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Incomplete Pass-Through in Mortgage Markets*

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Abstract

This paper studies the May 2023 change in the conforming mortgage upfront guarantee fee schedule. Consistent with incomplete pass-through, lenders raise rejection rates and sell fewer loans to the GSEs when fees rise. For small-dollar mortgages (SDMs), pass-through is near zero and rejection rates are more sensitive to fee increases. This implies that the overall incomplete pass-through is partly driven by liquidity-constrained borrowers and that the inequality in mortgage access via higher rejection rates on SDMs is partly driven by lenders' inability to pass costs onto SDM borrowers. Without offsetting effects from fee cuts, fee hikes reduced aggregate mortgage origination in 2023 by 8%.

Keywords: Mortgages, pass-through, inequality

JEL Classification: G21, J1, D63

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1 Introduction

Many important policies can be summarized as changes in the marginal cost of mortgage origination. For example, moving Fannie Mae and Freddie Mac (the GSEs) out of conservatorship or tightening monetary policy can be thought of as an increase in such costs.[1] Through these lenses, understanding how lenders respond to changes in the marginal cost of mortgage origination can tell us a lot about how the mortgage market might react to potential policy changes. If lenders can fully pass on marginal cost increases to borrowers, possibly due to a highly inelastic market demand curve, then policies that raise such costs would have little deadweight loss. On the other hand, if pass-through is incomplete, and, in response, lenders maximize profit by cutting mortgage credit supply, then the deadweight loss would be much greater. These two scenarios have very different welfare implications, especially if lenders choose to cut credit supply in a way that exacerbates existing inequalities in housing access (Bhutta et al., 2021; Goldstein and DeMaria, 2022).

With these motivations, this paper studies how recent changes in the conforming mortgage upfront guarantee fee (g-fee) schedule affects the lending and securitization decisions of lenders. The upfront g-fee schedule is a menu of fees, determined by a credit score and loan-to-value (LTV) ratio grid. Upfront g-fees are quoted as percentages of the mortgage loan amount. We focus on a set of changes in the g-fee schedule that was implemented on May 1$^{\text{st}}$, 2023 and caused the base g-fee to increase for some borrowers and decrease for other borrowers. The Federal Housing Finance Agency's (FHFA) goals for the changes are to improve the GSEs' capital position and mortgage credit access for borrowers with lower credit scores.

The changes serve as a natural experiment that fits nicely into the one-shock, discrete treatment dosage levels, difference-in-differences regression research design (Callaway et al., 2024). Mortgage applications in g-fee cells that experience fee changes belong to the treatment group and the rest fall into the control group. We use the confidential Home Mortgage Disclosure Act (cHMDA) data set, a nationally representative data set of the U.S. mortgage market, to study this policy change. Our analysis uses a sample of 30-year fixed-rate home purchase and refinance mortgage applications to show how the fee changes impacted rejection probability, note rate, total upfront costs, and lender securitization behavior.

We first explore the impact that the g-fee changes have on the probability that a mortgage application gets rejected. For pass-through analysis in lending markets, application rejection probability is a useful object to study for two reasons. First, it captures the supply-side decision for how much credit

[1] Mortgages and mortgage-backed securities (MBS) guaranteed by the GSEs enjoy low funding costs because of the implicit guarantee from the US government. Removing the GSEs out of government conservatorship likely implies that the cost of the guarantee will increase and get passed on to lenders.

lenders are willing to supply. Second, in markets where the price of the good has a flow component (e.g., interest payments), studying supply-side quantity adjustments (e.g., rejection probability) is informative because researchers need to correctly price the flow component, in the present value sense, in order to determine whether the estimated pass-through is complete, which can be a difficult task.

We find that the rejection probability for mortgage applications in the fee hike treatment group, on average, increased by 0.36% to 0.51%. For the elasticity, we find that a 1 percentage point (pp) increase in g-fee causes the rejection probability to increase by 1.4%. These are large effects, given that the sample average upfront g-fee is approximately 50 bps and the average rejection probability is 4%. We do not find any statistically significant effects on the fee-cut treatment group. Since fee cuts are concentrated among low credit score borrowers, the null result is potentially driven by lenders' unwillingness to take on more credit risk. Using rejection reason data, we confirm that the rejection probability results are unlikely to be principally driven by marginal applications being pushed over important debt-to-income (DTI) ratio limits. Overall, the results from this set of analysis suggest that lenders cannot fully pass on marginal cost increases to borrowers and reduce mortgage credit supply by increasing rejection rates to equate marginal cost to marginal benefit.

Next, we turn our attention to the impact on note rate. We find positive pass-through for both the g-fee-increase and the g-fee-decrease treatment groups, with the treatment effect size of approximately 5 bps increase and decrease for the respective groups. We compute a pass-through rate of 10.7 bps for a 1 pp decrease in g-fee and a pass-through rate of 16 bps for a 1 pp increase in g-fee. Another margin of adjustment that lenders can make to offset g-fee changes is the total upfront loan cost (including discount points and lender credits). Similarly to the note rate results, we document positive pass-through of similar magnitudes (5 to 7 bps) for the treated loans and pass-through rates of 25.9 bps for g-fee decreases and 13.3 bps for g-fee increases. We use a buy-up multiplier of 3.25 (Kalda et al., 2025) to compute the all-in price and estimate a symmetrical average total pass-through rate of 0.65, which is consistent with incomplete pass-through.

The cHMDA data set also allows us to study lenders' securitization decisions, another margin of quantity adjustment. We find that banks and non-banks respond to increases in g-fees differently. In response to g-fee increases, banks are less likely to sell mortgages to the GSEs and substitute by selling more loans to non-GSE buyers and holding more loans on their balance sheet. On the other hand, non-bank securitization behavior is largely unchanged. The weak response from non-banks makes sense because they have limited balance sheet capacity. Intuitively, we find some weak evidence that non-banks exhibit higher g-fee pass-through rates to upfront loan cost, possibly due to their higher balance sheet cost. In line with the rejection probability results, we do not find any statistically robust evidence that lender securitization behavior changes in response to g-fee decreases. Importantly, the effects from securitization behavior indicate incomplete pass-through because complete pass-through would leave the relative attractiveness across different securitization channels unchanged.

Why is pass-through incomplete? We use the small-dollar mortgage (SDM), loan amounts less than or equal to $150,000, segment to argue that the incomplete pass-through that we document can be partly explained by liquidity-constrained borrowers. SDM borrowers, on average, have lower income, lower credit score, are younger, and are more likely to be Black. All of these are intuitive proxies of liquidity constraint. From the lender's perspective, passing costs onto these borrowers may, at the margin, increase the loan's default risk to the extent that originating the mortgage becomes unattractive. Therefore, in this segment of the mortgage market, lenders ration credit (Stiglitz and Weiss, 1981) instead of increasing mortgage credit price in response to g-fee hikes. Relatedly, lenders may know that liquidity-constrained borrowers are price sensitive such that they are unwilling or unable to take out mortgages at higher prices and, hence, with lower expected profits from incomplete pass-through, lenders cut credit supply to these segments.

We find strong supporting evidence for this mechanism. First, for g-fee increases, the treatment effect is relatively larger for SDM applications, compared to applications for larger mortgages. The rejection probability for SDMs in the g-fee-increase treatment group increased by an additional 0.7%. In term of the elasticity, for a 1 pp increase in the g-fee, SDM applications face an additional 3.8% increase in the rejection probability. Fee cuts do not heterogeneously impact SDM application rejection probability, which is consistent with lenders' lack of appetite for making more risky loans. Second, for mortgage pricing (note rate and upfront costs), for the fee-increase treatment groups, the treatment effect is not statistically different from zero. Furthermore, we find that, not surprisingly, the pass-through rates among SDMs are statistically smaller than those of larger mortgages. Taken together, the results are consistent with the idea that lenders cannot pass on marginal cost increases to SDM borrowers and, therefore, need to make larger credit supply cuts to equate marginal benefit to marginal cost.

The SDM results suggest that the endogenous response by lenders undermined the policy's redistributional goals. More generally, the results imply that policies that increase the marginal cost of mortgage origination may have undesirable welfare implications. Incomplete pass-through among larger mortgages imply that, when marginal costs increase, borrowers who can afford more expensive mortgages do not have to pay as much as in the complete pass-through counterfactual. On the other hand, a larger proportion of SDM applicants, who are less well-off, lose access to the mortgage market because lenders cut SDM supply. Therefore, on net, these policies will likely increase the inequality in mortgage and housing access.

We end the paper by estimating the aggregate effect that the g-fee changes had on mortgage credit supply. To do so, we aggregate the loan-level data up to the g-fee cell-month level and apply the same difference-in-differences identification strategy. After the fee changes were announced, mortgage origination in the g-fee-increase treatment cells fell by approximately $71 million USD (207 loans). After the fee changes were implemented, mortgage originations in the g-fee-increase treatment cells fell by approximately $118 million USD (373 loans). In line with the application-level results, we do not find any statistically robust result for the g-fee-decrease treatment cells. Using these estimates, we calculate that fee hikes caused aggregate mortgage origination to fall by 1.4%, without offsetting effects from fee cuts. Since the cuts are concentrated among riskier borrowers (e.g., those with lower credit scores and higher LTV ratios), it is possible that, even though fee reductions make lending in these segments more profitable, lenders are unwilling to make riskier loans. Lastly, the aggregate decrease in mortgage credit supply suggests that the fee changes may not have improved the GSEs' capital position.

The asymmetrical credit supply response to fee cuts and hikes has important implications for monetary policy and economic inequality. The results suggest that, through the direct effects of changes in marginal financing costs, inequality in mortgage credit and, hence, housing access may worsen with every monetary policy cycle. When monetary policy eases, the benefits may not accrue equally among borrowers as lenders may be less willing to allocate more credit to riskier borrowers (e.g., those with low credits score and high LTV ratios). When monetary policy tightens, lenders broadly cut credit supply for everyone, but the marginal effect is especially large for credit-constrained borrowers (e.g., SDM borrowers). Therefore, targeted policies that are unrelated to profit margins (e.g., the Community Reinvestment Act (CRA)) are required to address this dimension of economic inequality.

The rest of the paper is organized as follows. Section 2 reviews the relevant literature and outlines our contribution. Section 3 discusses relevant institutional details. Section 4 describes the data sets that we use and the sample of analysis. Section 5 explains and validates the identification strategy. Section 6 develops testable hypotheses. Section 7 presents the results, and Section 8 concludes.

2 Literature Review and Contributions

This paper contributes to several strands of literature. First, the paper contributes to the large literature on the effects of GSE subsidization on the mortgage market (McKenzie, 2002; Wallison and Calomiris, 2009; Loutskina and Strahan, 2009; Loutskina, 2011; Adelino et al., 2012; Jeske et al., 2013; Elenev et al., 2016; DeFusco and Paciorek, 2017; Richardson et al., 2017; Gete and Zecchetto, 2018; Amornsiripanitch et al., 2025). Specifically, the paper speaks to the body of work that studies cost pass-through in the conforming mortgage market and the effects that changes in the price of the GSE subsidy have on mortgage application outcomes. To our knowledge, this literature generally concludes that pass-through is complete (Alexandrov et al., 2022; Ahsin, 2024; Fuster et al., 2024). We contribute to this line of work in several ways. Using data on lenders' loan approval and securitization decisions, we provide strong evidence that pass-through is incomplete, which has important welfare implications for the effects that policy changes have on access to mortgage credit and housing, especially when credit supply effects are asymmetrical with respect to fee changes. In turn, we are also the first to document how changes in the marginal cost of selling mortgages to the GSEs affect lenders' loan approval and securitization decisions.

There are two closely related papers that warrant a discussion. Kim et al. (2025) use similar data sets to study how the May 2023 g-fee changes affect home purchase mortgage origination, with a focus on the distributional consequences across borrowers with different levels of income and credit score. Using a counterfactual exercise, they find that, surprisingly, the GSEs' cross-subsidy toward lower credit score borrowers can have regressive effects along the income gradient. Our paper is orthogonal to this work in several ways. We focus on testing whether cost pass-through is complete and studying its consequences on mortgage credit access. Furthermore, we use the g-fee change experiment to explain why small-dollar mortgages face higher rejection probabilities. Ultimately, we see the two papers as complements.

Kalda et al. (2025) use the intersection of Fannie Mae data and the public version of HMDA data to estimate the pass-through of changes in upfront g-fee to mortgage note rate and upfront costs. They document complete pass-through for fee hikes but incomplete pass-through for fee cuts. While Kalda et al. (2025) make an important first step in understanding cost pass-through in the conforming mortgage market, our paper improves upon this work in several important ways. First, with access to detailed data on quantity adjustments (e.g., approval/rejection and securitization behavior), we use our quantity adjustment results to argue that cost pass-through in the conforming mortgage market is incomplete. Second, not being limited to the intersection of Fannie Mae data and the public version of HMDA data ($\approx$ 85,000 loans), our findings are more generalizable and we are able to quantify the aggregate effect that the policy has on the mortgage market. Third, we use the estimator proposed by Callaway et al. (2024), which, in a setting where the treatment variable is not binary, does not suffer from the same problems that the standard two-way fixed effects estimator does: the coefficient estimates are often biased and the estimates are not interpretable. Fourth, we use the g-fee change experiment to answer important open questions regarding differences in mortgage access across the loan size distribution and demographic groups.

This paper contributes to the growing literature on small-dollar mortgages. Brevoort (2022) and Goldstein and DeMaria (2022) show that small-dollar mortgages have higher rejection rates, interest rates, and upfront costs, relative to larger mortgages. This feature of the mortgage market is of great policy concern because it implies that the barrier to homeownership and cheaper financing via refinancing is especially high for minority and low-income borrowers (McCargo et al., 2019), who tend to disproportionately apply for SDMs. An open question in the small-dollar mortgage literature is: what are the drivers behind these pattern (Horowitz and Roche, 2023)? We use the g-fee change experiment to show that a large part of the rejection rate wedge can be explained by low cost pass-through, which implies that, for a given size of marginal cost increase, lenders make larger quantity reductions (e.g., higher rejection rates). D'Acunto and Rossi (2022) shows that increases in regulatory burden via the Dodd–Frank Wall Street Reform and Consumer Protection Act caused the loan size distribution of originated mortgages to shift rightward. Our findings suggest that the shift is caused by lenders' inability to sufficiently pass on the increased regulatory burden.

This paper also adds to the literature on disparity in mortgage access with respect to borrowers' demographics. Munnell et al. (1996), Bhutta et al. (2021), Bhutta and Hizmo (2021), Bartlett et al. (2020), and Mota et al. (2023) show that Black and Hispanic borrowers face higher rejection rates, note rates, and upfront costs. Using similar methods, Amornsiripanitch (2023) documents that rejection rate and loan cost rise with borrower's age. In the current paper, we use the g-fee change experiment to test whether these stylized facts can be explained by differences in pass-through and the accompanying rejection rate adjustments. We find null results for race, ethnicity, age, and gender. The null results can be interpreted as empirical evidence that rules out differences in pass-through via market segmentation, as can be identified by the g-fee change shock, as a potential mechanism for the well-documented disparity in mortgage access with respect to borrowers' demographics.

Lastly, we contribute to the larger economics literature that estimates and studies cost pass-through in different markets by using supply-side quantity adjustments to provide evidence for incomplete pass-through.[2] Most pass-through papers study commodity markets where the researchers observe price and quantity sold, the latter being an equilibrium object that is jointly determined by both the demand and supply side. Therefore, researchers must relying on estimates of changes in the equilibrium price to determine whether pass-through is complete. In this paper, we also observe lenders' decisions to extend or restrict credit supply, which, assuming that there is no change in borrowers' credit quality, purely reflects the supply-side response to changes in the marginal cost of lending. In commodity markets such as gas (Genakos and Pagliero, 2022), this would be equivalent to observing how much gas gas stations are willing to buy and store for potential customers. In the context of credit products such as mortgages, being able to observe supply-side adjustment is useful because the product's price has a flow component (e.g., interest payments), which makes it difficult to use price changes alone to interpret whether the estimated pass-through is complete.

3 Institutional Details

Mortgage backed securities (MBS) are bundles of mortgages that are sold to investors on the secondary market. Most MBSs are sold with guarantees that protect investors from losses in the case that mortgages within the MBS pool default. MBSs sold by the GSEs are one example of MBSs that carry this type of guarantee.

For the GSEs, guarantee fees are the primary source of revenue and must cover the total cost of securitization, which includes administrative cost and the cost of capital that covers losses from mortgages in the MBS pool.[3] Guarantee fees come in two forms: upfront and ongoing. Upfront g-fees, the focus of this paper, are paid by the seller when they send a mortgage to a GSE. Ongoing g-fees are monthly payments that the seller makes to the GSE that purchased the associated mortgage. Both forms of g-fee are typically passed on to borrowers in the form of higher interest rates(Bosshardt et al., 2023) and/or upfront loan origination costs.

G-fees have increased significantly over the past decade due to changes in regulatory requirements for the GSEs. The Enterprise Regulatory Capital Framework (ERCF), introduced in 2021, has the goals of (1) strengthening the safety and soundness of the housing finance system, (2) ensuring the GSEs fulfill their statutory goals, and (3) aligning pricing with expected financial performance and risks of the underlying mortgages. These goals can be achieved by increasing aggregate capital requirements and/or

[2]See Williams et al. (2014) for a summary and synthesis of the literature.
[3]See `https://www.fhfa.gov/policy/guarantee-fees`.

increasing capital requirements for certain groups of mortgages, classified by their observable credit risk characteristics. Compared to the prior regulatory framework, the Conservatorship Capital Framework (CCF), the ERCF has significantly higher capital requirements that required a "recalibration of upfront [g-fees]... to align the fee structure more closely with the risk factors utilized in ERCF and the level of capital required by ERCF."[4] Thus, the ERCF directly led to changes in the levels and distribution of the g-fees.

