Delays in Banks' Loan Loss Provisioning and Economic Downturns: Evidence from the U.S. Housing Market

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November 2021

Abstract

I study whether banks' loan loss provisioning contributes to economic downturns, by examining the U.S. housing market. Specifically, I examine the aggregate effects of banks' delayed loan loss recognition (DLR) on house prices during the Great Recession and the channels through which these potential effects arose. I construct ZIP-code-level exposure to banks' DLR before the crisis and compare high- and low-exposure ZIP codes during the crisis to examine the aggregate effects of banks' DLR on the housing market. I find that high-exposure ZIP codes experienced larger decreases in mortgage supply, larger increases in distressed sales, and larger decreases in house prices during the crisis. In addition, I conduct individual bank-level analyses and find that high-DLR banks reduced their mortgage supply more than low-DLR banks, and mortgages issued by high-DLR banks were more likely to become distressed during the crisis. Taken together, these findings suggest that banks' DLR was associated with nontrivial effects on the housing market during the Great Recession, and the effects of DLR on house prices were likely driven by both the credit-crunch and distressed-sales channels.

Keywords: Real Effects of Accounting; Banks; Loan Loss Provisioning; Housing Market; Crisis

JFL Classification Numbers: E21, G01, G21, M41, R20

* Accepted by Luzi Hail. This paper is based on my dissertation. I am grateful to my dissertation committee members for their guidance and support: Philip G. Berger (chair), John Gallemore, Christian Leuz, and Valeri Nikolaev. I also thank two anonymous reviewers, Ray Ball, John Barrios, Thomas Bourveau, Matthias Breuer, Jung Ho Choi, Hans B. Christensen, Rachel M. Geoffroy, João Granja, Seil Kim, Anya Kleymenova, Mark G. Maffett, Charles McClure, Michael Minnis, Maximilian N. Muhn, Shiva Rajgopal, Stephen G. Ryan, Haresh Sapra, Douglas J. Skinner, Robert Stoumbos, George P. Surgeon, Rimmy E. Tomy, Eric Zwick, and seminar participants at the 2018 CMU Accounting Mini Conference, University of Chicago, Rice University, University of California Berkeley, University of North Carolina, Massachusetts Institute of Technology, Tulane University, Columbia University, and Harvard University for their helpful comments. I gratefully acknowledge the Fama-Miller Center for Research in Finance and the Initiative on Global Markets at the University of Chicago Booth School of Business for providing the DataQuick data, Eric Zwick for providing access to the DataQuick database in his personal server, Gauri Bhat for the loan-type allowances data, and financial support from Columbia Business School. All errors are mine. An Online Appendix to this paper can be downloaded at http://research.chicagobooth.edu/arc/journal-of-accounting-research/online-supplements.

Forthcoming in the Journal of Accounting Research.

1. Introduction

The Great Recession of 2007–2009 sparked debate about accounting's role in financial stability and economic cycles. The timeliness of banks' loan loss provisioning is one of the most important issues in the debate, and its potential real effects have received considerable attention from bank regulators and central bankers for several reasons. Loan loss provisioning is the largest accrual in bank accounting, and estimating the amount and timing of loan loss provisioning can involve significant managerial discretion (Wahlen, 1994; Liu and Ryan, 1995, 2006; Ahmed et al., 1999; Jayaraman et al., 2019; Bischof et al., 2021). In addition, delays in loan loss provisioning may reinforce pro-cyclicality in banks' lending and weaken market discipline over banks' risk-taking (Laeven and Majnoni, 2003; Dugan, 2009; Beatty and Liao, 2011; Bushman and Williams, 2012, 2015; Acharya and Ryan, 2016; Wheeler 2018). Existing evidence on the effects of loan loss provisioning focus on bank-level behavior (e.g., Beatty and Liao, 2011; Bushman and Williams, 2015), but the individual effects might not be sufficient to capture the economy-wide effects, due to substitutions and spillovers. Thus, despite the centrality of the issue, whether loan loss provisioning led to significant economy-wide effects during the crisis remains an open question. In this paper, I examine the aggregate effects of banks' delayed loan loss recognition (DLR) on house prices during the Great Recession and the channels through which these potential effects arose.

I examine the U.S. housing market for several reasons. Households are major economic agents: their consumption expenditures were 65.3% of the U.S. GDP in 2006 (FRED Economic Data). In addition, mortgage lending is the most important business for commercial banks, accounting for about 70% of lending on bank balance sheets. Thus, the housing market is likely to be an important way in which bank accounting ripples through the real economy. Studies suggest that falling household wealth during the Great Recession, which resulted from the collapse of the housing market, reduced household consumption and aggravated the crisis (Mian et al., 2013; Mian and Sufi, 2014). For these reasons, examining the effects of banks' DLR on house prices can shed light on the role of bank accounting during economic downturns.

Banks' DLR could affect house prices through at least two channels: by influencing mortgage lending (i.e., the *credit-crunch channel*) and by influencing risk-taking (i.e., the *distressed-sales channel*). The credit-crunch channel suggests that high-DLR banks affect house prices through their lending. As banks delayed loss recognition, they created loss overhangs. Once the downturn started, banks became concerned about future capital inadequacy and thus reduced lending (Van den Heuvel, 2009; Beatty and Liao, 2011). As a result, the reduced credit supply pushed down house prices. On the other hand, the distressed-sales channel suggests that high-DLR banks affect house prices through their prior risk-taking behavior. High-DLR banks took more risks during good times, because DLR can weaken discipline by stakeholders due to less transparent financial reporting or a higher threshold for liquidation of the loan portfolio (Barth and Landsman, 2010; Bushman and Williams, 2012, 2015; Bertomeu et al., 2020; Gallemore, 2021). Consequently, once the crisis hit, homes with mortgages from high-DLR banks were more likely to be sold via foreclosures or short sales, pushing down house prices (Campbell et al., 2011).

To investigate the aggregate effects of DLR on the housing market, I construct the ZIP-code-level exposure to banks' DLR using the weighted average of individual banks' DLRs based on their market shares before the crisis (2004–2006). Then, I run a difference-in-differences analysis by comparing high- and low-exposure ZIP codes before and during the crisis. This approach is akin to the Bartik instrument, a research design exploiting the differential regional exposure to a common shock measured by the share of pre-determined characteristics (Breuer, 2021). I expect larger declines in the housing market if an area was more exposed to high-DLR (high exposure to treated units) banks before the crisis (a common shock).[1]

An obvious concern is that economic downturns likely influence both banks' behavior and local economic conditions; thus, controlling for demand effects is crucial. I employ several approaches to mitigate this concern. First, I include county-year fixed effects to control for any county-level time-varying economic conditions. Second, I include ZIP-code fixed effects to control for any ZIP-code-specific invariant characteristics that may drive house prices and banks' market entries. Finally, I include ZIP-code-level changes in income, business establishment, and employment to control for time-varying economic conditions. Notably, this approach captures any substitutions or spillovers at the ZIP-code level, because the aggregate housing-market outcomes are measured after accounting for substitution of lending activity across lenders and spillovers of foreclosures within the same ZIP code.

I find that high-exposure ZIP codes experienced larger decreases in mortgage supply during the crisis. A one-standard-deviation increase in ZIP-code-level exposure to DLR is associated with a 10.5-percentage-point-larger decline in mortgage supply during the crisis. I also find that high-exposure ZIP codes experienced more distressed sales during the crisis. A one-standard-deviation increase in ZIP-code-level exposure to DLR is associated with a 5.5- (1.0-) percentage-point-larger increase in foreclosure rates (short sales rates) during the crisis. Finally, I find that high-exposure ZIP codes experienced larger decreases in house prices during the crisis. A one-standard-deviation increase in ZIP-code-level exposure to DLR is associated with a 1.6- to

[1] This approach relies on the assumption that the banks' mortgage market shares at the ZIP-code level *before the crisis* are uncorrelated with the changes in housing market outcomes *during the crisis* after controlling for observables. Although the identifying assumption cannot be directly tested, it is akin to the parallel-trends assumption under the difference-in-differences approach (Breuer, 2021). I provide evidence that house-price changes before the crisis do not depend on the exposure to banks' DLR in Figure 3.

1.7-percentage-point-larger decline in house prices during the crisis. In dollar terms, these estimates imply a decrease in house prices of $3,360 to $3,570 for the median home sold in the U.S. in 2006.

Then, I further use an instrumental-variable (IV) approach to address two remaining concerns: (i) The ZIP-code-level exposure to banks' DLR could be non-random even within a county, and (ii) banks' DLR could be correlated with unobservable bank characteristics. In July 2001, the SEC issued Staff Accounting Bulletin 102 to discourage banks from recognizing excessive expected loan losses. I use the distance between a bank's headquarters and the closest SEC office to proxy for the SEC's influence on public banks' financial reporting and use it as an instrument for a bank's loan loss provisioning (Kedia and Rajgopal, 2011; Jayaraman et al., 2019). The IV estimation yields the same results with larger effects, confirming that OLS findings are unlikely driven by the unobservable local economy or bank characteristics.[2] Taken together, these findings suggest that banks' DLR is associated with nontrivial effects on the housing market during the Great Recession.

After showing the economy-wide effects associated with DLR, I conduct individual-bank-level analyses to further examine the credit-crunch and distressed-sales channels. To identify the credit-crunch channel, I compare mortgage supply by high- and low-DLR banks within the same ZIP code and year, thereby controlling for mortgage demand at the ZIP-code level. I also compare the supply of high- (conventional) and low- (Federal Housing Administration) capital-burden mortgages within the same bank to further control for unobservable time-varying bank

[2]Although the IV approach can reduce concerns for many unobservable bank characteristics, I make a caveat that the IV may still be correlated with management's risk-taking choices, which are often bundled with loan loss provisioning. To reduce this concern, in untabulated analysis, I create proxies for banks' risk-taking and re-estimate the IV regressions by controlling for them. The second-stage IV estimates become slightly smaller than the ones in Table 3, but they remain statistically significant, suggesting the IV estimates are not primarily driven by risk-taking proxies.

characteristics. Similarly, to identify the distressed-sales channel, I compare mortgage outcomes for high- and low-DLR banks within the same ZIP code to control for local economic conditions that drive mortgage distress. In this analysis, I identify mortgages originated by high- and low-DLR banks before the crisis and then track them through the crisis, comparing their likelihood of becoming distressed sales in the same ZIP code.

I find that high-DLR banks reduced the mortgage supply more than low-DLR banks during the crisis. This effect is stronger for low-capitalized banks, high-loss-overhang banks, and high-capital-burden loans, consistent with the credit-crunch theory that high-DLR banks became concerned about future capital inadequacy and thus decreased their lending (Beatty and Liao, 2011). I also find that mortgages by high-DLR banks in high-exposure ZIP codes were more likely to end up as distressed sales, which suggests that these banks took more risks before the crisis. Taken together, the bank-level evidence indicates that both the credit-crunch and distressed-sales channels likely drove the aggregate effects of DLR on house prices during the crisis.

This study contributes to research on the role of banks' loan loss provisioning during economic downturns. Prior studies suggest that banks' DLR can reinforce individual banks' lending procyclicality (e.g., Beatty and Liao, 2011) and weaken market discipline over banks' risk-taking (e.g., Bushman and Williams, 2012, 2015); thus, it could be an important contributor to the Great Recession. My paper provides several new implications to this discussion. First, using a novel approach relying on variation in geographic exposures to banks' DLR, I provide evidence on the economy-wide effects of DLR. My findings suggest that, in aggregate, banks' DLR is associated with nontrivial effects on housing-market outcomes during the crisis. However, the magnitude of the effects of DLR is relatively small compared with that of the cumulative decrease in house prices during the crisis, which suggests that banks' DLR was a contributing factor but not the main culprit for the Great Recession. Second, the aggregate effects of DLR on banks' mortgage lending and house prices are more salient during bad times than good times, possibly because the substitution across lenders becomes more challenging during bad times. Third, the paper strengthens prior evidence from studies on banks' DLR and the credit-crunch effect (e.g., Beatty and Liao, 2011). Acharya and Ryan (2016) point to two empirical challenges in the literature: supply effects need to be distinguished from demand effects, and financial-reporting effects need to be separated from other bank-characteristics effects. They call for stringent research designs to tighten the causal links between banks' loan loss provisioning and the credit-crunch effect. Although my paper cannot eliminate these empirical challenges, it answers this call by employing various novel research designs, including high-dimensional regional fixed effects, an IV approach, and a within-bank analysis with different types of mortgage loans.

This study also speaks to factors affecting the housing-market collapse during the Great Recession. Studies suggest factors that contributed to the housing-market boom and bust, such as mortgage credit supply (Mian and Sufi, 2009; Favara and Imbs, 2015; Adelino et al., 2020), the originate-to-distribute model (Purnanandam, 2011), distressed sales (Campbell et al., 2011; Mian et al., 2015), and private-label mortgage securitization (Mian and Sufi, 2021). However, to the best of my knowledge, no studies examine whether and how banks' financial-reporting discretion contributed to the housing-market crisis, despite the controversial debate on the role of banks' financial-reporting discretion during the Great Recession. My paper fills this gap by suggesting that banks' loan loss provisioning appears to have contributed to the housing crisis.

2. Background and Related Literature

2.1. Loan Loss Provisioning and Economic Downturns

The Great Recession sparked discussion of banks' financial reporting and their loan loss recognition in particular (Laux and Leuz, 2009, 2010; Barth and Landsman, 2010; Vyas, 2011; Beatty and Liao, 2011, 2014; Bushman and Williams, 2012, 2015; Huizinga and Laeven, 2012; Kothari and Lester, 2012; Acharya and Ryan, 2016; Wheeler, 2019; Bischof et al., 2021; Wheeler, 2021). Regulators and others have blamed delays in loan loss provisioning, under FAS 5's incurred loss model, for exacerbating the severity of economic downturns.[3] They argue that the model's "probable" threshold for loss accrual and its backward-looking nature induce banks to delay loss recognition in good times, creating an overhang of losses that carry forward to bad times. The loss overhang increases the concern for capital inadequacy, and banks reduce loan supply because raising capital is costly during downturns (Laeven and Majnoni, 2003; Dugan, 2009; Beatty and Liao, 2011; Acharya and Ryan, 2016).[4] Studies have examined the relationship between banks' loan loss provisioning and their lending and risk-taking. Beatty and Liao (2011) find that DLR is positively associated with lending pro-cyclicality. Bushman and Williams (2012, 2015) find that DLR is positively associated with capital market risk measures because DLR reduces banks' financial-reporting transparency and thereby weakens market discipline over their risk-taking.

However, despite the centrality of the issue, whether loan loss provisioning had significant economy-wide effects during the crisis remains an open question because evidence tends to focus on individual-bank behavior. Importantly, any individual behavior can be mitigated or magnified

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[3] FAS 5's incurred loss model requires banks to accrue credit losses only if those losses are incurred, probable, and reasonably estimable based on current information.

