Who Benefits from Bans on Employer Credit Checks?

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Eleven states limit employers' use of credit reports, and prominent politicians have proposed a national ban. This paper evaluates the success of these credit check bans in helping financially distressed individuals find employment. In the Survey of Income and Program Participation (SIPP), we identify those likely to directly benefit from credit check bans – unemployed individuals with recent financial trouble. Exploiting the staggered passage of state bans, we find that banning credit checks increases the likelihood of finding a job by twenty-five percent among people who have had trouble meeting their expenses. We find a small and statistically insignificant change in job-finding rates among people who have not had recent financial trouble and a statistically insignificant impact on minorities overall.

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1. Introduction

Credit reports are used for more than just extending credit. Landlords check credit reports to screen tenants, insurers check them to set premiums, and many employers check them to decide whom to hire (CFPB 2012). This last use is particularly controversial. Eleven states and several cities now limit the use of credit reports in employment (NCSL 2016), and Senators Bernie Sanders and Elizabeth Warren have co-sponsored legislation that would impose a national ban.

Supporters of the bans argue that the information in credit reports is unrelated to job performance and that their use imposes significant costs on vulnerable populations.[1] The use of credit reports in hiring may hinder the financial recovery of consumers who have suffered adverse events, such as illness or job loss.[2] It can also have a disparate impact on minority employment because minorities tend to have worse credit histories.[3] It can even deter qualified workers from applying for jobs if they believe that a bad credit history will disqualify them. By allowing individuals with poor credit history to pool with those with good credit, credit check bans aim to improve employment outcomes for those with bad credit. This paper evaluates whether credit check bans help achieve that goal.

Using data from the Survey of Income and Program Participation (SIPP), we estimate the impact of credit check bans on the job-finding rates of unemployed individuals with a recent history of financial distress. The SIPP is unique in offering information about both weekly

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[1] Using the NLSY79, Weaver (2015) finds that credit reports do not reveal character traits that are good predictors of employee performance.

[2] See https://www.warren.senate.gov/?p=press_release&id=917, where it is argued that "research has shown that an individual's credit rating has little to no correlation with his or her ability to be successful in the workplace," and also that, "[a] bad credit rating is far more often the result of unexpected medical costs, unemployment, economic downturns, or other bad breaks than it is a reflection on an individual's character or abilities."

[3] The EEOC used this theory to sue a series of companies for employment discrimination, but it was largely unsuccessful. See, e.g., *EEOC v. Kaplan Higher Education Corp*, 748 F.3d 749 (6th Cir. 2014).

employment status (for up to four years), as well as the financial situation of respondents. Specifically, respondents were asked, "During the past 12 months, has there been a time when you did not meet all of your essential expenses?" and then were asked about trouble paying specific types of bills, like housing and utilities. 28% of the sample of unemployed individuals report recent trouble meeting essential expenses. While the SIPP does not ask directly about most expenses that are reported to credit bureaus, we confirm in the FINRA National Financial Capability Study that people reporting difficulty meeting expenses are also likely to have negative signals on their credit reports; they are roughly three times more likely to have sub-prime credit scores, late payments, foreclosures, and bankruptcies. In sum, the financial questions in the SIPP, along with detailed information on job seeking, allow us to identify individuals likely to benefit from credit check bans.

Our empirical strategy uses the staggered passage of state credit check bans to estimate their effect on the job-finding rates of unemployed individuals with recent financial trouble. We use this difference-in-differences strategy in estimating Cox proportional hazard models of job finding. We find a significant increase, with credit check bans raising the likelihood that someone with recent financial trouble will find a job by about 25%. This evidence is robust to controlling for differences in individual characteristics which might reflect changing selection into unemployment; economic conditions and unemployment insurance benefits, which might be correlated with legal changes; and the adoption of ban-the-box policies that make it harder to consider an applicant's criminal record and may affect a similar population.

We perform several further checks of our empirical design. We estimate the effect of the bans on a group that is less likely to benefit from them – individuals without recent financial distress, both using the full sample or using matching to form a sample of non-distressed individuals who are observably similar to the financially distressed individuals. We estimate that the bans caused a small and statistically insignificant change in the job-finding hazard rates of the non-distressed. This is interesting in its own right, as it suggests that the laws may have had little to no negative impact on the non-distressed through a crowd-out effect. This can further be viewed as a placebo check, because a significant positive effect on the non-distressed might suggest that other factors that changed at the same time states implemented credit check bans had an independent influence on labor markets. We then make use of the non-distressed as a within-state comparison group in a triple-difference specification, and we estimate a very similar magnitude as in our double-difference specification. Additionally, we implement an event study specification and show that the differences in job-finding rates arise only after the implementation of credit check bans.

By estimating the positive effects of the ban on the intended beneficiaries, our paper contributes to a growing set of working papers examining the impact of these bans. We make two primary contributions. First, our data shows which individuals are experiencing financial distress, and we demonstrate that this measure captures a poor credit history. In contrast, other papers investigating these bans focus on the impact on either geographic areas with low average credit scores or on minority groups as a whole (Ballance, Clifford & Shoag 2017, Bartik & Nelson 2016, and Cortés, Glover & Tasci 2017). Second, our data shows a labor market outcome where we might expect to see the clearest response: the weekly flow out of unemployment and into a job. Some other studies examine changes in aggregate employment or job vacancies, although Bartik & Nelson (2016) investigate annual and quarterly job-finding rates.

Our paper provides evidence of the positive impact of the bans on the intended beneficiaries. In comparison, other papers largely find negative unintended consequences in low credit score counties and on minority groups, including worse employment outcomes, lower job creation, spillovers to default and credit supply, and some employment gains in very low credit score areas offset by losses in areas with slightly higher credit scores (Ballance, Clifford & Shoag 2017, Bartik & Nelson 2016, and Cortés, Glover & Tasci 2017). We discuss the results from these other papers on credit check bans in the next section. Our paper is also related to papers examining the employment effects of information leaving a credit report (Dobbie, Goldsmith-Pinkham, Mahoney & Song 2017, Herkenhoff, Phillips & Cohen-Cole 2016, and Bos, Breza & Liberman 2016). They find mixed employment effects when negative information drops off of credit reports after several years. Finally, our paper is related to the larger empirical literature studying the role of applicant information in employment more generally, including Wozniak (2015) on employment-related drug testing, Finlay (2009), Doleac & Hansen (2016), Holzer, Raphael & Stoll (2006), and Agan & Starr (forthcoming) on criminal history, and Autor & Scarborough (2008) on job testing.

2. Background

Around 60% of surveyed employers reported conducting a credit background check on some or all of their applicants in 2010 (SHRM 2010). These credit reports are marketed as providing information about an applicant's financial responsibility or risk of committing theft or fraud. However, critics argue that credit reports are not informative about worker productivity or risk, and that their use in hiring decisions can hinder the recovery of struggling individuals. Using the NLSY79, Weaver (2015) finds that the character-related portion of credit reports is not a good predictor of employee performance; the wages of employees who will have bad credit in the future grow at the same rate as employees who will not. In light of concerns like these, most states have considered legislation that would limit the use of credit reports, and Senators Sanders and Warren have co-sponsored a national prohibition on the use of credit reports in employment. As shown in Table 1, eleven states (along with several cities) have enacted a ban.[4]

Two features may limit the impact of the bans. First, even in the absence of a statute, an employer's use of credit reports may still risk litigation, as the U.S. Equal Employment Opportunity Commission has argued that their use has a disparate impact on minority employment and is therefore prohibited by Title VII. However, the EEOC's litigation efforts have been largely unsuccessful, in part because at least one court noted that the EEOC itself used credit reports to screen applicants for most of its positions.[5] Second, all existing bans include exceptions. Common exceptions include positions at financial institutions, positions with access to money, confidential information or proprietary information, managerial positions, and positions where a credit report is substantially job related or a bona fide occupation qualification (Phillips & Schein 2015). The ban imposed by Connecticut may have little effect as it permits the use of credit reports if the applicant consents.[6] These exceptions may limit the impact of the laws, especially if the credit reporting industry is correct in arguing that the reports are primarily used for managerial or financial positions.[7] However, consumer advocates argue that credit reports are used much more broadly (Traub, 2013),[8] and even when they are not used, applicants

[4] Because we use state-level data, we are unable to make use of the municipal bans.
[5] *EEOC v. Kaplan Higher Education Corp*, 748 F.3d 749 (6th Cir. 2014).
[6] We include Connecticut's credit check ban, but when we drop Connecticut from the sample, the magnitude and significance of our estimates remain similar.
[7] https://www.shrm.org/hr-today/trends-and-forecasting/research-and-surveys/pages/creditbackgroundchecks.aspx
[8] According to Traub, "Our survey of low- and middle-income households carrying credit card debt finds that approximately 1 in 7 of these households recall being asked by an employer or prospective employer to authorize a credit check. About the same proportion say they don't know whether they've ever been asked for an employment credit check" (Bartik & Nelson 2016). Notably, the national ban proposed by Senators Sanders & Warren would only exempt positions that require national security clearance or where a credit check is otherwise required by law.

may feel deterred from applying for jobs for which they believe that a bad credit history will disqualify them.

