# Ban the Box and Cross-Border Spillovers[*]

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Ban-the-box (BTB) policies prevent employers from asking job applicants about their criminal history until very late in the hiring process. The intent of these policies is to help individuals with criminal records find employment by reducing the stigma associated with arrest or conviction. However, such a policy could induce employers to statistically discriminate on the basis of observable characteristics, such as race (or current employment status), if employers believe that certain racial groups (or the unemployed) are more likely to have a criminal history. This paper studies whether labor market effects of BTB policies spill over into neighboring legal jurisdictions. Using the 2005-2014 waves of the American Community Survey and a difference-in-differences method, we find that annual earnings for employed, young, non-college-educated black men increase by 9.5 percent following the implementation of BTB in neighboring labor markets. Earnings for young, non-college-educated Hispanic and white men are flat. The effects for black men do not appear to be driven by changes in the composition of the sample of employed workers. We interpret these results as consistent with the theoretical predictions of the Black (1995) labor market search model with discrimination.

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# 1 Introduction

In recent years, policies aimed at helping the large formerly incarcerated population reintegrate into society have garnered bipartisan support. One such example are Ban-the-Box (BTB) laws, which require covered firms to remove questions about criminal convictions from job applications and delay background checks until later in the hiring process (see Figure 1 for an example of "the box"). These laws are intended to make it easier for individuals with a criminal history to obtain employment, and are becoming increasingly common: as of 2018, 35 states (including the District of Columbia) and more than 150 cities and counties have adopted BTB or similar "fair-chance" policies, covering 75 percent of the U.S. population (Avery, 2019).

While there is a growing literature on the effects of BTB on workers directly covered by these policies, this paper is the first to study how the implementation of a ban-the-box policy affects the labor market outcomes of individuals living in neighboring legal jurisdictions. A ban-the-box policy in one legal jurisdiction could affect labor market outcomes for individuals residing in a neighboring legal jurisdiction if both jurisdictions are part of the same local labor market. In our context, we use MSA-state pairs (e.g. the Illinois, Indiana, and Wisconsin portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA are distinct, neighboring MSA-state pairs). Of course, the implementation of BTB in a border jurisdiction can only be a credible improvement in outside options if individuals can and will realistically travel to a bordering jurisdiction for work.

To analyze the labor market effects of BTB on individuals living in neighboring jurisdictions, we use the 2005 to 2014 waves of the American Community Survey (ACS). We restrict our sample to young (25-34), non-college-educated black, Hispanic, and white men. We focus on these groups because the ACS does not contain information on criminal history, and these demographic groups are most likely to be affected by statistical discrimination.

We find that annual earnings for employed young, non-college-educated black men increase by 9.5 percent following implementation of BTB in bordering jurisdictions. Earnings do not increase for similarly situated Hispanic and white men. We view these findings as consistent with the fact that black men have substantially higher rates of contact with the criminal justice system (Bronson and Carson, 2019).

One may be concerned that the increase in annual earnings for young, non-college-educated black men is due to ban-the-box policies affecting the propensity of that demographic group either to search for work

or for employers to hire that demographic group. We find small but imprecise declines in the probability of employment among young, non-college-educated black men (about 1.5 percentage points). Even if one takes our imprecisely estimated coefficient at face value (and assumes that the employment losses are entirely for individuals at the bottom of the wage distribution), the magnitude of the change in employment is unlikely to be driving a 9.5 percentage point increase in annual earnings. We also test whether black workers are sorting into or out of BTB jurisdictions after this policy is implemented. We find that young, non-college-educated black men are no more or less likely to work outside their local area of residence following the implementation of BTB in a neighboring jurisdiction. In sum, implementation of BTB leads to sizeable increases in earnings for young, non-college educated black men in neighboring jurisdictions but no discernible (aggregate) impact on their probability of employment or location of employment.

Again, we do not directly observe criminal history, so it is possible that there are differential effects (by criminal history) of BTB on selection into employment or location of employment that offset each other. For example, if after the implementation of BTB in a neighboring jurisdiction, individuals with a criminal history who were working in the non-BTB area now find jobs in the BTB area, and the opposite occurs for individuals without a criminal history.

These results contribute to the existing literature on BTB policies and suggest that researchers should be careful in selecting control groups when evaluating these policies. The evidence on the effects of BTB on same-jurisdiction labor market outcomes is mixed. Craigie (2019) finds that BTB policies lead to large increases in public-sector employment for individuals with a self-reported criminal record. Shoag and Veuger (2016) find that individuals living in historically high-crime neighborhoods also experience employment gains. Jackson and Zhao (2017) and Rose (2019) find null effects for employment. Agan and Starr (2018), using an audit study, find that the black-white gap in job application callback rates increases dramatically following the implementation of Ban the Box, which suggests that when employers cannot discriminate on the basis of criminal history, they resort to race-based statistical discrimination. Doleac and Hansen (2018) find that the probability of employment for young, non-college-educated black and Hispanic men declines following the implementation of BTB, implying that the results found in Agan and Starr (2018) translate into aggregate labor market effects for racial minorities.

This paper also contributes to two broader literatures. First, we contribute to the literature on the effects of limiting information available to employers (Holzer et al., 2006; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2019). We study how this information restriction could affect workers in neighboring jurisdictions in addition to directly affected workers. To the extent that these effects spill over into other areas, researchers should be careful in how they select control groups when studying such policies. Second, we also contribute to the literature on how search frictions impact wages and employment (Mortensen, 2005; Hornstein et al., 2011; Hagedorn and Manovskii, 2013; Card et al., 2016). Ban-the-box policies are intended to alleviate search frictions for workers with a criminal record. The existing evidence has not reached a consensus on whether these policies work as intended for directly covered workers, but our results imply that many more workers are potentially affected. Indeed, our results suggest that BTB successfully alleviates search frictions for at least one set of workers, and in the process reduces any monopsonistic (i.e. wage-setting) power of their employers.

