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No Woman No Crime: Ban the Box, Employment, and Upskilling

Faculty Research Working Paper Series

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March 2016

RWP16-015

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No Woman No Crime

Ban the Box, Employment, and Upskilling[i]

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March 28, 2016

A sizable number of localities have in recent years limited the use of criminal background checks in hiring decisions, or "banned the box." Using LEHD Origin-Destination Employment and American Community Survey data, we show that these bans increased employment of residents in high-crime neighborhoods by as much as 4%. These increases are particularly large in the public sector. At the same time, we establish using job postings data that employers respond to ban-the-box measures by raising experience requirements. A perhaps unintended consequence of this is that women, who are less likely to be convicted of crimes, see their employment opportunities reduced.

[i] We thank Nikolai Boboshko for outstanding research assistance, and Jennifer Doleac for insightful comments.

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Large numbers of employers in the United States, if not most, include questions along the lines of "Have you ever been convicted of a crime?" in employment applications, or ask applicants to check a box to indicate that they have been convicted of a crime. Efforts to remove such questions gained steam over the last few decades as increasingly large numbers of Americans saw their chances of gainful employment limited by the interplay of mass incarceration and employers' reluctance to hire convicts. In response, various jurisdictions, government agencies, and private-sector firms decided to eliminate questions about applicants' criminal background or to mandate that employers do so, i.e., to "ban the box."

Our goal in this paper is to study the effects of this latter response - bans on questions about criminal records (early on) in employee screening processes - on the labor market prospects of various affected groups and on the way in which employers respond to them. The mere recency of these bans means that research on their consequences has so far been quite limited. We exploit variation in whether and when cities, counties and states implemented them to identify their significance using LEHD Origin-Destination Employment Statistics (LODES) data on employment outcomes. We do this, mostly, with difference-in-difference and triple-difference estimators that compare different groups or locations within cities as these cities adopt bans at different points in time.

Our first central result is that these policies raise the employment of residents of the top quartile of high-crime neighborhoods by as much as 4%. This robust increase is in large part driven by residents getting hired into relatively high-income jobs (over $40,000 in annual wage income) and finding work in the public sector.

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We then study a number of groups distinguished not by their place of residence, but by more permanent demographics. American Community Survey (ACS) Integrated Public Use Microdata Series data that allow us to tie outcomes to such individual characteristics indicate that women, who are less likely to have been convicted of crimes, see their labor market outcomes deteriorate, while low-skilled African Americans benefit. The most likely mechanism leading to worse employment outcomes from women is that employers raise their education and experience requirements in response to the elimination of questions concerning applicants' criminal records. We study this mechanism using data on online job postings, and find "upskilling" does indeed occur after the implementation of Ban the Box measures.

These results highlight both the importance of ban-the-box initiatives and some of their unintended consequences. In addition, this evidence runs counter to Holzer et al.'s (2006) finding that African Americans benefit from criminal background checks because they undermine a perceived necessity for statistical discrimination against them.

We proceed as follows. In the next section, we present background information on the role played by employee screening procedures and criminal records play in hiring processes, as well as the roll-out of the policies we study. We then turn to our theoretical framework and our empirical approach. The next two sections present our results, first for neighborhoods and then for different demographic groups, before we conclude.

I. Criminal Records in Employee Screening

In the early stages of interacting with potential employers, job seekers are often asked whether they have ever been convicted of a crime. In addition, many organizations run criminal background checks on potential employees, forcing applicants to respond truthfully. Estimates of the share of organizations carrying out such checks range from slightly fewer than half of all private-sector firms to practically all government agencies (Connerley et al., 2001). Oft-cited goals of these employee screening practices are to mitigate risk of fraud or criminal activity by employees (Hughes et al., 2013), to protect oneself from negligent hiring lawsuits (Connerley et al., 2001), or, more generally, to avoid employing persons of poor character, skills, and work ethic, or who are likely to be arrested again soon (Freeman, 2008; Gerlach, 2006). In addition, federal and state laws ban certain employers, including public-sector employers, from hiring ex-offenders for certain positions and/or mandate criminal background checks (Freeman, 2008).

Job applicants are thus likely to be confronted with inquiries regarding any past run-ins with the law, and they are also likely to be excluded from consideration or subjected to additional scrutiny by potential employers if they have experienced any (Stoll and Bushway, 2008). This affects a significant chunk of the population: as many as 65 million people are estimated to have been arrested and/or convicted of criminal offenses (Natividad Rodriguez and Emsellem, 2011). Different groups are affected to dramatically different extents. Whereas about one out of every three African-American males, and one out of six Hispanic males will spend time incarcerated over their lifetime (Bonczar, 2003), women are convicted at much lower rates, and account for only 7% of the federal and state prison population (Carson, 2015).

This state of affairs has long concerned some academics, activists and policymakers, because making it harder for convicts to find gainful employment may increase rates of recidivism while reducing the output and productivity of these potential workers (Henry and Jacobs, 2007; Nadich, 2014; The White House, 2015). In addition, the adoption of an applicant's criminal history as a key hiring criterion is presumed to have an adverse impact on minority applicants because African Americans and Hispanics represent a much larger share of arrestees and convicts than their population share (Henry, 2008).

To assuage such concerns, a sizable numbers of cities, counties, and states have adopted legislation or other measures that prohibit the use of criminal background questions in the early stages of application procedures, starting with the state of Hawaii in 1998. As Figure 1 and Appendix Table 1a and 1b show, in the last five years we have witnessed a veritable explosion of activity on this front. In 2015 the federal government followed suit via executive order (Korte, 2015). A number of private-sector employers, most prominently Home Depot, Koch Industries, Target, and Walmart, have recently adopted a policy of not asking prospective employees about their criminal history as well (Levine, 2015; Staples, 2013). In Figures 2 and 3 we show suggestive evidence of the impact the adoption of this policy has had at Walmart, the largest private employer in the United States with some 1.4 million domestic employees. Figure 2 shows that the ratio between the percentage of female employees at Walmart and its EEO-1 benchmark decreased after the company banned the box, while the opposite holds true for its share of African-American employees. Figure 3 shows that these changes were concentrated among non-managerial job categories.

Our goal in this paper is study what effects measures implemented by governments so far have had on labor market outcomes. There is a range of outcomes of interest, including employment, wages, and job requirements, involving different employers and different groups of (potential) employees.

