Know Thyself: Free Credit Reports and the Retail Mortgage Market*

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Abstract

Under imprecise creditworthiness information, borrowers may make erroneous credit decisions. Credit reports—which record one's creditworthiness—became free in the entire U.S. in 2005, while these had already been free in seven states. Exploiting this in a difference-in-differences setting, this paper shows that cheaper credit reports to consumers changed mortgage market outcomes, indicative of improvements in the borrower pool. Approval ratios and applications increased, whereas defaults decreased. Low-income quartile borrowers and *ex-ante* high-creditworthy areas saw larger increase in approvals. Additional findings, including increased interest rates, suggest a demand-driven pool improvement, as consumers receive precise creditworthiness signal from their reports.

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

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Borrowers may make mistakes in credit decisions when they have imprecise information of their creditworthiness. Those who mistakenly overestimate their bad creditworthiness may apply for credit and get denied, a pattern observed in the U.S. mortgage market. Among the 34% denied applications over 2000 to 2008, those rejected for credit history were twice more than those rejected for debt-to-income (DTI) ratio. What make these applications sub-optimal are the economic consequences of a rejection: a likely increase in rejection probability and interest rates on *any* future credit application and forfeiture of the application fees (~\$300 to \$400). With correct self-assessment of creditworthiness, the borrowers with bad credit history could save the rejection costs by not applying.

At the same time, those who mistakenly underestimate their creditworthiness may end up not applying for credit and incur the opportunity cost of lacking access to credit. Those who do not apply for credit despite needing it for they anticipate rejection are referred to as the *discouraged borrowers*, and various surveys reveal that such borrowers are not uncommon. Across all credit categories, Survey of Consumer Finances (1998–2007) (SCF) reveals that almost ~15% of the U.S. households are discouraged; while relating to mortgage credit, analysis of the Survey of Consumer Expectations (2013–2020) (SCE) suggests that about 13% of those who report as unlikely to apply for mortgage or refinance are discouraged, and that this tendency is associated with unawareness of one's credit reports and scores.

How do the retail mortgage market outcomes change when consumers' economic cost of accessing credit reports—an authoritative record of creditworthiness information—is reduced? This paper empirically examines this question and reports three key results. First, mortgage demand and approval ratio increased, and subprime population fraction decreased. Second, good quality borrowers seem to be behind the changes: origination was higher in more creditworthy areas and among prime consumers, and new mortgages were less likely to be defaulted upon. Finally, rejections due to credit-history decrease slightly and due to DTI do not.

What is the economic mechanism behind the link between credit reports and the market outcomes? The reports aid consumers self-assess their creditworthiness more accurately, enabling them to make better mortgage-related decisions. As the reports contain crucial financial information on consumers e.g., their creditworthiness, credit history and borrowing capacity (Figure I), lenders utilize those in deciding on mortgage applications. Thus, before applying for credit, consumers may learn their creditworthiness signaled by the report and decide on the application. Those with good signal may stay-in/enter the credit market, while those with bad may search for a suitable (subprime) lender, or do not apply (exit the market); therefore, the borrower pool improves due to better consumer sorting and results in higher approval ratio. Whether the demand for credit would increase or decrease depends on prior distribution of over- and under-estimators and of those who are unaware of the role of credit reports in a mortgage application. It is through this proposed *self-learning mechanism* that credit reports affect the mortgage market outcomes.

It is when the economic costs of accessing credit reports are high that reducing those would affect the market outcomes, but are the economic costs high? While the monetary cost of the reports are trivially small (historically just ~$8), and while the economic consequences of imprecise creditworthiness information are non-trivial, the usage of the reports among consumers is woefully low, indicating that the economic costs of the reports are indeed high.[1] Out of approximately 1 billion reports generated annually in the U.S. in early 2000's, a mere 1.6% were requested by consumers (Avery, Calem, & Canner, 2004). If we assume that all these reports were requested by potential mortgage applicants, less than 5% of the applications in 2004 could potentially have come from those who had checked their credit history. Similarly, even in 2020, almost 12% of the SCE respondents report not knowing their credit score, and 20% report having never checked their credit reports, or checked it more than two years ago.

Behind the high economic costs of the reports are potentially many factors: search costs (where to obtain the reports from?)[2], unawareness costs (what role do the reports play in credit/mortgage applications?), and illiteracy costs (what does the information in the reports mean and how to use it in credit decisions?).

This paper uncovers a causal link between credit reports cost and the mortgage market outcomes by using a natural experiment in the U.S.—the enactment of the federal *Fair and Accurate Transaction Act of 2003* (FACTA). Since 2005, the act allowed access to three annual free credit reports for all consumers through the website www.annualcreditreport.com, whereas seven states—Colorado, Georgia, Maine, Maryland, Massachusetts, New Jersey, and Vermont—had already been allowing their residents free credit reports.[3] So, consumers from all states except

[1] Imprecise information on creditworthiness may lead to: wrong self-assessment of creditworthiness (Perry, 2008), debt underestimation (Brown, Haughwout, Lee, & Van Der Klaauw, 2011), and worse financial outcomes (Courchane, Gailey, & Zorn, 2008).

[2] In 2000, the three consumer reporting agencies (CRAs)—Equifax, Experian, and Transunion—blocked calls of millions of consumers who wanted to discuss the content of their credit reports, and ended up settling a lawsuit for this (Federal Trade Commission, January 13, 2000).

[3] The details of enactment of the local laws by the pre-FACTA states are: 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.)

these seven pre-FACTA states saw a close-to-exogenous reduction in economic costs of the reports from 2005 onward. Now consumers could access reports with just a few clicks, whereas earlier it could take a week for the report to arrive after successfully making a request. The act also potentially raised general awareness about the reports as consumer interest in free credit reports measured using Google Search Interest for the keyphrase *"Free Credit Reports"* heightened in the treated states relative to the control. All in all, the experiment likely reduced the monetary, awareness and search cost of accessing the reports.

Drawing on the reduction in economic costs of credit reports, this paper makes causal inferences using a difference-in-differences (DID) research design. Control group consists of six pre-FACTA states, all except Maine since it enacted the law in 2003, when the FACTA too was enacted. Treatment group consists of the states bordering the six control states. Event year is 2005, sample period spans 2000 to 2008, and the DID estimator is two-way fixed effects estimator (TWFE). In other words, the *late-treated* states are the treatment, and the early-treated, the control. Since the design involves one-shot treatment, not staggered, and early-treated states are treated deep in the past outside the sample period, the framework in Goodman-Bacon (2021) suggests that the negative-weights issue of TWFE highlighted in Borusyak, Jaravel, and Spiess (2021), De Chaisemartin and d'Haultfoeuille (2020), and Sun and Abraham (2020) may not arise. More details appear in Section (2), Empirical Research Design.

This empirical design creatively mitigates the endogeneity in assignment of treatment and control groups. The treatment is assigned not by states' local laws, but by the federal law, FACTA, that was *binding* on all states. Then, the "control" is assigned to the pre-FACTA states owing to their local laws that occurred deep in the past. Notwithstanding, the FACTA enactment in 2003 could be an endogenous response to the prevailing conditions; however, the circumstances suggest otherwise. FACTA was not entirely a new law, its provisions were consolidated from an existing federal law, the *Fair Credit Reporting Act of 1970* (FCRA), set to expire in 2003 (via its amendment in 1996). It is when the FCRA was to expire that the Congress enacted FACTA to perpetuate the expiring provisions. The prevailing economic conditions had little to do with the expiration (Nott & Welborn, 2003). Also, the concern that provisions other than free credit reports may confound with treatment effects is mitigated too.

In a bid to separate the confounding effects of local economic conditions from the treatment

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.

effect, the sample focuses on a narrow geographic area consisting of *only* the counties at the border between the treated and control states, similar in spirit to the strategy used in Huang (2008) and Dube, Lester, and Reich (2010). Further, all key outcomes are analyzed at the census tract level, a sub-county micro area roughly encompassing a population of only about 4,000, allowing to flexibly sweep out any regional economic differences.

The null hypothesis in the paper is that cheaper credit reports has no effect on the mortgage market outcomes. The key finding is that the lower economic costs resulted in an increase of 1–2 percentage points in the approval ratio and 13.8%–16.0% in mortgage applications. In dollar terms, increase in origination due to higher approval was about $5.5 billion, and due to the demand increase, about $38.1 billion, aggregated *only* across the treated bordering counties (not all counties from treated states). The increase in approval ratio is consistent with an improvement in borrower pool under a self-learning mechanism. The increase in applications indicates that creditworthiness under-estimators are more than over-estimators, though this interpretation is masked by other factors that are explored later.

Were the investment-seeking borrowers behind the increased demand, as was the case during the financial crisis that unfolded just three years after the event? Tests focusing on the owner-occupied and non-owner-occupied mortgages suggest otherwise. While the non-owner-occupied mortgages saw slight uptick of 1 percentage point as the fraction of total and successful applications, the bulk of increase seems to have come from occupancy-seeking borrowers.

If the increase in mortgage origination and approval ratio was a result of improved borrower pool, the defaults on mortgages should decrease, or it should at least not increase. Over a six-year period since inception, the event-year mortgages from the treated areas were less likely to be defaulted upon than pre-event year ones, after accounting for the trends from the control areas. Besides, this superior performance persisted through the 2008 financial crisis.

Heterogeneity in effects across consumer characteristics further helps to characterize which borrowers/areas are more likely to benefit from free credit reports. The heterogeneity across consumer creditworthiness shows that approval ratio and mortgage applications increased more in the *ex-ante* high creditworthiness areas. This accords with the idea that the reports aid consumers in assessing their creditworthiness. Heterogeneity across borrower income reveals that while approval ratio increased for the low-income quartile borrowers, the number of applications did not. As lower income is associated with overestimation of one's creditworthiness (Perry, 2008), low-income quartile consumers are likely to correct for it by exiting, resulting in no significant increase in applications, but in higher approval ratio.

The support for the self-learning mechanism comes from the patterns that suggest increased mortgage-related cognizance among borrowers. The treated areas saw a decrease in credit-history denials—a modest 0.3 percentage points reduction and significant only in the *ex-ante* high rejection areas—and no decrease in debt-to-income denials, suggesting increased learning among borrowers about their credit history. Then, withdrawal of in-process applications, too, dropped by 0.9 percentage points, indicating that the tendency to formally apply to multiple lenders reduced, allowing borrowers to save the costs of multiple applications. *Finally*, the self-learning mechanism also predicts new entry, and the treated areas saw a 1 percentage point increase in the first-time homebuyers as a fraction of the originated mortgages.

A supply-driven increase in origination seems unlikely because in the treated areas mortgage interest rates increased, high-lenders-density areas did not see more origination or approvals vis-à-vis low-lender-density areas, private securitization did not increase, and subprime origination was 30 times smaller than prime origination.

All in all, imprecise creditworthiness information among consumers likely play a role in frequent mortgage denials due to credit history and in *discouraging* potential borrowers from applying for mortgage. Reducing consumers' economic costs of credit reports improves consumer outcomes on both these fronts, leading to improved borrower pool and higher demand for mortgage. This paper draws conclusions using the mortgage decisions, yet the findings may apply to all types of credit decisions. Moreover, as these findings are causal in nature, a policy intervention aimed at educating consumers of their creditworthiness may yield similar results.

