Price regulation and market power: Evidence from manufactured home loans

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Abstract

Regulation of loan prices is a controversial policy tool. Proponents argue that regulation protects borrowers from high prices, while detractors argue that restricting prices prevents borrowers from obtaining credit. Firm profits play a role in the effects of price regulation. If lenders are profitable enough to offer lower prices while continuing lending activity, borrowers can benefit from price regulation. In this paper, I study a 2014 price regulation in the manufactured home loan market. Manufactured homes, known colloquially as "mobile homes" or "trailers", are a source of affordable housing for approximately 17 million people in the United States. I find that loan prices fell in response to the 2014 regulation while a similar number of loans with prices near the cap were made, suggesting that borrowers benefited. This response is driven by the largest firm in the market, which made about 90% of affected loans. I show that this firm charges higher prices than other firms to observably similar borrowers, which fits with the finding that they were able to continue lending activity after the restriction was implemented. I then consider how stricter restrictions would affect borrowers and lenders. In order to conduct this counterfactual analysis, I develop and estimate a model of supply and demand for manufactured home loans, estimate borrowers' price sensitivity, and recover markups. Under progressively stricter rate restrictions, I find that borrowers initially gain surplus from lower prices but are eventually worse off due to the fall in credit supply.

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Regulation of credit markets that serve low income borrowers has proven controversial. Does restricting the interest rates that lenders are allowed to charge protect borrowers from predatory interest rates? Or does it shut borrowers out of credit markets and prevent them from borrowing to smooth consumption? Both effects are likely, and it is theoretically ambiguous which dominates.

Which effect dominates will largely depend on the ability of firms to continue their lending activity at lower prices. If the market is perfectly competitive, then lenders make no profit and will simply ration credit in response to interest rate restrictions. On the other hand, if firms make large profits, they may be able to remain profitable while offering borrowers lower rates. Interest rate regulation has the potential to have a large effect on household well-being. For example, regulating mortgage interest rates can either save a household money on their largest expense, or shut them out of homeownership.

Manufactured home loans are one financial product that has been affected by a price restriction in recent years. Factory-built manufactured homes, colloquially known as "mobile homes" or "trailers," are home to about 17 million people in the United States, whose median household income of $35,800 is substantially lower than the national median of $61,000 (IPUMS 2020). Manufactured homes make up a large share of the country's affordable housing—for comparison, 10.4 million Americans receive all types of federal rental assistance combined (CBPP 2021). Thus, the prices of manufactured home loans affect housing affordability for low-income Americans. Figure 1 is a picture of a single-wide manufactured home.

In 2014, the Consumer Financial Protection Bureau (CFPB) implemented a new rule under the Home Ownership and Equity Protection Act (HOEPA). One provision of this rule introduced additional restrictions and liabilities for very high-priced mortgages. After the HOEPA rule went into place, very few loans were made above the rate restriction. Therefore, it acted as a rate cap. Though this regulation applied to home purchase mortgages generally, most affected loans were for manufactured homes due to their relatively high rates compared with mortgages for site-built homes. The regulation applied to both types of manufactured home loans: mortgages, which are secured by the home and land, and chattel loans, which are secured by the home only.

Several factors contribute to high loan prices in the manufactured home loan market. First,

people borrowing to buy manufactured homes have relatively low incomes compared with those borrowing to buy site-built homes and thus the higher prices reflect a higher probability of default. Second, the market is highly concentrated. In the year before the HOEPA rule, nearly 40% of loans were made by one firm, which I refer to as the "dominant firm." This firm especially dominates the high-price segment of the market, making 90% of loans that would have been affected by the HOEPA rule in the year before the rule was enacted. In a 2016 American Banker article entitled "Time to End the Monopoly Over Manufactured Housing," Doug Ryan cites the lack of participation by the Government Sponsored Entities (GSEs) in the market as a reason for lack of competition in the market. Unlike other firms, he notes that the dominant firm "has no need for Fannie and Freddie since it accesses the capital markets through its parent Berkshire Hathaway." This contrasts with the site-built home mortgage market, in which many nonbank lenders originate mortgages and sell them to the GSEs. Third, it may be more difficult for borrowers to find the lowest price than in the site-built mortgage market. Many manufacturers also own lenders, as is the case for the dominant firm, which both manufactures homes and owns two lenders. Even though it is generally not legal for a dealer to refer a borrower to a lender, lenders focused on originating manufactured home loans are likely the easiest for borrowers to find, as opposed to general purpose mortgage lenders, who may make manufactured home loans but not advertise them widely.[1] In this paper, I study the effect of the HOEPA rule on borrowers and lenders using loan-level Home Mortgage Disclosure Act (HMDA) data, which provides national coverage of manufactured home loan applications and originations since 2004. HMDA distinguishes manufactured homes from site-built homes and provides detailed information on loan and borrower characteristics, such as loan amount, lender, borrower race/ethnicity, and income. Starting in 2018, HMDA includes more data such as loan term, fees, borrower age, property value, borrower debt-to-income ratio and whether the loan is a chattel loan or mortgage.

I use these data to analyze whether the dominant firm charges higher prices than other firms,

[1] This is due to loan originator compensation rules for mortgages. In the CFPB's 2014 report on manufactured housing, the loan originator rules are discussed with respect to manufactured housing dealers: "Advising consumers about a loan application and referring consumers to a creditor are [loan originator] activities, and many manufactured-housing retailers do not want to incur the cost of becoming a licensed LO. Thus, retailers report that, instead of referring a consumer to a particular creditor or two, they currently do not advise consumers about which creditors are most likely to accept their applications."

first simply controlling for observables and then developing an instrumental variables strategy exploiting the post-crisis exit of large banks from many credit markets. Concern has been raised in the press regarding the dominant firm's loan prices, such as in the 2015 *Seattle Times* article entitled "The mobile-home trap: How a Warren Buffett empire preys on the poor." If the dominant firm does charge higher prices for similar borrowers compared with other firms, offering those borrowers lower prices under an rate cap may be viable. My work is the first academic research I am aware of that studies the prices of the dominant firm compared with other firms.

I then analyze the effect of the HOEPA rule on rates, credit availability, and loan characteristics. Analyzing the changes in loan price and number of loans alone is not enough to determine the effect on borrowers. For example, it is possible that I could observe lower loan prices after the HOEPA rule but that firms compensate for lower prices by shifting towards less risky borrowers. I compare affected loans, defined as those near or above the rate limit, with unaffected loans on the basis of rates, borrower incomes, and loan amounts. I examine whether borrower and loan characteristics available as HMDA variables starting in 2018 exhibit changes at the rate limit. Since the dominant firm made about 90% of affected loans, any study of aggregate changes will largely reflect its behavior. For this reason, I separate the dominant firm's response from all other firms. To supplement this analysis, I also present evidence on the industry's reaction to the HOEPA rule in the form of investment in lobbying to change the rule.

Both OLS and IV strategies shows a substantial rate premium for the dominant firm of about 250 basis points, providing evidence that borrowers taking out loans from the dominant firm tend to pay higher prices. Furthermore, after the HOEPA rule, loan prices fell, with many loans offers at the rate limit. The number of loans stayed the same, and the rejection rate stayed on trend. I do not see evidence that the dominant firm made changes to loan amount, borrower income, fees, loan term, or down payments to compensate for the lower prices. Importantly, this finding does not hold for all other firms. For all other firms, I see post-HOEPA rule shifts towards fewer loans and towards higher income borrower and larger loans, suggesting low-income borrowers and borrowers seeking small loans may have had a harder time obtaining loans from small firms after the rule. Finally, I show that the dominant firm and industry group have lobbied to raise the

rate limit. If they had modified loan characteristics or shifted to a different type of borrowers in unobservable ways in order to keep profits constant under the HOEPA rule, it is unlikely that they would have invested resources into changing the cap.

Though these results collectively suggest that the HOEPA rule benefited borrowers, they are not informative about whether a lower rate restriction could further improve welfare. A stricter cap would have an ambiguous welfare effect—while a lower rate cap could further improve borrower welfare, the credit rationing that may occur under a stricter rate cap makes borrowers worse off. I develop and estimate a model of supply and demand in the manufactured home loan market to analyze this trade off. Using the richer 2018-2019 HMDA application data, I estimate each borrower's choice set using a random forest classifier that predicts whether each lender will offer a borrower a given loan. Using the 2018-2019 data substantially improves the accuracy of loan approval predictions as compared with earlier years when fewer borrower and loan characteristics are available. I estimate price sensitivity using an instrumental variables strategy that exploits price changes by a lender in other states as a cost shifter. I then estimate counterfactual borrower welfare, profits, and number of loan offers per borrower under rate restrictions stricter than the one currently in place.

In the counterfactual analysis, I find that borrower welfare peaks about 200 basis points below the current restriction. This optimal rate is still about 400 basis points above a typical rate offered to prime borrowers. Going so far as to restrict prices to the rates typically offered to mortgage borrowers with prime credit would result in a large loss in borrower surplus. Price restrictions unambiguously hurt firm profits. This counterfactual analysis has limits—for example, I assume that lower cost firms will be able to absorb an increase in loan volume as higher-cost firms exit the market, and I am only able to study loans below the current rate restriction. Though my analysis is restricted to loan offers below the rate restriction, peak average borrower surplus is higher than the average borrower surplus including loans at the current restriction. At the optimal rate, the riskiest borrowers may be shut out of the market, but their loss in surplus is outweighed by the gain in other borrowers' surplus due to lower prices. Alternatively, if policymakers chose to weight the consumer surplus of risky borrowers relatively heavily, then a higher rate cap would likely be

optimal.

My work contributes to two areas: the understanding of the effects of interest rate caps, and the understanding of the manufactured home loan market. The past work on interest rate caps in economics has generally found negative effects on borrowers. Cuesta and Sepulveda (2021) study an interest rate cap on consumer loans in Chile and conclude that it lowered consumer surplus but find that the cap could have increased surplus in a more concentrated market. Zinman (2010) finds that a cap on payday loans in Oregon lowered the number of loans and raised financial distress. Melzer and Shroeder (2017) find that auto dealer-lenders respond to usury limits by raising the prices of cars and substituting loan amount for interest rate. Rigbi (2013) finds that an increase in the interest rate cap for online consumer loans increased loan supply, without any change in loan amounts or default.

Though the work on interest rate caps has historically argued that interest rate caps harm borrowers, in recent years there have been more studies showing the opposite. Cox, Liu and Morrison (2021) study Small Business Administration loans and find in their counterfactual analysis that a "modest decrease" in the interest rate cap could increase borrower surplus. They also observe bunching at the interest rate cap and develop an estimator that uses the bunching to measure market power. Though the policies we study are similar, an advantage of my setting is that I observe data in years before the HOEPA rule was imposed, so I can directly observe the changes in number of loans, loan characteristics and borrower characteristics concurrent with the policy change. Fekrazad (2020) studies a tightening of the interest rate cap on payday loans in Rhode Island and finds that credit was not rationed, and borrower surplus increased due to lower prices. Several papers show that the benefits of lowered fees from the CARD Act of 2014 outweighed any credit rationing for borrowers, including Agarwal et al (2015), Nelson (2020) and Galenianos and Gavazza (2021).

I am aware of two pieces of work studying the 2014 HOEPA rule. Benzarti (2021) studies the wider mortgage market, and shows that loans near the HOEPA rate limit had lower prices after the regulation without a fall in loan supply. He argues that this shows that lenders want to avoid consumer protections. The CFPB (2021)'s policy paper documents the fall in loan prices after

HOEPA in the manufactured home market and the apparent lack of decrease in lending. My paper adds to these by analyzing the dominant firm's pricing compared to other firms, studying how different types of firms responded to the rule, and developing a model that allows me to analyze the effects of other potential regulations. Understanding the manufactured home loan industry is crucial to interpreting the effects of the HOEPA rule. Based on my analysis of the full Home Mortgage Disclosure Act (HMDA) dataset, including all purchase mortgages and refinances, 86% of loans made in 2013 that were over the future regulatory limit were manufactured home loans, and 76% of those loans were made by the dominant firm. Thus, even considering the mortgage market as a whole, most affected loans were manufactured home loans made by the dominant firm, so aggregate trends will largely reflect its actions.

The paper proceeds as follows. In Section 1, I further describe the manufactured home and manufactured home loan industries. In Section 2, I discuss the Home Mortgage Disclosure Act data and my source of foreclosure data. In Section 3, I study the dominant firm's pricing compared to other firms in preparation for Section 4, which studies the effect of the HOEPA rule on manufactured home loan for the dominant firm and all other firms. In Section 5, I develop a model of supply and demand in the manufactured home industry. In Section 6, I estimate the model and present the results. and use the model to estimate welfare under counterfactual rate spread restrictions. Section 7 concludes.

1 Manufactured Homes

Manufactured homes, also known colloquially as "mobile homes" or "trailers" are homes manufactured in a factory and then brought on a truck to the housing site rather than built on site like most single family homes in the United States.[2] The term "mobile homes" is somewhat of a misnomer because the homes are not particularly mobile, as most are never moved from their first location. After a home is brought to the land and set down, moving it generally costs the homeowner between $5000 and $10,000 and available data suggests that at least 80% of manufactured homes have not been moved from their first placement.[3] Thus, manufactured homes are not

[2] I refer to non-manufactured single family homes as "site-built."
[3] See https://prosperitynow.org/resources/facts-about-manufactured-housing-2019.

designed as a means to promote a mobile lifestyle like a recreational vehicle (RV) or "tiny home," but are a less expensive alternative to site built homes. Manufactured homes are over-represented at the less expensive end of the housing market. While only about 3% of first-lien purchase loans in 2018 were for manufactured homes as calculated from the Home Mortgage Disclosure Act data, for loans of less than $100,000, 8% were for manufactured homes and for loans of less than $30,000, 20% were for manufactured homes.[4] The average home-only loan in 2018 was for $64,000 and the average land and home loan was for $133,000, compared with the mean loan amount for site-built homes of $277,000. Schmitz (2020) explains first how, historically, the association representing large builders of site-built homes has lobbied for regulations to make manufactured homes less easily substituted for site-built homes. For example, many municipalities have strong restrictions or outright zoning bans on manufactured homes. He also details how within the manufactured housing industry, the Manufactured Housing Institute (an association of the largest producers) has blocked lower priced competitors.

The 2018 American Community Survey (ACS) shows that manufactured homes are home to approximately 17 million people and are most common in the rural South, where nearly 20% of households live manufactured homes (IPUMS 2020).[5] Figure 2 shows the proportion of households living in manufactured homes by county. It shows that in many counties in the rural South, over 30% of housing units are manufactured homes. Though the homes are sometimes in manufactured home communities or "trailer parks," where residents generally own their home but rent the land that the home is placed on, they are more commonly located on resident-owned land.[6] The Census Bureau's 2019 Manufactured Housing Survey shows that 65% of new homes were placed outside of manufactured home communities on resident-owned land. (Census 2020).

[4] In order to make an "apples to apples" comparison here, I only include manufactured home loans that were for both the home and land in these calculations.

[5] I calculated these figures from the ACS microdata, using the household and person weights

[6] For more work in economics on manufactured home communities, see Becker and Lemus (2019) and Becker and Rickert (2019), who study the how rents are set in these communities and Becker and Yea (2015), who study the asset value of the communities.

1.1 Manufactured home loan market

Manufactured home loans tend to have high prices relative to typical mortgages. About half of manufactured home loans are "chattel loans" rather than real estate loans, meaning that they are only secured by the home and not the land, even if the borrower also owns the land.[7] Chattel loans generally have higher prices than mortgages, even accounting for credit scores (CFPB 2021). CFPB (2021) also discusses reasons that landowners may choose chattel loans despite eligibility for mortgages, which tend to be cheaper. The reasons they discuss include that the borrowers may prefer not to risk their land in case of foreclosure, and that manufactured homes are titled as personal property by default in many states and borrowers may not be aware that they are eligible for a mortgage.

