Do Recreational Cannabis Laws Narrow the Black/White Gap in Marijuana Arrests? Evidence from Staggered Difference-in-Differences

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Abstract

Racial disparities in marijuana enforcement have become a leading justification for recreational cannabis legalization, because a possession arrest is not only a criminal-justice event but an economic one: it can trigger pretrial detention, a lasting criminal record, and the well-documented penalties that records impose on employment, earnings, housing, and credit. We ask whether legalization narrows the Black/White gap in marijuana arrest rates, treating the disparity ratio itself as the estimand rather than the arrest level of any single group. Using the FBI Uniform Crime Reporting "Arrests by Age, Sex, and Race" master files linked to Census race-specific population denominators, we build a jurisdiction-by-year panel spanning 2015 to 2020 that covers the 50 states and the District of Columbia, eleven of which adopted recreational cannabis laws during the window. We estimate staggered-adoption difference-in-differences models using the Callaway and Sant'Anna (2021) and Sun and Abraham (2021) estimators, which are robust to treatment-effect heterogeneity, alongside two-way fixed-effects benchmarks. Legalization reduces the log Black/White ratio of marijuana-possession arrests by roughly 0.34 to 0.39 log points, a narrowing of about 29 to 32 percent that is statistically significant and stable across estimators, control-group definitions, leave-one-state-out re-estimation, and controls for reporting composition. Placebo outcomes, driving under the influence and larceny-theft, show no comparable change. Decomposing the ratio, arrest rates fall for both groups but proportionally more for Black residents, so the gap narrows through compression of a very large Black arrest rate rather than through equalization. Our conclusion, that legalization narrows but does not close the possession gap and leaves broader drug-enforcement disparities intact, independently corroborates the comprehensive national estimates of Meinhofer, Rubli, and Cunningham (2026) through a deliberately different and transparent research design. We draw out the implications for cannabis policy as racial-equity and economic-mobility policy.

JEL: K42, I18, J15, J78

Introduction

Marijuana enforcement in the United States has long been marked by pronounced racial disparities. Survey evidence consistently indicates that Black and White Americans use marijuana at broadly similar rates, yet Black Americans have historically been arrested for possession at several times the rate of White Americans (Geller and Fagan 2010; Pierson et al. 2020). From an economic standpoint the arrest itself is only the first link in a longer causal chain. A possession arrest can lead to pretrial detention, fines and fees, and a criminal record, and a growing body of evidence shows that criminal records impose large and racially uneven costs in the labor market. Pager (2003) finds in an audit study that a criminal record sharply reduces employer callbacks and that the penalty is substantially larger for Black applicants, and Agan and Starr (2018) show experimentally that records are a major barrier to employment and that concealing them can widen the Black/White callback gap as employers substitute toward race-based inference. Because records also restrict access to housing, credit, occupational licenses, and public benefits, a disparity in low-level drug enforcement propagates into disparities in employment, earnings, and wealth, and, through household resources, into the opportunities of the next generation. The racial gap in marijuana arrests is therefore not only a criminal-justice concern but a question about the distribution of economic opportunity, which places it squarely within the scope of applied economics.

This economic logic is central to the policy debate. Advocates of recreational cannabis legalization argue that removing possession from the ambit of criminal law should mechanically shrink the enforcement disparity that possession generates, and many states have written this rationale, together with companion provisions for expungement and community reinvestment, directly into their statutes. Whether legalization delivers on the promise is an empirical question whose answer is not obvious in advance. Legalization changes the statutory status of possession but leaves in place a large enforcement apparatus, continued prohibition of sale and manufacture outside the licensed market, and rules on quantity, age, and public consumption, each of which can sustain discretionary enforcement that falls unevenly across groups. It is therefore possible for legalization to lower arrest levels for everyone while leaving the relative gap intact, or even to widen the relative gap if the offenses that remain illegal are enforced in a more racially skewed way than simple possession was.

This paper studies the effect of state recreational cannabis legalization on the Black/White disparity in marijuana arrest rates. We make three design choices that together define our contribution. First, we treat the racial disparity ratio itself as the object of analysis rather than studying arrest levels for a single group. Because the policy debate is framed explicitly in relative terms, the ratio is the policy-relevant estimand, and it cannot be read off a single group-specific regression. Second, we distinguish marijuana *possession* from *sale and manufacture*, which legalization treats very differently, and we separate marijuana offenses from the broader category of drug-abuse violations, which lets us test whether any effect is specific to cannabis or reflects a general shift in drug enforcement; we reinforce this with two non-drug placebo offenses that have no statutory connection to cannabis. Third, we bring heterogeneity-robust difference-in-differences estimators to bear on a staggered-adoption setting in which conventional two-way fixed-effects (TWFE) estimators are known to be biased (Goodman-Bacon 2021; Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021).

We are not the first to study this question. Meinhofer, Rubli, and Cunningham (2026) provide the most comprehensive national analysis to date, estimating the direct and spillover effects of legalization across arrests, incarcerations, and hospitalizations, and concluding that legalization reduces cannabis arrests for both Black and White populations while narrowing but not eliminating racial disparities. We view our contribution as complementary rather than duplicative. We isolate a single, transparent estimand, the population-based log arrest-rate ratio for possession; we subject it to a falsification design built on offense-specific and placebo comparisons; and we show that the result survives estimators, control groups, weighting, and controls for the composition of voluntary reporting. That an independent design on a different sample window reaches the same qualitative conclusion strengthens confidence in a literature where some jurisdiction-specific studies have instead found disparities that persist or widen (Firth et al. 2019; Sheehan, Grucza, and Plunk 2021).

