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**Working Paper**

## The unintended consequences of employer credit check bans on labor and credit markets

Working Paper, No. 1702

**Provided in Cooperation with:**

Koç University - TÜSİAD Economic Research Forum, Istanbul

*Suggested Citation:* Cortés, Kristle; Glover, Andrew; Tasci, Murat (2017) : The unintended consequences of employer credit check bans on labor and credit markets, Working Paper, No. 1702, Koç University-TÜSİAD Economic Research Forum (ERF), Istanbul

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KOÇ UNIVERSITY-TÜSİAD ECONOMIC RESEARCH FORUM  
WORKING PAPER SERIES

**THE UNINTENDED CONSEQUENCES OF  
EMPLOYER CREDIT CHECK BANS ON LABOR AND  
CREDIT MARKETS**

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Andrew Glover  
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Working Paper 1702  
January 2017

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# The Unintended Consequences of Employer Credit Check Bans on Labor and Credit Markets<sup>1</sup>

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December 12, 2016

## Abstract

Lenders have traditionally used credit reports to measure a borrower's default risk, but credit agencies also market reports to employers for use in hiring. Since the onset of the Great Recession, eleven state legislatures have restricted the use of credit reports in the labor market. We document that county-level unemployment rose faster in states that restricted employer credit checks and counties with more sub-prime citizens experienced larger increases in the unemployment rate than average. Using data from individual credit reports, we find that access to credit declines and delinquencies increase significantly after the state-level policy changes, especially for subprime borrowers.

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## 1) Introduction

Over the last twenty years, credit-reporting agencies have found a new market for credit reports: employers deciding whether to extend an offer to a job 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% used credit reports in the hiring decision for at least some jobs (Society for Human Resources, 2012). While nationally representative data on employer credit checks is nonexistent, 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 due to 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 level.<sup>2</sup> Eleven states (and some cities) have banned employer credit checks as of March 2016, the geographic distribution of which can be seen in Figure 1. Lawmakers voice concern that employer credit checks may create a poverty trap: a cycle in which a person loses her job, falls behind on debt payments, and then cannot find a job because of the deterioration in her credit. New York City passed a credit-ban law in 2015 and Brad Lander, the bill's sponsor, 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. We find that the laws are counterproductive: when a state bans employer credit checks, the average county experiences an increase in the unemployment rate relative to trend. Furthermore, we find that an equity-efficiency tradeoff cannot support the ban: we estimate a larger rise in unemployment for the counties with a larger share of households with sub-prime credit. We also document the negative effects of these credit check bans beyond labor markets. In particular, residents of these states experience an increase in

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<sup>2</sup> While these laws typically restrict the use of credit checks without necessarily banning them outright, for expositional simplicity, we will refer to them as "bans" in this paper.

delinquencies and potential credit constraints after the ban. This result is even more pronounced for individuals with sub-prime credit. Taken together, our results raise doubts that credit bans have had their intended effects of improving labor and credit markets, especially for the targeted population.

These results are consistent with the theoretical implications of Chen, Corbae and Glover (2014), henceforth CCG. They build a general equilibrium model in which households make borrowing and default decisions, which in turn generate credit scores. These credit scores have information content for the labor market because it provides a signal about a private component of worker productivity, modeled as exogenous differences in worker productivity that affects marginal revenue product of the firms. Since higher productivity workers will generate more output and receive higher wages, in equilibrium, they default less. This mechanism generates an endogenous link between the credit market behavior of agents and the labor market outcomes through credit score/history.<sup>3</sup> In a reasonably calibrated version of this environment, CCG shows that preventing access to individual credit scores for pre-employment screening purposes will reduce job-finding rate and potentially increase unemployment rate. Since this lack of access will remove some incentive for workers to repay their debts, it also leads to negative effects in the credit markets. In this paper, we largely confirm these implications with a rich set of individual and county-level data.

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 Local Area Unemployment Statistics (LAUS) and aggregated data from the Equifax credit panel. We estimate the average effect of an employer credit check ban using the change in quarterly unemployment rate in a treated county (relative to trend) versus the change for an untreated county.<sup>4</sup> We then estimate the relative effect for a bad-credit county as their change in unemployment relative to the average county in a treated state versus the same difference within an untreated state. We follow a similar approach for credit market

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<sup>3</sup> Note that in reality it is not clear why employers think credit scores signal something about the worker. However, CCG (2014) provides a simple endogenous mechanism that is consistent with this behavior. In general, as long as employers think that credit score has some signaling value, regardless of its actual correlation with worker productivity on the job, one can generate such a link between both markets.

<sup>4</sup> 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 state wide and using only contiguous counties.

outcomes, but rely on individual borrower level observations from Equifax. This data set provides us with a rich set of credit variables such as credit inquiries, credit scores, usage, delinquencies etc.

