Working Papers

FEDERAL RESERVE BANK OF BOSTON™

No. 16-10

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

Robert Clifford and Daniel Shoag

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 the lowest-credit score census tracts. The largest gains occurred in higher-paying jobs and in the government sector. At the same time, using a large database of job postings, we show that employers increased their 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 credit scores, for those under 22 years of age, and for blacks, groups commonly thought to benefit from such legislation.

JEL Classifications: J78, M50

Daniel Shoag is an assistant professor of public policy at the Harvard Kennedy School. Robert Clifford is a senior policy analyst and advisor at the Federal Reserve Bank of Boston. Their email addresses are dan_shoag@hks.harvard.edu and robert.clifford@bos.frb.org, respectively.

This paper, which may be revised, is available on the web site of the Federal Reserve Bank of Boston at: http://www.bostonfed.org/economic/wp/index.htm.

This version: March 2016

I. Introduction

The use of credit information for employment screening has increased significantly over the last two decades (see Figure 1). Industry surveys indicate that such screening is used by 47 percent of employers (Society for Human Resource Management 2012). This screening tool has come under fire, though, by politicians and community groups that claim it unfairly penalizes minority and other vulnerable 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."[1]

Although employer credit checks are now pervasive and state and local bans on the use of credit information have become increasingly popular, little research has been done to date on their economic impact.[2] In this paper, we explore this impact using data from the Federal Reserve Bank of New York Consumer Credit Panel/Equifax. These data contain a 5 percent random sample that is representative of all individuals in the 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 credit score distribution for extremely detailed geographies like census tracts and blocks. We pair this credit information with data on employment outcomes for these geographies from the LEHD Origin-Destination Employment

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

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

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-credit 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 (for example, those with an average credit score below 620) saw employment increase by roughly 1.9–3.3 percent. The origin-destination nature of the LODES data enables us to cleanly identify this effect by exploiting *within* tract-year variation in employment destinations. These gains, in percentage terms, were in relatively higher-paying jobs. Across industries, employers in the public sector were most affected by these bans, followed by those in transportation and warehousing, information, and in-home services. This pattern makes sense, as both compliance and previous use of credit 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. Employment in construction and food services declined among residents of low-credit 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.

Although employment increased in the lowest-credit score tracts following a ban, we find that these increases were mirrored by relative employment declines in mid-to-low credit 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-credit score areas began requiring college degrees and prior work experience following a ban on credit screening. This is important evidence of substitution across signals by employers. To our knowledge, this is the first demonstration of signal substitution in this large a context.

To explore the net impact of these bans on minority populations, we 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.[3] Finally, 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.

Relative to this literature, our paper makes three central contributions. First, it provides a cleanly identified estimate of the impact of an economically important screening ban that has

[3] 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.

not been studied previously in the literature. Second, the paper provides some of the first evidence of large-scale *signal substitution* by employers and confirms that this substitution has disparate impact across demographic groups. Lastly, the paper provides an empirical framework for convincingly identifying the impact of state and local labor laws that target attributes that cannot be easily linked at the individual level, like credit scores. Many labor market laws—like those prohibiting criminal background checks 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 describes the central identification strategies and estimates the baseline relationship between credit bans and employment in low-credit 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 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.[4]

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.[5] 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

[4] 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.

[5] 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.

randomly generated "fuzz factors" specific to each employer and establishment.[6] 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.[7] 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 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

[6] 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

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

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.

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.

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. 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 $\mu_1$and μ_2 (where $\mu_1 > k > \mu_2$) and standard deviation σ. Further, we suppose that a credit check provides an unbiased signal of an individual's true quality y, where y is normally distributed with mean-zero noise and standard deviation γ. Note that, as an unbiased signal, the average credit 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 credit 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 credit scores $\left(y_i < \frac{\sigma^2 + \gamma^2}{\sigma^2}k - \frac{\gamma^2}{\sigma^2}\mu_1\right)$ are now hired, whereas individuals with good credit scores from the disadvantaged group $\left(y_i > \frac{\sigma^2 + \gamma^2}{\sigma^2}k - \frac{\gamma^2}{\sigma^2}\mu_2\right)$ 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, although this may be partly attributable to the noisy data on credit checks and the fact that some industries are exempt. Still, despite the limitations of the data, we observe a meaningful decline in the use of employer credit checks.

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-credit score tracts in ban states with the evolution of similar tracts in non-ban states. This approach, which is illustrated in Figure A.1 in the appendix, 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 credit score tracts in a given year are controlled for by low average credit 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-credit score tracts, using a binary division. We do this in two ways.

Our first method of classifying tracts is by constructing the average credit 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 credit scores to be constructed. To manage this problem, we drop any tract for which the difference between the highest and lowest average credit 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 credit scores if the average credit score was below 620 (the conventional subprime line) in any quarter.

Our second method, rather than using average scores, classifies tracts as having low credit scores based on the fraction of the sample below the 620 threshold, and high credit scores otherwise. To keep things similar to the analysis above, we aimed for a threshold that would mark roughly 15 percent of tracts as having low credit scores. Therefore, we pooled observations across quarters, and marked a tract as having low credit 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.[8]

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

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

[8] Obviously, other indicators could be used to mark tracts as having low-credit score populations, but such measures are strongly correlated with the ones we used, and in robustness experiments our results do not appear sensitive to the use of alternative indicators.

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 $\alpha_{low\ credit \times t}$ is a year dummy multiplied by the low-credit score dummy to control for arbitrary employment trend differences between low- and high-credit score tracts. The final coefficient of interest β measures how low-credit score tracts in states with credit check bans fare relative to low-credit score tracts in other states and relative to arbitrary within-state trends.

