# "NO MORE CREDIT SCORE"
# EMPLOYER CREDIT CHECK BANS AND SIGNAL SUBSTITUTION

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In the past decade, most states have banned or have considered banning the use of credit checks in hiring decisions, a screening tool that is widely used by employers. Using new Equifax data on employer credit checks, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax, and the LEHD Origin-Destination Employment data, we show that these bans increased employment of residents in the lowest credit score areas. The largest gains occurred in higher-paying jobs and in the government-sector. At the same time, using a large database of job postings, we show that employers increased their demands for other signals of applicants' job performance, like education and experience. On net, the changes induced by these bans generate relatively worse outcomes for those with mid-to-low credit scores, for those under 22 years old, and for Blacks, groups commonly thought to benefit from such legislation.

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## I. Introduction

The use of credit information for employment screening has increased significantly over the last two decades (see Figure 1). Industry surveys indicate that such screening is used by 47 percent of employers (SHR 2012). This screening tool has come under fire, though, by politicians and community groups that claim it unfairly penalizes minority and other vulnerable applicants (Demos 2012). In response to these fears, a number of state governments have passed laws restricting the use of credit information by employers. The first of these laws was passed in Washington in 2007, and as of this writing, eleven states and three municipalities have such laws on the books. Thirty-one other states have considered similar laws. This practice has come under scrutiny at the federal level as well. For example, the Equal Employment Opportunity Commission recently counseled in a discussion letter that "if an employer's use of credit information disproportionately excludes African-American and Hispanic candidates, the practice would be unlawful unless the employer could establish that the practice is needed."[1]

Though employer credit checks are now pervasive and state and local bans on the use of credit information have become increasingly popular, there is currently little research on their economic impact. In this paper, we explore this impact using data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax. These data contain a 5 percent random sample that is representative of all individuals in the US who have a credit history and whose credit file includes the individual's social security number. This large dataset allows us to measure properties of the credit score distribution for extremely detailed geographies like Census tracts and blocks. We pair this credit information with data on employment outcomes for these

[1] "Title VII: Employer Use of Credit Checks," March 9, 2010. EEOC Office of Legal Counsel informal discussion letter. Washington, DC: Equality Employment Opportunity Commission. Available at: http://www.eeoc.gov/eeoc/foia/letters/2010/titlevii-employer-creditck.html

geographies from the LEHD Origin-Destination Employment Statistics (LODES). By comparing outcomes across tracts -- and within tracts, across employment destinations -- we are able to measure the relative impact of these laws on low credit score populations.

We find, robustly, that these bans raised employment in low-credit areas. Our baseline specifications indicate that low credit score tracts (e.g. average credit score below 620) saw employment increase by roughly 1.9-3.3 percent. The origin-destination nature of the LODES data enables us to cleanly identify this effect by exploiting *within* tract-year variation in employment destinations. These gains, in percentage terms, were in relatively higher-paying jobs. Across industries, employers in the public sectors were most affected by these bans, followed by those in transportation and warehousing, information, and in-home services. This pattern makes sense, as compliance is likely high in the public sector and highly regulated industries, such as transportation and information, which provide employees access to secure facilities, goods, people's residences and private information. Employment in construction and food services decline among residents of low credit score tracts following these bans, as people shift to better paying jobs. As expected, employment in the financial sector – which is typically exempted from these bans – is unaffected by the introduction of these laws.

Though employment increased in the lowest-credit tracts following a ban, we find that these increases were mirrored by relative employment declines in mid-to-low credit score tracts (e.g. those with average scores between 630 and 650). Using new data on 74 million online job postings collected by Burning-Glass Technologies, we rationalize this finding by exploring employer experience and education requirements for new hires. A larger fraction of jobs in low-credit score areas began requiring college degrees and prior work experience following a ban on credit screening. This is important evidence of substitution across signals by employers. To our knowledge this is the first demonstration of signal substitution in this large a context.

To explore the net impact of these bans on minority populations, we use data from the American Community Survey Integrated Public Use Micro Data. We compare labor market outcomes for Blacks in states with and without bans, relative to prior trends and conditional on individual controls. We find that the introduction of a ban is associated with a 1 percentage point increase in the likelihood of being unemployed for prime-age Blacks, relative to the contemporaneous change for whites. Thus, it appears that the prohibition of credit screening and the increased emphasis on other signals may actually, relatively, *hurt* minority applicants.

This paper contributes to an important empirical literature on signals in employer screening. Several studies (Bertrand and Mullainathan 2004, Kroft et al. 2013, Correll et al. 2007) have demonstrated the importance of implicit signals like race, work history, and family status in experimental contexts. Fewer studies have looked at the availability of such signals and their equilibrium effects in non-experimental context. Seminal papers in that vein include Autor and Scarborough (2008) and Wozniak (2015). Both papers demonstrate that some signals that seem to penalize minority applicants – a retail personality quiz and drug screening respectively – actually may not do so in equilibrium. Relatedly, Holzer, Raphael, and Stoll (2006) show that employers who check criminal records are more likely to hire blacks, though Finlay (2009) finds that people without criminal records from high-incarceration demographic groups do not have better labor market outcomes with increased testing. Adams (2004) provides evidence that legislation prohibiting the use of age by employers raises employment for older workers, and Goldin and Rouse (2000) shows that eliminating gender signals increases employment for women among musicians.[2] Finally, Balance, Sasser-Modetino, and Shoag (2015a, 2015b) show that employer demands for signals like education and experience are sensitive to labor market conditions in similar job vacancy data from Burning-Glass Technologies.

Relative to this literature, our paper makes three central contributions. First, it provides a cleanly identified estimate of the impact of an economically important screening ban that has not yet been studied in the literature. Second, the paper provides some of the first evidence of large-scale *signal substitution* by employers and confirms that this substitution has disparate impact across demographic groups. Lastly, the paper provides an empirical framework for convincingly identifying the impact of state and local labor laws that target traits that cannot be easily linked at the individual level, like credit scores. Many labor market laws fall into this category – like those prohibiting criminal background checks or those dealing with mental health issues – and the origin-destination identification framework described here has the potential to be useful in these situations.

Our paper also contributes to a growing literature on credit scores themselves, the information they contain, and their potential racial bias. Iyer et al. (2009) shows that credit score information is correlated with non-quantifiable signals of borrow quality, including appearance. Cohen-Cole (2011) shows that lenders treat credit scores differently in heavily black areas. Finally, several papers have shown that while credit scores differ across racial groups, these scores nevertheless contain information about creditworthiness not captured by demographic characteristics. (Avery 2012, Board of Governors 2007).

[2] Another related literature looks at the elimination of race as a signal in the admissions process. Yagan (2012) finds that eliminating race as an explicit signal had a large impact on law school admission, and Belasco et al. (2014) shows that schools with optional SAT submission policies are less diverse.

The paper proceeds as follows. Section two provides a brief description of the Consumer Credit Panel, LODES, and Burning Glass Data, along with summary statistics on tract level outcomes. It also briefly describes the theoretical framework underlying our empirical analysis. Section three describes the central identification strategies and estimates the baseline relationship between credit bans and employment in low credit score tracts. This section also explores the impact of these bans on outcomes by industry and wage bin. Section four introduces estimates using the Burning-Glass data that assess the impact of bans on education and experience requirements. Section five outlines our empirical approach for estimating minority outcomes following a ban in the American Community Survey, and section six concludes.

## II. Data and Theoretical Framework

This paper uses a number of different data sets, and their basic properties bear describing. We provide brief descriptions here, and more elaborate descriptions are provided in our online data appendix. Additionally, though the theoretical motivation for our analysis is relatively straightforward, we also briefly sketch the model underpinning our analysis at the end of this section.

*Equifax Employer Credit Checks*

For employers to obtain a credit file for a job applicant they need to request such information from a credit bureau. The inquiries stay on a credit bureau file for up to two years as “soft” inquiries, meaning they do not impact the credit score of the applicant. As one of the major credit bureaus in the United States, Equifax handles requests from employers for prospective employee’s credit profiles. Equifax provided the total number of employer credit checks listed on credit files in the month of November by state of residence for 2009 through 2014. These totals from Equifax represent the total number of inquiries on files as of the November of each respective year and not the total number of credit files with inquiries, as one credit file with multiple employer credit inquiries will be counted multiple times. Additionally, as one of the three major credit bureaus, Equifax only has information on employers that used their services for such inquiries and does not know when or how often other credit bureaus are used to conduct such inquiries. Thus, while we cannot study absolute changes in the number of employer checks, we can measure relative changes over time in the number of checks performed by this bureau.

*Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP)*

The CCP provides detailed quarterly data on a panel of US consumers from 1999 through the present. The unique sampling design provides a random, nationally representative 5 percent sample of US consumers, as well as the members of their households, with both a credit report and social security number. The dataset can be used to calculate national and regional aggregate measures of individual- and household-level credit profiles at very refined geographic levels (Census blocks and tracts). In addition to housing-related debts (mortgages, home equity lines of credit) this includes credit card, auto and student loans. The panel also provides new insights into the extent and nature of heterogeneity of debt and delinquencies across individuals and households (see Lee and Van der Klaaw, 2010, for further description).

