FINANCIAL INTEGRATION, HOUSING AND ECONOMIC VOLATILITY*

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November 2012

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 how housing-price shocks affected the economy during the boom years. We exploit variation in credit-supply subsidies across local markets from the 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 the economy, thereby increasing economic volatility.

*We thank seminar participants at Babson College, Boston College, Boston University, The European Central Bank, George Washington University, International Monetary Fund, NBER (and our discussant, Antoinette Schoar), The NYU/Federal Reserve Bank of New York/RFS conference on Financial Intermediation (especially Victoria Ivashina and Greg Nini for helpful suggestions), Oxford, University of Massachusetts at Amherst, University of Virginia-Darden, and Virginia Tech.

I. 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 this recession is partly attributed to the slow recovery of housing and the associated consumer debt overhang (Mian and Sufi, 2011).

This paper studies how financial integration affected links from housing to the overall economy during the recent boom (1994 to 2006). During this period, housing price co-movement declined, as the Sun Belt experienced dramatic booms relative to other regions. Figure 1 plots the average absolute growth shock of housing prices across local markets over time. Cross-market variation trends down (so co-movements trend up) during the 1970s and 1980s. Starting in the 1990s, however, variation in price shocks across markets stops falling and then begins to rise. This trend break coincides with changes in the financial and banking systems, which have become increasingly well integrated as deregulation allowed banks to form nationwide branch networks and as securitization allowed lenders to sell mortgages to the secondary market. Using panel data at the local level, we show that financial integration reduced the co-movement in housing prices across markets by allowing capital to flow to booming markets. Moreover, integration can amplify the effect of positive collateral shocks (e.g. housing-price booms) on debt capacity and economic outcomes by facilitating capital inflows. Consistent with this idea, we show that financial integration strengthened the effect of housing shocks on a broad set of local economic outcomes, thereby increasing economic volatility.

Financial integration may dampen or amplify economic shocks. Morgan, Rime and Strahan (2004) – MRS hereafter – show theoretically that integration's effect on volatility depends on the sources and magnitudes of shocks hitting the local economy. Integration insulates local economies from credit supply shocks.[1] During the 1980s and early 1990s, these shocks were a major source of business-cycle instability (Bernanke and Lown, 1991). The number of bank and S&L failures during the 1980s averages more than 150 per year (Kroszner and Strahan, 2008), and the collapse of the S&L industry amplified downturns in areas such as Texas and California. 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. MRS show empirically that state-level banking integration fostered by deregulation during the 1970s and 80s lowered volatility of state economies in these years.

MRS's theoretical model, however, also shows that integration, by allowing financial capital to flow away from depressed areas and into booming ones, can amplify local cycles. For example, if collateral values rise in a locality, borrower debt capacity and demand for credit increases; integration helps bring financial resources from abroad to satisfy higher credit demand. The influx of credit raises growth above what would have been possible in a stand-alone, or dis-integrated, financial system. These flows correspondingly reduce collateral values and growth in areas with relatively weak credit demand because these markets face capital outflows. Thus, capital flows generated by credit demand shocks will both raise the effect of collateral shocks on the economy and reduce co-movements in collateral values across financially connected markets.

[1] Ashcraft (2004), Becker (2007) and Gilje (2012), for example, show that the supply of local bank finance affects investment.

Beyond its effects on capital flows, integration is also 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/soft information (Rajan, Seru and Vig, 2010). 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.

In our two core tests, we evaluate first whether shocks to house prices stimulate the local (Central Business Statistical Area, or CBSA) economy, and second whether financial integration amplifies the stimulus. For identification, we exploit subsidies in financing from Government-Sponsored Enterprises (GSEs) - Fannie Mae and 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 near the threshold increase their housing demand when the threshold rises (Adelino, Schoar and Severino, 2011). While the jumbo-loan cutoff changes uniformly, its effects vary across CBSAs. For example, in Los Angeles - where about 5.3% of mortgages fall within 5% of the jumbo-loan cutoff - the change in the cut-off would have a bigger impact than in Wichita, Kansas - where this fraction is only 0.5%. Following this intuition, we build an instrument by multiplying the sensitivity to the jumbo threshold in a market *i* during year *t-1* by the change in the cutoff between years *t-1* and *t*. The increase in the cut-off level provides us with the time-series variation in the instrument. The sensitivity to cut-off movements, based on the share of borrowers to be affected (e.g. LA v. Wichita), provides us the cross-sectional variation. We further strengthen our identification by interacting the market sensitivity measures (capturing housing demand) with a measure of housing supply elasticity (Saiz, 2010). Thus, our instruments capture changes in housing prices exogenous to economic fundamentals in the local area.

We find that these instruments are valid. When the jumbo-loan threshold increases, local housing prices appreciate faster in markets with more borrowers constrained by the threshold in the prior year. This effect is stronger in markets with inelastic housing supply, where prices respond more to demand shocks because the physical supply of housing is limited by geographic obstacles. Armed with exogenous variation, 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 causes an increase in local GDP growth of about 0.3% and an increase in non-construction, non-finance employment growth of about 0.2%, suggesting that higher prices spill over to sectors not directly affected by housing.

The effect of house-price shocks strengthens with financial integration. We employ two measures of financial integration to document this effect. The first captures integration stemming from the growth of secondary market for mortgages; the second captures the extension of bank branch networks across markets. We find that in local areas one-standard deviation above the mean level of either measure of financial integration, the impact of a 1% housing price shock on outcomes increases by 0.15% to 0.45%, depending on the specification. Taken together – higher housing price volatility and increased sensitivity to house-price shocks – the results imply that financial integration has increased economic volatility, both by amplifying variation in collateral values (house prices) and by strengthening links from collateral to the overall economy.

We then provide 'microfoundations' for the effects of financial integration. We show that banks reallocate funds in response to housing price shocks, controlling for economic fundamentals in a locality. Furthermore, this movement of funds across markets – toward high-demand areas and away from low-demand areas – lowers housing price co-movements between financially integrated markets.

Our paper contributes to three strands of literature. First, many have argued that the so-called '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. Chaney, Sraer and Thesmar (2011) and Popov and Corradin (2012) further show that local investment and business creation increased with housing wealth. 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. 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 has been explored both across US states and also 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; Kalemni, Papaionnou and Peydro, 2010). Most of these studies explore the settings where supply of capital 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), Keys et al (2010), Demyanyk and Van Hemert (2010), Loutskina and Strahan (2011)). Yet these studies do little to explain why booms were concentrated in places like Florida, Arizona and California. 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 II, we briefly review the forces leading to more financial integration over time. In Section III, we describe our integration measures and estimate the relationship 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 use this model to generate an instrument for housing price changes to estimate its causal impact on the economy as a whole and test how that impact varies with financial integration. To support our interpretation of the main result, we then show in Section IV that banks do reallocate funds to booming markets, and that, as a result, housing price shocks become de-synchronized by financial integration. Section V concludes.

II. FINANCIAL INTEGRATION

Securitization integrates housing finance

The move toward integration in mortgage lending began with the activities of the Government-Sponsored Enterprises (GSEs) - The Federal National Mortgage Association (Fannie Mae) and the Federal Home Loan Mortgage Corporation (Freddie Mac). Together they have played the dominant role in fostering 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 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.

As shown in Loutskina and Strahan (2009), 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 only purchase mortgages below $417,000. The loan limit increases each year by the percentage change in the national average of single-family housing prices during the prior year, based on a survey of major lenders by the Federal Housing Finance Board. The limit is 50% higher in Alaska and Hawaii. Because the loan limit changes mechanically and only as a function of *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.

Deregulation integrates the branch-banking system

Securitization helped integrate markets for low-risk, prime mortgages but had much less impact on markets for high-risk mortgages or jumbo mortgages, which are much more likely to remain in the portfolio of local lenders (Loutskina and Strahan, 2011). Moreover, credit to small business is almost never securitized because it requires investments in local information and close monitoring of the business. The gradual development of nationwide branch banking thus worked in parallel with securitization to enhance and complete the process of financial integration for more information-sensitive forms of credit. Into the 1970s, legal and regulatory barriers prevented banks from operating across geographical and product markets. Over time, these barriers have eroded. The process began during the 1970s, when 12 states allowed unrestricted statewide branching while 16 prohibited branching entirely. Between 1970 and 1994, 38 states eased their restrictions on in-state branching. States also prohibited ownership of their banks by out-of-state bank holding companies. These barriers to integration began to fall when Maine passed a 1978 law allowing entry by out-of-state BHCs if, in return, banks from Maine were allowed to enter those states. Other states followed suit, allowing the creation of multi-state bank holding companies (Jayaratne and Strahan, 1996; Jayaratne and Strahan, 1998).

