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Since the Great Recession, 11 states have restricted employers' access to the credit reports of job applicants. We document that county-level vacancies decline between 9.5 and 12.4 percent after states enact these laws. Vacancies decline significantly in affected occupations but remain constant in those that are exempt, and the decline is larger in counties with many residents who are subprime borrowers. Furthermore, subprime borrowers fall behind on more debt payments and reduce credit inquiries post-ban. The evidence suggests that, counter to their intent, employer credit-check bans disrupt labor and credit markets, especially for workers who are subprime borrowers.

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"We want people who have bad credit to get good jobs. Then they are able to pay their bills, and get the bad credit report removed from their records. Unfortunately, the overuse of credit reports takes you down when you are down." --Michael Barrett (State Senator, D-Lexington, MA).

1) Introduction

During the last twenty years, credit-reporting agencies have found a new market for credit reports: employers deciding whether to extend a job offer to an applicant. The three largest credit reporting agencies (Experian, Equifax, and TransUnion) currently offer the service, and a 2009 survey of human resource managers at Fortune 500 companies found that 60 percent used credit reports in hiring decisions (Society for Human Resources, 2012). Additionally, a 2012 survey by the policy group DEMOS found that 25% of low- to medium-income households reported having their credit checked for a job application, and 10% claimed to have been denied a job because of bad credit (DEMOS, 2012).

In response to high unemployment and worsening credit conditions during the Great Recession, lawmakers introduced legislation to limit employer credit checks at the city, state, and national levels.[2] Eleven states have banned employer credit checks as of April 2017, the geographic distribution of which can be seen in figure 1. Lawmakers voice concern that employer credit checks may create a poverty trap. Brad Lander, who sponsored a 2015 credit-ban bill passed by New York City, provided a typical explanation for introducing the legislation: "Millions of Americans who have bad credit, would also be great employees," he said. "What they need to repair their credit is a job, and to make it harder for them to get a job is the definition of unfair" (Vasel, 2015).

In this paper, we estimate the response of key labor and credit market outcomes to the implementation of employer credit check bans. When a state bans employer credit checks, the average county experiences a substantial fall in vacancy creation relative to trend, by about 12 percent. This decline in job creation is likely caused by the bans since vacancies are unaffected in occupations that are exempt, but fall significantly in occupations subject to restrictions on employer credit checks. Furthermore, within states that pass a ban, we estimate a larger fall in

[2] While these laws typically restrict the use of credit checks without necessarily banning them outright, we will refer to them as "bans" for expositional simplicity.

vacancies in counties with a higher share of residents with subprime credit. We find no evidence that these laws improve labor or credit market conditions and are likely counterproductive.

If credit-check bans cause a deterioration in the labor market, then we may expect spillovers in to credit markets, either because people have less income with which to pay their bills or because their incentives to repay are weakened. A novel result in our paper is to document this spillover by studying individual credit reports. We find that sub-prime borrowers' reports show more delinquencies and fewer new credit inquiries after the ban, which is consistent with these individuals being less able (or willing) to make payments or access new lines of credit. Taken together, our results raise doubts that credit check bans have had their intended effects of improving labor and credit markets, especially for the targeted population.

Our estimates are consistent with the theoretical implications of Corbae and Glover (2017), henceforth "CG." They build a general equilibrium model in which households make borrowing and default decisions, which affect their credit histories. Credit histories provide information to employers because repayment rates are positively correlated with an unobservable component of worker productivity.[3] In a calibrated version of their economy, CG find that preventing employers from observing a job applicant's credit report increases the job-finding rate for bad-credit job seeker, but can reduce overall vacancy creation and thereby make it harder for the average (or median) job seeker to find employment. While we cannot measure the effects for bad-credit workers, because we do not have data with individual credit histories and employment status, our estimates imply a negative aggregate effect on job creation.[4]

Corbae and Glover's model also demonstrates how policies in the labor market may spill over into credit markets. Since bad credit no longer affects a worker's job-finding rate, her incentive to repay on time may be weakened, thereby reducing her repayment rate. In turn, lenders may restrict credit access to such borrowers. Our findings that subprime borrowers

[3] Corbae and Glover generate this correlation through worker heterogeneity in patience and unobservable effort in human capital accumulation. More patient workers have more (unobservable) human capital and higher repayment rates, since both require a costly investment in the present and accrue benefits in the future. In reality, it is not clear why employers value workers with good credit. As long as employers think that credit has signaling value, regardless of its actual correlation with worker productivity on the job, one can generate such a link between both labor and credit markets.

[4] Friedberg, Hynes, and Pattison (2017) find that people who report financial distress experience a reduction in unemployment duration following employer credit check bans. Their data does not have individual credit reports, so they must proxy using survey responses.

become delinquent on more accounts and have fewer new credit inquiries following a ban are consistent with these predictions.[5]

Our empirical approach leverages the staggered implementation of bans to estimate a difference-in-difference regression model. The labor market effects are primarily estimated using county-level observations from the Conference Board's Help Wanted Online (HWOL) data, measuring vacancies. We also use unemployment rate data from Local Area Unemployment Statistics (LAUS) program and aggregated data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (FRBNY CCP/Equifax) credit panel.[6] We estimate the average effect of an employer credit check ban using the change in log of vacancies (job postings) in a treated county (relative to trend) versus the change for an untreated county.[7]

We are particularly concerned with choosing an appropriate control group and verifying that treated and untreated counties look similar prior to the ban. We address this in three ways. First, we estimate our baseline specification using border counties, which allows us to control for arbitrary unobservable trends in local labor market conditions shared between adjacent counties. Second, we estimate the dynamic effects of bans, before and after they are enacted. Finally, we leverage within-state heterogeneity in exposure to bans by occupation (exempt versus non-exempt) and county-level sub-prime rates.

We follow a similar approach for credit market outcomes, but here we rely on individual borrower-level observations from FRBNY CCP/Equifax. This dataset provides us with a rich set of credit variables such as credit inquiries, credit scores, usage, and delinquencies. We also estimate the effects on credit variables by racial group, for which we use the Transunion/Epsilon credit panel. This unique data set has credit report data from Transunion, merged with demographic data from the marketing firm Epsilon.

[5] We say that our results are consistent with this theory rather than support it because other plausible mechanisms may be at play. For example, borrowers may experience longer unemployment durations, which could lead them to become more delinquent and have less access to credit.
[6] Federal Reserve Bank of New York Consumer Credit Panel/Equifax data is a subset of credit data maintained by Equifax, one of the large credit reporting agencies. The data are from a 5 percent sample of all individual credit records that Equifax maintains.
[7] We refer to a county as treated during a given quarter if it is located in a state with employer credit check bans in effect at that time. We estimate the model both statewide and using only contiguous counties.

To our knowledge, ours is the first study to analyze the effect of employer credit-check bans on local labor demand (i.e. job postings). We are also the first to consider the effect of these bans on credit markets and thereby highlight an overlooked potential cost of the policy. We are among the first to study the effect of these laws on labor market outcomes *in general*, though two recent papers by Clifford and Shoag (2016) and Bartik and Nelson (2016) are closely related. While they consider different outcome measures and implement different empirical strategies, these studies also find that employer credit-check bans have negative labor-market effects on their targeted populations.[8]

Clifford and Shoag (2016) estimate the effect of bans on log-employment at the census-tract level, using annual data Substantively, we differ in two ways. First, we use data on vacancies and unemployment rate rather than total employment. The ban's effect on vacancies and the unemployment rate are more salient than employment since they are directly related to individual's probability of being employed. For example, total employment may rise mechanically if the law motivates labor-force entry or immigration to the state post ban, which is nonetheless a negative outcome for the average worker if vacancies do not rise in proportion. Second, our use of quarterly data allows for an accurate coding of the effective date of bans.

Bartik and Nelson (2016) use the panel dimension of the Current Population Survey (CPS) as well as data aggregated from state unemployment insurance records to estimate the effect of employer credit-check bans. They are primarily focused on the average flow rates for different racial groups in the labor market and only find conclusive results for blacks. They report that bans reduced job-finding rates and increased the separation rates for blacks significantly. These results are consistent with the main findings in our paper. By highlighting the effects on the vacancies and unemployment rate, we complement the conclusion of Bartik and Nelson (2016).

Our paper is also related to several recent papers that study the interactions between the labor market and the credit markets, especially via the use of credit market information, e.g., Bos, Breza and Liberman (2015); Herkenhoff (2015); Herkenhoff, Phillips and Cohen-Cole (2016); and Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016). The most relevant

[8] These groups include individuals with mid to low credit scores, young workers, and blacks.

comparison is Bos, Breza and Liberman (2015), who study a regulatory change in Sweden that removed negative information (bankruptcy, defaults) from some borrowers' credit reports. They find that this change led to higher employment rates for the affected groups. Though the affected group is a limited segment of the population (previously defaulted pawnshop borrowers) in a different country (Sweden), their results confirm that credit market information can affect labor market outcomes.

In the context of American credit and labor markets, Herkenhoff, Phillips and Cohen-Cole (2016) and Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016) use the removal of the individual bankruptcy flag from consumer credit report as an instrument to estimate the effect of credit worthiness on labor market outcomes. The former study finds that bankruptcy flag removal affects labor supply: As credit terms improve, displaced workers take longer to find jobs and receive slightly higher wages upon reemployment, implying better sorting. Our estimated increase in unemployment rates is consistent with their results, though we find insignificant effects on earnings.

Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016) rely on the differential effects of the flag removal on labor market outcome variables for Chapter 13 filers relative to Chapter 7 filers. A Chapter 7 filer's default flag appears on her report for ten years after bankruptcy, while a Chapter 13 filer's flag is removed after only seven years. Based on outcomes for Chapter 13 filers within the three-year window after which their default flag is removed, Dobbie et al. estimate zero effects on employment and earnings and conclude that labor demand is insensitive to credit worthiness. Contrary to their estimates, we find significant effects in the labor market in response to credit check bans, especially for our more direct measure of labor demand, vacancies. We reconcile our results with theirs by noting that a seven- to ten–year-old bankruptcy flag may provide employers with little significant information about a potential hire since life-cycle components of labor productivity and other observable labor market experiences during the first seven years after bankruptcy likely swamp any signal provided by the bankruptcy flag.[9]

[9] Moreover, public sector employers in the United States are not allowed to use bankruptcy filings in hiring decisions. If, at the margin, employers respond to this constraint by hiring or retaining workers with a bankruptcy flag in order to ensure compliance, then the estimated effect of credit on labor demand will be biased towards zero.

2) Data and Empirical Approach

Table 1 details the timeline of law changes across states and Figure 1 maps the states that currently have laws in effect as of April 2017. Throughout our empirical analysis, we focus on the period 2005:Q1 through 2016:Q4. We use the date at which the law became enforceable to code our treatment flag, with the convention that dates falling within a quarter are coded as the beginning of that quarter (8 of 11 states began enforcing their bans at the start of a quarter). The resulting summary statistics for this flag are seen in the last columns of table 2. More than 10 percent of counties are affected by the credit check bans at the end of our sample period (out of 3,137) covering about 26.5 percent of the U.S. labor force.

a) Labor Market Data

Our principle labor market outcome is the county-level vacancy (job opening) data reported by the Conference Board (2017) as part of its Help Wanted OnLine (HWOL) data series. HWOL provides a monthly snapshot of labor demand at detailed geographical (state, metropolitan statistical area, and county) and occupational (6-digit SOC and 8-digit O*Net) levels since May 2005.[10] For the period in question, HWOL represents the bulk of the advertised job openings, as print advertising ceased to exist.[11] HWOL covers roughly 16,000 online job boards, including corporate job boards, and aims to measure unique vacancies by using a sophisticated unduplication algorithm that identifies unique advertised vacancies on the basis of several ad characteristics such as company name, job title/description, city, or state. In addition to reporting the total number of vacancies, HWOL data also include new vacancies that do not appear in those of the previous month. HWOL is not the only source of data on job openings, though. The Bureau of Labor Statistics (BLS) publishes nationally representative data, the Job Openings and Labor Turnover Survey (JOLTS), which measures also vacancies. However,

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[10] For a detailed description of the measurement concepts and data collection methodology, please see (The) Conference Board. (2017). *The Conference Board Help Wanted OnLine® (HWOL) at* https://www.conference-board.org/data/helpwantedonline.cfm.

[11] In fact, HWOL started as a replacement for the Conference Board's Help-Wanted Advertising Index of print advertising.

HWOL's detailed geographic- and occupation-level coverage makes it uniquely suitable for the empirical question we have in mind.[12]

Figure 2.a displays the average level of vacancies over time for two groups of states: treated and untreated.[13] On average, vacancies are higher in treated states, the list of which includes some populous states such as California and Illinois. Vacancies are procyclical for each group. Table 2 displays summary statistics for the log of vacancies at the county-level, the level of observation we use in our regression analysis. The average county seems to trend up during the sample period, briefly interrupted with a minor decline from 2008 to 2009. Taken together, these statistics suggest a large variation in the vacancy data across counties and states.

We prefer vacancies as our labor market measure, since it is closest to labor demand and allows us to exploit variation by exempt and affected occupations, but we are also interested in broader labor market variables. Specifically, we measure the change in unemployment, job-finding, and separation rates following a ban. We use county-level unemployment rates, as reported by the Bureau of Labor Statistics' LAUS program.[14] The job finding and separation rates we use are computed from CPS microdata at the state level. Since respondents in the survey are interviewed repeatedly for certain months one can create a panel from the observed transitions of workers.[15]

[12] JOLTS' publicly available data files do not have more detailed coverage than census regions and lacks any information on occupational characteristics. For most of the sample period, the general patterns reported in JOLTS and HWOL are reasonably close to each other. See, for instance, the relevant discussion in Sahin, Song, Topa and Violante (2014). Researchers identified a recent diversion between vacancy measures across these two sources, one which is attributed to a change in pricing on several online job boards (Cajner and Ratner, 2016). To the extent that fixed effects in our empirical specifications absorb these pricing changes, our results will be immune to significant bias by relying on HWOL.

[13] Note that the set "Ever Treated" includes all of the 11 states that had, at some point during the sample period, a credit check ban in effect. Similarly, the "Never Treated" group consists of the remainder of the states. Hence, each line has the same set of states consistently, even though states enacted their laws on different dates. The averages plotted in the figures use each state's labor force to weight the relevant labor market variable.

[14] We also have county-level observations for employment and labor force through LAUS. All these estimates for counties are produced through a statistical approach that also uses data from several sources, including the CPS, the CES program, state UI systems, and the Census Bureau's American Community Survey (ACS), to create estimates that are adjusted to the statewide measures of employment and unemployment.

[15] Each responded is interviewed for four months initially and then leaves the survey for eight months. They are interviewed again for four more months. For any given month, about 70 percent of the survey respondents are observed consecutively allowing us to create a panel to measure average job finding and separation hazards. Unfortunately, due to the size of the CPS and number of transitions, we cannot obtain estimates for more granular level than state. We follow Nekarda (2009) to minimize the bias induced by mobility.

Table 2 shows the summary statistics for unemployment rate on an annual basis. There is a clear rise in the average level nationwide throughout the Great Recession, as along with an increase in the standard deviation across counties during this same time period. The best source of household-level labor market data is the CPS. Unfortunately, sample size becomes relatively small at the county level for most counties in the U.S. This feature of the CPS limits us to LAUS for the county-level data on unemployment rates. Nevertheless, as in Bartik and Nelson (2016), we also use the CPS to get state-level unemployment rates in addition to job-finding and separation rates.

For our purposes, the relevant source of variation in unemployment is between counties in states that have enacted a ban at any time in our sample period and those that have not. This can be seen in figure 2.b, which plots the average unemployment rate by treated and untreated states over time Until the Great Recession, the unemployment rates in treated and untreated states were quite similar, but a gap began to appear in 2007:Q3 and grew throughout the recession. As the vertical lines indicate, most states enacted their credit check bans after the Great Recession. The difference between treated and untreated states was nearly 2 percentage points in 2010 and has only recently begun to shrink. We certainly cannot conclude from this plot alone that the laws have caused this decline, but it offers additional illustration of our findings.

The heterogeneity in unemployment rates between states with the ban and those without can be further understood by comparing job flows. Specifically, figure 2.c compares the job-finding rate between these two groups, and figure 2.d compares the separation rate. Starting around the same time as the divergence in unemployment rates, the job-finding rate is slightly lower in the states that have banned employer credit checks, but this rate alone is not enough to account for the difference in unemployment rates. The larger difference is between separation rates, which may mean that short-term employment spells have taken the place of credit checks as a screening device.

b) <u>Credit Market Data</u>

The FRBNY CCP/Equifax panel provides detailed quarterly data from Equifax on a panel of U.S. consumers and includes Equifax Risk Scores (credit scores) and other data on consumer credit reports. We use the data on consumer credit scores and estimate the effect of the ban as a function of the credit-score conditions within a county. The distribution of subprime borrowers across counties and over time is found in table 3. For this paper, we follow the literature and assume that the critical level for being subprime is an Equifax risk score of 620. Over the sample period we analyze, the average fraction of subprime borrowers within a county was 27 percent, declining from 29 percent to 25 percent over time. There is substantial variation across counties in our sample: A county in the 95th percentile has more than 45 percent of borrowers with subprime credit scores over the sample period compared to 20 percent for a county in the 25th percentile. This variation helps us identify the differential effects of the credit check bans for subprime households, which are commonly targeted by these policies.

The rich panel structure of the credit data allows us to test the effects at the individual level. Although the credit panel contains detailed geographic and credit information, it does not contain demographics beyond age. As Clifford and Shoag (2016) and Bartik and Nelson (2016) find, the bans had more pronounced effects on particular demographic groups. To compensate for this lack of demographic information in the FRBNY CCP/Equifax, we use the Transunion/Epsilon Credit Panel to measure variation across demographic groups. This credit panel provides the same credit market information as Equifax and also has merged demographic information from the marketing firm Epsilon.

Table 4 shows that the two panels are similar for the two credit market outcomes of interest, inquiries and delinquencies. Credit data in FRBNY CCP/Equifax are measured quarterly, and the mean number of credit inquiries during the previous three months is 0.6, whereas TransUnion is measured annually and has a mean of 0.8 from credit inquiries during the last six months. Breaking the data down by race shows that blacks request credit slightly more than Hispanics and whites.

Panel A of table 4 shows that on average, 12 percent of loans are delinquent for the FRBNY CCP/Equifax sample, whereas 10 percent are delinquent in TransUnion. Black borrowers are slightly more likely to have delinquent accounts: roughly 18 percent compared to whites at 8 and Hispanics at 13 percent.

The average credit score for Blacks in our sample is 567, so the higher number of credit inquiries and the higher percentage of delinquent loans are consistent with this average. Hispanics have better credit scores in the sample, with an average of 631. Whites have an average credit score of 720 and represent the largest subsample in our data.

