

Using High-Frequency Evaluations to Estimate Discrimination: Evidence from Mortgage Loan Officers*

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Abstract

We develop tests for discrimination that we apply to 25 years of mortgage lending. Our tests limit the scope for omitted variables in a conventional benchmarking test by combining high-frequency mortgage evaluations with the notion that economic incentives can mitigate subjective biases. Loan officers have monthly volume quotas that constrain their subjectivity on loans processed at month-end. Concurrently, applicant characteristics are time-invariant within-month. We estimate that loan officers' subjectivity contributes to at least half of the unexplained Black approval gap. The within-month approval gap is smaller for shadow banks, but not for FinTech lenders or banks in concentrated markets.

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1 Introduction

A wide range of fields—such as labor markets, the legal system, and credit markets—document racial and gender disparities. Yet whether these disparities are the result of discrimination by economic decision-makers—defined as an evaluator treating otherwise identical subjects from minority groups worse than subjects from the majority group—remains in dispute because of the limitations of empirical tests. Researchers increasingly use experiments and correspondence studies to test for discrimination (Bertrand and Duflo, 2017). Nonetheless, tests for discrimination that use observational data have several advantageous features. They are widely accessible to researchers and policymakers, and they are easy to replicate and scale.

Benchmarking tests (also known as audit tests) use observational data in a straightforward way to test for discrimination. They claim to find discrimination when minority group subjects receive worse evaluations than majority-group subjects. Benchmarking tests have immense potential to test for discrimination because they can be executed in real time and they impose few restrictions on the data. However, they are vulnerable to omitted variable bias—differences in group characteristics that the researcher does not observe can cause differences in evaluations.¹

The goal of our paper is to make progress toward identifying discrimination by limiting the scope for omitted variables in a conventional benchmarking test. We do so by combining the growing availability of high-frequency data on evaluations with the intuition behind Gary Becker’s seminal (1957) theory of discrimination. Specifically, we are motivated by the observation that evaluators are often subject to competitive market forces. And at such times, evaluators have less scope for subjective assessments. For example, employers that have immediate staffing needs can ill afford to turn away job applicants. TSA agents might reduce their screening of travelers when there are long queues. Police officers that have monthly quotas would issue tickets to all speeding drivers on the last day of the month. In this

¹Though the discussion is outside the scope of our paper, some settings are well-suited to using “outcome tests” to test for discrimination (Becker, 1957). Indeed, recent papers make substantial contributions to the econometrics of outcome tests and we refer readers to these works (see e.g., Arnold et al., 2018; Canay et al., 2020).

paper’s setting, mortgage lending, loan officers have monthly origination-volume quotas that afford them less scope to apply subjective preferences at month-end.

Our approach has a straightforward identification rationale that we define in Section 2. If the evaluations of subjects change within a short time interval, then unobserved time-invariant characteristics of subject groups are unlikely to drive such differences. We acknowledge that our approach leaves open the possibility of *time-varying* unobservable differences across subject groups. However, the time-varying unobservables, if confined to a tight sample window, are undoubtedly less troublesome than cross-sectional unobservables. And, moreover, researchers can investigate alternative interpretations that rest on time-varying unobservable factors.

We test for discrimination in the U.S. residential mortgage market by applying our approach to high-frequency data on mortgage applications. Figure 1 presents the key source of high-frequency variation. The figure uses data from the date-stamped version of the Home Mortgage Disclosure Act (HMDA) that covers the near-universe of mortgage applications from 1994 to 2018 with over 500 million loan applications across more than 28,000 lenders. It shows that the total volume of new mortgage originations increases by more than 150% on the last day relative to the first day of a given month. At the same time, the number of mortgage applications stays constant over the course of the month.

These within-month patterns reflect crucial features of mortgage lending that allow us to use a benchmarking test while limiting the scope for unobservable differences across groups. Loan officers tend to have monthly volume targets that determine their compensation, which contributes to the increase in originations at month-end.² In the spirit of Becker (1957), loan officers’ incentive to meet origination targets means that their subjective favoritism towards applicants has to attenuate at the end of the month. At the same time, application volume is time-invariant, which creates a wedge between demand and supply that allows us to disentangle the

²Though we are unable to obtain the compensation of individual loan officers, the most common compensation scheme includes commissions that are set based on the number of loans and the loan amount originated. For example, the Mortgage Bankers Association describes industry standards for loan officers’ compensation: [webpage link](#). Loan officers may also face disciplinary action if they fail to meet their quotas several months in a row (Tzioumis and Gee, 2013). Tzioumis and Gee (2013) and Cao et al. (2020) show that non-linear incentives at a large U.S. commercial bank and in two Chinese banks, respectively, cause end-of-month bunching.

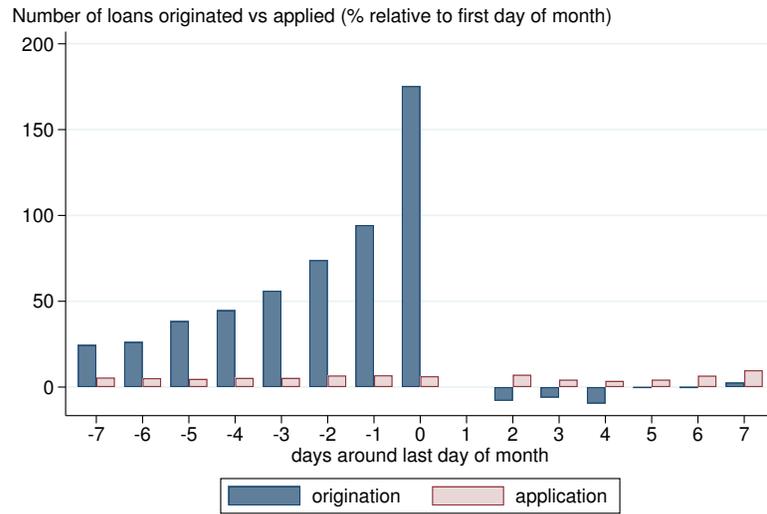


Figure 1: The figure uses data from HMDA on U.S. mortgage applications submitted between January 1994 to December 2018. The figure reports average daily application and origination volume in percentage terms relative to the first day of any given month.

component of loan officers’ decision-making that is orthogonal to observable and unobservable factors (e.g., applicant characteristics). Therefore, we can estimate the extent to which loan officers’ subjectivity towards applicants affects application outcomes by combining the within-month variation with a benchmarking test.

Utilizing this within-month variation, our tests for discrimination estimate the difference in approval rates between Black and other applicants at the start relative to the end of the month. Figure 2 summarizes our main finding. It shows the difference in application approval rates between Black and white applicants over the course of any given month. In the first seven days of the month, Black applicants have 20 percentage point lower approval rates than white applicants. The approval gap declines to just 10 percentage points on the last day of the month.

Regression analysis confirms the graphical evidence in Figure 2. The regression tests are saturated with a rich set of fixed effects that control for time-varying economic conditions at precise geographic levels (county-month), as well as lender-month fixed effects that control for factors, such as regulations, that affect lending at the institution level. The regressions also include applicant characteristics interacted with day-of-month fixed effects to allow lenders’ decision criteria to change

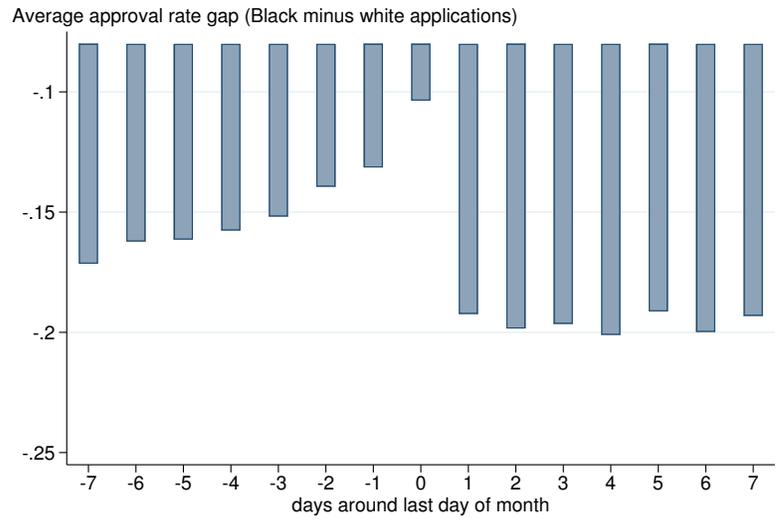


Figure 2: This figure uses the same data as Figure 1. The figure reports approval rates, which we define as the fraction of loans that are originated out of the total number of applications (excluding withdrawn applications). We present the difference between the Black approval rate and the white approval rate on each day.

flexibly over the course of the month. In our most stringent tests, the Black approval gap declines by 3 to 5 percentage points from the start to the end of the month. This constitutes a lower bound on the share of the Black approval gap that is due to loan officers' subjectivity, relative to the approval gap that can be attributed to unobservable group-differences. The estimates suggest that loan officers' subjective decision-making explains at least half of the Black approval gap after controlling for observable characteristics. Furthermore, these estimates are similar across different types of mortgage lending, such as FHA loans and refinances. This robustness across mortgage products helps exclude alternative explanations, such as possible financial incentives to close new-purchase mortgages on the last day of the month.

We use these estimates to assess the magnitude of discrimination in mortgage lending over the past several decades. Using a back-of-the-envelope calculation for the upper-bound of the costs of discrimination, if the Black approval gap on each day of the month was as small as it was on the last day, approximately 1.4 million more Black applications would have been approved between 1994 and 2018. This

difference in loan approvals corresponds to approximately \$213 billion (in 2018 dollars) total loan volume since 1994.³

Our approach to estimating discrimination hinges on simple assumptions that we derive and find support for via narrative and in the data. The first assumption is that loan officers have time-varying costs of being subjective. In our setting, loan officers have nonlinear contract incentives.⁴ Loan officers that miss their volume quotas will have less compensation and could be terminated.

The second assumption is that the characteristics of the subject pool are time-invariant. Indeed, we find that application volume, the share of Black applications, and loan application quality (for both Black and non-Black applications) are all constant over the course of the month. The remaining threat to identification is that there could be differential trends by race in the quality of applications that get processed over the course of the month. As evidence against this explanation, we find that high-quality and low-quality Black mortgages have similar amounts of bunching toward the end of the month.

We support, in two additional ways, the assumption that application quality is time-invariant. First, we use a new sample of HMDA data (post-2018) that includes the use of automated underwriting systems' (AUS) recommendations. The AUS recommendations are generated by computer algorithms—such as those produced by Fannie Mae and Freddie Mac—and they intend to offer a race-neutral evaluation of applications. We find that there is a racial gap in AUS recommendations—Black applicants are recommended for approval approximately 6 percentage points less frequently. However, the gap in AUS recommendations is nearly constant over the course of the month, which contrasts the *actual* Black approval gap. Also, our regression evidence is robust to including the AUS recommendation as a control variable. Second, we find that ex-post default rates are unrelated to the day of the

³These calculations do not account for the possibility that the same person(s) can submit multiple applications. The HMDA data does not track borrowers longitudinally.

⁴Importantly, the loan officer's optimal strategy under volume quotas would be to approve all applications. However, loan officers face several constraints. Lending institutions set origination standards that an application has to exceed and loan officers may have a fixed quantity of mortgage credit that they can distribute within a month. Loan officers can use their discretion and work to sidestep the origination standards by either using risk-based pricing or appealing to other "soft" criteria (e.g., the applicant holds other accounts with the bank).

month that the loan is originated. This suggests that there are no differences in application quality that we are unable to observe at the time of origination.

Though our main analysis tests for changes to the Black approval gap over the course of the month, our findings are robust to alternative null hypotheses for discrimination. Specifically, we consider the alternative null of no discrimination that the *share* of Black approvals is constant within-month. We find that Black applicants constitute a larger share of originations at month-end.

Finally, our approach allows us to evaluate the effect of policies and innovations on the distribution of credit. We consider three important features of modern mortgage lending: market concentration in banking, FinTech lending, and shadow banking. We find that market concentration and FinTech lending do not meaningfully affect the share of the Black approval gap due to loan officers' subjectivity. This result reflects the fact that our regressions include lender fixed effects; our tests estimate the component of loan officer subjectivity that occurs *within-lender*. Moreover, despite these changes to the banking sector, loan officer compensation incentives have largely remained constant throughout our sample, and human loan officers are even involved in mortgage processing at FinTech lenders (see e.g., Fuster et al., 2019). On the other hand, we find that shadow banks have a smaller within-month Black approval gap. We suspect that this is caused by shadow banks—owing to their lower regulatory requirements—having a larger presence in under-served communities.

Related Literature

This paper relates to advances in the literature on identifying discrimination by economic decision-makers. Our approach is akin to empirical papers that use changes to evaluation settings to identify discrimination. For example, Goldin and Rouse (2000) show that blind auditions reduce employment discrimination against female orchestra musicians. Police officers are less likely at night than during the day to pull over Black motorists because the driver's race is difficult to identify (Pierson et al., 2020). These studies identify discrimination by comparing situations in which evaluators can observe subject characteristics to situations in which they do not. Our approach is different because loan officer's knowledge of applicant charac-

teristics stays constant. We identify discrimination under certain assumptions about the applicant pool and the economic incentives of loan officers.

More specifically, our paper joins the literature on discriminatory lending practices in consumer credit markets. Our empirical approach is grounded in evidence that loan officers have significant discretion in loan processing decisions (Engelberg et al., 2012; Chen et al., 2016; Cortés et al., 2016; Demiroglu et al., 2021). Guided by these findings, we bring the confidential HMDA data to the question of lending discrimination.⁵ Recent papers advance the literature on discrimination in mortgage lending by obtaining richer data sets that allow for more control variables in a classic benchmarking test (Bartlett et al., 2021; Bhutta et al., 2021). Our approach to estimating discrimination is less reliant on cross-sectional control variables, although our approach can only suggest a lower bound on discrimination. Furthermore, most papers are unable to distinguish between taste-based and statistical discrimination (Bohren et al., 2019). Our approach can offer guidance about which type of discrimination is most likely. Also, many papers use evidence from confidential internal data from a single lending institution. In contrast, our paper uses the universe of U.S. mortgage applications over a 25-year period to connect racial disparities in lending to the incentives of individual loan officers. This allows us to address crucial questions about external validity, investigate the effects of the market structure, and quantify the scope of racial bias in mortgage markets.⁶

Furthermore, our paper contributes to a growing literature on how market structure and technology affect consumer lending. First, recent papers support classic theories arguing that competition reduces discrimination in consumer lending

⁵The literature can be traced back at least as far as the public release of HMDA data and the work of Munnell et al. (1996). Ladd (1998) summarizes much of the older literature and frames longstanding debates. Other foundational papers include Berkovec et al. (1994); Tootell (1996); Berkovec et al. (1998). Recent work studies differences in interest rates and fees (e.g., Bayer et al., 2018; Bartlett et al., 2021; Bhutta and Hizmo, 2021; Ambrose et al., 2021) and the effects of housing policy (e.g., Diamond et al., 2019).

⁶Some papers use different approaches to show that loan officers discriminate against underrepresented groups. Fisman et al. (2017, 2020) show that loan officers at Indian banks are more favorable to culturally-similar applicants. Beck et al. (2018) use data from an Albanian bank and Montoya et al. (2019) use a field experiment at a Chilean bank to uncover evidence of gender discrimination in consumer lending. Other papers study discrimination in auto lending (e.g., Butler et al., 2019; Lanning, 2021).

(Buchak and Jørring, 2017; Butler et al., 2019). Such papers suggest that racial discrimination declines because of changes to the composition of lending institutions. We find that discrimination by individual decision-makers can persist within organizations even in markets where there is more competition across institutions. Second, we contribute to the literature on the effects of FinTech lending on the allocation of credit (see e.g., Fuster et al., 2021; Bartlett et al., 2021; Tantri, 2021). We show that biases in human decision-making can survive advances in loan processing technology, similar to findings on the introduction of machine learning to judicial outcomes (Kleinberg et al., 2018).

Finally, we make a unique contribution to the literature on performance-based compensation, with a particular focus on financial intermediation. The effects of performance-based compensation have been studied in a range of settings, such as manufacturing (Oyer, 1998), software sales (Larkin, 2014), government contracts (Liebman and Mahoney, 2017), healthcare (Li et al., 2014; Gravelle et al., 2010), firm managers (Bandiera et al., 2007) and accounting (Murphy, 2000). The literature has also studied how performance incentives within banks affect loan officers' effort and performance (Agarwal and Ben-David, 2012; Cole et al., 2015), and information production (Hertzberg et al., 2010; Qian et al., 2015; Berg et al., 2020). Our paper contributes by studying variation in performance incentives combined with subjectivity in human decision-making.

2 The Framework for Identifying Discrimination

This section presents a formal discussion of our empirical setup. Existing frameworks for identifying discrimination face important challenges when dealing with differences in unobserved characteristics across subject groups. Our approach is to “filter out” these unobserved differences using high frequency data and to exploit changes to decision makers' incentives.

Our approach extends conventional tests for discrimination, called either benchmarking or audit. These tests compare the conditional likelihood that a minority subject group receives favorable treatment relative to the majority group, after controlling for observable subject characteristics. For instance, assume that the

decision-maker considers whether to approve loan applications. The researcher claims to have uncovered discrimination when she rejects the null of no difference in the conditional likelihood of favorable decisions between minority and majority groups (for example, Blacks and whites), and she finds that the likelihood of favorable decisions is smaller for the minority group. Specifically, the researcher claims discrimination against Black subjects when she finds that:

$$P(Y|W, X) > P(Y|B, X) \quad (1)$$

where $P(Y|R, X)$ is the probability of receiving a favorable decision, conditional on race $R \in \{W, B\}$ (white or Black) and a vector of characteristics X observed by the researcher. However, this approach is exposed to the criticism that the difference in the estimated conditional probability between white and Black subject groups might be driven by unobserved characteristics that are relevant for the decision maker's assessment, but are not included in the vector of controls X observed by the researcher. To illustrate, assume that there is a binary variable, unobserved by the researcher, $Z \in \{Z_L, Z_H\}$, such that the following assumptions are satisfied:

Assumptions Set (A)

No discrimination: $P(Y|W, Z_k, X) = P(Y|B, Z_k, X)$
for $k \in \{H, L\}$

Higher quality predicts higher favorable decision probability: $P(Y|R, Z_H, X) > P(Y|R, Z_L, X)$

On average white applicants have better unobservables: $P(Z_H|W, X) > P(Z_L|B, X)$

The inequality in favorable decisions formalized by equation (1) holds under the above assumption set when omitting the variable Z , even though *decision-makers do not discriminate when all of the characteristics are accounted for* (see Online Appendix A.I). The differences in the observed conditional probability of favorable decisions between races simply capture the differences in the unobserved characteristic. In the mortgage-lending setting, Black and white applicants have substantially different observable characteristics (see Table 1). Such differences raise concern that there might be also meaningful differences in unobservables.