The transition to full *inter*state banking and branching was fostered by passage of the Interstate Banking and Branching Efficiency Act of 1994 (IBBEA), which effectively permitted bank holding companies to enter other states without permission and allowed banks to operate branches across state lines (Rice and Strahan, 2010). With these legal changes, banks now may operate across many states and localities. Such cross ownership ties could, for example allow deposits collected in one market to finance information-intensive (i.e. non-securitizable) loans in another. Even with access to interbank markets or equity and bond markets, 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).

Despite the passage of IBBEA, states continue to exercise authority under this law to restrict or limit interstate branch entry. States, for example, had the option to opt into interstate branching immediately after passage of IBBEA or to wait until the default trigger date of June 1, 1997. Moreover, states that opposed 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 allowed states to employ various means to erect these barriers. States could 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 rather than 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.

Appendix Table 1 describes in detail how each state chose to deal with the possibility of interstate branching following IBBEA. States such as Illinois (since 2004), Massachusetts, and Ohio have the most open stance toward interstate entry; states such as Arkansas, Colorado and Montana have the most restrictive stance toward interstate entry. Following Rice and Strahan (2010), we use these four state powers to build a simple index of interstate branching restrictions that exhibits variation both across states and over time. The index equals zero for states that are most open to out-of-state entry. We add one to the index when a state adds any of the four barriers described above. The resulting index ranges from zero to four (Appendix Table 1). For most states that adopt branching in 1997 (i.e. states not choosing to opt in early), we set the index at 4 for 1994-1996; in subsequent year we set the index based on each state's policy choices. For example, Illinois 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 4 for 1994-1996; we reduce the index to 3 in 1997-2004; and we reduce it further to 0 in 2005-2006.

III. HOUSING PRICES AND ECONOMIC GROWTH

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

$$Y_{i,t} = \alpha_t + \gamma_i + \beta_1 \, \textit{House-Price Growth}_{i,t} + \textit{Control Variables} + \varepsilon_{i,t} \tag{1a}$$

and

$$Y_{i,t} = \alpha_t + \gamma_i + \beta_1 \, \textit{House-Price Growth}_{i,t} + \beta_2 \, \textit{Financial Integration}_{i,t} + \beta_3 \textit{Financial Integration}_{i,t} * \textit{House-Price Growth}_{i,t} + \textit{Control Variables} + \varepsilon_{i,t} \, . \tag{1b}$$

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

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: (i) personal income growth as reported by Bureau of economic Analysis, (ii) employment growth and (iii) employment growth without sectors directly affected by housing (construction and finance) as reported by Bureau of Labor Statistics, and, finally (iv) GDP growth reported by Moody's Analytics.[2] We estimate Equations (1a) and (1b) for a CBSA-year panel dataset with both year and CBSA fixed effects.

Table 1 reports summary statistics for housing price growth and for personal income, employment, and GDP growth during the sample period. The sample begins in 1994 because the deposit-based measure of financial integration (described below) 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 as well as becoming potentially endogenously related to local fundamentals.

GSE Housing-Finance Subsidies as Instruments for Housing Price Growth

Shocks to the overall economy will both affect and be 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

[2] The CBSA-year level GDP estimates are also available from Bureau of Economic Analysis (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.

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.[3] Potential home buyers receive a financing subsidy through the activities of the GSEs, who stand ready to buy mortgages that fall below the jumbo-loan cutoff and meet a set of credit-worthiness underwriting criteria. The cut-off is binding on borrowers, as is evident from the histogram of loan applications and loan approval rates presented in Figures 2A and 2B (adapted from Loutskina and Strahan (2009)). The large spike in loan applications and approval rates just below the jumbo cut-off indicates that the funding is both more abundant and cheaper below the jumbo loan cut-off. 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).

We exploit the idea that the impact of this increased subsidy varies across local housing markets. For example, in a market where all home prices fall below the jumbo-loan cutoff in *t-1*, home buyers would receive no incremental benefit from an increase in the cutoff in year *t*; as all potential homebuyers are already subsidized. In contrast, in markets with substantial demand near the jumbo-loan threshold, potential homebuyers would benefit greatly when the cutoff rises.

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

[3] 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.

mortgage lending in its portfolio.[4] Our sample covers a broad set of loans originated in the U.S. 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.

We use two instrumentation strategies to measure differences across markets in the impact of changes in the jumbo-loan cutoff on housing demand. First, we estimate the fraction of loan *applications* in CBSA *i* that are above the jumbo 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. This ratio captures the percentage of borrowers that would directly benefit from the change in the cut-off by getting access to more readily available and/or cheaper credit.

The second instrumentation strategy recognizes that a large fraction of home buyers reduce their borrowing to fall below the cut-off in year *t-1* (Figure 2A), but would still benefit from an increase in the jumbo-loan cutoff. For example, often home buyers will split their borrowing into a senior (non-jumbo) 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. To capture this portion of demand, we build an instrument equal to the total fraction of applications within 5% of the jumbo-loan cutoff (on either side) in year *t-1*, multiplied by the percentage change in the cutoff

[4] Any depository institution with a home office or branch in an CBSA must report HMDA data if it has made a home purchase loan on a one-to-four unit dwelling or has refinanced a home purchase loan and if it has assets above $30 million. Any non-depository institution with at least ten percent of its loan portfolio composed of home purchase loans must also report HMDA data if it has assets exceeding $10 million. Consequently, HMDA data does not capture lending activity of small or rural originators. U.S. Census shows that about 83 percent of the population lived in metropolitan areas over our sample period and hence the bulk of residential mortgage lending activity is likely to be reported under the HMDA.

between years *t-1* and *t*. These two instruments capture the sensitivity of credit demand across markets toward an exogenous shock in jumbo-loan threshold.

We further augment our set of instruments to capture housing supply conditions. Saiz (2010) creates a time-invariant measure of housing-supply elasticity for 263 CBSAs based on physical impediments to expansion in the housing stock, such as waterways, mountains, etc.[5] Housing demand shocks ought to affect prices more where supply elasticities are low, so we interact the elasticity measure with our instrument for demand.

Figure 3 illustrates our identification strategy graphically 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.

[5] Saiz shows that cities with high supply elasticity have both slower increases in housing prices over time and faster population growth, compared to 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.

The first-stage models thus take the following forms:

$$House\text{-}Price\ Growth_{i,t} = \alpha^{HP}_{t} + \gamma^{HP}_{i} + Other\ control\ variables + \qquad (2a)$$
$$+ \beta_1^{HP} Share\text{-}New\text{-}NJ_{i,t-1} + \beta_2^{HP} Share\text{-}New\text{-}NJ_{i,t-1}\ x\ Saiz\text{-}Elasticity_i + \varepsilon_{i,t},$$

$$House\text{-}Price\ Growth_{i,t} = \alpha^{HP}_{t} + \gamma^{HP}_{i} + Other\ control\ variables + \qquad (2b)$$
$$+ \beta_1^{HP} Share\text{-}Around\ NJ\ Cutoff_{i,t-1} * Change\ in\ Cutoff_{t} +$$
$$\beta_2^{HP} Share\text{-}Around\ NJ\ Cutoff_{i,t-1} * Change\ in\ Cutoff_t\ x\ Saiz\text{-}Elasticity_i + \varepsilon_{i,t},$$

where $Share\text{-}New\text{-}NJ_{i,t-1}$ equals the fraction of jumbo applications in CBSA *i* and year *t-1* that will fall below the jumbo-loan cutoff next year (year *t)*; $Share\text{-}Around\ NJ\ Cutoff_{i,t-1}$ equals the share of applications within +/- 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 measure gets absorbed by the CBSA effects.

