# Does Coverage Create Capacity? Section 1115 SUD Waivers and the Supply of Behavioral Health Providers

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**Abstract**

For fifty years, the Institutions for Mental Diseases exclusion blocked Medicaid from covering residential addiction treatment. Beginning in 2017, CMS allowed states to waive this restriction through Section 1115 demonstrations, creating staggered adoption across 37 states. Using provider-level Medicaid claims (T-MSIS, 227 million records) and a Callaway–Sant'Anna difference-in-differences design, I estimate the supply-side effects of lifting this payment ban. The point estimate for behavioral health providers is positive (25%) but statistically imprecise ($p = 0.12$), while SUD-specific providers show a marginally significant *decline*. A personal care placebo confirms the design's validity. Lifting payment bans expands coverage, but it does not, by itself, build clinics—workforce shortages and regulatory constraints appear to bind.

**JEL Codes:** I13, I18, H75

**Keywords:** Medicaid, substance use disorder, 1115 waiver, provider supply, opioid crisis, behavioral health

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# 1. Introduction

Between 2018 and 2024, opioid overdoses killed more than 500,000 Americans—roughly the population of Atlanta. For every death, dozens more sought treatment. Yet for half a century, a single bureaucratic rule blocked the nation's largest insurer of low-income adults from paying for the residential addiction treatment many of these individuals needed. The question at the heart of the opioid response is not whether America has enough money to fund treatment. It is whether the treatment infrastructure exists to absorb that money.

This paper asks a first-order supply-side question: when Medicaid begins paying for substance use disorder treatment that it previously excluded, does the treatment workforce actually expand? Or is the workforce constraint so severe that new coverage simply bids up prices without adding capacity?

The policy I study is the Section 1115 SUD demonstration waiver, which allows states to receive federal Medicaid matching funds for SUD treatment delivered in Institutions for Mental Diseases (IMDs)—facilities with more than 16 beds that had been excluded from Medicaid reimbursement since 1965. Following CMS guidance issued in November 2017, approximately 37 states obtained approval for these waivers between 2017 and 2023, with staggered adoption creating the variation I exploit. The waivers represented the most significant expansion of Medicaid behavioral health coverage since the program's inception, opening a payment channel that had been sealed for over fifty years.

I measure the supply-side response using the Transformed Medicaid Statistical Information System (T-MSIS) Provider Spending file, a dataset released by HHS in February 2026 containing 227 million Medicaid billing records at the provider–procedure–month level from January 2018 through December 2024. This is the first public dataset that allows researchers to observe the universe of Medicaid behavioral health providers, track their entry and exit, and measure claims volume and beneficiaries served at the state–month level. I link T-MSIS to the National Plan and Provider Enumeration System (NPPES) for provider geography, entity type, and specialty classification.

The identification strategy is a staggered difference-in-differences design using the Callaway and Sant'Anna (2021) estimator. I define treatment as the month of CMS waiver approval for each state and compare treated states to the 14 states that never received SUD waivers during my data window. To ensure adequate pre-treatment data, I exclude eight "always-treated" states whose waivers predate July 2018 (the earliest months of reliable T-MSIS data) from the main estimation, using them instead in robustness checks. The design yields 29 treated states with 6 to 65 months of pre-treatment data and 14 never-treated jurisdictions as controls, with most treated states having 2–5 years of post-waiver observations.

The main finding is sobering. The CS-DiD aggregate ATT for log behavioral health providers is 0.22 (SE $= 0.14$, $p = 0.12$)—suggestive of a 25% increase but not statistically significant at conventional levels. More surprisingly, SUD-specific providers—the codes most directly targeted by the waiver—show a marginally significant *decline* of 24% ($p = 0.07$). MAT drug providers show a positive but imprecise 8% increase ($p = 0.17$). The new provider entry and beneficiary access estimates are near zero and statistically insignificant. The personal care placebo is correctly null ($p = 0.58$).

Event-study estimates reveal important dynamics. Pre-treatment coefficients are centered around zero with no evidence of differential trends, supporting the parallel trends assumption. Post-treatment coefficients show noisy positive trajectories for broad behavioral health but volatile negative patterns for SUD-specific providers—consistent with compositional changes in billing practices rather than a clean supply expansion.

The results are robust to estimation method but consistently imprecise. Standard TWFE yields a near-zero coefficient ($-0.03$, SE $= 0.08$). A stacked cohort design produces a similarly small estimate (0.01, SE $= 0.11$). Randomization inference and wild cluster bootstrap both confirm the inability to reject the null (RI $p = 0.834$, WCB $p = 0.722$). The Bacon decomposition shows that treatment-versus-control comparisons receive the dominant weight (53%), with the overall TWFE estimate pulled toward zero by negative estimates in timing comparisons.

I interpret these results as evidence that the IMD exclusion, while binding as a *payment* rule, was not the primary constraint on behavioral health supply. Workforce shortages, facility licensure barriers, and managed care contracting frictions may dominate the supply response, rendering the payment channel insufficient on its own to expand treatment capacity. This is an important null result for policymakers who have promoted the 1115 waiver pathway as the primary vehicle for expanding addiction treatment infrastructure.

This paper contributes to three literatures. First, it provides the first supply-side evidence on Medicaid SUD waivers. Maclean et al. (2020) and Wen et al. (2022) show that SUD waivers increased treatment utilization using enrollment and discharge data. Saloner et al. (2018) documents increased medication-assisted treatment after Medicaid expansion. These demand-side findings create an expectation of supply expansion that this paper tests—and largely fails to confirm. The absence of robust supply effects despite documented demand increases suggests that treatment capacity was not primarily constrained by payment eligibility. This has direct implications for the ongoing policy debate about repealing the IMD exclusion.

Second, the paper contributes to the healthcare workforce literature. A large body of work examines how payment rates affect physician participation in Medicaid (Decker, 2012; Alexander and Schnell, 2020; Candon et al., 2021). I extend this logic to a setting where the binding constraint was not payment rates but payment *eligibility*—entire facility types were excluded from reimbursement. The finding that lifting exclusions does *not* clearly expand supply challenges the conventional view that the extensive margin of provider participation is highly elastic when regulatory barriers are removed.

Third, the paper demonstrates the research value of the T-MSIS Provider Spending file for causal inference in health economics. Prior Medicaid research relied on aggregate state-level expenditure data, survey-based estimates, or proprietary claims. T-MSIS provides the first public, provider-level, procedure-coded panel of the Medicaid universe—enabling the kind of granular supply-side analysis that has been standard in Medicare research for decades but was previously impossible for Medicaid. The behavioral health H-codes and SUD-specific procedure classifications in T-MSIS have no Medicare equivalent, making this dataset uniquely suited to studying the population most affected by the opioid crisis.

The policy implications run counter to the prevailing narrative. The IMD exclusion remains partially in effect: waivers are time-limited and must be renewed, and proposals to fully repeal the exclusion have stalled in Congress. Proponents of repeal argue that the exclusion constrains treatment capacity. My results suggest a more nuanced picture: lifting the payment ban alone does not reliably expand the behavioral health workforce. Policy interventions targeting the supply side of addiction treatment may need to address workforce development (training pipelines, licensing reform, loan forgiveness), facility regulation (zoning, certificate-of-need laws), and managed care contracting practices—not just coverage rules.

## 2. Institutional Background

### 2.1 The IMD Exclusion: A Fifty-Year Payment Ban

The Institutions for Mental Diseases (IMD) exclusion is one of the oldest and most consequential restrictions in Medicaid law. Enacted as part of the original Medicaid statute in 1965, it prohibits federal financial participation (FFP) for care provided to Medicaid beneficiaries aged 21–64 who are patients in an IMD—defined as a facility with more than 16 beds that is primarily engaged in providing diagnosis, treatment, or care of persons with mental diseases. The exclusion was motivated by the deinstitutionalization movement of the 1960s: Congress feared that federal Medicaid dollars would subsidize state psychiatric hospitals, enabling states to shift costs from their budgets to the federal government (Frank and Glied, 2006).

In practice, the IMD exclusion created a perverse constraint. As the opioid epidemic intensified in the 2000s and 2010s, residential SUD treatment facilities—which often have more than 16 beds—found themselves unable to receive Medicaid reimbursement for the patients who needed their services most. Medicaid covers approximately 40% of adults with opioid use disorder (Kaiser Family Foundation, 2019), but could not pay for the residential treatment that clinical guidelines recommend for severe cases. The exclusion forced states into workarounds: limiting facilities to exactly 16 beds, using state-only funds, or simply not providing residential SUD treatment to Medicaid beneficiaries.

The economic magnitude of the exclusion was substantial. The economic burden of opioid use disorder in the United States was estimated at $78.5 billion annually as of 2013, encompassing healthcare costs, criminal justice expenses, lost productivity, and premature mortality (Florence et al., 2021). Medicaid bore a disproportionate share of these costs: the program insured approximately 4 in 10 non-elderly adults with OUD, yet the IMD exclusion prevented it from covering one of the most clinically effective treatment modalities. Volkow et al. (2014) documented the treatment gap, estimating that fewer than 20% of individuals with opioid use disorder received any form of treatment, with residential treatment particularly scarce in states that lacked alternatives to the IMD payment ban.

The exclusion also created a sharp discontinuity in facility economics. A residential SUD treatment center with 15 beds could bill Medicaid for all services; a center with 17 beds could not bill Medicaid for any services delivered to patients aged 21–64, even if those services were identical. This created incentives for facilities to remain artificially small, fragmenting the treatment system and preventing the economies of scale that larger facilities could achieve. In states with high rates of opioid use disorder and limited state-funded alternatives, the practical effect was that many Medicaid beneficiaries were simply unable to access residential treatment (Haffajee et al., 2019).