This paper primarily relates to the literature on effects of information provision on credit market participants. This is the first paper to show that consumers' economic costs of credit reports, when lowered, leads to improved mortgage market outcomes in a way consistent with improved borrower pool. In a field experiment, Homonoff, O'Brien, and Sussman (2019) find that borrowers are less likely to default when provided with information on their FICO® scores. Similarly, bank customers who enroll in free FICO scores program are less likely to default, reduce credit utilization, and increase credit card spending (Mikhed, 2015). Moreover, when financial product disclosure is increased, sophisticated borrowers default less, and when financial product is standardized, unsophisticated borrowers default less (Kulkarni, Truffa, & Iberti, 2018). Also, consumers increase mortgage borrowing after bankruptcy flags from their credit reports are removed (Dobbie, Goldsmith-Pinkham, Mahoney, & Song, 2016). It also leads to aggregate welfare loss, cheaper credit for poorer defaulters, and expensive for poorer non-defaulters (Liberman, Neilson, Opazo, & Zimmerman, 2018).

This paper also relates to the extensive literature on financial literacy. Low financial literacy leads to detrimental economic outcomes, such as high mortgage delinquency and foreclosure (Gerardi, Goette, & Meier, 2010), poor mortgage choice (Moore, 2003), and large debt (Lusardi & Tufano, 2009; Stango & Zinman, 2009). Further, field experiments reveal that less financially literate distressed borrowers benefit less from loan-modification contracts (Hundtofte, 2017), and educational intervention improves consumers financial product purchases (Balakina, Balasubramaniam, Dimri, & Sane, 2020). This paper shows that providing free credit reports results in increased mortgage demand and lower defaults.

The rest of the 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

The FCRA governed consumer credit information-related laws before FACTA. Even under the FCRA, consumers had the right to see the contents of their credit reports, except for the credit score, under specific and restrictive provisions. For example, a consumer could receive a free report if he/she made a request within 60 days after receiving a notice of an *adverse action* taken against him or her on the basis of the information in the credit report (Avery, Calem, Canner, & Bostic, 2003).[4] The 1992 amendment to the FCRA mandated that the cost of disclosure of credit information should be reasonable, while that in 1996 capped the cost at $8. The latter also provisioned the law to lapse in 2003.

Even though the FCRA allowed free credit reports at the federal level under specific circumstances, consumers rarely proactively requested their credit report for own use. Out of ap-

[4] 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. Consumers can receive credit report free of charge once in 12 months if he or she makes a request to the CRA for the credit report and certifies that: (A) she/he is unemployed and intends to apply for employment in the 60 day period beginning on the date on which the certification is made; (B) she/he is a recipient of public welfare assistance; (C) she/he has reason to believe that the file on the consumer at the agency contains inaccurate information due to fraud.

proximately 1 billion credit reports generated annually, only 1.6% were disclosed to consumers (Avery et al., 2004). Of these 1.6%, only 5.25% were proactively requested by consumers, while 94.75% were disclosed to consumers under the FCRA provisions mentioned earlier (Nott & Welborn, 2003).[5] Thus, only 0.084% of all credit reports generated were disclosed to consumers as a result of their own request.

While the FCRA was taking shape at the federal level, many states enacted local laws providing more transparency in credit reporting by allowing residents free credit reports. For example, through the Senate Bill 133 in 1997, Colorado enacted its free credit report law on April 21 of that year. The free credit report provision appears in Section 4, paragraph (E) of this bill, which got added 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.*

Meanwhile, the FCRA was amended in 1996, a sunset clause was added to it mandating its expiry in 2003. In order to make its provisions permanent, the FACTA was enacted on December 4, 2003, with a new key provision added: free annual disclosure of credit reports to consumers by each of the three national credit reporting agencies.

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, the treatment group. Panel A of Figure (II) shows these states. The regression sample consists of only the counties lying at the borders of these focal states, shown in Panel B of the figure. The event is year 2005, when www.annualcreditreport.com was established to distribute the free credit reports.[6]

Also, contiguous-county design across state borders provide one of the most compelling identification strategies (Allegretto, Dube, Reich, & Zipperer, 2017), because idiosyncratic trends may not vary widely across neighboring areas and macroeconomic shocks affect them roughly at same time (Dube, Lester, & Reich, 2016). Similar research designs have been used in Huang (2008) and Dube et al. (2010). A synthetic control matching procedure too is a

[5] Breakdown of the 94.75% credit reports disclosed under FCRA provisions: 84% due to *adverse action*; 11.5% due to fraud claim; 0.4% due to unemployment, 0.1% due to consumer being on public assistance.

[6] The website was rolled-out in four phases over two months, from Dec 2004 to Jan 2005.

viable approach, though Allegretto et al. (2017) point out that it too may place greater weights on *nearby areas*.

The DID estimator

We utilize the two-way fixed-effects (TWFE) estimator, specified below:

$$\mathrm{Y}_{icsjt} = \beta_0 + \beta_1 \mathrm{Treatment}_{icsj} \times \mathrm{Post}_t + \delta \times \mathrm{Economic\ controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt} \quad (1)$$

where Y_{icsjt} is the outcome variable for a census tract i from a county c lying at the border between treatment state s and control state j. Recall that there are seven control states, thus j ranges from one to seven. t indexes years 2000–2008 and $Post_t$ takes value 0 for year $t < 2005$ and value 1 for year $t \geq 2005$. $Treatment_{icsj}$ is 0 for all the census tracts i in counties c from control states j, and is 1 for those from treatment states s. 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.

All regressions are estimated *with and without* co-variates. Referred to as *Economic controls* in the equation, the co-variates, when used, include a host of time-varying county- and state-level variables capturing local economic and credit conditions. These are—annual growth rate of county's income per capita, county's aggregate employment and state's gross domestic product (GDP), and number of mortgage lenders (in log) in a census tract.

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 encompassing just about 4,000 population. As census tracts are smaller geographic area than a county or a state, the associated fixed effects account for all state-level differences, including electoral landscape, recourse/non-recourse status etc.

$\gamma_{j,t}$ represents "*Border* $\times$ *Year*" fixed effects, the second of the two-way fixed effects. Here j refers to the border of a control state j. Owing to the interaction with year, these allow for a region-specific time trends that flexibly and robustly account for any time-varying regional shocks affecting bordering states.[7] Thus, together with the time-varying economic controls, these fixed effects are able to reasonably account for the confounding effects of local economic

[7] Consider a 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 (j), thus are grouped as one unit. Thus, the fixed effects only utilize the variation in the outcome variables *within* each such contiguous geographic areas.

shocks on the outcomes of interest, and allow us to cleanly estimate the desired treatment effect.

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

A key issue with the TWFE is that in *staggered* DID designs, it may aggregate individual treatment effects by assigning "negative weights" to them (Borusyak et al., 2021; De Chaisemartin & d'Haultfoeuille, 2020; Sun & Abraham, 2020). Since the TWFE is a 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). Note however that the current paper uses a *single-treatment* DID design, not staggered, thus the issue of heterogeneous treatment effects across time does not arise.[8] The second issue of treatment effect being heterogeneous across treated units is a noteworthy limitation. As the key estimates in the paper are robust in the sub-samples formed by removing each of the control states (together with the surrounding treated states) one at a time, this concern too appear to be mitigated.

Including time-varying co-variates is another potential source of bias in the TWFE (Goodman-Bacon, 2021), but the conclusions of this paper are mostly robust to this, as all the estimates are quantitatively and qualitatively similar either *with or without* the co-variates. Finally, the TWFE also requires random assignment of the treatment, which seem to hold as the timing and circumstances of the FACTA enactment, as discussed earlier, appear unrelated to state's choice.

In the end, the key assumption to identify causal effects using the TWFE is the parallel-trends assumption: the treated states would have had similar trends as the control states in the absence of the treatment. Though it is unverifiable, Panel A of Figure (III) plots the trend of mean approval ratio across the two groups before the event, and they seem to be parallel. Furthermore, Panel B of Figure (III) plots the coefficients (β_k) from regression of *Approval Ratio* according to the following specification:

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

where $Event_k = 1$ *if* $t = T - k$. $Event_k = 0$ *if* $t \neq T - k, k = \{-3, 4\}$. $T =$ *Event year* 2005. These coefficients represent the difference in approval ratio for the two groups over the years relative to

[8] The staggered adoption of the local laws by the early states (control group) over 1992–1997 too is not an issue since at least eight years have passed since the last adoption. Had the gap between the adoption by control group and the treatment been small, the dynamic treatment effect on the control (early-adopters) would have co-evolved with that on the later-treated units, compromising the parallel-trends assumption.

the pre-event year (2004). We see from the plot in Panel B 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, the two plots in Figure (III) together provide reasonable assurance that the parallel trend assumption is satisfied in the current setting.

Salience of the natural experiment

Whether the natural experiment was a salient event for consumers warrants validation, and some evidence suggest that their interest in free credit reports heightened after the experiment. The examination of the Search Interest data from Google Trends supports this. *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 (V)).[9] *Second,* the plot of differential search interest across the treated and control states using the Interest-by-subregion Google Trends data suggests that consumer interest in free credit reports heightened in the treated states in the year of website's establishment. Panel B of Figure (V) shows the mean of the popularity rank for the two groups each year from 2003 to 2008. We see that the keyphrase was equally popular in both the treatment and control states in the pre-event year 2004, but it became more popular in the treatment states in 2005.[10] Also, some anecdotal evidence suggest that the website issued about 52 million credit reports to consumers in the first two years (Wikipedia, n.d.).

A seemingly obvious conjecture for the DID design to work is that higher cost of credit reports leads to its lower usage. Given that a report costs only around USD 8, one may wonder whether this is large enough to keep consumers from obtaining their credit reports. Note that the economic costs of accessing credit reports may still be high for consumers, especially for financially less knowledgeable ones, as economic costs include the knowledge whether such reports exist, where to obtain these from, and what role do they play in a mortgage process.

The credit reports usage data do support the above conjecture. The usage were low in general, and were even lower in the treatment states before FACTA was enacted. *First*, recall that only 0.084% of about 1 billion credit reports issued annually are consumer-requested. Assum-

[9] Search interest, provided by Google, is a standardized index representing the degree of searches for the keyword(s) on Google at any time relative to the highest point during the period of the analysis, over a given region(U.S. in the present case). In the time series, a value of 100 is 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.

[10] From this plot, it may appear that control states' popularity after the event decreased. However, this occurs because the popularity measure is essentially a yearly ranking of states, 100 being the most popular; so an increase in rank of one state mechanically decreases rank of other.

ing each of these reports to be requested by a separate consumer, 0.84 million consumers accessed their credit reports, while the annual mortgage applications around 2004 numbered about 16.8 million. So, even if all these reports were requested by mortgage-seeking borrowers, *less than 5%* of the mortgage applicants had checked their credit reports.[11] *Second*, the data from testimony in the U.S. senate hearing confirms the stark difference in the usage of credit reports in the pre-FACTA states and the rest. Relative to the national average, its usage was 250% higher in GA, 204% higher in MD, 153% higher in CO, 35% higher in NJ, and 25% higher in MA (U.S. Senate. 108th Congress, 2004a).[12] *Third*, the SCE data suggest that even a decade after credit reports became free, the fraction of consumers who report being unaware of their credit score is ~12%, and those who checked their report either never or at least more than two years ago is ~20% (see the Baseline Results on survey evidence for details).