We interpret our results to mean that the implementation of BTB credibly improves the outside options of potentially affected workers in neighboring jurisdictions. In the context of the equilibrium labor market search model with discrimination in Black (1995), if BTB works as intended then currently employed workers with a criminal record will face lower costs to search at potential new employers in the BTB jurisdiction. The model predicts that these reduced frictions translate into higher wages for potentially affected workers because firms would need to raise wages in order to retain these workers. Employers might anticipate the statistical discrimination found by Agan and Starr (2018) and Doleac and Hansen (2018) and not treat the implementation of BTB as a credible improvement in outside options with which they might have to compete. However, if firms in the BTB area view current employment in the non-BTB area as a sufficiently positive signal of employee productivity, that could outweigh the negative effect of race-based statistical discrimination. Then BTB represents an improvement in outside options that is credible enough that current employers would need to re-negotiate with workers in order to retain them.

Our results are robust to a variety of alternative treatment assignments and model specifications. In particular, the results are consistent when assigning treatment status based on place of work (as opposed to place of residence) and based on larger geographic areas (MSAs instead of MSA-state pairs). Results are

also robust to restricting to workers employed full-time and full-year (as opposed to full-year workers) and all employed workers. Our preferred specification includes MSA-state pair-specific linear time trends; however, when we remove these trends, our estimate of the earnings effect for black men is smaller (4.6 percent) but remains economically and statistically significant. The results for Hispanic and white men continue to show small and imprecise decreases in earnings.

## 2    Theoretical Framework

Ban-the-box policies are intended to make it easier for individuals with a criminal history to find a job by preventing employers from discriminating on the basis of criminal history in the initial job application process. To understand how such a policy change might impact labor market outcomes for these workers, we rely on the search model with employer discrimination in Black (1995). In this model, there are two types of employers: one with a distaste for some minority group of workers and one without. In the context of BTB, we consider this minority group to be workers with a criminal record.[1] Two types of workers, those with and without a criminal record, sequentially search for jobs. The expected gains from search for each type of worker are a function of the probability of matching with each type of employer. But because workers with a criminal record obtain no benefit from searching at discriminating firms, they face higher search costs than workers without a criminal record. The presence of these search costs lowers the reservation wage of workers with a criminal record and thus grants some monopsonistic, or wage-setting, power to the non-discriminating employer. In equilibrium, workers with a criminal record receive lower wages than those without a criminal record even at non-discriminating employers.

While ban-the-box policies do not force employers to hire workers with a criminal record, they are intended to improve the probability of matching with *all* employers by helping affected workers get past a first hiring screen. In the context of Black (1995), BTB induces search (by workers with a criminal history) at firms that had previously discriminated against them, thereby lowering these workers' search costs. The model predicts that these lower search costs would translate into higher wages by reducing the monopsony power of the non-discriminating firms. Now consider an employed worker with a criminal record working

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[1]There are of course other dimensions along which employers may discriminate, such as race and gender, but we abstract away from those for the time being.

in a non-BTB jurisdiction that borders a BTB jurisdiction. His reservation wage increases once BTB is implemented because he can now search for jobs in both the non-BTB and the BTB jurisdiction, and in the latter jurisdiction, potential employers cannot immediately know whether he has a criminal history.[2] His current employer (in the non-BTB jurisdiction) must then pay him a higher wage in order to retain him, assuming he is able to secure a competing offer, as in search models where employers are able to counter outside offers (Postel-Vinay and Robin, 2002; Cahuc et al., 2006).

One might object to this prediction by noting that some research finds that statistical discrimination increases following implementation of BTB (Agan and Starr, 2018; Doleac and Hansen, 2018). Employers might expect them to anticipate such discrimination and not consider the employers covered by BTB as a credible outside option for the worker. However, it could also be the case that outside employers view the fact that the worker is currently employed as a strong, positive signal of his productivity that overcomes their preference for discrimination. Then BTB would credibly improve the worker's outside options and his current employer would need to pay a higher wage in order to retain him.

## 3 Data and Methods

### 3.1 Data

We use data on employment and earnings from the American Community Survey (ACS) from 2005 to 2014 obtained from IPUMS (Ruggles et al., 2019). Data on BTB laws come from Table 1 of Doleac and Hansen (2018). Figure 2 maps the jurisdictions with ban-the-box policies in place during our sample period. Following Doleac and Hansen (2018), we restrict our attention to young (ages 25-34), non-college-educated, black, Hispanic, and white men. We are primarily interested in the spillover effects of BTB on earnings, but we also examine the effects on employment and the probability of working outside one's metropolitan statistical area (MSA)-state pair of residence (we discuss this geographic unit in more detail below). We primarily focus on earnings as opposed to employment because BTB lowers the barrier to entry to other jobs for workers in non-BTB jurisdictions. This potential increase in competition in the labor market is more

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[2]We use "he" as we restrict our analysis to the labor market effects of BTB on men, who disproportionately comprise the formerly incarcerated population. In 2017, only 7 percent of prisoners were women (Bronson and Carson, 2019).

likely to impact earnings.

Our primary specification uses annual earnings for full-year (worked at least 50 weeks) workers. We use earnings for full-year workers as opposed to full-time, full-year workers in case one of the margins of response by employers is to adjust workers' hours in a way that moves them from part-time to full-time work, or vice versa. As robustness checks, we use annual earnings for full-time (worked at least 35 hours per week) full-year workers and annual earnings for all workers, and our results are consistent.

We classify an MSA-state pair as subject to a BTB policy if at any point in the year a policy is effective in that area. Of the 202 MSA-state pairs in our sample, 78 (39 percent) are ever treated and 124 are never treated. We assign treatment status based on the MSA-state pair rather than the MSA to obtain a more accurate measure of treatment. Figure 3 provides an illustration of our treatment assignment. When the city of Chicago implemented a BTB law in 2007, the Indiana and Wisconsin portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA became treated, even though they are in the Chicago MSA. The Indiana and Wisconsin portions of the Chicago MSA were not subject to BTB. We run an alternative specification where we assign treatment based on the MSA of work rather than residence (in our example, the Indiana and Wisconsin portions of the Chicago MSA would be considered as subject to BTB) and the results are the same.

We classify an MSA-state pair as belonging to the control group if, during our sample period, that MSA-state pair does not have a ban-the-box policy in place and it also does not border any other MSA-state pair with a ban-the-box policy in place. The Birmingham-Hoover, AL MSA-state pair is an example of an untreated MSA-state pair (see Figure 2). The Birmingham-Hoover, AL MSA-state pair borders four other MSA-state pairs, and none of those five MSA-state pairs (Birminham and the four bordering it) were subject to a ban-the-box policy during our sample period. We exclude from our analysis any MSA-state pair that implements a ban-the-box policy during our sample period.

