Banning the Box

The Labor Market Consequences of Bans on Criminal Record Screening in Employment Applications[i]

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Many localities have in recent years limited the use of questions about criminal history in hiring, or "banned the box." We show that these bans increased employment of residents in high-crime neighborhoods by up to 4%. This effect can be seen both across and within census tracts, in employment levels as well as in commuting patterns. The increases are particularly large in the public sector and in lower-wage jobs. We also establish that employers respond to Ban the Box measures by raising experience requirements. On net, black men benefit from the changes.

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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 have gained steam over the past couple of 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 on application documents 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, and we provide the first nationwide estimates of their impact.

We exploit variation in whether and when cities, counties and states implemented them to identify their significance using LEHD Origin-Destination Employment Statistics (LODES) on employment outcomes. We do this, mostly, with difference-in-difference, triple-difference, and quadruple-difference estimators that compare different groups and small neighborhoods within cities as these cities adopt bans at different points in time. For example, one specification compares residents of a census tract who work in a tract that became subject to Ban the Box rules to residents of the same tract who work in a tract that did not become subject to such rules, before and after implementation.

Our central finding 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 the public sector, where compliance is likely to be highest and which is often the central target of these bans. The greatest increases occur in the lowest-wage jobs.

These gains do not represent aggregate employment gains, but rather substitution across workers. We analyze the drivers of this shift using a large, novel data set of online job postings. We find that "upskilling," or increases in education and experience requirements, occurs after the implementation of Ban the Box measures, as employers substitute away from criminal background questions to other signals of employee quality. We then study the impact of this substitution across racial and gender lines using and American Community Survey (ACS) Integrated Public Use Microdata Series data. These data indicate that black men benefit, while women, especially black women, who are less likely to have been convicted of crimes, see their labor market outcomes deteriorate.

Altogether, these results highlight both the importance of Ban the Box initiatives and some of their perhaps unintended consequences. The evidence we find runs counter to the standard reporting of Holzer et al.'s (2006) classic finding that employers who check criminal records are more likely to hire African Americans. That cross-sectional comparison of the race of new hires by employers who use and do not use criminal background checks may reflect a significant amount of statistical discrimination, but it may reflect other factors as well. In any case, it is not a study of the changes caused by Ban the Box legislation.

Our results also contrast with some accounts in the popular press of the impact of Ban the Box rules. These accounts (e.g. The Economist (2016), and Vedantam (2016)), which typically cite two recent studies (Agan and Starr (2016) and Doleac and Hansen (2016)), suggest that statistical discrimination resulting from the introduction of Ban the Box rules is so severe that African-American men are made worse off in net terms. A superficial reading of the two studies mentioned earlier may lead one to share this conclusion, but we demonstrate that it is not supported by either our estimates or the estimates as reported in the studies themselves. In fact, this inference is likely to be incorrect except for relatively small subgroups. We will show that, instead, the results reported by Doleac and Hansen imply that black men on aggregate see their employment opportunities *increase* after Ban the Box rules are introduced. In addition, the results reported by Agan and Starr suggest that while call-back rates for young black applicants without a criminal record may be reduced, this is probably not the case for the full population of young black applicants. While the media discussion has shed admirable light on the potential for negative consequences from Ban the Box legislation, the ongoing policy debate would benefit from a more comprehensive assessment of its impact.

We proceed as follows. In the next section, we present background information on the role played by employee screening procedures and criminal records in hiring processes, as well as the roll-out of the policies we study. We also illustrate the effects such policies can have by highlighting the experience of Wal-Mart, the largest private-sector employer in the United States, which "banned the box" in 2010. In section II, we turn to our theoretical framework and the data we will draw upon. We then turn to our empirical results. We first discuss the impact of Ban the Box measures on employment in high-crime neighborhoods (section III), as well as the industries and income categories in which these employment effects materialize (section IV). Section V discusses employer responses, while section VI explores the consequences of Ban the Box measures for different demographic groups. Section VII concludes by discussing the implications of our findings for public policy and the mechanisms through which they materialize.

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. Roughly 17% of the job listings in the large database of postings collected by Burning Glass Technologies discussed below announce such checks in the advertisement itself, representing a lower bound: estimates of the share of organizations carrying them out 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; Council of Economic Advisers, 2016). 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).

Additionally, 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. Walmart voluntarily eliminated questions about applicants' criminal records in the early stages of application processes in 2010, and its experience since then highlights the central trade-off we observe throughout this paper and gives a first taste of our key findings. 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. To move from this illustrative example to a more comprehensive assessment of the impact of Ban the Box rules, we need both theory and data.

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). While there are many ways in which this situation can be modeled, we focus on two approaches: one emphasizes the potential for statistical discrimination when the criminal record signal is removed, as in Akerlof (1970) and Holzer (2006), while the other one emphasizes increased opportunity for applicants with criminal records in a world of systemic inequities within the criminal-justice system. These two models of the screening problem have

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

different empirical implications, and we will turn to the data to determine which one is more applicable here.

II.1.a Statistical Discrimination

Assume that there are two easily identifiable groups $x = 1, 2$, whites and African-Americans, 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 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 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 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 thus harm upstanding members of the disadvantaged group even if, on average, their signals are worse. This is statistical discrimination.

The net effect of eliminating the signal is, of course, still ambiguous if group 2 applicants with criminal records become more likely to be hired. A good example of this ambiguity is provided by Agan and Starr (2016). While they find convincing experimental evidence of statistical discrimination against young applicants with black-sounding names in an experiment that relates the introduction of Ban the Box rules to call-back rates for job interviews, the disparity in the prevalence of criminal records between blacks and whites is quite possibly large enough to compensate for this effect almost precisely. For example, let us use the cumulative arrest rates at age 23 for black and white males reported by Brame et al. (2014) as proxies for conviction rates. If we assume that the total number of applicants receiving call-backs remains constant after the introduction of Ban the Box policies, and we weigh Agan and Starr's call-back rates for black men with and without criminal records according to these conviction rate proxies, we find that the likelihood of a black applicant receiving a call back goes from 9.9% before to 9.8% after.

In addition to these two direct effects of Ban the Box policies, 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).

