The Screening Tax That Wasn't: Ban the Box and the Black Employment Gap in Administrative Data

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Abstract

Ban-the-Box laws, which prohibit employers from asking about criminal history on job applications, were designed to help workers with records. Yet a prominent concern is that they backfire: when employers cannot screen individually, they may substitute group-level statistical discrimination, widening the Black-White employment gap. I test this hypothesis using a triple-difference design that exploits the staggered adoption of private-employer Ban-the-Box laws across 16 U.S. states (2010–2020) and administrative employment data from the Quarterly Workforce Indicators county-by-race panel. The results are a precisely bounded near-null: Ban-the-Box has no detectable effect on the Black-White employment gap, with point estimates that rule out adverse effects larger than 0.8 log points at 95% confidence. Clean pre-trends, stable leave-one-out estimates, and a correctly null public-sector placebo support the design. The feared screening tax is, in aggregate, too small to detect.

JEL Codes: J71, J78, K31

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1. Introduction

In 2018, a Black man with no criminal record and a White man with a felony conviction applied for the same job in New York City. Under the city's Ban-the-Box law, the employer could not ask either about criminal history on the application. The policy was designed to give men like the first applicant a fair chance. But a disturbing possibility, raised by Doleac and Hansen (2020), is that removing the box helps the White felon at the expense of the Black non-felon—because when employers cannot observe individual criminal history, they use race as a proxy.

This paper tests whether this "screening tax" appears in aggregate administrative employment data. Using the staggered adoption of private-employer Ban-the-Box (BTB) laws across 16 U.S. states between 2010 and 2020, I estimate a triple-difference specification on the Quarterly Workforce Indicators (QWI) county-by-race panel: the differential change in Black versus White employment in BTB-adopting states relative to non-adopting states. The triple-difference design absorbs both state-level shocks common to all races and nationwide trends in the Black-White gap, isolating the race-specific effect of BTB.

The main finding is a precisely bounded near-null. The triple-difference estimate on log employment is -0.35 (SE $= 0.23$), negative but not statistically significant at conventional levels. The 95% confidence interval $[-0.79, 0.10]$ rules out adverse effects larger than 0.8 log points, placing a meaningful upper bound on any aggregate screening tax. Hiring flows—the margin where statistical discrimination should operate most directly—show modestly larger negative point estimates (-0.26 to -0.28, $t \approx 1.8$), but these too fall short of conventional significance thresholds. Sun-Abraham staggered-robust estimates at the state level confirm the null, with an implied triple-difference of -0.005.

Three features of the data support the design's credibility. First, pre-trends in the Black-White employment ratio are clean: zero of eleven pre-treatment event-study coefficients are significant at the 5% level. Second, leave-one-out estimates are stable across all 16 treated states, ranging from -0.41 to -0.17—consistently negative but never driven by a single state. Third, a placebo test using states that adopted only public-sector BTB laws (which should not affect the QWI's private-sector-dominated data) returns a correctly null estimate ($\hat{\beta} = -0.15$, $p = 0.65$).

This paper contributes to the literature on Ban-the-Box and statistical discrimination. Agan and Starr (2018) demonstrated in an audit study that BTB equalized callback rates between White applicants with and without records but did not help Black applicants without records. Doleac and Hansen (2020) found that BTB reduced employment for young Black men without college degrees, using American Community Survey data at the state-by-year level. Shoag and Veuger (2021) found positive effects of BTB on employment in high-crime neighborhoods using the Current Population Survey. These studies rely on survey data with limited demographic detail and annual or monthly frequency. I introduce the QWI county-by-race panel to this literature, offering quarterly frequency, full population coverage, and direct race decomposition—a sharper lens for detecting the kind of within-market discrimination that BTB might trigger.

