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

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Abstract:
In the past decade, most states have banned or 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 data, and the LEHD Origin-Destination Employment data, we show that these bans increased employment of residents in census tracts with the lowest average risk score. We do this using both employment outcomes and changes in worker commuting patterns. The largest gains occurred in higher-paying jobs and in the government sector. At the same time, using a large database of online job postings, we show that employers increased their demand 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 risk scores, for those under 22 years of age, 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), with industry surveys indicating use by approximately 47 percent of employers (Society for Human Resource Management 2012). This screening tool has come under fire, though, by politicians and community groups that claim it unfairly penalizes those who have suffered from unemployment, medical debt, family breakup, or "individual bad luck" (Traub 2016). Anti-screening activists further argue that there is no research linking credit history to job performance, credit reports often contain errors, and bankruptcy and other negative credit events are caused by outside events, not "over-consumption."[1] There is also concern that these checks might disproportionately affect minority applicants (Traub 2013). 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, 10 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 noted in a discussion letter, "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."[2]

Although employer credit checks are now pervasive and state and local bans on the use of credit information have become increasingly popular, until recently little research has been done to date on their economic impact.[3] 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

[1] https://www.eeoc.gov/eeoc/meetings/archive/5-16-07/klein.html#fn27. Referencing Elizabeth Warren, *The Over-Consumption Myth and Other Tales of Economics, Law, and Morality*, 82 WASH. U. L.Q. 1485, 1510 (2004)

[2] "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

[3] For examples of earlier research on the economic impact of pre-hiring credit checks see Bryan and Palmer (2012) and Weaver (2015).

five percent random sample that is representative of all individuals in the United States 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 Equifax Risk Score[4] (subsequently referred to as "risk 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), described in Section II below. By comparing outcomes across tracts—and within tracts, across employment destinations—we are able to measure the relative impact of these laws on low-risk score populations.

We find, robustly, that these bans raised employment in low-credit score census tracts. Our baseline specifications indicate that low-credit score tracts[5] (for example, those with an average credit score below 620) saw employment increase by roughly 3.7-7.5 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 sector were most affected by these bans, followed by those in transportation and warehousing, and in-home services. This pattern makes sense, as both compliance and previous use of risk score information in hiring are likely to have been high in the public sector and in highly regulated industries, such as transportation and information, which often provide employees access to secure facilities, goods, customers' residences, and private information. Employers failing to preform background checks in these industries could possibly be held legally liable for damage caused by the employee. Employment in construction and food services declined among residents of low-risk score tracts following these bans, as people shifted to better-paying jobs. As expected, employment in the financial sector, which is typically exempt from these bans, was unaffected by the introduction of these laws.

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[4] The Equifax Risk Score is a quantitative risk model designed to help predict the likelihood of a consumer becoming 90+ days delinquent within 24 months. Similar to credit score measures, the Equifax Risk Score for individuals ranges from 280—850. As with credit scores, a higher risk score indicates a lower probability of delinquency.

[5] Employers typically do not see an applicant's credit score per se when performing a background check, but rather a credit report that details the applicant's credit history and activity. We use the credit score as a handy summary of the information in this report and explore alternate measures as robustness checks

Although employment increased in the lowest-risk score tracts following a ban, we find that these increases were mirrored by relative employment declines in mid-to-low risk score tracts (for example, 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-risk 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 used data from the American Community Survey Integrated Public Use Micro Data. We compared 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 compared with the contemporaneous change for whites. Thus, it appears that the prohibition of credit screening and the increased emphasis on other signals may actually, relatively, *harm* minority applicants.

This paper contributes to an important empirical literature on signals in employer screening. Several studies (Bertrand and Mullainathan 2004, Kroft, Lange, and Notowidigdo 2013, Correll, Benard, and Paik 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 a 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) shows that employers who check criminal records are more likely to hire blacks, although 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.[6] Finally, Sasser Modestino, Shoag, and Ballance (2015a, 2015b) show, using similar job vacancy data from Burning Glass Technologies, that employer demands for signals like education and experience are sensitive to labor market conditions.

Very recently, there have been several papers exploring the relationship between the existence of credit score information and employment generally. Bos, Breza and Liberman (2016) look at the effect of a Swedish law that removed negative credit information from some reports and found significant employment increases. Conversely, Dobbie et al. (2016) and Herkenhoff, Phillips, and Cohen-Cohen (2016) find that removal of bankruptcy flags in credit reports – which occur after seven years for Chapter 13 filers and ten years for Chapter 7 filers – has small employment effects. This is perhaps not surprising, given the relatively minor impact of the flag's removal on credit scores themselves (approximately 10 to 15 points on average) documented by Dobbie et al. (2016). These studies explore the impact of the existence of this specific type of older credit information for all context (including borrowing), not just its availability to employers.[7]

Additionally, there are a handful of contemporaneous papers that look employer credit check bans in standard microdata series. Fredberg, Hynes, and Pattison (2016) use the SIPP to look at the impact of employer credit check bans for those who report having difficulty paying their bills. They find increases in the job finding hazard, though they detect no impact on minority groups (perhaps due to sample size issues). Bartik and Nelson (2016) use CPS data and find that

[6] 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, Rosinger, and Hearn (2014) shows that schools with optional SAT submission policies are less diverse than other schools.

[7] We explore the possibility that bankruptcy information might differ from other information in the credit report in appendix Tables A.1 and A.2. We find bankruptcy measures to be less correlated with credit scores and measures of derogatory marks and past due accounts than those measures are with each other. Further, bankruptcy based measures generally do not generate the same positive employment response to employer credit check bans. It is possible that this type of information is treated differently by employers. Accordingly, we focus on measures based on credit scores, accounts with derogatory marks, and past due accounts.

bans are associated with reduced job finding rates for Blacks using a difference-in-differences approach.

Relative to this literature, our paper makes three central contributions. First, it uses a rich and novel data set allows us to provide a highly robust and cleanly identified estimate of the impact of an economically important screening ban on employment outcomes. The identification does not rely on a single aspect of one's credit report, such as an older bankruptcy flag, or on indirect proxies such as race. 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. This analysis provides some of the first real world evidence of this phenomenon.[8] Lastly, the paper provides an empirical framework for convincingly identifying the impact of state and local labor laws that target attributes that cannot be easily linked at the individual level, like risk scores. Many labor market laws—like those dealing with undocumented workers or those dealing with mental health issues—fall into this category, 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. (2015) shows that credit score information is correlated with non-quantifiable signals of borrower 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, Brevoort, and Canner 2012, Board of Governors 2007).

The paper proceeds as follows. Section II 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 III

[8] An exception is Shoag and Veuger (2016), which uses a similar empirical framework to analyze the impact of laws preventing employments from asking about criminal records early in the employment process.

describes the central identification strategies and estimates the baseline relationship between credit bans and employment in low-risk score tracts. Section IV explores the impact of these bans on outcomes by industry and wage range. Using the Burning Glass data, Section V introduces estimates that assess the impact of bans on education and experience requirements. Section VI outlines our empirical approach for estimating minority outcomes following a ban, using data from the American Community Survey, and Section VI concludes.

II. Data and Theoretical Framework

This paper uses five different datasets, described briefly immediately below. These are the following: Equifax Employer Credit Checks data, the Federal Reserve Bank of New York Consumer Credit Panel/Equifax (CCP), the LEHD Origin-Destination Employment Statistics (LODES), Burning Glass Technologies Labor/Insight Data (BGT), and data from the National Conference of State Legislatures. Additionally, although 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

In order for employers to obtain a credit file for a job applicant, they must request such information from a credit bureau. The inquiries remain on a credit bureau file for up to two years as “soft” inquiries, meaning that they do not impact the credit or risk score of the applicant. Equifax, one of the major credit bureaus in the United States, handles requests from employers for prospective employees’ credit profiles. Equifax provided us the total number of employer credit checks listed on their 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 their files as of November of each respective year and not the total number of credit files with inquiries, as a credit file with multiple employer credit inquiries is counted multiple times. Additionally, as one of the three major credit bureaus, Equifax has information only on employers that used Equifax services for such inquiries and does not know when or how often other credit bureaus were 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 credit checks performed by Equifax.

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

The CCP provides detailed quarterly data on a panel of U.S. consumers from 1999 through the present. The unique sampling design used to obtain these data provides a random, nationally representative 5 percent sample of U.S. consumers who have both a credit report and social security number, as well as the members of their households. 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), these data include credit card debt and auto and student loans. The panel also offers the opportunity to gain new insights into the extent and nature of the heterogeneity of debt and delinquencies across individuals and households (see Lee and Van der Klaauw 2010 for further information).

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.[9]

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

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.[10] 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 Quarterly Workforce Indicators, namely, multiplying job counts by randomly generated "fuzz factors" specific to each employer and establishment.[11] This coarsening of the residence information 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, Kutzbach, and McKenzie (2014). This extra noise is intentionally random and is 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 more than seven million current online job openings that are updated daily from over 40,000 sources, including job boards, newspapers, government agencies, and employer sites.[12] 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 parse the text of each job ad into usable data elements. BGT mines over 70 job characteristics from free-text job postings, including employer name, location, job title, occupation, number of years of experience requested, and level of

[10] The Quarterly Census of Employment and Wages (QCEW) is a cooperative program involving the Bureau of Labor Statistics of the U.S. Department of Labor and the State Employment Security Agencies. The QCEW publishes a quarterly count of employment and wages reported by employers; the count covers 98 percent of U.S. jobs and is available at the county, MSA, state, and national levels by industry.

[11] The Quarterly Workforce Indicators are generated from federal and state administrative data on employers and employees combined with core U.S. Census Bureau censuses and surveys to produce a rich, quarterly dataset that tracks employment, hires, separations, job creation and destruction, and wages for stable employees and new hires. The Census Bureau draws a random fuzz factor from each establishment to produce random noise. http://lehd.ces.census.gov/doc/technical_paper/tp-2006-02.pdf

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

education required or preferred by the employer. These data allow 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, "web crawling" 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 and on 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 on individual employer sites, as the latter are updated less frequently. We access selected underlying job postings to validate many of the important elements 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

The National Conference on State Legislature (NCSL) has been collecting data on state initiatives regarding credit checks in employment screening. We collected these data from their website and through discussions with Heather Morton, a program principal at the NCSL, and state agencies. Figure 2 maps the location by status of U.S. state laws and selected city ordinances in place as of this writing, and Table 1 reports the years when the existing laws were enacted. These laws barred most employers from using credit checks in the hiring process, with the exception of financial institutions which were exempted from the laws in 9 out 10 states.[13]

Table 2 shows summary statistics for data from all of the above sources. By combining these datasets, we can estimate the baseline employment impact of these laws. We describe our estimation procedure in Section III.

[13] In Table A2 we provided additional details on the credit ban law including the date of passage, date implemented, and industry and occupational exemptions.

