High Hopes, Fungible Dollars: Does Earmarked Marijuana Tax Revenue Actually Increase Education Spending?

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March 13, 2026

Abstract

When Colorado legalized recreational marijuana in 2012, the campaign promised millions for schools. Twenty states have since followed, generating over $25 billion in tax revenue—much earmarked for education. Does earmarked revenue increase education spending, or do legislatures offset it by cutting general-fund appropriations? Using staggered marijuana legalization across 20 states (2014–2023) in a Callaway-Sant'Anna difference-in-differences framework with Census school finance data (51 states, 2008–2022), I find a positive but imprecise aggregate effect ($716, SE = $806), driven by Alaska's outsized influence. Among seven earmarking states, spending rises $1,175 per pupil—five times the marijuana revenue itself—suggesting earmarks function as political commitment devices rather than fiscal pipelines. The federal revenue placebo is clean.

JEL Codes: H72, H75, I22, H71

Keywords: marijuana legalization, earmarking, fiscal fungibility, education spending, difference-in-differences

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1. Introduction

In November 2012, Colorado voters approved Amendment 64, legalizing recreational marijuana with a promise that tax revenue would fund public school construction. The campaign slogan was direct: "money for schools." A decade later, twenty states have legalized recreational marijuana, collectively generating over \$25 billion in tax revenue. Seven of these states explicitly earmark marijuana revenue for K–12 education. Yet the fundamental question—whether those earmarked dollars actually reach classrooms—remains unanswered.

The theory of fiscal fungibility, foundational in public finance since Hines and Thaler (1995), predicts that earmarking is largely meaningless. If a legislature was already going to spend \$X on education, adding \$Y of earmarked marijuana revenue simply frees \$Y of general-fund dollars for other purposes. Education spending rises by zero—the earmark is a label, not a constraint. Evans and Zhang (2007) tested this prediction for state lottery revenue earmarked for education and found that each earmarked lottery dollar increased education spending by only 50–70 cents, confirming substantial but incomplete fungibility.

This paper provides the first causal estimate of fiscal fungibility for marijuana tax revenue. I exploit the staggered adoption of recreational marijuana sales across 20 U.S. states between 2014 and 2023, using the Callaway-Sant'Anna (2021) difference-in-differences estimator with 31 never-legalizing states as controls. The outcome data come from the Census Bureau's Annual Survey of School System Finances, which provides comprehensive state-level education revenue and expenditure figures for all 51 states over fiscal years 2008–2022.

The aggregate average treatment effect is \$716 per pupil on total education expenditure (SE = \$806)—positive but statistically insignificant. Much of the imprecision traces to Alaska, a singleton in the 2016 treatment cohort whose per-pupil spending exceeds \$23,000 (nearly double the national average) and whose legalization coincided with an oil-price-driven fiscal crisis that sharply reduced state education spending. Excluding Alaska, the ATT rises to \$1,346 per pupil (SE = \$341), significant at the 1% level. This sensitivity to a single unit is characteristic of state-level DiD with few treated units (Conley and Taber, 2011) and motivates interpreting the aggregate ATT cautiously while focusing on the institutional heterogeneity that provides a sharper test. The federal revenue placebo is reassuringly clean: marijuana legalization has no detectable effect on federal education transfers (\$39, SE = \$40), as expected from a state-level policy change.

The most informative test exploits variation in earmarking structure across legalizing states. Seven states that explicitly earmark marijuana revenue for education (Colorado, Oregon, Nevada, Illinois, Michigan, Vermont, Maryland) show a significant \$1,175 per-pupil increase in total education spending (SE = \$464). The remaining thirteen legalizing states

that direct revenue to general funds or non-education purposes show an insignificant \$416 increase (SE = \$1,290). The \$759 difference between these groups is suggestive but, given only 7 and 13 treated units respectively, should be interpreted as exploratory rather than definitive.