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

In Columns (2) and (5), we augment the term $\alpha_{state \times t}$, which 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-credit score tracts, taking into account any prior trends in specific state-level low-credit 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 35 additional jobs per year in tracts with low credit scores.

In addition to being interested in the average post-ban impact, we are also interested in the evolution of the employment response. To track this, we 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-credit score tracts.

To further test the robustness of this finding, we also re-ran our baseline specification, dropping, one at a time, each state with a ban on the use of credit information. These regressions produced a range of estimates between 1.9 and 4.3 percent, which closely bounds our initial results. Using data from the 2000 and 2010 Decennial Censuses, 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-credit score tracts, either within states or within counties, and the point estimates in both cases are close to zero.

Employment Effect: Within-Tract Identification

While the above results present a compelling case for the impact of these bans, the LODES employment data 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-credit score tracts to destinations with and without a ban.

In the paper's second (quadruple diff) identification approach, visualized in Figure A.2, we consider the evolution of employment for residents of tracts with high average credit scores and low average credit 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 credit score tracts in destination states that implement a ban relative to all of these other changes.

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:

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

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 the employment of low-credit score tracts' residents. . 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 5. 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-credit score tracts. These increases are measured relative to within-tract outcomes and relative to general trends in employment in destinations with a credit ban. The baseline impact across these specifications is roughly 6–8 percent within tract residence-destination state pairs, and a roughly 24 percent increase in cross-border commuting pairs. The base for these estimates is obviously smaller, and the implied employment gains from these larger percentages (13 and 3 jobs, respectively) are sensibly lower as a result. Again, this is evidence that the credit bans are impacting the distribution of employment even within tract-years. We believe it is difficult to conjecture a defensible omitted-variable-bias explanation for these results.

IV. Mechanism

The LODES employment data are rich, not just in their geographic detail, but also in that they break out employment by wage 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 A.1.

Across Wage Ranges

In Table 6, 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.

Across Industries

We show the impact of these credit check bans by industry in Tables 7 and 8. 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 7, 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.

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[9] and that otherwise-

[9] 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.

qualified employees are often rejected based on these checks.[10] This industry is closely followed by other services (largely in-home personal aides) 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 7 show the three industries with the next greatest impact—real estate, retail, and health care, which involve handling clients' financial information, an establishment's cash, or access to vulnerable clients and secure facilities.

Table 8 presents an interesting reflection of the large effects observed above. While employment increased generally in low-credit score tracts, it actually decreased in lower-wage industries like accommodations and food services and construction, which do not use credit checks intensely. Perhaps even more compelling is the fact, demonstrated in Columns (3), (4), and (5) of this panel, that employment in finance and insurance, professional services, and management of companies is unaffected by these bans. As mentioned above, these industries are generally exempt from the law, and, correspondingly, employment in these industries does not change in low-credit score tracts.

Across the Credit Score Distribution

As shown in the prior tables, we created dummies for low-credit 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 credit 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.

[10] "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

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

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 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 credit score profile fell below a cutoff of 620.[11] 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-credit 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 9, show that cities with lower credit scores experienced a greater increase in the share of jobs requiring these skills in states with a ban 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-credit 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

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

fraction of jobs in these low-credit 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 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 credit 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 credit scores. Therefore, our results are for the entire group in a state with the ban.

We begin with a regression of the form:

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

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 10. Columns (1–3) show that black unemployment rates were roughly 1 percentage point higher 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 an increase in the unemployment rate of roughly half this size, although this effect loses significance when state-specific young adult trends are controlled for.[12]

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.

VII. Conclusion

In this paper, we have shown that, even with fairly aggressive controls for potentially confounding trends, bans on credit checks in employment are associated with fewer employer credit checks and with employment gains in low-credit score areas. These gains happen in mid-to-high-wage jobs, with the biggest effect on public sector employment. These gains seem to happen alongside losses in tracts with slightly higher credit scores and relative reductions in employment and income for blacks. One explanation for this finding is that firms substitute towards other markers of worker quality, like education and experience, which we also document using new data on job postings. Overall these are intriguing results that should be useful for academics and for the ongoing policy debate regarding these bans. To our knowledge, this is the first analysis of these laws, and the first study to use data on employer

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

credit checks. 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 credit scores, that cannot be easily linked to individual labor market outcomes.

References

Adams, Scott J. 2004. "Age Discrimination Legislation and the Employment of Older Workers." *Labour Economics* 11(2): 219–241.

Aigner, Dennis J., and Glen G. Cain. 1977. "Statistical Theories of Discrimination in Labor Markets." *Industrial and Labor Relations Review* 30(2): 175–187.

Autor, David H., and David Scarborough. 2008. "Does Job Testing Harm Minority Workers? Evidence from Retail Establishments." *Quarterly Journal of Economics* 123(1): 219–277.

Avery, Robert B., Kenneth P. Brevoort, and Glenn Canner. 2012. "Does Credit Scoring Produce a Disparate Impact?" *Real Estate Economics* 40(S1): S65–S114.

Belasco, Andrew S., Kelly O. Rosinger, and James C. Hearn. 2014. "The Test-Optional Movement at America's Selective Liberal Arts Colleges: A Boon for Equity or Something Else?" *Educational Evaluation and Policy Analysis* 37(2): 206–223.

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

Bertrand, Marianne, and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review* 94(4): 991–1013.

Board of Governors of the Federal Reserve System. 2007. "Report to the Congress on Credit Scoring and Its Effects on the Availability and Affordability of Credit." (August).