We are among the first to study the effect of these laws on labor market outcomes. The two most closely related papers are Clifford and Shoag (2016) and Bartik and Nelson (2016). Clifford and Shoag (2016) estimate the effect of bans on log-employment (rather than the unemployment rate) at the census-tract level (rather than county) using annual data (rather than quarterly).<sup>5</sup> Substantively, we differ in two ways. First, our use of quarterly data allows for a very accurate coding for the effective date of bans. Second, we use the unemployment rate rather than the log of total employment. The unemployment rate is a more economically meaningful outcome since it captures an individual's probability of being employed. Log-employment may rise mechanically if the law motivates labor force entry or relocation to the post-ban state, which is nonetheless a negative outcome for the average worker if vacancies do not rise in proportion. Nevertheless, they report that, on net, these bans had relatively worse outcomes for the targeted populations.<sup>6</sup>

Bartik and Nelson (2016) use panel dimension of the Current Population Survey (CPS) and the data aggregated from state unemployment insurance records to look at the effects of the 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 unemployment rate, we complement the conclusion of Bartik and Nelson (2016).

Neither of the aforementioned studies considers credit market responses to bans. We are the first to estimate these effects and thereby highlight a novel channel through which the potential costs of this policy extend beyond the labor market.

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<sup>5</sup> The timing of the law flag is important and can change the estimated sign of the ban's effect. Not only does quarterly data allow for a more accurate coding, but using the effective date of the ban is important for coding California correctly. We provide a complete comparison with Clifford and Shoag in *Appendix: Coding the Ban*.

<sup>6</sup> These groups include individuals with mid-to-low credit scores, young workers and Blacks.

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) and Herkenhoff, Phillips and Cohen-Cole (2016) and Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016). The most relevant 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 re-employment, 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, which we reconcile by noting that a seven to ten year-old bankruptcy flag may provide employers with little significant information about a potential hire.

Life-cycle components of labor productivity and other observable labor market experiences during the first 7-years after bankruptcy likely swamps any information in the bankruptcy flag.<sup>7</sup>

## 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 March 2016. Throughout our empirical analysis, we focus on the period 2005:Q1 through 2014:Q4, which we think is sufficiently wide enough to capture the effects of the bans. 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. The resulting summary statistics for this flag are seen in Table 2. More than ten percent of counties are affected by the credit check bans at the end of our sample period.

### a) Labor Market Data

Our principle labor-market outcome is the county-level unemployment rate reported by the Bureau of Labor Statistics. County level data comes from the Local Area Unemployment Statistics (LAUS) program.<sup>8</sup> Table 3 shows the summary statistics for this variable on an annual basis. There is a clear rise nation-wide throughout the Great Recession, as well as an increase in the standard deviation across counties. The best source of household level labor market data is Current Population Survey (CPS). Unfortunately, sample size becomes relatively very 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 rate. Nevertheless, as in Bartik and Nelson (2016), we also use 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.a, which plots the average unemployment rate by treated and untreated

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<sup>7</sup> Moreover, public sector employers are not allowed to use bankruptcy filings in hiring decisions. If employers facing this constraint deliberately hire a worker with a flag to ensure compliance, then estimates will be biased towards zero.

<sup>8</sup> 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.



states over time.<sup>9</sup> Up until the Great Recession, the unemployment rates 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 two percentage points in 2010 and has only recently begun to shrink, though is still over half a percentage point. We certainly cannot conclude that the laws have caused this decline from this plot alone, but it is illustrative 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.b compares the job-finding rate between these two groups and Figure 2.c 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 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.

Another interesting comparison between these two groups of states is based on the market tightness measure. Figure 2.d plots market tightness, the ratio of vacancies to unemployment, across treated and untreated states. Treated states had a significantly higher rate of market tightness prior to the recession. Both groups experienced a sharp drop in labor market tightness over the Great Recession, but untreated states recovered somewhat more strongly.

In terms of labor market aggregates, we also analyze the effects of the credit check bans on wages in our paper. We rely on the Quarterly Workforce Indicators (QWI) database for this exercise. QWI source data is from Longitudinal Employer Household Dynamics (LEHD) linked employer-employee microdata. We can simultaneously measure average quarterly wages for all employees and new hires separately at the county level in QWI. This distinction is important: if the ban affects wages through screening then its effect will be more pronounced for new hires.

## b) Credit Market Data

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<sup>9</sup> Note that the set “Ever Treated” includes all of the 11-states that had, at some point during the sample period, effective credit check ban. Similarly the “Never Treated” group consists of the remainder of the states. Hence, each line has the same set of states over time, even though states enacted their laws on different dates. The averages plotted in the figures use each state’s labor force to weight their unemployment rates.

The Federal Reserve Bank of New York Consumer Credit Panel (NYCCP) provides detailed quarterly data from Equifax on a panel of US consumers and includes credit scores as well as 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 sub-prime rates across counties and over time is found in Table 4. For the purpose of our paper, we follow the literature and assume that the critical level for being sub-prime is a risk score of 620. Over the sample period we analyze, average fraction of subprime borrowers within a county was 27 percent, declining from 29 percent to 25 percent over time. There is a lot of variation across counties in our sample, with a county in the 95<sup>th</sup> percentile having more than 45 percent of borrowers sub-prime credit scores over the sample period. This variation helps us identify the differential effects of the credit check bans for this particular group of households, who 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 have demographics beyond age. As Clifford and Shoag (2016) and Bartik and Nelson (2016) find, the bans had more pronounced effects on particular demographic groups. We use a new dataset, 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 5 shows that the two credit market panels are similar for the two credit market outcomes of interest, inquiries and delinquencies. Equifax is measured quarterly and the mean number of credit inquiries over the previous 3 months is 0.6 whereas TransUnion is measured annually and has a mean of 0.8 from credit inquiries over the last 6 months. Breaking the data down by race shows that Blacks request credit slightly more than Hispanics and Whites.