Existing research that seeks to measure the effect of limits on the use of credit reports in employment differs by what types of limits, groups, and employment outcomes are studied. Some studies link credit reports to employment records and focus on the disappearance of old information from credit reports. In Sweden, Bos, Breza & Liberman (2016) find substantial positive employment effects on pawnshop borrowers when records of a default are removed after three years. In the U.S., though, the evidence suggests small or zero effects on employment when bankruptcy flags are removed from credit reports after 7-10 years. Herkenhoff, Phillips & Cohen-Cole (2016) find that upon the removal of a bankruptcy flag, transitions increase into the types of jobs that screen using credit reports. Dobbie, Goldsmith-Pinkham, Mahoney & Song (2017), linking bankruptcy filings to Social Security Administration employment records, find precise zero effects of bankruptcy flag removal on employment and earnings.[9]

Other recent working papers examine the impact of the same credit check bans as us, and find indirect evidence suggesting limited benefits but significant unintended consequences. Two papers compare areas with low average credit scores to areas with high average credit scores. Ballance, Clifford & Shoag (2017) find a net increase in jobs held by residents of very low credit score areas (Census tracts with an average score below 620), but these are offset by a net decrease in jobs in areas with only slightly higher credit scores (Census tracts with average scores between 630 and 650). Focusing on employers, Cortés, Glover & Tasci (2017) document

[9] Though not the focus, Dobbie et al. also include a test of the impact of a subset of credit check bans on annual employment (an indicator for any labor earnings) and earnings of individuals who had filed for bankruptcy 4 to 6 years earlier, and they find little effect. Our strategy differs in that we examine changes in the weekly job-finding rate (rather than annual employment and income) of unemployed individuals (rather than all bankruptcy filers) with recent trouble paying bills (rather than a bankruptcy filing 4-6 years past).

declines in job vacancies, especially in low-credit score counties. These papers provide evidence that credit check bans may harm people who are observably similar to those with bad credit (e.g. living in a low credit score area), if employers use the observable variable as a proxy for credit history. In addition to these negative employment effects, Cortés, Glover & Tasci (2017) find that bans were followed by increases in loan delinquencies and fewer credit inquiries, perhaps indicating a reduction in credit access. Though not tested empirically, another potential spillover is that bans reduce the information available to employers and may cause hiring decisions to become less efficient. Capturing all of these spillovers within a general equilibrium model, Corbae & Glover (2017) find that banning employer credit checks generates a small welfare loss.

Other recent research focuses on broader disadvantaged groups, who have worse credit on average, rather than disadvantaged neighborhoods. Ballance, Clifford & Shoag (2017) use information from an online vendor of job ads data to show that the bans led firms to rely more on education and experience, ultimately harming employment outcomes for Black applicants and young applicants. Similarly, Bartik & Nelson (2016) find that the bans reduced job-finding rates and increased separation rates for Blacks. These results imply, first, that members of the group with poor credit either do not benefit or are too small in number to affect outcomes for the whole group and, second, that those who resemble people with poor credit are directly harmed through statistical discrimination. Some papers use survey data to examine current employment (Ballance, Clifford & Shoag, in their analysis of the American Community Survey) or worker flows (Bartik & Nelson, in their analysis of the Current Population Survey panel, although the CPS is designed to follow housing units, not individuals); some use aggregated data on job postings and unemployment (Cortés, Glover, and Tasci); and some use administrative data on worker and job flows (Bartik & Nelson, in their use of the LEHD Job-to-Job data, and Ballance, Clifford & Shoag, in their use of the LEHD Origin-Destination Employment Statistics).

Relative to this literature, our data and empirical strategy offer several advantages. First, using the SIPP, we are able to observe individuals with recent financial trouble, rather than comparing across areas with different average credit scores or across demographic characteristics. We further verify in other data that people with recent trouble paying their bills are likely to have negative information on their credit reports and low average credit scores. This allows us to measure the impact of the bans on the intended beneficiaries, instead of on people living in locations or who are members of groups with a relatively high share of intended beneficiaries. Second, we focus on the employment outcome that may be most affected by the policy: the job-finding rates of unemployed individuals observed at a high frequency, instead of, for example, the annual job-finding rate, aggregate employment rates, or net job creation by location. While the SIPP is considerably smaller than the data sets used in some of the existing literature, we gain power by focusing on the group and outcome of direct interest. Lastly, we can make use of a within-state control group comprised of observably similar people who do not have trouble meeting essential expenses, so in some specifications we include state-by-year fixed effects that capture changes in labor markets within states that might be correlated with the passage of credit check bans.

3. Empirical Strategy

We estimate hazard models to investigate how credit check bans affect unemployment durations of financially distressed individuals. Our double-difference strategy uses the staggered passage of state credit check bans to estimate their impact on the job-finding hazard rate for unemployed people with recent trouble meeting essential expenses (whom we refer to as "financially distressed"). We also implement a triple-difference strategy that incorporates unemployed people who have not had such trouble as a within-state comparison group.

We use a Cox proportional hazard model to estimate the probability that an unemployed individual will find a job after τ weeks, conditional on being unemployed for τ -1 weeks. Our double-difference specification follows the set-up of Kroft & Notowidigdo (2016) and Chetty (2008), who also estimate unemployment durations with state and year fixed effects. We model the weekly unemployment exit hazard *h* for person *i* who has been unemployed in state *s* for τ weeks, beginning at time *t*, as

$$(1)\ \log(h_{ist}(\tau)) = \log(h_0(\tau)) + \beta Ban_{st} + X_{ist}\gamma + \delta_s + \tau_{y(t)}$$

where Ban_{st} is an indicator for an employer credit check ban being in effect in state *s* at time *t*, X_{ist} represents controls for individual characteristics, and δ_s and $\tau_{y(t)}$ are state and year fixed effects.[10] In the baseline specifications, X_{ist} consists of age, sex, years of education, and marital status. Additionally, following Chetty (2008), we include a dummy to adjust for the "seam" effect of panel surveys.[11] In all specifications, standard errors are clustered at the state level.

The key explanatory variable in our model is whether the unemployed spell begins in a state that has enacted a credit check ban, *Ban.* The coefficient β represents the change in the log of the job-finding hazard rate when credit is banned, after controlling for individual

[10] We use a continuous time Cox proportional hazard model, but our estimates are substantively unchanged if we instead use a complementary log-log specification, as in Bartik & Nelson (2016) and Meyer (1990), which accounts for the fact that the data are observed at discrete, weekly intervals. Supporting the proportional hazard assumption, the log-log plots of the survival function by ban status appear to be parallel. We also implement the Schoenfield residuals test, which computes the errors between the actual covariates and expected covariates of individuals failing at a certain time, and fail to reject the proportional hazard assumption.

[11] In panel surveys in which respondents are interviewed every few months about events in the intervening months, the respondents tend to report fewer changes within an interview than across interviews. In the SIPP, the interviews occur every 4 months and the seam effect leads to artificial spikes in job-finding rates during the 4th and 8th month.

characteristics X_{ist} and state and year fixed effects, and $-\beta$ is approximately equal to the change in the log of the unemployment duration.[12] If credit bans increase job-finding rates, β will be positive.

The causal interpretation of our estimates of β relies on the identification assumption that, in the absence of the credit ban, there would be no difference in the job-finding hazard rates for financially distressed individuals between the treatment and control states (after conditioning on other covariates).[13] While this assumption is not directly testable, we provide several checks of its plausibility.

First, we estimate the effect of the bans on a group that is less likely to benefit from them: individuals *without* a history of financial distress. There are two reasons to consider this group. On the one hand, in the spirit of a placebo check, they may reveal whether there are other changes happening in the labor market at the same time as the credit check bans are instituted; this would be a concern if we find a positive effect on the non-distressed that is similar to our estimated effect on the distressed. On the other hand, the non-distressed may be squeezed out of jobs that now go to the financially distressed; this would imply a negative effect of the bans on this group. In robustness checks, we also consider a subset of non-distressed individuals who match the distressed individuals on observable characteristics.

Second, we go on to make use of the non-distressed as a within-state comparison group, which helps control for other labor market trends that may differ in states that passed bans and

[12] This interpretation, which is used in Kroft & Notowidigdo (2016), relies on the fact that the log of the unemployment duration D is approximately equal to the inverse hazard ratio:

$$\log(D) \approx \log\left(\frac{1}{h}\right) = -\log(h).$$

[13] We do not face the problem that credit check bans may cause more financial distress, altering the composition of the treatment and control groups. As explained below, we measure instances of financial distress that occur in 2009 or 2010, when only three smaller states had a credit check ban in effect at that time. For the large majority of individuals in our sample, the financial distress occurred before their state ban became effective.

states that did not. Using a triple-difference specification, we test whether the changes in job-finding rates among the distressed differ significantly from the changes among the non-distressed in the same state and year. Since bans may affect both the distressed and the non-distressed in opposite directions, as we noted above, the non-distressed are not a true control group and the triple-difference estimate could differ from the double-difference estimate. We estimate the difference in weekly hazard rates among financially distressed individuals (indicated by *FD*) before and after a state credit ban becomes effective, relative to the difference in hazard rates among non-distressed individuals living in the same state and year:

$$(2)\ \log(h_{ist}(\tau)) = \log(h_0(\tau)) + \alpha\, FD_i \times Ban_{st} + X_{st}\,\gamma + \delta_s \times FD_i + \tau_{y(t)} \times FD_i + \delta_{s,y(t)}\,.$$

Equation (2) includes state × distressed fixed effects (that allow for different unemployment durations for financially distressed people across states), year × distressed fixed effects (that allow for changing unemployment durations nationwide among financially distressed people), and state × year fixed effects (that allow for different unemployment durations in a state that has passed a credit check ban or in any other state-year combination).

