DOES THE DISCLOSURE OF CONSUMER COMPLAINTS REDUCE RACIAL DISPARITIES IN THE MORTGAGE LENDING MARKET?*

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Abstract

The Consumer Financial Protection Bureau (CFPB) disclosed consumer complaint narratives in 2015. Utilizing a difference-in-differences design, I find that, following disclosure, CFPB-supervised lenders whose complaint narratives are disclosed are less prone to discriminate against minority borrowers in the mortgage lending market. This reduces racial disparities in interest rates, default rates, and rejection rates. The disclosure saves minority borrowers $93 million in interest payments and aids 15,000 minority households in securing loans annually, thereby narrowing the racial gap in homeownership. The effect operates through career consequences for loan officers at discriminating lenders, alongside monitoring from consumers and investors, and peer pressure.

Keywords: Disclosure; Banking; Discrimination; Racial gaps

JEL codes: D14; G28; J15.

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1 Introduction

Persistent racial and ethnic disparities in economic outcomes can be largely attributed to differential treatment observed in lending markets, particularly in the mortgage sector. Racial discrimination in mortgage lending is extensively documented in legal proceedings and academic studies, despite the presence of anti-discrimination laws. The significance of this issue is heightened by the fact that mortgages constitute the largest segment of consumer debt in the United States, accounting for 70.6% of total consumer debt and affecting more than 83 million homeowners. Yet these crucial financial products remain notably opaque to consumers, especially among minority borrowers, who often face information disadvantages due to financial literacy gaps and limited access to professional advice. Identifying and reducing such differential treatment in lending practices has been a central concern for policymakers. Since the enactment of the Home Mortgage Disclosure Act (HMDA) in 1975, disclosure has served as a central policy tool to identify and reduce racial disparities in lending. Researchers use standardized lending data from HMDA to document disparities in loan access, pricing, and contract terms across racial and ethnic groups. This institutionalized transparency enables a series of empirical studies (Munnell et al. (1996); Ross and Yinger (2002)) that build frameworks to quantify racial inequality in financial markets and support fair lending regulations. These studies define the prevailing academic approach to analyzing racial disparities in financial markets.

The Home Mortgage Disclosure Act (HMDA) establishes a foundation for monitoring fairness in mortgage markets and offers standardized data that support research and policy over the past half-century. While HMDA's disclosure requirements have provided valuable transparency, racial disparities have continued to be observed in mortgage lending markets. The 2008 financial crisis further exposed and amplified these inequities. Studies (Avery et al. (2005); Bayer et al. (2017); Bhutta et al. (2015); Ghent et al. (2014)) document persistent racial inequality in the lending market that demands continued attention. The Department of Justice also brought allegations against several major mortgage lenders for violating fair lending principles during the housing boom, with settlements exceeding $500 million.[1] In response, the Consumer Financial Protection Bureau (CFPB) was established in 2011 to strengthen consumer protection across financial markets.

The CFPB Complaint Database, established by the Consumer Financial Protection Bureau (CFPB), especially since 2015 with the disclosure of complaints with narrative information, serves as an important financial disclosure mechanism. This mechanism

[1] Regarding the two most substantial settlements, Bank of America reached a settlement of $335 million in 2011, followed by Wells Fargo's settlement of $175 million in 2012.

centers on individual consumer experiences and offers detailed narrative information about financial products, service experiences, and complaint categories. Compared to traditional disclosure tools represented by HMDA, the CFPB complaint disclosure serves as an important complement: it represents a second-generation disclosure approach that shifts focus from standardized loan records to consumer-reported narratives, from supply-side credit data to structured demand-side insights, from model-identified racial disparities to directly expressed perceptions of unfairness, from scholar-led analysis to broad public oversight, and from annual reporting cycles to near real-time feedback. This type of disclosure reduces barriers to participation, allowing broader stakeholder engagement and enabling more immediate responses through public corporate response mechanisms. Together with HMDA, the CFPB disclosure mechanism potentially complements existing tools and helps build a more comprehensive framework for financial market oversight.

Despite the rich consumer feedback collected by the CFPB Complaint Database since its inception, in contrast to the extensive empirical literature based on HMDA, there is limited systematic evidence on how this disclosure influences racial disparities in mortgage markets. This study addresses this gap and represents the first to examine the effect of CFPB complaint disclosure on racial disparities, laying the groundwork for future research on disclosure and financial equity and offering empirical insights to help policymakers refine regulatory tools in the mortgage lending market.

Utilizing a difference-in-differences design, this analysis demonstrates that following the Consumer Financial Protection Bureau's (CFPB) 2015 disclosure of consumer complaint narratives, lenders subject to CFPB oversight and required to disclose consumer complaints exhibit significantly reduced discrimination toward minority borrowers in the mortgage lending market. The policy improves crucial mortgage outcomes, including lower interest rates and fewer rejections for minority borrowers. This disclosure policy significantly reduces the previously elevated lending standards applied to minority borrowers, as evidenced by the convergence of default rates between minority and white borrowers after implementation. These findings demonstrate that the disclosure requirement effectively addresses the overly stringent standards that had disproportionately disadvantaged qualified minority applicants. The economic benefits are substantial: excess interest rates for minority borrowers fall by approximately 2.3 basis points, offsetting nearly 92% of the pre-policy racial interest rate gap and yielding estimated annual savings of $93 million in interest payments. Additionally, rejection rates for minority applicants drop by 1.7 percentage points, enabling about 15,000 additional minority households to obtain mortgage loans annually and helping narrow the racial homeownership gap. The effect operates through multiple channels: career consequences for loan officers at discriminating lenders, enhanced monitoring from consumers and investors,

and increased peer pressure.

To estimate the causal effect of information disclosure, this study leverages a policy change introduced by the Consumer Financial Protection Bureau (CFPB). The CFPB plays a central role in protecting consumers and advancing fairness in financial markets. Since its inception in 2011, the CFPB has collected complaints against financial institutions across a broad range of financial products, with mortgages representing one of the largest complaint sources. On June 25, 2015, the CFPB began publicly releasing consumer complaint narratives along with company responses. The disclosure rule applies asymmetrically across institutions: it is mandatory for all non-depository institutions, and applies to depository institutions only if they hold more than $10 billion in total assets. This selective policy creates a quasi-experimental setting to assess how increased transparency influences discriminatory behavior in the mortgage lending market.

This study exploits the natural experiment created by the CFPB's selective disclosure policy to assess its effect on racial disparities in mortgage lending. The analysis uses a difference-in-differences approach to compare changes in lending practices between CFPB-supervised lenders (treatment group) and non-CFPB-supervised lenders (control group) before and after the June 2015 implementation of the disclosure policy. To enhance identification, the framework is extended to a triple-differences design by adding borrower race as a third dimension, allowing for analysis of how the policy affects racial disparities in lending outcomes. Although CFPB-regulated institutions may differ from non-regulated institutions in size, the identification strategy focuses on changes in racial disparities rather than requiring strict comparability between treated and control groups. The approach relies on the parallel trends assumption—that in the absence of the policy, racial disparities would have followed similar trends across treated and control lenders. Empirical tests support this assumption, mitigating concerns about comparability and allowing for causal interpretation of the results.

Examining the impact of complaint disclosure on racial disparities raises two key empirical challenges. The first challenge is identifying discrimination accurately while accounting for omitted variable bias. To address this issue, the analysis follows Bartlett et al. (2022) and uses the Loan-Level Price Adjustment (LLPA) matrix from government-sponsored enterprises (GSE). This 8×9 grid adjusts guarantee fees based on credit scores and loan-to-value (LTV) ratios and serves as a standardized benchmark for credit risk.[2] By comparing interest rates within each matrix cell, the analysis identifies racial disparities unexplained by credit risk, thereby revealing potential discriminatory practices. The results indicate that treated lenders

[2]The loan-to-value (LTV) ratio is a measure comparing the amount of your mortgage loan with the appraised value of the property. The higher your down payment, the lower your LTV ratio.

respond to complaint disclosure by offering minority applicants improved terms in the form of lower interest rates. Additionally, I examine rejection rates and find that treated lenders also reduce rejection rates for minority applicants following the complaint disclosure policy, further contributing to improved mortgage market access for these borrowers.

This study also employs the outcome test framework (Becker, 1957, 1993) to separate racial bias from omitted variable bias and statistical discrimination. It examines how disclosure affects the default rates of minority borrowers.[3] If racial bias is present, marginal minority borrowers may have lower default rates than marginal white borrowers, as they face disproportionately stringent standards from lenders. My findings support this hypothesis. Specifically, relative to the control group, the initially lower default rate among minority borrowers in the treatment group rises following disclosure, indicating that increased transparency reduces racial gaps in approval thresholds.

These consolidated findings demonstrate that lenders subject to complaint disclosure regulation respond to policy changes by offering more equitable terms, including reduced rejection rates and interest rate differentials for minority applicants. The convergence of default rates between minority and white borrowers further confirms the reduction in discriminatory lending standards. This suggests that disclosure policies incentivize lenders to enhance service quality in order to reduce the likelihood of consumer complaints.

The evidence shows that treated lenders reduce discriminatory behavior against minority borrowers. However, the specific mechanisms through which disclosure mitigates such practices remain underexplored. I investigate multiple mechanisms through which the disclosure policy reduces racial disparities in mortgage lending, focusing on both internal organizational responses and external stakeholder reactions. One potential internal channel is behavioral changes among employees at disclosing institutions. To test this channel, the analysis uses the Nationwide Multistate Licensing System (NMLS) dataset, which provides comprehensive coverage of all loan officers in the United States, to examine the effects of mandatory complaint narrative disclosure on the career trajectories of loan officers. The results show that, after disclosure begins, loan officers at institutions subject to disclosure are more likely to experience downward career moves compared to those at non-disclosing institutions, as evidenced by more frequent transitions to smaller lenders and exits from top-tier institutions. In addition, promotion prospects for these loan officers decline after disclosure. These effects are more pronounced for officers at CFPB-supervised lenders with documented discrimination-related

[3]Lenders engage in statistical discrimination when they utilize race as a substitute for unobservable creditworthiness aspects to maximize profits. In this study, I use the term "statistical discrimination" to refer to decisions that maximize profits. Conversely, I employ the term "racial bias" to denote biased lending decisions. This bias could stem from particular preferences or incorrect beliefs. A more detailed description is provided in Section 5.

complaints than for those at CFPB-supervised lenders without such complaints.

I analyze responses from various external stakeholders to gain insights into additional mechanisms contributing to the effectiveness of the disclosure policy. First, I examine how consumers respond to disclosure. The results indicate that minority consumers incorporate information from complaint narratives and actively avoid lenders with discrimination-related complaints when applying for loans. I also find that peer lenders leverage insights from their competitors' disclosed weaknesses to inform their market entry decisions. Specifically, following the disclosure policy, competing lenders become more likely to establish branches in regions where incumbent lenders have been associated with discrimination complaints, thereby increasing competition in underserved markets and offering minority borrowers additional lending options. Simultaneously, the stock market may view the disclosure policy as an adverse signal, triggering significant reactions. Given potential litigation risks, investors may become more skeptical about the future performance of lenders involved in discriminatory practices. In line with growing attention to socially responsible investing, investors may reduce their investments in firms associated with discriminatory practices. Using an event study, I evaluate the market response following the implementation of the disclosure policy. I find that lenders under CFPB supervision exhibit lower market reactions, with sharper declines in firm value among those implicated in discriminatory behavior.

The persistently low homeownership rate among minority households remains a major policy concern. My research shows that disclosure increases mortgage approval rates among minority borrowers. Using county-year level data on homeownership by race, I examine the real effect of the disclosure policy. Specifically, I test whether improved credit access translates into higher homeownership rates among minority households. The results suggest that disclosure increases homeownership rates among minority households, with minimal effects on white households, thereby narrowing racial disparities in homeownership.

Notably, to further aid minority borrowers in the United States in mitigating potential discrimination in mortgage lending, I develop an online inquiry system.[4] This permits the public to directly and transparently access information regarding local lenders' discriminatory practices.

The rest of the paper is as follows. Section 2 describes the related background, existing research, and contributions of my research. Section 3 introduces the datasets used and the results of summary statistics. Section 4 reports the main analysis of the paper, the impact of disclosure on racial disparities in loan interest rates. Section 5 reports the outcome test for default rates. Section 6 reports the impact on rejection rates. Section 7 reports the impact of disclosure on loan officers' career paths. Section 8 presents the reactions of various

[4]For more details, see: `https://discriminationwarrior.com`

stakeholders to the disclosure policy. Section 9 examines the real effects of the disclosure policy on resident homeownership. Section 10 concludes.

2 Insitutional Background and Literature Review

2.1 Institutional Background of Complaints Disclosure

The Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 established the CFPB to safeguard consumers and promote fairness and competition in consumer financial markets. The CFPB began accepting consumer complaints regarding credit cards in July 2011. Over time, it expanded its scope to complaints about mortgages, bank accounts, credit reporting, and other financial products and services, covering 21 distinct product categories. These complaints typically involve accusations, such as customer service failures or allegations of misconduct by financial institutions. Consumers have reported over 178 types of issues in these complaints. After a pilot phase in 2012 and 2013, the CFPB gradually began to release information related to consumer complaints to the public.[5] On June 25, 2015, the CFPB publicly released the narratives of consumer complaints. The complaint database exclusively records complaints against institutions supervised by the CFPB, which include banks with total assets surpassing $10 billion and all nondepository mortgage originators. Complaints concerning depository institutions holding less than $10 billion in assets are not included in the database. They are directed to the relevant safety and soundness regulators.

In fact, the CFPB solicited comments on the issue before deciding to make complaint narratives public. They received 137 distinct responses from consumer groups, trade organizations, businesses, and individuals, along with 30,000 identical letters from individuals. Lenders and their associations objected to the disclosure of these narratives. They argued that releasing "unverified" consumer narratives might unfairly damage companies' reputations. The CFPB took steps to address these objections. First, the CFPB will confirm business relationships between the lenders and complainants to prevent fraud. Second, the CFPB will make lenders' responses to complaints public, allowing lenders to contest the complaints.

In contrast, consumer advocates, civil rights groups, and proponents of open governance supported the inclusion of narratives. They highlighted three key benefits: (i) empowering consumers with timely and relevant information to make informed decisions before purchase to promote customer-oriented businesses and uncovering unfair or deceptive practices

[5]On March 28, 2013, the CFPB disclosed complaints about mortgages and other products. Since then, the public has been able to download information related to the financial product type involved in the complaint, the submitting consumer's ZIP code, the submission date, the name of the implicated lender, and the lender's brief response.

post-purchase; (ii) aiding the Bureau and other stakeholders in identifying harmful trends before they escalate into widespread damage; and (iii) encouraging greater use of the database as a practical tool to promote accumulating data on consumer experiences in the financial sector. These benefits align with the CFPB's original intentions for this measure (CFPB (2015)).

This study focuses on complaints related to discrimination, disclosed after June 2015, and their role in driving the shift in racial disparities within the mortgage markets. Textual analysis of complaint narratives reveals a relatively consistent annual reporting of 700-900 discrimination-related complaints in the mortgage market. This result is depicted in panel A of Figure A1. Additionally, panel B of Figure A1 demonstrates that 5-7% of mortgage market complaint narratives are related to discrimination each year. This stable proportion highlights the persistent issue of discrimination. Given the smaller fraction of minority consumers in the lending market,[6] the 5-7% representation is particularly significant. This emphasizes the severity of discrimination problems.

Table A1 presents a consumer complaint example concerning "Applying for a mortgage or refinancing an existing mortgage," from CFPB's public dissemination. The components include "Date received", "Product", "Consumer complaint narrative", "Company", and "Company response to consumer", and others. Although personal data are de-identified in complaint narratives, claims of racial discrimination are evident, especially through statements like "I believe that I am being discriminated against because I disclosed my race as XXXX".[7] The company's reply to this particular situation is categorized as "closed with monetary relief," and the company does not dispute the complaint. These indicators suggest potential misconduct by the lender towards this consumer. The aggrieved party received compensation for the unfair treatment with the presence of the CFPB's complaint system.

2.2 Literature Review and Contribution

My research examines how the public disclosure of consumer complaint narratives affects racial disparities in the mortgage market. This study contributes to the literature in three ways: (1) it extends research on disclosure policies in finance and accounting by focusing on consumer-generated information disclosure; (2) it advances our understanding of racial inequalities in mortgage lending by providing new methods to reduce racial gaps; (3) it complements the literature on CFPB regulatory effectiveness by evaluating the bureau's role

[6] In the HMDA dataset spanning from 2011 to 2019, used in this study, loan applications from minority consumers make up 12.24% of total applications. However, this proportion diminishes to 11.55% when considering loans that receive approval.

[7] The race information is erased by the CFPB to protect consumer privacy.

in promoting fair lending practices.

Prior studies focus primarily on standardized, top-down disclosures issued by firms and regulatory agencies, such as financial reports, mandated filings, or supervisory reports (Bailey et al. (2006); Christensen et al. (2017); Gopalan (2022); Kleymenova and Tomy (2022); Pan et al. (2022)). In contrast, the role of consumer-generated information disclosures receives far less scholarly attention. Unlike traditional disclosures, complaint narratives from consumers offer a firsthand perspective, convey unstructured and diverse content, and occur in real-time, revealing aspects of market quality and firm behavior that traditional disclosures often miss. My research is the first to causally examine how the public disclosure of these bottom-up narratives—reflecting the collective insights of consumers—reduces information asymmetry in the mortgage market, one of the largest yet most opaque segments of consumer finance.

Moreover, existing literature lacks adequate analysis of the micro-level mechanisms through which disclosure affects market outcomes. My research addresses this gap by identifying specific channels through which disclosure policies influence discriminatory lending practices, particularly how disclosure creates career concerns that affect loan officers' lending decisions. My analysis of loan officers extends the literature on disclosure's impact on labor markets. While prior studies focus on executives (Tsolmon (2024)) or examine employee motivation under relative performance disclosure within specific banks (Casas-Arce et al. (2023)), my study investigates how disclosure affects career outcomes across the entire mortgage industry using data on all licensed loan officers.

This study provides a novel regulatory perspective on addressing racial inequalities in lending markets. Unlike prior research (Li (2023)) that examines how to reduce racial discrimination primarily through market competition channels, my research adopts a regulatory policy approach, showing that information disclosure serves as an effective tool to mitigate discrimination. This finding expands both the theoretical framework and practical methods for addressing racial disparities in financial services, while also connecting to research on disclosure policies in labor markets. For example, Bennedsen et al. (2022) examines a 2006 Danish law that requires firms to disclose gender-specific wage data, documenting effects on gender wage gaps and firm performance. My research demonstrates that disclosure, previously established in labor market contexts, can be effectively applied as a regulatory tool to address racial inequalities in lending markets, broadening our understanding of how transparency mechanisms can combat different forms of discrimination across diverse market settings.

This study also contributes to the literature on CFPB regulatory effectiveness. Prior research presents mixed views on CFPB impact: Fuster et al. (2021) and DeFusco et al. (2020) document positive effects in reducing risky lending practices, while critics argue it increases compliance costs and reduces credit supply (Neugebauer and Williams (2015); U.S.

Chamber of Commerce (2018)). My research adopts a different approach by examining the CFPB's role in reducing racial discrimination through its complaint disclosure policy. While Dou and Roh (2023) and Dou et al. (2023) analyze the effects of disclosing complaint counts on mortgage applications and approvals, I focus specifically on complaint narratives—textual descriptions that provide richer information—and their impact on mitigating discriminatory lending practices in the mortgage market.

3 Data Description and Summary Statistics

3.1 Data Description

1.HMDA-GSE Merged Dataset. The main dataset employed in this study is the integrated HMDA-GSE dataset. The Home Mortgage Disclosure Act (HMDA) compliance surveys are the sole data source of loan-level information regarding the applicant's race and ethnicity. This dataset includes 90% of mortgage originations in the U.S. as reported by Engel and McCoy (2016). The HMDA data contain essential details such as the applicant's income, race, ethnicity, loan amount, lender name, and the census tract of the property. I use the Government-Sponsored Enterprises (GSE) dataset to complement the above dataset. It offers detailed loan information such as interest rate, default, LTV, credit score, loan product, loan purpose, and loan term. These factors are crucial for understanding the personal characteristics of borrowers.

Since there is no direct connection between these two datasets, I employ the "fuzzy data matching" methods in accordance with Law and Mislang (2022) to merge them. The detailed process is explained in Appendix B. This combined dataset comprises all approved loans securitized by the GSEs from 2011 to 2019.[8] Additionally, the handling of loans with diverse terms differs within the real market. Therefore, I apply filtering to this dataset following Bartlett et al. (2022). This filtering is based on variables like credit scores, LTV, and loan amount. Detailed specifics are provided in Appendix B. The outcome is a pooled cross-sectional dataset at the loan-lender-year level. I use this dataset to examine whether disclosing consumer complaint narratives affects interest rates and default rates among minority borrowers.

2.HMDA Origination Dataset. Another important dataset used in this study is the HMDA origination dataset. Unlike the HMDA-GSE merged dataset, the original HMDA dataset includes unapproved loans. This allows me to assess the tangible effects of disclosing

[8]Given the profound impact of the COVID-19 pandemic, which has ravaged the globe, including the United States, since 2020, my research excludes samples after 2019 to mitigate the interference from this shock on the financial markets.

consumer complaint narratives on the rejection rates of minority borrowers. However, the HMDA dataset lacks enough loan attributes. So I adopt a strategy from Bartlett et al. (2022) to mitigate potential bias arising from omitted variables. As the borrower's credit score or LTV cannot be observed directly, I employ a substitution approach in my analysis. I use the HMDA-GSE merged dataset to calculate the median credit score and LTV at the census tract level. A comprehensive explanation of this process is in Appendix B.

3.CFPB Dataset. I apply textual analysis methods to CFPB's consumer complaint database to identify complaints related to racial discrimination following Li (2023). Specifically, I classify any use of the terms "discrimination," "unfair," "partial," "inequity," "prejudice," "injustice," or similar expressions as indicators of discrimination. If the narrative includes any of these terms, I consider the complaint to be related to racial discrimination. Further, I pair the annual number of discrimination-related complaints with both the HMDA-GSE merged dataset and the HMDA origination dataset. This merging process is based on ZIP codes and lender names from the CFPB's database.[9] In the merging process, most complaints are associated with a singular county. If a ZIP code encompasses multiple counties, following Dou and Roh (2023), I match it to the county with the highest population.

4.NMLS Dataset. In 2008, the Secure and Fair Enforcement for Mortgage Licensing Act of 2008 (SAFE Act) was enacted to protect consumers and mitigate mortgage market fraud by establishing minimum standards for licensing and registering mortgage loan originators. This legislation requires federal registration for loan officers employed by federally insured depositories or their subsidiaries, as well as those working for organizations overseen by the Farm Credit Administration. All other loan officers must obtain licenses from their respective state authorities. Moreover, the Act mandates that all pertinent information be recorded in the Nationwide Multistate Licensing System (NMLS), which was established as a comprehensive national platform for licensing and registration in response to the SAFE Act's requirements.

I obtain the files concerning branches, companies, licenses and registrations of loan officers, and other related files from the NMLS Consumer Access. Each loan officer is assigned a unique NMLS ID by the NMLS, which remains unchanged throughout their career in the mortgage industry even if they change employers. Thus, I can utilize this unique identifier to track the professional path of a loan officer within the mortgage market.

