# The Distributional Effects of GSE Pricing on Home Purchases

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**Abstract**

The guarantee fee (g-fee) pricing of government-sponsored enterprises (GSEs) plays an important role in distributing credit. We study the distributional effects of g-fee pricing on relative home purchase volumes using a stacked differences-in-differences design based on recent g-fee adjustments. We find large elasticities of relative home purchase volume to g-fee pricing, consistent with a calibrated model of frictional housing search. Furthermore, the elasticities are larger (smaller) for higher income borrowers among the subsidized (subsidizing) credit score groups. Overall, our results suggest that g-fee pricing has large distributional effects on home purchases that may be regressive in the income dimension.

**Keywords**: Government Sponsored Enterprises (GSEs), Securitization, Redistribution, US Mortgage Market.

**JEL classification**: G18, G21, G51

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# 1 Introduction

What are the allocative effects of the government-sponsored enterprises (GSEs') pricing of credit risk in terms of home purchases? Fannie Mae and Freddie Mac, through their guarantee fees (g-fees), influence the pricing of mortgage credit for over half of all U.S. homebuyers. While in principle these fees could reflect expected credit losses and capital costs, in practice they embed a cross-subsidy: safer borrowers with higher credit scores and lower loan-to-value ratios are typically estimated to pay more than their actuarially fair price, resulting in riskier borrowers being subsidized.[1] This design is longstanding, yet its consequences for credit access and the distribution of homeownership remain poorly understood.[2]

This paper provides new evidence on the effects of these cross-subsidies in terms of relative home purchases of U.S. households. In 2023, the Federal Housing Finance Agency (FHFA) implemented a major restructuring of loan-level price adjustments (LLPAs), changing g-fees in a highly non-linear way across credit score (CS) and loan-to-value (LTV) buckets. Leveraging the discontinuous nature of these changes, we estimate how borrowers' relative home purchase volumes respond to shifts in mortgage pricing, and how those responses vary across the income and CS distributions.

We document three main findings. First, relative home purchase volume is strongly sensitive to g-fee changes, even at narrow credit score cutoffs, with those receiving a reduction in g-fees increasing their home purchases relative to those experiencing an increase. This implies that GSE pricing has large distributional effects on the relative home purchase volumes of different borrower groups, and is consistent with a model of frictional housing search. Second, this responsiveness is not uniform: among lower-CS borrowers who receive subsidies, higher-income borrowers are more responsive. Among higher-CS borrowers who pay subsidies, higher-income borrowers are less responsive. Third, and as a consequence of the second point, the resulting redistribution may be regressive: the nominally progressive design of g-fee pricing,

[1] See, e.g. earlier analyses in Gerardi (2017) as well as FHFA g-fee report 2018, 2019–2021.

[2] This redistribution through g-fees is often controversial and subject to political debate, see media reports from e.g., Wall Street Journal, USA Today, and New York Times. Many market participants anticipate the second Trump administration will release the GSEs from conservatorship and alter g-fee pricing, and our analyses may help inform the potential effects of prospective policy actions on the distribution of home purchases.

charging less (in relative terms) to lower-CS borrowers and more to higher-CS borrowers, can benefit higher-income borrowers more than lower-income borrowers. These results highlight an important tradeoff between risk-based subsidies and income-based targeting in GSE mortgage policy.[3]

For identification of the effects of g-fees on home purchases, we use a stacked difference-in-differences (DID) design (Cengiz et al., 2019), exploiting a recent change in g-fees. Sellers of loans to the GSEs pay an upfront g-fee based on the loan-level price adjustment (LLPA), which differs by discrete grids of credit score and loan-to-value ratio (LTV). In January 2023, the GSEs announced significant changes to the LLPA, leading to differential changes to g-fees across LLPA grids. In particular, the differences in the g-fee changes were particularly large for borrowers in CS grids below and above 640, 680, 720, 760, and 780. By examining differences in home purchase volumes pre-and-post the new LLPA between borrowers with similar CS but located on the opposite sides of each of the five CS cutoffs (cutoff ± 10), we estimate semi-elasticities of relative home purchase volumes with respect to g-fees across the credit score distribution. We also estimate how these semi-elasticities differ by income conditional on credit scores, as earlier studies have found them to be only moderately correlated (Beer et al., 2018). Although the LLPA varies with LTVs, we do not use LTV cutoffs for identification because LTVs can be endogenous for households that are relatively liquidity unconstrained.

To achieve clean identification, we rule out many forms of borrower and lender confounders. First, a potential threat is time-varying shocks in demand that are correlated with CS. Our stacked DID compares narrow categories of CS above-and-below each cut-off, and include a separate time trend for the group of borrowers with CS near each CS cutoff. Second, CS manipulation is another potential concern. However, exact tri-bureau credit scores used for g-fee pricing are difficult to predict ex ante and are unlikely to be manipulated right around the thresholds.[4] Using

[3]One way of resolving this trade-off is for the GSEs to price based on income as well, but a recent attempt in this direction, by adding debt-to-income (DTI) ratios to the GSE pricing grid, came with operational challenges due to income uncertainty in the underwriting process and was ultimately abandoned as announced HERE. We show in Appendix Figure A.9 that credit risk is far more related to CS than DTI distribution in the Fannie Mae Single-Family Loan Performance Data. Given the operational challenges of directly pricing on income and the significantly greater importance of CS in predicting default risk compared to DTI, we take the set of factors that the GSEs intentionally price on, which are CS and LTV, as given for our analyses.

[4]Other papers using sharp credit score thresholds as identification include Keys et al. (2010); Collier and Ellis (2024).

panel data of mortgage borrowers matched to their credit records, we further conduct a series of tests directly examining credit score manipulation, and finding little evidence of differential manipulation around our thresholds pre-or-post policy. Third, examining pre-trends, we see little evidence of pre-trends in our estimates, which is evidence of limited lender and borrower anticipation. Fourth, an important advantage over a regression discontinuity approach, which only uses cross-sectional variation, employed in the earlier literature (Lo, 2017; Tsai, 2023) is that we allow for both time-invariant differences in underwriting standards at the CS cutoffs as well as time-varying changes to underwriting standards as long as those differences are attributable to g-fee changes.

Our estimates show that changes in g-fees have large effects on home purchases of those experiencing a decrease relative to those with similar CS but located on the other side of the CS cutoff. The estimated semi-elasticities of relative home purchases vary across the credit scores and income groups in interesting ways. Overall, a 100 basis point (bps) decline in g-fees, which by our estimates lead to a 27 to 32 bps decline in fee-adjusted interest rate, leads to a 13.9% increase in relative home purchase originations compared to borrowers who experienced less of a decline. This is larger than, though in a similar order of magnitude of, a 14% increase in FHA-likely borrowers in response to a 50 bps decline in FHA interest rates as explored in Bhutta and Ringo (2021). While Hurst et al. (2016) emphasize the cross-regional cross-subsidies induced by g-fees, our estimate implies that similar forces can have large allocative effects even within regions by shifting the composition of home purchasers along the credit score and income distributions. Furthermore, unlike FHA's pricing, which applies to a narrower segment of the market, GSE pricing apply across a broader market, making the allocative effects potentially more widespread.

The responsiveness of relative home purchases to g-fee reductions varies significantly by credit score and household income in non-linear ways. We measure borrower household income being relative to the area median income (AMI), and examine heterogeneity by borrower income in four categories: the bottom tercile (with income ranging between 1 to about 1.33 times of the AMI), the 2nd tercile (1.33 to 1.87 times of the AMI), between 66th to 90th percentile

(1.87 to 3.1 times of the AMI), and the over 90th percentile.[5] We exclude borrowers whose income is below the AMI due to a large proportion of them being first-time home buyers who are unaffected by the LLPA change in 2023.[6]

Among lower-CS borrowers with credit scores below 700, we observe a sharp income gradient in semi-elasticities. Low-income borrowers (below the 33rd percentile) respond with a semi-elasticity of 9.6%, likely reflecting binding constraints like liquidity and FHA dependence. Borrowers between the 33rd and 66th percentiles respond more strongly, at 14.9%, while those in the 66th to 90th percentile exhibit a substantially higher elasticity of 15.7%. Notably, borrowers in the top 10% of the income distribution (above the 90th percentile) respond the most within this low-credit group, with an estimated semi-elasticity of 25.3%. Together with the fact that the GSE share among lower-income, lower-CS home buyers is much lower than the FHA/VA share, this sharp income gradient in semi-elasticities suggests that lower-income, lower-CS borrowers are too financially constrained for GSE loans. As a result, GSEs' policies aimed at supporting low-CS borrowers may disproportionately benefit those at the top of the income distribution within that credit segment. Moreover, our additional analysis based on the National Mortgage Database shows that these higher-income, low-CS borrowers are less likely to be young, first-time home buyers than other low-CS borrowers.

In contrast, among higher-CS borrowers with credit scores of 740 and above, responsiveness is stable but dramatically reduced at the top of the income distribution. Borrowers in the bottom 33rd percentile of the income distribution have a semi-elasticity of 17.1%. Borrowers in the 33rd to 66th percentile of the income distribution and those in the 66th–90th percentile are not statistically significantly different. However, borrowers in the top 10% of the income distribution in this higher-CS segment have a semi-elasticity of 5.6%. These borrowers are more likely to obtain jumbo or non-agency loans, where g-fees do not apply, making them functionally insulated from GSE pricing changes. In the mid-CS borrowers with credit scores between 700 and 739, responsiveness to g-fee changes is elevated across all income groups and

[5]For context, the median AMI across MSA/MDs is $90,800, implying that our over 90th percentile cut-off of 3.1 is $281,480. The median income of households in this group is $392,000.

[6]First-time home buyers whose income is below the AMI were exempted from upfront g-fees in a 2022 policy HERE.

relatively stable.

Altogether, these results suggest that GSE cross-subsidies to lower-CS borrowers have significant distributional implications in terms of the allocation of home purchases, and these distributional effects may be regressive in the income dimension. Nevertheless, our large estimated semi-elasticities require further discussion. In theory, borrowers can reduce their home purchase value on the intensive margin in response to increases in borrowing cost instead of reducing their home purchases on the extensive margin. Yet, in a housing market equilibrium with a fixed number of sellers, not all borrowers will be able to downsize, and g-fee pricing that gives some borrowers an advantage over others may have large distributional effects in terms of who ends up purchasing the homes.

To understand the housing search frictions necessary to fit our large extensive-margin effect and to simulate the effects of counterfactual g-fee designs on housing markets, we calibrate a structural model of frictional housing search based on Jiang et al. (2024). Importantly, this model matches many in-sample housing search moments with reasonable parameters, including (1) buyers' weeks searched before purchase, (2) sellers' days on market, (3) number of offers before success, (4) average house price, (5) idiosyncratic house price dispersion, and (6) buyer pre-approval to home purchase success probability, which allows it to quantify housing search frictions in a serious way. In addition to rationalizing our large extensive-margin effect with reasonable parameters, our model also makes an out-of-sample prediction of a small intensive-margin elasticity of 3%, consistent with the 2% estimated in DeFusco and Paciorek (2017). Furthermore, it predicts out-of-sample a 21% increase in the share of FICO<680 borrowers in response to a 50 bps decline in FHA interest rates, which is larger but in the same magnitude as the 14% estimated in Bhutta and Ringo (2021). Our larger out-of-sample prediction may reflect tighter housing markets during our sample period.

Diving deeper into mechanisms, our model implies that approximately two-thirds of the large extensive-margin response to GSE pricing is due to an allocative effect across different buyers in housing search rather than a decrease in buyer entry due to higher interest rates: if we hold buyer entry constant in the model, but apply the GSE price changes to buyers already in the market, the relative home purchase semi-elasticities fall to 9.1 percentage points from a base

of 14.0 percentage points. Therefore, our model attributes a majority of the extensive margin effect to a competition channel, in which differences in financing costs can have large allocative effects, rather than extensive-margin effects on buyer entry. These effects may be particularly large for DTI-constrained borrowers, as they are more constrained in terms of increasing the attractiveness of their offers in response to seller demands.

# 2 Literature Review

Our paper is primarily related to the literature on GSE cross-subsidies. Previous papers studied how relatively homogeneous g-fee pricing leads to cross-subsidies across regions with different house price risk (Hurst et al., 2016), across borrowers with different prepayment speeds (Huh and Kim, 2023; Berger et al., 2024; Zhang, 2024), and across houses facing different climate risk (Ouazad and Kahn, 2022; Sastry et al., 2023; Gete et al., 2024). However, these cross-subsidies were mostly unintended consequences of the GSEs' relatively homogeneous pricing structure, whereas the GSEs' imperfect adjustment of their g-fees by credit scores and LTVs, as well as the level of their g-fees, are purposeful actions. We provide novel evidence on the effectiveness and distributional impacts of these purposeful g-fee pricing structures, showing that they have large allocative effects on home purchases. Our elasticities may also be useful for calibrating macroeconomic models of GSE reform.

Our paper also contributes to policy discussions regarding the role of GSEs, which have a large impact on the market through their credit guarantee (Lucas and McDonald, 2010; Jeske et al., 2013; Elenev et al., 2016; Gete and Zecchetto, 2018; Grundl and Kim, 2021; Gete et al., 2023; Bosshardt et al., 2024b) and the liquidity of their mortgage-backed securities (Vickery and Wright, 2013; Huh and Kim, 2022). The cross-subsidies embedded in GSE pricing strategies can be controversial. For example, Richardson et al. (2017) argue against GSE redistribution by noting that "any subsidies should be transparent, explicit, and on-budget; none of those characteristics apply to the cross-subsidies that would occur through a distorted PLMBS market [i.e. the GSEs]." Our paper contributes to increased transparency regarding GSE cross-subsidies by credit scores and LTVs as well as their impacts across borrower populations.

We also contribute to the literature by quantifying the housing search frictions necessary to explain a large extensive-margin combined with a small intensive margin elasticity of home purchase mortgage demand to interest rates. The intuition is not new, and may be attributable to Bhutta and Ringo (2021).[7] Our model implies that their intuition can indeed rationalize the elasticities found in the literature in a reasonably quantitative manner. However, we note that an important difference with Bhutta and Ringo (2021) is that not all of our semi-elasticities are limited to borrowers facing DTI constraints: while our estimated semi-elasticities are twice as large for DTI-constrained borrowers compared to non-DTI constrained borrowers, they are still sizable for non-DTI constrained borrowers. It may be reasonable for GSE mortgage borrowers, who may be wealthier and less financially constrained, to have a lower DTI target when making their home purchase offers as compared to FHA mortgage borrowers.[8]

Furthermore, our paper contributes to the literature on housing search. This literature is large, and includes Wheaton (1990), Krainer (2001), Albrecht et al. (2007), Novy-Marx (2009), Piazzesi and Schneider (2009), Genesove and Han (2012), Ngai and Tenreyro (2014), Head et al. (2014), Gabrovski and Ortego-Marti (2019), Ouazad and Rancière (2019), Piazzesi et al. (2020), Anenberg and Bayer (2020), Guren and McQuade (2020), Buchak et al. (2020), Gabrovski and Ortego-Marti (2021), Sagi (2021), Buchak et al. (2024b), and Jiang et al. (2024). Within this literature, our model is particularly related to Jiang et al. (2024), who we build upon, and Gerardi et al. (2024), which also uses a similar method of endogenizing buyer and seller flows.

An important consideration for putting our home purchase elasticities in context is the extent to which the g-fees are pass-through to borrowers. While several contemporaneous working papers examine this issue, we also contribute to this literature to a smaller extent,

[7] As Bhutta and Ringo (2021) note, the "stark difference in elasticities [between the small intensive margin elasticity and the large extensive margin elasticity found in the literature] highlights the fact that housing markets are not characterized by borrowers frictionlessly optimizing over a continuous set of options. Rather, frictional search with long wait times is the norm. Conditional on finding a home, many households will make the minimum down payment necessary to qualify for a loan. A small interest rate elasticity along the intensive margin under these conditions is not surprising. The frictional nature of housing markets also helps explain the large extensive margin elasticity."

[8] When matched with a house, DTI-constrained borrowers may be forced to reduce the attractiveness of their offers by credit supply constraints, and therefore may be more affected by housing search frictions compared to other borrowers that have more financial flexibility. However, other borrowers may also be affected by housing search frictions and become less able to win homes if they are unwilling to go higher than a certain level of target DTI.

by examining pass-through across banks and non-banks. Amornsiripanitch and Ricks (2024) examines the pass-through of these g-fees to borrowing costs over the same period, and find symmetric pass-through across g-fee increases and decreases that may be considered complete depending on the conversion factor used between interest rate and fees. In addition, they find elevated rejection rates in response to g-fee increases, which may also partially explain some of our elasticities. In contrast to the symmetric pass-through result, Kalda et al. (2025) finds some asymmetry in pass-through by the direction of g-fee change. We find in our stacked DID specification that a 100 bps decline in g-fees leads to declines in mortgage rates and net origination charges for GSE loans by 21 bps and 29 bps, respectively. Interestingly, we also find more pass-through to non-agency conforming mortgages among non-bank lenders, which is novel to this literature, and is potentially indicative of the pricing of aggregators who purchase loans from non-banks and subsequently keep some of them in portfolio (Benson et al., 2024; Zheng, 2024).

Finally, our paper adds to the literature on post-crisis changes in the US mortgage market. This includes the increased use of leverage regulation (DeFusco et al., 2020), regressive redistribution across loan sizes (D'Acunto and Rossi, 2022), changes in the price of financial intermediation (Fuster et al., 2017), the role of servicers in COVID-19 forebearance relief (Kim et al., 2024), the role of lender capacity constraints during COVID-19 (Fuster et al., 2021), and the effect of rising interest rates more broadly (Bosshardt et al., 2024a).