II. Theory and Data

We start this section by sketching a simple model of screening in hiring decisions, to generate insights grounded in theory as to what the consequences of Ban the Box legislation can be. To evaluate these possibilities we draw on a number of different data sets, and we present their basic characteristics, as well as summary statistics for our sample, in the remainder of this section.[1]

II.1 Theoretical Framework

We conceptualize the way in which employers approach the decision of whether to hire an applicant as a screening problem, as in Aigner and Cain (1977), Autor and Scarborough (2008), or Wozniak (2015). Assume that there are two easily identifiable groups x_1 and x_2 from which workers are drawn. Employers want to hire a worker of quality $w > k$, where k is a given threshold. The distribution of worker quality conditional on group origin is known to be normal, with means μ_1 and μ_2 (where $\mu_1 > k > \mu_2$) and standard deviation σ, as in Autor and Scarborough (2008). Now assume that information derived from the worker's answer to a question about his criminal record provides a signal of an individual's true quality $y = w + \varepsilon$, where ε is normally distributed mean-zero noise with standard deviation γ. Note that because this is an unbiased signal, fewer workers in group x_1 will check the box than in group x_2.

Employer v's expectation of a worker's quality is then a weighted sum of her prior and her signal, $E[quality|y\ x_i] = \frac{\gamma^2}{\sigma^2+\gamma^2}\mu_i + \frac{\sigma^2}{\sigma^2+\gamma^2}y$, and if it exceeds k_v, the applicant will be hired. Eliminating the signal has two effects. Some individuals from group x_1, the "advantaged" group, with criminal records will now be hired $\left(y_i < \frac{\sigma^2+\gamma^2}{\sigma^2}k - \gamma^2\mu_1\right)$, while some individuals from group x_2 without criminal records will not be $\left(y_i > \frac{\sigma^2+\gamma^2}{\sigma^2}k - \gamma^2\mu_2\right)$. Eliminating the signal can

[1] This section draws heavily on a similar section in Clifford and Shoag (2016).

thus harm upstanding members of the disadvantaged group even if, on average, their signals are worse. In addition, the employment differential between the groups can be exacerbated if employers respond to a ban by shifting to alternative, more precise signals, as in Clifford and Shoag (2016). On the other hand, if signals are biased against members of group x_2 but employers treat them as unbiased, eliminating the signal ceteris paribus helps the disadvantaged group. We now turn to the data we will use to determine which of these theoretical mechanisms dominates.

II.2 Data

National Employment Law Project

The National Employment Law Project Conference, as a part of its "Fair Chance" campaign, collects and disseminates data on city-, county- and state-level Ban the Box policies. Summaries of the bills and executive orders restricting or eliminating inquiries into applicants' criminal background that have been adopted at different levels of government are readily available in its guide on state and local policies and on its website (Natividad Rodriguez and Avery, 2016). Although these policies vary in their restrictiveness and in how comprehensively they apply to employers and producers, for the purpose of our analysis we do not draw such distinctions, partially to avoid arbitrary assignments of treatment regimes, and partially because we believe that sector-specific or public-sector-only measures may well have spillover effects on other sectors. Appendix Table 1 provides a list of state and local government entities that had passed Ban the Box measures by the end of 2013 and when they did so, while Figure 1 shows the cities in our sample, to be discussed below, that had passed such measures by then.

Crime Data

To identify high-crime neighborhoods, we draw from the National Neighborhood Crime Study (NNCS). This dataset includes tract-level information for seven of the FBI's crime index offenses. It covers 9,593 census tracts in 91 cities in 64 metropolitan areas. Because much of our empirical analysis relies on an identification approach that exploits variation in crime rates between census tracts, we limit those parts of our analysis to these cities. We rank census tracts based on the number of assaults and murders per capita, and label the 25% most violent tracts as "high-crime."

The LEHD Origin-Destination Employment Statistics

The LEHD Origin Destination Employment Statistics (LODES) data report employment counts at detailed geographies. The U.S. Census Bureau produces them using an extract of the Longitudinal Employer Household Dynamics (LEHD) data, which are in turn based on state unemployment insurance earnings data, Quarterly Census of Employment Wages (QCEW) data, and additional administrative, survey, and census data. The state data cover employers in the private sector and state and local government, and account for approximately 98 percent of wage and salary jobs in those sectors; the additional administrative include data on federal workers covered by the Unemployment Compensation for Federal Employees program. The LODES data are published as an annual cross-section from 2002 onwards, with each job having a workplace and residence dimension. The data are available for all states but Massachusetts.

A LODES place of work is defined by the physical or mailing address reported by employers in the Quarterly Census of Employment and Wages (QCEW), while workers' residence is derived from federal administrative records. For privacy purposes, LODES uses a variety of methods to shield workplace job counts and residential locations. Residence coarsening occurs at most at the census tract level, which is why we use that as our most granular level of analysis. Further explanation of this process can be found in Graham et al. (2014). The extra noise is intentionally random, meaning that while it might inflate our standard errors, it should not bias our results. Table 1 provides basic properties of the data at the tract-year and the origin tract-place destination-pair-year level.

Burning Glass Technologies Labor Insight Data

Burning Glass Technologies (BGT) is a leading provider of online job market data. Its Labor Insight analytical tool supplies detailed information on millions of job advertisements from 40,000 online sources including job boards and employers websites. This information is updated daily and collected by "spider" software tools that crawl across the web to parse ads into usable elements, including employer name, location, job title, occupation, and experience and education requirements and preferences. For our purposes, what is important here is that these allows for a granular geographical analysis of the education and experience demands associated with job postings. In total, we have access to data on over 74 million postings from over 4,000 cities between 2007 and 2014.[2] Basic summary statistics for these data are provided in Table 1.

American Community Survey Integrated Public Use Microdata Series

We use data from the American Community Survey Integrated Public Use Microdata Series provided by the Minnesota Population Centers to associate variation at the coarser state level with individual demographics. Basic features of the data are again provided in Table 1.

III. Employment Outcomes for Residents of High-Crime Areas

[2] Sasser Modestino, Shoag, and Ballance (2015) describe this process in more detail.

In this section we present our first key result: that the residents of high-crime neighborhoods benefit, on average, from Ban the Box legislation. We use two methods to identify the effect of such bans on the employment opportunities of these workers. The first one exploits variation in crime rates across different census tracts to identify potential workers affected by bans. We refer to these estimates as between-tract. The second one uses an additional layer of identifying variation: whether the tracts in which these residents work have adopted bans or not. We refer to this as within-tract variation.

III.1 Cross-Tract Identification

Our first estimator is a difference-in-difference estimator that works as follows. We compare employment for the residents of high-crime neighborhoods to employment for the residents of low-crime neighborhoods before and after the introduction of a ban. As discussed in the previous section, to identify high-crime and low-crime census tracts, in our baseline estimates we label the 25% most violent tracts high-crime and other tracts low-crime. We then estimate the following regression equation:

$$ln\ emp_{i,t} = \alpha_i + \alpha_{city \times t} + \alpha_{high\ crime \times t} + \beta_t\ x\ ban_{it}\ x\ high\ crime_{\ i} + \varepsilon_{it}, \quad (1)$$

where $emp_{i,t}$ is the number of residents of tract i employed in period t, α_i represents tract-level fixed effects, $\alpha_{city*year}$ controls for arbitrary trends at the city level with city-year pair fixed effects, and $\alpha_{high\ crime*year}$ controls for arbitrary, employment trends in high-crime versus low-crime tracts. We interact two dummies, for whether a tract had a ban in a certain year and whether it was a high-crime tract, to create our variable of interest.