[4] In response to this criticism, the FASB replaced the incurred loss model of estimating credit losses with the current expected credit loss (CECL) model in Accounting Standards Update (ASU) No. 2016-13. The CECL standard was initially set to take effect in January 2020 for SEC filers, except for smaller reporting companies. However, due to COVID-19, the CARES Act provides an option to delay the CECL adoption until the earlier of (1) the first date of an eligible financial institution's fiscal year that begins after the date when the COVID-19 national emergency is terminated, or (2) January 1, 2022 (as amended by the Consolidated Appropriations Act). In addition, the FASB further pushed back the effective date of CECL from January 2021 to January 2023 for smaller reporting companies, and from January 2022 to January 2023 for private and nonprofit entities.

at the aggregate level due to substitutions and spillovers. For example, any bank-level effects would be offset if other institutions substitute their actions. Conversely, an individual bank's activities might create spillover if other institutions follow its behavior and generate larger aggregate effects on the economy. Thus, whether banks' DLR indeed created large effects and contributed to the crisis, as some have argued, is an empirical question.

2.2. Credit Crunch, Distressed Sales, and House Prices

I hypothesize that banks' DLR could affect house prices through at least two channels: by influencing mortgage lending (the credit-crunch channel) and by influencing risk-taking (the distressed-sales channel). Notably, these two channels are not necessarily related to banks' reporting choices. However, if DLR is a contributing factor to banks' lending and risk-taking, these channels are plausible channels through which banks' financial reporting affects house prices.

Regarding the credit-crunch channel, studies suggest that credit supply significantly affects house prices (Mian and Sufi, 2009; Favara and Imbs, 2015; Adelino et al., 2020). Also, studies suggest that capital constraints lead to a contraction of lending (Bernanke and Lown, 1991). Theoretically, Van den Heuvel (2009) presents a model showing that banks might forgo lending opportunities to lower the risk of future capital inadequacy. Thus, if banks' DLR creates loss overhangs, leading to the concern for capital inadequacy during downturns (Beatty and Liao, 2011), high-DLR banks would reduce mortgage lending and thus push down house prices.

Regarding the distressed-sales channel, studies suggest that distressed sales such as foreclosures and short sales negatively affect house prices (Campbell et al., 2011; Mian et al., 2015).[5] Also, studies suggest that banks' high risk-taking can lead to more distressed sales during

[5] Distressed sales usually include foreclosures and short sales. A short sale means that the borrower sells the property for less than the outstanding mortgage balance under an agreement with the lender, and pays the proceeds to the lender

downturns (Demyanyk and Van Hemert, 2011). Theoretically, Bertomeu et al. (2020) present a model showing that banks may engage in excessive risk-taking when the threshold for recognizing losses is higher, because it undermines discipline on banks' ex-ante risk-taking. Also, Bushman and Williams (2015) argue that banks' DLR can weaken the monitoring of banks' risk-taking by outsiders, due to less transparent financial reporting. Thus, if high-DLR banks took more risks in the mortgage market during good times, once the crisis hit, mortgages from high-DLR banks were more likely to be distressed, putting further pressure on house prices.

3. Data, Variable Construction, and Sample

3.1. Data Sources

I use several data sources to construct the variables. To create bank-level DLR and control variables, I use financial-statement data from the Call Reports. To create ZIP-code-level exposure to banks' DLR and mortgage credit amounts, I use the Home Mortgage Disclosure Act (HMDA) data, which record most mortgage applications in the U.S.[6] To identify distressed sales, I use the recorder and assessor data from DataQuick, which contains deed-level information on ownership changes and loans secured by properties. I use two sources for housing price indices: Federal Housing Finance Agency (FHFA) and CoreLogic.[7] Finally, I obtain ZIP-code-level characteristics from various databases. From the American Community Survey (ACS), I draw data on

(Zhu and Pace, 2015). I define foreclosures as transactions categorized by DataQuick as financial institution-owned sales (REO) or foreclosure auctions, and short sales as transactions inferred as short sales by DataQuick. DataQuick uses a proprietary model to identify short sales (Ferreira and Gyourko, 2015).

[6] HMDA covers more than 8,800 lenders and accounts for approximately 80% of all home lending in the United States (Avery et al., 2007).

[7] FHFA's price indices are available yearly and provide broad geographic coverage (Bogin et al., 2019). CoreLogic's price indices are available monthly and provide less coverage but have various versions measured differently. I use CoreLogic's index based on Single Family Detached Home at the end of each year.

demographics, poverty, and education.[8] From the IRS's Statistics of Income (SOI), I construct average income and gross income growth rate. From the County Business Patterns (CBP), I build the employment and business establishment growth rate. I provide a more detailed description of data sources in section A.1 of the online appendix.

3.2. Measuring Banks' DLR and ZIP-Code-Level Exposure to Banks' DLR

My primary empirical strategy for the aggregate effects of DLR uses cross-sectional variation across ZIP codes in pre-crisis exposure to banks' DLR. This approach is similar to the Bartik instrument. It relies on the assumption that the bank's mortgage market shares at the ZIP-code level *before the crisis* are uncorrelated with the changes in housing-market variables *during the crisis* after controlling for observables.[9] To construct this measure, I estimate DLR at the bank level following the literature (Nichols et al., 2009; Beatty and Liao, 2011; Bushman and Williams, 2015; Gallemore, 2021) and then construct the ZIP-code-level exposure to banks' DLR using banks' mortgage market shares during the pre-crisis period from HMDA.

First, I estimate two regressions over the most recent 12 quarters for each bank during 2004–2006, requiring the bank to have data for all 12 quarters on the Call Reports.

$$\begin{aligned} LLP_{i,t} = \beta_0 + \beta_1 \Delta NPL_{i,t-1} + \beta_2 \Delta NPL_{i,t-2} + \beta_3 EBLLP_{i,t} + \beta_4 Tier1Ratio_{i,t-1} \\ + \beta_5 Size_{i,t-1} + \beta_6 CoIndex_{s,t} + \epsilon_{i,t}, \end{aligned} \tag{A1}$$

[8] I use "2007–2011 American Community Survey (ACS) 5-Year Estimates," which is the first survey containing information at the ZIP-code level. Variables from the ACS are not available yearly; thus, the same value is used for all years. Other ZIP-code-level controls are constructed annually.

[9] The Bartik instrument is named after Bartik (1991) and became popularized by Blanchard and Katz (1992). Originally, the Bartik instrument is formed by interacting local industry shares with national industry growth rates. However, any research design exploiting the differential regional exposure to a common shock (e.g., time) measured by the share of pre-determined characteristics (e.g., treatment status) is generally considered the Bartik approach, and identification is based on exogeneity of the shares (Goldsmith-Pinkham et al., 2020). Importantly, the instrument can be valid even when the shares are correlated with the levels of the outcome, because the Bartik approach asks whether differential exposure to common shocks leads to differential changes in the outcome. Breuer (2021) describes examples in accounting research using the Bartik instrument.

$$LLP_{i,t} = \beta_0 + \beta_1 \Delta NPL_{i,t+1} + \beta_2 \Delta NPL_{i,t} + \beta_3 \Delta NPL_{i,t-1} + \beta_4 \Delta NPL_{i,t-2} + \beta_5 EBLLP_{i,t} + \beta_6 Tier1Ratio_{i,t-1} + \beta_7 Size_{i,t-1} + \beta_8 CoIndex_{s,t} + \epsilon_{i,t}. \quad \text{(A2)}$$

The dependent variable, $LLP_{i,t}$, is the bank's loan loss provision divided by lagged total loans. The primary independent variables, $\Delta NPL_{i,t}$ and $\Delta NPL_{i,t+1}$, are the changes in non-performing loans divided by lagged total loans and are included only in equation (A2). $\Delta NPL_{i,t-1}$ and $\Delta NPL_{i,t-2}$ are included to control for prior loan-portfolio quality changes. $EBLLP_{i,t}$, the earnings before the loan loss provision and taxes divided by lagged total loans, is included to control for banks' incentives to smooth earnings (Ahmed et al., 1999; Bushman and Williams, 2012, 2015). $Tier1Ratio_{i,t-1}$, the lagged tier 1 capital ratio, is included to capture capital management (Liu and Ryan, 1995, 2006). $Size_{i,t-1}$ is the natural logarithm of lagged total assets. Finally, $CoIndex_{s,t}$, the coincident index measured for the state of banks' headquarters, is included to control for economic conditions.[10] I compute the bank-level DLR as a negative number of the incremental explanatory power of future and current changes in the non-performing loans for the current loan loss provision (Adjusted R^2 in (A1) – Adjusted R^2 in (A2)). If a bank incorporates information about its future and current changes in non-performing loans in a more timely fashion when determining its current-period loan loss provision, the incremental explanatory power is higher.

Then, I construct the ZIP-code-level pre-crisis exposure to banks' DLR as follows:

$$Exposure_z = \frac{1}{3} \sum_{t=2004}^{2006} \sum_{i \in z} \alpha_{i,z,t} \times DLR_{i,t}, \quad \text{(A3)}$$

[10] The coincident index is a comprehensive measure of economic activity at the state level (Khan and Ozel, 2016). The index is produced monthly by the Federal Reserve Bank of Philadelphia and calculated using models with four state-level inputs: nonfarm payroll employment, unemployment rate, average hours worked in manufacturing, and wage and salary disbursements deflated by the consumer price index.

where $\alpha_{i,z,t}$ is the mortgage market share of bank i at ZIP code z in year t, and $DLR_{i,t}$ is the bank-level DLR. That is, the ZIP-code-level exposure to banks' DLR is the weighted average of individual banks' DLRs based on their mortgage market shares before the crisis.

Figure 1 displays variation in geographic exposures to banks' DLR. The exposures are standardized to have a mean of 0 and a standard deviation of 1, and darker areas indicate more exposure to banks' DLR. Panel A displays county-level variation across the U.S., and Panel B shows ZIP-code-level variation in the exposure for three metropolitan areas (San Francisco, Chicago, and Miami). These figures suggest significant variation across regions in these exposures. Panel B confirms enough variation at the ZIP-code level, even within a county, which is critical to the empirical strategy using only within-county variation.

3.3. Sample Construction and Descriptive Statistics

I construct samples at the various bank and geographical levels. To examine the aggregate effects, I construct ZIP-code-level variables by aggregating originated mortgages from HMDA and housing transactions from DataQuick. The final ZIP-code-level sample contains 12,978 ZIP codes and 89,391 ZIP-years during 2004–2010. The final bank-level sample used to construct other datasets includes 4,621 banks and 24,957 bank-years during 2004–2010.

To examine the bank-level behavior, I construct (i) a bank-ZIP-code-level panel, (ii) a bank-MSA-level panel, and (iii) matched HMDA–DataQuick mortgage loan-level data. To construct the bank-ZIP-code-level panel, I aggregate originated mortgages by banks and ZIP codes from HMDA and link the Call Reports bank-level variables. The final bank-ZIP-code-level data contain 4,558 banks, 12,974 ZIP codes, 517,937 bank-ZIP codes, and 1,283,506 bank-ZIP-years during 2004–2009. To construct the bank-MSA-level panel, I use the same approach as for the bank-ZIP-code-level panel but aggregate originated mortgage loans by banks and MSAs. The final bank-MSA-level data contain 4,373 banks, 349 MSAs, 49,422 bank-MSAs, and 125,680 bank-MSA-years during 2004–2009. Finally, I construct the matched HMDA–DataQuick loan-level data by linking HMDA to DataQuick's loan data. Because DataQuick does not provide any information on the lender's identification except the name, I use a fuzzy match, based on the lender name, following Ferreira and Gyourko (2015). The final matched HMDA–DataQuick data contain 675,030 mortgage loans from 2,564 banks.[11] A detailed description of the sample-construction process is provided in section A.2 of the online appendix.

Table 1 presents descriptive statistics for the various samples.[12] Panel A reports the bank-level statistics; the bank-level data are used to construct critical explanatory variables. For example, *DLR High* is equal to 1 if the average DLR during 2004–2006 is above the bank-level sample median, and this variable is matched to other samples. Panel B compares the bank-level variables by high- and low-DLR banks for the pre-crisis and crisis periods. Although some variables are statistically different, both groups have economically similar observable characteristics, consistent with the DLR measure capturing an aspect of an accounting property beyond what is implied by observable bank characteristics. Panel C reports descriptive statistics of the ZIP-code-level variables. *Exposure* is the ZIP-code-level pre-crisis exposure to banks' DLR and is standardized to have a mean of 0 and a standard deviation of 1. Panel D compares ZIP-code-level dependent variables by high- and low-exposure ZIP codes for the pre-crisis and crisis periods. For the pre-crisis period, the mean changes in the logged FHFA home price index ($\Delta logHPI - FHFA$) is 0.090

[11] The fuzzy-match process is described in section A.2 of the online appendix. The matching rate between HMDA and DataQuick is about 21% (675,030 matched /3,160,554 originated loans for home purchases by identified banks during 2004–2006). The low rate has several explanations: (i) The DataQuick coverage is lower than HMDA coverage; (ii) I use a relatively strict criterion for the match, based on the lender's name; and (iii) banks usually make multiple mortgages with similar amounts in the same ZIP code, and only one of these duplicates is considered as a match.
[12] Continuous variables are winsorized at the 1st and 99th percentiles.

for high-exposure ZIP codes and 0.074 for low-exposure ZIP codes, implying both areas experienced rapid growth in house prices, but high-exposure ZIP codes experienced a more considerable increase. For the crisis period, the pattern of house-price growth is reversed. The mean of $\Delta logHPI - FHFA$ is -0.044 for high-exposure ZIP codes and -0.023 for low-exposure ZIP codes, implying both areas experienced large decreases in house prices, but high-exposure ZIP codes experienced a more considerable decline. In general, high-exposure ZIP codes show worse housing-market outcomes than low-exposure ZIP codes during the crisis.[13]

4. Empirical Approach and Results

4.1. Aggregate Effects of Banks' DLR: ZIP-Code-Level Analysis

I examine the aggregate effects of banks' DLR with a simple graphical analysis. Figure 2 indicates binned scatterplots of the exposure to banks' DLR versus various standardized housing outcomes during the crisis. Consistent with the credit-crunch and the distressed-sales hypotheses, the top-left and -right panels show that the exposure to banks' DLR is negatively associated with mortgage-supply changes and positively associated with distressed-sales rates. Also, the two bottom panels show that exposure to banks' DLR is negatively associated with house-price changes. Thus, overall, the graphical analysis suggests that banks' DLR is negatively associated with housing-market variables during the crisis.[14]

To formally estimate the aggregate effects of DLR, I use a difference-in-differences research design using a geographical variation. That is, ZIP codes with low exposure to banks'

[13] In Table A26 of the online appendix, the descriptive statistics of variables for the bank-ZIP-level, bank-MSA-level, and matched HMDA-DataQuick loan-level sample are reported.
[14] In Figure A1 of the online appendix, I plot figures showing associations between mortgage supply, distressed sales, and house prices. These figures confirm that the decrease in mortgage supply and the increase in distressed sales are strongly associated with the decrease in house prices as the prior studies suggest.