As with site-built home mortgages, the share of manufactured home loans originated by large banks nationally has fallen and the share of manufactured home loans originated by non-banks has risen since the 2008 financial crisis. Figure 3 shows market share over time for three different firm types. I divide depository institutions into the large banks subject to post-crisis regulation like stress testing and group all others into the "Other Depository" category.[8] [9] In other markets, this trend has been partly attributed to regulation. For example, Buchak et al. (2018) find that in the site-built home mortgage market, post-crisis regulation accounts for 60% of the growth of non-banks. In the small business lending market, Cortes et al. (2020) find that banks who performed worse on their stress tests cut back on risky small business lending more. I conduct a similar exercise, using their preferred measure of stress test exposure, for manufactured home loans. The results are in Figure 4, which shows that, consistent with the findings of these other papers, the correlation between performing well on stress tests and growth in manufactured home

[7] For other work in economics on manufactured home lending, see Capozza, Isrealsen and Thompson (2005), who study the chattel lending market and find that reposessed homes sell for about half of their sale price on average, and that homes that are more common tend to do better in the resale market. Canner, Passmore and Laderman (1999) discuss manufactured home lenders in the context of the rise in subprime specialty lenders and the associated increase in the denial rates reported in HMDA.

[8] Institutions included in "Other Depository" include small/community banks, credit unions and thrifts.

[9] The asset threshold for stress testing has changed somewhat over time. If a bank has ever been subject to the Comprehensive Capital Annual Review (CCAR) stress tests, I include them in the "Stress Tested" category. Because the banks who were not always included have a small market share in manufactured home lending, this decision changes the graph very little.

loans is positive. Compared with the markets studied in previous work, the nonbank sector of the manufactured home loan market is highly concentrated. From 2008 until 2016, the largest nonbank in the market, which is also the largest firm overall in the market, made more loans than all other nonbanks combined. Thus, the rise of nonbanks in manufactured home lending raises concerns about concentration and market power.

Figure 5 shows the rise of the dominant firm in the market to make a majority of manufactured home loans in the South, as calculated from the Home Mortgage Disclosure Act (HMDA) data.[10] By 2013, they had a national market share of nearly 40%. I focus on this firm as the dominant firm in the paper because it is many times larger than the next firm: in all years, the next largest firm had less than a 10% market share. The market is also concentrated according to overall measures of concentration. In 2018, 35% of manufactured home borrowers lived in counties where the Hirschman-Herfindahl Index (HHI) is above the "highly concentrated" threshold of 2500 used by the U.S. Department of Justice.

2 Data

My data source for mortgage originations is the Home Mortgage Disclosure Act (HMDA) data, which began identifying manufactured homes in 2004. Throughout my analysis, I study first-lien purchase loans for owner-occupied homes. Under HMDA, application-level data must be reported for all loans by all but very small loan originators. The thresholds for reporting vary slightly from year to year as they are adjusted for inflation, but for example in 2010, the guidelines roughly translate to requiring reporting from depository institutions with more than $39 million in assets or non-depository loan originators who originated more than 100 loans in the year.[11]

Prices are reported as rates spread, which is the amount above the "Average Prime Offer Rate" (APOR). The rates used in the price calculation are the Annual Percentage Rates (APR), which include fees not included in the interest rate alone. The prime offer rate is the APR regulators would expect a borrower with prime credit to be given for a loan based on its origination date and

[10] The dominant firm owns two manufactured home lenders, 21st Mortgage and Vanderbilt Mortgage and Finance, whose loans I combine for these calculations.

[11] For full details on HMDA reporting thresholds, see the Federal Financial Institutions Examination Council's guide here: https://www.ffiec.gov/hmda/guide.htm.

term. The rate was reported if more than 3% APOR before 2009 and if more than 1.5% APOR in 2009-2017, and was not censored beginning in 2018. Due to censoring, very few rate spreads for site built home mortgages were reported before 2018. Since manufactured home mortgages tend to have higher prices than site-built home mortgages, most originated manufactured home loans reported rate spreads before 2018. When HMDA started reporting loan term in 2018, the most common loan term for a manufactured home mortgage was 23 years. Based the Federal Financial Institution Examinations Council's (FFIEC) rate spread calculator, the mean rate spread of 5.18% corresponds to an APR of 9% on a 23 year loan in 2017, well above mortgage rates for site built homes.

As part of implementing the Dodd-Frank Act, the CFPB added new data that firms must report as part of the HMDA requirement starting in 2018. These make the data substantially richer and include whether a manufactured home loan is secured by the home and land or home only (i.e. whether it is a chattel loan or mortgage), whether a manufactured home borrower owns only the home or the home and the land, loan term, interest rate, debt to income ratio, property value, borrower age, and information on fees paid at loan origination. These variables are in addition to those reported in all years: loan amount, race, ethnicity, loan type (FHA/VA/USDA/Conventional) and borrower income.

I supplement the HMDA data with data on foreclosures from ATTOM Data Solutions. This data source aggregates data on loan originations and foreclosures from county recorders. With the help of the REFM Lab at the Haas School of Business, I merged ATTOM and HMDA on loan amounts, lender, date and census tract using the algorithm detailed in Bartlett, Morse, Stanton and Wallace (2021).[12] Both data sources specifically identify manufactured homes, so I only include manufactured homes in ATTOM as candidate matches for HMDA. I match around 150,000 loans from 2004 through 2017. Though neither data source specifies whether manufactured home loans are chattel loans or mortgages, it appears likely that the matched loans are mortgages rather than chattel loans. First, ATTOM records real property transactions. Second, the average loans amount of matched loans is $107,898, much larger than the average in the overall HMDA data of

[12] See the appendix of that paper for details on the matching algorithm.

$74,969. Mortgages tend to be larger loans than chattel loans since the borrower is buying both the home and the land. Third, the matched loans over-represent banks, who are more likely to operate in the mortgage market and under-represent lenders focused on chattel loans like the dominant firm. There is little information in existing sources about the prevalence of manufactured home loan foreclosures.

Table 1 shows summary statistics from the HMDA and ATTOM data. The first panel shows variables available in all years, the second shows those available in 2018-19 and the third shows foreclosure data. Notably, borrowers are relatively likely to live in rural areas (41%) and about a quarter have debt-to-income ratios over the 43% qualified mortgage limit. The average loan term is 25 years, not dissimilar to site built home mortgages. I observe 10% of the loans in the matched sample ending in foreclosure after an average period of 5.4 years. I define a foreclosure as a short sale, foreclosure, or REO happening before the next arm's length transaction or refinance. Most of the events are REOs.

The CFPB's 2021 study of manufactured home loans includes includes summary statistics on variables only available in the confidential version of HMDA, such as credit score. The mean credit score of a chattel loan borrower is 676, just slightly lower than the mean manufactured home loan borrower at 691. These are both markedly lower than the mean credit score of site built home borrowers, which is 739. They also find that the higher rejection rates in manufactured home borrowing compared with site built home loans is not fully explained by borrowers' credit scores: "Sub-prime consumers who applied for financing on a site-built home were more likely to be approved for a loan than super-prime consumers with chattel applications or prime consumers with MH mortgage applications."

3 Reduced Form Evidence

In this section, I present reduced form evidence on the relationship between loan price and whether the loan was obtained from the dominant firm using HMDA data on manufactured home loans. The dominant firm charges higher prices on average than the average of other firms. For example, in the 2018-2019 HMDA data, the dominant firm's average rate spread was 5.30% compared with

an average of 2.02% for all other firms. One potential explanation for this is that the dominant firm is charging a given borrower more than other firms on average, but another possibility is that the dominant firm lends to much riskier customers than other firms and that the price difference is explained by borrower risk. I investigate this using two strategies with the HMDA data. Though I would ideally use performance data to investigate this question, I use the HMDA data since it provides a much more complete and less potentially selected picture than the foreclosure data, where there are very few matched loans for the dominant firm.

In the first exercise, I examine how much of the dominant firm's higher prices is explained by borrower and loan characteristics. I estimate regressions of the form

$$r_{ict} = \alpha df_{ict} + X_{ict}\beta + \eta_t + \gamma_t + \epsilon_{ict}$$

where r_{ict} is the rate spread paid by borrower i in county c in year t, df_{ict} is an indicator for the loan being issued by the dominant firm, η_c are county fixed effects and γ_t are time fixed effects. X_{ict} is a vector of loan and borrower characteristics. In all years, X_{ict} contains borrower income, loan amount, borrower race and ethnicity, and loan type (i.e. FHA/VA/USDA/Conventional). In models using the 2018-19 data, I additionally control for variables available in those years: debt to income ratio, loan amount, property value, land ownership, loan secured by land, loan term in months and borrower age.[13]

Table 3 shows that accounting for the available control variables, the dominant firm charges a substantial rate spread premium. Columns (1)-(3) show the results using the 2010-2017 data. They show that adding available controls and county and year fixed effects reduces the rate spread premium charged by the dominant firm by less than 50 basis points to 230 basis points. These data have two restrictions not present in the 2018-2019 data: fewer variables available and rate spreads not reported unless they are at least 1.5% APOR. The censoring of rate spread has little effect on the dominant firm, as 96% of its rate spreads are high enough to be reported. It matters much more for all other firms, for whom half of rate spreads are unreported due to censoring. In

[13] Debt to income ratio, property value, and borrower age are reported in bins. I include dummy variables for each debt-to-income ratio bin and borrower age bin and use the property value and loan amount bins continuously since those bins are of equal distance. See HMDA documentation for details: https://ffiec.cfpb.gov/documentation/2018/

my preferred specification, I impute 1.5% as the rate spread for loans where it is missing. This raises the rate spread premium charged by the dominant firm, likely because it adds lower rates from other firms that would be missing without imputation. I discuss the decision to use this as my preferred specification using the uncensored 2018-2019 data in the next paragraph. Table **??** shows the results with the imputed rate spreads. In the specification with all controls and county and year fixed effects, the rate premium for the dominant firm is 286 basis points.

I also show that accounting for censoring using a tobit model produces similar results. For the tobit models, I use state rather than county fixed effects due to computational constraints. The tobit results in Table 4 show that using a tobit model gives the dominant firm a higher rate premium. For comparison with the tobit results, in Table 3 column (4) I show that using state rather than county fixed effect changes the results little.

The results using 2018-2019 data, where rate spread is not censored and additional variables are available, are presented in Table 5. These richer data yields similar results to the 2010-2017 data. In the first two columns, I artificially censor the rate spread at 1.5% to compare the results with that constraint to the uncensored results available for 2018-2019. Column (1) sets any rate spread under 1.5% to missing, similar to the results in Table 2. Column (2) then imputes those missing rates as 1.5%, and column (3) shows the results using the full range of the rate spread. The imputed rate spread results are much closer to the results using the full rates spread, though the rate spread premium in column (2) of 217 basis points is still lower than the rate spread in column (3). This makes sense, given that imputing rates at 1.5% is assigning some borrowers higher rates than they actually paid, who are almost all borrowing from firms other than the dominant firm.[14] The rate premium charged by the dominant firm in 2018-2019 is slightly lower than in earlier years, which may be explained in part by the rate spread restriction imposed in

[14] This guides my decision to use the imputed data as my preferred specification for the instrumental variables strategy in the next section– while it is of course from different years so we can't know definitively whether the censoring had the same effect on the 2010-2017 results, the 2018-2019 results suggest that imputing is more accurate and still biases the rate premium downwards.

2014.[15] Column (4) adds the additional controls available for these years. This lowers the rate premium charged by the dominant firm by 17 basis points, giving a rate spread premium for the dominant firm of 247 basis points. Column (5) shows that I obtain a similar rate premium for the dominant firm of 209 basis points using interest rate (only available in 2018-2019) rather than the APR-based rate spread. The R^2 value is higher for interest rate than rate spread as well, possibly because it removes variation from firms charging different fees that is present in the APR-based rate spread.

This section shows that the dominant firm's high rates are mostly not explained by observable borrower risk or loan characteristics. In the next section, I continue this analysis using an instrumental variables strategy to instrument for the likelihood that a loan is made by the dominant firm. These analyses provides important context for my subsequent study of the 2014 rate spread restriction. If the market were perfectly competitive and the dominant firm were simply charging higher rates because they serve higher risk borrowers, then they would not be able to charge similar borrowers lower rates after the restriction, and would need to respond to the rate spread restriction by either rationing credit or adjusting loan characteristics to compensate for lower rates.

3.1 Instrumental Variables Strategy

Though the results in the last section suggest that the dominant firm charges similar borrowers higher rates, it is possible that I am not fully accounting for borrower risk with the controls even in the 2018-2019 data. For example, I do not observe borrower credit scores. Thus, I turn to an instrumental variables strategy as an alternative to identify the rate premium charged by the largest firm. I use the shift in firm composition away from large banks and towards nonbanks as an instrument to estimate the rate premium charged by the largest firm.

I instrument for taking out a loan from the dominant firm with the proportion of loans in a

[15] The dominant firm has faced accusations that they overcharge minority borrowers relative to white borrowers, such as in a 2015 *Seattle Times* article article entitled "Minorities exploited by Warren Buffett's mobile-home empire." I do not find evidence in the 2018 and 2019 data that the dominant firm differentially charge minorities more than other firms do, though cannot rule out industry-wide discrimination. Though minorities do pay higher rates on average accounting for controls across all firms, if I include df×Hispanic interaction terms, both of the effects are negative.

county-year that were made by stress-tested banks, conditional on the full set of controls and fixed effects used in the OLS estimation in the previous section. The intuition of the first stage is that as large banks exited the market in the post-crisis regulatory environment and nonbanks replaced their market share, borrowers were more likely to take out a loan from the dominant firm. I add an additional control variable for a loan being taken out from another nonbank, since other nonbanks were growing in prominence simultaneously with the dominant firm as stress-tested banks were exiting the market. The exclusion restriction states that the only way that the proportion of loans made by large banks in a county is related to rate spread is whether the loan is made by the dominant firm, conditional on county and year fixed effects and time-varying controls. A potential violation of the exclusion restriction would be if large banks strategically exited from certain areas in manufactured home lending but not others due to market conditions. An example of a violation of this restriction would be if large banks pulled out manufactured home lending in counties worst affected by the recession first. The results discussed in the next paragraph change very little if I remove the control for mean county income over time, which provides some evidence that this particular violation is not a concern.

The results are presented in Table 6. Column (1) is the OLS model with the same controls and fixed effects as used in the column (2) IV model. The IV results in column (2) give a similar rate premium of 330 basis points to the OLS rate premium of 290 basis points. The first stage indicates that a 10 percentage point reduction in the proportion of loans made in the county-year by stress-tested banks is associated with an increase of 3.3 percentage point increase in obtaining a loan from the dominant firm.[16]

4 HOEPA rule

The previous sections analyze the dominant firm's prices compared with other firms in the market. This both contributes to our understanding of pricing in the manufactured home loan market and

[16] The first-stage F-statistic is 12.6. Recent work by Lee, McCrary, Moreira and Porter (2020) suggests that the traditional threshold of 10 for the first-stage F-statistic is not sufficient to avoid the problems posed by weak IV. Using their table of critical values, I determined that a corrected critical t-value for significance at the 5% given the first-stage F-statistic is 3.11. The t-statistic for the dominant firm coefficient is well above this critical value, at 4.28.

sets the stage for understanding how price restriction might affect the dominant firm in the market. I study a price restriction that primarily affected the dominant firm. If the dominant firm's higher prices were completely explained by borrower risk in a perfectly competitive market, then the firm would not be able to pass on lower rates to borrowers under a price restriction. My finding above that the dominant firm charges higher rates than other firms controlling for observables is consistent with the firm being able to lower borrowers' rates to some extent without making a negative return on the loan.