Panel A of Table 5 shows that on average, 12% of loans are delinquent for the Equifax sample, whereas 10% are in TransUnion. Black borrowers are slightly more likely to have delinquent accounts: roughly 18% compared to Whites at 8% and Hispanics at 13%.

The average credit score for Blacks in our sample is 567 so the higher credit inquiries and 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 sub-sample in our data.<sup>10</sup>

Panel B of Table 5 presents the same descriptive statistics for the group of subprime borrowers in both data sets. As expected, this group consist 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: workers lose their jobs and this drop in income causes them to fall behind on payments, then makes it harder to find a job because of the deterioration in their credit history. As CCG (2014) show, this is a distinct theoretical possibility in a general equilibrium model. If the passage of these bans are correlated with the outcome variables we focus, or other endogenous variables at the state-level, that will affect the interpretation of our results. We formally try to control for this in our panel regressions below. However, we think it is instructive to check whether we see any distinct patterns between the treated and untreated states over time on this dimension.

Unemployment rate, one of our key outcome variables, shows no difference between treatment groups until the end of the Great Recession (Figure 2.a). 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, not consistent with the narrative above (Figure 3.a). The states that enacted these bans 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 locations one expects to enact these bans just based on this dimension.

The same picture emerges if we look at median household income or the fraction of subprime borrowers (Figures 3.b and 3.c). Median household income in all states stagnated from

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<sup>10</sup> Table 2 in the Appendix shows the credit score breakdown by race in more detail.

2007 through 2013. Nevertheless, treated states had substantially higher median household income throughout the sample period, by almost \$10,000. Finally, Figure 3.d shows that the treated states had on average lower delinquency rates than the untreated states. In the absence of this evidence, one might claim that the credit check bans are just a response to adverse credit and labor market conditions in certain states. To the contrary, it looks like treated states had generally more favorable conditions, which should make them less likely to enact the bans a priori.

### 3) Results

We use the individual-level credit panel and the county-level panel with labor market data to test the effects of the credit checks bans. We are able to test the effects on the labor market outcomes using the county level data and explore the credit panel for the effects on the credit market outcomes individual borrowers face.

#### a) Labor Market Outcomes

In order to address this qualitative finding more formally, we run a regression of the following form at the county-level:

$$Unemployment\ Rate_{i,t} = \alpha_i + \gamma_t + \beta Ban_{i,t} + \varepsilon_{i,t}, \quad (1a)$$

Where  $\alpha_i$  is a county fixed effect and  $\gamma_t$  is a time fixed effect. The coefficient of interest in this regression,  $\beta$ , is identified from the average change in the unemployment rate for a county in a treated state before and after the law was passed relative to the national average. The estimated coefficient can be found in Column 1 of Table 6; it is statistically significant and economically large. The typical county unemployment rate was nearly half a percentage point higher in the time after its state banned employer-credit checks.

We also estimate the effect of employer credit check bans using only adjacent counties across state lines. For this specification we estimate a form of Equation (1) using all border-adjacent counties in the contiguous United States. Counties are grouped along with all neighboring counties outside of their state 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 it passes the ban (joined with all non-state neighbors) and our control is all other county pairs.

Following the specification of Dube, et al (2010), we link the county-pair through a time-varying fixed effect and estimate the equation:

$$Unemployment\ Rate_{i,p,t} = \alpha_i + \gamma_{p,t} + \beta Ban_{i,t} + \varepsilon_{i,t}, \quad (1b)$$

The estimated  $\beta$  coefficient is found in Column 3 of Table 6 with standard errors clustered at both the state and border levels. The point estimate from this specification is very similar to the economy wide estimate. This is reassuring since the pair-time fixed effect means that the ban's effect is identified from the treated county's change in unemployment relative to its non-treated neighbor in each period after the ban is enacted. Our estimate implies that a county in a post-ban state experiences a 0.4 %-point increase in the unemployment rate, relative to an adjacent county in a non-ban state.

The contiguous county specification's strength lies in allowing for extremely general, time varying unobservable factors to be shared between the adjacent counties. On the other hand, the effect may not extrapolate to interior counties. For example, the effect may be overstated if much of the increase in unemployment for treated counties 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.

The above regression provides evidence of a negative average effect of banning employer credit checks, but one may worry that they are biased if states with relatively bad economic conditions are more likely to pass a ban. As can be seen in Figure 3, this is not the case; if anything, the states that have banned employer-credit checks are in better shape than those that have not.

Even if these laws have a negative effect on average, lawmakers and their constituents may be in favor of a ban if it helps households with bad credit. We estimate the following regression model to test if such an equity-efficiency trade-off exists:

$$Unemployment\ Rate_{i,t} = \alpha_i + \gamma_{j,t} + \beta Ban_{i,t} * Fraction\ Subprime_{i,t} + \varepsilon_{i,t}, \quad (2)$$

Here again  $i$  indexes the county and  $\alpha_i$  are county-level fixed effects. Because we now have within-state variation in the relevant treatment 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 model the credit check ban is interacted with a county's fraction of borrowers with a credit score less than 620 in county  $i$  in time  $t$ , denoted here by Fraction Subprime.

