments: the share of borrowers in year $t-1$ that will become non-jumbo in year t (share new nonjumbo); and the total fraction of borrowers within $+/-$ 5% of the jumbo-loan cutoff in year $t-1$ times the change in the jumbo loan cutoff between $t-1$ and t and their interactions with the Saiz elasticity of housing supply. All regressions include time fixed effects, along with measures of industry structure and the health of the local banking system. Standard errors reported in parentheses are clustered by CBSA. $^{***}p<0.01$, $^{**}p<0.05$, and $^{*}p<0.1$.

Independent variables	Dependent variable: *Housing Price Growth*				
	(1)	(2)	(3)	(4)	(4)
Share Around Cutoff$_{i,t-1}$	-1.832^{***}	-1.865^{***}	-1.729^{*}		
	(8.12)	(7.38)	(1.86)		
Share Around Cutoff$_{i,t-1}$ × *Change in Cutoff*$_t$		7.186^{***}	12.35^{***}		
		(6.70)	(5.58)		
Share Around Cutoff$_{i,t-1}$ × *Change in Cutoff*$_t$ × *Saiz Elasticity of Housing Supply*$_i$			-5.134^{***}		
			(2.70)		
Share of New NJ Borrowers$_{i,t}$				0.864^{***}	1.975^{**}
				(7.48)	(2.28)
Share of New NJ Borrowers$_{i,t}$ × *Saiz Elasticity of Housing Supply*$_i$					-0.588^{**}
					(2.31)
Saiz Elasticity of Housing Supply$_i$	*Absorbed by CBSA fixed effects*				
CBSA dummies	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes	Yes
Number of observations	3,277	3,277	3,277	3,277	3,277
R^2	0.504	0.512	0.513	0.425	0.425

banks.[10] Finally, we include time and CBSA fixed effects. Thus, we ensure that the results are not affected by systematic differences in the level of housing prices (or price appreciation) across CBSAs. We cluster the standard errors at the level of the CBSA.

We find that both sets of instruments have statistically significant effects on housing price—the direct effect and the interactions. Moreover, the signs and magnitudes of the coefficients are economically sensible, as housing prices grow more slowly in markets with more constrained borrowers. For example, in the simple model (Column 1), *Share Around Cutoff* is negatively associated with subsequent house-price growth, consistent with the idea that this variable proxies for financial constraints. Column 2 shows that relaxing financing constraints, that is, increases in *Change in Cutoff*, matters more to subsequent house price growth in markets that have high levels of financial constraints. To assess economic significance of the whole set of instruments, consider Column 3. At the mean for both the *Change in Cutoff* (5.77%) and *Saiz Elasticity* (2.595), the marginal effect of *Share Around Cutoff* equals -1.79 $(=-1.73+12.35\times 0.0577-5.134\times 0.0577\times 2.595)$. This effect implies that a 1 standard deviation increase in *Share Around Cutoff* (2.04%) leads to a decline in housing price growth of a little less than 4%. Although we cannot identify the direct effect of *Change in Cutoff* (absorbed by the year effects), its marginal effect increases with *Share Around Cutoff* (because these are markets with more constrained borrowers) and decreases with the *Saiz Elasticity* (because prices react less where supply elasticity is high).[11]

3.1.1. Do collateral shocks stimulate the economy?

Armed with plausibly exogenous variation in local housing prices, we evaluate whether a shock to the housing sector propagates to the real economy. Table 3 reports IV estimates linking the exogenous component of housing price growth to economic outcomes Eq. (1). We estimate all models with time and CBSA fixed effects and with time-varying industry share variables (coefficients suppressed) and time-varying measures of banking system characteristics (coefficients suppressed).

Table 3 reports the results for four different measures of output: personal income growth (Columns 1 and 2), total employment growth (Columns 3 and 4), the growth of total employment excluding employment in financial firms and construction (Columns 5 and 6), and GDP growth (Columns 7 and 8). Employment growth without construction and finance allows us to test whether any effects that we observe spill over beyond segments directly tied to housing finance. We implement the two instrumentation strategies described above (Panels A and B). Each model is reported with and without

[10] The bank financial conditions are compiled from Reports of Income and Conditions (Call Reports) and weighted by the amount of deposits held by respective financial institutions in a given CBSA.

[11] One can argue that *Share Around Cutoff* is positively correlated with the level of prices in the cross section. Areas with higher levels of prices (and higher average loan sizes) are likely to have more constrained borrowers that either cannot get access to sufficient credit or have to split their borrowing into a senior conventional mortgage and nonconforming second lien mortgage. We, thus, also estimate our model using the average log of loan size from the prior year as a proxy for the set of financially constrained borrowers. This approach measures financial constraints less precisely than *Share Around Cutoff*, but the results reported in the Internet Appendix are similar to those reported here.

