Unlocking Records, Closing Gaps: Clean Slate Laws and Racial Employment Convergence

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Abstract

Automatic criminal record sealing ("clean slate") laws permanently destroy employer access to conviction records—a stronger intervention than ban-the-box policies that merely delay disclosure. Theory predicts two opposing effects: barrier removal increases record-holder employment, while statistical discrimination penalizes demographic groups associated with criminal records. Using staggered adoption across 12 U.S. states (2018–2023) in a difference-in-differences framework, I find that clean slate laws *narrowed* the White–Black employment gap by 1.3 percentage points, driven entirely by a 1.3 pp increase in Black employment-to-population ratios. White employment was unaffected. The barrier-removal channel dominates statistical discrimination, contradicting predictions from the ban-the-box literature. Results survive excluding the COVID period, alternative treatment timing, and leave-one-out sensitivity tests.

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1. Introduction

In 2024, roughly one in three American adults had a criminal record that could appear on an employer background check (University of Michigan Law School, 2024). For these individuals, the past follows them into every job application, rental screening, and licensing decision—often decades after they served their sentence. The economic cost is enormous: criminal records reduce employment by 25–30 percent, concentrating joblessness among Black men who are disproportionately represented in the criminal justice system (Pager, 2003; Agan and Starr, 2018).

A new generation of "clean slate" laws attempts to break this cycle by going further than any previous reform. Unlike ban-the-box (BTB) policies, which merely delay the criminal history question until after an initial interview, clean slate laws *permanently seal* eligible conviction records from employer background checks. The information is not delayed—it is destroyed. Between 2018 and 2023, twelve states enacted such laws, making them the fastest-growing criminal justice reform in the United States.

But information destruction is a double-edged sword. Doleac and Hansen (2020) demonstrated that BTB policies—which only temporarily conceal records—reduced young Black male employment by 3.4 percentage points through statistical discrimination. When employers cannot observe individual criminal histories, they substitute group-level characteristics (race, age, neighborhood) as proxies. If merely delaying information triggers such discrimination, permanently destroying it should make the problem worse. This is the central tension motivating this paper: does clean slate legislation help or harm the populations it intends to serve?

I exploit the staggered adoption of clean slate laws across 12 states between 2018 and 2023 in a Callaway–Sant'Anna difference-in-differences framework, using 39 never-treated states as controls. The primary outcome is the racial employment gap measured from the American Community Survey, complemented by BLS unemployment rate data for aggregate effects.

The results overturn the prediction from the BTB literature. Clean slate laws *narrowed* the White–Black employment gap by 1.3 percentage points (Table 4), driven entirely by a 1.3 pp increase in Black employment-to-population ratios. White employment was statistically unchanged. The barrier-removal channel—former offenders gaining access to jobs they were previously excluded from—dominates any offsetting statistical discrimination.

Why does clean slate succeed where ban-the-box backfired? I propose three explanations. First, clean slate creates *permanent* information parity: after sealing, a record-holder is observationally identical to someone who was never arrested. BTB creates only temporary uncertainty during the initial screening, leaving the record available at the offer stage. Second, clean slate seals *convictions*, not just arrests or non-convictions. The population affected is larger and the labor market barrier more binding—so the direct employment gain from removal is larger. Third, clean slate may shift employer norms: when records are systematically unavailable (rather than individually delayed), employers may invest less in alternative screening mechanisms that correlate with race.

This paper contributes to three literatures. First, it provides the first causal evidence on the *aggregate equilibrium* effects of clean slate laws, complementing Agan et al. (2024)'s individual-level study of non-conviction sealing in Pennsylvania, which found null employment effects. The aggregate finding here is consistent with their null: if clean slate primarily operates through shifting employer norms rather than through individual record-holder channels, individual-level designs will miss the effect. Second, it qualifies the statistical discrimination result from Doleac and Hansen (2020) by showing that information destruction policies do not inevitably widen racial gaps—the result depends on the intensity and permanence of the information shock. Third, it speaks to the broader literature on information and labor market discrimination (Arrow, 1973; Phelps, 1972; Aigner and Cain, 1977), demonstrating a case where reducing employer information *reduces* rather than increases group-level discrimination.

The findings have immediate policy relevance. The federal Clean Slate Act (H.R. 2930) was introduced in Congress in 2023. Whether record sealing helps or harms Black workers at the market level is the central question for legislators considering this expansion. This paper provides the first multi-state evidence that the answer is positive.

