Suspended, Not Terminated: Medicaid Coverage Continuity Across the Prison Door

Abstract

When individuals are incarcerated, states may either suspend or terminate their Medicaid eligibility. Under suspension, coverage is paused during incarceration and reactivated upon release without a new application; under termination, eligibility is ended entirely and the individual must reapply after release, navigating documentation requirements, processing delays, and bureaucratic complexity at a moment of acute vulnerability. I estimate the intent-to-treat (ITT) association between state Medicaid suspension policies and post-release Medicaid enrollment, employment, earnings, and mortality among state prison release cohorts, exploiting the staggered rollout of suspension across U.S. states between 2005 and 2022. The primary outcome is **point-in-time Medicaid enrollment at one, three, and five years post-release**, not "any enrollment" during those windows. Linking a newly constructed state-year policy panel triangulated from six independent sources to the Census Bureau's Criminal Justice Administrative Records System (CJARS) Justice Outcomes Explorer—which provides administratively linked aggregate outcomes for state prison release cohorts across 31 states—I follow the staggered-adoption inference framework of Callaway and Sant'Anna (2021) and the local-projections difference-in-differences (LP-DiD) estimator of Dube et al. (2023), with two-way fixed effects (TWFE) reported as a benchmark. Because the outcome is measured at the release-cohort level, the estimates reflect ITT effects of state suspension policy on the full release cohort, not individual-level treatment effects among those whose Medicaid was actually suspended. Suspension is associated with a 22.4 percentage-point increase in 1-year post-release Medicaid enrollment (LP-DiD, p = 0.001), with similar estimates from TWFE (22.2 pp) and Callaway-Sant'Anna (25.8 pp), and similarly large estimates at 3-year (18.7 pp) and 5-year (20.7 pp) horizons. I find no significant effects on W-2 employment or earnings. Mortality estimates are inconclusive across estimators and underpowered for the 3–6 percent reductions documented in the broader Medicaid-mortality literature. Identification rests on sparse never-treated support — Texas is the only never-treated state with non-missing Medicaid outcome rows (Georgia is in the 31-state policy panel but absent from the JOE Medicaid panel) — so I treat design-based randomization inference (RI) as a conservative stress test and the cross-estimator pattern, including not-yet-treated states as comparators under LP-DiD, as the strongest evidentiary basis. The enrollment finding survives leave-one-out checks (including dropping each never-treated state and dropping South Carolina), policy-coding sensitivity, weighted estimation, and a pre-ACA-only restriction; adding release-cohort composition controls attenuates the matched-sample estimate to roughly 14 pp but leaves the qualitative conclusion intact, and RI is more conservative and does not reach conventional thresholds. These estimates extend prior single-state evidence from South Carolina (Packham and Slusky, 2024) and

Wisconsin pre-release enrollment-assistance evidence (Burns et al., 2022) to a multi-state, longer-horizon design, and provide an implementation benchmark for the federal prohibition on terminating Medicaid eligibility solely because of inmate status that took effect on January 1, 2026 under Section 205 of the Consolidated Appropriations Act, 2024.

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1. Introduction

Each year, more than 600,000 individuals are released from state and federal prisons in the United States, entering a period of acute vulnerability during which the risk of overdose death is 12.7 times that of the general population (Binswanger et al., 2007). Access to health insurance—and to the primary care, behavioral health services, and medication-assisted treatment it facilitates—is critical during this transition. Yet for decades, a seemingly minor administrative distinction has determined whether returning citizens face a seamless coverage transition or must navigate the full burden of a new Medicaid application at the worst possible moment: whether their state suspends or terminates Medicaid eligibility during incarceration.

The period immediately following release from prison is among the most dangerous in the life course, and the acute-risk motivation applies most directly to coverage in the first year after release. Beyond the well-documented spike in overdose mortality, recently incarcerated individuals face elevated risks of suicide, cardiovascular events, and homicide in the first weeks and months after release (Binswanger et al., 2007). The longer-term horizons (3 and 5 years) test a distinct hypothesis: whether the initial coverage gain from suspension persists through Medicaid's ongoing redetermination cycles, or whether terminated individuals eventually overcome administrative barriers and converge with the suspended group. I return to this persistence question in Section 6.2. More than 80 percent of individuals in state prisons have at least one chronic health condition—including substance use disorders, serious mental illness, hypertension, and hepatitis C—yet the reentry process strips away the limited health infrastructure that the correctional system provides, often without establishing any bridge to community-based care (Mallik-Kane and Visher, 2008). Whether Medicaid coverage is available on the day of release—or only after weeks or months of navigating a bureaucratic application process—has direct consequences for health, well-being, and survival.

The Medicaid Inmate Exclusion Policy (Section 1905(a)(A) of the Social Security Act) prohibits federal reimbursement for health services delivered to inmates of any public institution, including both state and federal facilities. However, because federal inmates are not enrolled in state Medicaid programs, the suspension-termination distinction is operationally relevant only for state and local inmates. The MIEP does not require states to terminate the underlying Medicaid eligibility of incarcerated individuals. This regulatory silence has produced a natural experiment: states have adopted suspension policies—which pause eligibility and automatically reactivate coverage upon release—at different times between 2004 and 2025, creating the staggered variation in treatment timing that I exploit for identification. Termination, by contrast, requires individuals to submit a new application, gather documentation, and wait for a determination—imposing what Herd and Moynihan (2018) term "administrative burden" at a juncture when learning costs, compliance costs, and psychological costs are maximally binding.

Under termination, an individual released from prison must first learn that they are no longer enrolled in Medicaid and that they need to reapply. They must then comply with documentation requirements—providing proof of identity, residence, and income—at a moment when they may lack a stable address, identification documents, and the capacity to manage the simultaneous demands of securing housing, finding employment, and reporting to a parole officer. Suspension eliminates these burdens entirely by preserving the individual's existing eligibility and reactivating coverage automatically upon release.

Understanding the effects of this policy distinction is urgent for several reasons. First, federal law now prohibits states from terminating Medicaid eligibility solely because an individual is an inmate of a public institution, effective January 1, 2026, under Section 205 of the Consolidated Appropriations Act, 2024 (P.L. 118-42; CMS, 2025). The mandate substantially reduces the cross-state variation in suspension policy that made pre-mandate evaluation possible, making historical staggered adoption a uniquely valuable source of evidence — and an implementation benchmark for the states that were formerly termination states or whose suspension implementation remains incomplete. Second, the three states that still terminated Medicaid eligibility as of late 2025 — Alabama, Georgia, and Texas — must operationalize suspension under the new federal floor; the relevant policy question after January 2026 is no longer whether states formally suspend rather than terminate but whether they do so in ways that produce timely reactivation, successful renewals, and connection to care (CMS, 2025; Bandara et al., 2025). Third, the emerging Section 1115 Reentry Demonstration Waivers, which allow Medicaid-covered services in the 30 to 90 days before release, had been approved in 18 states as of mid-2025 (CMS, 2023; KFF, 2025). Understanding how suspension — which addresses the *continuity* of eligibility across the incarceration spell — interacts with these waivers — which address the *scope* of pre-release service coverage — is essential for designing comprehensive reentry coverage strategies. Fourth, the suspension-termination question tests a hypothesis central to the administrative burden literature: whether reducing friction in program access at critical life transitions can produce large improvements in take-up, even when eligibility itself is unchanged.

Despite the policy's importance, the existing quasi-experimental evidence base is thin and concentrated in a single state. Gollu and Zapryanova (2022) use a multi-state cross-sectional research design to estimate that suspension reduces recidivism by 2.9 percentage points at one year and 4.6 percentage points at three years, but examine only recidivism — not the coverage take-up mechanism through which suspension is hypothesized to operate. Packham and Slusky (2024) provide the most direct prior evidence on enrollment: using individual-level criminal justice and Medicaid claims data from South Carolina, they estimate that the state's combined suspension/simplified re-enrollment policy increased Medicaid enrollment within six months after release by 11.1 percentage points. Burns et al. (2022) study Wisconsin's pre-release Medicaid enrollment-assistance program — a related but distinct intervention that simplifies re-enrollment without preserving eligibility — and document large increases in post-release outpatient care use. What the existing literature does not provide is multi-state evidence on suspension's enrollment effect, longer-horizon (3- and 5-year) coverage trajectories, or administratively linked estimates of effects on employment, earnings, mortality, and housing assistance.

This paper fills that gap. I construct a novel state-year panel of suspension and termination policies from 2000 to 2025 by triangulating six independent policy sources (KFF, MACPAC, NACo, Gollu and Zapryanova, and others) and link it to the Census Bureau's Criminal Justice Administrative Records System (CJARS) Justice Outcomes Explorer (JOE), which provides aggregate, administratively linked release-cohort outcomes for state prison systems across 31 states. I implement two-way fixed effects (TWFE), the heterogeneity-robust local projections difference-in-differences (LP-DiD) estimator of Dube et al. (2023), and the Callaway and Sant'Anna (2021) group-time average treatment effect estimator, all of which address the negative weighting bias documented by Goodman-Bacon (2021) for staggered adoption designs.

I make two contributions. First, I provide the first multi-state evidence on the relationship between Medicaid suspension policies and post-release Medicaid enrollment trajectories — at one, three, and five years after release — for the 17 state prison release-cohort panels for which JOE reports Medicaid enrollment outcomes (drawn from a 31-state CJARS policy panel). The estimated intent-to-treat association is large: suspension is associated with an approximately 22 percentage-point increase in 1-year post-release Medicaid enrollment, with similar magnitudes at 3- and 5-year horizons. The estimate is robust across heterogeneity-robust estimators (TWFE, LP-DiD, Callaway-Sant'Anna) and survives a comprehensive battery of robustness checks. This complements — and substantially extends — the prior single-state individual-level evidence from South Carolina (Packham and Slusky, 2024) and the prior Wisconsin enrollment-assistance evidence (Burns et al., 2022). Second, I extend the evidence base beyond enrollment to formal employment, earnings, mortality, and housing assistance, using administrative linkages to W-2 records and the Census Numident; I find no evidence of effects on W-2 employment or earnings, while mortality estimates are exploratory and not robust across estimators. The paper also evaluates the historical pre-mandate period: because Section 205 of the Consolidated Appropriations Act, 2024 took effect on January 1, 2026, this study uses one of the last windows of meaningful staggered cross-state variation before universal adoption.

The remainder of this chapter is organized as follows. Section 2 provides institutional and policy background. Section 3 reviews the relevant prior literature. Section 4 describes the data sources and construction of the analysis sample. Section 5 presents the empirical methods. Section 6 reports the main results, robustness checks, and heterogeneity analyses. Section 7 discusses the findings in context. Section 8 concludes.

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2. Institutional and Policy Background

2.1 The Medicaid Inmate Exclusion Policy

Medicaid is a joint federal-state health insurance program enacted under Title XIX of the Social Security Act in 1965. States administer their own Medicaid programs subject to federal requirements and receive federal matching funds (FMAP, ranging from 50 to roughly 77 percent of state spending depending on per-capita income, plus a 90 percent enhanced match for the ACA expansion population). Eligibility, covered services, and provider payment rates vary substantially across states, but federal statute imposes a small set of structural constraints. One of the most consequential, for the population studied here, is the **Medicaid Inmate Exclusion Policy (MIEP)**.

The MIEP, codified in Section 1905(a)(A) of the Social Security Act since 1965, prohibits federal Medicaid reimbursement for health care services delivered to any "inmate of a public institution," with the narrow exception of inpatient hospital stays exceeding 24 hours (MACPAC, 2018). "Inmate of a public institution" has been interpreted by CMS to include individuals held involuntarily in state and federal prisons, county and local jails, and certain juvenile facilities; it does not include individuals on probation, parole, or home detention. The original rationale was that states and localities, as operators of correctional facilities, bear constitutional responsibility for inmate health care under the Eighth Amendment (*Estelle v. Gamble*, 429 U.S. 97 (1976)), and Congress did not intend for Medicaid to subsidize that obligation.

Critically, the MIEP is best understood as a federal *payment* exclusion rather than an *eligibility* exclusion. Incarceration does not itself make an individual ineligible for Medicaid; federal Medicaid funds simply cannot be used to pay for services provided while the individual is an inmate, except in limited circumstances (CMS, 2025). The statute restricts federal payment for services but does not mandate termination of the underlying Medicaid eligibility of incarcerated individuals. This distinction—between payment and eligibility—is the source of the policy variation I exploit. Because the federal statute is silent on eligibility, states have retained discretion over whether to suspend or terminate an enrollee's Medicaid upon incarceration. For decades, most states chose termination by default: state Medicaid agencies had no financial incentive to maintain eligibility records for individuals who could not generate federal matching funds, and corrections and Medicaid administrative systems were rarely integrated.

The practical consequences of this default were severe. Each year, approximately 600,000 individuals are released from state and federal prisons in the United States, and an additional ~9 million admissions and releases occur in county and local jails (Carson and Kluckow, 2023; Zeng, 2023). Under termination, individuals released from prison were effectively uninsured—regardless of prior enrollment—and had to navigate the full Medicaid application process: identifying the appropriate agency, gathering documentation (proof of identity, residence, citizenship, and income), completing application forms, and waiting 30 to 90 days for an eligibility determination. During this gap, individuals had no coverage for the prescriptions, behavioral health services, and primary care most urgently needed in the immediate post-release period—precisely the window during which the risk of overdose death is more than ten times the general-population rate (Binswanger et al., 2007).

2.2 The Emergence of Suspension Policies

CMS first explicitly authorized suspension as an alternative to termination in a State Medicaid Director Letter issued on May 25, 2004. The letter clarified that states could "suspend rather than terminate" eligibility during incarceration, with coverage reactivated upon release without requiring a new application. **Suspension** in this context means that the individual's Medicaid case is held in an inactive but identified status during incarceration—federal payment is still barred under the MIEP, but the underlying eligibility record is preserved, the case is automatically reactivated on release, and renewals can in principle be processed during the spell. **Termination** means the case is closed entirely; on release the individual must submit a new application, requalify on income and other criteria, supply documentation, and wait through a 30-to-90-day determination window with no coverage in the interim. The two regimes look identical from a federal-payment standpoint while incarcerated; they differ entirely in what happens at the prison door.

Following the 2004 guidance, states adopted suspension in three broad waves. Early adopters (2004–2009) included states with more developed Medicaid managed-care infrastructure or stronger commitments to criminal-justice reform. Mid-period adopters (2010–2015) coincided with the ACA, as expanding states recognized the value of preserving coverage for newly eligible populations. Post-ACA adopters (2016–2025) brought adoption to near-universal levels. As of January 2025, 46 states suspend Medicaid during incarceration, 3 terminate (Alabama, Georgia, Texas), and 2 have unclear status (KFF, 2025).

Implementation has varied across states. Some adopted time-limited suspension (30 days, 90 days, or up to one year, with conversion to termination thereafter), while others adopted indefinite suspension for the full incarceration duration. Scope also varies: some states apply suspension only to state prisons, while others extend it to county and local jails. The operational machinery required to make suspension work in practice is substantial: real-time data exchange between corrections agencies and the state Medicaid office to flag admissions, an automated process to lift the suspension flag on release, and procedures for processing renewals during the incarceration spell. Bandara et al. (2025) document that several de jure suspension states lack one or more of these operational components, producing a meaningful gap between the policy on paper and what an individual experiences at release.

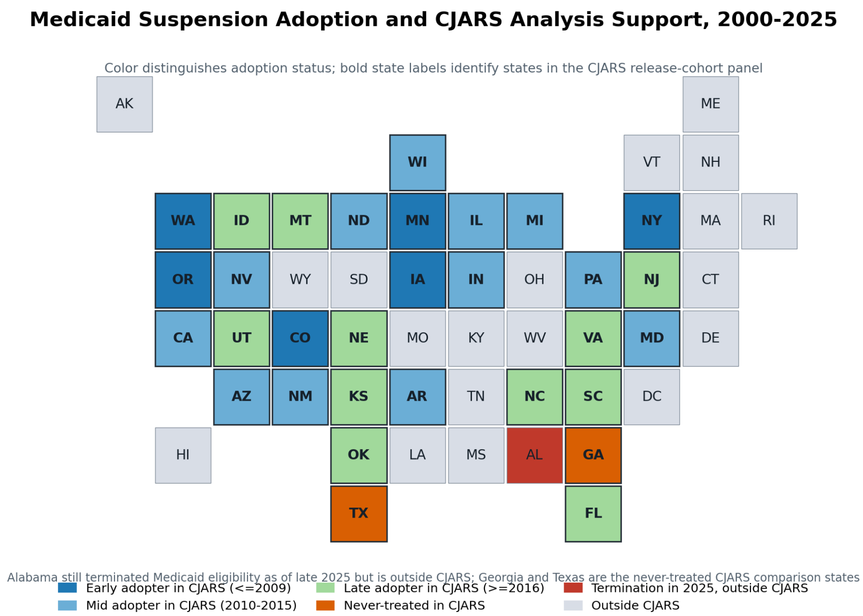

Figure 1: Adoption map

Note: This figure summarizes treatment timing and sample support for the adoption map. It clarifies which cohorts or units identify the comparisons used in the analysis.

2.3 The 2026 Federal Mandate

Section 205 of the Consolidated Appropriations Act, 2024 (P.L. 118-42) expanded the federal prohibition on terminating Medicaid and CHIP eligibility to all inmates of public institutions and, effective January 1, 2026, requires that states must not terminate Medicaid or CHIP eligibility solely because an individual is an inmate of a public institution (CMS, 2025). The mandate reflects bipartisan consensus informed by advocacy from NACo, NCSL, and MACPAC. With the mandate now in effect, the historical cross-state variation that enables this paper's quasi-experimental identification has been substantially reduced. CMS December 2025 guidance emphasizes that states retain choice among different suspension strategies — and that operationalizing the mandate requires data-sharing infrastructure between Medicaid agencies and carceral facilities, mechanisms to identify when a beneficiary becomes an inmate, and coordination on renewals during the incarceration spell (CMS, 2025). The relevant policy question after January 2026 is therefore no longer whether states formally suspend rather than terminate, but whether suspension is operationalized in a way that produces timely reactivation, successful renewals, and connection to care. For the three states that still terminated as of late 2025 (Alabama, Georgia, Texas) and for any state with incomplete de facto implementation, the estimates in this paper provide a benchmark for the coverage gains that pre-mandate state adoption produced.

2.4 The Administrative Burden Framework

The theoretical framework most directly applicable to the suspension-termination distinction is the administrative burden literature. Herd and Moynihan (2018) define administrative burdens as the experience of policy implementation as onerous, decomposing them into three components: (1) learning costs—knowing that programs exist and understanding eligibility requirements; (2) compliance costs—time and effort for paperwork and documentation; and (3) psychological costs—stigma, stress, and the emotional toll of navigating government systems.

Termination imposes all three burden types on reentering individuals at a moment when their capacity to overcome these burdens is at its lowest. Learning costs are high because individuals who have been incarcerated for months or years may be unaware of changes to Medicaid eligibility rules or application procedures. Compliance costs are extreme because documentation requirements—proof of identity, residence, and income—are precisely the things recently released individuals are least likely to have. Psychological costs compound the other burdens: stigma, frustration, and demoralization weigh on individuals simultaneously coping with the social isolation of incarceration.