Table 3
Instrumental variable (IV) regressions relating local economic growth to housing price growth.

This table reports IV regressions of four measures of real economic growth on instrumented housing price growth. The data are at Central Business Statistical Area (CBSA) year level. First stage results are reported in Table 2. All regressions include time and CBSA fixed effects, along with measures of industry structure and the health of the local banking system. Standard errors are clustered by CBSA. ***$p<0.01$, **$p<0.05$, and *$p<0.1$. GDP=Gross Domestic Product.

Independent variables	*Personal Income Growth*		*Total Employment Growth*		*Employment Growth w/o Construction or Finance*		*GDP Growth*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Share Around Cutoff, (Share Around Cutoff × Change in Cutoff) and (Share Around Cutoff × Change in Cutoff × Saiz Elasticity) used as identifying instruments.								
House Price Growth	0.460***	0.328***	0.370***	0.299***	0.259**	0.258***	0.373**	0.322***
	(4.52)	(5.23)	(3.65)	(5.71)	(2.51)	(4.41)	(2.56)	(4.24)
Lagged House Price Growth		−0.0982***		−0.0948***		(0.05)		−0.0995**
		(3.23)		(3.52)		(1.62)		(2.52)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,277	3,272	3,277	3,272	3,277	3,272	3,277	3,272
Hansen-*J* statistic	3.58	2.57	4.31	4.81	4.57	3.35	5.77	2.33
Kleibergen-Paap test	16.93***	73.82***	16.93***	73.82***	16.93***	73.82***	16.93***	73.82***
Panel B: Share New NJ Borrowers and (Share new NJ Borrowers × Saiz Elasticity) used as identifying instruments.								
House Price Growth	0.237***	0.178***	0.288***	0.204***	0.180***	0.169***	0.321***	0.232***
	(3.36)	(4.62)	(4.75)	(6.77)	(2.96)	(5.43)	(3.57)	(5.08)
Lagged House Price Growth		−0.030		−0.0511***		−0.011		−0.0580**
		(1.53)		(2.98)		(0.55)		(2.15)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,277	3,272	3,277	3,272	3,277	3,272	3,277	3,272
Hansen-*J* statistic	0.153	0.382	1.746	1.254	2.111	1.983	2.010	1.522
Kleibergen-Paap test	32.45***	156***	32.45***	156***	32.45***	156***	32.45***	156***

controlling for lagged house price growth. By including this lag, we alleviate the concern that the instruments, which depend on last year's distribution of home mortgages, pick up serial correlation in local growth rates.[12]

The coefficient estimates are statistically and economically significant in all specifications, averaging 0.34 for the broad outcomes (income, employment, GDP) and 0.22 for employment growth without finance and construction. An exogenous 1% increase in housing prices thus causes the local economy to expand by about 0.34 percentage points faster than otherwise. If the typical home is financed with 80% debt, a 1% price increase implies a 5% increase in equity. Thus, these coefficients suggest wealth effects around 0.07 (divide the coefficients by five), consistent with the admittedly wide range of estimates in the literature [see Mishkin (2007) for a review]. The coefficients on employment growth without segments directly tied to housing suggest that spillovers from higher collateral values raise output beyond the housing sector. Comparing Panels A and B, we find somewhat larger magnitudes using instruments based on the share of mortgages around the jumbo cutoff. All instrumental variable specifications pass the Kleibergen and Paap (2006) test for weak instruments and the Hansen overidentification test.[13]

Fig. 1 suggests that 1994, 1995, and 2006 are outliers in assessing changes in the jumbo-loan cutoff. In the first two years the cutoff is static, so these observations contribute little to our estimates because the instruments are zero then. In 2006, the cutoff rose by 16%. In the other years, its change ranged from 2% to 9%. We estimate our models without 1994–1995 and find similar results to those reported here, as these observations only contribute to pinning down the CBSA effects. We also estimate these models separately for the early (1994–2000) and late (2001–2006) portions of our sample. In these tests, we find that housing is positively and statistically significantly related to economic outcomes in both samples, with somewhat larger magnitudes in the earlier half of the sample.

3.1.2. Falsification tests of instrumentation strategy

One concern could be that the sensitivity of housing prices to the jumbo cutoff or the mass of borrowers around the cutoff, or both, might be spurious. To address this

[12] The *Share Around Cutoff* also varies over time within CBSA. Although we use the lag of this variable and built it based on loan applications instead of approved loans, time variation could arguably be related to credit demand that may exhibit serial correlation. To test whether such variation biases our models, we re-estimated all the results of Tables 3 and 4 after replacing *Share Around Cutoff*$_{i,t-1}$ with the time series average of this variable for each CBSA. In this setting, the *Change in Cutoff*$_t$ provides all of the within-CBSA variation over time, which, as we have argued, is not related to local fundamentals. The results are reported in the Internet Appendix and are similar to those reported here.

