Algorithmic Underwriting in High Risk Mortgage Markets*

Janet Gao
Georgetown University

Hanyi Livia Yi
Boston College

David Zhang
Rice University

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Abstract

We study the effects of a policy that increased the reliance on algorithmic underwriting for low-credit-score, high-leverage mortgage borrowers. Estimating the bunching of loans around the policy's debt-to-income threshold, we find a large credit expansion to affected borrowers with little changes in default risks. Such effects are more pronounced among non-Hispanic White borrowers and higher-income borrowers. These changes lead to real effects: low-credit-score individuals are more likely to move to better-rated school districts after the policy implementation. We use a structural approach to quantify the welfare implications of the policy change and isolate the credit supply channel. Overall, our results suggest that algorithmic underwriting can help increase financial inclusion while effectively controlling risk, conditional on observables. However, they can also generate disparate impact across racial groups and along the income distribution.

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

JEL classification: G18, G21, G51, O33

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1 Introduction

Policies targeting household lending often need to balance the benefits of financial inclusion of high-risk borrowers against the potential costs associated with increased default (Layton, 2023). Better access to mortgage markets for these borrowers could reduce gaps in homeownership rates (Eggers, 2001). At the same time, providing loans to such borrowers could amplify the risk exposures of financial institutions and government agencies. A key process that influences such a trade-off is loan underwriting, where lenders filter through applications and decide which loans to originate. In this process, lenders collect documents from applicants, verify their background and financial details, and assess the credit risk associated with the loans. 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 the role of algorithmic underwriting in affecting mortgage market outcomes, especially for 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 examine the policy's impact on loan quantities, performance, prices, and household mobility. We find that the increased reliance on algorithmic underwriting leads to a substantial expansion of credit for low-credit-score borrowers. Surprisingly, delinquency rates did not increase among affected borrowers, even in areas with worsening economic conditions. The credit expansion is 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 promote financial inclusion in markets otherwise excluded by lenders, while limiting credit risk exposure conditional on observables. However, our findings also highlight challenges associated with algorithmic underwriting, as it may yield disparate impacts across racial and income groups.

We assemble a large dataset to address our research questions. We start 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. It also contains borrower and property information such as the locations of purchased properties, borrower credit scores, and debt-to-income ratios. Importantly, the dataset also provides 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 ethnicity. We track the changes in 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. Both descriptive and regression analyses show a substantial increase in the number of loans issued to low credit score borrowers, particularly above the DTI ratio of 43. We also track changes in loan volume in each DTI bin. Compared to the pre-event period, the post-event period features more loans with DTI ratios above 43 and fewer loans immediately below this DTI cutoff. We then employ a counterfactual estimation approach to draw causal inferences regarding the effects of the regulation change (DeFusco, Johnson, and Mondragon, 2020). This approach utilizes high-credit-score borrowers, who are unaffected by the policy, as the placebo group, and considers the changes in DTI distribution among that group as the counterfactual for the changes in DTI distribution among the affected group. We validate the assumptions underlying this approach by showing that it can generate accurate estimates of the counterfactual distribution in a placebo year with no policy change. Using this approach, we find that the policy reform substantially increases the total quantity of loans for low-credit-score borrowers, by around 10.3%. Our evidence also suggests that the FHA policy reduces the origination of low-DTI loans by around 9% and pushes up the average DTI ratio by 1.3.

Given the large increase in credit quantity, a question naturally arises as to whether algorithmic underwriting increases borrowers' default probabilities. To answer this question, we first adopt a difference-in-difference method, comparing the changes in delinquency rates following the policy event between treated (low-credit-score) and control (high-credit-score) borrowers. Such a comparison is made for loans above (high-DTI) and below (low-DTI) the DTI cutoff of 43%, respectively. Despite a baseline default rate of 5.9%, we do not find evidence that delinquency rates increase more for low-credit-score loans compared to high-credit-score ones following the policy reform, either for high-DTI or low-DTI loans. We then utilize 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 significantly different from zero. A remaining concern is that AUS may grant credit to "fragile" borrowers, who are prone to defaults in worsening economic conditions. To address this concern, we show that delinquency rates do not increase even in areas with the highest increase in unemployment rate after the FHA policy. We continue to find no significant increases in alternative measures of delinquency rates, including less severe delinquencies and delinquencies over longer horizons. Combined, these results suggest that an increased reliance on algorithmic underwriting need not be correlated with an increase in default risk.

We next explore how the policy-induced credit expansion varies across racial and income groups. This analysis sheds light on an ongoing discussion regarding the potential disparate impact of algorithmic underwriting relative to human underwriting (Fuster, Goldsmith-Pinkham, Ramadorai, and Walther, 2022; Das, Stanton, and Wallace, 2023). Strikingly, despite the policy's focus on low-credit-score borrowers and the 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, but is weaker among Black and low-income borrowers. The number of loans increases by 12% (10%) for high-income (White) borrowers, but only 3% (1%) for low-income (Black) borrowers. At the same time, delinquency rates conditional on observables did not increase for any of the subgroups. Our results highlight the difficulty of increasing financial inclusion for minority and lower income individuals through algorithm underwriting.

One explanation for our results thus far is that algorithms having an advantage in processing hard information compared to humans. Such an advantage leads to a large credit expansion and no changes in default rates among affected borrowers. It can also explain the limited effects among Black and low-income borrowers, whose information is less represented in historical data. We also consider alternative mechanisms. First, we consider a simple capacity constraint channel, wherein algorithms alleviate the workload of human underwriters by directly approving loans, but do not utilize information differently from humans. Under this mechanism, we should expect 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. This suggests that the simple capacity constraint channel cannot explain our findings.

A second channel is that algorithmic underwriting can mitigate regulatory concerns associated with lending to high-risk market segments (i.e., a "regulatory concerns" channel). Using human underwriters, lenders may fear FHA scrutiny when issuing risky loans, resulting in overly stringent lending standards. FHA-endorsed algorithms may alleviate these concerns, facilitating the approval of loans to riskier households.[1] We evaluate this explanation 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, Kim, and Pence, 2023). Consistent with the predictions from the regulatory concerns channel, we find that the FHA policy led bank lenders to expand credit more than nonbank lenders. At the extensive margin, FHA mortgages increase by 16.1% from banks, but only around 11% for nonbanks.

Taken together, our findings so far could be consistent with both algorithm underwriting using hard information differently, and that it alleviates lender regulatory concerns, resulting in credit expansions without changing the average risk among approved borrowers. 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. As for the null results for the high-DTI group, while the declining use of manual

[1]The regulatory concerns channel can also be consistent with a weaker effect among Black applicants, if lenders' concerns about lawsuits on unequal lending standards make them provide more loans to those applicants.

underwriting potentially reduces the labor cost for lenders, such cost-saving may be small due to the rigidity in the labor markets. There may also be limited pass-through of underwriting costs to loan pricing due to lenders' market power.

We then explore the non-financial consequences of algorithmic underwriting for households. Specifically, we examine whether the policy-induced credit expansion increases household mobility to higher-quality neighborhoods, measured based on school district ratings. We focus on school quality because it is correlated with various other desirable neighborhood traits, and the quality of early education can critically shape upward mobility (Restuccia and Urrutia, 2004). Difference-in-Differences estimates suggest that low-credit-score individuals are more likely to move to higher-rated school districts compared to high-credit-score individuals living in the same zipcode, with the same gender, and in a similar age range after the policy change. We further use a two-stage-least-square (2SLS) approach to precisely link changes in school quality to the FHA policy implementation, and quantify the magnitude of the school quality change. The first stage shows that low-credit-score individuals are more likely to obtain a new FHA mortgage after the policy change. In the second stage, the predicted increase in mortgage access in turn leads to an increase in school quality. 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. 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 the access to credit for low-credit-score borrowers without aggravating credit risk exposure, and the effects vary across racial and income groups. While clearly identified, 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 changes in borrower demand. 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 borrower racial and income groups, to shed on the mechanisms driving the unequal benefits from algorithmic underwriting.

The structural estimations reveal that the removal of 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. At the same time, our estimates highlight the role of demand-side factors. While the supply of FHA loans increased to a lesser extent for Black and low-income borrowers, the discrepancy in supply only accounts for 34% to 50% of the total difference in credit across demographic groups. The rest of the gap is explained by the differences in the changes in credit demand. Overall, our structural approach helps us decouple the changes in credit supply and demand driven by the FHA policy, and sheds light on the sources of the disparate impacts generated by algorithmic underwriting across demographic groups.

Our study contributes to several strands of literature. First, we add to the burgeoning literature discussing the increasing use of technology in the mortgage underwriting (Tzioumis and Gee, 2013; Berg, 2015; Cortés, Duchin, and Sosyura, 2016; Foote, Loewenstein, and Willen, 2019; Fuster, Plosser, Schnabl, and Vickery, 2019; Giacoletti, Heimer, and Yu, 2021; Fuster et al., 2022; Johnson, 2023b; Das et al., 2023). Human loan officers are found to be influenced by volume-based incentives and errors (Tzioumis and Gee, 2013; Cortés et al., 2016; Giacoletti et al., 2021). In comparison, lender automation allows them to process loan applications faster, and respond more elastically to demand shocks (Fuster et al., 2019; Erel and Liebersohn, 2022). Yet, certain algorithms could aggravate the inequity of credit access across racial and gender groups (Fuster et al., 2022; Chu, Sun, Zhang, and Zhao, 2023; Das et al., 2023). We complement this literature by showing 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 leads to 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 prior evidence, we find that not all borrower groups benefit equally from automated underwriting.

Second, our paper is related to the broader literature on algorithmic underwriting in financial intermediation. In particular, in the auto loans market, Jansen, Nguyen, and Shams (2021) use a randomized experiment to show that algorithmic underwriting outperforms human underwriting for riskier and more complex auto loans.[2] Similarly, Costello, Down, and Mehta (2020) use a randomized controlled experiment among trade creditors (firms) to study the implications of AI-based lending models. We study the role of human-augmented algorithmic underwriting in a different market, i.e., 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 shed light on a broader set of consequences of algorithmic-based lending for households and government agencies, such as the financial inclusion of high-risk borrowers and their subsequent location choices, the risks borne by government agencies, and the distributional consequences across income and demographic groups.

