Algorithmic Underwriting in High Risk Mortgage Markets*

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Abstract

We study the effects of a policy that increased reliance on algorithmic underwriting for low-credit-score, high-leverage mortgage borrowers. Using a bunching-based approach, we document a large policy-induced credit expansion among affected borrowers, with little changes in default risks given observables. The credit expansion is larger among non-Hispanic White borrowers and higher-income borrowers. Post-policy, low-credit-score individuals are more likely to move to better-rated school districts. A structural approach helps quantify the welfare implications of the policy. Our results suggest a limited role of human discretion for most borrowers in this market and highlight challenges in increasing financial inclusion in certain groups.

Keywords: Algorithmic Underwriting, FinTech, Household Leverage, Racial Inequality in Mortgage Markets, Mobility, Financial Inclusion.

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1 Introduction

Policies targeting household lending must carefully balance the benefits of expanding financial inclusion for high-risk borrowers with the costs associated with increased default (Layton, 2023). Better access to mortgage markets for these borrowers can help reduce disparities in homeownership rates (Eggers, 2001). However, extending loans to high-risk borrowers could also elevate the risk exposure of financial institutions and government agencies. A crucial component that influences this trade-off is loan underwriting, where lenders collect and verify applicants' documents, assess their credit risks, and decide on loan approval. While a task traditionally performed by humans, underwriting has become increasingly automated over the past decades. By the mid-2000s, nearly all lenders had used automated underwriting systems (AUS) in some aspects of their lending practices (Wells, 2023).

How does the increasing reliance on algorithmic underwriting affect the tradeoff between financial inclusion and risk management? The prediction is not obvious *a priori*. On the one hand, algorithmic underwriting faces limitations in collecting and interpreting soft information, which may affect its ability to evaluate the credit risks of borrowers with unconventional income and opaque credit history. On the other hand, algorithms are potentially less prone to errors and more insulated from agency conflicts. Despite the prevalence of AUS, limited empirical evidence exists regarding its role in high-risk market segments.

This paper studies the consequences of increased reliance on algorithmic underwriting in a low-credit-score, high-leverage segment of the US mortgage market. We examine the effects of a policy change implemented by the Federal Housing Administration (FHA) in August 2016, which targets borrowers with credit scores below 620 and debt-to-income (DTI) ratios above 43%. Before this date, the FHA mandated manual underwriting for all such borrowers; after August 2016, the FHA allowed AUS to approve loans directly without human underwriting. We study the policy's impact on loan quantities, performance, prices, and household mobility. We find that the policy led to a substantial expansion of credit for affected borrowers with little change in their delinquency rates. The effects are larger among White and higher-income borrowers compared with Black and lower-income borrowers. The credit expansion leads to real effects: Low-credit-score borrowers are more likely to relocate to areas with better-rated public schools. These results support the notion that increased utilization of algorithmic underwriting can improve financial inclusion in markets otherwise excluded by lenders while effectively managing credit risk exposure, leading to real welfare benefits. However, our findings also highlight challenges associated with improving financial inclusion equally across racial and income groups.

We assemble a large dataset to address our research questions, starting with individual loan-level data provided by the Government National Mortgage Association ("Ginnie Mae"). This database covers the near-universe of FHA-insured loans, and includes information on loan contract terms such as interest rates, amount, maturity, and purpose; borrower and property information such as the locations of purchased properties, borrower credit scores, and debt-to-income ratios; and importantly, information on loan delinquency. We merge this data with the Home Mortgage Disclosure Act (HMDA) data using the FHA endorsements as the intermediate link. This merge allows us to observe borrower income and race/ethnicity. We track the residential location of individuals from a 1% randomized sample from Experian to measure household mobility. Finally, we obtain information from GreatSchools.org regarding the current rating of school districts and use it as a metric for the quality of neighborhoods.

We begin by analyzing changes to the quantity of loans around the adoption of the policy. Initial analysis suggests a substantial increase in the number of loans issued to affected borrowers, who have credit scores below 620 and DTI ratios above 43. We then employ a counterfactual estimation approach to draw causal inferences regarding the effects of the regulation change (DeFusco et al., 2020). This approach utilizes high-credit-score borrowers and low-credit-score borrowers with very low DTI, who are unaffected by the policy, as the control groups, and uses the changes in the DTI distribution among this group as the counterfactual for the changes among the affected group. Using this approach, we find that the policy reform increases the total quantity of loans taken out by low-credit-score borrowers by 10.3%. We validate that the control group generates a reasonable counterfactual distribution by using a placebo analysis set in a year with no policy change.

Given the large increase in credit quantity, a question naturally arises as to whether the increased reliance on algorithmic underwriting increases borrowers' default probabilities. To answer this question, we first adopt a difference-in-differences method, comparing the changes in delinquency rates following the policy event between treated (low-credit-score) and control (high-credit-score) borrowers. We make this comparison separately for loans above ("high-DTI") and below ("low-DTI") the DTI cutoff of 43%. Despite a baseline default rate of 5.9%, we do not find evidence that delinquency rates increase more for low-credit-score loans following the policy reform, either for high-DTI or low-DTI loans. We then use a triple-difference framework, comparing the differential effect of the policy on the delinquency rates of low-credit-score, high-DTI loans relative to all other groups. Again, the delinquency effect is not statistically different from zero. A remaining concern is that AUS may grant credit to "fragile" borrowers, who are prone to defaults when economic conditions worsen. To address this concern, we show that delinquency rates do not increase even in areas with the largest increase in unemployment rate during the sample period.[1] The results are also robust to alternative measures of delinquency rates, including less severe delinquencies and delinquencies over longer horizons including the COVID-19 forbearance period. Combined, these results suggest that an increased reliance on algorithmic underwriting need not be associated with an increase in default risk for loans granted within a DTI category.

We note that, while average delinquency rates within each DTI bin did not increase, the policy led to

[1]The recent literature suggests a majority of mortgage defaults are driven solely by negative life events (Ganong and Noel, 2023), which in part motivates our test based on changes in unemployment rates.

an influx of high-risk borrowers, which could increase the overall risk of FHA-insured loans. Through a back-of-the-envelope calculation, we infer that the policy increases the total dollar volume of FHA loans by 1.10%, and raises the average delinquency rates by 1.61%. To compensate for the increase in credit risk, the FHA could raise the annual mortgage insurance premium (MIP) by 1.82 bps for an average 7-year mortgage. This means that, while the policy improves the financial inclusion for higher-risk borrowers, it likely imposes some costs on lower-risk ones. We discuss the conditions under which these trade-offs can be examined.

We next explore how the policy-induced credit expansion varies across racial and income groups. This analysis adds to on an ongoing discussion regarding the potential disparate impact of algorithmic underwriting relative to human underwriting (Das et al., 2023). Despite the policy's focus on low-credit-score borrowers and FHA's prevalence among minority borrowers, we find that the overall increase in credit quantity is more pronounced among White borrowers and high-income borrowers, by 11% and 14%, respectively, but is weaker among Black and low-income borrowers, by only 1% and 4%, respectively. At the same time, there was no statistically significant increase in delinquency rates conditional on observables in any demographic groups. Later, we estimate the extent to which these effects come from credit supply and demand using a structural model, and discuss their respective potential origins. Overall, our results highlight the difficulty of increasing financial inclusion for minority and lower income individuals.

There can be several mechanisms for our credit expansion and delinquency results. First, it is possible that the risk profile of most borrowers in this market can be well characterized by hard information alone. Given that human underwriters collect and process additional information to assist risk assessment, our results suggest that this requirement to collect additional information, including soft information, did not significantly improve risk management. We refer to this as the "hard information" channel. Second, algorithmic underwriting can reduce lenders' concerns about FHA scrutiny, which may be particularly salient as False Claims Act litigation over delinquent loans is a significant concern for lenders over our time period (Frame et al., 2024). Since the algorithms are approved by the FHA, lenders may be less worried about potential pushback from the FHA on the loans they approve through algorithmic underwriting. We describe this as the "regulatory burden" channel. These two channels are not mutually exclusive: while the regulatory burden channel may account for the observed credit expansion, it alone cannot explain our delinquency results if the additional information collected by human underwriters significantly improves risk management for most borrowers in this market.

We also consider two alternative explanations for the credit expansion that are not directly related to underwriting decision-making. One is the "capacity constraint" channel, which suggests that algorithms alleviate the workload of human underwriters by directly approving loans, but do not make different decisions from humans. The other is the "lender entry" channel, whereby a heavier reliance on algorithms increases lender entry due to a reduction in expenses associated with hiring human underwriters. We design a few analyses to further explore these mechanisms.

First, we examine how loan denial rates change around the FHA policy. Using various data sources to approximate the credit scores and DTI ratios of loan applications, we show that loan denial rates for affected borrowers (low FICO and high DTI) significantly reduced following the FHA policy, even controlling for lender fixed effects. This result supports the argument that the increase in credit quantity is at least partially due to changes in underwriting decisions within lenders. Second, we directly evaluate the capacity constraint channel by examining how the credit expansion effect varies across lenders facing more and less capacity constraints. Under the capacity constraint channel, we should expect a larger credit expansions when lenders face a greater influx of loan applications, i.e., "lending congestion." We measure congestion based on the year-on-year growth in total mortgage application volume for a local loan market, defined by a lender-state. However, contrary to what this mechanism predicts, we find that the FHA policy change leads to greater credit expansion in *less* congested markets. Third, we directly assess the importance of the lender entry channel by comparing the share of low FICO and high DTI lending post-policy that was originated by existing versus new lenders. We find that 81% of low FICO and high DTI lending post-policy was originated by existing lenders, and only 19% by new lenders. As the volume of low FICO and high DTI lending expanded by over 100% post-policy, this result suggests that most of the credit expansion came from changing credit decisions of existing lenders rather than the entry of new lenders.

Finally, we assess the regulatory burden channel by examining the differential effects of the policy for lenders who are more and less prone to regulatory risks. We hypothesize that nonbank lenders face less regulatory concerns compared to bank lenders, as nonbank lenders have greater tolerance for risk and have significantly higher securitization rates compared to banks (Benson et al., 2023). Consistent with the predictions from the regulatory burden channel, we find that the FHA policy leads to larger credit expansions for low FICO borrowers among bank lenders (16%) compared with nonbank (11%) lenders.

Taken together, our findings so far are consistent with the hard information channel and the regulatory burden channel. As the last step of our reduced-form analysis, we examine the implications of the policy changes to borrowers, from two perspectives. We start by investigating the financial consequences, i.e., whether high-leverage borrowers experience a change in borrowing costs as a result of the policy change. We find no change in interest rates for high-DTI loans, and an economically small and statistically weak increase in interest rates for low-DTI loans. One potential explanation for this finding is changes in borrower composition: as higher-income borrowers increase leverage and move to the high-DTI category, lenders may consider the remaining low-DTI borrowers to be riskier than before, thus charging higher rates.[2]

We then explore the non-financial consequences of algorithmic underwriting for households. Specifically,

[2]While the reduced reliance on manual underwriting may have reduced labor cost for lenders, such cost-saving may be small due to the rigidity in the labor markets. Lenders' market power may also limit the pass-through of lower costs to loan pricing. Thus, we do not detect an increase among high-DTI borrowers.

we examine whether the policy-induced credit expansion increases household mobility to higher-quality neighborhoods, measured based on local school district quality. We focus on school quality because it can shape upward mobility and is often correlated with desirable neighborhood traits, such as low crime rates and good amenities (Restuccia and Urrutia, 2004). Using a two-stage-least-square (2SLS) framework, we find that low-credit-score individuals are more likely to obtain a new FHA mortgage, and the predicted increase in mortgage access in turn leads to an increase in school quality. These effects are obtained by benchmarking low-credit-score individuals against high-credit-score ones living in the same zipcode, with the same gender and age range after the policy change. The magnitude is economically meaningful. On average, school district ratings increased by approximately 1-2 points among compliers, equivalent to a shift from a 5-rated district to one rated between 6 and 7. We further conduct a placebo test using the subsample of renters to ensure that the results are not driven by other unobserved differences between the low- and high-credit-score groups during the sample period. Collectively, these results imply that mortgage access plays an important, long-lasting role in households' "moving to opportunity."

So far, we document that a greater reliance on AUS improves access to credit and neighborhood quality for low-credit-score borrowers without increasing average credit risk, and the effects vary across racial and income groups. However, our reduced-form analyses face limitations in quantifying the welfare consequences for borrowers and separating the effects of credit supply from that of credit demand. To overcome these limitations, we estimate a dynamic structural model. In this model, borrowers choose their mortgage loan sizes, and thus DTI, to maximize their expected utility given the interest rates and lenders' approval thresholds. By assuming that borrowers' demand for mortgages comes from a smooth parametric distribution which contrasts with lenders' approval rules that have sharp discontinuities, we can disentangle the policy-induced changes in credit supply from differences in borrower demand across sub-populations. We can also compute changes in consumer surplus under certain assumptions regarding the functional form. The key parameters are estimated by matching model moments with the empirical counterparts, including the DTI distribution with and without the manual underwriting mandate and the interest rate elasticity of mortgage demand. In this estimation, we also look at how credit supply changes for each of the racial and income groups, to shed light on the mechanisms driving the unequal benefits from algorithmic underwriting across groups.

Consistent with our reduced-form inferences, the structural estimations show that the removal of the manual underwriting mandate significantly increases the approval rates of high-DTI loans (i.e., credit supply) and improves consumer surplus. These effects are more pronounced for Non-Hispanic White and higher-income applicants compared to Black and lower-income ones, likely due to them having more favorable underwriting characteristics. At the same time, our estimates highlight the role of demand-side factors. Based on our model estimates, the discrepancy in supply accounts for only 32% the total difference in policy response across racial groups, and plays a bigger role in explaining the gap across income groups. Differences in credit demand, potentially due to liquidity or information frictions, explain a majority of the racial gap in take-up. Overall, our structural approach helps us decouple the changes in credit supply and demand driven by the FHA policy, and highlights the difficulty of increasing financial inclusion for high risk minority and lower income borrowers from both perspectives.

Our study contributes to several strands of literature. First, we add to the burgeoning literature analyzing the increasing use of technology in mortgage underwriting (Foote et al., 2019; Fuster et al., 2019, 2022; Johnson, 2023b; Das et al., 2023). Existing evidence suggests that human loan officers are influenced by volume-based incentives and errors (Tzioumis and Gee, 2013; Cortés et al., 2016; Giacoletti et al., 2023), while human risk managers can reduce delinquency (Berg, 2015). In comparison, Fuster et al. (2019) finds that FinTech lenders process loan applications faster and respond more elastically to demand shocks. However, their study focuses on FinTech lenders' advantages in collecting and verifying borrower information, without necessarily varying whether that information is subsequently processed by an algorithmic underwriting system or reviewed by a human underwriter who may gather additional hard and soft information. Other literature shows that increased lender automation can engender differential impact on the credit access across racial and gender groups, though the direction of the change depends on the context (Dobbie et al., 2021; Fuster et al., 2022; Bartlett et al., 2022; Erel and Liebersohn, 2022; Chu et al., 2023; Das et al., 2023; Howell et al., 2024). We complement this literature by examining the effect of algorithmic underwriting when human judgment is still present and used as a complement. Our results suggest that under some extent of human supervision, algorithmic underwriting can simultaneously allow for a large increase in credit supply with little change in loan default probabilities in the low-credit-score, high-leverage segment of mortgage markets. However, consistent with earlier evidence, we find that not all borrower groups benefit equally from this credit expansion.

Second, our paper is related to the broader literature on algorithmic underwriting in financial intermediation. In particular, in the auto loans market, Jansen et al. (2024) use a randomized experiment to show that algorithmic underwriting outperforms human underwriting for riskier and more complex auto loans.[3] Costello et al. (2020) study the implications of AI-based lending models for trade creditors. In a developing country context, Tantri (2021) and D'Acunto et al. (2022) study the role of algorithms in increasing efficiency and reducing discrimination in personal loans and peer-to-peer loans, respectively. We study the role of human-augmented algorithmic underwriting in the U.S. mortgage market, which has been particularly controversial due to its size and importance (Fuster et al., 2022; Das et al., 2023). Our findings in this market can inform a broader set of consequences of algorithmic-based lending for households and government agencies, such as the capacity for financial inclusion of high-risk borrowers and their subsequent location choices, the risks borne by government agencies, and the distributional consequences of the credit expansion across

[3] Jansen et al. (2024) find that algorithms approve fewer auto loans, but charge higher interest rates and are associated with lower default rates. In contrast, we find algorithm underwriting is associated with significant credit expansion but little change in interest rates or delinquency in the mortgage market.

income and racial groups.

Finally, our paper complements and expands the existing literature on the effects of household leverage policies. DeFusco et al. (2020) show that the Dodd-Frank "Ability-to-Repay" rule, which imposes restrictions on high DTI lending, led to a reduction in credit supply but had limited effects on mitigating default risks. Following their methodology, we analyze bunching behaviors around regulatory thresholds. Other studies based in the U.S. suggest that DTI restrictions not only directly affect house prices, but also generate spillover effects on groups that fall outside the established limits (Foote et al., 2010; Greenwald, 2018; Johnson, 2020, 2023a). Beyond the U.S. context, several studies further examine the implications of household leverage regulations for housing choices, household leverage, mortgage credit supply, and house prices (Van Bekkum et al., Forthcoming; Kinghan et al., 2022; Acharya et al., 2022; Tzur-Ilan, 2023; Laufer and Tzur-Ilan, 2021). Unlike the policies studied in prior work, the policy we analyze emerges from the variation in the relative weight of algorithms and human involvement in the underwriting process.

2 Institutional Background

To qualify for FHA insurance, mortgage lenders must abide by the FHA underwriting guidelines. The guidelines stipulate that all transactions, with certain exemptions, must be scored through the Technology Open To Approved Lenders (TOTAL) Mortgage Scorecard (see FHA Single Housing Policy Handbook 4000.1, Section II (A) (4)). The TOTAL Mortgage Scorecard is an algorithm introduced by the U.S. Department of Housing and Urban Development (HUD) in 2000 to assess the creditworthiness of mortgage applicants and predict mortgage default. The Scorecard takes over a hundred data elements as input, including the applicant's monthly income, house appraised value and sale price, loan amount, loan-to-value ratio, front-end and back-end DTI ratios, and more.[4] The Scorecard is designed to streamline the underwriting process and provide lenders with a quick and consistent evaluation of borrowers' creditworthiness.

The TOTAL Scorecard provides two process classifications: "Accept" or "Refer." Accept implies that the system determines that the borrower meets the FHA's underwriting guidelines and is eligible for an FHA-insured loan. This means the borrower's application can move forward in the approval process. Refer means that the information provided by the borrower is not sufficient for the system to make a clear decision. This occurs when the automated underwriting system finds the borrower eligible but cannot determine an approval. In such cases, a human underwriter must manually underwrite the loan and gather additional documentation to make a final decision. Our study in part evaluates the value of this additional information.

The manual underwriting process involves more human discretion. For borrowers with opaque credit

[4]A complete list of data elements can be found in the Appendix A of the AUS Developer's Guide: https://apps.hud.gov/pub/chums/aus-developers-guide-SOAP-MISMO.pdf (accessed April 2024).

histories or unconventional income sources, human underwriters can exercise judgment and are potentially more flexible than algorithms. For instance, for borrowers without a credit score, underwriters could rely on non-traditional credit reports or independently develop the borrower's credit history.[5] Borrowers also have a chance to explain how they intend to repay. Underwriters may approve an application if they deem the credit risks associated with the application acceptable. Meanwhile, human underwriters may reject applications when borrowers' documents overrate their income potential or under-represent their risk.[6] The manual underwriting process can take several weeks to complete, much longer than does automated underwriting.

Following the financial crisis, regulators have increased their focus on risk management in the US mortgage market, including the creation of Dodd-Frank Act provisions targeting household leverage (DeFusco et al., 2020). Consistent with this trend, effective April 2013, HUD updated the TOTAL Mortgage Scorecard to include a manual underwriting mandate for FHA borrowers with credit scores below 620 and a debt-to-income ratios exceeding 43.00% (Mortgagee Letter 2013-05). This change meant that borrowers falling into this category could not receive an "Accept" recommendation from the TOTAL Scorecard but would be downgraded to a "Refer" scoring recommendation, requiring any such FHA loan origination to have undergone human underwriting. However, this policy had little practical effect because FHA loans with credit scores below 620 were already rare following the financial crisis, likely due to the FHA's rules in evaluating lenders.[7] In August 2015, the FHA implemented a Supplemental Performance Metric that made it more feasible, in principle, for lenders to originate loans to low-credit-score borrowers.[8]

The manual underwriting mandate was lifted in August 2016 for FHA borrowers with credit scores below 620 and DTI ratios above 43%, likely due to considerations of financial inclusion.[9] Under the revision, borrowers in this category could once again receive "Accept" recommendations from the TOTAL Scorecard if they were determined to be creditworthy by the automated underwriting system. The TOTAL Scorecard Version 3 underwriting algorithm, which is machine-learning based, was applied throughout our study period, and no major changes to the underwriting algorithm occurred during our study period.[10] Anecdotal evidence suggest that the machine-learning algorithm in question may be a logistic regression (Fuster et al., 2022). We study the effects of the expanded use of algorithmic underwriting in August 2016 on credit supply and default risk. This policy change only affected highly levered, low-credit-score borrowers. Borrowers

[5] See FHA's Office of Single Family Housing Training Module 4, accessed on July 31, 2023: `https://www.hud.gov/sites/documents/FY16_SFHB_MOD4_UNDER.PDF`.

[6] See FHA's Training Module referenced in Footnote 5.

[7] See a description of the problem facing low credit score borrowers HERE and FHA's request for comments HERE.

[8] See the policy fact sheet HERE.

[9] See the description of the policy change HERE and the corresponding FHA TOTAL Scorecard update on page E-17 HERE. As described in the first reference, in March 2019, the FHA partially reinstated this policy by referring more high risk borrowers, especially those with multiple risk factors, to manual underwriting, but the volume impact of this partial reinstatement to credit score under 620, DTI over 43 borrowers was small as can be seen in Figure 1.

[10] See a description of the TOTAL scorecard and its changes HERE. The updates to its algorithm occurred in 2008, when version 2 was introduced, and in 2012, when version 3 was introduced.

whose credit scores above 620 and DTI significantly below 43 were not affected and can serve as the "control groups" in our analysis.

There are limited alternative mortgage options available to our treated group of low-credit-score borrowers during our sample period. GSE mortgages are not available to this group of borrowers. Subprime private label secularization was common before the financial crisis but their volume has fallen sharply in 2007-2008 (Frame et al., 2021). While in theory portfolio lending is a possible alternative to FHA lending, Kim et al. (2024b) shows that such lending is minimal for low-credit-score or highly levered borrowers. This means that any changes in FHA credit that we measure likely capture the overall changes in mortgage credit available to low-credit-score borrowers.