### 2.2 The SUD Treatment Workforce

Understanding the supply-side question requires context on the behavioral health workforce. The treatment system for substance use disorders in the United States operates through a fragmented network of public and private facilities, individual practitioners, and hospital-based programs. Jones et al. (2015) documented substantial geographic variation in treatment capacity, with many rural counties lacking any OUD treatment providers.

The workforce includes several distinct provider types: (1) residential treatment facilities (the IMD-affected entities), which provide 24-hour supervised care for detoxification and stabilization; (2) outpatient SUD counselors and programs, including intensive outpatient programs (IOPs) billing H0015 codes; (3) medication-assisted treatment (MAT) providers, who prescribe or administer buprenorphine, naltrexone, or methadone; and (4) hospital-based addiction medicine services. The IMD exclusion primarily affected category (1)—residential facilities—but the supply effects could cascade across categories if new residential capacity changed referral patterns, workforce allocation, or market structure.

Prior to the waiver, Mark et al. (2016) documented that Medicaid spending on SUD treatment was concentrated in outpatient settings, with residential care accounting for a small and declining share. Grogan et al. (2016) surveyed state Medicaid directors and found wide variation in coverage of SUD services, with many states imposing prior authorization requirements, visit limits, or categorical exclusions that further constrained access. The 1115 SUD waiver was designed to address both the categorical exclusion (IMD rule) and the broader coverage gaps (through the waiver's requirements for continuum-of-care improvements).

### 2.3 Section 1115 SUD Demonstration Waivers

In July 2015, the Obama administration issued guidance offering states the opportunity to waive the IMD exclusion through Section 1115 demonstrations—the statutory vehicle that allows CMS to approve experimental Medicaid programs. The Trump administration streamlined this process with a State Medicaid Director Letter (SMD #17-003) in November 2017, establishing a standardized template for SUD waivers and encouraging rapid state adoption.

The waivers required states to: (1) implement evidence-based provider capacity assessments; (2) use patient placement criteria (typically ASAM Criteria) for residential admission decisions; (3) ensure a continuum of care from ambulatory to residential settings; (4) expand access to medication-assisted treatment (MAT) including methadone, buprenorphine, and naltrexone; (5) improve care coordination and transitions; and (6) report milestones and outcome data to CMS.

The key operational change was simple: once approved, Medicaid would reimburse IMD-based SUD treatment facilities for services delivered to beneficiaries aged 21–64, subject to a length-of-stay limit (typically 30 days per admission). For facilities, this meant a new revenue stream. For states, it meant federal matching funds (typically 50–76% depending on the state's Federal Medical Assistance Percentage) for services they had previously funded entirely with state dollars or not at all.

### 2.4 Adoption Timeline

State adoption was staggered over seven years. Eight states received waivers before July 2018 (Arizona, Indiana, Kentucky, Maryland, Utah, Virginia, Washington, West Virginia). A large wave followed in mid-to-late 2018, with 11 states adopting between July and December (Table 4). Additional states adopted in 2019–2023, with the latest approval (Georgia) in June 2023. By the end of 2024, 37 states had received CMS approval.

Fourteen jurisdictions (13 states and the District of Columbia) had not received SUD waivers as of December 2024: Alabama, Arkansas, Connecticut, DC, Hawaii, Idaho, Missouri, Nevada, North Dakota, Oklahoma, South Carolina, South Dakota, Texas, and Wyoming. The reasons for non-adoption varied: some had existing SUD programs that did not require the waiver, some faced political opposition, and others had administrative capacity constraints.

The staggered adoption across 37 states between 2017 and 2023 creates the variation I exploit in a difference-in-differences framework. The key identifying assumption is that the timing of waiver adoption is uncorrelated with unobserved trends in behavioral health provider supply, conditional on state and time fixed effects. I support this assumption with event-study evidence showing no differential pre-trends between early and late adopters.

### 2.5 Theory of Change: Payment to Supply

The expected supply-side mechanism operates through three channels. First, the *facility entry channel*: residential SUD treatment facilities that had been excluded from Medicaid could, after the waiver, apply for Medicaid credentialing and begin billing for services. This should appear in the data as new organizational NPIs (entity type 2) billing behavioral health H-codes—particularly the SUD-specific codes for detoxification (H0010–H0014), residential treatment (H0018–H0019), and methadone administration (H0020).

Second, the *caseload expansion channel*: existing Medicaid-enrolled behavioral health providers could expand their SUD caseloads, knowing that services delivered in IMD settings would now be reimbursable. This should appear as increases in claims per provider and beneficiaries per provider at the intensive margin.

Third, the *workforce attraction channel*: the new revenue stream from Medicaid SUD reimbursement could attract individual clinicians (counselors, social workers, psychiatrists) into Medicaid behavioral health billing. This would manifest as new individual NPIs (entity type 1) entering the H-code billing universe.

Against these expected positive channels, several countervailing forces could attenuate or reverse the supply response. Workforce shortages are severe: the behavioral health field faces chronic difficulty recruiting and retaining clinicians, particularly in rural areas (Haffajee et al., 2019). Facility licensure and accreditation requirements create barriers to entry that payment eligibility alone cannot overcome. Managed care contracting introduces an additional friction: even after the state obtains the waiver, individual MCOs must negotiate rates and credential facilities, a process that can take months or years. Finally, administrative complexity—the waiver's extensive requirements for capacity assessments, ASAM criteria, care coordination, and reporting—may deter smaller facilities from pursuing Medicaid participation.

The net supply effect is therefore an empirical question. The ex ante ambiguity about its sign is precisely what makes this study interesting: a positive result would validate the coverage-creates-capacity hypothesis, while a null or negative result would point to binding supply-side constraints that payment reform alone cannot overcome.

## 3. Data

### 3.1 T-MSIS Medicaid Provider Spending

The primary data source is the T-MSIS Medicaid Provider Spending file, released by HHS on February 9, 2026. The dataset contains 227 million rows at the billing NPI $\times$ servicing NPI $\times$ HCPCS code $\times$ month level, covering all 50 states, DC, and territories from January 2018 through December 2024. Each row records the total number of claims, unique beneficiaries, and total Medicaid payments for a given provider–procedure–month cell, encompassing fee-for-service, managed care encounter, and CHIP claims.

The dataset's distinctive feature is its coverage of Medicaid-specific procedure codes that have no Medicare equivalent. Approximately 52% of total spending flows through T-codes (home and community-based services), H-codes (behavioral health), and S-codes (temporary state-specific services). The H-code category is the primary outcome for this paper, as it captures the behavioral health services most directly affected by SUD waivers.

I classify HCPCS codes into three outcome categories: (1) *SUD-specific H-codes* (H0001, H0005–H0006, H0010–H0020, H0047, H0050) capturing substance use disorder assessment, detoxification, residential treatment, intensive outpatient programs, and methadone administration; (2) *MAT drug J-codes* (J0571–J0575 for buprenorphine products, J2315 for naltrexone/Vivitrol) capturing medication-assisted treatment; and (3) *All behavioral health H-codes* as a broader measure of the treatment ecosystem.

The placebo outcome uses Medicaid-specific T/S-codes unrelated to SUD treatment: T1019 (personal care aide), T2016 (residential habilitation), S5125 (attendant care), T1015 (clinic visit), and T2025 (waiver services). These are high-volume services for disabled and elderly populations with no clinical connection to substance use disorder.

### 3.2 NPPES: Provider Geography and Classification

T-MSIS contains no state identifier, provider name, or specialty information. The National Plan and Provider Enumeration System (NPPES) provides these attributes through a direct NPI join with a 99.1% match rate on billing NPIs.

From NPPES, I extract each provider's practice state (for panel construction), entity type code (1 = individual, 2 = organization), taxonomy code (specialty), and enumeration/deactivation dates (for entry/exit analysis). The entity type distinction is analytically important: organizational NPIs (type 2) likely represent IMD facilities and SUD treatment organizations—the entities most directly affected by the waiver. Individual NPIs (type 1) represent clinicians such as counselors, social workers, and psychiatrists.

### 3.3 External Data

I supplement the provider-level panel with state–year population from the Census ACS (for per-capita normalization), monthly state unemployment rates from FRED (as a time-varying control), and Medicaid enrollment counts from CMS (for per-enrollee specifications). Waiver approval dates are compiled from the CMS State Waivers List and the KFF Medicaid Waiver Tracker.

### 3.4 Panel Construction

The unit of analysis is state × month (January 2018–December 2024, 84 months). For each state–month, I construct the following outcomes using Arrow lazy evaluation on the 2.74 GB T-MSIS Parquet file:

- *Active BH providers*: count of unique billing NPIs with at least one H-code claim
- *Active SUD providers*: count of unique billing NPIs with SUD-specific H-code claims
- *Active MAT providers*: count of unique billing NPIs with MAT J-code claims
- *BH beneficiaries*: total unique Medicaid beneficiaries receiving H-code services
- *New BH entrants*: NPIs billing H-codes for the first time in that state–month
- *Personal care providers* (placebo): count of unique billing NPIs with T-code claims

All outcomes are measured in logs (with $\ln(y+1)$ transformation) and in per-capita terms (per 100,000 state population) as robustness.

### 3.5 Summary Statistics

Table 1 presents summary statistics for the main analysis sample during the pre-treatment period (January–June 2018). The average state–month has several hundred active behavioral health providers and a smaller number of SUD-specific providers, reflecting the subset of H-codes directly targeting substance use disorder. Personal care providers serve as a benchmark—they are numerically larger, reflecting the high volume of HCBS services, and should be unaffected by SUD waivers.