The estimated effect is reported in Column 2 of Table 6. For every ten percentage point rise in the fraction of sub-prime households, the unemployment rises by an additional 0.16 percentage points following the ban. The range of county subprime shares in Table 4 gives context for this estimate – the inter-quartile difference is 15 percentage points. Therefore, as a state bans employer credit checks, a county in the 75th percentile (which has 34% subprime) would have unemployment increase by 2.4 percentage points more than a county in the 25th percentile (which has 19% subprime).

Employers may become more selective in the hiring process due to the regulation, which has implications for another labor market outcome: wages. To test the effects on wages, we repeat equations 1a, 1b and 2 but with *total wages* and *wages for new hires* as the dependent variables.<sup>11</sup> Table 7 details the results and shows that there is no statistically significant effect on either average wages of all workers or new hires. These results are in line with Dobbie, Goldsmith-Pinkham, Mahoney and Song (2016). They are also consistent with the theory presented in CCG (2014). As implicitly anticipated by policy makers, a policy change of banning the use of credit scores effectively implies a redistribution from high credit score to low credit score workers. Conditional on finding a job, wages will come down somewhat as more low credit score workers find jobs. In CCG (2014), low credit score workers are low productivity workers as well. Given our finding for higher unemployment rate across ban states, the effects of this direct channel could be present in the data as predicted by the theory. On the other hand, CCG (2014) show that general equilibrium effects through credit markets by distorting the incentives might offset this direct channel. In the next section, we show that in our data we find evidence of deteriorating credit market outcomes in response to the policy change. Therefore, we think the absence of any significant effects on wages is consistent with the theoretical framework we have in mind.

#### b) Credit Market Outcomes

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<sup>11</sup> The variable is defined as the log of average wages per employee, measured quarterly.

The effects of the credit check bans on credit market outcomes has not been studied prior to this paper. We follow same empirical strategy as with above and extend our analysis to the credit panel to address this channel. We run the following panel regression:

$$Y_{i,j,t} = \alpha_i + \gamma_t + \theta_j + \beta_0 Ban_{j,t} + \beta_1 Subprime_{i,t} + \beta_2 Ban_{j,t} * Subprime_{i,t} + \varepsilon_{i,t}, \quad (3)$$

In equation (3), 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 ( $\alpha_i$ ), county ( $\gamma_t$ ) and time ( $\theta_j$ ) fixed-effects. The outcome variables we are interested in capture the demand for credit use and the ability/willingness to pay debts. More specifically, we look at number of inquiries in the past three months (*inquiries*) and the fraction of accounts that are delinquent (*delinquencies*). The interaction term,  $Subprime_{i,t}$ , indicates whether the borrower has subprime credit.

Table 8 details the results using the New York Consumer Credit Panel. These data are measured quarterly and that level of detail is useful since various credit check bans go into effect in different quarters of the year. The first column of Table 8 presents the results for the outcome variable *inquiries*. Even though being a subprime borrower alone increases the number of inquiries, being in a state that bans credit checks 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. Unfortunately we cannot know if inquiries fell because borrowers requested fewer lines of credit or because financial institutions made fewer offers to borrowers in treated counties.

The second column of Table 8 provides further evidence about the deterioration of the credit outcomes. The rate of total loans delinquent increases for subprime borrowers after the ban is in place. The rate is 6% higher in areas with the credit check ban for subprime borrowers, although the rate decreases for all borrowers; it is statistically significant but economically small.

It is important to note that our results cannot conclusively distinguish between two potentially plausible explanations. We have already provided evidence that credit check bans increase unemployment rate at the county level. Hence, credit market outcomes might have deteriorated either because of these deteriorating labor market conditions (with the exception of

the business cycle) or due to behavioral changes. Our results are consistent with the incentive channel presented in CCG (2014).

Previous studies highlight that these laws hurt minorities most even if the intended purpose was to help exactly those populations in the labor force. To test the effect on different races, we use the TransUnion data that together with the marketing data from Epsilon, provides demographic data in addition to the credit outcomes. Table 9 and 10 show the results for total inquiries and rate of total loans delinquent, this time broken down by race. Column 1 of each table shows the results for the full sample. As with the Equifax data, the coefficient on the interaction variable is negative for the total inquiries, but the magnitude is five times larger in the TransUnion data. These data are measured annually and the inquiry variable is measured over the previous 6 months, compared to Equifax where it is measured quarterly and counts the total inquiries over the previous 3 months. The effect is similar across Whites, Blacks and Hispanics. The one difference is that the direct effect of the credit check ban is not significant for Blacks as it is for Whites and Hispanics, although still positive.

The results for the rate of total loans delinquent are surprising. While the coefficient for the full sample is positive and significant for the subprime borrowers in the affected States, Whites and Hispanics drive the result. Given that the sample shows the lowest average credit scores for Blacks, the credit check ban seems to have no effect on the delinquency rates. The rate of delinquent loans is positive for all subprime borrowers in all subsets of the sample but the credit check ban only significantly affects Whites and Hispanics. According to the summary statistics in Table 5, the percentage increase of 3% is roughly one third of the average rate of loans delinquent in the sample. The results show that subprime White and Hispanic borrowers are worse off after the credit check ban. The direct effect of the credit check ban is negative, but these findings suggest that these laws either do nothing to protect, or in fact even hurt the very people that they are trying to help.

