GSE RESTRICTIONS, CREDIT SUPPLY, AND RENTAL MARKET SPILLOVERS

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ABSTRACT

In 2021, the U.S. Treasury instituted caps to reduce Government-Sponsored Enterprise exposure to second-home and investment-property mortgages, leading to declines in their purchase of affected mortgages. The policy lowered credit supply to affected housing investors, with higher interest rates and lower originations. Bank and non-bank lenders display a similar supply response, suggesting that deposits offer no advantage in mortgage lending. Lenders adjust at both the portfolio level and by reallocating mortgages across local markets, suggesting that they manage credit provision market-by-market. Rental housing supply and condominium prices decline while rents increase, consistent with higher financing costs in the rental market.

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1. INTRODUCTION

Housing prices rose faster during the period surrounding the COVID-19 pandemic than even in the run-up before the 2008 Global Financial Crisis.[1] This boom coincided with a rapid expansion of loans purchased by the Government Sponsored Enterprises (GSEs).[2] During the early quarters of the pandemic, second-home and investment-property purchase transactions also rose sharply (Figure 1).[3] Although levels never reached the highs seen prior to the Global Financial Crisis of 2008, which was fueled by a credit boom (Mian and Sufi, 2022), the sharp increase in house transactions compounded the rising risk exposure at the GSEs.[4]

This paper studies how caps instituted in 2021 on GSE mortgage purchases of loans for second home and investment properties (which together we call "non-primary") affected these trends. As Figure 1 shows, the non-primary home share declines in the middle of 2021, coincident with the policy we study. We show that much of this decline is due to the policy. Detailed data on mortgage originations allow us to show that lenders comply with the constraints not only at the portfolio level, but also by reallocating credit across local markets. In addition, we show that because the policy raises the cost of financing non-owner-occupied properties, rents increase and prices fall, especially for condominiums, which are more exposed to the policy than single-family houses by being more likely to be used as rental properties.

At the end of the Trump-Pence Administration, the U.S. Treasury – the residual claimant in the GSEs – initiated a policy to strengthen the GSEs' capital ratios and reduce their risk, with

[1] According to the Case-Shiller national index, housing prices rose about 40% in the three years before the peak in mid-2006. In contrast, prices rose 45% between mid-2019 and mid-2022 (see: S&P CoreLogic Case-Shiller U.S. National Home Price Index available at the Federal Reserve Economic Data).
[2] See page 88 in the Fannie Mae's 2021 annual report and page 66 in the Freddie Mac's 2021 annual report.
[3] In this paper, we use the term "investment home" and "investment property" interchangeably.
[4] See "Existing home sales in 2020 hit highest point since 2006, but listings are at a record low", *CNBC*, January 22, 2021.

the ultimate aim of moving them out of government conservatorship and back into private hands.[5] The policy was implemented by amending the Preferred Stock Purchase Agreement (PSPA), which we describe in detail below. The amendment capped the GSEs' ability to invest in both risky loans (limiting exposure to low FICO-score borrowers, high loan-to-value loans, and high debt-to-income borrowers) and loans backing non-primary-residence homes (i.e., mortgages for either second home or investment properties). We study the impact of these changes on credit supply, housing and rental markets, and local real economic outcomes. Our results imply that the GSEs provided subsidies to segments of the U.S. mortgage market, as we find large adjustments by lenders when they exit these segments.[6]

We first focus on the conforming mortgage markets, where the GSEs are active, to test whether the policy change worked as intended. In the analysis, we separate the conforming market into three non-overlapping segments: 1) second-home and investment-property mortgages (non-primary); 2) risky (but not second-home or investment-property) mortgages; and, 3) safe, primary mortgages. The first two segments were potentially affected by the policy. In some of our tests, we also sub-divide the first group into second-home vs. investment-property mortgages.

Compared to safe mortgages, lenders reduce non-primary mortgage sales to the GSEs during the period of policy implementation, but the policy has little impact on sales of primary and risky mortgages. This non-result squares with existing evidence (which we verify) that the limits on risky mortgage purchases in the PSPA were rarely binding prior to the pandemic (Golding et al., 2021); hence, this part of the policy had little bite. There is no evidence of pre-trends, although our ability to fully explore this standard diagnostic is limited because most of the pre-period is

[5] Both the Obama and Trump Administrations made efforts to re-privatize Fannie Mae and Freddie Mac. As of this writing, the two GSEs remain under government ownership and control.

[6] In this paper we use the word banks and lenders interchangeably to mean all lenders that participated in the U.S. mortgage and show up in the CHMDA data.

dramatically affected by the pandemic. The quantitative effects are very large, with sales of affected mortgages to the GSEs falling by about 20 percentage points (pp).

Despite this large overall drop, we show that within the sample of affected mortgages, lenders skew their sales toward ex-ante riskier ones after the policy caps go into effect. This adjustment partially offsets the larger aim of the policy change, which is to lower the risk-exposure of the GSEs. Based on our regression analysis, mortgages to borrowers with a FICO score that is one standard deviation below average are about 0.8 percentage point more likely to be sold after the policy, and mortgages with CLTV that is one standard above average are more than 3.8 percentage points more likely to be sold.

We then show that the price of credit – mortgage interest rates – increases by about 10 basis points for second-home and investment-property mortgages, consistent with a decline in supply from the policy. In contrast, risky primary mortgages, where the policy had little effect on GSE sales, do not experience an increase in rates during the period of policy implementation. We decompose these effects based on lender type (banks vs. non-banks) and by the type of affected mortgage (second-home vs. investment-property mortgage). Both types of affected mortgages respond similarly, i.e., large declines in the likelihood of sale to the GSEs and increases in interest rates. The policy's effect on non-bank sales to GSEs exceeded that of banks, who are better able to substitute financing from the GSEs with their substantial balance sheet capacity, as deposits grew sharply during the policy period. We find, however, that non-banks increase private securitization much more than banks, while interest rates on mortgages originated by non-banks adjust similarly to that of bank-originated mortgages.

Next, we explore how lenders adjust the quantity of credit supplied when the GSEs reduce their purchase activities. We find very large adjustments across multiple margins, which implies

an important subsidy has been removed by the policy. First, credit origination by lenders constrained by the caps declines by about 18 pp in the affected segment. Lenders more exposed to the policy – based on the pre-policy percentage of affected mortgages sold to the GSEs – experience relative declines in both affected mortgage originations and mortgage applications, and the decline has a similar magnitude for both second-home and investment-property mortgages. Since the decline in originations is similar in magnitude for applications, the effects on quantity are happening because lenders discourage some borrowers from applying, or simply stop serving the market (as opposed to adjusting acceptance rates). Consistent with a causal effect of the policy, lenders do not adjust the quantity of either safe or risky primary mortgages.

Importantly, we find *no* difference in the supply response of banks vs. non-banks. Both lender types change prices and quantities in response to the policy with similar magnitude. The similar decline, on its face, seems surprising because banks can replace GSE sales either by funding lending on balance sheet (with deposits) or by increasing their sales to non-GSEs, whereas non-banks have only the latter source of substitute financing. Our results imply that deposit-financing does not offer banks a comparative advantage in mortgage lending in the presence of a viable, private securitization option for non-banks. This result helps explain the dramatic increase in market share of non-bank mortgage lenders over the past three decades (Acharya et al., 2024; Buchak, et al., 2024).

Second, we test for local effects. We construct a three-way panel dataset at the lender-county-time level, which allows us to test how the allocation of mortgage credit *across* each banks' markets changes in response to the policy. By absorbing lender-quarter fixed effects, we remove all lender heterogeneity and thus focus solely on how different levels of pre-policy exposure change origination behavior. We show that lenders which have sold more affected loans from a

given county to the GSEs before the policy reallocate their originations away from those areas (relative to other areas served by that same lender). Since this analysis controls for lender-time effects, the results imply that they manage credit not only at the portfolio level but also market-by-market.

Third, we find evidence that the decline in credit supply constrains local housing activity, with large effects on the rental market. In markets more exposed to the policy, rents increase and the supply of rental housing decreases, suggesting that limited GSE activity and increased financing costs are passed onto renters. Given that more than one-third of housing units in the US are rental units, these results suggest that limiting GSEs' subsidy to non-primary mortgages may hurt non-homeowners as an unintended consequence. Consistent with this channel, the price of condominiums (condos) falls relative to that of single-family homes during the policy period. Condos are more likely to be owned and rented out compared to single-family homes and, therefore, are more exposed to the policy's negative credit supply shock. Consistent with this idea, we also show a sharp decline in condo transactions financed by mortgages in localities more exposed to the policy. While the policy is relatively short-lived, the effects that it has on the rental market could be extended for at least one year as the typical lease has a one-year term and rental prices can be sticky (Baker, 2024).

We also test for, but do not find, effects on real activities. Neither building permits nor construction activity declines after the policy. Because the policy was only in effect for several months, its impact on real outcomes may have been attenuated.

Our paper contributes to several strands of literature. First, we contribute to recent studies of the housing boom during the pandemic. Several papers (Brueckner, Kahn, and Lin, 2023; Howard, Liebersohn, and Ozimek, 2023; Davis, Ghent, and Gregory, 2024) have emphasized the

impact of increased demand for residential properties due to the popularity of working at home, which increased sharply in early 2020 and continues to remain elevated relative to pre-pandemic levels. Gupta et al. (2022) find a flatter pricing gradient between inner cities and outlying suburbs. Mondragon and Wieland (2022), using cross-sectional evidence, attribute about half of the pandemic house-price increases to higher demand from agents working at home. Guren (2022) agrees, arguing that the pandemic housing boom, unlike the run-up before the Global Financial Crisis, did not come from unrealistic pricing expectations but from increased demand running into supply constraints. He argues, for example, that housing speculation was much more prevalent during the early 2000s. Our analysis can help explain why, as the sharp rise in housing speculation ended with the implementation of GSE activity limits in this market.

Beyond the direct effect of the pandemic on demand, monetary interventions led to low interest rates in general, and the aggressive quantitative easing starting in March 2020 dramatically increased the demand for agency mortgage-backed securities (MBS) from the Federal Reserve. Fuster et al. (2021) use this period to illustrate how operational frictions can raise the wedge between funding costs (which the Federal Reserve can influence) and mortgage supply in the primary markets; they show that these frictions helped technology-based lenders gain market share. Our paper, in contrast to most of this literature, focuses on policies which restrained (rather than fueled) some of these forces by limiting GSE participation in non-primary segments of the mortgage markets.

Second, we provide further evidence that GSE activities subsidize mortgage credit. Early research showed that the enhanced liquidity provided by GSE securitization raised credit supply (Loutskina and Strahan, 2009; Loutskina, 2011), focusing on differences between non-jumbo and jumbo segments of the market. Using a similar approach, several papers focus on the jumbo/non-

jumbo spread differential, which has exhibited substantial variation over time (DeFusco and Paciorek, 2017; Alexandrov, Conkling, and Koulayev, 2022; Adelino, Schoar, and Severino, 2024). Some argued that the GSEs enhanced the supply of credit to sub-prime and Alt-A mortgage markets during the 2000-2006 boom by buying these high-risk mortgages and private-label mortgage-backed securities (Wallison and Calomiris, 2009). Scharfstein and Sunderam (2013) show that the level of pass-through of GSE funding subsidies to the primary mortgage market, however, can be stymied by lender market power, which increased temporarily after the Global Financial Crisis. Several papers (Jeske, Krueger, and Mitman, 2013; Elenev, Landvoigt, and Van Nieuwerburgh, 2016; Gete and Zecchetto, 2018) apply general equilibrium models to study the impact of GSE subsidies on credit supply and examine the distributional implications if these subsidies were removed. McKenzie (2002) and Richardson, Van Nieuwerburgh, and White (2017) provide survey of this literature. Our paper provides the most direct evidence that limits to GSE subsidies reduce mortgage credit supply, which in turn flows through to higher rent and lower condo prices.

Third, we are the first to explore the impact of a policy directed toward restraining speculation in the U.S. housing market. In our setting, the policy was motivated by concern about GSE risk but, as a consequence, removed an important subsidy to speculators in housing markets. Most existing studies of government policies have focused on markets in Asia (Fu, Qian, and Yeung, 2016; Agarwal, Badarinza, and Qian, 2018; Deng et al., 2019; Chi, LaPoint, and Lin, 2020; Agarwal et al., 2021; Deng et al., 2022). A number of papers that study the U.S. housing markets have emphasized the important role speculators play in both volume and house-price dynamics (Nathanson and Zwick, 2018; DeFusco, Nathanson and Zwick, 2022), as well as their role in

mispricing (Chinco and Mayer, 2016) and potentially destabilizing housing markets (Gao, Sockin and Xiong, 2020; I. García, 2022; Mian and Sufi, 2022).

Lastly, our paper contributes to the literature on housing affordability for renters by being the first paper to document that, at the margin, a decrease in the GSEs' subsidy (or rising financing costs) to the non-primary segment of the conforming mortgage market causes rents to rise and rental housing supply to fall. Housing affordability, especially for renters, is an increasingly important policy debate and area of academic research (Ghent and Leather, 2021; Favilukis et al., 2023).[7] Larrimore and Schuetz (2017) find that low-income families are overburdened by rent payments, which leave them vulnerable to negative idiosyncratic financial shocks that can cause them to be evicted and put into even worse economic conditions (Bartfeld and Dunifon, 2006). Been et al. (2019) argue that the housing affordability is caused by insufficient housing supply, which, for decades, has been restricted by local construction restrictions (Glaeser and Gyourko, 2002; Gyourko and Molloy, 2015). Our results imply that any policy that reduces the GSEs' subsidy to the non-primary segment of the conforming mortgage market (e.g., increasing guarantee fees or privatization) will worsen affordability for renters.

2. SETTING

The Preferred Stock Purchase Agreement (PSPA) was created when Fannie Mae and Freddie Mac were taken into government conservatorship during the 2008 Global Financial Crisis. This agreement outlines the government's commitment to and governance of the two GSEs and grants the government control of ownership (79.9% of common equity). The PSPA was amended several times after 2008 to increase the GSEs' capital buffers, which act to protect taxpayers from

[7] See the Biden-Harris Administration's policy announcements for this particular issue at "Biden announces plan to cap rent hikes," *BBC*, 16 July 2024 and "Delivering Emergency Price Relief for American Families and Defeating the Cost-of-Living Crisis," *The White House*, January 20, 2025.

losses. Initially, these amendments focused on capital buffers by limiting dividends and other distributions to bolster reserves. On January 14, 2021, the U.S. Treasury together with the Federal Housing Finance Agency (FHFA), announced additional changes to the PSPA to further strengthen GSE capitalization, along with other changes aimed at limiting their risk exposure.

We focus on the 2021 amendment because it went further than earlier adjustments by restricting GSE activities in their core business of acquiring mortgages from private lenders. We focus on Purchase Caps affecting the following: limiting GSE acquisition of risky home purchase single-family mortgages (primary) to 6% and risky refinance mortgages to 3% of single-family mortgage acquisitions, based on a trailing 52-week period. Risky mortgages are those with two or more of: combined loan-to-value (CLTV) greater than 90%; debt-to-income ratio greater than 45%; and credit score less than 680. In addition, the amendment limits the GSE acquisition of mortgages secured by second homes or investment properties (non-primary) to 7% of single-family mortgage acquisitions over the preceding 52-week period.[8] The Purchase Cap policy was announced on January 14, 2021 and went into effect on April 1, 2021, but was suspended in September 2021.[9] We confirmed with GSE insiders that the purchase cap is implemented on a lender-by-lender basis. In January 2022, the FHFA announced that higher guarantee fees (for example, roughly tripling fees for second-home mortgages) for the affected mortgages would be used to limit risk rather than imposing hard constraints; this change effectively pushed in the same direction as the original policy, but with somewhat weaker effects.[10]

[8] See "Treasury Department and FHFA Amend Terms of Preferred Stock Purchase Agreements for Fannie Mae and Freddie Mac", *U.S. Department of the Treasury*, January 14, 2021.
[9] See Fannie Mae Lender Letter (LL-2021-08) and "FHFA and Treasury Suspending Certain Portions of the 2021 Preferred Stock Purchase Agreements", *Federal Housing Finance Agency*, September 14, 2021.
[10] See "Fannie Mae and Freddie Mac Single-Family Guarantee Fees in 2022", *Federal Housing Finance Agency*, May, 2024.

Given this history, we focus on the five quarters which surround the initial amendment to the PSPA, running from Q3, 2020 to Q3, 2021. The last two quarters of 2020 represent the pre-policy period; the first quarter of 2021 represents the announcement period; and the second and third quarters of 2021 represent the implementation/treatment period. For most of our analysis, we do not include data before Q3, 2020 because we want to avoid contaminating the results with the large effects of the COVID-19 pandemic on both the housing markets and the broader economy. We drop the quarters after Q3, 2021 because the policy was first suspended and then reintroduced in a weaker form, making it hard to infer the original policy's effects.