Bryan, Laura Koppes, and Jerry K. Palmer. 2012. "Do Job Applicant Credit Histories Predict Performance Appraisal Ratings or Termination Decisions?" *Psychologist-Manager Journal* 15(2): 106-27.

Cohen-Cole, Ethan. 2011. "Credit Card Redlining." *Review of Economics and Statistics* 93(2): 700–713).

Correll, Shelley J., Stephen Benard, and In Paik. 2007. "Getting a Job: Is There a Motherhood Penalty?" *American Journal of Sociology* 112(5): 1297–1339.

Finlay, Keith. 2009. "Effect of Employer Access to Criminal History Data on the Labor Market Outcomes of Ex-Offenders and Non-Offenders." *Studies of Labor Market Intermediation*: 89–126.

Goldin, Claudia, and Cecilia Rouse. 2000. "Orchestrating Impartiality: The Impact of “Blind” Auditions on Female Musicians." *American Economic Review* 90(4): 715–741.

Graham, Mathew R., Kutzbach, Mark J., and McKenzie, Brian. “2014. Design Comparison of LODES and ACS Commuting Data Products.” U.S. Census Bureau, Center for Economic Studies. 14–38.

Holzer, Harry J., Steven Raphael, and Michael A. Stoll. 2006. "Perceived Criminality, Criminal Background Checks, and the Racial Hiring Practices of Employers." *Journal of Law and Economics* 49(2): 451–480.

Iyer, Rajkamal, Asim Ijaz Khwaja, Erzo F. P. Luttmer, and Kelly Shue. 2015. "Screening Peers Softly: Inferring the Quality of Small Borrowers." *Management Science*.

Kroft, Kory, Fabian Lange, and Matthew J. Notowidigdo. 2013. “Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment." *Quarterly Journal of Economics* 128(3): 1123–1167.

Lee, Donghoon, and Wilbert Van Der Klaauw. 2010."An Introduction to the FRBNY Consumer Credit Panel." *Federal Reserve Bank of New York Staff Report* 479.

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

Modestino, Alicia Sasser, Daniel Shoag, and Joshua Ballance. 2015b. "Downskilling: Changes In Employer Skill Requirements Over The Business Cycle” Draft, Northeastern University.

Ruggles, Steven, Katie Genadek, Ronald Goeken, Josiah Grover, and Matthew Sobek. Integrated Public Use Microdata Series: Version 6.0 [Machine-readable database]. Minneapolis: University of Minnesota, (2015).

Society for Human Resource Management. 2012. “Background Checking: The Use of Credit Background Checks in Hiring Decisions.”

Traub, Amy. 2013. “Discredited: How Employer Credit Checks Keep Qualified Workers Out of a Job.” Demos.

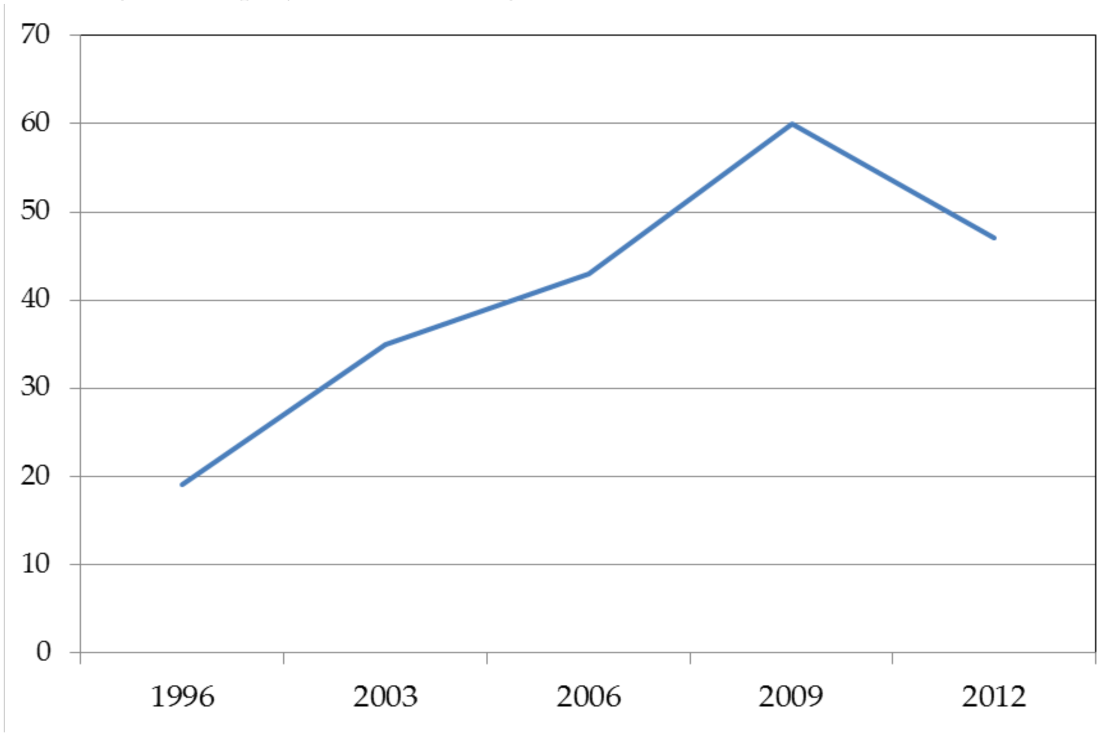
Weaver, Andrew. 2015. “Is Credit Status a Good Signal of Productivity?” *Industrial and Labor Relations Review* 6(4): 742-770.

Wozniak, Abigail. 2015. "Discrimination and the Effects of Drug Testing on Black Employment." *Review of Economics and Statistics* 97(3): 548–566.

