Internet Appendix

# Positive Bank-to-Bank Spillovers

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This internet appendix presents analyses that are omitted from the main paper for brevity. Section IA.1 describes sample construction. Section IA.2 presents evidence that shale shock is a positive shock on bank liquidity. Section IA.3 describes the construction of the main independent variable in a hypothetical network. Section IA.4 presents robustness tests of baseline results. Section IA.5 addresses alternate mechanisms of spillovers. Section IA.6 presents aggregate results at the county level.

*IA.1 Sample Construction*

In this section, I describe how I construct my sample. I begin by obtaining detailed home loan data from the Home Mortgage Disclosure Act (HMDA) database. Congress enacted HMDA in 1975 to improve public reporting of mortgage loans, and U.S. financial institutions are required to report HMDA data to their regulators if they meet certain criteria, such as a threshold for asset size and whether the institution has a home office or branch in a Metropolitan Statistical Area (MSA).[1,2] This is an annual database containing information on loan applications (regardless of whether or not they were approved), borrower demographics, lender details, and loan specifics such as loan amount and geographic location of the property.

This database provides a comprehensive coverage of the mortgage market. For example, Avery et al. (2010) note that in 2008, commercial banks filing HMDA carried 93% of the total mortgage dollars outstanding on commercial bank portfolios at the time. Although lenders with offices only in non-metropolitan areas are exempt from filing HMDA, as Dell'Ariccia, Igan, and Laeven (2012) note, 83.2% of the population in 2006 lived in metropolitan areas. Therefore, the data in HMDA are well representative of the residential mortgage lending activity in the U.S.

I then obtain lender information from the HMDA Lender file, constructed by Robert Avery at Federal Housing Finance Agency (FHFA).[3] This file gives information on the type of lender, such as whether it is a commercial bank or an independent mortgage bank. It also

[1] This law was enacted to ensure that lenders were serving the housing needs of their communities in an indiscriminatory way.

[2] Any depository institution that has a home office or branch in an MSA is required to file HMDA if it has made a home purchase loan on a one to four-unit dwelling or has refinanced a home purchase loan, and has assets above an annually adjusted threshold. Every December, the Consumer Financial Protection Bureau announces the threshold for the following year. For example, in 2007, this threshold was $36 million. Any non-depository institution (e.g., a mortgage company that does not accept deposits but raises funds for lending by borrowing from banks or capital markets) is required to file if at least 10% of its loan portfolio is composed of home purchase loans, and if it holds assets exceeding $10 million. See Dell'Ariccia, Igan, and Laeven (2012).

[3] This file is available at Neil Bhutta's webpage: https://sites.google.com/site/neilbhutta/data

matches every lender who filed a HMDA report in and after 1993 with the identification code (RSSD) used by the Federal Reserve. If a HMDA lender is a commercial bank, it provides RSSD for the bank. If the lender is a subsidiary of a bank, it matches the lender to the bank, and if it is a subsidiary of a bank holding company, it matches the lender to the lead bank in the holding company. If the lender is merged into another institution, the lender is matched with the acquiring institution.

Combining loan data from the HMDA loan files with lender information from the HMDA lender file, I construct a sample of non-trivial loans (loans greater than $50,000) that commercial banks originated during calendar years 2003 (start of shale boom) through 2017. I focus on commercial banks and remove all non-bank lenders, because most non-bank lenders fund mortgage lending with securitization (Gilje, Loutskina, and Strahan (2016)) such that their lending behavior is highly affected by funding conditions in the securitization market. As discussed in the main body of the paper, I focus on states with major shale activity, so I filter for loans originated in Arkansas, Louisiana, North Dakota, Oklahoma, Pennsylvania, Texas, and West Virginia.

For any given year, I limit my sample to lenders that filed HMDA in the prior year; that is, a lender in my sample originated at least one loan in the previous year.[4] This filter avoids any bias on loan growth due to lenders newly entering the business of mortgage lending. I keep conventional (loans not insured by government agencies) and Federal Housing Administration (FHA) loans, and drop loans guaranteed by Veteran's Affairs (VA) and Farmers Home Administration (FmHA).[5] HMDA also provides information on whether loans are sold as of the

[4] The total number of loans drops by 9.4% after filtering for lenders that originate at least one loan in the prior year.
[5] Filtering out VA and FmHA loans drops an additional 1.4% of loans.

calendar year end, and because loans that are originated to be sold are immediately sold within few months, I classify loans that are not sold within the given calendar year as loans that banks retain on their balance sheet (Rosen (2011), Berrospide, Black, and Keeton (2016), Duchin and Sosyura (2014), Gilje, Loutskina, and Strahan (2016)).

Next, using bank RSSD ID for each HMDA lender from the lender file, I match the lender to the highest bank holding company in the year of observation, and treat all banks belonging to the same bank holding company as one bank. This ensures that I capture connectedness of a bank properly. For example, two banks that do not appear linked because they operate in different counties may, in fact, be linked via another bank within the same bank holding company. Working at the bank holding company level avoids such issues. I aggregate lending by banks at the bank holding company level and study changes in lending at this level.

For each bank RSSD each year, I also obtain data on branch location and deposit amounts as of June 30 of a given year from the Summary of Deposits provided by the Federal Deposit Insurance Corporation (FDIC). For banks belonging to the same holding company, I sum up deposits at the holding company level. I use this information on branch deposits to construct geographic linkages and to capture a bank's exposure to boom counties, as described in the main body of the paper.

To construct bank control variables, I obtain data from the call report database (Report of Condition and Income), which provides detailed information on a bank's income statement, and on-balance sheet and off-balance sheet items. All financial institutions regulated by the Federal Reserve Bank, Federal Deposit Insurance Corporation (FDIC), and the Office of the Comptroller of the Currency (OCC) are required, on a quarterly basis, to file these reports. These reports are publicly available through the Federal Reserve Bank of Chicago. The control variables that I construct are described in detail in the main body of the paper. Furthermore, the construction of variables that capture market characteristics of counties is described in the main body of the paper.

*IA.2 Shale Shock as a Positive Shock*

In this section, I provide evidence that shale shock is a positive shock to bank liquidity by showing that it leads to increases in liquidity inflows in banks in the form of greater deposits.[6] Banks receive greater deposits as landowners in boom counties deposit the cash windfalls they receive from oil companies or use them to pay back their outstanding loans (Gilje, Loutskina, and Strahan (2016)). I show that bank deposits increase over time as bank exposure to shale well activity increases.

I capture a bank's exposure to shale well activity using several variables. First, I use a bank's contemporaneous share of deposits in boom counties. Given that the contemporaneous share of deposits could be affected by new deposits from the shock itself, I also consider lagged share of deposits in boom counties as my second measure. Third, I use OWN_BOOM_EXPOSURE, which is constructed as the weighted average of log of cumulative count of wells in local boom counties of a bank in a given year, where weights are the shares of deposits that the bank holds in each county each year (described in detail in Section IV.1). Again, because this measure uses contemporaneous shares of deposits as weights that could be affected by new deposits, I also consider OWN_BOOM_EXPOSURE that uses lagged shares of deposits as weights as my fourth measure.

I estimate the following model:

[6] I thank the anonymous referee for suggesting this test.

