Does the Disclosure of Consumer Complaints Reduce Racial Disparities in the Mortgage Lending Market?*

Xiang Li†

Fordham University

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.

*I am indebted to my advisor, Thomas Chemmanur, for his numerous guidance, support, and encouragement during my dissertation research. I would like to express my sincere gratitude to my other dissertation committee members, Philip Strahan, Hassan Tehranian, and Rawley Heimer, for their guidance and support. I want to sincerely thank Ningzhe Zhou, Junhui Ma, and Yuqin Lei for providing excellent research assistance. I sincerely thank the Conference of State Bank Supervisors (CSBS) for providing access to data from NMLS Consumer Access (https://nmlsconsumeraccess.org/). I am grateful for the valuable feedback and insights gained from participants and discussants at the following conferences and seminars: AEA, CFPB Research Conference, FARS, Conference on Diversity, Equity and Inclusion in Economics, Finance, and Central Banking, HEC Banking in the Age of Challenges Conference, CAPANA, MFA, CFEA, AREUEA Virtual Seminar, Virtual Household Finance Seminar, CUNY Baruch, Inter-Finance PhD seminar, FMA New Ideas Session, Haskell & White Academic Conference, Boca Corporate Finance and Governance Conference, AFA (Poster Session), Hawaii Accounting Research Conference, Loyola University Maryland Financial Ethics Conference, BC Graduate Research Symposium, Clermont Financial Innovation Workshop, Annual Conference on Pacific Basin Finance, Economics, Accounting and Management, and Fostering Inclusion Workshop. I am deeply grateful to the Loyola Financial Ethics Conference review committee for recognizing this paper with the Best Paper Award.

†Finance Department, Gabelli School of Business, Fordham University. Email: xianglifinance@gmail.com

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

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.

1 Introduction

Despite decades of anti-discrimination legislation, racial disparities persist significantly in the U.S. mortgage market (Ambrose et al. (2021); Bartlett et al. (2022); Cheng et al. (2015); Munnell et al. (1996); Ross and Yinger (2002)). These disparities manifest not only in loan access and pricing outcomes but more fundamentally in the market's lack of effective information feedback mechanisms to reveal and correct discriminatory practices. As the largest component of consumer debt—accounting for 70.6% of total household debt and affecting over 83 million homeowners—mortgage fairness is crucial for economic equality.

The U.S. relies on two complementary disclosure mechanisms to monitor fairness in mortgage lending. The traditional Home Mortgage Disclosure Act (HMDA), enacted in 1975, requires lenders to report standardized loan-level information, enabling regulators, researchers, and the public to document racial disparities from the supply side. A vast literature based on HMDA data has documented sizable racial differences in approval rates and pricing, shaping our understanding of racial inequality in credit markets and informing fair-lending policy for nearly half a century.[1] In 2015, the Consumer Financial Protection Bureau (CFPB) introduced a distinct, demand-side disclosure mechanism by publicly releasing consumer complaint narratives against supervised financial institutions through the CFPB Complaint Database. These narratives describe, in borrowers' own words, perceived unfair or discriminatory treatment in the mortgage lending market.

This setting provides a unique opportunity to identify how disclosure affects discriminatory lending. The CFPB's 2015 reform created plausibly exogenous variation

[1] Although HMDA data document large and persistent racial gaps, they do not allow researchers to evaluate whether disclosure changes behavior: comparable, comprehensive measures of racial disparities are unavailable for the pre-HMDA period, and information regarding discriminatory practices was historically fragmented across individual lenders.

in narrative complaint disclosure across lenders: institutions under CFPB supervision were required to disclose consumer complaint narratives, while similar non-supervised lenders were not. Coupled with detailed loan-level data, this distinction allows me to construct clean treatment and comparison groups and trace how racial gaps evolve before and after disclosure.

Behavioral research shows that the form of information matters: unlike the statistical information in HMDA, complaint narratives provide concrete and salient accounts of borrower-reported experiences, which attract more attention, are more easily encoded into memory, exert stronger influence on belief formation, and diffuse more readily than statistical summaries (Bruner (1986); Graeber et al. (2024); Shiller (2017)). By making alleged discriminatory conduct more vivid and cognitively accessible, narrative disclosure increases the visibility of such incidents. This heightened salience can intensify external monitoring by regulators, consumers, competitors, and investors, and strengthen accountability pressures on both institutions and individual loan officers.

Despite years of operation and the accumulation of rich consumer complaints in the CFPB Complaint Database, we know little about how this demand-side disclosure mechanism affects racial disparities. In stark contrast to the vast literature built on HMDA data, no systematic evidence exists on whether complaint disclosure can reduce racial inequalities in mortgage markets. This paper shows, for the first time, that CFPB complaint disclosure on racial disparities improves credit supply for minority borrowers, thus offering new insights into how different disclosure mechanisms promote financial fairness and providing empirical evidence for policymakers to optimize their regulatory toolkit.

Utilizing a difference-in-differences design, this analysis demonstrates that following the CFPB's 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: marginal minority borrowers, who previously exhibited lower default rates due to stricter standards, see their default rates rise toward parity with marginal white borrowers. 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, consistent with evidence that credit access generates real housing market effects (Adelino et al. (2025)). 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 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 rule applies only to institutions with more than $10 billion in total assets, creating 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,

[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.

thereby revealing potential discriminatory practices. The results indicate that treated lenders 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

[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.

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. This mechanism matters because loan officer behavior plays a critical role in shaping borrowers' credit access, with specific implications for minority applicants (Frame et al. (2025)). 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 moves to smaller lenders or exits from the industry. In addition, promotion prospects for these loan officers decline after disclosure. These effects are more pronounced for officers at CFPB-supervised lenders with high discrimination complaint intensity than for those at CFPB-supervised lenders with low complaint intensity.

I also analyze responses from various external stakeholders to gain insights into additional mechanisms contributing to the effectiveness of the disclosure policy. First, I examine how peer lenders leverage insights from competitors' disclosed weaknesses to inform market entry decisions. I find that, following the disclosure policy, competing lenders become more likely to establish branches in regions where incumbent lenders have been associated with discrimination complaints. This finding is consistent with Drechsler et al. (2022), who document that nonbank lenders expand into markets where traditional banks contract their lending. This competitive response is critical because bank competition plays a vital role in shaping credit supply (Drechsler et al. (2017); Wang et al. (2022)). By increasing competition in underserved markets and offering minority borrowers additional lending options, this mechanism empowers minority consumers to shop around and avoid lenders with high complaint intensity. The results indicate that minority borrowers actively shift their loan applications away from lenders implicated in discriminatory practices, taking advantage of the expanded set of alternatives made available through new market entry. 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 experience negative abnormal returns following the complaint disclosure, with especially sharp declines among those explicitly implicated in discriminatory behavior.

The persistently low homeownership rate among minority households remains a major policy concern. 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. This pattern is consistent with the earlier finding that disclosure expands mortgage approval for minority borrowers, effectively 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

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

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

2 Institutional 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 related to credit cards in July 2011. Over time, it expanded its coverage to include complaints about mortgages, bank accounts, credit reporting, and other financial products and services, ultimately spanning 21 distinct product categories. These complaints typically involve issues such as poor customer service or allegations of misconduct by financial institutions. Consumers have reported over 178 types of issues through these submissions. On June 25, 2015, the CFPB began publicly releasing consumer complaint narratives, providing detailed descriptions of consumers' experiences with financial products and services. The complaint database includes only complaints against institutions supervised by the CFPB, namely those with total assets exceeding $10 billion.[5] Public visibility of the CFPB's complaint portal has remained high since the Bureau began publishing consumer complaint narratives in 2015. As of late 2025, the CFPB website receives roughly 2.4 million sessions per month, implying an annualized volume of about 29 million.[6] The scale of website activity shows that the CFPB database has achieved significant public reach. Beyond direct borrower use, the reputational mechanism also spreads through secondary pathways such as media reporting and consumer advocacy efforts.

The CFPB solicited public comments before deciding to release complaint narratives. It received 137 unique responses from consumer groups, trade organizations, businesses, and individuals, along with 30,000 letters from individuals. Lenders and their associations opposed the disclosure of these narratives, arguing that the release of "unverified" consumer accounts could unfairly harm firms' reputations. The CFPB implemented procedures to address these concerns. First, it verifies the business relationship between the lender and the complainant to prevent fraud. Second, it makes lenders' responses publicly available, enabling them to dispute the complaints. Unlike Google reviews or other open review platforms, CFPB complaints are tied to verified consumer–firm relationships and processed within a structured company response and supervisory framework. Complaint volumes thus serve as a credible indicator of service quality, with persistently high counts often triggering heightened regulatory scrutiny and fines (Begley and Purnanandam (2021)).

In contrast, consumer advocates, civil rights groups, and open government proponents supported disclosing complaint narratives. They emphasized three main benefits: (i) empowering consumers with timely and relevant information to make informed decisions

[5] Complaints concerning depository institutions with less than $10 billion in assets are not included in the database and are forwarded to the appropriate safety and soundness regulators.

[6] For more details, see `https://analytics.usa.gov/consumer-financial-protection-bureau`.

before purchase, promoting customer-oriented business practices, and revealing unfair or deceptive conduct after purchase; (ii) helping the Bureau and other stakeholders identify harmful trends before they escalate into broader market failures; and (iii) encouraging greater use of the database as a practical tool for accumulating data on consumer experiences in the financial sector. These benefits are consistent with the CFPB's original objectives for this initiative (CFPB (2015)).

This study examines discrimination-related complaints disclosed after June 2015 and their link to racial disparities in the mortgage market. Text analysis of complaint narratives indicates that roughly 700–900 discrimination-related mortgage complaints are filed per year—about 5%–7% of all mortgage complaints. Given that only 6.7% of approved loans in our sample are made to minority borrowers (Table 1), the 5%–7% share is particularly salient, highlighting the severity of the underlying discrimination problem.

Table A1 presents an example from the CFPB's public complaint database related to "Applying for a mortgage or refinancing an existing mortgage." The complaint includes several fields, such as "Date received," "Product," "Consumer complaint narrative," "Company," and "Company response to consumer." Although personally identifiable information is removed, claims of racial discrimination remain apparent. One consumer states, "I believe that I am being discriminated against because I disclosed my race as XXXX."[7] In this case, the company's response is categorized as "closed with monetary relief," and the firm does not dispute the complaint. These details suggest possible misconduct by the lender. The consumer received compensation for the unfair treatment through the CFPB's complaint resolution system.

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

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 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 overlook. 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 and racial disparities in the mortgage market, one of the largest yet most opaque segments of the consumer finance industry.

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 mortgage loan officers.

This study provides a novel regulatory perspective on addressing racial inequalities in lending markets. Unlike prior research (Li (2025)) 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 study also contributes to the literature on the effectiveness of CFPB regulations. 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, and Li and Zhou (2024) demonstrate that CFPB supervision helps reduce racial gaps in the savings market, 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. In contrast to Dou and Roh (2023), who examine how the disclosure of complaint counts influences application volume without addressing racial disparities, my study investigates whether disclosure can mitigate discriminatory lending. I find that only the disclosure of complaint narratives reduces racial disparities in mortgage outcomes, whereas the disclosure of complaint counts has no effect. By providing richer and more nuanced information than simple counts, narratives offer concrete and salient accounts that attract greater attention and are more easily encoded into memory. They also exert stronger influence on stakeholder beliefs and behavior than abstract statistics can achieve. These attributes enable narratives to play a unique and effective role in curbing discrimination, revealing a previously overlooked channel through which transparency can promote equity in credit markets.

3 Data Description and Summary Statistics

3.1 Data Description

1. HMDA-GSE Merged Dataset. The primary dataset used in this study is the merged HMDA-GSE dataset. The HMDA data are the sole source of loan-level data containing information on applicants' race and ethnicity. This dataset covers 90% of mortgage originations in the U.S., as reported by Engel and McCoy (2016). The HMDA data provide key variables, including applicant income, race, ethnicity, loan amount, lender name, and the census tract of the property. I supplement this dataset with information from the Government-Sponsored Enterprises (GSE) dataset, which contains detailed loan characteristics such as interest rate, default status, LTV ratio, credit score, loan product, loan purpose, and loan term. These variables are essential for capturing borrower-level financial traits.

Since there is no direct linkage between the two datasets, I use the "fuzzy data matching" method described in Law and Mislang (2022) to merge them. The detailed procedure is provided in Appendix B. The resulting dataset includes all approved loans securitized by the GSEs from 2011 to 2019.[8] Since loans with different credit characteristics are subject to distinct risk-based pricing and approval standards, I filter the dataset following Bartlett et al. (2022), using variables such as credit score, LTV ratio, and loan amount. Appendix B provides further details on this procedure. The final sample is a pooled cross-sectional dataset

[8] Given the profound impact of the COVID-19 pandemic, which severely affected the U.S. and global financial markets starting in 2020, this study excludes post-2019 data to avoid distortion caused by this shock.

at the loan level. I use this dataset to examine whether the disclosure of consumer complaint narratives influences interest rates and default rates among minority borrowers.

2. HMDA Origination Dataset. Another key dataset used in this study is the HMDA origination dataset. Unlike the HMDA-GSE merged dataset, the original HMDA data include unapproved loan applications. This feature enables me to evaluate the effect of disclosing consumer complaint narratives on rejection rates among minority borrowers. However, the HMDA data lack sufficient loan-level attributes. To address potential bias from omitted variables, I adopt the strategy proposed by Bartlett et al. (2022). Because borrower credit scores and loan-to-value (LTV) ratios are not directly observed, I implement a substitution approach. Specifically, I use the HMDA-GSE merged dataset to compute the median credit score and LTV at the census tract level. Appendix B provides further details on this procedure.

3. CFPB Dataset. I apply textual analysis to the CFPB consumer complaint database to identify complaints related to racial discrimination, following Li (2025). Specifically, I classify narratives containing the terms "discrimination," "unfair," "partial," "inequity," "prejudice," "injustice," or similar expressions as indicative of discriminatory content. If a narrative includes any of these terms, I categorize the complaint as related to racial discrimination. As part of the quality-assurance process, I manually review complaints flagged by these keywords to ensure that the terms are used in contexts involving discriminatory or unfair treatment. In addition, I use ChatGPT to perform this classification, and the results are highly similar to those obtained from the textual analysis. Detailed large-language-model analyses are reported in Appendix E. For external validation, Li (2025) demonstrates that lenders receiving a higher number of discrimination-related complaints also exhibit significantly higher rejection rates for minority mortgage applicants, thereby supporting the construct validity of this measure. Then, I match discrimination-related complaints to both the HMDA-GSE merged dataset and the HMDA origination dataset using lender names from the CFPB database. To examine heterogeneous effects, I classify treated lenders into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median.

4. NMLS Dataset. I obtain data from the Nationwide Multistate Licensing System (NMLS),[9] a centralized registry that collects detailed information on mortgage loan originators, including branch affiliations, company records, licenses, and registrations. Each loan officer is assigned a unique NMLS ID that remains constant across employers, enabling me to track their professional trajectories over time. I focus on information related to employment histories and branch manager assignments, which allows me to observe turnover events and promotions to managerial roles. Since all licensed or registered loan officers must be reported in the NMLS, the dataset provides comprehensive coverage for examining how complaint disclosures affect career outcomes in the mortgage industry.

5. Additional Datasets. I also incorporate several supplementary datasets and indicators from external sources to support the identification of disclosure effects.

(i) I obtain stock market returns for publicly listed lenders from Wharton Research Data Services (WRDS). The dataset spans the period surrounding June 25, 2015, covering both the pre- and post-disclosure policy periods. It includes market-adjusted cumulative abnormal returns (CAR) and CAR based on the CAPM. WRDS also provides financial performance data for these lenders for the fourth quarter of 2014. I use these performance measures as control variables when analyzing market responses.

(ii) I use the Call Reports dataset to obtain total asset values for each institution in the

[9]The Nationwide Multistate Licensing System (NMLS) was created in response to the Secure and Fair Enforcement for Mortgage Licensing Act of 2008 (SAFE Act), which was enacted to protect consumers and reduce mortgage market fraud by establishing minimum standards for licensing and registering mortgage loan originators. The Act requires federal registration for loan officers employed by federally insured depository institutions or their subsidiaries, as well as those working for entities regulated by the Farm Credit Administration. All other loan officers must obtain licenses from their respective state regulators.

primary sample for the first quarter of 2015. This variable helps identify institutions affected by the disclosure policy. I classify institutions with total assets between $0 and $10 billion as the control group and those with assets between $10 and $100 billion as the treated group.[10] This filtering strategy improves the identification of causal effects by ensuring comparability across similarly sized institutions. The results are robust to alternative cutoff thresholds.

(iii) I use branch-level deposit data from the Summary of Deposits (SOD) dataset to supplement the HMDA origination dataset. This information is used to analyze consumer application behavior and rival lenders' responses.

(iv) The American Community Survey (ACS) provides household-level characteristics aggregated at the county level. I extract homeownership-related variables from 2011 to 2019. Homeownership is measured as the percentage of housing units occupied by owners, disaggregated by race. I use this measure to assess the real effect of the disclosure policy on racial disparities in homeownership.

3.2 Summary Statistics

Table 1 presents summary statistics for the treated and control institutions in the HMDA-GSE merged dataset. The sample covers the period from 2011 to May 2015, prior to the implementation of the CFPB's disclosure policy. The treated group consists of institutions with total assets between $10 and $100 billion. The control group includes institutions with assets below $10 billion. I use this dataset to examine the impact of complaint narrative disclosure on loan interest rates and default rates.

[10] Large institutions are excluded for the following reasons: after the implementation of the Dodd-Frank Act, bank holding companies (BHCs) with assets exceeding $100 billion became subject to enhanced supervision and regulatory stress tests under the Comprehensive Capital Analysis and Review (CCAR) framework starting in 2011. In addition, large institutions may differ from smaller institutions in terms of capital structure or benefit from economies of scale.

In treated institutions, 7% of loans are issued to minority consumers, compared to 8% in control institutions. I define minority consumers as Black or Hispanic individuals.[11] Treated institutions have an average interest rate of 4.11%, slightly lower than the 4.22% observed in control institutions. A similar pattern is found in default rates. Mean LTV ratios and credit scores are also comparable across the two groups. The average annual income of borrowers in the treated group is $107,000, compared to $100,000 in the control group. Average loan amounts are $239,000 and $247,000, respectively. These figures indicate that treated and control institutions display similar characteristics across key risk-related variables prior to the policy shock, supporting the validity of the group classification.

[Insert Table 1 about here]

4 CFPB Disclosure and Racial Gaps in Interest Rates

4.1 Research Design

This section investigates how lenders respond to the disclosure of complaint narratives during the loan application process. In this section, I focus specifically on the effect of disclosure on racial differences in interest rates for two main reasons. First, loan interest rates are a crucial factor for borrowers. Racial disparities in this domain have cost minority borrowers up to $450 million annually, as documented by Bartlett et al. (2022). Such unequal treatment has also attracted considerable attention from regulatory agencies.[12] Second, the

[11] Definitions of all variables used in this study are provided in Table A2. Table A3 reports summary statistics from 2011 to 2019. The last three variables in Table 1—Cash-out Refinance, Purchase, and Refinance—represent distinct loan purposes.

[12] Senators Elizabeth Warren and Doug Jones have expressed concerns to financial regulators about ongoing lender discrimination. Their statement references a recent study by Bartlett et al. (2022), which highlights disparities in interest rates between minority and white borrowers. The communication is available at: `https://www.warren.senate.gov/imo/media/doc/2019.6.10%20Letter%20to%20Regulators%20on%20Fintech%20FINAL.pdf`.

distinct pricing strategy applied to defaulted guaranteed GSE loans provides an effective lens to detect discriminatory practices by lenders.

To interpret pricing differences through the lens of discrimination, it is important to understand the legal framework that governs fair lending in the United States. According to U.S. fair-lending legislation, judicial rulings have affirmed that lenders are permitted to consider specific borrower characteristics when determining loan prices.[13] This remains lawful even if these proxy variables result in less favorable outcomes for minority borrowers, as long as the lender can demonstrate a legitimate business necessity. Courts have explicitly held that when a lending practice causes a disparate impact, the burden of proof shifts to the lender. Lenders must demonstrate that any policy, procedure, or practice is directly related to the applicant's creditworthiness.[14] In other words, differences in creditworthiness may justify variation in loan terms. However, charging higher interest rates to applicants in financially underserved areas or to those with limited capacity to shop around solely for the purpose of profit maximization is not justifiable.

Building on this legal distinction between justified and unjustified pricing differences, I follow the empirical framework developed by Bartlett et al. (2022) to identify discrimination and address concerns about omitted variable bias. Under this approach, all variables related to legitimate business necessity are observable. In this context, the GSEs set credit-risk-based pricing through a fee schedule that depends solely on the borrower's position within an 8×9 grid of LTV ratios and credit scores, known as loan-level price adjustments (LLPAs).

[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. III. 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.

Lenders are protected against credit risk by paying these LLPA fees. Figure A1 displays a representative Fannie Mae LLPA grid for single-family loans with a 360-month term. Freddie Mac, another major GSE, uses a comparable pricing system. As a result, a borrower's interest rate can be decomposed into three components: the base mortgage rate, the credit risk component (based on LTV and credit score), and a residual that captures strategic lender pricing. This study focuses on racial disparities in the residual component to isolate pricing differences attributable to lender behavior.

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's disclosure of complaint narratives. The baseline regression model is presented as follows:

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

My primary empirical framework relies on a triple-differences design at the mortgage loan level. I examine how mortgage interest rates change across racial groups following the disclosure. In Equation (1), i, l, and t denote loan applicants, lenders, and time (year-month), respectively. $Treat_l$ classifies lenders as treated or control based on whether lender l was subject to CFPB supervision in 2015, meaning the lender was covered by the disclosure policy. Defining treatment status in this way offers a key identification advantage: assignment is determined ex ante and is unaffected by post-policy outcomes, mitigating concerns about endogeneity arising from lenders' subsequent behavior or complaint activity. The treated group consists of institutions with total assets between \$10 and \$100 billion as of the first quarter of 2015, while the control group includes institutions with assets below \$10 billion. Institutions with total assets exceeding \$100 billion are excluded from the regression sample. This restriction mitigates confounding factors related to differences in regulatory exposure, capital structures, or economies of scale between large and small lenders. $Post_t$ equals one for June 2015 and beyond, marking the implementation of the CFPB disclosure policy. $Minority_i$ equals one if applicant i is Black or Hispanic, and 0 otherwise.[15] Accordingly, the coefficient β_1 in the triple-differences model captures the effect of disclosure on racial disparities in interest rates, estimating the change in the racial gap for treated lenders relative to control lenders whose complaint narratives remained undisclosed. The design is intended to capture a deterrence effect: once public disclosure becomes possible, supervised lenders may adjust pricing toward minority applicants even before being explicitly named in any complaint.

As discussed earlier, the pricing grid plays a central role in mitigating concerns about credit risk in my identification strategy. However, interest rates within each pricing cell may still vary over time or by loan purpose. Therefore, in Equation (1), I include fixed effects for each of the 8×9 grid cells (denoted as "bucket"), interacted with loan year, month, and loan purpose status. To further account for lender-specific pricing practices, time trends, and borrowers' shopping behavior, I include lender-by-year-month fixed effects.[16]

This specification tests whether borrowers of different races applying to the same lender in the same period face an identical menu of loan offerings and pricing policies. The model further includes fixed effects for borrower characteristics, including loan amount deciles,

[15] I do not classify Asian applicants as minorities because I do not find significant gaps between Asian and white applicants in my sample.