As shown in the summary statistics in Table 1, 10.9 percent of young, non-college-educated black men in our sample live in an MSA-state pair that borders an MSA-state pair with a BTB policy. Black men are slightly less likely to live in a BTB-adjacent MSA-state pair than Hispanic (14.3 percent) and white (12.9 percent) men.

The implementation of BTB in a border MSA-state pair can only be a credible improvement in outside options if individuals can and will realistically travel to a border MSA-state pair for work. While most people in our sample work and live in the same MSA-state pair, 15.9 percent of black men, 10.8 percent of Hispanic men, and 17.9 percent of white men work outside their MSA-state pair of residence (Table 1). Though small, we think these percentages are sufficiently large to represent the credibility of work opportunities in a border MSA-state pair as a viable outside option. These observed percentages (in the ACS) of workers who work outside their MSA-state pair are consistent with evidence from job-search websites. On Indeed.com, 27.8 percent of individuals living in the U.S. search for jobs in another state (Indeed Hiring Lab, 2014). Using data from CareerBuilder.com, Marinescu and Rathelot (2018) find that 11 percent of job applications are sent to jobs out of state.

## 3.2 Difference-in-Differences Identification and Assumptions

We estimate a difference-in-differences model in which an indicator for bordering an MSA-state pair with a BTB policy is the treatment variable. The control group consists of MSA-state pairs that neither have BTB nor border an MSA-state pair with a BTB policy. We exclude all MSA-state pairs with BTB from our analysis. There are two assumptions that must be satisfied in order for the difference-in-differences method to estimate a causal effect of BTB laws on employment and earnings in bordering jurisdictions. First, at the time of implementation of BTB laws, there are no other changes occurring in the treated jurisdictions (places that border BTB-affected areas) that affect employment and earnings, conditional on the control variables. Second, in the absence of the treatment (i.e. a BTB law becoming effective in a neighboring jurisdiction), trends in employment and earnings would be the same, conditional on the control variables, across the treatment and control jurisdictions.

## 3.3 Reduced-Form Earnings Equation

Our difference-in-differences model is a cluster-robust ordinary least squares model with MSA-state-specific linear time trends. The main specification is Equation 1. We run each regression separately by race (black, Hispanic, and white), and our sample is restricted to employed men between the ages of 25 and 34

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with no college degree.

$$ln(earnings)_{ijt} = \alpha + \beta \cdot borderBTB_{ijt} + \gamma_j + \delta_j \cdot t + FEs + \varepsilon_{ijt} \tag{1}$$

Our main outcome, $ln(earnings)_{ijt}$, represents the log of annual earnings for a full-year (worked at least 50 weeks) worker $i$ in MSA-state $j$ in year $t$. $borderBTB_{ijt}$ represents an indicator for whether individual $i$ lives in an MSA-state pair ($j$) that borders an MSA-state with a BTB policy in time $t$. $\gamma_j$ represent MSA-state-pair fixed effects. $\delta_j \cdot t$ represent MSA-state-pair-specific linear time trends. We include fixed effects for whether $i$ is currently in school, age, years of education, and Census division interacted with time. Note that while we do not explicitly include a term for year fixed effects, year fixed effects are perfectly collinear with the Census division interacted with time fixed effects (because our unit of time is a year). $\varepsilon_{ijt}$ represent the standard errors, which are clustered at the MSA-state pair level.

We also estimate the spillover effects of BTB on employment and the probability of working outside an individual's MSA-state of residence. For those specifications, we run a linear probability model with the same controls as in equation 1. Our dependent variables are $Pr(employment)$ or $Pr(work\ outside\ MSA)$.

## 3.4 Testing for Parallel Pre-Trends in Earnings

We conduct event studies to check for parallel pre-trends in annual earnings. The event studies are a non-parametric version of equation 1:

$$ln(earnings)_{ijt} = \alpha + \sum_{k=-5, k \neq -1}^{k=5} \beta_k \cdot borderBTB_{ijkt} + \gamma_j \cdot t + FEs + \varepsilon_{ijt} \tag{2}$$

$k$ refers to the number of years that the border BTB has been effective, with negative values indicating the number of years prior to adoption. We have omitted the year prior to a border BTB adoption, $t = -1$, which makes it the reference period.

Figure 4 plots the log of annual earnings relative to the year before ban the box is implemented in a neighboring MSA-state pair for young, non-college-educated black men, conditional on the controls. As seen in Figure 4, the coefficients are small and not individually statistically significantly different from zero. The

event study of annual earnings for Hispanic men is shown in Figure 5. Here the coefficients in the pre-period are larger but there does not appear to be a clear pre-trend and they are not individually statistically significantly different from zero. Figure 6 shows the event study for white men's annual earnings. Similar to Hispanic men, there is not a clear pre-trend, although one of the coefficients is statistically different from zero.

## 4    Results

Table 2 shows the results for the effect on annual earnings of living next to an MSA-state pair with BTB. After the implementation of BTB in a neighboring MSA-state, annual earnings for young, non-college-educated black men increase by 9.5 percent. This effect is statistically significant at the 1 percent level. From the event study (Figure 4), this increase in earnings is consistent in the five years after the implementation of BTB in a neighboring MSA-state, and the effect grows slightly over time.

In contrast, annual earnings for young, non-college-educated Hispanic men are flat (-0.9 percent). This null effect is imprecise, with a 95 percent confidence interval ranging from -5.3 percent to 3.5 percent. The event study (Figure 5) shows a general decline in annual earnings after ban the box is implemented in a neighboring MSA-state, although there is an anomalous uptick in earnings four years after the BTB effective date. Overall, we interpret the event study coefficients as noisy fluctuations around zero.

For young, non-college-educated white men, annual earnings are flat (coefficient rounds to 0.0 percent), and we can rule out changes in annual earnings of more than 2 percent at the 5 percent significance level. The dynamics shown in the event study (Figure 6) support this interpretation.

Given the findings of statistical discrimination in Agan and Starr (2018) and Doleac and Hansen (2018), this increase in annual earnings for young, non-college-educated black men might be unexpected. To test whether the increase in annual earnings is due to compositional effects (e.g. an increase in unemployment for black men at the bottom of the earnings distribution mechanically driving up average earnings), we examine the effects on employment and the probability of working outside one's MSA-state pair of residence.