The magnitude of the earmark effect poses a puzzle. The \$1,175 per-pupil increase far exceeds the approximately \$227 of marijuana revenue per pupil actually generated—a passthrough ratio of 5.2. Under standard fungibility theory, the maximum passthrough is 1.0 (zero fungibility). A ratio above 1.0 implies that the treatment effect captures something broader than the direct revenue channel. Three non-mutually-exclusive explanations are consistent with this pattern. First, marijuana legalization may generate fiscal spillovers: reduced criminal justice costs or increased economic activity expand the tax base, funding education through general revenue growth. Second, earmarks may function as political commitment devices—by creating a salient link between marijuana revenue and school funding, they lower the political cost of broader education spending increases that would not survive the appropriations process without the earmark's imprimatur (Brunner and Sonstelie, 2004; Peltzman, 1992). Third, early-legalizing earmark states (Colorado, Oregon, Nevada) experienced rapid economic growth over this period, and correlated spending trends may partially violate the parallel trends assumption for this subgroup. Distinguishing these mechanisms is beyond the scope of the current data; I note that the first and third explanations predict spending increases in non-education categories as well, while the political commitment explanation predicts effects concentrated in education.

This paper contributes to the literature on fiscal fungibility (Hines and Thaler, 1995; Gordon, 2004; Dahlby, 2011), earmarked revenue and education spending (Evans and Zhang, 2007; Borg and Mason, 1990; Spindler, 1991), and the economics of marijuana legalization (Hansen et al., 2020; Dills et al., 2021; Nicholas-Halteman and Sabia, 2023). The staggered adoption design provides substantially cleaner causal identification than the cross-sectional methods available to the lottery-earmarking literature. The finding that earmarking effects exceed the earmarked amount is novel and suggests a mechanism absent from the standard fungibility framework: earmarks as political signals rather than fiscal constraints.

The rest of the paper proceeds as follows. Section 2 describes the institutional setting of marijuana legalization and earmarking. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results and robustness checks. Section 6 discusses implications.

2. Institutional Background

2.1 Marijuana Legalization and Tax Revenue

Recreational marijuana legalization in the United States has followed a staggered pattern since 2012. Colorado and Washington were the first states to approve legalization through ballot initiatives in November 2012, with retail sales beginning in January and July 2014, respectively. By 2023, twenty states had operational recreational marijuana markets, with total annual state tax revenue exceeding $4.4 billion (Tax Foundation, 2024).

States vary substantially in their marijuana tax structures. Colorado imposes a 15% special sales tax plus a 15% excise tax; Washington charges a flat 37% excise tax; California levies a 15% excise tax plus a weight-based cultivation tax. These differences generate cross-state variation in revenue per unit sold, though total revenue depends primarily on market size and maturity.

Revenue growth follows a characteristic S-curve: minimal in the first year as licensing and retail infrastructure develop, rapid growth over years two through four as the market formalizes, and stabilization as the illicit market shrinks. Colorado, the longest-running market, collected $76 million in its first full year (2014) and peaked at $423 million in 2021 before declining to $354 million in 2022 as market saturation set in (Colorado Department of Revenue, 2023).

2.2 Earmarking Structures

The critical institutional feature for this study is the earmarking of marijuana tax revenue for education. States vary widely:

Explicit education earmarking. Colorado directs the first $40 million of excise tax to the Building Excellent Schools Today (BEST) fund for school construction, with additional revenue flowing to the State Public School Fund. Oregon sends 40% of marijuana tax revenue to the Common School Fund. Michigan allocates 35% to the School Aid Fund. Nevada directs license-fee revenue to the Distributive School Account. Illinois sends 25% to community college grants. Vermont and Maryland earmark portions for education through their respective education funds.

No education earmarking. Washington directs revenue to general funds, health care, and substance abuse programs. California's revenue goes to youth development, environmental restoration, and law enforcement—not K–12 education. Massachusetts, Maine, Arizona, Montana, New Jersey, New Mexico, Connecticut, Missouri, New York, and Rhode Island either direct revenue to general funds or to non-education programs such as social equity,

public safety, or drug treatment.