Yagan, Danny. 2012. "Law School Admissions Under the UC Affirmative Action Ban." University of California, Berkeley.

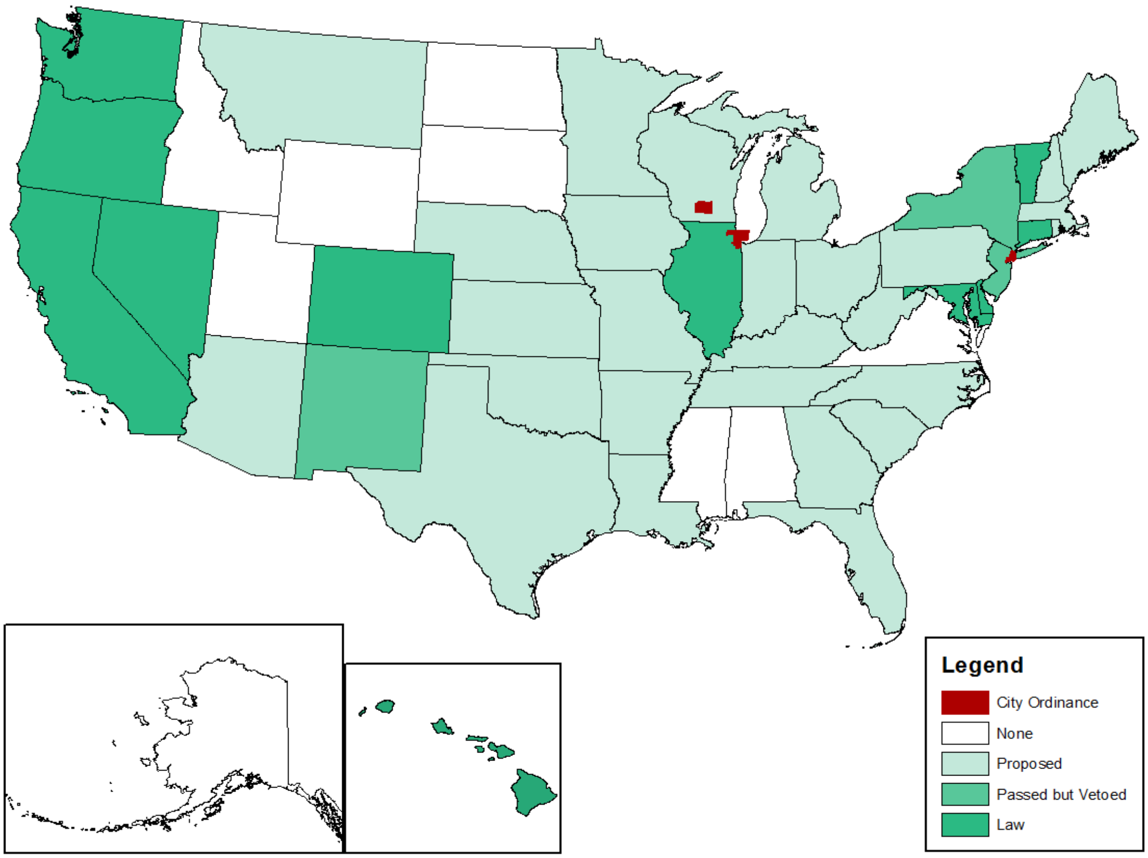
Figure 1: Use of Credit Checks by Employers, 1996–2012

Percentage of Employers Conducting Credit Checks



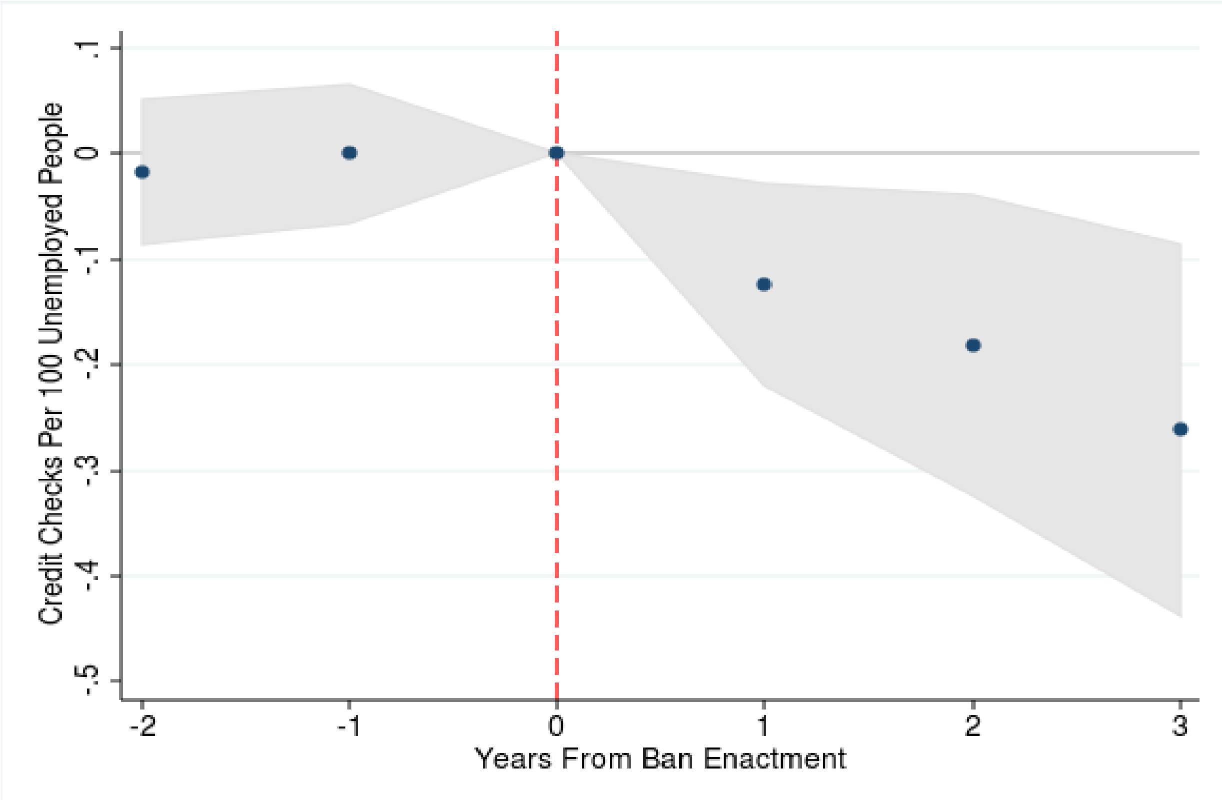
Source: Society for Human Resource Management, Survey of Hiring Managers, periodic Survey on the Use of Credit Checks in Hiring Decisions.