This paper focuses on a set of changes in the upfront guarantee fees charged by the GSEs as a result of the ERCF.[5] These g-fees, also called Loan-Level Pricing Adjustments (LLPAs), generally apply to conforming mortgages with loan terms greater than 15 years and up to 30 years. These loans are charged a base LLPA that varies by the credit score and LTV ratio combination at origination.[6] ERCF aims to increase the GSEs' capital requirements by aligning capital requirements with mortgages' expected financial performance and risk. In doing so, the ERCF created a credit-risk weight floor such that lower-risk attributes (i.e., low LTV and high credit score) saw relative increases in the minimum capital required per loan compared to high-risk attributes (i.e., high LTV and low credit score). The changes in relative capital requirements translated to the changes in upfront g-fees. The g-fee changes ranged from a decrease of 75 basis points to an increase of 200 bps of the loan amount. The full set of changes across credit-score-LTV combinations are summarized in Table 1. The g-fee changes were first announced by the FHFA on January 19$^{\text{th}}$, 2023.[7] The exact change to the fee schedule was announced by Fannie Mae on March 22$^{\text{nd}}$, 2023.[8] The changes were implemented on May 1$^{\text{st}}$, 2023.

The g-fee that the GSEs charge on any loan is determined by the settlement date for the targeted MBS pool and not the date that the loan was originated. For example, if a g-fee increase is scheduled to begin on May 1$^{\text{st}}$, 2023, then lenders will pay the higher fee on any mortgages sold into MBS pools with settlement dates of May 1$^{\text{st}}$ or later. An MBS with settlement date of May 1$^{\text{st}}$ will be composed of mortgage loans that were originated prior to the settlement date (May 1$^{\text{st}}$). This is why changes in g-fees are often announced a few months in advance. The time between announcement and implemen-

[4]See `https://www.fhfa.gov/sites/default/files/2024-05/GFee-Report-2022.pdf`. The Conservatorship Capital Framework (CCF) was established in 2017. It also increased capital requirements for the GSEs, among other things, with the goal of setting the GSEs up to exit conservatorship. See `https://www.fhfa.gov/document/fact-sheet-proposed-rule-on-enterprise-capital`.

[5]There were also g-fee increases for high-balance loans and second homes (implemented in April 2022) under the ERCF. These changes are not the focus of our analysis. In order to avoid confounding effects from the April 2022 change, we exclude second- and investment-home loan types from our sample of loans.

[6]The base LLPA can be augmented (increased) by additional risk-based factors (e.g., whether the loan is for cash-out refinance, is above the super-conforming limit, is an investment property, etc.). See `https://singlefamily.fanniemae.com/media/9391/display`. We also drop loans that are subjected to these adjustments from our sample.

[7]See `https://www.fhfa.gov/news/news-release/fhfa-announces-updates-to-the-enterprises-single-family-pricing-framework`.

[8]See Lender Letter LL-2023-01. `https://singlefamily.fanniemae.com/media/33241/display`.

tation allows lenders to make adjustments to their pricing schedules in anticipation of the fee schedule change. Loans originated right after the announcement of a change in the g-fee pricing schedule are likely securitized before the implementation date of the g-fee change and unaffected by the anticipated fee changes. In contrast, loans originated closer to the implementation date are likely to reflect the new pricing schedule since those loans will likely be securitized after the implementation date. More generally, the share of originated loans that account for the new pricing schedule change should increase the closer the origination date gets to the date of implementation of a g-fee change. Therefore, the identification strategy that we outline below is designed to capture both the announcement and the implementation effects.

4 Data

4.1 Data Sources

This paper mainly uses data from one source, which is the confidential version of the Home Mortgage Disclosure Act (cHMDA) data set. The data set is an application-level data set that contains detailed information on mortgage characteristics (e.g., loan amount, loan purpose, etc.), applicant characteristics (e.g., age, gender, race, ethnicity, etc.), and mortgage application outcomes (e.g., approved or rejected). For applications that were approved, but for which the mortgage may or may not have been originated, we observe the note rate that was offered. For originated mortgages, we observe the total upfront cost, points purchased, and securitization decisions; that is, whether the loan was sold and to which purchaser type (e.g., GSE or non-GSE), if at all. For the analyses presented below, we use the 2022 and 2023 vintages of cHMDA. Lender type (e.g., banks and non-banks) data was collected from the 2022 Avery File.[9]

One advantage that the cHMDA data set has for studying quantity and price adjustments to changes in marginal costs is that we can separately observe changes in borrower behavior (the demand side) and lender behavior (the supply side) in response to the shock, not just the equilibrium price and quantity outcomes. On the demand side, we can observe whether consumers apply for mortgages and, conditional on getting approved, whether the borrower decides to pursue financing. On the supply side, we can observe lenders' approval/rejection decisions. For example, when marginal cost increases cannot be fully passed onto consumers, suppliers choose to supply fewer units and, with a positive pass-through

[9] Available on Neil Bhutta's website. See `https://sites.google.com/site/neilbhutta/data`.

rate, the consumers demand fewer units because of the price increase (Weyl and Fabinger, 2013). The cHMDA data set allows us to explicitly observe and test for the supply side response, instead of just observing the decrease in total market-level quantity sold.

4.2 Sample Construction

We construct a sample of first-lien, single-unit, conforming, 30-year, fixed-rate home purchase and simple refinance mortgages backed by primary residences that have no special features.[10] Following the g-fee schedule presented in Table 1, we keep mortgages that have LTV values between zero and 97%, inclusive, and mortgages where the applicant's credit score is between 620 and 850, inclusive. To ensure that the mortgages are indeed conforming, we drop mortgage applications that were flagged by the automated underwriting system (AUS) as non-conforming (Bhutta et al., 2021; Amornsiripanitch, 2023) and mortgage applications that have DTI values greater than 45% or CLTV values greater than 97%.[11] We also dropped high-balance mortgage applications and mortgage applications that were associated with manufactured homes because these two groups of mortgages received a g-fee change shock beyond the ones presented in Table 1.[12] Lastly, for the regression analyses presented below, we keep mortgage applications that were processed (e.g., approved or denied) between July 2022 and December 2023.[13]

The final regression sample contains approximately 1.66 million mortgage applications. Summary statistics of key variables are presented in Panel A of Table 2. Loan pricing variables (e.g., note rate and upfront cost) have fewer observations because they are only defined for approved applications and originated loans. Securitization variables (e.g., sold to GSE and sold to non-GSE) have even fewer observations because they are only defined for originated mortgages. The non-bank variable is not defined for every observation because we use the 2022 vintage of the Avery File to classify lenders and so lenders that appear in 2023 but not in 2022 cannot be classified. Using the application-level sample,

[10] Special features include balloon payments, interest-only payments, non-standard amortization features, and prepayment penalties. All results hold if we exclude simple refinance mortgage applications from the sample.

[11] The DTI and CLTV filters follows Fannie Mae's Eligibility Matrix. See `https://singlefamily.fanniemae.com/media/20786/display`. Fannie Mae's Lender Letter LL-2023-06 states that DTI ratio-based LLPA updates were eliminated from the May 2023 change. See `https://singlefamily.fanniemae.com/media/36061/display`. The g-fee schedule change originally wanted to increase the upfront fee for mortgages with DTI values greater than 40% but this change was rescinded. See `https://www.fhfa.gov/news/news-release/fhfa-announces-rescission-of-enterprise-upfront-fees-based-on-debt-to-income-dti-ratio`. All results are robust to dropping applications with DTI values greater than 40% or including a set of DTI-LTV grid by year-month fixed effects to absorb the potential effect of the rescinded fee increases.

[12] High-balance loan is defined as a loan that is tied to a property located in a high-cost area with a loan amount that is greater than the national conforming loan limit but is lower than the high-cost area conforming loan limit. For the g-fee change for high-balance loans, see page 3 of Fannie Mae's LLPA Matrix. `https://singlefamily.fanniemae.com/media/9391/display`.

[13] We exclude the first half of 2022 to avoid potential contamination from the change in upfront g-fee for second- and investment-home mortgages. See `https://www.nahb.org/blog/2022/01/fhfa-to-impose-hefty-upfront-fees-on-second-home-purchases/`.

we also construct another sample where we aggregate application and origination volumes, measured in dollars and number of applications or loans, up to the cell-year-month level. Summary statistics of key variables are presented in Panel B of Table 2.

5 Methodology

5.1 Average Treatment Effect on the Treated

We use the one-shock difference-in-differences research design with discrete treatment dosage levels to estimate the causal impact of g-fee changes on the outcomes of interest. The two sets of fixed effects are: the g-fee cell fixed effects and the year-month fixed effects. Mortgages that fall into cells that experience g-fee changes belong to the treatment group and the rest belong to the control group. The new g-fee schedule was announced in January 2023 and implemented in May 2023. Therefore, the pre-period is July 2022 to December 2022, the announcement period is January 2023 to April 2023, and the implementation period is May 2023 to December 2023. Following Callaway et al. (2024), we estimate variants of the following application-level ordinary least squares (OLS) regression:

$$
\begin{aligned}
Y_i = \alpha &+ \beta_1(\Delta G < 0)_c \times \mathit{Announcement}_t \\
&+ \beta_2(\Delta G < 0)_c \times \mathit{Implementation}_t \\
&+ \beta_3(\Delta G > 0)_c \times \mathit{Announcement}_t \\
&+ \beta_4(\Delta G > 0)_c \times \mathit{Implementation}_t \\
&+ \gamma' \mathbf{x_i} + \mu_\mathbf{c} + \nu_\mathbf{t} + \epsilon_i.
\end{aligned} \tag{1}
$$

i indexes mortgage applications, c indexes g-fee cells, and t indexes year-months. The cell fixed effect is denoted by μ_c and the year-month fixed effect is denoted by ν_t. $\gamma' \mathbf{x_i}$ is the vector of standard application-level control variables (Bhutta et al., 2020, 2021; Bhutta and Hizmo, 2021; Amornsiripanitch, 2023; Amornsiripanitch et al., 2025). For robustness, we include lender-by-time and census tract fixed effects to account for differences across lenders (e.g., business opportunity and lending standard) and markets.[14] Refer to Appendix Section A.1 for more detail on how the control variables enter the regression equation. Standard errors are clustered at the g-fee cell level.

[14] All results are robust to the inclusion of county-by-time fixed effects, which account for time-varying local economic condition. We cannot include tract-by-time fixed effects because we lose too many singleton observations (Correia, 2015).

($\Delta G < 0$) equals one for applications that belong to g-fee cells that experience a g-fee cut on May 2023 and zero otherwise. ($\Delta G > 0$) equals one for applications that belong to g-fee cells that experience a g-fee hike and zero otherwise. Announcement equals one for January 2023 to April 2023 and zero otherwise. Implementation equals one for May 2023 onward and zero otherwise. β_1 gives the announcement-period average treatment effect for g-fee cut treatment group, relative to the control group. In the notation used by Callaway et al. (2024), this is the object ATT^0. The remaining β coefficients capture their respective average treatment effects.[15] The β coefficients in equation 1 can be interpreted as causal parameters if the parallel trend assumption holds. Throughout the paper, we present event study plots that support the assumption.

5.2 Identifying Assumptions

A potential concern with the proposed estimation method is whether the shock was anticipated by lenders and borrowers. If the shocks were anticipated and market participants changed their behavior in systematic ways, then we cannot say that the β coefficients are good estimates of the ATT^0. To test for anticipatory behavior, we can use the cell-year-month-level sample to estimate the following event study regression:

$$\begin{aligned} Y_{ct} = \alpha &+ \sum_{t}^{T} \beta_t^- (\Delta G < 0)_c \times Month_t \\ &+ \sum_{t}^{T} \beta_t^+ (\Delta G > 0)_c \times Month_t \\ &+ \mu_{\mathbf{c}} + \nu_{\mathbf{t}} + \epsilon_{ct}. \end{aligned} \tag{2}$$

c indexes g-fee cells, and t indexes year-months. $Month_t$ is an indicator variable for each year-month in the sample except for July 2022, which is the reference month. The β_t^- and β_t^+ coefficients are the event study coefficients for the g-fee cut and hike treatment groups, respectively. Figure 1 presents the regression results for when the outcome is the application volume, measured in millions of USD. The application volume is calculated as the sum of loan amounts on all applications submitted in each

[15]First-time homebuyers with low-to-medium income (LMI) are exempted from paying upfront g-fees (Kim et al., 2025). We cannot identify first-time homebuyers in cHMDA, which means that some untreated applications will be incorrectly assigned to the treatment group. This incorrect assignment biases the result against finding a statistically significant effect, implying that the estimates we present below are lower bounds. All results are robust to dropping LMI applicants or to the inclusion of a flexible set of control variables that aim to capture this exemption, i.e., separately, income bins interacted with g-fee cell fixed effects, income bins interacted with year-month fixed effects, age group indicator variables from Amornsiripanitch (2023) interacted with g-fee cell fixed effects, and age groups indicator variables interacted with year-month fixed effects.

g-fee cell by month block. Application date is used to compute this variable. We can see that there is no statistically significant pre-announcement pre-trend in application volume for either treatment group, which is consistent with the notion that, before the new fee schedule was announced, borrowers were unaware of the shock.

We can perform a similar exercise to check for anticipatory behavior on the supply side. Figure 2 presents the regression results for when the outcome is the processing volume, measured in millions of USD. Processing volume is calculated as the sum of loan amounts on all applications that lenders made decisions (approved or rejected) on in each g-fee cell by month block. The action date is used to compute this variable. Reassuringly, there is no statistically significant pre-announcement pre-trend in application volume for either treatment group, which supports the notion that, before the new fee schedule was announced, lenders were unaware of the shock.

Another important identifying assumption is that units in the sample cannot or did not manipulate that treatment status in such a way that is systematically related to the outcomes of interest. For example, it should not be the case that more sophisticated applicants, who possibly are of higher unobservable credit quality, systematically manipulate their credit-score-by-LTV-ratio combination in order to shift their applications into more favorable cells. Columns 1 and 2 of Table A1 present suggestive evidence that this sort of manipulation is not happening in the sample. Following equation 1, we regress observable applications' credit risk characteristics (e.g., DTI, income) onto the g-fee shock variables and we do not find that these characteristics systematically changed with respect to the shocks.[16]

A related change in consumer behavior that may threaten our identification strategy is that consumers may systematically shop more or less intensely with respect to the changes in g-fees. Such behavior may bias note rates and total upfront loan costs downward for mortgage applications that experience fee hikes and upward for mortgage applications that experience fee cuts (Mota et al., 2023). If this type of behavior is present in the data, we would expect to find that, on average, there are more unaccepted offers in fee hike cells and fewer unaccepted offers in fee cut cells. Table A2 presents suggestive evidence that this sort of behavior is not happening in the sample. Following equation 1, we regress an indicator variable that equals one hundred for mortgage applications that were approved but not originated onto the g-fee shock variables and we do not find any statistically significant result.

[16] We do not use LTV and credit score as the outcome variables because the g-fee shock is a function of LTV and credit score.

5.3 Average Total Pass-Through Rate

To estimate the average total pass-through rate of the changes in upfront g-fee to note rate and total upfront cost, we first estimate variants of the following OLS regression:

$$\begin{aligned} Y_i = \alpha &+ \sum_{d}^{D} \beta_d^A (\Delta G = d)_c \times Announcement_t \\ &+ \sum_{d}^{D} \beta_d^I (\Delta G = d)_c \times Implementation_t \\ &+ \gamma' \mathbf{x_i} + \mu_{\mathbf{c}} + \nu_{\mathbf{t}} + \epsilon_i. \end{aligned} \tag{3}$$

d indexes g-fee change shock dosage levels shown in Table 1 (e.g., -2%, -1.75%, ..., +0.5%, +0.75%). Using the estimated β coefficients, we can calculate the average total pass-through rate for fee hike and fee cut treatment groups during the implementation period, r^+ and r^- respectively, as:

$$\widehat{r^+} = \sum_{d^+}^{D^+} \left(\frac{\widehat{\beta^I}_{d^+}}{d^+} \right) \times \widehat{\mathbb{P}}(\Delta G = d^+ | \Delta G > 0), \tag{4}$$

$$\widehat{r^-} = \sum_{d^-}^{D^-} \left(\frac{\widehat{\beta^I}_{d^-}}{d^-} \right) \times \widehat{\mathbb{P}}(\Delta G = d^- | \Delta G < 0). \tag{5}$$

The plus and minus sign superscripts on the dosage levels d indicate that the sets of dosages differ for r^+ and r^-. $\widehat{\mathbb{P}}(\Delta G = d^+ | \Delta G > 0)$ is the within-sample weight of each treatment group d among the observations that belong to the treatment group that experienced a fee hike. $\widehat{\mathbb{P}}(\Delta G = d^- | \Delta G > 0)$ is the analogous object for the fee cut treatment group. Therefore, the average total pass-through rate is the weighted average of the scaled regression coefficients.[17] The same method can be used to compute the average total elasticity between g-fee and rejection probability. If we assume that pass-through is symmetrical, then we can estimate the average total pass-through rate across all treated observations, $(\Delta G \neq 0)$, as:

$$\widehat{r} = \sum_{d}^{D} \left(\frac{\widehat{\beta^I}_{d}}{d} \right) \times \widehat{P}(\Delta G = d | \Delta G \neq 0). \tag{6}$$

[17] This computation follows our conversation with the authors of Callaway et al. (2024).

One concern with this estimation method is that the estimated pass-through rates may be biased by the selection process that determines which applications become originated mortgages. If, with respect to the shock, mortgage applications that reached the origination stage are much riskier or safer than mortgage applications that did not reach the origination stage, then the results are biased estimates of the pass-through rates of interest. Columns 3 through 6 of Table A1 present suggestive evidence that selection with respect to the g-fee change shocks is minimal in the approved application sample, which we use to estimate the pass-through rate to note rate, and in the originated loan sample, which we use to estimate the pass-through rate to upfront cost. Specifically, we do not find strong evidence that observable credit risk characteristics of approved and originated loans changed significantly in response to the shocks.