Theoretical Framework

Employers' hiring decisions can be thought of as a screening problem, as in Aigner and Cain (1977) and Autor and Scarborough (2008). Because our finding that eliminating employer credit checks produces relatively worse outcomes for vulnerable groups may seem counterintuitive to some, we present a brief discussion of these authors' models to motivate the empirical analysis and results. A similar discussion can be found in Bartik and Nelson (2016). Therefore, we briefly outline below how the elimination of a credit score signal to employers could redistribute hiring decisions involving selection between candidates who belong to one or the other of two different groups away from the group with the lower average score.

To see this, suppose that workers come from two identifiable demographic groups x_1 and x_2, and that employers seek 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 μ_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, where y is normally distributed with mean-zero noise and standard deviation γ. Note that, as an unbiased signal, the average risk score of individuals in group 2 will be below the average score of those in group 1.

Employers' expectation of any individual's quality is a weighted sum of the individual's risk score y and his prior mean μ_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 risk scores $\left(y_i < \frac{\sigma^2+\gamma^2}{\sigma^2}k - \gamma^2\mu_1\right)$ are now hired, whereas individuals with good risk 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. With this theoretical 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 the Use of 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 are limited in that they represent only a small fraction of total employment-related credit checks. Nevertheless, we can use variation in the number of credit checks in ban and non-ban states over time to identify whether or not this type of state legislation induces a meaningful change in this segment of the market.

To test this, we first scale the total number of credit checks by (1) the number of unemployed residents and (2) the total number of 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 the number of employer credit checks. The magnitudes imply a roughly 7–11 percent reduction in the total number of credit checks. The reduction is statistically significant when clustering by state and does not appear to be driven by differences in prior trends, as Figure 3 shows. It is somewhat surprising that the measured decline is not larger, given that this behavior is now legally restricted. This may be partly attributable to the extremely noisy data on credit checks, data from a single and possibly non-representative firm, and the fact that some industries are exempt. It is important to note that these percentage declines cannot be directly compared to the employment results we discuss below. This is due to both the noise and representativeness issues discussed above, as well as the fact that the checks are not evenly distributed across jobs and census tracts. Our baseline estimates will be measured as percentage changes for low average risk score tracts, and these percentage changes are measured relative to the entire state. Still, despite the limitations of the data, we observe a meaningful decline in the use of employer credit checks that is novel and unrelated to prior trends.[14]

Employment Effect: Across-Tract Identification

Next, we examine the impact of credit check bans using a difference-in-differences (triple diff) approach, comparing the evolution of employment for residents of low-risk score tracts in ban states with the evolution of similar tracts in non-ban states. This approach, which is illustrated in appendix Figure A1, 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 myriad ways.

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. Shocks that affect all tracts in a state-year are controlled for by state-year fixed effects. Shocks that affect all low average risk score tracts in a given year are controlled for by low average risk score-year fixed effects. The treatment effect measures the change for low average credit tracts in states that implement a ban relative to all these other changes.

This same identification approach is also used with county-year fixed effects in place of state-year fixed effects as a robustness check. This test controls for arbitrary changes that affect all tracts in a county-year the same way, and measures the treatment effect relative to these controls.

The following paragraphs discuss in more detail how we operationalize this approach. To produce easily interpretable estimates, we first classify tracts as high- and low-risk score tracts,

[14] Our detailed data allow us to pursue an identification strategy that abstracts from across-state variation. As a result, we cannot identify the impact of these bans on aggregate outcomes. When we do use a standard difference-in-differences approach, we find no increase in statewide employment as a result of these bans (Table A3). This is consistent with the view that these bans operate primarily by inducing substitution across workers. Cortés et al. (2016) explore this issue at the county level and find small effects in the opposite direction (slightly higher unemployment).

using a binary division. We do this in two ways to begin with, though we explore additional measures below.

Our first method of classifying tracts is by constructing the average risk score for each tract and quarter in the CCP. There are a number of small tracts in the dataset for which the CCP sample is too small to enable reliable average risk scores to be constructed. To manage this problem, we drop any tract for which the difference between the highest and lowest average risk score by quarter is 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 risk scores if the average risk score was below 620 (the conventional subprime line) in any quarter.[15]

Our second method, rather than using average scores, classifies tracts as having low risk scores based on the fraction of the sample below the 620 threshold, and high risk scores otherwise. To keep things similar to the analysis above, we aimed for a threshold that would mark roughly 10 percent of tracts as having low risk scores. Therefore, we pooled observations across quarters, and marked a tract as having low risk scores if more than 38 percent of the individuals residing in that tract had scores below the line. To address the issue of sparsely populated tracts in this approach, we dropped any tract with a total sample below 50 inquiries. We show our results for both classification methods and explore several other measures in subsequent tables.

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

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

where i and t index tract and year. The first term α_i represents fixed effects for each tract. The second term $\alpha_{state \times t}$ represents state-year pair dummies and controls for arbitrary employment trends at the state level. The third term $\alpha_{low\ credit \times div \times t}$ is a year dummy multiplied by the low-risk score dummy multiplied by dummies for Census divisions to control for arbitrary employment trend differences between low- and high-risk score tracts across regions of the

[15] The tract level credit report measures are highly correlated over time. We demonstrate this in Table A4. For that reason, we use time invariant tract classifications. Unsurprisingly, the results are robust to relaxing that assumption as well.

country. The final coefficient of interest β measures how low-risk score tracts in states with credit check bans fare relative to low-risk score tracts in other states and relative to arbitrary within-state trends.

Our results are reported in Table 4. In Columns (1) and (4), we find that low-risk score tracts experienced 6.6-7.5 percent greater employment post-ban than the control group. The results are statistically significant, even when clustering the standard errors at the state level to control for arbitrary serial correlation and spatial correlation across tracts.[17] 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 controls 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-risk score tracts, taking into account any prior trends in specific state-level low-risk score 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 nonlinear 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 the creation of roughly 70 additional jobs per year in tracts with low risk 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 substituted 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 found no differential trends, relative to controls, before a ban's implementation. Afterward, however, there was a large and persistent increase in employment in low-risk score tracts. The impact becomes statistically significant in the second year and

[17] The results remain highly statistically significant when clustering at sub-state levels as well.

remains significant until five years after the ban, at which point we have little identifying variation.

We tested the robustness of this finding in several ways. First, we tested whether the introduction of credit checks was correlated with changes in other labor market laws using a difference-in-difference approach with state and year fixed effects. We found no significant relationship between changes in state-level minimum wages and "ban-the-box" legislation and the credit check bans. Next, we estimated the impact of credit check bans dropping, one at a time, each state with a ban on the use of credit information. In all of these regressions, we recovered a large, positive result within statistical range of our baseline estimates.[18] Similarly, we performed a placebo test in which we randomly assigned start dates for low credit check bans to non-ban states with the same timing and likelihood of the real bans, while coding the placebo laws in states with real bans as zero. We then re-estimated our regressions on 50 of these placebos. Our point estimate using the true laws exceeds all but 6-8 percent of these placebo point estimates.[19] Finally, using data from the American Community Survey summary files, we also explored the possibility that these findings reflect migration across tracts. We found no significant effect of credit check bans on population growth in low-risk score tracts, either within states or within counties, and the point estimates in both cases are close to zero (see Table A5).

In Table 5, we further test the robustness of our result to potential confounding using the Coarsened Exact Matching (CEM) procedure described in Iacus, King, and Porro (2008). This procedure uses the Sturgis binning algorithm to create exact matches between treated and non-treated tracts using a group of pre-period tract characteristic. Table 5 reports the result using seven different groups of match variables, which include tract demographics, education rates, industry shares, initial risk score profiles, and wage bins. These estimates, which use only tracts

[18] A previous draft controlled for low credit-year fixed effects in lieu of Census division-low credit- year fixed effects. The results were highly similar for all tests and are available on request.

[19] To create our placebo dataset, we dropped outliers in employment. We then probabilistically assign start dates for credit check bans across non-ban states using the probability of the realized bans. We then estimate the impact of the true ban on this same dataset.

similar to the treatment tracts on all dimensions, generate employment effects similar to those found in the baseline specifications in Table 4. The results range from 3.6 percent to 12.5 percent and are statistically significant in six of seven cases. Therefore, we conclude that our results are unlikely to be driven by differences in pre-trends.

Alternate Measures of Tract-Level Credit Reports

Risk scores are one way of summarizing the information in a credit report, and our proportion and average measure is just one way of classifying tracts where many residents have poor reports. Since employers may not see an applicants' exact score, it is worthwhile to test whether the results look similar using other, direct information from these credit reports. In Table 6, we construct measures from the CCP on the average number of revolving accounts with a major derogatory mark, the share of people with at least one major derogatory revolving account, the share of people with more than $1,000 past due on a revolving account, the share of people with major derogatory marks on more than 10% of revolving accounts, and the share of people with more than 50% of revolving debt past due. Again, we create a dummy for these measures signifying low credit following the procedure we used for risk scores (i.e. taking the bottom 10% of the distribution). We then use these credit measures in lieu of the risk score based measure in our baseline regression in Table 6. Again, we find similar results to the baseline results in Table 4. Tracts at the low of the distribution, along each of these measures, have employment gains following the ban of comparable magnitude to our baseline estimates. [20]

Employment Effect: Within-Tract Identification

While the above results present a compelling case for the impact of these bans, the LODES employment data are extremely rich and include information about employment by both place of residence and 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-risk score tracts to destinations with and without a ban.

[20] This makes sense given the high correlation between these measures, as demonstrated in Table A1.

In the paper's second (quadruple diff) identification approach, visualized in Figure A2, we consider the evolution of employment for residents of tracts with high average risk scores and low average risk scores, in destination states that eventually implement a ban and status quo states that do not. 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. Shocks that affect employment at the destination state from all residence tracts are controlled for by destination-year fixed effects. Shocks that affect employment in a residence tract in all destination states are controlled for by tract-year fixed effects. The treatment effect measures the change for residents of low average risk score tracts in destination states that implement a ban relative to all of these other changes.

To understand this identification, consider two Massachusetts tracts A and B (which never enact a ban) on the border of Connecticut (which does). Suppose tract A has low average risk scores and tract B does not. In this section, we propose to test whether the share of residents from town A who commute to Connecticut increases by more than the share of residents from town B, holding constant overall employment trends in A and B. In the previous section, we identified increases off total employment gains. Here we hold those gains constant, and estimate the impact from changes in commuting shares.

To operationalize this approach, we denote d as the destination state of employment and o as the origin or place of residence, and we estimate the following equation:

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

The fixed effects 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 in that tract by the residents of tracts with low average risk scores. We represent this identification assumption graphically in Figure A.2.