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



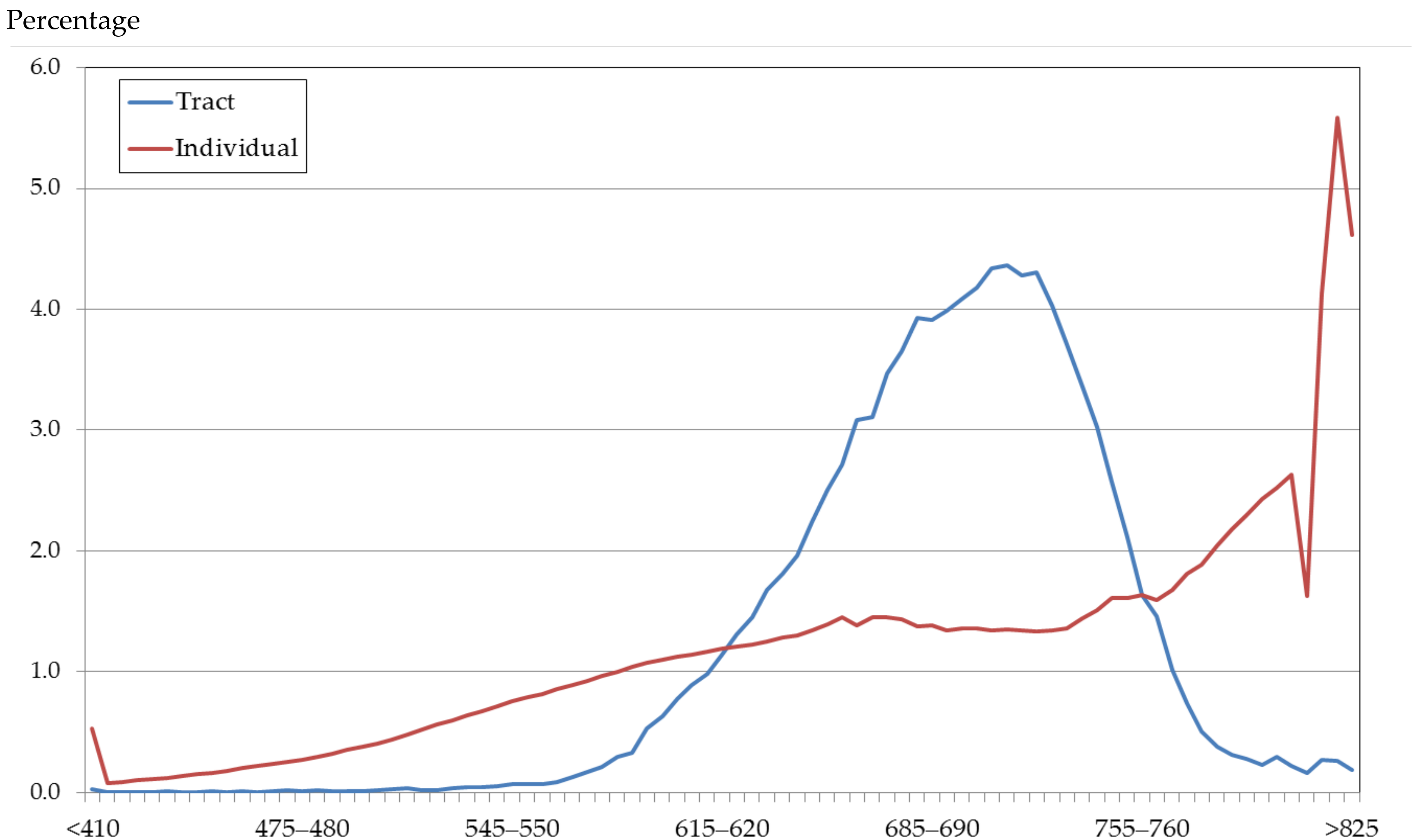
Source: Authors' calculations, based on Equifax data on employer credit checks.