Throughout our study period, lenders have an incentive to screen borrowers against their default risk. First, in the event of an FHA borrower delinquency, the cost of loan servicing can rise significantly.[11] Second, if lenders submits claims to the FHA for reimbursement for defaults, they run into the risk of the FHA discovering underwriting mistakes on the defaulted loans and holding them liable for the damages (see Parrott and Goodman (2019); Frame et al. (2024)). These institutional details imply that lenders are averse to borrower default.

3 Data and Variables

3.1 Ginnie Mae-HMDA Matched Sample

Our analysis primarily relies on a Ginnie Mae-HMDA matched sample. Ginnie Mae guarantees timely principal and interest payments for FHA-insured mortgages and publicly discloses the origination and performance of loans included in its MBS issues starting in September 2013. The Congressional Budget Office (CBO) estimates that the Ginnie Mae MBS issues make up about 97% of FHA insured mortgages.[12] The loan-level disclosure data we use is comparable to the data compiled by eMBS, which has been used in a number of recent studies including Fuster et al. (2021) and Kim et al. (2024a), with the latter describing it as is "essentially [...] the entire universe of FHA and VA mortgages."

The Ginnie Mae loan-level database contains a rich set of underwriting information including the debt-to-income ratio, credit score, property type, and loan purpose. Loan characteristics including the interest rate on the mortgages, the upfront and annual mortgage insurance premium (MIP), the loan amount, loan

[11] As explained in Goodman (2014): "The costs of servicing delinquent loans are much higher than the costs of servicing performing loans. [...] According to MBA estimates, non-reimbursable costs and direct expenses associated with the FHA's foreclosure and conveyance policies were two to five times higher than for GSE loans, even before the GSEs changed their compensatory fee schedule. In 2013, the annual cost of servicing a nonperforming loan was on average 15 times that of servicing a performing loan—$2,357 versus $156."

[12] See the breakdown HERE.

term, whether the mortgage is fixed-rate or an ARM, and the month of origination are also observed in the data. Furthermore, it contains information about the delinquency status of the mortgages.

We focus on new purchase mortgages and exclude streamline refinances, which have missing debt-to-income ratio and credit scores in the Ginnie Mae data. We further restrict the sample to fixed-rate, single-family, non-manufactured housing mortgages, which is the predominant form of FHA-insured mortgage lending during our sample period.

A limitation of the Ginnie Mae data is that it does not include information about the income of the borrower, the borrower's geographical location beyond state, or the borrower's race and ethnicity. We obtain these variables from the 2013–2017 Home Mortgage Disclosure Act (HMDA) data, and merge such information with the Ginnie Mae data via the publicly available FHA Single-Family endorsements data. Our matching process relies on variables such as the interest rate on the mortgage, the month of the endorsement and the property zip code. Details of this data and the matching procedure are provided in Appendix A.1.

The merged Ginnie Mae-HMDA database allows us to examine the change in origination volume in months around the FHA policy change. Our analysis focuses on the two-year window centered around August 2016, i.e., August 2015 to August 2017, excluding the month of the policy change (August 2016). We examine changes in origination volume in two ways. First, we compile a DTI-FICO bin-month panel, whereby DTI is categorized at the nearest integer level and FICO in bins of five. We then count the number of loans originated within a DTI integer grid, the FICO bins, and month. The log number of loans is used in our descriptive analyses (*Log(#Loans)*). Second, we compute the number of loans issued in each integer DTI grid per month for high-credit-score (above 620) and low-credit-score (below 620) groups, respectively. This loan count is used in the bunching analysis.

In later tests, we examine the changes in interest rate spreads and delinquency rates of loans originated during the two-year window around the policy change. These analyses rely on a loan-level sample. To compute interest rate spreads, we take the difference between the mortgage interest rate and the Freddie Mac Primary Mortgage Market Survey Rate (PMMS) during the month of origination.[13] Delinquency rates refer to the 90-day delinquency within two years of origination in our baseline analysis, which we expand by time horizon in robustness checks. When analyzing loan interest rates and delinquency, we control for loan characteristics such as the log of loan amount and the log of borrower household income in some specifications.

[13] Available at: https://fred.stlouisfed.org/series/MORTGAGE30US.

3.2 Experian Data

We track households' changes in address using data from Experian, a major credit bureau in the U.S. The data contain a 1% national sample of U.S. individuals who are randomly selected based on the last two digits of their social security number. The dataset describes detailed individual demographic and economic characteristics, such as the address (accurate to the census tract), age, sex, marital status, credit score, estimated income, and debt characteristics by category (auto, mortgage, credit card, student loan, medical debt, etc.).

We build an individual-year panel using annual Experian data from 2013 to 2019. We exclude the year 2016 from our sample because it includes both pre- and post-treatment periods. This panel dataset allows us to track individual addresses (accurate to the census tract) over time, thus identifying movers in a given year. It also allows us to identify those who obtain a new FHA mortgage in a given year. To measure *changes* in neighborhood quality, the regression sample is limited to years 2014-2019. We include controls for individual characteristics such as gender, marital status, and credit score.

3.3 School Ratings

Data on public school ratings in the US are obtained from GreatSchools.org. The data include the addresses of schools and their ratings in the most recent year as of 2022. The rating is based on a variety of school quality indicators and assesses how effectively each school serves all of its students. Ratings are on a scale of 1 (below average) to 10 (above average) and are based on information such as test scores, college readiness, academic progress, advanced courses, equity, discipline, and attendance data. To calculate a school district rating, we take the average rating across all schools located in the district. We merge the district ratings data with the Credit Bureau data based on the zip location of individuals. If a zip is located in more than one school district, we match the zip to the district that covers the most population in that zip based on Census crosswalks. Using the merged dataset, we define *d(School Rating)*, the year-on-year change in a household's local school rating.

3.4 Other Variables

We also examine the heterogeneity of effects across various characteristics of the borrower, lender, and local markets. First, we partition the sample based on borrower race and income levels. We consider three racial/ethnic categories: *Non-Hispanic White*, *Black*, and *Hispanic*. Here, *Non-Hispanic White* represents the sample of White borrowers excluding those of Hispanic origin. We also partition borrowers according to whether their relative household income exceeds the sample median. Relative household income is defined as the ratio of household income over the MSA median. This adjustment helps us compare across borrowers within the same broad geographical area, instead of comparing across those in far-apart regions, such as the

Northeast vs. the Southwest. Second, we look at the growth in mortgage demand faced by lenders across local markets. Specifically, we compute the year-on-year growth in application volume in each lender-state-year, and partition the sample according to whether a market's application growth is above or below the overall sample median. Finally, we separate the sample by bank and non-bank lenders. Non-banks are defined as independent mortgage lenders (IMBs) in the Avery file. FinTech lenders, as defined in Fuster et al. (2021), are not present in our market before or after the policy change.

3.5 Summary Statistics

Table 1 presents the definitions and summary statistics of the variables used in our study. At the loan level, the average loan in our sample has a 6-percentage-point probability of going into delinquency and an interest rate spread of 14 basis points, measured as the difference between the mortgage interest rate and the 30-year Freddie Mac survey rate. A typical borrower has a household annual income of $71,645. Around 61% of borrowers are Non-Hispanic White, while 12% are Black. In the individual-year panel derived from the Credit Bureau data, the average school district rating where an individual lives is about 5.3.

TABLE 1 ABOUT HERE

4 Effects on the Quantity of Credit

We first focus on how the FHA policy change affects the quantity of home purchase loans granted to households. We start by providing descriptive evidence on the changes in mortgage volume and then perform bunching estimation to sharpen causal inferences and quantify the shift in household leverage.

4.1 Descriptive Evidence

We first visually inspect how the quantity and composition of mortgage credit changed around the FHA policy reform. We plot the percentage of mortgage loans whose DTI ratios exceed 43% (i.e., high-DTI loan share) for borrowers below and above the 620 credit score cutoff, respectively. Figure 1 depicts these statistics. The red, dashed (blue, solid) line represents the percentage of mortgages issued to high-DTI borrowers among the ones with below-620 (above 620) credit scores. The vertical line indicates the month of the FHA removal of human underwriting requirement, i.e., August 2016. The two lines evolved in parallel prior to the policy reform, exhibiting little pre-event trend. In the pre-policy period, high-DTI loans accounted for around 8-9% of the total number of mortgage loans extended to low-credit-score borrowers. This fraction jumped sharply after the policy date, rising to 23% within two months and nearly 37% after 5 months. In contrast, there is no abrupt change in the high-DTI loan share among high-credit-score borrowers (i.e., FICO>620, the control group).

FIGURE 1 ABOUT HERE

We next look into how credit growth following the policy reform varies around the 43% DTI cutoff. To do so, we compute the loan growth rate (i.e., change in log number) from the 12-month pre-event window to the 12-month post-event window. This growth rate is computed separately for each DTI integer category (i.e., 20, 21, 22, ..., 56, 57) for low- and high-credit-score borrowers, respectively. Figure 2 reports the results. The horizontal axis represents DTI ratio in integer percentage points. We find that for low-credit-score borrowers, loan growth rates hover around zero for DTI ratios below 35, and become negative for DTI between 36 and 43. Above the 43 threshold, loan growth turns positive and economically large, reaching 133% at DTI of 44, and nearly 5 folds at DTI of 54. The graphical evidence yields several implications. First, the policy change had little impact on low-leverage borrowers, whose DTI lies below 35. Second, it seems to have reduced the number of borrowers taking out mortgages right below the 43 DTI threshold, and most importantly, increased the number of borrowers whose leverage exceeds the threshold. The tremendous growth of the high-leverage loans likely consists of both the switching of borrowers from below to above the 43 DTI threshold, and the influx of new, high-leverage borrowers in the market.

FIGURE 2 ABOUT HERE

4.2 Bunching Estimator

To sharpen our causal inferences, we adopt the empirical design developed in DeFusco et al. (2020) to quantify the changes in FHA credit. The core idea behind this design is to construct a counterfactual DTI distribution for low-credit-score borrowers in the absence of the policy change, and compare the actual DTI distribution with this counterfactual. In our setting, high-credit-score borrowers are not affected by the policy change, so the changes in DTI distribution among these borrowers are considered as the counterfactual case. At each DTI level, we compute the counterfactual fraction of loans among low-credit-score borrowers by summing up two parts: (1) the pre-policy fraction of loans among low-credit-score borrowers, and (2) the changes in the fraction of loans among high-credit-score borrowers (i.e., counterfactual growth).[14]

Notations and Assumptions

[14]This approach is modified from the standard bunching approach developed in the public finance literature, which involves fitting a polynomial to the observed distribution of a "running variable" while omitting the data immediately above and below the threshold, and then extrapolating this polynomial through the excluded region.

Before describing our methodology, it is useful to introduce some notations. We use n_d to represent the actual number of loans within DTI integer bin d. Subscripts h and l indicate borrowers with credit scores above or below 620. Superscripts pre and $post$ indicate event periods, i.e., before and after the policy change.

Thus, n_{hd}^{pre} and n_{hd}^{post} represent the actual number of loans among high-credit-score borrowers for DTI integer bin d before and after the policy event, respectively. Similarly, n_{ld}^{pre} and n_{ld}^{post} represent the actual number of loans among low-credit-score borrowers at DTI bin d before and after the policy event. $\hat{n}_{ld}^{post}$ denotes the *counterfactual* number of loans among low-credit-score borrowers for DTI bin d after the policy event.

Finally, we use N to represent the total number of loans across certain DTI ranges. N is introduced to normalize loan quantities and compute distribution fractions. The same subscripts (h, l) and superscripts (pre, $post$) apply. For example, N_l^{post} stands for the total number of low-credit-score loans extended in the post-event period. $\hat{N}_l^{post}$ denotes the corresponding, counterfactual number.

With the above notations, we lay out the following assumptions underlying the bunching estimation.

Assumption 1. *The market for high credit score borrowers (i.e., FICO>620) is not affected by the policy change.*

$$\hat{n}_{hd}^{post} = n_{hd}^{post} \tag{1}$$

Assumption 2. *There exists a maximum DTI bin $\bar{d}$ such that the total volume of low-credit-score loans with $DTI \leq \bar{d}$ is unaffected by the policy.*

$$\sum_{d=0}^{\bar{d}} \hat{n}_{ld}^{post} = \sum_{d=0}^{\bar{d}} n_{ld}^{post} \triangleq N_{l\bar{d}}^{post} \tag{2}$$

$N_{l\bar{d}}^{post}$ denotes the observed total number of low-credit-score loans with DTI below $\bar{d}$ extended after the policy event. Assumption 2 enables normalization that allows us to translate between the DTI distribution in the low- and high-credit-score markets. The normalization is needed because one market is significantly larger than the other. This assumption ensures that when we divide each of these bin counts by the corresponding total level of activity to the left of $\bar{d}$ in the relevant market, there is a region in which the ratios will be comparable. As seen from Figure 2, loan volume remained relatively constant for low credit score borrowers at DTI ratios below 35. Based on this pattern, it is reasonable to set $\bar{d} = 35$. In our analysis, we also experiment with $\bar{d}$ being 32, 34, and 36 to test the robustness of our findings.

Assumption 3. *The change in the (normalized) number of low CS loans in a given DTI bin between the pre- and post-periods would have been the same as the corresponding change in the high CS market in the absence of the policy.*

$$\frac{\hat{n}_{ld}^{post}}{N_{l\bar{d}}^{post}} = \frac{n_{ld}^{pre}}{N_{l\bar{d}}^{pre}} + \left(\frac{n_{hd}^{post}}{N_{h\bar{d}}^{post}} - \frac{n_{hd}^{pre}}{N_{h\bar{d}}^{pre}} \right) \tag{3}$$

Assumption 3 is the crucial assumption that establishes our counterfactual. It states that the distribution changes in the high-credit-score market represents the counterfactual for the low-credit-score market. The first term, $\frac{n_{ld}^{pre}}{N_{l\bar{d}}^{pre}}$ is the pre-event observed distribution of loans for each DTI grid in the low-credit-score market. The second term, $\left(\frac{n_{hd}^{post}}{N_{h\bar{d}}^{post}} - \frac{n_{hd}^{pre}}{N_{h\bar{d}}^{pre}} \right)$ is the changes in the normalized distribution of high-credit-score loans around the policy event. By taking the sum of the two terms, we assume that absent the policy reform, the changes in the DTI distribution among low-credit-score loans would have been the same as those among high-credit-score loans.

Figure 3 plots the actual and counterfactual distribution of loans at each DTI grid for low-credit-score borrowers. The red solid line represents n_{ld}, the actual number of loans issued for each DTI grid d, and the blue dashed line represents $\hat{n}_{ld}$, the counterfactual number of loans based on Assumption 3. We first notice a clear bunching of loans right below the DTI = 43 threshold in the counterfactual distribution. The number of loans spikes at 43, and drops at 44. Such a bunching pattern is visibly smaller in the actual, post-policy distribution. This contrast suggests that the requirement for human underwriting for low-DTI borrowers leads to the bunching of loans under the DTI= 43 threshold. Second, Figure 3 shows a large increase in the number of loans above the DTI= 43 threshold post-policy of over 100% for most DTI bins. The significant magnitude of this expansion is consistent with our the reduced form evidence in Figure 2.

FIGURE 3 ABOUT HERE

One concern with the above pattern is that we might be capturing a general trend of loosening lending standards towards highly levered, low-credit-score borrowers over time. If this is the case, we should observe the same pattern in a different point in time. We thus provide a placebo analysis in Appendix Figure B.2 where we use August 2015 as a pseudo event. Human underwriting was required for low-credit-score, high-DTI loans consistently throughout the 24-month event window around August 2015. Accordingly, we observe the bunching of loans at $DTI = 43$ both in the counterfactual and actual distributions, with no significant difference between the two around the pseudo event. This means that the reduction of bunching in Figure 3 is unlikely due to a general time trend, but instead directly related to the FHA policy.

FIGURE B.2 ABOUT HERE

Quantifying Credit Expansion

Under Assumptions 1 through 3, we quantify the changes in loan volume due to the FHA policy change regarding underwriting procedures. Our main focus is to identify the overall increase in credit above the unaffected DTI region, i.e., $DTI > \bar{d}$, also referred to as the "extensive margin" effect. Formally, it is defined as the fraction of loans granted to borrowers who would otherwise not have applied or been approved without the policy (i.e., counterfactual scenario):

$$\Delta Loans\ Originated = \frac{1}{\hat{N}_l^{post}} \sum_{d=\bar{d}}^{57} (n_{ld}^{post} - \hat{n}_{ld}^{post}) \tag{4}$$

The expression inside the parentheses indicates the additional number of low-credit-score loans with DTI above $\bar{d}$ due to the policy change. $\hat{N}_l^{post}$ denotes the counterfactual total number of low-credit-score loans extended after the policy event and is used as a scaling factor. The DTI variable is winsorized at the 1^{st} and 99^{th} percentiles and hence capped at 57. When computing this statistic, we use the Ginnie Mae-HMDA matched sample and focus on loans for purchasing single-family, non-manufactured housing issued from August 2015 to August 2017, i.e., 12 months before and after the regulation change, excluding August 2016. To obtain confidence intervals, we use a bootstrap procedure, drawing 1000 random samples and re-estimate the parameter at each iteration.

Results are reported in Table 2. In Column (1), we set the cap for "unaffected" DTI range $\bar{d}$ to be 35, following the pattern displayed in Figure 3. Results from the extensive margin suggest a significant increase by 10.3% among low FICO score borrowers overall. In Columns (2) through (4), we alternate $\bar{d}$ to be 32, 34, and 36. Effects remain highly statistically significant and stable in magnitude.

Table 2 About Here

Changes in DTI Distribution

The pattern shown in Figure 3 suggests that the policy change gave rise to a drastic shift in the DTI distribution. Not only did lenders expand credit provision, attracting new borrowers to enter the market and apply for a mortgage, existing borrowers may also decide to increase loan size after the policy change, increasing their DTI ratio from below to above 43. We label this latter as the "intensive margin" effect, and seek to quantify it in this section.

Following DeFusco et al. (2020), we measure the reduction in volume in range $\bar{d} \leq DTI \leq 43$ around the policy change. Again, we compare the fraction of loans in this range relative to the counterfactual scenario:

$$\Delta Low\ DTI\ Loans = \frac{1}{\hat{N}_l^{post}} \sum_{d=\bar{d}}^{43} (n_{ld}^{post} - \hat{n}_{ld}^{post}) \tag{5}$$

In the parentheses, $n_{ld}^{post} - \hat{n}_{ld}^{post}$ indicates the change in low-DTI loans compared to the counterfactual case without the policy at DTI d. We focus on the DTI ranging between $\bar{d}$ to the threshold 43 because below $\bar{d}$, loan quantity remains unaffected by the policy (Assumption 2). Table 2 shows the change in low-DTI loans to be about 8.6%. This means that 8.6% of low-credit-score borrowers increase their loan size to above $DTI = 43$ relative to the counterfactual scenario absent the policy change.

We caution the interpretation of the intensive margin for two reasons. First, $\Delta Low\ DTI\ Loans$ does not directly measure the intensive margin of the policy effects, but instead measures the net effect from the extensive and intensive margins over the low-DTI range ($[\bar{d}, 43]$). The extensive margin is not necessarily zero in this range, because the policy change may encourage households to take up mortgages below the DTI threshold. For example, some households may consider the policy as a signal for relaxed lending standards and enter the housing market. Yet, they could end up purchasing properties of moderate value, leading to a DTI ratio below 43. While such an entry effect may be small in magnitude, it can still offset partially the intensive margin effect, i.e., existing borrowers switching to high-DTI loans. This means that the absolute value of $\Delta Low\ DTI\ Loans$ may be a lower-bound of the intensive margin.

Second, borrowers may have some room for discretion when reporting their income around the DTI threshold, such as whether to include certain bonus income. Prior to the policy reform, borrowers may have a greater incentive to boost their income, so that their DTI ratio stays under the 43 threshold. This could lead us to over-estimate the intensive margin effects. However, this would not affect our extensive margin estimates as long as the manipulation does not extend beyond the DTI≤35 threshold, which is a condition that appears to hold given the parallel trends in the DTI≤35 region in Figure 2. We therefore focus our discussion and subsample analyses on the extensive margin estimates. Furthermore, our subsample results seem at odds with the direction of manipulation: if borrowers are less incentivized to manipulate their income upwards following the policy relaxing DTI constraints, we should see lower income borrowers having a higher extensive margin response post-policy, but instead we see the opposite.

Finally, we analyze the change in the average DTI ratio of approved loans. Formally, we define the change in the average DTI below:

$$\Delta Average\ DTI = \sum_{d=1}^{57} d \left(\frac{n_{ld}^{post}}{N_l^{post}} - \frac{\hat{n}_{ld}^{post}}{\hat{N}_l^{post}} \right) \tag{6}$$

This measure is a weighted average of DTI ratios, with the weights being the change in the share of loans at each DTI grid. In Table 2, we find that the FHA policy led to a sizeable increase in the reported DTI ratio of mortgages by around 1.3.

Taken together, results from our estimations suggest that the FHA policy led to a substantial increase in the origination of loans to low FICO borrowers along with an increase in DTI of originated loans.[15] The increase in DTI is driven by both borrowers switching from low- to high-DTI loans and the entry of new borrowers. In the remainder of the analysis, we focus on the overall increase in loan volume (i.e., the extensive margin), since that metric is less subject to noise and provides a more straightforward proxy of credit expansion.

5 Delinquency

Does the policy-induced credit expansion for low-credit-score, high-leverage borrowers engender greater risk exposure for lenders and the FHA? We seek to answer this question by examining how loan delinquency changes around the FHA policy reform.

We examine the changes in mortgage delinquency rates as well as interest rate spreads for low-FICO, high-DTI loans relative to other loans around the policy event using two research designs. First, we estimate a difference-in-difference regression for high-DTI and low-DTI loans separately:

$$Delinquent_{i,t} = \beta_1 Treated \times Post + \tau_t + \phi_f + \epsilon, \tag{7}$$

where i denotes loans, t denotes origination month, f denotes FICO and d denotes DTI. *Post* is an indicator for months after the policy change (August 2016). *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. Our coefficient of interest is β_1, which indicates the change in delinquency among the low-credit-score loans relative to the high-credit-score ones. We add controls, fixed effects, and interactive fixed effects in stages. Given that the FHA policy had little impact for borrowers with DTI under 35, we restrict the testing sample to loans with DTI ratio above $\bar{d} = 35$, to analyze the pricing and performance of loans affected by the FHA underwriting policy. For the main specification, we track whether the borrower incurs delinquency over the next two years for each loan, following Bosshardt et al. (2023). We perform robustness tests using different time horizons to measure delinquency.

