HARVARD Kennedy School
JOHN F. KENNEDY SCHOOL OF GOVERNMENT

"No More Credit Score" Employer Credit Check Bans and Signal Substitution

Faculty Research Working Paper Series

Robert Clifford
Federal Reserve Bank of Boston

Daniel Shoag
Harvard Kennedy School

February 2016

RWP16-008

“NO MORE CREDIT SCORE”
EMPLOYER CREDIT CHECK BANS AND SIGNAL SUBSTITUTION

Robert Clifford
Daniel Shoag

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, group commonly thought to benefit from such legislation.

Corresponding Author:

Daniel Shoag
Harvard Kennedy School
79 JFK Street
Cambridge, MA 02138
617-595-6325
Dan_shoag@hks.harvard.edu

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% 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.

Though 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 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% relative to trends within the state and low credit score tracts in states without these bans. 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 service decline among residents of low credit score tracts following these bans, as people shift to better jobs. As expected, employment in the financial sector – which is typically exempted from these bans – is unaffected by their introduction.

Though employment increased in the lowest-credit tracts (average below 620) following a ban, we find that these increases were mirrored by relative employment declines in mid-to-low credit score tracts (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 explore the net impact of this shift for 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 is of special import to policy-makers in New England. Connecticut and Vermont were among the first states to institute a ban on credit checks, and Rhode Island, Massachusetts, New Hampshire, and Maine have considered or are considering similar legislation. New England senators Elizabeth Warren (MA), Richard Blumenthal (CT), Patrick Leahy (VT), Edward Markey (MA), Jeanne Shaheen (NH), and Sheldon Whitehouse (RI) accounted for six out of the seven sponsors on recent legislation to extend this ban nationwide. Moreover, many of New England's metropolitan labor markets have disproportionately more young people, whose labor market outcomes are potentially affected by these bans. Quality research on the impact of these bans can meaningfully guide the ongoing policy discussions in this region.

This paper builds on a growing empirical literature on employer screening. Palmer and Koppes (2012) and Weaver (2015) show that lower credit scores are uncorrelated with employee performance. Autor and Scarborough (2008) and Wozniak (2015) demonstrate that some signals that seem to penalize minority applicants– a retail personality quiz and drug screening respectively – actually do not do so in equilibrium. 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. Finally, Balance, Sasser-Modetino, and Shoag (2015a, 2015b) show that employer demands for education and experience are sensitive to labor market conditions in similar job vacancy data from Burning-Glass Technologies.

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 some 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 just one of the 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% 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. [1]

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 jobs. 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 – 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,

[1] Other states have failed to supply data for some year: the data are unavailable for AZ and MS in 2004, and for NH and AR in 2003.

newspapers, government agencies, and employer sites.[2] 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

[2] 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.

Combining these data, we can estimate the baseline employment impact of these laws. We describe our estimation procedure in the next section.

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 that 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 μ_2 (where $\mu_1 > k > \mu_2$) and standard deviation σ. 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 γ. 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 meanμ_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 disadvantaged group even if, on average, they have worse signals.[3] 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

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

[3] In the 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.

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. The magnitudes imply a roughly 7-11% 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 these employment screens.

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 Consumer Credit Panel. 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 3). 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 to roughly 15% of total tracts as low credit. Therefore, we pool observations across quarters, and mark a tract as having low credit scores if more than 38% 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.[4]

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, α_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

[4] 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, β, 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.

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% 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.

In Columns (2) and (5), we augment the term $\alpha_{state \times t}$, which controlled for state level aggregate shocks, with the controls$\alpha_{state} \times \alpha_{low\ credit} \times t$. 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. 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.

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 5, 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.

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

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

The fixed effects α_{od} serve as a fixed effect for this tract-to-state of work pair. The fixed effect α_{ot} controls for arbitrary tracts in overall employment at the tract of residence level. The fixed effect α_{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 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 <u>outside</u> of states with credit bans, which shows cross border commuting. In both specifications we find large increases in employment for low credit score tracts, relative to the tract as a whole and the general trend for the destination, in destinations with a credit ban. The baseline impact across these specifications is roughly 6-8% within state and a roughly 24% increase in cross-border commuting (though the base is obviously smaller). Again, this is evidence that the credit-bans are impacting the distribution of employment even within tract-years.

