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Over the last decade, 11 states have restricted employers' access to the credit reports of job applicants. We document a significant decline in county-level vacancies after these laws were enacted: Job postings fall by 5.5 percent in affected occupations relative to exempt occupations in the same county and the same occupation nationwide. Cross-sectional heterogeneity in the estimated effects suggests that employers use credit reports as signals: Vacancies fall more in counties with a large share of subprime residents, while they fall less in occupations with other commonly available signals.

JEL codes: J08, J23, J63, J78

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‡This paper extracts from an earlier paper titled "The Unintended Consequences of Employer Credit Check Bans on the Labor and Credit Markets" and presents substantially updated research.

"We want people who have bad credit to get good jobs. Then they are able to pay their bills and get the bad credit report removed from their records. Unfortunately, the overuse of credit reports takes you down when you are down." --Michael Barrett (State Senator, D-Lexington, MA).

1. Introduction

Over the last 20 years, credit reporting agencies have started marketing credit reports to employers to use in hiring. The three largest credit reporting agencies (Experian, Equifax, and TransUnion) currently offer the service, and a 2009 survey of human resource managers at Fortune 500 companies found that 60 percent of respondents used credit reports in at least some hiring decisions (Esen, Schmit, and Victor 2012). Additionally, a survey by the policy group Demos found that 10 percent of low- to medium-income workers claimed bad credit as a reason for being denied a job (Demos 2012).

In response to high unemployment and worsening credit conditions during the Great Recession, lawmakers sought to limit employer credit checks at the city, state, and national levels. As of November 2018, 11 states have banned employer credit checks for at least some jobs, as seen in Figure 1. When introducing and lobbying for these laws, lawmakers voiced concern that employer credit checks may create a poverty trap: A person with bad credit cannot find a job and therefore cannot improve her credit. However, lawmakers chose to exempt certain occupations by allowing employers to continue viewing applicant credit reports if the employee would have access to large amounts of money or sensitive information, such as Social Security numbers. We use this heterogeneity in occupational exposure to employer credit check bans to identify the effect of restricting credit information on a proxy for labor demand: posted vacancies.

We find that vacancies fall by 5.5 percent in occupations that are subject to employer credit check bans, relative to vacancies in exempt occupations in the same county and national trends for the same occupation. Vacancies also decline more in counties with many subprime residents and in low-skill jobs, while hiring rates increase for those already employed, suggesting that credit reports are used as a signal to screen job applicants. Finally, occupations involving routine tasks experience larger vacancy declines than nonroutine occupations, indicating that credit reports contain more relevant information about a worker's fit in routine than in nonroutine jobs.

Our empirical framework is a triple-difference linear regression model. We identify the effect of employer credit check bans on vacancies by measuring the change in county-level vacancies for an affected occupation around the implementation of an employer credit check ban, relative to the path of vacancies for exempt occupations in the same county and vacancies for the affected occupation in other states. The strength of this model rests in the flexibility of the implied counterfactual path of vacancies predicted if the ban had not been imposed on a given county or occupation.

Our estimates identify the causal effect of employer credit check bans under the assumption that state legislators made exemptions for reasons other than the strength of labor demand for the exempt occupations. While we cannot test this assumption directly, legislators have not discussed the relative labor market conditions for specific occupations when introducing employer credit check bans. Furthermore, exemptions appear to be made for reasons orthogonal to a given occupation's labor market at the time of the ban: States typically exempt occupations in which workers can readily embezzle from the employer, commit fraud, or steal from customers. Furthermore, we find no evidence of pre-ban divergence in vacancies between affected and exempt occupations.

We focus on vacancies for two reasons. First, they are the most direct proxy for the demand of unemployed workers. This is important because other equilibrium outcomes, such as unemployment or job-finding rates, also reflect changes in the labor supply in response to employer credit checks. Second, we find our triple-difference model most convincing for causal inference, and we can assign exemption status to vacancies but not to unemployment or other equilibrium outcomes. Nonetheless, we estimate difference-in-difference models using the unemployment, job-finding, and separation rates. These estimates are less precise, but are qualitatively consistent with our vacancy results: Following an employee credit check ban, states experience higher unemployment and separation rates and lower job-finding rates.

Given the number of states that have already banned employer credit checks, we believe our estimates provide a compelling reason for lawmakers to re-evaluate the efficacy of employer credit check bans. However, we are also interested in the economic mechanisms through which workers' credit histories affect labor demand, which leads us to consider county and occupational heterogeneity in the effect of bans.[1]

First, we find that vacancies fall more for affected occupations in counties with a large share of subprime residents, a finding that is consistent with credit reports being a more valuable signal when labor markets are more adversely selected. Second, we estimate a larger decline in vacancies for occupations that employ workers with less than a college education, a finding that is consistent with employers coping with the loss of credit report information by substituting education as a signal, when such information is available. Third, we find larger vacancy declines in occupations that involve routine tasks relative to those that involve nonroutine tasks, a finding that suggests that the information provided by

[1] These estimates speak to recent theoretical work on the interaction of credit and labor markets. Donaldson, Piacentino, and Thakor (Forthcoming) posit a theory in which debt overhang suppresses vacancies by raising workers' reservation wages. Most directly related to our policy-based identification strategy is Corbae and Glover (2018), who develop a screening model in which employers use credit reports in hiring because repayment rates are positively correlated with an unobservable component of worker productivity.

credit reports is more relevant for routine jobs. Finally, we turn to state-level job flows to show that the job-to-job flow rate *rises* in industries that are affected by bans relative to those that are exempt, a finding that is also consistent with signal substitution, since current employment directly indicates the ability to do a job.

Since we are interested in the interaction of credit market information with labor demand, we naturally use data on both markets. Our primary labor market variable is county-level vacancies by occupation, for which we use the Conference Board's Help Wanted OnLine (HWOL) panel. We created the series of state-level employer credit check bans and occupational exemptions from the relevant legislation record, as summarized in Table 1. We measure a county's share of subprime residents from the Federal Reserve Bank of New York's Consumer Credit Panel/Equifax (FRBNY CCP/Equifax).[2] Finally, our measure of state and industry job-to-job flows is from the US Census Longitudinal Employer-Household Dynamics panel.

We discuss our data in detail in Section 3, but first we review some recent literature in Section 2. A reader who already understands the data may wish to first read Section 4 for results and Section 5 for a discussion of heterogeneous effects across counties and occupations. Section 6 concludes.

2. Literature Review

Several studies analyze the effects of employer credit checks on labor market outcomes. To our knowledge, ours is the first to study the effect of employer credit check bans on local labor demand (i.e., job postings). We are among the first to study the effect of these laws on labor markets in general, although two recent papers by Ballance, Clifford, and Shoag (2016) and Bartik and Nelson (2016) are

[2] The Federal Reserve Bank of New York's Consumer Credit Panel/Equifax data are a subset of credit data maintained by Equifax, one of the large credit reporting agencies. The data are from a 5 percent sample of all individual credit records that Equifax maintains.

closely related. While they consider different outcome measures and implement different empirical strategies, these studies also find that employer credit check bans have negative labor market effects on at least some workers, such as black job seekers. On the other hand, Friedberg, Hynes, and Pattison (2018) find that people reporting financial difficulties enjoy higher job-finding rates following a ban, a finding that is consistent with the law that forces employers to pool workers more.

Ballance, Clifford, and Shoag (2016) estimate the effect of bans on log-employment at the census-tract level, using annual data. Substantively, our labor market estimates differ in two ways. First, we use data on vacancies, unemployment, job-finding rates, and separation rates as our labor market outcomes, rather than total employment. The ban's effect on vacancies and labor market flow rates is more salient than its effect on employment, since vacancies and labor market flow rates are directly related to an individual's probability of being employed. For example, total employment may rise if the law motivates labor force entry or immigration to the state post-ban, which is nonetheless a negative outcome for the average worker if vacancies do not rise in proportion. Second, our use of quarterly data allows for an extremely accurate coding of the date at which a ban goes into effect.

Bartik and Nelson (2016) use the panel dimension of the Current Population Survey (CPS) as well as data aggregated from state unemployment insurance records to estimate the effect of employer credit check bans on individual flow rates from nonemployment to employment. They primarily focus on the average flow rates for different racial groups in the labor market and report that employer credit check bans significantly reduced job-finding rates and increased the separation rates for blacks. These results are consistent with the main findings in our paper.

Our paper is also related to several recent papers that study the interactions between the labor market and the credit markets, especially via the use of credit market information, e.g., Bos, Breza, and Liberman (2018); Herkenhoff (2018);

Herkenhoff, Phillips, and Cohen-Cole (2016); and Dobbie, et al. (2016). The most relevant comparison is Bos, Breza, and Liberman (2018), who study a regulatory change in Sweden that removed negative information (bankruptcy, defaults) from some borrowers' credit reports. They find that this change led to higher employment rates for the affected groups. Though the affected group is a limited segment of the population (people who previously defaulted on a pawnshop loan) in a different country (Sweden), their results confirm that credit market information can affect labor market outcomes.