Specifically, I focus on the data regarding employment histories and branch manager information. NMLS mandates that state-licensed institutions report not only the names and NMLS IDs of their loan officers but also details regarding their branch managers. This data

[9]Due to privacy concerns, the narratives of complaints before June 25, 2015, are not made public. Therefore, the discrimination-related complaints identified in this study pertain to the period after the implementation of the disclosure policy.

allows me to track the career trajectories of individual loan officers, including their current employers, branch locations, turnover occurrences in specific years, and advancements to branch managers. Given that all loan officers must be registered or licensed, with their records maintained in the NMLS, I utilize this comprehensive dataset to examine how complaint narrative disclosure affects the career paths of loan officers across the entire industry.

5.Additional Datasets. My research also incorporates several supplementary datasets and indicators from varied sources to identify the impacts of disclosure.

(i) I obtain stock market returns for publicly listed lenders from Wharton Research Data Services (WRDS). This dataset spans the period around June 25, 2015, covering both before and after the implementation of the disclosure policy. It includes market-adjusted cumulative abnormal returns (CAR) and CAR based on the CAPM. WRDS also offers financial performance data for these lenders for the fourth quarter of 2014. I use financial performance as control variables in analyzing market responses.

(ii) I use the Call Reports dataset to get the total assets of each bank in the primary dataset for the first quarter of 2015. This indicator identifies banks influenced by disclosure. Additionally, I consider banks with total assets ranging from 0 to 10 billion as the control group and banks in the 10-100 billion range as the treated group.[10] This filter facilitates the estimation of the causal effects of disclosure by ensuring comparability among banks of comparable size. The results are robust to different cutoffs.

(iii) I employ variables such as branch-level deposits from the Summary of Deposits (SOD) dataset to supplement the HMDA origination dataset. I use this dataset to build control variables in regressions analyzing consumer applications and rival banks' responses.

(iv) The American Community Survey (ACS) provides household-level characteristics aggregated at the county level. I select the homeownership-related variables from 2011 to 2019. Homeownership is measured as the percentage of housing units occupied by owners, considering various races. I employ this measurement to examine the real effect of disclosure policy on the racial gaps in homeownership.

3.2 Summary Statistics

Table 1 provides detailed summary statistics for the treated and control institutions from the primary HMDA-GSE merged dataset. This dataset ranges from 2011 to May 2015, before the enactment of the CFPB's disclosure policy. The treated group includes banks with

[10]The reasons for dropping big banks are as follows: following the implementation of the Dodd-Frank Act, bank holding companies (BHCs) with assets exceeding $100 billion have become the target of heightened supervision and are subjected to regulatory stress tests under the Comprehensive Capital Analysis and Review (CCAR) framework in 2011. Furthermore, larger banks, compared to smaller banks, may have distinct capital structures or exhibit economies of scale.

total assets ranging from 10 to 100 billion dollars and all nondepository mortgage originators, while control institutions include banks with assets below 10 billion. I use this dataset to assess the influence of disclosing consumer complaint narratives on both loan interest rates and default rates.

In treated banks, 7% of loans come from minority consumers, while in the control group, it is 8%. Minority consumers here refer to Black or Hispanic individuals.[11] In terms of interest rates, treated banks exhibit an average rate of 4.11%, slightly lower than the 4.22% in control banks. A comparable pattern is evident in default rates. The mean values for LTV ratios and credit scores are also similar between treated and control banks. Loan borrowers in the treated group have an average annual income of $107,000. In the control group, the average income is $100,000. Additionally, the average loan amount for these groups is $239,000 and $247,000, respectively. This table presents similar characteristics across critical variables for the treated and control groups. This similarity in risk attributes before the shock indicates it is reasonable to categorize treated and control groups in this study.

[Insert Table 1 about here]

4 CFPB Disclosure and Racial Gaps in Interest Rates

4.1 Research Design

This paper examines how lenders respond to the disclosure of complaint narratives during loan application processes. I account for three critical dimensions: racial disparities in interest rates, default rates, and loan rejections. In this section, I specifically focus on the influence of disclosure on racial differences in interest rates for two primary reasons. Firstly, loan interest rates are important for borrowers. Racial disparities in this area cost minority borrowers up to $450 million annually, as indicated by Bartlett et al. (2022). The presence of this unfair treatment also draws significant attention from regulatory bodies.[12] Secondly, the distinct loan pricing strategy in defaulted guaranteed GSE loans can effectively capture discriminatory practices within lenders.

According to U.S. fair-lending legislation, judicial rulings have affirmed that lenders

[11] As for other variables utilized in this study, their definitions can be found in Table A2. Table A3 shows the summary statistics from 2011 to 2019. Particularly, the final three variables featured in Table 1: Cash-out Refinance, Purchase, and Refinance, represent three distinct categories of loan purpose.

[12] Senators Elizabeth Warren and Doug Jones express concerns to financial regulators about persistent lender discrimination. This assertion references the recent study by Bartlett et al. (2022), which identifies disparities in interest rates between minority and white borrowers. Their communication can be accessed using this link: `https://www.warren.senate.gov/download/2019610-letter-to-regulators-on-fintech-final`.

are allowed to consider specific borrower characteristics when determining loan prices.[13] This remains legitimate even if these proxy variables result in less favorable outcomes for minorities as long as the lender can demonstrate that these variables serve a legitimate business necessity. Courts have explicitly determined that when a lending practice leads to a disparate impact, the responsibility falls on the defendant-lender. Lenders need to prove that any policy, procedure, or practice is associated with the applicant's creditworthiness.[14] In other words, variations in creditworthiness can justify differing lending outcomes. However, it is unjustifiable to charge higher rates to applicants in financially underserved areas or those with limited shopping tendencies with the aim of increasing profits.

I adopt a setting following Bartlett et al. (2022) to identify discrimination and address concerns about omitted variables. Using this method, all *legitimate business necessity* variables are observable. Within this context, the GSEs establish credit-risk pricing adjustments through a fee structure that relies solely on the borrower's position within an 8×9 matrix of LTV ratios and credit scores, known as loan-level price adjustments (LLPAs). Lenders are insured against credit risks by paying these LLPA fees. Figure A2 presents a typical Fannie Mae LLPA grid for single-family loans with a 360-month term. Freddie Mac, another primary entity, also employs a similar pricing model in the GSE loan sphere. Consequently, this can break down a borrower's interest rate into three parts. These three parts are the base mortgage rate, credit risk (determined by the borrower's LTV and credit score), and a residual that reflects strategic lender pricing. My study centers on the racial disparities that emerge in this strategic lender pricing to highlight potential disparities stemming from lenders.

I use the HMDA-GSE merged dataset to empirically analyze how the difference in interest rates between minority borrowers and white borrowers changes after the CFPB disclosures complaint narratives. The baseline regression model is presented as follows:

[13] U.S. fair-lending law encompasses both the Fair Housing Act and the Equal Credit Opportunity Act (ECOA), along with all related regulatory implementations and judicial interpretations about these acts.

[14] For more details, refer to A.B. & S. Auto Service, Inc. v. South Shore Bank of Chicago, 962 F. Supp. 1056 (N.D. Ill. 1997) which states: "[In a disparate impact claim under the ECOA], once the plaintiff has made the prima facie case, the defendant-lender must demonstrate that any policy, procedure, or practice has a manifest relationship to the creditworthiness of the applicant...". Also, see Lewis v. ACB Business Services, Inc., 135 F.3d 389, 406 (6th Cir. 1998) and Miller v. Countrywide Bank, NA, 571 F.Supp.2d 251, 258 (D. Mass 2008) for more insights.

$$
\begin{aligned}
&InterestRate_{ilt} \\
&\quad = \alpha + \beta_1 Treat_l \times Post_t \times Minority_i + \beta_2 Post_t \times Minority_i \\
&\quad + \beta_3 Treat_l \times Minority_i + \beta_4 Minority_i \\
&\quad + \mu_{Bucket \times LoanPurpose \times YearMonth} + \mu_{Lender \times YearMonth} \\
&\quad + \mu_{BorrowerCharacteristics} + \epsilon_{ilt}
\end{aligned} \tag{1}
$$

My primary empirical framework employs a triple-differences approach at the mortgage loan level. I investigate how mortgage interest rates change among different racial groups after the disclosure. In Equation (1), i, l, and t index loan applicants, lenders, and time (year-month), respectively. $Treat_l$ categorizes lenders into treated and control groups based on whether lender l was under CFPB supervision in 2015 (i.e., covered by the disclosure policy). For banks, this means those with total assets over $10 billion. In addition, all nondepository mortgage originators are included in the treated group. The regression sample only includes banks with total assets of less than $100 billion as of the first quarter of 2015 and all nondepository mortgage originators. This mitigates confounding factors arising from variations in regulatory pressure, capital structures, or economies of scale between large and small banks. $Post_t$ equals one if it is June 2015 or later, marking the effective initiation of the CFPB disclosure policy. $Minority_i$ equals one if the loan applicant i is Black or Latinx. Accordingly, the coefficient β_1 in the triple-difference estimation signifies the impact of the disclosure on racial gaps in interest rates. It estimates the difference in changes in racial gaps between treated lenders and control lenders whose complaint narratives are not disclosed to the public.

As stated earlier, the pricing grid is critical in mitigating the influence of credit risk in my identification. Nonetheless, the interest rates within the pricing grid may change with time or loan purposes. Therefore, in Equation (1), I integrate dummy variables corresponding to the 8x9 grid levels (referred to as "bucket" in the equation). These variables interact with the loan's year, month, and cash-out refinance status in my regression model. Additionally, my regression model considers lender interactions with the year and month. These fixed effects control for differences in lender pricing and temporal variations. Furthermore, I incorporate fixed effects related to various loan borrower characteristics. These include the loan amount deciles, applicant income deciles, applicant gender, and whether the applicant has a co-applicant. This comprehensive inclusion further controls for potential factors that may impact loan pricing. Standard errors are double clustered at the lender and year levels.[15]

[15]The findings hold steady when I cluster standard errors at the lender level, as shown in Table A4 in the Appendix.

4.2 Baseline Results

Table 2 presents the outcomes of my baseline model. Column (1) includes only the first two fixed effects in Equation (1). In column (2), I incorporate fixed effects for borrower characteristics. Lenders may have different pricing strategies (i.e., different markups) when using the pricing grid. So I interact the lender fixed effects with the bucket-purpose-time fixed effects in column (3). Furthermore, I construct a similar borrower identifier to approximate individual fixed effects.[16] I control for these fixed effects in column (4). This allows a direct comparison regarding the impacts of disclosure on borrowers with similar creditworthiness but different races. In the final column, I include joint lender-similar borrower fixed effects. Controlling for these joint fixed effects allows for a comprehensive analysis of the behaviors of nearly identical borrowers within the same lender before and after the implementation of the disclosure policy. It ensures that my estimation results are unaffected by the lender-consumer relationship.

[Insert Table 2 about here]

Table 2 displays the estimated coefficients on the *Minority* variable. These coefficients reveal that minority borrowers face interest rates that are 2.5 to 2.9 basis points higher compared to those of white borrowers before the disclosure policy. Since the inception of the Mortgage Bankers' Association Annual Performance Report in 2008, the average net production income has been 55 basis points.[17] Thus, an increase of 2.5 basis points in the interest rate equates to a 1.7 basis points rise in annual principal repayment. This constitutes 3.1% of the bank's annual net profit.[18] However, this discrepancy changes after the implementation of the CFPB disclosure policy. The estimated coefficient on the triple-difference term, $Treat \times Post \times Minority$, indicates that after disclosure, the interest rate applied to minority borrowers is reduced by 2.2 to 2.6 basis points relative to white borrowers. This reduction mitigates approximately 92% of the existing racial disparities. Evidently, the enforcement of the disclosure policy effectively saves minority mortgage borrowers an average of $93 million annually, due to reduced discrimination.[19] The insignificant coefficient

[16]The borrower fixed effect under scrutiny results from interplays among the following categorical variables: pricing grid, loan purpose, loan amount deciles, applicant income deciles, applicant gender, and co-applicant status. Leveraging these attributes allows me to segment loan applicants into extremely granular categories, thus identifying borrowers with remarkably similar traits. Given that borrower characteristic fixed effects are absorbed by similar borrower fixed effects due to collinearity, there is no necessity to control borrower characteristics in columns (4) and (5).

[17]For more details, see: `https://www.mba.org/news-and-research/newsroom/news/2023/04/06/imb-production-profits-falls-to-series-low-in-2022`

[18]Consider a mortgage loan where the home price is $200,000 and the down payment is 20%. With an annual interest rate of 4%, an increase to 4.025% would mean an additional annual repayment of approximately $27.7, representing 1.73 basis points of the principal (80% of $200,000 equals $160,000).

[19]According to a report by the Federal Reserve of New York, the market size of housing debt in 2022

of *Post*×*Minority* indicates that the racial gap in control banks remains unchanged after the CFPB disclosure policy is implemented. Table 2 also shows that the model effectively explains 76% to 82% of the variations in interest rates observed within my sample. The remaining unexplained variance might be attributed to strategic pricing. Further, to validate the applicability of the settings in this study, I use GSE loans from my dataset to replicate the primary results of Bartlett et al. (2022). These results are shown in Table A5 in the Appendix. I employ the same setting in Table 3 of Bartlett et al. (2022) to estimate the differences in purchase and refinance loan interest rates for minority borrowers. Remarkably, the sign and magnitude of the coefficients obtained are similar to the estimates of Bartlett et al. (2022), even though I use different matching methods to get the final dataset. This consistency indicates that my results are free of sample selection bias in the matching process.

Table A6 explores heterogeneity in the disclosure policy's effects on interest rates across different minority shares and credit score groups. This heterogeneity analysis offers insights into the policy's differential impacts on various segments of the mortgage market. Panel A shows that the coefficient for lenders with low minority borrower shares is -0.018 (significant at 1%) in column (1), while for those with high minority borrower shares, it is -0.078 (significant at 5%) in column (2). Thus, the mitigation of racial disparities is more pronounced for lenders with high minority shares, suggesting the disclosure policy has a stronger effect on lenders serving more minority borrowers. Panel B reveals the impact is strongest for borrowers with lower credit scores, with the largest coefficient (-0.148, significant at 1%) for the lowest credit score group in column (1). The effect diminishes as credit scores increase, from column (1) to column (7). These findings suggest the disclosure policy is particularly effective in reducing racial disparities among lenders serving more minority borrowers and for more vulnerable borrowers in the mortgage market.

Table A7 presents robustness tests using various fixed effects specifications, extending the analysis from Table 2. The table comprises three panels with progressively more localized fixed effects: Panel A adds state-year fixed effects, Panel B ZIP code-year fixed effects, and Panel C census tract-year joint fixed effects. This structure allows for control of increasingly localized time-varying factors that may affect interest rates. The coefficient on the key triple interaction term ($Treat \times Post \times Minority$) increases and remains significant as I introduce

is approximately $12.26 trillion. The Survey of Consumer Finances highlights that African-American and Latinx borrowers account for about 13.3% of the mortgage market. Data from the HMDA indicate that the loan amount processed by lenders under the CFPB regulation constitutes 35.75% of the total loan volume post-2015. Back-of-the-envelope calculations suggest that if the interest rate charged to minority borrowers falls by an average of 2.3 basis points following the disclosure, it will result in an annual decrease in their repayment by an amount equivalent to 1.6 basis points of the principal. Consequently, the minority borrowers within the lenders regulated by the CFPB in the total housing debt would benefit, resulting in an annual saving of approximately $93 million.

more refined region-time fixed effects across panels. In Panel A, the coefficient ranges from -0.021 to -0.027, from -0.030 to -0.040 in Panel B, and from -0.030 to -0.042 in Panel C with the most stringent fixed effects. This pattern indicates that more granular controls reduce omitted variable bias, allowing for a more precise estimation of the treatment effect. The stability of results across specifications suggests that unobserved time-varying factors at various geographic levels do not drive my findings, supporting a causal interpretation of the disclosure policy's impact. Table A7 thus reinforces my main conclusion about the policy's effectiveness in reducing racial disparities in mortgage lending.

One concern is that my specification imposes a cap on total assets under $100 billion for lenders, which may limit the sample and lead to selection bias. To mitigate this, I adjust the cap thresholds based on the setting shown in column (2) of Table 2 and illustrate the estimated triple interaction terms at different caps in Figure A3.

In Figure A3, I set a new cap every $5 billion, from $25 billion to $95 billion, and estimate at each level. I start with an initial cap of $25 billion, following Fuster et al. (2021). The significance of the coefficients in Figure A3 demonstrates the robustness of my results.

Another concern could be the potential differences in discount points chosen by minority and white borrowers.[20] This discrepancy may explain the considerably higher interest rates paid by minority borrowers compared to their non-minority counterparts in Federal Housing Administration (FHA) loans, as suggested by Bhutta and Hizmo (2021). However, this conclusion remains a subject of debate. Bartlett et al. (2022) identify continued discriminatory pricing within FHA loans even after accounting for discount points using a larger sample. In my study, I focus on GSE loans to address this debate, as both Bartlett et al. (2022) and Zhang and Willen (2021) identify instances of discriminatory pricing within the GSE mortgage market. Additionally, I demonstrate the robustness of my results by examining whether significant differences exist in racial gaps in discount points between the treated and control groups. Given that the HMDA only provides information about discount points after 2018, I utilize the HMDA-GSE merged dataset for 2018-2019 to examine disparities in discount points within the same framework as Table 2 (adding the interest rate as a control variable). The findings are in Table A8 in the Appendix. I do not find significant differences in racial gaps in discount points between treated and control lenders. This outcome indicates that even if racial gaps in discount rates may explain the observed racial gaps in interest rates, our treatment-control comparison within the difference-in-differences framework effectively

[20]In the mortgage market, borrowers have the option to pay discount points - an upfront lump sum to the lender in exchange for a reduced loan interest rate. Conversely, they can opt for negative points, receiving credits from the lender while agreeing to pay a higher loan interest rate. Hence, the observed racial differences in interest rates could result from endogenous discount point decisions if there is a difference in the preferences for discount points between minority and non-minority borrowers.

differences out such effects.

Finally, I adopt an event study approach to examine the dynamic impacts of disclosure. An important concern about the triple difference analysis is whether the racial gaps in both treated and control groups follow a parallel trend before the CFPB disclosure.

Figure 1 displays the quarterly coefficient estimates for racial gaps in interest rates within treated lenders compared to control lenders. I examine results from five quarters preceding the implementation of the disclosure policy (effective in the second quarter of 2015) and ten quarters post-disclosure. Any sample periods beyond this selected range are incorporated into the beginning periods (before 2014) and end periods (after 2017). The baseline periods encompass the fifth quarter before the disclosure's start date and prior periods.

[Insert Figure 1 about here]

From Figure 1, I observe no substantial difference in racial gaps in interest rates between the treated and control groups before implementing the disclosure policy. However, there is a sudden decrease in the racial gaps in interest rates for treated lenders compared to control lenders from the first quarter after the disclosure. This trend confirms that the racial gaps in interest rates in treated and control groups followed parallel trends before the policy's implementation. The treatment effects are only apparent after the enactment of the disclosure policy and last for an extended period.

Another concern is whether a different regulation coincides with the enforcement of the disclosure policy and could have concurrently influenced the racial disparities in interest rates. To alleviate this concern, I thoroughly examine changes in the CFPB's regulatory policies. I find no other concurrent regulations in 2015 that could plausibly influence my results.

4.3 Moderation Effects

In this section, I analyze the heterogeneous effects of the CFPB disclosure policy. I focus on how lenders respond to the disclosure of complaint narratives differently across various scenarios. These scenarios include: (i) the complaints against lenders involving discriminatory treatments, (ii) lenders located in areas with considerable Google search attention towards the CFPB, and (iii) increased risk of discrimination litigation against lenders.

In my initial series of examinations, I focus on discrimination complaints. Through textual analysis, I divide the treated group into two subgroups based on whether they received complaints about racial discrimination in 2015.[21] One subgroup consists of lenders under CFPB supervision with discrimination complaints. The other includes supervised lenders

[21]For the method of identifying whether complaints contain discrimination, please refer to Li (2023). Within the HMDA-GSE merged dataset utilized for the analysis in this section, 32.61% of treated lenders received complaints pertaining to discrimination in 2015.

without such complaints.

I conduct separate regressions for both groups following the setting of Equation (1). The results are presented in the first two columns in Table 3. Column (1) of Table 3 reports the changes in racial gaps after disclosure between treated lenders with discrimination complaints and control lenders. It shows a significantly negative coefficient on *Treat*×*Post*×*Minority*. In contrast, column (2) represents the outcomes for treated lenders without discrimination complaints. It reveals a negative but statistically insignificant coefficient regarding the triple interaction term. These findings underscore that disclosure more noticeably mitigates racial gaps among lenders already engaged in discriminatory practices.

In the second test, I examine the moderation effect of social attention on lenders' decision-making. This task is difficult due to the limited data on public interest in the disclosure. To address this challenge, I calculate the state-level variations in the Google Search Index for the term "CFPB" during the 12 months preceding the disclosure date. I assign a *High* indicator with a value of one for observations in states with values above the median, and a value of zero otherwise, following Dou et al. (2023).[22] Using this indicator, I divide the treated group into two subgroups according to the Google Search Index levels in the states where the lenders are located.

I use the same setting as the analysis of discrimination complaints. The findings are presented in columns (3) and (4) in Table 3. The outcome in column (3) is statistically significant, but the results in column (4) are not significant. These results imply that internet searches conducted by social groups, serving as an indicator of social attention, contribute to mitigating racial disparities in the mortgage market. Social awareness may prompt lenders to prioritize their reputation and curtail misconduct.[23]

[Insert Table 3 about here]

Finally, I explore whether disclosure has a greater impact on reducing racial disparities during periods with more litigation cases associated with racial discrimination. From 2011 to 2019, there were eight litigations concerning discrimination. The involved lenders incurred various penalties. Some were mandated to invest in subsidy programs aimed at assisting minority borrowers.[24] To evaluate the efficacy of these potential litigation risks in mitigating racial disparities, I modify Equation (1) by introducing a moderating variable, *Litigation*.

[22]Google monitors and compiles users' search volume by keywords for each state, then calculates the search volume index as the proportion of searches from a given state to those from the state with the highest search volume.

[23]Darian Dorsey, Chief of Staff of the CFPB, provides anecdotes about lenders linking executive bonuses to their responsiveness in handling complaints. This anecdotal evidence suggests that executives are incentivized to address complaints, as a portion of their bonuses is tied to the efficacy of the lenders' complaint resolution (Cortez (2015)).

[24]The data regarding these litigations are gathered from the CFPB website. For detailed information, see Table A10 in the Appendix.

It captures litigation intensity over time in terms of litigation number and penalty amount. These variables quantify the monthly occurrences of litigations and their corresponding penalties. While identifying this effect, including lenders involved in litigation within the sample may cause endogeneity bias. This is because lenders subject to legal penalties would have a strong motivation to minimize discriminatory actions. Hence, by observing the operation of their peers, I can assess the reactions of these peer lenders to potential litigation threats arising from disclosure.

After excluding samples involving litigated lenders, I proceed to estimate this moderation effect. The outcomes are detailed in Table A9. The first two columns use *Litigation* to present the total number of CFPB enforcements. The last two use *Litigation* to denote the penalty amount charged to lenders. Columns (2) and (4) take advantage of variations within the individual level using similar borrower fixed effects. These are consistent with the specification used in column (4) in Table 2. Column (1) indicates that when the total number of litigations increases by one in a given month, the additional interest for minority borrowers drops by six basis points in the post-disclosure period. Column (3) states that when the penalty of litigations increases by one million dollars in a month, minority borrowers experience a decrease of 1.2 basis points in the additional interest in treated lenders after disclosure. Both outcomes are statistically significant at the 1% level. These findings strongly suggest that the CFPB's action imposes litigation threats on lenders that the CFPB does not sue. Lenders are aware of this threat and might restrain illegal behavior to reduce their exposure to litigation risk.

