White Out Bad Policy: Pass-Through Under Costly Debt Sale (and its Reversal)*

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Abstract

I study how lenders pass through costs associated with debt sale downstream to borrowers. I show that when the cost of debt sale increases, lenders fully pass these costs to borrowers through both higher interest rates and upfront costs. I exploit the timing of an unanticipated FHFA policy that charged a 0.5% fee on the total loan amount sold to the GSE's. Using a difference-in-differences research design and high-frequency mortgage data, I find that the 50bp fee led to an 8bp increase in interest rates and 20bp increase in discount points, totaling a 100% pass-through. Geographic variation in loan, borrower, and market characteristics fail to explain any heterogeneity in pass-through. In concentrated markets, however, high market share lenders pass through 120% of these costs while low market share lenders pass through as little as 60%. Finally, I rule out an information-channel by conducting placebo tests on the policy's eventual reversal.

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1 Introduction

The secondary market for mortgage debt plays a pivotal role in household access to finance and broader macroeconomic conditions. This became most clear in the expansion of credit preceding the 2008 financial crisis and the proceeding government intervention that followed. Understanding how frictions within this market affect household balance sheets is critical to broader concerns over financial stability.

Using a high-frequency dataset of mortgage rate-locks and a quasi-experimental research design, I study how lenders pass through costs associated with debt sale downstream to borrowers. I show that when lenders face higher costs selling debt, they pass through 100% of this fee to borrowers in the form of higher interest rates and upfront fees. Moreover, in concentrated markets, high market share lenders pass through 120% of these costs while low market share lenders pass through as little as 60%.

Causally identifying pass-through is empirically challenging. Changes in funding costs may correlate with changes in market risk, reflecting borrower selection in lending markets. These changes in secondary market fees may also signal new information regarding local economic conditions, thus biasing estimates to reflect responses to new information as opposed to material costs. Furthermore, data limitations prevent measuring the immediate effect of these costs given the lag between loan application and closing dates. Even still, data often fails to reflect key loan terms reflecting the cost of borrowing, such as upfront costs paid by the borrower to lower their interest rate.

To overcome this empirical challenge, I use a difference-in-differences identification strategy exploiting a change in the fee structure for conforming loans sold on the secondary market. On August 12, 2020, the Federal Housing Finance Authority (FHFA) introduced the Adverse Market Refinance (AMR) fee. The FHFA announced that it would charge lenders a 0.5% AMR fee on the size of a loan purchased by Fannie Mae and Freddie Mac. Key to my research design, this policy only applied to conforming refinance loans. Furthermore, the announcement was a shock to the mortgage industry. Its leaders expressed concern and confusion over how the policy change conflicted with the actions of other federal agencies. Finally, I overcome data limitations by employing high-frequency mortgage rate-lock data, observing both interest rates and discount fees (upfront costs) reflecting immediate changes in upstream funding costs.

I show that when the cost of debt sale increases, lenders fully passed these costs to borrowers. I estimate that in the two-week period immediately following the FHFA announcement, refinance loans experienced only a 7.7 basis point increase in interest rates relative to purchase mortgages. This represents a 2.7% increase in rates relative to their pre-period averages. The effect is immediate, materializing within the first day of the announcement.

I further find that some of the upstream costs pass through to loan prices. When borrowers desire to reduce their interest rate, they simply purchase "points" as a percentage of their loan size. For every four points that a borrower buys, their interest rate falls by 1 percentage point. Following the FHFA announcement, I find that borrowers buy 0.1978 more points from lenders originating refinance loans relative to purchase mortgages. Converting the increase in interest rates into discount points to fully reflect the increased cost of borrowing, I find that lenders pass through 101.2% of the AMR fee to borrowers.

I next test for how pass-through correlates with local economic conditions. Using factors that previous research has found to correlate with refinancing demand and pass-through, I find virtually no statistically significant differential variation. Using pre-period geographic characteristics, I find that the treatment effect fails to vary with average loan terms (credit sore, LTV, DTI, and loan age), economic conditions (education, income, house prices, and employment), and market structure (market concentration, secondary market access, and bank size).

Nevertheless, I find substantial heterogeneity within counties with high market concentration. In particular, lenders with substantial market share in high concentration markets pass through 120% of upstream costs whereas lenders with low market share pass through as little as 60%. Importantly, market share fails to predict pass-through in competitive markets.

I validate my results in several ways. First, I expand my sample to include non-conforming loans. While my results may reflect systematic change in interest rates on refinance loans, I can account for these trends using untreated non-conforming refinance loans. I find that common trends for refinance loans across conforming status fail to explain the treatment effect that I estimate in my main results. Second, I conduct a placebo test using the FHFA's reversal and extension of the timing of the AMR fee. If refinance loans differed from purchase loans in a manner that correlated with the timing of the FHFA announcement, then this would be reflected in variation across periods where the treatment was functional and periods when it was no longer salient. Instead, I find that interest rates were elevated following the initial announcement, fell after the FHFA's reversal, and increased again as lenders approached the time of the AMR fee's extension.

Ultimately, my results show that when debt sale becomes costly, lenders pass these costs fully through to borrowers in the way of higher interest rates and upfront fees. I find that this pass-through is equally borne across various loan, borrower, and market characteristics at the geography level. As an exception, heterogeneity in pass-through appears to correlate with market concentration and market share, jointly. These results are key to policy makers interested in the link between financial stability and household balance sheets.

Related Literature

This paper contributes to several strands of the literature. First, this paper contributes to the literature exploring the relationship between secondary market prices and primary market outcomes. Since the financial crisis, significant research has found that capacity constraints affect the pass-through of prices between secondary and primary markets for mortgage loans (Fuster et al., 2013, 2017, 2021). Passing funding costs through to borrowers appears to function through multiple channels beyond interest rates, including origination fees (Liu, 2019; Benetton et al., 2023) and credit rationing (Lim, 2020). When funding costs fall, for example, then instead of a reduction in adverse selection, credit quality deteriorates due to credit expansion and advantageous selection (Tsai, 2023; Ahnert and Kuncl, 2020; Gete and Reher, 2020). Beyond lending markets, a growing literature explores the relationship between secondary market liquidity and equity issuance (Brugler et al., 2021; Bessembinder et al., 2015; Ellul and Pagano, 2006). This paper extends previous work by studying how an explicit increase in the cost of selling debt passes through to primary market financing costs.

This paper is also related to work on the refinancing channel of monetary policy. Prior research has identified several factors that may limit the mortgage refinancing channel of monetary policy. These factors include the average coupon on outstanding mortgages (Berger et al., 2021), home equity (Caplin et al., 1997; Beraja et al., 2018), employment (Defusco and Mondragon, 2020), income (Greenwald, 2018; Agarwal et al., 2021), financial constraints (Agarwal et al., 2013), market structure (Scharfstein and Sunderam, 2017), and fiscal policy (Scharlemann and van Straelen, 2022). I offer novel insight into how variation in pass-through of costly debt sale correlates with competition and market share, jointly.

The closest research to my paper is contemporaneous work by van Binsbergen and Grotteria (2024). Here, the authors similarly study high-frequency pass-through of funding costs and its interaction with local market concentration, albeit in the context of monetary-policy shocks. While their paper similarly finds full pass-through with no variation across market concentration, they find that pass-through takes four weeks to materialize. In contrast, my results indicate that costs fully pass through immediately through both discount points as well as interest rates. Furthermore, while concentration fails to explain variation in pass-through, there exists significant heterogeneity *within* concentrated markets.

My setting and data offer several advantages that facilitate these results. First, my setting exploits a shock exclusive to the funding cost of refinance mortgages, as opposed to a monetary policy shock affecting loan demand, bank balance sheets, and the broader macroeconomy. Moreover, my data includes the upfront costs that borrowers pay to bring down interest rates. The absence of discount points may severely bias estimates of pass-through down, which in my setting represent 40% of overall pass-through. Finally, I study high-frequency changes in mortgage terms at the moment of rate-lock, weeks in advance of mortgage closing. Hence, I am able to measure changes in lending behavior at the moment lenders would most immediately reflect these costs.

The remainder of this paper proceeds as follows. Section 2 describes the institutional details of the AMR fee. Section 3 describes the data, sample construction, and summary statistics. Section 4 estimates the effect of the FHFA announcement on interest rates and discount points. Section 5 explores the mechanism driving the effect of the FHFA announcement. Section 6 investigates the robustness of my results. Section 7 concludes.

2 Institutional Background

In this section, I outline the institutional details related to the FHFA Adverse Market Refinance Fee. In the first subsection, I describe the introduction of the fee itself. In the second subsection I describe the broader market's response and the reversal of the initial AMR fee.