In the context of American credit and labor markets, Herkenhoff, Phillips, and Cohen-Cole (2016) and Dobbie, et al. (2016) use the removal of the individual bankruptcy flag from consumer credit report as an instrument to estimate the effect of creditworthiness on labor market outcomes. The former study finds that the removal of a bankruptcy flag affects labor supply: As credit terms improve, displaced workers take longer to find jobs and receive higher wages upon re-employment. Our estimated increase in unemployment rates is consistent with their results, though we estimate insignificant effects on earnings.

Dobbie, et al. (2016) rely on the differential effects of removal of the flag on labor market outcome variables for Chapter 13 filers relative to Chapter 7 filers. A Chapter 7 filer's default flag appears on her report for 10 years after bankruptcy, while a Chapter 13 filer's flag is removed after only 7 years. Based on outcomes for Chapter 13 filers within the three-year window after which their default flag is removed, Dobbie, et al. estimate zero effects on employment and earnings and conclude that labor demand is insensitive to creditworthiness. Contrary to their estimates, we find significant effects on the labor market in response to credit check bans, especially for our more direct measure of labor demand, vacancies. We reconcile our results with theirs by noting that a 7- to 10–year-old bankruptcy flag may provide employers with little significant information about a potential hire, since the life-cycle components of labor productivity and other observable labor market experiences during the first 7 years after bankruptcy likely swamp any signal provided by the bankruptcy flag.[3] Furthermore, changes in job search behavior over the course of a year may offset the decline in vacancies that we estimate.

Finally, there is an existing literature on a similar policy that restricts employers from asking about criminal histories (so-called "ban-the-box" laws). Agan and Starr (2017) conduct an audit study and find that ban-the-box laws in New York and New Jersey reduce the callback rate of job applicants with historically black names, relative to applicants with historically white names. Doleac and Hansen (2018) estimate a decline in employment for young, low-skilled black men following the implementation of ban-the-box legislation at the MSA level. Their results suggest that a clean criminal background signals a worker's fit with the job, so the mechanism is similar to our finding that credit reports are used as signals in labor markets. Of course, "ban-the-box" laws would pose a threat to our identification if the jobs affected by employer credit check bans also had many applicants for whom criminal background checks were valuable and both laws were enacted around the same time. This is not an issue in practice, because ban-the-box laws and employer credit check bans have not typically been enacted at the same time and geographical levels, but more important because ban-the-box laws do not exempt the same occupations as employer credit check bans and are therefore absorbed in the county-by-time fixed effects included in our baseline specification.

[3] Moreover, public-sector employers in the United States are not allowed to use bankruptcy filings in hiring decisions. If, at the margin, employers with government contractors respond to this constraint by hiring or retaining workers with a bankruptcy flag to ensure compliance, then the estimated effect of credit on labor demand will be biased toward zero.

3. Data

Table 1 details the timeline of changes in the law across states and Figure 1 maps the states that have laws in effect as of April 2017. Throughout our empirical analysis, we focus on the period 2005:Q1 through 2016:Q4. We use the date at which the law became enforceable to code our treatment flag, with the convention that dates falling within a quarter are coded as the beginning of that quarter (8 of 11 states began enforcing their bans at the start of a quarter). The resulting summary statistics for this flag are seen in the last columns of the upper left panel in Table 2. More than 10 percent of counties are affected by the credit check bans at the end of our sample period (out of 3,137) covering 26.5 percent of the US labor force.

A. Labor Market Data

Our principal labor market outcome is the county-level vacancy (job opening) data reported by the Conference Board (2017) as part of its Help Wanted OnLine (HWOL) data series. HWOL provides a monthly snapshot of labor demand at detailed geographical (state, metropolitan statistical area, and county) and occupational (six-digit SOC and eight-digit O*Net) levels since May 2005.[4] For the period in question, HWOL represents the bulk of the advertised job openings, as print advertising declined in importance.[5]

HWOL covers roughly 16,000 online job boards, including corporate job boards, and aims to measure unique vacancies by using a sophisticated deduplication algorithm that identifies unique advertised vacancies based on several ad

[4] For a detailed description of the measurement concepts and data collection methodology, please see Conference Board (2017). *The Conference Board Help Wanted OnLine® (HWOL) at* https://www.conferenceboard.org/data/-helpwantedonline.cfm.

[5] In fact, HWOL started as a replacement for the Conference Board's Help-Wanted Advertising Index of print advertising.

characteristics such as company name, job title/description, city, or state. HWOL is not the only source of data on job openings, though. The Bureau of Labor Statistics (BLS) publishes nationally representative data, the Job Openings and Labor Turnover Survey (JOLTS), which also measures vacancies. However, HWOL's detailed geographic- and occupation-level coverage makes it uniquely attractive for our analysis.[6]

Specifically, our identification strategy relies on occupational heterogeneity in exposure to employer credit check bans within a given county. We assign exemption status by state and two-digit SOC code as outlined in Table 1.[7] The resulting sample will have observations on vacancies at the county level for up to 23 different two-digit occupations. Table 2 summarizes these data on vacancies. On average, affected occupations constitute a larger sample and have consistently stayed higher than the exempt occupations in levels. Both groups of vacancies present procyclicality, experiencing substantial declines on average during the Great Recession. We also use the occupational coding of HWOL when estimating the differential effects of bans by occupational education requirements and task composition, which we discuss in each relevant section.

Our baseline estimates only identify the effect of bans on affected occupations relative to exempt occupations, but not the levels of each. In order to estimate the

[6] JOLTS' publicly available data files do not have more detailed coverage than census regions and lack any information on occupational characteristics. For most of the sample period, the general patterns reported in JOLTS and HWOL are reasonably close to each other. See, for instance, the relevant discussion in Sahin, et al. (2014). Researchers identified a recent diversion between vacancy measures across these two sources, one that is attributed to a change in pricing on several online job boards (Cajner and Ratner, 2016). To the extent that fixed effects in our empirical specifications absorb these pricing changes, our results will be immune to significant bias by relying on HWOL.

[7] Exemptions could be assigned using six-digit SOC codes instead of two-digit. However, this requires more judgment calls and leads us to drop many county-quarter observations because frequently there are zero vacancies posted at the six-digit SOC county-quarter level of observation. Finally, using six-digit SOC codes limits the replicability of our main results because the Conference Board's six-digit HWOL data have stricter access restrictions than the two-digit data.

effects on exempt and affected occupations separately while retaining granular fixed effects, we use counties along the borders of adjacent states as one of them enacts a ban. The resulting sample contains vacancies in each occupation for each contiguous county pair in which one county eventually passes an employer credit check ban. Summary statistics for this sample are reported in Table 3.[8] Comparing the summary statistics for exempt and affected groups in the full sample (in Table 2) and those in the adjacent county sample shows how similar the samples look. We are reassured that our sample of adjacent counties resembles the nation as a whole.

We prefer vacancies as our proxy for labor demand not only because they are measured at the occupational level but also because employers control vacancies directly, making them one step of equilibrium interaction to desired labor demand. However, we also estimate the response of other labor market variables: the unemployment, job-finding, separation, and job-to-job transition rates following a ban. For unemployment, we use county-level data reported by the Bureau of Labor Statistics' LAUS program.[9] The job-finding and separation rates are computed from CPS microdata at the state level. Since respondents in the survey are interviewed repeatedly for certain months, one can create a panel from the observed transitions of workers.[10] Finally, the job-to-job flow rate is reported at the state level by the CPS LEHD program.

[8] We thank Alan Collard-Wexler for publicly posting his data set of US counties and their neighboring counties, which we used to create our contiguous county sample. Our data set for creating contiguous county pairs is available at https://sites.duke.edu/collardwexler/data/.

[9] We also have county-level observations for employment and labor force through LAUS. All of these estimates for counties are produced through a statistical approach that also uses data from several sources, including the CPS, the CES program, state UI systems, and the Census Bureau's American Community Survey (ACS), to create estimates that are adjusted to the statewide measures of employment and unemployment.

[10] Each respondent is interviewed for four months initially and then leaves the survey for eight months. They are interviewed again for four more months. For any given month, about 70 percent of the survey respondents are observed consecutively, allowing us to create a panel to measure

B. Credit Market Data

The FRBNY CCP/Equifax panel provides detailed quarterly data from Equifax on a panel of US consumers and includes Equifax risk scores (credit scores) and other data on consumer credit reports. We aggregate individual credit information to estimate the effect of the ban as a function of the subprime share within a county. The distribution of subprime borrowers across counties and over time is found in Table 4. For this paper, we follow the literature and assume that the critical level for being subprime is an Equifax risk score of 620. Over the sample period we analyze, the average fraction of subprime borrowers within a county was 27 percent, declining from 29 percent to 25 percent over time.

There is substantial variation across counties in our sample: The share of subprime residents for a county in the 95th percentile is over 45 percent, while the bottom quartile's share is only 20 percent. Within a county, there is also variation in this share over time, as shown in Figure 2. This figure shows deciles of the maximal quarter-on-quarter change in each county's subprime rate, relative to the 2005 average rate. The top decile of the variation in subprime rates has counties that experienced changes of 12 percent of the 2005 average in at least one quarter and even the least variable decile saw quarterly changes of 2.6 percent at some point.

4. Results

We use a county-level panel with labor market data and an individual-level credit panel to test the effects of the employer credit check bans. We primarily estimate

average job-finding and separation hazards. Unfortunately, because of the size of the CPS and the number of transitions, we cannot obtain estimates for more granular levels than state. We follow Nekarda (2009) to minimize the bias induced by mobility.

the effect on job creation (measured by the number of help wanted ads posted online) using county-level data.