6 Hypothesis Development

The pass-through literature (Auer and Chaney, 2009; Frankel et al., 2012; Alexandrov, 2014) provides the economic intuition that, if pass-through is incomplete, holding fixed product quality, profit-maximizing suppliers make quantity adjustments to equate marginal cost to marginal benefit in response to changes in marginal cost from sources such as taxes or subsidies. In the current context, this simple intuition yields the following null hypotheses that we test using the methodology outlined in Section 5.

Null Hypothesis 1 – *When upfront g-fee increases (decreases), lenders do not change mortgage application rejection rates.*

If lenders are able to fully pass on increases in marginal cost to borrowers, then only the price of the loan should adjust and there would be no firm-level quantity adjustment in the form of the amount of credit that the lender is willing to supply because the lender's marginal profit would remain constant. Approval/rejection decisions are solely made by the lender and, as such, any changes in these decisions can be interpreted as pure supply-side adjustments in response to the fee changes.[18]

Null Hypothesis 2 – *When upfront g-fee increases (decreases), lenders do not change their securitization behavior.*

Similarly to the economic reasoning behind the first hypothesis, complete pass-through rate implies that lenders' marginal profits remain the same after the g-fee schedule change and, therefore, there is no

[18]This hypothesis assumes that the credit quality of the application pool does not change with respect to the treatment, which we provide some supporting evidence for in Section 5.2.

need for them to adjust their securitization behavior. For example, when g-fees increase, lenders do not need to substitute for selling mortgages to the GSEs by selling mortgages to non-GSE entities and/or holding more mortgages on their balance sheet. We interpret rejections of the two null hypotheses as strong indications that pass-through is incomplete.

In the sections below, for completeness, we estimate the pass-through rates via note rate and upfront loan costs. However, it is tricky to determine whether the estimated pass-through rate via note rate is equivalent to the complete pass-through rate because interest payments are a stream of future cash flow, which begs the question of – what discount rate or present value multiplier should be applied to the stream of future cash flow such that the lender would be indifferent between selling the cash flow stream for its present value today and holding onto the loan and collecting future payments? One way to estimate this present value multiplier, also called the "buy-up" multiplier (Kalda et al., 2025), is to back it out using the spread between MBS pool pricing in the to-be-announced (TBA) market (Fuster et al., 2013). To perform this calculation, we need to assume which MBS pool each mortgage will be sold into and when so that we can use the correct TBA market price in the calculation. These assumptions make the resulting multiplier noisy, especially in fast-changing interest rate environments such as 2022 and 2023.[19] Therefore, we do not explicitly test null hypotheses that are directly related to the pass-through rates that we estimate, but, instead, we rely on null hypotheses related to lenders' quantity adjustments to determine whether pass-through of changes in marginal cost is complete.

7 Results

7.1 Pre-policy Analysis

As stated above, the new fee policy aims to improve the GSEs' capital position and facilitate access to mortgage credit for certain disadvantaged borrowers. In this section, we use the pre-policy distribution of mortgage applications across the upfront g-fee grid to evaluate how the fee changes might accomplish this goal. To conduct this descriptive analysis, we use the sample of mortgage applications submitted between January 2022 and December 2022 and that survive the filters described in Section 4. We exclude 2023 data to avoid any potential endogenous response on the demand side that may shift the distribution

[19] The FHFA uses an object called the present value multiplier (PVM) to annualized upfront fees. "For example, a loan with an upfront fee of 72 basis points and a PVM of 6 would have an annualized upfront fee of 72/6 = 12 basis points." Using this benchmark, a researcher can say that, given a PVM of 6, an estimated pass-through rate of upfront g-fee to note rate of 12 bps is considered to be equivalent to complete pass-through. See footnote 4 of `https://www.fhfa.gov/sites/default/files/2024-05/GFee-Report-2022.pdf`. The PVM also needs to be estimated and suffers from noise much like the buy-up multiplier.

of applications across g-fee cells. Each mortgage application is assigned a change in upfront g-fee based on its location on the g-fee schedule presented in Table 1. Fee changes can be converted from basis points to dollar amounts by multiplying the g-fee shock to the requested loan amount. It is important to note that this exercise assumes that the fee changes are fully passed on to borrowers, which may not be true, and abstracts away from endogenous demand- (e.g., changes in application decisions) and supply-side (e.g., changes in lending decisions) responses to the policy shock.[20] Nonetheless, this exercise is helpful in elucidating what policymakers may have, ex-ante, expected to result from the fee changes, which we can then later use to compare to what we find ex-post, presented below. It is also important to note that the sample of analysis consists only of a particular type of 30-year fixed-rate mortgages. However, since the GSEs' mortgage portfolios and the U.S. mortgage market are dominated by this type of mortgage, we believe that the results presented here should be representative to a first-order approximation.[21]

To evaluate the policy's goal of improving the GSEs' capital position, we take all originated loans in the 2022 sample and compute, for each loan, the pre-policy g-fee and the change in upfront g-fee. Then, we sum across all observations and compare the two amounts. We find that the policy would increase the GSEs' upfront g-fee revenue by approximately 6.2%. This number assumes that every originated conforming mortgage in the sample will eventually be sold to the GSEs. This assumption is not important as the percentage increase in fee revenue is similar if we instead use the sample of loans that were sold to the GSEs. Overall, this naive ex-ante analysis suggests that the fee changes are consistent with the FHFA's goal to increase the GSEs' upfront g-fee revenue and improve their capital position.

To evaluate the policy's re-distributional goals, we compute summary statistics of the g-fee changes for several segments of the mortgage market. Table 3 presents the results in basis points of loan amount (i.e., how upfront g-fees are quoted) and Table 4 presents the results in U.S. dollars. The first row shows that the average 2022 application would have experienced a 1.2 bps ($96) increase in upfront g-fee. The standard error implies that the mean is statistically different from zero. Although the average change in g-fee is economically small, the range and the standard deviation are large (32 bps or $1,023). With respect to race and ethnicity, Black borrowers, on average, would have benefited from the change with 4 bps ($21) less upfront g-fee mostly at the expense of Asian borrowers who, on average, would have seen an average of 6 bps ($267) increase in upfront g-fee. With respect to age, the new g-fee schedule favored older borrowers at the expense of younger borrowers. Borrowers in the 18-to-29 age group, on average, would pay almost 6 bps ($231) more in upfront g-fee, while borrowers who are 50 years old or older would, on average, save between 1.5 to 3 bps in upfront g-fee. With respect to income, the new fee

[20]For example, Kim et al. (2025) find that the g-fee changes disproportionately benefited high-income low-FICO borrowers because low-income low-FICO borrowers faced prohibitive liquidity constraints.

[21]For example, see Fannie Mae's 2024 10-K filing at `https://www.fanniemae.com/media/54826/display`.

schedule benefited lower-income borrowers at the expense of higher-income borrowers. Borrowers who make less than \$50,000 per year would, on average, enjoy a 6 bps (\$72) decrease in upfront g-fee, while borrowers who make more than \$100,000 per year would, on average, pay 3.5 bps (\$159) or more in upfront g-fee. Overall, this naive pre-policy analysis suggests that the fee changes may improve mortgage access for certain groups of disadvantaged borrowers, which is not surprising because the g-fee discounts are concentrated among lower-credit-score and higher-LTV parts of the fee schedule.

7.2 Rejection Probability

We begin our formal regression analysis by studying the impact that g-fee changes have on loan approval decisions. Table 5 presents the difference-in-differences regression results from our estimation of variants of regression equation 1. Column 1 presents the simple difference-in-differences result for rejection probability. In this specification, we include only the policy effect variables of interest, g-fee cell fixed effects, and year-month fixed effects. The results suggest that, during the announcement period, the rejection probability for the fee hike treatment group increased by 0.23%. During the implementation period, the treatment effect increased to 0.51%. There is no statistically significant result for the ($\Delta G < 0$) treatment group. Since the fee cuts are concentrated among low credit score borrowers, the null result may reflect lenders' unwillingness to make more loans in these riskier segments (Stiglitz and Weiss, 1981). Column 2 presents regression results from a specification where we include the full set of control variables and fixed effects. We find quantitatively similar results. The economic magnitude of the implementation period treatment effect is large; a 36 bps increase in rejection probability is approximately a 10% increase relative to the average rejection rate of 4% (see Table 2).

Using the methodology outlined in Section 5.3, we can estimate the average total elasticity between g-fee and rejection probability. Since the treatment effect for the ($\Delta G < 0$) treatment group is null, we calculate only the elasticity for the ($\Delta G > 0$) treatment group. We find that a 1 pp increase in g-fee increases the rejection probability by 1.4% with a 95% confidence interval of 0.3% and 2.5%. Assuming a linear effect beyond the local treatment region, this elasticity estimate implies that almost 20% of the sample average rejection rate (4%) can be explained by the existence of upfront g-fees.

The rejection rate effect may appear implausibly large relative to the fact that, for the average mortgage application in the ($\Delta G > 0$) treatment group, upfront g-fee increased only by approximately 30 bps (\$978).[22] This result can be rationalized by the fact that the mortgage lending business is

[22] Authors' calculation using the 2022 sample of mortgage applications from Tables 3 and 4.

a high-volume but low-margin business. For example, the Department of Justice's False Claims Act litigation against large banks during the Great Recession, which resulted in a $614 million settlement with J.P. Morgan Chase, "wiped out a decade of [the bank's] FHA profitability" (Frame et al., 2024).[23] Therefore, small changes in marginal cost that cannot be passed onto borrowers can induce large quantity adjustments.

To facilitate a causal interpretation of the result, we present the respective event study regression result in Figure 3. First, for the fee hike treatment group, the plot shows no statistically significant pre-trend in rejection probability, which implies that the parallel trend assumption holds. Furthermore, the point estimates begin to turn consistently positive, but not statistically significant, during the announcement period and the positive coefficients become statistically significant only in the implementation period. In line with the results presented in the first two columns of Table 5, the line for the fee cut treatment group shows a null result. Overall, the rejection probability results reject null hypothesis 1.

An alternative explanation of the rejection rate results is that the increase in g-fees pushed some treated mortgage applications over important DTI ratio limits and caused them to be rejected (Bhutta and Ringo, 2021). Table A3 presents some regression results that are inconsistent with this story. Column 1 presents results from a regression where the outcome variable is an indicator variable for whether the application was rejected for reasons related to its DTI ratio. We find no statistically significant result. Column 2 presents results from a regression where we regress the DTI ratio of rejected applications onto the g-fee change shock variables. If crossing DTI ratio limits were an important driver of the rejection results, then we should expect that the coefficients on the $(\Delta G > 0)$ terms to be positive and statistically different from zero. This is not the case.[24]

7.3 Note Rate

We next study the impact that g-fee changes have on note rate. Column 3 of Table 5 presents the no-controls difference-in-differences regression result. We find that, for both treatment groups, the pass-through rate is positive. The note rate on mortgages in the $(\Delta G < 0)$ treatment group decreased by

[23] See also `https://reports.jpmorganchase.com/investor-relations/2016/ar-ceo-letters.htm`.

[24] The results are quantitatively and qualitatively similar if we instead run a pooled regression using all applications and include interaction terms between an indicator variable for rejected applications and the g-fee shock terms. We cannot follow Bhutta and Ringo (2021) and perform an analysis where we compare, with respect to the g-fee shocks, rejection probabilities between treated applications that crossed an important DTI ratio limit and treated applications that did not cross such limits because, to perform this particular analysis, we have to make several assumptions. First, we need to assume how lenders split the g-fee shock between note rate and upfront cost. Second, we then need to assume a pass-through rate for each channel in order to classify the two types of treated applications. These assumptions would introduce noise into the regression and render the test uninformative.

3.6 bps during the announcement period and by 5 bps during the implementation period. With no announcement effect, the note rate on mortgages in the ($\Delta G > 0$) treatment group increased by 4 bps during the implementation period. The results remain quantitatively and qualitatively similar when we include the full set of control variables and fixed effects (column 4).

Figure 4 presents the accompanying event study regression result. The figure shows no statistically significant pre-trend before the announcement period for either treatment group. For the ($\Delta G > 0$) treatment group, the point estimates begin to turn positive and become statistically different from zero at the end of the announcement period. For the ($\Delta G < 0$) treatment group, the point estimates begin to turn negative during the announcement period and continue to drift downward throughout the implementation period. These patterns suggest that the parallel trend assumption holds.

Using the methodology outlined in Section 5.3, we estimate the implementation period average total pass-through rate for the ($\Delta G < 0$) treatment group to be 10.7 bps per a 1 pp change in upfront g-fee with a 95% confidence interval of 1.7 bps and 19.8 bps. For the ($\Delta G > 0$) treatment group, the implementation period average total pass-through rate is 16 bps with a 95% confidence interval of 5.4 bps and 26.6 bps.[25]

7.4 Upfront Cost

An important part of mortgage pricing is upfront cost (Mota et al., 2023), which serves as another channel that lenders can pass on changes in upfront g-fees to borrowers. We define total upfront cost as the total discount points and loan-related fees that the borrower paid before or at closing minus lender credits, scaled by the loan amount. Although discount points are used to lower the mortgage's interest rate, they present an opportunity for lenders to gain additional compensation because the value of discount points can vary across loans (Bhutta et al., 2020; Zhang and Willen, 2021; Amornsiripanitch, 2023). Therefore, in principle, lenders could profit from points that they sell. We subtract lender credit because, conceptually, we want our measure of total upfront cost to capture the net revenue that the lender makes at loan origination.

Column 5 of Table 5 presents the no-controls difference-in-differences regression result. We find that the g-fee pass-through rate to total upfront cost is positive for both treatment groups and the magnitude of the ATT is also very similar. Adding the full set of control variables and fixed effects does not materially change the results (column 6). Using the methodology from Section 5.3, we estimate the

[25] The point estimates that we use to compute the pass-through rates can be found in column 2 of Table A4.

implementation period average total pass-through rate for the ($\Delta G < 0$) treatment group to be 25.9 bps per a 1 pp change in upfront g-fee with a 95% confidence interval of 17.6 bps and 34.2 bps. For the ($\Delta G > 0$) treatment group, the implementation period average total pass-through rate is 13.3 bps with a 95% confidence interval of 3.7 bps and 22.9 bps.[26] Notably, the upfront cost pass-through rate is comparable to that of note rate, which implies that lenders use both channels to pass on marginal costs to borrowers.

Figure 5 presents the accompanying event study regression result. The figure shows no statistically significant pre-trend before the announcement period for either treatment group, which supports a causal interpretation of the results.

7.5 All-in Price

As mentioned in Section 6, it is difficult to assess whether the pass-through rate that we estimate using the MBS spread method is equivalent to the complete pass-through rate because the buy-up multiplier calculation requires us to make assumptions that render the resulting estimate noisy. Nonetheless, for completeness, we estimate the average total pass-through rate that incorporates the buy-up multiplier. Following Kalda et al. (2025), the buy-up multiplier for the sample period is 3.25. Using the multiplier, we can convert each mortgage's note rate and upfront cost into a measure of all-in price that is akin to the annual percentage rate (APR) by using the following formula:

$$P = C + 3.25 \times R. \tag{7}$$

P is the all-in price, measured in basis points. C is the upfront cost, measured in basis points of the mortgage loan amount. R is the note rate, measured in basis points. A nice feature of the all-in price is that, when we use it to estimate the average total pass-through rate, the resulting estimates are easy to interpret because, for example, an estimated pass-through rate of one can be interpreted as complete pass-through, as it implies that a 1 pp increase in g-fee leads to a 1 pp increase in all-in price.

We estimate the implementation period average total pass-through rate for the ($\Delta G < 0$) treatment group to be 0.65 with a 95% confidence interval of 0.33 and 0.96. A formal t-test reveals that we can reject the null hypothesis that the estimate is equivalent to one at the 5% statistical significance level. The estimated pass-through rate for the ($\Delta G > 0$) treatment group is 0.65 with a 95% confidence interval of

[26] The point estimates that we use to compute the pass-through rates can be found in column 3 of Table A4.

0.27 and 1.02. A formal t-test shows that we can reject the null hypothesis that the estimate is equivalent to one at the 10% statistical significance level.[27] Not surprisingly, estimating the average total pass-through rate using all treated observations (equation 6) yields an estimated pass-through rate of 0.65 that rejects the null hypothesis that the point estimate is equal to 1 at the 1% statistical significance level.[28]

Keeping the drawbacks discussed in Section 6 in mind, we interpret the resulting average total pass-through rate for the all-in price measure as being symmetrical but incomplete. Incomplete pass-through is consistent with the adjustment in rejection rates that we document in Section 7.2. The symmetry implies that lenders profit from g-fee decreases but do not increase the amount of mortgage credit that they are willing to supply to the respective segments of the market.

7.6 Securitization Decisions

This section uses the same empirical methods to explore the impact that g-fee changes have on lenders' securitization decisions and test the second null hypothesis. A useful feature of cHMDA that public HMDA data does not have is the universal loan identifier, which allows us to track a loan across multiple sales. For example, a loan may be originated and sold to a private company and then sold to the GSEs. A researcher who uses public HMDA would be forced to code this loan as being sold to a private company, while, in reality, it was sold the GSEs. The same problem applies to loans that were originated near the end of the reporting year because there is not enough time, with respect to the reporting rule, for them to be sold.[29] In short, cHMDA allows us to more accurately code lenders' securitization decisions.

Table 6 presents the regression results. It is helpful to consider the three outcome variables, GSE sale, non-GSE sale, and unsold, together as these are the three possible securitization outcomes for a mortgage that was originated and the coefficients from the same regression specification and sample sum to zero. The odd-numbered columns present the no-controls regression results. For the ($\Delta G < 0$) treatment group, we find little change in lenders' securitization decisions. On the other hand, for the ($\Delta G > 0$) treatment group, lenders substitute away from selling to the GSEs by selling to non-GSE buyers and holding more mortgages on their balance sheet. Figures 6 through 8 present the respective event

[27]The point estimates that we use to compute the pass-through rates can be found in column 4 of Table A4.

[28]In light of Kalda et al. (2025), we repeat the exercise on the sample of loans that were sold the GSEs and find that the pass-through for fee increases, fee decreases, and all treatment groups are 0.74, 0.71, and 0.73, respectively. The results are consistent with symmetric but incomplete pass-through. The finding implies that using a data set with better coverage of loans sold to the GSEs and the Callaway et al. (2024) estimator yield very different results.