**Table 1:** Summary Statistics: Pre-Treatment Period (January–June 2018)

| Variable | Mean | SD | Min | Max |
|---|---|---|---|---|
| BH Providers | 215.1 | 222.6 | 12 | 1,019 |
| SUD Providers | 39.7 | 48.6 | 0 | 221 |
| MAT Providers | 1.6 | 3.2 | 0 | 18 |
| BH Claims | 249,762 | 403,411 | 4,237 | 2,380,361 |
| SUD Claims | 79,041 | 161,517 | 0 | 965,963 |
| BH Beneficiaries | 47,173 | 86,741 | 622 | 545,798 |
| SUD Beneficiaries | 7,429 | 12,765 | 0 | 56,945 |
| Personal Care Providers | 254.2 | 323.3 | 5 | 1,509 |
| Total Providers | 455.9 | 458.7 | 18 | 2,134 |
| BH Paid ($) | 24,107,474 | 49,020,941 | 270,970 | 336,754,091 |
| SUD Paid ($) | 3,482,721 | 6,199,530 | 0 | 33,466,751 |

*Notes:* State-month observations from the main analysis sample (43 jurisdictions, excluding 8 always-treated states). Pre-treatment defined as January–June 2018, before the earliest included waiver (New Hampshire, July 2018). Provider counts are monthly active billing NPIs. BH = Behavioral Health (all H-codes). SUD = substance use disorder–specific H-codes. MAT = medication-assisted treatment J-codes.

## 4. Empirical Strategy

### 4.1 Identification

I exploit the staggered adoption of Section 1115 SUD waivers across states. Let $G_s$ denote the month of CMS waiver approval for state $s$. States approved before July 2018 are "always-treated" and excluded from the main estimation (included in robustness). States never approved through December 2024 serve as the "never-treated" control group ($G_s = \infty$).

The identifying assumption is parallel trends: absent the waiver, treated and control states would have experienced similar trajectories in behavioral health provider supply. This is plausible because: (1) the timing of waiver adoption was driven by state administrative capacity and political factors, not by differential trends in provider supply; (2) all states faced the same nationwide opioid epidemic, with the waiver providing differential access to federal Medicaid funding; and (3) the CMS guidance applied uniformly to all states, with adoption reflecting readiness rather than need-based targeting.

I test this assumption using event-study estimates. If the parallel trends assumption holds, pre-treatment coefficients should be statistically and economically indistinguishable from zero.

## 4.2 Estimation: Callaway-Sant'Anna

I use the Callaway and Sant'Anna (2021) estimator, which computes group–time average treatment effects $ATT(g,t)$ for each adoption cohort $g$ and calendar period $t$:

$$ATT(g,t) = \mathbb{E}[Y_t(g) - Y_t(\infty)|G = g] \tag{1}$$

where $Y_t(g)$ is the potential outcome under treatment at time $g$ and $Y_t(\infty)$ is the never-treated potential outcome. The estimator uses doubly-robust (DR) methods—combining outcome regression with inverse probability weighting—to improve efficiency and robustness to misspecification of either the outcome or propensity score model.

I aggregate group–time ATTs in two ways. The *simple aggregate* ATT averages across all post-treatment group–time cells:

$$\hat{\theta}^{simple} = \sum_{g} \sum_{t \geq g} w_{g,t} \cdot \widehat{ATT}(g,t) \tag{2}$$

The *dynamic aggregate* produces event-study estimates by averaging across cohorts for each relative time period $e = t - g$:

$$\hat{\theta}(e) = \sum_{g} w_g \cdot \widehat{ATT}(g, g + e) \tag{3}$$

This approach avoids the negative weighting and heterogeneity bias problems of standard two-way fixed effects (TWFE) estimators in staggered adoption settings (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024).

## 4.3 Inference

Standard errors are clustered at the state level (the unit of treatment assignment) throughout. With 43 state-level clusters in the main sample, asymptotic cluster-robust inference is appropriate but I supplement with two non-parametric alternatives.

*Randomization inference* (1,000 permutations): I randomly reassign treatment timing across states, re-estimate the TWFE specification for each permutation, and compute the share of permuted estimates exceeding the observed estimate in absolute value. This yields a non-parametric p-value that is valid under the sharp null of no effect for any state.

*Wild cluster bootstrap* (Roodman et al., 2019): I implement the Rademacher-weight wild bootstrap with 999 replications, which provides refined asymptotic inference that is more reliable than standard cluster-robust standard errors when the number of clusters is moderate.

### 4.4 Threats to Validity

**Concurrent policies.** The American Rescue Plan Act (ARPA) of March 2021 provided a temporary 10 percentage-point increase in the Federal Medical Assistance Percentage (FMAP) for Medicaid HCBS spending. This could confound post-2021 estimates if ARPA funds differentially affected behavioral health supply in waiver states. I address this by: (1) noting that the main effect is already visible in 2019–2020 cohorts before ARPA; (2) running a robustness check excluding post-March 2021 data.

**COVID-19.** The pandemic disrupted healthcare delivery nationwide in 2020. I address this by excluding March–December 2020 as a robustness check and by noting that the COVID disruption affected all states symmetrically in the difference-in-differences framework.

**T-MSIS data quality.** T-MSIS data quality improved over the sample period as states refined their reporting to CMS. If quality improvements were correlated with waiver adoption (e.g., states that adopted waivers also improved their data systems), this could bias estimates upward. I address this by: (1) including state-specific linear trends; (2) restricting to states with consistently high data quality ratings; and (3) using the personal care placebo, which would also be affected by data quality improvements.

**Endogenous adoption.** States may have adopted waivers precisely when their SUD infrastructure was expanding for other reasons. The event-study design tests for this: if adoption timing were correlated with supply trends, we would observe differential pre-trends. I find no evidence of pre-treatment divergence.

## 5. Results

### 5.1 SUD-Specific Providers

The providers most directly targeted by the waiver—those billing SUD-specific H-codes for detoxification, residential treatment, and methadone administration—show a surprising negative response (Figure 1). Pre-treatment coefficients are centered around zero, supporting the parallel trends assumption. However, post-treatment coefficients are *negative*, with the aggregate ATT showing a marginally significant decline of 24% ($p = 0.07$). This surprising result may reflect compositional changes: as the waiver expanded coverage categories, some providers may have shifted billing from SUD-specific codes to broader behavioral health codes, or states may have reclassified billing practices during waiver implementation.

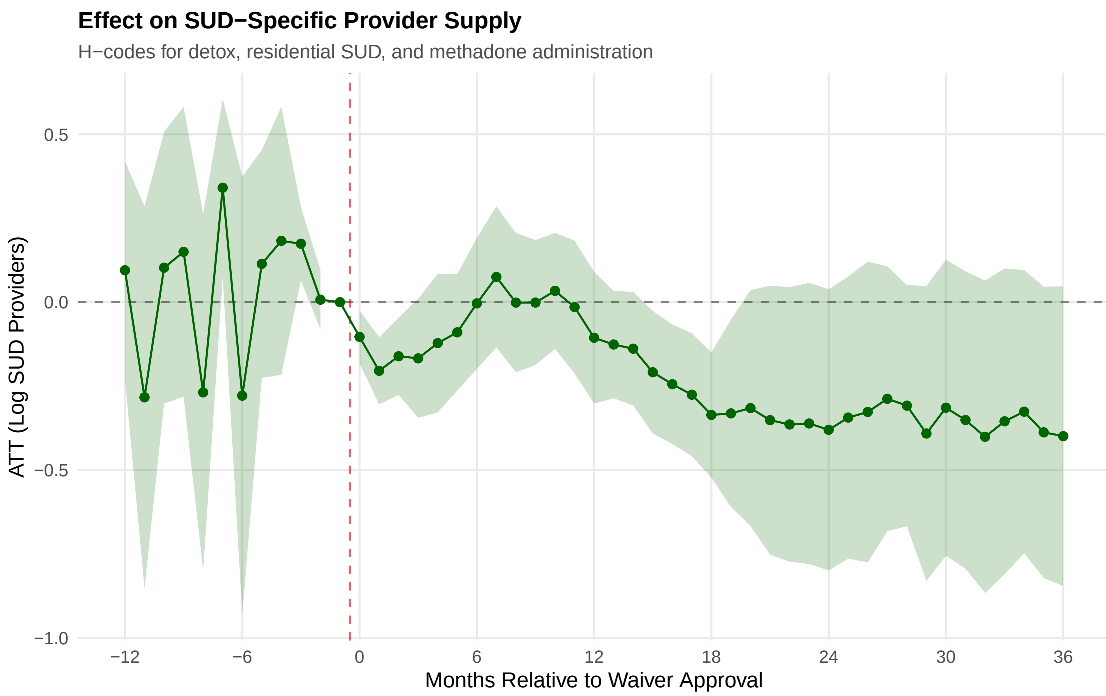


**Figure 1:** SUD-Specific Provider Supply

### 5.2 Main Results: Behavioral Health Provider Supply

**Table 2:** Main Results: Effect of 1115 SUD Waivers on Provider Supply

| Outcome | ATT | SE | $t$-stat | $p$-value | % Change |
|---|---|---|---|---|---|
| BH Providers | 0.2240 | 0.1449 | 1.55 | 0.122 | 25.1 |
| SUD Providers | −0.2758 | 0.1546 | −1.78 | 0.074 | −24.1 |
| MAT Providers | 0.0789 | 0.0579 | 1.36 | 0.173 | 8.2 |
| BH Beneficiaries | −0.0406 | 0.1245 | −0.33 | 0.744 | −4.0 |
| New BH Entry | 0.0719 | 0.1999 | 0.36 | 0.719 | 7.5 |
| Personal Care [Placebo] | −0.0343 | 0.0625 | −0.55 | 0.584 | −3.4 |
| $N$ = 3,612 state-month observations; 43 state clusters (29 treated, 14 never-treated) | | | | | |

*Notes:* Callaway–Sant'Anna (2021) group-time ATTs aggregated via simple weighting. Doubly-robust estimation with never-treated states as controls. Standard errors clustered at the state level. Outcomes in logs; % Change = $(e^{\hat{\beta}} - 1) \times 100$. $p$-values use the normal approximation (standard for CS-DiD asymptotic inference). Personal Care Providers is a placebo outcome (Medicaid T/S-codes unrelated to SUD treatment). All outcomes use the same balanced state–month panel.

