Who Benefits from Bans on Employer Credit Checks?

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Several U.S. states ban employers' use of credit reports in hiring decisions. This paper evaluates whether these bans help financially distressed individuals find employment. In the Survey of Income and Program Participation, we identify individuals likely to directly benefit – unemployed individuals with recent trouble meeting expenses. Exploiting the staggered passage of state laws, we find that banning credit checks increases the job-finding rates among financially distressed job seekers by about 28 percent. We also find an increase in the subsequent employment duration of the financially distressed who do find jobs, suggesting that they obtain more stable and permanent positions. Finally, we find a small, insignificant change in job-finding rates among the non-distressed, but we cannot rule out that this group is harmed by pooling with the financially distressed.

1. Introduction

In the United States, 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 when deciding whom to hire (CFPB 2012). Around 60% of surveyed employers conducted a credit background check on some or all of their applicants in 2010 (SHRM 2010), and 10% of unemployed job seekers report not being hired because of their credit report (Traub 2013).[1] Credit bureaus and background screening services market these reports as providing information about an applicant's character, financial prudence, or risk of committing fraud. Critics argue, however, that the information in credit reports is unrelated to worker productivity and their use hinders the job search of struggling individuals. In light of concerns like these, eleven U.S. states and several cities now limit the use of credit reports in employment (NCSL 2016), and several members of Congress have introduced legislation that would impose a national ban.

Do these credit check bans help job seekers with poor credit reports find jobs? The answer is not obvious. Even without bans, credit reports may not be the deciding factor in hiring decisions. Most firms allow job candidates to explain negative credit information (SHRM 2010) and interviews with hiring managers reveal a complex and informal decision-making process (Kiviat 2019). Moreover, firms may be wary of litigation when using credit reports, as the U.S. Equal Employment Opportunity Commission has argued that their use has a disparate impact on

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[1] According to Traub (2013), "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." Additionally, this survey found that 1 in 10 unemployed respondents and 1 in 7 respondents with a poor credit history report being advised that they were not hired because of their credit report. In a survey of over 1,000 hiring professionals, O'Brien and Kiviat (2018) found that a bad credit report makes hiring managers significantly less likely to recommend an applicant for hiring.

minority employment and is prohibited by Title VII.[2] Additionally, the bans may have little

impact on the use of reports. All existing bans include exemptions for certain jobs, commonly

including financial positions, managerial positions, and positions where a credit report is

substantially job-related (Phillips and Schein 2015).[3] Ballance, Clifford, and Shoag (2020) find,

nevertheless, that the number of employer credit checks declined after bans went into effect.

Even without credit reports, firms may still statistically discriminate by using other signals, such

as race, age, or education, as a proxy for negative credit information, which can lead to

unintended negative consequences.

In this paper, we estimate the effect of employer credit check bans on the job-finding

rates of people who are likely to have poor credit reports. We use the Survey of Income and

Program Participation (SIPP), which contains respondents' weekly employment status (for up to

five years) and measures of their financial standing. Respondents are asked, "During the past 12

months, has there been a time when you did not meet all of your essential expenses?" as well as

questions about trouble paying specific types of bills, like housing and utilities. Among

unemployed individuals in the SIPP, 28% report recent trouble meeting essential expenses. We

use these questions to identify people who are financially distressed and therefore likely to

benefit from the bans, confirming with the FINRA National Financial Capability Study that

answers to these questions are highly correlated with sub-prime credit scores, late payments,

foreclosures, and bankruptcies.

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[2] 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. EEOC v. Kaplan Higher
Education Corp, 748 F.3d 749 (6th Cir. 2014).
[3] https://www.shrm.org/hr-today/trends-and-forecasting/research-and-surveys/pages/creditbackgroundchecks.aspx.
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.

Using the staggered passage of state credit check bans, we then examine changes in the job-finding rates of these financially distressed job seekers. Upon enactment of the bans, the job-finding rate of financially distressed job seekers increases by 28% relative to distressed job seekers in non-ban states. Applying this increase to the average unemployment duration of 26 weeks, distressed job seekers find jobs roughly seven weeks earlier and earn an additional $3,700. The improvement in job-finding rates begins only after the enactment of the bans, is not sensitive to additional controls and sample changes, and occurs only among job seekers searching for non-exempt positions (based on their work history).

At the same time, we fail to find evidence of some potential drawbacks of credit check bans. One concern is that, if credit reports provide employers with useful information about applicants, banning this information will reduce the quality of employer-employee matches. We do not find evidence of declines in match quality using measures available in the SIPP: the duration of employment and probability of early separations. In fact, financially distressed new hires in ban states experience longer employment durations and reduced early separations compared to other distressed new hires, suggesting that the bans allow distressed job seekers to obtain more stable, permanent positions. We note, however, that the SIPP's information on employers and measures of match quality is limited, and a more complete analysis of the bans' impact on match quality is an important area for future work.

We also examine the effect of credit check bans on individuals without recent financial distress – a group that is less likely to benefit from bans and that could be harmed since they now pool with financially distressed job seekers. We find no significant change in the job-finding rates of the non-distressed, though we cannot reject small declines in their job-finding rate that, when weighted by the number of non-distressed job seekers, would offset the increased

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employment for the distressed. Consistent with this, we find no significant effect of the bans on overall employment.

Our paper complements recent work examining the impact of these same bans, and we provide a detailed comparison in the paper. Our contribution is twofold. First, we directly examine the intended beneficiaries: financially distressed job seekers. Second, we focus on the employment outcome that may be most affected by the policy – job-finding rates of unemployed individuals. Our results are quantitatively consistent with Ballance et al. (2020), which finds increased employment in neighborhoods (specifically Census tracts) with very low average credit scores, though they also find offsetting declines in employment in those with slightly higher credit scores.

Most recent papers examining these same bans emphasize unintended negative consequences. Bartik and Nelson (2019) and Ballance et al. (2020) find that the bans worsened labor market outcomes for members of minority groups and young workers. Cortés, Glover, and Tasci (2020) find reductions in vacancy postings in industries not exempted from the bans relative to exempt industries. The SIPP is not well suited for examining the bans' impact on members of minority groups or vacancy postings, but we conduct several tests and calculations showing the net effects are consistent with our estimates of a lack of change to overall employment.[4] Our paper complements this literature in that it helps pin down the benefits of employer credit check bans, which can be weighed against offsetting negative consequences.[5]

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[4] The SIPP sample has limitations (notably a small sample of black job seekers in states enacting bans and no data on job postings) that prevent us from replicating these results, though when we implement the same sample restrictions and definitions as the Current Population Survey imposes, the estimated effect of credit check bans becomes more negative and closer to the estimate in Bartik & Nelson (2019).

[5] For example, Corbae et al (2017) use a quantitative model to weigh positive impacts on those with poor credit against negative impacts on employer match quality and those with good credit.

Another literature examines the consequences of removing specific negative information from an individual's credit report, finding mixed effects on employment. Bos et al. (2018) analyzes the removal of default notices for pawnshop borrowers in Sweden after three years and Dobbie et al. (2020) and Herkenhoff et al. (2020) analyze the removal of bankruptcy flags from credit reports in the U.S after seven to ten years. Our estimated effect of the bans implies employment responses that are similarly sized to the positive employment effects found in Bos et al. (2018) and larger than the null effect in Dobbie et al. (2020) for bankruptcy flag removal. Our effect size is larger than those found for bankruptcy flag removal, perhaps because bans prevent employers from viewing all negative credit information, including missed payments and court judgments.

We also contribute to the broader literature assessing the role of information in hiring decisions. Our context provides an example in which limiting employers' information improves labor market outcomes for the intended beneficiaries (although still with, perhaps, unintended consequences). In contrast, ban-the-box, which prohibits employers from asking about credit history on an initial application, resulted in no increase in employment for those with a criminal history (Rose 2021; Jackson and Zhao 2017), while reducing callback rates and employment for black workers (Agan and Starr 2018, Doleac and Hansen 2020). Other settings have found that adding information sometimes helps (or fails to harm) members of minority groups. Wozniak (2015) shows that drug testing increased employment among low-skilled black men, while Autor and Scarborough (2008) find that job testing had no impact on minority hiring even though members of minority groups attained lower scores. In our setting, we find that one disadvantaged group benefited from the removal of information, though other subgroups may have been harmed.

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2. Data

2.1. State Laws

We gather information on the timing and features of state bans of employer credit checks from the National Conference of State Legislatures and relevant state statutes. Table 1 lists the ten states that enacted bans during our sample period and the effective (not enactment) date of each ban.[6] We also collect information on industries and occupations that are exempt from the bans. Most statutes include reasonably specific exemptions for industries or occupations such as management, finance, insurance, health, and security positions. We classify industries and occupations as exempt or non-exempt, applying the categorization of Bartik and Nelson (2019). Table 1 summarizes this information and Online Appendix Table OA1 provides additional detail. In the empirical analysis, we use this strict coding, which classifies an occupation or industry as exempt only if it is specifically listed in the statutes. We also apply a broader coding that incorporates vague exemptions in some statutes, such as if the employer has a "bona fide purpose" (Connecticut) or the information in a credit report is "substantially job-related" (Oregon, Maryland).

2.2. Survey of Income and Program Participation

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 whether households were suffering financial hardship. A further advantage is that the

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[6] Delaware enacted its ban in 2014, after our sample period ended.

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). Starting from all job separations that begin during the SIPP, we restrict the sample to prime-age individuals who have at least three months of work history in the survey, who are not on temporary layoff, and who report searching for a job. Online Appendix OA provides details on the sample construction. These restrictions leave 10,249 separate unemployment spells in the sample, with a total of 270,439 weekly observations. 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, having higher earnings and, as we explain below, slightly less financial distress, but also higher unemployment rates. In the ban states, unemployment durations are 1.16 weeks longer on average, and the unemployment rate is 1.32 percentage points higher, while individuals earn $165 more per month in pre-unemployment wages. Job seekers in ban states are also more likely to be married and Hispanic and less likely to be black. We include some of these economic and demographic variables as controls in our regressions.

Information on financial hardship comes from the Adult Well-Being interviews in the SIPP, which were conducted between May and August of 2010. These interviews ask households whether they had trouble meeting their essential expenses, such as housing or utility payments.

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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?" We chose to use information about financial distress from as early as possible in the SIPP sample, although the same questions were asked again in late 2011-early 2012. This allows us to observe financial distress before the enactment of most bans and before the start of most unemployment spells, and we examine the sensitivity of our results to excluding spells and bans that occurred before the question about financial distress.

Among the unemployed in the SIPP, 27% report that they are not able to meet all of their essential expenses (26% of spells in ban states, 29% in control states), compared to 18% of the full sample. This results in a sample of 2,888 unemployment spells of financially distressed individuals. We also examine outcomes for 7,361 unemployment spells of non-distressed individuals. Financially distressed job seekers, as shown in Online Appendix Table OA3, have lower pre-unemployment monthly wages than the non-distressed ($1,920 versus $2,520), a slightly lower level of education, and are more likely to be black, Hispanic, or female.

2.3. *Corroborating Information on Financial Distress*

In the SIPP, we use the broadest indicator of financial distress from the questions outlined above - failing to meet essential expenses - in our baseline analysis. The SIPP also asks about specific expenses, detailed in Online Appendix Table OA2, and we investigate the robustness of our results to measuring distress with missed bill payments (utilities and housing payments). The broad indicator serves as a useful proxy for negative information on a credit report for three reasons. First, the more specific questions do not cover delinquent credit card, auto, student, or medical debt, which are important components of credit reports. Second,

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individuals typically miss multiple types of payments, many of which are unobservable in the SIPP. The majority (78%) of our sample of unemployed individuals who fail to meet essential expenses also report missing a rent/mortgage, utility, or telephone payment, and surveys of financially struggling families indicate they often prioritize rent payments and tend to juggle or rotate missing payments on multiple debts (Tach and Greene, 2014; Morduch and Schneider, 2017).[7] Given the correlation across types of payments, it is unlikely that we could isolate the impact of missing specific payment types and so opt for the broadest measure.

Third, because of a similar question asked in the FINRA National Financial Capability Study (NFCS), we can confirm that failing to meet essential expenses is highly correlated with delinquency on types of payments that appear on credit reports. The NFCS is 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, which 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 (weighted) NFCS respondents find it very difficult to "cover your expenses and pay all your bills" compared with the 18% of (weighted) SIPP respondents who report not having been able to meet their essential expenses, suggesting these two measures are similar.[8]

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[7] We do not make use of information on households' outstanding debts to identify those with bad credit. The majority of our sample of unemployed individuals in the SIPP who report failing to meet expenses also report holding outstanding debt (79.5%), so, again, this is highly correlated with the broad measure of distress. Moreover, some of the households that report no outstanding debt may do so because they have recently filed for bankruptcy or are unable to obtain credit because of a poor credit history.

[8] To check whether differences in the period under question in the two surveys (last year vs. typical month) are important, we also check correlations between bad credit indicators and a NFCS question about spending relative to income *over the past year*. As shown in Online Appendix Table OA4, there is also a high correlation between a household's spending exceeding their income over the last year and indicators for bad credit.

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The NFCS also asks about several specific items that appear on credit reports, shown in Table 3.[9] 57% of respondents who found it very difficult to cover their expenses reported subprime credit scores (620 or less), compared to 19% of those who found it somewhat difficult or not difficult (the sample is those who have checked their credit score within 12 months). Additionally, those with difficulty covering their expenses were roughly three times more likely to report bankruptcies, foreclosures, and late payments. Thus, the broad measure of financial distress is highly correlated with several indicators of poor credit history.[10]

3. Empirical Strategy

We use the staggered passage of state credit check bans to estimate their impact on job-finding rates, examining the effects on those with recent trouble meeting essential expenses, whom we refer to as "financially distressed," and those who are not financially distressed. We further investigate the impact of bans with occupation-specific exemptions and the impact of bans on the employment durations of new hires.

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 specification follows the setup of Kroft and Notowidigdo (2016) and Chetty (2008). We model

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[9] The Survey of Consumer Finances also asks questions about family spending exceeded income over the previous 12 months, and, if so, how people made up the difference, including whether they got behind on payments. In the 2016 SCF, only 5% of households said that they postponed payments, filed bankruptcy, or renegotiated debts to make up the difference between spending and income. However, 25% of these same households in the SCF reported that they sometimes got behind or missed payments in the last year, consistent with the pattern observed in the NFCS data, where households with spending that exceeds income are much more likely to be recently delinquent on loan payments.

[10] 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.

the weekly unemployment exit hazard h for person i who has been unemployed in state s for τ weeks, with the spell beginning in month t, as

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

where Ban_{st} is an indicator for an employer credit check ban being in effect in state s at month t.[11] The term X_{ist} represents controls for individual characteristics, and δ_s and τ_t are state and month fixed effects.[12] 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.[13] In all specifications, we cluster our standard errors at the state level, and we subject our main estimates to tests obtained through randomization inference.

The key explanatory variable in our model is Ban_{st}, an indicator for whether the unemployed spell begins in a month after state s has enacted a credit check ban. The coefficient β represents the change in the log of the job-finding hazard rate when credit is banned, after controlling for individual characteristics X_{ist} and state and year fixed effects. Another interpretation is that $-\beta$ is approximately equal to the change in the log of the unemployment

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[11] Our observations are unemployment spells 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. Including individual fixed effects is infeasible, as there are only 37 financially distressed and 133 non-financially distressed individuals that have both a pre-ban and a post-ban spell.