We assemble a jurisdiction-by-year panel spanning 2015 through 2020, linking arrest counts by race and offense from the FBI's Uniform Crime Reporting (UCR) program to race-specific population denominators from the U.S. Census Bureau. Eleven states adopted recreational cannabis laws with effective dates inside or just before our sample window, which provides the staggered treatment timing we exploit for identification. Our primary outcome is the log ratio of Black to White age-aggregated arrest rates for marijuana possession.

Our central finding is that legalization narrows the possession disparity by approximately 0.34 to 0.39 log points, corresponding to a reduction of roughly 29 to 32 percent in the Black/White arrest-rate ratio. The estimate is stable across the Callaway and Sant'Anna (2021) and Sun and Abraham (2021) estimators, alternative control-group definitions, population weighting, leave-one-state-out re-estimation, and controls for the number and coverage of reporting agencies. It is not accompanied by comparable movements in the placebo outcomes. A decomposition shows that arrest rates decline for both groups after legalization, but proportionally more for Black residents, so the disparity narrows through compression of a large Black arrest rate rather than through equalization of the two rates. At the same time, the effect is concentrated in possession: disparities in sale and manufacture arrests, which remain illegal, and in drug enforcement more broadly are not significantly reduced, and the effect is smaller in the most populous states, so the nationally aggregated equity gain is more modest than an unweighted average would suggest.

These results qualify, without overturning, the case for legalization as racial-equity policy. Legalization substantially reduces the enforcement gap it is invoked to address, and because that gap is a gateway to criminal records, the reduction plausibly carries downstream economic benefits for the group most exposed to enforcement. But the gap narrows rather than closes, and the improvement does not extend to adjacent offenses, which implies that legalization is best understood as one component of an equity agenda rather than a complete remedy. The remainder of the paper proceeds as follows. Section 2 sets out the institutional context and situates the paper in the literature. Section 3 describes the data and the construction of the disparity outcome. Section 4 lays out the identification strategy and discusses the controls we can and cannot credibly add. Section 5 presents the results, and Section 6 discusses economic significance, mechanisms, external validity, limitations, and policy implications.

Background and Institutional Context

Marijuana enforcement, racial disparity, and its economic consequences

A large body of research documents that marijuana possession arrests fall disproportionately on Black Americans despite comparable use rates (Geller and Fagan 2010). Pierson et al. (2020), analyzing nearly one hundred million traffic stops, and Knox, Lowe, and Mummolo (2020), who formalize the inferential problems created by conditioning on a police-initiated stop, together establish both the scale of racial disparities in policing and the care required to interpret administrative enforcement records. Sampson and Lauritsen (1997) provide foundational context on disparities across the criminal-justice system as a whole. The stylized fact that motivates our analysis is not that Black Americans use marijuana more, but that, conditional on similar use, they are far more likely to be arrested for it. Possession enforcement is highly discretionary and is often a byproduct of proactive policing concentrated in particular neighborhoods, so the arrest gap reflects the geography and intensity of enforcement at least as much as any difference in underlying behavior.

The economic stakes follow from what an arrest sets in motion. A criminal record is a durable signal in thin information environments such as hiring and tenant screening, and the evidence that it depresses economic outcomes, more so for Black applicants, is among the more robust findings in the economics of discrimination (Pager 2003; Agan and Starr 2018). Records also interact with access to credit, housing, occupational licensing, and student aid, and the resulting losses in employment and earnings reduce household resources in ways that can transmit across generations. Viewed through this lens, a policy that compresses the racial gap in possession arrests is a policy that reduces differential exposure to the record-generating event, and its equity value should be assessed not only at the point of arrest but along the economic chain it initiates.

What legalization does and does not change

Understanding the likely effect of legalization requires attention to what the statutes actually do. Recreational legalization removes criminal penalties for possession of small quantities by adults, but it does not create an unregulated market. Sale and manufacture outside the licensed system remain illegal, possession above statutory thresholds remains an offense, and age limits, public-consumption bans, and impaired-driving laws all remain in force. Legalization therefore eliminates the single most common cannabis charge while leaving intact a set of adjacent offenses that continue to be enforced with discretion. This institutional detail generates our central empirical distinctions. If legalization narrows the disparity, we expect the effect to be concentrated in possession, muted or absent for sale and manufacture, and absent for the broad drug-abuse-violation aggregate that mixes cannabis with other substances. It also motivates our placebo logic: offenses unrelated to cannabis, such as impaired driving and larceny-theft, are enforced by the same agencies in the same years but have no statutory link to a cannabis law, so any movement in their disparity ratios would signal a confounding change in reporting or general enforcement rather than a treatment effect.