IV. Mechanism

The 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 identification strategy used in the beginning of the prior section.

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 $15K annually (in fact registering a slight decline). There is a sizeable percentage gain in employment in jobs paying between $15 and $40K a year, and an even larger percentage increase in jobs paying more than $40K 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 confirms to these patterns. In Columns (1) and (2), 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[5] and that otherwise qualified employees are often rejected based on these checks.[6] 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 two columns of Table 6 show the two industries with the next greatest impact – "Real Estate" and "Retail" – that involve handling clients' financial information or an establishment's cash.

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

[5] An industry board claims that 90% of medium to large trucking companies use DAC reports and other background checks when hiring drivers. http://www.truckingtruth.com/trucking_blogs/Article-3819/what-is-a-dac-report. http://www.eeoc.gov/eeoc/meetings/10-20-10/credit_background.cfm

[6] Transportation, Warehousing, and Logistics Workforce: A Job Market in Motion, The Workforce Boards of Metropolitain Chicago

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

In the prior tables, we created dummies for low-credit tracks. In this section, we relax that binary classification. Setting tracts with average scores about 670 as the benchmark, we track how employment evolved relative to this benchmark in bins of tracts based on average credit scores. The impact for each bin relative to the benchmark is plotted in Figure 6.

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 vacancies 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.[7] 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 month of experience on average.

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

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

To get traction on this question, 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.

Unfortunately, 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 – not merely the group living in low credit score locations.

We begin with a regression of the form:

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

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 β 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 unemployment rates were roughly 1% 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. [8]

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 African-Americans. 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).

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

References

Autor, David H., and David Scarborough. "Does job testing harm minority workers? Evidence from retail establishments." *The Quarterly Journal of Economics* (2008): 219-277.

Bertrand, Marianne, Dolly Chugh, and Sendhil Mullainathan "Implicit Discrimination." *American Economic Review* (2005) 95(2): 94-98.

Demos "Discredited: How Employer Credit Checks Keep Qualified Workers Out of A Job" (2012)

Finlay, Keith. "Effect of employer access to criminal history data on the labor market outcomes of ex-offenders and non-offenders." *Studies of Labor Market Intermediation*. University of Chicago Press, 2009. 89-125.

Graham, Matthew R., Mark J. Kutzbach, and Brian McKenzie. *Design Comparison Of Lodes And ACS Commuting Data Products*. No. 14-38. 2014.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll. "Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers*." *Journal of Law and Economics* 49.2 (2006): 451-480.

Koppes Bryan, Laura, and Jerry K. Palmer. "Do Job Applicant Credit Histories Predict Performance Appraisal Ratings or Termination Decisions?." *The Psychologist-Manager Journal* 15.2 (2012): 106-127.

Lee, Donghoon, and Wilbert Van der Klaauw. "An Introduction to the FRBNY Consumer Credit panel." *FRB of New York Staff Report* 479 (2010).

Modestino, Alicia Sasser, Daniel Shoag, and Joshua Ballance. "Upskilling: Do Employers Demand Greater Skill When Workers are Plentiful?." Draft, Northeastern University (2015a).

