Veuger, Stan; Shoag, Daniel

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Daniel Shoag
Harvard Kennedy School and Case Western Reserve University
Stan Veuger
American Enterprise Institute

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"Ban the Box" Measures Help High-Crime Neighborhoods[i]

Daniel Shoag[ii]

Stan Veuger[iii]

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Many localities have in recent years regulated the use of questions about criminal history in hiring, or "banned the box." We show that these regulations increased employment of residents in high-crime neighborhoods by up to 4%, consistent with the central objective of these measures. This effect can be seen in both aggregate employment patterns for high-crime neighborhoods and in commuting patterns to workplace destinations with this type of ban. The increases are particularly large in the public sector and in lower-wage jobs. This is the first nationwide evidence that these policies do, indeed, increase employment opportunities in neighborhoods with many ex-offenders.

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[ii] Case Western Reserve University Weatherhead School of Management and Harvard Kennedy School, dxs788@case.edu.
[iii] American Enterprise Institute for Public Policy Research, IE School of Global and Public Affairs, and Tilburg University. Corresponding Author: American Enterprise Institute, 1789 Massachusetts Avenue, Washington, DC 20036, stan.veuger@aei.org.

Slightly fewer than half of all private-sector firms and practically all government agencies in the United States 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 (Connerley et al., 2001). 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 (Pager et al., 2009; The Sentencing Project, 2019). 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" (Avery, 2019; Stacey and Cohen, 2017).

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 workers in high-crime neighborhoods. 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%; these are also the neighborhoods with the greatest population of workers with criminal records. 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. What this shows is that, perhaps surprisingly, Ban the Box measures can be seen as effective place-based policies.

The recency of Ban the Box measures means that research on their consequences has so far been limited. In addition, previous work, e.g. Doleac and Hansen (forthcoming), focused mainly on the distributional consequences of these policies along racial and age lines, in particular changes in outcomes for young black men, in order to identify potential unintended consequences of the bans. We focus instead on evaluating these policies by studying their impact on the labor market performance of workers with criminal records, the group specifically targeted, to see whether the policies' *intended* consequences materialized. We also use hyperlocal (census tract level) data that allow us to identify the beneficiaries of Ban the Box policies at a more granular level than the MSA-level changes studied by Doleac and Hansen.

The paper most directly related to our work is by Jackson and Zhao (2016), who study the introduction of Ban the Box in Massachusetts in late 2009. They link ex-offenders' criminal records to unemployment insurance quarterly wage records, and find that their employment does not vary much in the year after Ban the Box was introduced. Jackson and Zhao construct a control group of workers without criminal records, but can only match them to treated workers based on age and residential location, not on skill or educational attainment. This makes it difficult to adequately control for potentially differential trends stemming from the financial crisis that occurred at the same time.

We do not use individual-level criminal records. Instead, our contributions are that we provide nationwide estimates of the impact of Ban the Box rules on high-crime neighborhoods, which is where workers with criminal records are likely to reside; we present a broader range of identification strategies; and we are not restricted to a Ban the Box measure implemented at the very nadir of the Great Recession's labor market experience. We exploit variation in whether and when a range of 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.

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, the roll-out of the policies we study, and the conceptual framework within which we will evaluate their effectiveness. In Section II we introduce the data we will draw upon in that evaluation. Section III explains why we focus on high-crime neighborhoods: their residents are more likely to have criminal records. We then discuss the impact of Ban the Box measures on employment in such neighborhoods (section IV), and the industries and income categories in which these employment effects materialize (section V). Section VI concludes by discussing the implications of our findings for public policy.

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. For example, roughly 17% of the job listings in the large database of postings collected by Burning Glass Technologies, a leading provider of online job market data, announce such checks in the advertisement itself. This represents 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). This was followed by the Fair Chance Act, included in the 2020 National Defense Authorization Act, which restricted the use of criminal background questions by federal contractors as well as the federal government itself (see Craigie et al., 2019). Additionally, a number of private-sector employers, most prominently Home Depot, Koch Industries, Target, and Walmart, have also recently adopted a policy of not asking job applicants about their criminal history (Levine, 2015; Staples, 2013).

These policies reflect a conceptualization of the way in which employers approach the decision of whether to hire an applicant as a screening problem, similar to those in Aigner and Cain (1977), Autor and Scarborough (2008), or Wozniak (2015). Employers want to hire high-productivity workers, and try to assess the productivity of job applicants. They cannot necessarily rely on applicants' self-identification, as applicants have an incentive to present themselves as high-productivity even when they are low-productivity workers. Instead, employers rely on signals they receive about worker quality. One commonly used signal is the applicant's criminal history, which is taken to proxy for low productivity. If employers rely on this signal in the screening process, it makes it more difficult for applicants with criminal records to find suitable employment. If they do not, applicants with criminal records will find it easier to find work. Finally, if employers delay reliance on the criminal-records signal until later in the application process, as they (are forced to) do under Ban the Box policies, the signals collected earlier in the application process may reduce the weight placed on applicants' criminal record, which will also help such applicants.