5 CFPB Disclosure and Outcome Tests: Evidence from Default Rates

5.1 Research Design

In this section, I focus on the default rate of loans. Default actions depend on many factors beyond the sole influence of lenders. Thus, I employ the "outcome test" approach to identify racial discrimination and the corresponding impact of disclosure policy on it (Becker, 1957, 1993). Outcome tests assess whether loans provided to marginal minority borrowers yield higher profits compared to those to marginal white borrowers. If marginal minority borrowers indeed generate higher profits, it signifies that lenders establish a higher screening threshold for minority applicants due to racial prejudices. The profit that lenders earn from consumers depends on two main factors. One is the interest rate that directly affects the gains of lenders. The other is the default rate, which leads to losses for lenders. In prior analyses,

I demonstrate that minority borrowers face higher interest rates. This section primarily examines whether racial gaps also persist in default rates and whether the disclosure policy plays a mitigating role in this context. Detecting a substantial discrepancy in default rates between marginally qualified minority and white borrowers is critical. Such a difference would suggest that omitted variables or statistical discrimination are not the main cause but rather racial biases within lenders during the consumer selection procedure.[25]

However, conducting outcome tests accurately has at least two challenges (Butler et al. (2023)). Firstly, researchers cannot directly observe the literal marginal borrower. This is a situation known as the "infra-marginality problem" (Ayres (2002)). The definition of the marginal borrower is neither standardized nor publicly available across lenders. Thus, researchers can only identify them by controlling for covariates. However, inaccuracies can arise if these covariates fail to completely eliminate the creditworthiness disparity between minority borrowers and whites. If so, the difference in the default rates might not exclusively result from racial bias. In addition, if the distribution of creditworthiness for minorities remains lower than that for whites even after controlling for covariates, marginal minority borrowers might exhibit higher default rates than whites even without racial bias. As a result, I can observe lower default rates among marginal minority borrowers only when significant racial bias is present and capable of counterbalancing these confounding factors.

A second concern is that researchers cannot directly observe loan profitability. When focusing on the default rate, potential misestimations of the disparity between minority and white borrowers may occur. This can happen when other factors that influence lenders' profitability assessments are overlooked. For example, the prepayment risk might be higher for white borrowers. Lenders could decrease their evaluation of the profitability of white borrowers. In such cases, lenders might not require minority borrowers to maintain significantly lower default rates to generate profits higher than those from white borrowers. This makes it more difficult to observe lower default rates among minorities.

Conducting outcome tests by comparing default rates among marginal minority and white borrowers might not be perfect. However, the preceding analysis underscores the

[25]Discrimination encountered by minority borrowers in the lending market may arise from two potential sources: lenders' racial biases or statistical discrimination. Racial bias encompasses both taste-based discrimination and miscalibrated beliefs (Arnold et al. (2018); Becker (1957); Bordalo et al. (2016); Egan et al. (2022); Ewens and Townsend (2020)). Taste-based discrimination implies lenders' dissatisfaction with approving applications from minority borrowers or contentment with approving those from white borrowers. Miscalibrated beliefs involve lenders harboring incorrect stereotypes about minority borrowers. Since these biases are typically implicit and can contribute to discrimination, I combine them into a single group. An alternative explanation pertains to the concept of statistical discrimination (Arrow (1974); Ewens et al. (2014); Phelps (1972)). In the context of this research, statistical discrimination manifests when lenders treat minority and non-minority borrowers differently due to inherently higher risks associated with minority borrowers. As a result, from a profit-maximization perspective in business, it is plausible that lenders might reject a larger proportion of applications from minority borrowers.

conservative nature of this approach. The outcome test can only detect a significantly lower default rate among marginal minority borrowers compared to marginal white borrowers when substantial racial bias exists in lenders' consumer selection. Hence, any evidence of lower default rates for minorities should be regarded as compelling proof of discrimination. It indicates that lenders use stricter approval standards for minority applicants.

The outcome test method has proven effective in the auto loan market (Butler et al. (2023)). Following Butler et al. (2023), my research investigates racial disparities in the mortgage market. I examine the impact of the disclosure policy on racial gaps in default rates, particularly among marginal borrowers. If disparities in default rates between racial groups decrease after disclosure, specifically if marginal minority borrowers have higher default rates, it strongly suggests that disclosing complaint narratives encourages lenders to mitigate their stringent criteria. It leads to a more equitable screening standard for both minority and white borrowers' loan applications.

5.2 Outcome Test Results

I define default rates following Butler et al. (2023). A loan is considered defaulted when the borrower becomes delinquent for 90 or more days. I restrict the sample to individuals with credit scores below 660 to address the issue of identifying marginal borrowers. This approach significantly reduces the number of observations.[26] Due to the distinct factors influencing defaults and interest rates, the fixed effects designs used here are slightly different from those in Section 4. Equation (2) shows the identification strategy for the "outcome test" approach:

$$\begin{aligned} DefaultRate_{ilt} \\ &= \alpha + \beta_1 Treat_l \times Post_t \times Minority_i + \beta_2 Post_t \times Minority_i \\ &+ \beta_3 Treat_l \times Minority_i + \beta_4 Minority_i + \gamma X_{ilt} \\ &+ \mu_{Lender \times YearMonth} + \mu_{ZIP \times Year} + \epsilon_{ilt} \end{aligned} \tag{2}$$

In the equation, the symbols i, l, and t still represent loan applicants, lenders, and time (year-month), respectively. Table 4 outlines tests using a triple-difference approach to identify the impact of disclosure on racial disparities in default rates. This approach considers loan characteristics such as interest rate, purpose, and the logarithm of the loan amount. Applicant characteristics such as gender, the logarithm of income, and the presence of a co-applicant are represented by X_{ilt} in Equation (2). In addition, I incorporate ZIP-by-year fixed effects to capture potential influences on loan default rates stemming from changes in

[26]The handling of the sample here aligns with Section 4, where observations from large banks (those with total assets exceeding $100 billion) are excluded.

local economic conditions. I include pricing grid-loan purpose fixed effects in column (3) to better account for borrower creditworthiness.

Columns (1)-(3) present the discriminatory treatment experienced by minority borrowers in the subprime sample and the impact of the disclosure policy on mitigating this inequitable treatment. The negative and statistically significant results on the *Treat*×*Minority* coefficient confirm the existence of discrimination against minorities in treated lending institutions. The *Treat*×*Post*×*Minority* coefficient in column (2) shows an average increase of 34.2 percentage points (84.2% of one standard deviation) in the default rates of minorities within the treated group compared to the control group after disclosure. This outcome reflects that racial disparities in default rates are almost eradicated after disclosure. It emphasizes the strong impact of the disclosure policy.[27]

[Insert Table 4 about here]

In the last column of Table 4, I do not apply the cutoff set used to identify marginal borrowers. This full sample outcome does not offer sufficient evidence of discrimination against minorities. The full sample includes many borrowers distant from the credit provision margin. This makes the test less sensitive to margin differences. It implies that applying the test to the subprime borrower sample who are closer to the margin is an appropriate identification approach.

One concern is that my specification imposes a cap on total assets under $100 billion for lenders, which may limit the sample and lead to selection bias. To mitigate this, I adjust the cap thresholds based on the setting shown in column (1) of Table 4 and illustrate the estimated triple interaction terms at different caps in Figure A4.

In Figure A4, I set a new cap every $5 billion, from $25 billion to $95 billion, and estimate at each level. I start with an initial cap of $25 billion, following Fuster et al. (2021). The significance of the coefficients in Figure A4 demonstrates the robustness of my results.

6 CFPB Disclosure and Racial Gaps in Rejection Rates

6.1 Overall Trend Analysis

Previous results indicate that the disclosure policy nearly eliminates disparities in default rates among marginal borrowers of different races. These differences may result from lenders applying stricter admission criteria to minority consumers. The implementation of the disclosure policy has the potential to reduce lenders' propensity to maintain such strict and unfair standards. In this section, I focus on loan application denials. I aim to address the

[27] To address potential concerns about the parallel trend assumption, I also adopt an event study methodology to scrutinize the dynamic impacts of disclosure. For more details, refer to Table A11 in the Appendix.

following two questions: (i) whether this biased screening standard exists before the disclosure, and (ii) whether the disclosure policy mitigates these disparities.

Initially, I analyze the historical trend of loan application rejections across the full sample using the HMDA origination dataset. Figure 2 plots the racial disparities in residualized rejection rates, comparing lenders under CFPB supervision with those exempt from such oversight. This analysis focuses on the period surrounding the CFPB's disclosure policy in June 2015.

To mitigate concerns about differences in loan screening criteria between CFPB-supervised and unsupervised lenders, I calculate racial gaps in rejection rates separately for each group. By focusing on within-group differences (i.e., the gap between minority and non-minority borrowers within each lender group), I effectively control for baseline variations in loan approval standards.[28]

In addition, I use residualized rejection rates, which control for observable borrower risk characteristics and fixed effects specified in Equation (3). This residualization process allows me to focus on racial disparities not explained by borrower traits and helps minimize distortions arising from statistical discrimination.[29]

Figure 2 shows that prior to the disclosure policy, racial gaps in rejection rates exhibited parallel trends across CFPB-supervised and unsupervised lenders. Following the disclosure, the decline in excess rejection rates for minority borrowers is notably more pronounced among CFPB-supervised lenders. This observed pattern supports the parallel trends assumption and the identification strategy employed in the subsequent analysis.

[Insert Figure 2 about here]

6.2 Empirical Analysis

In this section, I empirically assess whether the disclosure policy alleviates the discrimination in loan accessibility faced by minorities. This evaluation is based on the HMDA origination dataset. I exclude the influence of large banks determined by total assets. This exclusion is in line with previous analyses. I employ the loan-level rejection dummy

[28] Assume CFPB-supervised lenders exhibit greater risk aversion compared to their unsupervised counterparts. In that case, borrowers would generally face higher rejection rates under CFPB-supervised lenders. Directly comparing rejection rates across lender groups would thus be problematic. However, by computing within-group racial gaps, I mitigate biases arising from different screening criteria.

[29] Statistical discrimination occurs when lenders make rational screening decisions based on observable characteristics rather than racial bias (Phelps (1972); Arrow (1974); Ewens et al. (2014)).

variable as the dependent variable. The model is as follows:

$$\begin{aligned} RejectionRate_{ilt} \\ &= \alpha + \beta_1 Treat_l \times Post_t \times Minority_i + \beta_2 Post_t \times Minority_i \\ &+ \beta_3 Treat_l \times Minority_i + \beta_4 Minority_i + \mu_{Lender \times Year} \\ &+ \mu_{Lender \times Similar\ \ Borrower} + \mu_{Lender \times Census\ \ Tract} \\ &+ \mu_{Census\ \ Tract \times Year} + \epsilon_{ilt} \end{aligned} \tag{3}$$

In Equation (3), the symbols i, l still represent loan applicants and lenders, respectively. The symbol t has now been modified to indicate the year. Table 5 presents the results of these regressions. The first two columns show the baseline outcomes. I adjust for lender-by-year fixed effects, lender-similar borrower joint fixed effects, and lender-census tract joint fixed effects in column (1).[30] I control for lender-similar borrower joint fixed effects to capture changes in the rejection rates for the same borrower-lender pairs before and after disclosure. This fixed effect helps eliminate the influence of confounding factors, such as changes in borrower credit characteristics and lender loan strategies, on my results.[31] I also introduce census tract-by-year fixed effects in column (2). My findings reveal that minority applicants are 3.5 percentage points less likely to get loan approval than white applicants in the control group, according to the coefficient on *Minority*. The coefficient on *Treat*×*Minority* reveals racial gaps in rejection rates increase by 4.1 percentage points in the treated group (column (2)). It indicates that minority applicants in treated lending institutions face a 7.6 percentage point decrease in loan approval likelihood compared to white applicants. A preliminary calculation implies that this equates to more than 70,000 minority applicants in treated lenders failing to obtain loans annually. These loans would have been granted if they were white.[32] The insignificant *Post*×*Minority* coefficient suggests

[30]Declined loans are included in the HMDA origination dataset, leading to a substantial increase in the total sample size. This expansion allows for a more granular fixed effects structure. Moreover, in the mortgage market, loan officers may discourage prospective minority applicants from applying or assist non-minority applicants in improving their likelihood of approval. If such practices occur systematically at the branch level, the lender–census tract fixed effects will capture branch-level discrimination in rejection rates.

[31]The "similar borrower" variable incorporated in this test aligns closely with those used in Section 4. The only difference is that I replace the credit score and LTV group from the HMDA-GSE merged dataset with the median credit score and LTV at the census tract level. This difference is due to the lack of individual-level credit score and LTV data in the HMDA dataset. Thus, the census tract median serves as an approximation. Bartlett et al. (2022) confirm that this approximation does not affect conclusions related to discrimination. For further clarification, please refer to Section 3.1.

[32]I multiply the annual aggregate of minority mortgage loan applications by the change in their probability of approval to calculate these estimates. The approval rate may either decrease due to discrimination or increase owing to the disclosure policy. Data from the HMDA origination dataset indicate that after 2015, the annual average of mortgage loan applications totals approximately 19.16 million. 34.91% of these applications are received by lenders regulated by the CFPB and 13.77% from minority borrowers. Based on these numbers,

that the racial gap in control lenders stays the same after the CFPB disclosure policy takes effect. However, the coefficient on *Treat*×*Post*×*Minority* shows that discrimination against minorities in loan access decreases by 1.7 percentage points after the disclosure policy is implemented in the treated group (column (2)). This implies that annually, over 15,000 minority households successfully obtain mortgage loans due to the disclosure policy.[33]

[Insert Table 5 about here]

I further explore the disclosure impact by investigating whether lenders respond differently to the disclosure of complaint narratives when the complaints received by lenders are related to discriminatory treatments.

Columns (3) to (4) in Table 5 illustrate the moderating effects of discrimination complaints. The *Treat*×*Minority* coefficient is positive and significant in column (3). It indicates that within the treated group, minority borrowers face higher rejection rates in lenders with noticeable discrimination. The coefficient on the same variable in column (4) has a decreased magnitude. It conveys that lenders without discrimination complaints have lower additional rejection rates. These findings further validate my discrimination measurement. The coefficient on *Treat*×*Post*×*Minority* shows the moderating effects of discrimination complaints. The result in column (3) indicates that lenders already engaged in discriminatory practices play a predominant role in driving the observed treatment effect following disclosure. Furthermore, I adopt an event study approach to examine the time-varying influences of disclosure, building upon the specification outlined in column (2). The findings are detailed in Figure A5 in the Appendix.

Table 5 shows that treated lenders are less likely to reject minority borrowers' applications after disclosure. However, there is a concern that the credit status of minorities changes after disclosure, with those having better credit or income becoming more likely to apply, making it harder to reject their applications. To address this alternative hypothesis, I test whether the credit status of minority applicants changes post-disclosure. I replace the dependent variables in columns (1) and (2) with the applicant's income, as demonstrated in Table A12. Table A12 reveals that the coefficient on the triple interaction terms, *Treat*×*Post*×*Minority*, is insignificant. This finding indicates that the disclosure does not cause significant changes in the credit profiles of minority applicants in treated financial institutions compared with controlled financial institutions. Therefore, the increased likelihood of loan approval for

I roughly calculate that there are about 0.92 million (19.16 million × 34.91% × 13.77%) minority mortgage loan applications per year in CFPB-supervised lenders. Therefore, roughly 70.92 thousand (0.92 million × 7.7%) applicants may be denied annually due to racial bias.

[33]Based on the previous framework, this calculation illuminates that the disclosure policy's implementation benefits an estimated 15.64 thousand (0.92 million × 1.7%) minority applicants annually. My estimates in this subsection may be conservative as the HMDA origination dataset does not encompass all applications in the U.S. mortgage market.

minority applicants in treated lenders is not due to better creditworthiness.

Overall, my findings reveal that the disclosure of consumer complaint narratives effectively reduces discrimination against minority borrowers in the mortgage market, leading to a narrowing of racial gaps in interest rates, rejection rates, and default rates. This indicates that CFPB-supervised lenders have enhanced their service quality, especially for minority borrowers. The improvement in service quality is further evidenced by the noticeable downward trend in the total number of mortgage loan complaints following the CFPB's disclosure of consumer complaints, as shown in Figure A6.

Furthermore, I also test how disclosure affects lenders' strategies for dealing with complaints after they occur by using the CFPB Complaint Database. I employ a difference-in-differences approach at the complaint level, leveraging a unique policy implementation timeline. The analysis reveals that after complaint narratives become publicly available,[34] financial institutions pay more attention to complaints accompanied by narratives relative to complaints without narratives. This increased responsiveness is reflected in two key outcomes: First, consumers submitting narrative complaints are more likely to receive monetary relief. Second, their complaints are more likely to receive timely responses from the financial institution. These findings underscore that the disclosure policy acts as a catalyst for lenders to strengthen complaint resolution practices and improve consumer satisfaction. Table A15 presents the detailed results, while Figure A7 illustrates the dynamic effects of the outcomes. Further details can be found in Appendix C.

7 CFPB Disclosure and Loan Officers' Career Paths

Prior analyses demonstrate that complaint narrative disclosure reduces lenders' discriminatory practices against minority borrowers. However, the mechanisms through which disclosure curtails lenders' discriminatory behavior remain unexplored. One potential channel is that disclosure disciplines loan officers' lending decisions by exposing their misconduct. Disclosure may discipline loan officers' behavior by affecting their career paths in two ways. From an internal perspective, the narratives provide detailed insights into consumer dissatisfaction with loan officers in CFPB-supervised lenders, including service quality issues, discriminatory practices, transaction process flaws, misinformation, and fraudulent activities.[35]

[34] CFPB officially implemented the disclosure policy on March 19, 2015. However, a temporal gap existed between the policy's finalization and the public unveiling of complaint narratives. It was not until June 25, 2015, that the narratives associated with authorized complaints were made accessible to the general public. By designating this interval as the pre-shock period, I conduct a comparative analysis of the relative changes in the resolution of complaints accompanied by narratives.

[35] For additional details on the Consumer Response Annual Report, see: `https://files.consumerfinance.gov/f/documents/cfpb_cr-annual-report_2023-03.pdf`

CFPB-supervised lenders might be concerned that loan officers with numerous complaints could adversely affect the institution's reputation, potentially leading to their dismissal. Externally, competing lenders are also likely to be responsive to complaints. They may be reluctant to hire loan officers from CFPB-supervised lenders with complaints, which can limit these officers' career opportunities. Considering both factors, loan officers in lenders with complaints may experience adverse effects on their career paths.

To examine this hypothesis, I construct a panel dataset covering the employment histories of all loan officers based on NMLS data. Using this comprehensive dataset, I employ a DID model at the individual level:

$$\begin{aligned} &Move\ Down_{it} \\ &\quad = \alpha + \beta_1 Treat_l \times Post_t + \beta_2 Treat_l + \beta_3 Post_t + \gamma X_{it} \\ &\quad + \mu_{Individual} + \mu_{Lender} + \mu_{Zip} + \mu_{Year} + \epsilon_{it} \end{aligned} \tag{4}$$

In this model, i, l, t index individual loan officer, lender, and year, respectively. Lenders with complaints publicly disclosed by the CFPB are classified as treated lenders. For loan officers working at these treated lenders, the variable $Treat_l$ is assigned a value of 1, while all others form the control group with a value of 0. The variable $Post_t$ indicates when the CFPB began disclosing complaint information. I assign a value of 1 to the variable $Post_t$ for 2015 and subsequent years, as the CFPB started making complaints public on June 25, 2015.

For the dependent variable $Move\ Down_{it}$, I employ two measures to capture downward movements in loan officers' careers. These measures are based on the hierarchies of lenders, which I define using the size of each lender.[36] The first measure compares the size of the lender an individual joins with their previous employer. Specifically, if the size of the lender an individual joins post-movement is smaller than the previous one, $Move\ Down_{it}$ is set to 1, otherwise 0. The second measure accounts for potential non-linear relationships between lender size and reputation. I identify the ten largest lenders based on their total unique loan officer count across all years in the sample period.[37] When an individual leaves one of these top 10 lenders for a non-top 10 lender, I consider it a downward movement and assign $Move\ Down_{it}$ a value of 1.

Additionally, I include a series of fixed effects to control for unobserved confounding factors. I incorporate individual fixed effects to account for time-invariant personal

[36] I define a lender's hierarchy by its size, determined by the number of loan officers employed annually, as larger firms typically offer better opportunities, possibly due to stronger reputations (Hong and Kubik (2003); Gao et al. (2020)).

[37] For the purpose of identifying the top 10 lenders, I calculate the total number of unique loan officers employed by each lender across all years in the sample. This approach ensures a consistent classification of the top 10 lenders throughout the study period.

characteristics like education, risk tolerance, and other traits. I control for lender fixed effects to remove time-invariant lender-specific characteristics that may influence employee turnover decisions. I also include zip code fixed effects to control for characteristics of the location, such as local economic and political conditions, and year fixed effects to account for time trends. Finally, I include the duration of an individual's career as a control variable. Standard errors are clustered at the lender level.

[Insert Table 6 about here]

Table 6 presents the results from the estimation of Equation (4). Columns (1) and (2) use *Move Down (move to smaller lenders)* as the dependent variable, while columns (3) and (4) employ *Move Down (move to non-top 10 lenders)* as the dependent variable. The results in columns (1) and (3) show that the coefficient of $Treat_l \times Post_t$ is significantly positive. This finding suggests that individuals in lenders affected by the CFPB disclosure policy are more likely to experience downward career movements after the policy's implementation. The economic significance of these effects is substantial. Following the disclosure, loan officers face 12.2% of a standard deviation increase in the likelihood of moving to a smaller lender. For moves from top-10 to non-top-10 lenders, the effect is even more pronounced, representing 31.3% of a standard deviation increase in the probability of such moves.

Additionally, I adopt an event study approach to examine the dynamic impacts of disclosure on loan officers' downward movements. Figure 3 displays the yearly coefficient estimates for *Move Down (smaller)* and *Move Down (top 10 to non-top 10)* for treated lenders compared to control lenders, respectively. I examine results from four years before 2015 - the starting point of the disclosure policy - and continue through six years after its implementation. In the estimation, the year immediately prior to the disclosure serves as the baseline period.

[Insert Figure 3 about here]

In Figure 3, I observe no substantial difference in loan officers' downward movements between the treated and control groups prior to the implementation of the disclosure policy. This observation supports the parallel trends assumption, which is crucial for the validity of the difference-in-differences approach. Following the policy's introduction, Figure 3 reveals a notable increase in instances of individuals' downward movements. This indicates that, after the implementation of the disclosure policy, loan officers in the treatment group are more likely to move to smaller lenders or transition from the top 10 to non-top 10 lenders compared to the control group.