Finally, our paper complements and expands upon 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, Gerardi, Goette, and Willen, 2010; 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 (Kinghan, McCarthy, and O'Toole, 2022; Acharya, Bergant, Crosignani, Eisert, and McCann, 2022; Tzur-Ilan, 2019; Van Bekkum, Gabarro, Irani, and Peydró, 2019). 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 quality 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

[2] Jansen et al. (2021) find that algorithms approve fewer auto loans, but charge higher interest rates and are associated with lower default rates. Their evidence suggests that algorithms can better process complex loans, and reduce the agency conflicts related to winning loan auctions. In contrast, we find algorithm underwriting is associated with significant credit expansion but little change in interest rates or delinquency in the mortgage market.

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.[3] 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.

The manual underwriting process involves more human discretion. For borrowers with opaque credit 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.[4] 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. At the same time, human underwriters may reject applications when borrowers' documents may overrate their income potential or under-represent their risk.[5] 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-

[3] 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).

[4] 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.

[5] See FHA's Training Module referenced in Footnote 4.

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.[6] 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.[7]

Importantly, the manual underwriting mandate was lifted in August 2016 for FHA borrowers with credit scores below 620 and DTI ratios above 43%.[8] 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.[9] 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 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. Subprime private label secularization was common before the financial crisis but their volume has fallen sharply in 2007-2008 (Frame, Gerardi, and Sexton, 2021). While in theory portfolio lending is a possible alternative to FHA lending, Kim, Liu, and Zhang (2024) 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, the 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.[10]

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

[7] See the policy fact sheet HERE.

[8] See the description of the policy change HERE. As described in the article, in March 2019, the FHA partially reinstated this policy by referring more credit score under 620, DTI over 43 borrowers (though not all credit score under 620, DTI over 43 borrowers) to manual underwriting, but the volume impact of this partial reinstatement was small as can be seen in Figure 1.

[9] 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.

[10] As explained in Goodman (2014): "The costs of servicing delinquent loans are much higher than the costs of

Second, after the borrower defaults and if the lender submits a claim to the FHA for reimbursement, the lender runs 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)). 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 disclosed loan-level origination and performance information on the universe of its MBS issues starting in September 2013. FHA mortgages are typically included in a Ginnie Mae MBS so as to take advantage of the Ginnie Mae's government guarantee. The Congressional Budget Office (CBO) estimates that the Ginnie Mae MBS issues make up about 97% of FHA insured mortgages.[11] The loan-level disclosure data we use is comparable to the data compiled by eMBS, which as been used in a number of recent studies on the FHA market including Fuster, Hizmo, Lambie-Hanson, Vickery, and Willen (2021) and Kim, Lee, Scharlemann, and Vickery (2022), with the latter describing it as "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 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 in its monthly performance files, which we use to calculate our delinquency variable.

Streamline refinances, which have limited credit score and income verification requirements, are available to borrowers during our study period and show up with missing debt-to-income ratio and credit scores in

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."

[11] See the breakdown HERE.

the Ginnie Mae loan level data. For this reason, we focus our analysis on new purchase mortgages. 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. The Ginnie Mae data is merged with HMDA data via the publicly available FHA Single-Family endorsements data as an intermediary link. 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 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 using two samples. 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.[12] 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.

[12]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. It contains a 1% national sample of U.S. individuals selected based on the last two digits of their social security number. This procedure leads to a random sample of individuals because the Social Security Administration sequentially assigns the last 4 digits of social security numbers to new applicants regardless of geographical location. 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, and more).

We build an individual-level annual 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 also allows us to track individual addresses (accurate to the census tract) over time, thus identifying those who moved in a given year. Moreover, it 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.

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 the following variables of interest: (1) *d(School Rating)*, the year-on-year change in a household's local school rating. (2) *Higher Rating*, an indicator for whether an individual moves to a location with a higher school rating. Both variables indicate changes in neighborhood quality.

3.4 Other Variables

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.

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 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.

When analyzing individual mobility, we include controls for individual characteristics such as gender, marital status, and credit score.

3.5 Summary Statistics

Table 1 presents the definitions and summary statistics of the variables used in our study. On average, around 57 loans are originated in each DTI-FICO bin-month. These bins are defined by a combination of DTI and FICO and are used in loan volume analysis in Table 2. For instance, each bin encompasses a single-unit change in DTI and a 20-unit change in FICO. This means that as the DTI (FICO score) increases or decreases by one (20), individuals are placed into different bins.

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

Our primary analysis focuses on the effect of the FHA policy change on the quantity of home purchase loans granted to households. To start, we provide descriptive evidence on the changes in mortgage volume. We then perform bunching estimation to sharpen causal inferences and separately quantify changes in mortgage take-up and the shift in household leverage.

4.1 Initial 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 where the corresponding DTI ratio exceeds 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-reform period, high-DTI loans accounted for around 8-9% of the total number of mortgage loans extended to low-credit-score borrowers. After the policy date, we observe a sharp jump in the high-DTI loan share among low-credit-score borrowers, rising to 23% within two months and reaching 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., the control group).

FIGURE 1 ABOUT HERE

We next look into the 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. We quantify these components in Section 4.2.

FIGURE 2 ABOUT HERE

We formally examine the loan growth patterns in a regression framework, which allows us to control for more covariates and sharpen our inferences. 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, Liu, and Wardlaw, 2022):

$$Log(E(loans)_{d,f,t}) = \beta_1 Treated \times Post + \beta_2 Treated + \tau_t + \phi_f + \delta_d, \tag{1}$$

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 2 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* × *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 near-zero coefficient shown in Column (4), which suggests little change in the low-DTI loan volume to low-credit-score borrowers.

TABLE 2 ABOUT HERE

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

$$\begin{aligned} Log(E(loans)_{d,f,t}) = {} & \gamma_1 Treated \times High\,DTI \times Post + \gamma_2 Treated \times High\,DTI \\ & + \gamma_3 Treated \times Post + \gamma_4 High\,DTI \times Post + \tau_t + \phi_f + \delta_d, \quad (2) \end{aligned}$$

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 2. 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 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 2, 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 3 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 (< 620) 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 for their low-credit-score counterparts. 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).[13]

Notations and Assumptions

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 $post$ indicates 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.

[13]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.

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

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

$$\hat{n}_{hd}^{post} = n_{hd}^{post} \tag{3}$$

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.*

$$\sum_{d=0}^{\bar{d}} \hat{n}_{ld}^{post} = \sum_{d=0}^{\bar{d}} n_{ld}^{post} \triangleq N_{l\bar{d}}^{post} \tag{4}$$

$N_{l\bar{d}}^{post}$ denotes the observed total number of low-credit-score loans right 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.

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.*

$$\frac{\hat{n}_{ld}^{post}}{N_{l\bar{d}}^{post}} = \frac{n_{ld}^{pre}}{N_{l\bar{d}}^{pre}} + \left(\frac{n_{hd}^{post}}{N_{h\bar{d}}^{post}} - \frac{n_{hd}^{pre}}{N_{h\bar{d}}^{pre}} \right) \triangleq \hat{\pi}_{ld}^{post} \tag{5}$$

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.

We define $\hat{\pi}_{ld}^{post}$ as the counterfactual fraction of low-credit-score loans for a given DTI bin in the post-event period. By construction, the counterfactual number of loans for DTI d is $\hat{n}_{ld}^{post} = \hat{\pi}_{ld}^{post} N_{l\bar{d}}^{post}$.

Figure 4 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 absent the policy reform. 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 barely present in the actual, post-policy distribution. This contrast is striking and suggests that the requirement for human underwriting for low-DTI borrowers leads to the bunching of loans under the DTI= 43 threshold. In addition, the actual and counterfactual distributions closely match each other at DTI ratios below 36. Based on this pattern, it is reasonable to set $\bar{d} = 35$, below which the actual distribution is not affected by the policy. In our analysis, we also experiment with $\bar{d}$ being 32, 34, and 36 to test the robustness of our findings.

Figure 4 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 Figure 5 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 4 is unlikely due to a general time trend, but instead related to the increased reliance on algorithmic underwriting.

Figure 5 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):

$$\Delta Loans\ Originated = \frac{1}{\hat{N}_l^{post}} \sum_{d=\bar{d}}^{57} (n_{ld}^{post} - \hat{n}_{ld}^{post}) \tag{6}$$

The expression inside the parentheses indicates the additional number of low-credit-score loans with DTI above $\bar{d}$ due to the policy change. This number is normalized by the total loan counts in the counterfactual scenario to account for changes in aggregate market conditions. The DTI variable is winsorized at the 1^{st} and 99^{th} percentiles and hence capped at 57. When computing this statistic, we bootstrap standard errors by 1000 replications to calculate the statistical significance of the results.

We calculate the above metric using the Ginnie Mae-Endorsement-HMDA matched sample. We focus on loans for purchasing single-family, non-manufactured housing issued during the period of August 2015 through August 2017, i.e., 12 months before and after the regulation change.

Results are reported in Table 3. In Column (1), we set the cap for "unaffected" DTI range $\bar{d}$ to be 35, following the pattern displayed in Figure 4. Results from the extensive margin suggest a significant increase by 10.3% for loans with DTI above $\bar{d}$. 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 3 ABOUT HERE

Note that these magnitudes differ from those generated from the OLS regression analyses (Table 2). 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 $\bar{d}$. The latter denominator is about five times the size of the former.

Changes in DTI Distribution

The pattern shown in Figure 4 suggests that the policy change gave rise to a drastic shift in the DTI distribution. Not only did lenders expanded 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:

$$\Delta Low\ DTI\ Loans = \frac{1}{\hat{N}_l^{post}} \sum_{d=\bar{d}}^{43} (n_{ld}^{post} - \hat{n}_{ld}^{post}) \tag{7}$$

In the parentheses, $n_{ld}^{post} - \hat{n}_{ld}^{post}$ indicates the reduction 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 3 shows the reduction in low-DTI loans to be about 8.6%. This means that at least 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, it is unlikely to affect 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 change in the average DTI ratio of approved loans. Formally, we define the change in average DTI the following:

$$\Delta Average\ DTI = \sum_{d=1}^{57} d \left(\frac{n_{ld}^{post}}{N_{l}^{post}} - \frac{\hat{n}_{ld}^{post}}{\hat{N}_{l}^{post}} \right) \tag{8}$$

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 3, we find that the FHA policy led to a sizeable increase in the DTI ratio of mortgages by around 1.3.