[13] Both tests account for clustering at the CBSA level.

concern, we build simulations based on pseudo-instruments, as in Bertrand, Duflo, and Mullainathan (2004) and Heider and Ljungqvist (2013).

Our actual instruments embed three separate sources of variation: (1) CBSA-year variation in the mass of borrowers within 5 percentage points of the jumbo cutoff; (2) time variation in the jumbo-loan cutoff; (3) cross-sectional variation in the Saiz elasticity measure. To validate the instruments, we run a total of five thousand simulations to test how well pseudo-instruments predict home price changes. In each simulation, we replace each dimension of the instruments (one at a time) with a random draw from the empirical distribution of that dimension of the variable. For example, in one set of simulations we draw randomly from the empirical distribution of *Share Around Cutoff* (with replacement), while leaving the other two parts of the instruments (*Change in Cutoff* and *Saiz Elasticity*) at their actual values. We estimate similar simulations for both the *Change in Cutoff* (a time series variable) and the *Saiz Elasticity* (a cross-sectional variable), drawing randomly from the empirical distributions of each (with replacement) while preserving the structure of the underlying panel.

The results (available in the Internet Appendix) show that all three dimensions of variation are important for identification. Out of five thousand simulations, we find only one instance in which coefficients from the first-stage regressions have similar sign and as much statistical significance as those using the actual instruments (reported in Table 2). Given that each simulation is based on two of the three parts of the actual instruments, we do observe explanatory power in some cases. For example, when we simulate only the *Change in Cutoff*, the direct effect of *Share Around Cutoff* is negative and statistically significant at the 5% level in about 87% of the simulations. This negative coefficient of the nonsimulated regressor reflects the slower housing price appreciation in markets with more financially constrained borrowers. When we simulate the *Saiz Elasticity*, we get some explanatory power from *Share Around Cutoff* and its interaction with the *Change in Cutoff*, but the effect of the interactions with *Saiz Elasticity* loses significance. Overall the simulations indicate that our instruments get power from all three sources of variation, so the relations are unlikely to be spurious.

3.2. Financial integration and the economic effects of housing shocks

To test how financial integration affects links from house price shocks (or, more generally, collateral shocks) to the economy, we interact *House Price Growth* with two measures of financial integration. These measures capture the two sources of financial integration discussed in Section 2: (1) the extension of bank branch networks, which allows reallocation of funds across markets within lenders; and, (2) the growth of mortgage secondary markets, which redistributes capital supply across local markets between lenders (and other investors).

3.2.1. Measuring financial integration

We build the *Deposit-Based Integration* measure from the distribution and ownership of bank branches and deposits across local markets. The measure uses information on total deposits, location, and ownership of all bank branches from the Federal Deposit Insurance Corporation's *Summary of Deposits*, available online annually from 1994 forward. *Deposit-Based Integration* equals the fraction of all deposits in a CBSA owned by a holding company that also owns deposits in one or more other CBSAs.[14]

Variation in *Deposit-Based Integration* depends on bank entry decisions into market i in year t, which in turn may reflect risk management or strategic motivations of potential entrants. Because economic or housing conditions of a particular market may play a role in this entry decision, the relations observed in Eq. (2) could be biased by reverse causality. If banks tend to enter overheated markets, for instance, then β_3 may be biased upward in an ordinary least squares setting. To eliminate this source of potential bias, we use instrument-based state-level restrictions on interstate branching. Following Rice and Strahan (2010), we build a simple index of interstate branching restrictions that exhibits variation both across states and over time. The index starts at four, its most restrictive value, for all states in 1994. The index then falls depending on each state's implementation of potential means to restrict entry. To understand the instrument, consider Illinois, which adopted interstate branching in 1997 but set a minimum age of 5 years for acquisitions, did not permit de novo branching by out-of-state banks, and did not permit single-branch purchases. In 2004, however, Illinois relaxed its policies across each of these three dimensions. Thus, for Illinois we set the branching index at four for 1994–1996, reduce the index to three in 1997–2004; and reduce it further to zero in 2005–2006. The resulting index ranges from zero to four, where four represents the highest level of barriers to entry by out-of-state banks. The regulatory index has both cross-state and over time variation, and it is strongly correlated with *Deposit-Based Integration*, controlling for year and CBSA-level fixed effects (as well as the other covariates in our model).