[16] Shopping behavior refers to the extent to which borrowers compare mortgage offers across multiple lenders. If white borrowers systematically engage in more extensive shopping, they may obtain lower rates even when lenders treat all applicants equally. The lender-by-year-month fixed effects help rule out this concern by comparing borrowers applying to the same lender during the same period.

applicant income deciles, applicant gender, and co-applicant status, to account for observable heterogeneity that may influence loan pricing. Standard errors are double-clustered at the lender and year levels.[17] By comparing borrowers within these lender–time cells and controlling for rich borrower-level characteristics, I isolate the portion of racial disparities that cannot be explained by credit risk or shopping behaviors and is most plausibly attributable to differential treatment by lenders.

4.2 Baseline Results

Table 2 presents the results of the baseline regression. Column (1) includes only the first two sets of fixed effects specified in Equation (1). In column (2), I add fixed effects for borrower characteristics. Lenders may apply different pricing markups when using the pricing grid, so in column (3), I interact lender fixed effects with bucket–loan purpose–time fixed effects. To approximate individual fixed effects, I construct a similar borrower identifier that captures granular borrower profiles.[18] Column (4) includes these fixed effects, enabling a direct comparison of disclosure effects between borrowers with similar creditworthiness but different racial identities. Finally, column (5) introduces joint lender–similar borrower fixed effects. Controlling for these joint effects enables a refined comparison of nearly identical borrowers within the same lender before and after the CFPB disclosure policy, ensuring that the estimates are not confounded by lender–borrower relationships.

[Insert Table 2 about here]

Table 2 displays the estimated coefficients on the *Minority* variable. These coefficients

[17]The results are robust to clustering at the lender level only, as reported in Table A5 in the Appendix.

[18]The borrower fixed effects are based on the interactions of several categorical variables: pricing grid, loan purpose, loan amount deciles, applicant income deciles, applicant gender, and co-applicant status. These dimensions allow for highly granular borrower segmentation, helping to identify individuals with nearly identical credit-relevant traits. Because borrower characteristic fixed effects are collinear with these constructed borrower fixed effects, they are excluded in columns (4) and (5).

indicate that, before the disclosure policy, minority borrowers paid interest rates 2.5 to 2.9 basis points higher than white borrowers.[19] Since the Mortgage Bankers Association began publishing its Annual Performance Report in 2008, the average net production income has been 47 basis points (or $1,077 per loan).[20] Therefore, a 2.5 basis point increase in interest rate corresponds to a 1.7 basis point rise in annual principal repayment, equivalent to approximately 3.6% of lenders' annual net profit.[21] This disparity narrows following the implementation of the CFPB disclosure policy. The estimated coefficient on the triple-difference term, $Treat \times Post \times Minority$, shows that minority borrowers experience a relative interest rate reduction of 2.2 to 2.6 basis points after the disclosure, effectively offsetting approximately 92% of the pre-existing racial gap. This policy change generates an estimated $93 million in annual savings for minority mortgage borrowers, primarily by reducing discriminatory pricing.[22] Finally, the coefficient on $Post \times Minority$ is statistically insignificant, suggesting that the racial interest rate gap did not change among control lenders whose complaint narratives were not disclosed. Table 2 also shows that the model explains 76% to 82% of the variation in interest rates within the sample. The remaining unexplained variation may be attributed to strategic pricing by lenders.

[19] I find that the disclosure policy does not significantly affect female borrowers. This may be due to the absence of systematic discrimination against women in mortgage lending, as many female borrowers apply jointly with their spouses.

[20] For more details, see: `https://www.mba.org/news-and-research/newsroom/news/2025/04/17/independent-mortgage-bankers-post-net-production-profits-in-2024`

[21] Consider a mortgage loan for a $200,000 home with a 20% down payment. Raising the interest rate from 4.00% to 4.025% increases the annual repayment by about $27.70, which represents 1.73 basis points of the principal (80% of $200,000 equals $160,000).

[22] According to a report by the Federal Reserve Bank of New York, total housing debt in 2022 was approximately $12.26 trillion. The Survey of Consumer Finances indicates that African-American and Hispanic borrowers represent 13.3% of the mortgage market. HMDA data show that lenders subject to CFPB oversight account for 35.75% of total loan volume after 2015. A back-of-the-envelope calculation suggests that a 2.3 basis point decline in minority borrowers' interest rates translates to an annual repayment reduction of 1.6 basis points of principal. Consequently, minority borrowers served by CFPB-regulated lenders would benefit, resulting in aggregate annual savings of approximately $93 million.

A key concern with the triple-differences analysis is whether the treated and control groups exhibited parallel trends in racial interest rate gaps before the CFPB disclosure. I adopt an event study approach to examine the dynamic effects of the disclosure policy.

Figure 1 plots the quarterly coefficient estimates of racial gaps in interest rates between treated and control lenders. As shown, there is no meaningful difference in racial gaps between the two groups prior to the implementation of the disclosure policy, indicating that pre-policy trends were parallel. Beginning in the first quarter following the policy's introduction, however, the racial gap in interest rates declines sharply among treated lenders relative to controls. This divergence persists over subsequent quarters, highlighting a sustained treatment effect driven by the disclosure intervention.

[Insert Figure 1 about here]

4.3 Robustness and Mechanism Tests

This section presents a series of robustness tests to validate the main findings. I first provide additional evidence on the mechanism through which narrative disclosure affects lender behavior. I then examine the sensitivity of the identification strategy to alternative specifications. Finally, I address potential confounding factors and alternative explanations.

To clarify the distinctive role of narrative disclosure, I use the CFPB's 2013 disclosure of its complaint database as a comparison. The 2013 policy change made complaint counts public but withheld detailed narratives. As shown in Table A10, this count-only disclosure did not reduce racial disparities in lending outcomes. These findings highlight that complaint counts alone provide limited accountability, whereas narratives provide actionable context that enables stakeholders to identify institutional problems and pursue targeted remedies for historically underserved borrowers. This effectiveness stems from narratives' distinct cognitive advantages: unlike abstract statistics, narratives are more easily encoded into memory, attract greater attention, and diffuse more readily across stakeholders, thereby amplifying their impact on lender behavior.

Appendix C also examines the heterogeneous effects of disclosure on interest rates. Using the textual analysis method developed in Section 3.1, I classify treated lenders into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median. As shown in Table A6, the reduction in racial disparities is significantly larger among lenders with high complaint intensity, suggesting that disclosure is more effective for lenders with a higher propensity for discriminatory practices. Table A6 also shows that the effect is stronger in regions with higher public attention to the CFPB, measured by Google search activity. In addition, Appendix C finds that the policy effect is amplified during periods of heightened litigation risk related to racial discrimination.

Having established the mechanism, I next assess the robustness of the baseline identification strategy. To assess the comparability of treated and control groups, I vary the asset cap thresholds and examine whether the parallel trends hold when restricting the sample to lenders with more similar asset sizes. Using the empirical setting from column (2) of Table 2, I adjust the cap in \$5 billion increments from \$25 billion to \$95 billion, beginning with a \$25 billion cutoff based on Fuster et al. (2021), and plot the estimated coefficients on the triple interaction term at each threshold in Figure A2. By narrowing the bandwidth of lender asset sizes, this exercise strengthens the validity of the parallel trends assumption by ensuring that treated and control lenders are more comparable in terms of institutional scale. The continued statistical significance of the coefficients across all specifications in Figure A2 confirms the robustness of the main results.

Table A8 presents robustness checks using alternative fixed effects specifications, extending the analysis in Table 2. The table includes three panels with progressively more granular geographic fixed effects: Panel A adds state–year fixed effects, Panel B includes ZIP Code–year fixed effects, and Panel C incorporates census tract–year fixed effects. This stepwise structure controls for increasingly localized, time-varying factors that could influence interest rates. The coefficient on the key triple interaction term ($Treat \times Post \times Minority$) increases in magnitude and remains statistically significant as more localized fixed effects are introduced. This pattern suggests that incorporating finer geographic controls reduces omitted variable bias and enhances the precision of the estimated treatment effect. The consistency of results across these increasingly stringent specifications indicates that unobserved time-varying factors at various geographic levels are unlikely to explain the findings, supporting a causal interpretation of the disclosure policy's impact.

As an alternative identification strategy, I employ a difference-in-discontinuities (diff-in-disc) design following Grembi et al. (2016) and Bennedsen et al. (2022). This approach estimates the change in the discontinuity at the $10 billion total asset threshold before and after the disclosure policy. By focusing on institutions located near this cutoff, the design improves the comparability between treated and control institutions and thus provides a credible identification of the policy's causal effect. The empirical results, reported in Table A9, show that after the disclosure policy was implemented, institutions above the $10 billion threshold significantly reduced interest rates for minority borrowers, while rates remained relatively stable for white borrowers.

To demonstrate that my sample is comparable to those used in prior studies, I replicate the primary results of Bartlett et al. (2022) using GSE loan data from my sample. Table A7 in the Appendix presents these replication results. I apply the same empirical specification as in Table 3 of Bartlett et al. (2022) to estimate interest rate differences for minority borrowers in both purchase and refinance loans. Despite using a different matching method to construct the final sample, the sign and magnitude of the coefficients closely resemble those reported by Bartlett et al. (2022). This consistency suggests that my results are not driven by sample selection bias.

Finally, I address potential confounding factors that might offer alternative explanations for the results. One potential concern relates to potential differences in discount point choices between minority and white borrowers.[23] Such differences are one possible explanation for the higher interest rates observed among minority FHA borrowers, as suggested by Bhutta and Hizmo (2021). However, this interpretation remains contested. Bartlett et al. (2022) find evidence of persistent discriminatory pricing in FHA loans even after controlling for discount points using a larger dataset. To avoid this confounding factor, my analysis focuses on GSE loans. Prior work by Bartlett et al. (2022) and Zhang and Willen (2021) also documents discriminatory pricing in the GSE market. Additionally, I test whether racial differences in discount points vary systematically between treated and control lenders. Because the HMDA dataset only began reporting discount point information in 2018, I use the HMDA-GSE merged sample from 2018 to 2019 to conduct this analysis within the same framework as Table 2, including interest rate as a control. Table A11 in the Appendix presents the results. I find no statistically significant differences in racial gaps in discount points between treated and control groups. This result suggests that, even if discount point behavior partly explains racial interest rate disparities, my difference-in-differences framework effectively differences out such effects through the treatment–control comparison.

To address concerns that other factors might explain the observed racial disparities,

[23] In the mortgage market, borrowers can pay discount points—upfront fees exchanged for lower interest rates—or opt for negative points, receiving lender credits in return for higher interest rates. If preferences for discount points systematically differ by race, observed interest rate disparities could reflect endogenous borrower decisions rather than lender pricing.

I conduct several robustness checks. I examine whether differences in rate lock periods, put-back risk, or fallout risk may drive the results. As detailed in Appendix D and Table A12, these analyses confirm that such factors cannot explain the reduction in racial gaps following disclosure.

The final potential threat to identification is whether a contemporaneous regulatory change or shift in litigation activity might have confounded the results. To address this, I review all CFPB regulatory changes in 2015 and find no other policy interventions that could plausibly explain the observed effects. I also examine whether the frequency of discrimination-related litigation changed around 2015 and find no significant difference in the number of cases filed before and after the disclosure policy, further ruling out these factors as confounders.

5 CFPB Disclosure and Outcome Tests: Evidence from Default Rates

5.1 Research Design

In this section, I examine the default rates of loans. Default outcomes are shaped by various factors beyond the sole control of lenders. To detect racial discrimination and assess the role of disclosure policy, I adopt the "outcome test" approach (Becker (1957, 1993)). This method evaluates whether loans issued to marginal minority borrowers generate higher profits than those extended to marginal white borrowers. If minority borrowers at the margin yield higher profits, it indicates that lenders apply stricter screening standards to minority applicants due to racial prejudice. Lender profits depend primarily on two components: interest rates, which determine revenue, and default rates, which cause losses. Earlier analysis shows that minority borrowers pay higher interest rates. This section focuses on whether racial disparities also appear in default rates and whether the disclosure policy helps reduce such disparities. Identifying significant differences in default rates between marginally qualified minority and white borrowers is essential. Such a gap would indicate that the observed disparities stem not from omitted variables or statistical discrimination but from lenders' taste-based racial biases in the selection process.[24]

However, conducting outcome tests accurately presents at least two major challenges (Butler et al. (2023)). First, researchers cannot directly observe the actual marginal borrower. This limitation is known as the "infra-marginality problem" (Ayres (2002)). The definition of a marginal borrower is neither standardized nor publicly disclosed across lenders. Therefore, researchers must approximate them by controlling for observable covariates. However, this approach may produce inaccurate results if the covariates do not fully account for differences in creditworthiness between minority and white borrowers. In such cases, disparities in default rates may reflect unobserved credit risk rather than racial bias. Moreover, if minority borrowers systematically exhibit lower creditworthiness than white borrowers—even after controlling for observable factors—marginal minority borrowers may still have higher default rates in the absence of bias. Consequently, lower default rates among marginal minority borrowers can only be observed if racial bias is both present and strong enough to offset these confounding effects.

[24]Discrimination against minority borrowers in lending can stem from two main sources: racial bias or statistical discrimination. Racial bias includes 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 reflects lenders' reluctance to approve loans for minority borrowers or preference for white borrowers. Miscalibrated beliefs refer to inaccurate stereotypes held by lenders about minority applicants. As these biases are often implicit and contribute jointly to discriminatory behavior, I group them together. Alternatively, statistical discrimination refers to differential treatment based on group-level risk profiles (Arrow (1974); Ewens et al. (2014); Phelps (1972)). In this study, it implies that lenders may reject more applications from minority borrowers due to perceived higher risk. From a profit-maximization standpoint, such behavior may appear rational but still results in disparate outcomes.

A second concern is that researchers cannot directly observe loan profitability. When relying on default rates, disparities between minority and white borrowers may be misestimated if other factors affecting lender profitability are omitted. For example, white borrowers may present higher prepayment risk, which can reduce their perceived profitability. In such cases, lenders may not require minority borrowers to have substantially lower default rates in order to yield higher profits than white borrowers. This dynamic makes it more difficult to detect lower default rates among minority borrowers.

Conducting outcome tests by comparing default rates between marginal minority and white borrowers is not without limitations. Nevertheless, the preceding analysis highlights the conservative nature of this method. The outcome test can only reveal a significantly lower default rate for marginal minority borrowers when strong racial bias is present in lenders' selection processes. Therefore, any observed evidence of lower default rates among minorities should be interpreted as strong evidence of discrimination. It suggests that lenders apply stricter approval standards to minority applicants.

The outcome test method has proven effective in the auto loan market (Butler et al. (2023)). Building on Butler et al. (2023), my research applies this approach to examine racial disparities in the mortgage market. Specifically, I evaluate the effect of the disclosure policy on racial gaps in default rates among marginal borrowers. If these gaps narrow after disclosure—particularly if the default rate of marginal minority borrowers increases relative to their own pre-disclosure level, approaching that of white borrowers—it provides strong evidence that disclosing complaint narratives prompts lenders to relax their screening criteria. This shift results in more equitable approval standards for minority applicants.

5.2 Outcome Test Results

I define default rates following Butler et al. (2023). A loan is classified as defaulted when the borrower is delinquent for 90 days or more. To identify marginal borrowers, I restrict the sample to individuals with credit scores below 660. This restriction substantially reduces the number of observations.[25] Equation (2) presents the identification strategy for the outcome test approach:

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

In Equation (2), the symbols i, l, and t represent loan applicants, lenders, and time (year-month), respectively. Table 3 presents results from a triple-difference specification used to estimate the impact of disclosure on racial disparities in default rates. The model controls for loan characteristics such as interest rate, loan purpose, and the logarithm of the loan amount. Applicant characteristics, including gender, the logarithm of income, and co-applicant status, are captured by X_{ilt} in Equation (2). To account for potential influences on loan default rates stemming from changes in local economic conditions, I include ZIP Code-by-year fixed effects. In column (3), I further control for borrower creditworthiness by incorporating pricing grid–loan purpose fixed effects.

Columns (1)–(3) present the discriminatory treatment faced by minority borrowers in the subprime sample and the effect of the disclosure policy in mitigating this

[25] The sample construction here is consistent with Section 4, where I exclude observations from large banks with total assets exceeding $100 billion.

inequity. The negative and statistically significant coefficient on *Treat*×*Minority* confirms the presence of discrimination against minorities in treated lending institutions. The *Treat*×*Post*×*Minority* coefficient in column (2) indicates an average increase of 34.2 percentage points—equivalent to 84.2% of one standard deviation—in the default rates of minority borrowers in the treated group relative to the control group following disclosure. This result suggests that racial disparities in default rates are nearly eliminated after disclosure, highlighting the strong influence of the disclosure policy.[26]

[Insert Table 3 about here]

In the last column of Table 3, I do not apply the credit score cutoff used to identify marginal borrowers. This full-sample analysis does not provide sufficient evidence of discrimination against minorities. The full sample includes many borrowers who are far from the credit approval margin, making the test less sensitive to margin-related differences. This result suggests that applying the outcome test to the subprime borrower sample—who are closer to the margin—is a more appropriate identification strategy.

One potential concern is that my specification imposes a total asset cap of $100 billion for lenders, which may affect the comparability between treated and control lenders due to differences in asset size. To address this concern, I vary the asset cap thresholds based on the specification in column (1) of Table 3 and present the estimated triple interaction coefficients under different caps in Figure A3. This robustness check ensures that the results are not driven by the specific choice of asset threshold used to define comparable lenders.

In Figure A3, I apply a series of asset caps, increasing in $5 billion increments from $25 billion to $95 billion, and estimate the model at each threshold. The initial cap of $25 billion follows the approach in Fuster et al. (2021). The statistical significance of the coefficients

[26]To address concerns regarding the parallel trends assumption, I also implement an event study framework to examine the dynamic effects of disclosure. See Table A14 in the Appendix for further details.

across these thresholds in Figure A3 supports the robustness of my results.

6 CFPB Disclosure and Racial Gaps in Rejection Rates

6.1 Overall Trend Analysis

Previous results suggest that the disclosure policy nearly eliminates disparities in default rates among marginal borrowers of different races. These disparities may stem from lenders applying stricter approval standards to minority applicants. The implementation of the disclosure policy may reduce lenders' incentives to maintain such unfair criteria. In this section, I turn to loan application denials and address two key questions: (i) Does a racially biased screening standard exist before the disclosure? (ii) Does the disclosure policy reduce these disparities?

I begin by analyzing the historical trend in loan application rejections using the full sample from the HMDA origination dataset. Figure 2 displays racial disparities in residualized rejection rates, comparing lenders subject to CFPB supervision with those not under its oversight. This analysis focuses on the period surrounding the CFPB's disclosure policy implemented in June 2015.

To address concerns about differences in loan screening criteria between CFPB-supervised and unsupervised lenders, I compute racial gaps in rejection rates separately for each group. By focusing on within-group differences—that is, the gap between minority and non-minority borrowers within each lender category—I effectively control for potential differences in overall approval rates between treated and control lenders.[27]

[27] Suppose CFPB-supervised lenders are more risk-averse than their unsupervised counterparts. In that case, borrowers would generally face higher rejection rates under CFPB-supervised institutions. Directly comparing rejection rates across lender groups would therefore be problematic. However, by calculating within-group racial gaps, I mitigate potential bias arising from differences in screening standards.

Additionally, I utilize residualized rejection rates, which control for observable borrower risk characteristics and the fixed effects specified in Equation (3). This residualization isolates racial disparities that are not attributable to borrower traits and helps reduce distortion caused by statistical discrimination.

Figure 2 shows that, prior to the disclosure policy, racial gaps in rejection rates followed parallel trends across CFPB-supervised and unsupervised lenders. After the disclosure, the decline in excess rejection rates for minority applicants is significantly more pronounced among CFPB-supervised lenders. This pattern supports the parallel trends assumption and validates the identification strategy used in the subsequent analysis.

[Insert Figure 2 about here]

6.2 Empirical Analysis

In this subsection, I empirically examine whether the disclosure policy reduces discrimination in loan accessibility for minority borrowers. The analysis relies on the HMDA origination dataset. The dependent variable is a loan-level rejection indicator. The estimation model is specified as follows:

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

In Equation (3), the symbols i and l continue to represent loan applicants and lenders, respectively, while t now refers to the year.[28] Table 4 reports the regression results. Columns (1) and (2) present the baseline estimates. In column (1), I include lender-by-year fixed effects, lender–similar borrower joint fixed effects, and lender–census tract joint fixed effects.[29] To account for borrower–lender pair dynamics, I include lender–similar borrower joint fixed effects to capture changes in rejection rates for the same borrower–lender matches before and after disclosure. This control mitigates bias from shifts in borrower credit characteristics and lender lending strategies.[30] In column (2), I additionally include census tract-by-year fixed effects. The results indicate that minority applicants in the control group are 3.5 percentage points less likely to receive loan approval than white applicants, based on the coefficient on *Minority*. The coefficient on *Treat*×*Minority* in column (2) suggests that rejection rate disparities increase by 4.1 percentage points among lenders subject to the disclosure policy. This implies that minority applicants in treated institutions are 7.6 percentage points less likely to obtain loan approval compared to white applicants. A rough estimate indicates that 70,000 minority applicants are denied loans annually under such treatment, despite being likely to receive approval had they been white.[31] The statistically insignificant coefficient

[28] The HMDA dataset is reported at the annual level, so the time index t corresponds to the calendar year.

[29] Declined loans are reported in the HMDA origination dataset, substantially increasing the sample size. This expansion allows for a more granular fixed effects structure. Moreover, in the mortgage market, loan officers may dissuade prospective minority applicants from applying or assist non-minority applicants in improving their approval prospects. If such practices are systematically embedded at the branch level, lender–census tract fixed effects capture this dimension of branch-level discrimination in rejection outcomes.

[30] The "similar borrower" variable used here is consistent with Section 4, except that I replace the individual credit score and LTV group from the HMDA-GSE merged dataset with the census tract-level median credit score and LTV. This adjustment is necessary due to the absence of individual-level data in the HMDA dataset. Bartlett et al. (2022) confirm that using tract-level medians yields comparable conclusions regarding discrimination. See Section 3.1 for further discussion.

[31] To estimate this figure, I multiply the total number of annual minority mortgage applications by the estimated change in approval probability. After 2015, HMDA data show an average of 19.16 million applications per year. Of these, 34.91% are submitted to CFPB-regulated lenders, and 13.77% are from minority applicants. This implies approximately 0.92 million minority applications annually in CFPB-supervised lenders (19.16 million × 34.91% × 13.77%). Applying the estimated 7.6% approval gap yields approximately 70,000 minority

on *Post×Minority* indicates no significant change in racial disparities within the control group following disclosure. However, the negative coefficient on *Treat×Post×Minority* in column (2) suggests that the disclosure policy reduces racial disparities in loan approval by 1.7 percentage points in the treated group. This corresponds to over 15,000 additional minority households securing mortgage loans each year due to the disclosure policy.[32] Figure A4 in the Appendix validates the parallel trends assumption underlying this identification strategy.

[Insert Table 4 about here]

I further examine the impact of disclosure by investigating whether lenders respond differently when the disclosed complaint narratives involve allegations of discriminatory treatment.