Turning to employment effects, we find small but imprecisely estimated effects of BTB on the probability of employment for young, non-college-educated black men (Table 3). The probability of employment declines

by 1.5 percentage point (p.p.), or 1.9 percent, for black men, but this effect is not statistically significantly different from zero. Similarly, for young, non-college-educated Hispanic and white men, we cannot reject the null hypothesis of no effect of a neighboring BTB on employment, and the coefficients are close to zero (-0.3 p.p. and -0.8 p.p., or -0.3 percent and -0.9 percent, respectively). It is possible that the small yet imprecise decline in employment for black men is driving the increase in earnings but we do not think that explanation is plausible given the relative magnitudes of each effect.

Our results for the probability of working outside one's MSA-state pair of residence are shown in Table 4. For young, non-college-educated black men, we are unable to reject the null hypothesis of no effect of BTB in a neighboring MSA-state pair on the probability of working outside one's MSA-state of residence. In addition, the coefficient is quite small (-0.3 p.p. or 2.7 percent of the mean). For young, non-college-educated Hispanic and white men, we do see some evidence that BTB in a bordering MSA-state pair leads to a lower likelihood of an individual working outside his MSA-state pair of residence. Young, non-college-educated Hispanic men are 2.7 p.p. (31 percent) less likely to work outside their MSA-state of residence after a neighboring MSA-state implements BTB. This effect is statistically significant at the 5 percent level. Young, non-college-educated white men are 2.1 p.p. (12.7 percent) less likely to work outside their MSA-state of residence. This effect is marginally statistically significant. The null effect for black men suggests that changes in the location of employment are not driving the increase in annual earnings. This finding complements that of Doleac and Hansen (2017), who find that the demographic composition of MSAs covered by BTB does not change following its implementation. Thus it appears that BTB does not induce geographic sorting across MSAs.

## 5    Robustness Checks

### 5.1    Annual Earnings of Full-Time Full-Year Workers

To test the sensitivity of our results to our definition of annual earnings, we restrict our sample further to full-time (worked at least 35 hours per week) full-year workers. Our results are shown in Table 5. For young, non-college-educated black men, annual earnings for full-time full-year workers increase by 9.9 percent

following the implementation of BTB in a neighboring MSA-state pair. This effect is statistically significant at the 1 percent level and is very similar to the result for full-year workers (9.5 percent). For young, non-college-educated Hispanic men, we cannot reject the null hypothesis of no effect of BTB in a neighboring MSA-state on annual earnings of full-time full-year workers. The coefficient is also close to zero (-0.9 percent) and is the same magnitude as that for full-year workers. For young, non-college-educated white men, we are also unable to reject the null hypothesis of no effect on annual earnings for full-time full-year workers, and the coefficient is small (1.4 percent). In terms of magnitude, it is larger than the result for full-year workers but these coefficients are not statistically different from each other.

## 5.2 Annual Earnings of All Workers

We also estimate the effects of BTB in a neighboring MSA-state on annual earnings for all workers (Table 6). Our results are in line with those for full-year and full-time full-year workers. For young, non-college-educated black men, the implementation of a BTB in a bordering MSA-state leads to a 7.2 percent increase in annual earnings for all workers. This effect is marginally statistically significant and is similar to but attenuated relative to our main results (for full-year workers). For young, non-college-educated Hispanic men, we cannot reject the null hypothesis of no effect of a bordering MSA-state BTB policy on annual earnings of all workers. The coefficient is relatively small (1.3 percent) and the opposite sign, though not statistically significantly different from our main (null) result for Hispanic men. For young, non-college-educated white men, we are also unable to reject the null hypothesis of no effect on annual earnings. The coefficient is small (-0.9 percent) and not statistically significantly different than our main result for white men. Overall, the results for all workers are consistent with our main results: annual earnings increase for young, non-college-educated black men but are flat for Hispanic and white men.

## 5.3 Assigning Treatment Based on MSA of Residence

While we prefer to assign treatment based on the MSA-state pair of residence, it is possible that the BTB status of the entire MSA is more relevant. This would be the case if many individuals commute across state lines within the same MSA for work. When we redefine treatment based on MSA of residence our results

are quantitatively similar to our main specification, where treatment is based on MSA-state pair of residence (Table 7). For young, non-college-educated black men, a bordering MSA BTB leads to a 6.3 percent increase in annual earnings (statistically significant at the 5 percent level). For young, non-college-educated Hispanic and white men, we fail to reject the null hypothesis of no effect of a border-MSA BTB on annual earnings. The coefficients for Hispanic and white men are also small (-0.7 percent for both). The estimate for black men is roughly two-thirds as large as our main results, but the interpretation is the same: BTB in neighboring MSAs leads to increases in annual earnings for black men.

## 5.4  Assigning Treatment Based on MSA-State of Work

As discussed in section 5.3, what is relevant for our analysis is the BTB policy where an individual works. Up until now, we have implicitly been instrumenting for the BTB status of one's place of work with the BTB status of one's place of residence. Instrumenting for the BTB status of one's place of work is the appropriate setup if changes in an individual's geographic location of work are correlated with both BTB status and annual earnings, which would violate the parallel trends assumption of the difference-in-differences empirical strategy (because then the control group would not be a valid counterfactual for the treatment group).

Given that we find some evidence that young, non-college-educated Hispanic and white men are less likely to work outside their MSA-state pair of residence after a BTB policy is implemented in a bordering MSA-state pair, we believe we are correct in instrumenting for the BTB status of the place of work. However, because we cannot reject the null hypothesis of no effect of a border BTB on black men's probability of working outside their MSA-state of residence, we also estimate a model where we assign treatment based on the MSA-state pair of work.

The specification where we assign treatment based on the MSA-state pair of work (Table 8) yields results that are almost identical to our main specification. The implementation of BTB in a neighboring MSA-state pair leads to a 9.1 percent increase in annual earnings for young, non-college-educated black men. This effect is statistically significant at the 1 percent level. In contrast, we are unable to reject the null hypothesis of no effect of a border-BTB policy on annual earnings for young, non-college-educated Hispanic and white men, and the point estimates are close to zero (0.2 percent and -0.7 percent, respectively). While the results for

Hispanic and white men should be interpreted with caution given the potential endogeneity issue, all three of these results are consistent with our main specification.