[12] 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 (2019) 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.

[13] 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. This "on seam" indicator is the only time-varying coefficient in the Cox proportional hazard model. All estimates are similar if we estimate a Cox proportional hazard model with only time-invariant coefficients.

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duration.[14] If credit check bans increase job-finding rates of distressed individuals, the effect of

the bans (β) will be positive. The non-distressed, however, may suffer, especially if they

resemble the distressed in other observable ways, because they are now forced to pool with the

distressed, or because they are squeezed out of jobs in the short-term that now go to the

financially distressed; these possibilities imply a negative effect of the bans (β) on this group.

The causal interpretation of our estimates of β for the financially distressed 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).[15] While this assumption is not directly

testable, we provide several checks of its plausibility. First, as mentioned, we estimate the effect

of the bans on the non-distressed, who might be harmed by a credit check ban. If this yielded an

estimate similar to that of the distressed, it would suggest that general improvements in the labor

markets of ban states might explain our results. Instead, we find an insignificant and small effect

of bans on the non-distressed.

Second, given that we find little effect on the non-distressed, we make use of them as a

within-state comparison group. This approach tests whether the changes in job-finding rates

among the distressed differ relative to 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,

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[14] 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:

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

[15] 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, and 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.

as we noted above, the non-distressed are not a pure control group. We estimate the difference in weekly hazard rates among financially distressed individuals (indicated by *Distress*) 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:

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

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).

We also make use of the exemptions from credit check bans in certain industries and occupations and implement an event study specification that includes leads of the treatment variable to test whether the differences in job-finding rates are present before the effective date of the credit check bans. Lastly, 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. Results

We report results from a series of hazard models to investigate how employer credit check bans affect unemployment durations for financially distressed job seekers. Our identification strategy uses the staggered passage of bans to compare changes in the job-finding hazard rate among individuals in states with and without bans. We then apply a similar strategy to examine the impact of the bans on the employment durations of new hires.

4.1. *Impact on Job-Finding Rates*

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We begin with 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 curves show the probability of remaining unemployed after t weeks for those with and without a history of financial distress. Before the bans, distressed job seekers are more likely to remain unemployed after t weeks of searching (as the survival curve for the distressed is consistently above the curve for the non-distressed). 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 similar and the log-rank test does not reject equality (p=0.736).[16] The shift in the survival curve suggests that bans improve the job-finding rates of financially distressed job seekers, and we explore this pattern formally in the remainder of this section.

4.1.1. Baseline Results

Table 4 reports the estimates from the Cox proportional hazard model in equation (1). Column 1 reports our main specification for the sample of financially distressed individuals in the ban and non-ban states. The coefficient on Ban_{st} of 0.28 indicates that bans increase the job-finding hazard of distressed individuals by 28% or, equivalently, reduces their expected unemployment durations by 28% (significant at the one-percent level). In Online Appendix OB, we confirm the statistical significance of the estimates using the randomization inference test proposed by MacKinnon and Webb (2020).[17]

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[16] To assess whether these differences in significance are due to the fact that there are fewer post-ban observations (969 vs. 1,678), we randomly selected 10 subsamples of 969 pre-ban 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.

[17] MacKinnon & Webb's (2020) proposed randomization inference approach using t-statistics performs well for a difference-in-differences setting with an unbalanced panel and relatively few treated clusters. We estimate a series

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The magnitudes of the estimates indicate that bans do much to narrow the gap in job-finding and separation rates between the distressed and non-distressed. In states that enacted bans, the pre-ban probability of a financially distressed job seeker finding employment within three months was 21.5%, while for the non-distressed it was 27.2%. The estimated 28% increase in the job-finding rate closes this gap, as is seen in the unemployment survival curves of Figure 1, which are statistically indistinguishable in the post-ban regime. The resulting reduction in unemployment durations is also economically significant. Applying this increase in the job-finding rate to the average unemployment duration of 26 weeks, individuals find jobs roughly seven weeks earlier and earn, on average, an additional $3,700.[18]

While the job-finding rates of the financially distressed improve, bans may harm non-distressed job seekers who can no longer distinguish themselves from distressed applicants. Column 2 examines this possibility by estimating the same specification for non-distressed individuals. In this case, the coefficient on Ban_{st} of 0.038 is small, positive, and statistically insignificant, indicating little change to the job-finding rates of non-distressed individuals living in ban states, though the width of the confidence intervals does not allow us to rule out decreased job finding. We also examine overall changes in job-finding rates for the combined sample of distressed and non-distressed in column 3, finding a positive but statistically insignificant effect.

Labor market trends correlated with bans would bias estimates of the changes in job-finding rates – biasing them upwards if labor markets were improving in locations that

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of 1,000 placebo specifications, each with the pattern of credit check bans randomly assigned to states. The actual test statistic based on the change in job-finding rates for the financially distressed is larger than 96.7% of placebo test statistics generated by randomly assigning laws to states. For non-distressed individuals, the test statistic is larger than only 23.4% of placebo test statistics.

[18] This calculation multiplies the seven-week reduction in unemployment duration by the mean post-unemployment weekly wage of $529.

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implemented bans or downward if labor markets were deteriorating (or not improving as fast) in those locations. Column 4 investigates this possibility by estimating equation (2), which uses the non-distressed as a within-state comparison group for the distressed and includes state-by-year fixed effects to control for changes in state-level labor market conditions. The coefficient on *Distress × Ban* captures the change in job-finding rates of the financially distressed relative to the change among the non-distressed in the same state and year (conditional on other controls). As noted earlier, the bans potentially affect job-finding rates for both groups, so the coefficient *Distress × Ban* estimates the difference in bans' effects on the two groups. The estimate of 0.284 (significant at the 1% level) is similar to the column 1 estimate for the distressed group and indicates that unemployment durations of financially distressed individuals fall by 28% relative to non-distressed individuals in the same state and year (after conditioning on other time-varying state-specific factors) after a credit check ban is established.

4.1.2. *Job-Specific Exemptions*

Every employer credit check ban exempts some industries and occupations, and we would not expect bans to help those looking for exempt jobs. Following Bartik and Nelson (2019), we use the industry and occupation of unemployed individuals' prior job to group workers into those seeking exempt or non-exempt employment. Since individuals tend to search in industries and occupations where they have prior experience, bans will have less of an impact on distressed job seekers whose last job was in an exempt industry or occupation.[19] The

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[19] We confirm in Online Appendix Table OA5 that individuals whose last job was exempt (job-specific measure) are significantly (20 percentage points) more likely to find an exempt position within one year.

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percentage of unemployment spells by individuals who last worked in an exempt position ranges

from 40.9% in Maryland to 13.2% in Nevada.[20]

For the unemployment spell *i* that began at time *t* in state *s* of a person with the job *j(i)*

prior to unemployment, we estimate the following Cox proportional hazard model:

(3) $$\log(h_{ist}(\tau)) = \log(h_0(\tau)) + \beta_0 Ban_{st} + \beta_1 Ban_{st} * Exempt_{s,j(i)} + X_{ist}\gamma +$$

$$\delta_s * Exempt_{s,j(i)} + \tau_t.$$

$Exempt_{s,j(i)}$ is an indicator for whether the individual's prior job *j(i)* is exempt from the credit

check ban in state *s*. The coefficient β_0 reflects the impact of bans on those who held non-

exempt positions, while $\beta_0 + \beta_1$ reflects the impact on those who last held exempt positions.

Since bans matter less for those searching for exempt jobs, we expect β_1 to be negative. We also

include individual-level controls X_{ist}, state-by-exempt-status fixed effects, and time (year-

month) fixed effects. In some specifications, we replace the state-job-specific exemption

measure with $JobExempt_{j(i)}$, an indicator for whether the past job j(i) is exempt in any state

statute banning credit checks at any time during our sample. This broader measure addresses the

fact that we ignore vague exemptions in some states (such as an exemption if credit history is

"substantially job-related"), as discussed in Section 2, which may lead us to underestimate

exempt jobs with the state-job-specific measure. Differential labor market trends between

exempt and non-exempt jobs may bias estimates from equation (3), so we also estimate a

specification allowing time-by-$JobExempt_{j(i)}$ fixed effects.

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[20] If we ignore the vague exemptions, the number of unemployed individuals in the SIPP who last worked in an exempt position to between 35% in Colorado to 0% in Washington with Oregon and Maryland also less than 10%. These differences do not substantially alter the estimated effect on job-finding rates. Online Appendix Table OA1 lists the share of unemployed that previously held exempt jobs for each state that enacted a ban.

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Table 5 reports the results from equation (3). The positive effects of credit check bans among the distressed are concentrated on those whose past jobs were not exempt. The estimate of β_0 in column 1 indicates that the bans significantly increase job-finding rates (by 29%) for financially distressed individuals whose past job was not exempt. For those whose last job was exempt, the estimated effect is negative at -0.071 ($\beta_0 + \beta_1$) and not statistically significant. Columns 2 and 3 use the job-specific (rather than state-job-specific) measure of exemptions and add time-by-$JobExempt_{j(i)}$ fixed effects, with little change to the estimates. The opposite pattern holds for non-distressed job seekers in columns 4-6. Non-distressed individuals whose past job was non-exempt experience (insignificant) declines in job-finding rates, while those whose past jobs are exempt from credit check bans experience increases in job-finding rates of 20-40 percent ($\beta_0 + \beta_1$), with the magnitude declining and becoming insignificant when we add interactions of exempt jobs with either state or year-month dummies.

As expected, therefore, bans improved job-finding rates for distressed job seekers who are likely to be seeking jobs in non-exempt industries and occupations. The improved job-finding rates for non-distressed applicants with a previous job in an exempt industry or occupation, which are significant in the base specification and sizeable throughout, is surprising since the bans have no direct effect on this group or these exempt positions. One possible explanation is a degree of labor market specialization. If financially distressed individuals now focus their job search on non-exempt positions, individuals with good credit and work experience may now face less competition within the exempt industries. Moreover, Cortés et al. (2020) finds that vacancy postings of exempt positions rise relative to those of non-exempt positions, which may also increase job-finding rates for the non-distressed in the exempt industries. While we do not observe application or search behavior, we do examine whether the bans altered transition rates

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into exempt and non-exempt jobs. Online Table OA5 shows that, after the ban, financially

distressed job seekers are less likely to obtain an exempt position and significantly more likely to

obtain a non-exempt position. Non-distressed job seekers, in contrast, are more likely to obtain

an exempt position, with little change in the probability of obtaining a non-exempt position.[21]

4.1.3. Event Study

Next, we conduct an event study analysis to detect pre-existing trends in unemployment

durations in treatment states. We estimate a version of equation (1) for the non-distressed and

add leads and lags of the Ban_{st} indicator:

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

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. 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. 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. To keep the panel balanced, we only include ban states CA, CT, IL, and MD,

which all have at least a 2.5-year pre-period and 1-year post-period, and we drop observations

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[21] The estimates in Online Appendix Table OA5 are statistically significant when including state and year fixed effects, but are insignificant though of similar magnitude when adding industry and occupation fixed effects.

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more than 1-year after the ban. We include the same controls as previously and add annual, state-level controls for the unemployment rate.[22]

Figure 2 plots the point estimates and 95% confidence intervals on the leads and lags for the distressed and non-distressed samples, and Online Appendix Table OA6 reports estimates and standard errors. For the financially distressed, the leads are generally small and not statistically different from zero, while the 6-month post-ban coefficient is positive and similar in magnitude to the double-difference estimate for all states (0.258 compared to 0.28). The post-ban estimates from the non-distressed sample are smaller and the longer-run post-$Ban_{s,t-6}$ coefficient is close to zero (0.0275). Thus, the results for the non-distressed display an absence of important pre-ban trends in job finding, while the results for the distressed suggest the same, but with wide confidence intervals.

4.1.4. Extensions and Robustness

The Online Appendix includes several additional robustness checks, which we summarize here. We first investigate the sensitivity to additional controls and concerns related to changes in selection into unemployment and the sensitivity to alternative samples. We then examine the sensitivity to alternative samples and definitions of financial distress. Finally, we examine heterogeneity in their impact in states with different labor market conditions.

Additional Individual, Economic, and Policy Controls

The bans may coincide with other changes in economic and legal conditions, including the Great Recession and resulting expansion of unemployment insurance. Online Appendix

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[22] The coefficients are similar if we include the full time period and all ban states (Online Appendix Figure OA1). The pattern of coefficients is also similar when we do not control for the state-level unemployment rate, though slightly less precise. We examine the robustness of the main results to state-level economic controls in Section 4.1.4.

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Table OA7 investigates the sensitivity of the estimates to the inclusion of controls for additional individual characteristics (race, ethnicity, industry, occupation, pre-unemployment wages) and state-level economic conditions (unemployment rate, home prices, manufacturing employment) and policy changes (max unemployment insurance, ban-the-box, Medicaid expansions). The estimated coefficient on *Ban* from equation (1) remains similar to the baseline estimate of 0.28 in Table 4 column 1, increasing to 0.36 with all controls included.

Selection into Unemployment

Bans may cause or be correlated with changes in the types of individuals who enter unemployment. In Online Appendix OC, we apply the method of Oster (2017) to calculate a bias-adjusted estimate of the effect under the assumption that selection on unobservable traits is proportional to the selection on observable traits. Our bias-adjusted coefficients are similar to the baseline coefficient, and larger when all observable controls are included.

The Timing of Financial Distress and Bans

We examine the sensitivity of the estimates to changes in the timing and definition of financial distress. The questions about financial distress were collected two years after the 2008 SIPP survey began, so some instances of financial distress may be caused by the unemployment spell. In Online Appendix Table OA8 (columns 3 and 4), we find similar results using the subset of unemployment spells that begin after individuals were asked about financial distress. We also find similar estimates when redefining financial distress as those that miss rent or utility payments (Online Appendix Table OA9). Additionally, the results are not sensitive to individually excluding each state that enacted a ban (Online Appendix Table OA10) or to excluding the three states that passed bans before financial distress was measured in the SIPP (Online Appendix Table OA11).

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Matched Sample of Non-Distressed Job Seekers

The estimates shown earlier in Table 4 compare the effects on distressed and non-distressed workers, and we find no negative effects of the bans on non-distressed job-finding rates. Non-distressed workers as a whole, however, may not serve as an adequate control group and the negative effects may be concentrated in a subset of non-distressed job seekers who are observably similar to distressed job seekers. We use propensity score matching to form a sample of non-distressed job seekers who are observably similar to the distressed on several economic and demographic characteristics described in Online Appendix Table OA3. This pre-processing can generate a more appropriate comparison group and more accurate estimates of the treatment effect (Ho, Imai, King, and Stuart 2007, Ferraro and Miranda 2014). When using the matched sample of non-distressed in Online Appendix Table OA8, the coefficient on *Ban* is now negative, at -0.0788 (s.e. 0.135), providing suggestive evidence of declines in job-finding rates among this subgroup of non-distressed job seekers.