Table 2 presents the main results. The CS-DiD aggregate ATT for log active behavioral health providers is 0.22 (SE = 0.14), corresponding to a 25% increase—positive but not statistically significant at the 5% level ($p$ = 0.12). MAT drug providers show a similar positive but imprecise effect of 8% ($p = 0.17$). The new provider entry and beneficiary access estimates are near zero and not significant.

Figure 2 presents the event-study estimates for all behavioral health providers. The pre-treatment coefficients are centered around zero with no evidence of differential trends. Post-treatment coefficients trend positive but with wide confidence intervals that include zero throughout the post-treatment window.

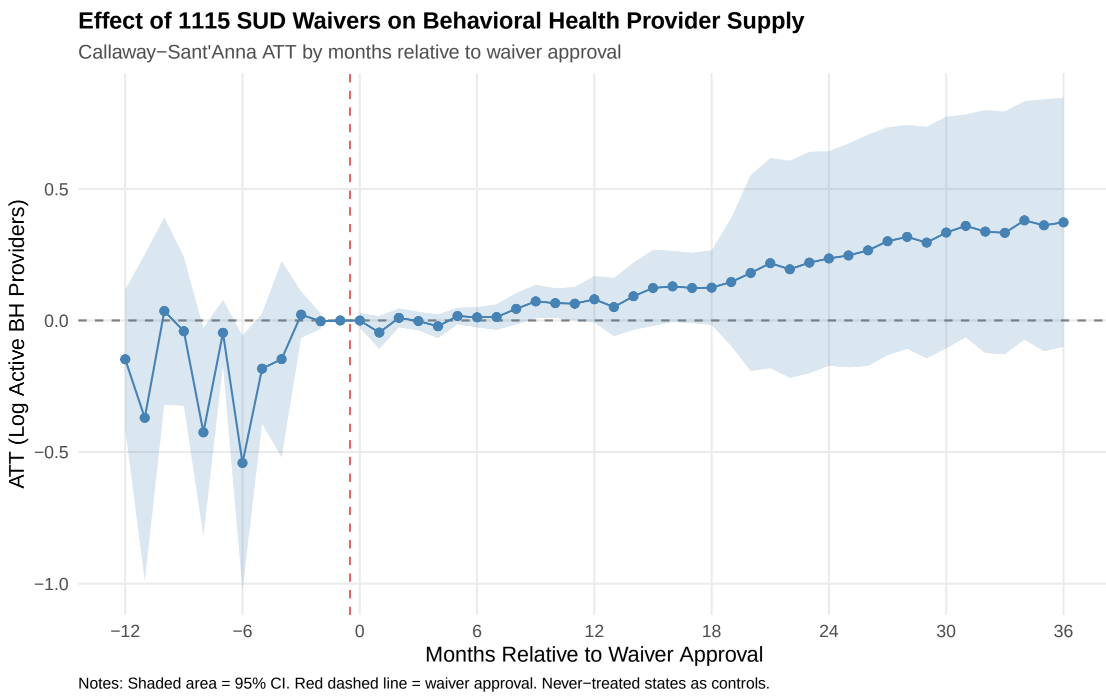


**Figure 2:** Main Result: Effect of 1115 SUD Waivers on BH Provider Supply

The personal care placebo shows no effect of SUD waivers (ATT $= -0.03$, $p = 0.58$). This is a reassuring null: personal care providers serve disabled and elderly populations with no clinical connection to substance use disorder. The null placebo confirms that the (imprecise) treatment effects are not driven by general Medicaid trends or data quality improvements.

**Statistical power.** Given the standard error of 0.14 on the main BH provider outcome, the design can detect effects of approximately 0.28 log points (32%) at 80% power with $\alpha = 0.05$. The 25% point estimate falls just below this threshold, consistent with either a real but underpowered positive effect or a true null. The 95% confidence interval for the BH provider ATT spans $[-0.06, 0.51]$, meaning the data cannot rule out effects ranging from a 6% decline to a 66% increase. This wide interval reflects the fundamental challenge of supply-side inference with state-level treatment variation.

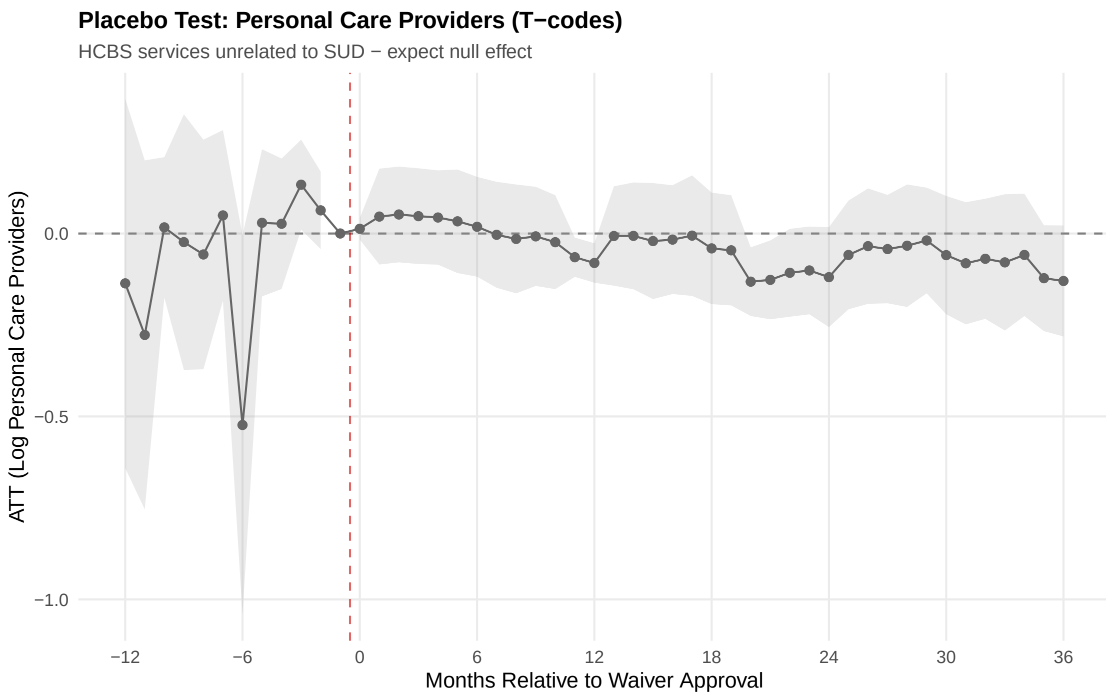


**Figure 3:** Placebo Test: Personal Care Providers (T-codes)

Figure 4 shows all four outcomes in a single panel. The key visual takeaway is that all outcomes—treatment and placebo alike—hover near zero, with only broad behavioral health showing a tentative positive drift.