## 5.5   No MSA-State-Specific Linear Time Trends

When we remove the MSA-state-specific linear time trends, we obtain results that are smaller but in line with our main results (Table 9). For young, non-college-educated black men, the implementation of BTB in a bordering MSA-state pair leads to a 4.6 percent increase in annual earnings (statistically significant at the 10 percent level). We cannot reject the null hypothesis of no effect of a border BTB on annual earnings for young, non-college-educated Hispanic and white men. These coefficients are also close to zero (-1.0 percent and -0.7 percent). While the results for black men are attenuated, they are still consistent with our main results.

## 6   Discussion

Reintegrating individuals with a criminal history into society has thus far proven difficult to accomplish: sixty-eight percent of individuals who are released from prison will recidivate within three years of their release (Alper et al., 2018). If some of these individuals are recidivating due to a lack of job opportunities in the formal sector, then any policy that can improve labor market outcomes for the formerly incarcerated might reduce recidivism. Additionally, any policy that can successfully improve labor market outcomes for individuals with a criminal history might also reduce racial disparities in labor market outcomes, as black men are incarcerated at 5.7 times the rate of white men and Hispanic men are incarcerated at 3.2 times the rate of white men (Bronson and Carson, 2019). However, these policies could potentially also have unintended consequences for other vulnerable populations. Previous work finds that when information about job applicants that employers want to know is hidden, employers resort to statistical discrimination based on observable characteristics that employers perceive to be correlated with the unobservable characteristic (Holzer et al., 2006; Autor and Scarborough, 2008; Wozniak, 2015; Bartik and Nelson, 2019).

Ban-the-box is a prominent example of a policy intended to help improve labor market conditions for the formerly incarcerated. While the existing literature studies the direct effects of BTB on jurisdictions

in which it is implemented, we extend the literature to look at how these policies may have effects that spill over into neighboring jurisdictions. We find that annual earnings increase by 9.5 percent for young, non-college educated black men living in jurisdictions neighboring those with BTB. In contrast, earnings are flat for similarly situated Hispanic and white men. This result does not appear to be driven by changes in the composition of these workers: we find small but imprecise declines in the probability of employment for black men but no change in the probability of working outside of their MSA-state of residence. These findings are consistent with BTB creating a credible outside option for black workers in BTB-adjacent areas. They also help to complete the picture of the winners and losers created by ban-the-box policies.

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# 8 Figures and Tables

Figure 1: Criminal Conviction Questions on Job Applications

Have you ever been convicted of a felony?  ☐ Yes  ☐ No

Date(s) / Nature of Offense(s): ⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯

Have you ever been convicted of a misdemeanor involving weapons, theft, dishonesty, or violence?  ☐ Yes  ☐ No

Date(s) / Nature of Offense(s) / Sentence Imposed: ⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯⎯

Source: job-applications.com

Figure 2: Jurisdictions with ban-the-box policies by December 2014
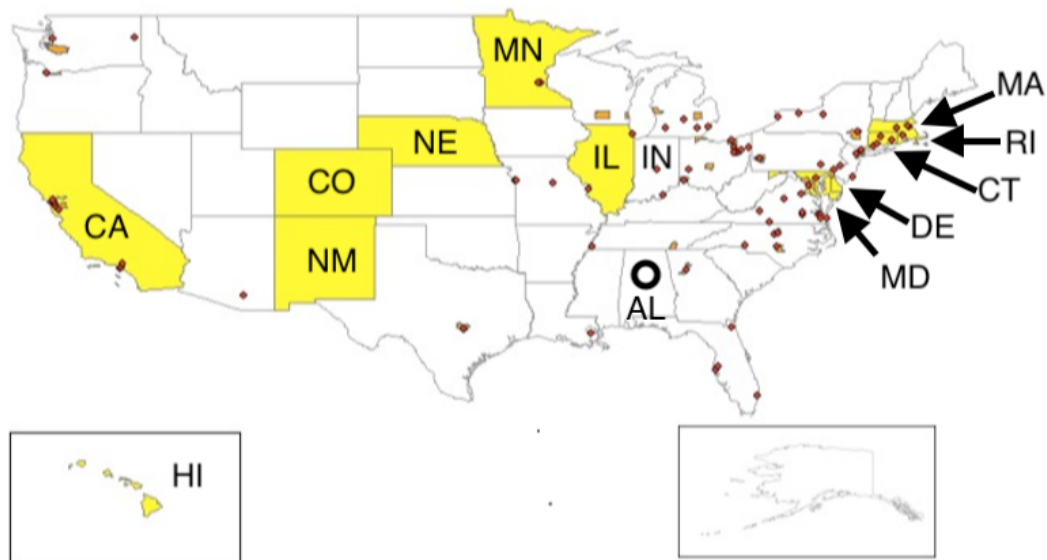


Jurisdictions with BTB policies are represented by yellow shading (state-level policies), orange shading (county-level policies), and red dots (city-level policies.)

Note: The shaded-in jurisdictions represent jurisdictions with any ban-the-box policy in place by Dcember 2014 (the end of our sample period). The hollow black circle in Alabama approximates the Birmingham-Hoover, AL MSA, which is an example of an untreated observational unit.
Source: Doleac and Hansen (2018) with labels and the hollow circle added by us

Figure 3: Map of Chicago-Naperville-Elgin, IL-IN-WI MSA
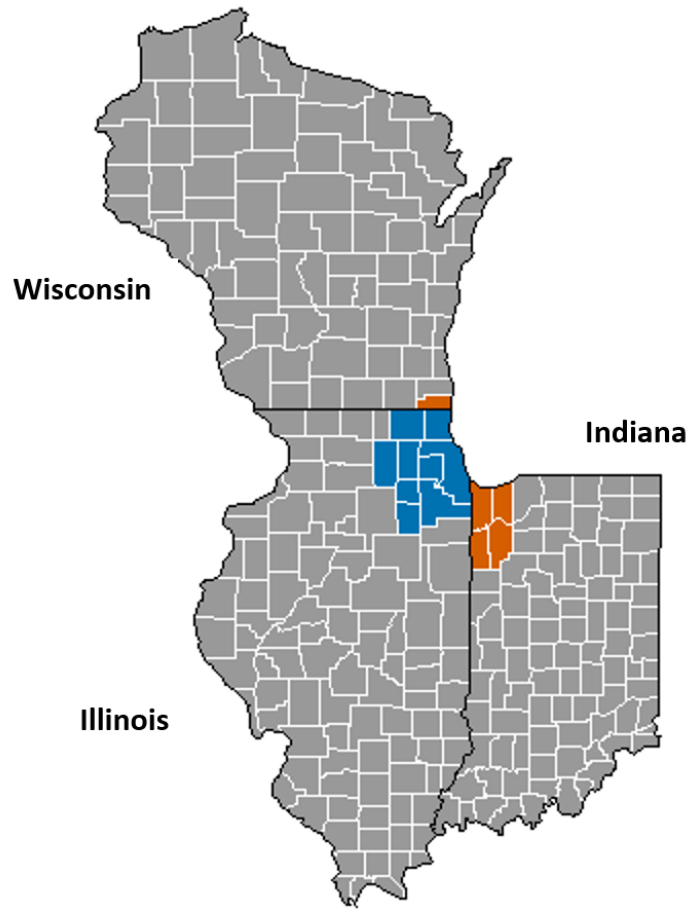