It is also worth pointing out that even though the credit reports issued under FACTA do not contain the numerical credit score, consumers are not left in the dark about it. In fact, the website actively asks consumers if they wish to retrieve their scores from any of the three CRAs, and provides a link to corresponding CRA's website for further steps. It is reasonable to expect that the economic costs of accessing credit reports and credit scores dramatically reduced to just a few clicks, which in the earlier system involved calling a CRA requesting for reports, and waiting for it to arrive through mail. In fact, in 2000, the three CRA's settled a lawsuit by Federal Trade Commission (FTC) for blocking calls of millions of consumers who wanted to discuss the content of their credit reports (Federal Trade Commission, January 13, 2000).

Finally, the effects documented in this paper are conservative measures of the true effect as the treatment here is of intention-to-treat (ITT) nature, since consumers who wanted to get their credit reports could do that before FACTA too, but at higher economic costs. Then, since the DID comparison here is between late- and early-treated units, the effects may also be referred to as the *average treated effects on the late-treated* (ATT-LT).

[11] This may be why the Federal Reserve Board actively encourage consumers to check their credit reports—*It can be especially helpful to see a copy of your credit report before you apply for, say a car loan, a mortgage, or a credit card*(Federal Reserve Bank of Philadelphia, 2015).

[12] Not only did the pre-FACTA states have higher usage of credit reports, but they also seem to have enjoyed better consumer credit environments: the rate of consumer bankruptcies was the lowest (second lowest) nationally in Vermont (Massachusetts) in 2002, and the interest rate on a conventional mortgage in Vermont and Massachusetts was below the national median (U.S. Senate. 108th Congress, 2004b).

3 Data

The key data used in this paper come from the U.S. mortgage data available under the *Home Mortgage Disclosure Act of 1975* (HMDA). HMDA is the most comprehensive source of mortgages application level data in the U.S.. HMDA data provide 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.[13] The sample period of this study is from 2000 to 2008. Since the experiment occurs at the beginning of 2005, extending the sample until 2008 allows for enough post-experiment observations. The data contain 190.4 million mortgage applications over the sample period.[14]

The application-level data from HMDA are aggregated to "$CensusTract \times Year$" panel in several steps. First, all observations that have state, county or census tract information missing or "NA", or state Federal Information Processing Standard (FIPS) code as "0", "00" or "0 " are dropped (2.5% of the observations), leaving 185.6 million mortgages with an identifiable county. Then, observations on three action types are removed: the covered loan 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 the pre-approval requests approved by the financial institutions but not accepted by the applicants, as this data, too, were included in HMDA only from 2004, and its reporting is not mandatory (0.025%). This leaves 150.7 million applications belonging to 77,526 unique census tracts (603,849 "$Census\ Tract \times Year$" observations). Finally, those census tracts are selected who belong to the bordering counties of the treated and control states using Census Bureau (n.d.) data (Panel B of Figure II). The HMDA regression sample consists of 11,942 census tracts—7,011 are treated and 4,931 are control—and 89,535 "$Census\ Tract \times Year$" observations.

Though the coverage of mortgages in the HMDA data is the largest, these lack some key application-level information, such as credit score. The data from the two GSEs—the Federal

[13] Until 2003, the census tracts in HMDA are from the Census 1990 definition, while those from the 2004 onward are from 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 Census Bureau (2006). 63% of the 1990 tracts in the U.S. did not see any change across the two censuses, and 15% of 1990 tracts underwent merger into various 2000 tracts.

[14] The sample includes mortgages for three purposes—home purchase, refinance, and home improvement, and all loan types (conventional loans, loans guaranteed by Veteran Administration (VA) and Farm Service Agency (FSA)/Rural Housing Administration (RHS), and loans insured by Federal Housing Administration (FHA)).

National Mortgage Agency (Fannie) and the Federal National Home Loan Mortgage Corporation (Freddie)—contain mortgage pricing related information including debt-to-income ratio, credit score, first-time homebuyer flag, investment purpose and more. The GSE data pertain to the 30-year fixed rate single family mortgages, the most popular mortgage type in the U.S.. Over the sample period, this data contain 33 million observations. Unfortunately, the property locations in this data are available at the coarser 3-digit zip code (henceforth, zip3) and state level. The crosswalk files provided by the U.S. Department of Housing facilitate mapping the zip3-state level information to the bordering counties included in the sample.[15] Aggregation these individual mortgages to zip3-state level yields 221 unique zip3-states (91 as control and 130 as treated) and 7,599 "$Zip3\text{-}State \times Quarter$" observations.

What is also important is to analyze the effect on commercial banks, the data for which come from "Call Reports" (FFIEC Forms 031/041). Matching the mortgage lenders in the HMDA data with the RSSD ID of commercial banks in the Call Reports requires several steps. For each mortgage application, HMDA data provide an agency code (lender's regulator) and a respondent ID, the combination of which is utilized to identify respective banks.[16] As some mortgage lenders are not banks, but the affiliates of commercial banks, they are linked using their parent entities (available in the HMDA Ultimate Panel data). If both a HMDA reporter and its parent entity yielded a successful match, the parent's match is kept. HMDA started providing RSSD ID for the reporters since 2004, so instead of agency code and respondent ID, this identifier is used for matching of data from subsequent years.

A few more data sources are used. The data on consumers' credit-related information are taken from Survey of Consumer Expectations (2013–2020) Credit Access Survey, a Federal Reserve Bank of New York rotating panel survey fielded since 2013 over internet every four months. The quarterly data on county subprime population comes from FRBNY and Equifax (n.d.). Data on county-level employment comes from the annual survey of County Business Patterns (CBP) (Census Bureau, 2000–2008). To map the zip code level variables of CBP to census tracts, (Missouri Census Data Center, 2010) is used. Finally, data on state level economic conditions are taken from the Bureau of Economic Analysis, and on population characteristics

[15] 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, I use the 2010 Q1 version of the 5-digit zip codes-to-county crosswalk file from Office of Policy Development and Research (n.d.). I remove all such 3-digit zip codes for which none of the underlying 5-digit zip codes lie within the bordering counties.

[16] A respondent ID equals the Federal Deposit Insurance Corporation (FDIC) Certificate ID if the lender's regulator is the FDIC and the Office of the Comptroller of the Currency (OCC) charter number if the OCC.

at census-tract level from Census 2000 (Manson, Schroeder, Van Riper, & Ruggles, 2019).

The key outcome variables of interest are the number of mortgage applications per 1000 adults in a census tract (*scaled applications*) and approval ratio. 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. Other variables of interest are the fraction of total applications denied for credit history or debt-to-income ratio and the fraction of total applications withdrawn by applicants while still under processing.

Summary Statistics

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 debt-to-income ratio.[17]

Panel B of Table (I) shows the comparison of the treatment and control groups in the pre-treatment period using t-test for difference in mean, the p-value for which are also shown. Results from the t-tests suggest that the control and treated census tracts differ in pre-treatment years in terms of mortgage-related variables, but are *similar* in the state- and county-level economic characteristics. The similarities in economic characteristics of treated and control counties support the comparison of outcomes across the two groups, whereas the differences in mortgage-related outcomes raise the concern that these groups may also differ on some unobserved characteristics, potentially causing an endogeneity issue. However, since a DID design can accommodate pre-existing differences between the treatment and control subjects so long as they satisfy the *parallel-trends* assumption, the concern stands mitigated.

4 Results

This section first shows the findings on credit reports usage and discouraged borrowers. The discussion of baseline results regarding effects of free credit reports on mortgage demand, approval ratio, house prices, and default rates follows. Then, results highlighting the self-learning mechanism and the role of supply- and demand-side factors are discussed.

[17] We see that the four ratios—the approval ratio, the two denial ratios, and the 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, denial reasons could be other than credit history or debt-to-income ratio. Third, an application might be denied for multiple reasons.

§A Baseline Results

Survey evidence on the Credit Reports Usage and Discouraged Borrowers

The SCE Credit Access Survey is a representative U.S. consumer survey covering many aspects of credit usage. The rotating panel nature of the survey allows for inferences about the population, aided by "Year-month" level fixed effects, standard error clustering, and sampling weights.

Columns (1) through (3) of Table (II) speak to the usage of credit scores and reports among the U.S. consumers. Column (1) shows the result from regressing the dummy indicator of whether a respondent has never checked or requested credit report on a constant; an estimated 8% of the population falls into this category. Similarly, the indicator variable in column (2) is 1 if respondent has either never checked, or checked it at least more than two years ago (infrequent checkers); a staggering 20% of the population belong to this category. Finally, in column (3), the indicator variable is 1 if a respondent reports that he/she does not know his/her credit score; an estimated 12% of the population does so.

One of the questions in the survey asks, how likely the respondent is to take mortgage and related credit in next 12 months; those who are very/somewhat unlikely to do so, or those who assign less than 10% probability to it, are asked for the reason. Classifying those who respond "I don't think I would get approved" as the *discouraged borrowers*, and then regressing this dummy on a constant gives an estimate of discouraged borrowers as the fraction of those unlikely to apply for mortgage credit. The coefficient in column (4) suggests that the proportion of discouraged borrowers is about 13%. Moving further, whether being discouraged is linked with consumer's usage of credit reports and scores is examined next. The indicator for discouraged borrowers is regressed separately on dummy variable for credit report usage and one's unawareness of credit score. Column (5) and (6) suggest that infrequent checkers and those unaware of their credit score are respectively 3% and 5% *more likely* to be discouraged.

In summary, lack of credit reports/scores usage and unawareness of one's creditworthiness are non-trivially prevalent, and among other things, these have consequences for being discouraged from applying for credit.

Effect of free credit reports on mortgage approval ratio, applications, and house prices

The results on the effects of lower economic costs of credit reports on mortgage approval ratio, number of (scaled) applications and house prices follow next. Each outcome variable is measured at the census tract level. The regression specification used here is from Equation (1); the coefficient of interest is "$Treat \times Post$", which estimates the change in the outcome variable in the treated areas relative to the control after the event. All specifications include *Census Tract* and "$Border \times Year$" fixed effects.

