The Pharmacy Exit That Didn't Overflow: Chain Pharmacy Medicaid Billing Cessation and Emergency Department Use

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Abstract

Over 1,300 CVS, Walgreens, and Rite Aid pharmacies ceased Medicaid billing between 2018 and 2024, driven by opioid litigation and corporate distress. Linking T-MSIS Medicaid claims to NPPES provider data across 3,013 ZIP codes, I find that chain pharmacy exits cause large declines in injectable drug claims (-1.23 log points) with clean pre-trends and deepening effects over 12 months. Despite these sharp service reductions, emergency department utilization shows no detectable increase ($+0.02$, $SE = 0.04$), even in ZIPs losing their last chain pharmacy. The pharmacy desert is real; the feared ER overflow is not.

JEL Codes: I12, I13, I18

Keywords: pharmacy deserts, Medicaid, chain pharmacy closures, emergency department, health access

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1. Introduction

In October 2023, Rite Aid filed for Chapter 11 bankruptcy, announcing the closure of over 500 stores (Rite Aid Corporation, 2023). Walgreens followed with plans to shutter 1,200 locations by 2025 (Walgreens Boots Alliance, 2024). These closures joined a broader wave: between 2018 and 2024, more than 1,300 CVS, Walgreens, and Rite Aid pharmacies stopped billing Medicaid for injectable drug services. Nearly 29 million Americans depend on a single "keystone" pharmacy for prescription access (Mathis et al., 2025), and the affected neighborhoods are disproportionately low-income and minority (Guadamuz et al., 2021). The policy fear is straightforward: when the pharmacy disappears, patients skip medications, conditions deteriorate, and they end up in the emergency room.

This paper tests that fear directly. I exploit the unprecedented wave of chain pharmacy closures as a natural experiment, identifying the causal effect of pharmacy exit on both pharmaceutical utilization and emergency department (ED) use among Medicaid beneficiaries. The closures were driven by opioid litigation liabilities and national corporate restructuring—not by local neighborhood health conditions—providing plausibly exogenous variation in pharmacy access at the ZIP-code level.

The data infrastructure is novel. I link the newly released T-MSIS Medicaid Provider Spending data (Centers for Medicare & Medicaid Services, 2026b)—covering 227 million claim records from 2018 through 2024—to the NPPES provider registry (Centers for Medicare & Medicaid Services, 2026a) to identify chain pharmacy NPIs and track their billing patterns over time. A pharmacy that stops billing Medicaid claims is classified as closed. This billing-based measure captures de facto service withdrawal, not just corporate announcements. I construct a panel of 3,013 ZIP codes containing chain pharmacy NPIs: 1,036 that experienced at least one closure and 1,977 that did not.

The results are striking in their asymmetry. Chain pharmacy closures cause large, precisely estimated declines in injectable drug (J-code) claims: -1.23 log points ($p < 0.001$), equivalent to roughly a 70% reduction. Pharmacy beneficiary counts fall by a similar magnitude (-1.19 log points). Spending on pharmacy services drops by 1.65 log points. A binned event study confirms clean pre-trends—coefficients in the 7–12 and 12+ months before closure are indistinguishable from zero—followed by a sharp break at closure that deepens over 12 months to -1.51 log points.

Yet emergency department utilization barely moves. The point estimate on ED claims is 0.019 log points with a standard error of 0.035, allowing us to rule out ED increases larger than roughly 9%. This null persists in the most vulnerable neighborhoods: ZIP codes that lost their *last* chain pharmacy show a pharmacy claims decline of -1.30 log points but an

ED coefficient of -0.046 ($p = 0.25$). If anything, ED use in these areas slightly *declines*.

Three additional analyses reinforce these findings. First, I instrument for pharmacy closure using Rite Aid's pre-bankruptcy ZIP-level presence, exploiting the corporate distress that is orthogonal to local health conditions. The first-stage F-statistic is 65.4, confirming instrument relevance; the IV estimate for ED visits is noisy (0.93, $SE = 1.12$) but consistent with the null from the reduced form. Second, leave-one-chain-out exercises confirm that the results are not driven by any single chain (coefficients range from -1.23 to -1.30). Third, the effects are robust to inverse hyperbolic sine transformations, pre-COVID and post-COVID subsamples, and alternative definitions of the treatment period.

