Bank Regulation, Construction Financing, and the Supply of New Housing

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March 12 2026

Abstract

We show that post-GFC bank regulatory changes intended to curb bank risk-taking contributed to the decline in the supply of new housing. With unique data on the financing and sales of nearly 18,000 homebuilders, we document a sharp decline in construction loans from large banks subject to stress testing following the 2013 reforms, a reduction that was not offset by increases in financing from other sources. Employing a difference-in-differences (DiD) design, we find that, relative to homebuilders who previously borrowed from smaller banks, the credit availability and new home sales of homebuilders who previously borrowed from stress-tested banks declined after the reforms. Using a county DiD design, we find that counties where stress-tested banks held greater pre-GFC market shares provided fewer construction loans and experienced greater house price appreciation after 2013.

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1 Introduction

In the aftermath of the 2008 Global Financial Crisis (GFC), the production of new single family homes in the U.S. declined considerably. As Figure 1 illustrates, housing production remained low relative to historical averages, even as the housing market recovered starting at the end of 2012. The resulting "short-fall" in housing supply, arising from this reduction in construction, has been estimated at between 1.5 million to 5.5 million units, and is often linked to what has been characterized as a housing affordability crisis.[1]

This paper establishes a causal link between the post-GFC slowdown in homebuilding and the tightening of bank regulation. These policies consider construction loans among the riskiest of bank assets, and as a result significantly reduced the tendency of banks to provide construction financing. Because banks historically provided more than 90% of construction credit, and because roughly 60% of U.S. single-family homes were built by small and medium-sized homebuilders that relied predominantly on such credit, the regulatory-induced reduction in bank lending materially reduced credit availability to homebuilders. As we show, the reduction in homebuilder credit is associated with a reduction in the supply of new housing.

Our analysis uses data from deeds that record financing and sales activity for virtually all U.S. houses. From this data, we construct a dataset of nearly 18,000 U.S. single-family homebuilders and the various financial institutions that provide them credit. Our analysis focuses on the 2013 implementation of two key post-crisis regulations: Basel III and Dodd–Frank Act (DFA) stress testing.[2] Basel III increases risk-based capital and liquidity requirements, while DFA stress tests

[1] The stagnation in housing production and the decline in housing affordability have been widely documented in academic literature and policy reports, e.g. Baum-Snow and Duranton (2025), Glaeser and Gyourko (2025), Treasury (2024), and Congress (2023).

[2] Although many other regulatory changes also occurred, these two were among the most consequential for the U.S. banking system. Basel III began phased implementation in 2013, with large, systemically important banks among the first to comply; the Federal Reserve issued the final DFA stress-testing rule in October 2012, and the first DFA stress

assess banks' capital adequacy under adverse scenarios and can require additional capital buffers. Because construction lending is considered highly risky in stress tests and is capital intensive under Basel III, these changes made construction lending less attractive for the large stress-tested banks.[3] Since these large banks held more than 70% of total banking assets and originated roughly 42% of construction loans during 2010–2012, their retrenchment could materially constrain homebuilders' access to credit if the reduction in loans is not offset by other sources of capital.

To better understand the economic setting, we use our data to document several interesting facts about the U.S. homebuilding industry and its financing. First, the industry includes a small number of large national homebuilders and tens of thousands of small local homebuilders who account for roughly 60% of all new homes built from 2010 to 2020.[4] Second, whereas large homebuilders raise considerable financing through corporate bonds and syndicated loans, smaller homebuilders rely almost exclusively on bank credit. Finally, the median homebuilder borrowed from only two banks between 2010 and 2020, and the median distance between the location of their collateral and the nearest bank branch of their lenders is approximately ten miles. The size distribution of homebuilders, their reliance on bank financing, and the persistence and local nature of banking relationships suggest that local shocks to bank credit supply can materially affect the availability of homebuilder credit.

Consistent with the hypothesis that post-crisis regulatory reforms induced large stress-tested banks to retrench from construction lending, we document a pronounced decline in their market

tests were conducted in 2013 on covered banks.

[3]Basel III increased capital requirements for banks, inducing the largest banks to significantly increase their risk-weighted capital ratio following the phased implementation (Cecchetti, Kress, and Schoenholtz, 2025). This reduced banks' appetite for construction lending, as higher capital requirements diminish banks' incentives to engage in risky lending (Repullo, 2004).

[4]Because most new housing starts are single-family homes, and the post-global financial crisis (GFC) decline in single-family starts is the primary driver of low housing supply (see Figure 1), we focus on single-family houses and henceforth refer to their builders simply as "homebuilders."

share following the 2013 reforms.[5] As shown in Figure 2, the share of stress-tested banks in bank-originated construction loans falls from 43% in 2011 to 19% in 2019. A similar pattern emerges at the local level: across the 500 most populous counties, the average market share of these banks declines from 36% in 2011 to 14% in 2019. The drop in market share is partly driven by banks' exit from certain local markets, as the total number of stress-tested bank-county lending pairs fell from approximately 1,100 in 2011 to roughly 550 by 2019. Importantly, we show that non-bank lenders did not meaningfully offset this decline, consistent with the view that construction credit requires intensive monitoring (Heitz, Martin, and Ufier, 2026).

To formally test our hypothesis that regulatory reforms reduced the availability of credit and constrained the supply of new housing, we use difference-in-differences (DiD) estimates. Specifically, we compare the amount of bank loans and home sales of homebuilders that borrowed from stress-tested banks prior to the 2013 reforms to the amount of bank loans and home sales of homebuilders that previously only borrowed from non-stress-tested banks. Our DiD regressions essentially make this comparison both before and after the 2013 reforms and control for homebuilder and year fixed effects. The null hypothesis in these tests is that homebuilders can switch flexibly across financing sources, implying that banking relationships are not material. We test this hypothesis against the alternative that the 2013 regulations had a stronger effect on stress-tested banks, and that banking relationships are economically meaningful.

Our estimates reject the null in favor of this alternative. We find that homebuilders with prior relationships with stress-tested banks experienced a 31.5% decline in borrowing after the reforms relative to contemporaneous changes experienced by homebuilders which only borrowed from non-stress-tested banks before the reform.[6] Consistent with the view that homebuilders rely heavily

[5] We define stress-tested banks as bank holding companies that participated in the Federal Reserve's DFA stress tests, which in total include 35 bank holding companies.

[6] Because homebuilders do not necessarily borrow or sell homes every year, some observations have zero borrowing or zero home sales. To accommodate non-negative outcomes with many zeros, our benchmark specification uses

on external financing, our DiD estimates imply that homebuilders with relationships with stress-tested banks experienced an 11.1% decline in home sales after the reform, relative to homebuilders with only non-stress-tested bank relationships.

We then provide three additional pieces of evidence that are consistent with our hypothesis. First, using the within-firm estimation approach of Khwaja and Mian (2008), we show that, for the same homebuilder, lending relationships with stress-tested banks are 12.4 percentage points more likely to be terminated than relationships with non-stress-tested banks after the reform, confirming that the reforms induced a credit supply shock to homebuilders. This adjustment occurs entirely through relationship termination rather than through reductions in lending within surviving relationships, which is consistent with stress-tested banks withdrawing from certain local construction lending markets. Second, consistent with disrupted lending relationships that compel affected firms to search more broadly for financing, we show that the borrowing distance for homebuilders with prior relationships with stress tested banks rises by 27.3% relative to other homebuilders after the regulatory reforms. Third, because loans secured by vacant land are considered highly risky under DFA stress tests and require substantial capital, stress-tested banks are likely to require borrowers to pledge other properties as collateral after the 2013 reforms. Consistent with this prediction, we find that loans issued by stress-tested banks are 11.8 percentage points less likely to be collateralized by vacant land after the regulatory reforms, compared to loans issued by other banks.

The above evidence indicates that the 2013 reforms significantly reduced the credit that stress-tested banks supplied to their homebuilder clients, which in turn limited their ability to produce new houses. To assess whether this credit supply shock affected the aggregate amount of housing supplied, we conduct a county-level DiD analysis. Specifically, we exploit cross-sectional variation in counties' pre-GFC (2004–2007) market shares of stress-tested banks in construction lending as

Poisson pseudo-maximum likelihood (PPML), a state-of-the-art approach in this setting (Cohn, Liu, and Wardlaw, 2022). In Section 4, we also estimate extensive- and intensive-margin specifications and obtain similar results.

a measure of exposure intensity, comparing more exposed counties to less exposed counties, both before and after the 2013 reforms. These estimates control for county and year fixed effects as well as time-varying economic conditions.

We document three sets of county-level results that together suggest a causal chain from the regulatory shock to housing market outcomes. First, a one-standard-deviation increase in the pre-GFC market share of stress-tested banks is associated with a 13.8% decline in construction loan originations after 2013. Second, consistent with the prediction that a contraction in construction lending constrains new housing supply and exerts upward pressure on prices, we find that a one-standard-deviation increase in the pre-GFC market share of stress-tested banks is associated with an additional 3.7% increase in house prices between 2013 and 2017. For a county with the average market share of stress-tested banks, the implied effect accounts for roughly one-fifth of observed house price growth over this period. Third, the contraction in credit supply significantly weakens the responsiveness of new housing construction to local price changes in more exposed counties. Event-study estimates confirm that these effects emerge precisely with the introduction of the stress tests, with no evidence of differential pre-trends.

Our paper contributes to three literature. The first literature examines the recent decline in homebuilding and its implications for housing affordability (see, e.g., Baum-Snow and Duranton (2025)). In contrast to our analysis, which focuses on banking regulations, this literature considers explanations based on geographic constraints (Baum-Snow and Han (2024)) and housing regulation (Glaeser and Gyourko, 2025). While it is likely that geographical constraints and regulation partially explains the decline in new housing, even cities like Dallas, Houston, Atlanta, and Phoenix, traditionally characterized by permissive regulation, abundant land, and historically robust housing supply, have experienced house price increases in the last decade that are dramatically greater than construction cost increases (Potter and Syverson, 2025). Our work offers an alternative

bank regulation-based explanation that seems to better align with the time-series pattern observed in the data.

We also contribute to the banking literature on capital requirements and stress testing by offering firm-level causal evidence that post-crisis reforms curtailed bank lending and produced real effects by reducing new housing supply. Prior studies (see, e.g., Gropp, Mosk, Ongena, and Wix (2019), Benetton (2021)) generally provide supporting evidence that higher capital requirements raise banks' cost of lending, and thereby reduce credit supply. Related work documents that large banks curtailed lending following the GFC in response to stress testing, with the sharpest reductions occurring in riskier segments such as commercial real estate credit (Acharya, Berger, and Roman (2018), Gete and Reher (2018), Cortés, Demyanyk, Li, Loutskina, and Strahan (2020)). In many other credit markets, smaller banks and fintech lenders appear to have stepped in to fill the gap (Cortés et al. (2020); Gopal and Schnabl (2022)). However, the distinctive features of construction lending, e.g., the need for intensive local monitoring, make it less attractive for remote lenders to fill the gap generated by exiting local bank branches.

Finally, our work is closely related to the literature on the real effects of financial frictions. A central premise of our argument is that approximately 60% of new homes are built and sold by small- and medium-sized homebuilders, a pattern that may reflect land-use regulations that limit project scale (D'Amico, Glaeser, Gyourko, Kerr, and Ponzetto, 2024). Small firms are typically more financially constrained (Fazzari, Hubbard, and Petersen, 1988) and face greater informational asymmetries with lenders (Petersen and Rajan, 1994), which increases the cost of switching financing relationships. Consequently, credit supply shocks can have disproportionately large effects on these homebuilders and as a consequence, affect the aggregate housing supply.

The remainder of this paper is organized as follows. Section 2 provides institutional background and presents key stylized facts. Section 3 summarizes the data source and sample construc-

tion. Section 4 outlines the bank and homebuilder DiD analysis. Section 5 examines county-level DiD patterns. Additional details on data construction are provided in the Appendix.

2 Institutional Background

2.1 U.S. Housing Supply and Housing Prices in the Post-GFC Era

Following the Global Financial Crisis (GFC), housing starts declined sharply. While multifamily housing starts gradually recovered to pre-GFC levels, single-family housing starts remained historically low throughout the 2010–2019 period. The total number of single-family housing starts during this period was about 56% of the total recorded between 2000 and 2009 and 61% of the total between 1990 and 1999. In contrast, the U.S. population in 2019 had grown approximately 8% relative to its 2009 level and 19% relative to 1999. This resulted in the lowest ratio of new single-family housing starts to population in recorded history (Figure 1).

This historically low level of new housing supply is unlikely to have been driven solely by demand-side factors. As shown in Figure 4, rents rose steadily during this period, and median single family housing prices recovered and surpassed their pre-GFC peak by 2013. In addition, the home vacancy rate declined to a historically low level. These patterns are consistent with robust housing demand in this period, and a supply response that was too weak to offset the effect of higher demand.

While the popular press points to regulations that inhibit supply responses, we could not find evidence of large changes in relevant regulations during our sample period. As shown in Figure 5, a comparison of the 2006 and 2018 Wharton Residential Land Use Regulation Index surveys reveals no significant changes in zoning restrictiveness, permitting requirements, density restrictions, or development approval processes before and after the financial crisis. This suggests that while the

level of land-use regulation may constrain housing supply in the long run, it cannot by itself explain the historically low level of new housing supply in the last decade.

Although the timing of land-use regulatory changes do not match the reduction in new housing supply, these regulations clearly affect housing supply and are likely to complement and perhaps amplify the channel we propose. As argued by D'Amico et al. (2024), restrictive land-use regulation fragments housing development into small projects, which in turn prevents homebuilders from achieving the scale needed to efficiently manage large, dispersed operations. This increases the share of small, credit-constrained local homebuilders relative to large homebuilders with access to public debt and equity markets, as well as other sources of private capital. In this sense, land-use regulation and bank regulation interact—the former shapes an industry structure that is uniquely vulnerable to the credit supply shock created by the latter.

Finally, construction costs do not explain the post-crisis surge in housing prices. Using RSMeans data and FHFA price indexes, Potter and Syverson (2025) show that increases in average construction costs are relatively modest, and that the cross-sectional correlation between increases in costs and increases in housing prices is virtually zero. Historically, the relationship between housing prices and construction costs has been weak for coastal metropolitan areas and relatively stronger for cities that have traditionally built housing in large volumes, such as Dallas, Houston, Atlanta, and Phoenix. However, over the past decade, even many of these high-volume cities have experienced housing prices rising dramatically faster than construction costs.

In summary, declines in the supply of new housing do not appear to be primarily driven by negative demand shocks, increased regulation, or higher construction costs. To understand why supply lagged during this period, we examine the U.S. homebuilding industry, which lies at the center of new housing supply.

2.2 An Overview of the US Homebuilding Industry

Single-family housing is the largest segment of residential construction in the United States, accounting for approximately 72% of housing starts in recent years. Within this sector, homebuilders play a pivotal role: they acquire land from developers, lease or purchase equipment from engineering and construction firms, hire labor, and manage the overall construction process.[7]

The homebuilding industry comprises a small number of large national homebuilders and a much larger number of small local homebuilders. According to the U.S. Economic Census for Construction, although the number of homebuilders has declined from its peak in 2007, there were still approximately 18,000 homebuilders operating in the United States in 2022. As shown in Figure 6, the majority of these builders are relatively small, with most producing fewer than 10 homes per year and the median builder completing approximately five units annually (see Table 1). In contrast, several large public homebuilders construct tens of thousands of homes each year. While large builders have captured an increasing share of the market, small homebuilders continue to play an important role in the years following the GFC, supplying 60% of new single-family homes during this period.

Although homebuilders vary considerably in size, they generally adhere to a standardized development process. The process begins with land acquisition, followed by determining the optimal timing for construction based on operational and market conditions. Once construction starts, homebuilders may market and sell units at different stages—prior to, during, or after completion. Upon completion, transactions are formally recorded in the deed registry. Depending on project

[7] U.S. single-family homebuilders fall into three categories: (1) operative builders, who acquire land and construct homes on their own lots; (2) custom builders, who build bespoke homes on lots owned by their clients; and (3) manufactured and modular home producers, who construct homes in a factory setting and transport them to the final site. Because operative builders account for 74% of single-family houses built between 2000 and 2025 and are the primary suppliers of new homes in the residential market, this study focuses on operative builders and refers to them simply as homebuilders.

complexity, the entire development cycle may range from several months to multiple years.

Market segments differ significantly between large and small homebuilders. Large homebuilders typically focus on large-scale developments in suburban areas and operate across multiple regions. In contrast, smaller homebuilders are highly localized, concentrating on specific regional markets. The median homebuilder operates in only three counties during 2010-2023, mostly within the same metropolitan statistical area (MSA). These small homebuilders are also highly homogeneous, often working within established, higher-density neighborhoods.

2.3 The Debt Financing of US Homebuilders

Housing development is capital intensive and debt financing plays a pivotal role for homebuilders' business model. For example, between 2012 and 2023, surveys on Acquisition, Development, and Construction (ADC) Financing conducted by the National Association of Home Builders (NAHB) reveal that homebuilders obtain construction financing for 55% of their new housing projects on average (NAHB, 2025).[8] Notably, this figure excludes additional financing obtained for land acquisition and development activities.

Large national homebuilders and smaller local homebuilders obtain debt financing from very different sources. As shown in Figure 7, large homebuilders primarily access the syndicated-loan and corporate-bond markets, whereas small and medium-sized builders rely mainly on collateralized construction loans. Banks are the predominant providers of such loans to small builders, supplying roughly 90% over 2010–2012. Following the 2013 regulatory reforms, banks' overall market share declined and large homebuilders shifted toward corporate bonds, yet banks still supplied about 85% of collateralized construction loans to small homebuilders.

[8] 2025 survey results and the historical survey results can be found in this link: https://www.nahb.org/news-and-economics/housing-economics/indices/adc-financing-survey.

This reliance on banks is likely driven by the high risk associated with construction lending, as the collateral is typically under construction. As evidence of this risk, NAHB's ADC survey indicates that the typical effective borrowing rate on construction loans is roughly 8 percentage points above the federal funds rate.[9] To mitigate risk, construction loans employ draw schedules that disburse funds against predetermined project milestones; payments are released incrementally only after specific phases have been completed and verified. As demonstrated by Heitz, Martin, and Ufier (2026), this on-site monitoring plays a central role in reducing risk. Furthermore, soft information—such as borrowers' reputations, relationships with independent inspectors who can help the verification of construction process, and local market conditions—is critical for lowering monitoring costs and mitigating risk in construction lending.

As further evidence of the importance of monitoring and soft information, we find that the median borrowing distance, approximated by the distance between the property used as collateral and the nearest branch of the lender, remains approximately ten miles across all years in the sample. Furthermore, the median homebuilder borrowed from only two different banks throughout the sample period, indicating substantial frictions in switching lenders.