To explore heterogeneity by lender characteristics, I split the treated group into two subsamples based on the intensity of discrimination-related complaints, measured as the ratio of complaint counts to total assets: those with above-median intensity (column (3)) and those with below-median intensity (column (4)). Columns (3) and (4) of Table 4 present the moderating effects of discrimination complaints. The *Treat×Minority* coefficient is positive and statistically significant in column (3), indicating that within the treated group, minority applicants face higher rejection rates at lenders with high complaint intensity. In contrast, the same coefficient in column (4) is smaller in magnitude, suggesting that lenders with low complaint intensity impose lower additional rejection rates on minority applicants. These results further validate the measurement of lender-level discrimination. The coefficient on *Treat×Post×Minority* captures the moderating role of discrimination complaints. The result in column (3) shows that the overall treatment effect is primarily driven by lenders engaged

applicants denied each year due to racial bias.

[32]Based on the same calculation method, 0.92 million minority applications × 1.7% implies approximately 15.64 thousand additional approvals annually. This estimate may be conservative because the HMDA dataset, while comprehensive, covers approximately 90% of U.S. mortgage applications.

in discriminatory practices.

As a robustness check, Appendix E develops an alternative approach using a large language model (LLM) to identify discrimination-related complaints. Unlike the keyword-based method, the LLM evaluates the full narrative context, capturing subtle patterns and comparative treatment differences that may not be reflected in explicit keywords. As shown in Table A16, the treatment effect remains stronger among lenders with high LLM-identified complaint intensity, consistent with the keyword-based results. This convergence across different measurement approaches strengthens confidence that the observed effects reflect genuine behavioral changes by lenders.

One potential concern regarding these findings is that the credit profiles of minority borrowers improve post-disclosure, with higher-income or more creditworthy individuals becoming more likely to apply, thus reducing rejection rates mechanically. To test this alternative explanation, I examine whether the credit characteristics of minority applicants change after disclosure. Specifically, I replace the dependent variable in columns (1) and (2) with applicant income and loan amount, as shown in Table A15. The results indicate that the coefficient on the triple interaction term, $Treat \times Post \times Minority$, is statistically insignificant. This finding suggests that the disclosure policy does not significantly alter the credit profiles of minority applicants in treated institutions relative to those in the control group. Therefore, the observed increase in loan approval rates for minorities is not driven by improved creditworthiness or loan characteristics.

Overall, my findings demonstrate that the disclosure of consumer complaint narratives effectively reduces discrimination against minority borrowers in the mortgage market. This leads to a narrowing of racial disparities in interest rates, rejection rates, and default rates. The results suggest that CFPB-supervised lenders have improved their service quality, particularly toward minority applicants. Further evidence of this improvement is shown by the pronounced decline in the total number of mortgage loan complaints following the CFPB's disclosure of consumer complaints. This decline indicates a broader enhancement in customer service quality after the policy change. As illustrated in Figure A5, the volume of complaints began to drop significantly soon after the narrative disclosure, reinforcing the interpretation that lenders—particularly those under CFPB supervision—became more attentive and proactive in addressing borrower concerns.

Furthermore, I examine how the disclosure policy affects lenders' strategies for handling complaints after they occur, using data from the CFPB Complaint Database. I apply a difference-in-differences approach at the complaint level, leveraging the unique timeline of the policy's implementation. The analysis shows that after complaint narratives are made publicly available,[33] financial institutions become more responsive to complaints that include narratives, relative to those without them. This increased responsiveness is evident in two key outcomes. First, consumers who submit narrative complaints are more likely to receive monetary relief. Second, these complaints are more likely to receive timely responses from the financial institution. These results suggest that the disclosure policy incentivizes lenders to improve complaint resolution practices and enhance consumer satisfaction. Table A17 reports the detailed estimates, and Figure A6 illustrates the dynamic effects. Additional discussion is provided in Appendix F.

Beyond these reactive improvements, Appendix G examines whether disclosure induces lenders to adopt forward-looking, process-oriented complaint management strategies. By analyzing the month-to-month persistence of complaint volumes, I find that the autocorrelation

[33]The CFPB officially implemented the disclosure policy on March 19, 2015. However, a time gap existed between the policy finalization and the public release of complaint narratives. These narratives were not accessible to the general public until June 25, 2015. I designate this interim period as the pre-shock window and compare changes in the resolution of complaints accompanied by narratives.

of complaints weakens significantly after narrative disclosure (Table A18), suggesting that lenders commit resources to improving service processes and preventing issues from recurring, rather than merely reacting case by case.

7 CFPB Disclosure and Loan Officers' Career Paths

Prior analyses demonstrate that the disclosure of complaint narratives reduces discriminatory practices by lenders against minority borrowers. However, the mechanisms through which disclosure curbs such behavior remain underexplored. One potential channel is that disclosure disciplines loan officers by exposing their misconduct.[34] Disclosure may influence loan officer behavior through career-related consequences along two dimensions. Internally, complaint narratives provide detailed accounts of consumer dissatisfaction in CFPB-supervised institutions, including concerns over service quality, discriminatory practices, transaction process deficiencies, misinformation, and fraud.[35] Lenders may worry that retaining officers who attract numerous complaints could harm institutional reputation, potentially resulting in termination. Externally, competing lenders may also respond to publicly available complaints. They might be reluctant to hire loan officers from CFPB-supervised institutions with complaint records, thereby limiting these officers' future employment opportunities. Taken together, these internal and external dynamics suggest that loan officers affiliated with lenders receiving complaints may face negative consequences for their career trajectories.

To test this hypothesis, I construct a panel dataset of loan officer employment histories

[34] Hanson et al. (2016) provide direct evidence of such misconduct using a correspondence experiment, showing that loan officers are less likely to respond to inquiries from minority borrowers and offer them less helpful information.

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

using data from the Nationwide Multistate Licensing System (NMLS). Based on this comprehensive dataset, I estimate a difference-in-differences model at the individual level:

$$\begin{aligned} &DownwardMobility_{it} \\ &\qquad = \alpha + \beta_1 Treat_l \times Post_t + \gamma X_{it} \\ &\qquad\quad + \mu_{Individual} + \mu_{Lender} + \mu_{ZIP\text{-}Code} + \mu_{Year} + \epsilon_{it} \end{aligned} \tag{4}$$

In this model, i, l, and t index individual loan officers, lenders, and year, respectively. Lenders with complaints publicly disclosed by the CFPB are classified as treated lenders. For loan officers employed by these treated lenders, the variable $Treat_l$ is set to 1; otherwise, it is set to 0 for those in the control group. The variable $Post_t$ indicates the period following the CFPB's public release of complaint narratives. 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 $DownwardMobility_{it}$, I construct two measures of downward career movement among loan officers. These measures rely on a lender hierarchy defined by annual firm size.[36] The first measure, *Move Down*, compares the size of the destination lender to that of the origin lender. If a loan officer moves to a smaller lender, it is set to 1, and 0 otherwise. The second measure, *Move Down or Exit*, broadens downward movement to include industry exits. If a loan officer either moves to a smaller lender or exits the industry, it is set to 1, and 0 otherwise. An exit is recorded when an officer has no NMLS-recorded affiliation with any lender. Additionally, I include a series of fixed effects to address unobserved confounding factors. Individual fixed effects control for time-invariant personal characteristics such as education, risk tolerance, and other traits. Lender fixed effects help distinguish between

[36] Annual firm size is measured by the number of loan officers employed. Larger lenders typically offer better career opportunities, possibly due to stronger reputations (see Hong and Kubik (2003) and Gao et al. (2020)).

changes in the institutional environment and individual-level responses. Following disclosure, lenders may reform their discriminatory lending practices, which affects the work environment for all loan officers at that institution. By including lender fixed effects, I control for such institution-wide changes, allowing me to isolate the impact of disclosure on individual loan officers' career outcomes. ZIP Code fixed effects control for local factors, including economic and political conditions, while year fixed effects capture common time trends. I also control for the duration of an individual's career as a measurement of individual ability. Standard errors are clustered at the lender level.

[Insert Table 5 about here]

Table 5 reports the results from estimating Equation (4). Columns (1) and (2) use *Move Down* as the dependent variable, while columns (3) and (4) use *Move Down or Exit* as the outcome. The results in columns (1) and (3) show that the coefficient on $Treat_l \times Post_t$ is significantly positive. This suggests that loan officers at lenders affected by the CFPB disclosure policy are more likely to experience downward career moves after its implementation. The economic magnitude of these effects is substantial. Following disclosure, loan officers face a 0.122 standard deviation increase in the likelihood of moving to a smaller lender. Using the broader definition that includes exits, the effect remains sizable, representing a 0.160 standard deviation increase in the probability of moving to a smaller lender or exiting.

Additionally, I adopt an event study approach to examine the dynamic effects of disclosure on loan officers' downward career movements. Figure 3 presents the yearly coefficient estimates for *Move Down* and *Move Down or Exit* , comparing treated lenders to control lenders.

[Insert Figure 3 about here]

Figure 3 shows no substantial difference in downward career movements between treated and control groups before the implementation of the disclosure policy. This pattern supports the parallel trends assumption, which is essential for the validity of the difference-in-differences approach. Following the policy's introduction, Figure 3 reveals a notable increase in downward transitions: loan officers in the treated group become more likely to move to smaller lenders and—under the broader definition—either move to smaller lenders or exit the industry, relative to the control group.

To examine heterogeneity in how disclosure affects loan officers, I proxy individual-level reputational risk using branch-level complaint data. Since the CFPB Consumer Complaints Database does not permit the identification of individual employees, I measure reputational exposure at the branch level, which is the most granular level available in the data. This is a compelling proxy for individual employee service quality, as the median number of loan officers per branch is just two. I link complaints to specific branches based on lender name, ZIP code, and year, and merge this information with the NMLS loan officer panel. This allows me to assess whether officers affiliated with branches that received discrimination-related complaints experienced stronger career disruptions following the policy change. The merged dataset includes both individual employment histories and measures of average service quality at the branch level. I then classify treated branches into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median between 2015 and 2022. Branches with above-median complaint intensity are assigned to the $1(Treat\_High\_Compl)$ group, and those with below-median intensity to the $1(Treat\_Low\_Compl)$ group. I modify Equation (4) accordingly to estimate separate treatment effects by subgroup. The modified equation is as follows:

$$\begin{aligned} DownwardMobility_{it} \\ &= \alpha + \beta_1 Treat\_High\_Compl_l \times Post_t \\ &\quad + \beta_2 Treat\_Low\_Compl_l \times Post_t \\ &\quad + \gamma X_{it} + \mu_{Individual} + \mu_{Lender} + \mu_{Zip} + \mu_{Year} + \epsilon_{it} \end{aligned} \tag{5}$$

Columns (2) and (4) of Table 5 report the regression estimates. In both specifications, the coefficient on $1(Treat\_High\_Compl_l) \times Post_t$ is positive and statistically significant, indicating that the CFPB disclosure policy increases the probability of downward mobility for officers at lenders with high complaint intensity. By contrast, the coefficient on $1(Treat\_Low\_Compl_l) \times Post_t$ is statistically indistinguishable from zero, implying no comparable post-policy change among officers at treated lenders with low complaint intensity. These findings reinforce the interpretation that affiliation with lenders with high complaint intensity is associated with greater likelihood of downward career movements following disclosure.

This result further supports the interpretation that employees affiliated with branches with high complaint intensity are more likely to experience downward career movements. Overall, my findings suggest that the CFPB disclosure policy influences loan officers' career mobility. When consumer complaints are made public, loan officers face heightened reputational risks, which increase the likelihood of experiencing downward career movements.

The disclosure of complaint narratives may not only influence the likelihood of loan officers experiencing downward career movements but also affect their chances of promotion to managerial positions. To examine this latter effect, I use $Promoted_{it}$ as the dependent variable in Equations (4) and (5). For each loan officer i, $Promoted_{it}$ takes the value of one in year t and all subsequent years if the officer is promoted to a managerial role in year $t + 1$. This definition of promotion encompasses both internal advancements within the same institution and transitions to managerial roles at external institutions. Thus, the coefficient on $Treat \times Post$ captures the effect of disclosure on promotion likelihood. The coefficients on $1(Treat\_High\_Compl_l) \times Post_t$ and $1(Treat\_Low\_Compl_l) \times Post_t$ measure the differential impacts on promotion prospects for loan officers in branches with high and low complaint intensity, respectively.

[Insert Table 6 about here]

Column (1) in Table 6 shows that the likelihood of loan officers being promoted to managerial positions in CFPB-supervised lenders declines following the disclosure of complaint narratives. This decline represents a 7.1% decrease relative to one standard deviation in the promotion rate, suggesting an economically meaningful effect. Furthermore, column (2) in Table 6 indicates that the negative effect is more pronounced for the $1(Treat\_High\_Compl)$ group. The difference in coefficients between the $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$ groups is statistically significant at the 1% level. This finding is reasonable, as loan officers at lenders with high complaint intensity are more likely to be perceived as underperforming, reducing their chances of being promoted either internally or externally.

Promotions of loan officers to managerial positions can occur either internally within their current branches or externally at other lending institutions. In columns (3) through (6), I examine how disclosure differentially affects these two types of career advancement. Specifically, columns (3) and (4) focus on external promotions to larger institutions. If the observed decline in overall promotions is primarily driven by a reduction in external opportunities, one would expect loan officers to experience greater difficulty in transitioning to financial institutions that are larger than their current employers.

To test this hypothesis, 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 6 suggests that disclosure is associated with a lower likelihood of external promotions to larger institutions among loan officers in supervised branches, although the estimated effect is not statistically significant. In contrast, column (4) shows that for loan officers in the $1(Treat\_High\_Compl)$ group, disclosure significantly reduces the probability of being promoted to a larger lender. Moreover, the difference in coefficients between the $1(Treat\_High\_Compl)$ group and the $1(Treat\_Low\_Compl)$ group is statistically significant at the 5% level.

These results suggest that the disclosure of complaint narratives reduces the information asymmetry faced by external lenders during the hiring process. Complaint records provide external institutions with additional information that may influence their hiring decisions. Consequently, while the overall impact of disclosure on external promotions is insignificant, distinguishing between loan officers with high and low complaint intensity reveals a significant negative effect for those associated with high complaint intensity.

To further explore the channels through which disclosure affects career advancement, I examine its effect on internal promotions. Specifically, I replace the dependent variables in Equations (4) and (5) with a dummy variable $Int\_Promoted_{i,t}$, which equals 1 if loan officer i is promoted within the same branch in year t. Column (5) of Table 6 shows that internal promotions significantly decrease following the disclosure of complaint narratives. Column (6) further indicates that the negative effect on internal promotions is more pronounced for the $1(Treat\_High\_Compl)$ group. The difference in coefficients between the $1(Treat\_High\_Compl)$ group and the $1(Treat\_Low\_Compl)$ group is statistically significant at the 5% level. This result may be attributed to the lower service quality demonstrated by loan officers at lenders with high complaint intensity, making them less suitable for managerial roles.

8 External Disciplinary Mechanisms

8.1 Rival Lender Competition

Previous sections have shown that lenders and loan officers significantly changed their behavior following disclosure. This section explores the mechanisms underlying these changes by examining external disciplinary forces. I analyze how three key stakeholders—rival lenders, consumers, and stock market investors—respond to disclosure and thereby create pressure that disciplines lenders.

I first examine whether the implementation of the disclosure policy has intensified competition among lenders in local markets. Publicly available complaint narratives may offer rival lenders valuable insights into the service quality of incumbent institutions, allowing them to identify underserved geographic segments. In response, competitors may strategically expand into areas where incumbent lenders are perceived to have engaged in discriminatory practices, thereby capturing market share by targeting minority borrowers.

To examine this possibility, I aggregate the HMDA origination data at the county-year level and compute the lender density within each county. I then use this measure as the dependent variable in the following regression model:

$$
\begin{aligned}
LenderDensity_{it} & \\
&= \alpha + \beta_1 Disc\_Compl\_Share_i \times Post_t \\
&+ \gamma X_{it} + \mu_{State \times Year} + \mu_{County} + \epsilon_{it}
\end{aligned} \tag{6}
$$

In Equation (6), i and t denote county and year, respectively. The dependent variable, *LenderDensity*, measures the total number of unique lender brands per ten thousand residents. The metric *Disc_ Compl_ Share* is calculated as the total number of discrimination-related complaints received in a county in 2015 divided by the number of loans issued to minority applicants.[37] The variable *Post* equals one for years following the implementation of the disclosure policy. The interaction term $Disc\_Compl\_Share \times Post$ captures the heterogeneous effects of disclosure across counties with different levels of discriminatory complaints. The vector X_{it} includes the lagged average loan approval rate and the lagged average bank deposits (Dou et al. (2024)). I also control for county fixed effects and state-by-year fixed effects to absorb local economic and temporal shocks. Standard errors are clustered at the county level.

[Insert Table 7 about here]

Table 7 presents the response of rival lenders to the disclosure policy. 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 after the policy implementation. The results suggest that more rival lenders enter markets with higher discrimination complaint shares following disclosure, potentially increasing competition.[38] These findings suggest that peer lenders strengthen their market positions by leveraging information from disclosed

[37] Given their limited market power, discrimination complaints filed against small lenders have minimal effects on regional outcomes. Therefore, when computing the complaint share at the regional level, I include all commercial banks, including large lenders with assets exceeding $100 billion.

[38] The results remain robust when clustering standard errors at both the county and year levels.

weaknesses in competitors' operations. The effects are more pronounced in counties with higher discrimination complaint shares, where rival lenders seize opportunities to enter local markets and intensify competition.[39] By providing minority borrowers with more lender options, increased competition may ultimately help protect them from discriminatory treatment in the mortgage lending market (Li (2025)). In Table A19 in the Appendix, I confirm these results using an alternative measure of disclosure exposure: *Amount Share*, defined as the market share held by treated lenders within each county in 2015.[40]

8.2 Consumer Market Selection

Having documented that disclosure intensifies competition by attracting rival lenders into markets with discriminatory practices, I next examine whether this increased competition translates into changes in consumer application behavior. With more lender options available, minority borrowers may be better positioned to avoid lenders associated with discriminatory practices. In this subsection, I examine whether minority borrowers shift their applications away from lenders with high complaint intensity following disclosure.

I use data aggregated from the HMDA origination dataset to analyze consumers' responses. The identification strategy follows the approach used in previous sections. The regression model is specified as follows:

$$\begin{aligned} &Application_{ilct} \\ &\quad = \alpha + \beta_1 Treat\_High\_Compl_l \times Post_t \times Minority_i \\ &\quad + \beta_2 Treat\_Low\_Compl_l \times Post_t \times Minority_i + \delta T_{ilt} + \gamma X_{lct} \\ &\quad + \mu_{Lender \times Year} + \mu_{Lender \times County} + \mu_{County \times Year} + \epsilon_{ilct} \end{aligned} \tag{7}$$

[39] Financial institutions are incentivized to utilize the complaint database to improve the quality of their mortgage services. They frequently benchmark their performance against competitors using database-derived metrics, as outlined in the Consumer Financial Protection Bureau (CFPB) 2013 report.

[40] A one standard deviation increase in *Amount Share* interacted with *Post* leads to an increase of 1.7 unique lender brands per ten thousand residents in the subsequent year, equivalent to 6.6% of one standard deviation, suggesting economically significant effects.

Equation (7) employs data aggregated at the county-lender-minority-year level. This structure implies that each lender l in county c contributes two observations i, corresponding to total applications from minority and non-minority applicants, respectively. The dependent variable, *Application*, is defined in two ways: (i) the logarithm of the number of mortgage applications from minority or white borrowers at the county-lender-year level, and (ii) the logarithm of the total dollar amount of such applications. To better capture consumers' responses to perceived discrimination, I classify CFPB-supervised lenders into two groups: $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$.[41] These treated groups are compared against lenders in the control group. The coefficients of primary interest, β_1 and β_2 in Equation (7), capture whether minority borrowers respond to increased market competition by shifting their applications away from lenders with high complaint intensity. The vector T_{ilt} includes the full set of terms from the triple-difference specification, including relevant interaction terms and their component variables.

Following Dou and Roh (2023), the control variables X_{lct} in Equation (7) include the following: (i) the proportion of mortgage applications approved by a lender in a given county, as higher approval rates may attract more applications; (ii) a dummy variable equal to one if the lender has a physical presence in the county during the given year; and (iii) the logarithm of total deposits collected by the bank's branches within the county for that year. In addition, I control for lender-by-year and county-by-year fixed effects to account for time-varying unobservable shocks at the lender and county levels. I also include lender-county fixed effects to proxy for branch-level heterogeneity.

[Insert Table 8 about here]

[41] In this analysis, I categorize each county-lender pair into one of two treatment groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median between 2015 and 2019. If a county-lender exceeds the median in a given year, it is assigned to the $1(Treat\_High\_Compl)$ group for that year; otherwise, it is assigned to the $1(Treat\_Low\_Compl)$ 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 total dollar amount of mortgage applications. Columns (2) and (4) include county-year fixed effects, whereas columns (1) and (3) do not. The consistency of results across these specifications confirms the robustness of the analysis. In all cases, the key coefficients exhibit a negative trend regardless of the dependent variable used. Economically, the magnitudes are sizable: for lenders with high complaint intensity, the post-disclosure estimates imply 16.7% fewer minority applications and 29.1% lower minority application amounts relative to control lenders, with columns using alternative fixed-effect sets yielding similar declines. In addition, the heterogeneity analysis shows that lenders in the $1(Treat\_High\_Compl)$ group show a stronger and statistically significant response compared to the $1(Treat\_Low\_Compl)$ group. These results suggest that minority borrowers shift their applications away from lenders with high complaint intensity when increased competition provides them with more alternatives.

8.3 Stock Market Discipline

Beyond competitive pressure from rival lenders and consumer market selection, stock market investors represent a third external disciplinary mechanism. In this subsection, I examine how stock market investors respond to the CFPB's 2015 enhancement of complaint disclosure. The public availability of complaint narratives enables the identification of lenders associated with discriminatory practices. I analyze whether the stock market reacts differently to lenders with high complaint intensity compared to those with low complaint intensity. Understanding investor responses to such disclosures offers insights into how reputational concerns and regulatory risks are incorporated into the valuation of financial institutions.

I construct a sample of 341 publicly traded financial institutions, each with complete daily return data over the relevant event windows. Using the U.S. Daily Event Study tool available on WRDS, I calculate 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 using two benchmark models: the Capital Asset Pricing Model (CAPM) and the Market-Adjusted Model (hereafter referred to as *Market*).

I then estimate cross-sectional regressions that examine the relationship between lenders' cumulative abnormal returns and their exposure to discrimination-related complaints.

$$CAR_i = \alpha + \beta_1 Treat\_High\_Compl_i + \beta_2 Treat\_Low\_Compl_i + \gamma X_i + \epsilon_i \tag{8}$$

In Equation (8), CAR denotes the abnormal return for each lender, calculated using either the CAPM or the Market-Adjusted Model across different event windows. I classify the institutions into three distinct groups: CFPB-supervised institutions with high complaint intensity in 2015 ($1(Treat\_High\_Compl)$); CFPB-supervised institutions with low complaint intensity ($1(Treat\_Low\_Compl)$); and unsupervised institutions, which serve as the control group. To address potential confounding factors, I control for total assets, equity-to-asset ratio, return on assets, and total deposits, all measured at the end of 2014.