Figure 2 reports aggregate time series patterns in mortgage lending around the policy announcement and implementation dates. Panel A reports the share of GSE mortgage purchases in each of the three categories affected by the policy. The share of second-home and investment-property mortgages exceeded the 7% limit during most quarters leading up to the policy. In contrast, during the pre-policy period, the share of risky mortgages – both for purchase and refinance – consistently remained well below the limit imposed by the policy (6% for purchase mortgages and 3% for refinance). As we will show, the policy likely had large effects on credit in the non-primary market but limited effects on primary, risky mortgages. We thus begin our formal empirical tests comparing the effect of both policies on credit supply. In later tests, when we consider second-order effects, we focus only on the non-primary markets.

Figure 2, Panel B reports the fraction of non-primary and risky loans sold to the GSEs over time. Consistent with Panel A, *only* non-primary mortgages decline during the treatment period. For them, we see a drop of about 40 percent, in relative terms, during the policy period. Both series exhibit sharp increases in the pre-policy period, which we attribute to the effects of the pandemic on housing markets, generally. Despite the large decline in sales of non-primary

mortgages to the GSEs, the fraction of non-primary mortgages held by originating lenders increases sharply (Panel C). This means that the decline in originations is substantially *smaller* than the decline in GSE sales of those mortgages. Thus, these mortgages take up more balance sheet capacity after the policy, which otherwise could have been used for other kinds of loans.

Our empirical design exploits the time series variation in the GSE Purchase Caps interacted with cross-sectional heterogeneity in exposure to the policy, as different localities have differential demands for non-primary and risky credit. For example, areas with high levels of vacation properties are likely to attract non-primary capital in the housing markets. Figure 3 reports a heat map of this heterogeneity, for both, at the county level, as well as at the census-tract level for three U.S. metropolitan areas (Boston, New York, and Chicago). These measures are built from 2020 data, which reflect activity prior to the policy's announcement.

3. EMPIRICAL METHODS AND RESULTS

We report tests at the loan level, the lender level, the lender-market level, and the market level. First, using loan-level data, we estimate the effect of the Purchase Caps on sales to the GSEs and interest rates. Second, we aggregate to the lender level and test how the policy affects origination and application volumes. These two tests establish how the policy affects credit supply in the conforming mortgage markets. Third, we expand the panel to the lender-county-time level to test for local effects; this empirical strategy allows us to fully remove both lender-time and county-time effects, thus focusing on the reallocation of credit across markets. Fourth, we consider how the policy's effect on credit supply affects housing markets and local economies, with data measured at the zip code or county level.

Information on mortgages comes from the Home Mortgages Disclosure Act (HMDA) data, and we have access to the confidential version (CHMDA) which, among other things, allows us to

observe each loan application's submission and decision dates. We use data from CoreLogic in some of our zip-level tests because these data allow us to capture condominium transactions.[11] We utilize Zillow's zip-code-level monthly rent price data and the American Community Survey's (ACS) zip-code-level annual data on rental-occupied units to analyze the rental supply. Additionally, we leverage CoreLogic data to examine housing prices and transactions. Finally, for tests on real effects, we use the U.S. Census Bureau Building Permits Survey to obtain county-level construction permits and the Quarterly Census of Employment and Wages data from the Bureau of Labor Statistics to obtain the county-level employment information of construction workers.

Table 1 reports summary statistics, for our samples at the different levels of aggregation. Panel A reports loan-level statistics; Panel B reports statistics at the lender-year-quarter level; Panel C reports statistics at the lender-county-year-quarter level; Panel D reports statistics on Zillow data on monthly rents and Panel E provides lower-frequency rental supply statistics from the ACS data; Panels F and G report statistics on CoreLogic data, and Panel H reports statistics on the real effects variables.

3.1 GSE Purchase Cap Policy and Conforming-Mortgage Lending

For the loan-level analysis, we follow several papers in the mortgage lending literature (Bhutta et al., 2020; Bhutta et al., 2022; Bartlett et al., 2022; Amornsiripanitch, 2023) and construct our sample from first-lien home purchase and refinance, single-family, 30-year fixed-rate conventional conforming mortgages. To ensure that we only include conforming mortgages, we follow the GSE seller's guide and loan-level price adjustment (LLPA) documents, and we drop

[11] The GSEs require borrowers to be natural persons. Exceptions include (1) inter vivos revocable trusts, (2) HomeStyle Renovation mortgages, and (3) land trusts in those states where the beneficiary is an individual. See Fannie Mae's Seller Guide.

mortgage applications where the main borrower's credit score is lower than 620, the loan amount exceeds the conforming loan limit, and at least one automated underwriting system (AUS) flagged the mortgage as being ineligible for GSE purchase.[12] We also exclude government-guaranteed mortgages such as VA, FHA, and Farmer Mac mortgages. To make the mortgages more comparable across treatment groups, we drop mortgages with balloon payments, interest-only payments, negative amortization or other non-amortizing features, or prepayment penalties. We keep mortgage applications that were originated between 2020Q3 and 2021Q3, inclusive. The filters leave us with approximately 6.8 million mortgages.[13]

We divide our sample into three segments. Non-primary mortgages are those backed by second homes or investment properties. Risky mortgages are those that are backed by the borrower's primary residences but meet at least two of the following "risky" criteria: (i) cumulative loan-to-value (CLTV) ratio above 90%; (ii) debt-to-income (DTI) ratio above 45%; and (iii) credit score lower than 680. The rest we refer to as "safe" mortgages.

To examine whether the GSE Purchase Cap Policy affects lenders' mortgage activities, we run variants of the following loan-level regression:

$$y_i = \delta_k + \gamma_{j,t} + \beta_1 Treated_i + \beta_2 Treated_i \times Announcement_t \qquad Eq.\ 1$$
$$+ \beta_3 Treated_i \times Implementation_t + Control\ Variables + \epsilon_i,$$

where i refers to loan, j refers to lender, k refers to census tract, and t refers to year-quarter. Equation 1 looks on its face like a panel regression, but it is not. We are not following the same loan over time. But we still need to control for local heterogeneity and lender-specific time trends,

[12] See Fannie Mae's Selling Guide and its Loan-Level Price Adjustment (LLPA) matrix.
[13] The special feature filters dropped approximately 10% of all conforming conventional 30-year fixed rate mortgages.

which we do with δ_k and $\gamma_{j,t}$ (fixed effects). These effects absorb the direct effect of both *Announcement* and *Implementation*.

The dependent variable in equation 1 is an indicator set to 100 for originated mortgages sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year, or the loan interest rate (expressed in basis points). The coefficients labeled δ_k and $\gamma_{j,t}$ indicate tract and lender by year-quarter fixed effects. For GSE sales, we estimate the equation using the linear probability model, given the large number of fixed effects. $Treated_i$ equals one if mortgage application i is associated with second homes or investment properties and zero otherwise; alternatively, $Treated_i$ equals one if mortgage application i is backed by a primary residence property but meets at least two risk metrics defined above. In some columns, we also report models without lender or lender-year-quarter fixed effects. Control (i.e., safe) mortgages are those associated with conforming mortgages that do not qualify for either treated group. $Announcement_t$ equals one for 2021Q1 and zero otherwise; $Implementation_t$ equals one for 2021Q2 and 2021Q3 and zero otherwise.

We also include the following loan-level controls: an indicator for refinancings, borrower characteristics (borrower's age (Amornsiripanitch, 2023), gender, race, ethnicity, credit score, income, and debt-to-income (DTI)) and loan characteristics (loan amount, cumulative loan-to-value (CLTV), whether the application was approved by the AUS, and the number of borrowers). The details on the control variables are described in the Internet Appendix. For brevity, we do not report the coefficients on these variables. Tract, year-quarter, and lender-by-year-quarter fixed effects are included in the regressions, as indicated in the tables. Heteroskedasticity-robust standard errors are clustered at the lender level.

Recall that Figure 2 suggests that the Purchase Caps bind for non-primary mortgages but not risky mortgages. Table 2 supports this claim, after absorbing the fixed effects and control variables. Panel A presents the regression results for GSE sale probability, comparing non-primary to safe primary mortgages. In all three columns, the coefficient estimates for *Non-primary x Announcement* and *Non-primary x Implementation* are negative and statistically significant. After the policy was announced, second-home and investment-property mortgages are less likely to be sold to the GSEs, compared to safe mortgages for primary residences. The negative effect mostly concentrates on the implementation period (i.e., 2021Q2 to 2021Q3). Economically, the probability of second-home and investment-property mortgages sold to the GSEs is about 23 percentage points lower than the probability of safe primary mortgages sold to the GSEs after the policy takes effect. This represents a reduction of 35% when compared to the average sales probability of 66%.

Panel B of Table 2 presents the regression results for GSE sale probability of risky relative to safe mortgages. Unlike the results reported in Panel A, the coefficient estimates for *Risky x Announcement* and *Risky x Implementation* are small and mostly statistically insignificant. Consistent with the aggregate trends (Figure 2), which show that the fractions of risky purchase loans and refinance mortgages are below the caps before the policy, the policy does not significantly affect the probability of risky mortgages being sold to the GSEs.

Table 3 tests whether the change in selling behavior varies with mortgage risk. We introduce interaction effects between the policy-cap indicators and three standard risk metrics: 1) the borrower's credit score divided by 100 (FICO); 2) CLTV ratio; and 3) DTI ratio. Two of these interact significantly with the post-implementation indicator. A one-standard-deviation decrease in credit score (=~ 42 points, or 0.42 in the regression) lowers the effect of the policy cap on sales

by about 0.8 percentage points (=~ -0.42 x -1.84). The effect of CLTV is larger: a one-standard-deviation increase (=0.169) reduces the policy cap's effect on sales by about 3.8 percentage points (=~ 0.169 x 22.65).[14] In other words, the distribution of loans sold after the policy skews toward those with greater credit risk. Thus, lenders offset some of the policy's intended effects, which were to reduce GSE risk exposure. The same results hold whether or not we include LLPA grid by year-quarter fixed effects to control for lenders' incentive to sell loans with respect to the GSEs' upfront guarantee fee schedule (Bartlett et al., 2022). In un-tabulated results, we have also considered whether ex-post risk (i.e., defaults) of sold loans increases after the policy but find little statistically significant results due to the tests' limited statistical power.

Table 4 presents the regression results for the interest rate on originated mortgages. Interest rates increase for non-primary mortgages during the policy period – by 10-11 basis points – but change little for risky mortgages once we absorb the full set of fixed effects. The rate increase is modest, representing approximately a 3% increase compared to the average interest rate 302 basis points.

To help establish causality, Figure 4 plots dynamic coefficient estimates (β_t) from the following version of the loan-level regressions above:

$$y_i = \delta_k + \gamma_{j,t} + \beta_0 Treated_i + \sum_t \beta_t Month_t \times Treated_{i_i} + Control\ Variables + \epsilon_i, \qquad \textit{Eq. 2}$$

where *Month*$_t$ equals one for year-month t and zero otherwise; δ_k and $\gamma_{j,t}$ indicate tract and lender by year-month fixed effects. The β_t coefficients are month-specific difference-in-differences (DiD) coefficients (with July 2020 acting as the reference month). As in the earlier models, we estimate

[14] See Appendix Table IA.1 for summary statistics on loan characteristics.

equation 2 with just non-primary and safe loans. $Treated_i$ equals one if mortgage loan i is associated with non-primary properties and zero otherwise. Panel A of Figure 4 plots coefficient estimates (β_t) for GSE sale probability. It shows that the significant effects begin at the announcement (not before) and then grow in magnitude after implementation; since we find no effects before the announcement, our results are likely causal. Panel B plots coefficient estimates (β_t) for loan interest rates; here, rates show a small pre-trend before the policy, then rise sharply after implementation.

Tables 5 and 6 show similar directional effects of the policy, regardless of the sub-segment of non-primary mortgages (second-home vs. investment properties) and lender type (bank vs. non-bank).

For second-home mortgages, the probability of sale to the GSEs falls by about 9 percentage points, while for investment properties, the probability falls by 32 percentage points (Table 5, column 1). The effect on GSE sales – the direct effect of the policy – exhibits no pre-trend in either sub-segment (Figures 5 and 6, Panel A). Interest rates increase by 12.5 basis points for second-home mortgages (with no pre-trend, as shown in Figure 5, Panel B) and by 4.5 basis points for investment-property mortgages. This latter result, however, is likely *understated*, as Figure 6, Panel B shows a significant pre-trend in the interest rate for the investment property sub-segment. That is, interest rates on investment properties relative to safe mortgages began to fall before the policy was announced. If we compare the difference between investment-property mortgages and the control group (safe ones) after this trend bottoms out (January 2020), the increase during the implementation period is about 20 basis points.

Table 6 extends the base specification by adding interaction terms based on lender type. Here, we find *no differential policy effect* on interest rates between banks and non-banks; this is

true for second-home and investment-property mortgages. For investment homes, sales to GSEs do drop more for non-banks than for banks. To understand the selling behavior better by lender type, Table 7 estimates the probability that a loan remains unsold (column 1), and the probability that a loan is sold to a non-GSE entity (column 2), again comparing banks with non-banks. Here we see sharp differences. Banks increase their holdings of both segments of non-primary mortgages – they use their access to deposits to fund these loans on balance sheet; non-banks, in contrast, exhibit weak declines in on-balance sheet finance (only significant in the second-home segment). But unlike banks, non-banks become much more likely to sell non-primary mortgages to non-GSE buyers when the policy caps are in effect. The magnitude is especially large in the investment segment (Table 7, Panel B). To summarize: both banks and non-banks sharply reduce their sales to GSEs; banks substitute heavily into on-balance sheet financing (with some additional sales to non-GSEs), while non-banks substitute *only* by selling more non-primary mortgages to non-GSEs.

Tables 4 through 7 provide some evidence that reduced access to GSEs lowers credit supply for non-primary mortgages, as their interest rates rise sharply regardless of originator type. These tests are incomplete, however, because they take the flow of applications as given. Next, we relax this assumption and analyze how the GSE purchase caps affect mortgage volumes.

3.2 Purchase Caps and Mortgage Volume

We construct a lender-quarter panel dataset, starting with all conforming conventional home purchase and refinance mortgage applications that were approved or denied between 2020Q3 and 2021Q3. We sum across loan amounts to compute the total quantity of credit for each lender-year-quarter cell. Loans are assigned to quarters based on the date on which the lending decision was made. It is important to note that the sample of loan applications that were used in

the aggregation process includes loans of all maturities and does not exclude loans with uncommon features such as balloon payments, interest-only payments, negative amortization features, other non-amortizing features, and prepayment penalties. The sample still excludes government-guaranteed and Farmer Mac mortgages. We relax the data filters here because we are interested in studying the total credit supply of conventional conforming home purchase and refinance mortgages that were provided in the sample period.

We report regressions analogous to equation 1 above, although now the models are true panels. The dependent variable equals either the log of the total originations or total applications for lender j in year-quarter t. Each model is reported separately for quantities based on whether or not a loan (or a loan application) is part of a treated group (non-primary mortgages or risky mortgages) or the control group (safe mortgages). In this framework, we focus on the impact of $Treatment_j$, an indicator set to one if more than 7% of a lender's mortgages sold to GSEs are non-primary (second-home or investment-property) in 2020 (i.e., before the policy announcement).[15] As in the loan-level regressions, $Announcement_t$ equals one for 2021Q1 and zero otherwise; $Implementation_t$ equals one for 2021Q2 and 2021Q3 and zero otherwise. We construct similar treatment-exposure indicators for the risky mortgage caps, but since these caps had little effect at the loan level, we include them as control variables and do not report their coefficients. We also include lender and year-quarter fixed effects. Heteroskedasticity-robust standard errors are clustered at the lender level.

Tables 8 and 9 present the volume results. For non-primary mortgages, the coefficient estimates for *Treatment x Implementation* are negative and statistically significant (Table 8, column 1). The effect is quantitatively large, with treated lenders cutting credit by about 18%

[15] The GSEs enforced the policy caps lender-by-lender, so only those lenders who had sold more than 7% prior to the policy would have faced a new constraint.

overall. Consistent with the loan-level pricing results, which suggest a negative supply effect, non-primary mortgage quantities fall more at lenders constrained by the GSE Purchase Cap policy. We see similar magnitudes for the two segments of the affected mortgages, second-home and investment-property mortgages. And magnitudes are similar for originations and applications (compare Panels A and B), suggesting that the decline in credit supply occurs through lower applications rather than higher rejection rates. Lower application volumes may be due both to higher mortgage interest rates, as shown above, and because lenders reduce marketing in the affected segments.

The results for both safe and risky mortgages are small and generally not statistically significant (Table 8, columns 3 and 4). Along with the pricing results, this non-result helps establish that the drop in non-primary credit reflects lower credit supply, rather than some omitted factor correlated with treatment exposure. We could think of the effect on risky and safe mortgage volumes as 'placebo' tests. Figures IA.1 and IA.2 plot the respective coefficients in a dynamic DiD framework, which shows no evidence of pre-trends in the volume data.

Table 9 reports an augmented model with lender-type interaction effects. As in Table 6, where non-bank originated non-primary mortgages exhibit the *same* price change from the policy, volumes also respond similarly regardless of lender type. That is, the triple interaction effects are never statistically significant. The policy elicits a similar supply response from both lender types, despite the fact that *only* banks have the option to replace GSE sales with on-balance sheet finance using deposits. But we showed earlier that non-banks dramatically expand their use of mortgage sales to non-GSEs during the policy period (recall Table 7). The results imply that deposit-financing does not offer banks a comparative advantage in mortgage lending in the presence of an

active, private securitization option for non-banks. This result helps explain the large increase in market share of non-bank mortgage lenders (Buchak, et al., 2024).

