

The Employment Effects of Ban-the-Box Preemption: Evidence from Indiana’s Senate Bill 312

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Abstract

In 2017, Indiana became the first U.S. state to preempt local “ban-the-box” (BTB) ordinances, prohibiting local governments from restricting employers’ ability to inquire about criminal history during hiring. Using a difference-in-differences design comparing Indiana to neighboring Midwestern states with Census PUMS microdata from 2014 through 2019, comprising 1.48 million working-age observations, I examine whether this policy reversal affected labor market outcomes. The main estimate suggests Indiana’s employment growth lagged control states by 0.50 percentage points after preemption. However, this effect is not concentrated among demographic groups most likely affected by criminal record screening. Young Black males with less than an associate’s degree—a proxy for high criminal record exposure based on documented disparities in criminal justice contact—show essentially no differential effect at +0.04 percentage points. Furthermore, the placebo group of college-educated workers exhibits a similar-magnitude effect, raising concerns about identification. These null findings suggest that BTB preemption had limited labor market impact, potentially because the preempted Indianapolis ordinance had narrow scope covering only city contractors and public sector workers, or because private employers already extensively used criminal background checks regardless of local BTB policies. This paper provides the first empirical evidence on what happens when ban-the-box protections are removed, contributing to ongoing policy debates about the effectiveness and appropriate scope of criminal record screening regulations.

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1. Introduction

The intersection of criminal justice and labor markets has attracted substantial scholarly and policy attention over the past two decades. With an estimated 70 to 100 million Americans possessing some form of criminal record, and with persistent racial disparities in criminal justice contact, policies governing how employers use criminal history information have significant implications for economic opportunity and inequality. Ban-the-box (BTB) policies, which delay employer inquiries about criminal history until later in the hiring process, represent one of the most widely adopted policy responses to these concerns. By 2020, over 35 states and 150 local jurisdictions had adopted some form of BTB policy, reflecting bipartisan support for “second chance” employment policies.

The existing empirical literature has extensively studied the effects of BTB adoption on labor market outcomes. The landmark study by Doleac and Hansen (2020) examines the unintended consequences of hiding criminal history information, finding that BTB policies reduced employment for young Black and Hispanic men without college degrees by 3 to 5 percentage points. They interpret this finding through the lens of statistical discrimination: when employers cannot observe individual criminal histories, they rely more heavily on demographic characteristics correlated with criminal records as screening proxies. In contrast, Agan and Starr (2018) use a correspondence audit methodology and find that BTB increased callback rates for applicants with criminal records while narrowing the racial gap in callbacks. These conflicting findings have generated substantial academic debate about the net welfare effects of BTB policies and the mechanisms through which they operate.

This paper contributes to the BTB literature by examining an understudied policy variation: what happens when ban-the-box protections are *removed*. In 2017, Indiana became the first state in the United States to preempt local BTB ordinances through Senate Bill 312. This legislation prohibited local governments from restricting employers’ ability to inquire about criminal history during hiring, effectively nullifying Indianapolis’s 2014 BTB ordinance and preventing other Indiana localities from adopting similar policies. The Indiana preemption provides a unique natural experiment to study the labor market effects of reversing BTB protections—a policy direction that has not been previously examined in the literature.

I employ a difference-in-differences research design comparing employment outcomes in Indiana to those in neighboring Midwestern states—Ohio, Michigan, and Illinois—before and after the July 2017 preemption. Using American Community Survey Public Use Microdata Sample (PUMS) records from 2014 through 2019, comprising 1.48 million working-age observations, I estimate the causal effect of BTB preemption on employment rates. The

analysis focuses on working-age adults between 18 and 64 years old, with particular attention to demographic subgroups that theory suggests should be most affected by criminal record screening policies.

The main findings can be summarized as follows. First, the aggregate difference-in-differences estimate suggests that Indiana’s employment growth lagged control states by approximately 0.50 percentage points following the preemption. Given baseline employment rates of approximately 72 percent, this represents a modest 0.7 percent relative decline. Second, and more notably, this aggregate effect is not concentrated among demographic groups most likely to be affected by criminal record screening. Young Black males with less than an associate’s degree—a commonly used proxy for high criminal record exposure given documented disparities in criminal justice contact—show essentially no differential effect at +0.04 percentage points. Third, the placebo group of college-educated workers, who should not be differentially affected by criminal record screening policies, exhibits a similar-magnitude negative effect of -0.95 percentage points. This pattern raises concerns about whether the estimated effects reflect the BTB preemption specifically or other factors differentially affecting Indiana during this period.

These findings contribute to the policy debate in several ways. Most directly, they suggest that BTB preemption—at least in contexts where the preempted policy had narrow scope—may have limited aggregate labor market consequences. This is potentially reassuring for policymakers concerned that preemption would substantially harm workers with criminal records, but it also suggests that narrowly-scoped BTB policies may have limited benefits in the first place. More broadly, the null findings raise questions about the mechanisms through which BTB policies affect labor markets and the conditions under which they are likely to be effective.

The remainder of this paper proceeds as follows. Section 2 provides institutional background on ban-the-box policies and Indiana’s preemption legislation. Section 3 reviews the related literature on criminal records and employment. Section 4 develops the conceptual framework and derives testable predictions. Section 5 describes the data and sample construction. Section 6 presents the empirical strategy and discusses threats to identification. Section 7 reports the main results and heterogeneity analysis. Section 8 discusses the findings and their implications. Section 9 concludes.

2. Institutional Background

2.1 Ban-the-Box Policies in the United States

Ban-the-box policies derive their name from the checkbox on job applications asking “Have you ever been convicted of a felony?” or similar criminal history questions. The policy’s core objective is to delay employer access to criminal history information until later in the hiring process—typically after an initial interview or the extension of a conditional job offer. Proponents argue that this delay gives applicants with criminal records an opportunity to demonstrate their qualifications and make a positive impression before being screened out based on their record. The underlying theory is that personal interaction reduces employer reliance on stereotypes and increases the probability that qualified applicants with records will be seriously considered.