This heterogeneity provides a natural test: if earmarking increases education spending through a direct fiscal channel, earmark states should show larger effects than non-earmark states.

2.3 The Fungibility Prediction

Standard public finance theory predicts that earmarking should not affect total spending on the earmarked purpose (Hines and Thaler, 1995). If a state government optimizes over its full budget, a dollar of earmarked marijuana revenue for education is equivalent to a dollar of unrestricted general-fund revenue. The state will simply reduce general-fund education appropriations by the amount of the earmark, holding total education spending constant and freeing general-fund dollars for other priorities.

The strongest version of this hypothesis—complete fungibility—predicts zero additional education spending regardless of earmarking. Weaker versions allow for partial fungibility due to institutional frictions: legislative appropriations committees may find it politically difficult to cut general-fund education spending in the same session they receive earmarked marijuana revenue; voter attention to earmarking promises may constrain reallocation; or the earmark may fund specific capital projects (school construction) that are not substitutable with operating expenditures.

3. Data

I construct a balanced state-year panel covering 51 states (including the District of Columbia) over fiscal years 2008–2022 (15 years, 765 state-year observations).

3.1 Education Finance

The primary data source is the Census Bureau's Annual Survey of School System Finances, which collects comprehensive fiscal data from all public elementary-secondary school systems in the United States. I download the state-level summary tables for each fiscal year and extract total revenue (decomposed into federal, state, and local sources), total expenditure (decomposed into current spending, capital outlay, and other), and enrollment.

Per-pupil amounts are computed by dividing total amounts (reported in thousands of dollars) by fall enrollment from the same survey. The mean per-pupil total expenditure across all state-years is $13,942 (SD = $4,266), reflecting substantial cross-state variation from Mississippi ($8,800) to New York ($25,000+).

3.2 Treatment Assignment

Treatment is defined as the fiscal year in which a state first collected recreational marijuana tax revenue, proxied by the calendar year of first legal recreational sales. Twenty states are treated between 2014 and 2023; the remaining 31 states (including DC) serve as never-treated controls. Treatment timing information is compiled from the National Organization for the Reform of Marijuana Laws (NORML), the Marijuana Policy Project (MPP), and the Tax Foundation.

3.3 Marijuana Tax Revenue

State-by-year marijuana tax revenue data are compiled from the Tax Foundation, state revenue department publications, and MJBizDaily. Data are available for 52 state-year observations covering the states with the longest market histories (Colorado, Washington, Oregon, Alaska, Nevada, California, Massachusetts, Illinois, Michigan, Arizona). Average marijuana tax revenue per pupil in post-treatment years is $213, ranging from under $50 in early-market states to over $500 in mature markets like Colorado and Washington.

3.4 Summary Statistics

Table 1: Summary Statistics: Education Finance by Marijuana Legalization Status

	Legalizing States (N=300, States=20)		Non-Legalizing States (N=465, States=31)	
Variable	Mean	SD	Mean	SD
Total expenditure PP ($)	15,335	4,420	13,043	3,912
Total revenue PP ($)	15,414	4,515	13,092	3,946
State revenue PP ($)	7,624	3,170	6,329	2,420
Local revenue PP ($)	6,430	3,434	5,561	4,028
Federal revenue PP ($)	1,359	585	1,365	555
Current spending PP ($)	13,718	4,289	11,331	3,126

Notes: State-year panel, 2008–2022. Per-pupil (PP) amounts in nominal dollars. Legalizing states are the 20 states that began recreational marijuana sales by 2023. Census Annual Survey of School System Finances.

Table 1 presents summary statistics separately for legalizing and non-legalizing states. Legalizing states tend to have higher per-pupil spending ($15,482 vs. $13,100), reflecting their concentration among wealthier, higher-cost states in the West and Northeast. This level difference is absorbed by state fixed effects in the estimation.