We expect housing prices to grow fastest in markets with a large mass of demand that would benefit from an increase in the jumbo cutoff; thus, we expect: $\beta_1^{HP} > 0$. Since house prices should react less if supply is elastic, we expect the interaction terms to offset, meaning $\beta_2^{HP} < 0$. We estimate Equations (2a) and (2b) with year and CBSA fixed effects, and we cluster the standard errors at the level of the CBSA.

Table 2 reports the first-stage equations (Eq. (2a) and (2b)) linking the two sets of instruments to house-price appreciation. 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 banks.[6] Finally, we include time and CBSA fixed effects. We report the models with and without CBSA fixed effects, although we only use the models with CBSA fixed effects in the second-stage IV models that follow. We find that both sets of instruments have statistically significant effects on housing price, both the direct effect and the interaction with the Saiz supply elasticity measure. Moreover, the signs of the coefficients are economically sensible, with higher elasticity lowering the positive effect of the GSE subsidy on home prices.

Do Collateral Shocks Stimulate the Economy?

Armed with plausible exogenous variation in local housing prices, we can 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 (Equation 1a). 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 & 2), total employment growth (columns 3 & 4), the growth of total employment excluding employment in financial firms and construction (columns 5 & 6), and GDP growth (columns 7 & 8). We implement the two instrumentation strategies described above (Panels A and B). Each model is reported with and without 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. Employment

[6] 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.

growth without construction and finance allows us to test whether any effects that we observe spillover beyond segments directly tied to housing finance.[7]

The coefficient estimates are statistically and economically significant in all specifications, ranging from 0.18 to 0.42 across the various economic indicators. An exogenous 1% increase in housing prices thus causes the local economy to expand by 0.18 to 0.42 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 ranging from 0.03 to 0.08 (divide each coefficient by five), consistent with the admittedly wide range of estimates in the literature (see Mishkin, 2007 for a review). The coefficients on total employment growth are a bit smaller than those on GDP growth, which makes sense because GDP includes all sources of production from local sources (i.e. it includes returns to capital as well as labor).[8] Moreover, the coefficient on employment growth without segments directly tied to housing suggests 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 Papp (2006) test for weak instruments and only two out of sixteen models fail Hansen's over-identification test (i.e., reject the null).[9]

[7] We further evaluated this regression models directly controlling for lagged dependent variable and find the results to be quantitatively and qualitatively similar.

[8] We have also estimated these models separately for the early (1994-2000) and late (2001-2006) portions of our sample. We find that housing is positively and statistically significantly related to economic outcomes in both samples, with somewhat larger magnitudes in the first half of the sample.

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

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 II: 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).

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 (FDIC) *Summary of Deposits*, available online annually from 1994 forward.[10] *Deposit-Based Integration* equals the fraction of all deposits in a CBSA that are owned by a holding company which also owns deposits in one or more other CBSAs.[11]

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. Since economic or housing conditions of a particular market may play a role in this entry decision, the relationships observed in equation (1b) could be biased by reverse causality. If banks tend to enter over-heated markets, for instance, then β_3 may be biased upward in an OLS setting. To eliminate this source of potential bias, we use an instrument based on the index of

[10] See http://www2.fdic.gov/sod/.

[11] 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.

restrictions on interstate branching described in Section II. This index ranges from zero to four, where four represents the highest level of barriers to entry by out-of-state banks (see Appendix Table 1).

The second measure of financial integration, *Market-Based Integration*, captures the fact that many mortgage lenders fund their investments exclusively through securitization markets, thereby integrating local housing finance with national or even global capital markets. For this measure, we use data from the HMDA database. *Market-Based Integration* 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 non-depository 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 ($\rho = 0.84$).[12] While we do not have a clean policy instrument for this dimension of integration, we believe that the measure is relatively exogenous as we use applications from the prior year, rather than contemporaneous mortgage origination decisions, and we use the number rather than dollar volume of applications. Thus we eliminate any mechanical relationship between *Market-Based Integration* measure and economic outcomes.

Results

Table 4 reports the results when we add the interactions between housing price growth and our two measures of financial integration (Equation 1b). We model both *House-Price Growth* and its interaction with *Financial Integration* as jointly endogenous to alleviate reverse causality, since economic booms will tend to raise both. *House-Price Growth* is instrumented

[12] We have also used a securitization-based measure of market-based integration and find similar results to those reported below.

using the *Share-Around NJ Cutoff* $_{i,t-1}$ **Change in Cutoff* $_t$ (as in Eq. 2b) and its interaction with Saiz elasticity.[13]

Panel A reports the model using *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* (lagged one year) and its interactions with the core set of housing price growth instruments. Panel C then reports the full model with both integration measures included.

The results suggest that house-price shocks have a greater impact on economic outcomes in financially integrated markets. We have strong identification in all models, passing the Kleibergen and Papp (2006) test for weak instruments, and failing to reject the over-identification restrictions in most models. Across all specifications, housing-price growth and financial integration are jointly significant at better than 1%. Moreover, the interaction terms are consistently positive across all models, meaning that better integration amplifies the causal impact of housing price shocks on economic outcomes. In Panels A and B, the interaction effects are statistically significant across all models. In Panel C, where we include both the *Deposit-Based* and *Market-Based* interactions, the interactive coefficients are consistently positive and are jointly significant at better than 1% levels. In these last models, however, we cannot reliably determine which specific dimension of financial integration matters most. This could be due to the lack of power stemming from a relatively short sample, or because the two

[13] In unreported set of tests we conduct analysis using the alternative set of instrument. We find the results to be similar.

integration measures are correlated (ρ=0.31). The interaction terms (ranging from one to three in Panels A and B) suggest that increasing financial integration (either dimension) by one standard-deviation (0.15) raises the effect of a 1% house-price shocks by 0.15%-0.45%.

Overall, these results suggest that 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 in integrated markets. In these areas, higher demand for credit draws financial resources from other localities.

IV. MICRO-FOUNDATIONS: FINANCIAL INTEGRATION AND HOUSE PRICE CO-MOVEMENTS

Bank-level Reallocation of Funds

We have argued 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 document that lenders do move capital in response to collateral appreciation, irrespective of local economic conditions. To address this question we use lender-CBSA level panel data on mortgage originations provided by the HMDA data. We utilize all types of lenders in the analysis, including both depository and non-depository institutions.

Figure 4 shows graphically that mortgage growth *within* lender is consistently higher in 'hot' markets relative to 'cold' ones. For each lender-year, we first divide the CBSA areas in which this lender lends into 'hot' (those with above-median housing price growth last year) and 'cold' (those with below-median growth). We then compute the average mortgage growth rate for the two market types each year. By construction, each lender operates in both market types every year. As the figure shows, lenders move credit from slower to faster growing areas. These differences are consistent across every year of our sample.

To analyze this issue more rigorously, we conduct a regression analysis following the same intuition. We test whether banks lend relatively more in the areas where they face relatively higher increase in housing prices:

$$Loan\ Growth_{i,t}^{k} = \beta \cdot House\ Price\ Growth_{i,t} + \theta' Local\ Economic\ Conditions_{i,t} +$$

$$+ \gamma_{k,t} + Applicant\ Characteristics_{i,t}^{k} + \varepsilon_{i,t}^{k} \qquad (3)$$

where $Loan\ Growth_{i,t}^{k}$ equals the growth in mortgages from year $t-1$ to year t in CBSA i by lender k.

Equation (3) tests how a lender reallocates its mortgage business across 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, so the remaining variation represents the lender's reallocation across the markets in which it operates. Since lenders operate in different sets of markets, the same market may be 'hot' for some lenders and 'cold' for others. The lender-year fixed effects also strip out variation in lender access to funds, which will vary only by lender-year as long access to deposits or capital market funds can be shared across markets served by a given lender.[14] Since we want to isolate the effect of collateral on capital flows, we control for a host local fundamentals: personal income growth, employment growth, GDP growth, population density, and the industry structure captured by labor shares.[15] We also control for the characteristics of the mortgage applicant pool a lender faces. Thus, we interpret

[14] 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) and Campello (2002)).

[15] In the unreported set of tests we control for up to a third degree polynomial of the local economic fundamentals. We also evaluate the Fama-McBeth estimates of the coefficients for the regression equation (3). Both robustness tests provide us with the results that are qualitatively and quantitatively similar to those reported in this paper.

the coefficient β as a measure of how lenders respond to changes in the value of local collateral. The standard errors are clustered at CBSA level.

Table 5 shows that lenders move capital into booming housing markets (and away from less-booming markets). Panel A reports the results for the effect of a contemporaneous increase in housing prices on lending, while Panel B uses lagged changes in housing prices. In column (2), we consider only those lenders with significant market presence by dropping all lenders with fewer than 10 loans originated in both years *t* and *t-1*. In addition, we offer several strategies to alleviate concerns about reverse causality (from mortgage growth to housing price appreciation). First, we report equation (3) with both contemporaneous and lagged changes in housing prices (column 2).[16] Second, we estimate the model without large lenders, whose credit decisions are more likely to drive prices (column 3). Third, we estimate the model without CBSA-years in the upper and lower quartiles of housing-price growth distribution (column 4). This filter removes both markets that are likely to be 'hot' for all lenders (those in the upper 25th percentile) and markets that are likely to be 'cold' for all lenders (those in the lower 25th percentile). Thus, we are left with markets that are likely to be 'hot' for one set of banks and 'cold' but are not booming (or busting) markets on average.

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 1.2 to 1.5, meaning that 1% relative growth in housing prices leads to 1.2%-1.5% more funds flowing to that market within a financial institution. The reaction to past increases in housing prices is less elastic but still significant both

[16] Note that the instruments used earlier, based on movements in the jumbo threshold, are not appropriate in modeling variation in mortgage lending because they would fail the exclusion restriction.

statistically and economically (0.35% to 0.85%), depending on the sample. So, banks do reallocate funds in response to collateral shocks, irrespective of economic fundamentals.

Market-Level Price Co-movements

As we have seen, financially integrated lenders move capital toward high-demand markets and away from low-demand markets. By looking at integration's effects on housing between market pairs, we can test how these capital flows affect prices. Imagine two CBSA markets – 'A' and 'B' – that are well integrated financially. A shock to prices in 'A' (and thus to credit demand there) will draw financial resources away from 'B', thus accommodating the credit demand and raising prices in A and lowering them in B. This capital flight suggests that financial integration ought to make house-price changes become less correlated as integration between two markets increases.[17] To test this hypothesis directly, we build a panel dataset of all CBSA market pairs and test how whether financial connections between markets reduces co-movement in prices. As in our earlier models, we focus on the 1994 to 2006 period.

We build $Co\text{-}movement_{i,j,t}$ across two markets i and j by comparing the housing price growth shocks between two markets. In a first-pass regression, we strip out year and CBSA effects to create a growth shocks:

$$Ln\ Housing\ Price_{i,t} - Ln\ Housing\ Price_{i,t-1} = \alpha_t + \gamma_i + growth\text{-}shock_{i,t}\,, \tag{4a}$$

We then use these growth shocks to build $Co\text{-}movement_{i,j,t}$ between two CBSAs in a given year:

$$Co\text{-}movement_{i,j,t} = -\left|\, growth\text{-}shock_{i,t} - growth\text{-}shock_{j,t} \right|. \tag{4b}$$

[17] House price variation driven by local credit supply shocks will tend to attenuate this effect.

An increase in *Co-movement*$_{i,j,t}$ captures more similarity in the housing prices of two CBSAs. To test whether financial integration reduces *Co-movement*, we estimate the following regression:

$$Co\text{-}movement_{i,j,t} = \alpha_t + \gamma_{i,j} + \beta Integration_{i,j,t} + Other\ Controls + \varepsilon_{i,j,t}\ . \qquad (4c)$$

Following our previous analysis, we build two measures of financial integration between CBSA market pairs. First, we build the *Common-Deposit Ratio,* equal to the share of deposits in two markets with a common ownership link (at bank holding company level). Higher values of *Common-Deposit Ratio* indicate a greater degree of shared deposits – greater integration – between CBSAs. The second measure is built in a similar manner using mortgage market connections rather than deposit (branch) connections. Using the HMDA data, we build the *Common-Loan Ratio*, equal to the sum of mortgage loans made by lenders lending in both markets, divided by total mortgage loans in both markets. Unlike the *Common-Deposit Ratio*, *Common-Loan Ratio* incorporates activity by both depositories and non-depository mortgage lenders.

Table 6 reports summary statistics for the pair-wise housing-interrelatedness measure *(Co-movement)* and for the two integration measures. On average, the difference in growth residuals between pairs of CBSAs equals about four percentage points. Nearly 40% of market pairs have some deposit ownership links, with an average *Common-Deposit ratio* of about 8%. Among the connected pairs, the *Common-Deposit* ratio averages about 21%. *Common-Loan Ratio* exhibits a higher mean and a greater fraction of CBSA pairs are integrated along this dimension, which makes sense because this integration measure accounts for all lenders, both depository and non-depository. Almost three-quarters of CBSA pairs share some common lenders, with an average *Common-Loan Ratio* above 50%.

Table 7 reports the estimation of Equation (4c), first using all CBSA pairs, and second using only CBSA pairs in different states. In these pair-wise models we accommodate time and CBSA-pair specific fixed effect, a total of a 65,508 unique fixed effects. These fixed effects remove factors such as geographical distance that may affect the similarity of housing markets between two CBSAs. We also include a variable capturing the 'distance' or similarity of the industry mix between pairs, equal to the sum of squared difference in industry shares (i.e. the Euclidean distance). This pair-wise factor will capture variation over time in the differences in industry mix between markets. We also report models where financial integration is interacted with an indicator based on the Saiz housing-supply elasticity.[18] Adding the interaction term allows us to test whether the impact of capital flows on price co-movements is greater where supply is less elastic, as one would expect. We group our data into clusters for each CBSA to build standard errors. So, although the models are built from several hundred thousand observations, there are just 372 independent clusters.

Consistent with capital flows, market pairs that are more integrated with each other have less commonality in house-price growth shocks. Both the *Common-Deposit ratio* and the *Common-Loan Ratio* consistently enter negatively across all models. We find that markets sharing bank deposits have house-growth shocks that are almost 1% (=0.21*-0.0488 – see Table 6 and Table 7A, column 2) less similar when both markets have below-median housing supply elasticity. This effect is substantial relative to the overall variation of these differential shocks (σ

[18] We code the housing-supply elasticity as zero if both markets have above-median elasticity, as ½ of one market is above the median, and as 1 if both markets are above median.

= 4.13% - see Table 6). Markets sharing lending resources also exhibit less similar growth shocks. For example, markets sharing lenders have house-growth shocks that are a bit more than 1% (=0.58*-0.0277 – see Table 6 & Table 7A, column 4) less similar when both markets have below-median housing supply elasticity. Again, the effect is much smaller in areas with high housing supply elasticity. The results also suggest that both dimensions of financial integration matter at the margin. In fact, when we include both together – columns 5 and 6 – magnitudes and statistical significance change very little. The results in Panel B, where we drop CBSA markets in the same state (e.g. LA-SF), are slightly larger in magnitude than those in Panel A, which makes sense because integration likely matters more between more distant pairs of markets.[19]

These results support the idea that capital flows affect collateral values. In markets that are financially connected, markets with high credit demand draw in financial capital from markets with lower demand, thereby reducing the correlatedness of collateral values between the two markets. In markets that share financial resources, housing price growth rates become *less similar*. This result is strong evidence that financial integration amplifies credit-demand shocks; capital flowing between these markets lowers the similarity in shocks to the value of collateral.

V. CONCLUSION

The Financial Crisis and subsequent Great Recession of 2007-2011 have emphasized for everyone the importance of a strong housing market to the economy. Housing prices not only increased sharply during the 2000s, but they also became more volatile and less correlated across local markets. For example, the housing boom was concentrated in Sun Belt areas like Florida,

[19] We have also estimated IV models using the branching restriction index as an instrument for the *Common-Deposit Ratio*. These results are consistent with the OLS reported here, with coefficient magnitudes that are somewhat larger.

California, Nevada and Arizona. We show that this regional variation can be explained in part by better financial integration. We demonstrate a causal link from housing to the overall 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 in the local economy. Our estimates suggest that a 1% rise in housing prices increase growth by about 0.3%. This effect is larger in localities that are better integrated with other markets through bank ownership ties and access to securitization markets. The results suggest that during the housing boom financial integration raised the effect of collateral shocks on the economy, thereby increasing economic volatility.