Branching integrates markets within an intermediary, whereas securitization integrates external markets across intermediaries. To capture the importance of this second dimension of integration, we look at the share of mortgage lenders funding their investments exclusively through external markets. For this measure, we use data from the HMDA database. The *Market-Based Integration* measure equals the fraction of loan applications in a CBSA i at time $t-1$ taken by lenders that rely solely on external capital markets to fund their loan originations, which includes all nondepository institutions (e.g., mortgage companies) and depository institutions without a branch presence in a given CBSA. This measure is highly correlated with the fraction of loans that are securitized or sold in a market ($\rho = 0.84$). While we do not have a clean policy instrument

[14] We define a banking company as the highest entity within a bank holding company for banks owned by holding companies or for the bank itself for stand-alone banks.

Table 4
Instrumental variable (IV) regressions relating local economic growth to housing price growth with financial integration interactions.

This table reports IV regressions of real economic growth on housing price growth, two measures of financial integration and their interactions. *Deposit-Based Integration* equals the fraction of deposits in a Central Business Statistical Area (CBSA) year owned by bank holding companies with deposits in other markets. *Market-Based Integration* equals the lagged fraction of loan applications taken by lenders that are fully external finance dependent (nondepository lenders plus depository institutions lending in CBSAs without a branch presence). House price growth and house price growth times financial integration are treated as endogenous variables. House price growth is instrumented following Table 3, Panel A. The interaction of house price growth and the deposit-based integration is instrumented via the branching restrictions index interacted with the Home Mortgage Disclosure Act based house price instruments. The market-based interaction term is instrumented using its value interacted with the HMDA based house-price instruments. The unit of observation is CBSA-year between 1994 and 2006. All regressions include time and CBSA fixed effects, along with measures of industry structure and the health of the local banking system. Standard errors are clustered by CBSA. $^{***}p<0.01$, $^{**}p<0.05$, and $^{*}p<0.1$. GDP=Gross Domestic Product.

Independent variables	*Personal Income Growth* (1)	*Total Employment Growth* (2)	*Employment Growth without Construction or Finance* (3)	*GDP Growth* (4)
Panel A: Financial integration based on deposit connections				
(i) House Price Growth	−0.55	−0.93	−0.88	−1.34
	(0.60)	(1.26)	(1.16)	(1.10)
(ii) House Price Growth × Deposit-Based Integration	0.993**	1.366**	1.251**	1.864**
	(1.99)	(2.17)	(2.42)	(2.32)
Deposit-Based Integration	−0.04	−0.05	−0.05	−0.08
	(0.82)	(1.29)	(1.15)	(1.27)
Time fixed effects	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes
Number of observations	3,277	3,277	3,277	3,277
Hansen-*J* statistic	5.06	5.64	4.58	3.56
Kleibergen-Paap test	16.95***	16.95***	16.95***	16.95***
Anderson-Rubin Chi^2 test that (i) and (ii) are jointly significant	26.06***	26.79***	13.89***	12.77***
Panel B: Financial integration based on market connections				
(i) House Price Growth	−1.091**	−0.43	−0.44	−0.31
	(2.39)	(1.27)	(1.23)	(0.73)
(ii) House Price Growth × Market-Based Integration	2.128***	1.126**	1.076**	1.003**
	(3.10)	(2.18)	(1.97)	(1.99)
Market-Based Integration	−0.0831***	−0.0397*	−0.0447*	(0.03)
	(2.65)	(1.66)	(1.81)	(0.88)
Time fixed effects	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes
Number of observations	3,136	3,136	3,136	3,136
Hansen-*J* statistic	1.417	3.636	1.567	1.795
Kleibergen-Paap test	37.09***	37.09***	37.09***	37.09***
Anderson-Rubin Chi^2 test that (i) and (ii) are jointly significant	20.66***	21.35***	16.00***	16.57***

for this dimension of integration, we believe that the measure is relatively exogenous as we use applications from the prior year, not contemporaneous mortgage origination or securitization decisions, and we use the number instead of dollar volume of applications. Thus, we eliminate any mechanical relations between *Market-Based Integration* measure and economic outcomes.

3.2.2. Results

Table 4 reports the results when we add the interactions between housing price growth and our two measures of financial integration [Eq. (2)]. We model both *House Price Growth* and its interaction with *Financial Integration* as jointly endogenous to alleviate reverse causality, because economic booms tend to raise both. *House Price Growth* is instrumented using the *Share Around Cutoff*$_{i,t-1}$, *Share-Around Cutoff*$_{i,t-1}$ × *Change in Cutoff*$_t$, and its interaction with *Saiz Elasticity* [as in Eq. (3)].

