Know Thyself: Access to Own Credit Report and The Retail Mortgage Market*

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Abstract

Borrowers may misestimate their probability of mortgage approval in the absence of precise signals of creditworthiness. Credit reports, which contain such signals, became easily accessible for all U.S. consumers since 2005, while it was already the case in seven states. A difference-in-differences strategy exploiting this change shows that pool quality of mortgage applicants improved as a result—approvals increased, whereas subsequent delinquencies decreased. These findings are consistent with a mechanism where under-estimators enter the applicant pool and over-estimators drop out, because easier access to credit reports reduces misestimation of one's own probability of mortgage approval. Additional findings rule out supply-driven explanations.

JEL Codes: D12, D83, G21, G28, L51

Keywords: Credit Reports, Information Provision to Consumers, Household Finance, Mortgages, Regulation of Credit Information

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> *More than half of potential homebuyers did not apply for mortgage because they feared they would be rejected, putting their dream of owning a home on pause. Among the ones who feared, nearly three quarters admitted they haven't taken any steps to find out if they qualify for a mortgage.*
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> —*LoanDepot LLC National Survey, 2014*

Borrowers may misestimate their probability of mortgage approval if they do not know about their own creditworthiness. Some may overestimate their chances. This may be why many consumers get rejected due to credit history even though it can be checked before applying. In fact, from 2000 to 2008, twice more applications were rejected due to credit history than were due to debt-to-income (DTI) ratio. Others may underestimate their chances and end up not applying for mortgages despite needing it, because they think they would be rejected (discouraged borrowers). About 13% of U.S. households report as being discouraged in their mortgage decisions (Survey of Consumer Expectations, 2013–2020, SCE).

The economic consequences of such misestimation may be large. The over-estimators incur rejection costs which they could have avoided had they not applied. These costs include an increase in the probability of rejection of all future credit applications and a potential rise in the interest rates. The under-estimators forsake the opportunity of achieving the American dream of owning a home, an opportunity some may have got if they checked their credit reports. While lenders almost always use information in credit reports to decide on consumer credit applications, only about 8.4% of the 190 million credit-using consumers requested their reports in a typical year in the early 2000's (Nott & Welborn, 2003, p.9). More importantly, 84% of the reports were sought by consumers *after* they were denied for a credit.

Motivated by these facts, this paper asks: Is there a link between ease of access to credit reports for consumers and their credit decisions? Specifically, how does consumers' easier access to credit reports affect mortgage demand, origination, and repayments, and what are the underlying mechanisms?

The primary finding is that easier access to credit reports improves mortgage market outcomes. Specifically, approval ratio increases and delinquencies decline subsequently, suggesting an improvement in the pool of mortgage applicants. A consumer *self-learning* mechanism appears to be at work. Those whose reports signal high approval probabilities may apply for credit (i.e., enter the applicant pool), while those whose reports signal otherwise may either apply to a subprime lender, or may not apply for credit (i.e., drop out of the pool). This consumer-driven sorting results in a better pool of potential borrowers, leading to an increase in approvals.

At the same time, the sorting may increase or decrease the demand for credit depending on the prior distribution of the over- and under-estimators of approval probabilities and those who may be unaware of the role the reports play in a credit assessment process.[1]

The federal *Fair and Accurate Transaction Act* (FACTA) was enacted in 2003 providing all U.S. consumers a right to access three credit reports for free annually. Subsequently, in 2005, www.annualcreditreport.com website was established from where consumers could access the free reports in just a few clicks (Figure I). Prior to this, consumers had to go through many procedural hurdles to access the reports. They needed to call or write a letter to the credit reporting agencies (CRAs) and pay about USD 8–9 to make a request for the reports.[2] If they succeeded in making the request, the reports could still take a week or more to arrive (Golinger & Mierzwinski, 1998).

Seven states—Colorado, Georgia, Maine, Maryland, Massachusetts, New Jersey, and Vermont—had enacted local laws allowing the state's residents to access free credit reports before FACTA was enacted.[3] The local laws in these *early* states had driven the usage of credit reports many times higher than the usage in other states. According to one study, the usage rate was 2 to 4 times higher in these states than the rest before FACTA (United States Senate, 2004, p. 506). Other evidence suggest that, relative to the national average, the usage of credit reports was 250% higher in GA, 204% higher in MD, 153% higher in CO, 35% higher in NJ, and 25% higher in MA (United States Senate, 2004, statement of Senator Bennett, p. 376).

Higher usage of credit reports in the early states appear to be a result of the local regulations related to consumer credit. Colorado residents, for example, would receive a notification whenever a negative piece of information was to be added to their credit reports. The notification also reminded them about their right to obtain a free credit report and provided a letter to obtain the reports (United States Senate, 2004, p. 564). Georgia had provisions of two free

[1] Section (5E) discusses other alternative interpretations in detail.

[2] The three national CRAs—Equifax, Experian, and Transunion—blocked calls of millions of consumers who wanted to discuss the content of their credit reports. In 2000, the CRAs settled a lawsuit brought against them for this action (Federal Trade Commission, January 13, 2000). Moreover, the trivial USD 8 could be paid either through a credit card over the phone or through a bank cheque mailed with the request letter, hence the consumers with no access to credit card or formal banking faced higher procedural hurdles (about 50% of Black and Hispanic households and 20% of non-hispanic white households, Federal Reserve Board of Governors (2007, P. 135, Table 4)).

[3] The legislative bills and the timing of enactment of the local free credit report laws for each of these states are as follows: CO in 1997 through Senate Bill (S.B.) 133; GA in 1996 through House Bill (H.B.) 1632; MD in 1992 through S.B. 20; NJ in 1997 through Assembly Bill (A.B.) 2787, enacted as New Jersey Fair Credit Reporting Act; MA in 1995 through S.B. 79; VT in 1992 through S.B. 453; and ME in 2003 through H.B. 419. A state resident is a person domiciled in the state or who maintains a permanent place of abode; and spends in the aggregate more than six months of the taxable year in the state.

credit reports annually. In effect, procedural hurdles in obtaining the reports likely explains the differential usage of the reports across the early states and the rest.

To examine the causal effect of easier access to credit reports for consumers and the mortgage market outcomes, this paper exploits the pre-existing difference in the usage of credit reports in the states which already had free credit report laws and the states bordering them that did not have such laws. The research design is a single-event difference-in-differences (DID) design. The control group consists of all the early states except Maine, since the local law in Maine and FACTA were enacted in the same year. These six control states—CO, GA, MD, MA, NJ, and VT—are referred to as the pre-FACTA states in this paper. The treatment group consists of all the 20 states bordering the six control states. In effect, *late-treated* states serve as the treatment group whereas *early-treated* states serve as the control group. The DID estimator is the two-way fixed effects (TWFE) estimator. The event year is 2005, the year in which the website was established, and sample period runs from 2000 to 2008.

In a bid to sweep out the confounding effects of the local economic conditions from the treatment effect, the sample is restricted to only the counties at the border between the treated and control states, similar in spirit to the empirical strategy of Huang (2008) and Dube, Lester, and Reich (2010). The key outcome variables are analyzed at the census tract level, a sub-county micro area that roughly encompasses a population of just about 4,000. Incorporating fixed effects at the census tract level further removes the effect of time-invariant differences across the states, such as recourse or non-recourse mortgages and judicial or non-judicial foreclosures.

The issue of endogeneity in the assignment of the treatment and control groups is considerably alleviated in this empirical strategy. While the "treatment" occurs in 2005 owing to the provisions of the federal act, FACTA,—binding on all U.S. states—the "control" is (was) effected by the state laws that were enacted over 1992 to 1997, several years *before* the treatment event. The treated states, thus, did not opt to receive treatment, they were mandated to do so. Moreover, the FACTA enactment in 2003 does not appear to be an endogenous response to the prevailing economic conditions. It was enacted to perpetuate the provisions of an existing federal law, the *Fair Credit Reporting Act of 1970* (FCRA), which was set to expire in 2003 (via its amendment in 1996) (Nott & Welborn, 2003). Hence, FACTA was essentially a repackaged

FCRA with a key novelty being the annual free credit reports provision. Since other FACTA provisions already existed under FCRA, they are unlikely to contaminate the treatment effect.[4]

Consumers displayed considerable interest in acquiring free credit reports through the website. Anecdotal evidence suggest the website issued about 52 million credit reports in the first two years (Wikipedia, n.d.). Given that about 190 million individuals in the U.S. had active credit in 2003 (U.S. House of Representatives, 2003, p.6), 14% of them accessed credit reports yearly from the website alone, a 66% increase over the 8.4% usage in the pre-event period. The act also appears to have raised general awareness about the reports. Interest in free credit reports, measured using Google Search Interest for the keyphrase "Free Credit Reports", heightened at the time website was established.

The primary finding is that making it easier for consumers to access their credit reports resulted in an increase of 13.9%–15.3% in the number of mortgage applications and 1 percentage point in the approval ratio, and the effect is concentrated in owner-occupied mortgages. The equivalent dollar amount aggregated across the treated bordering counties is about USD 2.7 billion due to the former effect and about USD 37 billion due to the latter. The increase in approval ratio is consistent with the self-learning mechanism that predicts that the applicant pool should improve. Furthermore, the higher number of applications indicates that mortgage borrowers on average tend to underestimate their probability of approval, a contrasting finding to the common belief that consumers tend to be overconfident.

Next, the trends in mortgage delinquencies across the two areas are analyzed. If an improved applicant pool underlies the increase in origination, the delinquency rate should decrease, or at least not increase. Indeed, relative to the mortgages from the control areas, those from the treated areas originated in the event-year were *less* likely to become delinquent, but those originated in the pre-event year were just as likely to become delinquent.

Considerable evidence points to the self-learning mechanism. First, mortgage-related cognizance among the borrowers seems to increase. Specifically, the treated areas saw a statistically significant decrease in credit-history related mortgage rejections in the *ex-ante* high rejection

[4] Utilizing the already-treated units as counterfactual for late-treated (treatment) units is problematic in staggered designs and may introduce a downward bias in a single-treatment designs such as the one in this paper, if the treatment effects continue to evolve in the control states during the estimation period and have not reached the steady state at the time of the treatment. However, in the current design this issue of non-attainment of steady state of treatment effects in the early-treated control states is mitigated, because early treatments occurred several years before the treatment event and before the start of the sample period. Section (2) discusses it in more detail. Also, the estimates are similar in a sub-sample which removes the last-treated control state and the surrounding treated states, further reassuring that the (non-)inclusion of last-treated control states do not alter the estimated effects. Section (4A) presents this analysis.

areas but no decrease in DTI-related denials. This points to an increased learning among borrowers about their credit history. Also, the fraction of total applications withdrawn while being processed dropped, indicating a reduced tendency to formally apply to multiple lenders and hence saving the costs of multiple applications. Second, the number of mortgage applications decreased in the over-estimating areas and increased in the under-estimating areas, where the two types of areas were identified using census tract-level proxies based on the average loan-to-income (LTI) ratio and the rejection pattern in the pre-event year. This differential change in the number of applications in the two areas support the predictions of the self-learning mechanism.

It is also important to understand which borrowers benefit more from easier access to credit reports. The areas with an *ex-ante* high creditworthiness saw a greater increase in approval ratios and mortgage applications. This is in line with the idea that the reports aid consumers in assessing their creditworthiness. Then, for the lowest-income-quartile borrowers, the approval ratios increased statistically significantly in the treated areas vis-à-vis the control areas, but the number of applications did not. Since lower income is associated with creditworthiness overestimation (Perry, 2008), the self-learning mechanism predicts that the correction for the overestimation for these consumers will result in fewer applications (drop out), leading to a rise in approval ratios but not in the number of applications. Third, treated areas saw proportionally more first-time homebuyers.

Finally, a host of tests examining the response of lenders rule-out the supply-side explanations for the increase in origination. Specifically, relative to the control areas, the treated areas saw (i) increased mortgage interest rates; (ii) the high-lenders-density areas did not see more origination or approvals vis-à-vis the low-lender-density areas; and (iii) private securitization of mortgages did not significantly increase. Furthermore, banks that had larger mortgage origination in the treated areas in the pre-event year saw higher financial performance post event.

All in all, the conclusion is that making it easier for consumers to access their credit reports brings about changes in the mortgage market that are indicative of an improvement in the applicant pool. Given that the findings are causal, policies aimed at encouraging consumers to check their credit reports and educating them about its role in credit approval may yield similar improvements, not just for the mortgage markets, but for any consumer credit market.

This paper primarily relates to the literature on effects of information provision on credit market participants. This is the first paper to show that making it easier for consumers to access credit reports leads to improved mortgage market outcomes in a manner consistent with an improved applicant pool. A field experiment by Homonoff, O'Brien, and Sussman (2019) reveals that borrowers are less likely to default when provided with information on their FICO® scores. Similarly, Mikhed (2015) shows that borrower participation in a free FICO scores program is associated with lower delinquencies, reduced credit utilization, and increased credit card spending. Using a pair of policy changes in Chile, Kulkarni, Truffa, and Iberti (2018) find that increasing disclosure about financial products leads to lower defaults for sophisticated borrowers, and that standardizing financial products leads to lower defaults for unsophisticated borrowers. Also, bankruptcy flag removal from the credit reports raises mortgage borrowing by consumers (Dobbie, Goldsmith-Pinkham, Mahoney, & Song, 2016), and lowers the cost of credit for poorer defaulters and increases it for poorer non-defaulters (Liberman, Neilson, Opazo, & Zimmerman, 2018).

This paper also speaks to the extensive literature on household financial literacy. Low financial literacy leads to mortgage delinquencies and foreclosures (Gerardi, Goette, & Meier, 2010), poor mortgage choice (Moore, 2003), large debt (Lusardi & Tufano, 2009; Stango & Zinman, 2009), and lower ability to benefit from loan-modification contracts when in distress (Hundtofte, 2017). On the other hand, educational intervention improves consumers' financial product purchases (Balakina, Balasubramaniam, Dimri, & Sane, 2020). This paper shows that making it easier for consumers to access their credit reports lowers mortgage delinquencies and raises mortgage application approval ratios and mortgage-related cognizance among borrowers.

The rest of this paper is organized as follows. Section (1) describes the U.S. laws related to consumers' access to credit reports, Section (2) presents the research design, and Section (3) describes the data this paper uses. Section (4) discusses the main results, and Section (5) contains supplementary results that aid interpretation of the main findings. Finally, Section (6) concludes the paper.

1 U.S. Laws Governing Consumers' Access to Credit Reports

Enacted in 1973, the FCRA was the first legislation regulating the information credit reporting agencies collect and the manner in which consumers could access it. The act provided consumers the right to see the contents of their credit reports, except for the credit score, under specific yet restrictive circumstances. Consumers could receive a free report if they made a request within 60 days after receiving a notice of an *adverse action* taken against them on the basis of the information in the report (Nott & Welborn, 2003).[5] An amendment to the FCRA in 1992 further mandated that the cost of disclosure of credit information should be reasonable, and the next amendment in 1996 capped the cost of the disclosure at USD 8 and provisioned that the FCRA would expire in 2003.[6]

It was when the FCRA was to expire that FACTA was enacted, specifically to perpetuate the FCRA's existing provisions while also adding the new annual free credit report provision. FACTA was signed into law on December 4, 2003, and *inter alia* it allowed for free annual disclosure of credit reports to consumers by each of the three national credit reporting agencies through a centralized source. Subsequently, the website—www.annualcreditreport.com—was established in 2005 to distribute the free credit reports.[7] Panel (A) and (B) of Figure(I) shows the homepage and the "frequently asked questions" section of the website, respectively and Figure(II) shows the summary page of a credit report obtained from the website.

Notwithstanding the federal regulations on consumer credit reporting, seven states (CO, GA, MA, MD, NJ, and VT) enacted local state laws over 1992 to 2003 that allowed their residents to access free credit reports (see Footnote (3) for details of the enactments). For example, Colorado enacted its free credit report law on April 21, 1997 through Senate Bill 133. Section 4, paragraph (E) of this bill added the following to Title 12 Article 14.3-104 of the Colorado Statute:

> (*E*): *Each consumer reporting agency shall, upon request of a consumer, provide the consumer with one disclosure copy of his or her file per year at no charge whether or not the consumer has made the request in response to the notification required in paragraph* (*a*) *of this subsection.*

[5] An adverse action notice can be sent to a consumer by the *user* of a consumer report (e.g. banks, financial institutions, insurance firms) or a debt collection agency affiliated with the CRA stating that the consumer's credit rating may be or has been adversely affected. Under the FCRA, a consumer can also receive credit report free of charge once in 12 months by making a request to the CRA certifying that she/he either: (A) is unemployed and intends to apply for employment in the 60 day period beginning on the date on which the certification is made; (B) is a recipient of public welfare assistance; or (C) has reason to believe that the file at the agency contains inaccurate information due to fraud.