After the 2008 financial crisis, the Dodd Frank Act expanded coverage of the 1994 Home Ownership and Equity Protection Act (HOEPA) to cover purchase mortgages, and I study the effects of the rates spread limit imposed as part of the rule. The CFPB issued the terms of the new rule in 2013 and it applied starting January 10, 2014. Under the new HOEPA rule, a loan is designated as a "high cost mortgage" if its terms are above the limits of any of three coverage tests. First, if the rate spread is more than 6.5% above the average prime offer rate (APOR) or 8.5% APOR for loans less than $50,000 secured by personal property (i.e. chattel loans). The loan is also designated a high cost mortgage if the point and fees are more than 5% of the loan amount for loans greater than or equal to $20,000 or the lesser of 8% or $1000 for loans less than $20,000. The last coverage test is that the loan is a high cost mortgage if there is a prepayment penalty charged more than 36 months after origination or greater than 2% of the prepayment.[17]

The rule did not forbid making a loan designated as a high cost mortgage, but places a number of conditions on the loans. For example, the borrower must go to HUD-approved home ownership counseling and bans certain loan features like balloon payment. Under the Truth in Lending Act, there are also increased damages for HOEPA loans and an extended statute of limitations. The CEO of Clayton Homes, which owns the two lenders who I refer to as the dominant firm, testified before the House Committee on Financial Services about the HOEPA rule and stated, "the Act would unfairly lump small balance loans used to purchase affordable manufactured housing into the same category as subprime predatory site-built mortgages ... due to the increased liabilities, responsibilities and stigma associated with making and obtaining a HOEPA 'high-cost mortgage,'

[17] Full information on the rule is available from the CFPB here: https://files.consumerfinance.gov/f/201603_cfpb_hoepa-compliance-guide.pdf

it is likely that a majority of these loans would not be made."[18] Even though the HOEPA rule did not forbid loans above the limit, this statement correctly predicted that very few loans would be made above the rate spread limit.

4.1 Lenders' response to HOEPA

Though the HOEPA rule did not specifically target manufactured homes, and allowed a higher threshold for chattel loans, it disproportionately affected manufactured homes loans due to their greater likelihood of having rate spreads above the limit. In this section, I examine how variables in HMDA were affected by the HOEPA rule. I first present the distribution of rate spreads before and after the HOEPA rule and show that the dominant firm's rate spreads bunched at the rate spread limits. Then, I examine trends in the number of loans and rejection rate before and after the rule was implemented. Following that, I look at how loan amounts and borrower incomes trended for loans affected by the HOEPA rule and loans unaffected, separately for the dominant firm and all other firms. This allows me to see whether lenders adjusted on those margins in response to the HOEPA rule, and whether the response differed by firm. In the next section, I supplement this analysis with an event study design that allows me to study the same trends, but accounting for control variables. Finally, for variables only available starting in 2018, I examine whether they change as they approach the rate spread limit.

First, I analyze the effect of HOEPA on rate spreads. Though HOEPA did not ban loans above the three limits set, lenders made very few loans over the rate spread limit after the HOEPA rule was imposed. Figure 6 shows the loans made by the dominant firm in 2013 and 2014, and the loans made by all other firms in 2013 and 2014. The vertical lines indicate the two rate limits: 6.5% rate spread and 8.5% rate spread. Since the dominant firm charges relatively high rate spreads, their loans were more likely to be affected by the HOEPA rule. In 2013, they made 90.3% of all manufactured home loans that would have been HOEPA high cost loans according to the rate spread limit. Rather than cutting loan supply in response to HOEPA, the number of loans from the dominant firm was nearly identical in 2013 and 2014. The bunching observed at the limits in

[18] The full testimony can be found here: https://financialservices.house.gov/uploadedfiles/112911clayton.pdf

2014 can be interpreted as a sign of market power as discussed in Cox, Liu and Morrison (2021), who observe similar bunching at the interest rate limit for Small Business Administration loans. If the lending market were perfectly competitive and firms were making no profit, then we should not observe bunching at the rate spread limits. In that perfectly competitive case, the dominant firm would not be able to offer the borrowers who had been borrowing at rates above the rate spread limit lower prices.

Second, I investigate how the number of affected and unaffected loans changed after HOEPA for the dominant firm and all other firms. Though the HOEPA rule lowered average rate spreads in the manufactured home loan market, it did not decrease the number of loans issued. In my analysis, I compare the number of loans near the HOEPA limit to loans not near the limit for the dominant firm and all other firms. I define the treatment group as being within 75 basis points of the HOEPA limit, where the bunching started in 2014 for the dominant firm as seen in Figure 6.[19] Figure 7 is the number of loans by year made by the dominant firm and all other firms, split into loans near the limit in panel (a) and loans not near the limit in panel (b). It shows that for the dominant firm, loans near the limit increased relative to loans unaffected by the limit at the time of treatment. It also shows that for all other firms, loans constrained by the limit decreased compared to unconstrained loans. Since the dominant firm made the vast majority of loans affected by HOEPA, the increase seen from the dominant firm outweighs the decrease from all other firms for loans near or above the limit in the aggregate. This result is consistent with the dominant firm making enough profits that it was able to absorb the lower rate spreads, while some smaller firms were no longer able to offer loans to borrowers who would have previously paid rates above the rates spread limit.

Third, I examine whether firms' likelihood of rejecting loans or borrowers' likelihood of applying changed in response to the HOEPA rule. It is more challenging to study the effects on application behavior because a rate spread is only recorded in HMDA if the loan is originated, so I cannot

[19] Though the bunching shown in Figure 6 is not precisely against each limit, from 2015 on the bunching was concentrated within 25 basis points of the limits, suggesting that firms may have become better at estimating how loan rates translated to APOR after a year of adjustment to the new policy. Benzarti (2021) formally tests the bunching in the greater mortgage market in 2014 and finds that the number of loans is significantly different starting at 75 basis points below each limit.

observe whether a loan that was not originated would have had a rate spread near the HOEPA rate spread limit. Thus, for application behavior, I simply compare the dominant firm to all other firms and consider the dominant firm as the "treated" group since a higher proportion of its loans were affected by the HOEPA rate spread limit. Figure 8 shows that while the number of applications to the dominant firm decreased from 2013-2014, it started to rise again in the years after.[20] From 2014-2015, the number of applications to all other firms decreased, suggesting that there are yearly fluctuations in the number of applications that are likely unrelated to the HOEPA rule. Figure 9 shows that the rejection rate from all other firms increased for one year in 2014 and then returned to a level below its pre-HOEPA rule average, while the rejection rate of the dominant firm smoothly trended down through the policy change.[21]

Next, I examine whether lenders responded to the HOEPA rule by changing loan amounts. One way firms could have compensated for lower rate spreads is to increase loan amounts and keep payments the same. In Figure 11, I examine the trends in loan amounts over time by whether loans are near the HOEPA limit, divided into the dominant firm and all other firms. Though looking at trends for loans not near the HOEPA limit is a helpful comparison group, the two groups are clearly different because those in the treated group are likely riskier borrowers. Figure 11 shows that starting in 2010, loans near and not near the HOEPA limit for the dominant firm had similar loan amounts. However, it is clear that the parallel trends assumption would not hold. Though the loan amount did increase for the treated group at the time of treatment by $5000, the increase is similar to the previous year's increase of $4000, which was unaffected by the HOEPA rule. In order to keep the payment for a $60,000 23-year loan constant in a drop from 12% APR to 10% APR, loan amount would need to increase to over $69,000. For all other firms, I also observe no discrete change in loan amounts at the time of the policy change. The percentage increase in

[20] Note that the number of loans not near the HOEPA limit from the dominant firm decreased in 2013-2014 while the number of loans increased near the limit, so it seems likely that the one year fall in applications comes from the former group.

[21] I use the rejection rate rather than acceptance rate due to apparent inconsistencies in the coding of the application data for incomplete or accepted but not originated. For example, until 2015 the dominant firm only used the coding "approved but not accepted" by the borrower for non-rejected and non-originated applications, but in 2015 they shifted to coding many of these applications as "incomplete" or "withdrawn", designations they had not used before. The total number of non-rejected, non-originated applications remained similar through this change, which did not occur concurrently with the HOEPA rule.

loan amounts is higher in the near-HOEPA limit group than the control group for all other firms and continues on that trend, potentially reflecting that firms responded to HOEPA over time by increasing loan amounts or that firms making smaller loans exited this segment of the market.

Another potential adjustment lenders could have made in response to HOEPA is to shift to lending to less risky borrowers. While I do not observe credit scores, I do observe borrower incomes and use that as a proxy for risk. Figure 13 shows borrower incomes by year and split into the affected and unaffected groups. In the dominant firm, average incomes in the affected and unaffected groups track each other closely other than a spike in the control group in 2013. The average borrower income in the affected group actually fell slightly from 2013-2014, showing that the dominant firm did not substitute towards more creditworthy borrowers to compensate for lower rates. Similar to loan amounts, all other firms have a greater increase in income for treated than control loans and continues to increase on a higher trend than the control group in subsequent years. This could reflect firms making fewer loans to risky borrowers and is consistent with the fall in number of loans from all other firms.

Finally, I examine whether variables only available starting in 2018 change as loans' rate spread approach the HOEPA limits. It's possible that lenders could have made up for the lower rate spreads under the HOEPA rule with higher fees, smaller down payments or longer loan terms, variables that are only available starting in 2018. I calculate the averages of these variables in 5 basis point increments starting at the cap and going down to 300 basis points below the cap. In the 2018 and 2019 data, I am able to differentiate between chattel loans and mortgages, so I apply the 8.5% APOR cap to only chattel loans of less than $50,000.[22] [23] Consistent with the loan amount analysis above, I do not see any evidence that the dominant firm is increasing loan amounts relative to property values for loans near the cap compared to all other loans. Down payments decline slightly for all other firms approaching the cap, showing that they are potentially compensating for

[22] For this analysis, I show the two subsidiaries of the dominant firm and all other firms separately. Figure 15, showing the average down payments demonstrates the importance of this distinction. Since the two subsidiaries have different down payments on average and bunch at very slightly different values under the cap, if I aggregate the two subsidiaries it will appear that there is a spike in the proportion down payment at the cap. For example, one subsidiary reports many rate spreads of 8.45 while the other reports many of 8.49, so if I calculate averages 5 basis point groups, one does not have any in the group immediately under the cap.

[23] Proportion paid as down payments are calculated as 1-(loan amount/property value) and may not be precise due to the grouping of these values in $10,000 groups.

the HOEPA rule by increasing loan amounts, though the decline is not strongly bunched against the cap. I complete the same exercise with fees and loan term, and find no evidence that firms are compensating for lower rate spreads by increasing fees or loan terms. Figure 16 shows the same exercise as down payments, conducted for loan terms. For the dominant firm, I do not see any increase in loan terms approaching the rate spread limit. Further, there is little variation in loan terms– about 90% of loans near the rate spread limit have the most common loan term of 23 years. Figures 17 and 18 show the exercise for total points and fees (reported for chattel loans) and total loan costs (reported for mortgages). Again, fees do not exhibit a large change at the cap for the dominant firm. The all-other-firms group is smaller than each of the subsidiaries alone near the cap, and may be noisier partly due to smaller size and differences in firm composition at different distances from the cap, which could potentially explain the fall in fees near the cap for all other firms.

4.2 Event Studies

In the previous section, I discuss how the observable variables themselves changed in response to the HOEPA rule. In this section, I supplement that analysis using an event study design. My setting does not have the features that would typically be desirable for an event study design; the treatment group is clearly different than the control group, and the time series analyzed in the last section show that the parallel trends assumption does not hold. However, an event study design is still a useful supplement to the above analysis because it allows me to look at the changes over time in outcomes for loans that were or were not affected by the HOEPA rule, accounting for control variables. If the trends change substantially with control variables, that would indicate that there are effects happening that cannot be understood by simply examining the time series. Using rate spread, loan amount and income as outcome variables, I estimate the following regression model and show the results in Figure 10.

$$y_{icjt} = \sum_t D_{icjt}\alpha_t + X_{icjt}\beta + \gamma_t + \eta_c + \xi_j + \epsilon_{icjt}$$

The outcome variable is y for borrower i in county c and year t borrowing from firm j. D_{icjt}

are indicators for being in the treatment group in each year, i.e. within 75 basis points of the HOEPA rate spread limit or above. X_{icjt} is a vector of controls including loan amount, borrower income, race, ethnicity, and type of loan (FHA/VA/USDA/Conventional). The model includes year, county fixed and firm fixed effects and I cluster standard errors at the county level.

First, the results presented in Figure 10 show a drop of close to 200 basis points in rate spread for affected loans after the HOEPA rule, much larger than any previous fluctuation. I conduct the same event study with the outcomes as ln(loan amount) and ln(income) and present the results in Figures 12 and 14 placed for comparison with the time series graphs of loan amount and income. These figures show that including controls has little effect on the coefficients for the loan amount for the dominant firm. I interpret this as meaning that the time trends in Figure 11 are robust to the inclusion of controls. For all other firms, including controls and fixed effects somewhat increases the increase in loan amount after HOEPA and increases the precision of the estimates. For income, Figure 14 shows the event study results. Including controls moves the dominant firm's income change to an insignificant level for most years.[24] For all other firms, including controls moves the pre-HOEPA rule coefficients closer to zero and maintains that the increase in incomes after HOEPA is statistically significant.

4.3 Proposed Policy Changes

Though the HMDA data does not show any evidence that the dominant firm adjusted on observable variables to compensate for the lower rate spreads under the HOEPA rule, it does not cover every loan characteristic. It is theoretically possible that the dominant firm made an adjustment to fees not reported in HMDA, like late fees, to compensate for lower rate spreads. Since I cannot directly investigate this type of change, I turn to an examination of the industry's lobbying actions in wake of the HOEPA change. There were efforts from 2013 until 2018 to raise the HOEPA rate spread limit for manufactured homes to a less binding level, in versions of the "Preserving Access to Manufactured Housing Act." According to lobbying disclosures from the Center for Responsive Politics, the bill was a top issue for the Manufactured Housing Institute (MHI), an industry group

[24] Note that the positive no-controls coefficients are due to the lower spike in the treatment group than control group for 2013.

formerly chaired by the general counsel of the dominant firm, and for the dominant firm itself[25]. In the 2016 election, the MHI's Political Action Committee made its four largest contributions to the four congressional representatives who sponsored this bill [26]. The bill was bipartisan, with two of the four sponsors coming from each major political party. It is not possible to determine exactly how much money was spent on lobbying for this bill, which also included a provision preventing manufactured home dealers from being designated mortgage originators if they did not receive compensation for the loan. The bill was either the most cited or tied for the most cited bill in lobbying disclosures for MHI from 2013-2018, which spent between $500,000 and $1 million in lobbying per year. It seems unlikely that the industry would spend substantial resources lobbying for a change in the HOEPA rate spread limit if they had found a way to modify non-rate spread characteristics of the loan to keep their profits constant.

Figures 19 and 20 show how the 2017 version of the Preserving Access to Manufactured Housing Act for the dominant firm and all other firms based on their lending in the last pre-HOEPA rule year, 2013. As of this writing, the efforts to raise the HOEPA rate spread limit have been unsuccessful. The 2017 version of the bill would have raised the rate spread limit to 10% for chattel loans less than $75,000, and kept it at 6.5% for all other loans. This would have substantially lowered the number of loans subject to the HOEPA rule. For the dominant firm, this would have decreased the percent of loans bound by the HOEPA rate spread limit from 35% to 14% and for all other firms from 2% to 1%, using rate spreads from the year before the HOEPA rule implementation.

5 Model

In the previous section I show that in the aggregate, the HOEPA rule lowered prices for borrowers without changing other variables. Given that prices fell and quantity of loans did not, the reduced form evidence alone points to an increase in welfare for borrowers as a result of the rate cap. This begs the question of whether a lower rate spread cap could improve borrower welfare even more.

[25] https://www.opensecrets.org/federal-lobbying/clients/summary?cycle=2018&id=D000000458 , https://www.opensecrets.org/federal-lobbying/clients/summary?cycle=2018&id=D000032651

[26] https://www.opensecrets.org/political-action-committees-pacs/manufactured-housing-institute/C00043463/candidate-recipients/2016

However, though a lower cap may further improve borrower welfare through lower prices, the effect is ambiguous. For each borrower, there is a rate at which the firm would expect to make zero profit on a loan, and if the rate cap is lowered below that, the lender will no longer offer a loan to the borrower. Thus the overall effect on welfare of any given rate cap is theoretically ambiguous. I develop a model of supply and demand for the manufactured home loan industry to analyze the welfare effects of lower rate caps for borrowers and lenders.

5.1 Demand

I model demand for manufactured home mortgages as a function of price and loan characteristics. The indirect utility of a borrower is:

$$V_{ijsl} = -\alpha_i r_{jsl} + X_{jsl}\beta + \xi_{jsl} + \epsilon_{ijsl}$$

where r_{jsl} is the rate spread of product j in state s offered by lender l. X_{jsl} is loan characteristics such as whether the loan is a chattel loan or mortgage.