Panel B of table 4 presents the same descriptive statistics for the group of subprime borrowers in both datasets. As expected, this group consists of borrowers who, on average, have more credit inquires and larger delinquent loan balances compared to the overall sample of borrowers. However, among subprime borrowers, different demographic groups do not show much of a difference in terms of total inquiries or delinquent balances. Substantially different means in credit market outcomes across races in panel A seems to be mostly explained by the fraction of subprime borrowers in each group.

c) Policy Endogeneity

The typical motivation for credit check bans has the poverty trap story in mind: workers lose their jobs, which causes them to fall behind on debt payments, which then makes it harder for them to find jobs because their credit rating declines. CG (2017) show that this is theoretically possible in a general equilibrium model. If the passage of these bans is correlated with our variables of interest, then our estimates cannot be interpreted as causal. We attempt to control for this in our panel regressions with fixed effects at extremely local levels, including a fully flexible local time trend using the adjacent-border county specification. We also test for pre-trends in an extension of the baseline specification with distributed lags in the treatment variable. In addition, we exploit the unique nature of the exemptions embedded in credit check bans for additional policy variations. These additional checks alleviate concerns about policy endogeneity for the primary variable of interest, vacancies. We leave the discussion of these formal checks for the next section and here, instead, focus on various salient labor and credit market variables in treated and untreated states over time to make the same argument.

The unemployment rate shows no difference between treatment groups until the end of the Great Recession (figure 2.b). The divergence following the recession implies a significant difference in response to the credit check bans at the county level in our empirical analysis. More interestingly, we see that poverty rates display a distinct difference between the treatment groups, albeit inconsistent with the endogeneity concern that states pass employer credit check bans because of poor economic conditions (figure 3.a). The states that enacted these bans, the treated states, had uniformly lower poverty rates than the untreated states. Although poverty rates increased substantially after the Great Recession, treated states do not stand out as high poverty areas.

The same picture emerges if we look at median household income or the fraction of subprime borrowers across states (figures 3.b and 3.c). Median household income in all states stagnated from 2007 through 2013. Nevertheless, treated states had substantially higher median household incomes throughout the sample period, higher by almost $10,000. Finally, figure 3.d shows that the treated states had lower delinquency rates on average than the untreated states. In the absence of this evidence, one might claim that the credit check bans are merely a response to adverse credit and labor market conditions in certain states. On the contrary, treated states had more favorable economic conditions than non-ban states.

3) Results

We use a county-level panel with labor market data and an individual-level credit panel to test the effects of the employer credit check bans. We primarily estimate the effect on job creation (measured by the number of help wanted ads posted online) using county-level data, and we use the credit panel to estimate the effects on credit market outcomes for individual borrowers.

a) Effects of the Credit Check Bans on Job Creation

We begin by estimating the following regression at the county level:

$$log(vacancies_{i,t}) = \alpha_i + \gamma_t + \beta\, Ban_{i,t} + \varepsilon_{i,t}, \qquad (1a)$$

where α_i is a county fixed effect and γ_t is a time fixed effect. The coefficient of interest in this regression, β, is identified from the average growth in vacancies (HWOL data) for a county in a treated state before and after the ban was passed, relative to the national average in the same periods. The estimated coefficient for vacancies is found in column (1) of table 5: it is statistically significant and economically large, implying a 12.4% decline in vacancies after the ban goes into effect. Column (2) shows that the estimates are similar for new vacancies alone (those posted in that quarter), though slightly smaller at 9.3%. Since the ban may reduce total vacancies because some employers withdraw those that they posted previously, we refer to the estimate using total vacancies as our baseline.[16]

This strong negative effect on job openings is counter to the goals of policymakers if it is in fact caused by the ban. However, we cannot observe the counterfactual – vacancies may have fallen even more in these counties if a ban had not been enacted. We attempt to address this concern by exploiting the structure of employer credit check bans: Every state that implemented these bans included some exemptions for specific occupations. These exemptions typically focus on occupations in which workers have access to financial or personal information (handling cash or access to payroll and social security information, for instance) on the job. We can therefore estimate the change in vacancies for occupations that are typically exempted from bans versus those that are typically covered.

We first divide vacancies into two groups for each county-quarter observation: exempt and nonexempt. The exempt group includes all vacancies that the Conference Board identifies as belonging to three specific 2-digit SOC codes, Business and Financial (SOC-13), Legal (SOC-23), and Protective Service (SOC-33) occupations. The nonexempt group consists of all other vacancies. Columns (3) and (4) in table 5 present the regression results for these cases when the dependent variable corresponds to total vacancies in exempt and nonexempt occupations, respectively. Vacancies in exempt occupations fall roughly an order of magnitude less than in affected occupations (-1.7% versus -12.6%). Furthermore, the decline is statistically indistinguishable from zero for exempt occupations, but statistically significant for affected occupations. The difference in effect between exempt and affected occupations suggests that bans *cause* a decline in vacancies in occupations where it is enforced.

In our baseline specification described in equation (1a), our control group consists of counties across the nation that have not been treated, no matter how spatially distant they are from those with a ban. This approach might induce bias in our estimates if there are unobserved differences at the county level that are time varying. This would be the case, for instance, if job

[16] Additionally, we run our regressions at the quarterly frequency by averaging the monthly numbers for the quarter, and this method might partly disguise high-frequency variation in new vacancies, making total vacancies more informative.

openings had time-varying differences in their trends between the treated counties and the untreated ones. This issue has long been recognized in the literature, leading researchers to focus on the adjacent counties along a state's border, effectively making the untreated neighboring county the relevant control and therefore controlling for flexible unobservable trends in local labor markets.[17]

We estimate the effect of employer credit check bans using only adjacent counties across state lines. Counties are grouped along with all neighboring counties outside of their respective states to form a cross-sectional unit of observation for county *i* in pair *p*. Our treatment group consists of counties along the border of a state after that state passes a ban, and our control is all other county pairs. The effect is identified from the change in vacancies in a treated county relative to the change in it's untreated paired neighbor. Following the specification of Dube et al. (2010), we link the county pair through a time-varying fixed effect and estimate the equation as follows:

$$log(vacancies_{i,p,t}) = \alpha_i + \gamma_{p,t} + \beta\, Ban_{i,t} + \varepsilon_{i,t}\,,, \qquad (1b)$$

The estimated β coefficient is found in column (1) of table 6 with standard errors clustered at both the state and border levels. It is reassuring that these estimates are significant and imply a similar decline to those that we observe in the nationwide estimates from equation (1a). The county-pair time fixed effect means that the ban's effect is identified from the treated county's change in vacancies relative to its nontreated neighbor in each period after the ban is enacted. Our estimate implies that a county in a postban state experiences a 9.5 percent decline in vacancies relative to the decline in an adjacent county in a nonban state, a number which is only slightly smaller than the 12.4 percent decline from the nationwide estimate. As in the previous model, we estimate the ban's effect on new vacancies alone and the effect on exempt versus nonexempt occupations only. A similar pattern emerges, and the causality suggested by our previous specification extends to the adjacent-county specification: Job postings for exempt occupations are essentially constant, whereas vacancies for nonexempt occupations decline by a statistically significant 9.7 percent (columns (3) and (4) of table 6).

[17] See for instance Dube, et al (2010), in the context of the effects of minimum wage legislation.

These estimates show that the decline in vacancies is robust to controlling for local unobservable labor market heterogeneity. In practice, however, we must be careful when extrapolating the contiguous-county estimates to interior counties. For example, the effect may be overstated if much of the decline in vacancies within a county is due to employers' relocating to the neighboring (untreated) state. On the other hand, the effect may be understated if many workers in the treated counties were already working in the adjacent state before the ban was implemented. We therefore prefer the nationwide estimate, but take confidence from the fact that the adjacent-border-county estimates are similar.

b) Dynamic Effects of the Ban and Pretreatment Trends

Our empirical specifications expressed in equations (1a) and (1b) measure the average effect in the long-run. It is conceivable that the effects on labor demand locally might change over time following the legislation and that the long-run coefficients we reported in those specifications might change significantly. On the other hand, vacancies may have been declining in treated states prior to the treatment, even relative to trends. We therefore estimate a flexible distributed-lags specification that captures vacancy dynamics around the implementation of bans. This approach has been found to be especially useful for studying the effects of staggered implementation of the treatment (policy change) across different jurisdictions with a difference-in-difference identification strategy.[18] Specifically, we allow for four-quarter leads of the credit check ban and up to four-quarter lags in the following form:

$$log(vacancies_{i,t}) = \alpha_i + \gamma_t + \beta_{-4} Ban_{i,t-4} + \beta_{-3} Ban_{i,t-3} + \beta_{-2} Ban_{i,t-2} + \beta_{-1} Ban_{i,t-1} + \beta_0 Ban_{i,t} + \beta_{+1} Ban_{i,t+1} + \beta_{+2} Ban_{i,t+2} + \beta_{+3} Ban_{i,t+3} + \beta_{+4} Ban_{i,t+4} + \beta_{>4} Ban_{i,t>4} + \varepsilon_{i,t}, \quad (2)$$

Typically, one hopes to estimate small and insignificant values for the coefficients indexed prior to the ban being implemented. If these coefficients were significantly negative then it would suggest that vacancies had fallen prior to the ban being implemented, raising the spectre of reverse causality. As reported in table 7 and illustrated in figure 4, we estimate insignificant and small coefficients for β_{-4} through β_{-2}, which alleviates these concerns. Interestingly, we estimate a positive estimate for β_{-1}, which may indicate that employers increased recruitment

[18] Some examples include Bertrand and Mullainathan (1999 and 2003) in the context of antitakeover legislation and Meer and West (2016) in the study of minimum wage legislation.

efforts in anticipation of losing credit checks as a screening device. Following the ban, the coefficients become both economically and statistically negative within two quarters and remain so, even beyond a year after the ban is implemented. The coefficient $\beta_{>4}$ captures the change in vacancies for treated counties, relative to untreated, after the first year of the ban and is close to our baseline estimate that ignored dynamics.