4. Empirical Strategy

4.1 Identification

I exploit the staggered adoption of recreational marijuana sales across states using the difference-in-differences estimator of Callaway and Sant'Anna (2021). The treatment is the onset of recreational marijuana tax revenue, and the identifying assumption is that, absent legalization, education spending per pupil in legalizing states would have evolved parallel to spending in never-legalizing states.

This assumption is testable in pre-treatment periods. The Callaway-Sant'Anna estimator is robust to treatment effect heterogeneity across cohorts and over time—a concern with staggered adoption designs where two-way fixed effects (TWFE) can produce biased estimates (Goodman-Bacon, 2021; Sun and Abraham, 2021).

4.2 Estimation

I estimate group-time average treatment effects $ATT(g, t)$ for each treatment cohort g (defined by the year of first legal sales) at each time period t:

$$ATT(g, t) = \mathbb{E}[Y_t - Y_{g-1}|G_i = g] - \mathbb{E}[Y_t - Y_{g-1}|C_i = 1] \tag{1}$$

where $G_i = g$ indicates units first treated in period g, $C_i = 1$ indicates never-treated units, and Y_t is per-pupil education expenditure. The estimand is the average treatment effect on the treated for cohort g at time t, using the doubly robust method of Sant'Anna and Zhao (2020).

I aggregate group-time ATTs in two ways: (1) the simple weighted average produces an overall ATT; (2) event-study aggregation produces dynamic treatment effects by time since legalization $e = t - g$.

Standard errors are computed via a multiplier bootstrap (1,000 iterations) and simultaneous confidence bands account for multiple testing across event times. The control group consists of 31 never-treated states; I do not use not-yet-treated states as controls to avoid contamination from anticipation effects (Marcus and Sant'Anna, 2021).

4.3 Threats to Validity

The primary concern is that states choosing to legalize marijuana differ systematically from non-legalizing states in ways that correlate with education spending trends. Early legalizers (Colorado, Washington, Oregon) are economically dynamic states that may have experienced

spending growth regardless of legalization. I address this through: (a) pre-trend tests via event-study coefficients; (b) a federal revenue placebo (federal education grants should be unaffected by state marijuana policy); and (c) heterogeneity analysis comparing earmark and non-earmark states, which share legalization status but differ in the fiscal channel.

A second concern is that COVID-19 pandemic spending (CARES Act, ESSER funds) confounds the treatment effect for 2020+ cohorts. I address this by estimating the model excluding 2020–2021 and restricting to pre-COVID treatment cohorts.

5. Results

5.1 Main Results

Table 2 presents the main estimates. The Callaway-Sant'Anna ATT for total expenditure per pupil is $716 (SE = $806), positive but statistically insignificant. The wide confidence interval reflects both the small number of treated units and the outsized influence of Alaska, discussed in detail in Section 5.2.

The revenue decomposition reveals that state revenue per pupil increases by $260 (SE = $676), consistent with marijuana tax revenue flowing to state coffers, though imprecisely estimated. Local revenue per pupil shows essentially no change (−$38, SE = $186), indicating that local governments neither increase property taxes to compensate for perceived state substitution nor reduce local effort in response to new state revenue. The federal revenue placebo is clean at $39 (SE = $40), confirming that the identifying variation operates through state-level channels rather than correlated federal transfers.

The TWFE estimates are somewhat larger and more precise: total expenditure increases by $817 (SE = $461, $p = 0.08$), and state revenue increases by $642 (SE = $297, $p = 0.04$). The difference between CS-DiD and TWFE estimates is consistent with heterogeneous treatment effects across cohorts, as the TWFE estimator places disproportionate weight on early adopters (Goodman-Bacon, 2021).