Suspension eliminates these burdens entirely. There are no learning costs (no action required), no compliance costs (no documentation needed), and minimal psychological costs (no bureaucratic interaction necessary). In a 2025 *Journal of Economic Perspectives* article, Herd and Moynihan update this framework with evidence that administrative burdens disproportionately affect marginalized populations. Recently incarcerated individuals represent perhaps the most extreme case: a population with compounding disadvantages (poverty, chronic illness, histories of trauma, stigmatized status) facing compounding barriers at a moment of acute life transition. The administrative burden framework predicts that friction reduction should produce the largest effects in precisely this context.

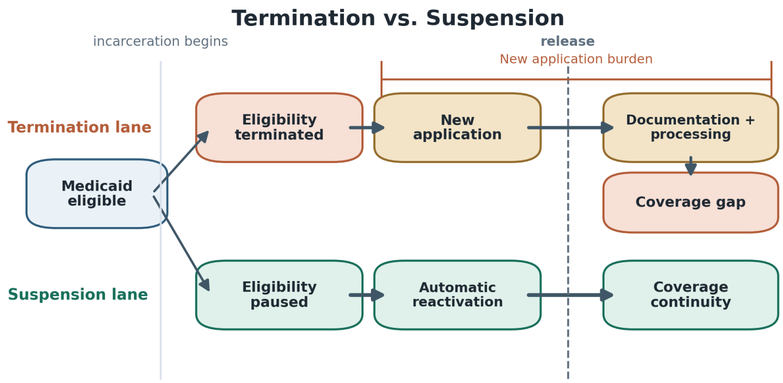

Figure 2: Reentry pathways

Note: This figure provides contextual structure for the reentry pathways. It summarizes the policy setting, mechanism, or empirical workflow used to interpret the estimates.

2.5 ACA Medicaid Expansion and Justice-Involved Populations

The ACA Medicaid expansion to adults earning up to 138 percent of the federal poverty level transformed the coverage landscape for justice-involved populations. Prior to the ACA, traditional Medicaid categories excluded the vast majority of single, non-disabled adults who constitute the bulk of the incarcerated population, leaving approximately 80 percent uninsured after release (Albertson et al., 2020). The expansion made an estimated 80 to 90 percent of justice-involved adults eligible in expansion states (Albertson et al., 2020; Camhi, Mistak, and Wachino, 2020).

The interaction between suspension and expansion is theoretically compelling. Suspension preserves existing eligibility; its value depends on individuals having eligibility to preserve. Before the ACA, many incarcerated individuals were not enrolled at admission, limiting suspension's beneficiary pool. After expansion, a much larger share entered prison with active coverage, suggesting the effect of suspension should be larger in expansion states—a hypothesis I test directly.

Several studies provide context. Aslim et al. (2022) find that expansion reduces recidivism by 11.5 percent, with larger reductions for violent and public-order offenders. A companion welfare analysis (Aslim, Mungan, and Yu, 2024) estimates benefit-cost ratios of $3.45 to $10.62 per dollar spent. Howell et al. (2023) find an 18 percentage-point coverage increase among criminal-justice-involved adults with substance use or mental health disorders but no significant change in treatment utilization—suggesting insurance access alone may be insufficient without complementary measures. Wen et al. (2017) show that expansion was associated with a 70 percent increase in Medicaid-covered buprenorphine prescriptions, with implications for the justice-involved population given elevated rates of opioid use disorder.

2.6 Section 1115 Reentry Demonstration Waivers

The CMS Reentry Section 1115 Demonstration opportunity, introduced in 2023, allows states to provide Medicaid-covered services to incarcerated individuals in the 30 to 90 days prior to release. As of mid-2025, 18 states had received approval, with 9 additional applications pending (KFF, 2025; CMS, 2023). These waivers address a distinct margin: suspension addresses the *continuity* of eligibility across incarceration, while waivers address the *scope* of covered services during the pre-release period. The two policies are complementary: suspension ensures active coverage upon release, while waivers ensure care initiation and provider relationships before release.

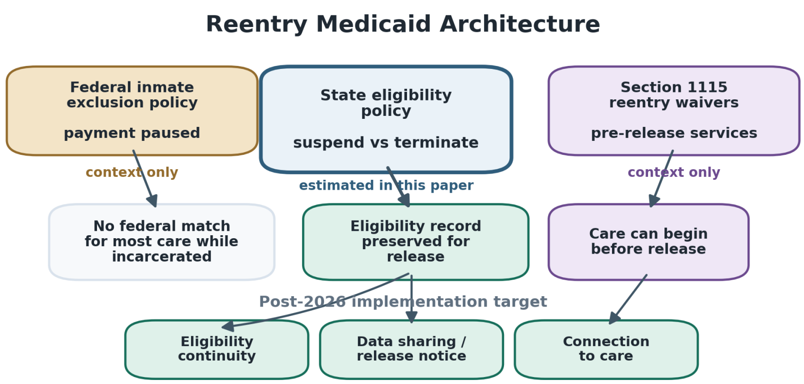

Figure 3: Reentry Medicaid architecture

Note: This figure provides contextual structure for the reentry Medicaid architecture. It summarizes the policy setting, mechanism, or empirical workflow used to interpret the estimates.

2.7 The Reentry Period as a Critical Transition

The reentry period is among the most challenging transitions in the life course. Binswanger et al. (2007) established that individuals released from Washington State prisons were 12.7 times more likely to die in the first two weeks post-release compared to the general population, with drug overdose as the leading cause. Mallik-Kane and Visher (2008) document that more than 80 percent of returning prisoners have chronic physical, mental, or substance abuse conditions. The barriers to health care during reentry are compounding: lack of insurance, lack of a medical home, geographic distance from facilities, inability to pay for medications, lack of transportation, stigma, and competing demands on time and attention. Health insurance does not eliminate all of these barriers, but it is a necessary precondition for accessing the health care system at scale.

On employment, Looney and Turner (2018) show that only 55 percent of formerly incarcerated individuals have any post-release earnings, and median earnings among workers fall below full-time minimum wage. The labor market challenges of returning citizens—employer discrimination, skills atrophy, licensing barriers, parole compliance demands—suggest that health coverage stability, while important, may not be sufficient to produce employment gains without complementary interventions.

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3. Literature Review

3.1 Prior Quasi-Experimental Evidence on Suspension and Reentry Coverage

Three prior quasi-experimental studies are most directly relevant to the present paper, each examining a different margin of the post-release coverage problem.

Gollu and Zapryanova (2022) is the only prior multi-state study of suspension and reentry outcomes. Published in the *Southern Economic Journal* and using individual-level administrative prison release data, the authors estimate that suspension reduces the probability of returning to prison within one year by 2.91 percentage points and within three years by 4.58 percentage points, with larger effects for Black individuals and repeat offenders. Their study has limitations the present paper addresses: it examines only recidivism, without measuring Medicaid enrollment, employment, or mortality — the outcomes representing the causal chain through which suspension is hypothesized to operate; it lacks linkage to Medicaid enrollment files, tax records, or death records; it relies primarily on cross-sectional variation across states rather than fully exploiting within-state adoption timing; and it predates recent advances in staggered-adoption DiD (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021).

Packham and Slusky (2024) provide the most directly relevant prior evidence on the enrollment mechanism. Using individual-level criminal justice records linked to Medicaid claims data from South Carolina, they estimate that the state's combined suspension/simplified re-enrollment policy increased Medicaid enrollment within six months after release by 11.1 percentage points, with corresponding increases in post-release health-care utilization. Their identification rests on the within-state policy change in a single state. The present paper complements that evidence by extending the analysis to multi-state staggered adoption, testing whether the South Carolina-style coverage gain replicates across 27 suspension-adopting states with heterogeneous implementation environments, and by examining 1-, 3-, and 5-year horizons rather than only the first six months.

Burns et al. (2022), in *JAMA Network Open*, study Wisconsin's pre-release Medicaid enrollment-assistance program — a related but distinct intervention that simplifies re-enrollment without preserving eligibility through the incarceration spell. They document large increases in post-release outpatient care use after program implementation. Their evidence speaks to a complementary administrative-friction-reduction mechanism (assisting with reapplication) rather than to suspension itself (preserving eligibility), and it focuses on care utilization rather than enrollment as the primary outcome. Read together with Packham and Slusky, the prior literature establishes that reducing administrative friction at reentry can substantially raise post-release Medicaid take-up and care utilization. The contribution of the present paper is to test whether the coverage-take-up channel generalizes outside South Carolina, persists at longer horizons, and translates into measurable changes in employment, earnings, mortality, and housing assistance.

3.2 Medicaid Coverage Continuity and Churning

The Medicaid "churning" literature provides theoretical grounding for why suspension should matter. The Office of the Assistant Secretary for Planning and Evaluation (2021) documents that typical beneficiaries are covered for fewer than 10 months per year and that nearly 25 percent experience a coverage change within a calendar year. Churning is associated with disruptions in physician care, reduced medication adherence, increased emergency department use, and worse health.

Most Medicaid disenrollment occurs not because individuals become ineligible but because they fail to complete redetermination—a form of administrative burden (Herd and Moynihan, 2018). States that have adopted continuous eligibility policies have seen substantial reductions in coverage disruptions, suggesting administrative friction rather than eligibility changes is the primary driver of coverage gaps.

The analogy to suspension-termination is direct: termination creates involuntary disenrollment that forces individuals to navigate the full enrollment process, while suspension functions as continuous eligibility across the incarceration spell. Goldman and Sommers (2020) provide evidence that ACA Medicaid expansion decreased coverage disruptions, with an estimated half a million fewer adults experiencing churning annually. This suggests that expansion both increased the number of individuals with coverage and improved coverage stability, potentially amplifying the value of suspension by increasing the pool of individuals with coverage to preserve. If suspension functions as a coverage-continuity mechanism analogous to continuous-eligibility policies, the hypothesis that it reduces post-release coverage gaps is well-grounded.

3.3 Medicaid and Mortality

The relationship between Medicaid coverage and mortality is one of the most debated questions in health economics. The Oregon Health Insurance Experiment found improvements in financial security, self-reported health, and depression but no statistically significant mortality effects, though the study was underpowered for mortality detection (Finkelstein et al., 2012; Baicker et al., 2013).

Quasi-experimental studies have found more consistent effects. Sommers, Baicker, and Epstein (2012) find that pre-ACA state expansions were associated with a 6.1 percent relative reduction in all-cause mortality. Borgschulte and Vogler (2020) estimate that ACA expansion reduced mortality by 3.6 percent. For justice-involved populations, the relationship may be particularly strong given the extreme post-release mortality risk: if suspension expedites Medicaid coverage and thereby access to medication-assisted treatment and mental health services, it could reduce the acute mortality spike. However, the causal chain from coverage to mortality is long and potentially attenuated.

3.4 Post-Release Health Outcomes and Reentry Barriers

Beyond the immediate mortality risk, returning citizens face elevated rates of emergency department utilization, hospitalization, and death from treatable conditions. The barriers to care during reentry are multifaceted: lack of insurance, lack of established provider relationships, geographic barriers, competing time demands, limited health literacy, and stigma from health care providers (Mallik-Kane and Visher, 2008). Mental health conditions and substance use disorders affect the ability to secure employment, stable housing, and family relationships. In this context, health insurance is not merely a health intervention but a stabilizing force supporting success across multiple reentry domains.

3.5 Insurance Loss, Crime, and Administrative Burden

Deza et al. (2024) study a mass Medicaid disenrollment in Tennessee in 2005 that removed coverage from 190,000 adults, finding significant increases in total crime rates, particularly non-violent crime. This provides mirror-image evidence for the suspension question: if losing coverage increases crime, then policies preserving coverage should reduce post-release criminal behavior.

The administrative burden literature extends well beyond Medicaid and incarceration. Studies of SNAP, housing assistance, tax credits, and other programs consistently find that simplifying enrollment produces meaningful increases in participation (Herd and Moynihan, 2025). In the Medicaid context, automatic and presumptive eligibility policies, ex parte renewals, and simplified application forms have all been associated with increased enrollment and reduced coverage gaps (Herd and Moynihan, 2025; Office of the Assistant Secretary for Planning and Evaluation, 2021). The Medicaid unwinding following the end of the COVID-19 continuous enrollment provision demonstrated the consequences of restoring administrative requirements: millions lost coverage when states resumed redetermination, even when many remained eligible (KFF, 2025).

My study examines an extreme case of administrative burden reduction in a population facing extreme disadvantage. Suspension does not merely simplify the application—it eliminates it entirely. The magnitude of the effect I estimate is consistent with the administrative burden framework's prediction that friction reduction should produce the largest effects when the target population has the fewest resources to overcome barriers.

3.6 Econometric Methods for Staggered Adoption

I follow the heterogeneity-robust staggered-DiD literature, using LP-DiD (Dube et al., 2023) as my preferred specification and Callaway and Sant'Anna (2021) as the primary cross-check, with TWFE retained only as a benchmark and diagnosed via the Goodman-Bacon (2021) decomposition. The heterogeneous-effects analysis interacting suspension with ACA expansion follows the triple-difference framework of Olden and Møen (2022) and Ortiz-Villavicencio and Sant'Anna (2025).

3.7 Gap in the Literature

Read together, the review reveals a clear set of gaps the present paper addresses:

1. **Multi-state evidence on enrollment.** Packham and Slusky (2024) document a large enrollment effect of suspension/simplified re-enrollment in South Carolina; Burns et al. (2022) document large utilization effects of Wisconsin's pre-release enrollment-assistance program. No prior study evaluates whether the coverage-take-up channel from suspension generalizes across multiple states with heterogeneous implementation environments.

2. **Longer-horizon coverage trajectories.** Packham and Slusky (2024) examine enrollment within six months of release; Gollu and Zapryanova (2022) examine 1- and 3-year recidivism. No prior study traces post-release Medicaid enrollment trajectories at 1-, 3-, and 5-year horizons across multiple states.

3. **Linked administrative outcomes beyond enrollment.** No prior study uses CJARS — linking criminal justice records to W-2 earnings and the Census Numident — to estimate effects on formal employment, earnings, mortality, and housing assistance for state prison release cohorts at the multi-state level.

4. **Heterogeneity by ACA expansion.** No prior study has formally examined whether the suspension effect is amplified by ACA Medicaid expansion — a theoretically important interaction because suspension's value depends on the size of the pre-incarceration enrolled population.

5. **Modern staggered-DiD methods.** No prior study applies LP-DiD (Dube et al., 2023), Callaway-Sant'Anna (2021), or related heterogeneity-robust estimators to this question.
6. **Pre-mandate evidentiary record.** Section 205 of the Consolidated Appropriations Act, 2024 took effect on January 1, 2026, substantially reducing the cross-state variation that enables quasi-experimental identification of suspension policy. The pre-mandate window analyzed here represents one of the last periods in which this variation can be exploited.

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4. Data

4.1 Overview

I combine five data sources: (1) the CJARS Justice Outcomes Explorer (JOE) Social Safety Net module for post-release Medicaid enrollment, employment, and mortality outcomes; (2) the JOE CJARS-based module for release cohort counts and demographic composition; (3) a novel state-year suspension/termination policy panel compiled from multiple policy trackers; (4) state-level ACA Medicaid expansion dates from KFF; and (5) state-level control variables from federal statistical agencies. All data sources are publicly available at no cost.

4.2 CJARS Justice Outcomes Explorer

The Criminal Justice Administrative Records System (CJARS) is a joint project of the U.S. Census Bureau and the University of Michigan that collects, harmonizes, and links criminal justice administrative records to Census Bureau survey and administrative data, including tax filings (W-2), federal program participation records, and mortality data from the Census Numident (Finlay, Mueller-Smith, and Papp, 2022). CJARS represents the most comprehensive effort to date to link criminal justice records to economic and health outcomes at the individual level.

The Justice Outcomes Explorer (JOE) is a public data product derived from CJARS that reports aggregated outcomes for justice-involved individuals by state, release cohort year, and post-release time horizon, available at joe.cjars.org/data. The JOE Social Safety Net module provides my primary outcome variables:

- **Medicaid enrollment rate**: Share of individuals enrolled at the specified post-release horizon (1-, 3-, or 5-year), measured through linkage to MSIS/T-MSIS enrollment records, available for cohorts released between 2005 and 2018.
- **W-2 employment rate**: Share with any W-2 earnings at the specified horizon. This does not capture self-employment, informal employment, or off-the-books work.

- **Mean W-2 earnings**: Mean of W-2 earnings including individuals with zero earnings, reflecting both the extensive and intensive margins.
- **SSI receipt rate**: Share receiving Supplemental Security Income, measured through SSA linkage. Available for a more limited set of state-years.
- **HUD housing assistance rate**: Share receiving HUD housing assistance (public housing, Housing Choice Vouchers, or project-based rental assistance).
- **All-cause mortality rate**: Share deceased by the specified horizon, measured through the Census Numident file.

Each outcome is measured at 1-, 3-, and 5-year post-release horizons for state prison release cohorts from 2000 to 2020. A key feature of JOE is that it provides state-level aggregates that can be linked to policy variables without requiring restricted microdata access.

4.3 Sample Construction

I construct the analysis sample through several steps. First, I download the JOE Social Safety Net and CJARS-based modules from joe.cjars.org/data. Second, I merge the JOE data with my state-year suspension policy panel, ACA expansion status panel, and state-level control variables by state FIPS code and year. Third, I impose sample restrictions:

- I exclude state-year cells with fewer than 25 individuals to satisfy Census Bureau disclosure requirements and ensure statistical precision. JOE suppresses cells below this threshold, so in practice this restriction is applied by the data provider.
- I restrict to state prison releases (excluding jail releases) because the CJARS data primarily track state prison systems and because my policy panel is most reliable for state prison policies. Some states apply suspension only to prisons, not jails, and jail release timing is often unpredictable.
- For the primary Medicaid enrollment analysis, I restrict to cohorts released between 2005 and 2018, reflecting the MSIS/T-MSIS data window in CJARS. For employment and mortality analyses, I use the full available range (2000–2020).

The resulting *policy panel* covers 561 state-year observations across 31 CJARS states for release cohorts 2000–2020. The *outcome panels* are narrower because JOE only reports a state's release-cohort outcomes when the underlying Census Bureau linkage is built for that state. In practice this means the same set of 17 states identifies every outcome: Medicaid enrollment (212 observations, cohorts 2005–2018, bounded by the MSIS/T-MSIS linkage window); W-2 employment and earnings (212 observations, same window); mortality and HUD housing assistance (264 observations, available back to 2000 from the Census Numident);

SSI receipt (99 observations, narrowest); and recidivism (the only outcome with a broader 22-state footprint). The 14 CJARS states with no JOE outcome rows for the variables in this paper—AR, CA, GA, IA, ID, MD, MI, ND, NJ, NM, NV, OR, SC, VA—are excluded from the outcome regressions; among them, Georgia is one of the two never-treated states in the policy panel and South Carolina is the state studied directly by Packham and Slusky (2024). Because mortality, employment, and earnings draw on the same 17-state footprint as Medicaid, the analysis is best understood as a 17-state design throughout, with the broader 31-state policy panel providing context for representativeness rather than identifying variation.

Representativeness of the 17-state outcome sample. The 17 states identifying the outcome regressions are AZ, CO, FL, IL, IN, KS, MN, MT, NC, NE, NY, OK, PA, TX, UT, WA, and WI. They include 16 of the 27 CJARS suspension adopters and one of the two never-treated states (Texas; Georgia is in the 31-state policy panel but absent from the JOE outcome panels for these outcomes). Average state characteristics in the 17-state sample versus the 14 CJARS states with no JOE outcome rows are similar on observable dimensions: pre-treatment unemployment (5.4 percent vs. 5.8 percent), age-adjusted overdose mortality (14.2 vs. 13.5 per 100,000), and poverty rates (12.7 percent vs. 13.3 percent). The 17-state sample is therefore not visibly selected on baseline economic conditions or overdose intensity. What it does select on, mechanically, is the existence of a built-out Census Bureau linkage, which correlates with state-level data infrastructure and CJARS partnership rather than with policy or population fundamentals; this is a representativeness caveat I revisit in §7.6.