Our goal is to refine existing approaches to address the identification problems due to the systematic differences in unobservables across subject groups. Rather than only testing for the differences in the likelihood of a favorable decision between racial groups, we use high-frequency data to test whether those differences vary over a short period of time. Because discrimination is determined by the subjective judgment of the evaluators, under the null of no discrimination, and if applicant characteristics remain constant over time, there shall be no change in the probability of favorable decisions for minority relative to majority candidates over time. On the other hand, discrimination would predict a change in the relative favorable decision probability over time.

To formalize this idea, let there be two time periods, $T \in \{Start, End\}$. Assume that evaluators have more scope to be subjective in period *Start* relative to period *End*. Then, in the presence of time-varying discrimination we expect to find:

$$P(Y|W, X, End) - P(Y|B, X, End) < P(Y|W, X, Start) - P(Y|B, X, Start) \quad (2)$$

where $P(Y|., X, .)$ is the probability of receiving a favorable decision, conditional on race (white or Black), a vector of observable characteristics X , and in a specific period (*Start* or *End*). Note that the presence of unobservable quality characteristics systematically correlated with race cannot alone explain the effects in equation (2). Consider the following set of assumptions that characterize a situation in which there is no discrimination:

Assumptions Set (B)

No discrimination: $P(Y|W, Z_k, X, T) = P(Y|B, Z_k, X, T)$
for $k \in \{H, L\}$

Higher quality predicts higher favorable decision probability: $P(Y|Z_H, X, T) > P(Y|Z_L, X, T)$

On average white applicants have better unobservables: $P(Z_H|W, X, T) > P(Z_H|B, X, T)$

No time pattern in subject group quality: $P(Z_H|R, X, Start) = P(Z_H|R, X, End)$

Stable Decision Criteria: $P(Y|Z_H, X, T) - P(Y|Z_L, X, T) = \lambda$

The first three assumptions are the same as in **Assumptions Set (A)**, while the last two assumptions state that the unobserved characteristics of the applicants, for both

whites and Blacks, are constant over time, and that their effect on decision making is constant over time. Jointly, these assumptions imply (see Online Appendix A.I):

$$P(Y|W, X, End) - P(Y|B, X, End) = P(Y|W, X, Start) - P(Y|B, X, Start).$$

Thus, the condition in equation (2) indeed amounts to a rejection of the null of no discrimination.

Online Appendix Section A.II considers whether our approach to estimating discrimination can distinguish between different theories of discrimination, specifically taste-based versus statistical discrimination. Our approach is unable to prove that a given mechanism causes discrimination. However, researchers can reasonably link the source of discrimination to the source of high-frequency variation. In the case of mortgage lending, loan officers observe the same information about applications whether they are processed at the start or the end of the month. As such, within-month changes to evaluations are more likely caused by subjective preferences than by imprecise inferences about applicants.

3 Mortgage Lending Data

Our analysis uses the confidential version of the HMDA data available to researchers in the Federal Reserve System. The dataset contains the largest sample of mortgage applications available in the U.S. The public version of the data includes information on applicant characteristics—race, gender, reported income, and location of the property—and an identifier for the lender that received the application. The data cover the entire geography of the U.S. over the period from January 1994 through December 2019. Moreover, the data provide information on mortgage contract characteristics, such as whether the application is for a new home purchase or refinancing, the loan amount, the lien, and whether the property is owner-occupied. The primary distinguishing feature of the confidential version of the HMDA data is that it contains the exact date the application was submitted and the date that the lender took action on the application, either by originating the loan or denying the application. Because the timing of lenders’ decisions is crucial to our study, Section 5.5 considers how the timing of originations and denials affects our analysis.

Table 1, columns (1) through (3), presents summary statistics for the different racial groups in the HMDA data. Approximately 7% of applicants are Black and 67% are white. The remaining 26% of observations are included in the category “Other race,” which includes all other race groups, as well as applications that do not specify race. The average Black applicant applies for a smaller loan, is more likely to be below-median income (59.8%, compared to 46% for whites and 47.8% for other), and is less likely to be approved (63.3%, compared to 80.7% for whites and 69% for other). Whites receive 73.7% of approved loans, Blacks receive 5.7%, and other races receive 20.6%.

We obtain additional information on the characteristics and performance of *originated* mortgages by merging HMDA with the Black Knight McDash (McDash) dataset (the merge follows the approach of Rosen, 2011). Individual observations in HMDA and McDash are merged using loan origination date, loan amount, zip code, lien type, loan type, loan purpose, and occupancy type (owner occupied, absentee or investment property). The match rate is approximately 60%.⁷ McDash provides information on delinquencies, defaults and future refinancings, along with additional information on loan characteristics, such as the mortgage interest rate, rate type (fixed or adjustable rate), the mortgage term, whether the loan is conforming, borrowers’ FICO scores, and the quality of the supporting documentation submitted by the borrower. Table 1 shows summary statistics for the merged sample (columns 4 to 6). Black borrowers obtain smaller loans and have lower income than other borrowers. Moreover, they are more likely to have FICO below prime (below 660) and loan-to-value (LTV) above 80%.

Finally, we obtain the extended version of the HMDA dataset, available for the years 2018 and 2019. This new version of HMDA contains additional underwriting information for all loan applications, including applicant FICO scores, LTVs, and debt-to-income ratios. Also, it contains the approval recommendation generated for each loan by the lender’s Automated Underwriting System (AUS). These are automated processes that provide computer generated approval recommendations. Several algorithms are used in the industry, developed either by private

⁷Lender and consumer identities were anonymized for the merged dataset used in this analysis. There are no within-month patterns in the match rates.

companies or government agencies, such as the Fannie Mae’s Automated Underwriting System. The last three columns of Table 1 present summary statistics for the new HMDA data. The racial composition of the sample and applicant characteristics are similar to that of the full HMDA sample.

4 Identifying Assumptions

Our identification strategy relies on high-frequency variation in evaluators’ economic incentives and hence their scope for subjective decision-making. This section provides support for the identification assumptions: (1) the applicant pool is time-invariant and (2) there is time-variation in loan officers’ incentives.

4.1 The Applicant Pool is Time-Invariant

Our tests identify discrimination under the assumption that the composition of the applicant pool is time-invariant. Figure 3 shows the composition of new applicants on each day of the month. Panel (a) shows that Black applications constitute approximately 7% of total applications on a given day, and that the fraction stays constant over the course of the month. This confirms our identifying assumption that the racial composition of applicants is time-invariant.

We also verify that other characteristics of the applicant pool—characteristics that could correlate with race—are constant over the course of the month. The HMDA data prior to 2018 has limited information on applicants’ creditworthiness. However, the data contain applicants’ income, which is an important input into lenders’ decision-making and likely correlates with other variables that determine whether an application is approved (e.g., credit scores). Panel (b) reports the fraction of applicants whose personal income is less than the median within the applicant’s county and in the same year. Panel (c) shows the fraction of new originations for applicants with below-median incomes. In both panels, we sort the sample into applications submitted by Black and white applicants. As such, these figures explore whether the quality of applications within and across races change over the course of the month. We find that application quality is constant.

Lastly, panel (d) studies the composition of the applicant pool. We do so to illustrate the types of applicants that loan officers can choose to work with over the course of the month. The figure plots outstanding applications—applications that have been submitted but have yet to be processed—in the lenders’ inventory on each day. Again, we find that the racial composition of the applicant pool and application quality are constant over the course of the month.

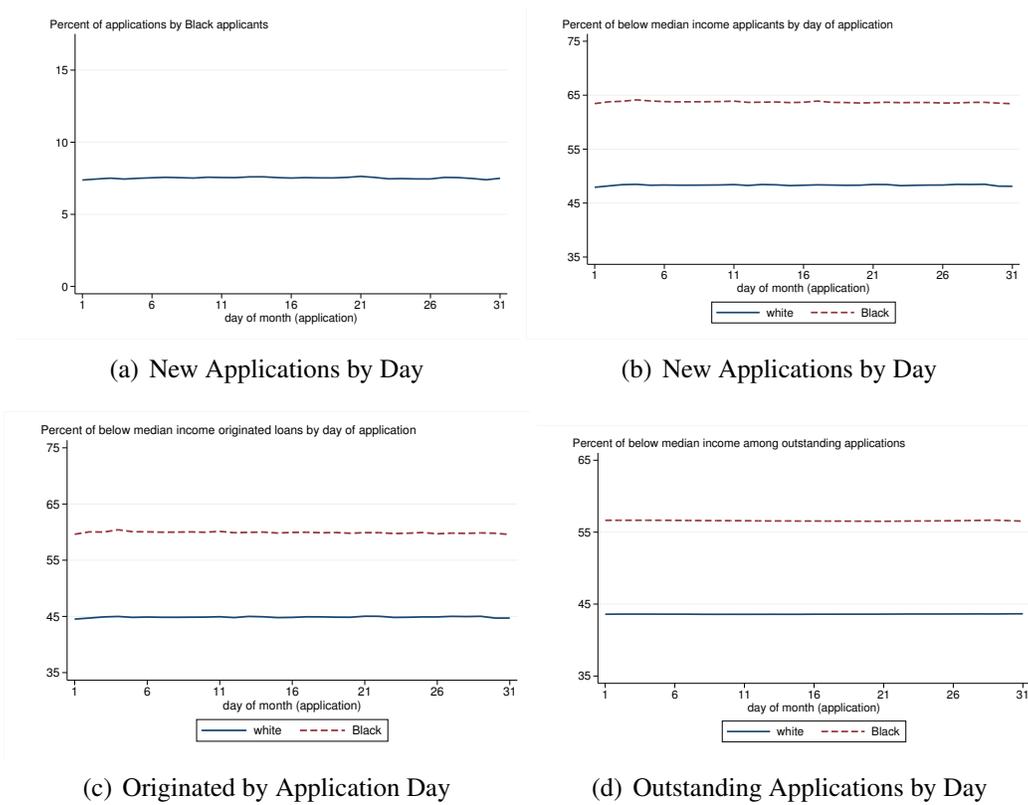


Figure 3: The data come from HMDA between January 1994 and December 2018. Panel (a) shows the average fraction of applications submitted by Blacks. Panel (b) shows the fraction of applicants, separately for Black and white applicants, whose income is less than their county’s median income in that year. Panel (c) shows the fraction of loans originated to applicants whose income is less than their county’s median income in that year. Panel (d) shows the fraction of outstanding applications submitted by applicants whose income is less than their county’s median income in the year.

4.2 Time-Variation in Subjective Assessments of Applicants

4.2.1 Monthly Volume Quotas and Bunching in Mortgage Originations

Loan officers tend to receive commissions that equal a percentage of the total amount they originate during the month. They can also receive bonuses for meeting monthly origination targets. Loan officers that fail to meet volume targets can be disciplined and risk getting fired.⁸ U.S. regulations, including directives from the Consumer Financial Protection Bureau (CFPB), acknowledge the use of volume-based incentives. U.S. law allows volume-based incentives but it restricts the use of commissions based on the terms and performance of individual loans (see, most recently, the dispositions of Regulation Z, implementing the Truth in Lending Act).⁹

We connect monthly volume quotas to the large increases in new originations at month-end.¹⁰ Figure 1, described in the introduction, presents the average volume of new originations per day relative to the first day of any given month. The volume of new mortgage originations grows over the course of the month, and is more than 150% larger on the last day relative to the first day of the month.

The evidence on end-of-month “bunching” in mortgage originations is robust across time and to seasonal factors. The end-of-month increase in originations occurs in every year of our sample, which suggests that the finding is not caused by business cycles, and is therefore unlikely to be caused by fluctuations in the demand for mortgages (see Online Appendix Figure A.1). Also, the end-of-month bunching occurs in every month of the calendar year (see Online Appendix Figure A.2).¹¹ This suggests that the finding is not caused by seasonality in mortgage demand.

⁸Volume targets are described in Tzioumis and Gee (2013) and in industry publications. For example, the Mortgage Bankers Association describes industry standards for loan officers’ compensation ([webpage link](#)), and consumer websites do as well ([webpage link](#) and [webpage link](#)).

⁹The most recent revision of Regulation Z, section on *Permissible Methods of Compensation*, describes compensation schemes and first outlines volume-based incentives ([webpage link](#)).

¹⁰It is beyond the scope of our paper to pin down a single mechanism that explains why volume targets cause the end-of-month bunching in originations. Nonetheless, to sketch plausible mechanisms, loan officers might find it optimal to exert less effort at the start of the month. Loan officers may also procrastinate or be overconfident that they can meet their quotas.

¹¹To highlight the quality of our micro-level data, recurring-day bank holidays have visible reductions in origination volume (Figure A.2).

Building on our graphical evidence, we use regression analysis to show that the month-end increase in originations is robust to potential confounding factors. We estimate the following regression:

$$\log(N_t) = \beta_{lw}I_{lw} + \beta_{fw}I_{fw} + a_{ym} + a_{dow} + a_{holiday} + e_t \quad (3)$$

where the dependent variable $\log(N_t)$ is the log of the number of originated mortgages in the U.S. on day t . The regression includes year-month, day-of-week, and bank-holiday fixed effects (a_{ym} , a_{dow} , and $a_{holiday}$, respectively). Indicator variables I_{lw} and I_{fw} are equal to one for days in the last week of the month and the first week of the following month, respectively. The coefficient of interest, β_{lw} (β_{fw}), measures the average difference between the origination volume in the last (first) seven days of the month, relative to the middle days of the month.

The regression estimates confirm that loan origination volume increases significantly in the last days of the month. Table 2, columns (1) to (3) estimate equation 3 using the log number of loans as the dependent variable. The point estimate of β_{lw} is 31%, and the estimate of β_{fw} is -15%, which implies a 46% increase in new originations over the course of the month. Column (4) sets the dependent variable as the log dollar volume of originated loans. The dollar value of originations increases by 50% from the start to the end of the month. Our findings are unlikely to be explained by lending seasonality because the estimates are robust to including a rich set of calendar time fixed effects (see e.g., Murfin and Petersen, 2016).

We also show that the end-of-month bunching in new originations is consistent with loan officers managing the inventory of applications over the course of the month. Figure A.3 in the Online Appendix shows the inventory of applications that await a decision (approval, denial, or withdrawal by the applicant) for each day within the month. There is a sharp drop in inventory over the last week of the month, driven by the spike in originations, and then a steady increase taking place over the first two weeks of the following month.

4.2.2 Linking Origination Volume to Loan Officers' Performance

Next, we connect loan officers' performance incentives to the end-of-month bunching in new originations. To do so, we consider how loan officers' monthly volume targets affect their incentive to increase origination volume. Specifically, we expect that loan officers have to increase the pace of new originations when they are not on track to meet their quotas. Though our data do not contain the origination targets set by each lender, we infer that loan officers' volume targets are a function of mortgage lending seasonality and the lender's internal projections. As such, we expect that each lender will have their own monthly benchmarks that are a function of their origination volume in prior years (e.g., a lender's origination volume in March 2012 is a reasonable estimate of their volume target in March 2013).

Accordingly, we construct a measure that approximates whether or not loan officers at a given lender are likely to be on track to meet their performance targets. The measure relates the current month's origination volume relative to prior year's:

$$RelPerf_{i,ym} = \frac{AvgVol_{i,ym}}{AvgVol_{i,ym'}} \quad (4)$$

where $AvgVol_{i,ym}$ is the average daily volume of mortgage loans that have been issued by lending institution i , in year-month ym , excluding the last 7 days of the month. The denominator is the average daily volume of mortgage loans issued by the same lending institution in month ym' , exactly one year before ym . The denominator of equation (4) proxies for institution i 's volume target, which is based on the performance in the same month of the previous year. We expect that loan officers are behind their volume targets when the value of $RelPerf_{i,ym}$ is small. Loan officers who are behind their volume targets have incentive to increase their lending before the month ends.

Indeed, origination volume at the end of the month increases by a larger amount when loan officers are more likely to miss their quotas. Figure 4 shows origination volume around the end of the month. The figure splits the sample into lenders that have values of $RelPerf_{i,ym}$ in the top quartile of lenders in a given month and lenders with values of $RelPerf_{i,ym}$ in the bottom quartile. The month-end increase in originations is substantially larger when $RelPerf_{i,ym}$ is in the bot-

tom quartile. This provides evidence that loan officers increase the pace of new originations at the end of the month in order to meet their performance targets, and suggests that the end-of-month increase in origination volume is caused by loan officers’ monthly volume quotas.