# 3 Background

The GSEs charge two types of g-fees to lenders selling loans to them: ongoing g-fees and upfront g-fees. The ongoing g-fees are monthly charges, which are typically set in bulk and do not vary by loan-level credit characteristics. The upfront g-fees is a one-time fee per loan paid at the time of the delivery. In the aftermath of the Great Financial Crisis, the GSEs introduced risk-based pricing of the upfront g-fees based on the loan level price adjustments (LLPAs), which adjust the upfront g-fees to a matrix of credit characteristics mainly determined by credit

score and LTV combinations.[9] This risk-based pricing for the GSEs stands in contrast with the Federal Housing Administration (FHA) and Department of Veteran Affairs (VA) loans. FHA and VA charge lenders a flat rate/premium proportional to the loan amount, regardless of the underlying credit characteristics, in exchange for providing insurance and guarantees against borrower default.

Figure 1(a) shows that the upfront fee generally increases for loans with higher credit risk (lower credit scores and higher LTVs). However, some empirical estimates suggest that the LLPAs do not fully adjust prices to underlying risk (Gerardi, 2017; Tsai, 2023).[10] There could be many reasons leading to the imperfect risk-based pricing, including operational inertia. Or it is possible that the LLPAs were set intentionally to achieve certain policy goals. We do not aim to speculate on such reasons, but instead study their distributional implications.

The LLPA had remained largely stable from the Great Financial Crisis until recently, when a series of adjustments were made to GSE pricing. This is the first set of major changes since their introduction of the LLPA in April 2008, which we take advantage of in our research. This LLPA change was announced on January 19, 2023. The GSEs implemented a set of changes to the upfront g-fees that took effect for loans purchased by the GSEs beginning May 1, 2023.[11] Importantly, first-time home buyers with low to moderate incomes (LMI) are exempt from the LLPA during this time period and are therefore unaffected by the May 2023 LLPA change.

Figure 1(b) shows the changes in upfront g-fees for different combinations of credit score and LTV ranges of home-purchase mortgages, measured in percentages of the loan amount. Red cells represent combinations where the LLPAs increased after May 1st, and green cells represent combinations where LLPAs decreased. Figure 1(b) also shows that the LLPA change can be very different for borrowers with similar credit risk but located in different discrete grids of the LLPA pricing matrix. For example, the g-fee for a borrower with the credit score of 679 and the LTV of 80 declined by 87.5 bps, while the g-fee for a borrower with the credit score of 680 and the LTV of 80 was unchanged. Although borrowers can potentially move along

[9] Fannie Mae refers to upfront fees as "loan level price adjustments," while Freddie Mac refers to them as "credit fees in price." Because the industry typically uses the term LLPAs, in this paper we adopt the Fannie Mae term.

[10] Recent FHFA g-fee reports note a gap between g-fees and costs in the relatively stable housing markets in 2018, 2019–2021.

[11] See the directive HERE and an overview of the changes HERE.

the LTV dimension in response to differences in g-fees across LTV bin, the discrete changes of LLPAs along the credit score dimensions dominate the revisions of the LLPA matrix, as borrowers with credit scores below certain cutoffs generally had more favorable changes in their g-fee. In fact, as long as their LTVs are greater than 60, g-fees were changed more favorably for all borrowers with credit scores just below 680 than those just above 680 conditional on their LTV choices.

Figure 1(c) presents the average change in g-fee by credit score bin, weighted by the LTV distribution of GSE loans originated before March 2023. Critically, the differences in changes in g-fees across credit score bins are larger around credit score cutoffs of 640, 680, 720, 760, and 780. Our research design will exploit these differential changes in the average LLPAs at these credit score cutoffs to identify the effects of the LLPA change on home purchase originations as well as other outcome variables. Section 6 discusses our identification strategy in more detail.

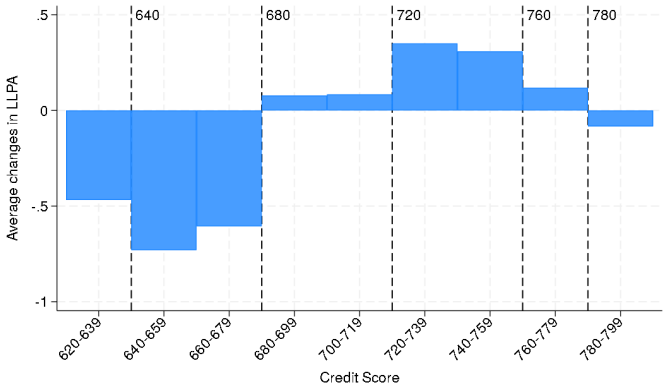
Figure 1: LLPA Matrix and Its Change in May 2023

| Representative Credit Score | LTV Range | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Applicable for all mortgages with terms greater than 15 years | | | | | | | | |
| | ≤ 60.00% | 60.01 – 70.00% | 70.01 – 75.00% | 75.01 – 80.00% | 80.01 – 85.00% | 85.01 – 90.00% | 90.01 – 95.00% | 95.01 – 97.00% | >97.00% |
| ≥ 740 | 0.000% | 0.250% | 0.250% | 0.500% | 0.250% | 0.250% | 0.250% | 0.750% | 0.750% |
| 720 – 739 | 0.000% | 0.250% | 0.500% | 0.750% | 0.500% | 0.500% | 0.500% | 1.000% | 1.000% |
| 700 – 719 | 0.000% | 0.500% | 1.000% | 1.250% | 1.000% | 1.000% | 1.000% | 1.500% | 1.500% |
| 680 – 699 | 0.000% | 0.500% | 1.250% | 1.750% | 1.500% | 1.250% | 1.250% | 1.500% | 1.500% |
| 660 – 679 | 0.000% | 1.000% | 2.250% | 2.750% | 2.750% | 2.250% | 2.250% | 2.250% | 2.250% |
| 640 – 659 | 0.500% | 1.250% | 2.750% | 3.000% | 3.250% | 2.750% | 2.750% | 2.750% | 2.750% |
| 620 – 639 | 0.500% | 1.500% | 3.000% | 3.000% | 3.250% | 3.250% | 3.250% | 3.500% | 3.500% |
| < 620[1] | 0.500% | 1.500% | 3.000% | 3.000% | 3.250% | 3.250% | 3.250% | 3.750% | 3.750% |

(a) LLPA Matrix for 2022

| Credit Score | LTV Range | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | Applicable for all loans with terms greater than 15 years | | | | | | | | |
| | ≤ 30.00% | 30.01 – 60.00% | 60.01 – 70.00% | 70.01 – 75.00% | 75.01 – 80.00% | 80.01 – 85.00% | 85.01 – 90.00% | 90.01 – 95.00% | >95.00% |
| ≥ = 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[1] | -0.500% | -0.375% | 0.000% | -0.875% | -0.250% | -0.375% | -0.625% | -1.000% | -2.000% |

(b) LLPA Changes in May 2023




(c) Average LLPA Changes by Credit Score

Note: Figure 1(a) shows the level of the LLPA for loans sold to the GSEs in 2022. Figure 1(b) shows the changes in the LLPA for home purchase loans effective in May 2023. The values are in percentages of the loan amount. Green cells represent decreases in g-fees, while red cells represent increases. Figure 1(c) shows the average LLPA by credit score bin, weighted by the LTV distribution of GSE loans originated before March 2023.

# 4 Data

Our main analysis relies on the confidential HMDA data from 2022 to 2023. The confidential HMDA data include loan origination dates, credit scores, property values, and loan amounts. This information allows us to determine the amount of the upfront g-fee and the change in the g-fee for each loan based on its characteristics, using the LLPA matrix shown in Figure 1(b).

We consider conventional loans (some of which are sold to the GSEs), FHA loans, and VA loans in our sample, and exclude USDA or Farm Service Guaranteed loans. We only consider the home-purchase loans and exclude refinance loans because the goal of this paper is to estimate the home purchase elasticity of the mortgage pricing and its welfare implications. As a side note, although the May 2023 LLPA changes also apply to refinance loans sold to the GSEs, our identification strategy based on credit score cutoffs may not work very well for refinance as refinance borrowers are more likely to be flexible in terms of the timing of loan application to be located in the LLPA cell with a more favorable g-fee change. Moreover, there were very low levels of refinancing activity due to the interest rate environment around the time when the LLPA change was implemented. As discussed earlier, we also exclude borrowers with low to moderate incomes (LMI) because first-time home buyers with LMI are exempt from upfront g-fees during this time period and are therefore unaffected by the May 2023 LLPA change. Because we cannot identify first-time home buyers using HMDA, we exclude all LMI borrowers.

We determine whether a loan was purchased by the GSEs using a HMDA field called "purchaser type," which provides information on the type of entity to whom the HMDA reporter sold the loan. We use Code 1 (Fannie Mae) and 3 (Freddie Mac) to capture the loans that were sold to the GSEs. We refer to conventional loans that are not sold to the GSEs as "non-agency loans" because they are typically held on a lender's portfolio or securitized into non-agency MBS. In our analysis, we group FHA loans and VA loans together into the same loan type because these two loan types have similar characteristics with relatively low credit scores and high LTVs and are securitized into Ginnie Mae MBS with explicit government guarantees.

The HMDA "purchaser type" field suffers from two known deficiencies. First, many small HMDA reporters may not directly sell the loans to the GSEs but would sell the loans to aggregators who subsequently sell the loans to the GSEs. In this case, these small HMDA reporters would not report the GSEs as the purchaser but another financial institution as the purchaser. We address this second deficiency by using a Universal Loan ID, which allows us to track a loan from its originator to its purchaser as long as both of them are HMDA reporters and eventually determine whether the purchaser sold the loan to the GSEs. An additional 17 percent of GSE loans are identified in this way.

Second, the "purchaser type" field has a censoring issue for some loans because loans that had not been sold by the end of the year but were eventually sold in the next year would be reported as "not sold." This data censoring issue mainly affects loans originated towards the end of the year, as the median interval from the date of origination to a loan being sold was about 16 days based on our calculation, with mean being 32 days and 95th percentile at 56 days. Thus, we drop loans originated in December of each calendar year from our sample.

## 4.1 Summary Statistics

Table 1 presents the summary statistics for our sample period separated by three loan types that we consider: GSE loans, FHA or VA loans, and non-agency loans. On average FHA and VA loans have much higher LTVs and lower credit scores than GSE and non-agency loans. The average property values are much higher for GSE and non-agency loans than for FHA or VA loans, while origination loan sizes are not much different across loan type, likely due to the offsetting effects of of LTVs. DTIs are lower for GSEs and non-agency loans than for FHA or VA loans, suggesting that households with lower incomes are less likely to choose GSE or non-agency loans. The average mortgage rate is about 5.6 percent in our sample period. Lastly, borrowers in the sample pay about $3,000 for points and origination fees net of lender credits, which is about 90 bps of the average loan origination balance.

We further illustrate how loan and borrower characteristics may vary by credit scores in Figure 2. Figure 2(a) shows the number of home-purchase loan originations in 2022 for each of the three loan types. Figures 2(b) and 2(c) plot the average LTV and the average household

income relative to the median family income of the relevant Metropolitan Statistical Area or Metropolitan Division by loan type and credit score, respectively. Together these three figures illustrate a large degree of heterogeneity in loan characteristics among households with different credit scores. This large heterogeneity implies that the effects of changes in g-fee pricing will be heterogeneous among households with different credit scores and incomes.

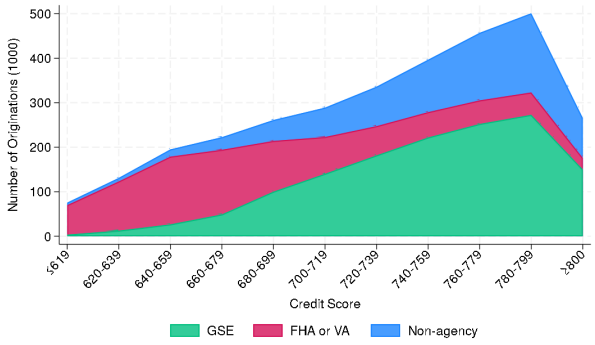
First, a vast majority of households with very low credit scores below 680 choose FHA or VA loans, which generally have very high LTVs (as shown in figure 2(b)). This data pattern is consistent with the facts that FHA and VA programs allow for very low down payment and the mortgage insurance premiums charged for FHA or VA loans are flat rates, regardless of the credit scores or LTVs. In contrast, GSEs loans require private mortgage insurance for loans with LTV greater than 80 percent. Moreover, the LLPA is higher for borrowers with lower credit scores and higher LTVs despite the GSEs' cross-subsidy for such borrowers. Accordingly, the average LTVs of the small number of GSE loans with very low credit scores are much lower than those for FHA loans, as shown in figure 2(b). Non-agency loans, which include loans kept on a lender's portfolio and loans securitized into private-label MBS, do not carry credit guarantee, causing the interest rate much higher for households with low credit scores. The premium of private mortgage insurance, which is required for conventional loans (GSE or non-agency loans), is also likely to be higher for loans with lower credit scores and higher LTVs.

Figure 2(a) shows that GSE originations account for significant shares of total loan originations for households with credit scores at least 680, suggesting that lower LLPAs for higher credit scores make GSE loans a cheaper option for such households than FHA or VA loans. Non-agency loans also account for higher shares for higher-score households likely because of their lower credit risks. Figure 2(b) shows that the average LTV of GSE loans for households with credit scores at least 680 is slightly higher than that for households with credit scores below 680, but the average LTV of GSE loans always remains much lower than the average LTV for FHA or VA loans because the LLPA is still higher for higher LTVs even for higher credit scores.

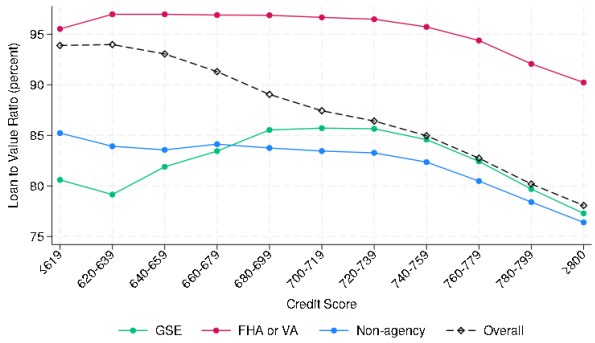
Figure 2(c) shows that, for most credit score bins, households with GSE or non-agency loans

generally have higher incomes relative to the median family income of the area where their homes are located. This implies that the GSEs' cross-subsidy for loans with lower credit scores may not benefit borrowers with lower relative incomes as much because they generally prefer FHA or VA loans. On the other hand, the cross-subsidy may have to be provided by households with higher credit scores but relatively lower relative incomes because the highest-income households are more likely to take non-agency loans.

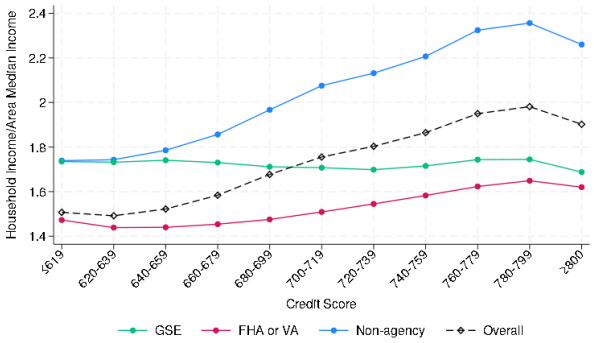
Figure 2: Loan Characteristics by Credit Score, Home Purchase Loans




(a) Number of Originations




(b) Average LTV




(c) Average Ratio of Household Income to Area Median Family Income

Note: Panel (a) shows the number of home-purchase loan originations for each loan type by credit score. Panel (b) shows the average LTVs of each loan type by credit score. The sample is loans originated in 2022, excluding December.

Because one of the key findings of this paper is that the LLPA changes along the credit dimension could be regressive in the income dimension, it is worthwhile to illustrate how the credit scores and income may differ. Figure 2(c) shows that there are clear differences in average income-to-AMI ratios across loan types and credit scores. However, even within the

Table 1: Summary statistics by loan type

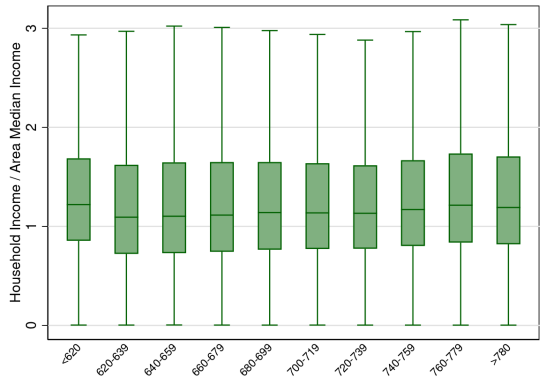
| | GSE | Non-Agency | FHA/VA | All |
|---|---|---|---|---|
| Credit Score | 752 | 749 | 689 | 731 |
| | (42) | (45) | (55) | (56) |
| LTV | 81.7 | 81.98 | 97.0 | 86.8 |
| | (15.6) | (16.0) | (8.2) | (15.4) |
| DTI | 37.7 | 36.6 | 44.4 | 39.7 |
| | (8.9) | (9.1) | (8.9) | (9.5) |
| Loan Amount ($000s) | 344.1 | 338.7 | 316.0 | 334.0 |
| | (165.7) | (187.5) | (137.7) | (161.7) |
| Property Value ($000s) | 436.7 | 435.4 | 330.4 | 401.5 |
| | (228.9) | (274.0) | (148.7) | (220.8) |
| Interest Rate (%) | 5.647 | 5.793 | 5.560 | 5.643 |
| | (1.227) | (1.414) | (1.205) | (1.256) |
| Net Origination Charges ($) | 3,084 | 2,849 | 2,827 | 2,960 |
| | (3,316) | (3,443) | (3,376) | (3,360) |
| Share of Loans (%) | 50.3 | 16.9 | 32.9 | 100 |

Note: The sample consists of loans originated in 2022 and 2023, excluding Decembers, limited to closed-end, non-reverse mortgages, 30-year fixed rate, non-IO, home-purchase mortgages secured by site-built, one-unit, first-lien, principal residence. Standard deviations are in parentheses.