To summarize: the GSE purchase caps bind strongly on non-primary mortgage activity, leading to credit contraction in that segment, as evidenced by higher interest rates and lower application and origination volumes for affected loans and lenders. In contrast, although the caps nominally limit other classes of risky loans, these caps do not bind. As a result, we see no change in GSE sales of risky loans and no evidence of changes in credit supplied to risky borrowers.

3.3 Local Effects

The policy shock presents the opportunity to study whether lenders manage their credit in response to shocks at the local level. As shown in Table 8, lenders restrict credit in the non-primary lending market at the portfolio level when GSE caps bind. In this section, we strip out any effect on credit at the portfolio level to ask: do lenders reallocate credit across markets (i.e., away from more exposed and toward less exposed ones)? To isolate the local channel, rather than estimate the overall effects at the loan or portfolio level, we test for effects *within* a given lender during a given period and compare how variation in exposure to the policy cap across individual markets affects lending in those localities. Thus, our measure of treatment (which now varies at the lender-market level) is set to one if the fraction of non-primary loans of a given lender sold to the GSEs exceeds 7% during the pre-period. This approach allows us to absorb potentially confounding effects at both the market-level and the lender-level (since we can construct a three-dimensional panel and absorb confounds with granular fixed effects). To do so, we focus on originations of non-primary mortgages as well as originations of risky and safe primary mortgages (as placebo tests).

With these data, we estimate panel models as follows:

$$ln(Mortgage\ Originations)_{j,c,t} = \gamma_{j,t} + \mu_{c,t} + \eta_{c,j} + \beta_1 Announcement_t \times Treatment_{c,j} + \beta_2 Implementation_t \times Treatment_{c,j} + Control\ Variables + \varepsilon_{j,c,t}. \quad Eq.\ 3$$

The panel varies across lenders (*j*), counties (*c*), and time (*t*), where time is measured quarterly for mortgage originations. *Announcement* and *Implementation* are defined as above. We estimate the regressions separately for the second-home and investment-property mortgages, and we also report models with and without branch interaction terms to test whether the policy effects differ by a lender's physical presence in the market.[16]

Since these models are estimated with lender-time fixed effects (due to the inclusion of $\gamma_{j,t}$), we are comparing how varying exposure to the policy cap across different markets correlates with lending decisions. Identification comes from reallocation across markets because the $\gamma_{j,t}$ effects fully control for portfolio-level variation in potentially confounding variables. Hence, we do not need to worry about heterogeneity at the lender level. We also remove variation in local credit demand by absorbing county-time fixed effects ($\mu_{c,t}$), as well as variation in a bank's overall importance to a county ($\eta_{c,j}$).[17] We build $Treatment_{c,j}$ to capture the market-by-market exposure to the policy caps on non-primary mortgages based whether or not lender *j*'s ratio of its 2020 non-primary loans sold to the GSEs in market *c* exceeds the 7% cap. Since we include both lender-time fixed effects and lender-geography fixed effects, reasonable control variables that we could potentially add to the regression are absorbed. Our coefficients of interest, β_1 and β_2, test whether lenders move funds away from more-affected markets due to the policy.

The results in Table 10 provide strong evidence that lenders reduce their exposure to both types of non-primary mortgages. The effect is slightly larger at the local level than at the portfolio

[16] We utilize the FDIC Summary of Deposits data to determine if a bank has a branch presence in a specific county.
[17] The results are similar when we exclude bank by county fixed effects.

level (22% vs. 18% decline), perhaps because some lenders increase non-primary mortgage provision in localities below the cap, as well as decreasing them in areas above the cap. (The difference-in-differences empirical design can only identify a relative comparison across more vs. less treated markets.) Moreover, the coefficients suggest that local reallocation begins to occur at the announcement of the policy, while the overall portfolio effects only emerge after implementation. The timing difference may be driven by the lender-level aggregation across markets where the cap binds and those where it does not. In the subset of data with just bank lenders, we find little impact of whether or not the bank has one or more branches in the local area (columns 3 and 6). Note that the economic magnitude of the policy effect is very similar in the bank subset, consistent with our earlier findings that banks and non-banks respond similarly to the policy.[18]

These results suggest that lenders respond to the policy caps by reducing lending in the more-exposed markets. This decline in credit supply (decline in quantity and increase in price) for non-primary mortgages should have its largest effects on properties which serve the rental market. Higher financing costs for the rental market ought to increase rents (by reducing the supply of rental properties) but also could lower the value of properties most likely to serve the rental market (i.e., condominiums and apartments). We turn to this implication next.

3.4 GSE Purchase Cap Policy, Rental Prices, Property Prices, and Real Effects

In this section we consider the broader potential local effects of the purchase caps on both real estate prices (single-family homes and condos) and residential construction.

We start by focusing on rents, which are affected by the policy because many second homes and investment properties financed by GSEs end up in the rental market (since they are not by

[18] Figure IA.3 presents dynamic difference-in-differences regression results to support the causal interpretation as there is no visible pre-tend in lending activity.

definition occupied by the owner). Rents are available by zip code and month from the Zillow Observed Rent Index.[19] Because the credit supply to non-primary mortgages falls due to the policy, we expect rents to rise but the price of properties to fall. In other words, higher financing costs for rental properties ought to raise the rent-to-price ratio.

To capture property prices, we exploit transaction-level data, which allows us to compare the policy's effect on single-family houses vs. condos; the latter group is more exposed to the policy because condos are more likely to be financed with non-primary mortgages.[20] As such, single-family houses act as a natural comparison group whose access to the non-primary mortgage market matters less than for condos. To further validate the financing channel, we then test whether property transactions are less likely to be financed with mortgages (as opposed to cash) during the period of policy implementation, again comparing the policy's effects on single-family houses vs. condos. Both transaction prices and financing status are constructed from CoreLogic. In the latter case, we aggregate up the zip-code-by-month level.

We report regressions on rents and mortgage financing at the zip-code-by-month level by estimating the following panel regressions:

$$Y_{j,t} = \gamma_j + \eta_t + \beta_1 Announcement_t \times Treatment_j + \beta_2 Implementation_t \times Treatment_j + Control\ Variables + \varepsilon_{j,t}, \quad \textit{Eq. 4}$$

where *j* represents zip codes and *t* represents months. We report outcomes on both the log level of rents, and the fraction of transactions financed with a mortgage. Because equation 4 represents outcomes at the market level, we focus on *Treatment* to measure how exposed a given market (zip

[19] The Zillow Observed Rent Index (ZORI) is constructed using the repeated sales approach applied to rental asking prices. For details, see Methodology: Zillow Observed Rent Index (ZORI).
[20] According to CoreLogic data, since 2010, 34% of purchased condos have been non-owner-occupied. This proportion is more than 60% higher than that of single-family homes.

code) is to the policy change. This variable equals the fraction of non-primary mortgages sold to the GSEs in the pre-period. To construct this measure, we start with the *Treatment* fraction calculated at the census-tract level (which is how CHMDA identifies property locations) and then aggregate up to the zip-code level as a weighted sum using the zip-census tract crosswalk data from the United States Department of Housing and Urban Development (HUD).[21]

To analyze property transaction prices, we estimate how condo prices respond to the policy relative to single-family homes, using regression with the following structure:

$$\begin{aligned} ln(Property\ Price)_i = {} & \eta_{j,t} + \beta_1 Announcement_t \times Condo_i \\ & + \beta_2 Implementation_t \times Condo_i \\ & + Control\ Variables + \varepsilon_i , \end{aligned} \qquad Eq.\ 5$$

Unlike equation 4, equation 5 is *not* a panel model, as *i* represents each property transaction which is observed just once; the subscripts *j* and *t* refer to zip-code and month. Time variation in local prices is fully absorbed by the $\eta_{j,t}$ fixed effects. $Condo_i$ is a dummy variable that equals to one if the property in the transaction is a condo or an apartment and zero if it is a single-family home.

The coefficients of interest (β_1 and β_2) represent the differential impact of the policy on condos compared to single-family homes. The approach identifies only the relative price effect of the policy on condos compared to single-family homes because the direct impact on prices overall gets absorbed by the zip code by year-month fixed effects. While this empirical design limits what we can identify (because we have no interpretation for time variation in the overall level of single-family home prices), it successfully eliminates confounding effects that would otherwise result in pre-policy trends. Equation 5 is conceptually analogous to the loan-level regressions (equation 1).

[21] See "HUD-USPS ZIP Code Crosswalk files."

As such, we also include a large set of control variables to capture the amenities of different property features.

Table 11 reports estimates of equation 4 using rental price as the outcome. We report both models with year-month and zip code fixed effects, and either with or without control variables for exposure to COVID-19. The COVID-19 controls include the total number of cases and the total number of deaths by county-month. In one of the specifications (Column 3), we allow the impact of the COVID-19 controls to have time-varying coefficients. The base model suggests that rents rise by about 1% more in zip codes which are one-sigma above average in *Treatment*, compared to the typical market (=0.08 x (0.064+0.038)). This effect is consistent across both specifications. Moreover, Appendix Figure IA.4 shows no statistically significant pre-policy effect of *Treatment* on rents. After treatment, the policy effect grows slightly over time, although the increase is not statistically significant.

Additionally, we use the American Community Survey (ACS) data to examine the effect of the policy on rental supply. Our sample comprises ACS data from 2020 and 2021, with 2021 designated as the implementation year. Table 12 reports the regression results. The results indicate that in areas more exposed to the policy, the supply of rental units decreases. Specifically, a one standard deviation increase in treatment is associated with a reduction in the supply of rental units by about 1% (=0.16 x 0.053). Although these results should be interpreted with caution since the ACS data is available only at an annual frequency, the evidence is consistent with the notion that higher financing costs reduce the supply of rental units in the market. These results suggest that limiting GSEs' subsidy to non-primary may hurt non-homeowners as an unintended consequence.

Table 13 reports estimates of variants of equation 5, with varying sets of property characteristic control variables, as well as variation in the set of markets. Property characteristics

include an indicator for condos, the number of bedrooms, the number of bathrooms, the year built, and the total square feet of the property. The odd-numbered columns include just the property characteristics, and the even-numbered ones introduce interactions between those and the year-month effects, with the condo indicator variable being the exception. Also, because many zip codes include very few condo transactions, we report estimates from regressions that include only zip codes that have at least ten transactions per month. These permutations lead to a similar conclusion, which is that condo prices decline by about 2% relative to single-family homes due to the policy constraining non-primary mortgage credit. As shown in Figure IA.5, we find no significant pre-trend in the dynamic difference-in-differences version of the regression.

Table 14 reports a second set of zip-level outcomes, where the dependent variable equals the fraction of transactions financed with a mortgage, going back to the panel data structure of equation 4. Because condos are more exposed to the policy than single-family homes, we report the effects separately for the two segments in columns 1 and 2, and then we report the difference in the mortgage share between the two segments in column 3. In all tests, we only include zip-month observations that contain at least ten transactions. The last three columns interact the COVID-19 controls with the time effects. Consistent with the decline in credit supply raising rents and lowering property prices, we find a relative decline in mortgaged transactions. In both segments, we find that, in more exposed zip codes, a smaller fraction of transactions is financed with mortgages after the policy. As expected, the decline is larger in the condo segment, and the difference between the two is statistically significant at the 10% level (e.g., column 3). Figure IA.6 reports the dynamic versions of these models. These figures suggest a slight pre-trend for the single-family home segment, but this effect is eliminated when we estimate the differential effect

on financing, as in column 3. This latter approach differences out any zip-month common shock in financing and helps eliminate any unobservable but possibly confounding variation.

Table 15 tests for real effects. In particular, we test whether activity in the home building industry falls in localities that are more affected by the policy caps. We explore three outcomes: wage and employment in the construction industry and building permits for one-unit homes. We keep the same five quarters as our baseline regression, 2020Q3 to 2021Q3. Policy effective period is a dummy variable set to one for 2021Q2 to 2021Q3. The *Treatment* variable is computed using the same method as our treatment calculation at the zip level but applied to the county level instead. We find no evidence that real economic activity declined due to the policy, although these tests are likely under-powered because we can only measure outcomes by county, which washes out a substantial portion of the variation in treatment exposure.

4. CONCLUSION

This paper shows that when the GSEs reduced their willingness to buy non-primary mortgages in the wake of the COVID-19 pandemic, credit supply declined in the affected markets, as expected. The decline in credit led to higher rents, lower supply of rental properties, and lower property values, as fewer non-owner-occupied properties were financed with access to GSE subsidies. Lenders reallocate credit for non-primary mortgages at the portfolio level, and they also tilt their lending away from markets where they are most exposed to the policy. Our local empirical design sweeps out portfolio-level effects with granular lender-time fixed effects. As such, the evidence suggests that lenders manage credit not only in a macro sense, which has been the main focus of the banking literature, but also market-by-market. The supply response of banks vs. non-banks exhibits no difference, suggesting that deposits do not provide banks with any comparative

advantage over non-banks in mortgage lending, as non-banks are able to aggressively substitute into the non-GSE securitization market.

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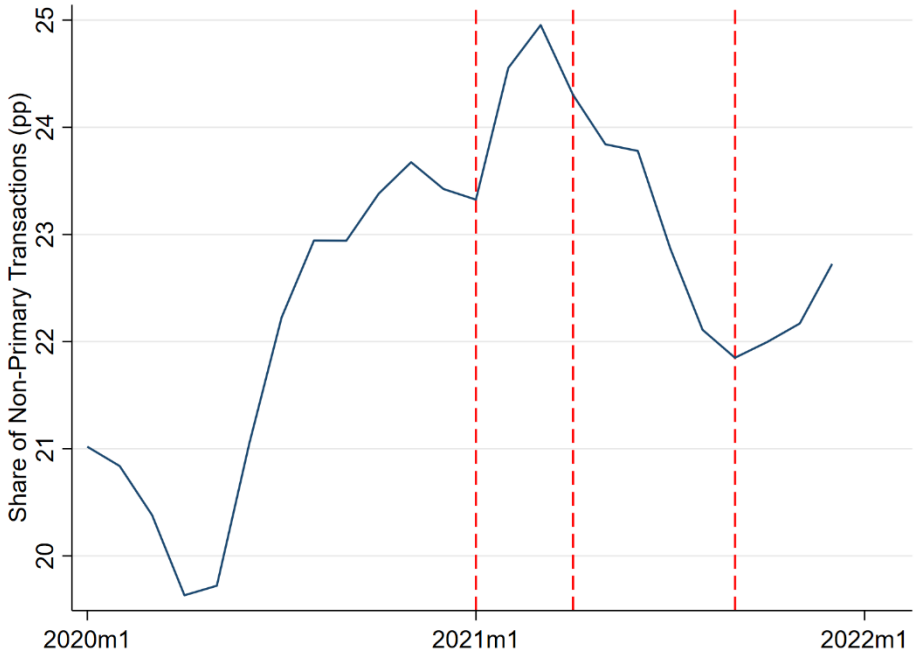
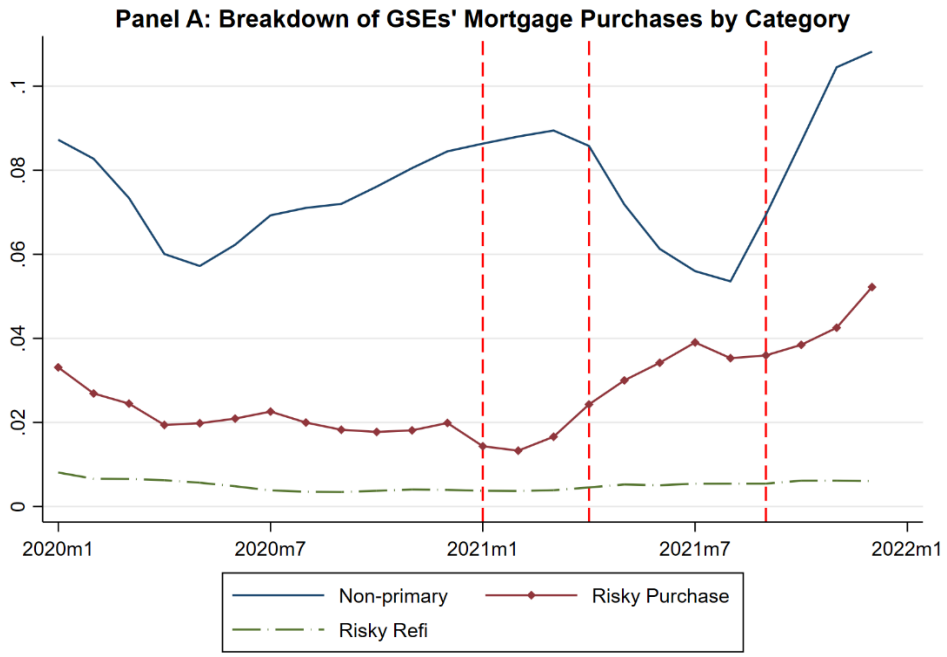
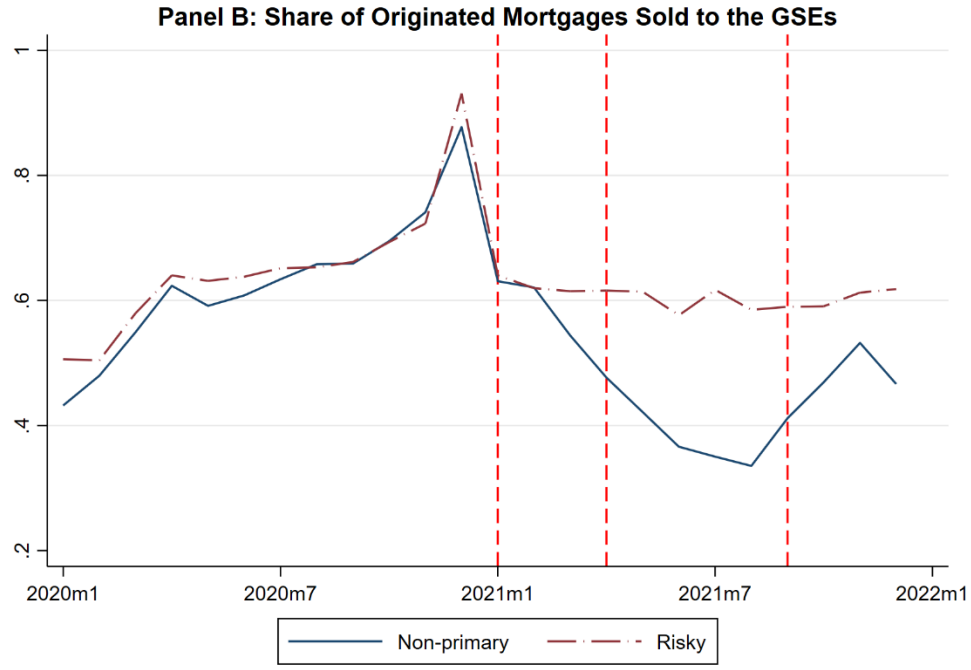


Figure 1: Nationwide Fractions of Non-Primary Transactions

Figure 1 presents the nationwide seasonality-adjusted trends of non-primary transactions, spanning 2020 to 2021. Non-primary transactions refer to the purchase of non-owner-occupied houses. To account for seasonal variations in the real estate market, we adjusted the data using monthly seasonality factors calculated 24 months prior. The three vertical lines indicate the announcement, implementation, and termination of the GSE Purchase Cap policy, respectively. Data source: CoreLogic.


