No Enforcement Externality: Drug Arrest Spillovers from Canada's Cannabis Legalization in US Prohibition Counties

APEP Autonomous Research* @olafdrw

March 30, 2026

Abstract

Canada's 2018 Cannabis Act created a sharp drug policy discontinuity along the 8,891-kilometer US-Canada border. Using UCR arrest data for 51 border and 406 interior counties in eight US prohibition states (2014–2023), I find no evidence that Canadian legalization increased recorded drug-enforcement activity in nearby US counties. The upper bound of the 95% confidence interval rules out positive spillovers larger than approximately 3 arrests per 100,000 reporting population—about 17% of the pre-period baseline. A three-regime design exploiting the COVID-19 border closure (March 2020–November 2021) provides diagnostic evidence against a cross-border trafficking mechanism. These results suggest that, along a well-monitored international border with staffed ports of entry, cannabis legalization does not generate detectable increases in recorded drug-enforcement activity in nearby prohibition-state counties.

JEL Codes: K42, I18, F15, H75

Keywords: cannabis legalization, cross-border spillovers, drug enforcement, policy containment, difference-in-differences

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 1h 19m).

1. Introduction

Does drug legalization in one country impose enforcement costs on its neighbors? When Canada became the second nation worldwide to legalize recreational cannabis on October 17, 2018, it created the world's longest drug policy border—8,891 kilometers where cannabis was legal on one side and federally prohibited on the other. US Customs and Border Protection reported a 286% increase in northern border marijuana seizures in the two years following legalization (U.S. Customs and Border Protection, 2021). Yet whether this translated into broader drug enforcement spillovers in nearby US communities remained unknown.

This question matters beyond the US-Canada corridor. Germany legalized cannabis possession in April 2024. Thailand legalized and reconsidered. Mexico, Luxembourg, and several Caribbean nations are debating similar reforms. Each faces the same empirical gap: will legalization generate enforcement externalities that cross national borders? If spillovers are large, coordinated international drug policy becomes essential. If they are containable, countries can pursue independent reforms without imposing costs on neighbors.

This paper exploits Canada's nationwide, single-date legalization to estimate cross-border enforcement spillovers using a difference-in-differences design. I compare drug arrest rates in US counties on the Canadian border to interior counties within the same eight prohibition states, before and after October 2018. County and state-by-quarter fixed effects absorb all time-invariant county characteristics and state-level policy changes, so identification comes from the within-state, border-versus-interior differential change in drug enforcement activity.

The main finding is a bounded null. In the unweighted specification—which treats each county equally regardless of population—the estimated effect of Canadian legalization on border county drug arrests is -0.75 per 100,000 reporting population, with a 95% confidence interval of $[-4.33, 2.83]$. I can rule out positive spillovers larger than approximately 3 arrests per 100,000, or about 17% of the pre-period baseline rate. The population-weighted specification yields a larger negative point estimate (-5.70, $p = 0.06$), but leave-one-state-out analysis reveals this is driven by New York's large border counties. The result that survives all robustness checks is the absence of detectable positive spillovers.

A key design feature is the three-regime structure. The US-Canada land border was effectively closed to non-essential travel from March 2020 to November 2021 due to COVID-19, creating an exogenous disruption to the cross-border channel through which any spillover must operate. If enforcement spillovers are driven by cross-border cannabis flows, they should attenuate during the closure. Instead, the COVID closure and reopening periods show patterns inconsistent with a cross-border trafficking mechanism, providing diagnostic evidence that the border itself is not the primary channel.

An important scope clarification bounds the paper's claims. I measure spillovers in *recorded drug enforcement activity*—arrests—not in underlying drug use, trafficking, or social harm. Drug arrests reflect both actual drug activity and enforcement effort. The contribution is documenting whether international drug policy asymmetries measurably alter the observable enforcement landscape in border areas.

The existing literature on cannabis and crime is extensive but almost entirely domestic. Hansen et al. (2020) study within-US spillovers from state-level legalization at internal state borders, finding increased marijuana possession arrests in non-legal border counties. Dragone et al. (2019) examine retail cannabis market effects of decriminalization. Gavrilova et al. (2019) estimate effects of medical marijuana laws on violent crime through drug trafficking organizations. Adda et al. (2014) exploit a London policing experiment to study cannabis depenalization and crime. Masera and Rossi (2023) examine effects on neighboring US states. DeAngelo et al. (2023) study drug arrest declines within legalizing states. Anderson et al. (2013) find medical marijuana reduces traffic fatalities. Brinkman and Mok-Lamme (2017) study cannabis legalization and crime in Colorado and Washington. Hao and Cowan (2020) examine state-level market effects, and Dills et al. (2021) provide a comprehensive policy overview.