2.1 Adverse Market Refinance Fee

During the COVID-19 Pandemic, Fannie Mae and Freddie Mac suffered significant costs and risk from suspending foreclosures and repayment for distressed borrowers. In order to recover any potential losses from this pandemic response, on August 12, 2020, the Federal Housing Finance Agency (FHFA) announced the Adverse Market Refinance Fee (AMR fee). The new fee charged lenders 0.5% on the total loan amount at the time that Fannie Mae or Freddie Mac would acquire the mortgage. This meant that the fee was not applicable to purchase mortgages, loans securitized under Ginnie Mae, and loans retained by lenders on their portfolios. While the timing of the original announcement is no longer available, I assume the precise time to be 7:30 PM Eastern Standard Time.[1]

The FHFA originally announced the fee for all loans acquired on or after September 1, 2020. This gave lenders only three week period between the announcement date and effective date. However, processing time alone takes several months for a newly originated loan before a lender is ready to sell the mortgage. Therefore, lenders expected to bear the cost of the

[1] Figure 1 displays the Mortgage Banker's Association response to the initial announcement at 8:30 PM Eastern Standard Time. For the purposes of this paper, I assume a lag of at least one hour between the initial announcement and subsequent response.

AMR fee for all loans originated immediately after the announcement. Naturally, this cost was expected to be passed on to households in the form of higher interest rates.

2.2 Market Response and Reversal

Leaders in the mortgage industry felt that the fee undermined efforts by the Federal Reserve to keep rates low. The Mortgage Bankers Association denounced the fee, stating that "the recent refinance activity has not only helped homeowners lower their monthly payments, but it is also reducing risk to the GSEs and taxpayers."[2] The Community Home Lenders Association similarly felt that the FHFA's policy would harm homeowners whose recent refinancing activity was helping strengthen their finances noting that "Congress, the Federal Reserve, and Treasury have been taking strong actions to support our economy."[3] Finally, trade groups for community banks and credit unions further expressed concern over harm to homeowner balance sheets from the increased cost of borrowing.

Of particular note, the announcement was a shock to lenders. In a joint statement addressing the policy change, a coalition of the industry's most prominent representatives described the FHFA announcement as a surprise. In fact, the signatories went as far as to express confusion over how the policy directly conflicted with the actions of other federal agencies.[4]. Following extensive lobbying by industry leaders, on August 25, 2020, two weeks after its initial announcement, the FHFA announced that it would push the timing of the AMR fee to December 1, 2020, as opposed to September 1, 2020. This resulted in a three-month delay in the timing of the policy. Furthermore, the FHFA would only apply the AMR fee to refinance mortgages with a loan size larger than $125,000.

3 Data

3.1 Optimal Blue

Optimal Blue is a lending services company that facilitates the process of locking in rates for borrowers prior to closing on a mortgage. The company provides a software platform for lenders to identify loan rates, determine eligibility, and lock in the loan's terms. Loan officers select one of various offers from intermediaries to fund a mortgage origination. If the mortgage closes, the intermediary is obligated to fund the loan. Optimal Blue data includes borrower and loan characteristics, such as the mortgage rate, credit score, debt-to-income

[2] Sorohan, Mike. "MBA Objects to GSE 'Adverse Market Refinance Fee'." *MBA Newslink*, 8/13/2020.

[3] "Industry, White House decry refi fee." *Dodd Frank Update*, 8/14/2020.

[4] Ramirez, Kelsey. "Housing industry turns against Fannie, Freddie's added refinance fee." *Housing Wire*, 8/14/2020

(DTI), loan-to-value (LTV), FHA status, loan amount, occupancy type, property type, and county. In addition, the data provides a timestamp for when the mortgage rate is locked-in for the borrower. The platform is used by more than 1,000 lenders, particularly smaller institutions, and accounts for at least one-third of recent loan originations.

Optimal Blue data provides several advantages over alternative data sets used by the literature. First, the data is incredibly granular, both in time and location. Whereas even proprietary data records mortgages at the monthly level, Optimal Blue provides data an exact timestamp for a given loan. Second, the data represents loan terms at the time of locking in a mortgage rate as opposed to at origination. Mortgage origination data will not immediately present the effects of a policy change because of the lag between a mortgage rate lock and its origination. Finally, Optimal Blue as a platform is primarily used by smaller institutions, further distancing the market participants in this study from any influence over the policy change itself.

3.2 Home Mortgage Disclosure Act

The Home Mortgage Disclosure Act requires that nearly the universe of all lenders operating within the United States report data on loan, property, and borrower characteristics for every mortgage application received. Few lenders are exempt from this requirement due to size, location, and loan volume. Loan characteristics include loan size, type, purchaser, lien status, lender identifier, and action taken. Borrower characteristics include income, race, ethnicity, and gender. Property characteristics include property type, occupancy status, state, county, and census tract. I use a crosswalk maintained by Robert Avery to identify parent companies associated with a given subsidiary so that analysis is at the bank level.

3.3 Single Family Loan-Level Dataset

In order to measure the average characteristics of outstanding loans held by the GSE's, I use Fannie Mae and Freddie Mac's Single Family Loan-Level Dataset. Fannie Mae and Freddie Mac loans represent 27 percent of all mortgages originated in 2019, both in number and volume of all mortgages originations. The dataset includes origination and performance characteristics, such as the loan amount, LTV ratio, DTI ratio, property type, loan purpose, three-digit zip, and delinquency status.

3.4 Other Data Sources

I collect data from the Bureau of Economic Analysis containing detailed annual measures of MSA level employment. For annual county level unemployment, I use the unemployment data provided by the Bureau of Labor Statistics. I also use zip-code level data on tax filings provided by the IRS.

3.5 Summary Statistics

Table 1 reports summary statistics for loan characteristics of loans originated between January 2019 and December 2019. Focusing on the full calendar year preceding the FHFA announcement allows me to use HMDA data from 2019 to compare the representative nature of loans featured in the Optimal Blue. In order to account for outliers, I retain only conforming loans with an initial mortgage balance less than or equal to $10 million, an initial combined loan-to-value ratio less than or equal to 150, and an initial interest rate less than 30 percentage points. I require that these loans have an initial term length of 30, 20, or 15 months. Given that Optimal Blue is a platform for secondary market sale, I retain HMDA loans that are sold after origination.

With the restrictions above, my 2019 sample of Optimal Blue data represents 1.84 million loans. This accounts for 47% of my 2019 sample of HMDA data, which holds 3.92 million loans. Comparing across the two samples, Optimal Blue and HMDA loans differ marginally across loan size, interest rate, LTV, and price. Optimal Blue data roughly matches the distribution of loan maturity, occupancy status, property type, and location within the HMDA sample. Finally, the differences across purchase and refinance mortgages appear similar for both Optimal Blue and HMDA data. For example, refinance loans are similarly larger with lower LTV's and interest rates across both samples relative to purchase mortgages. Given the preceding evidence, Optimal Blue data appears to be representative of the universe of sold mortgages as outlined by HMDA data.

4 Main Results

This section outlines the empirical strategy and reports results from OLS regressions. I find a 60% pass-through of the AMR fee on interest rates for refinance loans. I find that the remaining 40% is pass-through in the form of up-front costs. These results are robust to accounting for time-varying local economic conditions. The evidence in this section suggest that costly debt sale is entirely passed through to borrowers in the form of higher interest rates and fees.

4.1 Empirical Design

Estimating the causal effect of interest rate changes is challenging due to concerns over endogeneity with employment, asset prices, and the broader macroeconomy. Higher borrowing costs, for example, may reflect stronger demand for funds due to booming economic conditions. Furthermore, estimates may be biased to reflect strategic behavior as borrowers manipulate the timing or terms of their loans around a policy change. Finally, policies implemented during times of crisis often suffer from simultaneity bias, whereby estimates reflect the consequences of other polices offered to the same treated group.

To overcome the empirical challenges mentioned above, I exploit the FHFA announcement to identify the causal effect of an increase in the cost of debt sale. I use a difference-in-differences research design where I compare outcomes for conforming refinance mortgages (treated group) and conforming purchase mortgages (control group) before and after the FHFA's Adverse Market Refinance Fee announcement. I estimate versions of the following regression using the sample of conforming mortgages:

$$Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Refinance_i + \delta Refinance_i \times Post_t + \epsilon_{i,c,t} \qquad (1)$$

In the above specification, $Y_{i,c,t}$ represents outcomes for loan i in county c and time t. The dummy variable $Refinance_i$ takes a value of one if a loan i is a refinance loan. The indicator variable $Post_t$ equals one if an outcome is observed after the timing of the FHFA announcement (August 12, 2020). $X_{i,c,t}$ is a vector of loan characteristics, including interest rate, CLTV, loan size, FICO score, DTI ratio, loan price, and indicators for a 15-year loan, 20-year loan, primary residence, and single-family home. When the dependent variable is perfectly collinear with any of the control variables, the control variable is removed.

I include county-by-time fixed effects $\alpha_{c,t}$ to account for time-varying local economic conditions. This restricts identifying variation to changes over time across refinance and purchase mortgages originated at the same time and county. Hence, I am controlling for common shocks to a local economy that should affect demand for mortgage debt. These common shocks may include local employment, house prices, prices, and so on. Standard errors are clustered at the county-level to account for within-county residual correlation.

The coefficient of interest is δ, which measures the differential change in outcomes for refinance mortgages relative to purchase mortgages following the AMR fee announcement. The choice to announce the AMR fee on any random day should generate quasi-experimental variation across two discrete periods within a narrow window of time. Hence, the key identifying assumption is that, absent the announcement, outcomes across the refinance and purchase mortgages would have evolved in parallel within a narrow window of observation.