We report the results for all origin-destination pairs with more than five workers in Table 7. We do this both for the entire sample and for the sample of origin tracts located *outside* of states that have a credit ban, which indicates cross-border commuting. In both specifications we find large increases in employment for low-risk score tracts. We report the results for both our risk score measures and for measures based on direct information, such as the share of people with at least one major derogatory account. These increases are measured relative to within-tract outcomes and relative to general trends in employment in destinations with a credit ban.[21] The baseline impact across these specifications is roughly 3.8-5.8 percent within tract residence-destination state pairs, and a roughly 4.5-7.6 percent increase in cross-border commuting pairs. The base for these estimates is obviously smaller, and the implied employment gains from these larger percentages (767 and 752 jobs, respectively) are sensibly lower as a result. Again, this is evidence that the credit check bans are impacting the distribution of employment even within tract-years. We believe it is difficult to conjecture a defensible omitted-variable-bias explanation for these results.

Threats to Identification

When using a differences-in-differences-style identification strategy, one needs to be concerned about pre-existing or contemporaneous trends that might bias the estimates.

For example, one might be concerned that credit check bans were enacted in states with growing employment or in states where employment was growing disproportionately in low average risk score neighborhoods. We address this concern in numerous ways. First, we explicitly check for pre-trends in our baseline specification in Figure 5 and find none. Second,

[21] To understand the identification, we label a mixed-destination tract as a tract in whose residents work in more than one state. Our identification comes from employment trends across destinations in these mixed-destination tracts. Roughly 18.8% of employment in 2013 occurred in mixed-destination tracts in a destination with a credit-check ban. That includes tracts that are in states that themselves had a ban, provided some tract residents worked out of state. The second column in Table 5, which limits our analysis only to those states without bans, the share of employment working in ban states, is (intuitively) significantly smaller. In that regression, only 0.7% of total employment occurs in mixed-destination tracts with ban state destinations.

we include county-year fixed effects in Table 4, controlling for arbitrary differences in trends across counties. This allows us to identify off differences across tracts within a county. Third, we run tests that include state-specific linear trends for low average risk score neighborhoods and low average risk score neighborhood by census division by year fixed effects. These controls enable us to identify the impact of the ban off changes for low average risk score tracts relative to their own trends within the county and relative to trends for geographically close low risk score neighborhoods in other states. We find similar impacts of these bans when progressively adding all of these controls, which suggests that these types of biases did not have a large effect on our initial estimate. Finally, we find the same impact when using Coarsened Exact Matching to balance the sample along multiple, detailed dimensions (including the initial credit measures).[22] Each of these specifications and tests give similar results. Moreover, the similarity of these results to our baseline estimates signifies that 'double counting' via reductions in control tracts has a quantitatively small impact on our baseline estimates.[23]

What threats remain after these tests? Our test would remain biased if credit check bans were enacted in states experiencing a break in the relative employment of their low risk score neighborhoods relative to prior trends for those tracts. For example, suppose Connecticut enacted a credit check ban right as its low risk score neighborhoods grew over and above prior trends for those neighborhoods and trends for low risk score neighborhoods elsewhere in New England. If this correlation were not confined to Connecticut, but was systematic across all states implementing a ban, it would bias our estimates. Table 7 introduces a test that is robust to this possibility. Rather than identify the impact off differences in total employment outcomes for a tract, it identifies off differences in *commuting patterns*. We now explore whether residents of low average risk score tracts are more likely than residents of other tracts to commute to work in destinations with credit check bans, holding constant their overall employment

[22] Several of these estimates matched based on initial credit scores, effectively rendering the comparison as one between similar low average credit score tracts subject and not subject to the ban. This is analogous to a simple difference-in-difference estimate for low credit score tracts across ban and non-ban states.
[23] This makes sense given that tracts designated as low credit account for a small share of total employment.

outcomes. Once again, we find an impact of credit check bans on these outcomes. To relate this to the previous example, we now find that residents of low risk score tracts in Massachusetts have become more likely to commute to Connecticut, even controlling for the total number of employed people in those tracts. Thus, any omitted-variable bias story needs to account for both the increase in employment in low risk score tracts in Connecticut and the change in commuting patterns.

Now, it is impossible to rule out the potential for a complicated alternative counterfactual. Still, it is clear that straightforward bias stories about different cyclical trends or growth rates cannot explain these results. We believe that articulating an explanation that accounts for all of our findings in credit check bans do not have the effect claim they have is sufficiently difficult that, per Occam's razor, the best explanation is that we are indeed measuring the impact of these policies.

IV. Mechanism

The LODES employment data are rich, not just in their geographic detail, but also in that they break out employment by wage ranges and industry shares. These data are available for more categories and are better populated when one focuses 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 A1.

Across Wage Ranges

In Table 8, we break out our results by showing the impact on employment by LODES wage range. 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 per year, and an even larger percentage increase in jobs paying more than $40,000 per year. These results indicate that employer credit checks primarily kept workers out of "better" jobs, rather than the lowest wage ranges. Of course, the total number of jobs in this bin is smaller in these low average risk score tracts, so the larger percentage gains multiplies a much smaller base.

Across Industries

We show the impact of these credit check bans by industry in Tables 9a and 9b. This breakout presents an important sensitivity test of our results: the reliance on credit checks varies considerably across industries, and some industries were exempt 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 conforms strongly to these patterns. In Columns (1) and (2) of Table 9a, 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 for Human Resource Management 2012), and both sectors are—obviously—expected to comply with these laws. [25]

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[26] and that otherwise-qualified employees are often rejected based on these checks.[27] This industry is closely followed by real estate and information (for example, 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 for Human Resource Management (2012). Finally, the last three columns of Table 9a show the three industries with the next greatest impact—retail and health care, which involve handling clients' financial information, an establishment's cash, or access to vulnerable clients and secure facilities.

[25] Press accounts also describe the widespread use of credit credits by the federal government and some local government agencies. "*When 'bad' credit stands in the way of a good job*" USA Today (2/12/09) by Thomas Frank (http://usatoday30.usatoday.com/news/washington/2009-02-12-creditcheckinside_N.htm)
[26] 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.
[27] "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

Table 9b presents an interesting reflection of the large effects observed above. While employment increased generally in low-risk score tracts, it actually decreased in some lower-wage industries like accommodations and food services and construction, which do not use credit checks intensely. Perhaps even more compelling is the fact, demonstrated in Columns (4) and (5) of this panel, that employment in finance and insurance and professional services is unaffected by these bans. As mentioned above, these industries are generally exempt from the law, and, correspondingly, employment in these industries does not change in low-risk score tracts.

Across the Risk score Distribution

As shown in the prior tables, we created dummies for low-risk score 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 (with 670 being a typical "good score" threshold), we tracked how employment evolved relative to this benchmark for groups of tracts, based on their average risk scores. The impact for each average-score range 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 risk scores to those whose scores register as subprime or below.[28] Our employment results reflect relative outcomes that include this substitution. Over 90 percent of employment is accounted for by the residents of fair or better average risk score tracts (above 620), however, meaning that the absolute gains in low credit tracts are comparable to these results. In the next section, we explore data that illustrate how this redistribution was effected.

[28] We also tested the impact of credit bans on total employment and found no effect on total. See Table A3.

V. Shifts to Other Signals

To study changes in employer demands for other signals following a credit ban, we turned to a new dataset on online job postings used in Sasser Modestino, Shoag, and Ballance (2015a, 2015b). For this project, we used data on roughly 74 million job postings from 2007 through 2013. The smallest geography recorded for each posting is the city level. We matched these city-level observations to tracts, using the U.S. Post Office city name database, using *preferred* place names. To make sure we had a usable sample, we restricted our analysis to cities with over 75 job postings per year.

We once again classified cities using a binary approach, creating a dummy if the average risk score profile fell below a cutoff of 620.[29] We then ran regressions at the city-year level in the spirit of equation (1), which controls for aggregate outcomes within state-years and for arbitrary trends for low-risk score areas. Our dependent variables are the share of jobs requiring a college degree, and average experience required (in years). These variables were 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 10, show that cities with lower risk scores experienced a greater increase in the share of jobs requiring these skills in states with a ban than in states without a ban. This is true even when conditioning on a variety of fixed effects to account for aggregate shocks to both low-risk score 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 three months of experience on average. This is about a 22 percent increase in the fraction of jobs in these low-risk score 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 is less clear, however, is how this shift from credit checks to increased

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

demand for education and experience affects labor market outcomes for minority and other vulnerable groups. Put simply, do these bans (relatively) help or harm the people they were supposed to target?

VI. Vulnerable Populations

Unlike risk scores, race and age can be linked to employment outcomes directly at the individual level. To answer this question, therefore, we turned to data from the American Community Survey (Ruggles et al. 2015). As before, we used a difference-in-differences strategy, comparing outcomes for different groups in ban and non-ban states before and after the enactment of the ban. The groups we focused 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 did not permit us to match to the refined geographies we would have needed to recover meaningful variation in average risk scores. Therefore, our results are for the entire group in a state with the ban.

We begin with a regression of the form:

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

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 blacks or young people perform, relative to others in the state post-ban compared with the relative performance of those groups in the average non-ban state and relative to their performance preceding the ban. 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 11. Columns (1–3) show that black employment rates, conditional on labor force participation, were roughly 1 percentage point lower post-ban than the unemployment rates of other groups in the same state-year. This result is quite robust across specifications and controls. Columns (4–6) show that young people saw a decrease in the employment rate of roughly half this size, although this effect loses significance when state-specific young adult trends are controlled for.[30]

To demonstrate the time series of this impact, as well as the absence of pre-trends, we again estimate an "event study" impact by year relative to the ban for Black unemployment. The individual year coefficients and confidence intervals are plotted in Figure 7.

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

This finding makes sense in light of the cross-sectional distribution of signals in the population. Though we cannot link risk scores and race directly, we can link this signal to the black population share at the tract level. In Figure 8, we plot the average share of the population with a college degree divided by the average share of the population with risk scores above 620 for bins of tracts by the black share of the population. The figure clearly shows a strong declining ratio. Tracts with more blacks have a lower ratio of college graduates relative to the population with fair or better risk scores. This further supports the finding that signal substitution may have disparate racial impact.

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

[30] We find similar effects for income, with a roughly 1–2 percent decline for both groups. We also find similar employment declines when not conditioning on labor force participation.

credit checks and with employment gains in low-risk score areas. These gains occur in mid-to-high-wage jobs, with the largest effect on public sector employment. These gains appear to coincide with employment losses in tracts with slightly higher risk 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 credit check bans. These findings also contribute to the literature on statistical discrimination, and in particular 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, Chugh, and Mullainathan 2005). Finally, the origin-destination identification framework outlined in this paper can be used to convincingly identify labor market laws that target attributes, like risk scores, which cannot be easily linked to individual labor market outcomes.