Finally, we implement an event study specification that includes leads of the treatment variable to test whether the differences in job-finding rates are present prior to the effective date of the credit bans. If our identification strategy is correct, we expect these leads to be small and statistically insignificant. If, however, states that pass bans are experiencing different trends in job-finding rates prior to the enactment of the bans, it would be detected in these lead coefficients. We conduct several further robustness checks to address concerns about omitted variables, selection into unemployment, and the similarity of distressed and non-distressed individuals.

4. Data

A. State Laws

Information on state bans of employer credit checks come from the National Conference of State Legislatures. Table 1 lists the ten states that enacted bans during our sample period and the effective (not enactment) date of each ban.[14] For an unemployment spell that begins in state *s* at time (year-month) *t*, we assign a variable *Ban* that equals one if employer credit checks are banned in state *s* at time *t*. If we set *Ban* equal to one for bans that passed during an unemployment spell, it would artificially skew the sample of treated spells towards longer durations in our Cox proportional hazard model (with time-invariant covariates).

B. SIPP Data

We use data on individuals from the 2008 Survey of Income and Program Participation (SIPP), which surveyed 42,030 households in May 2008 and followed them every four months through December 2013. The advantages of the SIPP are that it reports weekly employment status, and it also asked twice whether households were suffering financial hardship. A further advantage is that the SIPP oversamples people with low income, increasing the size of the sample in which we are interested.

We focus our analysis on the duration of unemployment spells, measured in weeks, and build our sample in much the same way as Chetty (2008) did. Starting from all job separations that begin during the SIPP, we restrict the sample to prime-age individuals who have at least 3 months of work history in the survey, who are not on temporary layoff, and who report searching

[14] Delaware enacted its ban in 2014, after our sample period ended. This information is identical to that used by Cortés, Glover, & Tasci (2017). It is nearly identical to Bartik & Nelson (2016), except for a minor difference in the timing of the Oregon law. The Oregon law was scheduled to go into effect on July 1, 2010, but the Governor of Oregon declared it effective immediately on March 29, 2010.

for a job. Details on sample construction are provided in the Appendix.[15] These restrictions leave 10,249 separate unemployment spells in the sample, with a total of 270,439 weekly observations.[16] The unemployment spell ends when an individual reports working for at least one month. Following Chetty (2008), we censor unemployment durations at 50 weeks to reduce the influence of outliers and to focus on job-finding rates during the first year of unemployment.

Table 2 presents the summary statistics for the unemployment spells of individuals residing in the ban states (those that have passed or will pass bans) and in the control states. The ban and control states are different in some ways, with ban states having worse labor markets but, as we explain below, slightly less financial distress. In the ban states, unemployment durations are 1.16 weeks longer on average, the unemployment rate is 1.32 percentage points higher, and individuals earn $165 more per month in pre-unemployment wages. They are also more likely to be married and Hispanic and less likely to be Black. We include some of these variables as controls in our regressions. To address remaining concerns about differences between the ban states and control states, our empirical analysis makes use of a within-state comparison group (financially distressed vs. non-distressed individuals) and also tests for differences in pre-treatment trends between the ban and control states within an event study specification.

Information on financial hardship comes from the Adult Well-Being interviews, which were conducted between May and August of 2010. These interviews ask households whether

[15] Compared to Chetty, we broaden the sample to include women and those who do not receive unemployment benefits.

[16] Note that our observations are unemployment spells, not individuals, and many individuals suffer more than one unemployment spell. If we drop individuals with more than one unemployment spell (about half the sample), our coefficients are slightly larger and significant at the 10% level. If we drop individuals with more than two unemployment spells, our coefficients remain larger than those presented and are statistically significant at the 1% level. Including individual fixed effects is infeasible, as there are only 37 financially distressed and 133 non-financially distressed that have both a pre-ban and a post-ban spell.

they had trouble meeting their essential expenses, such as rent or mortgage payments or utility payments. We code an individual as financially distressed if they answer "Yes" to the most general question: "During the past 12 months, has there been a time when (you/anyone in hour household) did not meet all of your essential expenses?" This results in 2,888 unemployment spells and 77,487 weekly observations for the financially distressed sample. We chose to use information on financial distress from as early as possible in the SIPP sample, although the same questions were asked again in late 2011-early 2012. This conservative choice allows us to observe financial distress prior to the enactment of most bans; when the financial distress questions were first asked, only Washington, Hawaii, and Oregon had implemented credit bans. Also, in robustness checks, we show that our estimates are similar if we limit the sample to individuals who report financial distress prior to the beginning of their unemployment spell.

The SIPP asks several questions about financial hardship, and we use the broadest indicator of financial distress (failing to meet essential expenses) in our analysis. 28% of the unemployed respond that they are not able to meet all of their essential expenses (26% in ban states, 29% in control states), compared to 18% of the full SIPP sample. Individuals who are unable to meet their expenses have lower pre-unemployment monthly wages than the non-distressed ($1,920 versus $2,520), a slightly lower level of education and more Black, Hispanic and female members.[17] In the payment questions that follow, detailed in Appendix Table A1, the most common missed payment involves gas, oil, or electricity bills (19% of everyone who is unemployed), and then rent or mortgage (15%). We use the broad indicator as a proxy for negative information on a credit report, rather than specific missed payments, for three reasons. First, the specific questions do not cover delinquent credit card, auto, student or medical debt,

[17] The summary statistics for the distressed and non-distressed samples, along with those of the matched sample of distressed and non-distressed, are in the Appendix.

which are important components of credit reports. Second, many people answer yes to multiple specific questions, and these responses will likely be correlated with unobservable missed payments, so we cannot isolate the impact of missing specific payment types on job-finding rates. Third, and most important, using a comparable measure of financial distress in the FINRA data below, we show that difficulty meeting essential expenses is strongly correlated with a poor credit history.

C. Corroborating Information on Financial Distress

For the SIPP measure of financial distress to be a good proxy for having a poor credit history, it should reflect delinquency on payments that are reported to credit bureaus. We can test this in the FINRA National Financial Capability Study (NFCS), which asks similar questions about meeting expenses along with several other questions about information that appears in credit reports. We use this similar question in the NFCS to validate our proxy for poor credit history.

The FINRA Investor Education Foundation provides information and educational tools to promote financial literacy. As part of this effort, it undertakes the National Financial Capability Study (NFCS), a periodic survey of the financial situation of over 25,000 Americans, representative of each U.S. state. We use the 2009 State-by-State Survey to investigate credit information for people who report having trouble meeting their regular expenses. Rather than asking whether you, "did not meet all of your essential expenses," as the SIPP does, the NFCS asks, "In a typical month, how difficult is it for you to cover your expenses and pay all your bills?" As shown in Table 3, 17% of NFCS respondents find it very difficult to "cover your expenses and pay all your bills" compared with the 18% of all SIPP respondents who report not having been able to meet their essential expenses, suggesting these two measures are similar.[18]

The advantage of the NFCS is that it also asks about several specific items that appear on credit reports, shown in Table 3. One question asks about respondents' credit scores for the 42% of respondents who have checked their credit score within the last 12 months. Credit scores of 620 or less (generally considered to be poor or subprime) were reported by 57% of respondents who found it very difficult to cover their expenses, compared to 19% of those who found it somewhat difficult or not difficult. Many more answered questions about recent bankruptcies and late mortgage or credit card payments. Once again, individuals who had difficulty covering their expenses were roughly three times more likely to report negative credit information. Therefore, the information in the NFCS suggests that the SIPP financial distress is highly correlated with several measures of poor credit history.[19]

5. Estimation Results

We estimate a series of hazard models to investigate how employer credit check bans affect unemployment durations for individuals who have trouble paying their bills.[20] Our double-difference identification strategy uses the staggered passage of bans across states to compare changes in the job-finding hazard rate among financially distressed individuals in states with and without bans. Our triple-difference specification includes the non-distressed sample as well, providing a within-state comparison group.

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[18] These statistics are computed using surveys weights to make them nationally representative.

[19] Hsu, Matsa & Melzer (2016) validate another measure of financial distress in the SIPP, about mortgage delinquency, by showing that the frequency and geographic distribution of mortgage delinquency in the SIPP is highly correlated with the measure from the Mortgage Bankers Association's National Delinquency Survey over the same period.