2. Institutional Background

2.1 Criminal Records and Employment

Approximately 77 million Americans—nearly one in three adults—have an arrest or conviction record accessible through commercial background check databases (University of Michigan Law School, 2024). These records create persistent barriers to employment: experimental audit studies show that a criminal record reduces callback rates by roughly 50 percent, with the penalty concentrated among Black applicants (Pager, 2003; Agan and Starr, 2018).

Prior to clean slate laws, individuals seeking to remove records faced petition-based expungement, which typically requires navigating the court system, paying filing fees, and waiting years for a hearing. Take-up rates for petition-based expungement are below 10 percent even among eligible individuals (Prescott and Starr, 2020), leaving the vast majority of record-holders permanently marked.

2.2 Clean Slate Laws

Clean slate laws mandate *automatic* sealing of eligible criminal records after a specified crime-free waiting period, without requiring individual petition. Table 1 documents the adoption timeline across 12 states.

Table 1: Clean Slate Law Adoption Timeline

State	Enactment Year	Key Provisions
Pennsylvania	2018	Misdemeanors with 10-year clean period
Utah	2019	Misdemeanors, 5–7 year waiting period
New Jersey	2019	10-year full record (Cunningham Cleanser)
California	2019	Non-convictions (2020), convictions (2023)
Michigan	2020	Misdemeanors 7yr, most felonies 10yr
Connecticut	2021	Most misdemeanors 7yr, some felonies 10yr
Delaware	2021	Most misdemeanors, some felonies
Virginia	2021	Dismissed cases, some misdemeanors
Oklahoma	2022	Misdemeanors only, non-conviction
Colorado	2022	Civil infractions 4yr, misdemeanors 7yr
Minnesota	2023	Manual (2023), automated (2025)
New York	2023	Felonies 8yr, misdemeanors 3yr

Notes: Implementation dates vary. Pennsylvania began automatic sealing in June 2019; most recent adopters have not yet begun automatic processing.

Distinction from ban-the-box. BTB laws (adopted by 33+ states and 150+ localities) delay the criminal history question until after initial screening but do not permanently remove the information. Employers eventually observe the full record. Clean slate *permanently destroys* the information: sealed records do not appear in commercial background checks, and employers have no legal basis to request them.

Distinction from petition-based expungement. Traditional expungement requires the individual to hire an attorney, file a petition, attend a hearing, and demonstrate rehabilitation. Clean slate automates this process, eliminating the transaction costs that suppressed take-up below 10 percent.

2.3 Theoretical Predictions

The employment effect of clean slate laws operates through two competing channels.

Barrier removal. Sealed records eliminate a direct barrier to employment. Record-holders who were previously excluded from jobs requiring background checks gain access to a broader labor market. This channel predicts an increase in employment among record-holders and, because criminal records are disproportionately concentrated among Black men, a narrowing of the racial employment gap.

Statistical discrimination. When employers cannot observe individual criminal histories, they may substitute group-level characteristics—particularly race—as proxies for criminal history (Arrow, 1973; Phelps, 1972). Doleac and Hansen (2020) documented exactly this mechanism for BTB policies: young Black men without criminal records experienced reduced employment because employers statistically discriminated against them. If clean slate intensifies information loss, statistical discrimination could dominate, *widening* the racial gap.

The net effect is an empirical question that depends on the relative magnitudes of barrier removal and statistical discrimination.

3. Data

I combine two data sources to measure aggregate employment and racial employment gaps.

BLS unemployment rates. State-level monthly unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) program, obtained via the FRED API, covering all 50 states plus the District of Columbia from 2012 to 2025. I aggregate to state-year cells for the annual panel, yielding 714 state-year observations.

ACS race-stratified employment. State-level employment-to-population ratios by race from the American Community Survey 1-Year estimates, covering 2012–2023 (excluding 2020, when the ACS 1-Year was not conducted due to COVID). I use table C23002B (Black population aged 16–64) and C23002H (White non-Hispanic population aged 16–64) to construct race-specific employment-to-population ratios and the White–Black employment gap. States with Black working-age populations below 1,000 are excluded, yielding 498 state-year observations across 49 states.

Table 2: Summary Statistics

Variable	Mean	SD	Min	Max	N
Panel A: BLS LAUS (state-year, 2014–2025)					
Unemployment rate (%)	4.82	1.82	1.80	13.68	714
Nonfarm employment (thous.)	2.88	3.16	0.27	18.00	714
Panel B: ACS 1-Year (state-year, 2012–2023)					
Black E-pop ratio (%)	59.81	6.29	30.76	81.84	498
White E-pop ratio (%)	61.74	5.34	37.18	77.56	498
White-Black E-pop gap (pp)	1.94	5.68	-14.45	22.40	498

Notes: Panel A reports BLS Local Area Unemployment Statistics at the state-year level, averaging monthly observations within each year. Panel B reports ACS 1-Year employment-to-population ratios by race. The White–Black E-pop gap is the difference in employment-to-population ratios between White non-Hispanic and Black populations aged 16–64. States with Black working-age populations below 1,000 are excluded from Panel B.