Panel A reports the results based on *Deposit-Based Integration* to measure financial integration. In these models, the interaction term between financial integration and housing price growth is instrumented with the *Branching Restriction Index* and its interactions with the core set of housing price growth instruments. Panel B reports the model using *Market-Based Integration*. In these models, we instrument the interaction term using the *Market-Based Integration* and its interactions with the core set of housing price growth instruments.

The results suggest that house price shocks have a greater impact on economic outcomes in financially integrated markets. We pass the Kleibergen and Paap (2006) test for weak instruments and fail to reject the overidentification restrictions. Across all specifications, housing price growth and its interaction with financial integration are jointly significant at better than 1%. Moreover, the interaction terms are consistently positive and significant across all models, meaning that better integration amplifies the causal impact of housing price shocks on economic outcomes. While the direct effect of house price growth is negative (though not significant in most models), its total effect on outcomes is significant and positive across all models at the mean level of financial integration, with an average effect of 0.22%.[15] In markets 1 standard deviation above average in financial integration, a 1% increase in housing price growth increases growth in the broad outcomes by 0.43% (based on averaging results across the models). For markets 1 standard deviation below the mean level of financial integration, the effect of housing prices on outcomes is near zero.

These results support our main message: Housing stimulates economic outcomes, and financial integration strengthens this effect. Because credit supply can respond more elastically to increases in collateral values when local markets are better integrated, an increase in housing prices generates a larger positive spillover. We next test explicitly whether higher demand for credit from housing booms draws financial resources from connected localities.

4. Micro-foundations: financial integration, capital flows and growth divergence

In this section we provide two additional tests that integration allows capital to flow to areas with high demand for housing finance.

4.1. Within-lender capital flows

We argue that capital flows explain why financial integration amplifies the impact of house-price shocks on the overall economy during the boom. To test this mechanism directly, we estimate whether lenders move capital in response to collateral appreciation, irrespective of local economic conditions. We build a lender-CBSA-year-level panel data set on mortgage originations based on the HMDA data. All types of lenders are included in the analysis, depository and nondepository institutions alike. We use the following regression specification to evaluate whether financial intermediaries lend relatively more in areas where they face relatively higher increases in housing prices:

$$\begin{aligned} \textit{Loan Growth}_{i,t}^{k} = {} & \beta\, \textit{House Price Growth}_{i,t} \\ & + \theta' \textit{Local Economic Conditions}_{i,t} \\ & + \gamma_{k,t} + \alpha_i + \textit{Applicant Characteristics}_{i,t}^{k} + \varepsilon_{i,t}^{k}, \end{aligned} \tag{5}$$

where $\textit{Loan Growth}_{i,t}^{k}$ equals the growth in mortgage originations from year $t-1$ to year t in CBSA i by lender k.

Eq. (5) evaluates how a lender reallocates its mortgage business across its markets in response to increases in housing prices. The lender-year fixed effects ($\gamma_{k,t}$) difference out each lender's average mortgage growth in a given year. Consequently, the remaining variation represents the lender's reallocation of resources across the markets in which it operates in response to changes in local fundamentals including housing price changes. Because lenders operate in different sets of markets, the same market may be hot for some lenders (i.e., above mean within a lender-year) and cold for others. The lender-year fixed effects also strip out variation in lender access to funds, which vary only by lender-year as long as access to deposits or capital market funds can be shared across markets served by a given lender.[16] Because we want to isolate the effect of collateral on capital flows, we control for a host of local fundamentals: CBSA fixed effects, personal income growth, employment growth, GDP growth, population density, and the industry structure captured by labor shares. We also control for the characteristics of the mortgage applicant pool a lender faces.

The coefficient β in Eq. (5) is an elasticity, so a coefficient above one implies that banks expand lending more than in proportion to the increase in collateral values. Thus, an elasticity above one implies that the credit supply flows in sufficient quantity into booming markets to support economic spillovers from housing to other parts of the economy. Because the shocks vary only by CBSA, we cluster the standard errors at the CBSA level.

Table 5 shows that lenders move capital into booming housing markets and away from less-booming markets. Panel A reports the effect of a change in housing prices on lending growth. To identify whether a lender moves funds both away from internally cold market and toward hot ones, we test whether the effects of collateral differ for hot versus cold markets, where a hot market is one with above-median growth within a lender-year (Panel B).

Our empirical setup alleviates concern about reverse causality stemming from mortgage growth leading to housing price appreciation. We consider only within lender-year variation, so coefficients are estimated from variation in housing price growth across markets served by the same lender. The same market could therefore be classified as hot for some lenders and cold for others. Also, because the mortgage business is highly competitive, no individual lender is likely to have sufficient market power to move housing prices in a locality.[17]

To systematically support these claims, we estimate the model using only small lenders (those with less than 1% market share in a given CBSA), whose credit decisions are unlikely to drive prices. We also estimate the model without CBSA-years in the upper and lower quartiles of

[15] In these models, the effects of housing price shocks at the mean level of financial integration are smaller than the estimates in the simpler models of Table 3. This difference presumably reflects a small bias in the simpler setup that results from omitting the effect of financial integration.