[29]The universal loan ID allows us to solve this problem from loans originated near the end of 2022 but not 2023. However, all results are quantitatively and qualitatively similar if we drop loans that were originated in the last two months of 2023.

study plots, which all suggest that the parallel trend assumption holds. The even-numbered columns present the regression results with the full set of control variables and fixed effects. The non-GSE sale results are robust, while the unsold result is not, but, overall, the results still suggest that g-fee increases matter for lenders because they tend to substitute away from selling mortgages to the GSEs, which is consistent with incomplete pass-through.

An important feature of the conforming mortgage market is that there are banks and non-bank lenders, which have very different business models (Buchak et al., 2024; Amornsiripanitch et al., 2025). Banks have balance sheet capacity, which can help absorb changes in marginal cost associated with GSE securitization, while non-banks do not. We test whether the response to g-fee changes differ between banks and non-banks by interacting the policy variable of interest with a non-bank indicator variable. We include lender by g-fee cell and lender by year-month fixed effects so that we can interpret the triple interaction terms as the marginal difference in non-banks' response to g-fee changes, relative to banks.

Table 7 presents the regression results. The double interaction terms capture banks' response to the g-fee changes. Intuitively, we find that, in response to fee increases, banks are less likely to sell mortgages to the GSEs and make up for this gap by selling more mortgages to non-GSE entities and holding more mortgages on their balance sheet. We find little statistically significant result for fee cuts.[30] Comparing the relevant coefficients between the double and triple interaction terms (e.g., for column 2, -2.06 on $(\Delta G > 0) \times$ Implementation and 2.32 on $(\Delta G > 0) \times$ Implementation $\times$ Non-bank) shows that the coefficients have the same size but the opposite signs, which implies that non-banks, on net, do not respond to fee changes. This result makes sense because non-banks have no balance sheet capacity and largely rely on the originate to distribute (OTD) business model.[31]

Overall, the results suggest that changes in upfront g-fee, especially fee increases, cause banks to respond accordingly. The findings are consistent with the idea that lenders cannot fully pass on these fee increases to borrowers. Therefore, we interpret this set of results as rejecting null hypothesis 2. It is also important to note that this dimension of quantity adjustment is not consistent with the DTI ratio limit story. Specifically, if the rejection probability results were driven by marginal mortgage applications being pushed over important DTI ratio limits, then we would not expect to see any change in lenders' securitization behavior in response to the fee changes because lenders' profit margins on loans sold to the GSEs would remain unchanged.

[30] The announcement effect for the g-fee decrease treatment group is consistent with the idea that banks can raise their profit margin by holding onto loans and selling them when the fee decrease occurs.

[31] The non-bank result raises a question, which is whether this incapacity to respond on the sercuritization side has differential effects on non-banks' lending decisions. We find some weak evidence that non-banks pass on larger proportions of the g-fee changes to borrowers via upfront cost (see column 6 of Table A5).

7.7 Potential Mechanism – Liquidity Constraint

The lending and securitization results support the idea that lenders cannot fully pass on g-fee changes to borrowers. The obvious question is what are the mechanisms behind the incomplete pass-through?

Liquidity constraint is a potential candidate. Some borrowers have very tight liquidity constraints such that they can no longer afford to take out a mortgage when the price of mortgage credit increases. Knowing this, lenders raise the rejection rate for these applications due to lower expected profits from borrowers' unwillingness or inability to bear the cost, i.e., higher default risk. Empirically, we should expect to find large quantity adjustments in response to small increases in price. To give some empirical evidence for this mechanism, we analyze the heterogeneous effects that the fee changes have on small-dollar mortgage (SDM) applications, which tend to be submitted by lower-income, lower-credit-score, younger, and Black applicants, all intuitive proxies for liquidity constraint (McCargo et al., 2018, 2019; Goldstein and DeMaria, 2022; Horowitz and Roche, 2023).[32] In other words, we use loan amount as a proxy for liquidity constraint.[33] Additionally, the results presented below also answer an open question in the small-dollar mortgage literature – why do SDM applications face significantly higher rejection rates?[34]

7.7.1 Testable Predictions

Intuitively, lower marginal cost pass-through rates imply that, for each unit of change in marginal cost, lenders make larger quantity adjustments to equate marginal cost to marginal revenue. Therefore, policies that raise the marginal cost of mortgage origination disproportionately reduce the credit supply to the SDM segment and worsen the disparity in mortgage access with respect to race and income. The testable implications for incomplete pass-through are as follows.

Prediction 1 – *When upfront g-fee increases, rejection rate increases are larger for small-dollar mort-*

[32] We also confirm this notion in our sample. See Table A6.

[33] In this context, income is not a good candidate proxy for liquidity constraint because of the complication in assigning treatment status with respect to income, discussed in Section 5.1.

[34] One may suspect that low competition may be a contributor to the incomplete pass-through that we document. While plausible, we cannot rigorously provide empirical evidence for the competition channel, due to several reasons. First, in the mortgage context, it is challenging to define market segments and measure competition. For example, we cannot use geography because some mortgage lenders compete locally and others compete nationally. This fact renders county-level Herfindahl-Hirschman Index (HHI) useless. Second, according to the pass-through literature (Ritz, 2015; Genakos and Pagliero, 2022), without making additional assumptions about the structural parameters that determine the pass-through rate (e.g., the elasticity of demand, the elasticity of the inverse marginal cost curve, and the curvature of the demand function) (Weyl and Fabinger, 2013), there is no clear relationship between the degree of competition and the pass-through rate. It is beyond the scope of this paper to estimate these structural parameters. Therefore, we do not explicitly test whether the incomplete pass-through that we document is partly driven by low competition.

gages, relative to larger mortgages.

Prediction 2 – *Guarantee fee pass-through rates via note rate and upfront cost are smaller for small-dollar mortgages, relative to larger mortgages.*

We test the two predictions by adding triple interaction terms between the policy variables of interest and a small-dollar mortgage (SDM) indicator variable, which equals one for observations with loan amounts less than or equal to $150,000 (Brevoort, 2022) and zero otherwise. We include SDM by g-fee cell and SDM by year-month fixed effects so that we can interpret the triple interaction terms as the marginal effect that the g-fee changes have on SDM, relative to larger mortgages. If the identifying assumptions hold (Section 5.2), the natural experiment would also rule out the possibility that the SDM rejection rate wedge is purely driven by omitted variable bias (OVB) related to unobservable credit risk.

7.7.2 Rejection Probability

Table 8 presents the SDM regression results. Columns 1 and 2 present the rejection probability result. We find that, for the $(\Delta G > 0)$ treatment group, the marginal difference in ATT^0 for SDM applications (0.67% to 0.71%) is substantially larger than the ATT^0 on larger mortgage applications (0.29% to 0.43%). Much like the baseline rejection results (Table 5), there is no statistically significant effect for the $(\Delta G < 0)$ treatment group, which may be driven by lenders' lack of appetite for riskier loans, especially in the SDM segment.

Testable prediction 1 concerns the elasticity between rejection probability and changes in g-fee. Therefore, to properly test the prediction, we need to estimate the average total elasticity. Using the method from Section 7.2, we can test whether the weighted average of the scaled coefficients on the implementation-period SDM triple interaction terms (β_d^I) for the fee hike treatment groups in the following regression is positive and statistically different from zero:

$$\begin{aligned} Y_i = \alpha + \; ... + \sum_d^D \beta_d^A (\Delta G = d)_c \times \mathit{Announcement}_t \times SDM_i \\ + \sum_d^D \beta_d^I (\Delta G = d)_c \times \mathit{Implementation}_t \times SDM_i \\ + \gamma' \mathbf{x_i} + \mu_\mathbf{c} \times SDM_i + \nu_\mathbf{t} \times SDM_i + \epsilon_i. \end{aligned} \tag{8}$$

We find that the weighted average is 3.8% and is statistically significant at the 1% level, which means that, for a 1 pp increase in upfront g-fee, applications for small-dollar mortgages experience, on average, an additional 3.8% increase in rejection probability, relative to applications for larger mortgages.[35] This result confirms the first prediction.[36] The 3.8% elasticity estimate also implies that the existence of upfront g-fee can explain a large part of the higher conditional rejection rate that SDM applications face. Using the same sample of conforming mortgage applications, we estimate that, conditional on the standard set of control variables and fixed effects that the literature uses, SDMs face a 1.4% higher rejection probability than applications for larger mortgages.[37] Given that the average upfront g-fee for SDM applications in the sample is 57 bps, the 3.8% elasticity estimate implies that the existence of upfront g-fee alone can explain the entire 1.4% rejection rate wedge ($3.8\% \times 0.57 = 2.2\% \approx 1.4\%$).[38]

7.7.3 Pricing

Columns 3 through 6 of Table 8 present the difference-in-differences result for note rate and upfront cost. For both outcome variables, we find that the coefficients on the $(\Delta G > 0) \times Implementation \times SDM$ term are consistently negative, are similar in size to the coefficients on the $(\Delta G > 0) \times Implementation$ term, and are statistically different from zero. Qualitatively, this result implies that the ATT^0 for SDM mortgages is significantly smaller than that of larger mortgages. Using the results presented in columns 4 and 6, formal t-tests on the sum of the two coefficients reveal that the sums are not statistically different from zero, which suggests that, for SDMs, the ATT is not statistically different from zero. For the $(\Delta G < 0)$ treatment group, we do not find that the ATT is statistically different for small-dollar mortgages.

We formally test prediction 2 in the same way that we test prediction 1. For note rate and upfront cost, we find that the average total pass-through rate for small-dollar mortgages is 23.6 bps and 36.9 bps lower, respectively, than the average total pass-through rate for larger mortgages.[39] Both estimates are statistically different from zero at the 1% level. The same conclusion holds when we use all-in price as the outcome variable. The resulting difference in pass-through rate estimate is -1.16, which, when compared

[35] For the computation, we use the coefficients on the triple interaction terms ($(\Delta G = d)\times$ Implementation $\times$ SDM) from the first regression result presented in Table A7.

[36] Equation 8 omits the double interaction terms and certain fixed effect terms for ease of presentation. The regression result used to perform the statistical test includes the same set of fixed effects and control variables as the specification shown in column 2 of Table 8. We do not perform the same test for the $(\Delta G < 0)$ treatment group because we do not find a statistically significant difference in ATT.

[37] We get a similar estimate of the SDM rejection rate wedge when we expand the sample to include all years of the new cHMDA (2018 to 2023).

[38] Table A8 presents regression results that suggest that the SDM rejection rate result is not primarily driven by SDM applications being rejected for reasons related to changes in their DTI ratios.

[39] We use the coefficients on the triple interaction terms ($(\Delta G = d)\times$ Implementation $\times$ SDM)) from the second and third regressions presented in Table A7.

to the 0.65 pass-through rate from Section 7.5, implies that the average total pass-through rate for SDMs is essentially zero.[40] This set of results confirms testable prediction 2.[41] Overall, the empirical results are consistent with the idea that the incomplete pass-through that we document in the pooled sample is partly driven by segment-specific liquidity constraint.[42] Furthermore, the results suggest that the policy's goal to ease mortgage access for disadvantaged borrowers is undermined by lenders' endogenously cutting credit supply to SDM borrowers.

7.8 Heterogeneous Effects – Demographics

An open question in the mortgage literature is why do rejection rates and the cost of mortgage credit systematically vary across demographic groups? Different pass-through rates that arise from market segmentation with respect to demographic groups are a potential mechanism. It is a well-known fact that residential sorting with respect to race and age is a feature of American neighborhoods (Bayer and McMillan, 2005; Bayer et al., 2022). Therefore, it is conceivable that real estate and, hence, mortgage markets are segmented in ways that are highly correlated with demographics. Due to factors such as the degree of local competition and/or demand-side liquidity constraint, cost pass-through rates may differ across market segments. The g-fee changes offer an opportunity for us to test whether differences in cost pass-through can partially explain the tight correlation between borrowers' demographics and mortgage access. We test for this mechanism by estimating variants of the following regression equation:

$$\begin{aligned} Y_i = \alpha + \, ... + \beta_1 (\Delta G < 0)_c \times Implementation_t \times D_i \\ + \beta_2 (\Delta G > 0)_c \times Implementation_t \times D_i \\ + \gamma' \mathbf{x_i} + \mu_{\mathbf{c}} \times D_i + \nu_{\mathbf{t}} \times D_i + \epsilon_i. \end{aligned} \tag{9}$$

The announcement terms and the double interaction terms are omitted from the equation display for presentation purposes. D_i is the placeholder for the the demographic indicator variable of interest, which varies at the application level i. For example, in a regression where we only include non-Hispanic White and Black applicants, D_i would be the indicator variable $Black_i$, which equals one for Black applicants and zero otherwise. When Y is a measure of mortgage access and cost (e.g., rejection indicator, note rate, upfront cost, or all-in price), coefficients of interest, β_1 and β_2, test whether Black borrowers

[40] We use the coefficients on the triple interaction terms from the fourth regression presented in Table A7.

[41] We do not perform the same test for the ($\Delta G < 0$) treatment group because we do not find a statistically significant difference in the ATT.

[42] The SDM results are robust to dropping refinance applications and applications associated with expensive properties. In other words, the results are unlikely to be driven by well-off individuals who apply for small mortgages.

face a different pass-through rate and, respectively, rejection rate adjustment, relative to non-Hispanic White borrowers.

We test for differences in pass-through and accompanying rejection rate adjustment with respect to race (e.g., non-Hispanic White borrowers versus Hispanic, Black, Asian, and other minority borrowers), age (e.g., borrowers in the 18-to-29 age group versus borrowers in older age groups), and gender (e.g., male borrowers versus female and unknown gender borrowers). We do not find any robust statistically significant difference in pass-through between demographic groups, whether the regressions are estimated using the pooled sample or pairwise samples (e.g., non-Hispanic White versus Black borrowers and so on). Therefore, we conclude that differences in cost pass-through, as can be identified by the upfront g-fee changes, including those induced by market segmentation and competition, cannot explain the systematic differences in mortgage access across demographic groups.

7.9 Aggregate Effects

An important question for the g-fee changes is how do the changes impact aggregate mortgage credit supply? We answer this question by estimating variants of equation 2. Table 9 presents the results. Columns 1 and 2 report results for application volume, measured in millions of USD and number of applications. We find that, during the implementation period, application volume fell by approximately \$101 million (313 applications) per month in the ($\Delta G > 0$) treatment cells. We cannot interpret this marginal effect as purely coming from the demand side because the result is likely a combination of borrowers demanding less credit due to higher prices and lenders discouraging borrowers from applying for new loans (Amornsiripanitch et al., 2025). Our inability to observe applications that would have been filed without lenders' discouragement implies that the rejection rate results that we present above should be interpreted as the lower bound of lenders' credit supply adjustments. Figure 1 presents the event study plot for the dollar-value application volume regression, which suggests that the parallel trend assumption holds. We do not find any statistically significant result for fee decreases. Taken at face value, the null result implies that, in the respective treated cells, consumers are not particularly sensitive to credit price decreases and/or lenders are unable or unwilling to encourage more applications, which is consistent with the rejection probability results presented in Table 5.

Columns 3 and 4 report results for origination volume, i.e., the total credit supply for this segment. The estimates show that, during the implementation period, application volume fell by approximately \$118 million (373 applications) per month in the fee hike treatment cells. Intuitively, the effect sizes are slightly larger than those presented in columns 1 and 2 because these effects also include lenders' response in the form of higher rejection rates (Table 5). Once again, we do not find any statistically significant result for fee decreases. Figure 9 presents the event study plot for the dollar-value origination volume regression, which suggests that the parallel trend assumption holds. Since fee decreases are concentrated among more credit-constrained (e.g., low credit score and high LTV) borrowers, the asymmetrical effect is likely to be caused by lenders' unwillingness to extend more credit to these borrowers because discounts should be especially attractive to credit-constrained borrowers. Importantly, in contrast to the pre-policy analysis presented in Section 7.1, the lack of credit supply expansion for the fee cut treatment group implies that the FHFA's intent to make mortgage credit more accessible to disadvantaged borrowers is not met.

A back-of-the-envelope calculation using the coefficient estimates in column 3 suggests that the fee changes reduced mortgage origination by 8%.[43] Twenty percent of the effect occurred during the announcement period. Performing the same exercise on application volume and comparing the effect size with the origination volume result suggests that approximately 64% of the negative aggregate credit supply effect can be explained by the decrease in application volume in the treated g-fee cells.[44] This result also implies that at least half of the negative aggregate credit supply effect comes from higher rejection rates.[45] The negative aggregate credit supply effect calls into question whether, on net, the fee changes improved the GSEs' capital position.

8 Conclusion

This paper uses data on the conforming mortgage market to study whether the May 2023 changes in upfront guarantee fee are fully passed on to borrowers. We find strong empirical evidence that pass-through to prices is symmetrical, but incomplete. In response to fee increases, lenders raise application rejection rates. Lenders do not reduce rejection rates when fees decrease. Furthermore, banks sell

[43]For the fee hike treatment cells, we add back the monthly announcement and implementation effects onto the observed origination volume in 2023 and compute the total percentage reduction.

[44]For this exercise, we set the announcement effect on application volume to zero because of the lack of statistical significance in the ATT regression results presented in column 1 of Table 9. We find that 86% of the implementation period effect on origination volume comes from the reduction in application volume. This share is computed by dividing the implementation period effect on application volume by the implementation period effect on origination. The 64% number is the result of dividing the the implementation period effect on application volume by the total (announcement and implementation) effect on origination. The results are in the same order of magnitude if we instead use coefficient estimates from a specification where treatment groups are defined using discrete dosage levels.