This paper contributes to three literatures. First, it advances the emerging evidence on pharmacy deserts (Qato et al., 2019; Hirth et al., 2021; Look and Mott, 2023; Alexander and Qato, 2020). While prior work documents adherence declines when pharmacies close (Erixson et al., 2023; Anderson and Doshi, 2020), no study has traced the chain from pharmacy exit to downstream acute care utilization using administrative claims data. The null on ED visits resolves a key policy uncertainty: the pharmacy desert creates medication gaps but does not, at least in the short to medium run, generate the ER overcrowding that policymakers fear.

Second, the paper contributes to the literature on Medicaid access and health care utilization (Finkelstein et al., 2012; Baicker et al., 2014; Currie and Madrian, 2010). The finding that Medicaid beneficiaries absorb pharmacy closures without detectable increases in acute care suggests either effective substitution toward independent pharmacies, medication stockpiling, or—less optimistically—silent non-adherence that manifests in health consequences beyond the 14-month post-period observable in these data.

Third, the paper demonstrates the research value of the T-MSIS Medicaid Provider Spending data, released in February 2026. By linking provider-level billing to NPPES geography and taxonomy, I construct the first ZIP-month panel of Medicaid pharmacy utilization and show that it can support credible causal inference on health care access questions.

2. Institutional Background

Chain pharmacies as dual-service infrastructure. CVS, Walgreens, and Rite Aid operate over 20,000 retail locations in the United States. These establishments simultaneously function as Medicaid-billing pharmacies and, in many cases, SNAP-authorized food retailers. When one closes, the neighborhood may lose both pharmaceutical and food access in a single event. In the NPPES data, I identify 21,732 chain pharmacy NPIs using the community/retail pharmacy taxonomy codes (3336C0003X, 332B00000X, 333600000X) matched by organization

name.

The closure wave. The wave of chain pharmacy closures beginning in 2018 was driven by three corporate-level forces: (1) opioid litigation settlements that imposed billions in liabilities on the major chains; (2) declining prescription margins due to pharmacy benefit manager consolidation; and (3) post-COVID store optimization as in-person traffic remained below 2019 levels. Rite Aid's October 2023 bankruptcy was the most dramatic event, but CVS and Walgreens had been quietly closing underperforming locations since 2019. Of the 4,225 chain pharmacy NPIs that billed Medicaid during 2018–2024, 1,365 (32%) ceased billing before the end of the observation period, with closure rates accelerating from 2020 onward.

Why closures are plausibly exogenous. The critical identification assumption is that chain pharmacy closures are not driven by local health conditions. Three institutional features support this claim. First, chain-wide restructuring decisions are made at the corporate level based on lease terms, litigation exposure, and portfolio optimization—not on ZIP-level Medicaid utilization patterns. Second, Rite Aid's bankruptcy closures were determined by opioid settlement terms negotiated with state attorneys general, creating variation that is orthogonal to local pharmacy demand. Third, the timing of closures within chains follows corporate restructuring timelines rather than local economic shocks. The binned event study provides direct evidence: pre-closure pharmacy and ED claims show no differential trends in the 12+ months before closure.

3. Data

I combine two administrative datasets to construct a ZIP-month panel spanning January 2018 through December 2024.

T-MSIS Medicaid Provider Spending. The T-MSIS data contain 227 million claim records at the billing NPI × servicing NPI × HCPCS code × month level, covering all Medicaid fee-for-service, managed care, and CHIP claims. Each record reports total claims, unique beneficiaries, and total Medicaid payments. I extract two sets of outcomes: (1) J-code claims (injectable drug administration), which capture pharmacy-dispensed injectable medications including medication-assisted treatment for opioid use disorder; and (2) ED visit claims (E/M codes 99281–99285), which capture emergency department encounters regardless of the billing provider.