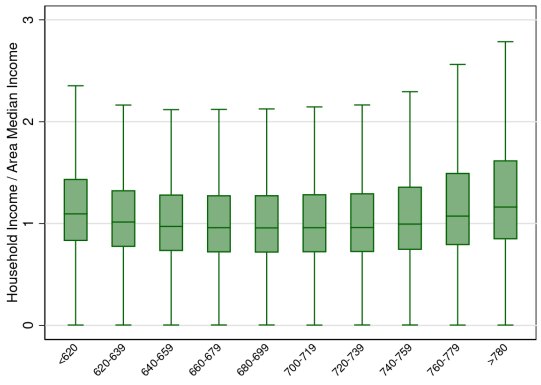
same loan type and credit score range, there is a wide dispersion of incomes. Figure 3 presents box plots of the income ratio over credit scores for different loan types. Notably for GSE loans, the median income-to-AMI ratios vary only slightly over the entire score range, and the inter-quartile ranges are substantial and largely similar over different score bins. The similar observations apply to non-agency loans and FHA/VA loans as well. In other words, high credit scores do not necessarily mean higher income. There are lower-income borrowers that have high credit scores, just as there are higher-income borrowers have low credit scores.

What does the divergence of income level and credit score imply in terms of the underlying population? To better understand that, we turn to another data source – the National Mortgage Database, a 1-in-20 sample of first-lien mortgages in the country, which includes information on the non-mortgage debts of the sampled mortgage borrowers from Experian. Figure 4 presents a set of binned scatter plots for mortgage borrowers who took home-purchase loans in 2022 and 2023. The horizontal axis is credit scores. As in our main specification, we exclude LMI borrowers from the sample. Within each measure, we overlay graphs for 4 different income

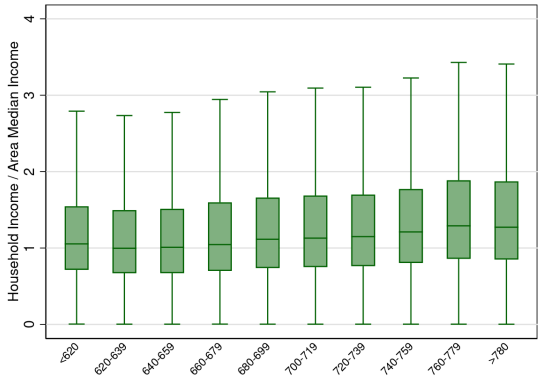
Figure 3: Box Plots of Household Income to Area Median Income Ratio by Credit Score and Loan Type




(a) GSE




(b) FHA/VA




(c) Non-agency

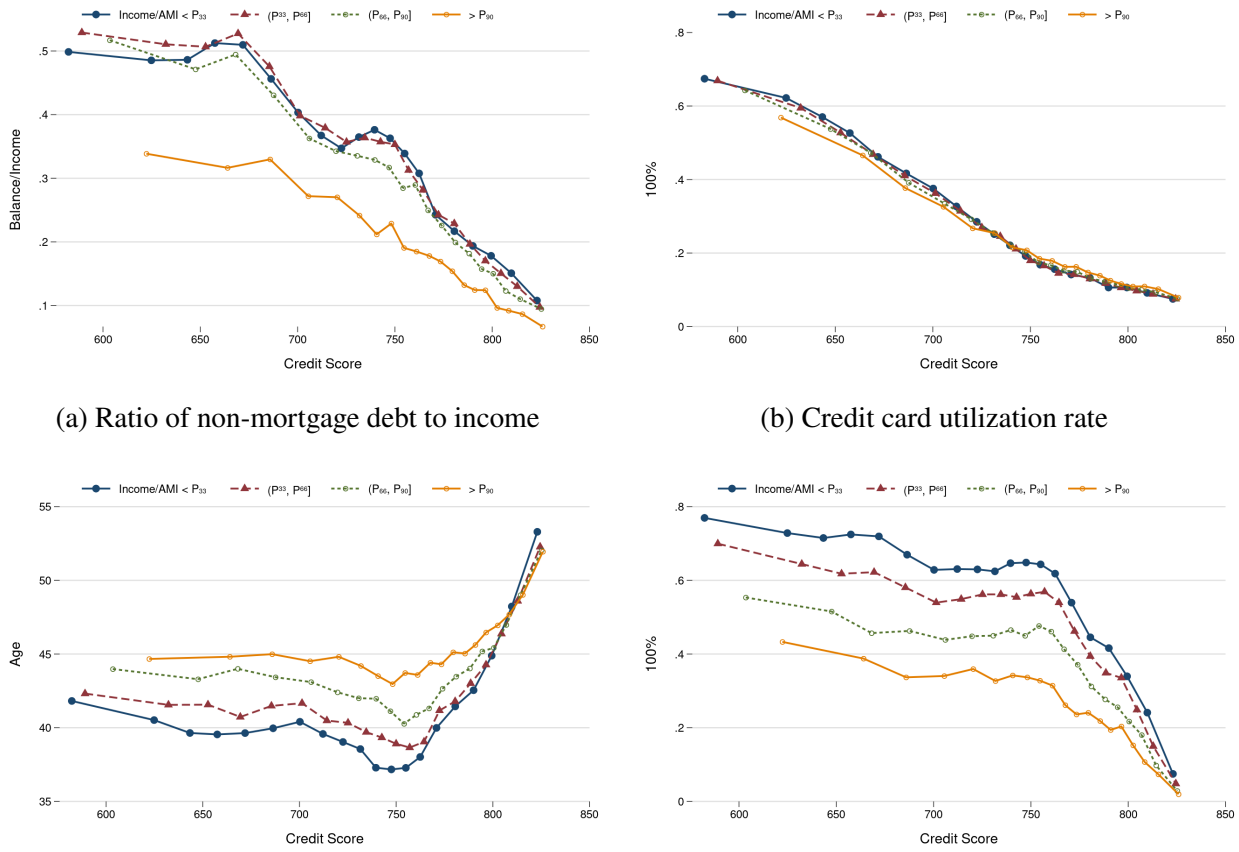
Note: The green bars in the figures span the first quartile to the third quartile of the household income to area median income ratio within each score bin. The horizontal line in the bar represents the median. The whiskers extend to max and minium values of the income ratios after outliers are removed. The data source is HMDA data, limited to first-lien home-purchase loans originated in 2022 and 2023.

groups using the same grouping as in our main specifications based on borrowers' income-to-AMI ratios: borrowers whose income level is in the bottom tercile, in the second tercile, in the top tericle and below 90th percentile, and those above the 90th percentile.

Figures 4(a) and 4(b) use two metrics to measure borrowers' financial constraints. Figure 4(a) plots the ratio of the total balance of all non-mortgage closed-end loans prior to mortgage origination to the mortgage applicant's income. Mortgage borrowers in the top income decile have the lowest non-mortgage debt burden relative to income. The divergence among the other groups is more nuanced, perhaps reflecting the interaction of credit demand and ability to borrow. Figure A.10 in Appendix provides a further breakout of borrowers' non-mortgage debt to sub-components (auto loan, student loan, and others). Credit card utilization rate in Figure 4(b) is an important factor in credit scoring models, and hence the divergence among different income groups is not as obvious as Figure 4(a). Figure 4(c) shows that high-income, low-CS borrowers (with credit score $< 700$) in the two income groups who make up for the top tercile are generally older than lower-income groups with similar credit scores, with an average age between 43 to 45. Finally, Figure 4(d) shows that the shares of first-time home buyers remain high among borrowers with lower CS. Higher-income groups tend to have lower shares of first-time home buyers, consistently across the CS distribution. These results suggest that a simple cross-subsidy towards lower-CS borrowers does not necessarily help first-time home buyers if higher-income, lower-CS borrowers respond more to the cross-subsidy.

Overall, Figure 4 provides evidence that borrower characteristics differ across both credit score and income dimensions. For instance, lower-income groups have higher levels of non-mortgage debt to income and are more likely to be first-time home buyers, even after controlling for credit scores. Consequently, changes to the LLPAs along the credit score dimension may impact borrowers of different income levels differently because of varying financial constraints, which we will explore in the next sections.

Figure 4: Financial Constraints, Age and First-time Home Buyer Status of Mortgage Borrowers by Credit Scores and Income Level




(a) Ratio of non-mortgage debt to income

(b) Credit card utilization rate

(c) Age

(d) Share of first-time home buyers

Note: The data source is the National Mortgage Database (NMDB), limited to first-lien home-purchase loans originated in 2022 and 2023. The equal-sized bins grouped by credit scores on the horizontal axis are calculated for each income group separately.

# 5 The effect of G-fees on borrowing costs

In this section we examine the effect of G-fees on borrowing costs using our stacked differences-in-differences specification, which is useful for putting our home purchase elasticities in context. Because mortgage pricing is multi-dimensional, we consider not only mortgage interest rates but also the upfront fees and points paid by the borrowers directly to the lenders. For the upfront

price, we calculate the net origination charges, defined as fees and points paid by borrowers net of lender credits. Examples of origination charges include origination fees, underwriting fees, application fees, as well as discount points borrowers pay to buy down the interest rate. We measure the net origination charges in percentages of the loan amount at origination.

Table 2: Effects on mortgage rates and net origination charges

| | Mortgage Rate | | | | Net Orig Charges | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | GSE | Non-agency conforming | Jumbo | FHA/VA | GSE | Non-agency conforming | Jumbo | FHA/VA |
| $1[t > Feb23]$ | -0.213*** | -0.050** | -0.010 | -0.024** | -0.290*** | -0.126*** | -0.084 | 0.002 |
| $\times E[-\Delta LLPA]$ | (0.013) | (0.024) | (0.049) | (0.012) | (0.021) | (0.024) | (0.051) | (0.021) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y | Y | Y |
| LLPA grid FE | Y | Y | Y | Y | Y | Y | Y | Y |
| Lender × County FE | Y | Y | Y | Y | Y | Y | Y | Y |
| N. Obs. | 376,955 | 172,715 | 40,818 | 186,739 | 371,913 | 168,892 | 40,420 | 182,131 |
| Adj. $R^2$ | 0.67 | 0.74 | 0.79 | 0.65 | 0.27 | 0.49 | 0.34 | 0.31 |

Note: The estimates of equation (1) for mortgage rates and net origination charges by loan type. Standard errors are clustered by credit score. Dec 2022 was excluded from the sample because of the end-of-year censoring issue in identifying whether a loan is sold to the GSEs in HMDA.

Table 2 presents the estimates of $\beta$ in equation (1) for mortgage rates and net origination charges by loan type. In addition to the fixed effects in equation (1), we also include extra controls for these regressions: the fixed effects for each LLPA pricing grid (a combination of CS bin, LTV bin, and whether a loan is high-balance loans larger than the national conforming loan limit) and the lender × county fixed effects.

The estimates show that lenders passed through the LLPA change both in the form of mortgage rates and net origination charges. Columns 1 and 4 show that a 100 bps LLPA decline led to declines in mortgage rates and net origination charges for GSE loans by 21 bps and 29 bps, respectively. We use these numbers for our model.

In Appendix 5, we provide additional details on the effects on borrowing costs. Figures A.1 and A.2 show that borrowing costs sharply diverged for credit scores just below and above each of the five CS cutoffs in the sample, suggesting that time-varying unobserved heterogeneity across CS points does not drive our estimates. Additionally, the event-study DID estimates in Figure A.3 show no pre-trends.

Table 2 also reports the spillover effects on other loans. We find some spillover effects for non-agency conforming loans, which are conventional loans that are eligible for GSE securitization but not sold to GSEs. This result suggests that the option to sell these loans to the GSEs affects the pricing of these loans to some degrees, even if they are not eventually sold to the GSEs. Table A.4 in Appendix provides evidence that the lower pass-through rate for non-agency conforming loans is because the price of bank-originated non-agency conforming loans does not respond to the LLPA change. We discuss how the effects on borrowing costs differ between banks and nonbanks in more detail in Section 6.3.2. Lastly, we find no spillover effects of the LLPA change on jumbo loans or FHA/ VA loans.

# 6 Effects of G-fees on Relative Home Purchases

We examine the effects of g-fees using the LLPA change in May 2023 in this section. As discussed in Section 3, the amount of upfront g-fee change in May 2023 is determined by the discrete grid of credit scores and LTVs. We exploit the discrete nature of this change for identification of the effects of g-fees. In particular, we use a difference-in-differences (DID) approach, which leverages differences in the amount of the LLPA changes based on whether a borrower's credit score is near a credit score cutoff (for example, 670-679 vs 680-689). We focus on differences in how much g-fees changed across borrowers with different credit scores rather than different LTVs because LTVs are more likely to be endogenous.

For estimation, we focus on borrowers with credit scores around the five different cutoffs: 640, 680, 720, 760, and 780, because the average differences in the g-fee changes were particularly large for borrowers in credit score grids below and above the four cutoffs. For credit scores around 680, g-fees declined by 68 bps for credit scores from 660 to 679 relative to credit scores from 680 to 699, using the LTV distribution prior to the g-fee change. For credit scores around 720, g-fees increased less by 26 bps for credit scores from 700 to 719 relative to credit scores from 721 to 739. For credit scores around 760, g-fees increased by 18 bps for credit scores from 740 to 759 relative to credit scores from 760 to 779. For credit scores around 780, g-fees increased by 19 bps for credit scores from 760 to 779 relative to credit scores from

780 to 799.

## 6.1 Regression specifications

### 6.1.1 Loan-level outcomes

We estimate the effects of g-fee changes for borrowers with credit scores around the four cutoffs in a stacked DID regression. For loan-level outcomes like interest rates, we estimate a linear regression of the following form, encompassing the five credit score cutoffs $f$:

$$y_i = \beta \times \mathbf{1}[t > Feb2023] \times E[-\Delta LLPA(cs, LTV)|\text{cs bin}] + \xi_{f,inc,t} + \xi_{cs} + \epsilon_i, \quad (1)$$

where the estimation sample is loans with credit scores ranging from $f-10$ to $f+9$ for the five cutoffs: 630–649, 670–689, 710–719, 750–769, and 770–789. Note that the size of the credit score range for each cutoff is 20 points, which is the same as the size of the LLPA credit score grid. Our regression just considers a different way to slice the credit score distribution from the one considered in the LLPA pricing. An important identification assumption is that there is no time-varying unobserved heterogeneity across CS points beyond our controls. This assumption would predict sharp discontinuities in trends of the outcome variable at the CS cutoffs after the LLPA change, which we verify in our analysis.

We assume that the treatment of the new LLPA became effective for loans originated in March 2023 or later because it typically takes up to two months before securitization into MBS issued by the GSEs. The treatment variable is $E[-\Delta LLPA(cs, LTV)|\text{cs bin}]$, which is the average LLPA change for each LLPA credit score bin over the LTV distribution in the pre-treatment period. The unit of the treatment variable is a percentage point, so the main coefficient of our interest $\beta$ measures the effects of a 100 bps LLPA decline on the outcome variable.

The regression includes the fixed effect $\xi_{f,inc,t}$ for cutoff ($f$), income bin ($inc$), and origination year-month ($t$), where loans with credit scores from $f-10$ to $f+9$ belong to the same $f$. This fixed effect sweeps out any time-varying heterogeneity across cutoffs, income bins, and origination times. With this fixed effect, $\beta$ is identified off of the difference in the

average LLPA changes between similar CS but located on different sides of the CS cutoff: CS from $f-10$ to $f-1$ and CS from $f$ to $f+9$. We consider 4 income bins, based on the distribution of a mortgage borrower's income relative to the area median income (AMI): (i) < 33th percentile, (ii) [33th percentile, 66th percentile), (iii) [66th percentile, 90th percentile), (iv) ≥ 90th percentile. The fourth income bin captures mainly borrowers with very large incomes relative to their corresponding AMIs (ranging from 3×AMI to more than 10×AMI), who may behave differently from other income groups.

Lastly, we also include the fixed effect $\xi_{cs}$ to control for time-invariant heterogeneity across credit scores $cs$. Moreover, when examining certain outcomes, we also include additional controls such as LTVs or the fixed effects for lenders. We cluster the standard errors by credit score.

### 6.1.2 Poisson regressions for number of loan originations

Because HMDA does not provide information about loans that would-be homebuyers who did not purchase homes, we use the number of home-purchase loan originations to examine the effects of the LLPA on home purchases. We estimate the Poisson regression of the following form:

$$E[y_{cs,inc,t}] = \exp(\beta \times \mathbf{1}[t > Feb2023] \times E[-\Delta LLPA(cs, LTV)|\text{cs bin}] + \xi_{f,inc,t} + \xi_{cs}), \tag{2}$$

where $y_{cs,inc,t}$ is the number of loan originations for credit score $cs$, income bin $inc$, and year-month $t$. Other variables are defined in the same way as in Equation (1), and the sample selection remains the same. The main coefficient of interest $\beta$ captures the effects of a 100 bps LLPA decline on relative home purchase volumes in terms of percentage changes between borrowers on the opposite sides of the CS cutoffs, i.e., the semi-elasticity of relative home purchase volumes. Note that we interpret $\beta$ as the effects on "relative" home purchase volumes because borrowers who benefited from the LLPA change can crowd out other borrowers on the other side of the CS cutoff if the house supply is limited and does not respond very much to the LLPA change in a short run. We will discuss this interpretation in more detail when we present

our estimate in Section 6.2.

**Concerns about endogenous credit score** A potential threat to the identification is the possibility that the CS on a mortgage application is endogenous to the LLPA change. Sharp differences in the LLPA around some CS cutoffs could, in theory, lead borrowers to take actions to shift their CS from one side of a CS cutoff to the other side. In this case, we would overestimate the effect of the LLPA change on home purchases.