[Insert Table 9 about here]

Table 9 presents the stock market's response to institutions in the $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$ groups relative to those not supervised by the CFPB. Columns (1) to (3) report results for the event windows of [-1, +1], [-2, +2], and [-3, +3] trading days, respectively. The results show that CFPB-supervised institutions experience a negative market reaction following the disclosure, with the effect being more pronounced for those with high complaint intensity. The estimated coefficients indicate that institutions in the $1(Treat\_High\_Compl)$ group suffer a decline in cumulative abnormal returns (CAR) of approximately 1.3 to 2.6 percentage points relative to unsupervised institutions. The differences between the $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$ groups are statistically significant for shorter event windows such as [-1, +1]. Columns (4) to (6) report CARs calculated using the Market-Adjusted Model and confirm the robustness of the findings.

These observations underscore the significant impact of the disclosure policy. The market interprets the release of complaint narratives as a negative shock, triggering substantial investor reactions. On the one hand, enforcement against discriminatory practices in the U.S. is stringent, and lenders found guilty of unlawful discrimination often face considerable economic and reputational penalties. This legal risk may cause investors to worry about affected institutions' future performance, prompting them to sell shares of lenders exposed to such risks.[42] On the other hand, growing interest in socially responsible investing suggests that investors may avoid institutions perceived as socially irresponsible. Lenders accused of unfair treatment of minority consumers may face reduced investor demand due to reputational concerns.[43]

The analyses in this section demonstrate that disclosure activates multiple external disciplinary mechanisms. Rival lenders increase competition, consumers shift their applications, and investors penalize discriminatory lenders through stock market reactions. Collectively, these forces create strong incentives for lenders to improve their practices.

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

[43]Prior research shows that concerns about inequality can significantly influence financial markets (Hartzmark and Sussman (2019); Pan et al. (2022)).

9 Real Effects of Disclosure

Previous sections show that disclosure improves lending outcomes for minority borrowers across multiple dimensions—lower interest rates, higher approval rates, and reduced discrimination. This section examines whether these improvements translate into a fundamental real effect: increased homeownership among minority households. Specifically, I analyze how disclosure affects homeownership rates across racial groups.

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 dataset. The merged dataset enables a direct assessment of the disclosure policy's effect on homeownership. The regression model I use is specified as follows:

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

Symbols i and t denote county and year, respectively. I use three distinct dependent variables to identify racial gaps in the analysis: (i) homeownership among minority residents, specifically Black and Hispanic individuals; (ii) homeownership among white residents; and (iii) the gap between the two measures.[44] To assess variation in disclosure impact, I continue to use the variable *Disc_ Compl_ Share*, which captures each county's exposure to the disclosure policy. To address potential bias from unobserved macroeconomic factors, I control for the growth rate of income per capita, the unemployment rate, and the logarithm of the county-level population. I also include county fixed effects and state-by-year fixed effects in

[44] I compute this variable by subtracting homeownership among white residents from that among minority residents. It captures the extent to which minority homeownership falls short relative to white homeownership. This measure is typically negative. Thus, a positive β_1 indicates a narrowing of racial gaps.

the specification.

[Insert Table 10 about here]

Table 10 presents the estimated outcomes. Column (1) reports the effect of the disclosure policy on minority residents. The results show that a one standard deviation increase in $Disc\_Compl\_Share \times Post$ is associated with a 0.87% rise in minority homeownership within a county, which equals 2.9% of one standard deviation in the homeownership rate. According to a report by the National Association of Realtors (NAR), the annual average growth rate of homeownership among Black Americans has been only 0.28% over the past decade.[45] This contrast highlights the potential strength of the disclosure policy in reducing racial disparities.

Column (2) reports an insignificant effect of disclosure on the homeownership rate of white residents, while column (3) shows a significantly positive effect 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 an improvement equivalent to 1.2% of a standard deviation in reducing racial gaps.

Overall, the findings demonstrate that the disclosure of complaint narratives has a meaningful and economically significant impact on reducing racial disparities in homeownership. Given that homeownership is a primary vehicle for wealth accumulation and intergenerational economic mobility, these results have important implications for addressing broader economic inequality. By improving minority households' access to mortgage credit, disclosure-based regulation can help narrow the persistent racial wealth gap and promote greater economic equity. These findings highlight the potential of transparency policies as effective tools for advancing social and economic justice within the financial system.

[45] For more details, please see: `https://www.nar.realtor/magazine/real-estate-news/nar-race-and-home-buying-report-black-homeownership-surges-but-still-lags-whites?`

10 Conclusion

This study provides the first comprehensive analysis of how the CFPB's disclosure of consumer complaint narratives affects racial disparities in mortgage lending. Using innovative identification strategies, I find that disclosure significantly reduces discrimination against minority borrowers. Following the policy's implementation, minority borrowers save approximately $93 million annually in mortgage-related costs, and more than 15,000 additional minority households obtain mortgage loans each year. The policy also narrows racial gaps in homeownership, demonstrating tangible real effects on economic equity. Mechanistic analyses reveal that disclosure operates through multiple channels: it disciplines loan officers through career consequences and activates external pressures from rival lenders, consumers, and capital markets.

Beyond the U.S. context, these findings have broader implications for financial regulation worldwide. I examine the status of financial consumer protection organizations in major developed countries, as shown in Table A20. At present, approximately 93% of developed countries have regulatory bodies 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.[46] My findings suggest that implementing similar disclosure mechanisms may hold substantial promise for reducing discriminatory practices and promoting financial inclusion and economic mobility globally.

To facilitate practical application of these findings, I develop an online platform that

[46] 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 A20 in the Appendix.

provides minority borrowers with transparent access to lender-level discrimination data.[47] The platform ranks lenders based on excess rejection rates and interest rate disparities applied to minority borrowers at detailed geographic levels.[48]

By democratizing access to this information, the platform empowers minority borrowers to make informed lending decisions and avoid discriminatory institutions, translating research findings into actionable tools for reducing racial disparities in mortgage markets.

References

Adelino, Manuel, Antoinette Schoar, and Felipe Severino, 2025, Credit supply and house prices: Evidence from mortgage market segmentation, *Journal of Financial Economics* 163, 103958.

Ambrose, Brent W., James N. Conklin, and Luis A. Lopez, 2021, Does borrower and broker race affect the cost of mortgage credit?, *Review of Financial Studies* 34, 790–826.

Arnold, David, Will Dobbie, and Crystal S. Yang, 2018, Racial bias in bail decisions, *Quarterly Journal of Economics* 133, 1885–1932.

Arrow, Kenneth J., 1974, *The theory of discrimination* (Princeton University Press, Princeton).

Ayres, Ian, 2002, Outcome tests of racial disparities in police practices, *Justice Research and Policy* 4, 131–142.

Bailey, Warren, G. Andrew Karolyi, and Carolina Salva, 2006, The economic consequences of increased disclosure: Evidence from international cross-listings, *Journal of Financial Economics* 81, 175–213.

Bartlett, Robert, Adair Morse, Richard Stanton, and Nancy Wallace, 2022, Consumer-lending discrimination in the FinTech era, *Journal of Financial Economics* 143, 30–56.

Becker, Gary S., 1957, *The Economics of Discrimination*, second edition (University of Chicago Press, Chicago), Second edition published in 1971.

Becker, Gary S., 1993, Nobel lecture: The economic way of looking at behavior, *Journal of Political Economy* 101, 385–409.

Begley, Taylor A., and Amiyatosh Purnanandam, 2021, Color and credit: Race, regulation, and the quality of financial services, *Journal of Financial Economics* 141, 48–65.

Bennedsen, Morten, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon, 2022, Do firms respond to gender pay gap transparency?, *Journal of Finance* 77, 2051–2091.

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

[48] Figure A7 and Figure A8 present example queries for ZIP Code 02135, a neighborhood in Boston. For complete platform functionality and additional details, see Appendix H.

Bhutta, Neil, and Aurel Hizmo, 2021, Do minorities pay more for mortgages?, *Review of Financial Studies* 34, 763–789.

Bordalo, Pedro, Katherine Coffman, Nicola Gennaioli, and Andrei Shleifer, 2016, Stereotypes, *Quarterly Journal of Economics* 131, 1753–1794.

Bruner, Jerome S., 1986, *Actual Minds, Possible Worlds* (Harvard University Press, Cambridge, MA).

Butler, Alexander W., Erik J. Mayer, and James P. Weston, 2023, Racial disparities in the auto loan market, *Review of Financial Studies* 36, 1–41.

Casas-Arce, Pablo, Carolyn Deller, F. Asís Martínez-Jerez, and José Manuel Narciso, 2023, Knowing that you know: incentive effects of relative performance disclosure, *Review of Accounting Studies* 28, 91–125.

CFPB, 2015, Disclosure of consumer complaint narrative data, *Federal Register* 80, 15572–15583, Docket No. CFPB-2014-0016. Final Policy Statement.

Cheng, Ping, Zhenguo Lin, and Yingchun Liu, 2015, Racial discrepancy in mortgage interest rates, *Journal of Real Estate Finance and Economics* 51, 101–120.

Christensen, Hans B., Eric Floyd, Lisa Yao Liu, and Mark Maffett, 2017, The real effects of mandated information on social responsibility in financial reports: Evidence from mine-safety records, *Journal of Accounting and Economics* 64, 284–304.

DeFusco, Anthony A., Stephanie Johnson, and John Mondragon, 2020, Regulating household leverage, *Review of Economic Studies* 87, 914–958.

Dou, Yiwei, Mingyi Hung, Guoman She, and Lynn Linghuan Wang, 2024, Learning from peers: Evidence from disclosure of consumer complaints, *Journal of Accounting and Economics* 77, 101620.

Dou, Yiwei, and Yongoh Roh, 2023, Public disclosure and consumer financial protection, *Journal of Financial and Quantitative Analysis* 58, 2164–2198.

Drechsler, Itamar, Alexi Savov, and Philipp Schnabl, 2017, The deposits channel of monetary policy, *Quarterly Journal of Economics* 132, 1819–1876.

Drechsler, Itamar, Alexi Savov, and Philipp Schnabl, 2022, How monetary policy shaped the housing boom, *Journal of Financial Economics* 144, 992–1021.

Egan, Mark, Gregor Matvos, and Amit Seru, 2022, When Harry fired Sally: The double standard in punishing misconduct, *Journal of Political Economy* 130, 1184–1248.

Engel, Kathleen C., and Patricia A. McCoy, 2016, *The subprime virus: Reckless credit, regulatory failure, and next steps* (Oxford University Press).

Ewens, Michael, Bryan Tomlin, and Liang Choon Wang, 2014, Statistical discrimination or prejudice? a large sample field experiment, *Review of Economics and Statistics* 96, 119–134.

Ewens, Michael, and Richard R. Townsend, 2020, Are early stage investors biased against women?, *Journal of Financial Economics* 135, 653–677.

Frame, W. Scott, Ruidi Huang, Erica Xuewei Jiang, Yeonjoon Lee, Will Shuo Liu, Erik J. Mayer, and Adi Sunderam, 2025, The impact of minority representation at mortgage lenders, *The Journal of Finance* 80, 1209–1260.

Fuster, Andreas, Matthew C. Plosser, and James I. Vickery, 2021, Does CFPB oversight crimp credit?, Staff report, Federal Reserve Bank of New York.

Gao, Janet, Kristoph Kleiner, and Joseph Pacelli, 2020, Credit and punishment: Are corporate bankers disciplined for risk-taking?, *Review of Financial Studies* 33, 5706–5749.

Gopalan, Yadav, 2022, The effects of ratings disclosure by bank regulators, *Journal of Accounting and Economics* 73, 101438.

Graeber, Thomas, Christopher Roth, and Florian Zimmermann, 2024, Stories, statistics, and memory, *Quarterly Journal of Economics* 139, 2181–2225.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano, 2016, Do fiscal rules matter?, *American Economic Journal: Applied Economics* 8, 1–30.

Hanson, Andrew, Zackary Hawley, Hal Martin, and Bo Liu, 2016, Discrimination in mortgage lending: Evidence from a correspondence experiment, *Journal of Urban Economics* 92, 48–65.

Hartzmark, Samuel M., and Abigail B. Sussman, 2019, Do investors value sustainability? a natural experiment examining ranking and fund flows, *Journal of Finance* 74, 2789–2837.

Hong, Harrison, and Jeffrey D. Kubik, 2003, Analyzing the analysts: Career concerns and biased earnings forecasts, *Journal of Finance* 58, 313–351.

Kleymenova, Anya, and Rimmy E. Tomy, 2022, Observing enforcement: Evidence from banking, *Journal of Accounting Research* 60, 1583–1633.

Law, Kody, and Nathan Mislang, 2022, The heterogeneity of bank responses to the fintech challenge, Working paper, Cornell University.

Li, Xiang, 2025, Bank competition and entrepreneurial gaps: evidence from bank deregulation, *Journal of Financial and Quantitative Analysis, Forthcoming.*

Li, Xiang, and Ningzhe Zhou, 2024, Shedding light on bias: Consumer complaint disclosure and racial equity in financial services, SSRN Working Paper 4908969, Fordham University.

Munnell, Alicia H., Geoffrey M.B. Tootell, Lynn E. Browne, and James McEneaney, 1996, Mortgage lending in Boston: Interpreting HMDA data, *American Economic Review* 86, 25–53.

Neugebauer, Randy, and Roger Williams, 2015, Reform the CFPB to better protect consumers, Congressional column, U.S. Congress.

Pan, Yihui, Elena S. Pikulina, Stephan Siegel, and Tracy Yue Wang, 2022, Do equity markets care about income inequality? evidence from pay ratio disclosure, *Journal of Finance* 77, 1371–1411.

Phelps, Edmund S., 1972, The statistical theory of racism and sexism, *American Economic Review* 62, 659–661.

Ross, Stephen L., and John Yinger, 2002, *The color of credit: Mortgage discrimination, research methodology, and fair-lending enforcement* (MIT Press).

Shiller, Robert J., 2017, Narrative economics, *American Economic Review* 107, 967–1004.

Tsolmon, Ulya, 2024, The role of information in the gender gap in the market for top managers: Evidence from a quasi-experiment, *Strategic Management Journal* 45, 680–715.

U.S. Chamber of Commerce, 2018, Consumer Financial Protection Bureau: Working towards fundamental reform, Center for Capital Markets Effectiveness.

Wang, Yifei, Toni M. Whited, Yufeng Wu, and Kairong Xiao, 2022, Bank market power and monetary policy transmission: Evidence from a structural estimation, *Journal of Finance* 77, 2093–2141.

Zhang, David Hao, and Paul S. Willen, 2021, Do lenders still discriminate? a robust approach for assessing differences in menus, NBER Working Paper 29559, National Bureau of Economic Research.

Figures and Tables

Figure 1. Estimated Dynamic Effects of Disclosure on Interest Rates

This figure plots quarterly estimates of racial disparities in interest rates between treated and control lenders. The Y-axis indicates the magnitude of the disparity, while the X-axis shows time relative to the policy, with quarter 0 denoting the implementation. The estimates are based on the merged HMDA–GSE dataset from 2011 to 2019. The regression specification follows column (2) in Table 2, where the omitted category serves as the baseline. Each black circle represents a point estimate, and the vertical lines denote 95% confidence intervals.

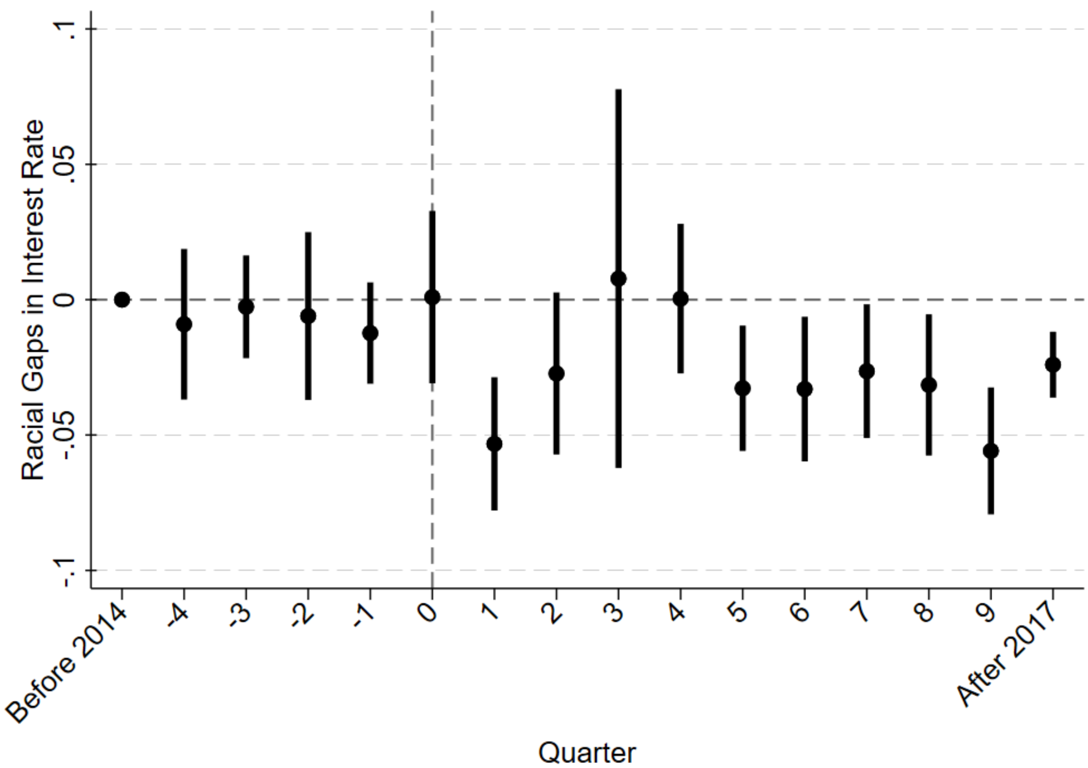


Figure 2. Racial Disparities in Rejection Rates Between CFPB-Supervised and Unsupervised Lenders

This figure presents average excess rejection rates for minority borrowers relative to non-minority borrowers across CFPB-supervised and unsupervised lenders. The data are sourced from the HMDA origination dataset. I use residualized rejection rates after controlling 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 specified in Equation (3). To smooth short-term fluctuations and better capture the long-run trend, I apply a three-year moving average. The vertical dashed line indicates mid-2015, when the CFPB began disclosing complaint narratives.

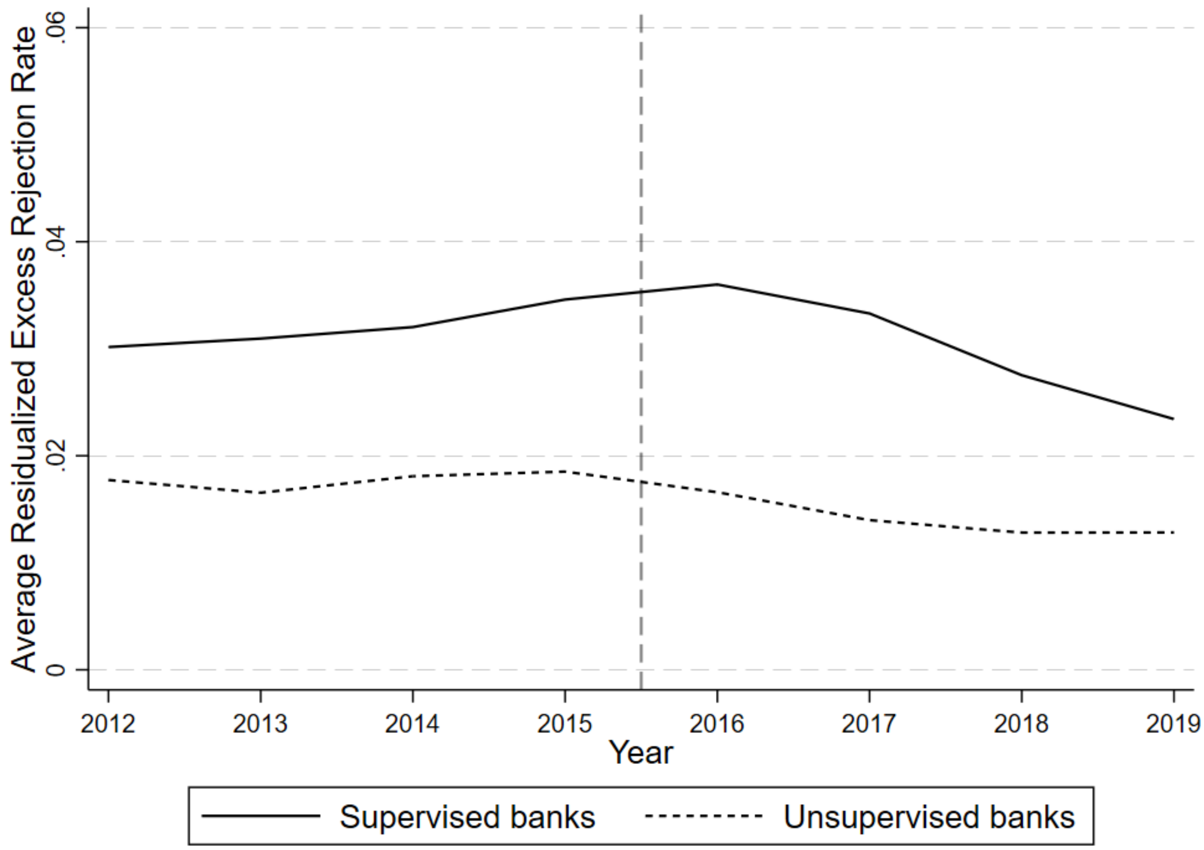
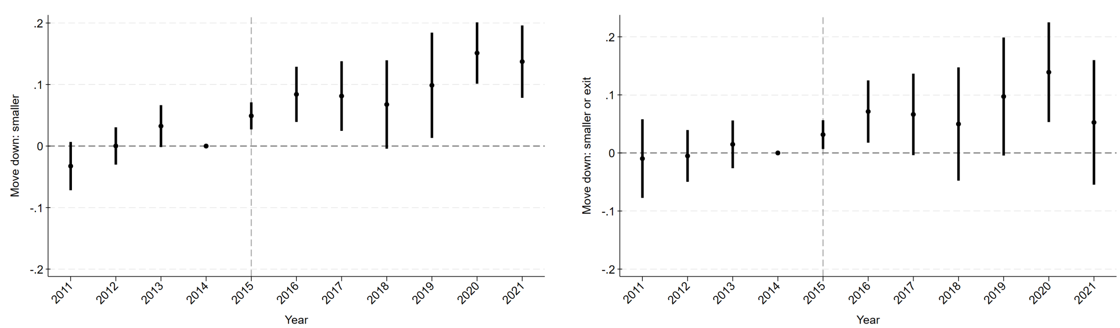


Figure 3. Estimated Effects of Disclosure on Loan Officers' Downward Career Movements

This figure presents results from an event study that examines the impact of the CFPB disclosure policy on loan officers' downward career movements. The event window spans from four years before to six years after 2015, the year the disclosure policy was implemented. The estimates are based on the NMLS–CFPB merged dataset covering 2011–2022. Observations from 2022 are excluded because career transitions into 2023 are not observable within the data. The analysis focuses on two outcomes: (a) *Move Down*, defined as switching to a smaller lender than the previous employer; and (b) *Move Down or Exit*, which captures switching to a smaller lender or exiting the industry. The Y-axis reflects the probability of such transitions, while the X-axis marks years relative to the policy implementation. The regression specification follows that of Table 5. Black circles represent annual coefficient estimates, and vertical lines denote the 95% confidence intervals.