A possible concern is that under a ban on the (early) use of a specific signal, employers will start relying (more) on other signals to proxy for productivity. Such signals may include education and experience (as in Clifford and Shoag, 2016) or race (as studied by Holzer et al. (2006), Agan and Starr's (2018), Craigie (2020), and Doleac and Hansen (forthcoming)), and may themselves negatively affect the employment prospects of other or overlapping marginalized groups of workers. We address this concern in more detail in Section VI. Even so, with Ban the Box measures in place, we would expect more applicants with criminal records to be hired. Such applicants are likely to live in high-crime neighborhoods, as we will see, and we should thus expect employment in such neighborhoods to increase. Let us turn now to the data we will use to test this prediction empirically.

II. 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 in a variety of ways. 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 Tables 1a and 1b provide 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." Figure 2 shows that the crime rate distribution of tracts displays significant skewness. While any specific number is arbitrary, we focus on the top 25% of high-crime tracts to strike a balance between on the on hand covering most high-crime places, not only true outliers, and on the other hand not covering those tracts where variation might be noise. As the figure shows, there is not much variation in the lower quartiles.

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.

Data on Parolees and Released Prisoners

We use data from the Justice Atlas of Sentencing and Corrections, produced by the Justice Mapping Center, on the number of released prisoners and parolees per capita at the census tract level. These data come from state-level departments of corrections, parole, and probation. In addition, we use the home addresses of parolees in the city of Atlanta as of April 12, 2016, from the Georgia State Board of Pardons and Paroles.

III. High-Crime Areas and Workers with Criminal Records

There is, unfortunately, no national data on employment outcomes for individuals with criminal records, the actual treatment group. In fact, the available data do not even allow for accurate tallies of the number of people with such records – estimates vary by the (tens of) millions (Brame et al., 2012; McGinty, 2015). Our focus in this paper is instead on neighborhoods with high crime rates. If workers with criminal records are more likely to live in such neighborhoods, and if Ban the Box measures work as intended, they should lead to better outcomes in these neighborhoods. This reasoning relies on the fact that individuals with criminal records are more likely to live in high-crime neighborhoods.

To establish this fact, we use data from the Justice Atlas of Sentencing and Corrections on released prisoners and parolees. Figure 3a and 3b plot rates of released prisoners and parolees per capita at the census tract level against the number of assaults and murders per capita from the NNCS data. To ease viewing, tracts are divided into equal-population bins. The figure shows that high-crime neighborhoods are home to significantly more parolees per capita and released prisoners, and, by implication, to 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.[1]

[1] Appendix Figure 1 serves as a robustness check on this finding. It uses addresses-level location data on parolees published by the Georgia State Board of Pardons and Paroles. We geocode these addresses, and combine them with geocoded violent crime data provided by the Atlanta Police Department at the tract level. This produces a

IV. Employment Outcomes for Residents of High-Crime Areas

In this section we present our central 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:

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

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

similar pattern to that generated using Justice Atlas data. Note that while property and drug crime rates are correlated with our measure, they are less reliable proxies, perhaps due to variation in reporting.

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.[2]

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.[3][4] 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 1,000 times. We find that our estimate using the true assignment of laws exceeds 96.6% 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 employment accounted for by residents of high crime tracts, our estimates are unlikely to be driven by them.[5]

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[2] 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.

[3] Appendix Table 2 shows that this result is not driven by concurrent population increases. Appendix Table 3 uses the Coarsened Exact Matching algorithm to match areas that did and did not become subject to Ban the Box regulations as a robustness check on our baseline results.

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

[5] We believe that these employment gains mostly 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

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%.[6]

Figure 4 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 post dummy included in in equation 1 above:

$$ln\ emp_{it} = \alpha_i + \alpha_{city \times t} + \alpha_{high\ crime \times t} + \beta_t \times high\ crime_i \times year\ dummies_{city,t} + \varepsilon_{it}\ , \qquad (2)$$

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.[7]

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

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.

[6] 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.

[7] Appendix Figure 2 shows our results separately for high-crime and low-crime tracts, both relative to medium-crime tracts. Employment in high-crime tracts increases somewhat, while employment in low-crime tracts decreases.

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:

$$ln\, emp_{od,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta \times ban_{dt} \times high\, crime_o + \varepsilon_{od,t}\ , \quad (3)$$

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. Tracts are classified as high- or low-crime tracts based on National Neighborhood Crime Study data from 1999, 2000, and 2001, well before the introduction of the bans, to ensure that crime levels are not endogenous. We limit the sample to origin-destination flows with at least 10 observations.

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.[8] Column 2 and 3 restrict the sample to observations with at least 20 and 30 commuters, respectively, which barely changes our estimates, suggesting that our results are not driven by the large numbers of origin-destination combinations with low numbers of commuters.

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, we explicitly check for pre-trends in our baseline specification in Figure 4 and find none. Second, we include city-year fixed effects in Table 2, controlling for arbitrary differences in trends across

[8] Appendix Figure 3 shows an event study graph similar to that in Figure 4, and again shows no significant pre-trend.