[45]We say "at least" because lenders can discourage borrowers from submitting an application when they know that it unlikely that the application will get approved. Therefore, some of the 64% effect can come from this channel, which can be interpreted as a supply-side adjustment that is akin to raising rejection rates (Bhutta et al., 2021).

fewer mortgages to the GSEs, sell more mortgages to non-GSE buyers, and hold more mortgages on their balance sheet. Non-banks do not adjust their securitization behavior, possibly due to lack of balance sheet capacity. We show that pass-through is near-zero and increases in rejection rates are large among small-dollar mortgages, which suggests that the overall incomplete pass-through is partly driven by high price elasticity of demand from liquidity-constrained borrowers. With asymmetrical credit supply response to fee changes, we estimate that the fee changes reduced aggregate mortgage origination by 1.4%.

Incomplete pass-through has important policy implications. Policies that increase the marginal cost of mortgage origination such as tightening monetary policy and increasing banking regulations will cause lenders to individually cut mortgage credit supply, which implies that mortgage credit will be less accessible via higher prices and lower quantity supplied. Similarly, moving the GSEs out of conservatorship may significantly reduce mortgage credit access because the GSEs would need to substantially increase g-fees to provide the same level of guarantee and liquidity for agency MBSs. The GSEs and agency MBSs have implicit guarantees from the U.S. government, which insights from the safe asset literature (Krishnamurthy and Vissing-Jorgensen, 2012; Gorton, 2017) would suggest that the current g-fee levels are too low to justify the level of secondary market liquidity that agency MBSs enjoy. Furthermore, the lack of credit supply expansion response to fee decreases implies that policies that lower the marginal cost of mortgage origination (e.g., subsidy and rate cuts) for low-credit-score and high-LTV borrowers are ineffective in easing mortgage credit access.

We use the same natural experiment to show that low pass-through can explain a large part of the elevated rejection rates that small-dollar mortgage applications face. The finding implies that any policy that increases the marginal cost of mortgage origination also, at the margin, increases disparity in mortgage access with respect to income, race, and age, characteristics that are strongly associated with applying for low-balance mortgages.[46] Lastly, incomplete cost pass-through cannot explain the stylized facts that small-dollar mortgages are more expensive and that the price of mortgage credit varies systematically across demographic groups.

[46]See Table A6.

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Figure 1: Event Study Plot – Application Volume

This figure plots the coefficients and their respective 95% confidence intervals from a cell-by-year-month-level event study regression where the application volume, expressed in millions of USD, is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

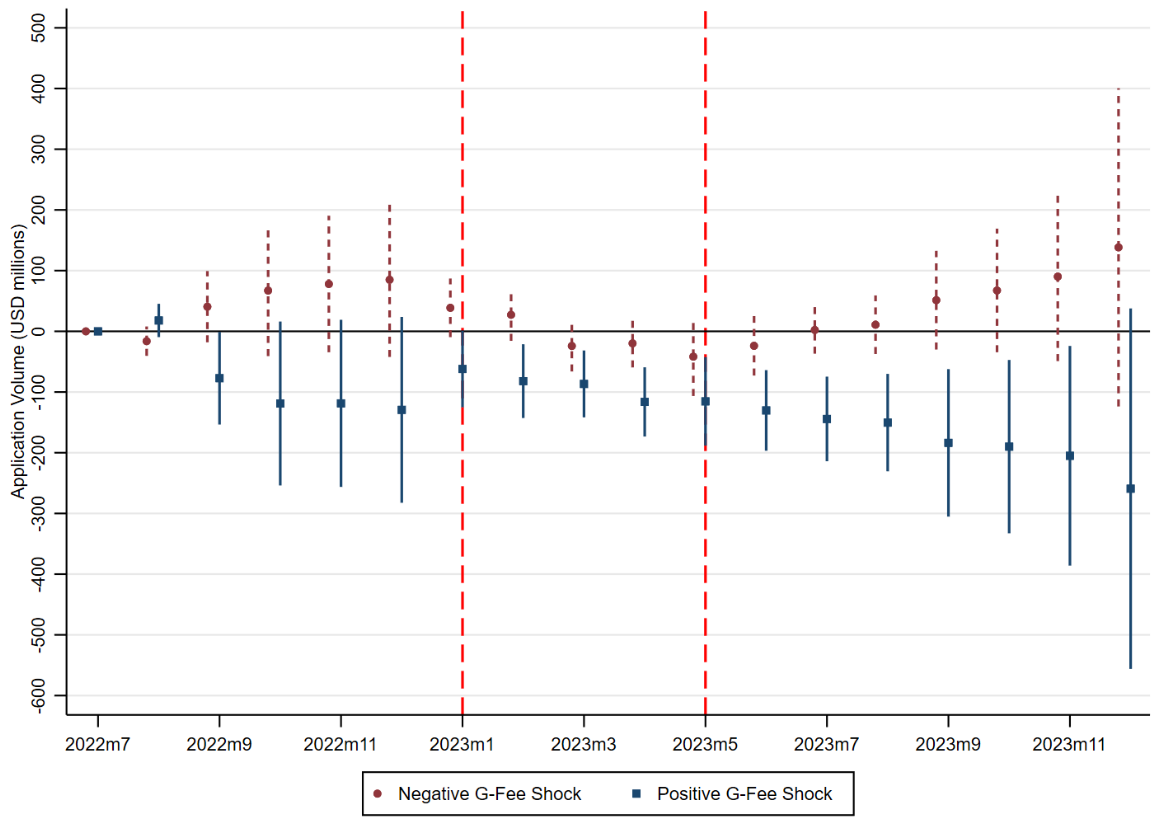

Figure 2: Event Study Plot – Processing Volume

This figure plots the coefficients and their respective 95% confidence intervals from a cell-by-year-month-level event study regression where the application processing volume, expressed in millions of USD, is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

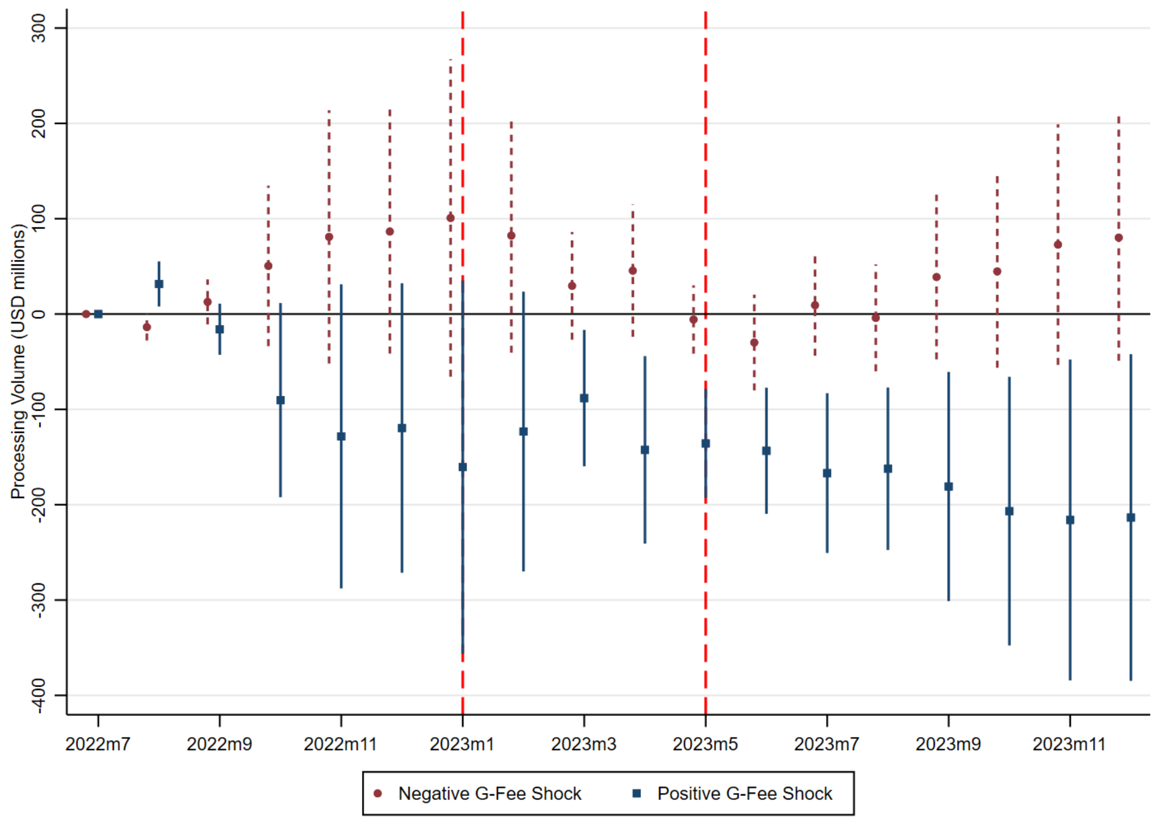

Figure 3: Event Study Plot – Rejection Probability

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where 100 times the rejection indicator variable is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

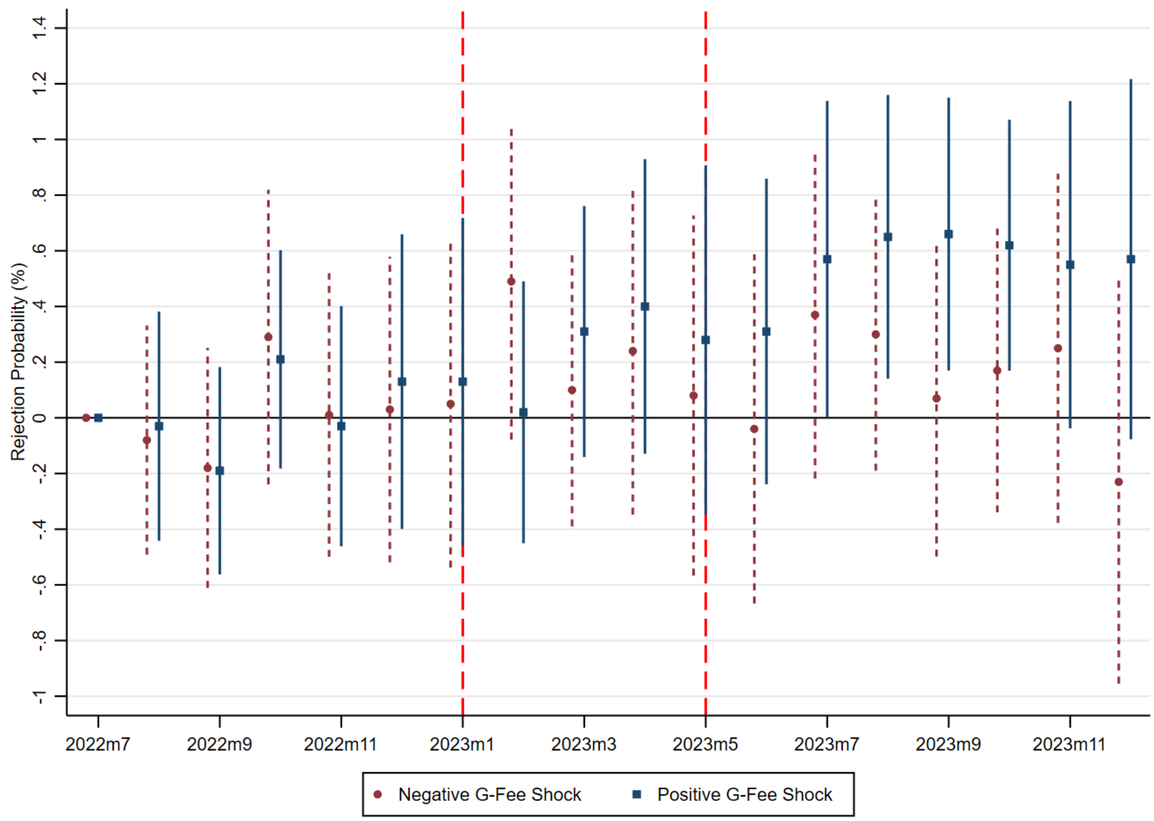

Figure 4: Event Study Plot – Note Rate

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where the loan's note rate, expressed in basis points, is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

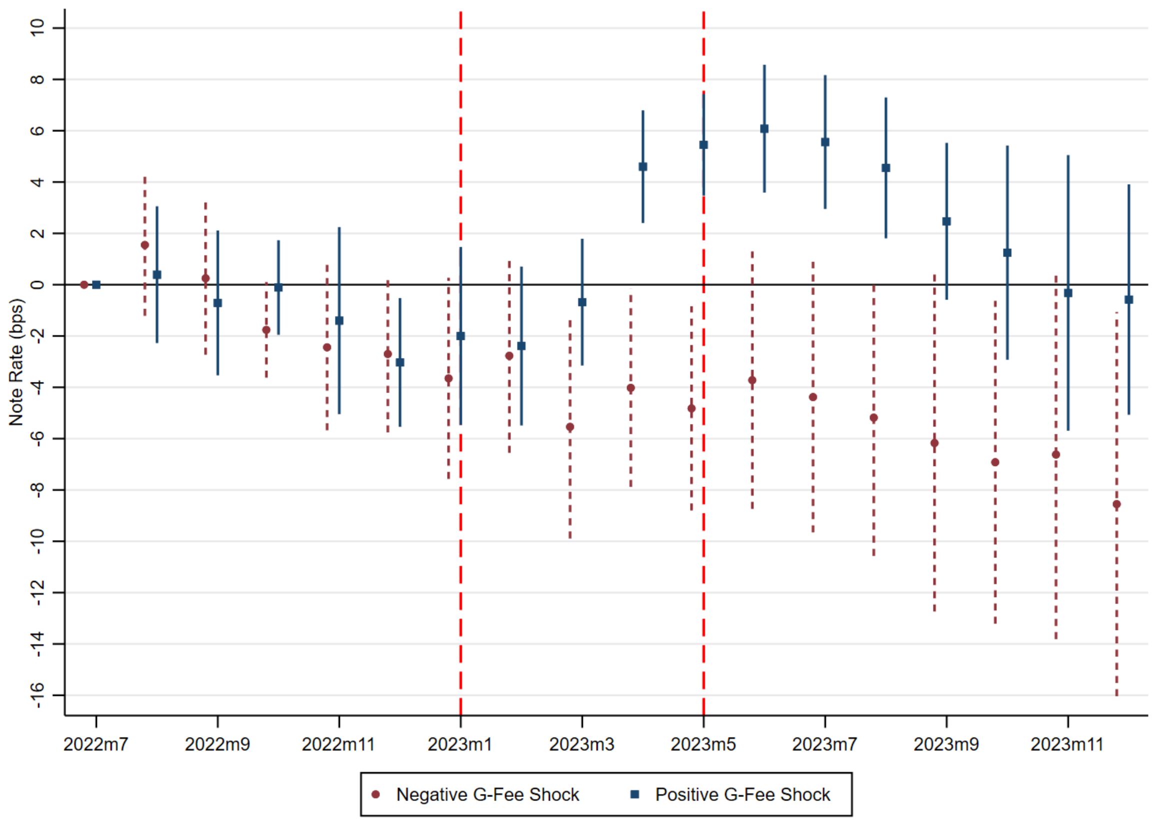

Figure 5: Event Study Plot – Upfront Cost

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where total upfront cost (including discount points and lender credit), expressed in basis points of loan amount, is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

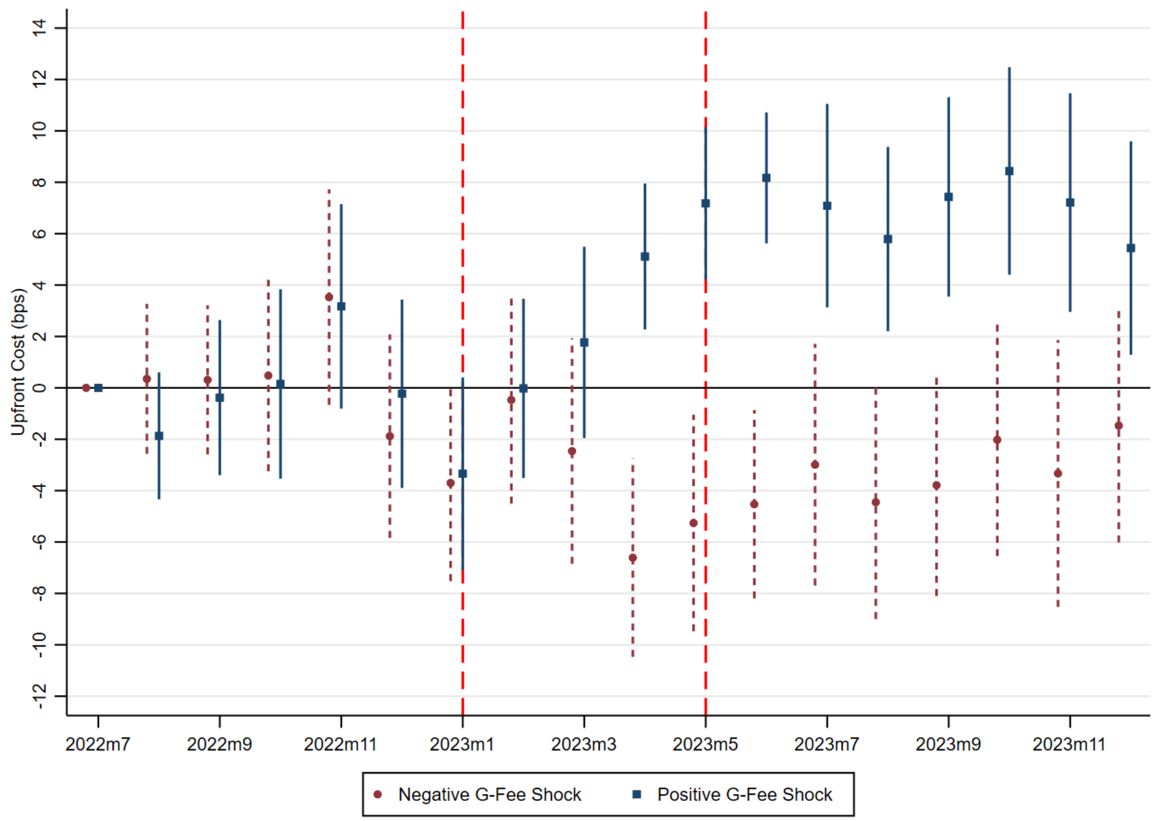

Figure 6: Event Study Plot – GSE Sale Probability

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where 100 times the GSE sale indicator variable is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