Furthermore, the CFPB consumer complaints database reveals heterogeneity in consumer satisfaction levels across lenders subject to the narrative disclosure requirement, with some having discrimination-related complaint records while others do not. Lenders with disclosed

complaints likely face greater reputational risks, potentially exerting a more pronounced impact on their employees' career trajectories. To test this hypothesis, I merge the CFPB consumer complaints database with the NMLS loan officer panel using the lender name, zip code, and year. This method enables an analysis of complaints associated with branches in specific zip codes. The merged dataset includes both the employment history of individuals and the average service quality of loan officers at each branch. I further divide lenders in the treatment group into two subgroups based on the receipt of discrimination-related complaints from 2015 to 2022. Specifically, a lender that receives any discrimination-related complaints in a given year is classified into the *Treat_Disc_Compl* group for that year. If a lender does not receive any such complaints, it is placed in the *Treat_Non_Disc_Compl* group. Based on this classification, I modify Equation (4) to split the treatment effect into two components, distinguishing between lenders with and without discrimination-related complaints. The modified equation is as follows:

$$\begin{aligned} Move\ Down_{it} \\ &= \alpha + \beta_1 Treat_Disc_Compl_l \times Post_t \\ &+ \beta_2 Treat_Non_Disc_Compl_l \times Post_t \\ &+ \beta_3 Treat_Disc_Compl_l + \beta_4 Treat_Non_Disc_Compl_l \\ &+ \beta_5 Post_t + \gamma X_{it} + \mu_{Individual} + \mu_{Lender} + \mu_{Zip} + \mu_{Year} + \epsilon_{it} \end{aligned} \tag{5}$$

Columns (2) and (4) of Table 6 show the regression results. In column (2), for the *Treat_Disc_Compl* group (as indicated by *1(Treat_Disc_Compl))*, CFPB's disclosure policy still has a significant positive effect on individuals' downward movements, while the coefficient on *1(Treat_Non_Disc_Compl)×Post* is not significant, indicating that individuals without discrimination-related complaints do not experience similar downward movements. In column (4), although both *1(Treat_Disc_Compl)×Post* and *1(Treat_Non_Disc_Compl)×Post* have significantly positive coefficients, the coefficient of *1(Treat_Disc_Compl)×Post* is larger. This finding further suggests that individuals who have received discrimination-related complaints are more prone to experience downward career movements.

Overall, my findings suggest that the CFPB disclosure policy impacts loan officers' career turnover. When consumer complaints are disclosed publicly, loan officers face increased reputational risk, which can negatively affect their career paths by increasing the likelihood of downward movements.

The disclosure of complaint narratives may not only affect the likelihood of loan officers experiencing downward career moves but also their chances of promotion to managerial positions. To examine this latter effect, I employ $Promoted_{it}$ as the dependent variable

in Equations (4) and (5). For each loan officer i, $Promoted_{it}$ is assigned a value of one for year t and subsequent years if he is promoted to a manager in year $t+1$. Thus, the coefficient on $Treat \times Post$ measures the disclosure's effect on promotion. The coefficients on $Treat_Disc_Compl_l \times Post_t$ and $Treat_Non_Disc_Compl_l \times Post_t$ capture the differential effects on promotion prospects for loan officers in branches with and without discrimination-related complaint records, respectively.

[Insert Table 7 about here]

Column (1) in Table 7 shows that the promotion of loan officers to managers in CFPB-supervised lenders decreases after the disclosure of complaint narratives. This reduction corresponds to a 7.1% decline relative to a standard deviation in the likelihood of loan officers being promoted to managerial positions after the disclosure, suggesting an economically significant effect. Furthermore, column (2) in Table 7 indicates that this negative effect is more pronounced for the $1(Treat_Disc_Compl)$ group, and the difference in coefficients between the $1(Treat_Disc_Compl)$ group and the $1(Treat_Non_Disc_Compl)$ group is significant at the 1% level. This is reasonable because compared to the $1(Treat_Non_Disc_Compl)$ group, loan officers at lenders with publicly disclosed discrimination complaints are likely to demonstrate poorer performance, making them less likely to be promoted internally and externally. The negative effect observed in the group without discriminatory complaints is probably because I only focus on the complaints related to discrimination. Lenders under CFPB supervision may receive other types of complaints due to unsatisfactory performance by loan officers, which could also affect their promotion opportunities.

Promotions of loan officers to managerial positions can occur either internally within their current branches or externally at different lenders. To examine the differential impact of disclosure on these two types of career advancement, I focus on external promotions to larger institutions. If the observed decline in promotions is primarily driven by a reduction in external opportunities, it should be more difficult for loan officers to move to larger financial institutions than their current employers.

To test this, I construct a dummy variable, $Ext_Promoted_Bigger_{i,t}$, which equals one if loan officer i is promoted in year t to a lender with more employees than their previous employer. I then re-estimate Equations (4) and (5) using this variable as the dependent variable.

Column (3) of Table 7 shows that disclosure does not significantly affect the likelihood of external promotions to larger institutions among loan officers in supervised branches. However, Column (4) reveals that for loan officers in the $1(Treat_Disc_Compl)$ group, disclosure significantly reduces the probability of being promoted to a larger lender. Furthermore, the difference in coefficients between the $1(Treat_Disc_Compl)$ group and

the 1($Treat_Non_Disc_Compl$) group is statistically significant at the 5% level.

These results suggest that the disclosure of complaint narratives reduces information asymmetry faced by external lenders in the hiring process. Complaint records provide external institutions with additional information that may influence their employment decisions. Consequently, while the overall impact of disclosure on external promotions is insignificant, distinguishing between loan officers with and without complaints reveals a significant negative effect for those with complaints.

To further explore the channels through which disclosure affects career advancement, I examine the effect on internal promotions. Specifically, I replace the dependent variables in Equations (4) and (5) with a dummy variable $Int_Promoted_{i,t}$, which is assigned a value of 1 if loan officer i is promoted within the same branch in year t. Column (5) in Table 7 shows that internal promotions significantly decrease after complaint narrative disclosure. This effect appears to be more pronounced than the impact on promotions to larger external lenders. Column (6) in Table 7 further reveals that this negative effect on internal promotions exists for both the 1($Treat_Disc_Compl$) group and the 1($Treat_Non_Disc_Compl$) group. However, the effect is significantly stronger for loan officers in lenders with discrimination-related complaints. The difference in coefficients between these two groups is statistically significant at the 5% level. This may be explained by the fact that loan officers in lenders receiving discriminatory complaints may exhibit lower service quality, potentially making them less suitable for managerial responsibilities.

8 Reaction of Other Stakeholders

8.1 Consumer's Reaction Towards the Disclosure

Previous analyses primarily focus on lenders' responses to the disclosure. However, the reactions of other stakeholders are equally important. I separately examine the responses of consumers, rival lenders, and stock market investors to the disclosure policy. This section focuses on consumers' reactions to the CFPB's disclosure initiative.

Existing literature provides substantial evidence that consumers incorporate information from product and service reviews into their purchasing decisions. Within the lending market, publicly disclosed complaint narratives may serve a similar role by providing signals about lenders' service quality and fairness. If consumers are able to utilize these narratives, they may actively avoid lenders associated with discriminatory practices and instead choose institutions perceived to offer fairer treatment, particularly when applying for mortgage loans.

I specifically examine how the disclosure affects minority borrowers' choice of lenders,

thereby shedding light on whether complaint narratives can empower consumers to mitigate potential discrimination in the mortgage lending market.

I use data aggregated from the HMDA origination dataset to discuss consumers' reactions. The identification strategy is consistent with the previous sections. The regression model takes the following form:

$$\begin{aligned} &Application_{ilct} \\ &\quad = \alpha + \beta_1 Treat_Disc_Compl_l \times Post_t \times Minority_i \\ &\quad + \beta_2 Treat_Non_Compl_l \times Post_t \times Minority_i + \delta T_{ilt} + \gamma X_{lct} \\ &\quad + \mu_{Lender \times Year} + \mu_{Lender \times County} + \mu_{County \times Year} + \epsilon_{ilct} \end{aligned} \tag{6}$$

Equation (6) uses data aggregated at the county-lender-minority-year level. This means that a lender, l, in a county, c, has two observations, i, representing the total applications from minority and non-minority applicants, respectively. The dependent variable *Application* has two forms in my analysis: (i) the log of the number of mortgage applications from minority or white borrowers aggregated at a specific county-lender-year level, and (ii) the log of the dollar amount of mortgage applications from minority or white borrowers aggregated at a specific county-lender-year level. Importantly, to better capture consumers' perceptions of discriminatory practices, I categorize CFPB-supervised lenders into two groups, *Treat_Disc_Compl* and *Treat_Non_Disc_Compl*.[38] These two treated groups are compared with the lenders in the control group. The coefficients represented by β_1 and β_2 in Equation (6) are of primary interest. They reflect whether consumers pay attention to and benefit from the disclosure of complaint narratives. In T_{ilt}, I control for other triple-difference-related items, such as interaction terms and single terms.

Following Dou and Roh (2023), control variables, X_{lct}, in Equation (6) include: (i) the proportion of mortgages that a lender approves in a specific county, as higher approval rates may lead to more applications; (ii) a dummy variable, assigned a value of one if the lending institution has a physical presence in the county for the given year; and (iii) the logarithm of the total deposits gathered by the bank's branches within the county during that year. Additionally, I control for lender-by-year and county-by-year fixed effects to absorb different time-varying unobservable shocks. I also use lender-county fixed effects as a proxy for branch-level fixed effects.

[Insert Table 8 about here]

[38] In this analysis, I categorize each county-lender into two treated groups based on the receipt of discrimination-related complaints during the years 2015-2019. Specifically, if a county-lender receives any discrimination-related complaints in a given year, it is classified into the *Treat_Disc_Compl* group for that year. Otherwise, it is categorized into the *Treat_Non_Disc_Compl* group.

Table 8 presents the main estimation results. In columns (1) and (2), the dependent variable is the total number of mortgage applications, while columns (3) and (4) report the results for the total dollar amount of mortgage applications. Columns (2) and (4) incorporate lender-county fixed effects, whereas columns (1) and (3) do not. The use of different model specifications does not materially affect the results, indicating the robustness of the analysis. In all cases, the key coefficients display a negative trend, regardless of the dependent variable employed. The *Treat_Disc_Compl* group (captured by the coefficient on *1(Disc_Compl)*) exhibits a more pronounced and statistically distinct pattern compared to the *Treat_Non_Disc_Compl* group (captured by the coefficient on *1(Non_Disc_Compl)*). The p-value for the Equality Test, which reports the t-test results for the difference between these two estimated coefficients, is presented in the table. These findings suggest that consumers extract information from publicly available complaint narratives following disclosure and subsequently reduce their engagement with lenders perceived to carry higher discrimination risks.

8.2 Rival Lenders' Reaction Towards the Disclosure

In this section, I explore whether the implementation of the disclosure policy has intensified competition among lenders in local markets. Publicly available complaint narratives may provide rival lenders with insights into the service quality of incumbent institutions, enabling them to identify areas where consumers are underserved. Rival lenders may strategically respond by expanding into regions where incumbent lenders are perceived to have engaged in discriminatory practices, thereby gaining market share by catering to minority borrowers.

To examine this possibility, I aggregate the HMDA origination data to the county-year level and calculate the density of lenders at the county level. I then use this measure as the dependent variable and estimate the following model:

$$\begin{aligned} LenderDensity_{it} & \\ &= \alpha + \beta_1 Disc_Compl_Share_i \times Post_t \\ &+ \gamma X_{it} + \mu_{State\times Year} + \mu_{County} + \epsilon_{it} \end{aligned} \tag{7}$$

In Equation (7), symbols i and t denote county and year, respectively. The dependent variable, *LenderDensity*, represents the total number of unique lender brands per ten thousand residents. The primary independent variable, *Share*×*Post*, captures the varying degrees of impact on market structures in different counties after disclosing. The metric *Disc_Compl_Share* is the total number of discrimination-related complaints received in a county in 2015 divided

by the number of loans issued to minority applicants.[39] I use this to measure the extent of the disclosure's impact across various counties. The variable *Post* equals one after the implementation of the disclosure policy. I interact *Disc_Compl_Share* with *Post* to identify the differential effects of disclosure across counties based on the specific proportions of discriminatory complaints. In this analysis, I incorporate X_{it} to represent the lagged average approval rate and the lagged average bank deposits (Dou et al. (2023)). Furthermore, I incorporate county-level fixed effects and state-by-year joint fixed effects to absorb the potential impact of local economic and temporal shocks. In the regressions, standard errors are clustered at the county level. These outcomes remain robust even when the standard error is clustered at both the county and year levels.

[Insert Table 9 about here]

Table 9 illustrates the response of rival lenders to disclosure. Columns (1) to (3) report the effects of disclosure on the total number of lenders per ten thousand residents, measured one, two, and three years following the disclosure. These effects are identified by the continuous treatment variable *Disc_Compl_Share*. The results imply that more rival lenders enter the market after the disclosure, potentially intensifying market competition. In Table A13 in the appendix, I also investigate the impacts of the disclosure across different counties using an alternative proxy. Specifically, I define *Amount Share* as the market share held by treated lenders within each county in 2015. The regression framework is in alignment with that of Table 9. I find the results are robust. This finding indicates that an increase of one standard deviation in *Amount Share* multiplied by *Post* within a county leads to the addition of 1.7 new unique lender brands for every ten thousand people in the following year. This amounts to 6.6% of one standard deviation. These findings suggest that peer lenders may enhance their market position by extracting insights from the disclosed weaknesses in their competitors' operations. These results are stronger in areas with inadequate customer service and instances of discriminatory practices. In other words, if a county is impacted by the disclosure, and the CFPB reveals evident inferior service quality (as reflected in an elevated number of discrimination complaints), rival lenders may seize this opportunity to penetrate the local market. Thereby, the competition will be heightened. Technological advancements can make it easier to collect and share these complaint narratives swiftly and precisely. These advancements are likely to increase the importance of such disclosures as a policy tool to encourage market competition.[40] The increased competition may help

[39]Given their limited market power, the disclosure of discrimination complaints by small lenders has minimal impact on regional outcomes. Therefore, when calculating the complaint share at the regional level, I do not drop the samples of large lenders with assets exceeding $100 billion. This means that my calculation of the complaint share includes all commercial banks.

[40]Lending institutions are incentivized to use the complaint database in order to enhance the quality of their

protect minority borrowers from discriminatory treatment in the mortgage lending market (Li (2023)).

8.3 Stock Market's Reaction Towards the Disclosure

In this section, I analyze how investors in the stock market respond to the CFPB's 2015 disclosure enhancement. The availability of complaint narratives allows me to identify lenders associated with discriminatory practices and examine whether the stock market reacts differently to such lenders compared to those without discrimination-related complaints. Understanding market reactions to such disclosures provides insights into how reputational concerns and regulatory risks are priced into financial institutions' valuations.

I construct a sample of 341 publicly traded financial institutions, all of which have complete daily return data over the relevant event windows. Using the U.S. Daily Event Study tool available on WRDS, I compute cumulative abnormal returns (CARs) over three event windows: [-1, +1], [-2, +2], and [-3, +3] days.

CAR is defined as the difference between a lender's daily return and the value-weighted CRSP market return. To ensure robustness, I calculate CARs under two benchmark models: the Capital Asset Pricing Model (CAPM) and the Market-Adjusted Model (referred to as *Market* in this section).

I then estimate cross-sectional regressions that relate lenders' cumulative abnormal returns to their exposure to complaints associated with discriminatory practices.

$$CAR_i = \alpha + \beta_1 Disc_Compl_i + \beta_2 Non_Disc_Compl_i + \gamma X_i + \epsilon_i \tag{8}$$

In Equation (8), CAR represents the abnormal returns for each lender. These returns are calculated using either the CAPM or the Market Adjusted Model across various event windows. I classify the institutions in the sample into three distinct groups for identification: those supervised by the CFPB with discrimination complaints in 2015, termed as $Disc_Compl$; those supervised by the CFPB without discrimination complaints in 2015, Non_Disc_Compl; and the institutions not supervised by the CFPB serving as a control group for the aforementioned categories. To account for potential confounding factors, I control for institutions' total assets, equity-to-assets ratio, return on assets, and total deposits, all measured as of the end of 2014.

[Insert Table 10 about here]

Table 10 presents the stock market's response to institutions in the $Disc_Compl$ group

mortgage services. This is due to their frequent assessment of their performance against competitors. They use metrics derived from the database, as outlined in the Consumer Financial Protection Bureau (CFPB) report of 2013.

(or *Non_Disc_Compl* group) relative to those not supervised by the CFPB. The first three columns show the outcomes for the event windows of [-1, +1], [-2, +2], and [-3, +3] trading days, respectively. Furthermore, I conduct a t-test for the two coefficients estimated in every column to assess whether significant differences exist between them. The results show that CFPB-supervised institutions experience a negative market reaction after the disclosure. Moreover, the market response appears more adverse for lenders involved in discrimination complaints. The estimated coefficients suggest that institutions in the Complaint group experience a drop in CAR ranging from approximately 1.3 to 2.6 percentage points compared to institutions not under supervision after the disclosure. The distinction between lenders with discrimination complaints and those without is statistically significant, especially within shorter event windows (such as [-1, +1]). Columns (4) to (6) in Table 10 use cumulative abnormal returns based on the Market Adjusted Model. They yield consistent results.

These observations highlight the significant results of the disclosure. They show that the market interprets the release of such information as a negative shock that leads to substantial reactions. On the one hand, the enforcement against discriminatory practices in the U.S. market is stringent. This results in considerable economic and reputational costs once courts have ruled on unlawful discriminatory lending practices. This situation might make investors worried about these lenders' future performance. Thereby, they will sell stocks associated with such possible legal risks.[41] On the other hand, investors might exhibit aversion to inequality, given the increasing significance of socially responsible investing (Hartzmark and Sussman (2019)). They might be reluctant to invest in lenders considered socially irresponsible because these institutions appear to have treated minority consumers unfairly.[42]

9 Real Effects of Disclosure

In my analysis of rejection rates in Section 6, I find that disclosing complaint narratives enables over 14,000 minority households to successfully obtain mortgage loans each year. To further validate the real-world effectiveness of the disclosure policy, I examine whether it contributes to reducing racial gaps in homeownership. Specifically, I assess the impact of disclosure on homeownership rates by race.

I obtain county-year level housing occupancy data by race from the American Community Survey (ACS) and merge it with county-year level data aggregated from the HMDA origination

[41] For instance, Wells Fargo's stock price dropped by 1.3 percent on a down day due to litigation and fines related to discriminatory practices. Refer to `https://www.reuters.com/article/us-wells-lending-settlement-idUSBRE86B0V220120712`.

[42] Previous research indicates that concerns regarding inequality might have a significant influence within the financial markets (Pan et al. (2022)).

dataset. The final merged dataset allows for an assessment of the disclosure policy's effect on homeownership. The regression model I employ is outlined as follows:

$$\begin{aligned} Homeownership_{it} \\ &= \alpha + \beta_1 Disc_Compl_Share_i \times Post_t \\ &+ \gamma X_{it} + \mu_{State \times Year} + \mu_{County} + \epsilon_{it} \end{aligned} \tag{9}$$

Symbols i and t represent county and year, respectively. It is important to highlight that I select three distinct dependent variables to identify racial gaps in the analysis: (i) homeownership among minority residents, specifically Black and Hispanic residents; (ii) homeownership among white residents; and (iii) the disparity between the two indicators.[43] In this analysis, I continue to use the metric *Disc_ Compl_ Share* to measure the magnitude of the disclosure's impact across various counties. To control for potential bias arising from unobservable macroeconomic variables, I incorporate the growth rate of income per capita, unemployment rates, and the logarithm of the population at the county level. I also include the county-level fixed effects and state-by-year joint fixed effects.

[Insert Table 11 about here]

Table 11 presents the estimated outcomes. Column (1) details the effect of the disclosure policy on minority residents. These findings indicate that a one standard deviation change in *Disc_ Compl_ Share* $\times$ *Post* results in a 0.87% increase in homeownership for minority residents within a county (corresponding to 2.9% of one standard deviation in homeownership). According to a report from the National Association of Realtors (NAR), the annual average growth rate of homeownership among Black Americans has been only 0.04% over the past decade.[44] This contrast supports the possible strong effect of the disclosure policy in mitigating racial disparities.

Column (2) reports an insignificant effect of disclosure on the homeownership rates of white residents, while column (3) shows a significantly positive impact on the gap between minority and white homeownership rates. These results support the inference that a one standard deviation increase in the independent variable leads to a 1.2% of a standard deviation improvement in closing racial gaps.

Overall, the findings demonstrate that the disclosure of complaint narratives has a meaningful and economically significant effect on reducing racial disparities. Consequently,

[43] I compute the variable by subtracting homeownership among white residents from homeownership among minority residents. This variable measures the extent to which homeownership among minority residents falls short compared to white residents. Its value is typically negative. Therefore, a positive β_1 signifies a reduction in racial gaps.

[44] For more details, please see: `https://www.nar.realtor/newsroom/more-americans-own-their-homes-but-black-white-homeownership-rate-gap-is-biggest-in-a-decade-nar`

complaint disclosure emerges as an effective policy tool that can be integrated into the broader framework of financial regulation.

10 Conclusion

Racial disparities remain a central concern among scholars and policymakers. This study presents the first comprehensive investigation into the impact of complaint narrative disclosure on racial disparities within the financial market. Employing innovative identification strategies, such as the GSE pricing grid and the outcome test methodology, I detect racial bias between minority and white borrowers in the mortgage market while controlling for differences in risk and creditworthiness.

Analyzing interest rates, default rates, and rejection rates for minority borrowers, the findings confirm that the implementation of the disclosure policy has significantly reduced pre-existing racial disparities. Following the policy's adoption, minority borrowers save an average of $93 million annually in mortgage-related costs, and over 15,000 minority households obtain mortgage loans each year.

Further analysis demonstrates that, after the public disclosure of complaint narratives, financial institutions provide more favorable resolutions for complaints accompanied by narratives compared to those without. Additionally, examining homeownership outcomes at the county level reveals that disclosure mitigates racial gaps in homeownership, suggesting that transparency in the lending market plays a critical role in reducing discrimination and promoting financial inclusion.

Finally, exploring the mechanisms underlying these effects, the evidence suggests that complaint narrative disclosure disciplines loan officers by influencing their career trajectories. Reactions from other stakeholders further support these findings and highlight additional channels through which the disclosure policy exerts its impact.

I survey the status of financial consumer protection organizations in major developed countries, as shown in Table A14. Currently, approximately 93% of developed countries have regulatory entities responsible for addressing and resolving customer complaints related to financial services. Half of these countries have dedicated financial regulators that prioritize consumer protection. However, aside from the CFPB in the United States, few financial regulatory agencies publicly disclose consumer complaint information.[45] My findings show that publicly exposing consumer complaints can reduce discriminatory and unfair practices in financial markets through multiple channels. These findings offer practical insights into

[45] Only a few countries provide limited references to such data, typically in the form of case studies or annual reports, rather than through full public disclosure. For additional details, see Table A14 in the Appendix.

financial system design and oversight. Other countries may consider this disclosure policy as a reference in developing strategies for addressing and publicizing consumer complaints. Adopting similar disclosure mechanisms may hold considerable promise in reducing global racial disparities.

My findings demonstrate that the public disclosure of customer complaints effectively mitigates discriminatory and unfair treatment of clients in the U.S. financial system through various channels. Other countries may draw valuable lessons from this disclosure policy and consider developing their own strategies for managing and disclosing consumer complaints. Such measures have the potential to enhance transparency, improve service quality, and promote fair treatment in global financial markets.

Lastly, I develop an online query platform to assist minority borrowers in the United States in avoiding potential bias in mortgage lending.[46] This platform offers direct and transparent access to data on lenders' discriminatory practices at detailed geographic levels. I evaluate each lender's level of unfair treatment using the excess rejection rates and interest rates applied to minority borrowers, and rank lenders accordingly.[47]

Users can easily obtain information about the degree of discrimination exhibited by lenders within their communities without requiring advanced data analysis skills. By lowering the barriers to accessing discrimination-related information, the platform empowers minority borrowers to enhance their financial literacy and make informed decisions, helping them avoid engaging with discriminatory lenders.