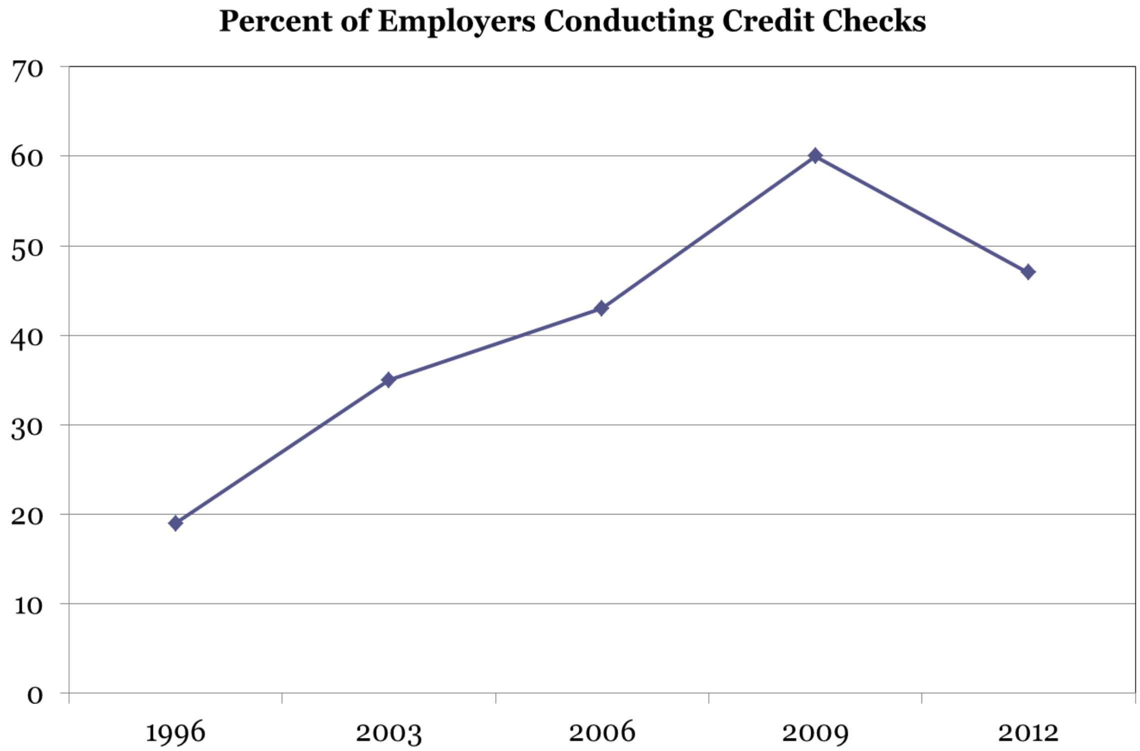
Modestino, Alicia Sasser, Daniel Shoag, and Joshua Ballance. "Downskilling: Changes in Employer Skill Requirements Over the Business Cycle" Draft, Northeastern University (2015b).

Society of Human Resource Managers "Background Checking- The Use of Credit Background Checks in Hiring Decisions" (2012)

Weaver, Andrew. "Is Credit Status a Good Signal of Productivity?." ILR Review (2015): 0019793915586382.

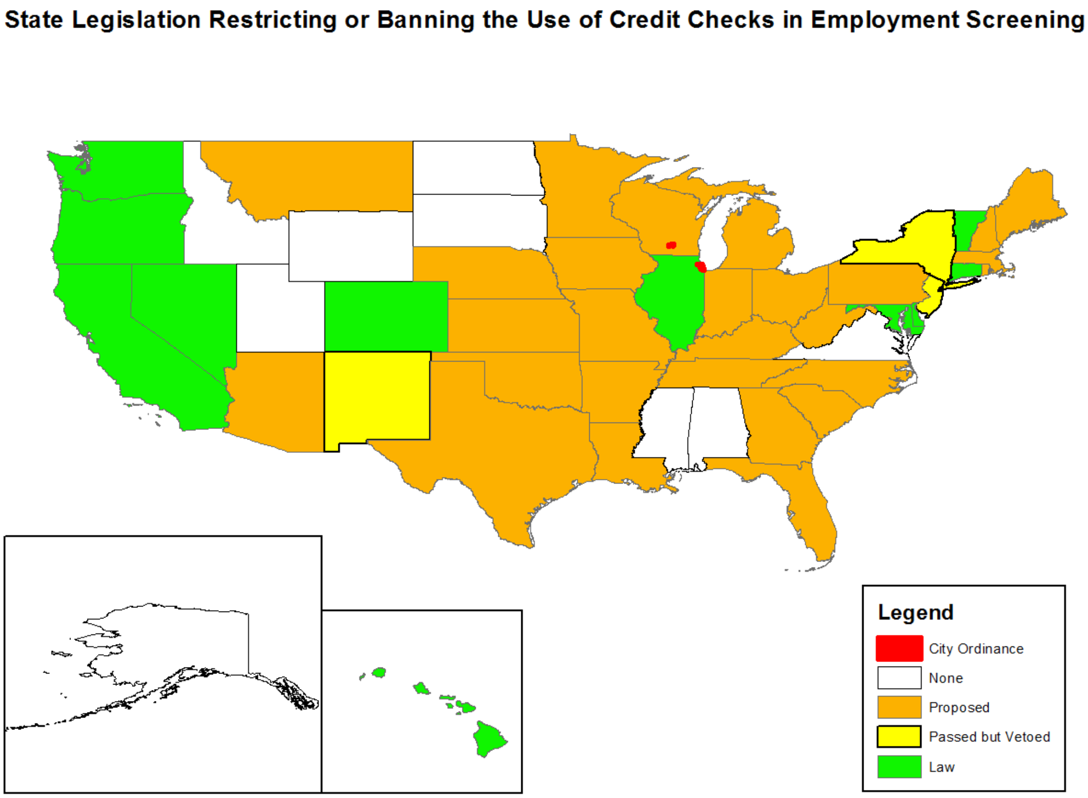
Wozniak, Abigail. "Discrimination and the effects of drug testing on black employment." *Review of Economics and Statistics* (2012).