[6] Even though the cost of credit reports was capped under the FCRA and even though consumers could access free credit reports under specific circumstances, on average only 8.4% of the credit-using consumers request the reports and 84% of the requesting consumers do so after they get denied for credit (after adverse action) (Nott & Welborn, 2003, p.9).

[7] The website was rolled-out in four phases over nine months, from December 2004 to September 2005. Phase (I) rollout was on Dec 1, 2004 in 13 states: *AK, AZ, CA, CO, HI, ID, MT, NV, NM, OR, UT, WA, and WY*. Phase (II) rollout was on March 1, 2005 in 12 states: *IL, IN, IA, KS, MI, MN, MO, NE, ND, OH, SD, and WI*. Phase (III) rollout was on June 1, 2005 in 11 states: *AL, AR, FL, GA, KY, LA, MS, OK, SC, TN, and TX*. Phase (IV) rollout was on September 1, 2005 in the remaining 14 states and Washington D.C. *CT, DE, DC, ME, MD, MA, NH, NJ, NY, NC, PA, RI, VT, VA, and WV*.

These state laws were likely an endogenous response to the local environment. For example, Vermont was the first state to enact its law, in 1992, because TRW (a credit reporting firm, now Experian) in 1991 *mistakenly* recorded the tax bills of each resident of the town of Norwich and 650 of the residents of the town of Woodstock as property tax liens, due to which these consumers would have been rejected for credit by every lender had they requested it (Associated Press News, December 24, 1992). In the same year, Maryland enacted a similar law.

2 Empirical Research Design

As discussed, this paper uses a DID setting in which six pre-FACTA states—CO, GA, MA, MD, NJ, and VT—constitute the control group and the states bordering these constitute the treatment group. Panel (A) of Figure (III) shows these states on the map of the contiguous U.S. The regression sample focuses only on those counties that lie at the borders between the treated and control states, and Panel (B) of Figure (III) shows these counties on the map of the contiguous U.S. The event year is 2005, the establishment year of the website. The sample period is chosen to be from 2000 to 2008 to allow for sufficient post-event observations.

An apparent issue with the empirical design of this paper is that already-treated states are being used as control states, while all the treatment states received the treatment at once in 2005. The DID estimates in this design could be biased downwards if the treatment effect in the control stats is claimed to have not reached the steady state, and the estimates could be unpredictable if the dynamic effects of the treatment are still evolving differently across different early-treated control states. The time it would take for treatment effects to reach a steady state is challenging to estimate. However, the fact that the already-treated units received treatment several years before the treatment event, with Colorado being the last control state to receive the treatment eight years before the event, and the fact that all control states received treatment before the start of the sample period offer assurance that the steady state might have been achieved in the control states. With these features of the design choice, the paper assumes that these early-treated states are in steady state, and thus the DID estimator would yield unbiased causal estimates.

A further assurance about the estimates are largely free from the treatment effects in the control states comes from a sub-sample analysis. Note that the control state that was treated the last, Colorado in 1997, is the most likely among all the control states to suffer from the treatment effect not having reached the steady state. Reassuringly, the effect size estimated in the sub-sample formed by removing Colorado—and the respective bordering treated states—yield estimates similar in size. This result is discussed in Section (4A).

Another issue with the empirical design is that the treatment effects may confound with effect of differences in state regulations on housing and mortgages—e.g., recourse versus non-recourse mortgages (Ghent & Kudlyak, 2011) and judicial versus non-judicial foreclosures (Gerardi, Lambie-Hanson, & Willen, 2013). Additionally, credit-related regulations that are enacted within the sample period, such as the adoption of Anti-predatory Lending laws (APL) by twenty U.S. states over 2000 to 2006 (Di Maggio & Kermani, 2017), may further aggravate the issue.[8]

However, the DID design makes the estimates robust to any state-level differences that do not change over the sample period, such as the recourse versus non-recourse mortgages and judicial and non-judicial foreclosures. The confounding effects of staggered regulatory adoption, too, get averaged out in the estimation when the timing of adoption and the states who adopt them are different from the timing of the natural experiment and the treated and control states. This issue is further alleviated by the fact that removing each of the control and associated treated states one at a time yields similar estimates for key outcome variables (discussed in the Results section).

The contiguous-county design across state borders also alleviates the potential confounding effects of local idiosyncratic trends, since such trends are not likely to vary widely across neighboring areas and macroeconomic shocks affect neighboring areas roughly at the same time (Dube, Lester, & Reich, 2016), making this design one of the most compelling identification strategies (Allegretto, Dube, Reich, & Zipperer, 2017).[9] A similar empirical approach has been used in Huang (2008) and Dube et al. (2010).

[8] Table (A1) in the Online Appendix lists the treated and control states and their status with respect to these regulations. The distribution of the three specific regulations across the treated and control states is as follows: 90% of the treated and all the control states have recourse mortgages; 45% of the treated and 83.3% of the control states have judicial foreclosures; and 35% of the treated states and 83% of control states adopted the APL laws within the sample period.

[9] In the context of this paper, another viable approach is to use a synthetic control matching procedure. However, this method, too, may place greater weights on *nearby areas* (Allegretto et al., 2017) as it replicates the unobserved counterfactual by taking a weighted average of the observable units. A benefit of a contiguous-county design is that the treatment and control areas map one-to-one to observable geographic areas, enabling tighter links to the real-world data, while a disadvantage is that economic environments across states are inherently different.

2A The DID estimator

The estimator used in the paper is the two-way fixed-effects (TWFE) estimator, specified as

$$Y_{icsjt} = \beta_0 + \beta_1 \times \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt} \ , \tag{1}$$

where Y_{icsjt} is the outcome variable measured in year t for a census tract i from a county c lying at the border between treatment state s and control state j. t ranges from 2000 to 2008, and j ranges from one to six, corresponding to each of the six control states. $Post_t$ takes value 0 for year $t < 2005$ and value *1* for year $t \geq 2005$. $Treat_{icsj}$ is 0 for all the census tracts i in counties c from control states j, and it is 1 for all the census tracts in counties from the treatment states s. β_1, the coefficient of interest, captures the treatment effect, which is the change in the dependent variable in the treated counties relative to the control counties occurring in the post-event period relative to the pre-event period. Standard errors are clustered at the county level to provide for correlation in error terms for the observations from census tracts belonging to the same county.

Economic controls in the equation represent the time-varying co-variates capturing the economic and credit conditions at the level of either census tract, county or state. While specific controls vary across different regression models, most commonly they include five variables: the natural log of number of mortgage lenders in a census tract; county-level house price index (Bogin, Doerner, & Larson, 2016, *index version 2000*); and the annual growth rate of county's income per capita, county's aggregate employment and state's gross domestic product (GDP). To ensure that the treatment effects are not influenced by the co-variates, all regressions are estimated both with and without the co-variates.

α_i represents *Census Tract* fixed effects, the first of the two-way fixed effects. These account for any time-invariant differences across census tracts at a highly granular geographic area that encompasses a population of just about 4,000. As census tracts are smaller geographic areas than a county or state, these fixed effects flexibly and fully account for any state-level time-invariant differences, including recourse and non-recourse mortgages, judicial and non-judicial foreclosures etc.

$\gamma_{j,t}$ represents *Border×Year* fixed effects, the second of the two-way fixed effects. Here j refers to the border of a control state j. These fixed effects are formed by the interaction of the border of each of the control states with year, allowing for each region to have its separate time trend (where a region consists of the counties at the border between a given control state and all contiguous states). Thus any regional shock that may affect the regions across different years are flexibly and robustly accounted for.[10]

All in all, the two fixed effects and the time-varying economic controls are expected to reasonably account for confounding effects of local economic conditions on the outcomes of interest, and thus should allow for cleaner estimation of the treatment effects.

2B Is the TWFE an appropriate estimator for the current DID design?

A key issue with the TWFE estimator is that in *staggered* DID designs, it may aggregate individual treatment effects by assigning "negative weights" to some of them (Borusyak, Jaravel, & Spiess, 2021; De Chaisemartin & d'Haultfoeuille, 2020; Sun & Abraham, 2020). Since the estimator is the variance-weighted average of the treatment effects, the negative weights occur in staggered designs when the treatment effects are heterogeneous across time and/or the treated units (Goodman-Bacon, 2021). Here the issue of heterogeneous treatment effects across time does not arise, since the current paper uses a *single-treatment* DID design, not a staggered treatment.[11] The other issue of the treatment effect being heterogeneous across treated units remains a noteworthy limitation. However, the results in the paper remain largely supportive of the conclusions, since the key estimates are robust across the sub-samples formed by removing, one at a time, each of the control states and the respective contiguous treated states.

Time-varying co-variates also potentially introduce bias in the estimator (Goodman-Bacon, 2021), but the conclusions of this paper are robust to this issue, as all the estimates are qualitatively and quantitatively similar, *either with or without* the co-variates. Finally, the TWFE also requires random assignment of the treatment. Though the assignment is not carried out randomly, the timing and circumstances of the FACTA enactment are unrelated to the states' actions, and thus the requirement is expected to be largely satisfied.

In the end, the TWFE relies on the parallel-trends assumption: the treated states would have had trends similar to that of the control states in the absence of the treatment. Though the assumption is unverifiable, the coefficient estimates for the effect size across the sample years shed light on it. Figure (IV) plots the coefficients (β_k) from regression of the *Approval Ratio*

[10] Consider the control state CO. All census tracts from the counties at the border between CO and the surrounding states—WY, UT, AZ, NM, OK, KS, and NE—take the same value for j, and thus are grouped as one unit (region). Thus the fixed effects only utilize the variation *within* each of the six such regions.

[11] The staggered adoption of the local laws by the control states over 1992–1997 is likely not a serious concern, since the post-event outcomes are measured at least eight years after the last adoption (by Colorado in 1997). Had the time gap was smaller, the dynamically evolving treatment effects in these early-treated control units *would* coincide with the effect in later-treated units, compromising the parallel-trends assumption.

according to the following specification:

$$Y_{icsjt} = \beta_0 + \sum_{k=T-3}^{T-1} \beta_k \text{ Treat}_{icsj} \times \text{Event}_k + \sum_{k=T+1}^{T+4} \beta_k \text{ Treat}_{icsj} \times \text{Event}_k + \alpha_i + \gamma_{j,t} + \varepsilon_{icsjt} \,, \quad (2)$$

where $Event_k = 1$ *if* $t = T - k$, $Event_k = 0$ *if* $t \neq T - k, k = \{-3, 4\}$, *and* $T =$ *Event year 2005*. The coefficients $\beta_k's$ represent the difference in approval ratio for the two groups over the years relative to the pre-event year 2004. We see from the plot that for the most part no significant difference exists between the treated and control census tracts before the event, but the difference becomes significant afterwards. Overall, this plot offers assurance that the parallel-trends assumption likely holds in this case.

2C Salience of the natural experiment and other validating assumptions

The central event of interest is the introduction of free credit reports, it should be validated whether consumers had interest in accessing their credit reports, or alternatively, whether the natural experiment was salient for them. The uptick in interest in free credit reports, measured using Search Interest data from Google Trends, strongly suggests so.[12] *First,* the search interest for the key phrase *Free Credit Report* heightened in Jan 2005, coinciding perfectly with the establishment of the website (Panel (A) of Figure (VI)). *Second,* plotting the search interest separately for the treated and control states using the Interest-by-subregion data in Panel (B) of Figure (VI) reveals that, although the interest in free credit reports was similar in both sets of states in the pre-event year 2004, the interest was more intense in the treated states relative to the control states after the event.[13] Also, as discussed before, almost 52 million credit reports were issued to consumers through the website in the first two years, representing an increase of 66% in the overall usage of the reports.

[12] The Google Trends data represent the degree of "search interest" for a given keyword at any time relative to the highest point during the period of analysis over a given region (U.S.). In the time series, a value of 100 represents the peak popularity for the term. A value of 50 means that the term is half as popular. In the cross-section, a value of 100 represents the location with the highest popularity of the keyword as a fraction of total searches in that location. A value of 50 indicates a location that is half as popular. A score of 0 means there were not enough data for this term. Google Trends data start from January 2004.

[13] An issue with analyzing cross-sectional trends in the search-interest data (interest-by-sub-region data) is that the data values are normalized by Google within the time interval for which the data are extracted. However, this can be overcome by first extracting the data *separately* for each time interval of interest (one-year intervals, in the current case), and then calculating the mean *separately* within each time-interval for each of the two sets of states. From this plot, it may appear that the popularity in the control states after the event decreased. However, this occurs because the popularity measure is essentially a yearly percentile ranking of states, with 100 being the most popular; so an increase in the rank of one state mechanically decreases the rank of others.

One may wonder, do consumers not access their credit reports before applying for mortgages, especially given that the cost of the reports is trivially small (at just about USD 8) relative to the size of the mortgage (about USD 200,000)? The credit report usage data from early 2000's indeed confirm that most consumers did not do so. The annual number of mortgage applications in that time period was about 15 million a year, while the number of consumers proactively requesting their credit reports was mere 0.84 million (5.25% of 16 million reports, Nott and Welborn (2003, p.9)).

The question then arises, why consumers tended to not seek their credit reports? Procedural hurdles in accessing the reports appears to be one reason, since the usage of the reports in the states with free credit report laws was many times higher than the others before the FACTA, even though the reports cost so little (United States Senate, 2004, p.376 and 506).

It is also worth pointing out that the credit reports issued under FACTA do not contain the numerical credit score. However, the consumers visiting the website were actively asked if they wished to retrieve their scores from any of the three CRAs. The website not only guided them on how to get the score from within the website itself, but also provided explanations on the role of the reports and scores (see Figure (I)). Hence, it seems reasonable that consumers could understand their creditworthiness better from the website.

Finally, the quantities estimated in this paper potentially under-reports the true treatment effect. This is because the treatment event affected both the (already-treated) control states and the treated states, and it may be argued that even the control states may experience the treatment effects to a certain degree. These estimates may also be labeled as the *average treated effects on the late-treated* (ATT-LT), because they rely on comparing the outcomes in the areas newly treated at once with the areas treated early over a period of time.

3 Data and Summary Statistics

This paper primarily draws on three publicly available datasets: *Home Mortgage Disclosure Act of 1975* dataset (HMDA data) for information on mortgage applications; Federal National Mortgage Agency (Fannie Mae) and the Federal National Home Loan Mortgage Corporation (Freddie Mac) dataset (GSE data) for information on mortgage delinquency performance; and the Call Reports (FFIEC Forms 031/041) for information on financial performance of banks. HMDA dataset provides application-level details on applicants' race and gender, income, loan amount, the financial institution handling the mortgage application, outcome of the application, and geographic location of the property at the census tract level.[14] The GSE dataset contains only mortgages purchased by Fannie Mae and Freddie Mac, and it covers only the 30-year fixed-rate single-family mortgages, the most popular mortgage type in the U.S. The mortgage-level information in these data include the interest rate, DTI ratio, credit score, first-time homebuyer status, investment purpose, and the first three digits of the zip code (zip3) of the mortgaged property. The mortgage performance information include repayment amount and delinquency status, both updated monthly. Finally, Call Reports contain detailed financial information of the U.S. banks.

The steps taken to process each of these datasets and to merge them are provided in Appendix (6), Data Appendix. Mortgages for all purposes and types in the HMDA dataset are included in the sample. The three purposes are—home purchase, refinance, and home improvement, and the three types are—conventional loans, loans guaranteed by Veterans Administration (VA) and Farm Service Agency (FSA)/Rural Housing Administration (RHS), and loans insured by the Federal Housing Administration (FHA). These application-level data are aggregated to the $Census\ Tract \times Year$ panel, resulting in 11,942 census tracts that belong to the bordering counties. There are 7,011 treated census tracts, 4,931 control census tracts, and 89,535 $Census\ Tract \times Year$ observations. Similarly, the mortgage-level GSE data are aggregated to the zip3-state level, leading to 221 unique zip3-states (91 as control and 130 as treated) and 7,599 $Zip3 - State \times Quarter$ observations.

Some other data are also collected from public sources. Survey data on consumers' credit usage are taken from the Survey of Consumer Expectations (2013–2020) Credit Access Survey, a Federal Reserve Bank of New York rotating panel survey fielded since 2013 over the internet every four months. The data on county-level subprime population come from FRBNY and Equifax (n.d.), on state-level economic conditions from the Bureau of Economic Analysis, on census-tract level population characteristics from Census 2000 (Manson, Schroeder, Van Riper, & Ruggles, 2019), and on county-level employment from the annual survey of County Business Patterns (CBP) (Census Bureau, 2000–2008).