Note: Each region shaded in gray and outlined in white represents a county. The black lines delineate the state borders. The blue shaded counties represent the Illinois portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA (which are excluded from our primary specification as the city of Chicago implemented a ban-the-box policy during our sample period). The orange shaded counties represent the Wisconsin and Indiana portions of the Chicago-Naperville-Elgin, IL-IN-WI MSA (which are part of our treatment group, as they border an MSA-state pair with a BTB policy but do not have their own BTB policy during the sample period).

Figure 4: Young, Non-College-Educated Black Men



Event Study: Log of Annual Earnings (Full-Year Workers)

Figure 5: Young, Non-College-Educated Hispanic Men
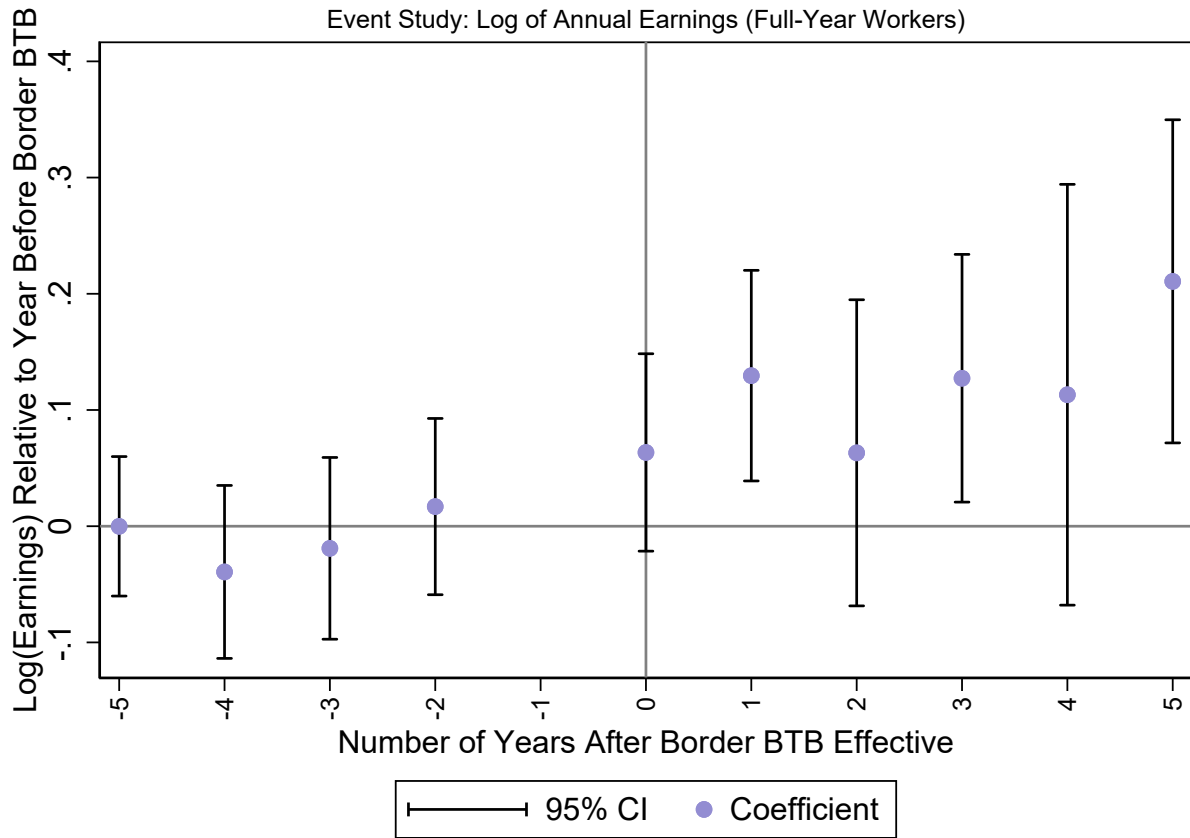
Event Study: Log of Annual Earnings (Full-Year Workers)
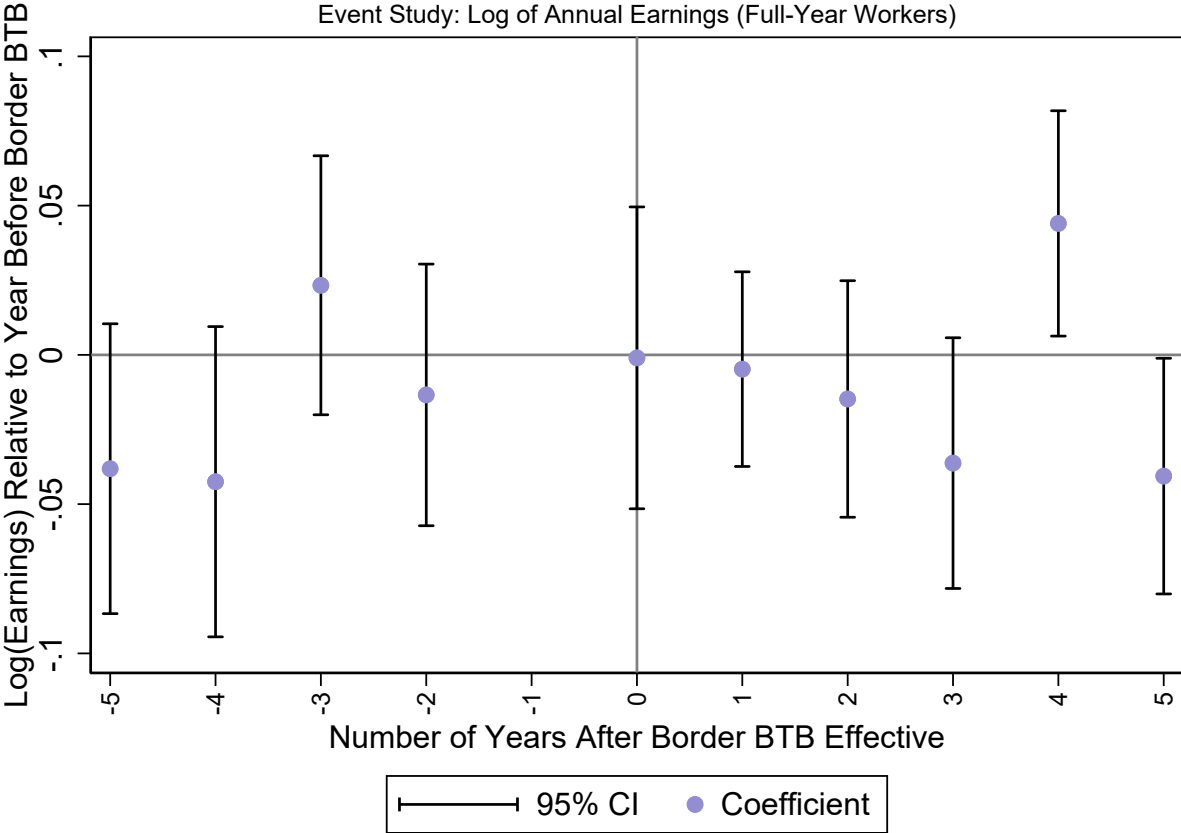
Figure 6: Young, Non-College-Educated White Men
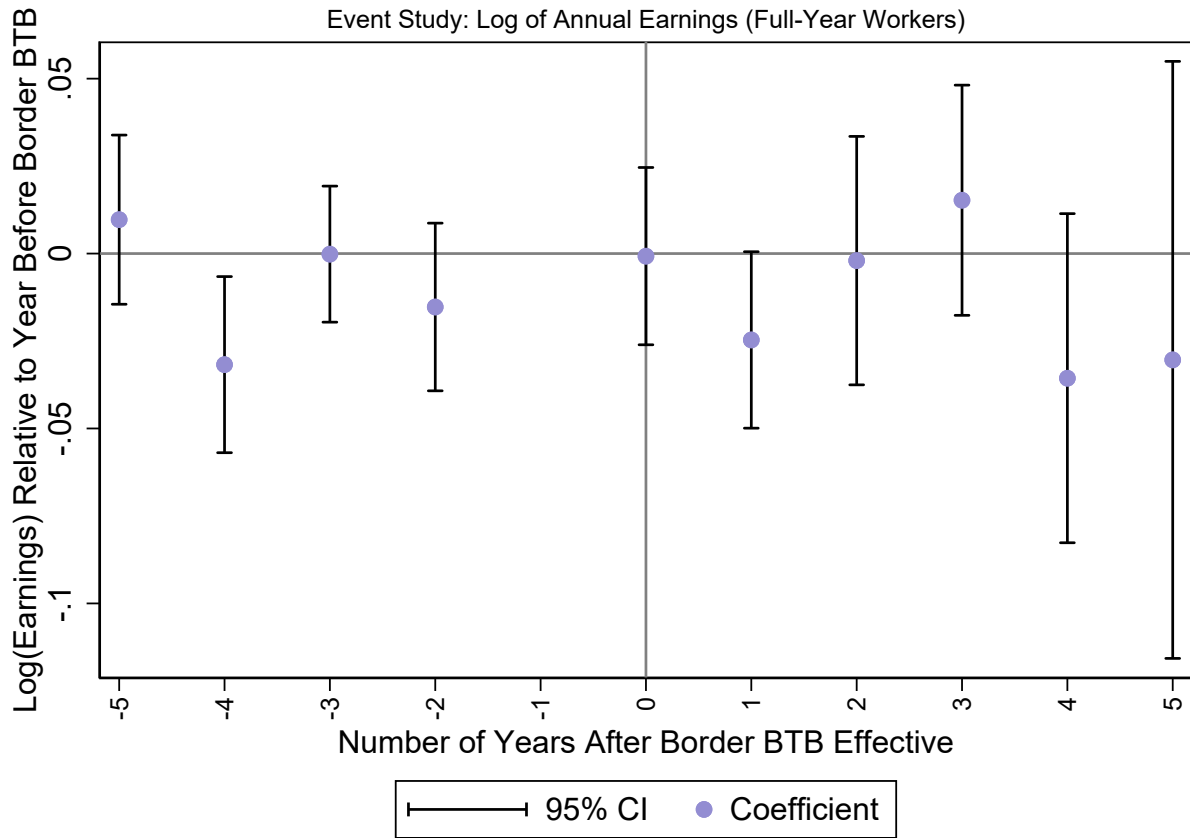
Table 1: Summary Statistics

|  | Black | SD | Hispanic | SD | White | SD |