(a) Move Down

(b) Move Down or Exit

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

This table reports summary statistics for key variables from the HMDA-GSE merged dataset, which I use to examine the effects of the disclosure policy on interest rates and default rates for mortgage loan applications. Treated lenders are defined as those with total assets between \$10 billion and \$100 billion prior to the policy, while control lenders have total assets below \$10 billion. All variables are averaged over the pre-disclosure period from 2011 to May 2015. The table presents unconditional means, standard deviations, and p-values for differences in means between the treated and control groups. Standard errors for the t-tests are clustered at the lender 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
Default Rate (Subprime Borrowers)	12,484	0.196	0.397	0.218	0.413	0.194	0.395	0.414
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
Purchase	340,320	0.492	0.500	0.514	0.500	0.489	0.500	0.712
Cash-out Refinance	340,320	0.181	0.385	0.163	0.370	0.184	0.388	0.390
Refinance	340,320	0.326	0.469	0.323	0.467	0.327	0.469	0.931

Table 2. Estimated Effects of Disclosure on Interest Rates

This table reports the estimated effects of complaint narrative disclosure on racial gaps in interest rates. The analysis uses the HMDA-GSE merged dataset covering 2011–2019. The dependent variable is the interest rate on originated fixed-rate mortgages. Each column presents estimates on four independent variables, while the main components—*Treat*, *Post*, and their interaction *Treat*×*Post*—are absorbed by fixed effects due to collinearity. The key coefficient of interest, *Treat*×*Post*×*Minority*, captures the effect of disclosure on the racial gap in interest rates. Column (1) includes fixed effects for lender-time and joint fixed effects for pricing grid, loan purpose, and time. Column (2) adds fixed effects for borrower characteristics. Column (3) replaces the separate fixed effects in column (1) with joint fixed effects for lender, pricing grid, loan purpose, and time. Column (4) builds on column (3) and substitutes borrower controls with more stringent similar borrower fixed effects. Column (5) extends column (2) by adding joint fixed effects for lenders and similar borrowers. Standard errors, shown in parentheses, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate 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. Estimated Effects of Disclosure on Default Rates Among Subprime Minority Borrowers

This table reports the estimated effects of complaint narrative disclosure on racial gaps in default rates, using data from the HMDA-GSE merged dataset covering 2011–2019. The dependent variable is an indicator for whether a loan becomes delinquent by 90 days or more. Columns (1)–(3) restrict the sample to borrowers with credit scores below 660, capturing results for subprime borrowers. Column (4) reports results for the full sample. Each column presents estimates on four independent variables, while *Treat*, *Post*, and their interaction *Treat*×*Post* are absorbed by fixed effects due to collinearity. The main coefficient of interest, *Treat*×*Post*×*Minority*, captures the effect of disclosure on racial gaps in default rates. Column (1) includes lender-time (year-month) and ZIP code-by-year fixed effects. Column (2) adds controls for loan characteristics—including interest rate, loan purpose, and the logarithm of loan amount—and applicant characteristics, such as gender, the logarithm of income, and the presence of a co-applicant. Column (3) builds on column (2) by adding pricing grid–loan purpose fixed effects. Column (4) applies the same specification as column (2) to the full sample. Standard errors, shown in parentheses, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate 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 Code×Year FE	YES	YES	YES	YES
Bucket×Loan Purpose FE			YES	
Observations	25,751	24,173	24,171	1,243,596
R-square	0.492	0.500	0.501	0.163

Table 4. Estimated Effects of Disclosure on Rejection Rates

This table reports the estimated effects of complaint narrative disclosure on racial gaps in rejection rates, using data from the HMDA origination dataset covering 2011–2019. The dependent variable is an indicator for whether a loan application is rejected. Each column presents estimates on four independent variables, while *Treat*, *Post*, and their interaction term *Treat*×*Post* are absorbed by fixed effects due to collinearity. The key coefficient of interest, *Treat*×*Post*×*Minority*, captures the effect of disclosure on racial disparities in rejection rates. Columns (1) and (2) report the baseline results. Column (1) includes lender-by-year fixed effects, joint fixed effects for lenders and similar borrowers, and joint fixed effects for lenders and census tracts. Column (2) further includes census tract-by-year fixed effects. Columns (3) and (4) examine heterogeneity based on the intensity of discrimination complaints (measured as the ratio of complaint counts to total assets), using the specification from column (2). Standard errors, shown in parentheses, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Baseline		Complaint Intensity	
			High	Low
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 5. Estimated Effects of Disclosure on Loan Officers' Downward Career Movements

This table reports the estimated effects 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 the CFPB consumer complaints database. Two dependent variables are used: (1) *Move Down*, which equals one if a loan officer moves to a smaller lender than their previous employer; and (2) *Move Down or Exit*, which equals one if a loan officer switches to a smaller lender or exits the industry. The coefficients on $Treat \times Post$ capture the treatment effects for loan officers at lenders subject to complaint narrative disclosure. CFPB-supervised lenders are further divided into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median during 2011–2022. I interact these indicators with *Post* to estimate differential effects. The coefficient on $\mathbf{1}(Treat\_High\_Compl) \times Post$ measures the effect of disclosure on downward movements among loan officers at CFPB-supervised branches with high complaint intensity. The coefficient on $\mathbf{1}(Treat\_Low\_Compl) \times Post$ captures the corresponding effect among CFPB-supervised branches with low complaint intensity. All regressions control for loan officer career duration. Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dep. Var =	Move Down		Move Down or Exit	
$Treat \times Post$	0.060*		0.056*	
	(0.036)		(0.032)	
$\mathbf{1}(Treat\_High\_Compl) \times Post$		0.073***		0.067***
		(0.019)		(0.016)
$\mathbf{1}(Treat\_Low\_Compl) \times Post$		0.056		0.053
		(0.039)		(0.035)
Control	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES
Lender FE	YES	YES	YES	YES
ZIP Code FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	1,050,019	1,050,019	1,134,451	1,134,451
R-squared	0.732	0.732	0.682	0.682

Table 6. Estimated Effects of Disclosure on Loan Officers' Promotion Prospects

This table reports the estimated effects of complaint narrative disclosure on loan officers' promotion prospects. Because managerial status data from the NMLS dataset is only available starting from 2014, the analysis covers the period from 2014 to 2022. Three dependent variables are used: (1) *Promoted*, an indicator 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 previous employer; and (3) *Int_Promoted*, equal to one if the loan officer is promoted within the same branch. The coefficients on $Treat \times Post$ capture the treatment effects for loan officers at lenders subject to complaint narrative disclosure. CFPB-supervised branches are further divided into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median during 2015–2022. I interact these group indicators with *Post* to estimate differential effects. The coefficient on $1(Treat\_High\_Compl) \times Post$ measures the effect of disclosure on promotion outcomes among loan officers at CFPB-supervised lenders with high complaint intensity. The coefficient on $1(Treat\_Low\_Compl) \times Post$ captures the corresponding effect among lenders with low complaint intensity. The last row of the table reports the p-value from a Wald test comparing the coefficients on $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$. All specifications control for the loan officer career duration. The variables *Treat*, $1(Treat\_High\_Compl)$, and $1(Treat\_Low\_Compl)$ are absorbed by lender fixed effects, while *Post* is absorbed by year fixed effects. Standard errors, shown in parentheses, are clustered at the lender level. The symbols *, **, and *** indicate 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_High_Compl)		-0.035***		-0.006*		-0.016***
×Post		(0.004)		(0.004)		(0.002)
1(Treat_Low_Compl)		-0.028***		-0.003		-0.014***
×Post		(0.005)		(0.005)		(0.002)
Control	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
ZIP Code 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***		0.044**		0.016**

Table 7. Rival Banks' Response to the Disclosure Policy

This table reports the estimated effects of complaint narrative disclosure on the entry of rival lenders. The analysis uses data from the HMDA origination dataset covering 2011–2019, aggregated at the county-year level. The dependent variable, *BankDensity*, is defined as the number of unique bank brands per ten thousand individuals at the county level. The table presents estimates for the interaction term *Disc_ Compl_ Share*×*Post*, while the components *Disc_ Compl_ Share* and *Post* are absorbed by fixed effects due to collinearity. This coefficient captures changes in the number of unique lender brands per ten thousand people in counties with greater exposure to the disclosure policy. The variable *Disc_ Compl_ Share* is constructed as the number of discrimination-related complaints received in a county in 2015 divided by the number of loans issued to minority applicants, serving as a proxy for the intensity of disclosure exposure. All regressions control for the growth rate of per capita income, the unemployment rate, county fixed effects, and state-by-year fixed effects. Columns (1)–(3) report outcomes for the current year (t), the following year ($t+1$), and two years ahead ($t+2$), respectively. Standard errors, shown in parentheses, are clustered at the county level. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
	T	T+1	T+2
Dep. Var =		Bank Density	
Disc_ Compl_ Share×*Post*	12.219***	13.773***	14.152***
	(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 8. Consumer Response to the Disclosure Policy

This table reports the estimated effects of complaint narrative disclosure on consumer mortgage application behavior, using data from the HMDA origination dataset over the period 2011–2019. The data are organized at the county-lender-minority-year level. The first dependent variable, *Application Number*, is the logarithm of the number of mortgage applications submitted by minority or white borrowers within a given county-lender-year. The second dependent variable, *Application Amount*, is the logarithm of the total dollar amount of such applications within the same context. The table presents estimates for two triple-difference terms: $1(Treat\_High\_Compl) \times Post \times Minority$ and $1(Treat\_Low\_Compl) \times Post \times Minority$. CFPB-supervised lenders are classified into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median between 2015 and 2019. The coefficient on $1(Treat\_High\_Compl) \times Post \times Minority$ captures the differential change in racial disparities in mortgage applications at CFPB-supervised lenders with high complaint intensity, relative to CFPB-unsupervised lenders, after disclosure. Similarly, the coefficient on $1(Treat\_Low\_Compl) \times Post \times Minority$ reflects the corresponding change among CFPB-supervised lenders with low complaint intensity, relative to CFPB-unsupervised lenders. All regressions control for (i) the percentage of mortgages a lender approves in a given county, (ii) an indicator equal to one if the lender has a physical presence in the county during that year, and (iii) the logarithm of total deposits held by the lender's branches in the county. Columns (1) and (3) include lender-by-year and lender-by-county fixed effects, while columns (2) and (4) introduce county-year fixed effects. Standard errors, shown in parentheses, are clustered at the county and year level. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dep. Var =	Application Number		Application Amount	
1(Treat_ High_ Compl)×Post	-0.167*	-0.160*	-0.291***	-0.284**
×Minority	(0.082)	(0.081)	(0.082)	(0.090)
1(Treat_ Low_ 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. Market Response to the Disclosure Policy

This table reports the estimated effects of complaint narrative disclosure on investor behavior in the stock market. The analysis uses data on all publicly listed lenders contained in the HMDA origination dataset. I use the U.S. Daily Event Study tool provided by WRDS to estimate cumulative abnormal returns (CARs) based on the disclosure date of June 25, 2015. The dependent variables are the CARs computed using the Capital Asset Pricing Model (CAPM) and the Market Adjusted Model (denoted as *Market*), across three event windows: $[-1, +1]$, $[-2, +2]$, and $[-3, +3]$ trading days. The main independent variables are two indicators: $1(Treat\_High\_Compl)$, which equals one for treated lenders with above-median ratio of discrimination complaints to total assets in 2015, and $1(Treat\_Low\_Compl)$, which equals one for treated lenders with below-median ratio. All regressions control for total assets, the equity-to-assets ratio, return on assets, and total deposits. A t-test is conducted to assess the difference between the coefficients on $1(Treat\_High\_Compl)$ and $1(Treat\_Low\_Compl)$. The symbols *, **, and *** indicate 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]
1(Treat_High_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)
1(Treat_Low_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 10. Estimated Effects of Disclosure on Homeownership

This table reports the real effects of complaint narrative disclosure on homeownership. The analysis uses data from the HMDA origination dataset covering 2011–2019, aggregated at the county-year level. The dependent variable, *Homeownership Ratio*, is sourced from the ACS datasets and measures homeownership rates by race at the county-year level. The table presents estimates for the interaction term $Disc\_Compl\_Share \times Post$, while the components $Disc\_Compl\_Share$ and $Post$ are absorbed by fixed effects due to collinearity. This coefficient captures changes in racial disparities in homeownership in counties more exposed to disclosure. The variable $Disc\_Compl\_Share$ is defined as the number of discrimination-related complaints filed in a county in 2015 divided by the number of loans issued to minority borrowers, and serves as a proxy for exposure to disclosure. All regressions control for the growth rate of per capita income, the unemployment rate, and the logarithm of population. County fixed effects and state-by-year fixed effects are included in all specifications. Columns (1)–(3) report outcomes for: (i) homeownership among minority residents (Black and Hispanic); (ii) homeownership among white residents; and (iii) the racial gap in homeownership, defined as the difference between minority and white homeownership rates. Standard errors, shown in parentheses, are clustered at the county level. The symbols *, **, and *** indicate 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**
$Share \times Post$	(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

Appendix A. Supplementary Figures and Tables

Figure A1. Illustrative Loan-Level Price Adjustment Grid Used by Fannie Mae

The figure presents Fannie Mae's 2018 loan-level price adjustment (LLPA) grid, as published in the Fannie Mae Selling Guide on June 5, 2018. A corresponding grid at Freddie Mac, referred to as the Credit Fees in Price chart, serves a similar function. These matrices determine the additional guarantee fee (g-fee) that lenders must pay to the GSE in exchange for mortgage guarantees, based entirely on the borrower's credit score and loan-to-value (LTV) ratio.

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 A2. Effect of CFPB Disclosure on Racial Interest Rate Gaps Across Lender Asset Caps

This figure illustrates the effect of CFPB disclosure on racial interest rate gaps across different lender asset caps. The horizontal axis plots the asset size limits used for selecting caps, increasing in $5 billion increments from $25 billion to $95 billion. For each cap threshold, I estimate a triple-difference model that includes a triple-interaction term. The vertical axis shows the corresponding coefficient estimates for this term, based on the specification in column (2) of Table 2. Each dot represents a point estimate, and the attached lines denote the confidence intervals at the 10% significance level.

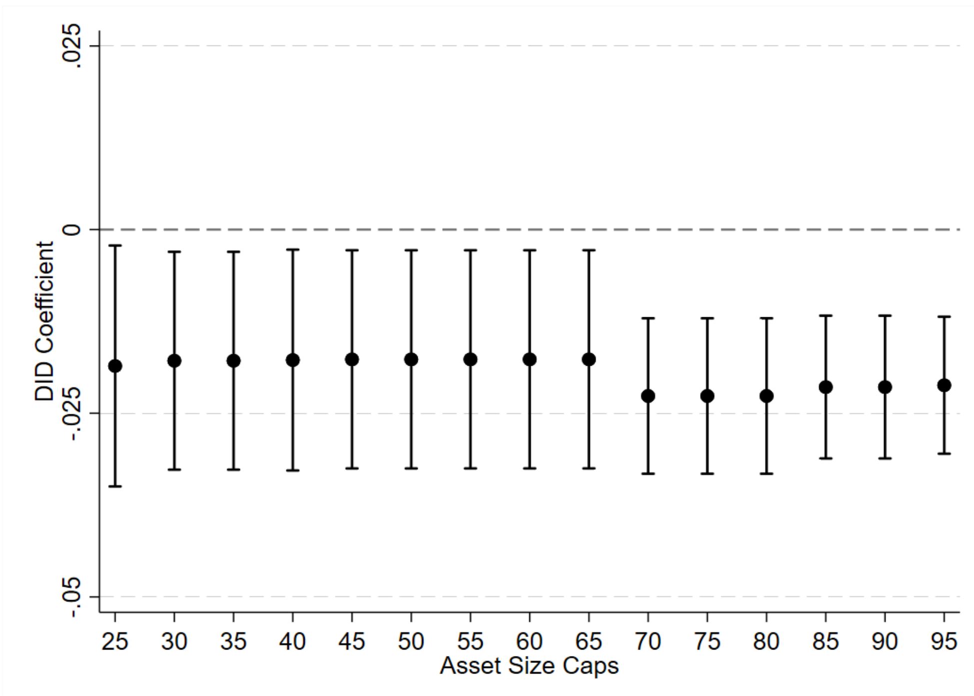


Figure A3. Effect of CFPB Disclosure on Racial Default Rate Gaps Across Lender Asset Caps

This figure illustrates the effect of CFPB disclosure on racial default rate gaps across different lender asset caps. The horizontal axis plots asset size limits used for selecting caps, increasing in $5 billion increments from $25 billion to $95 billion. At each cap, I estimate a triple-difference model that includes a triple-interaction term. The vertical axis displays the corresponding coefficient estimates based on the specification in column (1) of Table 3. Each dot represents a point estimate, and the attached lines indicate confidence intervals at the 10% significance level.

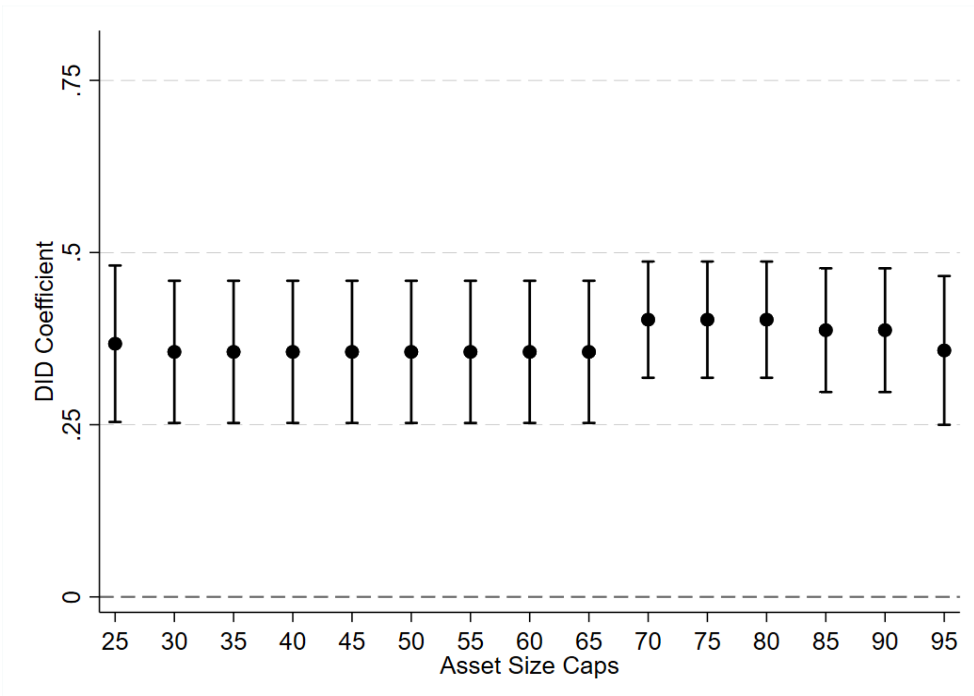


Figure A4. Dynamic Effect of CFPB Disclosure on Mortgage Rejection Rates

This figure presents yearly coefficient estimates for racial disparities in mortgage rejection rates between treated and control lenders. The year immediately prior to the disclosure serves as the reference period. The estimates are based on the HMDA dataset covering 2011–2019. The vertical axis measures the magnitude of racial disparities in rejection rates, and the horizontal axis indicates event time, with period zero denoting the year of policy implementation. The regression specification follows that of column (2) in Table 4. Black circles represent point estimates, and vertical lines indicate 95% confidence intervals.

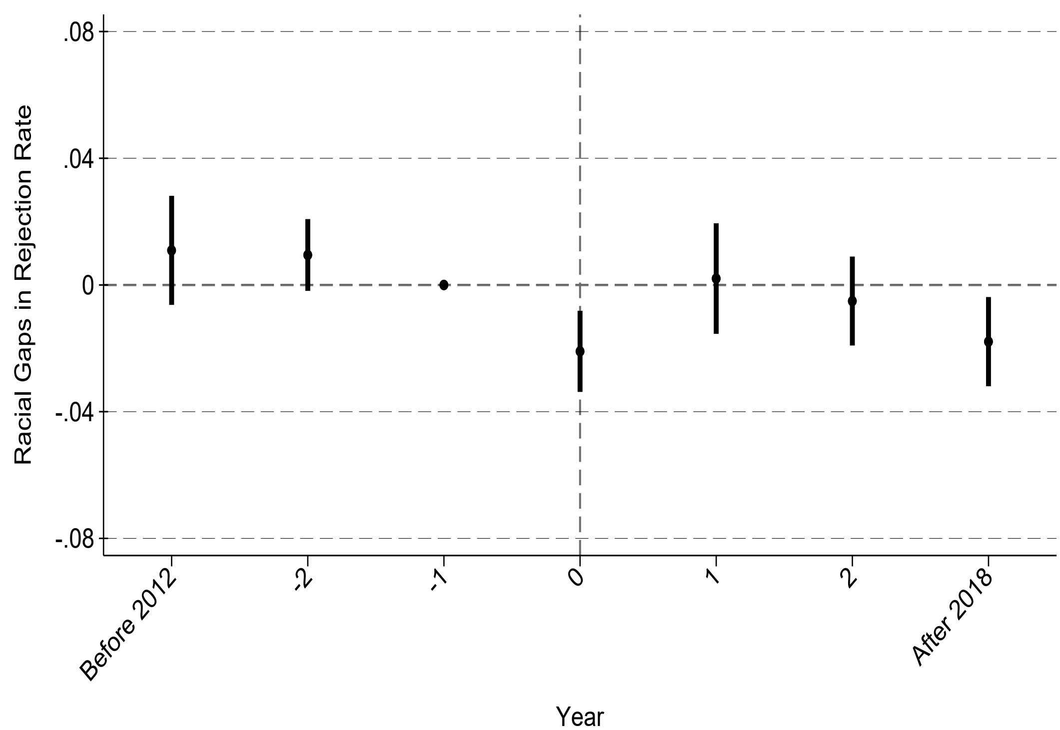


Figure A5. Trend in the Number of Mortgage Loan Complaints

This figure illustrates trends in the number of mortgage loan complaints from 2012 to 2019. The vertical axis reports the total number of complaints, and the horizontal axis is divided by month, with year labels indicated along the timeline. The red vertical line marks June 2015, when the CFPB began publicly releasing consumer complaint narratives.