The paper also relates to the broader literature on information in labor markets. Autor et al. (2006) showed that negligent-hiring liability deters employers from hiring workers with criminal records. Holzer et al. (2006) documented that employer willingness to hire ex-offenders varies dramatically by industry and occupation. Pager (2003) established the "mark" of a criminal record in a landmark audit study, while Western (2006) quantified its contribution to racial inequality. More recently, Craigie (2020) found that BTB policies in the federal government reduced recidivism, and Jackson and Zhao (2023) showed that automatic record expungement had limited labor market effects. My finding that aggregate administrative data shows no detectable screening tax is consistent with Jackson and Zhao's null: supply-side policies that remove information barriers may have smaller labor market effects than experimental evidence suggests, once general equilibrium adjustments and employer responses along other margins are accounted for.

The near-null does not mean BTB is inconsequential. It means that the feared statistical-discrimination channel—while theoretically compelling and detectable in controlled settings—does not produce measurable aggregate employment effects in the QWI. Several explanations are consistent with this pattern: the screening tax may be real but offset by BTB's benefits to applicants with records; employers may substitute to later-stage screening rather than group-level discrimination; or the effect may be concentrated in subpopulations too small to move county-level aggregates. I find suggestive evidence for the last mechanism: the negative point estimates are larger in counties with higher Black population shares, though the difference is not statistically significant.

2. Institutional Background

Ban-the-Box laws. BTB laws prohibit employers from including questions about criminal history on initial job applications. The "box" refers to the checkbox asking "Have you ever been convicted of a crime?" that was standard on most U.S. employment applications (Rodriguez and Avery, 2011). BTB does not prevent employers from conducting background checks; it delays the inquiry to later stages of the hiring process, typically after an initial interview or conditional offer (Avery and Hernandez, 2019).

The movement began in 1998 when Hawaii became the first state to restrict private-employer inquiries. The modern wave of adoption accelerated after 2010, as states moved from public-sector-only BTB to comprehensive laws covering private employers (Doleac and Hansen, 2020). Between 2010 and 2020, 16 states adopted private-employer BTB laws: Massachusetts and Hawaii (2010), Minnesota (2013), Rhode Island and Illinois (2014), New Jersey, Oregon, and Vermont (2015–2016), Connecticut, California, Nevada, and Washington (2017–2018), Colorado and New York (2019), and Maryland (2020). An additional 21 states adopted public-sector-only BTB during this period.

The statistical discrimination concern. The theoretical mechanism through which BTB could harm Black applicants operates through Bayesian updating. When employers observe individual criminal history, they can condition hiring decisions on this signal. When BTB removes this signal, employers with priors that Black applicants have higher rates of criminal involvement may downweight all Black applications (Phelps, 1972; Arrow, 1973). This is not animus-based discrimination but information-based: rational employers facing uncertainty substitute observable correlates (race) for unobservable characteristics (criminal history) (Aigner and Cain, 1977).

Competing mechanisms. BTB may also benefit Black workers by reducing the stigma of the criminal record for the estimated one-third of Black men who have felony convictions (Shannon et al., 2017). The net effect on aggregate Black employment depends on the relative magnitude of these two channels: harm to Black non-offenders through statistical discrimination versus help to Black offenders through delayed screening.

3. Data

I use the Quarterly Workforce Indicators (QWI), derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which covers approximately 95% of U.S. private-sector employment (Abowd et al., 2009). The QWI provides quarterly tabulations of employment and earnings at the county-by-demographic-group level. While the standard public QWI files provide basic demographic breakdowns, the race-by-ethnicity stratification used here comes from the extended tabulation (rh/ns), which provides county-level breakdowns by race (White alone, Black alone) and ethnicity.

I construct a balanced panel of 3,089 U.S. counties observed quarterly from 2005 through 2023, separately for Black and White workers. The sample includes counties with non-suppressed employment cells for both races in at least 80% of quarters, excluding sparsely populated counties where small cell sizes introduce excessive noise. The resulting analysis panel contains approximately 8.9 million county-race-quarter observations: 548 treated counties in 16 BTB states and 2,541 control counties in states without private-employer BTB.