NPPES Provider Registry. The National Plan and Provider Enumeration System provides the geographic link. Each NPI maps to a practice ZIP code, provider taxonomy, organization

Table 1: Summary Statistics: ZIP-Month Panel

	Treated ZIPs		Control ZIPs	
	Mean	(SD)	Mean	(SD)
Pharmacy claims (J-codes)	34.0	(95.9)	14.0	(85.2)
Pharmacy beneficiaries	29.0	(80.7)	11.7	(74.4)
Pharmacy spending ($)	1412.3	(15389.6)	1159.4	(22572.4)
ED visit claims	1435.4	(2422.5)	1759.2	(3288.8)
ZIP codes	1,036		1,977	
ZIP × months	54,171		68,603	

Notes: Unit of observation is ZIP code × month (January 2018–December 2024). Treated ZIPs experienced at least one chain pharmacy (CVS, Walgreens, or Rite Aid) billing cessation in T-MSIS during the sample period. Control ZIPs have chain pharmacy presence but no billing cessation. Pharmacy claims are Medicaid J-code (injectable drug) claims billed by chain pharmacy NPIs. ED visits are E/M codes 99281–99285 billed by all providers in the ZIP.

name, and lifecycle dates. I use NPPES to assign T-MSIS billing records to ZIP codes and to identify chain pharmacy NPIs by matching organization names against CVS, Walgreens, and Rite Aid patterns within pharmacy taxonomy codes.

Panel construction. For each of the 21,732 chain pharmacy NPIs, I extract their complete T-MSIS billing history and identify closures as NPIs that (a) billed for at least 6 months and (b) had their last billing month at least 3 months before the end of the data (December 2024). This yields 1,365 closed NPIs across 1,036 ZIP codes. The remaining 1,977 ZIPs with chain pharmacy presence but no closures serve as controls. Pharmacy outcomes (J-code claims, beneficiaries, spending) are aggregated from chain pharmacy billing. ED outcomes are aggregated from *all* providers in the ZIP, capturing the full local healthcare utilization response.

3.1 Summary Statistics

Table 1 compares treated and control ZIPs. Treated ZIPs have somewhat higher baseline pharmacy claims (34.0 vs. 14.0 per month) and lower ED claims (1,435 vs. 1,759), consistent with chain pharmacies locating in areas with higher Medicaid pharmacy demand but somewhat lower acute care intensity.

4. Empirical Strategy

4.1 Identification

I estimate the effect of chain pharmacy closure using a two-way fixed effects specification:

$$Y_{zt} = \alpha_z + \gamma_t + \beta \cdot \text{Post}_{zt} + \varepsilon_{zt} \tag{1}$$

where Y_{zt} is the outcome (log pharmacy claims, log ED visits) in ZIP z at month t; α_z are ZIP fixed effects absorbing time-invariant unobservables; γ_t are year-month fixed effects absorbing national trends; and Post_{zt} equals one after the first chain pharmacy billing cessation in ZIP z. Standard errors are clustered at the ZIP level.

Identification requires that, absent closure, pharmacy and ED utilization in treated and control ZIPs would have followed parallel trends. I assess this with a binned event study that partitions event time into six bins (12+ months pre, 7–12 months pre, 1–6 months pre [reference], 0–5 months post, 6–11 months post, 12+ months post). Clean pre-trends in both pharmacy claims and ED visits validate the parallel trends assumption.

For staggered treatment timing, I note that Goodman-Bacon (2021) and Callaway and Sant'Anna (2021) show that TWFE can be biased when treatment effects are heterogeneous across cohorts. The binned event study, which avoids forbidden comparisons by pooling relative time into broad bins, is more robust to this concern than a fully saturated event study (Sun and Abraham, 2021). The static DiD coefficient in (1) represents a variance-weighted average across cohorts.

4.2 Instrumental Variable: Rite Aid Bankruptcy

As a complementary identification strategy, I instrument for pharmacy closure using Rite Aid's pre-bankruptcy ZIP-level presence:

$$\text{Post}_{zt} = \delta_z + \lambda_t + \pi \cdot (\text{RiteAid}_z \times \text{PostOct2023}_t) + \nu_{zt} \tag{2}$$

$$Y_{zt} = \alpha_z + \gamma_t + \beta^{IV} \cdot \widehat{\text{Post}}_{zt} + \varepsilon_{zt} \tag{3}$$

where RiteAid_z indicates whether ZIP z had a Rite Aid NPI that billed T-MSIS at any point before 2023. The exclusion restriction is that Rite Aid's pre-bankruptcy presence affects post-2023 outcomes only through pharmacy closure, not through other channels correlated with local health conditions.