[14] Until 2003, the census tracts in the HMDA dataset are from the Census 1990 definition, while those from 2004 onward are from the Census 2000 definition. To facilitate the comparison of the tract-level data pre-2003 with post-2003, the census tract-level variables from 2000 to 2003 were scaled using the ratio of population residing in the 1990 tract definition to that in the 2000 definition using data from the Census Bureau (2006). Even though this process is an approximation and introduces some noise in the measurements, it is necessary. The approximation is limited to just 22% of the tracts across the U.S., since 63% of the 1990 census tracts did not see any change across the two censuses and 15% of the 1990 census tracts were wholly combined into various 2000 tracts.

The key outcome variables are scaled applications and approval ratio. The scaled applications is the number of mortgage applications per 1000 adults in a census tract, and the approval ratio is the ratio of the number of successful applications (action type "1" *or* "2" in the HMDA dataset) to the number of total applications in a census tract. Similarly, other variables of interest are defined at the census-tract level: the fraction of total applications withdrawn while still under processing and the fraction of total applications denied for credit history or DTI ratio.

Panel (A) of Table (I) shows the summary statistics for the key variables over the sample period. We see that the treated census tracts have fewer scaled applications, lower mortgage approval ratio, and higher denials related to credit history and DTI ratio.[15] Panel (B) of Table (I) shows the statistics for growth rate of each of the variables in the pre-event period, from 2000 to 2004, separately for the treatment and control groups. The p-value of the t-tests for the difference in mean growth rate of each variable across the two areas is also provided. The t-tests shows that the two areas experienced a different trend in mortgage applications and approval ratio over this time period. While applications were growing at a slower rate in the treated areas, approvals were growing at a higher rate. At a first glance it appears problematic that the treatment and control areas saw a different trend in the pre-event period, but notice that a continuation of this trend would have predicted a decrease in applications in treated areas and an increase in approval ratio. However, the estimated effect is a decrease in the number of applications and an increase in the approval ratio, alleviating the concern of pre-trend being estimated as the treatment effect. It is further re-assuring that the trend in most economic variables are not statistically different across the two areas, including house prices. These tests further lend credence to the parallel trend assumption for a DID design.

4 Results

This section presents the causal effects of easier access to credit reports on the mortgage market outcomes first and then the results highlighting the self-learning mechanism and the heterogeneity in the treatment effects.

[15] Notice that the four ratios—the approval ratio, two denial ratios, and withdrawal ratio—do not sum to one. There are three reasons for this. First, the reporting of the reason for denial is not mandatory under HMDA regulations; hence an application may be recorded as denied without any stated reason (70.81% of denied applications have at least one stated denial reason). Second, denials could be for reasons other than credit history or DTI ratio. Third, an application might be denied for multiple reasons.

4A Mortgage Market Outcomes

The effects of easier access to credit reports is examined on the number of mortgage applications per 1000 adults (scaled applications) and approval ratio. These variables are measured at the census tract level; the regression specification is from Equation (1); and all specifications include *Census Tract* and *Border*×*Year* fixed effects. The coefficient of interest is *Treat*×*Post*, which estimates the change in the outcome variable in the treated areas relative to the control after the event.

The regression results for scaled applications appear in Columns (1) and (2) of Table (II). The specification in the first column is without any co-variates, whereas that in the second includes controls for local economic conditions, namely, the natural log of number of HMDA lenders in a census tract, county-level house price index, and annual growth rates of county income per capita, county aggregate employment, and state GDP. The coefficients suggest that the applications rose in the treated tracts relative to the control tracts by 13.4–14.78, a 13.9–15.3% increase over the pre-treatment average of 96.3. In real terms, keeping the approval ratio in the treated areas at the pre-event level, the increase roughly translates to USD 37.8 billion, aggregated across the treated bordering counties.[16] The increase in applications indicates that more consumers tend to apply for credit once information about their creditworthiness becomes easier to access.

Columns (3) and (4) of Table (II) show the regression results for approval ratio. Coefficients on *Treat*×*Post* suggest that the ratio increased by about 1 percentage point in the treated tracts relative to the control. In dollar terms, keeping the number of applications in the treated areas at the pre-event level, a 1 percentage point increase in approval ratio corresponds to successful mortgages worth about USD 2.75 billion, aggregated across the treated bordering counties.[17] The effect at first may seem trivial, especially since approval ratios are commonly believed to be high, at upwards of 80%, but it is just about 52% in the treated tracts in the pre-event period.

[16] The average mortgage size in treated tracts in the pre-treatment period was about USD 150,597. Thus the demand for mortgage credit increased by about USD 2.0 million per 1000 adults per census tract (USD 150,597 × 13.4), by about USD 5.4 million per treated census tract (USD 2 million × 2.7 thousand adults per census tract), or by about USD 37.8 billion across *all treated tracts from bordering counties* (USD 5.4 million × 7,011 treated tracts).

[17] A 1 percentage point increase in approval ratio is equivalent to ~2.6 more successful applications per treated tract (96.27 applications per 1000 adults in the pre-treatment period × 0.01 × 2.7 thousand adults per treated tract), about 18,229 more successful applications across the treated bordering counties (2.6 applications × 7,011 treated tracts), or a ~USD 2.75 billion increase in mortgage origination across *all treated tracts from bordering counties* (18,229 × USD 150,597 average mortgage amount per application).

Approval ratio increased potentially because borrowers were better informed of their creditworthiness, as the establishment of the website did not affect other aspects of the mortgage process. Specifically, the website did not alter the content of the reports or lenders' access to the reports. It also could not affect any borrower characteristics such as their income, employment, or collateralizability of their assets. However, self-learning mechanism can explain increase in approvals. The website not only made it easy to access credit reports but also provided information on the role of credit reports and scores in credit applications (see the snapshot of the website in Figure (I)). Borrowers possibly learned about their creditworthiness from the reports and sorted themselves better in the market. Creditworthy borrowers stayed or entered the applicant pool whereas those with bad creditworthiness either dropped out of the pool or accessed credit form the subprime lenders, improving the quality of the pool. Later sections examine the mechanism in detail.

One of the concerns with the empirical design of this paper discussed earlier is that control states were already treated in the past. If the treatment effects in these early-treated states has not reached the steady state, the estimates may be downward biased. A sub-sample analysis relying on timings of treatment of control states could help to understand whether such pre-treatments cause fluctuations in the estimates. Among all the control states, the one treated the last, i.e., Colorado in 1997, is most likely to have not reached the steady state. Removing Colorado and surrounding states from the sample thus yields a sub-sample where arguably steady-state in the control states is more likely to have been achieved. Extend this sub-sampling exercise further, treatment effects are estimated for each of the sub-samples formed by removing one-by-one each of the control state and associated treated states. The results are presented for scaled applications and approval ratio in Panel (A) and (B) of Figure (V), respectively. The coefficients are from the regression specification Equation (1) with all controls included. Notice that the estimates are similar across all sub-samples, including when Colorado and its bordering states are removed. Thus, the already-treated status of the control states does not appear to create a major concern about the estimates being biased downwards. However, in so far as one believes that the treatment effects have not reached the steady state in the early-treated states, the estimates reported in this paper under-report the true treatment effects.

A noticeable limitation of the estimates above is that the mortgage supply in the U.S. had started to shrink from 2005, an antecedent of the financial crisis of 2008, and the post-event regression sample includes the years from 2005 to 2008. Hence, while it may be injudicious to claim that the effects estimated above are completely uncontaminated by these changes, the DID design ameliorates the issue to the extent that the market-wide forces evenly affect the neighboring counties across the state borders. In addition, the effects remain qualitatively and quantitatively similar when estimated in an alternative sample which restricts the post-event period to 2006 (see Section 5G, Robustness).

The financial crisis is also often argued to be a result of excessive mortgages taken by borrowers without means, often for investment purposes rather than for occupancy purposes, and this raises the question, whether the increase in the origination reported above is also driven by such borrowers. Table (III) examines this assertion using the same DID specification. The outcome variable is scaled applications for the owner-occupied mortgages in Columns (1) and (2) and for the non-owner-occupied category in Columns (3) and (4). The coefficients on *Treat*×*Post* suggest that the applications increased dramatically (and significantly) for the owner-occupied category in the treated areas vis-à-vis the control. The increase in applications for the latter category was an order of magnitude smaller. A further examination of compositional change across the two mortgage categories reassures the assertion. Columns (5) and (6) examine the scaled applications in the non-owner-occupied category as a fraction of total applications, and Columns (7) and (8) examine the same as a fraction of successful applications. The coefficients in these four columns indicate a modest 1 percentage point increase in non-owner-occupied mortgages at both the application and the origination stage. By and large, the investment-motivated demand does show a slight uptick, but does not appear to be the dominant reason behind the robust 15% increase in mortgage applications.

4B Mortgage Delinquencies

Results so far imply that more mortgages were originated after credit reports became easier to access. The question then arises, whether mortgage delinquencies too would increase as a result? If the origination increased owing to an improved applicant pool, the delinquencies would fall, or at least not rise. However, if the origination increased due to subprime lending while the pool stayed the same as before, the delinquencies would rise subsequently.

To examine the patterns in the delinquencies, the GSE data, which are a subset of the HMDA mortgages, are used. First, a mortgage vintage is defined as the collection of the mortgages originated in a given area—treated or control—in a given year—2004 (pre-event) or 2005 (post-event), leading to four vintages: the treated vintage in the pre- and post-event year, and the control vintage in the pre- and post-event year. Then, the rate of delinquency of a given vintage is defined as the ratio of the number of mortgages late on a scheduled payment by n days for the first time at a given age (measured in months since origination) to the total number of mortgages in that vintage. The rates are analyzed for delays of n= 30–89 days and 90–120 days.

Panel (A) of Figure (VII) shows the 30–59-day delinquency rate for the treated and control vintages for the year 2004 on the left-hand side and for the year 2005 on the right-hand side. The plot on the left reveals that, among the mortgages originated before the event, the delinquency rates of the treated and control vintages follow almost the same trend; whereas the plot on the right reveals that, among the mortgages originated in the year of the event (2005), the delinquency rate of the treated vintage is *lower* than that of the control vintage. Furthermore, the delinquency rates of the treated vintage becomes much lower than that of the control vintage during the financial crisis (48 months after 2004, or 36 months after 2005) than during the earlier periods.

To facilitate the comparison of monthly delinquency rates, Panel (B) of Figure (VII) plots the difference between the monthly delinquency rate of the treated and control vintage for 2005 minus the same difference for 2004 vintage. The mean value of this (double) difference is -.021 percentage points (statistically significant with $pval < 0.000$).

The reduction in delinquency appears to be driven by the improved pool quality as the composition of the borrower pool may change after access to credit reports becomes easier—new creditworthy borrowers may enter the market and those with poor creditworthiness may drop out. Section (III) provides an evidence consistent with such a change—the proportion of the first-time homebuyers in the originated mortgage pool increased.[18] While the empirical evidence on whether the first-time homebuyers default at a lower or higher rate than others is mixed (Kelly, 2008; Patrabansh, 2015), they appear to have a lower delinquency rate in the context of the current paper.

However, from lenders perspective these reductions may appear puzzling. It may be argued that lenders may utilize the surplus capital freed up from the improved borrower pool in lending to lower quality borrowers in a way that the average delinquency rate of the mortgage

[18] Suppose that lenders deny applicants whose *ex-ante* probability of default falls above some threshold, p^*. Assume that the average delinquency rate of originated loans in the pre-event period is p_1, where $0 \leq p_1 \leq p^*$. After the free credit report policy is implemented, suppose that an additional pool of applicants is motivated to request a mortgage, and they are subject to the same upper bound, p^*, but their delinquency rate is p_2, where $0 \leq p_2 \leq p^*$. It is clear that depending on the values of p_1 and p_2, the average delinquency rate after the event may increase or decrease, given that the delinquency rate of the new entrants is different from that of the older pool.

pool stay the same. While such an explanation is feasible, limited credit supply may prevent them from such doing so.

Overall, the reduction in the delinquency rates after the event suggests that the applicant pool improved, and one reason for this appears to be an increase in the share of first-time homebuyers.

4C Mechanism: Consumer Self-learning Channel

This section tests the self-learning mechanism.

(I) Increase in the mortgage-related cognizance among borrowers

If consumers learn more from their credit reports about their creditworthiness after the event, their decision regarding credit and mortgages will reflect it. In particular, as the reports contain the credit history of consumers, their cognizance of about it should increase. Consumers may thus be able to reduce the likelihood of rejections due to credit history by taking actions such as steps to improve the record before applying for credit or by applying to subprime lenders, who specialize in providing credit to those with poor credit history. At the same time, the likelihood of rejections due to DTI ratio may not change, as it is unlikely that consumers could boost their income strategically before applying for credit.

Similarly, an increase in cognizance would also affect applicants' tendency to withdraw mortgage applications that are still being processed (before the lender has made the decision). It is common for potential applicants to initiate several formal mortgage applications at once at different lenders to hedge against the uncertainty in approvals and mortgage terms. In doing so, they incur multiple non-refundable application costs, but in the end they take out a mortgage with only one lender and withdraw their applications from the others (in-process withdrawals). With an increase in cognizance of their creditworthiness, borrowers' uncertainty over approvals and credit terms decreases, and with that, they are likely to apply to *fewer* lenders at once. Thus the fraction of in-process withdrawals should decrease in the treated areas.[19]

[19] The withdrawal ratio over the 2000–2008 period is about 12%, indicating that in-process withdrawals are fairly common. Anecdotal evidence suggests that consumers tend to withdraw applications when they find a better offer from other lenders (Reddit Forum, n.d.). More importantly, credit reporting agencies do not penalize multiple applications if they are made within a short time period, as Equifax (n.d.) explains: "If you're shopping for a new auto or mortgage loan or a new utility provider, the multiple inquiries are generally counted as one for a given period of time. The length of this period may vary depending on the credit scoring model used, but it's typically from 14 to 45 days. This allows you to check at different lenders."

The first prediction is tested by regressing the fraction of total applications rejected for credit history and for DTI ratio. These outcomes are estimated separately for the entire sample and for a sub-sample of only those census tracts where the rate in the pre-event year 2004 was higher than the *regional mean*.[20] The reasons to separately focus on the *ex-ante* high-rejection areas are that the information in the reports are more valuable when the rejection rates are high, and the influence of the event on reason-specific rejection probabilities will be greater in the areas where rejections were frequent before the event. The second prediction is tested by regressing the withdrawal ratio, which is the fraction of total applications that are formally withdrawn by borrowers before lenders could make a decision.

These predictions are tested using the regression specification from Equation (1). Table (IV) shows the results. In Columns (1) through (4) we see that the fraction of applications denied due to credit history decreased by 0.3 percentage points in the treated tracts relative to the control, statistically significant in the *ex-ante* high-rejection-rate areas (Columns 3 and 4). The coefficients in columns (5) through (8) show that the DTI ratio denials did not decrease statistically significantly. Though the estimates carry only modest statistical significance, they indicate that the reason-specific rejection likelihoods changed in a manner consistent with potential borrowers becoming more cognizant of their credit history.[21] The estimates for the withdrawal ratio appear in Columns (9) and (10) and imply that it decreased by 0.9–0.11 percentage points in the treated tracts vis-à-vis the control.[22]

Overall, these findings point to an increase in mortgage-related cognizance among borrowers, consistent with the self-learning mechanism.

[20] The steps to calculate *regional mean* are as follows. A region is defined as the area encompassing a control (pre-FACTA) state and all the surrounding states. Consider the control state Colorado (CO) and all the surrounding treatment states. The regional mean for this region is the average rejection rate for the census tracts in all the counties at the border between CO and WY, UT, AZ, NM, OK, KS and NE. The regional means of rejection rates for all seven control states are calculated in this way, and a census tract is then classified as a "high rejection tract" if its rejection rate is more than the regional mean in 2004.

[21] A caveat of this analysis is that HMDA does not mandate lenders to report reasons for rejections, so if the reporting incentives of lenders were also influenced by the event, the estimates reported above would be the result of the changes in borrowers' cognizance and lenders' incentives. However, the incentives to report rejection reasons would need to change in the event year in a particular manner that varies across the treated and control areas, even for lenders that may operate in both areas. Such precise changes in incentives for reporting the reasons for rejections appear unlikely. Moreover, lenders reported reasons for rejection in 70.81% of the rejected applications in the sample.

[22] In economic terms, the drop is equivalent to ~2.34 fewer in-process withdrawals per treated tract or ~16,513 fewer withdrawn applications aggregated over the treated border counties. At an average cost of ~USD 400 per withdrawn application, this represents ~USD 6.6 million saving in upfront mortgage application fees.