Each borrower has a choice set C_i. The choice set variation by borrower reflects that some lenders will decline to lend to a borrower, so even in the same place, two borrowers may have different choice sets. For example, a borrower with a poor credit history may have a smaller choice set. The borrower will choose the loan that maximizes their indirect utility. The probability that a borrower chooses a loan is

$$s_{ijsl} = Pr\left(jsl\ chosen \mid C_i\right) = Pr\left(V_{ijsl} > V_{iksl}\right) \forall\ k \in\ C_i$$

5.2 Supply

Lender l's profit offering product j in state s is defined as

$$\Pi_{ijsl} = (r_{jsl} - c_{jsl})s_{ijsl}$$

Firm cost c_{jsl} represents both fixed costs the firm faces in issuing a loan, such as funding cost, as well as the risk associated with default, which will be higher for riskier borrowers. Since the probability of a borrower choosing an option is dependent on the rate spread, taking the first order

condition for r_{jsl} will yield the standard expression for optimal price, where optimal rate spread is a combination of the firm's costs and the markup.

$$r^*_{jsl} = \frac{1}{\alpha_i(1 - s_{ijslb})} + c_{jsl}$$

6 Estimation and Results

In this section, I explain how I construct each borrower's choice set, estimate the model laid out in the previous section, and identify borrowers' sensitivity to a loan's rate spread.

Counterfactual Choice Set

Each borrower's choice set depends on their personal risk and the lenders operating in their geographic area. First, I use a random forest classifier to non-parametrically predict which lenders in a borrower's area will offer him or her a loan. I then impose additional restrictions on the random forest predictions to ensure that I do not include loans in the choice set that are unlikely to be available for the borrower. For the model, I use the 2018 and 2019 HMDA data due to its increased information on loan and borrower characteristics compared to earlier years of HMDA.[27]

For each borrower, I construct a potential choice set of all lenders who made a loan in the borrower's county and year. Following Robles-Garcia (2020) and Benetton (2021), I group the smallest firms, which I define as fewer than 100 loans per year, into the outside option. This leaves about 150 firms nationally in each of 2018 and 2019. I define a product as a grouping of borrower and loan characteristics, and assume that each lender only offers each borrower one product. Most manufactured home loans are fully amortizing, fixed rate loans– 84% have no introductory rate period in HMDA and only 3% have interest-only payments. I define a product by lender, income decile, state, FHA status, low, high or moderate debt to income ratio, young borrower, chattel vs mortgage status, whether a borrower owns the land, above median loan amount, state and year. Within these groups, I impute counterfactual rate spreads in the potential choice sets as the mean rate of originated loans in that group. I assume that if the borrower chose a product type like

[27] I drop observations that have missing information for county, debt to income ratio, property value, loan secured by home vs. land, land ownership and income. Together, these make up less than 20% of observations.

an FHA loan or chattel loan (borrowers who own land can choose between a chattel loan and a mortgage), the loans in their counterfactual choice sets also have that characteristic. This may not be the case if, for example, lenders tend to guide borrowers towards a certain product.

I use the HMDA application data to train a random forest classifier to predict whether a given lender will accept a given borrower's application.[28] I run the model separately for 2018 and 2019 and get approximately 79% accuracy in each year. 79% accuracy means that 79% of the model's predictions in the test data are correct. The variables I use to predict acceptance are borrower debt-to-income ratio, borrower income, firm, loan amount, loan type (government insured or conventional), whether the borrower owns land, whether the loan in secured by land, property value, and the center latitude and longitude of the borrower's census tract or county in cases when the tract is missing.[29] The data are split into a training set to train the model and a testing set to test the model's accuracy.[30] Since the random forest is a classification algorithm, it directly predicts whether each borrower will be approved by a given lender in their county, and I include the loans predicted to be approved in the borrower's choice set.

The random forest classifier predicts that riskier borrowers will be offered fewer loans. Borrowers with $> 60\%$ debt to income ratios are predicted to have 80% fewer offers than the rest of the population. For each \$1000 increase in income, a borrower is predicted to have 0.14 more loan offers. I chose to use the random forest classifier because it captures important interactions between variables– for example, I see in the data that nonbanks appear more sensitive towards borrowers with high debt to income ratios than banks.

I eliminate any predicted acceptances of borrowers who do not own their land by companies who I observe making no chattel loans in HMDA. Since some firms only make mortgages and borrowers who don't own their land are restricted to chattel loans, these loans are not real options for the borrower. The random forest may still predict that the chattel borrower will be approved by the mortgage-only firm if, for example, the borrower has high income and a low debt-to income ratio. I also implicitly eliminate offers in demand estimation when there is no observable rate

[28] I use the RandomForestClassifier function in the scikit-learn package in Python.
[29] I construct the centers of census tracts from the Census Gazetteer files.
[30] I censor a small number of outliers in borrower income, property value and loan amount each at \$1 million, all representing less than 1% of observations.

spread in the product group, meaning that the firm did not make a loan with the given loan and borrower characteristics. This is an additional check that I am not including loans that are unlikely to be made in my counterfactual analysis, which could lead to an erroneously low optimal rate spread cap in the counterfactual analysis.

6.1 Demand

I estimate the demand model in two steps. The first stage of the model is estimated using logit, and then in the second stage I recover demand parameters, in a setup similar to Robles-Garcia (2020). I identify borrowers' sensitivity to price using an instrumental variables approach.

I assume that shock ϵ_{ijsl} is independent and identically distributed according to the Type I Extreme Value Distribution. Thus, the probability that a borrower i in state s will choose product j conditional on their choice set C_i can be written as

$$s_{ijsl} = Pr(chosen|C_i) = \frac{exp(-\alpha_i r_{jsl} + X_{jsl}\beta + \xi_{jsl})}{\sum_C exp(-\alpha_i r_{jsl} + X_{jsl}\beta + \xi_{jsl})}$$

In the first stage I estimate the log likelihood function, the standard logit where $y_{ijsl} = 1$ if the borrower chose the loan:

$$LL(\delta_{osl}) = \sum_i \sum_{j \in C_i} y_{ijsl}\, ln(s_{ijsl})$$

I estimate fixed effects δ_{osl} at the state-ownership-lender level. This means that, for example, I assume that the dominant firm has the same fixed effect for all borrowers who own their own land in Texas. I divide each firm into state and ownership groups to allow for borrowers to have a different preference for a firm depending, for example, on its presence in their local geographic area and by whether the firm mostly targets chattel or mortgage borrowers. Though a finer product definition as described above better captures the variation in rate spread that comes with borrower risk, if I use the full product definition for the fixed effects in the first stage, they may be estimated based on very few observations. Thus at this stage, I assume that within state-ownership groups, a firm has the same mean utility for borrowers. In the second stage, I use prices at the finer product level, allowing prices to reflect whether the firm offers a wide variety of products or only

lends to one type of borrower.

In the second stage, I recover the effect of prices by regressing on the fixed effects as shown below:

$$\delta_{osl} = -\alpha_b r_{jslb} + \gamma_l + \eta_t + \xi_{jslb}$$

I allow the coefficients to vary by income bin, denoted as b, and η_t are year fixed effects. I use an instrumental variables strategy to recover the coefficient on price. Since I do not directly observe cost shifters, I use a strategy similar to Nevo (2001) and Hausman (1996). The instrument I use is the mean of a firm's rate spreads for above or below median income borrowers in states other than the borrower's state in that year. The intuition of this instrument is that it captures firm-specific cost shifters, without picking up on local demand's influence on price. For example, if a firm's funding cost rises from one year to the next, this instrument would reflect the rise in costs as they are passed through to prices in other states. Calculating the instrument by income bin allows it to vary by borrower risk. This would for example capture if a firm faced increasing costs for riskier lenders over time. I obtain similar results with this version of the instrument, and use it in the results discussed in the next paragraph. A threat to this identification strategy would come from a national shift in demand for loans from a particular firm, for example from an advertising campaign. I estimate the demand model only for products with rates of 6.25% or lower, since those are the products unaffected by the HOEPA rule and those are the products included in the counterfactual.

The demand parameters are shown in Table 7. Below median income borrowers are slightly more price-sensitive than higher income borrowers. Figure 21 shows the fit of the model by plotting the actual national market share against

$$\frac{1}{N} \sum s_{ijsl} \; \forall \; j \in J$$

The two firms with large predicted market shares are the two subsidiaries of the dominant firm.

6.2 Supply

The median markup overall is 155 basis points, and the median markup for the dominant firm is 228 basis points.[31] For loans unaffected by HOEPA, i.e. loans with rate spreads of 6.25 and under, I calculate cost as observed rate spread – markup. This cost includes both default risk, funding cost and other costs like labor and staff. The costs have intuitive relationships with observable variables. For example, a 100% increase in a borrower's income lowers a lender's cost by 50 basis points, showing that less risky borrowers are predicted to have lower costs for firms. The borrower owning land decreases costs for lenders by 16 basis points, perhaps reflecting greater collateral value. I estimate the lowest cost firm type to be credit unions, followed by large banks, and then small banks and nonbanks. My counterfactual does not necessitate separating costs associated with default risk from funding costs– it simply states that when the rate cap falls below the firm's cost of making a loan, whatever the source of that cost, the firm will no longer issue the loan.

In order to assess my cost estimates, I turn to earnings report for the dominant firm. Though Berkshire Hathaway, which owns the dominant firm, does not report earnings specifically from manufactured housing finance, their 10-K reports do include earnings from manufactured housing overall, including manufacturing and finance of homes. In 2019, they reported earnings of $1.1 billion on revenue of $7.3 billion, for 15% pre-tax earnings. In 2018, they reported earnings of $911 million on revenue of $6 billion, for again 15% pre-tax earnings.[32] This profit is difficult to compare to mine both because it does not separate finance from manufacturing activities (though Berkshire Hathaway states that a "significant portion" of earnings come from finance), and it reflects income from the stock of their loans rather than originations from the past year. Since my estimated markups for the dominant firm are higher– around 25%– than the dominant firm's overall reported profit, I perform a second version of the rate spread cap counterfactual with markups halved in order to examine the sensitivity of the counterfactual to the exact level of

[31] For about the top 1% of markups, I get unrealistically high values. This happens when a borrower's predicted choice set is very small and one option is vastly preferred to the others, leading to a very high s_{ijsl}. I choose to restrict the markups to the 99th percentile, which is 500 basis points. This has little effect on my consumer welfare counterfactual, but not making this restriction leads to unrealistic firm profit estimates driven by the outliers.

[32] The reports can be found here: https://www.berkshirehathaway.com/reports.html

markups.

6.3 Counterfactual Analysis

In this section, I use the model to simulate borrower and lender surplus under lower rate spread caps. For lenders, a rate spread cap unambiguously lowers surplus, but for borrowers there is a trade-off. As rates decrease, borrower surplus rises due to lower prices. But as the rate cap becomes more binding, some firms will no longer be able to make a profit at the lower rate and will no longer make loan offers to some borrowers, lowering surplus.

I calculate borrower surplus as described in Train (2009):

$$CS_i = \frac{1}{\alpha_i} ln(\sum_{j=1}^{J} e^{V_{ij}}) + C$$

where C is a constant that becomes irrelevant as I consider changes in surplus. I do not consider the outside option as part of surplus. I start at the current level of rates unaffected by the cap– 6.25% rate spread, and calculate the change in surplus relative to surplus at 6.25% as I lower the cap by 25 basis points, uniform by loan size.[33] There are three possible outcomes for each offered loan:

1. Optimal rate r^* is greater than cap C, so the loan is unaffected and offered at the rate it would be offered at without the cap.
2. Optimal rate r^* is less than the cap C, but is C is greater than cost c. Thus, the firm can still offer the loan profitably, and will offer the loan with the rate set at the cap.
3. The cap C is less than the cost c. The firm cannot make a profit on the loan and therefore does not make a loan offer to the borrower.

This is similar to the counterfactual estimated in Cuesta and Sepulveda (2021). It is consistent with the bunching I observe from 2014 onwards– many borrowers are offered loans at the level of the cap. Figure 22 is an example that illustrates how the counterfactual works by plotting the

[33] About 3% of borrowers are not included in the analysis because they have no predicted loan offers below 6.25%. The peak average consumer surplus in the analysis is higher than consumer surplus calculated for all loans, including those above 6.25%, even making the conservative assumption that those 3% of consumers had lost all surplus at the optimal cap.

rate spreads of offered loans under a potential 300 basis point cap. Many borrowers are offered rate spreads at the cap, and loans with rate spreads below the cap are unaffected. Loans that have a cost greater than the cap are no longer offered.

Figure 23 shows the change in consumer surplus by income group, and Figure 24 shows the average increase in surplus at the optimal rate cap for low income borrowers by state. Comparing with Figure 5, we see that surplus increases more in states that have relatively concentrated markets or relatively low income borrowers. The change in surplus is displayed in basis points. Translating that to dollars, a fall of of 50 basis points for a $60,000 loan at 10% interest is worth $5600 in interest over the life of the loan. Though my result is different than Cuesta and Sepulveda (2021)'s result that a interest rate cap decreased welfare in the Chilean consumer loan market, it is consistent with their finding that an interest rate cap may improve welfare in a sufficiently concentrated market.

Figure 25 shows the mean number of loan offers per borrower. As expected, the number of loan offers is higher for higher-income (lower risk) borrowers. As the level of the rate cap decreases, fewer loan offers are available to each borrower.

Figure 26 shows average profit as the level of the rate cap decreases. If a firm no longer makes a loan due to lack of profitability, I consider the entire markup as lost profit in this calculation. I do the same for consumer surplus– if a borrower loses all loan options, then their change in consumer surplus is the negative of their initial surplus.

Comparing the loss in profit to the gain in consumer surplus shows that the loss in profit clearly outweighs the gain in consumer surplus in terms of total surplus. But since it is likely that manufactured home borrowers have a higher marginal utility of income than owners of manufactured home loan companies, that does not necessarily mean that a social planner should not impose a stricter rate cap.

In Figure 27 I conduct a check on my consumer surplus counterfactual by halving markups but keeping price sensitivity constant. In this way, both are working to keep the increase in surplus low, since in the model higher price sensitivity will cause the magnitude of the welfare increase under lower caps to be larger. There is still substantial gain possible from an rate spread cap in

this scenario– all borrowers are better off for about 200 basis points and high income borrowers are made better off by a stricter cap for longer.

In the welfare calculations, fixed effects account for the fact that borrowers prefer some lenders over others for non-price reasons. For example, the dominant firm generally has a high fixed effect because a lot of borrowers choose it despite high prices. This could reflect factors like the convenience of using the manufacturer's lender that are not in my model. Since the fixed effects are included in the welfare calculation, this means that losing the dominant lender has a large negative effect on surplus, whereas a losing a lender that few borrowers choose and thus has a low fixed effect has a smaller effect.

Conclusion

In this paper, I study a regulation whose introduction acted as a price cap in the manufactured home loan market. In the aggregate, it did not ration credit or make substantial changes to observable loan or borrower characteristics. This response was heterogeneous by firm and the aggregate result is driven by the largest firm in the market– smaller firms were more likely to cut supply, stop lending to risky borrowers and increase loan amounts.

I develop and estimate a model of the manufactured housing loan market and use the model to simulate counterfactual welfare under stricter rate restrictions. I find that borrower surplus peaks a little more than 200 basis points below the current level of the restriction, and that restricting rate spreads to prime mortgage prices would result in a large surplus loss.

Despite its importance to affordable housing in the United States, manufactured homes are often left out of discussions about affordable housing. Manufactured home borrowers face high prices above those that would be determined purely by credit risk due to market concentration and little government support of the market compared to site-built housing. This makes it more difficult for low-income Americans, especially in rural areas, to become homeowners.