The first column in Table 2 shows the results of this estimation. High-crime tracts subject to a ban see employment increase by 3.5% compared to high-crime tracts in cities that were not subject to a ban, even after controlling for tract-level fixed effects and arbitrary citywide trends. To test the strength of this result, we conducted a series of placebo tests. In each test, we randomly re-assign our existing set of ban the box law to placebo cities. By randomly re-assigning the time series of laws as opposed to using a purely probabilistic procedure, we ensure that each placebo has the same number of cities with a ban each year as the true distribution. We then re-estimate our baseline specification using the randomly assigned laws, and we repeat this procedure 100 times. We find that our estimate using the true assignment of laws exceeds 98% of the placebo estimates. We therefore feel confident that the relationship we find is not a spurious one.

The estimate reported in column 2, which is of remarkably similar economic and statistical significance, comes from a regression that, in addition, controls for separate linear time trends in employment for low- and high-crime tracts by city. Columns 3 and 4 show that are results remain significant if we cluster our standard errors at the city level, while columns 5 and 6 show that our results barely change if we define only the 10% most violent tracts as high-crime instead of the top 25%.

Figure 1 shows an event study style depiction of this impact as it evolves over time, estimated using separate dummies for each pre- or post-ban year as opposed to the single pre/post dummy included in in equation 1 above. We see no pre-trend that would lead us to believe that our estimates are somehow contaminated by divergent trends. This is reassuring, but not entirely surprising given that we control for arbitrary trends at the city level as well as between high-crime and low-crime neighborhoods. What we do see is effectively a level increase in high-crime area employment in the years after the ban is introduced, with minor fluctuations around our baseline 3.5% increase estimate.

III.2 Within-Tract Identification

The results in the previous subsection show quite convincingly that Ban the Box measures have a positive effect on the employment chances of the residents of high-crime areas. The level of detail reported in the LODES data allows us to test the robustness of this result by exploiting not just where people reside, but also where those same people commute to. That is, we know from the data where the residents of a given tract go to work, and in some cases their commutes take these residents both to destination tracts that are subject to and to destination tracts that are not subject to Ban the Box measures. In effect, what that means is that we estimate the following regression equation:

$$ln\, emp_{od,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta \times ban_{dt} \times high\, crime_o + \varepsilon_{od,t}\ , \quad (2)$$

where α_{od} represents tract-pair-level fixed effects that control for baseline differences across tract-to-tract flows between origin tract *o* and destination tract *d*, α_{d*t} controls for arbitrary trends at the destination level with destination-year fixed effects, and α_{o*t} controls for aggregate outcomes for the tract in a given year. These fixed effects allow us to study within-tract-year variation. What this variation allows us to learn about is the *differential* impact of a ban at a work location on the employment of residents of high-crime tracts compared to the residents of a low-crime tract, conditional on all of the included fixed effects.

We report our estimates in Table 3. Column 1 shows that the effect is an increase in employment of 4.1%, which is remarkably similar to our result from the previous subsection. Column 2 restricts the data to origin tracts without a ban, identifying the effect off cross-city commuting. This reduces the effect we find by about half – which is unsurprising, given that commuting flows within city are greater than between cities – but confirms the robustness of our results despite reduced power, as the effect remains both statistically and economically highly significant.

IV. The Mechanics of Improved Employment Outcomes in High-Crime Areas

The LODES data allow us to identify not just how many residents of given tracts are employed, but also what their wages are, that is, whether it is below $15,000 annually, between $15,000 and $40,000, or over $40,000, and in which industry category they work. We exploit these distinctions to demonstrate what types of work and what levels of remuneration the residents of high-crime areas manage to find and receive when Ban the Box measures are implemented. At this level of detail, the identification strategy of subsection III.1 is more feasible than that of subsection III.2, and we revert to the former.

IV.1 Wage Levels

Table 4 shows our results for different wage bins. The regressions we run here mimic the first column of Table 2, and allows us to estimate the increase in employment for residents of high-crime tracts subject to a ban compared to high-crime tracts in cities that were not subject to a ban, even after controlling for tract-level fixed effects and arbitrary citywide trends for the different wage bins. The estimates may seem, at first blush, surprising: they are greatest for our highest-income bin (at a little over 10%), and negligible for annual wages below $15,000. The next subsection offers a potential explanation for this result.

IV.2 Industries

Table 5 and 6 show our results split out by broadly defined industry.[3] The regressions we estimate in these two tables are again just like those in the first column of Table 2, this time with the sample split up by industry. Table 5 shows industries that witnessed a statistically significant increase in employment for the residents of high-crime neighborhoods of at least 3%, while Table 6 shows estimates for all other industries. These latter estimates are all smaller than 3% and not different from 0 at the 95% confidence level.

The industries with a large increase in high-crime area resident employment are, in order of percentage increase size, government (12.1%), information (5.3%), education (4.2%), real estate (4.1%), and health care (3.7%). Missing from this list are industries with large numbers of minimum-wage workers such as retail, accommodation, and food services, which may well explain the relatively high wages of ban beneficiaries. The most obvious explanation for this, in turn, is that many of the Ban the Box measures we study here apply principally to the public sector and that compliance there is likely to be higher. We show our estimates for the remaining industries in Table 6, where we find particularly small point estimates in the entertainment, waste management, and wholesale sectors.

V. Upskilling

Employers, of course, are free to adjust to the new labor market shaped by restrictions on inquiries into applicants' criminal history. We saw in subsection II.1 that there are various ways in which this may affect different groups, and that only the empirical evidence can tell who will benefit and who will not. In this section we look at whether employers substitute toward other signals after Ban the Box measures are implemented, while in the next section we investigate

[3] The industry categorization is the one used in the LODES data; assignments of jobs to different categories are determined there as well. Appendix 2 shows the crosswalk from this categorization to NAICS codes.

what the total effect of bans and demand side responses is for two sizable groups of particular interest, women and African-Americans.

To study the employer response, we use the data on job advertisements from BGT described in section II. The most detailed geographical level to which we can tie these ads is the city level, and this is the level of aggregation at which we estimate the degree of signal substitution. We do so by estimating regression equations in the spirit of equation 1, that is, of the following type:

$$skill\ level_{city\ t} = \alpha_{city} + \alpha_t + \beta_t\ x\ ban_{it} + \varepsilon_{it}, \qquad (3)$$

where *skill level*$_{city,t}$ is the skill-related dependent variable of interest, α_{city} represents city-level fixed effects, and α_t controls for year fixed effects. The dependent variables we study are average experience required (in years), the share of postings requiring no experience, and the share of postings requiring a college degree. In addition to this baseline specification, we test the robustness of our findings by including state-by-year fixed effects to allow for arbitrary trends (instead of year dummies), and linear city trends (again, instead of year dummies). Estimates are shown in Table 7.