[20] In the Appendix, we also test for but do not find any significant effect on wages, other measures of job quality, and job separations.

A. Baseline Results

Graphical Evidence

Before reporting the results of our double and triple-difference regressions, we present graphical evidence on job-finding rates. Figure 1 plots Kaplan-Meier survival curves before and after the ban went into effect, restricting the sample to the states that eventually ban credit checks. These non-parametric estimators show the probability of remaining unemployed after *t* weeks. Separate curves are plotted for those with and without a history of financial distress. Before the bans, the survival curve for the distressed is consistently above the curve for the non-distressed, indicating that those with a history of financial distress are more likely to remain unemployed after *t* weeks. A log-rank test rejects the equality of the distressed and non-distressed survival curves (p=0.015). After the bans, however, the survival curves of the distressed and non-distressed are more similar, and the log-rank test does not reject equality (p=0.736).[21] Our double and triple-difference regressions confirm that this result holds after controlling for covariates, year fixed effects, and making use of individuals both in and outside of these states as a comparison group.

Table 4 reports the main results from the Cox proportional hazard models for the weekly job-finding hazard rate. Column 1 reports the coefficients from equation (1), the double-difference specification for the sample of financially distressed individuals in ban and non-ban states, while column 2 reports the same specification for non-distressed individuals. Column 3 reports the coefficients from equation (2), the triple-difference specification that includes both the financial distressed and non-distressed in ban and non-ban states. In column 1, the coefficient

[21] To ensure that these differences are not due to the fact that there are fewer post-law observations (969 vs. 1,678), we randomly selected 10 subsamples of 969 pre-law observations. The p-values for the log-rank test of the difference between the distressed and non-distressed survival curves remain significant at the 5% in 7 out of 10 of these subsamples, and the p-values of the other 3 are 0.1, 0.19, and 0.24.

on *Ban* is 0.247 and statistically significant at the 5% level.[22] It indicates that financially distressed individuals living in a state with a credit check ban have expected unemployment durations that are roughly 25% lower than those living in states without a credit check ban, after controlling for state fixed effects, year fixed effects, and individual characteristics. To interpret this as the causal effect of banning credit checks, there must be no other changes affecting unemployment durations that are correlated with the enactment of credit check bans.

The non-distressed sample provides a way to investigate this identification assumption. If our strategy captures the impact of the bans, we would expect no impact on non-distressed job-finding rates, or perhaps a negative impact if they are squeezed out of jobs that now go to the financially distressed. However, if our positive results for the distressed are driven by statewide shocks to unemployment durations, then the coefficient on *Ban* may be similarly positive when estimated on the sample of non-distressed individuals. Column 2 reports the coefficients from equation (1) estimated on the sample of non-distressed individuals. Consistent with the identification assumption, the coefficient on *Ban* is smaller (0.048), and is statistically insignificant, indicating little difference in the unemployment durations of (and no apparent harm to) non-distressed individuals living in ban and non-ban states.

The triple-difference specification in equation (2) uses the non-distressed sample as a within-state comparison group. Combining the distressed and non-distressed samples allows us to include state and year fixed effects interacted with an indicator for financial distress, as well as state-by-year fixed effects that control for state-level time-varying unobserved shocks to unemployment duration. As noted above, the bans may affect both the distressed and the non-

[22] Given the relatively small number of state bans, one concern is that our estimate is sensitive to the inclusion a single state. In Appendix Table A2, we show that the point estimate and standard errors are similar in a set of regressions that individually excludes each state that enacted a ban. The largest change is that, when California is excluded, the estimated effect increases to 0.41.

distressed in opposite directions, so triple-difference estimate could differ from the double-difference estimate. The results are presented in column 3 of Table 4. The key coefficient in this specification is the interaction of financial distress *FD* with *Ban*. The estimate of 0.284 (significant at the 1% level) indicates that after the effective date a credit ban, unemployment durations of financially distressed individuals fall by 28% relative to non-distressed individuals in the same state and year (after conditioning on all other covariates). The state-by-year fixed effects are jointly statistically significant, indicating different patterns in employment hazards within states over time, and the state-distressed fixed effects are jointly significant, indicating fixed differences across states in the employment hazards of the financially distressed as opposed to the non-distressed; the year-distressed fixed effects are not jointly significant.

Overall, the results in Table 4 show that credit check bans reduce the unemployment durations of the financially distressed by about twenty-five percent, while having little effect on the unemployment durations of those who are not distressed. Besides mandating the pooling of distressed and non-distressed job applicants in the employment screening process, the laws may also change the job-seeking behavior of those who are financially distressed. The relatively large effect that we estimate could arise because banning the use of credit reports may encourage people with bad credit to apply for some jobs that they would not have otherwise.

B. Robustness

Event Study

We implement several additional checks on the robustness of our estimation results. First, we conduct an event study analysis to detect pre-existing trends in the unemployment durations of the treatment states. We estimate a version of equation (1) that includes leads and lags of the *Ban* indicator:

$$\text{(3)} \qquad \log(h_{ist}(\tau)) = \log(h_0(\tau)) + \sum_{i=-4}^{0} \beta_i \,\Delta Ban_{s,t-6i} + \beta_{1+} Ban_{s,t-6} + X_{ist}\gamma + \delta_s + \tau_t,$$

where Δ is a 6-month difference operator, so that the coefficients β_{-4} through β_{-1} capture the difference in unemployment durations between states where a ban will go into effect within 24, 18, 12, or 6 months, relative to the group of control states.[23] The coefficient β_0 captures the effect of the ban on unemployment spells that begin in the first six months after a ban becomes effective, and β_{1+}, which does not contain a difference operator, captures the net effect of the ban after the first six months. For example, with California's ban that became effective in January 2012, $Ban_{s,t-6}$ will first equal one six months later, in July 2012, and its coefficient β_{1+} will reflect the change in the unemployment durations of spells beginning after July 2012. Since we only include leads up to 24 months, these coefficients represent changes relative to the differences that existed across states more than 24 months before a ban.

Figure 2 plots the point estimates and 95% confidence intervals on the leads and lags for the distressed and non-distressed samples, and the values are reported in Table 5. There are relatively few observations in each of the 6-month lead and lag intervals, and thus the event study estimates are less precise than the double-difference estimates. Panel (a) presents the estimates for the distressed sample. Consistent with our identification assumption, none of the leads are statistically different from zero, and a Wald test of the joint significance does not reject that they are jointly equal to zero. The post-ban coefficients are positive and similar in magnitude to the main double-difference estimate (0.212 and 0.249 compared to 0.247), with the longer-run effect of $Ban_{s,t-6}$ (6+ month lag) statistically significant at the 10% level (p=0.07).

[23] The controls X_{ist} include individual controls for onseam, age, marital status, gender, and education, as well as the state-level unemployment rate at the start of the spell.

The estimates from the non-distressed sample in panel (b) are smaller in magnitude, never statistically different from zero, and the longer-run impact on $Ban_{s,t-6}$ is negative (-0.013).

Robustness to Additional Demographic, Economic, and Legal Controls

Next, we investigate the sensitivity of our estimate to additional demographic, economic, and legal controls. Column 1 of Table 6 reports the results from a specification with only state and year fixed effects. Column 2 adds the demographic controls from the baseline specification, and column 3 includes additional individual-level controls for a five-piece log linear spline in pre-unemployment wages, age-squared, indicators for Black and Hispanic status, and indicators for occupation and industry.[24] Column 4 adds economic controls for geographic variation in both labor markets and unemployment benefits during the Great Recession by including the state-level unemployment rate and maximum allowable weeks of unemployment insurance.[25] Finally, column 5 adds the share of individuals in the state that are covered by a "ban-the-box" law for public employers, which prevents criminal background checks until late in the hiring process.[26] The coefficients in columns are all significant at the 5% or 1% level. Across all specifications, the estimates remain similar (or slightly larger) in magnitude and statistical significance.

Selection into Unemployment

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[24] The occupation dummies are for the five high-level SOC occupation classifications for the individual's pre-unemployment occupation (and a dummy for missing). There are twelve industry classifications and a dummy for missing. "Mining, quarrying, and oil and gas extraction" was combined with "Agriculture, forestry, fishing, and hunting" because of the small number of observations. Pre-unemployment (monthly) wages, occupation, and industry are the values for the last month worked prior to the unemployment spell.

[25] We use the unemployment insurance coding from Mueller, Rothstein & von Wachter (2016), which includes data through 2012. For this reason, we drop the unemployment spells beginning in 2013 from the regressions reported in columns 4 and 5.

[26] Ban-the-box laws can cover public employers, private employees with government contracts, or all private employees. The laws covering public employers are by far the most common and are almost always passed first. We use the laws from Doleac & Hansen (2016) and consider an employee as covered by the law if there has been a law passed in her county of residence. The finest geography available in the public SIPP data is the state, so we use the county populations from the 2010 Census to determine the share of the state population that is covered.