The BTB policy movement began in Hawaii in 1998, which became the first state to restrict when private employers could inquire about criminal history. However, the policy gained significant momentum only after 2010, with rapid adoption by cities, counties, and states across the country. By 2020, thirty-seven states and over 150 cities and counties had adopted some form of BTB policy. These policies vary substantially in their scope and stringency across several dimensions.

First, policies differ in coverage. Some BTB laws apply only to public sector employment within the enacting jurisdiction. Others extend to private employers, typically with size thresholds exempting small businesses. For example, California’s BTB law applies to private employers with five or more employees, while Illinois exempts employers with fewer than fifteen workers. Second, policies vary in timing. Some require delaying criminal history questions until after an initial interview, while others delay inquiry until after a conditional job offer has been extended. Third, enforcement mechanisms range from complaint-based civil penalties to no formal enforcement at all. The heterogeneity in BTB implementation complicates efforts to estimate aggregate policy effects, as different policy designs may operate through different mechanisms.

Despite this heterogeneity, most BTB policies share a common structure: they prohibit employers from asking about criminal history on the initial job application and specify a later point in the hiring process when such inquiries are permitted. This structure reflects a balance between the interests of applicants with criminal records and employers’ legitimate interests in workforce safety and security.

2.2 The Indianapolis Ban-the-Box Ordinance

In December 2014, the Indianapolis City-County Council adopted a ban-the-box ordinance affecting employment within Marion County, the consolidated city-county that encompasses Indianapolis. The ordinance applied to two categories of employers: city government directly and private contractors providing services to the city. Public employees and employees of city contractors were prohibited from being asked about criminal history until after the applicant had been selected for an interview or had received a conditional offer of employment.

The scope of the Indianapolis ordinance was relatively narrow compared to comprehensive BTB policies that cover all private employers above a size threshold. Estimates suggest that the ordinance affected approximately 20,000 to 30,000 workers in city government and city contracting positions. This represents a small fraction of total employment in the Indianapolis metropolitan area, which exceeds one million workers. The narrow scope is important for interpreting the effects of preemption: reversing a policy that affected only a modest share of the labor market would be expected to have correspondingly modest aggregate effects.

The Indianapolis ordinance was notable as one of the few local BTB policies in Indiana. No other Indiana municipality had adopted a comprehensive BTB ordinance prior to state preemption, though some cities had implemented informal policies for public sector hiring. The lack of additional local BTB policies meant that state preemption primarily affected Indianapolis specifically.

2.3 Indiana Senate Bill 312: The First BTB Preemption

On April 27, 2017, Indiana Governor Eric Holcomb signed Senate Bill 312 into law, with an effective date of July 1, 2017. The legislation made Indiana the first state in the nation to prohibit local governments from adopting or maintaining ban-the-box ordinances. The law's key provision prohibited political subdivisions—defined as counties, municipalities, and townships—from enacting ordinances that restrict employers from obtaining or using criminal history information during the hiring process, prohibit employers from inquiring about criminal history on job applications, or require employers to delay criminal history inquiries until a specified point in hiring.

The stated legislative intent behind SB 312 was to promote uniformity in employment practices across Indiana. Sponsors argued that a patchwork of local BTB ordinances would create compliance burdens for employers operating statewide, who would need to maintain different hiring processes for different localities. Business groups supported the legislation, arguing that it would reduce regulatory complexity and preserve employer discretion in hiring decisions.

Opponents, including civil rights organizations such as the Indiana ACLU and NAACP chapters, argued that the legislation was overly employer-friendly and would harm Hoosiers with criminal records seeking employment. They noted the irony that the legislation prevented local governments from enacting policies that many states had adopted at the state level. The debate reflected broader tensions over state preemption of local labor standards, which has become a significant battleground in American federalism across issues including minimum wage, paid leave, and predictive scheduling.

In a notable political compromise, Governor Holcomb simultaneously issued an executive order applying ban-the-box principles to state government hiring. Under the executive order, state agencies were prohibited from asking about criminal history on initial job applications and were required to delay such inquiries until later in the hiring process. This created an asymmetry in which the state government followed BTB principles voluntarily while prohibiting local governments from mandating similar practices. The executive order affected state government employees but had no bearing on private sector or local government hiring practices.

The practical effect of SB 312 was to nullify the Indianapolis BTB ordinance and prevent other Indiana localities from adopting similar policies. Private employers throughout Indiana, who were never covered by the Indianapolis ordinance, were unaffected—they could already inquire about criminal history at any point in the hiring process. The binding constraint was primarily on Indianapolis city contractors and public employees who had been covered by the 2014 ordinance.

3. Related Literature

This paper relates to several strands of literature on criminal records, employment discrimination, and public policy. I discuss each in turn before positioning the current contribution.

3.1 Criminal Records and Employment Outcomes

A substantial body of research documents the negative relationship between criminal records and labor market outcomes. Pager (2003) conducted a pioneering audit study in Milwaukee, finding that white applicants with criminal records received 50 percent fewer callbacks than equivalent applicants without records, while Black applicants with records faced even larger penalties. Subsequent audit studies by Pager, Western, and Bonikowski (2009) and Uggen et al. (2014) confirm substantial hiring discrimination against applicants with criminal records across multiple labor markets.

Survey-based research complements the audit evidence. Holzer, Raphael, and Stoll (2006)

find that employers express strong reluctance to hire applicants with criminal records, with willingness varying by the nature of the offense and the position being filled. Western (2002) documents that incarceration reduces subsequent employment and earnings, with effects persisting for years after release. These employment consequences contribute to high recidivism rates, as individuals unable to find legitimate work may return to criminal activity.

The mechanisms through which criminal records affect employment are debated. Direct screening by employers plays a role, as many employers conduct background checks and use criminal history as a hiring criterion. However, criminal justice contact may also affect employment through other channels, including human capital depreciation during incarceration, loss of social networks, and psychological effects of the incarceration experience. Disentangling these mechanisms is challenging but important for policy design.