We see in column 1 that after Ban the Box measures are implemented, firms respond by raising the required number of years of experience in the job advertisements in our sample by about 5% of a year, or a little over two weeks. Allowing for arbitrary trends at the state level raises this number ever so slightly (see column 2). Our second measure of skill requirements, the share of postings that do not need experience at all, confirms that firms respond to the ban on criminal background questions by raising posted experience requirements: between 1% and 2% more of job postings after the introduction of a ban demand at least some prior experience (see column 3 and 4).

These increasing experience requirements are in line with what we see for educational requirements. There, in columns 5 and 6, after the passage of a Ban the Box measure, we see a statistically significant increase in the share of postings that require a college degree of up 1.5%, depending on the specification. In sum, firms respond to Ban the Box measures by shifting to the use of other signals, including increased education and experience requirements. In the next section we see how this, combined with the direct effects of Ban the Box measures, affects different demographic groups.

VI. Intended and Unintended Consequences

We discussed in the introduction that one of the motivations driving efforts to implement Ban the Box measures is to help minorities, in particular low-skilled African-American men, who are more likely to have been convicted of crimes than the population as a whole. In this section we analyze whether this objective is being met, and whether women, who are much less likely to have criminal records, suffer as a consequence. To study the questions we use ACS data that allows us to link employment outcomes to race and gender. We cannot tell where individuals live beyond that state level, so for this section we focus on variation created by the decisions of states to pass Ban the Box legislation.[4] This identification strategy is perhaps not as convincing as the ones employed in previous sections, and we consider the results we present here to be suggestive, not conclusive.

We first estimate regression equations of the following type:

$$y_{it} = \alpha_{\mathrm{black-state}} + \alpha_{\mathrm{state,t}} + \alpha_{\mathrm{black,t}} + \beta_{\mathrm{t}} \times \mathrm{ban}_{\mathrm{state,t}} \times \mathrm{black}_{\mathrm{it}} + \varepsilon_{\mathrm{it}}$$

[4] See Appendix Table 1A for the list of states that have done so and the years in which they did.

where the αs represent controls for arbitrary trends for African-Americans and for states, and for arbitrary racial differences across states. The sample here is limited to adult males with less than a high school degree. As Table 8 shows in column 1, we find significantly increased employment for low-skilled African-American men: the number of employed individuals in this category goes up by over 3%. This result holds when we allow for state-specific trend divergence for African-Americans (in column 2), and when we control for individual-level age and education characteristics. On the other hand, columns 4 through 9 show that te do not see an increase in income or wages, which may be due to the fact that the newly employed are relatively less skilled along unobservable dimensions than the group they are joining.

The observed increase in employment among low-skilled African-American males is one of the implicitly intended consequences of Ban the Box legislation. It suggests that the gains from not being asked to disclose criminal records, for this group, outweigh the detrimental impact of the shift to higher experience and education requirements.

The other side of the coin becomes apparent in Table 9, where we show estimates of similar regression equations for the full non-institutionalized population between age 19 and 65. We learn from those estimates that women see their likelihood of employment drop by a borderline statistically significant 0.2% - 0.4% (columns 1 through 3) and their wages and income by a little short of 1% (column 4 through 6 and column 7 through 9, respectively). This is an unintended consequence of Ban the Box legislation, but not necessarily an unexpected one, as women are much less likely to have been convicted of crimes than men.

VII. Discussion and Conclusion

We have reported three findings in this paper. Ban the Box measures 1) improve the labor market outcomes of residents of high-crime neighborhoods, 2) lead to signal substitution toward higher education and experience requirements by employers, and 3) help low-skilled African-American workers while probably harming the labor market opportunities female workers.

The first finding shows that Ban the Box legislation appears to have been successful if judged on the basis of its proclaimed proximate objective: making it easier for individuals with criminal records to find and retain employment. It has increased employment in the highest-crime neighborhoods by as much as 14%. The mechanism through which this happened seems quite straightforward: in all likelihood, employers who used to ask about an applicant's criminal history used to scare some potential employees away and used to choose not to interview some others. In addition, the normalization of incorporating applicants' criminal histories in the hiring process is likely to have led to a rise in the number of criminal background checks that were carried out, and Ban the Box measures appear to have stemmed this rise. Some suggestive evidence for this comes from the Survey of State Criminal History Information Systems, published by the Bureau of Justice of Statistics. The survey provides us with the number of background checks for reasons not directly related to the administration of the criminal justice system for 45 states in the years 2006, 2008, 2010, and 2012. We divide this number by the number of new hires in each state in the corresponding year as published by the Census Bureau in its Quarterly Workforce Indicators to create a measure of criminal background checks per hire. Estimating a regression of this measure on an indicator for whether a state has a implemented Ban the Box measures that contains year and state fixed effects shows that Ban the Box measures are associated with 0.16 fewer criminal background checks per hire, on a basis of only 0.26 background checks. This decrease is significant at the 95% confidence level.

Clifford and Shoag's (2016) research into the effect of eliminating credit checks found that employers shifted toward the adoption of other signals to screen potential employees. We identify a similar demand side response: data on online job advertisements from Burning Glass Technologies show an increase in education and experience requirements for new hires.

The combination of these first two findings is what led to the third one: that Ban the Box measures harm women and help African-Americans. This third finding is surprising in light of Holzer's (2006) findings regarding criminal background checks as benefiting African-Americans. This is especially so in light of the second finding, as it is through signal substitution that Holzer's effect materializes. But it ought not come as a surprise to Ban the Box advocates, as this third finding confirms one of the objectives of the push for these measures.