Columns (1) and (2) of Table (III) show the regression results for approval ratio; the former is a plain DID without any co-variates, the latter is with controls for local economic conditions included, i.e. the number of HMDA lenders (in log) in a census tract, and annual growth rates of county income per capita, county aggregate employment, and state GDP. Coefficients on "$Treat \times Post$" suggest that the ratio increased by about 1 percentage point in the treated tracts. It may seem trivial, as the mortgage approval ratios are commonly believed to be high, in the upwards of 80%, but in the sample counties, the average ratio in the pre-event period is just 52%. In real 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 about $2.75 billion more successful mortgages, aggregated across the treated bordering counties.[18]

Recall that lower economic costs of accessing credit reports does not affect the information lenders have on borrowers. Their access to the reports saw no change under the FACTA, neither the extent nor the scope of information contained in these reports. In fact, any factor relevant to mortgage decision, i.e. borrower's income, employment, collateralizability of their assets etc., saw no change due to the law. What is most likely to change is borrowers knowledge of their credit history and other information that *lenders* utilize to evaluate them on. This may lead to new entry by creditworthy borrowers and better matching with lenders among existing borrowers (creditworthy borrowers to prime lender and/or less-creditworthy borrowers to sub-prime lenders); all of which predict an increase in approval ratio. The later sections evaluate some of these claims rigorously, especially whether the increase is lender driven.

Columns (3) and (4) show the results for scaled applications. The scaled applications increased in the treated tracts by 13.4–16.6, a 13.9–17.2% 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 in applications roughly translates to $37.8 billion increase in mortgages, aggregated across the treated bordering counties.[19] The increase in applications suggests that on average

[18] 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 $\times$ 0.01 $\times$ 2.7 thousand adults per treated tract), about 18,229 more successful applications across the treated bordering counties (2.6 applications $\times$ 7,011 treated tracts), or a *~$2.75 billion increase in mortgage origination across all bordering treated tracts* (18,229 $\times$ $150,597 average mortgage amount per application).

[19] The average mortgage size in treated tracts in the pre-treatment period was about $150,597. Thus the demand for mortgage credit increased by about $2.0 million per 1000 adults per census tract ($150,597 $\times$ 13.4), by about $5.4

consumers tend to underestimate their creditworthiness when it comes to mortgage borrowing, a finding in contrast to other financial decision-making settings (Perry, 2008).

To ensure that the results are general and are not driven by some specific states, the above regressions are re-estimated over sub-samples formed by removing one at a time each of the control states and their surrounding treated states. The coefficients from regressions similar to Equation (1) with all controls included are plotted in Panel A and B of Figure (IV) for scaled applications and approval ratio, respectively. We see that the estimates are mostly similar across all sub-samples.

Coefficients on "$Treat \times Post$" in columns (5) and (6) quantify the changes in the growth rate of house prices. The regressions use census tract-level house price index from Bogin, Doerner, and Larson (2016), the index that starts in 2000. The coefficients suggest that the growth rate of house prices in the treated areas increased relative to the control by 1.7–1.8 percentage points after the event, though the estimates are statistically significant only at the 10% level. This finding is in line with that of Di Maggio and Kermani (2017): in their sample, house price growth rates increased by 3.3 percentage points following a 10% increase in mortgage origination.

It is worthwhile to point out a lingering concern that the post-event sample period of this study is 2005–2008. The housing market was volatile during this period, and mortgage supply in the U.S. had started to shrink since 2005. Given the timing of the natural experiment, it would be injudicious to claim that the effect size estimated above are immune to these changes. A respite here is that the difference-in-differences design ameliorates the issue to the extent that the market-wide forces evenly affect the neighboring counties across states. Nonetheless, what further ameliorates this concern is that even when the post-experiment sample is restricted to 2006, all conclusion hold (see Section (§E), Robustness).

It is also understood that one reason for the financial crisis was excessive mortgage borrowing by borrowers without means, often for investment motives vis-à-vis occupancy motives. Whether such borrowers are behind the increased origination in the current setting is important to examine. Table (IV) examines this by focusing on the changes in owner-occupied and non-owner-occupied mortgage category using the same DID specification. The dependent variable in columns (1) and (2) is the number of mortgage applications for the former category, and columns (3) and (4), for the latter. We see that the applications increased dramatically and

million per treated census tract (\$2 million $\times$ 2.7 thousand adults per census tract), or, by about *\$37.8 billion across the treated border counties* (\$5.4 million $\times$ 7,011 treated tracts).

significantly only for the owner-occupied category in the treated areas vis-à-vis the control, but not for the latter category. Investigating further, columns (5) and (6) examine the composition of non-owner-occupied category as a fraction of total applications, and columns (7) and (8) as a fraction of successful applications. The coefficients in these four columns suggest a modest 1 percentage point increase in non-occupancy mortgages at both the application and origination stage. All in all, the investment-motivated demand does show a slight uptick, but does not appear to be a dominant reason behind the robust 15% increase in the mortgage applications.

Mortgage defaults

We saw that lower economic cost of credit reports resulted in higher mortgage origination. The question then is, whether this would follow heightened, or diminished, mortgage defaults? If the rise in origination was a result of an improved borrower pool, mortgage defaults would decrease, or at least not increase. If the creditworthiness in the borrower pool deteriorated on average, but origination still increased due to subprime lending, the defaults would increase.

The GSE data, which contain a subset of the HMDA mortgages, allow us to compare the mortgage default rates. To compare the default rates, the paper first defines *monthly default rate* for mortgages originated in a given vintage year, 2004 or 2005, as a function of its age, measured in months since origination, denoted respective as $\text{Def}_{2004,age}$ and $\text{Def}_{2005,age}$. The monthly default rate at a given age is the ratio of number of mortgages that misses a scheduled payment by 30–59 days for the first time at that age to all the mortgages originated in the given vintage year. This rate is separately calculated for the treated and control areas. *Adjusted default rate*, defined below, then measures the differential probability to default at given age:

$$\text{Adjusted default rate}_{age} = (\text{Def}_{2005,age} - \text{Def}_{2004,age})_{treated} - (\text{Def}_{2005,age} - \text{Def}_{2004,age})_{control} \quad (2)$$

A negative adjusted default rate would imply that the mortgages from the treated areas are less likely to be defaulted upon than those from the control areas at a given age. The plot of adjusted default rate with age in Figure (VI) reveals that for most of the months within six years after origination, mortgages in the treated areas were *less likely* to be defaulted upon than those from the control areas. The mean value of the adjusted default rate is -0.012 percentage points (*p-value = 0.000*). Surprisingly, around the age of 48 months, which corresponds to the bust years after the financial crisis (48 months after 2005 is 2009), it is even more negative. This suggests that the performance of the treated mortgages was superior even during and after the crisis.[20] All in all, just as higher approval ratio is consistent with an improved *ex-ante* borrower pool, so too is lower *ex-post* defaults.

§B 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 on creditworthiness is likely to bind, perhaps useful for policy targeting. This can be achieved by examining the effect heterogeneity across pre-event characteristics; specifically, the borrower creditworthiness and income.

§B.1 Heterogeneous effects by creditworthiness of borrowers

Given that creditworthy borrowers are more likely to be granted mortgage credit, if credit reports aid consumers in assessing creditworthiness, easier access to the reports should lead to *more* increase in applications and approvals in areas where fraction of creditworthy consumers was higher before the event. This is because under the self-learning mechanism, the exit would be larger and the entry smaller in the subprime areas vis-à-vis the prime areas.

To test this, a county is classified as having high creditworthiness if its subprime population fraction is less than the *regional mean* before the event.[21] The year 1999 is chosen as the classification year following Mian and Sufi (2009), who suggest that such classifications should be done at a time well before the start of the housing boom as creditworthiness of an area endogenously evolves with the housing boom. The earliest year the data on county subprime fraction, (FRBNY & Equifax, n.d.), is publicly available is 1999.

Table (V) shows the results of regressing scaled applications and approval ratio separately using regression Equation (1) for counties with high and low creditworthiness. Columns (1) and (2) show that scaled application increased by 16.8–18.8 (17.4–19.5%), while columns (3)

[20] It may seem counter-intuitive at first that the newly treated areas could perform even better (less default) than the nearby areas which already had free credit reports. This is plausible provided that the event brings out changes in the borrower pool in the treated areas, but not in the control areas. Improved performance in control areas is coming from improved pool in the post-event period relative to the worst pool from pre-event period, whereas in control areas, there is no improvement—(good) pool in post-event period is the same as in the pre-event period.

[21] 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. 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. 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.

and (4) show that approval ratio increased by 2 percentage points in treated *ex-ante* high-creditworthiness counties vis-à-vis control counties with similar creditworthiness. The contrast emerges in the coefficients in columns (5) through (8), which estimate the effects in *ex-ante* low-creditworthiness treated areas with respect to control areas of similar creditworthiness. The increase is far smaller and barely statistically significant: 8.5–11.6 (8.8%–12%) for scaled application and 1 percentage point for approval ratio. The results support the self-learning mechanism and suggest that creditworthy borrowers are more likely to benefit from easier access to credit reports.

§B.2 Heterogeneous effects by income level of borrowers

The effect across the income levels of borrowers may be different as well, because the consequences of a mortgage rejection are more severe for low-income borrowers. Upon learning creditworthiness more precisely, the likelihood of exiting the credit market or gravitating to subprime lenders vis-à-vis entering is higher for low income borrowers. In other words, more exits would occur among low income consumers than high income ones.

The above conclusion regarding exit can also be reached when we analyze the effect from the lens of over- and under-estimation. Perry (2008) finds that lower income is associated with higher propensity to overestimate one's creditworthiness. Thus to the extent that credit reports aid consumers in self-assessing their creditworthiness correctly, the downward revision of creditworthiness is more likely to occur for low-income consumers, making it rational for them to exit. Though exits are not directly observable in data, it can be inferred from differences in increase in applications across different income groups.

The approval ratios should increase for borrowers in all income groups, though it's magnitude may differ. As the marginal propensity to lend to high-income consumers is more (Agarwal, Chomsisengphet, Mahoney, & Stroebel, 2018), the opportunity for borrower pool to improve is smaller for them than for low-income groups. Thus the ratio is more likely to be high for low-income consumers.

These predictions are tested next. Income quartile cut-offs are calculated in-sample each year, and then applications in each quartile are aggregated at the census-tract level. Panel A of Table (VI) shows the results of regressing scaled applications for the income quartiles using Equation (1). The scaled applications did not increase significantly for lowest quartile, but it increased for other three, and the magnitude of increase is larger for higher quartiles. This finding is in line with the prediction; as exits are more likely to be a rational choice for low-income consumers, the rise in number of applications would be small for them. Coefficients in Panel B of the table estimate the changes in the approval ratios. We see that it increased statistically significantly only for the lowest income-quartile, again consistent with the intuition.

Increase in demand for mortgage after the economic costs of credit reports were reduced suggests that mortgage borrowers tend to underestimate their creditworthiness. This is in contrast with commonly understood belief that consumers tend to overestimate creditworthiness (Perry, 2008). The analysis here suggests that low-income consumers indeed tend to overestimate and the high-income consumers tend to underestimate.

§C Mechanism: Consumer Self-learning Channel

The support for the self-learning mechanism is evident from the remarkable patterns that emerged in the treated areas after the event: increase in mortgage-related cognizance among borrowers and entry of new borrowers.

§C.1 Increase in mortgage-related cognizance among borrowers

The information in credit reports undoubtedly influence lender's assessment of a mortgage application, but just the fact that consumers could access the reports easily after the FACTA would not cause material changes in the market-level outcomes unless reasonably large number of consumers start to access the reports and use the information contained therein in their credit decisions. As the reports contain credit history of consumers, if consumers become more cognizant of this, there should be a decrease in rejection due to credit history, while there should be no change in rejections due to debt-to-income ratio.