The 20 non-CJARS states—including Alabama, the third termination state—are excluded entirely due to the absence of data-sharing agreements with the Census Bureau. Where the 17-state outcome sample binds the interpretation, I note it explicitly.

Estimand: intent-to-treat at the release-cohort level. Because JOE reports aggregate outcomes for all individuals released from state prison in a given state-year, the estimates in this paper are best interpreted as intent-to-treat (ITT) effects of state suspension policy on the average Medicaid enrollment rate among the full release cohort, not as individual-level treatment effects on those whose Medicaid was actually suspended. The ITT estimand is the policy-relevant one — what changes when a state moves from termination to suspension, averaged across all release cohort members — but it differs in interpretation from the individual-level effect estimated in single-state administrative-claims studies (Packham and Slusky, 2024). I cannot observe whether a given individual was enrolled in Medicaid before incarceration, whether their eligibility was actually suspended (rather than allowed to lapse), or whether coverage was reactivated automatically upon release. If only a subset of release cohort members had Medicaid eligibility to preserve at the time of incarceration, the implied effect on those individuals could be larger than the cohort-average ITT effect; if implementation gaps (Bandara et al., 2025) attenuate the de jure–de facto relationship, the cohort-average ITT effect understates the effect of fully-implemented suspension. Section 7.6 returns to these interpretive limits.

Medicaid outcome window definition. The Medicaid enrollment rate at horizon $h \in \{1, 3, 5\}$ is JOE's measure of the share of release-cohort members enrolled in Medicaid in the month at horizon h following release, derived from MSIS/T-MSIS enrollment records linked at the individual level by CJARS and aggregated to the state-year-cohort cell. The 1-year measure therefore captures point-in-time enrollment one year post-release, not "any enrollment within the first year"; the 3- and 5-year measures are the analogous point-in-time enrollment rates at three and five years post-release for the same cohort. This horizon-specific point-in-time measure is the JOE-published variable used by other studies of CJARS outcomes (Finlay, Mueller-Smith, and Papp, 2022) and is the only Medicaid-enrollment measure available without restricted-microdata access.

4.4 State Suspension/Termination Policy Panel

The most original data contribution of this study is a comprehensive state-year panel classifying each state's Medicaid eligibility policy for incarcerated individuals from 2000 to 2025. I classify each state-year as termination, time-limited suspension, or indefinite suspension by triangulating six independent sources:

1. **KFF State Health Facts** (2019 and 2025 snapshots)
2. **MACPAC Reports to Congress** (2018 and 2023 chapters)
3. **NACo (2015) Suspension/Termination Brief**, which reported that 38 states still terminated at the time
4. **Gollu and Zapryanova (2022) Appendix**
5. **KFF (2024) State Medicaid Eligibility Issue Brief**
6. **Bandara et al. (2025)**, providing qualitative implementation details

When sources disagree, I apply hierarchical prioritization: (1) state legislation, (2) KFF, (3) MACPAC, (4) peer-reviewed research. I assign each state a coding confidence rating; only one state has a low-confidence rating, and sensitivity analyses dropping it produce virtually identical results.

4.5 ACA Expansion Panel and State Controls

I code ACA expansion status from KFF. State-level controls include the unemployment rate (BLS LAUS), age-adjusted drug overdose mortality rate (CDC WONDER), and poverty rate (Census SAIPE). These are included in robustness specifications but not the primary model, following the convention that the main DiD estimate should capture the full policy effect.

4.6 Summary Statistics

The policy panel comprises 561 state-year observations across 31 CJARS states. Twenty-nine states adopted suspension during the sample period (27 in CJARS), with two never-treated states (Georgia, Texas). Within the 17-state outcome panel, Texas is the binding never-treated comparator; Georgia is in the policy panel but absent from the JOE outcome rows used for the regressions in this paper.

Table 1. Summary Statistics

	N	Mean	SD	Min	Median	Max
Panel A. Primary outcomes, 1-year horizon						
Medicaid enrollment	212	0.320	0.251	0.078	0.200	0.880
W-2 employment	212	0.536	0.074	0.350	0.540	0.680
W-2 earnings ($)	212	6,909	1,634	3,060	6,905	11,600
Mortality	264	0.017	0.005	0.006	0.016	0.048
Panel B. Medicaid at longer horizons						
Medicaid enrollment (3-yr)	207	0.287	0.187	0.091	0.210	0.800
Medicaid enrollment (5-yr)	201	0.277	0.166	0.093	0.210	0.740
Panel C. Cohort and state characteristics						
Incarceration spell length (mos.)	378	25.1	6.6	11.5	24.5	41.7
Unemployment rate (%)	561	5.58	2.13	2.20	5.00	13.70
Overdose mortality rate	554	13.9	6.5	2.6	12.7	44.3

	N	Mean	SD	Min	Median	Max
Poverty rate (%)	534	13.0	2.9	6.9	12.7	21.4

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.

Notes: Unit of observation is state-year. Medicaid, employment, and mortality outcomes are shares in [0, 1]. Earnings are reported in nominal dollars. Overdose mortality rate is CDC WONDER age-adjusted deaths per 100,000. The policy panel covers 31 CJARS states over 2000–2020; the JOE outcome panels (Medicaid, employment, earnings, mortality, HUD) are restricted to the same 17 reporting states, with Medicaid bounded further to cohorts 2005–2018 by the MSIS/T-MSIS linkage window.

Table 1 reports descriptive statistics for all variables in the analysis sample. The mean 1-year Medicaid enrollment rate is 32.0 percent (SD = 25.1%), reflecting substantial cross-state and temporal variation. This high variance is driven by the combination of pre-suspension states with low enrollment (many termination-era observations have enrollment below 15%) and post-suspension states with high enrollment (many post-adoption observations exceed 50%). Mean 1-year W-2 employment is 53.6 percent, mean W-2 earnings are $6,909, and mean 1-year mortality is 1.7 percent. Mean incarceration spell length is 25.1 months (approximately two years), reflecting the state prison population. Among secondary outcomes, 3-year and 5-year Medicaid enrollment rates are 28.7 percent and 27.7 percent, slightly below the 1-year rate, consistent with some coverage attrition over longer horizons. The mean state unemployment rate is 5.6 percent, mean overdose mortality rate is 13.9 per 100,000, and mean poverty rate is 13.0 percent.

Table 2. Pre-Adoption Balance, Adopting vs. Never-Treated States

	Adopting states	Non-adopting states	Difference	Norm. diff.
	Mean (SD)	Mean (SD)		
Panel A. Primary outcomes (pre-treatment)				
Medicaid enrollment (1-yr)	0.210 (0.109)	0.144 (0.041)	0.066	0.80
W-2 employment (1-yr)	0.552 (0.079)	0.513 (0.063)	0.039	0.55
W-2 earnings (1-yr, $)	6,814 (1,822)	6,867 (1,432)	−53	−0.03
Mortality (1-yr)	0.016 (0.005)	0.017 (0.004)	−0.001	−0.17
Panel B. Release-cohort characteristics				
Annual release cohort size	10,096 (7,002)	34,644 (23,043)	−24,549	−1.44
Share male	0.879 (0.027)	0.864 (0.022)	0.015	0.62
Share Black	0.302 (0.156)	0.388 (0.078)	−0.086	−0.70
Share Hispanic	0.149 (0.112)	0.133 (0.107)	0.015	0.14
Share young (15–24)	0.197 (0.050)	0.181 (0.022)	0.016	0.42
Incarceration spell (months)	22.6 (5.8)	29.9 (3.5)	−7.3	−1.53
Panel C. State-year controls				
Poverty rate (%)	12.5 (3.0)	15.3 (1.9)	−2.8	−1.10
Unemployment rate (%)	5.69 (2.23)	5.60 (1.93)	0.10	0.05
Overdose mortality rate	11.8 (5.0)	13.3 (5.6)	−1.5	−0.28

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.

Notes: Means (standard deviations) across pre-adoption state-year observations. “Adopting states” comprises all 27 CJARS states that adopted suspension during 2005–2022 observed in their pre-treatment years; “non-adopting states” comprises Georgia and Texas — the two never-treated states in the CJARS sample — across all years. Normalized difference is the mean difference divided by the pooled standard deviation. Release-cohort demographic shares are computed using the JOE release-cohort cohort-size variable (cohort size at the 5-year W-2 horizon) as the denominator within sex/race/age subgroup cells. Level differences are absorbed by state fixed effects in the main DiD design; the identifying assumption requires parallel trends rather than level equivalence.

The pre-adoption balance table (Table 2) compares adopting states (in pre-treatment years) to non-adopting states (Georgia and Texas across all years). Several differences emerge. Adopting states have higher pre-treatment Medicaid enrollment (21.0% vs. 14.4%), consistent with the possibility that states with larger Medicaid-enrolled populations were more likely to adopt suspension. Adopting states have shorter mean incarceration spells (22.6 vs. 29.9 months) and lower poverty rates (12.5% vs. 15.3%), potentially reflecting differences in sentencing practices and state fiscal capacity. The two never-treated states (Georgia and Texas) have substantially larger annual release cohorts than the average adopting state (~34,600 vs. ~10,100), reflecting both larger state populations and historically higher incarceration rates. Release cohort sex composition is similar across groups (~87% male in both), while non-adopters have a higher Black share of the release cohort (38.8% vs. 30.2%), reflecting the demographic composition of Texas and Georgia prison systems. These baseline differences are absorbed by state fixed effects in my DiD design, which controls for all time-invariant differences between states. The identifying assumption requires parallel trends, not level equivalence, which I assess through event study analyses in Section 6.

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5. Methods

5.1 Identification Strategy

I exploit the staggered adoption of Medicaid suspension policies across states between 2005 and 2022 to estimate the quasi-experimental association between suspension and post-release outcomes. The identifying variation comes from within-state, over-time changes in policy status, separated from time-invariant state characteristics (state fixed effects) and common temporal shocks (year fixed effects).

I implement two complementary strategies. First, a staggered DiD design estimating the average treatment effect of suspension adoption. Second, a heterogeneous DiD testing whether effects are amplified in ACA expansion states. The key identifying assumption is parallel trends: absent suspension adoption, treated states would have followed the same outcome trend as untreated states. I assess plausibility through event study analyses and pre-treatment coefficient tests.

5.2 Staggered Difference-in-Differences

TWFE Benchmark. The standard two-way fixed effects model is:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{PostSuspension}_{it} + \mathbf{X}_{it}'\delta + \varepsilon_{it}$$

where Y_{it} is the outcome for state i in release cohort year t, α_i and γ_t are state and year fixed effects, $\text{PostSuspension}_{it}$ is binary for post-adoption years, and $\mathbf{X}_{it}$ is a vector of time-varying controls (in robustness specifications). Standard errors are clustered at the state level (Bertrand, Duflo, and Mullainathan, 2004). State fixed effects absorb all time-invariant differences (Medicaid generosity, incarceration practices, demographics, political orientation); year fixed effects absorb common temporal shocks (national economic cycles, federal policy changes, secular trends).

I report TWFE as a benchmark, noting that with heterogeneous treatment effects and staggered adoption, TWFE may be biased due to negative weighting (Goodman-Bacon, 2021). I diagnose severity through the Goodman-Bacon decomposition (Appendix Figure A10).

LP-DiD (Primary). My preferred specification uses LP-DiD (Dube et al., 2023), which is heterogeneity-robust and uses only never-treated and not-yet-treated states as comparators at each horizon. The Callaway and Sant'Anna (2021) group-time ATT estimator serves as the primary cross-check; the two estimators address the negative-weighting bias documented by Goodman-Bacon (2021) through different routes, and I report agreement across them as the principal evidentiary basis.

Inference under sparse never-treated support. Because Texas is the only never-treated state with non-missing Medicaid outcome rows in the JOE panel (Georgia, the second never-treated state in the 31-state policy panel, has no JOE Medicaid rows for these cohorts), the cross-estimator pattern combined with LP-DiD's use of not-yet-treated comparators — not any single estimator — is the load-bearing piece of the design. Cluster-robust standard errors, wild cluster bootstrap (B = 999, Rademacher weights), and design-based randomization inference (500 permutations) provide a graded inferential hierarchy: the bootstrap and parametric SEs reject the null at conventional thresholds for Medicaid enrollment, while RI is conservative in this design because the number of admissible treatment-timing permutations is limited. I treat RI as a conservative stress test rather than as the primary inferential criterion, and I disclose this trade-off explicitly rather than relying on the more permissive parametric inference alone.

5.3 Event Study Specification

I estimate event study models to assess parallel trends and trace dynamic effects:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{k \neq -1} \beta_k \cdot \mathbf{1}[t - E_i = k] + \varepsilon_{it}$$

where E_i is the adoption year and the reference period is $k = -1$. Pre-treatment coefficients ($k < -1$) test the parallel trends assumption; post-treatment coefficients ($k \geq 0$) trace dynamic effects. I present both TWFE and LP-DiD event studies, noting differences when they arise.

5.4 Heterogeneous DiD by ACA Expansion

I interact the suspension indicator with ACA expansion status:

$$Y_{it} = \alpha_i + \gamma_t + \beta_1 \cdot \text{PostSuspension}_{it} + \beta_2 \cdot (\text{PostSuspension}_{it} \times \text{Expanded}_i) + \varepsilon_{it}$$

The coefficient β_1 captures the suspension effect in non-expansion states; $\beta_1 + \beta_2$ captures the total effect in expansion states; β_2 captures the differential effect. I supplement with a period-split approach estimating TWFE separately for pre-ACA (2000–2013) and post-ACA (2014–2020) periods.

5.5 Inference

All specifications cluster standard errors at the state level. To complement parametric inference, I report (i) the wild cluster bootstrap (B = 999, Rademacher weights, state-clustered) and (ii) randomization inference (500 permutations of adoption timing across states, holding the adoption pattern fixed). RI is conservative in this setting because the Medicaid outcome panel includes only one never-treated state with non-missing rows (Texas), which limits the number of admissible permutations; I therefore treat RI as a design-based stress test rather than the primary inferential criterion, and report it transparently alongside the cross-estimator agreement that constitutes the strongest evidence.

5.6 Robustness and Sensitivity Analyses

I conduct a comprehensive set of robustness and sensitivity checks:

1. **Pre-trends tests**: I examine pre-treatment coefficients in event study specifications, testing whether they are individually and jointly indistinguishable from zero.
2. **Placebo tests with randomized adoption timing**: The randomization inference procedure provides a placebo test, comparing the actual estimate to the distribution under randomly permuted treatment assignments.
3. **Leave-one-out analysis**: I re-estimate 31 times, each time dropping one state, to assess whether any single state drives the result.
4. **Policy coding sensitivity**: I re-estimate after dropping the one state with a low-confidence policy coding.

5. **Alternative cell size thresholds**: I use a minimum cell size of 50 (rather than 25) to assess sensitivity to smaller, noisier cells.
6. **Horizon sensitivity**: I compare estimates across 1-, 3-, and 5-year post-release horizons.
7. **Goodman-Bacon decomposition**: I decompose the TWFE estimate into constituent two-by-two comparisons to diagnose negative weighting.
8. **Controls sensitivity**: I compare uncontrolled TWFE estimates to those with time-varying controls (unemployment rate, overdose mortality rate, poverty rate).

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6. Results

6.1 Main Results: Medicaid Enrollment

Table 3. Main Results: Effect of Medicaid Suspension on Post-Release Outcomes, 1-Year Horizon

	Medicaid	Medicaid (+ controls)	Employment	Earnings	Mortality
	(1)	(2)	(3)	(4)	(5)
Suspension (post-adoption)	0.222***	0.230***	0.016	315.3	0.0005
	(0.051)	(0.041)	(0.011)	(272.3)	(0.0011)
LP-DiD estimate (same outcome)	0.224***	—	0.003	383.3	−0.0017**
	(0.018)		(0.029)	(339.2)	(0.0006)
Callaway-Sant'Anna estimate	0.258***	—	—	—	0.0001
	(0.046)				(0.0015)
Cohort-size-weighted TWFE	0.257***	—	0.023	351.4	0.0020*
	(0.062)		(0.020)	(368.5)	(0.0012)
Wild cluster bootstrap p	<0.001	<0.001	0.135	0.285	0.667
State fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	No	No
Observations	212	198	212	212	264
States	17	17	17	17	31

	Medicaid	Medicaid (+ controls)	Employment	Earnings	Mortality
R^2 (within)	0.214	0.268	0.034	0.021	0.003

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.

Notes: Cluster-robust standard errors (clustered at the state level) in parentheses. Each column is an outcome-specific regression of the stated dependent variable on the post-suspension indicator with state and year fixed effects. Rows 1–2 report the TWFE benchmark; rows 3–8 report alternative estimators for the same outcome. Column (2) adds time-varying controls (state unemployment rate, age-adjusted overdose mortality rate, and poverty rate). The LP-DiD (Dube et al., 2023) estimator uses only never-treated and not-yet-treated states as comparators at each horizon. The Callaway-Sant'Anna (2021) estimator reports the aggregated group-time average treatment effect using not-yet-treated as the control group. The cohort-size-weighted TWFE reweights by the JOE release-cohort cohort-size variable for each outcome. Wild cluster bootstrap p-values use Rademacher weights, B = 999, with state-level clustering. Medicaid and employment outcomes are shares; earnings are in nominal dollars; mortality is a share (annual deaths per 100 cohort members). The Medicaid, employment, and earnings outcomes are drawn from the same 17-state JOE outcome panel, restricted to cohorts 2005–2018 by the MSIS/T-MSIS linkage window; mortality and HUD outcomes are drawn from the same 17 states with a broader 2000–2020 window. Sample sizes therefore differ across columns (e.g., N=212 for Medicaid, N=264 for mortality) because of within-state year coverage rather than because more states identify mortality. *p<0.10, **p<0.05, ***p<0.01.

Table 3 presents the main results. Suspension is associated with a 22.2 percentage-point increase in 1-year post-release Medicaid enrollment under TWFE (SE = 0.051, $p < 0.001$), 22.4 pp under LP-DiD (SE = 0.018, p = 0.001), and 25.8 pp under the Callaway-Sant'Anna group-time average treatment effect estimator (SE = 0.046, $p < 0.001$). Adding time-varying controls to TWFE increases the estimate slightly to 23.0 pp ($p < 0.001$). The close agreement across three heterogeneity-robust estimators — including two (LP-DiD and Callaway-Sant'Anna) that are robust to the negative-weighting bias documented by Goodman-Bacon (2021) — indicates that the result is not estimator-specific. Wild cluster bootstrap p-values, which adjust for the small number of clusters (17 states with non-missing Medicaid outcomes, of which only Texas is never-treated), continue to reject the null at conventional thresholds for Medicaid enrollment ($p < 0.001$). Reweighting by the JOE release-cohort cohort-size variable produces a slightly larger estimate (25.7 pp), suggesting the population-weighted effect is at least as large as the unweighted state-average ITT effect.