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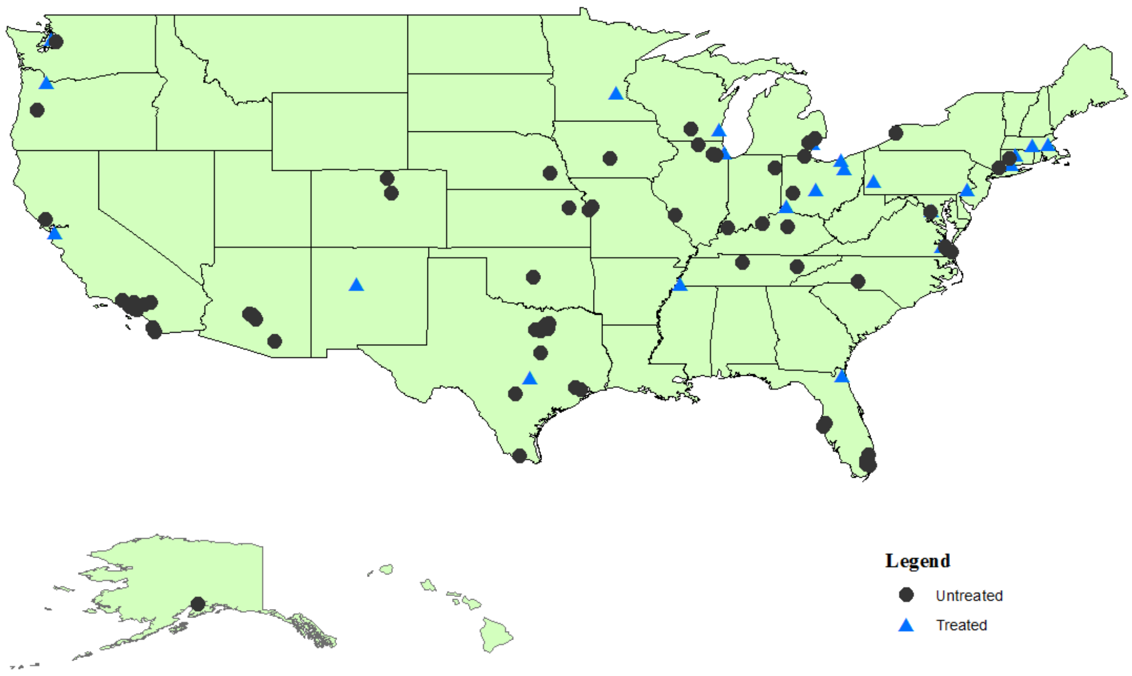
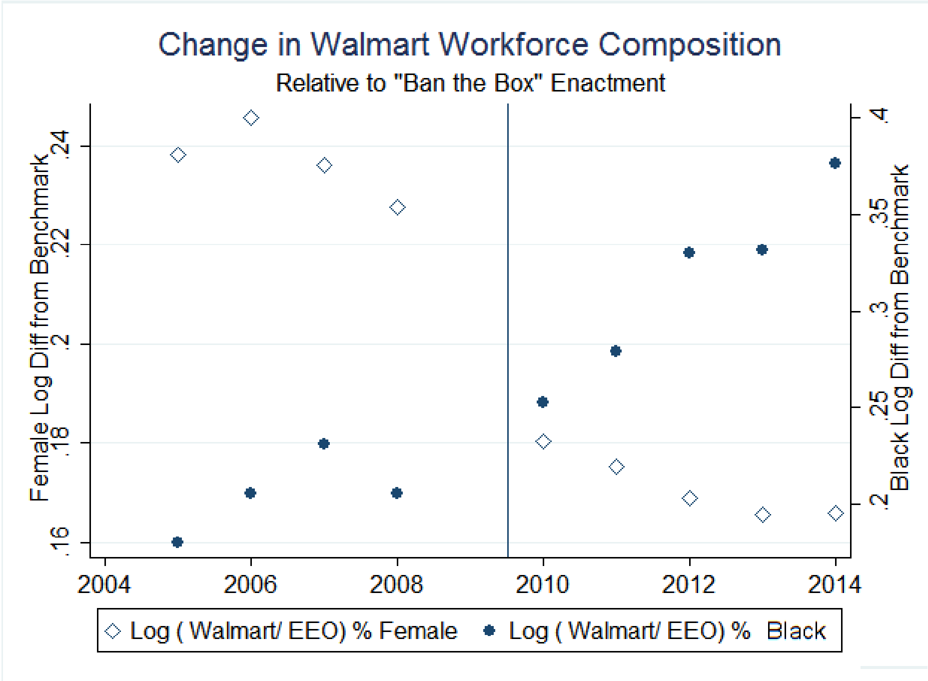
Figure 1: City Criminal Background Check Bans



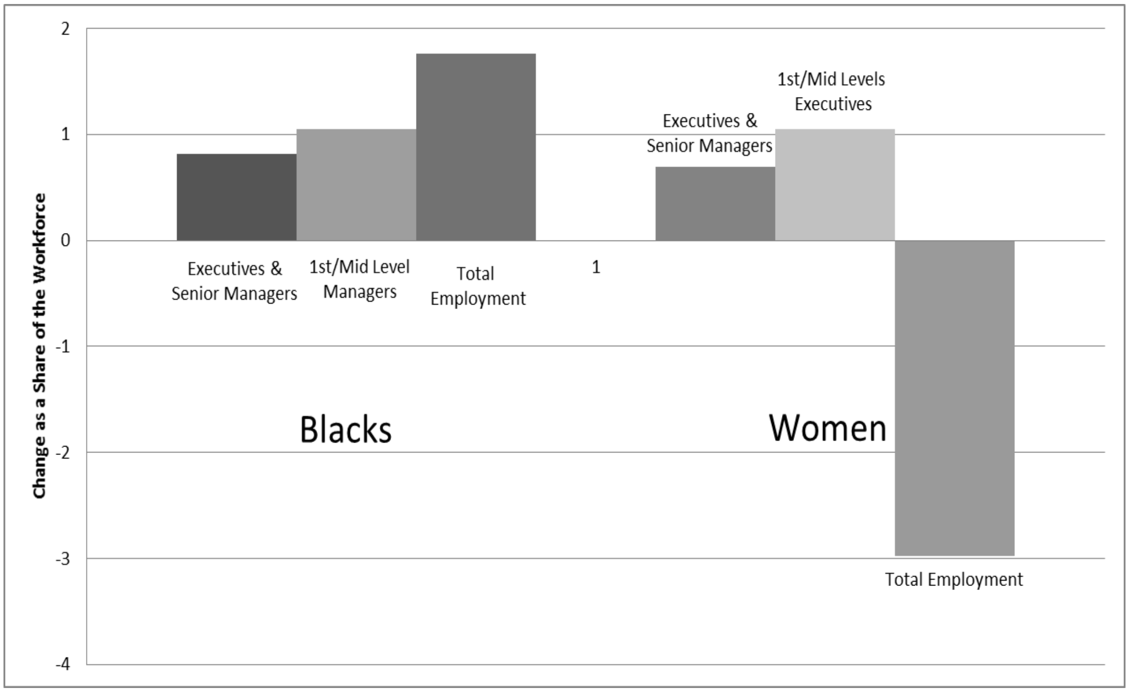
This map shows cities in our sample that had policies (treated) and that did not have policies (untreated) restricting the use of questions regarding criminal records in employment application procedures. Source: Natividad Rodriguez and Avery (2016).