(i) $$\Delta log(\text{DEPOSITS})_{i,t} = \alpha + \beta \text{OWN\_EXPOSURE\_TO\_SHALE\_WELL\_ACTIVITY}_{i,t} + Bank\ Controls_{i,t-1} + Bank\ F.E + Year\ F.E + \varepsilon_{i,t}$$

where $\Delta log(\text{DEPOSITS})_{i,t}$ is the change in the natural logarithm of deposits from 2002 (one year before the start of shale boom) to year *t* for bank *i*, and $\text{OWN\_EXPOSURE\_TO\_SHALE\_WELL\_ACTIVITY}_{i,t}$ is one of the variables described above. I also include lagged bank control variables, bank and year fixed effects, and cluster standard errors by bank. This regression includes all shocked and non-shocked banks. Furthermore, a potential issue in this study is selection bias from banks selecting into boom counties in order to gain from cash windfalls there, so I filter for banks that were local in a given county in 2002. Table IA.1 presents the results.

Column 1 uses contemporaneous share of deposits in boom counties as a proxy for a bank's exposure to shale well activity. Results show that deposits grow at a greater rate as the share of deposits in boom counties increases. To understand the economic magnitude of the results, consider a bank that has an average share of deposits in boom counties (=10.7%) and a bank that is not shocked. Compared to the non-shocked bank, deposits at the shocked bank grow at a 1.4 (=$e^{(0.130*0.107)}$-1) percentage points faster rate.[7] This result is in line with the results documented in Gilje, Loutskina, and Strahan (2016). Column 2 shows that these results are robust to using lagged share of deposits in boom counties.

Column 3 uses OWN_BOOM_EXPOSURE as a proxy for a bank's exposure to shale well activity. According to this column, compared to a non-shocked bank, deposits at a shocked bank

[7] Given the sample mean of 0.58 for Δlog(DEPOSITS), this difference in deposit growth rate corresponds to a log change that is 2.4% (=0.130*.107/0.58) of the sample mean.

having average OWN_BOOM_EXPOSURE observes increases in deposits at a 1.7 percentage points faster rate.[8] Finally, column 4 shows that these results are robust to using OWN_BOOM_EXPOSURE that is reconstructed using lagged deposit shares as weights.[9]

*IA.3 Construction of Boom Exposure of Linkages*

In this section, I describe the construction of OWN_BOOM_EXPOSURE in the hypothetical network presented in Figure 1. This network consists of two banks – one shocked and one not shocked. Let X be a non-shocked bank, local in counties *a* and *b,* where both counties are non-boom counties. Let Y be a shocked bank, also local in *a* and *b*. In addition to *a* and *b,* Y is local in other counties, including boom counties (not shown). Solid arrows represent lending in a market, and the numbers along the arrows represent a bank's shares of deposits in the markets. For instance, X holds 60% of its deposits in county *a*. This value represents X's exposure to *a* and thus its exposure to the local banks in *a*.

In this example, the non-shocked bank X is the subject bank – the one that is on the receiving end of spillovers and the one whose lending behavior I study. Bank Y is X's linkage bank. The first step in the construction of OWN_BOOM_EXPOSURE is to find Y's exposure to well activity in boom counties. I compute Y's weighted average exposure to the natural logarithm of cumulative well count in boom counties in a given year, where weights are Y's shares of

[8] Given the sample mean of 0.58 for Δlog(DEPOSITS) and 0.4503 for OWN_BOOM_EXPOSURE, this difference in deposit growth rate corresponds to a log change that is 2.8% (=0.0364*0.4503/0.58) of the sample mean.

[9] Later bank-year level regressions in the main body of the paper (e.g., Table 6 and 9) include control variables for contemporaneous market characteristics that a bank is exposed to since those regressions study a bank's exposure to home price changes or changes in a bank's lending behavior that one would expect to be affected by market characteristics. These variables are weighted average of market characteristics in counties where a bank is local, weights being the shares of deposits that the bank holds in each market. These market characteristics include log(population), log(per capita personal income), household debt-to-income ratio, unemployment rate, percent female population, percent minority population, and lagged percent change in home prices from the prior year. In unreported tables, I confirm that the regressions here that study changes in deposits are also robust to including controls for average market characteristics.

deposits in each boom county that year. In Figure 1, $Boom\ Exp_t^Y$ is Y's exposure to well activity in year *t*.

In the second step, I assign a weight to $Boom\ Exp_t^Y$ to capture X's sensitivity to spillovers from Y. X is linked with Y via counties *a* and *b*. To capture X's sensitivity to Y via county *a*, I weigh $Boom\ Exp_t^Y$ by the product of deposit shares of X and Y in *a* (i.e., 0.6 x 0.1), and to capture X's sensitivity to Y via county *b*, I use the product of deposit shares of X and Y in *b* (i.e., 0.4 x 0.2). Thus, the markets where X is more exposed to Y and the markets where X is likely to feel the shock of Y more are weighed more. So the weighted boom exposure of linkage Y is (0.6 * 0.1 + 0.4 * 0.2)$Boom\ Exp_t^Y$. Because this is a world of only two banks, and Y is X's only linkage, this expression is the final expression for BOOM_EXPOSURE_OF_LINKAGES for X (see Figure 1, part (i)).

Figure 1, part (ii) then extends this network to a network consisting an additional shocked bank Z, which is also local in counties *a* and *b*. In this case, BOOM_EXPOSURE_OF_LINKAGES for X is the weighted average of boom exposures of Y and Z. The expression for boom exposure of bank Z (i.e., $Boom\ Exp_t^Z$) weighed by X's sensitivity is (0.6 * $w_a^Z$ + 0.4 * $w_b^Z$ ) $Boom\ Exp_t^Z$ where $w_a^Z$ and $w_b^Z$ are the fractions of deposits that Z holds in counties *a* and *b*. The expression for weighted boom exposure of Y is as computed previously.

In the final step, I sum up weighted boom exposures of Y and Z. The final expression for BOOM_EXPOSURE_OF_LINKAGES for X is (0.6 * 0.1 + 0.4 * 0.2) $Boom\ Exp_t^Y$ + (0.6 * $w_a^Z$ + 0.4 * $w_b^Z$ ) $Boom\ Exp_t^Z$. This network can be extended to *n* banks, and BOOM_EXPOSURE_OF_LINKAGES for X in this network can be computed similarly.

*IA.4 Robustness Tests*

This section presents robustness tests of the baseline regressions presented in the main body of the paper.

*IA.4.1 Close Proximity to Boom Counties*

In my empirical analysis, I focus on studying the lending behavior of non-shocked banks in non-boom counties, thus separating bank-to-bank spillovers from the confounding effect of a bank's own exposure to boom events and direct demand effects of boom counties. However, even if a county is not shocked, it may still experience demand spillovers from neighboring boom counties. In order to address concerns of this confounding effect, I drop all county-year observations for counties that are in close proximity to a boom county. Specifically, I drop counties that are within 100 miles of any boom county. Column 1 of Table IA.2 presents the results and shows that results continue to hold. Moreover, the elasticity coefficient of BOOM_EXPOSURE_OF_LINKAGES increases in economic magnitude after dropping counties in close proximity to a boom county.