For example, a mortgage applicant's decision to add a co-borrower to increase the qualifying income could depend on the LLPA pricing for different credit scores, because the minimum CS among multiple borrowers on an application is used for underwriting. Thus, adding a co-borrower can only decrease the CS used for underwriting with the benefit of a larger qualifying income, and the LLPA change can alter the trade-off.

Another possibility is the CS manipulation. However, our identification does not require that there is no CS manipulation at all, but only that borrowers do not respond to the LLPA change in terms of their CS manipulation. For example, if home buyers generally clean up their non-mortgage borrowing before entering a house search to improve their CS, regardless of the LLPA change, our identification assumption would not be violated. It would be violated if only borrowers are less (more) likely to manipulate their CS in response to a decrease (increase) in the LLPA. If such a sophisticated manipulation is prevalent, we may overstate the effects of the LLPA on home-purchase originations.

However, it is likely to be very difficult to change one's credit scores in a short time in response to the LLPA change. The LLPA change was not uniform: around CS cutoffs such as 680 and 720, the LLPA declined more for CS below the cutoffs than for CS above the cutoffs. Around other CS cutoffs such as 760 and 780, the LLPA increased more for CS below the cutoffs than for CS above the cutoffs. Moreover, the CS used for underwriting is the median of three of credit scores from the three credit bureaus. Thus, it will be very challenging for borrowers to know on which side of a CS cutoff their CS is located and to manipulate their underwriting CS to shift across a CS cutoff.

In Appendix E, we provide multiple tests for the possibility of endogenous CS. We find

that the likelihood of having a co-borrower on a loan does not respond to the LLPA change. Moreover, we do not find evidence for the sophisticated CS manipulation.

## 6.2 Relative Home Purchase Volumes

We now examine the effects of the LLPA change on relative home purchase volumes between borrowers on the opposite sides of the CS cutoffs, using the Poisson regression in equation (2). Before estimating the regression, we examine data patterns around the give credit scores cutoffs in Figure A.4 in Appendix. We find that the total number of originations, encompassing not only GSE loans but also other loan types, changed sharply around the cutoff after the LLPA change in a way consistent with the effects on borrowing costs.

Table 3 presents estimates of $\beta$. As discussed earlier, these estimates are semi-elasticities in response to a 100 bps LLPA decline. The semi-elasticity for the total number of originations is almost 14% (column 1). Examining each loan type, the semi-elasticity for GSE loans is even higher at almost 30% (column 2), and the semi-elasticities for other loan types (columns 3–5) are much smaller, which suggest that GSE loan originations drove the effects on the total number of originations. Consistent with the spillover effects on borrowing costs of non-agency conforming loans in Table 2, the semi-elasticity for these loans (column 3) is positive. In contrast, the semi-elasticity for FHA loans (column 5) is negative, suggesting that there is a substitution between FHA/VA loans and GSE loans. This substitution effect is consistent with Bhutta and Ringo (2021), who also find a similar partial substitution after the 2015 cut in the FHA mortgage insurance premium.

We also estimate the event-study version of equation (2) and present the estimates in Figure A.5 in the Appendix. This figure shows no pre-trends, a smaller increase for March 2023, and larger increases for April and later, which is another piece of evidence that our main estimates reflect the causal effects of the change in the LLPA.

The relative home purchase origination semi-elasticities we estimate are large, with a 100 bps decrease in g-fees increasing total relative home purchases by 13.8%, as shown in Column (1) of Table 3. Assuming that the 100 bps decrease in g-fees is passed-through to borrowers 25 to 30 bps of fee-adjusted interest rate, as would be consistent with Table 2,

Table 3: Effects on relative home purchase origination

| | (1) Total | (2) GSE | (3) Non-agency conforming | (4) Jumbo | (5) FHA/VA |
|---|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.139*** | 0.296*** | 0.070** | 0.041 | -0.073*** |
| | (0.015) | (0.028) | (0.032) | (0.066) | (0.022) |
| Market share (pre-policy) | | 0.42 | 0.27 | 0.09 | 0.22 |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y |
| N. Obs. | 3,600 | 3,200 | 3,200 | 2,680 | 3,200 |
| Pseudo $R^2$ | 0.82 | 0.83 | 0.79 | 0.87 | 0.77 |

Note: this table presents Poisson regression estimates of equation (2) for the number of home-purchase originations by loan type. Standard errors are clustered by credit score.

this is approximately twice as large as the FHA loan demand elasticity of 15% for a 50 bps decrease in ongoing mortgage insurance premium (MIP) as estimated in Bhutta and Ringo (2021), which would imply an elasticity of relative home purchases of 7.5% for a 25 bps decrease in interest rate. Nevertheless, our semi-elasticities are well identified, as evidenced by our empirical analysis on the lack of a change in manipulation around our policy change. Moreover, our intensive-margin semi-elasticity estimate for the loan amount, which will be presented in Section 6.3, is much lower (close to zero, insignificant) than the extensive-margin semi-elasticity for home purchases, which is similar to Bhutta and Ringo (2021).

**What explains our large estimated semi-elasticities?** In Appendix B, we provide evidence that our semi-elasticity estimate is in part driven by much larger elasticities of 17% per 100 bps LLPA decline among borrowers with DTI close to the maximum DTI constraint, consistent with Bhutta and Ringo (2021) and Bosshardt et al. (2024a). However, the semi-elasticity estimate for borrowers with lower DTIs is still sizable at around 10% per 100 bps LLPA decline. This result suggests that even if the DTI constraint does not bind, some borrowers are still sensitive to the size of their interest payments. This sensitivity to interest payments is likely to be heightened during our sample period, characterized by high interest rates. In our data, 55% of borrowers with low DTIs purchased the average discount point of 110 bps of the loan size to reduce interest payments, although they can still qualify for loans without purchasing

points. This result suggests that even low-DTI borrowers are sensitive to the size of interest payments and may have a target amount of monthly payments when making a home-purchase decision.

Given this backdrop of many borrowers being sensitive to interest payments, housing market frictions resulting from a limited home supply may lead to heightened competition among home buyers bidding for homes, in which differences in financing costs can have large allocative effects. This competition channel is likely to be more salient in our setup because the LLPA adjustment in May 2023 led to differential changes across borrowers with similar credit scores but located on the opposite sides of certain CS cutoffs. These borrowers are likely to be competing for the same set of houses. Thus, the LLPA change may lead to large elasticities of home purchase volumes for borrowers who received a better mortgage pricing relative to similar borrowers who did not. This mechanism may not be as relevant in the FHA setting studied in the earlier literature because of seller discrimination against FHA borrowers, which may limit FHA borrowers to low competition homes (Gilbukh et al., 2024). On the other hand, it may be particularly relevant in our GSE setting where borrowers are participating in more competitive markets.

The competition mechanism we propose is as follows. For instance, if a borrower with a 679 credit score qualifies at 6.95%, and the borrower with a 680 credit score qualifies at 6.70%, on a 30-year fixed mortgage with a balance of $450,000, that 25 bps difference means the 680 credit score borrower can borrow $10,813 more while keeping the exact same monthly payment and DTI as the 679 borrower, which may make them more likely to win the home.[12] This effect may be particularly relevant to households with either a monthly payment target or want to offer up to a given fraction of their pre-qualified amount. Even if they do not adjust the offer price to anticipated mortgage terms, better anticipated mortgage terms may also make households more willing to offer more attractive non-price terms to the seller. In a multiple-offer situation in a hot market, where buyers might each have a 1 in 5 chance of winning, a 5 percentage point increase in the winning probability, going from 20% to 25%, would explain a 20% extensive

[12]This can be calculated by applying the mortgage amortization formula and taking the ratio of the monthly payment in the 6.95% case of $3289 and the monthly payment in the 6.7% case of $3153, with $3289/$3153*$450,000=$10,813.

margin response.

In Section 7, we build a structural model that features this competition channel. Our calibrated model generates semi-elasticities of relative home purchase volumes that match our reduced-form estimate.

### 6.2.1 Heterogeneous effects on the number of originations by credit score and income

As discussed earlier in Section 4.1, the share of GSE loans varies greatly across borrowers with different credit scores and incomes, suggesting that the semi-elasticity is likely to vary with credit scores and incomes. Table 4 presents Poisson regression estimates showing the heterogeneity, by interacting our main treatment variable with each income group bin.

We find that, among low-CS borrowers (CS $<$ 700), higher-income borrowers tend to have higher semi-elasticities in column 1. Lower-CS, lower-income borrowers are more likely to lack downpayments, and FHA or VA loans may offer better pricing because of their uniform insurance pricing. Thus, this difference in pricing between loan types may dampen the responsiveness of lower-CS, lower-income borrowers.

Among high-CS borrowers (CS $\geq$ 740, column 3), semi-elasticities do not vary very much with income, except for a very low semi-elasticity for borrowers with incomes above the 90th percentile. These borrowers with very high incomes account for about 13% among high-CS borrowers. As shown in Figure 2, loans for these high-CS borrowers with very high incomes (more than 3×AMI) are less likely to be sold to the GSEs because many of them demand jumbo loans or because the LLPA is higher than the expected cost implied by their credit risk (Buchak et al., 2024a).

These heterogeneous semi-elasticities suggest that a simple cross-subsidy from higher-CS borrowers to lower-CS borrowers may have unintended distributional consequences, benefiting higher-income, lower-CS borrowers disproportionately more at the expense of higher-CS borrowers, except for those with very high incomes. In light of our analysis based on the NMDB in Figure 4, borrowers who benefit more from the simple cross-subsidy – higher-income, low-CS borrowers – are less likely to be young, first-time home buyers than other low-CS borrowers.

Moreover, these distributional effects will be even more pronounced in an environment characterized by a very tight housing market with a limited supply of houses, where competition among home buyers is greater. We explore this aspect in more detail with a structural model in Section 7.

Table 4: Heterogeneous effects on the number of home-purchase loan originations by CS and income

| | (1) CS < 700 | (2) CS ∈ [700, 740) | (3) CS ≥ 740 |
|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.095*** | 0.177*** | 0.171*** |
| | (0.019) | (0.054) | (0.043) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times 1[\frac{\text{Income}}{AMI} \in (P_{33}, P_{66}]]$ | 0.054** | 0.095* | -0.029 |
| | (0.026) | (0.053) | (0.058) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times 1[\frac{\text{Income}}{AMI} \in (P_{66}, P_{90}]]$ | 0.068** | -0.045 | 0.064 |
| | (0.034) | (0.108) | (0.065) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times 1[\frac{\text{Income}}{AMI} > P_{90}]$ | 0.149** | -0.093 | -0.165*** |
| | (0.068) | (0.130) | (0.052) |
| Cutoff × Income bin × Orig YM | Y | Y | Y |
| CS FE | Y | Y | Y |
| N. Obs. | 1,440 | 720 | 1,440 |
| Pseudo $R^2$ | 0.92 | 0.89 | 0.89 |

Note: this table presents Poisson regression estimates of equation (2) for relative home purchase volumes. $P_n$ refers to the n-th percentile of $\frac{\text{Income}}{AMI}$. Standard errors are clustered by credit score.

## 6.3 Other outcome variables

### 6.3.1 Effects on loan size, home prices

In light of the elasticity of loan size with respect to mortgage rates in DeFusco and Paciorek (2017), the LLPA change may have also led to changes in the intensive margin, such as prices of homes purchased and the loan size. However, we do not find statistically significant effects on the average price of homes purchased and the loan amount, shown in Table A.3 in Appendix C. There are increases in the average price of homes purchased and the loan amount among GSE loans, while the effects are negative and insignificant for non-agency conforming loans. The small overall effects may reflect some substitution between GSE loans and non-agency loans. [This finding is consistent with Bhutta and Ringo (2021), who also find large home-purchase elasticities but small intensive margin elasticities. This finding is also consistent with a tight

housing market, where home buyers have difficulties in getting matched to houses instead of optimizing over a set of many available houses with different price points.]

### 6.3.2 Effects on nonbank share

Nonbanks account for a large share of originations sold to GSEs. We examine how the effects of the g-fee change differ by lender type in this section. We present the estimated effects on borrowing costs by banks and nonbanks, and the nonbank share of origination in Appendix D and summarize our results here.

On the effects on borrowing costs, Table A.4 in Appendix D shows that the pass-through of the g-fee change to the cost of GSE loans is similar between banks and nonbanks. Among non-agency conforming loans, however, the pricing of originated loans remained unchanged for banks, although nonbank-originated loans have a pass-through rate similar to GSE loans (close to the full pass-through). This difference in the pass-through rate between banks and nonbanks may reflect the difference in their funding source. Nonbank loans could be sold to aggregators, who may then sell some of them to the GSEs while keeping others in portfolio, while banks may expect to keep their non-agency conforming loans on their balance sheet.

The larger pass-through rate for non-agency conforming loans originated by nonbanks also lead to a 4.8 pp larger nonbank share of originations among that loan type (Table A.5 in Appendix D). The nonbank share among GSE loans remained unchanged because the pass-through rates are similar between banks and nonbanks. Overall, a 100 bps decline in the g-fees leads to a 2 pp increase in the nonbanks share among all home-purchase originations. This result adds to the evidence that GSE subsidies may be an important driver of the rise of non-banks (Buchak et al., 2024a).

Our results suggest that nonbanks are more sensitive to the cost of securitization because they cannot easily fund loans through their balance sheets (Buchak et al., 2024a). However, we still find that the effects on GSE loans are similar between banks and non-banks. The uniform semi-elasticity of relative home purchases using GSE loans among banks and non-banks is another piece of evidence that credit score manipulation may not drive our main results, because one would expect non-banks to be more likely to encourage the manipulation due to a

lower level of scrutiny from regulators (Buchak et al., 2018; Benson et al., 2024).

# 7 Housing search model

To better understand the housing search frictions required to rationalize our large extensive-margin effect, and to evaluate how alternative LLPA designs might influence market outcomes, we develop a structural model of frictional housing search, building on the framework in Jiang et al. (2024). Our model introduces two key innovations. First, we endogenize both buyer entry and seller listing behavior by incorporating logits for buyer and seller flows, allowing us to capture how GSE pricing changes alter participation on both sides of the market. Second, rather than modeling seller heterogeneity via patience, we focus on buyer heterogeneity in their reference mortgage interest rate, a natural channel in our setting, and allow sellers' willingness to transact to vary through i.i.d. shocks. Our model has an excellent fit in sample, and incorporates many realistic features of the housing market with reasonable parameters.

## 7.1 Setup

Our model has two stages. In the first stage, buyers and sellers decide to enter the market based on the expected future value of participating in the market. Buyers also choose their loan type, if they decide to enter, which in turn determines their reference interest rate. In the second stage, buyers and sellers play a continuous-time search and matching game that is standard in the literature. This two-stage setup allows for endogenous flows into housing search models.

In the first stage, buyers choose a mortgage from lender $j$, at LTV $l$, and FHA vs GSE vs non-agency conforming mortgage $f$ based on their credit score $c$, income bin $k$, and whether the lender is a bank or a non-bank $b(j)$. Their choice of mortgage maximizes:

$$u_{ickjlf} = \max_{jlf} \gamma^B V^B(r_{ckjlf}) + \xi_{cklfb(j)} + \epsilon_{ckjlft}, \tag{3}$$

and they use the outcome of the maximization in Equation (3), $r_i$, as their reference interest rate when deciding how much they value a house in the subsequent search and matching game.

Households may also choose not to proceed with searching for a home at all, in which case they take the outside option with a utility equal to 0. The inflow of buyers of type $r_i$ is then:

$$\mu^B(r_i) = \sum_{ckjlf} w_{ck} \mathbb{1}(r_i = r_{ckjlf}) \frac{\exp\left(\gamma^B V^B(r_{ckjlf}) + \xi_{cklfb(j)}\right)}{1 + \sum_{jlf} \exp\left(\gamma^B V^B(r_{ckjlf}) + \xi_{cklfb(j)}\right)} \tag{4}$$

where $w_{ck}$ is the share of buyers with credit score $c$ and income $k$ in the population. We assume that unmatched buyers exit the market at a rate of decay $\kappa$, which allows us to model the fact that not all buyers are successful in securing a home. In the baseline calibration, we set the outside share to 90%, and the matching success probability to 40%,[13] which in turn implies that 10% of US households are potential buyers and 10%*40% = 4% of US households are expected to transact at an annual rate. We view these as reasonable probabilities based on Gerardi et al. (2024).

The flow of sellers similarly depends on the elasticity to the expected seller value $V^S$ based on parameters $\gamma_0^S, \gamma_1^S$:

$$\mu^S = \frac{\exp(\gamma_0^S + \gamma_1^S V^S)}{1 + \exp(\gamma_0^S + \gamma_1^S V^S)}, \tag{5}$$

where $\gamma_0^S$ pins down the mean utility of entering the market as a seller and $\gamma_1^S$ pins down its sensitivity to the expected seller value $V^S$. In particular, $\gamma_1^S$ is related to the general equilibrium responses to GSE subsidies: if more buyers flow into the market in response to GSE subsidies, $\gamma_1^S$ pins down the level of seller response. If $\gamma_1^S = 0$, the number of transactions is fixed at the initial number of sellers. On the other hand, if $\gamma_1^S$ is large, the number of sellers becomes perfectly elastic, and the number of transactions is entirely pinned down by buyer flows.

In the second stage, buyers and sellers play a continuous-time matching game. Buyers value the house at $\rho^b$, but their value may be reduced by their reference interest rate by the amount of $\psi$ per 100 bps of interest rate. This reduction in value may reflect both the disutility of higher mortgage payments as well as any DTI supply constraints. Upon matching and paying price $P$,

[13] Based on an industry pre-approval to application conversion rate from LenderLogix of 58% HERE and an application to origination rate of 69%.

which we endogenize, the buyer's surplus is:

$$v^B(r_i) = \rho^b - \psi r_i - P, \tag{6}$$

For sellers, the surplus for a given price $P$ is:

$$v^S = P + \epsilon_{it}, \tag{7}$$

where sellers draw $\epsilon_{it} \sim N(0, \sigma)$ each time they match, representing an idiosyncratic urgency to sell. Sellers continue listing the home until their home is sold.