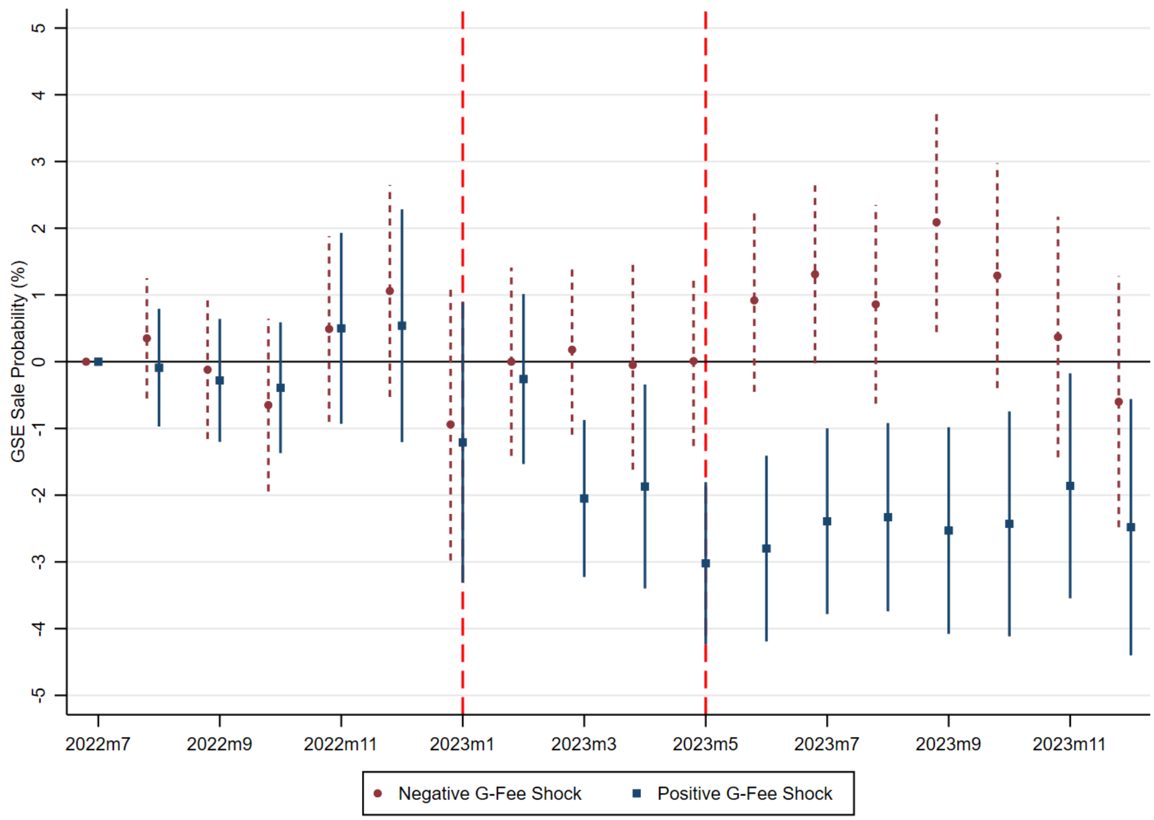

Figure 7: Event Study Plot – Non-GSE Sale Probability

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where 100 times the non-GSE sale indicator variable is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

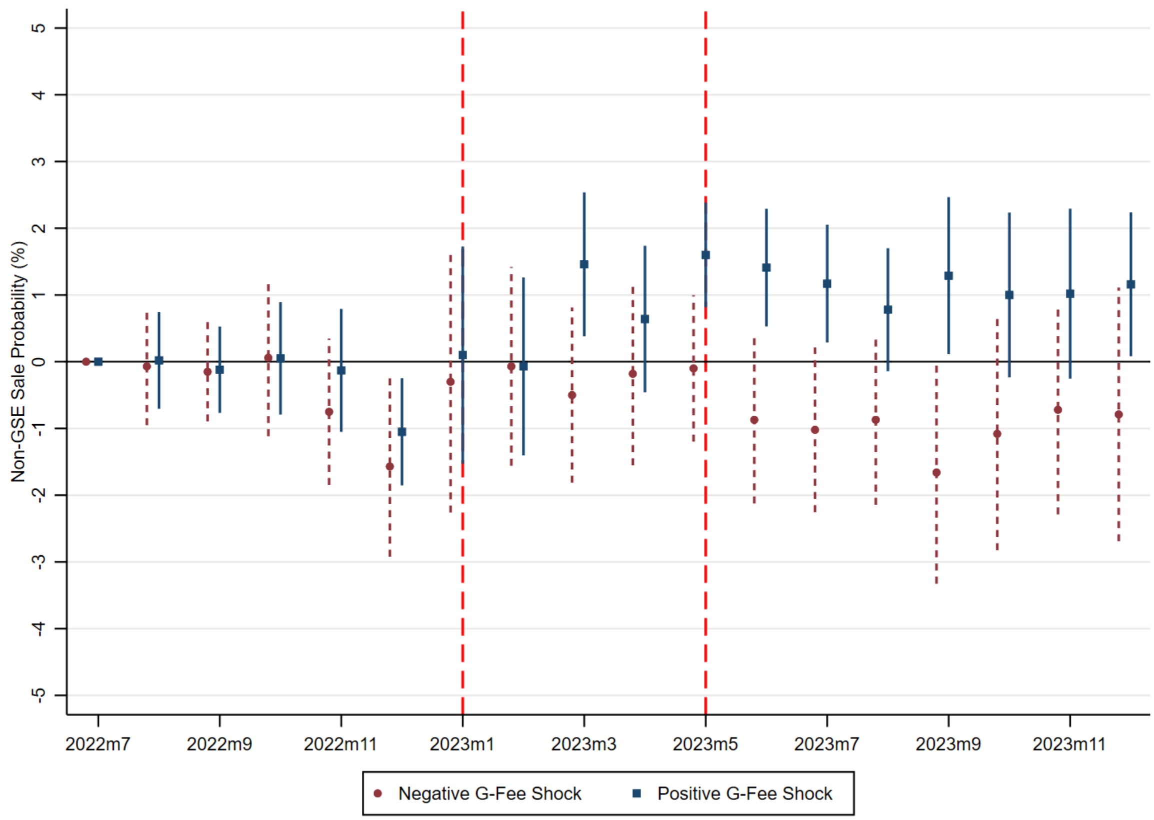

Figure 8: Event Study Plot – Unsold Probability

This figure plots the coefficients and their respective 95% confidence intervals from an application-level event study regression where 100 times the unsold indicator variable is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

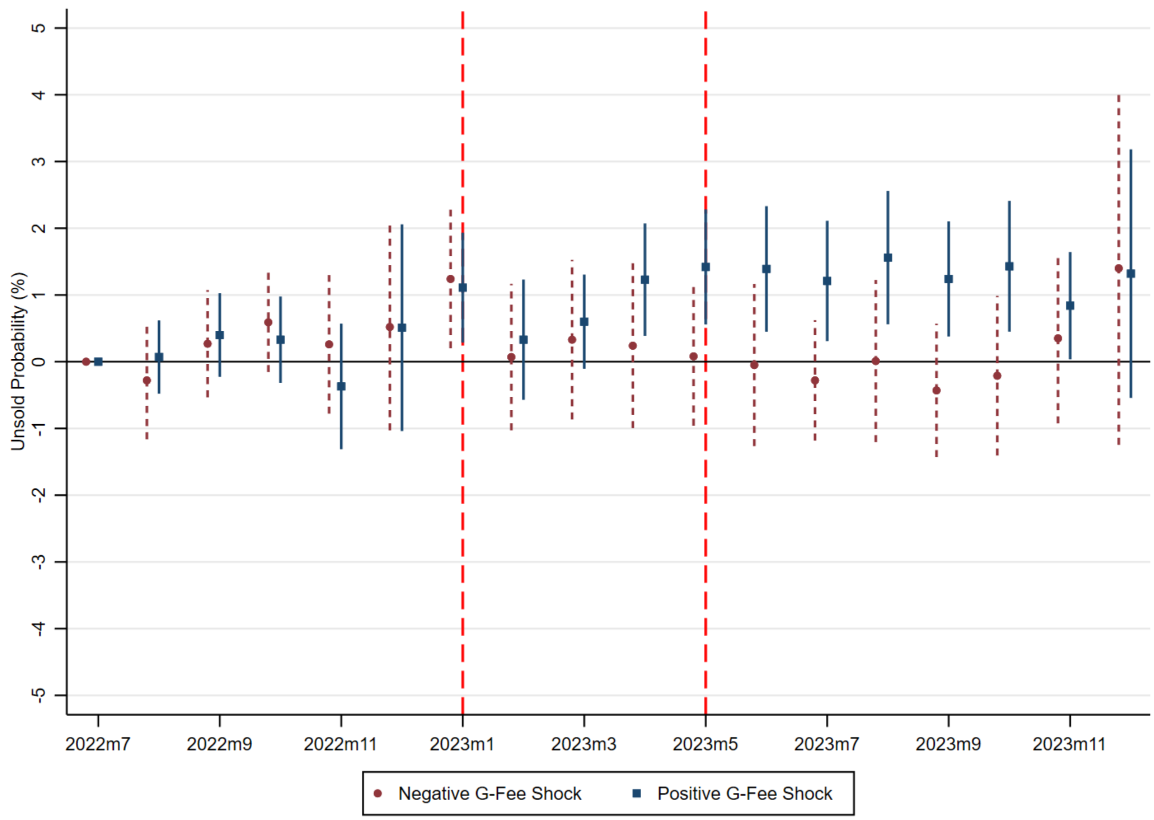

Figure 9: Event Study Plot – Origination Volume

This figure plots the coefficients and their respective 95% confidence intervals from a cell-by-year-month-level event study regression where the origination volume, expressed in millions of USD, is regressed onto the g-fee cut indicator variable ($\Delta G < 0$) interacted with year-month indicator variables and the g-fee hike indicator variable ($\Delta G > 0$) interacted with year-month indicator variables. The two vertical lines mark the announcement month, January 2023, and the implementation month, May 2023. G-fee cell and year-month fixed effects are included. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. Data source: cHMDA.

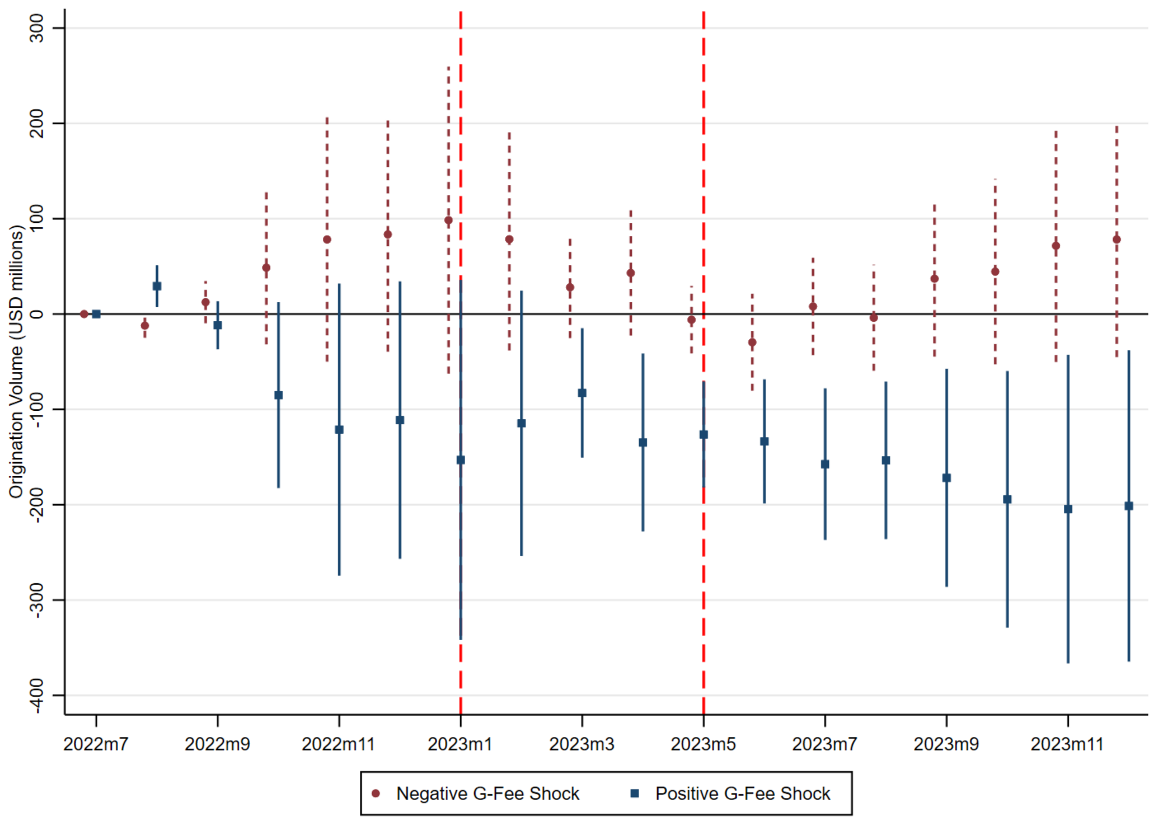

Table 1: Upfront Guarantee Fee Changes

This table presents the g-fee changes that occurred in May 2023. G-fee changes are presented as percent of loan amount. LTV ratio bins are presented in columns and credit score bins are presented in rows. Data sources: Fannie Mae and Freddie Mac.

	LTV Ratio (%)								
Credit Score	$x \leq 30$	$30 < x \leq 60$	$60 < x \leq 70$	$70 < x \leq 75$	$75 < x \leq 80$	$80 < x \leq 85$	$85 < x \leq 90$	$90 < x \leq 95$	$x > 95$
780+	0.000	0.000	-0.250	-0.250	-0.125	+0.125	0.000	0.000	-0.625
760-779	0.000	0.000	-0.250	0.000	+0.125	+0.375	+0.250	+0.250	-0.500
740-759	0.000	0.000	-0.125	+0.125	+0.375	+0.750	+0.500	+0.375	-0.250
720-739	0.000	0.000	0.000	+0.250	+0.500	+0.750	+0.500	+0.375	-0.250
700-719	0.000	0.000	-0.125	-0.125	+0.125	+0.500	+0.250	+0.125	-0.625
680-699	0.000	0.000	+0.125	-0.125	0.000	+0.375	+0.250	+0.125	-0.375
660-679	0.000	0.000	-0.250	-0.875	-0.875	-0.625	-0.500	-0.625	-1.000
640-659	-0.500	-0.500	-0.125	-1.250	-0.750	-0.750	-0.750	-0.875	-1.250
620-639	-0.500	-0.375	0.000	-0.875	-0.250	-0.375	-0.625	-1.000	-1.750
-619	-0.500	-0.375	0.000	-0.875	-0.250	-0.375	-0.625	-1.000	-2.000

Table 2: Sample Summary Statistics

This table presents summary statistics of key variables for the application-level (Panel A) and the cell-year-month-level (Panel B) samples. Dollar amounts are not adjusted for inflation. Data source: cHMDA.

Panel A: Application Level	n	Mean	Median	S.D.
Pre-shock guarantee fee (%)	1,665,264	0.56	0.50	0.60
Post-shock guarantee fee (%)	1,665,264	0.57	0.50	0.52
Rejected	1,665,264	0.04	0.00	0.20
Note rate (bps)	1,596,064	634.09	637.50	84.43
Upfront cost (bps)	1,548,455	206.32	184.67	129.42
Sold to GSE	1,548,455	0.70	1.00	0.46
Sold to non-GSE	1,548,455	0.17	0.00	0.37
Credit score	1,665,264	755.21	764.00	42.68
LTV (%)	1,665,264	79.23	80.00	17.62
DTI (%)	1,665,264	34.29	35.82	7.92
Applicant income ('000)	1,654,430	133.91	115.00	83.02
Loan amount ('000)	1,665,264	319.12	296.52	155.22
Refinance	1,665,264	0.05	0.00	0.21
AUS approved	1,665,264	0.94	1.00	0.24
Age	1,665,240	41.39	38.00	13.76
Female	1,665,264	0.60	1.00	0.49
Hispanic	1,665,264	0.13	0.00	0.33
Asian	1,665,264	0.09	0.00	0.29
Black	1,665,264	0.06	0.00	0.24
Other minority	1,665,264	0.01	0.00	0.11
Co-applicant	1,665,264	0.42	0.00	0.49

Panel B: Cell-Year-Month Level	n	Mean	Median	S.D.
Application volume (USD millions)	1,458	308.14	133.56	441.26
Application count	1,458	975.29	475.00	1,290.63
Originated loan volume (USD millions)	1,458	332.36	151.83	464.08
Originated loan count	1,458	1,041.15	518.00	1,350.55

Table 3: Guarantee Fee Changes and Application Characteristics – Basis Points

This table presents summary statistics on g-fee changes, expressed in basis points, and applicant characteristics. The statistics are computed using mortgage applications filed in 2022. i stands for the applicant's income in nominal USD. Data source: cHMDA.