Figure 1: Use of Credit Checks by Employers over Time



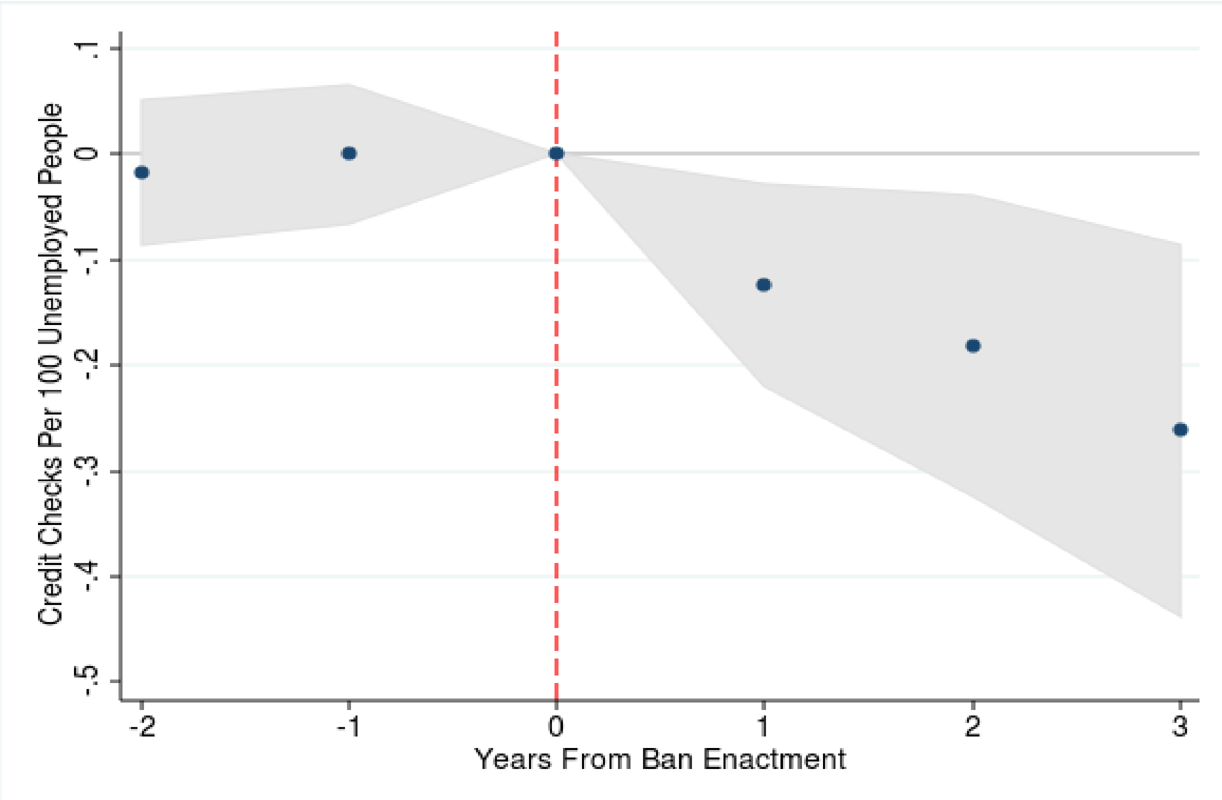
Source: Society of Human Resource Management, Survey of Hiring Managers.