(II) Proxy for Over- and Under-estimators of Creditworthiness

While the self-learning mechanism predicts that the number of mortgage applications after the event should decline for the overestimating borrower type and increase for the underestimating type, empirically testing these predictions is challenging for several reasons. First, most large-scale economic surveys do not collect information on such measures (Puri & Robinson, 2007), and such measures at regional level are not available. Exler, Livshits, MacGee, and Tertilt (2021) use financial literacy as proxy of over-optimism, whereas Puri and Robinson (2007) exploit the difference between the self-reported life expectancy and the measure derived from actuarial expectancy tables to estimate optimism. Unfortunately, these approaches cannot be applied in the context of the current paper. Second, the two borrower types are not distinguishable in the mortgage application or performance data. Third, since the prediction is that the underestimating type do not apply for mortgage before the intervention and the over-estimating type after the intervention, these two types of borrowers would not be captured in the pre- and post-event period systematically in the mortgage data.

Notwithstanding the above limitations, two area-based proxies are proposed to approximate over- and under-estimating borrower types: pre-event loan-to-income ratio (LTI) and pre-event rejection ratio. Then the predictions are tested by estimating the changes in the number of applications in the two areas.

Pre-event Loan-to-Income Ratio: The first proxy to classify the census tracts into under- and over-estimating types assumes that borrowers' decision to apply for mortgages is based on the LTI ratio. Potential borrowers may choose not to apply for credit when the LTI ratio of the credit application is too high, but the borrowers who over-estimate their creditworthiness may still apply. In this framework, there exists an optimum link between average LTI ratio of an area and expected number of applications in an area. If the number of applications is more than the optimum, the area has over-estimating borrowers, and if less, under-estimating borrowers.

Based on the above idea, first the following regression model is estimated in the pre-event year 2004 for all the treated areas.

$$\mathrm{N}_{icsjt} = \beta_0 + \beta_1 \mathrm{LTI}_t + \delta \times \text{Economic Controls} + \gamma_s + \varepsilon_{icsjt} \ . \tag{3}$$

The dependent variable is the scaled applications and the key independent variable is average LTI of a census tract calculated over all mortgage applications. Furthermore, state fixed effects

(γ_s) and the economic controls from before are added, and standard errors are clustered at the county level. Residuals from the above regression are then used for classifying the census tracts as over- and under-estimator types. The census tracts for which $\varepsilon_{icsjt} \geq 0$ are over-estimator type, and others are under-estimator type.

Having identified the census tracts into the two types, the change in scaled applications is evaluated next using the following regression:

$$\mathrm{N}_{icsjt} = \beta_0 + \beta_1 \mathrm{Post}_t + \delta \times \text{Economic Controls} + \gamma_s + \varepsilon_{icsjt} \ . \tag{4}$$

This regression is estimated over the pre-event year 2004 and the event year 2005 for the treated areas. $Post_t$ takes the value of 1 for the year 2005 and 0 for 2004. The remaining terms in the equation are the same as in Equation (3).

Table (V) shows the result of this regression. The coefficient in Column (1) suggests that the number of applications did not increase in the over-estimating areas after the event, whereas that in Column (2) suggests that applications increased by an order of magnitude more in the under-estimating areas after the event. This differential change in the number of applications in the two types of treated areas supports the self-learning mechanism.

Pre-event Rejection Pattern: The second proxy to classify the census tracts as over-estimating type relies on the rejection ratio in the pre-event year. The ideas is that borrowers in the areas where the *ex-ante* rejection ratios due to DTI ratio were small but due to the credit history were large are more likely to be the over-estimators, relative to the borrowers from other areas. This is because these rejection patterns fit the borrowers who mistakenly overestimate their credit-worthiness, apply for a mortgage, and thus are more likely to be rejected for (bad) credit history than for their repayment inability (high DTI). Following this reasoning, the census tracts in the sample are sorted into *tertiles* of the rejection ratios in the pre-event year 2004 for the DTI ratio and credit history, leading to nine sub-groups.

Table (VI) shows the results of separately regressing the scaled applications using Equation (1) for each of the nine sub-groups. Credit-history tertiles vary from the top to bottom of the table and DTI tertiles, from left to right. The overestimating sub-group corresponds to the third tertile of the credit history and the first tertile of the DTI ratio. We see that the treated areas saw the smallest increase in scaled applications relative to the control areas for the overestimating borrowers (6.60 versus 10.03 or 17.87, within the first DTI tertile) than the other borrowers.

This differential increase in number of applications is again in line with the prediction for the overestimating borrowers.

To summarize, using the two proxies above, areas that were more likely to have over- and/or under-estimating borrowers were identified, and the differential change in the number of applications in these two types of areas after the event supports the predictions of the self-learning mechanism.

4D Characterizing the Effect: Who benefits?

Characterization of the consumers and the areas that are more likely to benefit from easier access to credit reports may provide insights about those for whom the information frictions about creditworthiness are likely to be binding, and it may also be useful for policy targeting. The heterogeneity in the treatment effect across consumer creditworthiness and income is examined next.

(I) High creditworthy areas

Those borrowers who learn from their credit reports that they are not creditworthy would likely not apply for mortgage (drop-out effect). Since low-creditworthy areas are more likely to have such consumers, the stronger drop-out effect would lead to muted increase in applications such areas compared to high-creditworthiness areas after access to credit reports became easier. Similarly, approval ratio would increase more in high-creditworthiness areas, because high-creditworthiness borrowers are more likely to be approved for mortgage.

To test the differential treatment effect on applications and approval ratio in the areas with population of different creditworthiness, a county is classified as having high creditworthiness if its subprime population fraction is less than the *regional mean* before the event (Footnote (20) shows the steps to calculate regional mean). The year 1999 is chosen for the classification, because Mian and Sufi (2009) suggest that such classification should be done at a time well before the start of a housing boom, as creditworthiness of an area may endogenously evolve with the boom. The earliest year for which data on the county subprime fraction is publicly available is 1999 (FRBNY & Equifax, n.d.).

Table (VII) shows the results of regressing scaled applications and approval ratio separately using regression Equation (1) for counties with high and low creditworthiness. Within the *ex-ante* high-creditworthiness counties, vis-à-vis the control counties, the treated counties saw an increase of 16.84–18.04 (17.4–18.8%) in scaled applications (Columns 1 and 2) and a 2 percentage points increase in approval ratio (Columns 3 and 4). At the same time, within the *ex-ante* low-creditworthiness counties, vis-à-vis the control counties, the treated counties saw an increase of just 7.48–8.62 (7.8%–8.9%) in scaled applications (Columns 5 and 6) and a 1 percentage point increase in approval ratio (Columns 7 and 8), which is statistically significant at 10%. Taken together, these estimates support the self-learning mechanism and suggest that creditworthy borrowers are more likely to benefit from easier access to credit reports.

(II) Low income borrowers

The effect of easier access to credit reports may vary across borrowers of different incomes, because of three factors. First, the latent demand for credit for low-income borrowers is high, because lenders tend to exclude such borrowers from credit solicitations and because their propensity to lend to such borrowers is low (Agarwal, Chomsisengphet, Mahoney, & Stroebel, 2018). Second, the impact of mortgage rejection is higher for low-income borrowers, because the increased probability of future rejections resulting from a current rejection adversely affects them more than it affects the high-income borrowers. Third, lower income is associated with a higher likelihood to overestimate one's creditworthiness (Perry, 2008, Table III), hence such consumers are more likely to revise their creditworthiness downwards after learning their true creditworthiness from their credit reports. Thus, the number of applications is likely to increase by smaller amount for borrowers with low income. The approval ratio for them on the other hand is likely to increase due to drop out of over-estimating borrowers.

To test these predictions, first, the cut-offs for the income quartiles are calculated each year within the sample, and then the applications from each quartile are aggregated to the census-tract level and scaled by the population (measured in 1000's). The approval ratio is then calculated within each quartile.

Panel (A) of Table (VIII) shows the results of regressing the scaled applications separately for each of the income quartiles using Equation (1). We see that the scaled applications did not increase significantly for the lowest quartile, but increased significantly for the other three, and the increase was larger and statistically significant for these quartiles. The significant increase in applications among the higher quartiles but not among the lowest-quartile consumers is consistent with the prediction discussed above.

Panel (B) of Table (VIII) shows the results of regressing approval ratios separately for each of the income quartiles using Equation (1). We see that the ratios increased statistically significantly in the treated areas relative to the control areas only for the lowest income quartile borrowers, again consistent with the prediction.

(III) First-time homebuyers

First-time homebuyers tend to be younger adults. The 25$^{\text{th}}$ percentile and median of their age is 29 and 35 years, respectively (Raymond & Dill, May 20, 2015). Younger adults account for disproportionately high fraction of those lacking robust credit records (Federal Reserve Board of Governors, 2007, p.28). Hence, proportion of first-time homebuyers may increase after access to credit reports becomes easier.

The prediction for the first-time homebuyers is tested next. The outcome variable is defined as the ratio of the number of mortgages taken out by first-time homebuyers to the number of all originated mortgages that had known information on first-time homebuyer status. It is important to enumerate two limitations of this analysis. Whether an applicant is a first-time homebuyer is recorded only in the GSE data, not in the HMDA data. Also, since the property location information in the GSE data is only available at the 3-digit zip code level, the properties were mapped to the counties using simplified approximations (see Footnote (29) in Data Appendix). Accordingly, the regression is specified at the zip3-state level as follows:

$$\mathrm{Y}_{zsjt} = \beta_0 + \beta_1 \times \mathrm{Treat}_{zsj} \times \mathrm{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt} \,. \qquad (5)$$

Here, z indexes the areas delineated by a 3-digit zip code at the border of treated state s and control state j. α_{zs} is zip3-state fixed effects. γ_{jt} is the *Border*$\times$*Quarter* fixed effects, and it serves similar function as that of the *Border*$\times$*Year* fixed effects in Equation (1). The sample is limited to the zip3-state areas that come under the border counties of treated and control states.

Columns (1) and (2) of Table (IX) show the regression results. The coefficients suggest that the percentage of first-time homebuyers increased by 1 percentage point in the treatment areas relative to the control areas.[23] This finding is in line with the prediction that, following the event, more new entries should occur in the treated areas.[24]

4E A Demand- or Supply-side Effect?

The findings so far indicate that the likely explanation for the increase in origination in the treated areas relative to the control after the event is the self-learning mechanism, which is a demand-driven channel. However, a supply-driven explanation is also plausible. Even though it was only the consumers for whom credit reports became easier to access, lenders too understood this and could have responded by increasing the mortgage supply.

Even though the supply-driven explanation is plausible, many of the earlier findings favor the demand-driven explanation. First, an increase in applications and a decrease in in-process-applications withdrawals are a result of decisions that are taken solely by potential borrowers, and these quantities are mostly independent of lenders' influence. Second, it is the demand-driven mechanism under which the effects would be heterogeneous across consumer characteristics, as it was across creditworthiness and income. Additionally, given that the propensity of lenders to extend credit to low-income borrowers is low (Agarwal et al., 2018), and given that in the current setting we see that the approval ratio increased significantly for such borrowers vis-à-vis the high-income borrowers, the supply-driven explanation appears unlikely.

Notwithstanding the above suggestive evidence favoring the demand-side explanation, two outcomes more directly related to supply-side characteristics are examined next: mortgage interest rates and heterogeneous effect by *ex-ante* density of mortgage lenders.

(I) Mortgage interest rates on the GSE-repurchased mortgages

The changes in the mortgage interest rates after the event in treated and control areas can be utilized to examine whether the increase in origination was supply- or demand-driven. If it is the former, the rates would decrease; if the latter, they would increase.

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[23] About 6.7% of the observations within the hombebuyer data sample pertaining to the bordering counties do not have information on first-time homebuyer status. The specifications that alternatively define the outcome variable as the ratio of number of first-time homebuyers to *all mortgages* yield similar estimates, and these estimates are left unreported for brevity.

[24] A concern with this estimation is that the mortgage sample consists of only those that were purchased by the GSEs. However, as argued before, this selection would be an issue only if GSEs' incentives to purchase first-time homebuyer mortgages relative to their overall purchase from the treated counties increased relative to the control counties from the event year 2005 onward. Such a time- and location-specific change seems improbable.

The investigation of the rates needs to account for characteristics of the property and borrower-risk. Scharfstein and Sunderam (2016) argue that the prices (interest rate) at which lenders sell *conforming* mortgages to the GSEs materially vary only across three dimensions: credit score, loan-to-value ratio (LTV), and loan type (adjustable rate, fixed rate etc.).[25] Therefore, the residuals obtained from a regression of the rate on these dimensions approximately measure the lender-specific pricing schedule independent of the characteristics of borrowers and the mortgage. In the context of the current paper, only the first two dimensions are relevant, since the GSE sample only includes one type of mortgage—the 30-year fixed-rate single-family mortgage. Thus, when the rate is regressed on credit score, LTV, and the $Treat \times Post$ interaction term, the coefficient on the last term captures the change in the pricing schedule of lenders in the treated areas vis-à-vis the control areas after the event. If the lenders lowered the mortgage interest rates in the treated areas (in a bid to increase mortgage origination), the sign on the coefficient would be negative; if they raised the rates, the sign would be positive.

The regression specification is similar to Equation (5), but it is now specified at the loan level i as follows:

$$\text{Interest Rate}_{izsjt} = \beta_0 + \beta_1 \times \text{Treat}_{izsj} \times \text{Post}_t + \delta \times \text{Controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{izsjt} \ . \qquad (6)$$

Columns (3) and (4) of Table (IX) show the results of the regression. In Column (3), *Controls* are the two relevant pricing dimensions, credit score and CLTV (combined loan-to-value: the loan-to-value ratio inclusive of all loans secured by a mortgaged property); and in Column (4), to make the specification more rigorous, *Controls* additionally include DTI ratio, number of units comprising the mortgaged property, and percentage of mortgage insurance. Results show that the coefficients on $Treat \times Post$ in both the columns are positive and significant, at about 0.009–0.01 percentage points. Thus, if anything, lenders responded to the event by increasing, not decreasing, the risk-adjusted mortgage interest rates in the treated areas relative to the control, contrary to what a supply-driven explanation would predict.[26]

[25] Fannie Mae's mortgage pricing variation across these dimensions can be seen in its pricing schedule here: https://singlefamily.fanniemae.com/media/document/pdf/llpa-matrix-pdf

[26] The magnitude of the increase in the rates is tiny, potentially because interest rates on conforming (GSE-repurchased) loans do not vary across regions or with dimensions other than FICO scores, loan-to-value ratio, and loan type (Hurst, Keys, Seru, & Vavra, 2016).

(II) Heterogeneous effects by *ex-ante* density of mortgage lenders

If the increase in mortgage origination were driven by lenders, it would be greater in areas where the *ex-ante* density of lenders is high. To examine this, first, census tracts are classified into high and low lender-density groups: high if the number of HMDA mortgage lenders per adult in the pre-event year 2004 in a census tract was greater than the *regional mean* (defined in Footnote (20)), and low otherwise.

Columns (1) through (4) of Table (X) show the results of separately regressing, using Equation (1), the dollar origination volume (in 1000 USD) per adult for the two density groups. The estimates are smaller in magnitude and have weaker statistical significance for high-density tracts (Columns 2 and 4) vis-à-vis the respective low-density census tracts (Columns 1 and 3, respectively). Thus, among the *ex-ante* high-lender-density census tracts, relative to the control tracts, the treated tracts saw a much *smaller* increase than the treated tracts from the low-lender-density group relative to the respective control tracts. Moreover, the t-test for the difference in the coefficient of $Treat \times Post$ in high- and low- lender-density areas ($High - Low$) shows no statistical difference.

Columns (4) through (8) of the table show the above regressions for the approval ratio. The results are similar—there is no statistical difference in the increase in the approval ratio in areas with a high or low lender density.

The finding that the effects were stronger, not in the areas that had *ex-ante* high lender density, but in the areas that had an *ex-ante* low lender density is inconsistent with the supply-driven explanation.

5 Supplementary Discussion

5A Did origination increase due to rise in private mortgage securitization?

As private securitization of mortgages (selling the mortgage to non-government agencies) offers lenders higher commissions, a rise in the tendency to privately securitize could explain the increase in the mortgage origination of reported in the current paper (Keys, Mukherjee, Seru, & Vig, 2010). If this explanation is true, the fraction of originated mortgages that were sold to non-government entities would increase in the treated areas relative to the control after the event.