A. Effects of the Credit Check Bans on Job Posting

Our preferred empirical model is a triple-difference regression of the form:

$$(1) \qquad V_{i,o,t} = \alpha_{i,o} + \gamma_{i,t} + \beta\ Ban_{i,t} * Affected_{i,o} + \varepsilon_{i,o,t}\ ,$$

where the variable $V_{i,o,t}$ is the log of vacancies posted in county i at date t for occupation o, and the variables $Ban_{i,t}$ and $Affected_{i,o}$ are indicator variables. The ban indicator is equal to one only for those dates when county i is subject to a ban and the "Affected" indicator is one only for occupations that are subject to the ban in county i. The parameter of interest is β and $\alpha_{i,o}$ is a county-occupation fixed effect, $\gamma_{i,t}$ is a county-time fixed effect (measured quarterly), and $\mu_{o,t}$ is an occupation-time fixed effect.

The coefficient of interest, β, is identified from the growth in vacancies for affected occupations around the time that a state enacts a ban, relative to exempt occupations in that state and to growth in national vacancies in the affected occupation. The estimated coefficient for vacancies is found in column (1) of Table 5: It is statistically significant and economically large, implying a 5.5 percent decline in vacancies after the ban goes into effect.

We now explore how this relative decline arises by estimating the response of vacancies for affected and exempt occupations separately. This allows us to consider level effects on both affected and exempt occupations at the expense of dropping county-time fixed effects. However, we can still include very granular fixed effects for local labor market conditions by estimating the ban's effect using only contiguous counties along state borders, which allows us to include a county-pair-by-time-by-occupation fixed effect. Specifically, we estimate the regression

$$(2) \qquad V_{i,o,p,t} = \alpha_{i,o} + \gamma_{o,p,t} + \beta_1\ Ban_{i,t} + \beta_2\ Ban_{i,t} * Affected_{i,o} + \varepsilon_{i,o,p,t}.$$

The coefficient β_1 is identified from the difference in overall vacancies in treated counties as the ban goes into effect, relative to their neighboring counties in untreated states. The coefficient β_2 is then identified by the excess change in affected occupations relative to exempt occupations, again in treated states relative to their neighbors as the ban goes into effect.

Column (3) in Table 5 shows that we estimate a significantly negative β_2, while β_1 is insignificantly different from zero, which indicates that the entire post-ban decline in vacancies occurs in affected occupations.[11] Column (4) adds a control for a county's unemployment rate to equation (2), which captures some additional variation beyond county-pair-occupation fixed effects but the coefficients of interest are similar to column (3), both in magnitude and statistical significance.

B. Policy Endogeneity and Testing for Pre-Ban Divergence

Many legislators were concerned about weak labor markets when they proposed employer credit check bans, which raises classic endogeneity concerns if we were to estimate the effect of employer credit check bans on overall vacancies. However, legislators did not discuss relative labor market conditions to determine which types of jobs are exempt from the bans. Rather, the jobs that can continue to check applicants' credit reports are those in which employees have a greater scope for embezzlement, fraud, or theft. We therefore interpret the decline in vacancies for affected occupations relative to exempt occupations as being caused by the ban, rather than bans being imposed in response to the relative decline in vacancies for affected occupations.[12]

[11] Column (3) reports estimates from our triple-difference model in equation (1) using only the contiguous counties. While we lose power, the point estimate is very similar to that for the full sample of counties.

[12] While we use exemption status to overcome concerns of reverse causation, we have also looked at trends in various labor market aggregates in states that pass bans relative to those that don't. The

We would still overstate the effect of bans on vacancies if affected and exempt occupations were diverging leading up to the bans. We therefore estimate a distributed-lags specification of equation (1) that captures vacancy dynamics around the implementation of bans. This approach has been found to be especially useful for studying the effects of staggered implementation of the treatment (policy change) across different jurisdictions with a difference-in-difference identification strategy.[13]

We estimate the following equation:

$$(3) \quad V_{i,o,t} = \alpha_{i,o} + \gamma_{i,t} + \mu_{o,t} + \sum_{j=-4}^{5} \beta_j \, Ban_{i,t+j} * Affected_{i,o} + \varepsilon_{i,o,t},$$

where the variable $Ban_{i,t-j}$ equals one if county i implements an employer credit check ban at date t-j and zero otherwise, for j = -4 through 4, while $Ban_{i,t+5}$ remains equal to one for all dates more than four quarters after the ban goes into effect; the coefficients β_{-4} through β_5 therefore identify the difference between affected and exempt occupations relative to this difference a year plus before the ban. In this regression, a pre-ban divergence in vacancies between affected and exempt occupations manifests as significantly negative values of $\beta_{=4}$ through β_0.

As reported in Table 6 and illustrated in Figure 3, β_{-3} through β_0 are insignificant and small, while β_{-4} is significantly positive: affected occupations do not have significantly fewer vacancies before the ban than do exempt occupations. Following the ban, the coefficients become both economically and statistically negative within two quarters and remain so, even beyond a year after the ban is implemented, which is captured by the coefficient β_5. The long run (beyond one

states that have passed bans are somewhat better off by most measures. We present these data in Appendix A1.

[13] Some examples include Bertrand and Mullainathan (1999 and 2003) in the context of anti-takeover legislation and Meer and West (2016) in the study of minimum wage legislation.

year) is close to our baseline estimate and significantly negative at the 10 percent level.

C. Effects on Other Labor Market Variables

We prefer vacancies as our labor market outcome because it is most directly related to labor demand and we can leverage occupational variation in ban coverage to estimate our triple-difference specification, but policymakers may be more concerned with other labor market variables, such as the unemployment rate, job-finding rate, or job-separation rate. Furthermore, these aggregates are determined jointly in equilibrium, and so they provide a holistic view of labor market changes following the ban. We therefore estimate a difference-in-difference model for alternative labor market outcomes at both the state and the county levels, though we emphasize that these estimates may be subject to reverse causation.

In Table 7, we report how the unemployment rate changes post-ban, both in the nationwide sample and in the adjacent county sample. As seen in columns (1) and (4), there is a small positive, though statistically insignificant, change in the unemployment rate following a ban. The unemployment rate may not respond to the ban for various reasons. For example, the unemployed may search harder after the ban (because, for example, they have less access to debt and therefore cannot smooth their consumption as well). Whatever the reason, the fact that the unemployment rate does not *fall* substantially, while vacancies decline, means that the labor market slackens (the ratio of job openings to job seekers falls).

Moving to labor market flows, we would expect a decline in market tightness to generate a lower job-finding rate. We may also expect some change in the separation rate, though this could move in either direction. For example, if firms are less able to screen for good matches ex-ante, then there may be more workers fired after a short spell of employment. On the other hand, if workers know that they will have a harder time finding work upon quitting an existing job, then we may see a decline in separations. We therefore estimate a state-level regression using finding and separation rates as the dependent variables, the results of which are seen in columns (2) and (3) of Table 7.[14] The job-finding rate falls by 2.7 percent post-ban (this decline is marginally statistically significant with a T-statistic of -1.5), which is consistent with declining market tightness, while the separation rate rises by a similar amount (although this estimate is less precise).

5. Mechanism

We now use occupational heterogeneity in the value of employer credit checks as signals to explore the mechanisms through which bans affect labor demand. First, we estimate larger post-ban declines in vacancies in counties with a large share of subprime residents. This is consistent with credit reports being used to screen job applicants, since their informative value would be greater if the pool of unemployed was more adversely selected in the first place.

Second, we test the effect of alternative signals on the effect of employer credit check bans. We do this in two ways. For vacancies, we allow the effect of bans to differ by the education level of workers typically employed in a given occupation. We find larger declines in occupations that employ workers with less education, a finding consistent with employer credit checks being particularly useful screening devices when other signals are less readily available. We then estimate the effect of bans on job-to-job transition rates by occupational exemption status. Contrary to the case with vacancies, we find that job-to-job flows *rise* in occupations affected by the ban relative to those that are exempt, consistent with employment itself being an alternative signal to the credit report.

[14] We use state-level data for these rates because they are not well measured at the county level.

Finally, we ask what sort of information employers may be inferring from credit reports by estimating the effect of bans on jobs that involve routine tasks relative to those that involve nonroutine tasks. We estimate much larger declines in vacancies for jobs with routine tasks, a finding consistent with credit reports being informative about soft skills. These reasons align with the reasons that human resource managers report for using credit reports, which include preventing theft, reducing liability for negligent hiring, and assessing the overall trustworthiness of job applicants (Esen, Schmit, and Victor 2012).

A. Heterogeneous Effects by County-Level Subprime Rate

If employers prefer workers with good credit, then we expect them to respond more strongly to employer credit check bans when their hiring pool contains a large share of subprime people. We test for this mechanism by estimating the regression:

$$(4) \qquad V_{i,o,t} = \alpha_{i,o} + \gamma_{i,t} + \mu_{o,t} + \beta \, Ban_{i,t} * Subprime_i * Affected_{i,o} + \varepsilon_{i,o,t} \, ,$$

where the variable $Subprime_i$ measures the average share of residents in county i with subprime credit scores (below 620) in 2005, which is the first year for which we have credit report data and well before the first employer credit check ban (Washington's ban was passed in 2007).[15] All other variables and fixed effects are as previously defined.