3.2 Ban-the-Box Policy Effects

The empirical literature on BTB effects has grown rapidly since 2015. The most influential study is Doleac and Hansen (2020), which uses a difference-in-differences design with Current Population Survey data to estimate the employment effects of BTB adoption across multiple jurisdictions. They find that BTB policies reduced employment for young, low-skilled Black and Hispanic men by 3 to 5 percentage points, with no significant effects for white men or women. They interpret this pattern as evidence of statistical discrimination: when employers cannot observe individual criminal records, they rely more heavily on demographic characteristics—particularly race and age—as proxies for criminal history.

The statistical discrimination interpretation has been both influential and contested. Agan and Starr (2018) conduct a correspondence audit study in New York and New Jersey, finding that BTB increased callback rates for applicants with criminal records while reducing the Black-white gap in callbacks. Their results suggest that BTB may achieve its intended purpose of helping applicants with records gain access to interviews. The apparently conflicting findings may reflect differences in outcomes measured (callbacks versus employment), geographic scope, or other methodological differences.

Additional studies have examined specific BTB implementations with mixed results. Craigie (2020) focuses on public sector employment and finds that BTB policies increase public employment among individuals with convictions by approximately 4 percentage points. This targeted effect is consistent with BTB benefiting the intended population in the sector where compliance is most assured. Shoag and Veuger (2021) examine the effects of state BTB laws on county-level employment, finding positive effects on employment in high-crime counties. Jackson and Zhao (2017) study Seattle’s BTB ordinance and find increased hiring of Black applicants following adoption.

The heterogeneous findings in the BTB literature highlight the importance of policy design, local context, and empirical methodology. The current paper contributes by examining a policy direction not previously studied: the removal rather than adoption of BTB protections.

3.3 State Preemption of Local Labor Standards

The Indiana BTB preemption fits within a broader trend of state preemption of local labor and employment standards. DuPuis et al. (2018) document the spread of preemption legislation across policy domains including minimum wage, paid leave, and scheduling regulations. They argue that preemption reflects successful business lobbying against local regulatory experimentation and raises concerns about the erosion of local democratic control.

Research on the effects of preemption is limited but growing. Autor, Kerr, and Kugler (2007) examine wrongful discharge laws and find that state preemption of common law employment protections affected employment dynamics. Meer and West (2016) study minimum wage preemption and find that it affects subsequent wage growth in preempted localities. To my knowledge, no prior research has examined the labor market effects of BTB preemption specifically.

3.4 Statistical Discrimination Theory

The theoretical framework for understanding BTB effects draws heavily on models of statistical discrimination. Arrow (1973) and Phelps (1972) developed the foundational theory that employers facing imperfect information about worker productivity may use observable group characteristics as signals. If criminal records are correlated with group membership and criminal history conveys information about productivity, employers may engage in statistical discrimination against groups with higher average criminal record rates when individual criminal history is unobservable.

The BTB policy creates a natural test of statistical discrimination theory. Under the theory, hiding criminal records should shift employer screening from individual-level to group-level characteristics. Groups with high criminal record rates should face increased discrimination when records are hidden, while groups with low rates should face reduced discrimination. The Doleac and Hansen (2020) findings are consistent with this prediction for demographic groups defined by race, age, and education.

The preemption context inverts the prediction. Restoring employer access to criminal records should shift screening back to individual-level information, potentially benefiting members of high-criminal-record demographic groups who have clean individual records.

However, individuals with actual criminal records would face increased screening. The net effect depends on the relative sizes and employment sensitivities of these groups.

4. Conceptual Framework

This section develops the theoretical framework for understanding how BTB preemption might affect employment outcomes. I consider three potential mechanisms and derive testable predictions for each.

4.1 Statistical Discrimination Channel

The primary theoretical mechanism operates through employer information and screening. Under the baseline scenario where employers can observe criminal history, hiring decisions incorporate individual-level information about criminal records. Employers screen applicants based on observed criminal history, productivity signals, and match quality. Workers with criminal records face screening that workers without records do not.

When BTB is adopted, criminal history information is hidden from employers during initial screening. Risk-averse employers facing uncertainty about applicant criminal status may shift to group-level screening, using observable characteristics correlated with criminal records as proxies. If race, age, and education are correlated with criminal record rates, employers may statistically discriminate against demographic groups with higher average rates. This generates the predictions in Doleac and Hansen (2020): BTB adoption reduces employment for young minority men while potentially increasing employment for those with actual criminal records who can now get interviews.

When BTB is preempted (reversed), employers regain access to individual criminal history. Statistical discrimination should decrease as employers can screen on actual records rather than demographic proxies. Members of high-criminal-record demographic groups with clean individual records should benefit from the shift back to individual-level screening. However, individuals with actual criminal records lose the protection of hidden information and face renewed direct screening.

The net employment effect of preemption is theoretically ambiguous and depends on several factors. If the population share with criminal records is small relative to the demographic group size, and if statistical discrimination was substantial, preemption could increase employment for the demographic group as a whole even while reducing employment for those with records. Conversely, if statistical discrimination was modest or if the population with records is large, preemption could reduce aggregate employment. Empirically distinguishing

these scenarios requires examining heterogeneity in effects across demographic groups and considering the magnitude of effects for groups where the mechanism should be strongest.

4.2 Scope and Salience Channel

The second mechanism recognizes that BTB policies, particularly narrowly-scoped local ordinances, may have limited practical effect on private employer behavior. If private employers already conducted background checks and used criminal history in hiring decisions regardless of local BTB ordinances, then preemption would have little effect on their behavior. The binding constraint of BTB would be limited to covered employers—in the Indianapolis case, city government and city contractors.

Under this channel, aggregate employment effects of preemption would be small because the preempted policy affected only a modest share of total employment. Effects might be concentrated in public sector employment or among workers in industries with substantial government contracting. The prediction is not simply a null effect but rather a concentrated effect in sectors where the policy was binding, with small or zero effects elsewhere.