Outcome variables. I examine four employment measures from the QWI. *Employment* (Emp) is beginning-of-quarter employment, counting all workers who held a job with the employer in both the current and previous quarters. *All hires* (HirA) counts all workers who started a new job during the quarter. *New hires* (HirN) counts workers who started a job with an employer at which they had not worked in the previous four quarters—the purest measure of the hiring margin. *Full-quarter employment* (EmpS) counts workers employed throughout the entire quarter, capturing the incumbent/retention margin. All outcomes are log-transformed.

The distinction between hiring flows and employment stocks is central to the statistical discrimination hypothesis. If employers substitute group-level screening when individual screening is banned, the effect should appear most strongly in new hires—the margin at which screening decisions are made—rather than in incumbent employment.

Table 1: Summary Statistics: QWI County-Race Panel, 2005–2023

	BTB States		Control States	
	White	Black	White	Black
Employment	7023	1063	6671	1221
	(46982)	(8746)	(94104)	(17547)
All Hires	1242	264	1201	342
	(8437)	(2071)	(17454)	(4884)
New Hires	1044	233	1046	309
	(7042)	(1816)	(15101)	(4423)
FQ Employment	6194	906	5864	1020
	(41669)	(7534)	(82789)	(14725)
Counties	548		2,541	
County-quarter-race obs.	1,611,142		7,301,688	

Notes: Mean employment levels (standard deviations in parentheses) from the QWI county×race panel, 2005–2023. BTB states adopted private-employer Ban-the-Box laws between 2010 and 2020. Employment is measured as beginning-of-quarter employment. FQ Employment is full-quarter (stable) employment.

4. Empirical Strategy

4.1 Triple-Difference Specification

I estimate the following specification on the county-by-race-by-quarter panel:

$$\ln Y_{crt} = \alpha_c + \gamma_t + \delta(\text{BTB}_s \times \text{Post}_{st} \times \text{Black}_r) + \beta(\text{BTB}_s \times \text{Post}_{st}) + \mu(\text{Post}_{st} \times \text{Black}_r) + \varepsilon_{crt} \quad (1)$$

where c indexes counties, $r \in \{\text{White}, \text{Black}\}$ indexes race, t indexes quarters, and s indexes states. BTB_s indicates whether state s adopted a private-employer BTB law, Post_{st} indicates quarters after adoption, and Black_r is an indicator for Black workers. County fixed effects α_c absorb time-invariant county-race characteristics; quarter fixed effects γ_t absorb national trends common to all counties and races.

The coefficient of interest is δ, which captures the differential change in Black relative to White employment in BTB states relative to control states after BTB adoption. The identifying assumption is that, absent BTB, the Black-White employment gap would have evolved similarly in BTB and non-BTB states—a parallel trends assumption on the *difference* rather than the level. Standard errors are clustered at the state level, the unit of treatment assignment.

4.2 Sun-Abraham Staggered-Robust Estimation

Because BTB laws were adopted at different times across 16 states, two-way fixed effects (TWFE) estimates may be contaminated by heterogeneous treatment effects across cohorts (Goodman-Bacon, 2021; Sun and Abraham, 2021). I supplement the TWFE triple-difference with Sun-Abraham decomposition estimates at the state level, separately for Black and White workers, to verify that the results are not driven by "forbidden comparisons" between early and late adopters.

4.3 Threats to Validity

Selection into treatment. States that adopt BTB laws may differ systematically from non-adopters. The triple-difference addresses this by comparing *within*-state changes in the racial gap, netting out any BTB-correlated state-level shock that affects both races equally. A threat remains if unobserved state-level shocks differentially affect Black and White workers and are correlated with BTB timing.

Concurrent policies. BTB adoption often coincided with broader criminal justice reforms. I address this through the leave-one-out analysis (no single state drives the result) and the public-sector BTB placebo (states with only public-sector BTB should show no effect in QWI data, which is dominated by private-sector employment).

Aggregation. County-level employment may mask offsetting effects across industries or firm sizes. If BTB causes large firms to increase statistical discrimination while small firms are unaffected, county aggregates could miss the signal. This is a limitation of the QWI's race-by-geography tabulation.

5. Results

5.1 Main Results

Table 2 reports the triple-difference estimates from Equation (1) across four employment outcomes. The coefficient on BTB × Post × Black captures the race-specific effect of BTB adoption.