Heterogeneity Across Labor Market Conditions

The timing of the bans raises the question of whether the effects that we estimate are specific to the period around the Great Recession or may vary across the business cycle. Online Appendix Table OA12 investigates heterogeneity in the effect of bans by interacting the ban indicators in equation (1) with the state unemployment rate. For the financially distressed, the implied effect in low-unemployment states (u = 6.9%) and high-unemployment states (u = 10.7%) are nearly identical. For the non-distressed and the overall effect, however, the gap between low- and high-unemployment states is meaningful, with bans increasing job-finding rates in states with high unemployment and reducing job-finding rates in states with low unemployment. The differences, however, are imprecise and never statistically different from

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zero or each other. A limitation with this analysis is that, although there is some variation in unemployment rates across states, our sample covers a period of generally high unemployment and the relatively short duration of the SIPP prevents us from observing longer-run outcomes in tighter labor markets.

4.2. Impact on Employment Durations

Credit check bans make it easier for the distressed to find jobs, but limiting information in hiring decisions could undermine the quality of matches between employers and employees. We use the same strategy to examine the impact of the bans on measures of match quality available in the SIPP: the duration of employment and the probability of separation, including information on particular reasons that separations occur. We observe, at most, the first few years of post-ban employment, so our duration analysis focuses on early separations and, when investigating the probability of separation, we explicitly consider separations that occur in the first year.

4.2.1. Employment Durations

We form a sample of employment spells that begin when a job seeker transitions from unemployment to a job. This flow sampling restricts our sample to employment spells beginning in 2008 or later, and, because the SIPP covers 2008-2013, to only the initial years of those spells. Examining these initial years still offers an indication of match quality, since around one-third of employment spells end within the first quarter of employment and the job-exit hazard rate declines sharply during an employment spell (Hyatt and Spletzer 2017; Pries and Rogerson

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2019). For each spell, we calculate the number of weeks spent at that employer.[23] Online

Appendix OA provides more information on the sample construction and Online Appendix Table

OA13 reports the summary statistics. Our final sample consists of 9,313 employment spells, with

2,574 from individuals reporting financial distress.

With these employment spells, we model the weekly employment exit hazard h for

person i who has been employed in state s for τ weeks, with the employment starting in month t,

as

(5) $$\log(h_{ist}(\tau)) = \log(h_0(\tau)) + \beta Ban_{st} + X_{ist}\gamma + \delta_s + \tau_t.$$

The key explanatory variable is whether the employee started the job in a state and time when a

credit check ban was in effect, Ban_{st}. If $\beta > 0$, it would indicate that the employment spells end

more quickly and suggest that banning credit reports reduces match quality. Alternatively, if

banning credit reports have little impact on match quality, we would expect to see $\beta \approx 0$. Finally,

the credit check bans may allow financially distressed individuals to move into higher quality or

more stable jobs, resulting in fewer early exits and $\beta < 0$. As before, the baseline model also

includes an "on seam" indicator, controls for age, gender, education, and marital status, as well

as state and month fixed effects.

Table 6 reports the estimates from the Cox proportional hazard models for the weekly

job-exit hazard rate. Column 1 reports our main specification for the sample of financially

distressed individuals in the ban and non-ban states.   The coefficient on Ban_{st} is -0.27 and is

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[23] An employment spell ends when the job finder no longer reports working for that specific employer. Since
employment spells are generally longer than unemployment spells, we do not truncate the employment spells at 50
weeks like we do the unemployment spell. However, the estimates are similar if we censor the employment spells at
50 weeks. The estimates are also similar if we define the end of an employment spell as a transition to non-
employment rather than a separation from a specific employer.

statistically significant, indicating that financially distressed new hires in a state with a credit check ban have a lower job-exit hazard and longer expected employment durations than those hired without a ban, conditional on the other controls. Column 2 reports the same specification for non-distressed individuals, finding little change, and column 3 reports estimates for all the unemployed by combining the distressed and non-distressed samples. Finally, column 4 reports similar estimates when using both samples to control for state-year-specific shocks within equation (2).

4.2.2. *Separations from Employment*

In addition to employment durations, we also examine the types of exits that occur within the first year of employment, for example, involuntary separations, quits, and job-to-job flows. When a job spell ends during a wave of the SIPP (as opposed to occurring in between waves), the SIPP asks the respondent about why the job ended. [24] We estimate a linear probability model, where the dependent variable is an indicator for whether a given transition occurs within the first 52 weeks of employment.

Table 7 reports the estimates for financially distressed (panel A) and non-distressed (panel B) new hires. The dependent variable in column 1 is an indicator for whether an involuntary separation (layoffs, discharged/fired, business failure, temporary job) occurred during the first 52 weeks of employment. Financially distressed new hires are 12.9% less likely to have an involuntary separation when the bans are in place. The two types of involuntary separations - layoffs/discharges and job or business endings (reflecting either temporary jobs or business closures) – contribute equally to the overall decline in involuntary separations for the

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[24] As noted earlier, a significant share of transitions in the SIPP occur on the "seam" between waves.

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distressed (columns 2 and 3).[25] In column 4, quitting or leaving for personal reasons (e.g., school, retirement, childcare) declines for the distressed and the non-distressed. Finally, using the observed employment status in the month following the job separation, columns 5 and 6 examine whether the respondent transitions into employment or to non-employment (unemployment or out of the labor force). The distressed were more likely to transition to another job and less likely to transition to non-employment following the bans, with little change in the transition rates for the non-distressed.

Overall, the extended employment durations and declines in involuntary separations suggest that, after the bans, match quality improves among distressed new hires.[26] That banning information would increase match quality is surprising. One possibility is that credit reports contain little relevant information about match quality, so little is lost when credit reports are banned; external empirical evidence has found little correlation between credit reports and employee performance.[27] This, however, raises questions about why employers use credit reports in hiring and it would not explain the *increased* employment durations. Alternatively, some changes in match quality may not affect our measures of employment duration. For example, credit report information may predict costly but low probability events, such as embezzlement,

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[25] Of the observed layoffs/discharges in our sample of new employment spells for the financial distressed, over 70% were layoffs rather than firings. Of the job/business endings, over 80% were because the job was temporary and ended. We do not run regressions on these distinct subcategories because the samples become too small.

[26] The estimates do not compare differences between distressed and non-distressed new hires, so they are not informative about differences in average match quality between these two groups.

[27] Weaver (2015) finds that the character-related portion of credit reports does not predict employee performance as measured by wage growth. Dobbie et al (2020) find no correlation between previous bankruptcy filings and employment duration. Using information on employees at specific firms, Bryan and Palmer (2012) and Oppler, Lyons, Ricks & Oppler (2008) find little correlation between credit report data and job performance.

that are too infrequent to detect with changes in average employment duration.[28] A more comprehensive analysis of the impact of credit check bans on match quality is an important area for future work.

4.2.3. Extensions and Robustness

In the Online Appendix, we provide a series of robustness checks for our estimates. Event study estimates in Online Appendix Figure OA2 show that the decline in the job-exit hazard for the financially distressed occurs for those who find jobs after the ban goes into effect. Employment durations increase most in non-exempt positions (Online Appendix Table OA14) and the estimates are robust to additional economic, policy, and demographic controls (Online Appendix Table OA15). Online Appendix Table OA16 uses other measures of job quality (wages, full-time status, and salaried work). The point estimates are generally positive, but not statistically significant. Lastly, we investigate self-employment and geographic mobility as outcomes in Online Appendix Table OA17. Among financially distressed workers, we find little change in the stock of self-employed or transitions from unemployment into self-employment. Distressed job seekers are less likely to change residence during an unemployment spell, with the decrease driven by fewer intrastate moves, perhaps because they more quickly find a job in the local labor market.

5. Discussion and Conclusion

This paper evaluates the effect of recent employer credit check bans on the labor market outcomes of financially distressed job seekers. We find that the bans improve the job-finding

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[28] Kiviat (2019), interviewing 57 hiring managers, reports reasons include reducing the probability of theft, embezzlement, or other criminal activity, reducing the legal liability for negligent hiring and making inferences about honesty and moral character.

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rates of the financially distressed and reduce the probability that distressed new hires experience quick involuntary separations. For non-distressed job seekers, we find small, statistically insignificant changes, though we cannot reject small declines in their job-finding rate.

Our focus on the impact of the bans on the intended beneficiaries – financially distressed job seekers – complements recent papers also examining the impact of these bans (Ballance et al. 2020; Bartik and Nelson 2019; Cortés et al. 2020). These papers examine other outcomes (total employment, vacancy postings) and largely focus on unintended consequences for different subgroups (minority groups, young workers). Some use survey data to examine employment (Ballance et al. 2020 in their analysis of the American Community Survey) or worker flows (Bartik and Nelson 2019, in their analysis of the Current Population Survey panel); some use aggregated employer-based data on job postings (Cortés et al. 2020); and some use administrative data on worker and job flows (Bartik and Nelson 2019, who use the LEHD Job-to-Job data, and Ballance et al. 2020 who use the LEHD Origin-Destination Employment Statistics). We summarize how our estimates and analysis relate to these papers here and provide a detailed comparison in Online Appendix OD.

Generally, all papers agree that the bans caused little to no change in overall job-finding rates, separation rates, or employment in the population as a whole. Our 28% increase in the job-finding rate for distressed job seekers would increase the (steady-state) share of distressed individuals that are employed by about 2 percentage points. [29] Since only 18% of the labor force

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[29] Steady-state unemployment rates can be approximated as $\frac{s}{s+f}$, where s is the monthly job-separation rate and f is the monthly unemployment exit rate (see Shimer, 2012, for example). We apply the 28% increase to f, with f=0.17 and s=0.018, which were the average unemployment exit and job-separation rates for 2009 (Bureau of Labor Statistics). The steady-state employment rate would be higher if separation rates fall, but our estimated change in separation rates applies only to new hires and results in little change in the total separation rate.

is financially distressed, the impact on overall employment is limited. The most directly comparable results are those of Bartik and Nelson (2019) using the Current Population Survey (CPS). Like us, they find statistically insignificant changes in overall job-finding rates, and our point estimates are quite close when we replicate the sampling features of the CPS. We also find little change when directly estimating the effect of bans on overall employment and separation rates (Online Appendix Table OA24 and Table OA25). Similarly, Ballance et al. (2020) and Cortés et al. (2020) find little change in total employment or the unemployment rate.

Although the changes to overall labor market outcomes are consistently small, there are meaningful changes for some subgroups. Our estimates are consistent with the results of Ballance et al. (2020), which finds increased employment in Census tracts with very low average credit scores. Additionally, Corbae and Glover (2017) rationalizes the magnitude of our main estimate within a quantitative equilibrium search model. Their calibrated model implies that bans increase the job-finding rates of those with bad credit (bottom quintile) by 27%, quite similar to our estimated 28% increase.

Others find that bans harm people who are observably similar to those with bad credit, namely young workers and black workers, consistent with statistical discrimination. Ballance et al. (2020) uses 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. Bartik and Nelson (2019) finds that the bans reduced job-finding rates and increased job-separation rates for black individuals. We are unable to persuasively replicate or reject these effects on black workers. The SIPP contains relatively few black job seekers in the ban states, and our estimates of heterogeneity by race are imprecise and sensitive to dropping certain states (Online Appendix Tables OA18 and OA19).

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Outside of the context of these specific bans, our results relate to papers examining changes in labor market outcomes as specific negative information is deleted from credit reports. In Sweden, Bos, Breza, and Liberman (2018) finds substantial positive employment effects on pawnshop borrowers when records of default are removed after three years. In the U.S., though, the evidence suggests smaller or zero effects on employment when bankruptcy flags are removed from credit reports after 7-10 years. (Herkenhoff et al. 2020, Dobbie et al. 2020).[30] Our estimate for distressed job seekers implies employment responses that are similarly sized to those in Bos et al. (2018) and larger than the null effect of bankruptcy flag removal in Dobbie et al. (2020). [31]

The effects may differ because flag removal and credit check bans generate different types of responses. When an individual's bankruptcy flag is removed, it changes employers' information about that one individual. Credit check bans, in contrast, prevent (non-exempt) employers from viewing credit reports from all individuals, which may induce larger or general equilibrium responses. At the same time, bankruptcy flag removal scrubs the record from the information set of employers, landlords, lenders, and anyone else viewing that individual's credit report. This may induce changes in mobility and borrowing (as found in Herkenhoff et al. 2020) that would not be present with credit check bans, which only apply to employers.

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[30] Herkenhoff et al. (2020) find that upon the removal of a bankruptcy flag, there are more transitions into self-employment and also from self-employment into formal employment. Dobbie et al. (2020), linking bankruptcy filings to U.S. Social Security Administration employment records, find precise zero effects of bankruptcy flag removal on employment and earnings.

[31] While most of Dobbie et al. (2020) focuses on the removal of bankruptcy flags, a robustness check uses variation from the first 4 states that enact a ban to examine changes in annual employment of bankruptcy filers. We replicate their analysis in Online Appendix Table OA20 and Figure OA4, also finding no significant employment effects. Thus, our results indicate that the increases in weekly job-finding rates may not be apparent in coarser annual data. Differences in the point estimates may reflect that our sample consists of individuals in financial distress whereas Dobbie et al. (2020) examine outcomes specifically for those with a bankruptcy filing 4-6 years prior.

The smaller response to bankruptcy flag removal could also be because bankruptcies are less important for hiring decisions. Firms place more weight on non-bankruptcy negative credit events; across different job categories (e.g. senior executive, access to confidential information), 31-61% of employers listed accounts in debt collection as the most important credit report factor compared to 3-17% listing previous bankruptcies (SHRM 2012). Moreover, even after bankruptcy flag removal, most former bankruptcy filers continue to have poor credit reports, which could continue to hinder their job search. [32] The bans we examine prevent employers from seeing any of this negative credit information, including recent delinquencies, court judgments, and collections.

Comparing the results of these papers suggests that the impact of credit check bans may vary depending on what information is banned and who is subjected to the bans. Still, one implication of this paper is that it is possible for information bans to improve labor market outcomes for the intended beneficiaries. This result for credit check bans differs from the effects of ban-the-box policies, which prohibit employers from asking about criminal history on an initial application but resulted in no increase in employment for those with a criminal history (Rose 2021; Jackson and Zhao 2017). A key difference between these policies is that credit check bans prevent the employer from ever accessing credit reports, while ban-the-box only prevents employers from asking about criminal histories on the initial application. As discussed in Rose (2021), employers may simply defer criminal background checks until later in the

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[32] In Dobbie et al. (2020), removal of the Chapter 13 bankruptcy flag increases credit scores by roughly 10 points from a base of 596.5 in the year prior to removal. This is still quite low; the national mean credit score is 686 and the 25th percentile 607. Dobbie, Goldsmith-Pinkham, and Yang (2017) show that many bad credit indicators persist after bankruptcy. During the post-filing years, among approved (dismissed) Chapter 13 filers, 58.7% (59.6%) have delinquencies, 43% (58.4%) have collections, and 14.8 (21.6%) have charge-offs on their credit report.

interview process. Thus, for information restrictions to be successful, it may be important to truly ban the information rather than simply delaying access to it.

Although we find that the bans benefit financially distressed workers, a full welfare analysis must weigh these benefits against the costs associated with the bans. As mentioned, other recent work has documented worse labor market outcomes for certain groups, namely young workers and black workers, and Cortés et al. (2020) finds declines in job postings for non-exempt positions relative to exempt positions. The bans may also affect match quality in ways that are not captured by our analysis of employment durations, and a more complete analysis of the impact on match quality is an important area for future work. Finally, our analysis is limited to the effect of bans during a high-unemployment period and during the few years immediately following the bans. The costs and benefits may vary with labor market conditions or in the longer run.