Panel A: Breakdown of GSEs' Mortgage Purchases by Category
0
.02
.04
.06
.08
.1
2020m1
2020m7
2021m1
2021m7
2022m1
Non-primary
Risky Purchase
Risky Refi


Panel B: Share of Originated Mortgages Sold to the GSEs
.2
.4
.6
.8
1
2020m1
2020m7
2021m1
2021m7
2022m1
Non-primary
Risky

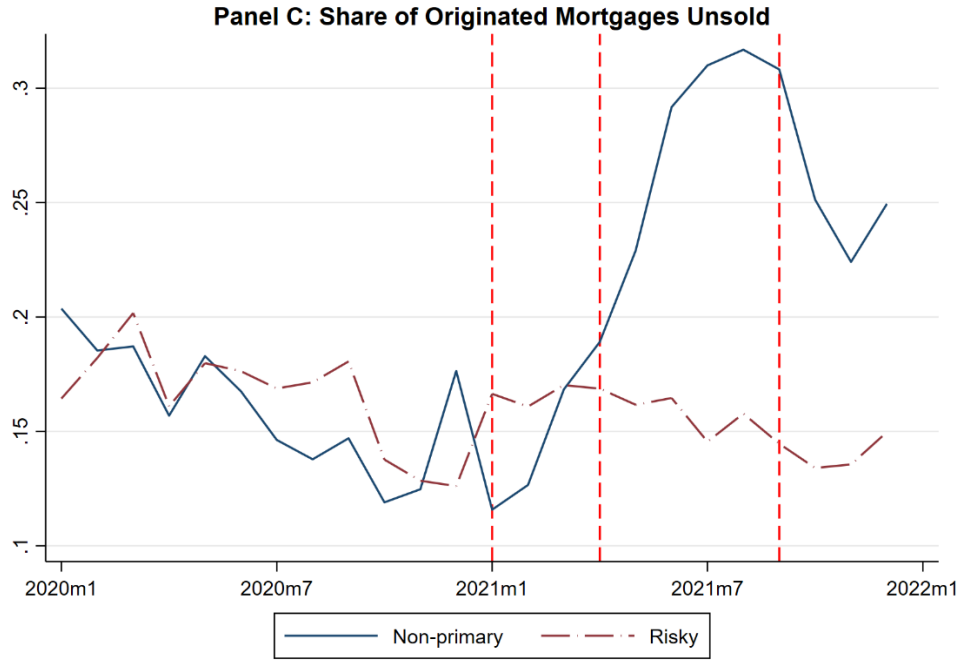
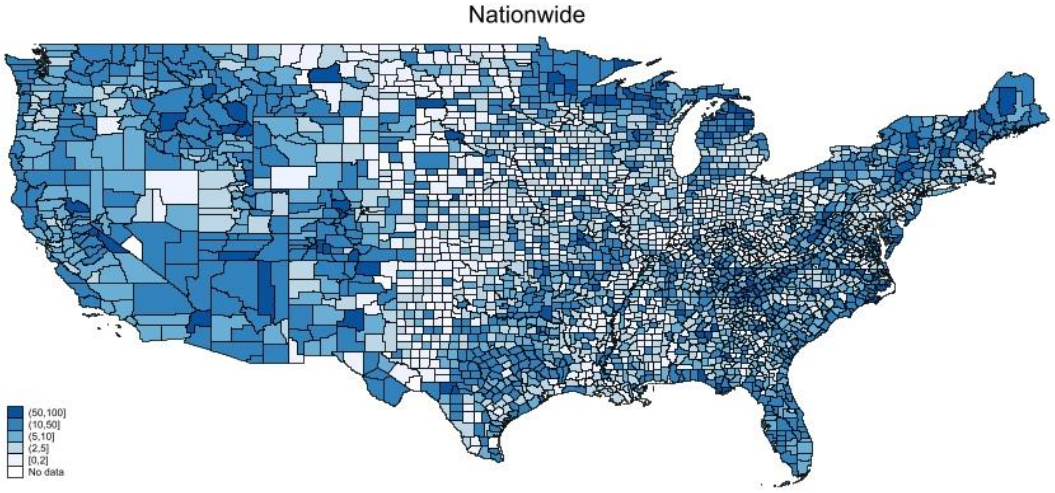


Figure 2: Time Series Plots of CHMDA Mortgage Trends

Panels A to C present nationwide trends in the conforming mortgage market. Panel A shows the non-primary (home purchase and refinance combined), risky home purchase, and risky refinance mortgages that are sold to the GSEs as fractions of all mortgages sold to the GSEs. Non-primary loans refer to mortgages that are backed by second homes or investment properties. Panel B shows the fractions of originated non-primary and risky mortgages sold to the GSEs. Panel C plots the fraction of originated non-primary and risky loans held on lenders' balance sheets. Conforming mortgages can be sold to the GSEs, sold to non-GSE entities, or held on the lenders' balance sheets. To account for seasonality in sale probability, induced by CHMDA's reporting convention, Panels B and C have been seasonally adjusted using the prior 24-month seasonality factors. Panel A is not adjusted for seasonality in sale probability because the sample only includes mortgages that have been sold to the GSEs. The three vertical lines in each figure represent the announcement, implementation, and termination of the GSE Purchase Cap policy, respectively. Data source: CHMDA.

Panel A: County-Level Exposure



Panel B: Tract-Level Exposure in Major Cities

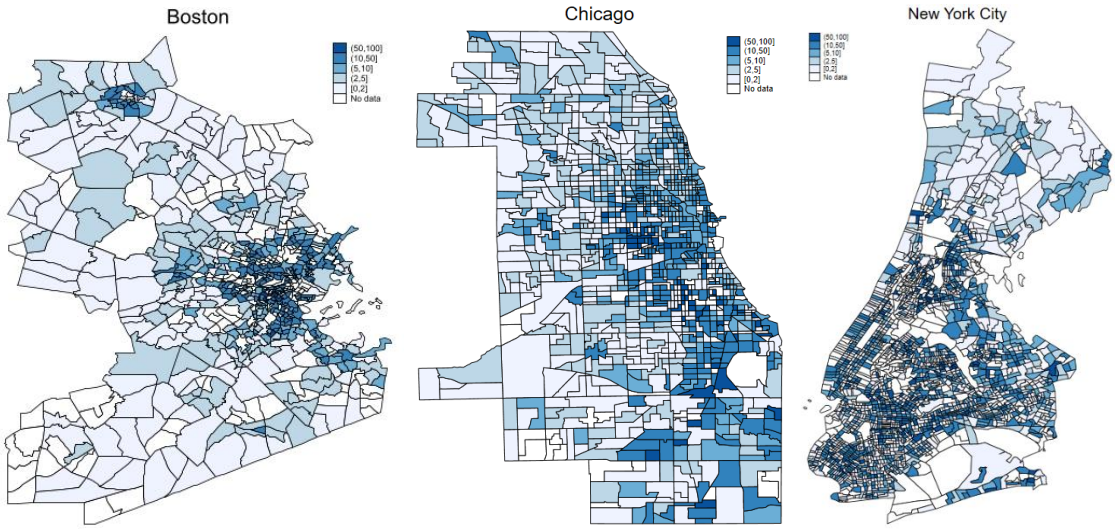


Figure 3. Geographic Exposure to the GSE Purchase Cap Policy

Panel A plots county-level exposure to the GSE Purchase Cap policy across the United States. County-level exposure to the GSE Purchase Cap policy is computed as the percentage of second-home and investment-property mortgages that the GSEs purchased in each county in 2020. Panel B plots tract-level exposure to the GSE Purchase Cap policy in three major cities: Boston, Chicago, and New York. Tract-level exposure to the GSE Purchase Cap policy is computed as the percentage of second-home and investment-property mortgages that the GSEs purchased in each census tract in 2020.

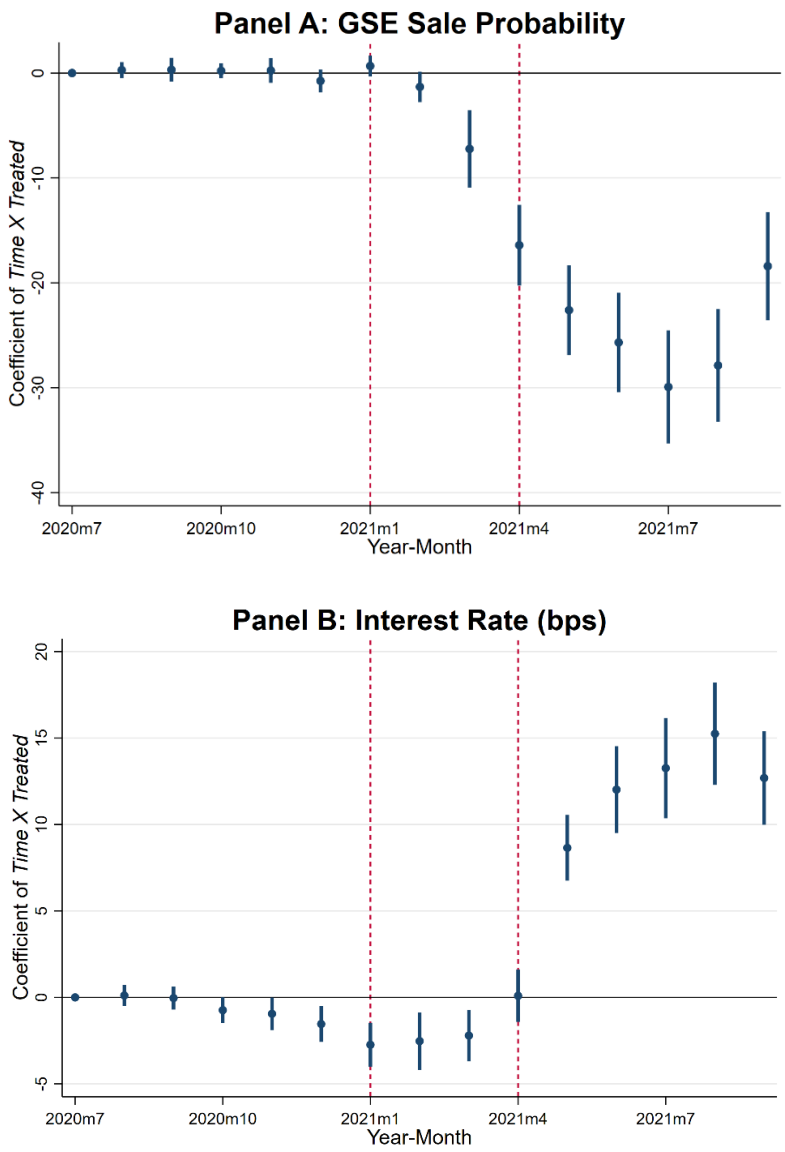


Figure 4. Loan-Level Parallel Trend Regressions – Non-Primary Mortgages

Panel A plots regression coefficients and their 95% confidence intervals from a loan-level OLS regression where the GSE sale indicator variable, multiplied by 100, is regressed onto year-month indicator variables interacted with the treatment indicator variable, which equals one for mortgages backed by second homes or investment properties and zero otherwise. The reference group is composed of "safe" conforming mortgages. The sample includes home purchase and refinance mortgages. The outcome variable equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. The regression specification includes control variables outlined in the Internet Appendix, lender by year-month and tract fixed effects. The sample includes mortgages that were originated between 2020Q3 and 2021Q3. The vertical dotted lines mark the month in which the GSE purchase cap policy was announced and implemented. Heteroskedasticity-robust standard errors are clustered at the lender level. Panel B plots the interest rate regression result. The outcome variable is the mortgage's interest rate, expressed in basis points. Data source: CHMDA.

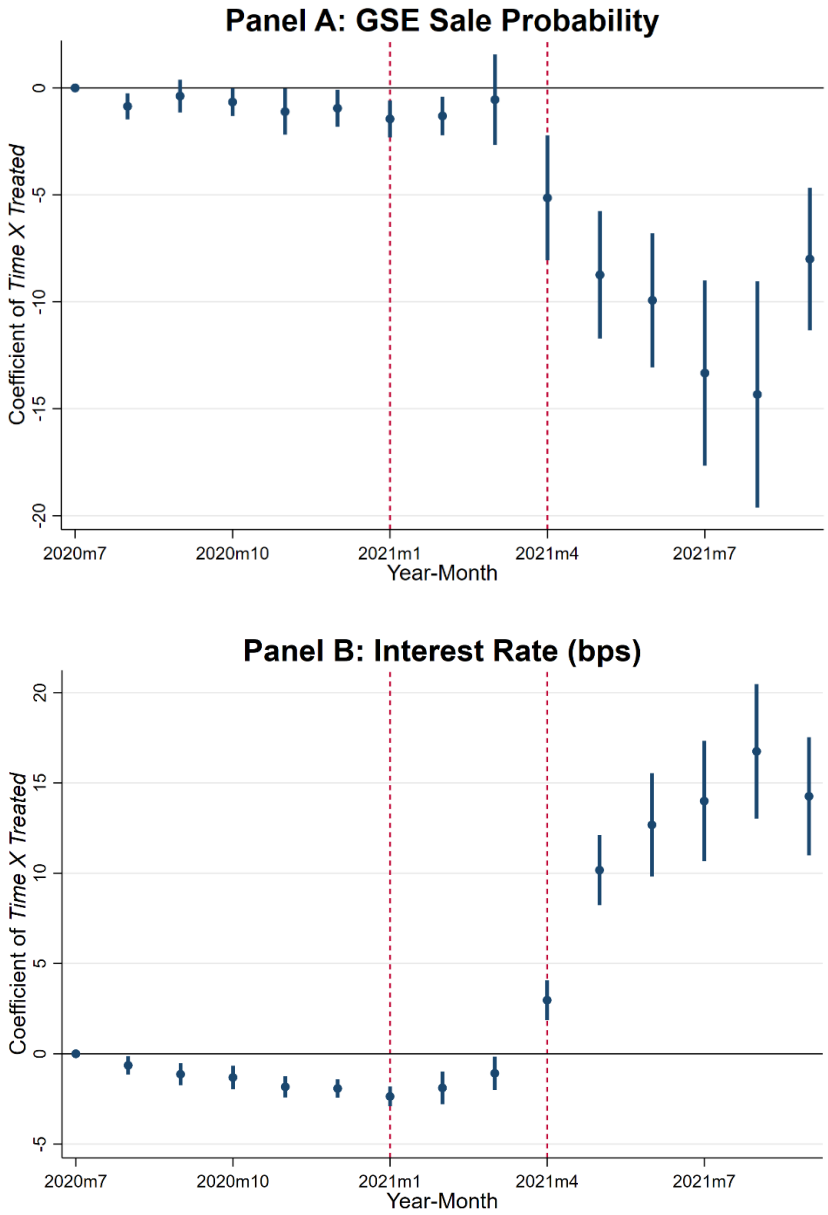


Figure 5. Loan-Level Parallel Trend Regressions – Second-Home Mortgages

Panel A plots regression coefficients and their 95% confidence intervals from a loan-level OLS regression where the GSE sale indicator variable, multiplied by 100, is regressed onto year-month indicator variables interacted with the treatment indicator variable, which equals one for mortgages associated with second homes and zero otherwise. The reference group is composed of "safe" conforming mortgage applications. The sample includes home purchase and refinance mortgages. The outcome variable equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. The regression specification includes control variables outlined in the Internet Appendix, lender by year-month, and tract fixed effects. The sample includes mortgages that were originated between 2020Q3 and 2021Q3. The vertical dotted lines mark the month in which the GSE purchase cap policy was announced and implemented. Heteroskedasticity-robust standard errors are clustered at the lender level. Panel B plots the interest rate regression result. The outcome variable is the mortgage's interest rate, expressed in basis points. Data source: CHMDA.