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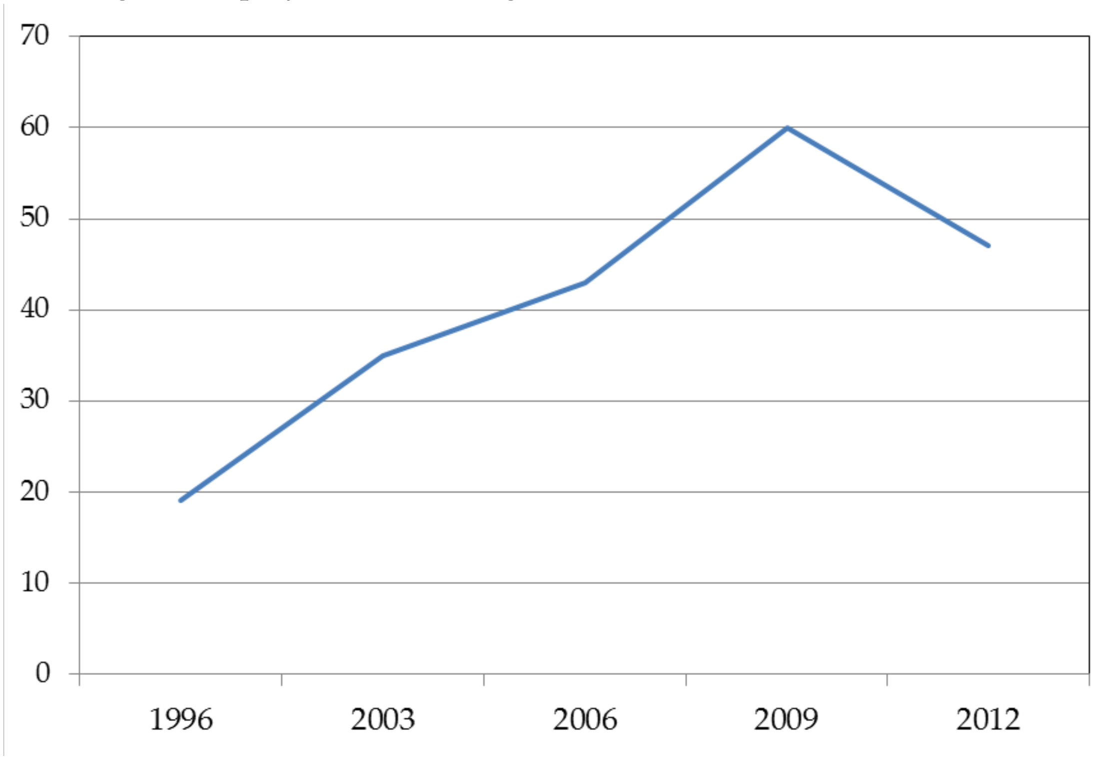
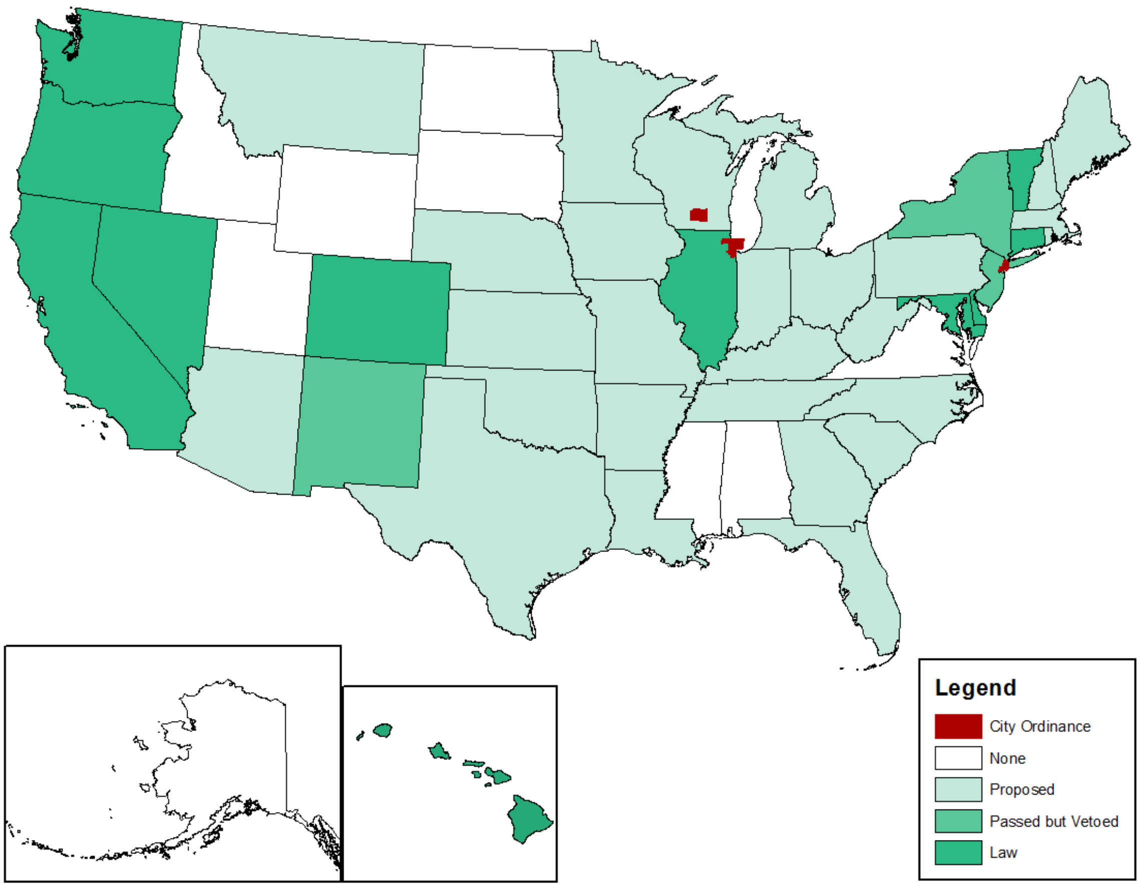
Figure 1: Use of Credit Checks by Employers, 1996–2012

Percentage of Employers Conducting Credit Checks



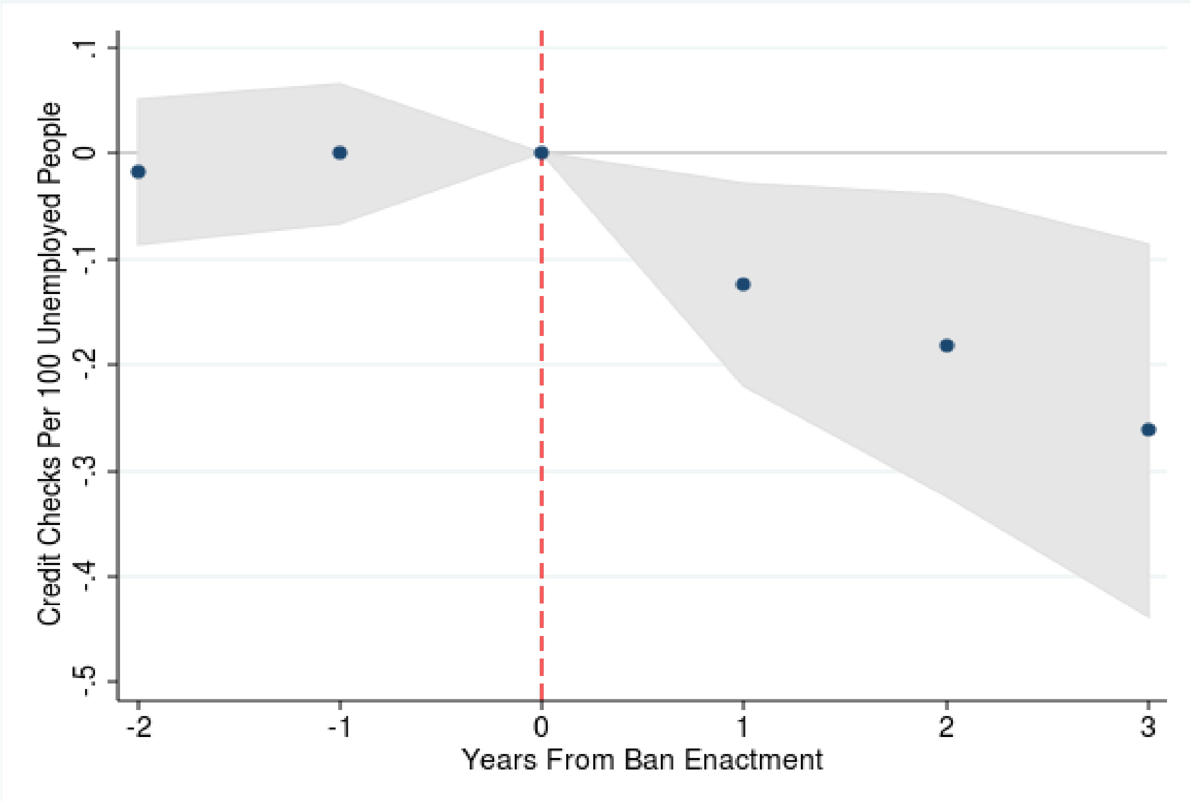
Source: Society for Human Resource Management, Survey of Hiring Managers, periodic Survey on the Use of Credit Checks in Hiring Decisions.

Figure 2: State Laws and City Ordinances Banning the Use of Credit Checks in Employment Screening, as of December 2015



Source: National Conference on State Legislatures.