Taken together, results from our bunching estimator suggest that the increased reliance on algorithmic underwriting leads to a substantial increase in the origination of high-DTI loans. This effect is driven both by borrowers switching from low-DTI to high-DTI loans and by the entry of new borrowers. In the remainder of the analysis, we focus on the overall increases in loan volume (i.e., the extensive margin), since that metric is less subject to noises 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. We follow a similar design outlined by Equations 1 and 2, except that we no longer use a DTI-FICO bin-month sample, but instead use a loan-level panel for these analyses. 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 each loan, we track whether the borrower incurs delinquency over the next two years, and regress the delinquency rate on the interaction of *Treated* and *Post*, as well as the triple interaction of *Treated* × *Post* × *High DTI*.

Results are reported in Table 4. Panel A reports results from the difference-in-difference analysis. 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 impose continuous controls as well as origination month fixed effects and FICO grid-by-DTI fixed effects. The controls include the log of loan amount and the log of borrowers' household income. Origination month fixed effects help remove macro-level changes in lending standards, while the FICO-DTI fixed effects allow us to fix loans of a certain risk profile and track their performance around the policy reform. In the next specification (Columns (2) and (5)), we include origination month-DTI fixed effects, which absorb overall changes in the ability to repay for households with a certain leverage category. In the last specification (Columns (3) and (6)), we add county fixed effects to remove geographical heterogeneity in default rates. Across all specifications, *Treated* $\times$ *Post* generates small and insignificant coefficients for both high- and low-DTI loans. This result suggests that the policy change does not affect the default rate of low-credit-score borrowers differently from high-credit-score borrowers in a statistically significant manner.

TABLE 4 ABOUT HERE

Panel B reports the results from the triple-difference regressions, comparing the differential changes in delinquency rates to treated borrowers between high- and low-DTI loans. Again, there is no statistical difference in the changes in delinquency rates between the two subsamples either.

One concern regarding our delinquency results could be that our test may not have the power to detect the policy effects. One may argue that delinquency rates have been low during 2015–2017, because housing prices and economic conditions have been stable or improving during that period. In situations where households are more prone to default, we may observe increases in delinquency rates in post-policy periods. Counter to this argument, we note that the average delinquency rate in our sample is not negligible, but hovers around 6% for overall, 12% for our treated group of low FICO borrowers, and 14% for low FICO, high DTI borrowers.[14] To further address this type of concerns, we conduct a robustness analysis in Table 5, where we separately look at the effect of the policy across locations with different unemployment growth rates. Unemployment growth is measured as the difference from one year prior to the policy change to one year after. To the extent that increases in unemployment rates are associated with higher mortgage defaults,

[14] 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. To the extent that AUS use allows for a credit expansion while controlling delinquency risk, it would be welfare enhancing based on the aforementioned view.

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

TABLE 5 ABOUT HERE

We test the parallel-trend assumption for the effects on delinquency rates. We perform the triple-difference analysis and analyze the differential changes in delinquency and interest rates for highly levered, low-credit-score borrowers in each of the 12 months centered around the policy date. Figure 6 reports the results. We do not observe significant pre-event trends.

FIGURE 6 ABOUT HERE

In Figure 7, 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. Again, the dots represent the point estimates of the triple-difference coefficients, while the vertical lines represent confidence intervals. If a heavier reliance on machine underwriting admitted more "fragile" borrowers who are prone to default during poor economic conditions, we should observe an increase in delinquency rate in areas with greater increases in unemployment rates. However, we do not find that to be the case. Delinquency rates remain unchanged across counties with better or worse economic conditions.

FIGURE 7 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 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.4, where we look at 3-year and 4-year delinquency rates. Our inferences remain unchanged. Finally, it is possible that our measure of delinquency, which focuses on 90-day delayed payment, is too severe and does not capture milder levels of borrower distress. In Table B.5, we evaluate less severe delinquencies, including 30-day and 60-day delinquencies, and continue to find no changes associated with the FHA policy reform.

Taken together, results from this section indicate that the credit expansion induced by the policy change does not come at the expense of greater credit risk exposure for lenders or the FHA. Despite there being an influx of borrowers at the high-DTI range, these borrowers do not face significantly higher interest rates. In contrast, interest rates do slightly increase for low-DTI loans after the policy reform, likely reflecting algorithmic adjustments to the shifting borrower types.

6 Heterogeneous Effects Across Borrower Race and Income

Next, we partition the sample based on borrowers' racial and income groups. This analysis helps shed light on the discussion regarding whether algorithmic underwriting can generate disparate impacts across races and income groups.

We construct three subsamples according to borrowers' ethnicity: Black, Hispanic, and White (Non-Hispanic). We then repeat the bunching estimation for each of the subsamples. Panel A of Table 6 reports the results from this heterogeneity analysis, both across racial groups and across high- and low-income borrowers. We find that the policy-induced increase in loan volume is largely concentrated on White borrowers, with the magnitude being 10.8%, similar to the full sample result. In contrast, such an effect is small in magnitude and statistically insignificant for Black borrowers.

Table 6 About Here

We also partition the sample by the median of borrowers' adjusted income, which is household income scaled by the MSA median level. As mentioned earlier, this location-based adjustment helps eliminate the heterogeneity created by cross-region differences in economic conditions and lending standards. Panel B of Table 6 reports the heterogeneous effects of the policy for higher and lower-income borrowers. We note that the increase in loan volume is uniformly stronger for higher-income borrowers than lower-income ones. Borrowers with above-median adjusted income experience a 13.6% increase in loan origination volume after the policy shift. We observe similar differences when partitioning the sample of borrowers into above-median and below-median income groups within their respective racial categories (Appendix Table B.1).

In Panel C, we examine the changes in delinquency rates for each of the racial and income groups around the FHA policy, and find no significant effects.

Collectively, our results suggest that the policy-induced credit expansion mostly affected White and higher-income individuals. These findings are consistent with the view that algorithmic underwriting has advantages in processing loan applications when there is rich historical data. To the extent that mortgage applications from lower income households are more difficult to process and therefore more affected by capacity constraints (Frazier and Goodstein, 2023; Fuster et al., 2021), these results are inconsistent with the "simple capacity constraint" argument, i.e., algorithms simply relieve human capacity constraints.

7 Economic Mechanisms

Our results so far suggest that a heavier reliance on algorithm underwriting expands credit supply without compromising risk management. The body of evidence is consistent with an efficiency channel, which suggests that algorithms can use hard information more efficiently than human underwriters. This allows them to approve more loan applications at a relatively high quality, without engendering higher credit risk.

As discussed earlier, there are other, potentially non-mutually-exclusive mechanisms through which the FHA policy led to a credit expansion. First, it is possible that algorithms do not use hard information differently compared to humans, but more reliance on mechanisms simply reduces the workload for human underwriters, thus relieving lenders' capacity constraints. This argument suggests that algorithms should expand credit quantity the most in situations where lenders face the greatest capacity constrained markets. Second, algorithmic underwriting can mitigates lenders' concerns regarding FHA scrutiny. Given that the algorithms are approved by the FHA, lenders may have less concerns about receiving FHA pushback on the loans they approve when relying on algorithmic underwriting.

In this section, we analyze the heterogeneous effects of the FHA policy on credit quantity across various dimensions. These analyses help shed light on the underlying economic mechanisms at play. In doing so, we focus 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 DTI ratio.

7.1 Lender Congestion

To start, 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 shrink its employment quickly in response to demand conditions, we expect them to face greater capacity constraints in markets with high application growth.

The simple 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 this is not the case. In fact, loan volume increases more in areas where lenders appear less constrained, by around 12%. In the most congested areas, the credit expansion at the extensive margin is economically small, around 5%. This result is again inconsistent with the simple capacity constraint channel.

TABLE 7 ABOUT HERE

7.2 Bank and Nonbanks

Next, we evaluate the regulatory concerns channel by examining how the FHA underwriting policy affected bank and nonbank lenders differently. To the extent that nonbank lenders face less stringent regulatory scrutiny and are able to securitize a greater share of their loan portfolio, 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.

In all, our evidence on the heterogeneous effects of the FHA policy indicates that the policy change led to a greater expansion of credit for higher income, White borrowers, less congested lenders, and bank lenders. These results are consistent with idea that algorithmic underwriting has advantages in processing loan applications when there is rich historical data, and can alleviate regulatory risk. They are inconsistent with the "simple capacity constraint" argument, i.e., algorithms simply relieve human capacity constraints without processing hard information differently. In other words, underwriting algorithms are not a simple substitution for human underwriters, but likely complement and enhance human judgment.

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 do not see changes in interest rate spreads among high-DTI loans, but there is a significant increase in rates for low-DTI loans. This might be caused by changes in borrower characteristics among the low-DTI borrowers. Namely, given that a significant portion of White and high-income borrowers switched to high-DTI loans after the policy shock, the remaining borrowers in the low-DTI pool may exhibit changes in characteristics that are rated as riskier by underwriting algorithms, thus leading to higher rates charged. In Panel B, we confirm that interest rates increase to a less extent for treated borrowers in the low-DTI sample relative to the high-DTI sample. The coefficient of *Treated* × *Post* × *High DTI* suggests that the differential change in interest rates for highly levered, low-credit-score borrowers is relatively small, around 3 basis points.

TABLE 8 ABOUT HERE

Finally, in Figure 8, we do not observe any pre-policy changes in interest rates, confirming that the previous findings are not driven by pre-existing trends.

FIGURE 8 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, Hendren, and Katz, 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, Chetty, DeLuca, Hendren, Katz, and Palmer, 2019). 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. We control for individual characteristics, including gender, marital status, age, and credit score. In some specifications, we also include origin zipcode-by-year interactive fixed effects or zipcode-by-gender and age interactive fixed effects to account for the possibility that upward mobility varies with gender, marital status, age, and location.