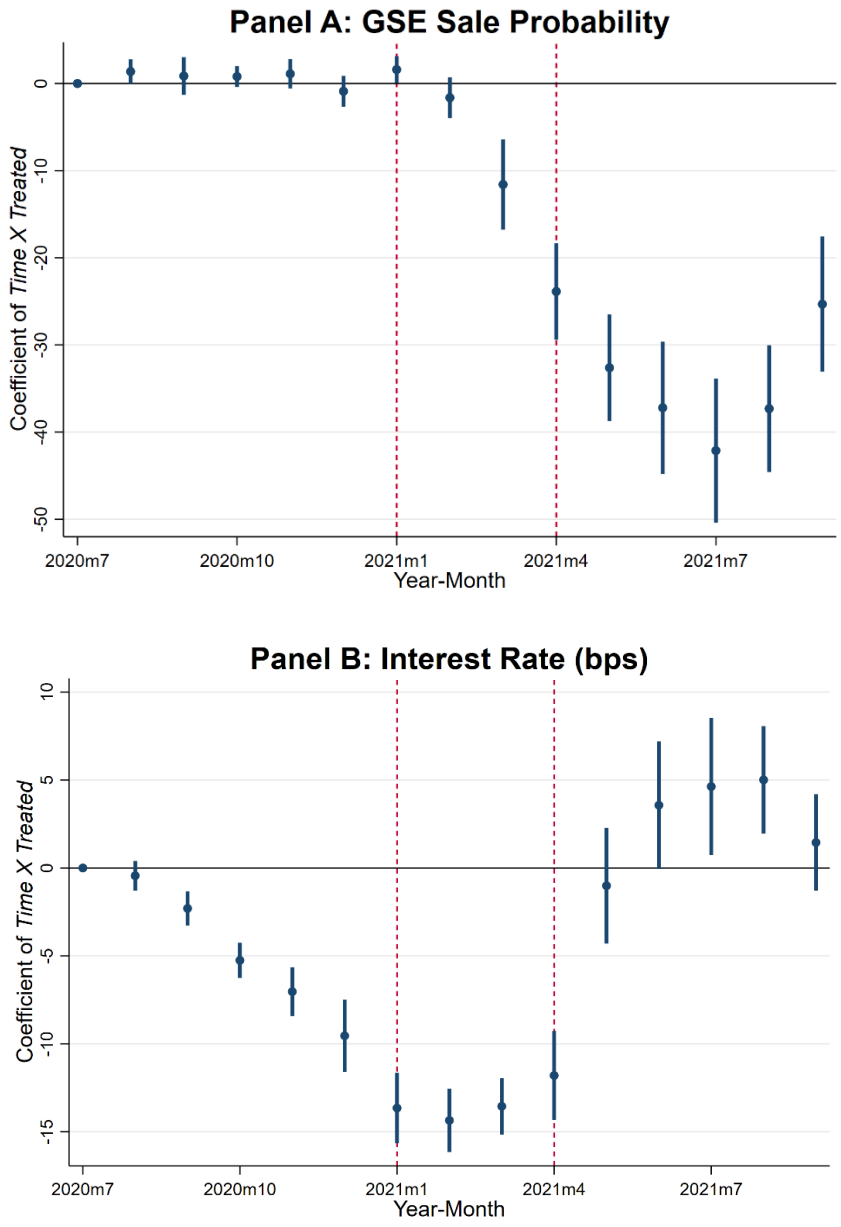


Figure 6. Loan-Level Parallel Trend Regressions – Investment-Home Mortgage

Panel A plots regression coefficients and their 95% confidence intervals from a loan-level OLS regression where the GSE sale indicator variable, multiplied by 100, is regressed onto year-month indicator variables interacted with the treatment indicator variable, which equals one for mortgages associated with investment homes and zero otherwise. The reference group is composed of "safe" conforming mortgage applications. The sample includes home purchase and refinance mortgages. The outcome variable equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. The regression specification includes control variables outlined in the Internet Appendix, lender by year-month, and tract fixed effects. The sample includes mortgages that were originated between 2020Q3 and 2021Q3. The vertical dotted lines mark the month in which the GSE purchase cap policy was announced and implemented. Heteroskedasticity-robust standard errors are clustered at the lender level. Panel B plots the interest rate regression result. The outcome variable is the mortgage's interest rate, expressed in basis points. Data source: CHMDA.

Table 1. Summary Statistics

This table presents summary statistics for the various data samples that we use in our analyses. The sample period is from 2020Q3 to 2021Q3. Panel A presents summary statistics on the loan-level sample. Panel B presents summary statistics on the lender-year-quarter-level sample. Panel C presents summary statistics on the lender-county-year-quarter-level sample. Panel D presents summary statistics on the zip-year-month-level sample. Panel E presents summary statistics on the zip-year-level sample. Panel F presents summary statistics on the zip-year-month-level sample. Panel G presents summary statistics on the transaction-level sample. Panels H presents summary statistics on the county-year-quarter-level sample. Refer to the Internet Appendix for additional details on variable definitions. Nominal dollar values are reported. Data sources: CHMDA, FDIC, Zillow, ACS, CoreLogic, Census Building Permit Survey, and Bureau of Labor Statistics Quarterly Census of Employment and Wages.

	Mean	Median	S.D.	N
	(1)	(2)	(3)	(4)
Panel A: Loan Level				
GSE Sale	0.66	1.00	0.47	6,805,482
Unsold	0.11	0.00	0.31	6,805,482
Non-GSE Sale	0.23	0.00	0.42	6,805,482
Interest Rate (bps)	302.36	299.00	34.77	6,802,556
Policy Exposure (Loan)				
Second-home Mortgage	0.04	0.00	0.21	6,805,482
Investment-home Mortgage	0.06	0.00	0.24	6,805,482
Risky Mortgage	0.04	0.00	0.19	6,805,482
Panel B: Lender-Year-Quarter Level				
Second-home Mortgage Application Volume (USD Millions)	11.99	2.47	26.08	7,247
Investment-home Mortgage Application Volume (USD Millions)	14.4	3.25	30.73	7,708
Risky Mortgage Application Volume (USD Millions)	19.3	3.75	47.94	6,415
Safe Mortgage Application Volume (USD Millions)	185.74	40.37	370.24	8,299
Second-home Mortgage Application Volume (USD Millions)	10.87	2.29	23.25	7,104
Investment-home Mortgage Application Volume (USD Millions)	12.5	2.97	26.26	7,582
Risky Mortgage Application Volume (USD Millions)	14	2.63	33.28	6,050
Safe Mortgage Application Volume (USD Millions)	167.76	35.95	334.14	8,290
Non-Bank	0.24	0	0.43	8,336
Policy Exposure (Lender)				
Non-primary Mortgage Treatment	0.47	0	0.5	8,560
Risky Home Purchase Mortgage Treatment	0.09	0	0.28	8,560
Risky Refinance Mortgage Treatment	0.03	0	0.17	8,560

	Mean	Median	S.D.	N
	(1)	(2)	(3)	(4)
Panel C: Lender-County-Year-Quarter Level				
Second-home Mortgage Application Volume (USD Millions)	0.66	0.34	1.03	148,322
Investment-home Mortgage Application Volume (USD Millions)	0.78	0.30	1.49	150,717
Bank	0.44	0	0.5	229,458
Branch	0.15	0	0.36	239,073
Policy Exposure (Lender-County)				
Non-primary Mortgage Treatment	0.54	1	0.5	239,073
Risky Home Purchase Mortgage Treatment	0.15	0	0.36	239,073
Risky Refinance Mortgage Treatment	0.06	0	0.24	239,073
Panel D: Zip-Year-Month Level (Zillow)				
Rent (USD)	1,699.21	1,577.39	561.1	40,303
ln(Rent)	7.39	7.36	0.33	40,303
Policy exposure (Zip)				
Treatment	0.09	0.07	0.08	40,303
Panel E: Zip-Year Level (Census ACS)				
Renter-occupied Housing Units	1,458.71	301.00	2,417.96	58,508
ln(Renter-occupied Housing Units)	5.79	5.71	1.98	58,508
Policy exposure (Zip)				
Treatment	0.11	0.05	0.16	58,508
Panel F: Zip-Year-Month Level (CoreLogic)				
% Mortgaged Transactions (Condos)	0.63	0.65	0.22	11,565
% Mortgaged Transactions (Single-Family Homes)	0.79	0.81	0.16	107,580
Policy exposure (Zip)				
Treatment	0.14	0.09	0.16	11,565
Panel G: Transaction Level (CoreLogic)				
Property Prices	380,699.69	291,000.00	341,493.36	7,653,659
ln(Property Prices)	12.55	12.58	0.80	7,653,659
Policy exposure (Transaction)				
Condo	0.13	0.00	0.33	7,653,659
Panel H: County-Year-Quarter Level				
One-unit Permit Growth	0.01	0.00	0.70	3,569
Average Construction Wage Growth	0.01	0.00	0.60	14,596
Construction Employment Growth	0.02	0.00	0.60	14,596
Policy exposure (County)				
Treatment	0.11	0.05	0.15	14,596

Table 2. GSE Purchase Cap and GSE Sale Probability

This table presents loan-level OLS regression results where the GSE sale indicator variable is regressed onto GSE purchase cap policy shock indicator variables. The outcome variable equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. In Panel A, Non-primary equals one for mortgages associated with second homes or investment properties and zero for safe mortgages associated with primary residences. In Panel B, Risky equals one for risky mortgages associated with primary residences and zero for safe mortgages associated with primary residences. Refer to the main text for details on the definition of risky. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Non-primary vs. Safe

	GSE Sale		
	(1)	(2)	(3)
Non-primary	-1.04	0.68	0.29
	[1.04]	[0.46]	[0.40]
Non-primary x Announcement	-3.78***	-3.65***	-2.99***
	[1.13]	[0.89]	[0.98]
Non-primary x Implementation	-19.48***	-23.15***	-22.71***
	[2.00]	[2.15]	[2.30]
Controls x Loan Purpose FE	Y	Y	Y
Tract FE	Y	Y	Y
Year-Quarter FE	Y	Y	
Lender FE		Y	
Lender x Year-Quarter FE			Y
Observations	6,556,469	6,556,426	6,556,069
R-squared	0.12	0.55	0.61

Panel B: Risky vs. Safe

	GSE Sale		
	(1)	(2)	(3)
Risky	0.57	0.57	0.44
	[1.17]	[0.45]	[0.36]
Risky x Announcement	-0.45	0.16	0.62*
	[0.92]	[0.53]	[0.34]
Risky x Implementation	-0.15	0.55	0.63
	[0.82]	[0.58]	[0.46]
Controls x Loan Purpose FE	Y	Y	Y
Tract FE	Y	Y	Y
Year-Quarter FE	Y	Y	
Lender FE		Y	
Lender x Year-Quarter FE			Y
Observations	6,076,949	6,076,900	6,076,528
R-squared	0.12	0.57	0.62

Table 3. GSE Purchase Cap, Loan Risk, and GSE Sale Probability

This table presents loan-level OLS regression results where the GSE sale indicator variable is regressed onto GSE purchase cap policy shock indicator variables. The outcome variable equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. Non-primary equals one for mortgages associated with second homes or investment properties and zero for safe mortgages associated with primary residences. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Credit Score is the main applicant's credit score divided by 100. CLTV is the mortgage's cumulative loan-to-value ratio. DTI is the mortgage's debt-to-income ratio. LLPA grid refers to Fannie Mae's loan-level price adjustment matrix. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

	GSE Sale	
	(1)	(2)
Non-primary	-3.67*	1.57
	[2.19]	[2.16]
Non-primary x Announcement	-0.96	-8.17**
	[3.64]	[3.96]
Non-primary x Implementation	-14.12**	-22.85***
	[6.52]	[7.83]
Non-primary x Credit Score	0.24	-0.17
	[0.26]	[0.25]
Non-primary x Announcement x Credit Score	-0.71	0.09
	[0.46]	[0.46]
Non-primary x Implementation x Credit Score	-2.71***	-1.84**
	[0.67]	[0.74]
Non-primary x CLTV	4.03**	0.72
	[1.60]	[1.39]
Non-primary x Announcement x CLTV	5.44**	7.04***
	[2.22]	[2.24]
Non-primary x Implementation x CLTV	19.03***	22.65***
	[5.00]	[5.41]
Non-primary x DTI	-1.66	-1.69
	[1.08]	[1.07]
Non-primary x Announcement x DTI	-0.71	-0.55
	[2.12]	[2.12]
Non-primary x Implementation x DTI	-3.83	-3.82
	[3.82]	[3.79]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
LLPA x Year-Quarter FE		Y
SE Cluster	Lender	Lender
Observations	6,500,542	6,500,537
R-squared	0.60	0.60

Table 4. GSE Purchase Cap and Interest Rate

This table presents mortgage-level OLS regression results where interest rate is regressed onto GSE purchase cap policy shock indicator variables. The outcome variable is the interest rate of the mortgage at origination, expressed in basis points. In Panel A, Non-primary equals one for mortgages associated with second homes or investment properties and zero for safe mortgages associated with primary residences. In Panel B, Risky equals one for risky mortgages associated with primary residences and zero for safe mortgages associated with primary residences. Refer to the main text for details on the definition of risky. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Non-primary vs. Safe

	Interest Rate (bps)		
	(1)	(2)	(3)
Non-primary	28.82***	28.91***	29.37***
	[0.74]	[0.80]	[0.80]
Non-primary x Announcement	-1.32***	-1.31***	-1.42**
	[0.46]	[0.50]	[0.61]
Non-primary x Implementation	10.91***	11.19***	10.50***
	[1.16]	[0.94]	[0.97]
Controls x Loan Purpose FE	Y	Y	Y
Tract FE	Y	Y	Y
Year-Quarter FE	Y	Y	
Lender FE		Y	
Lender x Year-Quarter FE			Y
Observations	6,553,829	6,553,786	6,553,426
R-squared	0.40	0.47	0.50

Panel B: Risky vs. Safe

	Interest Rate (bps)		
	(1)	(2)	(3)
Risky	-1.50*	-1.53**	-0.71
	[0.79]	[0.75]	[0.78]
Risky x Announcement	-1.27***	-1.02*	-1.15**
	[0.49]	[0.53]	[0.57]
Risky x Implementation	2.01***	2.38***	0.74
	[0.73]	[0.74]	[0.74]
Controls x Loan Purpose FE	Y	Y	Y
Tract FE	Y	Y	Y
Year-Quarter FE	Y	Y	
Lender FE		Y	
Lender x Year-Quarter FE			Y
Observations	6,074,529	6,074,480	6,074,110
R-squared	0.37	0.46	0.49

Table 5. GSE Purchase Cap, Second-Home and Investment-Property Mortgage GSE Sale Probability and Interest Rate

This table presents mortgage-level OLS regression results where GSE sale indicator variable or interest rate is regressed onto GSE purchase cap policy shock indicator variables. GSE Sale equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. In Panel A, Second equals one for mortgages associated with second homes and zero for safe mortgages associated with primary residences. In Panel B, Investment equals one for mortgages associated with investment homes and zero for safe mortgages associated with primary residences. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Second Home vs. Safe

	GSE Sale	Interest Rate (bps)
	(1)	(2)
Second	0.52**	-0.10
	[0.22]	[0.22]
Second x Announcement	-0.45	-0.25
	[0.47]	[0.30]
Second x Implementation	-8.92***	12.54***
	[1.39]	[1.24]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	6,131,811	6,129,544
R-squared	0.62	0.48

Panel B: Investment Property vs. Safe

	GSE Sale	Interest Rate (bps)
	(1)	(2)
Investment	0.48	52.34***
	[0.62]	[1.10]
Investment x Announcement	-4.80***	-8.54***
	[1.42]	[0.63]
Investment x Implementation	-32.27***	4.53***
	[3.45]	[1.09]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	6,256,499	6,254,001
R-squared	0.61	0.54

Table 6. GSE Purchase Cap, Second-Home and Investment-Property Mortgage GSE Sale Probability and Interest Rate – Bank versus Non-bank