*IA.4.2 Selection into Counties where Shocked Banks are Located*

It is also possible that banks may select into counties where shocked banks are present if they expect market conditions in those counties to improve due to the lending behavior of shocked banks. If this selection is motivated by loan demands in the area, then my results would be due to both supply and demand effects. I address this confounding effect in the following way: For every non-shocked subject bank, I find the first year that one of its linkage banks is shocked. Then, I filter for county-year observations only for those counties where this bank was local as of this year. In other words, I study the lending behavior of non-shocked banks only in counties where the banks were already local when one of their linkages were first shocked. Column 2 of Table IA.2 presents the results and shows that the elasticity coefficient of BOOM_EXPOSURE_OF_LINKAGES remains statistically significant and increases slightly in economic magnitude.

### *IA.4.3 Housing Market Conditions of Own Markets*

Next, I present robustness tests to further address potential confounding effects from the subject bank's own market exposure. Rather than responding to spillovers from linkage banks, the concern is that the subject bank increases home lending because the market under consideration is doing well. As mentioned before, all regressions include county-year fixed effects, which absorb market effects of the county in question. In this subsection, I present additional robustness tests that show that my results are not simply due to the subject bank's own market exposure.

In column 3 of Table IA.2, I interact my main independent variable with a dummy variable that identifies "good" and "bad" markets. Good markets are counties that undergo above median percent changes in home prices in the prior year, and bad markets are those that undergo below median percent changes in home prices in the prior year. Over my sample period, the median lagged percent change in home prices is 2.5%. Any confounding effect from own market exposure implies that banks increase lending more in good markets. However, column 3 of Table IA.2 shows that this interaction term is statistically insignificant, implying that spillovers in good markets are statistically indistinguishable from those in bad markets. Moreover, the magnitude of this interaction term is economically insignificant.

In column 4, I exclude the 15 best performing markets each year. These markets are counties that observe the largest percent changes in home prices in the previous year. Results continue to hold even after dropping these markets. The elasticity coefficient of BOOM_EXPOSURE_OF_LINKAGES is statistically significant and the magnitude is similar to the base regression of Table 3, column 2, implying that the results documented here are not simply due to good housing market conditions in counties under consideration.

*IA.4.4 Market Size Effects*

Furthermore, it is possible that the size of the housing markets may bias my results. One could argue that the results could be due to large markets. For instance, banks may engage in lending mostly in large markets because housing demand is generally higher in large markets, in which case my results would be confounded by demand effects. I address this concern by removing the 15 largest counties by loan count in the prior year, and present results in column 5 of Table IA.2. I find that results persist. The elasticity coefficient of BOOM_EXPOSURE_OF_LINKAGES is statistically significant and the economic magnitude increases after removing the largest markets.

Yet another possibility is that results are driven by the smallest counties. Banks may not engage in much lending in small counties, such that the growth in lending in these counties is based on few loans, thus adding noise to my results. This could bias the magnitude of the coefficient of BOOM_EXPOSURE_OF_LINKAGES. In column 1 of Table IA.4, I remove the 15 smallest counties by loan count in the previous year. I find that results persist, and that the magnitude of the coefficient is similar to the one in the base regression of Table 3, column 2. I conduct an additional test where I drop all bank-county-year observations based on fewer than 15 loans, and present results in column 2 of Table IA.4. The magnitude of the coefficient increases in this test and the result is statistically significant. Therefore, it is unlikely that my results are due to noise in the measurement of bank loan growth.

### *IA.4.5 Subject and Linkage Bank Size Effects*

In this subsection, I address the possibility that large banks may bias my results. First, large banks, because of their size, operate in a larger number of markets compared to small banks, such that they have a greater probability of having some exposure to shocked banks. Therefore, the results of my paper could be driven by large banks. Large banks also have greater capital and wider access to the capital markets, allowing them to increase lending faster than the rest of the banks, thus confounding the results of spillovers in this paper. I address this concern here.

First, in column 1 of Table IA.3, I include an interaction between BOOM_EXPOSURE_OF_LINKAGES and a dummy variable ABOVE_MEDIAN_SIZE, which identifies banks that have above median asset size each year. Results show that this interaction term is statistically insignificant, implying that the response of large banks is statistically not different from that of small banks. In column 2 of Table IA.3, I remove large banks from the regression. Specifically, I remove banks that have greater than $50 billion in assets. This cutoff corresponds to the asset size cutoff used by the Federal Reserve to define large banks. As the results show, the elasticity coefficient on BOOM_EXPOSURE_OF_LINKAGES remains statistically significant and the magnitude of the coefficient is similar to that in the base regressions of Table 3.

Next, I test the robustness of my results to size effects of linkage banks. Because large banks tend to operate in a large number of markets as mentioned before, I address concerns that my results might be driven by exposure to large linkages only. I reconstruct BOOM_EXPOSURE_OF_LINKAGES by removing very large linkages, i.e., linkages that have more than $250 billion in total assets. This cutoff corresponds to the asset size cutoff used by the Federal Reserve to define very large banks. As the summary statistics in Table 1 show, shocked banks are generally larger than non-shocked banks in my sample. Using a higher size cutoff allows me to keep enough number of shocked banks in my sample to study spillover effects. I present results in Column 3 of Table IA.3. Results are still statistically significant and the economic magnitude, while slightly smaller, is similar to the one in the base regression of Table 3, column 1.

Alternatively, small banks could also bias my results. Small banks generally have greater variation in loan growth that could bias the magnitude of regression coefficients. I address this concern by removing very small banks, defined as banks that have less than $100 million in total assets. I present results in the final column of Table IA.3. I find that BOOM_EXPOSURE_OF_LINKAGES remains statistically significant, and that the economic magnitude is similar to the ones in the base regressions.

Finally, I address concerns that loan growth observations of banks that create small numbers of loans could add noise to my results. To that end, I remove the 15 smallest banks by loan count (total number of loans originated at the bank level) in the previous year. I present results in Table IA.4, column 3. Results show that BOOM_EXPOSURE_OF_LINKAGES continues to remain statistically significant and is slightly larger than the coefficient in the base results of Table 3.

### *IA.4.6 Alternate Independent Variable*

In Table IA.5, I consider an alternate definition for my main independent variable. I reconstruct BOOM_EXPOSURE_OF_LINKAGES to capture linkage exposure to percent *growth* in the number of shale wells, rather than the cumulative count of shale wells. I call this variable BOOM_GROWTH_EXPOSURE_OF_LINKAGES. The construction of this variable is similar to BOOM_EXPOSURE_OF_LINKAGES except that, now, I consider a linkage bank's exposure to log change in the count of shale wells from 2003 (start of shale boom). For example, in the hypothetical network of Figure 1, I compute boom growth exposure of bank Y as follows:

(ii) $$\text{BOOM\_GROWTH\_EXP}_t^Y = \sum_c w_{c,t}^Y \left[ \log\left(c\_wells_{c,t}\right) - \log\left(c\_wells_{c,2003}\right)\right]$$

The rest of the construction of BOOM_GROWTH_EXPOSURE_OF_LINKAGES regarding the assignment of weights for the importance of the overlapping market to the subject bank and the linkage banks is similar to the one for BOOM_EXPOSURE_OF_LINKAGES.

Table IA.5 presents the base tests of Table 3 as well as the tests for retained and sold loans using this alternate independent variable. I find that my results remain. BOOM_GROWTH_EXPOSURE_OF_LINKAGES is positive and statistically significant, and this result is driven by spillovers coming from large linkages.[10] Compared to a bank that has BOOM_GROWTH_EXPOSURE_OF_LINKAGES at the mean value (=0.804), a bank that has a value one standard deviation higher (=0.804+1.621=2.425) increases its lending by 12.07 percentage points more. Furthermore, columns 3 and 4 indicate that the results are driven by increases in retained loans as opposed to sold loans, consistent with prior results.