[46] More information is available at: `https://discriminationwarrior.com`

[47] Figure A8 and Figure A9 illustrate specific examples of queries for ZIP code 02135, an area in Boston. For complete platform functionality and additional information, see Appendix D.

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Figures and Tables

Figure 1. Dynamic Trend of the Impact of Disclosure on Interest Rate

This figure represents the quarterly coefficient estimates for racial disparities in interest rates among treated lenders relative to control lenders. The illustration contains outcomes from five quarters before the second quarter of 2015 - the starting point of the disclosure policy - and continues through ten quarters after its application, that is, between 2014 and 2017. Samples outside of this chosen span are grouped into the initial and final periods. During the estimation, the fifth quarter prior to the disclosure and earlier periods are used as the baseline period. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The Y-axis reflects the magnitude of the racial disparities in interest rates, while the X-axis signifies varying periods, with period zero corresponding to the disclosure policy's implementation. The regression specification used to generate this figure is consistent with that of column (2) in Table 2. Black circles symbolize the coefficient estimates for different periods, and the black vertical lines denote the 95% confidence intervals.

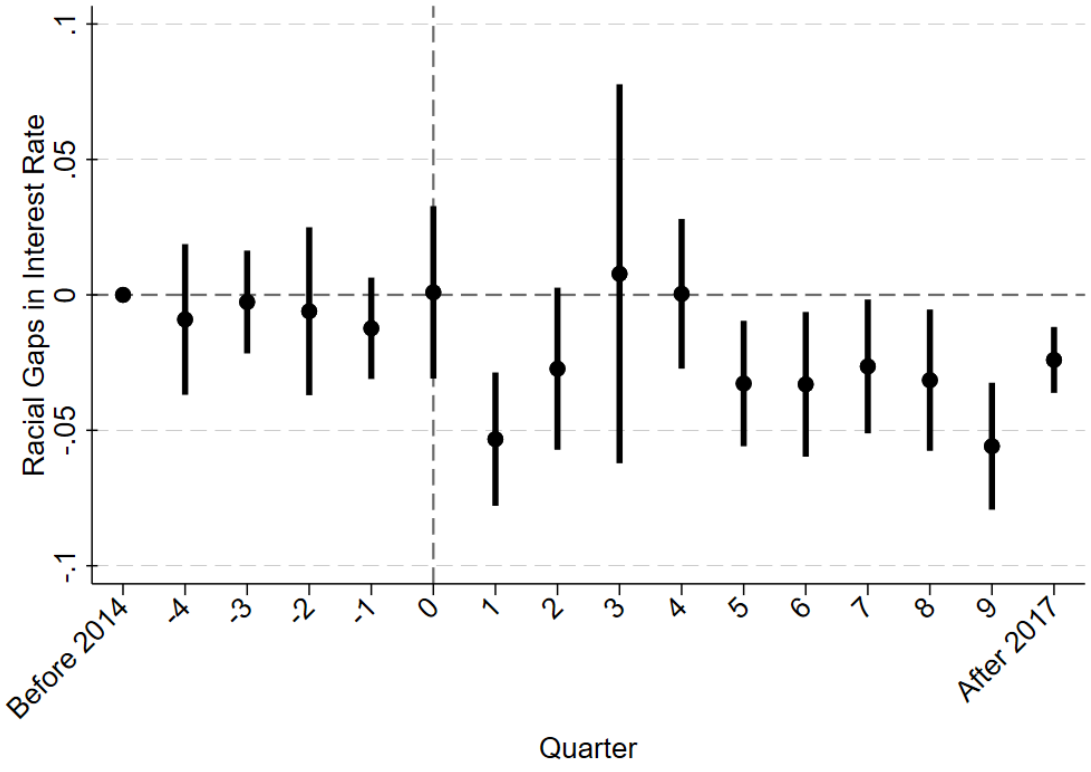


Figure 2. The Trend of Racial Disparities in Rejection Rates between CFPB-supervised and CFPB-unsupervised Banks

This figure shows the average excess rejection rates of minority borrowers relative to non-minority borrowers across both CFPB-supervised and CFPB-unsupervised banks from 2012 to 2019. The data for the diagram are drawn from the HMDA origination dataset. I utilize the residualized rejection rates after accounting for observable differences by applying lender-by-year fixed effects, lender-similar borrower fixed effects, lender-census tract fixed effects, and census tract-by-year fixed effects, as demonstrated in Equation (3). Furthermore, I utilize a 3-year moving average for a more accurate depiction of the actual trend. The figure features a vertical dashed line symbolizing mid-2015, the point in time when the CFPB disclosed the complaint narratives.

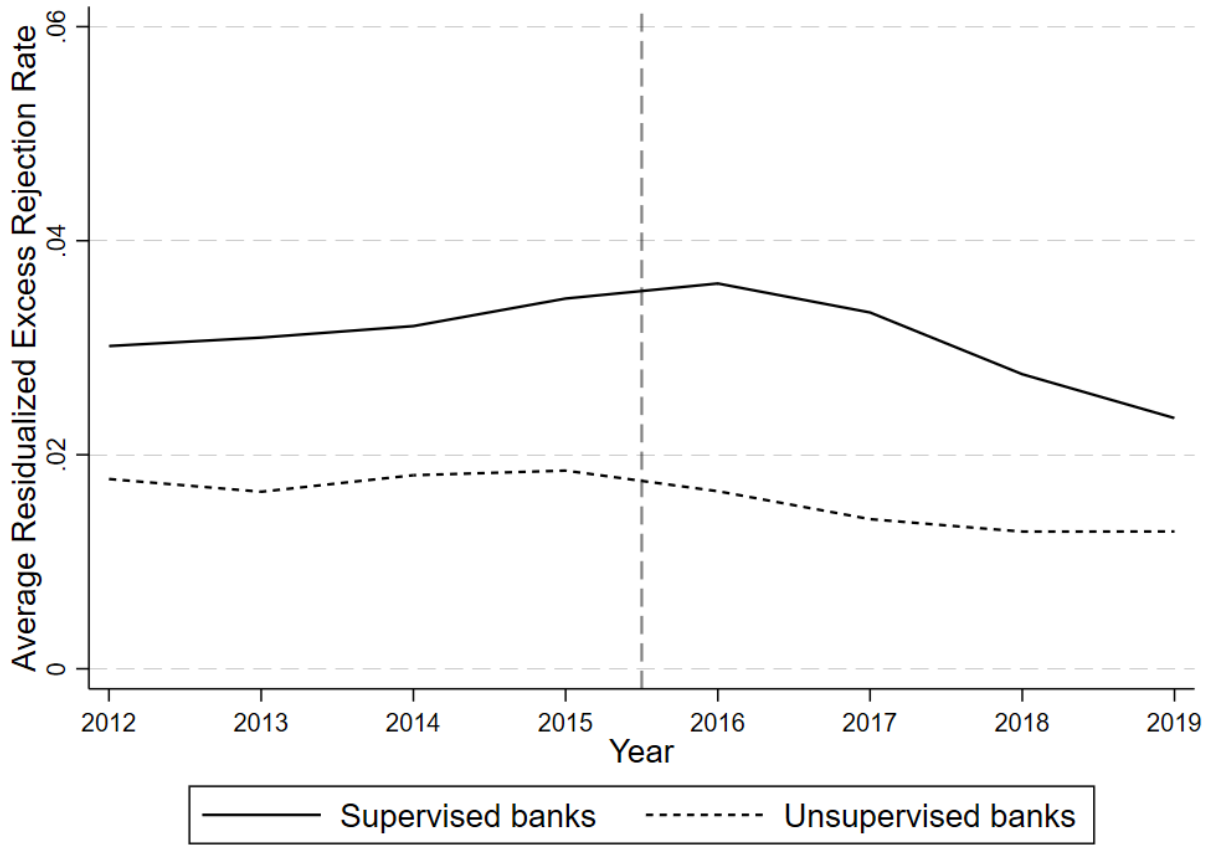
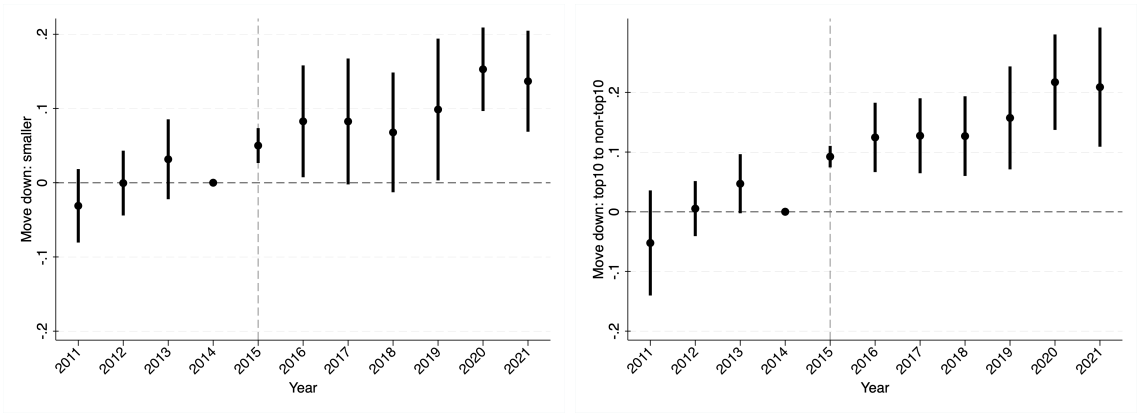


Figure 3. Dynamic Trend of the Impact of Disclosure on Loan Officer's Downward Movements

This figure represents the results of an event study examining the impact of the CFPB disclosure policy on loan officers' downward movements. It focuses on two key metrics: (a) *Move Down (move to smaller lenders)*, which indicates a loan officer moves to a smaller lender than their previous employer, and (b) *Move Down (move to non-top10 lenders)*, which indicates a loan officer transitions from one of the top 10 biggest lenders to a non-top 10 lender. The illustration contains outcomes from four years before 2015 - the inception of the disclosure policy - and continues through six years after its implementation. The year immediately preceding the disclosure serves as the baseline period in the estimation. The data are derived from the NMLS-CFPB merged dataset spanning 2011-2022. The sample from 2022 is excluded because I cannot observe career transitions from 2022 to 2023, which fall outside my sample period. The Y-axis represents the probability of a loan officer moving to a smaller lender or transitioning from a top-10 to a non-top 10 lender, while the X-axis denotes different years. The regression specification employed to generate this figure aligns with that of Table 6. The black circles indicate coefficient estimates for various periods, and the accompanying black vertical lines represent the 95% confidence intervals.



(a) Move Down (smaller)

(b) Move Down (top 10 to non-top 10)

Table 1. Summary Statistics of the HMDA-GSE Merged Dataset

This table presents summary statistics for the key variables derived from the HMDA-GSE merged dataset, which I use to identify the effects of the disclosure policy on interest rates and default rates when consumers apply for mortgage loans. The treated banks are those with total assets of 10 to 100 billion prior to the policy's implementation, while the control banks have total assets ranging from 0 to 10 billion. The variables are averaged from 2011 to May 2015, the pre-disclosure period. The table provides unconditional means, standard deviations, and p-values for the differences in means between the treated and control groups before treatment. The interest rate variable undergoes winsorization at the 1% level. For the t-test, standard errors are clustered at the bank level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$All(Treated\&Control)$			$Treated$		$Control$		$t-Test$
	$Observations$	$Mean$	SD	$Mean$	SD	$Mean$	SD	$p-Value$
Minority	340,320	0.082	0.274	0.067	0.250	0.084	0.278	0.466
Interest Rate	340,320	4.203	0.473	4.112	0.467	4.217	0.472	0.096
Default Rate	340,320	0.065	0.246	0.059	0.236	0.066	0.248	0.633
LTV	340,320	77.200	14.210	76.310	14.300	77.340	14.190	0.443
Credit Score	340,285	752.700	44.660	759.200	41.880	751.700	45.000	0.019
Gender	340,320	0.727	0.445	0.729	0.444	0.727	0.446	0.867
Income	338,509	100.900	74.740	107.100	88.200	99.910	72.350	0.117
Loan Amount	340,320	246.100	127.400	239.100	127.600	247.200	127.300	0.606
Co-applicant	340,320	0.534	0.499	0.559	0.496	0.530	0.499	0.236
Cash-out Refinance	340,320	0.181	0.385	0.163	0.370	0.184	0.388	0.390
Purchase	340,320	0.492	0.500	0.514	0.500	0.489	0.500	0.712
Refinance	340,320	0.326	0.469	0.323	0.467	0.327	0.469	0.931

Table 2. The Impact of Disclosure on Interest Rate

This table reports results on the effect of complaint narrative disclosure on the racial gaps in interest rates. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. Each column provides estimated coefficients on four independent variables, with other coefficient groups (*Treat*, *Post*, and their interaction term *Treat*×*Post*) being absorbed by fixed effects due to collinearity. The key estimated coefficient, *Treat*×*Post*×*Minority*, denotes the effect of disclosure on racial gaps in interest rates. In column (1), fixed effects for the interaction of lender and time, and pricing grid-loan purpose-time joint fixed effects are controlled. Column (2) incorporates controls for borrower characteristics. Column (3) combines the two fixed effects from column (1) into lender-pricing grid-loan purpose-time joint fixed effects. Column (4), building on column (3), replaces borrower characteristics with the more stringent similar borrower fixed effects. Column (5) introduces joint fixed effects between lenders and similar borrowers, based on column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var =	Interest Rate				
Treat×*Post*×*Minority*	-0.022***	-0.023***	-0.026***	-0.025***	-0.026**
	(0.006)	(0.006)	(0.005)	(0.004)	(0.011)
Post×*Minority*	-0.001	0.002	-0.001	-0.001	0.001
	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
Treat×*Minority*	-0.001	-0.008***	-0.000	-0.003	-0.001
	(0.003)	(0.002)	(0.004)	(0.004)	(0.006)
Minority	0.028***	0.025***	0.027***	0.029***	0.029***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.006)
Lender×Year-Month FE	YES	YES			YES
Bucket×Loan Purpose ×Year-Month FE	YES	YES			YES
Lender×Bucket×Loan-Purpose×Year-Month FE			YES	YES	
Borrower Characteristics FE		YES	YES		
Similar Borrower FE				YES	
Lender×Similar Borrower FE					YES
Observations	1,305,735	1,273,854	1,031,962	1,023,226	803,944
R-squared	0.756	0.772	0.799	0.808	0.816

Table 3. Moderation Effect of the Impact of Disclosure on Interest Rates: Discrimination Complaint and Search Attention

This table presents findings on the moderating effect of disclosure on racial disparities in interest rates, with data drawn from the HMDA-GSE merged dataset spanning the years 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The table illustrates heterogeneous outcomes related to discrimination complaints and Google search attention by representing the contrasting outcomes between two subgroups of the treated group, in comparison to the same control group. The treated group in columns (1) and (2) is categorized based on the incidence of discrimination complaints, whereas the treated group in columns (3) and (4) is categorized based on Google search attention. The estimated coefficient on *Treat*×*Post*×*Minority* identifies the treatment effect in the subsample analysis. All four columns control for joint fixed effects of lender-pricing grid-loan purpose-time as well as borrower characteristics fixed effects. Standard errors in this table, indicated in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Discriminatory Complaint		Google Search Index	
	Yes	No	High	Low
Dep. Var =	Interest Rate			
Treat×*Post*×*Minority*	-0.029**	-0.005	-0.036***	-0.008
	(0.010)	(0.013)	(0.004)	(0.008)
Lender×Bucket×Loan Purpose×Year-Month FE	YES	YES	YES	YES
Borrower Characteristics FE	YES	YES	YES	YES
Observations	982,645	984,165	983,185	977,903
R-squared	0.796	0.796	0.797	0.796

Table 4. Impact of Disclosure on Default Rates of Subprime Minority Borrowers

This table reports results on the effect of complaint narrative disclosure on the racial gaps in default rates. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The dependent variable denotes whether the loan falls into the delinquency of 90 or more days. Columns (1)-(3) limit the sample to those with credit scores under 660, showcasing the results concerning subprime minority borrowers, while column (4) outlines results from the entire sample. Each column provides estimated coefficients on four independent variables, with other coefficient groups (*Treat*, *Post*, and their interaction term *Treat*×*Post*) being absorbed by fixed effects due to collinearity. The key estimated coefficient, *Treat*×*Post*×*Minority*, denotes the effect of disclosure on racial gaps in default rates. Column (1) solely controls for lender-time (year-month) fixed effects and ZIP-by-year fixed effects, and column (2) incorporates controls for loan traits such as interest rate, loan purpose, and the logarithm of the loan amount, applicant characteristics like gender, the logarithm of income, and the presence of a co-applicant. Column (3) extends column (2) by including pricing grid-loan purpose fixed effects. The model specification in column (4) is consistent with column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Marginal Borrowers			Full
Dep. Var =	Default rate			
Treat×*Post*×*Minority*	0.362***	0.342***	0.343***	0.012
	(0.062)	(0.074)	(0.080)	(0.009)
Post×*Minority*	-0.014	-0.010	-0.013	0.008**
	(0.046)	(0.036)	(0.038)	(0.003)
Treat×*Minority*	-0.339***	-0.331***	-0.337***	-0.010
	(0.049)	(0.054)	(0.061)	(0.009)
Minority	0.031	0.028	0.030	0.012***
	(0.044)	(0.036)	(0.038)	(0.003)
Control		YES	YES	YES
Lender×Year-Month FE	YES	YES	YES	YES
ZIP×Year FE	YES	YES	YES	YES
Bucket×Loan Purpose FE			YES	
Observations	25,759	24,187	24,185	1,243,580
R-square	0.491	0.500	0.502	0.149

Table 5. The Impact of Disclosure on Rejection Rates

This table reports results on the effect of complaint narrative disclosure on the racial gaps in rejection rates. The data come from the HMDA origination dataset spanning 2011-2019. The dependent variable is the loan-level rejection status. Each column provides estimated coefficients on four independent variables, with other coefficient groups (*Treat*, *Post*, and their interaction term *Treat*×*Post*) being absorbed by fixed effects due to collinearity. The key estimated coefficient, *Treat*×*Post*×*Minority*, denotes the effect of disclosure on racial gaps in rejection rates. The first pair of columns present the fundamental results, where I account for lender-by-year fixed effects, lender-similar borrower combined fixed effects, and lender-census tract combined fixed effects in column (1), whereas I also include census tract-by-year fixed effects in column (2). Columns (3) and (4) display the moderation effects of discrimination complaints, based on the specification detailed in column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Baseline		Discriminatory Complaint	
			Yes	No
Dep. Var =	Rejection Rate			
Treat×*Post*	-0.019*	-0.017*	-0.033***	-0.007
×*Minority*	(0.009)	(0.008)	(0.010)	(0.005)
Post×*Minority*	-0.002	-0.003	-0.003	-0.003
	(0.006)	(0.005)	(0.005)	(0.005)
Treat×*Minority*	0.043***	0.041***	0.066***	0.019***
	(0.009)	(0.008)	(0.008)	(0.005)
Minority	0.034***	0.035***	0.035***	0.035***
	(0.004)	(0.004)	(0.004)	(0.004)
Lender×Year FE	YES	YES	YES	YES
Lender×Similar- Borrower FE	YES	YES	YES	YES
Lender×Census- Tract FE	YES	YES	YES	YES
Census Tract× Year FE		YES	YES	YES
Observations	17,383,935	17,377,535	15,822,650	15,951,820
R-squared	0.459	0.477	0.481	0.482

Table 6. The Impact of Disclosure on Loan Officers' Downward Movements

This table presents the impact of complaint narrative disclosure on loan officers' downward career movements. The analysis covers the period from 2011 to 2022, using data from the merged NMLS loan officer panel and CFPB consumer complaints database. I employ two dependent variables: (1) *Move Down (move to smaller lenders)*, equals one if a loan officer moves to a smaller lender than their previous employer; (2) *Move Down (move to non-top 10 lenders)*, equals one if a loan officer transitions from a top 10 to a non-top 10 lender. The coefficients for $Treat \times Post$ indicate the treatment effects for loan officers in lenders subject to complaint narrative disclosure. I further partition CFPB-supervised lenders into two groups based on their discriminatory complaint records during 2011-2022 and interact these indicators with *Post*. The estimated coefficients for *1(Treat_ Disc_ Compl)*×*Post* measure the effects on loan officers' downward movements in CFPB-supervised lenders with discriminatory complaint records after disclosure, while the coefficients for *1(Treat_ Non_ Disc_ Compl)*×*Post* capture the effects in CFPB-supervised lenders without discriminatory complaint records. Across all columns, the considerations include the duration of an individual's career as a control variable. *Treat*, *1(Treat_ Disc_ Compl)*, and *1(Treat_ Non_ Disc_ Compl)* are absorbed by lender fixed effects. *Post* is absorbed by year fixed effects. Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Move down		Move down	
Dep. Var =	move to smaller lenders		move to non-top10 lenders	
Treat×*Post*	0.060*		0.105***	
	(0.036)		(0.038)	
1(Treat_ Disc_ Compl)		0.073***		0.124***
×*Post*		(0.019)		(0.026)
*1(Treat_ Non_ Disc_*		0.056		0.100**
Compl)×*Post*		(0.039)		(0.041)
Control	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES
Lender FE	YES	YES	YES	YES
ZIP FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	1,050,019	1,050,019	1,050,019	1,050,019
R-squared	0.777	0.777	0.767	0.767

Table 7. The Impact of Disclosure on Loan Officers' Promotion Prospects

This table presents the impact of complaint narrative disclosure on loan officers' promotion prospects. Because managerial data from the NMLS dataset is available starting from 2014, the analysis covers the period from 2014 to 2022. I employ three dependent variables: (1) *Promoted*, a dummy variable equal to one if the loan officer is promoted to a managerial position; (2) *Ext_Promoted_Bigger*, equal to one if the loan officer is promoted to a lender with more employees than their former employer; (3) *Int_Promoted*, equal to one if the loan officer is promoted within the same branch. The coefficients for $Treat \times Post$ indicate the treatment effects for loan officers in lenders subject to complaint narrative disclosure. I further partition CFPB-supervised branches into two groups based on their discrimination-related complaint records during 2015-2022 and interact these indicators with *Post*. The estimated coefficients for $1(Treat_Disc_Compl) \times Post$ measure the effects on loan officers' promotion prospects in CFPB-supervised lenders with complaint records after disclosure, while the coefficients for $1(Treat_Disc_Non_Complaint) \times Post$ capture the effects in CFPB-supervised lenders without complaint records. The last row presents the p-value of the Wald test examining the difference between the coefficients for $1(Treat_Disc_Compl)$ and $1(Treat_Disc_Non_Complaint)$. Across all columns, the considerations include the duration of an individual's career as a control variable. *Treat*, $1(Treat_Disc_Compl)$, and $1(Treat_Non\ Disc_Compl)$ are absorbed by lender fixed effects. *Post* is absorbed by year fixed effects. Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var =	Promoted		Ext_Promoted_Bigger		Int_Promoted	
Treat×Post	-0.029***		-0.004		-0.014***	
	(0.004)		(0.005)		(0.002)	
1(Treat_Disc_Compl)		-0.035***		-0.006*		-0.016***
×Post		(0.004)		(0.004)		(0.002)
*1(Treat_Non_Disc_*		-0.028***		-0.003		-0.014***
Compl)×Post		(0.005)		(0.005)		(0.002)
Control	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
ZIP FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Lender FE	YES	YES	YES	YES	YES	YES
Observations	806,139	806,139	806,139	806,139	806,139	806,139
R-squared	0.839	0.839	0.811	0.811	0.894	0.894
P-value		0.001***		0.024**		0.029**