This table presents mortgage-level OLS regression results where GSE sale indicator variable or interest rate is regressed onto GSE purchase cap policy shock indicator variables. GSE Sale equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise. In Panel A, Second equals one for mortgages associated with second homes and zero for safe mortgages associated with primary residences. In Panel B, Investment equals one for mortgages associated with investment homes and zero for safe mortgages associated with primary residences. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Non-bank equals one if the lender is not a depository institution according to the Avery file and zero otherwise. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Second Home vs. Safe

	GSE Sale	Interest Rate (bps)
	(1)	(2)
Second	-0.13	-2.13***
	[0.45]	[0.46]
Second x Non-bank	0.93	3.07***
	[0.64]	[0.65]
Second x Announcement	-0.38	-0.09
	[0.52]	[0.36]
Second x Implementation	-9.13***	10.79***
	[1.67]	[0.92]
Second x Non-bank x Announcement	-0.09	-0.29
	[0.82]	[0.60]
Second x Non-bank x Implementation	0.52	2.17
	[2.56]	[2.18]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	5,896,405	5,894,230
R-squared	0.62	0.48

Panel B: Investment Property vs. Safe		
	GSE Sale	Interest Rate (bps)
	(1)	(2)
Investment	0.00	56.05***
	[1.14]	[0.89]
Investment x Non-bank	0.50	-5.19***
	[1.38]	[1.36]
Investment x Announcement	-2.97	-7.30***
	[3.45]	[0.63]
Investment x Implementation	-20.31***	5.60***
	[5.60]	[1.42]
Investment x Non-bank x Announcement	-2.53	-1.48
	[3.82]	[1.03]
Investment x Non-bank x Implementation	-16.33**	-1.47
	[7.01]	[1.95]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	6,018,188	6,015,886
R-squared	0.61	0.54

Table 7. GSE Purchase Cap, Unsold and Non-GSE Sale – Bank versus Non-Bank

This table presents application/mortgage-level OLS regression results where unsold or non-GSE sale indicator variable is regressed onto GSE purchase cap policy shock indicator variables. Unsold equals 100 if the mortgage was not sold by the end of the reporting calendar year and zero otherwise. Non-GSE Sale equals 100 if the mortgage was sold to a non-GSE entity by the end of the reporting calendar year and zero otherwise. In Panel A, Second equals one for mortgages associated with second homes and zero for safe mortgages associated with primary residences. In Panel B, Investment equals one for mortgages associated with investment properties and zero for safe mortgages associated with primary residences. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Non-bank equals one if the lender is not a depository institution according to the Avery file and zero otherwise. The sample includes conforming home purchase and refinance mortgages that were originated between 2020Q3 and 2021Q3. Controls × Loan Purpose FE represents the interaction between the loan purpose indicator (home purchase or refinance) and loan characteristic indicators, accounting for differences in lending standards across the two products. Refer to the Internet Appendix for details on the control variables. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Second Home vs. Safe

	Unsold	Non-GSE Sale
	(1)	(2)
Second	-0.16	0.29
	[0.40]	[0.22]
Second x Non-bank	0.25	-1.18***
	[0.50]	[0.38]
Second x Announcement	1.44***	-1.06***
	[0.37]	[0.37]
Second x Implementation	8.65***	0.48
	[1.65]	[0.73]
Second x Non-bank x Announcement	-0.89	0.98
	[0.59]	[0.64]
Second x Non-bank x Implementation	-6.28***	5.76***
	[2.12]	[1.72]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	5,896,405	5,896,405
R-squared	0.54	0.67

Panel B: Investment Property vs. Safe		
	Unsold	Non-GSE Sale
	(1)	(2)
Investment	0.40	-0.40
	[1.00]	[0.78]
Investment x Non-bank	0.82	-1.31
	[1.02]	[1.02]
Investment x Announcement	2.14	0.83
	[3.35]	[1.50]
Investment x Implementation	13.93**	6.38***
	[5.64]	[1.75]
Investment x Non-bank x Announcement	-1.18	3.72*
	[3.50]	[1.91]
Investment x Non-bank x Implementation	-8.85	25.17***
	[6.33]	[5.02]
Controls x Loan Purpose FE	Y	Y
Tract FE	Y	Y
Lender x Year-Quarter FE	Y	Y
Observations	6,018,188	6,018,188
R-squared	0.53	0.66

Table 8. GSE Purchase Cap and Mortgage Credit Supply

This table presents OLS regression results for GSE purchase cap policy treatment and conforming mortgage lending volume. Each observation is a lender by year-quarter cell. In Panel A, the dependent variables are the natural log of home purchase and refinance mortgage origination volumes. Calculated at the lender level, Treatment equals one if the proportion of the lender's 2020 non-primary home purchase and refinance loan amount sold to the GSEs relative to its total conventional conforming home purchase and refinance loan amount sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Panel B presents the results for application volumes. Refer to the Internet Appendix for additional details on the variable definitions. Heteroskedasticity-robust standard errors are clustered at the lender level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Origination Volume

	ln(Origination Volume USD)			
	Second Home	Investment	Risky	Safe
	(1)	(2)	(3)	(4)
Announcement x Treatment	-0.04	0.03	0.05	0.06***
	[0.03]	[0.03]	[0.04]	[0.02]
Implementation x Treatment	-0.18***	-0.18***	-0.01	0.02
	[0.03]	[0.03]	[0.04]	[0.02]
Year-Quarter FE	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y
Observations	7,033	7,540	5,986	8,285
R-squared	0.92	0.92	0.93	0.97

Panel B: Application Volume

	ln(Application Volume USD)			
	Second Home	Investment	Risky	Safe
	(1)	(2)	(3)	(4)
Announcement x Treatment	-0.04	0.04	0.02	0.06***
	[0.03]	[0.03]	[0.04]	[0.02]
Implementation x Treatment	-0.17***	-0.17***	0.02	0.02
	[0.03]	[0.03]	[0.03]	[0.02]
Year-Quarter FE	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y
Observations	7,178	7,669	6,383	8,295
R-squared	0.93	0.93	0.94	0.97

Table 9. GSE Purchase Cap Policy and Mortgage Credit Supply – Bank versus Non-Bank

This table presents OLS regression results for GSE purchase cap policy treatment and conforming mortgage lending volume. Each observation is a lender by year-quarter cell. In Panel A, the dependent variables are the natural log of home purchase and refinance mortgage origination volumes. Calculated at the lender level, Treatment equals one if the proportion of the lender's 2020 non-primary home purchase and refinance loan amount sold to the GSEs relative to its total conventional conforming home purchase and refinance loan amount sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. Non-bank equals one if the lender is not a depository institution according to the Avery file and zero otherwise. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Panel B presents the results for application volumes. Refer to the Internet Appendix for additional details on the variable definitions. Heteroskedasticity-robust standard errors are clustered at the tract level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

Panel A: Origination Volume

	ln(Origination Volume USD)			
	Second Home	Investment	Risky	Safe
	(1)	(2)	(3)	(4)
Announcement x Treatment	-0.06	0.03	0.03	0.05**
	[0.04]	[0.04]	[0.05]	[0.02]
Implementation x Treatment	-0.18***	-0.13***	-0.06	0.03
	[0.04]	[0.04]	[0.04]	[0.03]
Announcement x Non-bank	0.21***	0.27***	0.17**	0.07
	[0.06]	[0.06]	[0.07]	[0.05]
Implementation x Non-bank	-0.12*	-0.21***	0.24***	0.05
	[0.06]	[0.07]	[0.06]	[0.05]
Announcement x Non-bank x Treatment	-0.04	-0.11	0.03	0.02
	[0.07]	[0.07]	[0.09]	[0.07]
Implementation x Non-bank x Treatment	0.03	-0.08	0.05	-0.05
	[0.08]	[0.08]	[0.08]	[0.07]
Year-Quarter FE	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y
Observations	6,826	7,330	5,783	8,068
R-squared	0.93	0.93	0.93	0.97

Panel B: Application Volume

	ln(Application Volume USD)			
	Second Home	Investment	Risky	Safe
	(1)	(2)	(3)	(4)
Announcement x Treatment	-0.05	0.04	-0.00	0.05***
	[0.04]	[0.04]	[0.04]	[0.02]
Implementation x Treatment	-0.17***	-0.13***	-0.01	0.03
	[0.04]	[0.04]	[0.04]	[0.03]
Announcement x Non-bank	0.19***	0.23***	0.11*	0.07
	[0.06]	[0.05]	[0.06]	[0.05]
Implementation x Non-bank	-0.13**	-0.19***	0.28***	0.05
	[0.06]	[0.06]	[0.06]	[0.05]
Announcement x Non-bank x Treatment	-0.04	-0.10	0.02	-0.00
	[0.07]	[0.07]	[0.08]	[0.06]
Implementation x Non-bank x Treatment	0.04	-0.06	-0.00	-0.04
	[0.07]	[0.08]	[0.08]	[0.06]
Year-Quarter FE	Y	Y	Y	Y
Lender FE	Y	Y	Y	Y
Observations	6,967	7,456	6,179	8,078
R-squared	0.93	0.93	0.94	0.97

Table 10. Within-lender Non-Primary Mortgage Origination

This table presents OLS regression results at the county-lender-quarter level where the natural log of non-primary home purchase and refinance mortgage dollar amounts are regressed onto pre-policy lender-county-level policy exposure. Columns (1) through (3) present the results for second home mortgage origination volume. Columns (4) through (6) present the results for investment-home mortgage origination volume. Treatment equals one if the lender-county cell's 2020 non-primary home purchase and refinance loan amount sold to the GSEs divided by its total conventional conforming home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Bank equals one if the lender is a bank, according to the Avery file. Refer to the Internet Appendix for additional details on the variable definitions. Heteroskedasticity-robust standard errors are double clustered at both the lender level and the county level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA.

	ln(Second Home Origination)			ln(Investment Property Origination)		
	(1)	(2)	(3)	(4)	(5)	(6)
Announcement x Treatment	-0.17***	-0.17***	-0.13***	-0.21***	-0.23***	-0.23***
	[0.02]	[0.02]	[0.03]	[0.02]	[0.02]	[0.03]
Implementation x Treatment	-0.22***	-0.21***	-0.20***	-0.21***	-0.23***	-0.17***
	[0.02]	[0.02]	[0.03]	[0.01]	[0.02]	[0.03]
Announcement x Treatment x Bank		0.03			0.04*	
		[0.03]			[0.03]	
Implementation x Treatment x Bank		0.00			0.04*	
		[0.03]			[0.03]	
Branch			0.17			0.20
			[0.13]			[0.13]
Announcement x Branch			-0.01			-0.06*
			[0.04]			[0.04]
Implementation x Branch			-0.08*			-0.03
			[0.04]			[0.04]
Branch x Treatment			-0.10			-0.06
			[0.13]			[0.17]
Announcement x Treatment x Branch			0.01			0.13***
			[0.05]			[0.04]
Implementation x Treatment x Branch			0.00			0.02
			[0.04]			[0.03]
Lender-Year-Quarter FE	Y	Y	Y	Y	Y	Y
County-Year-Quarter FE	Y	Y	Y	Y	Y	Y
Lender-County FE	Y	Y	Y	Y	Y	Y
Sample	All lenders	All lenders	Banks	All lenders	All lenders	Banks
Observations	108,292	103,893	42,412	117,713	113,217	45,396
R-squared	0.78	0.78	0.79	0.83	0.83	0.83

Table 11. GSE Purchase Cap and Rent

The table presents OLS regression results where monthly zip-code-level rental prices, obtained from Zillow, are regressed on GSE purchase cap policy treatment variables at the zip code level. Each observation represents a zip-code-by-month cell. The outcome variable is the natural log of rent. Treatment is measured at the zip code level and aggregated as a weighted sum of the tract-level treatment measure using the HUD's zip-code-to-tract crosswalk. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. The sample period spans from July 2020 to September 2021. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. County-level COVID-19 controls include the total number of COVID-19 cases and deaths in each month. Additional details on variable definitions are provided in the Internet Appendix. Heteroskedasticity-robust standard errors are clustered at the zip code level. Data source: CHMDA, Zillow Observed Rent Index, and Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

	ln(Rent)		
	(1)	(2)	(3)
Announcement x Treatment	0.037***	0.038***	0.044***
	[0.008]	[0.008]	[0.008]
Implementation x Treatment	0.063***	0.064***	0.073***
	[0.013]	[0.013]	[0.013]
COVID-19 Controls		Y	
COVID-19 Controls x Year-Month FE			Y
Year-Month FE	Y	Y	Y
Zip Code FE	Y	Y	Y
Observations	40,303	40,303	40,303
R-squared	0.994	0.994	0.995

Table 12. GSE Purchase Cap and Rental Housing Supply

The table presents OLS regression results where the natural log of the number of annual renter-occupied housing units, obtained from the American Community Survey, is regressed on GSE purchase cap policy treatment intensity variables at the zip code level. Each observation represents a zip-code-by-year cell. The outcome variable is the natural log of renter-occupied housing units. Treatment is measured at the zip code level and aggregated as a weighted sum of the tract-level treatment measure using the HUD's zip-code-tract crosswalk. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. The sample period spans from 2020 to 2021. Implementation equals one for 2021 and zero otherwise. County-level COVID-19 controls include the total number of COVID-19 cases and deaths in each year. Additional details on variable definitions are provided in the Internet Appendix. Heteroskedasticity-robust standard errors are clustered at the zip code level. Data source: CHMDA, American Community Survey, and Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

	ln(Renter Occupied Housing Units)		
	(1)	(2)	(3)
Implementation x Treatment	-0.061***	-0.053***	-0.051***
	[0.014]	[0.014]	[0.014]
COVID-19 Controls		Y	
COVID-19 Controls x Year FE			Y
Year FE	Y	Y	Y
Zip Code FE	Y	Y	Y
Observations	58,508	58,508	58,508
R-squared	0.994	0.994	0.994

Table 13. GSE Purchase Cap and Property Prices

This table presents OLS regression results where log transaction price is regressed onto the condo indicator variable. Each observation is a transaction. Condo equals one if the property in the transaction is a condo or an apartment and zero if it is a single-family house. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Property characteristics include the number of bedrooms, the number of bathrooms, the year built, and the total square feet of the property. Columns (1) and (2) use the sample of zip codes with at least one condo transaction in any month from 2020Q3 to 2021Q3. Columns (3) and (4) use the sample of zip codes with at least 10 condo transactions in all months from 2020Q3 to 2021Q3. Refer to the Internet Appendix for additional details on the variable definitions. Heteroskedasticity-robust standard errors are clustered at the zip code level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA and CoreLogic.

	ln(Property Price)			
	(1)	(2)	(3)	(4)
Announcement x Condo	-0.011***	-0.015***	-0.011**	-0.008
	[0.002]	[0.002]	[0.004]	[0.005]
Implementation x Condo	-0.022***	-0.030***	-0.021***	-0.023***
	[0.002]	[0.002]	[0.004]	[0.005]
Sample	All zip codes		Zips with # monthly transactions >=10	
Property Characteristics	Y		Y	
Property Characteristics x Year-Month FE		Y		Y
Zip Code x Year-Month FE	Y	Y	Y	Y
Observations	7,531,088	7,531,088	891,380	891,380
R-squared	0.705	0.705	0.691	0.692

Table 14. GSE Purchase Cap and Housing Transactions

This table presents OLS regression results where shares of housing transaction types are regressed onto GSE purchase cap policy treatment variables. Each observation is a zip code by year-month cell. The outcome variable in Columns (1) and (4) is the percentage of mortgaged single-family home transactions among all single-family home transactions. The outcome variable in Columns (2) and (5) is the percentage of mortgaged condo transactions among all condo transactions. The outcome variable in Columns (3) and (6) is the difference between the percentage of mortgaged condo transactions over among condo transactions and the percentage of mortgaged single-family home transactions among all single-family home transactions. Treatment is measured at the zip code level and aggregated as a weighted sum of the tract-level treatment measure using the HUD's zip-code-to-tract crosswalk. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Columns (1) and (4) use the sample of zip codes with at least 10 single-family home transactions in all months from 2020Q3 to 2021 Q3. Columns (2) and (5) use the sample of zip codes with at least 10 condo transactions in all months from 2020Q3 to 2021 Q3. Columns (3) and (6) use the sample of zip codes with at least 10 single-family home transactions and 10 condo transactions in all months from 2020Q3 to 2021 Q3. County-level COVID-19 controls include the total number of COVID-19 cases and deaths in each month. Refer to the Internet Appendix for additional details on the variable definitions. Heteroskedasticity-robust standard errors are clustered at the zip code level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA, CoreLogic, and Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

	% Mortgaged Transactions					
	Single-Family Homes	Condos	Difference	Single-Family Homes	Condos	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Announcement x Treatment	-0.006	-0.091***	-0.029	-0.003	-0.080***	-0.029
	[0.008]	[0.017]	[0.025]	[0.008]	[0.017]	[0.026]
Implementation x Treatment	-0.021***	-0.074***	-0.032*	-0.018***	-0.057***	-0.035**
	[0.007]	[0.015]	[0.017]	[0.006]	[0.015]	[0.017]
COVID-19 Controls	Y	Y	Y			
COVID-19 Controls x Year-Month FE				Y	Y	Y
Year-Month FE	Y	Y	Y	Y	Y	Y
Zip Code FE	Y	Y	Y	Y	Y	Y
Observations	107,580	11,565	8,445	107,580	11,565	8,445
R-squared	0.805	0.829	0.588	0.806	0.831	0.589

Table 15. GSE Purchase Cap and Real Effects

This table presents OLS regression results where measures of local economic outcomes are regressed onto GSE purchase cap policy exposure. Each observation is a county by year-quarter cell. Columns 1 and 2 report the results for the growth rate of one-unit single-family house building permits. Columns 3 through 6 report the results for the growth rate of residential construction employment and wage. Treatment is calculated as, for 2020, the county's total dollar amount of non-primary home purchase and refinance loans sold to the GSEs divided by the county's total dollar amount of conventional conforming home purchase and refinance loans sold to the GSEs. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with the corresponding event time interaction terms. Announcement equals one for 2021Q1 and zero otherwise. Implementation equals one for 2021Q2 and 2021Q3 and zero otherwise. Refer to the Internet Appendix for details on the variable definitions. Heteroskedasticity-robust standard errors are clustered at the county level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: CHMDA, Census Building Permit Survey, and Bureau of Labor Statistics Quarterly Census of Employment and Wages.