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 2 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.

V. 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 they are below $15,000 annually, between $15,000 and $40,000, or over $40,000, and in which industry category they work. Note that this information is collected at the individual level: the LODES data effectively provides counts of residents in each industry or wage category. 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.

V.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.[10] 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.

V.2 Industries

Table 5 and 6 show our results split out by broadly defined industry.[11] 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. This finding confirms Craigie's (2020) estimates of dramatic increases

[10] Note that the data form a repeated cross-section: our identification strategy relies on the assumption that, conditional on arbitrary citywide trends, the industry and wage characteristics of tract-level migrant flows are not correlated with differential changes in the industry and wage characteristics of commuting flows to nearby tracts that do and do not become subject to Ban the Box rules.

[11] The industry categorization is the one used in the LODES data; assignments of jobs to different categories are determined there as well. Appendix Table 4 shows the crosswalk from this categorization to NAICS codes.

in public-sector employment for workers with criminal records in the NLSY. 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 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.

VI. Discussion

The central finding in this paper is that Ban the Box measures improve the labor market outcomes of residents of high-crime neighborhoods, a good proxy for the labor market outcomes of workers with a criminal record. Ban the Box legislation thus 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.[12]

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 do not study such upskilling responses from the demand side here, but they are likely to occur and would lead to the creation of groups of losers from the policy. We leave the question whether this response has indeed materialized to future work – but if it did, Ban the Box measures must have produced groups of losers in addition to the groups of workers it benefits.

Potential groups of losers from Ban the Box initiatives are the focus of Agan and Starr's (2018), Craigie's (2020), and Doleac and Hansen's (forthcoming) studies, which emphasize concerns about statistical discrimination, especially against African-Americans.[13] This type of consequence, while not in direct contradiction of Ban the Box advocates' immediate objectives, may give policymakers pause. Doleac and Hansen analyze CPS data using a difference-in-difference design and focus much of their write-up on young, low-skilled black men, who in their preferred specification become 3.4% percentage points less likely to be employed after the

[12] Column 1, 2, and 3 of Appendix Table 5 show this result as well as a scaling based on the number of unemployed individuals and a logarithmic scaling.
[13] Agan and Starr carry out a field experiment that looks at call-back rates as the main employment-related outcome variable, and their results are hard to compare directly to ours.

introduction of Ban the Box rules. Sampling variation aside, there are two obvious explanations for this effect on young, low-skilled black men. The first one is that, as Doleac and Hansen argue, employers respond to Ban the Box measures by engaging in (statistical) discrimination on the basis of race, which leads to job losses among members of those racial groups most likely to have criminal records, in particular African-Americans. A second, competing, explanation is that Ban the Box measures leads to a shift of labor market opportunity away from demographic groups that are less likely to have criminal records (such as young people) toward groups that are more likely to have criminal records (such as old people).

An intuitive way to distinguish between these two explanations is to look at older black men, who are more likely to have criminal records than young black men (Brame et al., 2012; McCauley, 2017; Shoag and Veuger, 2019). Doleac and Hansen report, in their Table 7, that employment for this group increases, suggesting that statistical discrimination on the basis of race alone is not what drives the worsening outcomes for younger black men. In fact, a back-of-the-envelope calculation that weights the effects reported in Doleac and Hansen's Table 7 for various groups of black men by their population shares suggests a slight increase in employment for black men between the ages of 25 and 64. When we replicate their results, we find a similar result: a small and statistically insignificant increase in employment for black men between the ages of 25 and 64. When we use our own cross-tract specification to study employment in the 25% of tracts with the greatest share of African-Americans based on the LODES data, we again find a small and statistically insignificant results. Finally, Craigie's triple-difference estimation using NLSY data confirms that there seems to have been no large racial backlash in response to Ban the Box rules. All this suggests that it is the second explanation set out above, jobs shifting from groups less likely to have criminal records to workers more likely to have criminal records, that accounts for the labor market consequences of Ban the Box policies. If employers had instead to turned to statistical discrimination on the basis of race to proxy for criminal records, one would have expected to see job losses, not gains, among older black men as well.

Policymakers may well be concerned about the distributional consequences of these policies – in that they make it so that workers less likely to have criminal records, including young workers, will face more labor market competition – but it is hard to argue that these are unintended, as opposed to logical, consequences of the policies in question.