The heavy reliance on banks for construction financing, combined with the concentration of borrowing sources, implies that any decline in bank credit supply would have a substantial impact on homebuilders' credit conditions and operations. In the next section, we examine how post-GFC regulatory changes may have reduced banks' credit supply to homebuilders.

[9]These loans also exhibit high loan-to-value ratios, typically exceeding 75%, as reported in survey results.

2.4 Post-GFC Banking Regulation and Its Implications for Construction Financing

Following the GFC, a number of regulatory reforms were proposed to mitigate risk in the banking system. These proposals culminated in two major U.S. reforms: implementation of the Basel III capital rules and the Dodd–Frank Act (DFA) stress-testing framework. The U.S. Basel III final rule was approved by federal banking regulators in July 2013. The DFA stress-testing implementing rule was finalized in October 2012, and the first comprehensive public release of stress-testing methodology and results occurred in March 2013. Following Gandhi and Purnanandam (2021), we treat 2013 as the year of DFA stress-testing enactment for our empirical analysis, as by that point banks had sufficient clarity on the DFA stress-testing framework.

Basel III raised risk-based capital requirements and tightened leverage constraints on U.S. banks.[10] The DFA stress-testing framework requires the regulators to conduct supervisory stress tests of systemically important bank holding companies and designated non-bank financial companies. In addition, the DFA requires banks with total consolidated assets above $10 billion, that are not designated as systemically important, to conduct annual company-run stress tests and report the results to their primary federal regulator.

The primary objective of the DFA stress testing is to assess whether banks can maintain required capital levels under hypothetical adverse conditions while continuing to extend credit to households and businesses. These tests simulate losses by asset class and the resulting capital ratios under at least two scenarios (e.g., baseline and severely adverse) and require public disclosure

[10] See Walter (2019) for a detailed history of U.S. capital regulation and the specific Basel III requirements. Under U.S. Basel III rules, banks must maintain a Common Equity Tier 1 (CET1) ratio of at least 4.5% plus a 2.5% capital conservation buffer, implying an effective CET1 requirement of 7%. The minimum Tier 1 and total capital ratios are 6% and 8%; including the capital conservation buffer raises these to 8.5% and 10.5%, respectively. Banks are also subject to a minimum leverage ratio of 4%, and global systemically important banks (G-SIBs) are also subject to the supplementary leverage ratio (SLR).

of aggregate and firm-level results. A bank is deemed to have "passed" if its projected capital ratios remain above the minimum regulatory capital requirements under Basel III throughout the planning horizon. Following the test, a bank holding company–specific capital buffer is added to its capital requirements based on the results. Consequently, maintaining capital adequacy and passing the stress tests are crucial for stress-tested banks.

Importantly, under the DFA stress-testing framework, construction lending is treated as among the riskiest asset classes. For example, in the Federal Reserve's 2025 supervisory stress test, construction and land development loans are projected to incur an average loss rate of 10.5%—comparable to the riskiest category of residential mortgages (borrower credit scores below 680 and loan-to-value ratios above 80%), which averages roughly 11%. By comparison, unsecured, non-investment-grade corporate loans face a lower average projected loss rate of about 6.9%. Because projected losses in supervisory stress tests directly depress projected capital ratios and feed into bank-specific capital buffers, exposures to construction loans disproportionately erode regulatory capital under adverse scenarios. Consequently, stress-tested banks have strong incentives to curtail construction lending to reduce the risk of failing the supervisory tests.

3 Data Source and Sample Construction

An important contribution of this paper is the construction of a new dataset that provides transaction-level information on debt financing, banking relationships, and new home sales for U.S. home-builders, enabling firm-level analysis of new housing supply. This section provides an overview of the data source and sample construction, with additional details in Appendix A.

3.1 Identifying Homebuilders and Their Borrowing Using U.S. Deed Records

The primary data source for constructing the homebuilders dataset is the U.S. property deeds records, provided by ATTOM Data Solutions. This dataset offers two key types of information that are essential to our analysis. First, it contains comprehensive records of property transactions across the United States, including details on buyers and sellers, transaction dates, sale prices, and property addresses. Second, it includes loan information when the loans are collateralized by real estate. This includes the names of borrowers and lenders, loan origination dates, loan amounts, and the specific properties used as collateral. By merging the deeds records with tax assessment files, we additionally observe detailed property characteristics such as the construction year, number of bedrooms and bathrooms, and building square footage.

The main challenge in using this dataset for our research is identifying a consistent list of homebuilders, as well as their associated property transactions and borrowing. In particular, ATTOM deed records do not provide a uniform seller identifier, so compiling a seller-level panel must rely on our own name cleaning and entity-resolution algorithms. To address this challenge, we developed a two-step approach. First, we identify a potential list of new home transactions in which the transaction year matches the construction year. The sellers associated with these transactions are then labeled as potential homebuilders. We then standardize homebuilder names to correct typos, spelling variations, and account for large homebuilders that sell properties via subsidiaries by compiling lists of subsidiaries for major public and private firms using manual searches and OpenAI-based models. We remove financial institutions, government agencies, and nonprofit organizations from the homebuilder list. Second, we define new house transactions as those involving houses sold by identified homebuilders, subject to a set of conditions. These conditions include but are not limited to: the transaction must be arm's length, not within the homebuilder group, not

a foreclosure. In Appendix A, we provide a detailed description of the homebuilder list cleaning process and the specific restrictions for new housing.

Using our list of homebuilders, we extract all deed records in which a homebuilder pledges real property as collateral for a loan and refer to these property-secured borrowings as "construction loans" throughout the paper.[11] In the United States, a lender must make their interest public to be considered a secured party. For a loan secured by real estate, this occurs through the recording of a mortgage at the local county register (Hinzen, Severino, and Nieuwerburgh, 2026). Therefore, our method will enable us to capture all real-estate–collateralized loans borrowed by homebuilders.

Our use of public records to study the financing decisions of firms and the credit allocation decisions of financial institutions makes a novel contribution to the literature. Contemporaneously, Hinzen, Severino, and Nieuwerburgh (2026) use deed records to construct measures of commercial real estate lending by regional banks. Similarly, Gopal and Schnabl (2022) use Uniform Commercial Code filings to study the small-business lending decisions of banks and nonbank financial institutions. Our unique contribution is to construct a firm-level, transaction-level dataset that covers most firms in an important industry and to use it to study housing supply, an issue highly relevant to household welfare.

Using tax assessment records, we determine the status of the collateral as of the most recent assessment date prior to each loan's origination and classify collateral into two categories: vacant land and non-vacant land. Because most non-vacant collateral consists of single-family homes (with a small share of condominiums), we refer to all non-vacant collateral as residential property. We also standardize lender names and assign unique lender identifiers. For banks and credit unions, we map and normalize lender names to their official regulatory names as listed in the Call

[11] Some loans collateralized by completed residential properties may appear under different product categories in lenders' systems (e.g., commercial or residential real estate loans). For consistency, we classify all property-secured borrowings by homebuilders as construction loans.

Reports. Lenders that cannot be matched to the Call Reports are manually reviewed and classified as nonbank financial institutions. Notably, if a lender is a non-bank subsidiary of a bank holding company, we link it to the associated parent bank holding company.

3.2 Public homebuilders

Publicly listed homebuilders disclose financial statements. Between 2010 and 2025, there were approximately thirty-five homebuilders that were either public companies or subsidiaries of public companies.[12] For these firms, we obtain financial statement information from Compustat, which provides standardized accounting data for publicly traded entities. To provide a comprehensive view of homebuilders' debt financing activities, we collect corporate bond issuance data from the LSEG Mergent Fixed Income Securities Database (FISD) and syndicated loan information from DealScan and PitchBook. FISD provides detailed characteristics of corporate debt instruments, while DealScan and PitchBook offer comprehensive information on syndicated lending. We manually search across these datasets to identify all bond and loan records associated with homebuilders in our sample.

3.3 Bank Data

The financial statement data for U.S. banks are derived from the Call Reports maintained by the Federal Financial Institutions Examination Council (FFIEC). Beginning in 2008, U.S. banks report single-family construction loans as a separate Call Report item, labeled "1–4 family residential construction loans." By definition, this item primarily includes loans collateralized by vacant land or single-family properties under construction, and therefore excludes loans secured by completed

[12] Many of these firms were subsequently acquired by other companies, as the industry underwent rapid consolidation. As of 2024, there were 21 publicly listed homebuilders.

residential properties. Although its coverage differs from our deed-based dataset, we use this item to calculate several summary statistics and plot figures to help validating our dataset.

Branch locations are sourced from the FDIC Summary of Deposits, which collects information from all FDIC-insured institutions as of June 30 each year. Merger and acquisition records for U.S. banks are obtained from the FDIC. We define an acquirer as the acquiring institution identified in the FDIC data, and an acquired bank as an institution labeled "out" in the same dataset.

Finally, detailed stress-test results are hand-collected from the final report for each stress test. These reports detail the stressed ratios produced by the tests. Beginning with the 2020 report, they provide loss rates for different asset classes, which allows us to infer the perceived riskiness of bank asset classes within the stress-testing framework.

3.4 County Level Economic Data

Finally, we obtain county-level macroeconomic controls from the Bureau of Economic Analysis (BEA). Local construction data is measured using the U.S. Census Bureau's counts of building permits authorized for new privately owned single-family and multifamily housing units. To proxy the level of local housing prices, we use the Zillow Home Value Index (ZHVI), which we interpret as a measure of the typical (median) single-family home value at the county level.

3.5 Summary Statistics of the Homebuilder and Construction Loans Dataset

Table 1 presents summary statistics for our sample of homebuilders and their associated construction loans. The typical homebuilder is small, locally focused, and has persistent banking relationship: the median builder completes approximately 5.2 transactions per year with total annual sales of $1.0 million, operates in 3 counties, and borrows from 2 banks over the sample period. We note that the reported sales amounts likely understate true values, as transaction prices are recorded

as zero in non-disclosure states.[13] The distribution is heavily right-skewed: the mean number of transactions (26.5) and mean sales amount ($9.7 million) substantially exceed their respective medians, reflecting the presence of a small number of large national builders. Homebuilders in our sample borrow in approximately 40% of the years they are active and sell homes in approximately 60% of years, implying that the data contain a substantial mass of zero observations—a feature that motivates our use of PPML estimation in DiD analyses.

The characteristics of the construction loans reinforce the view that this is a market in which bank regulation can have real effects. The median loan is $200,000 with a median maturity of one year, consistent with the project-specific, short-term nature of construction financing. The deeds data provide limited coverage of interest rates. From this restricted sample, we find a median interest rate spread over the risk-free rate of 5.8 percentage points, which is close to the survey evidence in NAHB (2025). However, this spread does not include additional points charged by banks at origination; incorporating these fees raises the effective interest spread to approximately 8 percentage points (NAHB, 2025), consistent with the high-risk nature of construction lending from the perspective of bank regulators. Finally, borrowing distance, measured as the distance between the collateral property and the nearest branch of the lending bank, has a median of approximately 9 miles, underscoring the local nature of construction lending relationships.

4 The Difference-in-Differences Analysis

In this section, we present empirical evidence that stress-tested banks reduced construction lending following the 2013 regulatory changes, relative to other banks. We first show that stress-tested banks' market share in construction lending declined after 2013, at both the aggregate and local

[13] Non-disclosure states do not require the recording of sale prices in deed records. This affects the level of our sales amount variable but does not bias our DiD estimates, which rely on number of houses sold.

levels. We then implement a difference-in-differences (DiD) design that compares homebuilders that borrowed from stress-tested banks prior to 2013 with those that borrowed only from other banks, before and after 2013. We find that both credit availability and home sales declined for homebuilders with prior relationships with stress-tested banks, relative to homebuilders that borrowed only from non-stress-tested banks.

We provide three additional pieces of evidence consistent with our hypothesis. First, using the within-firm estimation approach of Khwaja and Mian (2008), we show that banking relationships with stress-tested banks are more likely to be terminated than relationships with other banks after 2013 for the same homebuilder. Second, borrowing distance increases for homebuilders with prior relationships with stress-tested banks, relative to homebuilders that borrowed only from non-stress-tested banks, consistent with disrupted lending relationships that compel affected homebuilders to search more broadly for financing. Third, the share of construction loans secured by vacant land declines more for stress-tested banks after 2013 than for other banks.

4.1 Bank Regulatory Reforms and Bank Lending to Homebuilders

We begin by documenting how stress-tested banks' construction lending behavior evolved before and after the 2013 reforms. Because construction lending is treated as highly risky in stress tests and Basel III requires stress-tested banks to improve their capital ratios, these banks have strong incentives to curtail construction lending. We document two empirical facts consistent with this prediction. First, the market share of stress-tested banks in construction lending declined sharply after the reforms, both at the aggregate and local levels. Second, stress-tested banks withdrew entirely from some local markets, reducing the number of counties in which they originated construction loans.

The Market Share of Stress-Tested Banks. We first examine how the market share of stress-tested banks in construction-loan originations evolved over time. As shown in Figure 2, their share rose during and immediately after the GFC, mirroring the concurrent increase in these banks' asset share. Following the GFC, the share of bank assets held by stress-tested banks stabilized at roughly 70–75 percent, and from 2010 to 2012 their share of bank-originated construction loans likewise hovered around 42 percent. Beginning in 2013—coinciding with the introduction of the DFA stress tests—this share declined sharply, falling from about 40 percent in 2012 to 18 percent in 2017. A similar pattern emerges at the local level: the average market share of stress-tested banks in bank-originated construction lending across the 500 most populous counties dropped from 31 percent in 2012 to 15 percent in 2017 (Figure 2).

Importantly, we find that other sources of financing did not offset this drop in lending from stress-tested banks. As shown in Figure 7, except for large homebuilders that shifted significantly to the corporate bond market, nonbank sources of construction financing increased only modestly for small and medium-sized homebuilders, by approximately five percentage points during 2013–2017 relative to 2010–2012. Similarly, Figure 8 plots banks' market share in the construction-loan market using two sources: deeds records and the Flow of Funds. Both indicate that there was no substantial change in banks' overall construction-loan share before and after 2013. The two sources differ somewhat because the Flow of Funds captures only one-to-four-family construction loans, whereas our deeds records also include loans used for land acquisition and development. Nonbank lenders may find it easier to expand into these latter categories because the larger loan sizes help absorb nonbanks' higher per-loan monitoring costs, i.e., costs that arise in part from nonbanks' lack of local knowledge and established borrower relationships.

Geographic Footprint of Stress-Tested Banks. We next examine how the geographic footprint of stress-tested banks in construction lending evolved over time. Figure 9 plots the total number of counties served by all stress-tested banks, as well as the number of counties in which each of the five largest stress-tested banks originated construction loans. As the figure shows, both measures declined steadily after the 2013 reforms. The total number of counties in which stress-tested banks originated construction loans fell from approximately 1,100 in 2012 to roughly 550 by 2019, and several major institutions, most notably Citigroup and JPMorgan Chase, sharply reduced their geographic presence.[14] These patterns suggest that the 2013 regulatory reforms did not merely reduce the volume of construction lending by stress-tested banks but also led them to withdraw entirely from some local markets, further limiting homebuilders' access to credit in affected counties.

Taken together, these patterns are consistent with the view that Basel III and the DFA stress tests induced stress-tested banks to scale back construction lending. However, as descriptive evidence, they do not by themselves establish causality, as the observed changes may reflect shifts in homebuilder demand or broader secular trends coinciding with the introduction of stress testing. In the next section, we implement a DiD design to isolate the credit supply effect from these confounding factors.

[14] In this calculation, we count the total number of bank-county pairs in each year, where a bank-county pair is defined as a stress-tested bank that originated at least one construction loan in a given county during that year. For example, if two stress-tested banks each originated construction loans in the same county, that county contributes two bank-county pairs.

4.2 The Effects of Bank Regulation Reforms on Homebuilder Financing and Housing Supply

To provide evidence that stress-tested banks reduced their construction lending supply after 2013 relative to other banks, we conduct a DiD analysis comparing homebuilders that previously borrowed from stress-tested banks to those that borrowed only from other banks, before and after 2013. The underlying assumption is that the Basel III and DFA stress tests had a larger initial effect on banks subject to stress testing, and that homebuilders with prior banking relationships with these banks were therefore more exposed to the resulting credit supply contraction. In our benchmark specification, we estimate the following regression:

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta \text{Controls}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t} \tag{1}$$

Here, $y_{i,t}$ denotes a loan outcome variable for homebuilder i in year t. Because homebuilders do not necessarily borrow or sell houses every year, the data contain a substantial mass of zero observations. To account for this, our benchmark specification estimates a Poisson pseudo-maximum-likelihood (PPML) model, a state-of-the-art approach for non-negative outcomes with many zeros (Cohn, Liu, and Wardlaw, 2022). We then separately estimate the extensive margin (borrowing indicators) and the intensive margin (loan amounts conditional on borrowing). We include homebuilder fixed effects to account for the fact that exposed homebuilders tend to be slightly larger, as shown in Table 2.

For the PPML estimation, $y_{i,t}$ is the loan amount obtained by homebuilder i in year t. For the extensive margin, $y_{i,t}$ is a binary indicator $\phi^l_{i,t}$ equal to one if homebuilder i obtained any construction financing in year t. For the intensive margin, $y_{i,t}$ is the logarithm of total loan volume for homebuilder i in year t. The variable $Exposed_i$ is a binary indicator equal to one if homebuilder

i borrowed from at least one stress-tested bank during 2010–2012.[15] Firm fixed effects are denoted by α_i and year fixed effects by η_t. Firm fixed effects help account for the fact that borrowers of stress-tested banks tend to be larger, as shown in Table 2. We also report results from standard DiD regressions without fixed effects as a benchmark. Standard errors are clustered at the homebuilder level. If the Basel III regulations and DFA stress tests led stress-tested banks to reduce their lending to homebuilders, we expect $\gamma < 0$.

The benchmark results indicate that stress-tested banks reduced lending to homebuilders following the reforms. As shown in Table 4, we find that homebuilders that borrowed from stress-tested banks experienced a 31.5% decline in borrowing after the reforms, relative to contemporaneous changes among homebuilders that borrowed only from non-stress-tested banks.[16] This relative decline reflects an 8.2% increase in borrowing among homebuilders that borrowed only from non-stress-tested banks and an approximately 24% drop in borrowing among homebuilders that borrowed from stress-tested banks. At the extensive margin, homebuilders that borrowed from stress-tested banks are 6.4 percentage points less likely to borrow in a given year after the reforms, relative to contemporaneous changes among homebuilders that borrowed only from non-stress-tested banks. Conditional on borrowing, the loan amount borrowed by homebuilders that borrowed from stress-tested banks dropped by 30.0% after 2013, relative to homebuilders that borrowed only from non-stress-tested banks.