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Table 1: **Summary Statistics from HMDA and ATTOM**

2007-2017 HMDA	Mean	SD	Count
Rate Spread	5.18	2.50	525,399
Loan Amount, 1000s	76.34	52.78	838,459
Borrower income, 1000s	50.76	39.90	833,740
White, Non-Hispanic	0.83	0.42	779,237
Black, Non-Hispanic	0.06	0.23	779,237
Hispanic or Latino	0.08	0.29	779,237
Dominant Firm	0.32	0.47	838,459
Rural (non-MSA)	0.41	0.49	838,459
FHA/VA Loan	0.26	0.44	838,459
2018-19 HMDA:			
$>$ 43% debt to income ratio	0.23	0.43	221,745
Loan Term (Years)	24.52	6.13	222,946
Personal Property Loan	0.43	0.50	219,150
Implied proportion down payment	0.10	0.36	210,068
Interest rate	6.77	2.27	222,492
ATTOM:			
Foreclosed	0.10	0.30	156,351
Time to foreclosure (years)	5.37	2.76	16,388

HMDA loans are first-lien purchase loans for owner-occupied homes and only include originated loans. ATTOM loans are from the matched sample only. Foreclosure is defined as a foreclosure auction, REO or short sale happening before the next arm's length transaction or refinance.

Table 2: **OLS, Censored Rates not Imputed**

	(1) Rate Spread	(2) Rate Spread	(3) Rate Spread
Dominant Firm	2.714***	2.233***	2.299***
	(0.0601)	(0.0524)	(0.0628)
Controls	No	Yes	Yes
Year FE	No	No	Yes
County FE	No	No	Yes
R^2	0.280	0.449	0.517
N	369321	286130	286027

Standard errors in parentheses are clustered at the county level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

HMDA 2010-2017 first-lien purchase loans for owner-occupied homes. Controls are: borrower income, borrower race, borrower ethnicity, loan amount and loan type (Conventional/FHA/VA/USDA).

Table 3: **OLS, Censored Rates Imputed**

	(1) Rate Spread	(2) Rate Spread	(3) Rate Spread	(4) Rate Spread
Dominant Firm	3.574***	2.979***	2.856***	2.937***
	(0.0503)	(0.0607)	(0.0719)	(0.0701)
Controls	No	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
Geog. Fixed Effects	None	None	County	State
R2	0.413	0.467	0.538	0.498
N	556486	456036	455964	456036

Standard errors in parentheses are clustered at the county level

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

HMDA 2010-2017 first-lien purchase loans for owner-occupied homes. Controls are: borrower income, borrower race, borrower ethnicity, loan amount and loan type (Conventional/FHA/VA/USDA). Rate spreads with missing values are imputed with 1.5.

Table 4: **Tobit, Censoring at 1.5% rate spread**

	(1) Rate Spread	(2) Rate Spread	(3) Rate Spread
Dominant Firm	4.644***	3.428***	3.357***
	(0.0805)	(0.0686)	(0.0697)
Controls	No	Yes	Yes
Year FE	No	No	Yes
State FE	No	No	Yes
N	556486	456036	456036

Standard errors in parentheses are clustered at the county level.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

HMDA 2010-2017 first-lien purchase loans for owner-occupied homes. Controls are: borrower income, borrower race, borrower ethnicity, loan amount and loan type (Conventional/FHA/VA/USDA). Rate spreads with missing values are imputed with 1.5.

Table 5: **OLS, HMDA 2018-2019**

	(1) Rate Spread	(2) RS Imputed	(3) Rate Spread	(4) Rate Spread	(5) Interest Rate
Dominant Firm	1.483***	2.247***	2.642***	2.484***	2.099***
	(0.0368)	(0.0402)	(0.0488)	(0.0440)	(0.0329)
Controls	Yes	Yes	Yes	Yes	Yes
2018-19 Only Controls	No	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes
R2	0.596	0.637	0.604	0.654	0.727
N	116351	176530	176530	168391	173341

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

HMDA first-lien purchase loans for owner-occupied homes. Controls are: borrower income, borrower race, borrower ethnicity, loan amount and loan type (Conventional/FHA/VA/USDA). 2018-19 Controls are: age, debt to income ratio, land ownership, loan secured by land, loan term and property value. "RS Censored" outcome only uses observations with a rate spread of at least 1.5. "RS Imputed" outcome is rate spread with all values below 1.5 imputed as 1.5.

Table 6: **IV Results**

	(1) OLS Rate Spread	(2) IV Rate Spread
Dominant Firm	2.890***	3.420***
	(0.0681)	(0.799)
Other Nonbank	0.517***	0.644**
	(0.0526)	(0.203)
Controls	Yes	Yes
County and Year FE	Yes	Yes
F-Stat		12.6
N	455961	455961

Standard errors in parentheses are clustered at the county level.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

HMDA 2010-2017 first-lien purchase loans for owner-occupied homes. Controls are: borrower income, borrower race, borrower ethnicity, loan amount and loan type (Conventional/FHA/VA/USDA).

Table 7: **Demand Model Results**

	(1) Low Income	(2) High Income
Rate Spread	-0.92**	-0.71**
	(0.28)	(0.23)
N	31,174	32,841

Robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Model uses the 2018-2019 HMDA data. First stage logit has 3,720,277 borrower-product pairs. Second stage is run at product-state level. Instrument for price used as described in demand model section.

Figure 1: An example of a single-wide manufactured home. Source: Wikimedia Commons.

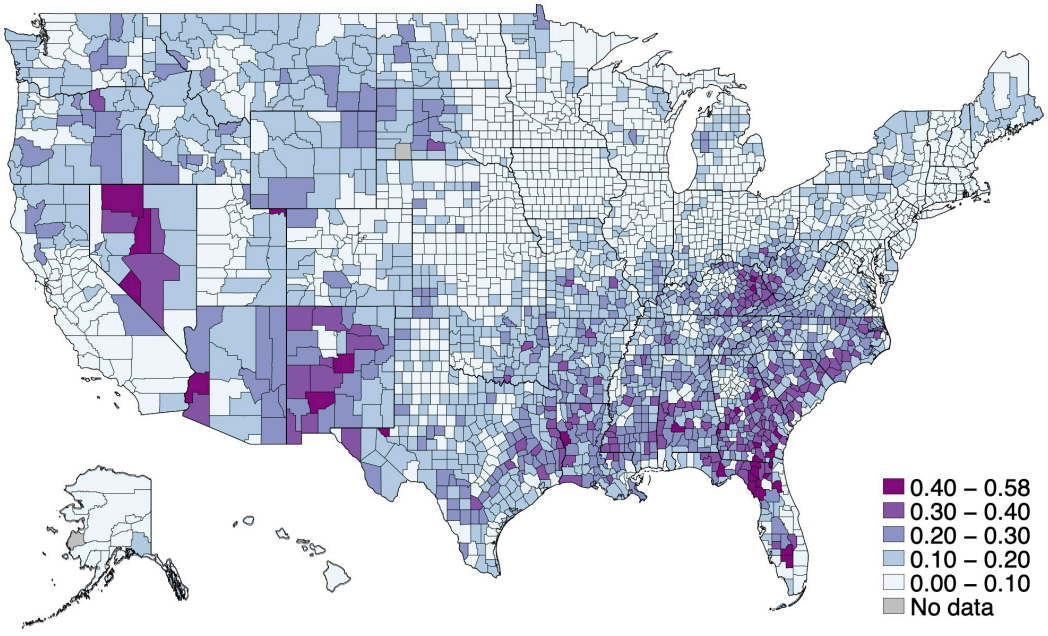


Figure 2: Proportion of housing units that are manufactured homes by county. Source: Calculations from American Community Survey 2015-2019, accessed through IPUMS NHGIS.

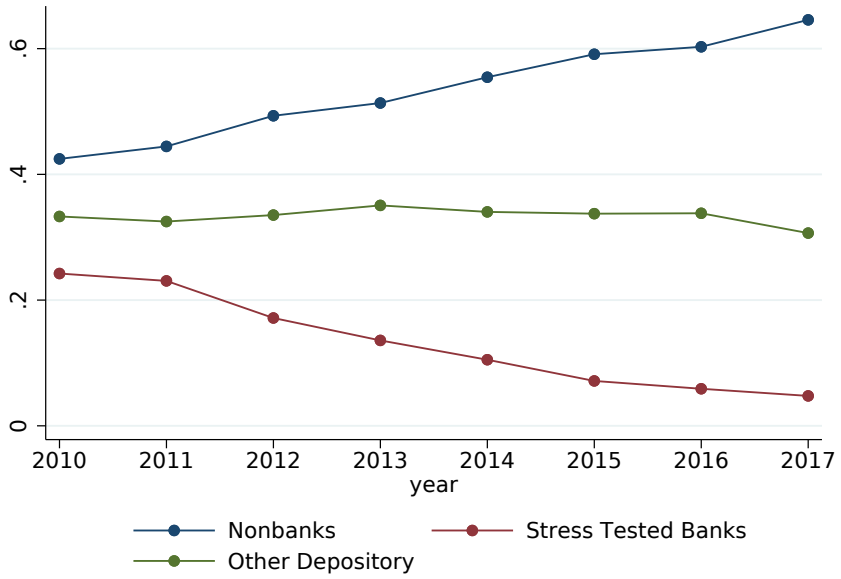


Figure 3: Market shares by firm type over time. Source: Calculations from HMDA.

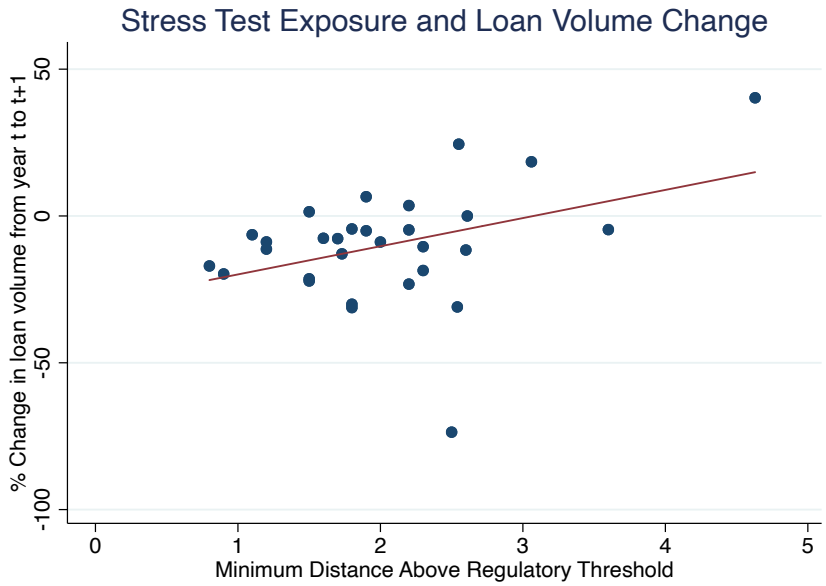
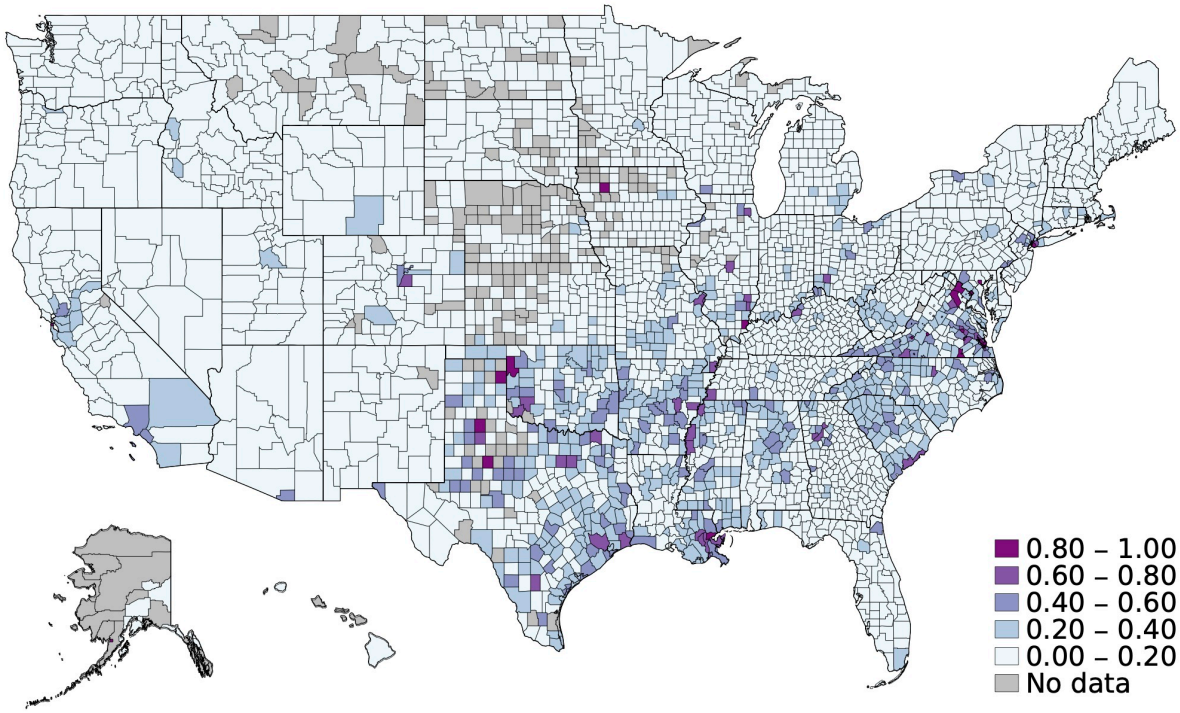
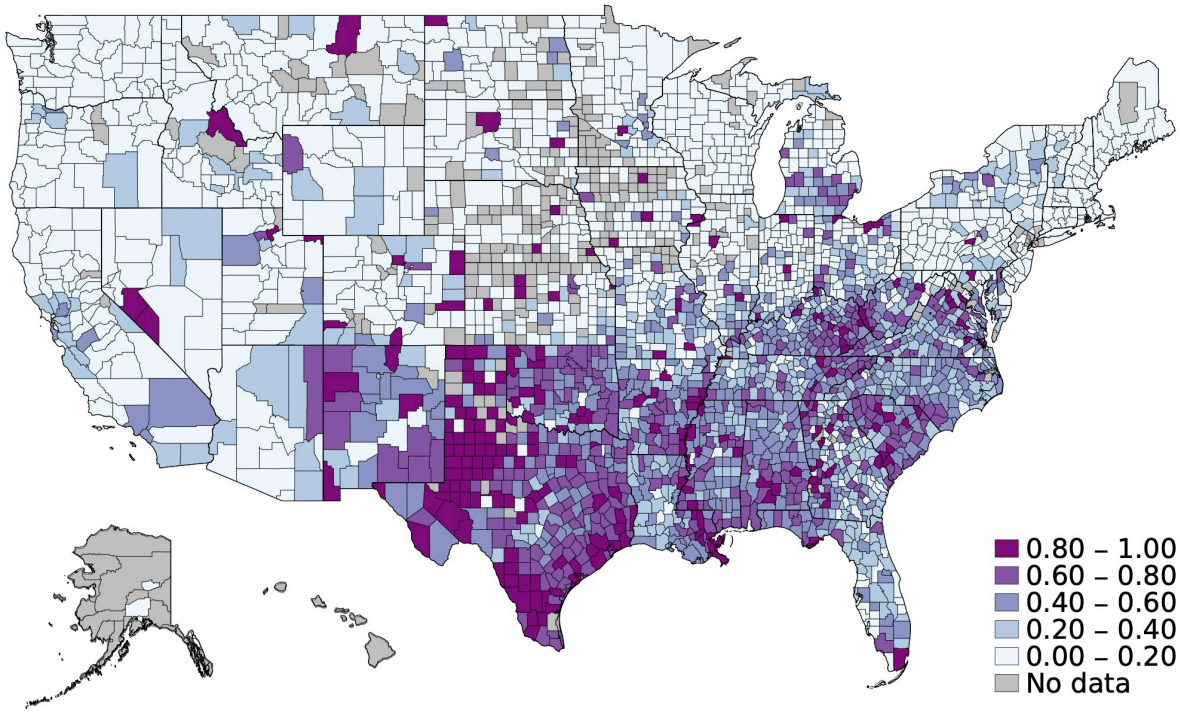


Figure 4: Change in loan volume plotted against CCAR performance from Federal Reserve Board's public CCAR results in the "Supervisory Severely Adverse" scenario. I use the preferred measure for stress test exposure from Cortes et al (2020), which is the minimum of the distances between each of the tier 1 capital ratio, total risk based capital ratio, leverage ratio and their regulatory minimums. The coefficient on the regression line shown is β =9.59 and has a p-value of 0.035. For consistency, I only use the loans of banks that were stress tested under the first round of CCAR. Each observation is a bank-year. In order to minimize the effect of loan changes for banks making a very small number of loans, I only include changes in bank-years that started with at least 100 loans. In practice, this means that the included banks are BB&T, Fifth Third, PNC, Suntrust, US Bank and Wells Fargo.