*The LEHD Origin-Destination Employment Statistics (LODES)*

The LODES data, which report employment counts at detailed geographies that can be matched to the CCP, are produced by the U.S. Census Bureau, using an extract of the Longitudinal Employer Household Dynamics (LEHD) data. State unemployment insurance reporting and account information and federal worker earnings records provide information on employment location for covered jobs and residential information for workers. The state data, covering employers in the private sector and state and local government, account for approximately 95 percent of wage and salary jobs. LODES are published as an annual cross-section from 2002 onwards, with each job having a workplace and residence dimension. These data are available for all states, save Massachusetts.[3]

For LODES, a place of work is defined by the physical or mailing address reported by employers in the Quarterly Census of Employment and Wages (QCEW). The residence location for workers in LODES is derived from federal administrative records. LODES uses noise infusion and small cell imputation methods to protect workplace job counts and synthetic data methods to protect the residential location of job holders. The protection of workplace counts uses the same procedure as the Quarterly Workforce Indicators (QWI), namely, multiplying job counts by randomly generated "fuzz factors" specific to each employer and establishment. This coarsening of the residence always occurs at least to the level of Census tracts, which is why we restrict ourselves to this level of refinement or larger in our analysis.  Further explanation of this process can be found in Graham et al (2014). This extra noise is intentionally random and injected into our dependent variable – meaning that while it might inflate our standard errors, it should not bias our results.

*Burning Glass Technologies Labor/Insight Data (BGT)*

Burning Glass Technologies (BGT) is one of the leading vendors of online job ads data. Their Labor/Insight analytical tool contains detailed information on the more than seven million current online job openings updated daily from over 40,000 sources including job boards,

[3] Other states have failed to supply data for some year: the data are unavailable for Arizona and Mississippi in 2004, and New Hampshire and Arkansas in 2003.

newspapers, government agencies, and employer sites.[4] The data are collected via a web crawling technique that uses computer programs called "spiders" to browse online job boards and other web sites and systematically text parse each job ad into usable data elements. BGT mines over seventy job characteristics from free-text job postings including employer name, location, job title, occupation, years of experience requested and level of education required or preferred by the employer. As such, this data allows geographical analysis of occupation-level labor demand by education and experience levels.

The collection process employed by BGT provides a robust representation of hiring, including job activity posted by small employers. The process follows a fixed schedule, "spidering" a pre-determined basket of websites that is carefully monitored and updated to include the most current and complete set of online postings. BGT has developed algorithms to eliminate duplicate ads for the same job posted on both an employer website as well as a large job board by identifying a series of identically parsed variables across job ads such as location, employer, and job title. In addition, to avoid large fluctuations over time, BGT places more weight on large job boards than individual employer sites, which are updated less frequently. The Labor/Insight analytical tool enables us to access the underlying job postings to validate many of the important components of this data source including timeframes, de-duplication, and aggregation. BGT then codes the data to reflect the skill requirements we use below. In total, we have access to data on over 74 million postings from 2007 through 2014.

*National Conference on State Legislatures*

[4] See http://www.burning-glass.com/realtime/ for more details.

The National Conference on State Legislature has been collecting data on state initiatives regarding credit checks in employment screening. We collected this data from their website and through discussions with Heather Morton, a program principal at the NCSL, and state agencies. A map of the laws in place as of this writing is shown in Figure 2, and a list of dates for existing laws are reported in Table 1.

Summary statistics for all of the above data sources is provided in Table 2. Combining these data, we can estimate the baseline employment impact of these laws. We describe our estimation procedure in section III.

*Theoretical Framework*

The hiring decision by employers can be thought of as a screening problem, as in Aigner and Cain (1977) and Autor and Scarborough (2008). Given our finding that eliminating employer credit checks produces relatively worse outcomes for vulnerable groups may be unintuitive to some, we feel that a brief discussion of their models helps to motivate the empirical analysis and results. Therefore we briefly outline below how the elimination of a credit score signal to employers could redistribute away from the group with the lower average score.

To see this, suppose workers come from two identifiable demographic groups $x_1$ and $x_2$, and employers are looking to hire people with quality above a given threshold $k$. Like Autor and Scarborough, we assume that conditional on group identity, the workers quality is known to be distributed normally with means $\mu_1$and $\mu_2$ (where $\mu_1 > k > \mu_2$) and standard deviation $\sigma$. Further, we suppose that a credit check provides an unbiased signal of an individual's true quality, $y$, containing normally distributed mean-zero noise with standard deviation $\gamma$. Note that, as an unbiased signal, the average credit score for individuals in group 2 will be below the average score of those in group 1.

Employer's expectation of any individual's quality is a weighted sum of their credit score *y* and their prior mean $\mu_i$, $E[quality|y\ x_i] = \frac{\gamma^2}{\sigma^2+\gamma^2}\mu_i + \frac{\sigma^2}{\sigma^2+\gamma^2}y$. Individuals whose expected quality exceeds k will be hired.

The elimination of the signal impacts two groups. Individuals from the advantaged group $x_1$ with poor credit scores $\left(y_i < \frac{\sigma^2+\gamma^2}{\sigma^2}k - \gamma^2\mu_1\right)$ are now hired, whereas individuals with good credit scores from the disadvantaged group $\left(y_i > \frac{\sigma^2+\gamma^2}{\sigma^2}k - \gamma^2\mu_2\right)$ are not. Thus, the elimination of the signal can redistribute employment opportunities away from the disadvantaged group even if, on average, they have worse signals.[5] With this theoretically possibility in hand, we now turn to our empirical analysis and investigate the real world impact of these laws.

## III. Baseline Results

*Impact of Legislation on Employer Credit Checks Themselves*

We begin by exploring the impact of a credit check ban on the frequency of employer credit checks. To our knowledge this is the first analysis of this type of data. As discussed above, the data from Equifax is limited in that it represents only a small fraction of total employment related credit checks. Nevertheless, we can use variation in the number of checks in ban and non-ban

[5] In the online Appendix, we further show how other substitution to other signals – like education or experience – can further increase the employment differential between the groups if these signals are more precise (reveal more information about) the higher productivity group. Again, similar results are derived in Autor and Scarborough (2008).

states over time to identify whether or not this state-legislation induces a meaningful change in this segment of the market.

To test this, we first scale the total number of checks by (1) the number of unemployed residents and (2) the number of total hires. We then regress these dependent variables – which measure the intensity with which these checks are used – on state and year fixed effects and an indicator for a statewide ban. The results, reported in Table 3, show that state bans are associated with significant overall declines in employer credit checks. The magnitudes imply a roughly 7-11 percent reduction in the total checks. The reduction is statistically significant when clustering by state and does not appear to be driven by differences in prior trends, as can be seen in Figure 3. It is somewhat surprising that the measured decline is not larger given this behavior is now legally restricted, though this may be partially attributable to the noisy data on checks and the fact that some industries are exempted. Still, despite the limitations of the data, we can observe a meaningful decline in the use of employer credit checks.

*Employment Effect: Across Tract Identification*

Next, we propose to identify the impact of credit check bans using a difference-in-differences approach, comparing the evolution of employment for residents of low credit score tracts in ban states relative to the evolution of similar tracts in non-ban states. This approach is particularly attractive in this setting, because the extreme geographic refinement of our data makes it possible to control for potentially confounding shocks in ban and non-ban states in a myriad of ways.

To produce easily interpretable estimates, we first classify tracts as high and low credit score using a binary division. We do this in two ways.

In our first approach, we begin by constructing the average credit score for each tract and quarter in the CCP. There are a number of small tracts in the data set for which the CCP sample is small and reliable average credit scores cannot be constructed. To deal with this problem we drop any tract for which the highest and lowest average credit score by quarter differ by more than 50 points (roughly 1 standard deviation in the cross sectional distribution, see Figure 4). For the remaining tracts, we classify tracts as having low credit scores if the average credit score lay below 620 (the conventional subprime line) in any quarter.

In our second approach, rather than using average scores, we classify tracts as having low credit scores based on the fraction of the sample below the 620 threshold. To keep things similar to the analysis above, we aimed for a threshold that would mark roughly 15 percent of tracts as having low credit scores. Therefore, we pool observations across quarters, and mark a tract as having low credit scores if more than 38 percent of the individuals residing in that tract have scores below the line. To address the issue of sparsely populated tracts, we drop any tract with a total sample below 50 inquiries in this approach. We show our results for both classification methods.[6]

Using these classifications, we begin by estimating the following regression:

$$\ln employment_{it} = \alpha_i + \alpha_{state \times t} + \alpha_{low\ credit \times t} + \beta \times low\ credit_i \times Ban_{state,t} + \varepsilon_{it} \qquad (1)$$

where *i* and *t* index tract and year. The first term, $\alpha_i$, represents fixed effects for each tract. The second term, $\alpha_{state \times t}$ , represent state-year pair dummies and controls for arbitrary employment trends at the state level. The third term, $\alpha_{low\ credit \times t}$ , is a year dummy multiplied by the low credit score dummy to control for arbitrary employment trend differences between low and high

[6] Obviously other indicators could be used to mark tracts as having low credit score populations, but such measures are strongly correlated and our results do not appear sensitive in robustness experiments.

credit tracts. The final coefficient of interest, $\beta$, measures how low credit score tracts in states with credit check bans fare relative to low credit score tracts in other states and relative to arbitrary within-state trends. The identification assumptions are explained graphically in Figure 5.