Because homebuilders rely heavily on external finance to fund land acquisition and construction, negative credit supply shocks can materially disrupt their operations and constrain housing supply. To examine this channel, we re-estimate regression (1), replacing $y_{i,t}$ with homebuilder

[15] The extensive margin captures changes in the probability of obtaining loans for homebuilders that borrowed from stress-tested banks, relative to homebuilders that borrowed only from non-stress-tested banks, following the proposal of Basel III and the implementation of DFA stress testing. The intensive margin measures, conditional on obtaining loans, changes in the amount of credit extended to homebuilders that borrowed from stress-tested banks compared to homebuilders that borrowed only from non-stress-tested banks after the regulatory changes.

[16] Percentage changes are computed as $\%\Delta = 100 \times (e^{\gamma} - 1)$ to account for the semi-elasticity interpretation.

i's housing sales outcome in year t: (i) the number of home sales (PPML model), (ii) a binary indicator $\phi_{i,t}^{h}$ equal to one if homebuilder i sells any new homes in year t (extensive margin), or (iii) the log number of home sales (intensive margin).

The results indicate that the credit supply contraction induced by the regulatory reforms significantly disrupted homebuilders' operations and constrained housing supply. As shown in Table 5, the PPML estimates indicate that home sales by homebuilders that borrowed from stress-tested banks decreased by 11.0% after the regulatory reforms, relative to homebuilders that borrowed only from non-stress-tested banks. Specifically, sales by homebuilders that borrowed only from non-stress-tested banks increased by about 24.7%, while sales by homebuilders that borrowed from stress-tested banks increased by only about 14.7%. At the extensive margin, the likelihood of homebuilders that borrowed from stress-tested banks selling in a given year dropped by 4.5 percentage points after the reforms, relative to homebuilders that borrowed only from non-stress-tested banks. Conditional on selling, the number of houses sold by homebuilders that borrowed from stress-tested banks dropped by 14.6% after the reforms, relative to homebuilders that borrowed only from non-stress-tested banks.

The dynamic pattern of these effects supports a causal interpretation. Event-study estimates from the PPML specification for loan amounts (Figure 12) and home sales (Figure 13) show that the pre-reform coefficients for 2010 and 2011 are close to zero and statistically insignificant in both cases, consistent with the parallel trends assumption. Beginning in 2013, the loan amount coefficients turn sharply negative and grow in magnitude, reaching -0.42 by 2016 before stabilizing at -0.41 in 2017. The home sales coefficients follow a similar pattern, turning negative in 2013 and declining steadily to -0.153 by 2017. The progressive widening of the gap between exposed and unexposed homebuilders in both credit availability and house sales is consistent with a persistent credit supply contraction rather than a transitory disruption, and the absence of differential

pre-trends supports the view that the divergence was driven by the 2013 regulatory reforms rather than by pre-existing differences between the two groups of homebuilders.

Discussion of spillover effects. A natural concern with our DiD design is that homebuilders that borrowed from stress-tested banks and those that borrowed only from non-stress-tested banks may interact within the same local markets. Such interaction can generate spillovers that contaminate the comparison and bias our estimated treatment effects; in other words, the stable unit treatment value assumption (SUTVA) may be violated (Berg, Reisinger, and Streitz, 2021).

We argue that the potential spillover effects do not undermine our identification. The identification of the main channel relies on the fact that homebuilders that borrowed from stress-tested banks are directly exposed to the credit supply shock, whereas other homebuilders are only indirectly affected. The most plausible spillover channel operates through the reallocation of credit demand: homebuilders that previously borrowed from stress-tested banks and subsequently switch to smaller banks may tighten credit conditions for other homebuilders who already borrow from those banks. Because this spillover would reduce the gap in outcomes between the two groups, it biases our estimates toward zero. The fact that we nonetheless find sizable and statistically significant DiD estimates reinforces the conclusion that the credit supply shock materially constrained homebuilders' credit availability and new housing supply.

4.3 Additional Evidence

Within-Firm Estimation. To strengthen the identification, we follow the within-firm estimation approach of Khwaja and Mian (2008), which compares changes in lending across different banking relationships for the same firm, thereby isolating credit supply effects from demand-side

influences. We estimate the following regressions:

$$\Delta Relationship_{i,b} = \beta_1 StressTested_b + \delta_i + \varepsilon_{i,b} \tag{2}$$

$$\Delta \ln Amount_{i,b} = \beta_1 StressTested_b + \delta_i + \varepsilon_{i,b} \tag{3}$$

Following Khwaja and Mian (2008), we collapse the time dimension into two periods—pre-reform (2010–2012) and post-reform (2013–2015)—to obtain more conservative standard errors. We refer to

$$\Delta Relationship_{i,b} = Relationship_{i,b,post} - Relationship_{i,b,pre} \tag{4}$$

as the extensive margin and

$$\Delta \ln Amount_{i,b} = \ln Amount_{i,b,post} - \ln Amount_{i,b,pre} \tag{5}$$

as the intensive margin. Here, $Relationship_{i,b,pre}$ ($Relationship_{i,b,post}$) is a binary indicator equal to one if bank b lends to firm i during the pre-reform (post-reform) period. Similarly, $Amount_{i,b,pre}$ ($Amount_{i,b,post}$) denotes the amount lent by bank b to firm i during the pre-reform (post-reform) period. $StressTested_b$ is a binary indicator equal to one if bank b is subject to DFA stress testing, and zero otherwise. The sample is restricted to builders with at least two pre-existing banking relationships, and the estimation includes only pre-existing bank-builder pairs. Since the credit supply shock occurs at the bank level, changes in lending from the same bank may be correlated across borrowers. We therefore cluster standard errors at the bank level.

The key component of both regressions is the firm fixed effect δ_i, which absorbs all firm-specific credit demand shocks occurring around the regulatory reforms. This allows us to test whether the same homebuilder, borrowing from multiple banks, experiences a differentially larger

decline in lending from stress-tested banks. Because the firm fixed effect holds constant the borrower's demand for credit, any differential change across a homebuilder's lending relationships can be attributed to differences in credit supply across lenders. Specifically, the extensive-margin regression (Equation 2) compares the probability that a lending relationship survives for relationships involving stress-tested banks versus non-stress-tested banks, holding constant firm-level demand. The intensive-margin regression (Equation 3) focuses on relationships that persist in both the pre- and post-reform periods and compares changes in lending volumes across stress-tested and non-stress-tested banks within the same firm.

The regulatory reforms significantly disrupted lending relationships between homebuilders and stress-tested banks. At the extensive margin, banking relationships with stress-tested banks are 12.4 percentage points more likely to be terminated than the same homebuilders' other banking relationships with non-stress-tested banks (Table 6). The OLS estimate without builder fixed effects is similar in magnitude, suggesting that compositional differences between homebuilders that borrow from stress-tested versus non-stress-tested banks play a limited role. The consistency between the two estimates reinforces the interpretation that the relationship disruption reflects a contraction in bank credit supply rather than differences in borrower characteristics.

At the intensive margin, conditional on the lending relationship surviving in both periods, the coefficient is statistically insignificant. This indicates that the primary channel through which stress-tested banks adjusted their construction lending was outright termination of lending relationships rather than gradual reduction of credit within surviving relationships. This is consistent with the geographic patterns documented in Figure 9, where we show that stress-tested banks reduced their construction lending in part by withdrawing entirely from local markets. The sharp extensive-margin response, combined with the null intensive-margin result, paints a coherent picture of how stress-tested banks implemented their withdrawal from construction lending following

the regulatory reforms

Borrowing Distance. When stress-tested banks retrench from construction lending, homebuilders that previously relied on them must switch lenders to either another existing lenders or new lenders to obtain financing. Because construction loans are specialized and local markets often have few lenders with the requisite underwriting expertise and balance-sheet capacity, the replacement lender is more likely to be located outside the builder's home market. We therefore expect the borrowing distance of homebuilders that borrowed from stress-tested banks to increase following the reforms. To formally test this hypothesis, we replace $y_{i,t}$ in regression (1) with the logarithm of homebuilder i's weighted-average borrowing distance in year t, where the borrowing distance of a loan is defined as the distance between the collateral property and the closest branch of the lending bank.

The empirical evidence supports this prediction. As shown in Table 7, the borrowing distance for homebuilders that borrowed from stress-tested banks increased by approximately 29.5% following the regulatory reforms, relative to homebuilders that borrowed only from non-stress-tested banks.[17] The result is robust to controlling for loan size (Column 3). To examine the dynamics of this effect, Figure 14 presents event-study estimates from a year-by-year interaction specification. The pre-reform coefficients for 2010 and 2011 are close to zero and statistically insignificant, consistent with the parallel trends assumption. Beginning in 2013, the coefficients become positive and increase steadily, reaching approximately 0.31 by 2017. This pattern indicates that the increase in borrowing distance emerged precisely with the introduction of the stress tests and persisted throughout the post-reform period, consistent with a permanent disruption of local lending relationships rather than a transitory adjustment.

[17] Computed as $e^{0.258} - 1 \approx 0.294$, based on the specification with firm and year fixed effects (Column 2).

Collateral Type. Loans collateralized by vacant land are considered highly risky under stress-testing frameworks. However, if borrowers can pledge other properties—such as existing structures or improved land—as collateral, the loan may be classified under a less risky category for both the bank and the regulatory framework. Consequently, banks subject to stress testing have an incentive to require borrowers to avoid using vacant land as collateral, effectively tightening the terms on which construction financing is available. We examine this hypothesis using the following regression:

$$\phi^{c}_{j,b,t} = \gamma\left(Post_t \times TreatedLoan_{j,b}\right) + \beta_c \text{Controls}_{j,b,t} + \alpha_b + \zeta_t + \varepsilon_{j,b,t} \tag{6}$$

Here, $\phi^{c}_{j,b,t}$ is a binary indicator equal to one if the collateral for loan j, issued by lender b in year t, is vacant land. The variable $TreatedLoan_{j,b}$ is a binary indicator equal to one if the issuing bank b of loan j is a stress-tested bank. Bank holding company fixed effects are denoted by α_b and year fixed effects by ζ_t. Bank holding company fixed effects control for time-invariant characteristics of each lender, such as business model and risk appetite. Year fixed effects absorb aggregate economic conditions that may affect banks' collateral requirements. Controls include the logarithm of loan amount and the logarithm of borrowing distance where indicated.

Our empirical results confirm that stress-tested banks shifted away from accepting vacant land as collateral after the regulatory reforms. As shown in Table 8, the difference-in-differences coefficient is negative and statistically significant across all specifications. In the specification with bank holding company and year fixed effects (Column 2), loans issued by stress-tested banks are 11.4 percentage points less likely to be collateralized by vacant land after 2013 compared to loans from other lenders. The result is robust to controlling for loan size and borrowing distance (Column 3). The magnitude is economically meaningful: relative to the baseline probability of using vacant

land as collateral (approximately 54%), the estimates imply a reduction of about 21%.

To examine the dynamics of this effect, Figure 15 presents event-study estimates from a year-by-year interaction specification. The pre-reform coefficients for 2010 and 2011 are positive, suggesting that stress-tested banks were, if anything, more likely to accept vacant land as collateral relative to other lenders before the reforms. This pattern reverses sharply beginning in 2013: the coefficient turns negative and is close to zero in 2013 (-0.003), becomes statistically significant by 2014 (-0.058), and grows steadily in magnitude through 2017 (-0.158). The progressive tightening of collateral standards is consistent with the phased implementation of Basel III capital requirements and the cumulative effect of successive rounds of stress testing. The stark reversal from positive pre-reform coefficients to increasingly negative post-reform coefficients provides compelling evidence that the shift in collateral composition reflects a regulatory-induced change in bank lending standards rather than a pre-existing trend.

5 County-Level Evidence: Credit Supply, House Prices, and Housing Supply Response

Our findings thus far indicate that stress-tested banks reduced construction lending following the 2013 regulatory reforms and that this retrenchment materially reduced credit availability to homebuilders with prior lending relationships with these banks. Because of the local, relationship-based nature of construction lending, and because most homebuilders are relatively small and rely heavily on external financing, the drop in credit availability is likely to have constrained new housing supply. Given that stress-tested banks collectively held more than 70% of total banking assets and accounted for more than 40% of bank-originated construction lending during 2010–2012, we expect that this credit supply shock reduced aggregate housing supply and, through the resulting

supply constraint, contributed to house price appreciation.

To test this hypothesis, we exploit cross-sectional variation in counties' pre-GFC exposure to stress-tested banks. Our identification assumption is that counties in which stress-tested banks accounted for a larger share of construction loan originations before the GFC experienced a more pronounced contraction in credit supply after the reforms. To assess this assumption, we estimate the following difference-in-differences specification:

$$y_{c,t} = \alpha_c + \delta_t + \beta \cdot \text{StressShare}_c \times \text{Post}_t + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t}, \tag{7}$$

where StressShare_c denotes the pre-GFC (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by the standard deviation of the stress-tested banks' market share. Post_t is an indicator equal to one for years 2013–2017 and zero for years 2010–2012. County fixed effects, α_c, absorb all time-invariant county characteristics, including geography, climate, and the baseline regulatory environment. Year fixed effects, δ_t, absorb aggregate macroeconomic conditions common across counties. The time-varying controls, $\mathbf{X}_{c,t}$, include income growth, GDP growth, and population growth. In our most demanding specification, we additionally interact the Wharton Residential Land Use Regulatory Index (WRLURI) with the post-period indicator to account for the possibility that counties with more restrictive land-use regulation experienced differential trends in construction lending and house prices. Standard errors are clustered at the county level. The sample is restricted to counties with an average pre-reform population exceeding 50,000.

Loan Originations and Stress-Test Loan Share. Counties with greater pre-reform exposure to stress-tested banks experienced significantly larger declines in construction-loan originations after 2013. A one-standard-deviation increase in pre-reform stress-test exposure is associated with a

13.8% decline in loan originations in the baseline fixed-effect specification. The estimate becomes larger in magnitude when we further control for land use regulation, suggesting that omitting regulatory stringency, if anything, attenuates the estimated effect. The decline in overall loan originations is driven specifically by the withdrawal of stress-tested banks: replacing the dependent variable with the contemporaneous county-year share of construction loans originated by stress-tested banks, we find that a one-standard-deviation increase in pre-reform exposure is associated with a 5.6 percentage point decline in the stress-test loan share. This rules out the alternative interpretation that the results simply reflect broader county-level contractions unrelated to the regulatory shock.

The dynamic pattern of these effects is consistent with a causal interpretation. Event-study estimates for both log loan originations (Figure 16) and the stress-test loan share (Figure 17) show pre-reform coefficients that are close to zero and statistically insignificant, supporting the parallel trends assumption. The coefficients turn sharply negative beginning in 2013 and remain persistently so through 2017, confirming that the decline in construction lending emerged precisely with the introduction of the stress tests rather than reflecting pre-existing differential trends.

House Prices. If the regulatory-induced reduction in construction lending constrains new housing supply while housing demand continues to recover, the resulting supply-demand imbalance should push house prices upward in more exposed counties. The data strongly support this prediction. A one-standard-deviation increase in pre-reform stress-test exposure is associated with 2.2% higher house prices after 2013 (Table 10). The estimate increases slightly to 2.9% when controlling for land use regulation.

Importantly, the event-study estimates (Figure 18) show that the regulatory reforms changed the trajectory of house prices. Prior to the reforms, the estimated coefficients were close to zero,

consistent with the parallel trends assumption. Beginning in 2013, the coefficients increase monotonically from 0.010 to 0.037 by 2017, indicating a progressive divergence in house prices between more and less exposed counties. This steadily widening gap is consistent with a supply constraint that becomes increasingly binding as housing demand continues to recover.

The magnitude of this effect is economically significant. By 2017, a one-standard-deviation increase in pre-reform stress-test exposure is associated with 3.7% additional house price appreciation. Since the mean county in our sample has a stress-test exposure of approximately 1.06 standard deviations, the implied effect at the mean is roughly 3.9% (0.037×1.06). Given that the average house price appreciation across counties in our sample from 2013 to 2017 was 19.9%, the regulatory-induced credit contraction accounts for approximately one-fifth of the total price increase in the typical county. This estimate suggests that the post-GFC tightening of bank regulation was a quantitatively important driver of the house price dynamics observed during this period.

Housing Supply Response. Our county-level DiD estimates demonstrate that counties with greater pre-GFC exposure to stress-tested banks experienced both larger declines in construction lending and faster house price appreciation. A natural question is whether this contraction in credit supply reduced the responsiveness of new housing construction to local price changes. In a well-functioning housing market, rising prices signal unmet demand and incentivize homebuilders to increase construction. However, if constraints on construction lending prevent homebuilders from obtaining the financing necessary to start new projects, this supply response is impaired: prices rise but construction does not follow. To test this prediction, we estimate the following triple-difference

specification:

$$\begin{aligned}\ln(\text{SFH})_{c,t} = \alpha_c + \delta_t + \beta_0 \cdot \ln(\text{Price}_{c,t}) + \beta_1 \cdot \text{StressShare}_c \times \text{Post}_t \\ + \beta_2 \cdot \ln(\text{Price})_{c,t} \times \text{Post}_t + \beta_3 \cdot \ln(\text{Price})_{c,t} \times \text{StressShare}_c \\ + \gamma \cdot \ln(\text{Price})_{c,t} \times \text{StressShare}_c \times \text{Post}_t + \mathbf{X}'_{c,t}\psi + \varepsilon_{c,t},\end{aligned} \tag{8}$$

where the coefficient of interest is γ, which captures the differential responsiveness of new single-family home supply to local house prices in counties with greater pre-GFC exposure to stress-tested banks. A negative γ indicates that more exposed counties exhibit a weaker housing supply response to rising prices after the reforms.

The credit contraction induced by the regulatory reforms significantly weakened the housing supply response in more exposed counties. As shown in Table 11, a one-standard-deviation increase in pre-reform exposure to stress-tested banks is associated with a 0.083 decline in the responsiveness of new home sales to local house prices. This estimate is robust to controlling for land-use regulation. These findings indicate that the contraction in credit supply not only reduced construction lending and contributed to house price appreciation, but also impaired the housing market's ability to translate price signals into new construction-a hallmark of a binding supply constraint. The event-study estimates (Figure 19) corroborate this interpretation: the pre-reform coefficients are close to zero and statistically insignificant, whereas the post-reform coefficients become negative and grow in magnitude through 2016, consistent with a progressively tightening constraint on homebuilders' ability to respond to rising demand.