4.2 Identification

The key identifying assumption in the specification above would be violated if refinance mortgages systematically differed from purchase mortgages in the period following the announcement for reasons unrelated to the announcement. The nature of the shock assuages these concerns. First, the FHFA's choice of the AMR fee should be uncorrelated with the identifying variation after controlling for common shocks to the local economy. While the choice of implementing the AMR fee might be related to rising uncertainty, that uncertainty should affect both purchase and refinance mortgages equally. Therefore, restricting identifying variation to a given period and county using county-by-time fixed effects should account for correlation between local economic conditions and the choice of the AMR fee.

Second, the announcement was unanticipated by market participants. Hence, borrowers were unlikely to strategically shift their borrowing to the period prior to the policy's announcement. Lenders, as evidenced by trade group objections, were also likely unaware of the policy change prior to the announcement. Therefore, lenders likely did not adjust their supply of credit around the shock.

Finally, the timing of the announcement and the research design should assuage concerns over simultaneity bias. In particular, forbearance polices, foreclosure suspensions, and repayment flexibility were policies that were offered for both purchase and refinance mortgages. Hence, controlling for common trends across both purchase and refinance mortgages should account for any changes in outcomes irrelevant to the AMR fee but related to other simultaneous policies.

Ultimately, the parallel trends assumption cannot be formally tested. However, by studying variation across refinance and purchase mortgages over time, I can assess the feasibility of the assumption. In particular, I estimate a model that replaces the interaction term $Refinance_i \times Post_t$ with a full set of treated-by-period interactions. I estimate specifications of the following form:

$$Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Refinance_i + \sum_{s=-N}^{N} \delta_s Refinance_i \times \mathbb{1}_{t=s} + \epsilon_{i,c,t} \quad (2)$$

Under the key identifying assumption, there should be no differential effect across refinance and purchase mortgages prior to the FHFA announcement. Hence, the coefficient δ_s should equal zero for periods preceding the AMR fee announcement. A lack of pre-trends followed by a sharp break in trend precisely at the time of the announcement would support the parallel trends assumption. In particular, it would indicate that refinance mortgages would not have experienced a differential change in outcomes, if not for the AMR fee announcement.

4.3 Effect of FHFA Announcement on Interest Rates

Table 2 presents estimates of regression Equation (1) using the mortgage interest rate as the dependent variable. Columns (1) through (5) report estimates using the Optimal Blue sample of conforming loans originated 13 days around the time of the FHFA announcement. The estimated coefficients measure the differential change in the mortgage interest rate over time across refinancing status.

In Column (1), absent any fixed effects, I estimate a 7.44 basis point increase in interest rates, statistically indistinguishable from zero at the 1% level. Relative to an average interest rate of 2.85 for refinance mortgages, this represents a 2.6% increase in rates. To assess the degree of pass-through, I use an industry rule-of-thumb where a one percent up-front payment of loan size equals a 25 basis point reduction in interest rates. Therefore a 50 basis point increase would translate to a 12.5 basis point increase in interest rates. Based on this industry standard, the FHFA announcement led to an immediate 60% pass-through to rates.

In Column (2), I restrict identification to within-county variation. Here, I am effectively comparing changes in interest rates over time across refinance status for loans within the same county. This accounts for slow-moving local economic conditions, unlikely to vary much within a 26-day window. In Column (3), I restrict identification to within-day variation. Here, I compare outcomes across refinance status for loans originated on the same day. This specification controls for daily macroeconomic changes that would affect all counties equally. Finally, in Column (4), I account for both macroeconomic changes and time-invariant local economic conditions separately. Across all columns, point estimates vary between 7.45 basis points and 7.59 basis points, significant at the 1% level.

I account for within-county and within-time fixed effects jointly in Column (5), thereby controlling for time-varying local economic conditions. Here, I am effectively comparing interest rates across refinancing status for loans originated on the same day within the same county over time. This represents my most restrictive specification to account for spurious factors affecting the local demand for mortgage credit. Even with this more demanding specification, I estimate a treatment effect of 7.70 basis points, statistically indistinguishable from zero at the 1% level. Taken together, the FHFA announcement brought lenders to respond by increasing interest rates, but these rates did not lead to full pass-through of costly debt sale to consumers. The next subsection will explore whether upfront costs in the form of discount points can account for the remaining 40% of full pass-through.

4.4 Effect of FHFA Announcement on Points

When selecting across a menu of contracts, borrowers have the opportunity to lower their future stream of monthly payments by paying down a percent of their loan immediately, also known as purchasing discount points. Each discount point costs one percentage point of the original loan amount and, as an industry rule-of-thumb, reduces the interest rate by 25 basis points. A borrower intending to pay a mortgage for the full life of the loan would benefit most to purchase discount points. In contrast, financially constrained borrowers and borrowers intending to prepay their mortgage in the near future benefit least from purchasing discount points. In addition, Agarwal et al. (2017) find that borrowers may sub-optimally purchase discount points, likely due to limited financial sophistication.

In the context of the FHFA announcement, interest rates may fail to reflect the AMR fee completely because borrowers choose to pay down a potentially higher interest rate using discount points. In order to test this, I estimate Equation (1) using the variable $Price_{i,c,t}$ as the outcome of interest. Here, $Price_{i,c,t}$ represents the price of the loan, measuring 100 at par value, prior to the purchase of any discount points. If the borrower purchases one discount point to receive a 25 basis point interest rate reduction, then the price of the loan falls to 99. I hypothesize that my estimate of δ in Equation (1) will be less than one, implying that loan prices fell following the FHFA announcement. Borrowers facing a potentially higher interest rate due to the AMR fee likely choose to select into a lower interest rate by purchasing discount points and therefore bringing the loan price down.

Table 3 presents estimates of regression Equation (1) using the mortgage price as the dependent variable. The estimated coefficients measure the differential change in up front costs across refinance status. Across Columns (1) to (5), I estimate that the price of refinance mortgages fell by about 19.8 basis points, statistically indistinguishable from zero at the 1% level. Given the relationship between mortgage price and interest rates, a 0.20 reduction in price is due to the borrower paying down the equivalent of 5 basis points in their interest rate. This would imply a pass-through of approximately 40%. The combined evidence from Table 2 and Table 3 suggests that lenders fully pass-through costly debt sale, 60% of which occurs through higher interest rates and 40% through higher upfront costs.

4.5 Assessing Pre-Trends

Figure 2 plots estimates of the regression Equation (2) using 95% confidence intervals. The estimates represent differential trends in outcomes across refinancing status for conforming loans around the time of the FHFHA announcement. Panel A plots estimates where the outcome variable is the loan interest rate. Panel B plots estimates where the outcome variable is the loan price. I normalize estimates to the day preceding the announcement and plot a 26-day window around this baseline date. The specification here is analogous to Column (5) of Table 2.

Under the key identifying assumption that outcomes for refinance loans would have evolved in parallel to purchase mortgages if not for the FHFA announcement, there should be no significant difference between the two loan types across the pre-period. Indeed, the figure shows that the treatment and control groups evolved roughly in parallel prior to the FHFA announcement. Refinance loans, however, experienced a stark increase in interest rates relative to purchase mortgages immediately following the FHFA announcement. In addition, refinance loans experienced a sharp decline in the price of the loan, indicating that borrowers increasingly purchased discount points to further reduce their interest rate.

5 Mechanism

In this section, I explore the mechanism driving my main results. I find that borrowers fail to reduce their loan size in response to the higher cost of financing. Furthermore, borrowers reduce their propensity to refinance by 4%. Finally, I find that my results fail to correlate with local economic conditions, such as county-level average loan terms, borrower characteristics, and market structure. Nonetheless, within highly concentrated markets, market share appears to predict pass-through, whereby high market share lenders pass through excess of 100% and low market share lenders pass through well below 100%.

5.1 Intensive and Extensive Margin

In this subsection, I explore the response of the intensive and extensive margins to the AMR Fee announcement. In particular, I study the degree to which loan size and refinancing activity change following the announcement.

Table 4 Column (1) presents estimates of regression Equation (1) using the log of loan amount as the dependent variable. The estimated coefficient measures the change in log of loan size across refinance status following the FHFA announcement. Key to interpreting this result, I include interest rate and loan price as control variables. While lenders increase the overall cost of borrowing following the AMR fee, borrowers may choose how to distribute this cost across various terms of the mortgage contract, including its loan size. Previously I demonstrated that borrowers chose to pay up-front costs in order to reduce the complete pass-through of the AMR fee to the interest rate. In Table 4 Column (1), by controlling for interest rate and loan price, I can assess how borrowers chose to reduce their loan size in order to further reduce the pass-through of the AMR fee to the overall cost of borrowing.

In Column (1), I find that loan size is economically and statistically indistinguishable from zero. This result is similar to previous papers that find that loan amounts are relatively inelastic to changes in the interest rate (Bhutta and Ringo, 2021; DeFusco and Paciorek, 2017; Fuster and Zafar, 2021).