DLR act as a control group, and ZIP codes with high exposure to banks' DLR act as a treatment group. Importantly, controlling for demand effects is crucial because demand for mortgage loans and housing must have decreased during the crisis. Thus, I include several fixed effects and control variables to control for demand effects. First, I include county-year fixed effects to control for any county-level changes in income, employment, or other variables that uniformly affect the same county in a given year. Including county-year fixed effects ensures that high-exposure ZIP codes are compared with low-exposure ZIP codes within the same county and year, and thus it also mitigates a concern that high-DLR banks systematically operated in regions with worse economic conditions during the crisis. Second, I include ZIP-code fixed effects to control for any ZIP-code-specific invariant characteristics such as socioeconomic conditions and geographical features that may drive house prices and banks' market entries. Finally, I include ZIP-code-level changes in income, business establishment, and employment to control for time-varying economic conditions. With these specifications, I estimate the following model[15]:

$$y_{z,t} = \beta Exposure_z \times Crisis_t + \gamma X_{z,t} + \delta_{c,t} + \lambda_z + \epsilon_{z,t}. \quad (1)$$

The dependent variables differ by test. For the credit-crunch channel, the dependent variables are $logCredit_{z,t}$ and $\Delta logCredit_{z,t}$, the natural logarithm of mortgage-origination amounts, and changes in the natural logarithm of mortgage-origination amounts in ZIP code z in year t. For the distressed-sales channel, the dependent variables are $Foreclosure\ Rate_{z,t}$ and $Short\ Sale\ Rate_{z,t}$, the number of foreclosed sales, and the number of short sales divided by the total number of housing transactions in ZIP code z in year t. Finally, for house prices, the dependent variables are $\Delta logHPI_{z,t}$, changes in the natural logarithm of either FHFA's or CoreLogic's price index in ZIP code z in year

[15] The main variables, *Exposure* and *Crisis*, are omitted from the equation because they are subsumed by the fixed effects.

t. The primary independent variable is $Exposure_z \times Crisis_t$, where $Exposure_z$ is the ZIP-code-level pre-crisis exposure to banks' DLR, and $Crisis_t$ is equal to 1 if the year is 2007–2009 for the loan-supply and house-price tests and 2007–2010 for the distressed-sales tests. Additionally, I estimate equation (1) with the indicator exposure measure $Exposure\ High_z$, which is equal to 1 if $Exposure_z$ is above the sample median. The ZIP-code-level control variables, $X_{z,t}$, include *Lag Tier 1 Cap at ZIP*, Δ*Employment*, Δ*Establishment*, Δ*Gross Income*, *logAve Income*, *HHI*, *Nonbank Share*, and Δ*logNonbank Credit*. Finally, I include county-year fixed effects, $\delta_{c,t}$, and ZIP-code fixed effects, λ_z, to control for unobservable geographic characteristics. All regressions are weighted by the population of the ZIP code, and all variables are defined in Appendix A.

First, I examine the aggregate effects of banks' DLR on mortgage supply. In Table 2, columns (1) and (2) of Panel A report the results with the continuous exposure measure for *logCredit* and Δ*logCredit* as the dependent variable. The coefficients of *Exposure*×*Crisis* are significantly negative for both columns (-0.142, $p<0.01$; -0.105, $p<0.01$), suggesting that high-exposure ZIP codes experienced a substantially larger decrease in mortgages than low-exposure ZIP codes during the crisis. The coefficient of -0.105 in column (2) implies that a one-standard-deviation increase in the exposure to banks' DLR is associated with a 10.5-percentage-point decrease in the growth rate of credit amounts during the crisis. Columns (1) and (2) of Panel B report the results with the indicator exposure measure, and the coefficients of *Exposure High*×*Crisis* are significantly negative for both columns (-0.165, $p<0.01$; -0.129, $p<0.01$). The coefficient of -0.129 in column (2) implies that the growth rate of credit in high-exposure ZIP codes is 12.9 percentage points lower than in low-exposure ZIP codes during the crisis. These findings suggest that banks' DLR was associated with an economically significant decrease in mortgage supply during the crisis.

Next, I examine the aggregate effects of banks' DLR on distressed sales. Columns (3) and (4) of Panel A in Table 2 report the results with the continuous exposure measure for *Foreclosure Rate* and *Short Sale Rate* as the dependent variable. The coefficients of *Exposure*×*Crisis* are significantly positive for both columns (0.055, $p<0.01$; 0.010, $p<0.01$), suggesting that high-exposure ZIP codes experienced larger increases in foreclosure and short-sale rates than low-exposure ZIP codes during the crisis. The coefficient of 0.055 (0.010) in column (3) ((4)) implies that a one-standard-deviation increase in the exposure to banks' DLR is associated with a 5.5- (1.0-) percentage-point increase in foreclosure rates (short-sale rates) during the crisis. Columns (3) and (4) of Panel B report the results with the indicator exposure measure, and the coefficients of *Exposure High*×*Crisis* are significantly positive for both columns (0.061, $p<0.01$; 0.010, $p<0.05$). The coefficient of 0.061 (0.010) in column (3) ((4)) implies that foreclosure rates (short-sales rates) are higher by 6.1 (1.0) percentage points in high-exposure ZIP codes than in low-exposure ZIP codes during the crisis. All these findings suggest that banks' DLR was associated with an economically significant increase in distressed sales during the crisis.

Finally, I examine the effect of banks' DLR on house prices. Given that high-exposure ZIP codes experienced larger decreases in mortgage supply and increases in distressed sales, I expect that they also experienced larger decreases in house prices than low-exposure ZIP codes during the crisis. Columns (5) and (6) of Panel A in Table 2 report the results with the continuous exposure measure for Δ*logHPI-FHFA* and Δ*logHPI-CoreLogic* as the dependent variable. The coefficients of *Exposure*×*Crisis* are significantly negative for both columns (-0.016, $p<0.10$; -0.017, $p<0.05$), suggesting high-exposure ZIP codes experienced larger decreases in house prices. The coefficient of -0.016 (-0.017) in column (5) ((6)) implies that a one-standard-deviation increase in the exposure to banks' DLR is associated with a 1.6- (1.7-) percentage-point decrease in the growth rate of the FHFA house-price index (the CoreLogic house-price index) during the crisis. In dollar terms, at the median sales prices of homes sold in the U.S. in 2006 from DataQuick ($210,000), the estimate implies a decrease in prices of $3,360 ($3,570). Columns (5) and (6) of Panel B report the results with the indicator exposure measure, and the coefficients of *Exposure High*×*Crisis* are significantly negative for both columns (-0.021, $p<0.05$; -0.012, $p<0.01$). The coefficient of -0.021 (-0.012) in column (3) ((4)) implies that the growth rate of FHFA house-price indexes (CoreLogic house price indexes) is 2.1 (1.2) percentage points lower in high-exposure ZIP codes than in low-exposure ZIP codes during the crisis. In dollar terms, the estimate implies a $4,410 ($2,520) decrease in price. Notably, the decrease in the ZIP-code-level house prices (FHFA HPI) from 2007 to 2010 has a mean of -15.3%, a median of -10.8%, and a first-percentile value of -80.8%. Thus, the magnitude of the effects of DLR is relatively small compared with that of the cumulative decrease in house prices during the crisis, which suggests that banks' DLR might have contributed to the crisis but was not likely the main culprit.[16]

The Bartik approach's key assumption is that the bank's mortgage market shares at the ZIP-code level *before the crisis* are uncorrelated with the housing-market outcomes *during the crisis* after controlling for observables. This assumption is akin to the parallel-trends assumption under the difference-in-differences approach (Breuer, 2021). Figure 3 plots the estimated treatment effects for the entire sample period by including interaction terms between the *Exposure* and *Year* indicators for every year except 2006, which serves as the benchmark (i.e., the coefficient is set to

[16] I check the plausibility of the magnitude of my estimates. Favara and Imbs (2015) present a one-percentage-point increase in mortgage credit results in a 0.12-percentage-point increase in house prices. I find that a one-percentage-point decrease in mortgage credit results in a 0.15-percentage-point decrease in house prices. The ratio is calculated based on the coefficient for the FHFA price index changes divided by the coefficient for credit amount changes using the continuous exposure measure (i.e., -0.016/-0.105=0.15). The slightly larger ratio is plausible because the estimates also reflect other channels' effects, including distressed sales.

0).[17] The coefficients are not statistically different from 0 or very close to 0 before the crisis, suggesting the effects of banks' DLR on house prices during the crisis is not likely driven by the fact that certain banks operated in certain areas before the crisis. (i.e., the bank's mortgage market shares at the ZIP-code level before the crisis are likely uncorrelated with changes in housing prices during the crisis). In addition, the figure suggests that the effects of DLR on house prices are more salient during bad times than good times.

4.2. Effects of Banks' DLR on House Prices: IV Analysis

I use an IV approach to address two remaining concerns: (i) The ZIP-code-level exposure to banks' DLR could be non-random, even within a county; and (ii) banks' DLR could be correlated to unobservable bank characteristics. The IV approach complements the primary strategy because it utilizes the exogenous shock to banks' DLRs instead of relying on the exogeneity of their market shares. Thus, even if the banks' market shares within the county were not exogenous, the IV analysis could help address the endogeneity concern conditional on the instrument's validity. I use the SEC's influence on U.S. public banks as an IV to isolate an exogenous shift in loan loss provisioning. The SEC opposes excessive early recognition of expected loan losses because it views this practice as building cookie jar reserves to smooth income (Beck and Narayanamoorthy, 2013; Balla and Rose, 2015; Jayaraman et al., 2019). The SEC's 1998 litigation against SunTrust Bank led the SEC to issue Staff Accounting Bulletin (SAB) 102 in July 2001 to discourage banks from recognizing excessive expected loan losses.[18] Balla and Rose (2015) find that public banks changed their loan loss provisioning practices in response to

[17] The estimation for house prices in Table 2 uses 2007–2009 for the crisis period, whereas Figure 3 presents the treatment effects up to the year 2010 to provide a clear pattern of the treatment effects even after the crisis period.
[18] The SEC was concerned that U.S. public banks were systematically overstating their loan loss reserves in 1997. In 1998, the SEC required SunTrust Banks to reverse its loan loss reserve by $100 million by restating its earnings for 1994–1996 (Balla and Rose, 2015).

the SEC's intervention more than privately held banks did. Because being public could be endogenous, I further employ the distance between the bank and the SEC office to proxy for the SEC's oversight (Kedia and Rajgopal, 2011; Jayaraman et al., 2019).[19] Given that this distance is plausibly exogenous to a bank's behavior in the housing market, I use *–log(distance)*×*Public* as an instrument for individual banks' DLR following an approach similar to that of Jayaraman et al. (2019).[20] Then, I construct a ZIP-code-level instrument using the weighted average of the individual instrument based on the banks' mortgage market shares:

$$SEC\ Influence\ ZIP_z = \frac{1}{3}\sum_{t=2004}^{2006}\sum_{i\in z}\alpha_{i,z,t} \times - log(Distance)_{i,t} \times Public_{i,t}. \qquad \text{(IV1)}$$

One plausible concern for the exclusion restriction is that public banks' market shares could be correlated with unobservable geographic characteristics. For example, public banks tend to operate in metropolitan areas, which typically experienced larger housing booms and busts. To address this concern, I include ZIP-code-level public banks' market shares and other fixed effects. By controlling for these variables, the identification is driven mainly by variation in the interaction term of the distance and the public bank indicator, not just by the market share of public banks. I then estimate the first-stage model as follows:

$$Exposure_z \times Crisis_t = \beta SEC\ Influence\ ZIP_z \times Crisis_t + \gamma X_{z,t} + \delta_{c,t} + \lambda_z + \epsilon_{z,t}. \qquad \text{(IV2)}$$

SEC Influence ZIP_z is the ZIP-code-level IV defined as in equation (IV1). The second-stage model is a modification of equation (1) as follows[21]:

$$\Delta logHPI_{z,t} = \beta \widehat{Exposure_z \times Crisis_t} + \gamma X_{z,t} + \delta_{c,t} + \lambda_z + \epsilon_{z,t}. \qquad \text{(IV3)}$$

[19] I consider the following SEC offices: headquarters – Washington D.C.; regional offices – New York City; Miami; Chicago; Denver; Los Angeles. District offices are excluded following Kedia and Rajgopal (2011).
[20] *Distance* is the distance between a bank's headquarters and the closest SEC office, and *Public* is an indicator variable equal to 1 if the bank or its parent holding company is publicly traded.
[21] I present IV analyses for the credit amounts and the distressed sales in Table A14 of the online appendix. The IV estimates are statistically significant and generally larger than the OLS estimates.

The primary independent variable is the instrumented variable, $\widehat{Exposure_z \times Crisis_t}$, from the first-stage model.[22] *Public Share*$_{z,t}$, the market share of public banks in ZIP code z in year t, is also controlled for, and all the ZIP-code-level controls and fixed effects from equation (1) are included.

Table 3 reports the IV estimations of equations (IV2) and (IV3). Column (1) shows a strong first-stage relation between the instrument and the exposure to banks' DLR.[23] The second-stage model is estimated in columns (2) and (3), and all coefficients are significantly negative. The coefficient in column (2) ((3)) suggests that a one-standard-deviation increase in the exposure to banks' DLR produces a 4.7- (3.5-) percentage-point decrease in the growth rate of the FHFA house-price index (CoreLogic house price index) during the crisis. The IV estimates can be larger than the OLS estimates for several reasons. First, measurement errors in the exposure measure would attenuate the OLS estimates but not the IV estimates. Second, a positive correlation between the exposure measure and any unobservable factors that positively affect house prices can attenuate the OLS estimates. For example, if high-DLR banks operated in areas hard hit by the crisis and the government intervened in these areas, it could also attenuate the OLS estimates.[24] Finally, banks' DLR may affect banks differently (i.e., heterogeneous treatment effect); then, the IV estimates provide the local average treatment effect (LATE) of the compliant subpopulation (Imbens and Angrist, 1994). Although pinning down the exact reason for the difference is

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[22] In the second stage, the main variable of interest is the interaction term of *Exposure* and *Crisis*. Thus, the interaction term of *SEC Influence ZIP* with *Crisis* is included in the first stage. Wooldridge (2002) suggests that the interaction term *zw*, where *z* is an instrument and *w* is exogenous, should be included in the first stage when we analyze a model with the interaction term *xw*, where *x* is endogenous and *w* is exogenous. Bun and Harrison (2019) introduce Rajan and Zingales (1998), Aghion et al. (2005), and Dougherty (2005) as well-known examples. Huber (2018) also uses the IV approach in a difference-in-differences design, which is similar to my approach.

[23] Cameron and Miller (2015) suggest that the popular rule of thumb from Stock et al. (2002) is developed with the assumption of i.i.d. errors, and thus, it is not feasible for clustered errors. Keeping this caveat in mind, I find that the partial F-statistic is 70.98 ($p<0.01$), which is significantly larger than the critical value of 8.96 suggested by Stock et al. (2002) and Larcker and Rusticus (2010) to avoid a weak instrument.

[24] For example, the U.S. government issued the "Housing and Economic Recovery Act of 2008 (HERA)" and implemented various programs to boost the housing market.

challenging, both the OLS and IV estimates suggest that banks' DLR was negatively associated with an economically significant decrease in house prices during the crisis.