Conditional on moving, we find that low-credit-score individuals (i.e. credit score below 620) are more likely to choose better or similar quality school districts after the policy change. We use a difference-in-difference approach akin to Equation 1. Results presented in Table 9 suggest that low-credit-score individuals are about 0.4-0.6 percentage point more likely to move to a higher-rated school district, a 1-1.6%

effect compared to the average probability of moving to better school districts of 37% in this sample. 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

Next, we use the information regarding mortgage initiations in the credit bureau data to precisely link the change in neighborhood quality to the FHA policy implementation, and quantify the magnitude of the neighborhood quality change. We conduct a two-stage-least-square (2SLS) analysis using the entire sample of credit bureau data, 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 link the changes in school district quality to the predicted likelihood of a new FHA purchase.

TABLE 10 ABOUT HERE

Table 10 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.

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.

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 how do these effects differ across demographic 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, Nagel, Yannelis, and Zhang (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:

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

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 may be more likely approved. We need to estimate optimal loan sizes L^* while accounting for the changes in mortgage acceptance 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 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:

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

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, γ represents the borrower's reduced demand for mortgage origination at higher interest rate r, 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, and if they faces a lower interest rate. The value of the outside option of not getting a mortgage, v_i^n, is normalized to zero.

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:

$$\tilde{L_i}(r) = \begin{cases} \arg\max_{L \in \mathcal{A}_i(\theta_i)} v_i^o(L, r) \text{ , if } \max_{L \in \mathcal{A}_i} v_i^o(L, r) \geq 0 \\ 0 \text{ , otherwise,} \end{cases} \tag{12}$$

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.

Consumers' choice sets $\mathcal{A}_i(\theta_i)$ is determined by their DTI and their perceived risk. We use θ to denote their perceived risks in the pre-period and θ' 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. For low DTI borrowers (below 43), we assume that lenders apply a maximum cutoff $\bar{s}_0$, above which the consumer cannot get a loan. For DTI between 43 and 50, lenders apply a more stringent cutoff, which we assume to be $\bar{s}_0 + \bar{s}_{1,0}$ in the pre-policy period and $\bar{s}_0 + \bar{s}_{1,1}$ in the post policy period (with both $s_{1,0}$ and $s_{1,1} \leq 0$). Similarly, we assume that for DTI above 50, the cutoff is $\bar{s}_0 + \bar{s}_{1,0} + \bar{s}_{2,0}$ in the pre-policy period and $\bar{s}_0 + \bar{s}_{1,1} + \bar{s}_{2,1}$ in the post policy period. DTI above 57 is not allowed in either period. Without loss of generality we let θ, θ' follow a standard Normal distribution, and estimate the underwriting cut-offs pre-and-post policy and across demographic and income subgroups.

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 on the extensive margin. 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 3 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 also plotted in Figure 4 for the full sample and estimated separately for our demographic and income subsamples.

We estimate borrowers' extensive margin response following an interest rate decrease following the approach introduced by Bhutta and Ringo (2021). Specifically, we take advantage of the 50 basis point cut in FHA mortgage insurance premium (MIP), which is applicable for mortgages with application dates on or after January 26, 2015. This cut is equivalent to a 50 bps reduction in interest rates to borrowers. Details of this estimation is included in Appendix C.1.1. Our full sample estimates match closely the parameters found in their paper. We repeat the analysis for lower credit score borrowers which is the focus of our study, and we estimate different extensive margin responses for each of our subsamples by borrower race and income.

Overall, we match our model to 18 moments. The first set of 8 moments are 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, where the bins have width 5 with the exception of 35–43 which is where our policy reduced bunching and in the over 50 range. 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, 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' extensive margin response to the MIP cut on top of what can be explained by a relaxation of DTI constraints with borrower DTI targets evaluated at the pre-policy interest rate r_0.

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

$$\min_{\theta}(\tilde{M}(\theta) - M)\hat{W}(\tilde{M}(\theta) - M)', \tag{13}$$

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, Levinsohn, and Pakes (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 captures differences in the proportion of take-up across the income and demographic groups by scaling both factors by the proportion of low credit score mortgages originated by a particular race or income demographic and dividing by the proportion of the particular race or income demographic with low credit scores in the population. We test the robustness of our model to alternative calibrations of $\bar{s}_0$ in Appendix Section C.3, and it does not significantly impact our results. Details of these calculations are shown in Appendix C.1.2.

The estimated parameters are presented in Panel A of Table 11. 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.

TABLE 11 ABOUT HERE

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 approval rates are lower for Black borrowers than non-Hispanic white borrowers across both DTI groups. Similarly, mortgage approval 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.

In the full sample, our estimates for γ, the borrower disutility from higher interest rates, is around 45. This magnitude can be interpreted relative to our estimate of ϕ. In particular, this implies that a one percentage point change in the borrowers' difference to their DTI target is equivalent to a 59 basis points decrease in their interest rate, which suggests that borrowers are highly sensitive to DTI constraints relative to the direct disutility from a higher interest rate.[15] This parameter also varies across demographic subgroups, being significantly higher for Black borrowers than non-Hispanic white borrowers. Hispanic borrowers' disutility from higher interest rates is not significantly different from zero, which suggests that their interest rate elasticity of demand is almost entirely explained by a relaxation of DTI constraints. Finally, our point estimates suggests that borrowers with lower income have a higher disutility from higher interest rates than borrowers with higher income. Our results are consistent with Black and lower income borrowers being more financially constrained and deriving higher utility from a lower interest rate.

In our full sample, our estimate of ψ is 0.270. As mentioned earlier, this magnitude can be interpreted relative to our estimate of γ, and implies that borrowers are highly sensitive to meeting their pre-policy DTI targets. By demographic groups, Black borrowers have a low estimated ψ exhibit little sensitivity to "under-leverage," whereas Hispanic borrowers' sensitivities are in between those of non-Hispanic white borrowers and Black borrowers. The differential sensitivity to target leverage, in addition to differential approval rates, 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 and larger extensive margin responses to the policy change.

Panel B of Table 11 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 each of our subsamples is shown in Appendix C.2, which are qualitatively similar to the full sample fit.

[15] This can be calculated as $\frac{\psi}{\gamma} = \frac{0.270}{45.5} = 59$bps.

9.4 Results

Table 12 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 12 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 11.2-percentage-point increase in consumer surplus, which is significantly higher compared to the welfare gain by Black borrowers (1.9 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 4.3 percentage points, compared to higher-income borrowers at 14.2 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 99 percentage points. Again, the eligibility for high-DTI loans increases significantly more for non-Hispanic White and higher income borrowers. The credit expansion of high-leverage mortgage loans for Black borrowers is about 64 percentage points, about half of the magnitude compared to non-Hispanic White borrowers. Hispanic borrowers are somewhere in the middle, with their eligibility rate increasing by around 94 percentage points. The third row shows that, for lower-income borrowers, the credit expansion (50%) is around a third of the magnitude for higher-income ones (152%). These results indicate that the increased reliance on machine underwriting has led to differential supply expansion by borrower race/ethnicity as well as income.

Recall that in Table 6, we found large differences in credit uptake by race and income. Such differences can be attributed to two sources, one is the difference in the 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:

$$\frac{Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_{full}\}) - Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_e\})}{Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_{full}\}) - Pr(Uptake|\psi_e, \gamma_e; \{\bar{s}_e\})} \tag{14}$$

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: $Pr(Uptake|\psi_{full}, \gamma_{full}; \{\bar{s}_e\})$, we compute 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 borrowers because their extensive margin results are similar to that of the full sample. Results in the first row suggest that around 34% 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 66% of the difference can be attributed to demand differences. For example, Black borrowers may have a lower ψ (i.e., the coefficient on DTI for borrower utility), which may reflect a less strict preference on house size or other constraints such as down payment being more binding. 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 50% 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. Note that the estimate is relatively noisy, with the confidence interval including 100%.

10 Discussion

Algorithmic underwriting is of increasing relevance in an era of big data. We study the impacts of increasing reliance on algorithmic underwriting in U.S. mortgage markets 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. At the same time, these consumer welfare gains are not equally distributed; instead, they concentrated on white and high-income borrowers. This disparate effect highlights the challenges associated with algorithmic underwriting on distributional outcomes.

A related policy question is whether the FHA should charge higher mortgage insurance premiums on low credit score loans due to their higher default risk, despite this risk being not detectably different following the removal of the human underwriting requirement. Layton (2023) suggests that the FHA's relatively uniform pricing across borrower credit scores may imply cross-subsidies across borrowers with different credit scores. Our paper finds that algorithmic underwriting can expand credit supply while keeping default risk relatively constant conditional on borrower credit scores, but the expansion of credit to low credit score borrowers may still increase the total amount of subsidies to those borrowers. Our results imply that such an increase in subsidies would primarily benefit higher income and non-Hispanic white borrowers. We focus on the effect of algorithmic underwriting on risk management, financial inclusion across racial and income groups, and neighborhood choice, and leave the question of whether these borrowers should be subsidized at all for future research.

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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). *# Loans* measures the number of loans in each grid once we collapse the sample into DTI-FICO bin-month grids. *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 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. *d(School Rating) Cond. Purchase* is 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, conditioning on the sample of individuals who have a new FHA purchase in a given year. *Higher Rating Cond. Moving* is an indicator variable that takes the value of one if *d(School Rating)* is greater than zero, conditional on the mover sample. *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
<u>*DTI-FICO Bin-Month Level*</u>						
# Loans	57.417	62.183	10.000	34.000	84.000	12,321
Log (# Loans)	3.250	1.523	2.303	3.526	4.431	12,321
<u>*Loan Level*</u>						
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
Higher Rating Cond. Moving	0.373	0.483	0.000	0.000	1.000	1,231,799
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: **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. 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

Table 3: **Intensive and Extensive Margin Effects on the Quantity of Credit**
This table examines the changes in the intensive and extensive 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 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-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. 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 4: **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. 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 1, Panel B reports the triple-difference analysis following Equation 2. *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 5: **Delinquency Rates Effects by Unemployment Rate Change Quartiles**