To measure the extensive margin, I modify regression Equation (1) by using the sample of conforming and non-conforming loans. Here, I assume that differential variation among refinance mortgages relative to purchase mortgages is uniform across conforming and non-conforming mortgages. Accounting for common shocks across conforming and non-conforming mortgages should restrict identification to exclusively the quasi-experimental variation associated with the FHFA announcement. To test this, I run OLS regressions and estimate a difference-in-differences regression of refinance propensity:

$$Refinance_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Conforming_i + \delta Conforming_i \times Post_t + \epsilon_{it} \tag{3}$$

The outcome variable $Refinance_i$ equals one for refinance loans and zero otherwise for loan i in county c and time t. The variable $Conforming_i$ is an indicator equal to one for conforming mortgages and zero otherwise. The coefficient of interest is δ, which measures the relative change in refinancing propensity for conforming loans relative to non-conforming mortgages following the FHFA announcement. I identify δ using variation in outcomes over time across conforming and non-conforming status, controlling for systematic time-varying shocks common to all loans. In other words, I am comparing the refinancing likelihood over time of two loans, conforming and non-conforming, originated in the same county and day.

The underlying assumption is that refinancing propensity would have evolved in parallel absent the FHFA announcement. I will assess the feasibility of this assumption by estimating changes in refinancing propensity over time. To do so, I estimate specifications of the following form:

$$Refinance_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Conforming_i + \sum_{s=-N}^{N} \delta_s Conforming_i \times \mathbb{1}_{t=s} + \epsilon_{i,c,t} \tag{4}$$

Table 4 Column (2) presents estimates of regression Equation (3) using an indicator for refinancing as the dependent variable. I find that relative refinancing propensity drops by 4.4%. Given that a 50 basis point fee should translate to a 12.5 basis point interest rate increase, this translates to an extensive margin semi-elasticity of interest rates equal to 35. A muted response on the intensive margin and a large extensive margin is similar to results from Bhutta and Ringo (2021).

To validate the research design, I employ a dynamic difference-in-differences specification. Figure 3 plots estimates of the regression Equation (4) using 95% confidence intervals. The estimates represent differential trends in relative refinancing propensity across conforming status around the time of the FHFHA announcement. The specification here is analogous to Column (2) of Table 4. As before, I find no differential trends in refinancing propensity prior to the announcement of the AMR fee. Following its announcement, I see a persistent decline in relative refinancing activity for the full period following the FHFA announcement.

5.2 Local Economic Conditions

In this subsection, I examine the degree to which factors related to loan terms, borrower characteristics, and the local economy explain the limited pass-through that I identify in my main results. Studying the interaction between borrower characteristics and the pass-through of costly debt sale faces two empirical challenges. First, Optimal Blue does not observe borrower balance sheets prior to refinancing. Thus, I cannot assess, for example, how a borrower's LTV on their *current* outstanding mortgage affects their interest rate demanded for new refinance mortgages. Second, even if this data was available, a change in the costs of refinancing will affect selection into the loan pool across borrower type. For example, if low LTV borrowers choose not to refinance when interest rates rise, then tests of heterogeneity across LTV would be biased down due to selection into the borrower pool. To overcome these challenges, I compare outcomes across average pre-period characteristics at the county-level. Since all localities participate in the mortgage market with varying intensity, pre-treatment geographic characteristics allow me to identify heterogeneity in treatment effects without biasing my estimates due to selection. I estimate versions of the following regression using the sample of conforming mortgages:

$$\begin{aligned} Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Refinance_i + \delta Refinance_i \times Post_t \\ + \vartheta High_c \times Refinance_i \times Post_t + \epsilon_{i,c,t} \end{aligned} \tag{5}$$

In the specification above, $High_c$ equals one if the pre-period average characteristic for county c is above median of county averages, weighted by observations. The coefficient δ identifies the effect of the FHFA announcement on conforming refinance mortgages across all counties. The coefficient ϑ identifies the differential effect for loans originated in above-median counties. Key to the interpretation of my results, ϑ identifies a treatment effect conditional on average characteristics, not the causal effect of those characteristics themselves. Nevertheless, these results are important to learn how local characteristics are correlated with pass-through when the funding costs increase exogenously.

In order to measure the degree of pass-through, my outcome variable will be measured in the following manner:

$$\overline{implied\ rate} = rate + 0.25 \times points \tag{6}$$

Here the outcome variable $\overline{implied\ rate}$ represents an implied rate had the borrower not purchased any discount points. Pass-through, therefore, will be compared to a 12.5 basis point change in the implied rate. This is derived from translated a 50 basis point fee into a pure interest rate change using the industry rule-of-thumb.

Table 5 presents estimates of ϑ from the regression Equation (5). Here, I show the degree to which refinance loans experienced differential changes in their implied interest rates following the FHFA announcement across loan characteristics. The $High_c$ variable equals one if pre-period geographic-level average loan characteristics are above median. In particular, each column respectively estimates the differential effect of above median three-digit-zip-level average loan age, FICO score, LTV, and DTI for non-delinquent GSE loans as of August 2020 originated prior to the year of the announcement. I adjust initial LTV using changes in loan balance and FHFA zip-level HPI data. I adjust debt-to-income using annual zip-level income data provided by the IRS dating back to 2005. Loans originated prior to this point use 2005 as their baseline income. Across all columns in Table 5, there appears no statistically significant differential variation correlated with loan characteristics.

Table 6 presents estimates using pre-period borrower and macroeconomic characteristics. Columns (1) through (6) respectively measure the share of borrowers taking a home mortgage interest deduction, the share of borrowers with a coupon below current market rates, the employment-to-population ratio, above high school education attainment, income, and the median top-tier home value. For Column (1), I measure how many borrowers hold outstanding current loans as of August 2020. I use 2019 IRS tax data on mortgage interest deductions to determine the share of current borrowers taking a deduction. For Column (2), I similarly use the GSE data to measure outstanding current loans as of August 2020. I use the two-week pre-period average interest rate to measure the share of current borrowers with pre-treatment interest rates below the average interest rate. Once again across all columns in Table 6, I find no differential pass-through of costly debt sale correlated with borrower characteristics.

Finally, Table 7 presents estimates using pre-period characteristics related to market structure. Columns (1) through (6) respectively define $High_c$ using the following characteristics at the county level: lender concentration defined by county-level HHI, the share of mortgages sold after origination, the share of loans originated by non-banks, the weighted average of how many counties a lender operates in, the loan application to bank employee ratio, and the market share of fintech firms. As before, Table 7 shows no differential effect on counties with above-median characteristics. One exception to this is Column (2), where above-median non-bank exposure correlates with some differential effect. However, both above- and below-median non-bank exposure nevertheless still correlate with near 100% pass-through.

Taken together, the preceding evidence suggests that there is little to no variation across key loan, borrower, and market characteristics. This is surprising given that previous research has found these characteristics critical to determining refinancing behavior of borrowers and pass-through of funding costs.

5.3 Lender Market Share

The AMR fee announcement led to an immediate increase in the cost of lending for borrowers and appears to be born uniformly across various loan, borrower, and regional characteristics. Naturally, an extension would further explore how lender characteristics interact with pass-through. Indeed, significant research following the 2008 financial crisis has argued for the importance of institution-specific factors affecting refinancing and modification propensity.

With this subsection, I explore one such lender-level feature, namely market share. Optimal Blue includes an indicator variable that classifies the level of origination volume by a given lender in the previous month. This indicator variable equals one if the lender was in the top 10% of lenders, two if the lender was in the top 10-25%, three if the lender was in the top 25-50%, and four if the lender was in the bottom 50% of lenders. While the lender identifier is anonymized, the market share indicator variable should be sufficient for the purposes of measuring how lender market power correlates with the pass-through of costly debt sale.

Previous work has argued that market power correlates with less sensitivity to changes in funding costs (Scharfstein and Sunderam, 2017; Wang et al., 2022). Here, monopolists will reduce mark ups when interest rates rise or extract monopoly rents when interest rates fall. Estimating the interaction of market power and pass-through is empirically challenging given that the cost of lending changes due to monetary policy, which simultaneously affects lender balance sheets, deposit markets, and access to external financing.

To overcome the empirical challenges mentioned above, I estimate versions of the following regression using the sample of conforming mortgages:

$$
\begin{aligned}
Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta Refinance_i + \delta Refinance_i \times Post_t + \\
\sum_{j=2}^{4} \vartheta_j Percentile_j \times Refinance_i \times Post_t + \epsilon_{i,c,t}
\end{aligned} \tag{7}
$$

In the above specification, $Y_{i,c,t}$ represents outcomes for loan i in county c and time t. The dummy variable $Refinance_i$ takes a value of one if a loan i is a refinance loan. The indicator variable $Post_t$ equals one if an outcome is observed after the timing of the FHFA announcement. The $Percentile_j$ dummy takes a value of one if a loan is originated by a lender in the j^{th} percentile of loan volume. Percentiles are defined in the following order: 0-10%, 10-25%, 25-50%, and 50-100%. The omitted group will be the 0-10% category.