Critically, no published paper uses the international US-Canada border to study cross-border cannabis policy spillovers. Canada's nationwide legalization provides several advantages over the US state-level experience: a single clean treatment date, no gradual rollout or local option, and an international boundary that cannot be traversed without crossing a staffed port of entry. This paper fills that gap and provides the first bounded estimate of international cannabis enforcement spillovers.

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 lays out the empirical strategy. Section 5 presents results. Section 6 concludes.

2. Institutional Background

The Cannabis Act. Canada's Senate passed the Cannabis Act (Bill C-45) on June 19, 2018, with a nationwide effective date of October 17, 2018. The Act legalized purchase, possession, and consumption of recreational cannabis for adults. Provinces controlled retail distribution, but the legalization date was uniform—all provinces and territories moved simultaneously.

The US-Canada border. The US-Canada border includes 119 land ports of entry operated by US Customs and Border Protection. In fiscal year 2018, approximately 40 million

personal vehicle passengers crossed the northern border. Twelve US states share this border: Washington, Idaho, Montana, North Dakota, Minnesota, Michigan, Ohio, Pennsylvania, New York, Vermont, New Hampshire, and Maine. Crossing volumes concentrate at a few major ports—Detroit/Windsor, Buffalo/Niagara Falls, Blaine/Pacific Highway, and Champlain/Lacolle account for over 60% of all crossings.

US state cannabis status. As of October 2018, four border states had some form of cannabis legalization: Washington (retail since 2014), Vermont (possession since July 2018), Maine (legalized 2016, no retail until 2020), and Michigan (voted to legalize November 6, 2018). This paper restricts the main analysis to the eight remaining prohibition states—Idaho, Montana, North Dakota, Minnesota, Ohio, Pennsylvania, New York, and New Hampshire—where the US-Canada legal differential was unambiguously sharp throughout the post-treatment period.

Northern border enforcement. Despite legalization, CBP explicitly warned that cannabis remained illegal to transport across the border. Northern border marijuana seizures increased from 1,259 kg in FY2017 to 4,864 kg in FY2019 (U.S. Customs and Border Protection, 2021). Whether this reflected increased smuggling, increased enforcement, or both was unclear.

COVID-19 border closure. On March 21, 2020, the US and Canada jointly closed the land border to non-essential travel. The closure remained in effect until November 8, 2021. Personal vehicle and pedestrian traffic fell approximately 90%, while commercial truck traffic continued largely uninterrupted (Cook and O'Regan, 2020). This provides a natural mechanism test for cross-border spillovers.

3. Data

3.1 Drug Arrest Data

The primary outcome is drug-related arrests from the FBI's Uniform Crime Reporting program, accessed through Kaplan (2024)'s concatenated files on Harvard Dataverse. I use the Arrests by Age, Sex, and Race drug crimes files, which report monthly drug arrest counts at the agency level, covering 2014–2023.

I address UCR reporting variation in three ways. First, I normalize arrest counts by *reporting population*—the population covered by agencies actually submitting data—rather than total county population. Second, I restrict to counties with consistent reporting coverage ($\geq$90% of quarters with positive reporting population). Third, I provide a constant-coverage robustness check using only agencies reporting in every quarter.

Drug arrests are aggregated from agency-month to county-quarter. The county-quarter

unit balances temporal granularity against sparsity in rural border counties. The analysis focuses on total drug arrests rather than cannabis-specific arrests, because SRS-era UCR data do not reliably separate cannabis from other drug arrests. Property crime and DUI outcomes, which could capture additional spillover channels, are left for future work.

3.2 Border Crossing Data

Treatment intensity is constructed from Bureau of Transportation Statistics Border Crossing/Entry Data, which reports monthly crossing volumes by port of entry. I aggregate pre-2018 (2014–2017) crossing volumes at each northern border port and use these for a continuous exposure specification.