Finally, we leverage occupational variation in exemptions from bans in the dynamic framework. For the exempt group of occupations, there is no significant difference in vacancies before or after the ban, as expected. Affected occupations, however, experience a significant fall in vacancies, which persists after the first year. As with the unconditional estimates, there is no significant difference for affected occupations before the ban. Figure 5 presents this differential effect on exempt and nonexempt industries dynamically. These estimates further suggest that employer-credit-check bans cause a decline in job creation in occupations where they are enforced.

c) Effects on Other Labor Market Variables

We prefer vacancies as our labor market outcome because it is most directly related to labor demand and we can leverage occupational variation in ban coverage, but policymakers may be more concerned with other labor market variables, such as the unemployment rate, job-finding rate, or job-separation rate. Furthermore, these aggregates are determined jointly in equilibrium, so provide a holistic view of labor-market changes following the ban.

In table 8, we report how the unemployment rate changes post ban, both in the nationwide sample and the adjacent county sample. As seen in columns (1) and (2), there is a small positive, though statistically insignificant, change in the unemployment rate following a ban. The unemployment rate may not respond to the ban for various reasons. For example, the unemployed may search harder after the ban (because, for example, they have less access to debt and therefore cannot smooth their consumption as well). Whatever the reason, the fact that the unemployment rate does not *fall* substantially, while vacancies decline, means that the labor market becomes slacker after the ban (the ratio of job openings to job seekers falls).

We would expect a decline in market tightness to translate into a lower job-finding rate. We may also expect some change in the separation rate, though this could move in either direction. For example, if firms are less able to screen for good matches ex-ante, then there may be more workers fired after a short spell of employment. On the other hand, if workers know that they will have a harder time finding work upon quitting an existing job then we may see a decline in separations. We therefore estimate a state-level regression using finding and separation rates as the dependent variables, the results of which are seen in columns (3) and (4) of table 8.[19] The job-finding rate falls by 2.7% post ban (this decline is marginally statistically significant with Z-stat of -1.5), which is consistent with declining market tightness, while the separation rate rises by a similar amount (although this estimate is less precise).

d) Labor Market Effects by County-level Subprime Rates

We find evidence that employer credit check bans have significant adverse effects in the labor market. Even if these bans have a negative effect on average, however, lawmakers (and their constituents) may be in favor of a ban if it helps regions or individuals with bad credit. For the labor market, we estimate whether this equity-efficiency tradeoff applies using the following county-level regression:

$$L_{i,t} = \alpha_i + \gamma_{j,t} + \beta_0\, Fraction\ Subprime_{i,t} + \beta_1\, Ban_{i,t} {}^{*} Fraction\ Subprime_{i,t} + \varepsilon_{i,t}\,, \qquad (3)$$

Here again, i indexes the county, and α_i are county-level fixed effects. The dependent variable $L_{i,t}$ stands for either the log of vacancies or the level of unemployment rate. Because we now have within-state variation in the treatment (the interaction of the ban with the fraction of subprime borrowers in the county) we can introduce state-time fixed effects. To do so, we index states by j so that $\gamma_{j,t}$ are state-time effects. In this empirical specification, the credit check ban is interacted with a county's fraction of borrowers with a credit score less than 620 in county i and time t, denoted here by *Fraction Subprime*. We also adapt the distributed-lags specification expressed in equation (2) in this context by replacing the leads and lags of the $Ban_{i,t}$ variables in the interaction term. Table 9 displays the regression results for equation (3) and its distributed-lags version for both dependent variables.

The estimated effect reported for vacancies in column (1) of table 9 is negative for the interaction term. Although the estimates are statistically insignificant at traditional levels, the point estimate implies that bans are actually worse for counties with many subprime households.

[19] We use state-level data for these rates because they are not well measured at the county level.

Moreover, the dynamic specification reported for all occupations in column (2) confirms this negative relationship, this time significantly, for every horizon beyond a quarter after the ban is enacted. The distinction between exempt and nonexempt groups also holds in this specification (columns (3) and (4)), suggesting that bans cause a larger decline in vacancies with many subprime residents. The last two columns of table 9 show that the negative effects on the unemployment rate are exacerbated for counties with more individuals with subprime credit.

To give some context to the magnitude of the decline in vacancies and the rise in unemployment rate vis-à-vis the fraction of subprime borrowers in a county, we plot in figures 6 the interquartile range difference predicted by the regression output in table 9. The range of county subprime shares in table 3 gives context for this estimate: the interquartile difference is 15 percentage points. Therefore, as a state bans employer credit checks, a county in the 75th percentile (which has a 34 percent subprime rate) would start experiencing an 8 percent fall in vacancies relative to a county in the 25th percentile (where 19 percent of residents are subprime), which gradually rises to 12 percent by the third quarter after the ban. This difference remains about 12 percent beyond the first year (figure 6). These post-treatment effects are statistically significant for the most part, even though prior to the ban these counties do not show any discernable difference in terms of labor demand.

As with the previous unemployment rate estimates, the unemployment rate does not respond much more strongly for counties with high subprime rates following the ban (though this response is significant) and is somewhat higher in these counties prior to the ban, as seen in column (6) of table 9. This implies that labor market tightness falls disproportionately more in counties with high shares of subprime residents, since vacancies fall more but the unemployment rate does not. While we cannot estimate the effect of subprime shares on the job-finding rate using our state-level data, these estimates suggest that finding rates fall disproportionately in counties with more subprime residents.

In summary, we do not find evidence that employer-credit-check bans benefit counties with large shares of subprime workers. If anything, credit check bans seem to adversely affect labor market these counties relative to the average.

e) <u>Credit Market Outcomes</u>

The effects of the credit check bans on credit market outcomes has not been studied prior to this paper. We follow a similar empirical strategy as with above and extend our analysis to the credit panel to estimate borrower-level changes post ban. We run the following panel regression:

$$Y_{i,j,t} = \alpha_i + \gamma_t + \theta_j + \beta_0 Ban_{j,t} + \beta_1 Subprime_{i,t} + \beta_2 Ban_{j,t}{}^{*} Subprime_{i,t} + \varepsilon_{i,t}, \quad (4)$$

In equation (4), we are looking for the effects of the ban on the credit market outcome variable $Y_{i,j,t}$ for an individual *i* in county *j* at time *t*. This specification controls for individual (α_i), county (γ_t), and time (θ_j) fixed-effects. We are interested in credit access and repayment decisions, so we estimate this model using new inquiries for credit over the previous three months (*inquiries*) and the fraction of accounts that are delinquent (*delinquencies*). The interaction term $Subprime_{i,t}$ indicates whether the borrower has a credit score below 620 at that time.

Table 10 reports these estimates using the FRBNY CCP/Equifax. These data are measured quarterly, which allows us to align the credit ban flag accurately since many bans became enforceable at the start of a quarter. The first column of table 10 presents the results for the outcome variable *inquiries*. Although a borrower's subprime status increases the number of inquiries, residing in a ban state significantly decreases the number of inquiries for subprime borrowers. Total inquiries are a proxy for credit access, so the ban could lead to borrowers' becoming more credit constrained. The dynamic response of inquiries shows smaller but significant declines up until a year post ban, as seen in column (2). Unfortunately, we cannot tell if inquiries fell because borrowers requested fewer lines of credit or because financial institutions made fewer offers to residents of treated counties.

The third column of table 10 reports the change in repayment behavior following a ban. Unconditionally, there is a small decline in delinquencies post ban (-0.6%), but the delinquency rate for subprime borrowers rises substantially. The subprime delinquency rate is 5.8% higher in areas with the credit check ban, a number that is large in economic terms, since 46% of accounts are delinquent for these borrowers. This decline arises only after the ban becomes enforceable, as seen in column (4), and remains significantly negative even after a year. As with inquiries, our data are silent about why subprime borrowers become delinquent on more accounts. This increase is consistent with either borrowers choosing to cease payments or being less able to repay their debts. While we cannot observe income or job status for these borrowers, Friedberg, Hynes, and Pattison (2017)'s finding that financially distressed workers have shorter unemployment durations post ban suggests that the rise in delinquencies for subprime borrowers may be strategic.

Previous studies (Clifford and Shoag, 2016; Bartik and Nelson, 2016) provided evidence that workers from minority groups were especially hurt by employer credit check bans. A natural question, then, is whether these groups were especially affected in the credit market. Previous research could not directly address this question because credit reports exclude demographic information. We overcome this limitation with a new dataset, the TransUnion-Epsilon merged panel, which has both credit report data (from TransUnion rather than Equifax) and demographics from Epsilon, a marketing firm. Tables 11 and 12 show the results for total inquiries and rate of total loans delinquent, respectively, this time broken down by race. Column 1 of each table shows the results for the full sample. As with the FRBNY CCP/Equifax data, the coefficient on the interaction variable is negative for the total inquiries, implying a net reduction in new credit.[20] The estimated effect of the ban for subprime workers is similar across Whites, Blacks, and Hispanics, at -0.486 inquiries, while the unconditional effect of the ban is slightly positive, at 0.085 inquiries (these point estimates are similar across groups).