The normative implications of the increased volatility that may come with financial integration depend on the source of credit-demand shocks. If shocks originate from rational assessments about local fundamentals, then financial integration improves allocative efficiency by allowing capital to flow toward high-return investments. Thus, integration may 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 facilitates over-investment, bubbles and crashes.

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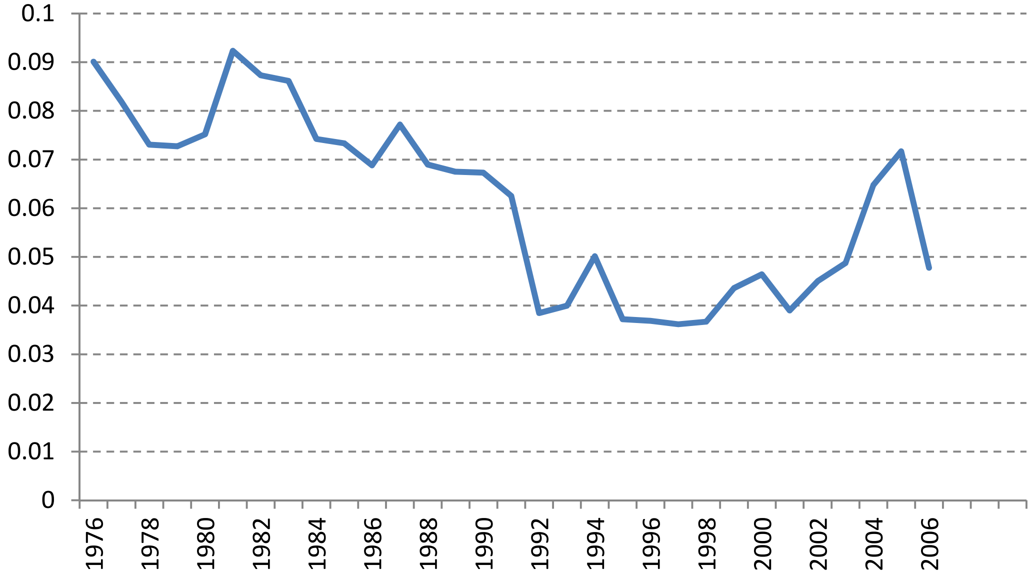
Figure 1: Cross-Market Variation in Housing Prices



This figure reports the average absolute growth shock in housing prices across CBSA markets from 1976 to 2006.

Figure 2A: Histogram of Loan Applications 1994-2006.

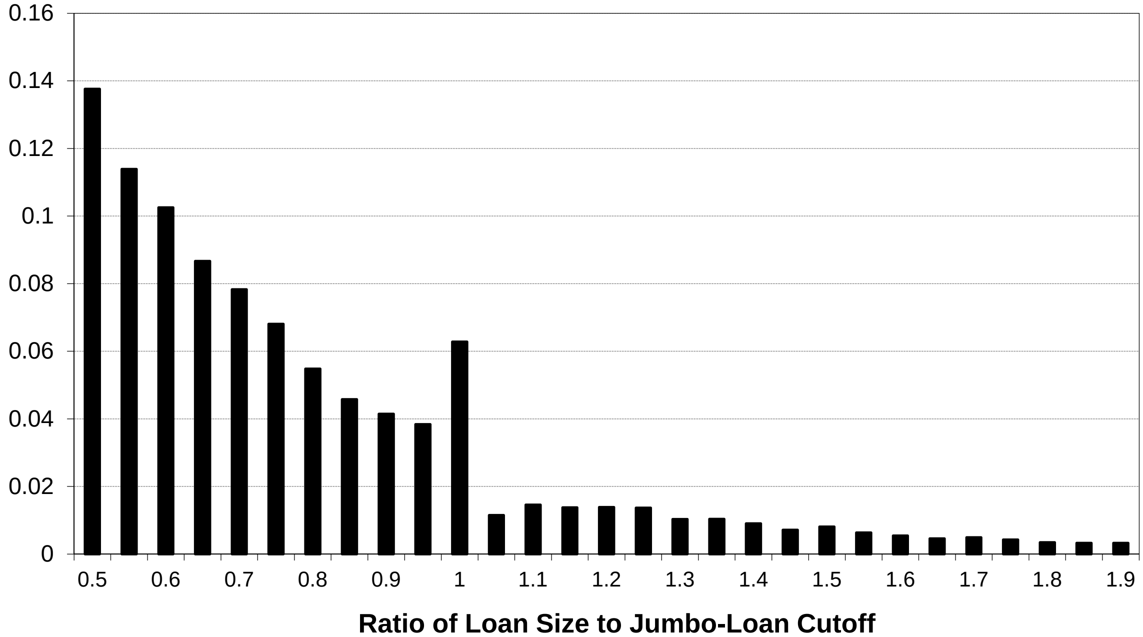


Figure 2B: Share of Approved Loan Applications 1994-2006.