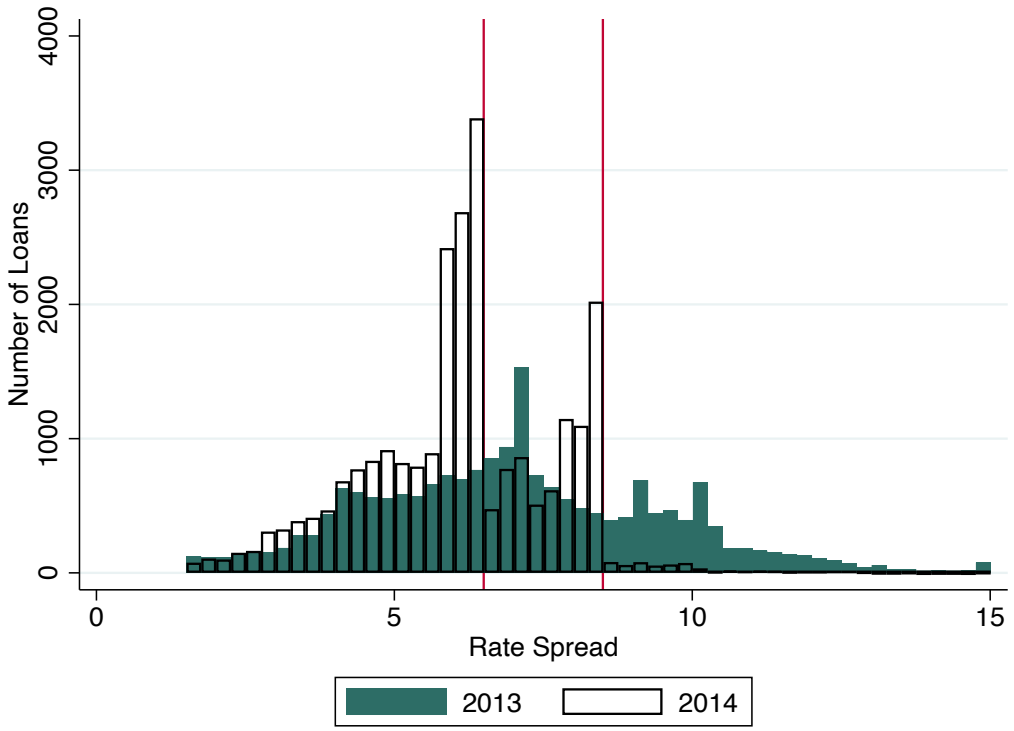


((a)) 2006

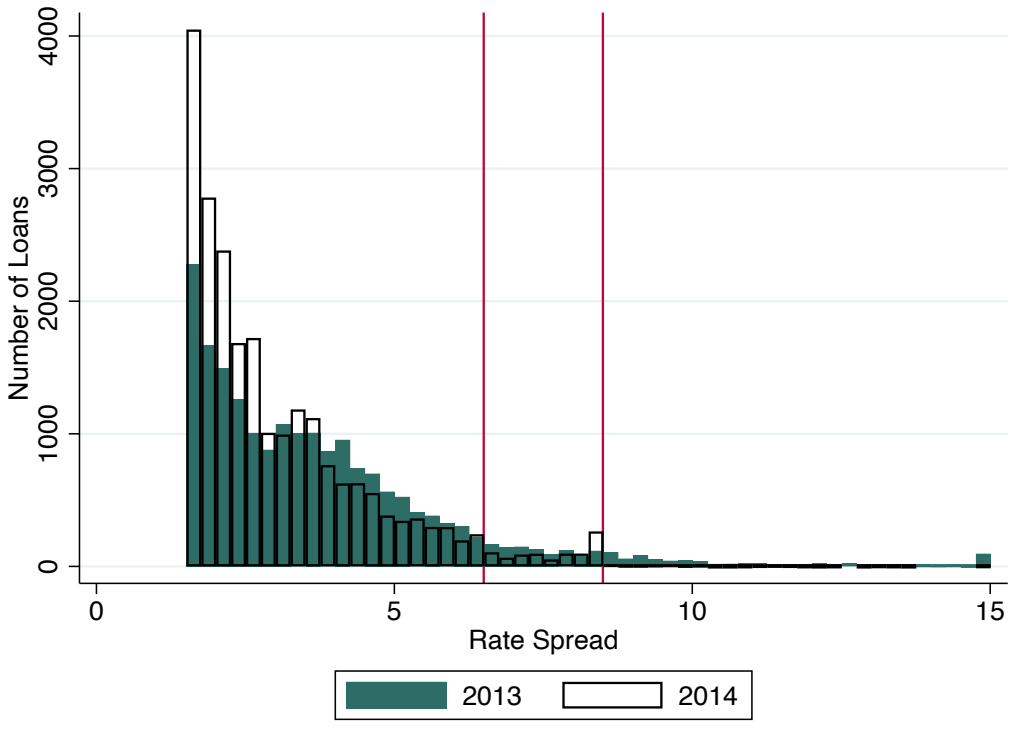


((b)) 2013

Figure 5: Proportion of manufactured home loans made by the dominant firm in each county in 2006 and 2013. Source: Calculations from HMDA.

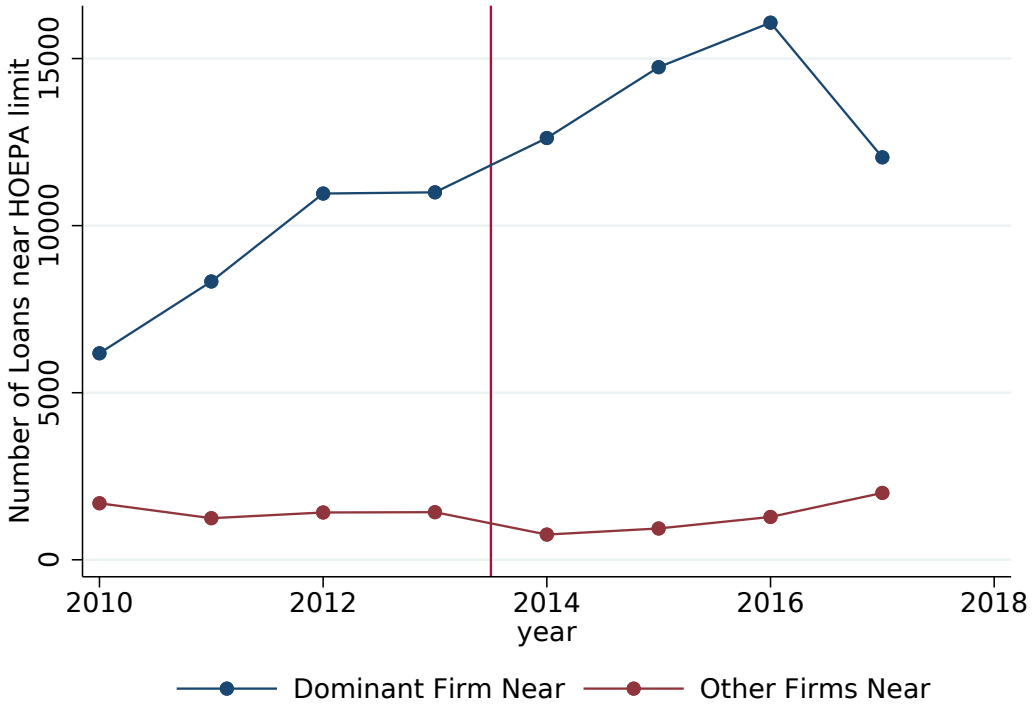


((a)) Dominant Firm Only

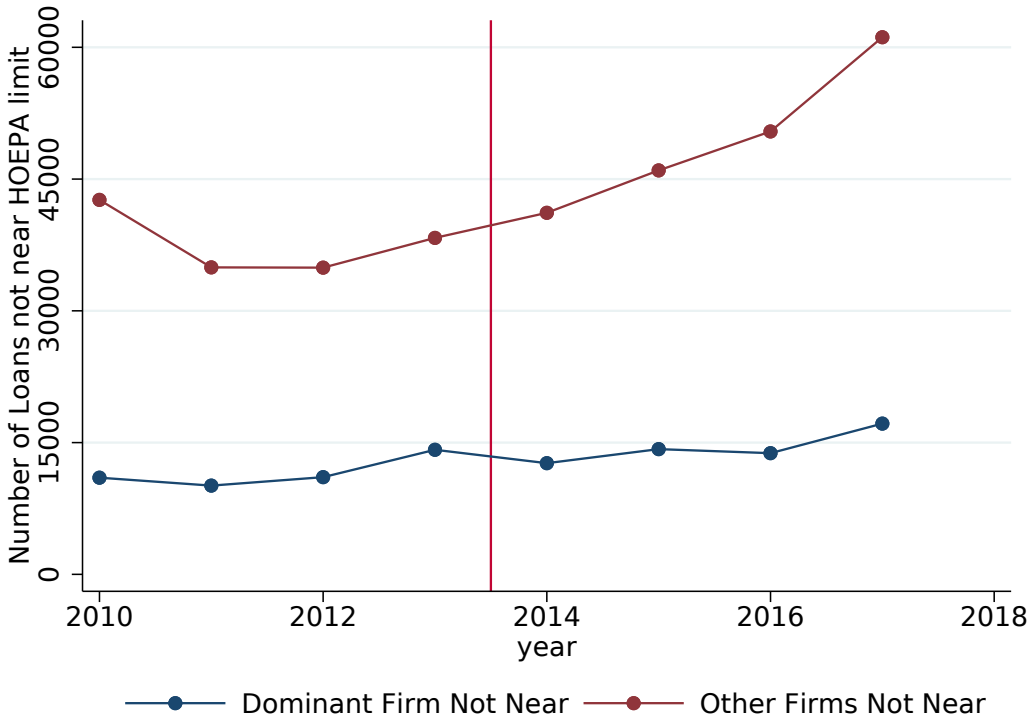


((b)) All Other Firms

Figure 6: Rate spreads of loans made before and after HMDA. The two rate limits are shown in red at 6.5% and 8.5%. Source: Calculations from HMDA.



((a)) Near HOEPA rate spread limit



((b)) Not Near HOEPA rate spread limit

Figure 7: Number of loans made each year. Loans that have rate spreads of 75 basis points below the rate limit or higher are classified as "near" the rate limit.

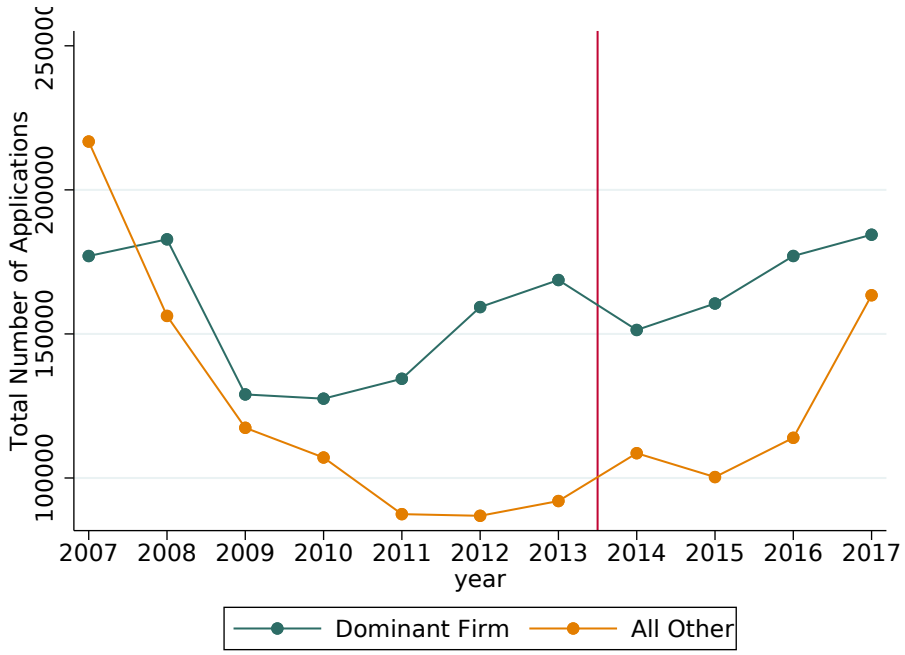


Figure 8: Number of loan applications over time. Source: Calculations from HMDA.

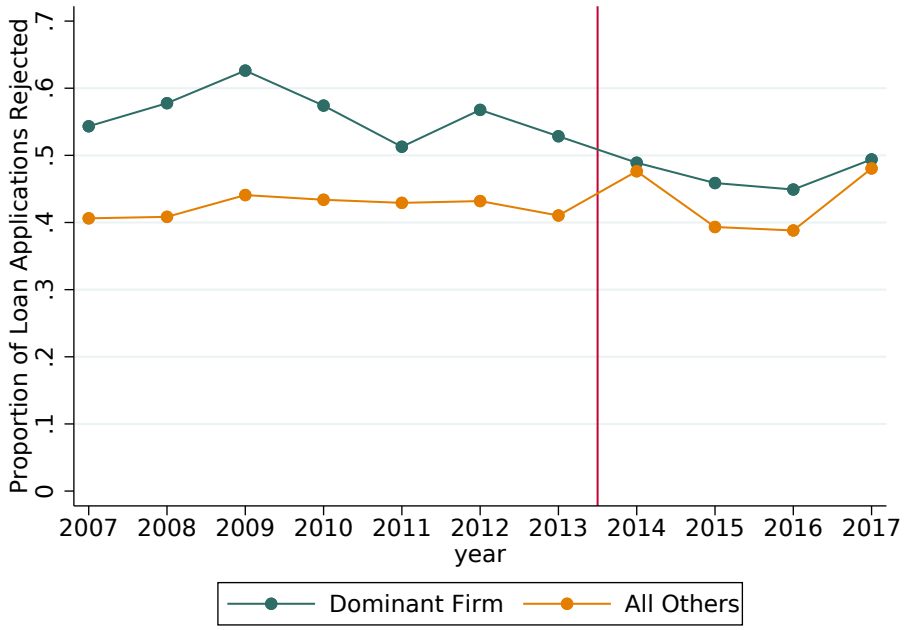


Figure 9: Proportion of loan applications rejected over time. Source: Calculations from HMDA.

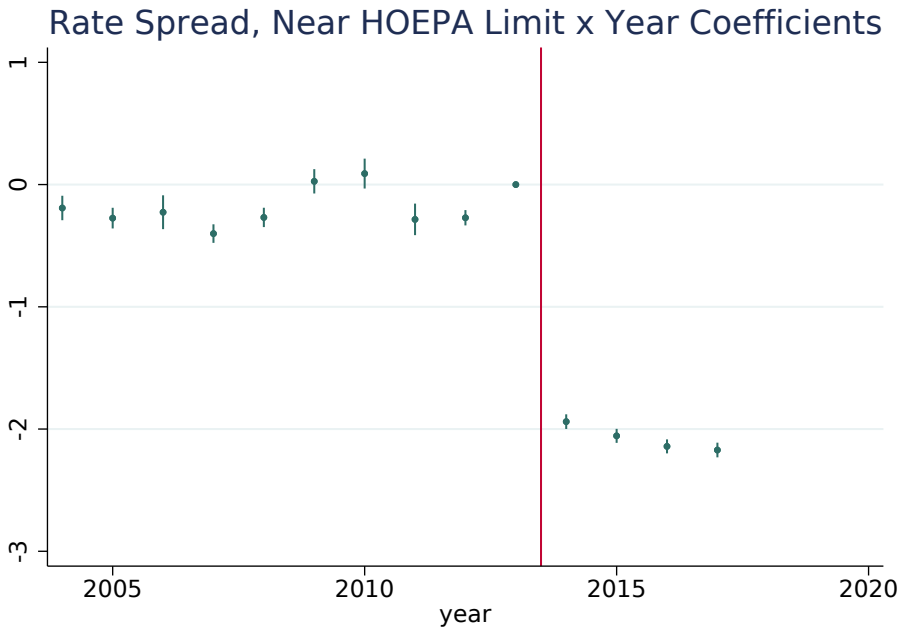


Figure 10: Proportion of loan applications rejected over time. Source: Calculations from HMDA.