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Table 1 State Employer Credit Check Bans

State	Effective Date	Exempt Occupations	Exempt Industries	Vague Exemption
Washington	7/22/2007	None	None	Yes
Hawaii	7/18/2009	Mgmt	Finance	Yes
Oregon	3/29/2010	Law Enf., Confid., Airport Sec.	Finance	Yes
Illinois	1/1/2011	Mgmt, Law Enf., Confid., Fid.	Finance, Law Enf., Debt Coll., Govt	No
Connecticut	10/1/2011	Mgmt, Confid., Fid.	Finance	Yes
Maryland	10/1/2011	Mgmt, Confid., Fid.	Finance	Yes
California	1/1/2012	Mgmt, Law Enf., Confid., Fid.	Finance	No
Vermont	7/1/2012	Mgmt, Law Enf., Confid., Fid.	Finance	No
Colorado	7/1/2013	Mgmt, Law Enf., Confid., Fid.	Finance, Law Enf., Space, Nat. Sec.	No
Nevada	10/1/2013	Mgmt, Law Enf., Confid., Fid.	Finance, Gaming	No

"Confid." refers to occupations with confidential information, "Fid." refers to occupations with fiduciary duties. Online Appendix Table OA1 provides more details on the specific duties, occupations, and industries included in each category.

Table 2 Summary Statistics for the Sample of Unemployment Spells

	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.	2,647		7,602			

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). Statistics for unemployment durations include censored observations. The unemployment rate is the state unemployment rate at the start of the unemployment spell.

Table 3 Financial Distress and Credit Scores

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

The table reports means from the FINRA Investor Education Foundation 2009 State-by-State National Financial Capability Study, comparing responses among individuals who find it "very difficult" to meet expenses to those who find it "somewhat difficult" or "not difficult." Observations are weighted to be nationally representative. Individuals with missing values or answering "Don't know" or "Prefer not to say" are dropped.

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

	Distressed (1)	Non-distressed (2)	Overall (3)	Distressed relative to Non-distressed (4)
Ban	0.280***	0.0376	0.0896	
	(0.0958)	(0.0859)	(0.0821)	
Distress × Ban				0.284***
				(0.103)
Observations	77,487	192,952	270,439	270,439
Number of Unemployment Spells	2,888	7,361	10,249	10,249
Year-month FE	X	X	X	
State FE	X	X	X	X
Year FE				X
State × Financial Distress FE				X
Year × Financial Distress FE				X
State × Year FE				X

All columns report coefficient estimates from semiparametric Cox proportional hazard models of the job-finding hazard rate. All specifications include an "on seam" indicator and controls for age, gender, years of education, and marital status. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table 5 Heterogeneity by Exemption Status of Previous Job

	Distressed			Non-distressed		
	(1)	(2)	(3)	(4)	(5)	(6)
Ban (β_0)	0.291***	0.306***	0.327***	-0.0639	-0.0346	-0.0555
	(0.0919)	(0.0979)	(0.105)	(0.0971)	(0.101)	(0.105)
Ban × Exempt (β_1)	-0.362			0.493***		
	(0.277)			(0.177)		
Ban × JobExempt		-0.314**	-0.404		0.229	0.272
		(0.149)	(0.366)		(0.153)	(0.177)
Observations	77,487	77,487	77,487	192,952	192,952	192,952
$\beta_0 + \beta_1$	-0.0710	-0.00815	-0.0775	0.429	0.194	0.217
p-value	0.811	0.963	0.839	0.00431	0.148	0.128
Demographic Controls	X	X	X	X	X	X
Year -month FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
State × exempt (state-job) FE	X			X		
State × exempt (job) FE		X	X		X	X
Year-month × exempt (job) FE			X			X

All regressions report estimates from the Cox proportional hazard model from estimating equation (3). In columns 1 and 4, "Exempt (state-job-specific)" is an indicator for whether an individual's most recent job was exempt from the credit check bans in his or her state. In the remaining columns, "Exempt (job-specific)" is an indicator for whether the past job was exempt from credit check bans in any state. "Demographic controls" consist of controls for on seam, age, marital status, years of education, and sex. The table also reports the sum of the Ban and Ban × Exempt coefficients and the corresponding p-value from a Wald test of the coefficient sum being equal to zero. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table 6 The Impact of Bans on Weekly Job-Exit Hazards

	Distressed	Non-distressed	Overall	Distressed relative to Non-distressed
	(1)	(2)	(3)	(4)
Ban	-0.270***	0.0144	-0.0543	
	(0.0981)	(0.0977)	(0.0790)	
Distress × Ban				-0.296*
				(0.153)
Observations	108,938	329,554	438,492	438,492
Number of Employment Spells	2,574	6,739	9,313	9,313
Year-month FE	X	X	X	
State FE	X	X	X	X
Year FE				X
State × Financial Distress FE				X
Year × Financial Distress FE				X
State × Year FE				X

All columns report coefficient estimates from semiparametric Cox proportional hazard models of the job-exit hazard rate. All specifications include an "on seam" indicator and controls for age, gender, years of education, and marital status. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table 7 The Impact of Bans on Separation Probabilities for New Hires

	Involuntary Separation (1)	Layoff / Discharged (2)	Job / Business Ended (3)	Quit / Personal Reason (4)	Transition to Employment (5)	Transition to Non-Employment (6)
Panel A. Financially distressed						
Ban	-0.129**	-0.0637**	-0.0656*	-0.0229	0.0393	-0.141***
	(0.0516)	(0.0298)	(0.0368)	(0.0514)	(0.0680)	(0.0504)
Observations	2,073	2,073	2,073	2,073	2,073	2,073
Panel B. Non-distressed						
Ban	0.0116	0.0302*	-0.0186	-0.0470**	0.00659	-0.00268
	(0.0248)	(0.0162)	(0.0121)	(0.0233)	(0.0345)	(0.0239)
Observations	5,423	5,423	5,423	5,423	5,423	5,423
State FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X

Dependent variables are indicators for separations within the first 52 weeks of employment. All regressions control for age, sex, education, and marital status. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Figure 1 Survival Curves (Probability of Remaining Unemployed, by Week)

Survival Curves before Bans

--- Distressed   ——•—— Non-Distressed

Survival Curves after Bans

--- Distressed   ——•—— Non-Distressed
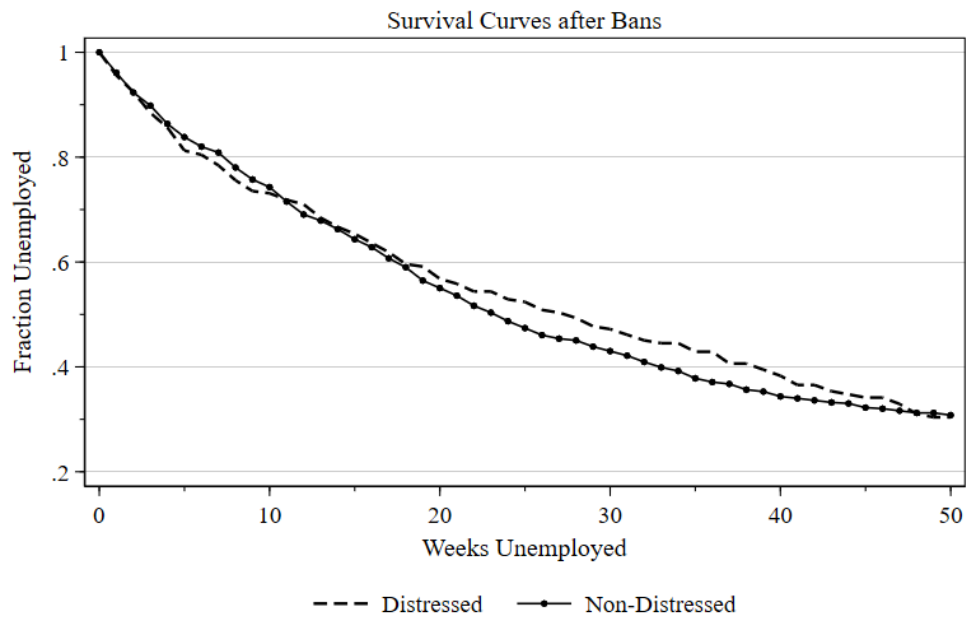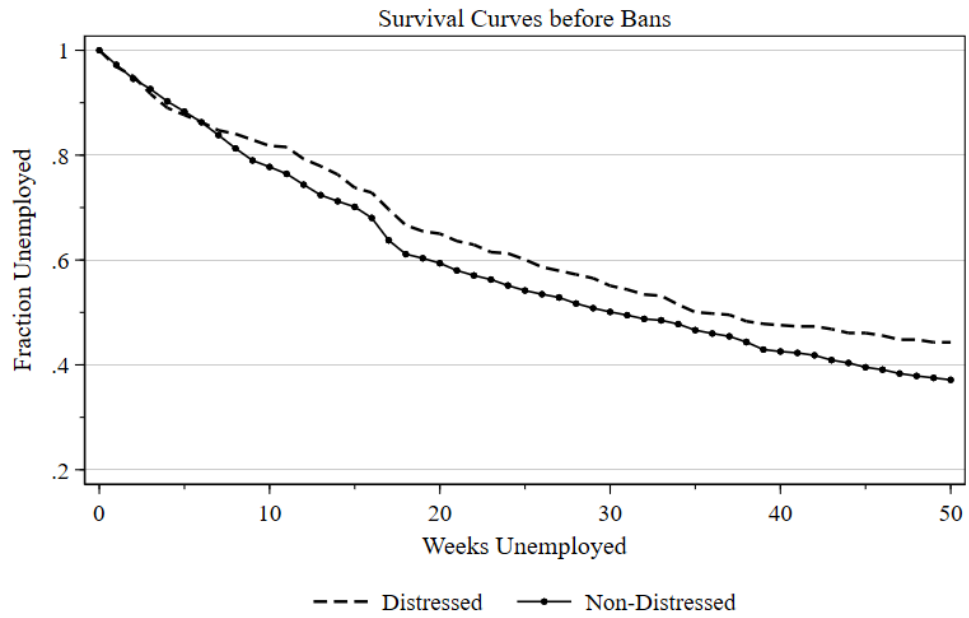
Figure 2 Event Study of the Impact of Credit Check Bans on the Job Finding Rate
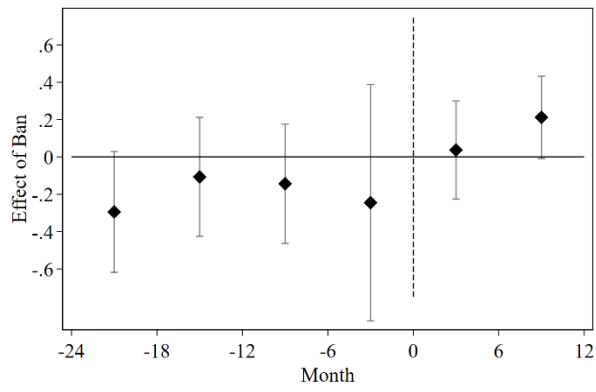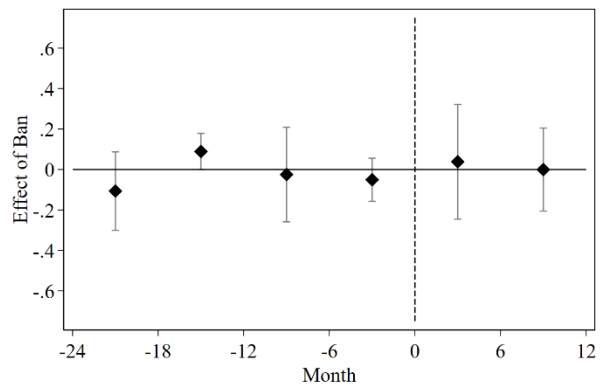
a) Distressed   b) Non-Distressed

Online Appendix for
"Who Benefits from Bans on Employer Credit Checks?"

Leora Friedberg, Richard M. Hynes & Nathaniel Pattison

Appendix OA: 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 42,030 households, although the sample changes due to attrition or as individuals enter sampled households. 35,269 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 19,685 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.

Measuring employment 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 start as a switch from ES=3,4,5 to ES=1,2. The SIPP also contains a number that identifies the same employer over time. The duration of the employment spell is the number of weeks with ES=1,2 and reports working for the same primary employer. We exclude spells of self-employment.

In the 2008 Survey of Income and Program Participation, we observe 23,884 individuals start a job (from non-employment) during the sample period. Restricting the sample to individuals between the ages of 18 and 65 leaves 20,527 individuals. We drop individuals who were on temporary layoff prior to starting the job, since they may have returned to a previously held job, which leaves 16,768 individuals. We then keep only those who actively searched for a job for at least one week, leaving 10,139 individuals. Of these, we keep the 7,629 who have

information on financial distress in the Wave 6 Adult Well-Being topical module. The final core

sample consists of 7,629 individuals who experience 9,313 employment spells.

Appendix OB: Randomization Inference

We also conduct a robustness check on the standard errors. We have 51 clusters, 10 of which are treated. When there are few treated clusters, it is possible that the cluster-robust t-statistics over-reject, though this problem is generally less of an issue with the number of clusters in our sample (see Conley and Taber, 2011; MacKinnon and Webb, 2017). Wild bootstrapped standard errors also severely under-reject when there are few treated clusters. A second issue is that our clusters are unbalanced, i.e., the number of observations per state is proportional to the state's population. In the case of differences-in-differences with relatively few treated clusters and an unbalanced panel, MacKinnon and Webb (2020) propose using a randomization inference procedure based on t-statistics and show that it performs better than other commonly used alternatives.

We implement the following procedure following MacKinnon and Webb (2020):

1. Estimate our baseline specification to calculate $\hat{\beta}$ and t_β, the estimates of the effect of bans and the t-statistic (based on standard errors clustered at the state).

2. Generate 1,000 t_r^* statistics to compare with t_β:

    a. We randomly choose ordered groups of 10 states and assign them placebo bans equal to the actual dates of the bans. That is, the first randomly selected state is assigned a "ban" date of 7/2007, the second state 7/2009, and so on.

    b. For each set of placebo states, estimate the baseline specification and obtain an estimate β_r^* and t-statistic t_r^* (also with standard errors clustered at the state).

3. Compare the actual t-statistic t_β to the distribution of "placebo" t-statistics t_r^*.

The reason to compare t-statistics, rather than coefficient estimates, is that the panel is unbalanced. If the placebo treatment group has fewer observations than the actual treatment group, then the placebo estimator would have a higher variance. The distribution of β_r^* in this case, would overestimate the variance of $\hat{\beta}$, while randomization inference assumes that the placebo and actual estimators have the same variance (see Ferman and Pinto, 2019 and MacKinnon and Webb, 2020). Indeed, when we conduct randomization inference comparing $\hat{\beta}$ to the distribution of β_r^*, 13.6% of placebo coefficients are larger (in absolute value) than the actual estimate, and this increases to 18.6% when the randomly chosen placebo states are small (bottom tercile of treated units). Comparing t-statistics improves the procedure by taking into account differences in the variance of the estimates. Online Appendix Figure OA5 compares the distributions of the placebo t-statistics to the actual t-statistics. In the sample of financially distressed individuals, t_β is larger (in absolute value) than 96.7% of placebo t-statistics. For the sample of non-distressed individuals, t_β is larger (in absolute value) than only 23.4% of placebo statistics. MacKinnon and Webb (2020) also show that these procedures tend to under-reject when larger clusters are treated, so these randomization inference "p-values" are likely conservative. Therefore, it does not seem that having few treated clusters is distorting inference.