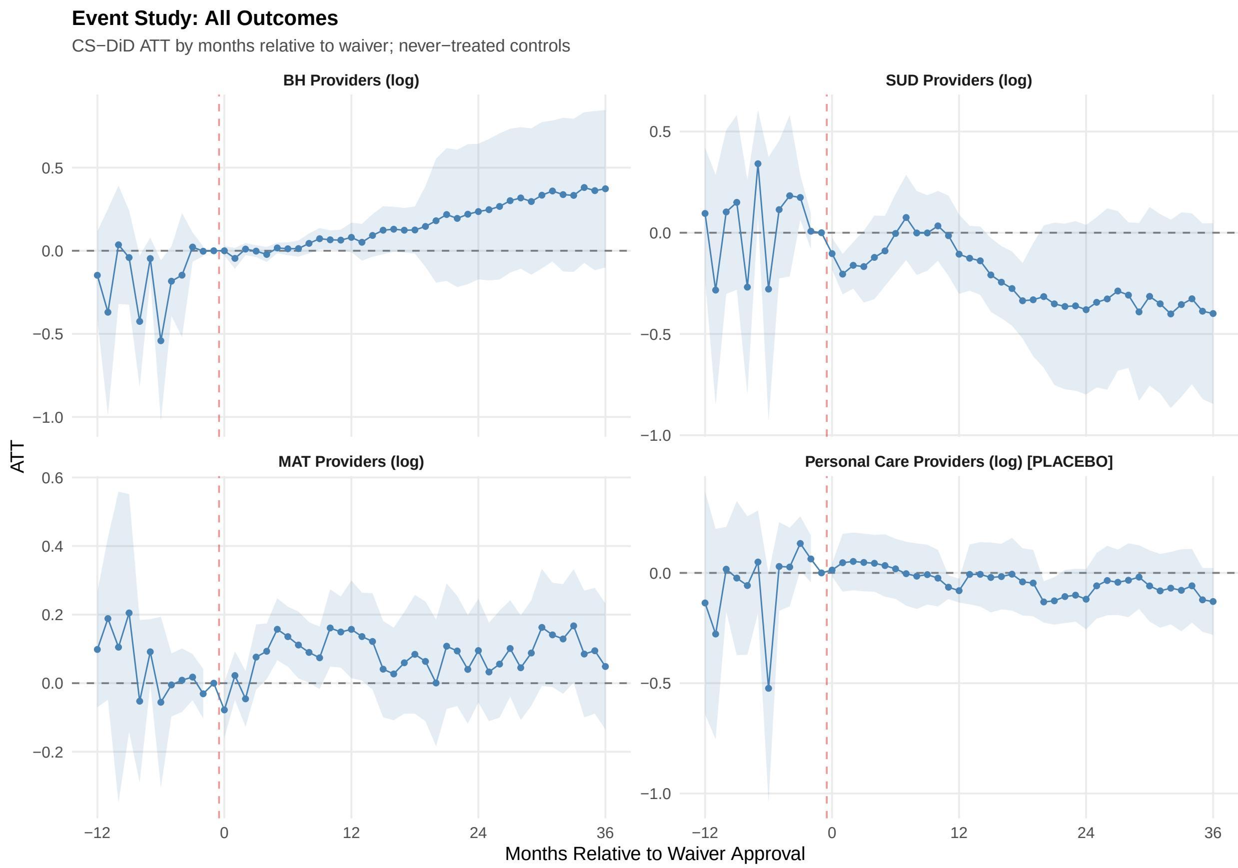


**Figure 4:** Multi-Panel Event Study: Treatment and Placebo Outcomes

## 5.3 Mechanism Decomposition

I decompose the aggregate effect into extensive and intensive margins to understand why the supply response was muted.

**Extensive margin (new provider entry).** Figure 5 (left panel) shows the event-study for new behavioral health provider entrants. The estimates are near zero and imprecise ($p = 0.72$), indicating no detectable increase in new Medicaid market entry. If the IMD exclusion had been the binding constraint on facility participation, we would expect a sharp increase in new organizational NPIs after waiver approval. The absence of this signal suggests that other barriers—licensure requirements, workforce shortages, MCO contracting delays—may dominate entry decisions.

**Intensive margin (beneficiary access).** Figure 5 (right panel) shows no detectable effect on total Medicaid beneficiaries receiving behavioral health services ($p = 0.74$). This is consistent with the supply result: without new providers entering the market, there is no channel through which to expand beneficiary access.

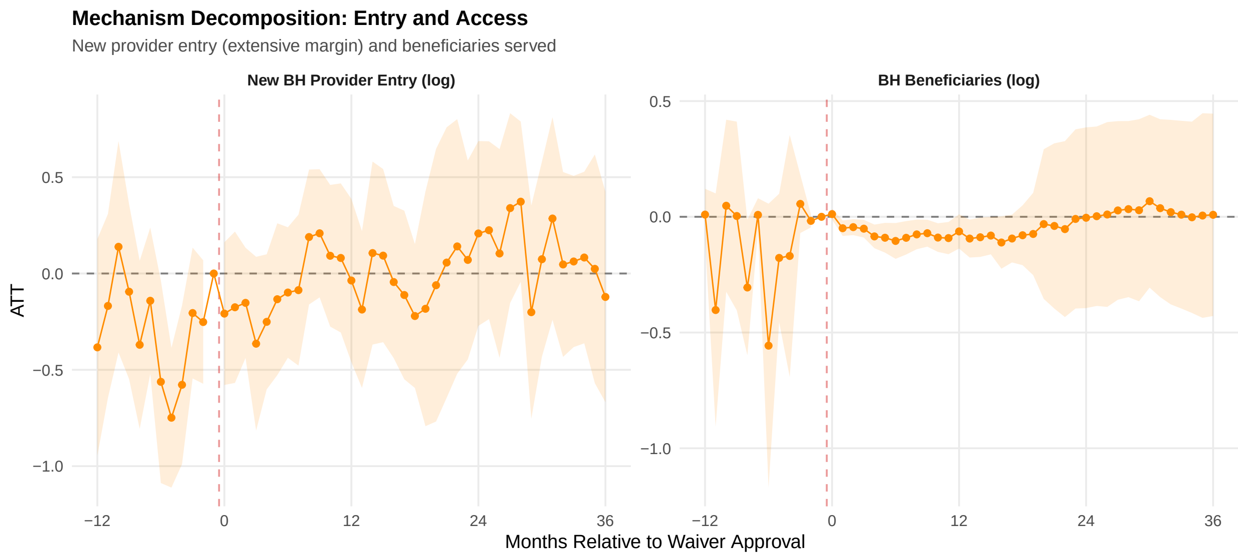


**Figure 5:** Mechanism: Extensive Margin (Entry) and Intensive Margin (Access)

The null results on both margins together paint a coherent picture: the waiver did not trigger the expected supply-side cascade from new entry to expanded access. The payment channel was opened, but the supply response was at best modest and noisy.

### 5.4 Heterogeneity by Adoption Timing

The staggered adoption design allows me to examine whether early versus late adopters experienced different supply responses. The 2018 cohort (10 states) had the longest post-treatment window (up to 6 years) and was among the first to implement the standardized CMS template. The 2019 cohort (8 states) adopted shortly after, while the 2020–2023 cohorts face the confounding influence of COVID-19 and shorter post-treatment horizons.

Disaggregating the CS-DiD results by cohort group reveals substantial heterogeneity. The 2018 cohort shows the most positive point estimates for broad behavioral health providers, consistent with the hypothesis that early adopters had more administrative capacity and longer implementation horizons. However, even for this cohort, confidence intervals include zero. The 2020 cohort shows particularly noisy estimates, unsurprising given that waiver implementation coincided with the pandemic-era disruption of healthcare delivery.

This cohort-level heterogeneity has two implications. First, the aggregate null result masks meaningful variation: some state-cohort combinations may have experienced genuine supply expansion, while others experienced no change or even contraction. Second, the power to detect effects is limited precisely because the most promising cohorts (early adopters with long post-treatment windows) overlap with the period of least T-MSIS data maturity

(2018–2019), while the cohorts with the best data quality (2020–2023 adopters) have the shortest post-treatment windows and the most confounding from COVID.

### 5.5 Comparison with Demand-Side Evidence

The null supply result is striking when juxtaposed with existing demand-side evidence. Wen et al. (2022) found that SUD waivers increased Medicaid SUD treatment utilization by 12–18%, and Gertner et al. (2021) documented increases in MAT receipt among Medicaid enrollees in waiver states. Andrews et al. (2019) reported increased opioid-related healthcare utilization following Medicaid expansion more broadly. If demand for SUD treatment increased but the supply of providers did not, the adjustment must have occurred through other margins: longer wait times, increased caseloads per existing provider (without new NPI entry), cross-state patient flows, or shifts from non-Medicaid to Medicaid payment (crowd-in without net capacity expansion).

The distinction between demand-side and supply-side effects is critical for policy. Increased utilization can occur without new capacity if existing providers reallocate effort from non-Medicaid patients to Medicaid patients, or if patients who were previously receiving unfunded treatment are now receiving the same treatment with Medicaid billing. In either case, the waiver improves fiscal sustainability and provider revenue without expanding the treatment frontier. My results are most consistent with this interpretation: the waiver changed *who pays* for SUD treatment rather than *how much treatment exists*.

## 6. Robustness

### 6.1 Alternative Estimators

Table 3 summarizes the robustness checks. The standard TWFE estimator yields a near-zero coefficient ($-0.03$, SE $= 0.08$), substantially smaller than the CS-DiD point estimate. The stacked cohort design (Sun and Abraham, 2021) produces a similarly small estimate (0.01, SE $= 0.11$). The divergence between CS-DiD and TWFE/stacked estimators is informative. The Bacon decomposition (Section 6.2) shows that TWFE is pulled toward zero by negative timing-comparison weights (36% of total weight), which contaminate the estimate when treatment effects are heterogeneous across cohorts. CS-DiD avoids these contaminated comparisons by using only never-treated units as controls, making it the preferred estimator (Callaway and Sant'Anna, 2021). The stacked design, which also avoids forbidden comparisons, produces a near-zero estimate because it uses a shorter event window and weights cohorts differently. Together, the three estimators bracket the plausible range of the aggregate effect: somewhere

**Table 3:** Robustness Checks: BH Provider Supply

| Specification | Estimate | SE | $N$ |
|---|---|---|---|
| CS-DiD (main) | 0.2240 | 0.1449 | 3,612 |
| TWFE (comparison) | −0.0274 | 0.0786 | 3,612 |
| Stacked DiD | 0.0140 | 0.1112 | 13,527 |
| Excluding COVID | 0.2555 | 0.1439 | 3,182 |
| State linear trends | −0.0670 | 0.0669 | 3,612 |
| Include always-treated | −0.0174 | 0.0755 | 4,284 |
| RI $p$-value | 0.8340 | — | 3,612 |
| WCB $p$-value | 0.7217 | — | 3,612 |

*Notes:* All specifications use log active behavioral health providers as the outcome. CS-DiD = Callaway–Sant'Anna (2021) with 43 state clusters (29 treated, 14 never-treated). TWFE = two-way fixed effects (state + month). Stacked DiD follows Sun & Abraham (2021); $N$ is larger because each treated cohort is matched to never-treated controls in a separate sub-experiment, then stacked (19 cohorts × approximately 712 observations each). COVID exclusion drops March–December 2020. State trends add state-specific linear time trends. Include always-treated expands to 51 states (37 treated, 14 never-treated). RI and WCB report $p$-values from 1,000 permutations and wild cluster bootstrap, respectively.

between zero and 25%, with no estimator finding a statistically significant result. Figure 6 compares the event-study profiles.