Effects of cannabis legalization

Research on the consequences of legalization has focused primarily on public health and crime. Cerdá et al. (2020) document changes in use and cannabis-use disorder following legalization, and Pacula and Smart (2017) review the broader evidence base. On crime, Dragone et al. (2019) and Wu, Wen, and Wilson (2020) find limited or favorable effects on non-drug offending, while Lane and Hall (2019) examine traffic-safety consequences. A closer set of papers examines enforcement disparities directly, and its findings are mixed. Firth et al. (2019) study Washington State and find reductions in adult marijuana-arrest disparities, while Plunk et al. (2019) and Sheehan, Grucza, and Plunk (2021) examine possession arrests among youths and adults following decriminalization and legalization and report reductions that vary by age and by the timing of reform, alongside rising disparities in states that did not change policy; Martins et al. (2021) study racial and ethnic differences in use. Most directly, Meinhofer, Rubli, and Cunningham (2026) estimate direct and spillover effects across a wide range of criminal-legal outcomes and conclude that legalization narrows but does not eliminate disparities, with spillovers to broader arrests and incarcerations that are generally insignificant. Our paper differs from this work in its estimand and its falsification design: we analyze the population-based log disparity ratio directly, decompose it by offense, and validate it against non-drug placebos, providing an independent check on the emerging consensus.

Methodology of staggered difference-in-differences

Recent econometric work shows that the TWFE estimator of a staggered treatment recovers a variance-weighted average of group-time effects that can be severely biased when effects are heterogeneous, in part because already-treated units are used as controls for later-treated units (Goodman-Bacon 2021; Chaisemartin and D’Haultfœuille 2020). Because our treatment rolls out over six years and the effect of legalization plausibly differs across cohorts and evolves over time, this bias is a first-order concern. We therefore adopt the Callaway and Sant’Anna (2021) group-time estimator and the Sun and Abraham (2021) interaction-weighted event-study estimator as our primary specifications, and we report TWFE only as a benchmark. We assess pre-trends in the spirit of Rambachan and Roth (2023) and note the closely related contributions of Borusyak, Jaravel, and Spiess (2024) and Imai and Kim (2021).

Data

Arrests

Arrest counts come from the FBI UCR “Arrests by Age, Sex, and Race” (ASR) master files, the canonical national source for arrests disaggregated by offense and race. For each year we use the twelve-month reporter file, which restricts attention to agencies that reported for a full twelve months and thereby yields internally consistent annual totals. For each agency-offense record we sum juvenile and adult counts to obtain all-ages arrest totals by race. We then aggregate to the state level for six offense categories: marijuana possession, marijuana sale and manufacture, the total of all drug-abuse violations (sale plus possession subtotals across all substances), and two non-drug placebo offenses, driving under the influence and larceny-theft. The modern flat master-file format is available on a consistent basis from 2015 onward, so we begin the panel in 2015 and end in 2020 in order to avoid the 2021 transition from the UCR Summary Reporting System to the National Incident-Based Reporting System, during which many agencies stopped reporting under the old format.

Population denominators

Race-specific population denominators come from the Census Bureau's Vintage 2020 state characteristics population estimates (file SC-EST2020-ALLDATA6), which provide population by state, single race, sex, age, and Hispanic origin. We use single-race White and Black populations, summed over both sexes, all ages, and all Hispanic-origin categories, to construct arrest rates per 100,000 residents of each group. Constructing rates on a common denominator basis is essential, because the disparity ratio would otherwise conflate differences in enforcement with differences in the size and age structure of each group's population.

Treatment timing

We code a state as treated in the first full calendar year in which its recreational cannabis legalization statute was in effect. Eleven states in our panel have effective dates within or just before the window: Alaska and Oregon (2015); California and Massachusetts (2016); Nevada and Maine (2017); Michigan and Vermont (2018); Illinois (2020); and Colorado and Washington, which legalized in late 2012 and are therefore treated throughout the sample. The remaining thirty-nine states and the District of Columbia, forty jurisdictions in all, had not implemented recreational legalization by the end of the window and serve as the comparison group. This staggered timing is the source of identifying variation, and it is precisely the setting in which heterogeneity-robust estimators are needed.

Outcome construction and descriptive patterns

Our primary outcome is the log Black/White ratio of age-aggregated arrest rates,

$$y_{st} = \log\left(\frac{r_{st}^{B} + 0.5}{r_{st}^{W} + 0.5}\right),$$

where r_{st}^{B} and r_{st}^{W} are Black and White arrests per 100,000 in state s and year t, and the additive constant of one-half handles the small number of state-years with zero recorded counts without materially affecting states with typical arrest volumes. Working in logs means the estimated treatment effects are interpretable as approximate proportional changes in the disparity ratio, which is the natural scale for a question framed in relative terms. The resulting panel contains 304 state-year observations; it is very slightly unbalanced because a small number of state-years are missing from the twelve-month reporter files.

Table 1 reports pre-period descriptive statistics, computed as means over 2015 and 2016. Marijuana-possession arrest rates are roughly three times higher for Black than for White residents in both groups of states, which confirms the large baseline disparity that motivates the analysis; the median Black/White possession ratio is 2.89 in the eleven ever-legal states and 3.38 in the forty never-legal jurisdictions. Two features of the table deserve emphasis. First, ever-legal states enter the window with somewhat lower baseline disparity ratios and lower arrest levels than never-legal jurisdictions, which is exactly why a simple comparison of adopters to non-adopters would be misleading and why a within-jurisdiction design is required. Second, drug-total arrest rates are also markedly higher for Black residents, so the disparity we study is not unique to cannabis; the empirical question is whether legalization moves the cannabis-specific gap in particular.