The magnitude is large by any standard. Relative to the sample mean enrollment rate of 32.0 percent, the 22 pp increase represents a 69 percent gain. Among pre-treatment non-adopting states, mean enrollment was only 14.4 percent; relative to that 14.4 percent baseline, the 22 pp estimate implies an increase of roughly 1.5 times the baseline level. This magnitude is consistent with the administrative burden interpretation: eliminating the requirement to file a new Medicaid application at a moment of acute instability dramatically increases take-up.

Comparison with prior single-state evidence. Table 4 places the present estimates alongside prior quasi-experimental evidence. Packham and Slusky (2024), using individual-level South Carolina criminal-justice and Medicaid claims data, estimate that the state's combined suspension/simplified re-enrollment policy increased Medicaid enrollment within six months of release by 11.1 percentage points. The present estimate of 22.4 percentage points at one year — roughly twice as large — is plausible given two design differences: (i) my outcome window is one year rather than six months, allowing more time for the suspension regime's reactivation channel to manifest; and (ii) my estimand is the state-cohort-average ITT effect across 27 adopting states with heterogeneous implementation environments, while Packham and Slusky's estimate reflects South Carolina's specific implementation. Burns et al. (2022), studying Wisconsin's pre-release Medicaid enrollment-assistance program, document large post-release outpatient care use increases — speaking to the same administrative-friction-reduction mechanism but on a different outcome (utilization) and through a different intervention (re-enrollment assistance rather than eligibility preservation). Read together, the evidence base now spans three states (South Carolina, Wisconsin, and 27 multi-state staggered adopters) and three administrative-friction-reduction interventions (suspension/simplified re-enrollment, pre-release enrollment assistance, eligibility-preserving suspension), with consistent evidence that reducing reentry administrative friction substantially raises post-release Medicaid take-up and care utilization.

Table 4. Comparison with Prior Quasi-Experimental Evidence

Study	Setting	Intervention	Design	Outcome	Estimate
Packham & Slusky (2024)	South Carolina (one state)	Suspension + simplified re-enrollment	RD/event-study, individual-level Medicaid claims	Enrollment within 6 months post-release	+11.1 pp
Burns et al. (2022)	Wisconsin (one state)	Pre-release Medicaid enrollment-assistance program	Pre/post within enrolled cohort	Post-release outpatient care use	Large positive effect on utilization
Gollu & Zapryanova (2022)	Multi-state	Suspension vs. termination (cross-sectional policy variation)	Individual-level recidivism, OLS with controls	Recidivism within 1 / 3 years	-2.9 pp / -4.6 pp

Study	Setting	Intervention	Design	Outcome	Estimate
This paper	17-state JOE outcome panel (within 31-state CJARS policy panel), staggered adoption 2005–2022	Suspension (eligibility preservation)	Staggered DiD (TWFE / LP-DiD / CS), state-year aggregates	Medicaid enrollment at 1 / 3 / 5 years	**+22.4 / +18.7 / +20.7 pp**

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.

To contextualize this effect size in the broader Medicaid take-up literature: ACA Medicaid expansion increased coverage among eligible adults by approximately 10 to 15 percentage points in the typical expansion state (Sommers et al., 2012). Ex parte renewals and related renewal-simplification policies have also been associated with meaningful reductions in churn-related disenrollment (Herd and Moynihan, 2025; Office of the Assistant Secretary for Planning and Evaluation, 2021). The 22 pp effect of suspension exceeds the typical coverage gains reported for ACA expansion and is also larger than the churn reductions generally observed under renewal-simplification policies — plausible given that the target population faces substantially greater barriers to enrollment than the typical Medicaid-eligible adult and that suspension represents a more complete elimination of administrative burden (automatic reactivation with no action required by the enrollee) than simplified application or renewal processes.

The close agreement between TWFE and LP-DiD is noteworthy. The Goodman-Bacon decomposition (Appendix Figure A10) confirms that problematic "already-treated vs. later-treated" comparisons receive relatively small weight, and their estimates are positive but smaller than the clean comparisons. This explains why the TWFE bias is modest: the problematic comparisons attenuate rather than reverse the estimate.

Cohort-composition robustness. Because the JOE outcome is measured at the release-cohort level rather than the individual level, a remaining identification concern is whether suspension adoption coincided with shifts in *who* was released. The §6.7 composition placebo tests show that share male, share Hispanic, and share young are stable across adoption, while share Black shifts modestly (+2.4 pp, p = 0.03). To assess whether this composition shift attenuates the headline result, I re-estimate the main Medicaid TWFE model adding release-cohort composition controls — share male, share Black, share Hispanic, share young, mean incarceration spell length, and log cohort size — drawn from the JOE CJARS-based module. Composition data are non-missing for 143 of the 212 state-year observations in the Medicaid sample, so the comparison must be made on the matched sample. Re-estimating the baseline TWFE on the matched sample (no composition controls) yields 16.4 pp (SE = 0.041, p = 0.002, N = 143); adding the composition controls attenuates the estimate to 13.9 pp (SE = 0.043, p = 0.007, N = 143); further adding state-year covariates (unemployment, overdose mortality, and poverty rates) yields 15.2 pp (SE = 0.041, p = 0.003, N = 130). Two readings follow. First, the matched-sample baseline (16.4 pp) is meaningfully smaller than the full-sample baseline (22.2 pp), so part of the headline magnitude reflects state-years for which composition data are unavailable rather than the additional explanatory power of composition. Second, conditional on the matched sample, composition controls produce a ~2.5 pp further attenuation, consistent with the §6.7 share-Black shift contributing modestly to — but not driving — the headline result. The composition-controlled estimate remains positive, statistically significant at conventional thresholds, and large in policy terms; the *qualitative* conclusion that suspension is associated with substantial increases in post-release Medicaid enrollment is not changed by the composition adjustment, even as the *quantitative* magnitude is somewhat attenuated.

Plausibility of the 22 pp magnitude. Because the ITT estimand averages over the full release cohort — including cohort members with no Medicaid eligibility to preserve at incarceration — the implied effect on the population whose eligibility was actually preserved is mechanically larger than 22 pp. A simple bound: if half of the release cohort entered incarceration with active Medicaid (consistent with post-ACA-expansion coverage rates among low-income justice-involved adults; Albertson et al., 2020), the implied "effect on those whose eligibility was suspended" would be on the order of 44 pp. If the share with active pre-incarceration Medicaid is 30 percent, the implied effect would be roughly 73 pp. These are not estimates — they are accounting bounds illustrating that the 22 pp ITT figure is mechanically consistent with reasonable beliefs about the share of the cohort with eligibility to preserve. The pre-ACA-only sensitivity (11.7 pp; §6.7) further anchors the magnitude: in years when fewer cohort members had Medicaid to preserve, the cohort-average ITT effect is correspondingly smaller, as the bound predicts.

6.2 Horizon Analysis: Persistence of Medicaid Effects

The Medicaid enrollment effect is persistent across post-release horizons. TWFE estimates are 22.2 pp at 1 year, 19.4 pp at 3 years (SE = 0.044, $p < 0.001$), and 20.7 pp at 5 years (SE = 0.058, $p < 0.001$). LP-DiD estimates at 3 years (18.7 pp, $p < 0.001$) corroborate this persistence.

Table A5. Horizon Sensitivity (TWFE)

Outcome	Horizon	Coef.	SE	N
Medicaid	1-year	0.222	0.051	212
Medicaid	3-year	0.194	0.044	205
Medicaid	5-year	0.207	0.058	200
Employment	1-year	0.016	0.011	212
Employment	3-year	0.003	0.011	205
Employment	5-year	-0.006	0.016	200
Mortality	1-year	0.0005	0.001	264
Mortality	3-year	0.001	0.001	231
Mortality	5-year	0.001	0.001	200
Earnings	1-year	$315	$272	212
Earnings	3-year	$281	$268	205
Earnings	5-year	$94	$348	200

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.

The persistence raises an important interpretive question: if suspension's primary mechanism is removing the administrative burden of reapplication, why does the effect not attenuate as terminated individuals eventually overcome those barriers? Standard administrative burden theory would predict shrinking impacts over time, as individuals gradually learn the system, gather documentation, and complete applications. The fact that the effect remains roughly 20 percentage points even at 5 years requires explanation.

Three mechanisms can account for the persistence. First, suspension may operate less through temporary "barrier removal" and more through a "default enrollment" channel. Once an individual is enrolled in Medicaid, continuous eligibility provisions and automatic (ex parte) redetermination processes keep them covered as long as they remain eligible. Under termination, the individual must not only complete an initial application but also successfully navigate every subsequent redetermination—each of which imposes its own administrative burden. Suspension places individuals on the "enrolled" side of this absorbing state, where the system's own momentum maintains coverage. Termination places them on the "unenrolled" side, where each year's redetermination represents another opportunity to fall through the cracks.

Second, the persistent effect is consistent with a substantial fraction of terminated individuals *never* regaining Medicaid coverage, not simply delaying enrollment. For a population facing compounding disadvantages—no stable address, no identification documents, active substance use disorders, cognitive impairment from traumatic brain injury or untreated mental illness—the administrative barriers to a new Medicaid application may be functionally permanent, not merely inconvenient. Five years may simply not be long enough for most of this population to independently navigate the enrollment process, particularly without the support of institutional programs specifically designed to assist with reentry.

Third, the CJARS JOE data measure coverage rates for *release cohorts* at each horizon, not the same individuals tracked longitudinally. The 5-year estimate captures a mix of cohort-level coverage rates reflecting cumulative policy exposure across multiple release years, not necessarily the same individuals maintaining coverage for 5 consecutive years. This cross-sectional aggregation means the persistent effect partly reflects each year's new release cohort benefiting from suspension, rather than a single cohort staying enrolled indefinitely.

Whether persistent Medicaid enrollment at 5 years is unequivocally good deserves consideration. Higher long-term enrollment could signal that individuals remain in poverty and are not achieving economic self-sufficiency—a concern reinforced by the null employment and earnings results. However, for a population with 80+ percent rates of chronic health conditions, sustained Medicaid coverage is better interpreted as appropriate utilization of a safety-net program by an eligible population with high medical needs than as a failure to "graduate" from public insurance. The null employment result suggests that suspension does not create work disincentives (enrollment is not displacing employment) but rather that the labor market barriers facing returning citizens—employer discrimination, licensing restrictions, skills atrophy—operate through entirely separate channels that health insurance alone cannot address.

I confirm this interpretation with three quantitative tests. First, a formal attenuation test comparing the 1-year effect (22.2 pp) to the 5-year effect (20.7 pp) cannot reject the null of no attenuation ($z = 0.20$, $p = 0.838$). The 1.6 percentage-point difference is well within sampling error. Second, a stacked-horizon regression that estimates the 1-year effect and tests for differential effects at 3 and 5 years confirms this: the 3-year differential is -5.6 pp ($p = 0.335$) and the 5-year differential is -1.7 pp ($p = 0.821$), neither statistically distinguishable from zero. Third, raw enrollment trends (Figure A-X) show that the gap between suspension and termination states is stable across release cohort years from 2005 through 2018 at all three horizons—the divergence does not narrow over calendar time.

These tests directly address the concern that administrative burden theory would predict shrinking impacts. The data are consistent with a permanent coverage-trajectory difference rather than a temporary barrier that terminated individuals eventually overcome.

The slight decline from 1-year (22.2 pp) to 3-year (19.4 pp) may reflect Medicaid churning through annual redetermination (Goldman and Sommers, 2020). Stabilization between 3 and 5 years (19.4 pp vs. 20.7 pp) suggests churning-induced attrition is concentrated early and a core coverage gain is maintained.

6.3 Mortality

Mortality estimates are inconclusive across estimators and the design is underpowered to resolve them. TWFE returns a near-zero estimate ($\hat{\beta} = 0.0005$, SE = 0.001, p = 0.650), LP-DiD a small reduction (-0.17 pp, SE = 0.0006, p

= 0.029), Callaway-Sant'Anna essentially zero (0.0001, SE = 0.002), and the cohort-size-weighted TWFE points in the opposite direction (+0.0020, SE = 0.001). The wild cluster bootstrap p-value (0.667) and the controls-augmented TWFE (p = 0.553) further indicate that no single sign or magnitude is robust.

The interpretive question is not which estimator is "right" — none is decisive — but what the design can and cannot detect. Against a baseline 1-year mortality rate of 1.65 percent in pre-treatment cells, the minimum detectable effect at 80 percent power is approximately 0.30 percentage points (~18 percent of baseline). The design therefore has reasonable power to detect mortality reductions of ~18 percent or larger, but is underpowered for the 6 percent (Sommers, Baicker, and Epstein, 2012) or 3.6 percent (Borgschulte and Vogler, 2020) reductions documented in the broader Medicaid-mortality literature; a TOST equivalence test against a $\pm$3.6 percent baseline-relative bound does not reject equivalence-failure ($p_{\max} = 0.46$). I therefore treat mortality as exploratory and unresolved: the present design can rule out very large effects but cannot adjudicate whether suspension produces the modest mortality gains that the broader Medicaid-mortality literature would predict. Resolving this requires individual-level data with greater statistical power. Section 7 returns to the "coverage is necessary but not sufficient" interpretation that connects the strong enrollment finding, the inconclusive mortality finding, and the null employment finding.

6.4 Employment and Earnings

I find no significant effect on employment or earnings. TWFE estimates for 1-year W-2 employment are 1.6 pp (SE = 0.011, p = 0.148); LP-DiD estimates are essentially zero (0.3 pp, SE = 0.029, p = 0.916). Earnings estimates are positive but noisy (TWFE: $315, SE = $272, p = 0.264; LP-DiD: $383, SE = $339, p = 0.341). The null result is consistent across 1-, 3-, and 5-year horizons (Table A5).

The null employment result is informative. It suggests suspension operates primarily through health coverage—its intended channel—rather than broader economic stabilization. Several mechanisms explain why a large coverage gain does not translate into employment effects. First, employment barriers for returning citizens—employer discrimination, licensing restrictions, skills atrophy, parole demands—are not addressed by health insurance. Health coverage may be necessary for employment stability (by preventing health crises that disrupt work) but not sufficient (because other barriers remain binding). Second, W-2 employment captures only formal-sector work; if suspension-induced coverage enables pursuit of lower-paying formal employment or channels individuals into SSI, net effects could be attenuated. Third, employment effects may emerge at horizons beyond my 5-year window if coverage access enables treatment for chronic conditions that eventually improve employment capacity. Fourth, state-level data may lack power to detect modest employment effects (a 1-2 pp effect would be substantively meaningful but difficult to detect with 212 clustered observations).

6.5 Secondary Outcomes

HUD housing assistance shows a small TWFE estimate (0.1 pp, SE = 0.001, p = 0.310) but a larger LP-DiD estimate (0.8 pp, $p < 0.001$). The LP-DiD result suggests Medicaid coverage may facilitate housing program access, potentially because enrollment establishes a documented connection to the safety net. However, the TWFE-LP-DiD divergence warrants caution.

SSI receipt shows no effect (TWFE: -0.03 pp, p = 0.909; N = 97). The small sample severely constrains detection, and SSI is a slow-moving outcome (application and determination take months to years).

Reincarceration shows negative but insignificant estimates (-2.2 pp at 1 year, SE = 0.027, p = 0.428; -2.4 pp at 3 years, p = 0.355), directionally consistent with Gollu and Zapryanova (2022) but underpowered in state-level data.

6.6 Heterogeneous DiD by ACA Expansion

I estimate two complementary specifications. The first (preferred for interpretation) uses time-INVARIANT expansion status: $\hat{\beta}_1$ on $\text{PostSuspension}_{it}$ captures the suspension effect in non-expansion states, and $\hat{\beta}_2$ on $\text{PostSuspension}_{it} \times \text{Expanded}_i$ captures the differential effect for expansion states. The second uses a TIME-VARYING expansion indicator $\text{Expansion}_{it} = \mathbf{1}[t \geq \text{expansion year}_i]$, which more cleanly separates the within-state activation of expansion from the cross-state classification.

Static-expansion DDD (preferred). The suspension-expansion interaction yields a positive Medicaid enrollment coefficient ($\hat{\beta}_2 = 0.195$, SE = 0.132, p = 0.158), implying a larger total effect in expansion states ($\hat{\beta}_1 + \hat{\beta}_2 = 0.075 + 0.195 = 0.270$ pp) than non-expansion states ($\hat{\beta}_1 = 0.075$ pp). This is consistent with the hypothesis that suspension matters more when more individuals have coverage to preserve, but neither coefficient achieves conventional significance — likely reflecting the limited power with only two never-treated states, both non-expansion.

Time-varying expansion DDD (identification limit). Replacing Expanded_i with Expansion_{it} exposes a fundamental identification limit in the Medicaid sample: among the 17 states with non-missing Medicaid 1-year outcomes (cohorts 2005–2018), no state-year cell is both pre-suspension and ACA-active. Every state that adopted both suspension and ACA expansion did so in an order that puts ACA activation either before suspension adoption or in the post-suspension window. The interaction $\text{PostSuspension}_{it} \times \text{Expansion}_{it}$ is therefore collinear with Expansion_{it} once state and year fixed effects are absorbed, and the interaction coefficient is not separately identified. This is itself an informative finding: in the sample available, the policy combination of "ACA expansion, but not yet suspension" is essentially absent, so the heterogeneous-DiD interaction cannot be cleanly identified by within-state activation timing. The static-expansion DDD reported above remains identifiable because it cross-classifies states by expansion status rather than by within-state expansion timing.

The period-split analysis shows TWFE estimates of 11.7 pp (SE = 0.050, p = 0.035) pre-ACA (N = 134) and 14.7 pp (SE = 0.081, p = 0.089) post-ACA (N = 78). The 26 percent larger post-ACA point estimate is consistent with the expansion hypothesis, though the difference is not statistically significant.

The lack of significance likely reflects the limited power of the heterogeneous DiD. With only two never-treated states (Georgia and Texas), the comparison group offers minimal variation in the expansion dimension. Both never-treated states are non-expansion states, which means the expansion interaction is identified primarily by comparing the within-state effect of suspension across expansion and non-expansion adopting states, without the clean between-group comparison that would be available with more never-treated states.

For other outcomes, the interaction patterns are less informative. Employment shows a positive base effect in non-expansion states (2.4 pp, $p < 0.10$) with a negative interaction (-1.0 pp, $p > 0.10$), suggesting no amplification in expansion states. For mortality, the base effect in non-expansion states is positive (0.18 pp, $p < 0.10$) with a negative interaction (-0.16 pp, $p > 0.10$). These patterns are difficult to interpret and may reflect confounding or small-sample artifacts rather than genuine heterogeneity.

6.7 Robustness

I present a comprehensive set of robustness and sensitivity analyses that, taken together, provide strong support for the main Medicaid enrollment finding. Table A6 summarizes the key robustness results.

Raw cohort trends. Figures 1–4 plot raw outcome trends by adoption cohort (early, mid, and late adopters versus never-treated states) over calendar time for each primary outcome. For Medicaid enrollment (Figure 1), treated cohorts diverge sharply upward after their respective adoption dates, while never-treated states (Georgia, Texas) remain flat—providing visual confirmation of the treatment effect before any regression adjustment. The raw trends for employment (Figure 2) and earnings (Figure 3) show no comparable divergence, consistent with the null findings for these outcomes. The raw mortality trends (Figure 4) are noisy and do not show a clear visual pattern of divergence, consistent with the mixed mortality results.