Column (1) of Table 8 reports our estimate of a higher county-level subprime fraction on vacancies for occupations affected by the ban. This effect is negative and strongly significant. The range of county subprime shares in Table 4 gives

[15] We think keeping the subprime fraction variable at its 2005 levels also insulates it from the cyclical changes that occurred over the next several years in the data due to changing financial conditions. Nevertheless, in an earlier version of this paper, we used the current quarter subprime fraction and found similar results.

context for this estimate: The interquartile difference is 15 percentage points. Therefore, as a state bans employer credit checks, a county in the 75th percentile (which has a 34 percent subprime rate) would experience a 3.5 percent fall in vacancies in affected occupations relative to a county in the 25th percentile (where 19 percent of residents are subprime).

The larger decline in vacancies in affected occupations in counties with more subprime workers is consistent with a theory of credit reports as signals, which are more valuable when the labor market is more adversely selected. From a policy perspective, the effects are worse for the areas for which legislators profess concern when implementing the ban, suggesting that these consequences truly are unintended.

B. Heterogeneous Effects by Occupational Skill Requirements

If employers are restricted from using credit reports as signals of a worker's suitability to their jobs, then we expect them to use other observables instead of credit checks. In turn, occupations in which other signals are common should respond less to employer credit check bans than those with few alternatives. We test this logic using education, which is a classic signal of unobservable worker ability (Spence 1978), by estimating the regression

$$(5)\quad V_{i,o,t} = \alpha_{i,o} + \gamma_{i,t} + \mu_{o,t} + \beta\, Ban_{i,t} * NoCollege_{o} + \varepsilon_{i,o,t}$$

where the indicator variable $NoCollege_o$ is one if occupation o typically employs workers with less than a college education and zero otherwise.

The Conference Board's HWOL data enable us to map a subset of the occupational data into an education code that matches with the predominant education level for workers in that occupation. This information is not necessarily listed in the ad but is assigned based on the occupational coding using BLS mapping.[16] Based on this categorization, 37 percent of all vacancies posted were in occupations associated with a college education or higher.

Column (2) of Table 8 reports our estimate of β = -0.146 from the above regression, which is significant at the 5 percent level. This is consistent with low-education occupations being more reliant on credit reports as signals of unobservable worker ability, relative to high-education occupations.

C. State-Level Job-to-Job Flows

Another signal of workers' unobservable ability is their employment history. For example, Kroft, Lange, and Notowidigdo (2013) perform a field experiment and find that unemployment duration has a strong negative effect on callback rates, a finding that Jarosch and Philossoph (2018) rationalize in a signaling model with adverse selection. Following this literature, we expect current employment to be a positive signal of a worker's suitability for other jobs. We therefore estimate the effect of job-to-job flow rates in affected versus exempt industries when states enact employer credit check bans by estimating the regression

$$(6) \quad J2J_{s,n,t} = \alpha_{s,n} + \gamma_{s,t} + \mu_{n,t} + \beta\ Ban_{s,t} * Affected_{s,n} + \varepsilon_{s,n,t}$$

where s refers to a state, n refers to an industry, and t to a date. The variable J2J measures total hires *from employment* as a fraction of the stock of all jobs in each state-industry at that date, which we then transform logarithmically.[17]

[16] The BLS assigns a typical level of education needed for entry into an occupation and has eight different categories. A detailed description of the categories can be found here: https://www.bls.gov/emp/documentation/education/tech.htm.

[17] These data are publicly available from the US Census Bureau as part of the Longitudinal Employer-Household Dynamics database. A Unix script for downloading the raw data in CSV format is available at http://andyecon.weebly.com/lehd.html. Industry is only available at the two-digit level, which leads us to code finance and public administration as exempt.

Column (3) of Table 8 presents our estimate of $\beta = 0.016$, which is statistically significant at the 5 percent level. This means that job-to-job hiring rises in industries affected by employer credit check bans, relative to exempt industries in the same state. This is consistent with employers using other signals as substitutes for credit checks, one of which is a job applicant's current employment status.

D. Heterogeneous Effects by Occupational Task Composition

While our estimates show that vacancies decline in occupations affected by employer credit check bans, and the pattern of heterogeneity is consistent with a signaling theory of credit reports' value to employers, we do not know precisely what information in the credit report is useful to employers. One possibility is that an unobservable component of cognitive ability allows a person to both perform complicated tasks and to better plan and budget their personal expenses, so good credit correlates with productivity on the job. Alternatively, people with good credit may just be more responsible in all dimensions, which signals that they will be punctual and professional employees, even if their actual ability to perform the job is the same as that of a person with bad credit.

While we cannot disentangle the precise reason that employers value credit report information, we estimate the differential response of vacancies in occupations in which workers perform routine tasks, relative to those in which they do nonroutine work. We follow Jaimovich and Siu (Forthcoming) for classification of the routine and nonroutine jobs, which results in about 36 percent of our sample being vacancies posted for routine-task occupations.[18] We then estimate the following regression

[18] Our classification follows Jaimovich and Siu (Forthcoming) and we code the following two-digit occupations as routine: Sales and Related Occupations (41), Office and Administrative Support Occupations (43), Construction and Extraction Occupations (47), Installation, Maintenance, and

(7) $V_{i,o,t} = \alpha_{i,o} + \gamma_{i,t} + \mu_{o,t} + \beta\ Ban_{s,t} * Routine_o + \varepsilon_{i,o,t}$,

where $Routine_o$ indicates the task content of the occupation.

Column (3) of Table 8 presents our estimate of β = -0.117, which is significant at the 1 percent level. After a ban is enacted, vacancies fall substantially more in occupations that involve mostly routine tasks, relative to occupations that involve nonroutine tasks, even in the same county. This suggests that credit reports are not being used to infer a worker's unobservable ability at creative or nonroutine tasks.

6. Conclusion

In 2007, Washington was the first state to restrict employers' use of credit reports in hiring. Ten more states have adopted such policies since, and federal legislation has been proposed. These bans are intended to break a cycle in which limited employment opportunities cause financial distress, which further reduces labor market opportunities. The deep downturn in economic activity and severe housing market crisis experienced during the Great Recession provided policymakers with a strong motivation to introduce such legislation. We show that these laws have likely reduced vacancy postings in occupations for which employer credit checks have been forbidden.

Our estimates are consistent with the predictions of a micro-founded model of adverse selection in labor markets in which credit reports provide a signal of a worker's job fit (Corbae and Glover (2018)). First, vacancies decline more in counties in which a large share of residents have subprime credit, a finding consistent with credit reports being more valuable signals in markets that are more adversely selected. Second, we estimate smaller effects for occupations or job flows

Repair Occupations (49), Production Occupations (51), and Transportation and Material Moving Occupations (53). The remaining two-digit occupations are coded as nonroutine, with the exception of Farming, Fishing, and Forestry Occupations and Military Specific Occupations, which are excluded from the analysis.

that have other readily observable signals of worker quality: Vacancies decline less in occupations that typically require a college education, and industries affected by employer credit check bans shift hiring away from the unemployment pool and toward poaching from other employers.

While our data cannot definitively test the entirety of their model, the fact that the changes we estimate are accompanied by widespread welfare losses in Corbae and Glover (2018) suggests that employer credit check bans may reduce labor market efficiency and welfare.

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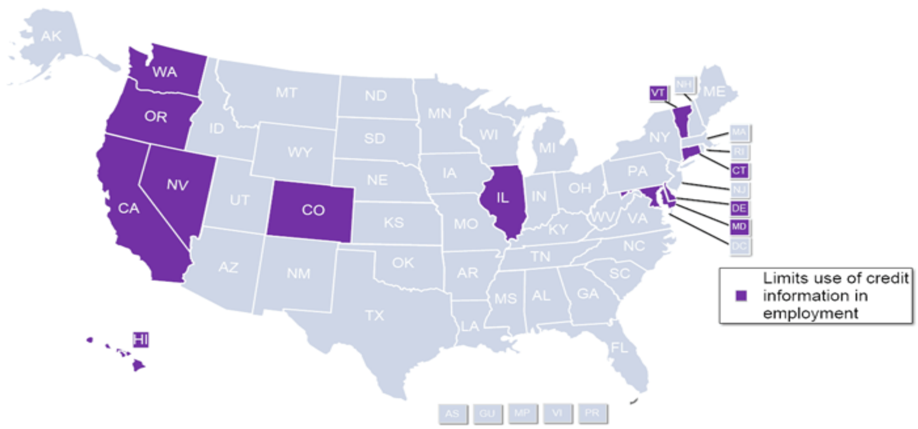

FIGURE 1. CREDIT CHECK BAN LEGISLATION

Notes: State legislation recorded by the National Conference of State Legislatures.