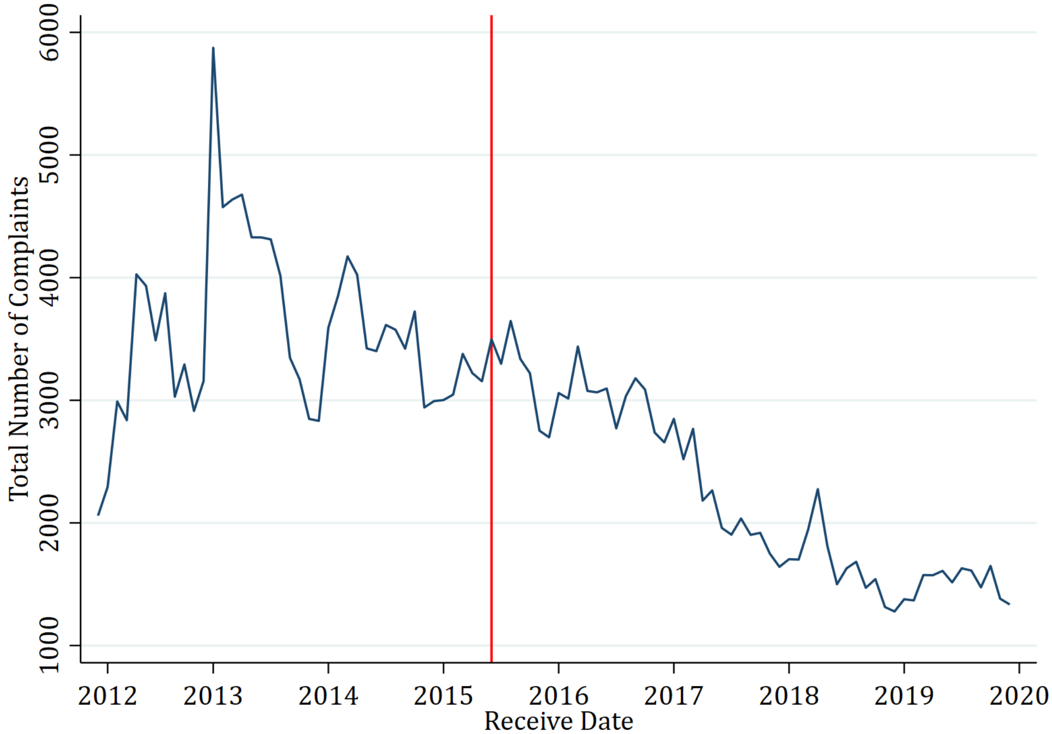
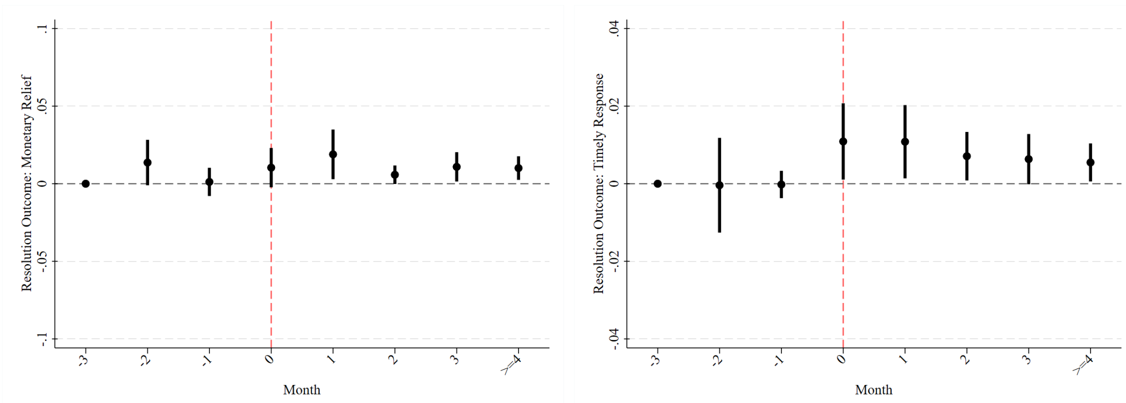


Figure A6. Dynamic Effect of CFPB Disclosure on Complaint Resolution Outcomes

This figure presents results from an event study examining the effect of the CFPB disclosure policy on complaint resolution outcomes. The analysis focuses on two outcome variables: (a) Monetary Relief, which indicates whether a complaint results in financial compensation, and (b) Timely Response, which measures the promptness of the institution's reply. The vertical axis shows coefficient estimates for the double-interaction terms, and the horizontal axis indicates event time, with period zero marking the disclosure date. The regression specification corresponds to that of Table A17. Black circles represent point estimates, and vertical lines denote confidence intervals at the 10% significance level.



(a) Monetary Relief

(b) Timely Response

Figure A7. Example of Lender Rankings Based on Racial Interest Rate Disparities

This figure presents rankings from the Discrimination Warrior website, highlighting racial disparities in interest rates for ZIP Code 02135, a neighborhood in Boston. The platform allows users to select either interest rates or rejection rates and input a ZIP Code. Based on the input, the website generates a ranked list of lenders, showing the severity of estimated discrimination against minority applicants.

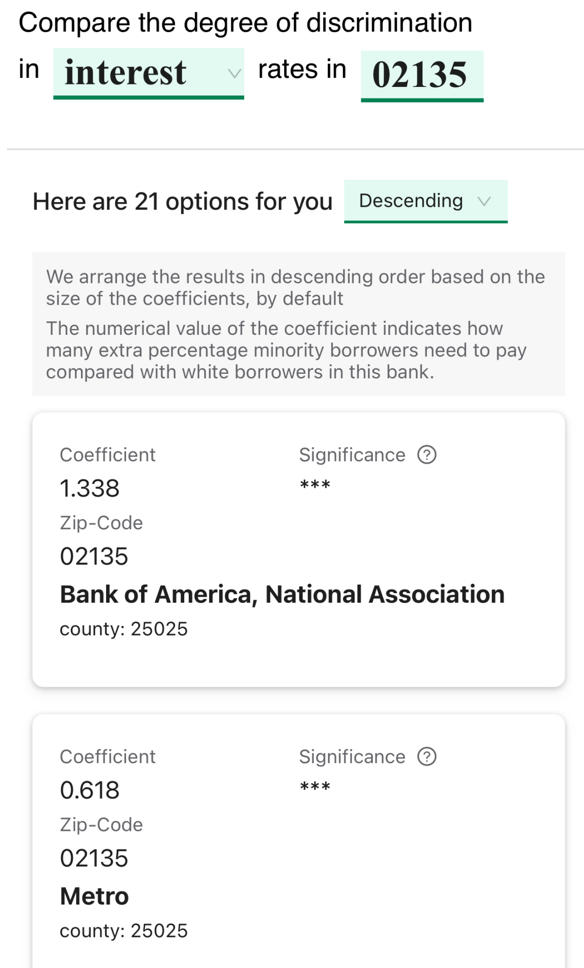


Figure A8. Example of Lender Rankings Based on Racial Rejection Rate Disparities

This figure presents rankings from the Discrimination Warrior website on racial disparities in loan rejection rates for ZIP Code 02135, a neighborhood in Boston. The platform allows users to select either interest rates or rejection rates and input a ZIP Code. Based on the selection, the website displays a ranked list of lenders according to the estimated severity of rejection rate disparities affecting minority applicants.

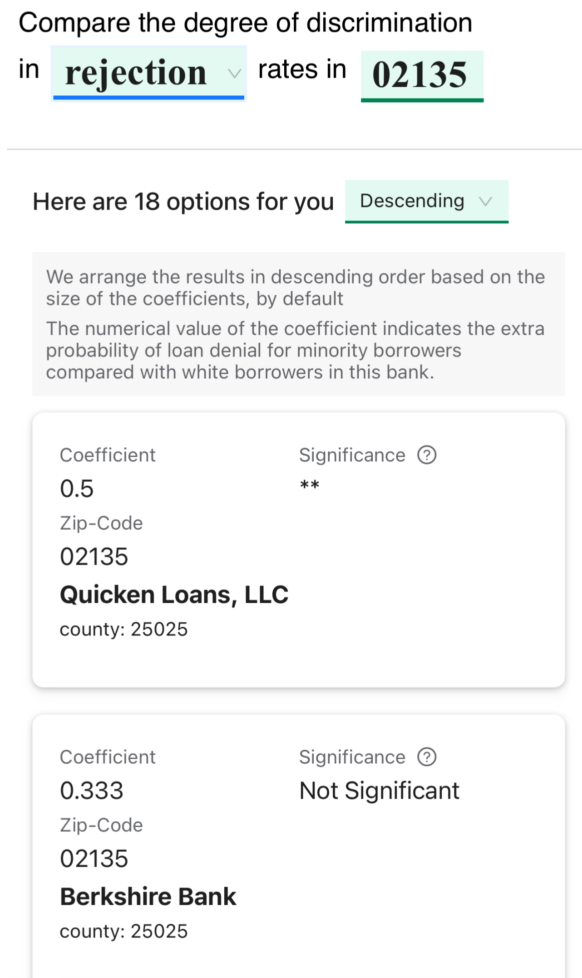


Table A1. Illustrative Example of a Mortgage Discrimination Complaint

This table presents a consumer complaint related to "Applying for a mortgage or refinancing an existing mortgage," as publicly disclosed by the CFPB. The example includes fields 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. ... I have savings of $30,000.00. The house I was looking to purchase cost $180,000.00.
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 rejected at application, marked as 1 for rejection, 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 Hispanic 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.
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

Continued on next page

Table A2 – *Continued from previous page*

Variable	Definition
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.
NMLS 2011-2022	
Promoted	An indicator variable equal to 1 in the current year and all subsequent years if the officer is a non-manager in the current year and a manager in the next year, and 0 otherwise.
External Promotion to Larger Lender	An indicator variable equal to 1 in the current year and all subsequent years if the officer becomes a manager at a different lender and the destination lender's size (measured by the number of employees) in the next year exceeds the origin's size in the current year, and 0 otherwise.
Internal Promotion	An indicator variable equal to 1 in the current year and all subsequent years if the officer becomes a manager within the same employer in the next year, and 0 otherwise.
Move to Smaller Lender	An indicator variable equal to 1 in the current year and all subsequent years if the officer changes employer in the next year and the destination lender's size in the next year is smaller than the origin's size in the current year, and 0 otherwise.

Continued on next page

Table A2 – *Continued from previous page*

Variable	**Definition**
Move to Smaller Lender or Exit	An indicator variable equal to 1 in the current year and all subsequent years if the officer either moves to a smaller lender or has no NMLS-recorded employer in the next year, and 0 otherwise.
Other Sources	
Complaints Number	A continuous variable indicating the number of complaints a bank received in 2015, with data gathered from the CFPB dataset.
Penalty	A continuous variable indicating the amounts of fines related to discrimination lawsuits a lender faces 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 Loan-Level Datasets Used in the Analysis

This table reports summary statistics for the key variables constructed from the Loan-Level Datasets used in this paper. Panel A presents results based on the HMDA-GSE merged dataset, while Panel B reports statistics from the HMDA origination dataset. These datasets allow me to examine the effects of the disclosure policy on interest rates, default rates, and rejection rates, as well as conduct additional analyses. In both panels, I report the number of observations, mean, standard deviation, minimum, and maximum values for the main variables. The sample period spans 2011 to 2019, and all lenders included have total assets below $100 billion, consistent with the sample restriction applied 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. Summary Statistics of Loan-Officer Variables Used in the Analysis

This table reports descriptive statistics for the variables used in the loan-officer regressions. Panel A presents statistics for downward-mobility outcomes using a sample from 2011–2022. Panel B presents statistics for promotion outcomes; this sample begins in 2014 because manager-related information is only available in the NMLS data from that year onward. In both panels, the number of observations, mean, standard deviation, minimum, and maximum values are reported for each variable.

	(1)	(2)	(3)	(4)	(5)
Variables	N	Mean	SD	Min	Max
Panel A: Downward Mobility Outcomes					
Move Down	1,050,019	0.417	0.493	0.000	1.000
Move Down or Exit	1,134,451	0.485	0.500	0.000	1.000
Treat	1,050,019	0.101	0.302	0.000	1.000
Treat_High_Compl	1,050,019	0.010	0.102	0.000	1.000
Treat_Low_Compl	1,050,019	0.091	0.288	0.000	1.000
Tenure (years)	1,050,019	3.917	3.024	0.000	10.000
Panel B: Promotion Outcomes					
Promoted	806,139	0.215	0.411	0.000	1.000
External Promoted to Bigger	806,139	0.055	0.228	0.000	1.000
Internal Promoted	806,139	0.078	0.268	0.000	1.000

Table A5. Effect of CFPB Disclosure on Interest Rates (Clustered at the Lender Level)

This table reports robustness checks of the estimates presented in Table 2, using standard errors clustered at the lender level. The estimates assess the effect of complaint narrative disclosure on racial interest rate gaps. The analysis uses the HMDA-GSE merged dataset covering 2011–2019. The dependent variable is the interest rate on originated fixed-rate mortgages. Each column reports coefficients on four independent variables; the *Treat*, *Post*, and *Treat×Post* terms are absorbed by fixed effects due to collinearity. The coefficient on *Treat×Post×Minority* captures the effect of disclosure on racial disparities in interest rates. Column (1) includes fixed effects for lender-time interactions and for pricing grid-loan purpose-time interactions. Column (2) adds controls for borrower characteristics. Column (3) replaces the two fixed effects from column (1) with joint fixed effects for lender-pricing grid-loan purpose-time. Column (4) builds on column (3) and replaces borrower characteristics with similar borrower fixed effects. Column (5), based on column (2), introduces joint fixed effects for lenders and similar borrowers. Standard errors, shown in parentheses, are clustered at the lender level. *, **, and *** indicate statistical 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 A6. Heterogeneous Effects of Disclosure on Interest Rates: Roles of Discrimination Complaints and Search Attention

This table reports the heterogeneous effects of disclosure on racial disparities in interest rates, using data from the HMDA-GSE merged dataset spanning 2011–2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The table presents heterogeneous effects based on discrimination complaints and Google search attention by comparing two subgroups within the treated group to a common control group. In columns (1) and (2), the treated group is divided by the intensity of discrimination complaints (measured as the ratio of complaint counts to total assets). In columns (3) and (4), the treated group is divided by the level of Google search attention. The estimated coefficient on *Treat*×*Post*×*Minority* captures the treatment effect in each subgroup. All specifications include joint fixed effects for lender, pricing grid, loan purpose, and time, as well as fixed effects for borrower characteristics. Standard errors, shown in parentheses, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Complaint Intensity		Google Search Index	
	High	Low	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 A7. Replication Results: Racial Interest Rate Differentials

This table replicates Table 3 from Bartlett et al. (2022). The analysis uses the HMDA-GSE merged dataset from 2011 to 2019 and applies the same model specification as in the original study. The dependent variable is the interest rate on originated fixed-rate mortgages. The key independent variable equals one if the borrower is Black or Hispanic, and zero otherwise. Standard errors are reported in parentheses and are clustered at the lender level. *, **, and *** indicate statistical 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 A8. Effect of CFPB Disclosure on Interest Rates under Alternative Fixed Effects Specifications

This table extends the analysis in Table 2 by examining the effect of CFPB disclosure on interest rates under alternative fixed effects specifications. The analysis uses the HMDA-GSE merged dataset from 2011 to 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*, which captures the effect of disclosure on racial interest rate disparities. Panels A, B, and C implement state-year, ZIP Code-year, and census tract-year joint fixed effects, respectively, offering progressively localized perspectives. Across all panels, columns apply different fixed effects strategies. Column (1) includes lender-time and pricing grid-loan purpose-time joint fixed effects. Column (2) adds controls for borrower characteristics. Column (3) replaces the fixed effects from column (1) with joint lender-pricing grid-loan purpose-time fixed effects. Column (4) builds on column (3) and replaces borrower characteristics with similar borrower fixed effects. Column (5), based on column (2), introduces joint fixed effects between lenders and similar borrowers. Standard errors, reported in parentheses, are clustered at the lender and year level. *, **, and *** indicate statistical 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 ZIP Code×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)
ZIP Code×Year FE	YES	YES	YES	YES	YES
Observations	1,276,328	1,244,272	997,712	988,428	765,448
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

Continued on next page

Table A8 – *Continued from previous page*

	(1)	(2)	(3)	(4)	(5)
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 A9. CFPB Disclosure and Racial Disparities in Interest Rates: Identification through Difference-in-Discontinuities

This table evaluates how CFPB disclosure affects racial gaps in interest rates using a difference-in-discontinuities design at the \$10 billion asset threshold. The sample includes depository institutions under CFPB supervision at this cutoff. I exclude affiliates of treated institutions whose total assets are below \$10 billion, because they remain supervised and would violate key assumptions. The design contrasts pre- and post-disclosure discontinuities at the threshold. I estimate the specification separately for minority and white borrowers.

$$\begin{aligned} \mathit{InterestRate}_{i,l,t} = {} & \alpha_0 + \alpha_1\big(\mathit{Assets}_{l,t} - 10 \text{ billion}\big) + \mathit{Over10}_{l,t}\big(\beta_0 + \beta_1\big(\mathit{Assets}_{l,t} - 10 \text{ billion}\big)\big) \\ & + \mathit{Post}_t\Big[\gamma_0 + \gamma_1\big(\mathit{Assets}_{l,t} - 10 \text{ billion}\big) \\ & + \mathit{Over10}_{l,t}\big(\delta_0 + \delta_1\big(\mathit{Assets}_{l,t} - 10 \text{ billion}\big)\big)\Big] \\ & + \mu_{Lender} + \mu_{Bucket \times LoanPurpose} + \varepsilon_{i,l,t}. \end{aligned} \tag{10}$$

$\mathit{InterestRate}_{i,l,t}$ is lender l's mortgage interest rate at time t for loan applicant i. $\mathit{Over10}_{l,t}$ equals one if total assets exceed \$10 billion (measured in 2015 Q1), and Post_t equals one for periods after 2015 Q2. I include lender fixed effects and bucket–purpose fixed effects. The coefficient δ_0 is the difference-in-discontinuities (diff-in-disc) estimator, capturing the treatment effect of CFPB disclosure. Columns (1) and (3) report results for minority borrowers, while columns (2) and (4) report results for white borrowers. Columns (1)–(2) only include lender fixed effects, whereas columns (3)–(4) additionally include bucket–purpose fixed effects. All specifications focus on institutions with total assets between \$5 billion and \$15 billion, with the cutoff at \$10 billion. Reported p-values correspond to cross-group equality tests (H_0: Minority = White) within each regression specification. Standard errors are clustered at the institution level (in parentheses). *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Minority	White	Minority	White
Dep. Var =	Interest Rate			
Diff-in-Disc	-0.074*	-0.013	-0.065*	-0.010
	(0.036)	(0.045)	(0.033)	(0.043)
p-value	0.036		0.050	
Bandwidth	[5,15]			
Lender FE	YES	YES	YES	YES
Bucket × Loan Purpose FE			YES	YES
Observations	1,943	24,966	1,935	24,959
R-squared	0.154	0.085	0.336	0.232

Table A10. Effects of the 2013 CFPB Database Launch on Racial Gaps in Mortgage Interest Rates

This table reports the effects of the 2013 CFPB database launch on racial gaps. *Post2013* equals one for periods after 2013Q1 and zero otherwise. The dependent variable is the interest rate on originated fixed-rate mortgages. The key coefficient, *Treat×Post2013×Minority*, captures the effect of the 2013 CFPB database launch on the racial gap in interest rates. For comparability, each column employs the same set of fixed effects as Table 2. Panel A restricts the sample to January 2011–June 2015 to avoid contamination from the 2015 narrative disclosure, whereas Panel B uses the full 2011–2019 period as a robustness check. Standard errors, shown in parentheses, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Dep. Var =	Interest Rate				
Panel A. January 2011–June 2015					
Treat×Post2013×Minority	0.004	-0.001	0.003	-0.000	0.013
	(0.006)	(0.006)	(0.007)	(0.006)	(0.014)
Post2013×Minority	-0.007***	-0.003*	0.003**	0.004	-0.005
	(0.001)	(0.001)	(0.001)	(0.004)	(0.005)
Treat×Minority	-0.005	-0.006	0.000	-0.002	-0.016
	(0.007)	(0.007)	(0.009)	(0.007)	(0.013)
Minority	0.034***	0.027**	0.024**	0.026***	0.036***
	(0.006)	(0.006)	(0.006)	(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	337,076	335,182	242,922	231,518	157,184
R-squared	0.757	0.767	0.795	0.812	0.826
Panel B. Full Sample 2011–2019					
Treat×Post2013×Minority	-0.013	-0.019	-0.021	-0.022	-0.004
	(0.010)	(0.010)	(0.012)	(0.012)	(0.013)
Post2013×Minority	-0.006	0.000	0.003	0.002	-0.006**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)

Continued on next page

Table A10 – *Continued from previous page*

	(1)	(2)	(3)	(4)	(5)
Treat×Minority	-0.005	-0.007	-0.000	-0.001	-0.016
	(0.007)	(0.006)	(0.008)	(0.007)	(0.009)
Minority	0.034***	0.027***	0.024***	0.026***	0.035***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)
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 A11. Racial Disparities in Mortgage Discount Points

This table examines racial disparities in mortgage discount points between treated and control groups. The analysis uses the HMDA-GSE merged dataset for 2018–2019. The dependent variable is the number of discount points selected by borrowers. The coefficient on *Treat* × *Minority* tests whether minority borrowers in the treated group differ from those in the control group in their use of discount points. Column (1) includes lender-time interaction fixed effects and pricing grid-loan purpose-time joint fixed effects. Column (2) adds controls for borrower characteristics. Column (3) replaces the fixed effects in column (1) with joint lender-pricing grid-loan purpose-time fixed effects. Column (4) builds on column (3) and replaces borrower characteristics with similar borrower fixed effects. Column (5), based on column (2), introduces joint fixed effects between lenders and similar borrowers. Standard errors, reported in parentheses, are clustered at the lender and year level. *, **, and *** indicate statistical 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)	(20.193)	(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 A12. Effect of Disclosure on Fallout Rates

This table reports the regression results examining the impact of disclosure on loan fallout rates. The dependent variable is an indicator that equals one if an approved loan application fails to close (Fallout), and zero otherwise. The model employs a triple-difference framework with three key variables: $Treat$, an indicator for lenders subject to the disclosure requirement; $Post$, an indicator for the post-disclosure period; and $Minority$, an indicator for minority borrowers. The key coefficient of interest is the triple interaction term ($Treat \times Post \times Minority$), which captures how the disclosure requirement differentially affects the racial gap in loan fallout rates between treated and control lenders. Standard errors, displayed in parentheses, are clustered at the lender level. The symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dep. Var =	Fallout	
Treat×Post×Minority	-0.004	-0.004
	(0.006)	(0.005)
Treat×Minority	0.004	0.004
	(0.005)	(0.005)
Post×Minority	-0.005*	-0.005
	(0.003)	(0.003)
Lender × Year FE	YES	YES
Lender × Similar Borrower FE	YES	YES
Lender × Census Tract FE	YES	YES
Census Tract × Year FE		YES
Observations	8,523,551	8,492,584
R-squared	0.473	0.493

Table A13. Moderating Effect of Litigation Risk on the Impact of CFPB Disclosure on Interest Rates

This table reports the moderating effect of litigation risk on the relationship between CFPB disclosure and racial disparities in interest rates. The analysis uses the HMDA-GSE merged dataset covering 2011–2019. The dependent variable is the interest rate on originated fixed-rate mortgages. The continuous variable *Litigation* serves as the moderator. Columns (1) and (2) use the number of litigations, while columns (3) and (4) use total penalty amounts (measured in millions of dollars). Columns (1) and (3) include joint fixed effects for lender-pricing grid-loan purpose-time and fixed effects for borrower characteristics. Columns (2) and (4) replace borrower characteristics with similar borrower fixed effects. Standard errors, reported in parentheses, are clustered at the lender and year-month level. *, **, and *** indicate statistical 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 A14. Dynamic Effect of CFPB Disclosure on Default Rates

This table reports coefficient estimates for racial disparities in default rates between treated and control lenders. For interpretability, I aggregate every three quarters into a single bin. The estimates are based on the HMDA-GSE merged dataset covering 2011–2019. Period zero corresponds to the implementation of the disclosure policy. The regression specification follows that of column (2) in Table 3. Standard errors, reported in parentheses, are clustered at the lender and year-quarter level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