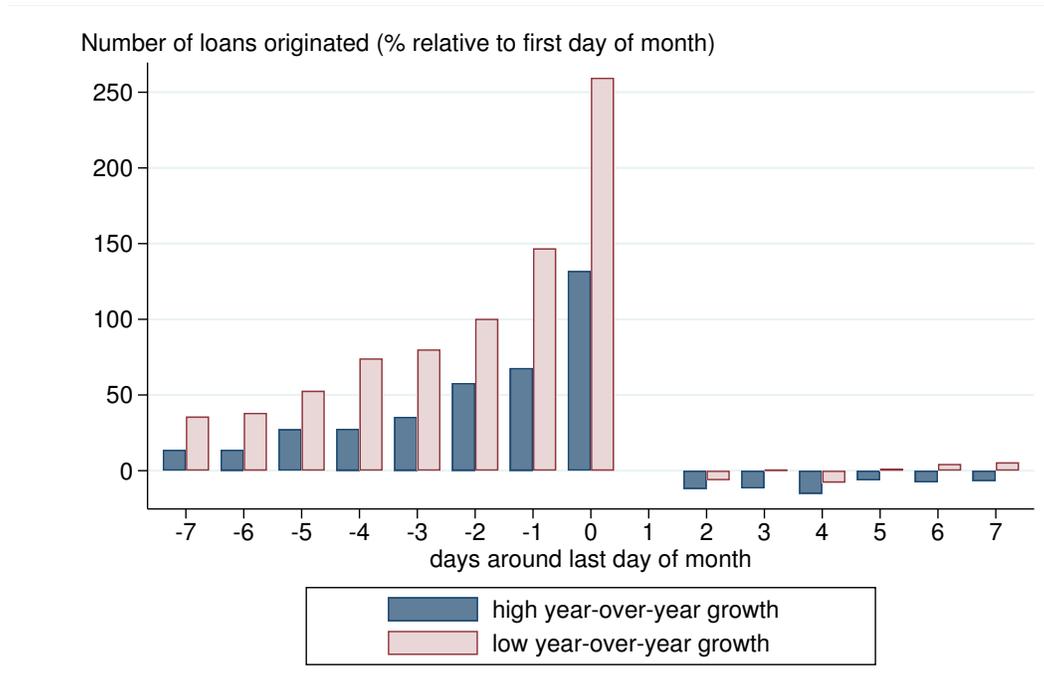


Figure 4: The data come from HMDA between January 1994 and December 2018. The figure shows average loan origination volume in percentage terms relative to the first day of any given month. We sort lenders by the volume of originations that they have made prior to the last week of a given month relative to the total number of originations in the same month of the previous year (defined according to equation 4). High (low) year-over-year growth indicates that the lender is in the top (bottom) quartile of loan originations relative to the prior year. This sample sorting proxies for lenders that are ahead of (high) or behind (low) their volume quotas heading into the last week of the month.

Furthermore, we show that the month-end increase in originations is unlikely to be caused by lenders “window-dressing” in order to meet the criteria of regulatory exams. Specifically, lenders might increase originations to disadvantaged neighborhoods in an effort to meet the requirements of upcoming Community Reinvestment Act (CRA) examinations.¹² Columns (5) and (6) in Table 2 sort lenders

¹²CRA exams are administered by four different federal regulators—the Federal Reserve Board (FRB), the Federal Deposit Insurance Corporation (FDIC), the Office of the Comptroller of the

by whether or not they have a CRA exam scheduled in the following month. Then, separately for the two samples, we estimate the specification in equation (3), in which the dependent variable is the logarithm of daily origination volume. Lenders increase originations at month-end regardless of whether they have upcoming CRA examinations. The month-end increase in lending is actually smaller for institutions that are subject to CRA exams.

5 Testing for Discrimination Using High-Frequency Evaluations

In this section, we use the modified benchmarking test developed in Section 2 to test for discrimination in mortgage lending.

5.1 High-Frequency Benchmarking Test

We test for discrimination in mortgage lending by estimating how the difference in approval rates across race change within any given month. Our tests use the following linear probability regression specification:

$$\begin{aligned}
 Appr_j = & \delta_{lw,Black} (I_{lw,j} \times I_{Black,j}) + \delta_{fw,Black} (I_{fw,j} \times I_{Black,j}) + \delta_{lw} I_{lw,j} + \quad (5) \\
 & + \delta_{fw} I_{fw,j} + \delta_{Black} I_{Black,j} + BX_j + a_{ym,county} + a_{ym,lender} + a_{dow} + a_{holiday} + u_j
 \end{aligned}$$

where the unit of observation is a loan application. The dependent variable $Appr_j$ equals one if the loan is approved, zero otherwise. Indicator variables $I_{fw,j}$ and $I_{lw,j}$ equal one if the loan is originated or denied in the first week or the last week of the month, respectively. $I_{Black,j}$ is equal to one for Black applicants. X_j is a vector that contains the characteristics of mortgage application j : loan amount, conforming loan status, loan type (conventional, or government guaranteed or insured, such as FHA, VA, and USDA loans), occupancy type (owner occupied or absentee),

Currency (OCC), and the Office of Thrift Supervision (OTS)—and they are conducted every two years for large banks and every five years for small- and medium-size banks. Banks know in advance the exam dates. CRA exams review the lender’s fair lending practices and are designed to ensure that the bank meets the credit needs of disadvantaged communities in the markets it serves.

loan purpose (new purchase or refinancing), and applicant income. Year-month-county, year-month-lender, day of the week, and holiday fixed effects are $a_{ym, county}$, $a_{ym, lender}$, a_{dow} , and $a_{holiday}$, respectively. The coefficients of interest, $\delta_{lw, Black}$ and $\delta_{fw, Black}$, capture the average approval rate for Black applicants in the last and first week of the month, both relative to the other days of the month.

Table 3 uses the HMDA data to test the regression model in equation (5). Because we find that the estimates are robust across specifications, we describe estimates from the specification with the most stringent fixed effects (column 4). The point estimate of $\delta_{lw, Black}$, which captures the abnormal approval rate for Black applicants in the last week of the month, is equal to 2.7 ppt. The estimate of $\delta_{fw, Black}$, the abnormal approval rate in the first week of the month, is equal to -0.7 ppt. This implies that the likelihood of approval for Black applications relative to other applications increases by 3.4 ppt if the application is processed in the last seven days of the month (reported in the table row labeled “last-first (black-other)”).¹³

The estimates of the within-month change in approval rates for Black applicants are large. For context, we estimate that Black applicants are 6.8 ppt less likely to be approved after controlling for observables (the coefficient estimate on $I_{Black, j}$). This estimate signifies the amount of discrimination against Black applicants according to a conventional benchmarking test. However, a conventional benchmarking test is unable to determine whether the 6.8 ppt difference is caused by bias or whether it reflects unobserved heterogeneity across races. On the other hand, because our empirical design suppresses the cross-sectional variation across applicants’ races, we can attribute the within-month approval gap of 3.4 ppt to loan officers’ subjectivity. As such, the ratio of the within-month difference to the unconditional difference—3.4 divided by 6.8, or 50%—approximates the share of the observed Black approval gap that can be attributed to subjective decision-making. In other words, we attribute at least half of the Black approval gap to bias.

¹³Online Appendix Table A.5, columns (4) and (5) present estimates of logistic regression models that are analogous to the linear probability model in equation (5). The logistic regressions produce estimates that are similar to the linear probability estimates. We prefer to use a linear probability model throughout our main analysis because the OLS estimator is better at accommodating the complete set of fixed effects that affect approval decisions.

These regression tests confirm the graphical evidence in Figure 2 that the Black approval gap declines over the course of the month. We also plot changes to the approval gap over the course of the month based on estimates from the saturated regression model in Table 3, column (4). Figure 5 plots the average day-by-day residual difference in approval rates after controlling for application characteristics. The approval gap approximately equals 7 ppt in the first seven days of the month. It shrinks to approximately 1 ppt on the last day of the month. Therefore, after controlling for loan characteristics, there is almost no difference in application approval rates across races on the last day of any given month.

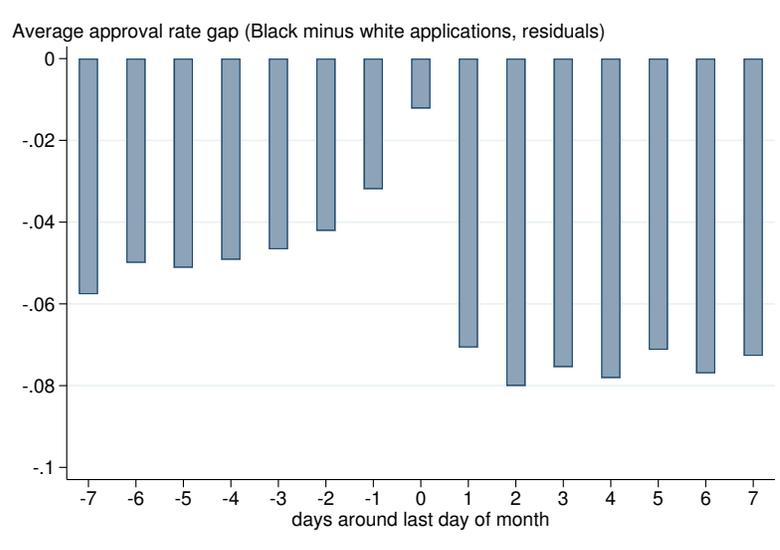


Figure 5: The data come from HMDA between January 1994 and December 2018. The regression residuals come from the regression described in equation (5) and analogous to the specification estimated in column (4) of Table 3. After estimating the regression, we take the average regression residuals on each day, separately for Black and white applicants, and compute the (controlled for) approval gap on each day.

The regressions in Table 3 also convey insight into how differences across lending institutions affect lending to Black mortgage applicants. Notably, the literature attributes much of the Black approval gap to different lending institutions catering to different types of borrowers. We gain insight into the role of selection across institutions by examining the effect of lender fixed effects on the regression estimates. Including lender fixed effects reduces the magnitude of the un-interacted

coefficient on I_{Black} from -0.10 in column (2) to -0.07 in column (3). This result implies that lender fixed effects are a crucial source of unobservable variation driving the Black approval gap. On the other hand, lender fixed effects have a negligible effect on the within-month approval gap. The within-month Black approval gap is 0.040 without lender fixed effects and 0.035 with them. These results suggest that our approach captures a component of loan officer decision-making that exists within lenders and is invariant to institutional differences. These results are reassuring for our empirical design and the interpretation of the findings, because incentive compensation schemes are widely used across lending institutions.

The month-end decline in the Black approval gap is highly robust (see Online Appendix Table A.1 for the following tests). The results are robust to controlling for applicants' gender, and including Black-year fixed effects. Furthermore, the estimates are unaffected by replacing calendar-month fixed effects with fixed effects that span the end and start of successive calendar months (e.g., January 15 to February 14). Lastly, the estimates are robust to interacting application characteristics—the loan-level control variables—with indicators for first and last week of the month. This allows the effect of application characteristics on approval decisions to flexibly change over the course of the month. For example, this regression specification would allow applicants' incomes to have a stronger effect on loan decisions at different times within a month.

Finally, Table 4 estimates our modified benchmarking test on different subsamples of the HMDA data. Doing so helps exclude alternative interpretations of our findings. We find that the Black approval gap declines by approximately the same amount (0.035) for both new purchases and refinances (columns 1 and 2, respectively). Consequently, our findings are unlikely to be explained by borrowers strategically closing at the end of the month in order to minimize the costs of housing transitions (e.g., not having to pay rent and a mortgage at the same time). Furthermore, the Black approval gap declines by a similar amount when we restrict the sample to conforming or conventional loans (columns 3 and 4, respectively). This suggests that our findings cannot be explained by financial incentives for FHA loans to close at the end of the month. Specifically, FHA mortgages originated before January 2015 required borrowers to pay the entire interest for the month when

paying off the loan or refinancing, including interest payments that have not yet accrued. To further corroborate that this effect does not drive our results, column (5) shows that the approval gap declines by just as much for the sample of loans restricted to conventional (non-FHA) new purchases. In general, alternative explanations based on borrowers' incentive to close at the end of the month relate to the possibility that financially constrained borrowers prefer closing at month-end to minimize advance interest payments at the time of closing. Contrary to this explanation, we find that higher income borrowers—who are presumably less financially constrained—have a larger decline in the end-of-month approval gap than lower income borrowers (columns 6 and 7).

5.2 Challenges to Identification of Time-Varying Discrimination

The evidence that the Black approval gap nearly disappears at month-end is consistent with loan officers having less scope for subjective decision-making when they have monthly volume quotas, as outlined by the framework in Section 2. Yet we consider plausible challenges to the interpretation that the within-month change in approval rates is evidence of discrimination.

Instead of conjuring specific alternative explanations, consider how the empirical design limits the scope for alternative theories. Our empirical strategy rules out the possibility of time-invariant unobserved differences across applicant groups. Notably, the within-month variation in approval rates cannot be explained by variation across lenders because the estimates are robust to lender fixed effects.

Therefore, any candidate alternative explanation has to have within-month variation and also has to have differential effects on Black applicants relative to other applicants. Not only does this confine alternative explanations to factors that vary within the month, but it also gives us an avenue to test alternative theories. For example, suppose that the indicator variable for Black applicants reflects other unobserved characteristics, such as the loan's risk profile, and that loan officers delay processing high-risk applications. If application risk explains the convergence in approval rates across race over the course of the month, then the observed riskiness of loan applications would explain within-month changes in approval rates. Put simply, we would expect to find that originations of observably high-risk appli-

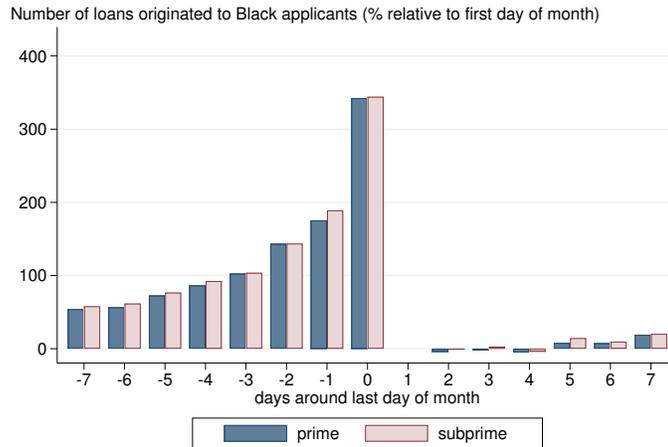
cations submitted by Black applicants would have more month-end bunching than low-risk applications.

Guided by these bounds on alternative theories, we can confront alternative interpretations broadly by examining measures of loan quantity over the course of the month. We start by studying applicant credit scores because they are likely the most important input into lenders' decision-making on approval decisions and mortgage pricing. Credit scores would also correlate with other application attributes that could form the basis for alternative explanations. For example, credit scores directly measure the ex-ante risk of the application, and low credit score applicants would be more likely to file low-documentation applications. As Section 3 describes, the data only contains credit scores for applications that are approved. As such, we study the quantity of new originations instead of approval rates. However, such tests are similar to testing for differences in approval rates because we have shown that mortgage demand does not vary within the month.

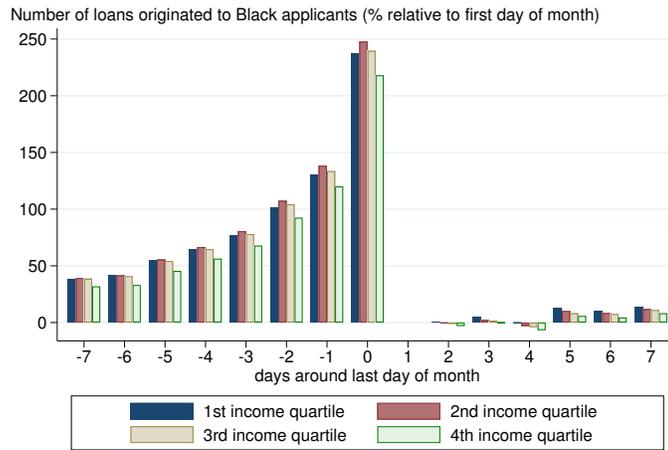
We find that alternative explanations related to application quality are unlikely to explain the within-month approval gap. Figures 6 and A.4 (in the Online Appendix) plot the quantity of new originations sorted by credit scores for applications submitted by Blacks and whites, respectively. The volume of originations for prime-credit-score ($FICO \geq 660$) and subprime ($FICO < 660$) Black applicants are nearly identical over the course of the month (Figure 6(a)). Also, the end-of-month bunching of originations is larger for Blacks than whites for both prime and subprime applicants (comparing the levels in Figure 6(a) to those in Figure A.4(a)). We would have expected to find relatively more end-of-month bunching for subprime and low-income Black applicants if the results simply reflected differences in application risk factors.

We find similar results when we sort the volume of new originations into quartiles by applicant incomes (Figure 6(b) and Figure A.4(b)). Testing for end-of-month bunching across applicant incomes not only fortifies evidence from sorting by credit scores but also allows us to present evidence from the full HMDA sample. We find that there is substantial end-of-month bunching for all four income quartiles. Moreover, in each corresponding quartile, the end-of-month bunching for Black applicants is significantly larger than for white applicants. These findings

cast further doubt on alternative explanations related to within-month variation in application risk.



(a) Effects for Prime and Subprime Black Applicants



(b) Effects by Income Quartile Black Applicants

Figure 6: Both figures display origination volume in percentage terms relative to the first day of the month for Black applicants. Panel (a) uses the merged HMDA and Black Knight McDash sample from 1994 to 2018. It sorts applicants into those with prime (660 or higher) or subprime FICO scores. Panel (b) uses the un-merged HMDA sample from January 1994 to December 2018. It sort applicants into quartiles based on their personal income within their county in the same year.

5.3 Tests for Omitted Time-varying Characteristics: Ex-Ante Credit Quality

The empirical design that we use to estimate discrimination limits the scope of omitted variables to factors that change between the start and the end of the month. The regression tests suppress cross-sectional differences across races. However, we acknowledge that other characteristics—including characteristics that can correlate with race—might also vary within the month. We address this possibility by assessing the extent to which observable characteristics vary within the month by using the HMDA-McDash sample and the recently available HMDA data that includes loan origination recommendations from Automated Underwriting Systems (AUS).

We start by directly testing whether certain types of loans are more or less likely to be originated over the course of the month using the HMDA-McDash merged sample. Specifically, Table 5 tests the regression specification in equation (5), but replaces the dependent variable with variables that measure loan quality for originated loans: an indicator for subprime loans (column 1), loan-to-value (LTV) ratios (column 2), an indicator for low-documentation loans (column 3), and the interest rate (column 4). We find that, of the four loan characteristics, only low-documentation loans are more likely to be originated in the last week of the month, but the effect is only weakly statistically significant. Even so, Black applicants with low-documentation applications are relatively *less* likely than white applicants to be originated in the last week of the month. These results suggest that our empirical design disentangles the effect of race from other loan characteristics—characteristics that correlate with race in the cross-section.¹⁴

Next, we compare the differences between AUS recommendations and lenders' actual loan decisions around the end of the month by using the extended HMDA data available for 2018 and 2019. Figure 7 plots the difference between AUS recommendations made for Black and white applicants on each day. The figure also plots the difference between Black and white approval rates. Effectively, the figure

¹⁴To also provide an out-of-sample test of our methodology, we test for changes in end-of-month approvals for other underrepresented groups. Table A.2 presents regression estimates of equation (5) testing whether the female approval rate converges at month-end to the male approval rate. Column (4) reports the regression results with the full set of controls and shows that the the female approval gap shrinks from 2.4 ppt in the first week of the month to 1 ppt in the last week of the month. See e.g., Goldsmith-Pinkham and Shue (2021) for other studies on gender disparities in housing markets.

demonstrates how the AUS system evaluates Black applicants relative to how Black applicants are evaluated by lenders.