LP-DiD event studies. Figures 5–8 present LP-DiD event study estimates. For Medicaid enrollment (Figure 5), pre-treatment leads ($t = -5$ through $t = -2$) are small and centered near zero, supporting the parallel trends assumption. A clear positive jump at treatment onset ($t = 0$) persists through post-treatment lags at approximately 20–25 pp, consistent with an immediate and lasting effect. Employment (Figure 6), earnings (Figure 7), and mortality (Figure 8) event studies show near-zero pre-treatment leads and post-treatment

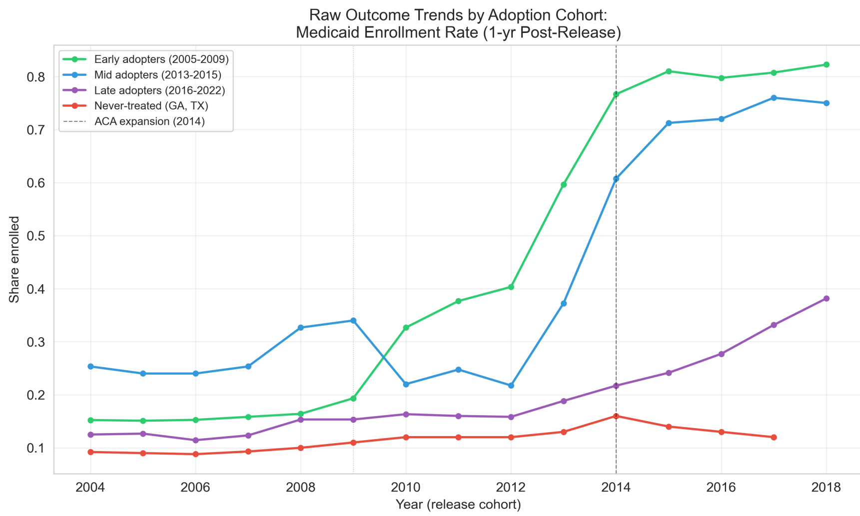

Figure 4: Figure 1. Raw Medicaid enrollment trends

Note: This figure shows raw trends for the 1. Raw Medicaid enrollment trends. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

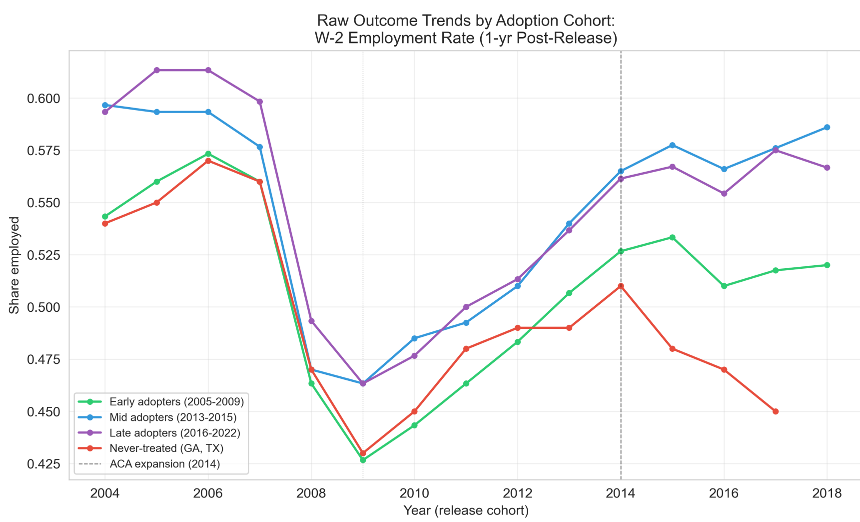

Figure 5: Figure 2. Raw employment trends

Note: This figure shows raw trends for the 2. Raw employment trends. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

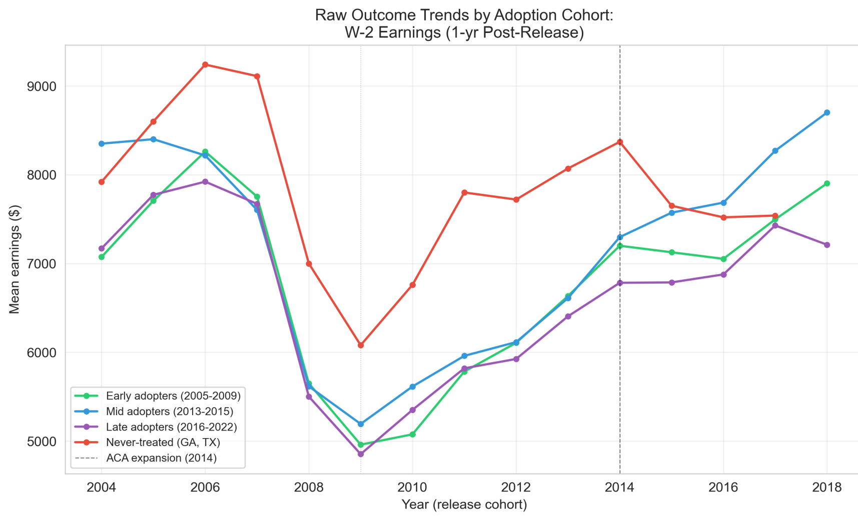

Figure 6: Figure 3. Raw earnings trends

Note: This figure shows raw trends for the 3. Raw earnings trends. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

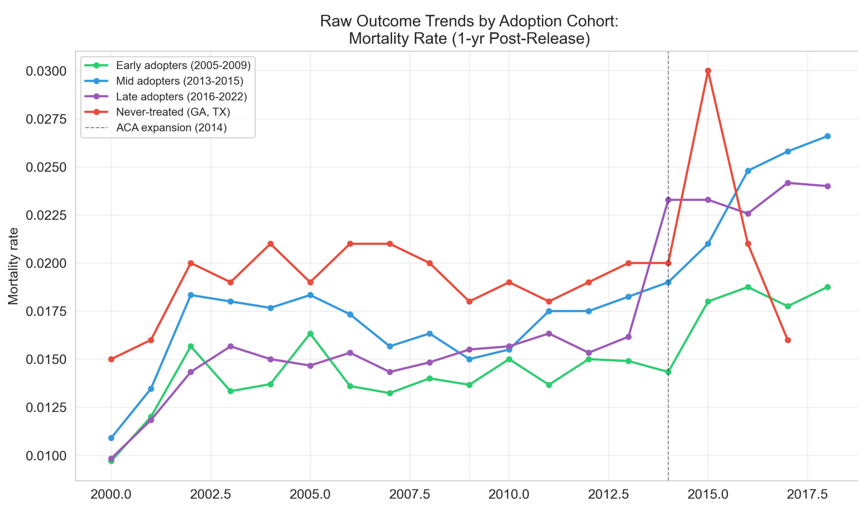

Figure 7: Figure 4. Raw mortality trends

Note: This figure shows raw trends for the 4. Raw mortality trends. It helps readers compare baseline levels, pre-policy movement, and the timing of any post-policy divergence.

coefficients consistent with the static regression findings.

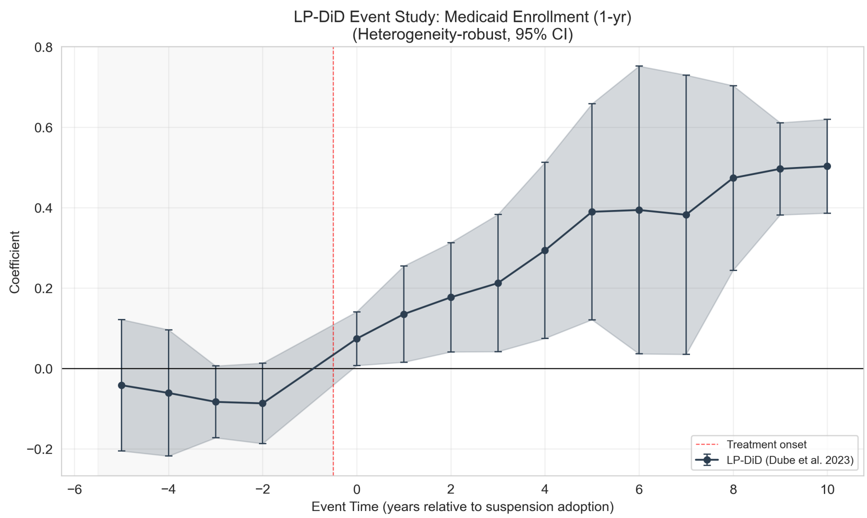

Figure 8: Figure 5. LP-DiD event study: Medicaid

Note: This figure plots event-time estimates for the 5. LP-DiD event study: Medicaid. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

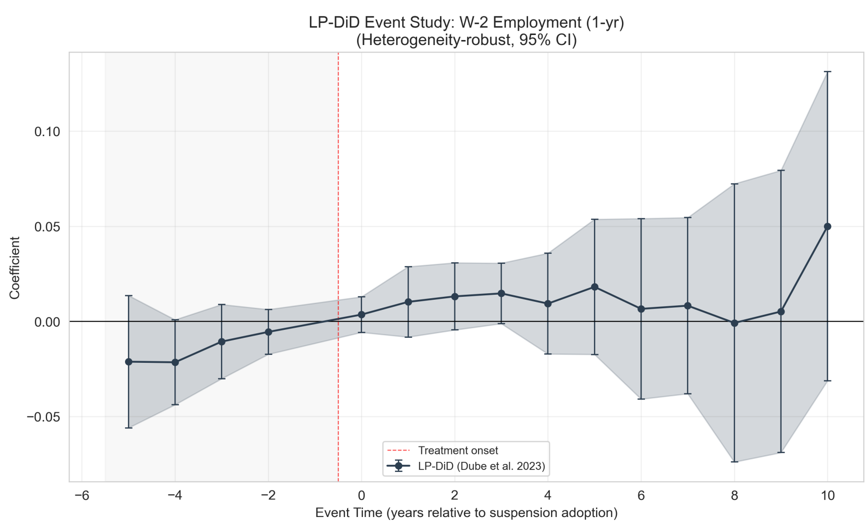

Figure 9: Figure 6. LP-DiD event study: employment

Note: This figure plots event-time estimates for the 6. LP-DiD event study: employment. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

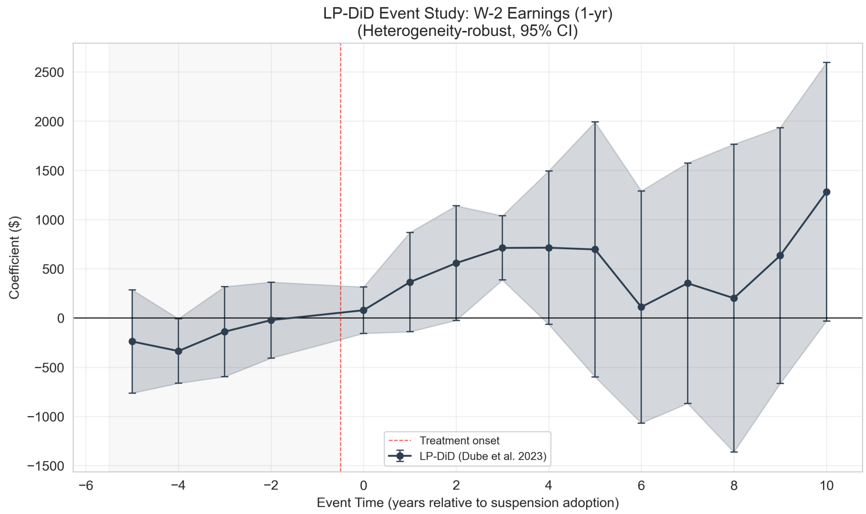

Figure 10: Figure 7. LP-DiD event study: earnings

Note: This figure plots event-time estimates for the 7. LP-DiD event study: earnings. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

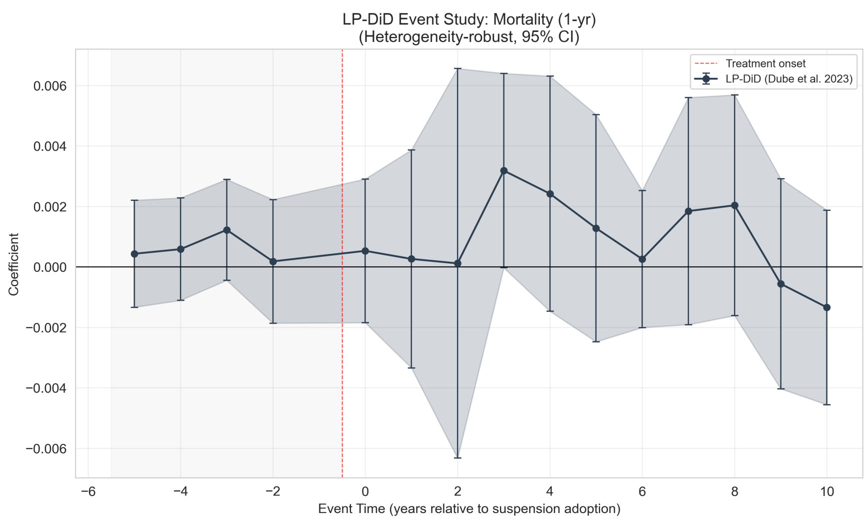

Figure 11: Figure 8. LP-DiD event study: mortality

Note: This figure plots event-time estimates for the 8. LP-DiD event study: mortality. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

LP-DiD pre-trends are substantially cleaner than TWFE event studies (Appendix Figures A1–A4), consistent with Bacon decomposition evidence that TWFE pre-trend contamination arises from problematic already-treated comparisons (Appendix Figure A10). This supports my choice of LP-DiD as primary estimator.

Goodman-Bacon decomposition. Appendix Figure A10 reveals that the TWFE estimate is primarily driven by clean comparisons, with problematic already-treated comparisons receiving small weight and producing positive but attenuated estimates.

Placebo tests. The TWFE randomization-inference p-value for Medicaid enrollment is 0.062 (500 permutations), indicating that the actual estimate lies near the extreme of the placebo distribution but does not clear conventional significance thresholds in this design-based exercise. The TWFE placebo mean is 0.122, reflecting some mechanical association from the staggered structure, while the actual TWFE estimate is 0.222. The LP-DiD randomization-inference exercise is more conservative: the design-based p-value is 0.153, with a placebo mean near zero (0.007). I therefore treat the randomization evidence as supportive but more cautious than the parametric LP-DiD and Callaway-Sant'Anna estimates.

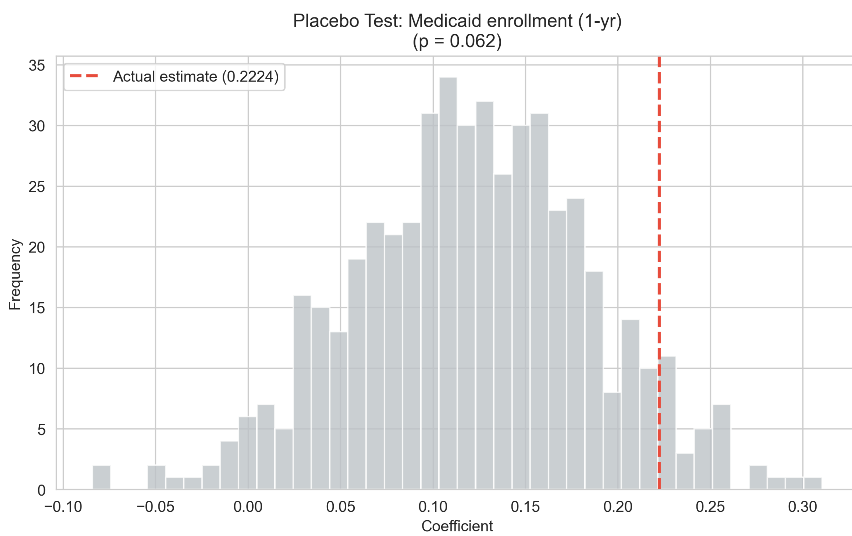

Figure 12: Appendix Figure A7. Randomization inference: Medicaid

Note: This figure reports a falsification or placebo check for the a7. Randomization inference: Medicaid. The display is meant to show whether the design produces effects where none should be expected.

Leave-one-out. The Medicaid enrollment estimate ranges from 0.206 to 0.259 when dropping each state in turn (maximum deviation: 0.036 from the full-sample estimate of 0.222). No single state drives the result. The stability across leave-one-out iterations is particularly reassuring given the relatively small number of states (31): if the result were driven by an outlier state with unusual adoption timing or outcome trajectory, the estimate would shift substantially when that state is dropped.

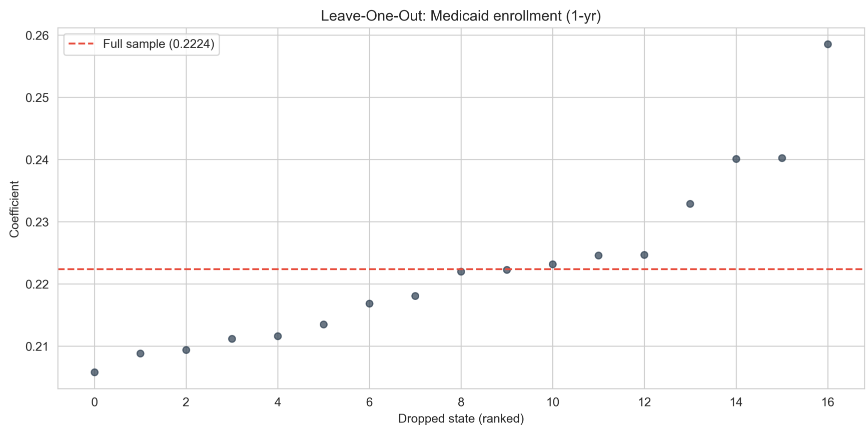

Figure 13: Appendix Figure A8. Leave-one-out: Medicaid

Note: This figure reports a robustness or sensitivity check for the a8. Leave-one-out: Medicaid. It shows how the main estimate changes under alternative assumptions, samples, or specifications.

Policy coding sensitivity. Dropping the one state with a low-confidence policy coding yields an estimate of 0.209, virtually unchanged from the full-sample estimate of 0.222 (a change of 0.013 or 6%). This indicates the result is not sensitive to potential misclassification of treatment status for the ambiguous case.

Alternative cell size thresholds. Using a minimum cell size of 50 (rather than 25) yields an estimate of 0.222, identical to baseline. All cells in the Medicaid enrollment sample exceed the 50-person threshold, so this sensitivity check is confirmatory.

Controls sensitivity. Adding time-varying controls (unemployment rate, overdose mortality rate, poverty rate) to TWFE moves the estimate from 0.222 to 0.230. The fact that adding controls moves the estimate *away from zero* rather than toward it is reassuring: it suggests that observable confounders, if anything, attenuate the uncontrolled estimate. This implies that states adopted suspension during periods when their outcomes were trending relatively unfavorably on observables, meaning the uncontrolled estimate may slightly understate the true effect. Omitted variable bias (from confounders correlated with the included controls) is unlikely to explain the positive finding.

Coefficient comparison. Appendix Figure A6 shows estimates tightly clustered around 22–23 pp across all specifications.

Horizon sensitivity. Effects persist across 1-year (22.2 pp), 3-year (19.4 pp), and 5-year (20.7 pp) horizons with no reversal.

Cohort-size weighting. Reweighting TWFE by the JOE release-cohort cohort-size variable (`N_medicaid_y1`) yields 25.7 pp (SE 6.2 pp), slightly larger than the unweighted 22.2 pp. The population-weighted estimate gives more weight to large-cohort states (Texas, California, Florida) and less to small-cohort states; that the weighted estimate is at least as large as the unweighted state-average suggests the effect is not concentrated in small idiosyncratic states.