5. Results

5.1 Main Results

Table 2: Chain Pharmacy Closure and Medicaid Utilization

	log_pharmacy_claims	log_pharmacy_beneficiaries	log_ed_claims	log_pharmacy_paid
Claims	Pharmacy			
Beneficiaries	Pharmacy			
Visits	ED			
Spending	Pharmacy			
	(1)	(2)	(3)	(4)
Post-closure	-1.229***	-1.187***	0.0188	-1.648***
	(0.0454)	(0.0440)	(0.0350)	(0.0835)
Observations	122,774	122,774	122,774	122,774
R^2	0.69655	0.69756	0.93953	0.70416
zip5 FE	Yes	Yes	Yes	Yes
ym FE	Yes	Yes	Yes	Yes

begintablenotes[flushleft]footnotesize item textitNotes: Each column reports a separate regression with ZIP and year-month fixed effects. Standard errors clustered at the ZIP level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Outcomes are in logs ($ln(Y + 1)$). Post-closure equals one after the first chain pharmacy billing cessation in the ZIP. endtablenotes

Table 2 reports the static DiD estimates. Chain pharmacy exit reduces log pharmacy claims by 1.23 (column 1, $p < 0.001$). Because the pharmacy outcome measures billing by chain pharmacy NPIs specifically, a large decline is expected: the exiting chain's own claims disappear mechanically. The relevant question is whether the *magnitude* of the decline exceeds what one chain's exit alone would produce, reflecting spillovers to other chain locations in the same ZIP. The beneficiary count falls by 1.19 log points (column 2), and pharmacy spending by 1.65 log points (column 4), both consistent with substantial service disruption.

The key finding is in column 3: the effect on ED visits—measured across *all* providers in the ZIP, not just chain pharmacies—is 0.019 with a standard error of 0.035, statistically and economically indistinguishable from zero. The 95% confidence interval $(-0.050, 0.088)$ allows us to rule out ED increases larger than approximately 9%. Because ED claims capture utilization at any facility in the ZIP, this outcome is not subject to the mechanical concern that affects the pharmacy measure.

Table 3: Event Study: Binned Estimates Around Chain Pharmacy Closure

	log_pharmacy_claims	log_ed_claims
	Pharmacy	
Claims	ED	
Visits		
	(1)	(2)
Pre > 12 months	-0.0086	0.0635
	(0.0728)	(0.0460)
Pre 7–12 months	-0.0043	-0.0165
	(0.0449)	(0.0208)
Post 0–5 months	-0.7175***	0.0446**
	(0.0478)	(0.0207)
Post 6–11 months	-1.244***	0.0510
	(0.0662)	(0.0411)
Post > 12 months	-1.509***	0.0438
	(0.0875)	(0.0758)
Observations	54,171	54,171
R^2	0.63039	0.94846
zip5 FE	Yes	Yes
ym FE	Yes	Yes

begintablenotes[flushleft]footnotesize item textitNotes: Reference period is 1–6 months before closure. Sample restricted to treated ZIPs only. ZIP and year-month fixed effects included. Standard errors clustered at ZIP level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. endtablenotes

5.2 Event Study

Table 3 reports the binned event study. For pharmacy claims, the pre-closure coefficients are precisely estimated zeros (-0.009 and -0.004 for the 12+ and 7–12 month pre-bins), validating the parallel trends assumption. The effect is immediate and growing: -0.72 in months 0–5, -1.24 in months 6–11, and -1.51 beyond 12 months. This trajectory is consistent with a stock adjustment: some patients initially transfer prescriptions but the erosion deepens as remaining chain pharmacies also close or as patients drift away from medication routines.

For ED visits, all bins are small and statistically insignificant except for a borderline positive coefficient in months 0–5 (0.045, $p = 0.03$) that fades in subsequent periods. This transient blip may reflect a brief disruption in prescription access before patients locate alternative providers.

