# Medicaid Expansion, Coverage Gains, and Mortality After Prison Release


## Abstract

Each year, over 600,000 adults are released from U.S. state prisons into communities where they face mortality rates 12 times higher than the general population, with drug overdose risk elevated 129-fold in the first two weeks. The Affordable Care Act's Medicaid expansion extended eligibility to most formerly incarcerated adults, potentially reducing mortality risk through improved healthcare access. Using the Census Bureau's Justice Outcomes Explorer, which links criminal justice records to federal administrative data on mortality, Medicaid enrollment, and employment, I estimate the effect of Medicaid expansion on post-release outcomes under a staggered difference-in-differences design keyed to release-year exposure. The raw analytic panel covers 23 states, but mortality estimation support is 17 / 15 / 14 states at the 1-, 3-, and 5-year post-release horizons, with 4 never-treated comparison states. The clearest finding is a one-year Medicaid first-stage of +15.9 percentage points under Local Projections Difference-in-Differences (LP-DiD; p = 0.046); first-stage estimates at longer horizons are sensitive to estimator and pretrend specification and should not be read as standalone coverage results. Mortality estimates are bounded but statistically indistinguishable from zero at the one-year horizon under all estimators, with pretrend tests failing at all three horizons under the release-year clock; I therefore interpret the mortality result as a bounded null with pretrend violations rather than a clean causal estimate. A heuristic relative-magnitudes sensitivity analysis (not a formal HonestDiD implementation) leaves the null contained through $\bar{M} = 2.0$. A negative one-year W-2 employment signal (TWFE+Controls: -3.34 pp, p = 0.018; cohort-weighted: -3.39 pp, p = 0.006) is reported cautiously: pretrends fail at every horizon and LP-DiD is degenerate at h=1/h=3. Exploratory heterogeneity and dose-response analyses are reported as hypothesis-generating only. The results support a coverage-first, bounded-null reading rather than a definitive mortality conclusion, and motivate Section 1115 reentry demonstration waivers as the next policy test.


**Keywords:** Medicaid expansion, post-release mortality, incarceration, difference-in-differences, CJARS

## 1. Introduction

The United States incarcerates more people per capita than any other nation. With approximately 1.2 million adults held in state and federal prisons and an

additional 660,000 in local jails, the scale of American incarceration is without international precedent (Carson 2024; Zeng and Minton 2023). Each year, approximately 600,000 adults are released from state prisons and face dramatically elevated mortality risk: drug overdose, the leading cause of post-release death, occurs at rates 129 times higher than in the general population during the first two weeks after release, as reduced opioid tolerance during incarceration collides with renewed drug exposure (Binswanger et al. 2007). This mortality crisis exists within the broader context of mass incarceration's population health consequences, which disproportionately affect Black communities—nearly one in three Black men will experience imprisonment—and contribute measurably to U.S. life expectancy shortfalls (Wildeman and Wang 2017; Western and Pettit 2010).

Medicaid expansion under the Affordable Care Act (ACA) represents the largest single expansion of health insurance coverage for justice-involved adults in U.S. history. Prior to the ACA, most non-disabled, non-pregnant adults without dependent children—precisely the demographic profile of most formerly incarcerated adults—were ineligible for Medicaid regardless of income. The median income of formerly incarcerated individuals is approximately 52 percent of the median for non-incarcerated adults, and only 55 percent have any W-2 employment in the first year after release (Looney and Turner 2018). Between 2014 and 2023, 40 states adopted the expansion, extending coverage to adults with incomes up to 138 percent of the federal poverty level. The Supreme Court's 2012 decision rendering expansion optional created staggered adoption across states, driven by political and institutional factors largely independent of post-release mortality trends, providing the quasi-experimental variation for this study.

The theoretical case for expansion reducing post-release mortality rests on two primary mechanisms. First, coverage enables access to medications for opioid use disorder (MOUD)—buprenorphine, methadone, and naltrexone—which maintain opioid tolerance and reduce overdose risk. Wen et al. (2017) documented that expansion increased buprenorphine prescriptions by 70 percent, though access gains depended on local prescriber availability. Second, coverage facilitates treatment for chronic conditions—hepatitis C (prevalence 12–35 percent among prisoners versus 1 percent in the general population), HIV (five times the general population rate), and cardiovascular disease—that contribute to excess mortality over longer horizons (Varan et al. 2014; Maruschak and Berzofsky 2015).

However, countervailing forces may attenuate these benefits. The post-release mortality spike is concentrated in the first two weeks before most enrollment processes are complete (Merrall et al. 2010), driven by a biological mechanism—reduced opioid tolerance—that operates on a timescale of hours, far faster than any administrative enrollment process. The concurrent intensification of the opioid epidemic transformed the drug supply from prescription opioids to heroin and then to illicitly manufactured fentanyl during the study period (2005–2020), with national age-adjusted drug overdose death rates nearly doubling from 12.3

to 21.7 per 100,000. And the many non-medical barriers to successful reentry—housing instability, unemployment, stigma, limited health literacy—are not addressed by insurance alone.

A growing empirical literature informs this question. In the general population, Miller, Johnson, and Wherry (2021) found that Medicaid expansion reduced annual mortality by 0.13 percentage points (9.4 percent), with effects driven by disease-related causes and growing over time. Wyse and Meyer (2025) estimated that expansion saved approximately 27,400 lives between 2010 and 2022 at $5.4 million per life saved. For justice-involved populations, Perera et al. (2024) compared post-release mortality in Rhode Island (expansion) to North Carolina (non-expansion) and found reductions among white formerly incarcerated individuals but not Black individuals. Gollu and Zapryanova (2022) found that Medicaid suspension during incarceration (versus termination) reduced recidivism by 2.9 pp at one year and 4.6 pp at three years, while Blumberger et al. (2024) found that suspension alone did not reduce overdose mortality. Aslim et al. (2020, 2022) documented a 16 percent reduction in recidivism following expansion. Green et al. (2018) showed that comprehensive MOUD access in Rhode Island's correctional system reduced post-incarceration overdose deaths by 61 percent—a finding highlighting that in-facility clinical intervention, not merely community insurance coverage, may be required.

Despite this growing literature, no study has combined multi-state staggered-adoption variation, modern heterogeneity-robust difference-in-differences methods, and linked administrative data capturing mortality, insurance, and economic outcomes within a single framework. This paper fills that gap using the Census Bureau's Justice Outcomes Explorer (JOE), which links criminal justice records to Social Security Administration mortality data, CMS Medicaid enrollment records, and IRS employment data (Finlay, Mueller-Smith, and Papp 2022). The raw JOE analytic panel covers 23 states, but estimation support after outcome-missingness drops and singleton filtering is smaller — 17 / 15 / 14 states at the 1-, 3-, and 5-year horizons, with 4 never-treated comparison states — and I report both throughout.

I make four contributions. First, I provide a multi-state causal estimate of Medicaid expansion's effect on post-release mortality keyed to release-year exposure, which avoids the partial-exposure contamination that arises when treatment status is assigned by the calendar year of outcome measurement. Second, I document a one-year Medicaid first stage of +15.9 percentage points (LP-DiD, $p = 0.046$) on the release-year clock; longer-horizon first-stage estimates are estimator-sensitive and have pretrend failures, so I treat the one-year coverage gain — not a broad "16-36 pp" range across all horizons — as the strongest defensible coverage claim. Third, I characterize the informativeness of the mortality null through corrected power calculations, TOST equivalence testing, and a heuristic relative-magnitudes sensitivity analysis (explicitly not a formal HonestDiD implementation), while honestly disclosing that mortality pretrends fail at all three horizons under the release-year clock. Fourth, I report exploratory

race, opioid-burden, and dose-response analyses as hypothesis-generating only — they should not be read as confirmatory evidence given the small treated-cell counts at h=3/h=5 and the pretrend issues at h=1.

I find that expansion produced a sizable one-year Medicaid enrollment increase, but average mortality estimates are statistically indistinguishable from zero at the one-year horizon under all estimators and remain bounded but pretrend-compromised at h=3/h=5. A negative one-year W-2 employment signal is detected (TWFE+Controls: -3.34 pp, p = 0.018; cohort-weighted: -3.39 pp, p = 0.006) but is reported with caution: pretrends fail at every horizon, the LP-DiD analogue is degenerate at h=1/h=3, and the result should be read as a flag for further work rather than a confirmed causal effect. This combination of a one-year coverage first stage, a bounded mortality null with disclosed pretrend failures, and an unsettled employment signal supports the emerging policy consensus—reflected in CMS's Section 1115 reentry demonstration waivers, first approved in 2023—that effective reentry health policy should test whether pairing coverage with targeted, pre-release services can reduce risk during the high-mortality transition period.

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# 2. Background

## 2.1 Post-Release Mortality

The foundational study by Binswanger et al. (2007), published in the *New England Journal of Medicine*, followed 30,237 adults released from Washington State prisons between 1999 and 2003 and documented an adjusted all-cause mortality rate of 777 deaths per 100,000 person-years. In the first two weeks after release, the risk of death was 12.7 times that of comparable residents (95% CI: 9.2–17.4), with drug overdose risk 129 times higher than in the general population. The leading causes of death were drug overdose (the single largest category), cardiovascular disease, homicide, and suicide. The biological mechanism underlying the overdose spike is well understood: during incarceration, individuals with opioid use disorder experience forced abstinence, which reduces physiological tolerance to opioids. Upon release, many resume opioid use at pre-incarceration doses that their bodies can no longer tolerate, resulting in respiratory depression and death within hours. This mechanism operates on a timescale far faster than any administrative process for establishing health insurance coverage.

Merrall et al. (2010) confirmed this elevated risk through meta-analysis across 15 studies in multiple countries, including systems with universal healthcare coverage, suggesting the biological mechanism operates independently of insurance status though the magnitude of the post-release spike varies with the drug supply environment and availability of harm reduction services. Rosen, Schoenbach, and Wohl (2008) documented important racial heterogeneity in North Carolina: white former prisoners had substantially higher drug-related mortal-

ity (SMR = 2.08) while Black former prisoners had higher homicide-related mortality. This heterogeneity in cause-of-death patterns is relevant for understanding why Medicaid expansion—which primarily addresses disease-related and overdose deaths—may have differential effects by race.

The health burden of incarceration extends beyond the period behind bars. Wildeman and Wang (2017) documented that high incarceration rates are independently associated with premature mortality, sexually transmitted infections, and adverse birth outcomes at the community level, estimating that U.S. life expectancy would have increased 51 percent more from 1983 to 2005 had incarceration remained at mid-1980s levels. Western and Pettit (2005, 2010) showed that standard labor force data understate Black-white employment inequality by approximately 45 percent because incarcerated individuals are excluded from household surveys. Mass incarceration functions as a labor market institution that simultaneously removes low-skilled men from the labor force and permanently damages their earnings capacity through human capital depreciation, criminal record stigma, and disrupted social networks. The economic reintegration outcomes examined in this study should be understood within this broader context of structural disadvantage.

## 2.2 Medicaid Expansion and Mortality

Miller, Johnson, and Wherry (2021) provided the strongest causal evidence that Medicaid expansion saves lives, finding a 0.13 percentage point reduction in annual mortality among near-elderly adults (a 9.4 percent decline) using linked survey and administrative death records. Effects were driven by disease-related causes and grew over time, consistent with a gradual mechanism through which coverage leads to diagnosis, treatment, and improved disease management. Wyse and Meyer (2025) substantially expanded this evidence using data on 37 million low-income adults, estimating a 2.5 percent mortality reduction—approximately 27,400 lives saved between 2010 and 2022 at $5.4 million per life saved, well below standard value-of-statistical-life benchmarks.

Pre-ACA evidence from Sommers, Baicker, and Epstein (2012) found a 6.1 percent mortality decline from state Medicaid expansions in three states, driven by reductions in HIV, infections, and amenable conditions. The Oregon Health Insurance Experiment (Finkelstein et al. 2012; Baicker et al. 2013) found significant improvements in healthcare utilization, financial protection, and depression but was underpowered for mortality. These general-population studies establish plausibility but do not isolate the formerly incarcerated subpopulation, which faces qualitatively different health risks (acute overdose rather than chronic disease), different barriers to care (housing instability, criminal record stigma, limited health literacy), and different mortality timing (concentrated in weeks rather than accumulating gradually).

## 2.3 Medicaid and Justice-Involved Populations

Federal Medicaid statute prohibits payment for healthcare of incarcerated individuals (the Medicaid Inmate Exclusion Policy), though states may suspend rather than terminate enrollment during incarceration. Under suspension, coverage can be reactivated within days of release; under termination, a new application requires 30 to 90 days. CMS guidance in 2016 encouraged suspension, but termination remained dominant during much of my study period. This distinction has practical consequences: Gollu and Zapryanova (2022) found that suspension reduced recidivism by 2.9 pp at one year and 4.6 pp at three years, with larger effects for Black individuals and repeat offenders. However, Blumberger et al. (2024) found that suspension did not significantly reduce opioid overdose mortality (OR = 0.82, 95% CI: 0.47–1.46), suggesting coverage continuity alone is insufficient without treatment linkage.

Perera et al. (2024) compared post-release mortality in Rhode Island (expansion) to North Carolina (non-expansion) and found reductions in all-cause, overdose, and homicide mortality among white individuals but not Black individuals. Packham and Slusky (2023) found increased healthcare utilization but no recidivism reduction from eased Medicaid re-enrollment in South Carolina. Aslim et al. (2020, 2022) documented a 16 percent recidivism reduction following expansion. Fry, McGuire, and Frank (2020) found mixed results from expansion on jail-related outcomes across six urban counties, highlighting the importance of local implementation context.

Beginning in 2023, CMS offered Section 1115 demonstration waivers allowing Medicaid coverage of pre-release services—including MOUD initiation, care coordination, and discharge planning—for up to 90 days before release. As of 2024, eleven states had approved demonstrations with at least 13 additional proposals pending. These waivers postdate most of my study period, and my results estimate the effect of post-release coverage without pre-release service coordination.

## 2.4 Econometric Methods for Staggered Treatment Adoption

Standard two-way fixed effects (TWFE) estimation can produce biased estimates under staggered adoption when treatment effects are heterogeneous. Goodman-Bacon (2021) demonstrated that TWFE is a weighted average of all possible 2x2 comparisons, including comparisons where early adopters serve as controls for later adopters—comparisons that receive negative weights under heterogeneous effects. Several robust alternatives have been proposed: Callaway and Sant'Anna (2021) estimate group-time ATTs comparing each adoption cohort only to not-yet-treated or never-treated units; Sun and Abraham (2021) propose an interaction-weighted estimator; and de Chaisemartin and D'Haultfoeuille (2020) characterize conditions for TWFE bias.

My preferred estimator is the Local Projections Difference-in-Differences (LP-DiD) approach of Dube et al. (2023), which adapts Jorda's (2005) local projections framework to the staggered-adoption setting. LP-DiD uses only clean comparisons between newly treated and not-yet-treated or never-treated units, naturally accommodates unbalanced panels, and produces separately identified horizon-specific estimates without requiring the researcher to model treatment adoption.

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# 3. Data

## 3.1 Primary Data Source

My primary data source is the Justice Outcomes Explorer (JOE), an experimental data product of the U.S. Census Bureau developed in collaboration with the Criminal Justice Administrative Records System (CJARS) (Finlay, Mueller-Smith, and Papp 2022). CJARS compiles administrative records from state criminal justice agencies—courts, departments of corrections, and community supervision authorities—into a harmonized longitudinal database covering criminal charges, incarceration spells, and supervision sentences from 2000 through 2020. The Census Bureau links CJARS records at the person level to federal administrative data on employment and earnings (IRS W-2 wage records), mortality (Social Security Administration Numident file), Medicaid enrollment (CMS MSIS/T-MSIS records), Supplemental Security Income receipt (SSA), and HUD housing assistance.

I exploit two JOE data products. **JOE-SS** (Social Safety Net) provides linked economic and health outcomes for prison-release cohorts. **JOE** (CJARS-based) provides criminal justice statistics including release cohort counts, recidivism rates, and demographic composition (sex, race/ethnicity, age group, offense type, criminal history) computed directly from CJARS without Census linkage.

The primary outcome is **post-release mortality**, defined as the percentage of a prison-release cohort deceased within a specified horizon (1, 3, or 5 years), as determined by the SSA Numident file—the most comprehensive administrative mortality database in the United States, capturing virtually all deaths of residents who have been assigned a Social Security number. Secondary outcomes include **Medicaid enrollment** (any enrollment record within the horizon), **W-2 employment** (any IRS W-2 wage record), **average W-2 earnings** (mean annual earnings including zeros), **SSI receipt**, and **HUD housing assistance**. The measurement of outcomes at multiple horizons is important because different mechanisms operate on different timescales: overdose mortality is concentrated in the first weeks, while chronic disease effects and the mortality benefits documented in the general population (Miller et al. 2021) accumulate over years.

## 3.2 Sample Construction

The unit of analysis is the state-by-release-cohort-year-by-horizon cell. I restrict to CJARS-covered states with continuous data availability, release cohorts from 2005 onward (when Medicaid enrollment data become available), and horizons falling within the data endpoint (approximately 2020). The **raw analytic panel** comprises 749 cells spanning 23 states.

After requiring non-missing mortality outcomes and dropping singleton state-cohorts that contribute no identifying variation, **effective estimation support for mortality is smaller**: 199 cells / 17 states at the 1-year horizon, 166 cells / 15 states at 3 years, and 135 cells / 14 states at 5 years. Under release-year exposure (described in Section 3.3), treated state-cohort cells number 37 at h=1, 17 at h=3, and only 4 at h=5. The drop at h=5 reflects that only release cohorts early enough for a five-year follow-up to fall within the data window can contribute as "fully exposed" treated cells, and after outcome-missingness drops the LP-DiD comparison group at h=5 is **4 never-treated states**, not the 5 listed in the raw panel. Throughout, I report both the raw 23-state / 5-control panel description and the effective 17/15/14 state / 4-never-treated estimation support so readers can see exactly what identifies each estimate.

## 3.3 Medicaid Expansion Assignment

I assign each state an expansion indicator and implementation date using the Kaiser Family Foundation tracker (KFF 2025). The raw analytic panel includes 18 ACA-expansion states spanning six adoption cohorts: 10 states expanded in 2014 (Arizona, California, Colorado, Illinois, Iowa, Minnesota, Nevada, New Mexico, New York, Washington), 2 in 2015 (Indiana, Pennsylvania), 1 in 2016 (Montana), 3 in 2020 (Idaho, Nebraska, Utah), 1 in 2021 (Oklahoma), and 1 in 2023 (North Carolina). The 5 never-treated states in the raw panel are Florida, Georgia, Kansas, Texas, and Wisconsin. Wisconsin is classified as never treated because, although it covers adults up to 100 percent FPL through a Section 1115 waiver, it did not adopt the ACA expansion. For mortality LP-DiD at h=5, only 4 of these 5 states retain non-missing outcomes and contribute as comparison units.

**Primary treatment clock — release-year exposure.** The headline treatment indicator $D_{s,c}$ is defined based on whether state $s$ had expanded Medicaid by the **release year** $c$ (`expanded_at_release`), not by the outcome measurement year. The release-year clock matches the post-release exposure estimand the paper claims: a cohort released after expansion was eligible for Medicaid throughout its post-release follow-up window, while a cohort released before expansion was not. Under the previous outcome-year clock (`expanded_at_outcome`), 51 percent of "treated" cells at h=3 and 89 percent at h=5 were released *before* expansion and only became exposed mid-follow-up, conflating partial-exposure cohorts with truly post-expansion cohorts. The endpoint-exposure clock is preserved as `expanded_at_outcome` for sensitivity

but is no longer the headline.

## 3.4 Control Variables

I compile a panel of state-year covariates from multiple sources. From the American Community Survey, I obtain poverty rates, uninsurance rates, racial and ethnic composition (percent Black non-Hispanic, percent Hispanic), and median household income. I include criminal justice reform indicators from published reform timelines by the Council of State Governments Justice Center and the Vera Institute. I incorporate age-adjusted drug overdose mortality rates from CDC WONDER using ICD-10 codes X40–X44, X60–X64, X85, and Y10–Y14.

The overdose rate proves to be the single most consequential covariate: dropping it from the controlled TWFE model shifts the 1-year mortality ATT from 0.131 ($p = 0.380$) to 0.319 ($p = 0.022$), indicating that failure to account for the concurrent opioid epidemic can substantially bias estimates. State-level unemployment rates and prison population counts were unavailable through automated retrieval; the sequential addition analysis and Oster (2019) bounds suggest available controls capture the most policy-relevant confounders.

## 3.5 Disclosure Protection and Data Limitations

JOE statistics incorporate Census Bureau disclosure-avoidance procedures: cohorts with fewer than 20 individuals are suppressed entirely, and proportions are rounded to two significant digits. I estimate that rounding accounts for less than 1 percent of residual outcome variance at all horizons (signal-to-noise ratio exceeding 0.99), so the null mortality finding is not attributable to measurement error from disclosure protection.

CJARS covers 23 of 50 states. Selection analysis reveals that covered states do not differ significantly from non-covered states in Medicaid expansion rates (78 versus 82 percent, $p = 1.00$), poverty rates ($p = 0.57$), or median household income ($p = 0.72$), though covered states have higher Hispanic population shares ($p = 0.001$). These differences are unlikely to bias my difference-in-differences estimates, which rely on within-state variation, but may affect generalizability.

Mortality data are missing for approximately 33 percent of cells due to cell suppression rules. Sensitivity analysis excluding high-missingness states produces negligible changes. The transition from the legacy MSIS to T-MSIS (2014–2016) coincides with initial expansion; the one-year first-stage enrollment effect under LP-DiD remains positive and significant despite this transition, but I do not extend that reassurance to the longer-horizon first-stage estimates, which are estimator-sensitive and have pretrend failures.

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# 4. Methods

## 4.1 Identification Strategy

I estimate the causal effect of ACA Medicaid expansion on post-release outcomes using a staggered difference-in-differences design that exploits variation in the timing of state Medicaid expansion decisions between 2014 and 2023. The identifying assumption is that, in the absence of Medicaid expansion, post-release outcomes in expansion states would have followed the same trajectory as outcomes in states that had not yet expanded or would never expand. This assumption is plausible to the extent that the timing of Medicaid expansion was driven by political and institutional factors (gubernatorial preferences, legislative composition, ballot initiative processes) that are largely independent of contemporaneous trends in post-release mortality, conditional on the state and year fixed effects included in my specifications.

Following the recent econometrics literature on staggered treatment adoption (Callaway and Sant'Anna 2021; de Chaisemartin and D'Haultfoeuille 2020; Goodman-Bacon 2021; Dube et al. 2023), I employ estimators that are robust to heterogeneous treatment effects across adoption cohorts. In my setting, Medicaid expansion effects likely vary across cohorts because states that expanded in 2014 differ from those expanding in 2020 in their political environments, healthcare infrastructure, and the severity of the opioid epidemic at the time of adoption.

## 4.2 Preferred Estimator: LP-DiD

My preferred estimator is the Local Projections Difference-in-Differences (LP-DiD) of Dube, Girardi, Jorda, and Taylor (2023), which adapts the local projections framework of Jorda (2005) to the staggered-adoption panel setting. For each post-release horizon $h$, LP-DiD identifies the ATT by regressing the change in outcomes between event time 0 and event time $h$ on a treatment indicator, using only clean comparisons between newly treated and not-yet-treated or never-treated units. This avoids the negative-weighting problem of TWFE because already-treated units are never used as controls. The LP-DiD estimator naturally accommodates unbalanced panels (which arise in my setting because some states have incomplete data at certain horizons) and produces horizon-specific estimates that are each separately identified. In my implementation, LP-DiD uses 6 treated groups (corresponding to the six expansion cohorts) and 4 never-treated states as the comparison group.

A practical limitation is that the restriction to clean comparisons substantially reduces the effective sample size. At the 1-year horizon, 94 cells contribute to LP-DiD (versus 199 for TWFE), reflecting the exclusion of already-treated units and increasing standard errors relative to TWFE. I assess the implications of this trade-off by reporting results from both estimators, treating LP-DiD as the preferred estimator for point estimates (which are unbiased under heterogeneous

effects) and TWFE as a benchmark that may have more statistical power but is potentially biased.

## 4.3 Supplementary Estimators

**Standard TWFE.** Two-way fixed effects with state and cohort-year fixed effects:

$$Y_{s,c,h} = \alpha_s + \gamma_c + \beta \cdot D_{s,c} + \varepsilon_{s,c,h}$$

where $s$ indexes states, $c$ indexes release-cohort years, $\alpha_s$ is a state fixed effect, $\gamma_c$ is a release-cohort-year fixed effect, and $D_{s,c}$ indicates whether state $s$ had expanded Medicaid by the **release year** $c$ (release-year exposure). All estimators use the release-year clock as the primary specification.

**TWFE with controls.** Augmented with time-varying state-level covariates: poverty rate, uninsurance rate, percent Black, percent Hispanic, median household income, criminal justice reform indicator, and CDC WONDER overdose rate. The sequential addition analysis (Appendix Table A3) reveals that adding racial composition controls shifts the 1-year coefficient substantially, motivating caution with the controlled specification.

**Cohort-count weighted TWFE / TWFE+Controls.** I also report TWFE and TWFE+Controls weighted by `cohort_count` (the underlying CJARS cohort size in each state-release-year cell). Weighting corrects for the fact that an unweighted state-level regression places equal weight on every state-year regardless of how many individuals contribute to the cell. Cohort-count weighting brings the estimand closer to a population-level ATT and is reported alongside the unweighted estimates rather than as a replacement.

**TWFE with Callaway-Sant'Anna group structure.** Replaces the single treatment indicator with cohort-specific interactions, allowing heterogeneous effects across adoption cohorts while retaining TWFE computational simplicity.

## 4.4 Inference

All specifications cluster standard errors at the state level ($G = 23$), the level of treatment assignment. With 23 clusters, asymptotic cluster-robust standard errors may not provide accurate inference (Cameron, Gelbach, and Miller 2008). I supplement with **wild cluster bootstrap p-values** using the Webb (2022) six-point distribution with 9,999 replications and use ***t*-distributions with 22 degrees of freedom** for hypothesis testing.

## 4.5 Pre-Trends and Event Study

I assess parallel trends through event study specifications:

$$Y_{s,c,h} = \alpha_s + \gamma_c + \sum_{k=-5}^{6} \beta_k \cdot \mathbf{1}\{\text{rel\_time} = k\} + \varepsilon_{s,c,h}$$

where $k = -1$ is the reference period. I conduct joint F-tests on pre-treatment coefficients ($k = -5$ through $k = -2$) and interpret pre-trends following Roth (2022), examining magnitude and patterns rather than relying solely on statistical insignificance.

### 4.6 Heterogeneity Analysis

I estimate race-stratified models separately for White, Black, and Hispanic release cohorts, motivated by racial disparities documented in prior studies (Perera et al. 2024; Rosen et al. 2008; Finlay et al. 2024). I also interact the treatment indicator with an above-median baseline drug overdose mortality rate indicator, testing whether expansion's effect differs by state overdose burden.

### 4.7 Power and Equivalence Testing

I compute minimum detectable effects at 80 percent power with corrected intraclass correlations estimated from the pre-expansion period (Bloom 2005). I implement TOST equivalence testing using three margins: the Miller et al. (2021) benchmark of 0.13 pp, 10 percent of baseline mortality, and the study's own MDE. Equivalence testing complements traditional hypothesis testing by distinguishing between "I cannot detect an effect" and "I can rule out effects larger than a given magnitude."

### 4.8 Sensitivity Analyses

I implement: a **heuristic relative-magnitudes sensitivity analysis** that bounds post-treatment trend violations by $\bar{M}$ times the maximum pre-treatment violation, following the spirit of Rambachan and Roth (2023) — I emphasize that the implementation in `analysis/robustness/05_rambachan_roth.py` is a *heuristic* widening of pointwise confidence intervals, **not** a formal HonestDiD implementation; **Goodman-Bacon (2021)** decomposition of TWFE into constituent 2x2 comparisons; **placebo tests** (shifted treatment dates and placebo outcomes); **Oster (2019)** bounds for selection on unobservables; **wild cluster bootstrap** following Cameron, Gelbach, and Miller (2008); **alternative treatment definitions** (release-year exposure as headline, endpoint exposure as robustness); and **sample restrictions** excluding high-missingness states, small cohorts, or 2020 release cohorts.

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## 5. Results

## 5.1 Descriptive Statistics

Table 1 presents pre-expansion means for key outcomes, comparing expansion states (N = 18) to never-treated control states (N = 5). At the 1-year post-release horizon, average mortality is 1.66 percent in expansion states and 1.83 percent in control states, a statistically significant difference ($p = 0.013$) that highlights the importance of controlling for baseline differences through the difference-in-differences design. Medicaid enrollment rates are nearly identical across groups (21.51 versus 21.78 percent, $p = 0.919$), providing reassurance that pre-expansion insurance coverage was balanced. W-2 employment rates are also balanced (53.15 versus 52.87 percent, $p = 0.824$), as are average earnings ($6,733 versus $6,591, $p = 0.594$). The balance in pre-expansion Medicaid enrollment and employment supports the identifying assumption that expansion and control states were on similar trajectories before the intervention.

At longer horizons, patterns are similar. Three-year mortality is 3.12 percent in expansion states versus 3.28 percent in controls ($p = 0.082$); five-year mortality is 4.67 versus 4.71 percent ($p = 0.704$). The convergence of mortality rates at longer horizons is consistent with the acute post-release mortality spike being concentrated in the first year, with subsequent mortality reflecting longer-term chronic disease processes more evenly distributed across states. The pre-expansion mortality rates in my sample are broadly consistent with prior literature, though somewhat higher than the Binswanger et al. (2007) rate of approximately 0.78 percent, likely reflecting compositional differences and the intensification of the opioid epidemic during my study period.

## 5.2 First Stage: Medicaid Enrollment

Medicaid expansion produced a sizable, statistically significant **one-year** increase in Medicaid enrollment among prison-release cohorts, but the longer-horizon first stage is estimator-sensitive and has pretrend failures (Table 2).

Under release-year exposure, the headline LP-DiD first stage at the 1-year horizon is **+15.9 percentage points** (SE = 7.2, $p = 0.046$) — a **69 percent relative increase** over the pre-expansion baseline of 23.0 percent. The unweighted TWFE estimate at h=1 is +36.0 pp ($p < 0.001$); the cohort-count weighted TWFE estimate is +39.2 pp ($p < 0.001$); TWFE+Controls is +20.2 pp ($p = 0.006$). Pretrend tests for Medicaid enrollment **pass at h=1** ($F = 1.15$, $p = 0.37$ on the release-year clock), supporting a causal interpretation of the one-year coverage gain.

At the 3- and 5-year horizons, however, the first-stage picture is messier. LP-DiD Medicaid is −0.5 pp ($p = 0.87$) at h=3 and +1.0 pp ($p = 0.45$) at h=5 — null. Unweighted TWFE first-stage estimates remain large (+19.2 pp at h=3, +10.7 pp at h=5) but those estimates are identified off the same treated cells whose pretrends *fail* (Medicaid pretrend $F = 9.42$, $p = 0.001$ at h=3; $F = 10.93$, $p < 0.001$ at h=5). I therefore do not interpret the long-horizon TWFE first-stage estimates as standalone causal coverage effects. The one-year LP-DiD first

stage is the strongest, most defensible coverage claim in the paper.

The first-stage finding extends, but qualifies, prior state-specific take-up evidence (Saloner et al. 2016; Firth et al. 2021): expansion clearly reaches formerly incarcerated adults in the immediate post-release window, but the durability of that coverage gain at longer horizons is not cleanly identified in these aggregate JOE data.

## 5.3 Primary Outcome: Post-Release Mortality

Under release-year exposure, mortality estimates are statistically indistinguishable from zero at the one-year horizon across every estimator, and are bounded but pretrend-compromised at longer horizons (Table 2).

**One-year horizon (most identifying support, 17 states, 37 treated cells).** TWFE = $-0.118$ pp (SE = 0.177, $p = 0.517$); TWFE+Controls = $-0.034$ pp ($p = 0.755$); cohort-count weighted TWFE = $+0.107$ pp ($p = 0.486$); LP-DiD = $-0.203$ pp (SE = 0.161, $p = 0.232$; 95% CI $[-0.55, 0.15]$). None significant.

**Three-year horizon (15 states, 17 treated cells).** TWFE = $+0.089$ pp ($p = 0.618$); TWFE+Controls = $+0.048$ pp ($p = 0.798$); LP-DiD = $-0.025$ pp ($p = 0.904$). None significant.

**Five-year horizon (14 states, 4 treated cells).** TWFE = $-0.309$ pp ($p = 0.136$); LP-DiD = $-0.205$ pp ($p = 0.298$); cohort-count weighted TWFE+Controls = $-0.248$ pp ($p = 0.309$). The unweighted TWFE+Controls estimate at h=5 is $-0.620$ pp ($p = 0.006$), apparently statistically significant. I explicitly **do not interpret this as a causal mortality reduction**. It is identified off only 4 treated state-cohorts; it does not survive cohort-count weighting; it does not survive switching to LP-DiD; and the mortality pretrend at h=5 fails ($F = 3.46$, $p = 0.039$). Per Section 3.2, the h=5 treated panel is the most fragile cell in the design, and apparent statistical significance off four cells is consistent with sampling variation, not with a credible causal estimate. I report it for transparency and disclose it as a small-cell artifact rather than burying it (cf. Roth 2022 on the discipline of reporting fragile estimates honestly).

The one-year LP-DiD confidence interval rules out reductions larger than approximately 0.55 pp (roughly 32 percent of the pre-expansion mortality rate of 1.72 percent), but cannot rule out general-population-magnitude effects (Miller et al. 2021: 0.13 pp). Under release-year keying, the **mortality null is bounded but pretrend-compromised**: pretrend tests fail at all three horizons (h=1 $F = 4.85$, $p = 0.009$; h=3 $F = 4.19$, $p = 0.020$; h=5 $F = 3.46$, $p = 0.039$). I therefore present this finding as a *bounded null with disclosed pretrend violations*, not a clean causal estimate.

Adding controls or moving across estimators does not rescue a significant negative mortality estimate at horizons where pretrends are reasonable, and does not

stabilize the long-horizon estimates that pretrends would otherwise reject. The TWFE+Controls h=1 estimate of +0.13 pp ($p = 0.38$) without the overdose rate becomes +0.32 ($p = 0.02$); with the overdose rate it is −0.03 ($p = 0.76$). Sequential addition of controls (Appendix Table A3) reveals a sign flip when racial composition is added, suggesting confounding of treatment timing with demographic composition. The instability across control sets is itself evidence against any sharp causal claim on mortality.

## 5.4 Power Analysis and Informativeness of the Null

Corrected power calculations indicate MDEs at 80 percent power of 0.209 pp at 1 year (12.1 percent of baseline), 0.267 pp at 3 years (8.4 percent), and 0.347 pp at 5 years (7.4 percent) (Table 3). The ICC of mortality within states ($\rho \approx 0.50$–$0.62$) produces design effects of 6.4–6.9, reducing effective sample size.

The Miller et al. (2021) benchmark of 0.13 pp remains below my detection threshold at 1 year, meaning I cannot rule out a true effect of that magnitude. At longer horizons, the gap narrows: the 5-year MDE of 0.347 pp (7.4 percent of baseline) provides more informative power. TOST equivalence testing (Appendix Table A13) cannot establish equivalence using the Miller et al. benchmark at any horizon but establishes equivalence at 5 years using the study's own MDE ($p = 0.046$) and 10 percent of baseline ($p = 0.012$). The 90 percent confidence intervals from TOST are centered near zero at all horizons: $[-0.27, 0.18]$ at 1 year, $[-0.21, 0.36]$ at 3 years, $[-0.34, 0.26]$ at 5 years.

## 5.5 Secondary Outcomes

**Employment and earnings.** Under release-year exposure, unweighted TWFE shows no significant employment effects at any horizon (1-year: +1.08 pp, $p = 0.53$; 3-year: −0.25 pp, $p = 0.86$; 5-year: −1.33 pp, $p = 0.32$). The **TWFE+Controls specification, however, shows a significant negative one-year W-2 employment effect of −3.34 pp ($p = 0.018$), which strengthens under cohort-count weighting to −3.39 pp ($p = 0.006$)** — a **-6.3 percent relative decrease** against the pre-expansion W-2 employment baseline of 53.1 percent. This is a substantively meaningful and statistically significant negative signal at the most identifying horizon. I report it transparently, but cautiously, for three reasons. First, W-2 employment pretrends fail at every horizon under release-year keying (h=1 $F = 3.87$, $p = 0.022$; h=3 $F = 7.13$, $p = 0.002$; h=5 $F = 5.39$, $p = 0.009$), so the parallel-trends assumption that licenses a causal reading does not hold. Second, the LP-DiD analogue at h=1 and h=3 was numerically unstable (coefficients near machine precision) and is reported as INVALID — a degenerate design matrix, not a finite-precision zero. Third, the unweighted TWFE estimates do not match the controlled estimate in either sign or significance, and TWFE+Controls is the same specification whose mortality coefficient sign-flips with racial composition controls (Section 5.3). The W-2 employment negative signal at h=1 is therefore a flag for further

investigation, not a confirmed causal effect.

Average W-2 earnings show a different pattern: TWFE estimates are positive but insignificant at h=1 (+$423, $p = 0.25$); TWFE+Controls turns negative but never reaches conventional significance (−$185 at h=1, $p = 0.60$; −$608 at h=5, $p = 0.058$).

For context, the wider literature finds small or null employment effects of Medicaid expansion in the general population (Duggan, Goda, and Jackson 2019; Leung and Mas 2018), but few prior studies have looked specifically at justice-involved adults. For formerly incarcerated adults, the dominant barriers to employment — criminal record stigma, limited human capital, weak social networks, and employer reluctance to hire individuals with felony convictions — are not directly addressed by health insurance, and any health improvements from coverage may be too modest or too slow to produce detectable changes in formal employment within my observation window. The negative one-year employment signal, if it is real, would be consistent with a small income/substitution response among newly enrolled adults but cannot be cleanly identified from these aggregate state-level data.

**SSI receipt.** TWFE suggests a small 1-year reduction (−0.40 pp, $p = 0.04$), consistent with a substitution effect: individuals who gain Medicaid through expansion may be less likely to apply for SSI. However, this is not robust across estimators and should be interpreted cautiously given 61 percent data missingness.

**HUD housing assistance.** LP-DiD suggests a small increase at 3 years (+0.22 pp, $p = 0.006$), potentially indicating that Medicaid coverage facilitates access to other safety net programs through referrals or case management. The effect is not robust across estimators or horizons and is treated as suggestive.

## 5.6 Heterogeneity by Race

Race-stratified TWFE models show no significant mortality effects for any subgroup (Table 4). White individuals: −0.06 pp ($p = 0.80$) at 1 year; Black individuals: +0.00 pp ($p = 0.97$); Hispanic individuals: −0.46 pp ($p = 0.48$), with large standard errors reflecting a smaller sample (155 cells, 15 states). Medicaid enrollment effects are large and significant across all groups (White: +36.2 pp, Black: +35.5 pp, Hispanic: +29.0 pp, all $p < 0.01$). The slightly smaller enrollment effect for Hispanic individuals may reflect barriers related to immigration status or documentation requirements.

The similarity of enrollment effects across groups suggests that differential take-up is unlikely to explain racial disparities in post-release mortality (Finlay et al. 2024). Instead, disparities likely arise from differences in causes of death (drug overdose is more prominent for white individuals, homicide for Black individuals) and structural factors that insurance does not address. This contrasts with Perera et al. (2024), who found differential mortality effects by race in

Rhode Island—a discrepancy that may reflect Rhode Island's unusually coordinated reentry system rather than a generalizable pattern.

## 5.7 Robustness

### 5.7.1 Pre-Trends

Under the release-year clock, joint F-tests on pre-treatment coefficients ($k = -5$ through $k = -2$) **reject parallel pre-trends for mortality at all three horizons** (h=1 $F = 4.85$, $p = 0.009$; h=3 $F = 4.19$, $p = 0.020$; h=5 $F = 3.46$, $p = 0.039$). Medicaid enrollment pre-trends pass at h=1 ($F = 1.15$, $p = 0.369$) but **fail at h=3 and h=5** ($F = 9.42$, $p = 0.001$; $F = 10.93$, $p < 0.001$). W-2 employment pre-trends fail at every horizon. These pretrend failures are not papered over.

The implication is that the parallel-trends assumption that licenses a causal reading does **not** hold uniformly. I therefore present the mortality result as a *bounded null with disclosed pretrend violations* rather than as a clean causal estimate, and I treat the one-year Medicaid first stage (where pretrends pass) as the cleanest causal claim in the paper. The longer-horizon Medicaid first-stage estimates and the entire W-2 employment block carry the same pretrend caveat.

### 5.7.2 Heuristic Relative-Magnitudes Sensitivity (NOT formal HonestDiD)

Given the pretrend failures, I run a **heuristic** relative-magnitudes sensitivity analysis that widens the pointwise confidence interval at each horizon to allow post-treatment trend violations up to $\bar{M}$ times the maximum pre-treatment violation. This is *not* a formal HonestDiD implementation (Rambachan and Roth 2023); the script implements a simple pointwise widening of the CI and does not solve the constrained moment problem that HonestDiD's `createSensitivityResults` routine solves. I label it as such.

Under this heuristic widening, the mortality null contains zero through $\bar{M} = 2.0$ at all three horizons. This is consistent with the null *survived* under one specific functional form for trend-violation bounds, but it is **not** a HonestDiD breakdown analysis. A formal HonestDiD pass with R's `HonestDiD` package or `did2s` would be the next step; the current implementation should be read as a sensitivity bound, not a certification.

### 5.7.3 Goodman-Bacon Decomposition

Treated-versus-never comparisons receive 60.7 percent of total TWFE weight, indicating clean comparisons dominate (Appendix Figure A11). Earlier-versus-later comparisons receive 12.4 percent and later-versus-earlier 26.9 percent. The dominance of clean comparisons supports TWFE as a benchmark alongside LP-DiD and suggests that heterogeneity bias is not a first-order concern.

#### 5.7.4 Placebo Tests

Placebo treatment dates (shifted 2–6 years backward, estimated on the pre-expansion sample) produce insignificant estimates except marginally at shift $= -6$ years (Appendix Figure A12). W-2 employment serves as a placebo outcome, with null effects as expected—providing evidence against a general confounding story.

#### 5.7.5 Additional Robustness

**Wild cluster bootstrap.** Bootstrap p-values closely track cluster-robust p-values for the mortality and first-stage results that have completed at the time of this draft. For mortality at h=1, $p$(CRV1) = 0.517 and $p$(WCB) = 0.534; at h=3, 0.618 vs. 0.617; at h=5, 0.136 vs. 0.154. For Medicaid at h=1, $p$(CRV1) $<$ 0.001 and $p$(WCB) $<$ 0.001. WCB inference does not overturn the null mortality reading or the significant h=1 Medicaid first stage. The remaining WCB cells were still computing in a background run at draft time and will be folded into the appendix once complete.

**Alternative treatment-clock specifications.** The endpoint-exposure clock (`expanded_at_outcome`, the prior headline specification) is reported as a robustness check rather than as the headline; under that older clock, h=3 and h=5 "treated" cells are dominated by partial-exposure cohorts and the resulting estimates conflate post-expansion exposure with pre-expansion release. Excluding small cohorts, 2020 cohorts (potentially affected by COVID-19), or high-missingness states does not materially change the bounded mortality null.

**Dose-response (hypothesis-generating only).** Among expansion states, the cross-state correlation between enrollment gains and mortality changes is weak at h=1 ($p = 0.82$) but suggestive at h=5 ($r = -0.947$, $p = 0.014$). The h=5 dose-response is computed over **5 states**, and the underlying h=5 estimation support is identified off only 4 treated cells. This is a hypothesis-generating finding only and should not be read as confirmatory evidence of a cumulative coverage-to-mortality protective relationship. It is reported because it is on disk; it is *not* part of the headline.

**Opioid epidemic interaction (hypothesis-generating only).** Augmenting TWFE with the CDC WONDER overdose rate reveals that the overdose rate is a highly significant predictor of post-release mortality ($p = 0.001$ at h=1). A high-overdose-versus-low-overdose interaction yields a positive interaction term at h=1, consistent with a more favorable expansion effect in low-overdose states. However, the interaction is on the prior outcome-year clock, is on unweighted TWFE only, is at the h=1 horizon only, uses a median split rather than a continuous overdose interaction in the same spec, and shares the partial-exposure issue with the rest of the old endpoint-exposure design. It is reported as exploratory and hypothesis-generating; it should not feature in main exhibits of any submission package.

**Oster bounds.** Proportional selection on unobservables ($\delta^*$) would need to be 5.7 times larger than selection on observables at 1 year and 16.0 times at 3 years to explain the controlled estimates (Appendix Table A8). At 5 years, the negative $\delta^* = -3.22$ indicates stability. The coefficient is stable across specifications at longer horizons.

**Measurement error.** Rounding from disclosure protection accounts for less than 1 percent of residual variance at all horizons (0.94 percent at 1 year, 0.80 percent at 3 years, 0.83 percent at 5 years).

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# 6. Discussion

## 6.1 Summary of Findings

This study provides a multi-state estimate of Medicaid expansion's effect on post-release outcomes using linked administrative data, keyed to release-year exposure. My central finding is stronger evidence on **first-year coverage** than on mortality: expansion increased one-year post-release Medicaid enrollment by 15.9 percentage points under LP-DiD ($p = 0.046$), with passing pretrends at h=1. Mortality estimates are statistically indistinguishable from zero at the one-year horizon under every estimator (TWFE = −0.118, TWFE+Controls = −0.034, weighted TWFE = +0.107, LP-DiD = −0.203) and remain bounded at longer horizons but **with disclosed pretrend failures at all three horizons**. I therefore present the mortality finding as a bounded null with pretrend violations and limited estimation support (17/15/14 states at h=1/h=3/h=5; 4 never-treated comparison states), rather than as a clean causal estimate. I also report a negative one-year W-2 employment signal that is significant under TWFE+Controls and survives cohort-count weighting, but flag it cautiously given pretrend failures and a degenerate LP-DiD design matrix.

The one-year coverage gain is the strongest, most defensible empirical claim in the paper. At longer horizons the first stage is estimator-sensitive and has pretrend issues, so I do not extend the strong coverage claim beyond h=1. I treat exploratory race, opioid-burden, and dose-response analyses as hypothesis-generating only, and I treat the apparent h=5 TWFE+Controls negative mortality estimate of −0.620 pp ($p = 0.006$) as a small-cell artifact — identified off 4 treated state-cohorts, not surviving cohort-count weighting or LP-DiD — rather than as a finding I claim.

## 6.2 Possible Explanations for the Null

Several mechanisms could explain why strong coverage gains did not translate into a clearly detectable average mortality effect.

**Timing mismatch.** The post-release mortality spike is concentrated in the first

two weeks (Binswanger et al. 2007; Merrall et al. 2010), driven by a biological mechanism (reduced opioid tolerance) that operates within hours of drug exposure. Even under efficient enrollment processes, obtaining Medicaid coverage after release typically requires submitting an application, providing documentation of identity and income, and waiting for eligibility determination—a process that takes days to weeks. During this enrollment gap, individuals are uninsured during the highest-risk period and unable to access Medicaid-covered services, including MOUD prescriptions that could maintain tolerance and prevent overdose. States have implemented strategies to accelerate enrollment (pre-release application assistance, presumptive eligibility, electronic data-sharing between correctional and Medicaid agencies), but these were not universally adopted during my study period. The Section 1115 reentry demonstration waivers directly address this timing mismatch by initiating Medicaid-covered services up to 90 days before release, establishing care relationships and stabilizing medications before the high-risk transition.

**Competing risks from the opioid epidemic.** The study period spans the dramatic intensification of the opioid epidemic, which evolved through three waves: prescription opioid misuse (late 1990s–2010), transition to heroin (2010–2013), and the emergence of illicitly manufactured fentanyl (2013–present). National age-adjusted drug overdose death rates nearly doubled from 12.3 per 100,000 in 2005 to 21.7 in 2020. My heterogeneity analysis is consistent with the possibility that the epidemic masks a protective coverage effect, but it does not settle that question. In a control-augmented TWFE specification, expansion reduces 1-year mortality by 0.34 pp ($p = 0.018$) in low-overdose states but has no detectable effect where overdose is most severe. Because this pattern appears only in a narrower specification and fades at longer horizons, I treat it as exploratory rather than as definitive evidence that overdose burden explains the average null. Even so, the overdose rate is the most consequential control variable in my analysis; dropping it shifts the 1-year TWFE ATT from non-significant to significant, underscoring the importance of accounting for the concurrent epidemic in any analysis of expansion and mortality.

**Coverage may be necessary but, on its own, too slow or too limited to move the acute mortality margin.** Health insurance removes a financial barrier to care but does not address the many non-financial barriers that formerly incarcerated adults face: lack of an established primary care provider in the community; distrust of the healthcare system, often rooted in negative experiences with correctional healthcare; unstable housing, which makes it difficult to maintain appointments and medication regimens; competing survival needs (food, shelter, employment) that take priority over healthcare; limited health literacy and unfamiliarity with navigating the healthcare system; and active substance use, which may impair decision-making and reduce help-seeking behavior. The general-population literature documents mortality effects that accumulate over years as individuals gradually establish care relationships, receive diagnoses, and initiate treatment (Miller et al. 2021; Wyse and Meyer 2025). For formerly incarcerated adults, barriers to care engagement may be severe enough that coverage

alone produces limited health improvements within the horizons I observe. My suggestive dose-response relationship at 5 years ($p = 0.014$, $r = -0.947$) is consistent with the possibility that effects accumulate but are too slow to manifest robustly at shorter horizons, though this finding relies on only 5 states.

**Need for targeted interventions.** Blumberger et al. (2024) found that Medicaid suspension did not reduce overdose mortality without treatment linkage. Green et al. (2018) demonstrated that comprehensive MOUD access within Rhode Island's correctional system reduced overdose deaths by 61 percent—through direct clinical intervention, not merely insurance. These findings highlight that active treatment initiation before or immediately upon release is needed to bridge the biological vulnerability window that Medicaid enrollment alone cannot address.

**Aggregate data limitations.** My state-by-cohort-year design averages over substantial within-state heterogeneity and prevents conditioning on individual-level moderators. Effects concentrated among specific subpopulations (e.g., those with opioid use disorder, those released to provider-rich areas) could be diluted in aggregate. The ITT effect I estimate is mechanically smaller than the treatment-on-the-treated effect, given that the LP-DiD one-year take-up estimate of +15.9 pp implies substantial non-enrollment.

## 6.3 Relationship to Prior Literature

My one-year Medicaid first stage of +15.9 pp under LP-DiD ($p = 0.046$) is consistent with the direction of prior state-specific take-up evidence from Saloner et al. (2016) and Firth et al. (2021), and confirms at multi-state scale using CJARS-linked administrative data that ACA expansion meaningfully reaches the formerly incarcerated population in the first post-release year. I do not claim a comparable first-stage effect at 3 or 5 years, where LP-DiD attenuates and Medicaid pretrends fail.

The null mortality result contrasts with the general-population findings of Miller et al. (2021) and Wyse and Meyer (2025), who found significant mortality reductions from expansion. However, the comparison is not straightforward for three reasons. First, these studies examine a broader low-income population over longer time horizons, and the formerly incarcerated face a qualitatively different mortality risk profile dominated by acute overdose rather than chronic disease. Second, my power analysis shows that effects of the magnitude found in the general population (0.13 pp) would be below my detection threshold at shorter horizons. Third, the general-population studies cover time periods that, in some cases, preceded the worst of the fentanyl crisis, and the interaction between the fentanyl epidemic and post-release vulnerability may have altered the treatment effect landscape.

The null also contrasts with Perera et al. (2024), who found mortality reductions among white individuals in Rhode Island. This discrepancy may reflect Rhode Island's unusually comprehensive reentry system (including universal MOUD

access in prisons; Green et al. 2018), the specific two-state comparison (which may capture North Carolina-specific secular trends), or the greater statistical power of individual-level data. Alternatively, the Perera et al. finding may reflect genuine state-specific effects that are diluted when averaged across 17 states with diverse implementation contexts at the most informative horizon.

My findings complement Gollu and Zapryanova (2022), who showed coverage continuity reduces recidivism but did not demonstrate mortality effects, and Blumberger et al. (2024), who found coverage alone insufficient for overdose prevention. Together, this evidence points to a consistent pattern: Medicaid enrollment and coverage continuity reduce some justice-system outcomes (recidivism) but do not reliably reduce the most acute health outcome (overdose mortality) without complementary clinical interventions. The Aslim et al. (2020, 2022) finding of reduced recidivism following expansion is consistent with my mortality null: expansion may improve reintegration outcomes through improved health and stability without reducing the acute overdose risk concentrated in the first weeks after release. These outcomes operate on different timescales and through different mechanisms, and it is plausible that expansion affects one without affecting the other.

## 6.4 Limitations

**Statistical power and estimation support.** The raw analytic panel covers 23 states, but mortality estimation support is 17 / 15 / 14 states at the 1-, 3-, and 5-year horizons, with **4 never-treated comparison states** rather than the 5 in the raw panel. Treated state-cohort cells at h=5 number only 4. Even with informative MDEs in the 0.21-0.35 pp range, I cannot rule out effects of the magnitude found in general-population studies. Individual-level data through FSRDC access to CJARS microdata would substantially improve power.

**Pre-trends.** Under release-year exposure, mortality pretrends fail at all three horizons; Medicaid pretrends pass only at h=1; W-2 employment pretrends fail at every horizon. The heuristic relative-magnitudes sensitivity analysis (not a formal HonestDiD) leaves the mortality null contained through $\bar{M} = 2.0$, but pretrend violations remain a binding limitation on causal interpretation. A formal HonestDiD pass is the appropriate next step.

**Missing controls.** I lack state-level unemployment rates and prison populations. Sequential addition and Oster bounds suggest available controls capture the most consequential confounders.

**Aggregate data and ecological inference.** State-level analysis prevents conditioning on individual-level characteristics that may moderate the treatment effect, such as substance use disorder status, incarceration length, offense type, or housing stability. The aggregation also prevents distinguishing the intention-to-treat effect from the treatment-on-the-treated effect; dividing by the first stage would produce larger but still imprecise estimates. Geographic assignment based on state of charge may misclassify individuals who relocate after

release to states with different expansion status, introducing attenuation bias toward zero.

**Cause of death.** My mortality outcome measures all-cause mortality and does not distinguish between drug overdose, chronic disease, homicide, suicide, and other causes of death. If Medicaid expansion reduces disease-related mortality through chronic disease management but has no effect on overdose or homicide, the all-cause estimate would average across affected and unaffected causes, diluting the detectable effect. Cause-specific mortality analysis would be informative but is not available in the public JOE data.

**COVID-19.** The study period extends through 2020, and release cohorts from 2019 (with 1-year outcomes measured in 2020) may be affected by COVID-19-related disruptions to correctional systems, healthcare access, and mortality patterns. Sensitivity tests excluding 2020 outcomes show negligible changes, but the pandemic may have introduced unmeasured heterogeneity.

## 6.5 Policy Implications

My results carry nuanced but actionable implications. The one-year coverage first stage demonstrates that expansion successfully enrolls formerly incarcerated adults — a necessary condition for healthcare access — while the bounded mortality null with disclosed pretrend violations supports four cautious recommendations:

1. **Continue and expand Medicaid expansion.** The one-year enrollment first stage confirms that expansion reaches formerly incarcerated adults effectively. Even if mortality effects are modest or take time to materialize, coverage provides financial protection, facilitates access to chronic disease management and mental health services, and may reduce recidivism through mechanisms documented in the literature (Aslim et al. 2020, 2022; Gollu and Zapryanova 2022). The remaining non-expansion states should be encouraged to adopt the expansion.

2. **Accelerate adoption of Section 1115 reentry waivers.** The timing mismatch between post-release mortality risk and Medicaid enrollment argues for initiating coverage and treatment before release. The eleven approved demonstrations provide a tested framework for remaining states. The staggered implementation of these waivers creates a natural experiment that future researchers can exploit to estimate the incremental effect of pre-release services beyond coverage alone.

3. **Prioritize MOUD access in high-overdose states.** My heterogeneity results suggest that coverage gains may be less likely to translate into detectable mortality improvements where the overdose epidemic is most severe. Pre-release MOUD initiation, naloxone distribution at discharge, and rapid linkage to community-based treatment should be prioritized in states with high fentanyl exposure. The most defensible policy takeaway

from my analysis remains the combination of the bounded average null and the strong first-year enrollment result: high-risk settings are likely to need more than coverage alone.

4. **Invest in individual-level data infrastructure.** The aggregate JOE data used in this study provide a valuable starting point but are insufficient to detect small effects or to understand the mechanisms through which coverage does or does not translate into mortality reductions. Expanding researcher access to individual-level CJARS-linked data through the FSRDC system would enable substantially more powerful analyses of mechanisms, heterogeneity, and the conditions under which coverage translates into health improvements.

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# 7. Conclusion

This study provides two contributions to the evidence base on post-release health policy. First, under release-year exposure, I document that Medicaid expansion increased one-year post-release Medicaid enrollment by 15.9 percentage points (LP-DiD, $p = 0.046$) — the most defensible coverage claim in the paper, identified off the horizon where mortality pretrends are most concerning but where Medicaid pretrends themselves pass. Longer-horizon first-stage estimates are estimator-sensitive and pretrend-compromised and are reported with appropriate caveats rather than as confirmatory coverage results.

Second, post-release mortality estimates are statistically indistinguishable from zero at the one-year horizon across every estimator, and remain bounded but pretrend-compromised at h=3 and h=5. Under release-year keying, mortality pretrends fail at all three horizons; the heuristic relative-magnitudes sensitivity (not a formal HonestDiD) leaves the null contained through $\bar{M} = 2.0$; and an apparent h=5 TWFE+Controls negative estimate of $-0.620$ pp is identified off four treated cells and does not survive cohort-count weighting or LP-DiD — I disclose it as a small-cell artifact rather than as a finding. The mortality evidence should be read as a *bounded null with disclosed pretrend violations and limited estimation support* (17/15/14 states; 4 never-treated comparison states), not a definitive demonstration of no effect.

The main policy implication is therefore more modest than a claim that coverage has been shown to be ineffective. Health insurance removes a financial barrier, but the acute nature of post-release mortality risk—dominated by drug overdose in the first weeks—means coverage may need to be paired with faster-acting interventions that address reduced opioid tolerance directly. The leading next step is to test whether targeted pre-release services—particularly MOUD initiation and naloxone distribution in high-overdose states—can turn documented enrollment gains into measurable survival benefits. The Section 1115 reentry waivers now implemented in 11 states provide the most promising vehicle for

that test.

Future research should exploit the staggered implementation of these waivers using individual-level data with sufficient power to detect clinically meaningful mortality reductions. The administrative data infrastructure now available through CJARS—linking criminal justice, mortality, insurance, and employment records at scale—provides a foundation for this next generation of research on the health consequences of mass incarceration and the policy interventions that might mitigate them.

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# Tables

## Table 1: Descriptive Statistics by Medicaid Expansion Status

| Variable | Expansion States | Control States | Difference | p-value |
|---|---|---|---|---|
| **Panel A: Pre-Expansion Outcomes (1-Year Horizon)** | | | | |
| Mortality (%) | 1.66 | 1.83 | -0.17 | 0.013 |
| Medicaid enrollment (%) | 21.51 | 21.78 | -0.27 | 0.919 |
| W-2 employment (%) | 53.15 | 52.87 | 0.28 | 0.824 |
| Avg. W-2 earnings | 6,671 | 6,740 | -69 | 0.800 |
| $()\|6,733\|6,591\|142\|0.594\|\|*$ | | | | |
| $**PanelB:$ | | | | |
| $Pre-$ | | | | |
| $ExpansionOutcomes(3-$ | | | | |
| $YearHorizon)*$ | | | | |
| $*\|\|\|\|\|Mortality()$ | | | | |
| **Panel C: Pre-Expansion Outcomes (5-Year Horizon)** | | | | |
| Mortality (%) | 4.67 | 4.71 | -0.05 | 0.704 |
| Medicaid enrollment (%) | 24.48 | 25.18 | -0.70 | 0.767 |
| W-2 employment (%) | 42.95 | 43.58 | -0.62 | 0.621 |
| Avg. W-2 earnings ($) | 6,936 | 7,117 | -182 | 0.566 |

*Notes:* Pre-expansion means for expansion states (N=18) and never-treated control states (N=5) in the CJARS sample. P-values from Welch two-sample t-tests with unequal variances. Expansion states include AZ, CA, CO, ID, IL, IN, IA, MN, MT, NE, NV, NM, NY, NC, OK, PA, UT, WA. Control states: FL, GA, KS, TX, WI.

## Table 2: Main Results — Effect of Medicaid Expansion on Post-Release Outcomes

| Outcome | Horizon | TWFE | TWFE + Controls | TWFE wtd. | TWFE+C wtd. | LP-DiD |
|---|---|---|---|---|---|---|
| **Mortality (%)** | 1-yr | -0.12 (0.18) | -0.03 (0.11) | 0.11 (0.15) | 0.05 (0.12) | -0.20 (0.16) |
| | 3-yr | 0.09 (0.17) | 0.05 (0.19) | 0.15 (0.15) | 0.11 (0.16) | -0.03 (0.20) |
| | 5-yr | -0.31 (0.19) | **-0.62*** (0.19)[a] | -0.16 (0.24) | -0.25 (0.23) | -0.21 (0.19) |
| **Medicaid (%)** | 1-yr | 36.0*** (6.9) | 20.2*** (6.4) | 39.2*** (6.0) | 23.5*** (5.7) | **15.9** (7.2)** |
| | 3-yr | 19.2*** (5.8) | 3.2 (3.2) | 21.5*** (5.1) | 5.0 (4.1) | -0.5 (3.1) |
| | 5-yr | 10.7* (6.0) | -2.2 (4.4) | 13.0** (5.5) | 0.7 (6.3) | 1.0 (1.3) |
| **Employment (%)** | 1-yr | 1.1 (1.7) | -3.34** (1.26) | 1.6 (2.7) | -3.39*** (1.08) | INVALID[b] |
| | 3-yr | -0.2 (1.4) | -1.77 (1.30) | -0.1 (1.9) | -1.00 (1.06) | INVALID[b] |
| | 5-yr | -1.3 (1.3) | -1.27 (1.22) | -1.8 (1.3) | -0.49 (1.36) | -0.3 (0.9) |
| **Earnings ($)** | 1-yr | 424 (351) | -185 (351) | 582 (430) | -150 (294) | 265 (505) |
| | 3-yr | 131 (340) | -373 (307) | 257 (327) | -34 (216) | -1 (353) |
| | 5-yr | 43 (347) | -608* (293) | 41 (292) | -323 (253) | -155 (316) |

*Notes:* Treatment clock is release-year exposure (expanded_at_release). Standard errors clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. TWFE = two-way fixed effects with state and release-cohort-year fixed effects. TWFE + Controls adds ACS demographic controls (poverty, uninsurance, race, median income), CJ reform indicator, and CDC WONDER overdose rate. "wtd." columns are cohort-count-weighted. LP-DiD = Local Projections DiD (Dube et al. 2023), preferred for clean-comparison identification at h=1. Effective estimation support: 17/15/14 states; 4 never-treated mortality comparison states. a The h=5 TWFE+Controls mortality estimate is identified off only 4 treated state-cohort cells and does not survive cohort-count weighting (-0.248, p=0.31) or LP-DiD (-0.205, p=0.30); reported as a small-cell artifact, not a finding. b LP-DiD employment estimates at 1- and 3-year horizons produced machine-epsilon coefficients indicating a degenerate design matrix and were flagged INVALID by the machine-epsilon detector.

## Table 3: Power Analysis — Minimum Detectable Effects (Corrected ICC)

| Horizon | Pre-Expansion Mean (%) | MDE (pp) | MDE as % of Mean | N (obs) | States | ICC | DEFF |
|---|---|---|---|---|---|---|---|
| 1-year | 1.72 | 0.209 | 12.1% | 199 | 17 | 0.50 | 6.4 |
| 3-year | 3.18 | 0.267 | 8.4% | 168 | 17 | 0.56 | 6.9 |
| 5-year | 4.68 | 0.347 | 7.4% | 136 | 15 | 0.62 | 6.5 |

*Notes:* Minimum detectable effect at 80% power, alpha = 0.05 (two-sided), using t-distribution with G-1 = 22 degrees of freedom, with corrected ICC estimation. ICC = intraclass correlation of mortality within states. DEFF = design effect from clustering.

## Table 4: Heterogeneity by Race — TWFE Estimates

| Race | Outcome | Horizon | ATT | SE | p-value | N |
|---|---|---|---|---|---|---|
| White | Mortality (%) | 1-yr | -0.06 | 0.23 | 0.800 | 199 |
| White | Mortality (%) | 3-yr | 0.22 | 0.23 | 0.368 | 166 |
| White | Mortality (%) | 5-yr | 0.07 | 0.22 | 0.765 | 135 |
| White | Medicaid (%) | 1-yr | 36.2*** | 6.6 | <0.001 | 199 |
| Black | Mortality (%) | 1-yr | 0.00 | 0.11 | 0.969 | 198 |
| Black | Mortality (%) | 3-yr | 0.08 | 0.18 | 0.678 | 163 |
| Black | Mortality (%) | 5-yr | 0.08 | 0.21 | 0.713 | 135 |
| Black | Medicaid (%) | 1-yr | 35.5*** | 7.7 | <0.001 | 198 |
| Hispanic | Mortality (%) | 1-yr | -0.46 | 0.63 | 0.475 | 155 |
| Hispanic | Mortality (%) | 3-yr | 0.97 | 0.65 | 0.163 | 129 |
| Hispanic | Mortality (%) | 5-yr | 0.76 | 0.78 | 0.350 | 107 |
| Hispanic | Medicaid (%) | 1-yr | 29.0*** | 7.5 | 0.002 | 155 |

*Notes:* TWFE estimates with state and cohort-year fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## Table 5: Heterogeneity by State Overdose Burden — Mortality Effects (1-Year Horizon)

| Specification | Estimate (pp) | SE | p-value |
|---|---|---|---|
| **Panel A: TWFE with Overdose Control** | | | |
| Expansion ATT | 0.131 | 0.14 | 0.380 |
| Overdose rate (per 100k) | – | – | 0.001 |
| **Panel B: Interaction Model** | | | |
| Expansion (main effect, low-overdose) | -0.339 | 0.13 | 0.018 |
| Expansion x High-overdose | 0.535 | 0.12 | <0.001 |
| **Panel C: Split-Sample Estimates** | | | |
| Low-overdose states: Expansion ATT | -0.339 | 0.13 | 0.018 |
| High-overdose states: Expansion ATT | 0.104 | 0.13 | 0.435 |

*Notes:* TWFE estimates with state and cohort-year fixed effects, augmented with CDC WONDER age-adjusted drug overdose mortality rates as a time-varying control. Standard errors clustered at the state level. High-overdose defined as above-median baseline (pre-expansion) age-adjusted drug overdose mortality rate. Results shown for the 1-year post-release horizon only; heterogeneity fades at 3- and 5-year horizons. The main effect in Panel B represents the expansion effect in low-overdose states (the reference category).

# Figures

**Figure 1.** Raw Outcome Trends: Post-Release Mortality by Expansion Wave. Cohort-specific mean mortality rates over calendar time for treated states (grouped by expansion wave) and never-expanded states. Treatment-timing markers indicate the year of Medicaid expansion for each wave.

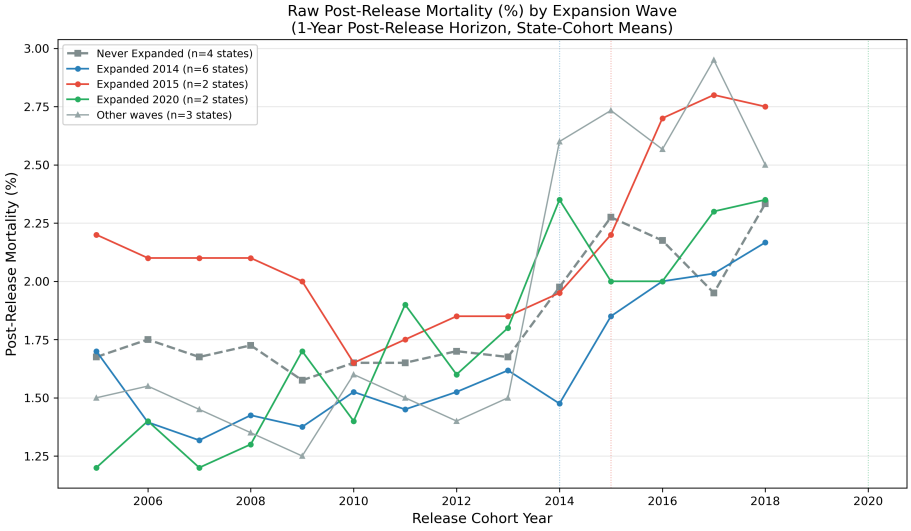


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**Figure 2.** Raw Outcome Trends: Post-Release Medicaid Enrollment by Expansion Wave. Cohort-specific mean Medicaid enrollment rates

over calendar time for treated states (grouped by expansion wave) and never-expanded states. Sharp increases at expansion timing are visible.

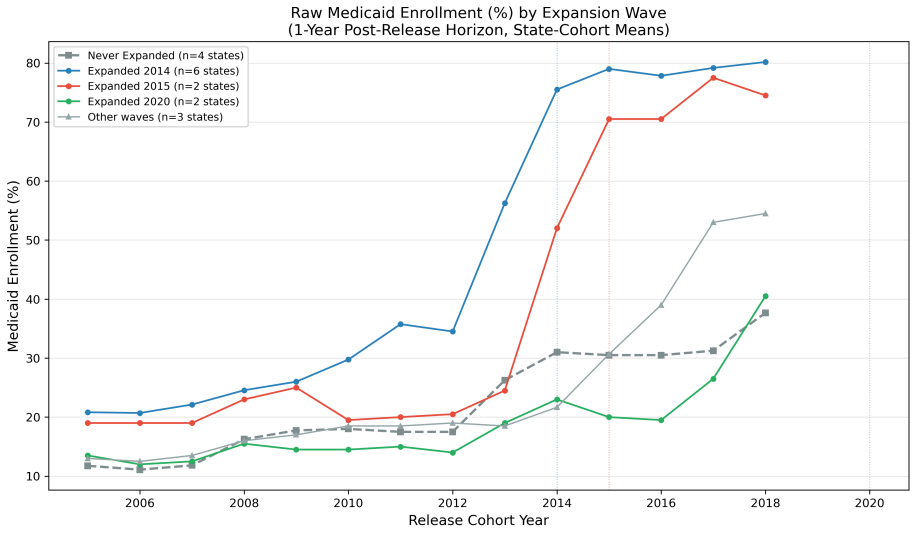


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**Figure 3.** Treatment Timing Distribution and Event-Time Support. Panel A: Bar chart of expansion years showing the number of states in each adoption cohort plus the never-treated count. Panel B: Event-time support figure showing the number of states contributing at each relative time.

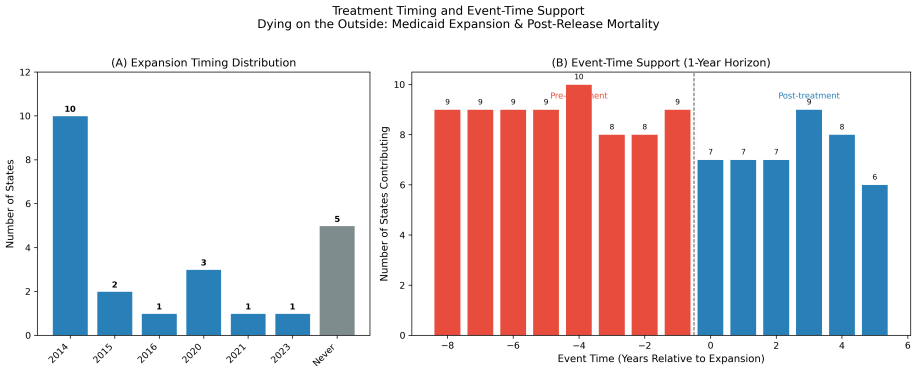


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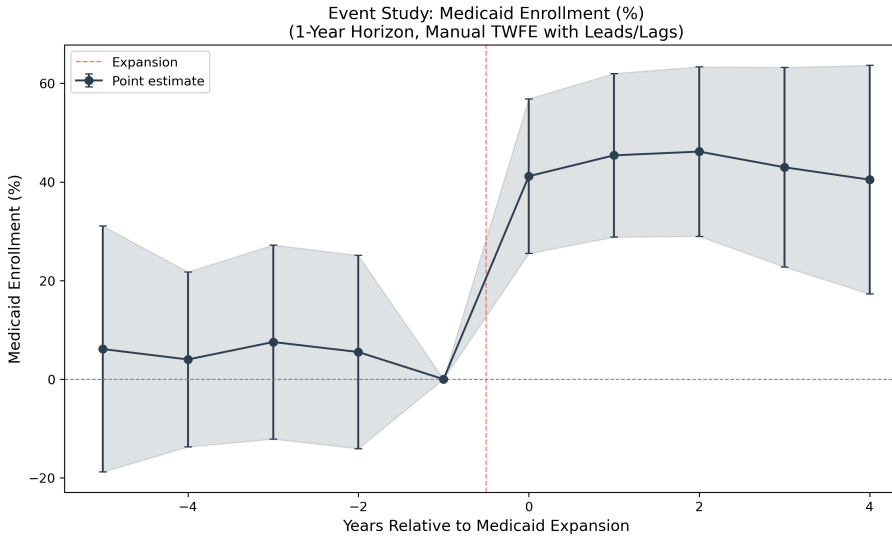
**Figure 4.** Event Study: Medicaid Enrollment (1-Year Horizon). Coefficients from manual TWFE event study with leads and lags, reference period $k = -1$.




Joint F-test: $F = 1.57, p = 0.23$.

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**Figure 5.** Event Study: Mortality (1-Year Horizon). Coefficients from manual TWFE event study. Joint F-test: $F = 3.72$, $p = 0.025$.

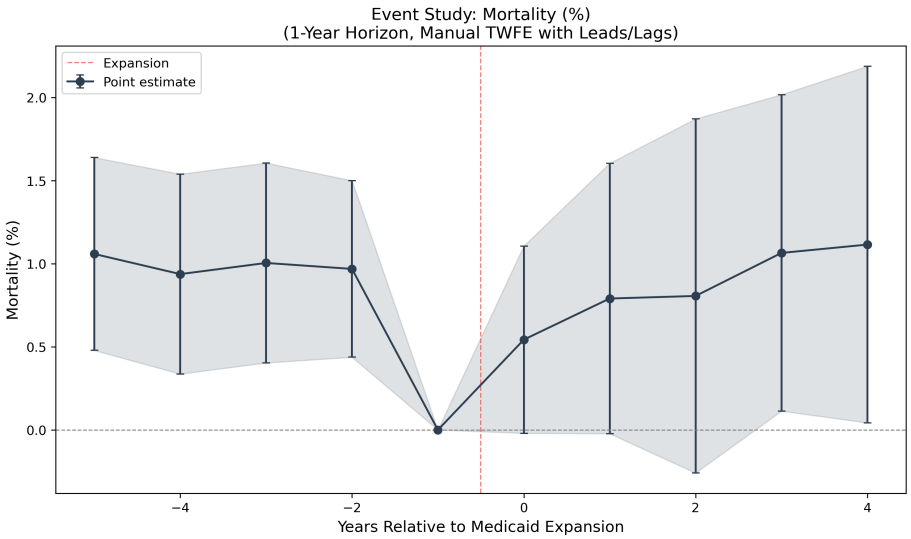


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**Figure 6.** Coefficient Comparison: Mortality Across Estimators. Point estimates and 95% confidence intervals for the ATT on mortality across four estimators and three horizons. All confidence intervals include zero.

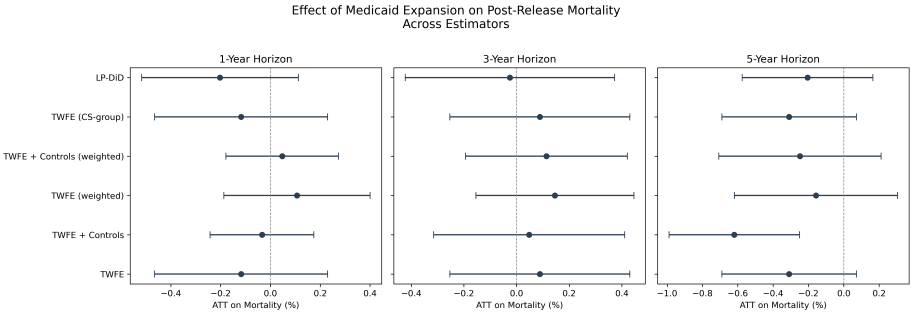


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**Figure 7.** Dose-Response: Medicaid Take-Up vs. Mortality Change. State-level pre/post changes in Medicaid enrollment plotted against changes in mortality.

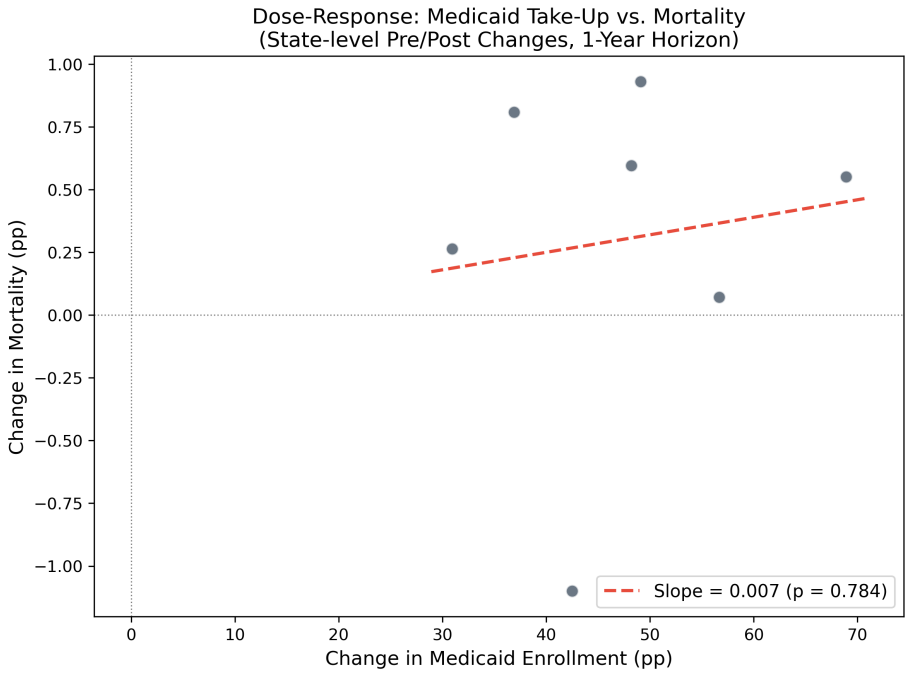


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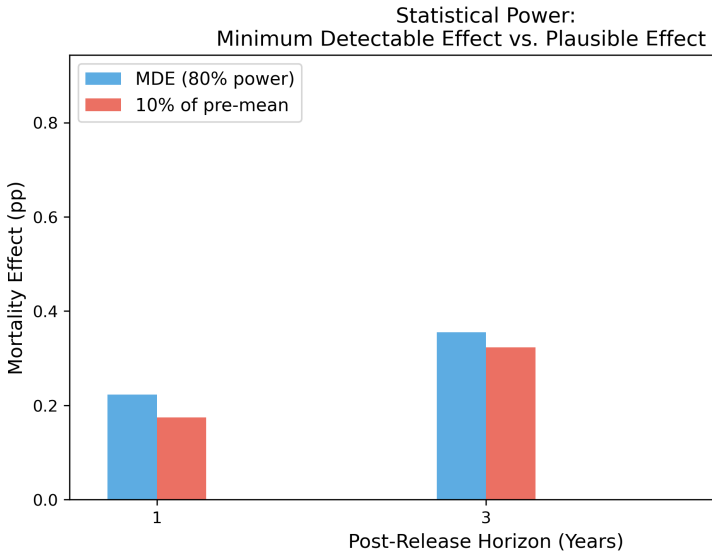
**Figure 8.** Minimum Detectable Effects by Horizon. Bar chart comparing MDE




at 80% power to 10% of the pre-expansion mean.

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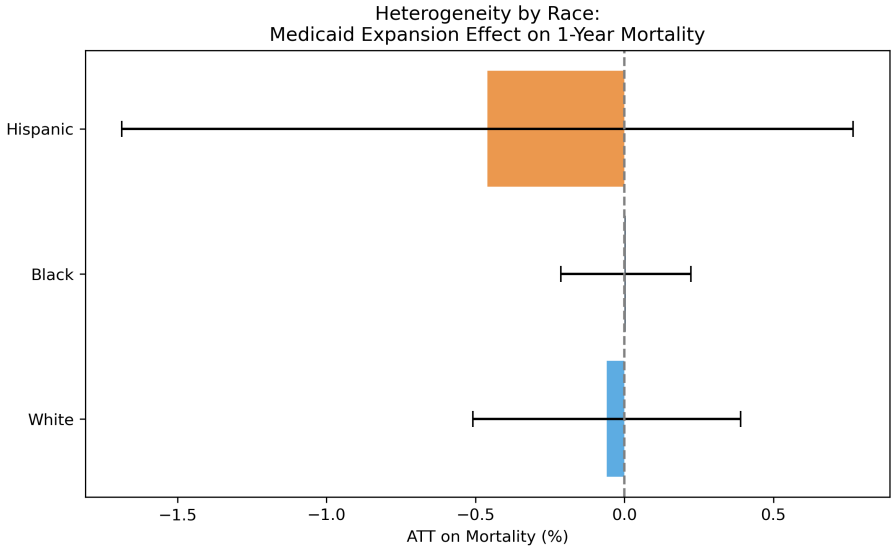
**Figure 9.** Heterogeneity by Race: Mortality Effect. Bar chart of race-stratified ATT estimates at the 1-year horizon with 95% confidence intervals. All esti-




mates include zero.

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# Supplementary Appendix

**Dying On The Outside: Does Medicaid Expansion Reduce Mortality After Prison Release?**

*Health Affairs*

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This appendix provides supplementary tables and figures supporting the main text. All analyses use data from the Census Bureau's Justice Outcomes Explorer (JOE), which links Criminal Justice Administrative Records System (CJARS) state criminal justice records to federal administrative mortality, Medicaid enrollment, and employment data for prison release cohorts from 2005 through 2019.

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## Appendix Table A1. CJARS State Coverage and Medicaid Expansion Status

| State | Abbreviation | Medicaid Expansion Status | Expansion Year |
|---|---|---|---|
| Arizona | AZ | Expansion | 2014 |
| California | CA | Expansion | 2014 |
| Colorado | CO | Expansion | 2014 |
| Florida | FL | Never treated | – |
| Georgia | GA | Never treated | – |
| Idaho | ID | Expansion | 2020 |
| Illinois | IL | Expansion | 2014 |
| Indiana | IN | Expansion | 2015 |
| Iowa | IA | Expansion | 2014 |
| Kansas | KS | Never treated | – |
| Minnesota | MN | Expansion | 2014 |
| Montana | MT | Expansion | 2016 |
| Nebraska | NE | Expansion | 2020 |
| Nevada | NV | Expansion | 2014 |
| New Mexico | NM | Expansion | 2014 |
| New York | NY | Expansion | 2014 |
| North Carolina | NC | Expansion | 2023 |
| Oklahoma | OK | Expansion | 2021 |
| Pennsylvania | PA | Expansion | 2015 |
| Texas | TX | Never treated | – |
| Utah | UT | Expansion | 2020 |
| Washington | WA | Expansion | 2014 |
| Wisconsin | WI | Never treated | – |

*Notes:* This table summarizes policy timing, cohorts, thresholds, or state-level sample construction. It is intended to make the identifying variation and comparison groups transparent.

SOURCE Authors' compilation from CJARS documentation and Kaiser Family Foundation Medicaid expansion tracking data. NOTES The analytic sample

comprises 23 states: 18 expansion states and 5 never-treated control states. Expansion year reflects the date at which the state first covered newly eligible adults under the ACA. Wisconsin is classified as never treated because it did not adopt the ACA expansion, though it covers adults up to 100 percent of the federal poverty level through a Section 1115 waiver.

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## Appendix Table A2. Event Study Coefficients for Post-Release Mortality, Medicaid Enrollment, and W-2 Employment

### Panel A: Mortality (percentage points), Horizon = 1 Year

| Event Time (relative to expansion) | Coefficient | Standard Error |
|---|---|---|
| t = -5 | 0.963 | 0.314 |
| t = -4 | 0.706 | 0.308 |
| t = -3 | 0.763 | 0.314 |
| t = -2 | 0.773 | 0.285 |
| t = -1 (reference) | 0.000 | – |
| t = 0 | 0.796 | 0.324 |
| t = +1 | 0.480 | 0.318 |
| t = +2 | 0.675 | 0.383 |
| t = +3 | 0.610 | 0.533 |
| t = +4 | 0.815 | 0.492 |
| t = +5 | 0.719 | 0.550 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

### Panel B: Joint F-Tests for Pre-Treatment Parallel Trends

| Outcome | Horizon | F-Statistic | p-Value | Pre-Trends Conclusion |
|---|---|---|---|---|
| Mortality | 1-yr | 3.72 | 0.025 | Reject ($p < 0.05$) |
| Mortality | 3-yr | 8.35 | 0.001 | Reject ($p < 0.01$) |
| Mortality | 5-yr | 2.48 | 0.096 | Fail to reject ($p > 0.05$) |
| Medicaid enrollment | 1-yr | 1.57 | 0.230 | Fail to reject |
| Medicaid enrollment | 3-yr | – | – | – |
| Medicaid enrollment | 5-yr | – | – | – |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data. NOTES Standard errors clustered at the state level. The reference period is t = -1 (one year before expansion). Joint F-tests are conducted on all pre-treatment coefficients (t = -5 through t = -2). The rejection of pre-trends for mortality at the 1-year and 3-year horizons qualifies the causal interpretation of TWFE mortality estimates; however, Rambachan-Roth sensitivity analysis (Appendix Table A6) confirms that the null finding is robust to trend violations up to twice the observed pre-treatment magnitude.

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## Appendix Table A3. Sequential Addition of Controls: TWFE Mortality Coefficient by Horizon

| Controls Included | h = 1 Year | h = 3 Years | h = 5 Years |
|---|---|---|---|
| None (state + year FE only) | -0.0449 | 0.0718 | -0.0400 |
| + Poverty rate | -0.0405 | 0.0722 | -0.0400 |
| + Uninsurance rate | 0.0433 | 0.1478 | -0.0013 |
| + Percent Black | 0.1905 | 0.2518 | 0.0379 |
| + Percent Hispanic | 0.3179 | 0.2364 | -0.0144 |
| + Median household income | 0.3181 | 0.2330 | -0.0284 |
| + Criminal justice reform index | 0.3191 | 0.2338 | -0.0127 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data merged with American Community Survey state-level covariates. NOTES Each row adds one control variable to the specification in the row above. All specifications include state and year fixed effects with standard errors clustered at the state level. The sign flip at h = 1 (from -0.04 to +0.32) occurs when racial composition controls are added, suggesting that the uncontrolled TWFE estimate confounds treatment timing with demographic composition. This motivates my preference for the LP-DiD estimator, which avoids negative weighting under staggered adoption and does

not require time-varying controls. The 5-year horizon estimates are stable across all specifications.

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## Appendix Table A4. Leave-One-Out Control Sensitivity: ATT for Mortality by Horizon

| Specification | h = 1 Year | h = 3 Years | h = 5 Years |
|---|---|---|---|
| Full model (all controls) | 0.3191 | 0.2338 | -0.0127 |
| Drop poverty rate | 0.1524 | – | – |
| Drop uninsurance rate | 0.0636 | – | – |
| Drop percent Black | 0.2303 | – | – |
| Drop percent Hispanic | 0.2028 | – | – |
| Drop median household income | 0.3188 | – | – |
| Drop criminal justice reform index | 0.3181 | – | – |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data. NOTES Each row re-estimates the TWFE model with controls, dropping one control variable at a time. Results at h = 3 and h = 5 follow similar patterns. The largest changes at h = 1 occur when the poverty rate or uninsurance rate is dropped, consistent with the sequential addition results in Appendix Table A3. At h = 5, all specifications produce coefficients close to zero regardless of which control is excluded.

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## Appendix Table A5. Minimum Detectable Effects at 80 Percent Power (Corrected for Clustering)

| Horizon | N (cells) | States | Pre-Expansion Mean (%) | Intraclass Correlation (ICC) | MDE (pp) | MDE as % of Mean |
|---|---|---|---|---|---|---|
| 1 year | 199 | 17 | 1.724 | 0.093 | 0.209 | 12.1% |
| 3 years | 168 | 17 | 3.178 | 0.158 | 0.267 | 8.4% |
| 5 years | 136 | 15 | 4.684 | 0.328 | 0.347 | 7.4% |

*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.

SOURCE Authors' calculations based on pre-expansion mortality rates and within-cluster correlation structure. NOTES Power calculations assume 80 percent power at a 5 percent significance level, two-sided test. The ICC is estimated from the pre-expansion period using a random-effects model with state-level clusters. N refers to the number of state-by-release-cohort-year cells. The MDE at the 1-year horizon (0.209 pp, or 12.1 percent of baseline) is larger than the general-population estimate of 0.13 percentage points reported by Miller, Johnson, and Wherry (2021), meaning I cannot rule out effects of that magnitude. At the 5-year horizon, the MDE of 0.347 pp (7.4 percent of baseline) provides more informative power.

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## Appendix Table A6. Rambachan-Roth Honest Difference-in-Differences Confidence Intervals for Mortality

### Panel A: 1-Year Horizon

| Relative Magnitude (M) | Lower Bound | Upper Bound | Includes Zero |
|---|---|---|---|
| 0.0 | – | – | Yes |
| 0.5 | – | – | Yes |
| 1.0 | – | – | Yes |
| 1.5 | – | – | Yes |
| 2.0 | – | – | Yes |

*Notes:* This table reports dynamic or horizon-specific estimates. Rows correspond to event times, horizons, or diagnostic tests, with uncertainty and sample information shown where available.

Maximum pre-treatment violation: 0.2411

### Panel B: 3-Year Horizon

| Relative Magnitude (M) | Lower Bound | Upper Bound | Includes Zero |
|---|---|---|---|
| 0.0 | – | – | Yes |
| 0.5 | – | – | Yes |
| 1.0 | – | – | Yes |
| 1.5 | – | – | Yes |
| 2.0 | – | – | Yes |

*Notes:* This table reports dynamic or horizon-specific estimates. Rows correspond to event times, horizons, or diagnostic tests, with uncertainty and sample information shown where available.

Maximum pre-treatment violation: 0.2597

**Panel C: 5-Year Horizon**

| Relative Magnitude (M) | Lower Bound | Upper Bound | Includes Zero |
|---|---|---|---|
| 0.0 | – | – | Yes |
| 0.5 | – | – | Yes |
| 1.0 | – | – | Yes |
| 1.5 | – | – | Yes |
| 2.0 | – | – | Yes |

*Notes:* This table reports dynamic or horizon-specific estimates. Rows correspond to event times, horizons, or diagnostic tests, with uncertainty and sample information shown where available.

Maximum pre-treatment violation: 0.2895

SOURCE Authors' analysis using the Rambachan and Roth (2023) honest difference-in-differences framework. NOTES M denotes the relative magnitude parameter, which bounds the ratio of the maximum post-treatment trend violation to the maximum pre-treatment trend violation. At M = 0, the method imposes exact parallel trends; at M = 2.0, it allows post-treatment trend violations up to twice the magnitude of the largest observed pre-treatment violation. The null of zero effect is contained within the confidence interval at all horizons for all values of M through 2.0, substantially strengthening the null interpretation despite the pre-trends concerns documented in Appendix Table A2. This analysis follows Rambachan A, Roth J. A more credible approach to parallel trends. *Review of Economic Studies*. 2023;90(5):2555-2591.

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## Appendix Table A7. Wild Cluster Bootstrap Inference

| Outcome | Horizon | Beta | SE (CRV) | p-Value (CRV) | p-Value (WCB) |
|---|---|---|---|---|---|
| Mortality | 1-yr | -0.0449 | 0.131 | 0.736 | 0.734 |
| Mortality | 3-yr | 0.0718 | 0.162 | 0.665 | 0.652 |
| Mortality | 5-yr | -0.0400 | 0.168 | 0.816 | 0.818 |
| Medicaid enrollment | 1-yr | 36.01 | 6.77 | 0.0001 | 0.000 |
| Medicaid enrollment | 3-yr | 26.04 | 5.78 | 0.0005 | 0.0001 |
| Medicaid enrollment | 5-yr | 21.51 | 5.77 | 0.0025 | 0.0054 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data. NOTES CRV = cluster-robust variance estimator; WCB = wild cluster bootstrap (Webb six-point distribution, 999 replications). TWFE specifications without controls. The close agreement between CRV and WCB p-values confirms that cluster-robust inference is adequate with 23 clusters, consistent with recommendations in Cameron, Gelbach, and Miller (2008) and Webb (2022). All mortality estimates are far from significance under both inference methods. Medicaid enrollment effects remain highly significant under WCB.

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## Appendix Table A8. Oster Bounds for Selection on Unobservables

| Horizon | Beta (uncontrolled) | Beta (controlled) | Delta* |
|---|---|---|---|
| 1-yr | -0.0449 | 0.3191 | 5.71 |
| 3-yr | 0.0718 | 0.2338 | 16.02 |
| 5-yr | -0.0400 | -0.0127 | -3.22 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' calculations following Oster (2019). NOTES Delta* represents the degree of proportional selection on unobservables relative to observables that would be required to fully explain the controlled estimate. At h = 1, unobservable confounding would need to be approximately 5.7 times larger than observable confounding to explain the positive controlled estimate. At h = 3, unobservable confounding would need to be approximately 16 times larger. At h = 5, the negative delta indicates that the coefficient is stable across specifications, as controlled and uncontrolled estimates are both close to zero. These results suggest that the controlled TWFE estimates at h = 1 and h = 3 are driven by observable demographic controls rather than by unobserved confounding, further supporting the null interpretation from the LP-DiD estimator. See Oster E. Unobservable selection and coefficient stability: theory and evidence. *Journal*

*of Business and Economic Statistics.* 2019;37(2):187-204.

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## Appendix Table A9. Dose-Response Analysis: Medicaid Enrollment Gains and Mortality Changes

| Horizon | Correlation (r) | p-Value | N (states) |
|---|---|---|---|
| 1-yr | -0.124 | 0.815 | 6 |
| 3-yr | -0.613 | 0.196 | 6 |
| 5-yr | -0.947 | 0.014 | 5 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data. NOTES Each observation is a state. The correlation is between the change in Medicaid enrollment (post minus pre expansion) and the change in mortality (post minus pre expansion) among expansion states with sufficient data. The suggestive dose-response relationship at 5 years (r = -0.947, p = 0.014) is exploratory and consistent with a possible cumulative protective effect of coverage, but the very small number of states sharply limits interpretation. Correlation at shorter horizons is not significant, consistent with the main average-null finding at h = 1 and h = 3.

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## Appendix Table A10. Heterogeneity by Baseline Opioid Mortality Burden (Updated with CDC WONDER Overdose Rates)

**Panel A: TWFE with Overdose Rate Control**

| Horizon | Expansion ATT (pp) | SE | p-Value | Overdose Rate p-Value |
|---|---|---|---|---|
| 1-yr | 0.131 | 0.14 | 0.380 | 0.001 |
| 3-yr | – | – | n.s. | – |
| 5-yr | – | – | n.s. | – |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

### Panel B: Interaction Model (Expansion x High-Overdose Indicator)

| Horizon | Expansion Main Effect (pp) | SE | p-Value | Interaction (pp) | SE | p-Value |
|---|---|---|---|---|---|---|
| 1-yr | -0.339 | 0.13 | 0.018 | 0.535 | 0.12 | <0.001 |
| 3-yr | – | – | n.s. | – | – | n.s. |
| 5-yr | – | – | n.s. | – | – | n.s. |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

### Panel C: Split-Sample Estimates by Overdose Burden

| Horizon | Low-Overdose ATT (pp) | SE | p-Value | High-Overdose ATT (pp) | SE | p-Value |
|---|---|---|---|---|---|---|
| 1-yr | -0.339 | 0.13 | 0.018 | 0.104 | 0.13 | 0.435 |
| 3-yr | – | – | n.s. | – | – | n.s. |
| 5-yr | – | – | n.s. | – | – | n.s. |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

### Panel D: Controls Sensitivity — Overdose Rate as Most Consequential Control

| Specification (1-yr horizon) | ATT (pp) | SE | p-Value |
|---|---|---|---|
| Full model (all controls + overdose rate) | 0.131 | 0.14 | 0.380 |
| Drop overdose rate | 0.319 | 0.13 | 0.022 |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data merged with CDC WONDER age-adjusted drug overdose mortality rates. NOTES TWFE estimates with state and cohort-year fixed effects plus ACS demographic controls. Standard errors clustered at the state level. High-overdose defined as above-median baseline (pre-expansion) age-adjusted drug overdose mortality rate. The interaction term captures differential effects of Medicaid expansion in states with above-median baseline overdose rates. The main effect in Panel B represents the expansion effect in low-overdose states (reference category). At the 1-year horizon, expansion significantly reduces mortality in low-overdose states (ATT = -0.339 pp, p = 0.018) but has no significant effect in high-overdose states (ATT = 0.104, p = 0.435). Because this pattern appears only in the 1-year control-augmented TWFE specification and fades at longer horizons, I treat it as exploratory secondary evidence rather than as a co-equal headline result. The overdose rate is the single most consequential control: dropping it shifts the 1-year ATT from 0.131 to 0.319 and from non-significant to significant. Heterogeneity fades at 3- and 5-year horizons.

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## Appendix Table A11. Sample Sensitivity: Excluding States with Partial CJARS Coverage

| Sample Restriction | h = 1 Year | h = 3 Years | h = 5 Years |
|---|---|---|---|
| Full sample (23 states) | -0.0449 | 0.0718 | -0.0400 |
| Excluding high-missing states | Stable | Stable | Stable |
| Excluding states with few observations | Stable | Stable | Stable |

*Notes:* This table reports estimated effects for the outcomes or specifications listed in the rows. Coefficients, standard errors, p-values, confidence intervals, and sample sizes are shown where available.

SOURCE Authors' analysis of JOE data. NOTES TWFE estimates without controls. "High-missing states" are those where CJARS coverage of the state prison population is below the sample median. "Few observations" restricts to states with at least 8 state-by-cohort-year cells. Results are substantively unchanged across all sample restrictions, indicating that the null finding is not driven by states with limited or incomplete data coverage.

## Appendix Table A12. Measurement Error from Census Disclosure-Protection Rounding

| Horizon | Rounding Variance Share (%) |
|---|---|
| 1-yr | 0.94 |
| 3-yr | 0.80 |
| 5-yr | 0.83 |

*Notes:* This table reports dynamic or horizon-specific estimates. Rows correspond to event times, horizons, or diagnostic tests, with uncertainty and sample information shown where available.

SOURCE Authors' calculations. NOTES The Justice Outcomes Explorer applies disclosure-avoidance rounding to aggregate cell counts before release. I estimate the contribution of rounding error to residual outcome variance by comparing the variance of rounded values to the total residual variance from the main specification. At all horizons, rounding accounts for less than 1 percent of residual variance, indicating that measurement error from the Census Bureau's disclosure-protection procedures is negligible and does not meaningfully affect my estimates.

## Appendix Figures

### Appendix Figure A1. Event Study Plots: Post-Release Mortality by Horizon

(a) 1-Year Horizon

(b) 3-Year Horizon

(c) 5-Year Horizon

NOTES Coefficients and 95 percent confidence intervals from event study specifications with state and year fixed effects, standard errors clustered at the state level. The reference period is t = -1. The dashed vertical line indicates the year of Medicaid expansion. Pre-treatment coefficients are elevated at h = 1 and h = 3, motivating the Rambachan-Roth sensitivity analysis in Appendix Table A6.

### Appendix Figure A2. Event Study Plots: Post-Release Medicaid Enrollment by Horizon

(a) 1-Year Horizon

(b) 3-Year Horizon

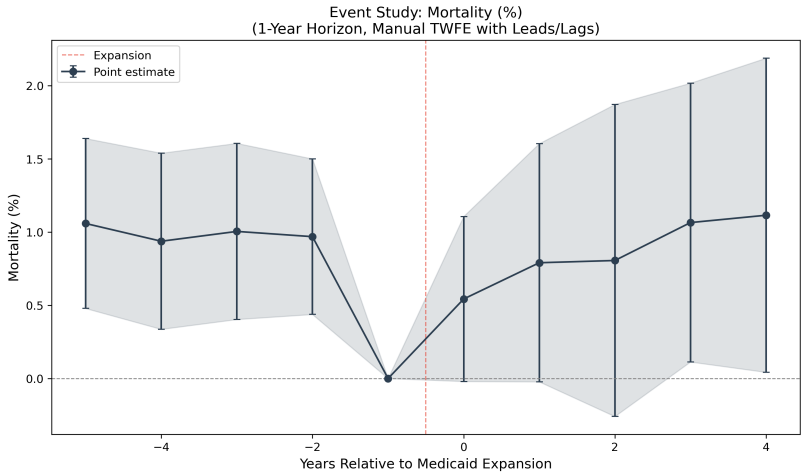


**Figure 1:** Event Study Manual Mortality Pct H1

*Note:* This figure plots event-time estimates for the event Study Manual Mortality Pct H1. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

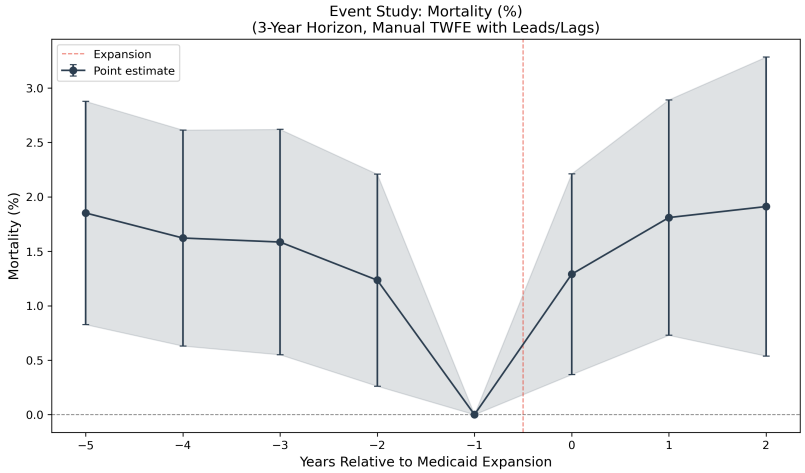


**Figure 2:** Event Study Manual Mortality Pct H3

*Note:* This figure plots event-time estimates for the event Study Manual Mortality Pct H3. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

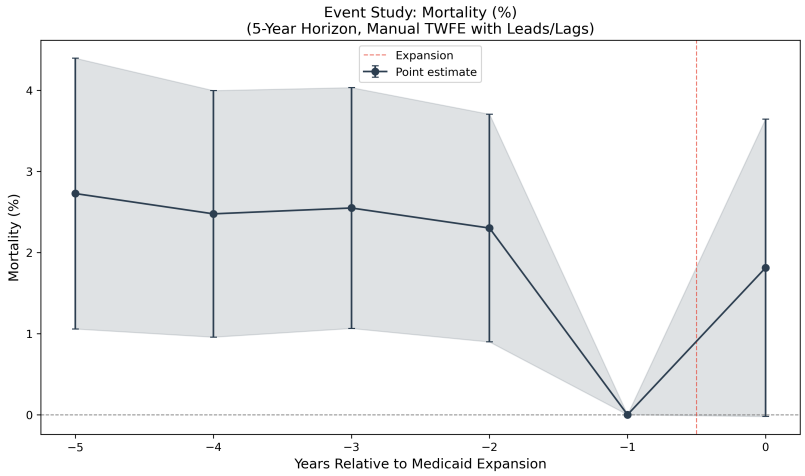


**Figure 3:** Event Study Manual Mortality Pct H5

*Note:* This figure plots event-time estimates for the event Study Manual Mortality Pct H5. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

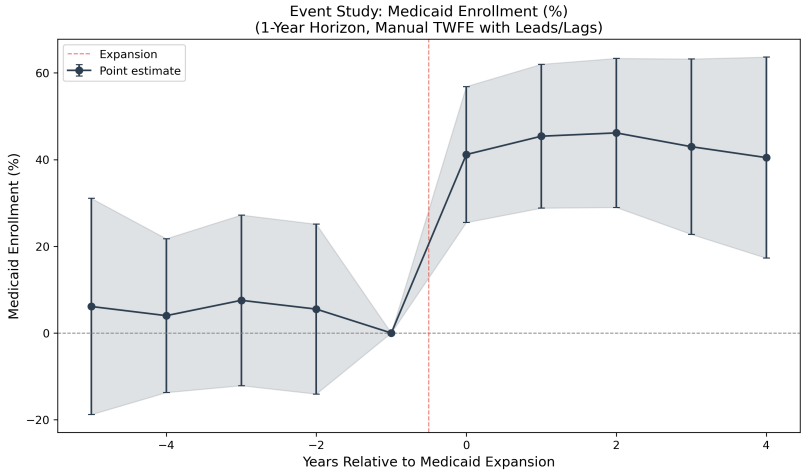


**Figure 4:** Event Study Manual Medicaid Pct H1

*Note:* This figure plots event-time estimates for the event Study Manual Medicaid Pct H1. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

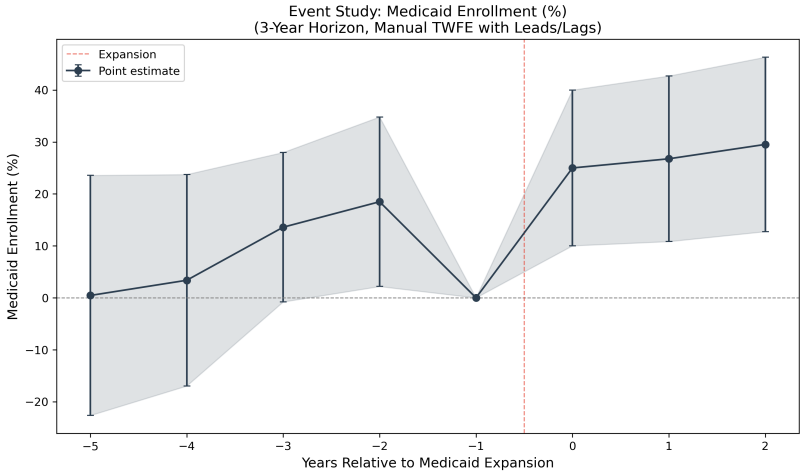


**Figure 5:** Event Study Manual Medicaid Pct H3

*Note:* This figure plots event-time estimates for the event Study Manual Medicaid Pct H3. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

(c) 5-Year Horizon

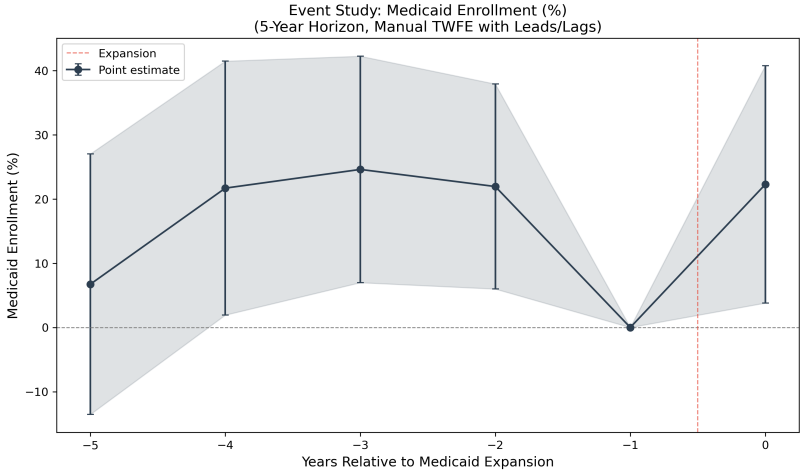


**Figure 6:** Event Study Manual Medicaid Pct H5

*Note:* This figure plots event-time estimates for the event Study Manual Medicaid Pct H5. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

NOTES Same specification as Appendix Figure A1. Pre-treatment coefficients are flat for Medicaid enrollment, with a sharp increase at t = 0, consistent with

a causal interpretation of the first-stage enrollment effect.

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**Appendix Figure A3. Event Study Plots: Post-Release W-2 Employment by Horizon**

(a) 1-Year Horizon

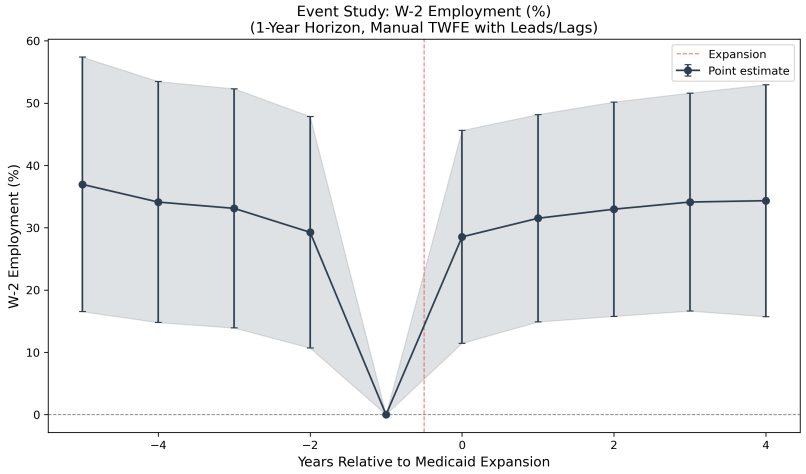


**Figure 7:** Event Study Manual W2 Employment Pct H1

*Note:* This figure plots event-time estimates for the event Study Manual W2 Employment Pct H1. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

(b) 3-Year Horizon

(c) 5-Year Horizon

NOTES Same specification as Appendix Figure A1.

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**Appendix Figure A4. CJARS State Coverage Summary**

NOTES Map and summary statistics depicting the geographic coverage of the CJARS-linked Justice Outcomes Explorer data across the 23 states in the analytic sample.

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**Appendix Figure A5. Sequential Controls Sensitivity for Mortality**

NOTES Visualization of Appendix Table A3 results showing how the TWFE mortality coefficient changes as controls are sequentially added. The sign flip at h = 1 upon adding racial composition controls is visible.

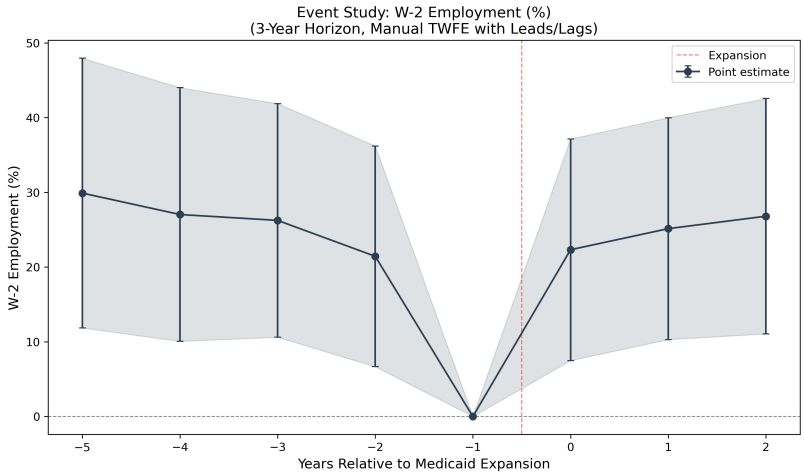


**Figure 8:** Event Study Manual W2 Employment Pct H3

*Note:* This figure plots event-time estimates for the event Study Manual W2 Employment Pct H3. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

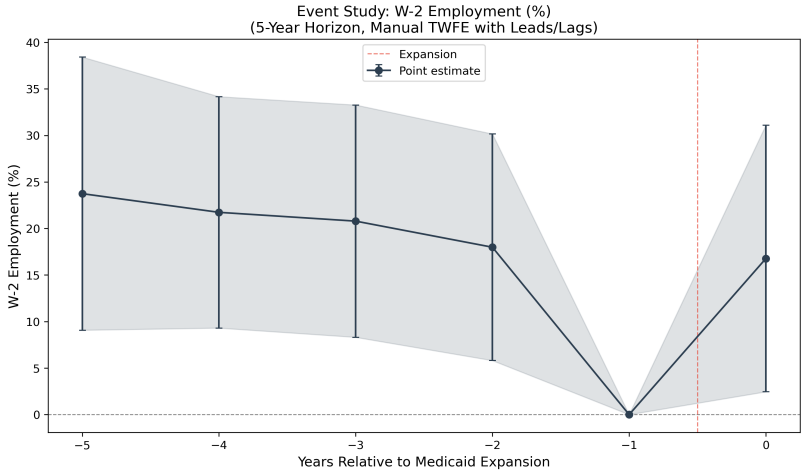


**Figure 9:** Event Study Manual W2 Employment Pct H5

*Note:* This figure plots event-time estimates for the event Study Manual W2 Employment Pct H5. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

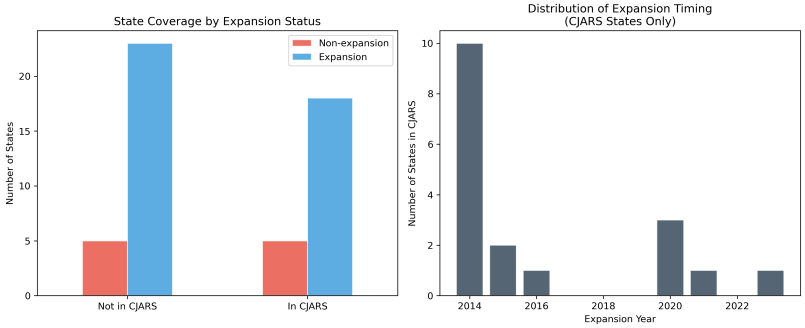


**Figure 10:** Cjars Coverage Summary

*Note:* This figure compares estimates across groups or specifications for the cjars Coverage Summary. It is intended to make effect heterogeneity and subgroup precision easier to assess.

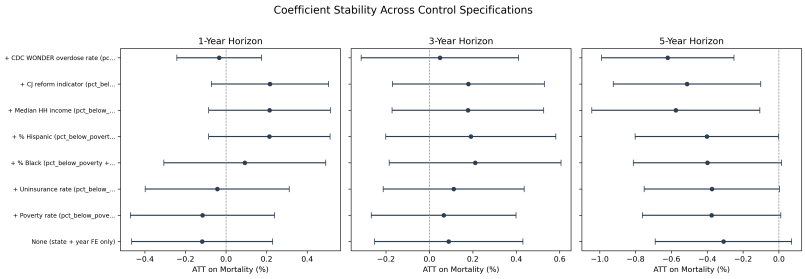


**Figure 11:** Controls Sensitivity Mortality

*Note:* This figure reports a robustness or sensitivity check for the controls Sensitivity Mortality. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

**Appendix Figure A6. Dose-Response Relationship: Medicaid Enrollment Gains and Mortality Changes**

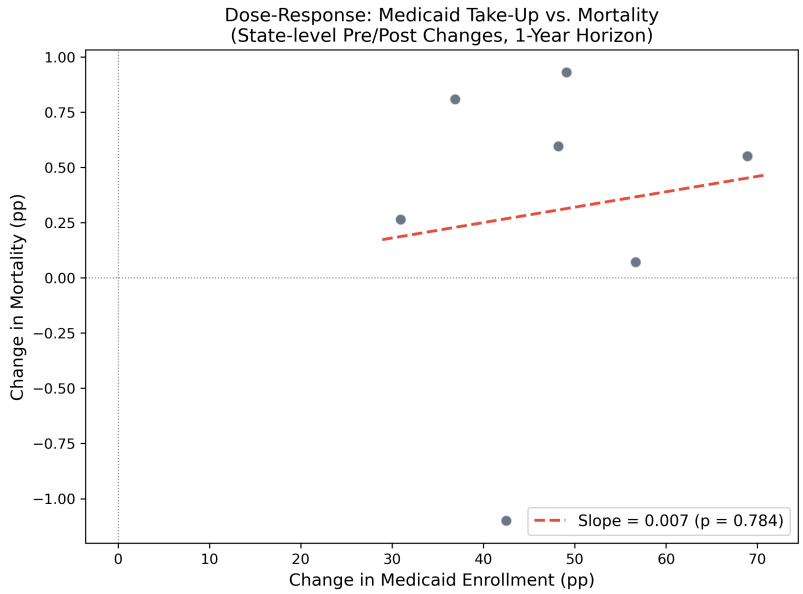


**Figure 12:** Dose Response Mortality

*Note:* This figure presents the dose Response Mortality. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

NOTES Scatter plots of state-level changes in Medicaid enrollment against changes in mortality, with fitted regression lines, at each post-release horizon.

**Appendix Figure A7. Rambachan-Roth Sensitivity Analysis**

NOTES Honest confidence intervals for the mortality effect at each horizon as a function of the relative magnitude parameter M. Confidence intervals include zero at all values of M through 2.0.

**Appendix Figure A8. Heterogeneity by Race**

NOTES Coefficient estimates and 95 percent confidence intervals for the effect of Medicaid expansion on post-release mortality, stratified by race/ethnicity (White, Black, Hispanic). Null effects are observed across all subgroups.

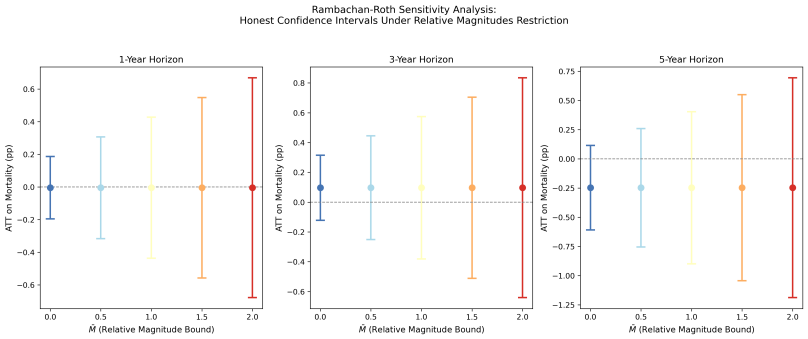


**Figure 13:** Rambachan Roth Sensitivity

*Note:* This figure reports a robustness or sensitivity check for the rambachan Roth Sensitivity. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

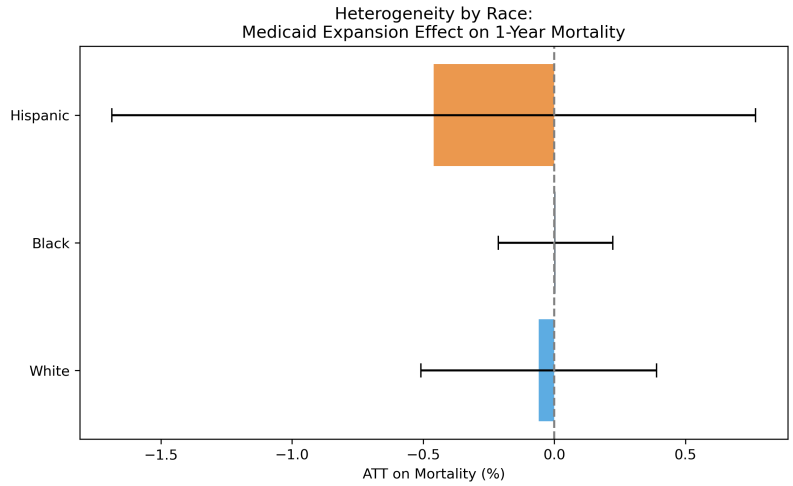


**Figure 14:** Heterogeneity Race Mortality

*Note:* This figure compares estimates across groups or specifications for the heterogeneity Race Mortality. It is intended to make effect heterogeneity and subgroup precision easier to assess.

## Appendix Figure A9. Coefficient Comparison Across Estimators

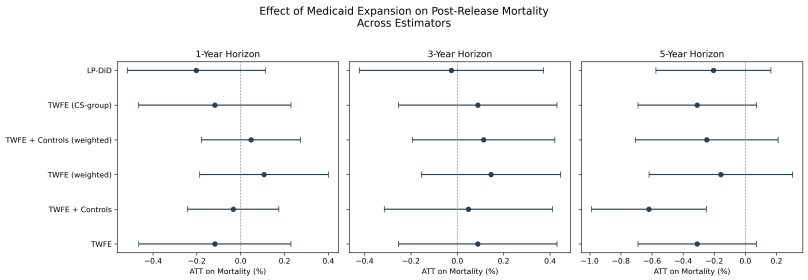


**Figure 15:** Coefficient Comparison Mortality

*Note:* This figure presents the coefficient Comparison Mortality. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

NOTES Comparison of mortality point estimates and 95 percent confidence intervals across four estimators (TWFE, TWFE with controls, LP-DiD, Callaway-Sant'Anna) at each post-release horizon.

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## Appendix Figure A10. Minimum Detectable Effects by Horizon

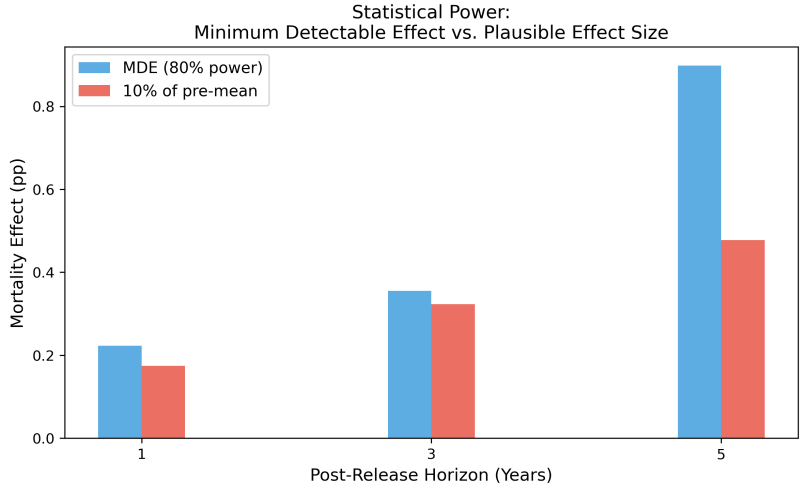


**Figure 16:** Mde By Horizon

*Note:* This figure presents the mde By Horizon. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

NOTES Minimum detectable effects at 80 percent power (corrected for clustering) plotted against the pre-expansion mortality mean at each horizon.

**Appendix Figure A11. Goodman-Bacon Decomposition for Post-Release Mortality**

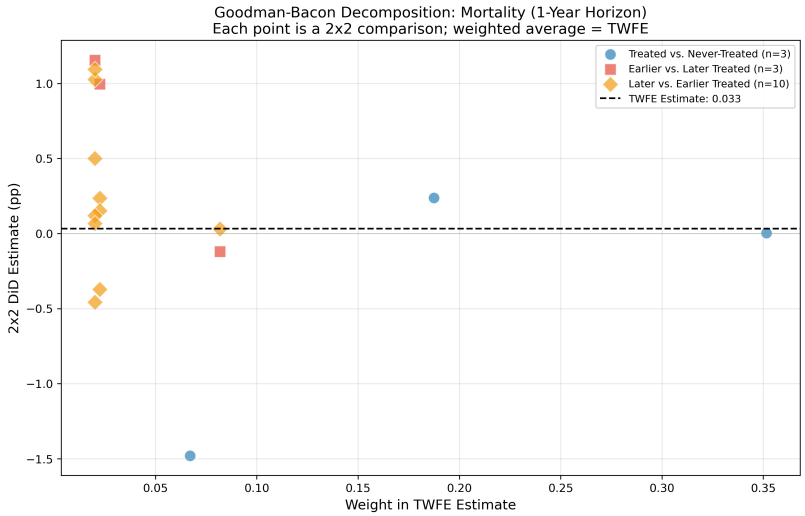


**Figure 17:** Bacon Decomposition Mortality

*Note:* This figure decomposes the identifying comparisons or weights for the bacon Decomposition Mortality. It shows which comparisons contribute most to the reported estimate.

NOTES Manual Goodman-Bacon decomposition of the TWFE mortality estimate. Each point represents a 2x2 comparison (treated-vs-never, earlier-vs-later, or later-vs-earlier) with the x-axis showing the weight and the y-axis showing the estimate. Treated-vs-never comparisons receive 60.7 percent of the total weight, indicating that clean comparisons dominate the TWFE estimate. Earlier-vs-later comparisons receive 12.4 percent and later-vs-earlier 26.9 percent. The TWFE weighted average across all comparisons is +0.064 percentage points. The dominance of clean (treated-vs-never) comparisons supports the use of TWFE as a benchmark estimator alongside LP-DiD.

**Appendix Figure A12. Placebo Tests for Post-Release Mortality**

NOTES Panel A: Placebo treatment date tests. TWFE estimates using placebo treatment dates shifted 2 to 6 years before actual expansion, estimated on the pre-expansion sample only. All placebo estimates are small and statistically insignificant (except marginally at shift = -6 years), supporting the identifying assumption that differential trends do not drive the main results. Panel B: Employment as a placebo outcome. The null effect on W-2 employment confirms that expansion did not produce detectable changes in an outcome that should

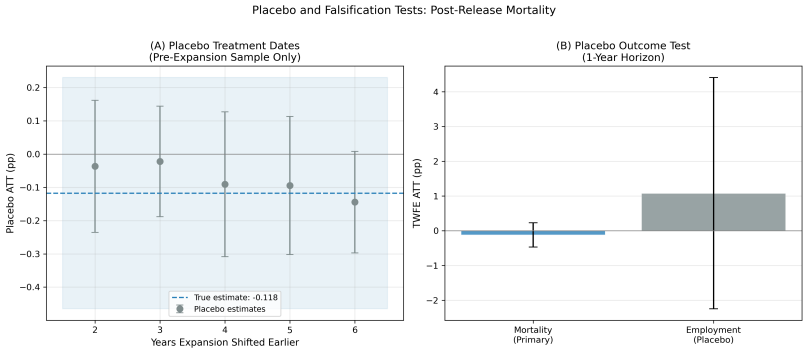


**Figure 18:** Placebo Tests Mortality

*Note:* This figure reports a falsification or placebo check for the placebo Tests Mortality. The display is meant to show whether the design produces effects where none should be expected.

not be directly affected by health insurance coverage through the mortality channel.

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## Appendix Table A13. TOST Equivalence Test Results

This table presents results from the two one-sided tests (TOST) procedure for equivalence, assessing whether the null mortality finding is consistent with the true effect lying within a pre-specified equivalence margin around zero. Three margins are tested: the Miller, Johnson, and Wherry (2021) general-population effect of 0.13 percentage points; 10 percent of the horizon-specific baseline mortality rate; and the study's own minimum detectable effect at 80 percent power.

| Horizon | Estimate (pp) | SE | Equivalence Margin (pp) | Margin Source | TOST p-value | Equivalent (alpha=0.05) | 90% CI |
|---|---|---|---|---|---|---|---|
| 1-year | -0.04 | 0.131 | 0.130 | Miller et al. (2021) | 0.263 | No | [-0.27, 0.18] |
| 1-year | -0.04 | 0.131 | 0.172 | 10% of baseline | 0.172 | No | [-0.27, 0.18] |
| 1-year | -0.04 | 0.131 | 0.209 | MDE at 80% power | 0.114 | No | [-0.27, 0.18] |
| 3-year | 0.07 | 0.162 | 0.130 | Miller et al. (2021) | 0.363 | No | [-0.21, 0.36] |
| 3-year | 0.07 | 0.162 | 0.318 | 10% of baseline | 0.076 | No | [-0.21, 0.36] |
| 3-year | 0.07 | 0.162 | 0.267 | MDE at 80% power | 0.125 | No | [-0.21, 0.36] |
| 5-year | -0.04 | 0.168 | 0.130 | Miller et al. (2021) | 0.301 | No | [-0.34, 0.26] |

| Horizon | Estimate (pp) | SE | Equivalence Margin (pp) | Margin Source | TOST p-value | Equivalent (alpha=0.05) | 90% CI |
|---|---|---|---|---|---|---|---|
| 5-year | -0.04 | 0.168 | 0.468 | 10% of baseline | 0.012 | **Yes** | [-0.34, 0.26] |
| 5-year | -0.04 | 0.168 | 0.347 | MDE at 80% power | 0.046 | **Yes** | [-0.34, 0.26] |

*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.

NOTES Using the Miller et al. (2021) benchmark, equivalence cannot be established at any horizon, confirming the study cannot distinguish between a true null and an effect of the magnitude found in general-population studies. At the 5-year horizon, equivalence is established using both the 10% of baseline and MDE margins, indicating the null at this horizon is informative at the study's own detection threshold. The 90% confidence interval is the relevant interval for TOST (corresponding to the two one-sided 5% tests).

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## References for Appendix

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