II.1.b. Increased Opportunity

Assume again that there are two easily identifiable groups $x = 1, 2$, whites and African-Americans, from which workers are drawn. Employers want to hire a worker of quality $w > k$, where k is a given threshold that is set so as to hire a given number of workers. 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 $\sigma > 0$. There are two signals available to potential employers, and employers use these to weed out applicants with $E(w) < k$ after receipt of each signal. The first signal, a worker's response to a question about his criminal history, provides an unbiased estimate of the quality of workers of type 1, $y_{1i} = w_i + \varepsilon_i$, where ε_i is normally distributed mean-zero noise with standard deviation $\gamma > 0$, but, unbeknownst to the employer it provides a biased estimate of the quality of workers of type 2, $y_{2i} = w_i - b + \varepsilon_i$, for example as a consequence of inequities in the criminal-justice system. The second signal, z, of which we can think as an in-person interview, provides an unbiased signal $z_i = w + u_i$ for both groups, where u_i is normally distributed mean-zero noise with standard deviation $\delta > 0$. We assume that the relative costs of acquiring the two signals and the benefits of accurate screenings combine to make it so that employers operate as follows in the absence of regulation. They acquire the costless signal (1), update their expectation of worker quality (2), cease consideration of workers of expected quality below k (3), acquire the costly signal (4), update their expectation of worker quality (5), and extend a job offer to all remaining candidates of expected quality above k, who all accept the job offer (6). If Ban the Box regulations are in place, step (1) becomes step (4) and vice versa.

In this setting, it follows that Ban the Box legislation has the potential to help workers of type 2, while harming workers of type 1. Throughout the recruiting process, an employer's expectation of a worker's quality is a weighted sum of her prior and her accumulated signals. Without Ban the Box measures in place, this means that at step (3), $E[quality|y\ x_i] = \frac{\gamma^2}{\sigma^2+\gamma^2}\mu_i + \frac{\sigma^2}{\sigma^2+\gamma^2}y_{xi}$, and if it exceeds k, the applicant makes it to the interview stage. After the interview stage, $E[quality|y\ z\ x_i] = \frac{\gamma^2+\delta^2}{\sigma^2+\gamma^2+\delta^2}\mu_i + \frac{\sigma^2+\delta^2}{\sigma^2+\gamma^2+\delta^2}y_{xi} + \frac{\sigma^2+\gamma^2}{\sigma^2+\gamma^2+\delta^2}z_i$. With Ban the Box measures in place, at step (3), $E[quality|z\ x_i] = \frac{\delta^2}{\sigma^2+\delta^2}\mu_i + \frac{\sigma^2}{\sigma^2+\delta^2}z_i$, while expected worker quality at step 5 is not affected by the change. This leads us the following proposition:

PROPOSITION: If $\frac{\sigma^2}{\sigma^2+\delta^2} \geq \frac{\sigma^2}{\sigma^2+\gamma^2}$, Ban the Box measures make it so that weakly more applicants of type 2 will receive job offers.

PROOF: It follows immediately from $E[y_{2i} - z_i] < 0$ that if $\frac{\sigma^2}{\sigma^2 + \delta^2} \geq \frac{\sigma^2}{\sigma^2 + \gamma^2}$, $\frac{\gamma^2}{\sigma^2 + \gamma^2}\mu_i + \frac{\sigma^2}{\sigma^2 + \gamma^2} y_{xi} < \frac{\delta^2}{\sigma^2 + \delta^2}\mu_i + \frac{\sigma^2}{\sigma^2 + \delta^2} z_i$. This means that $E[quality|y\ x_i]$ < $E[quality|z\ x_i]$, that is, for a type-2 worker of given quality w_i the likelihood of exceeding the k threshold at step (3) is higher with Ban the Box in place for all values of k, which is not the case for type-1 workers. The value of k will be higher in this situation, which will eliminate at least zero type-1 workers. As a consequence, more applicants of type 2 will receive job offers. That is, with Ban the Box measures in place, more type-2 applicants will be hired.

Alternatively, if $\frac{\sigma^2}{\sigma^2 + \delta^2} \leq \frac{\sigma^2}{\sigma^2 + \gamma^2}$, the employment differential between the groups can be exacerbated: the interview signal is precise is enough that it weeds out more type-2 applicants, who are lower-skilled on average, than the biased but imprecise criminal history signal would. As before, if employers respond to Ban the Box measures by shifting to alternative, more precise signals, as in Clifford and Shoag (2016), this will also harm type-2 applicants.

In addition, if the criminal history signal is not an unbiased signal of worker quality, that is, if it suggests that workers with a criminal record are low-quality workers even though they are not, then Ban the Box measures will lead to a shift toward applicants with low first signals *within* both groups of applicants. Moving the biased signal back, so as to let a stronger prior develop, can then harm applicants without a criminal record when compared to applicants of the same type with a criminal record. In response, they will attempt to find work in industries without Ban the Box measures (if any), generating general-equilibrium effects that can produce changes in outcomes even in industries not subject to a ban. The relative importance of these general-equilibrium effects for different groups will be greater for groups with a higher prevalence of criminal records.

Which one of these two forces, increased statistical discrimination or bias reduction, can better explain the results of Ban the Box policies is ultimately an empirical question. We now turn to the data we will use to test which of these potential consequences of Ban the Box measures have materialized in the past few years.

II.2 Data

National Employment Law Project

The National Employment Law Project, 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. Such spillovers can arise from a variety of origins. For example, sector-specific Ban the Box measures may create a new social norm that guides employers throughout the economy. In addition, Ban the Box measures may produce spillover effects in general equilibrium, as workers without criminal records may be displaced from directly affected sectors but find employment in other industries. The latter effect resembles the general-equilibrium spillovers from trade shocks in Monte (2016). 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, and is based on crime data from 1999, 2000, and 2001. This early provenance of the data ensures that crime levels are not driven by the effects of Ban the Box measures. 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 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 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 2013.[2] Basic summary statistics for these data are provided in Table 1.

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

Georgia State Board of Pardons and Paroles Data

The Georgia State Board of Pardons and Paroles provides data on the home addresses of parolees in the state. We use these data for the city of Atlanta as of April 12, 2016.

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.

III. Employment Outcomes for Residents of High-Crime Areas

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.

There is, unfortunately, no national data on employment outcomes for individuals with prior criminal records. In fact, the available data do not even allow for accurate tallies of the number of people with such records – estimates vary by (tens of) millions. We therefore use employment of the residents of high-crime census tracts as a proxy measure for employment of tracts where many individuals have criminal records. This measure relies on the assumption that individuals with criminal records are more likely to live in high-crime neighborhoods. We test this assumption using data on the location of individuals with known criminal records in the (unique as far as we are aware) addresses-level location data on parolees published by the Georgia State Board of Pardons and Paroles. We geocoded these address, and combined them with geocoded violent crime data provided by the Atlanta Police Department at the tract level.

Figure 4 shows the relationship between crime and parolee residence across tracts, after controlling for log tract population. To ease viewing, tracts are divided into equal-population bins based on residualized violent crime per capita, our proxy for the preponderance of residents with criminal records. The figure shows that high-crime neighborhoods (particularly those in the top 25% of the crime distribution) are home to significantly more parolees per capita, and, by implication, significantly more people with a criminal record. This relationship is evident in the figure and is highly statistically significant. Going forward we will use this proxy, then, to identify tracts where people are more likely to have criminal records and to be affected by Ban the Box legislation.