We find that the AUS system recommends that Black applicants should be approved approximately five to eight percentage points less frequently than white applicants. This suggests that Black approval rates should be approximately five to eight percentage points lower when using the objective, race-neutral criteria contained in the loan application. Notably, however, the Black AUS-recommendation gap is relatively constant over the course of the month. On the other hand, the actual Black approval gap declines significantly. The Black approval gap nearly converges to the AUS-recommendation gap on the last day of the month.

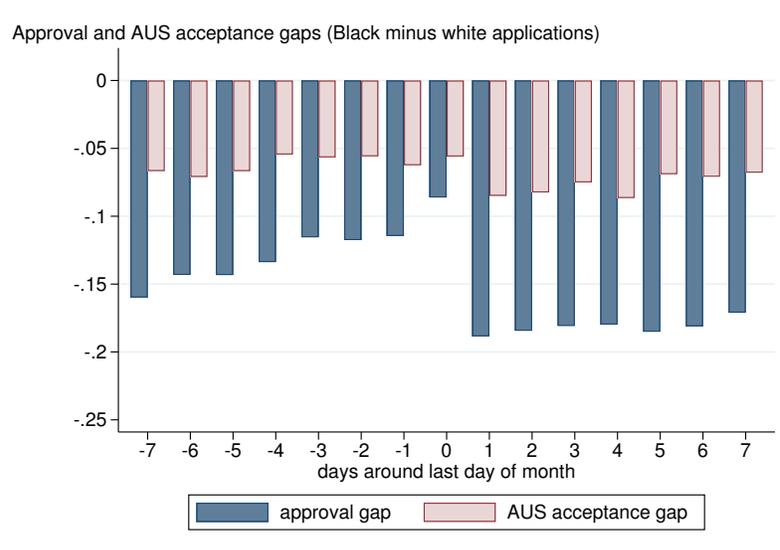


Figure 7: This figure uses the “new” extended HMDA dataset, which includes loan applications in 2018 and 2019. The figure reports approval rates defined as the fraction of loans that are originated out of the total number of applications (excluding withdrawn applications). It presents the difference between Black and white applicant’s approval rates. The figure also shows the difference in approval *recommendations* from lenders’ Automated Underwriting Systems.

We also show that our regression estimates on the within-month Black approval gap are robust to controlling for AUS recommendations to account for differences across applicants (Table 6). Column (1) provides a baseline comparison between the old and new HMDA data by using the new data but not including any control variables that are not available in the old data. Column (2) expands the

set of control variables to include indicators for different quintiles of FICO scores, loan-to-value ratios, and debt-to-income ratios (all of which are not available in the old HMDA data). Columns (3) through (5) control for the AUS recommendations. Column (5) includes indicators for the type of AUS system used to evaluate the loan. The regressions confirm that the Black-white approval gap is 3 to 4.5 percentage points smaller at the end of the month. The estimates are robust to the inclusion of the new control variables, including the AUS recommendations.

5.4 Tests for Omitted Time-varying Characteristics: Ex-Post Outcomes

Lastly, we study the ex-post performance of originated loans. Table 7 estimates the regression specification in equation (5), but sets the dependent variable equal to one for mortgages that face a 90-day delinquency within 5 years after origination, and zero otherwise.¹⁵ Column (1) includes the full sample of originations. Columns (2) through (4) restrict the sample to loans that might be considered risky or difficult to evaluate at the time of origination. Column (2) restricts the sample to subprime loans ($FICO < 660$), column (3) to high loan-to-value loans ($LTV > 80\%$), and column (4) to low documentation loans. All regressions include the full set of loan-level control variables available in the merged HMDA-McDash data.

Even though loans to Black borrowers are approximately five percentage points more likely to become delinquent, we find that the day-of-the-month that the loan is originated does not meaningfully affect delinquency rates. First, loans to non-Black borrowers become delinquent at the same rate whether they are originated at the start or the end of the month. Next, loans to Black borrowers originated in the last week of the month are not statistically more likely to become delinquent than the the baseline delinquency rate for Blacks. Only loans originated to Blacks in the first week of the month are more likely to become delinquent but the difference relative to the baseline Black delinquency rate is small in magnitude and is mainly confined to the low-documentation sample.

¹⁵Our findings are robust to alternative measures of financial distress. Online Appendix Table A.3 repeats the same analysis, but sets the dependent variable equal to one for loans that defaulted within 5 years after origination, while Table A.4 sets the dependent variable equal to one for loans that were terminated (due to default or refinancing) within 5 years.

These findings on ex-post performance help interpret the within-month approval gap that we estimate in our modified benchmarking tests. Though we show that the within-month approval gap is robust to various observable measures of credit quality, some *unobservable* factors might be orthogonal to *ex-ante* observable characteristics. However, these unobservable differences across groups would cause differences in ex-post performance, which we can observe and we test for in Table 7. Therefore, these tests suggest that the within-month empirical design is not biased by unobservable differences that vary within the month. Furthermore, by showing that there are no within-month differences in ex-post delinquency rates, these findings directly counter the explanation that loan officers approve riskier loans at the end of the month.

5.5 The Timing of Originations

Though we show that application volumes are constant within any given month, a lingering challenge to our interpretation of the within-month Black approval gap is that it might reflect differences in how long it takes to complete the origination process. In this section, we provide evidence that time-to-origination is unlikely to explain why the Black approval gap declines at month-end.

We start by showing that the within-month approval gap is robust to accounting for the time to action—origination or denial—on the loan (Table 8). We find that estimating equation (5) while controlling for time-to-action does not significantly affect the coefficients on the within-month approval gap (column 1). We also estimate the approval gap using sub-samples of the data sorted on time-to-action: 1 to 30 days, 31 to 60 days, 61 to 90 days and more than 90 days (columns 2 through 5, respectively). The within-month Black approval gap is present in all four sub-samples. Also, the estimates exhibit no patterns across sub-samples.

Next, we examine how processing times change over the course of the month. Table 9 contains regressions that set time-to-origination as the dependent variable. Columns (1) and (2) regress time-to-origination on indicator variables for the first and last week of the month (and the full set of fixed effects and loan level controls). There is no difference in the time to origination for loans issued at the start versus the end of the month. Originations to Black applicants take three days longer on

average. Moreover, Black applications take a half-day longer to originate when they close in the last week of the month. This effect is small relative to the average time-to-origination.

These findings address concern over a crucial feature of the HMDA data. Specifically, the origination date might be different than the date when loan officers make their decisions. Our analysis uses the origination date as a proxy for lenders' approval decisions because the data does not contain distinct records of "approval" dates. For this reason, our tests account for the delay between approval-decision and origination dates by having a wide period—the last seven days of the month—under which loan officers have incentive to meet origination quotas. Furthermore, by showing that time-to-origination is not a relevant confound, we gain additional comfort that the delay between loan decisions and origination dates does not explain our findings.

Beyond the empirical evidence, we describe the institutional details of the origination process that help explain why a delay between loan officers' decisions and originations is unlikely to confound our interpretation. In the loan origination process, lending institutions send borrowers a closing disclosure document that says the borrower is "cleared to close." The document confirms the mortgage conditions and closing costs. This information is provided only after confirmation that the applicant's documentation is satisfactory and the application meets the underwriting standards.¹⁶ At the lending institution, loan "processors" prepare the mortgage documentation in collaboration with the loan officer. They set a closing date, on which the loan documentation will be signed, and the mortgage will be originated. However, loans can be denied between the "cleared to close" and the close date because institutions will recheck borrowers' financial information, such as employment status and credit score, during this period.

¹⁶Applicants provide information on their income, credit history, and the collateral for the loan. Lenders process this information, verifying the borrower's income, the appraisal of the property, and the Title to make sure that the asset does not already have pending claims. These features of the underwriting process are overseen by the underwriter and tend to take several weeks. If the information provided is incomplete, or the underwriter establishes that the loan does not meet underwriting standards, the lender may deny the application, or require additional information or guarantees from the prospective borrower. Loan officers have much leeway in determining the outcome of these proceedings because they coordinate the interactions between the lender and the borrower.

In practice, the processor tends to set the closing date within a few days of the cleared to close communication.¹⁷ And, there is ample reason for buyers, sellers, and the lender to complete the origination process quickly after clear-to-close. First, sellers might pressure buyers to close quickly. Borrowers might even be bound by signed clauses that state the buying agreement is valid only if funds are provided by a certain date. Second, buyers and lenders will try to close before the expiration of interest rate locks. Lenders issue interest rate locks at the time of application to guarantee that loan conditions do not change during the underwriting process.

How does the lag time between cleared-to-close and origination affect our empirical design and the interpretation of our results? Notably, loan officers need to finalize originations—not just clear-to-close—before the end of the month to meet origination targets. Thus, loan officers have little incentive to stall the closing process and might even work with the loan processor to more quickly finalize originations. Indeed, we find that time-to-origination does not confound the approval gap. Therefore, so long as our empirical tests allow a sufficient time frame at the end of the month to accommodate the lag between decision and closing, there is little reason to suspect that this feature of the data biases our empirical design.

6 Alternative Benchmarks for Discrimination

The standard benchmarking test characterizes the null of no discrimination as there being no difference between the approval rates of minority and majority applicants. Thus, following the literature, our tests in Section 5 build on the benchmarking approach by studying changes in approval rates—we characterize the no-discrimination null as the gap in approval rates between Black and other applicants being constant over the month. This section explores alternative ways to measure changes in discrimination using the ratio of mortgage approvals across races.

Using changes in approval rates to define discrimination has the drawback that Black approval rates are evaluated against unconditional approval rates which

¹⁷The Consumer Financial Protection Bureau introduced the “Know Before You Owe” rule in October 2015. The rule imposed a minimum 3-day waiting period between the receipt of the closing disclosure document and the closing date. The rule was implemented so that borrowers had enough time to review the documentation and contact their lawyer before committing to the mortgage.

may also change within-month, and the regression specification used to estimate the benchmarking test does not fully control for changes to unconditional approval rates. To illustrate this challenge with the approximate numbers from the HMDA data, suppose that 800 applications (100 Black, 700 non-Black) out of 1,000 applicants are approved and 200 are denied (60 Black, 140 non-Black). The approval rate for Blacks would be 62.5% and the approval rate for non-Blacks would be 83.3%, which would give an approval gap of 20.8 percentage points. Suppose that lenders decide to find and approve 30% more applicants (holding constant the total number of denials), and they do so allocating the new originations proportionally across races, according to the historical shares of originations, and thus without explicitly considering race or any characteristic correlated with race. The lender would then approve 130 Black applicants and 910 non-Black applicants, and the approval gap would shrink to 18.2 percentage points.

As such, an alternative way to characterize the no-discrimination null is to test for changes in the *share* of loans that are originated to Black applicants. In the preceding example, notice that Blacks constitute one-eighth of all originations even after the lender decides to approve more loans. We evaluate this alternative null first by testing for changes in approval ratios over the course of the month, and second by evaluating how the approval gap would be expected to change following a proportional change in unconditional approvals.

6.1 Changes to the Ratio of Originations

We find that the share of loans originated to Black applicants increases significantly at the end of the month. Figure 8 plots the share of all approved applications submitted by Black applicants on a given day. On the first day of the month, Black applicants account for approximately 5.5% of approved loans. In the last week of the month, the share increases steadily, reaching just over 6.5% on the last day of the month. Similarly, Figure A.5 in the Online Appendix shows the share of originations to Black applicants on each day in the first and last week of the month. The ratio increases sharply in the last week of the month.¹⁸

¹⁸Furthermore, we study whether the within-month convergence in approval gap is sensitive to the share of Black applicants that a lender processes. Figure A.6 shows that the within-month change

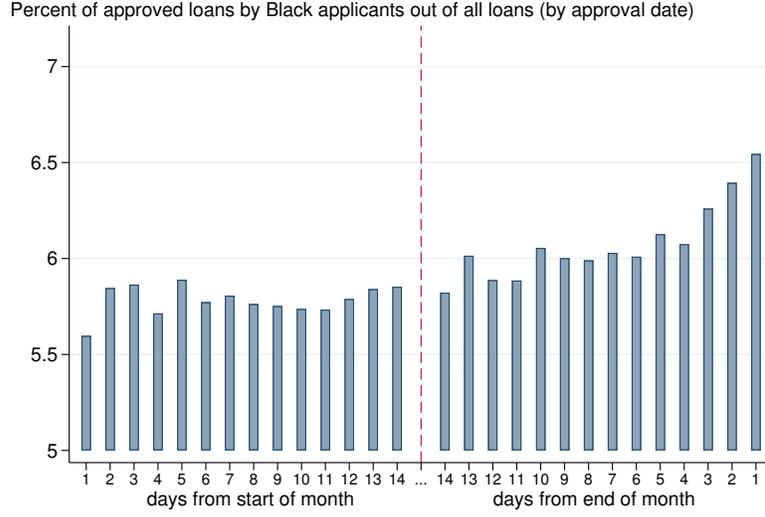


Figure 8: The data come from HMDA between January 1994 and December 2018. The figure shows the fraction of loans that are originated to Black applicants on each day.

We also test for changes to the ratio of new originations using regression analysis at the loan-level (Online Appendix Table A.5). We use the full sample of originations and denials to estimate:

$$I_{Black,j} = \delta_{lw,Appr} (I_{lw,j} \times Appr_j) + \delta_{fw,Appr} (I_{fw,j} \times Appr_j) + \delta_{lw} I_{lw,j} + \delta_{fw} I_{fw,j} + \delta_{Appr} Appr_j + BX_j + a_{ym,county} + a_{ym,lender} + a_{dow} + a_{holiday} + v_j \quad (6)$$

where $I_{Black,j}$ equals one for applications submitted by Black borrowers and $Appr_j$ equals one for approved (originated) loans. The other variables are the same as in equation (5). The coefficient on the interaction term between the last and first week of the month and the indicator for approvals— $\delta_{lw,Appr}$ and $\delta_{fw,Appr}$ —should not be statistically different from zero under the null that the share of loans originated to Blacks is constant.

in approvals occurs across the full range of lenders. The figure reports the median share of approved loans from Black applicants, along with the 25th and 75th percentiles, across lenders that issued at least 10 loans per day on average over the year. The median share is close to 5.5% in the first two weeks of the month. However, it steadily increases, across the entire distribution, in the last week of the month. On the last day of the month, the median share is close to 6.5%, the 25th percentile is 4% and the 75th percentile is 10%.

The regression estimates provide evidence that the share of originations to Blacks increases at the end of the month, even after controlling for application characteristics. This rejects the alternative null of no discrimination. The coefficient estimates of $\delta_{lw,Appr}$ are statistically significant and positive (columns 1 through 3 of Table A.5). The estimates are robust to different fixed effects and loan level controls, and the magnitude is large. The relative increase in the share of Black originations (out of Black and white originations) is between 3.4% and 6.6%, a magnitude which is roughly consistent with the increase in approval rates measured in Table 3.

6.2 Benchmarking Test Under the Null of a Proportional Change in Approvals

Furthermore, we derive an alternative counterfactual for our approval gap test. In our prior tests, we evaluate the within-month change in the Black approval gap against the null of zero difference. Instead, we can evaluate the within-month change against the null that allows for a proportional change in loan originations over the course of the month.

We find that the within-month change in the Black approval gap is substantially larger than would be predicted under the null that there is a proportional change in origination volume. Figure A.7(a) plots the Black approval gap on each day. It also includes a line that shows how the approval gap would change if origination volume increases as much as it does in the data while holding constant the share of loans originated to Black applicants. Though the approval gap decreases at the end of the month, the size of the hypothetical decrease is significantly smaller than the approval gap decrease in the data. Indeed, the counterfactual decline in the Black approval gap is equal to just half of the decline observed in the data.

We can draw similar conclusions—that the within-month decrease in Black approval gap is too large to be explained by the increase in origination volume—from our baseline regression analysis (see equations 5 and A.3, and the regression estimates in Table 3). We calculate conditional estimates of the change in the approval gap at month-end under the assumption that the conditional increase in origination volume matches the one in the data, but origination shares by race remain

constant. We estimate that the approval gap would shrink by only 1-1.5%, which accounts for just 30%-40% of the magnitudes we estimate in Table 3.

Alternatively, we assess the counterfactual effect attributable to the increase in originations by estimating how large the increase would have to be in order to match the reduction in Black approval gap under the assumption that the share of Black originations stays constant. Figure A.7(b) shows daily origination volume relative to the first of the month. The figure also includes a calculation for how large the increase in originations would need to be in order to match the magnitude of the reduction in the Black approval gap. Under the assumption that shares across races stay constant, the increase in origination volume at the end of the month would need to be 22% to 45% larger than in the data.

7 The Market Structure of Lending and Discrimination

Our approach to estimating discrimination enables us to study the effect of policies on the quantity of discrimination. In this section, we explore how innovations to mortgage lending affect the share of the Black approval gap that can be attributed to loan officers' subjectivity.

To assess the effect of mortgage-lender characteristics, we estimate the regression equation:

$$\begin{aligned}
Appr_j = & \delta_{lw,Black,Z} (I_{lw,j} \times I_{Black,j} \times Z_{lender}) \\
& + \delta_{fw,Black,Z} (I_{fw,j} \times I_{Black,j} \times Z_{lender}) \\
& + \delta_{lw,Black} (I_{lw,j} \times I_{Black,j}) + \delta_{fw,Black} (I_{fw,j} \times I_{Black,j}) + \delta_{lw} I_{lw,j} + \delta_{fw} I_{fw,j} \\
& + \delta_{lw,Z} (I_{lw,j} \times Z_{lender}) + \delta_{fw,Z} (I_{fw,j} \times Z_{lender}) + \delta_{Black} I_{Black,j} + \delta_{Black,Z} (I_{Black,j} \times Z_{lender}) \\
& + BX_j + a_{ym,county} + a_{ym,lender} + a_{dow} + a_{holiday} + e_j.
\end{aligned} \tag{7}$$

This specification augments equation (5) with interaction terms for various lender characteristics. The variable Z_{lender} is an indicator for the type of lending institution. The regression captures the effect of lender characteristic Z_{lender} on approval rates (coefficients $\delta_{lw,Z}$ and $\delta_{fw,Z}$), and on the approval rates for Black applicants (coefficients $\delta_{lw,Black,Z}$ and $\delta_{fw,Black,Z}$).