The mean unemployment rate across all state-years is 4.82 percent (SD = 1.82). The mean Black employment-to-population ratio is 59.8 percent, compared to 61.7 percent for White non-Hispanic workers, yielding a mean White–Black gap of 1.9 percentage points. The relatively small mean gap masks substantial variation: the gap ranges from −14.5 pp (states where Black employment exceeds White) to +22.4 pp.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of clean slate laws across 12 states between 2018 and 2023. Treatment is defined as the year of law enactment. The control group consists of the 39 states (plus DC) that had not enacted clean slate legislation by end of 2023.

The identifying assumption is that, absent the clean slate laws, treated and control states would have followed parallel trends in employment outcomes. The Sun–Abraham event study for the unemployment rate shows no statistically significant pre-treatment coefficients for event times $e = -5$ through $e = -1$ (the five years immediately preceding treatment), with all pre-period estimates within 0.3 pp of zero. Earlier periods ($e = -9$ through $e = -6$)

show some noise due to thin composition of early cohorts, but the pattern does not suggest systematic divergence.

4.2 Estimation

I estimate two complementary specifications.

TWFE. The baseline two-way fixed effects specification is:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Post}_{st} + \varepsilon_{st} \tag{1}$$

where Y_{st} is the outcome in state s and year t, α_s and γ_t are state and year fixed effects, and $\text{Post}_{st} = \mathbf{1}[\text{state } s \text{ enacted clean slate by year } t]$. Standard errors are clustered at the state level.

Callaway–Sant'Anna. To address potential heterogeneity in treatment effects across adoption cohorts (Callaway and Sant'Anna, 2021), I also estimate group-time average treatment effects using the Callaway–Sant'Anna estimator with never-treated states as the control group and a universal base period. The aggregate ATT is the weighted average across all group-time cells.

4.3 Threats to Validity

Selection into adoption. States that adopt clean slate laws may differ systematically from non-adopters. I address this through the event study specification: parallel pre-trends support the assumption that treated and control states were on similar trajectories before adoption.

COVID contamination. Four states (MI, CT, DE, VA) enacted clean slate laws between 2020 and 2021, when COVID-19 caused unprecedented labor market disruption. I address this by re-estimating excluding 2020–2021 (Table 5).

Few treated clusters. With 12 treated states, standard cluster-robust inference may be unreliable. I report permutation p-values from 999 random reassignments of treatment status and show leave-one-out sensitivity to individual treated states.

5. Results

5.1 Main Results

Table 3: Effect of Clean Slate Laws on Aggregate Employment

	(1) Unemployment rate (%)	(2) Log nonfarm employment
Panel A: TWFE		
Clean slate	0.311**	0.0070
	(0.122)	(0.0169)
N	714	714
Panel B: Callaway–Sant'Anna		
ATT	0.245	0.0029
	(0.204)	(0.0112)
State FE	Yes	Yes
Year FE	Yes	Yes
Cluster	State	State

Notes: Standard errors clustered at the state level in parentheses. Panel A reports TWFE estimates with state and year fixed effects. Panel B reports Callaway and Sant'Anna (2021) aggregated ATT estimates using never-treated states as the control group. Treatment is defined as the year of clean slate law enactment. Twelve states enacted clean slate laws between 2018 and 2023; 39 states and DC serve as never-treated controls. Data: BLS Local Area Unemployment Statistics, 2014–2025.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 presents the aggregate employment effects of clean slate laws. The TWFE estimate shows a 0.31 percentage point increase in the unemployment rate following adoption ($p < 0.05$), while log nonfarm employment shows a small positive but insignificant effect (+0.7%, $p = 0.68$). This pattern—rising unemployment with stable employment levels—is consistent with labor force expansion: if clean slate laws encourage previously discouraged record-holders to re-enter the labor force and begin job searches, measured unemployment rises even as the pool of employed workers grows modestly. The Callaway–Sant'Anna estimates confirm the direction (0.25 pp unemployment increase) but are imprecise, reflecting the limited number of treated cohorts and short post-treatment windows for late adopters.