Note: This figure reports the results of the regression:

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

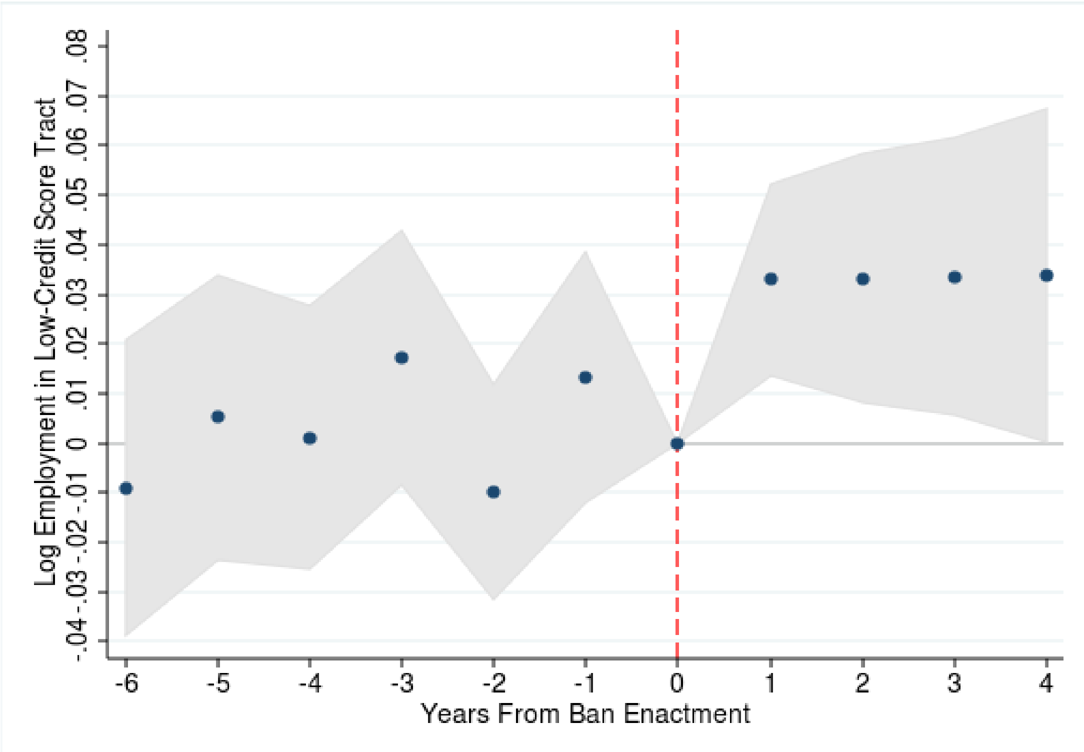
where s indexes state and t indexes year. Observations are state-year for 2009–2014. The graph shows the beta coefficients with confidence intervals. Standard errors are clustered by state.

Figure 4: Distribution of Tract Average Scores, Q4 2015



Source: FRBNY/Equifax CCP.

Figure 5: Impact of Credit Check Ban on Employment



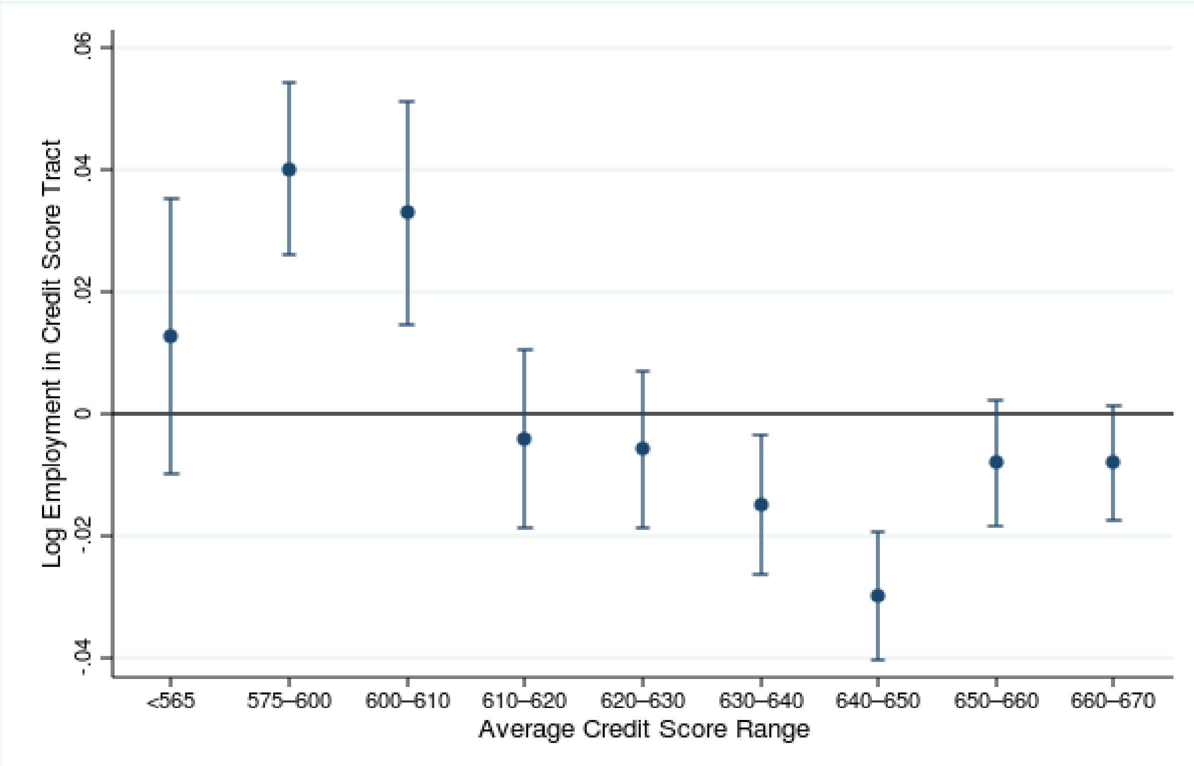
Source: Authors' calculations, based on Federal Reserve Bank of New York/Equifax Consumer Credit Panel (FRBNY/Equifax CCP) and U.S. Census Bureau LEHD Origin-Destination Employment Statistics (LODES) data.