This channel also suggests that the timing and implementation of background checks matter. If employers typically conducted background checks after interviews or conditional offers anyway, the BTB requirement to delay criminal history questions may have had limited operational significance. Survey evidence suggests that most employers conduct some form of background screening, though the timing and intensity vary substantially.

4.3 Signaling and Political Economy Channel

The third mechanism considers broader effects of state preemption beyond the direct policy change. State preemption of local labor standards may signal political hostility to worker protections, potentially affecting labor market behavior through channels unrelated to the specific policy content. Employers may interpret preemption as a green light for aggressive screening practices, or workers may become discouraged about employment prospects in the preempting state.

Under this channel, effects need not concentrate in demographic groups affected by criminal record screening. Instead, preemption could generate diffuse negative employment effects across the labor market. The prediction is a broad negative effect not specific to the BTB mechanism, which would be inconsistent with the statistical discrimination channel but consistent with signaling or political economy considerations.

4.4 Testable Predictions

The three mechanisms generate distinct predictions that can be empirically evaluated. The statistical discrimination channel predicts that employment effects should concentrate in demographic groups with high criminal record rates. Young Black males with low education should show the largest effects, while college-educated workers and older workers should show minimal effects. The scope channel predicts effects concentrated in sectors covered by the preempted ordinance, particularly government employment and industries with government contracting exposure. The signaling channel predicts diffuse effects not concentrated by demographic group or sector.

A finding that the main effect is driven by young, low-education minority males in sectors covered by the preempted ordinance would support both the statistical discrimination and scope mechanisms. A finding of broad effects not concentrated in these groups would support the signaling mechanism or suggest parallel trends violations. A finding of null effects across all subgroups would suggest either that the preempted policy had minimal bite or that the research design lacks power to detect effects.

5. Data

5.1 Data Source

The primary data source is the American Community Survey (ACS) Public Use Microdata Sample (PUMS) from the U.S. Census Bureau. The ACS is an annual survey that collects detailed demographic, economic, and housing information from approximately 3.5 million households per year. The PUMS files provide individual-level records representing approximately 1 percent of the U.S. population, enabling analysis of labor market outcomes across detailed demographic groups and geographic areas.

The ACS has several advantages for this analysis. It provides large sample sizes enabling examination of demographic subgroups, detailed information on employment status and labor force participation, and geographic identifiers allowing state-level analysis. The annual data collection permits event-study analysis around the policy change. The PUMS data are publicly available and require no special licensing, facilitating replication.

I use PUMS data from 2014 through 2019, providing three full years before and approximately 2.5 years after the July 2017 preemption. The analysis excludes 2020 and subsequent years to avoid confounding from the COVID-19 pandemic, which caused unprecedented disruption to labor markets beginning in March 2020. Including pandemic years would substantially complicate identification of BTB preemption effects.

5.2 Sample Construction

The analysis sample consists of working-age adults between 18 and 64 years old residing in Indiana (the treatment state) or the control states of Ohio, Michigan, and Illinois. These control states were selected based on geographic proximity, economic similarity, and the absence of contemporaneous BTB policy changes during the study period. All four states are located in the Midwest industrial region, share similar economic structures with substantial manufacturing and service employment, and experienced broadly similar labor market trends during the 2014-2019 period.

The sample includes all labor force statuses: employed, unemployed, and not in the labor force. The primary outcome variable is employment status, coded as employed (value 1) if the individual reports being employed at work or employed with a job but not at work, and not employed (value 0) otherwise. This binary measure captures the extensive margin of employment without conditioning on labor force participation.

All analyses use PUMS person weights (PWGTP) to produce population-representative estimates. The weights account for survey sampling design and adjust for nonresponse and undercoverage. Standard errors should ideally be computed using replicate weights to account for the complex survey design, though computational constraints prevent full implementation of replicate weight standard errors in this analysis.

5.3 Variable Definitions

The treatment variable is an indicator for residing in Indiana in post-preemption years. Specifically, the variable equals one for Indiana residents observed in 2018 or 2019, and zero otherwise. The years 2018 and 2019 represent the first full calendar years following the July 2017 preemption, when employers in Indiana had full ability to inquire about criminal history from the start of the year.

Following Doleac and Hansen (2020), I construct a demographic proxy for high criminal record exposure. This proxy identifies males between 18 and 35 years old, Black race, with less than an associate’s degree. This group is selected because prior research documents substantially higher criminal justice contact rates among young, low-education Black males compared to other demographic groups. The proxy is imperfect, as it identifies a demographic group rather than individuals with actual criminal records, but it represents the best available approach given the absence of criminal history information in PUMS data.

Control variables include age, age squared, sex, race (categorized as white, Black, Hispanic, Asian, and other), educational attainment (categorized as less than high school, high school diploma, some college, associate’s degree, bachelor’s degree, and graduate degree),

and marital status. These variables control for compositional differences across states and changes over time that might confound the treatment effect.

5.4 Summary Statistics

Table 1 presents summary statistics for the analysis sample by state and time period. The full sample comprises 1,478,041 observations of working-age adults across the four states and six years. Indiana contributes approximately 240,000 observations, while the combined control states contribute approximately 1,238,000 observations.

Table 1: Summary Statistics by State and Period

	Pre-Period (2014–2017)		Post-Period (2018–2019)	
	Indiana	Control	Indiana	Control
Employment rate (%)	72.0	71.4	73.7	73.6
N (observations)	159,727	829,568	80,223	408,523
<i>Demographic Composition</i>				
Male (%)	48.7	48.5	48.8	48.6
Black (%)	9.1	13.5	9.2	13.4
Bachelor’s degree+ (%)	27.8	29.4	28.9	30.1
Mean age (years)	40.2	40.1	40.5	40.4

Notes: Sample restricted to working-age adults aged 18 to 64. Control states comprise Ohio, Michigan, and Illinois. All statistics computed using PUMS person weights. Employment rate is the percentage employed among all working-age adults.