Table 8. Consumer Reaction to the Disclosure

This table presents the impact of complaint narrative disclosure on the application behavior of consumers, drawing data from the HMDA origination dataset for the period 2011 to 2019 and organizing it at the county-lender-minority-year level. The first dependent variable, referred to as *Application Number*, is the log of the number of mortgage applications from minority or white borrowers within at a specific county-lender-year level, while the second, known as *Application Amount*, is the log of the dollar value of mortgage applications from such borrowers within the same context. The table highlights the estimated results for two main independent variables, *1(Disc_ Compl)×Post×Minority* and *1(Non_ Disc_ Compl)×Post×Minority*, both of which are triple-difference estimated coefficients. In creating these variables, I divide CFPB-supervised banks into two groups based on discrimination-related complaints during 2015-2019 and then multiply each with the product of *Post* and *Minority*, resulting in two triple terms. The estimated coefficient for *1(Disc_ Compl)×Post×Minority* indicates racial gaps in banks with discriminatory practices under CFPB supervision, compared to the control group of CFPB-unsupervised banks, as influenced by disclosure. Similarly, *1(Non_ Disc_ Compl)×Post×Minority* shows the changes in racial gaps in CFPB-supervised banks without such discriminatory complaints. Across all columns, the considerations include (i) the percentage of mortgages that a lender approves in a specific county; (ii) an indicator variable valued at one, denoting the bank's physical presence in the county for that year; and (iii) the logarithm of the total deposits collected by the bank's branches within the county during that year. Additional controls include lender-by-year and county-by-year fixed effects in columns (1) and (3), with the introduction of lender-county fixed effects in columns (2) and (4). Standard errors, shown in parentheses, are clustered at the county and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dep. Var =	Application Number		Application Amount	
1(Disc_ Compl)×Post	-0.167*	-0.160*	-0.291***	-0.284**
×Minority	(0.082)	(0.081)	(0.082)	(0.090)
1(Non_ Disc_ Compl)	-0.049	-0.043	-0.077**	-0.074***
×Post×Minority	(0.032)	(0.033)	(0.031)	(0.017)
P-value for Equality Test	0.0925	0.0983	0.0112	0.0336
Control	YES	YES	YES	YES
Lender×Year FE	YES	YES	YES	YES
Lender×County FE	YES	YES	YES	YES
County×Year FE		YES		YES
Observations	1,029,001	1,028,467	1,029,001	1,028,467
R-squared	0.821	0.831	0.832	0.841

Table 9. Rival Bank's Reaction to the Disclosure

This table reports results on the effect of complaint narrative disclosure on the entry of rival banks. The data, sourced and consolidated from the HMDA origination dataset for the years 2011 to 2019, are aggregated at the county-year level. The dependent variable, *BankDensity*, is calculated as the number of unique bank brands per ten thousand individuals at the county level. The table showcases the estimated results for the *Disc_ Compl_ Share*×*Post* independent variable, with other coefficient groups (*Disc_ Compl_ Share* and *Post*) being subsumed by fixed effects due to collinearity. This coefficient identifies changes in the unique bank brand number per ten thousand people in counties affected by the disclosure. I employ the measure *Disc_ Compl_ Share*, defined as the total number of discrimination-related complaints received per county in 2015 divided by the number of loans issued to minority applicants, to assess the extent of exposure to the effects of disclosure. In all columns, I account for the growth rate of income per capita and unemployment rates and control for county-level fixed effects and state-by-year joint fixed effects. Columns (1) to (3) display variations in the total number of unique banks per ten thousand individuals in the present period (t), the next year (t+1), and two years later (t+2). Standard errors, shown in parentheses, are clustered at the county level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	T	T+1	T+2
Dep. Var =	Bank Density		
*Disc_ Compl_*	12.219***	13.773***	14.152***
Share×*Post*	(3.616)	(3.775)	(3.879)
Control	YES	YES	YES
State×Year FE	YES	YES	YES
County FE	YES	YES	YES
Observations	21,586	18,393	16,041
R-squared	0.759	0.781	0.792

Table 10. Market Reaction to the Disclosure

This table reports results on the effect of complaint narrative disclosure on the behavior of investors in the stock market. The data for analysis come from all listed banks within the HMDA origination dataset. Using the U.S. Daily Event Study function offered by WRDS, I estimate their cumulative abnormal return (CAR) based on the disclosure date of June 25, 2015. The dependent variables represent the CAR calculated based on the Capital Asset Pricing Model (CAPM) and the Market Adjusted Model (referred to as *Market*), during the three event windows of [-1, +1], [-2, +2], and [-3, +3] trading days. The table primarily showcases two independent variables, namely a dummy representing treated banks that have received discrimination complaints in 2015, labeled as *Disc_ Compl*, and a dummy symbolizing treated banks that have not received discrimination complaints in 2015, termed *Non_ Disc_ Compl*. In all columns, I control for the banks' total assets, equity-to-assets ratio, return on assets, and total deposits, and execute a t-test to compare the coefficients of two primary independent variables. The robust standard errors are reported in parentheses. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Dep.Var =	CAR (CAPM)			CAR (Market)		
	[-1, +1]	[-2, +2]	[-3, +3]	[-1, +1]	[-2, +2]	[-3, +3]
Disc_ Compl	-0.013**	-0.026***	-0.018*	-0.013**	-0.026***	-0.019*
	(0.006)	(0.009)	(0.010)	(0.006)	(0.009)	(0.010)
Non_ Disc_ Compl	-0.004	-0.015**	-0.013	-0.004	-0.015**	-0.013
	(0.004)	(0.007)	(0.011)	(0.004)	(0.007)	(0.011)
P-value for Equality Test	0.041	0.183	0.550	0.040	0.184	0.506
Control	YES	YES	YES	YES	YES	YES
Observations	341	341	341	341	341	341
R-squared	0.033	0.095	0.027	0.033	0.101	0.031

Table 11. The Impact of Disclosure on Homeownership

This table reports results on the real effect of complaint narrative disclosure on homeownership. The data, sourced and consolidated from the HMDA origination dataset for the years 2011 to 2019, are aggregated at the county-year level. The dependent variable, *Homeownership Ratio*, is extracted from the ACS datasets and represents housing homeownership across different races at the county-year level. The table showcases the estimated results for the *Disc_Compl_Share*×*Post* independent variable, with other coefficient groups (*Disc_Compl_Share* and *Post*) being subsumed by fixed effects due to collinearity. This coefficient identifies changes in the homeownership of different races and racial gaps in homeownership in counties affected by the disclosure. I employ the measure *Disc_Compl_Share*, defined as the total number of discrimination-related complaints received per county in 2015 divided by the number of loans issued to minority applicants, to assess the extent of exposure to the effects of disclosure. In all columns, I account for the growth rate of income per capita, unemployment rates, and logarithm of population, and control for county-level fixed effects and state-by-year joint fixed effects. Columns (1) to (3) display variations in (i) homeownership among minority residents, specifically Black and Hispanic residents; (ii) homeownership among white residents; and (iii) the difference between the two indicators. Standard errors, shown in parentheses, are clustered at the county level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	Minority	White	Diff
Dep.Var =	Homeownership Ratio		
*Disc_Compl_*	0.593**	0.081	0.527**
Share×*Post*	(0.257)	(0.069)	(0.246)
Control	YES	YES	YES
State×Year FE	YES	YES	YES
County FE	YES	YES	YES
Observations	4,815	4,815	4,815
R-squared	0.850	0.958	0.804

Internet Appendix for Does the Disclosure of Consumer Complaints Reduce Racial Disparities in the Mortgage Lending Market?

Appendix A. Supplementary Figures and Tables

Figure A1. Number and Fraction of Complaints about Discrimination in the Mortgage Market

Figure A2. Sample Representation of the GSE Grid

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Figure A4. Impact of CFPB Disclosure on Racial Gaps in Default Rates across Asset Size Caps

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Appendix B. Clarifications on the Process of Merging Datasets

HMDA-GSE Merged Dataset: Linkage Process

HMDA-GSE Merged Dataset: Filtering Process

HMDA Origination Dataset: Variable Supplementation Process

Appendix C. CFPB Disclosure and Complaints' Resolution Outcomes

CFPB Disclosure and Complaints' Resolution Outcomes

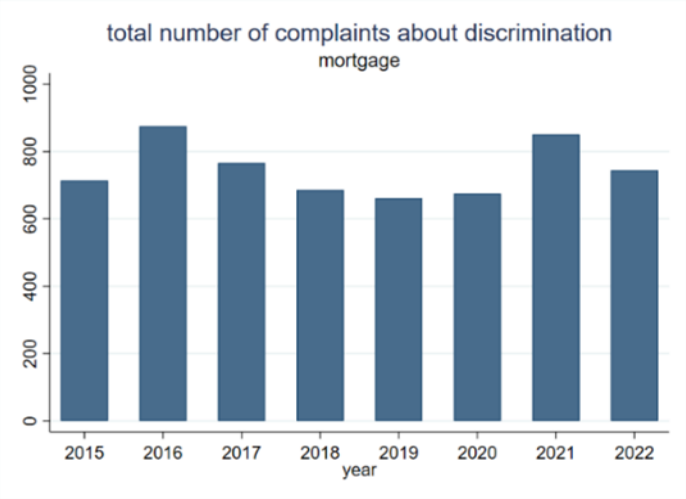
Appendix D. Website Ranking Banks for Discrimination

Website Ranking Banks for Discrimination

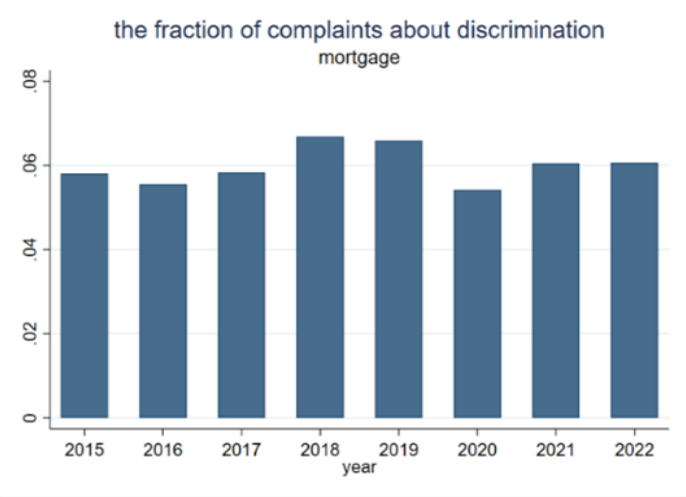
Appendix A. Supplementary Figures and Tables

Figure A1. Number and Fraction of Complaints about Discrimination in the Mortgage Market

These two graphs respectively depict the total number and proportion of consumer complaints related to discrimination in the mortgage market regulated by the CFPB from 2015 to 2022.



(a) Total number of complaints about discrimination



(b) The fraction of complaints about discrimination

Figure A2. Sample Representation of the GSE Grid

The figure illustrates the 2018 LLPA (loan-level price adjustment) grid of Fannie Mae, sourced from the Fannie Mae Selling Guide, published on June 5, 2018. Fannie Mae's LLPA grid has a corresponding grid at Freddie Mac, known as the Credit Fees in Price chart. These matrices dictate the additional g-fee (guarantee fee) that lenders owe the GSE for the mortgage guarantee, fully determined by credit score and LTV.



Table 1: All Eligible Mortgages – LLPA by Credit Score/LTV Ratio

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

Figure A3. Impact of CFPB Disclosure on Racial Gaps in Interest Rates across Asset Size Caps

This figure shows the effect of disclosure on racial gaps in interest rates across various asset size caps. The horizontal axis represents the asset size limits used for selecting caps. I conduct an estimation every $5 billion from $25 to $95 billion. The vertical axis displays the coefficients for the triple-interaction terms in the triple-difference model estimated at different cap thresholds. These coefficients come from the model specified in column (2) of Table II. The dots indicate the coefficient values, while the lines extending from each dot represent the confidence intervals, set at a 10% significance level.

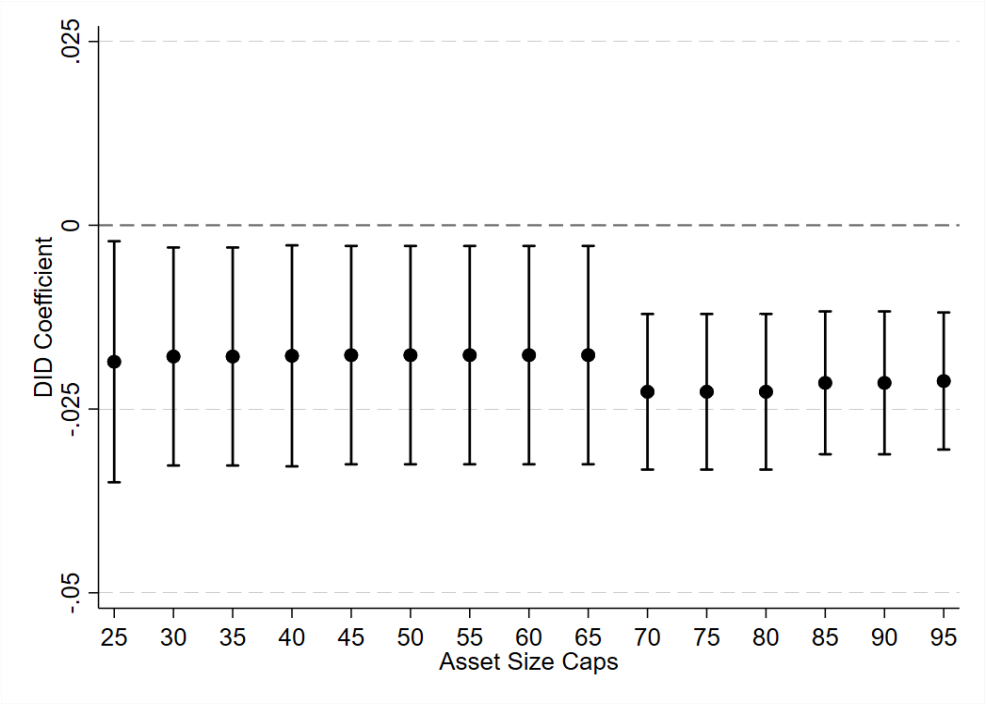


Figure A4. Impact of CFPB Disclosure on Racial Gaps in Default Rates across Asset Size Caps

This figure shows the effect of disclosure on racial gaps in default rates across various asset size caps. The horizontal axis represents the asset size limits used for selecting caps. I conduct an estimation every $5 billion from $25 to $95 billion. The vertical axis displays the coefficients for the triple-interaction terms in the triple-difference model estimated at different cap thresholds. These coefficients come from the model specified in column (1) of Table IV. The dots indicate the coefficient values, while the lines extending from each dot represent the confidence intervals, set at a 10% significance level.

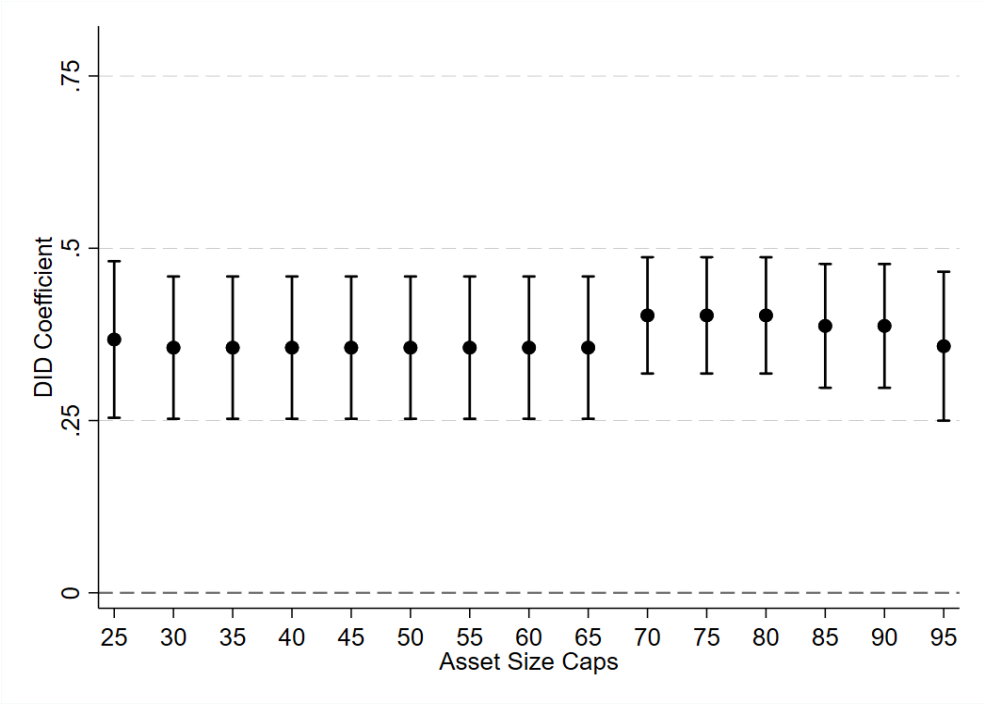


Figure A5. Dynamic Trend of the Impact of Disclosure on Rejection Rate

This figure represents the yearly coefficient estimates for racial disparities in rejection rates among treated lenders relative to control lenders. The illustration contains outcomes from three years before 2015 - the starting year of the disclosure policy - and continues through three years after its application, that is, between 2012 and 2018. Samples outside of this chosen span are grouped into the initial and final periods. During the estimation, the first year prior to the disclosure is used as the baseline period. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The Y-axis reflects the magnitude of the racial disparities in rejection rates, while the X-axis signifies varying periods, with period zero corresponding to the disclosure policy's implementation. The regression specification used to generate this figure is consistent with that of column (2) in Table V. Black circles symbolize the coefficient estimates for different periods, and the black vertical lines denote the 95% confidence intervals.

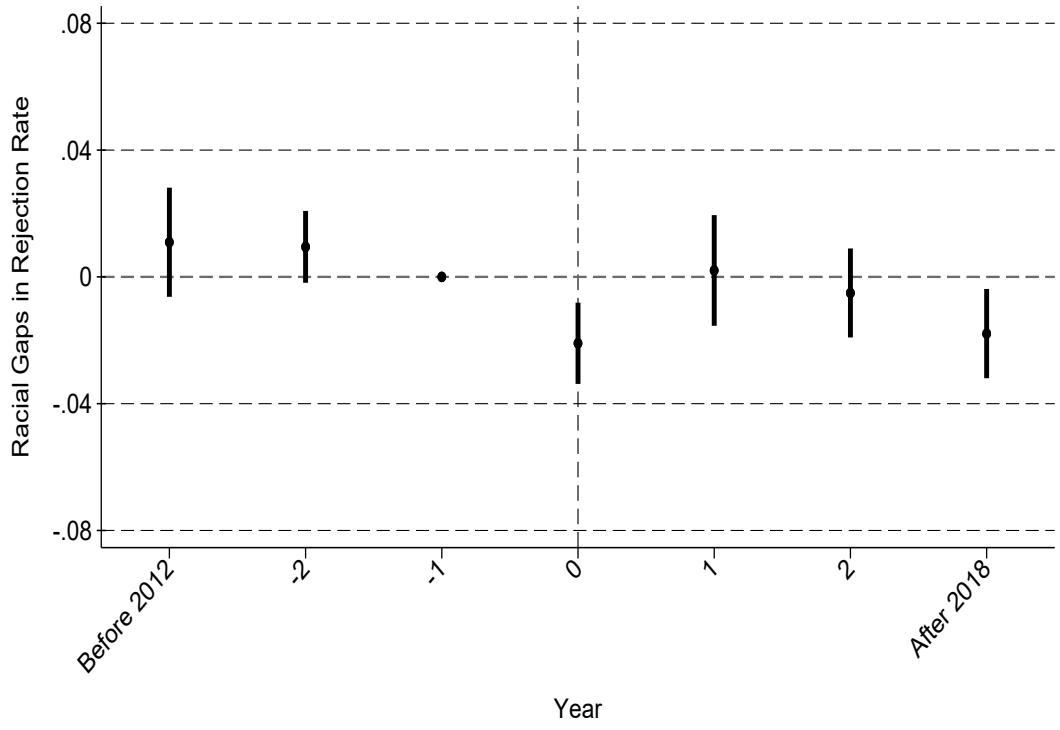


Figure A6. Trend in the Number of Complaints of Mortgage Loan

This figure illustrates the trends in the number of mortgage loan complaints from 2012 to 2019. The y-axis represents the total number of complaints, while the x-axis is divided by months, with corresponding year labels indicated. The red vertical line marks the June 2015 milestone, when the CFPB publicly released the narratives of consumer complaints.

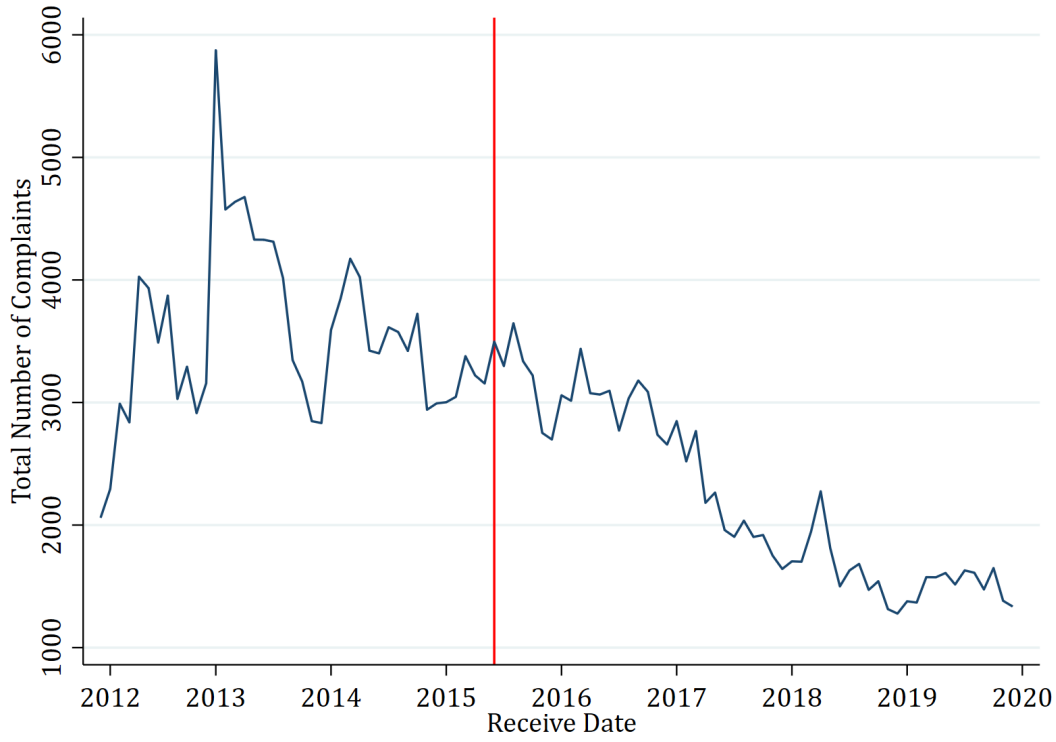
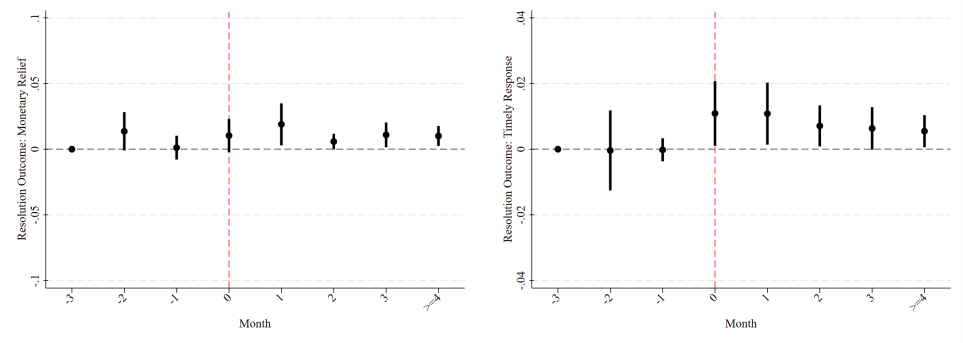


Figure A7. Dynamic Trend of the Impact of Disclosure on Complaints' Resolution Outcomes

This figure presents the results of an event study examining the impact of the CFPB disclosure policy on complaint resolution outcomes. It focuses on two key metrics: (a) Monetary Relief, which indicates whether complaints submitted to the CFPB lead to financial compensation for the complainant, and (b) Timely Response, which evaluates the promptness of the institution's response. The analysis spans a period from three months before the implementation of the disclosure policy on June 25, 2015, to the end of 2016. Observations beyond the four-month post-implementation window are aggregated into the final period. The estimation process uses the third month before the disclosures as the reference period. The vertical axis represents the magnitude of the coefficients for the double-interaction terms, while the horizontal axis denotes the various periods, with period zero marking the time of disclosure. The regression specification employed to produce this figure aligns with Table VI.. The black circles symbolize the coefficient estimates for each period, and the black vertical lines indicate the confidence intervals at a 10% significance level.