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:

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

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

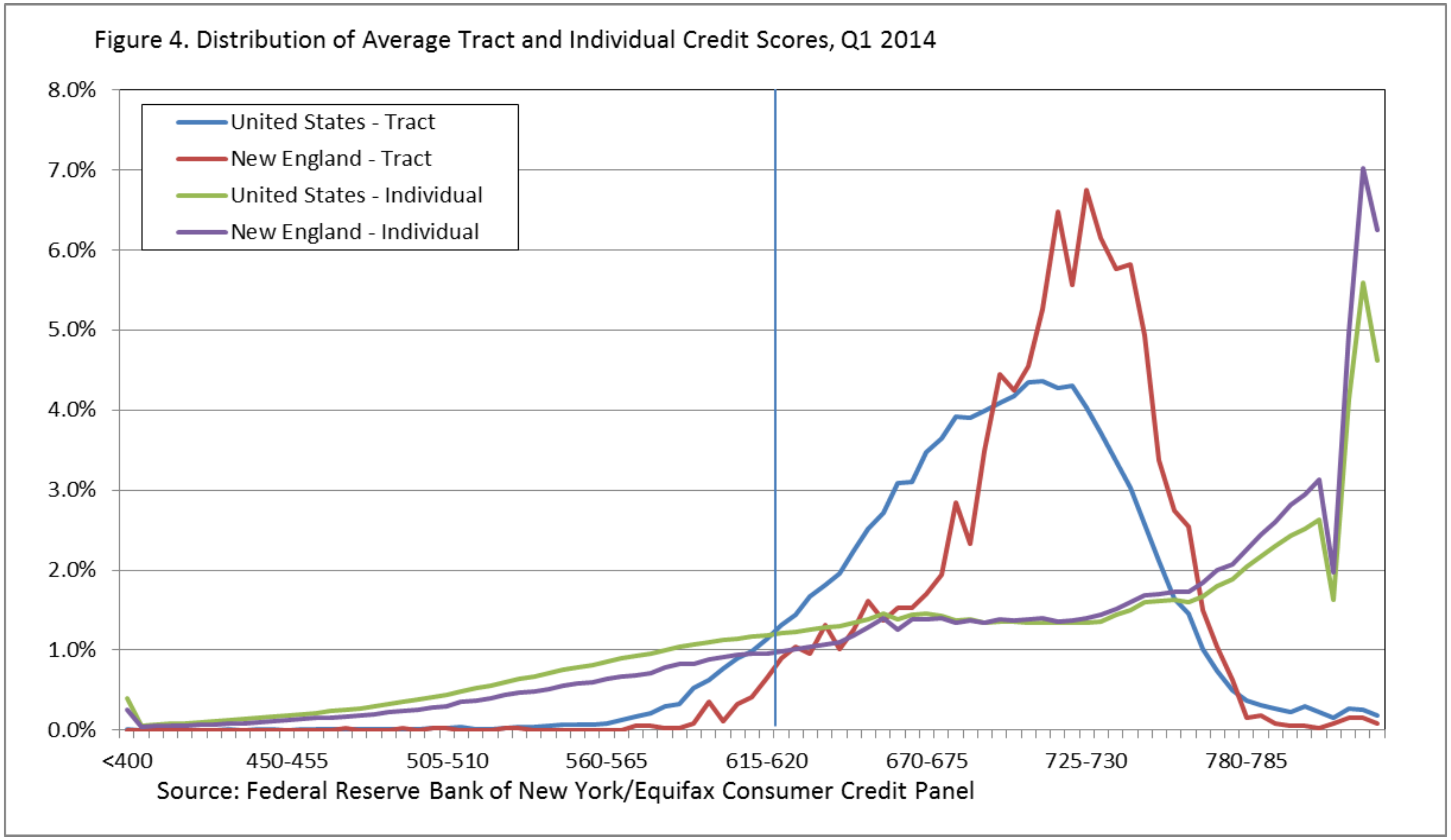
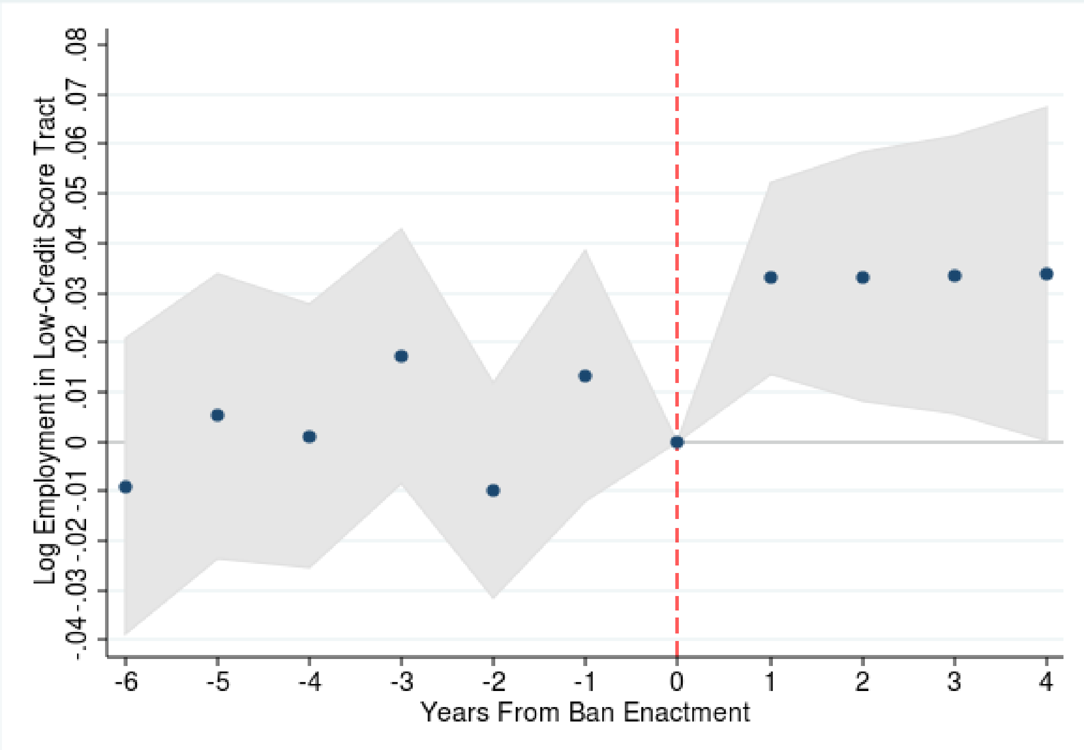