Table 1
Pre-period descriptive statistics (2015 to 2016 means)

Variable (2015 to 2016 mean)	Ever-legal (11 states)	Never-legal (40 jurisdictions)
Black MJ-possession arrests / 100k	222.1	608.0
White MJ-possession arrests / 100k	71.8	160.4
Black/White possession ratio (median)	2.89	3.38
Black drug-total arrests / 100k	766.2	1165.5
White drug-total arrests / 100k	265.7	368.1
Black population (millions)	0.67	0.82
Reporting agencies (count)	245	299

Arrest rates are per 100,000 residents of the indicated race. Ever-legal states are the eleven that adopted recreational cannabis laws; never-legal jurisdictions are the remaining thirty-nine states and the District of Columbia (forty jurisdictions). Reporting agencies are twelve-month UCR reporters. Ratios are reported as medians because arrest-rate ratios are right-skewed.

Figure 1 plots population-weighted mean disparity ratios by treatment status over the sample window. In the possession panel, the Black/White ratio in ever-legal states declines relative to never-legal states: the never-legal series drifts upward while the ever-legal series falls back toward it, so the two converge by 2020. In the panel for all drug-abuse violations, by contrast, the two groups move roughly in parallel at very different levels, with no comparable convergence. This raw pattern previews the regression results and is consistent with a treatment effect specific to marijuana rather than a general change in drug enforcement. Because these are unadjusted means, we rely on the formal estimates below for inference, but the visual contrast between the two panels is informative.

Empirical Strategy

Estimators and identification

We estimate the average effect of legalization on treated states using the group-time average treatment effect framework of Callaway and Sant'Anna (2021). Let G_s denote the year state s becomes treated, with $G_s = 0$ for never-treated states. The building block is

$$ATT\left(g,t\right)=\mathbb{E}\left[y_{st}\left(g\right)-y_{st}\left(0\right)\mid\ G_{s}=g\right],$$

the average treatment effect for cohort g at time t, identified under a conditional parallel-trends assumption using never-treated states (and, in a robustness check, not-yet-treated states) as controls together with a universal base period. We aggregate the $ATT\left(g,t\right)$ into a single overall effect and into an event-study profile indexed by time relative to legalization. We complement this with the Sun and Abraham (2021) interaction-weighted estimator and a TWFE benchmark,

$$y_{st}=\alpha_{s}+\lambda_{t}+\beta\ \mathrm{Post}_{st}+\epsilon_{st},$$

where Post_{st} indicates that state s is legal in year t, α_{s} and λ_{t} are state and year fixed effects, and β is the TWFE benchmark. Standard errors are clustered at the state level, and the Callaway and Sant'Anna (2021) estimates use the multiplier bootstrap, which delivers uniform confidence bands for the event-study profile.

Identification rests on the assumption that, absent legalization, disparity ratios in adopting and non-adopting states would have evolved in parallel, conditional on state and year effects. The economic content of this assumption is that adopters and non-adopters were not already on divergent enforcement-disparity trajectories for reasons unrelated to the statute. We probe it in two complementary ways. First, we inspect the event-study pre-trends. Second, and because the pre-period is short for later adopters, we place substantial weight on the placebo outcomes: impaired driving and larceny-theft are enforced by the same agencies in the same years but have no statutory connection to cannabis, so a genuine cannabis-specific effect should leave their disparity ratios unchanged, whereas a confound operating through reporting composition or a general enforcement shift would tend to move them as well.

Which time-varying controls are appropriate?

An applied reader will ask whether state-level time-varying covariates, such as unemployment, income, poverty, police budgets, incarceration, demographic composition, or the partisan control of state government, should enter the specification. We take a deliberate position on this question rather than adding covariates mechanically. Two considerations govern the choice. The first is the bad-control problem: several candidate covariates are plausibly outcomes of legalization rather than pre-determined confounders. Police expenditures, incarceration counts, and arrests for other offenses can all respond to a cannabis statute, so conditioning on them would absorb part of the treatment effect and bias the estimate toward zero; these variables are excluded on principle. The second consideration is that the design already guards against the confounds that the admissible controls, local economic conditions in particular, would be meant to proxy. If legalization coincided with economic shocks that changed policing intensity in general, those shocks would move the placebo disparity ratios for impaired driving and larceny-theft; they do not. In this sense the placebo outcomes function as a joint control for time-varying local conditions that is agnostic about functional form.

We nonetheless implement the robustness check that is most specific to our data source. The primary threat in voluntary UCR data is not the macroeconomy but differential reporting: if the set of agencies contributing to a state's totals changes around legalization in a way correlated with the racial composition of enforcement, the disparity ratio could move mechanically. We therefore re-estimate the effect controlling for the log number of reporting agencies and the log coverage population, and weighting by these measures of reporting intensity, using only variables internal to the UCR file and pre-determined with respect to the racial disparity in cannabis enforcement. We report these checks in Section 5. We do not merge external macroeconomic panels: constructing them reliably at the state-year level would require vintage-matched series from Bureau of Labor Statistics local-area unemployment and Census small-area income and poverty estimates, and given the bad-control and placebo arguments above, the marginal identifying value would be low relative to the risk of introducing measurement error into a published result. We prefer to be explicit about this choice rather than to overstate the specification.