|---|---|---|---|---|---|---|
| Border MSA-State BTB | 0.109 | 0.311 | 0.143 | 0.350 | 0.129 | 0.335 |
| Employed | 0.782 | 0.413 | 0.899 | 0.301 | 0.892 | 0.311 |
| Work Outside MSA-State Pair* | 0.159 | 0.366 | 0.108 | 0.310 | 0.179 | 0.383 |
| Annual Earnings ($)* | 29,457 | 20,664 | 29,759 | 19,813 | 38,487 | 25,811 |
| Work Full Year | 0.647 | 0.478 | 0.748 | 0.434 | 0.750 | 0.433 |
| Work Full-Time Full Year | 0.573 | 0.495 | 0.698 | 0.459 | 0.697 | 0.459 |
| Age | 29.36 | 2.903 | 29.421 | 2.875 | 29.411 | 2.886 |
| In School | 0.101 | 0.302 | 0.057 | 0.231 | 0.090 | 0.287 |
| High School Degree | 0.444 | 0.497 | 0.366 | 0.482 | 0.445 | 0.497 |
| Some College, No Degree | 0.404 | 0.491 | 0.236 | 0.425 | 0.448 | 0.497 |
| $N$ | 26,226 |  | 60,879 |  | 143,186 |  |
| $N$ (Full-Year Workers) | 16,966 |  | 45,546 |  | 107,438 |  |

Note: *conditional on working full year (at least 50 weeks). Sample restricted to black, Hispanic, and white men between the ages of 25 and 34 with no college degree. Data Source: American Community Survey, 2005-2014.