Figure 2: Walmart “Ban the Box” Case Study



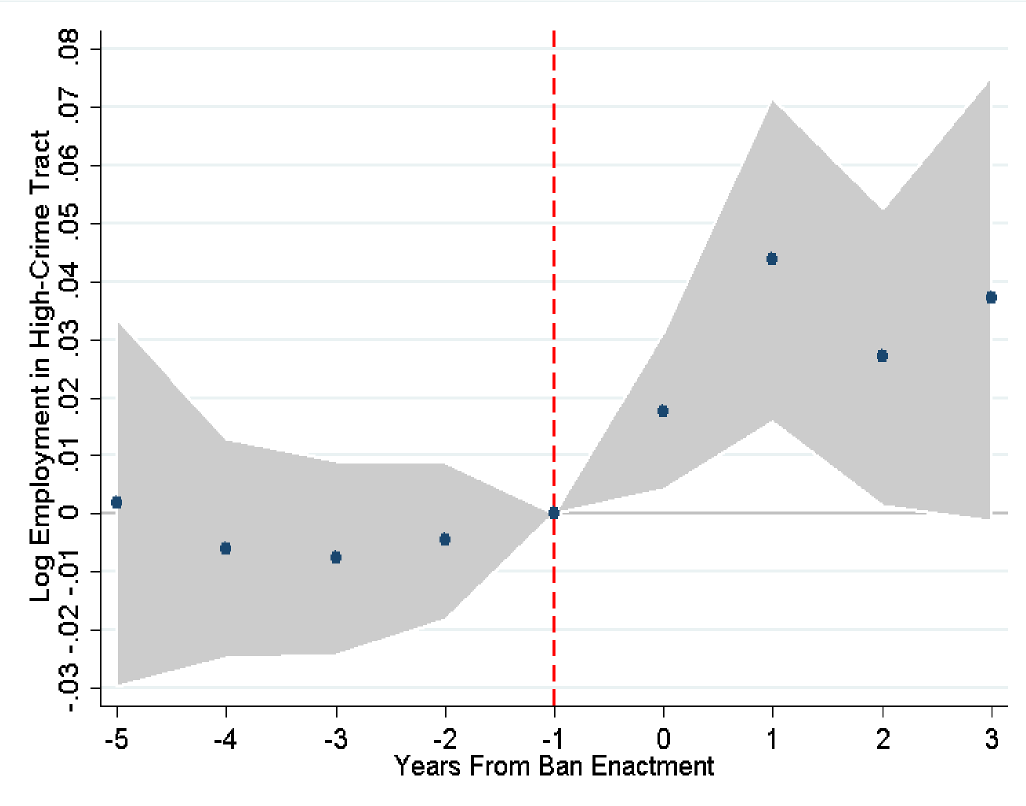
Note: This figure shows the log difference between Walmart’s total employee demographics and its EEO-1 benchmark before and after the company “banned the box.” Data on both company and benchmark demographics are taken from Walmart Diversity and Development Reports for the years indicated.

Figure 3: Walmart Change by Occupation from (2008-2012)



Note: This figure shows the change in the share of Wal-Mart executives & senior managers, first- and mid-level managers, and total employees who were black and female from 2008 to 2012. Wal-Mart "banned the box" in 2010. These data are collected from Wal-Mart Diversity and Development Reports. Overall, the share of Wal-Mart employees who were women fell by nearly 3 percentage points. This decrease was concentrated at the bottom end of the wage spectrum, as the share female among executives and managers actually increased during this period. Conversely, the share of Wal-Mart employees who are black increased by roughly 1.75 percentage points. This increase was larger for non- managerial positions.

Figure 4: Event Study Graph of Credit Check Ban Implementation



Note: This figure reports the results of the regression:

$$\ln \text{emp}_{it} = \alpha_i + \alpha_{\text{city} \times t} + \alpha_{\text{high crime} \times t} + \beta_t \times \text{high crime}_i \times \text{years to ban}_{\text{city},t} + \varepsilon_{it}$$

where α_i are tract-level fixed effects, $\alpha_{\text{city*t}}$ are city-year pair fixed effects, and to create our variable of interest we interact a dummy for high-crime tract with a count variable for the number of years to or from enactment of the ban. The figure depicts estimates of the coefficients βt for t = -5 … 3, where 0 is the year of ban enactment, engulfed by their 95% confidence intervals. Standard errors are clustered at the zip code area level. See the text for more detail on variable construction and interpretation of estimates."

Table 1: Sample Characteristics

VARIABLES	Mean	Standard Deviation	5th Percentile	95th Percentile	Period	Observations
Tracts of Residence (annual)					*2002-2013*	
Total Employment (persons)	1610.6	839.8	434	3103		123,667
Employment Below $15K	439.5	217.8	129	828		123,540
Employment from $15K to $40K	633.6	337.5	168	1250		123,507
Employment Above $40K	540.3	430.1	79	1367		123,318
Origin and Destination Flows (annual)					*2002-2013*	
Total Employment (persons)	133.9	266.6	12	682		186,809
Employment with Out-of-City Destination	129.8	216.0	12	583		54,067
City - Occupations					*2007-2014*	
Share of Postings Requiring an Associate Degree	0.03	0.08	0	0.17		851,690
Years of Experience Required	1.3	1.2	0	3.7		851,690
Individuals					*2002-2014*	
Wage Income	43314.8	57176.5	3600	114000		12,741,421
Hourly Income	20.1	34.8	2.6	49.4		12,741,421

Note: Data are from the American Community Survey, the LEHD Origin-Destination Employer Statistics, and Burning Glass Technologies Labor Insight.

Table 2: Baseline Results

VARIABLES	(1) Log Employment	(2) Log Employment	(3) Log Employment	(4) Log Employment	(5) Log Employment	(6) Log Employment
High Crime Tract $_i$ × City Check Ban $_t$	0.035**	0.034***	0.037**	0.035*	0.029*	0.035*
	(0.015)	(0.012)	(0.016)	(0.021)	(0.016)	(0.021)
Controls						
High Crime x Year Fixed Effects	X	X	X	X	X	X
City x Year Fixed Effects	X	X	X	X	X	X
City High Crime Trends		X		X		X
High Crime Tract Percentile Definition	> 75th	> 75th	> 75th	> 75th	> 90th	> 95th
Standard Errors Cluster	Zip	Zip	City	City	Zip	Zip
Observations	123,667	123,667	123,925	123,925	123,667	123,667
R-squared	0.947	0.947	0.946	0.946	0.947	0.947

Note: This table reports estimates of regressions of the following form:

$$\ln \text{emp}_{i,t} = \alpha_i + \alpha_{\text{city x t}} + \alpha_{\text{high crime} \times t} + \beta_t \times \text{ban}_{\text{city},t} \times \text{high crime}_i + \varepsilon_{it}$$

where $emp_{i,t}$ is the number of residents of tract *i* employed in period *t*, α_i represents tract-level fixed effects, $\alpha_{city*year}$ controls for arbitrary trends at the city level with city-year pair fixed effects, and $\alpha_{high\ crime*year}$ controls for arbitrary, nationwide high-crime-tract trends. We interact dummies for whether a tract had a ban in a certain year and whether it was a high-crime tract to create our variable of interest. The estimates reported in columns 2, 4 and 6 comes from a regression that, in addition, controls for separate linear time trends in employment for low- and high-crime tracts by city. Observations are at the tract-year level. Standard errors are clustered at either the zip code area level or city level and are reported in parentheses. Data are from the LEHD Origin-Destination Employer Statistics, the National Neighborhood Crime Study, and the National Employment Law Project. See the main text for additional details on variables construction and estimate interpretation. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Origin - Destination Based Results