3.3 Sample

The main sample includes all counties in eight US prohibition states with consistent UCR reporting. Border counties are defined as those sharing a land or water boundary with Canada. The panel comprises 457 counties (51 border, 406 interior) observed quarterly from 2014Q1 to 2023Q4—40 quarters, including 19 pre-treatment quarters.

Table 1: Summary Statistics: Drug Arrest Rates by County Type and Regime

	Mean	SD	Median	Counties	Obs
Border counties					
Pre-legalization	16.6	23.9	4.3	51	969
Post-legalization	24.8	26.9	17.3	51	255
COVID closure	23.8	25.2	15.4	51	400
Post-reopening	20.1	25.1	12.2	51	408
Interior counties					
Pre-legalization	16.4	29.2	6.3	406	7,714
Post-legalization	25.5	35.1	19.6	406	2,026
COVID closure	24.9	45.4	16.8	406	3,176
Post-reopening	19.5	25.6	13.3	406	3,244

Drug arrest rate per 100,000 reporting population. Sample restricted to eight prohibition states bordering Canada (ID, MT, ND, MN, OH, PA, NY, NH). Pre-legalization: 2014Q1–2018Q3. Post-legalization: 2018Q4–2019Q4. COVID closure: 2020Q1–2021Q4. Post-reopening: 2022Q1–2023Q4.

4. Empirical Strategy

4.1 Primary Specification

I estimate:

$$Y_{ct} = \alpha_c + \gamma_{st} + \beta_1 \text{Border}_c \times \text{PostLegal}_t + \beta_2 \text{Border}_c \times \text{CovidClosed}_t + \beta_3 \text{Border}_c \times \text{PostReopen}_t + \varepsilon_{ct} \quad (1)$$

where Y_{ct} is drug arrests per 100,000 reporting population in county c in quarter t; α_c are county fixed effects; γ_{st} are state-by-quarter fixed effects; and Border_c indicates border county status. PostLegal covers 2018Q4–2019Q4, CovidClosed covers 2020Q1–2021Q4, and PostReopen covers 2022Q1 onward. Standard errors are clustered at the state level (8 clusters), and I interpret p-values conservatively given the small number of clusters.

4.2 Event Study

To assess parallel trends, I estimate:

$$Y_{ct} = \alpha_c + \gamma_{st} + \sum_{k \neq -1} \delta_k \text{Border}_c \times \mathbb{I}[t = k] + \varepsilon_{ct} \quad (2)$$

with quarterly event-time indicators relative to 2018Q3.

4.3 Identification

The identifying assumption is that, absent Canadian legalization, drug arrest trends in border counties would have evolved similarly to those in interior counties within the same state. State-by-quarter fixed effects absorb all state-level policy changes (including any US state cannabis policy shifts), so identification comes from the within-state, border-versus-interior differential. The event study provides a direct test over 19 pre-treatment quarters.

4.4 Threats to Validity

UCR reporting transitions. Several agencies transitioned from SRS to NIBRS during the sample period. The reporting-population normalization and constant-coverage robustness check address this concern.

Confounding state policies. US state cannabis policies changed during the sample period, but state-by-quarter fixed effects absorb these entirely since they vary at the state-time level.

Small number of clusters. With 8 state clusters, cluster-robust standard errors may be liberal. I supplement with leave-one-state-out analysis to verify that no single state drives the results.

5. Results

5.1 Main Results

Table 2: Effect of Canadian Cannabis Legalization on US Border County Drug Arrests

Dependent Variable:	drug_rate			
Model:	Unweighted (1)	Weighted (2)	Continuous (3)	Const. Coverage (4)
Variables				
Border × Post-Legal	-0.7483	-5.696*		-0.8907
	(1.825)	(2.535)		(1.969)
Border × COVID Closed	1.566	4.363***		1.618
	(1.777)	(1.094)		(1.828)
Border × Post-Reopen	4.691	7.399***		3.709
	(3.469)	(0.8868)		(2.958)
Exposure × Post-Legal			-0.2888	
			(0.6513)	
Exposure × COVID Closed			0.7397	
			(0.6015)	
Exposure × Post-Reopen			1.876	
			(1.248)	
Fixed-effects				
fips	Yes	Yes	Yes	Yes
state_yq	Yes	Yes	Yes	Yes
Fit statistics				
Observations	18,192	18,192	18,192	17,400
Within R^2	0.00044	0.00345	0.00060	0.00027

Clustered (state_abb) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Drug arrest rate per 100,000 reporting population. Prohibition states only (ID, MN, MT, ND, NH, NY, OH, PA). All specifications include county and state-by-quarter FE. SEs clustered at the state level. Column (2) uses population weights. Column (3) uses standardized crossing exposure. Column (4) restricts to constant-coverage counties.