Taken together, the county-level evidence reveals a coherent causal chain from the regulatory shock to real outcomes: the 2013 reforms reduced construction lending in counties with greater pre-reform exposure to stress-tested banks, thereby constraining new housing supply, contributing to faster house price appreciation, and weakening the responsiveness of housing construction to

local price signals. These findings suggest that the post-GFC tightening of bank regulation had material, and likely unintended, consequences for the U.S. housing market.

6 Conclusion

During the post-GFC period, U.S. housing supply, particularly in the single-family segment, remained at historically low levels, a pattern widely linked to the ongoing housing affordability crisis. This paper provides causal evidence that post-GFC banking reforms, i.e., the Basel III framework and DFA stress tests, reduced the supply of construction credit from the banking sector and, in turn, contributed to the decline in U.S. housing supply.

Leveraging a novel dataset covering the financing and sales activities of nearly 18,000 homebuilders, we show that stress-tested banks retrenched from construction lending following the 2013 reforms. Our homebuilder-level DiD analysis and within-firm estimates indicate that this decline reflects a credit supply shock and that it reduced credit availability for homebuilders that had borrowed from stress-tested banks, thereby constraining their ability to supply new homes. At the county level, we find that counties with greater pre-GFC exposure to stress-tested banks experienced larger declines in construction loan originations, faster house price appreciation, and a weaker responsiveness of new housing construction to local price changes after the reforms. By 2017, the regulation-induced contraction in credit accounts for roughly one-fifth of the average house price increase in the typical county.

These findings carry important policy implications. They offer a bank-regulation-based explanation for the persistent post-GFC shortfall in U.S. housing supply, complementing existing accounts that emphasize land-use regulation, labor shortages, and rising construction costs. More broadly, they highlight a central trade-off in prudential policy design. While capital regulation

and stress testing strengthen bank resilience and promote financial stability, they can generate significant real costs in credit markets that rely on relationship lending and intensive monitoring. Construction lending—characterized by project-specific risk assessment, local market knowledge, and ongoing borrower oversight—is precisely the type of lending most vulnerable to increases in capital requirements. Policy designs that preserve prudential objectives while recognizing these informational and monitoring features of construction lending may help mitigate unintended contractions in housing supply.

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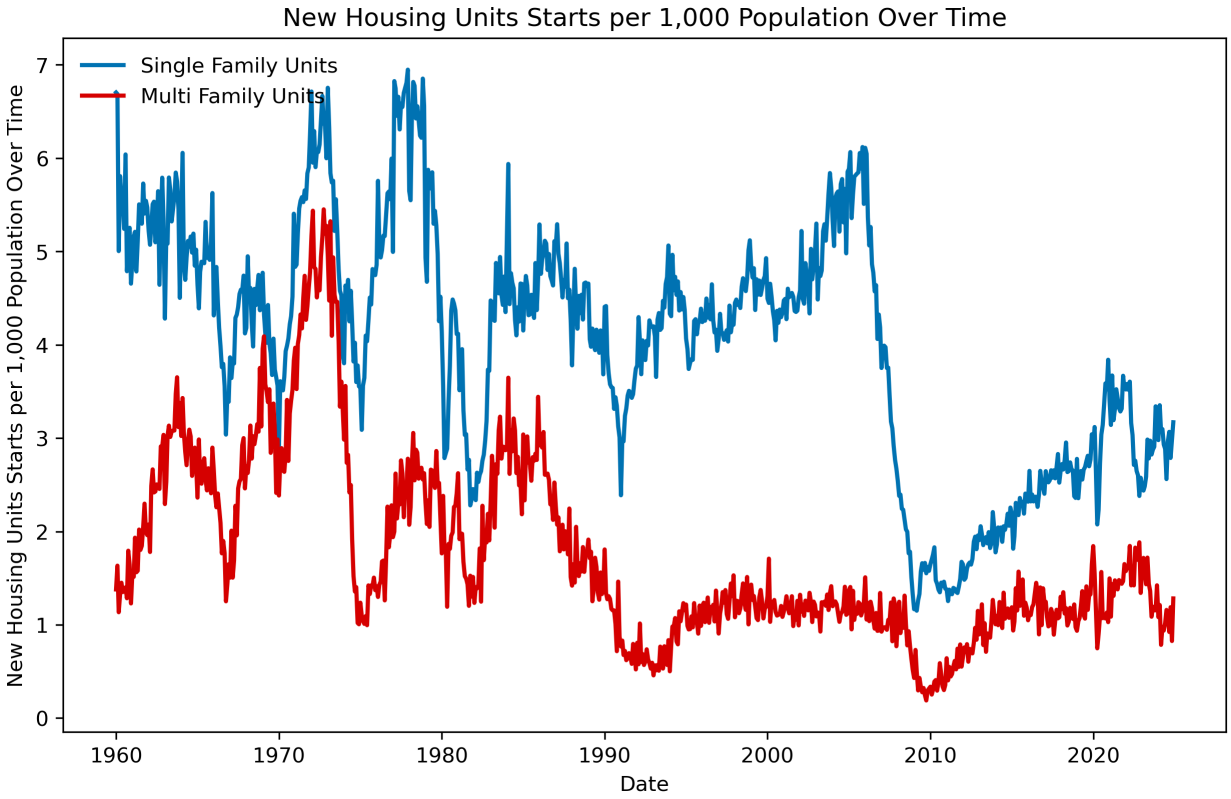


Figure 1: New Housing Units Starts per 1,000 Population Over Time. This figure plots the number of new single-family housing starts per 1,000 residents and new multi-family housing starts per 1,000 residents over time. Data on housing starts and population are obtained from the U.S. Census Bureau and accessed through the Federal Reserve Economic Data (FRED) platform, from which we construct the measures shown in the figure. Data on housing starts are seasonally adjusted and annualized by FRED.

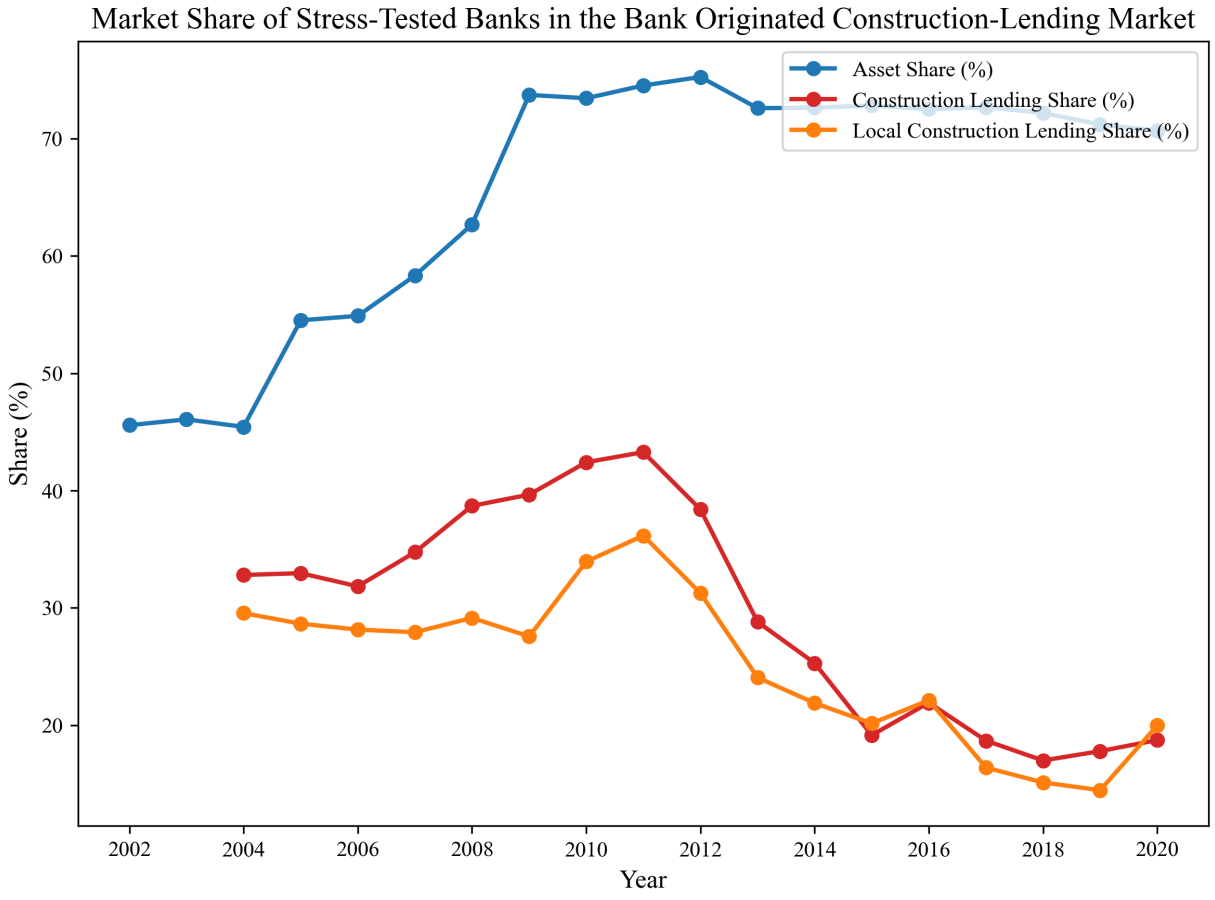


Figure 2: Market Shares of the Stress-Tested Banks in the Bank Originated Construction-Lending Market. This figure reports the share of total banking assets held by stress-tested banks and their share of bank-originated construction lending over 2004-2020. Asset shares are computed from Call Reports; construction-lending shares are constructed from deed records, excluding loans originated by nonbank lenders. The local construction-lending share is calculated as the average market share of bank-originated construction lending by stress-tested banks across the 500 most populous counties.



Figure 3: Market Shares of the Stress-Tested Banks in the Construction-Lending Market. This figure reports stress-tested banks' share of construction lending over 2004–2020. Construction-lending shares are computed from deed records and include all lenders (banks and nonbanks). The local construction-lending share is defined as the average market share of construction lending by stress-tested banks across the 500 most populous counties.

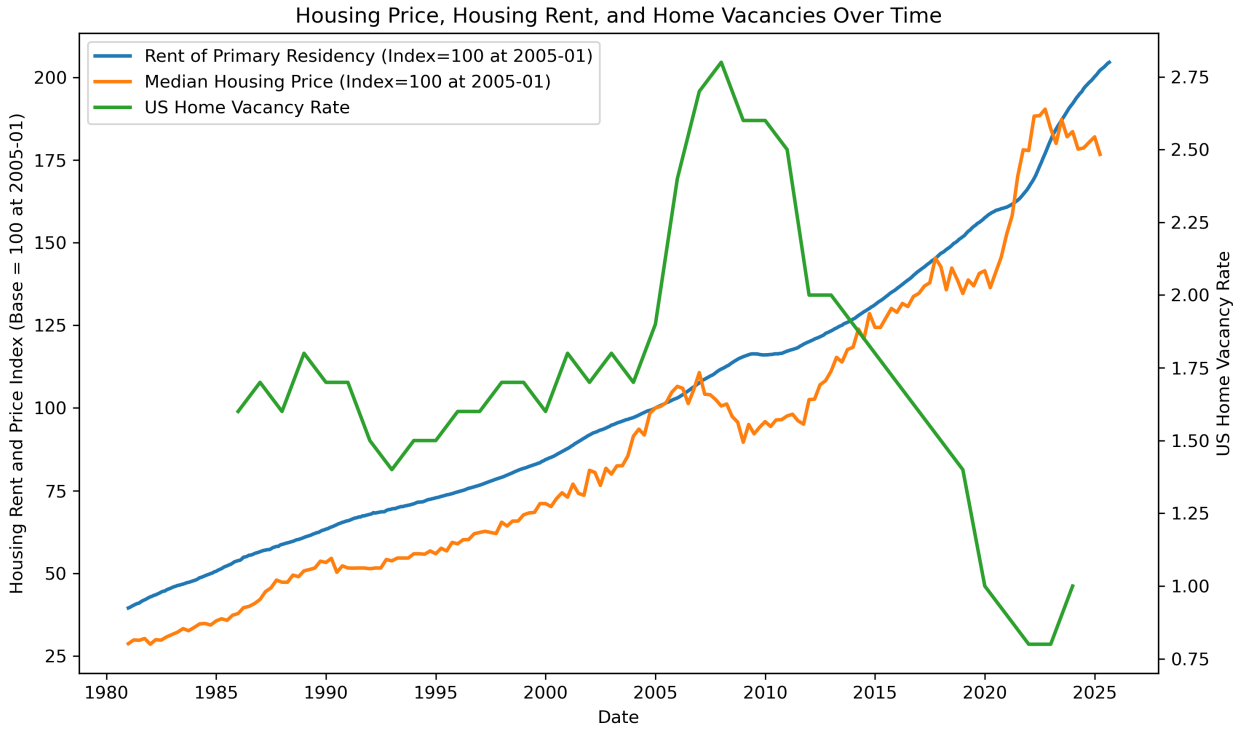


Figure 4: US Housing Price, Rent, and Home Vacancies over Time. This figure presents the Median Sales Price of Houses Sold, the Consumer Price Index for All Urban Consumers: Rent of Primary Residence (U.S. City Average), and the Home Vacancy Rate over time. For housing prices and rents, values are normalized using their January 2005 level as the base (index = 100). All underlying data are sourced from the U.S. Census Bureau and obtained through the Federal Reserve Economic Data (FRED) platform.

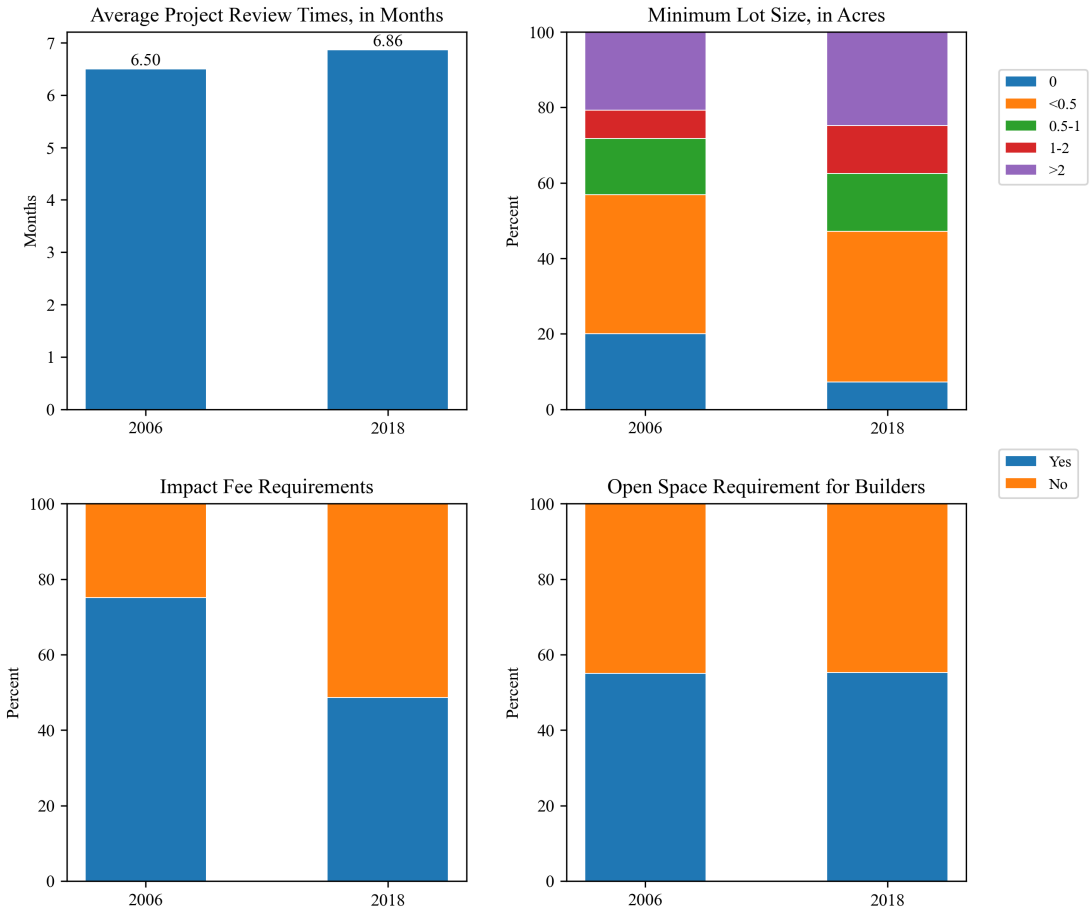


Figure 5: Changes in Land Regulatory Restrictions, 2006-2018. This figure measures changes in the local land-use regulatory environment by comparing responses to identical survey questions from the Wharton Residential Land Use Regulation Index (WRLURI) conducted in 2006 by Gyourko, Saiz, and Summers (2008) and in 2018 by Gyourko, Hartley, and Krimmel (2021). The top-left panel reports changes in average project review times, measured in months. Review times correspond to the typical duration required for residential development projects that require either rezoning approval or subdivision approval. The top-right panel reports the distribution of minimum lot size requirements, measured in acres. The bottom-left panel reports whether local jurisdictions require the payment of impact fees by residential developers. The bottom-right panel reports whether jurisdictions impose open space requirements on residential development. Such requirements are often motivated by environmental or amenity considerations, including the preservation of green space and the mitigation of environmental externalities.