[10] Note that the number of observations in Table IA.5 is different from that in the base regressions in Table 3. This is because the regressions in Table IA.5 drop observations for 2003 given that boom growth is measured relative to 2003 and BOOM_GROWTH_EXPOSURE_OF_LINKAGES would, therefore, be 0 for any observation in 2003.

In unreported tables, I consider yet another definition for my independent variable. This variable is similar to BOOM_EXPOSURE_OF_LINKAGES except that I redefine boom counties using the definition in Gilje, Loutskina, and Strahan (2016). Specifically, I redefine a boom county as a county that has more than 17 shale wells in a given year. I find that results are robust to this alternate definition as well.

*IA.5 Alternate Mechanisms of Spillovers*

This section discusses alternate mechanisms for spillovers documented in this paper.

*IA.5.1 Liquidity Channel*

First, I discuss the possibility that the same mechanism that causes shocked banks to increase lending in non-boom counties also causes spillovers from shocked to non-shocked banks in non-boom counties. Gilje, Loutskina, and Strahan (2016) argue that banks "export" liquidity from boom counties to non-boom counties because a liquidity shock allows banks to originate loans that were previously difficult to originate due to contracting frictions. In the context of this paper, one could argue, for example, that the increases in home prices due to the lending behavior of shocked banks could lead homeowners to sell their homes, resulting in prepayments and thus an influx of liquidity for non-shocked banks. Increased liquidity could then lead these non-shocked banks to originate loans that they were previously not able to ("liquidity channel").

However, the results already presented in the main body of the paper contradict this argument. First, banks increase lending only in markets where shocked banks exist locally. However, if spillovers were to occur via the "liquidity channel," there is no obvious reason why banks would increase lending only in markets where shocked banks are present and not where they are not present. Banks should be able to "export" liquidity from one market to another irrespective of whether shocked banks exist or not. Second, the "liquidity channel" also implies that financially constrained banks increase lending more. These banks should have more trouble originating new loans, and increased liquidity should allow them to increase lending to a greater extent than banks that have financial slack. However, I find that spillovers are driven by banks with financial slack.

### *IA.5.2 Investor Channel*

Alternatively, one could argue that the results are due to investors who provide funds to banks. Specifically, these investors could learn from the lending behavior of shocked banks and the subsequent positive impact on home prices. They could then increase their funds to non-shocked banks, who then expand lending. In other words, the results could represent investor effects as opposed to bank effects. In order to test this hypothesis, I consider the behavior of banks dependent on wholesale funds and study how they respond to spillovers. Because wholesale funds are short term and less risky, it is easy for wholesale fund investors to quickly increase their supply of funds to banks if they believe spillovers have a positive impact on the housing markets. In this case, banks dependent on wholesale funds would respond more strongly to spillovers.

Therefore, for each bank in each year, I construct *Wholesale-to-Assets Ratio*. Wholesale funds include large-time deposits, deposits booked in foreign offices, subordinated debt and debentures, gross federal funds purchased, repos, and other borrowed money (includes commercial papers) (Acharya and Mora (2015)). Then I construct a dummy variable

HIGH_WHOLESALE-TO-ASSETS_RATIO, which identifies banks having above median *Wholesale-to-Assets Ratio* in a given year. In column 1 of Table IA.6, I include an interaction between BOOM_EXPOSURE_OF_LINKAGES and HIGH_WHOLESALE_DEPENDENCE, where HIGH_WHOLESALE_DEPENDENCE is the variable HIGH_WHOLESALE-TO-ASSETS_RATIO just described. I find that this interaction term is statistically and economically insignificant.

Given prior results where banks with financial slack increase lending only in bad economies, I test whether wholesale fund dependent banks behave similarly and increase their lending only in bad economies. To that end, in column 2, I include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BORROWER_CREDIBILITY, and HIGH_WHOLESALE_DEPENDENCE, where BORROWER_CREDIBILITY is UNEMPLOYMENT_RATE and HIGH_WHOLESALE_DEPENDENCE is HIGH_WHOLESALE-TO-ASSETS_RATIO. However, this interaction term is not statistically significant. Moreover, this interaction term is negative, inconsistent with wholesale dependent banks increasing lending in bad economies as a function of boom exposure of linkages.

Following the same vein of tests as before, I now consider BANK_UNEMPLOYMENT_EXP, and ask whether banks that operate more in bad economies and that are dependent on wholesale funds increase lending more in response to shock exposure of linkages. Specifically, in column 3, I include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BORROWER_CREDIBILITY, and HIGH_WHOLESALE_DEPENDENCE, where BORROWER_CREDIBILITY is BANK_UNEMPLOYMENT_EXP and HIGH_WHOLESALE_DEPENDENCE is HIGH_WHOLESALE-TO-ASSETS_RATIO. Again, this term is statistically insignificant and negative.

In columns 4, 5, and 6, I repeat the tests using an alternate measure for wholesale dependence of banks. I use *Core Deposits-to-Assets* ratio, which I construct for each bank each year. The literature argues that banks that rely less on core deposits rely more on wholesale funds and use core deposits-to-assets ratio as a measure to capture reliance on wholesale funding (Dagher and Kazimov (2015)). Core deposits include transaction deposits, savings deposits, and time deposits less than $100,000 (Acharya and Mora (2015)). I then construct a dummy variable HIGH_CDA, which identifies banks having above median *Core Deposits-to-Assets Ratio* in a given year.

Column 4 considers the double interaction between BOOM_EXPOSURE_OF_LINKAGES and HIGH_CDA*;* column 5 considers a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, UNEMPLOYMENT_RATE, and HIGH_CDA*;* and column 6 considers a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BANK_UNEMPLOYMENT_EXP, and HIGH_CDA. Again, none of these interaction terms are statistically significant. While the signs of the interaction terms are consistent with arguments of investor effects, the statistical insignificance of these terms, combined with the results in columns 1 through 3, render these arguments weak.

### *IA.5.3 Bank Health*

Another hypothesis that seems consistent with the results is that rising home prices improve the value of under-water loans originated in depressed areas and held on bank balance sheets, and that the resulting improvement in bank health allows banks to lend more.[11] However, the finding that the better capitalized banks drive spillovers is inconsistent with this hypothesis. Nevertheless, I conduct additional tests to address this hypothesis.

[11] I thank the anonymous referee for suggesting this alternate explanation for spillovers.

First, I compare the behavior of banks that had exposure to greater home price declines versus those that had exposure to smaller home price declines in the prior year. If the alternate hypothesis is driving the results, then one can expect banks with exposure to greater home price declines in the prior year to experience more improvement in their health when home prices increase in the current year, thus allowing them to lend more.