Figure 3: Impact of Credit Check Ban on Employer Use of Credit Checks



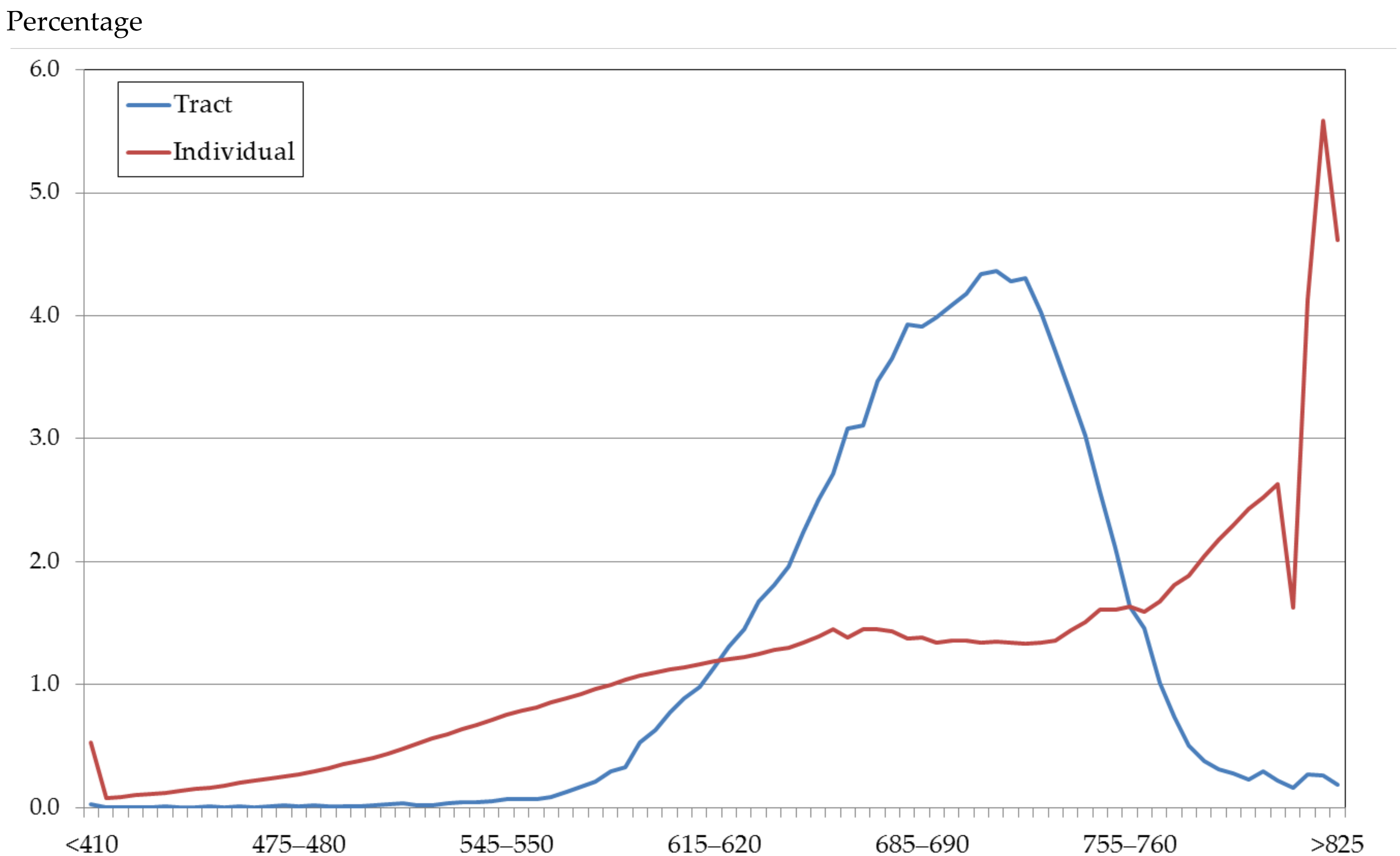
Source: Authors' calculations, based on Equifax data on employer credit checks.

Note: This figure reports the results of the regression:

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

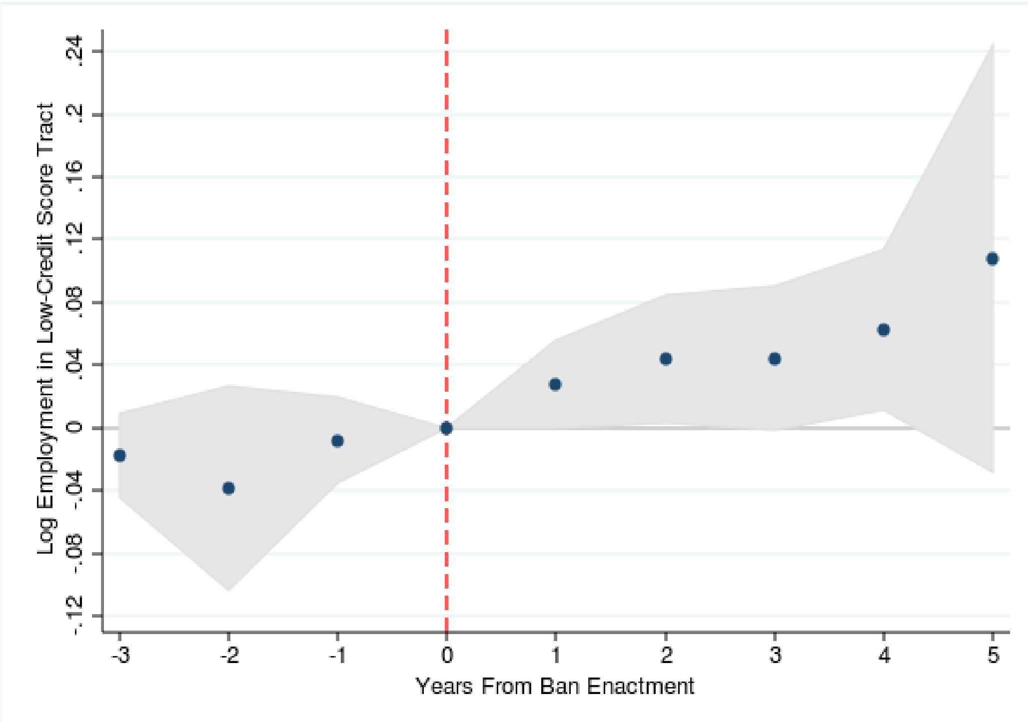
where s indexes state and t indexes year. Observations are state-year for 2009–2014. The graph shows the beta coefficients with confidence intervals. Standard errors are clustered by state.

Figure 4: Distribution of Tract Average Scores, Q4 2015



Source: FRBNY/Equifax CCP.

Figure 5: Impact of Credit Check Ban on Employment



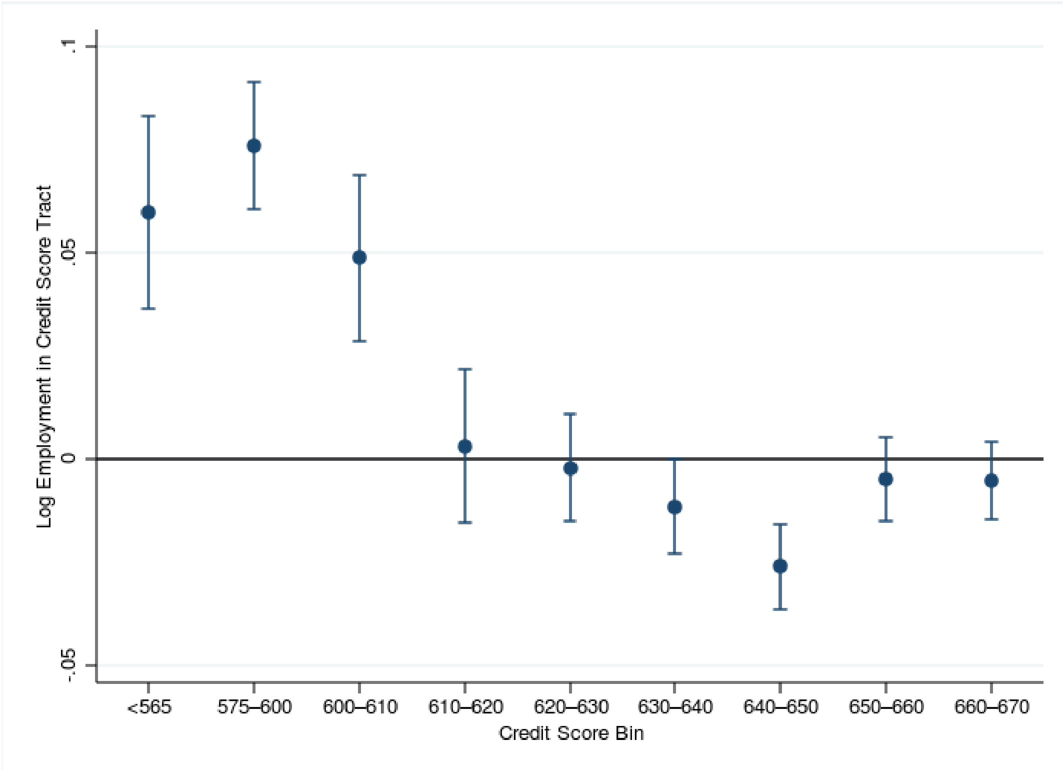
Source: Authors' calculations, based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel (FRBNY/Equifax CCP) and U.S. Census Bureau LEHD Origin-Destination Employment Statistics (LODES) data.

Note: This figure reports the results of the regression:

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

where α_i are tract-level fixed effects, $\alpha_{\text{low credit*div*t}}$ are low credit-census division-year pair fixed effects, and $\alpha_{\text{state*t}}$ are state-year pair fixed effects. Observations are tract-year for 2002–2013. The figure shows the beta coefficients and their confidence intervals. Standard errors are clustered by state.

Figure 6: Impact of Credit Check Ban on Employment by Average Risk Score Range



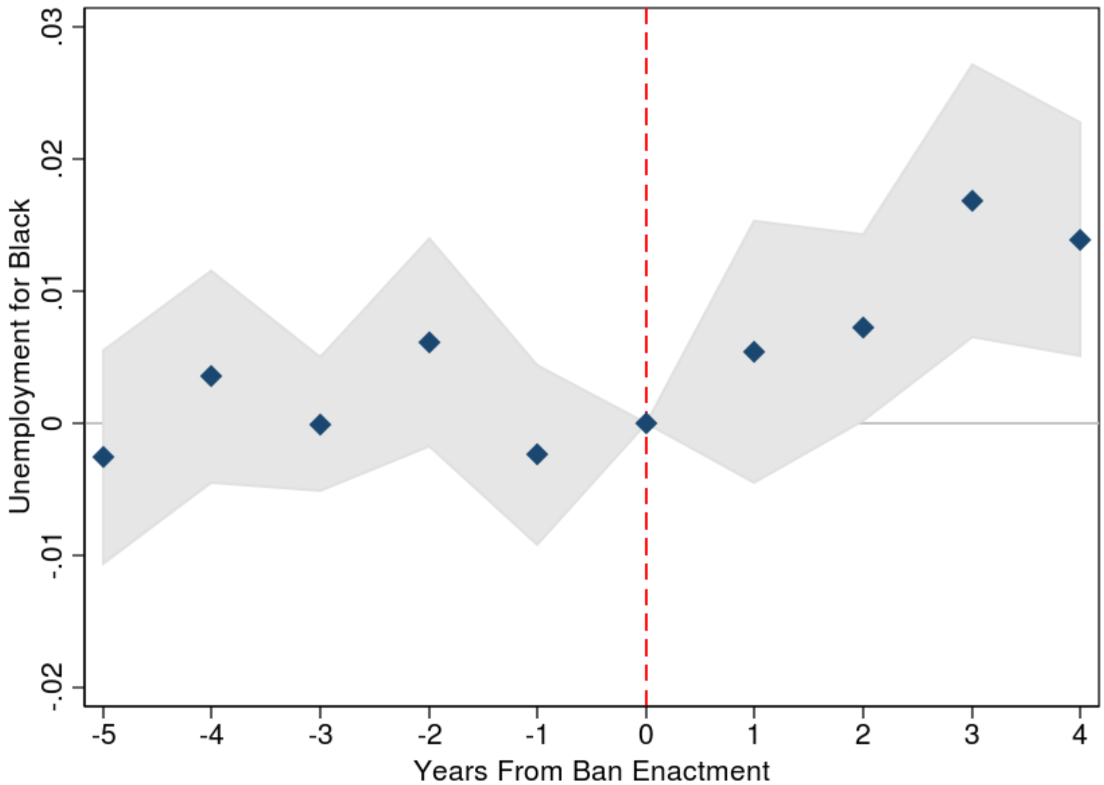
Source: Authors' calculations, based on FRBNY/Equifax CCP and LODES data.