Figure 5: Event Study Graph of Credit Check Ban Implementation

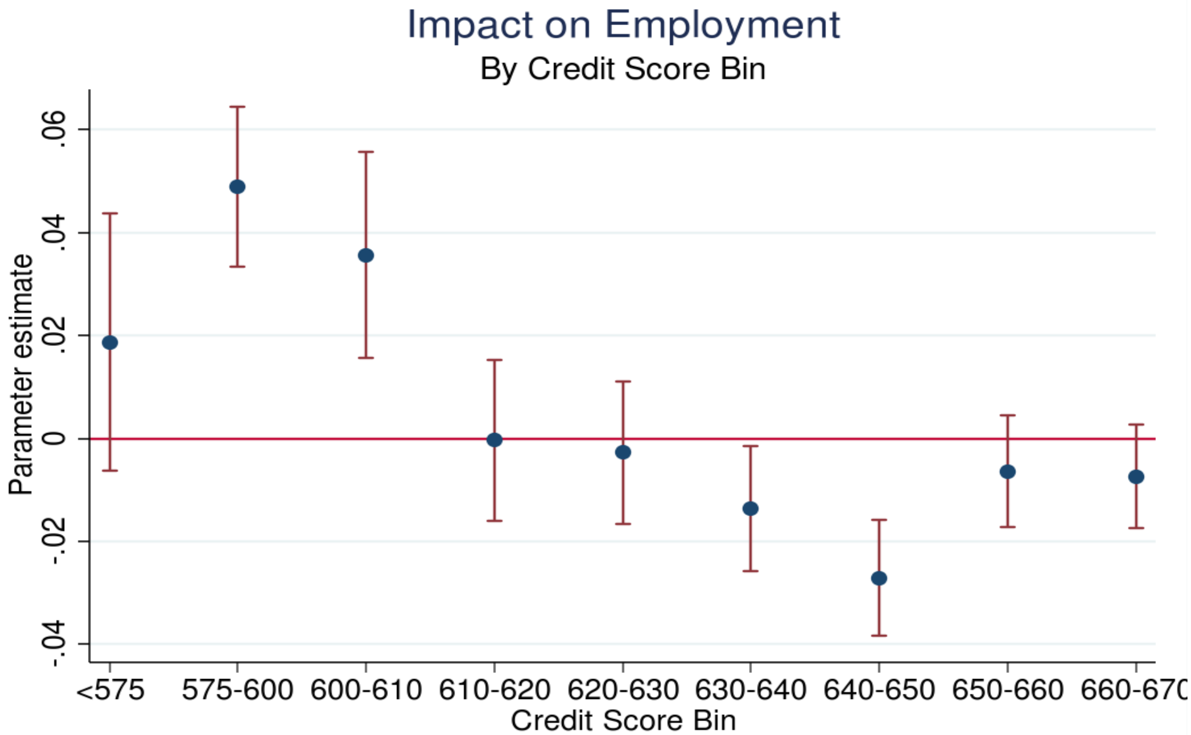


Note: This figure reports the results of the regression:

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

where α_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 6: Impact By Credit Score Bin



Note: This figure reports the results of the regression:

$$\begin{aligned}\text{lnemp}_{it} = {} & \alpha_i + \alpha_{\text{state}\times t} + \alpha_c \times \text{credit check ban}_{st} + \beta_1 \times \text{credit check ban}_{st} \times 1(\text{Credit Bin } 1)_i + \cdots \\ & + \beta_n \times \text{credit check ban}_{st} \times 1(\text{Credit Bin } N)_i + \varepsilon_{it}\end{aligned}$$

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: Information from the NCSL

Table 2: Summary Statistics of Key Variables

VARIABLES	Mean	Standard Deviation	Min	Max	Observations
Tract-Year Level					
Total Employment	1768	881.2	1	16140	591119
Employment Below $15K	494.3	236.7	1	5953	492137
Employment from $15K to $40K	679.9	348.2	1	4558	492086
Employment Above $40K	594.6	426.8	1	7046	491658
Average Lowest Quarter Credit Score	675.7	44.0	531.3	784.4	591087
Fraction with Credit Below 620	0.24	0.12	0	0.69	591119
Origin Tract-State Destination Pair-Year Level					
Total Employment	828.4	1021.8	6	16004	1055573
Employment with Out-of-State Destination	52.6	117.3	6	3185	577827
City-Year Level					
Share of Postings Requiring a College Degree	0.2	0.11	0.002	0.914	27121
Avg. Years of Experience Required	1.22	0.65	0	6.41	27121
Average Lowest Quarter Credit Score	682	34.54	544.5	816	27106
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), the Equifax/ Federal Reserve Bank of NY Consumer Credit Panel, Equifax, and Burning-Glass Technologies. Descriptions of all variables are

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 clustered by state. Hires 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:

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

where α_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 arbitray, nationwide-low credit tract trends. Regressions (2) and (5) also control for seperate 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-Dest 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:

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