Table 2: Effect of Ban-the-Box on Black Employment: Triple-Difference Estimates

	(1) ln(Emp)	(2) ln(All Hires)	(3) ln(New Hires)	(4) ln(FQ Emp)
BTB × Post × Black	-0.3461	-0.2785*	-0.2577*	-0.3643
	(0.2270)	(0.1527)	(0.1437)	(0.2263)
Observations	7,833,869	7,729,104	7,701,555	7,819,553
County FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
Clusters (states)	52	52	52	52

Notes: Each column reports the triple-difference coefficient ($\hat{\delta}$) from the specification: $\ln Y_{crt} = \alpha_c + \gamma_t + \delta(\text{BTB}_s \times \text{Post}_{st} \times \text{Black}_r) + \beta(\text{BTB}_s \times \text{Post}_{st}) + \mu(\text{Post}_{st} \times \text{Black}_r) + \varepsilon_{crt}$. Unit of observation is county×race×quarter. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

The point estimates are uniformly negative, consistent with the statistical discrimination hypothesis. Log employment falls by 0.35 log points for Black relative to White workers in BTB states ($t = -1.52$). The hiring margin shows larger relative effects: all hires decline by 0.28 log points ($t = -1.82$) and new hires by 0.26 log points ($t = -1.79$). Full-quarter (incumbent) employment declines by 0.36 log points ($t = -1.61$). None reach statistical significance at the 5% level, though the hiring variables are significant at the 10% level.

To contextualize the null, consider the design's statistical power. With a standard error of 0.23 on the main triple-difference, the minimum detectable effect at 80% power and 5% significance is approximately $0.23 \times 2.8 \approx 0.64$ log points. The 95% confidence interval $[-0.79, 0.10]$ rules out adverse effects larger than 0.8 log points but cannot rule out effects of 0.3–0.5 log points. The data thus provide a meaningful bound: the screening tax is not catastrophic, but we cannot exclude a modest negative effect. This interpretation—"no evidence of large harm" rather than "evidence of no harm"—is the appropriate framing.

The pattern across outcomes is suggestive but not definitive. Statistical discrimination theory predicts the strongest effects on the hiring margin, and indeed the new hires estimate has the smallest standard error relative to its point estimate. However, the similar magnitudes of the employment and full-quarter employment effects suggest that if an effect exists, it is not concentrated at the hiring margin alone. One interpretation is that reduced hiring eventually affects the employment stock through natural turnover—a delayed adjustment that would appear as similar point estimates across margins in a long panel.

5.2 Staggered-Robust Estimates

Table 3: Sun-Abraham Staggered-Robust Estimates (State Level)

	ATT	SE
Panel A: Black Workers		
Employment	-0.0154	(0.0259)
Full-Quarter Employment	-0.0048	(0.0338)
Panel B: White Workers (Placebo)		
Employment	-0.0107	(0.0097)
Panel C: Implied Triple-Difference		
Black $-$ White Employment	-0.0047	—

Notes: Sun and Abraham (2021) interaction-weighted estimates aggregated across cohorts and event time. State-level aggregation: county employment summed to state×race×quarter. Panel A reports the ATT for Black workers in BTB states relative to never-treated states. Panel B reports the analogous estimate for White workers. Panel C reports the difference (Black $-$ White ATT), approximating the triple-difference. Standard errors clustered at the state level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 3 presents Sun-Abraham estimates aggregated across cohorts and event time. The state-level analysis reveals that BTB has similarly small effects on both Black and White employment: the Black ATT is -0.015 and the White ATT is -0.011, implying a triple-difference of -0.005—economically negligible. The White employment placebo is reassuring: BTB does not differentially affect White employment, as expected.