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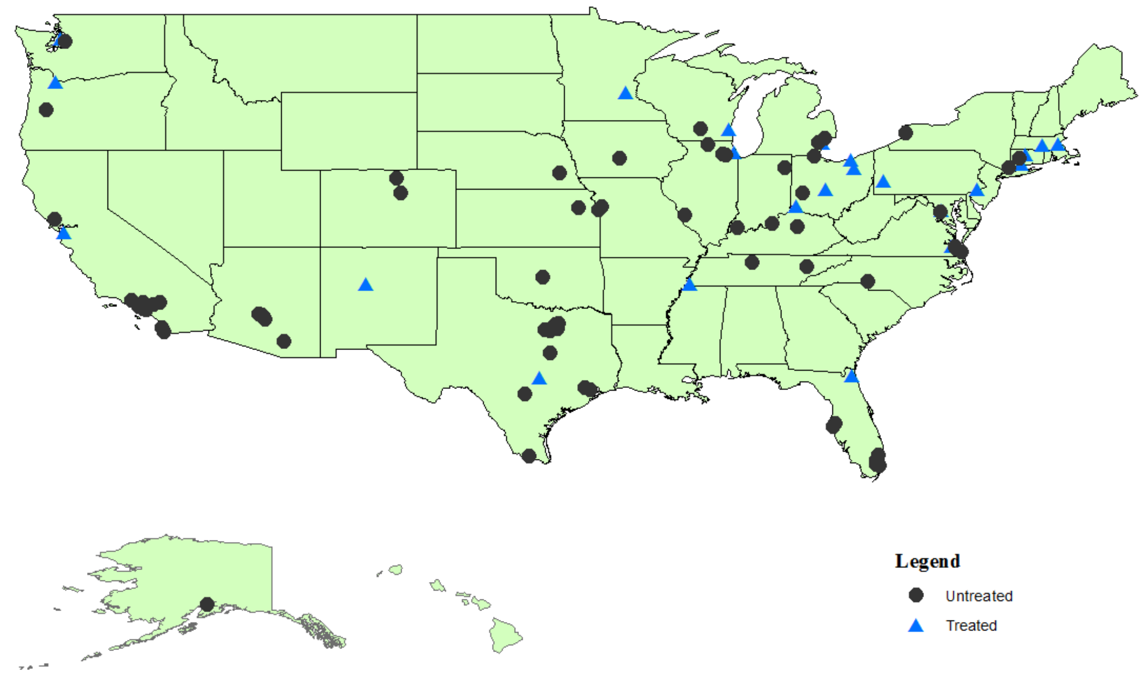
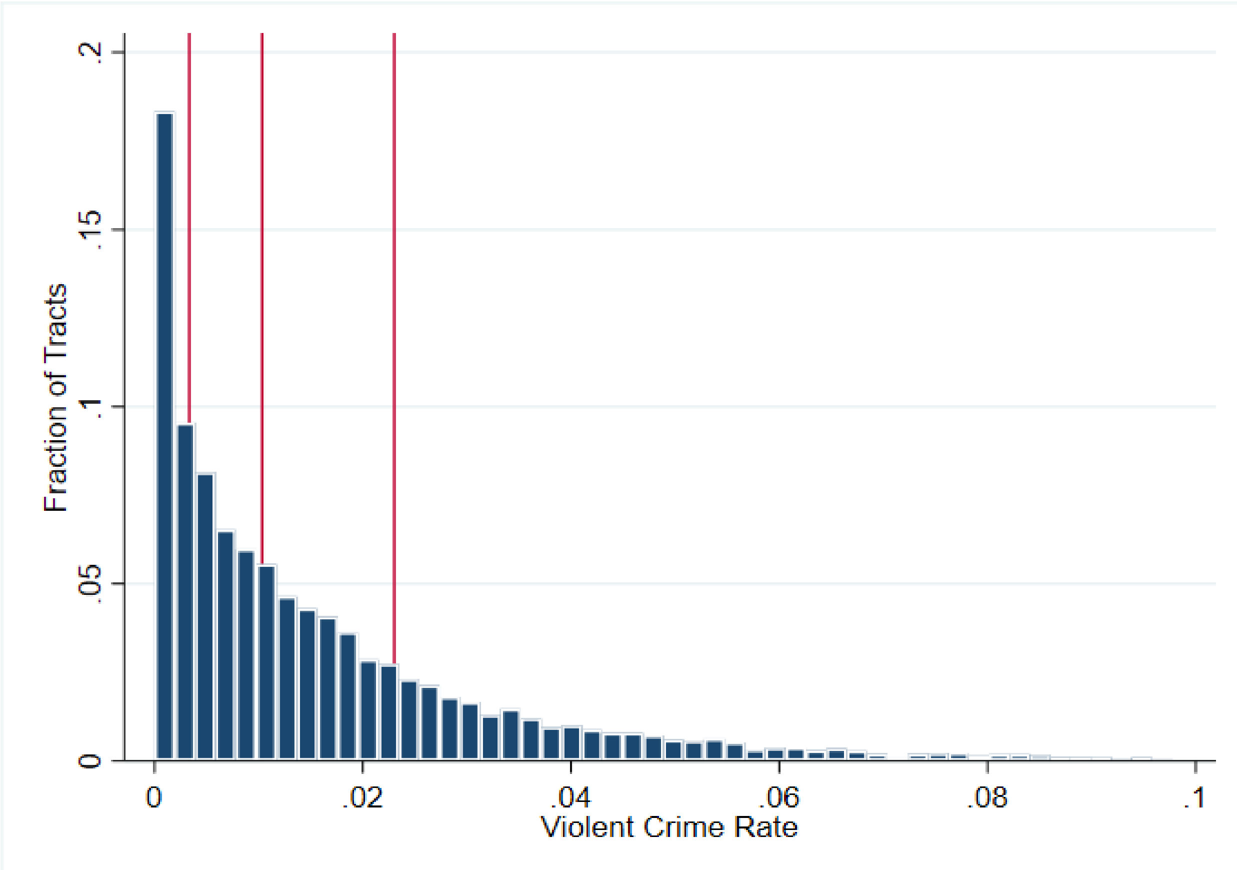
Figure 1: City Criminal Background Check Bans