where α_{od} controls for baseline differences across tracts-destination pairs with tract-destination-level fixed effects,α_{d*t} controls for arbitrary trends at the destination level with destination-year fixed effects, and α_{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-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

Table 6: Origin - Destination Based Results

VARIABLES	(1) Log Emp Wage<$15K	(2) Log Emp Wage>$15K & Wage<$40K	(3) Log Emp Wage>$40K
Average Score Measure			
Low Credit-Score Tract $_i$ × 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:

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

where α_i control for baseline differences across tracts with tract-level fixed effects,$\alpha_{state*year}$ 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 arbitray, 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	Transp & 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	486296	490126	488413	487324	485840	483641	491034	490184
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:

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

where α_i control for baseline differences across tracts with tract-level fixed effects,$\alpha_{state*year}$ controls for arbitrary trends at the state or county level with state or county-year pair fixed effects, and $\alpha_{low\ credit\ score*year}$ controls for arbitray, 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:

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

where α_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 arbitray, 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 Subsitution -- College Degrees

Variables	(1) Share Requires	(2) Share Requires BA	(3) Share Requires	(1) Log Experience	(2) Log Experience	(3) Log Experience
State Credit Ban $_t$	-0.00185	0.00711**		0.0364**	0.0420**	
	(0.00261)	(0.00329)		(0.0155)	(0.0199)	
Low Credit Score City $_i$ × 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:

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

where α_i control for baseline differences across cities with city-level fixed effects,$\alpha_{\text{state*year}}$ controls for arbitrary trends at the state o level with state-year pair fixed effects, and $\alpha_{\text{low credit score*year}}$ controls for arbitray, 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)	(3)	(4)	(5)	(6)
	Unemployed			Unemployed		
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:

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

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