Similarly, the increased cognizance would also affect the in-process application withdrawals. Borrowers tend to initiate multiple formal mortgage applications at several lenders because of the uncertainty in approvals and mortgage terms. They incur multiple non-refundable application costs, and in the end, they take the mortgage out with one lender while withdraw from the others (in-process withdrawals).[22] Thus, increase in borrower cognizance

[22] The withdrawal ratio was about 12% over the 2000–2008 period. Some anecdotal evidence suggest that consumers tend to withdraw application when they find a better offer from other lenders (Reddit Forum, n.d.). More importantly, credit reporting agencies do not penalize multiple applications if those are made within a short time period. According to Equifax (n.d.), "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."

of their creditworthiness should result in reduced fraction of in-process withdrawals.

The first prediction can be tested by regressing the fraction of total applications rejected for a given reason. Noting that the value of information in the reports is high when rejection rates are high, these outcomes are estimated for all the census tracts as well as for only those that had *ex-ante* high rejection rates. The latter are the tracts where the rejection rate in the pre-event year 2004 was higher than the *regional mean* (Footnote (21)). Another reason to separately focus on the high rejection areas is that the rejections-for-a-given-reason would not be affected if there are too few rejections in the first place.

The second prediction is tested by regressing the fraction of total applications that are formally withdrawn by the borrower before the lender has reached a decision. The specification for these regressions is the same as the one used in the baseline results, the Equation (1).

Table (VII) 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 only in the *ex-ante* high rejection rate areas (columns 3 and 4). The coefficients in columns (5) through (8) show that the debt-to-income ratio denials did not decrease statistically significantly. These results, despite having modest statistical significance, indicate that the rejection owing to different reasons changed in a manner consistent with consumers learning more about their credit history.[23] As explained before, borrowers in the *ex-ante* high-rejection areas would value the information in the reports more.

The estimates for withdrawal ratio appear in columns (9) and (10) and imply a reduction of 0.9–0.11 percentage points in the treated tracts vis-à-vis the control.[24] Overall, both the patterns point to an increase in mortgage-related cognizance among borrowers, consistent with the self-learning mechanism.

§C.2 New entry: Increase in first-time homebuyers

About 15% of households in the SCF survey of early 2000's and 13% of the respondents in the recent SCE surveys report as being *discouraged*, and this tendency is associated with lack of

[23] Another caveat to note regarding rejection reasons is that HMDA does not mandate lenders to report rejection reasons, so if their reporting incentives changed with the event, we would incorrectly attribute lender-induced change to consumer self-learning. However, it seems unlikely as lenders reported a rejection reason in over 70.81% of the rejections. Also, incentives to report rejection reasons would need to change differently in a particular manner between treated and control tracts in the event year to bias the above results, an unlikely scenario.

[24] 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 ~$400 per withdrawn application, this represents ~U.S. $6.6 million saving in upfront mortgage application fees.

knowledge and usage of credit reports/scores. With access to credit reports becoming easier, the credit applications may increase. Also, the self-learning mechanism predicts that to the extent consumers overestimate rejections and do not apply for credit, there would be entry of creditworthy borrowers into the credit markets. The proportion of first-time homebuyers in the mortgage market allows to examine this.

The information on first-time homebuyer is available in the GSE data, though not in the HMDA data. With reasonable approximations (see Footnote 15), the property location information in the GSE data can be mapped to counties. The outcome variable of interest is the ratio of the number of mortgages to the first-time homebuyers to the number of (all) originated mortgages with known information on first-time homebuyer status. The regression is now specified at the zip3-state level, different from previous census tract-level regressions:

$$Y_{zsjt} = \beta_0 + \beta_1 \text{Treatment}_{zsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt} \tag{3}$$

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×Quarter" fixed effects, similar to that in (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 (VIII) shows the regression results. Coefficients in columns (1) and (2) show that the percentage of first-time homebuyers increased by 1 percentage point in the treatment areas relative to the control areas.[25] This finding is in line with prediction that following the event, more new entry should occur in the treated areas.[26]

§D A Demand- or Supply-side Effect?

The previous results indicate that the treated areas saw increased mortgage demand and origination after the event, and a demand-driven consumer self-learning channel is the likely explanation. However, a supply-driven explanation is plausible as well: in the natural experiment, lenders too were exposed to the knowledge that consumers' access to credit reports became

[25] About 6.7% of the observations within the hombebuyer data sample pertaining to the bordering counties do not have the information on first-time homebuyer status. In unreported specifications that alternatively define the outcome variable as the ratio of number of first-time homebuyers to *all mortgages* yield similar coefficients.

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

easy and free, and so, they increased the mortgage supply.

Many of the earlier findings favor the demand-driven explanation. Increase in applications and a decrease in in-process application withdrawals indicate a demand-driven mechanism as these decisions are consumer determined and are mostly independent of lenders' influence. Furthermore, under a demand-driven mechanism, the effects should be heterogeneous in relevant consumer characteristics, as was the case for creditworthiness and income. Besides, recall that propensity of lenders to extend credit to low-income borrowers is low (Agarwal et al., 2018), yet in the current setting we see that the approval ratio increased significantly for them vis-à-vis the high-income borrowers, making lender-driven effect unlikely.

Notwithstanding, mortgage interest rates and heterogeneous effect by lender's density allow us to further examine whether the supply-side was instrumental in the increased mortgage origination.

§D.1 Interest rates on the GSE-repurchased mortgages

The price at which lenders can sell *conforming* mortgages to the GSEs materially vary only across three dimensions, credit score, loan-to-value ratio (LTV), and loan type (Scharfstein & Sunderam, 2016).[27] Since the GSE sample in this paper include only 30-year fixed rate mortgages, only the first two are relevant. The residuals in a regression of interest rate on the first two attributes roughly reflect lenders pricing schedule independent of borrower risk and mortgage characteristics. Thus, including the "$Treat \times Post$" in the above regression allows testing whether lenders lowered their pricing in the treated areas vis-à-vis control areas in a bid to increase mortgage origination, in which case the sign on the associated coefficient would be negative. The prediction is tested next using a regression similar to Equation (3) but specified at the loan level i:

$$\text{Interest Rate}_{izsjt} = \beta_0 + \beta_1 \text{Treatment}_{izsj} \times \text{Post}_t + \delta \times \text{Controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{izsjt} \qquad (4)$$

Columns (3) and (4) of Table (VIII) shows the results of the regression. Controls in column (1) are the two relevant pricing variables, credit score and CLTV (combined loan-to-value, it is loan-to-value ratio inclusive of all loans secured by a mortgaged property). In column (2), the following controls are added to make the specification more rigorous: debt-to-income ratio,

[27] The pricing schedule published of Fannie Mae is available at https://www.fanniemae.com/content/pricing/llpamatrix.pdf

number of units comprising the mortgaged property, and percentage of mortgage insurance coverage. The coefficient on "$Treat \times Post$" is 0.009–0.01 percentage points, positive and significant. These estimates suggest that, if anything, lenders increased the risk-adjusted mortgage interest rates in the treated areas rather than lowering it, contradicting the idea of a supply-driven increase.[28]

§D.2 Heterogeneous effects by density of mortgage lenders

If the increase in mortgage origination were driven by lenders, it would be greater in areas where the 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 is more than the *regional mean*, defined in Footnote (21), low otherwise.

Columns (1) through (4) of Table (IX) show the results of separately regressing dollar origination volume (in 1000 USD) per adult for the two groups. The regression specification is the same as Equation (1). The estimates are smaller in magnitude and have weaker statistical significance for high-density tracts (columns 2 and 4) vis-à-vis the low-density ones (columns 1 and 3, respectively). Thus, high-lender-density treated areas saw smaller increase than low-lender-density treated areas after controlling for concurrent changes in the control areas with comparable lender density. 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.

Then, columns (4) through (8) of the table repeats the analysis with approval ratio as the outcome variable. 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.

Overall, these findings suggest that the effects were stronger in low-lender density areas (severe low lender density areas are sometimes referred to as banking deserts) and are inconsistent with the explanation that increase in mortgage origination and approval ratio were solely lender driven.

[28]The magnitude of the increase in the rates is tiny, potentially because of two reasons. First, 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). Second, the supply of mortgages in the U.S. is highly elastic because of the large-scale purchases by the GSEs of conforming mortgages in the secondary market.

5 Supplementary Discussion

§A Did origination increase due to rise in private securitization?

An alternative explanation for increased mortgage origination is that higher commissions from private (non-government) securitization led lenders to increase the mortgage supply (Keys, Mukherjee, Seru, & Vig, 2010). If increased approval in the current context were due to private securitization, the fraction of originated mortgages being sold to non-government (private securitization) entities would increase in the treated areas.

Table (X) examines the above prediction by employing Equation (1). The outcome variables are the fraction of total applications that lenders originated and (1) sold to non-government entities, (2) sold to the four GSEs (Fannie Mae, Freddie Mac, Ginnie Mae, and Farmer Mac), and (3) did not sell. Columns (1) and (2) show that there is no change (increase) in the fraction of mortgages sold to the private entities (non-GSEs); columns (3) and (4) show that the fraction of mortgages sold to the GSEs increased; and column (5) and (6) show that the fraction of unsold mortgages did not change either. Thus, the evidence does not support the conjecture that increase in private securitization in the treated areas could explain increase in origination.

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

It may be argued that increased mortgage origination is due to an increase in the subprime credit (Mian & Sufi, 2009). Employing the comprehensive HMDA data and location-based proxies of creditworthiness, Table (V) already suggest that effect of free credit reports was stronger in the prime counties/census tracts than in the subprime. These proxies are informative and widely used (Di Maggio & Kermani, 2017; Mian & Sufi, 2009), but are imprecise. By restricting ourselves to the GSE sample, we can use precise application-level credit scores.

Table (XI) shows the results of regressing separately the number of prime (credit score ≥ 620) and subprime *originated* mortgages in zip3-state areas using Equation (3). 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. In conclusion, the increased origination did not disproportionately go to subprime consumers. These estimates, however, are not directly comparable to the previous as the observation unit here is zip3-state, not census tracts which was the unit in the previous regressions.

These results come with the same selection issue that applied to the previous results utilizing the GSE data. Same argument as before allays this concern. In addition, before 2007 the GSEs sought to buy more subprime, not prime, mortgages to combat the housing bust (Elul, Gupta, & Musto, 2020), thus their changing incentives appears not to be a critical concern here.