Wild cluster bootstrap. With only 31 clusters and only two never-treated states, asymptotic cluster-robust inference may understate uncertainty. The wild cluster bootstrap (Rademacher weights, B = 999, state-level clustering) returns $p < 0.001$ for Medicaid enrollment, virtually identical to the cluster-robust p-value, confirming the inference is not artifacts of finite-cluster asymptotics. WCB p-values for employment (0.135), earnings (0.285), and mortality (0.667) are also similar to their cluster-robust counterparts.

Drop-TX (the binding never-treated state for outcomes). Within the 17-state Medicaid outcome panel, Texas is the only never-treated state with non-missing Medicaid outcome rows (Georgia, the other CJARS never-treated state, has no JOE Medicaid rows for these cohorts; South Carolina is also absent from the JOE Medicaid panel and so cannot be dropped from this regression). Dropping Texas attenuates the estimate from 0.222 to 0.209 (SE 0.052, p = 0.001). Because the design rests almost entirely on Texas as the never-treated comparator within the Medicaid outcome panel, the LP-DiD use of not-yet-treated states is not just a technical preference but a substantive one — it widens the effective comparison group beyond the single never-treated state. This is the cleanest characterization of the support thinness flagged in §5.5.

Pre-ACA-only restriction. Restricting to release cohorts 2005–2013 (the period before ACA Medicaid expansion took effect) yields a Medicaid 1-year estimate of 11.7 pp (SE 4.95 pp, p = 0.035, N = 134). The point estimate is roughly half the full-sample 22 pp but remains positive and statistically significant. This is reassuring on two fronts: (i) the result is not entirely an artifact of the post-ACA expansion period; and (ii) the magnitude in the pre-ACA window aligns closely with Packham and Slusky's (2024) South Carolina estimate of 11.1 pp, supporting the interpretation that the larger full-sample estimate reflects the post-ACA amplification of the suspension-coverage channel as more incarcerated adults entered prison with active Medicaid eligibility to preserve.

Composition / placebo outcomes. I test whether suspension adoption is associated with discontinuous changes in pre-determined release-cohort characteristics (sex, race, age, Hispanic-share). The estimates on share male (0.003, p = 0.52), share Hispanic (-0.009, p = 0.31), and share young (-0.004, p = 0.49) are small and indistinguishable from zero. The estimate on share Black is 0.024 (p = 0.03), pointing to a small post-adoption shift in cohort racial composition that may reflect concurrent criminal-justice reforms in adopting states; this is a modest concern that I revisit in §7.6 limitations.

Sharpened q-values across outcome battery. Anderson (2008) sharpened false-discovery-rate q-values across the full outcome battery (Medicaid/employment/earnings/mortality at 1/3/5 years; HUD and SSI at 1 year) leave Medicaid 1y, 3y, and 5y all at $q < 0.02$; no other outcome survives multiple-testing adjustment at 5 percent. This is consistent with Medicaid enrollment being the only outcome the design has clear power to detect.

6.8 Heterogeneity by Overdose Environment and Fentanyl Era

Three exploratory heterogeneity analyses speak to whether the suspension effect depends on the overdose environment. First, splitting states into above- and below-median baseline overdose mortality, the suspension $\times$ high-overdose interaction is small and insignificant for both Medicaid enrollment (0.021, p = 0.80) and 1-year mortality (0.0010, p = 0.57). Second, comparing pre-fentanyl (cohorts 2005–2013) and fentanyl-era (cohorts 2014–2018) windows, the suspension $\times$ fentanyl-era interaction is large and significant for Medicaid enrollment (+0.337, p = 0.002): the suspension-enrollment effect is substantially larger in the post-2014 fentanyl-era window than in the pre-2014 window. The fentanyl-era $\times$ mortality interaction, by contrast, is small and not significant (-0.0014, p = 0.38). Third, the static-expansion DDD reported in §6.6 finds a positive but underpowered ACA-expansion interaction.

These patterns are mutually consistent with two readings: the suspension-enrollment effect grew over time as more states expanded Medicaid (and so more incarcerated adults entered prison with eligibility to preserve), but the absence of a parallel mortality interaction suggests that even a larger coverage gain in the higher-overdose-risk fentanyl era did not translate into measurable cohort-level mortality reductions in this sample. This is consistent with the §6.3 interpretation that coverage is necessary but not sufficient to interrupt the post-release acute-mortality channel; the policy lever required to reduce post-release mortality may be MOUD initiation, naloxone distribution, and care coordination, not merely insurance coverage. I present these heterogeneity results as exploratory; the design is underpowered for detecting modest mortality interactions.

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7. Discussion

7.1 Summary of Findings

This paper provides the first multi-state evidence on the relationship between Medicaid suspension during incarceration and post-release Medicaid enrollment trajectories, complementing prior single-state individual-level evidence from South Carolina (Packham and Slusky, 2024) and Wisconsin pre-release enrollment-assistance evidence (Burns et al., 2022). Linking a newly constructed 31-state suspension policy panel to administratively linked CJARS Justice Outcomes Explorer release-cohort outcomes for the 17 states with built-out Census Bureau outcome linkages, I find a large and persistent enrollment effect: suspension is associated with an approximately 22 percentage-point increase in 1-year post-release Medicaid enrollment, with similar magnitudes at 3- and 5-year horizons. The estimate is consistent across TWFE, LP-DiD, and Callaway-Sant'Anna estimators, survives wild cluster bootstrap inference and a comprehensive robustness battery (leave-one-out including dropping each never-treated state and South Carolina, policy-coding sensitivity, cohort-size weighting, pre-ACA-only restriction, composition placebos, and Anderson sharpened q-values), and is interpretable as a release-cohort intent-to-treat effect rather than an individual-level treatment effect. Mortality estimates are inconclusive across estimators and the design is underpowered for the 3–6 percent relative reductions documented in the broader Medicaid-mortality literature; W-2 employment and earnings effects are null. The pattern is consistent with the interpretation that coverage is necessary but not sufficient for post-release health and economic stabilization.

7.2 Interpretation Through the Administrative Burden Lens

The 22 pp enrollment increase is among the largest effects documented in the administrative burden literature, consistent with the prediction that friction reduction should produce the largest effects when barriers are greatest. Suspension eliminates all three burden types: no learning costs (no action required), no compliance costs (no documentation needed), and minimal psychological costs (no bureaucratic interaction). The completeness of this burden reduction, combined with the extreme vulnerability of the target population, explains the magnitude.

The persistence of the effect at 3- and 5-year horizons requires extending the standard administrative burden framework. If suspension merely removed a one-time barrier, effects should attenuate as terminated individuals eventually complete applications. Instead, suspension appears to function as a "default enrollment" mechanism: once enrolled, individuals benefit from the Medicaid system's own continuity infrastructure (ex parte renewals, continuous eligibility provisions), while terminated individuals face repeated administrative barriers at each annual redetermination. The persistent effect is therefore better understood not as a single barrier removal but as placement onto a self-sustaining coverage trajectory versus exclusion from one. This interpretation is consistent with the Medicaid churning literature (Office of the Assistant Secretary for Planning and Evaluation, 2021), which documents that most disenrollment occurs through redetermination failure rather than loss of eligibility.

The administrative burden framework also helps explain why the effect is so large relative to other coverage interventions. ACA expansion changed *eligibility*; suspension changes *access to existing eligibility.* For a population where the barriers to completing an application are near-insurmountable—no stable address, no identification documents, no income documentation, overwhelming competing demands—eliminating the application requirement produces a larger coverage gain than expanding who is eligible.

My results also contribute to the broader debate about whether administrative burdens are best understood as rational disincentives (which filter out individuals who do not value the benefit sufficiently) or as arbitrary barriers (which exclude individuals who are fully eligible and would benefit from coverage). The 22 pp enrollment gain suggests that the vast majority of individuals deterred by the termination-reapplication process are, in fact, eligible for and would benefit from Medicaid coverage. Termination does not filter out ineligible individuals; it excludes eligible individuals who cannot overcome administrative barriers. This interpretation is consistent with Herd and Moynihan's (2025) argument that administrative burdens in safety-net programs primarily function as exclusionary mechanisms rather than as efficient screening devices.

7.3 Relation to Prior Literature

My Medicaid enrollment finding complements and extends the prior literature in several important ways. Most directly, it fills the mechanism gap in Gollu and Zapryanova (2022). Their finding that suspension reduces recidivism is consistent with a causal chain running from suspension through Medicaid enrollment through health care access through improved health and behavioral outcomes through reduced criminal behavior. Without observing Medicaid enrollment, they could not confirm the first link in this chain. I now provide that confirmation: suspension does indeed increase Medicaid enrollment, and by a large magnitude. My recidivism estimates (-2.2 pp at 1 year, -2.4 pp at 3 years) are directionally consistent with their findings (-2.9 pp and -4.6 pp) but lack statistical power in state-level JOE data, likely because individual-level data provide greater precision for detecting effects on binary outcomes.

The finding extends the Medicaid expansion literature by examining a complementary lever: not whether individuals are eligible but whether eligibility is preserved across incarceration. The distinction matters because eligibility without enrollment does not provide coverage, and enrollment requires overcoming administrative barriers that may be prohibitive for recently released individuals. My results suggest that the administrative channel—the ease of maintaining coverage—is as important as the eligibility channel for this population.

The null employment result is consistent with other evidence that insurance gains alone do not guarantee utilization changes (Howell et al., 2023). Together, these findings suggest a pattern in which coverage access is necessary but insufficient for broader economic stabilization. Employment may require complementary interventions—job training, ban-the-box legislation, transitional employment programs, occupational licensing reform—operating through distinct channels. Health coverage provides a platform for health stabilization, but the labor market barriers facing returning citizens are too severe to be overcome by insurance alone.

The mortality result, if confirmed in future work, would extend the Medicaid-mortality literature (Sommers et al., 2012; Borgschulte and Vogler, 2020) to the justice-involved population. The post-release period represents a uniquely high-risk window for mortality, and interventions providing coverage access during this window could have outsized benefits. However, given the estimator sensitivity, I view this as a hypothesis for future investigation rather than an established result.

7.4 Coverage Is Necessary But Not Sufficient

A central interpretive insight from this paper — building on the broader Medicaid-mortality literature on ACA expansion — is that **strong evidence on coverage take-up does not, on its own, settle the question of whether coverage translates into better health and economic outcomes**. The suspension-enrollment effect estimated here (~22 pp at one year, persistent at five years) is among the largest documented in the administrative-burden literature, yet effects on W-2 employment and earnings are null and effects on all-cause 1-year mortality are inconclusive across estimators. This pattern is mutually consistent with three observations.

First, the post-release period is dominated by acute mortality risk in which the *speed* of access matters more than the *fact* of coverage. Overdose deaths in the first weeks post-release require buprenorphine or methadone initiation in those weeks; insurance reactivation that arrives even a few weeks late, or that arrives without a coordinated handoff to a treating clinician, may not avert the death. Bandara et al. (2025) document that even where suspension is the de jure policy, de facto reactivation is often delayed and rarely accompanied by warm-handoff care coordination — and that fewer than half of states with suspension policies have data-sharing infrastructure adequate to notify Medicaid agencies of an individual's release in real time.

Second, the broader Medicaid-mortality literature (Sommers et al., 2012; Borgschulte and Vogler, 2020) typically estimates aggregate mortality reductions of 3–6 percent following ACA expansion among general adult populations measured over horizons of several years. The justice-involved population at horizons of one to five years post-release faces both higher baseline mortality (1.7 percent vs. ~0.5 percent in general adult populations) and a different composition of acute risks. Detecting a 3–6 percent relative reduction in this population would require substantially more statistical power than 31 state-year clusters and 264 observations provide; the design's minimum detectable effect (~18 percent relative reduction) is too coarse to settle the question.

Third, the null employment finding has the same character: insurance coverage may be necessary to prevent health crises that disrupt work, but it is not sufficient to overcome the labor-market barriers facing returning citizens — employer discrimination, occupational licensing restrictions, parole compliance demands, ban-the-box variation, and skills atrophy. These barriers are not addressed by health insurance and operate through entirely separate channels that the present paper does not study.

The constructive reading of these results is that the strong enrollment effect should not be overinterpreted as a sufficient policy lever for post-release health and economic stabilization. Suspension solves the *coverage* problem at reentry; it does not solve the *care continuity* problem, the *MOUD initiation* problem, or the *labor-market reintegration* problem. Subsequent policy and research should treat suspension as a foundational layer on which additional interventions (Section 1115 reentry waivers, mandatory MOUD initiation prior to release, naloxone distribution, transitional employment programs, ban-the-box) can build, rather than as a stand-alone intervention sufficient to close the post-release health-and-economic gap.

7.5 Policy Implications

Implementation of the 2026 mandate. My findings support the policy rationale behind Section 205 of the Consolidated Appropriations Act, 2024: preserving Medicaid coverage during the incarceration-to-release transition yields large enrollment gains and is likely a foundational input to any successful reentry-coverage strategy. With the federal mandate now in effect (January 1, 2026), the central implementation question is no longer whether states formally suspend rather than terminate but whether they operationalize suspension in ways that produce timely reactivation, successful renewals during incarceration, and connection to care upon release. For the states that were termination states as of late 2025 (Alabama, Georgia, Texas) and for any state with incomplete de facto implementation, my estimates provide a practical benchmark for the coverage gains pre-mandate state adoption produced (~22 pp ITT at one year), though that figure should be treated as an ecological intent-to-treat estimate rather than a guaranteed forecast. The scale is large in practical terms: these three states account for roughly 100,000 annual prison releases, so a 22 percentage-point benchmark implies on the order of 22,000 additional returning citizens maintaining or regaining Medicaid coverage each year if implementation produces gains similar to historical suspension adoption. Implementation planners can use this benchmark for budgeting (the increased enrollment will generate federal matching funds), workforce planning (more enrollees will require provider capacity), and performance monitoring (coverage rates materially below this benchmark in the first year of mandate implementation may indicate operational failures rather than headline-policy failures).

Managed-care organizations and provider capacity as implementation actors. A 22 pp enrollment increase among returning citizens — concentrated in cohorts with elevated rates of substance-use disorder, serious mental illness, and chronic conditions — represents a substantial increase in demand for behavioral-health, MOUD, and primary-care services within Medicaid

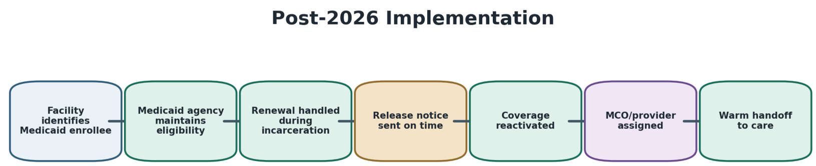

Figure 14: Post-2026 implementation

Note: This figure provides contextual structure for the post-2026 implementation. It summarizes the policy setting, mechanism, or empirical workflow used to interpret the estimates.

managed-care networks. In the 41 states that contract with managed-care organizations (MCOs) for at least part of their Medicaid program, MCOs are the operational implementers of post-release reactivation: they assign primary-care providers, manage prior authorization for MOUD, and bear capacity risk for behavioral-health utilization spikes. Two implications follow. First, MCO contracts that did not previously incorporate reentry-coverage spikes into capitation rate-setting will need to do so under the mandate, and rate-setting that fails to anticipate the demand surge documented here may produce either MCO underperformance or provider-network access failures. Second, network-adequacy standards for Medicaid (CMS access-monitoring rules) should be evaluated specifically for behavioral-health and MOUD adequacy in counties with high reentry volume; uniform statewide adequacy standards may mask county-level capacity gaps that bind precisely where suspended coverage is reactivated. The Howell et al. (2023) finding that ACA-expansion-induced insurance access alone did not change mental-health treatment utilization for justice-involved adults underscores that demand-side coverage gains cannot substitute for supply-side capacity.

Complementarity with Section 1115 waivers. Suspension and pre-release Section 1115 reentry waivers are complementary rather than substitutive. Suspension preserves the *eligibility* margin across incarceration; 1115 waivers expand the *scope of covered services* in the 30 to 90 days before release (CMS, 2023). The two policies address different margins of the care-discontinuity problem: suspension addresses the administrative barrier to post-release coverage access, while 1115 waivers address the clinical barrier to pre-release treatment initiation and care-coordination handoff. States implementing both policies simultaneously should, in principle, see synergistic effects on care utilization and post-release outcomes, although this interaction has not yet been evaluated empirically.

Coverage continuity beyond reactivation. The slight attenuation at longer horizons (22 pp at 1 year, 19 pp at 3 years, 21 pp at 5 years) suggests that suspension alone does not solve Medicaid churning. Combining suspension with continuous-eligibility guarantees (e.g., 12 months of guaranteed continuous eligibility post-release) could further improve coverage continuity and amplify downstream health benefits.

Targeted burden reduction at other transitions. The administrative-burden lens also has implications outside the criminal-justice context. Analogous high-friction transitions — aging out of foster care, exiting homelessness shelters, leaving inpatient psychiatric care, transitioning from pediatric to adult Medicaid, leaving long-term care facilities — may similarly benefit from automatic enrollment or coverage-preservation defaults at the moments when individual capacity to navigate paperwork is at its lowest.

7.6 Limitations

Ecological design. My analysis uses state-year-level data from JOE, which provides aggregate outcomes rather than individual-level transitions. This design cannot rule out compositional changes: if suspension-adopting states simultaneously experienced shifts in who is released (e.g., toward individuals with higher baseline Medicaid eligibility rates or lower mortality risk), estimates could partially reflect selection rather than the policy effect. Pre-trends tests and controls stability mitigate this concern by showing that outcome trends were parallel before adoption and that observable confounders do not explain the effect, but they cannot eliminate the possibility of unobservable compositional changes. The ecological design also prevents heterogeneity analysis by individual characteristics (age, race, sex, offense type, incarceration length) and reduces statistical power relative to individual-level data, which may explain my inability to detect effects on employment and recidivism.

Incomplete state coverage. CJARS covers 31 of 51 states, with coverage determined by state data-sharing agreements with the Census Bureau. The 20 missing states—including Alabama, the third termination state—may differ systematically from included states. If missing states are disproportionately those with weaker administrative infrastructure (which might make suspension less effective) or with different incarcerated populations, my estimates may not generalize to the full set of states. However, my sample includes 27 of 29 suspension-adopting states and 2 of 3 termination states, providing reasonable coverage of both treatment conditions.

Limited never-treated states. Only two never-treated states (Georgia, Texas) appear in the CJARS sample. This constrains the comparison group for both standard and heterogeneity-robust estimators. The LP-DiD estimator partially addresses this by using not-yet-treated states as comparators (providing more comparison units), but the small number of never-treated units remains a limitation. This is particularly constraining for the ACA expansion interaction analysis, where both never-treated states are non-expansion states, limiting my ability to identify the expansion interaction effect.

De jure versus de facto implementation. My policy panel captures the de jure adoption of suspension—when the policy was formally enacted or announced—but Bandara et al. (2025) document substantial gaps between de jure policy and de facto implementation. These gaps include poor data-sharing infrastructure between carceral facilities and Medicaid agencies (preventing timely notification of release), unpredictable jail release dates (complicating reactivation of suspended coverage), and neglected renewal processes for suspended beneficiaries (leading to coverage lapses despite nominal suspension). This measurement error in the treatment variable would attenuate my estimates toward zero, suggesting that my findings may be conservative: the effect of fully and effectively implemented suspension could be larger than what I estimate from de jure adoption.