#### **4) Conclusion**

In 2007, the state of Washington was the first to pass a ban on credit report checks for employers and since then ten states have followed suit. The intention of these bans is to help



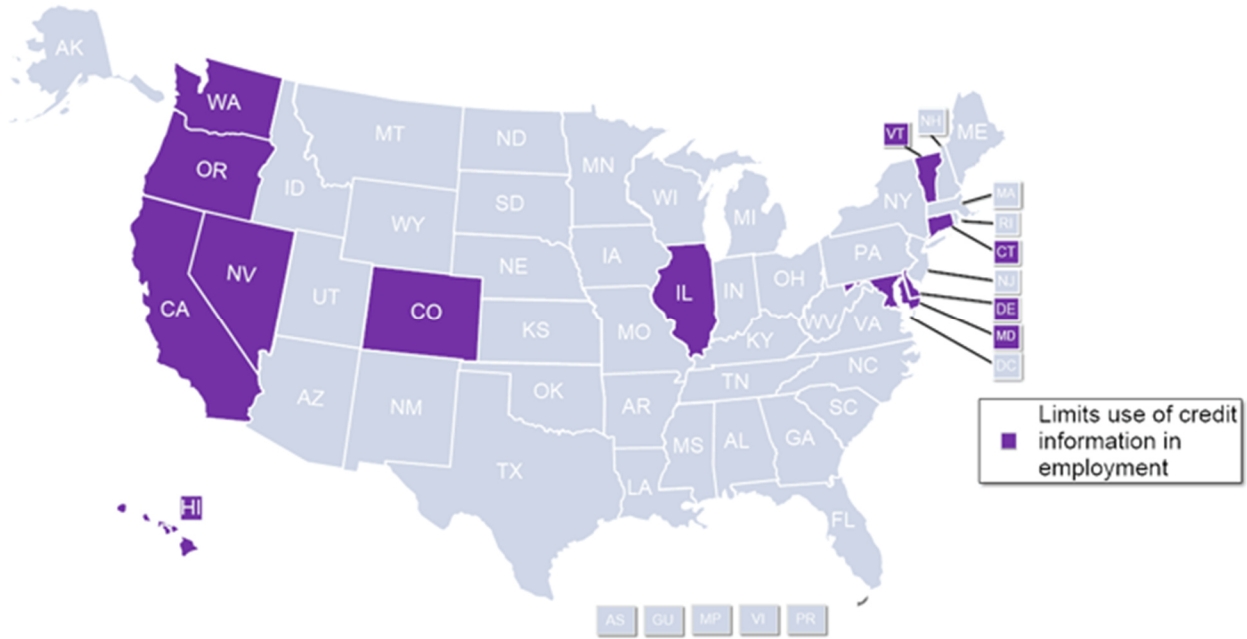
break the potential cycle of limited employment opportunities and bad credit management. The deep downturn in economic activity and severe housing market crises experienced during the Great Recession provided policy makers with plenty of anecdotal evidence to legitimize the implementation of similar policy proposals in many states. Using both labor and credit market data we not only show that these laws made getting a job more difficult, but also they reduced incentives for borrowers to repay their loans and resulted in negative credit market outcomes as well.

Our paper is the only empirical study we know of on this subject, that explicitly tests the predictions of a theoretical model designed to address the effects of such a policy change, namely CCG (2014). We confirm the implications of this theoretical framework with various data sources. Taking away a relatively cheap screening device, credit reports, seem to lower employment opportunities at the county level. Furthermore, individuals in affected states seem to experience adverse credit market outcomes as well, as predicted by CCG (2014). Understanding the actual mechanism through which behavior in both markets interact with each other requires further research. Since, we do not have a rich individual level panel data consisting of observables on both labor and credit markets, it is hard to explain the underlying mechanism with certainty. Nevertheless, our findings suggest that policy makers should exercise caution when contemplating to ban employers' use of credit report checks.

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**Figure 1: Credit Check Ban Legislation**