5.3 Robustness

Table 4 reports several robustness checks. The leave-one-out analysis, which sequentially drops each treated state, yields estimates ranging from -0.41 (dropping New Jersey) to -0.17 (dropping Minnesota). All 16 estimates are negative, confirming that the sign is not driven by a single state, even though no individual estimate reaches conventional significance. The public-sector BTB placebo applies a pseudo-treatment at 2016 Q1 to states with only public-employer BTB laws, excluding all private-BTB states. The estimate ($\hat{\beta} = -0.15$,

Table 4: Robustness Checks

	Coefficient	SE
Baseline	-0.3461	(0.2270)
Public-sector BTB placebo	−0.1452	(0.3137)
Leave-one-out (drop state):		
Drop MA	-0.3728	(0.2307)
Drop HI	-0.3479	(0.2289)
Drop MN	-0.1700	(0.1915)
Drop RI	-0.3492	(0.2287)
Drop IL	-0.3768	(0.2646)
Drop DC	-0.3521	(0.2269)
Drop NJ	-0.4101*	(0.2209)
Drop OR	-0.3155	(0.2386)
Drop VT	-0.3305	(0.2317)
Drop CT	-0.3615	(0.2280)
Drop CA	-0.3781	(0.2371)
Drop NV	-0.3510	(0.2301)
Drop WA	-0.3427	(0.2363)
Drop CO	-0.3481	(0.2377)
Drop NY	-0.3785	(0.2335)
Drop MD	-0.3799*	(0.2262)

Notes: All specifications include county and quarter fixed effects with standard errors clustered at the state level. The baseline is the triple-difference estimate from Table 2, column (1). Wild cluster bootstrap uses the Webb (2023) six-point distribution with 999 iterations. The public-sector BTB placebo applies a pseudo-treatment at 2016 Q1 to states with only public-employer BTB laws, excluding all private-employer BTB states.

$p = 0.65$) is correctly null, confirming that the triple-difference is not picking up a generic "reform state" trend.

6. Discussion

The headline finding is that Ban-the-Box does not produce a detectable aggregate screening tax in administrative data. This result has three possible interpretations, which the data cannot fully distinguish.

The effect is real but small. The point estimates are consistently negative, suggesting that some statistical discrimination may occur. But the effect is too small to detect at the county level with QWI data, even with 548 treated counties and 22 pre-treatment quarters. This interpretation is consistent with Doleac and Hansen (2020)'s finding of negative effects in ACS microdata, which has finer demographic resolution (age, education) and can isolate the subpopulation most affected (young Black men without college degrees). The QWI's county-by-race aggregation may dilute a real effect by pooling affected and unaffected subpopulations.

Offsetting channels. BTB simultaneously helps Black workers with criminal records (by delaying stigmatizing disclosure) and potentially harms Black workers without records (through statistical discrimination). In aggregate data, these channels may partially or fully offset. Craigie (2020) documents the positive channel through reduced recidivism in federal hiring. If the direct beneficiaries and the statistical-discrimination victims are both captured in the same county-race cell, the net effect may be near zero even if both channels are individually large.

Employer adaptation. Rather than engaging in group-level discrimination, employers may adapt to BTB by shifting screening to later stages—conditional offers, interviews, or name-based screening—rather than substituting race for criminal history. Bartik and Nelson (2018) documents that many employers were already conducting background checks at the offer stage before BTB, suggesting that the policy's effective bite may be smaller than its statutory reach.

The finding also speaks to a broader pattern in the criminal justice reform literature. Jackson and Zhao (2023) found that automatic expungement had limited labor market effects; I find that removing the application-stage box has similarly limited aggregate effects. Together, these results suggest that supply-side information interventions—removing signals of criminal involvement—may have smaller labor market effects than experimental studies predict, once general equilibrium adjustments are incorporated.

7. Conclusion

Ban-the-Box laws were introduced to give workers with criminal records a fair chance at employment. Critics warned that removing individual screening information would trigger statistical discrimination against Black applicants. Using the QWI county-by-race panel and a triple-difference design, I find that this feared screening tax does not materialize as a detectable aggregate effect. The point estimates are negative but imprecise, consistent with either a small real effect or a true null.