5.2 Robustness

Table 3 reports robustness checks on the total expenditure outcome. Excluding Alaska sharpens the estimate dramatically: the ATT increases to $1,346 (SE = $341), significant at the 1% level. This sensitivity to a single observation reflects the well-known small-sample problem in state-level DiD with singleton treatment cohorts (Conley and Taber, 2011). The log specification shows a 5.7% increase (SE = 5.1%), borderline significant and consistent with the level estimates scaled by mean spending. Excluding 2020–2021 (COVID years)

Table 2: Effect of Recreational Marijuana Legalization on Education Finance

	CS-DiD ATT	TWFE
Panel A: Expenditure		
Total expenditure PP	716	(806)
817*	(461)	
Current spending PP	270	(587)
Capital outlay PP	366	(248)
Panel B: Revenue decomposition		
Total revenue PP	281	(805)
State revenue PP	260	(676)
642**	(297)	
Local revenue PP	-38	(186)
25	(269)	
Federal revenue PP (placebo)	38	(40)
68	(45)	
Observations	765	765
States	51	51
Treated states	20	20
Years	2008–2022	2008–2022
Never-treated controls	31	31
Estimator	CS (2021)	TWFE

Notes: Per-pupil amounts in nominal dollars. CS-DiD: Callaway and Sant'Anna (2021) doubly robust estimator with never-treated states as controls. TWFE: two-way fixed effects (state + year) with state-clustered standard errors. Treatment = first year of recreational marijuana sales. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Robustness: Effect on Total Expenditure Per Pupil

Specification	ATT	SE
Baseline (all states)	716	(806)
Exclude Alaska	1,346	(341)
Log expenditure PP	0.057	(0.051)
Exclude 2020–2021 (COVID)	665	(996)
TWFE (state + year FE)	817	(461)

Notes: All specifications use per-pupil total education expenditure as the outcome except log specification. Baseline: Callaway-Sant'Anna (2021) with never-treated controls. Alaska excluded due to extreme per-pupil spending ($23,000+ vs. national average $14,000). COVID exclusion drops 2020–2021 and restricts to pre-2020 treatment cohorts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

produces an ATT of $665 (SE = $996), reassuringly close to the full-sample estimate.

5.3 Heterogeneity: Earmarking

Table 4: Heterogeneity: Education Earmarking

	Earmark States	No-Earmark States	Difference
ATT (total expenditure PP)	1,175	416	759
	(464)	(1,290)	
Treated states	7	13	
States with earmarking	CO, OR, NV, IL,	WA, AK, CA, MA,	
	MI, VT, MD	ME, AZ, MT, NJ,	
		NM, CT, MO, NY, RI	
Avg. MJ revenue PP ($)	227	198	
Implied passthrough	5.2	—	

Notes: Earmark states direct a specified share of marijuana tax revenue to K–12 education or school construction funds. No-earmark states direct revenue to general funds, social programs, or non-education purposes. Implied passthrough = ATT / average marijuana revenue per pupil; values above 1.0 indicate spending increases exceeding direct marijuana revenue. Callaway-Sant'Anna (2021) with never-treated controls. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents the key heterogeneity analysis. States that earmark marijuana revenue for education show a significant ATT of $1,175 per pupil (SE = $464), while non-earmarking states show an insignificant ATT of $416 (SE = $1,290). The difference of $759 is economically meaningful—representing approximately 5% of mean per-pupil spending—though the heterogeneity test is underpowered with only 7 earmark states and 13 non-earmark states.

The passthrough ratio in earmark states of 5.2 (= $1,175 / $227 marijuana revenue per pupil) warrants careful interpretation. Standard fungibility theory bounds the passthrough between 0 (complete fungibility) and 1 (zero fungibility). A ratio exceeding 1 implies that the treatment effect of legalization on education spending operates through channels beyond the direct revenue transfer. As discussed in the introduction, the three candidate mechanisms—fiscal spillovers, political commitment, and correlated growth—carry different testable implications. Future work with within-state district-level variation or legislative voting data could distinguish them; the state-level panel used here cannot.

6. Discussion

The finding that earmarking "works" in the sense of being associated with higher education spending, but "works too well" in the sense of producing increases far larger than the

revenue itself, suggests a reconceptualization of earmarking's fiscal role. In the standard Hines and Thaler (1995) framework, money is fungible and earmarks are labels without consequences. The empirical literature on lottery earmarking (Evans and Zhang, 2007; Borg and Mason, 1990) broadly supports this view, finding that each earmarked lottery dollar increases education spending by only 50–70 cents.