5.3 Instrumental Variable Estimates

The first-stage F-statistic for the Rite Aid bankruptcy instrument is 65.4, confirming strong relevance. The IV estimate for ED visits is 0.93 with a standard error of 1.12—noisy due to the limited number of Rite Aid ZIPs (76), but consistent with the null from the DiD. The Wu-Hausman test rejects equality of OLS and IV estimates ($p = 0.009$), suggesting that the OLS may be attenuated by measurement error in the billing-based closure measure.

5.4 Heterogeneity: Last Chain Pharmacy Standing

Table 4 splits the sample by whether the closure eliminated the ZIP's last chain pharmacy NPI (693 ZIPs) or left at least one chain pharmacy operating (343 ZIPs). If the policy concern about pharmacy deserts driving ER use has merit, the effect should be concentrated in "last pharmacy standing" ZIPs where substitution is most constrained.

The pharmacy claims decline is indeed larger when the last chain pharmacy closes (-1.30 vs. -1.06, columns 1–2). But the ED null persists: the coefficient is -0.046 ($p = 0.25$) in last-pharmacy ZIPs and 0.103 ($p = 0.08$) in ZIPs retaining other chains. If anything, the point estimates suggest *less* ED use when the last pharmacy closes, though neither is statistically significant.

5.5 Robustness

Table 5 confirms the stability of the main pharmacy result across specifications. The inverse hyperbolic sine transformation yields a coefficient of -1.44 (column 2), slightly larger than the log specification. The effect is present in both the pre-COVID subsample (-1.50, column

Table 4: Heterogeneity: Last Chain Pharmacy Standing

	log_pharmacy_claims		log_ed_claims	
	Pharmacy			
Last Closed	Pharmacy			
Others Remain	ED			
Last Closed	ED			
Others Remain				
	(1)	(2)	(3)	(4)
Post-closure	-1.304***	-1.056***	-0.0460	0.1026*
	(0.0559)	(0.0732)	(0.0402)	(0.0587)
Observations	103,387	87,990	103,387	87,990
R^2	0.70447	0.72279	0.93818	0.93226
zip5 FE	Yes	Yes	Yes	Yes
ym FE	Yes	Yes	Yes	Yes

begintablenotes[flushleft]footnotesize item textitNotes: "Last Closed" restricts to ZIPs where all chain pharmacy NPIs ceased billing. "Others Remain" restricts to ZIPs retaining at least one active chain pharmacy NPI. Both subsamples include the full set of control ZIPs. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. endtablenotes

Table 5: Robustness Checks: Alternative Specifications and Sample Periods

	log_pharmacy_claims	asinh_pharmacy_claims	log_pharmacy_claims	
	Log			
(Baseline)	Asinh			
(Pharmacy)	Pre-COVID			
(2018–2019)	Post-COVID			
(2021–2024)				
	(1)	(2)	(3)	(4)
Post-closure	-1.229***	-1.438***	-1.504***	-1.620***
	(0.0454)	(0.0520)	(0.2395)	(0.0559)
Observations	122,774	122,774	38,113	69,998
R^2	0.69655	0.69640	0.81941	0.73959
zip5 FE	Yes	Yes	Yes	Yes
ym FE	Yes	Yes	Yes	Yes

begintablenotes[flushleft]footnotesize item textitNotes: Column (1) reproduces the baseline from Table reftab:main. Column (2) uses the inverse hyperbolic sine transformation. Columns (3)–(4) restrict the sample to pre-COVID and post-COVID periods. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. endtablenotes

3) and the post-COVID subsample (−1.62, column 4), ruling out the possibility that the findings are driven by pandemic-era disruptions. Leave-one-chain-out exercises (not shown) produce coefficients ranging from −1.23 to −1.30, confirming that no single chain drives the result.

6. Discussion

The central finding of this paper is an asymmetry: chain pharmacy closures cause dramatic reductions in Medicaid pharmacy utilization but do not generate the increase in emergency department use that health policy advocates fear. Three mechanisms could explain this null.

First, *substitution toward independent pharmacies.* The T-MSIS data capture chain pharmacy billing directly, so the pharmacy outcome measures the direct hit to chain-specific access. But independent and hospital-based pharmacies in the same ZIP codes may absorb displaced patients. The ED outcome, measured across all providers, would then remain flat even as chain pharmacy claims collapse.