TABLE 1: DATES WHEN BANS WENT INTO EFFECT

State	Date of Effective Law Change	Exempt Occupations (SOC)	Neighboring States
CA	1/1/2012	13, 23, 33	NV, AZ, OR
CO	7/1/2013	13, 23, 33	UT, WY, NE, KS, OK, NM
CT	10/1/2011	13, 23, 33	MA, NY, RI
DE	5/8/2014	13, 23, 33	MD, NJ, PA
HI	7/1/2009	11, 13, 23, 33	none
IL	1/1/2011	13, 23, 33	IN, KY, MO, IA, WI
MD	10/1/2011	13, 23, 33	DE, PA, VA, WV
NV	10/1/2013	11, 13, 23, 33	AZ, CA, ID, OR, UT
OR	3/29/2010	13, 23, 33	CA,ID, NV,WA
VT	7/1/2012	13, 23, 33	MA,NH, NY
WA	7/22/2007	13, 23, 33	ID, OR

Notes: Table displays the states that have enacted employer credit check bans, the date when the ban went into effect, the two-digit SOC codes that we code as exempt occupations, and the neighboring states that are included in the contiguous county specification. State legislation recorded by the National Conference of State Legislatures.

TABLE 2: DISTRIBUTION OF LABOR MARKET VARIABLES AND BAN FLAG – COUNTY LEVEL

	Law Flag			Affected Occupations (Log Vacancies)		
Year	Obs.	Counties with Law in Effect	States with Law in Effect	Obs.	Mean	Std. Dev.
2005	3,141	0	0	124,719	1.76	1.89
2006	3,141	0	0	171,931	1.75	1.94
2007	3,141	39	1	173,153	1.82	1.99
2008	3,141	39	1	175,414	1.84	1.98
2009	3,141	44	2	176,518	1.73	1.91
2010	3,140	80	3	183,186	1.82	1.94
2011	3,138	214	6	190,763	1.94	1.97
2012	3,138	286	8	198,676	2.09	1.97
2013	3,138	367	10	202,288	2.16	1.98
2014	3,137	370	11	206,358	2.23	1.98
2015	3,137	370	11	207,426	2.29	2.00
2016	3,137	370	11	206,886	2.26	1.98
All Years	--	--	--	2,217,318	1.96	1.97
	Exempt Occupations (Log Vacancies)			Unemployment Rate		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
2005	14,306	1.39	1.87	12,546	5.46	2.00
2006	20,211	1.39	1.93	12,546	4.97	1.88
2007	20,553	1.44	1.97	12,560	4.89	1.87
2008	20,903	1.47	1.95	12,560	5.83	2.25
2009	20,331	1.36	1.86	12,560	9.07	3.35
2010	20,979	1.42	1.88	12,556	9.38	3.31
2011	21,849	1.46	1.89	12,548	8.73	3.13
2012	23,209	1.50	1.92	12,548	7.86	2.89
2013	24,452	1.53	1.91	12,548	7.38	2.80
2014	24,263	1.64	1.91	12,547	6.25	2.47
2015	25,106	1.63	1.92	12,544	5.53	2.14
2016	24,612	1.65	1.91	12,544	5.25	1.99
All Years	260,774	1.49	1.91	150,607	6.72	3.03

Notes: Table displays summary statistics of variables used in analysis. “Law Flag” table displays number of states and counties that have passed employer credit check bans in each year. “Affected Occupations” and “Exempt Occupations” tables display cross-sectional means and standard deviations of log-vacancies for each year. “Unemployment Rate” displays cross-sectional means and variances across counties for the unemployment rate.

TABLE 3: DISTRIBUTION OF VACANCIES IN ADJACENT COUNTY SAMPLE – COUNTY LEVEL

	Exempt Occupations (Log Vacancies)			Affected Occupations (Log Vacancies)		
Year	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
2005	6,914	1.31	1.78	61,213	1.76	1.84
2006	9,686	1.38	1.86	83,503	1.75	1.92
2007	9,700	1.51	2.03	83,226	1.83	1.96
2008	10,044	1.53	1.96	84,934	1.84	1.95
2009	10,360	1.38	1.83	84,780	1.70	1.91
2010	10,306	1.44	1.84	88,001	1.83	1.92
2011	10,862	1.45	1.79	92,021	1.96	1.96
2012	11,609	1.45	1.89	95,154	2.08	1.93
2013	11,807	1.55	1.94	96,040	2.13	1.95
2014	11,781	1.63	1.91	99,023	2.20	1.95
2015	12,496	1.65	1.96	99,417	2.29	1.96
2016	11,998	1.68	1.88	99,758	2.25	1.96
All Years	127,563	1.50	1.89	1,067,070	1.97	1.94
	Ever Affected (Log Vacancies)			Never Affected (Log Vacancies)		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
2005	11,129	2.13	1.99	56,998	1.86	1.91
2006	14,883	2.17	2.06	78,306	1.62	1.88
2007	15,112	2.26	2.09	77,814	1.70	1.94
2008	15,179	2.23	2.08	79,799	1.72	1.92
2009	15,043	2.09	2.03	80,097	1.59	1.87
2010	15,406	2.26	2.04	82,901	1.71	1.88
2011	16,112	2.26	2.10	86,771	1.84	1.91
2012	16,078	2.48	2.08	90,685	1.93	1.90
2013	16,818	2.44	2.11	91,029	2.00	1.92
2014	16,850	2.53	2.10	93,954	2.07	1.92
2015	17,159	2.57	2.13	94,754	2.15	1.93
2016	17,113	2.62	2.11	94,643	2.11	1.92
All Years	186,882	2.34	2.08	1,007,751	1.86	1.91

Notes: Table displays summary statistics for vacancies in the adjacent-county sample. "Affected Occupations" and "Exempt Occupations" tables display cross-sectional means and standard deviations of log-vacancies for each year in this sample. "Ever Affected" refers to the county pairs for which credit check bans are active at some point in the sample. "Never Affected" refers to the neighboring counties in the sample that never had a ban.

TABLE 4: DISTRIBUTION OF SUBPRIME BORROWERS USING EQUIFAX RISK SCORE – COUNTY LEVEL

		Fraction Subprime					
Year	Obs.	Mean	25th pct	Median	75th pct	95th pct	Std. dev.
2005	12,560	0.29	0.20	0.27	0.36	0.5	0.12
2006	12,560	0.29	0.20	0.27	0.36	0.5	0.12
2007	12,559	0.28	0.20	0.27	0.36	0.49	0.12
2008	12,556	0.28	0.20	0.27	0.35	0.48	0.11
2009	12,556	0.28	0.20	0.27	0.35	0.47	0.11
2010	12,548	0.26	0.20	0.26	0.34	0.46	0.11
2011	12,548	0.27	0.19	0.25	0.33	0.46	0.11
2012	12,548	0.26	0.19	0.25	0.33	0.46	0.11
2013	12,546	0.26	0.18	0.24	0.32	0.45	0.11
2014	12,544	0.25	0.17	0.24	0.32	0.44	0.11
2015	12,544	0.24	0.17	0.23	0.31	0.44	0.11
2016	12,544	0.24	0.17	0.23	0.31	0.43	0.11
All years	150,613	0.27	0.19	0.26	0.34	0.47	0.11

Notes: Table displays moments of cross-sectional distribution of subprime shares across counties in each year. Fraction of subprime borrowers in a county is determined by counting the number of borrowers residing in each county with an Equifax risk score below 620.

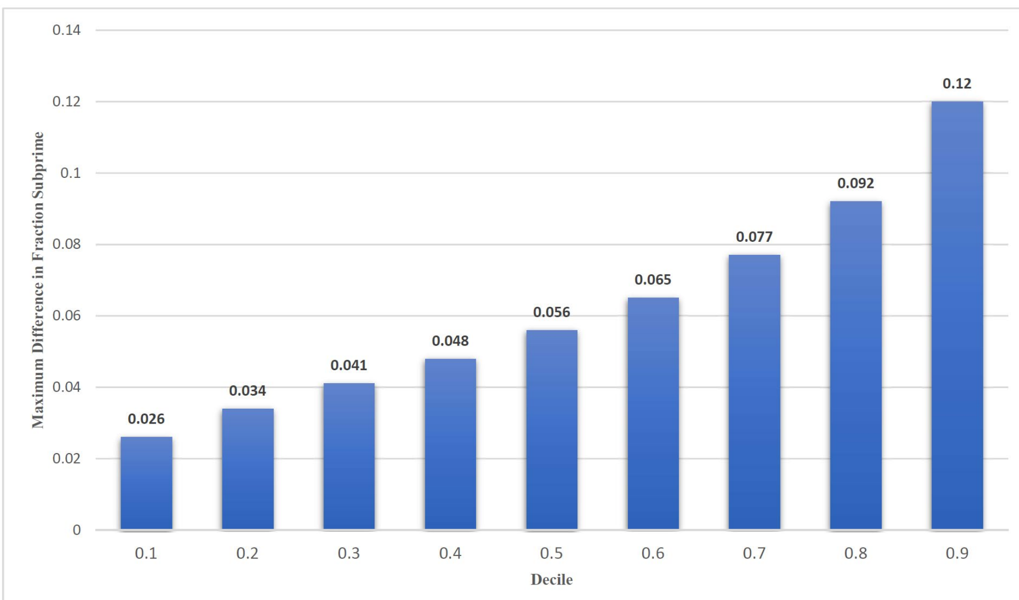

FIGURE 2. WITHIN-COUNTY VARIATION IN FRACTION SUBPRIME

Notes: This figure plots the distribution of a measure of within-county variation in the fraction of subprime borrowers over time. The measure of variation we use is the maximum absolute quarter-on-quarter change in the subprime share for a given county, relative to the cross-sectional average value in 2005.