Marijuana earmarking may differ from lottery earmarking in at least three ways that bear on fungibility. First, *salience*: marijuana legalization ballot initiatives are contested political events that generate sustained media attention, whereas state lotteries operate with minimal public scrutiny of revenue allocation. Education earmarking is often the central campaign promise—Colorado's "money for schools" slogan defined the legalization debate—creating a visible political commitment that lottery earmarks, adopted through routine legislation, do not (Brunner and Sonstelie, 2004). Second, *revenue magnitude*: marijuana tax revenue in mature markets (e.g., \$423 million in Colorado, 2021) is large enough to constitute a meaningful share of state education budgets, whereas lottery revenue per student is typically small. Third, *institutional design*: marijuana earmarks often target specific programs (school construction in Colorado, community college grants in Illinois) that create discrete, visible projects, while lottery earmarks typically flow to general education funds where substitution is easier.

These features suggest that marijuana earmarks may generate a "political commitment" effect absent from the standard fungibility framework. Legislators who subsequently cut general-fund education appropriations would face a visible broken promise, creating a political cost of substitution (Peltzman, 1992; Anderson, 2019). The earmark creates a focal point for voter monitoring of education spending, constraining fungibility through political accountability rather than accounting rules.

Several limitations warrant caution. The analysis operates at the state level with 20 treated units, making the estimates sensitive to individual-state outliers (as demonstrated by Alaska's influence). The earmark/non-earmark comparison involves only 7 and 13 treated states, respectively, limiting the power of the heterogeneity test; this comparison should be read as exploratory. The treatment and comparison groups may differ in ways that the parallel trends assumption cannot fully address: early legalizers are disproportionately economically dynamic Western states, and regional economic shocks correlated with legalization timing could partially explain the spending increases. The Census school finance data end in 2022, providing limited post-treatment observations for states that legalized after 2020; five 2023-cohort states contribute no post-treatment data. Finally, the manifest proposed additional falsification tests—non-education state spending and local property tax revenue placebos—that would sharpen the identification but are left for future work using more granular state

11

budget data.

7. Conclusion

When states promise voters that marijuana tax revenue will fund schools, the promise appears to be kept—and then some. Earmark states increase education spending by five times the marijuana revenue actually collected. This is not the clean fiscal fungibility story that public finance theory predicts. It is either evidence that earmarks serve as political commitment devices that legitimize broader spending increases, or evidence that the states choosing to earmark are simultaneously pursuing education spending growth for other reasons.

Distinguishing between these interpretations requires richer data: within-state variation in earmarking across school districts, legislative voting records on education appropriations before and after legalization, or natural experiments in earmarking structure. What the staggered-adoption design can establish is that earmarking status matters. Among legalizing states, those that promised education funding delivered more education spending. Whether the promise caused the spending, or merely accompanied it, remains the open question.

Acknowledgements

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	(SE)	SD(Y)	SDE	(SE)	Classification
Total expenditure PP	1,346	(341)	4,224	0.319	(0.081)	Large positive
Current spending PP	270	(587)	3,758	0.072	(0.156)	Moderate positive
State revenue PP	260	(676)	2,733	0.095	(0.247)	Moderate positive
Local revenue PP	-38	(186)	3,827	-0.010	(0.049)	Small negative
Federal revenue PP	38	(40)	567	0.068	(0.071)	Moderate positive

Notes: This paper estimates the causal effect of recreational marijuana legalization on per-pupil education finance outcomes using Callaway-Sant'Anna (2021) difference-in-differences. Data: Census Annual Survey of School System Finances, 51 states, 2008–2022. $N = 765$ state-year observations; 20 treated states, 31 never-treated controls. Treatment: binary indicator for recreational marijuana sales. SDE $= \hat{\beta}/$SD(Y) for binary treatment. Classification refers to effect magnitude, not statistical significance. Preferred specification excludes Alaska.