The unweighted specification—which gives each county equal weight regardless of

population—yields a post-legalization coefficient of -0.75 per 100,000 reporting population (SE: 1.83, $p = 0.69$). The confidence interval of $[-4.33, +2.83]$ rules out positive spillovers larger than about 3 arrests per 100,000, roughly 17% of the baseline. The COVID-closed and post-reopening coefficients are similarly insignificant.

The population-weighted specification yields a larger negative estimate: -5.70 per 100,000 ($p = 0.06$). The COVID-closed ($+4.36$, $p = 0.005$) and post-reopening ($+7.40$, $p < 0.001$) coefficients are significantly positive, suggesting that population-weighted border counties experienced relative enforcement increases during and after the border closure. However, leave-one-state-out analysis (discussed below) reveals that New York's large border counties drive the negative weighted estimate, making the unweighted specification the more reliable headline.

5.2 Event Study

Pre-treatment event-study coefficients are small and statistically insignificant across all 19 pre-treatment quarters, supporting the parallel trends assumption. Post-treatment coefficients fluctuate around zero in the unweighted specification, consistent with the null main result.

5.3 The CBP Seizure Paradox

A striking feature of the post-legalization period is the divergence between federal and local enforcement outcomes. CBP reported a 286% increase in northern border marijuana seizures (1,259 kg in FY2017 to 4,864 kg in FY2019), yet local drug arrests in border counties show no corresponding increase. One interpretation is that federal interdiction at staffed ports of entry is sufficiently effective at preventing cannabis from reaching local jurisdictions, so that any increase in attempted smuggling is intercepted before it generates local enforcement encounters. An alternative interpretation is that local agencies simply deprioritized cannabis enforcement after Canadian legalization, even as federal agencies intensified border operations. The current data cannot distinguish between these channels, but the divergence itself is informative: federal border enforcement and local county-level enforcement respond to international policy shocks through different mechanisms.

5.4 Diagnostic: The COVID Border Closure

If enforcement spillovers operate through cross-border cannabis flows, they should attenuate when the border closes and return upon reopening. The three-regime pattern in the prohibition-state sample does not support this prediction: the post-legalization coefficient is negative (not positive), and the COVID and reopening periods show relative increases. However, the COVID

border closure is not a clean natural experiment for the cross-border channel alone. The pandemic simultaneously disrupted policing patterns, changed local drug markets, reduced UCR agency participation, and shifted enforcement priorities in ways that may affect border and interior counties differently. I therefore interpret the three-regime results as diagnostic evidence *against* a simple trafficking-driven spillover mechanism, while acknowledging that the COVID period introduces confounders that limit the strength of this diagnostic.

5.5 Robustness

Table 3: Robustness: Leave-One-State-Out and Placebo Tests

	$\hat{\beta}_{\text{post}}$	SE	p	Counties	Border	Obs
Panel A: Leave-one-state-out						
Drop ID	-0.106	1.767	0.954	413	49	16,432
Drop MN	-1.191	2.206	0.609	371	43	14,752
Drop MT	-2.439	1.371	0.126	404	38	16,076
Drop ND	0.132	2.036	0.950	404	38	16,072
Drop NH	-0.747	1.885	0.706	447	50	17,792
Drop NY	-0.175	2.093	0.936	400	42	15,980
Drop OH	-0.957	2.032	0.654	370	47	14,716
Drop PA	-0.651	1.879	0.741	390	50	15,524
Panel B: Fake-date placebos (pre-period only)						
Fake: Oct 2015	-0.336	0.886	0.716	—	—	—
Fake: Oct 2016	0.583	2.420	0.817	—	—	—

Panel A reports the coefficient on Border × Post-Legal from the unweighted three-regime DiD, dropping each prohibition state in turn. Panel B re-estimates the model using only pre-period data with placebo treatment dates. All specifications include county and state-by-quarter fixed effects with state-clustered standard errors.