Table (XI) shows the result of regressing, using Equation (1), three outcome variables: the fraction of total applications that lenders originated and (i) sold to non-government entities, (ii) sold to the four GSEs (Fannie Mae, Freddie Mac, Ginnie Mae, and Farmer Mac), and (iii) did not sell. The estimates suggest that private securitization did not increase (Columns 1 and 2); government securitization increased (Columns 3 and 4); and the fraction of unsold mortgages did not change (Columns 5 and 6). Overall, private securitization does not appear to a reason behind the increase in origination.

5B Did origination increase due to subprime lending? Credit score-based evidence

It may be argued that the increase in mortgage origination was due to an increase in the subprime credit (Mian & Sufi, 2009). Using the comprehensive HMDA data and location-based proxies of creditworthiness, Table (VII) already suggests that the effect of free credit reports was stronger in the prime counties/census tracts than in the subprime. Even though these proxies are informative and widely used (e.g., Di Maggio & Kermani, 2017; Mian & Sufi, 2009), they are imprecise. The GSE sample contains the application-level credit scores, and thus it can be used to precisely examine the patterns in prime and subprime origination.

Table (XII) shows the results of regressing separately the number of prime (credit score $\geq$ 620) and subprime *originated* mortgages in zip3-state areas using Equation (5). Columns (1) and (2) show that the number of prime mortgages increased by 308–312 in the treated zip3-state areas relative to similar control zip3-state areas, whereas columns (3) and (4) imply that subprime mortgages increased only by ~10 applications, which is 30 times smaller. Thus the increase in mortgages did not disproportionately go to subprime consumers. Note that these estimates are not directly comparable to the previous regressions, as the observation unit here is zip3-state, not census tracts, and the outcome variable is not scaled by population. Also, these results suffer from the same selection issue that existed with the previous results utilizing the GSE sample, and the same argument that was made before—the implausibility that the incentives of GSEs changed across the contiguous sample counties around the event—alleviates it. In addition, Elul, Gupta, and Musto (2020) show that to combat the onset of the housing bust before 2007, the GSEs sought to buy more subprime, not prime, mortgages. Hence, had the GSEs not changed their buying pattern, the estimates of subprime origination would have been even lower and the contradiction of the subprime hypothesis even stronger.

5C Effect on banks

The analysis in the paper so far has focused on evaluating the effects on borrowers, but it is the lenders who ultimately evaluate the credit decisions, and hence the effect on banks is evaluated next.

Analyzing the effects on banks has a few notable limitations. First, banks are not the dominant mortgage originators. Despite being 80% of mortgage lenders by number, banks accounted for just 37% of mortgage lending in 2005 (Avery, Brevoort, & Canner, 2007), and thus the conclusions drawn from studying banks may not generalize to all mortgage lenders. Second, since many banks operate across states, their treatment and control status in this natural experiment is not binary, but continuous. To solve this issue, it is assumed that the continuous treatment intensity is proportional to a bank's *ex-ante* mortgage origination in the treated states relative to the activity in treated and control states combined. Following this, a bank is classified as "control" if in the pre-event year 2004 the ratio of mortgage amounts it originated in the control states to those in the treated and control states combined was larger than the cross-sectional median in that year across all banks in the sample. The regression equation is

$$Y_{bt} = \beta_0 + \beta_1 \times \text{Treat}_b \times \text{Post}_t + \delta \times \text{Bank controls}_{bt} + \alpha_l + \gamma_t + \varepsilon_{bt} \ , \tag{7}$$

where Y_{bt} represents the five outcome variables: net interest margin (NIM), return on equity (RoE), return on assets (RoA), marketing and professional expenses, and savings deposit; b indexes the banks; $Treat_b$ is 1 if a bank is treated and 0 otherwise; $Post_t$ is 1 if $year \geq 2005$ and 0 otherwise; year t represents year-quarter; α_i is bank fixed effects; γ_t is year-quarter fixed effect; and *Bank controls* include banks' log total assets, share of liquid assets to total assets, and cost of deposit.[27]

The regression results in Table (XIII) show that treated banks saw a 10–11 basis-points increase in NIM (Columns 1 and 2), a 0.88–0.97 percentage-points increase in RoE (Columns 3 and 4), and a 0.07–0.08 percentage-points increase in RoA (Columns 5 and 6). At the same time, marketing expenses decreased by about 52–56 thousand USD (Columns 7 and 8), and

[27] NIM is the ratio of net interest income (sum of RIAD4074 and RIAD4301) to earning assets. I use the definition of earning assets from St. Louis Fred: it is the sum of RCFD0071, RCFD1350, RCFD2122, RCFD3545, RCFD1754, and RCFD1772 (https://fred.stlouisfed.org/series/USNIM). RoE is the ratio of net income (RIAD4340) to book value of equity. RoA is the ratio of net income to book value of total assets. Liquid assets is the sum of RCFD1754, RCFD1773, RCFD3545, RCFD1754, RCFD3545, and RCFD1350. Marketing and other professional services is RIAD0497, expressed in 1000 USD. Saving deposits is RIAD0093, expressed in 1000 USD. Cost of deposit is the ratio of RIAD4073 to earning assets.

saving deposits decreased by 0.77–0.86 million USD (Columns 9 and 10). These results are qualitatively and quantitatively similar when lenders are classified into treatment and control groups using cross-sectional mean instead of the median.

All in all, the effect of the event on financial performance of the banks seems to be positive.

5D Survey evidence on current trends in credit report usage and discouraged borrowers

A representative survey of U.S. consumers, the SCE Credit Access Survey captures the usage of credit reports and scores among consumers and also their planned credit usage. The rotating panel structure of the survey allows for regression analyses that can accommodate fixed effects and clustering of the standard errors at the *Year*$\times$*Month* level. Furthermore, sampling weights allow one to make inferences that can be generalized to the population. The data used for this analysis span from 2013 to 2020.

Columns (1) through (3) of Table (XIV) throw light on the average usage of credit scores and credit reports. To do this, an indicator (dummy) variable for a given characteristic is regressed on a constant. In Column (1) the indicator variable is 1 when a respondent has never checked or requested a credit report and 0 otherwise; in Column (2) it is 1 when the respondent has either never checked his or her credit report or checked it at least more than two years ago (infrequent checkers), and it is 0 otherwise; and in Column (3) it is 1 in when respondent does not know his or her credit score and 0 otherwise. The estimated value of the constant in each case is rather startling: an estimated 8% of the population has *never* checked or requested a credit report (Column 1), about 20% of the population are infrequent checkers of credit reports (Column 2), and almost 12% of the population does not know own credit score (Column 3).

Columns (4) through (6) of Table (XIV) analyze the phenomenon of discouraged borrowers. One of the questions in the survey asks how likely the respondent is to take out a mortgage and related credit in the next 12 months. Those who are very or somewhat unlikely to do so, or those who assign a less than 10% probability to it are asked for the reason. Defining the indicator variable *discouraged borrowers* as 1 for those who respond "I don't think I would get approved", and then regressing it on a constant provides an estimate of the discouraged borrowers. The regression result in Column (4) suggests that among those not planning to take out mortgages, the fraction of those who are doing so because they are discouraged is about 13%. Furthermore, an indicator for discouraged borrowers is regressed separately on the dummy variable for *infrequent checkers* and for *unawareness* about credit score. Columns (5) and (6) show that the coefficient on both the independent variables is positive and significant, i.e. infrequent checkers and those unaware of their credit score are respectively 3% and 5% *more likely* to be discouraged.

Taken together, these findings imply that the low usage of credit reports and scores remain non-trivially prevalent among retail consumers even two decades after the reports became free, and the tendency of not seeing one's credit reports also contributes to potential borrowers becoming discouraged from applying for credit.

5E Alternative Mechanisms

(I) Salience

It is plausible that the establishment of the website increased the salience among consumers about their creditworthiness. The website explicitly provided educative information on credit reports and scores, and the role these play in a credit applications process (Panel B of Figure I). At first glance it appears that this educative information itself could lead to the effects documented in this paper. However, while an increase in salience would make consumers more aware of their creditworthiness, consumers would need to act on the creditworthiness information in their reports for the credit market outcomes to change. At the same time, as the website simultaneously increased educative awareness and made it easier to access the reports, separating the effects of salience from self-learning becomes challenging. The findings in this thus paper should be interpreted as coming from an increase self-learning coupled with an increase in salience.

(II) Borrowers know their true type but do not know that lenders know it

An alternative mechanism based on asymmetric information in which borrowers *privately know* their true creditworthiness type, but do not know what lenders know about them, is plausible. Using free credit reports, borrowers learn that the creditworthiness information on them available at lenders are proportional to their true creditworthiness type. Hence, under the non-trivial search/application cost, borrowers with poor creditworthiness (bad type) self-select out. The applicant pool now improves relative to the situation in which borrowers do not know that a lender has information about their true type and optimistically expect that the information is better than what is warranted by their credit reports. Note that the improvement occurs here due to the self-selecting-out by bad type, but not by self-selecting-in by good type, since all borrowers *privately know* their true type. However, since under the self-learning mechanism, borrowers themselves have imperfect information of their true type, both selecting-in by good borrowers and selecting-out by bad borrowers contribute to pool improvement after credit reports become free.

The empirical findings are consistent primarily with the self-learning mechanism. We saw that in the treated areas both the mortgage applications and first-time homebuyers fraction increased, not decreased. Both these findings provide evidence of selecting-in by borrowers, which is plausible only under the *self-learning* mechanism.

Another valid concern is that in assessing mortgage applications, together with the credit reports, lenders use private information that they may accumulate through relationship lending. This attenuates the effects of free credit reports. The concern is partially alleviated by the fact that lenders necessarily look at credit reports and scores when assessing credit applications.[28]

5F Market-based Solutions for the Lack of Creditworthiness Information among Consumers

Making it easier to access credit reports for consumers appears to impact positively the consumers and lenders. A question then arises, whether financial intermediaries or targeted financing arrangements could have solved the issue of lack of creditworthiness information among borrowers? While empirically answering this question is challenging, it appears that mortgage brokers are one such intermediary who could provide creditworthiness-related education to the potential borrowers. It is likely that they do provide such education to some extent, but with a cost that may be as high as 3% of the mortgage amount. Similarly, banks and other lenders too could provide some education to consumers seeking advice related to credit.

One reason an intermediaries-based solution by-and-large did not emerge to educate consumers of their creditworthiness information could be the lack of incentives for such intermediaries. To understand this, let us consider a few hypothetical intermediary business models. An intermediary could offer to educate all consumers at no cost to them of their creditworthiness and charge them a commission for education when they formally make a mortgage application. Such an arrangement would be tenuous, because consumers who learn that they are creditwor-

[28] Experian (n.d.) explains: "Not all lenders think the same way, and they may have different ways of making their decisions. But all of them will look at some key factors to help them decide. These include: information on your credit report including your credit history and public record data."

thy would have the incentive to mis-report that they are not seeking (mortgage) credit, and then they would shop for credit from other lenders. At the same time, other lenders have the incentive to compete for these aware creditworthy consumers, facilitating the consumer's search for credit and taking away any potential rent the intermediary could have sought. In addition, such an intermediary-cum-lender would face a serious conflict of interest to mis-educate consumers that their creditworthiness is low in order to lure them into accepting high-cost credit product and to discourage them from shopping at other lenders.

An alternative business model for an intermediary could be where it charges a cost to consumers to educate them of their creditworthiness. Mortgage brokers likely provide this service, bundled with other mortgage-related assistance. This model does not address the issue of discouraged borrowers—consumers who hold the misperception that their creditworthiness is not good would end up not availing this service. Moreover, just as the business model discussed previously, this arrangement suffers from the conflict of interest issue.

In summary, educating consumers of their creditworthiness through supply-side solution (e.g., through banks) and intermediary-based solution (e.g., mortgage brokers) are feasible, but a robust market solution did not emerge likely due to absence of incentives for such market participants. This gap was filled through the regulatory means providing creditworthiness information for free to all consumers, and the mortgage market outcomes improved as a result.

5G Robustness

Since the natural experiment utilized in this paper occurred in the year 2005, the sample period is chosen to be from 2000 to 2008 to allow for enough post-experiment observations. As the experiment is close to the financial crisis of 2008, it is crucial to ensure that the results are not caused by the unique lending environment that existed in 2007–2008. To this end, all the regressions were re-estimated by excluding the observations for the years 2007 and 2008. Mostly the results are qualitatively and quantitatively similar and are left unreported for brevity.

6 Conclusion

Several large-scale surveys in the U.S. indicate that a non-trivial proportion of consumers do not check their credit reports and do not know their credit scores. At the same time, data from retail mortgage market reveal patterns that suggest that many consumers misjudge their creditworthiness when making mortgage decisions. Motivated with the idea that credit reports may aid consumers to self-assess their creditworthiness, this paper uses a natural experiment to examine how do the mortgage market outcomes change when accessing their credit reports becomes easier for consumers.

The federal *Fair and Accurate Transactions Act of 2003* (FACTA) has made access to credit reports easy and free through a website since 2005 for all U.S. consumers. It took just a few clicks to access one's reports at the website, whereas earlier one needed to make a request for a report by calling or mailing a letter to the credit reporting agencies. Even after successfully making the request, one often needed to wait more than a week to receive the reports through mail. Prior to FACTA, seven states had local laws that made it easy for state's residents to access their credit reports and drove the usage of credit reports locally many times higher relative to the usage in the rest of the states.

This paper exploits the pre-existing differences in consumer usage of credit reports in the states where the reports were free versus where it was not in a difference-in-differences setting. The counties at the border between the early-adopting states and the contiguous states constitute the sample, former serving as the control group and the latter as the treatment group. The primary finding is that the applicant pool in the mortgage market improved after it became easier for consumers to access their credit reports, as approval ratios increased and subsequent delinquencies decreased. Moreover, mortgage applications increased, more credit was originated to creditworthy borrowers, more first-time homebuyers took out mortgages, and financial performance of mortgage-lending banks improved.

Taken together, the findings support the idea that easier access to credit reports to consumers aids them in making better mortgage-related decisions. Any policy intervention aimed at educating consumers of their creditworthiness may bring about similar improvements in other retail credit markets as well.

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Figure I: The Annualcreditreport.com Website

Panel (A) of this figure shows a screenshot of the homepage of the website www.annualcreditreport.com, as it existed on September 25, 2005. **Panel (B)** of this figure shows a screenshot of the Frequently Asked Questions section of the website, as it existed on March 18, 2006. Both the screenshots are taken from the archive of the website stored at the Wayback Machine.

Panel A: Homepage of the Website

Panel B: Frequently Asked Questions Section of the Website

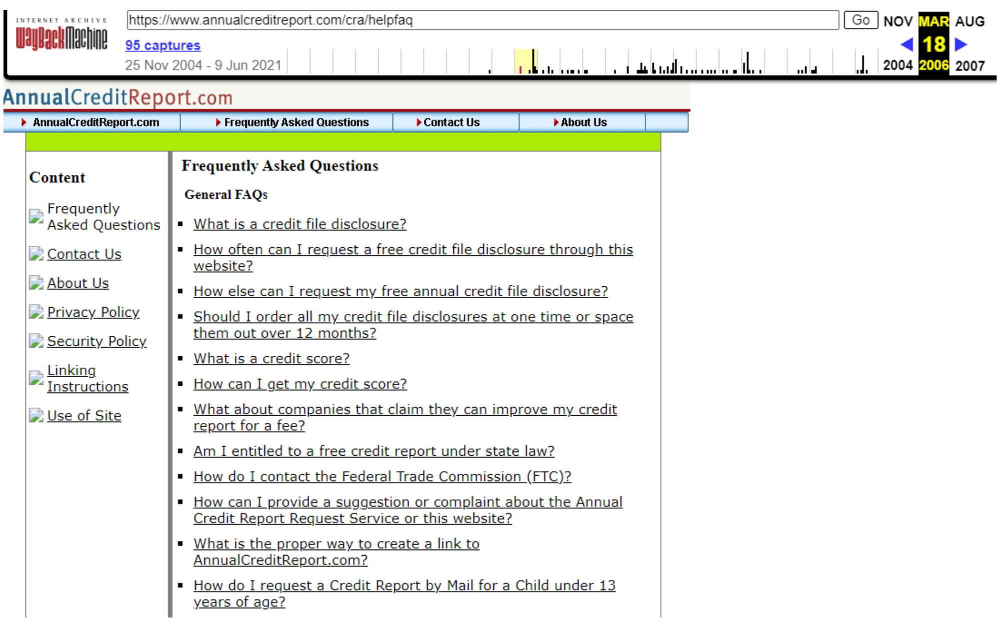


Figure II: A Sample Credit Report

This figure shows the summary page of a free credit report obtained from the website www.annualcreditreport.com for free under the *Fair and Accurate Transaction Act of 2003*. The specific credit history-related details are not shown. The report contains, among other things, the details of the consumer's active accounts, debt-to-credit ratio, and an indication of the available borrowing capacity.