Results

Main estimates

Table 2 presents the main results. Recreational legalization reduces the log Black/White ratio of marijuana-possession arrests by 0.364 log points under the Callaway and Sant'Anna (2021) estimator (standard error 0.213) and by 0.390 under TWFE (standard error 0.148); the Sun and Abraham (2021) estimate, reported in the robustness table below, is 0.344 (standard error 0.102). These magnitudes correspond to a narrowing of the possession disparity ratio of roughly 29 to 32 percent. Effects on marijuana sale and manufacture are negative but imprecise (Callaway-Sant'Anna estimate -0.571, standard error 0.438), consistent with sale remaining illegal and with the small and noisy arrest counts in that category. Effects on the broad drug-abuse-violation ratio are small and statistically indistinguishable from zero under both estimators, indicating that the response is specific to marijuana rather than a general shift in drug enforcement; this echoes the generally insignificant spillovers to broader enforcement reported by Meinhofer, Rubli, and Cunningham (2026). Critically, the two placebo outcomes, driving under the influence and larceny-theft, show no significant change, exactly as expected if the design isolates cannabis-specific enforcement.

Table 2
Effect of recreational cannabis legalization on racial arrest disparities

2–3(lr)4–5 Outcome (log Black/White rate ratio)	Callaway-Sant'Anna		Two-way FE	
	ATT	SE	ATT	SE
Marijuana possession	-0.364^{*}	(0.213)	-0.390^{***}	(0.148)
Marijuana sale/manuf.	-0.571	(0.438)	-0.334	(0.212)
All drug-abuse violations	-0.057	(0.062)	-0.029	(0.062)
Driving under influence (placebo)	0.051	(0.037)	-0.002	(0.050)
Larceny-theft (placebo)	-0.060	(0.052)	-0.172	(0.105)

Outcome is the log Black/White arrest-rate ratio for each offense. The Callaway-Sant'Anna estimator uses never-treated controls, a universal base period, and the multiplier bootstrap; the reported standard error is the simultaneous (uniform) bootstrap standard error. Two-way FE clusters standard errors on state. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The economic significance of the possession estimate is substantial. A baseline ratio near three means Black residents are arrested for possession at about three times the White rate; a reduction of roughly 30 percent moves that ratio toward the low twos, a compression of one of the most visible disparities in American criminal justice. Because the possession arrest is a gateway to a criminal record, and because records carry the racially uneven labor-market penalties documented by Pager (2003) and Agan and Starr (2018), a narrowing of this magnitude plausibly translates into a meaningful reduction in differential exposure to the economic harms that follow an arrest, even though, as the mechanism analysis shows, the gap remains large after reform.

Event-study dynamics

Figure 2 plots the Callaway and Sant'Anna (2021) event-study estimates. We read these dynamic coefficients with caution and do not rest the paper's conclusions on any single one of them. The consistently formatted master files begin in 2015, so the pre-period is short for the later-adopting cohorts, and each event-time cell is identified from a small number of states; the year-one post-treatment coefficient for possession, in particular, is estimated so imprecisely that its confidence interval spans a wide range and is uninformative on its own. What the figure does support is modest and qualitative: the pre-treatment coefficients for possession do not trace a systematic trend of narrowing in the years before legalization, and the post-treatment coefficients are negative and consistent in sign with the aggregate estimate. For the broad drug-violation ratio in the right panel, there is no sustained post-legalization decline. The strongest evidence for our conclusion is not the dynamic path but the convergence of the aggregate estimates across three estimators, the stability under the robustness checks below, and the null placebo outcomes. We present the event study for transparency about dynamics and pre-trends, not as the load-bearing element of the identification.

Mechanism: whose arrests fall?

A narrowing of the ratio is consistent with two very different underlying stories with opposite welfare interpretations: the gap could close because Black arrests fall, or because White arrests rise. To distinguish them we estimate the effect of legalization on each group's log arrest rate separately, using the same Callaway-Sant'Anna design (Table 3). Legalization reduces the Black marijuana-possession arrest rate by 1.261 log points and the White rate by 0.897 log points, and both effects are highly significant. In level terms these coefficients imply that the Black possession arrest rate falls by roughly seventy percent and the White rate by roughly sixty percent. The disparity therefore narrows not because White arrests rise but because arrests fall for everyone and proportionally more for Black residents, compressing an initially much larger Black arrest rate. This is a genuine equity improvement rather than an artifact of one group's rate moving toward the other, and its direction matches the group-specific declines reported by Meinhofer, Rubli, and Cunningham (2026). Because the Black rate starts so much higher, however, a larger proportional decline still leaves a substantial absolute gap, which is why the disparity narrows rather than disappears.

Table 3
Mechanism: effect on group-specific marijuana-possession arrest rates

Outcome	ATT	SE
Black arrest rate (log)	-1.261^{***}	(0.315)
White arrest rate (log)	-0.897^{***}	(0.205)

Callaway-Sant'Anna estimates on the log arrest rate per 100,000 residents for each racial group. $^{***}p < 0.01$.

What the decomposition can and cannot tell us about channels

The decomposition establishes *that* arrests fall more for Black residents but not *why*, and it is important to separate the channels the data can speak to from those they cannot. The channel most directly supported by the evidence is the mechanical removal of the possession offense itself: when the most common cannabis charge is decriminalized, the arrests it generated disappear, and because those arrests were disproportionately of Black residents, their removal compresses the ratio. This reading is reinforced by the offense-specific pattern, in which the effect is concentrated in possession and absent in the broad drug aggregate. Other plausible channels that we can describe but not directly test include shifts in police resource allocation away from low-level cannabis stops, changes in enforcement priorities and in the use of the odor of cannabis as a pretext for searches, and prosecutorial declination of marginal cases. Companion equity provisions such as automatic expungement and reinvestment operate on the stock of past records rather than on the flow of new arrests, so they are unlikely to drive our flow-based estimates, although they matter greatly for the downstream economic consequences. We flag these as hypotheses consistent with the results rather than as findings, because a state-year arrest panel cannot adjudicate among them; doing so would require incident-level data on stops, searches, and charging decisions.