Results are reported in Panel A of Table 3. Columns (1) through (3) present results for high-DTI loans; while Columns (4) through (6) report results for low-DTI loans. For each sample, we start with a relatively sparse specification (Columns (1) and (4)), and only impose origination month fixed effects and FICO fixed effects. Origination month fixed effects help remove macro-level changes in lending standards, while the FICO fixed effects allow us to compare loans of similar credit scores. In the next specification (Columns (2) and (5)), we include origination month-by-DTI fixed effects to account for the time-varying ability to repay for households with certain levels of leverage, FICO by DTI fixed effects to compare loans of similar

[15] In Appendix B.3, we study quantity effects in a difference-in-differences framework and reach similar conclusions.

credit scores and leverage ratios. The controls include the natural logarithm of loan amount and borrowers' application income. In the last specification (Columns (3) and (6)), we add county and lender fixed effects to control for time-invariant county and lender characteristics that may affect default rates, including selection into certain counties and changes in lender composition.

TABLE 3 ABOUT HERE

Across all specifications, the coefficients on *Treated* × *Post* are economically small and statistically insignificant for both high- and low-DTI loans. This result suggests that the policy change did not increase the default rate of low-credit-score borrowers relative to high-credit-score ones. If anything, the coefficient loadings are mostly negative, suggesting a small drop in the average delinquency rate, conditional on observables.

We next estimate triple-difference regressions, comparing the changes in delinquency rates to treated borrowers between high- and low-DTI loans:

$$\begin{aligned} Delinquent_{i,t} = \gamma_1 Treated \times High\,DTI \times Post + \gamma_2 Treated \times High\,DTI \\ + \gamma_3 Treated \times Post + \gamma_4 High\,DTI \times Post + \tau_t + \phi_f + \epsilon, \quad (8) \end{aligned}$$

where *High DTI* is a dummy variable that equals one if the DTI ratio is above 43, and zero otherwise. Panel B of Table 3 reports the results. Again, there is no statistically significant difference in the changes in delinquency rates between the two subsamples, with the coefficient loadings on the triple interactions being negative and small.

A concern is that our test may not have sufficient power to detect changes in delinquency resulting from the policy. One may argue that delinquency rates were low for mortgages originated around 2015–2017, because house prices and economic conditions were stable or increasing during that period. Counter to this argument, we note that the average 2-year delinquency rate in our sample is not negligible, but hovers around 6% overall, 12% for the treated group of low FICO borrowers, and 14% for low FICO, high DTI borrowers.[16] To further address this concern, we conduct an additional analysis in Table 4, where we separately examine effects of the policy across locations with different unemployment growth rates. Unemployment rate growth is measured as the difference between local unemployment rates in the year prior to the policy change and the year after. To the extent that negative life events are the primary driver of mortgage defaults (Ganong and Noel, 2023), the above concern would suggest that the FHA policy change should induce higher delinquency rates in areas with the highest unemployment growth. However, we do not find this to be the case. Even in counties that experienced the highest increase in unemployment rate, we continue to see muted effects of

[16]Despite the high delinquency rates, the FHA views these mortgages as having positive net social benefit by enabling borrowers to purchase homes earlier (McFarlane, 2010), at the cost of a transfer from the FHA. We analyze the effect of the policy on the FHA market and conduct a back-of-the-envelope welfare evaluation in Appendix Section C.

the policy shock on delinquency rates. If anything, delinquency rates have declined for the treated group in those counties.

TABLE 4 ABOUT HERE

We provide evidence supporting the parallel-trend assumption for the delinquency tests. We perform dynamic triple-difference analysis and analyze the differential changes in delinquency for highly levered, low-credit-score borrowers around the policy date. Figure 4 reports the results. The dots represent the point estimates of the triple-difference coefficients, while the vertical lines represent confidence intervals. We do not observe significant pre-event trends.

FIGURE 4 ABOUT HERE

In Figure 5, we report the changes in delinquency rates around the policy event with different local economic conditions, measured by county unemployment growth rates. Panel A (D) reports the changes in delinquency in counties with the bottom (top) quartile of unemployment growth. Suppose a heavier reliance on machine underwriting admitted more "fragile" borrowers who are prone to default during poor economic conditions. In that case, we should observe an increase in delinquency rate in areas with greater unemployment rate growths. However, we do not find that to be the case. Delinquency rates remain unchanged across counties with better and worse economic conditions.

FIGURE 5 ABOUT HERE

We provide multiple additional analyses to test the robustness of our findings and address remaining concerns regarding delinquency rates. It is possible that we do not find any significant effects on delinquency rates because our test is performed on a restricted sample of DTI above 35. In Panel A of Table B.2, we switch to the full sample that includes all DTI categories. In this expanded sample, we continue to find no significant changes in delinquency rates either in the high-DTI or low-DTI range. A remaining concern could be that we are unable to detect meaningful changes in delinquency rates in the limited time horizon that we focus on. We address this concern in Table B.3, where we look at 3-year, 4-year, and 5-year delinquency rates. Our inferences remain unchanged. We note that the 4-year and 5-year delinquency rates includes the COVID-19 forbearance period, and our results suggest that the uptake of this forbearance has not differentially increased for low FICO, high DTI borrowers post-policy. Finally, it is possible that our measure of delinquency, which focuses on 90-day delayed payment, does not capture milder levels of borrower distress. In Table B.4, we evaluate less severe delinquencies, including 30-day and 60-day delinquencies, and continue to find no changes associated with the FHA policy reform.

We note that, while we do not detect increases in delinquency rates within each DTI bin, there is a significant increase in high-DTI loans granted after the policy change, which could lead to an increase in overall, unconditional delinquency rates. In Appendix Section C, we conduct a back-of-the-envelope calculation regarding the effects of the policy on loan quantity and default. Based on our estimates, the policy increased total FHA loan volume by 1.10%, and consequently, unconditional delinquency rates by 1.61%. This means that, to break even in the long-run, the FHA would need to increase their mortgage insurance premium (MIP) by 1.61%, which translates to an additional 1.82 bps for an average 7-year mortgage. In other words, the policy likely improved the financial inclusion for higher-risk borrowers, while imposing some costs on lower-risk ones.

Based on the above estimates, we also provide some discussions regarding the welfare implications of the policy. If we apply equal utility weights across borrowers as in Jansen et al. (2022), the FHA policy that we analyze can be welfare-improving if the demand elasticity of loan size to MIP is lower than 0.27. Empirical estimates of loan size demand to interest rates from DeFusco and Paciorek (2017) indeed satisfy this condition.[17] However, in reality, high-risk borrowers may under-estimate their default probabilities and the cost of foreclosures or short-sales, which could lead to a lower interest rate elasticity of demand. Under such frictions, the policy-induced credit expansion could engender greater costs to other FHA borrowers and decrease borrower welfare.

There are several potential explanations for why delinquency rates did not increase after the FHA policy. A natural explanation is that the additional information collected by human underwriters has limited incremental value relative to the hard information used by the FHA's underwriting algorithm. Second, it is possible that human underwriters do gather valuable information, but loan officers are able to push against their recommendations due to volume incentives, making human underwriting less effective. We believe this channel is unlikely, as lenders expressed significant concerns about FHA scrutiny over defaults during our sample period (Frame et al., 2024). Third, human underwriters' role can be weakened if lenders purposefully extended loans to high-risk borrowers to comply with the Community Reinvestment Act (CRA), despite the recommendations of human underwriters. However, this argument should not be the main driver of our results, as the majority of the high-risk loans in our sample are made by non-bank lenders who are not subject to such CRA requirements (Panel B of Table 7). Furthermore, a subsample analysis excluding CRA census tracts, which we present in Appendix Table B.5, shows similar results. Fourth, borrowers in this market may exhibit homogeneous risk profile, leaving little to no role for risk management, regardless of whether it is performed by human or algorithms. This explanation seems unlikely, given that Berg (2015) document a prominent role for risk managers in discerning borrower risk in high-risk mortgage markets. Regardless of the explanation, our results suggest a limited incremental risk management role of human

[17] DeFusco and Paciorek (2017) estimate the mortgage demand-to-interest rate elasticity to be 0.023–0.03, which is a magnitude smaller than the upper bound of 0.27.

underwriting for most borrowers in this market. We dissect potential mechanisms underlying our findings in greater detail in Section 7.

6 Heterogeneous Effects Across Borrower Race and Income

Next, we analyze the differential effects of the policies across racial and income groups. This analysis helps shed light on the ongoing discussion regarding whether algorithmic underwriting generates disparate impacts across different demographic groups.

We construct three subsamples according to borrowers' ethnicity: Black, Hispanic, and White (Non-Hispanic). We do not separately analyze the effects for Asian borrowers because they comprise only 3.2% of the sample of low-credit-score and highly-levered borrowers, suggesting relatively low statistical power. We then repeat the bunching estimation for each of the subsamples. Panel A of Table 5 reports the results. We find that the policy-induced increase in loan volume is largely concentrated on White and Hispanic borrowers, with magnitudes around 10.8-10.9%, similar to the full sample result. In contrast, such an effect is small in magnitude (1.4%) and statistically insignificant for Black borrowers.

TABLE 5 ABOUT HERE

We also partition the sample by the median of borrowers' adjusted income, which is household income scaled by the MSA median income. As mentioned earlier, this location-based adjustment helps remove the heterogeneity created by cross-region differences in economic conditions and lending standards. Panel B of Table 5 reports the heterogeneous effects of the policy for higher and lower-income borrowers. The increase in loan volume is uniformly stronger for higher-income borrowers than lower-income ones. Borrowers with above-median (below-median) adjusted income experience a 13.6% (3.8%) increase in loan volume after the policy change. We observe similar differences when interacting income with race by partitioning the sample based on income within their respective racial categories (Appendix Table B.6). In Panel C of Table 5, we also separately examine whether delinquency effects vary by borrower race and income, and continue to find muted effects across all groups.

Collectively, our results suggest that the FHA credit expansion mostly affected White and higher-income individuals. These findings imply that the policy generates limited improvements in the financial inclusion of disadvantaged population. We assess the relative importance of supply side and demand side mechanisms behind this result in Section 9.

7 Economic Mechanisms

Our results so far suggest that a heavier reliance on algorithm underwriting can expand credit supply without necessarily compromising risk management. This suggests that human underwriters provide little benefit in limiting risk exposure in this market. Our evidence could be consistent with a few underlying channels. First, in this market, borrowers' credit risk can be well characterized by hard information alone, so that the information collected by human underwriters provides limited value. We label this as the "hard information" channel. Second, algorithmic underwriting can mitigate lenders' concerns regarding FHA scrutiny. Given that the algorithms are approved by the FHA, relying more on the algorithm can reduce lenders' concerns that the FHA may push back on the loans they approve when relying on algorithmic underwriting. We label this channel as the "regulatory burden" channel. We note that the hard information and regulatory burden channels are not mutually exclusive. While the regulatory burden channel can explain the credit expansion, it cannot necessarily explain our delinquency results if the additional information collected by human underwriters significantly improves risk management for most borrowers in this market.

Beyond regulatory burden, several other explanations can explain the credit expansion. First, the credit expansion could be consistent with a capacity constraint channel, i.e., more reliance on algorithms reduces the workload for human underwriters, relieving lenders' capacity constraints. This argument suggests that algorithms should expand credit quantity more when lenders operate in more capacity constrained markets. Second, the credit expansion results may be explained by an increase in lender entry due to the reduction in underwriting costs. To the extent that the FHA policy represents a relaxation in underwriting requirements, it may have allowed lenders to hire fewer human underwriters or pay for fewer underwriting hours. Such a cost reduction may have incentivized lenders to enter this market without altering their credit decisions conditional on entering.

Below, we design several analyses to shed light on the economic mechanisms discussed above. First, we look into the likelihood and the reasons for loan denial using the HMDA data. Second, we analyze the heterogeneous effects of the FHA policy on credit quantity across lenders. This analysis focuses on the extensive margin effect (i.e., Δ*Loans Originated*), as it is a more direct measure of credit expansion and is not subject to the manipulation of the DTI ratio. Third, we directly compare the share of high-DTI, low-FICO lending post-policy from existing versus new lenders in this market. Overall, we find evidence suggesting that lenders approved more high-DTI loans after the policy, thus a simple capacity constraint channel or lender entry cannot fully explain our results. Moreover, we find evidence supporting the regulatory burden channel, but inconsistent with the capacity constraint channel and the lender entry channel.

7.1 Loan Denial

We examine the changes in denial rates around the FHA policy, for two reasons. First, it helps quantify the extent to which the credit expansion induced by the FHA policy arises from increases in credit supply, rather than changes in application volume. Second, it helps evaluate alternative explanations for the credit expansion such as the capacity constraint channel and the lender entry channel. If the FHA policy simply allowed lenders to process more applications without altering their credit decisions, but not necessarily affect approval likelihood, nor differentially affect the denial rates between high- and low-DTI loans.

Information on denial decisions comes from the HMDA database. HMDA provides an indicator "Action Taken", where a value of 3 indicates denial. We create an indicator *Denial* accordingly. We apply similar empirical designs as the ones outlined in Equations 7 and 8, regressing the denial measure on the interaction of low-FICO borrowers and *Post*, and with an indicator for high-DTI borrowers in the triple-difference framework.

An important caveat is that HMDA does not provide information regarding applicants' credit scores or DTI to the public, thus we must estimate such information. We approximate the likelihood that an applicant has a lower-than-620 credit score using the fraction of individuals whose FICO score is under 620 in a county in 2015, based on our Experian data. We approximate the applicant's DTI ratio using the ratio of debt payment over income, and estimate their debt payment using the sum of mortgage payment (implied by the mortgage balance and the average FHA interest rate in a given year), the FHA mortgage insurance premium, and the average remaining debt payment. The average remaining debt payment is imputed for each racial group by income category based on the implied average remaining debt payment in our HMDA-Ginnie Mae data, where the income category is in bins of size \$1,000.[18] We then define *High Approx DTI* as an indicator for whether the applicant's estimated DTI is above 43.

Table 6 provides the results. Panel A reports the results from the DID framework and Panel B reports the results from the triple-difference framework. Results from both panels indicate that denial rates reduce significantly among the affected (low-FICO and high-DTI) borrowers. This is reassuring and suggests that the observed credit expansion is not merely due to an increase in the number of loans being processed, but also driven by a change in approval rates. Importantly, these results hold conditional on lender fixed effects, which suggests that changes in lender composition do not fully explain the credit expansion. Furthermore, denial rates barely changed for low-DTI borrowers. This, again, suggests that the change in approval rates is

[18] More specifically, we approximate $\tilde{DTI}_i = \frac{\text{Scheduled Mortgage Payment}(B_i, \bar{r}_t + \text{FHA ongoing MIP}) + \text{Imputed Remaining Debt Payment}}{\text{Income}}$, where B_i is the application mortgage balance, $\bar{r}_t$ is the average interest rate on FHA loans in the year of the HMDA mortgage application (3.96% in 2015, 4.17% in 2017), and the FHA ongoing MIP is equal to 0.85% in our time period. The imputed remaining debt is equal to the average implied remaining debt payment among originated loans for borrowers in each racial group by income category in our HMDA-Ginnie Mae sample, where Implied Remaining Debt Payment $= DTI_i * \text{Income} - \text{Scheduled Mortgage Payment}(B_i, \bar{r}_i + \text{FHA ongoing MIP})$, where DTI_i is the observed DTI, $\bar{r}_i$ is the observed interest rate, and income category is in bins of size \$1,000.

limited to high DTI borrowers, and that underwriting decisions did change within lenders.

TABLE 6 ABOUT HERE

7.2 Lender Congestion

Next, we analyze the differential effects of the FHA policy change on credit quantity in situations where lenders likely face more or less capacity constraints to process loan applications. We gauge lenders' capacity constraints using the growth in loan application volume across different local markets. Specifically, we compute the year-on-year growth in loan applications for each lender-state. To the extent that lenders cannot expand and adjust their local employment quickly in response to demand conditions (Fuster et al., 2021), we expect them to face greater capacity constraints in markets with higher application growth.

The capacity constraint channel predicts that algorithms should be more effective in cases where lenders face greater capacity constraints. However, results in Panel A of Table 7 suggest otherwise. In fact, loan volume increases more in areas where lenders appear less constrained, by around 12%. In the more congested areas, however, the credit expansion is smaller, around 5%. This result is again inconsistent with the capacity constraint channel.

TABLE 7 ABOUT HERE

7.3 Bank and Nonbanks

Next, we evaluate the regulatory burden channel by examining whether and how the FHA underwriting policy affected bank and nonbank lenders differently. To the extent that nonbank lenders face less stringent regulatory scrutiny and can securitize a greater share of their loan portfolio (Benson et al., 2023), they should be less concerned about regulatory risk compared to bank lenders. If the reliance of algorithmic underwriting helps lender overcome regulatory concerns, we expect such effects to be more pronounced among banks compared to nonbanks.

We repeat the bunching estimator for subsamples of loans from bank and nonbank lenders. Results are presented in Panel B of Table 7. Consistent with the above conjecture, the increase of loan quantity at the extensive margin is relatively higher for bank lenders than nonbank lenders.

7.4 Lender Entry

Finally, we directly evaluate the extent to which our credit expansion results are driven by lender entry into the market. Panel C of Table 7 presents the share of low FICO, high DTI loans post-policy that are originated by existing versus new lenders. We define existing lenders as lenders who has originated at least one low-FICO, high-DTI loan in our pre-policy period. As Panel C of Table 7 shows, 81% of low-FICO, high-DTI loans are originated by existing lenders post-policy, and 19% by new lenders. Recall that Figures 2 and 3 suggest that the quantity of low-FICO, high-DTI loans expanded over 100% in the post-policy period, which implies that lender entry is unlikely to be the main driver of the policy-induced credit expansion.

In all, our evidence on the heterogeneous effects of the FHA policy indicates that the policy led to a greater expansion of credit for higher income, White borrowers, less congested lenders, and bank lenders. These results are consistent with the argument that algorithmic underwriting has advantages in processing loan applications when there is rich historical data (hard information channel), and the argument that FHA-approved algorithms can alleviate regulatory risk (regulatory burden channel). They are inconsistent with the capacity constraint argument or the lender entry argument, i.e., algorithms simply relieve human capacity constraints without altering loan approval decisions by lenders.

8 Implication for Borrowers

In this section, we design two analyses to investigate the potential impact of the FHA policy on borrowers. First, we look into the changes in borrowing costs for high-leverage borrowers. Second, we track households' location choices and examine whether the credit policy allows them to migrate to higher-quality neighborhoods.

8.1 Loan Pricing

While the FHA policy in 2016 provided easier access to credit to low-credit-score, high-leverage borrowers, those incremental borrowers may face heavier debt burdens if they face higher interest charges. To assess this concern, we directly analyze the changes in the interest rate spreads charged on the low- and high-DTI loans around the policy shock.

Table 8 reports the results. The format of this table follows closely that of the delinquency analysis. From Panel A, we find no changes in interest rate spreads among high-DTI loans, but a statistically significant increase for low-DTI loans in some specifications. This might be caused by changes in borrower characteristics among the low-DTI borrowers. In Panel B, we confirm that interest rates increase less for treated borrowers in the low-DTI sample relative to the high-DTI sample. The coefficient of *Treated* $\times$ *Post* $\times$ *High DTI* suggests that the differential change in interest rates for highly levered, low-credit-score borrowers is relatively small, around 3 basis points. In Panel B of Appendix Table B.2, we repeat the robustness analyses where we look at the full sample of loans instead of loans with DTI above 35. Results are similar to the base analysis.

Table 8 About Here

Finally, in Figure 6, we do not observe any pre-policy changes in interest rates, confirming that the previous findings are not driven by pre-existing trends.

Figure 6 About Here

8.2 Mortgage Access and Neighborhood Choice

Recent evidence establishes that neighborhood quality varies substantially across regions, and higher-opportunity neighborhoods can significantly enhance individuals' long-term outcomes (Chetty et al., 2016). Of particular importance is the quality of public schools, because education quality not only plays a crucial role in shaping upward income mobility (e.g., Restuccia and Urrutia, 2004), but also tends to correlate with other desirable neighborhood attributes, including safety. However, barriers impede household mobility, such as information frictions, search difficulties, and credit and liquidity constraints (Bergman et al., 2024). In this section, we investigate the impact of increased mortgage access stemming from changes to lender underwriting regulations on individuals' subsequent neighborhood choices, with a specific focus on public school quality. This analysis sheds light on the effects of lender underwriting rules on "moves to opportunity."

For this analysis, we rely on the credit bureau data, which is an individual-year panel that allows us to track how people's addresses change over time. We compute the year-on-year change in a household's local school rating (*d(School Rating)*) for a given individual and examine whether the implementation of the FHA policy and the subsequent change in one's access to mortgage enables her to move to better school districts. Given that the credit bureau data does not contain information regarding mortgages' DTI ratios, we are unable to separately examine the effect of the policy change on high- and low-DTI borrowers. Instead, we compare individuals with a credit score above and below 620 in 2015, the year before the policy implementation.

The detailed information regarding mortgage initiations in the credit bureau data allows us to link the change in neighborhood quality precisely to the FHA policy implementation. To this end, we conduct a two-stage-least-square (2SLS) analysis where the outcome variable for the first stage is *New Purchase FHA*, an indicator for whether an individual obtained a new FHA mortgage in a given year (excluding refinancing). Then, in the second stage, we further link the changes in school district quality to the predicted likelihood of a new FHA purchase. We control for individual characteristics, including gender, marital status, age, and credit score. In some specifications, we also include origin zipcode-by-year fixed effects or zip-by-gender, age, and marital status fixed effects to account for the possibility that location-specific upward mobility varies with gender, marital status, and age.

Table 9 presents the 2SLS results. In the first stage, the treated group experience a statistically significant increase in the likelihood of getting an FHA mortgage. The F-statistics are between 313 and 380 across different specifications, evidence of a strong instrument. In the second stage, the estimates suggest that the increased mortgage access leads to a meaningful increase in the quality of the school districts where individuals reside. On average, school district ratings increased by approximately 1.1-1.9 units, equivalent to a shift from a 5-rated district (the sample average) to one rated between 6 and 7. The effects are similar when we layer on various fixed effects to control for potential differences arising from local economic conditions and preferences in each gender and age group.