	One-unit Permit Growth		Construction Employment Growth		Average Construction Wage Growth	
	(1)	(2)	(3)	(4)	(5)	(6)
Announcement x Treatment	-0.12	-0.11	0.07	0.10	0.17	0.21*
	[0.21]	[0.21]	[0.11]	[0.12]	[0.12]	[0.12]
Implementation x Treatment	0.14	0.14	-0.03	0.01	0.03	0.07
	[0.20]	[0.19]	[0.07]	[0.07]	[0.07]	[0.07]
Unemployment Rate		0.02		0.01		0.02*
		[0.02]		[0.01]		[0.01]
GDP Growth		0.00**		0.00		0.00
		[0.00]		[0.00]		[0.00]
ln(Per Capita Income)		-1.24		1.11***		1.26***
		[1.13]		[0.34]		[0.35]
Year-Quarter FE	Y	Y	Y	Y	Y	Y
County FE	Y	Y	Y	Y	Y	Y
Observations	3,569	3,569	14,596	14,596	14,596	14,596
R-squared	0.11	0.11	0.07	0.07	0.06	0.06

Internet Appendix

Loan-Level Variable Definition (CHMDA)

This section defines the variables used in regressions results presented in Tables 2 through 7. The choice of loan-level control variables and their definitions largely follows Amornsiripanitch (2023) and Bhutta et al. (2020). Credit score, CLTV ratio, and DTI ratio indicator variables interacted to form a credit score-CLTV-DTI grid. The loan purpose (home purchase or refinance) indicator variable is interacted with each loan characteristic indicator variable to allow for differences in lending standard across the two products. Regression results presented in Table 3 do not include CLTV, DTI, and credit score indicator variables because these loan characteristics are included in the regressions as continuous variables and interacted with the treatment indicator variables.

GSE Sale Indicator Variable – Equals 100 if the mortgage was sold to Fannie Mae or Freddie Mac by the end of the reporting calendar year and zero otherwise.
Interest Rate (bps) –The mortgage interest rate at origination, expressed in basis points.
Unsold Indicator Variable – Equals 100 if the mortgage was not sold by the end of the reporting calendar year and zero otherwise.
Non-GSE Indicator Variable – Equals 100 if the mortgage was sold to a non-GSE entity by the end of the reporting calendar year and zero otherwise.
Non-primary Indicator Variable – Equals one for mortgages associated with second homes or investment properties and zero for safe mortgages associated with primary residences.
Risky Indicator Variable – equals one for risky mortgages associated with primary residences and zero otherwise. Refer to the main text for details on the definition of risky.
Loan Amount Indicator Variables – Applications are sorted into groups according to their loan amounts. The reference group is made up of applications with loan amounts between 0 and $50,000. The remaining groups are formed by $50,000 increments of loan amount up to $749,999. The final group is made up of loans with loan amounts greater than $749,999.
Income Indicator Variables – Applications are sorted into groups according to the applicant's annual income. The reference group is made up of applications with income values between 0 and $50,000. The remaining groups are formed by $25,000 increments of income values up to $499,999. The final group is made up of loans associated with applicants with income values greater than $499,999. Loans that have missing income values form a separate group.
CLTV Indicator Variables – Applications are sorted into groups according to the loan's CLTV value. CLTV values from 0 to 19 form one group. CLTV values from 30 to 79 form 10-points groups. CLTV values from 80 to 94 form 5-points groups. CLTV values from 95 to 100 form 1-point groups. CLTV values from 101 to 110 form one group. CLTV values from 111 to 120 form one group. CLTV values greater than 120 form one group. Missing CLTV values form one group. Negative CLTV values form one group. Note that, following Bartlett et al. (2022), applications that have LTV lower than 30 or greater than 130 are dropped so that the sample conforms to the GSEs' purchasing requirements. CLTV accounts for other debt associated with the property over and above the loan being considered.
DTI Indicator Variables – Applications are sorted into groups according to the loan's DTI value. DTI values between 0 and 30 form 5-points groups. DTI values between 31 and 60 form 1-point groups. DTI values from 61 to 80 form one group. DTI values from 81 to 100 form one group. DTI values greater than 100 form one group. Missing DTI values from one group. Negative DTI values form one group.

Applicant Credit Score Indicator Variables – Applications are sorted into groups according to the applicant's credit score value. Credit score values from 620 to 850 are broken into 10-points groups. Credit scores of 850 or greater form a group. To conform to the GSEs' purchasing requirements, applications where the main applicant's credit score is lower than 620 are dropped.
Co-applicant Credit Score Indicator Variables -- Applications are sorted into groups according to the co-applicant's credit score value. Missing credit scores form one group. Negative credit scores form one group. Credit scores between 0 and 299 form one group. Credit scores from 300 to 499 are broken down into two 100-point groups. Credit scores from 500 to 579 form one group. Credit score values from 580 to 849 are broken into 10-points groups. Credit scores of 850 or greater form a group.
Co-applicant Indicator Variable – An indicator variable that equals one if the application has two applicants and zero otherwise.
Age Group Indicator Variables – A set of indicator variables that captures the age group in which the applicant associated with each loan application belongs to. The age groups are 18 to 24, 25 to 29, 30 to 39, 40 to 49, 50 to 59, 60 to 69, 70 or older, and missing age. The regression uses mortgages associated with applicants in the first age group as the reference group. The missing age group indicator variable is included in the estimation but omitted from the regression outputs.
Female – An indicator variable that equals one if there is at least one female applicant associated with the loan application and zero otherwise.
Asian – An indicator variable that equals one if there is at least one Asian applicant associated with the loan application and zero otherwise.
Black – An indicator variable that equals one if there is at least one Black applicant associated with the loan application and zero otherwise.
Hispanic – An indicator variable that equals one if there is at least one Hispanic applicant associated with the loan application and zero otherwise.
Other Minority – An indicator variable that equals one if there is at least one minority applicant who is not Asian or Black associated with the loan application and zero otherwise.
Unknown Sex – An indicator variable that equals one if there is at least one applicant whose sex is unknown and zero otherwise.
Both Sexes – An indicator variable that equals one if, for at least one applicant, the applicant reported being both male and female and zero otherwise.
Unknown Race – An indicator variable that equals one if there is at least one applicant whose race is unknown and zero otherwise.
Unknown Ethnicity – An indicator variable that equals one if there is at least one applicant whose ethnicity is unknown and zero otherwise.
AUS Approved – An indicator variable that equals one if the loan application was approved by at least one AUS and zero otherwise.
LLPA Grid Fixed Effects – Following Barlett et al. (2022), each LTV by credit score cell of Fannie Mae's 2021 loan-level pricing adjustment matrix is coded as an indicator variable. This set of indicator variables is only included in adverse selection regressions (Tables 3, IA.2, IA.3, and IA.4).
Non-bank – Equals one if, for the data year, the Avery file indicates that the lender is of TYPE 40 or 41 and zero otherwise. Non-Bank is set to missing for TYPE = 11-14, 21-14, and 32-33.

Lender-Level Regression Variable Definition (CHMDA)

This section defines outcome and control variables used in lender-level regression results presented in Tables 8 and 9.

ln(Origination Volume USD) – The lender-quarter level natural log of originated mortgage (in USD) backed by second-home, investment-home, risky or safe mortgages.
ln(Application Volume USD) – The lender-quarter level natural log of mortgage application volume (in USD) for second-home, investment-home, risky or safe mortgages.
Treatment (Risky Purchase) – Equals to one if the proportion of the lender's 2020 conventional risky home purchase loan amount sold to the GSEs relative to its total conventional home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 6% and zero otherwise. This variable is separately interacted with Announcement and Implementation indicator variables.
Treatment (Risky Refi) – Equals to one if the proportion of the lender's 2020 conventional risky refinance loan amount sold to the GSEs relative to its total conventional home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 3% and zero otherwise. This variable is separately interacted with Announcement and Implementation indicator variables.
Non-bank – Equals one if, for the data year, the Avery file indicates that the lender is of TYPE 40 or 41 and zero otherwise. Non-bank is set to missing for TYPE = 11-14, 21-14, and 32-33.

Lender-County Level Regression Variable Definition

This section defines outcome and control variables used in within-lender regression results presented in Table 10.

ln(Second Home Origination) – The lender-county-quarter level natural log of second-home purchase and refinance mortgage originated (USD).
ln(Investment Property Origination) – The lender-county-quarter level natural log of investment-home purchase and refinance mortgage originated (USD).
Treatment (Risky Purchase) – Equals to one if the proportion of the lender-county cell's 2020 risky home purchase loan total dollar amount sold to the GSEs relative to its total dollar amount of conventional conforming home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 6% and zero otherwise. This variable is separately interacted with Announcement and Implementation indicator variables.
Treatment (Risky Refi) – Equals to one if the proportion of the lender-county cell's 2020 risky refinance loan total dollar amount sold to the GSEs relative to its total dollar amount of conventional conforming home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 3% and zero otherwise. This variable is separately interacted with Announcement and Implementation indicator variables.
Branch – An indicator variable that equals one if a bank has branch presence in that county-year cell, based on the FDIC's Summary of Deposits data.
Bank – Equals one when Non-bank equals zero. Bank is set to missing for TYPE = 11-14, 21-14, and 32-33 in the Avery file.

Rental and Housing Market Variable Definition

This section defines outcome and control variables used in zip-code-level housing market regression results presented in Tables 11, 12, and 14.

ln(Rent) – The natural log of monthly zip-code-level rental price from the Zillow Observed Rent Index (ZORI).
ln(Renter-occupied Housing Units) – The natural log of monthly zip-code-level renter occupied housing units obtained from the American Community Survey (ACS).
% Mortgaged Transactions (Single-Family Homes) – The percentage of mortgaged single-family home transactions over all single-family home transactions.
% Mortgaged Transactions (Condos) – The percentage of mortgaged condo transactions over all condo transactions.
% Mortgaged Transactions (Difference) – % Mortgaged Transactions (Condos) minus % Mortgaged Transactions (Single-Family Homes).
Treatment – For 2020, the zip code's total dollar amount of second-home and investment-property purchase loan sold to the GSEs divided by the zip code's total amount dollar of conventional conforming home purchase and refinance loan sold to the GSEs. The zip code level loan amount is aggregated from tract level loan amount using zip Code and Tract crosswalk mapping.
Treatment (Risky Purchase) – For 2020, the zip code's total dollar amount of risky purchase loan sold to the GSEs divided by the zip code's total dollar amount of conventional conforming home purchase and refinance loan sold to the GSEs. The zip code level loan amount is aggregated from tract level loan amount using zip code and tract crosswalk mapping. This variable is separately interacted with Announcement and Implementation indicator variables.
Treatment (Risky Refi) – For 2020, the zip code's total dollar amount of risky refinance loan sold to the GSEs divided by the zip code's total dollar amount of conventional conforming home purchase and refinance loan sold to the GSEs. The zip code level loan amount is aggregated from tract level loan amount using zip code and tract crosswalk mapping. This variable is separately interacted with Announcement and Implementation indicator variables.
COVID-19 Cases – The county-monthly-level COVID-19 cases obtained from the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.
COVID-19 Death – The county-monthly-level COVID-19 deaths obtained from the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University.

Transaction-level Housing Market Variable Definition

This section defines the outcome and control variables used in transaction-level housing market regression results presented in Table 13.

ln(Property Price) – The natural log of the transaction price of the property.
Condo – An indicator variable that equals to one if the property in the transaction is a condo or an apartment and zero if it is a single-family home.
 # Bedrooms– The number of bedrooms in the property.
 # Bathrooms– The number of bathrooms in the property.
Year Built – The built year of the property.

Square Feet – The total square feet of the property.

Real Effects Variable Definition

This section defines outcome and control variables used in county-level real effects regression results presented in Table 15.

Treatment – For 2020, the county's total amount of non-primary home purchase loan sold to the GSEs divided by the county's total amount of conventional conforming home purchase and refinance loan sold to the GSEs.
Treatment (Risky Purchase) – For 2020, the county's total amount of risky home purchase loan sold to the GSEs divided by the county's total amount of conventional conforming home purchase and refinance loan sold to the GSEs. This variable is separately interacted with Announcement and Implementation indicator variables.
Treatment (Risky Refi) – For 2020, the county's total amount of risky refinance loan sold to the GSEs divided by the county's total amount of conventional conforming home purchase and refinance loan sold to the GSEs. This variable is separately interacted with Announcement and Implementation indicator variables.
One-unit Permit Growth – The growth rate of the number of building permits for one-unit structures in the county, calculated as the change in one-unit building permits from t-1 to t (numerator) divided by the mid-point between t-1 and t (denominator).
Construction Employment Growth – The growth rate of the employment level of the residential construction workers in the county, calculated as the change in the employment level from t-1 to t (numerator) divided by the mid-point between t-1 and t (denominator). .
Average Construction Wage Growth – The growth rate of the average wage for the residential construction workers in the county, calculated as the change in the average wage from t-1 to t (numerator) divided by the mid-point between t-1 and t (denominator). .
Unemployment Rate – The county-level annual unemployment rate.
GDP Growth – The county-level annual GDP growth rate.
ln(Per Capita Income) – The natural log of the county-level per capita income in a given year.



Figure IA.1. Parallel Trends for Lender-Level Mortgage Application Volume

Panel A plots regression coefficients and their 95% confidence intervals from a lender-by-year-month-level OLS regression where the natural log of conventional conforming home purchase and refinance second-home mortgage application volume in dollars is regressed onto year-month indicator variables interacted with the lender-level GSE purchase cap policy treatment indicator variable that equals one if the proportion of the lender's 2020 non-primary home purchase and refinance loan amount sold to the GSEs relative to its total conventional conforming home purchase and refinance loan volume sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are defined in a similar manner and are interacted with year-month indicator variable interaction terms and are included as control variables. Year-month and lender fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the lender level. The vertical dotted lines mark the month in which the GSE purchase cap policy was announced and implemented, respectively. Panel B, C, and D present the plots for investment-property, risky, and safe mortgages, respectively. Data source: CHMDA.