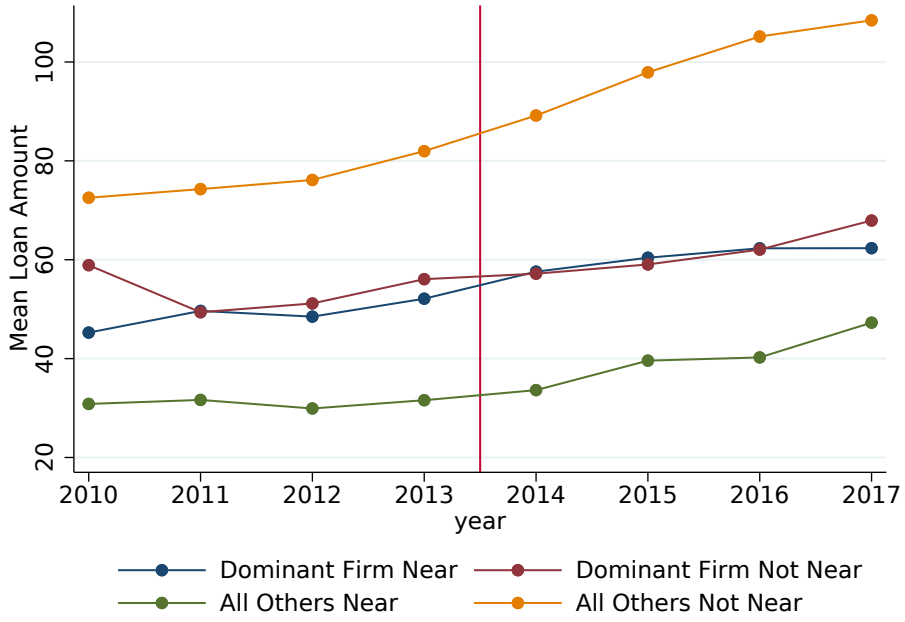
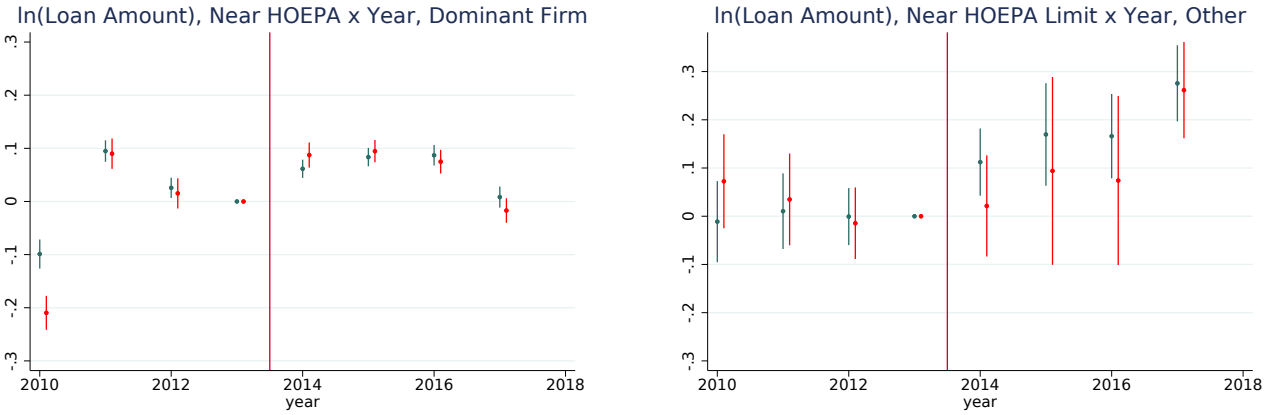


Figure 11: Mean loan amounts over time. Loans that have rate spreads of 75 basis points below the rate limit or higher are classified as "near" the rate limit. Source: Calculations from HMDA.



((a)) Dominant Firm

((b)) All Other Firms

Figure 12: Loan Amount Event Studies. Red Coefficients=No Controls or Fixed Effects, Green Coefficients=With Controls and Fixed Effects. Bars show 95% confidence intervals, standard errors clustered at the county level.

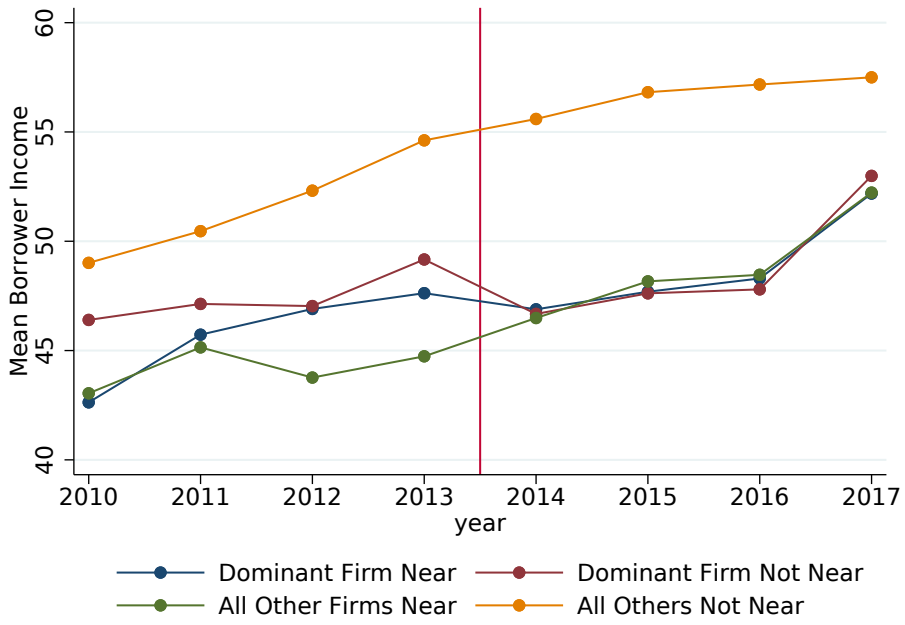
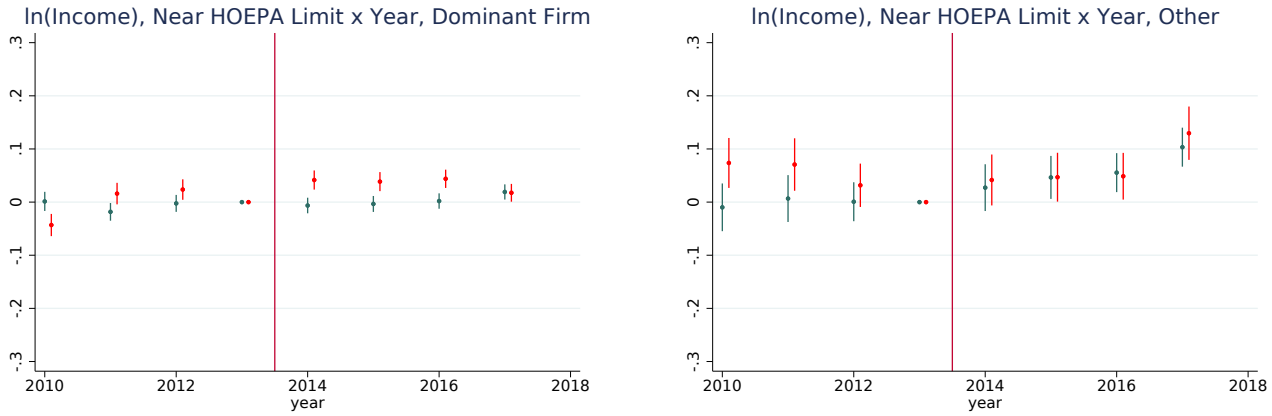


Figure 13: Mean borrower incomes over time. Loans that have rate spreads of 75 basis points below the rate limit or higher are classified as "near" the rate limit. Source: Calculations from HMDA.



((a)) Dominant Firm

((b)) All Other Firms

Figure 14: Income Event Studies. Red Coefficients=No Controls or Fixed Effects, Green Coefficients=With Controls and Fixed Effects. Bars show 95% confidence intervals, standard errors clustered at the county level.

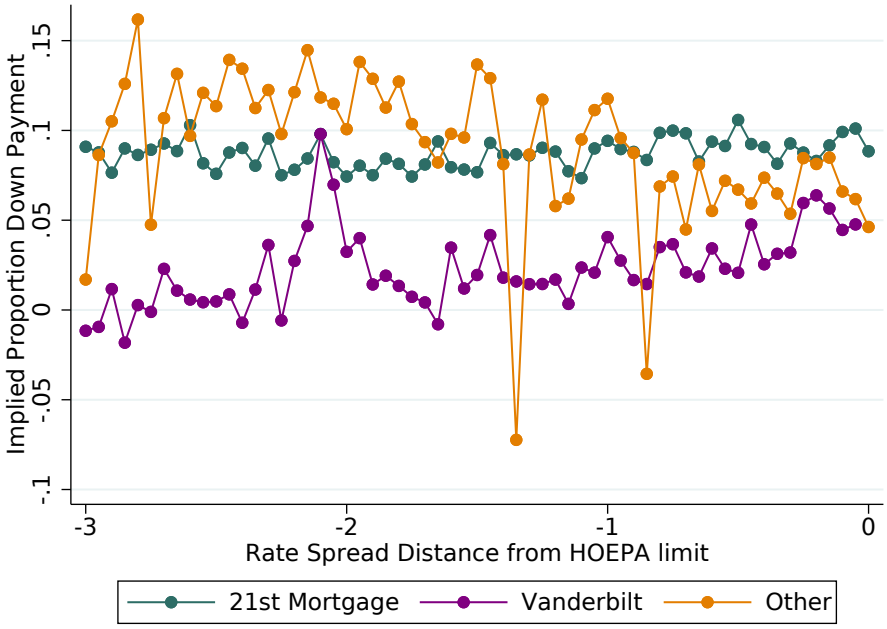


Figure 15: Proportion of property value not borrowed in mortgage by rate spread distance from the HOEPA limit. Source: Calculations from 2018-2019 HMDA.

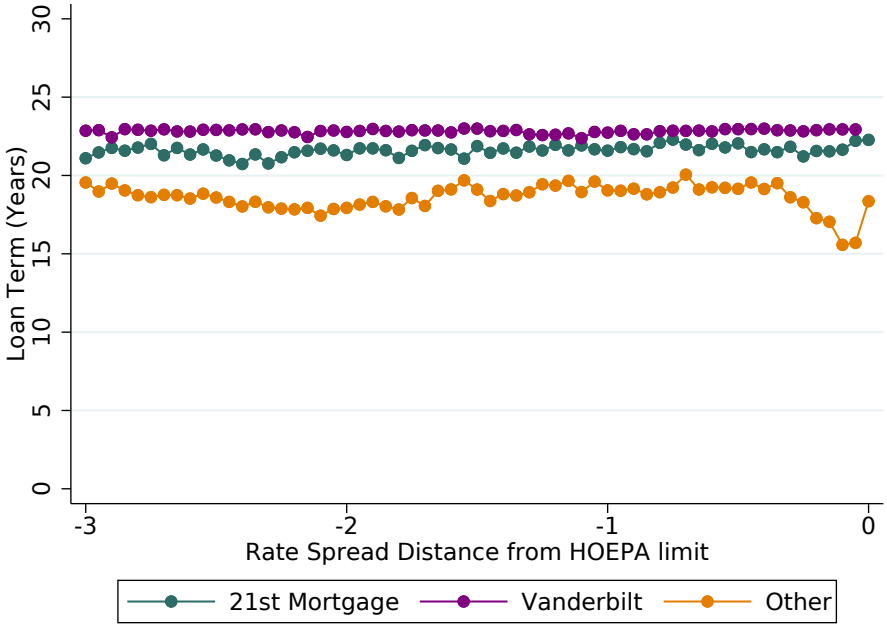


Figure 16: Mean loan term by rate spread distance from the HOEPA limit. Source: Calculations from 2018-2019 HMDA.

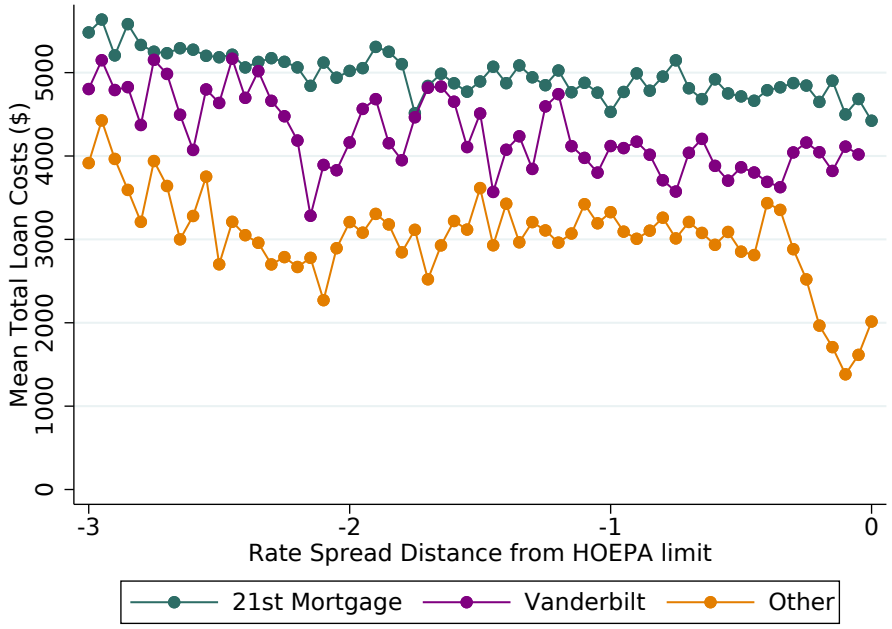


Figure 17: Total loan costs by rate spread distance from the HOEPA limit. Source: Calculations from 2018-2019 HMDA.

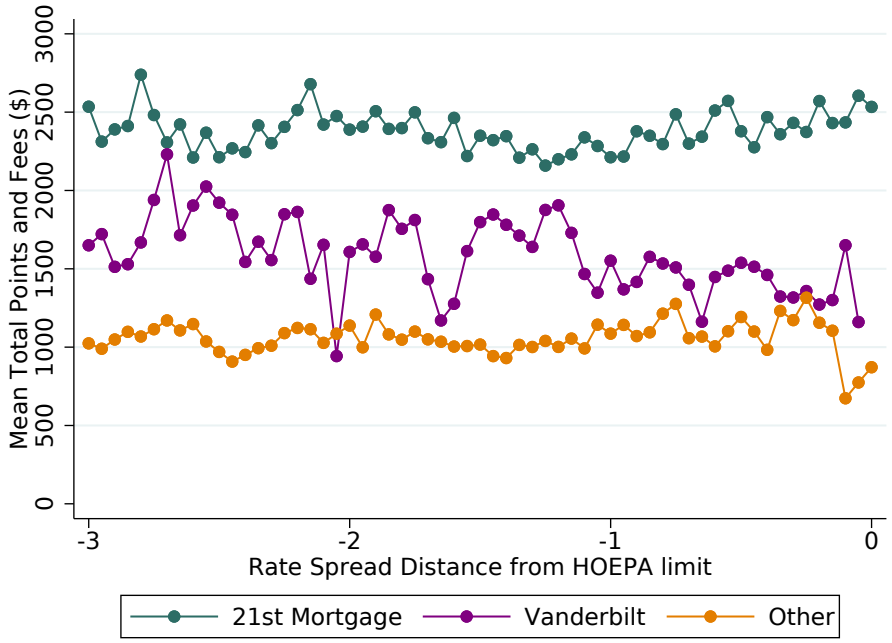


Figure 18: Total points and fees by rate spread distance from the HOEPA limit. Source: Calculations from 2018-2019 HMDA.