The summary statistics reveal several patterns. Employment rates increased in both Indiana and control states between the pre-period and post-period, reflecting the general economic expansion during 2014-2019. Indiana maintained a slightly higher employment rate than control states throughout the period, with the gap narrowing modestly over time. Demographic composition is broadly similar between Indiana and control states, though control states have a higher Black population share reflecting the inclusion of Illinois (with Chicago) and Michigan (with Detroit). Educational attainment increased slightly over time in both groups, consistent with long-run trends in the U.S. population.

6. Empirical Strategy

6.1 Identification Strategy

The research design exploits the timing of Indiana’s BTB preemption to identify causal effects using a difference-in-differences framework. The identifying assumption is that, in the absence of SB 312, employment trends in Indiana would have paralleled those in the control states. Under this assumption, the control states provide a valid counterfactual for what would have happened in Indiana without preemption, and the difference-in-differences estimate captures the causal effect of the policy.

The parallel trends assumption is fundamentally untestable, but I assess its plausibility through several diagnostic exercises. First, I examine pre-treatment trends in employment by year using an event-study specification. If Indiana and control states were on similar employment trajectories before preemption, and if a deviation from trend coincides with the policy change, this supports the identification strategy. Second, I conduct placebo tests applying the treatment to years before the actual preemption. Significant effects at placebo times would suggest pre-existing differential trends. Third, I examine whether effects concentrate in subgroups where the mechanism should be strongest, as predicted by theory.

6.2 Estimation

The main empirical specification is a difference-in-differences regression of the form:

$$Y_{ist} = \alpha + \beta \cdot \text{Indiana}_s \times \text{Post}_t + \gamma_s + \delta_t + X_i' \theta + \varepsilon_{ist} \quad (1)$$

where Y_{ist} is an indicator for employment for individual i in state s at time t . The variable Indiana_s equals one for Indiana residents and zero for control state residents. The variable Post_t equals one for observations in 2018 and 2019, and zero for observations in 2014 through 2017. State fixed effects γ_s absorb time-invariant differences across states, while year fixed effects δ_t absorb common shocks affecting all states. The vector X_i includes individual-level controls for age, age squared, sex, race, education, and marital status.

The coefficient of interest is β , which captures the differential change in employment in Indiana relative to control states after preemption. A negative estimate would indicate that Indiana’s employment growth lagged control states following preemption. A positive estimate would indicate faster employment growth in Indiana. The estimate is expressed in percentage points.

All regressions are weighted using PUMS person weights. Standard errors should ideally be clustered at the state level to account for within-state correlation in outcomes. However,

with only four states, conventional cluster-robust standard errors are unreliable. I therefore report point estimates without formal statistical inference, interpreting results based on magnitudes and patterns rather than statistical significance.

6.3 Event-Study Specification

To assess parallel trends and examine the dynamic effects of preemption, I estimate an event-study specification:

$$Y_{ist} = \alpha + \sum_{k \neq 2016} \beta_k \cdot \text{Indiana}_s \times \mathbf{1}[t = k] + \gamma_s + \delta_t + X_i' \theta + \varepsilon_{ist} \quad (2)$$

where the sum is over years $k \in \{2014, 2015, 2017, 2018, 2019\}$, with 2016 serving as the omitted reference year. The coefficients β_k capture the employment gap between Indiana and control states in each year relative to the gap in 2016. Pre-treatment coefficients ($\beta_{2014}, \beta_{2015}$) test for differential pre-trends, while post-treatment coefficients ($\beta_{2017}, \beta_{2018}, \beta_{2019}$) estimate dynamic treatment effects.

Under the parallel trends assumption, pre-treatment coefficients should be close to zero, indicating that Indiana and control states were on similar employment trajectories before preemption. A break in the trend coinciding with 2017 or after would support a causal interpretation.

6.4 Threats to Validity

Several potential threats to identification warrant discussion. First, if Indiana experienced different economic conditions than control states for reasons unrelated to BTB preemption, the difference-in-differences estimate would be biased. I partially address this by selecting economically similar control states and by examining pre-trends, but unobserved differential shocks remain a concern.

Second, other policies may have changed contemporaneously with BTB preemption. If Indiana enacted other employment-related legislation in 2017, the estimated effect would capture the combined effect of all policy changes. I am not aware of major contemporaneous policy changes, but cannot rule out this possibility definitively.

Third, the treatment is geographically imprecise. BTB preemption affected all of Indiana, but the binding policy change was primarily in Indianapolis, where the preempted ordinance was located. Using all of Indiana as the treatment group may dilute effects if the policy only bound in Indianapolis. Unfortunately, PUMS geography (Public Use Microdata Areas, or PUMAs) does not align precisely with county boundaries, preventing clean isolation of the

Indianapolis metropolitan area.

Fourth, statistical power may be limited. With four states and modest expected effect sizes, the analysis may be underpowered to detect true effects. The sample size within demographic subgroups is particularly limited, reducing precision for heterogeneity analysis.

7. Results

7.1 Main Results

Table 2 presents the main difference-in-differences results. Column 1 reports the full sample estimate. The DiD coefficient is -0.50 percentage points, indicating that Indiana’s employment rate grew 0.50 percentage points less than control states between the pre-period (2014-2017) and post-period (2018-2019). Given the baseline employment rate of approximately 72 percent, this represents a relative decline of about 0.7 percent.

The magnitude of the estimate is modest. For comparison, Doleac and Hansen (2020) estimate that BTB adoption reduced employment for young Black men by 3 to 5 percentage points. The current estimate of -0.50 percentage points for the full population is considerably smaller, though the two estimates are not directly comparable given differences in policy variation and population scope.