Robustness and falsification

Table 4 collects robustness checks for the possession-disparity outcome. Panel A varies the estimator, control group, weighting, and functional form. The estimate is essentially unchanged when we use not-yet-treated rather than never-treated states as controls (-0.367), and the Sun and Abraham (2021) estimate (-0.344, standard error 0.102) closely matches the headline figure. The result is not driven by any single influential state: dropping Colorado, California, Washington, or Illinois one at a time leaves the TWFE estimate in a tight band from -0.394 to -0.456, and if anything it becomes more negative when California is excluded. Population-weighted TWFE attenuates the estimate to -0.134 (standard error 0.117), which we read as genuine treatment-effect heterogeneity rather than fragility: the effect is smaller in the largest states, most notably California, so weighting by population down-weights the states with the largest narrowing, while the unweighted and cohort-robust estimators, which are not dominated by a few large states, agree closely. Expressing the outcome as the disparity ratio in levels rather than logs yields -0.687 (standard error 0.361), a decline of about seven-tenths of a point in the ratio itself, consistent in sign and significance with the log specification.

Panel B addresses the threat that is specific to voluntary UCR reporting, namely that the composition of reporting agencies changes around legalization. Controlling for the log number of reporting agencies leaves the estimate essentially unchanged (-0.407, standard error 0.148), and controlling for the log coverage population, which requires dropping the District of Columbia because its coverage denominator is unpopulated in the file, yields -0.372 (standard error 0.153). Restricting to jurisdictions that report in all six years gives -0.393 (standard error 0.149). Weighting by the number of reporting agencies attenuates the estimate to -0.174 (standard error 0.115), the same large-state pattern seen under population weighting, since populous states host more agencies. Taken together with the null placebo outcomes in Table 2, these checks indicate that the possession result is not an artifact of differential reporting and support a causal interpretation.

Table 4
Robustness checks (marijuana-possession disparity)

Specification	**ATT**	**SE**
Panel A. Estimator, controls, weighting, functional form		
CS not-yet-treated	-0.367^{**}	(0.173)
CS never-treated	-0.364^{**}	(0.172)
Sun-Abraham ATT	-0.344^{***}	(0.102)
TWFE pop-weighted	-0.134	(0.117)
TWFE drop CO	-0.395^{***}	(0.148)
TWFE drop CA	-0.456^{***}	(0.145)
TWFE drop WA	-0.394^{***}	(0.148)
TWFE drop IL	-0.396^{**}	(0.160)
TWFE ratio (level)	-0.687^{*}	(0.361)
TWFE + log reporting agencies	-0.407^{***}	(0.148)
TWFE + log coverage pop. (excl. DC)	-0.372^{**}	(0.153)
TWFE balanced panel (all 6 years)	-0.393^{***}	(0.149)
TWFE weighted by reporting agencies	-0.174	(0.115)

Each row re-estimates the effect on the Black/White possession-arrest disparity under an alternative specification. Rows use the log-ratio outcome except “TWFE ratio (level).” The two Callaway-Sant’Anna (CS) rows in Panel A report analytic pointwise standard errors, which is why the CS never-treated standard error (0.172) is smaller than the simultaneous bootstrap standard error for the same point estimate in Table 2 (0.213). Panel B rows are two-way fixed-effects specifications with state-clustered standard errors estimated on the same panel; the coverage-population row omits the District of Columbia, whose coverage denominator is unpopulated in the UCR file. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Discussion and Conclusion

Interpreting the effect

Recreational cannabis legalization substantially narrows the Black/White gap in marijuana-possession arrests, by roughly 29 to 32 percent in relative terms, and the result survives the estimators designed for staggered adoption, alternative control groups, leave-one-out re-estimation, and controls for reporting composition, while the placebo offenses are unaffected. This is a clear and policy-relevant finding, and it does not stand alone: it independently reproduces the qualitative conclusion of the most comprehensive national study, Meinhofer, Rubli, and Cunningham (2026), from a different sample window, a different outcome construction, and a falsification design built on offense-specific and placebo comparisons. The convergence of two quite different approaches is reassuring in a literature where some jurisdiction-specific analyses have found disparities that persist or widen.

Three features of the evidence temper an unqualified endorsement of legalization as racial-equity policy. First, the gap narrows but does not close. Arrest rates fall for both groups, so the compression comes from a proportionally larger decline in an initially much larger Black rate, and a substantial absolute disparity remains after reform. Second, the effect is concentrated in possession. Disparities in sale and manufacture arrests, which remain illegal, are not significantly reduced, and neither is the disparity in drug enforcement more broadly, where our estimates are close to zero, again consistent with the limited spillovers in Meinhofer, Rubli, and Cunningham (2026). Legalization thus addresses the specific channel it removes from criminal law and leaves adjacent channels largely intact. Third, the effect is smaller in the most populous states, so the nationally aggregated equity gain is more modest than the unweighted average would suggest.