TABLE 9 ABOUT HERE

We provide two caveats to our second-stage estimates. First, those estimates may also capture school rating improvements driven by the intensive margin effects (the ability to obtain *larger* mortgages), as documented in Section 4.2. Second, the estimates represent local average treatment effects for high-leverage, high-risk households, but may not generalize to the population of low-risk households. For households with easy access to mortgages and potentially already living in desirable neighborhoods, an increase in credit supply may not trigger an immediate shift in neighborhood quality.

Another potential concern is that other unobserved differences between the low-credit-score and high-credit-score groups during the sample period drive the shift in neighborhood choice, rather than the expansion of mortgage credit. If this were true, our observed effects would also show up among population of borrowers that are unlikely to be affected by the policy change, such as renters. In Appendix Table B.7, we repeat our analysis using individual-year observations of renters as a placebo group, and find no significant changes in their school ratings, alleviating this concern.

9 Structural Model

Our analysis so far suggests that FHA's manual underwriting requirement restricts credit to highly levered, low-credit-score borrowers. The restriction has limited effects on the risk exposure to the government agency, and has differential impacts on households' credit access across racial and income groups. While the evidence is clear, the reduced form analysis cannot fully address some important questions. For example, how does the increased reliance on algorithmic underwriting affect borrower welfare? How does the policy affect the approval rates of high DTI mortgages (i.e., arising from the direct approval by the AUS)? And to what extent do the supply-side differences contribute to the gaps in mortgage takeup across racial and income groups?

We seek to answer these questions by estimating a structural model with heterogeneous borrowers and endogenous household leverage decisions. This structural approach allows us to gauge the welfare impact of the policy change and to disentangle the effects from changes in household demand and changes in credit supply.

9.1 Model Setup

Our consumer welfare analysis builds on the framework of Jansen et al. (2022), with the addition of borrower demand estimation that accounts for rejections and bunching at DTI limits. The model extends from $t = 0, ..., T$, with T being the maturity of a mortgage loan, and contains a continuous mass of borrowers, each indexed by i. A borrower derives a concave utility from consumption each period $u(\cdot)$. They have an initial wealth of w_0 and can take out a mortgage to consume at $t = 0$. Their discount rate is β. Each period, they have an exogenous default rate of δ. If the borrower defaults, they are left with c_D to consume till the end of the timeline.

Let L be the mortgage principal amount, r be the interest rate, and ϕ be the fraction of principal paid each period as a function of r. Given the interest rate, the borrower maximizes their total expected utility by choosing the optimal loan amount L^*. Specifically, omitting the subscript i for brevity and focusing on a single borrower, the borrower's value function can be written as:

$$V(r) = \max_L u_0(w_0 + L) + \sum_{t=1}^{T} \beta^t (1-\delta)^t u(w_t)(1 - u'(w_t)\phi(L,r)) + \sum_{t=1}^{T} (1-\delta)^{t-1}\delta \sum_{\tau=t}^{T} \beta^\tau u(c_D) \quad (9)$$

We denote $L^*(\hat{r})$ as the borrower's optimal loan amount at interest rate $\hat{r}$. Jansen et al. (2022) show that, under certain assumptions, the borrower's value function $V(r)$ can be written as:

$$V(r) = \bar{V} + \underbrace{\left[\sum_{t=1}^{T} \beta^t (1-\delta)^t u'(w_t)\right]}_{\text{Utility weight}} \underbrace{\left[\int_r^\rho L^*(\hat{r}) \frac{d\phi}{dr} d\hat{r}\right]}_{\text{Borrower surplus triangle}}, \quad (10)$$

where $\bar{V}$ is the borrower's utility if they did not obtain a loan; ρ is the maximum interest rate at which the borrower demands a non-zero loan amount; and $\frac{d\phi}{dr}$ is the derivative of the per-period payment with respect to the interest rate. "Borrower surplus triangle" represents the changes in consumer welfare with every increment of interest rate. Normalizing the utility weight to 1, we can compute the changes in consumer welfare as a result of the FHA underwriting policy by taking the difference of $V(r)$ between the pre- and post-policy windows. We then sum up the welfare change across all borrowers in our sample.

Recall that a large fraction of the policy effects arise from the extensive margin, i.e., individuals are more likely to apply for a mortgage and their applications are more likely approved. We need to estimate optimal loan sizes L^* while accounting for the changes in mortgage approval for borrowers in each DTI bucket. To do so, we quantify the borrower surplus triangle by estimating a structural model of borrower demand for mortgages and fitting the model to several key empirical moments: the DTI distributions in the pre- and post-policy regimes, the extensive margin response to the policy change, and borrowers' extensive margin elasticity of demand to interest rates prior to the policy change.

In the description below, we bring back the borrower identifier i to allow for borrower heterogeneity. We model borrower i's utility from taking out a loan of size L as a linear function of DTI and interest rate r:

$$v_i^o(L, r) = -\psi|d^*_{i,r_0}(r) - d_{i,r_0}(L)| + \xi^o + \epsilon_i^o \tag{11}$$

where d^*_{i,r_0} is the borrower's target DTI at the pre-policy interest rate r_0, $d_{i,r_0}(L)$ is the borrower's actual DTI as a function of loan size L evaluated at the pre-policy interest rate r_0, ψ is the borrower's disutility from not achieving their target DTI, ξ^o is a constant, and ϵ_i^o is a logit error. Thus, the borrower's utility increases if their DTI approaches their target. The value of the outside option of not getting a mortgage, v_i^n, is normalized to zero.

The borrower's target DTI may change as interest rates change (DeFusco and Paciorek, 2017). Therefore, we allow the borrower's target DTI to adjust based on the difference between the post-policy interest rate r and the pre-policy interest rate r_0, as in Bosshardt et al. (Forthcoming):

$$d^*_{i,r_0}(r) = d^*_{i,r_0}(r_0) - \gamma(r - r_0) \tag{12}$$

where parameter γ can be interpreted as the semi-elasticity of the borrower's target DTI for a given percentage point increase in the interest rate. We fit γ to the semi-elasticity of loan size to interest rates of -2.5, based on estimates from DeFusco and Paciorek (2017).

Note that we only allow interest rates to affect the borrower's loan demand by altering the borrower's target DTI in Equation (12), but not directly affect the borrower's demand for taking out a loan in Equation (11). This is because the recent literature finds only small direct effects of interest rates on borrower's demand (Bhutta and Ringo, 2021; Bosshardt et al., Forthcoming).[19] Furthermore, since our policy's effect on interest rates was small in general, we expect little effect from either channel.

Consumers' choice sets $\mathcal{A}_i(\theta_i)$ is determined by their DTIs and their perceived risks. We denote perceived risks as θ_i in the pre-period and θ_i' in the post period. During the underwriting process in our model, lenders apply cut-offs to applicant characteristics and accept borrowers with θ below the cutoffs. In

[19]These results were found in the FHA sample following a MIP cut in the Bhutta and Ringo (2021) and in response to rising interest rates overall in Bosshardt et al. (Forthcoming).

the pre-period, this decision rule may be represented as:

$$\mathcal{A}_i(\theta_i) = \begin{cases} \emptyset & \text{if } \theta_i > \bar{s}_0 \\ \{\text{DTI} < 43\} & \text{if } \theta_i \leq \bar{s}_0 \\ \{\text{DTI} < 50\} & \text{if } \theta_i \leq \bar{s}_0 + \bar{s}_{1,0} \\ \{\text{DTI} \leq 57\} & \text{if } \theta_i \leq \bar{s}_0 + \bar{s}_{1,0} + \bar{s}_{2,0} \end{cases} \tag{13}$$

where $\bar{s}_{1,0}, \bar{s}_{2,0} \leq 0$. In the post-period, this decision rule may be represented as:

$$\mathcal{A}'_i(\theta_i) = \begin{cases} \emptyset & \text{if } \theta'_i > \bar{s}_1 \\ \{\text{DTI} < 43\} & \text{if } \theta'_i \leq \bar{s}_1 \\ \{\text{DTI} < 50\} & \text{if } \theta'_i \leq \bar{s}_1 + \bar{s}_{1,1} \\ \{\text{DTI} \leq 57\} & \text{if } \theta'_i \leq \bar{s}_1 + \bar{s}_{1,1} + \bar{s}_{2,1} \end{cases} \tag{14}$$

where $\bar{s}_{1,1}, \bar{s}_{2,1} \leq 0$.

This decision rule is fairly general, and can capture a wide range of supply-side responses to the policy. We justify the generalizability of this setup by discussing an example.

Let $\{m_i^h, m_i^s\}$ be the hard and soft information of borrower i, and let m_i^s be obtainable only via human underwriting at a cost $c_i(m_i^h)$. To be clear, some of the information obtained via human underwriting may also be characterized as hard information, but we use the nomenclature of soft information to describe the information obtained by human underwriting to make clear its distinction from inputs to the AUS in this example. Suppose that the eligibility for a DTI<43 FHA mortgage is determined only by hard information, $e(m_i^h)$, and this is not affected by the policy. The policy affects the lenders' incentives surrounding the approval of DTI≥43 mortgages among low credit score borrowers, conditional on them being approved for a DTI<43 mortgage or $e(m_i^h) = 1$.

Suppose further that, pre-policy, the lender's policy for DTI≥43 mortgages is to obtain the soft information for borrowers of type m_i^h via manual underwriting if and only if their expected profit is higher than the cost of soft information acquisition:[20]

$$\int_0^\infty E_{l,0}(NPV|m_i^h, m_i^s)dG(m_i^s|m_i^h) - c_i(m_i^h) \geq 0 \tag{15}$$

where $E_{l,0}$ is the expectation of the lender pre-policy, $G(m_i^s|m_i^h)$ is the expected distribution of soft informa-

[20]We thank the discussant, Prof. Ansgar Walther, for proposing this framework which we generalize by allowing soft information acquisition costs c_i to depend on m_i^h.

tion m_i^s conditional on hard information m_i^h, and the lender accepts only loans where the lender has obtained the soft information and:

$$E_{l,0}(NPV|m_i^h, m_i^s) \geq 0. \tag{16}$$

Post-policy, lenders only make the decision of whether to obtain soft information when the applicant has been rejected by the AUS:

$$\int_0^\infty E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 0, m_i^s) dG(m_i^s|m_i^h) - c_i'(m_i^h) \geq 0 \tag{17}$$

And the lender's acceptance policy becomes:

$$E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 1) > 0 \text{ or } \{E_{l,1}(NPV|m_i^h, m_i^s) > 0 \text{ and soft information is obtained}\} \tag{18}$$

This is a fairly rich supply side model with two dimensions of borrower heterogeneity, the decision of whether to have a human screen a borrower or not that depends on their hard information, decision-making after screening that can flexibly depend on the expected payoffs, all interacted with the presence or absence of AUS recommendations. The cost of soft information acquisition, $c_i(m_i^h)$, is also allowed to flexibly shift to $c_i'(m_i^h)$ in the post period. This model is accommodated in our supply side estimation. In Appendix D.1, we show how θ_i and θ_i' can be constructed based on m_i^h, m_i^s even for this very flexible supply side.

Finally, given a lender decision $\mathcal{A}_i(\theta_i)$, the borrower maximizes their utility by deciding whether to get a mortgage and if so, what size of a loan to get, subject to lenders' approval. The observed loan size $\tilde{L}_i(r)$ thus follows a censored distribution:

$$\tilde{L}_i(r) = \begin{cases} \arg\max_{L \in \mathcal{A}_i(\theta_i)} v_i^o(L, r) \text{ , if } \max_{L \in \mathcal{A}_i} v_i^o(L, r) \geq 0 \\ 0 \text{ , otherwise,} \end{cases} \tag{19}$$

where $\mathcal{A}_i(\theta_i)$ represents the range of loan amount that can be accepted by a lender conditional on their perceived risk θ_i. For borrowers who are not able to get a mortgage at all, $\mathcal{A}_i = \emptyset$ and the borrower chooses the outside option with zero utility. The borrowers' utility conditional on their choice of $\tilde{L}_i(r)$ subject to constraint $\mathcal{A}_i$ implies a borrower surplus which we compute.

9.2 Moments

We fit our model to the borrowers' extensive margin response to the policy shock, their DTI distribution with and without the policy, and the borrowers' interest rate elasticity of demand. For the borrowers' extensive margin response to the policy shock and their DTI distribution with and without the policy, we use our bunching estimates from Section 4.2. In particular, we use the first row of column (1) of Table 2 for the full sample extensive margin response to the policy and the first row of of Table 4 for the subsamples. We compute the DTI distribution with and without the policy based on our bunching estimates, which is plotted in Figure 3 for the full sample and estimated separately for our demographic and income subsamples. The borrowers' interest rate semi-elasticity of demand is set to -2.5, based on estimates from DeFusco and Paciorek (2017) and following Bosshardt et al. (Forthcoming).

Overall, we match our model to 18 moments. The first set of 8 moments is the observed DTI distribution with the policy, for which we match on the mean plus the fraction of loans in 7 bins from 20 to 57. In the matching, we look at DTI bins of every five integer points except at 35–43 (policy threshold) and above 50. The second set of 8 moments are the counterfactual DTI distribution without the policy, for which we again match on the mean plus the fraction of loans in the same 7 bins. We also match on the extensive margin response to the human underwriting policy, which we call the policy elasticity, as well as the borrowers' estimated extensive margin response to a 50 bps interest rate cut.

9.3 Identification and Estimation

In terms of identification, $\mu_d, \sigma_d, \omega_d$ are identified by the general shape of the empirical DTI distribution which we assume to follow a skewed Normal distribution, whereas the under-writing cut-offs $\bar{s}_{1,0}, \bar{s}_{1,1}$ are identified by the bunching in the DTI 35–43 range relative to the DTI 43–45 range with and without the policy. Similarly, the under-writing cut-offs $\bar{s}_{2,0}, \bar{s}_{2,1}$ are identified by the increase in mass in the DTI 45–50 range relative to the DTI over 50 range with and without the policy. ψ is identified by the extensive margin response to the policy conditional on the relaxation of the DTI constraint. Finally, γ is identified by the borrowers' interest rate semi-elasticity of demand.

We estimate the model via generalized method of moments (GMM). The objective function is:

$$\min_{\theta}(\tilde{M}(\theta) - M)\hat{W}(\tilde{M}(\theta) - M)', \tag{20}$$

where $\tilde{M}$ is the vector of model implied moments at parameter θ, M is the vector of moments we match to, and $\hat{W}$ is the weighting matrix. We use a two-step GMM procedure, where we first use an identity weighting matrix and secondly use the optimal weighting matrix implied by the results of the first step.

We estimate 9 model parameters, and allow all the parameters to vary flexibly in each of the subsamples. To parametrize the model, we assume that d^*_{i,r_0} follows a skewed normal distribution with three parameters $\mu_d, \sigma_d, \omega_d$. θ_i and θ_i' are normalized to standard normal distributions with no loss of generality, and we estimate the underwriting cut-offs at 43 and 50 with and without the policy, $\bar{s}_{1,0}, \bar{s}_{2,0}, \bar{s}_{1,1}, \bar{s}_{2,1}$. Finally, we estimate the borrower's disutility from a higher interest rate γ and their disutility from meeting their DTI target ψ.

Of the remaining model parameters, ξ^o is not estimated but instead calibrated to the mortgage take-up rate among borrowers with a credit score less than 620 in our Experian data in a nested fixed-point as in Berry et al. (1995). Similarly, eligibility for a low DTI mortgage $\bar{s}_0$ is calibrated to the proportion of low credit score households who are employed and have more than $20,000 in non-housing assets or are already homeowners. In subsample analyses, we capture differences in the proportion of take-up across the race and income groups by scaling both factors by the proportion of low-credit-score mortgages from a particular race or income group and dividing by the proportion of population from that group with low credit scores. We test the robustness of our model to alternative calibrations of $\bar{s}_0$ in Appendix Section D.4, and it does not significantly impact our results. Details of these calculations are shown in Appendix D.2.

The estimated parameters are presented in Panel A of Table 10. In particular, the mean of the target DTI distribution across subsamples is between 0.35 to 0.40, the standard deviation is between 0.10 to 0.13, and the skewness is between 0.30 to 1.21.

<u>TABLE 10 ABOUT HERE</u>

There is some variation in the cut-offs $\bar{s}_{1,0}, \bar{s}_{2,0}, \bar{s}_{1,1}, \bar{s}_{2,1}$ which should be interpreted in the context of the calibrated $\bar{s}_0$ which varies by demographic subgroup. The estimated cutoffs for low- and high-DTI groups both with and without the policy (i.e., $\bar{s}_0 + \bar{s}_{1,0}$, $\bar{s}_0 + \bar{s}_{1,0} + \bar{s}_{2,0}$, $\bar{s}_0 + \bar{s}_{1,1}$, and $\bar{s}_0 + \bar{s}_{1,1} + \bar{s}_{2,1}$) are uniformly higher for non-Hispanic White applicants than Black applicants. This means that mortgage eligibility rates are lower for Black borrowers than non-Hispanic White borrowers across both DTI groups. Similarly, mortgage eligibility rates are lower for lower income households than higher income households across both DTI groups. Consistent with the existence of borrowers who crossed-over the the threshold, all subgroups experienced an increase in approval rates at 43 with the policy as $\bar{s}_{1,1}$ is lower than $\bar{s}_{1,0}$ for all subgroups. This means that the near zero take-up rate among Black borrowers we document cannot be purely explained by supply-side factors.

Our estimate for γ, the sensitivity of borrowers' target DTI to interest rate changes, varies from 0.857 to 1.030. As mentioned earlier, this is to fit the borrowers' interest rate semi-elasticity of demand to -2.5, based on estimates from DeFusco and Paciorek (2017).

In our full sample, our estimate of ψ is 0.270. By demographic groups, Black borrowers have a low estimated ψ and exhibit little sensitivity to "under-leverage," whereas Hispanic borrowers' sensitivities are in between those of non-Hispanic white borrowers and Black borrowers. The low sensitivity of Black borrowers may be due to them having limited resources for down payments, or having information constraints. This helps explain why Black households have little extensive margin response to the relaxation of the manual underwriting policy targeting high-DTI loans. We also find high-income borrowers have higher DTI sensitivity compared to low-income borrowers, consistent with the former group having a stricter preference for house size.

Panel B of Table 10 presents the fit of our model for each of the moments in the full sample in terms of the target moments, the model-implied moments, and the differences between the two. Despite having only half of the number of parameters as the number of moments, the model fits the target moments fairly well. The model fit in the subsamples is shown in Appendix D.3, which are qualitatively similar to the full sample fit.

9.4 Results

Table 11 presents our model results in terms of the policy's effect on consumer surplus as well as borrower eligibility for high DTI loans. We also dissect the source of the policy impact at the extensive margin.

TABLE 11 ABOUT HERE

Panel A presents the changes in consumer surplus brought about by the FHA policy change. We report the results from the full sample followed by results from the subsamples partitioned by race/ethnicity and income. Results from the full sample suggest that the policy change leads to a large increase in consumer surplus, by 11 percentage points. In the second row, we present the changes in consumer surplus for each ethnicity group. Consistent with the extensive margin effects, we find that non-Hispanic White borrowers derive an 11.6-percentage-point increase in consumer surplus, which is significantly higher compared to the welfare gain by Black borrowers (2.3 percentage points). Hispanic borrowers also gain significant consumer surplus from the policy, with a magnitude similar to non-Hispanic White borrowers. This result confirms that consumer surplus is mostly correlated with the extensive margin rather than the small differences in interest rates. Consistently, the third row shows that lower-income borrowers gain significantly less surplus, at 4.6 percentage points, compared to higher-income borrowers at 14.8 percentage points.

Panel B reports the percentage increases in the eligibility rate of high-DTI (above 43) loans from before to after the FHA policy change. These estimates represent the increase in the directly approval by the AUS post-policy, thus an expansion of credit supply. From the full-sample estimates (first row), we find a large and significant increase in the eligibility for high-DTI loans by 96 percentage points. Again, the eligibility for high-DTI loans increases significantly more for non-Hispanic White and higher income borrowers, at 115 percentage points. The credit expansion of high-leverage mortgage loans for Black borrowers is about 61 percentage points, less than two thirds of the magnitude compared to non-Hispanic White borrowers. Hispanic borrowers are somewhere in the middle, with their eligibility rate increasing by around 92 percentage points. The third row shows that, for lower-income borrowers, the credit expansion (52%) is around a third of the magnitude for higher-income ones (141%). These results indicate that the increased reliance on machine underwriting leads to differential supply expansion by borrower race/ethnicity as well as income.

Why did the increased reliance on machine underwriting lead to differential supply expansion by borrower race, given that race cannot be a direct input to the AUS (Bhutta et al., Forthcoming)?[21] There are at least two explanations. One is that borrowers of different racial groups have different input characteristics to the AUS. The other is that, driven by concerns about lawsuits for unequal lending standards, lenders may have relaxed human underwriting criteria for Black borrowers prior to the policy (although there is limited evidence of such negative overlays by race in the literature). Thus, as the switch to algorithmic underwriting reduced this concern, Black borrowers observe a smaller credit expansion.

Recall that in Table 5, we found large differences in credit uptake by race and income. Such differences can be attributed to two sources, one is the differential increase in credit supply across groups (i.e., the eligibility of high-DTI loans) and other is the difference in credit preferences across groups. An example of the latter dimension is that non-White borrowers may be constrained by liquidity or less informed of the policy change, so that they cannot take full advantage of the credit expansion. Leveraging on our model, we can decompose these two sources and assess to what extent the differences can be attributed to credit supply vs. borrower preference. We do so by computing the following statistics:

$$\frac{Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_{full}\}) - Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_e\})}{Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_{full}\}) - Pr(Uptake|\psi_e, \gamma_e; \{\bar{s}_e\})} \tag{21}$$

Where ψ and γ are borrower preference parameters and $\{\bar{s}_{full}\}$ is the eligibility standards for high-DTI loans. The subscript *full* represents the parameter values estimated for the full sample borrowers, and e represents the parameter values of a specific demographic group (i.e., Black, lower-income, etc.). Thus, $Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_{full}\})$ indicates the loan uptake rates for the full sample borrowers, and $Pr(Uptake|\psi_e, \gamma_e; \{\bar{s}_e\})$ is the loan uptake of the subgroup. In this expression: $Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_e\})$ represents a "pseudo" uptake rate for the demographic group by artificially assigning it the preferences of the average borrower in the population. This fraction informs us what percentage of the difference in loan uptake between the full population and the subgroup is driven by supply-side differences.

Take low-income group as an example. We first compute the difference in the average credit uptake rate of high-DTI loans between the full sample and the low-income borrowers. We then artificially assign the preference of an average borrower in the full sample to the low-income group, and recompute the differences in credit uptake rates between the two groups. This step essentially allows us to "hold-fix" the preference parameters and let the supply expansion (eligibility parameters) to drive the changes in credit uptake. As we take the ratio of the two differences, the result indicates what fraction of the difference in credit uptake is driven by supply-side factors rather than borrower preferences.