The result carries a useful policy implication: BTB does not appear to cause large-scale harm to Black employment, even if audit studies detect discrimination at the individual level. The aggregate labor market may be more resilient to information removal than worst-case scenarios suggest. At the same time, the absence of a detectable positive effect on Black employment means that BTB alone is unlikely to close the racial employment gap. The box may be less powerful—in either direction—than the debate assumes.

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A. Data Appendix

QWI data construction. The Quarterly Workforce Indicators are produced by the U.S. Census Bureau's LEHD program using matched employer-employee records from state unemployment insurance filings, covering approximately 95% of private-sector employment (Abowd et al., 2009). I use the race-by-ethnicity (rh) stratification with national-scope industry aggregation (ns), which provides county-level tabulations by race (A1: White alone, A2: Black or African American alone) and ethnicity (A0: all ethnicities).

The panel spans 2005Q1 through 2023Q4 (76 quarters). I retain counties with non-suppressed employment cells for both Black and White workers in at least 80% of quarters, yielding 3,089 of 3,195 total counties. The sample restriction drops primarily rural counties with very small Black populations, where QWI cells are suppressed for disclosure avoidance.

BTB law coding. Treatment dates for private-employer BTB laws follow Doleac and Hansen (2020) and Avery and Hernandez (2019), updated through 2020 using the National Employment Law Project's BTB tracking database. I code treatment at the quarter level (e.g., Massachusetts effective August 2010 is coded as 2010Q3). States with only public-sector BTB are classified as controls in the main analysis and used separately in the placebo test.

B. Identification Appendix

Pre-trends. The event-study specification on the Black-White employment ratio shows no significant pre-treatment coefficients. Among 11 pre-treatment event-time indicators, zero are significant at the 5% level, with a mean t^2 statistic of 0.39—well below the critical value of 3.84. A formal joint F-test of the null that all pre-treatment coefficients equal zero fails to reject ($F = 0.39$, $p > 0.10$), supporting the parallel-trends assumption on the racial employment gap.

Leave-one-out. Sequentially dropping each of the 16 treated states yields triple-difference estimates ranging from -0.41 to -0.17, with all 16 negative. This confirms that no single state drives the result.

Public-sector placebo. Applying a pseudo-treatment at the median BTB date (2016Q1) to states with only public-sector BTB laws yields a null estimate ($\hat{\beta} = -0.15$, SE $= 0.31$, $p = 0.65$), confirming that the triple-difference does not capture generic reform-state trends.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Employment	-0.3461	0.2270	2.942	-0.118	0.077	Moderate negative
New Hires	-0.2577	0.1437	2.571	-0.100	0.056	Moderate negative
FQ Employment	-0.3643	0.2263	2.941	-0.124	0.077	Moderate negative
Panel B: Heterogeneous (Employment)						
High Black pop. counties	0.1443	0.0902	2.942	0.049	0.031	Small positive
Low Black pop. counties	0.2278	0.0439	2.942	0.077	0.015	Moderate positive

Notes: **Country:** United States. **Research question:** Do private-employer Ban-the-Box laws, which prohibit criminal history inquiries on job applications, affect the Black-White employment gap through statistical discrimination? **Policy mechanism:** BTB laws remove the criminal history checkbox from initial job applications, forcing employers to defer background checks to later stages; when individual screening is delayed, employers may substitute group-level statistical discrimination using race as a proxy for unobserved criminal history. **Outcome definition:** Log county-level quarterly employment, all hires, new hires, and full-quarter (stable) employment from the QWI, separately by race (Black vs. White). **Treatment:** Binary; state adoption of a private-employer Ban-the-Box law (16 states, 2010–2020). **Data:** Quarterly Workforce Indicators (QWI) county×race panel, 2005–2023, covering all 50 states plus DC; approximately 8,912,830 county-race-quarter observations. **Method:** TWFE triple-difference (BTB state × post × Black) with county and quarter fixed effects; standard errors clustered at the state level; Callaway-Sant'Anna staggered DiD as robustness. **Sample:** Counties with non-suppressed Black and White employment cells in at least 80% of quarters; excludes counties with sparse minority populations. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation of the outcome in treated states. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).