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:

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

where $emp_{i,t}$ is the number of residents of tract i employed in period t, α_i represents tract-level fixed effects, α_{city*t} controls for arbitrary trends at the city level with city-year pair fixed effects, and $\alpha_{high\ crime*t}$ 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. We cluster standard errors at the city level (the typical treatment level), but our results are robust to clustering at the state or zip code area level and wild bootstrapping.[3]

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 arbitrary high-crime tract and citywide trends.[4][5] 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 laws 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. Moreover, while displacement effects are a concern, given the small fraction of

[3] Though we have nearly 90 clusters, we also test whether our estimates are statistically significant under tests that account for small numbers of clusters. In particular, we conduct a wild bootstrap estimate of our baseline specification following Cameron, Gelbach, and Miller (2008). We find that our baseline t-statistic is in the top 5.4% of bootstrap estimates. This suggests that our significance tests are not overly inflated by a small number of clusters.

[4] Appendix Table 3 shows that this result is not driven by concurrent population increases.

[5] Similar tests show that aggregate employment is not significantly affected by the introduction of Ban the Box regulations.

employment accounted for by residents of high crime tracts, our estimates are unlikely to be driven by them.[6]

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 through 6 allow for high-crime tract employment trends that vary by census division, while columns 5 and 6 show that our results barely change if we define only the 10% or 5% most violent tracts as high-crime instead of the top 25%.[7]

Figure 5 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.

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[6] As made explicit in section II.1, we believe that these employment gains represent substitution by employers across workers rather than absolute job gains. As such, our empirical estimates here pick up both employment increases in high-crime neighborhoods and employment decreases in other neighborhoods within the same city. As a result, our point estimates are not the absolute gain in high crime neighborhoods. Nevertheless, since high-crime neighborhoods represent a smaller fraction of neighborhoods, and even more so of overall employment, our point estimates are likely to be close to the absolute gain. For example, when we restrict our sample to cities in which high-crime neighborhoods contain less than 20% of total employment, we actually estimate a slightly larger effect (a 5.8% increase in employment), and not a smaller one. This suggests to us that most of the movement comes from the treated tracts as opposed to displacement from baseline declines.

[7] A regression analogous to the regression in column 2 but for the subsample of high-crime neighborhoods only produces an estimate of 4.1%, significant at the 10% confidence level. This specification eliminates within-city cross-tract substitution, yet yields similar results.

One last concern one may have is that Ban the Box measures would be systematically correlated with other, similar legislation. As far as we have been able to determine, this is not the case. Not only are Ban the Box measures typically standalone initiatives, they are also not correlated with perhaps the most similar type of legislation in terms of motivation and target population, bans on credit checks in application procedures. Using data on such bans from Clifford and Shoag (2016), we find no correlation between the adoption of credit check bans and Ban the Box measures between 2007 and 2013. The correlation is insignificant for each year, and fluctuates in sign (positive for 2010, 2011, and 2012; negative for the remaining years). In addition, we find no relationship between changes in state minimum wage laws and Ban the Box measures during the period we study. This strengthens our conviction that the effects we find are not spurious or driven by unrelated concurrent public policies.

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 work. 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 destination tracts that are not subject to Ban the Box measures. In effect, what that means is that we estimate the following regression equation:

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

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 solely off cross-city commuting. This increases the effect we find (as a percentage) by a factor of four – which is unsurprising, given that commuting flows within city are greater than between cities – and confirms the robustness of our results despite reduced power, as the effect is both statistically and economically highly significant despite the commuting friction introduced.[8]

III.3 Threats to Identification

When using a differences-in-differences-style identification strategy, one needs to be concerned about pre-existing or contemporaneous trends that might bias the estimates.

For example, one might be concerned that Ban the Box policies were enacted in cities or regions with growing employment or in regions or cities where employment was growing disproportionately in high-crime neighborhoods. We address this concern in numerous ways. First,

[8] Appendix Figure 1 shows an event study graph similar to that in Figure 5, and again shows no significant pre-trend.

we explicitly check for pre-trends in our baseline specification in Figure 5 and find none. Second, we include city-year fixed effects in Table 2, controlling for arbitrary differences in trends across cities. This allows us to identify off differences across tracts within a city. Third, we run tests that include city-specific linear trends for high-crime neighborhoods and high-crime neighborhood by census division by year fixed effects. These controls enable us to identify the impact of the ban off changes for high-crime tracts relative to their own trends within the city and relative to trends for geographically close high-crime neighborhoods in other cities. We find similar impacts of these bans when progressively adding all of these controls, which suggests that these types of biases did not have a large effect on our initial estimate.

What threats remain after these tests? Our test would remain biased if Ban the Box laws were enacted in cities experiencing a break in the relative employment of their high-crime neighborhoods relative to prior trends for those tracts. For example, suppose Boston enacted a Ban the Box law right as its high-crime neighborhoods grew over and above prior trends for those neighborhoods and trends for high-crime neighborhoods elsewhere in New England. If this correlation were not confined to Boston, but was systematic across cities, it would bias our estimates. Table 3 introduces a test that is robust to this possibility. Rather than identify the impact off differences in total employment outcomes for a tract, it identifies off differences in commuting patterns. We now explore whether residents of high-crime tracts are more likely than residents of other tracts to commute to work in BTB destinations, holding constant their overall employment outcomes. Once again, we find an impact of BTB policy on these outcomes. To relate this to the previous example, we now find that residents of high-crime tracts in New Hampshire have become more likely to commute to Boston, even controlling for the total number of employed people in those tracts. Thus any omitted-variable bias story needs to account for both the increase in employment in high-crime tracts in Boston and the change in commuting patterns.

Now, it is impossible to rule out the potential for a complicated alternative counterfactual. Still, it is clear that straightforward bias stories about different cyclical trends or growth rates (see Appendix Table 3 for an explicit check of the latter[9]) cannot explain these results. We believe that articulating an explanation that accounts for all of our findings in which Ban the Box policies do not have the effect claim they have is sufficiently difficult that, per Occam's razor, the best explanation is that we are indeed measuring the impact of these policies.