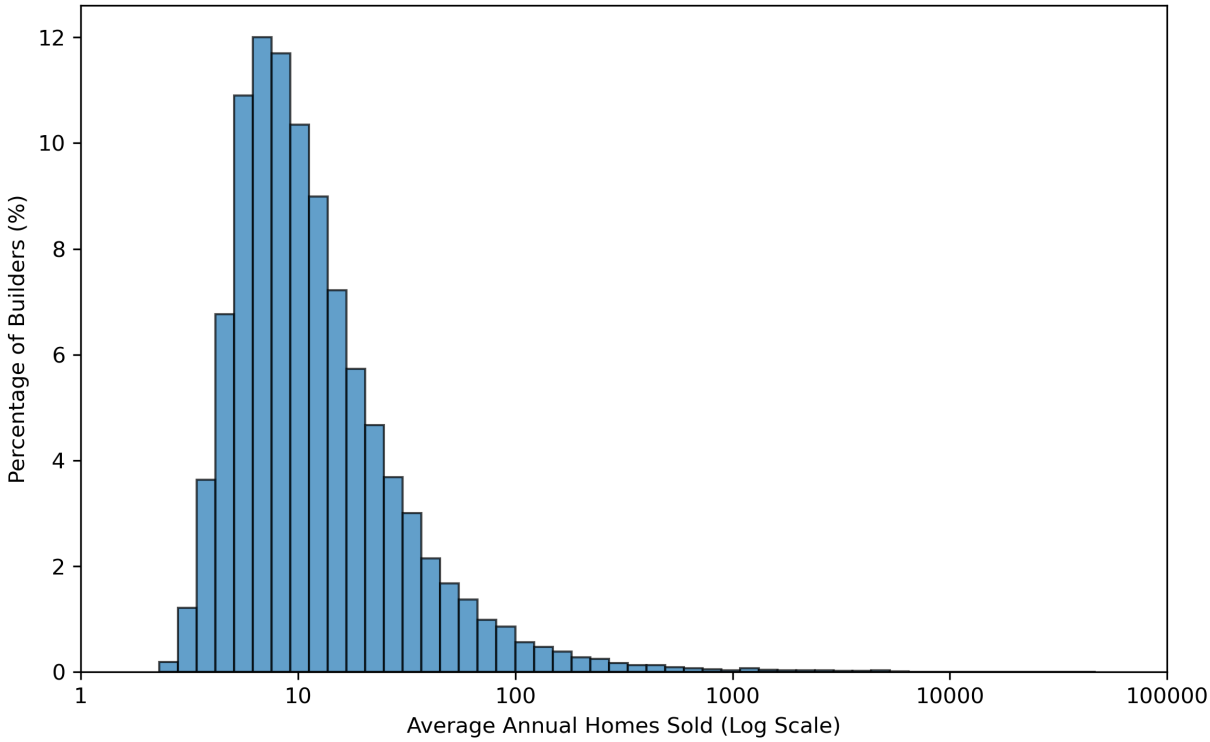


Figure 6: Distribution of Homebuilders by Average Annual Sales. This figure shows the distribution of homebuilders' average annual sales over the sample period 2011–2023. For each builder, we compute the average number of homes sold per year during the sample period and plot the distribution of this measure across all builders.

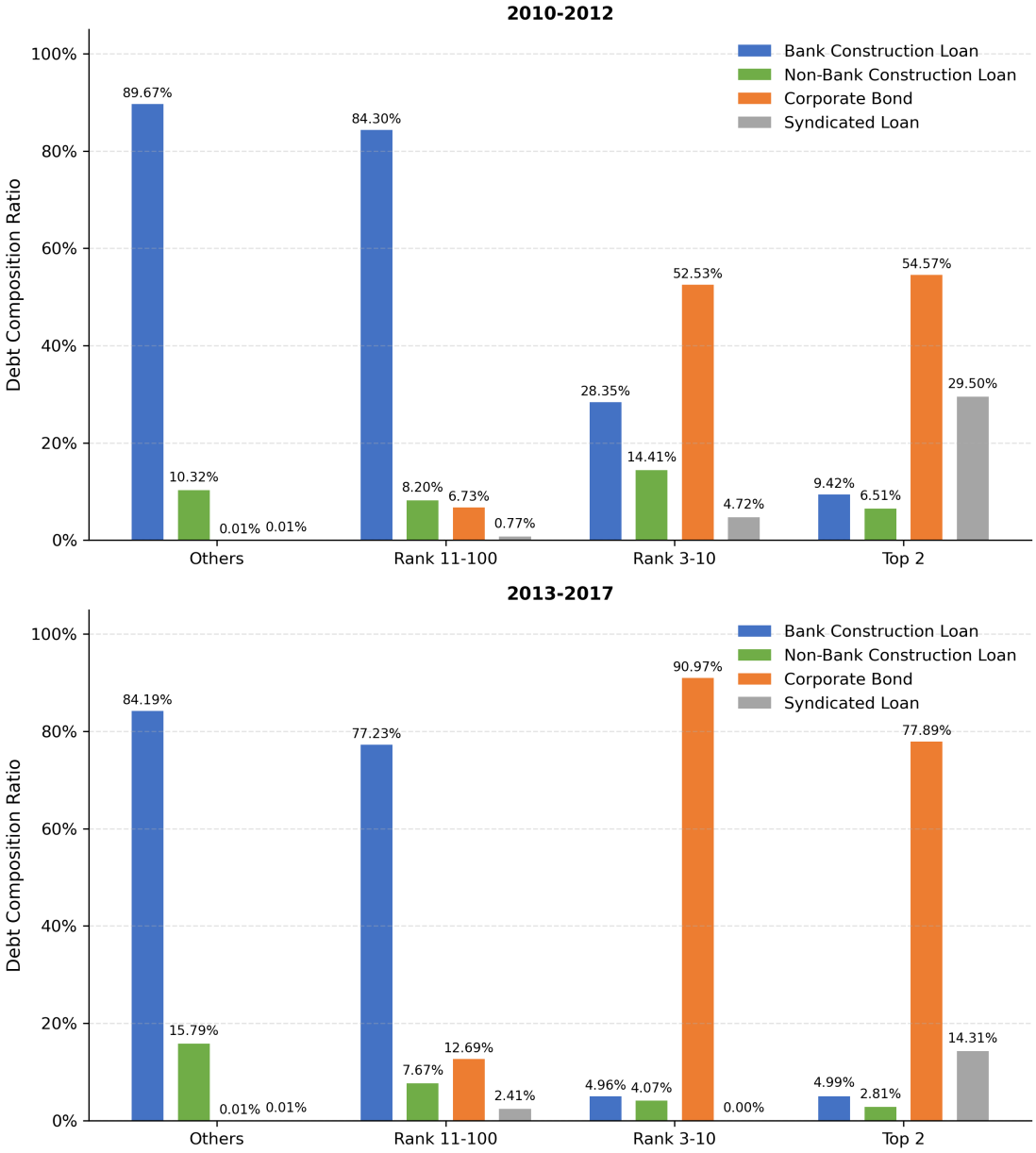


Figure 7: Debt Composition by Builder Size Group before and after 2013. This figure plots the mean shares of construction loans, corporate bonds, and syndicated loans in total debt for homebuilders, grouped by total home sales during 2010-2012 and 2013-2017. Builders are classified into four size categories, i.e., Top 2, Ranks 3–10, Ranks 11–100, and Others. Bars show the average composition across firms within each group. Construction loan data are from our Deeds-constructed dataset; syndicated-loan and corporate-bond data are from PitchBook.

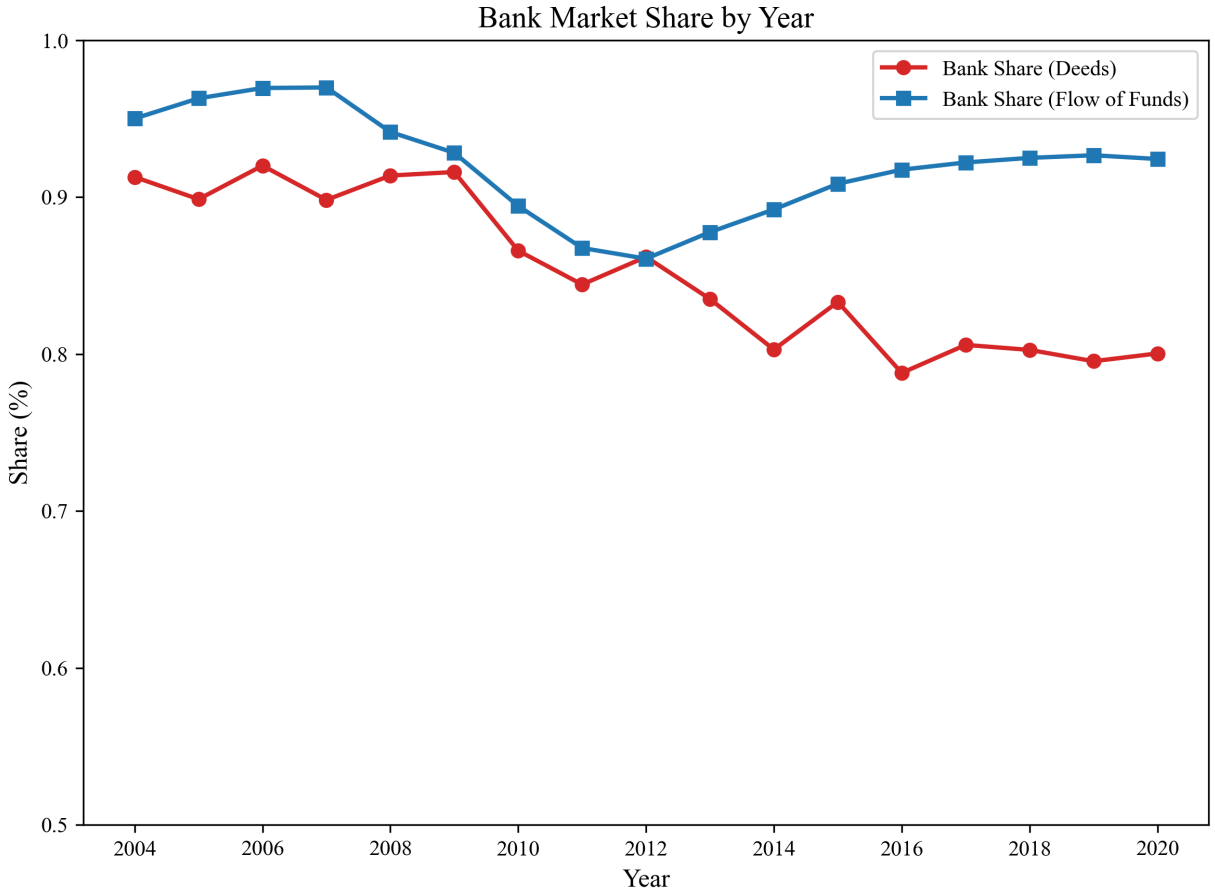


Figure 8: Construction Loans from Banks and All Sectors. This figure plots the annual share of bank-originated construction loans from 2004 to 2020. The red line measures bank market share computed from Deed records. The blue line measures bank market share from the Federal Reserve's Flow of Funds (Z.1 release), which captures aggregate outstanding one-to-four family construction loans held by commercial banks as a fraction of total credit market lending.

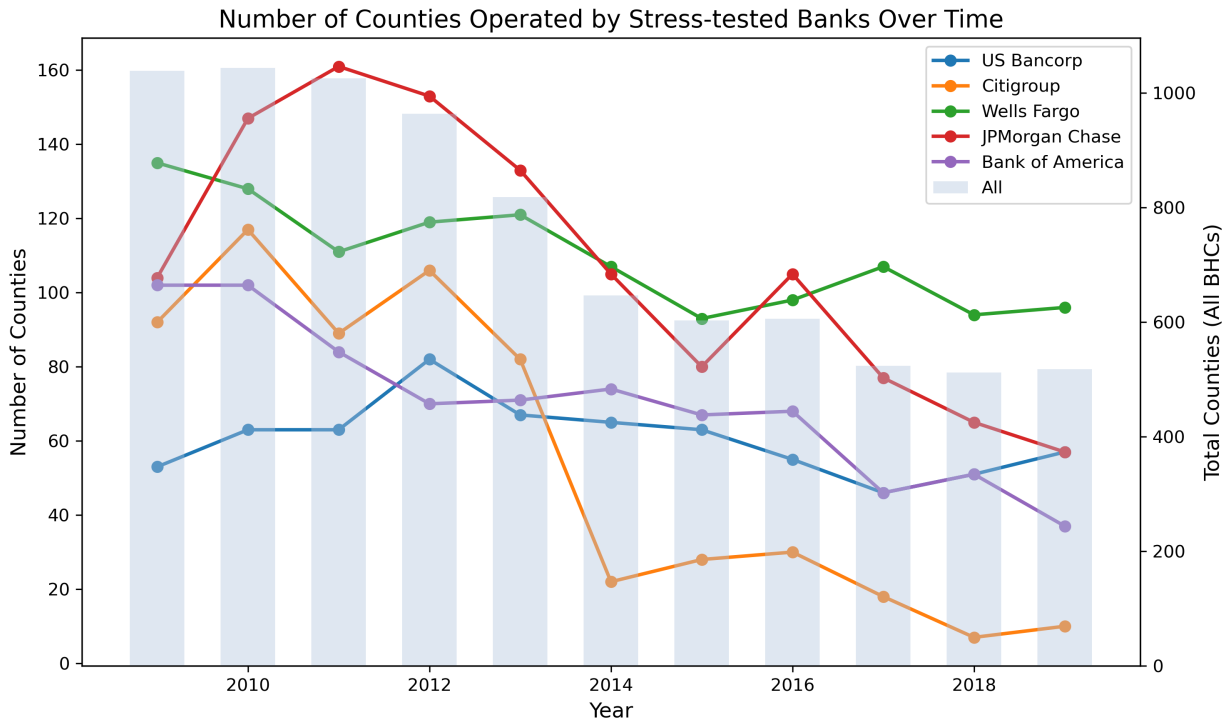


Figure 9: Geographic Footprint of Stress-Tested Banks in Construction Lending. This figure plots the number of counties in which each of the five largest stress-tested banks originated construction loan from 2009 to 2019 (left axis), along with the total number of counties served by all stress-tested banks (gray bars, right axis).

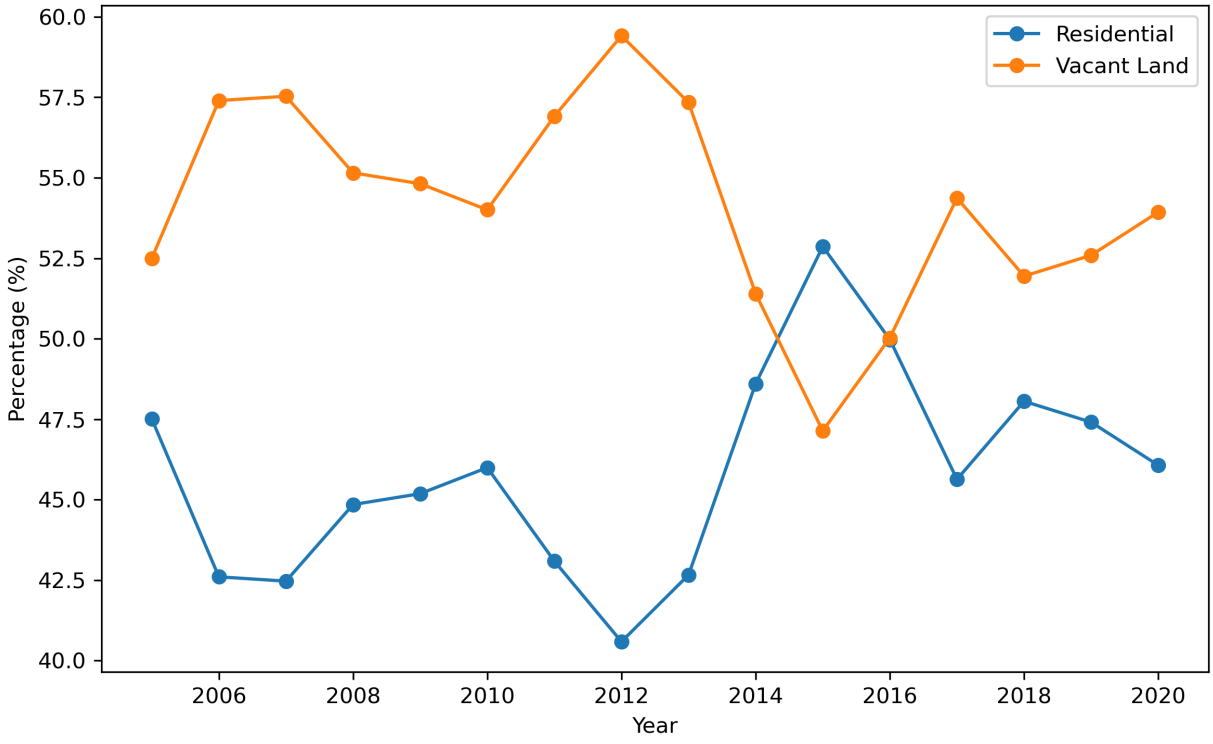


Figure 10: Composition of Construction Loan Collateral Types. This figure reports the percentage of construction financing collateralized by vacant land and residential properties. Most residential properties are single-family houses, although some are condominiums.

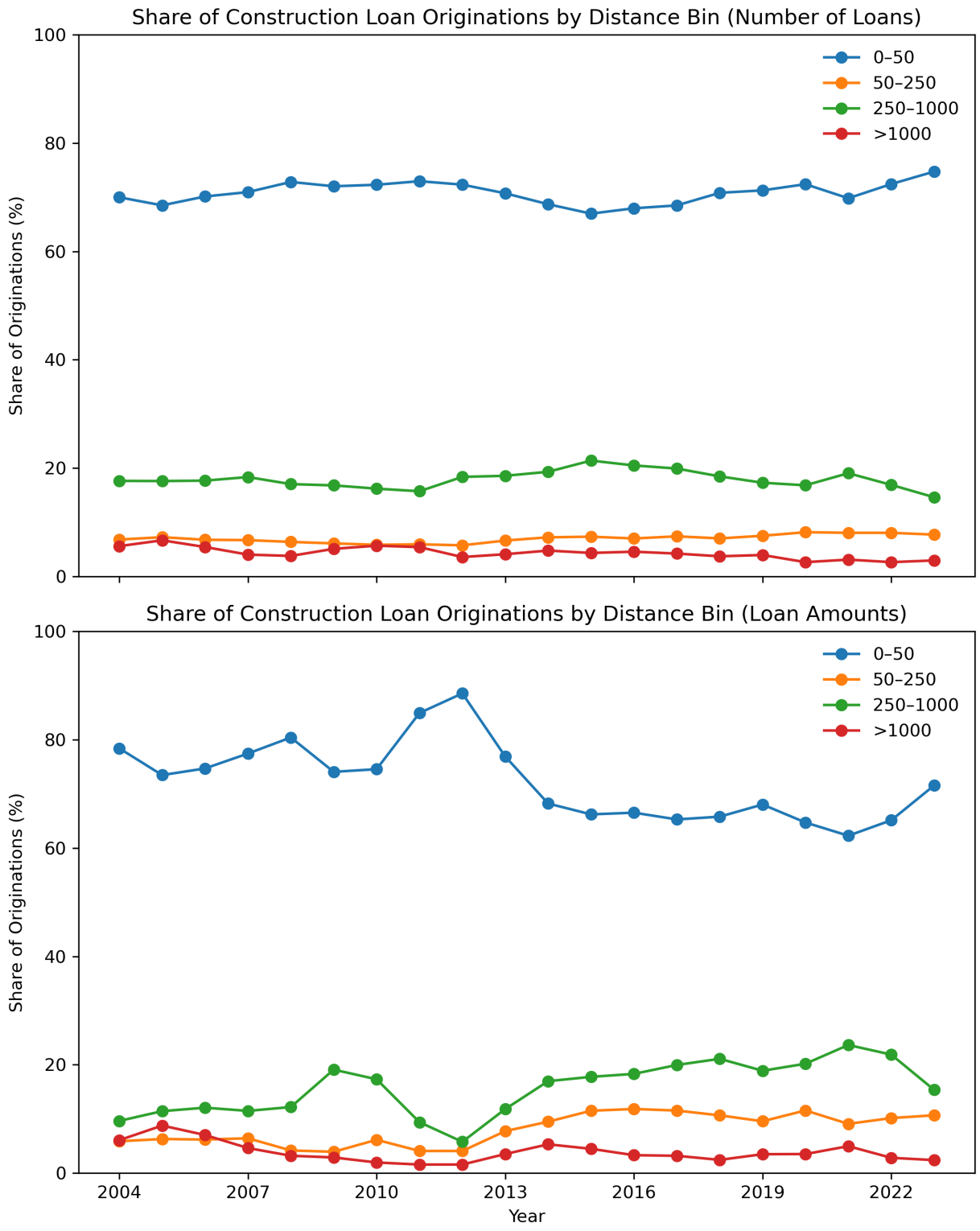


Figure 11: Time Series of Construction Loan Originations by Distance Bin. This figure plots the share of construction loan originations across different lender–builder distance bins over time. The top panel shows the distribution based on the number of loans, and the bottom panel shows the distribution based on loan amounts.