Therefore, for each bank in each year, I construct a dummy variable LOW_BANK_EXPOSURE_TO_ΔHPI(%), which identifies banks that have below median value for LAGGED_BANK_EXPOSURE_TO_ΔHPI(%). LAGGED_BANK_EXPOSURE_TO_ΔHPI(%) is computed as the weighted average of percent changes in home prices in the bank's local markets in the prior year, where weights are the bank's shares of deposits in each market. In column 1 of Table IA.7, I include an interaction between BOOM_EXPOSURE_OF_LINKAGES and LOW_BANK_HEALTH, where LOW_BANK_HEALTH is the variable LOW_BANK_EXPOSURE_TO_ΔHPI(%). I find that this interaction term is statistically insignificant. Furthermore, the sign of the estimate is negative inconsistent with the alternate hypothesis.

Given prior results where banks with financial slack increase lending only in bad economies, I test whether banks having LOW_BANK_EXPOSURE_TO_ΔHPI(%)=1 behave similarly and increase lending only in bad economies. To that end, in column 2, I include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BORROWER_CREDIBILITY, and LOW_BANK_HEALTH, where BORROWER_CREDIBILITY is UNEMPLOYMENT_RATE and LOW_BANK_HEALTH is LOW_BANK_EXPOSURE_TO_ΔHPI(%). However, this interaction term not statistically significant and negative.

Similar to prior tests, in column 3, I test whether banks having LOW_BANK_EXPOSURE_TO_ΔHPI(%) increase lending only if they have high exposure to bad economies. So I include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BORROWER_CREDIBILITY, and LOW_BANK_HEALTH, where BORROWER_CREDIBILITY is BANK_UNEMPLOYMENT_EXP and LOW_BANK_HEALTH is LOW_BANK_EXPOSURE_TO_ΔHPI(%). Again, this term is statistically insignificant and negative.

Next, I compare the behavior of banks that had good asset quality versus those that had bad asset quality in the prior year. If spillovers occur because of an improvement in the value of assets, which mainly consist of loans in a bank, then banks that have bad asset quality ex-ante should respond more strongly to spillovers. Therefore, for each bank in each year, I construct a dummy variable LOW_ASSET_QUALITY, which identifies banks having above median value for ASSET_QUALITY in the prior year, where ASSET_QUALITY is the ratio of total loan charge-offs to total loan value. Because data on charge-offs on just mortgage loans are missing for many observations, this variable includes total charge-offs on all loans.

In columns 4-6, I repeat the tests in 1-3 using LOW_ASSET_QUALITY for LOW_BANK_HEALTH. Column 4 shows that the interaction between BOOM_EXPOSURE_OF_LINKAGES and LOW_ASSET_QUALITY is statistically and economically insignificant. Column 5 shows that the triple interaction between BOOM_EXPOSURE_OF_LINKAGES, UNEMPLOYMENT_RATE, and LOW_ASSET_QUALITY is not statistically significant. This interaction term is also negative, inconsistent with low asset quality banks increasing lending in bad economies as a function of boom exposure of linkages. Finally, column 6 shows that the triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BANK_UNEMPLOYMENT_EXPOSURE, and LOW_ASSET_QUALITY is statistically and economically insignificant.

### *IA.6. County Aggregates*

In this section, I study aggregate lending of all non-shocked banks in each non-boom county. Such a study will shed light on the aggregate economic magnitude of spillovers at the county level. It is possible that banks that have higher BOOM_EXPOSURE_OF_LINKAGES simply outcompete others with lower BOOM_EXPOSURE_OF_LINKAGES in picking up loan demand. They may be able to do so if they happen to have stronger branch presence (and therefore higher BOOM_EXPOSURE_OF_LINKAGES) in areas where shocked banks are present. Stronger branch presence implies easier access to borrowers and greater information advantage. If banks compete away loans from one another, there may be no net increase in lending on an aggregate county level amongst the non-shocked banks. In this subsection, I show that there is an economically significant impact of spillovers at the county level and that banks are not simply outcompeting one another.

To that end, I first construct loan growth at the county level. As before, I only consider non-shocked banks in non-boom counties in order to separate the impact of own shock exposure from the impact of spillovers. I take the size-weighted average of loan growth (log change in loans originated) of all non-shocked banks in a non-boom county each year. I also construct BOOM_EXPOSURE_OF_LINKAGES at the county level. I construct this variable as the size-weighted average of BOOM_EXPOSURE_OF_LINKAGES of all non-shocked banks in each county and year. These variables are summarized in Table IA.8. Then I study how this county-level BOOM_EXPOSURE_OF_LINKAGES affects county-level growth in lending.

Because this study is at a county-year level, there is no way to fully absorb market effects as in the base regressions that include county-year fixed effects. Instead, I include state-year fixed effects. Specifically, I estimate the following model:

(iii)
$$\Delta\log\left(\mathrm{MORTGAGE\_LENDING}_{c,t}\right) = \alpha + \beta\mathrm{COUNTY\_BOOM\_EXPOSURE\_OF\_LINKAGES}_{c,t} + Market\ Controls_{\ c,t} + State - Year\ F.E + \varepsilon_{c,t}$$

where $\Delta\log\left(\mathrm{MORTGAGE\_LENDING}_{c,t}\right)$ and $\mathrm{COUNTY\_BOOM\_EXPOSURE\_OF\_LINKAGES}_{c,t}$ are constructed as described above for each non-boom county *c* in each year *t*. In addition to state-year fixed effects, I also include controls for contemporaneous market characteristics, which include log(POPULATION), log(PER_CAPITA_PERSONAL_INCOME), household DEBT-TO-INCOME ratio, UNEMPLOYMENT_RATE, PERCENT_FEMALE_POPULATION, PERCENT_MINORITY_POPULATION, and lagged percent change in home prices. And I cluster standard errors by county.

Table IA.9 presents the results.[12] Column 1 presents results for growth of all loans in the sample; column 2 presents results for retained loans; and column 3 presents results for sold loans. As column 1 shows, there is an aggregate increase in lending at the county level as a function of COUNTY_BOOM_EXPOSURE_OF_LINKAGES. A county that has a value one standard deviation (=0.949) higher than the mean (=0.479) for COUNTY_BOOM_EXPOSURE_OF_LINKAGES observes 19 percentage points more growth in lending than a county that has

[12] For reasons similar to the ones described in footnote 15 in the main body of the paper, this table has more observations than the ones in the summary statistics of Table IA.8. To be consistent with the sample used for the summary statistics in Table 1, Table IA.8 presents county characteristics for the sample of non-shocked banks in non-boom counties used in the base regressions of model (7). The county characteristics summarized are for those counties that remain after singleton observations are dropped in that sample. In Table IA.8, I use all county-year observations, including those that are dropped in the base regressions, although any singletons in the county-year level regressions of model (iii) are dropped, resulting in the differences in the number of observations here versus Table IA.8. Just as in footnote 15, I rerun the tests of Table IA.9 for only those county-year observations that are included in the sample used in the base regressions and find similar results (in unreported tables).

COUNTY_BOOM_EXPOSURE_OF_LINKAGES at the mean. Columns 2 and 3 show that these results are being driven by growth in retained loans, consistent with prior results. Therefore, these results show that positive spillovers have a significant on-balance sheet impact on an aggregate county level, and that non-shocked banks are not simply outcompeting one another.