Note: This figure reports the results of the regression:

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

where α_i are tract-level fixed effects, $\alpha_{\text{state*t}}$ 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 zip.

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



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

Note: This figure reports the results of the regression:

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

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

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 state 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 Credit Score	675.7	44.0	531.3	784.4	591,087
Fraction with Credit 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 Credit 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-Credit 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-Credit Score Tract $_i$ ×						
State Credit Ban $_t$	0.0330***	0.0220**	0.0308***			
	(0.0116)	(0.0108)	(0.0099)			
Proportion Measure						
Low-Credit Score Tract $_i$ ×						
State Credit Ban $_t$				0.0230**	0.0186*	0.0201**
				(0.0109)	(0.0101)	(0.0098)
Controls						
Low-Credit Score Tract × Year Fixed Effects	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	591,119	591,119	591,119	619,632	619,632	619,632
R-squared	0.962	0.962	0.975	0.961	0.961	0.974

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

Note: This table reports regressions of the form:

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

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, nationwide, low-credit tract trends. Regressions reported in columns (2) and (5) also control for separate linear time trends in employment for low- and higher-credit score tracts by state. Observations are tract-year for 2002–2013. Standard errors are clustered by zip code. The low-credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceeding 38 percent.
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Impact of Destination State Credit Check Ban on Origin-Destination Employment

VARIABLES	(1) Log Employment $_{it}$	(2) Log Employment $_{it}$
Average Score Measure		
Low-Credit Score Origin Tract $_i$ × Destination State Ban $_t$	0.0867 ***	0.2414***
	(.0240)	(.0274)
Proportion Credit Measure		
Low-Credit Score Origin Tract $_i$ × Destination State Ban $_t$	0 .0605***	0.2399***
	(.0234)	(.0267)
Controls		
Origin-Destination Fixed Effects	X	X
Destination-Year Fixed Effects	X	X
Origin-Year Fixed Effects	X	X
Sample	Origin-Destination Pairs with Employment >5	
	All States	Origin States w/o Ban
Observations	1,055,573	842,746
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, α_{d*t} controls for arbitrary trends at the destination level with destination-year fixed effects, and α_{o*t} controls for aggregate outcomes for the tract in the year. These fixed effects allow us to study within-tract year variation. Column (2) restricts the data to tracts in states without a current credit check ban, identifying the effect 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. Standard errors are clustered by tract. The low-credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceeding 38 percent.

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

Table 6: 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-Credit Score Tract $_i$ x State Ban $_t$	0.00465	0.0368***	0.112***
	(0.00871)	(0.00935)	(0.0154)
Controls			
Low-Credit Tract x Year Fixed Effects	X	X	X
State x Year	X	X	X
Observations	492,137	492,086	491,658
R-squared	0.962	0.965	0.967

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

Note: This table reports regressions of the form:

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

where 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. Wage bins are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by zip code. The low-credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceeding 38 percent.
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Impact of Credit Check Ban on Employment by Industry—Large Response

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

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

Note: This table reports regressions of the form:

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

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, nationwide, low-credit tract trends. Industry assignments are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by zip code. The low-credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceeding 38 percent.

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

Table 8: Impact of Credit Check Ban on Employment—Small Response

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

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

Note: This table reports regressions of the form:

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

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, nationwide, low-credit tract trends. Industry assignments are constructed by LODES. Observations are tract-year for 2002–2013. Standard errors are clustered by zip code. The low-credit score measures are, alternately, a dummy for lowest average score for the tract across time falling below 620 or the fraction of scores below 620 exceeding 38 percent.
*** p<0.01, ** p<0.05, * p<0.1

Table 9: Signal Substitution: Impact of Credit Check Ban on Employer Education and Experience Requirements

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

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

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-credit score measure is a dummy for the average score falling below 620.

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

Table 10: Vulnerable Populations: Impact of Credit Check Ban on Unemployment of Blacks and Youths

VARIABLES	(1)	(2) Unemployed	(3)	(4)	(5) Unemployed	(6)
Black x State Ban	0.0111***	0.0109***	0.0122***			
	(0.00298)	(0.00289)	(0.00323)			
Young x State Ban				0.00644*	0.00716*	0.00293
				(0.00353)	(0.0039)	(0.00266)
Controls						
State x Year	X	X	X	X	X	X
Black/Young x State	X	X	X	X	X	X
Black/Young x Year	X	X	X	X	X	X
Individual Demographics		X			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:

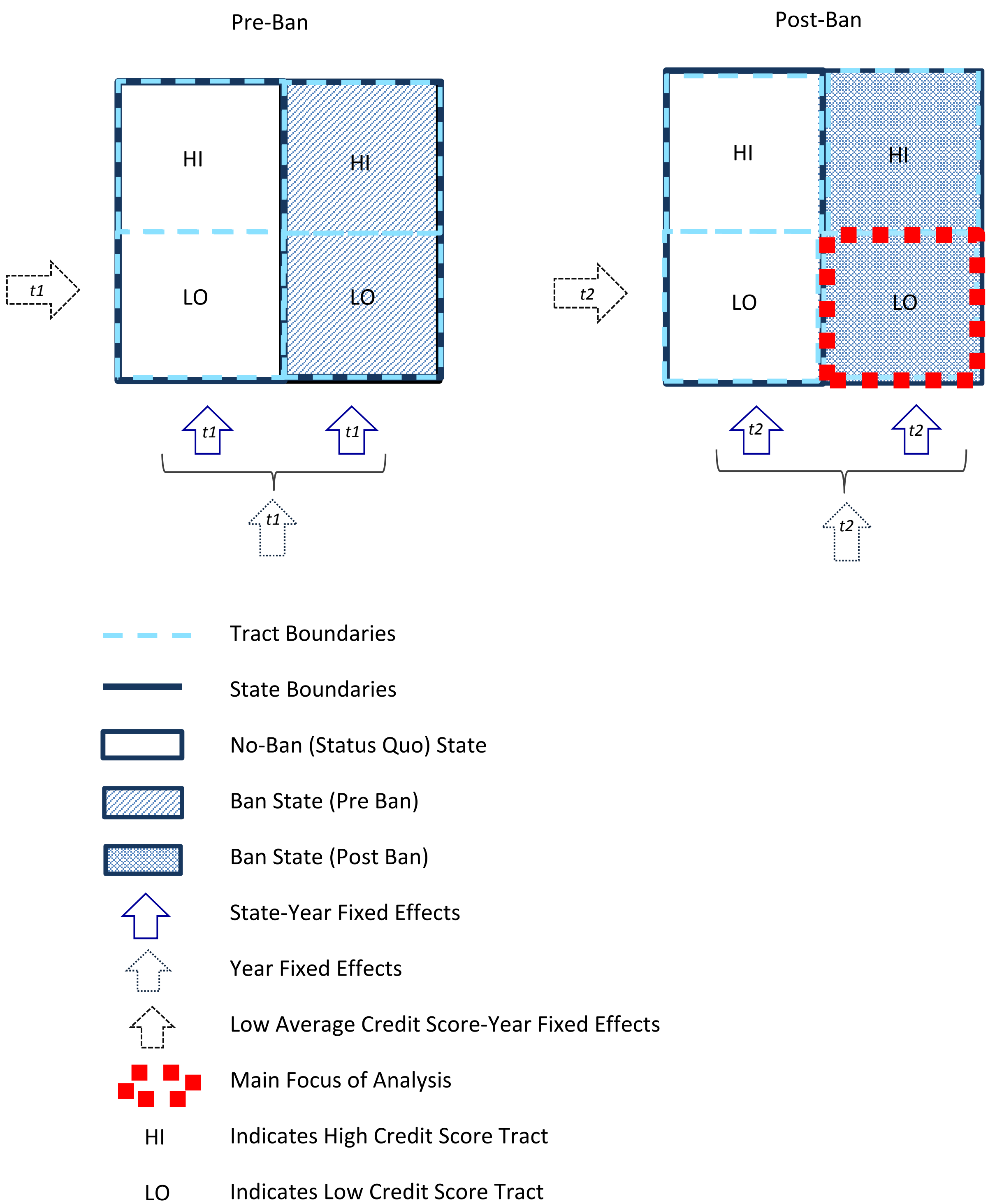
$$\text{unemployed}_{i,t} = \alpha_{group-state} + \alpha_{state-year} + \alpha_{black-year} + \gamma X_{i,t} + \beta_{\text{t}} \times \text{credit check ban}_{\text{st}} \times \text{group}_i + \varepsilon_{i,t},$$

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.

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

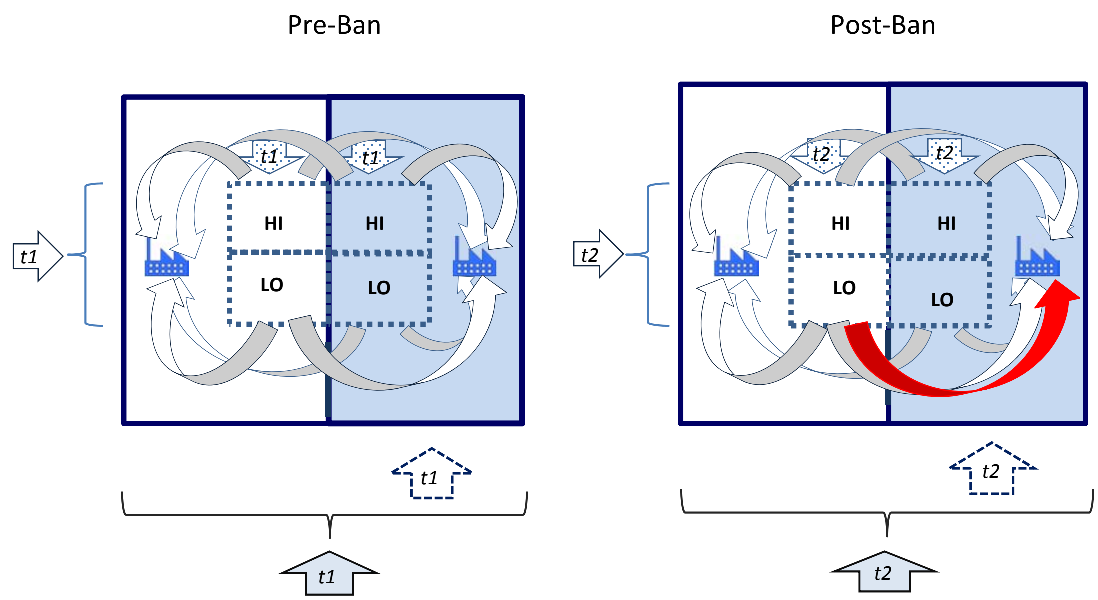
Appendix

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



Tract Boundaries

State Boundaries

Destination State

Residence State

Residence-Work Destination Fixed Effects (also shown as)

Destination-Year Fixed Effects

Year Fixed Effects

Tract-Year Fixed Effects

Differential Employment Impact of Ban, Given All Fixed Effects

HI Indicates High Credit Score Tract

LO Indicates Low Credit Score Tract

Source: Authors' conceptualization.