Our results are reported in Table 4 below. In Columns (1) and (4), we find that low credit score tracts experienced 2.3-3.3 percent greater employment post-ban relative to the control group. The results are statistically significant, even when clustering the standard errors at the zip code level to control for arbitrary serial correlation and spatial correlation across tracts. We are not aware of any directly comparable estimate, but for context, Wozniak (2015) finds that legislation enabling drug testing shifts minority employment in testing sectors by 7-30 percent.

In Columns (2) and (5), we augment the term $\alpha_{state \times t}$, which controlled for state level aggregate shocks, with the controls $\alpha_{state} \times \alpha_{low\ credit} \times time$. The new regression estimates the impact of bans on low credit score tracts, taking in to account any prior trends in specific state level low-credit employment tracts. In Columns (3) and (6), we use county-year dummies, $\alpha_{county \times t,}$ in lieu of state-year ones. These controls allow for any non-linear pattern of employment changes and identify the impact of the ban by *comparing tracts within county-years*. Despite these rather involved controls, the data continue to suggest employment effects. This log effect, when evaluated at the median, implies roughly 35 additional jobs per year in tracts with low credit scores.

In addition to being interested in the average post-ban impact, we are also interested in the evolution of the employment response. To track this, we substitute out the $Ban_{state,t}$ term in equation (1) for a series of dummies representing years relative to a ban's passage. The coefficient and confidence intervals for these dummies are plotted in Figure 6, showing the event-study effect. We find that there were no differential trends, relative to controls, before a ban's implementation. Afterward, however, there is a large and persistent increase in employment in low credit score tracts.

To further test the robustness of this finding, we also re-run our baseline specification dropping each state with a ban on the use of credit information one at a time. These regressions produce a range of estimates between 1.9 to 4.3 percent, which closely bound our initial results. We also explored the possibility that these findings reflect migration across tracts using data from the 2000 and 2010 Decennial Census. We found no significant effect of credit check bans on population growth in low credit tracts, both within states and within counties, and the point estimates in both cases are close to zero.

*Employment Effect: Within-Tract Identification*

While the above results present a compelling case for the impact of these bans, the LODES employment data is extremely rich and includes information about employment both by place of residence and by place of work. This origin-destination information makes it possible to identify the impact of credit bans within tracts for tracts whose commuting zones bridge ban and non-ban states. For these border areas, we can compare employment outcomes for low and high credit score tracts to destinations with and without a ban.

Specifically, notating *d* as the destination state of employment and *o* as the origin or place of residence, we estimate

$$\ln employment_{o,d,t} = \alpha_{o\times t} + \alpha_{od} + \alpha_{d\times t} + \beta \times low\ credit_{o} \times Ban_{d,t} + \varepsilon_{o,d,t} \qquad (2)$$

The fixed effects $\alpha_{od}$ serve as a fixed effect for this tract-to-state of work pair. The fixed effect $\alpha_{ot}$ controls for arbitrary tracts in overall employment at the tract of residence level. The fixed effect $\alpha_{dt}$ controls for arbitrary state-trends in employment at the destination. Conditional on all of these fixed effects, the coefficient β measures the differential impact of a ban at the destination on employment originating from low credit score tracts. We represent this identification assumption graphically in Figure 7.

We report the results, for all origin-destination pairs with more than 5 workers, in Table 5 below. We do this both for the entire sample and for the sample of origin tracts located *outside* of states with credit bans, which shows cross border commuting. In both specifications we find large increases in employment for low credit score tracts. These increases are measured relative to within tract-outcomes and relative to general trends in employment in destinations with a credit ban. The baseline impact across these specifications is roughly 6-8 percent within tract-destination state pairs and a roughly 24 percent increase in cross-border commuting pairs. The base for these estimates is obviously smaller, and the implied employment gains from these larger percentages (13 and 3 jobs respectively) are sensibly lower as a result. Again, this is evidence that the credit-bans are impacting the distribution of employment even within tract-years. We believe that it is difficult to conjecture an omitted variable bias explanation for these results.

## IV. Mechanism

The LODES employment data are rich, not just in their geographic detail, but also in that they break out employment by wage bins and industry shares. These data are available for more categories and better populated when focusing on tracts as a whole, rather than on origin-destination pairs. Therefore, in this section, we revert to the first identification strategy used in the beginning of the prior section and represented in Figure 5 .

*Across Wage Bins*

In Table 6, we break out our results by exploring the impact on employment by LODES wage bin. We find no increase in employment among jobs paying less than $15,000 annually. There is a sizeable percentage gain in employment in jobs paying between $15,000 and $40,000 a year, and an even larger percentage increase in jobs paying more than $40,000 a year. These results indicate that employer credit checks primarily kept workers out of “better” jobs, rather than the lowest wage bins.

*Across Industries*

We explore the impact of these credit check bans by industry in Tables 7 and 8. This breakout presents an important sensitivity test for our results: the reliance on credit checks varies considerably across industries and some industries were exempted from these bans. It is also reasonable to expect that different industries will be more or less likely to comply with these new laws.

The pattern we find strongly conforms to these patterns. In Columns (1) and (2) of Table 8, we show that far and away the largest impact is on employment in the public sector – either directly by the government or indirectly in education. Both of these sectors relied heavily on credit checks (Society of Human Resource Managers, 2012), and both sectors are – obviously – expected to comply with these laws.

The second largest impact occurs in transportation and warehousing, an industry that provides access to secure goods, facilities, and sensitive client information. Industry publications indicate both that credit and background checks are widely used in this industry[7] and that otherwise qualified employees are often rejected based on these checks.[8] That industry is closely followed by "Other Services" (largely in-home personal aides) and "Information" (e.g. cable installers), both of which provide employees access to people's homes. Again, this was a major reason listed for running credit checks in Society of Human Resource Managers (2012). Finally the last three columns of Table 7 show the three industries with the next greatest impact – "Real Estate", "Retail", and "Health Care" – that involve handling clients' financial information, an establishment's cash, or access to vulnerable clients and secure facilities.

Table 8 presents an interesting reflection of the large effects observed above. While employment increased generally in low credit score tracts, it actually decreased in lower wage industries like "Accommodations and Food Services" and "Construction" that do not intensely use credit checks. Perhaps even more compelling is the fact, demonstrated in Columns (3), (4), and (5) of this panel, that employment in "Finance and Insurance", "Professional Services", and "Management of Companies" is unaffected by these bans. As mentioned above, these industries are generally exempted from the law, and correspondingly, employment in these industries does not change in low credit score tracts.

*Across the Credit Score Distribution*

[7] An industry board claims that 90 percent of medium to large trucking companies use DAC (Drive-A-Check) reports and other background checks when hiring drivers. See http://www.truckingtruth.com/trucking_blogs/Article-3819/what-is-a-dac-report.

[8] "Transportation, Warehousing, and Logistics Workforce: A Job Market in Motion", The Workforce Boards of Metropolitan Chicago. Available at: http://www.workforceboardsmetrochicago.org/Portals/0/Uploads/WBMC_TWL_Rprt.pdf

In the prior tables, we created dummies for low-credit tracts. We measured how these tracts evolved relative to a ‘reference group’ that included all other tracts. In this section, we relax that binary classification. Setting tracts with average scores above 670 as the omitted, reference group (670 being a typical “good score” threshold), we track how employment evolved relative to this benchmark for bins of tracts based on their average credit scores. The impact for each average score range relative to the benchmark is plotted in Figure 8.

The figure shows employment gains for tracts with an average score below 620, with the greatest gains occurring for the lowest scoring tracts. The employment effect becomes negative, just above this threshold, with the greatest employment losses occurring between 630 and 650.

While not definitive, this is strong suggestive evidence that the credit check bans redistributed employment from workers with mid-to-low credit scores to those whose scores register as subprime or below. In the next section, we explore data that illustrates how this redistribution was effected.

## V. Shifts to Other Signals

To study changes in employer demands for other signals following a credit ban, we turn to a new data set on online job postings used in Balance, Sasser-Modestino, and Shoag (2015).
For this project, we use data on roughly 74 million job postings from 2007 through 2013. The smallest geography recorded for each posting is the city level. We match these city level observations to tracts using the US Post Office city name data base using *preferred* place names. To make sure we have a usable sample, we restrict our analysis to cities with over 75 jobs postings per year.

We once again classify cities using a binary approach, creating a dummy if the average credit score profile falls below a cutoff of 620.[9] We then run regressions at the city-year level in the spirit of equation (1), which control for aggregate outcomes within state-years and arbitrary trends for low credit areas. Our dependent variables are the share of jobs requiring a college degree, and average experience required (in years). These variables are created by averaging with equal weight the experience and college education requirements of all postings in a given city and year. Our regressions, reported in Table 9, show that cities with lower credit scores experienced a greater increase in the share of jobs requiring these skills in states with a ban. This is true even when conditioning on a variety of fixed effects to account for aggregate shocks to both low credit scores cities nationally and to states with bans generally. The results indicate a roughly 5 percentage point increase in the share of jobs explicitly mentioning a college degree, relative to the rest of the state in that year, and an additional 3 months of experience on average. This is about a 22 percent increase in the fraction of jobs in these low credit cities requiring a college degree and a 26 percent increase in the average months of required experience.