TABLE 5: BASELINE REGRESSIONS - EXEMPTION STATUS

	Dependent Variable: Log (Vacancies)			
	(1)	(2)	(3)	(4)
	Full Sample	Adjacent County Sample		
Credit check ban * Affected	-0.055**	-0.061	-0.095**	-0.089**
	(0.026)	(0.040)	(0.045)	(0.039)
Credit check ban			0.016	0.031
			(0.046)	-0.048
unemployment rate				-0.012**
				(0.006)
Fixed Effects				
County x Time	Yes	Yes	No	No
Pair x Time x Occupation	No	Yes	Yes	Yes
Occupation x Time	Yes	Yes	Yes	Yes
County x Occupation	Yes	Yes	Yes	Yes
Number of clusters	(50, 23)	(212,1103)	(212,1103)	(212,1103)
Observations	2,475,932	613,402	613,402	612,752
R-squared	0.922	0.979	0.980	0.980

Standard errors clustered at the state level

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

Notes: This table reports OLS regressions for the dependent variable log(vacancies) for each occupation *o,* in county *c (or* county pair *p)* at time *t* (quarterly). Column (1) displays the results from our full sample, whereas (2) - (4) display the results for the adjacent-county subsample. Standard errors are clustered by state and occupation in column (1) and by borders and state-by-occupation tuples in the adjacent-county sample.

TABLE 6: DYNAMIC EFFECTS OF CREDIT CHECK BANS ON LABOR DEMAND

	Dependent Variable: Log (Vacancies)				Log(J2J)
	(1)	(2)	(3)	(4)	(5)
	Exemption Status	Subprime Fraction	Less Skilled	Routine	Exemption Status
Interaction with credit check ban, *t-4*	0.041**	0.180*	0.048	-0.000	-0.006
	(0.019)	(0.100)	(0.064)	(0.044)	(0.025)
Interaction with credit check ban, *t-3*	-0.001	0.022	0.032	-0.011	-0.014
	(0.019)	(0.073)	(0.061)	(0.035)	(0.026)
Interaction with credit check ban, *t-2*	-0.027	-0.128	-0.025	-0.062*	0.007
	(0.024)	(0.108)	(0.037)	(0.031)	(0.016)
Interaction with credit check ban, *t-1*	-0.046	-0.195	-0.001	-0.067*	0.009
	(0.034)	(0.145)	(0.056)	(0.033)	(0.012)
Interaction with credit check ban, *t*	-0.002	0.032	0.001	-0.044	0.004
	(0.036)	(0.148)	(0.064)	(0.038)	(0.012)
Interaction with credit check ban, *1+1*	-0.052**	-0.253***	-0.097	-0.084*	0.025
	(0.024)	(0.089)	(0.067)	(0.048)	(0.016)
Interaction with credit check ban, *t+2*	-0.074***	-0.316***	-0.151**	-0.128**	0.023*
	(0.023)	(0.090)	(0.062)	(0.046)	(0.013)
Interaction with credit check ban, *t+3*	-0.104***	-0.434***	-0.079	-0.090*	0.032**
	(0.036)	(0.141)	(0.068)	(0.052)	(0.008)
Interaction with credit check ban, *t+4*	-0.057	-0.209	-0.092	-0.095**	0.017
	(0.036)	(0.133)	(0.085)	(0.045)	(0.022)
Interaction with credit check ban, *t>4*	-0.056*	-0.243*	-0.164*	-0.137**	0.017**
	(0.033)	(0.127)	(0.087)	(0.049)	(0.008)
County x Time FE	Yes	Yes	Yes	Yes	Yes
County x Occupation FE	Yes	Yes	No	Yes	Yes
Occupation x Time FE	Yes	Yes	No	Yes	Yes
County x Education FE	No	No	Yes	No	No
Education x Time FE	No	No	Yes	No	No
Number of clusters	(50,23)	(50,23)	(50,8)	(50,23)	(50/20)
Observations	2,475,932	2,473,367	976,287	2,475,932	59,481
R-squared	0.922	0.922	0.943	0.922	0.97

Standard Errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports OLS regressions for two dependent variables; log(vacancies) for each county i, at date (quarter) t, for occupation o, in columns (1) through (4), and job-to-job flows for a state s, in industry n at date (quarter) t, in column (5). Column (1) reports the result for the dynamic version of our baseline specification expressed in equation (3) in the text. Analogously, columns (2) – (5) display the dynamic versions of equation (4), (5), (7), and (6), respectively. We have two-way clustered standard errors for each regression reported in the table. The first set of clusters always refers to the number of states and the second group indicates the appropriate number of occupations, education groups, or industries.

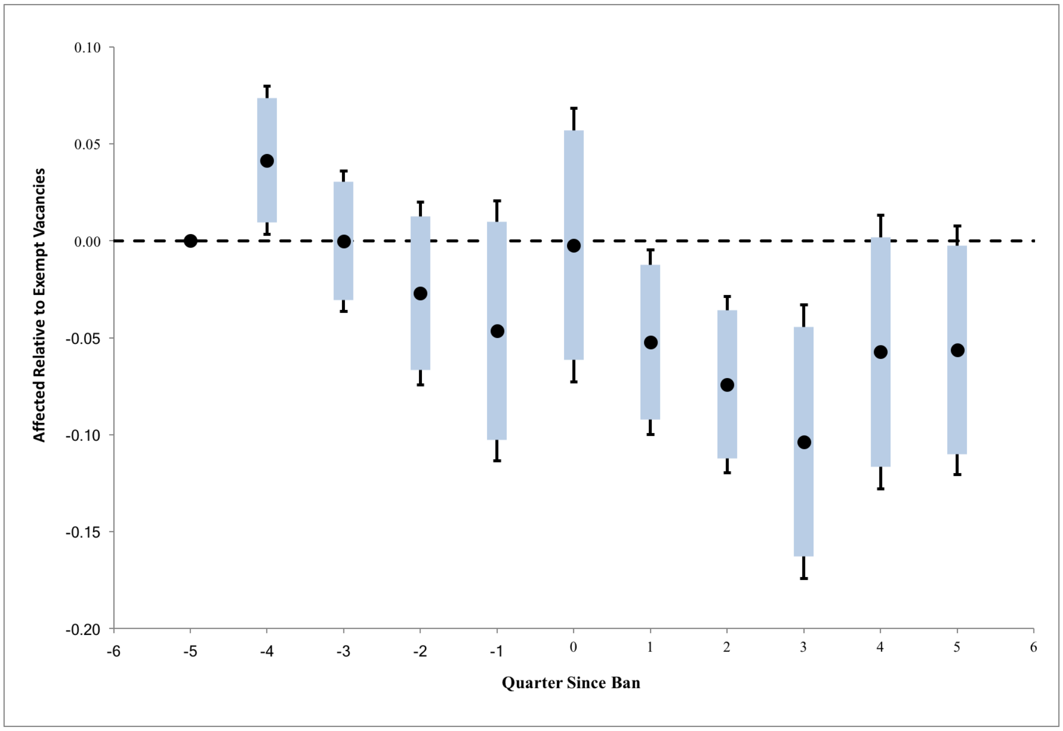

FIGURE 3: DYNAMICS OF VACANCIES FOR AFFECTED RELATIVE TO EXEMPT OCCUPATIONS

Notes: Figure displays estimates of β_j from regression equation (3) in the text. Solid circles correspond to point estimates, while solid box error bars correspond to 90% confidence intervals and capped lines correspond to 95% confidence intervals. Quarters -5 and 5 correspond to average in periods more than one year before and after the ban and all differences are relative to periods more than one year before the ban (-5 is zero by construction).

TABLE 7: CHANGES IN OTHER LABOR MARKET OUTCOMES POST-BAN

	Nationwide Samples			Adjacent County
	Unemployment Rate	Separation Rate	Finding Rate	Unemployment Rate
	(1)	(2)	(3)	(4)
Credit check ban	0.150	0.027	-0.027	0.220
	(0.186)	(0.021)	(0.018)	(0.223)
County / State FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	No
County pair x Time FE	No	No	No	Yes
Number of clusters (states or states & borders)	51	51	51	256
Observations	150,607	150,607	71,704	71,704
R-squared	0.826	0.826	0.932	0.931

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

Notes: This table reports OLS regressions for various labor market measures at a quarterly frequency. Column (1) reports the regression results for dependent variable, unemployment rate at the county level. Columns (2) and (3) report the regression coefficients with state-level (log) separation and job-finding rates as the dependent variable. Column (4) reports regression results for unemployment rate focusing only on adjacent-border counties.