VARIABLES	(1) Log Employment	(2) Log Employment
High Crime Origin Tract $_i$ × City Crime Check Ban Destination $_t$	0.041***	0.178***
	(0.015)	(0.046)
Controls		
Origin-Destination Fixed Effects	X	X
Destination-Year Fixed Effects	X	X
Origin-Year Fixed Effects	X	X
Sample		Origin-Destination Pairs with Employment >10
	All States	Origin Places w/o Law
Observations	186,809	54,067
R-squared	0.970	0.968

Note: This table reports estimates of regressions of the following form:

$$\ln \text{emp}_{od,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta \times \text{ban}_{dt} \times \text{high crime}_{o} + \varepsilon_{od,t}$$

where α_{od} controls for baseline differences across tracts-destination pairs with tract-destination-level fixed effects, α_{d*t} controls for arbitrary trends at the destination level with destination-year fixed effects, and α_{o*t} controls for aggregate outcomes for the tract in the year. Column 2 restricts the data to origin tracts in places without a ban, identifying the effect off cross-border commuting. Observations are tract-destination years and standard errors are clustered by tract and are reported in parentheses. Data are from the LEHD Origin-Destination Employer Statistics, the National Neighborhood Crime Study, and the National Employment Law Project. See the main text for additional details on variables construction and estimate interpretation. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Employment by Income

VARIABLES	(1) Log Emp Wage<$15K	(2) Log Emp Wage>$15K & Wage<$40K	(3) Log Emp Wage>$40K
High Crime Tract $_i$ × City Crime Check Ban $_t$	0.00465	0.0368***	0.112***
	(0.00871)	(0.00935)	(0.0154)
Controls			
High Crime x Year Fixed Effects	X	X	X
State x Year	X	X	X
Observations	492,137	492,086	491,658
R-squared	0.962	0.965	0.967

Note: This table reports regressions of the same form as column 1 of Table 2, but with the sample split into three subsamples. Wage bins are from LODES. Observations are still at the tract-year level. Standard errors are clustered at the zip code area level and are reported in parentheses. Data are from the LEHD Origin-Destination Employer Statistics, the National Neighborhood Crime Study, and the National Employment Law Project. See the main text for additional details on variables construction and estimate interpretation. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Employment by Industry -- Large Response

VARIABLES	(1) Information	(2) Real Estate	(3) Education	(4) Health Care	(5) Government
High Crime Tract $_i$ × City Crime Check Ban $_t$	0.053***	0.041**	0.042**	0.037**	0.121***
	(0.020)	(0.019)	(0.018)	(0.016)	(0.027)
Controls					
High Crime x Year Fixed Effects	X	X	X	X	X
Place x Year Fixed Effects	X	X	X	X	X
Observations	122,303	122,198	122,656	122,731	122,374
R-squared	0.903	0.842	0.920	0.920	0.893

This table reports regressions of the same form as column 1 of Table 2, but with the sample split into industry subsamples. Industry assignments are from LODES. Observations are at the tract-year level. Standard errors are clustered at the zip code area level and are reported in parentheses. Data are from the LEHD Origin-Destination Employer Statistics, the National Neighborhood Crime Study, and the National Employment Law Project. See the main text for additional details on variables construction and estimate interpretation. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Employment by Industry -- No Response

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Agriculture, Forestry and Fishing	Natural Resource Extraction	Utilities	Construction	Manufacturing	Wholesale	Retail
High Crime Tract i × City Crime Check Ban t	0.010	0.006	0.024	0.023	0.012	0.003	0.017
	(0.045)	(0.056)	(0.025)	(0.018)	(0.016)	(0.019)	(0.016)
Observations	95,831	67,440	116,633	122,923	123,047	122,609	122,875
R-squared	0.712	0.886	0.715	0.922	0.937	0.900	0.917

	(8)	(9)	(10)	(11)	(12)	(13)	(14)
VARIABLES	Finance	Professional Services	Management	Waste Management	Entertainment	Accommodation & Food Services	Transportation & Warehousing
High Crime Tract i × City Crime Check Ban t	0.012	0.011	0.011	0.003	-0.000	0.032	0.012
	(0.017)	(0.020)	(0.023)	(0.020)	(0.015)	(0.019)	(0.017)
Observations	122,487	122,630	121,916	122,857	122,167	123,006	123,010
R-squared	0.911	0.915	0.845	0.908	0.821	0.917	0.893
Controls							
High Crime x Year Fixed Effects	X	X	X	X	X	X	X
City x Year Fixed Effects	X	X	X	X	X	X	X

This table reports regressions of the same form as column 1 of Table 2, but with the sample split into industry subsamples. Industry assignments are from LODES. Observations are at the tract-year level. Standard errors are clustered at the zip code area level and are reported in parentheses. Data are from the LEHD Origin-Destination Employer Statistics, the National Neighborhood Crime Study, and the National Employment Law Project. See the main text for additional details on variables construction and estimate interpretation.
*** p<0.01, ** p<0.05, * p<0.1

Tabel 7: Upskilling

	Average Experience		Share of Postings Needing No Experience		Share of Postings Needing a BA	
	(1)	(2)	(3)	(4)	(5)	(6)
City or State Ban	0.0492**	0.0703**	-0.0111**	- 0.0198***	0.00914***	0.0148***
	(0.0216)	(0.0342)	(0.00462)	(0.00713)	(0.00315)	(0.00484)
Year FE	X		X		X	
City FE	X	X	X	X	X	X
State-Year FE		X		X		X
City Trends						
Observations	21,675	21,670	21,675	21,670	21,675	21,670
R-squared	0.765	0.795	0.728	0.775	0.802	0.816

Note: This table reports estimates of regression of the following type:
$\textit{skill level}_{city,t} = \alpha_{city} + \alpha_t + \beta_t \times \textit{ban}_{it} + \varepsilon_{it}$, where $\textit{skill level}_{city,t}$ is the skill-related dependent variable of interest, α_{city} represents city-level fixed effects, and α_t controls for year fixed effects. The dependent variables we study are average experience required (in years), the share of postings requiring no experience, and the share of postings requiring a college degree. In addition to this baseline specification, we test the robustness of our findings by including state-by-year fixed effects to allow for arbitrary trends instead of year dummies. Standard errors in parentheses clustered by city. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact on African-Americans

VARIABLES	(1) Employed	(2) Employed	(3) Employed	(4) Log Income	(5) Log Income	(6) Log Income	(7) Log Hourly Wage	(8) Log Hourly Wage	(9) Log Hourly Wage
Black x State Ban	0.044***	0.031*	0.033*	-0.035	-0.078	-0.089	0.008	-0.042	-0.049
	(0.011)	(0.018)	(0.017)	(0.071)	(0.108)	(0.095)	(0.040)	(0.070)	(0.056)
Controls									
State x Year	X	X	X	X	X	X	X	X	X
Female x State	X	X	X	X	X	X	X	X	X
Female x Year	X	X	X	X	X	X	X	X	X
Individual Demographics			X			X			X
Black x State Linear Trends		X	X		X	X		X	X
Observations	8,790,749	8,790,749	8,790,749	6,583,802	6,583,802	6,583,802	6,583,802	6,583,802	6,583,802
R-squared	0.018	0.018	0.057	0.016	0.016	0.241	0.019	0.019	0.230