This substitution to other, potentially less informative signals would be expected in a model of employer search. What's less clear, however, is how this shift from credit checks to increased demand for education and experience affects labor market outcomes for minority and other vulnerable groups. Put simply, do these bans (relatively) help or hurt the people they were supposed to target?

## VI. Vulnerable Populations

[9] We have experimented with other low-credit markets and, again, found very similar results.

Unlike credit scores, race and age can be linked to employment outcomes directly at the individual level. To answer this question, therefore, we turn to data from the American Community Survey. As before, we use a difference-in-differences strategy, comparing outcomes for different groups in ban and non-ban states before and after their enactment. The groups we focus on are Blacks and people below the age of 22, as both groups are the purported beneficiaries of these laws.

The unit of observation is now the individual, rather than the credit tract. The public use versions of these data do not permit us to match to the refined geographies we would need to recover meaningful variation in average credit scores. Therefore, our results are for the entire group in a state with the ban.

We begin with a regression of the form:

$$y_{it} = \alpha_{state-year} + \alpha_{state-race/age} + \alpha_{year-race/age} + \gamma X_{it} + \beta \times race/age_i \times Ban_{state,t} + \varepsilon_{it} \quad (3)$$

where the fixed effects control for aggregate conditions in each state and year, average conditions for a group in a state, and the national conditions for the group. The coefficient $\beta$ measures how African-Americans or young people perform, relative to others in the state post-ban, differently than average for the state. Note that the aggregate effect of the ban (the un-interacted *Ban* regressor) cannot be identified separately from the state-year fixed effects. We also report specifications that add in individual level controls (education, age/race where applicable, and sex), as well as specifications that control for linear, state-specific trends in outcomes for racial groups.

The results are reported in Table 10. Columns (1-3) show that Black unemployment rates were roughly 1 percentage point higher post-ban than other groups in the same state-year. This result is quite robust across specifications and controls. Columns (4-6) show that, young people saw an increase of roughly half this size, though this effect losses significance when controlling for state-specific young adult trends. [10]

The interpretation of this result seems to be that, relative to other groups, these bans contribute to worsening labor market outcomes for Blacks and young people. While this effect is only *relative*, it does suggest that the bans are not primarily assisting those they intend to target.

## VII. Conclusion

In this paper, we have shown that, even with fairly aggressive controls for potentially confounding trends, bans on credit checks in employment are associated with fewer employer credit checks and employment gains in low-credit score areas. These gains happen in mid- to high-wage jobs, with the biggest effect on public sector employment. These gains seem to happen alongside losses in tracts with slightly higher credit scores, and relative reductions in employment and income for Blacks. One explanation for this finding is that firms substitute towards other markers of worker quality, like education and experience, which we also document using new data on job postings. Overall these are intriguing results that should be useful for academics and for the ongoing policy debate regarding these bans. To our knowledge this is the first analysis of these laws, and the first study to use data on employer credit checks. These finding also contribute to the literature on statistical discrimination, and in particular also tie to the findings of Autor and Scarborough (2008) and Wozniak (2015) that highlight the importance of worker quality signals in overcoming statistical and implicit discrimination (Bertrand et al., 2005). Finally, the origin-destination identification framework outlined in this paper can be used

[10] We find similar effects for income, with a roughly 1-2 percent decline for both groups.

to convincingly identify the labor market laws that target traits, like credit scores, which cannot be easily linked to individual labor market outcomes.

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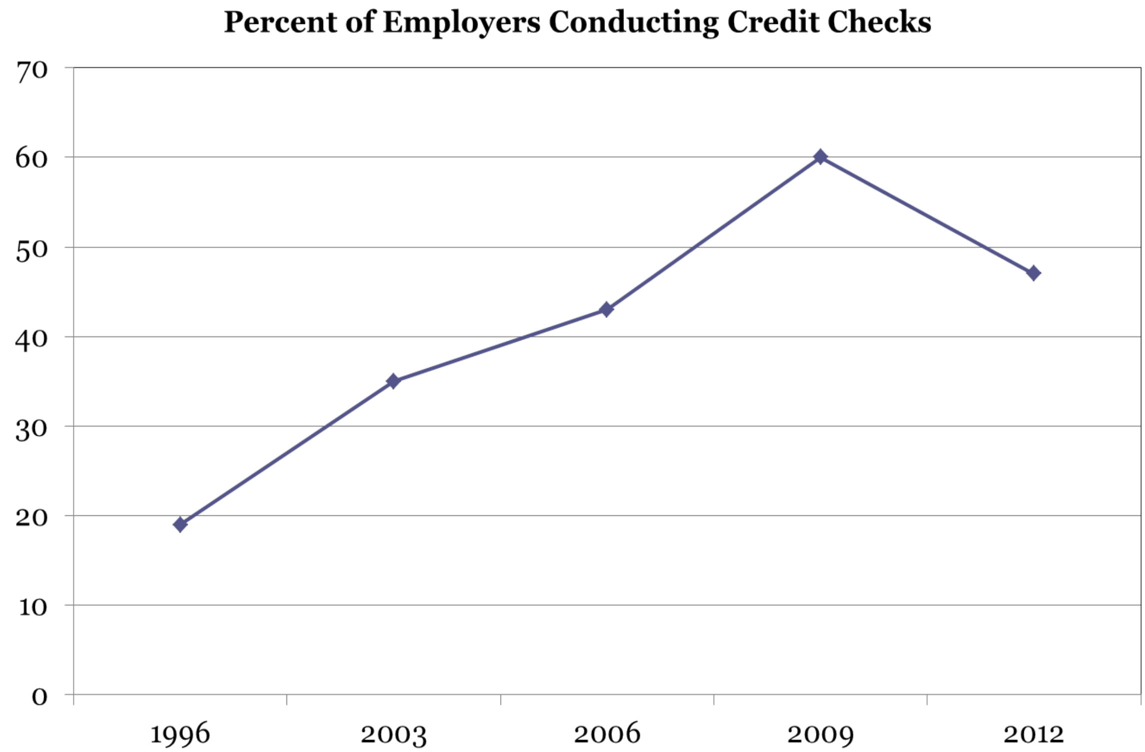
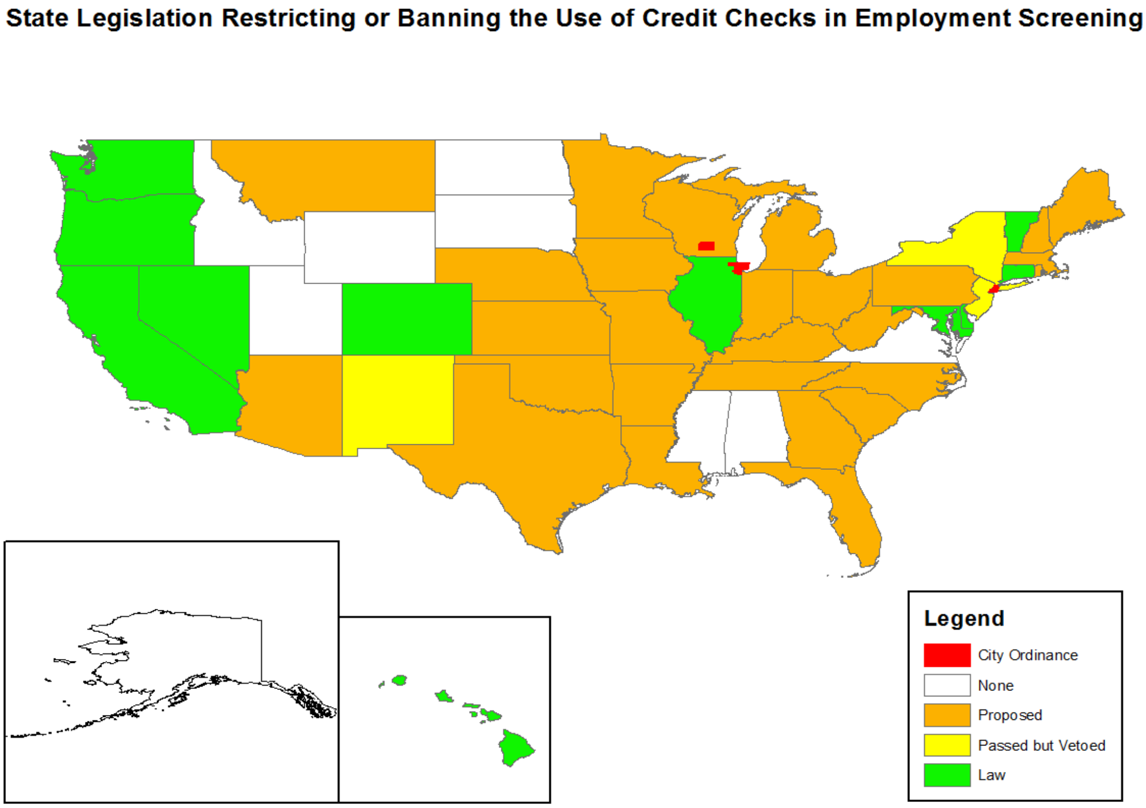
**Figure 1: Use of Credit Checks by Employers Over Time**




Source: Society of Human Resource Management, Survey of Hiring Managers.