Table 2: Effect of Border BTB on Annual Earnings for Full-Year Workers

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Black | Hispanic | White |
| Border BTB | 0.095*** | -0.009 | 0.000 |
|  | (0.035) | (0.022) | (0.010) |
| MSA-Time Trends | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.096 | 0.117 | 0.104 |
| Dep Var Mean | 10.12 | 10.15 | 10.40 |
| $N$ | 16,039 | 44,534 | 105,702 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed for the full year (at least 50 weeks). Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.

Table 3: Effect of Border BTB on Employment

|                            | (1)      | (2)       | (3)      |
|                            | Black    | Hispanic  | White    |
|----------------------------|----------|-----------|----------|
| Border BTB                 | -0.015   | -0.003    | -0.008   |
|                            | (0.020)  | (0.008)   | (0.006)  |
| MSA-Time Trends            | Yes      | Yes       | Yes      |
| Age FE                     | Yes      | Yes       | Yes      |
| In-School FE               | Yes      | Yes       | Yes      |
| Yrs. of Edu. FE            | Yes      | Yes       | Yes      |
| Census Division x Time FE  | Yes      | Yes       | Yes      |
| $R^2$                      | 0.101    | 0.037     | 0.035    |
| Dep Var Mean               | 0.782    | 0.899     | 0.892    |
| $N$                        | 26,220   | 60,878    | 143,186  |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34. Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.

Table 4: Effect of Border BTB on Probability of Working Outside MSA for Full-Year Workers

|                            | (1)      | (2)       | (3)      |
|                            | Black    | Hispanic  | White    |
|----------------------------|----------|-----------|----------|
| Border BTB                 | -0.003   | -0.027**  | -0.021*  |
|                            | (0.021)  | (0.011)   | (0.012)  |
| MSA-Time Trends            | Yes      | Yes       | Yes      |
| Age FE                     | Yes      | Yes       | Yes      |
| In-School FE               | Yes      | Yes       | Yes      |
| Yrs. of Edu. FE            | Yes      | Yes       | Yes      |
| Census Division x Time FE  | Yes      | Yes       | Yes      |
| $R^2$                      | 0.220    | 0.149     | 0.203    |
| Dep Var Mean               | 0.112    | 0.087     | 0.165    |
| $N$                        | 16,039   | 44,534    | 105,702  |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed for the full year (at least 50 weeks). Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.

Table 5: Effect of Border BTB on Annual Earnings for Full-Time Full-Year Workers

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Black | Hispanic | White |
| Border BTB | 0.099*** | -0.009 | 0.014 |
|  | (0.026) | (0.020) | (0.009) |
| MSA-Time Trends | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.094 | 0.132 | 0.106 |
| Dep Var Mean | 10.21 | 10.19 | 10.46 |
| $N$ | 14,304 | 41,620 | 98,363 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed full-time for the full year (at least 50 weeks for at least 35 hours per week). Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.

Table 6: Effect of Border BTB on Annual Earnings for All Workers

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Black | Hispanic | White |
| Border BTB | 0.072* | 0.013 | -0.009 |
|  | (0.038) | (0.026) | (0.014) |
| MSA-Time Trends | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.089 | 0.090 | 0.092 |
| Dep Var Mean | 9.89 | 10.00 | 10.24 |
| $N$ | 20,513 | 54,728 | 127,681 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed. Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.

Table 7: Effect of Border BTB on Annual Earnings for Full-Year Workers (Treatment Assigned by MSA)

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Black | Hispanic | White |
| Border BTB | 0.063** | -0.007 | -0.007 |
|  | (0.032) | (0.022) | (0.011) |
| MSA-Time Trends | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.094 | 0.115 | 0.103 |
| Dep Var Mean | 10.12 | 10.15 | 10.39 |
| $N$ | 15,259 | 43,551 | 97,898 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed for the full year (at least 50 weeks). Treatment group lives next to an MSA with BTB but does not have BTB in their own MSA. Control group neither borders a BTB MSA nor lives in a BTB MSA.

Table 8: Effect of Border BTB on Annual Earnings for Full-Year Workers (Treatment Assigned by MSA-State of Work)

|  | (1) | (2) | (3) |
|---|---|---|---|
|  | Black | Hispanic | White |
| Border BTB | 0.091*** | 0.002 | -0.007 |
|  | (0.034) | (0.023) | (0.010) |
| MSA-Time Trends | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.098 | 0.120 | 0.102 |
| Dep Var Mean | 10.15 | 10.15 | 10.40 |
| $N$ | 15,691 | 43,956 | 105,598 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed for the full year (at least 50 weeks). Treatment group works next to an MSA-state with BTB but does not have BTB in their own MSA-state of work. Control group neither borders a BTB MSA-state nor works in a BTB MSA-state.

Table 9: Effect of Border BTB on Annual Earnings for Full-Year Workers (Two-Way FE's)

|  | (1) Black | (2) Hispanic | (3) White |
|---|---|---|---|
| Border BTB | 0.046* | -0.010 | -0.007 |
|  | (0.027) | (0.017) | (0.011) |
| MSA-Time Trends | No | No | No |
| MSA-State FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |
| Age FE | Yes | Yes | Yes |
| In-School FE | Yes | Yes | Yes |
| Yrs. of Edu. FE | Yes | Yes | Yes |
| Census Division x Time FE | Yes | Yes | Yes |
| $R^2$ | 0.085 | 0.112 | 0.100 |
| Dep Var Mean | 10.12 | 10.15 | 10.40 |
| $N$ | 16,039 | 44,534 | 105,702 |

Note: Standard errors clustered at the MSA-state level. Sample restricted to black, Hispanic, or white men with less than a college degree between the ages of 25 and 34 who are employed for the full year (at least 50 weeks). Treatment group lives next to an MSA-state with BTB but does not have BTB in their own MSA-state. Control group neither borders a BTB MSA-state nor lives in a BTB MSA-state.