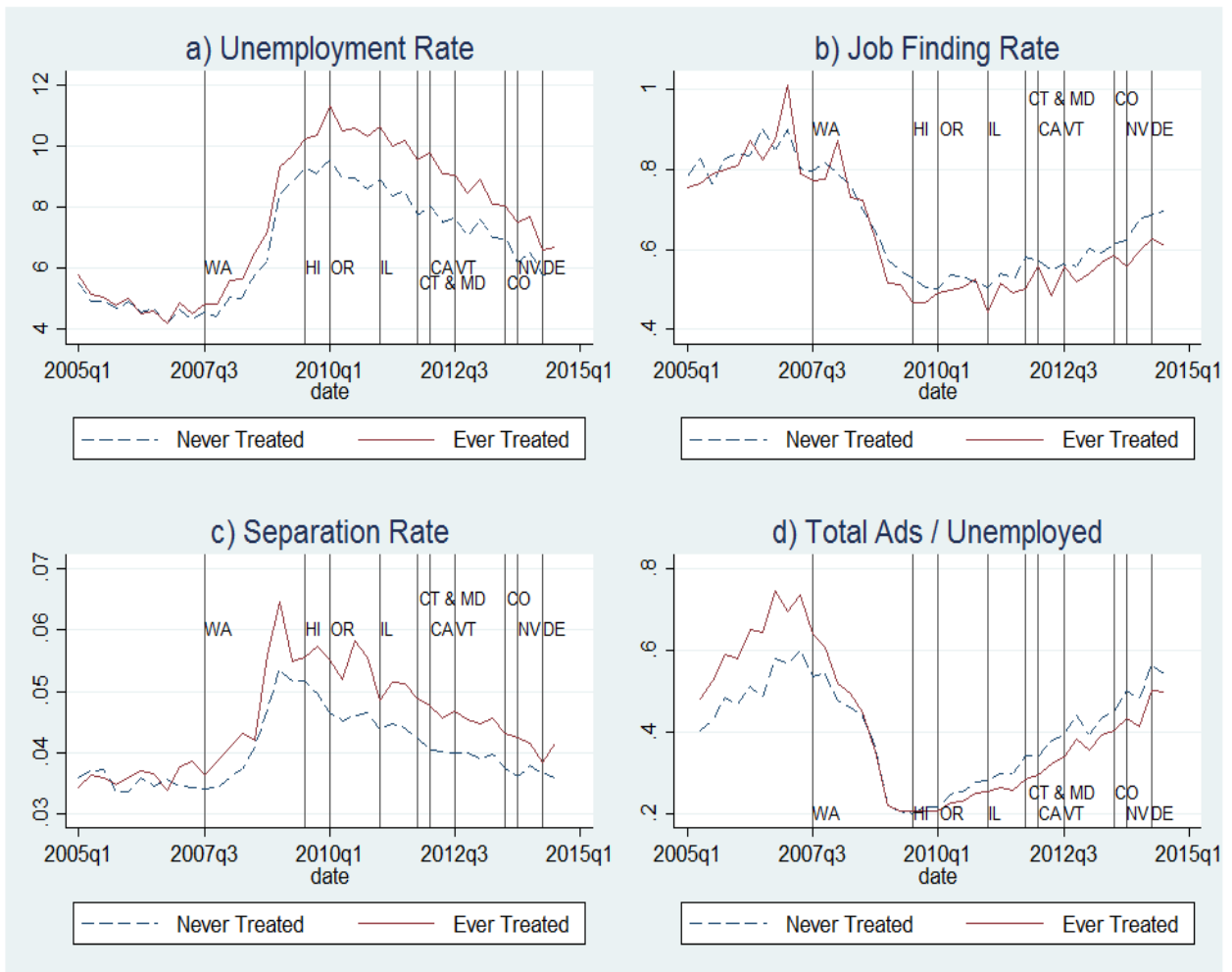


Source: National Conference of State Legislatures

**Table 1: Dates When Bans Went Into Effect**

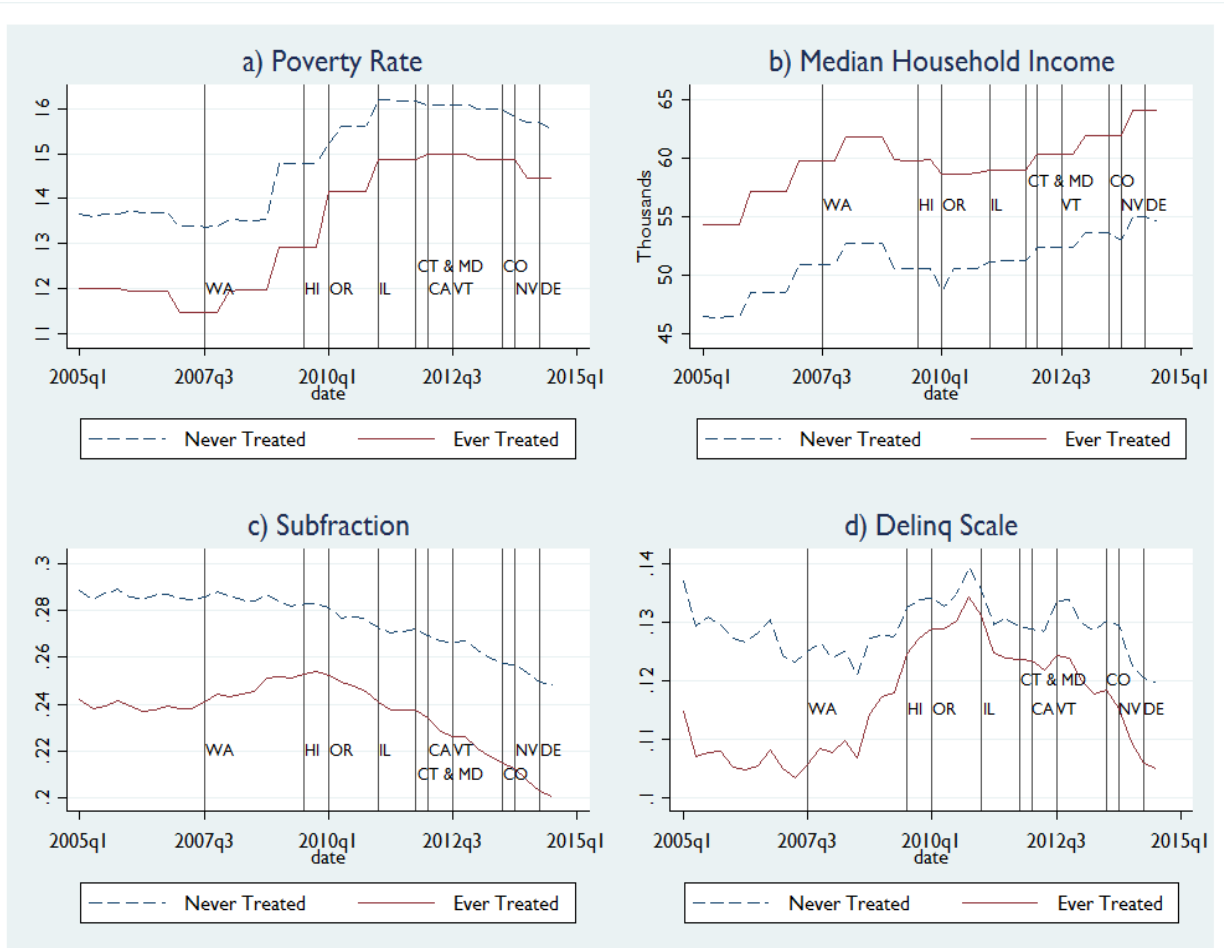
State	Date of Effective Law Change	Neighboring States
CA	1/1/2012	NV, AZ, OR
CO	7/1/2013	UT, WY, NE, KS, OK, NM
CT	10/1/2011	MA, NY, RI
DE	5/8/2014	MD, NJ, PA
HI	7/1/2009	None
IL	1/1/2011	IN, KY, MO, IA, WI
MD	10/1/2011	DE, PA, VA, WV
NV	10/1/2013	AZ, CA, ID, OR, UT
OR	3/29/2010	CA, ID, NV, WA
VT	7/1/2012	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 CPS state level aggregates, where 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 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. Fraction of subprime borrowers (Subfraction) and the percentage of loans that are delinquent come from Equifax. The samples (never treated and ever treated) consist of the same states over time with each treated state indicated by a vertical line when the ban comes into effect.