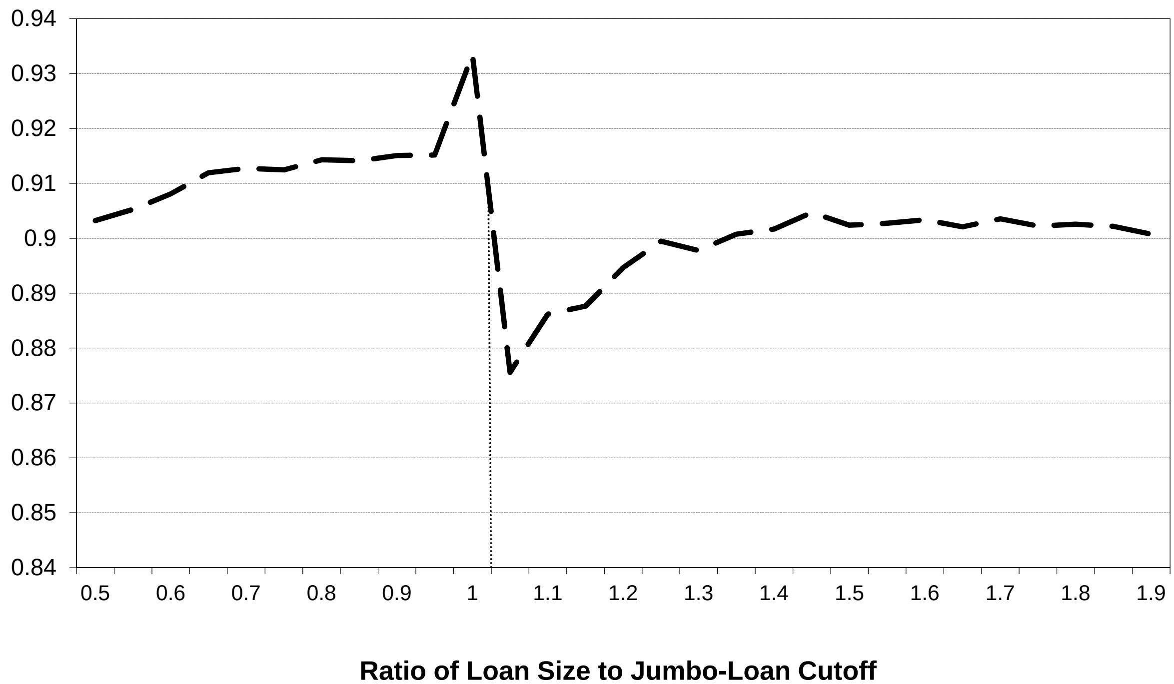
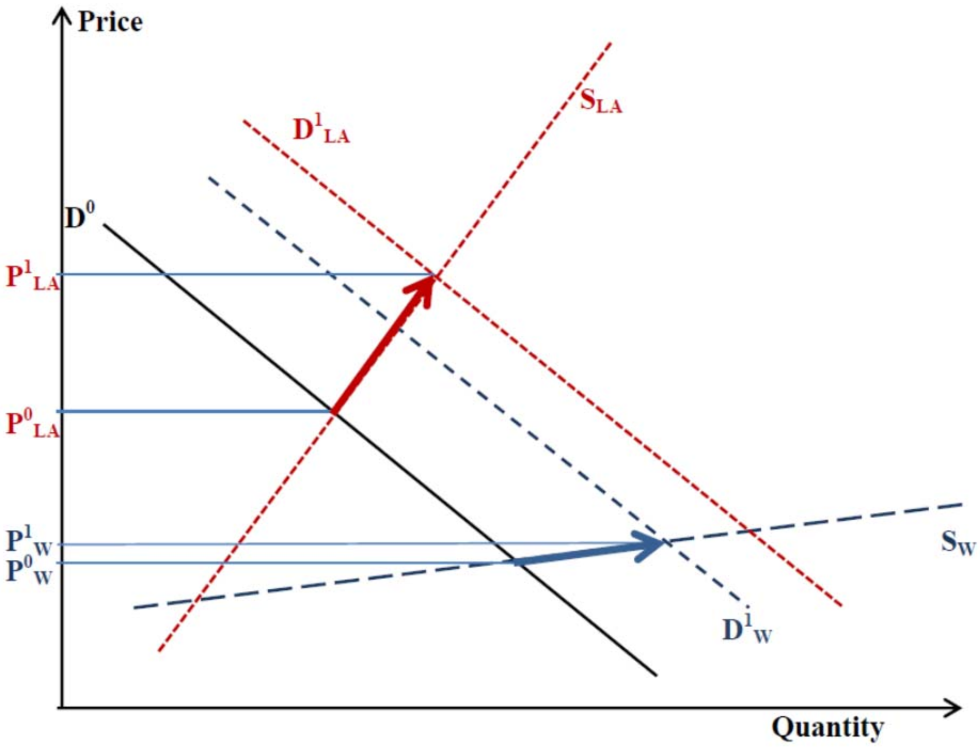
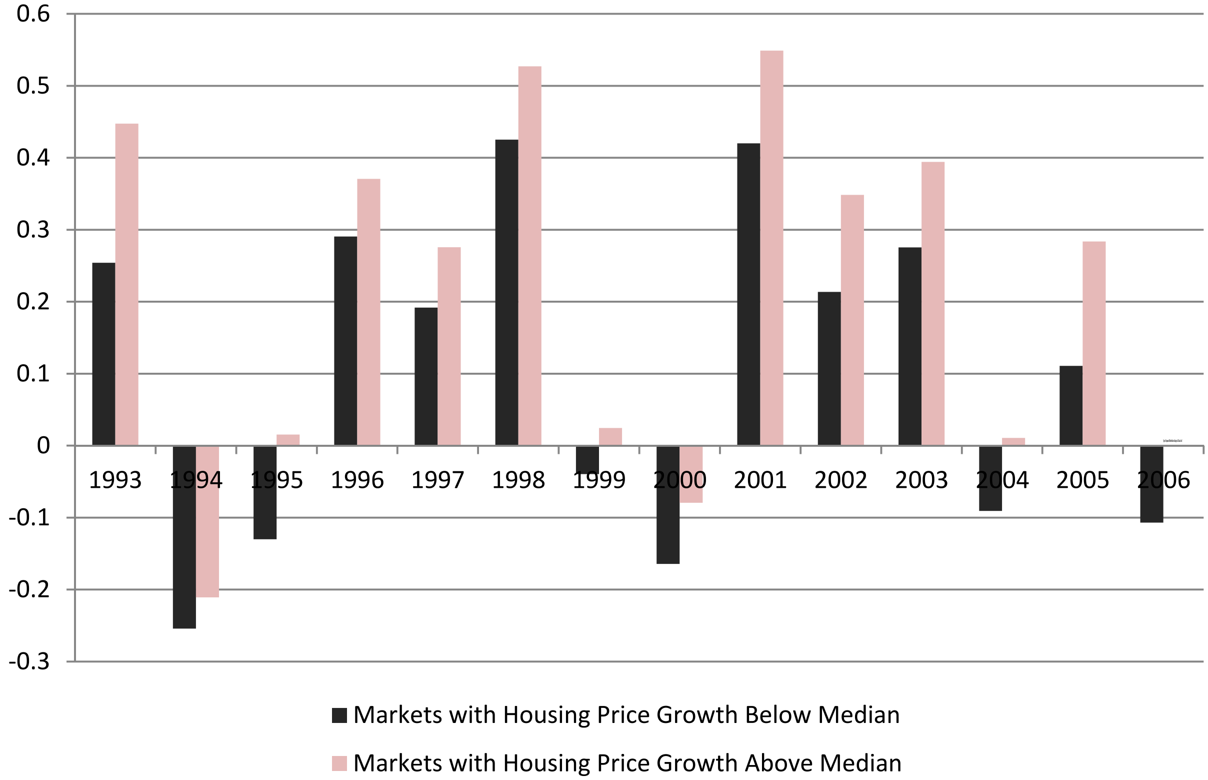


Figure 3: Responses of Different Markets to Changes in GSE Loan Cut-off



The graph illustrates the responses of two hypothetical markets to changes in the GSE loan cut-off. The subscript LA represents Los-Angeles CA and subscript W represents Wichita KS. 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 cut-off (D^1_{LA} and D^1_W). The graph illustrates the corresponding changes in the housing prices.

Figure 4: Average Origination Growth across Hot and Cold Housing Markets within Lenders



Sample includes all lenders, both depository and non-depository. Growth is computed within lender, so each pair of bars has observations from same set of lenders.

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. The *Deposit-Based Integration* equals the fractoin of deposits in a CBSA-year owned by banking companies with deposits in other markets. The *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 CBSA areas without a branch presence).

	Mean	***StDev***
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%
CBSA level GDP growth	5.39%	3.04%
Instruments		
Share of New Non-Jumbo borrowers	0.357%	0.788%
Share Near the Jumbo Cutoff * Change in Cutoff	0.092%	0.145%
Saiz Measure of Housing Supply Elasticity	2.595	1.422
Financial Integration		
Deposit-Based Integration	81.4%	15.3%
Market-Based Integration	59.7%	15.2%

Table 2: First-Stage Regressions: Housing Price Growth

This table reports regressions of housing price growth by CBSA-year on two sets of instruments: (i) the share of borrowers in year t-1 that will become non-jumbo in year t (share new non-jumbo), and (ii) 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, * p<0.1

Dependent Variable:	*Housing Price Growth*			
	(1)	(2)	(3)	(4)
Share of New Non-Jumbo borrowers	0.536**	-	1.975**	-
	(2.38)	-	(2.28)	-
Share of New Non-Jumbo borrowers	-0.364*	-	-0.588**	-
* Saiz Elasticity of Housing Supply	(1.95)	-	(2.31)	-
Share Around Jumbo Cutoff * Change in Cutoff	-	6.469***	-	13.57***
	-	(5.71)	-	(3.13)
Share Around Jumbo Cutoff * Change in Cutoff	-	-2.952***	-	-5.104***
* Saiz Elasticity of Housing Supply	-	(3.09)	-	(3.85)
Saiz Elasticity of Housing Supply	-0.00316***	-0.00211***	-	-
	(4.36)	(3.44)	-	-
CBSA dummies	no	no	yes	yes
Time fixed effects	yes	yes	yes	yes
Industry structure	yes	yes	yes	yes
Banking Sector Controls	yes	yes	yes	yes
Observations	3,277	3,277	3,277	3,277
R^2	0.273	0.282	0.425	0.421

Table 3: 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 is at CBSA-year level. First stage results are reported in Table 5. 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, * p<0.1

Panel A: This panel uses s*hare new NJ* and (s*hare new NJ* •*Saiz elasticity)* as identifying instruments.

	Personal Income Growth		*Total Employment Growth*		*Employment Growth w/o Construction or Finance*		*GDP Growth*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 Housing Price Growth		-0.030		-0.0511***		(0.01)		-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
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***

Panel B: This panel uses *(share around cutoff • change in cutoff)* and *(share around cutoff • change in cutoff • Saiz elasticity)* as identifying instruments.