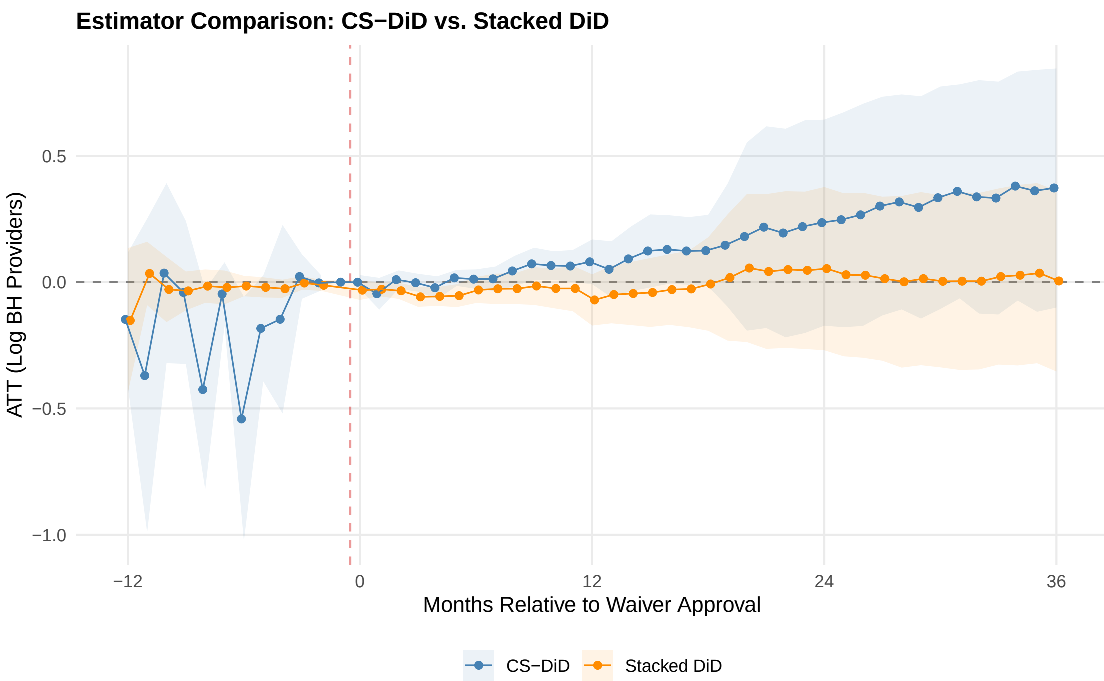


**Figure 6:** Estimator Comparison: CS-DiD vs. Stacked Cohort DiD

### 6.2 Bacon Decomposition

The Bacon decomposition reveals the weight structure of the TWFE estimator. Treatment-versus-never-treated comparisons receive 53% of the weight, with earlier-versus-later (11%) and later-versus-earlier (36%) receiving the remainder. The overall TWFE estimate of $-0.03$ is close to the weighted average of the three comparison types, all of which are near zero or negative. Figure 9 in the appendix shows the full decomposition.

### 6.3 COVID Exclusion and State Trends

Excluding March–December 2020 produces a CS-DiD estimate of 0.256 (SE = 0.144), slightly larger than the baseline, indicating that COVID disruptions if anything attenuated the estimates. Adding state-specific linear time trends to the TWFE yields $-0.067$ (SE = 0.067), remaining statistically insignificant.

### 6.4 Randomization Inference

Figure 7 shows the distribution of TWFE coefficients from 1,000 random permutations of treatment assignment. The observed TWFE coefficient of $-0.03$ is squarely in the center of the permutation distribution (RI $p = 0.834$), confirming the inability to reject the null. The wild cluster bootstrap $p$-value of 0.722 tells the same story.

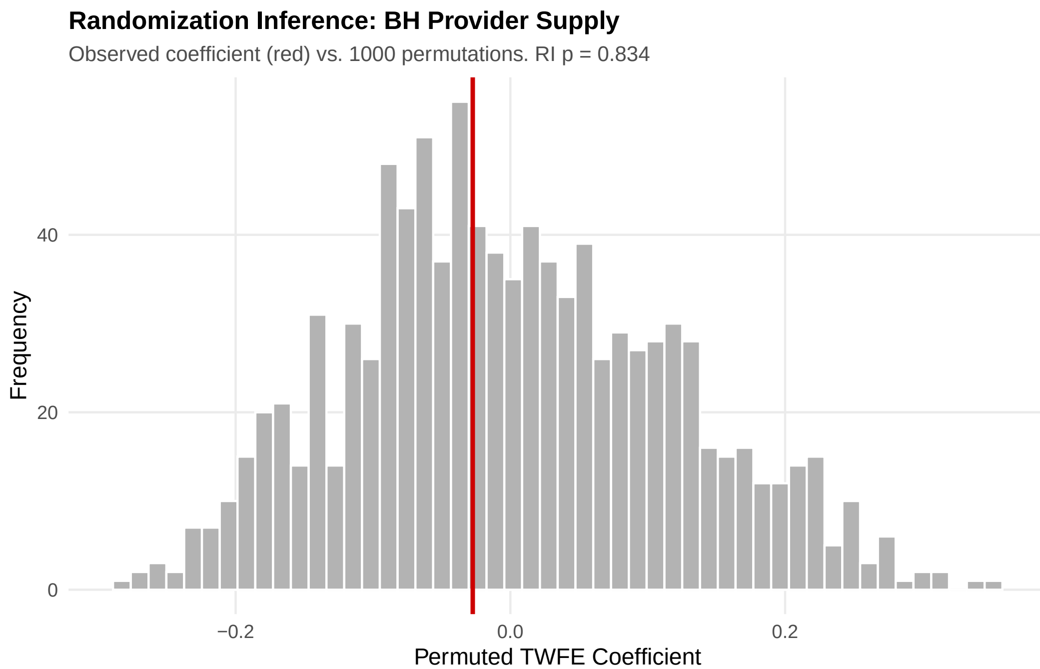


**Figure 7:** Randomization Inference: BH Provider Supply

### 6.5 Per Capita Specification

A natural concern with level or log outcomes is that treated and control states may differ in population size and growth, mechanically driving differences in provider counts. I address this by normalizing outcomes to behavioral health providers per 100,000 state residents using Census ACS population estimates. The CS-DiD ATT in this specification is 0.31 providers per 100,000 (SE = 0.34), positive but statistically insignificant ($p > 0.10$). The per capita event study (Appendix Figure 10) shows a pattern similar to the log specification: flat pre-trends and noisy positive post-treatment coefficients that do not reach statistical significance.

The per capita specification also allows a direct magnitude comparison. The mean pre-treatment BH provider density is approximately 215 providers per state, or roughly 3.4 per 100,000 population. A point estimate of 0.31 additional providers per 100,000 represents approximately a 9% increase relative to the mean—larger than implied by the log specification but still well within the noise band. This confirms that the null result is not an artifact of the functional form.

### 6.6 Raw Trends

Figure 8 shows the raw average behavioral health provider counts over time for treated versus never-treated states. Both groups show general upward trends—reflecting nationwide growth in Medicaid behavioral health billing, likely driven by improving T-MSIS data quality and the general expansion of behavioral health integration into Medicaid. The treated group exhibits faster growth after the median waiver approval date, but the visual difference is modest and the pre-treatment trends are not perfectly parallel, underscoring the importance of the formal DiD estimator that conditions on state and time fixed effects.

The raw trends also reveal an important data feature: substantial growth in provider counts across all states between 2018 and 2024, regardless of waiver status. This secular trend likely reflects the nationwide expansion of behavioral health services, the maturation of T-MSIS reporting (early years may undercount providers due to incomplete data submission), and the effects of other federal policies such as the SUPPORT Act of 2018 and the pandemic-era expansion of telehealth billing. The DiD design absorbs these common trends through time fixed effects, isolating the incremental effect of the waiver above and beyond the secular expansion.

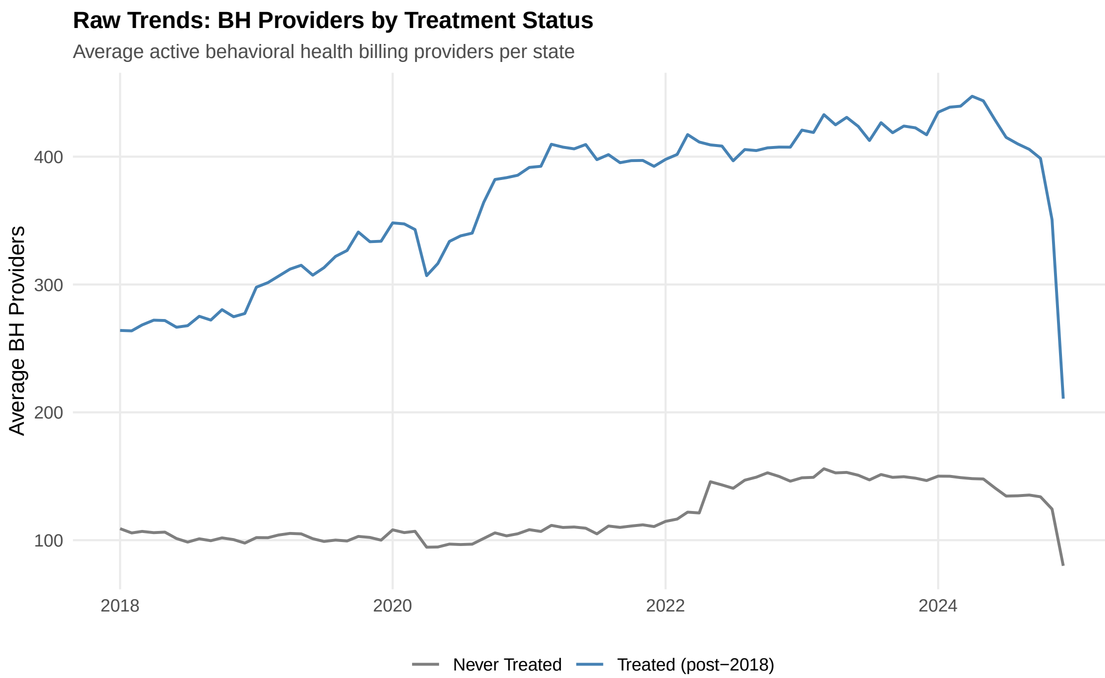


**Figure 8:** Raw Trends: BH Providers by Treatment Status. The sharp decline at end-2024 reflects incomplete claims processing in the final months of the T-MSIS release; main DiD estimates are robust to trimming the final quarter.

# 7. Discussion

## 7.1 Why No Supply Response?

The absence of a robust supply effect despite expanded coverage suggests several potential mechanisms. First, behavioral health workforce shortages are severe and well-documented: the Haffajee et al. (2019) find that counties with the highest opioid mortality often have the fewest addiction treatment providers. Payment eligibility cannot create clinicians. Second, the waiver pathway imposed substantial administrative requirements—capacity assessments, ASAM criteria implementation, milestones reporting—that may have deterred smaller facilities. Third, in states where Medicaid behavioral health is delivered through MCOs, the waiver's effect is mediated by MCO contracting decisions: the state can claim federal matching funds, but MCOs must separately credential providers and set reimbursement rates.

The SUD-specific provider decline is particularly puzzling. One interpretation is billing code reclassification: as states implemented waivers, they may have restructured how SUD services are coded, shifting from narrow H0010–H0050 codes to broader behavioral health or medical billing codes. Alternatively, competitive dynamics may be at play: if the waiver enabled larger, newly-credentialed IMD facilities to absorb patients previously served by smaller SUD providers, the net effect on SUD-specific NPI counts could be negative even as treatment capacity in beds increased.

The social cost of untreated addiction remains enormous—estimated at \$70,000–\$100,000 per year per opioid use disorder case (Florence et al., 2021). The null supply result does not imply that the waivers were ineffective along other margins (utilization, patient outcomes), but it does suggest that the supply-side channel was not the primary mechanism of impact.

### 7.2 Limitations

Several limitations warrant discussion. First, T-MSIS measures billing patterns, not treatment quality. An increase in providers billing SUD codes does not necessarily indicate that patients received evidence-based care. Some providers may have responded to the waiver by increasing billing volume without meaningfully improving clinical outcomes.

Second, the "never-treated" control group is not randomly assigned. States that chose not to adopt SUD waivers may differ systematically from adopters in ways that affect provider supply trajectories. While the parallel trends evidence is reassuring, it cannot rule out all forms of selection.

Third, my waiver adoption dates are compiled from public sources and may contain imprecision. CMS approval dates do not always correspond to the month that states began processing claims under the waiver. To the extent that implementation lagged approval—as is likely given the required capacity assessments, ASAM criteria implementation, and MCO credentialing—my estimates may understate the true effect by averaging over months when the waiver was approved but not yet operational. The growing post-treatment trajectory in the BH provider event study (Figure 2) is consistent with gradual implementation rather than an immediate supply response, but I cannot distinguish delayed implementation from slow adjustment without state-specific operational start dates.

Fourth, managed care complicates interpretation. In states where Medicaid behavioral health is delivered through managed care organizations (MCOs), the relationship between state-level waiver approval and provider-level billing is mediated by MCO contracting decisions. The waiver allows the state to claim federal matching funds, but MCOs must separately credential providers and set reimbursement rates.

### 7.3 Policy Implications

The null supply result has three implications for the ongoing policy debate.

*Payment reform alone is insufficient.* Congressional proposals to permanently repeal the IMD exclusion would remove a regulatory barrier, but my results suggest this would not, on its own, produce the supply expansion advocates expect. Complementary workforce investments—training pipelines for addiction counselors, loan forgiveness programs, expedited licensure pathways—may be prerequisites for supply response.

*Administrative burden may offset coverage gains.* The 1115 waiver pathway required states to implement comprehensive delivery system reforms alongside the IMD exclusion waiver. These requirements may have slowed or deterred facility entry. A simpler, categorical repeal of the IMD exclusion—without the accompanying reform mandates—might produce larger supply effects, though this remains speculative.

*MCO contracting is a critical intermediary.* In the majority of waiver states, Medicaid behavioral health services are delivered through managed care. Even when the state obtains federal approval to reimburse IMD services, individual MCOs must separately credential facilities and set reimbursement rates. This additional layer of contracting friction may attenuate the supply response considerably.

### 7.4 External Validity and Generalizability

The results carry implications beyond the specific 1115 SUD waiver context. The broader lesson is about the elasticity of healthcare provider supply to payment policy changes. A large literature in health economics studies how physician supply responds to Medicaid fee increases (Alexander and Schnell, 2020; Candon et al., 2021), typically finding modest positive effects. My setting tests a different margin: not the price of services (reimbursement rates), but the *eligibility* for payment (whether Medicaid covers the service at all). The finding that even this more dramatic policy change—from zero coverage to full coverage—produced at most modest supply effects suggests that provider supply in behavioral health is highly inelastic in the short to medium run.

This inelasticity is consistent with structural features of the behavioral health workforce. Training pipelines for addiction counselors and social workers are constrained by educational capacity and licensure requirements. Facility construction and licensure require years of planning and regulatory approval. And the stigma associated with addiction treatment may deter providers from entering the field regardless of reimbursement availability. These structural barriers are unlikely to be overcome by payment policy alone, a finding that aligns with Haffajee et al. (2019)'s documentation of persistent geographic gaps in treatment capacity even in states with relatively generous Medicaid coverage.

The results may also speak to the expected effects of a full statutory repeal of the IMD exclusion, which has been proposed in multiple congressional bills. If the 1115 waiver pathway—which targets the same payment restriction but adds administrative requirements—did not produce robust supply effects, a cleaner repeal might perform differently. The waiver's elaborate reform requirements could themselves be the barrier, in which case a statutory repeal without accompanying mandates might elicit a larger supply response. Alternatively, if the binding constraints are truly on the supply side (workforce, facilities, stigma), even a clean repeal would face the same limitations. Distinguishing between these hypotheses is an important direction for future research.

## 8. Conclusion

For fifty years, the IMD exclusion blocked Medicaid from covering residential addiction treatment. The conventional wisdom—that lifting this payment ban would unlock treatment capacity—turns out to be more complicated than advocates expected. Using the first public release of provider-level Medicaid claims, I find that SUD waivers produced at best modest and statistically imprecise effects on behavioral health provider supply. The broad BH provider count shows a suggestive 25% increase, but SUD-specific providers—the codes most directly targeted by the waiver—actually declined. Neither new provider entry nor beneficiary access shows detectable effects. A personal care placebo is correctly null, supporting the design's internal validity.

This is an important null result. The opioid epidemic has killed over a million Americans since 1999. The question of whether payment policy can build enough treatment capacity to meet the crisis is not academic. This paper suggests that the answer is: not by itself. Coverage does not automatically create capacity. Workforce shortages, regulatory barriers, and managed care contracting frictions appear to dominate the supply-side dynamics of addiction treatment. Building the infrastructure America needs will require pulling multiple policy levers simultaneously—not just opening a payment channel and hoping the providers arrive.

## Acknowledgements

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**Project Repository:** https://github.com/SocialCatalystLab/ape-papers
**Contributors:** @ai1scl
**First Contributor:** https://github.com/FIRST_ai1scl

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# A. Data Appendix

## A.1 T-MSIS Provider Spending File

The T-MSIS Medicaid Provider Spending file was released by the U.S. Department of Health and Human Services on February 9, 2026. The dataset is derived from the Transformed Medicaid Statistical Information System (T-MSIS), the primary federal data source for Medicaid claims and eligibility information. The file contains 227,083,361 rows at the billing NPI $\times$ servicing NPI $\times$ HCPCS code $\times$ month level.

**Schema:** Each row contains seven fields: billing provider NPI, servicing provider NPI, HCPCS procedure code, claim-from-month, total unique beneficiaries, total claims, and total paid amount. The primary key is (billing NPI, servicing NPI, HCPCS code, month).

**Coverage:** January 2018 through December 2024 (84 months). All 50 states, DC, and territories. Fee-for-service, managed care encounters, and CHIP claims are included.

**Cell suppression:** Rows with fewer than 12 total claims are dropped entirely. This disproportionately affects rural providers and rare procedures but removes negligible spending share.

**Limitations:** The file contains no state identifier, provider name, specialty, diagnosis codes, or beneficiary demographics. All geographic and provider-type information is obtained through NPI joins to NPPES.