**Table 2: Summary of Ban Flags – County Level**

Year	Obs.	Law Flag	
		Counties with Law in Effect	States with Law in Effect
2005	3,128	0	0
2006	3,128	0	0
2007	3,128	39	1
2008	3,128	39	1
2009	3,128	44	2
2010	3,142	80	3
2011	3,142	214	6
2012	3,142	286	8
2013	3,142	367	10
2014	3,004	367	10

**Table 3: Unemployment Rate – County Level**

Year	Obs.	Unemployment Rate	
		Mean	Std. Dev.
2005	12,487	5.46	1.99
2006	12,488	4.96	1.87
2007	12,488	4.88	1.85
2008	12,488	5.83	2.24
2009	12,488	9.07	3.35
2010	12,544	9.38	3.31
2011	12,544	8.72	3.13
2012	12,544	7.85	2.88
2013	12,544	7.38	2.79
2014	12,091	6.34	2.47
All Years	124,706	6.99	3.11

**Table 4: Distribution of Subprime Borrowers – County Level**

Year	Obs.	Fraction Subprime				
		Mean	25th Pct	Median	75th Pct	95th Pct
2005	12471	0.29	0.2	0.27	0.36	0.5
2006	12474	0.28	0.2	0.27	0.36	0.5
2007	12476	0.28	0.2	0.27	0.36	0.49
2008	12470	0.28	0.2	0.27	0.35	0.49
2009	12467	0.28	0.2	0.27	0.35	0.48
2010	12543	0.27	0.2	0.26	0.34	0.47
2011	12554	0.26	0.19	0.25	0.33	0.47
2012	12547	0.26	0.19	0.25	0.33	0.46
2013	12545	0.26	0.18	0.25	0.33	0.46
2014	12098	0.25	0.17	0.24	0.32	0.45
All Years	124645	0.27	0.19	0.26	0.34	0.48

**Table 5: 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	0.8234608	0	1.200338
TU (All)	1,958,371	1.30322	1	1.756801
TU (White)	853,181	1.329992	1	1.78953
TU (Black)	131,385	1.395517	1	1.797092
TU (Hispanic)	184,111	1.385175	1	1.802283

	Total Loans Delinquent			
	Obs.	Mean	Median	Std. Dev
Equifax (All)	9,119,384	0.4552683	0.4	0.4050401
TU (All)	1,230,827	0.457442	0.333	0.4766307
TU (White)	580,403	0.4412087	0.333	0.4710095
TU (Black)	83,024	0.4473313	0.333	0.4703455
TU (Hispanic)	125,204	0.4714432	0.333	0.4766454



**Table 6: County Level Unemployment Rate**

This table reports the regression of the effect of the credit check ban on the unemployment rate in county  $i$ , time  $t$  with standard errors clustered by State (including Washington DC). Column (3) has results from the contiguous county specification, so has clustering at both the state and border level.