Appendix OC: Coefficient Stability

One potential concern is that credit check bans alter selection into unemployment. In Table OA15, we show that our coefficient of interest is stable as additional controls for demographic, economic, and legal characteristics are included. Assuming that selection on these observables is correlated with selection on unobservables, the stability of the estimated ban effect when adding controls suggests that selection into unemployment does not play a large role. However, Oster (2017), 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. The intuition is that, when controls are added, the change in the coefficient of interest can only be deemed "small" or "large" when compared to the quality of the additional controls, measured by the change in R-squared. A small change in the coefficient of interest is not very informative if the additional controls themselves explain little of the dependent variable.

We implement the calculation of Oster (2017) in this section to provide a bias-adjusted estimate of the ban effect. First, to apply the method, we estimate the OLS analog of our Cox specification

(OA1) $$\log(D_{ist}) = \alpha + \beta ban_{st} + X_{ist}\gamma + \delta_s + \tau_{y(t)} + \varepsilon_{ist},$$

a differences-in-differences 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. Spells that are right-censored because they exceed 50 weeks or attritted remain in the sample. Online Appendix Table OA15 reports the estimates from this specification for various sets of controls. Reassuringly, if we ignore the remaining censoring issue and run this OLS, the estimates of the change in unemployment durations are similar to our Cox regressions.

Using these uncontrolled and controlled regressions, Oster (2017) provides two measures to assess the robustness of the results to unobserved selection. Both methods require an assumption on *Rmax*, which is the theoretical R-squared that would be obtained from a regression on all relevant observable and unobservable variables. Oster (2017) suggests using *Rmax=1.3$\tilde{R}$,* where $\tilde{R}$ is the R-squared from the regression with observable controls (in our case, $\tilde{R}$ is 0.107 in column 6 of Table OA21). A larger *Rmax* allows for a greater role for unobservables, so we take a more conservative approach and set *Rmax=0.4*, which is roughly four times as large as the R-squared with all observable controls in column 6 of Table OA21. Given this assumption on *Rmax*, one can calculate the value δ, which reflects how important unobservable selection must be, relative to observable selection, in order to explain the result. Second, one can calculate a bias-adjusted estimate of β. This requires an assumption about the degree of unobserved selection relative to observed selection, and we follow Oster (2017) and set this value equal to 1. The bottom rows of Table OA21 report these δ and bias-adjusted β coefficients. In order to explain the results, the unobservable characteristics must be 1.23-3.16 times as important as the observable controls in explaining unemployment durations. The bias-adjusted β coefficients remain similar to the main estimates, are actually larger when all controls are included in Column 6 because the coefficient in Column 6 is larger in magnitude than the no control coefficient in Column 1.

Appendix OD: Comparison with Related Literature

This appendix conducts additional analysis to compare and reconcile our estimates with those of other papers examining employer credit check bans, namely Cortés, Glover, and Tasci (2020) (hereafter CGT), Ballance, Clifford, and Shoag (2020) (hereafter BCS), and Bartik & Nelson (2019) (hereafter BN).

OD.1. Data and Sample Differences

The employment outcomes in BCS are from the LEHD LODES data. These provide employment counts at detailed geographies and are constructed from administrative data from state unemployment insurance system and federal worker earnings records, which together cover approximately 95 percent of wage and salary jobs. These data are geographically aggregated counts of workers in different industries and wage bins and do not contain additional demographic information, so we cannot directly compare our sample to individuals underlying the LODES data.

BN uses the CPS when examining job-finding rates, in addition to the LEHD job-to-job flows. The SIPP and CPS have identical survey universes and sampling frames (Sae-Ung, Sissel, and Mattingly, 2007). Important differences include that (i) the SIPP follows movers while the CPS does not, (ii) the SIPP respondents collect information about weekly employment while the CPS collects information about monthly employment, and (iii) the SIPP oversamples households from high-poverty areas. We have examined the role of these three differences in reconciling our estimates with those of BN.

BN is the paper most directly comparable to our analysis. The other papers use geographic aggregates and so do not provide summary statistics containing demographic information about race or earnings. We compare our sample to that of BN in Table OA22. The samples are largely similar in the share of state populations accounted for by each race/ethnicity

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group and the average weekly wage, though our wages for Hispanic workers are reported to be lower. Our sample has rates of 4-year college degrees that are 3-5 percentage points lower, and our employment rates, defined as the share of the labor force that is employed, are also systematically lower. The difference in employment rates may be due to our use of a stricter definition of employment, requiring that they be employed for the full month. Other sources of differences may be due to the SIPP's oversampling of lower-income households and differences in the periods included in our sample (2008-2013 vs. 2003-2018). Overall, the economic and demographic characteristics of the sample used in our paper are quite similar to those of BN. A shortcoming of the SIPP for examining racial heterogeneity, as we discuss below, is that there are relatively few observations of black job-seekers in ban states.

OD.2. Overall Labor Market Outcomes

Overall Job-Finding Rates

Most comparable to our main estimates of the job-finding rate are the estimates of BN, which uses a hazard model estimated with unemployment duration data from the CPS. Like us, they find no statistically significant change in overall job-finding rates, though our point estimate of the change in the log job-finding hazard is larger than that of BN. We estimate a coefficient of 0.09 (se 0.082), whereas BN finds coefficients that range from -0.016 to 0.007 (se around 0.03) in Table 4 Panel A2. The estimates of the overall effect in both our paper and BN are imprecise and not statistically different from zero or from each other. Assuming no covariance, we cannot reject that the two estimates are equal and the p-value of this test is 0.33.

The difference between the point estimates may be due to the different sampling structures of the CPS and SIPP.[33] The SIPP measures unemployment at a higher frequency (weekly vs. monthly with large gaps). In contrast, the matched CPS used in BN conducts monthly surveys of households (by address) for up to four consecutive months, recording the labor status as of the week of each interview. Since the surveys are monthly, the matched CPS misses short spells of unemployment that occur between the surveys (Kaitz 1970; Keifer, Lundberg, and Neumann 1985). Nekarda (2009) shows that this monthly time aggregation causes the CPS to understate the true number of transitions between unemployment and employment by 15 to 24 percent, relative to the SIPP, which measures weekly unemployment status. Indeed, in our sample, 9.3 percent of the unemployment spells last for three weeks or less. Some of these short spells are unobserved in the monthly CPS data.

A second difference between the SIPP and the CPS is that the SIPP tracks households, whereas the CPS surveys the same address. Therefore, the matched CPS used in BN will not include households that moved during an unemployment spell. We mimic this in the SIPP by dropping the 10.3% of observations where the household moved during the unemployment spell.

Table OA23 reports results from this exercise. In column 1, we replicate our baseline specification. In column 2, we use the monthly unemployment duration. The point estimate declines from 0.0896 to 0.047. In column 3, we again use the monthly unemployment durations, but estimate the complementary log-log model of BN. The point estimate falls to 0.0421. Comparing the estimates in Columns 1 and 3 suggests that the different sampling structures of

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[33] One possible explanation is that the SIPP oversamples low-income, and so our sample may contain a higher proportion of the financially distressed. If this oversampling contributes to the difference, it may be exacerbated if the oversampled group or the financially distressed are more likely to have multiple unemployment spells in the sample, since our observations are of the spell rather than the sample. Estimating a weighted hazard model, using the SIPP sampling weights, did not narrow the gap

the SIPP and CPS can account for more than 50% of the difference between our estimates and those of BN.

Columns 4 and 5 show the estimates when using the sample that drops both short spells and movers. In column 5, which uses the complementary log-log model of BN, the estimate falls to 0.0274. This is close to the BN estimates of 0.00537 (Table 4 Panel A Column 1) and the p-value of the difference is 0.80.

In summary, modifying the sampling structure of the SIPP so that it matches the CPS reduces the difference between our point estimate and those of Bartik & Nelson by 75%. Our point estimate falls to 0.0274 with a 95% confidence interval of [-0.138, 0.193], compared to Bartik & Nelson's estimate of 0.005 with a 95% confidence interval of [-0.05, 0.0625]. There is substantial overlap in the confidence intervals and the difference in the estimates is far from being statistically significant (p-value of 0.80).

In one table, CGT also estimates the response of job-finding rates. Unlike our paper and BN, which use individual-level data with hazard models, CGT examines state-level or county-level average quarterly job-finding rates within a linear regression model, also estimated with data from the CPS. An advantage of our and BN's estimates using a hazard model with high-frequency individual-level data is that they make use of finer variation in unemployment durations (weekly and monthly vs. quarterly) and allow individual-level controls to be included.

Using state-level job-finding rates from the CPS, CGT estimates an imprecise 2.7% (standard error of 2.2%) decrease in job-finding rates in their Table 5 column 3. CGT also includes an estimate using a measure of job-finding rates inferred from aggregated county-level unemployment insurance claims data used in Hagedorn, Karahan, Manovskii, and Mitman (2013). They estimate a 1.5% decline that is marginally significant at the 10% level. We do not

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have access to these claims data, so we cannot do a formal comparison. There are reasons to prefer the CPS estimates to this administrative unemployment insurance data. The CPS is the primary labor force survey used to measure unemployment, and Hagerdorn et al., the source of this alternative data and strategy, use UI claims data only because their strategy requires county-level estimates (and the CPS is not representative at the county-level). Additionally, the coverage of UI claims data is limited to the first 26 weeks of job loss and only contains unemployment spells by individuals claiming UI. Even at the peak of the recession, less than half of unemployment spells claimed UI (Hagerdorn et al., 2013). Additionally, the average spell duration over the sample period exceeded 30 weeks. The selected sample and censoring after 26 weeks may cause the estimates to differ. Even with these differences, the employment outcome estimates in CGT's Table 5 are small and imprecise, confirming the other estimates of little overall labor market effects.

Overall Employment and Unemployment Rates

All papers agree that the bans had little effect on overall employment or unemployment rates. Measuring overall employment, CGT, BCS, and our paper estimate changes in either the unemployment rate or overall employment. Using the Current Population Survey (CPS), CGT finds a statistically insignificant 1.2 percentage point increase in the overall unemployment rate, though the standard error is 3.8 percentage points (CGT Table 7). Using the LODES data, BCS finds small and insignificant changes in overall (log) employment (Online Appendix Table OA3). We also find no changes in overall employment upon the enactment of a ban when estimating a linear probability model resembling our main specification. As seen in Table OA24, there are no significant changes in employment rates, with the point estimates indicating a one percentage point increase in employment among the financially distressed (columns 1 and 2) and smaller changes for the non-distressed and the overall labor market (columns 3-6).

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Separation Rates and Employment Durations

Our estimates of changes in overall separations and employment durations are generally consistent with the related literature. Our Table 6 columns 5 and 6 report no change in overall employment durations of new hires. While not directly comparable, BN finds overall declines in involuntary separation rates among new hires of around 2 percentage points (Table 5 Panel B). To provide a more comparable estimate, we replicate the analysis of BN within the SIPP. Using the CPS, BN uses a sample of newly hired individuals and estimates a linear probability model where the dependent variable is an indicator for an involuntary job separation within 1-14 months of hiring, depending on when the CPS matched sample respondent obtained a job.[34] We replicate this strategy in the SIPP by estimating a linear probability model where the dependent variable is an indicator for an involuntary separation within the first six months of employment. We choose six months because it in the middle of BN's 1-14 month range.

Table OA25 reports the estimated effect of bans on the probability of an involuntary separation. The first column shows a small and insignificant effect on overall separations, and the estimate remains similar when adding state economic and policy controls in column 2. Likewise, BN finds 1-2 percentage point declines in separation rates, though the estimates shrink when adding additional economic and policy controls. Column 3 interacts the post-ban indicator with indicators for Black or Hispanic status. As in BN, the point estimates suggest increases in separations for Black new hires and declines for Hispanic new hires, though neither estimate is statistically significant.

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[34] Specifically, they observe involuntary separations for new hires at horizons ranging from 1 to 14 months, depending on when they obtained their job during the 16 months of the CPS survey.

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CGT also uses the CPS, as in BN, to estimate the impact on separation rates, estimating a 2.7% (not percentage point) increase in separation rates, although their estimate is imprecise with a 95% confidence interval of [-1.42, 6.8] (Table 5 column 2). Since the average separation rate is less than 10% (BN Table 3), the 2.7% increase translates into a 0.27 percentage point increase, which is small and within the confidence intervals of our estimates and those of BN. Again, CGT uses average state-level quarterly separation rates, which reflect the geographic average among new all workers, not just new hires. Though not statistically different, the imprecision and different point estimates in CGT and BN, which both use the CPS, may be due to differences in the dependent variable, data (state-level vs. individual new hires), or inclusion of controls. In summary, when using individual-level data and controls, both BN and our paper find 1.5-2 percentage point declines in involuntary separation rates.

Vacancy Rates

While not directly comparable to changes in employment or job-finding rates, two of the papers also examine changes in job vacancy rates and characteristics. The SIPP does not contain this employer-side information, but we still discuss the results in other papers and their relation to our results.

BCS shows that cities with lower risk scores experience an increase in job postings requiring experience or education, consistent with signal substitution within a model of statistical discrimination. This substitution does not alter the overall number of postings but may create heterogeneity in the job-finding rates of distressed applicants with different levels of education.

CGT estimates a 5.5% decline in vacancies within non-exempt occupations upon the enactment of the bans relative to exempt occupations in the same county. This relative decline in vacancies, in their back-of-the-envelope calculation, translates into a 2.9% reduction in the job-finding rate of job seekers searching in non-exempt occupations, holding search behavior and the

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unemployment rate constant. In Table OA26, we estimate the change in job-finding rates by exemption status for the full sample (distressed plus non-distressed). The point estimates of the change in job-finding rates for non-exempt jobs range from 0.022 to 0.065, and the 2.9% reduction calculated in CGT is well within the confidence intervals. Additionally, CGT estimates the change in vacancies for non-exempt occupations relative to the change for exempt occupations. Thus, their estimate reflects the change in the gap between the two types of vacancies around the enactment of the ban. Exempt vacancies are not a pure control group, however, since they may be indirectly affected by the bans. Indeed, as seen in Table OA26, job-finding rates in exempt industries rise relative to those in exempt industries. Thus, in our data, the difference in job-finding rates between exempt and non-exempt industries grows upon the enactment of the bans, consistent with the change in vacancies reported in CGT. Finally, we note that the 2% reduction in the overall job-finding rate implied by CGT's estimates (70% of job seekers in exempt occupations multiplied by a 2.9% increase in employment durations) is well within the 95% confidence intervals of our estimated change in employment.

OD.3. Heterogeneity in the Impact on Subgroups

Heterogeneity by Credit Score

First, Corbae and Glover (2017) develops a general equilibrium model taking into account the effects of bans on vacancy postings and job-finding rates across borrowers with different credit scores. This model rationalizes the magnitude of our estimates of the impact on the job-finding rate. The model implies that, upon the enactment of the bans, job-finding rates of those with bad credit (bottom quintile) increase by 27%, quite similar to our 28%. Additionally, consistent with our estimates, the model predicts a slight overall *increase* in job-finding rates due to general equilibrium effects on wages.