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, which involves larger numbers of workers, is more informative 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,

[9] Unfortunately, we do not have reliable annual population estimates by census tract. We therefore run a regression using changes between decennial population estimates in an attempt to mimic the baseline as closely as possible.

even after controlling for tract-level fixed effects and arbitrary citywide trends for the different wage bins. The estimates are as one would probably expect: they are greatest for our lowest-income bin (at a little over 4%), and statistically insignificantly different from zero for annual wages over $15,000. That said, the point estimates for different income bins do not differ significantly from one another. 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.[10] 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 while Table 6 shows estimates for all other industries. These latter estimates are all smaller than 4% 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%), and real estate (4.1%). 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 similar effects we found for different wage bins. The most obvious explanation for this 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. In addition, most of the private-sector firms who voluntarily ceased the practice of asking about applicants' criminal history, such as Walmart, are active in the retail

[10] 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.

industry. We show our estimates for the remaining industries in Table 6, where we find particularly small point estimates in the management, waste management, and wholesale sectors.

Overall, we find that the impact of BTB policies is concentrated in the industries and wage bins one would expect, which is reassuring.

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 what the total effect of bans and demand side responses is for two sizable groups of particular interest, women and African-Americans. These two groups are quite different along the dimensions that are relevant here: whereas 29% of non-institutionalized women age 19 to 65 hold a bachelor's degree or more, only 15% of African-American men do, according to the 2005-2014 American Community Survey. And while black men have a 28.5% chance of being incarcerated during their lifetime, the corresponding number is only 1.1% for women (Bonczar and Beck, 1997). To put it differently, the ratio of African-American men with college degrees to African-American men with criminal histories is far lower than that same ratio for both white and African-American women.

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 to 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:

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

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). Estimates are shown in Table 7.

We see in column 1 that firms respond to bans by raising the 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 percentage points more of job postings after the introduction of a ban demand at least some prior experience, from a base of 38% (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 to 1.5 percentage point on a base of 14%, 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.[11]

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 African-American men, who are more likely to have been convicted of crimes than the population as a whole. We analyze here whether this objective is being met, and whether women, who are much less likely to have criminal records, suffer as a consequence. To study this we use ACS data that link employment outcomes to race and gender. For most individuals we cannot tell where they live beyond the state level, so for this section we focus on variation created by the decisions of states to pass Ban the Box legislation.[12] This identification 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 estimate regression equations of the following type for the non-institutionalized population between age 19 and 65:

$$\ln employed_{it} = \alpha_{\mathrm{group,s}} + \alpha_{\mathrm{group,t}} + \gamma \text{ x } X_{it} + \beta_{group} \times \mathrm{ban}_{\mathrm{state,t}} \times \mathrm{group}_{\mathrm{it}} + \varepsilon_{\mathrm{it}}$$

where the αs represent controls for arbitrary trends for demographic groups and for states, and for arbitrary racial differences across states and where we control for individual-level age and education characteristics. As Table 8 shows in row 1, we find significantly increased employment for African-American men: the number of employed individuals in this category goes up by around 3%. This result holds for African-Americans overall and when we include

[11] Note that the introduction of a Ban the Box measure does not significantly affect the total number of job postings in a given location.
[12] See Appendix Table 1A for the list of states that have done so and the years in which they did.

only those in the labor force (column 2, 4, 6, and 8). It also holds when we allow for state-specific trend divergence for African-Americans (in column 3, 4, 7, and 8), and when we control for arbitrary county-year trends (5, 6, 7, 8) – and for combinations of those different features, by implication.

The observed increase in employment among African-American males is one of the 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 requirements. This finding is confirmed qualitatively in recent work by Doleac and Hansen (2016). While their central focus is on the heterogeneity of employment effects across age and skill groups, in particular on the disemployment effect they find for young black men, they find that Ban the Box increases the employment of black men overall by between 1 and 2%, depending somewhat on how one weighs the different subgroups they study (age 25-34 and 35-64, with and without college degrees).

Noting the importance of the public sector in driving our neighborhood-based results, we turn to data on the demographics of state and local employment provided by the Equal Opportunity Employment Commission for additional external validation of our findings on race and gender. The EEOC data, which are available by state, report public-sector employment by sex and race for large public sector employers. Using these data, we study the impact of Ban the Box legislation enacted in Massachusetts, Minnesota, and New Mexico in 2009 and 2010 on the share of public employees that are black men. From 2009 to 2013, the share of full-time public-sector employees who are black men increased by 7.1% (6.2%, 8.1%, and 11.2%, respectively), relative to a national decline in this share over 1.8% over the same time frame. Similarly, the share of new hires that are black men for these states went up by 5% relative to the national trend. Combined with the fact that roughly 1 in 5 employed African-Americans work in the public sector, these results lend further support for our finding of increased employment for black males.

The other side of the coin becomes apparent in row 2 and 3 of Table 8, where we show the estimates for white women and black women. We learn from those estimates that white women see their likelihood of employment drop by a sometimes statistically significant 0.2% - 0.4%, while black women see their likelihood of employment drop by an often significant 2%. 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. To sum up: black men gain, mostly to the detriment of black women. For policymakers who are concerned about a lack of "marriageable" black men and about family inequality (see e.g. Lundberg et al., 2016), this shift may well be an attractive one.

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) increase employment among African-American men while probably reducing employment for some 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 4%. 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. Regressing this measure on an indicator for whether a state has implemented Ban the Box measures while controlling for 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.[13]

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 Burning Glass postings also show a decrease in the number of job advertisements that mention criminal background checks, mirroring the Survey of State Criminal History Information Systems results.[14]

[13] Column 1, 2, and 3 of Appendix Table 4 show this result as well as a scaling based on the number of unemployed individuals and a logarithmic scaling.

[14] Appendix Table 4 provides more detail on these estimates.

The combination of these first two findings is what led to the third one: that Ban the Box measures lower the employment of women and while increasing the employment of African-American men. This third finding is surprising in light of press accounts of Holzer et al.'s (2006), Agan and Starr's (2016), and Doleac and Hansen's (2016) findings, which tend to highlight the effects of signal substitution and statistical discrimination. This is especially so in light of the second finding, as it is through signal substitution. Our alternative model of the screening problem (presented in section II.1.b) explains how this can be the case. It also highlights a key difference between Ban the Box rules and full-fledged bans on criminal-background checks. Ban the Box rules allow employers to maintain the reassuring fallback of the criminal-background check: for some applicants a strengthened prior may suffice to overcome a criminal records, but for others it will not. A full-fledged ban on criminal-background checks, on the other hand, may lead some employers to take no (perceived) risks and engage in aggressive statistical discrimination. All in all, our results show that Ban the Box rules accomplish most of what advocates have promised they would.