Note: This figure reports the results of the regression:

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

where α_{i} are tract level fixed effects, $\alpha_{\text{low credit*div*t}}$ are low credit-census division-year pair fixed effects, and $\alpha_{\text{state*t}}$ are state-year pair fixed effects. Observations are tract-year for 2002–2013. The figure shows the beta coefficients, which measure the relative impact of the ban in tracts with these scores, compared with the benchmark response of tracts with average scores above 670.

Figure 7: Impact of Credit Check Ban on Unemployment for Blacks

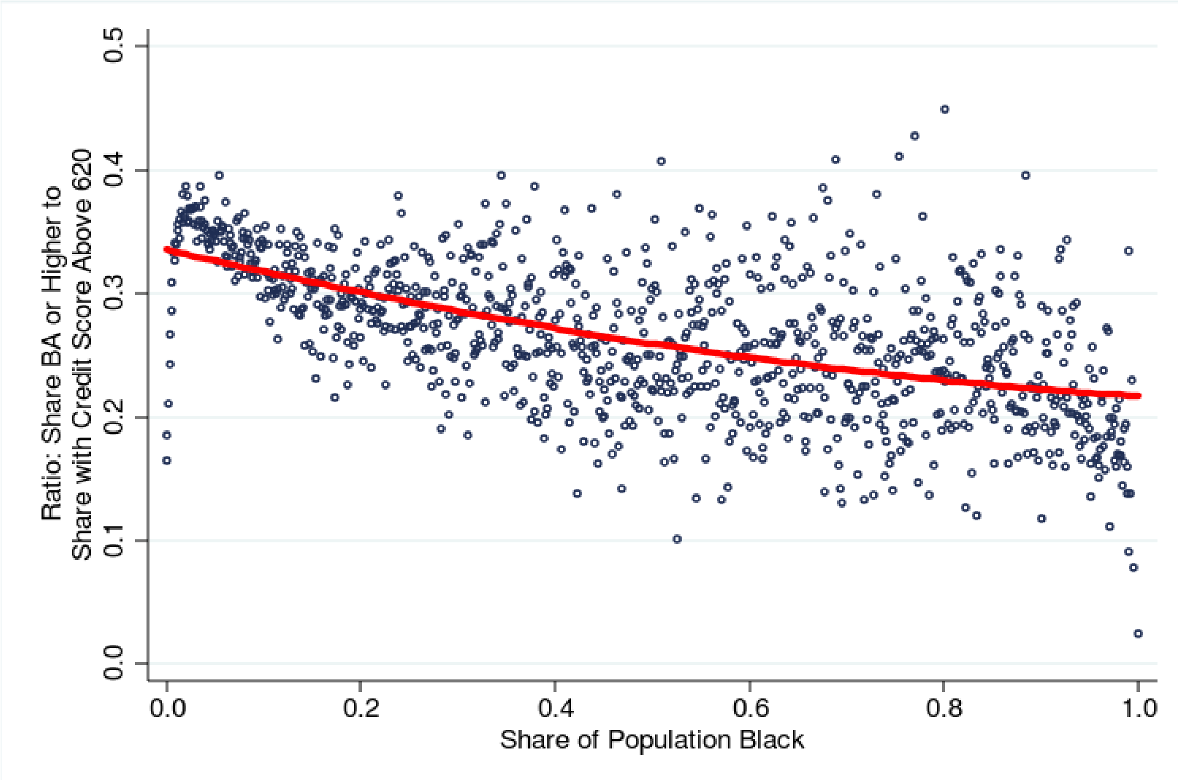


Note: This figure reports the results of the regression:

$$\text{Unemploy}ed_{i,t} = \alpha_{race\times state} + \alpha_{race\times t} + \alpha_{state\times t} + \beta_t \times race \times \text{Years to Ban}_{s,t} + \varepsilon_{i,t},$$

where $\alpha_{\text{racexstate}}$ are race-state pair fixed effects, α_{racext} are race year pair fixed effects, and $\alpha_{\text{state*t}}$ are state-year pair fixed effects. Observations are from the ACS from 2005–2014 and are at the individual level. The figure shows the beta coefficients, which measure the relative impact on unemployment (a dummy at the individual level) by year relative to ban. Standard errors are clustered by state.

Figure 8: Proportion of Share College to Share with Risk Score Above 620 by Share Black



Source: Authors' calculations, based on FRBNY/Equifax CCP and US Census Bureau 2010 Decennial Census Summary Files.

Note: This figure calculates the fraction of individuals with a BA or higher divided by the fraction of individuals in the Equifax data with a risk score about 620 for census tracts in 2010. This ratio is plotted against the share of the census tract that is black in the Decennial Census Summary Files. The figure shows that the ratio of college educated people to people with non-poor credit is declining in the share of the population that is black.

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 as of December 2015	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

Source: Authors' analysis of information from the National Conference of State Legislators and respective laws 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 Risk Score	675.7	44.0	531.3	784.4	591,087
Fraction with Risk Score 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 Risk Score	682	34.54	544.5	816	27,106
State-Year Level					
Employer Credit Check Per 100 Hires	0.165	0.073	0.034	0.494	238
Employer Credit Check Per 100 Unemployed	1.268	0.648	0.303	3.746	244

Source: Authors' calculations based on data from the LODES, Equifax, FRBNY/Equifax CCP, and Burning Glass Technologies.

Table 3: Impact of Credit Check Ban on Employer Use of Credit Check

VARIABLES	(1) Checks per 100 Unemployed $_{it}$	(2) Checks per 100 Hires $_{it}$
State Credit Ban $_{it}$	-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

Source: Authors' calculations, based on employer credit check data from Equifax and hires data from Quarterly Workforce Indicators.

Note: The hires data exclude Massachusetts. Observations are state-year for 2009–2014. Standard errors are clustered by state. 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: Impact of Credit Check Ban on Low-Risk Score Tract Employment

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-Risk Score Tract i ×						
State Credit Ban t	0.0656**	0.0405***	0.0562**			
	(0.0269)	(0.0136)	(0.0237)			
Proportion Measure						
Low-Risk Score Tract i ×						
State Credit Ban t				0.0754***	0.0373**	0.0501**
				(0.0243)	(0.0181)	(0.0249)
Controls						
Census Division x Low-Risk Score Tract × Year FE	X	X	X	X	X	X
State × Year Fixed Effects	X	X		X	X	
County × Year Fixed Effects			X			X
State Low-Credit Tract Trends		X			X	
Observations	589,202	589,202	586,168	618,398	618,398	615,272
Dependent Variable Means (Levels)	1774.874	1774.874	1778.258	1730.827	1730.827	1734.475
Dependent Variable Means For Low Credit Tracts (Levels	1361.736	1361.736	1362.323	1212.333	1212.333	1215.565
R-squared	0.96	0.96	0.97	0.950	0.951	0.968

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division low-credit tract trends. Regressions reported in columns (2) and (5) also control for separate linear time trends in employment for low- and higher-Risk Score tracts by state. Observations are tract-year for 2002–2013. Standard errors are clustered by state. The low-risk 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 exceeding 38 percent.

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

Table 5: Impact of Credit Check Ban on Low-Risk Score Tract Employment Using Matching Estimator

VARIABLES	(1) Log Employment $_{it}$	(2) Log Employment $_{it}$	(3) Log Employment $_{it}$	(4) Log Employment $_{it}$	(5) Log Employment $_{it}$	(6) Log Employment $_{it}$	(7) Log Employment $_{it}$
Tract Matching Criteria	Tract Emp.; Risk Score; Black Share; College Share	Black Share; Latino Share; College Share;	Service Emp. Share; Manufacturing Share; High Wage Share	Service Emp. Share; Traded Emp. Share; Low Wage Share	Tract Emp.; Tract Riskscore; Black Share; College Share; Traded Share	Tract Emp.; Tract Riskscore; Black Share; College Share; Good Producing Emp. Share	Tract Emp.; Tract Riskscore; Black Share; Latino Share; Service Producing Emp. Share
Average Score Measure							
Low-Risk Score Tract i × State Credit Ban t	0.0894***	0.0498*	0.0772***	0.0737**	0.125***	0.0894***	0.0355
	(0.0218)	(0.0274)	(0.0271)	(0.0309)	(0.0307)	(0.0218)	(0.0397)
Controls							
Census Division × Low-Risk Score Tract × Year Fixed Effects	X	X	X	X	X	X	X
State × Year Fixed Effects	X	X	X	X	X	X	X
Observations	74,436	100,348	275,920	376,369	31,690	74,436	24,614
Dependent Variable Means (Levels)	1,350	1,524	1,668	1,747	1,316	1,350	1,259
Dependent Variable Means For Low Credit Tracts (Levels)	1,242	1,323	1,378	1,386	1,227	1,242	1,212
R-squared	0.951	0.958	0.956	0.958	0.950	0.951	0.954

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table uses the Coarsened Exact Matching method described in Iacus, King, and Porro (2008) to match treated and non-treated tracts using a group of pre-period tract characteristics using the Sturges binning algoritm. Demographic variables (black share, hispanic share, and college share) are from the 2000 Census; Tract Riskscore measure is the average score by tract in the 2006 Equifax CCP; tract employment, industry shares, and employment shares by wage bin are constructed using the 2006 LODES data.

We report regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division low-credit tract trends. Observations are tract-year for 2002–2013. Standard errors are clustered by state.

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

Table 6: Impact of Credit Check Ban on Low-Risk Score Tract Employment Using Alternate Measures of Low Credit

VARIABLES	(1) Log Employment $_{it}$	(2) Log Employment $_{it}$	(3) Log Employment $_{it}$	(4) Log Employment $_{it}$	(5) Log Employment $_{it}$
Alternative Low Credit Measure × State Credit Ban t					
Avg. Number Revolving Accounts with Major Derog Mark >24 months ago	0.0519***				
	(0.0181)				
Share of Persons with at least 1 Major Derogatory Revolving Account		0.0606***			
		(0.0192)			
Share of Persons with >$1000 past due revolving account balance			0.0572**		
			(0.0257)		
Share of Persons with Major Derog Mark on >10% of Revolving Accounts				0.0597***	
				(0.0149)	
Share of Persons with >50% of Revolving Debt Past Due (w/Update w/in last 3 months)					0.0296
					(0.0191)
Controls					
Census Division × Low-Risk Score Tract × Year Fixed Effects	X	X	X	X	X
State × Year Fixed Effects	X	X	X	X	X
Observations	559,632	559,636	559,682	559,632	559,632
Dependent Variable Means (Levels)	1,812	1,816	1,813	1,812	1,812
Dependent Variable Means For Low Credit Tracts (Levels)	1,490	1,503	1,509	1,490	1,490
R-squared	0.950	0.951	0.950	0.950	0.950

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division low-credit tract trends. Observations are tract-year for 2002–2013. Standard errors are clustered by state.