The results are shown in Panel C. We omit the results for the non-Hispanic White as well as Hispanic

[21] Bhutta et al. (Forthcoming) also note that conditional on input characteristics, lender overlays on top of AUS decisions play a limited role in causing disparities in mortgage approvals.

borrowers because their extensive margin results are similar to that of the full sample. Results in the first row suggest that around 32% of the muted extensive margin response for Black borrowers can be attributed to a more limited supply expansion for these borrowers. This also means that 68% of the difference can be attributed to demand differences. For example, Black borrowers may face other constraints such as down payment and information constraints, which dampens their demand response despite a lower but still significant supply expansion. Results in the second row suggest that credit supply plays a larger role in explaining the differences in the extensive margin responses across income levels. Around 62% of the increase in loan uptake by lower-income borrowers can be attributed to the differences in supply expansion. In contrast, our estimates suggest that a much higher fraction of the increased credit uptake for higher-income borrowers is explained by credit supply, at 143%.

10 Conclusion

Algorithmic underwriting is increasingly important in financial markets today. We study the impacts of the increasing reliance on algorithmic underwriting in a high-risk segment of the U.S. mortgage market by examining an FHA policy that transitioned from pure human underwriting to human-augmented algorithmic underwriting for low-credit-score, high-leverage borrowers. We document that the policy change led to sizable gains in credit supply and consumer welfare without significantly increasing default rates conditional on observables. These results suggest that a growing reliance on algorithmic underwriting can potentially improve underwriting efficiency, as the incremental risk management value of requiring human discretion appears to be limited for most borrowers in this market. At the same time, these consumer welfare gains are not equally distributed; instead, they are concentrated on White and high-income borrowers. The disparate effects highlight the difficulties associated with increasing financial inclusion in certain segments of the market, which may justify further study.

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Table 1. Summary Statistics

Panel A describes the summary statistics of the Ginnie Mae-Endorsements-HMDA matched sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy change. Panel B describes the summary statistics of the sample of individuals in the 1% national representative sample of credit bureau annual records from 2014 to 2019 (excluding 2016). *Delinquency* is an indicator variable that takes the value of one if the loan is more than 90 day delinquent within two years of the first payment date. *Rate Spread* measures the mortgage interest rate spread over the 30-year Freddie Mac survey rate. *FICO* measures the FICO score of the borrower. *DTI* measures the borrower's debt-to-income ratio. *Low FICO* is an indicator variable that takes the value of one if the borrower's FICO score is below 620. *High DTI* is an indicator variable that takes the value of one if the borrower's DTI is greater than or equal to 43. *Income* measures the borrower's income in thousands. *Loan Amount* measures the amount of the loan in thousands. *Non-Hispanic White* is an indicator variable that takes the value of one if the borrower's race is reported as White and ethnicity is not reported as Hispanic. *Black (Hispanic)* is an indicator variable that takes the value of one if the borrower's race (ethnicity) is reported as Black (Hispanic). *School Rating* measures the average rating of the school district where an individual lives. *School Rating Cond. Purchase* measures the average rating of the school district, conditioning on the sample of individuals who have a new FHA purchase in a given year. *d(School Rating)* is the change in school district rating for an individual from the previous year to the current year. *d(School Rating) Cond. Purchase* is *d(School Rating)*, computed for the sample of individuals who have a new FHA purchase in a given year. *New Purchase FHA* is an indicator variable that takes the value of one if an individual has obtained a new FHA mortgage purchase in a given year. *Age* measures the age of the individual. *Female* is an indicator variable that takes the value of one if an individual is a female. *Married* is an indicator variable that takes the value of one if an individual is married. *FICO* measures the FICO score of the borrower reported in the credit bureau data.

Panel A: Ginnie Mae-HMDA Sample

	Mean	SD	P25	Median	P75	N
Delinquency	0.059	0.236	0.000	0.000	0.000	703,140
Rate Spread	0.138	0.424	-0.155	0.095	0.390	705,267
FICO	678.363	47.882	644.000	672.000	708.000	705,267
DTI	41.238	9.194	34.970	42.100	48.330	705,267
Low FICO	0.075	0.264	0.000	0.000	0.000	705,267
High DTI	0.460	0.498	0.000	0.000	1.000	705,267
Income	71.645	38.911	45.000	64.000	89.000	705,267
Log(Income)	4.148	0.495	3.807	4.159	4.489	705,267
Loan Amount	202.549	102.579	130.000	184.000	254.000	705,267
Log(Loan Amount)	12.091	0.512	11.768	12.123	12.441	705,267
Non-Hispanic White	0.609	0.488	0.000	1.000	1.000	705,267
Black	0.119	0.324	0.000	0.000	0.000	705,267
Hispanic	0.165	0.372	0.000	0.000	0.000	705,267

Panel B: Credit Bureau Sample

	Mean	SD	P25	Median	P75	N
School Rating	5.294	1.340	4.400	5.200	6.158	10,698,445
School Rating Cond. Purchase	5.182	1.253	4.333	5.134	6.000	35,967
d(School Rating)	0.002	0.525	0.000	0.000	0.000	10,698,445
d(School Rating) Cond. Purchase	-0.022	1.059	0.000	0.000	0.000	35,967
New Purchase FHA	0.003	0.058	0.000	0.000	0.000	10,698,445
Age	51.879	19.243	36.000	51.000	65.000	10,698,445
Female	0.498	0.500	0.000	0.000	1.000	10,698,445
Married	0.562	0.496	0.000	1.000	1.000	10,698,445
FICO	684.384	107.236	604.000	692.000	784.000	10,698,445

Table 2. Extensive and Intensive Margin Effects on the Quantity of Credit
This table examines the extensive and intensive margin changes in loan origination volume around the changes in underwriting regulations, using the methodology described in Section 4.2. Δ*Loans Originated* refers to the increase in the total number of new purchase loans extended to low FICO borrowers as a fraction of the number of new purchase loans extended to low FICO borrowers in the absence of the policy. Δ*Average DTI* refers to the average increase in measured DTI of these new purchase loans as a result of the policy. Δ*Low DTI Loans* refers to change in low-DTI loans as a fraction of the total number of new purchase loans extended to low FICO borrowers as a result of the policy change. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Standard errors are reported in parentheses and are computed from 1,000 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Baseline	Alternative Specifications		
	$\bar{d}$ = 35	$\bar{d}$ = 32	$\bar{d}$ = 34	$\bar{d}$ = 36
	(1)	(2)	(3)	(4)
Δ*Loans Originated*	0.103***	0.103***	0.101***	0.101***
	(0.016)	(0.020)	(0.017)	(0.014)
Δ*Average DTI*	1.324***	1.335***	1.326***	1.329***
	(0.139)	(0.139)	(0.139)	(0.139)
Δ*Low DTI Loans*	-0.086***	-0.084***	-0.087***	-0.088***
	(0.009)	(0.012)	(0.010)	(0.008)
Observations	648,119	648,119	648,119	648,119

Table 3. Delinquency Rates

This table examines the changes in mortgage delinquency rates around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Panel A reports results from the DID analysis following Equation 7, Panel B reports the triple-difference analysis following Equation 8. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount and log of borrower household income. Column (1) of Panel B additionally controls for the remaining triple interaction terms. Variable definitions are provided in Table 1. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00867	-0.00648	-0.00323	-0.000640	-0.000317	0.00144
	(0.0115)	(0.0120)	(0.0123)	(0.00572)	(0.00594)	(0.00624)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324256	323512	323245	203345	202698	202375
R^2	0.028	0.031	0.060	0.029	0.032	0.065

Panel B. Delinquency, Triple-Difference Results

Dep. Var.: *Delinquency Rate*	(1)	(2)	(3)
Treated × *High DTI* × *Post*	-0.00810	-0.00640	-0.00522
	(0.0116)	(0.0130)	(0.0129)
Treated × *Post*	-0.000290	0.000962	0.00111
	(0.00543)	(0.00588)	(0.00568)
High DTI × *Post*	0.00112	-0.00363	-0.00163
	(0.00117)	(0.00248)	(0.00382)
Controls		Yes	Yes
Month FE	Yes		
FICO FE	Yes		
FICO-DTI FE		Yes	Yes
Month-DTI FE		Yes	Yes
County FE		Yes	Yes
Lender FE			Yes
Observations	527604	526104	526046
R^2	0.028	0.050	0.055

Table 4. Delinquency Rates Effects by Unemployment Rate Change Quartiles

This table examines the changes in mortgage delinquency around the 2016 FHA policy change, across borrowers in regions in each quartile of changes in unemployment rate. Unemployment rate change is the percentage change from year $t-1$ to t. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued from August 2015 to August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. The outcome variable is 90-day delinquency rates. *Low FICO* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *High DTI* (*Low DTI*) represents borrowers with DTI above 43 (35 to 43). *Post* indicates whether the loan is extended after the regulation change in August 2016. Variable definitions are provided in Table 1. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: DID, High DTI Loans ($DTI > 43$)

Dep. Var.: *Delinquency Rate* Sample: Unemp Growth	(1) Qtile 1 (Lowest)	(2) Qtile 2	(3) Qtile 3	(4) Qtile 4 (Highest)
Treated × *Post*	-0.00638	0.00609	0.0148	-0.0342
	(0.0121)	(0.0139)	(0.0216)	(0.0246)
Controls	Yes	Yes	Yes	Yes
FICO-DTI FE	Yes	Yes	Yes	Yes
Month-DTI FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	81060	82117	82102	77359
R^2	0.065	0.064	0.068	0.068

Panel B: DID, Low DTI Loans ($35 \leq DTI \leq 43$)

Dep. Var.: *Delinquency Rate* Sample: Unemp Growth	(1) Qtile 1 (Lowest)	(2) Qtile 2	(3) Qtile 3	(4) Qtile 4 (Highest)
Treated × *Post*	-0.00800	-0.000925	0.0136***	0.0121
	(0.00703)	(0.00678)	(0.00385)	(0.00805)
Controls	Yes	Yes	Yes	Yes
FICO-DTI FE	Yes	Yes	Yes	Yes
Month-DTI FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	94309	93615	93909	97038
R^2	0.067	0.064	0.070	0.063

Panel C: Triple Difference, $DTI \geq 35$

Dep. Var.: *Delinquency Rate* Sample: Unemp Growth	(1) Qtile 1 (Lowest)	(2) Qtile 2	(3) Qtile 3	(4) Qtile 4 (Highest)
Treated × *Post* × *High DTI*	-0.00198	0.00704	0.00203	-0.0470*
	(0.0213)	(0.0168)	(0.0267)	(0.0260)
Controls	Yes	Yes	Yes	Yes
FICO-DTI FE	Yes	Yes	Yes	Yes
Month-DTI FE	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes
Observations	175644	175916	176214	174685
R^2	0.058	0.059	0.064	0.059

Table 5. Heterogeneity by Income and Race

This table examines the changes in loan origination volume and delinquency rates around the changes in underwriting regulations for subsamples of borrowers. Δ*Loans Originated* refers to the increase in the total number of new purchase loans extended to low FICO borrowers as a fraction of the number of new purchase loans extended to low FICO borrowers in the absence of the policy. Panel A (B) examines the heterogeneous effects of credit origination across borrower race (income categories), and Panel C reports the heterogeneous effects for delinquency rates across borrower race and income. The methodology is described in Section 4.2. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Standard errors are reported in parentheses and are from 1,000 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Heterogeneity Across Race

Race:	(1) Non-Hispanic White	(2) Black	(3) Hispanic
Δ*Loans Originated*	0.108*** (0.018)	0.014 (0.040)	0.109** (0.043)
Observations	428,086	83,120	112,658

Panel B. Heterogeneity Across Income Categories

Income:	(1) Below Median	(2) Above Median
Δ*Loans Originated*	0.038 (0.025)	0.136*** (0.019)
Observations	324,061	324,058

Panel C. Delinquency Rates Across Income and Race

Dep. Var: *Delinquency Rate* (90-day)	High DTI (>43)		Low DTI ($35 \leq DTI \leq 43$)	
	(1)	(2)	(3)	(4)
Non-Hispanic White	-0.0064 (0.00697)	-0.00334 (0.00578)	0.00467 (0.0089)	0.004 (0.00909)
Black	0.0236 (0.0285)	0.0316 (0.027)	-0.00611 (0.011)	-0.000334 (0.0122)
Hispanic	-0.0366 (0.0229)	-0.0352 (0.0241)	-0.0103 (0.0159)	-0.0124 (0.0147)
Income Below Median	0.0000724 (0.0122)	0.00283 (0.0112)	0.000234 (0.00754)	0.0017 (0.0083)
Income Above Median	-0.00967 (0.0135)	-0.0061 (0.0144)	0.00158 (0.00855)	0.00385 (0.00812)
Controls	Yes	Yes	Yes	Yes
Month FE	Yes		Yes	
FICO-DTI FE	Yes	Yes	Yes	Yes
Month-DTI FE		Yes		Yes
County FE		Yes		Yes
Lender FE		Yes		Yes

Table 6. HMDA Denial Probabilities

This table examines changes in mortgage denial rates around the FHA underwriting policy change. The sample is limited to applications for FHA mortgages in 2015 and 2017 HMDA data. *%Low FICO (County)* is a continuous variable measuring the fraction of individuals with FICO score under 620 in a given county in 2015 in our Experian data. *High Approx DTI* is an indicator variable that equals one when the approximate DTI>= 43. The approximate DTI is computed based on estimated debt payment over income. Estimated debt payment is the sum of the mortgage payment implied by the application mortgage balance, the average FHA interest rate in a given year, and the FHA MIP, and the imputed remaining debt payment for each racial group and income category, where the income category is in bins of size $1,000. *Controls* include the log loan amount. *Denial* is defined as an indicator variable for "Action Taken"=3 in the HMDA data. Incomplete and withdrawn applications are dropped. Standard errors are reported in parentheses and are double clustered by lender and county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Denial, Difference-in-difference Results

Sample	High Approx DTI (> 43)			Low Approx DTI (≤ 43)		
Dep. Var.: *Delinquency*	(1)	(2)	(3)	(4)	(5)	(6)
% Low FICO (County) × Post	-0.168**	-0.160**	-0.131*	-0.0441	-0.0234	-0.00764
	(0.0745)	(0.0749)	(0.0671)	(0.0400)	(0.0397)	(0.0329)
Controls		Yes	Yes		Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Lender FE			Yes			Yes
Observations	1160173	1160173	1159896	735110	735110	734842
R^2	0.014	0.026	0.095	0.018	0.020	0.085

Panel B. Denial, Triple-Difference Results

Dep. Var.: *Delinquency Rate*	(1)	(2)	(3)
% Low FICO (County) × High Approx DTI × Post	-0.131**	-0.136**	-0.132**
	(0.0624)	(0.0639)	(0.0574)
% Low FICO (County) × Post	-0.0340	-0.0148	0.00151
	(0.0398)	(0.0394)	(0.0338)
High Approx DTI × Post	-0.00168	-0.000807	-0.000643
	(0.00693)	(0.00681)	(0.00488)
% Low FICO (County) × High Approx DTI	0.358***	0.336***	0.309***
	(0.0692)	(0.0719)	(0.0642)
High DTI	0.0140***	0.0274***	0.0328***
	(0.00433)	(0.00467)	(0.00481)
Controls		Yes	Yes
County FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Lender FE			Yes
Observations	1895482	1895482	1895205
R^2	0.014	0.022	0.087

Table 7. Heterogeneity by Lender Types

This table examines changes in loan origination volume around the FHA underwriting policy change for subsamples of lenders. Δ*Loans Originated* refers to the increase in the total number of new purchase loans extended to low FICO borrowers as a fraction of the number of new purchase loans extended to low FICO borrowers in the absence of the policy. Panel A reports the effects across markets with different levels of congestion, Panel B reports results for bank and nonbank lenders, and Panel C reports the share of low FICO, high DTI loans post-policy that were made by existing versus new lenders in this market. Lender congestion is measured by year-on-year application growth during the year of origination in a local market, defined as a lender-state. Non-banks are defined as independent mortgage lenders (IMBs) in the Avery file. Existing lenders in this market are defined by lenders that have made at least one low FICO, high DTI loan in our pre-policy period. FinTech lenders, as defined in Fuster et al. (2021), are not present in our market before or after the policy change. The methodology is described in Section 4.2. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Standard errors are reported in parentheses and are from 1,000 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Heterogeneity Across Lender Congestion

Lender Congestion:	(1) Below Median	(2) Above Median
Δ*Loans Originated*	0.118***	0.046**
	(0.023)	(0.023)
Number of Observations	300,854	299,129

Panel B. Heterogeneity Across Bank and Nonbank Lenders

	(1) Bank	(2) Non-Bank
Δ*Loans Originated*	0.130***	0.091***
	(0.034)	(0.018)
Number of Observations	180,259	467,850

Panel C. Share of New Versus Existing Lenders Post-Policy

	(1) Existing Lenders	(2) New Lenders
Share of low FICO, high DTI loans	0.805***	0.195***
	(0.004)	(0.004)
Number of Lenders	224	394

Table 8. Interest Rate Spreads

This table examines the changes in interest rate spreads around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Panel A reports results from the DID analysis following Equation 7, and Panel B reports the triple-difference analysis following Equation 8. The dependent variable is the interest rate spreads relative to the Freddie Mac Survey rate. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount and log of borrower household income. Variable definitions are provided in Table 1. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Interest Rate Spreads, Difference-in-Difference

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Interest Rate Spreads*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00223	0.0147	0.0121	0.0394***	0.0336**	0.0225
	(0.0212)	(0.0230)	(0.0216)	(0.0105)	(0.0109)	(0.0120)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	325187	324425	324153	204076	203415	203092
R^2	0.230	0.245	0.461	0.255	0.272	0.502

Panel B. Interest Rate Spreads, Triple-Difference

Dep. Var.: *Interest Rate Spreads*	(1)	(2)	(3)
Treated × *High DTI* × *Post*	0.00506	-0.0189	-0.0118
	(0.00943)	(0.0151)	(0.0178)
Treated × *Post*	-0.00621	-0.0315***	0.0277***
	(0.00537)	(0.00660)	(0.00383)
High DTI × *Post*	0.0341***	0.0345**	0.0235*
	(0.0110)	(0.0146)	(0.0127)
Controls		Yes	Yes
Month FE	Yes		
FICO FE	Yes		
FICO-DTI FE		Yes	Yes
Month-DTI FE		Yes	Yes
County FE			Yes
Lender FE			Yes
Observations	529267	527842	527673
R^2	0.243	0.259	0.474

Table 9. Mortgage Access and the Quality of Neighborhoods: 2SLS

This table uses 2SLS specifications to examine the effect of mortgage access on moves to opportunity. The sample includes individuals in the 1% national representative sample of credit bureau annual records from 2014 to 2019 (excluding 2016), and is merged with the school rating data based on the location of individuals. The unit of observation is an individual-year. Panel A reports first-stage estimates where the dependent variable is an indicator *New Purchase FHA* that equals one if an individual has obtained a new FHA mortgage purchase in a given year. Panel B reports second-stage estimates of the new FHA mortgage purchase on changes in school quality due to moving. *d(School Rating)* equals the difference between the rating of the school district where the individual currently lives and the rating of the school district where she lived in the previous year. *Treated (2015)* is an indicator that equals one if the borrower's credit score is below 620 in 2015, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in 2016. Individual characteristics include indicators for gender, marital status, and Treat (2015). Age group fixed effects are dummy variables for each of five-year age categories (i.e., 20–24, 25–29, etc.). A placebo test of this analysis for renters is shown in Appendix Table B.7. Standard errors are reported in parentheses and are clustered by county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. First Stage, Obtaining FHA Mortgage

Dep. Var.: *New Purchase FHA*	(1)	(2)	(3)
Post × Treat (2015)	0.0019***	0.0018***	0.0018***
	(0.0001)	(0.0001)	(0.0001)
Individual Char	Yes	Yes	Yes
Year FE	Yes		Yes
FICO FE	Yes	Yes	Yes
Zipcode FE	Yes		
Zipcode-Year FE		Yes	
Gender-Zipcode FE			Yes
Age Group-Zipcode FE			Yes
Married-Zipcode FE			Yes
Observations	10,698,445	10,690,370	10,698,445
R^2	0.01	0.01	0.03
F-statistic	380.40	313.03	319.34

Panel B. Second Stage, Changes in School Quality

Dep. Var.: *d(School Rating)*	(1)	(2)	(3)
New Purchase FHA	1.9332***	1.1625**	1.8315***
	(0.5196)	(0.5414)	(0.5302)
Individual Char	Yes	Yes	Yes
Year FE	Yes		Yes
FICO FE	Yes	Yes	Yes
Zipcode FE	Yes		
Zipcode-Year FE		Yes	
Gender-Zipcode FE			Yes
Age Group-Zipcode FE			Yes
Married-Zipcode FE			Yes
Observations	10,698,445	10,690,370	10,698,445

Table 10. Model estimates

This table displays our structural model parameter estimates for our full sample and within race/ethnicity as well as income subsamples in Panel A, and the fit for our full sample estimates in Panel B. In Panel A, $\mu_d, \sigma_d, \omega_d$ are parameters that define the shape of the consumers' pre-policy DTI target. $\bar{s}_{1,1}, \bar{s}_{2,1}, \bar{s}_{1,0}, \bar{s}_{2,0}$ are parameters that define the acceptance cut-off for higher DTI loans with and without the policy. ψ represents the borrowers' disutility from not meeting their DTI target, and γ represents the borrowers' disutility utility from paying a higher interest rate. GMM standard errors are reported in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively. In Panel B, $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 2, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Panel A. Model parameter estimates