Managed-care implementation heterogeneity. Medicaid managed-care penetration varies substantially across states and over time, and managed-care plans often control the operational steps that matter after reactivation: network assignment, prior authorization, behavioral-health access, care management, and pharmacy continuity. State fixed effects absorb time-invariant differences in managed-care infrastructure, but they do not fully address differential changes in managed-care capacity or contract design that coincide with suspension adoption. If suspension states were also improving managed-care reentry operations, the coverage and access effects could partly reflect those implementation complements rather than eligibility preservation alone.

Limited observation window. Medicaid enrollment data in CJARS are available only for cohorts released between 2005 and 2018, tied to the MSIS/T-MSIS linkage window. This prevents me from observing effects for the most recent adopters (post-2018) and limits the sample size. Similarly, SSI data are available only for a subset of years, severely limiting power for that outcome. I cannot assess very long-run impacts beyond 5 years post-release.

Potential SUTVA violations. The stable unit treatment value assumption requires that one state's policy adoption does not affect outcomes in other states. This could be violated if individuals strategically relocate to suspension states upon release or if federal policy attention to suspension creates spillover effects on termination states. I view these concerns as relatively minor: the choice of release state is largely determined by the location of the correctional facility, not by the individual.

Measurement. W-2 employment captures only formal-sector employment reported to the IRS. Individuals engaged in informal, gig, or off-the-books work are classified as non-employed, potentially masking effects on total work activity. Mortality is measured through the Census Numident, which is high-quality but may miss some deaths, particularly of individuals who are undocumented or transient.

7.7 A Post-2026 Research Agenda

With Section 205 of the Consolidated Appropriations Act, 2024 in effect since January 1, 2026, the research agenda on Medicaid suspension shifts from cross-state evaluation of *whether* states should suspend to within-state evaluation of *how* well they implement. Five high-priority research directions follow from the present paper.

1. **Implementation-quality heterogeneity within the mandate era.** CMS December 2025 guidance describes a menu of operational suspension strategies, ranging from indefinite suspension with automated reactivation to time-limited suspension with manual reapplication processes (CMS, 2025). The newly mandated states (Alabama, Georgia, Texas) and any state with weak prior implementation (Bandara et al., 2025) provide an implementation-quality natural experiment. Synthetic-control or interrupted-time-series designs comparing post-mandate coverage trajectories to pre-mandate trends — and to a never-treated counterfactual reconstructed from suspension states — can identify whether implementation strategy matters.

2. **Section 1115 reentry waivers and suspension interaction.** As of mid-2025, 18 states had approved 1115 reentry waivers and 9 more were pending (KFF, 2025). The independent and joint effects of (i) eligibility-preserving suspension and (ii) pre-release service-coverage waivers can be identified by the staggered rollout of waivers within the now-uniform suspension regime. The relevant outcomes — care utilization, MOUD initiation, post-release inpatient and ED use — speak directly to whether the "coverage is necessary but not sufficient" interpretation in §7.4 is empirically right.

3. **Individual-level CJARS analysis through FSRDC.** The most valuable extension is restricted-access CJARS microdata. Individual-level data would (a) replace the ITT estimand at the release-cohort level with a treatment-on-treated estimand among individuals whose Medicaid was actually suspended; (b) enable heterogeneity by race, sex, age, offense type, and incarceration spell length; (c) substantially increase statistical power for mortality and employment outcomes that the design here cannot resolve; and (d) measure the *timing* of post-release Medicaid reactivation (day-of-release vs. month-1 vs. month-3) — the mechanism that connects suspension to acute mortality avoidance.

4. **MOUD initiation and care-coordination interventions layered on top of suspension.** The "coverage is necessary but not sufficient" reading of the present results implies that the next-generation reentry-coverage research question is which complementary interventions translate eligibility into care: in-prison MOUD initiation; warm-handoff care coordination; naloxone distribution at release; transportation vouchers; transitional case-management. Several of these are now testable as experimental or quasi-

experimental policy variation under the post-mandate regime.

5. **Long-run impacts beyond five years.** The 1- to 5-year horizons available in JOE cannot speak to long-run effects on health, employment, recidivism, or mortality. As CJARS-MSIS/T-MSIS linkages extend forward in time, longer-horizon evaluation of pre-mandate suspension cohorts becomes feasible, and may reveal effects that are too small or too distal to detect at five years.

The federal mandate did not end the research agenda — it shifted it from "should states suspend?" to "how do I make suspension translate into coverage, care, and outcomes?" The estimates in this paper are most useful as a coverage benchmark against which post-mandate implementation can be evaluated.

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8. Conclusion

This paper provides the first multi-state evidence on the relationship between Medicaid suspension during incarceration and post-release coverage trajectories. Linking a newly constructed 31-state suspension policy panel to administratively linked CJARS release-cohort outcomes for 17 reporting states, I find a large, persistent intent-to-treat coverage effect: suspension is associated with a roughly 22 percentage-point increase in 1-year post-release Medicaid enrollment, robust across TWFE, LP-DiD, and Callaway-Sant'Anna estimators and across a comprehensive robustness battery. The effect persists at 3- and 5-year horizons. Effects on W-2 employment and earnings are null. Mortality estimates are inconclusive: the design has reasonable power to detect 18 percent or larger relative reductions but is underpowered for the 3–6 percent relative reductions documented in the broader Medicaid-mortality literature.

These findings arrive at a transitional policy moment. Section 205 of the Consolidated Appropriations Act, 2024 took effect on January 1, 2026, prohibiting states from terminating Medicaid eligibility solely because an individual is an inmate of a public institution (CMS, 2025). The cross-state variation that made this paper's quasi-experimental identification possible is therefore now historical. My estimates support the policy rationale underlying the mandate, complement the prior single-state individual-level evidence (Packham and Slusky, 2024; Burns et al., 2022), and provide an implementation benchmark — approximately 22 pp at one year — against which post-mandate state implementation can be evaluated. They also support a more cautious second message: a strong coverage effect is not, on its own, evidence that the post-release acute-mortality and labor-market gaps will close. Coverage is necessary but not sufficient.

More broadly, the results indicate that reducing administrative burdens at critical life transitions — when individuals face the greatest barriers to accessing safety-net programs — can produce substantial improvements in program take-up. The suspension-termination distinction, seemingly a minor administrative choice embedded in the technical details of Medicaid policy, has been an important determinant of whether returning citizens maintain health coverage through the most vulnerable period of their lives. The lesson extends beyond Medicaid and beyond the justice-involved population: at moments of acute life transition, when individuals face compounding barriers and overwhelming demands, the friction of a government application can be the difference between coverage and no coverage. Translating that coverage gain into measurably better health and economic outcomes is the next-generation policy and research challenge.

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Appendix: Suspension vs. Termination

Table A1: Summary Statistics

	N	Mean	SD	Min	Median	Max
Panel A. Primary outcomes, 1-year horizon						
Medicaid enrollment	212	0.320	0.251	0.078	0.200	0.880
W-2 employment	212	0.536	0.074	0.350	0.540	0.680
W-2 earnings ($)	212	6,909	1,634	3,060	6,905	11,600
Mortality	264	0.017	0.005	0.006	0.016	0.048
Panel B. Longer-horizon and secondary outcomes						
Medicaid enrollment (3-yr)	207	0.287	0.187	0.091	0.210	0.800
Medicaid enrollment (5-yr)	201	0.277	0.166	0.093	0.210	0.740
HUD housing (1-yr)	264	0.011	0.005	0.002	0.010	0.031
SSI receipt (1-yr)	99	0.043	0.010	0.021	0.045	0.060
Incarceration spell length (mos.)	378	25.1	6.6	11.5	24.5	41.7
Panel C. State-year controls						
Unemployment rate (%)	561	5.58	2.13	2.20	5.00	13.70
Overdose mortality rate	554	13.9	6.5	2.6	12.7	44.3
Poverty rate (%)	534	13.0	2.9	6.9	12.7	21.4
Panel D. Policy variable						

	N	Mean	SD	Min	Median	Max
Post-suspension indicator	561	0.296	0.457	0	0	1

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.

Notes: Unit of observation is state-year. Medicaid, employment, HUD, SSI, and mortality outcomes are shares in [0, 1]. Earnings are reported in nominal dollars averaged across the release-cohort members. Overdose mortality rate is CDC WONDER age-adjusted deaths per 100,000. Sample covers 31 states over 2000–2020; the Medicaid outcomes are restricted to 17 states for cohorts 2005–2018 by the MSIS/T-MSIS linkage window.

Table A2: Pre-Adoption Balance

	Adopting states	Non-adopting states	Difference	Normalized diff.
	Mean (SD)	Mean (SD)		
Panel A. Primary outcomes				
Medicaid enrollment (1-yr)	0.210 (0.109)	0.144 (0.041)	0.066	0.80
W-2 employment (1-yr)	0.552 (0.079)	0.513 (0.063)	0.039	0.55
W-2 earnings (1-yr, $)	6,814 (1,822)	6,867 (1,432)	−53	−0.03
Mortality (1-yr)	0.016 (0.005)	0.017 (0.004)	−0.001	−0.17
Panel B. Release-cohort characteristics				
Incarceration spell (months)	22.6 (5.8)	29.9 (3.5)	−7.3	−1.53
Annual release cohort size	10,096 (7,002)	34,644 (23,043)	−24,549	−1.44
Share male	0.879 (0.027)	0.864 (0.022)	0.015	0.62
Share Black	0.302 (0.156)	0.388 (0.078)	−0.086	−0.70
Share Hispanic	0.149 (0.112)	0.133 (0.107)	0.015	0.14
Share young (15–24)	0.197 (0.050)	0.181 (0.022)	0.016	0.42
Panel C. State controls				

	Adopting states	Non-adopting states	Difference	Normalized diff.
Unemployment rate (%)	5.69 (2.23)	5.60 (1.93)	0.10	0.05
Overdose mortality rate	11.8 (5.0)	13.3 (5.6)	−1.5	−0.28
Poverty rate (%)	12.5 (3.0)	15.3 (1.9)	−2.8	−1.10

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.

Notes: Means (standard deviations) across pre-adoption state-year observations. "Adopting states" comprises all states that adopted suspension during 2005–2022 observed in their pre-treatment years; "non-adopting states" comprises Georgia and Texas, the two never-treated states in the CJARS sample, across all years. Normalized difference is the mean difference divided by the pooled standard deviation. Release-cohort demographic shares are computed from JOE state-data subgroup cells using the release-cohort cohort-size variable (cohort size at the 5-year W-2 horizon) as the denominator within sex/race/age subgroups. Level differences in Panels A–C are absorbed by state fixed effects in the main DiD design; the identifying assumption requires parallel trends rather than level equivalence.

Table A3: Main Results Across Estimators and Outcomes, 1-Year Horizon

	Medicaid	Medicaid (+controls)	Employment	Earnings	Mortality
	(1)	(2)	(3)	(4)	(5)
Panel A. Two-way fixed effects (TWFE)					
Suspension	0.222***	0.230***	0.016	315.3	0.0005
	(0.051)	(0.041)	(0.011)	(272.3)	(0.0011)
Panel B. LP-DiD (Dube et al., 2023)					
Suspension	0.224***	—	0.003	383.3	−0.0017**
	(0.018)		(0.029)	(339.2)	(0.0006)
Panel C. Callaway–Sant'Anna (2021)					

	Medicaid	Medicaid (+controls)	Employment	Earnings	Mortality
Suspension (aggregated ATT)	0.258***	—	—	—	0.0001
	(0.046)				(0.0015)
Panel D. TWFE, cohort-size weighted					
Suspension	0.257***	—	0.023	351.4	0.0020*
	(0.062)		(0.020)	(368.5)	(0.0012)
Panel E. Inference					
Wild cluster bootstrap *p*	<0.001	<0.001	0.135	0.285	0.667
Specification details					
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Time-varying controls	No	Yes	No	No	No
Observations	212	198	212	212	264
States	17	17	17	17	31
R^2 (within, TWFE Panel A)	0.214	0.268	0.034	0.021	0.003

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.

Notes: Cluster-robust standard errors (state-level) in parentheses. Each column is an outcome-specific specification with state and year fixed effects. Panel A is the benchmark TWFE; Panel B uses the LP-DiD estimator of Dube et al. (2023), which uses only not-yet-treated and never-treated states as comparators at each horizon; Panel C uses the Callaway–Sant'Anna (2021) group-time ATT estimator with not-yet-treated controls; Panel D is TWFE reweighted by the JOE release-cohort cohort-size variable for each outcome; Panel E reports Rademacher wild cluster bootstrap p-values (B = 999). Medicaid and employment outcomes are shares; earnings are nominal dollars; mortality is the share deceased within the horizon. The Medicaid sample is restricted to 17 states (cohorts 2005–2018) by the MSIS/T-MSIS linkage window. *p<0.10, **p<0.05, ***p<0.01.

Table A4: Heterogeneous DiD by ACA Medicaid Expansion Status

	Medicaid	Employment	Earnings	Mortality
	(1)	(2)	(3)	(4)
Panel A. Static-expansion interaction (main)				
Suspension	0.075	0.024*	479.0	0.0018*
	(0.130)	(0.012)	(642.0)	(0.0010)
Suspension × Expanded	0.195	−0.010	−216.0	−0.0016
	(0.132)	(0.017)	(661.0)	(0.0016)
Panel B. Period-split TWFE				
Suspension × Pre-ACA (2000–2013)	0.117**	0.007	−3.0	−0.0000
	(0.050)	(0.009)	(300.0)	(0.0007)
Suspension × Post-ACA (2014–2020)	0.147*	0.008	−157.0	−0.0013
	(0.081)	(0.008)	(300.0)	(0.0035)
Panel C. Time-varying expansion DDD (identification limit)				
Suspension × Expansion_{it}	*collinear*	*collinear*	*collinear*	*collinear*
Specification details				
State FE, Year FE	Yes	Yes	Yes	Yes
Observations (static)	212	212	212	264
Observations (pre-ACA)	134	134	134	186
Observations (post-ACA)	78	78	78	78

Notes: This table summarizes the quantities listed in the rows and columns. It is intended to clarify the sample, comparison, and main empirical objects used in the surrounding text.

Notes: Cluster-robust standard errors (state-level) in parentheses. Panel A reports the heterogeneous-DiD interaction with time-invariant ACA expansion status as the cross-classifying dimension. Panel B reports TWFE coefficients on the suspension indicator estimated separately in pre- and post-ACA-expansion periods. Panel C reports the outcome of an attempted time-varying expansion DDD: in the Medicaid sample, every state that adopted both suspension and

ACA expansion did so in an order that makes the interaction $\text{PostSuspension}_{it} \times \text{Expansion}_{it}$ collinear with Expansion_{it} net of fixed effects, and the interaction coefficient is not separately identified (see §6.6). *p<0.10, **p<0.05, ***p<0.01.

Table A5: Horizon Sensitivity (TWFE), Years 1, 3, and 5 Post-Release

	Medicaid	Employment	Earnings ($)	Mortality
1-year horizon				
Suspension	0.222***	0.016	315.3	0.0005
	(0.051)	(0.011)	(272.3)	(0.0011)
Observations	212	212	212	264
3-year horizon				
Suspension	0.194***	0.003	281.1	0.0014
	(0.044)	(0.011)	(268.3)	(0.0012)
Observations	205	205	205	231
5-year horizon				
Suspension	0.207***	−0.006	93.8	0.0007
	(0.058)	(0.016)	(348.0)	(0.0014)
Observations	200	200	200	200
Specification details				
State FE, Year FE	Yes	Yes	Yes	Yes

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.

Notes: Cluster-robust standard errors (state-level) in parentheses. Each cell is a separate TWFE regression of the stated outcome on the post-suspension indicator with state and year fixed effects. *p<0.10, **p<0.05, ***p<0.01.

Table A6: Robustness Summary, Medicaid Enrollment (1-Year)

	Coefficient	SE / CI	N / States	Notes
Panel A. Estimator robustness				
TWFE (baseline)	0.222***	(0.051)	212 / 17	Cluster-robust SE
TWFE + time-varying controls	0.230***	(0.041)	198 / 17	Unemployment, overdose, poverty
LP-DiD (Dube et al. 2023)	0.224***	(0.018)	—	Heterogeneity-robust
Callaway–Sant'Anna (2021)	0.258***	(0.046)	—	Not-yet-treated controls
Cohort-size-weighted TWFE	0.257***	(0.062)	212 / 17	Weights = N_medicaid_y1
Panel B. Inference robustness				
Wild cluster bootstrap *p*	—	< 0.001	—	B = 999, Rademacher
Randomization inference *p*	—	0.062	500 perms	Design-based
Panel C. Sample / subsample robustness				
Leave-one-out (range)	[0.206, 0.259]	max dev. 0.036	—	Across 17 states
Drop Georgia (never-treated)	0.222***	(0.051)	212 / 16	Main result unchanged
Drop Texas (never-treated)	0.209**	(0.052)	212 / 16	Modest attenuation
Drop South Carolina	0.222***	(0.051)	212 / 16	Matches Packham-Slusky scope
Pre-ACA only (2005–2013)	0.117**	(0.050)	134 / 17	Aligns with Packham-Slusky 11.1pp
Post-ACA only (2014–2018)	0.147*	(0.081)	78 / 17	ACA amplification
Panel D. Policy-coding and cell-size robustness				
Drop low-confidence policy codes	0.209***	—	—	1 state dropped
Cell size N >= 50 (vs. N >= 25)	0.222***	(0.051)	—	Identical (all cells >= 50)
Panel E. Multiple-testing adjustment				

	Coefficient	SE / CI	N / States	Notes
Anderson (2008) sharpened q	0.222	q = 0.005	—	Across 14-outcome battery

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.

Notes: Column 1 reports the coefficient on the post-suspension indicator for Medicaid 1-year enrollment; column 2 reports the cluster-robust standard error in parentheses (or p-value / CI where relevant); column 3 reports sample size and cluster count; column 4 describes the specification. All TWFE specifications include state and year fixed effects with cluster-robust SE clustered at the state level. The Anderson (2008) sharpened q-value is computed across the full outcome battery (Medicaid, employment, earnings, mortality at 1/3/5 years; HUD and SSI at 1 year). *p<0.10, **p<0.05, ***p<0.01.

Appendix Figures

Appendix Figure A1. TWFE Event Study — Medicaid Enrollment (1-yr).

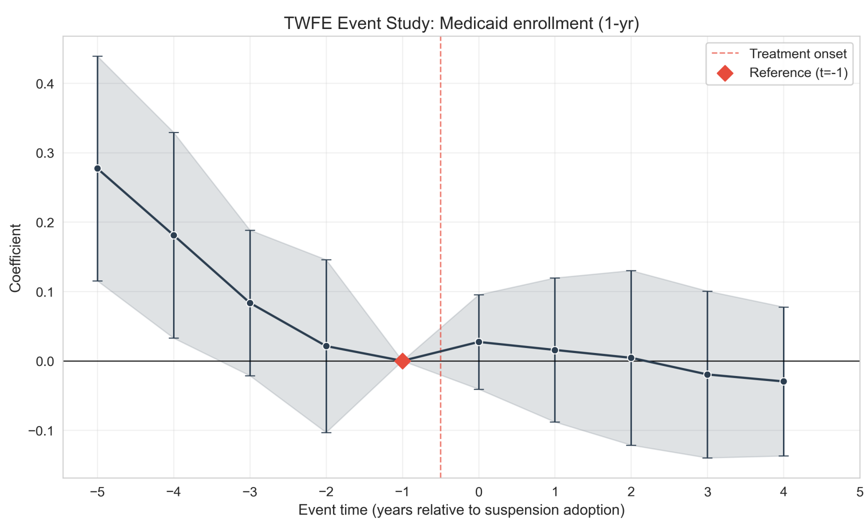

Figure 15: A1

Note: This figure presents the es twfe medicaid y1. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

Appendix Figure A2. TWFE Event Study — W-2 Employment (1-yr).