	Personal Income Growth		*Total Employment Growth*		*Employment Growth w/o Construction or Finance*		*GDP Growth*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
House-Price Growth	0.425***	0.314***	0.323***	0.285***	0.235**	0.252***	0.361***	0.316***
	(4.37)	(4.90)	(3.41)	(5.57)	(2.37)	(4.36)	(2.65)	(4.21)
Lagged Housing Price Growth		-0.0919***		-0.0883***		(0.05)		-0.0968**
		(2.99)		(3.46)		(1.60)		(2.47)
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
Observations	3,277	3,272	3,277	3,272	3,277	3,272	3,277	3,272
Hansen-J statistic	2.39	0.15	3.283**	1.66	3.137*	1.17	2.55	2.15
Kleibergen-Paap test	21.52***	107.1***	21.52***	107.1***	21.52***	107.1***	21.52***	107.1***

Table 4: 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 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 (non-depository 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 5, column 4. The interaction of house-rice growth and the deposit-based integration is instrumented via the branching restrictions index interacted with the HMDA 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, * p<0.1

Panel A: Financial Integration based on Deposit Connections

	Personal Income Growth	*Total Employment Growth*	*Employment growth w/o Construction or Finance*	*GDP Growth*
	(1)	(2)	(3)	(4)
House-Price Growth	-1.73	-2.601**	-2.185**	-2.593**
	(1.61)	(2.49)	(2.16)	(2.18)
House-Price Growth * Deposit-Based Integration	2.345*	3.211***	2.647**	3.256**
	(1.90)	(2.68)	(2.27)	(2.34)
Deposit-Based Integration	-0.101*	-0.135**	-0.111**	-0.144**
	(1.76)	(2.39)	(2.00)	(2.21)
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
Observations	3,277	3,277	3,277	3,277
Hansen-J statistic	0.37	1.93	1.99	1.14
Kleibergen-Paap test	9.78**	9.78**	9.78**	9.78**
Anderson-Rubin Chi^2 test	19.84***	31.00***	14.63***	13.48***

Panel B: Financial Integration based on Market Connections

	Personal Income Growth	*Total Employment Growth*	*Employment growth w/o Construction or Finance*	*GDP Growth*
	(1)	(2)	(3)	(4)
House-Price Growth	-1.065**	-0.38	-0.47	-0.31
	(2.24)	(1.21)	(1.44)	(0.77)
House-Price Growth * Market-Based Integration	2.068***	1.046**	1.085**	0.985*
	(2.99)	(2.16)	(2.12)	(1.65)
Market-Based Integration	-0.0809***	-0.0361*	-0.0460**	(0.03)
	(2.63)	(1.72)	(2.10)	(0.97)
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
Observations	3,136	3,136	3,136	3,136
Hansen-J statistic	0.456	2.419	1.520	1.810
Kleibergen-Paap test	23.36***	23.36***	23.36***	23.36***
Anderson-Rubin Chi^2 test	31.81***	27.32***	16.94***	18.69***

Panel A: Financial Integration based on Deposit and Market Connections

	Personal Income Growth	*Total Employment Growth*	*Employment growth w/o Construction or Finance*	*GDP Growth*
	(1)	(2)	(3)	(4)
House-Price Growth	-1.13	-1.665**	-0.96	-1.20
	(1.21)	(2.07)	(1.25)	(1.23)
House-Price Growth * Deposit-Based Integration	0.10	1.571*	0.62	1.08
	(0.09)	(1.74)	(0.69)	(0.98)
House-Price Growth * Market-Based Integration	1.879***	0.74	0.844*	0.72
	(2.76)	(1.51)	(1.71)	(1.20)
Deposit-Based Integration	0.01	(0.06)	(0.02)	(0.05)
	(0.01)	(1.55)	(0.53)	(0.95)
Market-Based Integration	-0.0760**	(0.03)	-0.0395*	(0.02)
	(2.47)	(1.25)	(1.81)	(0.68)
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
Observations	3,136	3,136	3,136	3,136
Hansen-J statistic	3.81	4.43	8.858**	4.50
Kleibergen-Paap test	11.93***	11.93***	11.93***	11.93***
Anderson-Rubin Chi^2 test	32.16***	28.93***	19.79***	18.81***

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. 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, * p<0.1

	Sample			
	All Lenders	Lenders with ≥10 applications in a CBSA	Lenders with <5% market share in CBSA	2nd and 3rd Quartile of House-Price Growth
Panel A: Response to Contemporaneous Increase in Housing Prices				
	(1)	(2)	(3)	(4)
House-Price Growth	1.317***	1.503***	1.318***	1.180***
	(15.95)	(21.90)	(15.95)	(5.35)
Local GDP Growth	-0.0378	-0.194	-0.0303	0.204
	(0.31)	(1.59)	(0.24)	(1.27)
Local Personal Income Growth	-0.114	0.117	-0.11	-0.0829
	(0.67)	(0.60)	(0.64)	(0.44)
Local Employment Growth	0.701***	0.784***	0.690***	0.385**
	(3.72)	(4.67)	(3.59)	(2.08)
Log CBSA Population	0.00591	0.0175***	0.00674	-0.0003
	(1.00)	(4.39)	(1.14)	(0.06)
Applicant Pool Controls	Yes	Yes	Yes	Yes
Local Industry Structure Controls	Yes	Yes	Yes	Yes
Bank*Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	559,984	256,793	546,796	272,014
R^2	38.8%	57.0%	38.7%	41.7%
Panel B: Response to Lagged Increase in Housing Prices				
Lagged House-Price Growth	0.350***	0.630***	0.347***	0.849***
	(4.05)	(6.56)	(3.97)	(3.05)
Local GDP Growth	0.17	0.0218	0.18	0.0761
	(1.26)	(0.16)	(1.30)	(0.48)
Local Personal Income Growth	0.449**	0.751***	0.456**	0.546***
	(2.46)	(3.65)	(2.44)	(2.78)
Local Employment Growth	0.897***	1.001***	0.886***	0.456**
	(4.22)	(4.80)	(4.08)	(2.56)
Log CBSA Population	0.008	0.0190***	0.009	0.003
	(1.40)	(4.68)	(1.54)	(0.43)
Applicant Pool Controls	Yes	Yes	Yes	Yes
Local Industry Structure Controls	Yes	Yes	Yes	Yes
Bank*Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	559,753	256,684	546,602	275,381
R^2	38.7%	56.6%	38.6%	41.7%

Table 6: Summary Statistics for Measures of Integration and Housing Price Interrelatedness

This table reports summary statistics for measures of financial integration and housing price interrelatedness at the level of CBSA-pair-years.

	Mean	Std. Dev.
Common-Deposit Ratio (% of shared deposits between market pairs)	8.04%	14.26%
Common-Deposit Ratio (% of shared deposits between market pairs), when positive	20.57%	16.26%
Common-Deposit Ratio > 0 indicator	38.97%	N/A
Common-Loan Ratio (% of shared deposits between market pairs)	40.68%	30.50%
Common-Loan Ratio (% of shared deposits between market pairs), when positive	57.96%	18.01%
Common-Loan Ratio > 0 indicator	70.17%	N/A
House Price Interrelatedness$_{ijt}$ = - |Residual House-Price Growth$_{it}$ - Residual House-Price Growth$_{jt}$|	-4.07%	4.13%

Table 7: Housing Price Co-movement Across Markets

measures of financial integration between the two market pairs. The dependent variable is constructed as follows: first, we regress housing price growth on a CBSA fixed effect and year fixed effect and save the residual. We use the absolute value of this growth residual as the growth shock in market i, year t. Interrelatedness equals the share of deposits in a market pair owned by a banking company operating in both markets; interrelatedness indicator equals 1 for market pairs that have any deposits owned by a banking company operating in both markets. Above median elasticity equals 0 if both pairs of markets have a housing supply elasticity below the median; it equals 1/2 if one of the two markets has above median elasticity; and it equals 1 if both markets are above the median. Distance between employment shares is a Euclidian distance between industry structures in two CBSAs. CBSA-level elasticities are from Saiz (2010). Each model includes time effects and CBSA-pair fixed effects. Standard errors are clustered by CBSA. *** p<0.01, ** p<0.05, * p<0.1