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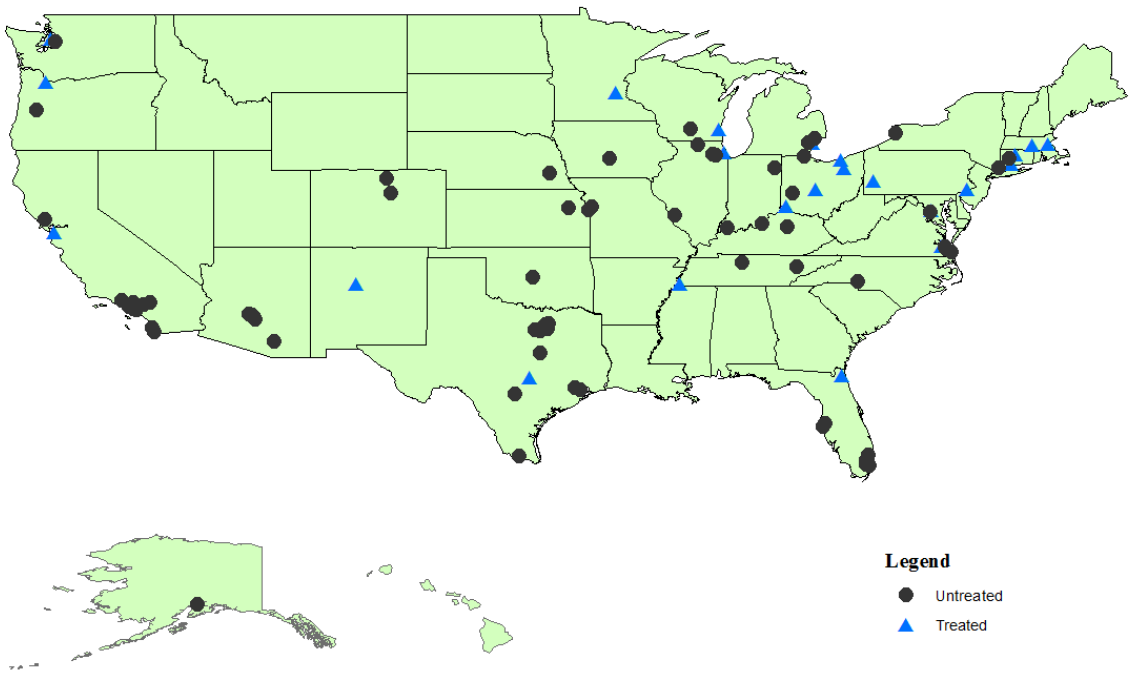
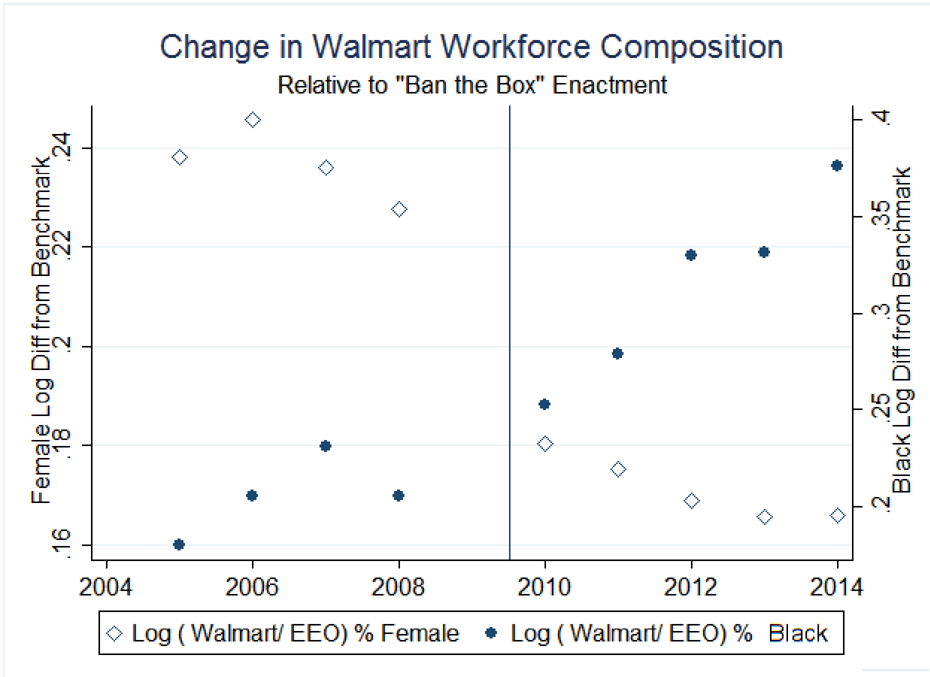
Figure 1: City Criminal Background Check Bans



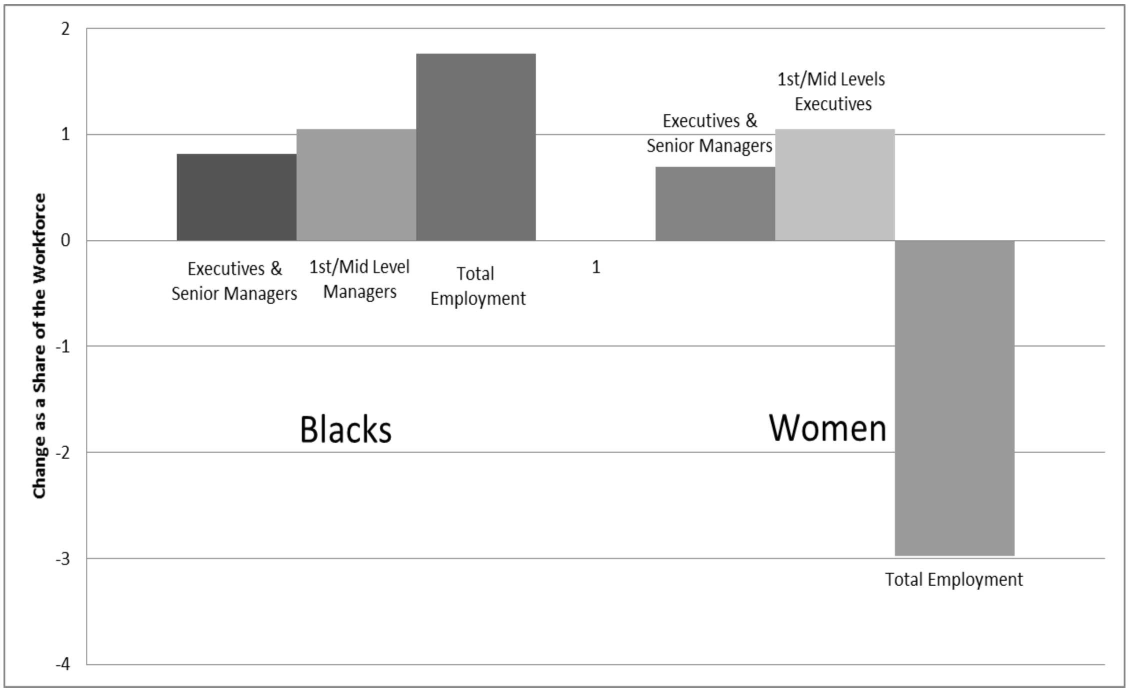
This map shows cities in our sample that had policies (treated) and that did not have policies (untreated) restricting the use of questions regarding criminal records in employment application procedures. Source: Natividad Rodriguez and Avery (2016).