BCS estimates the impact of the bans across geographic areas varying levels of credit risk, with greater employment in areas with a high percentage of low credit score individuals, which is qualitatively similar to our estimate of improved job-finding rates among the distressed. Their main classification defines a low-risk-score tract as one where the average risk score was below 620 (the subprime threshold), and around 10% of tracts are classified as low-risk-score tracts. These low-risk-score tracts experienced 3.5-7.5 percent greater employment post-ban than the control group, which consists of other low-risk-score tracts within the same Census division. Given that, on average, 60% of the population is employed, this translates into a 3-percentage point increase in the share employed in low-risk-score tracts.

To compare the quantitative impact of our results, we can compute the implications of changes in job-finding rates for steady-state employment. The steady-state is when flows into employment equal flows out of employment. Assuming no change in labor force participation, the steady-state is when

$$e \cdot s = u \cdot f$$

where e is the stock of employed, s is the separation rate, u is the stock of unemployed, and f is the job-finding rate. Therefore, steady-state employment equals

$$e = \frac{f}{s + f} LF$$

where $LF = u + e$ which is assumed to be constant. If the separation rate s is also constant, we can examine the impact of a change in f on the employment rate. Using pre-ban job finding rate $f = 0.17$ and separation rate $s = 0.018$. After the bans, the estimated 28% increase in the job-finding rate would cause employment for the financially distressed to increase by 2 percentage points. Thus, although the 28% increase in the job-finding rate is large, the implied employment increase is slightly smaller than the 3-percentage point increase in low-risk-score

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tracts found in BCS. BCS's estimate becomes even larger when considering that not all residents of low-risk-score tracts will have a poor credit history, so BCS's increase is driven by the subset of residents with poor credit.

Three factors help reconcile these results. First, the comparison group in BCS consists of tracts with higher credit scores. Thus, the estimates reflect the change in employment relative to these higher credit score tracts, which may have been negatively affected by the ban. Indeed, BCS finds offsetting *declines* in employment among nearby neighborhoods with average credit scores between 630 and 650. Thus, their estimates reflect the change in the gaps between low- and higher risk score tracts, whereas our estimates reflect changes in job-finding rates of distressed job-seekers relative to other distressed job-seekers. Second, our measure of financial distress corresponds to the broader group of very low (< 620) plus low (620-650) credit scores, since 18% of individuals (27% of unemployed) report financial distress in the SIPP. Thus, averaging the increased employment in the very low tracts with the decreased employment in the low tracts will bring BCS's estimates closer to the steady-state employment levels implied by our change in job-finding rates. Finally, the event-study version of BCS's estimates shows that the effect of bans is smaller for the first three years, with the largest point estimate in year 5. Our analysis focuses on the first few years after the bans since the SIPP covers 2008-2013, when the point estimates in BCS are smaller.

Heterogeneity by Race

Both BN and BCS examine heterogeneity in the impact among members of minority groups. BN find that black job-finding hazards declined by 14 percent after a credit check ban, while new black hires became 4 percentage points more likely to experience involuntary separation shortly after being hired. Population average job-finding rates and white workers' job-finding rates show little change after PECC bans, and the estimates for Hispanic workers are

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sometimes positive or statistically indistinguishable from those for whites. BCS shows that black employment rates, conditional on labor force participation, were roughly 1 percentage point lower post-ban than the unemployment rates of other groups in the same state-year. They also find that young people saw a decrease in the employment rate of roughly half this size, although this effect loses significance when controlling for state-specific young adult trends.

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 OA18 formally tests whether banning employer credit checks affected job-finding rates overall or for socioeconomically disadvantaged groups that have higher rates of poor credit. In contrast to other studies, we do not find that the bans have a statistically significant effect on minority employment outcomes, though a small sample size makes our estimates noisy. The signs of the coefficients in columns 2 and 3 suggest that black job seekers 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. White job seekers had 18.3% shorter unemployment spells, and the estimate is significant at the 10% confidence level.

Again, the most comparable estimates to our analysis in Table OA18 are the changes in job-finding rates in BN. Unlike BN, we find a small positive point estimate for the effect of bans on the job-finding rates of black job seekers, and a negative effect on the job-finding rates of Hispanic job seekers. Neither estimate is statistically significant. Some of the differences may be because the CPS does not contain unemployment spells with short (less than one month) durations and that it does not track households that move. Table OA19 estimates equation (1) separately for the racial groups, dropping short unemployment spells and movers to match the

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analysis of BN. For black job seekers, these sample modifications reduce our point estimate of the effect of bans from 0.0274 to -0.135. This is now similar to the estimates of BN of -0.11 to -0.14 (BN Table 4 Panel A). However, this change also exacerbates the difference between our estimates for Hispanic job losers and those of BN.

Our view is that the SIPP is poorly suited for examining heterogeneity by race. Our estimates are generally imprecise due to the relatively small number of observations of minorities. For example, the estimate for Black respondents has a standard error of 0.449. Another issue with examining the racial heterogeneity using the SIPP is that minority job seekers in ban states are concentrated in California and Illinois. The results are highly sensitive dropping these states. For example, Table OA19 columns 5 and 6 show significant changes in the estimates for Black and Hispanic respondents change when California is dropped. (Our main results included in the paper are not sensitive to dropping CA or any other state.)

In summary, our estimates for black respondents – a focus in BN and BCS – become more similar when we match the sampling structure of the CPS. However, the SIPP estimates of heterogeneity by race are imprecise and sensitive to dropping certain states. Even before making these adjustments, our estimates are not statistically different from the comparable estimates in BN.

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Table OA1 Exemptions from Credit Check Bans

		WA	HI	OR	IL	CT	MD	CA	VT	CO	NV
	Share of Unemp. Exempt (%)	29.2	33.3	35.4	17.5	21.4	40.9	13.9	18.8	34.7	13.2
Panel A. Exempted Jobs/ Job Duties											
Management	Set the direction of a business		X		X	X	X	X		X	X
	Access to high-level trade secrets				X	X	X	X			X
	Access to corporate financial info				X						
	Access to payroll info					X			X	X	
	Provide administrative support for executives									X	
	Direct employees using independent judgment	X									X
Legal	Law enforcement			X	X			X	X	X	X
Confidentiality	Access to clients' financial info (non-retail)			X	X	X	X	X	X	X	X
	Access to clients' personal confidential info				X	X				X	X
Fiduciary	Signatory power / custody of corporate accounts				X	X	X	X	X	X	X
	Unsupervised access to marketable assets				X	X					
	Unsupervised access to cash				X	X		X			
Miscellaneous	Control over digital security systems										
	Airport security			X							
Panel B. Exempted Industries											
Finance	Banking and related activities	X	X	X	X	X	X	X	X	X	X
	Savings institutions, including credit unions	X	X	X	X	X	X	X	X	X	X
	Securities, commodities, funds, trusts, etc.					X	X	X		X	
	Insurance carriers and related activities				X	X		X		X	
Law Enforcement	Law Enforcement and Corrections				X					X	
	Department of Natural Resources				X						
Miscellaneous	Gaming										X
	Space Research									X	
	National Security									X	
	Debt Collection				X						
	Other state and local agencies				X						
Panel C. General and Undefined Exemption											
	Substantially job-related	X		X			X				
	Bona fide occupational qualification or purpose		X			X					

In Table 1, Mgmt. includes "set the direction of a business or business unit," "access to high-level trade secrets," "access to corporate financial info," "access to payroll info," "provide administrative support for executives," or "direct employees using independent judgment." Law Enf. includes law enforcement and airport security. Confid. includes "access to clients' financial info" or "access to clients' personal confidential info." Fiduciary includes "Signatory power/ custody of corporate accounts," "unsupervised access to marketable assets" or "unsupervised access to cash." Finance includes "banking and related activities," "Savings institutions, including credit unions," "Securities, commodities, funds, trusts, etc.," or "Insurance carriers and related activities." Law Enf includes "Law enforcement and corrections" or "Department of natural resources." Space means "Space Research" and Govt means "other state and local agencies."

Table OA2 Financial Hardship in the SIPP

Financial Hardship Questions	Mean Full SIPP	Mean Unemployed
Did you not meet all of your essential expenses?	0.18	0.27
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	0.18
Did the gas or electric company turn off service, or the oil company not deliver oil?	0.02	0.04
Did the telephone company disconnect service because payments were not made?	0.04	0.07
Did you need to see a dentist but not go?	0.08	0.14
Did you need to see a doctor or go to the hospital but not go?	0.10	0.16
Observations	78,230	7,829

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.

	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	27.02	18.22	0.70
Law	0.09	0.10	0.10	0.09	0.28	0.85
Pre-unemp. monthly wage	1,920	2,520	0.00	1,882	1,676	0.40
Education	12.16	12.91	0.00	12.19	2.41	0.71
Age	36.42	36.59	0.56	36.22	13.48	0.57
Female	0.49	0.46	0.02	0.49	0.50	0.64
Married	0.40	0.43	0.00	0.41	0.49	0.65
Black	0.17	0.12	0.00	0.18	0.38	0.65
Hispanic	0.19	0.15	0.00	0.18	0.38	0.20
Unemp. rate	8.64	8.66	0.67	8.64	1.88	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.

To produce the matched sample, 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. The estimates and precision for the non-distressed group are similar if we use many-to-one matching.

Table OA4 Financial Distress and Spending Relative to Income

Spending relative to income over the last year:	Spending more than income	About equal	Spending less than income
Credit score less than 620	37.7%	26.0%	16.1%
Bankruptcy in last two years	3.1%	2.6%	2.0%
Foreclosure in last two years	4.7%	2.6%	2.4%
Late on mortgage in last two years	35.3%	20.3%	12.8%
Charged late fee on credit card in last year	46.8%	25.7%	16.6%
Charged credit card over the limit fee in last year	30.3%	15.0%	8.6%
Share of observations	20%	36%	43%
Observations	5,513	9,935	11,796

The table reports means from the FINRA Investor Education Foundation 2009 State-by-State National Financial Capability Study among individuals answering each question. Observations are weighted to be nationally representative. Question: "Over the PAST YEAR, would you say your [household's] spending was less than, more than, or about equal to your [household's] income? Please do not include the purchase of a new house or car, or other big investments you may have made."

Table OA5 Changes in Transitions to Exempt and Non-Exempt Jobs

	Distressed Sample				Non-distressed Sample			
	Exempt Job		Non-Exempt Job		Exempt Job		Non-Exempt Job	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ban	-0.071*	-0.044	0.28***	0.23***	0.041**	0.032	0.011	0.024
	(0.038)	(0.030)	(0.039)	(0.044)	(0.017)	(0.024)	(0.019)	(0.023)
Last job exempt		0.19***		-0.19***		0.21***		-0.18***
		(0.022)		(0.024)		(0.012)		(0.017)
State FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Controls		X		X		X		X
Ind. & Occ. FE		X		X		X		X
Observations	2,469	2,469	2,469	2,469	6,299	6,299	6,299	6,299

One year after job loss, an individual can be i) working in an exempt position, ii) working in a non-exempt position, or iii) remain without a position. Using the sample of unemployed individuals that we observe for a year after job loss, we estimate the following linear probability model:

$$y_{ist} = \alpha + \beta ban_{st} + X_{ist}\gamma + \delta_s + \tau_t + \varepsilon_{ist},$$

where y_{ist} is an indicator for having an exempt position one year after job loss. This table shows coefficients on Ban from LPM regressions on whether individuals have an exempt or non-exempt position one year after job loss. A job is considered exempt if either its industry or occupation is exempt from credit check bans in any state. Controls include age, education, marital status, and sex. "Last job exempt" is an indicator for whether the unemployed individual's last job was exempt. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Title: Table OA6 Coefficients of the Event Study Specification

Columns: ΔBan_{s,t+24}, ΔBan_{s,t+18}, ΔBan_{s,t+12}, ΔBan_{s,t+6}, ΔBan_{s,t}, Ban_{s,t-6}

Distressed row:
-0.248, -0.0541, -0.0786, -0.179, 0.0894, 0.258**
(0.161), (0.163), (0.172), (0.325), (0.147), (0.111)

Non-Distressed:
-0.0730, 0.126**, 0.0179, 0.00800 (with a - above it), 0.0685, 0.0275
(0.100), (0.0517), (0.119), (0.0633), (0.147), (0.110)

Table OA6 Coefficients of the Event Study Specification

	$\Delta Ban_{s,t+24}$	$\Delta Ban_{s,t+18}$	$\Delta Ban_{s,t+12}$	$\Delta Ban_{s,t+6}$	$\Delta Ban_{s,t}$	$Ban_{s,t-6}$
Distressed	-0.248	-0.0541	-0.0786	-0.179	0.0894	0.258**
	(0.161)	(0.163)	(0.172)	(0.325)	(0.147)	(0.111)
Non-Distressed	-0.0730	0.126**	0.0179	-0.00800	0.0685	0.0275
	(0.100)	(0.0517)	(0.119)	(0.0633)	(0.147)	(0.110)

This table reports the coefficients and standard errors from the leads and lags of the event study in specification (4) estimated separately on the distressed and non-distressed samples. Controls are for individual age, sex, years of education, marital status and the state unemployment rate. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table OA7 Robustness to Additional Controls

	No Controls (1)	Basic Controls (2)	Extended Controls (3)	Economic Controls (4)	Ban the Box (5)	Housing, Medicaid, Manuf. (6)
Ban	0.313***	0.280***	0.311***	0.342***	0.377***	0.360***
	(0.0896)	(0.0958)	(0.106)	(0.0874)	(0.0750)	(0.0680)
Black			-0.240***	-0.233***	-0.231***	-0.231***
			(0.0838)	(0.0866)	(0.0865)	(0.0864)
Hispanic			0.182**	0.183**	0.184**	0.184**
			(0.0728)	(0.0773)	(0.0770)	(0.0774)
Age-squared			-0.0005**	-0.0005***	-0.0005***	-0.0005***
			(0.000186)	(0.000178)	(0.000178)	(0.000178)
Unemp. rate				-0.0963**	-0.0907**	-0.102*
				(0.0440)	(0.0433)	(0.0595)
Max UI Benefit (weeks)				0.00829	0.00850	0.00869
				(0.00538)	(0.00534)	(0.00536)
Ban-the-Box Share					-0.231***	-0.229**
					(0.0881)	(0.0912)
Log(home price index)						-0.469
						(0.808)
Medicaid expansion						0.0272
						(0.0904)
Log(manufacturing emp.)						-0.0522
						(1.474)
State FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Demographic Controls		X	X	X	X	X
Wage Spline			X	X	X	X
Industry & Occupation FEs			X	X	X	X
Observations	77,487	77,487	77,487	74,712	74,712	74,712

All regressions report estimates from the Cox proportional hazard model from estimating equation (1) on the sample of individuals with trouble paying bills. "Demographic controls" consist of controls for on seam, age, marital status, years of education, and sex. 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. Wage spline is a 5-piece log-linear spline in pre-unemployment wages. Column 4 adds controls for the monthly state unemployment rate and the monthly 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 (2020). Column 6 adds the annual log of the state FHFA home price index, an indicator for whether the state's Affordable Care Act Medicaid expansion was in effect at the start of the unemployment spell (from Sommers et al., 2013), and controls for the Quarterly Census of Employment and Wages log of total state manufacturing employment.