1. Summary

Review this summary for a quick view of key information contained in your Equifax Credit Report.

Report Date	Apr 14, 2020
Credit File Status	No fraud indicator on file
Alert Contacts	0 Records Found
Average Account Age	5 Months
Length of Credit History	8 Months
Accounts with Negative Information	0
Oldest Account	DISCOVER BANK (Opened Aug 29, 2019)
Most Recent Account	AMERICAN EXPRESS (Opened Jan 10, 2020)

Credit Accounts

Your credit report includes information about activity on your credit accounts that may affect your credit score and rating.

Account Type	Open	With Balance	Total Balance	Available	Credit Limit	Debt-to-Credit	Payment
Revolving	2	2	$606	$11,044	$11,650	5.0%	$70
Mortgage							
Installment							
Other							
Total	2	2	$606	$11,044	$11,650	5.0%	$70

Other Items

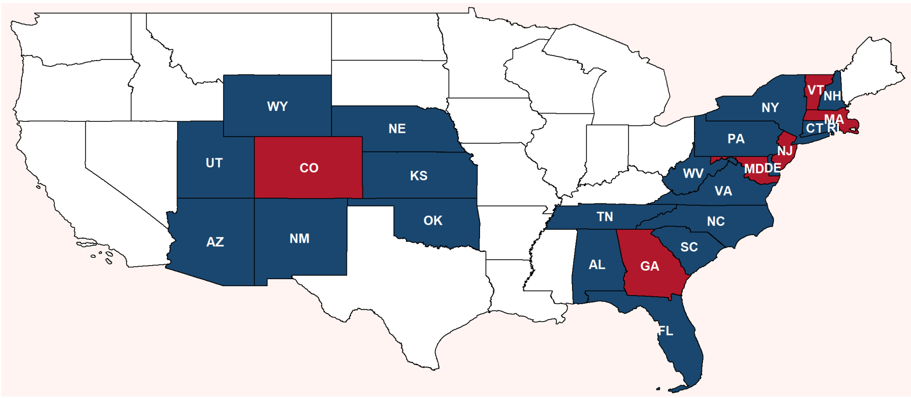
Your credit report includes your Personal Information and, if applicable, Consumer Statements, and could include other items that may affect your credit score and rating.

Consumer Statements		0 Statements Found
Personal Information		3 Items Found
Inquiries		2 Inquiries Found
Most Recent Inquiry	DISCOVER BANKAug 27, 2019	
Public Records		0 Records Found
Collections		0 Collections Found

Figure III: Empirical Research Design

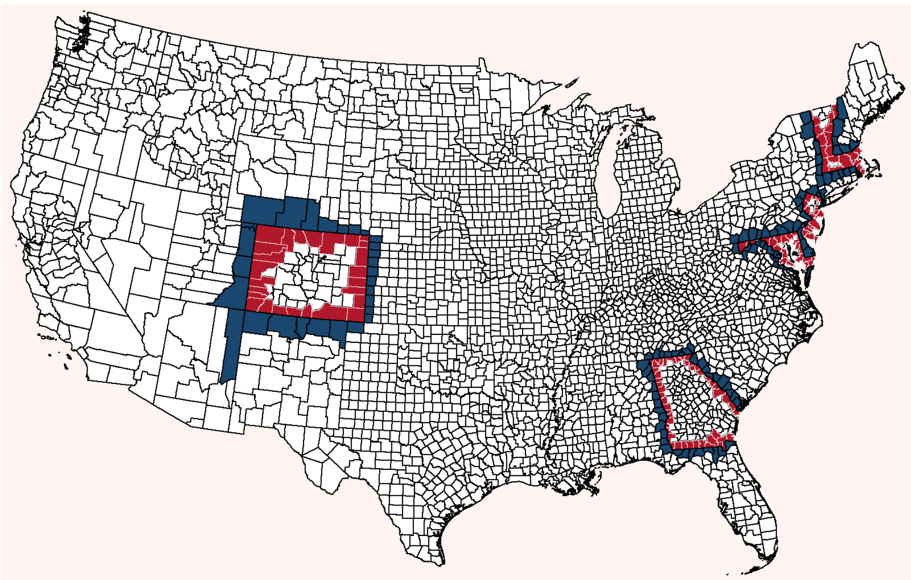
Panel (A) of this figure shows on the map of the contiguous U.S. the states utilized in the difference-in-differences (DID) setting. Seven U.S. states had enacted free credit report laws prior to the FACTA enactment in 2004: CO (1997), GA (1996), MD (1992), NJ (1997), MA (1995), VT (1992), and ME (2003). All except ME constitute the control group, and the 26 states surrounding the control group are the treatment. **Panel (B)** of this figure shows on the map of the contiguous U.S. the counties included in the estimation sample. These are the counties at the border between the treatment and control states.

Panel A: Treatment and Control States



Control: Pre-FACTA States
Treatment: States surrounding the pre-FACTA states

Panel B: Sample Counties from the Treatment and Control States



Control: Counties at the border of pre-FACTA States
Treatment: Counties which belong to the bordering states and are adjacent to the control counties

Figure IV: Examining the Parallel Trends

This figure shows the coefficients β_k from regressing *Approval Ratio* using the following specification:

$$Y_{icsjt} = \beta_0 + \sum_{k=T-3}^{T-1} \beta_k \text{ Treatment}_{icsj} \times \text{Event}_k + \sum_{k=T+1}^{T+4} \beta_k \text{ Treatment}_{icsj} \times \text{Event}_k + \alpha_i + \gamma_{j,t} + \varepsilon_{icsjt} ,$$

where $\text{Event}_k = 1$ if $t = T - k$. $\text{Event}_k = 0$ if $t \neq T - k, k = \{-3, 4\}$. $T =$ Event year 2005. Coefficients are estimated with respect to the base year 2004 ($j = 0$). The x-axis shows year relative to the pre-event year 2004; i.e., T = +1 is the first treated year, 2005. The y-axis shows the coefficients β_k. The 95% confidence intervals of $\beta_k s$ are also shown. The regression includes *Border*$\times$*Year* and *Census Tract* fixed effects. Other terms in the equation are the same as those in Equation 1. Standard errors are clustered by county.

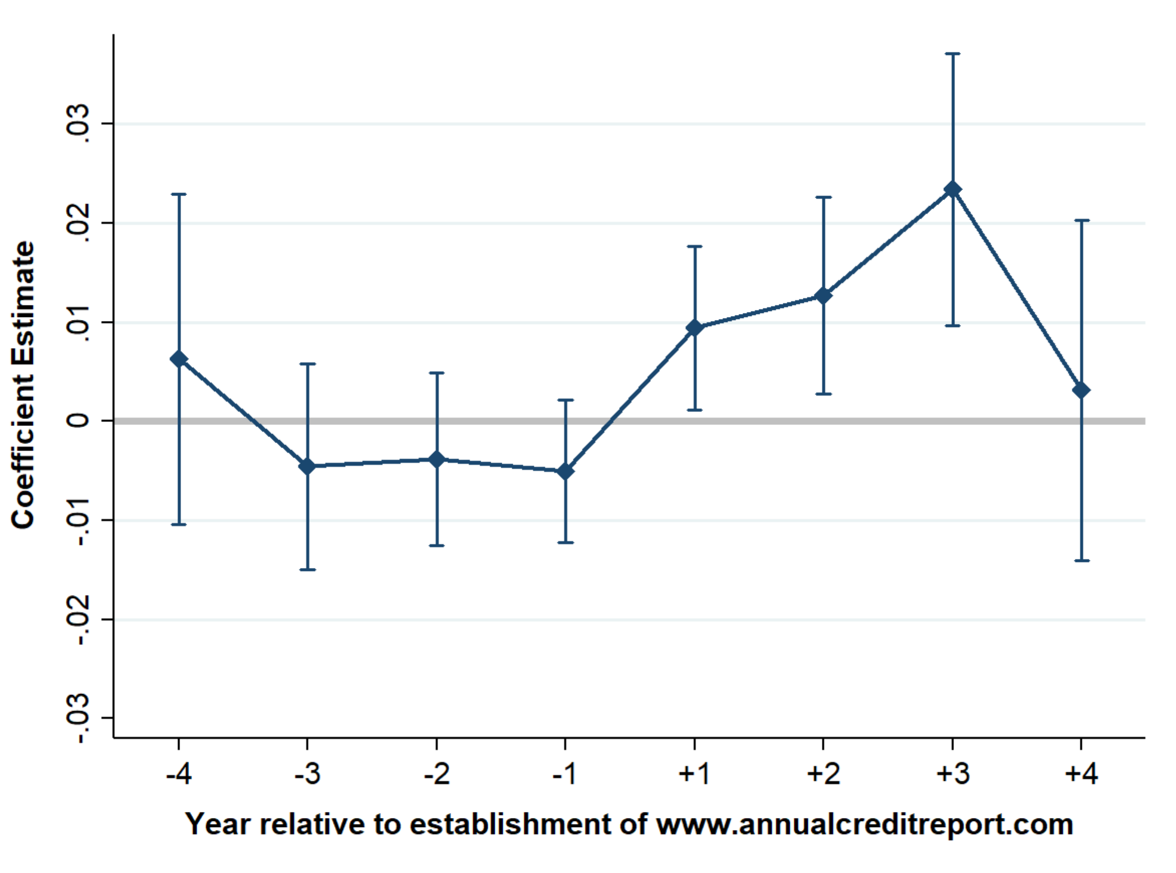
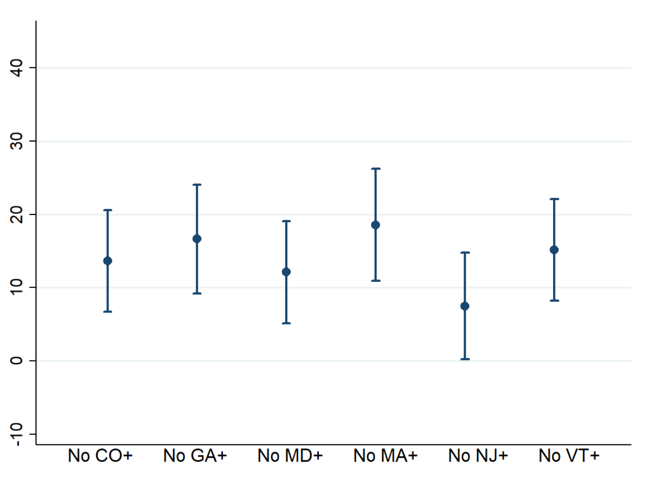


Figure V: Subsample Analysis

Panel (A) of this figure shows the estimates for changes in the number of applications per 1000 adults (scaled applications) in the sub-samples formed by removing from the full sample one by one each of the six regions (area encompassing a given control state and the corresponding contiguous treated states). **Panel (B)** of this figure shows the estimates for changes in approval ratio in the sub-samples formed by removing from the full sample one by one each of the six regions (area comprising of a control state and the bordering treated states). For example, the coefficient corresponding to "No CO+" respresents the estimate when Colorado and its surrounding states are removed from the estimation sample. The regressions specifications behind the estimates are the same as those in Table(II). The bands around the estimates show 95% confidence intervals.

Panel A: Number of Applications (per 1000 Adults in a Census Tract)



Panel B: Approval Ratio

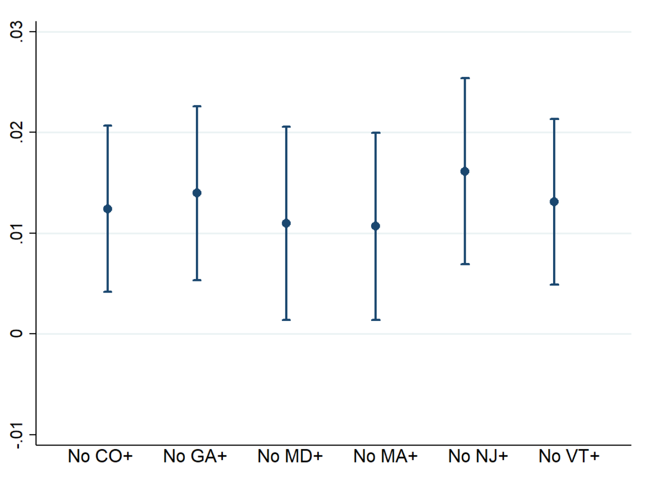
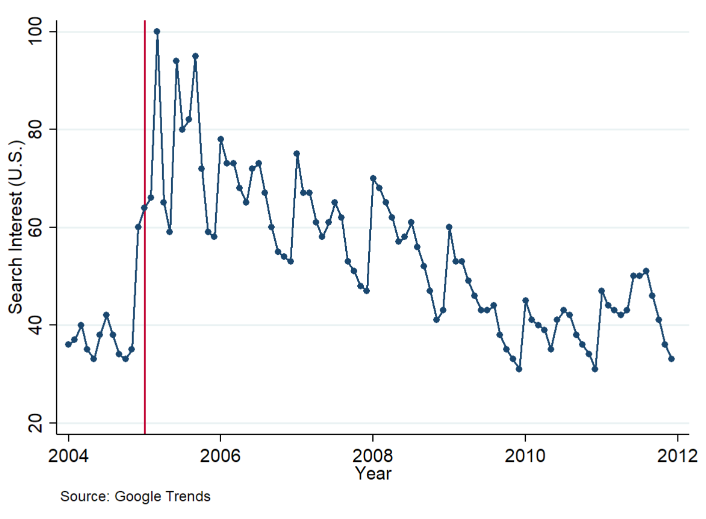


Figure VI: Search Interest in Free Credit Reports: Google Trends

This figure plots the search interest in free credit reports using Google Trends data. **Panel (A)** of this figure shows the plot of *Search Interest* for the keyphrase *Free Credit Report* in the US from Jan 1, 2004 till Dec 31, 2011. Numbers on the vertical axis represent search interest relative to the highest point on the chart during this period. A value of 100 (50) represents the peak popularity (half of the peak popularity) for the keyphrase. A value of 0 means there was not enough data. **Panel (B)** of this figure shows the difference in the mean search interest for treated and control states for the same keyphrase from 2004 to 2008 using the interest-by-subregion data from Google. These data are computed within the time period for which the data are extracted from Google. A value of 100 represents the location with the highest popularity of the search term as a fraction of total searches in that location, and a value of 50 indicates a location where it is half as popular. To overcome the issue of data-value normalization by Google, first, the data were *separately* extracted for each one-year interval, and then the means were *separately* calculated within each time interval for each of the two sets of states.