TABLE 8: BASELINE REGRESSIONS - INSPECTING THE MECHANISM

	Dependent Variable: Log (Vacancies)			log(J2J)
	(1)	(2)	(3)	(4)
	Subprime	Less Skilled	Routine	Exempt
Credit check ban * Affected * Fraction subprime	-0.234**			
	(0.093)			
Credit check ban * Less than College		-0.146*		
		(0.072)		
Credit check ban * Routine			-0.117***	
			(0.041)	
Credit check ban * Affected				0.016**
				(0.006)
Fixed Effects				
County x Time	Yes	Yes	Yes	No
Pair x Time	No	No	No	No
Occupation x Time	Yes	Yes	Yes	Yes
County x Occupation	Yes	Yes	Yes	Yes
Number of Clusters	(50, 23)	(50,8)	(50,23)	(50,20)
Observations	2,473,367	976,287	2,475,932	58,141
R-squared	0.923	0.943	0.922	0.9703

Standard errors clustered at the state level
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table reports OLS regressions for two dependent variables; log(vacancies) for each county i, at date (quarter) t, for occupation o, in columns (1) through (3), and job-to-job flows for a state s, in industry n at date (quarter) t, in column (4). Column (1) reports the result for the baseline specification exploring the role of heterogeneity by county-level subprime rates expressed in equation (4) in the text. Analogously, columns (2) – (4) display the estimates of the regression coefficients from specifications (5), (7), and (6), respectively. We have two-way clustered standard errors for each regression reported in the table. The first set of clusters always refers to the number of states and the second group indicates the appropriate number of occupations, education groups, or industries.

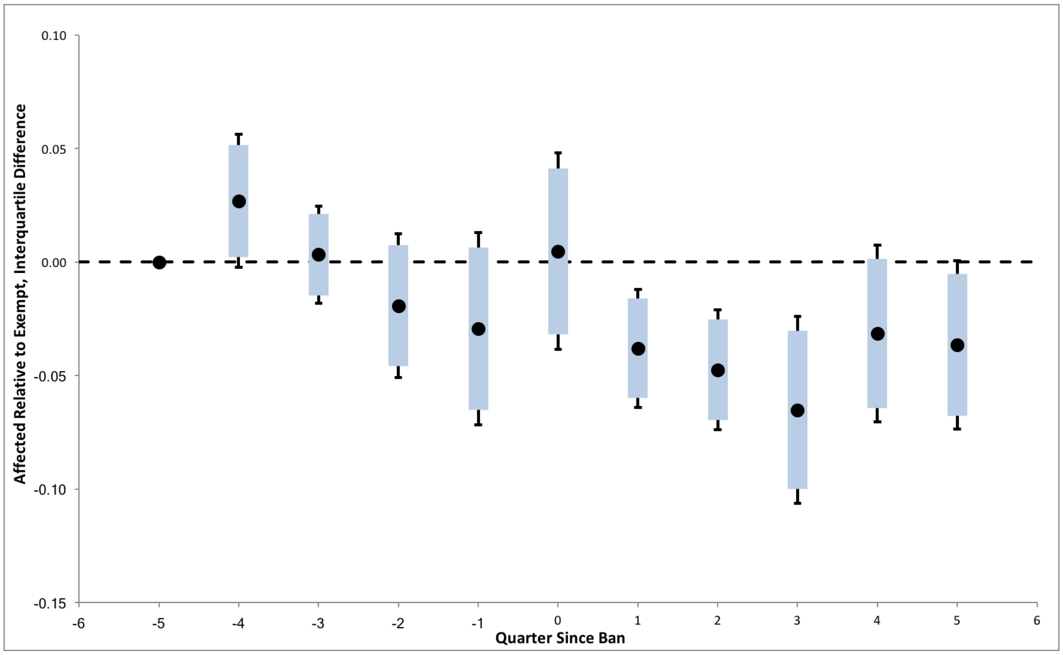

FIGURE 4: DYNAMICS OF VACANCIES BY COUNTY SUBPRIME RATE

Notes: Figure displays estimates of β_j from regression equation (4) in the text, scaled by the interquartile range of county-level subprime rates. Solid circles correspond to point estimates, while solid box error bars correspond to 90% confidence intervals and capped lines correspond to 95% confidence intervals. Quarters -5 and 5 correspond to average in periods more than one year before and after the ban and all differences are relative to periods more than one year before the ban (-5 is zero by construction).

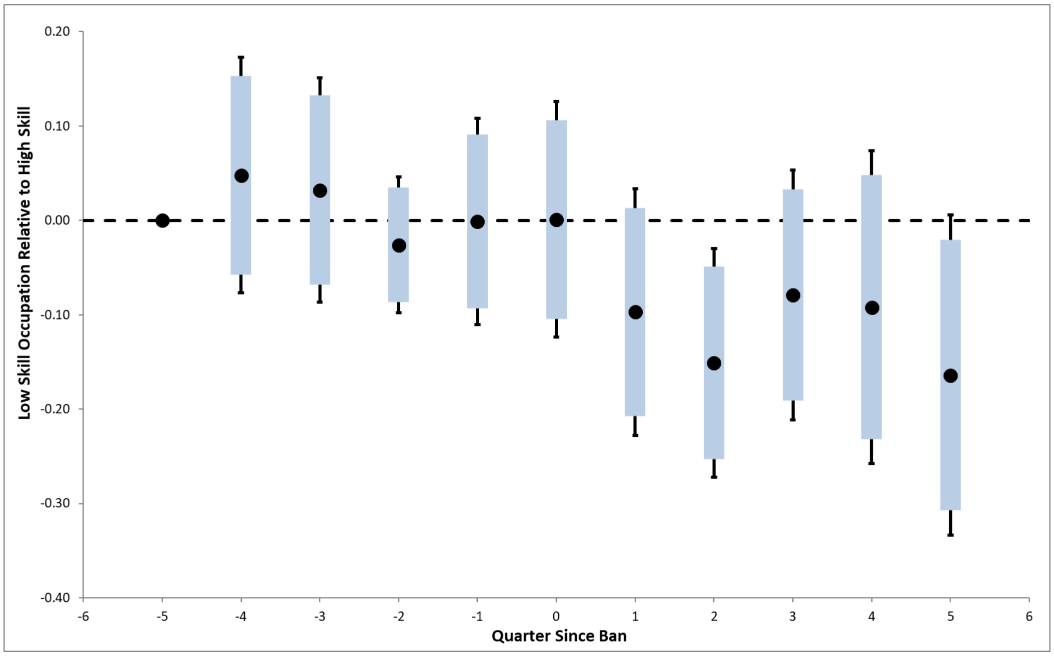

FIGURE 5: DYNAMICS OF VACANCIES BY OCCUPATIONAL SKILL

Notes: Figure displays estimates of β_j from regression equation (5) in the text. Solid circles correspond to point estimates, while solid box error bars correspond to 90% confidence intervals and capped lines correspond to 95% confidence intervals. Quarters -5 and 5 correspond to average in periods more than one year before and after the ban and all differences are relative to periods more than one year before the ban (-5 is zero by construction).

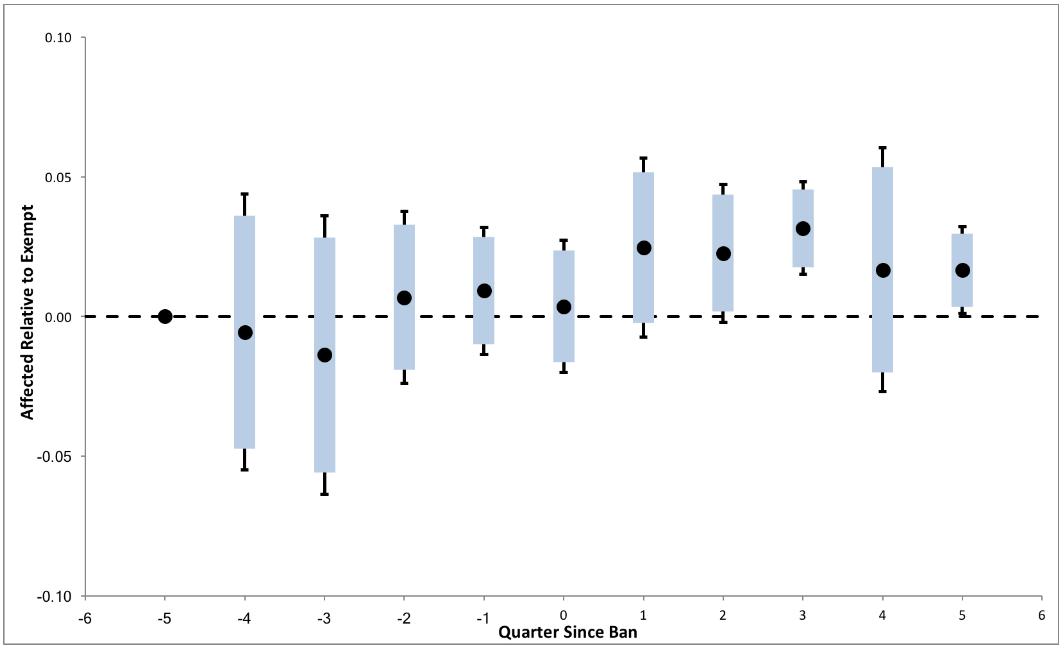

FIGURE 6: DYNAMICS OF JOB-TO-JOB FLOW IN AFFECTED RELATIVE TO EXEMPT INDUSTRIES

Notes: Figure displays estimates of β_j from regression equation (6) in the text. Solid circles correspond to point estimates, while solid box error bars correspond to 90% confidence intervals and capped lines correspond to 95% confidence intervals. Quarters -5 and 5 correspond to average in periods more than one year before and after the ban and all differences are relative to periods more than one year before the ban (-5 is zero by construction).