(a) Monetary Relief (b) Timely Response

Figure A8. An Example of Rankings for Banks' Discrimination Based on Racial Disparities in Interest Rates

This figure displays discrimination in interest rates from the Discrimination Warrior website pertaining to zip code 02135, which is a region in Boston. Visitors to the site can select either interest rates or rejection rates and enter the relevant zip code. Upon entering, the website sequentially presents results showing the extent of discrimination against minorities by banks, ranked in order of severity.

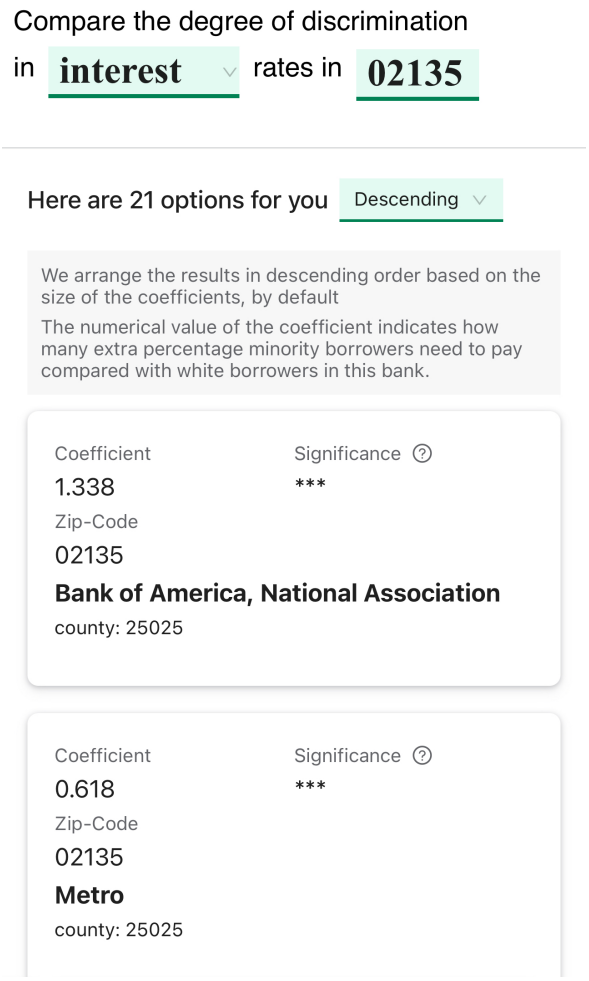


Figure A9. An Example of Rankings for Banks' Discrimination Based on Racial Disparities in Rejection Rates

This figure displays discrimination in rejection rates from the Discrimination Warrior website pertaining to zip code 02135, which is a region in Boston. Visitors to the site can select either interest rates or rejection rates and enter the relevant zip code. Upon entering, the website sequentially presents results showing the extent of discrimination against minorities by banks, ranked in order of severity.

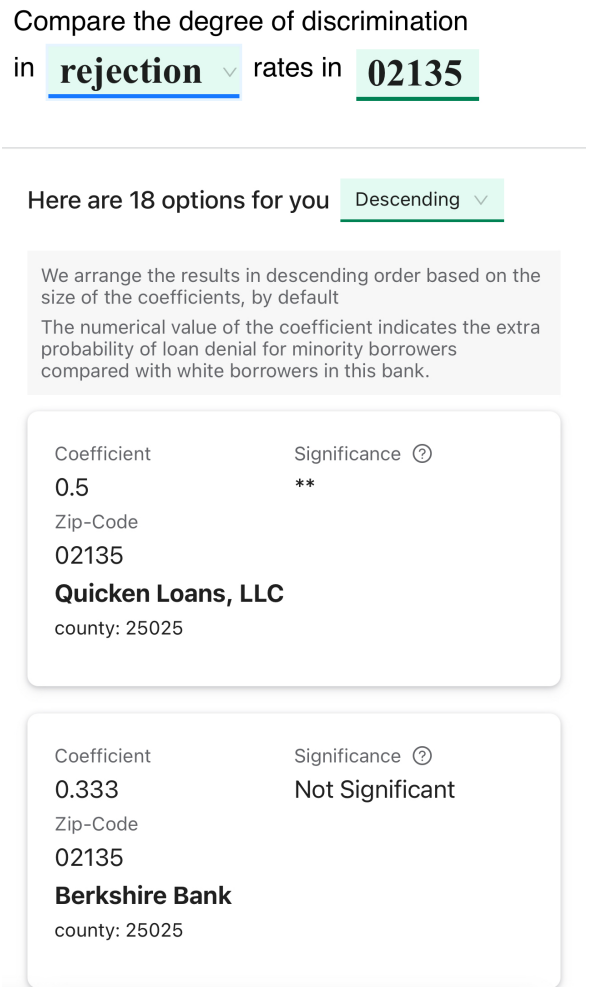


Table A1. Discrimination Complaint Example

This table displays a consumer complaint example concerning "Applying for a mortgage or refinancing an existing mortgage," as publicly disseminated by the CFPB. This includes components such as "Date received", "Product", "Consumer complaint narrative", "Company", and "Company response to consumer", among others.

Date received	2021/1/28
Product	Mortgage
Subproduct	Conventional home mortgage
Issue	Applying for a mortgage or refinancing an existing mortgage
Consumer complaint narrative	I was denied a mortgage loan from Bank of America for a property in XXXX XXXX, NJ on XX/XX/2021. I haven't received written confirmation yet, but the verbal reasoning is due to my employment history and employment gaps. The loan officer sounded very condescending when she told me that I was denied. It doesn't make sense to me to be denied for that reason alone as my employment history was stated on Day 1 and I was pre-qualified for the loan. To make matters worse, I was denied after having an appraisal done on the property so I was fairly far into the process with a refund unlikely for the $570.00 I was charged for the appraisal. I believe that I am being discriminated against because I disclosed my race as XXXX on Section X of the XXXX loan application. I would greatly appreciate it if this could be looked into to ensure that Bank of America didn't discriminate against me by showing that they also denied mortgage loans to people of other races, particularly XXXX people, with similar credit, income or debt-to-income ratio, savings, educational, and employment backgrounds as me. Quick summary of my background: I have excellent credit, my credit score is over XXXX. My 2 employment gaps greater than 30 days were related to school. I have a XXXX XXXX XXXX and currently in XXXX XXXX seeking a XXXX. I work full time as a mortgage loan advisor where I earn over $45000.00 annually. I have savings of $30000.00. The house I was looking to purchase cost $180000.00.
Company	BANK OF AMERICA, NATIONAL ASSOCIATION
State	PA
Submitted via	Web
Company response to consumer	Closed with monetary relief
Company disputed	No

Table A2. Variable Definitions

Variable	Definition
HMDA LARS 2011-2019	
Rejection	An indicator variable indicating whether a loan was approved at application, marked as 1 for approval, and 0 otherwise.
Application Number	A continuous variable indicating the number of applications received by a ZIP code in different years.
Minority	An indicator variable indicating if the loan applicant is a minority, marked as 1 for Latinx or African American, and 0 for Asian or White.
Gender	An indicator variable indicating if the loan applicant is a man, marked as 1 for male, and 0 for female.
Loan Amount	A continuous variable indicating the amount of a particular loan.
Income	A continuous variable indicating the income level of the loan applicant.
Co-applicant	An indicator variable indicating whether the loan has a co-applicant or not.
Census Tract	A categorical variable indicating the 11-digit code of the Census Tract of a loan.
Zip Code	A categorical variable indicating the 5-digit zip code of the bank's location.
County	A categorical variable indicating the 5-digit county code of the bank's location.

Continued on next page

Table A2 – *Continued from previous page*

Variable	**Definition**
State	A categorical variable indicating the 2-digit state code where the bank is situated.
GSE 2011-2019	
Interest Rate	A continuous variable indicating the interest rate of a loan
Default	An indicator variable represents if a loan has not been repaid within a designated term. It is marked as 1 if the duration of default exceeds 90 days, and 0 otherwise.
LTV Group	An indicator variable categorizing the continuous Loan-To-Value (LTV) into eight divisions (effectively seven). This classification follows the method of Bartlett et al. (2022).
Credit Score Group	An indicator variable categorizing the continuous Credit Score into eight sections. The categorization is in line with Bartlett et al. (2022).
Loan Purpose	An indicator variable indicating the category of a loan, with the primary types being Cash-out Refinance, Purchase, and Refinance in this study.
Other Sources	
Complaints Number	A continuous variable indicating the number of complaints a bank received in 2015, with data gathered from the CFPB dataset.

Continued on next page

Table A2 – *Continued from previous page*

Variable	**Definition**
Penalty	A continuous variable indicating the amounts of fines related to discrimination lawsuits a bank face in a given year, with data gathered from the CFPB website.
Google Index Diff	A continuous variable indicating the increase in the Google Index for a state in 2015 relative to 2014, where Google Index refers to the search volume for the CFPB.
Population	A continuous variable indicating the population of a county in different years, sourced from the Census dataset.
Total Deposits	A continuous variable measuring the total deposits of a bank in different years, sourced from SOD.
Total Assets	A continuous variable indicating the total assets of a bank sourced from WRDS.
Equity	A continuous variable indicating the equity of a listed bank sourced from WRDS.
Net Profit	A continuous variable indicating the net profit of a listed bank sourced from WRDS.
CAR (CAPM)	A continuous variable indicating the cumulative abnormal returns of a listed bank, sourced from WRDS.
CAR (Market)	A continuous variable indicating the market-adjusted returns of a listed bank, sourced from WRDS.

Table A3. Summary Statistics for the Main Datasets

This table presents summary statistics for the key variables drawn from the main datasets utilized in this paper. Panel A shows the summary statistics results for the HMDA-GSE merged dataset, while panel B provides the summary statistics for the HMDA origination dataset. These datasets enable me to identify the effects of the disclosure policy on interest rates, default rates, and rejection rates, along with other extended analyses. Within both panels, I report the number of observations, mean, standard deviation, minimum, and maximum values for the main variables. The time frame for these samples spans from 2011 to 2019, with a restriction that banks in the samples have total assets of less than 100 billion dollars, consistent with the treatment in the main text.

Panel A: HMDA-GSE Merged Dataset

	(1)	(2)	(3)	(4)	(5)
Variables	N	Mean	SD	Min	Max
Minority	1,330,621	0.111	0.314	0	1
Interest Rate	1,330,621	4.299	0.499	3.375	5.625
Default Rate	1,330,621	0.081	0.273	0	1
LTV	1,330,621	77.855	14.414	30	95
Credit Score	1,330,482	747.941	45.054	634	816
Gender	1,330,621	0.679	0.467	0	1
Income	1,297,045	101.048	69.420	0	5,602
Loan Amount	1,330,621	259.205	128.733	40	1,375
Co-applicant	1,330,621	0.478	0.500	0	1
Cash-out Refinance	1,330,621	0.197	0.398	0	1
Purchase	1,330,621	0.569	0.495	0	1
Refinance	1,330,621	0.234	0.423	0	1

Panel B: HMDA Origination Dataset

	(1)	(2)	(3)	(4)	(5)
Variables	N	Mean	SD	Min	Max
LTV (Census Tract)	31,634,238	78.348	9.935	42	95
Credit Score (Census Tract)	31,634,238	754.214	31.365	659	810
Gender	31,634,238	0.698	0.459	0	1
Income	31,066,586	108.162	80.602	16	539
Loan Amount	31,632,905	250.134	159.282	29	1,000
Co-applicant	31,634,238	0.498	0.500	0	1
Purchase	31,634,238	0.427	0.495	0	1
Refinance	31,634,238	0.573	0.495	0	1

Table A4. The Impact of Disclosure on Interest Rate (Clustered at the Lender Level)

This table reports results on the effect of complaint narrative disclosure on the racial gaps in interest rates, with standard errors clustered at the level of the lender. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. Each column provides estimated coefficients on four independent variables, with other coefficient groups (*Treat*, *Post*, and their interaction term *Treat×Post*) being absorbed by fixed effects due to collinearity. The key estimated coefficient, *Treat×Post×Minority*, denotes the effect of disclosure on racial gaps in interest rates. In column (1), fixed effects for the interaction of lender and time, and pricing grid-loan purpose-time joint fixed effects are controlled. Column (2) incorporates controls for borrower characteristics. Column (3) combines the two fixed effects from column (1) into lender-pricing grid-loan purpose-time joint fixed effects. Column (4), building on column (3), replaces borrower characteristics with the more stringent similar borrower fixed effects. Column (5) introduces joint fixed effects between lenders and similar borrowers, based on column (2). Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var =	Interest Rate				
Treat×Post×Minority	-0.022**	-0.023**	-0.026**	-0.025**	-0.026**
	(0.010)	(0.010)	(0.011)	(0.011)	(0.012)
Post×Minority	-0.001	0.002	-0.001	-0.001	0.001
	(0.005)	(0.005)	(0.006)	(0.005)	(0.005)
Treat×Minority	-0.001	-0.008	-0.000	-0.003	-0.001
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Minority	0.028***	0.025***	0.027***	0.029***	0.029***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Lender×Year-Month FE	YES	YES			YES
Bucket×Loan- Purpose×Year-Month FE	YES	YES			YES
Lender×Bucket×Loan-Purpose×Year-Month FE			YES	YES	
Borrower Characteristics FE		YES	YES		
Similar Borrower FE				YES	
Lender×Similar Borrower FE					YES
Observations	1,305,735	1,273,854	1,031,962	1,023,226	803,944
R-squared	0.756	0.772	0.799	0.808	0.816

Table A5. Replication - Interest Rate Differentials

This table replicates Table 3 in Bartlett et al. (2022). The utilized data are sourced from the HMDA-GSE merged dataset for the years 2011-2019, with the same model specification as in Bartlett et al. (2022). The dependent variable is the interest rate associated with originated fixed-rate mortgages. The independent variable is assigned a value of 1 if the borrower is Black or Latinx, and 0 in other cases. Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	Purchase	Refinance
Dep. Var =	Interest Rate	
Minority Borrower	0.036***	0.014***
	(0.004)	(0.003)
Lender×Year-Month FE	YES	YES
Bucket×Loan Purpose×Year-Month FE	YES	YES
Amount decile FE	YES	YES
Observations	1,034,355	792,779
R-squared	0.755	0.762

Table A6. Heterogeneous Impact of Disclosure Policy on Interest Rates by Lender Minority Share and Credit Score Categories

This table presents the disclosure policy's effects on interest rates across varying levels of the minority share of the lender's loan portfolio and credit score groups. The analysis uses HMDA-GSE merged data for 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The key explanatory variable is the triple interaction term *Treat* × *Post* × *Minority*, measuring the disclosure policy's effect on racial disparities in interest rates. Panel A compares interest rates between lenders with low and high minority borrower shares. Panel B extends the analysis to diverse credit score groups. The table employs lender-year-month and bucket-loan purpose-year-month joint fixed effects as well as borrower characteristics fixed effects. Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

Panel A. Heterogeneity Analysis of Firm Minority Share

	(1)	(2)
	Low share	High share
Dep. Var =	Interest Rate	
Treat×*Post*×*Minority*	-0.018***	-0.078**
	(0.003)	(0.032)
Lender×Year-Month FE	YES	YES
Bucket×Loan Purpose×Year-Month FE	YES	YES
Borrower Characteristics FE	YES	YES
Observations	719,358	553,157
R-squared	0.792	0.736

Panel B. Heterogeneity Analysis of Credit Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var =	Interest Rate						
Treat×*Post*×*Minority*	-0.148***	0.042	0.012	-0.038***	-0.039*	-0.030	-0.019**
	(0.042)	(0.000)	(0.018)	(0.008)	(0.019)	(0.016)	(0.006)
Lender×Year-Month FE	YES	YES	YES	YES	YES	YES	YES
Bucket×Loan Purpose×Year-Month FE	YES	YES	YES	YES	YES	YES	YES
Borrower Characteristics FE	YES	YES	YES	YES	YES	YES	YES
Observations	15,700	29,250	48,780	89,785	121,104	143,020	775,687
R-squared	0.740	0.731	0.726	0.716	0.721	0.742	0.768

Table A7. The Effect of Disclosure Policy on Interest Rates with Various Fixed Effects

This table extends the analysis from Table II, examining the impact of disclosure on interest rates and incorporating various fixed effects to verify result robustness. The analysis uses HMDA-GSE merged data for 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The key explanatory variable is the triple interaction term *Treat* × *Post* × *Minority*, measuring the disclosure policy's effect on racial disparities in interest rates. Panels A, B, and C employ state-year, zip code-year, and census tract-year joint fixed effects respectively, providing increasingly localized perspectives. Across all panels, the columns employ different fixed effects strategies as follows: Column (1) controls for lender-time interaction and pricing grid-loan purpose-time joint fixed effects; Column (2) adds borrower characteristics; Column (3) combines column (1) fixed effects into lender-pricing grid-loan purpose-time joint fixed effects; Column (4) replaces borrower characteristics with similar borrower fixed effects; and column (5) introduces lender-similar borrower joint fixed effects based on column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var =	Interest Rate				
Panel A. The Impact of Disclosure on Interest Rate with State×Year FE					
Treat×Post×Minority	-0.021***	-0.023***	-0.027***	-0.026***	-0.025*
	(0.006)	(0.006)	(0.005)	(0.005)	(0.012)
State×Year FE	YES	YES	YES	YES	YES
Observations	1,305,735	1,273,854	1,031,962	1,023,226	803,941
R-squared	0.759	0.774	0.801	0.811	0.819
Panel B. The Impact of Disclosure on Interest Rate with Zipcode×Year FE					
Treat×Post×Minority	-0.030***	-0.031***	-0.040***	-0.038***	-0.036**
	(0.006)	(0.005)	(0.004)	(0.005)	(0.014)
Zipcode×Year FE	YES	YES	YES	YES	YES
Observations	1,276,328	1,244,272	997,712	988,428	765,448

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Table A7 – *Continued from previous page*

	(1)	(2)	(3)	(4)	(5)
R-squared	0.785	0.797	0.825	0.834	0.847
Panel C. The Impact of Disclosure on Interest Rate with Census Tract×Year FE					
Treat×*Post*×*Minority*	-0.033***	-0.030***	-0.042***	-0.036***	-0.035***
	(0.007)	(0.007)	(0.009)	(0.007)	(0.010)
Census Tract×Year FE	YES	YES	YES	YES	YES
Observations	1,177,363	1,144,897	887,700	876,715	642,999
R-squared	0.816	0.827	0.855	0.865	0.882
Lender×Year-Month FE	YES	YES			YES
Bucket×Loan Purpose×Year-Month FE	YES	YES			YES
Lender×Bucket×Loan Purpose×Year-Month FE			YES	YES	
Borrower Characteristics FE		YES	YES		
Similar Borrower FE				YES	
Lender×Similar Borrower FE					YES

Table A8. Racial Disparities in Discount Points

This table examines the difference in racial disparities concerning discount points between the treated and control groups. The data used are derived from the HMDA-GSE merged dataset for the years 2018-2019. The dependent variable is the discount points chosen by borrowers. The calculated outcome of *Treat×Minority* in the table tests whether minority borrowers in the treat and control groups exhibit different preferences for discount points. In column (1), fixed effects for the interaction of lender and time, and pricing grid-loan purpose-time joint fixed effects are controlled. Column (2) incorporates controls for borrower characteristics. Column (3) combines the two fixed effects from column (1) into lender-pricing grid-loan purpose-time joint fixed effects. Column (4), building on column (3), replaces borrower characteristics with the more stringent similar borrower fixed effects. Column (5) introduces joint fixed effects between lenders and similar borrowers, based on column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var =	Discount Point				
Treat×Minority	-32.608	4.682	28.480	7.167	12.367
	(42.508)	(0.000)	(29.802)	(34.008)	(41.194)
Control	YES	YES	YES	YES	YES
Lender×Year-Month FE	YES	YES			YES
Bucket×Loan-Purpose×Year-Month FE	YES	YES			YES
Lender×Bucket×Loan-Purpose×Year-Month FE			YES	YES	
Borrower Characteristics FE		YES	YES		
Similar Borrower FE				YES	
Lender×Similar Borrower FE					YES
Observations	260,127	249,639	206,534	195,851	121,219
R-squared	0.360	0.447	0.517	0.575	0.630

Table A9. Moderation Effect of the Impact of Disclosure on Interest Rates: Litigation Risk

This table presents findings on the moderating effect of disclosure on racial disparities in interest rates, with data drawn from the HMDA-GSE merged dataset spanning the years 2011-2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The table employs the continuous variable *Litigation* as the moderating variable, with the first two columns discussing the number of litigations and the last two discussing the penalty amounts (in one million dollars). Columns (1) and (3) control for joint fixed effects of lender-pricing grid-loan purpose-time as well as borrower characteristics fixed effects, whereas columns (2) and (4) replace borrower characteristics with more stringent similar borrower fixed effects. Standard errors in this table, indicated in parentheses, are clustered at the lender and year-month level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Number		Penalty	
Dep. Var =	Interest Rate			
Treat×*Post*×*Minority*×*Litigation*	-0.063***	-0.047**	-0.012***	-0.010***
	(0.012)	(0.019)	(0.003)	(0.004)
Lender×Bucket×Loan Purpose×Year-Month FE	YES	YES	YES	YES
Borrower Characteristics FE	YES		YES	
Similar Borrower FE		YES		YES
Observations	972,054	963,349	972,054	963,349
R-squared	0.795	0.805	0.795	0.805

Table A10. Summary of CFPB Litigation Penalty

Bank Name	**Date Filed**	**Civil Money Penalty**	**Loan Subsidy Program Investment**
Washington Federal	9/10/2013	$34,000	-
Mortgage Master, Inc.	9/10/2013	$425,000	-
National City Bank	23/12/2013	-	-
Provident Funding Associates, L.P.	28/5/2015	-	-
Hudson City Savings Bank, F.S.B.	24/9/2015	$5.5 million	$25 million
BancorpSouth Bank	29/6/2016	$3 million	$4 million
Nationstar Mortgage LLC	15/3/2017	$1.75 million	-
Freedom Mortgage Corporation	5/6/2019	$1.75 million	-
Townstone Financial, Inc. and Barry Sturner	15/7/2020	-	-