Figure 2: Walmart "Ban the Box" Case Study



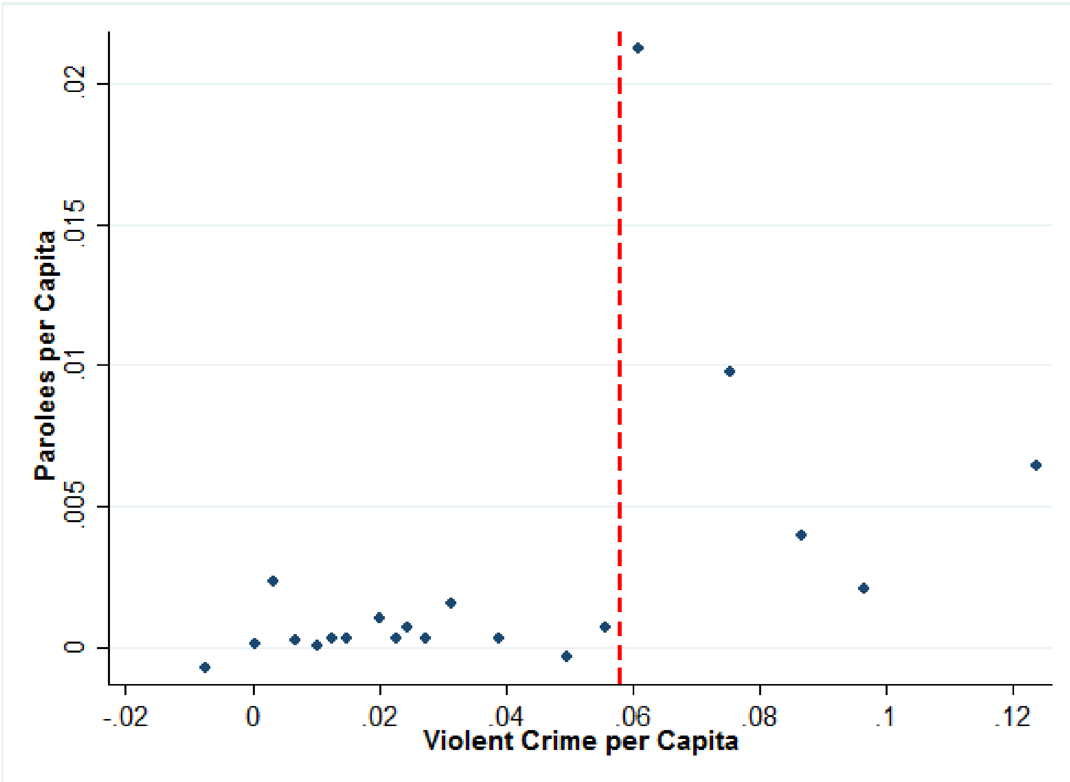
Note: This figure shows the log difference between Walmart's total employee demographics and its EEO-1 benchmark before and after the company "banned the box." Data on both company and benchmark demographics are taken from Walmart Diversity and Development Reports for the years indicated.

Figure 3: Walmart Change by Occupation



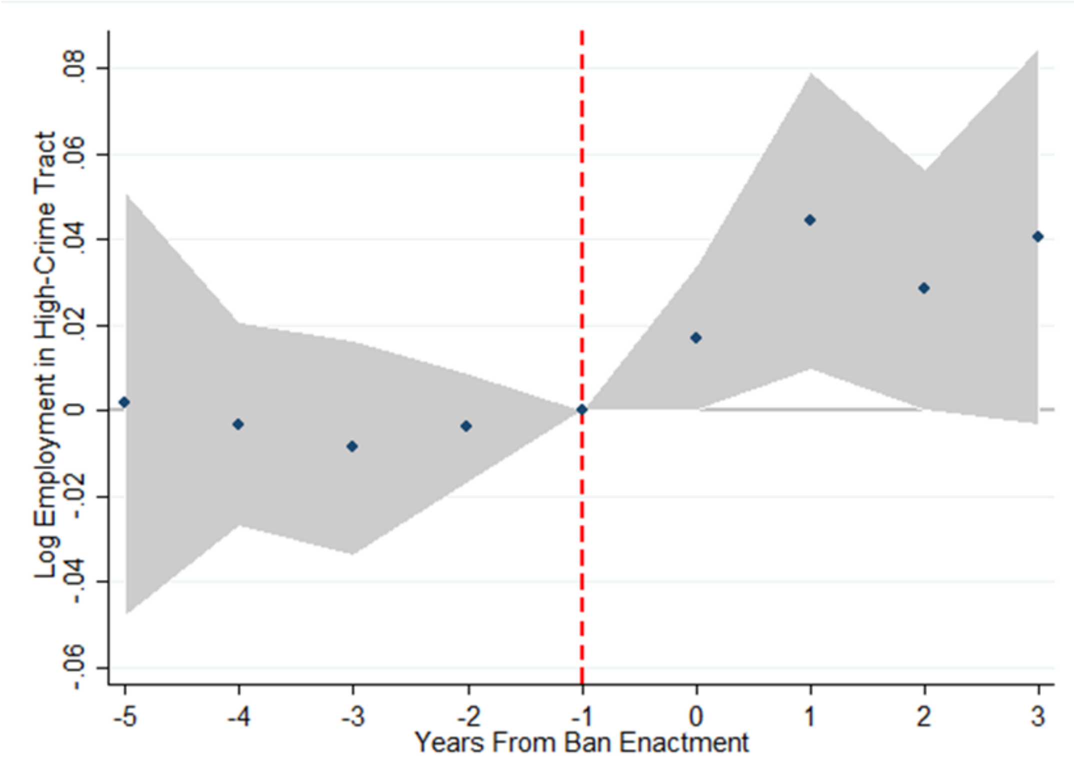
Note: This figure shows the change in the share of Wal-Mart executives and senior managers, first- and mid-level managers, and total employees who were black and female, from 2008 to 2012. Wal-Mart "banned the box" in 2010. These data are from Wal-Mart Diversity and Development Reports. Overall, the 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: Crime and Location of Parolees



This figure shows the relationship between parolees per capita and violent crime per capita at the tract level for Atlanta, Georgia, residualized by controlling for log population. To the right of the dashed line are the five bins (out of 20) that we classify as high crime. Information on addresses of current parolees is from the Georgia State Board of Pardons and Paroles website. Crime data is for the years 2009-2016 and is provided by the Atlanta Police Department. We drop outlier tracts with very high (> 8,000) and low (<2,000) numbers of residents.