**Table IA.1: Shale Shock as a Positive Shock**

This table reports regressions (at the bank-year level) of a bank's percent growth in deposits from year 2002 to year *t* on the bank's OWN_EXPOSURE_TO_SHALE_WELL_ACTIVITY. The sample in this regression includes both shocked and non-shocked banks with branch presence in non-boom counties, and the sample period includes years from 2003 to 2017. Column 1 uses contemporaneous share of deposits in boom counties as a proxy for a bank's exposure to shale well activity, while column 2 uses lagged share of deposits in boom counties. Column 3 uses OWN_BOOM_EXPOSURE as the proxy. This variable captures a bank's weighted average exposure to well activity in boom counties where weights are the bank's shares of deposits in each county (described in detail in the text). Column 4 uses OWN_BOOM_EXPOSURE that uses lagged shares of deposit as weights in its construction. All regressions include bank and year fixed effects. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | $\Delta$log(DEPOSITS)$_{02\text{-}t}$ | | | |
|---|---|---|---|---|
| | Share of Deposits in Boom Counties | Lagged Share of Deposits in Boom Counties | Own Boom Exposure | Own Boom Exposure (lagged weights) |
| | 1 | 2 | 3 | 4 |
| OWN_EXPOSURE_TO_SHALE_WELL_ACTIVITY | 0.130*** | 0.119*** | 0.0364*** | 0.0343*** |
| | (3.828) | (3.778) | (3.851) | (3.869) |
| log(TOTAL_ASSETS) | 0.719*** | 0.720*** | 0.713*** | 0.715*** |
| | (15.332) | (15.381) | (15.048) | (15.122) |
| NET_INCOME/ASSETS | 0.0552 | 0.0758 | 0.0285 | 0.0573 |
| | (0.052) | (0.071) | (0.027) | (0.054) |
| CAPITAL/ASSETS | -0.833* | -0.831* | -0.873** | -0.876** |
| | (-1.914) | (-1.914) | (-2.001) | (-2.010) |
| ASSET_QUALITY | 0.300 | 0.298 | 0.271 | 0.273 |
| | (0.278) | (0.276) | (0.251) | (0.253) |
| MORTGAGES/ASSETS | 0.0997 | 0.0986 | 0.0998 | 0.0976 |
| | (0.679) | (0.670) | (0.677) | (0.660) |
| LIQUIDITY_RATIO | -0.00175 | -0.000196 | -0.00921 | -0.00700 |
| | (-0.021) | (-0.002) | (-0.110) | (-0.083) |
| UNUSED_COMMITMENTS_RATIO | 0.521*** | 0.522*** | 0.522*** | 0.523*** |
| | (3.143) | (3.153) | (3.125) | (3.136) |
| ALL/ASSETS | -2.420 | -2.445 | -2.276 | -2.298 |
| | (-0.910) | (-0.920) | (-0.857) | (-0.866) |
| C&I_LOANS/ASSETS | 0.458** | 0.457** | 0.455** | 0.453** |
| | (2.583) | (2.573) | (2.543) | (2.529) |
| Constant | -8.851*** | -8.864*** | -8.774*** | -8.789*** |
| | (-13.936) | (-13.968) | (-13.666) | (-13.715) |
| Bank Fixed Effects | Y | Y | Y | Y |
| Year Fixed Effects | Y | Y | Y | Y |
| Observations | 5940 | 5940 | 5940 | 5940 |
| Adjusted R-squared | 0.899 | 0.899 | 0.900 | 0.900 |

**Table IA.2. Robustness Test: Market Effects**

This table reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_EXPOSURE_OF_LINKAGES in various robustness specifications. BOOM_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Column 1 excludes counties that are within 100 miles of boom counties. Column 2 keeps only counties where the subject bank is local as of the first year one of its linkages is shocked. Column 3 includes an interaction between BOOM_EXPOSURE_OF_LINKAGES and a dummy variable GOOD_MARKET, which identifies markets that have above median percent changes in home prices in the previous year. Column 4 excludes the 15 best performing markets; these are the markets that observe the largest percent changes in home prices in the previous year. Column 5 excludes the 15 largest markets by loan count each year. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | | | |
|---|---|---|---|---|---|
| | Drop Markets Close to Boom Markets | Local Markets as of the year of First Linkage Shock | Good vs Bad Markets | Remove Best Markets | Remove Largest Markets by Loan Count |
| | 1 | 2 | 3 | 4 | 5 |
| BOOM_EXPOSURE_OF_LINKAGES | 0.862** | 0.0767* | 0.0641 | 0.0628** | 0.136*** |
| | (2.131) | (1.891) | (1.324) | (1.987) | (2.605) |
| BOOM_EXPOSURE_OF_LINKAGES X GOOD_MARKET | | | 0.000176 | | |
| | | | (0.003) | | |
| EXPOSURE_TO_ΔHPI(%)_IN_OTHER_MARKETS | -0.0267 | 1.502 | 2.145 | 2.635* | 1.616 |
| | (-0.016) | (1.085) | (1.572) | (1.844) | (1.015) |
| County-Year Fixed Effects | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y |
| Observations | 8684 | 14002 | 16539 | 15976 | 13555 |
| Adjusted R-squared | 0.084 | 0.079 | 0.066 | 0.068 | 0.074 |

**IA.3. Robustness Test: Bank Size Effects**

This table presents robustness of results to bank size effects. It reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_EXPOSURE_OF_LINKAGES. BOOM_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Column 1 includes an interaction between BOOM_EXPOSURE_OF_LINKAGES and a dummy variable ABOVE_MEDIAN_SIZE, which identifies banks that have above median size (total assets) in a given year. Column 2 excludes large subject banks, defined to be banks larger than $50 billion in total assets. Column 3 excludes very large linkage banks, defined to be banks larger than $250 billion in total assets. Column 4 drops small size subject banks, defined to be banks smaller than $100 million in total assets. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | | |
|---|---|---|---|---|
| | Large vs Small Banks | Remove Large Subject Banks | Remove Very Large Linkages | Remove Small Subject Banks |
| | 1 | 2 | 3 | 4 |
| BOOM_EXPOSURE_OF_LINKAGES | 0.0544 | 0.0624** | 0.0569* | 0.0624* |
| | (1.353) | (1.992) | (1.689) | (1.883) |
| EXPOSURE_TO_ΔHPI(%)_IN_OTHER_MARKETS | 2.346* | 3.062** | 2.133 | 1.955 |
| | (1.726) | (2.242) | (1.562) | (1.353) |
| BOOM_EXPOSURE_OF_LINKAGES X ABOVE_MEDIAN_SIZE | 0.0205 | | | |
| | (0.503) | | | |
| County-Year Fixed Effects | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y |
| Observations | 16539 | 15743 | 16539 | 14568 |
| Adjusted R-squared | 0.068 | 0.068 | 0.066 | 0.057 |

**Table IA.4: Small Size Effect**

This table reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_EXPOSURE_OF_LINKAGES in various robustness specifications. BOOM_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Column 1 drops the 15 smallest counties by loan count each year. Column 2 drops bank-county-year observations based on fewer than 15 loans. Column 3 drops the 15 smallest banks by loan count each year. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | $\Delta$log(LOANS_ORIGINATED) | | |
|---|---|---|---|
| | Drop Smallest 15 Markets by Loan Count | Drop Bank-County-Year obs with <15 loan count | Drop Smallest 15 Banks by Loan Count |
| | 1 | 2 | 3 |
| BOOM_EXPOSURE_OF_LINKAGES | 0.0640** | 0.121** | 0.0662** |
| | (2.057) | (2.314) | (2.137) |
| EXPOSURE_TO_ΔHPI(%)_IN_OTHER_MARKETS | 2.140 | 4.491** | 3.293** |
| | (1.567) | (2.435) | (2.508) |
| County-Year Fixed Effects | Y | Y | Y |
| Control Variables | Y | Y | Y |
| Observations | 16481 | 11150 | 16043 |
| Adjusted R-squared | 0.059 | 0.122 | 0.071 |