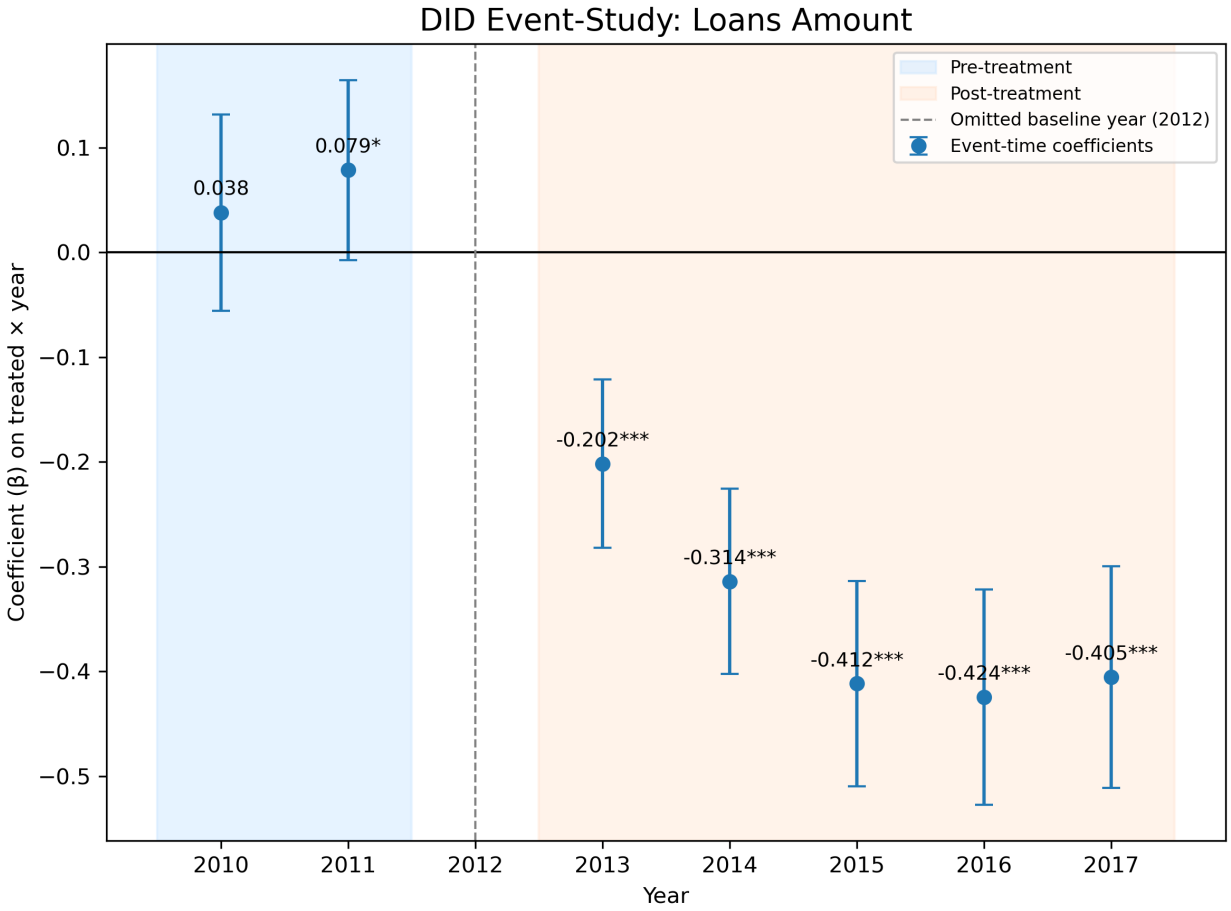


Figure 12: DID Event-Study: Loans Outcome. This figure reports the estimated coefficients from a difference-in-differences (DiD) event-study specification for loan outcomes. Each marker plots the interaction between the treatment indicator and year fixed effects, with 95% confidence intervals shown as error bars. The omitted baseline year is 2012 (vertical dashed line). The shaded areas denote the pre-treatment period (2010–2011) and the post-treatment period (2013–2016). Estimates are obtained using a Poisson regression model.

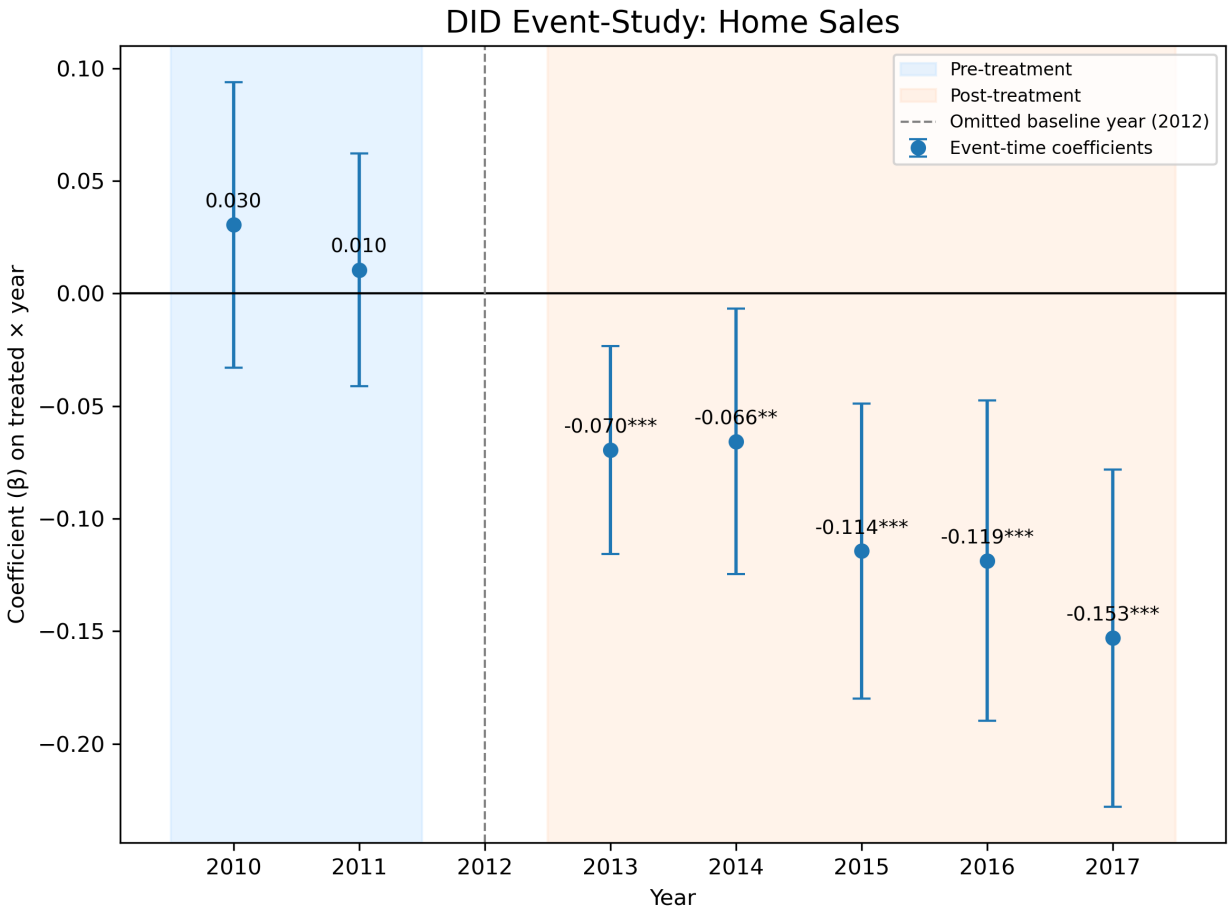


Figure 13: DID Event-Study: Home Sales. This figure reports the estimated coefficients from a difference-in-differences (DiD) event-study specification for home sales. Each marker plots the interaction between the treatment indicator and year fixed effects, with 95% confidence intervals shown as error bars. The omitted baseline year is 2012 (vertical dashed line). The shaded areas denote the pre-treatment period (2010–2011) and the post-treatment period (2013–2016). Estimates are obtained using a Poisson regression model.

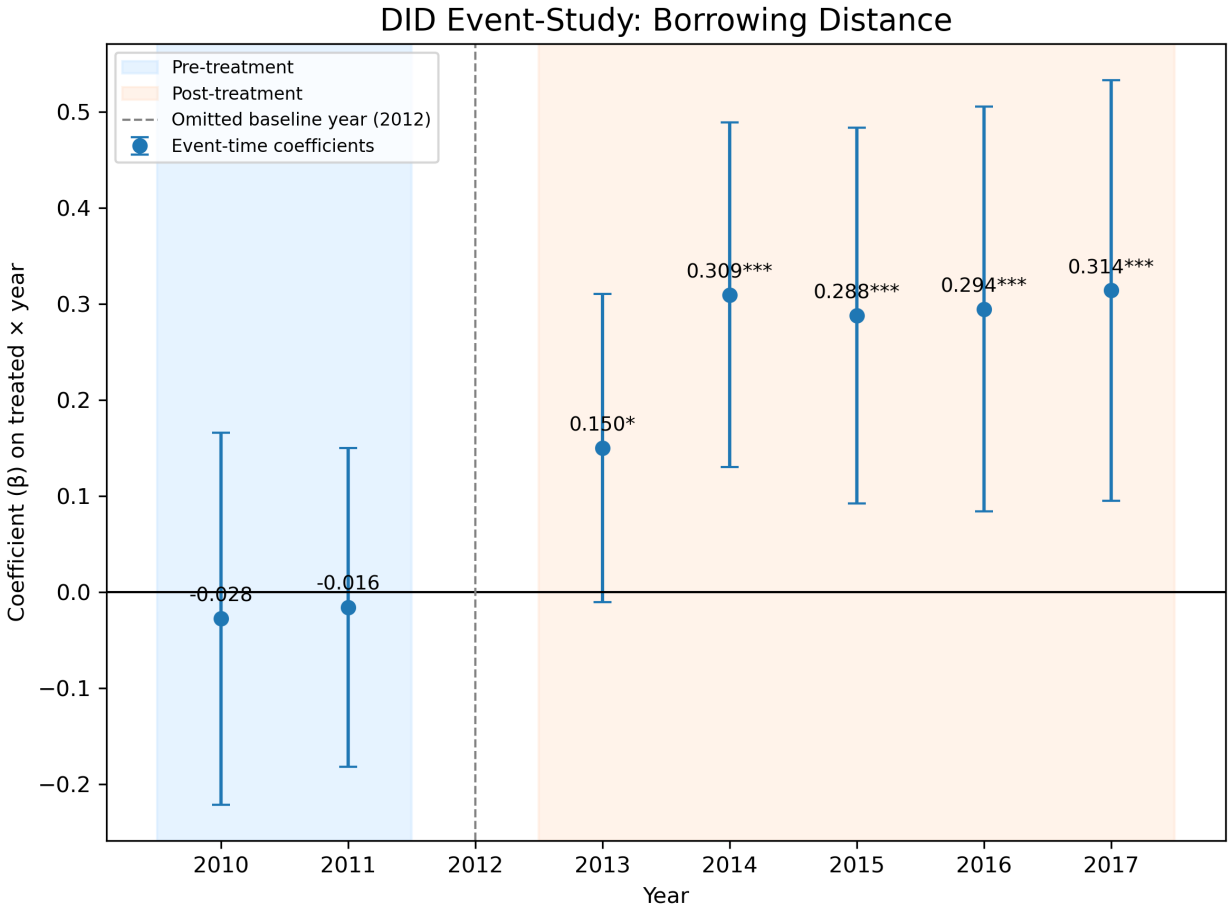


Figure 14: DID Event-Study: Borrowing Distance. This figure reports the estimated coefficients from a difference-in-differences (DiD) event-study specification for homebuilders' borrowing distance. The dependent variable is the log of the loan-amount weighted average lending distance (in miles) for each homebuilder-year. Each marker plots the interaction between the treatment indicator and year fixed effects, with 95% confidence intervals shown as error bars. The omitted baseline year is 2012 (vertical dashed line). The shaded areas denote the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). Firm and year fixed effects are included, with standard errors clustered at the homebuilder level.

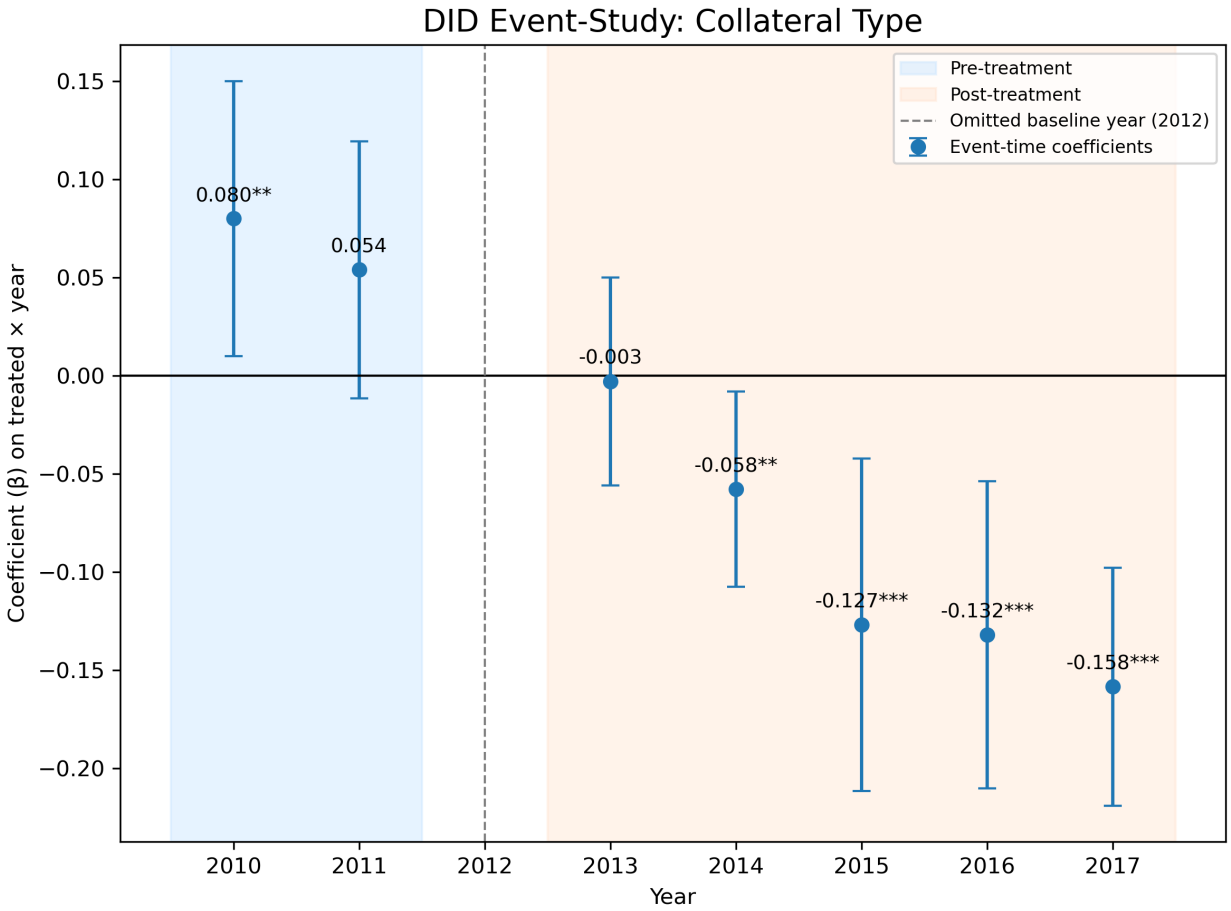


Figure 15: DID Event-Study: Collateral Type. This figure reports the estimated coefficients from a difference-in-differences (DiD) event-study specification for the use of vacant land as collateral. The dependent variable is a binary indicator equal to one if the loan is secured by vacant land. Each marker plots the interaction between the treatment indicator (whether the lender is a stress-tested bank) and year fixed effects, with 95% confidence intervals shown as error bars. The regression controls for log loan amount and log lending distance. The omitted baseline year is 2012 (vertical dashed line). The shaded areas denote the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). Lender and year fixed effects are included, with standard errors clustered at the lender level.