Efficiency, equity, and economic mobility

Two economic lenses help interpret these findings. On efficiency grounds, possession arrests consume police, court, and correctional resources to enforce a prohibition whose social benefits are contested, and legalization frees those resources; our results say nothing against that efficiency case and, by showing that the associated enforcement burden fell disproportionately on Black residents, suggest that the freed resources were being applied unequally. On equity grounds, the relevant welfare object is not the arrest but its economic sequel. Because possession arrests are a gateway to criminal records, and records depress employment and earnings more for Black applicants (Pager 2003; Agan and Starr 2018), a compression of the arrest gap reduces differential exposure to a well-identified source of labor-market disadvantage and, through household resources, to intergenerational disadvantage. This is where the equity value of legalization is largest and also where it is most incomplete: legalization reduces the flow of new possession records but does nothing on its own about the stock of existing records, which is why automatic expungement is a natural complement if the economic-mobility goal is taken seriously.

External validity

Because the estimate is identified from eleven adopting states with diverse enforcement environments and is stable when any one of the largest adopters is removed, it is unlikely to be an artifact of a single state's experience. The heterogeneity we document, with smaller effects in the largest states, is itself an external-validity lesson: the disparity reduction a given state can expect depends on its baseline enforcement intensity and on how much of its cannabis enforcement was concentrated in simple possession to begin with. States considering legalization primarily for its equity dividend should not assume the average effect will apply uniformly, and should expect a smaller relative narrowing where possession enforcement was already comparatively low.

Limitations

Several limitations qualify the conclusions, and we state them in the balanced spirit this design requires. UCR reporting is voluntary and incomplete; although we restrict to twelve-month reporters and show in Panel B that the estimate is robust to controls for reporting composition and to a balanced-panel restriction, we cannot rule out subtler compositional changes correlated with legalization. The panel spans only 2015 to 2020, which gives the later-adopting cohorts a short pre-period and limits the power of the pre-trend test; this is why we lean on placebos and on the agreement of multiple estimators rather than on the dynamic coefficients, and it means our estimates capture relatively short-run effects rather than a long-run steady state. Arrests measure enforcement, not underlying use or offending, so our estimates speak to the enforcement gap and not to any disparity in behavior. We study Black and White arrest rates and do not analyze Hispanic enforcement, which the UCR race fields do not cleanly separate. Finally, our estimate is the reduced-form effect of the legalization package as implemented; we cannot separately identify the contributions of the statutory change itself, shifting police priorities, and prosecutorial discretion, and, as discussed above, we deliberately avoid conditioning on post-treatment variables such as police budgets or incarceration that would bias that reduced-form effect.

Policy implications

For policymakers weighing legalization as a racial-equity intervention, the evidence supports a specific and bounded claim. Legalization does what its mechanics imply: it removes the most common cannabis charge and thereby compresses the racial disparity that charge generated, with a relative narrowing on the order of thirty percent that is unlikely to be a statistical accident. What it does not do is close the gap, extend the improvement to the cannabis offenses that remain illegal or to drug enforcement generally, or address the stock of records already imposed. These boundaries point to complementary policies rather than to a different verdict on legalization. Automatic, no-cost expungement addresses the record stock and is where the labor-market and mobility payoff is concentrated. Attention to the enforcement of the offenses that remain, sale and manufacture outside the licensed market and quantity and public-consumption rules, is needed if the residual disparity is not to migrate into the charges that survive legalization. And because the broad drug-enforcement gap does not move, equity goals that extend beyond cannabis will require instruments aimed at policing and prosecution directly. Legalization is, on this evidence, a real but partial equity intervention whose economic value is realized most fully when paired with policies that act on the consequences of past enforcement and on the enforcement that remains.

Declarations

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Competing interests

The author declares no competing interests.

Ethics approval

Not applicable. This study analyzes de-identified, publicly available, aggregate administrative data and does not involve human participants or animal subjects. No institutional review board approval was required.

Consent to participate

Not applicable.

Consent for publication

Not applicable.

Clinical trial number

Clinical trial number: not applicable.

Data availability

The data supporting the findings of this study are derived from publicly available sources. Arrest counts by offense, age, sex, and race were obtained from the FBI Uniform Crime Reporting Program's "Arrests by Age, Sex, and Race" master files, available through the National Archive of Criminal Justice Data (https://www.icpsr.umich.edu/sites/nacjd/home). Race-specific population denominators were obtained from the U.S. Census Bureau's Vintage 2020 state characteristics population estimates (file SC-EST2020-ALLDATA6), available at https://www.census.gov/. Both sources are in the public domain. The constructed jurisdiction-by-year analysis panel and the code used to generate the results are available from the author on reasonable request.

Code availability

The code used to construct the analysis panel and generate all results is available from the author on reasonable request.