**Table IA.5: Alternate Independent Variable**
This table reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_GROWTH_EXPOSURE_OF_LINKAGES. BOOM_GROWTH_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to growth in well activity from 2003 to *t* in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Column 2 breaks BOOM_GROWTH_EXPOSURE_OF_LINKAGES into two parts: BOOM_GROWTH_EXPOSURE_OF_LARGE_LINKAGES that captures boom growth exposure of linkages that have above median asset size amongst shocked banks in a given county, and BOOM_GROWTH_EXPOSURE_OF_SMALL_LINKAGES that captures boom growth exposure of linkages that have below median asset size. Columns 1 and 2 study all loans; column 3 studies retained loans; and column 4 studies sold loans. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | | |
|---|---|---|---|---|
| | All Loans | All Loans | Retained Loans | Sold Loans |
| | 1 | 2 | 3 | 4 |
| BOOM_GROWTH_EXPOSURE_OF_LINKAGES | 0.0668** | | 0.0638** | 0.0292 |
| | (2.113) | | (1.992) | (1.253) |
| BOOM_GROWTH_EXPOSURE_OF_LARGE_LINKAGES | | 0.167* | | |
| | | (1.939) | | |
| BOOM_GROWTH_EXPOSURE_OF_SMALL_LINKAGES | | -0.0236 | | |
| | | (-0.296) | | |
| EXPOSURE_TO_ΔHPI(%)_IN_OTHER_MARKETS | 1.574 | 1.480 | 1.431 | 2.262* |
| | (1.132) | (1.065) | (1.040) | (1.737) |
| County-Year Fixed Effects | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y |
| Observations | 14515 | 14515 | 14515 | 14515 |
| Adjusted R-squared | 0.066 | 0.066 | 0.059 | 0.015 |

**Table IA.6. Boom Exposure of Linkages, Wholesale Dependence, and Borrower Credibility**
This table studies the interaction between boom exposure of linkages, wholesale dependence, and borrower credibility, where borrower credibility is UNEMPLOYMENT_RATE or BANK_UNEMPLOYMENT_EXPOSURE. It reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_EXPOSURE_OF_LINKAGES. BOOM_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Columns 1 through 3 use wholesale funds-to-assets ratio to capture bank dependence on wholesale funds, while columns 4 through 6 use core deposits-to-assets ratio. Columns 1 and 4 include an interaction between BOOM_EXPOSURE_OF_LINKAGES and HIGH_WHOLESALE_DEPENDENCE, which takes the value 1 for banks having above median wholesale dependence each year. Columns 2 and 5 include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, UNEMPLOYMENT_RATE, and HIGH_WHOLESALE_DEPENDENCE. Columns 3 and 6 include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BANK_UNEMPLOYMENT_EXPOSURE, and HIGH_WHOLESALE_DEPENDENCE. BANK_UNEMPLOYMENT_EXPOSURE is the weighted average of unemployment rates in a subject bank's local markets. Weights are the shares of deposits that the bank holds in each market. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | | | | |
|---|---|---|---|---|---|---|
| | Wholesale-to-Assets Ratio | | | Core Deposits-to-Assets Ratio | | |
| | | UNEMP_RATE | BANK_UNEMP_EXP | | UNEMP_RATE | BANK_UNEMP_EXP |
| | 1 | 2 | 3 | 4 | 5 | 6 |
| BOOM_EXPOSURE_OF_LINKAGES | 0.0480 | 0.0270 | -0.0339 | 0.0690** | 0.0464 | 0.00856 |
| | (1.134) | (0.147) | (-0.173) | (2.027) | (0.299) | (0.055) |
| HIGH_WHOLESALE_DEPENDENCE | 0.0343 | 0.171 | 0.0321 | 0.0112 | -0.137 | -0.0667 |
| | (0.481) | (0.854) | (0.133) | (0.162) | (-0.719) | (-0.293) |
| BOOM_EXPOSURE_OF_LINKAGES X HIGH_WHOLESALE_DEPENDENCE | 0.0213 | 0.0911 | 0.122 | -0.0128 | 0.159 | 0.148 |
| | (0.505) | (0.561) | (0.694) | (-0.296) | (1.082) | (0.936) |
| BOOM_EXPOSURE_OF_LINKAGES X BORROWER_CREDIBILITY | | 0.452 | 1.617 | | 0.389 | 1.127 |
| | | (0.136) | (0.465) | | (0.142) | (0.415) |
| HIGH_WHOLESALE_DEPENDENCE X BORROWER_CREDIBILITY | | -2.361 | 0.138 | | 2.684 | 1.409 |
| | | (-0.718) | (0.035) | | (0.836) | (0.367) |
| BOOM_EXPOSURE_OF_LINKAGES X BORROWER_CREDIBILITY X HIGH_WHOLESALE_DEPENDENCE | | -1.354 | -1.916 | | -3.274 | -2.999 |
| | | (-0.459) | (-0.613) | | (-1.277) | (-1.090) |
| BORROWER_CREDIBILITY | | | 6.537 | | | 5.963 |
| | | | (1.411) | | | (1.462) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Exposure_to_ΔHPI(%)_in_Other_Markets | 2.169 | 2.174 | 2.225 | 2.142 | 2.134 | 2.185 |
| | (1.588) | (1.596) | (1.630) | (1.569) | (1.566) | (1.600) |
| County-Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| Observations | 16539 | 16539 | 16539 | 16539 | 16539 | 16539 |
| Adjusted R-squared | 0.066 | 0.066 | 0.066 | 0.066 | 0.066 | 0.066 |