This table examines the changes in interest rate spreads and mortgage performance around the changes in underwriting regulations, across borrowers in regions with different changes in unemployment rate. Unemployment rate change is measured as 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 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. 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 a subsample of 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 6: **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. Panel A examines the heterogeneous effects across borrower race, Panel B shows the heterogeneous effects across borrower income categories, Panel C reports the heterogeneous effects for delinquency rates across borrower demographics. 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. 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***	0.014	0.109**
	(0.018)	(0.040)	(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	0.136***
	(0.025)	(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	-0.00334	0.00467	0.004
	(0.00697)	(0.00578)	(0.0089)	(0.00909)
Black	0.0236	0.0316	-0.00611	-0.000334
	(0.0285)	(0.027)	(0.011)	(0.0122)
Hispanic	-0.0366	-0.0352	-0.0103	-0.0124
	(0.0229)	(0.0241)	(0.0159)	(0.0147)
Income Below Median	0.0000724	0.00283	0.000234	0.0017
	(0.0122)	(0.0112)	(0.00754)	(0.0083)
Income Above Median	-0.00967	-0.0061	0.00158	0.00385
	(0.0135)	(0.0144)	(0.00855)	(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 7: **Heterogeneity by Lender and Loan Markets**

This table examines the changes in the intensive and extensive margin changes in loan origination volume around the changes in underwriting regulations for subsamples of lenders and loan markets. Panel A reports the effects across markets with different levels of congestion, and Panel B reports results for bank and nonbank lenders. 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. 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. 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

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

Panel B. Heterogeneity Across Bank and Nonbank Lenders

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

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 1, and Panel B reports the triple-difference analysis following Equation 2. 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. In Panel C, each coefficient represents the triple-difference coefficients from a separate regression. *Non-Hispanic White* represents coefficients from a subsample of Non-Hispanic White borrowers. *Black* represents coefficients from a subsample of Black borrowers and *Hispanic* represents coefficients from a subsample of Hispanic borrowers. *Above-Median Income* and *Below-Median Income* represent samples of borrowers classified into based on whether their relative household income is above or below the sample median. Relative household income is the ratio of household income relative to the median family income level of the MSA. 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: Reduced-Form**

This table examines the changes in the rating of school districts where individuals reside this year compared with last year around the changes in underwriting regulations. The sample includes individuals who moved in a given year 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. $\mathbb{1}$*(Higher Rating)* equals one if the rating of the school district where the individual currently lives is higher than the rating of the school district where she lived in the previous year, and zero otherwise. *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.: $\mathbb{1}$*(Higher Rating)*	(1)	(2)	(3)
Post × *Treat (2015)*	0.0044***	0.0057***	0.0062***
	(0.0016)	(0.0017)	(0.0017)
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	1,229,254	1,210,601	1,152,002
R^2	0.31	0.34	0.40

Table 10: **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.). 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 11: **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 3, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Panel A. Model parameter estimates

	Full Sample	Race/Ethnicity Subsample			Income	
		Non-Hispanic White	Black	Hispanic	Below Med	Above Med
μ_d	0.359***	0.352***	0.383***	0.365***	0.403***	0.348***
	(0.00106)	(0.00272)	(0.00564)	(0.0046)	(0.00211)	(0.00248)
σ_d	0.123***	0.125***	0.103***	0.120***	0.102***	0.133***
	(0.000786)	(0.00273)	(0.00307)	(0.00368)	(0.00116)	(0.00335)
ω_d	0.873***	0.893***	0.580***	1.06***	0.309***	1.030***
	(0.0172)	(0.0605)	(0.0951)	(0.0654)	(0.0243)	(0.0516)
$\bar{s}_{1,1}$	-0.184***	-0.261***	-0.0453***	-0.130***	-0.188***	-0.213***
	(0.0114)	(0.0225)	(0.0164)	(0.0233)	(0.0157)	(0.0189)
$\bar{s}_{2,1}$	-0.150***	-0.225***	-0.0962***	-0.136***	-0.224***	-0.167***
	(0.0110)	(0.0203)	(0.0227)	(0.0261)	(0.0152)	(0.0194)
$\bar{s}_{1,0}$	-0.622***	-0.753***	-0.325***	-0.566***	-0.449***	-0.804***
	(0.0131)	(0.0217)	(0.0213)	(0.0360)	(0.0209)	(0.0208)
$\bar{s}_{2,0}$	-0.0114	-0.0272	-0.0197*	-0.104	-0.0112	-0.0129
	(0.00782)	(0.0121)	(0.116)	(0.015)	(0.0109)	(0.0132)
ψ	0.270***	0.384***	0.0106	0.215***	0.152***	0.306***
	(0.029)	(0.0646)	(0.0203)	(0.0447)	(0.042)	(0.0463)
γ	45.5**	45.053**	158.713***	7.516	68.442***	42.390
	(17.801)	(21.518)	(32.487)	(43.541)	(15.379)	(26.984)

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	0.168	0.007
$43 < DTI_1 \leq 45$	0.079	0.066	-0.013
$35 < DTI_1 \leq 43$	0.372	0.369	-0.003
$30 < DTI_1 \leq 35$	0.142	0.143	0.001
$25 < DTI_1 \leq 30$	0.082	0.083	0.001
$20 < DTI_1 \leq 25$	0.036	0.035	-0.001
Avg DTI ($\overline{DTI_1}$)	0.403	0.399	-0.004
DTI Distribution, Pre-Policy			
Fraction of loans in range			
$DTI_0 > 50$	0.085	0.082	-0.002
$45 < DTI_0 \leq 50$	0.081	0.084	0.003
$43 < DTI_0 \leq 45$	0.036	0.037	0.001
$35 < DTI_0 \leq 43$	0.494	0.490	-0.004
$30 < DTI_0 \leq 35$	0.158	0.158	0.000
$25 < DTI_0 \leq 30$	0.089	0.092	0.003
$20 < DTI_0 \leq 25$	0.041	0.039	-0.002
Avg DTI ($\overline{DTI_0}$)	0.390	0.386	-0.004
Policy elasticity	0.103	0.103	0.000
Interest rate elasticity	0.225	0.226	0.001

Table 12: **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 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 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 11. 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		10.980***	
		[9.485, 12.333]	
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	11.245***	1.881	11.428***
	[9.871, 12.477]	[-2.582, 5.889]	[4.921, 16.170]
Income:	Below Median		Above Median
	4.320***		14.430***
	[1.821, 6.430]		[12.037, 16.499]

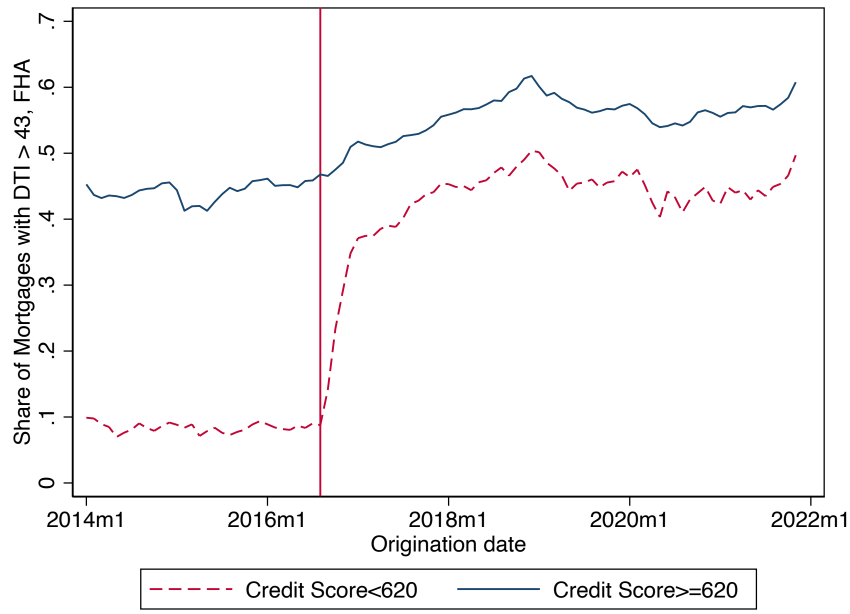
Panel B: % Changes in High-DTI Eligibility

Full Sample		99.430***	
		[92.656, 105.788]	
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	111.704***	63.729***	94.218***
	[103.696, 120.710]	[56.765, 71.157]	[78.483, 111.205]
Income:	Below Median		Above Median
	49.763***		152.373***
	[44.826, 55.145]		[143.491, 161.917]

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

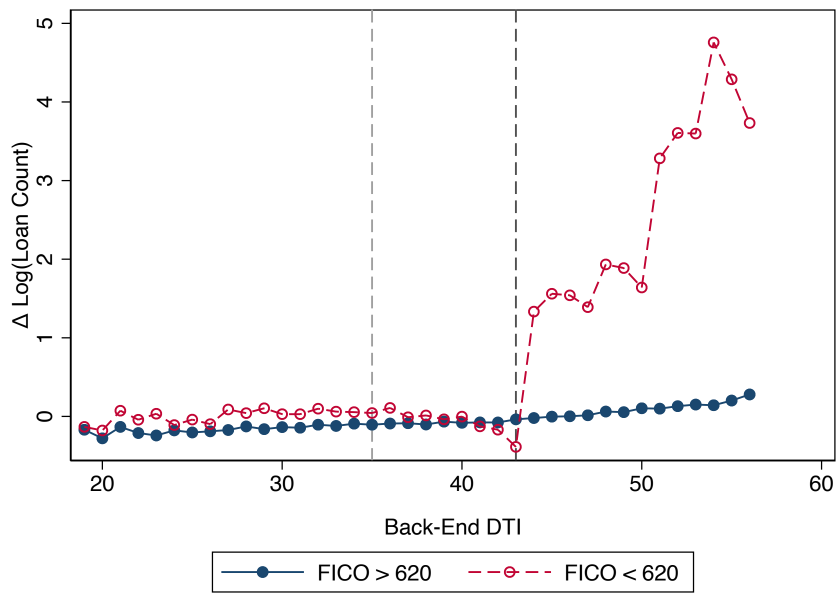
Race/Ethnicity:	Non-Hispanic White	Black	Hispanic
	-	34.101***	-
	-	[27.725, 55.887]	-
Income:	Below Median		Above Median
	50.240***		120.054***
	[39.376, 80.175]		[79.871, 383.755]