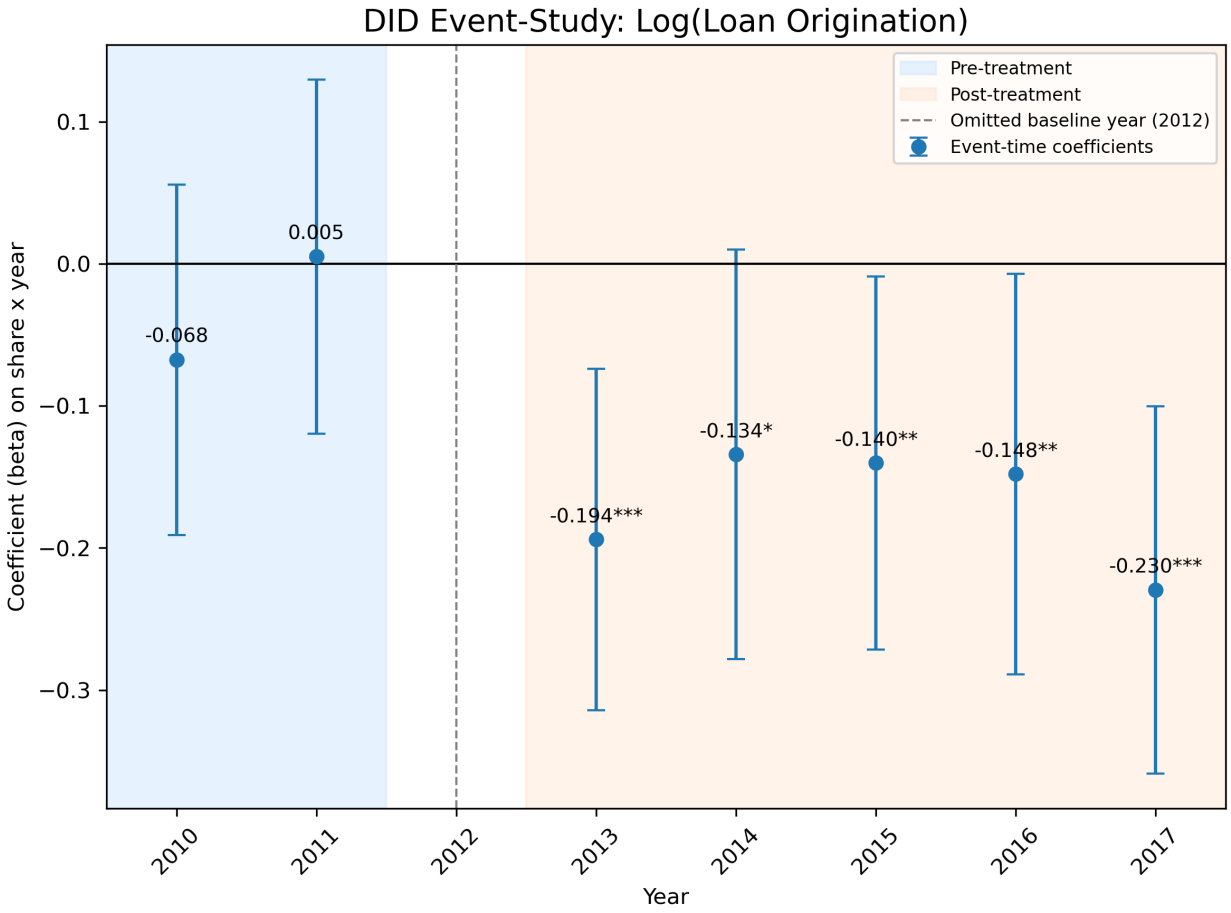


Figure 16: County-Level Event Study: Stress-Tested Loan Share and Loan Origination. This figure plots the estimated coefficients β_t from the event-study specification: $\ln(\text{Loan Origination})_{c,t} = \alpha_c + \delta_t + \sum_t \beta_t \cdot \text{StressShare}_c \times \mathbf{1}[\text{year} = t] + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t}$, where StressShare_c is the pre-reform (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation. α_c denotes county fixed effects, δ_t denotes year fixed effects, and $\mathbf{X}_{c,t}$ includes time-varying controls for income growth, GDP growth, and population growth. The omitted baseline year is 2012 (vertical dashed line). Error bars represent 95% confidence intervals based on standard errors clustered at the county level. The shaded areas indicate the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). The sample includes counties with a pre-reform average population above 50,000.

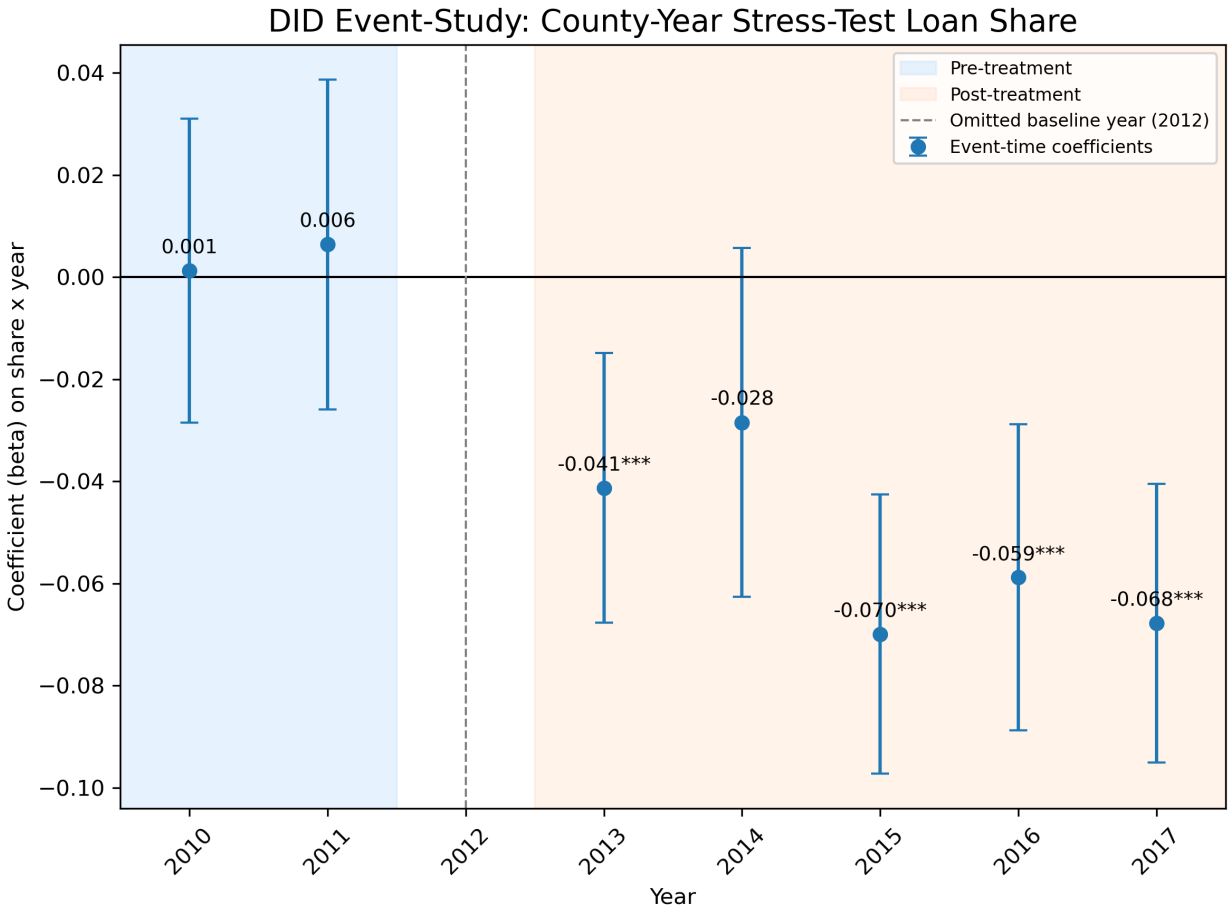


Figure 17: County-Level Event Study: Stress-Tested Loan Share and County-Year Stress-Test Loan Share. This figure plots the estimated coefficients β_t from the event-study specification: $\text{StressShare}_{c,t} = \alpha_c + \delta_t + \sum_t \beta_t \cdot \text{StressShare}_c \times \mathbf{1}[\text{year} = t] + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t}$, where the dependent variable is the contemporaneous county-year share of construction loans originated by stress-tested banks, and StressShare_c is the pre-reform (2004–2007) share. α_c denotes county fixed effects, δ_t denotes year fixed effects, and $\mathbf{X}_{c,t}$ includes time-varying controls for income growth, GDP growth, and population growth. The omitted baseline year is 2012 (vertical dashed line). Error bars represent 95% confidence intervals based on standard errors clustered at the county level. The shaded areas indicate the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). The sample includes counties with a pre-reform average population above 50,000.

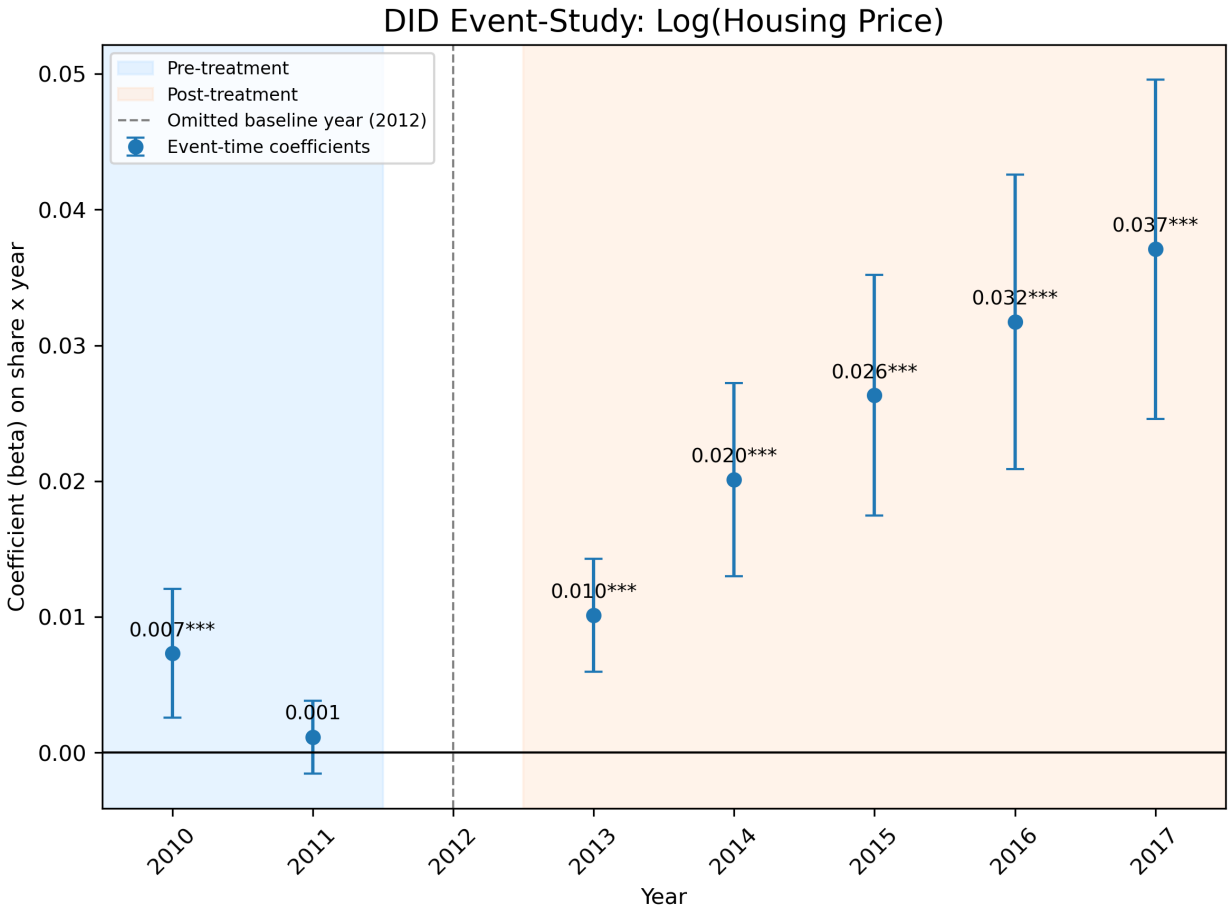


Figure 18: County-Level Event Study: Stress-Tested Loan Share and Housing Price. This figure plots the estimated coefficients β_t from the event-study specification: $\ln(\text{Housing Price})_{c,t} = \alpha_c + \delta_t + \sum_t \beta_t \cdot \text{StressShare}_c \times \mathbf{1}[\text{year} = t] + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t}$, where StressShare_c is the pre-reform (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation. α_c denotes county fixed effects, δ_t denotes year fixed effects, and $\mathbf{X}_{c,t}$ includes time-varying controls for income growth, GDP growth, and population growth. The omitted baseline year is 2012 (vertical dashed line). Error bars represent 95% confidence intervals based on standard errors clustered at the county level. The shaded areas indicate the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). The sample includes counties with a pre-reform average population above 50,000.

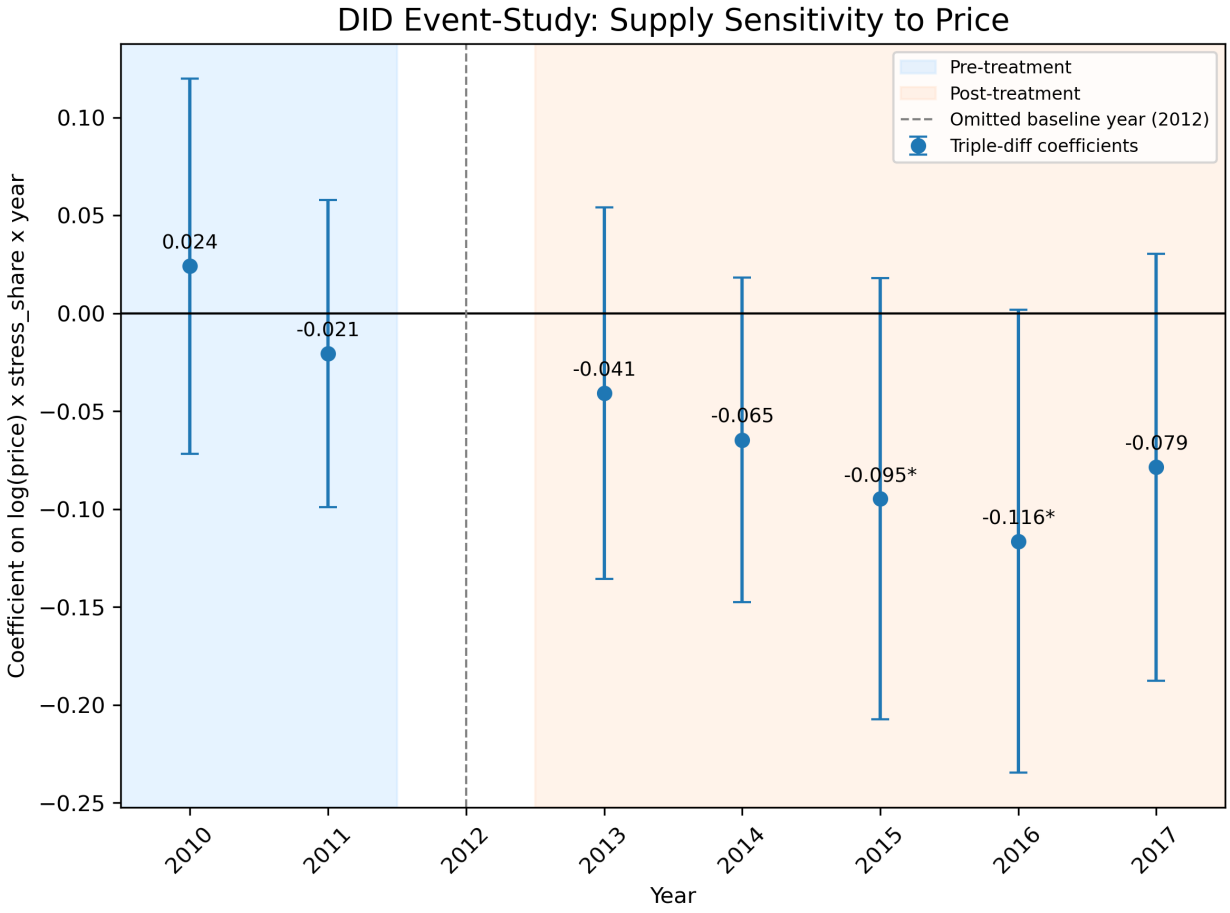


Figure 19: County-Level Event Study: Stress-Tested Loan Share and Housing Supply Response. This figure plots the estimated triple-difference coefficients γ_t from the specification: $\ln(\text{SFH Sales})_{c,t} = \alpha_c + \delta_t + \sum_t \beta_t \cdot \text{StressShare}_c \times \mathbf{1}[\text{year} = t] + \sum_t \phi_t \cdot \ln(\text{Price})_{c,t} \times \mathbf{1}[\text{year} = t] + \sum_t \gamma_t \cdot \ln(\text{Price})_{c,t} \times \text{StressShare}_c \times \mathbf{1}[\text{year} = t] + \mathbf{X}'_{c,t}\psi + \varepsilon_{c,t}$, where StressShare_c is the pre-reform (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation. The plotted coefficients γ_t capture the differential housing supply elasticity—the responsiveness of new construction to price changes—in counties with greater pre-reform exposure to stress-tested banks. α_c denotes county fixed effects, δ_t denotes year fixed effects, and $\mathbf{X}_{c,t}$ includes time-varying controls for income growth, GDP growth, and population growth. The omitted baseline year is 2012 (vertical dashed line). Error bars represent 95% confidence intervals based on standard errors clustered at the county level. The shaded areas indicate the pre-treatment period (2010–2011) and the post-treatment period (2013–2017). The sample includes counties with a pre-reform average population above 50,000.

Table 1: Summary Statistics of Homebuilder and Construction Loans. This table summarizes key statistics for homebuilders and their associated construction loans. For the number of transactions, sales amounts, and loan amounts, we first calculate the average for each firm over the period 2010–2022. These firm-level averages are then used to compute the overall summary statistics. The 'number of counties' represents the total number of counties where a homebuilder sold houses during 2010–2022. The 'number of banking relationships' indicates the total number of different banks from which homebuilders borrowed money during the same period. The interest rate spread is calculated as the difference between the loan's interest rate and the risk-free rate at the time of issuance. Finally, the borrowing distance is the distance between the property used as collateral and the closest branch of the bank that issued the loan.

Variable	Obs	Mean	Std. Dev.	p25	p50	p75
Panel A: Builder Characteristics						
No. of Transactions	18,278	26.5	467.6	3.3	5.2	10.4
Sales Amount ($ million)	18,278	9.7	185.1	0.3	1.0	2.7
No. of County	18,278	6.0	14.5	1.0	3.0	6.0
Loan Amount ($ million)	18,278	3.1	9.9	0.4	0.9	2.3
No. of Banking Relationships	18,278	4.2	6.2	1.0	2.0	5.0
Annual Borrowing Probability	18,278	0.4	0.2	0.1	0.4	0.6
Annual Sales Probability	18,278	0.6	0.3	0.4	0.6	0.9
Panel B: Construction Loans Characteristics						
Loan Size ($ million)	818,665	0.6	4.3	0.1	0.2	0.3
Maturity	393,579	3.5	6.8	1.0	1.0	2.0
Interest Rate Spread	28,393	7.5	4.3	4.5	5.8	10.0
Borrowing Distance	329,791	179.9	352.2	3.1	8.8	160.2

Table 2: Summary Statistics of the Exposed and Unexposed Homebuilders during 2010-2012. This table summarizes key statistics for homebuilders before the reforms. For the number of transactions, sales amounts, and loan amounts, we first calculate the average for each firm over the period 2010–2012. These firm-level averages are then used to compute the overall summary statistics. The 'number of counties' represents the total number of counties where a homebuilder sold houses during 2010–2012. The 'number of banking relationships' indicates the total number of different banks from which homebuilders borrowed money during the same period.