We start by estimating the effects of FinTech lending. FinTech lenders use computer algorithms to assist human decisions in loan processing. FinTech lenders can process applications more quickly (Fuster et al., 2019). We use the data provided by Buchak et al. (2018) to classify FinTech lenders. They define FinTech lenders as those that have a large online presence and that receive the majority of mortgage applications online. In addition, we restrict the sample to the years between 2014 and 2018 because FinTech lending became widespread in recent years and the FinTech classification in Buchak et al. (2018) is based on recently-compiled information about lenders.

We find that FinTech lending does not significantly affect the within-month Black approval gap. Table 10, column (1), presents estimates of equation (7) where we set Z_{lender} equal to one if the lender is classified as a FinTech lender, zero otherwise. We find that FinTech lenders have a larger increase in approval rates for non-Black applicants in the last week—the coefficient $\delta_{lw,Z}$ is close to 3% and statistically significant. However, the incremental effect for Black applicants is not significant. The coefficients on $\delta_{lw,Black,Z}$ and $\delta_{fw,Black,Z}$ are statistically indistinguishable from zero, and their difference is also not statistically significant at conventional confidence levels. Though this result may seem surprising, human loan officers are still involved in the application process at FinTech lenders. Loan officers work with applicants during the origination process even if the application is submitted online.

Next, we study the effects of shadow banking. We use the Buchak et al. (2018) data on shadow banks, which they define as mortgage lenders that do not take deposits. Column (2) sets Z_{lender} equal to one if the lender is a shadow bank, zero otherwise. The within-month increase in approval rates for white applicants is smaller by approximately 2.8 percentage points for shadow banks than other banks. Moreover, the marginal increase in approval rates for Black applicants ($\delta_{lw,Black,Z} - \delta_{fw,Black,Z}$) is smaller by 1.8 percentage points for shadow banks. Thus, shadow banks have a smaller increase in originations at month-end. They also have a smaller Black approval gap at month-end. We speculate that this result can be explained by shadow banks having a larger presence in underserved neighborhoods (Buchak et al., 2018).

Next, we estimate the effects of market structure on the within-month Black approval gap. We start by focusing on local (county-level) market concentration, which we measure in two ways. We calculate, in each county and year, the share of total mortgages originated by the four institutions with the largest number of originations (column 3) and the Herfindahl-Hirschman Index (HHI) based on the share of mortgage originations in the previous year (column 4). Related to market structure, we also estimate the effects of the size of lending institutions. We split lenders into two groups based on whether their total origination volume is above or below the median across lenders in a given year (column 5). Across all three measures, we find no evidence that market concentration and institution size meaningfully affects the within-month Black approval gap.

In summation, we find that shadow banking reduces the within-month approval gap, while FinTech lending and measures of market concentration do not. However, our findings do not imply that FinTech and market competition have no effect on the cross-section of mortgage lending. By including lender fixed effects in all regression tests, our empirical design suppresses cross-sectional differences across lenders. Instead, the within-month approval gap captures behavior *within institutions*. As such, our tests estimate the extent to which these market factors affect the subjective decision-making of institutions' loan officers. Despite innovations to the market structure of lending, loan officers continue to have volume-based compensation incentives and they still influence the application process.

8 Conclusions

Tests for discrimination are often unconvincing because subject groups can have different unobserved characteristics. We develop an approach to limit the omitted variables problem in a conventional benchmarking test by combining high-frequency evaluations with variation in decision-makers' economic incentives, and consequently, their scope to be subjective. Using this approach, we provide new estimates of historical discrimination in mortgage lending. Loan officers tend to have monthly origination quotas and they increase lending at month-end to meet these targets. Then, we show that the approval rate gap for Black applicants attenuates by

half at month-end, when loan officers have incentive to approve more applicants. These results are not explained by within-month patterns in application volume by race nor application risk or quality.

Our findings have implications for the distribution of credit in consumer credit markets. Over the past several decades, legislation such as the Community Reinvestment Act and the Equal Credit Opportunity Act has been introduced to counteract historical inequities in credit access (e.g., red-lining; Appel and Nickerson, 2016; Aaronson et al., 2021). Such legislation is designed to modify the actions of lending institutions. We show that key institutional differences across lenders—FinTech and competition across lenders—do not attenuate loan officers’ behavior. This suggests that policies targeted toward institutions will have limited effect so long as individual decision-makers have discretion to allocate credit.

Our findings call to question why biases persist in mortgage markets despite meaningful changes to its market structure over the past several decades. Theory suggests that competition reduces taste-based discrimination (Becker, 1957). Though many loan officers have to exceed origination targets in order to stay employed, their labor market has significant barriers to entry. Loan officers tend to need at least a bachelor’s degree in a subject like finance or business, and they have to be licensed. Moreover, loan officers often find borrowers through referrals by real estate agents.

Two recommendations emerge from our study. First, the collection of high-frequency data on evaluations, combined with our approach, can be used to measure discrimination in many settings. Second, we suggest that enhanced data collection on the behavior of individual decision-makers within institutions can help researchers and policy-makers understand the effects of subjective decision-making.

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Tables

	HMDA 1994-2018			HMDA-McDash 1994-2018			New HMDA 2018-2019		
	White	Black	Other Race	White	Black	Other Race	White	Black	Other Race
Observations	16,882,565	1,850,734	6,483,189	29,909,913	2,556,790	7,128,756	2,481,456	260,872	756,638
Shares of Applications	66.95%	7.34%	25.71%	-	-	-	70.92%	7.46%	21.62%
Approval Rate	80.72%	63.25%	69.00%	-	-	-	86.58%	76.35%	82.00%
Average Loan Amount (\$ 1,000)	156.21	125.12	176.69	198.34	162.88	231.67	245.24	218.36	294.30
Share Low Income Apps	46.02%	59.75%	47.80%	49.46%	64.73%	50.27%	49.83%	62.12%	48.58%
Share Conforming	87.24%	89.23%	87.09%	94.58%	97.15%	91.65%	-	-	-
Share Primary Residence	91.08%	91.40%	90.25%	91.77%	92.49%	90.54%	92.29%	94.32%	89.79%
Share New Purchases	41.64%	42.73%	32.95%	48.36%	54.34%	44.87%	56.78%	59.67%	50.95%
Share of Originated Loans	73.72%	5.73%	20.55%	75.54%	6.46%	18.00%	73.51%	6.35%	20.14%
Share Below Prime	-	-	-	11.98%	31.05%	12.02%	11.33%	26.32%	10.24%
Share High-LTV	-	-	-	37.30%	58.36%	31.97%	56.42%	75.66%	52.20%
AUS Approval Rate	-	-	-	-	-	-	84.54%	77.65%	83.44%

Table 1: US-level summary statistics, across different race groups, for the historical HMDA loan applications data over the years from 1994 to 2018 (5% sample of the data), the merged sample of originated loans from HMDA and McDash over the years from 1994 to 2018, and for the new HMDA data layout for years 2018 and 2019 (20% sample of the data). *Share of Applications* is the share of applications belonging to each group out of the total, *Share Low Income Apps* is the fraction of applicants with income below the median in the county and year of the application (within each group), *Share Conforming* is the fraction of conforming loans (within each group), *Share Primary Residence* is the fraction of loans for which the collateral is the primary residence of the applicant (within each group), *Share New Purchases* is the fraction of loans for new house purchase (within each group), *Approval Rate* is the fraction of approved loans (within each group), and *Share of Originations* is the fraction of originated loans belonging to each group of applicants, out of the total. *Share Below Prime* is the fraction of loans, within each race group, issued to applicants with FICO below 660, while *Share High-LTV* is the fraction of loans issued to applicants with origination LTV higher than 80%. In the new HMDA sample covering 2018 and 2019, the *AUS Approval Rate* is the fraction of loans that, within each race group, was recommended for approval by the Automated Underwriting Systems (AUS) used by the lender.

	U.S.-Level					
	(1) log(Num Loans)	(2) log(Num Loans)	(3) log(Num Loans)	(4) log(\$ Amount)	(5) log(Num Loans) CRA	(6) log(Num Loans) non-CRA
<i>lastweek</i>	0.25*** (0.052)	0.31*** (0.016)	0.31*** (0.012)	0.36*** (0.013)	0.18*** (0.013)	0.36*** (0.013)
<i>firstweek</i>	-0.22*** (0.052)	-0.15*** (0.016)	-0.15*** (0.012)	-0.14*** (0.013)	-0.11*** (0.013)	-0.18*** (0.013)
Holiday FE	NO	YES	YES	YES	YES	YES
Day-of-Week FE	NO	YES	YES	YES	YES	YES
Month FE	NO	YES	NO	NO	NO	NO
Month-Year FE	NO	NO	YES	YES	YES	YES
<i>last – first</i>	0.47	0.46	0.46	0.50	0.29	0.54
<i>p – value</i>	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N	9131	9131	9131	9131	9131	9131
r2	0.0064	0.91	0.95	0.95	0.95	0.94

Table 2: The table reports regression estimates of the abnormal loan originations volume in the last and first week of the month (see equation 3). In columns (1) to (3) the dependent variable is the log of the number of originations per day in the United States. In column (4), the dependent variable is the total dollar amount of loan originations per day in the United States. In columns (5) and (6) the dependent variable is the log number of originations, respectively, for lenders subject to CRA examination and not subject to CRA examination. *lastweek* and *firstweek* are dummies equal to one, respectively, in the first and last week of the month. The different columns present estimates based on different choices of lender and seasonality fixed effects. Estimates are based on the sample of HMDA mortgage originations from 1994 to 2018.

	(1) approval All	(2) approval All	(3) approval All	(4) approval All
<i>lastweek</i>	0.057*** (0.0037)	0.048*** (0.0035)	0.044*** (0.0031)	0.043*** (0.0031)
<i>firstweek</i>	-0.022*** (0.0025)	-0.021*** (0.0021)	-0.019*** (0.0018)	-0.020*** (0.0017)
<i>black</i>	-0.12*** (0.0069)	-0.10*** (0.0062)	-0.070*** (0.0053)	-0.068*** (0.0043)
<i>black</i> × <i>lastweek</i>	0.036*** (0.0021)	0.032*** (0.0023)	0.028*** (0.0022)	0.027*** (0.0023)
<i>black</i> × <i>firstweek</i>	-0.010*** (0.0015)	-0.0081*** (0.0014)	-0.0073*** (0.0024)	-0.0072*** (0.0015)
log(income)		0.095*** (0.0061)	0.073*** (0.0039)	0.071*** (0.0036)
log(loan amount)		0.031*** (0.0075)	0.0089*** (0.0029)	0.0076*** (0.0023)
is conforming		0.13*** (0.0093)	0.092*** (0.0057)	0.090*** (0.0057)
Other Loan-Level Controls	NO	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year FE	YES	YES	YES	NO
County FE	YES	YES	YES	NO
Lender FE	NO	NO	YES	NO
Month-Year-County	NO	NO	NO	YES
Month-Year-Lender	NO	NO	NO	YES
<i>last</i> − <i>first</i>	0.079	0.068	0.063	0.063
<i>p</i> − <i>value</i>	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i>)	0.13	0.11	0.099	0.097
<i>p</i> − <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	0.046	0.040	0.035	0.034
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0000	0.0000	0.0000	0.0000
N	19641147	18464497	18463245	17898939
r2	0.041	0.092	0.23	0.32

Table 3: The table reports individual loan-level regression estimates of abnormal approval rates in the last and first week of the month (see equation 5). The dependent variable is a dummy that takes value 1 if a loan application is originated and 0 if it is denied. *lastweek* and *firstweek* are dummies equal to one, respectively, if the action on the application is taken in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with the *p*-value of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) approval New Purchases	(2) approval Refinancing	(3) approval Conforming	(4) approval Conventional	(5) approval Conventional New Purchases	(6) approval Above County Median Income	(7) approval Below County Median Income
<i>black</i>	-0.070*** (0.0036)	-0.056*** (0.0059)	-0.067*** (0.0043)	-0.069*** (0.0049)	-0.073*** (0.0047)	-0.074*** (0.0046)	-0.059*** (0.0044)
<i>lastweek</i>	0.035*** (0.0021)	0.051*** (0.0049)	0.043*** (0.0032)	0.042*** (0.0033)	0.034*** (0.0025)	0.037*** (0.0029)	0.049*** (0.0033)
<i>firstweek</i>	-0.014*** (0.0011)	-0.025*** (0.0026)	-0.020*** (0.0018)	-0.019*** (0.0017)	-0.012*** (0.0012)	-0.017*** (0.0015)	-0.022*** (0.0019)
<i>black</i> × <i>lastweek</i>	0.028*** (0.0018)	0.025*** (0.0041)	0.026*** (0.0022)	0.025*** (0.0026)	0.025*** (0.0021)	0.029*** (0.0017)	0.022*** (0.0025)
<i>black</i> × <i>firstweek</i>	-0.0063*** (0.0015)	-0.0096*** (0.0025)	-0.0067*** (0.0015)	-0.0065*** (0.0017)	-0.0045** (0.0020)	-0.0096*** (0.0024)	-0.0050** (0.0018)
Loan-Level Controls	YES	YES	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES	YES	YES	YES
<i>last</i> − <i>first</i>	0.049	0.076	0.063	0.061	0.046	0.054	0.072
<i>p</i> − <i>value</i>	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i>)	0.084	0.11	0.096	0.092	0.075	0.092	0.099
<i>p</i> − <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	0.035	0.034	0.033	0.031	0.029	0.038	0.027
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N	7046904	8705784	16392963	15969777	5665143	8592046	8759206
r2	0.33	0.35	0.32	0.32	0.35	0.31	0.36

Table 4: The table re-estimates the specification in column (4) of Table 3 (including holiday, day-of-week, month-year-county and month-year-lender fixed effects, as well as loan-level controls) on subsamples of the HMDA dataset based on loan characteristics. All results are based on individual loan-level regression estimates of abnormal origination rates in the last and first week of the month (see equation 5). The dependent variable is a dummy that takes value 1 if a loan application is originated and 0 if it is denied. In columns (1) and (2), the sample is restricted, respectively, to new purchase loans and refinancings. In columns (3), (4), and (5), the sample is restricted, respectively, to conforming and conventional loans, and conventional loans for new house purchases. Finally, in columns (6) and (7), we restrict the sample, respectively, to applicants with income above and below the median in their county. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) Subprime (FICO < 660)	(2) Origination LTV (%)	(3) Low Documentation	(4) Origination Interest Rate
<i>lastweek</i>	0.00029 (0.00079)	0.0062 (0.070)	0.042* (0.023)	0.00096 (0.0011)
<i>firstweek</i>	0.00059 (0.00042)	-0.15*** (0.028)	-0.0012 (0.0012)	0.00016 (0.00094)
<i>black</i>	0.12*** (0.0066)	3.09*** (0.24)	0.0014 (0.0034)	0.0013 (0.0019)
<i>black</i> × <i>lastweek</i>	0.0020 (0.0014)	-0.32*** (0.081)	-0.018** (0.0068)	-0.00076 (0.00068)
<i>black</i> × <i>firstweek</i>	0.0031* (0.0016)	0.098* (0.054)	-0.00042 (0.0011)	0.00022 (0.00071)
LTV	0.0016*** (0.00035)		0.00032 (0.00055)	0.00043 (0.00044)
FICO 620:659		-1.59*** (0.46)	0.019 (0.025)	-0.00015 (0.010)
FICO 660:719		-2.83*** (0.56)	0.043 (0.040)	-0.020* (0.011)
FICO 720:759		-3.99*** (0.71)	0.051 (0.051)	-0.036*** (0.011)
FICO 760:799		-7.34*** (0.74)	0.052 (0.054)	-0.051*** (0.0099)
FICO ≥ 800		-10.6*** (0.67)	0.060 (0.052)	-0.065*** (0.0081)
Standard Loan-Level Controls	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES
<i>last</i> – <i>first</i>	-0.0003	0.1500	0.0430	0.0008
<i>p</i> – <i>value</i>	0.7600	0.0830	0.0690	0.6800
<i>last</i> – <i>first</i> (<i>black</i>)	-0.0014	-0.2600	0.0250	-0.0002
<i>p</i> – <i>value</i> (<i>black</i>)	0.6100	0.0077	0.1500	0.9100
<i>last</i> – <i>first</i> (<i>black</i> – <i>other</i>)	-0.0011	-0.4200	-0.0180	-0.0010
<i>p</i> – <i>value</i> (<i>black</i> – <i>other</i>)	0.6600	0.0005	0.0130	0.3700
N	27701585	27701585	18125835	24187005
r2	0.26	0.51	0.51	0.78

Table 5: The table reports regression estimates of the difference in characteristics between mortgages originated in the last and first week of the month. The dependent variables are a dummy equal to one for subprime loans (with FICO < 660, column 1), the mortgage LTV at origination (column 2), a dummy equal to one for mortgages for which the applicant provided low documentation (column 3), and the mortgage interest rate at origination (column 4). The *Standard Loan-Level Controls* are the controls for application characteristics used in column (4) of Table 3. *lastweek* and *firstweek* are dummies equal to one in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with their *p-values*. Standard errors are clustered by lender and year. Estimates are based on the merged sample of HMDA and Black Knight McDash data from 1994 to 2018.