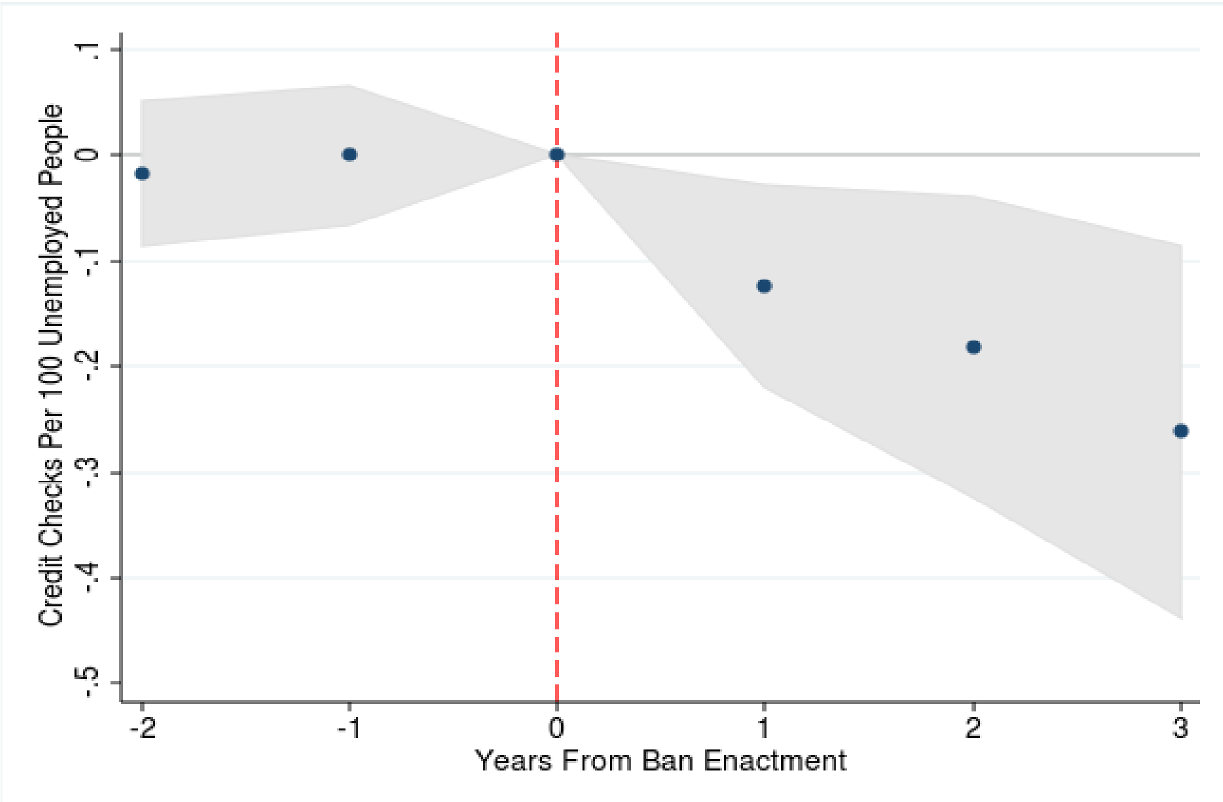
Note: These data come the Society for Human Resource Management's periodic Survey on the Use of Credit Checks in Hiring Decisions.

**Figure 2: State Credit Check Bans**




Source: National Conference on State Legislatures

**Figure 3: Impact of Ban on Employer Credit Checks**

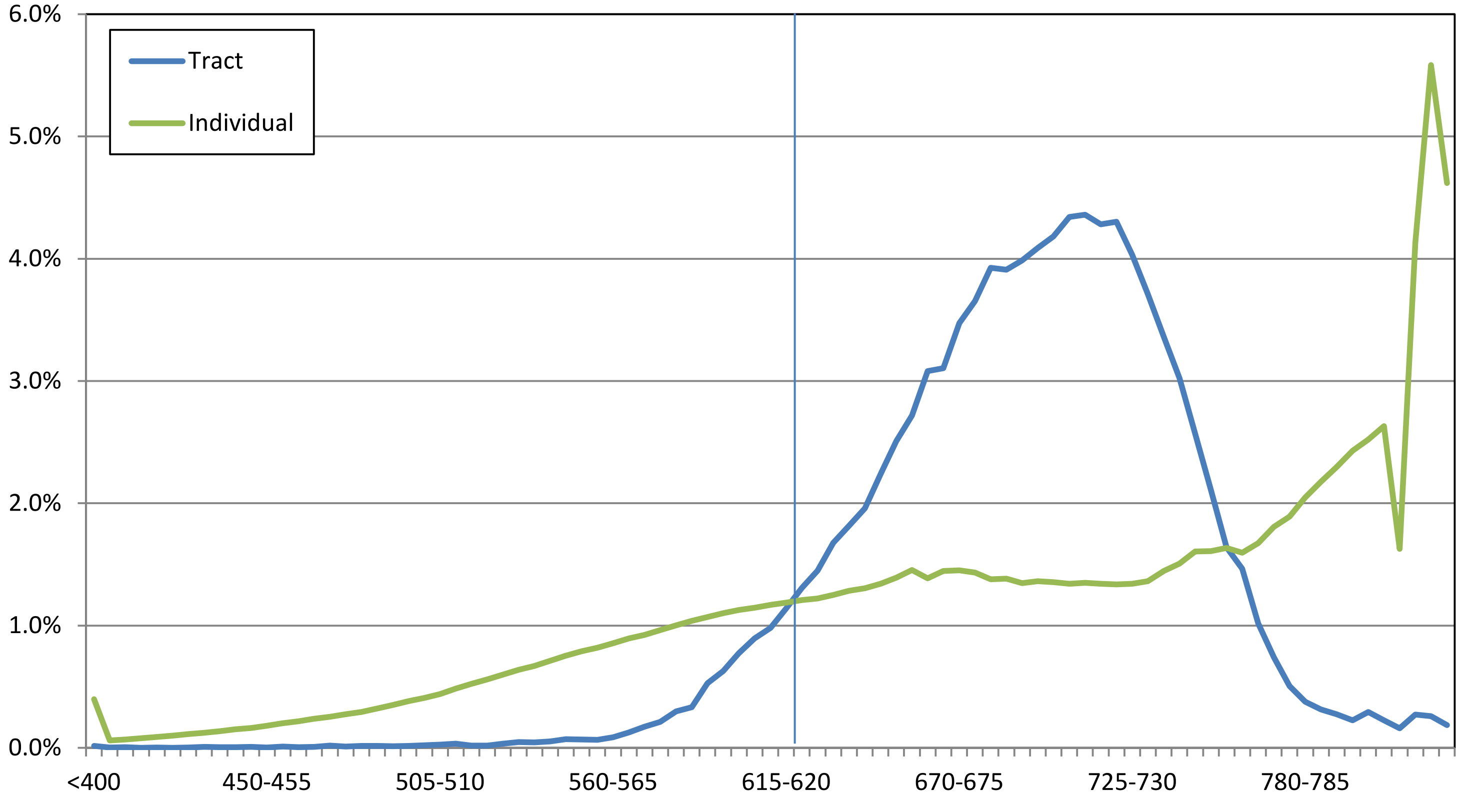



Note: This figure reports the results of the regression:

$$checks\ per\ unemployed_{s,t} = \alpha_s + \alpha_t + \beta_t \times credit\ check\ ban_s \times years\ from\ ban_{s,t} + \varepsilon_{s,t}$$

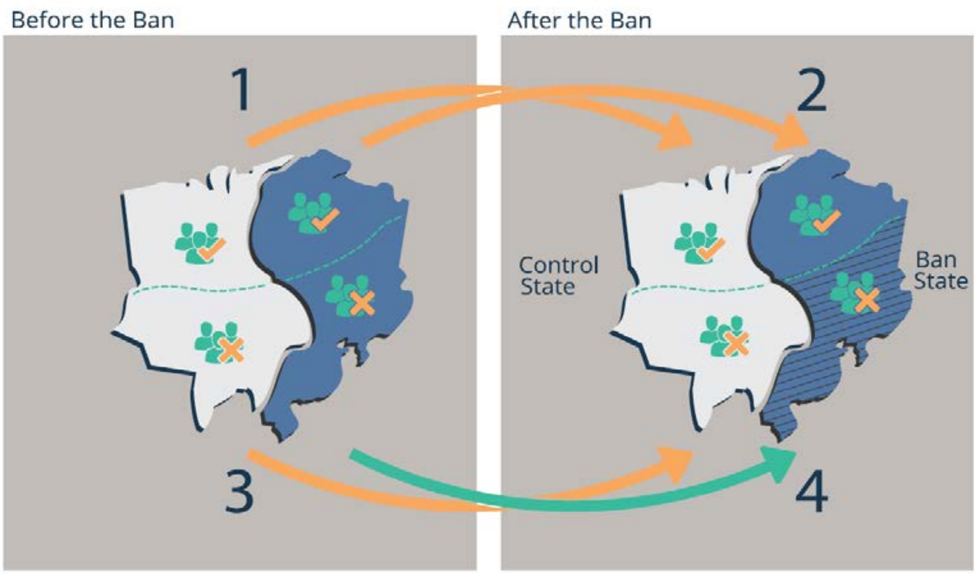
where s indexes state and t indexes year. The graph shows the coefficients beta and the confidence interval. Standard errors are clustered by state. Data from Equifax for 2009-2014. See text for details.