**Table IA.7 Boom Exposure of Linkages and Bank Health**

This table studies the interaction between boom exposure of linkages, bank health, and borrower credibility, where borrower credibility is UNEMPLOYMENT_RATE or BANK_UNEMPLOYMENT_EXPOSURE. It reports regressions of a bank's percent growth in home lending in a given county and year on the bank's BOOM_EXPOSURE_OF_LINKAGES. BOOM_EXPOSURE_OF_LINKAGES captures the exposure of a bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-shocked banks in non-boom counties from 2003 to 2017. Columns 1 through 3 use bank exposure to percent changes in home prices in the prior year to capture a bank's health, while columns 4 through 6 use asset quality (=ratio of total loan charge-offs to total loan value) in the prior year. Columns 1 and 4 include an interaction between BOOM_EXPOSURE_OF_LINKAGES and LOW_BANK_HEALTH, which takes the value 1 for banks having below median bank health each year. Columns 2 and 5 include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, UNEMPLOYMENT_RATE, and LOW_BANK_HEALTH. Columns 3 and 6 include a triple interaction between BOOM_EXPOSURE_OF_LINKAGES, BANK_UNEMPLOYMENT_EXPOSURE, and LOW_BANK_HEALTH. BANK_UNEMPLOYMENT_EXPOSURE is the weighted average of unemployment rates in a subject bank's local markets. Weights are the shares of deposits that the bank holds in each market. All regressions include county-year fixed effects and bank control variables. Standard errors are clustered by bank, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | | | | |
|---|---|---|---|---|---|---|
| | Bank Exposure to ΔHPI(%) | | | Asset Quality | | |
| | | UNEMP_RATE | BANK_UNEMP_EXP | | UNEMP_RATE | BANK_UNEMP_EXP |
| | 1 | 2 | 3 | 4 | 5 | 6 |
| BOOM_EXPOSURE_OF_LINKAGES | 0.0762** | 0.102 | 0.0406 | 0.0569 | 0.0793 | 0.0453 |
| | (1.975) | (0.617) | (0.245) | (1.619) | (0.513) | (0.283) |
| LOW_BANK_HEALTH | 0.123 | 0.636** | 0.774** | 0.0250 | -0.154 | -0.258 |
| | (1.410) | (2.150) | (2.141) | (0.281) | (-0.700) | (-1.011) |
| BOOM_EXPOSURE_OF_LINKAGES X LOW_BANK_HEALTH | -0.0314 | 0.0235 | 0.0969 | 0.0176 | 0.0342 | 0.00374 |
| | (-0.489) | (0.129) | (0.489) | (0.493) | (0.223) | (0.023) |
| BOOM_EXPOSURE_OF_LINKAGES X LOW_BANK_HEALTH X BORROWER_CREDIBILITY | | -1.051 | -2.335 | | -0.287 | 0.307 |
| | | (-0.313) | (-0.679) | | (-0.101) | (0.104) |
| BOOM_EXPOSURE_OF_LINKAGES X BORROWER_CREDIBILITY | | -0.499 | 0.646 | | -0.422 | 0.236 |
| | | (-0.177) | (0.230) | | (-0.155) | (0.084) |
| LOW_BANK_HEALTH X BORROWER_CREDIBILITY | | -8.865** | -11.15** | | 3.206 | 4.917 |
| | | (-1.999) | (-2.057) | | (0.957) | (1.223) |
| BORROWER_CREDIBILITY | | | 11.56** | | | 3.553 |
| | | | (2.517) | | | (0.793) |
| EXPOSURE_TO_ΔHPI(%)_IN_OTHER_MARKETS | 2.266* | 2.314* | 2.452* | 2.166 | 2.215 | 2.261* |
| | (1.653) | (1.695) | (1.787) | (1.583) | (1.621) | (1.656) |

| | | | | | | |
|---|---|---|---|---|---|---|
| County-Year Fixed Effects | Y | Y | Y | Y | Y | Y |
| Control Variables | Y | Y | Y | Y | Y | Y |
| Observations | 16539 | 16539 | 16539 | 16539 | 16539 | 16539 |
| Adjusted R-squared | 0.067 | 0.067 | 0.068 | 0.066 | 0.066 | 0.067 |

**Table IA.8. County Summary Statistics**

This table presents summary statistics for county-year variables in this paper. The sample consists of all counties in the main sample of Table 3 and spans years 2003 through 2017. Panel A summarizes market characteristics, Panel B summarizes boom exposure variable, and Panel C summarizes mortgage lending variables. Δlog(LOANS ORIGINATED) is the percent growth in loans originated from time *t-1* to *t*. Δlog(LOANS_ORIGINATED)(Retained Loans)) and Δlog(LOANS_ORIGINATED)(Sold Loans) are defined similarly for loans that are retained in bank portfolios and loans that are sold respectively.

Panel A: Market Characteristics

| | N | Mean | SD |
|---|---|---|---|
| | (County-Year Variation) | | |
| log(POPULATION) | 3349 | 11.073 | 1.25 |
| log(PERSONAL_INCOME) | 3349 | 10.389 | 0.258 |
| DEBT-TO-INCOME | 3349 | 1.483 | 0.818 |
| UNEMPLOYMENT_RATE | 3349 | 0.06 | 0.018 |
| PERCENT_FEMALE_POPULATION | 3349 | 0.505 | 0.014 |
| PERCENT_MINORITY_POPULATION | 3349 | 0.145 | 0.141 |
| LAGGED_ΔHPI(%) | 3349 | 0.027 | 0.039 |

Panel B: Boom Exposure Variables

| | N | Mean | SD |
|---|---|---|---|
| | (County-Year Variation) | | |
| COUNTY_BOOM_EXPOSURE_OF_LINKAGES | 3349 | 0.479 | 0.949 |

Panel C: Mortgage Lending Variables

| | N | Mean | SD |
|---|---|---|---|
| | (County-Year Variation) | | |
| Δlog(LOANS_ORIGINATED) | 3349 | 0.617 | 1.576 |
| Δlog(LOANS_ORIGINATED)(Retained Loans) | 3349 | 0.575 | 1.601 |
| Δlog(LOANS_ORIGINATED)(Sold Loans) | 3349 | 0.463 | 1.877 |

**Table IA.9. County Aggregates**

This table reports regressions at the aggregate county-year level. It presents regressions of county level growth in home lending on county level boom exposure of linkages. County level loan growth is the size weighted average of loan growth (log change in loans originated) of all non-shocked banks in a non-boom county and year. COUNTY_BOOM_EXPOSURE_OF_LINKAGES is the size weighted average of BOOM_EXPOSURE_OF_LINKAGES of all non-shocked banks in a non-boom county and year. BOOM_EXPOSURE_OF_LINKAGES for each bank captures the exposure of the bank's shocked geographic linkages to well activity in boom counties (described in detail in the text). The sample in this regression includes non-boom counties from 2003 to 2017. Column 1 presents results for all loans; column 2 presents results for retained loans; and column 3 presents results for sold loans. All regressions include state-year fixed effects. Standard errors are clustered by county, and t-statistics are reported in parenthesis. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

| | Δlog(LOANS_ORIGINATED) | | |
|---|---|---|---|
| | All Loans | Retained Loans | Sold Loans |
| | 1 | 2 | 3 |
| COUNTY_BOOM_EXPOSURE_OF_LINKAGES | 0.170*** | 0.156*** | 0.00550 |
| | (3.780) | (3.521) | (0.187) |
| log(POPULATION) | -0.142*** | -0.136*** | 0.0661*** |
| | (-5.264) | (-5.257) | (3.249) |
| log(PERSONAL_INCOME) | 0.0549 | 0.138 | 0.0321 |
| | (0.275) | (0.772) | (0.197) |
| DEBT-TO-INCOME | -0.0684* | -0.0557* | -0.0514* |
| | (-1.947) | (-1.667) | (-1.922) |
| UNEMPLOYMENT_RATE | -2.810 | -0.789 | -4.260** |
| | (-1.121) | (-0.334) | (-2.171) |
| PERCENT_FEMALE_POPULATION | 4.705** | 3.902** | 2.577 |
| | (2.380) | (2.181) | (1.597) |
| PERCENT_MINORITY_POPULATION | 0.836** | 0.673** | 0.192 |
| | (2.543) | (2.181) | (0.810) |
| LAGGED_ΔHPI(%) | -0.744 | -1.151 | 1.259 |
| | (-0.838) | (-1.254) | (1.314) |
| Constant | -0.631 | -1.296 | -1.646 |
| | (-0.322) | (-0.719) | (-1.016) |
| State-Year Fixed Effects | Y | Y | Y |
| Observations | 5008 | 5008 | 5008 |
| Adjusted R-squared | 0.141 | 0.114 | 0.111 |