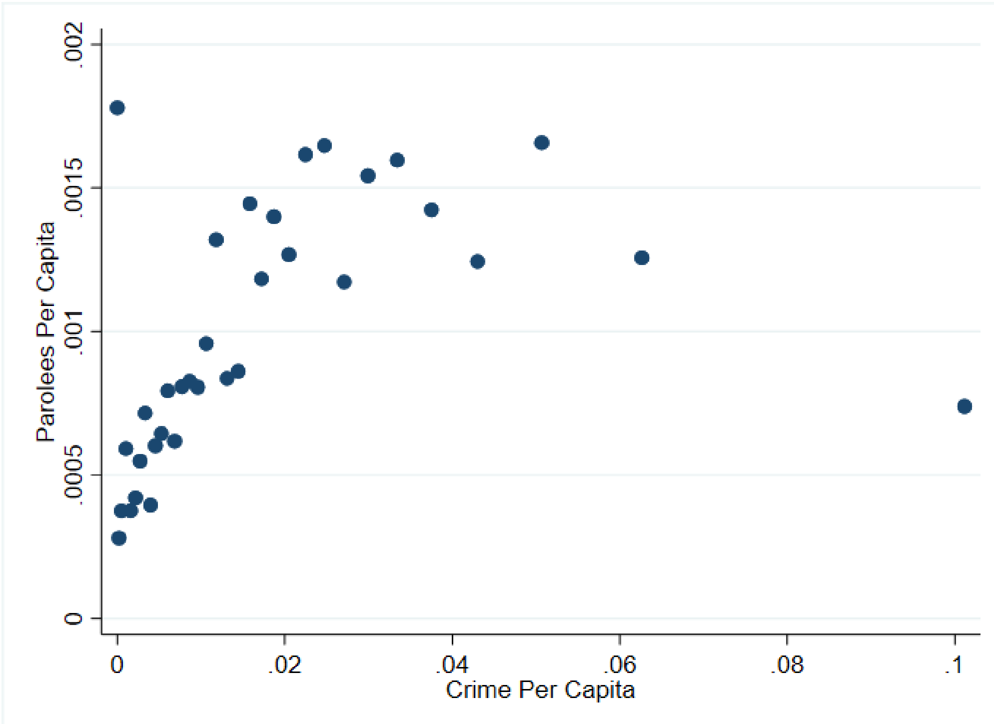
This map shows cities in our sample that had policies (treated) and that did not have policies (untreated) restricting the use of questions regarding criminal records in employment application procedures. Source: Natividad Rodriguez and Avery (2016).

Figure 2: Distribution of Crime Rates by Census Tract



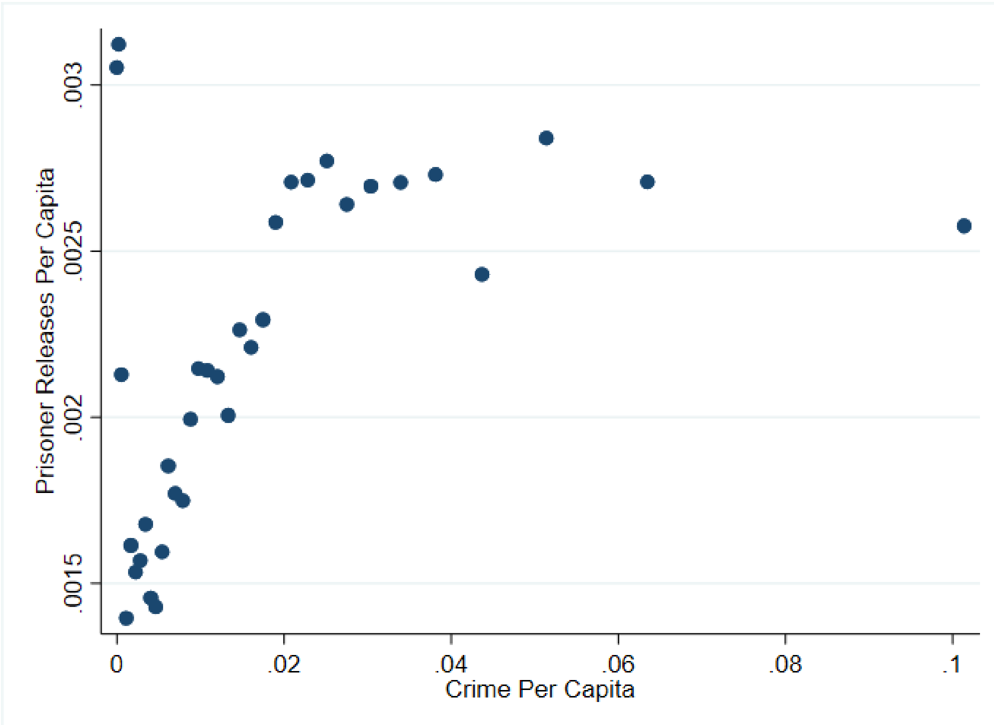
Note: This figure shows the distribution of rates of violent crime (assault and murder) per capita by tract. Vertical red bars indicate quartile cutoffs. Crime data are from the National Neighborhood Crime Study.