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Figures

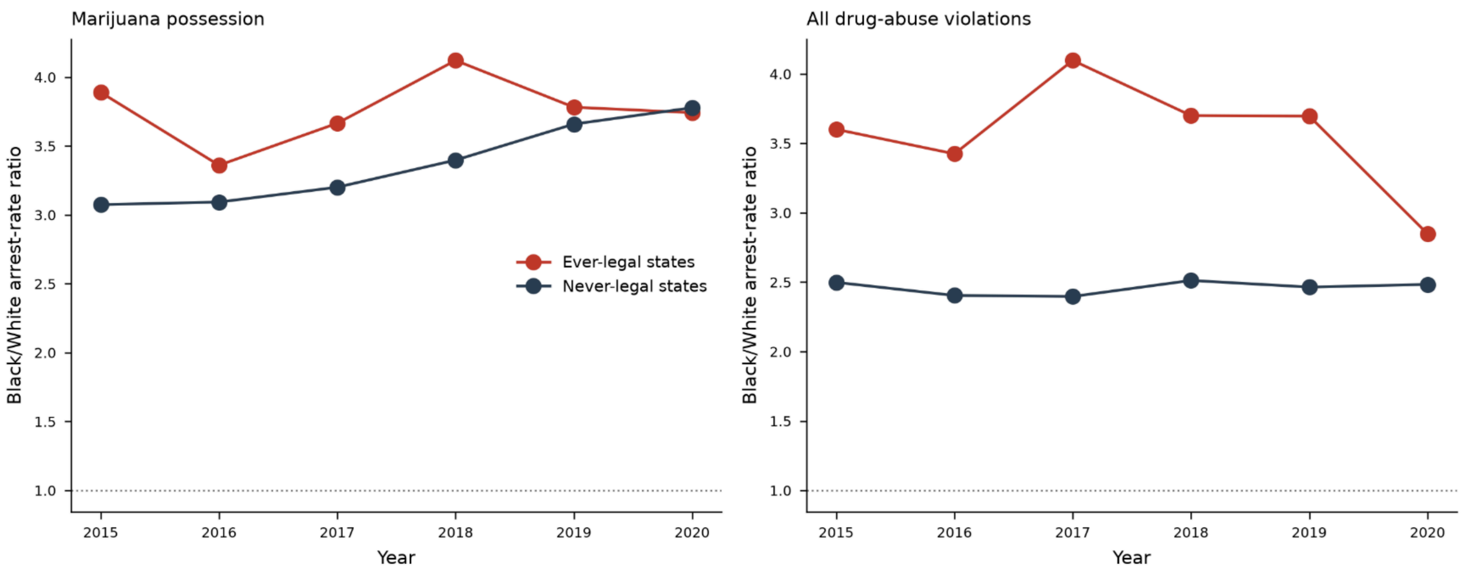

Figure 1

Black/White arrest-rate ratios by treatment status, 2015 to 2020. Left: marijuana possession. Right: all drug-abuse violations. Series are population-weighted means across states within each group. The dotted horizontal line marks parity (a ratio of one).

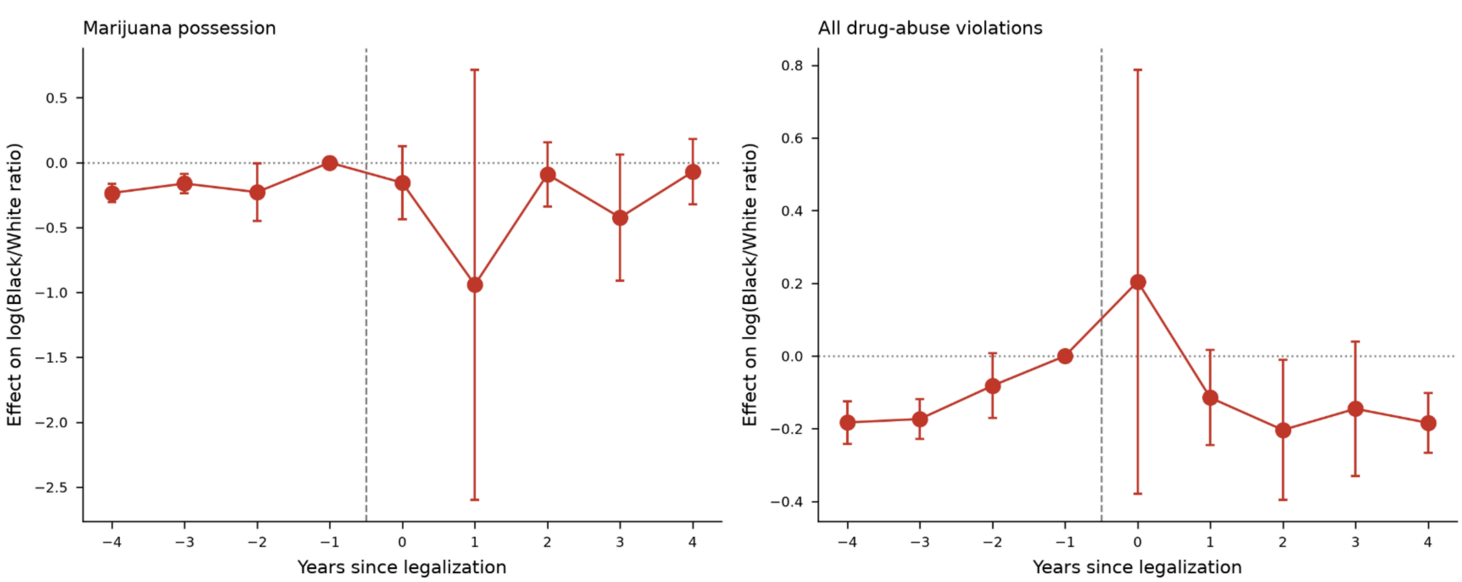

Figure 2

Event-study estimates (Callaway-Sant'Anna) of the effect of legalization on the log Black/White arrest-rate ratio. Left: marijuana possession. Right: all drug-abuse violations. Points are event-time coefficients and bars are 95 percent confidence intervals; the reference period is the year before legalization (event time -1).