Finally, one notable concern is that the IV *SEC Influence ZIP* may still be correlated with other management decisions that are often bundled with loan loss provisioning, such as risk-taking choices. To reduce this concern, in untabulated analysis, I explicitly control for risk-taking proxies. As proxies, I use *Z-score* and *Asset Vol*, following existing studies (e.g., Berger et al., 2014). Then, I construct ZIP-code-level variables before the crisis (*Z-score ZIP Pre* and *Asset Vol ZIP Pre*) and re-estimate the IV regressions by including *Z-score ZIP Pre*×*Crisis* and *Asset Vol ZIP Pre*×*Crisis*. The second-stage IV estimates become smaller but remain statistically significant (-0.034, p<0.01; -0.031, p<0.01), suggesting the IV results are not likely driven by management's risk-taking choices. However, I acknowledge that the IV analysis cannot comprehensively eliminate the concern that some unobservable bank characteristics that are correlated with the IV may exist.

4.3. Identifying the Credit-Crunch Channel: Bank-Level Analysis

After showing the economy-wide effects, I investigate the effects of DLR at the bank level to provide further evidence on how high-DLR banks affected the housing market. In a credit crunch, high-DLR banks would reduce loan supply during the crisis. Figure 4 shows the mortgage amounts originated by different lenders in high- and low-exposure ZIP codes. The top-left panel presents total mortgage amounts by all lenders and suggests that demand for mortgage loans decreased during the crisis for both high- and low-exposure ZIP codes. However, high-exposure ZIP codes experienced a larger credit cycle than low-exposure ZIP codes, consistent with banks' DLR being positively associated with lending pro-cyclicality. Also, the top-right panel indicates the total mortgage amounts by high-DLR banks during the crisis were significantly smaller in high-exposure ZIP codes than in low-exposure ones. Notably, similar to Figure 3, the difference between high- and low-exposure ZIP codes is much more pronounced during the crisis, suggesting banks' financial reporting plays a more significant role during bad times.[25]

To identify the credit-crunch effect, demand effects need to be distinguished from supply effects, and financial-reporting effects need to be separated from other bank characteristics. I employ several approaches to mitigate these concerns. First, I conduct the bank-ZIP-code-level analysis with ZIP-year and bank fixed effects. Including ZIP-year fixed effects ensures that high-DLR banks are compared with low-DLR banks within the same ZIP code and year, controlling for local demand, because the assumption that local economic conditions are similar within the ZIP code and year is reasonable. The inclusion of bank fixed effects controls for time-invariant bank characteristics such as the business model and loan-portfolio composition. I estimate the following regression model from 2004 – 2009:

$$VOL_{i,z,t} = \beta_1 DLR\ High_i \times Crisis_t + \beta_2 Y_{i,t} + \beta_3 Y_{i,t} \times Crisis_t + \delta_{z,t} + \lambda_i + \epsilon_{i,z,t}. \qquad (2)$$

The unit of observation is bank-ZIP-year. The dependent variable, $VOL_{i,z,t}$, is the volume of mortgages originated by bank i in ZIP code z in year t, and it is normalized by the total new mortgage amounts in the same ZIP-year and multiplied by 100. $DLR\ High_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median, and $Crisis_t$ is equal to 1 if the year is 2007–2009. The bank-level control variables, $Y_{i,t}$, include *logAssets*, *Cash*, *Deposits*, *Lag Tier 1 Capital Ratio*, *Loans to Deposits*, *Loan Loss Reserve*, *Non-performing Loan*, *ROA*, and *Off-Balance-Sheet Rate*. Additionally, the interaction of $Y_{i,t}$ and $Crisis_t$ is included to allow banks' characteristics to have different effects before and during the crisis.

[25] This result is similar to that of Jiménez et al. (2017), who find that firms were not affected by the introduction of dynamic provisioning during good times, because they could easily substitute credit from less affected banks. Similarly, the U.S. mortgage-lending market is competitive, so borrowers may switch to another lender when a certain bank cannot lend to them. However, the substitution across lenders becomes more challenging during bad times as lenders tend to contract their lending during bad times.

Finally, ZIP-year fixed effects, $\delta_{z,t}$, and bank fixed effects, λ_i, are included. All regressions are weighted by the ZIP-code population, and all variables are defined in Appendix A.

Table 4 presents the results from the estimation of equation (2). Column (1) provides the result without fixed effects, and the coefficient of *DLR High×Crisis* is significantly negative (-0.438, p<0.01), suggesting that high-DLR banks reduced loan supply more than low-DLR banks during the crisis. Column (2) provides results with the ZIP-year and bank fixed effects. The coefficient diminishes (-0.360, p<0.01), suggesting that the loan-supply decrease was driven partly by local economic conditions or unobservable bank characteristics. However, the coefficient remains statistically significant even with extensive fixed effects. The coefficient of -0.360 implies that high-DLR banks decreased their supply of mortgages by 0.36% of the total ZIP-code-level mortgage origination relative to low-DLR banks during the crisis. The magnitude is economically meaningful, because it accounts for approximately 18% of the mean and 11% of the standard deviation of $VOL_{i,z,t}$. Columns (3) and (4) present results for the sample below- and above-median values of *Lag Tier 1 Capital Ratio* by bank and year. That is, I separately examine the credit-crunch effect for low- and high-capitalized banks. The coefficient of *DLR High×Crisis* is significantly negative for low-capitalized banks (-0.454, p<0.01), whereas the coefficient for high-capitalized banks is statistically insignificant. Also, columns (5) and (6) present results for the sample with below- and above-median values of lagged *ALWN* (*Loan Loss Reserve* to *Non-performing Loan*) by bank and year. *ALWN* captures how much a bank has accumulated loan loss allowance compared with its current non-performing loans; thus, it proxies for potential loss overhangs. The coefficient of *DLR High×Crisis* is significantly negative for low-ALWN banks (-

0.444, p<0.01) but insignificant for high-ALWN banks.[26, 27] These results suggest that the credit-crunch effect arises only for low-capitalized and high-loss-overhang banks, consistent with the credit-crunch theory (Beatty and Liao, 2011).

The prior approach cannot address the concern that unobservable bank characteristics could be correlated with banks' loan loss provisioning and lending decisions. To mitigate this concern, I exploit within-bank variation in different mortgages by comparing conventional and Federal Housing Administration (FHA) loans.[28] A conventional loan is a regular mortgage not guaranteed by any government agency, whereas an FHA loan is a mortgage insured by the FHA. FHA loans have a zero-risk weight in calculating banks' risk-weighted assets, have lower credit risks than conventional loans, and can easily be sold to government-sponsored enterprises (GSEs). These loans impose a lower capital burden on banks, and thus, I expect that high-DLR banks tightened their supply of conventional loans significantly more than FHA loans during the crisis. The key idea is that loans within the same bank and region are likely to share unobservable demand and bank variables that may affect the bank's lending. To better understand this approach, consider the following two equations:

$$\begin{aligned} VOL^{C}{}_{i,m,t} = {} & \beta_1^C DLR\ High_i \times Crisis_t + \beta_2^C Y_{i,t} + \beta_3^C Y_{i,t} \times Crisis_t \\ & + Unobservable\ demand_{m,t}\ or\ bank\ variables_{i,t} + \epsilon_{i,m,t}^{C}, \end{aligned} \tag{3a}$$

[26] In these regressions, comparing the coefficients for the partitioned samples is difficult due to the highly complex fixed-effects structure. However, when I run the credit-crunch analyses with triple-interaction terms in regressions (e.g., *DLR High×Crisis×Low Cap 1*), the triple-interaction terms are all statistically significant (untabulated).
[27] In the cross-sectional analyses based on the lagged capital ratio and ALWN, the sample observations are unbalanced because large banks tended to maintain a lower capital ratio and lower allowances and were present in more ZIP codes. To rule out the concern that the findings are merely driven by the size split, I explicitly control for the bank size and conduct a horserace between variables of interest in Table A18 of the online appendix. I find that the inclusion of the size variable does not significantly change the coefficients of the variables of interest.
[28] This approach is similar to that of Loutskina and Strahan (2009, 2011) who exploit the inability of Fannie Mae and Freddie Mac to purchase jumbo mortgages to examine the impact of banks' liquidity on mortgage supply.

$$VOL^{F}{}_{i,m,t} = \beta_1^F DLR\ High_i \times Crisis_t + \beta_2^F Y_{i,t} + \beta_3^F Y_{i,t} \times Crisis_t$$
$$+ Unobservable\ demand_{m,t}\ or\ bank\ variables_{i,t} + \epsilon_{i,m,t}^F. \tag{3b}$$

The unit of observation in these regressions is bank-MSA-year.[29] The dependent variable, $VOL^{C}{}_{i,m,t}$ or $VOL^{F}{}_{i,m,t}$, is the volume of conventional or FHA mortgages originated by bank *i* at MSA *m* in year *t*, and they are normalized by the total new mortgage amounts in the same MSA-year and multiplied by 100. Importantly, each equation may contain unobservable demand-side or bank-specific variables that potentially bias estimation of the effect of banks' DLR on mortgage supply. Then, the difference between the two equations removes unobservable variables as follows:

$$VOL^{C}{}_{i,m,t} - VOL^{F}{}_{i,m,t} = (\beta_1^C - \beta_1^F) DLR\ High_i \times Crisis_t + (\beta_2^C - \beta_2^F) Y_{i,t}$$
$$+ (\beta_3^C - \beta_2^F) Y_{i,t} \times Crisis_t + \delta_{m,t} + \lambda_i + \eta_{i,m,t}. \tag{3c}$$

The estimation of equation (3c) provides the effect of banks' DLR on their supply of high-capital-burden loans (conventional loans) compared with that of low-capital-burden loans (FHA loans). The bank-level control variables $Y_{i,t}$ and the interaction of $Y_{i,t}$ and $Crisis_t$ are included as in equation (2). I include MSA-year fixed effects, $\delta_{m,t}$, to control for unobservable common conditions that affect an MSA in a given year, and bank fixed effects, λ_i, to control for unobservable time-invariant bank characteristics, as in equation (2). All regressions are weighted by the MSA population.

Panel A of Table 5 presents results from the estimations of equations (3a) through (3c). Columns (1) through (3) report the results for the volume of conventional loans, the volume of FHA loans, and the difference in the volume of conventional and FHA loans as the dependent variable. In columns (1) and (2), the coefficients of *DLR High*×*Crisis* are significantly negative (-0.064, p<0.01; -0.015, p<0.01), suggesting that high-DLR banks reduced both conventional and

[29] I define local markets at the MSA level instead of the ZIP-code level in these regressions because many banks supply only one type of loan in a given ZIP code and year.

FHA loan supply more than low-DLR banks during the crisis. In column (3), the coefficient of *DLR High*×*Crisis* is significantly negative (-0.046, p<0.01), suggesting that high-DLR banks reduced conventional loan supply more than FHA loan supply during the crisis. The coefficient of -0.046 implies that high-DLR banks decreased their conventional-loan volume by 0.046% of the total MSA-level mortgage originations, relative to FHA loans, during the crisis. The magnitude is approximately 9% of the mean (0.496) and 4% of the standard deviation (1.290) of the dependent variable ($VOL^C - VOL^F$), which is economically meaningful. In Table A19 of the online appendix, I also separately examine the credit-crunch effect for low- and high-capitalized banks and low- and high-ALWN banks. I find that the coefficients of *DLR High*×*Crisis* are stronger for low-capitalized banks and low-ALWN banks, consistent with the credit-crunch theory.

In addition, I use the loan-level approval decision as the dependent variable. Studies use approval rates to capture better loan-supply decisions conditional on the number of applications (Loutskina and Strahan, 2009, 2011; Xie, 2016; Dou et al., 2018; Kim et al., 2019). This approach allows for more stringent fixed effects, which mitigates a concern that local demand may affect the two types of mortgages differently. I include year-loan-type, bank-loan-type, ZIP-year, race, and loan-purpose fixed effects. Year-loan-type fixed effects control for unobservable time-varying demand for different kinds of mortgages. Bank-loan-type fixed effects control for unobservable heterogeneous demand for different types of mortgages within the same bank. ZIP-year fixed effects control for unobservable loan demand in the same ZIP code and year. Finally, race and loan-purpose fixed effects control for unobservable heterogeneous demand by borrowers of different races (Asian, black, white, etc.) and different loan purposes (home purchase vs. refinancing). Panel B of Table 5 presents the results of the loan-approval decision. The coefficient of *Conventional*×*DLR High*×*Crisis* is -0.040 (-0.042) in column (1) ((2)), suggesting that high-

DLR banks reduced their approval rates for conventional loans by 4.0 (4.2) percentage points more than FHA loans during the crisis for the full sample (the sample excluding jumbo loans). Again, in Table A20 of the online appendix, I examine low- and high-capitalized banks and low- and high-ALWN banks. The coefficients of *Conventional*×*DLR High*×*Crisis* are significantly negative only for low-capitalized banks and low-ALWN banks. Because the approval decision based on the triple-interaction term provides a relative effect but does not speak to the overall volume effect, the results cannot be interpreted as the credit-crunch effect per se. However, these findings are consistent with the previous results on the credit-crunch effect by high-DLR banks.

Finally, I use an IV approach to isolate an exogenous shift in banks' loan loss provisioning. I use *–log(distance)*×*Public* as an instrument for banks' DLR. Table A21 of the online appendix reports the IV estimations at the bank-ZIP level. I find that the credit-crunch effect is stronger for low-capitalized banks and low-ALWN banks, consistent with the OLS results.

4.4. Identifying the Distressed-Sales Channel: Loan-Level Analysis

Next, I examine another channel through which banks' DLR may affect house prices: distressed sales. High-DLR banks would have taken more risk before the crisis, and homes with mortgages originated by them would be more likely to be sold via foreclosures or short sales, leading to further declines in house prices during the crisis. I begin with a descriptive analysis of the relation between exposure to banks' DLR and geographical characteristics during the pre-crisis. Table A22 of the online appendix shows that exposure to banks' DLR is associated with higher nonbank share, larger minority populations, and smaller populations with a bachelor's degree or higher. These results suggest that high-DLR banks operated in regions with socioeconomic conditions that are positively associated with potentially higher credit risks. Similarly, Table A23 of the online appendix suggests that the mortgages approved by high-DLR banks are associated with a higher loan-to-income ratio, minorities, lower owner-occupancy, and a higher likelihood of being sold, which could be associated with higher credit risks. The descriptive analyses suggest that high-DLR banks took more risk before the crisis.