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

Alternative measures of low credit are constructed using the Equifax CCP. We use the maximum avergage value for each measure by tract and quarter over the period 2002Q1 to 2015Q2 to identify a census tract as "low credit." Our low credit measures include the average number of revolving accounts with an *old* major derogatory mark (i.e. one which occurred at least 24 months ago); the share of periods within a tract with at least one major derogatory mark on a revolving account; the share of persons with >$1000 past due for all revolving accounts; the share of persons with a major derogatory mark on at least 10% of all revolving accounts; and the share of persons whose current (i.e. with an update in the past three months) revolving debat balance is over 50% past due. We identify tracts with shares in the 90th or greater percentile for these measures as low credit, which resuts in roughly the same number of low-credit tracts as our risk score measures.

Table 7: Impact of Destination State Credit Check Ban on Origin-Destination Employment

VARIABLES	(1) Log Employment $_{it}$	(2) Log Employment $_{it}$
Average Score Measure		
Low-Risk Score Origin Tract i × Destination State Ban t	0.0382**	0.0484***
	(0.0168)	(0.0169)
Proportion Credit Measure		
Low-Risk Score Origin Tract i × Destination State Ban t	0.00374	0.0451***
	(0.0163)	(0.0164)
Alternate Low Credit Measures		
Avg. Number Revolving Accounts with Major Derog Mark >24 months ago × Destination State Ban t	0.0582**	0.0763***
	(0.0246)	(0.0263)
Share of Persons with at least 1 Major Derogatory Revolving Account × Destination State Ban t	0.0474**	0.0547***
	(0.0200)	(0.0210)
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 >10	
	All States	Origin States w/o Ban
Observations	579,818	485,753
Dependent Variable Means (Levels)	767	752
Dependent Variable Means For Low Credit Tracts (Levels)	167	169
R-squared	0.994	0.994

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

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 α_{od} controls for baseline differences across tract-destination pairs with tract-destination-level fixed effects, $\alpha_{d\times t}$ controls for arbitrary trends at the destination level with destination-year fixed effects, and $\alpha_{o\times t}$ 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 of cross-border commuting. Because the means 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 year for 2002–2013, and we cluster at the tract level. The low-risk 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 exceeding 38 percent. Additionally, we use two alternative measures of low credit to construct dummy variables as decribed in the footnote of Table 5. Dependent variable means are based on the estimation sample using the average score measure. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact of Credit Check Ban on Employment by Wage Range

VARIABLES	(1) Log Employment Wage<$15K	(2) Log Employment Wage>$15K & Wage<$40K	(3) Log Employment Wage>$40K
Average Score Measure			
Low-Risk Score Tract i x State Ban t	0.0113	0.0662**	0.176***
	(0.0202)	(0.0297)	(0.0333)
Controls			
Census Division x Low-Risk Score Tract × Year FE	X	X	X
State x Year	X	X	X
Observations	588,942	588,905	588,800
Dependent Variable Means (Levels)	488	673	615
Dependent Variable Means For Low Credit Tracts (Level	457	631	275
R-squared	0.941	0.949	0.954

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division low-credit tract trends. Wage bins are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by state. The low-risk 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 exceeding 38 percent.
*** p<0.01, ** p<0.05, * p<0.1

Table 9a: Impact of Credit Check Ban on Employment by Industry—Large Response

	(1)	(2)	(3)	(5)	(6)	(7)	(8)
Log Employment in:	Government	Education	Transp. & Warehousing	Information	Real Estate	Retail Trade	Health Care
Low-Risk Score Tract i × State Credit Ban t	0.219**	0.206***	0.125***	0.0880***	0.0359*	0.0863***	0.0974***
	(0.0861)	(0.0747)	(0.0119)	(0.0284)	(0.0185)	(0.0237)	(0.0258)
Controls							
Census Division x Low-Risk Score Tract × Year FE	X	X	X	X	X	X	X
State x Year	X	X	X	X	X	X	X
Sample	Tracts with Industry Employment >10						
Observations	565,925	574,876	562,899	528,466	498,913	576,744	575,916
Dependent Variable Means (Levels)	83	176	64	47	33	209	176
Dependent Variable Means For Low Credit Tracts (Levels)	64	117	60	31	29	165	117
R-squared	0.866	0.897	0.873	0.880	0.829	0.906	0.916

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division, low-credit tract trends. Industry assignments are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by state. The low-risk 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 exceeding 38 percent.

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

Table 9b: Impact of Credit Check Ban on Employment—Small Response

Variables	(1)	(2)	(3)	(4)	(5)
	Log Employment in:				
	Accommodation & Food Services	Construction	Utilities	Finance & Insurance	Professional Services
Low-Risk Score Tract i × State Credit Ban t	-0.00652	-0.00399	-0.0367	-0.0218	-0.0246
	(0.0154)	(0.0195)	(0.0604)	(0.0244)	(0.0156)
Controls					
Census Division x Low-Risk Score Tract × Year FE	X	X	X	X	X
State x Year	X	X	X	X	X
Sample	Tracts with Industry Employment >10				
Observation	574,441	565,986	227,956	566,505	566,859
Dependent Variable Means (Levels)	154	90	20	82	107
Dependent Variable Means For Low Credit Tracts (Levels)	141	68	17	53	60
R-squared	0.907	0.907	0.720	0.912	0.928

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division, low-credit tract trends. Industry assignments are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by state. The low-risk 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 exceeding 38 percent.

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

Table 10: Signal Substitution: Impact of Credit Check Ban on Employer Education and Experience Requirements

Variables	(1) Share BA Required	(2) Share BA Required	(3) Share BA Required	(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 Risk Score City i x State 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

Source: Authors' calculations based on FRBNY/Equifax CCP and Burning Glass Technologies data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, nationwide, low-credit tract trends. The share of postings requiring a BA and the average years of experience required by all city-year postings are constructed from Burning Glass Technology data. Observations are postal city-years for 2007 and 2010–2013. Standard errors are clustered by city. The low-risk score measure is a dummy for the average score falling below 620.

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

Table 11: Vulnerable Populations: Impact of Credit Check Ban on Employment of Blacks and Youths

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES		Employed			Employed	
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			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

Source: Authors' calculations based on FRBNY/Equifax CCP and U.S. Census Bureau, American Community Survey.

Note: This table reports regressions of the form:

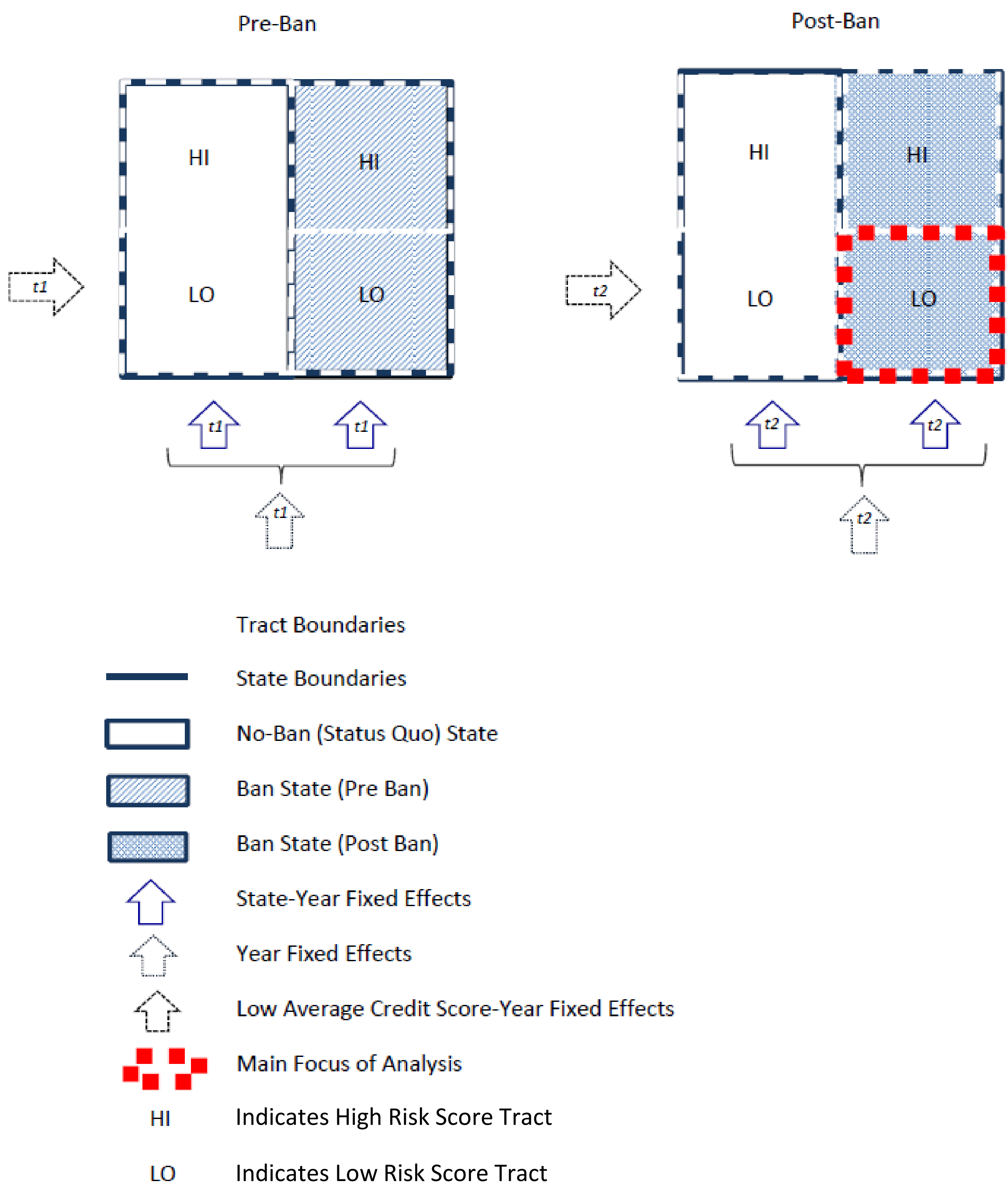
$$\text{employed}_{i,t} = \alpha_{group-state} + \alpha_{state-year} + \alpha_{black-year} + \gamma X_{i,t} + \beta_{\mathrm{t}} \times \text{credit check ban}_{\mathrm{st}} \times \text{group}_{i} + \varepsilon_{i,t},$$

where the α's control for arbitrary trends for blacks and for states, and for arbitrary racial differences across states. Specification 2 controls for education dummies, age/race dummies where not already controlled for by the fixed effects, and gender. Observations are individual-year for 2005–2013. Standard errors are clustered by state.