Panel A: Time Series Search Interest in the U.S. for the Terms "Free Credit Report"



Panel B: Cross-sectional Search Interest for the Terms "Free Credit Report"

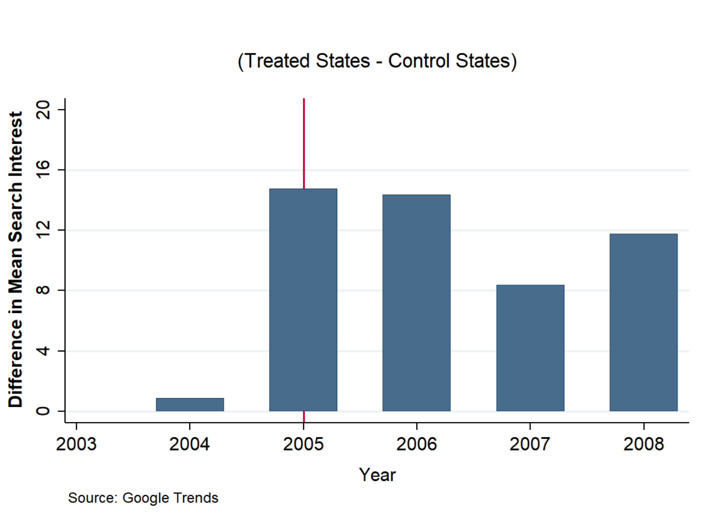
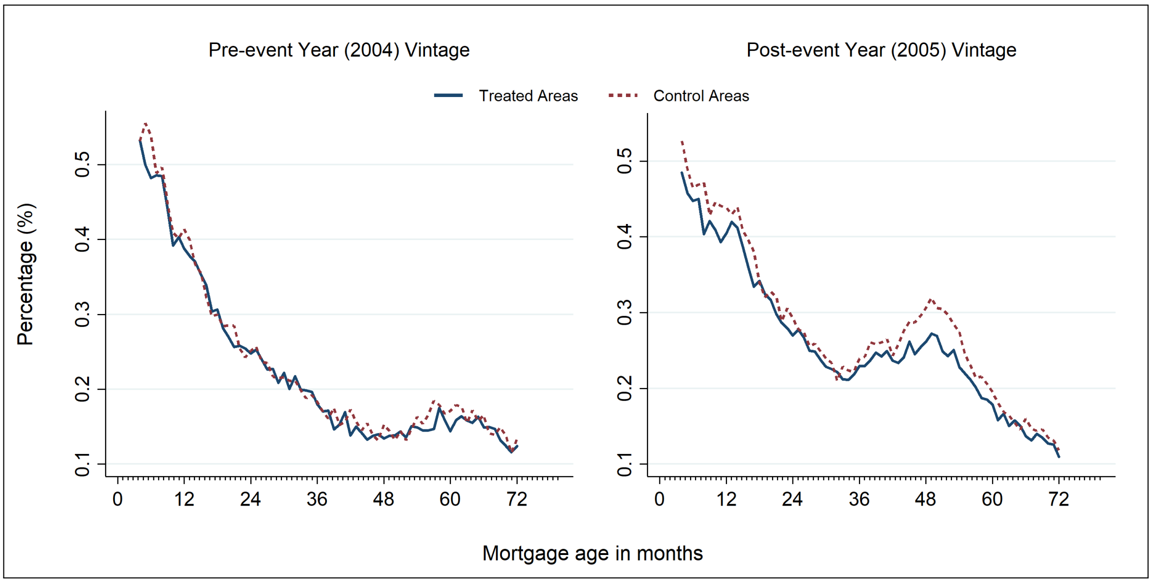


Figure VII: Effect on Mortgage Delinquencies

This figure plots the delinquency rates of various mortgage vintages with their age (measured in months). A mortgage vintage is a collection of mortgages originated in a given area—treated or control—in a given year—2004 (pre-event) or 2005 (post-event). Delinquency rate is calculated at each age as the ratio of the number of mortgages becoming delinquent for the first-time to the total number of mortgages in the respective vintage. **Panel (A)** shows 30–59-day delinquency rates separately for treated and control areas for 2004 on the left-hand side and for 2005 on the right-hand side. **Panel (B)** shows the difference between the monthly delinquency rate of treated and control mortgages in a given month (calendar time). These plots are based on the 30-year fixed-rate single-family mortgages purchased by Fannie Mae and Freddie Mac.

Panel A: 30–59-day Delinquency Rate



Panel B: Adjusted Delinquency Rate

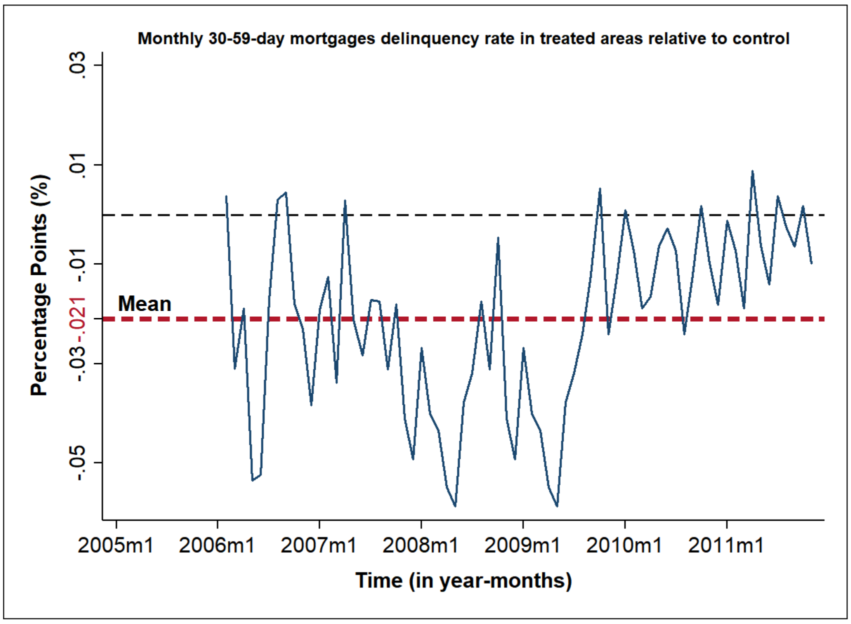


Table I: Summary Statistics

Panel A shows the statistics for each variable for the entire time period (2000–2008) computed separately for full sample and for treatment and control groups. Panel B shows the statistics for growth rate (trend) of each variable over the pre-treatment period (2000–2004) and the p-values obtained from a t-test for the difference in the growth rates of each variable across control and treatment groups. *Scaled applications, N,* is the number of mortgage applications in a census tract scaled by the population aged 18 to 64 years in the tract (scaled applications). *Approval ratio* (*Aprv.*) is the ratio of the number of successful applications (action type "1" *or* "2" in the HMDA dataset) to the number of total applications in a census tract. *Deny Credit Hist Ratio* and *Deny Debt-to-inc Ratio* are the ratio of applications denied due to credit history and debt-to-income ratio, respectively, to the number of total applications in a census tract. *Withdrawal Ratio* is the ratio of applications expressly withdrawn by the applicant to the number of total applications in the census tract. The bottom five variables together are referred to as *Economic Controls* in the regressions. These are: (i) *Num. Lenders* (*log*), the number of unique mortgage lenders in a census tract (expressed in natural log); (ii) *House Price Index*, a county-level house price index (version 2000) from Bogin et al. (2016); (iii) Δ *Inc per capita*, the annual growth rate of income per capita at the county level; (iv) Δ *Emp.*, the annual growth rate of employment by all establishments at the county level; and (v) Δ *State GDP*, the annual growth rate of the state gross domestic product.

Panel A: Full Sample (2000 – 2008)

	Full Sample				Treatment Group (T)				Control Group (C)			
	N	Mean	SD	Med.	N	Mean	SD	Med.	N	Mean	SD	Med.
Scaled Applications (N)	86004	83.11	74.78	66.04	50012	72.24	70.58	56.21	35992	98.22	77.78	77.46
Approval Ratio (Aprv.)	81902	0.63	0.13	0.64	46525	0.61	0.13	0.62	35377	0.66	0.12	0.66
Deny Credit Hist Ratio	81902	0.06	0.04	0.05	46525	0.06	0.05	0.05	35377	0.05	0.04	0.04
Deny Debt-to-inc Ratio	81902	0.03	0.03	0.03	46525	0.03	0.03	0.03	35377	0.03	0.02	0.03
Withdrawl Ratio	81902	0.12	0.05	0.12	46525	0.12	0.06	0.12	35377	0.12	0.04	0.11
Num. Lenders (log)	82461	3.16	0.78	3.30	48500	3.01	0.85	3.19	33961	3.36	0.60	3.42
House Price Index	2259	0.04	0.06	0.04	1134	0.05	0.07	0.04	1125	0.04	0.05	0.04
Δ Inc per capita	2262	0.01	0.09	0.01	1142	0.01	0.10	0.01	1120	0.01	0.09	0.01
Δ Emp	1881	134.36	30.37	125.42	927	135.50	31.52	126.66	954	133.26	29.18	124.33
Δ State GDP	74	0.05	0.03	0.05	45	0.05	0.04	0.06	29	0.05	0.02	0.05

Panel B: Trend During the Pre-event Period (2000 – 2004)

	Treatment Group (T)				Control Group (C)				Slope Difference	
	N	Mean	SD	Med.	N	Mean	SD	Med.	(Δ T$-$ Δ C)	p-val
Δ Scaled applications (N)	5469	-0.26	0.44	-0.27	3980	-0.09	0.42	-0.09	-0.173	0.000
Δ Approval Ratio (Aprv.)	5049	0.16	0.27	0.10	3911	0.13	0.19	0.10	0.026	0.000
Δ Deny Credit Hist Ratio	4940	-0.12	4.61	-0.34	3886	-0.20	3.19	-0.37	0.081	0.350
Δ Deny Debt-to-inc Ratio	4810	0.10	8.23	-0.33	3819	-0.07	3.79	-0.37	0.172	0.233
Δ Withdrawl Ratio	5035	0.11	0.65	0.00	3906	0.11	0.51	0.03	0.003	0.827
Δ Num. Lenders (log)	5078	0.09	11.94	-0.05	3309	-0.02	0.24	0.00	0.106	0.610
Δ House Price Index	103	0.32	0.17	0.30	106	0.31	0.14	0.29	0.015	0.471
Δ^2 Inc per capita	126	-1.16	21.71	-0.22	125	0.20	6.21	-0.37	-1.357	0.502
Δ^2 Emp	127	-0.47	5.30	-0.65	124	-2.19	28.78	-0.67	1.717	0.509
Δ^2 State GDP	20	0.36	0.68	0.09	6	-0.19	0.35	-0.17	0.555	0.069

Table II: Mortgage Applications and Approval Ratio

This table reports the estimates of the effect of easier access to credit reports on the number of mortgage applications and approval ratio. The regression specification is from Equation (1):

$$\mathrm{Y}_{icsjt} = \beta_0 + \beta_1 \mathrm{Treat}_{icsj} \times \mathrm{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variable in Columns (1) and (2) is *N*, the number of applications per 1000 adults in a census tract (scaled applications). The dependent variable in Columns (3) and (4) is *Aprv.*, the approval ratio in a census tract. The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include the *Border*×*Year* fixed effects (FE) and the *Census Tract* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
	N	N	Aprv.	Aprv.
Treat × Post	13.46***	14.78***	0.01***	0.01***
	(2.96)	(3.61)	(2.82)	(2.62)
Economic Controls	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.806	0.821	0.740	0.733
Observations	85996	79579	81858	75513

Table III: Owner-occupied and Non-owner-occupied Mortgage Applications

This table examines the changes in (i) owner-occupied mortgage applications, (ii) non-owner-occupied applications, (iii) non-owner-occupied mortgages as the fraction of total applications, and (iv) as fraction of successful applications. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variable in Columns (1) through (4) is *N*, the number of applications per 1000 adults in a census tract (scaled applications). In Columns (1) and (2), *N* includes only the owner-occupied category mortgage applications; in Columns (3) and (4), only the non-owner-occupied category. The dependent variable in Columns (5) and (6) is *%N*, the share of non-owner-occupied mortgage applications in all applications. The dependent variable in Columns (7) and (8) is the share of non-owner-occupied mortgage applications in successful applications. The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Border*×*Year* fixed effects (FE) and *Census Tract* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Owner		Non-owner		% Non-owner in all appl.		% Non-owner in succ. appl.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N	N	N	N	%N	%N	%N	%N
Treat × Post	12.95***	14.14***	0.82*	0.96*	0.01**	0.01*	0.01**	0.01*
	(2.91)	(3.58)	(1.67)	(1.69)	(2.01)	(1.74)	(1.98)	(1.92)
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.808	0.824	0.755	0.757	0.086	0.080	0.086	0.079
Observations	85996	79579	85996	79579	81858	75513	81817	75472

Table IV: Self-learning Mechanism: Increase in Mortgage-related Cognizance among Borrowers

This table reports the estimates of the treatment effect on the fraction of mortgage applications denied for credit history and debt-to-income ratio, and the in-process withdrawal ratio. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variable in Columns (1) through (4) is *%C.Hist*, the ratio of the number of denied applications due to credit history (debt-to-income ratio) to the total number of mortgage applications in a census tract. The dependent variable in Columns (5) through (8) is *%DTI*, the ratio of the number of denied applications due to debt-to-income ratio to the total number of mortgage applications in a census tract. The dependent variable in Columns (9) and (10) is *%WDR*, the ratio of number of borrower-withdrawn applications before the lender reached a decision to the total number of mortgage applications in a census tract. *High-Denial Areas* refers to those census tracts where denial per capita in the pre-event year 2004 was more than the regional mean of denials across the census tracts. The coefficient associated with the *Treat×Post* interaction term captures the change in the fraction of mortgage applications denied due to a given reason in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Border×Year* fixed effects (FE) and *Census Tract* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	All Areas		High Denial Areas		All Areas		High Denial Areas		All Areas	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	% C.Hist	% C.Hist	% C.Hist	% C.Hist	% DTI	% DTI	% DTI	% DTI	%WDR	%WDR
Treat × Post	-0.003	-0.003	-0.003**	-0.003*	-0.002	-0.002	-0.002	-0.002	-0.009***	-0.010***
	(-1.49)	(-1.37)	(-2.03)	(-1.78)	(-1.04)	(-1.39)	(-1.43)	(-1.47)	(-2.92)	(-4.23)
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.541	0.534	0.575	0.569	0.267	0.266	0.319	0.322	0.340	0.340
Observations	81858	75513	38731	36098	81858	75513	38731	36098	81858	75513

Table V: Self-learning Mechanism: Loan-to-Income Ratio as a Proxy for Over- and Under-estimators of Creditworthiness

This table reports the result of regressing the number of mortgage applications on the sub-samples of census tracts classified as over-estimators and under-estimators. This classification is based on the residuals obtained from regressing *N*, the number of applications per 1000 adults (scaled applications), using the following cross-sectional regression equation (see Equation 3):

$$\mathrm{N}_{icsjt} = \beta_0 + \beta_1 \mathrm{LTI}_t + \delta \times \text{Economic Controls} + \gamma_s + \varepsilon_{icsjt}.$$

This regression includes only the treated tracts only for the pre-event year 2004 (making it a cross-sectional regression). *LTI* in this equation is the loan-to-income ratio averaged across all mortgage applications in a census tract in the year 2004. Then, a census tract is classified as over-estimator if the residual ε_{icsjt} >=0 and as under-estimator otherwise.

Subsequently, the change in *N* is estimated separately for the above two types of census tracts using the following equation (see Equation 4):

$$\mathrm{N}_{icsjt} = \beta_0 + \beta_1 \mathrm{Post}_t + \delta \times \text{Economic Controls} + \gamma_s + \varepsilon_{icsjt}.$$

The sample includes two years—the pre-event year 2004 and the event year 2005. $Post_t$ takes value of 1 for the year 2005 and 0 for the year 2004. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *State* fixed effects (FE). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses, and the number of observations is reported in square brackets below the t-statistics. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Over-estimator Tracts	Under-estimator Tracts
	(1)	(2)
	N	N
Post	0.72	7.94***
	(0.27)	(6.82)
Economic Controls	Yes	Yes
State FE	Yes	Yes
Cluster (County)	Yes	Yes
R^2 (Adj.)	0.592	0.754
Observations	4546	5922

Table VI: Self-learning Mechanism: Rejection Patterns as a Proxy for Over-estimators of Creditworthiness

This table reports the treatment effect for the number of mortgage applications and approval ratio, estimated separately for the census-tract tertiles created by sorting them independently on the rejection ratios for credit history and DTI. *C. Hist.* and *DTI* respectively represent the ratio of the number of mortgage applications rejected for credit history or DTI to the total number of mortgage applications in a census tract. The tertiles for these two ratios are calculated in the pre-event year 2004. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variable is *N*, the number of applications per 1000 adults (scaled applications). The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Border*×*Year* fixed effects (FE) and *Census Tract* FE. Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses, and the number of observations is reported in square brackets below the t-statistics. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

		N		
	C. Hist. Tertiles ↓	DTI Tertiles →		
		1	2	3
		(1)	(2)	(3)
Treat × Post	1	17.87**	18.08***	11.67**
t-statistics		(2.23)	(3.15)	(2.57)
Observations		[12832]	[7484]	[4497]
Treat × Post	2	10.03**	10.37**	9.66*
t-statistics		(2.14)	(2.55)	(1.94)
Observations		[6976]	[9901]	[7805]
Treat × Post	3	6.60	9.28**	4.23
t-statistics		(1.57)	(2.19)	(1.03)
Observations		[4821]	[7016]	[13125]

Table VII: Effect Heterogeneity by Consumer Creditworthiness

This table reports the estimates of the effect of easier access to credit reports on the number of mortgage applications per 1000 adults, (scaled applications, N) and the approval ratio ($Aprv.$) in *ex-ante* low- and high-creditworthiness areas. A county is "subprime" if its subprime population fraction is more than the *regional mean* subprime population fraction in 1999. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The coefficient associated with the *Treat×Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Border×Year* fixed effects (FE) and *Census Tract* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Ex-ante High Creditworthiness (Prime Counties)				*Ex-ante* Low Creditworthiness (Subprime Counties)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N	N	Aprv.	Aprv.	N	N	Aprv.	Aprv.
Treat × Post	16.84**	18.04***	0.02***	0.02***	8.62	7.48*	0.01*	0.01
	(2.33)	(2.69)	(3.20)	(2.99)	(1.65)	(1.76)	(1.73)	(1.26)
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.802	0.824	0.777	0.773	0.825	0.831	0.679	0.675
Observations	39082	35346	38006	34315	46610	43956	43743	41116