Note: This table reports regressions of the form:

$$y_{it} = \alpha_{\text{black}-\text{state}} + \alpha_{\text{state,t}} + \alpha_{\text{black,t}} + \gamma \times X_{it} + \beta_{\text{t}} \times \text{ban}_{\text{state,t}} \times \text{black}_{\text{it}} + \varepsilon_{\text{it}}$$

where α represents controls for arbitrary trends for African-Americans and for states, and for arbitrary racial differences across states. The data are from the American Community Survey from 2005 to 2014 for non-institutionalized individuals between 19 and 65 years old. Sample limited to adult males with less than a high school degree, and to individuals in the labor force for column 4 through 9. Specification 3, 6, and 9 control for education dummies and age. Standard errors are clustered by state. See text for additional details. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Impact on Women

VARIABLES	(1) Employed	(2) Employed	(3) Employed	(4) Log Income	(5) Log Income	(6) Log Income	(7) Log Hourly Wage	(8) Log Hourly Wage	(9) Log Hourly Wage
Female x State Ban	-0.002	-0.004**	-0.003*	-0.008**	-0.013**	-0.007*	-0.009***	-0.012**	-0.007*
	(0.002)	(0.002)	(0.002)	(0.004)	(0.007)	(0.004)	(0.003)	(0.005)	(0.003)
Controls									
State x Year	X	X	X	X	X	X	X	X	X
Female x State	X	X	X	X	X	X	X	X	X
Female x Year	X	X	X	X	X	X	X	X	X
Individual Demographics			X			X			X
Female x State Linear Trends		X	X		X	X		X	X
Observations	18,205,477	18,205,477	18,205,477	12,741,421	12,741,421	12,741,421	12,741,421	12,741,421	12,741,421
R-squared	0.021	0.021	0.066	0.040	0.040	0.250	0.031	0.031	0.234

Note: This table reports regressions of the form:

$$y_{it} = \alpha_{\text{female-state}} + \alpha_{\text{state,t}} + \alpha_{\text{female,t}} + \gamma \times X_{it} + \beta_{\text{t}} \times \text{ban}_{\text{female,t}} \times \text{female}_{\text{it}} + \varepsilon_{\text{it}}$$

where α represents controls for arbitrary trends for women and for states, and for arbitrary gender differences across states. The data are from the American Community Survey from 2005 to 2014 for non-institutionalized individuals between 19 and 65 years old, and to individuals in the labor force for column 4 through 9. Specification 3, 6, and 9 include age, race and education dummies. Standard errors are clustered by state. See text for additional details. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1A: Ban the Box Legislation

States with Bans	Date	Lodes	Burning-Glass	ACS
California	2013		X	X
Hawaii	1998		X	X
Massachusetts	2010	X	X	X
Minnesota	2009	X	X	X
New Mexico	2010	X	X	X
Rhode Island	2013		X	X

Counties with Bans	Date	Lodes	Burning-Glass	ACS
San Francisco County, CA	2005		X	
Alameda County, CA	2007	X	X	
Santa Clara County, CA	2012		X	
Muskegon County, MI	2012		X	
Durham County, NC	2012			
Cumberland County, NC	2011		X	
Cuyahoga County, OH	2012	X	X	
Summit County, OH	2012	X	X	
Hamilton County, OH	2012	X	X	
Lucas County, OH	2013		X	
Franklin County, OH	2012	X	X	
Stark County, OH	2013		X	
Multnomah County, OR	2007	X	X	
Hamilton County, TN	2012		X	
Travis County, TX	2008	X	X	
Milwaukee County, WI	2011	X	X	

Note: This table shows states and counties in our samples that had adopted measures restricting the use of questions regarding criminal records in employment application procedures by 2013. Source: Natividad Rodriguez and Avery (2016).

Appendix Table 1B: Ban the Box Legislation

Cities with Bans	Date	Lodes	Burning-Glass
Pasadena, CA	2013		X
San Francisco, CA	2005		X
Richmond, CA	2013		X
Carson, CA	2012		X
Oakland, CA	2007	X	X
Compton, CA	2011		X
Berkeley, CA	2008		X
East Palo Alto, CA	2007		
Hartford, CT	2009	X	X
Bridgeport, CT	2009		X
New Haven, CT	2009	X	X
Norwich, CT	2008		X
Washington, DC	2011	X	X
Wilmington, DE	2012		X
Clearwater, FL	2013		X
Tampa, FL	2013		X
Jacksonville, FL	2009	X	X
Atlanta, GA	2012		X
Chicago, IL	2006	X	X
Boston, MA	2004	X	X
Worcester, MA	2009	X	X
Cambridge, MA	2008		X
Baltimore, MD	2007		X
Detroit, MI	2010	X	X
Kalamazoo, MI	2010		X
St. Paul, MN	2006		X
Minneapolis, MN	2006	X	X
Kansas City, MO	2013		X
Spring Lake, NC	2012		X
Carrboro, NC	2012		X
Durham, NC	2011		X
Atlantic City, NJ	2011		X
Newark, NJ	2012		X
Buffalo, NY	2013		X
New York, NY	2011		X
Cleveland, OH	2011	X	X
Akron, OH	2013		X
Cincinnati, OH	2010	X	X
Canton, OH	2000		X
Philadelphia, PA	2011	X	X
Pittsburgh, PA	2012	X	X

Providence, RI	2009		X
Memphis, TN	2010	X	X
Austin, TX	2008	X	X
Norfolk, VA	2013		X
Richmond, VA	2013		X
Portsmouth, VA	2013		X
Virginia Beach, VA	2013		X
Newport News, VA	2012	X	X
Petersburg, VA	2013		X
Seattle, WA	2009	X	X

Note: This table shows cities that had adopted measures restricting the use of questions regarding criminal records in employment application procedures by 2013. Source: Natividad Rodriguez and Avery (2016).

Appendix Table 2: LODES Industry Classification

LODES Industry	NAICS
Agriculture, Forestry and Fishing	11
Natural Resource Extraction	21
Utilities	22
Construction	23
Manufacturing	31-33
Wholesale	42
Retail	44-45
Transportation & Warehousing	48-49
Information	51
Finance	52
Real Estate	53
Professional Services	54
Management	55
Waste Management	56
Education	61
Health Care	62
Entertainment	71
Accommodation & Food Services	72
Government	92

Note: This table provides a crosswalk between the LODES industry categorization and NAICS codes.