By including county-by-time fixed effects $\alpha_{c,t}$, I restrict identifying variation to differential changes over time across refinance and purchase mortgages originated at the same time and county. Hence, I am effectively comparing how outcomes vary across lender market share within the same county and on the same day. Standard errors are clustered at the county-level to account for within-county residual correlation.

Table 8 presents estimates of regression Equation (7) using the implied mortgage interest rate as the dependent variable, as defined by Equation (6). Columns (1) through (3) report estimates using the sample of conforming loans originated 13 days around the time of the announcement. The estimated coefficients measure the differential change in the implied interest rate over time, across refinancing status and lender market share.

In Column (1), I estimate that the top 10% of lenders increased their implied rate by 13.6 basis points, statistically significant at the 1% level. As a percent of the 12.5 basis point benchmark for full pass-through, this represents 109% pass-through of the AMR fee to borrowers. In contrast, lenders outside the top percentile passed through less than 100% of the AMR fee to borrowers, ranging between a pass-through of 85% to 90%. These estimates are statistically significant at the 1% level. This result runs counter to previous research documenting weaker correlation between market power and pass-through. In contrast, I find that top lenders excessively pass costs downstream to borrowers and that weaker lenders are less sensitive.

Importantly, these results seem to contradict the results from the previous subsection where I explored how market concentration interacts with pass-through. The underlying assumption is that market concentration should proxy for the lending behavior of market participants. Namely, concentrated markets should reflect the behavior of lenders with market power whereas competitive markets should reflect the behavior of lenders without market power. In order to disentangle these conflicting results, I estimate regression Equation (7) using restricted samples of loans in low concentration counties and high concentration counties separately.

Table 8 Columns (2) and (3) present results for low and high concentration counties, respectively. In Column (2), I find that the differential effect of lender market share is statistically insignificant across all percentiles of lender market share. This implies that for low concentration counties, namely counties with high competition, the pass-through of the AMR fee is uniform across lenders.

In Column (3), I find the top 10% of lenders increase the implied interest rate by 14.8 basis points, statistically significant at the 1% level. This represents a pass-through of almost 120%. In contrast, lenders in the 10-25% and 25-50% percentiles have a pass-through of 90% and 85%, respectively. More stark still, the bottom 50% of lenders pass through only 60% of the AMR fee to borrowers. These estimates are significant at the 1% level.

Taken together the preceding results suggest that market power correlates with pass-through, but only in highly concentrated markets. In such low competition markets, high market share lenders pass through well excess of 100% of costly debt sale to borrowers. In contrast, low market share lenders pass through well below the full cost to borrowers.

6 Robustness

In this section, I assess the robustness of my main results. First, I show that my results are unchanged when accounting for systematic changes across conforming status for refinance loans. Second, I find that following the FHFA's reversal on the AMR policy, previously treated loans failed to experience an increase in interest rates.

6.1 Triple Difference

As mentioned above, I cannot formally test the parallel trends assumption as the counterfactual inherently cannot be known. Nevertheless, I can approximate systematic changes in refinance loans following treatment by controlling for common trends across treated and control units over time. However, this may still leave residual variation among refinance loans that might spuriously bias estimates of the treatment effect.

To resolve this empirical challenge, I turn to outcomes for non-conforming mortgages. Suppose that any systematic differential variation among refinance mortgages relative to purchase mortgages was uniform across conforming and non-conforming mortgages. Put differently, assume that the identifying variation that may be correlated with trends in refinance mortgages more generally is shared across both conforming and non-conforming refinance mortgages. With this assumption in mind, accounting for common shocks across conforming and non-conforming refinance mortgages should restrict identification to exclusively the quasi-experimental variation associated with the FHFA announcement.

To test this, I run OLS regressions and estimate a difference-in-difference-in-differences regression of loan outcomes:

$$
\begin{aligned}
Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \beta_1 Refinance_i + \beta_2 Conforming_i + \beta_3 Refinance_i \times Conforming_i \\
+\delta_1 Refinance_i \times Post_t + \delta_2 Conforming_i \times Post_t \\
+\varphi Conforming_i \times Refinance_i \times Post_t + \epsilon_{it}
\end{aligned}
\tag{8}
$$

The variable $Conforming_i$ is an indicator equal to one for conforming mortgages and zero otherwise. The coefficient on $Refinance_i \times Post_t$ no longer identifies the causal effect of the FHFA announcement. Instead, δ_1 captures the average change in outcomes for refinance loans across conforming and non-conforming mortgages following the FHFA announcement. The coefficient of interest is φ, which measures the differential change in outcomes for conforming refinance mortgages relative to both conforming purchase mortgages and non-conforming refinance mortgages following the FHFA announcement. I identify φ using variation in outcomes over time across refinance and purchase mortgages, controlling for systematic time-varying shocks to refinance and conforming loan outcomes separately.

Table 9 presents estimates of regression Equation (8) using the implied interest rate as the dependent variable. The estimated coefficients measure the differential change in implied rates across refinance status relative to non-conforming mortgages. This specification exploits variation within refinance status and within conforming status. In my fully specified model, I restrict identification to variation over time for a given county on a given day. In Column (1), I estimate that the refinance rate increased by almost 11 basis points, statistically indistinguishable from zero at the 1% level. The size of the estimate is relatively similar to that found in Table 2, indicating that systematic trends in refinance mortgages fail to explain the increase in interest rates for conforming refinance mortgages.

I further investigate whether the correlation between lender market share and pass-through appears to persist even after accounting for systematic trends in refinancing activity. In Column (2), I estimate the change in the implied rate for refinance loans using only the sample of loans originated by the top 10% of lenders, defined by loan volume. I find that high market share lenders increase the implied rate by 13.58 basis points, statistically indistinguishable from zero at the 1% level. This estimate represents a 109% pass-through of the

AMR fee.

In Column (3), I estimate the change in the implied rate using the sample of loans originated by the bottom 90% lenders, as defined by loan volume. Once again, I find an economically weak 5.31 basis point increase in the implied rate. This estimate is statistically different from zero at the 1% level and represents a pass-through of only 40%.

Taken together, these estimates reinforce the results from earlier. Namely, the FHFA announcement led to an increase in the cost of borrowing fully reflecting the higher fee. Furthermore, this increase was concentrated most on lenders with high market share, whereas low market share lenders passed only 40% of the cost of debt sale to borrowers downstream.

6.2 Placebo Test

Previous work has used high-frequency changes in federal funds futures rate around the time of FOMC announcements to proxy for shocks to market expectations over future interest rates. The underlying assumption is that within a narrow window of time, the Federal Reserve's announcement on interest rate policy is likely the only relevant news that markets respond to. These shocks then serve as an instrument for generating plausibly causal elasticities of interest rate changes. Nakamura and Steinsson (2018) argue that monetary policy announcements affect expectations over both monetary policy and economic fundamentals. Ultimately, this may violate the exclusion restriction, whereby federal funds futures rate shocks proxy for news regarding the economy more broadly and not interest rates alone.

In a similar manner, the estimates that I identify in this paper may reflect changes in how investors and lenders perceive of the riskiness of conforming refinance loans. This seems like a reasonable critique given that the FHFA announced the AMR fee in response to a concern over increasing costs and uncertainty. However, upon further reflection, it seems less plausible given the evidence presented above. First, the use of county-by-time fixed effects immediately accounts for changes in both current macroeconomic conditions as well as changes in expectation over future conditions that affect all mortgage originations. Second, if the FHFA announcement reveals new information regarding the riskiness of refinance mortgages in particular, then this should be more so reflected in non-conforming refinance loans. Since non-conforming refinance mortgages are inherently riskier and face the same economic conditions, they should experience an even stronger response to the FHFA announcement. Instead, relative to both purchase and non-conforming mortgages, I find that conforming refinance mortgages experience higher interest rates following the announcement.

Finally, to assess this concern directly, I employ a placebo test with a group of conforming refinance mortgages that were later exempt from the AMR fee. Exploiting this reversal should help assuage concerns over competing interpretations of the initial treatment effect.

It becomes increasingly difficult to explain the discrete thresholds and arbitrary timing of each effect as reflective of changes in news regarding economic fundamentals as opposed to changes in the applicability of the AMR fee.

Rates should respond to the reversal announcement in a manner reflecting the timing of the reversal and the lag between rate locks, closing on a mortgage, and ultimately selling a loan to the GSE's. In particular, since there was no meaningful loan size threshold in its initial policy, the effect immediately following the first FHFA announcement should not vary across loan size. In the period immediately following the second announcement, the treatment effect should reverse to reflect the choice to delay the AMR fee by three months. This reversal should also not vary across loan size since all conforming refinance loans experienced the initial treatment. Finally, in the period following the immediate reversal period, mortgage rates should begin to respond to the December 1^{st} deadline. This period should be long enough to reflect the lag between initial rate lock and the sale of the mortgage to the GSE's. Key to the placebo test, only in this final period should the loan size threshold matter in determining the effect of the FHFA policy on mortgage rates. Here, the revised FHFA policy should only affect loans above the $125,000 threshold.