One notable concern about the distressed-sales channel is that local economic conditions might drive mortgage distress during the crisis. I exploit granular mortgage loans and housing-transaction data to mitigate this concern. I first identify mortgages originated by high- and low-DLR banks before the crisis, using the matched HMDA-DataQuick loan-level data. I then track those mortgages over the crisis and compare their likelihoods of resulting in distressed sales in high-exposure ZIP codes. To test the distressed-sales channel, I estimate the following model:

$$\begin{aligned} Distressed\ Sale_{i,j,crisis} &= \beta_1 Exposure\ High_z \times DLR\ High_i \\ &+\beta_2 X_{z,t} + \beta_3 Y_{i,t} + \beta_4 W_{i,j,t} + \delta_i + \gamma_t + \kappa_r + \lambda_z + \epsilon_{i,j,crisis}. \end{aligned} \tag{4}$$

The generalized dependent variable is $Distressed\ Sale_{i,j,crisis}$, for which I use three different specific variables: *Foreclosure*, *Short Sale*, and *Any Distressed Sale*. These indicator variables are equal to 1 if mortgage j originated by bank i was foreclosed, became a short sale, or became either foreclosed or a short sale, during 2007–2010.[30] The primary independent variable is $Exposure\ High_z \times DLR\ High_i$, where $Exposure\ High_z$ is an indicator variable equal to 1 if $Exposure_z$ is above the ZIP-code-level sample median and $DLR\ High_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median. The same ZIP-code-level control variables, $X_{z,t}$, are included as in equation (1). The same bank-level control variables, $Y_{i,t}$, are included as in equation (2), and the mortgage-level control variables, $W_{i,j,t}$, include *logAmount*,

[30] I extend the crisis period to 2010 for the distressed-sales analysis, because I can only observe the date of housing transactions were closed, not dates when houses were initially foreclosed or became a short sale. Therefore, I allow one more year for a mortgage that initially became distressed in 2007–2009 to show up as a distressed sale in the data.

logIncome, *Loan-to-Income*, *Male*, *Ethnicity*, *Owner-Occupancy*, *Jumbo*, *Conventional*, and *Sold*. Finally, I include bank δ_i, year γ_t, race κ_r, and ZIP λ_z fixed effects.

Table 6 presents the results from the estimation of equation (4). Columns (1) through (3) report the results for the indicator variable of being foreclosed, being a short sale, and being either foreclosed or a short sale during 2007–2010. In columns (1) and (3), the coefficients of *Exposure High*×*DLR High* are significantly positive for *Foreclosure* and *Any Distressed Sale* (0.010, $p<0.01$; 0.011, $p<0.01$). The coefficient of 0.010 (0.011) in column (1) ((3)) implies that mortgages by high-DLR banks in high-exposure ZIP codes were 1.0 (1.1) percentage points more likely to be foreclosed (distressed sales) than mortgages by low-DLR banks during the crisis. The coefficient for *Short Sale* in column (2) is insignificant. The insignificant result could be because short sales are much less common than foreclosures, and errors in the fuzzy match could worsen matching and lead to less statistical power. In columns (4) through (6), I separately examine three types of mortgages: conforming, jumbo, and FHA loans.[31] The coefficients for conforming and jumbo loans are significantly negative (0.010, $p<0.05$; 0.013<0.01), but the coefficient for FHA loans is insignificant, suggesting that the distressed-sales effect arose only for conforming and jumbo loans. Overall, although the result is insignificant for short sales, the findings are consistent with the hypothesis that high-DLR banks took more risk before the crisis; thus, their mortgages became distressed more frequently during the crisis.

I emphasize that the results of Table 6 do not establish a causal story that high-DLR banks took more risks because of their DLR. Alternatively, more distressed sales by high-DLR banks

[31] By regulation, the government-sponsored enterprises (GSEs), such as Fannie Mae and Freddie Mac, may purchase only mortgages below a specific amount. The conforming loan limit (or the jumbo cutoff) varies by county and year. In 2009, the conforming loan limit for single-family mortgages ranged from $417,000 to $794,000. Mortgages with amounts exceeding the conforming loan limit are called jumbo loans.

could result from a greater bundle of bank characteristics, and DLR might just be a good proxy for banks' risk-taking. To examine this alternative story, I conduct a horserace between the DLR measure and the risk-taking proxies (z-score and asset volatility). Table A24 of the online appendix shows that the risk-taking proxies are positively correlated with distressed sales during the crisis in general. Once the risk-taking proxies are included, the coefficients of *Exposure High*×*DLR High* become smaller but remain statistically significant. These results suggest DLR is correlated with these risk-taking proxies, yet it seems to capture a unique aspect of banks' risk-taking. Given that no perfect proxies for banks' risk-taking exist, the alternative interpretation that DLR is a good proxy for risk-taking still seems empirically noteworthy and interesting.

Also, one may be concerned that the argument that banks' DLR weakens monitoring by stakeholders may be less applicable for mortgage loans, because mortgage loan loss provisions are generally less subject to management discretion (Acharya and Ryan, 2016; Bhat et al., 2021). To mitigate this concern, in Table A17 of the online appendix, I examine the effects of DLR by three different loan types—real estate, commercial, and consumer—following Bhat et al. (2021).[32] The exposure measures based on real estate loans show consistent results with the exposure measure based on all loans. Also, in Table A25 of the online appendix, I re-run equation (4) with the DLR measures by loan type based on Bhat et al.'s (2021) data to further check the plausibility of the monitoring mechanism. I find that distressed sales mainly vary with the real-estate-loan-based measure, but less with the consumer- and commercial-loan-based measures, consistent with Bushman and Williams' (2015) suggestion that stakeholders monitor less risk-taking in mortgage loans, due to delays in mortgage loan loss provisions.

[32] I thank Gauri Bhat, Josha Lee, and Stephen Ryan for sharing the loan-type allowances data in Bhat et al. (2021).

4.5. Robustness Tests for Demand Effects, Heterogeneity, Reallocation, and Other Concerns

I further examine whether the decrease in local demand drives the main findings. I create proxies for the decline in local demand: the percentage changes in the total number of employees, in the total number of business establishments, in the total adjusted gross income from 2006 to 2009, and the first principal component of the three demand proxies. In Figure A2 of the online appendix, I plot binned scatterplots of the ZIP-code-level exposure to DLR to these demand proxies. With the control variables and the county fixed effects, the ZIP-code-level exposure to DLR is not negatively correlated with the demand proxies, suggesting that it does not merely capture the decrease in demand. Also, I conduct a horserace between the exposure measure and the demand proxies in Table A1 of the online appendix. The idea of this analysis is similar to the test in Altonji et al. (2005), which includes proxies for potential concerns in the regression and checks whether the magnitude of the coefficient on the variable of interest moves significantly. If the local demand is the primary driver of the results, I expect the coefficients of $Exposure \times Crisis$ to change substantially. Contrary to this concern, the statistical significance and magnitudes of the coefficients of $Exposure \times Crisis$ remain similar to those in Table 2. Assuming that my proxies properly capture the local demand changes, the results mitigate the concern that the local-demand decrease is the primary driver of the findings.

Next, because the primary empirical strategy relies on within-county variation, I examine whether the effects of banks' DLR are heterogeneous across counties with different sizes. Table A2 of the online appendix presents the results for counties with populations greater than 1 million (County Pop > 1M), counties with populations less than or equal to 1 million but greater than 0.3 million (1M ≥ County Pop > 0.3M), and counties with populations less than or equal to 0.3 million (County Pop ≤ 0.3M). In general, the effects of banks' DLR increase in county population, meaning that the treatment effects are stronger where ZIP codes are more diverse and heterogeneous. The effects of banks' DLR for smaller counties are statistically significant in most outcomes. However, the results for *Short Sales Rate*, Δ*logHPI - FHFA*, and Δ*logHPI - CoreLogic* are not statistically significant for counties with populations less than or equal to 0.3 million (County Pop ≤ 0.3M). Several explanations are possible. First, cross-sectional variation within the county is the primary source for identifying the effects of banks' DLR in my research design. Thus, larger variation within the same county likely leads to larger estimated effects. Notably, counties with populations less than or equal to 0.3 million (County Pop ≤ 0.3M) contain only about 7.3 ZIP codes, on average, whereas counties with populations greater than 1 million contain 86.9 ZIP codes on average. Thus, with the stringent fixed effects and numerous control variables, only a small variation is left to estimate the effects of banks' DLR for small counties. Second, prior studies suggest a significant heterogeneity in housing-demand sensitivity to supply conditions (e.g., DeFusco and Paciorek, 2017; Adelino et al., 2020; Fang and Munneke, 2021). Because large metropolitan counties tend to be where housing market demand is more sensitive to economic conditions, the effects of banks' DLR are also more likely extensive in larger counties. Figure A3 of the online appendix plots the average housing-price levels and changes by different county size over the sample period. Consistent with the above conjecture, the housing cycle increases in county population, suggesting housing-market outcomes in larger counties are likely more sensitive to economic factors. Despite all these potential explanations, the research design using within-country variation may not eliminate confounding factors for large metropolitan counties; thus, I make a caveat that the OLS estimates in Table 2 can be overstated.

In addition, I examine the concern that a reallocation of mortgage lending could occur across banks or even ZIP codes, which can bias estimates from the difference-in-differences approach if control-group units benefit from a negative shock to treated units (Berg et al., 2021). To mitigate the concern, I investigate whether neighboring ZIP codes' exposure to DLR influences the focal ZIP code's housing outcomes, following Breuer and Breuer (2020). I construct the average neighboring ZIP codes' exposure to DLR with different radii: 10, 25, and 50 miles.[33] Then, I run the ZIP-code-level regression by controlling for these neighboring ZIP codes' exposures to DLR. If the substitution/reallocation is the primary driver of the results, I expect the coefficient of *Exposure*×*Crisis* to change substantially. Contrary to this concern, in Table A15 of the online appendix, the coefficients of *Exposure*×*Crisis* remain statistically and economically similar to those in Table 2. Further, in Table A16 of the online appendix, I find that the effect of banks' DLR on mortgage supply is larger for ZIP codes with less mortgage-loan substitutability by other institutions. This finding suggests that the limited substitution among mortgage lenders is likely to explain why DLR creates larger effects during bad times than good times.

I conduct a series of robustness tests. In Table A3 of the online appendix, I run the ZIP-code-level tests without any fixed effects to show that my results are not driven by a particular choice of fixed effects. The signs of coefficients remain the same, and the magnitudes of coefficients are within reasonable boundaries even without fixed effects. In Table A4 of the online appendix, I conduct the ZIP-code-level tests after excluding the four "sand states" (Arizona, California, Florida, and Nevada), which are notorious for boom-and-bust housing markets; one might be concerned that these states alone drive my results. I find that the magnitudes of coefficients are generally smaller than those in the tests with the full sample; however, the coefficients' statistical significance remains significant, which confirms that sand states do not

[33] To measure the distance between ZIP codes, I use the ZIP Code Distance Database provided by NBER. The distance is the great-circle distance, the shortest distance between two points on the surface of a sphere. For more detailed explanations, see https://www.nber.org/research/data/zip-code-distance-database.

solely drive the results. In Tables A5 – A11 of the online appendix, I conduct ZIP-code-level tests with different model specifications for DLR and the ZIP-code-level exposure to DLR to address the concern that my findings are sensitive to the model specification. Notably, in Table A7 of the online appendix, I construct DLR following a new approach, suggested by Beatty and Liao (2020), using the additions to nonaccruals (ΔNAL) instead of the changes in non-performing loans (ΔNPL). In all these variations, the signs and statistical significance of coefficients remain similar. In Tables A12 and A13 of the online appendix, I conduct ZIP-code-level tests with more stringent control variables. I also find that the signs and statistical significance of coefficients remain similar. Thus, overall, my findings remain robust to the various modifications.

5. Discussion of the Results and Conclusion

I examine the aggregate effects of banks' delayed loan loss recognition (DLR) on house prices during the Great Recession and the channels through which these potential effects arose. I find that high-exposure ZIP codes experienced larger decreases in mortgage supply, larger increases in distressed sales, and larger decreases in house prices during the crisis. The bank-level analyses suggest that the aggregate effects were likely driven by high-DLR banks reducing mortgage supply and their mortgages being distressed during the crisis. To gauge the results' economic significance, I estimate the impact of house-price changes on household consumption. I use a formula from Berger et al. (2018) for the individual response of consumption to a house-price shock. The estimated aggregate reduction in consumption associated with banks' DLR is about \$24.51 ~ \$26.04 billion, which is about 0.27% ~ 0.29% of U.S. household consumption in 2006 (\$9.021 trillion, FRED Economic Data).[34] Although this exercise relies on many assumptions,

[34] A more detailed description of this estimation appears in section C of the online appendix.

the back-of-envelope calculation suggests that banks' loan loss provisioning could have contributed to the Great Recession but was not the main culprit.

My paper provides implications for the new current expected credit loss (CECL) model. The CECL model allows management more discretion and judgment in loan loss provisioning than the incurred loss model (Walker, 2019). Thus, in the absence of mechanisms to limit the opportunistic use of this discretion, management will keep its ability to delay loss recognition under the new regime and may create similar issues during economic downturns. Given that I do not study the CECL standard, a caveat is that my findings' magnitudes may not generalize to the new regime; however, the mechanisms I identify may generalize to the new regime.

I conclude by acknowledging two limitations of the paper. First, although I employ various novel empirical strategies, I cannot entirely separate demand effects from supply effects. Instead, my strategy is to eliminate potential confounders one by one with different methods and provide collective evidence consistent with the supply-side explanation. Second, I do not identify a causal relationship between banks' DLR and their risk-taking. Instead, my findings only suggest that banks' DLR is positively associated with their risk-taking in the mortgage market; thus, the distressed-sales channel is a plausible mechanism through which banks' DLR contributed to the crisis. Future research should continue to address these difficult issues.

