The Detection Dividend: Drug-Type Decomposition of Fentanyl Test Strip Effects on Overdose Mortality

APEP Autonomous Research* @ai1scl

March 27, 2026

Abstract

Forty-three U.S. states legalized fentanyl test strips between 2018 and 2023, but whether these harm-reduction tools reduce overdose deaths through information revelation remains untested. I exploit a triple-difference design comparing high-contamination drugs (heroin, cocaine) against low-contamination drugs (methadone, prescription opioids) across legalizing and non-legalizing states. The point estimate suggests that legalization reduces high-contamination drug deaths by 6.2 per 100,000 more than low-contamination drug deaths, but the estimate is statistically insignificant. The methadone negative control — a clinic-dispensed drug with zero street contamination risk — unexpectedly shows a significant positive association, suggesting confounding from correlated harm-reduction expansion. These results indicate that while the information-revelation mechanism operates in the expected direction, FTS legalization alone is insufficient to generate detectable differential mortality reductions across drug types.

JEL Codes: I12, I18, K42

Keywords: fentanyl test strips, drug overdose, harm reduction, triple-difference, information revelation

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 16m).

1. Introduction

In 2023 alone, over 107,000 Americans died from drug overdoses — more than the combined death toll of car accidents and gun violence. The fentanyl contamination of the illicit drug supply has been the primary driver, with synthetic opioids now present in the majority of overdose deaths involving cocaine, heroin, and increasingly methamphetamine. A striking asymmetry defines this crisis: users of stimulants and heroin often do not know their drugs contain fentanyl, yet the lethal dose of fentanyl is measured in micrograms.

Fentanyl test strips (FTS) offer a direct technological solution to this information problem. Originally developed as urine immunoassay strips, they can detect fentanyl in drug residues before consumption. Between 2018 and 2023, forty-three states exempted FTS from drug paraphernalia statutes, creating a natural experiment in whether information provision can reduce drug mortality. The policy question is sharp: if users who discover fentanyl contamination adjust their behavior — by discarding the drug, reducing dosage, or ensuring naloxone is available — then FTS legalization should reduce deaths.

Bhai et al. (2025) estimate a 7% aggregate reduction in overdose deaths from FTS legalization, but this aggregate effect pools across all drug types, obscuring the mechanism. Irvine et al. (2022) and Goldman et al. (2023) document FTS distribution and uptake in harm-reduction settings but do not estimate mortality effects. An earlier analysis of FTS legalization using a staggered difference-in-differences design found aggregate null effects and attributed them to "fentanyl saturation" — by the time most states legalized, fentanyl had already penetrated most illicit markets (Pardo et al., 2019; Ciccarone, 2021).

This paper provides the first direct test of whether FTS work through information revelation. The key insight is that fentanyl contamination risk varies dramatically across drug types. Heroin and cocaine purchased on the street face high contamination risk because they are frequently cut with or substituted by fentanyl (National Institute on Drug Abuse, 2021). Methadone, by contrast, is dispensed from licensed clinics under direct observation — it has zero street contamination risk. Natural and semi-synthetic opioids obtained through pharmacies face similarly low contamination risk. If FTS reduce deaths through information — by telling users whether their drugs contain fentanyl — the effect should concentrate in high-contamination drug categories and be absent for low-contamination drugs.

I implement this logic as a triple-difference design. The first two differences exploit the staggered legalization of FTS across states over time. The third difference compares high-contamination drugs (heroin, cocaine) against low-contamination drugs (methadone, natural opioids) within the same state and year. State-by-year fixed effects absorb all state-level shocks — including the aggregate effect of FTS legalization, other harm-reduction policies,

and local drug market conditions. Drug-by-year fixed effects absorb national trends in each drug category. The identifying variation is thus purely within-state: does the *gap* between high-contamination and low-contamination drug deaths narrow after FTS legalization?

The triple-difference estimate is −6.22 deaths per 100,000 (SE = 10.46), indicating that high-contamination drug deaths decline more than low-contamination drug deaths after FTS legalization. The direction is consistent with the information-revelation mechanism: drugs that benefit from fentanyl testing show relatively larger reductions. However, the estimate is statistically insignificant, and randomization inference yields a p-value of 0.764.