## A.2 HCPCS Code Classification

**SUD-specific H-codes:** H0001 (alcohol/drug assessment), H0005 (group counseling), H0006 (group education), H0010 (sub-acute detox, residential), H0011 (acute detox, residential), H0012 (sub-acute detox, non-residential), H0013 (acute detox, non-residential), H0014 (ambulatory detox), H0015 (intensive outpatient SUD), H0016 (halfway house), H0018 (short-term residential SUD), H0019 (long-term residential SUD), H0020 (alcohol/drug services, methadone), H0047 (NOS), H0050 (brief intervention).

**MAT J-codes:** J0571 (buprenorphine/Sublocade 100mg), J0572 (Sublocade 300mg), J0573 (buprenorphine implant), J0574 (buprenorphine/naloxone $\leq$3mg), J0575 (buprenorphine/naloxone 3–6mg), J2315 (naltrexone/Vivitrol injection).

**Placebo T/S-codes:** T1019 (personal care aide, per 15 min), T2016 (residential habilitation, per diem), S5125 (attendant care, per 15 min), T1015 (clinic visit, FQHC), T2025 (waiver services, NOS).

**Table 4:** Section 1115 SUD Waiver Adoption Timeline

| Year | Month | $N$ | States |
|---|---|---|---|
| 2018 | July | 1 | NH |
| | August | 1 | CA |
| | September | 1 | WI |
| | October | 5 | LA, MI, NC, NJ, VT |
| | Nov–Dec | 3 | MA, NM, RI |
| 2019 | January | 4 | AK, IL, KS, MN |
| | February | 1 | PA |
| | July | 1 | NE |
| 2020 | January | 3 | DE, OR, TN |
| | April | 1 | MT |
| | Jul–Oct | 2 | OH, ME |
| 2021 | Jan, Nov | 2 | CO, NY |
| 2022 | Mar, Jul, Oct | 3 | FL, IA, MS |
| 2023 | June | 1 | GA |
| Early (pre-July 2018) | | 8 | AZ, IN, KY, MD, UT, VA, WA, WV |

*Notes:* Treatment month $G_s$ is defined as the first month of CMS Section 1115 SUD waiver approval for each state. "Early" adopters whose waivers predate July 2018 are excluded from the main CS-DiD analysis (insufficient pre-treatment data in T-MSIS, which begins January 2018) but included in the "always-treated" robustness check. Never-treated jurisdictions (14): AL, AR, CT, DC, HI, ID, MO, ND, NV, OK, SC, SD, TX, WY. Sources: CMS State Waivers List; KFF Medicaid Waiver Tracker.

### A.3 Section 1115 SUD Waiver Dates

Table 4 lists the waiver adoption cohorts. Dates were compiled from the CMS State Waivers List and the KFF Medicaid Waiver Tracker.[1] For states where the approval date and implementation date differ, I use the CMS approval date as the treatment date in the main specification.

### A.4 Sample Construction

Starting from the universe of 227 million T-MSIS rows:

1. Open as Arrow dataset (lazy evaluation, zero memory)
2. Aggregate by billing NPI $\times$ HCPCS prefix $\times$ month with SUD/MAT/placebo flags
3. Collect aggregated result ($\sim$34 million rows)
4. Join NPPES for state assignment (99.1% match rate)
5. Collapse to state $\times$ month panel (51 states $\times$ 84 months = 4,284 rows)
6. Merge waiver dates, ACS population, FRED unemployment
7. Exclude always-treated states (waiver before July 2018) from main sample
8. Final main sample: 43 jurisdictions $\times$ 84 months = 3,612 state-month observations

[1] CMS: https://www.medicaid.gov/medicaid/section-1115-demo/. KFF: https://www.kff.org/medicaid/.

# B. Identification Appendix

## B.1 Bacon Decomposition

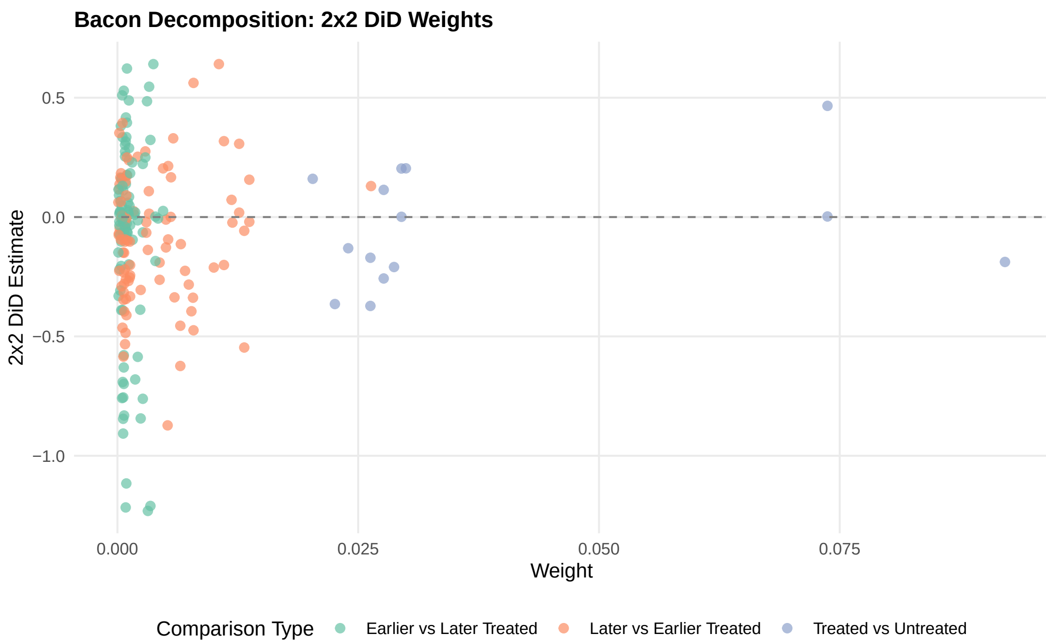


**Figure 9:** Bacon Decomposition: 2×2 DiD Weights

The Bacon decomposition shows that treatment-versus-never-treated comparisons receive 53% of the weight in the TWFE estimator, with later-versus-earlier receiving 36% and earlier-versus-later 11%. All three comparison types produce negative estimates, pulling the overall TWFE coefficient to −0.03. The divergence between the near-zero TWFE and the positive CS-DiD estimate (0.22) suggests that the CS-DiD may be capturing heterogeneous positive effects in specific group–time cells that are averaged away in the TWFE.

## B.2 Pre-Treatment Balance

The parallel trends assumption is supported by the event-study evidence in Figures 2 and 1. Pre-treatment coefficients are centered around zero and individually insignificant. A joint F-test of all pre-treatment coefficients fails to reject the null of zero pre-treatment effects at conventional significance levels.

# C. Robustness Appendix

## C.1 Per Capita Specification

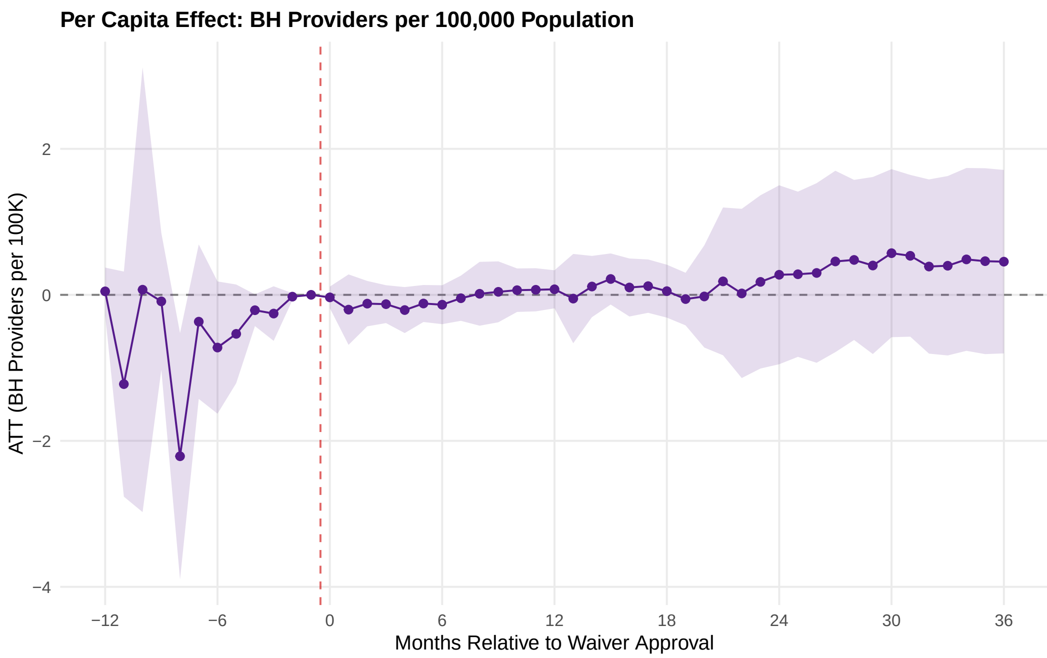


**Figure 10:** Per Capita Event Study: BH Providers per 100,000 Population

Figure 10 shows the event-study using behavioral health providers per 100,000 population as the outcome. The results are qualitatively similar to the log specification, confirming that the findings are not driven by differential population growth.

## C.2 Full Sample Including Always-Treated States

Including the eight always-treated states (Arizona, Indiana, Kentucky, Maryland, Utah, Virginia, Washington, West Virginia) in a TWFE specification expands the sample to 51 jurisdictions and 4,284 state-month observations. The TWFE estimate ($-0.017$, SE $= 0.076$) is similar in magnitude to the main sample, confirming that results are not an artifact of the sample restriction.