Another concern is that the bans may cause or be correlated with unobserved changes in the types of individuals who enter unemployment, though the lack of an effect on the non-distressed individuals rules out changes in selection that affect both the distressed and non-distressed equally. While we cannot explicitly test for changes in selection on *unobserved* characteristics, the stability of the coefficient on *Ban* in Table 6 demonstrates that there is little impact due to selection on *observable* characteristics. The coefficient remains similar or increases in magnitude as we move from no controls in column 1 to a full set of controls for demographic and economic characteristics in the later columns. Assuming that selection on unobservable characteristics is proportional to selection on observable characteristics, little movement in the estimated effect provides reassurance about changes unobserved selection into unemployment.[27]

Robustness to Sample Changes

Finally, in Table 7, we address concerns related to the construction of our samples of distressed and non-distressed unemployed individuals. Column 1 repeats the results from the baseline specification of Table 4. First, we address concerns about reverse causality between unemployment duration and financial distress. The questions about financial distress were collected in wave 6, two years after the 2008 SIPP survey began, so some individuals may be reporting financial distress that is caused by their unemployment spell. In columns 2 (with baseline controls) and 3 (with extended controls), we drop these individuals so that the sample consists only of unemployment spells that begin *after* individuals were asked about financial distress. These estimates for both the distressed (Panel A) and non-distressed (Panel B) remain

[27] Altonji, Elder & Taber (2005) and Oster (forthcoming) make the point that, when examining coefficient stability to gauge the importance of unobserved selection, it is important to also examine movements in R-squared. Using the OLS version of the hazard specification, we implement the exercise suggested in Oster (forthcoming) and our bias-adjusted coefficient is very similar to the baseline coefficient. Details on this exercise are in Appendix B.

similar to those from the full sample, though the smaller sample causes the estimates to be less precise. This similarity suggests that the timing of the questions about financial distress relative to the start of the unemployment spell does not influence our estimates.

Another possible criticism is that the non-distressed unemployed are not an appropriate comparison group for the distressed unemployed. In Table A3, we report the covariate balance of the two groups. The distressed unemployed have lower pre-unemployment monthly wages than the non-distressed ($1,920 versus $2,520), a slightly lower level of education and more Black, Hispanic and female members. To ensure that the difference in the effect of credit check bans is not due to these differences in group characteristics, we use propensity score matching to form a more similar comparison group among non-distressed individuals.[28] This strategy follows the literature arguing that pre-processing data before parametric estimation can produce more accurate estimates of the treatment effect (Ho, Imai, King, & Stuart 2007, Ferraro and Miranda 2014). Table A3 shows that the covariate balance between the groups improves substantially in the matched sample; there are no longer any statistically significant differences in observable characteristics between the two groups.

We then estimate the difference-in-difference Cox proportional hazard models on these matched samples. These estimates are in column 4 of Table 7. All individuals from the distressed sample in Panel A are matched, so the estimate is unchanged, at 0.247. For the non-distressed in Panel B, however, the coefficient on *Ban* is now negative, at -0.0596. Though not precisely

[28] We estimate a probit model for the likelihood of being distressed conditioning on pre-unemployment wage, education, age, sex, marital status, race, ethnicity, and the unemployment rate. Then, we apply single nearest-neighbor matching without replacement to select non-distressed individuals that have similar likelihoods of being distressed as the distressed individuals based on observable characteristics. The support of the distressed sample is within the support of the non-distressed.

estimated, the sign of this estimate is consistent with the bans redistributing jobs from non-distressed to distressed individuals.

C. Effects by Race and Ethnicity

Finally, following other papers, we estimate whether the bans have a harmful effect on minority groups as a whole. To do this we focus on samples of Black, Hispanic, and White (not Hispanic) unemployed individuals. Table 8 formally tests whether banning employer credit checks affected job-finding rates overall or for socioeconomically disadvantaged groups that have higher rates of poor credit. When we pool together the financially distressed sample (who comprise 28% of the unemployed in the SIPP and experience around a 25% reduction in unemployment duration) and the non-distressed (who experience a small and insignificant change) in column 1, the overall effect of credit check bans is to reduce unemployment durations by a statistically insignificant 9.7%. In contrast to other studies, we do not find that the bans have a statistically significant effect on minority employment outcomes, though small sample size make our estimates noisy. The signs of the coefficients in columns 2 and 3 suggest that Blacks had shorter unemployment spells and Hispanics had longer unemployment spells after the effective date of a ban, but these estimates fall well short of statistical significance. Whites had 18.3% shorter unemployment spells, and the estimate is significant at the 10% confidence level.

To compare the estimates to each other, we pooled the samples of Black with non-Hispanic White individuals or Hispanic with non-Hispanic White individuals. These specifications follow those in equation (2), except the financial distress indicator *FD* is replaced with either and indicator for Black status (in the first regression) or Hispanic status (in the second regression). The coefficient on the interaction of the Black indicator with the *Ban* variable is -0.012 (s.e. 0.36), and the coefficient on the interaction of Hispanic indicator with the *Ban* variable is -0.072 (s.e. 0.17). The effect is relatively small and is statistically insignificant in both regressions, which indicates that there are no statistically significant differences in the impact of the law on minority groups relative to non-Hispanic White individuals. However, the population sizes in the SIPP limit the power of our tests, and we cannot rule out economically meaningful effects.

6. Conclusion

Employers regularly check applicants' credit reports, but this practice is controversial. Prominent national politicians, including Senators Sanders and Warren, have introduced legislation that would ban the use of credit reports for employment purposes, and nearly every state has considered its own limits. A key motivation for the bans is a desire to help individuals who experience financial hardship. We provide evidence that existing bans generated a substantial improvement in job search outcomes of people who are likely to have bad credit. After the bans, the re-employment hazard of the financially distressed increases by about 25%. This magnitude is similar (though opposite in direction) to the effect of the newly unemployed receiving a severance payment or EITC tax rebate (Chetty 2008, LaLumia 2013). Our estimates suggest that employers treat such information as a negative signal of productivity or that people with bad credit were deterred from applying for jobs for which they believed a bad credit history would disqualify them. Society may place extra value on the employment of financially distressed individuals if the resulting income reduces use of the social safety net, reduces negative externalities of default or if the social welfare function values redistribution to these types.

Other papers have examined the impact of the bans on aggregate employment and job vacancies, so our estimates for job-finding rates are not directly comparable. However, after forming a matched sample, we find a (statistically insignificant) decline in job-finding rates of non-distressed individuals that are observably similar to distressed individuals. While our analysis focuses directly on people who are experiencing difficulties that affect their access to credit, our findings are broadly consistent with the results of Ballance, Clifford, & Shoag (2017), who find employment increases in extremely low credit score areas, employment losses in slightly higher credit score areas, and no net effect on employment overall. While another motivation for the bans is a desire to help members of minority groups, the existing literature suggests that credit check bans may depress minority employment as a result of statistical discrimination. We fail to confirm this finding in our data, but our standard errors are large enough that we cannot rule out economically meaningful effects.

It is worth noting that the effects we estimate occurred during the high-unemployment period of the Great Recession, and perhaps banning the use of negative information may be more important during slack labor markets. Moreover, since many of the bans are recent, our estimates largely reflect the short-run response. In future research, it may be possible to use linked employment and credit report data to look for more precise and long-run effects of eliminating the use of credit reports.

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Table 1 State Employer Credit Check Bans

State	Effective Date
Washington	7/22/2007
Hawaii	7/15/2009
Oregon	7/10/2010
Illinois	1/1/2011
Connecticut	10/1/2011
Maryland	10/1/2011
California	1/1/2012
Vermont	7/1/2012
Colorado	7/1/2013
Nevada	10/1/2013
Delaware	5/8/2014

Information on the states that enacted credit bans is from the National Conference of State Legislatures, and the dates that the bans went into effect were obtained from a combination of articles, press releases, and industry reports. The information in this table is identical to that in Cortés, Glover, & Tasci (2017).

Table 2 Summary Statistics for the Sample of Unemployment Spells by States

	Ban States		Control States		Difference	p-value
	Mean	Std. Dev.	Mean	Std. Dev.		
Duration	27.2	18.2	26.1	18.1	1.16	0.005
Financially distressed	0.26	0.44	0.29	0.45	-0.02	0.014
Pre-unemp. monthly wage	2,473	2,400	2,308	2,515	165.51	0.003
Education	12.5	3.0	12.8	2.3	-0.24	0.000
Age	36.7	13.1	36.5	13.1	0.19	0.527
Female	0.48	0.50	0.47	0.50	0.01	0.316
Married	0.46	0.50	0.41	0.49	0.05	0.000
Black	0.08	0.28	0.15	0.36	-0.07	0.000
Hispanic	0.29	0.45	0.12	0.32	0.17	0.000
Unemployment rate	9.64	1.87	8.31	1.80	1.32	0.000
Obs.	7,602		2,647			

The data are individual-level unemployment spells from the 2008 SIPP, covering 2008-2013. Ban States and Control States show the means and standard deviations of the covariates for unemployment spells in states that never enacted a credit check ban (control states) and the states in Table 1 that eventually enact a ban (ban states). Unemployment durations are censored at 50 or due to attrition, and the means include censored observations. *Financially distressed* indicates the percentage answering "*Yes"* to the question, "*During the past 12 months, has there been a time when (you/anyone in hour household) did not meet all of your essential expenses?*" This question was asked in the Adult Well-Being interview in Wave 6, May-August 2010. The unemployment rate is the state unemployment rate at the start of the unemployment spell.