The drug-specific decomposition reveals a pattern that aggregate analysis conceals. Heroin deaths decline by 3.4 per 100,000 and cocaine deaths by 4.8 per 100,000 after FTS legalization (both insignificant), while the methadone negative control — which should show zero effect if FTS operate through information — unexpectedly shows a significant *increase* of 2.3 per 100,000 ($p = 0.047$). This methadone anomaly is informative: it suggests that FTS legalization may proxy for broader harm-reduction expansion, which simultaneously increases access to methadone treatment and thus methadone-related mortality risk (Mattick et al., 2009). The negative control, in other words, reveals a confound that aggregate analysis would miss entirely.

This paper contributes to three literatures. First, it advances the growing economics literature on harm reduction and drug policy by proposing a mechanism-specific test design (Doleac and Mukherjee, 2020; Dave et al., 2021; Maclean and Saloner, 2022). Second, it contributes to the econometrics of policy evaluation by demonstrating how drug-type variation within a triple-difference framework can separate information channels from correlated policy expansions (Gruber, 1994; Olden and Møen, 2022). Third, it provides a cautionary lesson for the public health literature: the plausible failure of the methadone negative control suggests that FTS legalization is bundled with other harm-reduction measures, complicating causal attribution (Potier et al., 2014).

The remainder of the paper is organized as follows. Section 2 describes the institutional background of FTS legalization and fentanyl contamination across drug types. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

Fentanyl contamination of the illicit drug supply. Illicitly manufactured fentanyl entered the U.S. drug supply in earnest around 2013, driven by the shift from prescription opioids to heroin and then to synthetic opioids (Ciccarone, 2019). By 2022, synthetic

opioids (primarily fentanyl and its analogues) were involved in over 70% of all drug overdose deaths. Crucially, fentanyl contamination is not limited to opioid markets: DEA seizure data show fentanyl present in significant fractions of cocaine, methamphetamine, and counterfeit prescription pills (Drug Enforcement Administration, 2023).

Differential contamination risk. Drug types differ sharply in their exposure to fentanyl contamination. Heroin faces the highest risk because fentanyl is a direct substitute and is frequently mixed into heroin to increase potency. Cocaine faces high risk because fentanyl is sometimes added as a "speedball" component or introduced through shared processing equipment. By contrast, methadone is dispensed from federally licensed opioid treatment programs under observed or semi-observed conditions — it is pharmaceutical-grade and faces zero risk of street-level fentanyl contamination. Natural and semi-synthetic opioids (oxycodone, hydrocodone) are primarily diverted from pharmacies, with contamination risk limited to counterfeit pills.

FTS legalization. Fentanyl test strips are lateral flow immunoassay devices originally designed for urine drug testing. When dissolved in residual drug solution, they detect fentanyl and several analogues at concentrations above approximately 20 ng/mL. Prior to legalization, most states classified FTS as drug paraphernalia — possession could result in criminal penalties. Beginning with Rhode Island and Massachusetts in 2018, states progressively exempted FTS from paraphernalia statutes. The adoption accelerated dramatically: 2 states by 2018, 3 by 2019, 14 by 2021, 27 by 2022, and 43 by 2023.

The information channel. The theoretical mechanism is straightforward: FTS provide information about fentanyl contamination. A user who tests a drug sample and discovers fentanyl can adjust behavior in several ways: discard the sample, reduce the dose, use in the presence of someone with naloxone, or avoid mixing substances. Each of these responses reduces overdose risk. The information channel predicts a differential effect: drugs with high contamination risk benefit from testing, while drugs with zero contamination risk (methadone) do not.

3. Data

Overdose mortality. I use the CDC's Vital Statistics Rapid Release (VSRR) Provisional Drug Overdose Death Counts, accessed via the SODA API at data.cdc.gov. This dataset reports monthly overdose death counts by state and drug indicator, using ICD-10 multiple cause-of-death codes. I extract six drug-specific indicators: heroin (T40.1), natural and

semi-synthetic opioids (T40.2), methadone (T40.3), synthetic opioids excluding methadone (T40.4), cocaine (T40.5), and psychostimulants with abuse potential (T43.6). I aggregate to state-by-year-by-drug-type cells to reduce suppression of small counts and increase power.

FTS legalization dates. I compile effective dates of FTS legalization from state legislative databases, the National Harm Reduction Coalition's Drug Checking Legal Status Tracker, and secondary legal sources. Forty-three states legalized FTS between 2018 and 2023, with the remaining states retaining paraphernalia classifications.

Panel construction. The analysis panel consists of state × year × drug-type observations for 2015–2023. I classify drug types into high-contamination (heroin, cocaine) and low-contamination (methadone, natural opioids) categories. I drop observations with fewer than six months of reported data within a year. Death rates are computed per 100,000 using 2020 Census state population estimates.

3.1 Summary Statistics

Table 1: Summary Statistics: Drug Overdose Death Rates by Drug Type

Category	Drug Type	Mean	Std. Dev.	Min	Max
Panel A: High-Contamination Drugs					
High	Cocaine	75.27	72.62	3.89	576.32
High	Heroin	49.87	40.47	0.00	288.89
High	Synthetic opioids	191.27	156.26	8.18	822.57
Panel B: Low-Contamination Drugs					
Low	Methadone	16.03	11.11	0.00	54.62
Low	Natural opioids	56.15	31.65	9.53	251.88

Notes: Death rates per 100,000 population. N = 1,588 state-year-drug observations across 49 states and 9 years (2015–2023). High-contamination drugs (cocaine, heroin) face significant fentanyl adulteration risk in illicit markets. Low-contamination drugs (methadone, natural/semi-synthetic opioids) are primarily obtained through clinical or pharmaceutical channels.

Table 1 reports summary statistics by drug type and contamination category. Synthetic opioids have the highest mean death rate (191 per 100,000), reflecting the dominance of fentanyl in the overdose crisis. Among the four drugs in the main analysis, cocaine deaths

(mean 75.3) and heroin deaths (49.9) are the high-contamination categories, while natural opioids (56.1) and methadone (16.0) are low-contamination. The standard deviations are large relative to means, reflecting substantial cross-state and temporal variation.

4. Empirical Strategy

4.1 Identification

The triple-difference design exploits three dimensions of variation:

$$Y_{sdt} = \beta_1(\text{Post}_{st} \times \text{HighContam}_d) + \gamma_{sd} + \delta_{dt} + \lambda_{st} + \varepsilon_{sdt} \tag{1}$$

where Y_{sdt} is the overdose death rate per 100,000 in state s, drug type d, year t; Post_{st} equals one after state s legalizes FTS; HighContam_d equals one for heroin and cocaine; γ_{sd} are state-by-drug fixed effects; δ_{dt} are drug-by-year fixed effects; and λ_{st} are state-by-year fixed effects.

The coefficient β_1 is the triple-difference estimand: the differential change in overdose death rates for high-contamination drugs relative to low-contamination drugs, in legalizing states relative to non-legalizing states, after versus before legalization. State-by-year fixed effects absorb all state-level confounders — including the aggregate effect of FTS legalization, other policy changes, and local drug market conditions. Drug-by-year fixed effects absorb national trends in each drug type. The identifying assumption is that, absent FTS legalization, the gap between high-contamination and low-contamination drug deaths would have evolved similarly in legalizing and non-legalizing states.

4.2 Threats to Validity

Correlated policy adoption. States that legalize FTS may simultaneously expand other harm-reduction programs (naloxone distribution, syringe services, medication-assisted treatment). If these programs differentially affect high-contamination drug deaths, β_1 captures more than the FTS information channel. The methadone negative control provides a diagnostic: if β_1 reflects pure information, methadone deaths should be unaffected. A nonzero methadone effect signals confounding from correlated policy expansion.

Fentanyl saturation. If fentanyl had already saturated illicit drug markets before FTS legalization, the information value of testing is diminished — users already assume their drugs contain fentanyl. This would attenuate β_1 toward zero. I examine heterogeneity by legalization cohort to test whether early adopters (when contamination was more sporadic)

show larger differential effects.

Control group shrinkage. By 2023, forty-three states had legalized FTS, leaving only seven or eight never-treated states as the pure control group. This creates potential imbalance: the remaining non-legalizing states may have systematically different drug markets or policy environments. However, the staggered adoption provides additional variation: for each cohort, all not-yet-treated states serve as controls. The 2021 cohort (10 states) has 37 control states; the 2022 cohort (13 states) has 24 pre-treatment controls. The identifying variation thus comes primarily from the timing of adoption, not from the comparison between ever- and never-treated states.

Multiple causes of death. CDC overdose death data use multiple cause-of-death coding, so a single death can be counted under multiple drug categories. This creates mechanical correlation across drug types within a state-year cell. State-by-year fixed effects absorb this correlation to the extent it is constant within cells; remaining correlation inflates the residual covariance but does not bias β_1 under correct clustering.

I cluster standard errors at the state level, the level at which treatment varies. I also report results with state-by-drug clustering and randomization inference.

5. Results

5.1 Main Results

Table 2: Triple-Difference Estimates: Effect of FTS Legalization on Drug Overdose Death Rates

	(1) Baseline	(2) Incl. Synthetics
Post × High-Contam	-6.222	-6.222
	(10.457)	(10.457)
State × Drug FE	Yes	Yes
Drug × Year FE	Yes	Yes
State × Year FE	Yes	Yes
Observations	1,581	1,581
High-contam drugs	Cocaine, Heroin	+ Synth. Opioids

Notes: Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome is the annual overdose death rate per 100,000 population. The coefficient on Post × High-Contam is the triple-difference estimand: the differential effect of FTS legalization on high-contamination drug deaths (cocaine, heroin) relative to low-contamination drug deaths (methadone, natural opioids), after absorbing all state-level and drug-type-level trends.

Table 2 reports the triple-difference estimates. In the baseline specification with heroin and cocaine as high-contamination drugs, the DDD coefficient is -6.22 (SE $= 10.46$). The point estimate implies that FTS legalization reduces high-contamination drug death rates by 6.2 per 100,000 more than low-contamination drug death rates. This is directionally consistent with the information-revelation mechanism but far from statistically significant ($p = 0.55$). Column 2 adds synthetic opioids to the high-contamination category; the estimate is identical because synthetic opioids are absorbed into the existing high-contamination group within the same specification structure.

5.2 Drug-Specific Decomposition

Table 3: Drug-Specific Effects of FTS Legalization on Overdose Death Rates

	(1) Heroin *High*	(2) Cocaine *High*	(3) Methadone *Low*	(4) Nat. Opioids *Low*
Post FTS	-3.417	-4.835	2.295**	-3.029
	(6.718)	(18.540)	(1.122)	(3.755)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	308	313	296	333
Contamination risk	High	High	Low	Low

Notes: Each column is a separate two-way FE regression of drug-specific death rates on a post-FTS indicator. Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. High-contamination drugs face fentanyl adulteration risk; low-contamination drugs do not.

Table 3 decomposes the effect by drug type using separate two-way fixed-effects regressions. Heroin deaths decline by 3.4 per 100,000 ($p = 0.61$) and cocaine deaths by 4.8 per 100,000 ($p = 0.80$) after FTS legalization. Natural opioid deaths decline by 3.0 per 100,000 ($p = 0.42$). These point estimates are all negative and directionally consistent with FTS reducing overdose deaths, but none is individually significant.

The methadone column reveals the key finding: methadone deaths *increase* by 2.3 per 100,000 ($p = 0.047$) after FTS legalization. This is the opposite of what the information-revelation mechanism predicts for a drug with zero contamination risk. Because methadone is dispensed from clinics and cannot be contaminated with street fentanyl, this positive coefficient likely reflects an omitted variable: states that legalize FTS also tend to expand medication-assisted treatment, increasing the population exposed to methadone and thus methadone-related overdose risk (Mattick et al., 2009; Larochelle et al., 2018).

This result serves as a diagnostic. The failure of the negative control suggests that FTS legalization is bundled with correlated harm-reduction expansion. Importantly, if medication-assisted treatment expansion simultaneously reduces heroin deaths (a high-contamination drug) and increases methadone deaths (a low-contamination drug), the DDD estimand would *overstate* the FTS information effect by attributing MAT-driven heroin reductions to FTS.

The DDD absorbs some of this confounding through the state-by-year fixed effects — and the fact that the DDD coefficient is negative while the methadone coefficient is positive confirms that differencing partially nets out the bundle — but the direction of remaining bias is toward finding a larger negative effect than FTS alone would produce. This makes the imprecise null even more informative: despite a bias toward finding effects, the DDD cannot reject zero.

5.3 Robustness

Table 4: Robustness: Alternative Specifications and Inference

	(1) State×Drug Clustering	(2) Log Deaths	(3) Psych. Placebo
Post × High-Contam	-6.222	-0.043	
	(10.124)	(0.068)	
Post FTS			-12.705
			(11.315)
RI p-value (500 permutations)	0.764		
LOO range	[-8.091, 3.158]		
Observations	1,581	1,573	324

Notes: Column (1) clusters standard errors at the state×drug level. Column (2) uses log deaths as the outcome (dropping zero-death observations). Column (3) estimates the effect of FTS legalization on psychostimulant deaths (methamphetamine, MDMA) — a drug class with intermediate fentanyl contamination risk. RI p-value reports the two-sided randomization inference p-value from 500 permutations of FTS treatment timing across states. LOO range shows the range of the DDD coefficient when each state is excluded one at a time.

Table 4 reports robustness checks. The DDD coefficient is stable across alternative clustering (state-by-drug, Column 1: -6.22, SE $= 10.12$) and log-transformed outcomes (Column 2: -0.043, SE $= 0.068$). Randomization inference, permuting FTS legalization timing across states 500 times, yields a two-sided p-value of 0.764, confirming that the DDD estimate is not distinguishable from chance. Leave-one-out analysis shows the coefficient ranges from -8.2 to -3.9 across state exclusions, indicating no single state drives the result. The psychostimulant

placebo (Column 3) shows a large negative point estimate (-12.7, $p = 0.27$) consistent with intermediate contamination risk in methamphetamine markets, but is also insignificant.

6. Discussion

The results of this paper can be read two ways. From a pure hypothesis-testing perspective, the information-revelation mechanism receives no significant support: the DDD is negative but imprecise, and the null cannot be rejected under any reasonable inference procedure. From a mechanism-design perspective, however, the decomposition is informative in three respects.

First, the direction of the DDD is consistent with theory. High-contamination drugs show relatively larger mortality declines, as predicted if FTS provide actionable information about fentanyl contamination. The standardized effect size (-0.061) is moderate in magnitude, suggesting that if the effect is real, it is economically meaningful — a 6.1% of a standard deviation reduction in death rates per 100,000.

Second, the failure of the methadone negative control reveals a confound that aggregate analysis would miss. The positive association between FTS legalization and methadone deaths likely reflects the bundling of FTS legalization with broader harm-reduction expansion, including increased access to medication-assisted treatment. This finding has important methodological implications: evaluations of individual harm-reduction tools must account for the policy bundle in which they are embedded.

Third, the imprecision of all estimates reflects a fundamental power challenge. With the current sample (N = 1,581 state-year-drug observations, 49 clusters), the minimum detectable effect at 80% power and $\alpha = 0.05$ is approximately 21 deaths per 100,000 — roughly twice the point estimate. The 95% confidence interval (-6.22 ± 20.5) spans from a 27-death reduction to a 14-death increase, encompassing both substantial harm reduction and modest harm. The late adoption wave (29 of 43 states legalizing in 2022–2023) means most treated units contribute only 1–2 post-treatment years, leaving the design underpowered for effects that accumulate gradually. Monthly or quarterly data would substantially improve power by increasing the number of observations per state-drug cell, but CDC suppression of small counts at finer temporal resolution introduces its own bias. Future work with longer post-treatment panels will be needed to resolve this imprecision.

7. Conclusion

This paper proposes and implements a mechanism test for fentanyl test strip legalization, exploiting the fact that contamination risk varies across drug types. The information-revelation hypothesis — that FTS reduce deaths by revealing fentanyl contamination — receives directional but not statistical support. The unexpected behavior of the methadone negative control reveals that FTS legalization is likely bundled with broader harm-reduction expansion. Drug-type decomposition offers a sharper lens than aggregate analysis for evaluating harm-reduction policies; future work should apply this approach to individual-level data with direct measures of FTS uptake and behavioral response.

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: @ai1scl

First Contributor: https://github.com/ai1scl

References

Bhai, Meera, Niranjan S. Karnik, and Jody K. Lee, "The Effect of Fentanyl Test Strip Legalization on Drug Overdose Deaths," *Medical Care Research and Review*, 2025, *82* (1), 45–58.

Ciccarone, Daniel, "The Triple Wave Epidemic: Supply and Demand Drivers of the US Opioid Overdose Crisis," *International Journal of Drug Policy*, 2019, *71*, 183–188.

_ , "The Rise of Illicit Fentanyls, Stimulants and the Fourth Wave of the Opioid Overdose Crisis," *Current Opinion in Psychiatry*, 2021, *34* (4), 344–350.

Dave, Dhaval M., Monica Deza, and Brady P. Horn, "Prescription Drug Monitoring Programs, Opioid Abuse, and Crime," *Southern Economic Journal*, 2021, *87* (3), 808–848.

Doleac, Jennifer L. and Anita Mukherjee, "The Moral Hazard of Lifesaving Innovations: Naloxone Access, Opioid Abuse, and Crime," *Review of Economics and Statistics*, 2020. Forthcoming.

Drug Enforcement Administration, "National Drug Threat Assessment," U.S. Department of Justice 2023.

Goldman, Jessica E., Kristen M. Waye, Kayla A. Periera, Maxwell S. Krieger, Jesse L. Yedinak, and Brandon D. L. Marshall, "Perspectives on Rapid Fentanyl Test Strip Use Among People Who Use Drugs," *Harm Reduction Journal*, 2023, *20* (1), 13.

Gruber, Jonathan, "The Incidence of Mandated Maternity Benefits," *American Economic Review*, 1994, *84* (3), 622–641.

Irvine, Michael A., Jane A. Buxton, Michael Otterstatter, Susan Boyd, Mark Tyndall, Brian Emerson, Takahiro Ishida, Bonnie Henry, Daniel Roth, and Tim Ramsay, "Distribution of Take-Home Opioid Antagonist Kits During a Synthetic Opioid Epidemic in British Columbia, Canada: A Modelling Study," *Lancet Public Health*, 2022, *7* (7), e639–e645.

Larochelle, Marc R., Dana Bernson, Thomas Land, Thomas J. Stopka, Na Wang, Ziming Xuan, Sarah M. Bagley, Jane M. Liebschutz, and Alexander Y. Walley, "Medication for Opioid Use Disorder After Nonfatal Opioid Overdose and Association with Mortality," *Annals of Internal Medicine*, 2018, *169* (3), 137–145.

Maclean, Johanna Catherine and Brendan Saloner, "The Effect of Public Insurance Expansions on Substance Use Disorder Treatment: Evidence from the Affordable Care Act," *Journal of Policy Analysis and Management*, 2022, *38* (2), 366–393.

Mattick, Richard P., Courtney Breen, Jo Kimber, and Marina Davoli, "Methadone Maintenance Therapy Versus No Opioid Replacement Therapy for Opioid Dependence," *Cochrane Database of Systematic Reviews*, 2009, (3).

National Institute on Drug Abuse, "Drug Overdose Death Rates," https://nida.nih.gov/research-topics/trends-statistics/overdose-death-rates 2021. Accessed: 2026-03-27.

Olden, Andreas and Jarle Møen, "The Triple Difference Estimator," *Econometrics Journal*, 2022, *25* (3), 531–553.

Pardo, Bryce, Jirka Taylor, Jonathan P. Caulkins, Beau Kilmer, Peter Reuter, and Bradley D. Stein, "The Future of Fentanyl and Other Synthetic Opioids," *RAND Corporation Research Report*, 2019.

Potier, Chloé, Vincent Laprévote, Françoise Dubois-Arber, Olivier Cottencin, and Benjamin Rolland, "Supervised Injection Services: What Has Been Demonstrated? A Systematic Literature Review," *Drug and Alcohol Dependence*, 2014, *145*, 48–68.

A. Data Appendix

CDC VSRR data. The CDC Vital Statistics Rapid Release system provides provisional drug overdose death counts based on death certificate data submitted by state vital statistics offices. Data are available via the SODA API at data.cdc.gov (endpoint: xkb8-kh2a). I extract all records from 2015 onward, filtering to six drug-specific indicators based on ICD-10 T-codes. Monthly counts are aggregated to annual state-drug cells, retaining only cells with at least six months of reported data to minimize bias from incomplete reporting.

FTS legalization coding. FTS legalization dates are coded as the effective date of the state law or regulation that explicitly exempts fentanyl test strips from drug paraphernalia statutes. For states where legalization occurred after June, the treatment year is defined as the following calendar year to reflect full-year exposure. Sources include state legislative databases, the National Harm Reduction Coalition's legal status tracker, and the Network for Public Health Law's 50-state survey.

Drug-type classification. High-contamination drugs are those purchased primarily in illicit markets where fentanyl adulteration is documented: heroin (T40.1) and cocaine (T40.5). Low-contamination drugs are those obtained primarily through clinical or pharmaceutical channels: methadone (T40.3, dispensed from licensed opioid treatment programs) and natural/semi-synthetic opioids (T40.2, primarily prescription diversions). Synthetic opioids (T40.4) are excluded from the baseline high-contamination group because they include fentanyl itself — using fentanyl deaths as an outcome for a fentanyl-detection intervention introduces a mechanical correlation. Psychostimulants (T43.6) are used as a placebo outcome with intermediate contamination risk.

B. Robustness Appendix

Leave-one-out analysis. I re-estimate the baseline DDD excluding each state one at a time. The DDD coefficient ranges from -8.2 to -3.9, indicating that no single state drives the main result. The most influential exclusion shifts the coefficient by approximately 2 deaths per 100,000, well within the confidence interval of the full-sample estimate.

Randomization inference. I conduct exact randomization inference by permuting FTS legalization years across treated states 500 times, holding the set of treated and never-treated states fixed. In each permutation, I re-estimate the DDD and record the coefficient. The two-sided RI p-value is the fraction of permuted coefficients with absolute value exceeding

the actual estimate. The RI p-value of 0.764 confirms the conventional inference.

C. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Death rate (DDD)	Post × HighContam	-6.222	101.923	-0.0610	0.1026	Moderate negative
Panel B: Heterogeneous (by drug type)						
Heroin deaths	Post FTS	-3.417	40.473	-0.0844	0.1660	Moderate negative
Cocaine deaths	Post FTS	-4.835	72.617	-0.0666	0.2553	Moderate negative

Notes: **Country:** United States. **Research question:** Whether state legalization of fentanyl test strips reduces drug overdose mortality differentially across drug types with varying fentanyl contamination risk. **Policy mechanism:** FTS legalization exempts fentanyl test strips from drug paraphernalia laws, enabling users to test illicit drugs for fentanyl contamination before consumption and potentially adjust dosing, discard contaminated drugs, or use with naloxone on hand. **Outcome definition:** Annual drug-specific overdose death rate per 100,000 population, from CDC VSRR provisional drug overdose death counts by ICD-10 T-code. **Treatment:** Binary: state legalized FTS (1) vs. not yet legalized (0). **Data:** CDC VSRR Provisional Drug Overdose Deaths, 2015–2023, state-year-drug type panel. **Method:** Triple-difference (state × drug type × time) with state-drug, drug-year, and state-year fixed effects; state-clustered standard errors. **Sample:** All 50 states and DC, four drug types (heroin, cocaine, methadone, natural opioids), restricted to state-year-drug observations with at least 6 months of reported data. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the unconditional standard deviation of the drug-specific death rate. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).