	Full Sample	Race/Ethnicity Subsample			Income	
		Non-Hispanic White	Black	Hispanic	Below Med	Above Med
μ_d	0.355***	0.387***	0.402***	0.361***	0.395***	0.315***
	(0.00213)	(0.00387)	(0.00397)	(0.00758)	(0.00535)	(0.00187)
σ_d	0.130***	0.105***	0.103***	0.131***	0.105***	0.175***
	(0.00169)	(0.00174)	(0.00225)	(0.00726)	(0.00263)	(0.00333)
ω_d	0.981***	0.304***	0.369***	1.200***	0.418***	1.890***
	(0.0288)	(0.0492)	(0.0529)	(0.167)	(0.0719)	(0.0689)
$\bar{s}_{1,1}$	-0.188***	-0.231***	-0.0784***	-0.145***	-0.173***	-0.230***
	(0.0118)	(0.0232)	(0.0129)	(0.0241)	(0.0166)	(0.0188)
$\bar{s}_{2,1}$	-0.180***	-0.214***	-0.115***	-0.144***	-0.219***	-0.203***
	(0.0108)	(0.0211)	(0.0196)	(0.0258)	(0.0175)	(0.0176)
$\bar{s}_{1,0}$	-0.616***	-0.740***	-0.347***	-0.572***	-0.448***	-0.792***
	(0.0145)	(0.0213)	(0.0153)	(0.0311)	(0.0191)	(0.00965)
$\bar{s}_{2,0}$	-0.0105**	-0.0107	-0.0417***	-0.0103	-0.0100	-0.0698***
	(0.00448)	(0.0219)	(0.0137)	(0.0176)	(0.00949)	(0.0104)
ψ	0.264***	0.357***	0.0332	0.229***	0.135***	0.306***
	(0.0342)	(0.0554)	(0.0256)	(0.0491)	(0.0359)	(0.0282)
γ	0.954***	0.946***	1.030***	1.000***	0.927***	0.857***
	(0.0736)	(0.0711)	(0.0791)	(0.0775)	(0.0767)	(0.072)

Panel B. Model fit for full sample

Parameter	Target	Model	Difference
DTI Distribution, Post-Policy			
Fraction of loans in range			
$DTI_1 > 50$	0.113	0.119	0.006
$45 < DTI_1 \leq 50$	0.161	0.167	0.006
$43 < DTI_1 \leq 45$	0.079	0.068	-0.012
$35 < DTI_1 \leq 43$	0.372	0.370	-0.002
$30 < DTI_1 \leq 35$	0.142	0.143	0.001
$25 < DTI_1 \leq 30$	0.082	0.080	-0.003
$20 < DTI_1 \leq 25$	0.036	0.038	0.003
Avg DTI ($\overline{DTI_1}$)	0.403	0.400	-0.003
DTI Distribution, Pre-Policy			
Fraction of loans in range			
$DTI_0 > 50$	0.085	0.083	-0.002
$45 < DTI_0 \leq 50$	0.081	0.085	0.004
$43 < DTI_0 \leq 45$	0.036	0.036	0.000
$35 < DTI_0 \leq 43$	0.494	0.489	-0.005
$30 < DTI_0 \leq 35$	0.158	0.160	0.002
$25 < DTI_0 \leq 30$	0.089	0.087	-0.002
$20 < DTI_0 \leq 25$	0.041	0.043	0.002
Avg DTI ($\overline{DTI_0}$)	0.390	0.386	-0.004
Policy elasticity	0.103	-1.928	-2.031
Interest rate semi-elasticity	-0.025	-0.026	-0.001

Table 11. Model results

This table examines the changes in consumer surplus and DTI>43 eligibility following the policy. The percent change in consumer surplus is defined as the post-policy consumer surplus divided by the counterfactual consumer surplus without the policy multiplied by one hundred and minus one hundred. The percent change in DTI>43 eligibility is defined as the post-policy model-implied eligibility for DTI>43 mortgages divided by the counterfactual model-implied eligibility without the policy multiplied by one hundred and minus one hundred. The percent differences in extensive margin response attributable to supply side differences is computed as the percent of the extensive margin response difference relative to the full sample that is closed when the supply side effects that is specific to each demographic and income group is applied to the full sample borrower model demand parameters. The point estimates are from the model's point estimates as presented in Table 10. The 95% confidence intervals computed via 1,000 parameter draws from their estimated covariance matrix are shown in square brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A: % Changes in Consumer Surplus

Full Sample		11.605***	
		[9.836, 13.054]	
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	11.802***	2.315	12.523***
	[9.761, 13.306]	[-0.888, 5.369]	[8.968, 15.239]
Income:		Below Median	Above Median
		4.564***	14.824***
		[2.418, 6.223]	[13.680, 15.820]

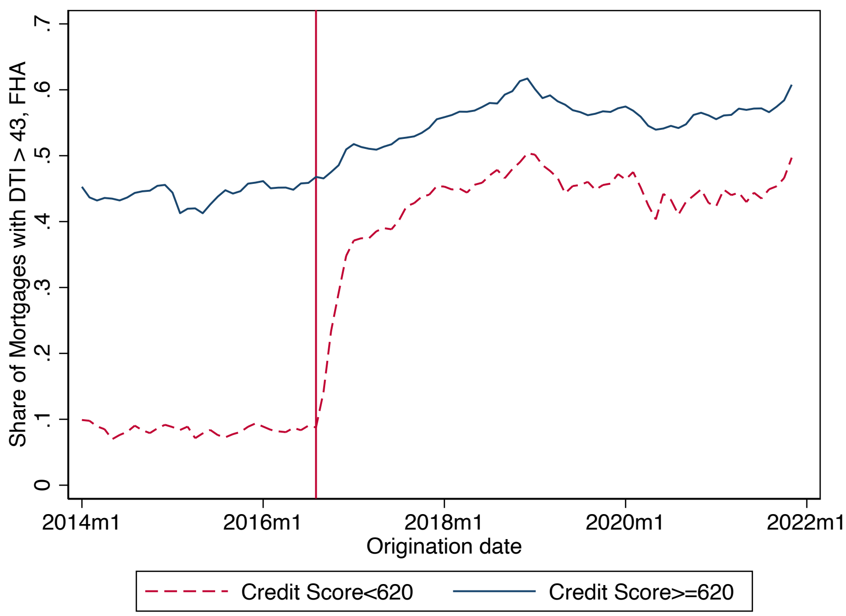
Panel B: % Changes in High-DTI Eligibility

Full Sample		96.582***	
		[92.063, 101.410]	
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	115.285***	61.305***	92.997***
	[103.781, 126.955]	[55.008, 67.024]	[79.069, 107.771]
Income:		Below Median	Above Median
		52.612***	141.351***
		[48.558, 57.029]	[134.086, 149.609]

Panel C: % Differences in Extensive Margin Response Attributable to Supply Side Differences

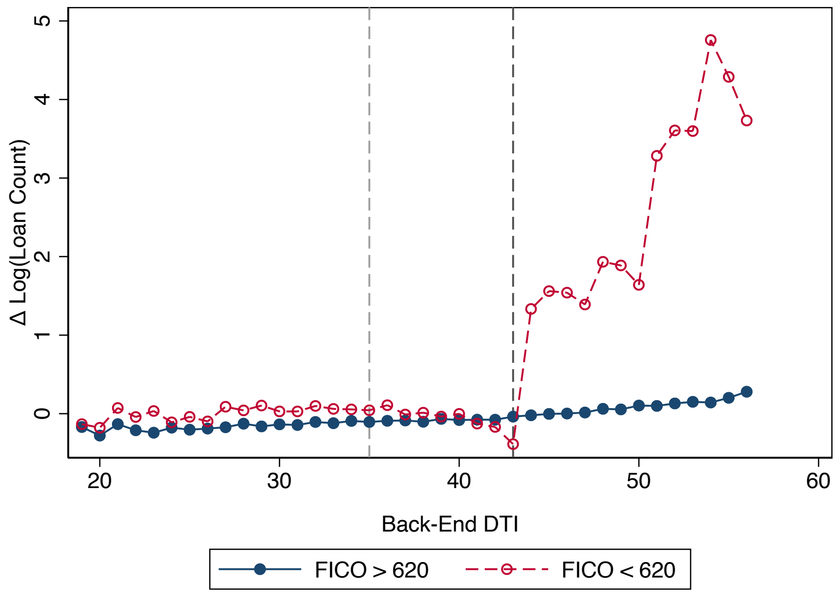
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	-	31.861***	-
	-	[23.646, 45.209]	-
Income:		Below Median	Above Median
		62.024***	143.729***
		[45.706, 82.186]	[106.537, 214.966]

Figure 1. Effect of the policy change on the share of high DTI mortgages



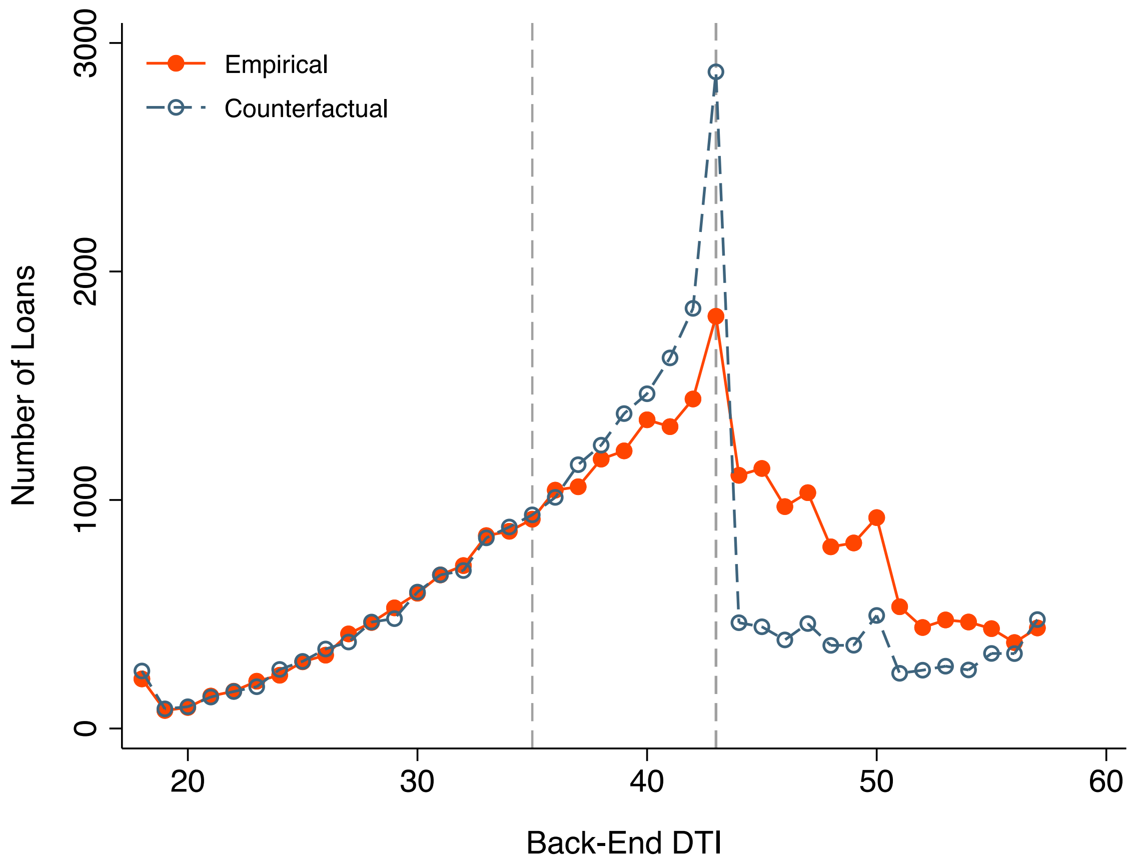
Note: This figure plots the share of FHA new purchase mortgages with DTI greater than or equal to 43 by their month of origination. The sample is the full sample of FHA loans in our Ginnie Mae data from January 2014 to January 2022. The red, dashed line represents borrowers with credit score less than 620 (i.e., low-credit-score) and the blue solid line represents borrowers with credit score greater than or equal to 620 (i.e., high-credit-score). The policy month of August 2016 is marked via a vertical red line. The effect of the policy change in our Ginnie Mae-Endorsements-HMDA sample is shown in Appendix Figure B.1.

Figure 2. Loan growths around the FHA removal of human underwriting mandate



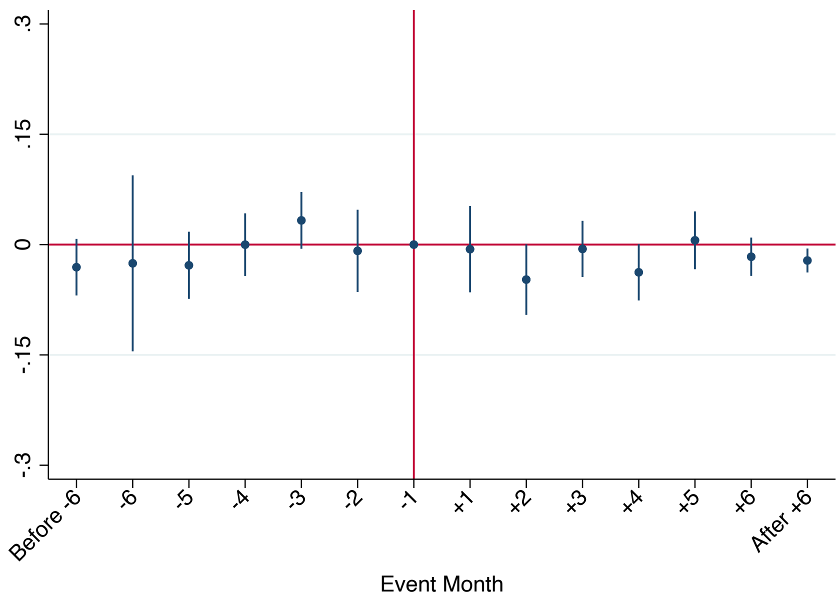
Note: This figure plots the log difference of the number of loans between the 12 months after the policy and the number of loans 12 months before the policy by DTI. The loan counts include all FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Dashed lines are drawn at DTI equals 43, above which the policy takes into affect, and at DTI equals 35, at or below which we assume is unaffected by the policy for our baseline bunching analysis. We show that this assumption along with a parallel trends assumption fits the data well for DTI≤35 borrowers in Figure 3.

Figure 3. Effect of the policy change on loan quantities by DTI



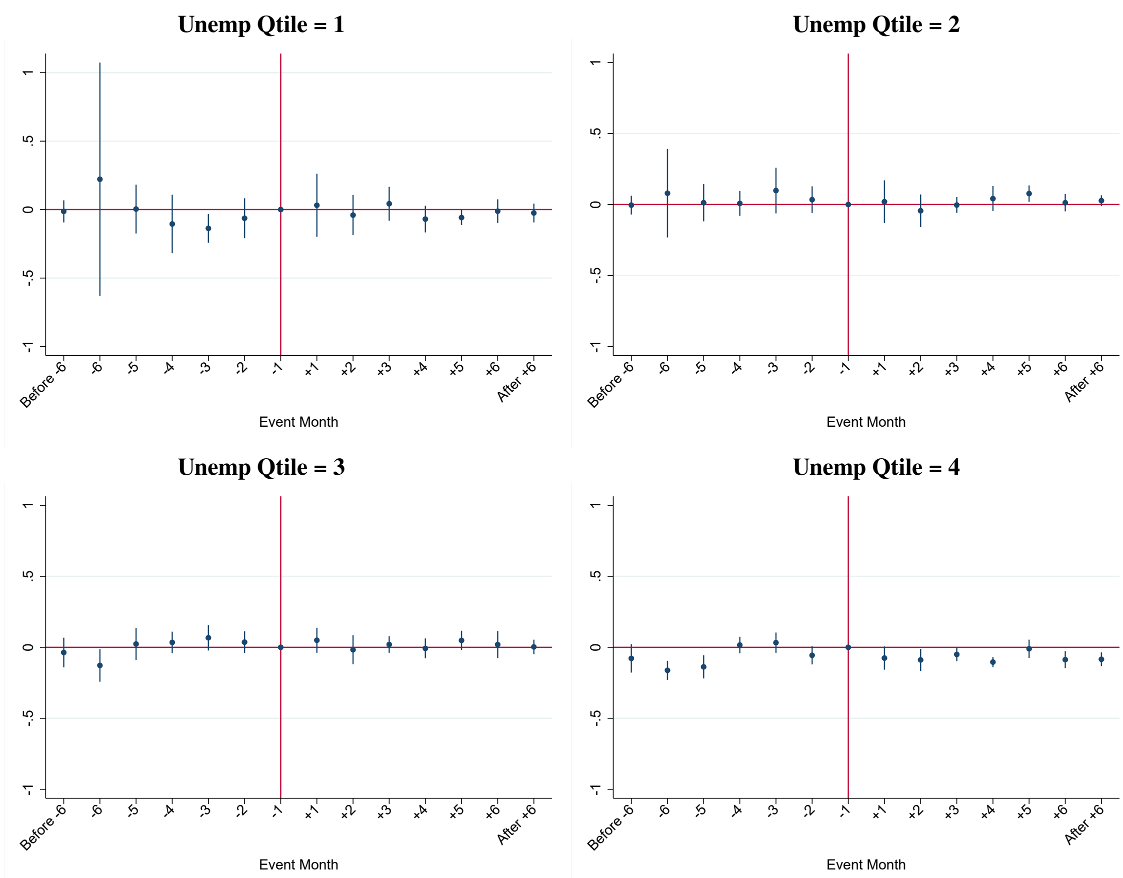
Note: This figure plots empirical and counterfactual numbers of FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample during 12 months after the policy based on the methodology described in Section 4.2. DTI is winsorized at the 1st and 99th percentiles and rounded down to the nearest integer. Dashed lines are drawn at DTI equals 43, above which the policy takes into affect, and at DTI equals 35, at or below which we assume is unaffected by the policy for our baseline bunching analysis. We show in this figure that this assumption along with a parallel trends assumption fits the data well for DTI≤35. We also plot the empirical and counterfactual numbers for a placebo period of one year prior to our policy in Appendix Figure B.2.

Figure 4. Trends in delinquency rates



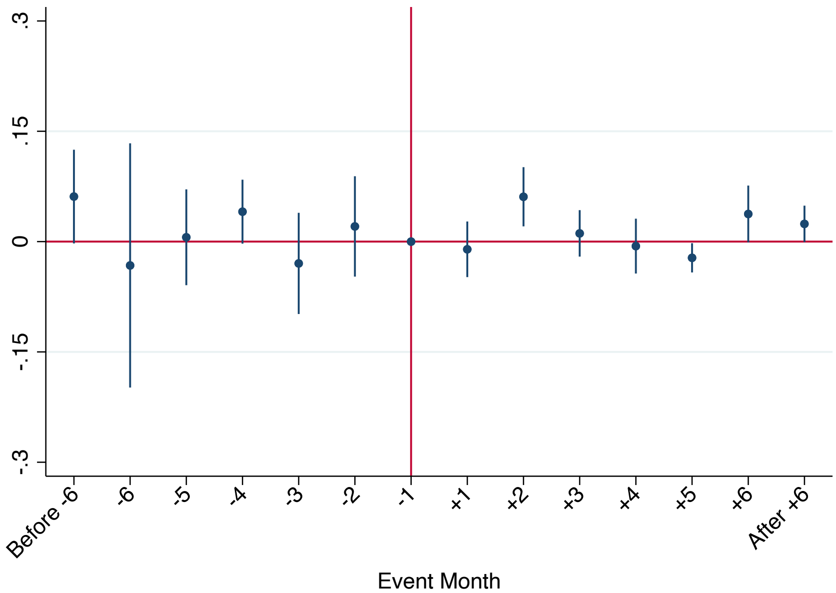
Note: We estimate dynamic triple-difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. The outcome variable is the 90-day delinquency indicator measured in the two years post origination. The fixed effects and control variables are the same as those used in Table 3 Panel B Column (3). We use the month prior to August 2016 as the base period for estimation (Event Month = -1).

Figure 5. Trends in delinquency by quartiles of unemployment rate change



Note: We estimate dynamic triple-difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. The outcome variable is 90-day delinquency indicator measured in the two years post origination. The fixed effects and control variables used are the same as those used in Table 3 Panel B Column (3). We use the month prior to August 2016 as the base period for estimation (Event Month = -1). We split the samples based on the quartile of unemployment rate growth, measured as the year-on-year change in unemployment rate in the county of the loan application.

Figure 6. Trends in interest rate spreads



Note: We estimate dynamic triple-difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. The outcome variable is mortgage interest rate spread. The fixed effects and control variables are the same as those used in Table 8 Panel B Column (3). We use the month prior to August 2016 as the base period for estimation (Event Month = -1).

Internet Appendix

This appendix supplements the empirical analysis of this paper. Below is a list of the sections contained in this appendix.

Table of Contents

A Data construction

A.1 The Ginnie Mae-HMDA match

We merge the Ginnie Mae and HDMA data using FHA endorsements as an intermediate link. The FHA endorsements data contains the universe of single-family mortgages insured by the FHA and is published on the U.S. Department of Housing and Urban Development (HUD)'s website.[1]

To merge the Ginnie Mae data and FHA endorsements, we take a two step approach. In the first step, we exact match on the property state, interest rate, the balance of the mortgage rounded down to the nearest 1000, whether the mortgage is fixed rate, the mortgage purpose, and whether the mortgage's endorsement month is within 3 months of origination. In the second step, we take the unique matches from the first step and identify a seller-lender correspondence by keeping only the Ginnie Mae sellers that are among the top 10 sellers associated with the matched endorsement FHA lender (sponsor) and that have a market share of at least 5% associated with the matched endorsement FHA lender (sponsor). As the average seller market share is 57% for the top seller associated with each sponsor, this is a fairly permissive restriction. Overall, we were able to uniquely merge 62% of Ginnie Mae loans to FHA endorsements.

To merge the HMDA data and FHA endorsements, we also take a two step approach. In the first step, we match on the whether the property's zip code in the endorsement data contains a Census tract with a positive residential ratio that is associated with the HMDA data as found in HUD's March 2016 cross-walk,[2] the balance of the mortgage rounded to the nearest 1000, the mortgage purpose, and whether the mortgage's endorsement month is either in the HMDA's year of origination or within 3 months of it. In the second step, we take the unique matches from the first step and identify a lender-FHA sponsor correspondence by keeping only the HMDA lenders that have a market share of at least 20% associated with the matched endorsement FHA sponsor. As in theory the correspondence between HMDA lenders and FHA sponsors should be one-to-one and the average market share for the top lender associated with each sponsor in our first step matched sample is is 91%, this is a fairly permissive restriction. Overall, we were able to uniquely merge 81% of FHA endorsements to HMDA loans.

Linking the datasets together, we obtain a total unique match rate of 49%. Restricting to new purchase, single-family, non-manufactured housing mortgages in our sample period, the match rate is 43%. We use only the uniquely matched loans for our empirical analyses. To alleviate concerns about match quality, we

[1]https://www.hud.gov/program_offices/housing/rmra/oe/rpts/sfsnap/sfsnap.

[2]https://www.huduser.gov/portal/datasets/usps_crosswalk.html

also run our extensive margin and loan performance analysis on the Ginnie Mae sample alone, and obtain similar qualitative results. These results are tabulated in Table A.1 and Table A.2.