[16] This assumption is consistent with evidence that multi-bank holding companies reallocate deposits and cash flow across subsidiaries (e.g., Houston, James, and Marcus, 1997; Campello, 2002).

[17] In robustness tests we estimate Eq. (5) using the same instrumentation strategy for home prices used earlier. The results (reported in the Internet Appendix) show somewhat higher magnitude and similar statistical significance to those reported in Table 5. However, we are not confident that the instruments used for house price growth (those based on movements in the jumbo threshold) are not appropriate in modeling variation in mortgage lending because said instruments could fail the exclusion restriction. The Internet Appendix presents a discussion of this argument.

Table 5
Reallocation of mortgage credit across markets.

This table reports how lenders reallocate mortgage credit across markets in response to relative housing price appreciation in Central Business Statistical Areas (CBSAs). The dependent variable equals the growth in mortgage credit extended by lender-CBSA-year. The independent variable equals the growth in housing prices. We control for local economic fundamentals. All regressions include a full set of lender *year fixed effects, local industry structure characteristics, and lender specific average applicant characteristics. Standard errors are clustered at the CBSA level. ***$p<0.01$, **$p<0.05$, and *$p<0.1$. GDP=Gross Domestic Product.

Independent variables	All lenders (1)	Lenders with < 1% market share in CBSA (2)	Second and third quartiles of house price growth (3)	Lenders with ≥ 10 applications in a CBSA (4)
Panel A: Response to a change in house prices				
House-Price Growth	1.745***	1.772***	1.574***	1.781***
t-stat, Coefficient=0	(22.26)	(21.23)	(6.50)	(23.67)
t-stat, Coefficient=1	(9.51)	(9.25)	(2.37)	(10.38)
Local GDP Growth	−0.107	−0.0949	0.223	−0.212*
	(0.96)	(0.79)	(1.20)	(1.72)
Local Personal Income Growth	−0.153	−0.189	−0.00331	0.00847
	(0.80)	(0.92)	(0.02)	(0.04)
Local Employment Growth	0.546***	0.545***	0.0868	0.631***
	(3.37)	(3.10)	(0.39)	(3.88)
Log CBSA Population	−0.07	−0.0838	−0.1240	0.0143
	(0.85)	(0.91)	(0.88)	(0.16)
Applicant pool controls	Yes	Yes	Yes	Yes
Local industry structure controls	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes
Bank × year fixed effects	Yes	Yes	Yes	Yes
Number of observations	559,984	484,514	272,014	256,793
R^2	39.2%	38.3%	41.9%	57.3%
Panel B: Response to house prices, high- versus low-growth markets				
House-Price Growth	1.638***	1.663***	1.238***	1.729***
t-stat for coefficient=0	(15.57)	(14.97)	(4.13)	(15.73)
t-stat for coefficient=1	(6.07)	(5.97)	(0.79)	(6.63)
House-Price Growth if < Median	0.298**	0.321**	0.39	0.183*
	(2.37)	(2.42)	(1.35)	(1.95)
Indicator, if *House Price Growth* < Median	−0.0206**	−0.0219**	−0.0270*	−0.011
	(2.35)	(2.28)	(1.65)	(1.12)
Local GDP Growth	−0.111	−0.0992	0.226	−0.217*
	(0.99)	(0.82)	(1.22)	(1.75)
Local Personal Income Growth	−0.159	−0.196	−0.00264	0.00276
	(0.84)	(0.96)	(0.01)	(0.01)
Local Employment Growth	0.543***	0.542***	0.0876	0.632***
	(3.38)	(3.11)	(0.39)	(3.91)
Log CBSA Population	−0.06	−0.0741	−0.1250	0.019
	(0.73)	(0.80)	(0.89)	(0.22)
Applicant pool controls	Yes	Yes	Yes	Yes
Local industry structure controls	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes
Bank × year fixed effects	Yes	Yes	Yes	Yes
Number of observations	559,984	484,514	272,014	256,793
R^2	39.2%	38.4%	41.9%	57.3%

housing price growth distribution. This filter removes both markets that are likely to be hot for most lenders (those in the upper 25th percentile) and markets that are likely to be cold for most lenders (those in the lower 25th percentile). Thus, we are left with markets that are hot for one set of banks and cold but not booming (or busting) on average. We also report a robustness test that uses only those observations in which lenders have significant market presence by dropping all lenders with fewer than ten loans originated in both years t and $t-1$.