In order to leverage this heterogeneity, I augment Equation (8) in three important ways. First, I account for the three-month delay by expanding the window of observations from 13 days around the initial announcement by an additional 90 days. Second, I replace the $Post_t$ variable with indicator variables for three discrete periods of time. Here, $Post_1$ equals one for the first 13 days during which the AMR fee was considered effective, $Post_2$ equals one for the next 13 days immediately following the FHFA reversal, and $Post_3$ equals one for the period beyond 26 days. Third, I interact each indicator variable $Refinance_i$, $Conforming_i$, and $Post_t$ with an indicator $Below_i$ for mortgages with a loan size below $125,000. I estimate regressions of the following form, whereby the fully interacted model is shortened for brevity:

$$\begin{aligned} Y_{i,c,t} = \alpha_{c,t} + X'_{i,c,t}\gamma + \sum_{j=1}^{3} \varphi_j Conforming_i \times Refinance_i \times Post_j + \\ \sum_{j=1}^{3} \theta_j Below_i \times Conforming_i \times Refinance_i \times Post_j + \epsilon_{it} \end{aligned} \tag{9}$$

In the specification above, φ_j identifies the baseline effect of the FHFA announcement on conforming refinance mortgages across each period $j = 1, 2, 3$. Furthermore, θ_j identifies the differential effect of the FHFA announcement on conforming refinance mortgages below the $125,000 threshold across each period $j = 1, 2, 3$.

The hypothesis underlying this placebo test is that the effect identified in this paper reflects changes in expectations over costs and not economic fundamentals. Hence, the coefficient θ_j should equal zero for periods $Post_1$ and $Post_2$. Furthermore, in period $Post_3$, given the lag between rate locks and loan sales, θ_3 should reflect the differential effect of the AMR fee on conforming refinance mortgages across on loan size. In particular, I hypothesize that θ_3 should be negative, reflecting the lack of treatment experienced by loans below \$125,000 as the December 1^{st} deadline draws near.

Table 10 presents estimates of regression Equation (9) using the implied interest rate as the dependent variable. Columns (1) through (3) report estimates using the Optimal Blue sample of loans originated 13 days prior to the time of the FHFA announcement and 90 days following the announcement. The estimated coefficients measure the differential change in the mortgage interest rate over time and across refinancing status, conforming status, and loan size.

In Column (1), using the sample of all lenders, I estimate a 11.44 basis point increase in the implied rate for conforming refinance mortgages in period $Post_1$, the first 13 days following the announcement. The point estimate is statistically indistinguishable from zero at the 1% level. This is similar in size to the effect estimated in Table 2. Importantly, loans below \$125,00 experience a statistically equivalent change in interest rates. This means that loan size did not correlate with the size of the treatment effect immediately following the FHFA announcement. Subsequent columns progressively add county and day fixed effects, both separately and jointly. I find no meaningful difference across estimates, both in size and statistical significance.

For $Post_2$, the 13-day period marked by the FHFA's reversal, I estimate a 2.13 basis point increase in the implied rate. This increase is statistically significant at the 1% level. Once again, loan size does not meaningfully correlate with the size of the treatment effect in this period. Ideally, the point estimate in $Post_2$, the period spanning 13 days to 26 days following the initial FHFA announcement, would be zero. However, it is possible that the cost of borrowing for conforming refinance mortgages increased over time. This does not necessarily invalidate the estimate from Table 2 under the assumption that within the first 13 days, interest rates should not vary significantly over time.

The point estimate for the period $Post_3$ further validates this assumption. Here, I estimate a 13.86 basis point increase in the implied rate, representing a 2.42 basis point increase relative to the period $Post_2$. This implies that as lenders originated loans closer to the December cutoff, interest rates reflected the new timing of the FHFA's AMR fee. This difference across periods is similar to the estimate for period $Post_1$. Hence, both in period $Post_1$ and $Post_3$, the treatment of a 50 basis point increase in fees for debt sale leads to a roughly 90% pass-through of costs.

Critical to this placebo test, only in period $Post_3$ do loans below $125,000 experience an implied rate significantly different from larger loan sizes. Here, I find that the differential effect is -19.22 basis points for these smaller loans. Adding the baseline treatment effect to the differential effect indicates that small loans in period $Post_3$ experience no increase in implied cost of borrowing. In sum, loans below $125,000 experience no differential effect in periods $Post_1$ and $Post_2$, followed by effectively no increase in implied rates in period $Post_3$. This pattern appears reflective of a treatment effect due to pass-through as opposed to information on the riskiness of treated loans. It would be difficult to argue that such a discrete pattern is reflective of news regarding the riskiness of loans below an arbitrary threshold of loan size and at intervals $Post_1, Post_2$, and $Post_3$.

To further corroborate this, I test whether the patterns in lender market share correlate with the timing of the policy. In Column (2), I estimate the change in the implied rate for refinance loans using only the sample of loans originated by lenders in the top 10% of originations. In Column (3), I estimate the change in the implied rate using the sample of loans originated by lenders in the bottom 90% of originations. Key to my placebo test, I find that the same pattern exists independent of lender type. Namely, both types of lenders experience no differential effect in pass-through across loan size until the final period, $Post_3$, as the new AMR fee policy becomes binding.

Taken together, these estimates reinforce the results from earlier. Namely, the FHFA announcement led to an increase in the cost of borrowing, however this was concentrated most on lenders with high market share.

7 Conclusion

This paper identifies the effect of costly debt sale on pass-through. Using high-frequency mortgage rate-lock data and a quasi-experimental research design, I show that when debt sale becomes costly, lenders will fully pass through higher fees downstream to borrowers. This pass-through is shared across interest rates (60%) and upfront fees (40%). This pass-through fails to correlate with geographic-level average loan terms, borrower characteristics, and market structure. Nevertheless, within highly concentrated markets, market share appears to predict significant heterogeneity in pass-through. Finally, I validate my results using robustness exercises and placebo tests, accounting for differential trends across conforming status and loan size.

The results in this paper suggest that costs in the secondary market for mortgages fully pass through to borrowers in primary markets. Furthermore, within concentrated markets, heterogeneity is key to predicting the size of pass-through. Policy makers concerned with the degree of pass-through should focus on this heterogeneity in assessing the impact of changes to the cost of supplying credit. Taken together, this paper provides novel insight into how secondary market frictions can affect household balance sheets and broader financial stability.

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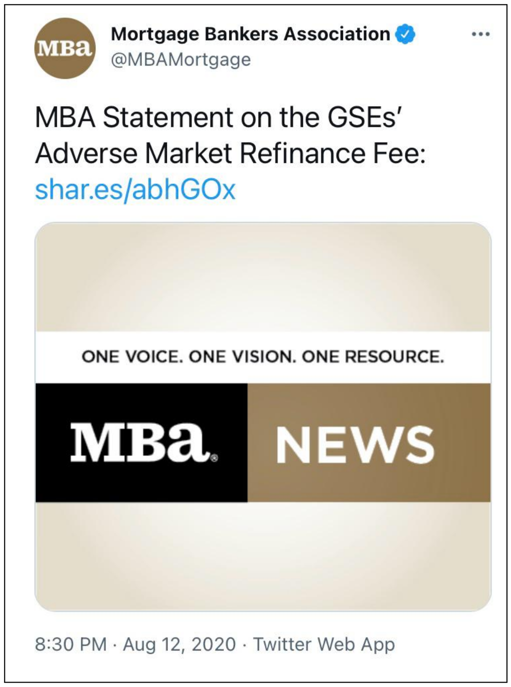
Figure 1. Timing of FHFA Announcements