Table A.1. Extensive and Intensive Margin Effects: Ginnie Mae Sample

This table examines the extensive and intensive margin changes in loan origination volume around the changes in underwriting regulations, using the methodology described in Section 4.2. Δ*Loans Originated* refers to the increase in the total number of new purchase loans extended to low FICO borrowers as a fraction of the number of new purchase loans extended to low FICO borrowers in the absence of the policy. Δ*Average DTI* refers to the average increase in measured DTI of new purchase loans as a result of the policy. Δ*Low DTI Loans* refers to change in low-DTI loans as a fraction of all new purchase loans as a result of the policy change. The sample is our Ginnie Mae sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Standard errors are reported in parentheses and are computed from 1,000 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	Baseline	Alternative Specifications		
	$\overline{d}$ = 35	$\overline{d}$ = 32	$\overline{d}$ = 34	$\overline{d}$ = 36
Δ*Loans Originated*	0.081***	0.083***	0.080***	0.083***
	(0.010)	(0.013)	(0.011)	(0.009)
Δ*Average DTI*	1.657***	1.450***	1.552***	1.768***
	(0.109)	(0.093)	(0.102)	(0.114)
Δ*Low DTI Loans*	-0.084***	-0.082***	-0.084***	-0.084***
	(0.006)	(0.008)	(0.007)	(0.006)
Bootstrap Replications	1000	1000	1000	1000
Number of Observations	1,687,799	1,687,799	1,687,799	1,687,799

Table A.2. Delinquency Rates: Ginnie Mae Sample

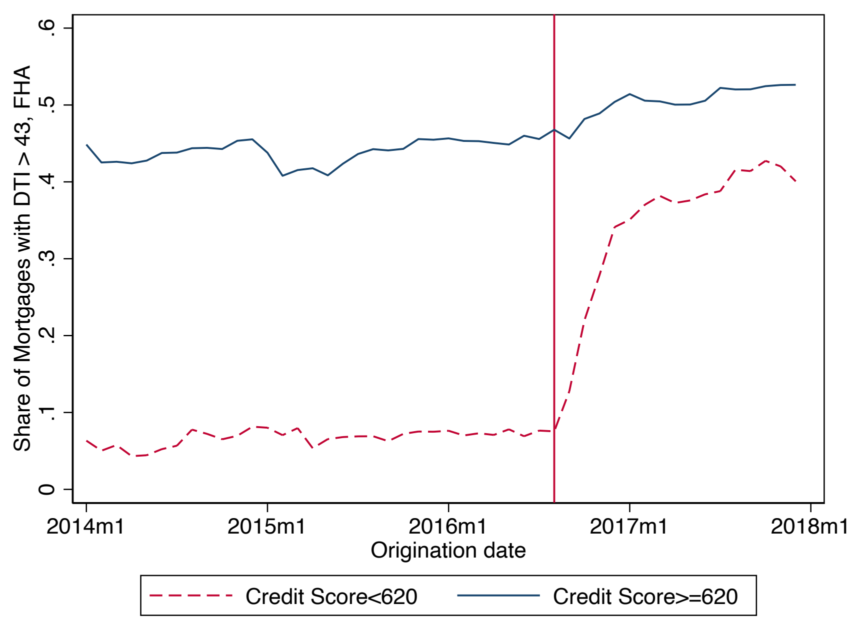
This table examines the changes in mortgage delinquency rates around the changes in underwriting regulations. The sample is our Ginnie Mae sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. The table report results from the DID analysis following Equation 7. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount. We cannot control for borrower income and county and lender fixed effects as we do in Columns (3) and (6) of Table 3, because those variables come from HMDA data. Instead, we include state fixed effects in Columns (3) and (6). Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency*	(1)	(2)	(3)	(4)	(5)	(6)
Low FICO*Post	-0.00445	-0.000993	0.00119	0.00335	0.00455	0.00543
	(0.00996)	(0.0109)	(0.0106)	(0.00386)	(0.00391)	(0.00384)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
State FE			Yes			Yes
Observations	770295	770279	770278	487894	487885	487885
R^2	0.028	0.029	0.042	0.028	0.029	0.038

B Alternative specifications of main results

B.1 Effect of the policy change in matched sample

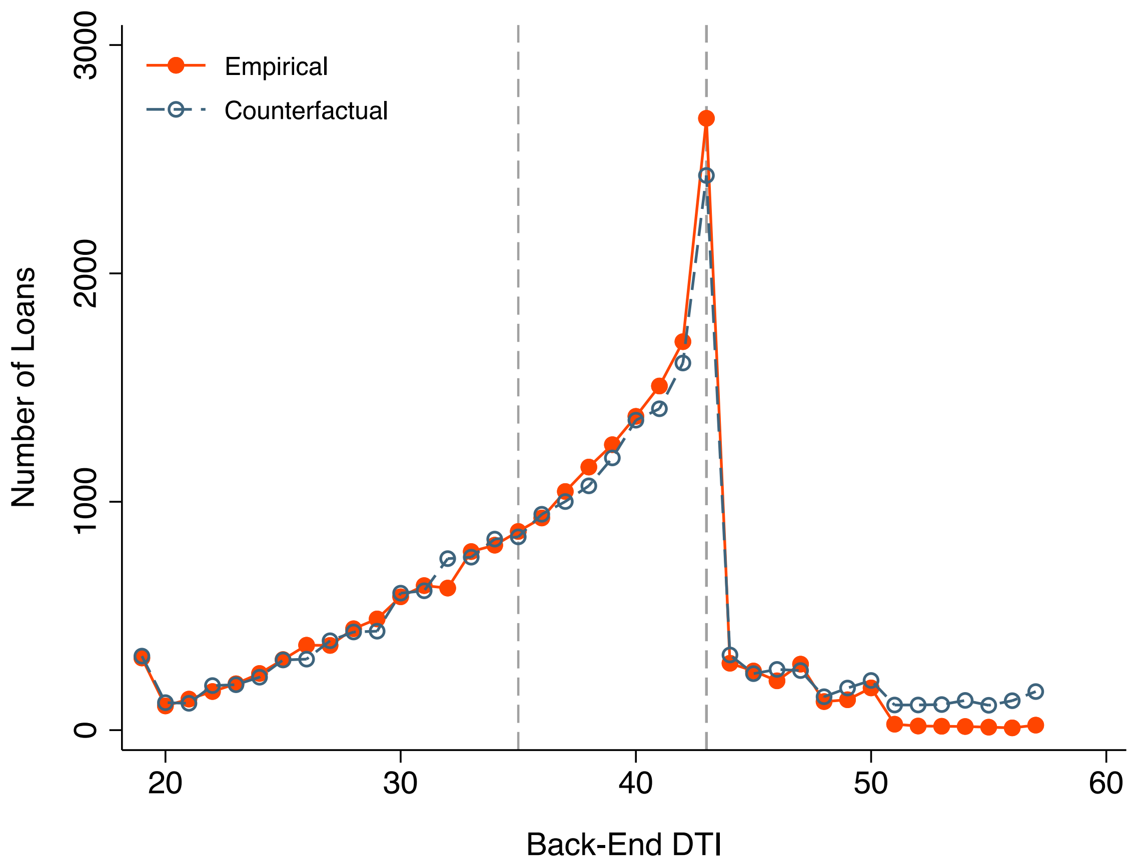
Figure B.1. Effect of the policy change, Ginnie Mae-HMDA sample



Note: This figure plots the share of FHA new purchase, single-family, non-manufactured housing mortgages with an DTI greater than or equal to 43 by their month of origination. The sample is the Ginnie Mae-HMDA sample from January 2015 to December 2017. Data for borrowers with a credit score less than 620 and a credit score greater than or equal to 620 are separately plotted. The policy month of August 2016 is marked via a vertical red line.

B.2 Placebo bunching analysis

Figure B.2. Placebo analysis, using August 2015 as the treatment date



Note: This figure plots empirical and counterfactual numbers of FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample 12 months after a placebo treatment date of August 2015 based on the methodology described in Section 4.2. DTI is winsorized at the 1st and 99th percentiles and rounded down to the nearest integer. Dashed lines are drawn at DTI equals 43, above which the policy takes into affect, and at DTI equals 35, at or below which we assume is unaffected by the policy for our baseline bunching analysis.

B.3 Quantity effects in difference-in-differences frameworks

In this section, we examine the loan growth patterns in a regression framework, which allows us to control for covariates. To test the changes in loan volume for a DTI category, we aggregate the loans from the Ginnie Mae-Endorsements-HMDA matched sample by DTI-FICO bin-month grids. FICO scores are binned by every 20 increment and DTI ratios are binned by integer percentage points.

Using this DTI-FICO bin-month panel, we perform two analyses. The first is a difference-in-difference analysis, where we compare the loan growth for borrowers with below-620 credit scores (i.e., "treated group") and above-620 credit scores (i.e., "control group"). We perform this analysis for high-DTI and low-DTI loans separately, and within each DTI group compare the loan volumes between the treated and control borrowers over time. Given that loan volume is measured in counts, we estimate a Poisson regression (Cohn et al., 2022):

$$Log(E(loans)_{d,f,t}) = \beta_1 Treated \times Post + \beta_2 Treated + \tau_t + \phi_f + \delta_d, \tag{22}$$

where d represents an integer DTI grid, f a FICO bin, and t a month. *Treated* is an indicator for low-credit-score borrowers that are affected by the policy (FICO$<$ 620). *Post* is an indicator for months after the policy change (August 2016). Our coefficient of interest is β_1, which indicates the increase in low-credit-score loans relative to high-credit-score ones. The error term is omitted since the left hand side is the log of the expected loan volume. We add fixed effects in stages, starting with a specification with no fixed effects, then adding month fixed effects (τ_t), FICO bin fixed effects (ϕ_f) and DTI fixed effects (δ_d). The error term is omitted since the left hand side is the log of the expected loan volume rather than the log of the actual loan volume as in a log regression. In the most rigorous specification, we further include DTI-month interactive fixed effects.

Panel A of Table B.1 reports the results. Columns (1) and (2) present results for the high-DTI sample; while Columns (3) and (4) present results for the low-DTI sample. For each sample of loans, we start with a regression with no fixed effects, and then impose origination time (indicated by year-month) fixed effects. *Treated* $\times$ *Post* carries positive, significant coefficients for high-DTI loans, but not for low-DTI loans. The interactive coefficient β_1 is 1.22 in Column (2), suggesting an increase in loan volume by 1.22 log points (239%) for high-DTI, low-credit-score borrowers. This stands in contrast to the small negative coefficient shown in Columns (3) and (4), which suggests small changes in the low-DTI loan volume to low-credit-score borrowers.[3]

[3]Note that these magnitudes differ from those generated from bunching. This is mostly because the two methods use different bases for comparison. The OLS regressions use the pre-policy counts of high-DTI, low-credit-score loans, while the bunching regressions use the counterfactual counts of all low-credit-score loans with a DTI above $\bar{d}$.

Table B.1 About Here

Our second regression analysis is a triple-different Poisson regression, comparing the differential loan growth between high-DTI and low-DTI loans:

$$\begin{aligned} Log(E(loans)_{d,f,t}) = {} & \gamma_1 Treated \times High\,DTI \times Post + \gamma_2 Treated \times High\,DTI \\ & + \gamma_3 Treated \times Post + \gamma_4 High\,DTI \times Post + \tau_t + \phi_f + \delta_d, \end{aligned} \tag{23}$$

where *High DTI* is a dummy variable that equals one if the DTI ratio is above 43, and zero otherwise. Results are reported in Panel B of Table B.1. The triple interaction term *Treated* × *High DTI* × *Post* generates a positive and statistically significant coefficient, suggesting that high-DTI loan volume increases more for low-credit-score borrowers than for high-credit-score ones following the FHA policy change. These results are consistent with the patterns shown in Figure 1 and Figure 2.

In Figure B.3, we test the parallel trend assumption related to our policy shock. In particular, we seek to verify whether the increases in lending volume to highly levered, low-credit-score borrowers started prior to August 2016. We repeat the estimation of Equation 23, but replacing *Post* with an array of indicators for each month before and after the policy reform. The month prior to the policy date is absorbed as the base period. Our results suggest that there is no relative change in the volumes of low-credit-score, high-DTI loans prior to the implementation of the policy, while such volumes increase drastically immediately afterwards. This result helps address concerns that our quantity effects might be driven by pre-existing trends.

Figure B.3 About Here

Table B.1. Origination Volume: Descriptive Evidence

This table examines the changes in mortgage origination volume around the changes in underwriting regulations using a Poisson regression. The sample is derived from the Ginnie Mae-HMDA matched sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. We aggregate the sample into each DTI-FICO bin-month grid. The dependent variable is the number of loans originated in a grid. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. FICO scores are grouped into bins with widths 20. Panel A reports results from difference-in-difference regressions. Panel B reports results from a triple-difference framework. In both panels, *Low FICO* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *High DTI* indicates the sample of loans where borrower DTI exceeds 43, and *Low DTI* represents the sample with DTI at or below 43. *Post* indicates whether the loan is extended after the regulation change in August 2016. Variable definitions are provided in Table 1. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

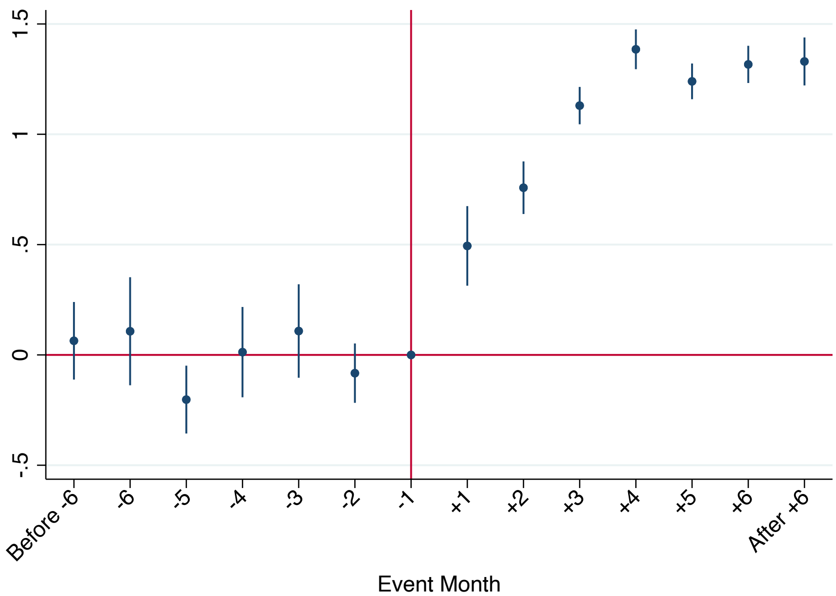
Panel A. Difference-in-difference Results

Sample	High DTI (> 43)		Low DTI (≤ 43)	
Dep. Var.: *#Loans*	(1)	(2)	(3)	(4)
Treated × *Post*	1.226***	1.222***	-0.0435	-0.0361
	(0.0872)	(0.0883)	(0.0579)	(0.0542)
Treated	-2.761***	-2.797***	-1.030***	-1.055***
	(0.112)	(0.115)	(0.0591)	(0.0581)
Post	0.107		-0.0947	
	(0.108)		(0.0938)	
Month FE		Yes		Yes
Observations	4216	4216	8105	8105
Pseudo-R^2	0.2418	0.3260	0.1173	0.1781

Panel B. Triple-Difference Results

Dep. Var.: *#Loans*	(1)	(2)
Treated × *High DTI* × *Post*	1.269***	1.264***
	(0.0899)	(0.0949)
Treated	-1.030***	-1.055***
	(0.0595)	(0.0582)
High DTI	0.381***	0.381***
	(0.113)	(0.117)
Treated × *High DTI*	-1.731***	-1.741***
	(0.119)	(0.123)
Treated × *Post*	-0.0435	-0.0376
	(0.0581)	(0.0540)
High DTI × *Post*	0.201***	0.202***
	(0.0274)	(0.0123)
Post	-0.0947	
	(0.0947)	
Month FE		Yes
Observations	12321	12321
Pseudo-R^2	0.2091	0.2750

Figure B.3. Dynamic effect of the policy change on low FICO, high DTI loan origination volume



Note: We estimate dynamic triple difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. We utilize Ginnie Mae-HMDA matched loans from August 2015 to August 2017 (excluding August 2016) and aggregate the sample into each DTI-FICO bin-month grid. We utilize a Poisson regression where the outcome variable is the number of loans originated in a grid. We estimate Equation 23. The fixed effects and control variables used are the same as those used in Table B.1 Panel B Column (2). We use the month prior to August 2016 as the base period for estimation (Event Month = -1).

B.4 Delinquency and Interest Rate Spreads: Full Sample

Table B.2. Delinquency and Interest Rates Results in Full Sample

This table examines the changes in mortgage delinquency rates and interest rates around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Panel A reports delinquency results, and Panel B reports interest rate results. The regression specification follows the DID analysis in Equation 22. Delinquency rates are measured as 90-day, 2-year delinquency rates. Interest rate spreads are measured relative to the Freddie Mac Survey rate. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Controls include log of loan amount and log of borrower household income. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. Delinquency Rates, Difference-in-difference

Sample	High DTI (> 43)			Low DTI (≤ 43)		
Dep. Var.: *Delinquency Rate*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00651	-0.00648	-0.00453	0.00436	0.00396	0.00446
	(0.0116)	(0.0120)	(0.0124)	(0.00387)	(0.00382)	(0.00370)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	323522	323522	323325	379609	379609	379490
R^2	0.030	0.031	0.054	0.033	0.034	0.052

Panel B. Interest Rate Spreads, Difference-in-Difference

Sample	High DTI (> 43)			Low DTI (≤ 43)		
Dep. Var.: *Interest Rate Spreads*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00223	0.0147	0.0121	0.0394***	0.0336**	0.0225
	(0.0212)	(0.0230)	(0.0216)	(0.0105)	(0.0109)	(0.0120)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
FICO FE	Yes			Yes		
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	325187	324425	324153	204076	203415	203092
R^2	0.230	0.245	0.461	0.255	0.272	0.502

B.5 Delinquency Results: Alternative Measures

Table B.3. Delinquency Rates: Longer Time Horizon

This table examines the changes in mortgage delinquency rates around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Panel A reports results from the DID analysis following Equation 22 for 3 year delinquencies, Panel B reports results from the DID analysis following Equation 22 for 4 year delinquencies. Panel C reports results from the DID analysis following Equation 22 for 5 year delinquencies. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount and log of borrower household income. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. 3 Year Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (3 year)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.0137	-0.0114	-0.00505	0.00581	0.00601	0.00808
	(0.0115)	(0.0114)	(0.0121)	(0.00684)	(0.00708)	(0.00755)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324266	323522	323251	203353	202706	202379
R^2	0.048	0.052	0.080	0.043	0.047	0.079

Panel B. 4 Year Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (4 year)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00679	-0.00587	0.00227	0.00426	0.00401	0.00678
	(0.0114)	(0.0115)	(0.0130)	(0.00662)	(0.00658)	(0.00749)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324266	323522	323251	203353	202706	202379
R^2	0.053	0.057	0.087	0.052	0.056	0.090

Panel C. 5 Year Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (4 year)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00747	-0.00625	0.00170	-0.00375	-0.00414	-0.00163
	(0.0107)	(0.0111)	(0.0111)	(0.00524)	(0.00526)	(0.00530)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324256	323512	323245	203345	202698	202375
R^2	0.043	0.048	0.082	0.047	0.052	0.089

Table B.4. Delinquency Rates: 30 and 60 Day Measures
This table examines the changes in mortgage delinquency rates around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Panel A reports results from the DID analysis following Equation 22 for 2 year, 30-day delinquencies, Panel B reports the DID analysis following Equation 22 for 2 year, 60-day delinquencies. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount and log of borrower household income. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Panel A. 30-Day Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (30 day)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.00174	0.00587	0.0114	0.00402	0.00379	0.00897
	(0.0135)	(0.0138)	(0.0136)	(0.00621)	(0.00621)	(0.00675)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324266	323522	323251	203353	202706	202379
R^2	0.062	0.066	0.096	0.071	0.076	0.111

Panel B. 60-Day Delinquency, Difference-in-difference Results

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (60 day)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.0128	-0.00982	-0.00527	0.00867	0.00858	0.0109
	(0.00968)	(0.00962)	(0.0102)	(0.00499)	(0.00483)	(0.00582)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes		Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	324266	323522	323251	203353	202706	202379
R^2	0.040	0.044	0.075	0.043	0.047	0.082

Table B.5. Delinquency Rates: excluding CRA tracts

This table examines the changes in mortgage delinquency rates around the changes in underwriting regulations. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. Low-Income Census tracts that qualify for the Community Reinvestment Act (CRA) assessment in the year of mortgage origination are excluded from this analysis. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. *Treated* is an indicator that equals one if the borrower's credit score is below 620, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in August 2016. *High DTI* (*Low DTI*) represents a subsample of borrowers with DTI above 43 (less than or equal to 43). Borrowers with DTI below 35 are unaffected by the policy and are excluded from the sample. Controls include log of loan amount and log of borrower household income. Standard errors are reported in parentheses and are double clustered by DTI (integer level) and origination month. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Delinquency Rates, Difference-in-difference

Sample	High DTI (> 43)			Low DTI ($35 \leq DTI \leq 43$)		
Dep. Var.: *Delinquency (90 day)*	(1)	(2)	(3)	(4)	(5)	(6)
Treated × *Post*	-0.0110	-0.00863	-0.00623	-0.00514	-0.00511	-0.00355
	(0.00734)	(0.00764)	(0.00768)	(0.00358)	(0.00360)	(0.00365)
Controls		Yes	Yes		Yes	Yes
Month FE	Yes			Yes		
FICO FE	Yes			Yes		
FICO-DTI FE		Yes	Yes			Yes
Month-DTI FE		Yes	Yes			Yes
County FE			Yes			Yes
Lender FE			Yes			Yes
Observations	263393	262749	262449	165756	165210	164858
R^2	0.028	0.032	0.064	0.029	0.033	0.070

B.6 Heterogeneity of Credit Expansion by Income and Race

Table B.6. Heterogeneity of Credit Expansion by Income and Race
This table examines the changes in loan origination volume around the FHA policy change in August 2016 while partitioning the sample based on borrower income within their respective racial/ethnic categories. The estimation uses the methodology described in Section 4.2. Changes in loan origination is measured as the increase in the total number of new purchase originations for low FICO borrowers as a fraction of the number of new purchase originations in the absence of the policy. The sample is our Ginnie Mae-HMDA sample of FHA single-family, non-manufactured housing, home purchase mortgages issued during the period of August 2015 through August 2017, excluding August 2016, the month of the policy implementation. DTI is winsorized at the 1st and 99th percentiles and rounded up to the nearest integer. Standard errors are reported in parentheses and are from 1,000 bootstrap replications. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	(1)	(2)	(3)	(4)
Race:	Black	Black	White	White
Income:	High	Low	High	Low
Δ*Loans Originated*	0.037	-0.060	0.134***	0.045
	(0.052)	(0.056)	(0.022)	(0.029)
Number of Observations	36,178	46,942	223,904	204,182

B.7 Mortgage Access and the Quality of Neighborhoods: Robustness

Table B.7. Mortgage Access and the Quality of Neighborhoods: Placebo Test Using Renters

The unit of observation is an individual-year. The sample is limited to individual-year observations in Experian who have a dwelling status equal to "A", which denotes living in an apartment, condo, or another multi-family unit, who are likely to be renters (Butler et al., 2019). The outcome variable is *d(School Rating)*, the difference between the rating of the school district where the individual currently lives and the rating of the school district where she lived in the previous yea. *Treated (2015)* is an indicator that equals one if the borrower's credit score is below 620 in 2015, and zero otherwise. *Post* indicates whether the loan is extended after the regulation change in 2016. Individual characteristics include indicators for gender, marital status, and Treat (2015). Age group fixed effects are dummy variables for each of five-year age categories (i.e., 20–24, 25–29, etc.). Standard errors are reported in parentheses and are clustered by county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

Dep. Var.: *d(School Rating)*			
	(1)	(2)	(3)
Post × *Treat (2015)*	0.0004	0.0004	0.0015
	(0.0023)	(0.0026)	(0.0026)
Individual Char	Yes	Yes	Yes
Year FE	Yes		Yes
FICO FE	Yes	Yes	Yes
Zipcode FE	Yes		
Zipcode-Year FE		Yes	
Gender-Zipcode FE			Yes
Age Group-Zipcode FE			Yes
Married-Zipcode FE			Yes
Observations	1,292,148	1,277,712	1,258,218
R^2	0.08	0.12	0.19

C Quantifying Policy Effects on the FHA Market

In this section, we seek to understand the implications of the FHA policy for the agency as well as for high-risk borrowers. We will also discuss the overall welfare implications for the average FHA borrower.