We find that the results are robust to changes in the set of lenders and markets included in the sample. In Panel A, the coefficients range from 1.57 to 1.78, meaning that 1% relative growth in housing prices leads to 1.6%–1.8% more funds flowing to that market within a financial institution. The coefficient is statistically greater than one in all four specifications. In Panel B, we find a greater effect of capital moving away from cold markets [elasticity=1.984=(1.663+0.321), Column 2] compared with capital moving into hot markets (elasticity=1.663). Thus, not only do banks move funds into booming markets, but they also remove funds from busting markets. The results confirm that capital flows from lower growth markets toward higher growth markets. These flows of capital should lead to divergence in broad economic outcomes relative

Table 6
Instrumental variable (IV) regressions relating local economic growth to own and external shocks to house prices.

This table reports IV regressions of measures of real economic growth on instrumented housing price growth within the given Central Business Statistical Area (CBSA) year, plus the growth of housing prices in external markets connected by deposit (branch) networks (Panel A) or by mortgage-lending connections (Panel B). The external house price growth equals the average growth in all other markets, weighted by the fraction of commonly owned branches from the prior year (deposit weighted) or by the fraction of commonly owned mortgage loan applications from the prior year. We instrument for house price growth, using the instruments that correspond to those in Table 4. All regressions include time and CBSA fixed effects, along with measures of industry structure and the health of the local banking system. Standard errors are clustered by CBSA. ***$p<0.01$, **$p<0.05$, and *$p<0.1$. GDP=Gross Domestic Product.

Independent variables	*Personal Income Growth*		*Total Employment Growth*		*Employment Growth without Construction or Finance*		*GDP Growth*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Financial integration based on deposit connections								
House Price Growth	0.450***	−0.265	0.396***	−0.723	0.292***	−0.776	0.394***	−1.060
	(6.48)	(0.32)	(5.70)	(1.09)	(4.16)	(1.12)	(4.37)	(0.97)
External House Price Growth (deposit weighted)	−0.381***	−0.268***	−0.295***	−0.200***	−0.136	−0.088	−0.316***	−0.253**
	(4.388)	(3.27)	(3.40)	(2.62)	(1.54)	(1.00)	(2.80)	(2.37)
House Price Growth × Deposit Based Integration		0.688*		1.155		1.143		1.559
		(1.73)		(1.52)		(1.43)		(1.55)
Deposit-Based Integration		−0.026		−0.041		−0.042		−0.067
		(0.60)		(1.16)		(1.12)		(1.18)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,264	3,264	3,264	3,264	3,264	3,264	3,264	3,264
Hansen-*J* statistic	3.30	7.04	1.77	9.64	2.41	7.90	4.61	2.92
Kleibergen-Paap test	51.32***	16.31***	51.32***	16.31***	51.32***	16.31***	51.32***	16.31***
Panel B: Financial integration based on market connections								
House Price Growth	0.462***	−1.103**	0.374***	−0.404	0.262***	−0.421	0.377***	−0.285
	(5.63)	(2.31)	(4.73)	(1.15)	(3.31)	(1.13)	(3.64)	(0.64)
External House Price Growth (loan weighted)	−0.215***	−0.017	−0.123**	−0.203	−0.171*	−0.104	0.0152	−0.140
	(3.397)	(0.03)	(2.03)	(1.27)	(1.69)	(1.62)	(0.57)	(1.63)
House Price Growth × Market-Based Integration		2.145***		1.083**		1.048*		0.965
		(3.02)		(2.06)		(1.88)		(1.50)
Market-Based Integration		−0.0839**		−0.039		−0.0441*		−0.025
		(2.57)		(1.60)		(1.73)		(0.81)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry structure	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Banking sector controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CBSA fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,264	3,123	3,264	3,123	3,264	3,123	3,264	3,123
Hansen-*J* statistic	6.59	1.41	9.59	3.67	8.04	1.61	8.89	1.82
Kleibergen-Paap test	33.38***	31.62***	33.38***	31.62***	33.38***	31.62***	33.38***	31.62***

to what one would observe in the absence of financial integration.

4.2. External housing shocks and growth divergence

Financially integrated lenders move mortgage credit toward high-demand markets and away from low-demand markets. We argue that this movement of capital tends to lead to divergence in growth rates across financially connected markets. In our final model, we offer a direct test of this implication.