VARIABLES	Unemployment Rate	Unemployment Rate	Contiguous County Unemployment Rate
Credit Check Ban	0.414*** (0.152)		0.411*** (0.132)
Credit Check Ban*Fraction Subprime		1.614*** (0.442)	
Fraction Subprime		0.0141 (0.153)	
County Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes		
State*Time Fixed Effects		Yes	
County Pair*Time Fixed Effects			Yes
Number of clusters (States / Borders)	51	51	256
Observations	124,706	124,551	58,140
R-squared	0.839	0.912	0.936

Standard Errors Clustered at the State Level

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

**Table 7: County Level Wage Results**

This table reports the regression of the effect of the credit check ban on the wages for all workers and for new hires in county  $i$ , time  $t$  with standard errors clustered by State (including Washington DC). Column (3) and (6) have results from the contiguous county specification, so has clustering at both the state and border level.

VARIABLES	Log Wage		Contiguous County		Contiguous County	
	(All)	(All)	(All)	(New Hires)	(New Hires)	(New Hires)
Credit Check Ban	-0.0079 (0.0092)		0.0135 (0.0155)	-0.0173 (0.0144)		0.0194 (0.0170)
Credit Check Ban*Fraction Subprime		0.0202 (0.0275)			0.0382 (0.0432)	
Fraction Subprime		-0.0141 (0.0098)			-0.0181 (0.0192)	
Observations	121,789	121,455	58,140	121,676	121,372	58,140
R-squared	0.913	0.929	0.998	0.715	0.747	0.993
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	No	No	Yes	No	No
State*Time FE	No	Yes	No	No	Yes	No
Pair*Time FE	No	No	Yes	No	No	Yes
Number of Clusters	51	50	256	51	50	256

Std. Errors Clustered at State Level Except Contiguous County Estimates (Clustered at Both State and Border)

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

**Table 8: Equifax Credit Panel Inquiries and Delinquencies**

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.

VARIABLES	Total Inquiries	Rate of Total Loans Delinquent
Credit Check Ban*Subprime	-0.116*** (0.011)	0.0581*** (0.007)
Credit Check Ban	0.028** (0.013)	-0.00637** (0.003)
Subprime	0.066*** (0.008)	0.260*** (0.002)
County Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes
Borrower Fixed Effects	Yes	Yes
Number of clusters	51	51
Observations	29,974,569	37,147,443
R-squared	0.158	0.66

Standard Errors Clustered at the State Level

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

**Table 9: TransUnion Credit Panel Inquiries by Race**

This table reports the regression of the effect of the credit check ban on total inquiries using the annual TransUnion Consumer Credit Panel, broken down by Race.

VARIABLES	Total Inquiries			
	All	White	Black	Hispanic
Credit Check Ban*Subprime	-0.486*** (0.042)	-0.479*** (0.0468)	-0.481*** (0.0496)	-0.459*** (0.0215)
Credit Check Ban	0.0849*** (0.0196)	0.0653*** (0.0191)	0.0652 (0.0404)	0.132*** (0.0345)
Subprime	0.263*** (0.0168)	0.259*** (0.0144)	0.232*** (0.0212)	0.280*** (0.0203)
Observations	8,344,097	4,895,036	295,125	663,454
R-squared	0.337	0.338	0.383	0.344
Borrower FE	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

Std. Errors Clustered at the State Level

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

**Table 10: TransUnion Credit Total Loans Delinquent by Race**

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.

VARIABLES	Rate of Total Loans Delinquent			
	All	White	Black	Hispanic
Credit Check Ban*Subprime	0.0312*** (0.00408)	0.0316*** (0.00412)	0.00345 (0.00977)	0.0429*** (0.00892)
Credit Check Ban	-0.00247* (0.00137)	-0.00253** (0.00104)	0.0042 (0.0038)	-0.00789*** (0.00276)
Subprime	0.350*** (0.00514)	0.348*** (0.00477)	0.329*** (0.00376)	0.352*** (0.012)
Observations	6,955,336	4,272,570	225,946	551,051
R-squared	0.549	0.537	0.533	0.544
Borrower FE	Yes	Yes	Yes	Yes
Zip Code FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes

Std. Errors Clustered at the State Level

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

## 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 one if the ban was in effect at the beginning of that quarter. Our dates are therefore quite different from those used by Clifford and Shoag (2016), as can be seen below.

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Comparison with Clifford and Shoag (2016)

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State	Exact Date of Ban	First Quarter Ban in Effect	Year Coded by Clifford and Shoag (2016)
CA	1/1/2012	2012Q1*	<b>2010</b>
CO	7/1/2013	2013Q3*	<b>2013</b>
CT	10/1/2011	2011Q4*	2012
DE	5/8/2014	2014Q2	n/a
HI	7/1/2009	2009Q3*	<b>2009</b>
IL	1/1/2011	2011Q1*	<b>2010</b>
MD	10/1/2011	2011Q3*	<b>2011</b>
NV	10/1/2013	2013Q3*	<b>2013</b>
OR	3/29/2010	2010Q1	2010
VT	7/1/2012	2012Q3*	<b>2012</b>
WA	7/22/2007	2007Q3	<b>2007</b>

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

As can be seen, coding quarterly allows for a precise match in 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).

The largest difference between coding conventions occurs for California, for which there is a two-year discrepancy. This is because California's state legislature passed a ban in 2010, but the governor vetoed it. Only after the ban was passed again in 2011 did the governor relent and sign the law, which went into effect at the beginning of 2012.