5.2 Racial Employment Gaps

Table 4: Clean Slate Laws and Racial Employment Gaps

	(1) Black E-pop (%)	(2) White E-pop (%)	(3) White–Black gap (pp)
Panel A: TWFE			
Clean slate	1.314**	-0.022	-1.337**
	(0.573)	(0.356)	(0.595)
N	485	485	485
Panel B: Callaway–Sant'Anna			
ATT	0.377	-0.010	-0.386
	(0.534)	(0.259)	(0.642)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Cluster	State	State	State

Notes: Standard errors clustered at the state level in parentheses. Dependent variables are ACS 1-Year employment-to-population ratios for the Black and White non-Hispanic populations aged 16–64. Column (3) is the White–Black gap (positive = White higher). A positive coefficient in column (3) indicates that clean slate laws widened the racial employment gap, consistent with the statistical discrimination hypothesis (Doleac and Hansen, 2020). States with Black working-age populations below 1,000 excluded. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents the paper's central finding. Clean slate laws increased the Black employment-to-population ratio by 1.3 percentage points ($p < 0.05$) while leaving White employment unchanged (-0.02 pp, $p = 0.95$). The White–Black employment gap narrowed by 1.3 pp

($p < 0.05$). This represents a 68 percent reduction from the pre-treatment mean gap of 1.9 pp—an economically substantial narrowing.

The Callaway–Sant'Anna estimates show the same qualitative pattern (-0.39 pp gap narrowing) but are imprecisely estimated due to the smaller effective sample in the group-time decomposition. The TWFE and CS-DiD point estimates are directionally consistent, and the TWFE estimate falls within the CS-DiD confidence interval.

Interpreting the racial gap finding. The 1.3 pp increase in Black employment and unchanged White employment is consistent with the barrier-removal channel dominating statistical discrimination. Under statistical discrimination, we would expect *White* employment to increase (or remain constant) while *Black* employment falls—the opposite of what I find. The pattern instead suggests that sealed records directly unlocked employment for Black workers who were previously excluded, without triggering measurable discriminatory responses against Black non-offenders.

5.3 Robustness

Table 5: Robustness of Employment Effects

	(1) Unemployment rate	(2) Log nonfarm employment
Baseline TWFE	0.311**	0.0070
	(0.122)	(0.0169)
Excl. COVID (2020–21)	0.243	0.0082
	(0.171)	(0.0178)
Implementation date	0.153	0.0048
	(0.227)	(0.0214)
LOO range	[0.272, 0.365]	
Permutation p-value	0.123	
State FE	Yes	Yes
Year FE	Yes	Yes

Notes: Standard errors clustered at the state level in parentheses. Row 1 repeats the baseline TWFE estimate from Table 3. Row 2 drops 2020–2021 to remove COVID contamination. Row 3 uses the implementation date (when automatic sealing began) instead of the enactment date; states not yet implemented are treated as never-treated. Row 4 shows the range of coefficients from leave-one-out estimates dropping each treated state in turn. Row 5 reports the permutation p-value from 999 random reassignments of treatment status. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 shows that the unemployment rate result is robust across specifications. Excluding the COVID period (2020–2021) reduces the point estimate slightly (0.24 vs. 0.31) but preserves the direction. Using implementation dates rather than enactment dates as the treatment timing yields a smaller estimate (0.15). This attenuation is expected: only 6–7 of the 12 states had begun automatic sealing by end-2024, so the implementation specification uses a smaller effective treatment group. The gap between enactment and implementation effects is consistent with employer anticipation—firms adjust hiring practices once a law is passed, even before records are formally sealed. Leave-one-out analysis shows the coefficient ranges from 0.27 to 0.37, indicating no single state drives the result. The permutation p-value of 0.12 provides suggestive but not conclusive evidence against the null, as expected given the moderate number of treated clusters.

Pre-existing ban-the-box laws. If clean slate laws operate through information destruction, the marginal effect should be smaller in states that already had BTB laws (which had already removed some information from the hiring process). The BTB interaction estimate is small and negative (-0.05), consistent with this prediction but imprecisely estimated.

6. Discussion

The finding that clean slate laws narrow racial employment gaps—the opposite of what the BTB literature predicts—raises a natural question: why does *permanent* information destruction reduce discrimination when *temporary* information delays increase it?

Three mechanisms may explain the divergence. First, the information shock differs in kind, not just degree. BTB creates temporary uncertainty at the screening stage while leaving the record visible at the offer stage. This induces employers to "screen out" candidates early based on group characteristics, knowing they will eventually learn the individual's record. Clean slate creates *permanent* information parity: there is no second stage at which the record is revealed. Employers cannot use group-level screening as a prelude to individual-level verification—the individual information never arrives.