Table 3 Financial Distress and Credit Scores, FINRA Survey

Difficulty meeting expenses and paying bills	**Very difficult**	**Somewhat/Not difficult**
Credit score less than 620	57%	19%
Bankruptcy in last two years	5%	2%
Foreclosure in last two years	7%	2%
Late on mortgage in last two years	52%	15%
Charged late fee on credit card in last year	59%	21%
Charged credit card over the limit fee in last year	42%	11%
Share of observations	17%	83%
Observations	4,818	22,826

Observations are from the FINRA Investor Education Foundation 2009 State-by-State National Financial Capability Study. Observations are weighted to be nationally representative. Individuals answering “Don’t know” or “Prefer not to say” are dropped when calculating the percentages shown in the table.

Table 4 The Impact of Credit Check Bans on Weekly Job-Finding Hazards

	Double Difference		**Triple Difference**
	Distressed Sample	Non-Distressed Sample	
	(1)	(2)	(3)
Ban	0.247**	0.048	
	(0.115)	(0.092)	
Financial Distress × Ban			0.284***
			(0.103)
On Seam	1.627***	1.719***	1.689***
	(0.071)	(0.053)	(0.049)
Age	-0.0135***	-0.0139***	-0.0141***
	(0.002)	(0.001)	(0.001)
Female	-0.217***	-0.141***	-0.167***
	(0.059)	(0.025)	(0.027)
Education	0.011	0.0383***	0.0340***
	(0.014)	(0.010)	(0.010)
Married	0.135***	0.100***	0.117***
	(0.042)	(0.032)	(0.026)
Observations (unemployment spells at weekly level)	77,487	192,952	270,439
Number of Spells	2,888	7,391	10,249
State FE	X	X	X
Year FE	X	X	X
State × Financial Distress FE			X
Year × Financial Distress FE			X
State × Year FE			X

The data are individual-level weekly job-finding hazards for the unemployed from the 2008 SIPP. All columns report estimates from semiparametric Cox proportional hazard models. *Ban* equals 1 if credit checks were banned in state *s* at the start of the unemployment spell, and *Financial Distress* equals 1 if the individual answers *"Yes"* to the question *"During the past 12 months, has there been a time when (you/anyone in hour household) did not meet all of your essential expenses?"* in the Wave 6 Adult-Wellbeing questionnaire. *On Seam* is an indicator for being on the seam between interviews to adjust for the seam effect.

Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01

Columns 1 and 2 report coefficients from equation (1) estimated on the distressed and non-distressed samples, respectively. Column 3 reports coefficients from equation (2) estimated on the pooled sample of distressed and non-distressed unemployment spells.

Table 5 Coefficients of the Event Study Specification

	$\Delta \mathbf{Ban}_{s,t+24}$ 24 mo. lead	$\Delta \mathbf{Ban}_{s,t+18}$ 18 mo. lead	$\Delta \mathbf{Ban}_{s,t+12}$ 12 mo. lead	$\Delta \mathbf{Ban}_{s,t+6}$ 6 mo. lead	$\Delta \mathbf{Ban}_{s,t}$	$\mathbf{Ban}_{s,t-6}$ 6+ mo. lag
Distressed	-0.169	-0.008	-0.064	-0.038	0.212	0.249*
	(0.123)	(0.186)	(0.193)	(0.280)	(0.198)	(0.137)
Non-Distressed	-0.03	0.08	-0.0151	-0.157	0.075	-0.013
	(0.121)	(0.054)	(0.115)	(0.100)	(0.143)	(0.098)

This table reports the coefficients and standard errors from the leads and lags of the event study in specification (3) estimated separately on the distressed and non-distressed samples.

Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01

Table 6 Robustness to Additional Controls

	No Controls (1)	Basic Control (2)	Extended Controls (3)	Economic Controls (4)	Ban the Box (5)
Ban	0.290***	0.247**	0.270**	0.343***	0.374***
	(0.0970)	(0.115)	(0.125)	(0.109)	(0.0954)
On Seam		1.627***	1.624***	1.645***	1.645***
		(0.0707)	(0.0705)	(0.0684)	(0.0684)
Age		-0.0135***	0.0191	0.0260**	0.0267**
		(0.00206)	(0.0123)	(0.0119)	(0.0119)
Female		-0.217***	-0.182***	-0.197***	-0.199***
		(0.0591)	(0.0665)	(0.0662)	(0.0666)
Education		0.0109	0.0239*	0.0234*	0.0236*
		(0.0143)	(0.0131)	(0.0137)	(0.0136)
Married		0.135***	0.0627	0.0751*	0.0749*
		(0.0420)	(0.0397)	(0.0423)	(0.0421)
Black			-0.220***	-0.222***	-0.221***
			(0.0807)	(0.0833)	(0.0831)
Hispanic			0.172**	0.173**	0.174**
			(0.0729)	(0.0785)	(0.0787)
Age-squared			-0.000427**	-0.000515***	-0.000523***
			(0.000167)	(0.000159)	(0.000160)
Unemp. rate				-0.0761*	-0.0709
				(0.0440)	(0.0438)
Max UI Benefit (weeks)				0.00146	0.00141
				(0.00137)	(0.00137)
Ban-the-Box Share					-0.186**
					(0.0826)
State FE	X	X	X	X	X
Year FE	X	X	X	X	X
Wage Spline			X	X	X
Industry & Occupation FEs			X	X	X
Observations	77,487	77,487	77,487	74,712	74,712

All regressions report estimates from the Cox proportional hazard model from estimating equation 1 on the distressed sample of unemployment spells. Wage spline is a 5-piece log linear spline in pre-unemployment wages. Column 4 adds controls for the state unemployment rate and the maximum state unemployment benefit duration in weeks from Mueller, Rothstein & von Wachter (2016). Column 5 adds the share of the state population that is covered by public ban-the-box laws as coded in Doleac & Hansen (2016).

Standard errors are clustered by state. Significance levels: *=.1, **=.05 and ***=.01

Table 7 Robustness to Sample Changes

	Baseline	Pre-unemp. Distressed Sample		Matched Sample
	(1)	(2)	(3)	(4)
Panel A. Financially distressed				
Ban	0.247**	0.222*	0.279*	0.247**
	(0.115)	(0.132)	(0.144)	(0.115)
Number of Spells	2,888	1,499	1,303	2,888
Obs.	77,487	37,093	34,318	77,487
Panel B. Non-distressed				
Ban	0.0478	0.0114	0.0807	-0.0596
	(0.0918)	(0.121)	(0.122)	(0.120)
Number of Spells	7,361	3,927	3,415	2,888
Obs.	192,952	94,966	87,613	78,123
State FE	X	X	X	X
Year FE	X	X	X	X
Basic Demographic	X	X	X	X
Extended Demographic			X	
Wage Spline			X	
Industry & Occupation FEs			X	

All regressions report estimates from the Cox proportional hazard model from estimating equation 1. Panel A is estimated on the sample of financially distressed unemployed, and Panel B on the sample of non-distressed unemployed. Column 1 repeats the baseline estimates from Table 4. Columns 2 and 3 restrict the sample to unemployment spells that begin after the individuals answered the question about financial distress. Column 4 uses the pre-processed non-distressed sample as described in the text. Basic demographic characteristics include age, sex, marital status, and education. Extended demographic characteristics adds individual controls for dummies for industry and occupation, age-squared, black and Hispanic status, and a 5-piece log linear spline in pre-unemployment wages, as well as the state unemployment rate and the maximum state unemployment benefit duration in weeks.