**Figure 4: Distribution of Tract Average Scores**




Source: Federal Reserve Bank of New York/Equifax Consumer Credit Panel

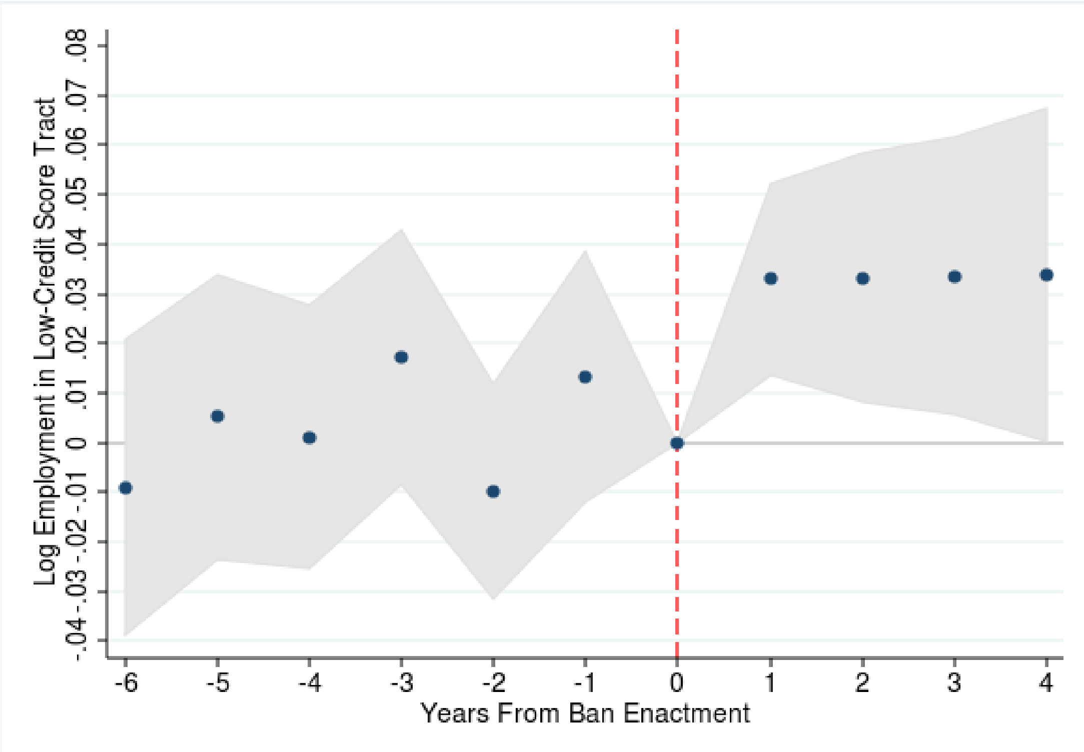
**Figure 5: Illustration of the First Identification Approach**




Note: This figure illustrates the paper's first (triple diff) identification approach.

We consider the evolution of employment for residents of tracts with high average credit scores (populated by the checked individuals) and low average credit scores (individuals with an ×), in states that eventually implement a ban and status quo states that do not. Measures are constructed by tract of residence. Baseline differences across tracts are controlled for by tract fixed effects. Shocks that affect all tracts within a given year are controlled for by year fixed effects. These changes are represented here by the change over time for high average credit score tracts in the status quo state (arrow 1). Shocks that affect all tracts in a state-year are controlled for by state-year fixed effects. These changes are represented here by the change over time for high average credit score tracts in treatment state (arrow 2). Shocks that affect all low average credit score tracts in a given year are controlled for by low average credit score-year fixed effects. These changes are represented here by the change over time for low average credit tracts in the status quo state (arrow 3). The treatment effect measures the change for low average credit tracts in states that implement a ban (arrow 4) relative to all of these other changes. This same identification approach is also used with county-year fixed effects in place of state-year fixed effects (arrow 2) as a robustness check in Table 4. This test then controls for arbitrary changes that affect all tracts in a county-year the same way, and measure the treatment effect relative to these controls.

**Figure 6: Event Study Graph of Credit Check Ban Implementation**

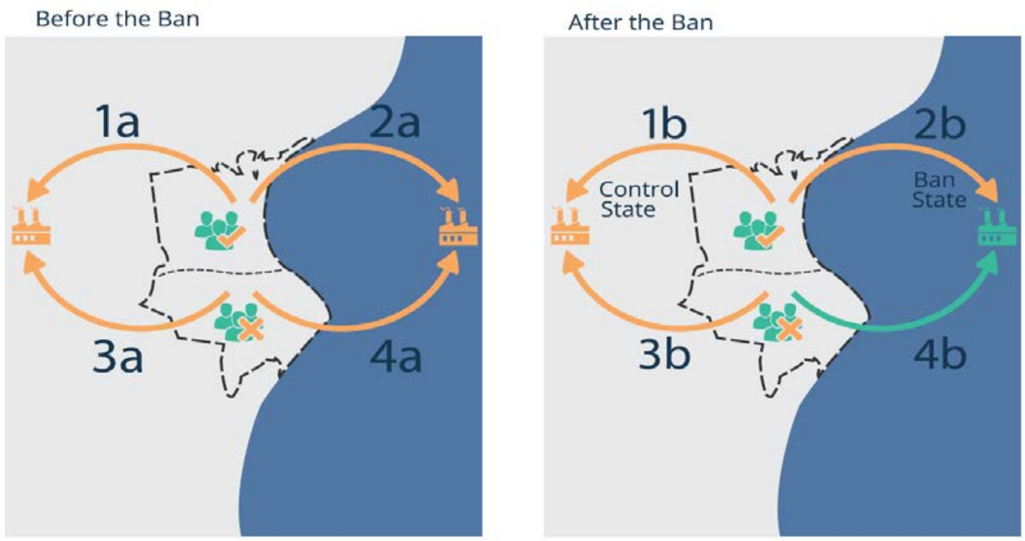



Note: This figure reports the results of the regression:

$$\text{lnemp}_{i,t} = \alpha_i + \alpha_{\text{state}\times t} + \alpha_{\text{low credit}\times t} + \beta_t \times \text{low credit}_i \times \text{Years to Ban}_{s,t} + \varepsilon_{i,t}$$

where $\alpha_i$ are tract level fixed effects, $\alpha_{\text{state*t}}$ are state-year pair fixed effects, and the coefficients beta and their confidence interval are reported. Standard errors are clustered by zip. See text for details.

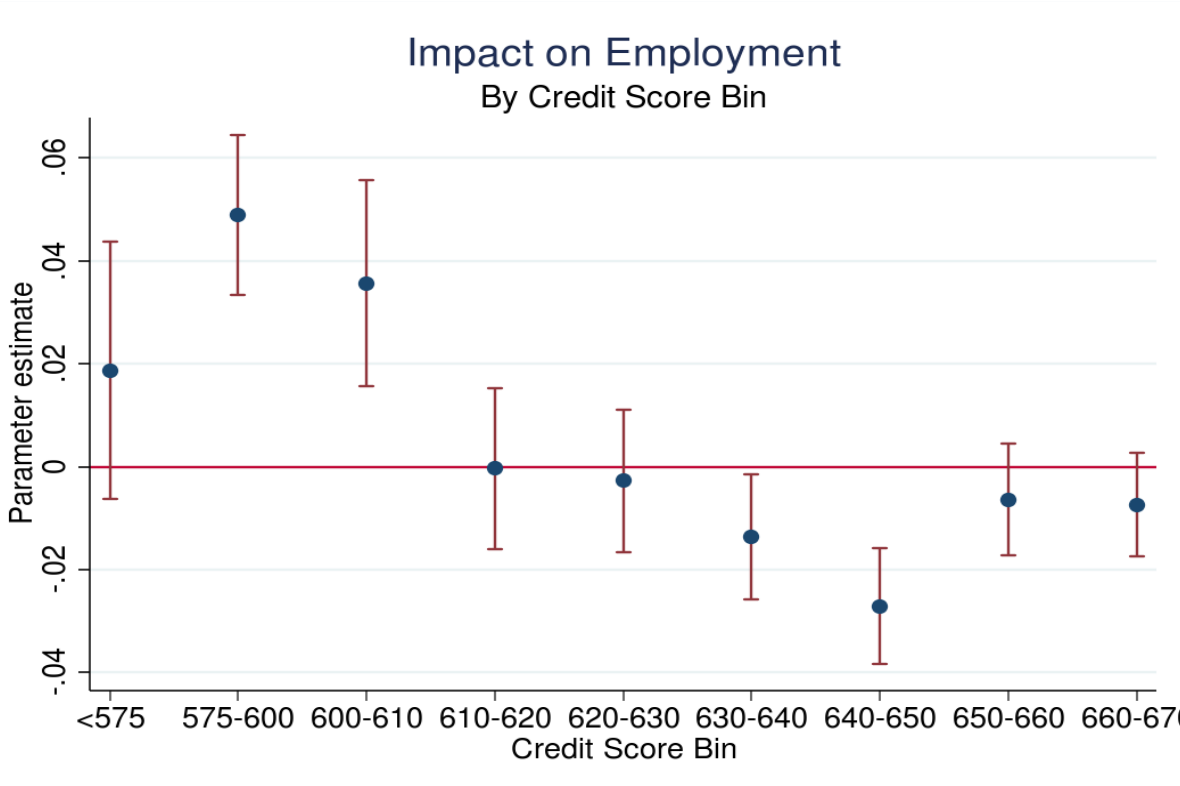
**Figure 7: Illustration of the Second Identification Approach**




Note: This figure illustrates the paper's second (quadruple diff) identification approach.