The effect of employer credit bans for delinquency rates exhibit interesting differences across racial groups. The net effect of the ban is a 3.12% increase in delinquency rates for subprime individuals, an economically significant increase— on average, subprime individuals are delinquent on 46% of loans (see table 4). Somewhat surprisingly, the increase is driven entirely by Whites and Hispanics: For Blacks, the point estimate is both economically and statistically insignificant. Since previous research has estimated worse labor market effects for black workers, the fact that their delinquency rate is unaffected by the ban may indicate that they are made no better off by an employer credit check ban, while Whites and Hispanics are made worse off. As in the Equifax panel, the unconditional effect on delinquencies is an order of magnitude smaller for Blacks than other groups, and is statistically insignificant.

[20] The magnitude of the effect is larger in the TransUnion data because it is an annual panel rather than the quarterly Equifax.

We are the first to study the spillover of employer credit check bans into the credit market, and our estimates highlight the importance of a holistic view of the consequences from such bans. On net, our estimates indicate that employer credit check bans have negative effects on credit markets, especially for subprime workers. As with the labor market, employer credit check bans have negative unintended consequences in credit markets.

4) Conclusion

In 2007, the state of Washington was the first to pass a ban on credit checks for employers, and since then, ten states have done so. These bans are intended to break the cycle in which limited employment opportunities cause financial distress, which in turn may further reduce labor market opportunities. The deep downturn in economic activity and severe housing market crisis experienced during the Great Recession provided policymakers with a strong motivation to introduce such legislation. Using both labor and credit market data, we show that these laws have likely reduced job creation, while being associated with reduced credit access and increased delinquency rates.

We are the first paper to study the effects of employer-credit-check bans on variables motivated by a theoretical model (CG (2017)) and we find support for many implications of their theory. Job vacancy postings fall after the introduction of the credit check ban for nonexempt occupations. This is consistent with the mechanism in CG (2017), where removing a cheap screening device (credit reports) reduces matching efficiency. Their model also predicts a reduction in repayments and therefore credit supply, which is consistent with our credit market estimates. While our data cannot definitively prove that the mechanism from CG (2017) is generating our results, the fact that the changes we estimate are accompanied by widespread welfare losses in their model suggests that policymakers should be cautious when banning employer credit checks.

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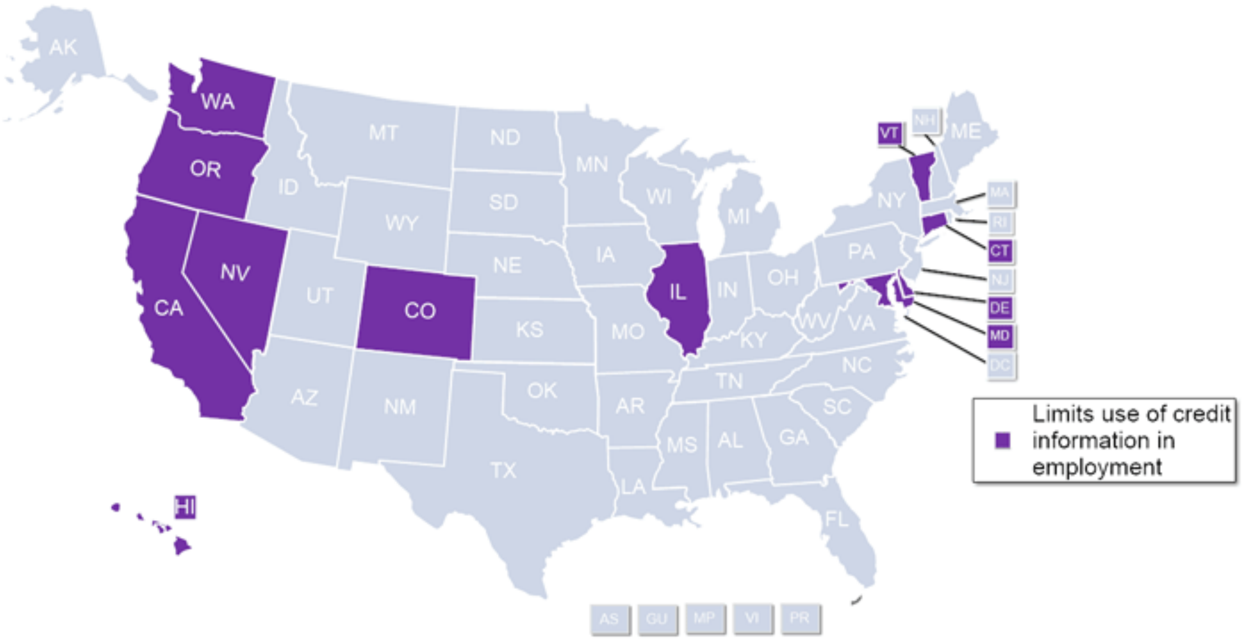
Figure 1: Credit Check Ban Legislation