Sample	Mean	S.D.	S.E.	1^{st}	5^{th}	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	95^{th}	99^{th}
All	1.20	32.45	0.03	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
Black	-3.95	39.25	0.13	-100.00	-87.50	-62.50	-25.00	0.00	25.00	37.50	50.00	75.00
Hispanic	0.69	35.94	0.09	-100.00	-62.50	-50.00	-12.50	0.00	25.00	37.50	50.00	75.00
Asian	5.90	30.30	0.09	-87.50	-50.00	-25.00	-12.50	0.00	25.00	37.50	50.00	75.00
18 – 29	5.77	36.73	0.07	-87.50	-62.50	-50.00	-12.50	12.50	37.50	50.00	50.00	75.00
30 – 39	1.94	32.66	0.05	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
40 – 49	-0.16	32.58	0.06	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
50 – 61	-1.46	30.37	0.06	-87.50	-62.50	-37.50	-12.50	0.00	12.50	37.50	50.00	50.00
62+	-3.00	23.91	0.06	-87.50	-50.00	-25.00	-12.50	0.00	0.00	25.00	37.50	50.00
Female	0.88	32.22	0.03	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
Single borrower	0.61	32.49	0.03	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
Two borrowers	2.07	32.36	0.04	-87.50	-62.50	-37.50	-12.50	0.00	25.00	37.50	50.00	75.00
$i < 50k$	-6.31	34.98	0.09	-100.00	-62.50	-62.50	-25.00	0.00	12.50	37.50	50.00	75.00
$50k \leq i < 100k$	-0.28	33.26	0.04	-87.50	-62.50	-50.00	-12.50	0.00	25.00	37.50	50.00	75.00
$100k \leq i < 150k$	3.54	31.51	0.05	-87.50	-62.50	-25.00	-12.50	0.00	25.00	37.50	50.00	75.00
$150k \leq i < 200k$	4.35	30.57	0.07	-87.50	-62.50	-25.00	-12.50	0.00	25.00	37.50	50.00	75.00
$200k \leq i$	3.90	29.99	0.07	-87.50	-62.50	-25.00	-12.50	0.00	25.00	37.50	50.00	75.00

Table 4: Guarantee Fee Changes and Application Characteristics – USD

This table presents summary statistics on g-fee changes, expressed in USD, and applicant characteristics. Dollar amounts are calculated by multiplying g-fee changes with loan amounts. The statistics are computed using mortgage applications filed in 2022. i stands for the applicant's income in nominal USD. Data source: cHMDA.

Sample	Mean	S.D.	S.E.	1^{st}	5^{th}	10^{th}	25^{th}	50^{th}	75^{th}	90^{th}	95^{th}	99^{th}
All	95.82	1,023	0.82	-3,274	-1,618	-1,031	-390	0	635	1,375	1,841	2,830
Black	-20.64	1,201	3.96	-3,274	-2,428	-1,625	-585	0	681	1,407	1,845	2,805
Hispanic	92.14	1,137	2.84	-3,274	-2,098	-1,328	-437	0	748	1,467	1,920	2,830
Asian	266.96	1,164	3.28	-3,274	-1,615	-1,049	-399	0	1,021	1,800	2,270	2,830
18 – 29	230.64	1,040	1.85	-2,862	-1,610	-1,057	-363	286	880	1,461	1,859	2,760
30 – 39	131.46	1,096	1.54	-3,274	-1,784	-1,124	-425	0	742	1,515	1,999	2,830
40 – 49	57.74	1,088	1.97	-3,274	-1,872	-1,163	-445	0	611	1,429	1,935	2,830
50 – 61	5.24	944	1.86	-3,274	-1,567	-973	-385	0	395	1,144	1,633	2,678
62+	-54.84	660	1.60	-2,253	-1,053	-690	-281	0	0	582	1,058	2,100
Female	85.87	1,029	1.07	-3,274	-1,655	-1,050	-400	0	623	1,377	1,845	2,830
Single borrower	78.39	960	1.00	-3,024	-1,528	-970	-375	0	570	1,265	1,710	2,681
Two borrowers	121.37	1,108	1.39	-3,274	-1,848	-1,144	-423	0	736	1,514	1,995	2,830
$i < 50k$	-71.85	537	1.32	-1,649	-1,032	-728	-313	0	177	540	748	1,230
$50k \leq i < 100k$	21.44	843	1.11	-2,498	-1,534	-975	-370	0	485	1,040	1,350	1,995
$100k \leq i < 150k$	159.44	1,122	1.71	-3,274	-1,955	-1,119	-406	0	845	1,564	1,977	2,830
$150k \leq i < 200k$	216.26	1,250	2.77	-3,274	-2,126	-1,209	-463	0	1,008	1,870	2,316	2,830
$200k \leq i$	208.93	1,304	3.20	-3,274	-2,134	-1,313	-540	0	962	2,025	2,427	2,830

Table 5: Lending Decisions

This table presents application-level lending decision regression results. The outcome variable in columns 1 and 2 is an indicator variable that equals 100 rejected applications and zero otherwise. The outcome variable in columns 3 and 4 is the note rate on the loan, expressed in basis points. The outcome variable in columns 5 and 6 is the total upfront cost, expressed as basis points of loan amount. ($\Delta G < 0$) equals one for applications in fee cut cells and zero otherwise. ($\Delta G > 0$) equals one for applications in fee hike cells and zero otherwise. Announcement equals one for applications that were processed between January 2023 and April 2023 and zero otherwise. Implementation equals one for applications that were processed from May 2023 onward and zero otherwise. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)	(5)	(6)
	Rejected		Note Rate (bps)		Upfront Cost (bps)	
$(\Delta G < 0) \times$ Announcement	0.22*	0.11	-3.60**	-3.72***	-3.87***	-4.48***
	[0.12]	[0.15]	[1.42]	[1.32]	[1.15]	[1.25]
$(\Delta G < 0) \times$ Implementation	0.13	0.07	-5.04**	-5.47**	-4.05***	-6.96***
	[0.18]	[0.13]	[2.53]	[2.28]	[1.50]	[1.46]
$(\Delta G > 0) \times$ Announcement	0.22**	0.25*	0.73	1.01*	1.35	0.71
	[0.11]	[0.14]	[0.77]	[0.58]	[0.84]	[0.73]
$(\Delta G > 0) \times$ Implementation	0.51***	0.36***	4.11***	4.81***	7.21***	4.71***
	[0.15]	[0.13]	[1.23]	[1.09]	[1.12]	[1.26]
Cell FE	Y	-	Y	-	Y	-
Year-month FE	Y	-	Y	-	Y	-
Cell FE $\times$ Refi	-	Y	-	Y	-	Y
Tract FE	-	Y	-	Y	-	Y
Lender $\times$ Year-month FE	-	Y	-	Y	-	Y
Controls $\times$ Refi	-	Y	-	Y	-	Y
Observations	1,665,264	1,656,694	1,596,064	1,587,252	1,548,455	1,539,484
R-squared	0.02	0.21	0.48	0.70	0.06	0.48

Table 6: Securitization Decisions

This table presents application-level securitization decision regression results. The outcome variable in columns 1 and 2 is an indicator variable that equals 100 if the loan was sold to Fannie Mae or Freddie Mac and zero otherwise. The outcome variable in columns 3 and 4 is an indicator variable that equals 100 if the loan was sold to a non-GSE entity and zero otherwise. The outcome variable in columns 5 and 6 is an indicator variable that equals 100 if the loan was not sold and zero otherwise. $(\Delta G < 0)$ equals one for applications in fee cut cells and zero otherwise. $(\Delta G > 0)$ equals one for applications in fee hike cells and zero otherwise. Announcement equals one for applications that were processed between January 2023 and April 2023 and zero otherwise. Implementation equals one for applications that were processed from May 2023 onward and zero otherwise. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)	(5)	(6)
	GSE-Sale		Non-GSE Sale		Unsold	
$(\Delta G < 0) \times$ Announcement	-0.28	-0.44	0.05	0.10	0.23	0.34**
	[0.43]	[0.28]	[0.40]	[0.36]	[0.31]	[0.16]
$(\Delta G < 0) \times$ Implementation	0.67	-0.33	-0.55	0.26	-0.12	0.08
	[0.40]	[0.27]	[0.40]	[0.26]	[0.36]	[0.20]
$(\Delta G > 0) \times$ Announcement	-1.43***	-0.81***	0.78**	0.38	0.64***	0.43***
	[0.40]	[0.26]	[0.37]	[0.36]	[0.20]	[0.14]
$(\Delta G > 0) \times$ Implementation	-2.51***	-1.29***	1.35***	1.05***	1.16***	0.24
	[0.49]	[0.26]	[0.36]	[0.28]	[0.33]	[0.18]
Cell FE	Y	-	Y	-	Y	-
Year-month FE	Y	-	Y	-	Y	-
Cell FE × Refi	-	Y	-	Y	-	Y
Tract FE	-	Y	-	Y	-	Y
Lender × Year-month FE	-	Y	-	Y	-	Y
Controls × Refi	-	Y	-	Y	-	Y
Observations	1,548,455	1,539,484	1,548,455	1,539,484	1,548,455	1,539,484
R-squared	0.07	0.53	0.01	0.45	0.10	0.68

Table 7: Securitization Decisions – Banks and Non-Banks

This table presents application-level OLS regression results for differential securitization response to changes in guarantee fees across lender type. The outcome variables, $(\Delta G < 0)$, $(\Delta G > 0)$, Announcement, and Implementation are defined as in previous tables. Non-bank equals one if the lender is a non-bank as classified by the 2022 Avery file and zero otherwise. Unclassified lenders are dropped. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)	(5)	(6)
	GSE-Sale		Non-GSE Sale		Unsold	
$(\Delta G < 0) \times$ Announcement	-1.96***	-2.08***	0.56**	0.54**	1.41***	1.54***
	[0.43]	[0.48]	[0.22]	[0.23]	[0.35]	[0.38]
$(\Delta G < 0) \times$ Implementation	-0.83	-0.89	0.41*	0.40	0.42	0.49
	[0.62]	[0.65]	[0.24]	[0.25]	[0.55]	[0.56]
$(\Delta G > 0) \times$ Announcement	-1.95***	-2.03***	0.80***	0.88***	1.15***	1.14***
	[0.39]	[0.38]	[0.23]	[0.25]	[0.34]	[0.33]
$(\Delta G > 0) \times$ Implementation	-2.84***	-2.66***	1.49***	1.41***	1.34**	1.25**
	[0.63]	[0.63]	[0.21]	[0.21]	[0.52]	[0.51]
$(\Delta G < 0) \times$ Announcement $\times$ Non-bank	2.37**	2.55***	-0.70	-0.68	-1.67***	-1.87***
	[0.93]	[0.92]	[0.70]	[0.70]	[0.38]	[0.41]
$(\Delta G < 0) \times$ Implementation $\times$ Non-bank	0.91	0.93	-0.40	-0.35	-0.51	-0.57
	[0.87]	[0.84]	[0.56]	[0.54]	[0.59]	[0.58]
$(\Delta G > 0) \times$ Announcement $\times$ Non-bank	1.92**	1.96**	-0.80	-0.81	-1.13***	-1.15***
	[0.88]	[0.84]	[0.70]	[0.69]	[0.36]	[0.35]
$(\Delta G > 0) \times$ Implementation $\times$ Non-bank	2.36**	2.28**	-0.87	-0.80	-1.49***	-1.48***
	[0.98]	[0.90]	[0.56]	[0.52]	[0.54]	[0.49]
Lender $\times$ Cell FE	Y	Y	Y	Y	Y	Y
Lender $\times$ Year-month FE	Y	Y	Y	Y	Y	Y
Cell FE $\times$ Refi	-	Y	-	Y	-	Y
Tract FE	-	Y	-	Y	-	Y
Controls $\times$ Refi	-	Y	-	Y	-	Y
Observations	1,507,613	1,503,582	1,507,613	1,503,582	1,507,613	1,503,582
R-squared	0.52	0.56	0.45	0.49	0.67	0.70

Table 8: Lending Decisions – Small-Dollar Mortgages

This table presents application-level lending decision regression results for small-dollar mortgage applications. The outcome variables, ($\Delta G < 0$), ($\Delta G > 0$), Announcement, and Implementation are defined as in previous tables. SDM equals one for small-dollar mortgage applications, defined as applications with loan amount less than or equal to $150,000, and zero otherwise. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)	(5)	(6)
	Rejected		Note Rate (bps)		Upfront Cost (bps)	
$(\Delta G < 0) \times$ Announcement	0.24	0.08	-3.63**	-3.90***	-3.11**	-3.89***
	[0.15]	[0.15]	[1.51]	[1.37]	[1.21]	[1.17]
$(\Delta G < 0) \times$ Implementation	0.07	0.02	-6.01**	-6.49***	-4.13***	-6.28***
	[0.16]	[0.11]	[2.49]	[2.16]	[1.29]	[1.36]
$(\Delta G > 0) \times$ Announcement	0.30**	0.25*	0.28	0.6	2.71***	1.65**
	[0.13]	[0.14]	[1.01]	[0.72]	[0.86]	[0.68]
$(\Delta G > 0) \times$ Implementation	0.43***	0.29***	2.69*	3.73***	7.51***	6.04***
	[0.13]	[0.11]	[1.38]	[0.93]	[1.09]	[1.26]
$(\Delta G < 0) \times$ Announcement $\times$ SDM	0.00	0.31	-2.33	-1.78	-2.88	-2.85
	[0.41]	[0.33]	[1.90]	[1.74]	[2.22]	[1.74]
$(\Delta G < 0) \times$ Implementation $\times$ SDM	0.38	0.41	-0.68	-0.14	1.07	-2.42
	[0.33]	[0.30]	[2.40]	[2.28]	[2.69]	[2.17]
$(\Delta G > 0) \times$ Announcement $\times$ SDM	-0.03	0.24	-2.64	-3.21**	-7.36***	-7.82***
	[0.48]	[0.42]	[1.73]	[1.36]	[2.51]	[1.88]
$(\Delta G > 0) \times$ Implementation $\times$ SDM	0.71**	0.67***	-4.53***	-5.63***	-8.00***	-9.42***
	[0.29]	[0.23]	[1.32]	[1.03]	[2.70]	[2.20]
Loan Amount Controls Omitted	Y	Y	Y	Y	Y	Y
SDM $\times$ Cell FE	Y	Y	Y	Y	Y	Y
SDM $\times$ Year-month FE	Y	Y	Y	Y	Y	Y
Lender $\times$ Year-month FE	-	Y	-	Y	-	Y
Cell FE $\times$ Refi	-	Y	-	Y	-	Y
Tract FE	-	Y	-	Y	-	Y
Controls $\times$ Refi	-	Y	-	Y	-	Y
Observations	1,665,264	1,656,694	1,596,064	1,587,252	1,548,455	1,539,484
R-squared	0.02	0.21	0.48	0.70	0.16	0.50

Table 9: Aggregate Credit Supply Effects

This table presents cell-year-month-level credit supply regression results. The outcome variables in columns 1 and 2 are the mortgage application volume, measured in millions of USD and application count, respectively. The outcome variables in columns 3 and 4 are the mortgage origination volume, measured in millions of USD and loan count, respectively. $(\Delta G < 0)$ equals one for applications in fee cut cells and zero otherwise. $(\Delta G > 0)$ equals one for applications in fee hike cells and zero otherwise. Announcement equals one for applications that were processed between January 2023 and April 2023 and zero otherwise. Implementation equals one for applications that were processed from May 2023 onward and zero otherwise. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)
	Application Volume		Origination Volume	
	USD millions	#	USD millions	#
$(\Delta G < 0) \times$ Announcement	-36.9	-92.58	26.92	120.49
	[31.28]	[68.35]	[20.87]	[79.55]
$(\Delta G < 0) \times$ Implementation	-5.57	-5.64	-10.12	-18.72
	[14.36]	[45.31]	[17.96]	[53.07]
$(\Delta G > 0) \times$ Announcement	-15.71	-101.51	-71.29**	-206.66**
	[33.98]	[74.78]	[28.01]	[101.39]
$(\Delta G > 0) \times$ Implementation	-101.25***	-312.87***	-117.86***	-372.51***
	[23.89]	[78.97]	[28.26]	[90.70]
Cell FE	Y	Y	Y	Y
Year-month FE	Y	Y	Y	Y
Observations	1,458	1,458	1,458	1,458
R-squared	0.91	0.92	0.96	0.96

A Appendix

A.1 Regression Variable Definition

This section presents the definitions of right-hand-side variables that are not fully defined in the main text and gives details on the way in which the control variables are included in each regression.

Age Group Indicator Variables – A set of indicator variables that captures the age group that the main applicant belongs to. The age groups are 18 to 29, 30 to 39, 40 to 49, 50 to 61, 62 or older, and missing. The first age group is used as the reference group.

Female – Equals one if a borrower is female and zero otherwise.

Asian – Equals one if a borrower is Asian and zero otherwise.

Black – Equals one if a borrower is Black and zero otherwise.

Other minority – Equals one if a borrower belongs to a racial minority group that is not Asian or Black and zero otherwise.

Hispanic – Equals one if a borrower is Hispanic and zero otherwise.

Unknown ethnicity – Equals one if a borrower's ethnicity is unknown and zero otherwise.

Unknown gender – Equals one if a borrower's gender is unknown and zero otherwise.

Both genders – Equals one if a borrower reported being both male and female and zero otherwise.

Unknown race – Equals one if a borrower's race is unknown and zero otherwise.

Co-applicant – Equals one if there are two borrowers on the application and zero otherwise.

Refinance – Equals one if the mortgage is a simple refinance mortgage and zero otherwise.

DTI indicator variables – Applications are sorted into groups according to the loan's DTI ratio value. DTI ratio values between 0 and 30 form 5-point groups. DTI ratio values between 31 and 45 form 1-point groups.

Income indicator variables – Applications are sorted into groups according to the main applicant's annual income. The reference group is made up of applicants 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 applications associated with applicants with income values greater than $499,999. Applications that have missing income values form a separate group.

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 $1 and $100,000. The remaining groups are formed by $100,000 increments of loan amounts up to $500,000. The final group is made up of loans with loan amounts greater than $500,000.

AUS approved – Equals one if the application was approved by at least one AUS and zero otherwise.

Non-bank – Equals one if the TYPE variable in the Avery File equals 40 or 41 and zero otherwise.

SDM – Equals one if the loan amount is less than or equal to $150,000 and zero otherwise.

Additional Comments

Unless stated otherwise, all application-level regressions that include the full set of control variables and fixed effects use the same set of control variables, which are listed above. The full set of control variables include all demographic variables (e.g., race, ethnicity, gender, and age), co-applicant by refinance indicator variable, g-fee cell fixed effects by refinance indicator variable, AUS Approved by refinance indicator variable, income bin by refinance indicator variable, DTI bin by refinance indicator variable, loan amount bin by refinance indicator variable, lender by year-month fixed effects, and tract fixed effects. These interactions are labeled as "Controls $\times$ Refi" in the regression tables. The table labels indicate when certain controls are omitted. For example, small-dollar mortgage regressions (Table 8) exclude loan amount control variables.