Standard errors are clustered by state. Significance levels: *=.1, **=.05 and ***=.01

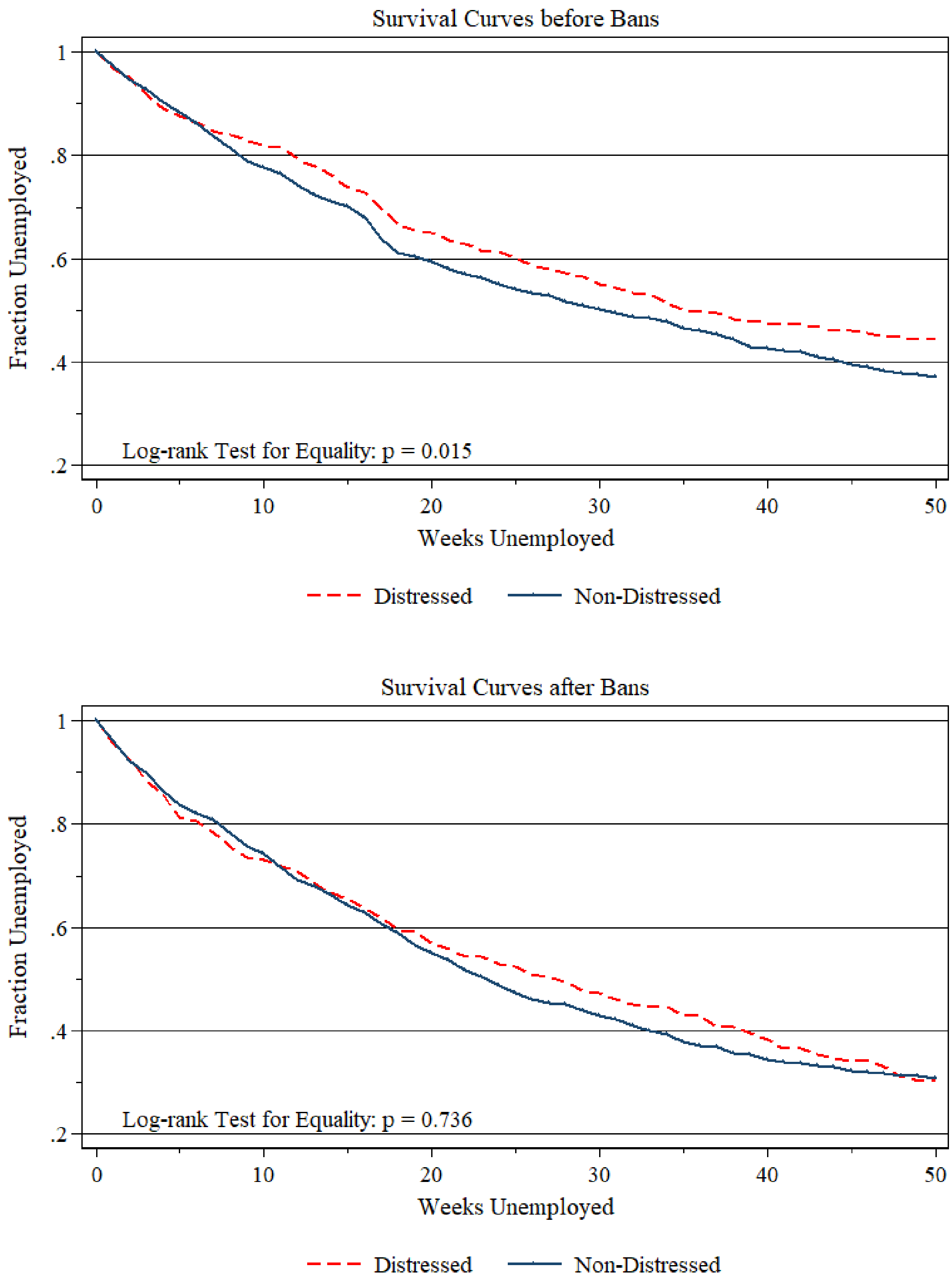
Table 8 Impact of Bans on Minority Groups

	All (1)	**Black** (2)	**Hispanic** (3)	**Non-Hispanic White** (4)
Ban	0.0969	0.122	-0.162	0.183*
	(0.0853)	(0.245)	(0.171)	(0.103)
On Seam	1.694***	1.809***	1.903***	1.611***
	(0.0488)	(0.094)	(0.087)	(0.047)
Age	-0.0137***	-0.00966**	-0.0148***	-0.0158***
	(0.000773)	(0.004)	(0.002)	(0.001)
Female	-0.168***	(0.103)	-0.400***	-0.127***
	(0.0264)	(0.085)	(0.066)	(0.035)
Education	0.0324***	0.0861***	-0.009	0.0491***
	(0.00953)	(0.019)	(0.008)	(0.011)
Married	0.113***	0.073	0.202***	0.0795***
	(0.0251)	(0.128)	(0.050)	(0.027)
Observations	270,439	40,241	44,514	165,271
Number of Spells	10,249	1,397	1,661	6,467
State FE	X	X	X	X
Year FE	X	X	X	X

The data are individual-level weekly job-finding hazards for the unemployed from the 2008 SIPP. All columns report estimates from semiparametric Cox proportional hazard models. Ban equals 1 if credit checks were banned in state s at the start of the unemployment spell, and Financial Distress equals 1 if the individual answers "*Yes*" to the question "*During the past 12 months, has there been a time when (you/anyone in hour household) did not meet all of your essential expenses?*" in the Wave 6 Adult-Wellbeing questionnaire. On Seam is an indicator for being on the seam between interviews to adjust for the seam effect. The samples are restricted to all individuals (both distressed and non-distressed), Black individuals, Hispanic individuals, and non-Hispanic White individuals.

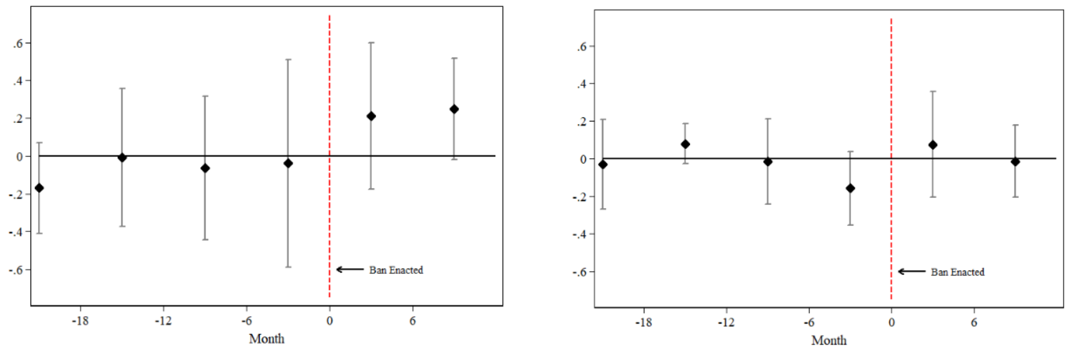
Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01

Figure 1 Survival Curves (Probability of remaining unemployed, by week)



The sample is restricted to the states that eventually ban employer credit checks. Each figure shows Kaplan-Meier survival curves for the distressed and non-distressed and the p-value of a log-rank test for equality of the survival curves.

Figure 2 Event Study of the Impact of Credit Check Bans on Unemployment Durations



a) Distressed

b) Non-Distressed

This figure plots the coefficients and 95% confidence intervals from the event study specification. This table reports the coefficients and standard errors from the leads and lags of the event study in specification (3) estimated separately on the distressed and non-distressed samples. The coefficients and standard errors presented are in Table 5.

Appendices

Appendix A: Data Construction

Measuring unemployment duration

Weekly employment status (ES) in the SIPP can take the following values:

1. With a job – working
2. With job - not on layoff, absent
3. With job - on layoff, absent
4. No job - looking for work or on layoff
5. No job - not looking for work and not on layoff

We define a job separation as a switch from ES=1,2 to ES=3,4,5. The duration of the unemployment spell is the number of weeks with ES=3,4,5, starting at the date of the job separation and ending when the individual reports a full month of work (ES=1 or ES=2). The unemployment spell is considered a temporary layoff if the individual reports ES=3 at any point in the spell. An individual is considered to be actively searching for a job if ES=4 at any point during the spell. Our sample construction below will focus on unemployment durations of active job searchers.

Sample construction

Our sample construction largely follows Chetty (2008), though we include women and individuals who are not receiving unemployment benefits. The 2008 Survey of Income and Program Participation starts with a sample of 105,663 individuals in 40,030 households. Of these, 31,570 individuals experienced at least one job separation during the sample period. Restricting the sample to individuals between the ages of 18 and 65, who are observed for at least three months, and have at least three months of wage history leaves 19,865 individuals. We drop individuals on temporary layoff, since they may not have been searching for a job, which leaves 16,385 individuals. We then keep only those actively searching for a job for at least one week, to eliminate people who dropped out of the labor force, leaving 10,054 individuals. Of these, we keep the 7,829 who have information on financial distress in the Wave 6 Adult Well-Being topical module. The final core sample consists of 7,829 individuals who experience 10,249 unemployment spells.

Appendix B: Coefficient Stability

In Table 9, we show that our coefficient of interest is stable as additional controls are included. Oster (forthcoming), building on Altonji, Elder & Taber (2005), shows that to be informative about unobserved selection, these changes in coefficient values should be scaled by changes in R-squared. We implement the calculation of Oster (forthcoming) in this section.

First, to get an R-squared value, we estimate the OLS analog of our Cox specification

$$\log(D_{ist}) = \alpha + \beta ban_{st} + X_{ist}\gamma + \delta_s + \tau_{y(t)} + \varepsilon_{ist},$$

a difference-in-difference regression on the sample of defaulters. This OLS regression does not account for right-censoring. To avoid right-censoring caused by the end of the SIPP, we drop all unemployment spells that begin after 2012. Right-censoring from spells exceeding 50 weeks or from attrition remain. Table A2 reports the estimates from this specification with and without controls. Reassuringly, if we ignore the remaining censoring issue and run this OLS, the results are similar to our Cox regressions.