Panel A: First Mention of FHFA AMR Fee

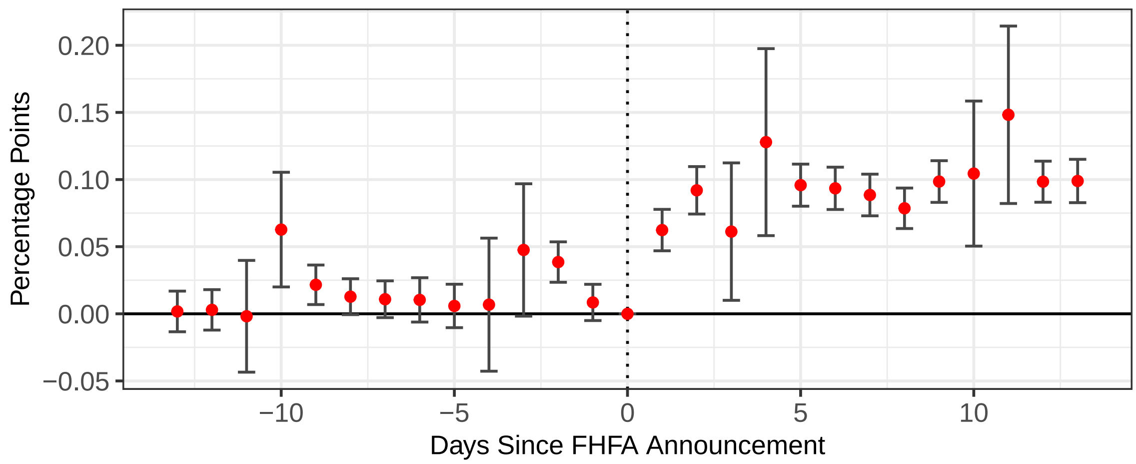


Panel B: First Mention of FHFA Reversal

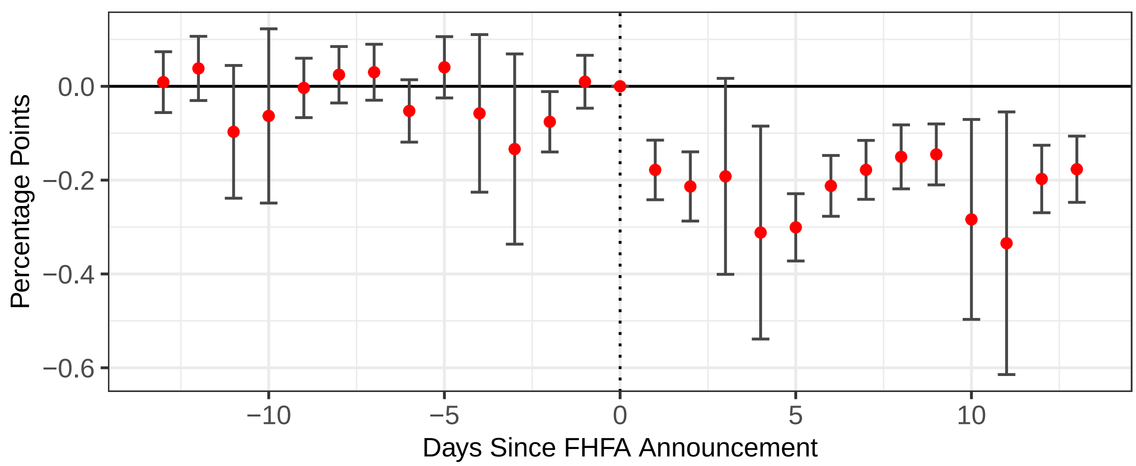
Note: This figure displays initial responses to FHFA announcements on Twitter. Panel A reports the time stamp for the Mortgage Banker's Association response to the FHFA's initial announcement regarding the AMR fee. Panel B reports the time stamp for a reponse by the CFA of Bankrate.com regarding the FHFA's reversal on the AMR fee's timing.

Figure 2. Dynamic Difference-in-Differences Estimates

Panel A: Interest Rate

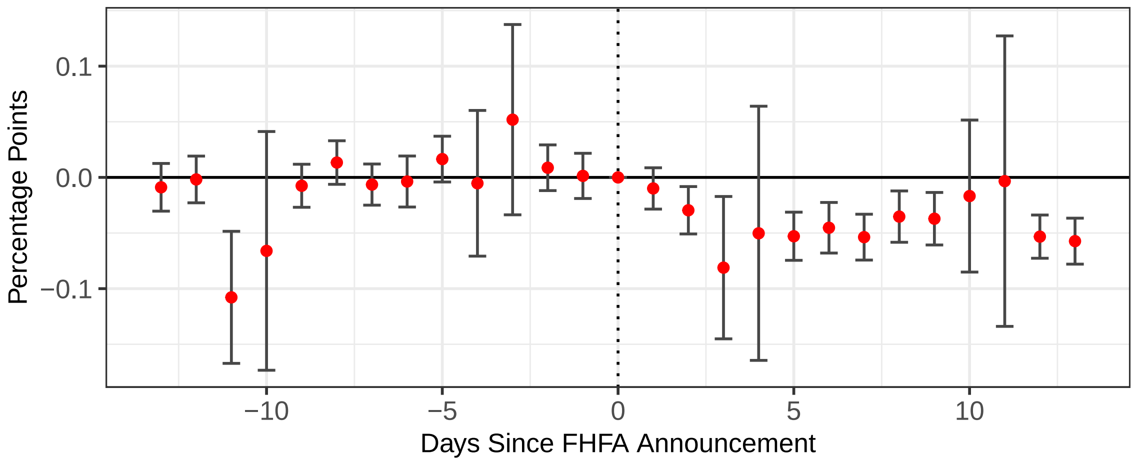


Panel B: Loan Price



Note: This figure plots estimates from a dynamic difference-in-differences specification. The *Post* indicator is replaced with indicator variables for a 13-day window around the baseline day prior to the FHFA announcement. Panel A plots estimates where the dependent variable is the interest rate. Panel B plots estimates where the dependent variable is the price of loan.

Figure 3. Dynamic Difference-in-Differences Estimates: Refinancing Propensity



Note: This figure plots estimates from a dynamic difference-in-differences specification. The *Post* indicator is replaced with indicator variables for a 13-day window around the baseline day prior to the FHFA announcement. The dependent variable equals one for refinance loans and zero otherwise.

Table 1. Summary Statistics

	Optimal Blue		HMDA	
	Mean	SD	Mean	SD
Panel A: Refinance Loans				
Loan Size (000's)	277.79	140.76	282.40	180.00
Interest Rate	4.08	0.60	4.15	0.73
CLTV	69.31	15.36	67.73	16.24
Price	99.67	1.21	99.71	0.93
DTI < 36%	0.56	0.50	0.50	0.50
15-Year Term	0.16	0.37	0.19	0.39
20-Year Term	0.06	0.23	0.07	0.26
Primary Occupancy	0.91	0.29	0.91	0.28
Single-Family	0.99	0.08	0.99	0.07
Large County	0.47	0.50	0.49	0.50
Number of Obs	784,609		1,758,230	
Panel B: Purchase Loans				
Loan Size (000's)	261.17	136.03	266.53	170.06
Interest Rate	4.26	0.55	4.24	0.79
CLTV	82.78	14.45	83.66	16.17
Price	99.75	1.09	99.90	0.73
DTI < 36%	0.51	0.50	0.46	0.50
15-Year Term	0.05	0.22	0.06	0.23
20-Year Term	0.01	0.09	0.01	0.11
Primary Occupancy	0.87	0.34	0.88	0.33
Single-Family	0.99	0.10	0.99	0.09
Large County	0.42	0.49	0.43	0.49
Number of Obs	1,054,003		2,161,044	

Note: This table reports mean and standard deviation values of loan characteristics for mortgages originated in 2019 across data collected from Optimal Blue and HMDA. Panel A reports statistics for refinance loans. Panel B reports statistics for purchase loans. Loan characteristics include loan size (in thousands of dollars), the original interest rate, the combined loan-to-value ratio, and the price of the loan. Other characteristics include indicator variables for mortgages with a debt-to-income ratio below 36%, a 15- or 20-year loan term, primary occupancy status, single-family property status, and a property within the top 100 largest counties. Mortgages are restricted to loans with a loan-to-value ratio below 150, loan size below $10 million, interest rate below 30%, and a loan term of either 15, 20, or 30 years. HMDA data is restricted to loans with a reported purchaser.

Table 2. Effect of FHFA Announcement on Mortgage Interest Rates: Difference-in-Differences Estimates

	Interest Rate				
	(1)	(2)	(3)	(4)	(5)
Constant	5.156***				
	(0.0284)				
Refinance	0.0671***	0.0644***	0.0669***	0.0642***	0.0633***
	(0.0026)	(0.0026)	(0.0026)	(0.0026)	(0.0029)
Post	0.0160***	0.0150***			
	(0.0020)	(0.0020)			
Refinance× Post	0.0744***	0.0758***	0.0745***	0.0759***	0.0770***
	(0.0027)	(0.0027)	(0.0027)	(0.0027)	(0.0025)
County		Yes		Yes	
Day			Yes	Yes	
County×Day					Yes
Observations	399,382	399,382	399,382	399,382	399,382

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the interest rate for refinance mortgages. Observations are at the loan-county-minute level. The outcome variable equals the offered interest rate on a mortgage at the time of a rate-lock. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 5 report estimates from difference-in-differences regressions that control for refinance status over time. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 3. Effect of FHFA Announcement on Mortgage Price: Difference-in-Differences Estimates

	Price				
	(1)	(2)	(3)	(4)	(5)
Constant	97.31***				
	(0.0976)				
Refinance	0.0419***	0.0593***	0.0430***	0.0600***	0.0641***
	(0.0118)	(0.0101)	(0.0119)	(0.0101)	(0.0108)
Post	-0.0440***	-0.0472***			
	(0.0076)	(0.0076)			
Refinance× Post	-0.1978***	-0.1964***	-0.1980***	-0.1966***	-0.1978***
	(0.0098)	(0.0094)	(0.0099)	(0.0095)	(0.0103)
County		Yes		Yes	
Day			Yes	Yes	
County×Day					Yes
Observations	399,382	399,382	399,382	399,382	399,382

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the price for refinance mortgages. Observations are at the loan-county-minute level. The outcome variable equals the price on a mortgage at the time of a rate-lock. Columns 1 to 5 report estimates from difference-in-differences regressions that control for refinance status over time. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 4. Effect of FHFA Announcement on Loan Amount and Refinancing: Difference-in-Differences Estimates

	Log(Loan Amount) (1)	Refinance (2)
Refinance× Post	-0.0008 (0.0033)	
Conforming× Post		-0.0442*** (0.0034)
County-Day	Yes	Yes
Observations	399,382	496,909

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on loan amount and refinancing propensity. Observations are at the loan-county-minute level. In Column 1 the outcome variable equals the log of the loan amount on a mortgage at the time of a rate-lock. In Column 2 the outcome variable equals one if the loan originated at the time of a rate-lock is a refinance loan. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). The Conforming dummy takes a value of one if a the loan is a conforming mortgage. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample in Column 1 includes only conforming mortgages. The sample in Column 2 includes conforming and non-conforming mortgage.