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

Table OA8 Robustness to Sample Changes

	Baseline (1)	Matched Sample (2)	Pre-unemp. Distressed Sample (3)	(4)
Panel A. Financially distressed				
Ban	0.280***	0.280***	0.302**	0.293**
	(0.0958)	(0.0958)	(0.134)	(0.123)
Number of Spells	2,888	2,888	1,499	1,303
Obs.	77,487	77,487	37,093	34,318
Panel B. Non-distressed				
Ban	0.0376	-0.0788	0.00502	0.00701
	-0.0859	(0.135)	(0.117)	(0.117)
Number of Spells	7,361	2,888	3,927	3,415
Obs.	192,952	78,021	94,966	87,613
State FE	X	X	X	X
Year-month 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. Column 2 uses the pre-processed non-distressed sample as described in the text. Columns 3 and 4 restrict the sample to unemployment spells that begin after the individuals answered the question about financial distress. 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 OA9 The Impact of Credit Check Bans on Those Missing Bill Payments

	Distressed (1)	**Missed Payment** (2)	**Missed Rent/Mortgage or Utilities** (3)	**No Missed Payment** (4)
Ban	0.280***	0.255**	0.296***	0.199
	(0.0958)	(0.103)	(0.109)	(0.246)
Distressed X Ban				0.284***
				(0.103)
On Seam	1.626***	1.592***	1.590***	1.733***
	(0.0712)	(0.0680)	(0.0727)	(0.181)
Age	-0.0132***	-0.0122***	-0.0112***	-0.0265***
	(0.00197)	(0.00244)	(0.00221)	(0.00728)
Female	-0.224***	-0.249***	-0.244***	0.0410
	(0.0587)	(0.0594)	(0.0614)	(0.149)
Education	0.0117	0.0113	0.0145	0.0454
	(0.0149)	(0.0161)	(0.0166)	(0.0303)
Married	0.131***	0.136**	0.126**	0.150
	(0.0445)	(0.0566)	(0.0566)	(0.119)
Observations	77,487	61,620	59,513	15,867
Year-month FE	X	X	X	X
State FE	X	X	X	X

All regressions report estimates from the Cox proportional hazard model from estimating equation (1). Column 1 shows the baseline sample of those who report failing to meet expenses. Column 2 restricts the baseline sample to individuals who also report missing a payment (miss rent/mortgage, evicted, miss utilities, utilities cut off, phone cut off). Column 3 restricts the baseline sample to those who report missing either a rent/mortgage or utilities payment. Column 4 restricts the baseline sample to those who report no missed payments.

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

Table OA10 Results Excluding Individual States

Excluded State	Ban Effect	Standard Error
WA	0.290***	(0.0967)
HI	0.286***	(0.0988)
OR	0.255***	(0.0942)
IL	0.262**	(0.117)
CT	0.268***	(0.0991)
MD	0.279***	(0.0958)
CA	0.381***	(0.0921)
VT	0.283***	(0.0954)
CO	0.273***	(0.0920)
NV	0.283***	(0.0996)

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).

	Distressed (1)	Non-distressed (2)	Overall (3)	Distressed relative to Non-distressed (4)
Ban	0.270***	0.0222	0.0756	
	(0.0979)	(0.0933)	(0.0877)	
Distressed X Ban				0.262**
				(0.110)
On Seam	1.641***	1.731***	1.706***	1.704***
	(0.0695)	(0.0535)	(0.0487)	(0.0484)
Age	-0.0134***	-0.0141***	-0.0139***	-0.0141***
			(0.000860)	
	(0.00212)	(0.000948)		(0.000813)
Female	-0.232***	-0.140***	-0.170***	-0.167***
	(0.0593)	(0.0261)	(0.0268)	(0.0280)
Education	0.0108	0.0403***	0.0337***	0.0350***
	(0.0157)	(0.0102)	(0.00988)	(0.0108)
Married	0.125***	0.120***	0.124***	0.122***
	(0.0474)	(0.0321)	(0.0271)	(0.0270)
Observations	73,751	183,070	256,821	256,821
Year-month FE	X	X	X	
State FE	X	X	X	X
Year FE				X
State × Financial Distress FE				X
Year × Financial Distress FE				X
State X Year FE				X

This table repeats Table 4, but drops the three states that passed bans before financial distress is measured in the SIPP: WA, HI, and OR. 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, 2 and 3 report coefficients from equation (1) estimated on the distressed, non-distressed, and full samples, respectively. Column 4 reports coefficients from equation (2) estimated on the pooled sample of distressed and non-distressed unemployment spells.

	Distressed (1)	Non-distressed (2)	Overall (3)
(A) Ban	0.287	-0.023	0.05
	(0.10)	(0.13)	(0.11)
(B) Ban × Unemp. rate	-0.002	0.08	0.054
	(0.03)	(0.06)	(0.04)
Unemp. rate	-0.07	-0.04	-0.045
	(0.04)	(0.03)	(0.03)
Number of Unemp. Spells	2,888	7,361	10,249
Year-month FE	X	X	X
State FE	X	X	X
Controls	X	X	X
High Unemp. Ban (u = 10.7%) (A) + σ × (B)	0.283	0.131	0.155
	(0.10)	(0.11)	(0.09)
Low Unemp. Ban (u = 6.9%) (A) - σ × (B)	0.292	-0.176	-0.055
	(0.14)	(0.21)	(0.17)

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 state-level unemployment rate is de-meaned so that the ban coefficients are comparable to other estimates. The bottom rows of the table compare the implied effect in states with unemployment rates one standard deviation above or below the mean rate.
Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table OA13 Summary Statistics for the Sample of Employment Spells by States

| | Ban States | | Control States | | Difference | p-value |
	Mean	Std. Dev.	Mean	Std. Dev.		
Employment Duration	50.6	49.9	52.1	51.4	-1.49	0.220
Financially distressed	0.26	0.44	0.28	0.45	-0.02	0.106
Education	12.6	2.9	12.8	2.3	-0.17	0.004
Age	35.1	12.7	35.1	12.6	0.07	0.804
Female	0.49	0.50	0.49	0.50	0.01	0.647
Married	0.43	0.50	0.38	0.49	0.05	0.000
Black	0.08	0.27	0.16	0.37	-0.08	0.000
Hispanic	0.28	0.45	0.13	0.33	0.16	0.000
Unemployment rate	9.61	1.89	8.22	1.83	1.39	0.000
Obs.	2,364		6,949			

The data are individual-level employment 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). *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 OA14 Heterogeneity of Employment Duration by Exemption of Job from Bans

	Distressed			Non-distressed		
	(1)	(2)	(3)	(4)	(5)	(6)
Ban	-0.299**	-0.292**	-0.331***	0.00922	0.0117	0.0377
	(0.132)	(0.134)	(0.129)	(0.148)	(0.147)	(0.130)
Ban X Exempt	0.449			0.0474		
(state-job-specific exemptions)	(0.649)			(0.437)		
Ban X JobExempt		0.280	0.444		-0.00293	-0.101
(job-specific exemptions)		(0.451)	(0.405)		(0.289)	(0.219)
Observations	108,938	108,938	108,938	329,554	329,554	329,554
Sum of Ban and Ban X Exempt	-0.291	-0.105	0.0350	0.196	-0.0339	-0.00493
p-value	0.532	0.753	0.934	0.425	0.816	0.979
Demographic Controls	X	X	X	X	X	X
Year -month FE	X	X	X	X	X	X
State FE	X	X	X	X	X	X
State X exempt (state-job) FE	X			X		
State X exempt (job) FE		X	X		X	X
Year-month X exempt (job) FE			X			X

All regressions report estimates from the Cox proportional hazard models. In columns 1 and 4, "Exempt (state-job-specific)" is an indicator for whether individual's more recent job was exempt from the credit check bans in his or her state. In the remaining columns, "Exempt (job-specific)" is an indicator for whether the past job was exempt from credit check bans in any state. "Demographic controls" consist of controls for on seam, age, marital status, years of education, and sex. The table also reports the sum of the Ban and Ban X Exempt coefficients and the corresponding p-value from a Wald test of the coefficient sum being equal to zero. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table OA15 Robustness of Employment Durations to Additional Controls

	No Controls	Basic Controls	Extended Controls	Economic Controls	Ban the Box	Housing, Medicaid, Manuf.
	(1)	(2)	(3)	(4)	(5)	(6)
Ban	-0.254**	-0.270***	-0.320***	-0.314***	-0.300**	-0.250*
	(0.109)	(0.0981)	(0.0942)	(0.119)	(0.126)	(0.133)
Black			-0.0708	-0.0800	-0.0772	-0.0784
			(0.0659)	(0.0618)	(0.0615)	(0.0626)
Hispanic			-0.0692	-0.0582	-0.0555	-0.0564
			(0.0792)	(0.0788)	(0.0787)	(0.0795)
Age-squared			0.000388	0.000432	0.000432	0.000422
			(0.000252)	(0.000266)	(0.000266)	(0.000266)
Unemp. rate				0.00371	0.0106	0.0571
				(0.0498)	(0.0479)	(0.0636)
Max UI Benefit (weeks)				0.00489	0.00490	0.00496
				(0.00476)	(0.00477)	(0.00481)
Ban-the-Box Share					-0.156	-0.170
					(0.102)	(0.110)
Log(home price index)						0.925
						(0.689)
Medicaid expansion						-0.0738
						(0.121)
Log(manufacturing emp.)						0.980
						(1.591)
State FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Demographic Controls		X	X	X	X	X
Industry & Occupation FEs			X	X	X	X
Observations	108,938	108,938	108,938	105,737	105,737	105,737

All regressions report estimates from the Cox proportional hazard model from estimating equation (5) on the sample of individuals with trouble paying bills. "Demographic controls" consist of controls for on seam, age, marital status, years of education, and sex. 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 (2020). Column 6 adds the annual log of the state FHFA home price index, an indicator for whether the state's Affordable Care Act Medicaid expansion was in effect at the start of the unemployment spell (from Sommers et al., 2013), and controls for the Quarterly Census of Employment and Wages log of total state manufacturing employment.

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

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Table OA16 Changes in Job Quality

	Wage growth (1)	Log wage (2)	Full-time (3)	Salaried (4)
Ban	-0.00798	0.0842	0.0378	0.0374
	(0.0675)	(0.114)	(0.0840)	(0.0522)
Pre-unemployment controls				
Log wage		0.246***		
		(0.0260)		
Full time			0.236***	
			(0.0364)	
Salaried				0.311***
				(0.0285)
Observations	1,380	1,380	1,517	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. Significance levels: *=.1, **=.05 and ***=.01.

Table OA17 Changes in Self-Employment and Mobility

Dependent variable	Self-Employed	Self-Employed (unincorporated)	Self-Employed	Self-Employed (small)	Move	Move State
Sample:	All financially distressed		Financially distressed job finders			
	(1)	(2)	(3)	(4)	(5)	(6)
Ban	0.00227	0.00128	0.00751	-0.00157	-0.0562*	-0.00845
	(0.00974)	(0.0115)	(0.0122)	(0.0117)	(0.0290)	(0.0219)
State FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Demographic Controls	X	X	X	X	X	X
Observations	25,649	25,649	2,888	2,888	2,888	2,888

Columns 1 and 2 report estimates from a linear probability model of the effect of bans on annual observations of the full sample of financially distressed individuals. The dependent variable in column 1 is an indicator for owning a business at some point during the year, and column 2 excludes incorporated businesses from the definition. Columns 3 and 4 use the same outcomes, but restrict the sample to financially distressed job finders, i.e. that transition from unemployment to employment. Columns 5 and 6 uses the same sample of job finders, but replaces the dependent variable with an indicator of whether they moved addresses (column 5) or states (column 6) during the unemployment spell or the first month of employment. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

Table OA18 Impact of Bans on Minority Groups

	All (1)	Black (2)	Hispanic (3)	Non-Hispanic White (4)
Ban	0.0896	0.0274	-0.234	0.178*
	(0.0821)	(0.281)	(0.183)	(0.0957)
On Seam	1.691***	1.805***	1.894***	1.607***
	(0.0492)	(0.0922)	(0.0874)	(0.0469)
Age	-0.0138***	-0.0109***	-0.0148***	-0.0158***
	(0.000821)	(0.00345)	(0.00284)	(0.00102)
Female	-0.169***	-0.0886	-0.428***	-0.131***
	(0.0259)	(0.0863)	(0.0795)	(0.0344)
Education	0.0324***	0.0839***	-0.00967	0.0490***
	(0.00927)	(0.0225)	(0.00799)	(0.0109)
Married	0.117***	0.118	0.207***	0.0792***
	(0.0263)	(0.120)	(0.0507)	(0.0281)
Observations	270,439	40,241	44,514	165,271
Number of Unemployment Spells	10,249	1,397	1,661	6,467
Year -month FE	X	X	X	X
State 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.

Table OA19 Examining Unemployment Durations by Race

	(1) All	(2) Black	(3) Hispanic	(4) Non-Hispanic White	(5) Black - Drop CA	(6) Hisp. - Drop CA
Ban	0.0336	-0.135	-0.399**	0.0896	-0.307	-0.0798
	(0.0802)	(0.449)	(0.182)	(0.118)	(0.554)	(0.171)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	48522	7184	8156	29587	6858	5389

Estimates are from a Cox proportional hazard model of the unemployment exit hazard as in equation (1). Sample uses monthly unemployment durations, drops durations of less than one month, and drops movers. Standard errors clustered at the state level are in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table OA20 Investigating Dobbie et al. (2020) Specification

| | Dependent Variable: Employed in year t | | | |
| | Distressed Sample | | Full Sample | |
	(1)	(2)	(3)	(4)
Ban	0.0143	0.0149	0.00268	0.00144
	(0.0104)	(0.0100)	(0.00246)	(0.00254)
State FE	X	X	X	X
Year FE	X	X	X	X
Restrict states		X		X
Observations	263,428	248,179	1,382,359	1,308,990

This table shows coefficients on Ban from LPM regressions on an annual employment indicator. Employment is defined as being employed at some point during the calendar year. Standard errors are clustered at the state level. "Restrict states" restricts the sample to the states used in Dobbie et al. (2020), namely the control states plus WA, HI, CA, IL, and OR.