	More Exposed		Less Exposed	
Variable	Mean	Median	Mean	Median
No. of Transactions	80.33	9.00	12.44	4.00
No. of County	6.61	3.00	3.01	2.00
Loan Amount ($ million)	11.78	2.40	2.11	0.67
No. of Banking Relationships	3.10	2.00	1.63	1.00
Annual Borrowing Probability	0.80	1.00	0.68	0.67
Annual Sales Probability	0.89	1.00	0.81	1.00
Observation	1,965		7,366	

Table 3: Summary Statistics of High vs. Low Exposure Counties by Period. This table compares county-level characteristics across three periods for counties with high and low exposure to stressed banks. High exposure counties are those with above median pre-GFC stress-tested bank market share, computed within the sample of counties with pre-crisis population above 50,000. Level variables (Panel A) report average values within each period. Growth rates (Panel B) report the mean annual growth rate within each period.

	High Exposure			Low Exposure		
Variable	2004–2006	2007–2009	2010–2012	2004–2006	2007–2009	2010–2012
Panel A: Level						
House Price	214,908	210,214	177,914	196,867	190,056	169,678
SFH Permits	2,392	929	654	781	371	268
Per Capita Income	33,696	37,314	39,549	32,564	36,526	39,257
Population	386,621	400,855	413,935	189,434	193,206	197,543
Panel B: Annual Growth Rate (%)						
House Price Growth	9.08%	−3.96%	−3.37%	8.31%	−2.27%	−2.25%
SFH Permits Growth	−3.44%	−28.95%	9.15%	−3.87%	−20.69%	9.09%
Income Growth	4.82%	1.59%	3.49%	4.47%	2.37%	3.65%
Population Growth	1.73%	1.35%	0.89%	0.92%	0.83%	0.55%
GDP Growth	3.83%	−0.17%	1.86%	3.03%	−0.28%	1.98%
Observations		419			419	

Table 4: The Effect of Basel III and DFA Stress Test on Homebuilders' Credit Availability. This table summarizes the results from running the following regressions:

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta_1 Post_t + \beta_2 Exposed_i + \beta \text{Controls}_{i,t} + \varepsilon_{i,t}$$

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta \text{Controls}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t}$$

Columns (1)–(2) estimate loan amounts in levels using a Poisson pseudo–maximum likelihood (PPML) specification. The dependent variable in Columns (3)–(4) is a binary indicator $\phi^l_{i,t}$ that equals one if homebuilder i obtained any construction financing in year t, while the dependent variable in Columns (5)–(6) is the logarithm of the total loan volume by homebuilder i in year t. The variable $Exposed_i$ is a binary indicator equal to one if homebuilder i borrowed from a stress-tested bank during 2010–2012. $Post_t$ is a binary indicator equal to one if year $t \geq 2013$. Firm fixed effects are denoted by α_i and year fixed effects by η_t. Standard errors are clustered at the homebuilder level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	Loan-Poisson		Loan-Extensive Margin		Loan-Intensive Margin	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Exposed	-0.373***	-0.379***	-0.064***	-0.064***	-0.325***	-0.357***
	(0.034)	(0.034)	(0.009)	(0.009)	(0.031)	(0.030)
Exposed	1.025***		0.125***		0.955***	
	(0.032)		(0.007)		(0.032)	
Post	0.082***		-0.106***		0.339***	
	(0.019)		(0.004)		(0.014)	
Constant	13.908***	14.967***	0.684***	0.645***	13.540***	13.968***
	(0.019)	(0.007)	(0.003)	(0.001)	(0.014)	(0.004)
Firm Fixed Effects	No	Yes	No	Yes	No	Yes
Year Fixed Effects	No	Yes	No	Yes	No	Yes
R^2 / Pseudo R^2	0.059	0.598	0.020	0.405	0.067	0.598
Observations	72,784	72,481	74,648	74,648	45,146	44,129

Table 5: The Effect of Basel III and DFA Stress Test on Homebuilders' Housing Sales. This table summarizes the results from running the following regressions:

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta_1 Post_t + \beta_2 Exposed_i + \beta \text{Controls}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t}$$

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta \text{Controls}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t}$$

Columns (1)–(2) estimate new house amounts in levels using a Poisson pseudo–maximum likelihood (PPML) specification. The dependent variable in Columns (3)–(4) is a binary indicator $\phi^h_{i,t}$ that equals one if homebuilder i sold any houses in year t, while the dependent variable in Columns (5)–(6) is the logarithm of the housing sales by homebuilder i in year t. The variable $Exposed_i$ is a binary indicator equal to one if homebuilder i borrowed from a stress-tested bank during 2010–2012. $Post_t$ is a binary indicator equal to one if year $t \geq 2013$. Firm fixed effects are denoted by α_i and year fixed effects by ζ_c. Standard errors are clustered at the homebuilder level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	Sales-Poisson		Sales-Extensive Margin		Sales-Intensive Margin	
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Exposed	-0.101***	-0.117***	-0.045***	-0.045***	-0.128***	-0.158***
	(0.029)	(0.027)	(0.008)	(0.008)	(0.026)	(0.024)
Exposed	0.774***		0.080***		0.644***	
	(0.033)		(0.007)		(0.030)	
Post	0.221***		-0.012***		0.184***	
	(0.015)		(0.004)		(0.011)	
Constant	1.995***	3.028***	0.801***	0.811***	1.590***	1.844***
	(0.017)	(0.006)	(0.003)	(0.001)	(0.012)	(0.003)
Firm Fixed Effects	No	Yes	No	Yes	No	Yes
Year Fixed Effects	No	Yes	No	Yes	No	Yes
R^2 / Pseudo R^2	0.051	0.632	0.004	0.380	0.040	0.670
Observations	72,786	72,720	74,648	74,648	58,584	58,344

Table 6: Within-Builder Estimation: The Effect of Stress Testing on Bank-Builder Lending. This table reports results from the following cross-sectional regression estimated following Khwaja and Mian (2008):

$$\Delta Y_{i,j} = \beta_1 \, StressTest_j + \alpha_i + \varepsilon_{i,j}$$

The unit of observation is a bank-builder pair. We collapse the panel into a pre-period (2010–2012) and a post-period (2013–2015) by loan amounts within each period. The dependent variable in columns (1)–(2) is the change in lending indicator, $\Delta \mathbb{1}(Loan)$, which equals 0 if an existing relationship continues and -1 if an existing relationship is terminated (extensive margin). The dependent variable in columns (3)–(4) is $\Delta \log(\text{Loan})$, the change in the log of average loan amount, defined only for pairs with positive lending in both periods (intensive margin). The variable $StressTest_j$ is a binary indicator equal to one if bank j is a stress-tested bank. Builder fixed effects are denoted by α_i. The sample is restricted to builders with relationships with at least two banks and the banking relationship is restricted to pre-existing relationships. Standard errors are clustered at the bank level. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	Extensive Margin		Intensive Margin	
	(1)	(2)	(3)	(4)
Stress Test	-0.162^{***}	-0.124^{***}	0.123	0.102
	(0.045)	(0.046)	(0.083)	(0.088)
Builder Fixed Effects	No	Yes	No	Yes
Within-R^2	0.022	0.016	0.003	0.002
Observations	5,363	5,363	1,703	1,703

Table 7: The Effect of Basel III and DFA Stress Tests on Homebuilders' Borrowing Distance. This table summarizes the results from running the following regression:

$$y_{i,t} = \gamma(Post_t \times Exposed_i) + \beta \text{Controls}_{i,t} + \alpha_i + \eta_t + \varepsilon_{i,t}$$

The dependent variable is the logarithm of the homebuilder–bank distance, weighted by loan amounts, for homebuilder i in year t. The variable $Exposed_i$ is a binary indicator equal to one if homebuilder i borrowed from a stress-tested bank during 2010–2012. $Post_t$ is a binary indicator equal to one if year $t \geq 2013$. Firm fixed effects are denoted by α_i and year fixed effects by η_t. Standard errors, clustered at the homebuilder level, are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	(1)	(2)	(3)
Post × Exposed	0.495***	0.258***	0.277***
	(0.067)	(0.065)	(0.067)
Exposed	−0.185***		
	(0.059)		
Post	−0.122***		
	(0.036)		
Control	Yes	No	Yes
Firm Fixed Effects	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes
R^2 / Within-R^2	0.004	0.001	0.002
Observations	26,724	26,384	25,274

Table 8: The Effect of Basel III and DFA Stress Tests on Collateral Type. This table summarizes the results from running the following regressions:

$$\phi^c_{j,b,t} = \gamma\left(Post_t \times TreatedLoan_{j,b}\right) + \beta_0 TreatedLoan_{j,b} + \beta_1 Post_t + \beta_c \text{Controls}_{j,b,t} + \varepsilon_{j,b,t}$$

$$\phi^c_{j,b,t} = \gamma\left(Post_t \times TreatedLoan_{j,b}\right) + \beta_c \text{Controls}_{j,b,t} + \alpha_b + \zeta_t + \varepsilon_{j,b,t}$$

The dependent variable $\phi^c_{j,b,t}$ is a binary indicator that equals one if loan j issued by lender b in year t is collateralized by vacant land, and zero otherwise. The variable $TreatedLoan_{j,b}$ is a binary indicator equal to one if lender b is a stress-tested bank. $Post_t$ is a binary indicator equal to one if year $t \geq 2013$. Controls include the logarithm of loan amount and the logarithm of borrowing distance where indicated. Bank fixed effects are denoted by α_b and year fixed effects by ζ_t. Standard errors, clustered at the bank level, are reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	(1)	(2)	(3)
Post × TreatedLoan	-0.082^{**}	-0.114^{***}	-0.119^{***}
	(0.035)	(0.026)	(0.027)
TreatedLoan	0.166^{***}		
	(0.039)		
Post	-0.010		
	(0.014)		
Control	Yes	No	Yes
Bank Fixed Effects	No	Yes	Yes
Year Fixed Effects	No	Yes	Yes
R^2 / Within-R^2	0.007	0.001	0.004
Observations	135,256	193,485	134,861

Table 9: County-Level DiD: Loan Origination. This table reports estimates from the panel difference-in-differences specification:

$$y_{c,t} = \alpha_c + \delta_t + \beta \cdot \text{StressShare}_c \times \text{Post}_t + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t},$$

where StressShare_c is the pre-GFC (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation. Post_t equals one for years 2013–2017 and zero for 2010–2012. α_c and δ_t denote county and year fixed effects. Columns (1)–(3) use $\ln(\text{Loan Origination})_{c,t}$ as the dependent variable; columns (4)–(6) use the contemporaneous county-year stress-test loan share. Column (1) and (4) include no time-varying controls. Columns (2) and (5) add income growth, GDP growth, and population growth. Columns (3) and (6) further add $\text{WRLURI}_c \times \text{Post}_t$ to control for land use regulation. Standard errors are clustered at the county level. The sample includes counties with pre-reform average population above 50,000. $^{*}\,p < 0.1$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

	$\ln(\text{Loan})_{c,t}$			Stress-Test $\text{Share}_{c,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{StressShare}_c \times \text{Post}_t$	-0.143^{***}	-0.149^{***}	-0.199^{***}	-0.057^{***}	-0.056^{***}	-0.061^{***}
	(0.046)	(0.046)	(0.051)	(0.009)	(0.009)	(0.010)
Controls	No	Yes	Yes	No	Yes	Yes
WRLURI × Post	No	No	Yes	No	No	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,039	5,039	3,528	5,039	5,039	3,528

Table 10: County-Level DiD: Housing Price. This table reports estimates from the panel difference-in-differences specification:

$$\ln(\text{Price})_{c,t} = \alpha_c + \delta_t + \beta \cdot \text{StressShare}_c \times \text{Post}_t + \mathbf{X}'_{c,t}\gamma + \varepsilon_{c,t},$$

where StressShare_c is the pre-GFC (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation. Post_t equals one for years 2013–2017 and zero for 2010–2012. α_c and δ_t denote county and year fixed effects. Column (1) includes no time-varying controls. Column (2) adds income growth, GDP growth, and population growth. Column (3) further adds $\text{WRLURI}_c \times \text{Post}_t$ to control for land use regulation. Standard errors are clustered at the county level. The sample includes counties with pre-reform average population above 50,000. $^{*}\,p < 0.1$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

	$\ln(\text{Price})_{c,t}$		
	(1)	(2)	(3)
$\text{StressShare}_c \times \text{Post}_t$	0.025^{***}	0.022^{***}	0.029^{***}
	(0.004)	(0.004)	(0.005)
Controls	No	Yes	Yes
WRLURI × Post	No	No	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	5,308	5,308	3,722

Table 11: County-Level DiD: Housing Supply Response. This table reports estimates from the panel triple-difference specification:

$$\begin{aligned}\ln(\text{SFH})_{c,t} = \alpha_c + \delta_t + \beta_1 \cdot \text{StressShare}_c \times \text{Post}_t + \beta_2 \cdot \ln(\text{Price})_{c,t} \times \text{Post}_t \\ + \gamma \cdot \ln(\text{Price})_{c,t} \times \text{StressShare}_c \times \text{Post}_t + \mathbf{X}'_{c,t}\psi + \varepsilon_{c,t},\end{aligned} \tag{10}$$

where StressShare_c is the pre-GFC (2004–2007) share of construction loans originated by stress-tested banks in county c, normalized by one standard deviation, and Post_t equals one for years 2013–2017. The coefficient of interest is γ, which captures the differential housing supply response to local house prices in counties with greater pre-reform exposure to stress-tested banks. α_c and δ_t denote county and year fixed effects. Column (1) includes no time-varying controls. Column (2) adds income growth, GDP growth, and population growth. Column (3) further adds $\text{WRLURI}_c \times \text{Post}_t$. Standard errors are clustered at the county level. The sample includes counties with pre-reform average population above 50,000. $^{*}\,p < 0.1$; $^{**}\,p < 0.05$; $^{***}\,p < 0.01$.

	$\ln(\text{SFH Sales})_{c,t}$		
	(1)	(2)	(3)
$\ln(\text{Price})_{c,t} \times \text{StressShare}_c \times \text{Post}_t$	-0.093^{**}	-0.083^{**}	-0.098^{**}
	(0.043)	(0.042)	(0.044)
Controls	No	Yes	Yes
WRLURI × Post	No	No	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	5,222	5,222	3,665

A Details on Data Cleaning and Sample Construction

A.1 Identifying Potential Builders

We provide a real (anonymized) example of the builder identification process. Table A1 documents the property's transaction history: it was sold in 2016 from the seller (Anonymous 1) to a buyer (Anonymous 2) and later transferred again in 2022 to new owners (Anonymous 3). We define a seller as a potential homebuilder if it is the seller of a newly constructed home. Specifically, we classify a property as newly built when its recorded year of construction is within a narrow window around the transaction year—no more than one year before and no more than two years after the sale year. In this example, Anonymous 1 qualifies as a potential home builder because it sold the property shortly after construction, whereas Anonymous 2 does not, as its subsequent sale in 2022 was unrelated to new construction.

Table A1: Transaction Records

Year	Seller	Buyer	Construction Year
2016	Anonymous 1	Anonymous 2	2015
2022	Anonymous 2	Anonymous 3	2015

In order to focus on a final list of home builders of our interest, we manually remove financial institutions, government agencies, and nonprofit organizations (e.g., Habitat for Humanity).

A.2 Construction of the Builder–Sales Panel

Name Harmonization: Based on our builder list, we uniquely identify each builder over time, even when the same builder appears under different names in the raw data. We begin by constructing a dictionary of standardized keywords to collapse common abbreviations (e.g., "BLDRS" for

"BUILDERS") and obvious typos (e.g., "BUILDERSS" for "BUILDERS"). We then harmonize builder identities using a composite fuzzy-matching procedure that combines phonetic and semantic similarity. Specifically, we apply the Python Metaphone algorithm to capture phonetic resemblance across builder names and use the SentenceTransformer framework to compute semantic vector similarity between name strings. This dual approach allows us to systematically identify and collapse variant spellings, abbreviations, and minor lexical differences into a single canonical builder identifier, thereby mitigating measurement error arising from inconsistent naming conventions in transaction records.

Subsidiaries of Builders: We develop a novel procedure to identify subsidiaries of large homebuilders by combining information scraped from SEC Form 10-K filings with company records from OpenCorporates and outputs from OpenAI-based language models. Using this information, we consolidate new construction transaction records of subsidiaries under their respective parent companies. This step is critical, as many subsidiaries operate under names that bear little resemblance to their parent firm, which would otherwise lead to substantial misclassification in transaction-level data. For example, Table A2 presents a subset of subsidiaries associated with UCP, Inc, identified from 10-K filings and company records from OpenCorporates.

Table A2: Examples of Subsidiaries of Builders

Parent Company	Subsidiary Company
UCP, Inc.	Benchmark Builders North Carolina, LLC
	BMC Carnation, LLC
	BMC Cornerstne Ripon II, LLC
	UCP Barclay III, LLC
	UCP Soledad, LLC
	Fresno Land and Cattle Company, LLC
	...

ChatGPT Prompt: We next use the gpt-4o-search-preview model, which has the capability

to conduct online searches, to complete and verify the list of subsidiary information. The full prompt used in this step is reported in Figure A1. After obtaining the model's output, we combine this information with transaction-level data on the geographic locations of homes sold by each builder. This additional validation step allows us to distinguish between subsidiaries operating under different names but belonging to the same parent company, and distinct small builders that happen to share similar names but operate independently in different local markets.

Provide information about the given US builder {XXX}:
please answer the following:
1. Where in the United States do they build homes?
2. Are they currently an independent company or have they been acquired by another homebuilding company?
3. Do they go by any other names?
Answer directly and do not repeat the questions.

Figure A1: ChatGPT Prompt.

Builder-Sales Panel: Using the harmonized builder identifiers, we construct a panel of transaction of newly constructed single-family homes for each builder and year. The data include detailed information on buyers and sellers, transaction dates, sale prices, and property addresses. By merging deed records with tax assessment files, we additionally observe rich property characteristics, including the year of construction, the number of bedrooms and bathrooms, and building square footage. We exclude all non–arm's-length transactions and foreclosure-related sales from the sample.