We consider the evolution of employment for residents of tracts with high average credit scores (populated by the checked individuals) and low average credit scores (individuals with an ×), in destination states that eventually implement a ban (in blue) and status quo states that do not (in white). Baseline differences across residence-work destination pairs are controlled for by residence-work destination fixed effects. Shocks that affect all tracts within a given year are controlled for by year fixed effects. These changes are represented here by change in employment for residents of high average credit score tracts in the status quo state (the change from arrow 1a to 1b).Shocks that affect employment at the destination state from all residence tracts are controlled for by destination-year fixed effects. These changes are represented here by the change in employment for residence of high average credit score tracts in the treatment state (the change from arrow 2a to 2b).Shocks that affect employment in a residence tract in all destination states are controlled for by tract-year fixed effects. These changes are represented here by the change in employment for residence of low average credit score tracts in the status quo state (the change from arrow 3a to 3b). The treatment effect measures the change for residents of low average credit tracts in destination states that implement a ban (arrow 4a to 4b) relative to all of these other changes.

**Figure 8: Impact By Credit Score Bin**




Note: This figure reports the results of the regression:

$$\mathrm{lnemp}_{i,t} = \alpha_i + \alpha_{\mathrm{state} \times t} + \alpha_c \times \text{credit check ban}_{st} + \beta_1 \times \text{credit check ban}_{st} \times 1(\text{Credit Bin } 1)_i + \cdots + \beta_n \times \text{credit check ban}_{st} \times 1(\text{Credit Bin } N)_i + \varepsilon_{i,t}$$

where αi are tract level fixed effects, $\alpha_{state*t}$ are state-year pair fixed effects, and the coefficients beta and their confidence interval are reported. The coefficients measure the relative impact of the ban in tracts with these scores, compared to the benchmark response of tracts with average scores above 670. Observations are tract-year, and standard errors are clustered by zip. See text for details.

**Table 1 : State Credit Check Bans**

| State with Bans | Date | Financial Industry Exception |
|---|---|---|
| California | 2010 | Yes |
| Colorado | 2013 | Yes |
| Connecticut | 2012 | Yes |
| Hawaii | 2009 | Yes |
| Illinois | 2010 | Yes |
| Maryland | 2011 | Yes |
| Nevada | 2013 | Yes |
| Oregon | 2010 | Yes |
| Vermont | 2012 | Yes |
| Washington | 2007 | No |
| New England States Currently Considering a Ban | | Bills |
| Maine | | L.D. 1195 |
| New Hampshire | | H.B. 357, H.B. 1405 (passed) and S.B. 295 (passed) |
| Massachusetts | | H.B. 1731, H.B. 1744 |
| Rhode Island | | S.B. 2587 |

Note: Authors' analysis of the information from National Conference of State Legislators and respective state laws passed in each state.

**Table 2: Summary Statistics of Key Variables**

| VARIABLES | Mean | Standard Deviation | Min | Max | Observations |
|---|---|---|---|---|---|
| *Tract-Year Level* | | | | | |
| Total Employment | 1768 | 881.2 | 1 | 16,140 | 591,119 |
| Employment Below $15K | 494.3 | 236.7 | 1 | 5,953 | 492,137 |
| Employment from $15K to $40K | 679.9 | 348.2 | 1 | 4,558 | 492,086 |
| Employment Above $40K | 594.6 | 426.8 | 1 | 7,046 | 491,658 |
| Average Lowest Quarter Credit Score | 675.7 | 44.0 | 531.3 | 784.4 | 591,087 |
| Fraction with Credit Below 620 | 0.24 | 0.12 | 0 | 0.69 | 591,119 |
| *Origin Tract-State Destination Pair-Year Level* | | | | | |
| Total Employment | 828.4 | 1021.8 | 6 | 16,004 | 1,055,573 |
| Employment with Out-of-State Destination | 52.6 | 117.3 | 6 | 3185 | 577,827 |
| *City-Year Level* | | | | | |
| Share of Postings Requiring a College Degree | 0.2 | 0.11 | 0.002 | 0.914 | 27,121 |
| Avg. Years of Experience Required | 1.22 | 0.65 | 0 | 6.41 | 27,121 |
| Average Lowest Quarter Credit Score | 682 | 34.54 | 544.5 | 816 | 27,106 |
| *State-Year Level* | | | | | |
| Employer Credit Check Per 1,000 Hires | 1.65 | 0.73 | 0.34 | 4.94 | 238 |
| Employer Credit Check Per 1,000 Unemployed | 12.68 | 6.48 | 3.03 | 37.46 | 244 |

Note: Data are from the LEHD Origin-Destination Employer Statistics (LODES), Equifax, Federal Reserve Bank of New York Consumer Credit Panel/Equifax, and Burning-Glass Technologies. Descriptions of all variables are in the text.

**Table 3: Impact of Ban on Employer Credit Checks**

| VARIABLES | (1) Checks per 100 Unemployed $_{it}$ | (2) Checks per 100 Hires $_{it}$ |
|---|---|---|
| State Credit Ban Destination $_{t}$ | -0.132** | -0.0114** |
| | (0.0514) | (0.00465) |
| *Controls* | | |
| State Fixed Effects | X | X |
| Year Fixed Effects | X | X |
| Observations | 244 | 238 |
| R-squared | 0.936 | 0.937 |

Note: Data on employer credit checks are from Equifax. Observations are state-year for 2009-2014. Standard errors are clustered by state. Hires are taken from QWI data, which exclude Massachusetts. We drop cells with fewer than 500 checks due to concerns about data error.

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

**Table 4: Baseline Results**

| VARIABLES | (1) Log Employment $_{it}$ | (2) Log Employment $_{it}$ | (3) Log Employment $_{it}$ | (4) Log Employment $_{it}$ | (5) Log Employment $_{it}$ | (6) Log Employment $_{it}$ |
|---|---|---|---|---|---|---|
| *Average Score Measure* | | | | | | |
| Low Credit-Score Tract $_i$ × State Credit Ban $_t$ | 0.0330*** | 0.0220** | 0.0308*** | | | |
| | (0.0116) | (0.0108) | (0.00990) | | | |
| *Proportion Measure* | | | | | | |
| Low Credit Score Tract $_i$ × State Credit Ban t | | | | 0.0230** | 0.0186* | 0.0201** |
| | | | | (0.0109) | (0.0101) | (0.00982) |
| *Controls* | | | | | | |
| Low Credit x Year Fixed Effects | X | X | X | X | X | X |
| State x Year Fixed Effects | X | X | | X | X | |
| County × Year Fixed Effects | | | X | | | X |
| State Low-Credit Trends | | X | | | X | |
| Observations | 591,119 | 591,119 | 591,119 | 619,632 | 619,632 | 619,632 |
| R-squared | 0.962 | 0.962 | 0.975 | 0.961 | 0.961 | 0.974 |

Note: This table reports regressions of the form:

$$\ln \text{emp}_{i,t} = \alpha_i + \alpha_{\text{state(county)}\times t} + \alpha_{\text{low credit score}\times t} + \beta_t \times \text{credit check ban}_{s,t} \times \text{low credit score}_i + \varepsilon_{i,t}$$

where $\alpha_i$ control for baseline differences across tracts with tract-level fixed effects, $\alpha_{\text{state/county*year}}$ controls for arbitrary trends at the state or county level with state or county-year pair fixed effects, and $\alpha_{\text{low credit score*year}}$ controls for arbitrary, nationwide-low credit tract trends. Regressions (2) and (5) also control for separate linear time trends in employment for low and higher credit score tracts by state. Observations are tract-years, and standard errors are clustered by zip code. The low credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceed thirty-eight percent. See text for additional details.