A.2 Appendix Tables

Table A1: Credit Risk and Guarantee Fee Change Shock

This table presents application-level OLS regression results where observable credit risk characteristics are regressed onto the g-fee change treatment variables. In columns 1 and 2, the sample includes all submitted applications. In columns 3 and 4, the sample includes all approved applications. In columns 5 and 6, the sample includes all originated mortgages. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	DTI (%)	ln(Income)	DTI (%)	ln(Income)	DTI (%)	ln(Income)
$(\Delta G < 0) \times$ Announcement	-0.043	-0.001	-0.02	-0.001	-0.022	0.000
	[0.044]	[0.003]	[0.040]	[0.003]	[0.041]	[0.003]
$(\Delta G < 0) \times$ Implementation	-0.091	-0.005	-0.071	-0.005	-0.081	-0.004
	[0.135]	[0.003]	[0.129]	[0.003]	[0.128]	[0.003]
$(\Delta G > 0) \times$ Announcement	0.007	0.000	0.017	0.000	0.012	0.000
	[0.041]	[0.003]	[0.043]	[0.003]	[0.044]	[0.003]
$(\Delta G > 0) \times$ Implementation	0.067	-0.005	0.069	-0.005	0.062	-0.005
	[0.116]	[0.004]	[0.116]	[0.004]	[0.116]	[0.004]
Sample	All Applications		Approved		Originated	
DTI Controls Omitted	Y	-	Y	-	Y	-
Income Controls Omitted	-	Y	-	Y	-	Y
Cell FE $\times$ Refi	Y	Y	Y	Y	Y	Y
Tract FE	Y	Y	Y	Y	Y	Y
Lender $\times$ Year-month FE	Y	Y	Y	Y	Y	Y
Controls $\times$ Refi	Y	Y	Y	Y	Y	Y
Observations	1,656,694	1,641,878	1,587,616	1,575,403	1,539,484	1,527,523
R-squared	0.338	0.638	0.356	0.64	0.357	0.64

Table A2: Approved but Not Originated Mortgage Applications

This table presents application-level OLS regression results where the outcome variable is an indicator variable that equals 100 if the application was approved but the borrower decided not to originate the mortgage and zero otherwise. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)
	Approved Not Originated	
$(\Delta G < 0) \times$ Announcement	0.03	-0.06
	[0.14]	[0.10]
$(\Delta G < 0) \times$ Implementation	-0.04	-0.05
	[0.14]	[0.10]
$(\Delta G > 0) \times$ Announcement	0.22	0.15
	[0.14]	[0.10]
$(\Delta G > 0) \times$ Implementation	0.19	0.08
	[0.15]	[0.09]
Cell FE	Y	-
Year-month FE	Y	-
Cell FE $\times$ Refi	-	Y
Tract FE	-	Y
Lender $\times$ Year-month FE	-	Y
Controls $\times$ Refi	-	Y
Observations	1,665,264	1,656,694
R-squared	0.00	0.16

Table A3: DTI Ratio Limit Test

This table presents application-level OLS regression results where DTI-related outcome variables are regressed onto the g-fee change treatment variables. The outcome variable in column 1 is an indicator variable that equals 100 if the application was rejected because of high DTI ratio and zero otherwise. The outcome variable for column 2 is the application's DTI ratio, expressed in percentage points. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)
	Rejected for DTI	DTI (%)
$(\Delta G < 0) \times$ Announcement	0.01	-0.32
	[0.03]	[0.353]
$(\Delta G < 0) \times$ Implementation	0.03	-0.22
	[0.03]	[0.46]
$(\Delta G > 0) \times$ Announcement	-0.03	-0.58
	[0.03]	[0.44]
$(\Delta G > 0) \times$ Implementation	-0.03	0.26
	[0.03]	[0.43]
Application Sample	All	Rejected
DTI Controls Omitted	-	Y
Cell FE $\times$ Refi	Y	Y
Tract FE	Y	Y
Lender $\times$ Year-month FE	Y	Y
Controls $\times$ Refi	Y	Y
Observations	1,656,694	40,477
R-squared	0.24	0.60

Table A4: Lending Decision Effects by Treatment Dosage Level

This table presents application-level OLS regression results where lending outcome variables are regressed onto by-dosage-level g-fee shock indicator variables. There is no -2% shock term because we drop observations with credit scores less than 620. Refer to the appendix for a detailed discussion of control variables. Announcement effect terms are included in the regressions but omitted from the table for brevity. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)
	Rejected	Note Rate (bps)	Upfront Cost (bps)	All-in Price (bps)
$(\Delta G = -1.75\%)\times$ Implementation	-2.82***	-6.98***	-5.96	-25.71***
	[0.53]	[1.94]	[3.85]	[7.86]
$(\Delta G = -1.25\%)\times$ Implementation	-2.13***	-26.82***	-26.02***	-114.11***
	[0.36]	[1.56]	[3.90]	[7.90]
$(\Delta G = -1\%)\times$ Implementation	-0.24	-21.34***	-29.54***	-99.71***
	[1.11]	[1.54]	[4.23]	[9.13]
$(\Delta G = -0.875\%)\times$ Implementation	-0.82***	-22.23***	-29.63***	-105.38***
	[0.28]	[1.18]	[1.85]	[4.34]
$(\Delta G = -0.75\%)\times$ Implementation	-0.97	-26.10***	-28.50***	-118.46***
	[0.65]	[0.88]	[2.04]	[3.78]
$(\Delta G = -0.625\%)\times$ Implementation	0.19	-13.34***	-6.47	-51.28***
	[0.16]	[2.27]	[4.93]	[11.92]
$(\Delta G = -0.5\%)\times$ Implementation	0.17	-11.50***	-7.35	-46.55***
	[0.42]	[2.23]	[4.95]	[12.43]
$(\Delta G = -0.375\%)\times$ Implementation	-0.39	-13.13***	-6.24***	-49.53***
	[0.81]	[2.23]	[1.03]	[8.26]
$(\Delta G = -0.25\%)\times$ Implementation	0.21	-3.08**	-4.09**	-14.84***
	[0.15]	[1.41]	[1.89]	[4.17]
$(\Delta G = -0.125\%)\times$ Implementation	0.16	0.18	-5.09***	-5.17
	[0.12]	[1.02]	[0.71]	[3.53]
$(\Delta G = +0.125\%)\times$ Implementation	0.22	1.87	0.54	6.56
	[0.14]	[1.63]	[1.52]	[5.48]
$(\Delta G = +0.25\%)\times$ Implementation	0.49***	4.43**	3.87***	17.79**
	[0.13]	[1.72]	[1.46]	[6.95]
$(\Delta G = +0.375\%)\times$ Implementation	0.47***	6.77***	7.98***	29.53***
	[0.16]	[1.03]	[1.64]	[4.18]
$(\Delta G = +0.5\%)\times$ Implementation	0.21	6.17***	6.73***	26.62***
	[0.16]	[1.16]	[1.20]	[4.63]
$(\Delta G = +0.75\%)\times$ Implementation	0.55***	11.50***	12.60***	50.57***
	[0.18]	[1.18]	[2.53]	[6.34]
Announcement Effect Terms	Y	Y	Y	Y
Cell FE × Refi	Y	Y	Y	Y
Tract FE	Y	Y	Y	Y
Lender × Year-month FE	Y	Y	Y	Y
Controls × Refi	Y	Y	Y	Y
Observations	1,656,694	1,587,252	1,539,484	1,539,453
R-squared	0.21	0.70	0.48	0.72

Table A5: Lending Decisions – Banks and Non-Banks

This table presents application-level OLS regression results for differential responses to changes in guarantee fees across lender type. The outcome variables, ($\Delta G < 0$), ($\Delta G > 0$), Announcement, and Implementation are defined as in previous tables. Non-bank equals one if the lender is a non-bank as classified by the 2022 Avery file and zero otherwise. Unclassified lenders are dropped. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)	(3)	(4)	(5)	(6)
	Rejected		Note Rate (bps)		Upfront Cost (bps)	
$(\Delta G < 0) \times$ Announcement	-0.02	-0.12	-3.02**	-2.80**	-5.16***	-5.01***
	[0.22]	[0.24]	[1.40]	[1.32]	[1.18]	[1.13]
$(\Delta G < 0) \times$ Implementation	0.15	0.13	-5.24**	-5.10**	-4.37***	-4.72***
	[0.15]	[0.14]	[2.42]	[2.39]	[1.42]	[1.22]
$(\Delta G > 0) \times$ Announcement	0.26	0.25	1.84**	1.64**	-0.44	-0.45
	[0.21]	[0.23]	[0.73]	[0.72]	[1.25]	[0.89]
$(\Delta G > 0) \times$ Implementation	0.49***	0.32**	4.10***	4.29***	5.50***	3.04**
	[0.15]	[0.14]	[1.16]	[1.14]	[1.43]	[1.19]
$(\Delta G < 0) \times$ Announcement $\times$ Non-bank	0.41	0.38	-0.88	-1.11	1.65	1.02
	[0.25]	[0.24]	[0.89]	[0.87]	[1.13]	[1.31]
$(\Delta G < 0) \times$ Implementation $\times$ Non-bank	-0.08	-0.02	-0.44	-0.42	-2.54	-2.44*
	[0.16]	[0.16]	[0.80]	[0.68]	[1.59]	[1.26]
$(\Delta G > 0) \times$ Announcement $\times$ Non-bank	0.16	0.08	-1.28	-0.88	1.33	1.46
	[0.20]	[0.21]	[0.81]	[0.84]	[1.09]	[1.05]
$(\Delta G > 0) \times$ Implementation $\times$ Non-bank	-0.02	0.08	0.85	0.75	1.85	3.02**
	[0.16]	[0.17]	[0.70]	[0.72]	[1.69]	[1.35]
Lender $\times$ Cell FE	Y	Y	Y	Y	Y	Y
Lender $\times$ Year-month FE	Y	Y	Y	Y	Y	Y
Cell FE $\times$ Refi	-	Y	-	Y	-	Y
Tract FE	-	Y	-	Y	-	Y
Controls $\times$ Refi	-	Y	-	Y	-	Y
Observations	1,623,391	1,619,790	1,554,992	1,551,078	1,507,613	1,503,582
R-squared	0.13	0.25	0.68	0.71	0.31	0.51

Table A6: Applicant Characteristics and Small-Dollar Mortgages

This table presents summary statistics and two-tailed t-test results for differences in observable characteristics between small-dollar mortgage (SDM) applications and other mortgage applications. Nominal USD amounts are reported.

	SDM = 1	SDM = 0	Difference	p-value
Property value (USD thousands)	200.52	455.27	-254.75	< 0.01
Income (USD thousands)	67.56	151.66	-84.1	< 0.01
Credit score	744.27	756.95	-12.68	< 0.01
DTI (%)	32.05	34.6	-2.55	< 0.01
LTV (%)	68.99	80.85	-11.86	< 0.01
Black (%)	6.42	5.81	0.61	< 0.01
Hispanic (%)	10.74	12.88	-2.14	< 0.01
Asian (%)	3.06	10.35	-7.29	< 0.01
Other minority (%)	1.23	1.09	0.14	< 0.01
Age	40.76	45.35	-4.59	< 0.01

Table A7: Lending Decision Effects by Treatment Dosage Level – Small-Dollar Mortgages

This table presents three sets of application-level OLS regression results where lending outcome variables are regressed onto by-dosage-level (d) g-fee shock indicator variables interacted with event time indicator variables and the small-dollar mortgage (SDM) indicator variable. There is no -2% shock term because we drop observations with credit scores less than 620. The complete set of interaction terms are included in the regression but omitted from the table for presentation purposes. The set of control variables and fixed effects follows the specification presented in the even-numbered columns of Table 8, respectively. Announcement effect terms are included in the regressions but omitted from the table for brevity. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

Treatment Dosage (d)	-1.75%	-1.25%	-1.00%	-0.875%	-0.75%	-0.625%	-0.50%	-0.375%	-0.25%	-0.125%	0.125%	0.25%	0.375%	0.50%	0.75%
(1) Rejected															
($\Delta G = d$)× Implementation	-1.30**	-2.44**	0.26	-0.80***	-1.57**	0.08	0.28	-0.15	0.14	0.06	0.13	0.38***	0.43***	0.21*	0.44**
	[0.6]	[1.08]	[2.17]	[0.14]	[0.72]	[0.14]	[0.31]	[0.5]	[0.13]	[0.09]	[0.12]	[0.12]	[0.13]	[0.12]	[0.2]
($\Delta G = d$)× Implementation × SDM	-3.15***	1.25	-1.26	-0.05	2.94***	0.63	-0.38	-0.7	0.56	1.14***	0.89***	1.30**	0.41	-0.14	1.13
	[1.17]	[2.78]	[3.26]	[1.21]	[0.48]	[0.56]	[0.82]	[0.88]	[0.39]	[0.39]	[0.2]	[0.56]	[0.35]	[0.39]	[0.84]
(2) Note Rate (bps)															
($\Delta G = d$)× Implementation	-8.97***	-31.08***	-25.13***	-22.31***	-26.86***	-15.23***	-13.17***	-13.96***	-4.40***	-1.20	0.83	3.11**	5.53***	5.34***	10.42***
	[1.95]	[1.28]	[1.46]	[1.00]	[0.80]	[1.77]	[1.50]	[0.96]	[1.34]	[0.81]	[1.50]	[1.47]	[0.86]	[0.91]	[0.87]
($\Delta G = d$)× Implementation × SDM	9.30***	15.19***	16.06***	-1.24	3.36	8.66***	8.01***	7.11	1.57	-3.85***	-4.79**	-4.89***	-5.12***	-7.71***	-9.12***
	[3.27]	[1.50]	[1.51]	[2.49]	[2.34]	[3.21]	[2.80]	[5.69]	[2.27]	[0.95]	[2.00]	[0.89]	[1.53]	[2.34]	[2.60]
(3) Upfront Cost (bps)															
($\Delta G = d$)× Implementation	-31.69***	-31.36***	-25.88***	-29.93***	-27.84***	-4.92	-6.10	-8.00***	-3.80**	-4.47***	1.75	5.01***	9.32***	8.06***	13.86***
	[3.85]	[2.95]	[4.26]	[1.15]	[1.90]	[4.10]	[5.19]	[1.14]	[1.75]	[0.72]	[1.54]	[1.36]	[1.57]	[1.14]	[1.64]
($\Delta G = d$)× Implementation × SDM	66.10***	19.05***	-7.83**	3.12	0.32	-4.01	-5.66**	8.31**	0.24	-2.5	-6.42**	-8.59**	-11.31***	-11.17***	-15.40*
	[6.47]	[4.48]	[3.41]	[7.66]	[5.52]	[4.39]	[2.82]	[3.70]	[2.47]	[2.68]	[3.05]	[3.73]	[2.30]	[2.91]	[9.12]
(4) All-in Price (bps)															
($\Delta G = d$)× Implementation	-48.38***	-134.09***	-106.44***	-105.09***	-118.25***	-55.38***	-50.14***	-54.64***	-18.89***	-9.18***	4.18	14.52**	26.69***	25.07***	48.16***
	[8.23]	[6.52]	[9.12]	[3.36]	[3.35]	[9.21]	[10.09]	[4.29]	[3.75]	[2.84]	[4.66]	[5.93]	[3.36]	[3.52]	[4.34]
($\Delta G = d$)× Implementation × SDM	75.82***	70.07***	38.87***	-5.5	1.58	20.78	16.72**	34.18	4.00	-16.25***	-22.06***	-27.07***	-29.14***	-37.02***	-46.54**
	[13.02]	[7.61]	[7.10]	[11.04]	[6.13]	[16.42]	[8.25]	[22.82]	[9.25]	[5.19]	[8.02]	[6.41]	[7.08]	[7.64]	[19.22]

Table A8: DTI Ratio Limit Test – Small-Dollar Mortgages

This table presents application-level OLS regression results where DTI-related outcome variables are regressed onto variants of the g-fee change treatment variables. The outcome variable in column 1 is an indicator variable that equals 100 if the application was rejected because of high DTI ratio and zero otherwise. The outcome variable for column 2 is the application's DTI ratio, expressed in percentage points. Refer to the appendix for a detailed discussion of control variables. Heteroskedasticity-robust standard errors are clustered at the g-fee cell level. *, **, and *** denote 10%, 5%, and 1% statistical significance levels, respectively. Data source: cHMDA.

	(1)	(2)
	Rejected for DTI	DTI (%)
$(\Delta G < 0) \times$ Announcement	0.04	-0.62
	[0.04]	[0.46]
$(\Delta G < 0) \times$ Implementation	0.03	-0.34
	[0.03]	[0.44]
$(\Delta G > 0) \times$ Announcement	-0.03	-0.83**
	[0.03]	[0.39]
$(\Delta G > 0) \times$ Implementation	-0.03	0.13
	[0.03]	[0.42]
$(\Delta G < 0) \times$ Announcement $\times$ SDM	-0.21	0.95
	[0.13]	[0.88]
$(\Delta G < 0) \times$ Implementation $\times$ SDM	-0.03	0.39
	[0.09]	[0.89]
$(\Delta G > 0) \times$ Announcement $\times$ SDM	-0.04	0.68
	[0.12]	[1.30]
$(\Delta G > 0) \times$ Implementation $\times$ SDM	0.00	0.35
	[0.06]	[0.92]
Application Sample	All	Rejected
Loan Amount Controls Omitted	Y	Y
DTI Controls Omitted	-	Y
SDM $\times$ Cell FE	Y	Y
SDM $\times$ Year-month FE	Y	Y
Lender $\times$ Year-month FE	Y	Y
Cell FE $\times$ Refi	Y	Y
Tract FE	Y	Y
Controls $\times$ Refi	Y	Y
Observations	1,656,694	40,477
R-squared	0.24	0.59