Table A11. Dynamic Trend of the Impact of Disclosure on Default Rates

This table represents the coefficient estimates for racial disparities in default rates among treated lenders relative to control lenders. The results contain outcomes from five quarters before the second quarter of 2015 - the starting point of the disclosure policy - and continue through eleven quarters after its application. Samples outside of this chosen span are grouped into the initial and final periods. During the estimation, the fifth quarter prior to the disclosure and earlier periods are used as the baseline period. For brevity, I combine three quarters into one bin. The data come from the HMDA-GSE merged dataset spanning 2011-2019. The period zero corresponds to the disclosure policy's implementation. The regression specification used to generate this table is consistent with that of column (2) in Table IV. Standard errors, shown in parentheses, are clustered at the lender and year-quarter level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)
	Default Rate
$Minority \times Treat \times Year$ (-4, -2)	-0.195
	(0.198)
$Minority \times Treat \times Year$ (-1)	-0.055
	(0.164)
$Minority \times Treat \times Year$ (0)	-0.385
	(0.221)
$Minority \times Treat \times Year$ (1)	0.657***
	(0.166)
$Minority \times Treat \times Year$ (2, 4)	0.319*
	(0.171)
$Minority \times Treat \times Year$ (5, 7)	0.188
	(0.570)
$Minority \times Treat \times Year$ (8, 10)	0.251
	(0.145)
$Minority \times Treat \times Year$ ($\geq$ 11)	0.170
	(0.161)
Control	Yes
Lender × Year-Month FE	Yes
Zip × Year FE	Yes
Observations	24,173
R-square	0.500

Table A12. The Impact of Disclosure on Incomes

This table reports results on the effect of complaint narrative disclosure on the racial gaps in incomes. The data come from the HMDA origination dataset spanning 2011-2019. The dependent variable is the loan-level income status. Each column provides estimated coefficients on four independent variables, with other coefficient groups (*Treat*, *Post*, and their interaction term *Treat*×*Post*) being absorbed by fixed effects due to collinearity. The key estimated coefficient, *Treat*×*Post*×*Minority*, denotes the effect of disclosure on racial gaps in incomes. The first pair of columns present the fundamental results, where I account for lender-by-year fixed effects, lender-similar borrower combined fixed effects, and lender-census tract combined fixed effects in column (1), whereas I also include census tract-by-year fixed effects in column (2). Standard errors, shown in parentheses, are clustered at the lender and year level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dep. Var =	Income	
$Treat \times Post \times Minority$	-0.017	-0.008
	(0.019)	(0.018)
$Post \times Minority$	-0.003	0.003
	(0.017)	(0.014)
$Treat \times Minority$	0.044	0.041
	(0.031)	(0.030)
$Minority$	-0.217***	-0.219***
	(0.007)	(0.006)
Lender × Year FE	YES	YES
Lender × Similar Borrower FE	YES	YES
Lender × Census Tract FE	YES	YES
Census Tract × Year FE		YES
Observations	25,516,856	25,514,347
R-squared	0.657	0.664

Table A13. Rival Bank Reaction to the Disclosure - Another *Share* Measurement

This table reports results on the effect of complaint narrative disclosure on the entry of rival banks, utilizing an independent variable that differs from the one used in Table IX. The data, sourced and consolidated from the HMDA origination dataset for the years 2011 to 2019, are aggregated at the county-year level. The dependent variable, *BankDensity*, is calculated as the number of unique bank brands per ten thousand individuals at the county level. The table showcases the estimated results for the *Amount Share*×*Post* independent variable, with other coefficient groups (*Amount Share* and *Post*) being subsumed by fixed effects due to collinearity. This coefficient identifies changes in the unique bank brand number per ten thousand people in counties affected by the disclosure. Unlike Table IX, I use the measure *Amount Share*, defined as the market share held by treated banks within each county in 2015, to evaluate the extent of exposure to the effects of disclosure. In all columns, I account for the growth rate of income per capita and unemployment rates and control for county-level fixed effects and state-by-year joint fixed effects. Based on the *Amount Share*, columns (1) to (3) display variations in the quantity of unique banks per ten thousand individuals in the present period (t), the next year (t+1), and two years later (t+2). Standard errors, shown in parentheses, are clustered at the county level. The symbols *, **, and *** correspond to significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	T	T+1	T+2
Dep. Var =	Bank Density		
AmountShare	7.334***	10.685***	11.526***
×*Post*	(2.219)	(2.533)	(2.798)
Control	YES	YES	YES
State×Year FE	YES	YES	YES
County FE	YES	YES	YES
Observations	21,503	18,341	15,998
R-squared	0.759	0.782	0.793

Table A14. Financial Regulatory Authorities in Developed Countries

Country	Financial Regulatory Authority	Specifically Established for Consumer Protection	Formed Year	Receiving Complaints	Starting Year of Receiving Complaints	Complaints Disclosure
Australia	Australian Financial Complaints Authority	Yes	2018	Yes	2018	No
Austria	Austrian Financial Market Authority	No	2002	Yes	2002	No
Belgium	Financial Services and Markets Authority	Yes	2011	Yes	2011	No
Canada	The Financial Consumer Agency of Canada	Yes	2001	Yes	2001	No
Croatia	Croatian Financial Services Supervisory Agency	No	2005	Yes	2005	No
Czech Republic	Czech National Bank	No	1993	Yes	2021	No
Denmark	Danish Financial Complaint Board	Yes	2015	Yes	2015	No
Finland	Finnish Financial Ombudsman Bureau	Yes	2009	Yes	2009	No
France	Autorité de Contrôle Prudentiel et de Résolution	No	2010	No	-	-

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Table A14 – *Continued from previous page*

Country	Financial Regulatory Authority	Specifically Established for Consumer Protection	Formed Year	Receiving Complaints	Starting Year of Receiving Complaints	Complaints Disclosure
Germany	Federal Financial Supervisory Authority	No	2002	Yes	2002	No
Hungary	Hungarian National Bank	No	1924	Yes	2020	No
Iceland	Complaints Committee on Transactions with Financial Firms	Yes	2022	Yes	2022	No
Ireland	Financial Services and Pensions Ombudsman	Yes	2017	Yes	2018	No
Italy	Institute for the Supervision of Insurance	No	2013	Yes	2013	No
Japan	Supervision Bureau	No	2000	No	-	-
Korea	Financial Supervisory Service	No	1998	Yes	2011	No
Netherlands	Netherlands Authority for the Financial Markets	No	2002	Yes	2002	No
New Zealand	Financial Markets Authority	No	2011	Yes	2011	No

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Table A14 – *Continued from previous page*

Country	Financial Regulatory Authority	Specifically Established for Consumer Protection	Formed Year	Receiving Complaints	Starting Year of Receiving Complaints	Complaints Disclosure
Norway	Norwegian Financial Services Complaints Board	Yes	2014	Yes	2014	No
Portugal	Bank of Portugal	No	1846	Yes	2020	No
Singapore	Financial Industry Disputes Resolution Centre	Yes	2005	Yes	2005	No
Slovakia	National Bank of Slovakia	No	1993	Yes	2020	No
Slovenia	Financial Consumer Protection Department	Yes	2015	Yes	2015	No
Spain	Bank of Spain	No	1782	Yes	2004	No
Switzerland	Swiss Banking Ombudsman	Yes	1993	Yes	1993	No
United Kingdom	Financial Ombudsman Service	Yes	2001	Yes	2001	No
United States	**Consumer Financial Protection Bureau**	**Yes**	**2011**	**Yes**	**2011**	**Yes**

Table A15. Impact of CFPB Disclosure on Complaints' Resolution Outcomes

This table presents the findings regarding the impact of CFPB disclosure on the resolution outcomes of complaints. The dataset employed for estimating this table comprises complaint-level data spanning from March 19, 2015, to December 31, 2016. The table concentrates on two resolution outcomes: Monetary Relief, a dummy variable that assumes a value of one if the complaint is resolved with monetary compensation for the consumer, and Timely Response, a dummy variable that takes a value of one if the institution's response is prompt. Concerning the independent variables, three crucial elements are incorporated into the difference-in-difference model: $Treat$, $Post$, and $Treat \times Post$. The key estimated coefficient, $Treat \times Post$, signifies the impact of disclosure on the resolution outcomes of complaints. All columns account for institution, zip, and year-month fixed effects. Standard errors, displayed in parentheses, are double clustered at both the institution and year-month levels. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dep. Var =	Monetary Relief	Timely Response
Treat×*Post*	0.006*	0.006***
	(0.003)	(0.002)
Treat	0.007*	-0.006**
	(0.004)	(0.002)
Post	-0.001	-0.003
	(0.002)	(0.002)
Institution FE	YES	YES
Zip FE	YES	YES
Year-Month FE	YES	YES
Observations	62,096	62,096
R-squared	0.225	0.421

Appendix B. Clarifications on the Process of Merging Datasets

HMDA-GSE Merged Dataset: Linkage Process

My research utilized two datasets, namely the Home Mortgage Disclosure Act (HMDA) and Government-Sponsored Enterprises (GSE), which contain valuable information about loan origination. Unfortunately, there is currently no commonly available mapping file that connects these two datasets. Following Law and Mislang (2022), I adopt the "fuzzy data matching" methods to overcome this challenge. These methods leverage the substantial overlap in variables shared by both datasets. When the information in these overlapping fields demonstrates consistent patterns between the two datasets, my matching process becomes efficient.

To establish a connection between the HMDA and GSE datasets, I make use of the tools provided by the *Fedmatch* R package developed by Cohen et al. (2021). This specific matching approach involves separating each dataset based on geography and lender ID. Subsequently, I apply the matching algorithm at the loan level.

Regarding the initial step, the GSE dataset includes lender names, which I integrated into the HMDA dataset using the Robert Avery file. However, for small lenders whose names present challenges in recognition within the GSE dataset, I employed accurate name matching techniques based on Jaccard string similarity and manual proofreading. Once I successfully match the lender IDs, I discuss the loan matching process.

To enhance the accuracy of the matching process, I divide each dataset into smaller partitions based on lender ID and geographic information. However, it's important to note that there is no exact correspondence between the two datasets in terms of geography. I use 3-digit zip codes to correspond to HMDA's census tracts and counties. In the "grid" formed by lender ID and geographic information, I match each loan based on observable characteristics at the time of loan origination. Furthermore, it's worth mentioning that certain variables

in the GSE dataset have undergone changes since 2018. As a result, I have several additional variables available for use as matching references, including interest rate, manufactured property status, and loan purpose.

HMDA-GSE Merged Dataset: Filtering Process

To omit loans not conforming to standard lending criteria, I implement filtering on this dataset according to Bartlett et al. (2022), resulting in a final sample of 1,869,345 mortgage loans. During the filtering process, I remove GSE loans with credit scores below 620 (10,310 observations), LTVs under 0.3 (76,779 observations), and above 0.95 (121,430 observations). I also remove loans with interest rates below 2.75% or higher than 8% (10,273 observations), loans amounting to less than $40,000 (14,207 observations), and loans whose term is not equal to 360 months (769,736 observations). Thus, the final sample includes 56.53% (1,056,807 observations) of purchase mortgages and 43.47% (812,538 observations) of refinance loans.

Please note that for the actual analysis, I utilize the subsample from this final collection, where total assets range from $0 to $100 billion to perform the primary analysis. This subsample consists of 1,330,621 loans, accounting for 71.18% of the total sample. This refined sample allows for a more accurate and comprehensive examination of the mortgage market, thereby aiding in comprehending borrower and lender attributes, loan conditions, as well as the impact of the disclosure on pricing and approval rates.

HMDA Origination Dataset: Variable Supplementation Process

Besides dictating the loan interest rate, the lender also possesses the authority to either endorse or dismiss a loan application entirely. After the 2008 mortgage crisis, numerous lenders have imposed their personalized, more rigorous, approval stipulations over and above those stipulated by the GSE. Hence, although a loan application may garner creditworthiness approval within

the GSE underwriting system, the lender retains the privilege to refuse the application.

A significant caveat is that I lack loan-level data on several crucial underwriting variables, such as the borrower's credit score or LTV ratio, since rejected loans are never issued. These data constraints imply that, unlike the interest-rate analysis in the main paper, any analysis of rejection rates is invariably subject to the omitted variable problem addressed in the introduction part. I cannot be completely confident that any discrepancies in rejection rates are attributable to discrimination rather than differences in unobservable variables. Nonetheless, the HMDA data permit me to control for loan-level lender, year, borrower income, and loan amount. In addition, I employ proxies for these in my analysis using the median credit scores and LTVs of the census tract, which I estimate using the merged HMDA-GSE data. Controlling for these variables, along with census tract level median credit scores and LTVs, mitigates the omitted-variable problem (Bartlett et al. (2022)). I also apply the same filter to the loan amount and loan term that is used for variables in the HMDA-GSE merged dataset.[1]

Moving towards the matching process, I carry out it in three stages. In the first stage, I retain the census tract information, time information (i.e., year), LTV, and credit score information in the HMDA-GSE dataset. Consequently, I obtain the applicants' LTV and credit scores for multiple loan records per census tract per year. I then calculate the median of LTV and credit scores at the census tract level (Bartlett et al. (2022)). I maintain the two median variables produced here along with the census tract and year, and I can then easily aggregate the data to the census tract-year level.

Subsequently, I match the LTV and credit score indicators at the census tract-year level back to the original HMDA using the census tract and year. Since LTV and credit score information primarily come from the GSE, and this database only includes records of loan applications that were not rejected, I cannot find corresponding values in the original HMDA dataset for about

[1]Before 2018, the HMDA origination datasets do not include the loan term variable, so I employ the filter for loan term only in the years after 2018.

21.65% of the samples in this step.

Finally, to discuss the impact of complaints disclosure in more detail, I match the number of bank complaints published by the CFPB in 2015 and the total bank assets values from the Call Report database for the first quarter of 2015 (Fuster et al. (2021)) into the aforementioned dataset using the arid (an identification number of banks that is defined by CFPB) and year,[2] to obtain the revised HMDA dataset to analyze the influence of disclosure on rejection. The final dataset comprises 42,198,230 loans, with 31,634,238 observations (accounting for 74.97% of the full sample) belonging to lenders whose total assets range from $0 to $100 billion.

[2]According to the CFPB, the arid is created by concatenating an institution's agency code, as defined by the CFPB, with the Respondent ID of an institution. The agency code is an integer that corresponds to the institution's regulatory agency, ranging from 1 to 9. Respondent ID is an institution identity, one of the following: OCC charter number, FDIC certificate number, NCUA charter number, National Information Center (NIC) RSSD, or federal tax ID. For further details, please visit: `https://ffiec.cfpb.gov/documentation/publications/loan-level-datasets/panel-data-fields-v1andhttps://ffiec.cfpb.gov/documentation/faq/identifiers-faq`.

Appendix C. CFPB Disclosure and Complaints' Resolution Outcomes

In the main text, I examine how disclosure disciplines lenders to improve service quality in order to reduce the occurrence of complaints beforehand. Furthermore, this section tests how disclosure affects lenders' appeasement strategies for dealing with complaints after they occur by using the CFPB complaints dataset.

Following the CFPB's disclosure of consumer complaints, it is hypothesized that lenders may improve their resolution outcomes to ensure customer satisfaction. This potential improvement could be driven by the need to maintain a positive public image, enhance consumer trust, comply with regulatory standards, and gain a competitive advantage. By addressing complaints effectively, lenders might aim to avoid negative publicity, retain customers, and demonstrate their commitment to high service standards, thereby fostering a better overall reputation and regulatory compliance.

Motivated by this hypothesis, I explore the impact of publicly disclosing complaint narratives on the resolution outcomes of complaints filed with the CFPB. Utilizing data from the CFPB complaints dataset, the research spans the period from March 19, 2015, to December 31, 2016, and employs a difference-in-difference approach for analysis.

As of March 19, 2015, consumers are granted the choice to make their complaint narratives publicly available when lodging complaints with the CFPB. Nevertheless, the CFPB refrains from revealing these narratives to the public until June 25, 2015, allowing regulated financial institutions a grace period. The CFPB disclosure policy officially commences on June 25, 2015, when the bureau begins openly sharing complaint narratives. As a result, the pre-disclosure phase in this study is restricted to the period between March 19 and June 25, 2015. During this timeframe, consumers submitting complaints to the CFPB encounter identical circumstances as those filing complaints after June 25, 2015. This arrangement aids in alleviating self-selection bias on the part of consumers, as it is assumed that consumers' decision-making regarding whether to disclose

complaints with narratives does not display systematic variations between the pre-disclosure and post-disclosure periods.

The sample period terminates on December 31, 2016, for two primary reasons. Firstly, focusing solely on data prior to 2017 eliminates the potential influence of contrasting political administrations. The CFPB, instituted during President Barack Obama's tenure, is designed to prioritize consumer interests. However, President Donald Trump, who assumed office in January 2017, and his administration hold unfavorable views of regulations targeting firms (Haendler and Heimer (2021)). Secondly, the equilibrium in data structure pre- and post-disclosure warrants consideration. Given the brief three-month duration of the pre-disclosure period in this study and the annually increasing volume of complaints received by the CFPB, the sample period is constrained to pre-2017 to ensure the pre-disclosure and after-disclosure samples are balanced.

I ultimately retain 62,096 samples from the mortgage market on which I focus. The regression model is as follows:

$$\begin{aligned} Y_{i,t} \\ &= \alpha + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \mu_{Institution} \\ &+ \mu_{Zip} + \mu_{YearMonth} + \epsilon_{i,t} \end{aligned} \tag{1}$$

In my study, complaints are categorized into treatment and control groups based on the public availability of their narratives. $Treat_i$ equals 1 for complaints with narratives accessible to the public, and 0 otherwise. $Post_t$ represents the period when the CFPB grants unrestricted access to complaint narratives, taking a value of 1 for dates on or after June 25, 2015.

Regarding the outcome variables, $Y_{i,t}$, two distinct indicators are employed to assess the resolution outcomes of complaints. One is whether complaints submitted to the CFPB are resolved with monetary compensation.[3] The other

[3]Complaints submitted to the CFPB can be resolved through several means: Closed, Closed with explanation, Closed with monetary relief, Closed with non-monetary relief, and Untimely response. Monetary relief is a resolution outcome favored by consumers, and previous research has demonstrated that this type of resolution exhibits significant racial disparities (Haendler and Heimer (2021)).

is whether the institution's response is timely.[4] These outcome variables are all represented in binary form. In Equation (1), the coefficient of interest is β_1, which quantifies the relative improvement in resolution outcomes for complaints with narratives compared to those without, following the full implementation of the disclosure policy.

Furthermore, various fixed effects are incorporated to account for the influence of unobservable factors. Institution fixed effects control for variations across institutions, and zip code fixed effects capture potential impacts arising from changes in local political and economic conditions. Year-month fixed effects absorb heterogeneous effects that fluctuate over time. To address correlation within institutions and across time, standard errors are clustered by institution and year-month.

Table A15 reveals that the coefficient of $Treat \times Post$ in column (1) is statistically significantly positive, suggesting that the probability of complaints being resolved with monetary relief increases by 0.6% following the implementation of the disclosure policy, which equates to 3.55% of a standard deviation. Subsequently, the impact of the disclosure policy on the efficiency of complaint responses is assessed. The coefficient for $Treat \times Post$ in column (2) is positive and significant at the 1% level, while the coefficient of $Treat$ is significantly negative. These results imply that prior to the public availability of narratives, complaints accompanied by narratives are processed less promptly compared to complaints without narratives. However, this situation is reversed after disclosure, with institutions handling complaints with narratives more efficiently than standard complaints. Although the estimated coefficient suggests that timely responses to complaints only increase by 0.6% after disclosure, equivalent to 5.26% of the standard deviation.

These findings support the notion that disclosing complaint narratives to the public significantly enhances complaint resolution outcomes. Concurrently, the estimation results of the individual terms (such as $Treat$) suggest that

[4]Timely response refers to "Whether the company provided a timely response." As per the CFPB stipulations, companies must submit a response within 15 days of receiving a complaint. However, in exceptional situations, companies may petition for an extension of up to 60 calendar days to furnish their conclusive response to the complaint.

merely providing an option to disclose narratives might not improve the circumstances for consumers who opt to make their complaint narratives publicly available. The actual catalyst for change is the public disclosure of these narratives. This disclosure effectively captures the attention of institutions and may motivate them to be more inclined to offer favorable treatment to consumers' demands in order to safeguard their reputations.

The accuracy of estimates provided by the difference-in-difference model relies on the fulfillment of the parallel trend assumption. To ensure the reliability of my estimation, I conduct an event study based on the framework outlined in Equation (1). As depicted in Figure A7, I designate June 25, 2015, as the policy event date and examine the dynamic treatment effects spanning three months prior to and four months following this date (observations beyond the fourth month are consolidated into the fourth month).

Panels (a) and (b) of Figure A7 illustrate the dynamic results for the two outcomes, respectively. In the months preceding the disclosure, the effects do not exhibit significant deviations from zero, and the estimated coefficients for the pre-trend generally remain stable over time, indicating the absence of abnormal trends. Crucially, the improvement in response outcomes manifests almost instantaneously at the time of disclosure. Although the magnitude of improvement effects varies across different outcomes, they all demonstrate long-lasting impacts. The estimated coefficients for the fourth month and subsequent periods consistently align with the coefficients observed in the initial period, reinforcing the enduring nature of the improvement.

Appendix D. Website Ranking Banks for Discrimination

To enhance support for minority borrowers in the United States and to address potential discrimination in mortgage lending practices, I establish an online platform. This website prominently features an analytical framework that ranks lenders based on indicators of racial discrimination at the ZIP code level. Specifically, I ascertain whether minority borrowers are subjected to higher loan interest rates or face increased rejection probabilities by specific lenders. Methodologically, the analysis is refined to ZIP code specificity by including only data pertaining to a single ZIP code in each regression model. The dataset is consistent with those employed in prior research concerning interest rates or rejection rates. The regression model I employ is outlined as follows:

$$Outcomes_{ilt} = \alpha + \beta Minority_i \times Lender\ id_l + FEs + \epsilon_{ilt} \tag{2}$$

In Equation (2), the symbols i, l, and t represent borrower, lender, and year, respectively. *Outcomes* represent the interest rates or rejection rates for minority borrowers i in lenders l. *Minority* equals one if the borrower i is Black or Latinx. *Lender id* specifically identifies lender l. Accordingly, the coefficient β is instrumental in uncovering the disparities in interest rates or rejection rates between minority and non-minority borrowers within lender l. A β value greater than zero would suggest the presence of racial discrimination in lender l, manifesting as higher interest rates or elevated rejection probabilities for minority borrowers. The regression employs the same fixed effects setting consistent with previous examinations of interest rates or rejection rates.

I rank the lenders by the degree of discrimination in each ZIP code, which is determined by the magnitude of the β coefficient in the above results. These rankings, alongside detailed discrimination levels, are made publicly accessible through a website I named Discrimination Warrior (`https://discriminationwarrior.com`). This platform, upon the input of a specific ZIP code, displays the extent of discrimination practiced by lenders in that

locality against minority borrowers, as evidenced by the analyzed interest and rejection rates. Figure A8 and Figure A9 present the results for ZIP code 02135, a region in Boston. This initiative aims to provide the public with direct and transparent access to local lenders' discriminatory practices.

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