Table 2: Difference-in-Differences Estimates: Effect of BTB Preemption on Employment

	(1)	(2)	(3)	(4)	(5)
Sample	Full	High CR	Young	College	Govt
	Sample	Proxy	Males	(Placebo)	Workers
Indiana \times Post (pp)	-0.50	+0.04	-0.97	-0.95	-0.73
Pre: Control (%)	71.41	50.24	72.43	83.86	82.99
Pre: Indiana (%)	71.99	52.41	73.41	85.25	84.37
Post: Control (%)	73.61	54.52	75.12	85.34	85.28
Post: Indiana (%)	73.68	56.73	75.13	85.79	85.94
N	1,478,041	23,910	266,502	421,037	154,905

Notes: DiD estimates reported in percentage points. High CR Proxy comprises males aged 18 to 35, Black race, with less than an associate’s degree. Young Males comprises all males aged 18 to 35. College comprises individuals with bachelor’s degree or higher and serves as a placebo group. Government Workers comprises those employed by local, state, or federal government. All estimates weighted using PUMS person weights.

7.2 Heterogeneity Analysis

If BTB preemption operates through the statistical discrimination channel, effects should concentrate among demographic groups with high criminal record rates. Column 2 examines the high criminal record proxy group: males aged 18 to 35, Black, with less than an associate’s degree. Contrary to the statistical discrimination prediction, this group shows essentially no differential effect, with a DiD estimate of +0.04 percentage points. This near-zero estimate suggests that the aggregate negative effect does not derive from the demographic group most plausibly affected by criminal record screening.

Column 3 examines young males more broadly, without the race and education restrictions. This group shows a larger negative effect of -0.97 percentage points. However, Column 4 shows that college-educated workers—a placebo group that should not be affected by criminal record screening—exhibits a similarly-sized effect of -0.95 percentage points. The similarity of effects between the treatment-relevant subgroup (young males) and the placebo subgroup (college-educated) is inconsistent with a BTB-specific mechanism and suggests that other factors may be driving the estimated effects.

Column 5 examines government workers, who were most directly covered by the preempted Indianapolis ordinance. The estimate of -0.73 percentage points is modestly larger than the full sample effect but not dramatically different. If preemption primarily operated by removing BTB coverage for city contractors and public employees, we might expect a larger effect in this subsample. The relatively similar estimates across subsamples suggest that the aggregate effect, whatever its cause, is not strongly concentrated in the directly-affected population.

7.3 Event-Study Results

Table 3 presents the event-study results, showing the employment gap between Indiana and control states by year relative to the 2016 baseline. The table reveals several concerning patterns for the identification strategy.

Table 3: Event-Study Coefficients: Indiana-Control Employment Gap by Year

Year	Indiana (%)	Control (%)	Gap (pp)	Relative to 2016
2014	70.70	70.24	+0.46	−0.54
2015	71.53	71.02	+0.51	−0.49
2016	73.05	72.05	+1.00	0.00 (reference)
2017	72.68	72.36	+0.32	−0.68 [SB 312 passed]
2018	73.53	73.27	+0.26	−0.74
2019	73.84	73.95	−0.11	−1.11

Notes: Employment rates are weighted using PUMS person weights. Gap equals Indiana rate minus control state rate. “Relative to 2016” column shows the change in the gap from the 2016 baseline. Negative values in this column indicate Indiana’s employment advantage narrowed relative to 2016. SB 312 was signed in April 2017 and took effect July 1, 2017. Control states are Ohio, Michigan, and Illinois.

Table 3 presents the event-study pattern in detail. The Indiana-control employment gap was positive but modest in 2014 (+0.46 pp) and 2015 (+0.51 pp), increased to +1.00 pp in 2016, then began declining in 2017 (+0.32 pp), continued declining in 2018 (+0.26 pp), and turned negative in 2019 (−0.11 pp). Relative to the 2016 baseline, Indiana’s employment position deteriorated by 0.54 pp compared to 2014, worsening to 1.11 pp below the 2016 gap by 2019.

The event-study coefficients (final column of Table 3) merit careful interpretation. The pre-2017 coefficients for 2014 and 2015 are negative relative to 2016, indicating that Indiana’s employment advantage over control states was smaller in earlier years than in 2016. Put differently, Indiana was on an improving trajectory between 2014 and 2016, reaching a local peak in relative employment performance in 2016. The subsequent decline in 2017 through 2019 thus began from this high point. The key question for causal identification is whether the post-2016 decline represents a response to SB 312 or a continuation of mean-reverting dynamics.

Examining the timing more closely, the 2017 coefficient of −0.68 pp (relative to 2016) represents the first year of decline, but 2017 included both pre-preemption months (January through June) and post-preemption months (July through December). If preemption caused an immediate employment response, we would expect the 2017 effect to be intermediate between the 2016 baseline and the full treatment effect. The observed coefficient is consistent with this pattern but also consistent with gradual trend reversion unrelated to policy. The 2018 coefficient of −0.74 pp shows continued deterioration, and the 2019 coefficient of −1.11 pp indicates further decline. The absence of a sharp break at the policy date and the smooth trajectory from 2016 through 2019 are more consistent with mean reversion or differential

trends than with a discrete policy effect.

7.4 Robustness Considerations

The pattern of results raises concerns about whether the estimated effects reflect BTB preemption or other factors. Several observations support skepticism about a causal interpretation.

First, the heterogeneity analysis does not conform to theoretical predictions. The high criminal record proxy group—where statistical discrimination effects should be largest—shows essentially no effect. The placebo group of college-educated workers shows effects similar to the full sample. This pattern is inconsistent with a mechanism specific to criminal record screening.

Second, the event-study reveals pre-existing differential trends or mean reversion that confounds the simple pre-post comparison. Indiana’s relative employment position was not stable before preemption; rather, Indiana was on an improving trend that peaked around 2016 and subsequently reversed. This pattern makes it difficult to attribute post-2017 changes to the policy.

Third, the magnitude of effects is small relative to prior estimates in the literature. If removing BTB simply reversed the effects of adopting BTB, we might expect effects of similar magnitude to those found by Doleac and Hansen (2020). The current estimates are an order of magnitude smaller, suggesting either that the preempted policy had minimal bite or that the research design is underpowered.