Figure 3A: Crime and Parolees



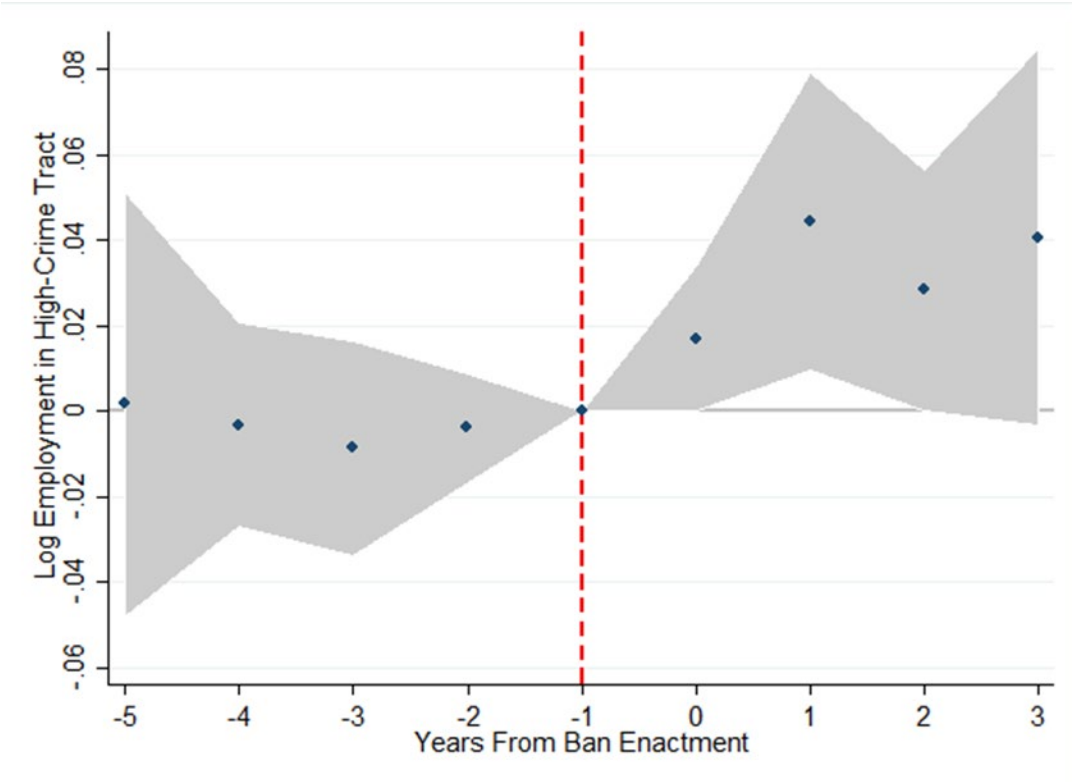
Note: This figure plots parolees per capita against violent crime per capita, controlling for population. To ease visibility, observations, which are at the census tract level, are grouped into bins according to the amount of crime per capita, and we plot average parolees per capita for each bin. Information on parolees is from the Justice Atlas of Sentencing and Corrections website and is available for 17 states. Crime data are from the National Neighborhood Crime Study.

Figure 3B: Crime and Prisoner Releases



Note: This figure plots prisoner releases per capita against violent crime per capita, controlling for population. To ease visibility, observations, which are at the tract level, are grouped into bins according to the amount of crime per capita, and we plot average prisoner releases per capita for each bin. Information on prisoner releases is from the Justice Atlas of Sentencing and Corrections website and is available for 20 states. Crime data are from the National Neighborhood Crime Study.

Figure 4: Event Study Graph of Ban Implementation



Note: This figure reports the results of the regression:

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

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 series of year dummies for each year 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.cx

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

Note: Data are from the LEHD Origin-Destination Employer Statistics.