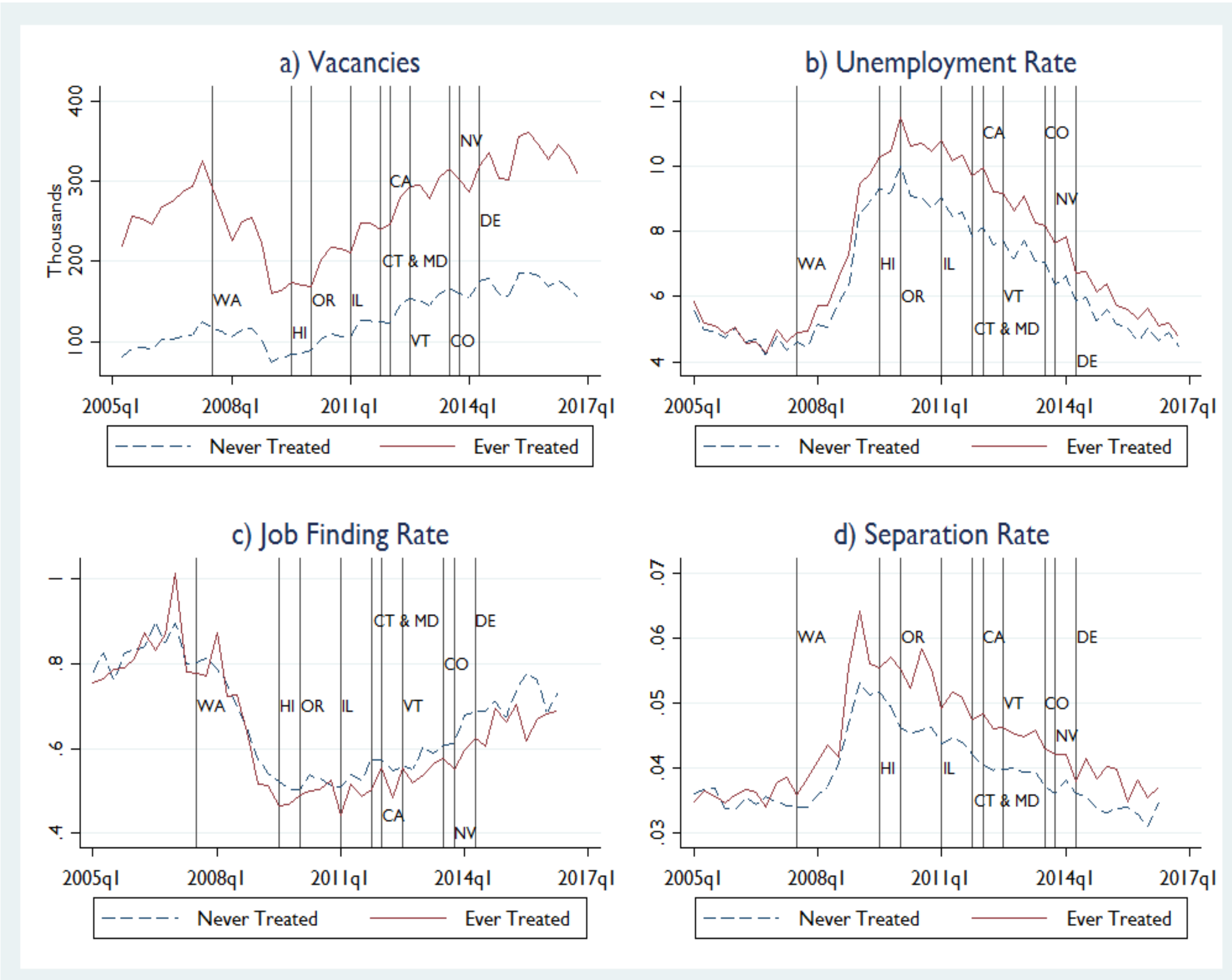


Source: National Conference of State Legislatures

Table 1: Dates When Bans Went into Effect

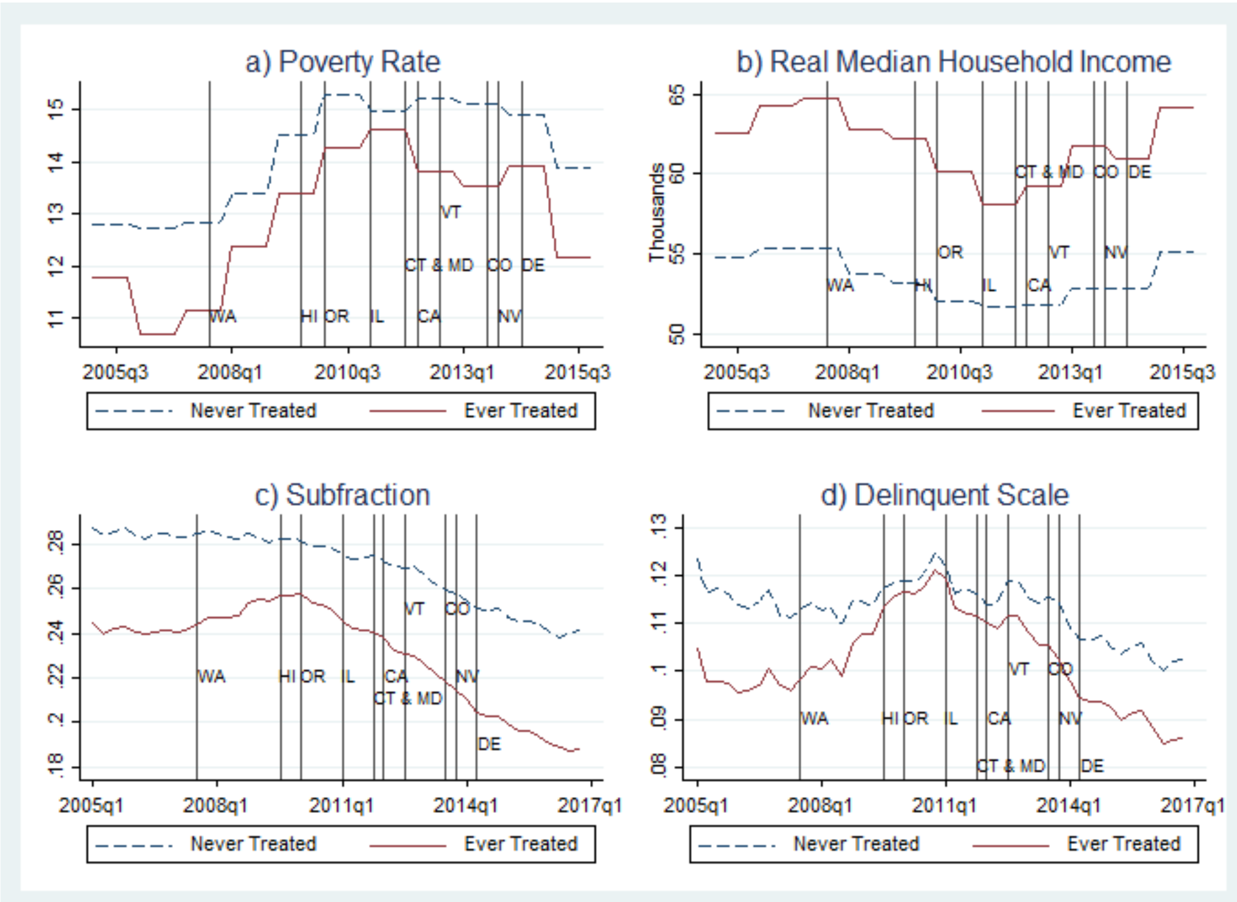
State	Date of Effective Law Change	Neighboring States
CA	1/1/2012	NV, AZ, OR
CO	7/1/2013	UT, WY, NE, KS, OK, NM
CT	10/1/2011	MA, NY, RI
DE	5/8/2014	MD, NJ, PA
HI	7/1/2009	none
IL	1/1/2011	IN, KY, MO, IA, WI
MD	10/1/2011	DE, PA, VA, WV
NV	10/1/2013	AZ, CA, ID, OR, UT
OR	3/29/2010	CA,ID, NV,WA
VT	7/1/2012	MA,NH, NY
WA	7/22/2007	ID, OR

Figure 2: Labor Market Outcomes across States and Credit Check Bans



Source: The data come from HWOL and CPS state-level aggregates, in which each state is weighted by its labor force. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect.

Figure 3: Poverty, Income and Credit Market Outcomes across States and the Credit Check Bans



Source: The data for Poverty Rate and Median Household Income come from Small Area Income and Poverty Estimates (SAIPE) conducted by the Census Bureau. The fraction of subprime borrowers (Subfraction) and the percentage of loans that are delinquent come from FRBNY CCP/Equifax. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect.

Table 2: Distribution of Labor Market Variables and Ban Flag – County Level

	Log (Vacancies)		Unemployment		Law Flag	
Year	Mean	Std. dev.	Mean	Std. dev.	Counties with law in effect	States with law in effect
2005	4.22	2.09	5.46	2.00	0	0
2006	4.31	2.11	4.97	1.88	0	0
2007	4.41	2.15	4.89	1.87	39	1
2008	4.49	2.11	5.83	2.25	39	1
2009	4.46	1.99	9.07	3.35	44	2
2010	4.62	2.01	9.38	3.31	80	3
2011	4.79	2.02	8.73	3.13	214	6
2012	5.05	1.97	7.86	2.89	286	8
2013	5.17	1.95	7.38	2.80	367	10
2014	5.25	1.96	6.25	2.47	370	11
2015	5.38	1.92	5.53	2.14	370	11
2016	5.34	1.90	5.25	1.99	370	11
All years	4.80	2.05	6.72	3.03	--	--

Table 3: Distribution of Subprime Borrowers – County Level

		Fraction Subprime					
Year	Obs.	Mean	25th pct	Median	75th pct	95th pct	Std. dev.
2005	12560	0.29	0.20	0.27	0.36	0.5	0.12
2006	12560	0.29	0.20	0.27	0.36	0.5	0.12
2007	12559	0.28	0.20	0.27	0.36	0.49	0.12
2008	12556	0.28	0.20	0.27	0.35	0.48	0.11
2009	12556	0.28	0.20	0.27	0.35	0.47	0.11
2010	12548	0.26	0.20	0.26	0.34	0.46	0.11
2011	12548	0.27	0.19	0.25	0.33	0.46	0.11
2012	12548	0.26	0.19	0.25	0.33	0.46	0.11
2013	12546	0.26	0.18	0.24	0.32	0.45	0.11
2014	12544	0.25	0.17	0.24	0.32	0.44	0.11
2015	12544	0.24	0.17	0.23	0.31	0.44	0.11
2016	12544	0.24	0.17	0.23	0.31	0.43	0.11
All years	150613	0.27	0.19	0.26	0.34	0.47	0.11

Table 4: Credit Panel Summary Statistics, Equifax and TransUnion

Panel A: All Borrowers

	Total Inquires			
	Obs.	Mean	Median	Std. dev
Equifax (All)	30,011,039	0.590	0	0.983
TU (All)	8,360,807	0.759	0	1.364
TU (White)	4,902,086	0.688	0	1.299
TU (Black)	297,091	1.003	0	1.576
TU (Hispanic)	666,081	0.924	0	1.504

	Total Loans Delinquent			
	Obs.	Mean	Median	Std. dev
Equifax (All)	37,203,721	0.126	0	0.292
TU (All)	6,996,168	0.098	0	0.282
TU (White)	4,289,566	0.075	0	0.247
TU (Black)	229,326	0.186	0	0.367
TU (Hispanic)	555,662	0.134	0	0.32

Panel B: Subprime Borrowers

	Total Inquires			
	Obs.	Mean	Median	Std. dev
Equifax (All)	9,442,835	0.823	0	1.200
TU (All)	1,958,371	1.303	1	1.757
TU (White)	853,181	1.330	1	1.790
TU (Black)	131,385	1.396	1	1.797
TU (Hispanic)	184,111	1.385	1	1.802

	Total Loans Delinquent			
	Obs.	Mean	Median	Std. dev
Equifax (All)	9,119,384	0.455	0.400	0.405
TU (All)	1,230,827	0.457	0.333	0.477
TU (White)	580,403	0.441	0.333	0.471
TU (Black)	83,024	0.447	0.333	0.470
TU (Hispanic)	125,204	0.471	0.333	0.477

Table 5: Changes in Vacancies Post Ban - National Sample

	Dependent Variable: Log (Vacancies +1)			
	(1)	(2)	(3)	(4)
	All occupations		Exempt	Nonexempt
	Total	New	Total	Total
Credit check ban	-0.124*	-0.0929	-0.017	-0.126*
	(0.0649)	(0.0554)	(0.0404)	(0.0648)
County fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Number of clusters (states / borders)	51	51	51	51
Observations	147,542	147,542	147,542	147,542
R-squared	0.965	0.966	0.946	0.964

Standard errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is the log of the Conference Board's HWOL data on vacancies, our proxy measure for labor demand. Column (2) focuses on new vacancies instead of total vacancies reported. Exempt occupations include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33).

Table 6: Changes in Vacancies Post Ban - Adjacent Border Counties

	Dependent Variable: Log (Vacancies +1)			
	(1)	(2)	(3)	(4)
	All occupations		Exempt	Nonexempt
	Total	New	Total	Total
Credit check ban	-0.0948**	-0.0993**	-0.0313	-0.0974**
	(0.0467)	(0.0433)	(0.0426)	(0.0473)
County fixed effects	Yes	Yes	Yes	Yes
County pair*time fixed effects	Yes	Yes	Yes	Yes
Number of clusters (states & borders)	256	256	256	256
Observations	70,218	70,218	70,218	70,218
R-squared	0.983	0.983	0.975	0.983

Standard errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is the log of the Conference Board's HWOL data on vacancies, our proxy measure for labor demand. Column (2) focuses on new vacancies instead of total vacancies reported. Exempt occupations include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33).