§C Effect on banks

So far we focused on evaluating the effects on borrowers. The effect on banks is evaluated next, though with some caveats. First, commercial banks are not the dominant mortgage originators. Despite being 80% of mortgage lenders by number, banks accounted for just 37% of the mortgage lending in 2005, thus the conclusions drawn from studying banks may not be same for all lenders (Avery, Brevoort, & Canner, 2007). Second, many banks operate across states, so their treatment and control status in this natural experiment is continuous, rather than binary. The treatment intensity is proportional to bank's *ex-ante* mortgage activity in the treated and control states. Nonetheless, we can examine this question by classifying a bank as treated if in the pre-event year 2004 the ratio of mortgage it originated in control states to that in treated *and* control states combined is larger than the cross-sectional average. The regression equation is:

$$Y_{bt} = \beta_0 + \beta_1 \text{Treatment}_b \times \text{Post}_t + \delta \times \text{Bank controls}_{bt} + \alpha_l + \gamma_t + \varepsilon_{bt} \tag{5}$$

where Y_{bt} is the outcome variable (net interest margin (NIM), return on equity (RoE), and return on assets (RoA)). b indexes the banks; $Treatment$ is 1 if a bank is treated; $Post_t$ is 1 if year $\geq$ 2005; year t is year-quarter; α_i is bank fixed effects; γ_t year-quarter fixed effect; and $Bank\ controls$ include banks' log total assets, share of liquid assets to total assets, and cost of deposit.[29]

Regression results in Table (XII) show that treated banks saw a 6 basis points increase in NIM (columns 1 and 2), a 0.75–0.76 percentage points increase in RoE (columns 3 and 4), and a 0.07–0.08 percentage points increase in RoA (columns 5 and 6). Therefore, recognizing the caveats described above, the outcomes from free credit reports seems positive for the commercial banks as well.

[29] 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. Cost of deposit is the ratio of RIAD4073 to earning assets.

§D An alternative mechanism based on information asymmetry

An alternative mechanism based on asymmetric information is plausible in which borrowers *privately know* their true creditworthiness type, but do not know what lenders know about them. Using free credit reports, borrowers learn that the information on them that lenders have is proportional to their true type. Hence, under the non-trivial search/application cost, bad borrowers self-select out. The borrower pool now improves relative to the situation in which borrowers do not know that 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 self-selecting-out by bad borrowers, but not by self-selecting-in by good borrowers as all borrowers *privately know* their true type. However, under the self-learning mechanism, borrowers themselves have imperfect information of their true type, thus both selecting-in by good borrowers 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 the 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 such as those accumulated 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 borrowers.[30]

§E Robustness

Since the natural experiment utilized in this paper takes place in the year 2005, the sample period is chosen 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 when re-estimated by excluding the observations for years 2007 and 2008 yield similar results despite having only two post-experiment observations. These results are unreported for brevity.

[30] 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."

6 Conclusion

A non-trivial proportion of consumers do not check their credit reports regularly, and do not know their credit scores. Various data suggest that consumers err in credit decisions in a manner consistent with them having imperfect information of their creditworthiness. Credit reports contain crucial creditworthiness information, and can aid them in credit-related decisions.

This paper examines the effect of lowering the consumers' economic cost of credit reports on the mortgage market outcomes. The enactment of the federal *Fair and Accurate Transactions Act of 2003* (FACTA) allowed all U.S. consumers to access three free credit reports annually from 2005, while seven states already had local laws permitting their residents to obtain the reports for free. The causal link is established by deploying this close-to-exogenous reduction in the cost of the reports in a difference-in-differences setting. Here, the border counties of the early-adopting states constitute the control group, and those of the surrounding states the treatment.

The key finding is that reducing consumers' economic costs of the reports improves mortgage market outcomes in a way consistent with improvements in the borrower pool, and benefits both consumers and lenders. Specifically, free credit reports resulted in an increase in mortgage demand and approval ratio, more credit to creditworthy borrowers, a reduction in defaults and subprime population proportion, more first-time homeowners, and better financial performance of lenders.

Though the findings pertain to the mortgage decisions of consumers, they broadly hold true for all consumer credit decisions when they have imperfect information of their creditworthiness. Furthermore, the causal nature of these findings implies that a policy intervention aimed at educating consumers of their creditworthiness may yield similar results.

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Figure I: A Sample Credit Report

This figure shows the summary page of a 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

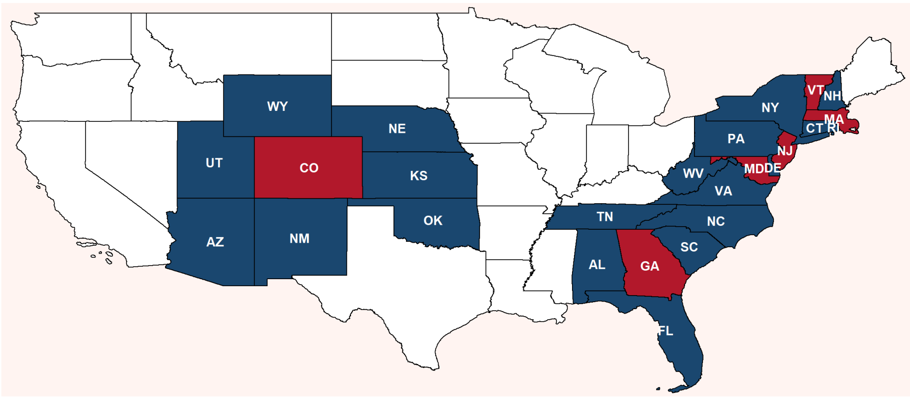
Your credit report includes your Personal Information and, if applicable, Consumer Statements, and could include other items that may affect your credit score and rating.

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 II: Empirical Research Design

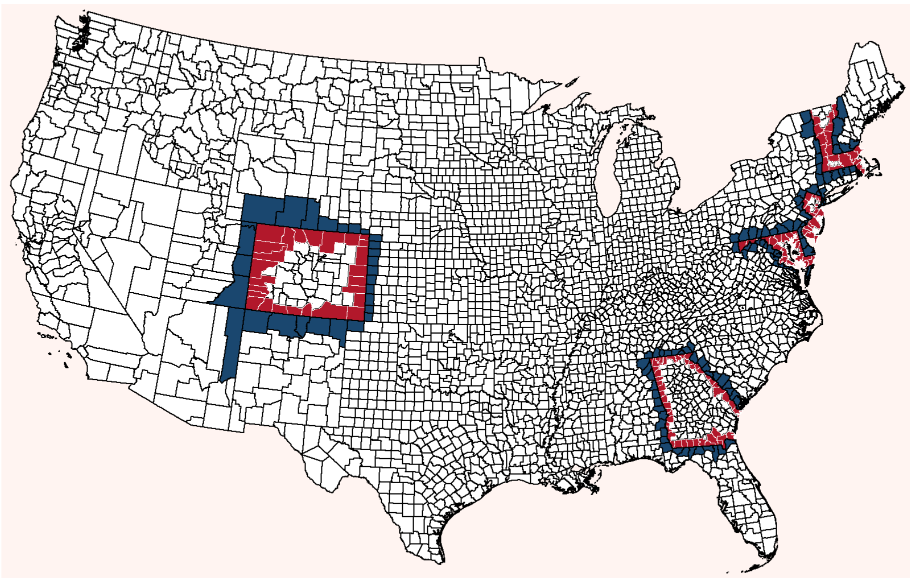
Panel A of this figure shows on the map of the contiguous U.S. the states utilized in the difference-in-differences (DID) setting. Seven U.S. states had enacted free credit report laws prior to the FACTA enactment in 2004: CO (1997), GA (1996), MD (1992), NJ (1997), MA (1995), VT (1992), and ME (2003).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

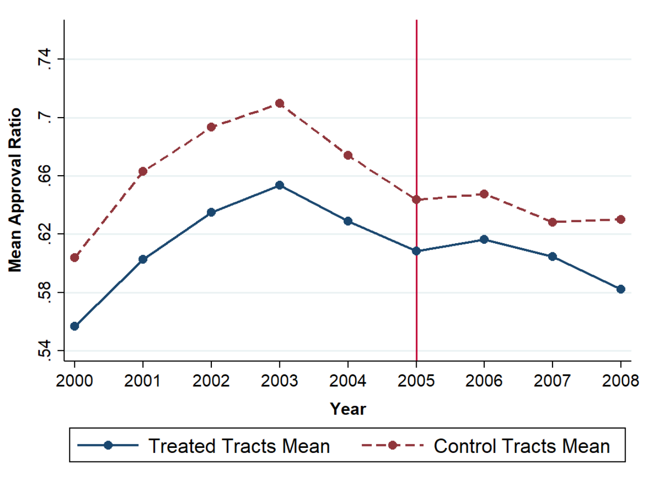
Figure III: Examining the Parallel Trends

Panel A of this figure shows the mean approval ratio in the treated and control census tracts. **Panel B** of this figure shows the coefficients β_k from regressing *Approval Ratio* using the 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 interval of β_k 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.

Panel A: Mean Approval Ratio in Treated and Control Areas



Panel B: Coefficient Estimates of Approval Ratio by Years to Treatment

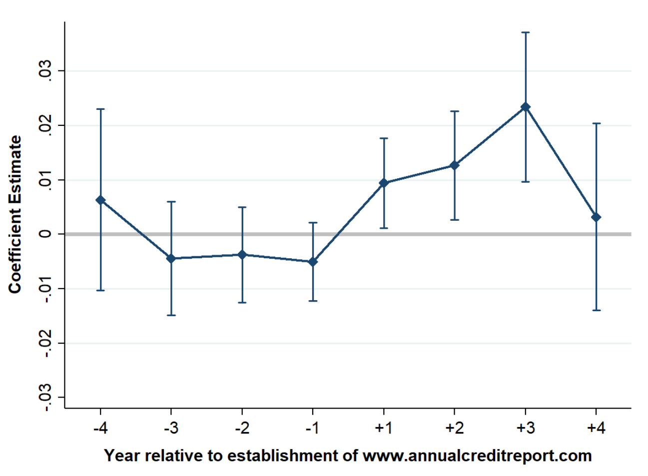
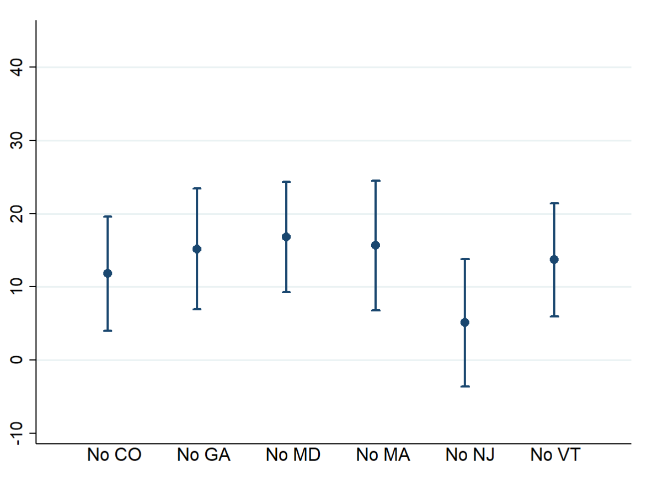


Figure IV: Subsample Analysis

Panel A of this figure shows the estimates for changes in number of applications per 1000 adults (scaled applications) when each control state is removed one by one. **Panel B** of this figure shows the estimates for changes in approval ratio when each control state is removed one by one. For example, the coefficient corresponding to "No CO" respresents the estimate when Colorado and its surrounding states were removed from the estimation sample. The regressions specifications behind the estimates are the same as those in Table(III). The bands around the estimates show 90% confidence intervals.

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



Panel B: Approval Ratio

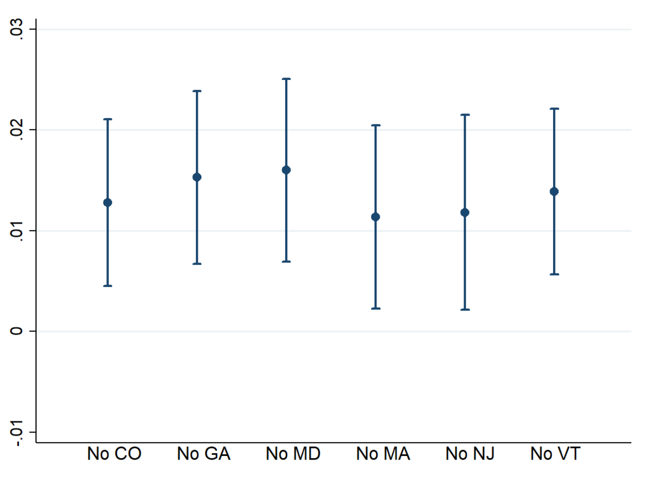
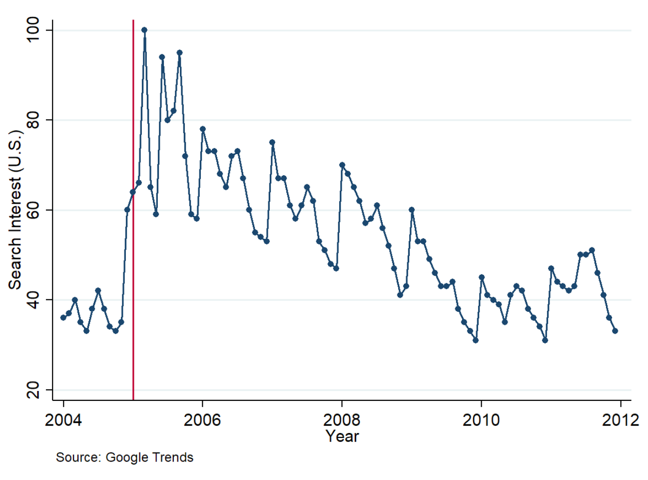


Figure V: Consumer Interest in Free Credit Reports: Google Trends

This figure shows the consumers interest in free credit report 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 mean popularity rank of treatment and control states for the same keyphrase from 2004 to 2008. The popularity score of each state ranges from 0 to 100 and is calculated every year. A value of 100 represents the location with the highest popularity of the search term as a fraction of total searches in that location. A value of 50 indicates a location where it is half as popular.

Panel A: Google Search Interest for the Term "Free Credit Report" in the U.S.



Panel B: Relative Popularity of the keyphrase *Free Credit Report*

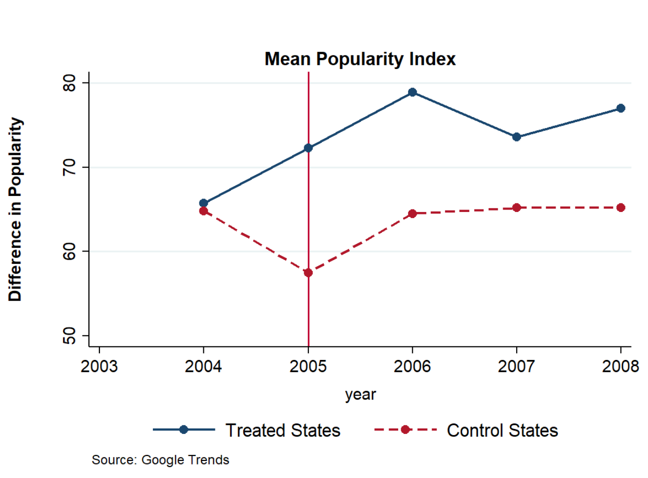


Figure VI: Effect of Free Credit Report on Mortgage Defaults

This figure shows the adjusted default rate for the sample of 30-year fixed-rate mortgages purchased by Fannie Mae and Freddie Mac. I separately calculate the percentage of total mortgages originated in the pre-event year 2004 and the post-event year 2005 which went into default in a month post-origination, $\text{Def}_{2005,age}$ and $\text{Def}_{2004,age}$, for the treated and control zip3-state areas respectively. A mortgage is in default when the scheduled payment is delayed by 30–59 days for the first time. I then calculate the adjusted default rate as:

$$\text{Adjusted Default Rate}_{age} = (\text{Def}_{2005,age} - \text{Def}_{2004,age})_{treated} - (\text{Def}_{2005,age} - \text{Def}_{2004,age})_{control}$$

where *age* represents months since origination.

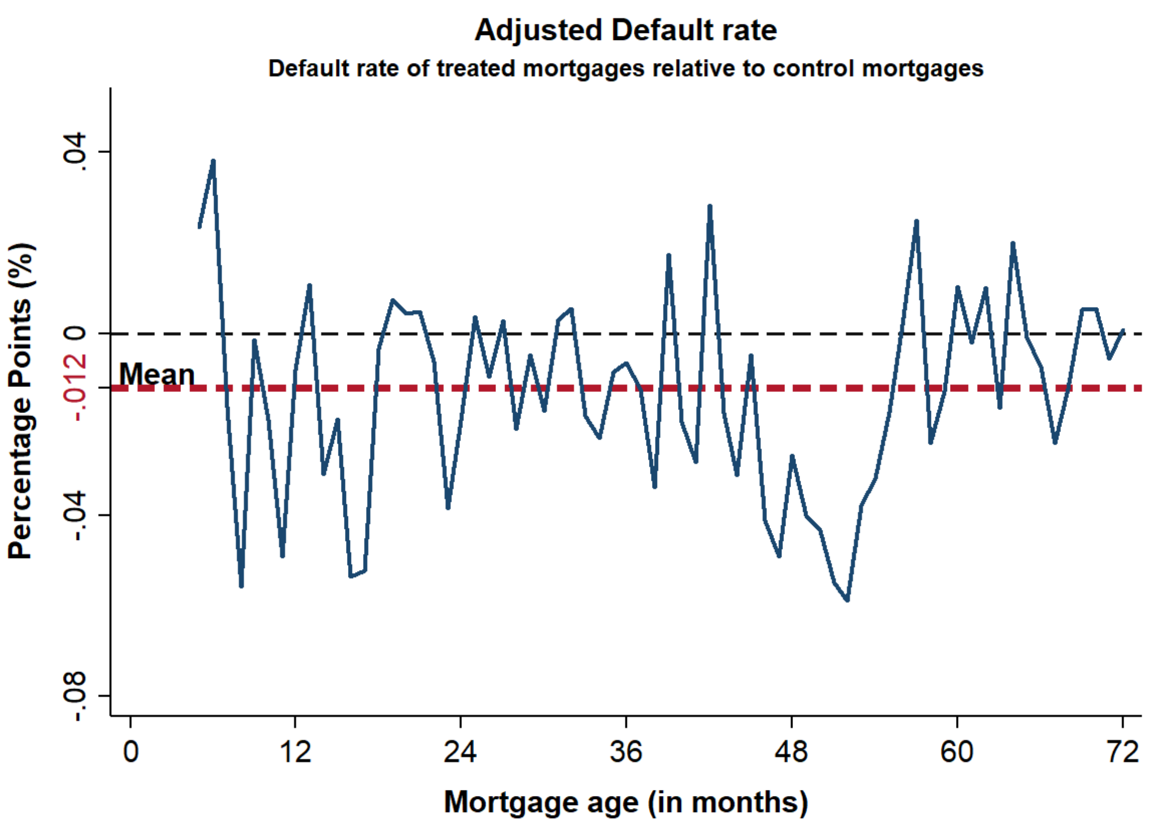


Table I: Summary Statistics

Panel A shows the statistics for the full sample time period (2000–2008). Panel B shows the statistics for the pre-treatment period (2000–2004) and the p-values for the t-test for difference in the control and treatment group. *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 following four variables constitute *Economic Controls*. *Num. Lenders* (*log*) is the number of unique mortgage lenders in a census tracts, expressed in log. Δ *Inc per capita* is the annual growth rate of income per capita at the county level, Δ *Emp.* is the annual growth rate of the employment by all establishments at the county level, and Δ *State GDP* is the annual growth rate of the state gross domestic product.

Panel A: Full Sample (2000 – 2008)

	Full Sample				Control Group (C)				Treatment Group (T)			
	N	Mean	SD	Med.	N	Mean	SD	Med.	N	Mean	SD	Med.
Scaled Applications (N)	86017	83.09	74.77	66.04	36002	98.18	77.76	77.44	50015	72.23	70.57	56.22
Approval Ratio (Aprv.)	81914	0.54	0.13	0.55	35386	0.57	0.12	0.58	46528	0.52	0.14	0.53
Deny Credit Hist Ratio	81914	0.06	0.04	0.05	35386	0.05	0.04	0.04	46528	0.06	0.05	0.05
Deny Debt-to-inc Ratio	81914	0.03	0.03	0.03	35386	0.03	0.02	0.03	46528	0.03	0.03	0.03
Withdrawl Ratio	81914	0.12	0.05	0.12	35386	0.12	0.04	0.11	46528	0.12	0.06	0.12
Num. Lenders (log)	82477	3.16	0.78	3.30	33974	3.36	0.60	3.42	48503	3.01	0.85	3.19
Δ Inc per capita	2259	0.04	0.06	0.04	1125	0.04	0.05	0.04	1134	0.05	0.07	0.04
Δ Emp	2262	0.01	0.09	0.01	1120	0.01	0.09	0.01	1142	0.01	0.10	0.01
Δ State GDP	73	0.05	0.03	0.04	29	0.05	0.02	0.04	44	0.05	0.03	0.04

Panel B: Pre - Treatment Sample (2000 – 2004)

	Full Sample				Control Group (C)				Treatment Group (T)				(C-T)
	N	Mean	SD	Med.	N	Mean	SD	Med.	N	Mean	SD	Med.	p-val
Scaled applications (N)	47923	110.16	83.62	92.99	20015	129.53	86.39	108.37	27908	96.27	78.68	82.84	0.000
Approval Ratio (Aprv.)	46584	0.55	0.14	0.56	19798	0.58	0.13	0.60	26786	0.52	0.14	0.53	0.000
Deny Credit Hist Ratio	46584	0.06	0.04	0.05	19798	0.06	0.04	0.05	26786	0.07	0.05	0.06	0.000
Deny Debt-to-inc Ratio	46584	0.03	0.02	0.03	19798	0.03	0.02	0.03	26786	0.03	0.02	0.03	0.000
Withdrawl Ratio	46584	0.12	0.05	0.11	19798	0.12	0.04	0.11	26786	0.13	0.05	0.12	0.000
Num. Lenders (log)	44383	3.36	0.73	3.48	17987	3.53	0.60	3.59	26396	3.24	0.78	3.39	0.000
Δ Inc per capita	1255	0.04	0.06	0.04	625	0.04	0.05	0.04	630	0.04	0.07	0.04	0.620
Δ Emp	1254	0.01	0.09	0.01	622	0.01	0.09	0.01	632	0.00	0.10	0.01	0.290
Δ State GDP	39	0.05	0.02	0.05	17	0.05	0.02	0.05	22	0.05	0.02	0.06	0.543