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)	(3)
	Log Employment	Log Employment	Log Employment
High Crime Origin Tract $_i$ × City Ban Destination $_t$	0.041***	0.045***	0.039***
	(0.015)	(0.013)	(0.014)
Controls			
Origin-Destination Fixed Effects	X	X	X
Destination-Year Fixed Effects	X	X	X
Origin-Year Fixed Effects	X	X	X
Sample	Origin-Destination Pairs with Employment >10	Origin-Destination Pairs with Employment >20	Origin-Destination Pairs with Employment >30
Observations	178,208	115,969	87,393
R-squared	0.970	0.977	0.981

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. Observations are tract-destination years and standard errors are clustered by tract and are reported in parentheses. Different columns drop observations with commuting flows below 10, 20, and 30 workers. 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)	(2)	(3)
	Log Emp Wage< $15K	Log Emp Wage>$15K & Wage<$40K	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)	(2)	(3)	(4)
	Information	Real Estate	Education	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

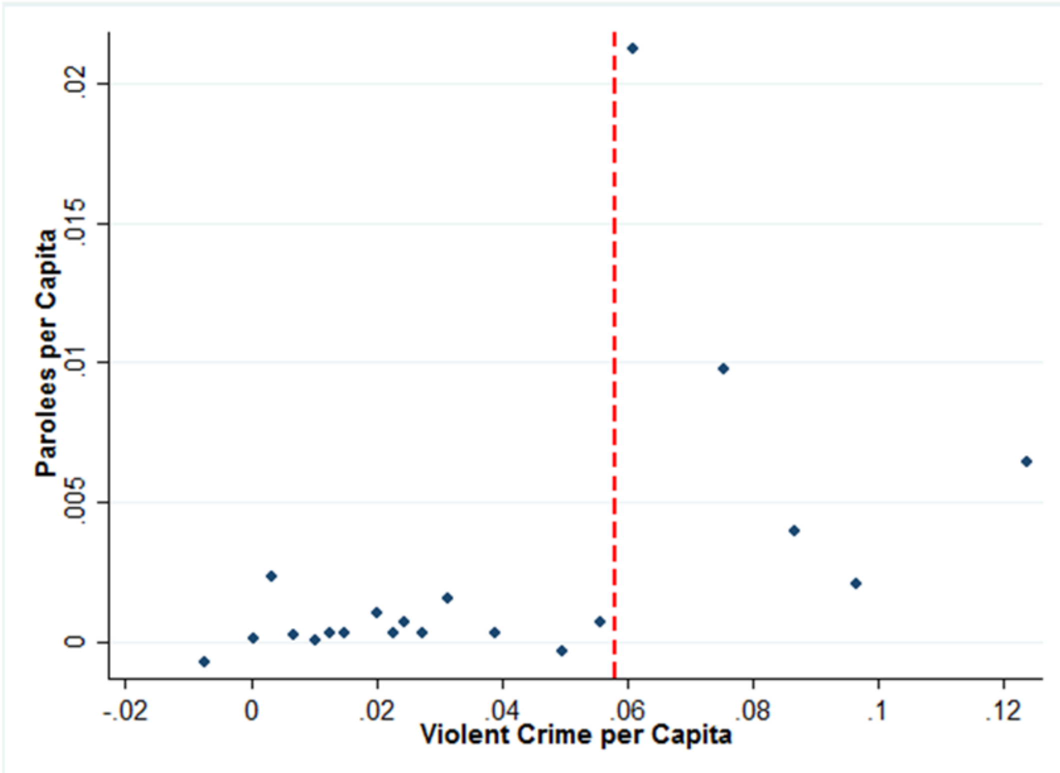
This table reports regressions of the same form as column 1 of Table 2, but with the sample split into industry subsamples. Industry assignments are from LODES. Observations are at the tract-year level. Standard errors are clustered at city level and are reported in parentheses. Data are from the LEHD 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)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Agriculture, Forestry and Fishing	Natural Resource Extraction	Utilities	Construction	Manufacturing	Wholesale	Retail	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)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Finance	Professional Services	Management	Waste Management	Entertainment	Accommodation & Food Services	Transportation & Warehousing	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

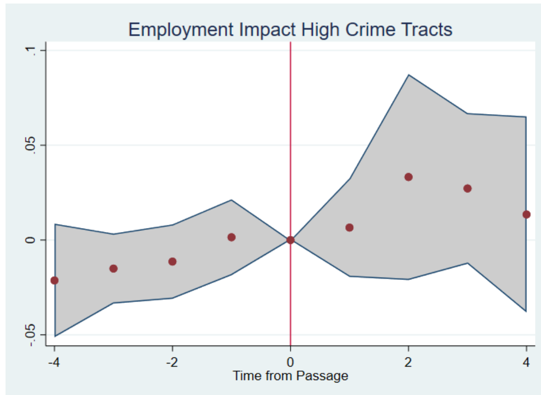
Appendix Figure 1: Crime and Location of Parolees



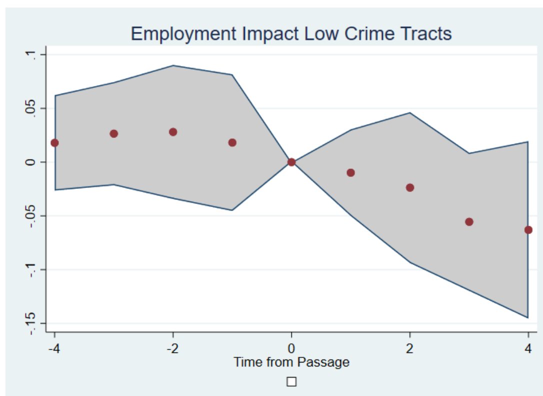
This figure shows the relationship between parolees per capita and violent crime per capita at the tract level for Atlanta, Georgia, residualized by controlling for log population. To the right of the dashed line are the five bins (out of 20) that we classify as high crime. Information on addresses of 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.