	(1) approval	(2) approval	(3) approval	(4) approval	(5) approval
<i>black</i>	-0.080*** (0.0049)	-0.022*** (0.0021)	-0.042*** (0.0054)	-0.017*** (0.0018)	-0.016*** (0.0017)
<i>lastweek</i>	0.028*** (0.0019)	0.023*** (0.0016)	0.023*** (0.0017)	0.021*** (0.0016)	0.021*** (0.0016)
<i>firstweek</i>	-0.018*** (0.0018)	-0.015*** (0.0016)	-0.016*** (0.0021)	-0.014*** (0.0021)	-0.014*** (0.0021)
<i>black</i> × <i>lastweek</i>	0.027*** (0.0027)	0.021*** (0.0024)	0.020*** (0.0024)	0.019*** (0.0023)	0.018*** (0.0022)
<i>black</i> × <i>firstweek</i>	-0.018*** (0.0028)	-0.013*** (0.0024)	-0.015*** (0.0025)	-0.013*** (0.0026)	-0.012*** (0.0025)
<i>AUS approved</i>			0.41*** (0.022)	0.32*** (0.025)	0.36*** (0.019)
Standard Loan-Level Controls	YES	YES	YES	YES	YES
New Loan-Level Controls (2018-2019 HMDA)	NO	YES	NO	YES	YES
AUS Type Controls	NO	NO	NO	NO	YES
Holiday FE	YES	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES	YES
<i>last</i> − <i>first</i>	0.045	0.038	0.039	0.035	0.035
<i>p</i> − <i>value</i>	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i>)	0.090	0.073	0.074	0.066	0.066
<i>p</i> − <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	0.045	0.035	0.035	0.031	0.031
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
N	3544630	3544630	2819531	2537705	2537705
r2	0.25	0.38	0.30	0.36	0.36

Table 6: The table reports individual loan-level regression estimates of abnormal approval rates in the last and first week of the month (see equation 5), based on the new extended HMDA data (which contain more detailed information on applicants and loan characteristics) available for 2018 and 2019. *Standard Loan-Level Controls* stands for the set of controls used in column (4) of Table 3, which are also available in the HMDA sample covering the years from 1994 to 2018. *New Loan-Level Controls* stands for the new controls available for 2018 and 2019. In particular, we include dummies for quintiles of the applicants’ debt-to-income ratios, FICO scores, and loan-to-value ratios. In columns (3), (4), and (5) we include a dummy equal to one when we observe that the Automated Underwriting System (AUS) used by the lender recommended approval of the loan. *AUS Type Controls* is a set of dummies selecting the different types of AUS models used by lenders. The dependent variable for all regressions is a dummy that takes value 1 if a loan application is originated and 0 if it is denied. *lastweek* and *firstweek* are dummies equal to one, respectively, if the action on the application is taken in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 20% random sample of the new HMDA data for 2018 and 2019.

	(1) 5-Year Delinquency	(2) 5-Year Delinquency FICO < 660	(3) 5-Year Delinquency LTV > 80%	(4) 5-Year Delinquency Low Docs
<i>black</i>	0.045*** (0.0044)	0.055*** (0.0038)	0.051*** (0.0029)	0.048*** (0.0067)
<i>lastweek</i>	0.00012 (0.00074)	0.0022* (0.0013)	-0.000075 (0.00086)	-0.00034 (0.0012)
<i>firstweek</i>	0.00098 (0.00074)	0.0022 (0.0015)	0.0022* (0.0010)	0.0014 (0.00099)
<i>black</i> × <i>lastweek</i>	0.0015 (0.0019)	0.0039* (0.0021)	0.00037 (0.0020)	-0.0072 (0.0052)
<i>black</i> × <i>firstweek</i>	0.0052** (0.0018)	0.0047 (0.0033)	0.0047* (0.0023)	0.0078** (0.0030)
All Loan-Level Controls	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES
<i>last</i> − <i>first</i>	-0.00086	0.00001	-0.0023	-0.0018
<i>p</i> − <i>value</i>	0.53	1.00	0.22	0.30
<i>last</i> − <i>first</i> (<i>black</i>)	-0.0046	-0.00077	-0.0066	-0.017
<i>p</i> − <i>value</i> (<i>black</i>)	0.25	0.89	0.068	0.029
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	-0.0037	-0.00078	-0.0044	-0.015
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.22	0.84	0.15	0.038
N	18797106	3065611	5870489	5278008
r2	0.26	0.25	0.26	0.34

Table 7: The table reports regression estimates of the difference in performance between mortgages originated in the last and first week of the month. The dependent variable is a dummy equal to one for mortgages for which we observe a 90-days delinquency within 5 years after origination. In column (2), the sample is restricted to subprime loans (FICO < 660). In column (3) the sample is restricted to high loan-to-value loans (LTV > 80%), and in column (4) to loans with low documentation. *All Loan-Level Controls* stands for the set of controls for application characteristics used in column (4) of Table 3, augmented with FICO bins and LTV (see Table 5). *lastweek* and *firstweek* are dummies equal to one in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with their *p-values*. Standard errors are clustered by lender and year. Estimates are based on the merged sample of HMDA and Black Knight McDash data from 1994 to 2018.

	(1) approval	(2) approval 1-30 Days TTA	(3) approval 31-60 Days TTA	(4) approval 61-90 Days TTA	(5) approval > 90 Days TTA
<i>lastweek</i>	0.038*** (0.0026)	0.053*** (0.0033)	0.027*** (0.0031)	0.027*** (0.0030)	0.023*** (0.0034)
<i>firstweek</i>	-0.019*** (0.0018)	-0.016*** (0.0017)	-0.017*** (0.0017)	-0.019*** (0.0027)	-0.017*** (0.0027)
<i>black</i>	-0.056*** (0.0039)	-0.072*** (0.0050)	-0.043*** (0.0032)	-0.037*** (0.0035)	-0.030*** (0.0032)
<i>black</i> × <i>lastweek</i>	0.023*** (0.0020)	0.023*** (0.0025)	0.021*** (0.0020)	0.015*** (0.0023)	0.015*** (0.0037)
<i>black</i> × <i>firstweek</i>	-0.0094*** (0.0017)	-0.0043** (0.0019)	-0.015*** (0.0027)	-0.0053 (0.0033)	-0.0056 (0.0036)
log(TTA)	0.11*** (0.0069)				
Loan-Level Controls	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES	YES
<i>last</i> – <i>first</i>	0.057	0.069	0.044	0.046	0.040
<i>p</i> – <i>value</i>	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> – <i>first</i> (<i>black</i>)	0.089	0.096	0.080	0.065	0.061
<i>p</i> – <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> – <i>first</i> (<i>black</i> – <i>other</i>)	0.032	0.027	0.036	0.020	0.021
<i>p</i> – <i>value</i> (<i>black</i> – <i>other</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
N	16503563	9038519	4907587	1420393	1332868
r2	0.35	0.40	0.29	0.37	0.42

Table 8: The table reports individual loan-level regression estimates of abnormal approval rates in the last and first week of the month (see equation 5), controlling for time to action (TTA), defined as the number of days between the application date and the date in which action (origination or denial) is taken on the loan. In column (1), the log of TTA is included as a control. In columns (2) to (5), the sample is restricted to loans with TTA, respectively, between 1 and 30 days, between 31 and 60 days, between 61 and 90 days, and longer than 91 days. The dependent variable is a dummy that takes value 1 if a loan application is approved and 0 if it is denied. *lastweek* and *firstweek* are dummies equal to one, respectively, if action on the application is taken in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) Time-To-Orig.	(2) Time-To-Orig.	(3) Time-To-Den.
<i>lastweek</i>	-0.49** (0.15)	-0.51*** (0.15)	0.73*** (0.12)
<i>firstweek</i>	-0.55*** (0.13)	-0.54*** (0.13)	0.73*** (0.16)
<i>black</i>		3.08*** (0.39)	-0.85*** (0.16)
<i>black</i> × <i>lastweek</i>		0.26* (0.13)	0.13 (0.14)
<i>black</i> × <i>firstweek</i>		-0.26** (0.11)	-0.065 (0.16)
Loan Level Controls	YES	YES	YES
Holiday FE	YES	YES	YES
Day-of-Week FE	YES	YES	YES
Month-Year-County	YES	YES	YES
Month-Year-Lender	YES	YES	YES
<i>last</i> − <i>first</i>	0.059	0.024	-0.0050
<i>p</i> − <i>value</i>	0.79	0.91	0.98
<i>last</i> − <i>first</i> (<i>black</i>)		0.54	0.19
<i>p</i> − <i>value</i> (<i>black</i>)		0.028	0.28
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)		0.52	0.19
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)		0.0031	0.24
N	11924971	11924971	3915189
r ²	0.29	0.29	0.40

Table 9: The table reports individual loan-level regression estimates of abnormal mortgage processing time in the last and first week of the month. Our estimates are based on the regression specification introduced in equation (5), but with dependent variable equal to time to origination, or time to denial. In columns (1) and (2), the sample is restricted to originated loans, and the dependent variable is the time to origination, defined as the number of days between the application date and the origination date. In column (3), the sample is restricted to denied applications, and the dependent variable is the time to denial. *lastweek* and *firstweek* are dummies equal to one, respectively, if action on the application is taken in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) approval $Z = I_{Fintech}$	(2) approval $Z = I_{ShadowBank}$	(3) approval $Z = I_{HighTop4}$	(4) approval $Z = I_{HighHHI}$	(5) approval $Z = I_{LargeBank}$
<i>black</i>	-0.093*** (0.0043)	-0.11*** (0.0038)	-0.063*** (0.0046)	-0.063*** (0.0046)	-0.063*** (0.0046)
<i>lastweek</i>	0.035*** (0.0031)	0.054*** (0.0081)	0.045*** (0.0030)	0.045*** (0.0030)	0.045*** (0.0030)
<i>firstweek</i>	-0.020*** (0.0034)	-0.024*** (0.0037)	-0.021*** (0.0019)	-0.021*** (0.0019)	-0.021*** (0.0019)
<i>black</i> × <i>lastweek</i>	0.027*** (0.0034)	0.031*** (0.0035)	0.026*** (0.0022)	0.026*** (0.0023)	0.026*** (0.0022)
<i>black</i> × <i>firstweek</i>	-0.0074*** (0.0024)	-0.0087*** (0.0024)	-0.0079*** (0.0015)	-0.0074*** (0.0014)	-0.0079*** (0.0015)
<i>black</i> × <i>Z</i>	-0.013 (0.0089)	0.050*** (0.0076)	-0.011*** (0.0033)	-0.012*** (0.0033)	-0.011*** (0.0033)
<i>lastweek</i> × <i>Z</i>	0.029*** (0.010)	-0.020** (0.0086)	-0.0035*** (0.0010)	-0.0040*** (0.0010)	-0.0035*** (0.0010)
<i>firstweek</i> × <i>Z</i>	-0.0069 (0.0046)	0.0075* (0.0042)	0.0034*** (0.00088)	0.0036*** (0.00084)	0.0034*** (0.00088)
<i>black</i> × <i>lastweek</i> × <i>Z</i>	0.0072 (0.0049)	-0.0095** (0.0046)	0.0014 (0.0020)	0.00063 (0.0019)	0.0014 (0.0020)
<i>black</i> × <i>firstweek</i> × <i>Z</i>	0.00063 (0.0033)	0.0083* (0.0049)	0.0027 (0.0020)	0.0016 (0.0022)	0.0027 (0.0020)
Loan-Level Controls	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES	YES
<i>last</i> − <i>first</i> (<i>black</i>)	0.089	0.12	0.100	0.10	0.100
<i>p</i> − <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> , <i>Z</i>)	0.096	0.10	0.099	0.099	0.099
<i>p</i> − <i>value</i> (<i>black</i> , <i>Z</i>)	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> , <i>Z</i> − <i>noZ</i>)	0.0066	-0.018	-0.0013	-0.0010	-0.0013
<i>p</i> − <i>value</i> (<i>black</i> , <i>Z</i> − <i>noZ</i>)	0.15	0.022	0.63	0.71	0.63
N	4159023	4354784	17898971	17898971	17898971
r2	0.26	0.26	0.32	0.32	0.32

Table 10: The table reports individual loan-level regression estimates of abnormal approval rates for Black applicants in the last and first week of the month, interacted with lender and local market characteristics (see equation 7). *lastweek* and *firstweek* are dummies equal to one, respectively, in the first and last week of the month. *black* is a dummy equal to one for Black applicants. $I_{Fintech}$ is a dummy equal to one for loan applications submitted to Fintech lenders. $I_{ShadowBank}$ is a dummy equal to one for loan applications submitted to shadow banks. $I_{HighTop4}$ is a dummy equal to one in counties where the share of the top 4 originators is above median. $I_{HighHHI}$ is a dummy equal to one in counties where the HHI index based on lenders origination shares is above median. $I_{LargeBank}$ is a dummy equal to one for lenders with size above median. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, and for Black applicants and lender (or market) groups dummies, along with their *p-values*. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 2014 to 2018 in columns (1) and (2), and a 5% random sample of the HMDA data from 1994 to 2018 in columns (3), (4), and (5).

Online Appendix:

**Using High-Frequency Evaluations to Estimate Discrimination:
Evidence from Mortgage Loan Officers**

(intended for online publication)

A.I Identifying Time-Varying Discrimination

We show how under the assumptions in **Assumption Set (A)**, favorable decision probabilities for whites and Blacks are different. The probability, conditional on race and other observable characteristics, is equal to:

$$\begin{aligned} P(Y|R, X) &= \frac{P(Y,R|X)}{P(R|X)} = \frac{P(Y,R,Z_H|X)+P(Y,R,Z_L|X)}{P(R|X)} = \frac{P(Y|R,Z_H,X)P(Z_H,R|X)+P(Y|R,Z_L,X)P(Z_L,R|X)}{P(R|X)} \\ &= P(Y|R, Z_H, X)P(Z_H|R, X) + P(Y|R, Z_L, X)P(Z_L|R, X) \end{aligned}$$

where $Z \in \{Z_L, Z_H\}$ is a binary unobservable characteristic, X is a vector of observable characteristics and $R \in \{W, B\}$ is the applicants' race (white or Black). Then, the difference in favorable decision probabilities for whites and Blacks is equal to:

$$\begin{aligned} P(Y|W, X) - P(Y|B, X) &= \\ &= [P(Y|W, Z_H, X)P(Z_H|W, X) + P(Y|W, Z_L, X)P(Z_L|W, X)] - [P(Y|B, Z_H, X)P(Z_H|B, X) + P(Y|B, Z_L, X)P(Z_L|B, X)] \\ &= P(Y|Z_H, X)[P(Z_H|W, X) - P(Z_H|B, X)] + P(Y|Z_L, X)[P(Z_L|W, X) - P(Z_L|B, X)] \\ &= [P(Y|Z_H, X) - P(Y|Z_L, X)][P(Z_H|W, X) - P(Z_H|B, X)] > 0 \end{aligned}$$

where we use $P(Y|Z_H, X) = P(Y|W, Z_H, X) = P(Y|B, Z_H, X)$ and $P(Y|Z_L, X) = P(Y|W, Z_L, X) = P(Y|B, Z_L, X)$ from the assumption of no discrimination, and $P(Z_H|R, X) = 1 - P(Z_L|R, X)$. Then, $P(Z_H|W, X) - P(Z_H|B, X) > 0$ from the assumption of higher unobservable quality characteristics for whites, and $P(Y|Z_H, X) - P(Y|Z_L, X) > 0$.

We now turn to the comparison of the favorable decision probabilities for whites and Blacks at the beginning and the end of the month, under **Assumption Set (B)**. The difference in the probability between whites and Blacks is equal to:

$$\begin{aligned}
& P(Y|W, X, T) - P(Y|B, X, T) = \\
& = P(Y|W, Z_H, X, T)P(Z_H|W, X, T) + P(Y|W, Z_L, X, T)P(Z_L|W, X, T) - P(Y|B, Z_H, X, T)P(Z_H|B, X, T) - P(Y|B, Z_L, X, T)P(Z_L|B, X, T) \\
& = P(Y|W, Z_H, X, T)[P(Z_H|W, X, T) - P(Z_H|B, X, T)] + P(Y|W, Z_L, X, T)[P(Z_L|W, X, T) - P(Z_L|B, X, T)] \\
& = P(Y|Z_H, X, T)[P(Z_H|W, X, T) - P(Z_H|B, X, T)] + P(Y|Z_L, X, T)[P(Z_L|W, X, T) - P(Z_L|B, X, T)]
\end{aligned}$$

where $T \in \{Start, End\}$, and we use the assumption of no discrimination: $P(Y|Z, W, X, T) = P(Y|Z, B, X, T) = P(Y|Z, X, T)$. Exploiting the calculations above, we can then derive the properties of the change in the difference between favorable decision probabilities at the beginning and the end of the month:

$$\begin{aligned}
& [P(Y|W, X, End) - P(Y|B, X, End)] - [P(Y|W, X, Start) - P(Y|B, X, Start)] = \\
& = P(Y|Z_H, X, End)[P(Z_H|W, X, End) - P(Z_H|B, X, End)] + P(Y|Z_L, X, End)[P(Z_L|W, X, End) - P(Z_L|B, X, End)] \\
& \quad - P(Y|Z_H, X, Start)[P(Z_H|W, X, Start) - P(Z_H|B, X, Start)] - P(Y|Z_L, X, Start)[P(Z_L|W, X, Start) - P(Z_L|B, X, Start)] \\
& = [P(Y|Z_H, X, End) - P(Y|Z_L, X, End)][P(Z_H|W, X) - P(Z_H|B, X)] \\
& \quad - [P(Y|Z_H, X, Start) - P(Y|Z_L, X, Start)][P(Z_H|W, X) - P(Z_H|B, X)] = 0
\end{aligned}$$

where we use $P(Z_H|R, X, Start) = P(Z_H|R, X, End) = P(Z_H|R, X)$, and $P(Z_L|R, X, Start) = P(Z_L|R, X, End) = P(Z_L|R, X)$, based on the assumption that applications quality does not change over the month, and $P(Z_H|R, X) = 1 - P(Z_L|R, X)$. The condition is equal to zero since $P(Y|Z_H, X, End) - P(Y|Z_L, X, End) = \lambda$ and $P(Y|Z_H, X, Start) - P(Y|Z_L, X, Start) = \lambda$, due to the stable decision criteria assumption. Thus, the rejection of the null that the empirical counterpart of the condition above is equal to zero in the data, leads to a rejection of **Assumption Set (B)**.

A.II Distinguishing Taste-Based from Statistical Discrimination

This section explores the extent to which our estimation approach can distinguish between the two broad categories of discrimination. Under “taste-based” discrimination, minorities are subject to disparate treatment because evaluators have animus toward them. Under “statistical” discrimination, evaluators are uncertain about the abilities of any given subject. Evaluators form their beliefs after observing the subject’s race. Minorities are subject to disparate treatment when evaluators have developed beliefs that minority subjects have worse abilities. In the case of statistical discrimination, evaluators do not need to have accurate beliefs about minorities to apply disparate treatment (see e.g., Bohren et al., 2020).