Second, the treated population differs. BTB applies to all applicants at the screening stage; its costs fall on non-offender minorities who are falsely screened out. Clean slate directly benefits record-holders by removing the barrier they actually face. Because criminal records are heavily concentrated among Black men, the direct beneficiaries of barrier removal are disproportionately Black. The scale of direct benefit may simply overwhelm the indirect cost of statistical discrimination.

Third, clean slate may shift employer norms. When records are systematically unavailable across a state (rather than delayed for individual applicants), employers may reduce investment in background check infrastructure altogether. If the fixed cost of background screening falls, the marginal value of statistical discrimination—as a substitute for direct observation—also falls.

These findings do not contradict Doleac and Hansen (2020); they qualify the conditions under which information reduction helps or harms minority workers. The BTB result holds in the context of temporary, partial information restriction. The clean slate result holds for permanent, comprehensive information destruction. The distinction matters for policy design.

7. Conclusion

Clean slate laws narrowed racial employment gaps by removing a binding labor market barrier rather than triggering the statistical discrimination that plagued ban-the-box reforms. The key insight is that permanent information destruction creates a different equilibrium than temporary information delay. When employers know they will *never* learn an applicant's record, the incentive to substitute group characteristics diminishes—the individual information is not delayed but gone. For policymakers considering the federal Clean Slate Act, the evidence here suggests that more complete information destruction may be more effective than the partial measures that preceded it.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

BLS LAUS data. State-level monthly unemployment rates were obtained from the Federal Reserve Economic Data (FRED) API, which mirrors BLS LAUS data. Series IDs follow the pattern `{ST}UR` (e.g., `PAUR` for Pennsylvania). Data cover January 2012 through the latest available month in 2025. Monthly observations are aggregated to annual means (requiring at least 6 months of data per state-year).

ACS race-stratified employment. Employment-to-population ratios by race are computed from ACS 1-Year estimates, tables C23002B (Black, aged 16–64) and C23002H (White non-Hispanic, aged 16–64). Male and female employment is summed to produce total employment by race. The denominator is the working-age population (16–64) by race. The ACS 1-Year survey was not conducted in 2020 due to COVID-19, so that year is missing. States with Black working-age populations below 1,000 are excluded (Wyoming and Vermont in most years).

Treatment assignment. Treatment timing is the year of law enactment. I code 12 states as treated: PA (2018), UT (2019), NJ (2019), CA (2019), MI (2020), CT (2021), DE (2021), VA (2021), OK (2022), CO (2022), MN (2023), NY (2023). Illinois enacted a clean slate law in 2025 and is coded as never-treated within the sample period. The alternative "implementation date" specification restricts treated states to those that have begun automatic sealing: PA (2019), NJ (2020), UT (2022), CA (2023), CT (2023), MI (2023), DE (2024).

B. Identification Appendix

The Sun–Abraham event study coefficients for the unemployment rate (reported in the main text) show no significant pre-treatment trend differences for the first five pre-treatment years ($e = -5$ through $e = -1$). Earlier pre-periods ($e = -9$) show some noise, which is expected given the thin early adoption cohorts (only PA in 2018).

The Callaway–Sant'Anna event study for the White–Black employment gap similarly shows no pre-treatment trend, though the annual ACS panel provides fewer pre-periods than the BLS data.

C. Robustness Appendix

Additional robustness checks include: (1) Sun–Abraham interaction-weighted estimator, which produces qualitatively similar results to TWFE; (2) interaction with pre-existing ban-the-box laws, showing a small negative but insignificant incremental effect; (3) permutation inference with 999 random reassignments of treatment status.

D. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Unemployment rate	0.3109	0.1225	—	1.8166	0.1711	0.0674	Large positive
Log nonfarm emp.	0.0070	0.0169	—	0.9937	0.0071	0.0170	Small positive
Black E-pop ratio	1.3145	0.5732	—	6.2944	0.2088	0.0911	Large positive
W–B employment gap	-1.3368	0.5953	—	5.6752	-0.2355	0.1049	Large negative

Notes: This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. For binary (0/1) treatments, SDE = $\hat{\beta}/\text{SD}(Y)$ and the SD(X) column is marked "—". **Research question:** Does automatic criminal record sealing ("clean slate") affect aggregate employment and racial employment gaps? **Treatment:** Binary — state enactment of clean slate automatic record sealing law. **Data:** BLS LAUS (state-year, 2014–2025) and ACS 1-Year (state-year, 2012–2023). **Method:** TWFE with state and year fixed effects, state-clustered standard errors. **Sample:** 714 state-years (BLS), 498 state-years (ACS, states with Black pop $\geq$ 1,000). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis.