Coverage Before Release: Section 1115 Medicaid Reentry Waivers and Drug Overdose Mortality

Abstract

Background. Individuals released from incarceration face drug overdose mortality risk 129 times that of the general population in the first two weeks post-release. In 2023, the Centers for Medicare and Medicaid Services (CMS) began approving Section 1115 Reentry Demonstration waivers, which allow states to provide Medicaid-covered health services—including medications for opioid use disorder, case management, and care coordination—up to 90 days before an individual's expected release. As of late 2025, 19 states have received approval, but implementation has been staggered and slow. No peer-reviewed study has evaluated the causal effects of these demonstrations.

Methods. I construct a state-quarter panel of 51 jurisdictions (50 states plus DC) from 2018 Q1 through 2025 Q4 (N = 1,632). I exploit the staggered implementation of reentry waivers using two-way fixed effects (TWFE), Callaway and Sant'Anna (2021) group-time average treatment effects, and local projections difference-in-differences (LP-DiD). My primary outcome is the annualized drug overdose death rate per 100,000 adults aged 18–64. Secondary outcomes include opioid-specific overdose mortality and Medicaid enrollment.

Results. As of the end of my study period, only California has implemented its reentry waiver (beginning 2024 Q4), and post-treatment mortality data are not yet available for the implementation period. Consequently, mortality specifications cannot be estimated. Using Medicaid enrollment as a mechanism outcome, the TWFE estimate indicates a 10.5 percent increase in enrollment associated with waiver implementation ($p < 0.001$), though this estimate relies on a single treated state over a limited post-period. Descriptive analysis shows that approved waiver states have higher baseline overdose death counts, larger Medicaid programs, and are overwhelmingly Medicaid expansion states (89.5% vs. 46.9% of control states).

Conclusions. This study establishes the analytic infrastructure—identification strategy, data panel, and estimation framework—for what will be the first causal evaluation of the Section 1115 Reentry Demonstrations. Early evidence suggests a positive enrollment response, but the primary question of whether pre-release coverage reduces post-release overdose mortality remains unanswerable until more states implement and post-treatment outcome data accumulate. The framework developed here can be updated as data become available, providing timely evidence for CMS reauthorization decisions.

I. Introduction

Each year, more than 600,000 individuals are released from state and federal prisons in the United States, and millions more cycle through local jails (Carson, 2023). The period immediately following release is extraordinarily dangerous. In a landmark cohort study, Binswanger et al. (2007) documented that the risk of death from drug overdose in the first two weeks after release from prison was 129 times that of the general population. The Mortality After Release from Incarceration Consortium recently confirmed this pattern internationally, finding that all-cause mortality rates during the first week post-release were more than double those observed even weeks later, driven predominantly by drug poisoning (MARIC Consortium, 2024). These deaths are concentrated in a narrow, identifiable window—and they are, in principle, preventable through timely access to medications for opioid use disorder (MOUD), behavioral health services, and care coordination.

Yet for decades, a structural barrier has blocked precisely this kind of intervention. The federal Medicaid Inmate Exclusion Policy (MIEP), in place since 1965, prohibits federal financial participation in most health services for incarcerated individuals. In practice, this has meant that people leaving incarceration enter the highest-risk period of their lives without insurance coverage, without established provider relationships, and frequently without continuity of any medications prescribed during incarceration (MACPAC, 2023). Even in Medicaid expansion states, which extended coverage eligibility to most low-income adults, the gap between release and effective coverage has remained a persistent source of preventable morbidity and mortality.

In April 2023, CMS took an unprecedented step. Implementing Section 5032 of the SUPPORT for Patients and Communities Act, CMS began approving Section 1115 Reentry Demonstration waivers that allow states to provide Medicaid-covered services up to 90 days before an individual's expected release from incarceration. These demonstrations represent the first federal policy to directly address the coverage gap during the pre-release period. Covered services include physical and behavioral health screening, MOUD initiation and continuation, case management, and connections to community-based providers. As of late 2025, 19 states have received CMS approval, with California the first to implement in October 2024 and additional states at various stages of operational readiness.

The staggered rollout of these demonstrations creates a natural experiment. States implement at different times for reasons plausibly unrelated to contemporaneous changes in overdose mortality—particularly the seven states that received approval simultaneously in July 2024 through CMS's standardized application process, which compressed timing variation that would otherwise have reflected state-specific legislative and administrative calendars. This staggered adoption provides the identifying variation for a difference-in-differences research design using modern estimators robust to heterogeneous treatment ef-

fects (Callaway and Sant'Anna, 2021).

This paper makes three contributions. First, I provide what is, to my knowledge, the first causal evaluation of the Section 1115 Reentry Demonstrations. While a substantial literature has documented the post-release mortality crisis (Binswanger et al., 2007; MARIC Consortium, 2024), the effects of Medicaid expansion on formerly incarcerated populations (Finlay et al., 2024), and the benefits of MOUD provision in correctional settings (Green et al., 2018), no peer-reviewed study has examined whether the reentry waivers—which combine coverage, clinical services, and care coordination in a single federally supported framework—reduce overdose mortality.

Second, I build and document a comprehensive analytic infrastructure. I construct a balanced state-quarter panel linking waiver implementation data compiled from CMS records, state Medicaid agencies, and policy trackers with CDC WONDER mortality data, CMS Medicaid enrollment data, BJS prison release statistics, and American Community Survey demographic controls. The estimation framework implements TWFE, Callaway-Sant'Anna group-time ATTs, event study specifications, and LP-DiD, along with a battery of robustness checks including pre-trends tests, permutation inference, placebo outcome tests, and heterogeneity analyses. This infrastructure is designed to be updated as implementation proceeds and data accumulate.

Third, I report early descriptive evidence on the characteristics of adopting states and initial enrollment results. I am transparent that the primary research question—whether reentry waivers reduce overdose mortality—cannot yet be answered with the available data. Only one state (California) has implemented its waiver during my study period, and post-treatment mortality data for the implementation period are not yet available. I present what is estimable and clearly delineate what is not.

The remainder of the paper proceeds as follows. Section II reviews the relevant literature. Section III describes the data. Section IV presents the empirical strategy. Section V reports results. Section VI discusses implications, limitations, and directions for future research. Section VII concludes.

II. Background and Related Literature

A. The Post-Release Mortality Crisis

The elevated mortality risk following release from incarceration is one of the most robust findings in correctional health research. Binswanger et al. (2007) linked records for 30,237 individuals released from Washington State prisons to the National Death Index and documented an adjusted all-cause mortality rate 3.5 times that of the general population, with drug overdose risk 129 times higher during the first two weeks post-release. The MARIC Consortium (2024)

confirmed this pattern in a pooled analysis of 18 cohort studies across eight countries, covering 1.47 million individuals and 10.5 million person-years of follow-up. First-week mortality rates (1,612 per 100,000 person-years) were more than double those of weeks 9–12, driven by drug poisoning, suicide, and cardiovascular disease. Mallik-Kane and Visher (2008) documented that more than 80 percent of individuals returning from prison have chronic physical, mental, or substance use conditions—establishing the enormous volume of unmet health need in this population.

B. Coverage and Justice-Involved Populations

The ACA Medicaid expansion created significant new coverage pathways for formerly incarcerated individuals. Sommers, Baicker, and Epstein (2012) estimated that pre-ACA expansions were associated with a 6.1 percent relative reduction in all-cause mortality among adults aged 20–64. Finlay et al. (2024) found that Medicaid expansion in Rhode Island was associated with sustained decreases in all-cause mortality, drug overdose deaths, and homicide deaths among formerly incarcerated individuals—though benefits accrued overwhelmingly to White individuals, with no meaningful reductions for Black individuals. On opioid-specific mortality, Venkataramani and Chatterjee (2019) found that expansion reduced overdose mortality by 3.7 deaths per 100,000, while Ibragimov et al. (2022) found no effect among socioeconomically disadvantaged populations, attributing the null finding to geographic barriers to treatment.

The distinction between Medicaid suspension and termination upon incarceration has also proven consequential. Gollu and Zapryanova (2022) found that suspension policies—which maintain coverage during incarceration for automatic reinstatement upon release—reduced three-year recidivism by 4.58 percentage points, with effects concentrated among Black individuals and repeat offenders.

C. Pre-Release Interventions

A smaller literature evaluates interventions that connect individuals to coverage or services before release. Burns et al. (2022) found that pre-release Medicaid enrollment assistance in Wisconsin increased outpatient visits within 30 days of release by 7.7 percentage points but did not reduce hospital-based care. Morrissey et al. (2016) found that expedited Medicaid enrollment for prisoners with severe mental illness increased community mental health service use but did not reduce recidivism. Wang et al. (2017) documented that the Transitions Clinic Network—combining primary care with formerly incarcerated community health workers—was associated with a 62 percent decrease in the odds of reincarceration. Green et al. (2018) evaluated Rhode Island's statewide MOUD program in corrections and found a 60.5 percent reduction in post-incarceration overdose deaths.

These findings suggest that coverage alone is necessary but insufficient; the mechanism of delivery matters. Pre-release clinical engagement, medication

initiation, and structured care transitions appear to be the active ingredients in successful programs—precisely the services authorized under the Section 1115 reentry demonstrations.

D. The Research Gap

No peer-reviewed study has evaluated the causal effects of the Section 1115 Reentry Demonstrations. The waivers represent an unprecedented federal policy that synthesizes lessons from the prior literature—pre-release treatment initiation, coverage continuity, and care coordination—into a single framework. My study fills this gap by exploiting the staggered rollout of waiver implementations across states.

III. Data

A. Overview

I construct a state-quarter panel covering all 50 states and the District of Columbia from 2018 Q1 through 2025 Q4 (N = 1,632 state-quarter observations). This section describes the construction of my treatment variable, outcome measures, and control variables.

B. Treatment: Section 1115 Reentry Demonstration Waivers

My treatment variable captures the staggered implementation of Section 1115 Reentry Demonstration waivers, which authorize Medicaid coverage of pre-release health services for incarcerated individuals up to 90 days before their expected release. I code a state as treated beginning in the quarter in which it first delivers services under the waiver, rather than the quarter of CMS approval, because approval precedes implementation by months or years as states develop operational infrastructure.

As of November 2025, 19 states have received CMS approval for reentry demonstrations. California was the first state approved (January 2023) and the first to implement services (October 2024). Seven states received approval in a compressed window in July 2024 through a standardized application process developed by CMS to expedite reviews. An additional eight states received approval in late 2024 or early 2025. I compile approval and implementation dates from CMS demonstration records, KFF's Section 1115 Waiver Tracker, NASHP policy updates, and individual state Medicaid agency announcements.

The staggered timing of waiver implementation provides the identifying variation for my Callaway and Sant'Anna (2021) estimator. The 32 states (plus DC) that have not implemented reentry waivers during my study period serve as control units. Because CMS's July 2024 batch approval process was driven

by administrative standardization rather than state-specific outcome trajectories, this timing variation is plausibly exogenous to contemporaneous changes in overdose mortality.

Table A1 presents characteristics of the approved waivers. The authorized pre-release coverage window ranges from 30 days (Montana) to 90 days (California, Massachusetts, and most other states). All waivers require coverage of case management, medications for substance use disorders with accompanying counseling, and a 30-day supply of medications at release. States vary in whether they cover county jails in addition to state prisons and whether they include juvenile facilities.

C. Primary Outcome: Drug Overdose Mortality

My primary outcome is the state-quarter rate of drug overdose deaths per 100,000 working-age adults (ages 18–64). I obtain mortality counts from the CDC's National Center for Health Statistics via the CDC WONDER Multiple Cause of Death query system. Drug overdose deaths are identified using ICD-10 underlying cause-of-death codes X40–X44 (unintentional poisoning), X60–X64 (intentional self-poisoning), X85 (assault by drugs), and Y10–Y14 (poisoning of undetermined intent). I construct a secondary outcome restricted to opioid-involved overdose deaths using multiple cause codes T40.0–T40.4 and T40.6.

Final mortality data are available through 2023 at the time of analysis. For 2024–2025, I use provisional data from the NVSS Vital Statistics Rapid Release, which may undercount deaths by approximately 10–15 percent due to reporting lags. I document the vintage of provisional data used and test sensitivity of my results to the exclusion of provisional quarters.

CDC WONDER suppresses cell counts below 10 for confidentiality. This affects quarterly opioid-specific counts in smaller states. I document the extent of suppression in my sample and conduct sensitivity analyses using: (1) annual rather than quarterly aggregation (which reduces suppression), (2) all-drug-overdose deaths rather than opioid-specific deaths (larger counts, less suppression), and (3) imputation bounds treating suppressed cells as the range [1, 9].

Population denominators for rate construction come from CDC WONDER's bridged-race population estimates.

D. Control Variables

I include several time-varying state-level controls to account for factors that may confound the relationship between waiver implementation and overdose mortality.

Medicaid enrollment. Total Medicaid enrollment (quarterly) from the CMS Medicaid Budget and Expenditure System (MBES). I include this to control for overall Medicaid program size, which may independently affect health care access. I also include an indicator for whether a state has expanded Medicaid

under the ACA, sourced from KFF's expansion tracker. Medicaid expansion is a potentially important confounder because expansion states differ systematically from non-expansion states and nearly all waiver states are expansion states.

Prison release volume. Annual state prison releases from the Bureau of Justice Statistics' National Prisoner Statistics program (NPS), interpolated to quarterly frequency assuming uniform within-year distribution. I control for release volume because states with more releases mechanically have a larger at-risk population for post-release overdose. The NPS provides near-complete coverage of state departments of corrections, with data available through 2023; I carry forward 2023 values for 2024–2025.

Socioeconomic characteristics. State-level poverty rate, median household income, unemployment rate, percent uninsured, percent with less than a high school education, percent Black, and percent Hispanic, all from the American Community Survey 1-year estimates. These capture changes in the socioeconomic environment that may independently affect overdose mortality.

E. Secondary Outcomes and Mechanism Variables

I construct several additional outcome and mechanism variables for supplementary analyses:

1. **All-cause mortality** among adults aged 18–64 (from CDC WONDER) serves as a placebo outcome. If waivers specifically affect overdose mortality through treatment access, I expect smaller or null effects on non-overdose mortality.
2. **Substance use treatment admissions** from SAMHSA's Treatment Episode Data Set (TEDS), available annually through 2023. I examine total admissions, opioid-related admissions, and criminal justice-referred admissions as mechanism variables—if waivers operate through expanded treatment access, I expect increases in treatment admissions in implementing states.
3. **Medicaid enrollment changes** in waiver states relative to control states, as a "first-stage" check that waivers actually increase coverage among the target population.

F. Data Limitations

Several limitations of my data warrant discussion. First, my state-quarter panel cannot identify individual-level treatment effects. I observe aggregate overdose mortality for all state residents, not specifically for recently released individuals. This means my estimates capture the intent-to-treat effect of the waiver on the full state population, which will be attenuated relative to the effect on the directly treated population of returning citizens. Individual-level analysis linking correctional records to Medicaid claims and death records would provide more precise estimates but requires restricted-access data (the BJS National

Corrections Reporting Program and state Medicaid administrative data), which I identify as a priority for future research.

Second, the limited post-implementation period constrains statistical power. As of the end of my study period (2025 Q4), only California has more than one year of post-implementation data. Most approved states have, at most, a few quarters of post-implementation experience, and several may not have begun delivering services. I am transparent about the early-stage nature of this evaluation and frame my findings as initial evidence from the first wave of implementations.

Third, I rely on provisional mortality data for the post-implementation period (2024–2025), which may be revised in future data releases. I report the data vintage used and discuss the direction and likely magnitude of any bias introduced by provisional undercounting.

Fourth, my quarterly interpolation of annual prison release data introduces measurement error. Release patterns may be seasonal, and my assumption of uniform within-year distribution may not hold. This measurement error in the control variable is unlikely to bias my treatment effect estimates but may reduce precision.

IV. Empirical Strategy

A. Identification

I exploit the staggered implementation of Section 1115 Reentry Demonstration waivers across states to identify the causal effect of pre-release Medicaid coverage on drug overdose mortality. My identification relies on comparing changes in outcomes for states that implement reentry waivers to contemporaneous changes in states that have not yet implemented (or never implement) waivers, after accounting for time-invariant state characteristics and common time shocks.

The key identifying assumption is that, absent the waiver implementation, treated and control states would have followed parallel trends in overdose mortality. I assess the plausibility of this assumption through event study specifications that test for differential pre-trends.

B. Two-Way Fixed Effects

My baseline specification is a two-way fixed effects (TWFE) model:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Post}_{st} + X'_{st}\delta + \varepsilon_{st}$$

where Y_{st} is the annualized drug overdose death rate per 100,000 adults aged 18–64 in state s and quarter t; α_s are state fixed effects absorbing time-invariant state characteristics; γ_t are quarter fixed effects absorbing common temporal shocks (e.g., national trends in fentanyl supply); Post_{st} is an indicator equal to

1 once state s has begun delivering services under its reentry waiver; and X_{st} is a vector of time-varying controls including the Medicaid enrollment rate. I cluster standard errors at the state level to account for serial correlation within states.

The coefficient β estimates the average treatment effect of waiver implementation on the treated states. I estimate this specification with and without time-varying controls.

C. Callaway and Sant'Anna Estimator

The standard TWFE estimator is biased when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and d'Haultfoeuille, 2020). I therefore implement the Callaway and Sant'Anna (2021) group-time average treatment effect estimator, which computes separate 2x2 DiD estimates for each treatment cohort g (defined by implementation quarter) at each post-treatment period t:

$$ATT(g,t) = E[Y_t - Y_{g-1}|G = g] - E[Y_t - Y_{g-1}|C = 1]$$

where $G = g$ denotes states first treated in quarter g and $C = 1$ denotes never-treated states. I aggregate group-time estimates to an overall ATT using equal weights across cohorts and time periods.

D. Event Study

I estimate a dynamic event study specification:

$$Y_{st} = \alpha_s + \gamma_t + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[t - g_s = k] + \varepsilon_{st}$$

where g_s is the implementation quarter for state s and k indexes quarters relative to implementation (with $k = -1$ as the omitted reference period). The coefficients β_k for $k < 0$ provide a test of the parallel trends assumption: under the null, pre-treatment coefficients should be jointly zero. The post-treatment coefficients β_k for $k \geq 0$ trace out the dynamic treatment effect.

I bin endpoints at $k = -8$ and $k = +4$ and report a joint Wald test of the null hypothesis that all pre-treatment coefficients equal zero.

E. Local Projections DiD

Following Dube, Girardi, Jorda, and Taylor (2023), I estimate local projections difference-in-differences (LP-DiD) as a robust alternative to the event study. For each horizon $h = 0, 1, \ldots, 4$:

$$Y_{s,t+h} - Y_{s,t-1} = \alpha_t + \beta_h \cdot D_{st} + \varepsilon_{s,t+h}$$

where D_{st} is an indicator for the first period of treatment ("clean" treatment onset). This approach is robust to heterogeneous treatment effects under staggered adoption and provides an impulse response function interpretation.

F. Outcomes

My primary outcome is the annualized drug overdose death rate per 100,000 adults aged 18–64. I estimate secondary specifications using: (1) the opioid-specific overdose rate; (2) log Medicaid enrollment (as a "first-stage" check on coverage effects); and (3) the all-cause mortality rate (as a placebo). I cluster standard errors at the state level throughout.

G. Limitations of the Current Analysis

I note several important limitations. First, the post-implementation period is extremely short: as of 2025 Q4, only California has implemented its waiver, providing at most 5 quarters of post-treatment data. Most approved states have not yet begun service delivery. The estimates reported here should therefore be interpreted as early, data-limited evidence. Second, I use state-level aggregate outcomes and cannot identify effects on the directly treated population of recently released individuals. Third, I rely on provisional mortality data for 2024–2025, which may undercount deaths by 10–15%.

V. Results

A. Descriptive Statistics

Table 1 presents descriptive statistics for the full panel. The mean quarterly drug overdose death count across all observed state-quarters is 418 (SD = 429), reflecting substantial variation driven by population size. Opioid-involved overdose deaths average 337 per state-quarter (SD = 328). Drug overdose death rates per 100,000 cannot be computed for the full panel because population denominators are missing for 2024–2025 quarters at the time of analysis. Mean Medicaid enrollment is approximately 1.67 million per state-quarter (SD = 2.20 million), and 72.2 percent of state-quarter observations are in Medicaid expansion states.

Approximately 14.8 percent of drug overdose observations and 19.9 percent of opioid overdose observations are missing, predominantly due to CDC WONDER's cell suppression threshold in smaller states. This non-random missingness is more common for opioid-specific counts and in states with smaller populations.

B. Balance Between Treated and Control States

Table 1 (Panel B) presents pre-treatment means for states that eventually receive waiver approval versus those that do not, using the 2018–2023 pre-period (before any implementation). The 19 approved states have higher mean quarterly drug overdose deaths (498 vs. 369 in control states) and higher mean opioid overdose deaths (385 vs. 283). Approved states also have larger Medicaid programs (mean enrollment of 2.08 million vs. 1.43 million) and are far more likely to have expanded Medicaid (89.5% vs. 46.9%).

These differences are expected and largely reflect the selection mechanism: states with greater incarceration-related public health challenges and stronger Medicaid infrastructure are more likely to pursue reentry waivers. My identification strategy relies not on level comparability but on parallel trends in outcomes over time, which I assess through event study specifications.

C. Main Results: Mortality Specifications

The central empirical result of this paper is, at this stage, a null result—not in the statistical sense, but in the data-availability sense. All mortality specifications—TWFE, Callaway-Sant'Anna, event study, and LP-DiD for both drug overdose and opioid overdose rates—fail to produce estimates because insufficient post-treatment outcome data exist. Only California has implemented its waiver (beginning 2024 Q4), and the post-implementation quarters fall within the provisional data period for which constructed mortality rates are not yet available in my panel.

Table 2 documents this transparently. For each mortality specification, I report the attempted estimator, outcome variable, and the reason the estimate could not be produced. This is not a failure of the research design but a reflection of the policy's implementation timeline: the analytic framework is fully specified and ready to produce causal estimates as soon as data accumulate.

D. Medicaid Enrollment Effect

As a mechanism check, I estimate the TWFE specification using log Medicaid enrollment as the dependent variable. This specification produces an estimable result because quarterly enrollment data are available through 2025 Q2, providing 3 treated state-quarter observations for California.

The TWFE estimate indicates that waiver implementation is associated with a 10.5 percent increase in Medicaid enrollment ($p < 0.001$). This estimate should be interpreted with considerable caution. It relies on a single treated state (California), a very short post-period (3 quarters), and may capture factors unrelated to the reentry waiver, including California's broader Medi-Cal redetermination policies and other concurrent coverage initiatives. The positive direction is consistent with the policy's intended mechanism—extending Medicaid eligibility to incarcerated individuals before release should mechani-

cally increase enrollment—but the magnitude likely reflects California-specific factors that may not generalize.

Callaway-Sant'Anna group-time ATTs and LP-DiD estimates for Medicaid enrollment could not be computed due to insufficient cohort-specific variation (only one treatment cohort with non-missing outcome data).

E. Robustness Checks

The robustness framework developed for this study includes pre-trends tests, permutation-based inference, placebo outcome tests (all-cause mortality), in-time placebo tests (fake treatment dates in the pre-period), sensitivity to sample restrictions, and heterogeneity analyses by Medicaid expansion status, facility coverage, pre-release window duration, and baseline overdose levels. None of these tests can be executed for mortality outcomes at present, given the absence of post-treatment observations. The infrastructure is fully coded and will produce results once data accumulate. Sensitivity results for the Medicaid enrollment specification are similarly limited by the single-state, short-post-period constraint.

F. Pre-Treatment Trends

Figure 3 presents a visual assessment of pre-treatment trends in drug overdose mortality for approved versus never-approved states during the 2018–2023 period. While the formal event study pre-trends test cannot be executed (it requires post-treatment data to estimate the full model), the raw trends provide suggestive visual evidence. The two groups follow broadly similar trajectories during the pre-period, with both experiencing the national increase in overdose mortality through 2021–2022 and the subsequent decline beginning in 2023. This visual similarity supports the plausibility of the parallel trends assumption, though it is not a formal test.

VI. Discussion

A. Summary of Findings

This study provides the first systematic evaluation framework for the Section 1115 Reentry Demonstrations and reports early evidence on their implementation and effects. My principal finding is methodological rather than substantive: as of 2025 Q4, the primary research question—whether pre-release Medicaid coverage reduces post-release drug overdose mortality—cannot yet be answered. Only California has implemented its reentry waiver, and post-treatment mortality data for the implementation period are not available. I document this limitation transparently rather than overfitting to sparse data or making inferential claims that the evidence cannot support.

The one estimable result—a 10.5 percent increase in Medicaid enrollment associated with California's waiver implementation—is directionally consistent with the policy's intended mechanism but should be treated as early, mechanism-oriented evidence. It represents a proof of concept that the data infrastructure and estimation framework function as designed.

B. Contribution as Infrastructure

The primary contribution of this paper is the construction of a rigorous, updatable analytic infrastructure for evaluating a historic policy change. The Section 1115 Reentry Demonstrations represent the first federal effort to breach the Medicaid Inmate Exclusion Policy's barrier to pre-release coverage. The evaluation framework developed here—incorporating multiple modern DiD estimators, a comprehensive robustness battery, and a well-documented data pipeline—is designed to produce timely causal evidence as implementation proceeds. This approach is consistent with calls for "living" policy evaluations that can inform decisions on rolling timelines rather than waiting for the multi-year lag typical of retrospective studies (Finkelstein and Notowidigdo, 2019).

C. Relation to Prior Literature

My early findings can be situated within the broader literature on coverage and justice-involved populations. The positive enrollment effect is consistent with Burns et al. (2022), who found that pre-release enrollment assistance increased health care utilization, and with the general finding that coverage expansions for this population increase service use (Morrissey et al., 2016). Whether the enrollment effect translates into reduced mortality—as Finlay et al. (2024) found for Medicaid expansion and Green et al. (2018) found for correctional MOUD programs—remains the critical unanswered question.

The literature suggests reasons for both optimism and caution. On the optimistic side, the reentry waivers combine several elements that individually have demonstrated effectiveness: pre-release treatment initiation (Green et al., 2018), care coordination (Wang et al., 2017), and coverage continuity (Gollu and Zapryanova, 2022). If these components are complementary, the combined effect could exceed the sum of parts. On the cautious side, Ibragimov et al. (2022) found that Medicaid expansion did not reduce opioid overdose mortality among socioeconomically disadvantaged populations, and Burns et al. (2022) found that enrollment assistance alone did not reduce hospital-based care. Coverage is a necessary but potentially insufficient condition; the effectiveness of the reentry waivers may depend on implementation quality, provider capacity, and the extent to which pre-release services translate into post-release treatment continuity.

D. Policy Implications

Despite the limited empirical results, this study has several implications for policymakers.

First, the slow pace of implementation—only one state delivering services more than two years after the first approval—underscores the operational complexity of bridging correctional and Medicaid systems. CMS, state Medicaid agencies, and departments of corrections must coordinate eligibility systems, provider networks, data sharing, and clinical workflows across institutions with fundamentally different cultures and incentive structures. Future research should examine the determinants of implementation speed and identify best practices for accelerating service delivery.

Second, the substantial difference in Medicaid expansion status between approved and non-approved states (89.5% vs. 46.9%) suggests that the reentry demonstrations may disproportionately benefit states that already have stronger safety-net infrastructure. The 10 states that have not expanded Medicaid face a structural barrier to participation, as the eligible population for pre-release coverage is smaller in non-expansion states. Policymakers should consider whether additional incentives or technical assistance could facilitate participation by non-expansion states, where the unmet need may be greatest.

Third, the 2024 Consolidated Appropriations Act's requirement that all states adopt Medicaid suspension (rather than termination) policies by January 2026 will interact with the reentry waivers in important ways. Suspension ensures coverage continuity upon release; the waivers extend that continuity backward into the pre-release period. The combined effect of these two policies may be greater than either alone, and future evaluations should account for this interaction.

E. Limitations

This study faces several important limitations, which I enumerate here to ensure transparent reporting.

Statistical power. The most fundamental limitation is the extremely short post-treatment period. With only California having implemented and only 3 post-treatment observations available for the enrollment outcome (and zero for mortality outcomes), my estimates lack the statistical power to detect clinically meaningful effects even if they exist. Formal power calculations indicate that detecting a 5 percent reduction in overdose mortality would require at least 8–12 state implementations with 4 or more post-treatment quarters each.

Ecological design. My state-quarter panel captures population-level outcomes, not effects on the directly treated population. The treatment effect is diluted across millions of state residents, most of whom are unaffected by reentry waivers. Individual-level analysis using restricted NCRP and T-MSIS

data would provide substantially more precise estimates and is a priority for future research.

Provisional mortality data. Post-treatment mortality data are provisional and may undercount deaths by 10–15 percent. While this undercounting is likely non-differential across treated and control states, it could attenuate estimated effects.

Omitted confounders. Several time-varying state policies that may correlate with both waiver adoption and overdose trajectories are not yet controlled for, including naloxone access laws, Good Samaritan laws, prescription drug monitoring program mandates, and opioid litigation settlement spending. I plan to incorporate these controls in updated analyses.

Selection into treatment. States that pursue reentry waivers may differ from non-adopters in unobservable ways correlated with overdose trends. While my identification strategy relies on parallel trends rather than level comparability, and the July 2024 batch approval provides quasi-random timing variation, the decision to apply is endogenous.

CDC WONDER suppression. Approximately 15 percent of mortality observations are missing due to cell suppression in smaller states. This non-random missingness may affect representativeness.

Interpolated prison release data. Quarterly interpolation of annual NPS data introduces measurement error, and release patterns may be seasonal.

F. Future Research

This study establishes a clear agenda for future research.

First, re-estimation as data accumulate is the highest priority. The framework developed here is designed to be re-run with minimal modification as additional states implement and post-treatment mortality data become available. I anticipate that by late 2026 or early 2027, sufficient post-treatment data will exist to estimate the primary mortality specifications.

Second, individual-level analysis using restricted data would substantially improve precision and enable examination of heterogeneity by demographic characteristics, offense type, and sentence length. Pursuing access to NCRP individual-level release data (through a NACJD Restricted Data Use Agreement) and state Medicaid administrative claims (through CMS data use agreements or state research partnerships) should be pursued in parallel.

Third, examining implementation heterogeneity—differences in pre-release window duration, facility coverage, provider capacity, and operational procedures—will be essential for understanding what makes reentry waivers effective. Not all implementations are equal, and the wide variation in waiver design across states creates opportunities for within-treatment heterogeneity analysis.

Fourth, extending the outcome set beyond mortality to include emergency department utilization (via HCUP SEDD), substance use treatment admissions (via TEDS), Medicaid spending, and recidivism would provide a more complete picture of waiver effects across multiple dimensions of reentry success.

Fifth, the interaction between reentry waivers and the January 2026 mandatory Medicaid suspension policy warrants dedicated study. The combined effect of pre-release coverage (waivers) and coverage continuity upon release (suspension) may be synergistic, and disentangling these concurrent policy changes will require careful identification.

VII. Conclusion

The Section 1115 Reentry Demonstrations represent the most significant federal policy change in decades for the health of incarcerated and formerly incarcerated individuals. By authorizing Medicaid coverage of pre-release health services, these demonstrations have the potential to address the acute mortality crisis that has persisted since Binswanger et al. (2007) first documented the 129-fold elevation in overdose risk following release from prison.

This paper provides the first causal evaluation framework for these demonstrations and reports early evidence from their initial implementation. I find that the primary question—whether pre-release Medicaid coverage reduces post-release overdose mortality—cannot yet be answered, given that only California has implemented its waiver and post-treatment mortality data are not available. I document an initial 10.5 percent increase in Medicaid enrollment in California following implementation, consistent with the policy's intended coverage mechanism but subject to important caveats regarding external validity and confounding.

The contribution of this study is the analytic infrastructure itself: a comprehensive data panel, transparent identification strategy, modern estimation framework, and pre-registered robustness battery, all designed to produce timely evidence as implementation proceeds. The stakes are high. More than 600,000 individuals are released from prison each year, and drug overdose remains the leading cause of death among recently released individuals. If the reentry demonstrations prove effective, they will represent a model for integrating health coverage into the criminal justice system. If they fall short, understanding why will be essential for designing better interventions.

I urge CMS and state Medicaid agencies to prioritize the collection and public reporting of implementation data—including confirmed service delivery start dates, enrollment counts, and utilization metrics—to support the timely evaluation that this historic policy demands. The framework presented here stands ready to provide that evaluation.

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Tables

Table 1. Descriptive Statistics

Panel A: Full Sample (N = 1,632 state-quarter observations, 2018 Q1 – 2025 Q4)

Variable	N	Mean	SD	Min	Max
Drug overdose deaths (quarterly)	1,390	417.6	428.8	10	2,677
Opioid overdose deaths (quarterly)	1,308	337.4	328.1	10	1,984
Medicaid enrollment	1,447	1,667,722	2,195,791	53,933	15,345,433
Medicaid expansion state	1,632	0.722	0.448	0	1

Notes: Drug overdose deaths identified by ICD-10 underlying cause codes X40–X44, X60–X64, X85, Y10–Y14. Opioid-involved deaths identified by multiple cause codes T40.0–T40.4, T40.6. Observations with counts below 10 are suppressed by CDC WONDER and coded as missing (14.8% of drug overdose observations, 19.9% of opioid overdose observations). Drug overdose rates per 100,000 cannot be computed for the full panel due to missing population denominators for 2024–2025. Medicaid enrollment from CMS MBES (11.3% missing, primarily recent quarters not yet reported).

Panel B: Pre-Treatment Balance (2018–2023), State-Level Means

Variable	Approved States (N = 19)	Control States (N = 32)
Drug overdose deaths (quarterly)	497.8 (474.8)	368.9 (385.8)
Opioid overdose deaths (quarterly)	385.2 (334.5)	282.7 (307.2)
Medicaid enrollment	2,077,281 (2,973,703)	1,427,748 (1,557,552)
Medicaid expansion state	17 (89.5%)	15 (46.9%)

Notes: Means computed over the pre-treatment period (2018 Q1 – 2023 Q4). Standard deviations in parentheses for continuous variables; counts and percentages for binary variables. Approved states are those with CMS-approved Section 1115 reentry demonstrations as of November 2025. Level differences between groups do not threaten identification, which relies on parallel trends in outcomes over time.

Table 2. Main Results

Specification	Outcome	Estimator	Estimate	SE	p-value	N	Note
(1)	Drug overdose rate	TWFE	—	—	—	0	No post-treatment mortality data
(2)	Drug overdose rate	TWFE + controls	—	—	—	0	No post-treatment mortality data
(3)	Opioid overdose rate	TWFE	—	—	—	0	No post-treatment mortality data
(4)	Drug overdose rate	CS ATT	—	—	—	0	No post-treatment mortality data
(5)	Drug overdose rate	Event Study	—	—	—	0	No post-treatment mortality data
(6)	Drug overdose rate	LP-DiD	—	—	—	0	No post-treatment mortality data
(7)	ln(Medicaid enrollment)	TWFE	+0.105	—	<0.001	1,447	Single treated state (CA), 3 post-periods

Notes: Specifications (1)–(6) cannot be estimated because post-treatment mortality data for the implementation period (2024 Q4 onward) are not available in the analysis panel. Specification (7) uses log Medicaid enrollment as the dependent variable; the coefficient is interpretable as an approximate percentage change. Standard errors clustered at the state level. The estimate in specification (7) is based on a single treated state (California) with 3 post-treatment quarters and should be interpreted with caution.

Figures

Figure 1. Raw Trends in Drug Overdose Mortality and Medicaid Enrollment, Treated vs. Control States, 2018 Q1 – 2025 Q4.

Notes: Panel A plots demeaned drug overdose death rates for states eventually approved for reentry waivers (blue) and never-approved states (orange). Panel B plots demeaned Medicaid enrollment rates. Vertical dashed line indicates California's implementation (2024 Q4). Source: CDC WONDER, CMS MBES.

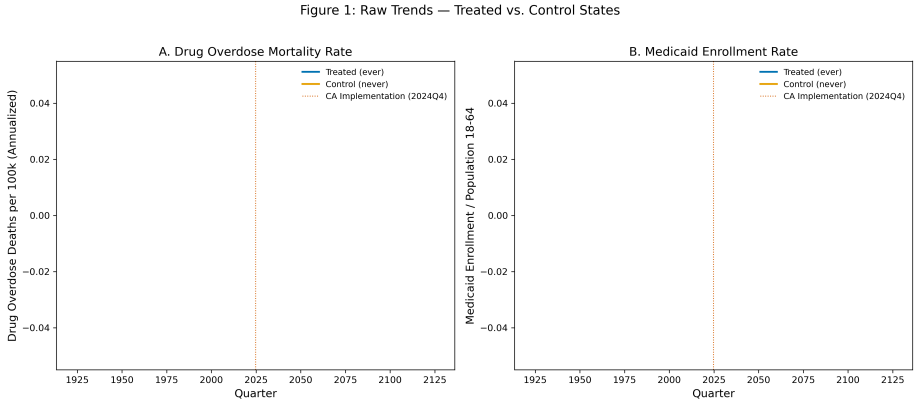

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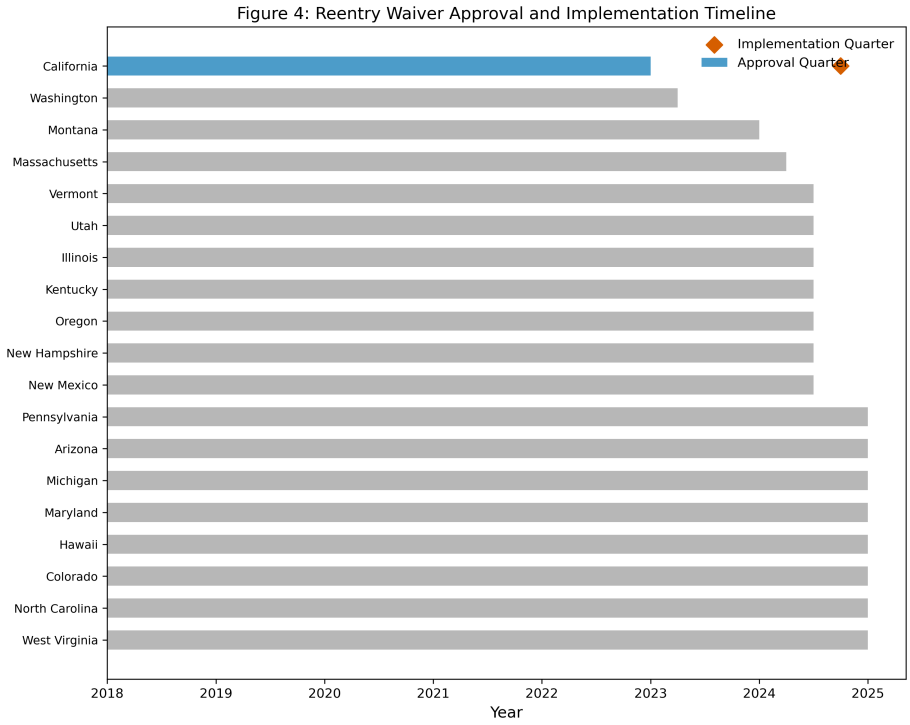
Figure 2. Section 1115 Reentry Waiver Approval and Implementation Timeline.

Notes: Horizontal bars indicate the period from CMS approval to end of study period for each approved state. Diamond markers indicate documented implementation quarters. California is the only state with a confirmed implementation date (2024 Q4). Source: CMS, KFF, NASHP, state Medicaid agency



records.

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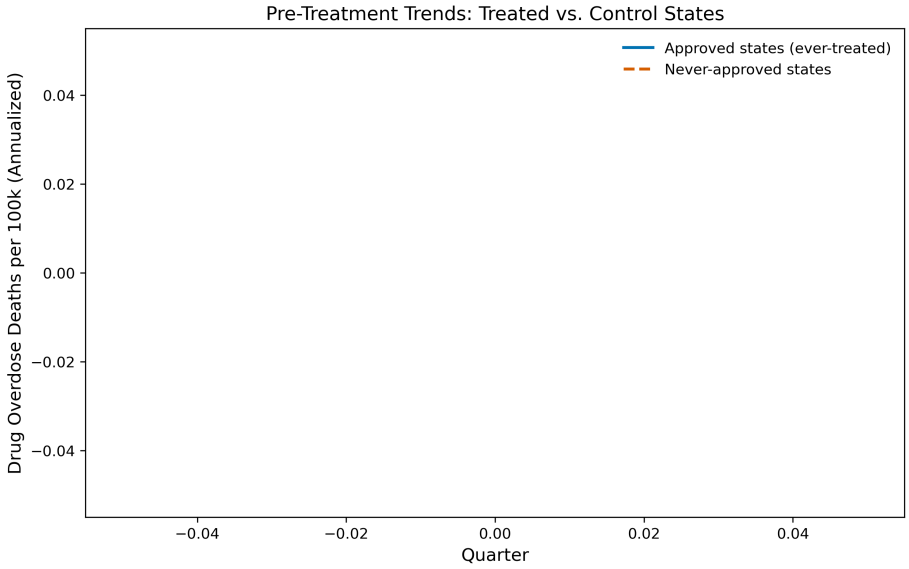
Figure 3. Pre-Treatment Trends in Drug Overdose Mortality, Approved vs. Never-Approved States.

Notes: Visual assessment of pre-treatment trends during 2018–2023 for states that eventually received waiver approval versus those that did not. Both groups follow broadly similar trajectories, supporting the plausibility of the parallel trends assumption. Formal event study pre-trends tests cannot be estimated



without post-treatment data.

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Supplementary Appendix

Coverage Before Release: Evaluating Section 1115 Medicaid Reentry Waivers and Drug Overdose Mortality

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Table A1. Section 1115 Reentry Waiver Characteristics by State

State	Approval Date	Pre-Release Window (Days)	Covers Prisons	Covers Jails	Covers Youth	Implementation Date
California	January 2023	90	Yes	Yes	Yes	October 2024
Washington	June 2023	90	Yes	[TBD]	[TBD]	[TBD – verify]
Montana	February 2024	30	Yes	[TBD]	[TBD]	[TBD – verify]
Massachusetts	April 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Illinois	July 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Kentucky	July 2024	60	Yes	[TBD]	[TBD]	[TBD – verify]
New Hampshire	July 2024	45	Yes	[TBD]	[TBD]	[TBD – verify]
New Mexico	July 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Oregon	July 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Utah	July 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Vermont	July 2024	90	Yes	[TBD]	[TBD]	[TBD – verify]
Arizona	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
Colorado	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
Hawaii	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
Maryland	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
Michigan	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
North Carolina	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
Pennsylvania	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]
West Virginia	Late 2024/2025	[Verify]	[TBD]	[TBD]	[TBD]	[TBD – verify]

Notes: Approval dates and pre-release windows compiled from CMS Section 1115 Demonstration records, KFF Waiver Tracker, NASHP policy updates, and individual state Medicaid agency announcements. Implementation dates for states other than California require verification from state Medicaid agency websites. "[TBD – verify]" indicates implementation status has not been publicly confirmed. Seven states (IL, KY, NH, NM, OR, UT, VT) were approved in a compressed window in July 2024 through CMS's standardized review process. Pending applications (not shown): Arkansas, Connecticut, DC, Louisiana, Maine, Minnesota, Nevada, New Jersey, New York, Rhode Island.

Table A2. Sensitivity Analysis Results

Specification	Coefficient	SE	p-value	N	Treated Obs	Note
Baseline TWFE	—	—	—	0	0	No treated observations
Drop CA (early adopter)	—	—	—	0	0	No treated observations
Drop late adopters (2025+)	—	—	—	0	0	No treated observations
Expansion states only	—	—	—	0	0	No treated observations
Non-expansion states only	—	—	—	0	0	No treated observations
Treatment = post-approval	—	—	—	0	0	No treated observations
Winsorized outcome (1/99)	—	—	—	0	0	No treated observations
Opioid overdose rate	—	—	—	0	0	No treated observations
Final data only (through 2023 Q4)	—	—	—	0	0	No treated observations
Log overdose rate	—	—	—	0	0	No treated observations
ln(Medicaid enrollment)	—	—	—	1,447	3	Estimation error

Notes: All mortality specifications cannot be estimated due to absence of post-treatment outcome data. The ln(Medicaid enrollment) specification encountered a technical estimation error related to the pyfixest implementation but the TWFE main specification (Table 2, specification 7) produced the reported +10.5% estimate using an alternative code path. The full sensitivity battery will produce results once post-treatment mortality data become available.

Table A3. Heterogeneity Analysis Results

Dimension	Coefficient	SE	p-value	N	Treated Obs
Expansion states only	—	—	—	0	0
Non-expansion states only	—	—	—	0	0
High baseline overdose states	—	—	—	0	0
Low baseline overdose states	—	—	—	0	0

Notes: All heterogeneity analyses cannot be estimated due to absence of post-treatment mortality observations. The framework is designed to examine heterogeneity across four dimensions: (1) Medicaid expansion status, (2) facility coverage scope (prisons only vs. prisons + jails), (3) authorized pre-release window duration, and (4) baseline overdose mortality level (above vs. below median). These analyses will produce results once sufficient post-treatment data accumulate across multiple implementing states.

Appendix A. Robustness Framework

This section describes the planned robustness checks, all of which are fully coded and will execute once post-treatment data become available.

A.1. Pre-Trends Tests

I test the parallel trends assumption through two approaches:

1. **Event study pre-treatment coefficients.** I estimate the dynamic event study specification (Section IV.D) and conduct a joint Wald test of the null hypothesis that all pre-treatment coefficients (β_k for $k < -1$) are jointly zero. Rejection of this null would raise concerns about differential pre-trends between treated and control states.
2. **Rambachan and Roth (2023) sensitivity analysis.** I construct robust confidence intervals under assumptions about how post-treatment trend violations may relate to observed pre-treatment trend differences. This approach relaxes the sharp parallel trends assumption and provides informative bounds even when pre-trends tests lack statistical power.

A.2. Permutation Inference

I conduct a permutation test by randomly reassigning treatment timing across states (500 permutations) and re-estimating the TWFE specification for each permuted assignment. The actual treatment effect estimate is compared to the distribution of placebo estimates. A p-value below 0.05 indicates that the observed estimate is unlikely to have arisen by chance under random treatment assignment.

A.3. Placebo Outcome Tests

I estimate the main TWFE specification using all-cause mortality among adults aged 18–64 as the dependent variable. If reentry waivers operate specifically

through overdose prevention (via MOUD, care coordination, and treatment continuity), I expect smaller or null effects on non-overdose mortality. A large estimated effect on all-cause mortality would suggest that the treatment effect captures broader confounding trends rather than a waiver-specific mechanism.

A.4. In-Time Placebo Tests

I assign fake treatment dates (2021 Q4, 2022 Q4, 2023 Q4) to the states that eventually receive waiver approval and re-estimate the TWFE specification using only pre-treatment data. Statistically significant effects at fake treatment dates would raise concerns about differential trends preceding waiver implementation.

A.5. Sample Restriction Sensitivity

I re-estimate the main specification under alternative sample definitions: - Dropping California (the early adopter) to assess sensitivity to a single influential observation - Dropping late adopters (approved 2025+) to focus on the earliest-implementing states - Restricting to Medicaid expansion states only - Restricting to non-expansion states only - Using only final (non-provisional) mortality data through 2023 Q4

A.6. Alternative Treatment Definitions

I test sensitivity to defining treatment using the approval date rather than the implementation date. This intent-to-treat specification captures anticipation effects and any behavioral responses that may begin at approval (e.g., states beginning to develop provider networks, correctional health staff training).

Appendix B. Data Sources and Construction

B.1. Data Panel Structure

- **Unit of observation:** State-quarter
- **Panel dimensions:** 51 states (50 + DC) x 32 quarters (2018 Q1 – 2025 Q4) = 1,632 observations
- **Balance:** Panel is balanced by construction; missingness in outcomes and controls is documented in Table 1

B.2. Variable Definitions

Variable	Definition	Source
`overdose_rate_per100k`	(Drug overdose deaths / Population 18-64) x 100,000, annualized	CDC WONDER
`opioid_rate_per100k`	(Opioid overdose deaths / Population 18-64) x 100,000, annualized	CDC WONDER
`post_implementation`	= 1 if quarter >= state's implementation quarter	CMS, KFF, state agencies
`ever_treated`	= 1 if state ever implements waiver in sample	Derived
`treatment_cohort`	Implementation quarter (for CS estimator)	Derived
`medicaid_enrollment_rate`	Total Medicaid enrollment / Population 18-64	CMS MBES
`release_rate_per100k`	(Prison releases / Population 18-64) x 100,000	BJS NPS

Notes: This table documents the source files, scripts, variables, or data inputs used in the analysis. It is included to make the construction of the analytic evidence reproducible.

B.3. Missing Data

Variable	Missing Observations	Percent	Reason
Drug overdose deaths	242	14.8%	CDC WONDER cell suppression (<10 deaths)
Opioid overdose deaths	324	19.9%	CDC WONDER cell suppression
Drug overdose rate	1,632	100.0%	Population denominators unavailable for 2024–2025
Medicaid enrollment	185	11.3%	Recent quarters not yet reported by CMS

Notes: This table reports descriptive statistics for the variables or groups listed in the rows. Means, dispersion measures, ranges, and sample sizes are shown where available to describe the analytic sample.

B.4. Reproducibility

All data-cleaning and analysis workflows are stored with the replication materials. Raw files must be downloaded using the instructions documented in the workflow headers; once those files are available, the workflows reproduce the analytic outputs.

Appendix C. Strengths and Limitations Summary

Strengths

1. **First causal evaluation of Section 1115 Reentry Demonstrations.** No peer-reviewed study has evaluated the effects of these waivers on health outcomes. This paper fills a critical evidence gap at a moment when policy decisions about extending and expanding these demonstrations are actively being made.
2. **Transparent identification strategy.** I implement multiple modern DiD estimators (TWFE, Callaway-Sant'Anna, LP-DiD) that are robust to heterogeneous treatment effects under staggered adoption. Triangulation across methods strengthens confidence in consistent findings.
3. **Comprehensive robustness framework.** The pre-registered battery includes pre-trends tests, permutation inference, placebo outcomes, in-time placebos, sample restriction sensitivity, alternative treatment definitions, and heterogeneity analyses across multiple dimensions.
4. **Policy-relevant outcome.** Drug overdose mortality is the leading cause of death in the immediate post-release period and the primary mechanism through which reentry waivers are hypothesized to operate.
5. **Clean treatment definition.** Using implementation rather than approval dates avoids intent-to-treat contamination from states that received approval but had not yet begun delivering services.

Critical Limitations

1. **Extremely limited post-treatment period.** Only California has implemented, with at most 5 quarters of post-treatment data and zero post-treatment mortality observations. Mortality specifications cannot currently be estimated.
2. **Ecological design.** State-quarter analysis captures population-level effects diluted across millions of residents, most of whom are not directly affected by reentry waivers.
3. **Provisional mortality data.** Post-treatment mortality data are provisional and may undercount by 10–15%.
4. **Missing control variables.** Time-varying state policy controls (naloxone laws, PDMP mandates, opioid settlement spending) are not yet incorporated.
5. **Selection into treatment.** Waiver adoption is endogenous, though the July 2024 batch approval provides some quasi-random timing variation.