Table 5. Effect of FHFA Announcement on Loan Amount and Refinancing: Heterogeneity in Loan Characteristics

	Implied Rate			
	(1)	(2)	(3)	(4)
Refinance× Post	0.1218***	0.1310***	0.1274***	0.1266***
	(0.0049)	(0.0053)	(0.0051)	(0.0060)
High × Refinance × Post	0.0096	-0.0090	-0.0017	-0.0002
	(0.0070)	(0.0072)	(0.0068)	(0.0075)
County-Day	Yes	Yes	Yes	Yes
Observations	396,206	396,206	395,956	396,145

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the implied interest rate. Observations are at the loan-county-minute level. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). The High dummy takes a value of one if a loan is originated in a county with above above-median pre-period county characteristics. Columns 1 through 4 respectively measure High using three-digit-zip average loan age, FICO score, LTV, and DTI for non-delinquent GSE loans as of August 2020 originated prior to the year of the announcement. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 6. Effect of FHFA Announcement on Implied Rate: Heterogeneity in Borrower Characteristics

	Implied Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Refinance× Post	0.1263***	0.1286***	0.1280***	0.1267***	0.1302***	0.1295***
	(0.0055)	(0.0049)	(0.0053)	(0.0051)	(0.0054)	(0.0053)
High × Refinance × Post	0.0007	-0.0043	-0.0028	-0.0003	-0.0073	-0.0058
	(0.0075)	(0.0067)	(0.0073)	(0.0073)	(0.0073)	(0.0073)
County-Day	Yes	Yes	Yes	Yes	Yes	Yes
Observations	396,148	396,120	399,382	399,382	399,288	399,382

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the implied interest rate. Observations are at the loan-county-minute level. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). The High dummy takes a value of one if a loan is originated in a county with above above-median pre-period county characteristics. Columns 1 through 6 respectively measure High using the share of borrowers taking a home mortgage interest deduction, the share of borrowers with a coupon below current market rates, the employment-to-population ratio, above high school education attainment, income, and the median top-tier home value. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 7. Effect of FHFA Announcement on Implied Rate: Heterogeneity in Market Characteristics

	Implied Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
Refinance× Post	0.1240***	0.1331***	0.1270***	0.1292***	0.1266***	0.1319***
	(0.0053)	(0.0055)	(0.0053)	(0.0055)	(0.0053)	(0.0057)
High × Refinance × Post	0.0054	-0.0127*	-0.0010	-0.0052	0.0003	-0.0112
	(0.0072)	(0.0072)	(0.0073)	(0.0072)	(0.0074)	(0.0071)
County-Day	Yes	Yes	Yes	Yes	Yes	Yes
Observations	399,372	399,347	399,337	399,372	393,335	399,278

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the implied interest rate. Observations are at the loan-county-minute level. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). The High dummy takes a value of one if a loan is originated in a county with above above-median pre-period county characteristics. Columns 1 through 6 respectively measure High using county-level lender concentration (HHI), the share of mortgages sold after origination, the share of loans originated by non-banks, the weighted average of a lenders network of counties, the loan application to bank employee ratio, and the market share of fintech firms. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 8. Effect of FHFA Announcement on Implied Rate: Lender Market Power

	Implied Rate		
	All Counties	Low HHI	High HHI
	(1)	(2)	(3)
Refinance× Post	0.1360***	0.1267***	0.1484***
	(0.0048)	(0.0068)	(0.0062)
$Percentile_2$ × Refinance × Post	-0.0219**	-0.0121	-0.0354***
	(0.0088)	(0.0117)	(0.0127)
$Percentile_3$ × Refinance × Post	-0.0261***	-0.0150	-0.0416***
	(0.0094)	(0.0138)	(0.0126)
$Percentile_4$ × Refinance × Post	-0.0304**	0.0191	-0.0714***
	(0.0155)	(0.0234)	(0.0200)
County-Day	Yes	Yes	Yes
Observations	399,382	207,146	192,226

Note: This table reports difference-in-differences estimates of the effect of the FHFA announcement on the implied interest rate. Observations are at the loan-county-minute level. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). The $Percentile_j$ dummy takes a value of one if a loan is originated by a lender in the j^{th} percentile of loan volume. Percentiles are defined in the following order: 0-10%, 10-25%, 25-50%, 50-100%. The top 10% of lenders represents the omitted percentile category. Column 1 reports estimates across all counties. Column 2 reports estimates from counties with below-median HHI. Column 3 reports estimates from counties with above-median HHI. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes only conforming mortgages.

Table 9. Effect of FHFA Announcement on Implied Interest Rates: Triple-Difference Estimates

	Implied Rate		
	All Lenders	Top Lenders	Other Lenders
	(1)	(2)	(3)
Conforming	0.2281***	0.2515***	0.2019***
	(0.0062)	(0.0074)	(0.0094)
Refinance	-0.2338***	-0.2679***	-0.1616***
	(0.0074)	(0.0098)	(0.0130)
Conforming × Refinance	0.2804***	0.3011***	0.2371***
	(0.0079)	(0.0098)	(0.0134)
Conforming× Post	0.0222***	0.0065	0.0450***
	(0.0061)	(0.0082)	(0.0098)
Refinance× Post	0.0188**	0.0001	0.0557***
	(0.0089)	(0.0113)	(0.0140)
Conforming× Refinance × Post	0.1073***	0.1358***	0.0531***
	(0.0096)	(0.0121)	(0.0154)
County-Day	Yes	Yes	Yes
Observations	496,909	294,640	202,269

Note: This table reports triple-difference estimates of the effect of the FHFA announcement on the implied interest rate for refinance mortgages. Observations are at the loan-county-minute level. The outcome variable equals the implied interest rate on a mortgage at the time of a rate-lock. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 3 report estimates from triple-difference regressions that control for refinance status and conforming status over time. The Conforming dummy takes a value of one if a the loan is a conforming mortgage. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Post dummy takes a value of one if a loan is rate-locked on or after the minute of the announcement (August 12, 2020 7:30 PM EST). Column 1 reports estimates from the sample of all lenders. Column 2 reports estimates using the sample of only the top 10% of lenders by loan volume. Column 3 reports estimates using the sample of only the bottom 90% of lenders by loan volume. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 13-day window around the time of the announcement. The sample includes all conforming and non-conforming mortgages.

Table 10. Effect of FHFA Announcement on Mortgage Interest Rates: Placebo Test

	Interest Rate		
	(1)	(2)	(3)
	All Lenders	Top Lenders	Other Lenders
Conforming× Refinance $\times Post_1$	0.1144***	0.1282***	0.0877***
	(0.0081)	(0.0107)	(0.0125)
Conforming× Refinance $\times Post_2$	0.0213***	0.0475***	-0.0162
	(0.0079)	(0.0111)	(0.0124)
Conforming× Refinance $\times Post_3$	0.1386***	0.1798***	0.0687***
	(0.0055)	(0.0071)	(0.0084)
Below× Conforming× Refinance $\times Post_1$	-0.0041	0.0545	-0.0515
	(0.0359)	(0.0430)	(0.0663)
Below× Conforming× Refinance $\times Post_2$	0.0199	0.0609	0.0144
	(0.0368)	(0.0473)	(0.0694)
Below× Conforming× Refinance $\times Post_3$	-0.1922***	-0.2216***	-0.0797*
	(0.0240)	(0.0292)	(0.0416)
County-Day	Yes	Yes	Yes
Observations	2,572,562	1,504,818	1,067,744

Note: This table reports estimates of the effect of the FHFA announcement on the interest rate for conforming refinance mortgages above and below the FHFA threshold over time. Observations are at the loan-county-minute level. The outcome variable equals the implied interest rate on a mortgage at the time of a rate-lock. The outcome variable is multiplied by 100 in order to interpret coefficients as percentage point changes. Columns 1 to 3 report estimates from triple-difference regressions that control for refinance status and conforming status over time. The Conforming dummy takes a value of one if a the loan is a conforming mortgage. The Refinance dummy takes a value of one if the loan is a refinance mortgage. The Below dummy takes a value of one if a loan size is below $125,000. The indicator variable τ_j for $j = 1, 2, 3$ indicates the timing of a loans origination in the period following the FHFA announcement (August 12, 2020 7:30 PM EST). Column 1 reports estimates from the sample of all lenders. Column 2 reports estimates using the sample of only the top 10% of lenders by loan volume. Column 3 reports estimates using the sample of only the bottom 90% of lenders by loan volume. Standard errors are reported in parentheses and are clustered at the county level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively. Data covers the period of a 90-day window around the time of the announcement. The sample includes all conforming and conforming mortgages.