Using these uncontrolled and controlled regressions, Oster (forthcoming) develops a method of calculating the bias-adjusted coefficient, taking into account both the change in the coefficient and R-squared as controls are introduced. This formula adjusts requires an assumption about the degree of unobserved selection relative to observed selection, then calculates a coefficient adjusted for bias from selection. Implementing this formula, our bias-adjusted coefficient is -0.290, virtually identical to our baseline estimate.[29] If we also include the ban-the-box controls in column 5 of Table 6, the bias-adjusted coefficient is -0.305.

[29] We use the suggested values for the unknown components of the calculation. Specifically, we assume $R_{max} = 1.3\widetilde{R}$ (130% of the controlled R-squared) and that the ratio of unobserved to observed selection, δ, equals 1.

Appendix C: Job Quality and Job Separations

Credit check bans may not only help distressed individuals find jobs more quickly, but they may also help the distressed find better jobs. Additionally, credit check bans may increase job separations. The potential reduction in job match quality, as employers have less information about applications, may result in more separations. Credit check bans may also help employed individuals with bad credit switch to new jobs. In this appendix, we test for effects on job quality and job separation rates, but find no statistically significant results.

Job Quality

We examine whether credit bans result in the financially distressed finding better jobs. Following LaLumia (2013), we use OLS regressions to estimate the effect of credit check bans on wage growth (difference between the log of the post- and pre-unemployment wages) and finding a full-time or salaried position. Sample sizes are smaller because some unemployment spells are censored, and other spells are missing data on job characteristics.

Results from these regressions are shown in Table A5. There is no statistically significant evidence that the credit check bans improve job quality, but the point estimates are generally positive. Column 1 shows a small decline in wage growth that is statistically insignificant. Column 2 shows that, controlling for the log of pre-employment wages, banning credit checks causes no statistically significant increase in post-employment wages. In column 3, the outcome is an indicator for whether the usual hours worked is greater than 35. In column 4, the outcome is whether the new job is in a salaried position. Both of these show positive but statistically insignificant effects of the ban on job quality.

Job Separations

We also test whether the bans have an impact on job separations. Following Bansak & Raphael (2008), we define job separations using changes in the SIPP's primary employer ID. SIPP interviewers record the identity of the respondent's primary and secondary employers and assign each an ID number. If the respondent changes employers, the next available ID number will be assigned to the new employer. If the respondent leaves the labor force or becomes unemployed, the employer ID will be set to "not in universe." For each wave of the SIPP, if the respondent's primary employer ID in the first reference month does not equal the primary or secondary employer ID four months later, then we code the individual as having separated from his primary employer. If there is a new employer ID four months later, we code the separation as having ended in a new job. If the respondent's employment status four months later is either "no job all month, on layoff or looking for work all weeks" or "no job all month, at least one but not all weeks on layoff or looking for work," we code the separation as having ended in unemployment.

We estimate regressions of the following form:

$$y_{ist} = \alpha + \beta ban_{st} + X_{ist}\gamma + \delta_s + \tau_{y(t)} + \varepsilon_{ist},$$

where y_{ist} is an indicator for quarterly job separation, job separation ending in a new job, or job separation ending in unemployment. The regressions include individual controls for age and age-squared, sex, years of education, marital status, an indicator for Black, an indicator for Hispanic, the state unemployment rate, and fixed effects for state, year, industry, and occupation.

Table A6 reports the coefficients on the *Ban* indicator. There are no statistically significant changes in separations for either distressed or non-distressed individuals. The point estimates indicate a 1 percentage point decline in quarterly job separations for distressed individuals (on a mean of 12.5%) and a 0.2 percentage point decline in job separation rates for non-distressed individuals (on a mean of 8.1%).

Table A1 Financial Hardship in the SIPP

Financial Hardship Questions	Mean Full SIPP	Mean Unemployed
Did you not meet all of your essential expenses?	0.18	0.28
Did you not pay the full amount of the rent or mortgage?	0.09	0.15
Were you evicted?	0.00	0.01
Did you not pay the full amount of the gas, oil, or electricity bills?	0.12	0.19
Did the gas or electric company turn off service, or the oil company not deliver oil?	0.02	0.04
Did you need to see a dentist but not go?	0.04	0.07
Did you need to see a doctor or go to the hospital but not go?	0.08	0.15
Did the telephone company disconnect service because payments were not made?	0.10	0.18
Observations	78,230	10,249

This table shows the incidence of financial distress, based on questions in the Adult Well-Being interview in Wave 6, May-August 2010, among SIPP respondents and the subsample of SIPP respondents with unemployment spells. The means for the full SIPP are for respondents in the Adult Well-Being interview and are weighted to be nationally representative.

Table A2 Results Excluding Individual States

Excluded State	Ban Effect	Standard Error
WA	0.257**	(0.116)
HI	0.253**	(0.118)
OR	0.219*	(0.114)
IL	0.208	(0.143)
CT	0.216**	(0.110)
MD	0.253**	(0.122)
CA	0.414***	(0.106)
VT	0.250**	(0.115)
CO	0.239**	(0.110)
NV	0.252**	(0.117)

Each row shows the estimated effect of the ban after excluding observations from the specified states. The estimates are from the main specification in equation (1).

Table A3 Covariate Balance of Distressed and Non-Distressed

	Full Sample			Matched Sample		
	Mean of Distressed	Mean of Non-Distressed	p-value of Difference	Mean of Distressed	Mean of Non-Distressed	p-value of Difference
Duration	26.83	26.21	0.12	26.83	27.03	0.68
Law	0.09	0.10	0.10	0.09	0.09	0.85
Pre-unemp. monthly wage	1,920	2,520	0.00	1,920	1,882	0.40
Education	12.16	12.91	0.00	12.16	12.19	0.71
Age	36.42	36.59	0.56	36.42	36.22	0.57
Female	0.49	0.46	0.02	0.49	0.49	0.64
Married	0.40	0.43	0.00	0.40	0.41	0.65
Black	0.17	0.12	0.00	0.17	0.18	0.65
Hispanic	0.19	0.15	0.00	0.19	0.18	0.20
Unemp. rate	8.64	8.66	0.67	8.64	8.64	0.98
Obs.	2,888	7,361		2,888	2,888	

The data are individual-level unemployment spells from the 2008 SIPP. Distressed and Non-Distressed show the means and standard deviations for unemployment spells among individuals answering “Yes” or “No”, respectively, to the question, “During the past 12 months, has there been a time when (you/anyone in hour household) did not meet all of your essential expenses?” This question was asked in the Adult Well-Being interview in Wave 6, May-August 2010. Unemployment durations are censored at 50 or due to attrition, and the means include censored observations. The unemployment rate is the state unemployment rate at the start of the unemployment spell. The first three columns show the covariate balance for the full sample of distressed and non-distressed. The last three columns show the covariate balance for the non-distressed sample that is pre-processed using propensity score matching as described in the text.

Table A4 Sensitivity of OLS Estimates to Additional Controls

	Dep. Var: Log(unemp. duration)	
	No Controls	Full Controls
	(1)	(2)
Ban	-0.305***	-0.294***
	(0.104)	(0.0813)
State FE	X	X
Year FE	X	X
Controls		X
Observations	2,692	2,692
R-squared	0.002	0.033

Estimates are from OLS regressions of equation A1. Column 2 includes the full set of controls from Table 9 column 4. Standard errors clustered at the state-level are in parentheses.

Table A5 Effects on Job Quality

	Wage growth	Log wage	Full-time	Salaried
	(1)	(2)	(3)	(4)
Ban	-0.00798	0.0842	0.0667	0.0374
	(0.0675)	(0.114)	(0.0642)	(0.0522)
Pre-unemployment controls				
Log wage		0.246***		
		(0.0260)		
Full time			0.293***	
			(0.0266)	
Salaried				0.311***
				(0.0285)
Observations	1,380	1,380	2,114	1,981

Table reports coefficients from OLS regressions on the sample of financially distressed. All specifications include state and year fixed effects (determined at the start of the unemployment spell) and individual controls for age, education, marital status, and sex. Standard errors are clustered at the state level.

Table A6 Effects on Job Separations

	Distressed Sample			Non-distressed Sample		
	Job Separation	New Job	Unemployed	Job Separation	New Job	Unemployed
	(1)	(2)	(3)	(4)	(5)	(6)
Ban	-0.0105	-0.00527	0.000253	-0.00221	-0.00345	-0.000648
	(0.00679)	(0.00366)	(0.00307)	(0.00325)	(0.00250)	(0.00130)
Controls	X	X	X	X	X	X
State FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Ind. & Occ. FE	X	X	X	X	X	X
Mean Dep. Var.	0.125	0.06	0.032	0.081	0.045	0.013
Observations	51,207	51,207	51,207	287,974	287,974	287,974

This table shows coefficients on Ban from LPM regressions on quarterly employment transitions. Job Separation is an indicator for separating from a primary employer. New job is an indicator for a separation that results in a new employer within the quarter. Unemployed is an indicator for a separation that results in unemployment. All specifications include controls for individual age and age-squared, sex, years of education, years of employment, marital status, an indicator for Black, an indicator for Hispanic, the state unemployment rate, and fixed effects for state, year, industry, and occupation. Standard errors are clustered at the state level.