Figure 5: Event Study Graph of Ban Implementation



Note: This figure reports the results of the regression:

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

where α_i are tract-level fixed effects, α_{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 β_t for $t = -5 \ldots 3$, where *0* is the year of ban enactment, engulfed by their 95% confidence intervals. Standard errors are clustered at the city level. See the text for more detail on variable construction and interpretation of estimates.

Table 1: Sample Characteristics

	Mean	Standard Deviation	5th Percentile	95th Percentile	Period	Observations
Tracts of Residence (annual)					*2002-2013*	
Total Employment (persons)	1607.5	841.799	425	3102		123,925
Employment Below $15K	438.2	218.7	125	828		
Employment from $15K to $40K	631.6	338.9	162	1249		
Employment Above $40K	537.7	338.8091	75	1365		
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					*2007-2013*	
Share of Postings Requiring a Bachelor's Degree	0.14	0.09	0.04	0.33		21,675
Share of Postings not Listing Experience Requirements	0.62	0.13	0.41	0.82		21,675
Years of Experience Required	1.16	0.59	0.45	2.35		21,675

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

Table 2: Baseline Results

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Employment	Log Employment	Log Employment	Log Employment	Log Employment	Log Employment
High Crime Tract $_i$ × City Ban $_t$	0.035**	0.034***	0.037**	0.035*	0.029*	0.035*
	(0.016)	(0.021)	(0.020)	(0.018)	(0.018)	(0.018)
Controls						
High Crime x Year Fixed Effects	X	X				
High Crime x Year Fixed Effects x Census Division			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
Observations	123,925	123,925	123,925	123,925	123,925	123,925
R-squared	0.946	0.946	0.946	0.946	0.946	0.946

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

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

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. Columns 3 to 6 replace $\alpha_{high\ crime*year}$ with $\alpha_{high\ crime*year*census\ division}$ to allow for different high-crime-tract trends for each census division. Observations are at the tract-year level. Standard errors are clustered at the 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

	(1)	(2)
	Log Employment	Log Employment
High Crime Origin Tract $_i$ × City 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 Places	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

	(1) Log Emp Wage< $15K	(2) Log Emp Wage>$15K & Wage<$40K	(3) Log Emp Wage>$40K
High Crime Tract $_i$ × City Ban $_t$	0.044**	0.027	0.031
	(0.017)	(0.020)	(0.032)
Controls			
High Crime x Year Fixed Effects	X	X	X
City x Year Fixed Effects	X	X	X
Observations	123,775	123,742	123,555
R-squared	0.936	0.947	0.953

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 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 5: Employment by Industry -- Large Response

	(1) Information	(2) Real Estate	(3) Education	(4) Government
High Crime Tract $_i$ × City Ban $_t$	0.053*	0.041*	0.042*	0.121**
	(0.027)	(0.023)	(0.022)	(0.059)
Controls				
High Crime x Year Fixed Effects	X	X	X	X
City x Year Fixed Effects	X	X	X	X
Observations	122,436	122,333	122,859	122,545
R-squared	0.903	0.844	0.921	0.894

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 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 6: Employment by Industry -- No Response

	(1) Agriculture, Forestry and Fishing	(2) Natural Resource Extraction	(3) Utilities	(4) Construction	(5) Manufacturing	(6) Wholesale	(7) Retail	(8) Health Care
High Crime Tract$_i$ × City Ban$_t$	0.010	0.006	0.024	0.024	0.013	0.003	0.019	0.037
	(0.036)	(0.043)	(0.030)	(0.026)	(0.015)	(0.033)	(0.021)	(0.024)
Observations	95,770	66,724	116,695	123,112	123,245	122,810	123,094	122,951
R-squared	0.711	0.885	0.715	0.923	0.937	0.902	0.918	0.921

	(9) Finance	(10) Professional Services	(11) Management	(12) Waste Management	(13) Entertainment	(14) Accommodation & Food Services	(15) Transportation & Warehousing	(16) Other
High Crime Tract$_i$ × City Ban$_t$	0.012	0.013	0.003	0.001	0.032	0.032	0.012	-0.000
	(0.027)	(0.036)	(0.026)	(0.022)	(0.030)	(0.019)	(0.017)	(0.033)
Observations	122,663	122,830	122,022	123,068	122,301	123,006	123,191	122,676
R-squared	0.912	0.916	0.846	0.908	0.823	0.917	0.895	0.890
Controls								
High Crime x Year Fixed Effects	X	X	X	X	X	X	X	X
City x Year Fixed Effects	X	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 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 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
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:

$$\textit{skill level}_{city,t} = \alpha_{city} + \alpha_t + \beta_t \text{ x } \textit{ban}_{it} + \varepsilon_{it},$$

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. Standard errors in parentheses clustered by city. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact on Demographic Groups