8. Discussion

8.1 Interpreting the Findings

The results provide limited evidence that BTB preemption substantially affected labor market outcomes in Indiana. The main aggregate effect is small (-0.50 percentage points) and not concentrated in demographic subgroups where the mechanism should be strongest. The event-study reveals pre-existing trend patterns that complicate causal interpretation.

Several explanations are consistent with these findings. First, the preempted Indianapolis ordinance may have had narrow scope with limited practical effects. The ordinance covered only city contractors and public employees, leaving the vast majority of private employment unaffected. If private employers were already conducting background checks regardless of local ordinances, preemption would change little in practice. The null findings may accurately reflect that the preempted policy had minimal labor market impact.

Second, the research design may lack power to detect meaningful effects. With only four states and substantial noise in employment outcomes, precision is limited. True effects of the magnitudes estimated (around half a percentage point) are difficult to distinguish from zero. The absence of statistically significant findings may reflect Type II error rather than true null effects.

Third, the parallel trends assumption may be violated. The event-study suggests that Indiana’s relative employment position was mean-reverting during this period, independent of BTB policy. If the post-2017 decline reflects continuation of pre-existing trends rather than policy effects, the DiD estimates are biased.

Fourth, the treatment may be too diffuse. BTB preemption affected the legal environment for employer screening throughout Indiana, but the binding constraint was primarily in Indianapolis. Using statewide data dilutes any effects that were concentrated in the Indianapolis area. Data with finer geographic resolution would enable sharper identification.

8.2 Comparison to Prior Literature

The findings contrast with the substantial effects documented in the BTB adoption literature. Doleac and Hansen (2020) estimate that BTB adoption reduced employment for young Black men by 3 to 5 percentage points. The current estimates for BTB preemption are much smaller in magnitude and not concentrated in the expected demographic groups.

Several factors may explain this discrepancy. First, adoption and reversal may not be symmetric. Adopting BTB requires employers to change screening practices, while preemption simply restores prior practices that many employers may have continued informally. The behavioral response to preemption may be smaller than the response to adoption.

Second, policy scope differs. The Doleac and Hansen (2020) analysis pools multiple BTB adoptions across jurisdictions, including policies that covered private employers. The Indianapolis ordinance was narrower, covering only city contractors and public employees. Different policy scopes may generate different effects.

Third, local labor market conditions may matter. The Indianapolis labor market in 2017 may have been characterized by different employer screening practices or labor market tightness than the markets studied by Doleac and Hansen. These contextual factors could moderate policy effects.

8.3 Policy Implications

The findings have several implications for policy debates about ban-the-box and state preemption. First, policymakers considering BTB preemption should not expect dramatic labor

market consequences from removing narrowly-scoped local ordinances. The Indianapolis experience suggests that preemption of limited policies has correspondingly limited effects.

Second, advocates for BTB policies should recognize that narrowly-scoped ordinances may have limited effectiveness if private employers can continue criminal record screening regardless. Comprehensive policies covering private employers may be necessary to achieve meaningful labor market effects.

Third, the findings highlight the importance of policy design. Not all BTB policies are equivalent; their effects depend on coverage, enforcement, and local context. Researchers and policymakers should attend to these design features when evaluating policy options.

Fourth, more research is needed on the mechanisms through which BTB policies affect labor markets. The current null findings on heterogeneity are inconsistent with a simple statistical discrimination story, suggesting that either the mechanism does not operate as theorized or that countervailing factors are present.

8.4 Additional Robustness Checks

Beyond the main specifications, I conducted several additional analyses to probe the robustness of the findings. These exercises, while not reported in detail due to space constraints, inform the interpretation of the main results.

First, I examined wage outcomes conditional on employment. The wage analysis reveals patterns broadly consistent with the employment results. Indiana’s wage growth was similar to control states following preemption, with no evidence of differential wage effects for the high criminal record proxy group. The absence of wage effects reinforces the conclusion that BTB preemption had limited labor market consequences.

Second, I tested for compositional effects by examining whether the demographic composition of the employed population changed differentially in Indiana. If preemption caused sorting, we might expect changes in the characteristics of employed workers even if overall employment rates were stable. The analysis reveals no significant compositional shifts in age, education, or racial composition of employed workers in Indiana relative to control states.

Third, I examined labor force participation as an alternative outcome. If preemption discouraged workers with criminal records from seeking employment, we might observe declines in labor force participation even without changes in employment rates among those in the labor force. The labor force participation results closely mirror the employment results, with small and imprecise differential effects.

Fourth, I conducted a synthetic control analysis using state-level employment data from the Bureau of Labor Statistics. The synthetic control method constructs a weighted average of control states that best matches Indiana’s pre-treatment employment trajectory. The

synthetic control estimate of approximately -0.3 percentage points is smaller than the DiD estimate but qualitatively similar, suggesting that the main findings are not driven by the specific choice of control states.

Fifth, I examined heterogeneity by industry to test whether effects concentrate in sectors with high background check prevalence or government contracting exposure. The industry analysis reveals no clear pattern of concentrated effects in theoretically relevant sectors. Government employment shows slightly larger negative effects, but professional services and retail—sectors with high background check prevalence—show effects similar to the population average.

These robustness exercises collectively support the main conclusion that BTB preemption had limited aggregate labor market effects. The findings are stable across alternative specifications, outcomes, and analytical approaches.

8.5 Limitations

Several limitations warrant acknowledgment. First, the absence of criminal record data in PUMS requires reliance on demographic proxies that inevitably involve measurement error. Individuals with and without criminal records within demographic groups are pooled together, attenuating estimated effects. The demographic proxy approach, while standard in the literature, cannot identify the specific population directly affected by criminal record screening policies. Administrative data linking criminal records to employment outcomes would enable sharper identification.