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

**Table 5: Origin-Destination Based Results**

| VARIABLES | (1) Log Employment $_{it}$ | (2) Log Employment $_{it}$ |
|---|---|---|
| *Average Score Measure* | | |
| Low Credit-Score Origin Tract $_i$ × | | |
| State Credit Ban Destination $_t$ | .0867193 *** | .2414879*** |
| | (.0240412) | (.0274679) |
| *Proportion Credit Measure* | | |
| Low Credit-Score Origin Tract $_i$ × | | |
| State Credit Ban Destination $_t$ | .060553*** | .2399823*** |
| | (.023404) | (.0267035) |
| *Controls* | | |
| Origin-Destination Fixed Effects | X | X |
| Destination-Year Fixed Effects | X | X |
| Origin-Year Fixed Effects | X | X |
| Sample | Origin-Destination Pairs with Employment >5 | |
| | All States | Origin States w/o Law |
| Observations | 1,055,573 | 842,746 |
| R-squared | 0.994 | 0.994 |

Note: This table reports regressions of the form:

$$\ln \text{emp}_{o,d,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta_t \times \text{credit check ban}_{d,t} \times \text{low credit score}_o + \varepsilon_{o,d,t}$$

where $\alpha_{od}$ controls for baseline differences across tract-destination pairs with tract-destination-level fixed effects, $\alpha_{d*t}$ controls for arbitrary trends at the destination level with destination-year fixed effects, and $\alpha_{o*t}$ controls for aggregate outcomes for the tract in the year. These fixed effects allow us to study within-tract year variation. Column (2) restricts the data to tracts in states without a current credit check ban, identifying the effect off cross-border commuting. Because the mean of these cells are lower, the same absolute increase in employment is associated with larger log changes, as is evident in the table. Observations are tract-destination years, and standard errors are clustered by tract. The low credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceed thirty-eight percent. See text for additional details.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

**Table 6: Employmeny by Wages**

| VARIABLES | (1)<br>Log Employment Wage<$15K | (2)<br>Log Employment Wage>$15K & Wage<$40K | (3)<br>Log Empployment Wage>$40K |
|---|---|---|---|
| *Average Score Measure* | | | |
| Low Credit-Score Tract $_i$ x | | | |
| State Credit Ban $_t$ | 0.00465 | 0.0368*** | 0.112*** |
| | (0.00871) | (0.00935) | (0.0154) |
| *Controls* | | | |
| Low Credit x Year Fixed Effects | X | X | X |
| State x Year | X | X | X |
| Observations | 492,137 | 492,086 | 491,658 |
| R-squared | 0.962 | 0.965 | 0.967 |

Note: This table reports regressions of the form:

$$\ln \text{emp in wage bin}_{i,t} = \alpha_i + \alpha_{state*t} + \alpha_{low\ credit\ score \times t} + \beta_t \times \text{credit check ban}_{s,t} \times \text{low credit score}_i + \varepsilon_{i,t}$$

where $\alpha_i$ control for baseline differences across tracts with tract-level fixed effects, $\alpha_{\text{state*year}}$ controls for arbitrary trends at the state or county level with state or county-year pair fixed effects, and $\alpha_{\text{low credit score*year}}$ controls for arbitrary, nationwide-low credit tract trends. Wages bins are constructed by LODES. Observations are tract-years, and standard errors are clustered by zip code. The low credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceed thirty-eight percent. See text for additional details.

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

**Table 7: Employment by Industry -- Large Response**

| Variables | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | Log Employment in: | | | | |
| | Government | Education | Transpo. & Warehousing | Other Services | Information | Real Estate | Retail Trade | Health Care |
| Low Credit Score Tract $_i$ × State Credit Ban $_t$ | 0.193*** | 0.111*** | 0.078*** | 0.077*** | 0.065*** | 0.040*** | 0.029*** | 0.028*** |
| | (0.01) | (0.008) | (0.009) | (0.008) | (0.01) | (0.011) | (0.007) | (0.007) |
| Controls | | | | | | | | |
| Low Credit x Year Fixed Effects | X | X | X | X | X | X | X | X |
| State x Year | X | X | X | X | X | X | X | X |
| Observation | 486,296 | 490,126 | 488,413 | 487,324 | 485,840 | 483,641 | 491,034 | 490,184 |
| R-squared | 0.909 | 0.931 | 0.914 | 0.918 | 0.903 | 0.875 | 0.948 | 0.95 |

Note: This table reports regressions of the form:

$$\ln \text{emp in industry}_{i,t} = \alpha_i + \alpha_{state*t} + \alpha_{\text{low credit score}\times t} + \beta_t \times \text{credit check ban}_{s,t} \times \text{low credit score}_i + \varepsilon_{i,t}$$

where αi control for baseline differences across tracts with tract-level fixed effects, α state*year controls for arbitrary trends at the state or county level with state or county-year pair fixed effects, and α low credit score*year controls for arbitrary, nationwide-low credit tract trends. Industry assignments are constructed by LODES. Observations are tract-years, and standard errors are clustered by zip code. The low credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceed thirty-eight percent. See text for additional details.

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

**Table 8: Employment by Industry -- Small Response**

| Variables | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | Log Employment in: | | |
| | Accommodation & Food Services | Construction | Finance & Insurance | Professional Services | Management of Companies |
| Low Credit Score Tract $_i$ × State Credit Ban $_t$ | -0.023*** | -0.023*** | 0.014 | 0.005 | 0.001 |
| | (0.007) | (0.008) | (0.008) | (0.008) | (0.013) |
| Controls | | | | | |
| Low Credit x Year Fixed Effects | X | X | X | X | X |
| State x Year | X | X | X | X | X |
| Observation | 490,326 | 489,699 | 488,547 | 488,561 | 479,722 |
| R-squared | 0.943 | 0.935 | 0.932 | 0.943 | 0.876 |

Note: This table reports regressions of the form:

$$\ln \text{emp in industry}_{i,t} = \alpha_i + \alpha_{state*t} + \alpha_{\text{low credit score}\times t} + \beta_t \times \text{credit check ban}_{s,t} \times \text{low credit score}_i + \varepsilon_{i,t}$$

where $\alpha_i$ control for baseline differences across tracts with tract-level fixed effects, $\alpha_{\text{state*year}}$ controls for arbitrary trends at the state or county level with state or county-year pair fixed effects, and $\alpha_{\text{low credit score*year}}$ controls for arbitrary, nationwide-low credit tract trends. Industry assignments are constructed by LODES. Observations are tract-years, and standard errors are clustered by zip code. The low credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceed thirty-eight percent. See text for additional details.

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

**Table 9: Signal Substitution -- College Degrees**

| Variables | (1) Share Requires BA | (2) Share Requires BA | (3) Share Requires BA | (1) Log Experience Required | (2) Log Experience Required | (3) Log Experience Required |
|---|---|---|---|---|---|---|
| State Credit Ban $_t$ | -0.00185 | 0.00711** | | 0.0364** | 0.0420** | |
| | (0.00261) | (0.00329) | | (0.0155) | (0.0199) | |
| Low Credit Score City $_i$ x State Credit Ban $_t$ | 0.0616*** | 0.0517*** | 0.0513*** | 0.306** | 0.258** | 0.250** |
| | (0.0180) | (0.0175) | (0.0177) | (0.127) | (0.112) | (0.113) |
| *Controls* | | | | | | |
| City Fixed Effects | X | X | X | X | X | X |
| Low Credit x Year Fixed Effects | X | X | X | X | X | X |
| State Trends | | X | | | X | |
| State x Year Fixed Effects | | | X | | | X |
| Observation | 27,121 | 27,121 | 27,121 | 27,139 | 27,139 | 27,139 |
| R-squared | 0.785 | 0.793 | 0.802 | 0.794 | 0.789 | 0.807 |

Note: This table reports regressions of the form:

$$\text{skill}_{i,t} = \alpha_i + \alpha_{state*t} + \alpha_{\text{low credit score}\times t} + \beta_t \times \text{credit check ban}_{state*t} \times \text{low credit score}_i + \varepsilon_{i,t}$$

where $\alpha_i$ control for baseline differences across cities with city-level fixed effects, $\alpha_{state*year}$ controls for arbitrary trends at the state level with state-year pair fixed effects, and $\alpha_{\text{low credit score*year}}$ controls for arbitrary, nationwide-low credit city trends. The share of postings requiring a BA and the average year of experience required by all city-year postings are constructed from Burning-Glass data. Observations are postal city-years, and standard errors are clustered by city. The low credit score measure is a dummy for the average score falling below 620 . See text for additional details.

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

**Table 10: Vulnerable Populations**

| VARIABLES | (1) | (2) Unemployed | (3) | (4) | (5) Unemployed | (6) |
|---|---|---|---|---|---|---|
| Black x State Ban | 0.0111*** | 0.0109*** | 0.0122*** | | | |
| | (0.00298) | (0.00289) | (0.00323) | | | |
| Young x State Ban | | | | 0.00644* | 0.00716* | 0.00293 |
| | | | | (0.00353) | (0.0039) | (0.00266) |
| *Controls* | | | | | | |
| State x Year | X | X | X | X | X | X |
| Black/Young x State | X | X | X | X | X | X |
| Black/Young x Year | X | X | X | X | X | X |
| Individual Demographics | | | | | X | |
| Black/Young x State Linear Trends | | | X | | | X |
| Observations | 12,278,302 | 12,278,302 | 12,278,302 | 12,278,302 | 12,278,302 | 12,278,302 |
| R-squared | 0.014 | 0.038 | 0.014 | 0.018 | 0.036 | 0.018 |

Note: This table reports regressions of the form:

$$employed_{i,t} = \alpha_{\text{group-state}} + \alpha_{\text{state-year}} + \alpha_{black-year} + \gamma \times X_{i,t} + \beta_{\text{t}} \times \text{credit check ban}_{\text{st}} \times \text{group}_{\text{i}} + \varepsilon_{\text{i,t}}$$

where α control for arbitrary trends for blacks and for states, and for arbitrary racial differences across states. The data are from the American Community Survey from 2005 to 2013. Specification 2 controls for education dummies, age/race dummies where not already controlled for by the fixed effects, and gender. Standard errors are clustered by state. See text for additional details.

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