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



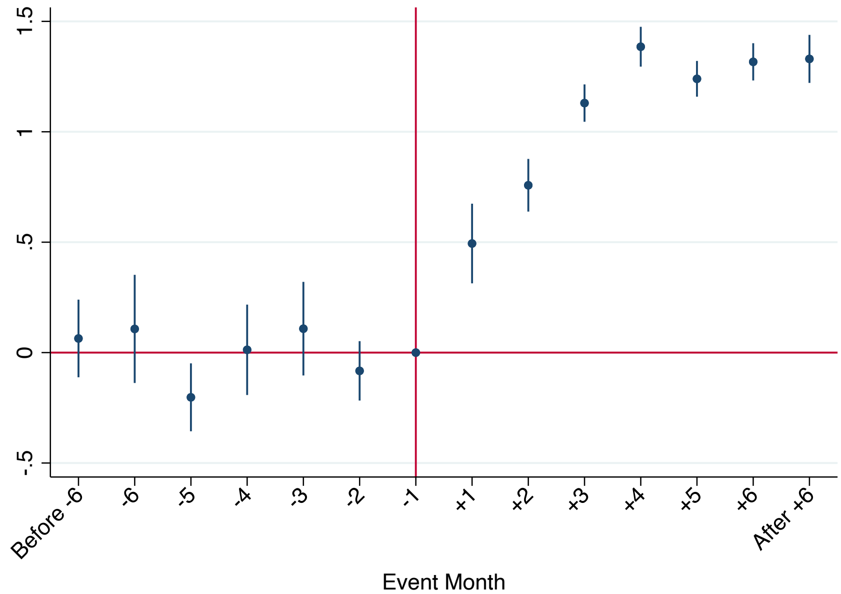
Note: This figure plots the share of FHA new purchase mortgages with an DTI greater than or equal to 43 by their month of origination. The sample is the full sample of FHA loans in our Ginnie Mae data from January 2014 to January 2022. Data for borrowers with a credit score less than 620 and a credit score greater than or equal to 620 are separately plotted. The policy month of August 2016 is marked via a vertical red line. 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



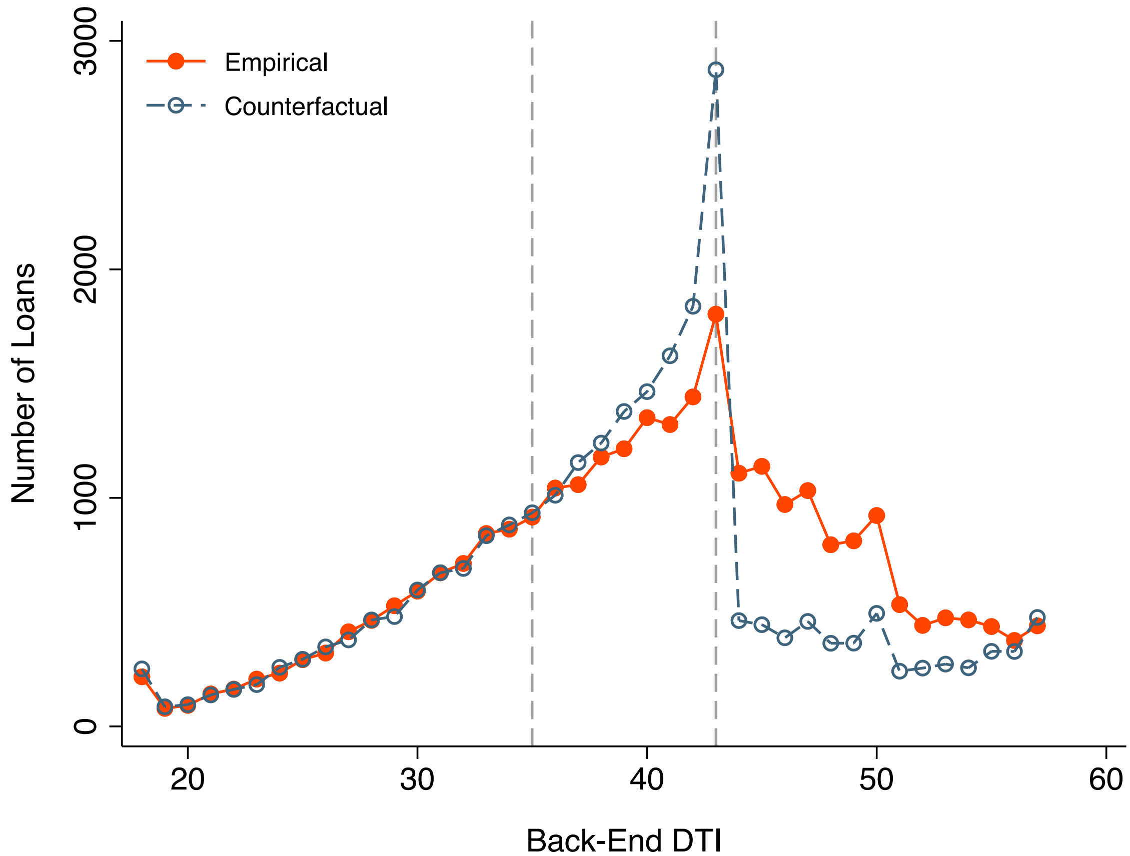
Note: This figure plots the log difference of the number of FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample 12 months after the policy and the number of loans 12 months before the policy by DTI. DTI is winsorized at the 1st and 99th percentiles 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 4.

Figure 3: Dynamic effect of the policy change on loan origination volume



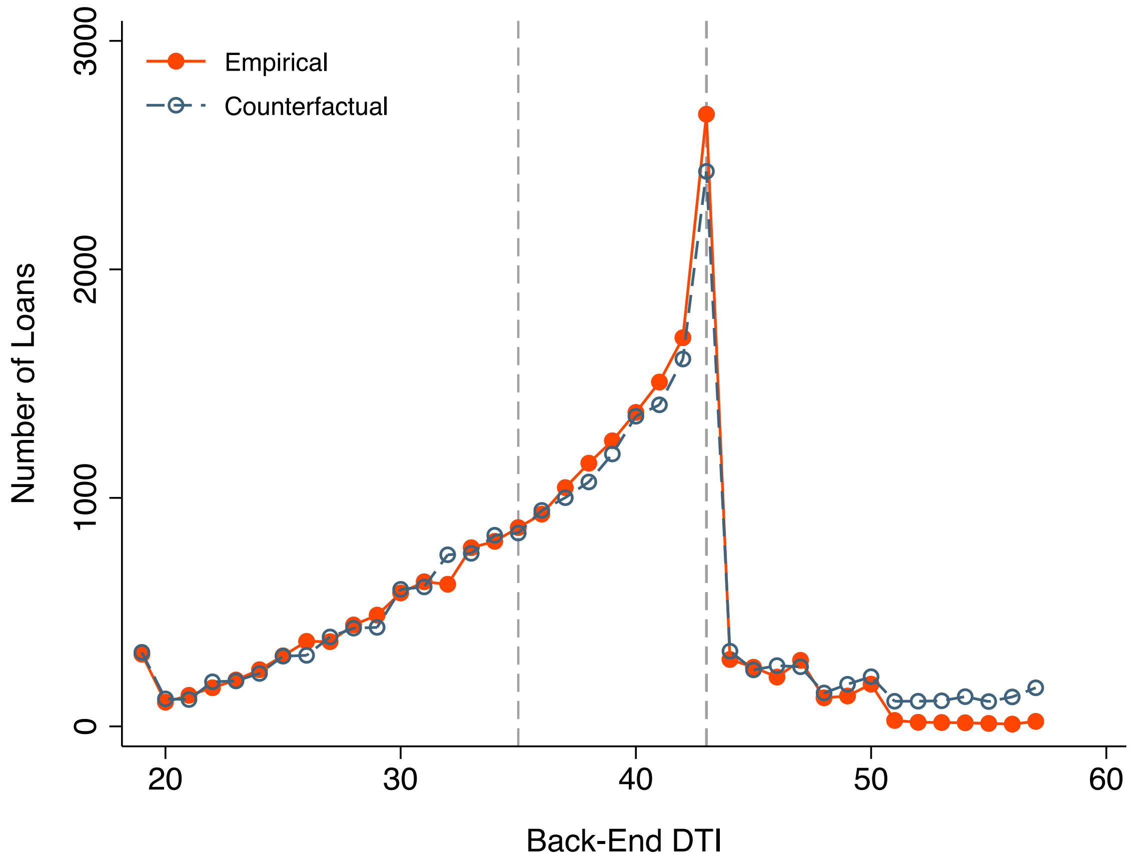
Note: We estimate dynamic triple difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. We utilize Ginnie Mae loans from August 2015 to August 2017 and aggregate the sample into each DTI-FICO bin-month grid. We utilize a Poisson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

Figure 4: Effect of the policy change on loan quantities by DTI



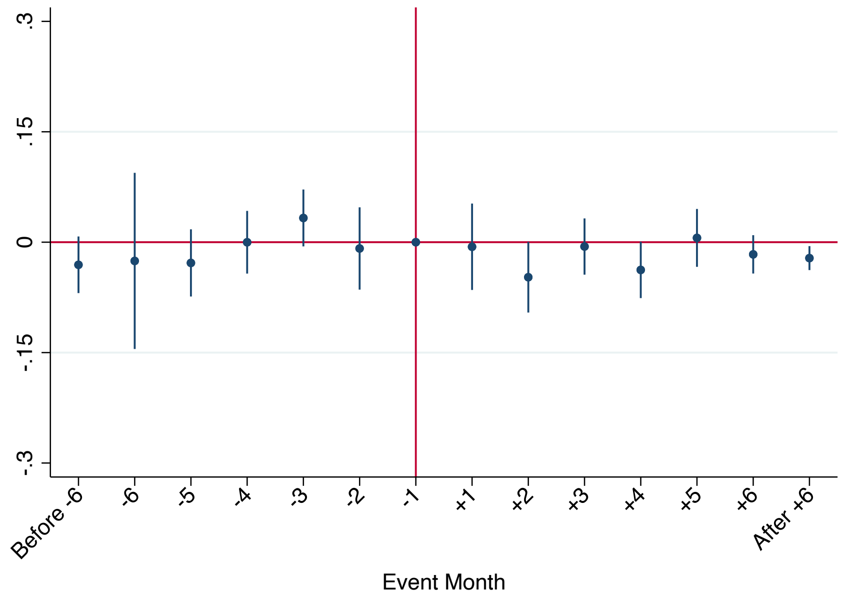
Note: This figure plots empirical and counterfactual number of FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample 12 months after the policy based on the methodology described in Section 4.2. DTI is winsorized at the 1st and 99th percentiles 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.