We consider evaluators who, over a short time-period, for example a month or a week, evaluate subjects i . Each evaluator j has perceived net benefits from making decisions that favor subject i equal to $U^j(X_i, Z_i, R_i, t)$, where X_i and Z_i are vectors of observable and unobservable (from the perspective of the researcher) characteristics, R_i is the subjects’ race (e.g., $R_i = W$ for a white applicant and $R_i = B$ for a Black applicant), and t is the point in time in which the evaluation is conducted.

The evaluator’s net benefits can be decomposed into two components:

$$U^j(X_i, Z_i, R_i, t) = b^j(X_i, Z_i, R_i, t) + E_j[u_i|X_i, Z_i, R_i, t], \quad (\text{A.1})$$

where $b^j(X_i, Z_i, r_i, t)$ is the subjective net benefit of evaluator j conditional on all characteristics, and $E_j[u_i|V_i, Z_i, R_i, t]$ is the statistical component. The statistical component can be written as

$$E_j[u_i|X_i, Z_i, R_i, t] = E[u_i|X_i, Z_i, R_i, t] + \tau_j(R_i, t), \quad (\text{A.2})$$

where $\tau_j(\cdot)$ is the bias of decision maker j when forming expectations conditional only on the information about the race of an applicant.

We can then use our stylized framework to characterize different types of discrimination, for example against Black subjects with respect to white subjects:

- Taste-based discrimination: $b^j(X_i, Z_i, W, t) > b^j(X_i, Z_i, B, t)$

- Statistical discrimination: $\tau_j(W, t) > \tau_j(B, t)$

The decision maker will take a decision favorable to subject i as long as the net benefit is positive:

$$b^j(X_i, Z_i, R_i, t) + E_j[u_i|X_i, Z_i, R_i, t] + v_{i,j,t} > 0,$$

where $v_{i,j,t}$ is a random preference shock, i.i.d. across subjects and evaluators, and independent of information on subject characteristics and evaluators' beliefs. We can then introduce the variable $y_{i,j,t}$, which is equal to one if subject i receives a favorable decision from evaluator j at time t , and has likelihood function:

$$\begin{aligned} \mathcal{L}(y_{i,j,t}) &= Pr(y_{i,j,t} = 1)^{I(y_{i,j,t}=1)} [1 - Pr(y_{i,j,t} = 1)]^{1-I(y_{i,j,t}=1)} \\ Pr(y_{i,j,t} = 1) &= E[y_{i,j,t}|X_i, Z_i, R_i, j, t] = F(X_i, Z_i, R_i, j, t). \end{aligned}$$

If we assume the function $F(X_i, Z_i, R_i, j, t)$ can be approximated with a linear specification, then we can write:

$$y_{i,j,t} = \beta_1 r_i + \beta_2 (r_i \times t) + \eta X_i + \phi Z_i + a_t + \epsilon_{i,j,t} \quad (\text{A.3})$$

where $r_i = 1$ if $R_i = B$. Equation (A.3) can be estimated in the data. Within this specific framework, we can state the predictions from the previous section along the following lines:

1. The two types of discrimination listed above (driven by taste or statistical) would cause estimates of $\beta_1 < 0$. However, as the previous section outlines, β_1 will be a biased estimate unless the researcher fully controls for observable (X_i) and unobservable (Z_i) characteristics, or r_i is uncorrelated with any omitted characteristics.
2. Estimates of β_2 will be different from zero if the magnitude of discrimination changes over time, regardless of the type of discrimination. If subject pool characteristics (X_i and Z_i) are not correlated with the evaluation time t , estimates of β_2 will be unbiased even if the researcher does not perfectly control for time-invariant characteristics.

How can this approach distinguish between different theories of discrimination? In principle, any type of discrimination can be subject to high-frequency fluctuations, and thus produce non-zero estimates of β_2 . However, if the unobserved variation across subject pools and the evaluator's statistical inference problem are time-invariant, our approach allows the researcher to attribute discrimination to the source of time-variation in the evaluator's decision-making.

Consider the case of statistical discrimination. Statistical discrimination is caused by the evaluators' statistical inference problem. Therefore, the researcher can reasonably assume the findings are caused by statistical discrimination if she can provide evidence of time-variation in the evaluators' information set. Now consider taste-based discrimination. The evaluator's subjective preferences against minorities causes disparate treatment. The researcher can assume taste-based discrimination if she has evidence that evaluators' subjectivity is time-varying.

Our empirical analysis focuses on residential mortgage lending in the U.S. Our source of time-variation in evaluations is the fact that loan officers have monthly volume quotas. These monthly volume quotas generate within-month variation in loan officers' subjectivity. At the same time, loan officers observe the same information about applications that they process at the start of the month relative to the end of the month. As such, discrimination due to within-month differences in evaluations is more likely to be driven by loan officers' subjective preferences, rather than inference problems.

A.III Additional Figures and Tables

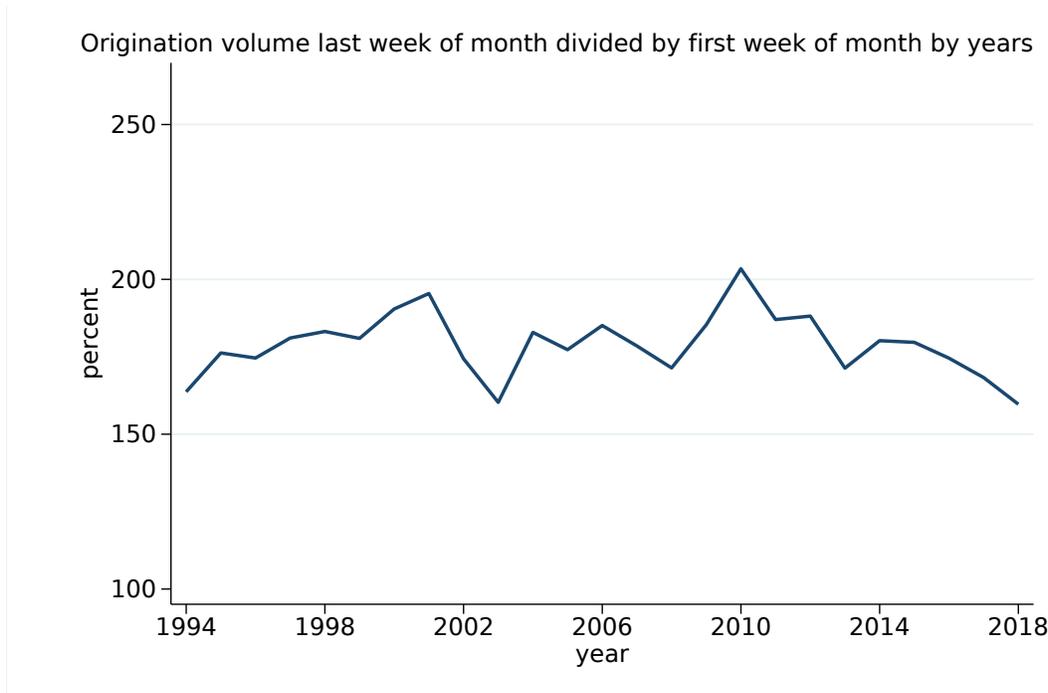


Figure A.1: The figure shows the ratio of average mortgage origination volume in the last week of the month over average mortgage origination volume in the first week of the month, for each year over the period from 1994 to 2018. The evidence is based on the HMDA data from January 1994 to December 2018.

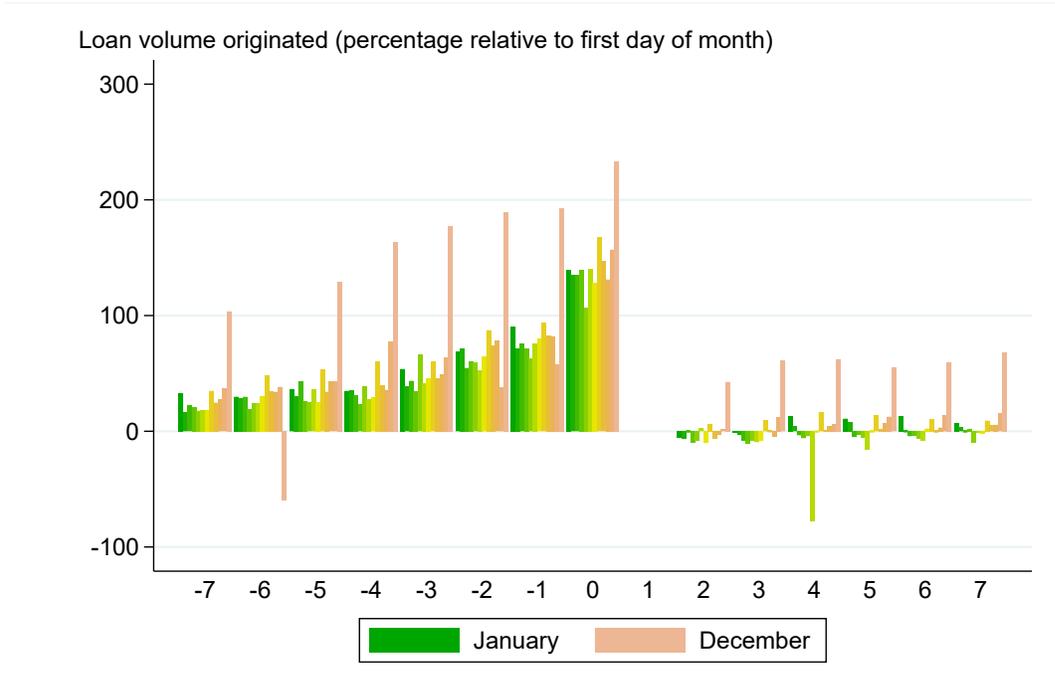


Figure A.2: The figure shows average percentage abnormal daily loan origination volume (measured as number of originations) in the U.S., for the last eight days of the month, and the first seven days of the following month, separately for each calendar month (January to December) over the sample period from January 1994 to December 2018. Abnormal volume is computed with respect to loan origination volume on the first day of the following month. The evidence is based on the HMDA data from January 1994 to December 2018.

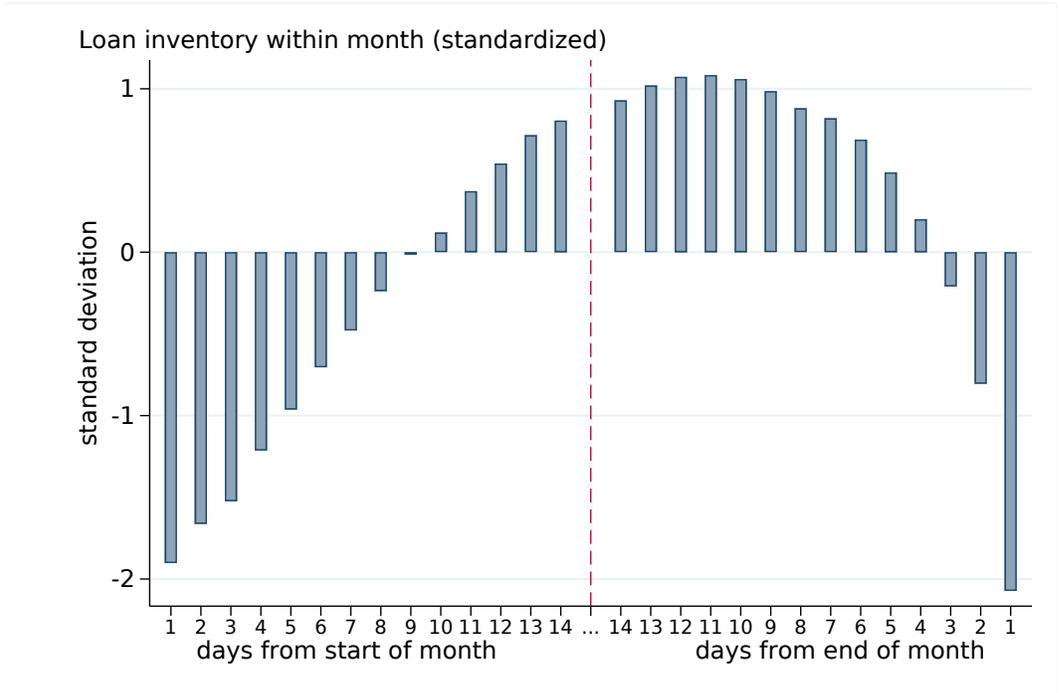
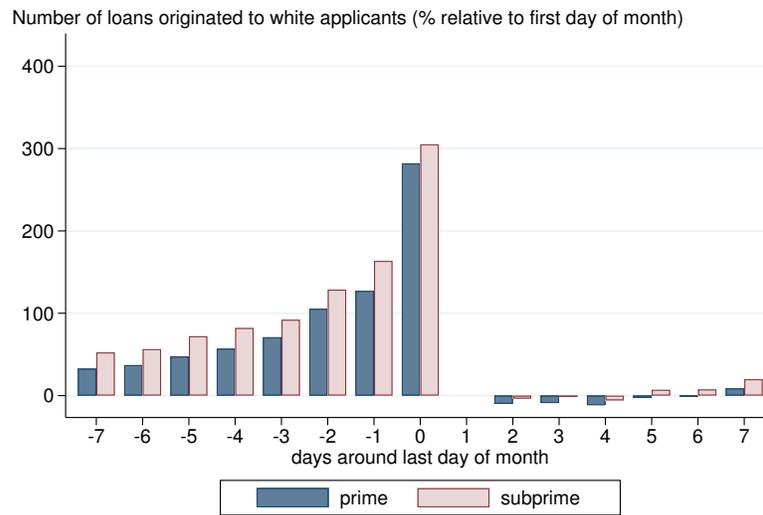
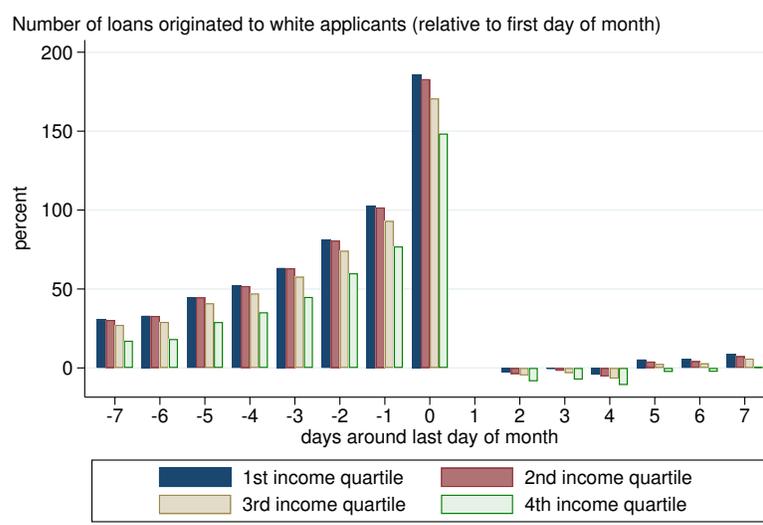


Figure A.3: This figure shows the within-month fluctuation, at the level of the entire United States, in the average number of loan applications in inventory (awaiting action) by day of the month. Inventory size is standardized to have mean of zero and standard deviation of one. The evidence is based on the HMDA data from January 1994 to December 2018.



(a) Effects for Prime and Subprime White Applicants



(b) Effects by Income Quartile White Applicants

Figure A.4: Panel (a) of the figure shows average percentage abnormal daily loan origination volume in the U.S., for whites with prime (660 or higher) and subprime FICO. Abnormal volume is reported for the last eight days of the month, and the first seven days of the following month, and is computed with respect to loan origination volume on the first day of the following month for each applicant group. Estimates are based on the merged sample of HMDA and Black Knight McDash data from 1994 to 2018. Panel (b) of the figure shows average percentage abnormal daily loan origination volume separately for whites belonging to different income quartiles within county. The results in this panel are based on the HMDA sample from January 1994 to December 2018.

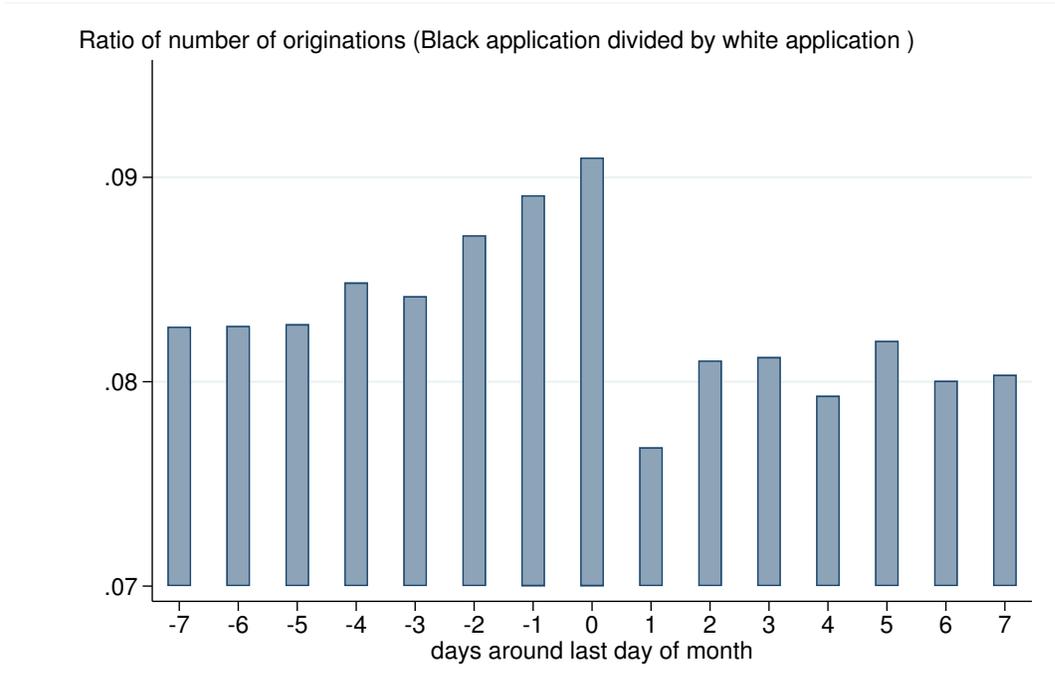


Figure A.5: This figure shows the ratio of the number of originations to Black applicants, over originations to white applicants, for each day in the last and first week of the month, at the level of the entire U.S. The evidence is based on the HMDA data from January 1994 to December 2018.