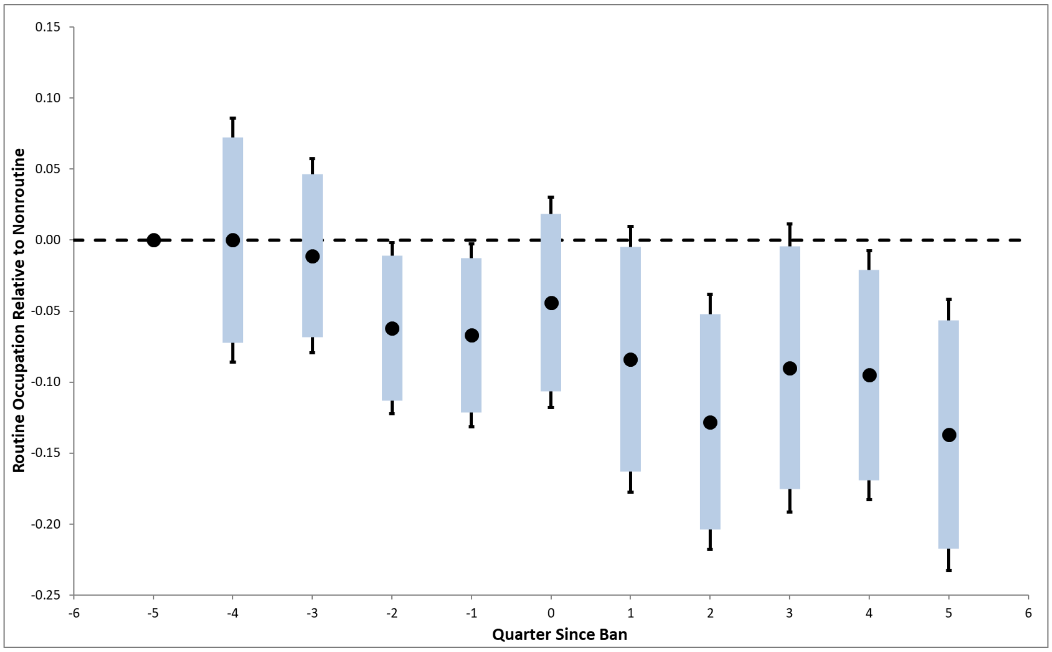

FIGURE 7: DYNAMICS OF VACANCIES BY TASK TYPE

Notes: Figure displays estimates of β_j from regression equation (6) in the text. Solid circles correspond to point estimates, while solid box error bars correspond to 90% confidence intervals and capped lines correspond to 95% confidence intervals. Quarters -5 and 5 correspond to average in periods more than one year before and after the ban and all differences are relative to periods more than one year before the ban (-5 is zero by construction).

APPENDIX A1

Figures A1 and A2 compare aggregate time series for the group of states that have passed an employer credit check ban as of December 2018 with the states that have not yet passed a ban. In most dimensions, ban states are better off over this time span than non-ban states. Figure A1 shows that they have lower poverty rates, higher median income, lower rates of subprime credit, and fewer households with delinquencies on their credit reports. Figure A2 shows that they tend to have similar job-finding rates, but slightly higher separation and unemployment rates, at least in the years after the start of the Great Recession.

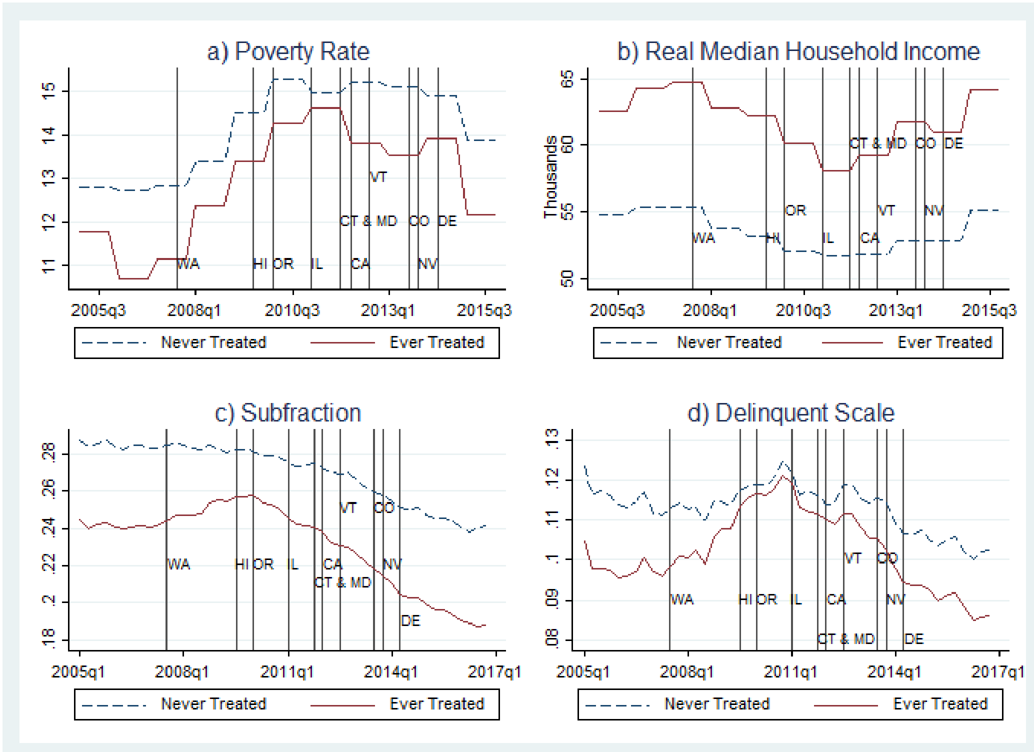

FIGURE A1. POVERTY, INCOME, AND CREDIT MARKET OUTCOMES ACROSS STATES AND THE CREDIT CHECK BANS

Notes: The data for Poverty Rate and Median Household Income come from Small Area Income and Poverty Estimates (SAIPE) conducted by the Census Bureau. The fraction of subprime borrowers (Subfraction) and the percentage of loans that are delinquent come from FRBNY CCP/Equifax. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect

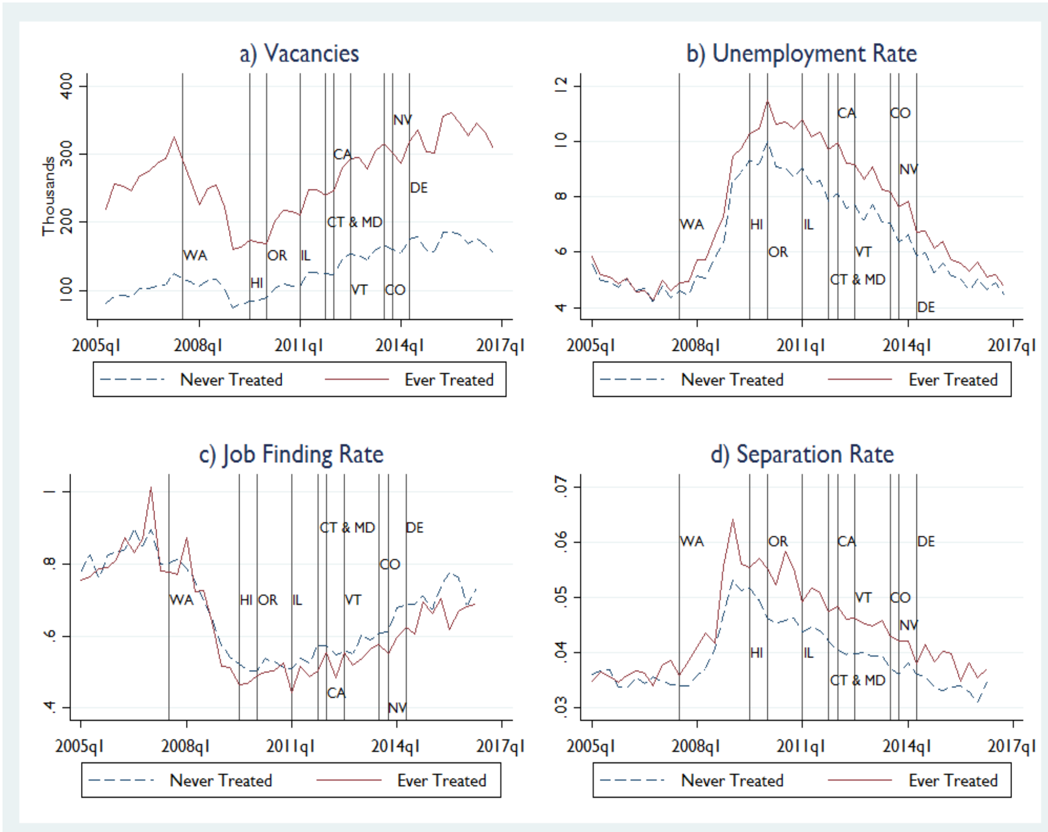

FIGURE A2. CREDIT CHECK BANS AND LABOR MARKET OUTCOMES ACROSS STATES

Notes: The data come from HWOL and CPS state-level aggregates, in which each state is weighted by its labor force. The samples (never treated and ever treated) consist of the same states over time, with each treated state indicated by a vertical line signifying when the ban comes into effect.