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Appendix A. Description of Variables

Variable	Description	Source
Dependent Variables		
ΔlogHPI - FHFA	Natural logarithm of changes in FHFA home-price index at the ZIP-code level.	FHFA
ΔlogHPI - CoreLogic	Natural logarithm of changes in CoreLogic home-price index at the ZIP-code level.	CoreLogic
logCredit	Natural logarithm of new mortgage amounts at the ZIP-code level.	HMDA
ΔlogCredit	Natural logarithm of changes in new mortgage amounts at the ZIP-code level.	HMDA
VOL	New mortgage amounts by an individual bank at the ZIP-code level divided by the total new mortgage amounts in the same ZIP-year and multiplied by 100.	HMDA
VOL^C	New conventional mortgage amounts by an individual bank at the MSA level divided by the total new mortgage amounts in the same MSA-year and multiplied by 100.	HMDA
VOL^F	New FHA mortgage amounts by an individual bank at the MSA level divided by the total new mortgage amounts in the same MSA-year and multiplied by 100.	HMDA
$VOL^C – VOL^F$	Difference between VOL^C and VOL^F.	HMDA
Foreclosure Rate	Number of foreclosed sales divided by the total number of housing transactions in the same ZIP code and the same year; foreclosures include *REO Liquidation (type S)* and *Foreclosure Auction (type A)* in the DataQuick's transaction file.	DataQuick
Short Sale Rate	Number of short sales divided by the total number of housing transactions in the same ZIP code and the same year; short sales include *Inferred Short Sale (type I)* in the DataQuick's transaction file. A short sale is a transaction in which the borrower sells the property for less than the outstanding mortgage balance under the agreement with the lender and pays the proceeds to the lender.	DataQuick
Foreclosure	An indicator variable equal to 2 if a mortgage is foreclosed during 2007–2010, and 0 otherwise.	DataQuick
Short Sale	An indicator variable equal to one if a mortgage becomes a short sale during 2007–2010, and 0 otherwise.	DataQuick
Any Distressed Sale	An indicator variable equal to 1 if a mortgage is foreclosed or becomes a short sale during 2007–2010, and 0 otherwise.	DataQuick
Approved	An indicator variable equal to 1 if a mortgage application is approved, whether or not the borrower accepts the loan, and 0 otherwise.	HMDA
Approved by High-DLR	An indicator variable equal to 1 if a mortgage is approved by a high-DLR bank.	HMDA
Explanatory Variables of Interest		
DLR	The adjusted R-squared from estimating equation (A1) minus the adjusted R-squared from estimating equation	Call Reports

	(A2); both equations are estimated within each bank over the prior 12 quarters during 2004–2006.	
DLR High	An indicator variable equal to 1 if the average of *DLR* during 2004–2006 is above the bank-level sample median, and 0 otherwise.	Call Reports
Exposure	Weighted average of individual banks' *DLR* based on banks' mortgage market shares during 2004–2006 at the ZIP-code level. This variable is standardized to have a mean of 0 and a standard deviation of 1.	Call Reports, HMDA
Exposure High	An indicator variable equal to 1 if *Exposure* is above the ZIP-code-level sample median, and 0 otherwise.	Call Reports, HMDA
Crisis	An indicator variable equal to 1 if the year is 2007, 2008, or 2009 (also equal to 1 if the year is 2010 for the distressed-sales analysis), and 0 otherwise.	
Conventional	An indicator variable equal to 1 if an application is conventional, and 0 otherwise.	HMDA
SEC Influence Bank	A bank-level instrumental variable defined as -*log(Distance)*×*Public*, where *Distance* is the distance between a bank's headquarters and the closest SEC office, and *Public* is an indicator variable equal to 1 if the bank or its parent holding company is publicly traded.	SEC, HMDA
SEC Influence ZIP	A ZIP-code-level instrumental variable and the weighted average of *–log(distance)*×*public* based on banks' mortgage market shares during 2004–2006 at the ZIP-code level.	SEC, HMDA
Control Variables – ZIP-code Level		
Lag Tier 1 Cap at ZIP	Weighted average of individual banks' *Lag Tier 1 Capital Ratio* based on their mortgage market shares at the ZIP-code level.	Call Reports, HMDA
ΔEmployment	Percentage change in the total number of employees at the ZIP-code level.	CBP
ΔEstablishment	Percentage change in the total number of business establishments at the ZIP-code level.	CBP
ΔGross Income	Percentage change in the total adjusted gross income at the ZIP-code level; data for the year 2003 are missing, so the percentage change for the year 2004 is calculated based on the year 2002 and is then annualized.	IRS SOI
logAve Income	Natural logarithm of the average adjusted gross income at the ZIP-code level (in $ thousands).	IRS SOI
HHI	Herfindahl-Hirschman Index; the sum of squared market shares of all lenders at the ZIP-code level.	HMDA
Nonbank Share	Nonbank lenders' market share at the ZIP-code level; nonbanks are lenders not under the regulatory oversight of OCC, FRS, FDIC, NCUA, or OTS.	HMDA
ΔlogNonbank Credit	Changes in the natural logarithm of originated mortgage loan amounts by nonbanks at the ZIP-code level.	HMDA
logPopulation	Natural logarithm of the total population at the ZIP-code level, estimated based on the collected data from 2007–2011.	ACS

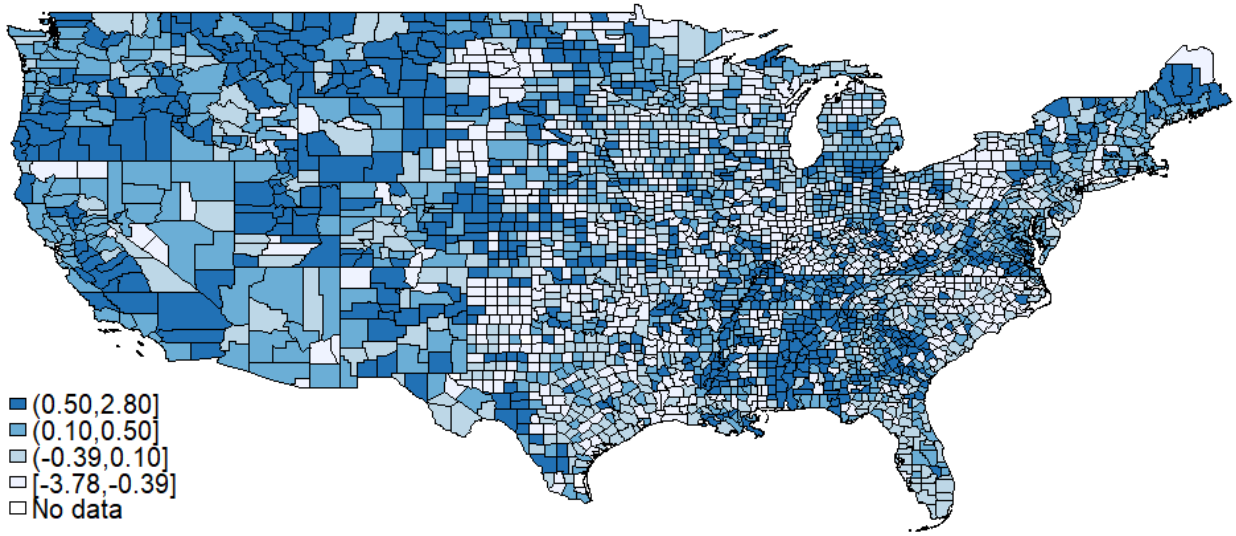
% African American	Percentage of Black or African American population at the ZIP-code level, estimated based on the collected data from 2007–2011.	ACS
% Hispanic	Percentage of Hispanic or Latino population at the ZIP-code level, estimated based on the collected data from 2007–2011.	ACS
% Poverty Population	Percentage of population below the poverty level at the ZIP-code level, estimated based on the collected data from 2007–2011.	ACS
% with Bachelor or Higher	Percentage of population with a bachelor's degree or higher at the ZIP-code level; the value is estimated based on the collected data from 2007–2011.	ACS
Public Share	Public banks' market share at the ZIP-code level.	HMDA
Z-score ZIP Pre	Weighted average of individual banks' *Z-score* based on banks' mortgage market shares during 2004–2006 at the ZIP-code level. This variable is standardized to have a mean of 0 and a standard deviation of 1.	Call Reports, HMDA
Asset Vol ZIP Pre	Weighted average of individual banks' *Asset Vol* based on banks' mortgage market shares during 2004–2006 at the ZIP-code level. This variable is standardized to have a mean of 0 and a standard deviation of 1.	Call Reports, HMDA
Control Variables – Bank Level		
logAssets	Natural logarithm of total assets.	Call Reports
Cash	Cash divided by total assets.	Call Reports
Deposits	Deposits divided by total assets.	
Lag Tier1 Capital Ratio	Lagged value of Tier 1 capital divided by risk-weighted assets.	Call Reports
Loans to Deposits	Loans and leases (net of unearned income) divided by total deposits.	Call Reports
Loan Loss Reserve	Allowance for loan and lease losses divided by total assets.	Call Reports
Non-performing Loan	Loan not accruing interest or accruing interest but 90 days or more past due (net of debt securities and other assets) divided by total assets.	Call Reports
ROA	Net income divided by total assets.	Call Reports
Off-Balance-Sheet Rate	Number of mortgages sold in the calendar year divided by the total number of approved mortgages in the same year.	HMDA
ALWN	Loan loss reserve divided by the non-performing loan.	Call Reports
Distance	Distance between banks' headquarters and their closest SEC office; Headquarters – Washington D.C.; Regional Offices – New York City, Miami, Chicago, Denver, and Los Angeles.	SEC
Public	An indicator variable equal to 1 if the bank or its parent holding company (identified using "RSSD9348 – ID of the regulatory high holder" or "RSSD9364 – ID of the financial high holder" from Call Reports) is publicly traded; trading status is identified using the CRSP-FRB link table (20161231).	Federal Reserve Bank of New York
Z-score	[Average (ROA) + Average (Equity/Total Assets)]/Standard deviation (ROA), where the means of	Call Reports

	ROA and Equity/Total Assets and the standard deviation of ROA are computed over the previous 12 quarters.	
Asset Vol	Standard deviation (Total Assets)/Average (Total Assets), where the standard deviation and the mean of Total Assets are computed over the previous 12 quarters.	Call Reports
Control Variables – Loan Level		
logAmount	The natural logarithm of the mortgage amount (in $ thousands).	HMDA
logIncome	The natural logarithm of the mortgage applicant's annual income (in $ thousands).	HMDA
Loan-to-Income	Loan amount divided by mortgage applicant's annual income.	HMDA
Male	An indicator variable equal to 1 if the mortgage applicant is male, and 0 otherwise.	HMDA
Ethnicity	An indicator variable equal to 1 if the mortgage applicant is Hispanic or Latino, and 0 otherwise.	HMDA
Owner-Occupancy	An indicator variable equal to 1 if the mortgaged home is owner-occupied, and 0 otherwise.	HMDA
Jumbo	An indicator variable equal to 1 if the mortgage size is larger than the conforming limit set by Fannie Mae and Freddie Mac.	HMDA, FHFA
Sold	An indicator variable equal to 1 if the mortgage is sold in the calendar year.	HMDA
Minority	An indicator variable equal to 1 if the mortgage applicant is Hispanic, Latino, African American, and 0 otherwise.	HMDA
Variables for DLR construction		
LLP	Loan loss provision divided by lagged total loans.	Call Reports
ΔNPL	Change in non-performing loans divided by lagged total loans.	Call Reports
EBLLP	Earnings before the loan loss provision and taxes divided by lagged total loans.	Call Reports
Tier 1 Ratio	Tier 1 capital divided by risk-weighted assets.	Call Reports
Size	Natural logarithm of total assets.	Call Reports
CoIndex	Coincident index at the state level.	Call Reports
ConsLoans	Consumer loans divided by total loans.	Call Reports
ReLoans	Real estate loans divided by total loans.	Call Reports
Charge-Offs	Loan charge-offs divided by lagged total loans.	Call Reports

Figure 1. Regional Exposure Variation

The figures present variation in regional exposure to banks' delayed loan loss recognition. Panel A presents a county map of regional exposure to banks' DLR. Panel B presents ZIP-code maps for three metropolitan areas: from left to right, the San Francisco area (San Francisco County and San Mateo County), the Chicago area (Cook County), and the Miami area (Miami-Dade County). Darker shadings reflect higher exposure.

Panel A: Exposure to Banks' DLR at the County Level



Panel B: Exposure to Banks' DLR at the ZIP-Code Level

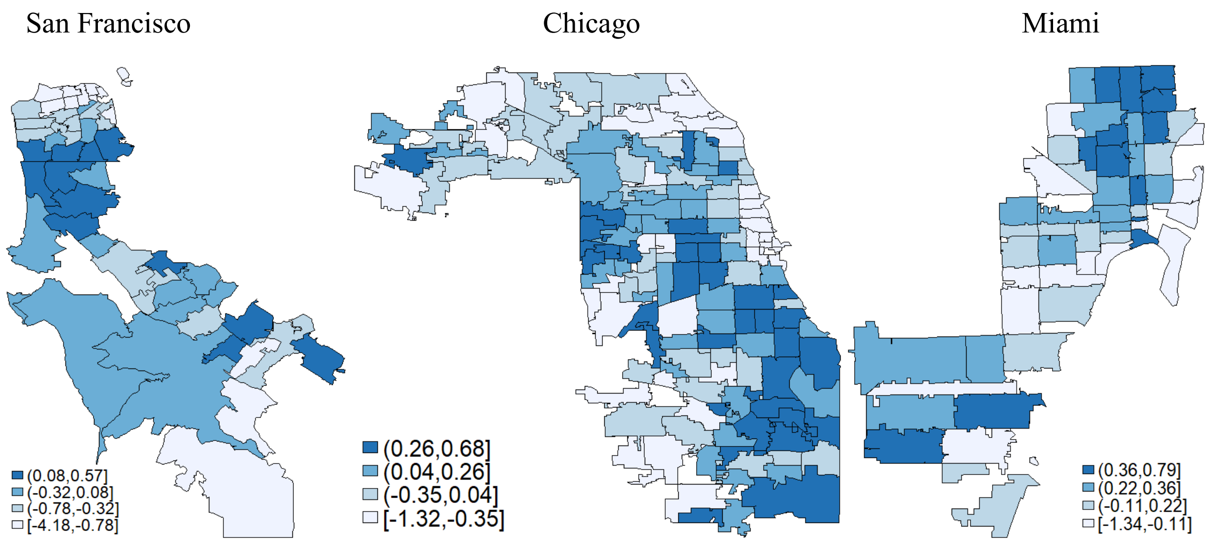


Figure 2. Binned Scatterplots for Credit Amounts, Distressed Sales, and House Prices

The figures present binned scatterplots of the ZIP-code-level exposure to banks' DLR versus standardized housing market outcomes (i.e., mean 0 and sd 1). The top-left panel plots the log difference in total new mortgage amounts from 2007 to 2010 on the y-axis. The top-right panel plots the average rate of distressed sales during the crisis on the y-axis. The bottom left panel plots the log difference in the Federal Housing Finance Agency (FHFA) home-price index from 2007 to 2010 on the y-axis. The bottom-right panel plots the log difference in CoreLogic's home-price index from 2007 to 2010 on the y-axis. All the averages of the ZIP-code-level control variables during the crisis and county fixed effects are controlled for, except Δ*Nonbank Credit*.