To evaluate welfare implications, we need to quantify the effect of the policy on the dollar volume of FHA credit and loan delinquencies. We first estimate changes in dollar volume. To do so, we partition borrowers into ten groups by income, and assume that within each income group, FHA loan amount is proportional to the average DTI. Under this assumption, we write the post-policy credit amount for the treated and counterfactual group as V_l, $\hat{V}_l$, repectively. They are defined as:

$$V_l = \sum_q \bar{m}^{post}_{ldq} n^{post}_{ld}, \tag{24}$$

$$\hat{V}_l = \sum_q \bar{m}^{post}_{ldq} \hat{n}^{post}_{ld}, \tag{25}$$

where $\bar{m}^{post}_{ldq}$ is the average mortgage balance for low credit score borrowers within the DTI bin d in income quantile q, n^{post}_{ld} is the number of loans for low credit score borrowers within each DTI bin d post policy, and $\hat{n}^{post}_{ld}$ is the counterfactual number of loans for low credit score borrowers within each DTI bin d without the policy. Results are shown in Panel A of Table C.1.

We next compute the effect of the policy on delinquency rates. Since our policy change did not significantly affect delinquency rates conditional on DTI, the changes in delinquency rates depend on the shift in the DTI distribution. We denote the average delinquency rate post-policy with d_l and the average delinquency rates in the counterfactual case without the policy as $\hat{d}_l$. d_l and $\hat{d}_l$ can be computed as:

$$d_l = \frac{1}{N^{post}_l} d^{post}_{ld} n^{post}_{ld}, \tag{26}$$

$$\hat{d}_l = \frac{1}{\hat{N}^{post}_l} d^{post}_{ld} \hat{n}^{post}_{ld}, \tag{27}$$

where d^{post}_{ld} is the average delinquency rate of low credit score borrowers within each DTI bin, n^{post}_{ld} is the number of loans in each DTI bin, N^{post}_l is the total number of loans among low credit score borrowers, $\hat{n}^{post}_{ld}$ is the counterfactual number of loans for low credit score borrowers in each DTI bin without the policy, and $\hat{N}^{post}_l$ is the total counterfactual number of loans for low credit score borrowers. We also calculate the differences between d_l and $\hat{d}_l$ in levels and percentages, and display them in Panel B of Table C.1.

Estimates in Table C.1 suggest that the policy increased FHA loan volume by 1.10% while also increasing FHA delinquency rates by 1.61%. In the long run, the FHA's break-even constraints would require them to increase the mortgage insurance premium (MIP) by at least 1.61%. The MIP during our sample period is 175 bps upfront and 85 bps per year. For an average loan of 7-year duration, this implies an MIP of 110 bps per year (i.e., 175/7 + 85). A 1.61% increase of MIP from 110 bps is an increase of 1.82 bps. This also implies an annual per-dollar subsidy of 1.82/1.10% = 165 bps for the loans involved in the credit expansion.

Finally, we conduct a back-of-the-envelope calculation of the policy's welfare effects. As mentioned above, the policy likely increased the cost of an average FHA mortgage, but also allowed a group of high-risk borrowers to enter the market, thus improving financial inclusion. To analyze welfare implications, we need to compare the costs borne by the average FHA borrower and benefits for the high-risk borrowers. In this comparison, we adopt the approach of Jansen et al. (2022), and apply equal welfare weights across borrowers following their approach.

For the high-risk new entrants, we compute the gain in their welfare using the framework introduced in Jansen et al. (2022). Let ρ be the shadow rate that prevents them from entering the market, and let r be the interest rates they receive after the policy change. At rate r, these borrowers enter the market and receive loans of quantity q. Thus, their welfare gain can be represented by $\Delta W = 0.5\frac{\rho - r}{q}$ (i.e., the welfare triangle). Let e denote their demand elasticity, we have $e = \frac{d(q)}{q \times d(r)}$. Given that the change from ρ to r leads to market entry, the percentage of quantity change is 100%. Thus $e = \frac{1}{\rho - r}$, and $\Delta W = 0.5q/e$.

For the average FHA borrowers, the policy change led to an incremental cost of 1.82 bps, which is relatively small. It is reasonable to assume that such a minor change in interest rate does not significantly depress mortgage demand. Hence, the monetary cost for those borrowers should be $0.0182 * Q$ (in percentage point terms), where Q is the total dollar volume of FHA mortgages.

For the policy to be welfare improving, we need $0.5q/e \geq 0.0182Q$. The estimates of Panel A, Table C.1 indicate that the new entrants' mortgage volume represents 1.1% of total volume of the FHA market, i.e., $q/Q = 1.1\%$. This means that the FHA policy is welfare improving if $e \leq 0.5\frac{q}{Q}/0.0182 = 0.274$. DeFusco and Paciorek (2017) estimate the demand elasticity of FHA borrowers to be 0.023–0.03, which satisfies the above condition by an order of magnitude. Therefore, the policy appears to be welfare improving under Jansen et al. (2022)'s framework.

However, in reality, high-risk FHA borrowers may be subject to behavioral frictions and underestimate their default probabilities and the costs of foreclosures and short-sales. Under such frictions, the policy-induced expansion could lead to greater costs borne by the average borrower and become welfare-reducing.

Table C.1. Effect of Policy on the FHA Market
This table examines the changes in delinquency and dollar volume due to the policy. The sample consists of FHA single-family, non-manufactured housing, home purchase mortgages issued over the treatment period is the period of September 2016 through August 2017. Delinquency rates are measured as 90-day, 2-year delinquency rates. Panel A shows the results for dollar volume, and panel B for delinquency rates. Dollar volumes are approximated based on Equation (24), and are reported in millions USD. Standard errors are reported in parentheses and are from 1,000 bootstraps. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively and are labelled to the differences.

Panel A. Dollar Volume, FHA Market

Dep. Var: *Dollar Volume ($ millions)*	(1) With Policy	(2) No Policy	(3) Difference (1)-(2)	(4) % Difference ((1)-(2))/(2)*100
Treated (FICO < 620)	5,990***	5,189***	802***	15.5***
	(37)	(69)	(66)	(1.49)
Full Sample	73,411***	72,609***	802***	1.10***
	(103)	(121)	(66)	(0.09)

Panel B. Delinquency Rates, FHA Market

Dep. Var: *Delinquency Rate* (90-day)	(1) With Policy	(2) No Policy	(3) Difference (1)-(2)	(4) % Difference ((1)-(2))/(2)*100
Treated (FICO < 620)	12.92***	12.45***	0.47***	3.75***
	(0.60)	(0.48)	(0.19)	(1.47)
Full Sample	5.85***	5.76***	0.09***	1.61***
	(0.06)	(0.05)	(0.02)	(0.34)

D Model details

D.1 An equivalence result

In this section we construct a mapping from $\{m_i^h, m_i^s\}$ to θ_i in both the pre and post periods. In the pre-period, such a θ_i can be defined as:

$$\theta_i = \begin{cases} -1 \text{ if } e(m_i^h) = 0 \\ 0 \text{ if } e(m_i^h) = 1 \text{ and } \left\{\text{screening}(m_i^h) = 0 \text{ or } E_{l,0}(NPV|m_i^h, m_i^s) < 0\right\} \\ 1 \text{ if } e(m_i^h) = 1 \text{ and } \left\{\text{screening}(m_i^h) = 1 \text{ and } E_{l,0}(NPV|m_i^h, m_i^s) \geq 0\right\} \end{cases} \tag{28}$$

where $\text{screening}(m_i^h) = 0$ is given by $\int_0^\infty E_{l,0}(NPV|m_i^h, m_i^s)dG(m_i^s|m_i^h) - c_i(m_i^h) < 0$, and $\text{screening}(m_i^h) = 1$ is given by $\int_0^\infty E_{l,0}(NPV|m_i^h, m_i^s)dG(m_i^s|m_i^h) - c_i(m_i^h) \geq 0$. The required thresholds would then be $\bar{s}_0 = -1$, $\bar{s}_0 + \bar{s}_{1,0} = 0$. Note that while this θ_i distribution is discrete, it can be trivially turned into a continuous distribution with full support by replacing $\theta_i = -1$ with θ_i being drawn from a continuous distribution with full support with a maximum of -1, and analogously for the $\theta_i = 0$ and $\theta_i = 1$ cases. This continuous distribution can then be assumed to be a standard Normal distribution, without loss of generality as long as the thresholds $\bar{s}_0, \bar{s}_0 + \bar{s}_{1,0}$ is allowed to adjust.

In the post-period, such a θ_i can be defined as:

$$\theta_i' = \begin{cases} -1 \text{ if } e(m_i^h) = 0 \\ 0 \text{ if } e(m_i^h) = 1 \text{ and } \{E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 1) \leq 0 \text{ and } \left\{\text{screening}'(m_i^h) = 0 \text{ or } E_{l,1}(NPV|m_i^h, m_i^s) < 0\right\}\} \\ 1 \text{ if } e(m_i^h) = 1 \text{ and } \{E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 1) > 0 \text{ or } \left\{\text{screening}'(m_i^h) = 1 \text{ and } E_{l,1}(NPV|m_i^h, m_i^s) \geq 0\right\}\} \end{cases} \tag{29}$$

where $\text{screening}'(m_i^h) = 0$ is given by $\int_0^\infty E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 0, m_i^s)dG(m_i^s|m_i^h) - c_i'(m_i^h) < 0$, and $\text{screening}'(m_i^h) = 1$ is given by $\int_0^\infty E_{l,1}(NPV|m_i^h, AUS(m_i^h) = 0, m_i^s)dG(m_i^s|m_i^h) - c_i'(m_i^h) \geq 0$. The required thresholds would again be $\bar{s}_0 = -1$, $\bar{s}_0 + \bar{s}_{1,1} = 0$. By keeping $\bar{s}_0$ constant across the pre-and-post periods, the structural assumption in our example is that borrower i's eligibility for a FHA DTI<43 mortgage did not change following the policy, which is motivated by the fact that the policy did not affect FHA mortgages in this DTI range.

D.2 Take-up rate and eligibility rate

The take-up rate, which we calibrate ξ_0 to, is calibrated to the share of borrowers with credit score below 620 that holds a mortgage in our Experian data. For the full sample during our sample period, this number is 9.88%. In subsamples, we scale this number by the proportional differences in take-up among the group by multiplying it by the proportion of low credit score mortgage originations (borrowers with credit score under 620 in our CoreLogic-HMDA merge) in each subsample and then dividing by the proportion of low credit score households (households with credit score under 600 in Survey of Consumer Payment Choice data, the closest category to 620) of a subsample in the population. The scale factor is listed in the Table D.1 below.

For the eligibility rate of borrowers for getting a FHA which we calibrate s_0 to, low DTI (DTI<43) mortgage, we use the proportion of households with at least $20,000 in non-housing assets or that are already homeowners in the SCPC data for those with a credit score under 600, which is their closest category to 620. This fraction is 25.42% in the full sample. This suggests that about 38.9% of borrowers who are eligible for a mortgage obtained one.[4] For sub-samples, we apply the same scale factor to the take-up rate as in Table D.1, implicitly assuming that the proportional differences in take-up are explained by the proportional differences in eligibility. These eligibility calibrations are shown in the last row of Table D.1. We test the sensitivity of our model to alternative calibrations of s_0 in Appendix Section D.4.

Table D.1. Scale factor for take-up rate This table presents the scale factor we apply to the take-up rate for each race/ethnicity and income subsample. The proportion of low credit originations is computed using our CoreLogic-HMDA merge during our sample period for borrowers with a credit score under 620. The proportion of low credit score households is computed using 2016 Survey of Consumer Payment Choice (SCPC) data for households with a credit score under 600, which is the closest category to 620. The ratio of the two represents the extent to which each sub-population takes up more mortgages than the average, and is the scale factor we apply to take-up rate in each subpopulation.

	Race/Ethnicity Subsample			Income	
	Non-Hispanic White	Black	Hispanic	Below Med	Above Med
Proportion of low credit originations	59.48%	14.71%	15.52%	75.35%	23.99%
Proportion of low credit score households	48.28%	27.68%	15.32%	79.27%	20.72%
Scale factor	1.23	0.53	1.01	0.95	1.16
Implied s_0 (full sample $s_0 = \Phi^{-1}(0.2542) = -0.661$)	-0.487	-1.103	-0.651	-0.701	-0.541

[4] The ratio of 9.88% and 25.42%.

D.3 Additional model fit results

Table D.2. Model fit for the non-Hispanic white demographic subsample

This table displays our structural model fit for the non-Hispanic white demographic subsample. $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 5, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.094	0.097	0.003
$45 < DTI_1 \leq 50$	0.144	0.149	0.005
$43 < DTI_1 \leq 45$	0.074	0.065	-0.009
$35 < DTI_1 \leq 43$	0.373	0.374	0.001
$30 < DTI_1 \leq 35$	0.156	0.157	0.001
$25 < DTI_1 \leq 30$	0.096	0.095	-0.001
$20 < DTI_1 \leq 25$	0.044	0.041	-0.003
$\overline{DTI}_1$	0.394	0.390	-0.004
$DTI_0 > 50$	0.068	0.068	0.000
$45 < DTI_0 \leq 50$	0.071	0.071	0.001
$43 < DTI_0 \leq 45$	0.033	0.032	-0.001
$35 < DTI_0 \leq 43$	0.481	0.482	0.001
$30 < DTI_0 \leq 35$	0.173	0.173	0.001
$25 < DTI_0 \leq 30$	0.103	0.104	0.001
$20 < DTI_0 \leq 25$	0.049	0.046	-0.003
$\overline{DTI}_0$	0.381	0.376	-0.004
Policy elasticity	0.108	-2.348	-2.456
Interest rate semi-elasticity	-0.025	-0.027	-0.002

Table D.3. Model fit for the Black demographic subsample

This table displays our structural model fit for the non-Hispanic white demographic subsample. $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 5, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.136	0.140	0.004
$45 < DTI_1 \leq 50$	0.195	0.199	0.004
$43 < DTI_1 \leq 45$	0.092	0.080	-0.012
$35 < DTI_1 \leq 43$	0.363	0.363	0.000
$30 < DTI_1 \leq 35$	0.118	0.124	0.006
$25 < DTI_1 \leq 30$	0.063	0.061	-0.002
$20 < DTI_1 \leq 25$	0.026	0.023	-0.002
$\overline{DTI}_1$	0.418	0.414	-0.004
$DTI_0 > 50$	0.099	0.098	-0.001
$45 < DTI_0 \leq 50$	0.116	0.114	-0.003
$43 < DTI_0 \leq 45$	0.042	0.048	0.006
$35 < DTI_0 \leq 43$	0.522	0.518	-0.004
$30 < DTI_0 \leq 35$	0.123	0.126	0.003
$25 < DTI_0 \leq 30$	0.067	0.063	-0.005
$20 < DTI_0 \leq 25$	0.022	0.024	0.001
$\overline{DTI}_0$	0.405	0.404	-0.001
Policy elasticity	0.014	-0.219	-0.233
Interest rate semi-elasticity	-0.025	-0.026	-0.001

Table D.4. Model fit for the Hispanic demographic subsample

This table displays our structural model fit for the non-Hispanic white demographic subsample. $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 5, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.145	0.147	0.003
$45 < DTI_1 \leq 50$	0.189	0.191	0.003
$43 < DTI_1 \leq 45$	0.084	0.075	-0.009
$35 < DTI_1 \leq 43$	0.369	0.368	-0.001
$30 < DTI_1 \leq 35$	0.124	0.126	0.003
$25 < DTI_1 \leq 30$	0.059	0.062	0.003
$20 < DTI_1 \leq 25$	0.024	0.022	-0.002
$\overline{DTI}_1$	0.419	0.414	-0.005
$DTI_0 > 50$	0.106	0.106	-0.001
$45 < DTI_0 \leq 50$	0.096	0.099	0.003
$43 < DTI_0 \leq 45$	0.042	0.043	0.001
$35 < DTI_0 \leq 43$	0.514	0.513	-0.001
$30 < DTI_0 \leq 35$	0.143	0.139	-0.004
$25 < DTI_0 \leq 30$	0.065	0.069	0.004
$20 < DTI_0 \leq 25$	0.028	0.024	-0.004
$\overline{DTI}_0$	0.403	0.400	-0.003
Policy elasticity	0.109	-1.496	-1.605
Interest rate semi-elasticity	-0.025	-0.026	-0.001

Table D.5. Model fit for the income below median subsample

This table displays our structural model fit for the non-Hispanic white demographic subsample. $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 5, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.109	0.115	0.006
$45 < DTI_1 \leq 50$	0.176	0.181	0.005
$43 < DTI_1 \leq 45$	0.085	0.073	-0.012
$35 < DTI_1 \leq 43$	0.384	0.383	-0.001
$30 < DTI_1 \leq 35$	0.134	0.135	0.001
$25 < DTI_1 \leq 30$	0.071	0.073	0.002
$20 < DTI_1 \leq 25$	0.030	0.029	-0.001
$\overline{DTI}_1$	0.408	0.405	-0.003
$DTI_0 > 50$	0.111	0.110	-0.002
$45 < DTI_0 \leq 50$	0.104	0.105	0.001
$43 < DTI_0 \leq 45$	0.046	0.047	0.001
$35 < DTI_0 \leq 43$	0.484	0.480	-0.004
$30 < DTI_0 \leq 35$	0.138	0.140	0.002
$25 < DTI_0 \leq 30$	0.072	0.074	0.002
$20 < DTI_0 \leq 25$	0.032	0.030	-0.002
$\overline{DTI}_0$	0.402	0.398	-0.003
Policy elasticity	0.038	-0.911	-0.949
Interest rate semi-elasticity	-0.025	-0.024	0.001

Table D.6. Model fit for the income above median subsample

This table displays our structural model fit for the non-Hispanic white demographic subsample. $\overline{DTI}_1, \overline{DTI}_0$ represents the mean DTI with and without the policy, respectively. The number within each DTI bin represents the fraction of loans that fall within the DTI bin, with subscript 1 indicating the DTI distribution with the policy and subscript 0 indicating the counterfactual DTI distribution without the policy. The policy elasticity is pulled from Table 5, and the interest rate semi-elasticity is from DeFusco and Paciorek (2017).

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.117	0.119	0.002
$45 < DTI_1 \leq 50$	0.147	0.153	0.006
$43 < DTI_1 \leq 45$	0.074	0.061	-0.013
$35 < DTI_1 \leq 43$	0.360	0.366	0.006
$30 < DTI_1 \leq 35$	0.150	0.154	0.003
$25 < DTI_1 \leq 30$	0.093	0.089	-0.004
$20 < DTI_1 \leq 25$	0.042	0.041	-0.001
$\overline{DTI}_1$	0.398	0.396	-0.003
$DTI_0 > 50$	0.066	0.065	-0.001
$45 < DTI_0 \leq 50$	0.067	0.066	-0.001
$43 < DTI_0 \leq 45$	0.030	0.029	-0.001
$35 < DTI_0 \leq 43$	0.500	0.500	0.000
$30 < DTI_0 \leq 35$	0.171	0.173	0.003
$25 < DTI_0 \leq 30$	0.100	0.100	0.000
$20 < DTI_0 \leq 25$	0.046	0.047	0.001
$\overline{DTI}_0$	0.382	0.378	-0.004
Policy elasticity	0.136	-2.591	-2.727
Interest rate semi-elasticity	-0.025	-0.024	0.001

D.4 Model robustness

Table D.7. Model results, robustness check

This table displays our structural model results for alternative calibrations of s_0 for the full sample of borrowers. The calibrations of s_0 as an inverse Normal function Φ^{-1} of the different proportion of borrowers that are eligible for a low DTI mortgage are shown in the column headers. The percent change in consumer surplus is defined as the post-policy consumer surplus as a percent of the the counterfactual consumer surplus without the policy minus one hundred. The percent change in DTI>43 approvals is defined as the post-policy model implied approval rate for DTI>43 mortgages as a percent of the the counterfactual model implied approval rate without the policy minus one hundred. The 95% confidence interval computed via 1,000 parameter draws from their estimated values and covariance matrix is shown in square brackets. *, **, and *** indicate statistical significance at the 10%, 5%, and 1%, respectively.

	$s_0 = \Phi^{-1}(0.15)$	$s_0 = \Phi^{-1}(0.25)$	$s_0 = \Phi^{-1}(0.35)$
Consumer surplus change (%)	12.159***	11.389***	11.207***
95% Confidence Interval	[10.707, 13.247]	[9.797, 12.801]	[9.903, 12.279]
Percent change in DTI>43 approvals	95.582***	100.842***	98.799***
95% Confidence Interval	[92.112, 99.175]	[96.210, 105.931]	[95.585, 102.061]