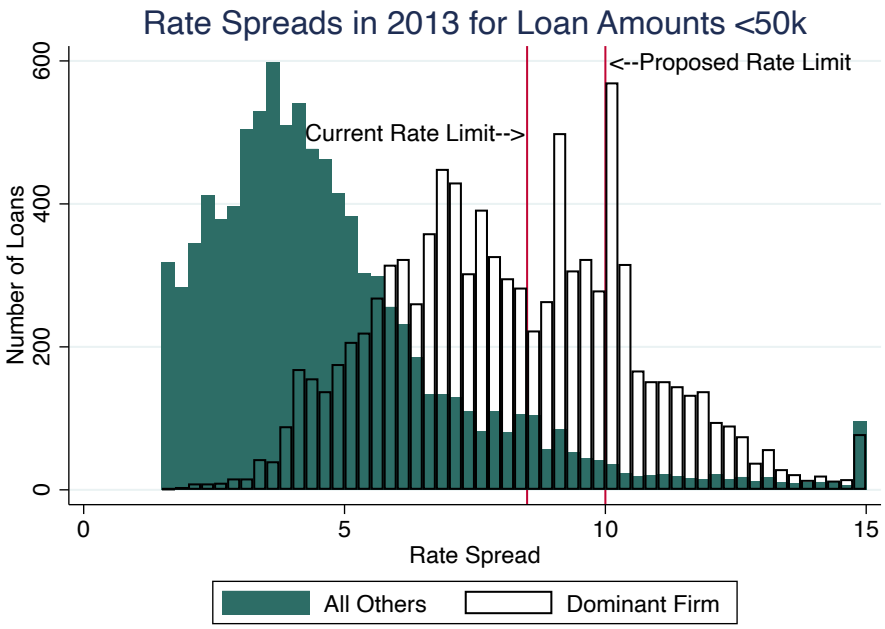


Figure 19: Loans of less than $50,000. Source: Calculations from HMDA.

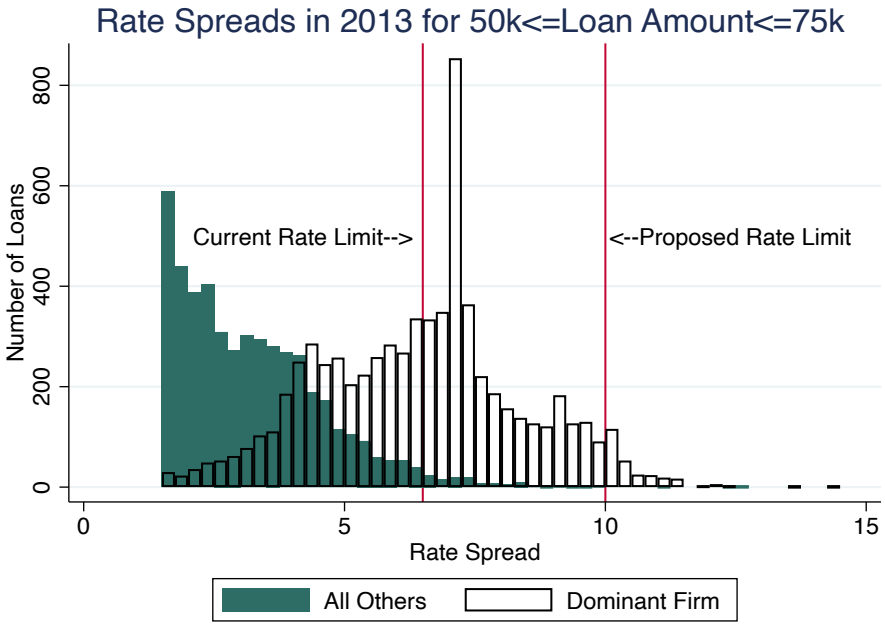


Figure 20: Loans from $50,000 to $75,000. Source: Calculations from HMDA.

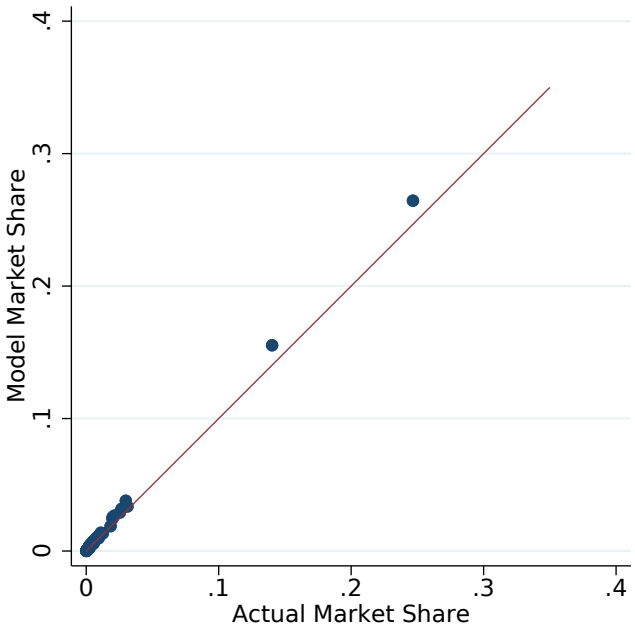


Figure 21: Actual vs Model Market Shares

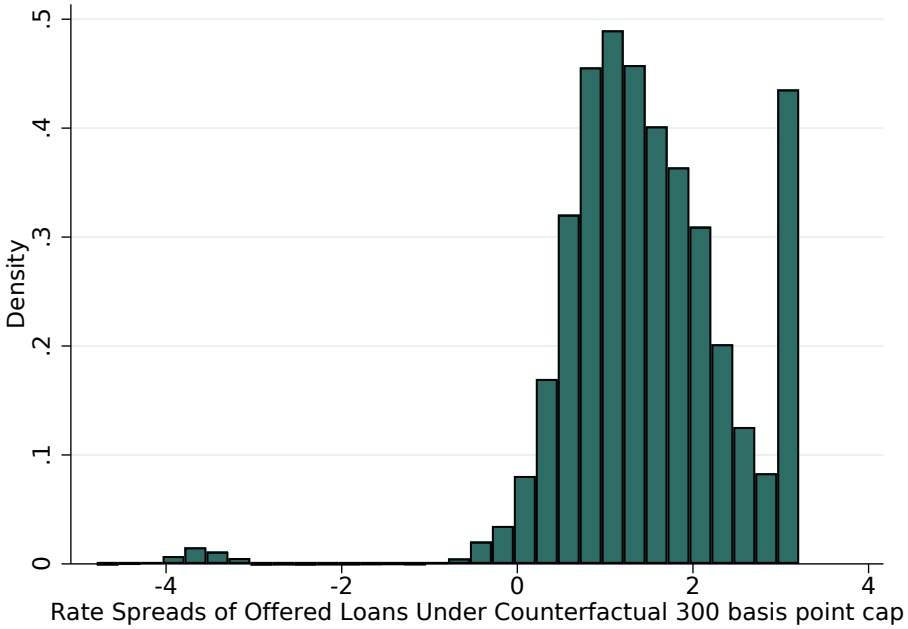


Figure 22: Distribution of counterfactual rate spreads offered under 300 basis points rate spread cap.

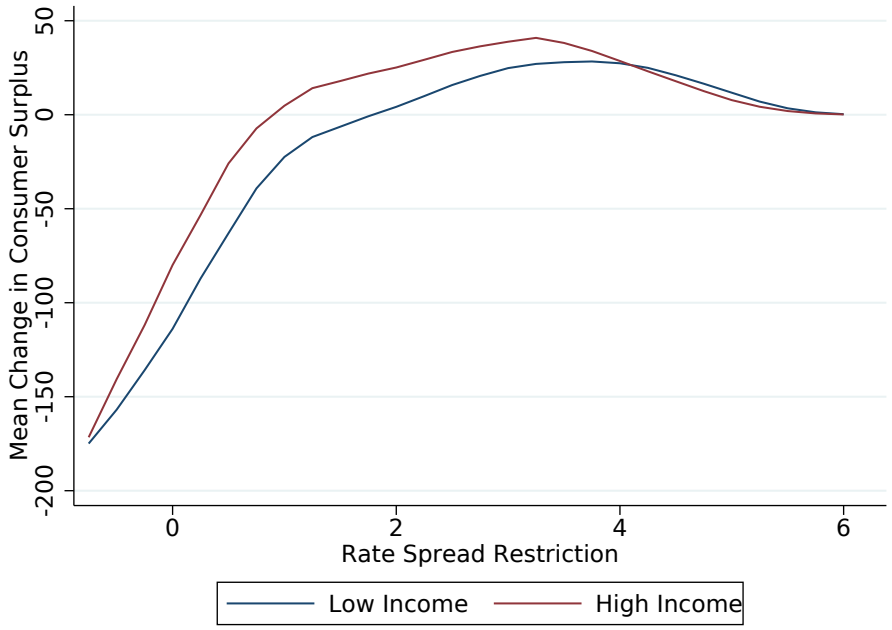


Figure 23: Average consumer surplus change varying the rate limit.

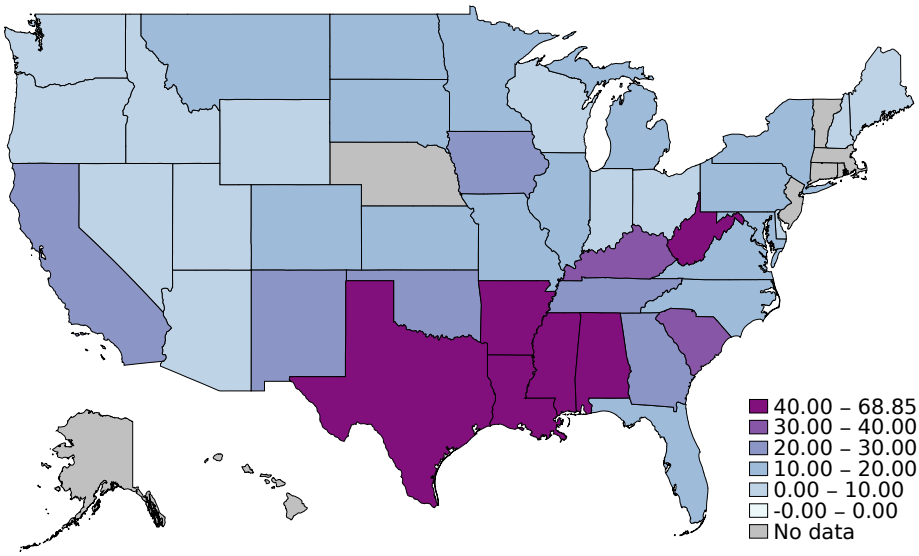


Figure 24: Average consumer surplus change under a 400 basis point rate spread limit for below-median income borrowers. I exclude states with fewer than 100 borrowers.

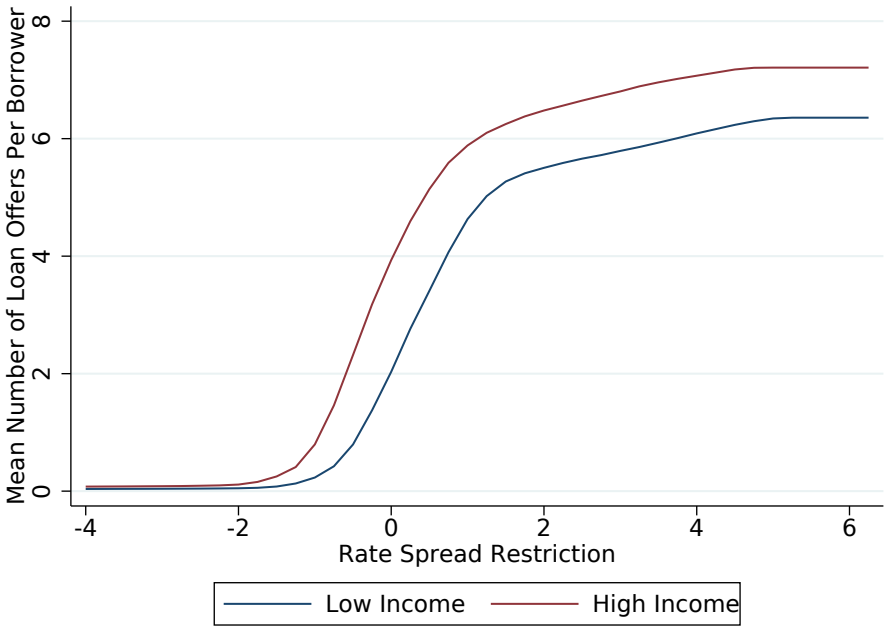


Figure 25: Mean number of loan offers per borrower under counterfactual levels of a rate spread limit.

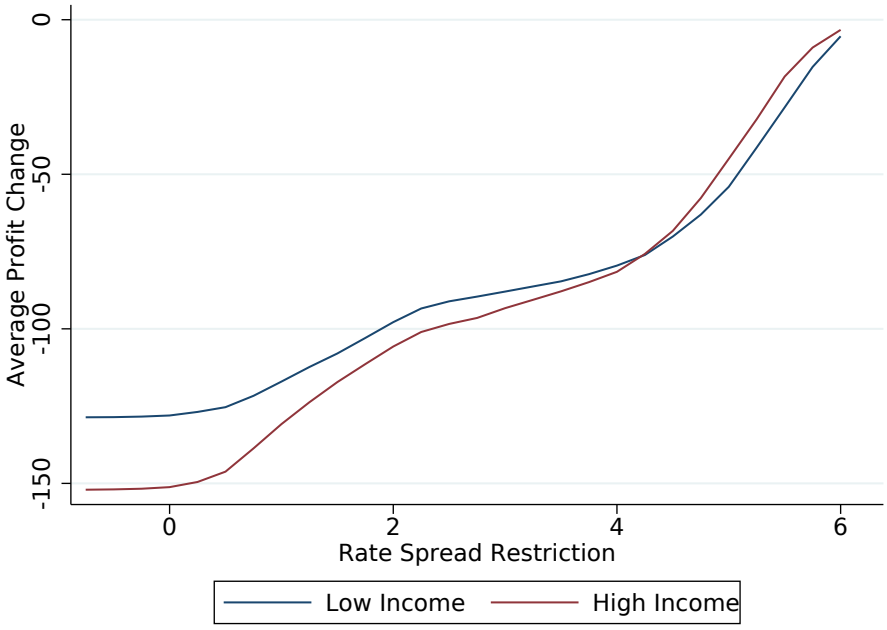


Figure 26: Mean firm profit per loan under counterfactual levels of a rate spread limit.

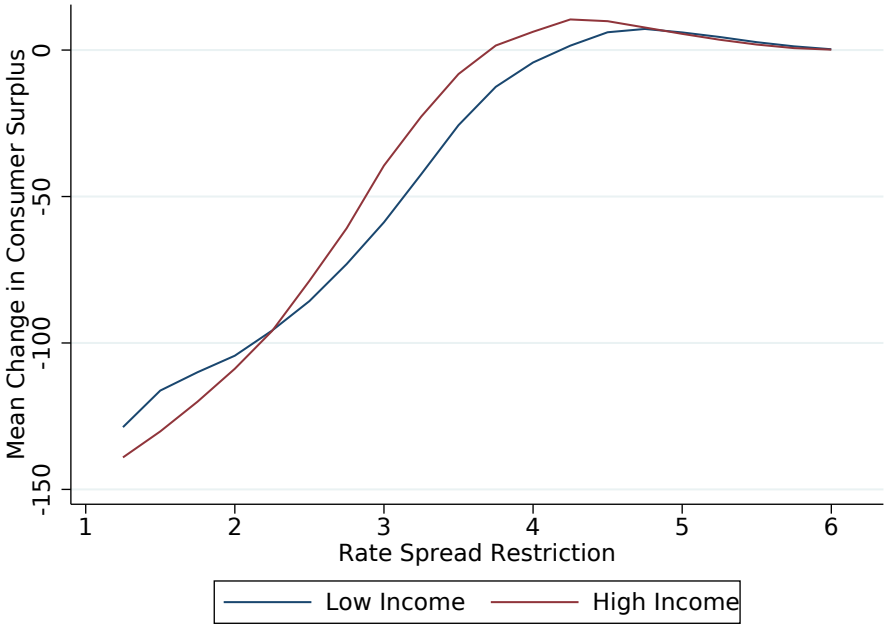


Figure 27: Average consumer surplus change varying the rate limit with markups halved.