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

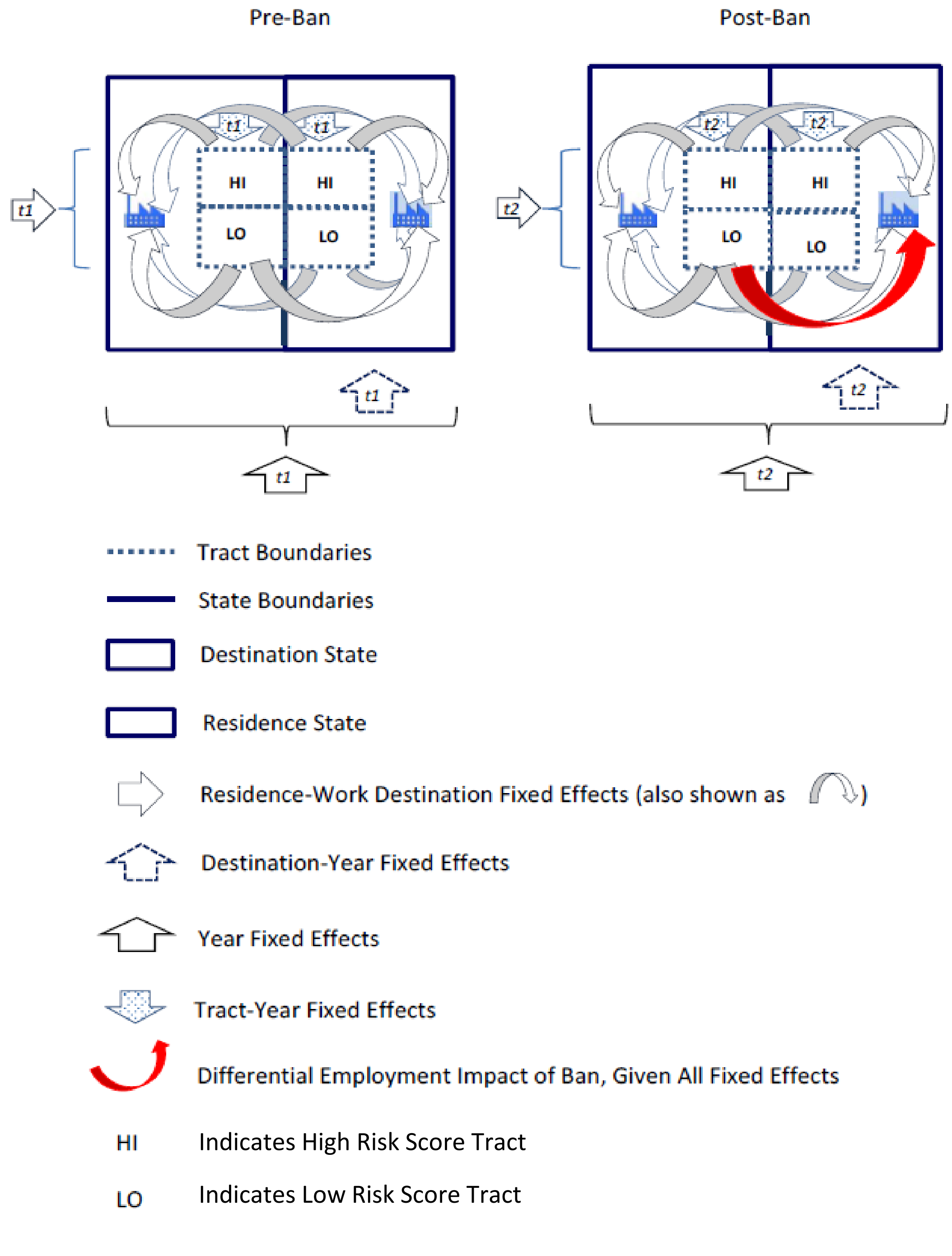
Appendix

Figure A.1. Illustration of the First (Triple Diff) Identification Approach



Source: Authors' conceptualization.

Figure A.2. Illustration of the Second (Quadruple Diff) Identification Approach



Source: Authors' conceptualization.

Table A1: Correlation between Alternative Measures of Low Credit

	Equifax Risk Score	Avg. Number Revolving Accounts with Major Derog Mark >24 months ago	Share of Persons with at least 1 Major Derogatory Revolving Account	Share of Persons with Major Derog Mark on >10% of Revolving Accounts	Share of Persons with >$1000 past due revolving account balance	Share of Persons with >50% of Revolving Debt Past Due (w/Update w/in last 3 months)	Share of Persons with Ch. 7 Bankruptcy Flag (Filed or Discharged)	Share of Persons with Ch. 13 Bankruptcy Flag (Filed or Discharged)	Share of Persons with Ch. 7 Bankruptcy Debt Discharged	Share of Persons with Ch. 13 Bankruptcy Debt Discharged
Equifax Risk Score	1.00									
Avg. Number Revolving Accounts with Major Derog Mark >24 months ago	-0.87	1.00								
Share of Persons with at least 1 Major Derogatory Revolving Account	-0.96	0.92	1.00							
Share of Persons with Major Derog Mark on >10% of Revolving Accounts	-0.96	0.91	1.00	1.00						
Share of Persons with >$1000 past due revolving account balance	-0.87	0.87	0.90	0.90	1.00					
Share of Persons with >50% of Revolving Debt Past Due (w/Update w/in last 3 months)	-0.90	0.86	0.93	0.93	0.91	1.00				
Share of Persons with Ch. 7 Bankruptcy Flag (Filed or Discharged)	-0.32	0.37	0.35	0.35	0.24	0.22	1.00			
Share of Persons with Ch. 13 Bankruptcy Flag (Filed or Discharged)	-0.43	0.40	0.43	0.43	0.35	0.37	0.27	1.00		
Share of Persons with Ch. 7 Bankruptcy Debt Discharged	-0.32	0.37	0.35	0.35	0.23	0.22	0.99	0.27	1.00	
Share of Persons with Ch. 13 Bankruptcy Debt Discharged	-0.32	0.30	0.32	0.32	0.25	0.27	0.24	0.76	0.25	1.00

Source: Authors' calculations based on data from the FRBNY/Equifax CCP.

Notes: Risk Score is the minimum (lowest) average riskscore for a tract over the sample period, 2005Q1 to 2014Q2. All other measures are the maximum (highest) average level/share over the sample period.

Table A2: Impact of Credit Check Ban on Low-Risk Score Tract Employment Using Bankruptcy Concentration as Proxy for Low Credit

VARIABLES	(1) Log Employment $_{it}$	(2) Log Employment $_{it}$	(3) Log Employment $_{it}$	(4) Log Employment $_{it}$
Dependent Variable Means (Levels)				
Average Score Measure × State Credit Ban t				
Chapter 13 Bankruptcy: Filed or Discharged	0.00214			
	(0.0201)			
Chapter 13 Bankruptcy: Discharged		0.0248***		
		(0.00876)		
Chapter 7 Bankruptcy: Filed or Discharged			-0.0232*	
			(0.0130)	
Chapter 7 Bankruptcy: Discharged				-0.0228*
				(0.0125)
Controls				
Census Division × Low-Risk Score Tract × Year Fixed Effects	X	X	X	X
State × Year Fixed Effects	X	X	X	X
Observations	559,795	559,796	559,751	559,748
Dependent Variable Means (Levels)	1,798	1,797	1,804	1,804
Dependent Variable Means For Low Credit Tracts (Levels)	1,393	1,382	1,403	1,402
R-squared	0.951	0.951	0.950	0.950

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

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

where the α's control for baseline differences across tracts with tract-level fixed effects, for arbitrary trends at the state or county level with state or county-year pair fixed effects, and for arbitrary, census division low-credit tract trends. Regressions reported in columns (2) and (5) also control for separate linear time trends in employment for low- and higher-risk score tracts by state. Observations are tract-year for 2002–2013. Standard errors are clustered by state.

We construct dummy variables for low-credit tract using chapter 7 and 13 bankruptcy concentration within a tract. Similar to Table 5, we use the maximum avergage value for each bankruptcy measure by tract and quarter over the period 2002Q1 to 2015Q2 to identify a census tract as "low credit." We identify tracts with shares in the 90th or greater percentile for these measures as low credit, which resuts in roughly the same number of low-credit tracts as our riskscore measures.

Table A3: Aggregate Effect of Credit Check Ban on Employment

	(1)
	Log Employment $_{it}$
State Credit Ban $_t$	-0.00565
	(0.0363)
Controls	
Tract FE	X
Year Fixed Effects	X
Observations	619,235
Dependent Variable Means (Levels)	1729.373
Dependent Variable Means For Low Credit Tracts (Levels)	1209.168
R-squared	0.810

Source: Authors' calculations based on FRBNY/Equifax CCP and LODES data.

Note: This table reports regressions of the form:

$$\ln \text{emp in industry}_{i,t} = \alpha_i + \alpha_{tract} + \alpha_{year} + \beta_t \times \text{credit check ban}_{s,t} + \varepsilon_{i,t},$$

where the α's control for baseline differences across tracts and years. Observations are tract-year for 2002–2013. Standard errors are clustered by state.

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

Table A4: Correlation between Tract Risk Score over Time

Year	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014
2005	1									
2006	0.9914	1								
2007	0.9812	0.9906	1							
2008	0.9718	0.9794	0.9902	1						
2009	0.9631	0.9694	0.9788	0.9905	1					
2010	0.9571	0.9628	0.9709	0.9805	0.9906	1				
2011	0.9537	0.9594	0.9667	0.9745	0.9815	0.991	1			
2012	0.9498	0.9557	0.9626	0.9692	0.974	0.9813	0.9913	1		
2013	0.9469	0.9527	0.9592	0.965	0.9684	0.9745	0.9828	0.992	1	
2014	0.9439	0.9497	0.9554	0.9605	0.9628	0.9681	0.9757	0.9828	0.9921	1

Note: Includes census tracts with at least 10 persons sampled in a given quarter.

Table A5: Impact of Credit Check Ban on Population Growth, 2010-2014

VARIABLES	(1) Log Population Growth	(2) Log Population Growth	(3) Log Population Growth
Average Score Measure			
Low-Risk Score Tract i x State Ban t	-0.00586	-0.000835	0.000974
	(0.00479)	(0.00422)	(0.00535)
Controls			
State FE		X	
County FE			X
Observations	48,920	48,920	48,920
R-squared	0.004	0.016	0.066

Source: Authors' calculations based on FRBNY/Equifax CCP, decennial Census, and American Community Survey Data

Note: This table reports regressions of the form:

$$\ln(2014\text{ pop})_i - \ln(2010\text{ pop})_i = \propto \text{low credit}_i + \gamma\text{ state credit check ban}_s(\text{state fe})(\text{county fe})\ \beta \times \text{credit check ban}_s \times \text{low credit score}_i + +\varepsilon_{i,t,}$$

where the α is a dummy variable for low credit tracts defined using our average score measures and γ is either (1) a dummy for states with a credit check ban (2) state fixed effects, or (3) county fixed effects. Our dependent variable is the log change in tract population over the period 2010-2014. Standard errors are clustered by state.

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