Table VIII: Effect Heterogeneity by Income Level of Consumers

This table reports estimates of the effect of easier access to credit reports on the number of mortgage applications (Panel A) and the approval ratio (Panel B) for each of the income quartiles. The income quartiles are calculated every year for a given census tract. The dependent variable in Panel A is *N*—the number of applications per 1000 adults (scaled applications)—and in Panel B is *Aprv.*—the approval ratio in a census tract. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The coefficient associated with the *Treat×Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Border×Year* fixed effects (FE) and *Census Tract* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Number of Applications per 1000 adults

	Income Quartile 1		Income quartile 2		Income Quartile 3		Income quartile 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N	N	N	N	N	N	N	N
Treat × Post	0.23	0.11	2.15**	2.08***	2.65**	2.60***	3.98*	4.26**
	(0.16)	(0.08)	(2.56)	(2.69)	(2.37)	(2.85)	(1.90)	(2.37)
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.760	0.764	0.772	0.776	0.740	0.744	0.659	0.674
Observations	87465	79579	87465	79579	87465	79579	87465	79579

Panel B: Approval Ratio

	Income Quartile 1		Income quartile 2		Income Quartile 3		Income quartile 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Aprv.	Aprv.	Aprv.	Aprv.	Aprv.	Aprv.	Aprv.	Aprv.
Treat × Post	0.01*	0.01**	0.01	0.01	0.00	0.00	-0.00	0.00
	(1.91)	(2.18)	(1.25)	(0.99)	(0.38)	(0.26)	(-0.40)	(0.33)
Economic Controls	No	Yes	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.316	0.306	0.338	0.327	0.308	0.304	0.169	0.160
Observations	71181	65205	71709	65717	71823	65842	71239	65326

Table IX: First-time Homebuyers and Mortgage Interest Rates in the GSE Data

This table reports the estimates of the treatment effect on the fraction of first-time homebuyers and interest rate using the GSE data. In Column (1) and (2), the dependent variable is the ratio of the number of mortgages taken by first-time homebuyers to total number of mortgages for which the information on first-time homebuyers is not missing, calculated at the zip3-state area level. The regression specification is from Equation (5):

$$Y_{zsjt} = \beta_0 + \beta_1 \text{Treat}_{zsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt}.$$

In Columns (3) and (4), the dependent variable is interest rate on the GSE mortgages (in percentages). The regression specification is from Equation (6):

$$\text{Interest Rate}_{izsjt} = \beta_0 + \beta_1 \text{Treat}_{izsj} \times \text{Post}_t + \delta \times \text{Controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{izsjt}.$$

The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated zip3-state areas vis-à-vis the control. *Economic Controls* include annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). *Credit Score* & *CLTV Control* refers to borrower's credit score and the combined loan-to-value ratio. *Mortgage Controls* include *DTI ratio*, *number of units in the property*, and *mortgage insurance percentage*. All regressions include *Zip3−State* fixed effects (FE) and *Border*×*Quarter* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	First-time Borrower Ratio		Interest Rate (%)	
	(1)	(2)	(3)	(4)
Treat × Post	0.011***	0.010**	0.009***	0.010***
	(2.70)	(2.47)	(13.36)	(12.06)
Economic Controls	No	Yes	-	-
Credit Score & CLTV Control	-	-	Yes	Yes
Mortgage Controls	-	-	No	Yes
Zip3-State FE	Yes	Yes	Yes	Yes
Border × Qtr FE	Yes	Yes	Yes	Yes
Cluster Zip3-State	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.694	0.695	0.731	0.758
Observations	7593	7593	7579052	3512619
Reg. Unit	Zip3-state Aggregate	Zip3-state Aggregate	Individual Mortgage	Individual Mortgage

Table X: Effect Heterogeneity by Lenders Density

This table reports the estimates of the treatment effect on the mortgage origination volume (in 1000 USD) per adult and the approval ratio, calculated separately for census tracts having a high and low density of mortgage lenders per capita in 2004. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variable in Columns (1) through (4) is the volume of mortgages originated (in 1000 USD) per adult in a census tract. The dependent variable in Columns (5) through (8) is *Aprv.*, the approval ratio of mortgage applications at census tract level. *Low* (*High*) refer to census tracts having a lower (higher) number of HMDA lenders than the *regional mean* number of HMDA lenders in a census tract (see Footnote 20 for definition of regional mean). The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Difference* [*High - Low*] shows the result of the t-test for the difference in coefficients of *Treat*×*Post* in specifications *High* and *Low*. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Volume (in 1000 USD) per Adult				Aprv.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low	High	Low	High	Low	High	Low	High
Treat × Post	0.002**	0.001	0.002***	0.001	0.015***	0.010*	0.015***	0.008
	(2.20)	(1.16)	(2.79)	(1.43)	(3.07)	(1.95)	(2.94)	(1.53)
Difference [High - Low]		-0.001		-0.001		-0.006		-0.006
p-value		(0.599)		(0.486)		(0.482)		(0.463)
Economic Controls	No	No	Yes	Yes	No	No	Yes	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.643	0.570	0.643	0.617	0.751	0.716	0.743	0.712
Observations	60202	25490	55557	23745	57126	24623	52521	22910

Table XI: Did Origination Increase due to Rise in Private Mortgage Securitization?

This table reports the estimates of the treatment effect on the approval ratio estimated separately for mortgages sold to non-GSEs, sold to GSEs, and not sold. The regression specification is from Equation (1):

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treat}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}.$$

The dependent variables are the fraction of total mortgage applications originated and sold to the non-GSEs (Columns 1 and 2); originated and sold to the GSEs (Columns 3 and 4); approved and not sold by the lending institution (Columns 5 and 6). All the dependent variables are calculated at the census tract level. The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. *Economic Controls* include five variables: the natural log of number of mortgage lenders in a census tract, county-level house price index, and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Sold to Non-GSE		Sold to GSE		Not Sold	
	(1)	(2)	(3)	(4)	(5)	(6)
	Fraction	Fraction	Fraction	Fraction	Fraction	Fraction
Treat × Post	-0.004	0.001	0.048**	0.047***	0.003	0.004
	(-0.28)	(0.06)	(2.49)	(2.63)	(0.36)	(0.60)
Economic Controls	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
Border × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (County)	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.008	-0.003	0.003	-0.003	0.036	0.010
Observations	81858	75513	81858	75513	81858	75513

Table XII: Did Origination Increase due to Subprime Lending? Credit Score-based Evidence

This table reports the estimates of the treatment effect on the number of mortgages originated to prime and subprime borrowers by government sponsored enterprises (GSEs) Fannie Mae and Freddie Mac. The regression specification is from Equation (5):

$$Y_{zsjt} = \beta_0 + \beta_1 \text{Treat}_{zsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt}.$$

The dependent variable in Column (1) is *N-Prime*, the number of mortgages originated to prime borrowers (credit score$\geq$620) in a given zip3-state area. The dependent variable in Column (2) is *N-Subprime*, the number of applications to subprime borrowers (credit score$<$620) in a given zip3-state area. The coefficient associated with the *Treat*$\times$*Post* interaction term captures the change in the dependent variable in the treated zip3-state areas relative to the control zip3-state areas. *Economic Controls* include the number of mortgage lenders in a census tract and annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). All regressions include *Zip3*$-$*State* fixed effects (FE) and *Border*$\times$*Quarter* FE. All variables are defined in Table (I). Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
	N-Prime	N-Prime	N-Subprime	N-Subprime
Treat $\times$ Post	325.63***	325.87***	11.28**	11.40**
	(3.54)	(3.44)	(2.24)	(2.23)
Economic Controls	No	Yes	No	Yes
Zip3-State FE	Yes	Yes	Yes	Yes
Border $\times$ Qtr FE	Yes	Yes	Yes	Yes
Cluster Zip3-State	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.761	0.762	0.795	0.796
Observations	7599	7599	7599	7599

Table XIII: Effect on Banks

Panel (A) of this table reports the estimates of the treatment effect on financial performance of banks. The regression specification is from Equation (7):

$$Y_{bt} = \beta_0 + \beta_1 \text{Treat}_b \times \text{Post}_t + \delta \times \text{Bank Controls}_{bt} + \alpha_l + \gamma_t + \varepsilon_{bt}.$$

The dependent variables are: (i) *NIM* (Net Interest Margin) is the ratio of net interest income to earning assets (in percentages); (ii) *RoE* (Return on Equity) is the ratio of net income to book value of equity (in percentages); (iii) *RoA* (Return on Assets) is the ratio of net income to book value of total assets (in percentages); (iv) *Mkt. Exp.* (marketing expenditure) is the expense on marketing and other professional services (RIAD0497), expressed in 1000 USD; and (v) *Dep.* (Deposit) is bank's saving deposit (RIAD0093) expressed in 1000 USD. The coefficient associated with the *Treat*×*Post* interaction term captures the change in the dependent variable for the treated banks relative to the control banks. *Bank Controls* include: natural log of the total assets (in $1000); cost of deposit (ratio of total interest expense to total earning assets, expressed in percentages); and share of liquid assets in total assets (in percentages). All regressions include *Year−Quarter* fixed effects (FE) and *Bank* FE. Standard errors are clustered by county. t-statistics are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	NIM (%)	NIM (%)	RoE (%)	RoE (%)	RoA (%)	RoA (%)	Mkt. Exp.	Mkt. Exp.	Dep.	Dep.
Treat × Post	0.10***	0.11***	0.88***	0.97***	0.07***	0.08***	-56.17***	-52.78***	-860.91***	-767.79***
	(5.62)	(6.30)	(6.66)	(7.53)	(5.62)	(6.77)	(-4.34)	(-4.15)	(-8.89)	(-8.40)
Bank Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (Bank)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.869	0.873	0.587	0.598	0.556	0.573	0.696	0.699	0.846	0.853
Observations	85847	85847	85847	85847	85847	85847	76619	76619	76619	76619

Table XIV: Survey Evidence on the Credit Reports Usage and Discouraged Borrowers

This table reports the regression results from the SCE Credit Access Survey. *Never* is 1 if a respondent has never checked his/her credit score (Q. *N23*). *Infrequently* is 1 if a respondent has never checked it or last checked it more than two years ago (Q. *N23*). *Unaware* is 1 if a respondent does not know his/her credit score (Q. *N22*). *Dscrgd* is 1 if a respondent said "I do not think I would get approved" in *Q. N19*. Note that this question (*Q. N19*) is a conditional question in the survey. Hence the observations in specifications (4–6) include only the responses in which (i) for *Q. N17A*, respondent selected *very unlikely* or *somewhat unlikely* to apply for mortgage/home-based loan, or refinance, or (ii) for *Q. N17B*, mentioned the probability of applying for mortgage or to refinance as less than 10%. All regressions include $Year \times Month$ fixed effects (FE). Standard errors are clustered by survey's Year×Month. p-values are reported below the coefficients in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Check Credit Report		Know Credit Score	Mortgage-discouraged Borrowers		
	(1)	(2)	(3)	(4)	(5)	(6)
	Never	Infrequently	Unaware	Dscrgd	Dscrgd	Dscrgd
Check Infrequently					0.03**	
					(0.05)	
Unaware						0.05*
						(0.06)
Constant	0.08***	0.20***	0.12***	0.13***	0.13***	0.13***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Cluster (Year-Month)	Yes	Yes	Yes	Yes	Yes	Yes
FE (Year-Month)	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.007	0.007	0.007	0.003	0.004	0.005
Observations	19231	19231	20275	9059	9058	9058

Appendix

Data Appendix

The HMDA data contain 190.4 million mortgage applications over the sample period (2000–2008). These application-level data were aggregated to the *Census Tract*×*Year* panel in several steps. First, all observations that had state, county or census tract information missing or "NA", or state Federal Information Processing Standard (FIPS) code of "0", "00" or "0 " were dropped (2.5% of the observations), leaving 185.6 million mortgages with an identifiable county. Then, observations on three action types were removed: covered loans purchased by the financial institutions from other institutions (18.80%), as these are not borrower initiated; pre-approval requests denied by financial institutions (0.01%), as these data were included in HMDA reporting only from 2004; and pre-approval requests approved by the financial institutions but not accepted by the applicants, as these data, too, were included in the HMDA reporting only from 2004, and this reporting is not mandatory (0.025%). This leaves 150.7 million applications belonging to 77,526 unique census tracts (603,849 *Census Tract*×*Year* observations). Finally, with the help of the county adjacency data from the Census Bureau (n.d.), those census tracts that belong to the bordering counties of the treated and control states were selected. This led to the HMDA regression sample: 89,535 *Census Tract*×*Year* observations consisting of 11,942 unique census tracts of which 7,011 are treated and 4,931 are control.

The GSE data contain 33 million observations over the sample period. The property locations in this data do not contain the census tract information, but only the first 3 digits of the zip code (zip3) and state. Hence, to identify the mortgages from the zip3-states that lie within the bordering counties of the sample, the zip code-to-county crosswalk file provided by the U.S. Department of Housing was used.[29] Then, aggregating the individual mortgages to the zip3-state level and restricting the sample to only those zip3-states that lie within the sample border counties yielded 221 unique zip3-states (91 control and 130 treated) and 7,599 *Zip3-State*×*Quarter* observations.

Finally, the mortgage lenders in the HMDA data were matched with the commercial banks in the Call Reports (FFIEC Forms 031/041) data using lenders' Federal Deposit Insurance Cor-

[29] Areas delimited by 3-digit zip codes do not align with the county borders. Hence, to identify the 3-digit zip codes that lie along the county borders, first, a crosswalk file of 5-digit zip codes to county is obtained from the Office of Policy Development and Research (n.d., 2010 Q1 version). Then all such 3-digit zip codes are filtered out from the sample for which none of the underlying 5-digit zip codes lie within the bordering counties.

poration (FDIC) certificate ID, or Office of the Comptroller of the Currency (OCC) charter number (henceforth, the identifiers). Call Reports contain information on banks' identifiers and also a unique id called RSSD ID. At the same time, HMDA data contain a lender's agency code (lender's regulator) and a respondent ID. A respondent ID equals the FDIC Certificate ID if the lender's regulator is the FDIC; and it equals the OCC charter number if the regulator is the OCC.

Some HMDA mortgage lenders are the affiliates of the commercial banks, but are not banks themselves. Such lenders were matched using their parent entities (available in the HMDA Ultimate Panel data). If both an HMDA reporter and its parent entity had a successful match in the call reports, the parent's match was kept. Finally, the RSSD ID began to be directly available in the HMDA data from 2004, so the matching was done for subsequent years using this ID, instead of the combination of the agency code and respondent ID.

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Online Appendix

Table A1: Sample States and Status of Selected Regulations

This table lists all the states included in the sample and their treatment and control status. For each state, it also indicates whether mortgages are recourse or non-recourse (Ghent & Kudlyak, 2011), whether foreclosures are judicial or non-judicial (Gerardi et al., 2013), and whether and when the state adopted Anti-predatory Lending laws (APL) (Di Maggio & Kermani, 2017).

State	Treatment (T) / Control (C)	Recourse (R)/ Non-Recourse (NR)	Judicial (J) / Non-judicial (NJ)	APL (Adoption Month, Year)/ Non-APL (NAPL)
Alabama	T	R	NJ	NAPL
Arizona	T	NR	NJ	NAPL
Colorado	C	R	NJ	APL (Jul, 2003)
Connecticut	T	R	J	APL (Jan, 2002)
Delaware	T	R	J	NAPL
Florida	T	R	NJ	NAPL
Georgia	C	R	J	APL (Mar, 2003)
Kansas	T	R	J	NAPL
Maryland	C	R	J	APL (Oct, 2002)
Massachusetts	C	R	J	APL (Nov, 2004)
Nebraska	T	R	J	NAPL
New Hampshire	T	R	NJ	NAPL
New Jersey	C	R	J	APL (Nov, 2003)
New Mexico	T	R	J	APL (Jan, 2004)
New York	T	R	J	APL (Apr, 2003)
North Carolina	T	NR	NJ	APL (Jul, 2000)
Oklahoma	T	R	NJ	NAPL
Pennsylvania	T	R	J	NAPL
Rhode Island	T	R	NJ	APL (Dec, 2006)
South Carolina	T	R	J	APL (Jan, 2004)
Tennessee	T	R	NJ	NAPL
Utah	T	R	NJ	NAPL
Vermont	C	R	J	NAPL
Virginia	T	R	NJ	NAPL
West Virginia	T	R	J	APL (Jun, 2000)
Wyoming	T	R	NJ	NAPL