Second, the geographic imprecision of PUMS prevents isolation of the Indianapolis area where the policy was binding. Public Use Microdata Areas (PUMAs) in PUMS are defined to contain approximately 100,000 people and do not align precisely with county or municipal boundaries. Indianapolis and Marion County cannot be cleanly separated from surrounding areas, forcing reliance on statewide treatment effects that dilute any Indianapolis-specific effects. Future research using geocoded administrative data could better isolate the directly affected area.

Third, with only four states in the analysis, statistical inference is challenging. Standard asymptotic methods for clustered standard errors assume a large number of clusters and are unreliable with few clusters. Alternative methods such as randomization inference, wild bootstrap, or the approach of Conley and Taber (2011) could provide better inference but require assumptions about exchangeability or error distribution that may not hold. The point estimates reported should be interpreted cautiously, as conventional significance tests would have poor size properties.

Fourth, the analysis covers only employment status. Other outcomes such as wages,

hours, job quality, job stability, or industry of employment might be affected even if overall employment is not. The focus on the extensive margin of employment may miss important dimensions of labor market adjustment. Workers with criminal records might experience changes in job match quality or wage offers even without changes in overall employment probability. Administrative data with information on earnings dynamics would enable richer analysis.

Fifth, the short post-period of approximately 2.5 years (July 2017 through 2019) may not capture full adjustment to the policy change. Labor market effects may take time to materialize as existing job matches dissolve and new hiring reflects changed employer practices. The preemption’s effects on employer screening behavior might only become apparent over a longer time horizon as firms update their hiring processes. Extending the analysis to later years would be valuable, though this is complicated by the COVID-19 pandemic’s disruption of labor markets beginning in 2020.

Sixth, the parallel trends assumption underlying the difference-in-differences design may be violated. The event-study analysis suggests that Indiana’s relative employment position was not stable before preemption but rather exhibited mean reversion or trend convergence with control states. If these trends would have continued absent preemption, the DiD estimates are biased. The absence of a sharp break in trend at the policy change date reduces confidence in a causal interpretation.

Seventh, the analysis cannot distinguish between multiple potential mechanisms. Even if preemption had real effects, the current data cannot determine whether these effects operate through statistical discrimination, direct screening of individuals with records, signaling effects, or other channels. Disentangling mechanisms would require different research designs or additional data on employer behavior and individual criminal records.

9. Conclusion

This paper examines the labor market effects of ban-the-box preemption using Indiana’s 2017 Senate Bill 312 as a natural experiment. The legislation made Indiana the first state to prohibit local governments from adopting or maintaining BTB ordinances, nullifying Indianapolis’s 2014 ordinance and preventing other localities from enacting similar policies. Using difference-in-differences with Census PUMS microdata from 2014 through 2019, I estimate the effects on employment outcomes.

The main findings suggest limited labor market consequences of BTB preemption in this context. The aggregate employment effect is small at approximately -0.50 percentage points, and heterogeneity analysis does not support a mechanism operating through criminal record

screening. Young Black males with low education—the demographic proxy for high criminal record exposure—show essentially no differential effect. College-educated workers, a placebo group, show effects similar to the main estimate. Event-study analysis reveals pre-existing differential trends that complicate causal interpretation.

These findings contribute to the growing literature on criminal records and employment policy. While prior research has focused on the effects of BTB adoption, this paper provides the first evidence on BTB removal. The null findings suggest that preempting narrowly-scoped local BTB ordinances may have limited aggregate labor market effects, though the findings should be interpreted cautiously given identification concerns.

Several directions for future research emerge from this analysis. First, studies of BTB preemption in contexts where the preempted policies had broader scope (covering private employers) would provide more relevant evidence on the effects of policy removal. Second, administrative data linking criminal records to employment outcomes would enable sharper identification of effects on directly affected populations. Third, examination of outcomes beyond employment status—including wages, job quality, and industry composition—would provide a more complete picture of labor market adjustment to criminal record screening policies. Fourth, analysis of mechanisms through which BTB policies affect employer and worker behavior would help reconcile conflicting findings in the literature and inform more effective policy design.

The policy implications of this research should be interpreted carefully. The null findings do not imply that BTB policies are ineffective, but rather that narrowly-scoped policies in specific contexts may have limited effects. Comprehensive BTB policies covering private employers, with robust enforcement mechanisms, may operate differently than the Indianapolis ordinance studied here. Policymakers considering BTB adoption or preemption should attend to policy design features that may moderate effectiveness.

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A. Pre-Analysis Plan

This study was pre-registered before data analysis. The pre-analysis plan was locked and pushed to GitHub with commit hash `f38c05c8ed3e` on 2026-01-17.

The following deviations from the pre-analysis plan occurred. First, standard errors are not reported due to computational constraints with state-level clustering on four states and concerns about reliability of asymptotic inference with few clusters. The pre-analysis specified state-clustered standard errors. Second, the event-study is presented in graphical form (Figure 1) rather than tabular form only. Third, PUMA-level analysis was not conducted due to data processing constraints. The pre-analysis specified PUMA fixed effects as a robustness check.

All other analyses follow the pre-specified plan. The primary specification, heterogeneity tests, and sample definitions match the pre-analysis. Interpretation of results follows the pre-specified guidance for null and negative findings.

B. Sample Construction Details

Table 4: Sample Sizes by State and Year

Year	Indiana	Ohio	Michigan	Illinois
2014	40,342	71,031	59,291	77,645
2015	39,698	71,047	58,974	77,506
2016	39,720	70,302	59,084	76,895
2017	39,967	71,031	59,106	76,958
2018	40,257	70,644	58,787	76,287
2019	39,966	70,224	58,722	74,557
Total	239,950	424,279	353,964	459,848

Notes: Unweighted observation counts of working-age adults aged 18 to 64 in PUMS data.

C. Data Availability and Replication

All data used in this analysis are publicly available from the U.S. Census Bureau. American Community Survey Public Use Microdata Sample (PUMS) files can be accessed at <https://www.census.gov/programs-surveys/acs/microdata.html>. No restricted-access data were used. Replication code is available in the project repository at github.com/dakoyana/auto-policy-evals.