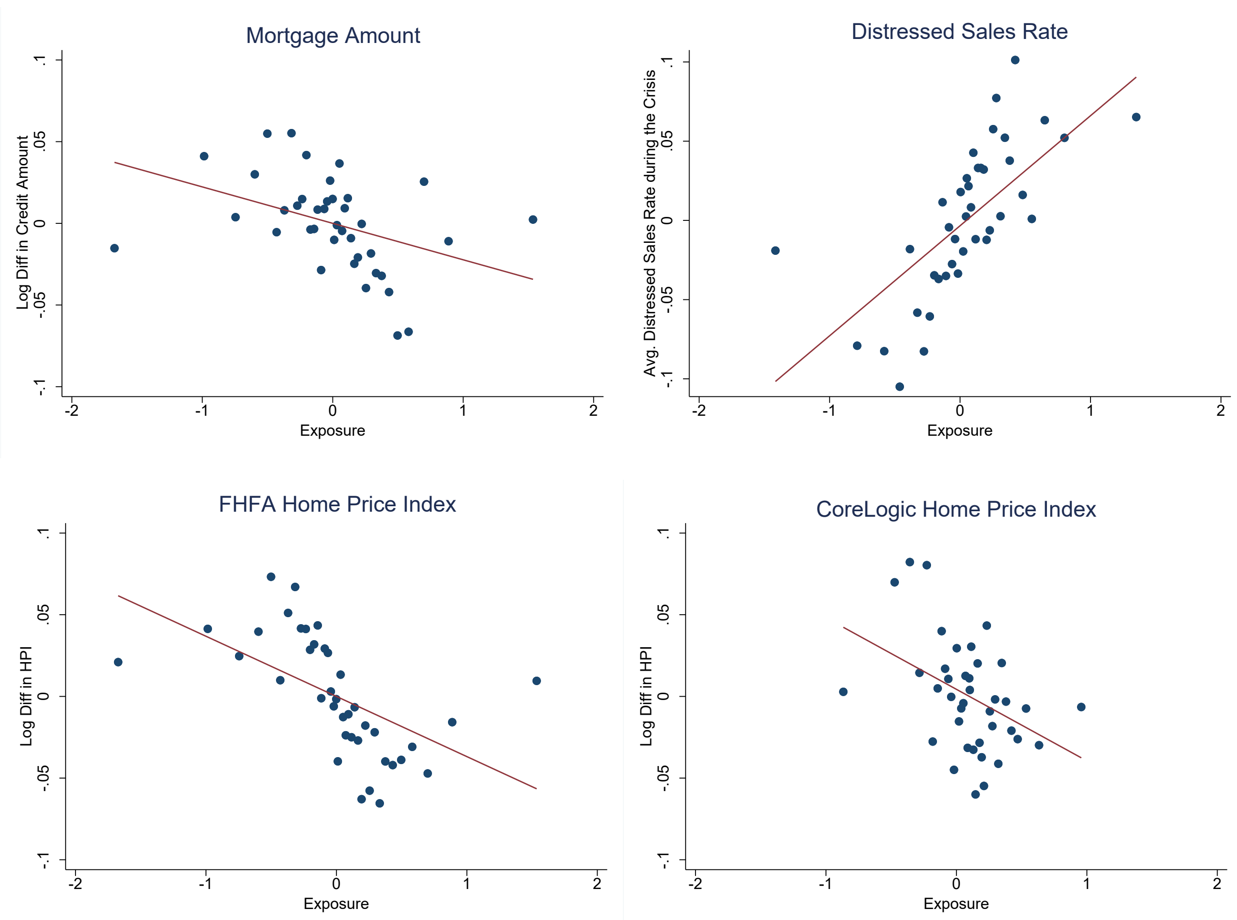


Figure 3. The Effect of the ZIP-Code-Level Exposure to Banks' DLR on House Prices

The figures display OLS regression coefficients and 95% confidence intervals based on standard errors clustered at the state level. The complete set of control variables with fixed effects in equation (1) is included, and regressions are weighted by the population of the ZIP code. To map out the pattern of exposure to banks' DLR, I include the interaction terms between the *Exposure* and *Year* indicators for every year except 2006, which serves as the benchmark period (i.e., the coefficient is set to zero). The left panel uses the log difference in the Federal Housing Finance Agency (FHFA) home-price index as a dependent variable, and the right panel uses the log difference in the CoreLogic home-price index as a dependent variable.

$$\Delta logHPI_{z,t} = \sum_{t=2004\,(\neq 2006)}^{2010} \beta_t Exposure_z \times I_t + \gamma X_{z,t} + \delta_{c,t} + \lambda_z + \epsilon_{z,t}$$

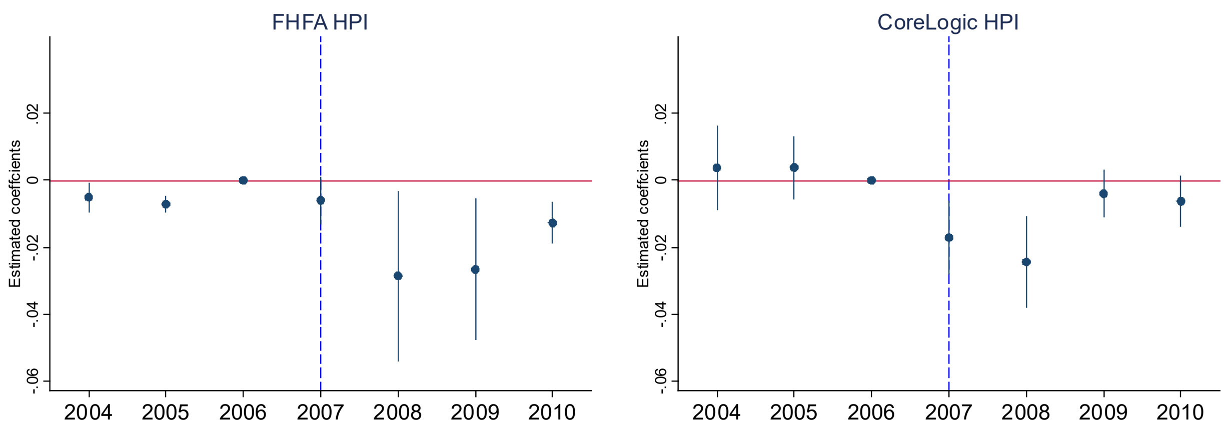


Figure 4. Time Series of Mortgage Amounts Originated by Institutions

The figures present total mortgage amounts originated by different lenders in above- (high) and below- (low) median ZIP-code-level exposure to banks' DLR. The top-left panel plots total mortgage amounts by *all lenders* in high- and low-exposure ZIP codes. The top-right panel plots total mortgage amounts by *high-DLR banks* in high- and low-exposure ZIP codes. The bottom-left panel plots total mortgage amounts by *low-DLR banks* in high- and low-exposure ZIP codes. The bottom-right panel plots total mortgage amounts by *nonbank lenders* in high- and low-exposure ZIP codes. All amounts are indexed to 2004.

Table 1. Descriptive Statistics

This table presents descriptive statistics (mean, standard deviation, and first through third quartiles) for the variables defined at various levels. Panel A reports the bank-level statistics. Panel B compares the bank-level variables by high- and low-DLR banks for the pre-crisis and crisis periods. Panel C reports the ZIP-code-level statistics. Panel D compares the dependent variables by high- and low-exposure ZIP codes for the pre-crisis and crisis periods. All variables are defined in Appendix A.

Panel A: Bank-Level Variables

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	Mean	Std. Dev.	P25	Median	P75
DLR High	24,957	0.500	0.500	0.000	1.000	1.000
logAssets	24,957	12.539	1.266	11.668	12.366	13.151
Cash	24,957	0.048	0.046	0.023	0.034	0.055
Deposit	24,957	0.818	0.082	0.781	0.834	0.875
Lag Tier1 Capital Ratio	24,957	0.136	0.060	0.102	0.119	0.149
Loans to Deposits	24,957	0.879	1.811	0.732	0.855	0.965
Loan Loss Reserve	24,957	0.010	0.006	0.007	0.009	0.011
Non-performing Loan	24,957	0.012	0.020	0.002	0.006	0.014
ROA	24,957	0.006	0.013	0.004	0.008	0.012
Off-Balance-Sheet Rate	24,957	0.236	0.325	0.000	0.000	0.485

Panel B: High- vs. Low-DLR Banks for the Pre-crisis and the Crisis Period

	High DLR Bank		Low DLR Bank		Difference	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	Mean	N	Mean	High - Low	t-stat
Pre-crisis (Year 2004 - 2006)						
logAssets	5,488	12.440	5,582	12.473	-0.034	-1.393
Cash	5,488	0.039	5,582	0.040	-0.001	-0.718
Deposit	5,488	0.818	5,582	0.816	0.002	1.181
Lag Tier1 Capital Ratio	5,488	0.140	5,582	0.136	0.004***	3.007
Loans to Deposits	5,488	0.900	5,582	0.869	0.031	0.830
Loan Loss Reserve	5,488	0.008	5,582	0.009	-0.000**	-2.424
Non-performing Loan	5,488	0.005	5,582	0.005	-0.000**	-2.012
ROA	5,488	0.011	5,582	0.010	0.000**	2.462
Off-Balance-Sheet Rate	5,488	0.207	5,582	0.217	-0.010*	-1.691
Crisis (Year 2007 - 2010)						
logAssets	6,994	12.578	6,893	12.633	-0.055**	-2.571
Cash	6,994	0.055	6,893	0.055	0.000	0.147
Deposit	6,994	0.819	6,893	0.817	0.002	1.602
Lag Tier1 Capital Ratio	6,994	0.136	6,893	0.133	0.003***	3.262
Loans to Deposits	6,994	0.899	6,893	0.850	0.049*	1.724
Loan Loss Reserve	6,994	0.011	6,893	0.011	-0.000**	-2.381
Non-performing Loan	6,994	0.018	6,893	0.019	-0.000	-1.570
ROA	6,994	0.003	6,893	0.003	0.001*	1.944
Off-Balance-Sheet Rate	6,994	0.252	6,893	0.258	-0.007	-1.158

Table 1. Continued

Panel C: ZIP-Code-Level Variables

VARIABLES	(1) N	(2) Mean	(3) Std. Dev.	(4) P25	(5) Median	(6) P75
Dependent Variables						
ΔlogHPI - FHFA	89,391	0.016	0.090	-0.035	0.014	0.067
ΔlogHPI - CoreLogic	41,276	-0.005	0.110	-0.066	-0.005	0.055
logCredit	89,391	10.644	1.238	9.737	10.655	11.556
ΔlogCredit	89,391	-0.112	0.293	-0.290	-0.110	0.065
Foreclosure Rate	60,598	0.117	0.137	0.019	0.066	0.161
Short-Sale Rate	60,598	0.031	0.045	0.000	0.013	0.042
Main Explanatory Variables						
Exposure	89,391	0.000	1.000	-0.430	0.132	0.579
Exposure High	89,391	0.501	0.500	0.000	1.000	1.000
ZIP-Code-Level Control Variables						
Lag Tier 1 Cap at ZIP	89,391	0.037	0.016	0.025	0.033	0.045
ΔEmployment	89,391	0.004	0.101	-0.048	-0.000	0.045
ΔEstablishment	89,391	0.004	0.052	-0.025	0.000	0.028
ΔGross Income	89,391	0.130	0.920	-0.002	0.039	0.075
logAve Income	89,391	3.919	0.439	3.641	3.833	4.106
HHI	89,391	0.060	0.037	0.036	0.050	0.073
Nonbank Share	89,391	0.260	0.114	0.177	0.252	0.335
ΔlogNonbank Credit	89,391	-0.120	0.463	-0.385	-0.099	0.167
ZIP-Code-Level Census Variables						
logPopulation	89,391	9.475	0.956	8.742	9.634	10.248
% African American	89,391	10.669	16.712	1.100	3.600	12.000
% Hispanic	89,391	10.936	15.594	1.900	4.500	12.500
% Poverty Population	89,389	9.581	6.967	4.300	7.900	13.000
% with Bachelor's or Higher	89,391	27.205	15.750	15.300	22.500	35.700
IV Analysis Variables						
SEC Influence	89,391	-1.249	0.482	-1.504	-1.168	-0.903
Public Share	89,391	0.278	0.115	0.192	0.263	0.350

Table 1. Continued

Panel D: High- vs. Low-Exposure ZIP Codes

	High Exposure ZIP		Low Exposure ZIP		Difference	
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	N	Mean	N	Mean	High - Low	t-stat
Pre-crisis (Years 2004–2006)						
ΔlogHPI - FHFA	19,062	0.090	18,949	0.074	0.015***	22.480
ΔlogHPI - CoreLogic	9,513	0.086	8,079	0.072	0.014***	11.254
logCredit	19,062	10.979	18,949	10.766	0.213***	16.629
ΔlogCredit	19,062	-0.077	18,949	-0.119	0.042***	15.495
Foreclosure Rate	13,119	0.045	11,995	0.045	0.000	0.508
Short-Sale Rate	13,119	0.009	11,995	0.008	0.001***	9.390
Crisis (Years 2007–2010)						
ΔlogHPI - FHFA	25,694	-0.044	25,686	-0.023	-0.020***	-32.707
ΔlogHPI - CoreLogic	12,772	-0.079	10,912	-0.052	-0.027***	-25.506
logCredit	25,694	10.490	25,686	10.461	0.029***	2.778
ΔlogCredit	25,694	-0.158	25,686	-0.087	-0.070***	-25.694
Foreclosure Rate	18,357	0.210	17,127	0.123	0.087***	55.820
Short Sale Rate	18,357	0.055	17,127	0.039	0.016***	29.286
All Periods						
% African American	44,756	11.980	44,635	9.354	2.626***	23.561
% Hispanic	44,756	12.872	44,635	8.995	3.877***	37.453
% Poverty Population	44,754	10.295	44,635	8.866	1.429***	30.812
% with Bachelor's or Higher	44,756	24.680	44,635	29.736	-5.057***	-48.627
logAve. Income	44,756	3.854	44,635	3.984	-0.130***	-44.841

Table 2. Effects of ZIP-Code-Level Exposure to Banks' DLR on Housing Market

This table presents regressions of various housing market variables on ZIP-code-level exposure to banks' DLR using the following model:

$$y_{z,t} = \beta Exposure_z \times Crisis_t + \gamma X_{z,t} + \delta_{c,t} + \lambda_z + \epsilon_{z,t}.$$

The dependent variable is $logCredit_{z,t}$, $\Delta logCredit_{z,t}$, *Foreclosure Rate*$_{z,t}$, *Short Sale Rate*$_{z,t}$, $\Delta logHPI_{z,t}$-*FHFA*, or $\Delta logHPI_{z,t}$-*CoreLogic*. The primary independent variable is the interaction term of the exposure measure and the crisis-period indicator. Panel A reports the results with the continuous variable $Exposure_z$, ZIP-code-level exposure to banks' DLR. Panel B reports the results with the indicator variable *Exposure High*$_z$, which is equal to 1 if $Exposure_z$ is above the sample median. $Crisis_t$ is equal to 1 if the year is 2007–2009 for the credit amounts and house prices, and is equal to 1 if the year is 2007–2010 for the distressed sales. The ZIP-code-level controls $X_{z,t}$ include *Lag Tier 1 Cap at ZIP*, Δ*Employment*, Δ*Establishment*, Δ*Gross Income*, *logAve Income*, *HHI*, *Nonbank Share*, and Δ*logNonbank Credit*. County-year fixed effects $\delta_{c,t}$ and ZIP-code fixed effects λ_z are included. All variables are defined in Appendix A. Regressions are weighted by the population of the ZIP code. Standard errors in parentheses are clustered at the state level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

Panel A: Results with the Continuous Exposure Measure

VARIABLES	(1) logCredit	(2) ΔlogCredit	(3) Foreclosure Rate	(4) Short Sale Rate	(5) ΔlogHPI - FHFA	(6) ΔlogHPI - CoreLogic
Exposure×Crisis	-0.142***	-0.105***	0.055***	0.010***	-0.016*	-0.017**
	(0.038)	(0.024)	(0.019)	(0.003)	(0.009)	(0.008)
Observations	73,513	73,513	58,907	58,907	73,513	33,439
ZIP Controls	YES	YES	YES	YES	YES	YES
County-Year FE	YES	YES	YES	YES	YES	YES
ZIP FE	YES	YES	YES	YES	YES	YES
Adj. Overall R-squared	0.970	0.808	0.882	0.877	0.896	0.926
Adj. Within R-squared	0.247	0.525	0.111	0.025	0.036	0.023

Panel B: Results with the Indicator Exposure Measure

VARIABLES	(1) logCredit	(2) ΔlogCredit	(3) Foreclosure Rate	(4) Short Sale Rate	(5) ΔlogHPI - FHFA	(6) ΔlogHPI - CoreLogic
Exposure High×Crisis	-0.165***	-0.129***	0.061***	0.010**	-0.021**	-0.012***
	(0.039)	(0.023)	(0.017)	(0.004)	(0.009)	(0.004)
Observations	73,513	73,513	58,907	58,907	73,513	33,439
ZIP Controls	YES	YES	YES	YES	YES	YES
County-Year FE	YES	YES	YES	YES	YES	YES
Zip FE	YES	YES	YES	YES	YES	YES
Adj. Overall R-squared	0.970	0.806	0.880	0.876	0.896	0.925
Adj. Within R-squared	0.240	0.522	0.100	0.019	0.035	0.017

Table 3. Effects of ZIP-Code-Level Exposure to Banks' DLR on House Prices: IV Analysis

This table presents instrumental-variable regressions of house-price changes on ZIP-code-level exposure to banks' DLR. The first-stage and the second-stage models are estimated as follows:

1st Stage: $Exposure_z \times Crisis_t = \beta SEC\ Influence\ ZIP_z \times Crisis_t + \gamma X_{z,t} + \delta_{c,t} + \epsilon_{z,t}$,

2nd Stage: $\Delta logHPI_{z,t} = \beta \widehat{Exposure_z \times Crisis_t} + \gamma X_{z,t} + \delta_{c,t} + \epsilon_{z,t}$.