Figure 5: Placebo analysis, using August 2015 as the treatment date



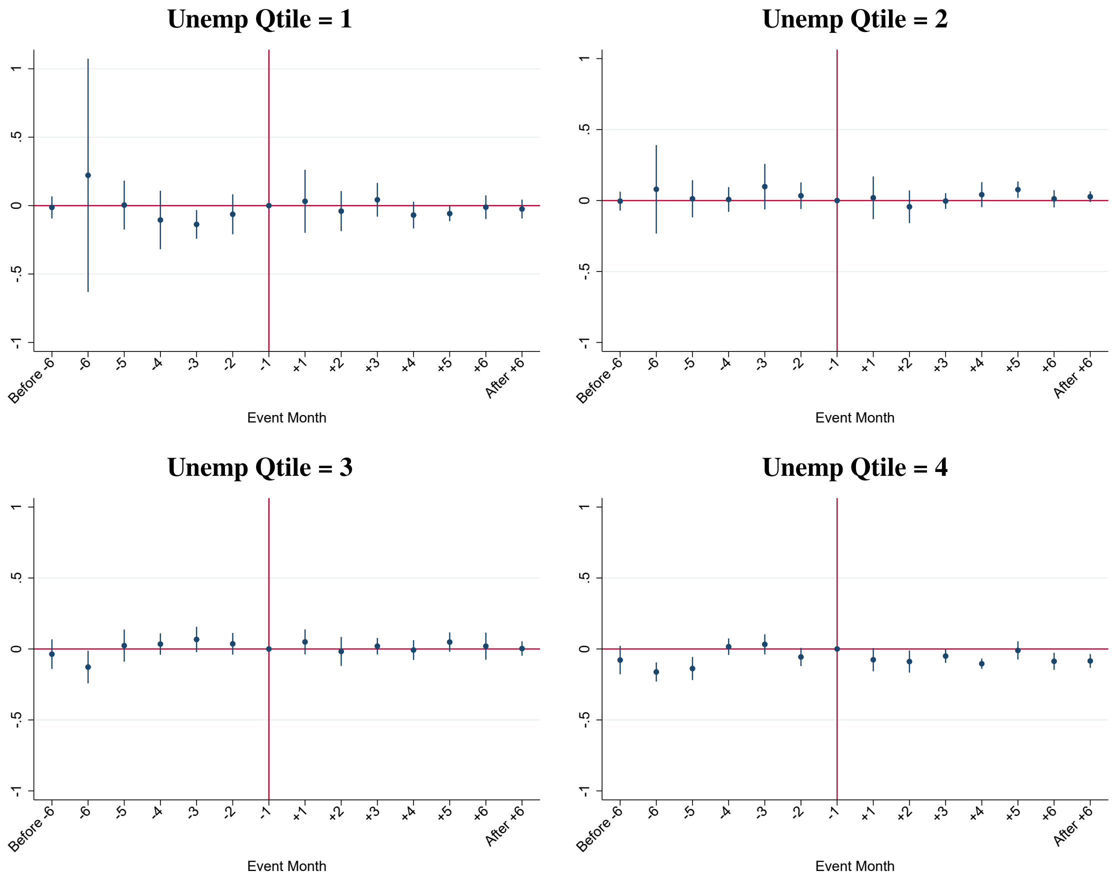
Note: This figure plots empirical and counterfactual number of FHA single-family, non-manufactured housing new purchase mortgages in our Ginnie Mae-Endorsements-HMDA sample 12 months after a placebo treatment date of August 2015 based on the methodology described in Section 4.2. DTI is winsorized at 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.

Figure 6: Trends in delinquency rates



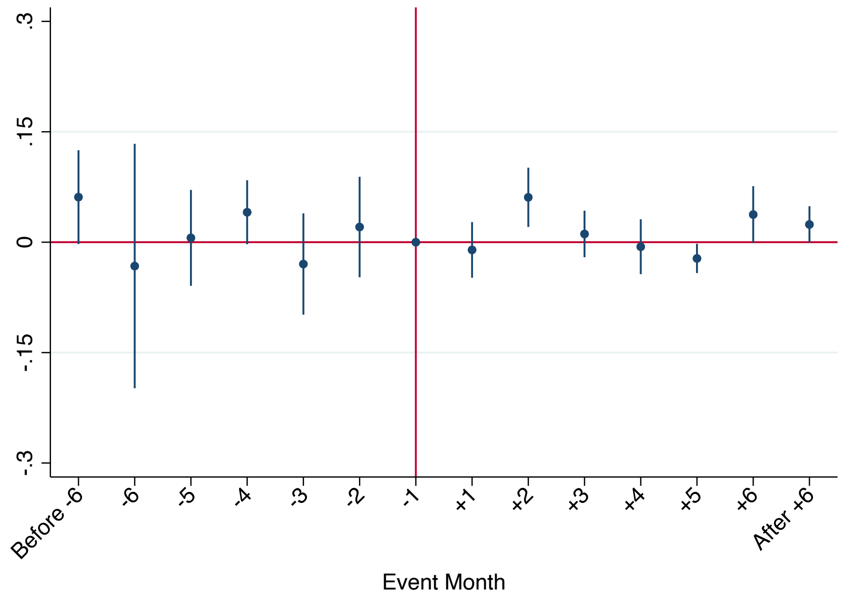
Note: We estimate dynamic triple Difference regressions and plot the coefficient estimates on the event month indicators and the two-tailed 95% confidence intervals. The outcome variable is the 90-day delinquency indicator measured in the two years post origination. The fixed effects and control variables are the same as those used in Table 4 Panel B Column (3). We use the month prior to August 2016 as the base period for estimation (Event Month = -1).

Figure 7: 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 4 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.

Figure 8: 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%. We use only the uniquely matched loans for our empirical analyses. To alleviate concerns about match quality, we also run our extensive margin and loan performance analysis on the Ginnie Mae sample alone, and obtain similar qualitative results. Furthermore, our extensive margin results by borrower demographics are also corroborated by a smaller CoreLogic-HMDA matched sample.

[1] https://www.hud.gov/program$_o$$ffices/housing/rmra/oe/rpts/sfsnap/sfsnap$.

[2] https://www.huduser.gov/portal/datasets/usps$_c$$rosswalk.html$

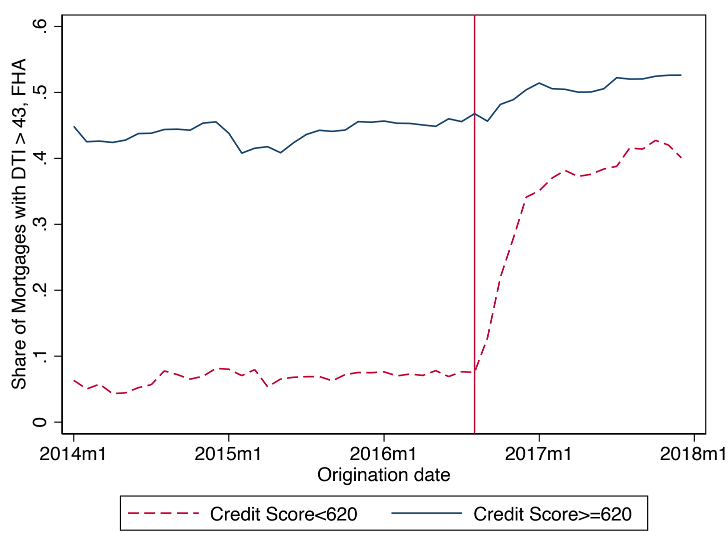
A.2 The CoreLogic-HMDA match

We also construct a match between our CoreLogic Loan-Level Market Analytics (LLMA) data and HMDA data, where the match was conducted at the year-loan amount-zip-loan type-property type-loan purpose-owner occupancy level. In the 11.7% cases where multiple CoreLogic loans share the same year-loan amount-zip-loan type-property type-loan purpose-owner occupancy characteristics, a random CoreLogic loan is kept.

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-Endorsements-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 Heterogeneity by Income and Race

Table B.1: **Heterogeneity by Income and Race**

This table examines the changes in the intensive and extensive margin changes in loan origination volume around the changes in underwriting regulations for subsamples of borrowers in different income and race/ethnicity groups, using the methodology described in Section 4.2. Extensive margin refers to 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. Intensive margin (DTI) refers to the average increase in measured DTI of new purchase mortgage originations as a result 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. 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) Black, High Income	(2) Black, Low Income	(3) White, High Income	(4) White, Low Income
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.3 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. 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 1. 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	Yes	Yes
Month FE	Yes			Yes		
FICO-DTI FE	Yes	Yes	Yes	Yes	Yes	Yes
Month-DTI FE		Yes	Yes		Yes	Yes
County 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.4 Delinquency and Interest Rate Spreads: Heterogeneity Across Race and Income

Table B.3: **Delinquency and Interest Rates: Heterogeneity Across Race and Income**
This table examines the changes in delinquency and interest rate spreads around the changes in underwriting regulations for subsamples of racial and income groups. 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. The sample is restricted below from 35. Panel A reports delinquency results, and Panel B reports interest rate results. The regression specification follows the DID analysis in Equation 1. Delinquency rates are measured as 90-day, 2-year delinquency rates. Interest rate spreads are measured relative to the Freddie Mac Survey rate. Each coefficient represents the triple-difference coefficients from a separate regression. *Non-Hispanic White* represents coefficients from a subsample of Non-Hispanic White borrowers. *Black* represents coefficients from a subsample of Black borrowers and *Hispanic* represents coefficients from a subsample of Hispanic borrowers. *Above-Median Income* and *Below-Median Income* represent samples of borrowers classified into based on whether their relative household income is above or below the sample median. Relative household income is the ratio of household income relative to the median family income level of the MSA. *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. 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, Heterogeneous Effects

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	-0.00334	0.00467	0.004
	(0.00697)	(0.00578)	(0.0089)	(0.00909)
Black	0.0236	0.0316	-0.00611	-0.000334
	(0.0285)	(0.027)	(0.011)	(0.0122)
Hispanic	-0.0366	-0.0352	-0.0103	-0.0124
	(0.0229)	(0.0241)	(0.0159)	(0.0147)
Income Below Median	0.0000724	0.00283	0.000234	0.0017
	(0.0122)	(0.0112)	(0.00754)	(0.0083)
Income Above Median	-0.00967	-0.0061	0.00158	0.00385
	(0.0135)	(0.0144)	(0.00855)	(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