Table 7: Dynamics of Vacancies Around Ban

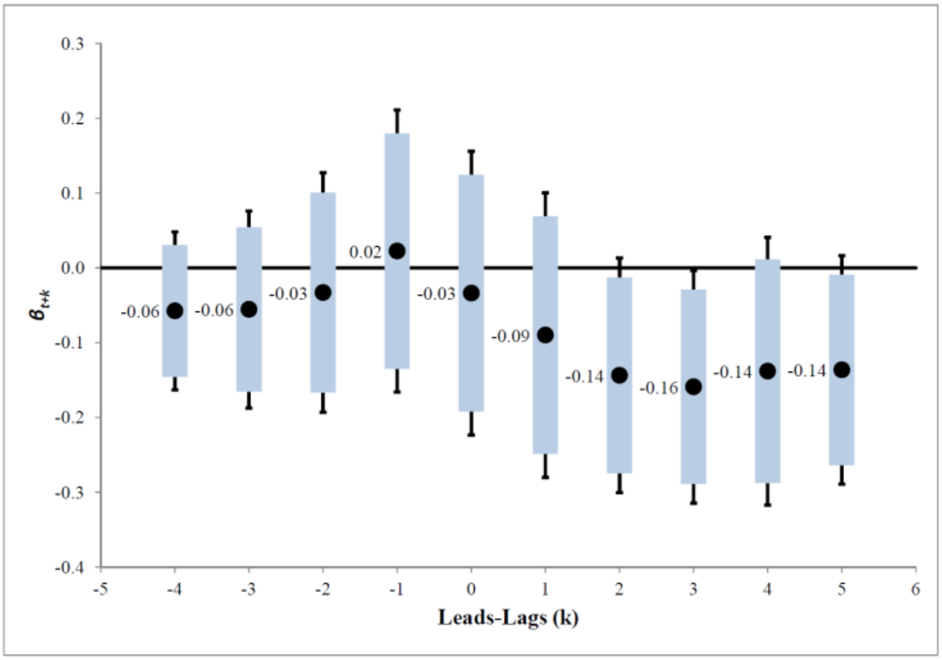
	Log (Vacancies +1)			
	(1)	(2)	(3)	(4)
	All occupations		Exempt	Nonexempt
	Total	New	Total	Total
Credit check ban, *t-4*	-0.0575	-0.0668	-0.0205	-0.0579
	(0.0526)	(0.0489)	(0.0480)	(0.0517)
Credit check ban, *t-3*	-0.0556	-0.0386	-0.0197	-0.0558
	(0.0656)	(0.0538)	(0.0514)	(0.0640)
Credit check ban, *t-2*	-0.0330	-0.00910	4.31e-05	-0.0332
	(0.0797)	(0.0649)	(0.0696)	(0.0779)
Credit check ban, *t-1*	0.0224	0.0179	0.0715	0.0174
	(0.0939)	(0.0715)	(0.0816)	(0.0927)
Credit check ban, *t*	-0.0338	-0.0386	-7.28e-05	-0.0361
	(0.0945)	(0.0812)	(0.0641)	(0.0939)
Credit check ban, t+*1*	-0.0899	-0.0936	0.0167	-0.0965
	(0.0948)	(0.0745)	(0.0560)	(0.0937)
Credit check ban, *t+2*	-0.144*	-0.126*	-0.00632	-0.148*
	(0.0781)	(0.0649)	(0.0393)	(0.0784)
Credit check ban, *t+3*	-0.159**	-0.110*	0.0209	-0.167**
	(0.0775)	(0.0636)	(0.0429)	(0.0776)
Credit check ban, *t+4*	-0.138	-0.0965	-0.0289	-0.140
	(0.0892)	(0.0763)	(0.0420)	(0.0902)
Credit check ban, *t>4*	-0.136*	-0.0991	-0.0208	-0.137*
	(0.0760)	(0.0654)	(0.0503)	(0.0756)
County ffxed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Number of clusters (states / borders)	51	51	51	51
Observations	147,542	147,542	147,542	147,542
R-squared	0.965	0.966	0.947	0.965

Standard Errors clustered at the state level

*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variable is the log of the Conference Board's HWOL data on vacancies, our proxy measure for labor demand. Column (2) focuses on new vacancies instead of total vacancies reported. Exempt occupations include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33).

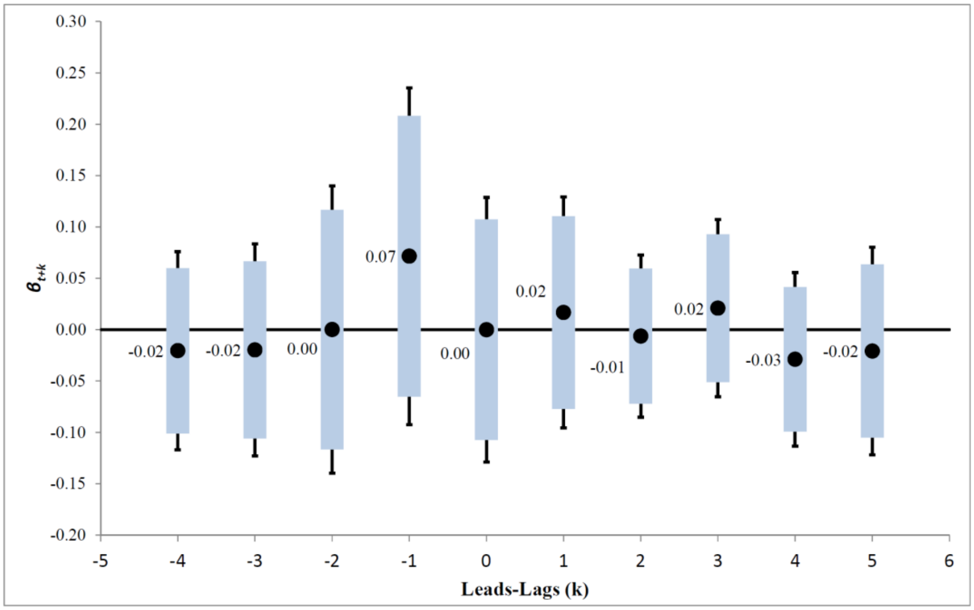
Figure 4: Dynamics of Vacancies Around Ban



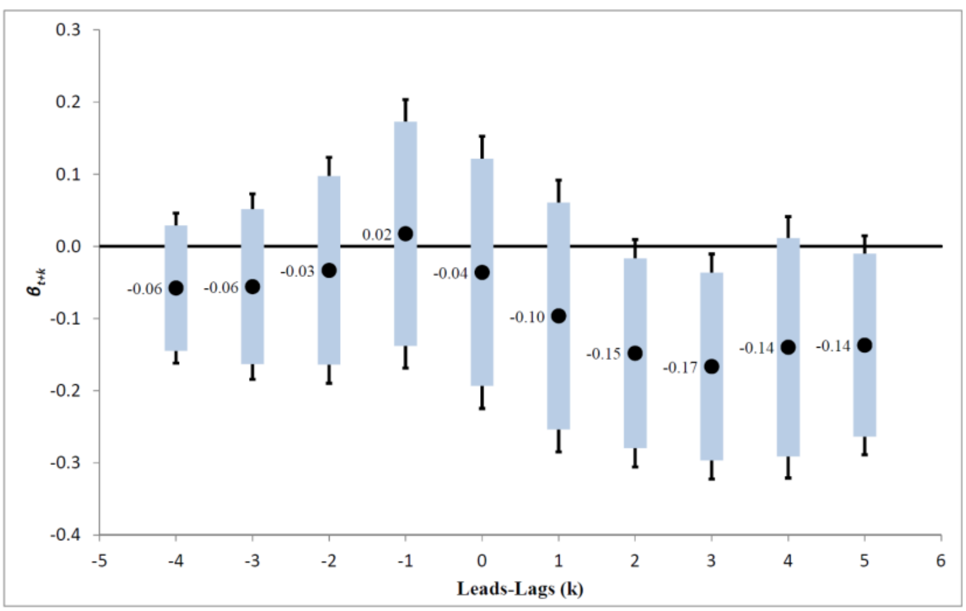
Note: Shaded areas and bars represent 90 percent and 95 percent confidence bands around the coefficient estimates.

Figure 5: Dynamics of Vacancies Around Ban by Occupation Type

a) **Exempt Occupations**



b) **Affected Occupations**



Note: Shaded areas and bars represent 90 and 95 percent confidence bands around the coefficient estimates, respectively. Exempt occupations include Business and Financial Operations (SOC-13), Legal (SOC-23) and Protective Service Occupations (SOC-33).

Table 8: Changes in Other Labor Market Outcomes Post Ban

	Nationwide Samples			Adjacent
	UR	Sep Rate	Find Rate	UR
Credit check ban	0.150	0.027	-0.027	0.220
	(0.186)	(0.021)	(0.018)	(0.223)
County / State Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	
County pair*time fixed effects				Yes
Clusters	State			State/Border
Observations	150,607	2,346	2,346	71,704
R-squared	0.826	0.492	0.662	0.931

*** p<0.01, ** p<0.05, * p<0.1

Note: Column (1) reports the regression results based on specification (1a) in the text for dependent variable, unemployment rate. Column (4) reports regression results for unemployment rate focusing on only adjacent-border counties, using specification (1b). Columns (2) and (3) report the regression coefficients from a specification of (1a) with state-level (log) separation and finding rates as the dependent variable.