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

Table A15. Effect of CFPB Disclosure on the Income and Loan Amount Distribution of Mortgage Borrowers

This table reports the effect of complaint narrative disclosure on racial disparities in borrower income and loan amount. The analysis uses the HMDA origination dataset covering 2011–2019. The dependent variables are borrower income and the logarithm of loan amount, both measured at the loan level. Each column presents coefficient estimates for four independent variables. The *Treat*, *Post*, and *Treat* × *Post* terms are absorbed by fixed effects due to collinearity. The key coefficient, *Treat* × *Post* × *Minority*, captures the effect of disclosure on racial differences in dependent variables. Columns (1) and (3) include lender-by-year fixed effects, joint fixed effects for lenders and similar borrowers, and joint fixed effects for lenders and census tracts. Columns (2) and (4) further add census tract-by-year fixed effects. In the baseline specification, the similar-borrower fixed effects are constructed based on the interactions of pricing grid, loan purpose, loan amount deciles, applicant income deciles, applicant gender, and co-applicant status. In these regressions, to mitigate multicollinearity, the similar-borrower fixed effects exclude applicant income deciles when borrower income is the dependent variable and exclude loan amount deciles when the logarithm of loan amount is the dependent variable. Standard errors, reported in parentheses, are clustered at the lender and year level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
Dep. Var=	Income		Amount	
Treat × *Post* × *Minority*	-0.013	-0.006	-0.002	0.001
	(0.020)	(0.020)	(0.005)	(0.005)
Post × *Minority*	-0.001	0.005	0.005	-0.002
	(0.018)	(0.015)	(0.003)	(0.003)
Treat × *Minority*	0.036	0.034	0.002	-0.000
	(0.030)	(0.029)	(0.005)	(0.005)
Minority	-0.215***	-0.218***	0.017***	0.021***
	(0.007)	(0.007)	(0.003)	(0.003)
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
Observations	24,125,319	24,122,302	23,942,609	23,939,317
R-squared	0.671	0.678	0.767	0.773

Table A16. Estimated Effects of Disclosure on Rejection Rates: LLM-Based Discrimination Detection

This table reports the estimated effects of complaint narrative disclosure on racial gaps in rejection rates using LLM-identified discrimination complaints. The analysis uses data from the HMDA origination dataset covering 2011–2019. The dependent variable is an indicator for whether a loan application is rejected. The key coefficients of interest, $Treat\_H \times Post \times Minority$ and $Treat\_L \times Post \times Minority$, capture the effect of disclosure on racial disparities in rejection rates for lenders with high and low LLM-identified discrimination complaint intensity (measured as the ratio of complaint counts to total assets), respectively. Column (1) examines CFPB-supervised lenders with above-median LLM-identified complaint intensity, while column (2) analyzes those with below-median intensity. Both specifications include lender-by-year fixed effects, joint fixed effects for lenders and similar borrowers, joint fixed effects for lenders and census tracts, and census tract-by-year fixed effects, consistent with the specification in column (2) of Table 4. Standard errors, are two-way clustered at the lender and year levels. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
Dep. Var =	Rejection	
	High LLM Complaint Intensity	Low LLM Complaint Intensity
$Treat\_H \times Post \times Minority$	-0.029***	
	(0.008)	
$Treat\_L \times Post \times Minority$		-0.017*
		(0.008)
$Treat\_H \times Minority$	0.071***	
	(0.004)	
$Treat\_L \times Minority$		0.030***
		(0.008)
Lender × Year FE	Yes	Yes
Lender × Similar Borrower FE	Yes	Yes
Lender × Census Tract FE	Yes	Yes
Census Tract × Year FE	Yes	Yes
Observations	15,004,734	16,702,481
R-squared	0.486	0.479
Diff. in Triple Interaction ($H - L$)	-0.012***	
Permutation Test (p-value)	0.002	

Table A17. Effect of CFPB Disclosure on Complaint Resolution Outcomes

This table presents the effect of CFPB disclosure on complaint resolution outcomes. The analysis uses complaint-level data from March 19, 2015, to December 31, 2016. The table focuses on two outcome variables. *Monetary Relief* is a dummy variable equal to one if the complaint results in monetary compensation for the consumer. *Timely Response* is a dummy variable equal to one if the institution responds promptly. The difference-in-differences model includes three key independent variables: $Treat$, $Post$, and $Treat \times Post$. The coefficient on $Treat \times Post$ captures the effect of disclosure on complaint resolution. All regressions include fixed effects for institution, ZIP Code, and year-month. Standard errors, reported in parentheses, are clustered at both the institution and year-month levels. *, **, and *** indicate statistical 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 Code FE	YES	YES
Year-Month FE	YES	YES
Observations	62,096	62,096
R-squared	0.225	0.421

Table A18. Disclosure and Month-to-Month Persistence in Consumer Complaints

This table examines whether the public release of complaint narratives affects the month-to-month persistence of complaints at the institutional level. The estimation sample covers complaints from the mortgage market between March 19, 2015, and December 31, 2016. The dependent variables are the number of complaints for institution i in month t+1, based on institution-month level data for two outcomes: (i) all complaints ($ComplNum_{i,t+1}$) and (ii) complaints with narratives ($NarrComplNum_{i,t+1}$). The key independent variables include contemporaneous complaint volumes in month t and their interactions with a policy dummy, $Post_t$, which captures the effect of disclosure on complaint trends. $Post_t$ equals one for months after June 2015 (when narratives became public) and zero during the buffer window (March to June 2015), representing the period when the CFPB began publicly releasing complaint narratives. All specifications include institution fixed effects and year-by-month fixed effects. Standard errors are clustered at the institution level and are shown in parentheses. The symbols *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	ComplNum$_{t+1}$	NarrComplNum$_{t+1}$
$ComplNum_t$	0.460***	
	(0.035)	
$ComplNum_t \times Post_t$	-0.132***	
	(0.041)	
$NarrComplNum_t$		0.410***
		(0.037)
$NarrComplNum_t \times Post_t$		-0.143***
		(0.050)
Institution FE	YES	YES
Year-Month FE	YES	YES
Observations	39,942	39,942
R-squared	0.978	0.967

Table A19. Rival Bank Responses to CFPB Disclosure: Alternative *Share* Measurement

This table reports the effect of complaint narrative disclosure on the entry of rival banks, using an alternative independent variable from that in Table 7. The analysis is based on the HMDA origination dataset from 2011 to 2019, aggregated at the county-year level. The dependent variable, *BankDensity*, is defined as the number of unique bank brands per 10,000 individuals in a county. The table presents estimates for the interaction term *Amount Share* × *Post*, while the main effects *Amount Share* and *Post* are absorbed by fixed effects due to collinearity. This coefficient captures changes in bank brand density in counties with greater exposure to the disclosure policy. Unlike Table 7, this specification uses *Amount Share*—defined as the 2015 market share of treated banks in each county—to measure disclosure exposure. All regressions control for income per capita growth, unemployment rates, county fixed effects, and state-by-year fixed effects. Columns (1) through (3) report the effects of *Amount Share* on bank density in the current year (t), the following year (t+1), and two years later (t+2), respectively. Standard errors, reported in parentheses, are clustered at the county level. *, **, and *** indicate statistical 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 A20. 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	-	-

Continued on next page

Table A20 – *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

Continued on next page

Table A20 – *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**

Appendix B. Clarifications on the Process of Merging Datasets

HMDA-GSE Merged Dataset: Linkage Process

This study uses two datasets: the Home Mortgage Disclosure Act (HMDA) and Government-Sponsored Enterprises (GSE) data, both providing detailed information on mortgage originations. No publicly available mapping file links these two datasets. Following Law and Mislang (2022), I apply a "fuzzy matching" approach to address this issue.

To link the HMDA and GSE datasets, I implement a two-step matching procedure using the *Fedmatch* R package developed by Cohen et al. (2021).

In the first step, I match lenders across the two datasets. I begin by aligning lender identifiers using the Robert Avery mapping file, which provides standardized lender names. For smaller lenders whose names are inconsistently reported or hard to trace in the GSE data, I apply fuzzy name-matching techniques based on Jaccard string similarity, followed by manual verification. This step produces a consistent lender ID that can be used across datasets.

In the second step, I perform loan-level matching within each lender–location cell. After successfully aligning lender IDs, I partition both datasets by lender ID and geographic information. Because the two datasets do not share a common geographic identifier, I use three-digit ZIP codes to approximate matches between HMDA census tracts and GSE counties. Within each resulting subset, I match individual loans based on observable origination characteristics. For post-2018 loans, additional variables—such as interest rate, loan purpose, and manufactured housing status—serve as useful anchors for refining the match.

This two-stage process ensures that loan-level matches are only attempted where lender identity and geographic overlap are reliably established.

HMDA-GSE Merged Dataset: Filtering Process

To exclude loans that do not meet standard lending criteria, I filter the dataset following Bartlett et al. (2022), resulting in a final sample of 1,869,345 mortgage loans. I remove GSE loans with credit scores below 620 (10,310 observations), LTV ratios below 0.30 (76,779 observations) or above 0.95 (121,430 observations). I also exclude loans with interest rates below 2.75% or above 8% (10,273 observations), loan amounts under $40,000 (14,207 observations), and loans with terms other than 360 months (769,736 observations). The final sample consists of 56.53% purchase mortgages (1,056,807 observations) and 43.47% refinance loans (812,538 observations).

For the main analysis, I use a subsample of loans from the final dataset with total lender assets between \$0 and \$100 billion. This subsample contains 1,330,621 loans, representing 71.18% of the total sample. This design choice ensures that any observed differences in loan outcomes can more plausibly be attributed to the disclosure intervention, rather than structural differences between large and smaller lenders.

HMDA Origination Dataset: Variable Supplementation Process

Besides setting loan interest rates, lenders also decide whether to approve or reject loan applications. After the 2008 mortgage crisis, many lenders adopted stricter approval standards beyond those required by the GSEs. Thus, even if a loan application meets creditworthiness requirements under the GSE underwriting system, the lender may still refuse the application.

A key challenge in analyzing rejection rates is the lack of loan-level information on certain underwriting variables—such as borrower credit scores and loan-to-value (LTV) ratios—since rejected loans are not originated. To address this issue, I leverage several features of the data. First, the HMDA dataset allows me to control for lender and year fixed effects, borrower income, and loan amount at the loan level. Second, I construct tract-level proxies for credit scores and LTV ratios using the merged HMDA-GSE data, following the approach of Bartlett et al. (2022). Including these proxies helps account for otherwise unobserved underwriting characteristics and significantly mitigates omitted-variable bias. Finally, to ensure consistency with the main analysis, I apply the same filters on loan amount and loan term as those used in the HMDA-GSE merged dataset.[49]

Using the matched HMDA-GSE dataset, I construct census tract-level proxies for underwriting variables. Specifically, I retain census tract, year, LTV, and credit score information and compute tract-year medians for LTV and credit score, following Bartlett et al. (2022). These tract-level aggregates are then merged back into the main HMDA sample to serve as proxies for otherwise unobserved applicant characteristics.

Because the tract-level LTV and credit score indicators are derived from the GSE dataset—which only includes originated loans—these proxies cannot be constructed for all HMDA observations. When I merge the aggregated values back to the full HMDA dataset using census tract and year identifiers, about 21.65% of the observations are unmatched due to missing GSE coverage. These cases are excluded from analyses that require the proxy

[49]Before 2018, the HMDA origination data do not report loan term, so this filter is applied only after 2018.

variables.

Finally, to examine the impact of complaint disclosure in greater detail, I merge the number of bank complaints published by the CFPB in 2015 and total bank assets from the first-quarter 2015 Call Report into the dataset. The merge uses the *arid* (a bank identifier defined by the CFPB) and year.[50] The final dataset used to analyze the effect of disclosure on loan rejection includes 42,198,230 loans. Among these, 31,634,238 observations (74.97%) come from lenders with total assets between $0 and $100 billion.

[50] According to the CFPB, the *arid* is created by concatenating an institution's agency code, as defined by the CFPB, with its Respondent ID. The agency code is an integer ranging from 1 to 9 that identifies the institution's regulatory agency. The Respondent ID is one of the following: OCC charter number, FDIC certificate number, NCUA charter number, National Information Center (NIC) RSSD, or federal tax ID. For details, see: `https://ffiec.cfpb.gov/documentation/publications/loan-level-datasets/panel-data-fields-v1`

Appendix C. Heterogeneous Effects of Disclosure on Interest Rates

This appendix examines the heterogeneous effects of the CFPB disclosure policy by assessing how lenders respond to complaint narrative disclosures under various conditions. Specifically, I consider three scenarios: (i) lenders with high versus low discrimination complaint intensity, (ii) lenders operating in regions with high levels of Google search activity related to the CFPB, and (iii) lenders exposed to an elevated risk of discrimination-related litigation.

In the initial phase of my analysis, I focus on complaints related to discrimination. Following the textual analysis method developed by Li (2025), I classify the treated lenders into high- and low-complaint groups based on whether their ratio of discrimination-related complaints to total assets exceeds the median in 2015. The first subgroup comprises CFPB-supervised lenders with above-median complaint intensity, while the second includes supervised lenders with below-median intensity.

I estimate separate regression models for the two subgroups based on the specification in Equation (1). The results are presented in the first two columns of Table A6. Column (1) reports the change in racial gaps following disclosure between treated lenders with high complaint intensity and the control group. The coefficient on $Treat \times Post \times Minority$ is significantly negative. In contrast, column (2) presents the results for treated lenders with low complaint intensity, showing a negative but smaller coefficient on the same interaction term. These findings suggest that disclosure is more effective in reducing racial disparities among lenders with a higher propensity for discriminatory practices, as public visibility increases reputational and regulatory incentives to adjust their pricing behavior.

In the second test, I examine whether public attention moderates lenders' responses to disclosure. I construct a measure of state-level changes in Google search activity for the term "CFPB" by comparing average search index values during the 12 months following the disclosure date with those in the 12 months preceding the disclosure date. Following Dou and Roh (2023), I define a *High* indicator equal to one for states with above-median increases in search activity, and zero otherwise.[51] Using this indicator, I split the treated group into two subgroups based on the magnitude of public attention shifts in the states where lenders are located.

[51] Google compiles keyword-specific search volumes for each state and computes the search index as the proportion of searches from a given state relative to the state with the highest search volume.

I adopt the same empirical specification as in the analysis of discrimination complaints. The results are reported in columns (3) and (4) of Table A6. The coefficient estimate in column (3) is statistically significant, while the coefficient in column (4) is not. These findings suggest that internet search activity—used as a proxy for social attention—helps reduce racial disparities in the mortgage market. Heightened social awareness leads lenders to place greater emphasis on reputational concerns and reduce misconduct.[52]

Finally, I examine whether disclosure has a stronger effect in reducing racial disparities during periods of heightened litigation related to racial discrimination. Between 2011 and 2019, eight such cases were filed, and the involved lenders faced various penalties. In some cases, lenders were required to invest in subsidy programs designed to support minority borrowers.[53] To assess the influence of litigation risk on the effectiveness of disclosure, I modify Equation (1) by introducing an interaction term involving a moderating variable, *Litigation*, which captures the intensity of litigation over time based on the number of cases and associated penalty amounts. These measures reflect the monthly frequency of litigation and the magnitude of penalties imposed. To address potential endogeneity concerns, I focus on peer lenders who are not directly penalized, thereby evaluating how the threat of litigation influences market-wide responses to disclosure. Including lenders directly involved in litigation may introduce endogeneity, as such lenders are more likely to reduce discriminatory behavior in response to legal sanctions.

After excluding observations involving litigated lenders, I estimate the moderation effect of litigation risk. The results are presented in Table A13. Columns (1) and (2) use *Litigation* to measure the total number of CFPB enforcement actions, while columns (3) and (4) define *Litigation* as the monetary value of penalties imposed on lenders. Columns (2) and (4) exploit individual-level variation and incorporate similar borrower fixed effects, consistent with the specification used in column (4) of Table 2. Column (1) shows that when the number of enforcement cases increases by one in a given month, the additional interest charged to minority borrowers declines by six basis points during the post-disclosure period. Column (3) indicates that a one-million-dollar increase in total penalties in a month is associated with

[52]Darian Dorsey, Chief of Staff at the CFPB, provides anecdotal evidence that some lenders tie executive bonuses to their responsiveness in resolving complaints. This practice implies that executives are incentivized to improve complaint handling, as part of their bonuses is linked to the effectiveness of the lenders' resolution process (Cortez, 2015).

[53]The data on these litigation cases are obtained from the CFPB website.

a 1.2 basis point reduction in the additional interest paid by minority borrowers at treated lenders after disclosure. Both estimates are statistically significant at the 1% level. These findings provide strong evidence that CFPB enforcement actions exert a deterrent effect on lenders not directly targeted by litigation. Anticipating regulatory scrutiny, such lenders may curb unlawful behavior to reduce exposure to future legal risk.

Appendix D. Ruling Out Alternative Explanations for Racial Disparities

While the main analysis documents significant racial disparities in mortgage outcomes and their reduction following disclosure, it is important to consider alternative explanations that could account for these observed disparities. This section examines three potential risk-based explanations: (1) fallout risk, where minority borrowers might be more likely to abandon loan applications after rate offers; (2) rate lock periods, where minority borrowers might systematically choose longer (and more expensive) rate lock periods; and (3) put-back risk, where lenders might perceive minority borrowers as posing higher repurchase risk. I evaluate whether these factors can explain the observed racial gaps in mortgage pricing and rejection rates.

Fallout Risk

Fallout risk refers to the risk that a borrower might not complete a mortgage application after receiving a rate offer, which can be costly for lenders who pre-hedge when they lock in rates. If minority borrowers have higher fallout rates than white borrowers, lenders might charge them higher interest rates to compensate for this additional risk.

To address this concern, I analyze whether differences in fallout rates between minority and white borrowers could drive my finding of reduced racial gaps following disclosure. Using HMDA data, I examine whether the racial differences in fallout rates changed differently between treated and control lenders after the disclosure policy (a triple-difference approach). As shown in Table A12, the estimated triple interaction term is not significant, indicating that the fallout patterns between minorities and whites remain similar.

These results suggest that fallout behavior does not explain my main findings, since the triple-difference strategy directly tests and rules out racial divergence in fallout rates following the policy.

Rate Lock Periods

Mortgage originators offer borrowers the option to lock in interest rates for a specified period, and longer lock periods typically come with higher costs. If minority borrowers systematically request longer rate lock periods than white borrowers, this could explain some of the observed racial disparities in interest rates.

Zhang and Willen (2021) match HMDA data with Optimal Blue rate lock data, controlling for key factors like rate lock month and borrower characteristics. They find significant pricing gaps, with Black and Hispanic borrowers facing higher rates than non-Hispanic white borrowers, even after these controls. These findings suggest that rate lock period differences alone cannot account for the racial disparities observed in mortgage pricing.

Put-back Risk

Another potential explanation for racial disparities in mortgage rates is differences in put-back risk, the risk that a loan originator might be forced to repurchase a loan if it fails to meet certain quality standards. If lenders perceive minority borrowers as posing higher put-back risk, they might charge higher interest rates to compensate.

Bartlett et al. (2022) conduct a comprehensive analysis to rule out put-back risk as an explanation for observed racial disparities. They strategically examine subsamples where put-back risk should be minimal:

- Loans issued after 2013, when new regulations significantly reduced put-back risk, especially for GSE loans
- High-quality loans with excellent credit scores and low LTVs, which have very low default risk and thus minimal put-back risk
- Differences between banks and non-banks, as non-banks face lower regulatory scrutiny regarding put-back risk

Across all these analyses, they find consistent racial gaps in interest rates. These tests demonstrate that put-back risk is not the primary driver of racial disparities. The persistence of racial gaps even in low-risk subsamples rules out put-back risk as a primary explanation.

The consistency of these findings strengthens the interpretation that the observed racial disparities reflect discriminatory practices rather than these alternative risk-based explanations.

Appendix E. AI-Driven Identification of Discriminatory Practices in Mortgage Lending

Methodology

Identifying patterns of discrimination in consumer complaint narratives is essential for addressing systemic disparities, especially within the mortgage lending sector, yet this critical issue remains underexplored. The CFPB database, with its rich narratives specifically detailing mortgage-related disputes, offers an invaluable resource for uncovering these subtle patterns and evaluating the implications of disparate treatment on access to mortgage financing.

Extant literature features preliminary efforts to analyze the CFPB database text. These include studies such as Bian et al. (2023), who applied a lexicon-based zero-shot learning model to classify fraud complaints, and Li (2025), who explored discrimination-related topics using word list approaches. Building significantly on these foundational inquiries, I introduce a novel analytical framework that capitalizes on Large Language Models (LLMs), specifically OpenAI's advanced GPT-4o, for the nuanced detection of discrimination patterns within complaint narratives. This shift is warranted, as LLMs demonstrate superior capacity for contextual reasoning and identifying implicit bias (de Kok, 2025; Ludwig et al., 2025) compared to conventional methods.

My implementation adheres to the best practices for LLM applications established by de Kok (2025), specifically employing a hierarchical prompt engineering methodology to analyze CFPB complaints. This approach structures the discrimination identification task into five sequential components: beginning with general instructions to ensure comprehensive task understanding; followed by a precise definition of discrimination, emphasizing both consumers' subjective feelings and objective treatment comparisons; third, establishing classification standards covering various forms of bias; fourth, providing reasoning process guidance to ensure systematic analysis; and finally, setting strict output format specifications to guarantee consistent and reliable results.

To enhance the model's discerning capabilities, I employ the widely adopted few-shot learning approach by incorporating two contrasting examples into the prompt. The first example serves as a negative baseline, illustrating a service issue (e.g., a bank's penny acceptance policy) that is non-discriminatory. Conversely, the second provides a positive case detailing explicit discrimination, such as a qualified minority consumer being denied a mortgage loan while similar non-minority applicants receive approval. Presenting the negative case first helps the model establish a clear boundary for non-discriminatory issues, thereby significantly reducing the misclassification of routine operational grievances as genuine discriminatory incidents, which reflects the true distribution of complaint types.

Prompt Structure

The implemented prompt structure is adopted for this study:

You are an AI assistant specialized in analyzing consumer complaints for the Consumer Financial Protection Bureau (CFPB). Your task is to identify discrimination-related content in complaint narratives and determine if the complainant experienced discrimination or unfair treatment.

1. General Instructions
(1) Carefully read and analyze the above narrative.
(2) Determine if this complaint contains content related to discrimination or unfair treatment.
(3) Consider a broad definition of discrimination, including any indication of unfair or unequal treatment that the complainant perceives.