Dependent Variable	*House Price Co-movement* $_{i,j,t}$ = - |*Residual House-Price Growth* $_{i,t}$ - *Residual House-Price Growth* $_{j,t}$ |					
Panel A: All CBSA Pairs	(1)	(2)	(3)	(4)	(5)	(6)
Common-Deposit Ratio	-0.0245***	-0.0488***	-	-	-0.0229***	-0.0376***
	(7.85)	(8.39)	-	-	(7.40)	(6.91)
Common-Deposit Ratio *	-	0.0383***	-	-	-	0.0243***
Above Median Elasticity	-	(5.23)	-	-	-	(4.00)
Common-Loan Ratio	-	-	-0.0137***	-0.0277***	-0.0132***	-0.0243***
	-	-	(6.09)	(8.34)	(5.85)	(7.47)
Common-Loan Ratio *	-	-	-	0.0176***	-	0.0136***
Above Median Elasticity	-	-	-	(3.81)	-	(2.93)
Distance between Employment Shares	-0.0144**	-0.008	-0.0143**	-0.00631	-0.0144**	(0.01)
	(2.01)	(1.06)	(2.03)	(0.88)	(2.05)	(0.96)
Time Effects	yes	yes	yes	yes	yes	yes
CBSA-Pair Fixed Effects	yes	yes	yes	yes	yes	yes
Number of Observations	707,256	568,671	707,256	568,671	707,256	568,671
R^2	39.50%	39.50%	39.60%	39.70%	39.70%	39.80%
Panel B: All CBSA Pairs in different states						
Common-Deposit Ratio	-0.0252***	-0.0502***	-	-	-0.0235***	-0.0384***
	(-7.695)	(-8.276)	-	-	(-7.240)	(-6.697)
Common-Deposit Ratio *	-	0.0393***	-	-	-	0.0247***
Above Median Elasticity	-	(5.18)	-	-	-	(3.88)
Common-Loan Ratio	-	-	-0.0144***	-0.0287***	-0.0139***	-0.0250***
	-	-	(-6.026)	(-8.276)	(-5.788)	(-7.339)
Common-Loan Ratio *	-	-	-	0.0179***	-	0.0136***
Above Median Elasticity	-	-	-	(3.76)	-	(2.85)
Distance between Employment Shares	-0.0144**	-0.008	-0.0144**	-0.00674	-0.0145**	(0.01)
	(-1.967)	(-1.088)	(-1.983)	(-0.920)	(-2.008)	(-0.993)
Time Effects	yes	yes	yes	yes	yes	yes
CBSA-Pair Fixed Effects	yes	yes	yes	yes	yes	yes
Number of Observations	685,625	551,032	685,625	551,032	685,625	551,032
R^2	39.70%	39.70%	39.80%	39.90%	39.90%	40.00%

Appendix Table 1
State Interstate Branching Laws: 1994 - 2005

This tables lists the index of interstate branching restrictions, effective date of interstate branching regulation changes, and each of the four provisions: the minimum age of the institution for acquisition, allowance of de novo interstate branching, allowance of interstate branching by acquisition of a single branch or portions of an institution, and the statewide deposit cap on branch acquisitions. The index is set to zero for states that are most open to out-of-state entry. We add one to the index when a state adds any of the four barriers just described. Specifically, we add one to the index: if a state imposes a minimum age on target institutions of interstate acquirers of 3 or more years; if a state does not permit de novo interstate branching; if a state does not permit the acquisition of individual branches by an out-of-state bank; and if a state imposes a deposit cap less that 30%. The index ranges from zero to four.

State	Branching Restrictivness Index	Effective Date	Minimum Age of Institution (Bank or Branch) for Acquisitions	Allows de novo Interstate Branching	Interstate Branching by Acquisition of Single Branch or Portions of an Institution	Statewide Deposit Cap on Branch Acquisitions
Alabama	3	5/31/1997	5 years	No	No	30%
Alaska	2	1/1/1994	3 years	No	Yes	50%
Arizona	2	8/31/2001	5 years	No	Yes	30%
Arizona	3	9/1/1996	5 years	No	No	30%
Arkansas	4	6/1/1997	5 years	No	No	25%
California	3	9/28/1995	5 years	No	No	30%
Colorado	4	6/1/1997	5 years	No	No	25%
Connecticut	1	6/27/1995	5 years	Yes	Yes	30%
Delaware	3	9/29/1995	5 years	No	No	30%
DC	0	6/13/1996	No	Yes	Yes	30%
Florida	3	6/1/1997	3 years	No	No	30%
Georgia	3	5/10/2002	3 years	No	No	30%
Georgia	3	6/1/1997	5 years	No	No	30%
Hawaii	0	1/1/2001	No	Yes	Yes	30%
Hawaii	3	6/1/1997	5 years	No	No	30%
Idaho	3	9/29/1995	5 years	No	No	None
Illinois	0	8/20/2004	No	Yes	Yes	30%
Illinois	3	6/1/1997	5 years	No	No	30%
Indiana	1	7/1/1998	5 years	Yes	Yes	30%
Indiana	0	6/1/1997	No	Yes	Yes	30%
Iowa	4	4/4/1996	5 years	No	No	15%
Kansas	4	9/29/1995	5 years	No	No	15%
Kentucky	3	3/22/2004	No	No	No	15%
Kentucky	3	3/17/2000	No	No	No	15%
Kentucky	4	6/1/1997	5 years	No	No	15%
Louisiana	3	6/1/1997	5 years	No	No	30%
Maine	0	1/1/1997	No	Yes	Yes	30%
Maryland	0	9/29/1995	No	Yes	Yes	30%
Massachusetts	1	8/2/1996	3 years	Yes	Yes	30%
Michigan	0	11/29/1995	No	Yes	Yes	None
Minnesota	3	6/1/1997	5 years	No	No	30%
Mississippi	4	6/1/1997	5 years	No	No	25%
Missouri	4	9/29/1995	5 years	No	No	13%
Montana	4	10/1/2001	5 years	No	No	22%
Montana	4	9/29/1995	N/A	N/A	N/A	Increases 1% per year from 18% to 22%
Nebraska	4	5/31/1997	5 years	No	No	14%
Nevada	3	9/29/1995	5 years	Limited	Limited	30%
New Hampshire	0	1/1/2002	No	Yes	Yes	30%
New Hampshire	1	8/1/2000	5 years	Yes	Yes	30%
New Hampshire	4	6/1/1997	5 years	No	No	20%
New Jersey	1	4/17/1996	No	No	Yes	30%
New Mexico	3	6/1/1996	5 years	No	No	40%
New York	2	6/1/1997	5 years	No	Yes	30%
North Carolina	0	7/1/1995	No	Yes	Yes	30%
North Dakota	1	8/1/2003	No	Yes	Yes	25%

North Dakota	3	5/31/1997	No	No	No	25%
Ohio	0	5/21/1997	No	Yes	Yes	30%
Oklahoma	1	5/17/2000	No	Yes	Yes	20%
Oklahoma	4	5/31/1997	5 years	No	No	15%
Oregon	3	7/1/1997	3 years	No	No	30%
Pennsylvania	0	7/6/1995	No	Yes	Yes	30%
Rhode Island	0	6/20/1995	No	Yes	Yes	30%
South Carolina	3	7/1/1996	5 years	No	No	30%
South Dakota	3	3/9/1996	5 years	No	No	30%
Tennessee	1	3/17/2003	3 years	Yes	Yes	30%
Tennessee	1	7/1/2001	5 years	Yes	Yes	30%
Tennessee	2	5/1/1998	5 years	No	Yes	30%
Tennessee	3	6/1/1997	5 years	No	No	30%
Texas	2	9/1/1999	No	Yes	Yes	20%
Texas	4	8/28/1995	N/A	N/A	N/A	20%
Utah	1	4/30/2001	5 years	Yes	Yes	30%
Utah	2	6/1/1995	5 years	No	Yes	30%
Vermont	0	1/1/2001	No	Yes	Yes	30%
Vermont	2	5/30/1996	5 years	No	Yes	30%
Virginia	0	9/29/1995	No	Yes	Yes	30%
Washington	1	5/9/2005	5 years	Yes	Yes	30%
Washington	3	6/6/1996	5 years	No	No	30%
West Virginia	1	5/31/1997	No	Yes	Yes	25%
Wisconsin	3	5/1/1996	5 years	No	No	30%
Wyoming	3	5/31/1997	3 years	No	No	30%

Source: Johnson and Rice (2008)