Table OA21 Sensitivity of OLS Estimates to Additional Controls

	No Controls (1)	Basic Controls (2)	Extended Controls (3)	Economic Controls (4)	Ban the Box (5)	Housing, Medicaid, Manuf. (6)
Ban	-0.351***	-0.348***	-0.345***	-0.332***	-0.339***	-0.352***
	(0.116)	(0.0954)	(0.103)	(0.102)	(0.112)	(0.111)
Black			0.233***	0.229***	0.229***	0.228***
			(0.0732)	(0.0740)	(0.0742)	(0.0743)
Hispanic			-0.136*	-0.135*	-0.134*	-0.135*
			(0.0687)	(0.0691)	(0.0696)	(0.0700)
Age-squared			0.000367**	0.000369**	0.000371**	0.000369**
			(0.000137)	(0.000141)	(0.000141)	(0.000141)
Unemp. rate				0.122***	0.120***	0.0882*
				(0.0383)	(0.0404)	(0.0520)
Max UI Benefit (weeks)				-0.00797	-0.00803	-0.00782
				(0.00523)	(0.00522)	(0.00536)
Ban-the-Box Share					0.0590	0.0876
					(0.0732)	(0.0828)
Log(home price index)						-0.339
						(0.650)
Medicaid expansion						-0.0411
						(0.0782)
Log(manufacturing emp.)						-1.197
						(1.199)
State FE	X	X	X	X	X	X
Year-month FE	X	X	X	X	X	X
Demographic controls		X	X	X	X	X
Wage Spline			X	X	X	X
Industry & Occupation FEs			X	X	X	X
Observations	2,469	2,469	2,469	2,469	2,469	2,469
R-squared	0.068	0.090	0.103	0.106	0.106	0.107
Δ		3.091	3.159	1.547	1.713	1.233
Bias-adjusted β		-0.296	-0.280	-0.147	-0.203	-0.413

Estimates are from OLS regressions of equation A1 on the sample of individuals with trouble paying bills. "Demographic controls" consist of controls for age, marital status, years of education, and sex. 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 (2020). Column 6 adds the annual log of the state FHFA home price index, an indicator for whether the state's Affordable Care Act Medicaid expansion was in effect at the start of the unemployment spell, and controls for the Quarterly Census of Employment and Wages log of total state manufacturing employment. The table also present δ and bias-adjusted β statistics from Oster (forthcoming) using an Rmax value of 0.4. Standard errors are clustered by state. Significance levels: *=.1, **=.05 and ***=.01.

Table OA22 Comparison of SIPP and CPS Samples

	SIPP Sample		CPS (Bartik & Nelson Table 2)	
	Ban states	Non-Ban states	Ban states	Non-Ban states
Black				
Share of State Adult Population	10%	15%	9%	14%
Employment Rate	83%	83%	87%	90%
Average Weekly Wage ($)	$777	$617	$776	$655
Share of Workers with 4-year College Degree	26%	21%	31%	24%
Hispanic				
Share of State Adult Population	27%	12%	21%	11%
Employment Rate	84%	87%	90%	93%
Average Weekly Wage ($)	$581	$544	$645	$633
Share of Workers with 4-year College Degree	11%	13%	14%	17%
White				
Share of State Adult Population	64%	74%	70%	75%
Employment Rate	89%	91%	94%	95%
Average Weekly Wage ($)	$988	$849	$989	$866
Share of Workers with 4-year College Degree	41%	34%	45%	37%

The sample consists of SIPP respondents between the ages of 18 and 54 during the fourth reference month. We defined workers or the employed as those with a job for the full month. The "employment rate" is the share of those in the labor force that are employed. Weekly wage is the average among the employed.

Table OA23 Replicating CPS Sampling Structure in the SIPP

VARIABLES	(1) Baseline	(2) Dropping Short Spells	(3) Dropping Short Spells - CLOGLOG	(4) Drop Short Spells & Movers	(5) Drop Short Spells & Movers - CLOGLOG
Ban	0.0896	0.0470	0.0421	0.0336	0.0274
	(0.0821)	(0.0821)	(0.0857)	(0.0802)	(0.0846)
Age	-0.0138***	-0.0134***	-0.0141***	-0.0145***	-0.0154***
	(0.000821)	(0.000890)	(0.000923)	(0.000973)	(0.00102)
Female	-0.169***	-0.189***	-0.201***	-0.146***	-0.156***
	(0.0259)	(0.0270)	(0.0288)	(0.0279)	(0.0299)
Education	0.0324***	0.0318***	0.0340***	0.0301***	0.0324***
	(0.00927)	(0.00920)	(0.00986)	(0.00896)	(0.00967)
Married	0.117***	0.120***	0.127***	0.141***	0.151***
	(0.0263)	(0.0292)	(0.0306)	(0.0288)	(0.0307)
Constant			-2.537***		-2.454***
			(0.233)		(0.257)
Observations	270,439	57,338	57,327	48,522	48,511

Column 1 replicates our baseline specification from Table 4 column 3. Column 2 uses monthly unemployment durations, dropping spells that last less than one month. Column 3 estimates the complementary log-log model of Bartik & Nelson (2019). Columns 4 and 5 drop short spells and movers. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.

| | **Dependent Variable: Employed in year t** | | | | | |
| | **Distressed Sample** | | **Non-distressed Sample** | | **Full Sample** | |
	(1)	(2)	(3)	(4)	(5)	(6)
Ban	0.0104	0.00896	-0.00159	-0.00363	0.000517	-0.00129
	(0.00950)	(0.00862)	(0.00328)	(0.00338)	(0.00306)	(0.00311)
Controls		X		X		X
State FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Mean Dep. Var.	0.746	0.746	0.842	0.842	0.824	0.824
Observations	25,649	25,649	109,223	109,223	134,872	134,872

This table shows coefficients on Ban from linear probability models regressions on an annual employment indicator. Our measure of employment equals one if the individual is employed for at least one full month during the calendar year, and we include one observation per individual for each year, as in Dobbie et al. (2020) and Bos et al. (2018), which measure whether an individual reports positive labor income during the year. Online Appendix Figure OA3 presents the event study version of these specifications. Controls are for individual age, sex, years of education, and marital status. Standard errors are clustered at the state level.

Table OA25 Changes in the Probability of an Early Separation

	(1) Involuntary Separation	(2) Involuntary Separation	(3) Involuntary Separation
Ban	-0.0131 (0.0203)	-0.0154 (0.0193)	-0.0141 (0.0228)
Ban X Black			0.0282 (0.0387)
Ban X Hispanic			-0.0127 (0.0114)
State FE	Yes	Yes	Yes
Year-month FE	Yes	Yes	Yes
Economic and policy controls	No	Yes	Yes
Observations	7880	7880	7880

Sample consists of new hires in the SIPP. Standard errors in parentheses
All regressions control for age, sex, education, and marital status. Economic and policy controls consist of those in Table OA21 column 6.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table OA26 Job-Finding Rates by Exemption Status (Full Sample)

	Full Sample		
	(1)	(2)	(3)
Ban	0.0224	0.0499	0.0646
	(0.0835)	(0.0835)	(0.0861)
Ban X Exempt	0.365***		
(state-job-specific exemptions)	(0.138)		
Ban X JobExempt		0.134	0.0987
(job-specific exemptions)		(0.121)	(0.143)
Observations	270,439	270,439	270,439
Sum of Ban and Ban X Exempt	0.387	0.184	0.163
p-value	0.00440	0.152	0.263
Demographic Controls	X	X	X
Year -month FE	X	X	X
State FE	X	X	X
State X exempt (state-job) FE	X		
State X exempt (job) FE		X	X
Year-month X exempt (job) FE			X

All regressions report estimates from the Cox proportional hazard model from estimating equation (3). In column 1, "Exempt (state-job-specific)" is an indicator for whether an individual's most recent job was exempt from the credit check bans in his or her state. In the remaining columns, "Exempt (job-specific)" is an indicator for whether the past job was exempt from credit check bans in any state. "Demographic controls" consist of controls for on seam, age, marital status, years of education, and sex. The table also reports the sum of the Ban and Ban X Exempt coefficients and the corresponding p-value from a Wald test of the coefficient sum being equal to zero. Standard errors are clustered at the state level. Significance levels: *=.1, **=.05 and ***=.01.
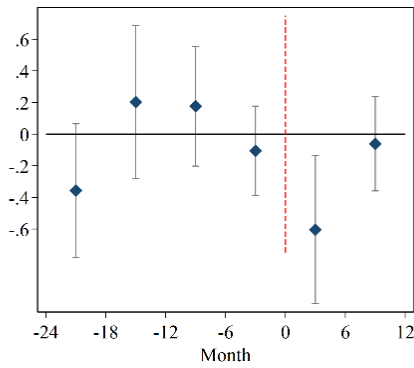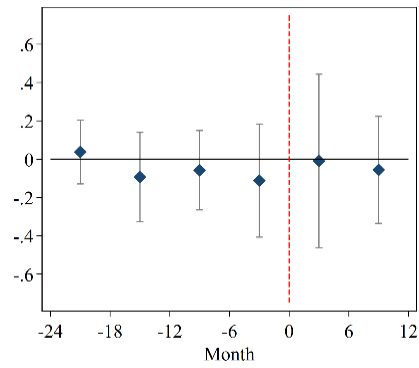
a) Distressed   b) Non-Distressed

This figure plots the coefficients and 95% confidence intervals from the event study specification but does not restrict the states or time period in the sample.

Figure OA2 Event Study of the Impact of Credit Check Bans on the Employment Exit Rate
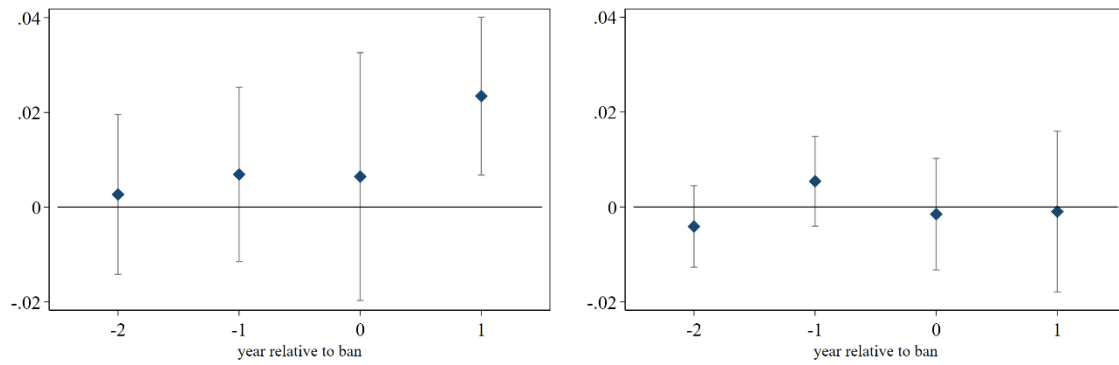

a) Distressed   b) Non-Distressed

This figure plots the coefficients and 95% confidence intervals of the impact of bans on the employment exit rate from the event study specification Cox proportional hazard model in specification (3) estimated separately on the distressed and non-distressed samples.

Figure OA3 Event Study of the Impact on Overall Employment

a) Distressed Employment   b) Non-Distressed Employment
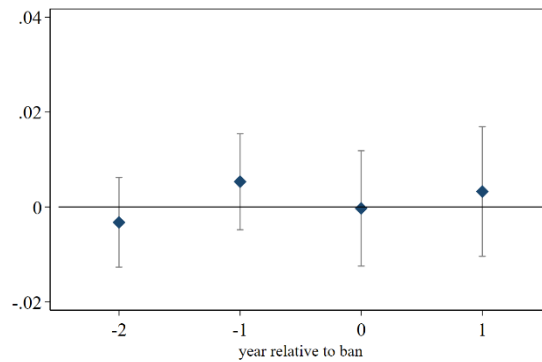
c) Overall Employment

This figure plots the coefficients from an event study version of specification (5). To keep the panel balanced, we only include ban states CA, CT, IL, and MD, which all have at least a 2.5-year pre-period and 1-year post-period and we drop observations more than 1-year after the ban.
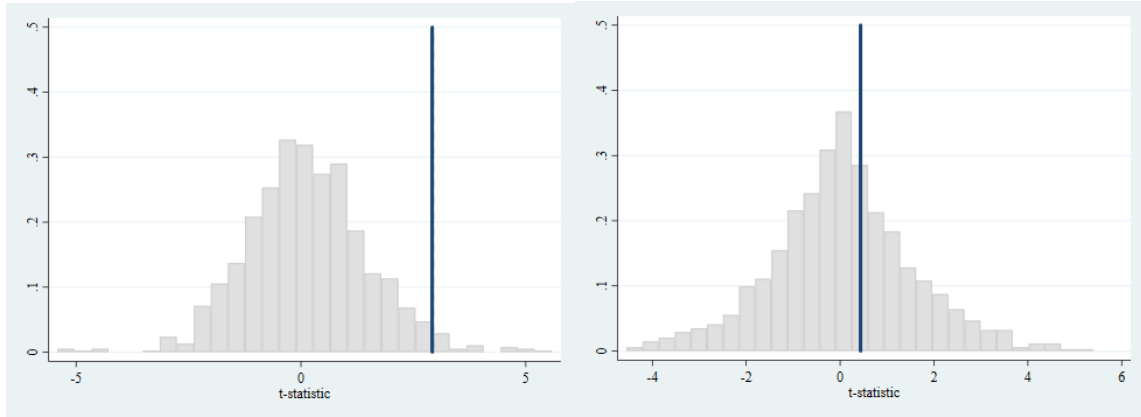
Figure OA4 Results Excluding Individual States
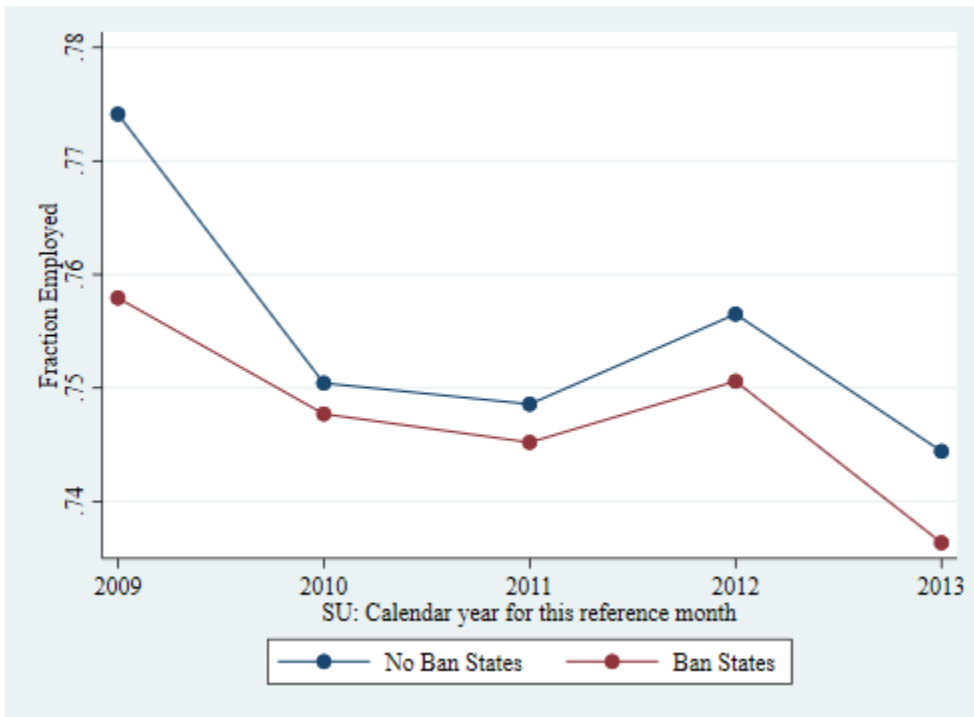
This figure shows the fraction of financially distressed that are employed for the control states (no ban) and the ban states CA, IL, and OR, as in Dobbie et al. (2020).

Figure OA5 Randomization Inference: Distribution of t-statistics

(a) Distressed   (b) Non-distressed

Comparison of actual t-statistic t_β to the placebo distribution.

This figure shows the distribution of placebo test statistics and the value of the actual test statistic from the randomization inference procedure discussed in Online Appendix OB.