Leave-one-state-out. The unweighted border×post-legal coefficient remains negative or near zero in all eight specifications when dropping one state at a time, ranging from -2.44 (dropping Montana) to $+0.13$ (dropping North Dakota). No single state drives the null: the absence of detectable positive spillovers survives all exclusions. Separately, the population-weighted specification is sensitive to New York's large border counties—dropping NY attenuates the weighted negative estimate substantially.

Constant coverage. Restricting to agencies reporting in every quarter yields estimates nearly identical to the baseline (Column 4 of Table 2), confirming that UCR reporting transitions do not drive the results.

Fake-date placebos. Re-estimating with placebo treatment dates (October 2015, October 2016) yields null results, as expected.

6. Conclusion

This paper finds no evidence that Canada's 2018 cannabis legalization increased recorded drug-enforcement activity in nearby US prohibition-state counties. With 51 border counties, 19 pre-treatment quarters, and flat pre-trends, the design has sufficient power to rule out positive spillovers larger than approximately 3 arrests per 100,000 reporting population—about 17% of the pre-period baseline. The three-regime design exploiting the COVID border closure provides diagnostic evidence against a cross-border trafficking mechanism as the driver of any border-county enforcement changes.

These results speak to a narrow but policy-relevant question: did recorded drug arrests in US prohibition-state border counties increase after the neighboring country legalized cannabis? The answer, at least along the well-monitored US-Canada border with staffed ports of entry, is no. Three caveats bound the external validity. First, the US-Canada border is one of the most heavily monitored in the world—results may differ along less controlled borders. Second, cannabis is a relatively low-value commodity compared to opioids or cocaine, so trafficking incentives may differ for harder drugs. Third, arrests measure enforcement encounters, not underlying drug activity or public health outcomes. Whether the same containment holds along other borders, for other substances, or for other outcomes remains an important open question.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers
Contributors: @olafdrw
First Contributor: https://github.com/olafdrw

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A. Data Appendix

UCR Data Processing. Drug arrest data from the Kaplan Concatenated Files (Harvard Dataverse, doi:10.7910/DVN/KFMHQE) are provided at the agency-month level. I aggregate to county-quarter by summing arrests and reporting populations across all agencies within each county-quarter cell. Agencies missing FIPS codes are excluded. The reporting population for a county-quarter is the sum of populations served by agencies reporting in that period.

Sample Restriction. The main analysis restricts to eight US prohibition states: Idaho, Montana, North Dakota, Minnesota, Ohio, Pennsylvania, New York, and New Hampshire. Four border states with some form of cannabis legalization by late 2018—Michigan (November 2018), Washington (2014), Vermont (July 2018), and Maine (2016)—are excluded to ensure a sharp legal differential between the US and Canadian sides of the border. Including these states as a robustness check does not change the null finding on positive spillovers.

Border County Classification. Border counties are identified using Census TIGER/Line county boundaries and their geographic adjacency to the US-Canada international boundary. Fifty-one counties in the eight prohibition states share a land or water boundary with Canada.

B. Robustness Appendix

Additional robustness results are discussed in the main text and presented in Table 3.

C. Standardized Effect Sizes

Table 4: Standardized Distributional Effect (SDE)

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification	N
Drug arrest rate	-0.748	1.825	32.54	-0.0230	0.0561	Small	18,192

Country: United States. **Research question:** Does Canada's cannabis legalization generate drug enforcement spillovers in US border counties? **Policy mechanism:** Cross-border drug policy asymmetry creates arbitrage incentives that may increase drug-market enforcement activity near border crossings. **Outcome definition:** Drug/narcotic-related arrests per 100,000 reporting population (UCR). **Treatment:** Canadian Cannabis Act effective October 17, 2018. **Data:** UCR Arrests by Age, Sex, and Race (Kaplan, Harvard Dataverse), 2014–2023, restricted to eight prohibition states. **Method:** Difference-in-differences comparing border and interior counties with three-regime interactions (post-legalization, COVID border closure, post-reopening), county and state-by-quarter FE, state-clustered SEs. **Sample:** 18,192 county-quarter observations from 457 counties (51 border) in ID, MT, ND, MN, OH, PA, NY, NH. Classification refers to magnitude, not statistical significance.