A buyer with a higher $r_i$ has less surplus and therefore their offer is less attractive to the seller, but if the seller has a high need to sell the home (i.e., if $\epsilon_s$ is large), they may sell anyways. Crucially for our purposes, $\sigma$ corresponds to price dispersion, whereas $\psi$ relates to the semi-elasticity of relative home purchases to g-fee changes in an allocative sense. The reason why the effect of $\psi$ is primarily allocative is that, holding fixed seller flows, the total number of transactions is pinned down by the number of sellers, and so a larger $\psi$ makes it more likely for a home buyer with a lower interest rate to win the home and crowd out other home buyers.

In a stationary equilibrium, a mass of buyers $M^B$ and sellers $M^S$ are available for matching. The matching function is:

$$m(M^B, M^S) = \alpha \left( \sum_i M^B(r_i) \right)^{\phi} \left( M^S \right)^{1-\phi} \tag{8}$$

where $\alpha$ represents the overall matching rate, and $\phi$ represents how imbalances in $M^B, M^S$ affects the match rate.

The equilibrium price is determined by Nash bargaining with the seller's bargaining strength being $\theta$, where the surplus is split. The price conditional on trade is:

$$P(\epsilon_s, r_i) = V^S + \theta(\rho^b - \psi r_i + \epsilon_s - V^B(r_i) - V^S) \tag{9}$$

Both buyers and sellers are forward-looking. Buyers discount the future at rate $\delta$ and exit at rate $\kappa$, capturing exit in response to inability to match with a home. Sellers are assumed to keep their homes on the market forever, but discount the future at a rate $\delta + \kappa^s$, with a higher discount rate capturing sellers' urgency to sell. The seller's urgency to sell $\kappa^s$ helps pins down housing search moments such as days on market.

## 7.2 Model solution

The model solution is given in Proposition 1.

**Proposition 1.** *Given primitives:*

$$\gamma^B, \xi_{cklfb(j)}, \gamma_0^S, \gamma_1^S, \rho^b, \psi, \sigma, \alpha, \phi, \delta, \kappa, \kappa^s \tag{10}$$

*A stationary equilibrium of this model is described by the stationary seller and buyer value functions $V^S, V^B(r_i)$, the stationary seller and buyer masses $M^S, M^B(r_i)$, matching rates $\lambda^S$, $\lambda^B$, and trade cutoff function $\bar{\epsilon}(r_i)$ with $G(r_i) = Pr(\epsilon_s > \bar{\epsilon}(r_i))$, which satisfies the following conditions:*

*Trade cut-offs (condition for which a transaction occurs following a match):*

$$\epsilon_s \geq V^S + V^B(r_i) - (\rho^b - \psi r_i) = \bar{\epsilon}(r_i) \tag{11}$$

*Matching rates:*

$$M^S \lambda^S = \sum_i M^B(r_i)\lambda^B = \alpha \left( \sum_i M_B(r_i) \right)^{\phi} M_S^{1-\phi} \tag{12}$$

*Flow equality:*

$$M^B(r_i)\lambda^B G(r_i) + \kappa M^B(r_i) = \mu^B(r_i) \tag{13}$$

$$M^S \lambda^S \frac{\sum_i M^B(r_i) G(r_i)}{\sum_i M^B(r_i)} = \mu^S \tag{14}$$

*where $\mu^B(r_i)$ satisfies Equation (4) and $\mu^S$ satisfies Equation (5).*

*Bellman equations:*

$$(\delta + \kappa)V^B(r_i) = \lambda^B(1-\theta)\int_{\epsilon_s > \bar{\epsilon}(r_i)} (\rho^b - \psi r_i + \epsilon_s - V^B(r_i) - V^S)dN(\epsilon_s) \tag{15}$$

$$(\delta + \kappa^s)V^S = \lambda^S\theta\sum_i \frac{M^B(r_i)}{\sum_i M^B(r_i)}\int_{\epsilon_s > \bar{\epsilon}(r_i)} (\rho^b - \psi r_i + \epsilon_s - V^B(r_i) - V^S)dN(\epsilon_s) \tag{16}$$

*Proof.* The trade-cutoffs come from the match surplus term being positive in Equation (9). The rest follows by model construction.

□

This model can be solved numerically, with two outer loops. The computational details of the model are shown in Appendix Section E.3. The expected price is:

$$\bar{P} = \sum_i \frac{M^B(r_i)}{\sum_i M^B(r_i)}\int P(\epsilon_s, r_i)M^B(r_i)\lambda^B G(r_i)\mathbb{1}(\epsilon_s > \bar{\epsilon}(r_i))dN(\epsilon_s) \tag{17}$$

## 7.3 Identification

We obtain $\delta = 0.05, \alpha = 1$ from Jiang et al. (2024). For the other internally calibrated parameters, while all parameters affect the model, intuitively each of them captures different variation in the data. We calibrate:

- $\xi_{cklf}$: to market shares of that lender/product type by income and credit score,
- $\rho^b$: to the average house price, equal to $432,950 for 2023,
- $\gamma^B$: to the elasticity of substitution between bank and non-bank lenders following g-fee changes, equal to 0.019 from Appendix Table A.5,
- $\psi$: to the total home purchase elasticity to g-fee changes, equal to 13.9% from Table 3. We assume that g-fees pass-through to banks and non-banks based on Table A.4, and that households conservatively use a factor of 0.2 when converting from origination fees as a fraction of the loan amount to the interest rate,
- $\sigma$: to the idiosyncratic log standard deviation of house prices, equal to 9.3% from Jiang et al. (2024),

- $\kappa^s$: to the sellers' days on market (DOM), equal to 39, from Realtor.com,
- $\kappa$: to the number of buyer offers before success, equal to 2.5, from NAR survey of home buyers,[14]
- $\phi$: to the number of weeks buyers search for their homes, equal to 12, from NAR survey of home buyers.[15]

The sellers' flow rate responses to the value of being a seller, $\gamma_1^S$, is not identified in our data. We use a fixed point to calibrate the seller flow rate $\mu^S$ to a buyer success probability of 40%.[16] Since we calibrated the buyer outside option to 90%, our assumptions imply that 10% of households are searching for a home to buy and 4% of households ends up being successful in each time period, which may be realistic based on Gerardi et al. (2024). The calibrated $\mu^S$ in turn implies a value of $\gamma_0^S$ for each assumed level of $\gamma_1^S$, which allows us to solve our model counterfactuals under an assumption about $\gamma_1^S$. For our baseline analyses, we assume that $\gamma_1^S = 0$, which implies that the number of transactions is fixed at the initial number of sellers. Future analyses will examine the sensitivity of our results to different assumptions about $\gamma_1^S$.

## 7.4 Calibration

Table 5 shows the calibrated model parameters.

Table 5: Calibrated model parameters

| Parameter | Value |
|---|---|
| $\kappa$ | 0.3392 |
| $\kappa^s$ | 1.3442 |
| $\psi$ | 13.4572 |
| $\rho^b$ | 64.7758 |
| $\sigma$ | 13.7576 |
| $\phi$ | 0.8272 |
| $\gamma^b$ | 0.1150 |

[14] Available HERE.
[15] Available HERE.
[16] Based on an industry pre-approval to application conversion rate from LenderLogix of 58% HERE multiplied by a application to origination rate of 69%.

Among our calibration parameters, $\kappa$ implies that the buyers drop out at a rate of 0.34 per year. $\kappa^s$ implies a seller discount rate of 1.34 + 0.05 = 1.39 per year, or approximately 7.7% monthly, which reflects the sellers' high average urgency to sell and helps fit DOM moments given the observed idiosyncratic price dispersion of house prices. $\psi$ implies that that a 1 percentage point rise in interest rate reduces the buyers' valuation of the house by 13.4572/64.7758=20.7%, which may be reasonable because an interest rate increase from 5% to 6% raises the monthly mortgage payment by 11.6%, plus the buyer may become DTI constrained. An alternative interpretation of $\psi$ is that, for a 100 bps change in interest rate ($\Delta r$ = 1.00), the seller becomes $\psi/\sigma$ = 13.4572/13.7576 = 0.98 standard deviations more willing to accept the buyer's offer, which could capture the different ways the buyer made their offer better. $\sigma$ reflects the idiosyncratic variation in sellers' urgency to sell and is used to fit price dispersion moments. $\phi$ is calibrated to housing search moments and is consistent with the 0.84 value typically estimated in the literature, and $\gamma^b$ is calibrated to non-bank substitution rates.

Table 6 shows the model's fit to targeted housing market search and lender type choice moments. The model fits the targeted moments well, in terms of the buyer's expected search time before success of 12 weeks, sellers' DOM, the buyers' expected number of offers before success, the expected price, the idiosyncratic price dispersion in terms of the standard deviation of log price, and our estimated relative home purchase semi-elasticities as well non-bank share changes to a 100 bps g-fee rise. These results imply that a large relative home purchase elasticity to 100 bps in g-fee changes is expected under reasonable housing search calibrations.

Table 6: Comparison of model output to targeted moments

| **Statistic** | **Model Output** | **Target** | **Difference** |
|---|---|---|---|
| Buyer weeks to success | 11.803 | 12.000 | -0.197 |
| Seller days on market (DOM) | 38.955 | 39.000 | -0.045 |
| Expected number of offers before success | 2.486 | 2.500 | -0.014 |
| Expected price ($10,000s) | 43.251 | 43.295 | -0.044 |
| Std. dev. of log price | 0.092 | 0.093 | -0.001 |
| Relative home purchase semi-elasticity (100 bps g-fee) | 0.140 | 0.139 | 0.001 |
| Non-bank share change (100 bps g-fee) | 0.0178 | 0.019 | -0.001 |

Table 7 examines the model's implications for untargetted moments. The first row of Table 7 shows that the intensive margin elasticity of home prices to a +100 bps change in interest rate is only -2.6%, consistent with the -2% estimate in DeFusco and Paciorek (2017). This implies that the main effect of higher interest rates is in making households less able to buy a home, rather than reducing their transaction price conditional on a successful purchase. The second row of Table 7 shows that the expected change in CS<680 share following a 50 bps FHA MIP cut is 23.3%, which is larger than but in the same magnitude as the 14% estimate of Bhutta and Ringo (2021). Tighter market conditions over our calibration period may explain some of this difference.

Our model-implied relative home purchase elasticities to a 100 bps change in g-fee represent a mix of two parameters: $\gamma^b$, which determines the inflow rate of buyers as they respond to their expected value of housing search conditional on the interest rate, and $\psi$ during the housing search. The third row of Table 7 examines the relative importance of these two channels by shutting down the former and keeping buyer flows in Equation (4) constant, while simulating the effect of the May 2023 LLPA change. The result is a 9.2% relative home purchase elasticity at our CS cut-offs, as opposed to 14.0% in the baseline model, suggesting about two thirds of the relative home purchase elasticity is due to an allocative effect rather than an entry effect.

Table 7: Model implications for untargeted moments

| **Statistic** | **Model Output** |
|---|---|
| Intensive margin elasticity (+100 bps interest rate) | -0.026 |
| Growth in CS<680 share following 50 bps FHA MIP cut | 0.210 |
| Relative home purchase semi-elasticity (100 bps g-fee, constant flows) | 0.092 |

# 8 Discussion

GSE redistribution by borrower credit scores and LTV is controversial and generally non-transparent. In this paper, we make progress on estimating their real effects on home purchases by exploiting a set of recent g-fee changes. We find large effects on relative home purchases by the affected populations, consistent with a model of frictional housing search. These effects

imply that GSE pricing has significant distributional outcomes in terms of the composition of successful home purchasers. Furthermore, our results imply that these distributional outcomes may be regressive. These results may inform the ongoing policy discussions of GSE reform.

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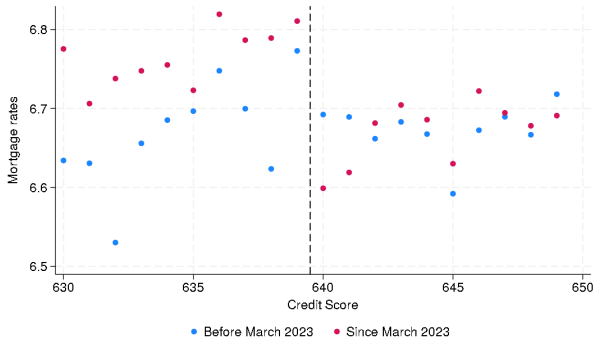
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# Internet Appendix

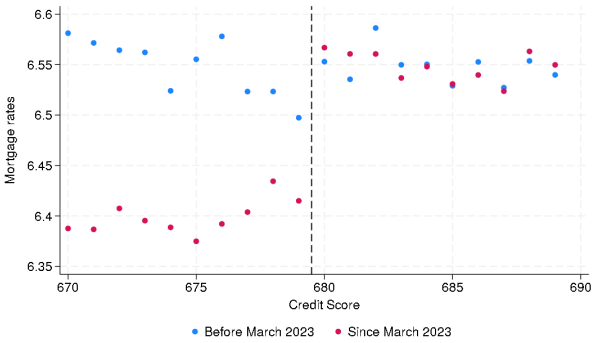
## .1 Data patterns around each credit score cutoff

Figures A.1 and A.2 presents data patterns driving the estimates graphically after residualizing mortgage rates for GSE loans by LTVs, the lender $\times$ county fixed effects, and the time fixed effects. In all panels, there is a visibly clear change at the cutoff before and after the LLPA change. Moreover, mortgage rates changed in a way that is consistent with relative changes in the LLPA around each credit score cutoff. For example, borrowers with credit scores below 680 saw a decline in mortgage rates relative to those on the other side of the 680 cutoff because the former group had a larger decline in the LLPAs. Borrowers with credit scores below the 760 cutoff saw an increase in mortgage rates relative to those on the other side of the 760 cutoff because the former group had a smaller increase in the LLPAs. We also report figures for net origination charges in Appendix [xx], which also show similar patterns.

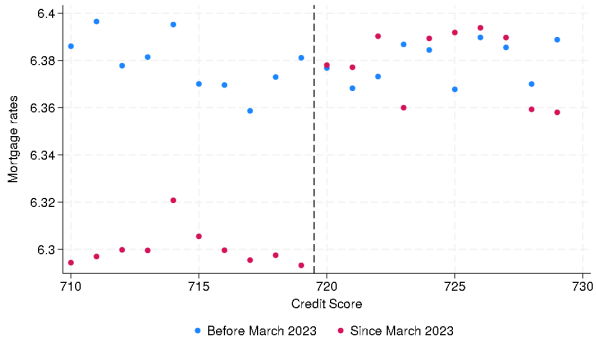
Figure A.1: Mortgage rates of GSE loans



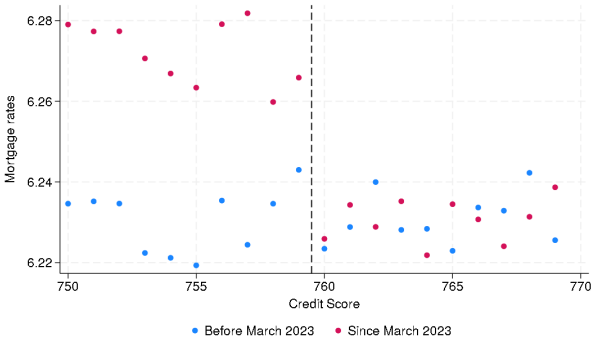

(a) credit score cutoff 640



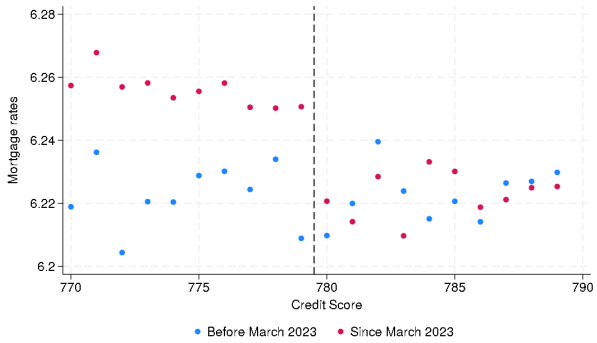

(b) credit score cutoff 680




(c) credit score cutoff 720



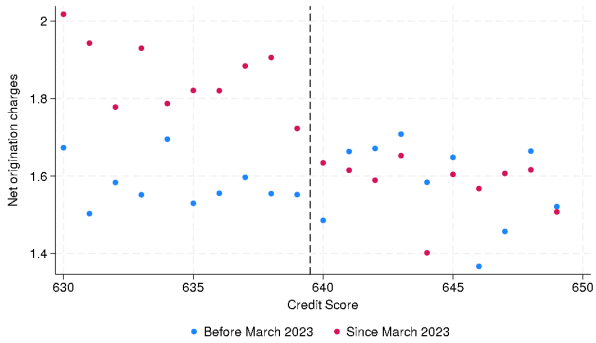

(d) credit score cutoff 760



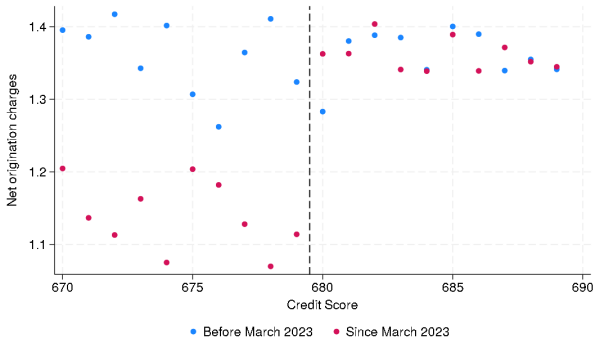

(e) credit score cutoff 780

Note: this figure presents mortgage rates of GSE loans for each credit score point, residualized by income bin-by-origination year-month fixed effects and lender-by-county fixed effects.