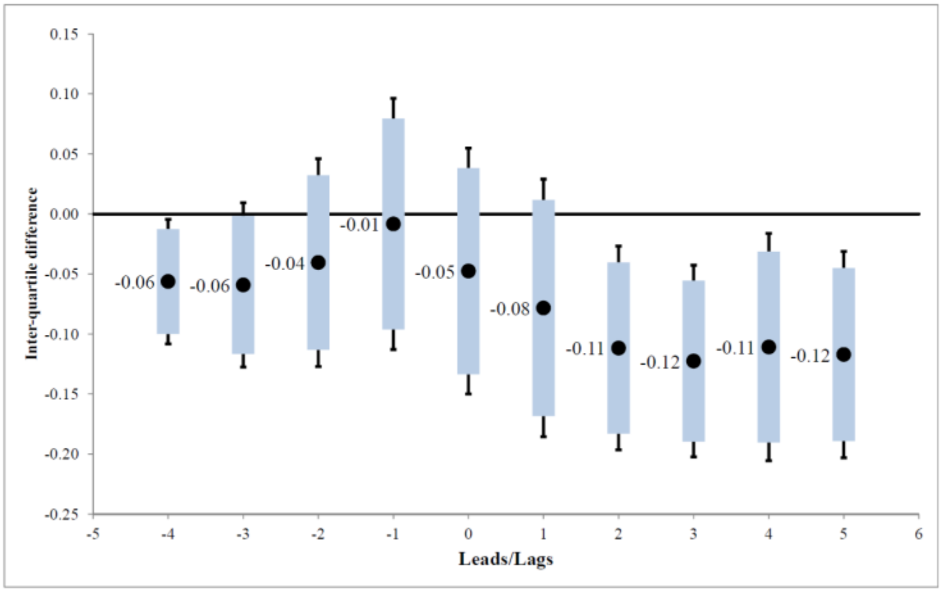
Table 9: Vacancies and Unemployment Rate by Share Subprime

Dependent Variable	Log (Vacancies)				Unemployment Rate	
	(1)	(2)	(3)	(4)	(5)	(6)
	All occupations		Exempt	Affected		
CC ban*fraction Subprime	-0.00251				0.022***	
	(0.00373)				(0.008)	
Fraction subprime	-0.000485	-0.00107	0.000448	-0.00119	0.005**	0.009**
	(0.000717)	(0.000842)	(0.000690)	(0.000851)	(0.002)	(0.004)
Interaction t-4		-0.00269	-0.000901	-0.00270		0.048***
		(0.00174)	(0.00177)	(0.00171)		(0.016)
Interaction t-3		-0.00288	-0.00146	-0.00286		0.032**
		(0.00235)	(0.00192)	(0.00230)		(0.015)
Interaction t-2		-0.00163	-0.000259	-0.00165		0.032**
		(0.00291)	(0.00273)	-0.003		(0.012)
Interaction t-1		0.000512	0.00261	0.000299		0.030*
		(0.00357)	(0.00336)	(0.00352)		(0.017)
Interaction t		-0.00211	-0.000596	-0.00218		0.025
		(0.00355)	(0.00280)	(0.00351)		(0.021)
Interaction t+1		-0.00416	0.000518	-0.004		0.023
		(0.00368)	(0.00223)	-0.004		(0.016)
Interaction t+2		-0.00638**	-0.000415	-0.00655**		0.032***
		(0.00300)	(0.00149)	(0.00301)		(0.008)
Interaction t+3		-0.00710**	0.000744	-0.00743**		0.028***
		(0.00285)	(0.00173)	(0.00283)		(0.010)
Interaction t+4		-0.00632*	-0.00155	-0.00639*		0.012
		(0.00336)	(0.00188)	(0.00339)		(0.013)
Interaction t>4		-0.00674**	-0.00113	-0.00677**		0.015
		(0.00304)	(0.00218)	(0.00302)		(0.009)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State*time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	50	51	51	51	50	51
Observations	147,426	147,473	147,473	147,473	150,537	150,585
R-squared	0.971	0.965	0.947	0.965	0.906	0.826

*** p<0.01, ** p<0.05, * p<0.1

Note: Columns (1) displays regression results following the specification in equation (3). The remaining columns incorporate the variation in the subprime fraction of the borrowers in a county to implement the distributed lag specification expressed in equation (2). Interaction $t+k$, refers to the interaction of the fraction subprime variable with the respective dummy for the credit check ban in the distrtibuted lag specification for $t+k$.

Figure 6: Dynamics of Vacancies Around Ban Interacted with Subprime Rate



Note: Point estimates indicate the predicted difference in the dependent variable between an average county in the 75th percentile and one in the 25th percentile (in terms of the variable *fraction subprime*). Shaded areas and bars represent 90 percent and 95 percent confidence bands around the coefficient estimates.

Table 10: Dynamics of Equifax Credit Panel Inquiries and Delinquencies Around Ban

Dependent Variable	Total inquiries		Rate of total loans delinquent	
	(1)	(2)	(3)	(4)
Credit check ban*Subprime	-0.116***		0.0581***	
	(0.011)		(0.007)	
Credit check ban	0.028**		-0.00637**	
	(0.013)		(0.003)	
Subprime	0.066***		0.260***	
	(0.008)		(0.002)	
Interaction Subprime * Ban t-4		-0.009		0.003
		(0.007)		(0.006)
Interaction Subprime * Ban t-3		-0.007		-0.001
		(0.007)		(0.004)
Interaction Subprime * Ban t-2		-0.027***		-0.003**
		(0.008)		(0.001)
Interaction Subprime * Ban t		0.007		0.004**
		(0.014)		(0.002)
Interaction Subprime * Ban t+1		-0.018		0.002
		(0.021)		(0.003)
Interaction Subprime * Ban t+2		-0.049*		0.007**
		(0.027)		(0.004)
Interaction Subprime * Ban t+3		-0.042*		0.010**
		(0.025)		(0.004)
Interaction Subprime * Ban t+4		-0.043*		0.010***
		(0.024)		(0.003)
Interaction Subprime * Ban > t+4		-0.035		0.016*
		(0.048)		(0.008)
County fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Borrower fixed effects	Yes	Yes	Yes	Yes
Number of clusters	51	51	51	51
Observations	29,974,569	28,804,982	37,147,443	37,146,820
R-squared	0.158	0.194	0.660	0.671

Standard Errors Clustered at the State Level

*** p<0.01, ** p<0.05, * p<0.1

Note: This table reports the regression of the effect of the credit check ban on inquiries and delinquencies using the Quarterly New York Fed Equifax Consumer Credit Panel. Columns (1) and (3) estimate the regression in equation (4). Columns (2) and (4) report the coefficients on the

Table 11: TransUnion Credit Panel Inquiries by Race

	Total Inquries			
Dependent Variable	All	White	Black	Hispanic
Credit check ban*Subprime	-0.486***	-0.479***	-0.481***	-0.459***
	(0.042)	(0.047)	(0.050)	(0.022)
Credit check ban	0.085***	0.065***	0.065	0.132***
	(0.020)	(0.019)	(0.040)	(0.035)
Subprime	0.263***	0.259***	0.232***	0.280***
	(0.017)	(0.014)	(0.021)	(0.020)
Borrower fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Observations	8,344,097	4,895,036	295,125	663,454
R-squared	0.337	0.338	0.383	0.344

Std. Errors clustered at the state level

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports the regression of the effect of the credit check ban on total inquries using the annual TransUnion Consumer Credit Panel, broken down by race.

Table 12: TransUnion Credit Total Loans Delinquent by Race

	Rate of Total Loans Delinquent			
Dependent Variable	All	White	Black	Hispanic
Credit check ban*Subprime	0.031***	0.032***	0.003	0.043***
	(0.004)	(0.004)	(0.010)	(0.009)
Credit check ban	-0.002*	-0.003**	0.004	-0.008***
	(0.001)	(0.001)	(0.004)	(0.003)
Subprime	0.350***	0.348***	0.329***	0.352***
	(0.005)	(0.005)	(0.004)	(0.012)
Borrower fixed effects	Yes	Yes	Yes	Yes
Zip code fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Observations	6,955,336	4,272,570	225,946	551,051
R-squared	0.549	0.537	0.533	0.544

Std. errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1
Note: This table reports the regression of the effect of the credit check ban on total loans delinquent using the annual TransUnion Consumer Credit Panel, broken down by Race.

APPENDIX: CODING THE BAN FLAG

The estimated effect of banning employer credit checks relies on the coding of the date at which the law began to affect economic decisions. Our convention is to code the flag as "1" if the ban was in effect at the beginning of that quarter. This allows an exact match for all but three states, since many went into effect at the beginning of a quarter.

Coding the Ban: Quarterly Versus Annually

State	Exact Date of Ban	First Quarter Ban in Effect	Year Coding
CA	1/1/2012	2012:Q1*	2012*
CO	7/1/2013	2013:Q3*	**2013**
CT	10/1/2011	2011:Q4*	2012
DE	5/8/2014	2014:Q2	n/a
HI	7/1/2009	2009:Q3*	**2009**
IL	1/1/2011	2011:Q1*	2011*
MD	10/1/2011	2011:Q3*	**2011**
NV	10/1/2013	2013:Q3*	**2013**
OR	3/29/2010	2010:Q1	2010
VT	7/1/2012	2012:Q3*	**2012**
WA	7/22/2007	2007:Q3	**2007**

* Denotes exact match, bold denotes at least six-month discrepancy with enforcement date

As can be seen, coding quarterly allows for a precise match for all but three of the states, while an annual coding could match at most two states precisely (California's ban went into effect on January 1, 2012 and Illinois's went into effect January 1, 2011).