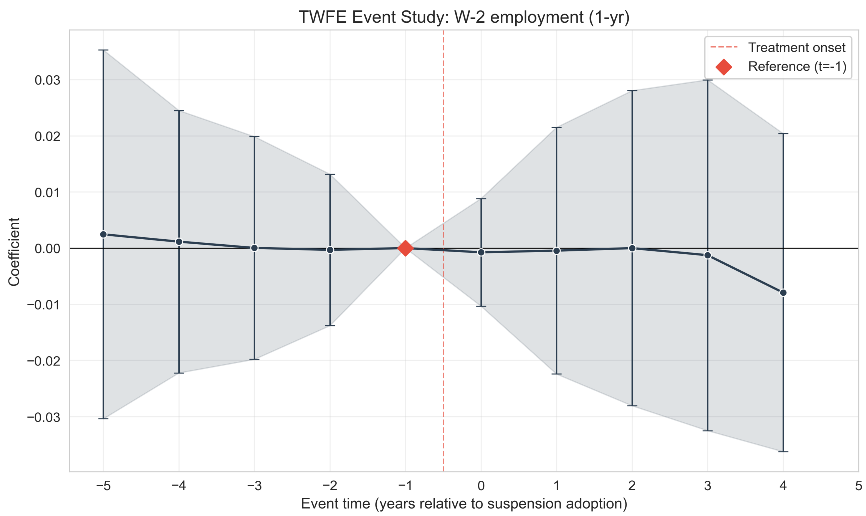

Figure 16: A2

Note: This figure presents the es twfe employment y1. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

Appendix Figure A3. TWFE Event Study — W-2 Earnings (1-yr).

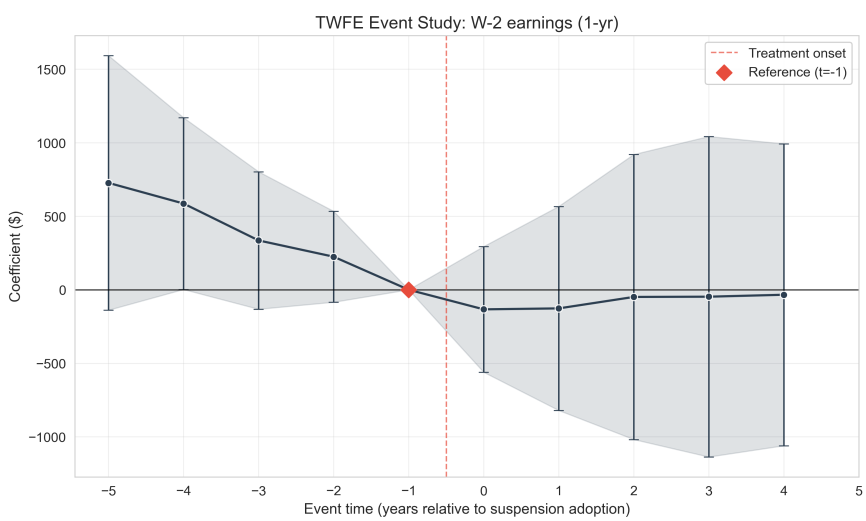

Figure 17: A3

Note: This figure presents the es twfe earnings y1. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

Appendix Figure A4. TWFE Event Study — Mortality (1-yr).

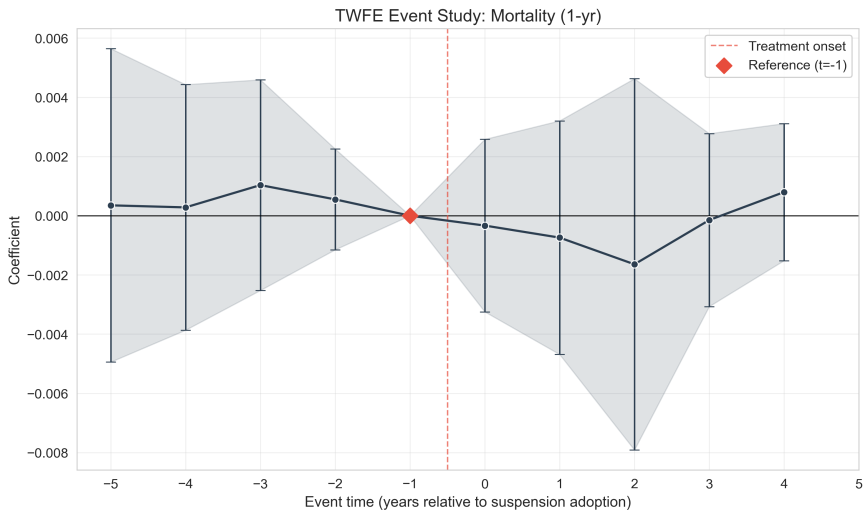

Figure 18: A4

Note: This figure presents the es twfe mortality y1. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

Appendix Figure A5. Policy Adoption Timeline.

Appendix Figure A6. Coefficient Comparison Across Estimators.

Appendix Figure A7. Placebo Distribution — Medicaid Enrollment.

Appendix Figure A8. Leave-One-Out — Medicaid Enrollment.

Appendix Figure A9. Placebo Distribution — Mortality.

Appendix Figure A10. Goodman-Bacon Decomposition — Medicaid Enrollment.

Appendix Figure A11. Alternative Scale — Earnings: Levels vs. Log LP-DiD Event Study.

Appendix Figure A12. TWFE vs. LP-DiD Overlay — Medicaid Enrollment.

Appendix Figure A13. LP-DiD Randomization Inference.

References

Bandara, S., B. Saloner, H. Maniates, M. Song, and N. Krawczyk. 2025. "Implementation of Carceral Medicaid Suspension and Enrollment Programs." *Health & Justice* 13: 3.

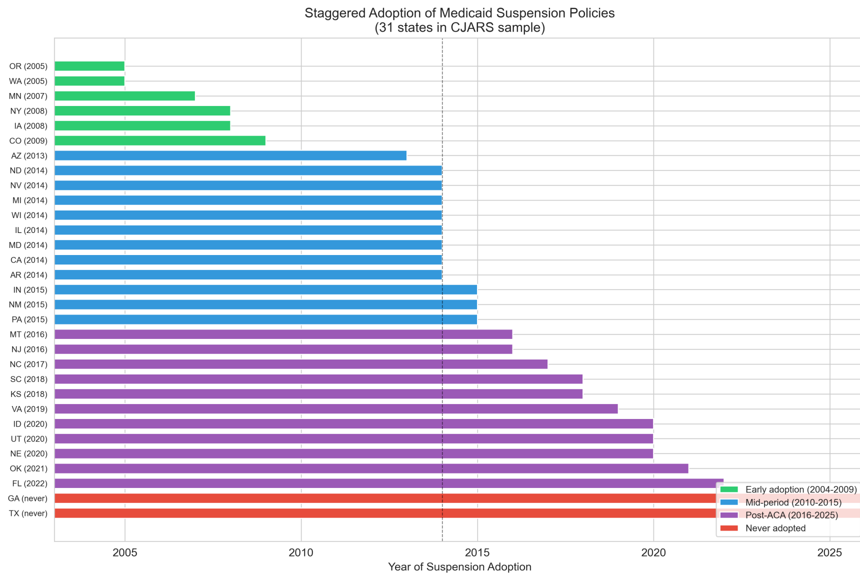

Figure 19: A5

Note: This figure summarizes treatment timing and sample support for the revision s1 adoption timeline. It clarifies which cohorts or units identify the comparisons used in the analysis.

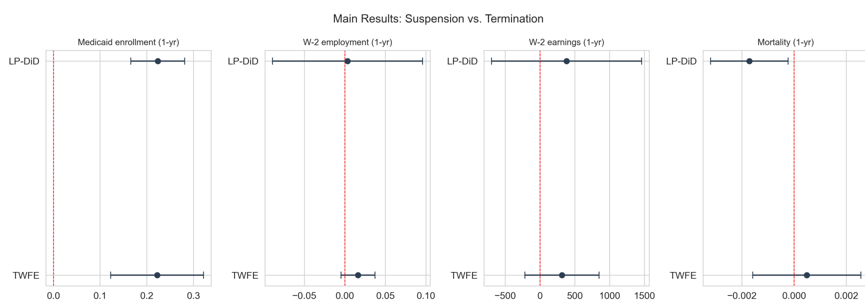

Figure 20: A6

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

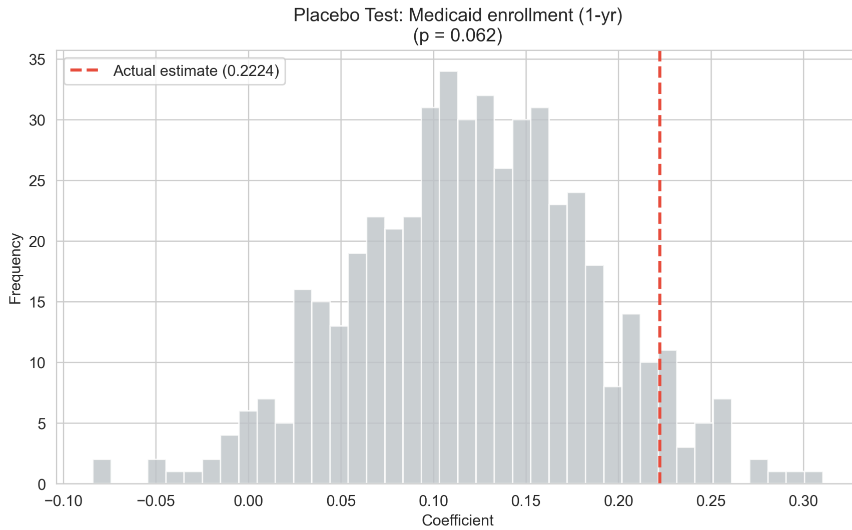

Figure 21: A7

Note: This figure reports a falsification or placebo check for the placebo medicaid y1. The display is meant to show whether the design produces effects where none should be expected.

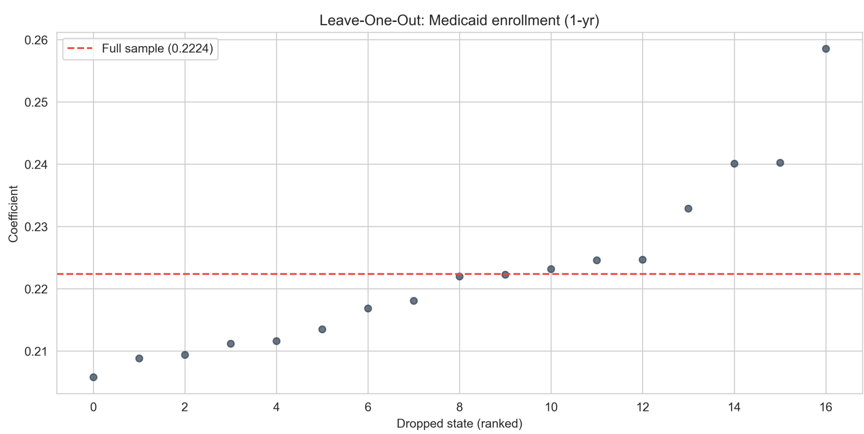

Figure 22: A8

Note: This figure presents the loo medicaid y1. It is included to make the empirical design, sample structure, or headline result easier to read alongside the surrounding text.

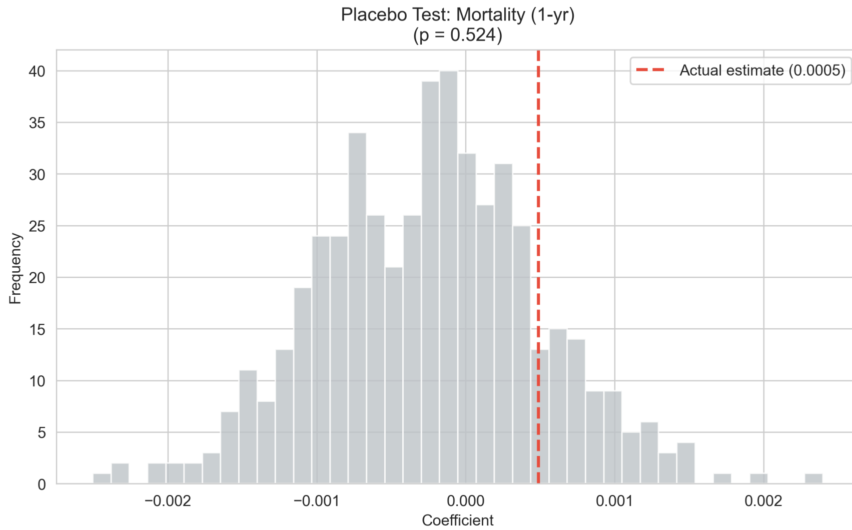

Figure 23: A9

Note: This figure reports a falsification or placebo check for the placebo mortality y1. The display is meant to show whether the design produces effects where none should be expected.

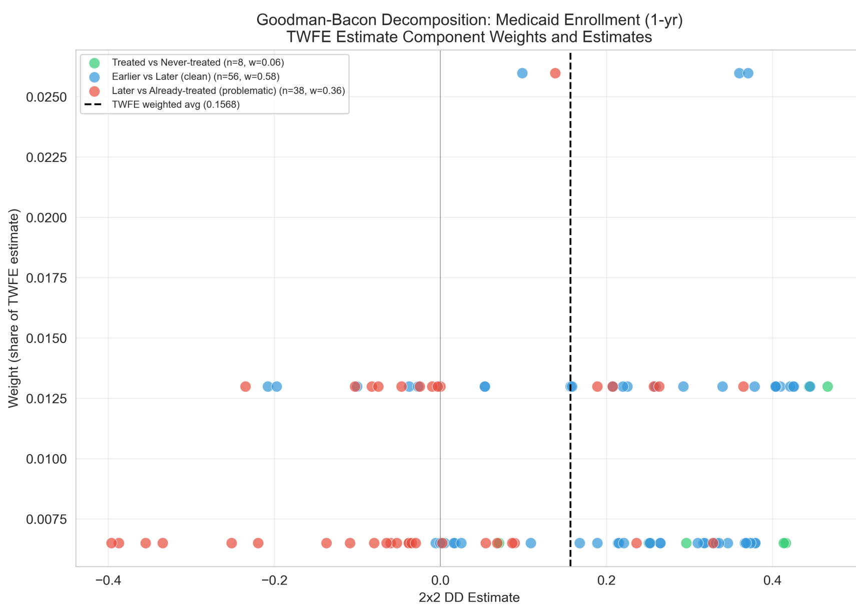

Figure 24: A10

Note: This figure decomposes the identifying comparisons or weights for the decomp medicaid y1. It shows which comparisons contribute most to the reported estimate.

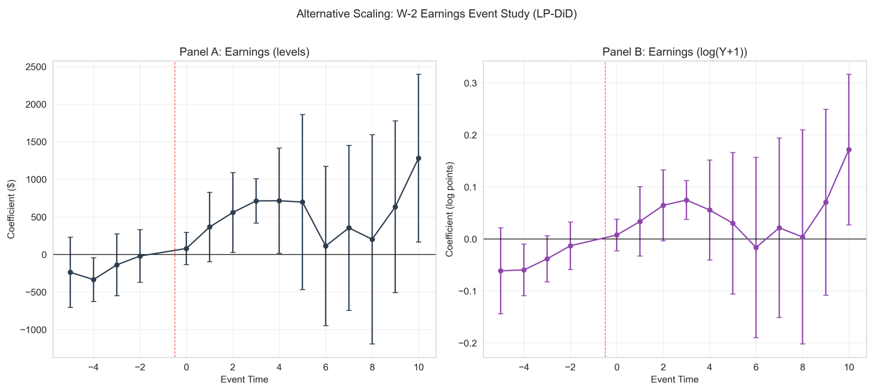

Figure 25: A11

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

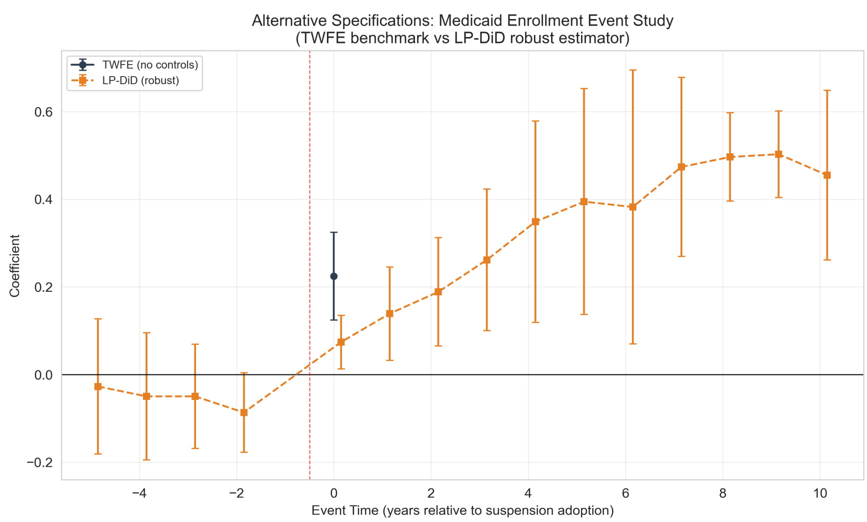

Figure 26: A12

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

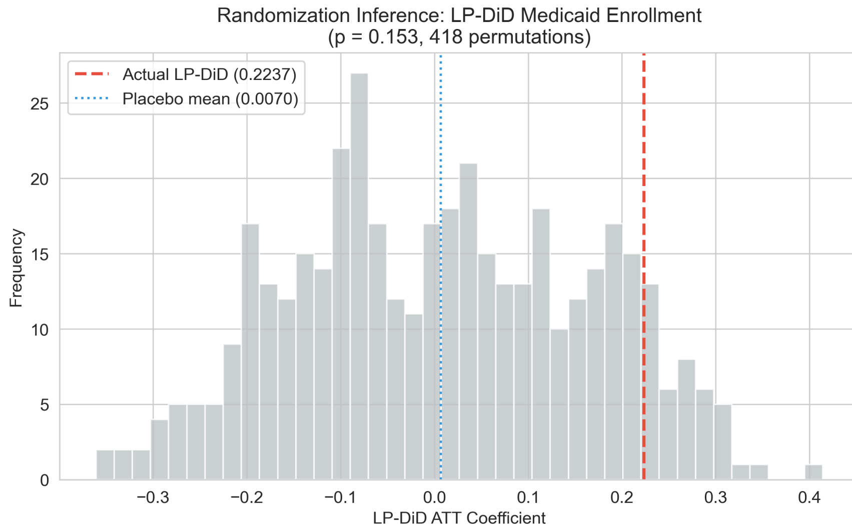

Figure 27: A13

Note: This figure plots event-time estimates for the revision m3 ri lpdid. Points show period-specific effects relative to the omitted reference period, with uncertainty intervals where reported.

Burns, M. E., S. Cook, L. M. Brown, L. Dague, S. Tyska, K. Hernandez Romero, C. McNamara, and R. P. Westergaard. 2022. "Association Between Assistance With Medicaid Enrollment and Use of Health Care After Incarceration Among Adults With a History of Substance Use." *JAMA Network Open* 5 (1): e2142688.

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