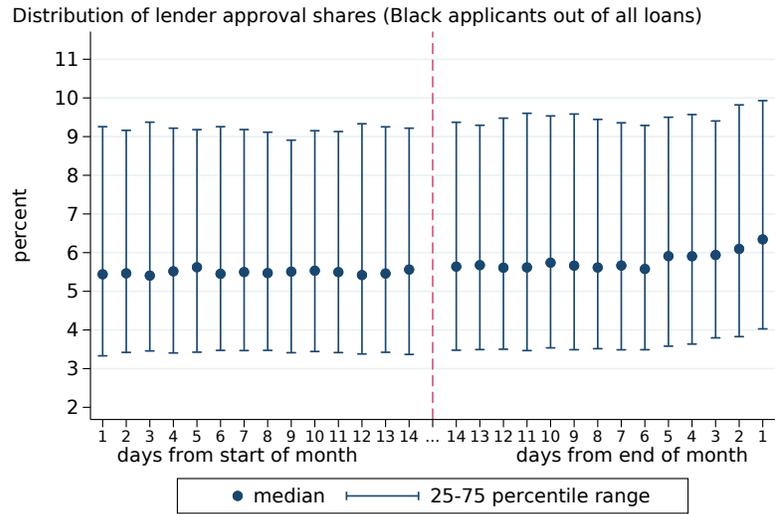
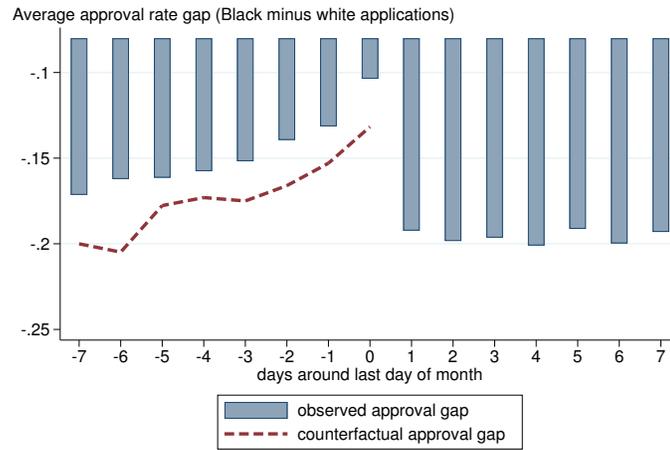
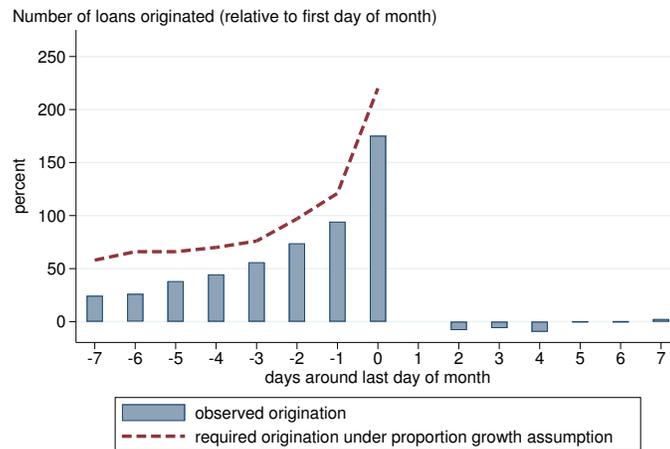


Figure A.6: The figure shows the distribution (median, 25th percentile and 75th percentile) of the share of loans originated to Blacks, out of all originations, at the lender level, and on each day in the first and last two weeks of the month. The sample contains all lenders that originate on average at least 10 loans per-day, and is constructed using the HMDA data from January 1994 to December 2018.



(a) Approval Gap



(b) Origination Volume

Figure A.7: Panel (a) shows the difference between the fraction of approved loans for Blacks and whites, at the U.S.-level, and on each of the last eight days of the month and the first seven days of the following month. In the last week of the month, we overlay to the graph a dashed line, showing the change in the approval gap generated by an increase in origination volume, matching the one we observe in the data, but with a constant share of loans originated to Black applicants on each day (equal to the unconditional average share across all days). Panel (b) shows average percentage abnormal daily loan origination volume in the U.S., for the last eight days of the month, and the first seven days of the following month. Along similar lines as in Panel (a), we include for the last week of the month a dashed line, showing the increase in origination volume that would be needed to match the corresponding approval gap on each day in the data, but under the restriction of a constant share of loans originated to Black applicants on each day. The evidence is based on the HMDA data from January 1994 to December 2018.

	(1) approval	(2) approval	(3) approval	(4) approval	(5) approval	(6) approval
<i>black</i>		-0.049*** (0.0036)	-0.067*** (0.0043)	-0.068*** (0.0044)	-0.064*** (0.0043)	-0.067*** (0.0042)
<i>lastweek</i>	0.043*** (0.0030)	0.043*** (0.0027)	0.043*** (0.0030)	0.043*** (0.0031)		0.071*** (0.012)
<i>firstweek</i>	-0.020*** (0.0017)	-0.019*** (0.0017)	-0.020*** (0.0017)	-0.019*** (0.0017)		-0.013** (0.0050)
<i>black</i> × <i>lastweek</i>	0.027*** (0.0021)	0.027*** (0.0018)	0.027*** (0.0021)	0.028*** (0.0021)		0.024*** (0.0020)
<i>black</i> × <i>firstweek</i>	-0.0074*** (0.0015)	-0.0086*** (0.0013)	-0.0073*** (0.0015)	-0.0072*** (0.0015)		-0.0067*** (0.0015)
<i>female</i>			-0.017*** (0.0025)			
<i>lastday</i>					0.081*** (0.0068)	
<i>firstday</i>					-0.019*** (0.0032)	
<i>black</i> × <i>lastday</i>					0.046*** (0.0039)	
<i>black</i> × <i>firstday</i>					-0.0069** (0.0033)	
<i>black</i> -Year	YES	NO	NO	NO	NO	NO
Loan-Level Controls	YES	YES	YES	YES	YES	NO
Loan-Level Controls-Inter	NO	NO	NO	NO	NO	YES
Holiday FE	YES	YES	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES	YES	YES
Lender FE	YES	YES	YES	YES	YES	YES
Month-Year-County	YES	NO	YES	YES	YES	YES
Month-Year-Census Tract	NO	YES	NO	NO	NO	NO
Month-Year-Lender	YES	YES	YES	YES	YES	YES
<i>last</i> − <i>first</i>	0.063	0.062	0.063	0.062	0.10	0.084
<i>p</i> − <i>value</i>	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i>)	0.097	0.098	0.097	0.097	0.15	0.12
<i>p</i> − <i>value</i> (<i>black</i>)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	0.034	0.036	0.034	0.035	0.053	0.031
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
N	17898971	12725901	17898971	17893870	17893870	17898971
r2	0.32	0.54	0.32	0.32	0.32	0.32

Table A.1: The table reports several robustness checks for the main results in Table 3, based on individual loan-level regression estimates of abnormal origination rates in the last and first week of the month (see equation 5). The dependent variable is a dummy that takes value 1 if a loan application is originated and 0 if it is denied. Across all columns, the regression specifications include all the controls used in column (4) of Table 3. In column (1), we interact the *black* dummy with dummies for each year in our sample. In column (2), we replace the county dummies with census tract dummies and interact them with time controls (year-month). In column (3), we include an additional control for applicant gender. In column (4), we change the definition of month in the time fixed effect, so that each month begins on its 15th calendar day, and ends on the 14th calendar day of the following month. In column (5), we maintain the modified definition of months for the fixed effects, and we focus on changes in the black-applicants gap between the last and first day of the month. Thus, *lastday* and *firstday* are dummies equal to one if the action on the application is taken, respectively, on the first and last day of the month. Finally, in column (6), we interact loan-level controls with the first and last week of the month dummies. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek* (*lastday* and *firstday*), and for the difference of the interaction coefficients for Black applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) approval All	(2) approval All	(3) approval All	(4) approval All
<i>lastweek</i>	0.053*** (0.0036)	0.044*** (0.0034)	0.042*** (0.0030)	0.041*** (0.0030)
<i>firstweek</i>	-0.021*** (0.0025)	-0.020*** (0.0020)	-0.019*** (0.0017)	-0.019*** (0.0016)
<i>female</i>	-0.082*** (0.011)	-0.050*** (0.0085)	-0.024*** (0.0030)	-0.021*** (0.0026)
<i>female</i> × <i>lastweek</i>	0.017*** (0.0023)	0.016*** (0.0021)	0.011*** (0.0016)	0.011*** (0.0016)
<i>female</i> × <i>firstweek</i>	-0.0043*** (0.00091)	-0.0034*** (0.00084)	-0.0031*** (0.00087)	-0.0032*** (0.00076)
Loan-Level Controls	NO	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year FE	YES	YES	YES	NO
County FE	YES	YES	YES	NO
Lender FE	NO	NO	YES	NO
Month-Year-County	NO	NO	NO	YES
Month-Year-Lender	NO	NO	NO	YES
<i>last</i> − <i>first</i>	0.074	0.064	0.061	0.060
<i>p</i> − <i>value</i>	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>female</i>)	0.096	0.084	0.075	0.074
<i>p</i> − <i>value</i> (<i>female</i>)	0.0000	0.0000	0.0000	0.0000
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	0.022	0.020	0.015	0.014
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0000	0.0000	0.0000	0.0000
N	19641147	18464529	18463277	17898971
r2	0.044	0.092	0.23	0.32

Table A.2: The table repeats the analysis in Table 3, but the *black* indicator variable is replaced with a *female* indicator variable equal to one for female applicants. All regressions are based on individual loan-level regression estimates of abnormal origination rates in the last and first week of the month (see equation 5). The dependent variable is a dummy that takes value 1 if a loan application is originated and 0 if it is denied. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for female applicants, along with the *p-value* of tests of the null that each difference is equal to 0. Standard errors are clustered by lender and year. Estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.

	(1) 5-Year Default	(2) 5-Year Default FICO < 660	(3) 5-Year Default LTV > 80%	(4) 5-Year Default Low Docs
<i>lastweek</i>	-0.00011 (0.00027)	-0.00016 (0.00047)	-0.00031 (0.00033)	0.00042 (0.00077)
<i>firstweek</i>	0.00044 (0.00027)	0.0011 (0.00064)	0.00097* (0.00054)	0.00049 (0.00041)
<i>black</i>	0.0033** (0.0012)	0.0011 (0.0012)	0.0017 (0.0015)	0.0075*** (0.0024)
<i>black</i> × <i>lastweek</i>	-0.0013** (0.00059)	-0.0020 (0.0011)	-0.0022*** (0.00062)	-0.0039** (0.0016)
<i>black</i> × <i>firstweek</i>	0.0020*** (0.00060)	0.0011 (0.00096)	0.0025** (0.00095)	0.0033*** (0.0011)
All Loan-Level Controls	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES
<i>last</i> − <i>first</i>	-0.0006	-0.0012	-0.0013	-0.0001
<i>p</i> − <i>value</i>	0.2800	0.2300	0.1400	0.9400
<i>last</i> − <i>first</i> (<i>black</i>)	-0.0038	-0.0043	-0.0060	-0.0073
<i>p</i> − <i>value</i> (<i>black</i>)	0.0015	0.0036	0.0023	0.0003
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	-0.0033	-0.0031	-0.0047	-0.0072
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.0003	0.0380	0.00080	0.0020
N	20732913	3729582	6606008	5617655
r2	0.12	0.16	0.15	0.22

Table A.3: The table reports regression estimates of the difference in performance between mortgages originated in the last and first week of the month. The dependent variable is a dummy equal to one for mortgages that defaulted within 5 years after origination. In column (2), the sample is restricted to subprime loans (FICO < 660). In column (3) the sample is restricted to high loan-to-value loans (LTV > 80%), and in column (4) to loans with low documentation. *All Loan-Level Controls* stands for the set of controls for application characteristics used in column (4) of Table 3, augmented with FICO bins and LTV (see Table 5). *lastweek* and *firstweek* are dummies equal to one in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with their *p-values*. Standard errors are clustered by lender and year. Estimates are based on the merged sample of HMDA and Black Knight McDash data from 1994 to 2018.

	(1) 5-Year Termination	(2) 5-Year Termination FICO < 660	(3) 5-Year Termination LTV > 80%	(4) 5-Year Termination Low Docs
<i>lastweek</i>	-0.0026 (0.0022)	-0.00060 (0.0012)	-0.0041* (0.0020)	0.0023 (0.0038)
<i>firstweek</i>	0.0010 (0.0014)	0.00032 (0.0020)	0.0018 (0.0016)	-0.00028 (0.0017)
<i>black</i>	-0.054*** (0.0083)	-0.038*** (0.0063)	-0.061*** (0.0091)	-0.044*** (0.0086)
<i>black</i> × <i>lastweek</i>	-0.0024* (0.0014)	-0.0043*** (0.0012)	-0.0014 (0.0015)	-0.0088** (0.0039)
<i>black</i> × <i>firstweek</i>	0.0018 (0.0018)	0.0013 (0.0019)	0.0033 (0.0021)	0.0036** (0.0014)
All Loan-Level Controls	YES	YES	YES	YES
Holiday FE	YES	YES	YES	YES
Day-of-Week FE	YES	YES	YES	YES
Month-Year-County	YES	YES	YES	YES
Month-Year-Lender	YES	YES	YES	YES
<i>last</i> − <i>first</i>	-0.0036	-0.00092	-0.0059	0.0026
<i>p</i> − <i>value</i>	0.2900	0.7700	0.0780	0.6200
<i>last</i> − <i>first</i> (<i>black</i>)	-0.0078	-0.0066	-0.0110	-0.0099
<i>p</i> − <i>value</i> (<i>black</i>)	0.1100	0.0440	0.0067	0.1200
<i>last</i> − <i>first</i> (<i>black</i> − <i>other</i>)	-0.0042	-0.0057	-0.0047	-0.0120
<i>p</i> − <i>value</i> (<i>black</i> − <i>other</i>)	0.1200	0.0410	0.1000	0.0047
N	20732913	3729582	6606008	5617655
r ²	0.22	0.28	0.25	0.32

Table A.4: The table reports regression estimates of the difference in performance between mortgages originated in the last and first week of the month. The dependent variable is a dummy equal to one for mortgages that were terminated (due to default or refinancing) within 5 years after origination. In column (2), the sample is restricted to subprime loans (FICO < 660). In column (3) the sample is restricted to high loan-to-value loans (LTV > 80%), and in column (4) to loans with low documentation. *All Loan-Level Controls* stands for the set of controls for application characteristics used in column (4) of Table 3, augmented with FICO bins and LTV (see Table 5). *lastweek* and *firstweek* are dummies equal to one in the first and last week of the month. *black* is a dummy equal to one for Black applicants. The table also reports estimates of the difference between the coefficients for the dummies *lastweek* and *firstweek*, and for the difference of the interaction coefficients for Black applicants, along with their *p-values*. Standard errors are clustered by lender and year. Estimates are based on the merged sample of HMDA and Black Knight McDash data from 1994 to 2018.

	(1) black	(2) black	(3) black	(4) approval	(5) approval
<i>lastweek</i>	-0.0014** (0.00058)	-0.00072 (0.00053)	-0.00073 (0.00057)	0.31*** (0.0014)	0.32*** (0.0015)
<i>firstweek</i>	-0.00063 (0.00071)	-0.00020 (0.00050)	-0.000075 (0.00055)	-0.12*** (0.0015)	-0.12*** (0.0015)
<i>approval</i>	-0.080*** (0.0033)	-0.049*** (0.0017)	-0.050*** (0.0018)		
<i>approval</i> × <i>lastweek</i>	0.0053*** (0.00066)	0.0019*** (0.00060)	0.0021*** (0.00069)		
<i>approval</i> × <i>firstweek</i>	0.00011 (0.00063)	0.00057 (0.00051)	0.00041 (0.00060)		
<i>black</i>				-0.63*** (0.0026)	-0.55*** (0.0028)
<i>black</i> × <i>lastweek</i>				0.070*** (0.0046)	0.064*** (0.0047)
<i>black</i> × <i>firstweek</i>				-0.0086* (0.0049)	-0.0074 (0.0051)
Loan Level Controls	YES	YES	YES	YES	YES
Holiday FE	YES	YES	YES	NO	YES
Day-of-Week FE	YES	YES	YES	NO	YES
Month-Year FE	YES	YES	NO	NO	YES
Lender FE	NO	YES	NO	NO	NO
County FE	NO	YES	NO	NO	NO
State FE	NO	NO	NO	NO	YES
Month-Year-County	NO	YES	YES	NO	NO
Month-Year-Lender	YES	NO	YES	NO	NO
<i>last</i> − <i>first</i> (<i>approval</i>)	0.0052	0.0013	0.0017		
<i>p</i> − <i>value</i> (<i>approval</i>)	0.0000	0.042	0.035		
N	14510888	14030895	13465036	19064873	18464655
r2	0.037	0.19	0.26	-	-

Table A.5: The table reports several alternative tests for discriminatory behavior based on high-frequency (within-month) variation. All estimates are based on loan level data, and samples including all originated and denied loans to Black and white applicants. In columns (1), (2), and (3) the dependent variable is a dummy equal to one for Black applicants. The variable *approval* is a dummy equal to one for originated loans, *lastweek* and *firstweek* are dummies equal to one for loans originated or denied in the last and first week of the month. The table also reports, for the first three columns, estimates of the difference in the coefficients for the interactions between the approval dummy and the last and first week dummies, along with the *p-value* for the null that the difference between the coefficients is equal to 0. In the first three columns, standard errors are clustered by lender and year. In columns (4) and (5), we estimate logit models, which predict approval probabilities at the loan level. Across all columns, estimates are based on a 5% random sample of the HMDA data from 1994 to 2018.