SEC Influence ZIP$_z$ is an instrumental variable defined as $\frac{1}{3}\sum_{t=2004}^{2006}\sum_{i\in z}\alpha_{i,z,t} - log(Distance)_{i,t} \times Public_{i,t}$, where *Distance* is the distance between a bank's headquarters and the closest SEC office, and *Public* is an indicator variable equal to 1 if the bank or its parent holding company is publicly traded. The dependent variable is $\Delta logHPI_{z,t}$, changes in natural logarithm of either FHFA's or CoreLogic's price index at ZIP code z in year t. The primary independent variable is $\widehat{Exposure_z \times Crisis_t}$, the instrumental variable from the first-stage model. Column (1) reports the first-stage result, and columns (2) and (3) report the second-stage results. The ZIP-code-level controls $X_{z,t}$ include *Public Share*, *Lag Tier 1 Cap at ZIP*, Δ*Employment*, Δ*Establishment*, Δ*Gross Income*, *logAve Income*, *HHI*, *Nonbank Share*, and Δ*logNonbank Credit*. County-year fixed effects $\delta_{c,t}$ and ZIP-code fixed effects λ_z are included. All variables are defined in Appendix A. Regressions are weighted by the population of the ZIP code. Standard errors in parentheses are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

VARIABLES	(1) 1st Stage Exposure×Crisis	(2) 2nd Stage ΔlogHPI - FHFA	(3) 2nd Stage ΔlogHPI - CoreLogic
IV: SEC Influence ZIP×Crisis	1.054***		
	(0.125)		
Exposure×Crisis		-0.047**	-0.035***
		(0.023)	(0.012)
Public Share	0.149	-0.063**	-0.018
	(0.110)	(0.028)	(0.011)
Observations	73,513	73,513	33,439
ZIP Controls	YES	YES	YES
County-Year FE	YES	YES	YES
ZIP FE	YES	YES	YES
Adj. Overall R-squared	0.848		
Adj. Within R-squared	0.283	-0.021	0.010
Partial F-Stat.	70.98***		

Table 4. Effect of Banks' DLR on Mortgage Loan: Bank-ZIP Level

This table presents regressions of new mortgage amounts at the bank-ZIP level on high-DLR banks using the following model:

$$VOL_{i,z,t} = \beta_1 DLR\ High_i \times Crisis_t + \beta_2 Y_{i,t} + \beta_3 Y_{i,t} \times Crisis_t + \delta_{z,t} + \lambda_i + \epsilon_{i,z,t}.$$

The dependent variable is the volume of mortgages originated by bank i at ZIP code z in year t, and it is normalized by total new mortgage amounts in the same ZIP-year and multiplied by 100. The primary independent variable is $DLR\ High_i \times Crisis_t$, where $DLR\ High_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median, and $Crisis_t$ is equal to 1 if the year is 2007–2009. Columns (1) and (2) report the results for the full sample, columns (3) and (4) present results for the sample below- and above-median values of *Lag Tier 1 Capital Ratio* by bank and year, and columns (5) and (6) present results for the sample below- and above-median values of lagged *ALWN* by bank and year. The bank-level controls $Y_{i,t}$ include *logAssets*, *Cash*, *Deposit*, *Lag Tier 1 Capital Ratio*, *Loans to Deposits*, *Loan Loss Reserve*, *Non-performing Loan*, *ROA*, and *Off-Balance-Sheet Rate*. The interaction term of $Y_{i,t}$ and $Crisis_t$ is included. ZIP-year fixed effects $\delta_{z,t}$ and bank fixed effects λ_i are included for columns (2) – (6). All variables are defined in Appendix A. Regressions are weighted by the population of the ZIP code. Standard errors in parentheses are clustered at the state level. $***$, $**$, and $*$ denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

VARIABLES	(1) Full Sample VOL	(2) Full Sample VOL	(3) Low Cap 1 VOL	(4) High Cap 1 VOL	(5) Low ALWN VOL	(6) High ALWN VOL
DLR High×Crisis	-0.438***	-0.360***	-0.454***	0.047	-0.444***	-0.038
	(0.124)	(0.113)	(0.149)	(0.036)	(0.154)	(0.062)
DLR High	-0.466***					
	(0.078)					
Crisis	-4.887***					
	(1.143)					
Observations	1,283,506	1,283,407	961,140	313,769	807,693	471,388
Bank Controls	YES	YES	YES	YES	YES	YES
Bank Controls*Crisis	YES	YES	YES	YES	YES	YES
ZIP-Year FE	NO	YES	YES	YES	YES	YES
Bank FE	NO	YES	YES	YES	YES	YES
Adj. Overall R-squared	0.217	0.407	0.424	0.264	0.451	0.259
Adj. Within R-squared		0.028	0.031	0.003	0.033	0.012

Table 5. Effect of Banks' DLR on Mortgage Loan: Bank-MSA and Application Level

This table presents the effect of banks' DLR on mortgage loans at the bank-MSA level and application level. Panel A estimates three models at the bank-MSA level as follows:

$$VOL^{C}{}_{i,m,t} = \beta_1^C DLR\ High_i \times Crisis_t + \beta_2^C Y_{i,t} + \beta_3^C Y_{i,t} \times Crisis_t + \delta_{m,t}^C + \lambda_i^C + \epsilon_{i,m,t}^C,$$

$$VOL^{F}{}_{i,m,t} = \beta_1^F DLR\ High_i \times Crisis_t + \beta_2^F Y_{i,t} + \beta_3^F Y_{i,t} \times Crisis_t + \delta_{m,t}^F + \lambda_i^F + \epsilon_{i,m,t}^F,$$

$$\begin{aligned} VOL^{C}{}_{i,m,t} - VOL^{F}{}_{i,m,t} = {} & (\beta_1^C - \beta_1^F) DLR\ High_i \times Crisis_t + (\beta_2^C - \beta_2^F) Y_{i,t} \\ & + (\beta_3^C - \beta_2^F) Y_{i,t} \times Crisis_t + \delta_{m,t} + \lambda_i + \eta_{i,m,t}. \end{aligned}$$

The dependent variables are the volume of conventional loans, the volume of FHA loans, and the difference between conventional loans and FHA loans originated by bank *i* at MSA *m* in year *t*, and they are normalized by total new mortgage amounts in the same MSA-year and multiplied by 100. The primary independent variable is $DLR\ High_i \times Crisis_t$, where $DLR\ High_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median, and $Crisis_t$ is equal to 1 if the year is 2007–2009. The bank-level controls $Y_{i,t}$ include *logAssets*, *Cash*, *Deposit*, *Lag Tier 1 Capital Ratio*, *Loans to Deposits*, *Loan Loss Reserve*, *Non-performing Loan*, *ROA*, and *Off-Balance-Sheet Rate*. The interaction term of $Y_{i,t}$ and $Crisis_t$ is included. MSA-year fixed effects $\delta_{m,t}$ and bank fixed effects λ_i are included. All variables are defined in Appendix A. Regressions are weighted by the population of the MSA. Standard errors in parentheses are clustered at the state level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

Panel A: Effect of Banks' DLR on Mortgage Origination: Bank-MSA Level

VARIABLES	(1) Full Sample VOL^C	(2) Full Sample VOL^F	(3) Full Sample VOL^C - VOL^F
DLR High×Crisis	-0.064***	-0.015***	-0.046***
	(0.015)	(0.003)	(0.012)
Observations	125,493	125,493	125,493
Bank Controls	YES	YES	YES
Bank Controls*Crisis	YES	YES	YES
MSA-Year FE	YES	YES	YES
Bank FE	YES	YES	YES
Adj. Overall R-squared	0.549	0.381	0.534
Adj. Within R-squared	0.017	0.077	0.011

Table 5. Continued

Panel B presents regressions of mortgage approval on high-DLR banks at the application level using the following model:

$$Approved_{i,j,t} = \beta_1 Conventional_{i,j,t} \times DLR\ High_i \times Crisis_t + \beta_2 DLR\ High_i \times Crisis_t + \beta_3 Y_{i,t} + \beta_4 Y_{i,t} \times Crisis_t + \beta_5 W_{i,j,t} + year * loan\ type, bank * loan\ type, ZIP * year, race, loan\ purpose\ FEs + \epsilon_{i,j,t}.$$

The dependent variable is $Approved_{i,j,t}$, an indicator variable equal to 1 if a mortgage application j is approved by bank i in year t. The primary independent variable is $Conventional_{i,j,t} \times DLR\ High_i \times Crisis_t$, where $Conventional_{i,j,t}$ is an indicator variable equal to 1 if an application is conventional, $DLR\ High_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median, and $Crisis_t$ is equal to 1 if the year is 2007–2009. The bank-level controls $Y_{i,t}$ include *logAssets*, *Cash*, *Deposit*, *Lag Tier 1 Capital Ratio*, *Loans to Deposits*, *Loan Loss Reserve*, *Non-performing Loan*, *ROA*, and *Off-Balance-Sheet Rate*. The interaction term of $Y_{i,t}$ and $Crisis_t$ is included. The mortgage-level controls $W_{i,j,t}$ include *logAmount*, *logIncome*, *Loan-to-Income*, *Male*, *Ethnicity*, *Owner-Occupancy*, and *Jumbo*. All variables are defined in Appendix A. Standard errors in parentheses are clustered at the state level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

Panel B: Effect of Banks' DLR on Mortgage Approval: Application Level

VARIABLES	(1) Full Sample Approve	(2) Excluding Jumbo Loans Approve
Conventional×DLR High×Crisis	-0.040***	-0.042***
	(0.007)	(0.007)
DLR High×Crisis	0.029***	0.030***
	(0.008)	(0.008)
Observations	17,068,214	15,987,719
Bank Controls	YES	YES
Bank Controls*Crisis	YES	YES
Mortgage Controls	YES	YES
Race FE	YES	YES
Loan Purpose FE	YES	YES
ZIP-Year FE	YES	YES
Bank-Loan Type FE	YES	YES
Year-Loan Type FE	YES	YES
Adj. Overall R-squared	0.136	0.135
Adj. Within R-squared	0.020	0.019

Table 6. Effects of Banks' DLR on Distressed Sales: Matched Loan Level

This table presents regressions of distressed sales on high-DLR banks using the matched loan sample between HMDA and DataQuick based on the following model:

$$Distressed\ Sale_{i,j,crisis} = \beta_1 Exposure\ High_z \times DLR\ High_i$$
$$+\beta_2 X_{z,t} + \beta_3 Y_{i,t} + \beta_4 W_{i,j,t} + \delta_i + \gamma_t + \kappa_r + \lambda_z + \epsilon_{i,j,crisis}.$$

The dependent variable is *Distressed Sale*$_{i,j,crisis}$, which can be three different variables: *Foreclosure*, *Short Sale*, and *Any Distressed Sale*. These indicator variables are equal to 1 if a mortgage j originated by bank i is foreclosed, became a short sale, or became either foreclosed or a short sale during 2007–2010. The primary independent variable is *Exposure High*$_z$×*DLR High*$_i$, where *Exposure High*$_z$ is an indicator variable equal to 1e if *Exposure*$_z$ is above the ZIP-code-level sample median, and *DLR High*$_i$ is an indicator variable equal to 1 if the average of DLR during 2004–2006 is above the bank-level sample median. Columns (1) – (3) report results for the full sample, and columns (4) – (6) report results for the conforming loan sample, jumbo loan sample, and FHA loan sample, respectively. The ZIP-code-level controls $X_{z,t}$ include *Lag Tier 1 Cap at ZIP*, Δ*Employment*, Δ*Establishment*, Δ*Gross Income*, *logAve Income*, *HHI*, *Nonbank Share*, and Δ*logNonbank Credit*. The bank-level controls $Y_{i,t}$ include *logAssets*, *Cash*, *Deposits*, *Lag Tier 1 Capital Ratio*, *Loans to Deposits*, *Loan Loss Reserve*, *Non-performing Loan*, *ROA*, and *Off-Balance-Sheet Rate*. The mortgage-level controls $W_{i,j,t}$ include *logAmount*, *logIncome*, *Loan-to-Income*, *Male*, *Ethnicity*, *Owner-Occupancy*, *Jumbo*, *Conventional*, and *Sold*. Bank δ_i, year γ_t, race κ_r, and ZIP λ_z fixed effects are included. All variables are defined in Appendix A. Standard errors in parentheses are clustered at the state level. ∗∗∗, ∗∗, and ∗ denote significance at the 1%, 5%, and 10% levels in two-tailed tests.

	(1)	(2)	(3)	(4)	(5)	(6)
	Full	Full	Full	Conforming	Jumbo	FHA
VARIABLES	Foreclosure	Short Sale	Any Distressed Sale	Any Distressed Sale	Any Distressed Sale	Any Distressed Sale
Exposure High	0.010***	0.001	0.011***	0.010**	0.013***	0.001
×DLR High	(0.003)	(0.001)	(0.003)	(0.004)	(0.004)	(0.009)
Observations	674,578	674,578	674,578	525,303	120,050	26,722
ZIP Controls	YES	YES	YES	YES	YES	YES
Bank Controls	YES	YES	YES	YES	YES	YES
Mortgage Controls	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Race FE	YES	YES	YES	YES	YES	YES
Bank FE	YES	YES	YES	YES	YES	YES
ZIP FE	YES	YES	YES	YES	YES	YES
Adj. Overall R-squared	0.068	0.021	0.077	0.079	0.085	0.048
Adj. Within R-squared	0.004	0.002	0.005	0.005	0.005	0.001