Panel C. Interest Rate Spreads, Heterogeneous Effects

Dep. Var: *Rate Spread*	High DTI (>43)		Low DTI ($35 \leq DTI \leq 43$)	
	(1)	(2)	(3)	(4)
Non-Hispanic White	0.0143	0.00943	0.0506***	0.0322**
	(0.0338)	(0.0305)	(0.0121)	(0.0122)
Black	-0.0321**	-0.0359*	0.00573	0.00116
	(0.0147)	(0.0193)	(0.0161)	(0.0189)
Hispanic	0.0651**	0.0613*	0.0331	0.0279
	(0.0261)	(0.0289)	(0.0191)	(0.0208)
Income Below Median	-0.0166	-0.0167	0.0365**	0.0225
	(0.0262)	(0.0271)	(0.013)	(0.0136)
Income Above Median	0.0421	0.0324	0.0304**	0.0232*
	(0.026)	(0.0237)	(0.00962)	(0.0114)
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

B.5 Delinquency Results: Alternative Measures

Table B.4: **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. 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 1 for 3 year delinquencies, Panel B reports results from the DID analysis following Equation 1 for 4 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

Table B.5: **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. 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 1 for 2 year, 30-day delinquencies, Panel B reports the DID analysis following Equation 1 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

C Model details

C.1 Moment estimation

C.1.1 Interest rate elasticities at the extensive margin

We use a CoreLogic-HMDA matched sample to estimate borrower interest rate elasticities. The sample is described in Appendix A.2, which includes information such as the month of origination, whether the mortgage is a FHA mortgage or not, loan amount, borrower income and race.

We use a regression discontinuity approach with a triangular kernel following Bhutta and Ringo (2021), but with a 6 month window rather than a 25 week window and with the policy month of February 2015 rather than the exact application date. This is because we only have information on the month of origination rather than the application date. Figure 1(b) of Bhutta and Ringo (2021) shows that the MIP cut had an immediate and persistent effect on FHA shares, with market shares being fairly flat around the policy change, which suggests that the effect may be estimable even with a coarser date variable. Indeed, in the full sample we estimate a FHA share elasticity of 15.9%, which closely parallels that of 15.7% implied by Figure 1(b) of Bhutta and Ringo (2021).[3]

To estimate an elasticity that better matches the characteristics of our sample, we repeat the estimation for a group of borrowers with credit scores below 660. The 660 cut-off is used rather than 620 because GSE eligibility begins at 620. In this group, the FHA elasticity of demand for a 50bps decrease in rate is 22.5%. In subsamples, it is 23.3% for non-Hispanic white borrowers, 63.3% for Black borrowers, 9.3% for Hispanic borrowers, 29.3% for low income borrowers, and 22.4% for higher income borrowers.

C.1.2 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 C.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 C.1, implicitly assuming that the proportional differences in take-up are explained by the proportional differences in eligibility. As proportional differences in take-up across subsamples may be explained by factors other than eligibility, we test

[3] Based on the WebPlotDigitizer tool, accessible at https://automeris.io/WebPlotDigitizer/, Figure 1(b) of Bhutta and Ringo (2021) implies that the FHA market share jumped from 22.9% pre-policy to 26.5% post policy, or an increase of $\frac{.265-.229}{.229} = 15.7\%$.

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

Table C.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

the sensitivity of our model to alternative calibrations of s_0 in Section C.3, and find that it does not significantly impact our results.

C.2 Additional model fit results

Table C.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 6, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.094	0.097	0.002
$45 < DTI_1 \leq 50$	0.144	0.150	0.006
$43 < DTI_1 \leq 45$	0.074	0.061	-0.014
$35 < DTI_1 \leq 43$	0.373	0.379	0.005
$30 < DTI_1 \leq 35$	0.156	0.159	0.003
$25 < DTI_1 \leq 30$	0.096	0.092	-0.004
$20 < DTI_1 \leq 25$	0.044	0.043	-0.001
$\overline{DTI}_1$	0.394	0.390	-0.004
$DTI_0 > 50$	0.068	0.067	-0.001
$45 < DTI_0 \leq 50$	0.071	0.071	0.001
$43 < DTI_0 \leq 45$	0.033	0.031	-0.002
$35 < DTI_0 \leq 43$	0.481	0.481	0.000
$30 < DTI_0 \leq 35$	0.173	0.176	0.004
$25 < DTI_0 \leq 30$	0.103	0.103	0.000
$20 < DTI_0 \leq 25$	0.049	0.048	-0.001
$\overline{DTI}_0$	0.381	0.376	-0.004
Policy elasticity	0.108	0.107	-0.001
Interest rate elasticity	0.233	0.233	-0.001

Table C.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 6, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.136	0.143	0.007
$45 < DTI_1 \leq 50$	0.195	0.198	0.003
$43 < DTI_1 \leq 45$	0.092	0.077	-0.015
$35 < DTI_1 \leq 43$	0.363	0.359	-0.004
$30 < DTI_1 \leq 35$	0.118	0.128	0.010
$25 < DTI_1 \leq 30$	0.063	0.064	0.001
$20 < DTI_1 \leq 25$	0.026	0.024	-0.001
$\overline{DTI}_1$	0.418	0.413	-0.005
$DTI_0 > 50$	0.099	0.099	0.000
$45 < DTI_0 \leq 50$	0.116	0.114	-0.003
$43 < DTI_0 \leq 45$	0.042	0.049	0.007
$35 < DTI_0 \leq 43$	0.522	0.514	-0.008
$30 < DTI_0 \leq 35$	0.123	0.128	0.006
$25 < DTI_0 \leq 30$	0.067	0.065	-0.002
$20 < DTI_0 \leq 25$	0.022	0.024	0.002
$\overline{DTI}_0$	0.405	0.404	-0.002
Policy elasticity	0.014	0.017	0.003
Interest rate elasticity	0.633	0.636	0.003

Table C.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 6, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.145	0.147	0.002
$45 < DTI_1 \leq 50$	0.189	0.191	0.002
$43 < DTI_1 \leq 45$	0.084	0.077	-0.007
$35 < DTI_1 \leq 43$	0.369	0.368	-0.001
$30 < DTI_1 \leq 35$	0.124	0.127	0.003
$25 < DTI_1 \leq 30$	0.059	0.061	0.002
$20 < DTI_1 \leq 25$	0.024	0.022	-0.002
$\overline{DTI}_1$	0.419	0.414	-0.005
$DTI_0 > 50$	0.106	0.102	-0.004
$45 < DTI_0 \leq 50$	0.096	0.098	0.002
$43 < DTI_0 \leq 45$	0.042	0.045	0.003
$35 < DTI_0 \leq 43$	0.514	0.514	0.000
$30 < DTI_0 \leq 35$	0.143	0.141	-0.002
$25 < DTI_0 \leq 30$	0.065	0.068	0.003
$20 < DTI_0 \leq 25$	0.028	0.024	-0.004
$\overline{DTI}_0$	0.403	0.400	-0.003
Policy elasticity	0.109	0.109	0.000
Interest rate elasticity	0.093	0.093	0.000

Table C.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 6, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.104	0.109	0.006
$45 < DTI_1 \leq 50$	0.176	0.184	0.008
$43 < DTI_1 \leq 45$	0.085	0.067	-0.018
$35 < DTI_1 \leq 43$	0.391	0.394	0.004
$30 < DTI_1 \leq 35$	0.134	0.135	0.002
$25 < DTI_1 \leq 30$	0.072	0.071	-0.002
$20 < DTI_1 \leq 25$	0.028	0.029	0.000
$\overline{DTI}_1$	0.407	0.405	-0.002
$DTI_0 > 50$	0.111	0.106	-0.006
$45 < DTI_0 \leq 50$	0.104	0.108	0.004
$43 < DTI_0 \leq 45$	0.046	0.046	0.000
$35 < DTI_0 \leq 43$	0.484	0.484	-0.001
$30 < DTI_0 \leq 35$	0.138	0.141	0.003
$25 < DTI_0 \leq 30$	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	0.039	0.001
Interest rate elasticity	0.294	0.293	0.000

Table C.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 6, and the interest rate elasticity is estimated in Appendix Section C.1.1.

Parameter	Target	Model	Difference
$DTI_1 > 50$	0.119	0.120	0.001
$45 < DTI_1 \leq 50$	0.152	0.158	0.006
$43 < DTI_1 \leq 45$	0.077	0.064	-0.013
$35 < DTI_1 \leq 43$	0.357	0.365	0.008
$30 < DTI_1 \leq 35$	0.148	0.150	0.001
$25 < DTI_1 \leq 30$	0.089	0.086	-0.003
$20 < DTI_1 \leq 25$	0.042	0.038	-0.004
$\overline{DTI}_1$	0.400	0.396	-0.004
$DTI_0 > 50$	0.066	0.066	-0.001
$45 < DTI_0 \leq 50$	0.067	0.067	0.000
$43 < DTI_0 \leq 45$	0.030	0.028	-0.002
$35 < DTI_0 \leq 43$	0.500	0.505	0.004
$30 < DTI_0 \leq 35$	0.171	0.171	0.000
$25 < DTI_0 \leq 30$	0.100	0.098	-0.002
$20 < DTI_0 \leq 25$	0.046	0.043	-0.002
$\overline{DTI}_0$	0.382	0.378	-0.004
Policy elasticity	0.136	0.135	-0.001
Interest rate elasticity	0.224	0.224	0.000

C.3 Model robustness

Table C.7: **Model results, robustness check for the Black subsample**
This table displays our structural model results for alternative calibrations of s_0 for Black 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 divided by 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 divided by 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.

	$s_0 = \Phi^{-1}(0.10)$	$s_0 = \Phi^{-1}(0.15)$	$s_0 = \Phi^{-1}(0.20)$
Consumer surplus change (bps)	2.014	1.821	2.124
95% Confidence Interval	[-5.638, 8.629]	[-3.714, 6.374]	[-2.552, 6.452]
Percent change in DTI>43 approvals	63.808***	58.817***	58.961***
95% Confidence Interval	[52.422, 76.090]	[52.645, 64.962]	[51.559, 67.078]