2. Key Elements of Discrimination (HIGHEST PRIORITY)
When analyzing the complaint, pay special attention to these two critical elements:

(1) COMPLAINANT'S PERCEIVED TREATMENT
** Direct statements indicating discrimination based on race or ethnicity*
** Emotional language about unfair treatment specifically tied to racial identity*
** References to racial bias, prejudice, or stereotyping*
** Mentions of being treated differently due to race/ethnicity*
** Expressions of disrespect or dignity violations related to racial background*
** Terms suggesting racial targeting or profiling*

(2) COMPARATIVE TREATMENT
- Direct comparisons to how others were treated
- References to "other customers" or "everyone else"
- Mentions of different treatment based on any characteristics

- Observations about preferential treatment given to others
- Examples of inconsistent policies or standards

3. Forms of discrimination
When analyzing the complaint, consider the following forms of discrimination or unfair treatment:
(1) Explicit or implicit discrimination based on protected characteristics (race and ethnicity)
(2) Consider the following forms of discrimination or unfair treatment:
- *Approval discrimination: Higher likelihood of application rejections*
- *Interest rate discrimination: Being charged higher interest rates*
- *Service quality discrimination: Poorer service experience or staff attitudes*
- *Fee discrimination: Being charged higher or additional fees*
- *Any other form of unfair or unequal treatment perceived by the complainant*

4. Reasoning Process
Use your reasoning skills to analyze the narrative. Consider the following questions:
(1) Does the complaint explicitly mention discrimination or unfair treatment?
(2) Are there implicit indications of discriminatory practices or unfair treatment?
(3) Does the complaint describe treatment that aligns with any of the discrimination types mentioned above?
(4) Does the complainant express feeling unfairly treated, even if not using the word "discrimination"?
(5) Is there a comparison made between the complainant's treatment and that of others, suggesting potential disparity?

5. Output Format
After your analysis, provide your output in the following JSON format:

{
"is_discrimination": "<Binary classification (0 or 1) of the complaint>",
"confidence_score": "<Confidence score (0.0-1.0)>",
"reason": "<Reason for your classification>"
}

Where:

- "is_ discrimination" should be either 0 (the complaint does not contain discrimination-related content) or 1 (the complaint contains discrimination-related content or unfair treatment)
- "confidence_ score" should be a decimal between 0.0 and 1.0, representing your level of confidence in the classification
- "reason" should be a brief explanation of why you classified the complaint as you did, focusing on the elements that led to your decision. Be sure to mention the complainant's feelings of unfair treatment or any comparisons they made to others' experiences, if present.

Remember to focus on discrimination-related issues and unfair treatment. Consider all aspects of the complaint that might indicate potential discrimination or unfair practices, even if not explicitly stated.
Provide only the JSON output, with no additional text before or after.

Here is the complaint narrative you need to analyze:
{COMPLAINTS INPUT HERE}

Here are some examples:

<COMPLAINT_ NARRATIVE>
"TOO BIG TO FAIL AND CARE We are in our XXXX, after our last child went off to college in XX/XX/XXXX, we decided to downside and move into a smaller place. In XX/XX/XXXX, we paid down on our dream home. This is an XXXX and half bedroom condo in the heart of XXXX – - (no car needed) XX/XX/XXXX, we were informed that BOA had sold their XXXX units to XXXX XXXX, an out of state acquisition company. A meeting was held on XX/XX/XXXX, by the new purchasers. At this meeting, we the few owners were informed that because they (new owners) were the majority stakeholders, they could do away with the condo Bylaws. Thus the Bylaws were amended and the new board appointed. We were told the following : Sell our units to the new owners at the same price which was given to them by BOA Continue to live in our unit – -knowing that the building will no longer abide by the condo Bylaws, but instead will be operated and managed as an apartment complex. We were also informed that we will never be able to sell our unit on the open market because of the rental average of the building. Our condo was purchased in good faith. We 'd everything that was right under the law. The present situation we are in is no fault of ours. The housing market tanked, and BOA did what was best for them – -sold their units to the highest bidder – - NOT CARING ABOUT HOW THEIR DECISION WOULD AFFECT THEIR HONEST

CUSTOMERS THAT PURCHASED AND HAVE MORTGAGES WITH THEM. – – TOO BIG TO CARE. The investors on the other hand needed to realize a profit quickly changed the Bylaws to protect their investment. Who cares about ordinary Americans like us no one? We are the casualty of the housing bubble. We could have walked away them this situation like many other American, but we did n't because we are honest FOOLS."

<ideal_output>

[{
"is_discrimination": 0,
"confidence_score": 0.9,
"reason": "The complaint narrative does not contain any explicit or implicit indications of discrimination based on race or ethnicity. The complainant describes a situation involving changes to condo bylaws and the impact of a housing market decision by a bank and investors. The focus is on the perceived unfairness of the situation due to market forces and business decisions, rather than any discriminatory treatment based on protected characteristics. There is no mention of being treated differently compared to others based on race or any other protected characteristic."
}]

<COMPLAINT_NARRATIVE>

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}.

<ideal_output>

[{
"is_discrimination": 1,
"confidence_score": 0.95,

"reason": "The complaint explicitly alleges racial discrimination in mortgage lending by Bank of America. The complainant directly states 'I believe that I am being discriminated against because I disclosed my race' on the loan application. They specifically request an investigation to compare their loan denial with approvals for people of other races who have similar qualifications. The complainant provides detailed evidence of their strong qualifications (excellent credit score over XXXX, stable employment with $45,000 annual income, $30,000 in savings, advanced education) to support their belief that the denial was based on race rather than legitimate lending criteria. This represents a clear case of perceived discrimination in lending practices based on a protected characteristic (race), with the complainant making explicit comparisons to how others of different races might be treated in similar circumstances."
}]

Implementation and Results

The resulting output is provided in a standardized JSON structure comprising three critical fields: (1) a binary indicator classifying the presence or absence of discrimination; (2) a confidence score (ranging from zero to one) used to quantify the model's uncertainty regarding the classification; and (3) a detailed reasoning explanation that supports the final classification decision.

The LLM framework was applied to identify discrimination within mortgage complaint narratives spanning the 2015-2022 period. Across the full sample, which contained 30,658 complaints with narrative descriptions, 1,365 were identified as containing discriminatory content, resulting in an overall incidence rate of 4.45%.

In the study sample drawn from HMDA-GSE data, minorities account for only 6.7% of approved mortgage loans, yet discrimination-related complaints represent 4.45% of all mortgage complaints. Given the relatively small share of minority borrowers, this complaint rate is disproportionately high, highlighting the severity of discrimination issues in the mortgage market.

Illustrative Examples

Below, I provide several specific complaint examples demonstrating the GPT-4o model's capacity to identify perceived discrimination in mortgage lending:

Company: CITIBANK, N.A.
Complaintid: #1290200
Year: 2015
Complaint Narrative: *My HAMP loan was transferred to CitiMortgage about 2.5 years ago. Something went wrong with the transfer of paperwork and I have been subject to harassment by Citi ever since. Three weeks after the transfer XX/XX/XXXX, when I had n't received any further paperwork including the monthly statement and bill, I called Citi to ask where I should send the payment for that month. They told me I was in the foreclosure process since I had n't paid my bill in 3 months. They refused to accept that I was not with their company during that time period and refused to send any paperwork. Since that time, Citi has threatened me with letters and phone calls and at one point, a $ XXXX " lawyers fee " for not paying them what is not owed them. It took hiring a lawyer to get the fee removed, but Citi has continued to refuse to acknowledge my multiple requests to fix the start date. They messed up my escrow payments, and have recently removed me from the HAMP program without my consent, stating I no longer meet the qualifications since I refuse to pay them what is not owed them. I am at my wit 's end. I am ready to sell my house just to get rid of the harassment. If this is what home ownership is, it 's not worth it. Please help.*
Discrimination: 0
Confidence Score: 0.9
Reason: The complaint describes issues related to the transfer of a HAMP loan to CitiMortgage, including problems with paperwork, foreclosure threats, and removal from the HAMP program. However, there is no mention or implication of discrimination based on race or ethnicity. The complainant does not express feeling unfairly treated due to any protected characteristic, nor are there comparisons to how others were treated. The issues appear to be related to administrative errors and customer service rather than discriminatory practices.

Company: WELLS FARGO & COMPANY
Complaintid: 1307891
Year: 2015
Complaint Narrative: *Ref : CFPB Complaint # XXXX I believe that Wells Fargo Home Mortgage engaged a dishonest and unscrupulous marketing tactic meant to drive business in which I am a victim of their offer of an exceedingly low mortgage rate meant to attract*

customers know full well that the teaser rate (the " bait ") was not available. XXXX the loan process was well underway and WFHM knew that after my considerable investment of time and finances with the imminent danger of surrendering a large sum of money if I backed out of the deal, WFHM proceeded to offer the higher rate (the " switch "), thus earning greater commission. I believe WFHM delayed submitting my application for {$10000.00}. down payment assisitance from XXXX, which was consequently denied because the amount of the loan would be significantly lowered, thus WFHM would earn a lesser commission. When I consider the fact that Wells Fargo Bank has a history with the XXXX involving racial discrimination, my experience navigating through unreasonable obstacles that seemingly were meant to deny the loan, after consulting with White customers who have done business WFHM, and being included as a " Class Member " of XXXX separate court actions against Wells Fargo Bank which were, to the best of my understanding ; each brought forward by XXXX complaintant, I think there is reason to believe that I have been a victim of racial discrimination by Wells Fargo Bank. I believe the figures contained in the Settlement Statements are deceptive and have been fixed to scam and overcharge.

Discrimination: 1

Confidence Score: 0.9

Reason: The complainant explicitly mentions a belief of being a victim of racial discrimination by Wells Fargo Bank. They reference Wells Fargo's history of racial discrimination and compare their experience with that of White customers, suggesting differential treatment. The narrative includes perceived unfair treatment in the loan process, such as the bait-and-switch tactic and denial of down payment assistance, which the complainant attributes to racial bias. These elements align with perceived discrimination based on race, making this a clear case of alleged racial discrimination in lending practices.

These examples illustrate the LLM's discriminatory pattern recognition capability. The first complaint (Citibank) involves service failures but lacks any discrimination indicators, correctly classified as non-discriminatory. The second complaint (Wells Fargo) explicitly references racial discrimination, the bank's discriminatory history, and differential treatment compared to White customers, appropriately classified as discriminatory. This demonstrates how the LLM distinguishes between general service issues and genuine discrimination claims through contextual analysis.

Robustness Check: Regression Analysis

Applying this LLM framework to the HMDA origination dataset, I re-classify discrimination complaints and re-estimate the baseline rejection rate regressions.

Table A16 presents the estimated effects of disclosure on rejection rates using LLM-identified discrimination complaints. To explore heterogeneity by lender characteristics, I split the treated group into two subsamples based on the intensity of LLM-identified discrimination-related complaints, measured as the ratio of complaint counts to total assets: those with above-median intensity (column (1)) and those with below-median intensity (column (2)). Both specifications include the same fixed effects structure as columns (3) and (4) of Table 4, enabling direct comparison with the keyword-based approach.

Columns (1) and (2) of Table A16 present the moderating effects of LLM-identified discrimination complaints. The $Treat\_H \times Minority$ coefficient is positive and statistically significant in column (1), indicating that within the treated group, minority applicants face higher rejection rates at lenders with high LLM-identified complaint intensity. In contrast, the $Treat\_L \times Minority$ coefficient in column (2) is smaller in magnitude, suggesting that lenders with low LLM-identified complaint intensity impose lower additional rejection rates on minority borrowers. These results further validate the LLM-based measurement of lender-level discrimination.

The coefficient on $Treat\_H \times Post \times Minority$ captures the moderating role of LLM-identified discrimination complaints. The result in column (1) shows that the overall treatment effect is primarily driven by lenders engaged in discriminatory practices, consistent with the keyword-based results in Table 4. Furthermore, a permutation test with 500 replications reveals that the treatment effect in column (1) is significantly larger in magnitude than in column (2) (difference = -0.012, $p = 0.002$), verifying the heterogeneity of the shock.

Appendix F. CFPB Disclosure and Complaints' Resolution Outcomes

The main text examines how disclosure disciplines lenders to improve service quality and reduce the occurrence of complaints beforehand. This section further tests how disclosure affects lenders' appeasement strategies in handling complaints after they occur, using the CFPB complaints dataset.

Following the CFPB disclosure of consumer complaints, lenders may improve complaint resolution outcomes to protect their public image and strengthen consumer trust. More effective resolution can reduce negative publicity, help retain customers, and signal compliance with consumer-protection standards.

Motivated by this hypothesis, I examine the effect of publicly disclosing complaint narratives on the resolution outcomes of complaints filed with the CFPB. The analysis uses CFPB complaint data from March 19, 2015, to December 31, 2016, and applies a difference-in-differences approach.

As of March 19, 2015, consumers could choose to make their complaint narratives publicly available when filing complaints with the CFPB. However, the CFPB did not release these narratives until June 25, 2015, creating a buffer period during which financial institutions were not yet subject to public scrutiny. The disclosure policy officially began on June 25, 2015, when the CFPB started publishing complaint narratives. The pre-disclosure period in this study is therefore defined as March 19 to June 25, 2015. During this period, consumers filing complaints faced the same opt-in mechanism as those after June 25, 2015; however, their narratives remained unpublished. This design helps mitigate concerns that complaints with and without narrative information differ systematically in unobserved ways—a concern that is less severe in this short event window, during which the distribution of complaint types and reporting behavior is unlikely to shift meaningfully.

The sample period ends on December 31, 2016, for two reasons. First, restricting the analysis to data before 2017 avoids potential confounding effects from changes in political administration. The CFPB, established during President Barack Obama's tenure, was designed to prioritize consumer interests. In contrast, President Donald Trump, who took office in January 2017, and his administration expressed unfavorable views toward regulations targeting firms (Haendler and Heimer 2021). Second, ensuring comparability between the pre- and post-disclosure samples is important. Because the pre-disclosure period in this study lasts only three months and the number of complaints submitted to the CFPB increases annually, limiting the post-disclosure period to 18 months (through December 2016) helps maintain a more balanced sample and avoids confounding trends in complaint volumes.

I retain 62,096 mortgage-related consumer complaints for the analysis. The regression model is specified as follows:

$$\begin{aligned} Y_{i,t} & \\ &= \alpha + \beta_1 Treat_i \times Post_t + \beta_2 Treat_i + \beta_3 Post_t + \mu_{Institution} \\ &+ \mu_{Zip} + \mu_{YearMonth} + \epsilon_{i,t} \end{aligned} \tag{11}$$

In this study, complaints are classified into treatment and control groups based on the public availability of their narratives. $Treat_i$ equals one if a complaint narrative is publicly available and zero otherwise. $Post_t$ indicates the period after the CFPB began releasing complaint narratives, taking a value of one on or after June 25, 2015, and zero otherwise.

Regarding the outcome variables, $Y_{i,t}$, two indicators are used to measure complaint resolution outcomes. The first is whether a complaint filed with the CFPB is resolved with monetary compensation.[54] The second is whether the institution provides a timely response.[55] Both outcome variables are binary. In Equation (11), the coefficient of interest, β_1, measures the relative improvement in resolution outcomes for complaints with publicly disclosed narratives compared with those without after the full implementation of the disclosure policy.

Several fixed effects are included to account for unobservable factors. Institution fixed effects control for differences across institutions, while ZIP Code fixed effects capture local political and economic conditions. Year-month fixed effects absorb time-varying shocks. Standard errors are clustered at the institution and year-month levels to address within-institution and temporal correlation.

Table A17 shows that the coefficient on $Treat \times Post$ in column (1) is positive and statistically significant, indicating that the probability of complaints being resolved with monetary relief increases by 0.6 percentage points after the disclosure policy, equivalent

[54] Complaints submitted to the CFPB can be resolved in several ways: Closed, Closed with explanation, Closed with monetary relief, Closed with non-monetary relief, and Untimely response. Monetary relief is preferred by consumers, and prior research documents significant racial disparities in this outcome (Haendler and Heimer 2021).

[55] Timely response refers to "Whether the company provided a timely response."

to 0.036 standard deviations of the outcome variable. Column (2) examines the effect of disclosure on the timeliness of complaint responses. The coefficient on $Treat \times Post$ is positive and significant at the 1% level, while the coefficient on $Treat$ is significantly negative. These results suggest that, before the disclosure of narratives, complaints with narratives were processed less promptly than those without narratives. After disclosure, this pattern reverses, with institutions responding more quickly to complaints with narratives. The estimated coefficient implies that timely responses increase by 0.6 percentage points after disclosure, corresponding to 0.053 standard deviations of the outcome variable. These findings suggest that publicly disclosing complaint narratives significantly improves complaint resolution outcomes by drawing institutional attention and encouraging lenders to respond more favorably in order to protect their reputations.

The validity of the difference-in-differences estimates depends on the parallel trends assumption. To test this assumption, I conduct an event study using the framework in Equation (11). As shown in Figure A6, June 25, 2015, is designated as the policy event date.

Panels (a) and (b) of Figure A6 show the dynamic effects for the two outcomes. In the months before disclosure, the estimated coefficients do not significantly differ from zero, and the pre-trend remains stable over time, consistent with the parallel trends assumption. The improvement in response outcomes appears almost immediately after disclosure. Although the magnitude of the effects varies across outcomes, all show persistent impacts. The estimated coefficients in the fourth month and thereafter remain consistent with those in the initial post-disclosure period, confirming the durability of the improvement.

Appendix G. Disclosure of Complaint Narratives and the Persistence of Consumer Complaints

This section tests whether disclosure induces lenders to adopt forward-looking, process-oriented complaint management strategies. Specifically, I examine whether the autocorrelation of complaint volumes between adjacent months weakens following disclosure.

Because lenders anticipate market scrutiny from disclosure, they may improve service processes, strengthen response mechanisms, increase transparency in product terms, and tighten compliance controls. These ex-ante adjustments address customer dissatisfaction earlier, thereby reducing the number of issues that escalate into formal complaints. As a result, the correlation of complaints across months weakens, reflecting a shift from reactive case-by-case handling to proactive, process-oriented management.

On March 19, 2015, the CFPB finalized its narrative-disclosure policy and launched a transition window for firms to prepare internal workflows and consent procedures. Complaints filed during this buffer period were collected under the new rules, but their narratives would not be made public until the database update at the end of June. This design provides a clean pre-policy window (March 19–June 24, 2015), during which lenders already anticipate disclosure, but narratives are still non-public, followed by the public-release phase beginning June 25, 2015. I leverage this timing to structure my identification and tests.

To test whether this forward-looking management is reflected in the temporal persistence of complaints, I examine whether the autocorrelation of complaint volumes between adjacent months weakens following disclosure, using a balanced sample from March 2015 to December 2016. Based on institution-month-level complaint numbers, the empirical model is specified as follows:

$$\begin{aligned} &\mathit{Complaints}_{i,t+1} \\ &\qquad = \alpha + \beta_1\, \mathit{Complaints}_{i,t} + \beta_2\, \mathit{Complaints}_{i,t} \times \mathit{Post}_t \\ &\qquad\quad + \mu_{\text{Institution}} + \mu_{\text{YearMonth}} + \varepsilon_{i,t} \end{aligned} \tag{12}$$

In the model, $\mathit{Complaints}_{i,t+1}$ denotes two outcomes for institution i in month t+1: (i) total complaints $\big(\mathit{ComplNum}_{i,t+1}\big)$ and (ii) complaints with narratives $\big(\mathit{NarrComplNum}_{i,t+1}\big)$. The regressors comprise the corresponding complaint counts in month t and their interactions with a policy dummy, Post_t, which equals 1 from June 2015 onward, when the CFPB began releasing complaint narratives. Coefficient β_1 captures persistence in complaints across adjacent months, and β_2 identifies how that persistence shifts after the policy takes effect. All specifications include institution fixed effects and year-month fixed effects, with standard errors clustered at the institution level.

Across both columns in the Table A18, complaint counts in the current and following month are positively correlated ($\beta_1 > 0$). However, the interaction term $Complaints_{i,t} \times Post_t$ is negative and statistically significant, indicating that persistence weakens after narratives are disclosed. This pattern suggests that once narratives are public, the carryover of complaints into the next month diminishes. Quantitatively, the estimated autocorrelation weakens by approximately 28.7% for total complaints and 34.9% for narrative complaints. Consistent with my hypothesis, the results support a forward-looking, process-oriented response—institutions commit resources to processes that prevent issues from recurring rather than merely reacting case by case.

Appendix H. Website Ranking Lenders for Discrimination

To improve support for minority borrowers in the United States and address potential discrimination in mortgage lending, I develop an online platform. The website features an analytical framework that ranks lenders by indicators of racial discrimination at the ZIP Code level. Specifically, I test whether minority borrowers face higher loan interest rates or higher rejection rates from specific lenders. The analysis is conducted at the ZIP Code level by including only data from a single ZIP Code in each regression. The dataset is consistent with those used in prior studies on interest rates and rejection rates. The regression model is specified as follows:

$$Outcomes_{ilt} = \alpha + \beta Minority_i \times Lender\ id_l + FEs + \epsilon_{ilt} \tag{13}$$

In Equation (13), i, l, and t denote borrower, lender, and year, respectively. *Outcomes* refers to either the interest rate or the rejection rate for minority borrower i at lender l. *Minority* equals one if borrower i is Black or Hispanic and zero otherwise. *Lender id* uniquely identifies lender l. The coefficient β captures the disparity in interest rates or rejection rates between minority and non-minority borrowers within lender l. A positive β indicates racial discrimination by lender l, reflected in higher interest rates or rejection rates for minority borrowers. The regression uses the same fixed effects as in previous analyses of interest rates and rejection rates.

I rank lenders within each ZIP Code by the degree of discrimination, measured by the magnitude of the β coefficient from the above results. These rankings, along with detailed measures of discrimination, are publicly available on a website called *Discrimination Warrior* (`https://discriminationwarrior.com`). Users can enter a ZIP Code to view estimated interest rate and rejection rate disparities for local lenders against minority borrowers. Figure A7 and Figure A8 show the results for ZIP Code 02135, a region in Boston. This initiative provides the public with direct and transparent access to evidence of local lenders' discriminatory practices.

References

Bartlett, Robert, Adair Morse, Richard Stanton, and Nancy Wallace, 2022, Consumer-lending discrimination in the fintech era, *Journal of Financial Economics* 143, 30–56.

Bian, Bo, Michaela Pagel, Huan Tang, and Devesh Raval, 2023, Consumer surveillance and financial fraud, Technical report, National Bureau of Economic Research.

Cohen, Gregory J, Jacob Dice, Melanie Friedrichs, Kamran Gupta, William Hayes, Isabel Kitschelt, Seung Jung Lee, W Blake Marsh, Nathan Mislang, Maya Shaton, et al., 2021, The us syndicated loan market: Matching data, *Journal of Financial Research* 44, 695–723.

Cortez, Nathan, 2015, Agency publicity in the internet era, Working paper, Southern Methodist University.

de Kok, Ties, 2025, Chatgpt for textual analysis? how to use generative LLMs in accounting research, *Management Science* 71, 7888–7906.

Dou, Yiwei, and Yongoh Roh, 2023, Public disclosure and consumer financial protection, *Journal of Financial and Quantitative Analysis* 58, 2164–2198.

Haendler, Charlotte, and Rawley Heimer, 2021, The financial restitution gap in consumer finance: Insights from complaints filed with the CFPB, SSRN Working Paper 3766485.

Law, Kody, and Nathan Mislang, 2022, The heterogeneity of bank responses to the fintech challenge, Technical report, Cornell University.

Li, Xiang, 2025, Bank competition and entrepreneurial gaps: evidence from bank deregulation, *Journal of Financial and Quantitative Analysis Forthcoming*.

Ludwig, Jens, Sendhil Mullainathan, and Ashesh Rambachan, 2025, Large language models: An applied econometric framework, NBER Working Paper 33344, National Bureau of Economic Research.

Zhang, David Hao, and Paul S. Willen, 2021, Do lenders still discriminate? a robust approach for assessing differences in menus, NBER Working Paper 29559, National Bureau of Economic Research.