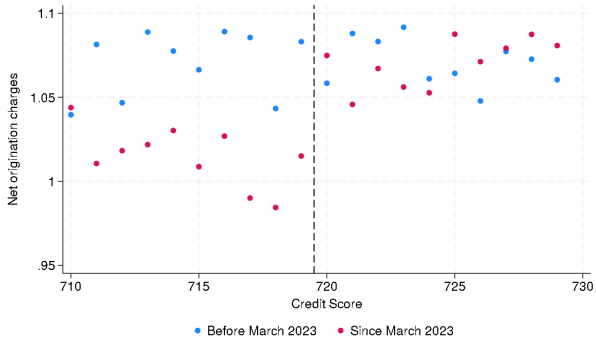
Figure A.2: Net origination charges of GSE loans (% of loan amounts)



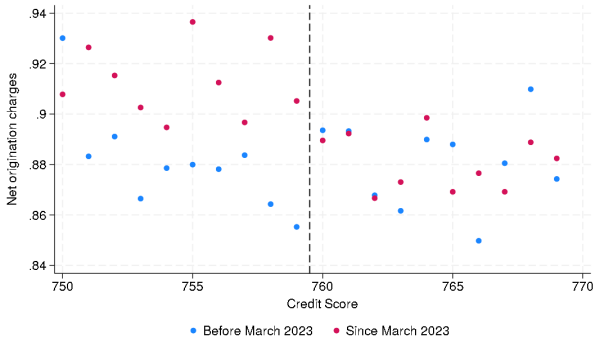

(a) credit score cutoff 640



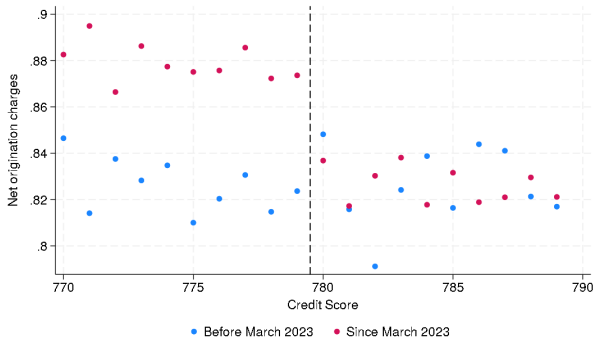

(b) credit score cutoff 680




(c) credit score cutoff 720




(d) credit score cutoff 760



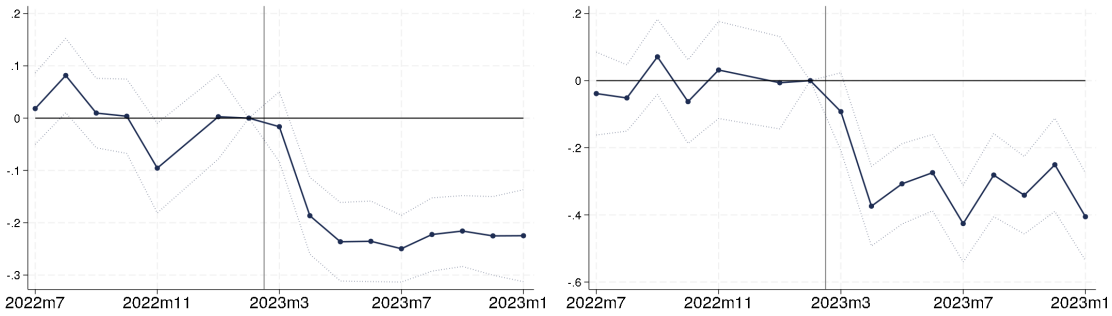

(e) credit score cutoff 780

Note: this figure presents net origination charges of GSE loans for each credit score point, residualized by income bin-by-origination year-month fixed effects and lender-by-county fixed effects.

## .2 Event-study DID results

Figure A.3 presents the estimates of an event-study DID of equation (1), where we interact the treatment variable $E[-\Delta LLPA(cs, LTV)|\text{cs bin}]$ with the month fixed effects. These results show that the estimate for GSE loans in columns (1) and (4) in Table 2 are indeed driven by changes that happened for loans originated in March 2023 or later. The effects in March 2023 are much smaller than later months because only a small fraction of loans originated in March are sold to the GSEs in May or later. Moreover, although the new LLPA was announced in January 19, 2023, we find no effects for loans originated in January or February, also suggesting that a lender's limited ability to warehouse new originations prior to securitization. This limited warehousing ability is likely to stem from the fact that nonbank lenders account for a vast majority of originations. Nonbanks tend to have higher funding costs for warehousing originations prior to securitization (Jiang, 2023), and their funding for originations often include covenants that make them securitize new originations within a certain period of time (Kim et al., 2018).

Figure A.3: Event-study DIDs for borrowing costs for GSE loans



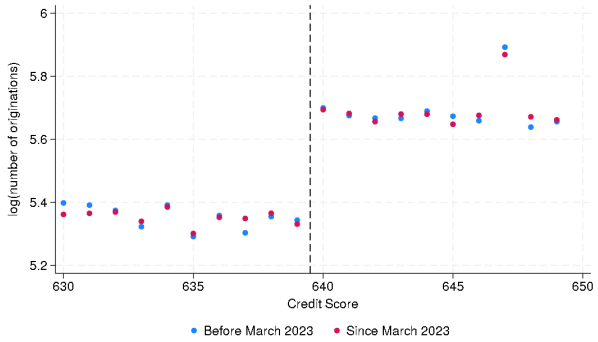

(a) Mortgage rates

(b) Net origination charges

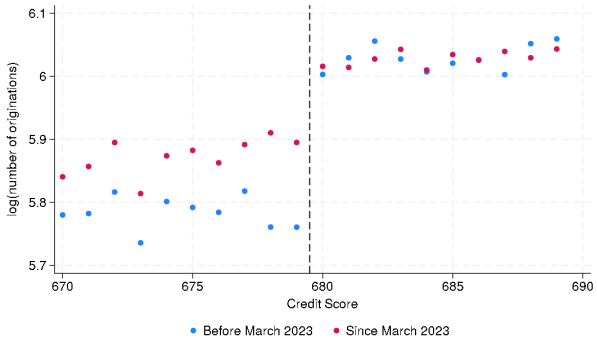
Note: coefficient estimates and their 95% confidence intervals in the event-study version of equation (1).

# A Additional material for the effects on home purchases

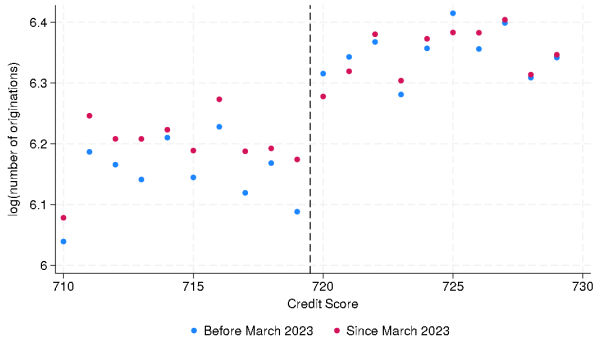
Figure A.4: Log of total number of home-purchase loan originations




(a) credit score cutoff 640



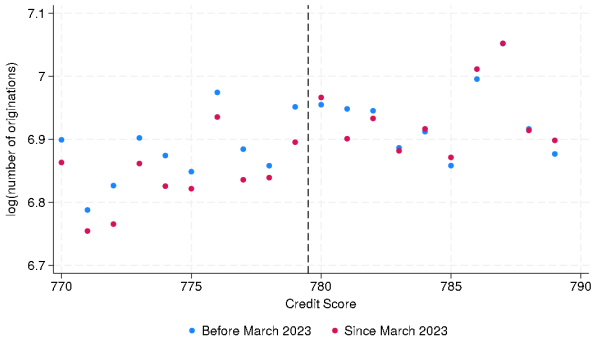

(b) credit score cutoff 680




(c) credit score cutoff 720




(d) credit score cutoff 760




(e) credit score cutoff 780

Note: this figure presents log of total home-purchase originations for each credit score point, residualized by origination year-month fixed effects.

Table A.1: Heterogeneous effects on the number of home-purchase loan originations by credit score and income by loan type

| | (1) CS < 700 | (2) CS ∈ [700, 740) | (3) CS ≥ 740 | (4) Full Sample |
|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.303*** | 0.075 | 0.379*** | 0.241*** |
| | (0.036) | (0.070) | (0.037) | (0.034) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income | 0.006 | 0.090 | -0.014 | 0.026 |
| | (0.042) | (0.068) | (0.041) | (0.032) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times CS$ | | | | -0.122* |
| | | | | (0.074) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income $\times CS$ | | | | 0.029 |
| | | | | (0.072) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y |
| N. Obs. | 7,680 | 3,840 | 7,680 | 19,200 |
| Pseudo $R^2$ | 0.68 | 0.79 | 0.85 | 0.87 |

(a) Number of GSE loan originations

| | (1) CS < 700 | (2) CS ∈ [700, 740) | (3) CS ≥ 740 | (4) Full Sample |
|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.091** | 0.144** | 0.130** | 0.133*** |
| | (0.041) | (0.058) | (0.051) | (0.043) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income | 0.028 | -0.114* | -0.034 | -0.038 |
| | (0.045) | (0.068) | (0.040) | (0.033) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times CS$ | | | | 0.068 |
| | | | | (0.096) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income $\times CS$ | | | | -0.082 |
| | | | | (0.078) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y |
| N. Obs. | 8,620 | 4,320 | 8,640 | 21,580 |
| Pseudo $R^2$ | 0.58 | 0.65 | 0.72 | 0.77 |

(b) Number of non-agency conforming loan originations

| | (1) CS < 700 | (2) CS ∈ [700, 740) | (3) CS ≥ 740 | (4) Full Sample |
|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.410 | -0.305 | 0.414** | 0.111 |
| | (0.437) | (0.359) | (0.162) | (0.166) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income | -0.103 | -0.011 | -0.164** | -0.085 |
| | (0.174) | (0.188) | (0.067) | (0.076) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times CS$ | | | | -0.420 |
| | | | | (0.533) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income $\times CS$ | | | | 0.044 |
| | | | | (0.210) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y |
| N. Obs. | 5,620 | 4,020 | 8,480 | 18,120 |
| Pseudo $R^2$ | 0.38 | 0.48 | 0.70 | 0.74 |

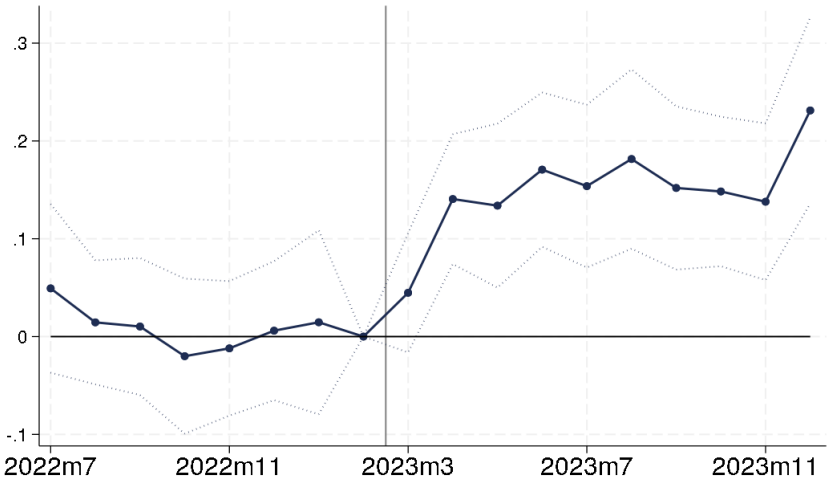
(c) Number of jumbo loan originations

| | (1) CS < 700 | (2) CS ∈ [700, 740) | (3) CS ≥ 740 | (4) Full Sample |
|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | -0.075*** | 0.016 | -0.090 | -0.075* |
| | (0.017) | (0.074) | (0.077) | (0.044) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income | -0.058** | 0.028 | -0.009 | -0.045 |
| | (0.029) | (0.161) | (0.115) | (0.073) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times CS$ | | | | -0.017 |
| | | | | (0.062) |
| $1[t > Feb23] \times E[-\Delta LLPA] \times$ Income $\times CS$ | | | | -0.006 |
| | | | | (0.110) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y |
| N. Obs. | 8,640 | 4,320 | 8,640 | 21,600 |
| Pseudo $R^2$ | 0.88 | 0.81 | 0.73 | 0.84 |

(d) Number of FHA/VA loan originations

Note: this table presents Poisson regression estimates of equation (2) for number of home purchases originations. We measure the income by the median point of incomes relative to the area median income in each income bin. We subtracted the median income relative to the area median income in these regressions. We also subtracted 740 from credit scores and divided it by 100. Standard errors are clustered by credit score.

Figure A.5: Event-study DIDs for the total number of home-purchase originations




Note: coefficient estimates and their 95% confidence intervals in the event-study version of equation (2).

# B Semi elasticity of relative home purchases by DTI

We examine whether the semi-elasticity of relative home purchase volume varies with DTI, in light of findings that the binding DTI constraint is the main mechanism through which interest rates influence home purchases (Bhutta and Ringo, 2021; Bosshardt et al., 2024a). Because the actual DTI is affected by the LLPA change directly, we construct the counterfactual DTI ("cDTI", henceforth) to compare borrowers that paid different LLPAs.

Specifically, our construction of the cDTI involves analyzing two groups of credit scores, depending on whether their average LLPA ($E[\Delta LLPA]$) increased or decreased. For credit scores with $E[\Delta LLPA] < 0$, we reverse this reduction on actual DTIs of loans originated *after* the LLPA reduction. This allows us to determine what their DTIs would have been if the LLPA had remained at its previous, higher level, and to identify potentially binding constraints. For credit scores with $E[\Delta LLPA] > 0$, we apply this increase to calculate cDTIs of loans originated *before* the increase. This allows us to identify borrowers whose DTIs would have been binding if they had originated loans after the increase in the LLPA.

Moreover, we also take into account the effects of net discount points purchased (points purchased net of lender credits) on the actual DTI in constructing the cDTI. Because of the high interest rate environment during our sample period, a majority of borrowers had some discount points purchased. Thus, to make cDTIs comparable across borrowers with different points purchased, the cDTI is constructed assuming no points purchased by undoing the effects of the actual points purchased on interest rates.

Defining $PI(r_{it})$ to be the principal and interest payment with interest rate $r_{it}$ of borrower

$i$ originating a loan in time $t$, the cDTI is calculated as follows:

$$cDTI_{it} = DTI_{it} + \frac{PI(\tilde{r_{it}}) - PI(r_{it})}{\text{income}}, \tag{18}$$

where $r_{it}$ and $\tilde{r_{it}}$ refer to the actual and counterfactual interest rates. The latter is defined as follows:

$$\tilde{r_{it}} = \begin{cases} r_{it} + \frac{NetPoints - E[\Delta LLPA]}{3}, & \text{if } E[\Delta LLPA] < 0 \text{ and } t > \text{Feb23} \\ r_{it} + \frac{NetPoints + E[\Delta LLPA]}{3}, & \text{if } E[\Delta LLPA] > 0 \text{ and } t \leq \text{Feb23} \\ r_{it} + \frac{NetPoints}{3}, & \text{otherwise.} \end{cases} \tag{19}$$

Table A.2 shows the semi-elasticity estimates for borrowers with different cDTI. The maximum DTI allowed for GSE loans is 50%, and there is a soft DTI limit at 45% for GSE loans. Moreover, the premium for the private mortgage insurance for conventional loans with LTVs greater than 80% can be also higher for loans with DTIs above 45%. Thus, we consider borrowers with low cDTI below 40%, borrowers with cDTI close to the binding constraints, and borrowers with binding cDTI (greater than 45%).

Comparing the results in columns 1 to 3 reveals that the semi-elasticites increase in cDTI. Borrowers with low cDTI have the semi-elasticity at 10% (col 1), much lower than the semi-elasticity of 19% for borrowers with binding cDTI (col 3). Columns 5 to 9 show that these high semi-elasticities for borrowers with high cDTI are driven by thier increased home-purchase originations of GSE loans and conventional conforming loans, whose interest rates were directly affected by the LLPA change.

Table A.2: Effects on the relative home-purchase loan originations by counterfactual DTI

| | Total | | | GSE | | | Conventional conforming | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) cDTI≤ 40 | (2) cDTI> 40 | (3) cDTI> 45 | (4) cDTI≤ 40 | (5) cDTI> 40 | (6) cDTI> 45 | (7) cDTI≤ 40 | (8) cDTI> 40 | (9) cDTI> 45 |
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.104*** | 0.181*** | 0.193*** | 0.261*** | 0.322*** | 0.393*** | 0.186*** | 0.270*** | 0.370*** |
| | (0.020) | (0.017) | (0.020) | (0.031) | (0.041) | (0.058) | (0.023) | (0.035) | (0.041) |
| Share of loan type | 0.55 | 0.45 | 0.25 | 0.25 | 0.18 | 0.09 | 0.42 | 0.27 | 0.13 |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N. Obs. | 3,600 | 3,600 | 3,600 | 3,600 | 3,600 | 3,600 | 3,600 | 3,600 | 3,600 |
| Pseudo $R^2$ | 0.85 | 0.73 | 0.71 | 0.83 | 0.77 | 0.72 | 0.85 | 0.77 | 0.73 |

Note: this table presents Poisson regression estimates of equation (2) for relative home purchases originations. Standard errors are clustered by credit score.