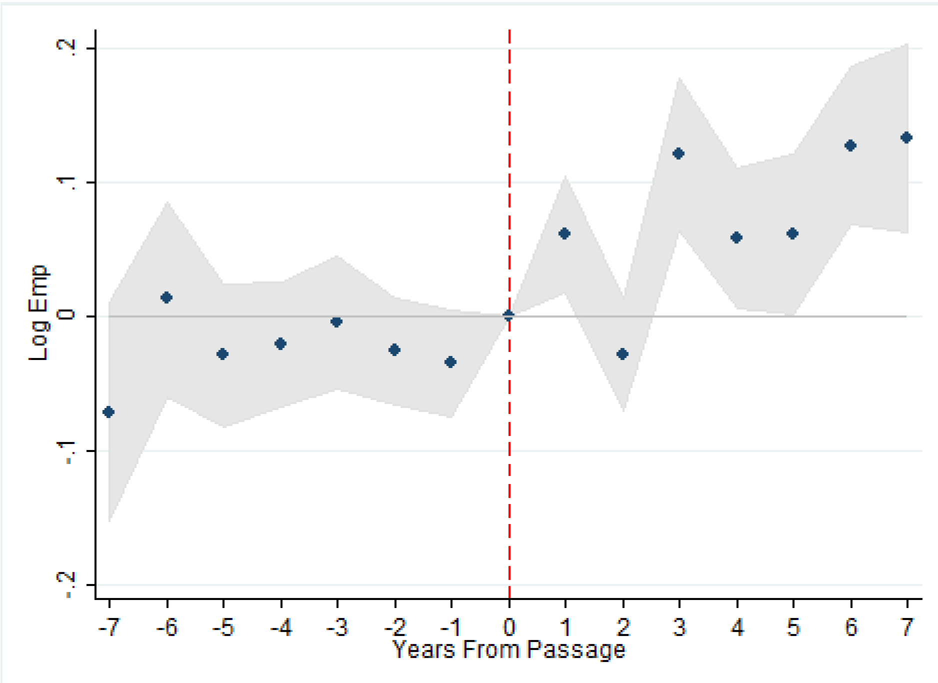
	(1) Employed	(2) Employed	(3) Employed	(4) Employed	(5) Employed	(6) Employed	(7) Employed	(8) Employed
Black Men x State Ban	0.024***	0.024***	0.026***	0.052***	0.035***	0.028***	0.048**	0.084***
	(0.005)	(0.003)	(0.008)	(0.006)	(0.012)	(0.01)	(0.023)	(0.029)
White Women x State Ban	-0.004	-0.003	-0.003*	-0.002	-0.006***	-0.004***	0.002	0.000
	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)
Black Women x State Ban	-0.019	-0.016***	-0.026***	-0.006	-0.019	-0.021***	-0.02**	0.024
	(0.015)	(0.006)	(0.006)	(0.02)	(0.024)	(0.007)	(0.008)	(0.034)
Controls								
Group x State Linear Trends			X	X			X	X
Group x State	X	X	X	X	X	X	X	X
Group x Year	X	X	X	X	X	X	X	X
State x Year	X	X	X	X				
County x Year					X	X	X	X
Sample	Full	In Labor Force	Full	In Labor Force	Full	In Labor Force	Full	In Labor Force
Observations	14,664,744	11,093,399	14,664,744	11,093,399	8,059,895	6,182,821	8,059,895	6,182,821
R-squared	0.0738	0.038	0.0738	0.0381	0.0737	0.0408	0.0738	0.0409

Note: This table reports regressions of the form:

$$\ln \text{employed}_{i,t} = \alpha_{group,s} + \alpha_{group,t} + \gamma \times X_{it} + \beta_{group} \times \text{ban}_{st} \times \text{group}_i + \gamma \text{ x } X_{it} + \varepsilon_{i,t}$$

where the αs control for arbitrary state and time trends for each demographic group. The data are from the American Community Survey from 2005 to 2013. All specifications control for age and education dummies X_{it}. Specifications 2, 4, 6 and 8 limit the sample to individuals who are currently in the labor force. Standard errors are clustered by state. See text for additional details. *** p<0.01, ** p<0.05, * p<0.1

Appendix Figure 1: Event Study Graph of Ban Implementation (Within-Tract)



Note: This figure reports the results of the regression:

$$ln\, emp_{od,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta \times years\ from\ ban_{dt} \times high\ crime_{o} + \varepsilon_{od,t},$$

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. 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 β_t for t = *-7 ... 7*, where *0* is the year of ban enactment, engulfed by their 95% confidence intervals. Standard errors are clustered at the tract level, and we drop origin-destination pairs where commuting flows fall below 10. See the text for more detail on variable construction and interpretation of estimates.

Appendix Table 1A: Ban the Box Legislation

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

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			
Franklin County, OH	2012	X	X	
Stark County, OH	2013			
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		
San Francisco, CA	2005		X
Richmond, CA	2013		
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		
Tampa, FL	2013		
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		
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		
New York, NY	2011		X
Cleveland, OH	2011	X	X
Akron, OH	2013		
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		
Richmond, VA	2013		
Portsmouth, VA	2013		
Virginia Beach, VA	2013		
Newport News, VA	2012	X	X
Petersburg, VA	2013		
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.

Appendix Table 3: Population Changes

Variables	(1)	(2)	(3)	(4)
	Log Population Δ	Log Population Δ	Log Population Δ	Log Population Δ
High Crime Tract $_i$ × City Ban $_i$	-0.021	-0.009	-0.028	-0.015
	(0.026)	(0.023)	(0.057)	(0.036)
City Ban $_i$	-0.107***	-0.333*	-0.105***	-0.332*
	(0.015)	(0.179)	(0.031)	(0.187)
High Crime Tract $_i$	-0.120***	-0.087***	-0.117***	-0.086***
	(0.018)	(0.018)	(0.023)	(0.023)
Cluster Variable	Zip	Zip	City	City
City Fixed Effects		X		X
Observation	10,486	10,486	10,496	10,496
R-squared	0.033	0.104	0.032	0.099

Note: This table reports regressions of the form:

$$\log \text{Population } \Delta_i = \beta_0 + \beta_1 \times \text{ban}_i + \beta_2 \times \text{high crime}_i + \beta_3 \times \text{ban}_i \times \text{high crime}_i + \varepsilon_i$$

for tracts that our in our main sample. The population change is calculated from 2009 to 2014. Data are from the 2000 Census and the 2009-2014 American Community Survey. Columns (2) and (4) include city fixed effects. Standard errors are clustered either by zip code or city. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 4: Ban the Box Impact on Background Checks

	(1)	(2)	(3)	(4)	(5)
	Bureau of Justice Statistics: Non-Criminal Background Checks by State-Year			Fraction of Jobs Mentioning Criminal Background: Job Postings by City-Year	
	$\text{Log(Checks}_{st})$	Checks per Hire_{st}	Checks per Unemployed_{st}	Log(Criminal Record Check Postings)	Log(Fraction Criminal Record Check)
$\text{Ban}_{\text{state, t}}$	-0.837**	-0.162***	-0.970*		
	(0.396)	(0.048)	(0.502)		
$\text{Ban}_{\text{city, t}}$				-.069*	-.100**
				(.043)	(.050)
Controls					
Year Fixed Effects	X	X	X	X	X
State/City Fixed Effects	X	X	X	X	X
Observation	164	172	179	488,561	479,722
R-squared	0.96	0.83	0.80	0.943	0.876

Note: This table shows the relationship between Ban the Box measures and employer requests for and announcements of criminal background checks. All regressions use a difference-in-differences specification using year fixed effects and state or city fixed effects. Regressions (1)-(3) use data from the Bureau Justice Statistics Survey of State Criminal History Information Systems that are available for 2006, 2008, 2010, and 2012 for most states. We eliminate a clear data error for Washington state in 2008. Information on the number of hires and unemployed by state come from the Bureau of Labor Statistics. Regressions (4) and (5) are run on city-level observations and use data from Burning Glass Technologies. We count a job posting as announcing a criminal background check if it mentions "criminal background check" or "criminal record check." The period covered is 2011-2015. Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1