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

This table reports the regression results from the SCE Credit Access survey. *Never* is one if a respondent has never checked his/her credit score (Q. *N23*). *Infrequently* is one if respondent has never checked it or last checked it more than 2 years ago (Q. *N23*). *Unaware* is one if respondent don't now his/her credit score (Q. *N22*). *Dscrgd* is one if 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 to apply 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

Table III: Mortgage Applications, Approval Ratio, and House Prices

This table reports the estimates of the treatment effect of free credit reports on the number of mortgage applications, approval ratio, and growth in house prices. The regression specification is:

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

N, *Aprv.*, and ΔHPI are the number of applications per 1000 adults (scaled applications), the approval ratio in a census tract and growth in house prices at census tract level, respectively. *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). The coefficient associated with the $Treat \times Post$ interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. All regressions include the $Border \times Year$ 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)	(5)	(6)
	Aprv.	Aprv.	N	N	ΔHPI	ΔHPI
Treat × Post	0.01***	0.01***	13.43***	16.63***	1.83*	2.00*
	(2.80)	(2.84)	(2.95)	(3.79)	(1.88)	(1.94)
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.)	0.740	0.730	0.806	0.816	0.682	0.693
Observations	81871	76437	86010	80546	24927	23806

Table IV: Owner-occupied and Non-owner-occupied Mortgages

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

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

The dependent variable in columns (1) through (4) is the number of applications per 1000 adults in a census tract (scaled applications), N. In columns (1) and (2), N measures owner-occupied category mortgage applications only; in columns (3) and (4), non-owner-occupied only. The dependent variable in columns (5) and (6) is the non-owner-occupied mortgage as a fraction of total applications, and in columns (7) and (8), as the fraction of originated applications. *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). 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. 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, % of all appl.		Non-owner, % of succ. appl.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	N	N	N	N	%	%	%	%
Treat × Post	12.92***	15.99***	0.82*	1.00*	0.01**	0.01*	0.01**	0.01*
	(2.90)	(3.75)	(1.67)	(1.80)	(2.02)	(1.74)	(2.05)	(1.91)
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	0.819	0.755	0.757	0.086	0.080	0.085	0.079
Observations	86010	80546	86010	80546	81871	76437	81785	76349

Table V: Characterizing the Effect: Heterogeneity by Consumer Creditworthiness

This table reports the estimates of the treatment effect of free 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:

$$\text{Y}_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

$Economic\ Controls$ 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). The coefficient associated with the $Treat \times Post$ captures the change in the dependent variable in the treated census tracts relative to the control census tracts. All regressions include $Border \times Year$ 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	16.82**	18.80***	0.02***	0.02***	8.59	11.66**	0.01*	0.01*
	(2.33)	(2.66)	(3.19)	(3.29)	(1.64)	(2.42)	(1.71)	(1.78)
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	0.822	0.777	0.772	0.825	0.826	0.679	0.672
Observations	39076	35703	38000	34644	46631	44558	43763	41703

Table VI: Characterizing the Effect: Heterogeneity by Income Level of Consumers

This table reports estimates of the treatment effect of free credit reports on the number of mortgage applications and the approval ratio for each income quartile. The regression specification is:

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

N and *Aprv.* are the number of applications per 1000 adults (scaled applications) and the approval ratio in a census tract, respectively. Income quartiles are calculated every year for a given census tract. *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). 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. 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	0.49	2.14**	2.38***	2.64**	3.03***	3.97*	4.83***
	(0.16)	(0.35)	(2.54)	(3.11)	(2.35)	(3.19)	(1.90)	(2.61)
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	0.760	0.772	0.773	0.740	0.741	0.659	0.672
Observations	87479	80546	87479	80546	87479	80546	87479	80546

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
	(1.92)	(2.24)	(1.24)	(1.18)	(0.37)	(0.34)	(-0.41)	(0.19)
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	0.307	0.338	0.326	0.308	0.297	0.169	0.156
Observations	71190	66014	71718	66523	71832	66632	71248	66062

Table VII: 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 in-process withdrawal ratio. The regression specification is:

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

The outcome variables are: *%C.Hist*, *%DTI*, and *%WDR*. *%C.Hist* (*%DTI*) is 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. *% WDR* is the ratio of number of borrower-withdrawn applications before the lender reached a decision. *High Denial Areas* are the census tracts where denial per capita in the pre-event year 2004 was more than the regional mean of denials across the census tracts. *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). The coefficient associated with the *Treat* $\times$ *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. All regressions include *Border* $\times$ *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 $\times$ Post	-0.003	-0.003	-0.003**	-0.003*	-0.002	-0.002	-0.002	-0.002	-0.009***	-0.010***
	(-1.47)	(-1.52)	(-2.01)	(-1.80)	(-1.03)	(-1.17)	(-1.43)	(-1.35)	(-2.92)	(-3.95)
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 $\times$ 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	0.532	0.575	0.568	0.267	0.264	0.319	0.320	0.340	0.338
Observations	81871	76437	38737	36447	81871	76437	38737	36447	81871	76437

Table VIII: First-time Homebuyers and Mortgage Interest Rate 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 homebuyer is not missing, calculated in at zip3-state area level. The regression specification is:

$$\text{Y}_{zsjt} = \beta_0 + \beta_1 \text{Treatment}_{zsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt}. \quad \text{See Eq. (3).}$$

In columns (3) and (4), the dependent variable is interest rate on the GSE mortgages, and the regression specification is:

$$\text{Interest Rate}_{izsjt} = \beta_0 + \beta_1 \text{Treatment}_{izsj} \times \text{Post}_t + \delta \times \text{Controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{izsjt}. \quad \text{See Eq. (4).}$$

Economic Controls include annual growth rate of county income per capita, county aggregate employment, and state gross domestic product (GDP). *Mortgage controls* refer to *credit score* and *combined loan-to-value* (*CLTV*) in column (3); it refers to three more controls *debt-to-income ratio*, *number of units in the property*, and *mortgage insurance percentage* in column (4). The coefficient associated with the *Treat* $\times$ *Post* interaction term captures the change in dependent variable in the treated zip3-state areas vis-a-vis the control. 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.

	First-time Borrower Ratio		Interet Rate (%)	
	(1)	(2)	(3)	(4)
Treat $\times$ Post	0.011**	0.010**	0.009***	0.010***
	(2.55)	(2.32)	(13.36)	(12.06)
Economic Controls	No	Yes	-	-
Mortgage Controls	-	-	Yes	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.)	0.697	0.697	0.731	0.758
Observations	7594	7594	7579052	3512619
Reg. Unit	Zip3-state Aggregate	Zip3-state Aggregate	Individual Mortgage	Individual Mortgage

Table IX: Effect Heterogeneity by Lenders Density

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

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

Low (*High*) identifies a census tract having a lower (higher) number of HMDA lenders than the *regional mean* number of HMDA lenders (per census tract) within the bordering counties between the given control state and all the treatment states surrounding it in 2004 (See Footnote 21). *Difference* [*High - Low*] shows the result of the t-test for the difference in coefficients of $Treat \times Post$ in specifications *High* and *Low*. The dependent variable in columns 1 through 4 is volume of mortgages originated (in 1000 USD) per adult in a census tract. The dependent variable in columns 4 through 8 is the approval ratio of mortgage applications at census tract-level. $Economic\ Controls$ 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). The coefficient associated with the $Treat \times Post$ interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. 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				Approval Ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low	High	Low	High	Low	High	Low	High
Treat × Post	0.002**	0.001	0.003***	0.002*	0.015***	0.010*	0.016***	0.009*
	(2.21)	(1.16)	(3.06)	(1.66)	(3.07)	(1.92)	(3.15)	(1.73)
Difference [High - Low]		-0.001		-0.001		-0.006		-0.007
p-value		(0.592)		(0.498)		(0.474)		(0.413)
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	0.571	0.636	0.616	0.751	0.716	0.740	0.709
Observations	60210	25497	56188	24073	57134	24629	53135	23212

Table X: Did Origination Increase due to Rise in Private 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:

$$Y_{icsjt} = \beta_0 + \beta_1 \text{Treatment}_{icsj} \times \text{Post}_t + \delta \times \text{Economic Controls} + \alpha_i + \gamma_{jt} + \varepsilon_{icsjt}. \quad \text{See Eq. (1).}$$

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. *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). The coefficient associated with the *Treat* $\times$ *Post* interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. 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 $\times$ Post	-0.004	0.001	0.048^{**}	0.047^{***}	0.001	0.002
	(-0.28)	(0.05)	(2.49)	(2.78)	(0.11)	(0.52)
Economic Controls	No	Yes	No	Yes	No	Yes
Census Tract FE	Yes	Yes	Yes	Yes	Yes	Yes
Border $\times$ 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	0.055	0.028
Observations	81871	76437	81871	76437	81871	76437

Table XI: 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:

$$Y_{zsjt} = \beta_0 + \beta_1 \text{Treatment}_{zsj} \times \text{Post}_t + \delta \times \text{Economic controls} + \alpha_{zs} + \gamma_{jt} + \varepsilon_{zsjt}. \quad \text{See Eq. (3).}$$

The dependent variable in column 1 is *Number of mortgages originated to Prime Borrowers* (credit score$\geq$620) in a given zip3-state area. The dependent variable in column 2 is *Number of applications to subprime borrowers* (credit score$<$620) in a given zip3-state area. *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). 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. 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 × 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 × 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 XII: Effect of Free Credit Reports on Banks

This table reports the estimates of the treatment effect on banks. The regression specification is:

$$Y_{bt} = \beta_0 + \beta_1 \text{Treatment}_b \times \text{Post}_t + \delta \times \text{Bank Controls}_{bt} + \alpha_l + \gamma_t + \varepsilon_{bt}. \quad \text{See Eq. (5).}$$

NIM is Net Interest Margin. It is the ratio of net interest income to earning assets, expressed in percentage. *RoE* is Return on Equity. It is the ratio of net income to book value of equity, expressed in percentage. *RoA* is Return on Assest. It is the ratio of net income to book value of total assets, expressed in percentage. Bank Controls include: natural log of the total assets expressed in USD 1000; share of liquid assets in total assets, expressed in percentage; and cost of deposit (ratio of total interest expense to total earning assets, expressed in percentage). *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). The coefficient associated with the $Treat \times Post$ interaction term captures the change in the dependent variable in the treated census tracts relative to the control census tracts. All regressions include $Border \times Year$ fixed effects (FE) and *Census Tract* 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)
	NIM (%)	NIM (%)	RoE (%)	RoE (%)	RoA (%)	RoA (%)
Treat × Post	0.06***	0.06***	0.75***	0.76***	0.07***	0.08***
	(5.49)	(5.96)	(5.13)	(5.32)	(5.18)	(5.53)
Bank Controls	No	Yes	No	Yes	No	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster (Bank)	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (Adj.)	0.807	0.814	0.586	0.597	0.556	0.573
Observations	85847	85847	85847	85847	85847	85847