# C Additional material for the effects on other outcomes

Table A.3: Effects on loan amounts and home prices

| | log(House price) | | | | | log(Loan amount) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | All loans | GSE | Non-agency conforming | Jumbo | FHA/VA | All loan | GSE | Non-agency conforming | Jumbo | FHA/VA |
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.005 | 0.019*** | -0.007 | 0.003 | 0.005* | 0.004 | 0.028*** | -0.003 | 0.010 | 0.006* |
| | (0.003) | (0.007) | (0.008) | (0.017) | (0.003) | (0.003) | (0.007) | (0.008) | (0.014) | (0.003) |
| CS cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| County FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N. Obs. | 1,073,021 | 461,844 | 269,059 | 60,953 | 280,239 | 1,073,567 | 462,932 | 269,850 | 59,888 | 279,980 |
| Adj. $R^2$ | 0.63 | 0.52 | 0.59 | 0.43 | 0.62 | 0.55 | 0.47 | 0.53 | 0.48 | 0.60 |

Note: this table presents loan-level regression estimates of equation (1) for log of the price of the house purchased and log of loan amount. Standard errors are clustered by credit score.

# D Additional material on financial intermediation

Table A.4: Effects on borrowing costs by bank vs nonbank

| | Mortgage Rate | | | Net Orig Charges | | |
|---|---|---|---|---|---|---|
| | (1) GSE | (2) Non-agency conforming | (3) Conventional conforming | (4) GSE | (5) Non-agency conforming | (6) Conventional conforming |
| $1[t > Feb23]$ | -0.187*** | -0.014 | -0.082*** | -0.259*** | -0.032 | -0.118*** |
| $\times E[-\Delta LLPA]$ | (0.028) | (0.024) | (0.020) | (0.038) | (0.023) | (0.018) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y |
| LLPA grid FE | Y | Y | Y | Y | Y | Y |
| Lender × County FE | Y | Y | Y | Y | Y | Y |
| N. Obs. | 92,497 | 115,448 | 237,750 | 91,536 | 113,670 | 234,709 |
| Adj. $R^2$ | 0.71 | 0.77 | 0.75 | 0.26 | 0.45 | 0.35 |

(a) Banks

| | Mortgage Rate | | | Net Orig Charges | | |
|---|---|---|---|---|---|---|
| | (1) GSE | (2) Non-agency conforming | (3) Conventional conforming | (4) GSE | (5) Non-agency conforming | (6) Conventional conforming |
| $1[t > Feb23]$ | -0.221*** | -0.196*** | -0.208*** | -0.297*** | -0.401*** | -0.313*** |
| $\times E[-\Delta LLPA]$ | (0.014) | (0.045) | (0.012) | (0.023) | (0.064) | (0.020) |
| Cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y |
| LLPA grid FE | Y | Y | Y | Y | Y | Y |
| Lender × County FE | Y | Y | Y | Y | Y | Y |
| N. Obs. | 284,009 | 56,967 | 388,039 | 279,924 | 54,916 | 380,893 |
| Adj. $R^2$ | 0.67 | 0.71 | 0.66 | 0.27 | 0.37 | 0.28 |

(b) Nonbanks

Note: this table presents Poisson regression estimates of equation (2) for number of home purchases originations. We measure the income by the median point of incomes relative to the area median income in each income bin. We subtracted the median income relative to the area median income in these regressions. We also subtracted 740 from credit scores and divided it by 100. Standard errors are clustered by credit score.

Table A.5: Effects on nonbank share of originations

| | (1) All loans | (2) Conventional conforming | (3) GSE | (4) Non-agency conforming |
|---|---|---|---|---|
| $1[t > Feb23] \times E[-\Delta LLPA]$ | 0.019*** | 0.035*** | -0.002 | 0.048*** |
| | (0.005) | (0.008) | (0.008) | (0.014) |
| CS cutoff × Income bin × Orig YM | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y |
| County FE | Y | Y | Y | Y |
| N. Obs. | 1,088,460 | 732,540 | 442,874 | 223,766 |
| Adj. $R^2$ | 0.13 | 0.11 | 0.10 | 0.13 |

Note: this table presents loan-level regression estimates of equation (1) for the nonbank share of originations by loan type. Standard errors are clustered by credit score.

# E Analysis on credit score manipulation

## E.1 Presence of co-borrowers

If there is a co-borrower on a loan, the minimum CS among multiple borrowers on the same loan is used for underwriting. Thus, having a co-borrower would often decrease the underwriting CS, although it may increase the qualifying income. For this reason, the decision to have a co-borrower could be endogenous to the LLPA. For example, when the LLPA decreased more for CS 679 than CS 680, the downside of having a co-borrower with the CS below 680 has decreased, and more households may decide to include co-borrowers with lower CS on their loans. In that case, the DID regression would overstate the semi-elasticity for relative home purchases because the relative change in originations around the cutoff would be just driven by a downward shift in the underwriting CS because of more loans with co-borrowers.

We test whether the LLPA change led to changes in the presence of co-borrowers. Among the five credit score cutoffs in our sample, the LLPA declined more for CS right below the 680 and 720 cutoffs than CS right above each of the two cutoffs. Thus, including a co-borrower with CS below these cutoffs became less costly, and we may see more loans with co-borrowers with the underwriting CS below these cutoffs after the LLPA change. By contrast, in the rest of the credit score cutoffs (640, 760, and 780), the LLPA increased more for CS right below the cutoffs than CS right above the cutoffs. Thus, the hypothesis would predict fewer loans with co-borrowers for the underwriting CS below the cutoffs.

Table A.6 presents the regression results separately for the two types of CS cutoffs. We find no support for the hypothesis of a strategic decision to have a co-borrower.

Table A.6: Effects on whether a loan has a co-borrower

| | subsample 1: CS cutoff $\in \{680, 720\}$ | | | subsample 2: CS cutoff $\in \{640, 760, 780\}$ | | |
|---|---|---|---|---|---|---|
| | (1) Conventional conforming | (2) GSE | (3) Non-agency conforming | (4) Conventional conforming | (5) GSE | (6) Non-agency conforming |
| $1[t > Feb23] \times E[-\Delta LLPA]$ | -0.002 | 0.010 | -0.012 | 0.009 | 0.014 | 0.019 |
| | (0.009) | (0.012) | (0.019) | (0.008) | (0.010) | (0.012) |
| CS cutoff × Income bin × Orig YM | Y | Y | Y | Y | Y | Y |
| CS FE | Y | Y | Y | Y | Y | Y |
| County FE | Y | Y | Y | Y | Y | Y |
| N. Obs. | 197,428 | 125,164 | 71,927 | 733,246 | 542,382 | 404,942 |
| Adj. $R^2$ | 0.03 | 0.02 | 0.05 | 0.03 | 0.03 | 0.03 |

Note: this table shows loan-level regression in equation (1) for whether there is a co-borrower on a loan. Columns 1 to 3 use the subsample of CS cutoffs of 680 or 720, where the LLPA declined more for credit scores right below the cutoffs than those right above the cutoffs. Columns 4 to 6 use the subsample of CS cutoffs of 640, 760, or 780, where the LLPA increased more for credit scores right below the cutoffs than those right above the cutoffs.

## E.2 Credit scores evolution prior to origination

In this section, we examine the possibility of changes in credit score manipulation in response to the LLPA change, using the CRISM data. The advantage of CRISM is that the data provides information about a history of different credit score types for the six months prior to mortgage origination. Thus, we can see if there is an unusual change in a borrower's CS history prior to origination in response to the LLPA change.

The downside of CRISM is that it lacks information about a borrower's income. Thus, it is difficult to identify whether a borrower's income is above the area median income, thereby figuring out whether the May LLPA change is applied to the borrower. Thus, we only consider borrowers with CS around the 680 and 720 cutoffs because the LLPA for borrowers with credit scores right below these cutoffs declined more than for borrowers with credit scores right above the same cutoffs in both the LLPA exemption for the LMI first-time home buyers and the LLPA change in May 2023 for other borrowers. In other words, borrowers with CS just below one of the two cutoffs (e.g., 670–679 or 710–719) will pay a lower LLPA than those with CS just above the cutoff (e.g., 680–689 or 720–729) in 2023, regardless of borrowers' incomes. In this context, our DID identification assumption will be violated if borrowers below the cutoffs are less likely to manipulate their credit scores above the cutoff in response to the LLPA change. We test this hypothesis in two ways.

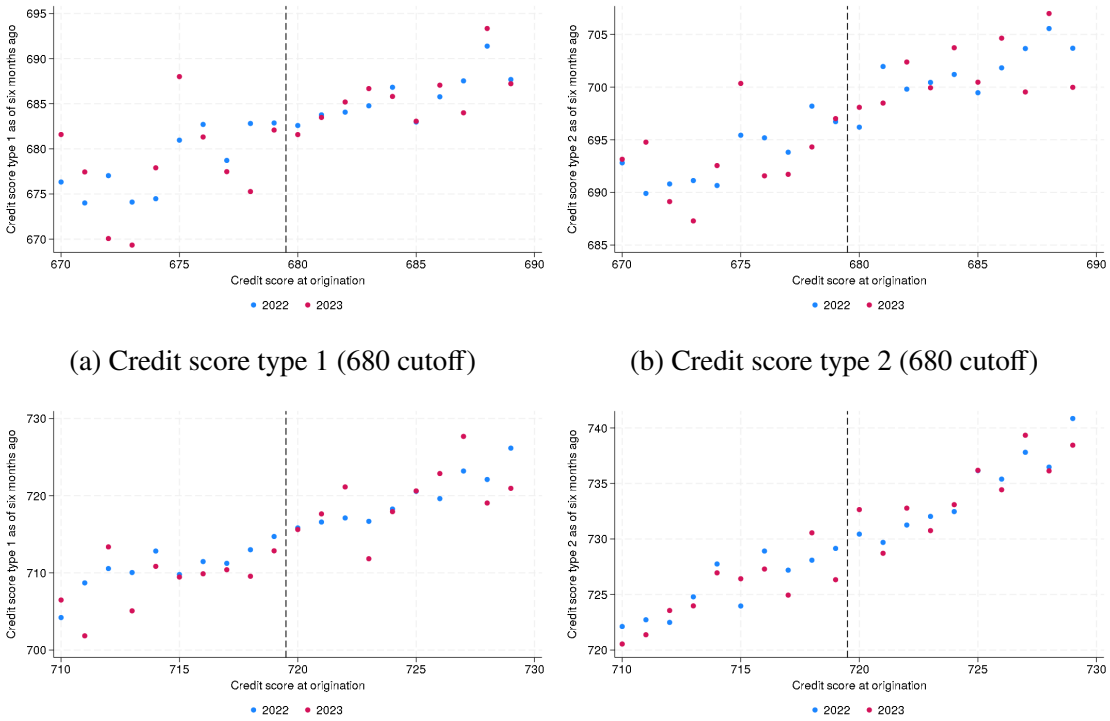
### E.2.1 Credit scores around cutoffs at the time of origination

First, we test the manipulation hypothesis by examining CS as of six months prior to origination for borrowers with underwriting CS around one of the two cutoffs at the time of origination. If borrowers can manipulate their CS, it will be less costly (not only in terms of money but also time) to stop the manipulation once the credit score crosses a cutoff used for LLPA pricing. In our context, in 2022 when the new LLPA change became effective, borrowers may have incentives to manipulate their credit scores to be just over the 680 or 720 cutoff. In 2023, they will have smaller incentives to do the manipulation with the new LLPA pricing. Thus, the manipulation hypothesis would predict that, in 2022, borrowers with underwriting CS right above one of the two cutoffs are more likely to have a larger increase in their CS over the six months prior to the origination, compared with borrowers with origination credit scores right below the cutoffs. Then in 2023, there will be fewer borrowers with underwriting CS just above one of the two cutoffs, and there increases in CS above the cut-off should be smaller.

However, data patterns for GSE loan borrowers in Figure A.6 are not consistent with the manipulation hypothesis. Aside some noises, six-month-ago CS are generally increasing in origination credit scores and smooth around the cutoffs both in 2022 and 2023. In addition, the relationship between six-month-ago CS and underwriting CS does not seem to have changed in 2023.

We also estimated the DID regression of two types of six-month-ago CS by comparing borrowers with underwriting CS around one of the two cutoffs in 2022 and 2023. The independent variable is whether the origination CS is below the cut-off. The manipulation hypothesis implies that those above the cut-off should have a lower increase in CS compared to those below the cutoff in 2023 as compared to 2022, which would imply a positive coefficient on 1[CS<cutoff] $\times$ 1[t>Jan2023]. The estimates presented in Table A.7 confirm that there was no statistically significant change in the relationship in 2023, with coefficients on being below the cut-off being negative which goes against the direction of a change in manipulation.

Figure A.6: Credit scores as of 6 months prior to origination




(a) Credit score type 1 (680 cutoff)

(b) Credit score type 2 (680 cutoff)

(c) Credit score type 1 (720 cutoff)

(d) Credit score type 2 (720 cutoff)

Note: residualized by origination year-month and zipcode

### E.2.2 Comparing CS at the initial credit check and underwriting CS

The second way to test the manipulation hypothesis is to check whether borrowers who checked their CS prior to origination change their CS to be on the opposite side of CS cutoffs. That is, instead of using the underwriting CS as the independent variable, we use their CS at the time of their first credit inquiry. CRISM provides information about when a credit inquiry was made and the CS at the time of the inquiry.

We restrict the sample to borrowers who never had any credit check before they first appeared

Table A.7: Regressions with six-month-ago CS as the dependent variable

| | Cutoff = 680 | | Cutoff = 720 | |
|---|---|---|---|---|
| | (1) CS type 1 | (2) CS type 2 | (3) CS type 1 | (4) CS type 2 |
| 1[CS < cutoff] × 1[t ≥ Jan2023] | -0.452 | -0.309 | -1.750 | -0.519 |
| | (1.960) | (1.605) | (1.675) | (0.846) |
| Orig YM FE | Y | Y | Y | Y |
| Zip FE | Y | Y | Y | Y |
| N. Obs. | 28,814 | 28,814 | 65,573 | 65,573 |
| Adj. $R^2$ | 0.03 | 0.03 | 0.02 | 0.04 |

Note:

in CRISM as of 6 months prior to origination. This sample selection excludes borrowers who potentially know their CS before appearing in CRISM and potentially engage in manipulation. Among borrowers who never had a credit check as of 6 months prior to origination, we identify CS at the time of the initial mortgage inquiry using two types of credit scores, which we call "initial CS."

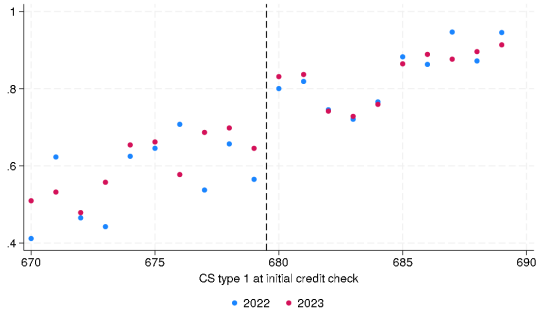
We note that as CRISM reports the initial CS from only one agency, whereas the tri-agency median credit score is used for origination. Furthermore, borrowers may correct their credit report after making the initial inquiry, thus changing their CS, and so some noise in the relationship between the initial check and the origination CS is expected. But, for a change in manipulation to explain our result, we would need the relationship between initial CS and origination CS to change over time.

More specifically, if there is a manipulation, where a borrower with the initial CS below a cutoff manipulates their credit scores, so their origination credit scores are located above the cutoff, this manipulation behavior will only violate our identification assumption if it changes over time. Empirically, this would imply that the share of borrowers with the origination credit score above the cutoff decreases in 2023 for borrowers with initial CS below the cutoff. This would imply a negative coefficient in front of 1[Initial CS<cutoff] × 1[t>Jan2023].

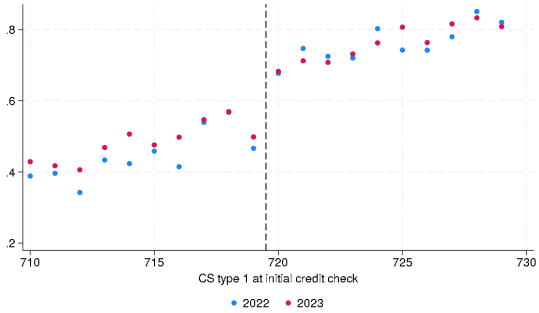
Figure A.7 shows data patterns that are not consistent with the sophisticated manipulation behavior that would respond to the LLPA change. If anything, panels (a) and (b) show that credit scores are rather persistent over time instead of being subject to manipulation. The sophisticated manipulation would predict that those with initial CS below the cutoff would be less likely to have their origination credit scores above the cutoff after the LLPA change. The data do not seem to support the hypothesis.

Table A.8 presents the estimates of the DID regression testing the hypothesis. The estimate in column 3 is marginally significant, but the coefficient estimate has the opposite sign to what a change in manipulation hypothesis predicts.