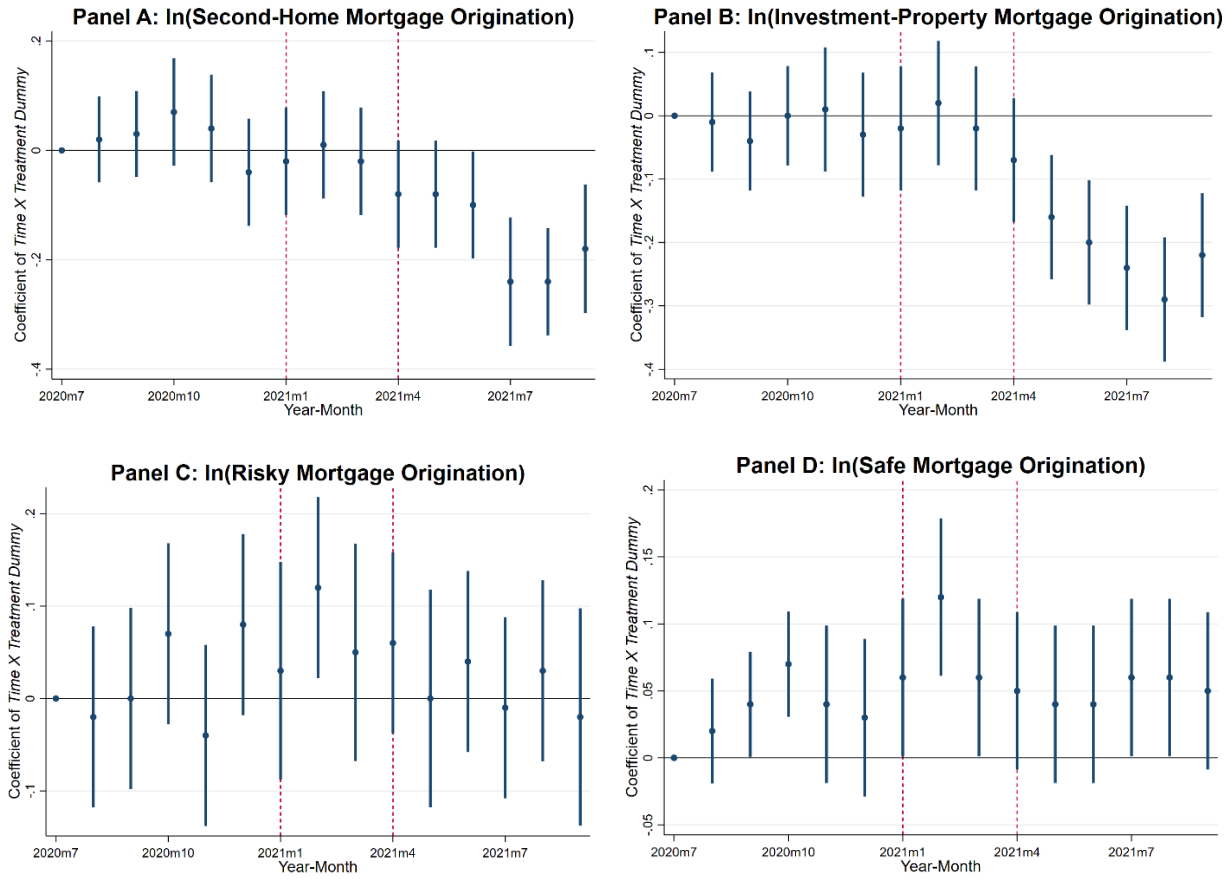


Figure IA.2. Parallel Trends for Lender-Level Mortgage Origination Volume

Panel A plots regression coefficients and their 95% confidence intervals from a lender-by-year-month-level OLS regression where the natural log of conventional conforming home purchase and refinance second-home mortgage origination volume in dollars is regressed onto year-month indicator variables interacted with the lender-level GSE purchase cap policy treatment indicator variable that equals one if the proportion of the lender's 2020 non-primary home purchase and refinance loan amount sold to the GSEs relative to its total conventional conforming home purchase and refinance loan volume sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are defined in a similar manner and are interacted with year-month indicator variable interaction terms and are included as control variables. Year-month and lender fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the lender level. The vertical dotted lines mark the month in which the GSE purchase cap policy was announced and implemented, respectively. Panel B, C, and D present the plots for investment-home, risky, and safe mortgages, respectively. Data source: CHMDA.

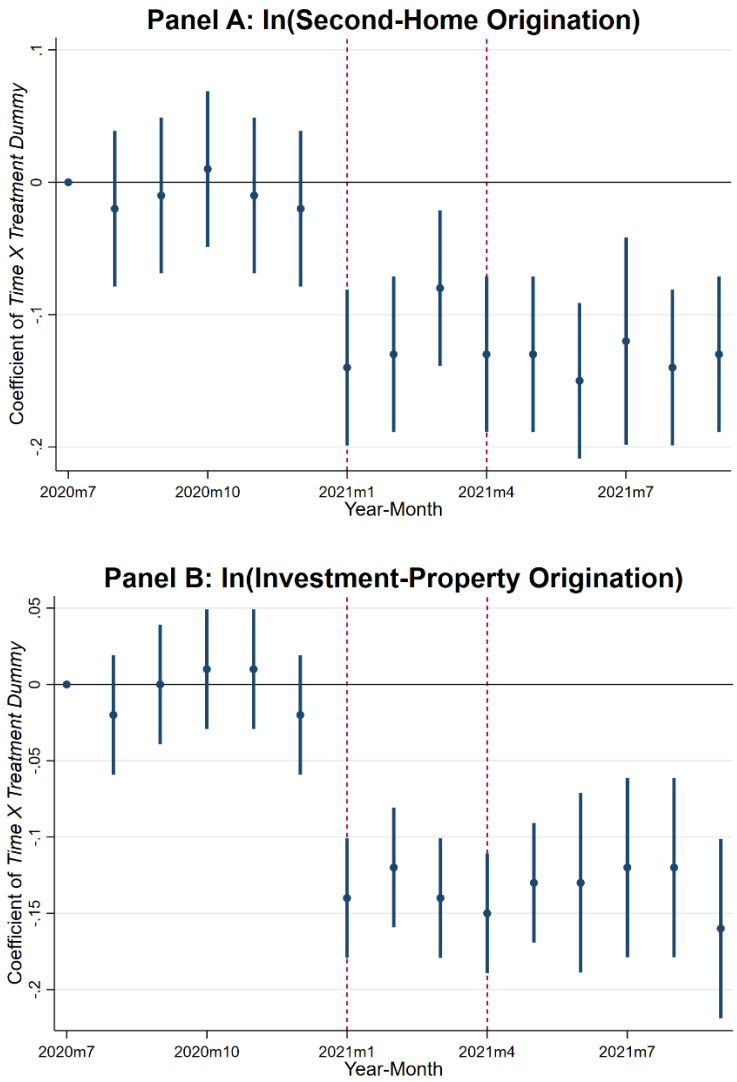


Figure IA.3. Parallel Trends for Lender-County-Level Mortgage Origination

Panel A plots regression coefficients and their 95% confidence intervals from a lender-county-year-month-level OLS regression where the natural log of conventional conforming home purchase and refinance second-home mortgage origination volume in dollars is regressed onto year-month indicator variables interacted with the lender-county-level GSE purchase cap policy non-primary mortgage treatment indicator variable which equals one if the lender-county cell's 2020 non-primary home purchase and refinance loan amount sold to the GSEs divided by its total conventional conforming home purchase and refinance mortgage sold to the GSEs in 2020 is greater than 7% and zero otherwise. Treatment (Risky Purchase) and Treatment (Risky Refi) are similarly defined, and both are included as control variables, along with their corresponding interaction terms with year-month indicator variables. Lender by year-month, county by year-month, and lender by county fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the lender level. Panel B presents the results for investment-home mortgages. The vertical dotted line marks the month in which the GSE purchase cap policy was announced and implemented. Data source: CHMDA.

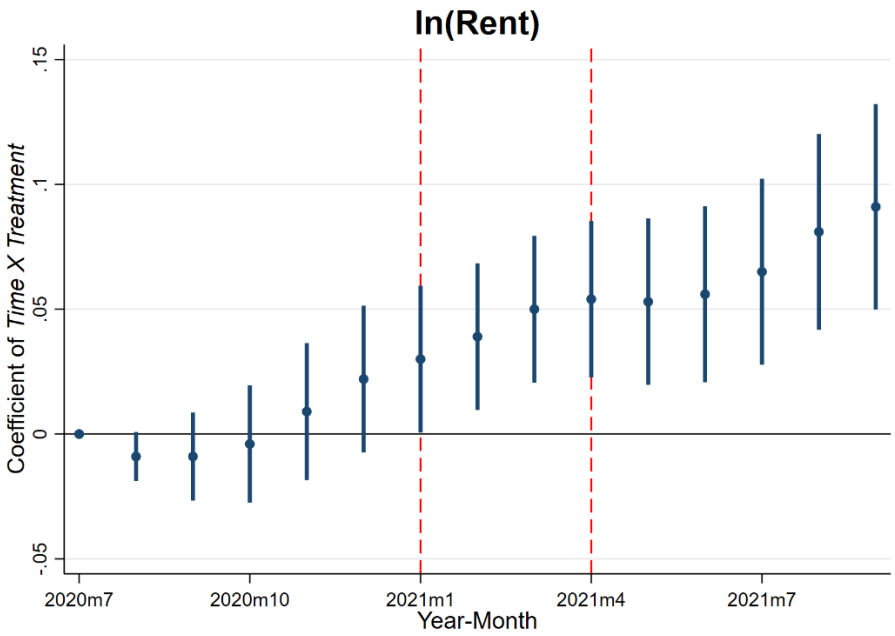


Figure IA.4. Parallel Trends for Rental Price

Figure IA.4 plots regression coefficients and their 95% confidence intervals from a zip-code-month-level OLS regression where the natural log of the rental price is regressed onto year-month indicator variables interacted with the zip-code-level GSE purchase cap treatment variable. The outcome variables are defined as described in Table 11. Treatment is a measure of each zip's exposure to the policy. The regression specification includes risky mortgage treatment controls, county-level COVID-19 controls, zip code fixed effects, and year-month fixed effects. The vertical dotted line marks the month in which the GSE purchase cap policy was announced. Heteroskedasticity-robust standard errors are clustered at the zip code level. Data source: Zillow.

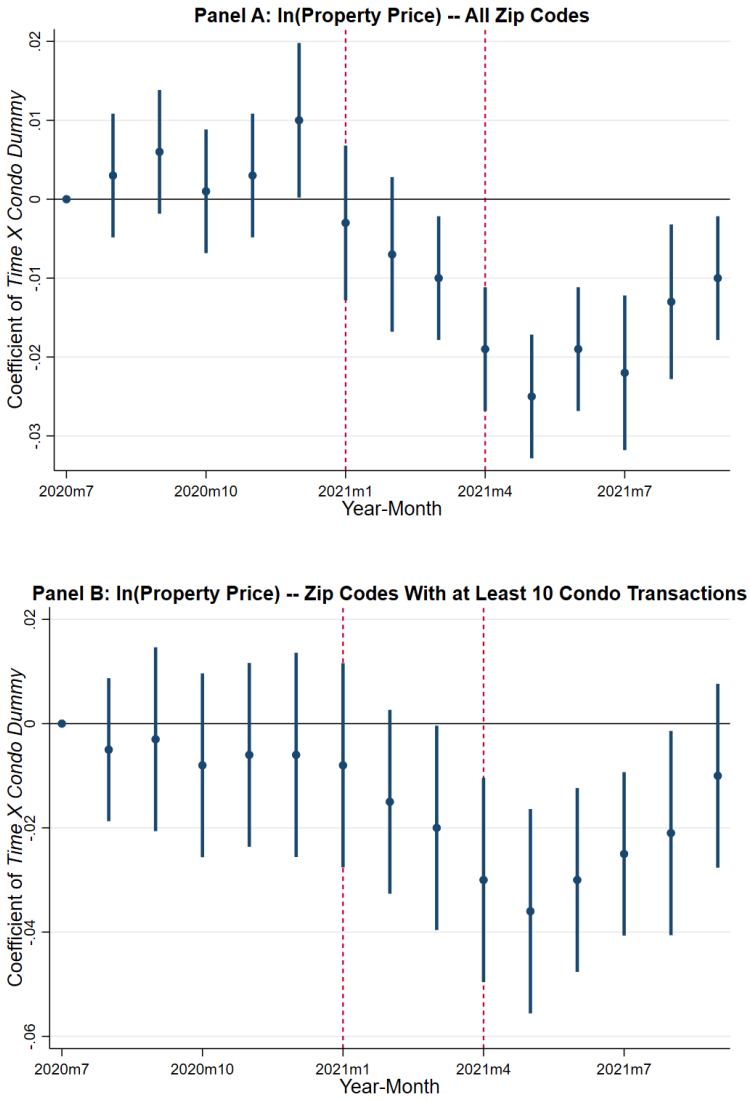


Figure IA.5. Property Price Parallel Trends

Figure IA.5 plots regression coefficients and their 95% confidence intervals from a transaction-level OLS regression where the natural log of the property transaction price is regressed onto year-month indicator variables interacted with the condo dummy, which equals one if the property in the transaction is a condo or an apartment and zero if it is a single-family house. The outcome variables and regression specifications for Panels A and B correspond to those presented in columns (2) and (4) of Table 13. Panel A uses the sample of zip codes with at least one condo transaction in any month from 2020Q3 to 2021Q3. Panel B uses the sample of zip codes with at least 10 condo transactions in all months from 2020Q3 to 2021Q3. The regression specification includes property characteristics, zip code times year-month fixed effects. The vertical dotted line marks the month in which the GSE purchase cap policy was announced. Heteroskedasticity-robust standard errors are clustered at the zip code level. Data source: CoreLogic.

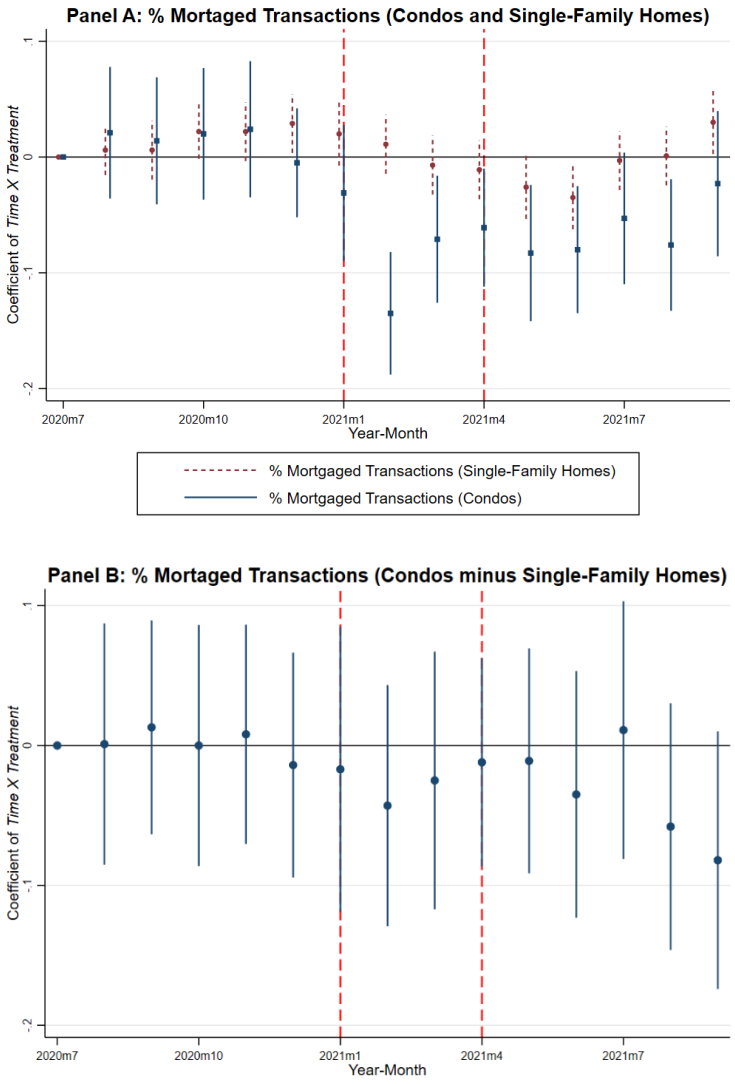


Figure IA.6. Parallel Trends for Property Transaction Financing

Figure IA.6 plots regression coefficients and their 95% confidence intervals from a zip-code-month-level OLS regression where the percentage of mortgaged transactions are separately regressed onto year-month indicator variables interacted with the zip-code-level GSE purchase cap treatment variable. The outcome variables are defined as described in Columns (1) – (3) of Table 14. Treatment is a measure of each zip code's exposure to the policy. The sample of zip codes with at least 10 condo transactions in all months from 2020Q3 to 2021Q3 is used when the outcome variable is % Mortgaged Transactions (Condos) in Panel A. The sample of zip codes with at least 10 single-family home transactions in all months from 2020Q3 to 2021Q3 is used when the outcome variable is % Mortgaged Transactions (Single-family Homes) in Panel A. The sample of zip codes with at least 10 condo transactions and 10 single-family home transactions in all months from 2020Q3 to 2021Q3 is used in Panel B. The regression specification includes risky mortgage treatment controls, county-level COVID-19 controls, zip code fixed effects, and year-month fixed effects. The vertical dotted line marks the month in which the GSE purchase cap policy was announced. Heteroskedasticity-robust standard errors are clustered at the zip code level. Data source: CoreLogic.

Table IA.1. Summary Statistics of Loan-Level Control Variables

This table presents summary statistics for mortgage application characteristics that are used in our loan-level regression analyses. The sample period is from 2020Q3 to 2021Q3. Dollar values are reported in nominal terms. Refer to the Internet Appendix for additional details on variable definitions. Data source: CHMDA.

	Mean	Median	S.D.	N
	(1)	(2)	(3)	(4)
Application characteristics				
Applicant Age	44.52	42	13.85	6,805,319
Female	0.59	1	0.49	6,805,482
Unknown Sex	0.08	0	0.27	6,805,482
Both Sexes	0	0	0.03	6,805,482
Hispanic	0.11	0	0.31	6,805,482
Unknown Ethnicity	0.16	0	0.37	6,805,482
Asian	0.1	0	0.3	6,805,482
Black	0.05	0	0.21	6,805,482
Other Minority	0.01	0	0.1	6,805,482
Unknown Race	0.17	0	0.37	6,805,482
Co-Applicant Indicator	0.42	0	0.49	6,805,482
Applicant Credit Score	758.53	768	41.98	6,794,163
Co-Applicant Credit Score	765.24	776	40.98	1,035,311
CLTV (%)	74.87	78.98	16.82	6,762,873
DTI (%)	33.99	34.88	9.7	6,782,478
Income (USD Thousands)	118.52	99	77.19	6,739,442
Loan Amount (USD Thousands)	305.57	278.91	149.92	6,805,480
AUS Approved	0.97	1	0.18	6,805,482
Home Purchase Mortgage	0.51	1	0.5	6,805,482