Appendix Figure 2: Event Study Graph of Ban Implementation in High- and Low-Crime Tracts

A

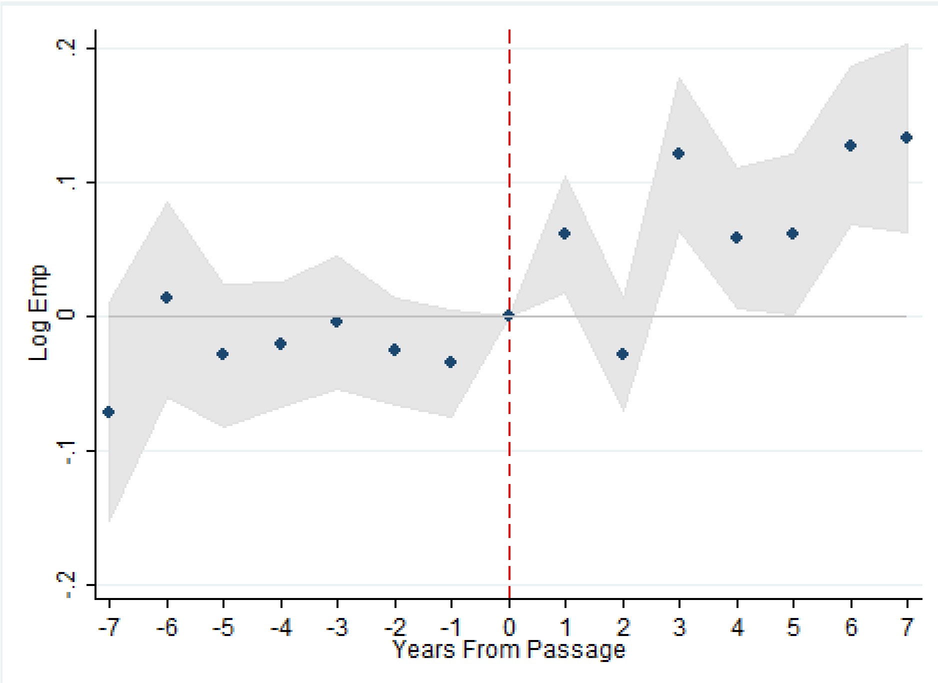


B



This figure shows the results from Figure 4 separately for high-crime (Panel A) and low-crime (Panel B) tracts, both relative to medium-crime tracts.

Appendix Figure 3: Event Study Graph of Ban Implementation (Within-Tract)



Note: This figure reports the results of the regression:

$$ln\ emp_{od,t} = \alpha_{od} + \alpha_{d\times t} + \alpha_{o\times t} + \beta \times year\ dummies_{dt} \times high\ crime_o + \varepsilon_{od,t},$$

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 series of year dummies for each year 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	ACS
California	2013		
Hawaii	1998		X
Massachusetts	2010		X
Minnesota	2009	X	X
New Mexico	2010	X	X
Rhode Island	2013		

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

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

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 3: Baseline Results with Matching Specification

	(1)	(2)	(3)	(4)	(5)
	Log Employment	Log Employment	Log Employment	Log Employment	Log Employment
High Crime Tract $_i$ × City Ban $_t$	0.035**	0.076***	0.075***	0.045**	0.049**
	(0.014)	(0.016)	(0.017)	(0.020)	(0.024)
Controls					
High Crime x Year Fixed Effects	X	X	X	X	X
City x Year Fixed Effects	X	X	X	X	X
Census Tract Fixed Effects	X	X	X	X	X
High Crime Tract Percentile Definition	> 75th	> 75th	> 75th	> 75th	> 75th
Coarsened Exact Matching Specification	None	Demographic	Economic	Industry	All
Observations	123,667	122,059	122,253	111,549	123,667
R-squared	0.947	0.946	0.957	0.951	0.956

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

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

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. For column 2, we use sample weights created with coarsened exact matching (CEM) with tract-level demographic variables (i.e. percentage black, percentage female, and percentage over the age of 65). For column 3, we use CEM sample weights created with tract-level economic variables, including median household income and the unemployment rate. For column 4, we use CEM sample weights created with industry mix variables. For column 5, we use CEM sample weights created from using all the variables in the three aforementioned categories. Observations are at the tract-year level. Standard errors are clustered at the zip-code 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

Appendix Table 4: 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 5: 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