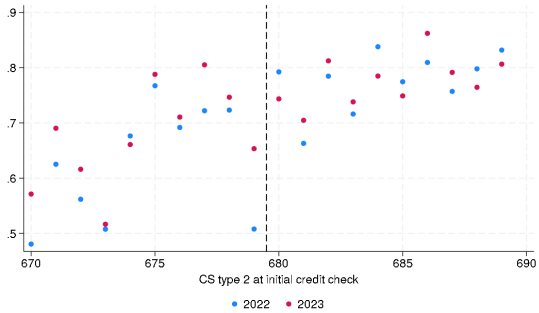
Figure A.7: Share of borrowers with origination credit scores above cutoff



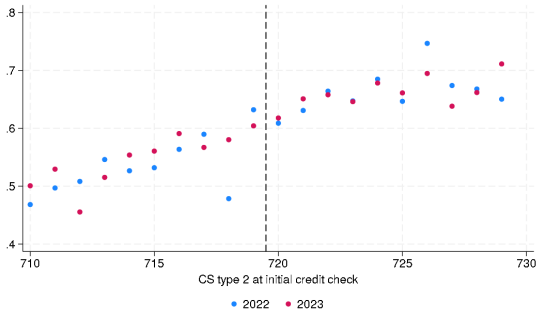

(a) 1[Origination CS ≥ 680];
(by credit score type 1 at initial check)




(b) 1[Origination CS ≥ 720];
(by credit score type 1 at initial check)




(c) 1[Origination CS ≥ 680];
(by credit score type 2 at initial check)




(d) 1[Origination CS ≥ 720];
(by credit score type 2 at initial check)

Note: residualized by origination year-month and zipcode

Table A.8: Credit score manipulation (in terms of initial CS)

| | Cutoff=680, Dep. Var. = 1[Origination CS ≥ 680] | | Cutoff=720, Dep. Var. = 1[Origination CS ≥ 720] | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | Initial CS type 1∈ [670, 689] | Initial CS type 2∈ [670, 689] | Initial CS type 1∈ [710, 729] | Initial CS type 2∈ [710, 729] |
| 1[Initial CS< cutoff] × Post | 0.039 | 0.049 | 0.038* | 0.013 |
| | (0.059) | (0.037) | (0.020) | (0.021) |
| Orig YM FE | Y | Y | Y | Y |
| Zip FE | Y | Y | Y | Y |
| N. Obs. | 3,080 | 2,310 | 9,627 | 8,897 |
| Adj. $R^2$ | 0.13 | 0.01 | 0.13 | 0.02 |

Note:

## E.3 Computational Details of Housing Search Model

As noted in Jiang et al. (2024), a feasible computational technique is to first assume $M^S, M^B, V^B, V^S$, then solve for the matching rates $\lambda^S, \lambda^B$ using Equation (12), then update $G(r)$ using flow equality using Equation (13), then invert to find trade cut-offs $\bar{\epsilon}(r_i)$, then updating $V^B, V^S$ via Bellman equations, then re-solving for trade-cutoffs $\bar{\epsilon}(r_i)$ in Equation (11), then updating $M^B$ using Equation (13), until convergence. Instead of fully updating to the new implied values each iteration, a dampening parameter of 0.3 where each new value is equal to (1-0.3) times the older value plus 0.3 times the new value is helpful for achieving convergence across many runs.

Then, we use an outer loop that conducts a bisection search over $M^S$ to solve for a desired $\mu^S$ as a model primitive, where $\mu^S$ is calibrated to the buyer success probability $s$ and must therefore be equal to $\sum_i \mu^B(r_i)s$. Following that, we use a second outer loop incorporating the first outer loop to solve for the inflow parameters $\gamma^B, \xi_{cklfb(j)}$, the latter of which is via nested fixed point following BLP (Berry et al., 1995). Counterfactual simulations involve going through the same two-outer-loop procedure, but not updating the $\xi_{cklfb(j)}$ in the second outer loop. We construct model counterparts to the empirical May 2023 relative home purchase elasticities to g-fee changes by comparing borrowers around each CS bin above and below the cut-offs, computing the relative home purchase semi-elasticities within each of the five cut-offs we use in the main empirical analysis, and equal weighting them as we do in the main empirical analysis.

## E.4 Estimating GSE subsidies

We obtain back-of-the-envelope estimates of the extent of the GSE subsidy across credit score and LTV bins via a model of borrower default and prepayment. Suppose that borrowers default at a rate $\lambda_{d,t}$ and pay down their principal at a rate $\lambda_{p,t}$, where $\lambda_{p,t}$ would include both the regularly scheduled amortization pay down as well as prepayment. The loss given default is $L_t$. The upfront g-fee is $g^u$, and the on-going g-fee is $g^o$. The value of the mortgage to the GSEs with discount rate $\delta$ and capital costs $k$ is then:

$$V^{GSE}(g^u, g^o) = g^u + E\sum_t \delta^t l_t(g^o - LGD(S_t)\lambda_d(S_t) - k) \tag{20}$$

where $l_t = l_{t-1}(1 - \lambda_p(S_t) + \lambda_d(S_t))$ is the fraction remaining of the balance of the mortgage at time $t$, and $k$ is the capital cost. We use a two-state pricing model, where states $S_t = \{0, 1\}$ are whether the economy is in a housing crash. The default and principal paydown rates are then:

$$\lambda_p(S_t) = \lambda_p^0, \lambda_d(S_t) = \lambda_d^0, \text{ if } S_t = 0, \tag{21}$$

$$\lambda_p(S_t) = \lambda_p^1, \lambda_d(S_t) = \lambda_d^1, \text{ if } S_t = 1. \tag{22}$$

We calibrate the value of $\lambda_d(S_t), \lambda_p(S_t), LGD(S_t)$ by taking the mean of their respective values within each credit score, LTV, and DTI group from the Fannie Mae Single-Family Performance Data from 2000 to 2019, where the housing crash years are taken to be 2008 to 2013 ($S_t = 1$), and the rest being non-housing crash years ($S_t = 0$). Notably, to compute the $LGD(S_t)$, we follow the steps in Fannie Mae's loss data analysis tutorial.[1] Credit score and LTV groups come from the LLPA grid, while DTI is split into three groups: $\{\leq 30$, This procedure includes the effects of private mortgage insurance which reduces GSE losses at LTVs greater than 80% as well as the foregone interest that were covered by Fannie Mae to be paid to investors. While $S_t = 1$ occurred in 30% of the years in the data, we downweight the probability of being in a housing crash state going forward by assuming that $S_t = 1$ has a 10% chance of occurring. We set the on-going GSE revenue to $g^o = 0.49\% - 0.12\%$, consistent with the our sample period's on-going g-fee of 49 basis points minus operating costs of 6 basis points and a 10 basis point statutory surcharge.[2]

Finally, we obtain our estimate GSE capital cost $k$ by taking the GSE retained capital levels by credit score bin and LTV from deRitis and Zandi (2014), and multiply it by a 10% capital cost, as per the return on equity of FDIC-insured institutions (deRitis and Zandi, 2014). We also assume a discount rate of $\delta = 0.96$, consistent with the risk-free rate over this time period. Notably, the estimates of both the costs of retained equity capital $k$ as well as the discount

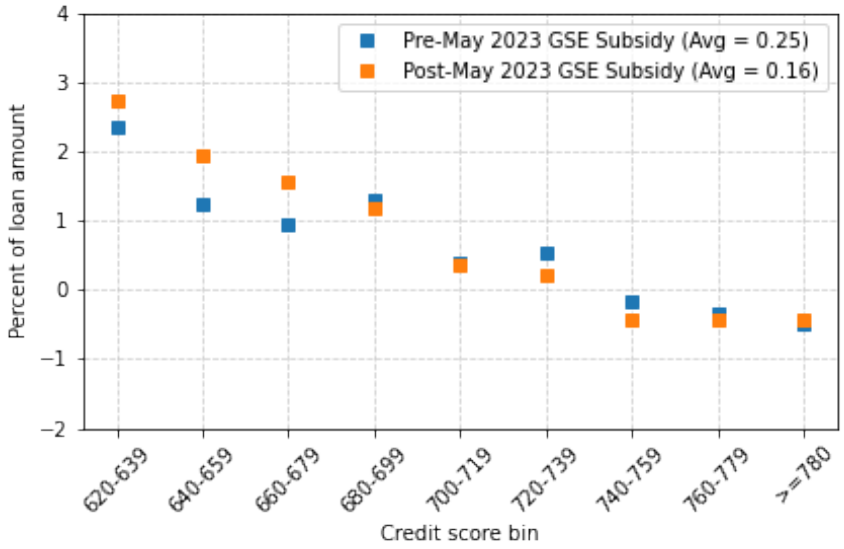
[1] Available HERE.

[2] See discussion by the Urban Institute HERE.

rate $\delta$ are likely too low compared to what would be required of large systemically important financial institutions holding similar risks. Therefore, our estimate of the GSE subsidy, based on the GSEs' current capital requirements as well as funding costs, would understate the extent of the GSE subsidy from the social perspective.[3]

Our estimated GSE subsidy $-V^{GSE}(g^u, g^o)$ for each CS bin, pre-and-post May 2023, is presented in Figure A.8, where we present the average subsidy across CS bins in the LLPA matrix taking into account the LTV distribution within each CS bin. In general, we see significant cross-subsidy from high to low CS borrowers and from low to high LTV borrowers, which has increased after May 2023. This cross-subsidy is consistent with estimates from FHFA in even non-crisis periods, as described in Gerardi (2017). The overall positive level of the subsidy of 25 basis points pre-May 2023 and 16 basis points post-May 2023 reflects the fact that the GSEs are not earning the return on equity that FDIC-insured institutions earn on their retained capital, which can be viewed as a subsidy from their equity holders (i.e. the Treasury) to homeowners.

Figure A.8: GSE subsidy by credit score, pre-and-post May 2023




Note: this figure presents the GSE-volume weighted estimated GSE subsidy across purchase mortgages by credit score bins, pre-and-post the May 2023 LLPA change. The raw estimated subsidies by credit score and LTV bins are shown in Appendix Table A.9.

[3]For more discussion of GSE capital costs, see Ambrose and Warga (2002) and Richardson et al. (2017).

Table A.9: Estimated GSE subsidy by credit score and LTV, at current capital levels

| credit score / LTV | ≤60 | 60.01–70 | 70.01–75 | 75.01–80 | 80.01–85 | 85.01–90 | 90.01–95 | > 95 |
|---|---|---|---|---|---|---|---|---|
| ≥ 780 | -1.025 | -0.674 | -0.675 | -0.852 | -0.090 | -0.046 | -0.042 | -0.531 |
| 760–779 | -0.891 | -0.577 | -0.566 | -0.733 | 0.064 | 0.071 | 0.027 | -0.398 |
| 740–759 | -0.843 | -0.480 | -0.448 | -0.572 | 0.091 | 0.279 | 0.180 | -0.242 |
| 720–739 | -0.657 | 0.234 | 0.122 | 0.026 | 0.802 | 0.941 | 0.910 | 0.647 |
| 700–719 | -0.584 | 0.119 | -0.155 | -0.283 | 0.611 | 0.791 | 0.750 | 0.673 |
| 680–699 | -0.299 | 1.165 | 0.708 | 0.530 | 1.290 | 1.589 | 1.697 | 1.992 |
| 660–679 | -0.139 | 1.000 | 0.297 | -0.007 | 0.375 | 1.255 | 1.482 | 2.320 |
| 640–659 | -0.392 | 1.459 | 0.308 | 0.347 | 0.860 | 1.607 | 2.188 | 2.945 |
| 620–639 | 0.678 | 2.402 | 1.661 | 1.500 | 2.432 | 2.089 | 3.633 | 5.469 |
| **max - min** | 1.703 | 3.076 | 2.336 | 2.351 | 2.521 | 2.135 | 3.675 | 6.000 |

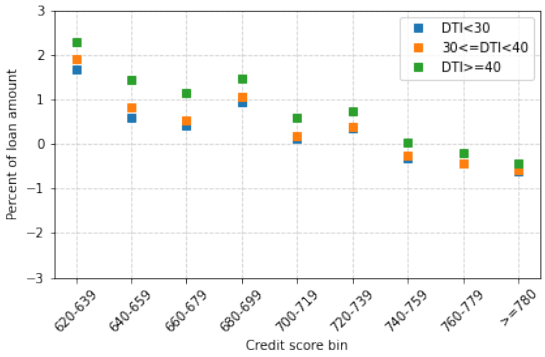
(a) Pre-May 2023

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| ≥ 780 | -1.025 | -0.424 | -0.425 | -0.727 | -0.215 | -0.046 | -0.042 | 0.094 |
| 760–779 | -0.891 | -0.327 | -0.566 | -0.858 | -0.311 | -0.179 | -0.223 | 0.102 |
| 740–759 | -0.843 | -0.355 | -0.573 | -0.947 | -0.659 | -0.221 | -0.195 | 0.008 |
| 720–739 | -0.657 | 0.234 | -0.128 | -0.474 | 0.052 | 0.441 | 0.535 | 0.897 |
| 700–719 | -0.584 | 0.244 | -0.030 | -0.408 | 0.111 | 0.541 | 0.625 | 1.298 |
| 680–699 | -0.299 | 1.040 | 0.833 | 0.530 | 0.915 | 1.339 | 1.572 | 2.367 |
| 660–679 | -0.139 | 1.250 | 1.172 | 0.868 | 1.000 | 1.755 | 2.107 | 3.320 |
| 640–659 | 0.108 | 1.584 | 1.558 | 1.097 | 1.610 | 2.357 | 3.063 | 4.195 |
| 620–639 | 1.053 | 2.402 | 2.536 | 1.750 | 2.807 | 2.714 | 4.633 | 7.219 |
| **max - min** | 2.078 | 2.826 | 3.109 | 2.697 | 3.466 | 2.760 | 4.856 | 7.126 |

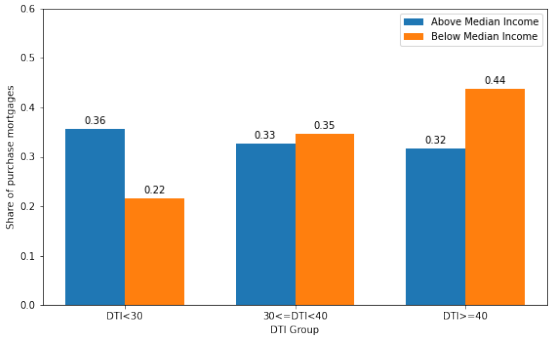
(b) Post-May 2023

Note: this table presents the estimated GSE subsidy across purchase mortgages for different credit score/LTV bins, pre-and-post the May 2023 LLPA change.

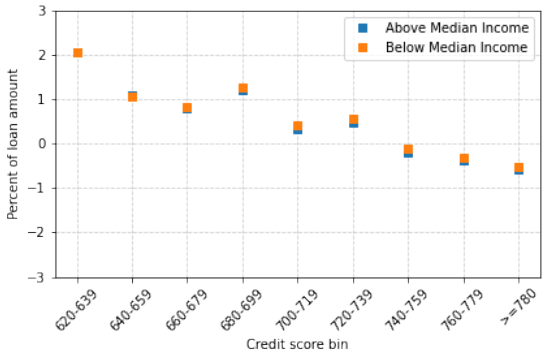
Figure A.9: GSE subsidy by DTI and income




(a) GSE subsidy by CS and DTI




(b) DTI by income group



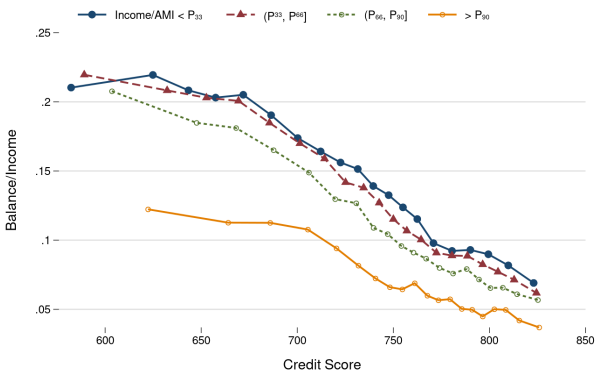

(c) GSE subsidy by CS and income group, as implied by DTI

Note: This figure presents our estimates of GSE subsidies by DTI group in Panel (a), the distribution of DTI by income group in Panel (b), and by above/below median income borrowers based on their DTI distribution in Panel (c).

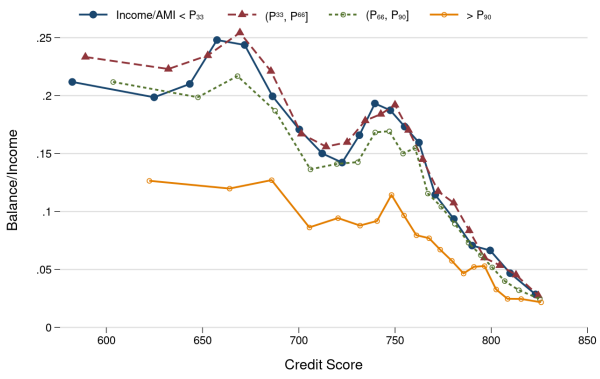
# F Additional material for financial characteristics of borrowers by credit score and income group

Figures A.10(a) to A.10(c) break out the total non-mortgage debt into several components, including auto loan, student loans, and other (such as personal loan) balances. Notably, the differences between income groups are always greater on the lower end of credit score spectrum. The lower the income, the higher is the auto loan balance burden relative to borrower income among all non-LMI groups. The LMI borrowers had lowest auto loan balance-to-income ratio probably because had difficulty qualifying for auto loans. Groups 2 have slightly higher student loan debt burden than Group 1 and Group 1 had higher student loan burden than Group 3, for borrowers with scores below 700. Moreover, Figure A.10(c) demonstrates that Group 3 have higher debt burden from other types of loans, including personal loans.

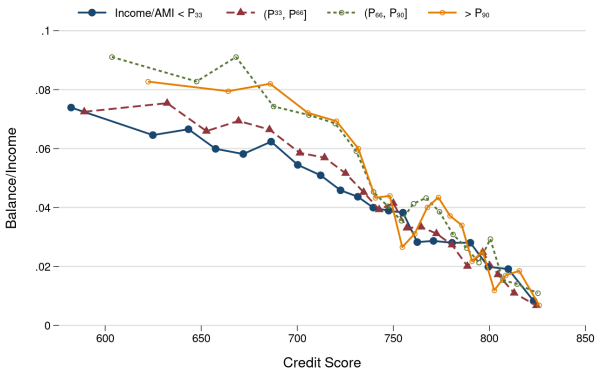
Figure A.10: Financial Constraints of Mortgage Borrowers by Credit Score and Income Level




(a) Ratio of auto loan balance to income




(b) Ratio of student loan balance to income




(c) Ratio of other closed-end loan balance to income