To test this hypothesis, we augment the models reported in Tables 3 and 4 [recall Eqs. (1) and (2)] with a weighted-average measure of external housing price shocks, where weights correspond to a given local market's degree of financial integration with every other local market. We build the external housing price shock in two ways. First, we compute the weighted-average housing price growth rate for market i from all other markets ($j\sim=i$), using weights equal to the relative fraction of commonly owned deposits between market i and market j. In this calculation, markets that have greater financial connections contribute more to the computation of the weighted average. Hence, variation in the external shocks depends on both the average level of housing price growth in other markets and the degree of deposit connectedness to this market. In our second measure, we compute a similar weighted average, with weights equal to the share of commonly originated mortgage loans by lenders operating in both markets.

Thus, we report regressions with the following structure:

$$Y_{i,t}=\alpha_t+\gamma_i+\beta_1\,House\ Price\ Growth_{i,t} +\gamma\,External\ House\ Price\ Growth_{i,t} +Control\ Variables+\varepsilon_{i,t} \qquad (6)$$

and

$$Y_{i,t} = \alpha_t + \gamma_i + \beta_1 \, House\ Price\ Growth_{i,t} + \gamma \, External\ House\ Price\ Growth_{i,t}$$
$$\beta_2 \, Financial\ Integration_{i,t} + \beta_3 \, Financial\ Integration_{i,t} * Own\ House\ Price\ Growth_{i,t}$$
$$+ Control\ Variables + \varepsilon_{i,t}. \quad (7)$$

These regressions are equivalent to Eqs. (1) and (2) with the addition of one variable, *External House Price Growth*. If capital flows toward booming areas and away from busting ones, then the new variable ought to enter with a negative coefficient ($\gamma < 0$). As before, we control for time and CBSA effects, as well as time-varying covariates controlling for industry mix and the health of the local banking system. We instrument for the own house price growth rate and its interaction with financial integration as in Table 4, but we treat the external housing price shock as exogenous.

Table 6 reports the results. These regressions are similar to those reported in Tables 3 and 4. Panel A contains models using deposits to measuring financial integration (*Deposit-Based Integration*), which correspond to Panel A of Table 3 and Panel A of Table 4. Panel B contains models using integration measures based on lending and the importance of market-based lenders (*Market-Based Integration*), which correspond to Panel A of Table 3 and Panel B of Table 4.

In the first four models across the two panels (Columns 1–4 in Panels A and B), introducing the external house price shock has little impact on the effect of *House Price Growth* on outcomes (compared with the coefficients in Table 3). At the same time, the effect of the *External House Price Growth* enters the equation negatively in most specifications and with statistical significance at the 10% level or better in six of eight. In the models with financial integration (Columns 5 through 8 in Panels A and B), we also find consistent signs and magnitudes, with the coefficient on *External House Price Growth* always negative and the interaction between financial integration and *House Price Growth* always positive (and with magnitudes similar to those from Table 4). Across all 16 of the models, the external effect has a coefficient that averages about −0.18, suggesting that when housing prices in external markets grow 1% faster, the own market economy grows about 0.18% slower. The effect of the external house price shocks on the economy is uniformly smaller than the own-market effect of housing prices, which makes sense because frictions in both external markets (e.g., via securitization) and within a bank's own internal capital market (e.g., via branch connections) slow the movements of capital between markets. These results further confirm the mechanism underlying how integration affects the propagation of collateral shocks.

5. Conclusion

Financial integration tends to be thought of as a mechanism to smooth shocks, and previous studies suggest that integration does reduce the effect of financial disturbances on economic outcomes. In this paper, however, we show that financial integration amplified local economic shocks stemming from housing booms. We demonstrate a causal link from housing to the overall local economy, using variation in the impact of credit supply subsidies from the GSEs to construct an instrument for housing price changes that is unrelated to economic conditions. Our estimates suggest that a 1% rise in housing prices increase growth by about 0.3%. The effect of the own house-price shock is larger in localities that are better integrated, and external house price shocks in financially connected markets negatively affect local economic outcomes. The results suggest that during the housing boom financial integration raised the effect of collateral shocks on local economies by allowing capital to flow to high-growth areas, thereby increasing economic volatility and growth divergence.

During the 2000s, housing prices not only increased, but they also became more volatile and less correlated across local markets. The housing boom was concentrated in Sun Belt areas such as Arizona, California, Florida, and Nevada. These areas also experienced broad economic booms. The existing literature does not explain this regional variation, but our study suggests that better financial integration played a role by allowing rapid capital flows. The normative implication of increased volatility and growth divergence that may come with financial integration depends on the source of credit-demand shocks. If shocks originate from rational assessments about local fundamentals, then financial integration unambiguously improves efficiency by allowing capital to flow toward its highest return. Thus, integration would improve investment and lead to higher long-run growth. If, however, demand shocks originate from irrational exuberance about, say, an unsustainable path for future housing prices, then integration could foster overinvestment, bubbles, and crashes.

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