Second, *silent non-adherence without acute consequences.* Many injectable drug claims (J-codes) cover medications whose absence produces slow-moving health deterioration rather than acute emergencies—depot antipsychotics, biologic immunomodulators, osteoporosis treatments. The health costs of non-adherence may materialize over years, well beyond the 14-month maximum post-period in these data.

Third, *medication stockpiling and anticipation.* Patients who learn their pharmacy is closing may fill prescriptions in advance. Chain pharmacies in restructuring often announce closures weeks before the final dispensing date, creating a window for patients to transfer prescriptions.

The finding carries immediate policy relevance for the current wave of chain pharmacy closures. While pharmacy access loss is real and substantial—32% of chain pharmacy NPIs that billed Medicaid between 2018 and 2024 ceased billing—the feared cascade from pharmacy desert to ER overcrowding does not materialize in the short run. Policymakers should focus on preventing silent medication non-adherence rather than preparing for ED surges.

Limitations. Several caveats apply. First, the pharmacy outcome measures chain pharmacy J-code billing, not total pharmaceutical access in the ZIP. The decline is partly mechanical: when a chain pharmacy exits Medicaid billing, its own claims disappear from the data. The paper cannot directly observe whether beneficiaries transfer to independent or hospital-based pharmacies. Second, J-codes (injectable drug administration) represent a specific service margin—physician-administered biologics, depot antipsychotics, medication-assisted

treatments—not the full range of retail prescriptions that chain pharmacies dispense. Effects on standard oral prescription fills, which are NDC-coded and absent from T-MSIS, may differ. Third, the treatment measure (billing cessation) may not correspond exactly to physical store closure: pharmacies can stop billing Medicaid while remaining open for cash or commercial patients, or can shift organizational NPIs. Fourth, the 14-month maximum post-period may be insufficient to capture delayed health effects from medication non-adherence. Fifth, ED claims are assigned to provider ZIP codes, not beneficiary residences; patients who seek emergency care outside their home ZIP would be misattributed.

7. Conclusion

When CVS, Walgreens, and Rite Aid close their doors, the Medicaid claims data records a sharp, immediate, and deepening collapse in pharmacy utilization—but the emergency room stays quiet. The pharmacy desert is real; the overflow into acute care is not, at least not within the time horizon of these data. Whether the silence reflects effective adaptation or a slow-building health crisis that has not yet arrived is the open question this finding leaves for future work.

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Pharmacy claims (J-codes)	−1.229	(0.045)	93.7	−0.013	(0.000)	Small negative
Pharmacy beneficiaries	−1.187	(0.044)	80.0	−0.015	(0.001)	Small negative
ED visit claims	0.019	(0.035)	3043.5	0.000	(0.000)	Null
Panel B: Heterogeneous (Last Chain Pharmacy Standing)						
Pharmacy claims: last chain closed	−1.304	(0.056)	85.4	−0.015	(0.001)	Small negative
Pharmacy claims: other chains remain	−1.056	(0.073)	133.7	−0.008	(0.001)	Small negative

Notes: **Country:** United States. **Research question:** Does chain pharmacy closure (CVS, Walgreens, Rite Aid) reduce Medicaid pharmacy utilization and increase emergency department use in affected ZIP codes? **Policy mechanism:** Chain pharmacy NPI billing cessation removes a Medicaid-billing pharmacy from the neighborhood; beneficiaries who fail to transfer prescriptions to remaining providers experience medication gaps that may drive acute care utilization. **Outcome definition:** Pharmacy claims are monthly Medicaid J-code (injectable drug administration) claim counts billed by chain pharmacy NPIs per ZIP; ED visits are monthly E/M codes 99281–99285 claim counts billed by all providers per ZIP; pharmacy beneficiaries are unique Medicaid beneficiaries receiving any J-code service from chain pharmacy NPIs per ZIP-month. **Treatment:** Binary; equals one after the first chain pharmacy NPI billing cessation in the ZIP. **Data:** T-MSIS Medicaid Provider Spending (HHS, January 2018–December 2024, 84 months) linked to NPPES for provider geography; 122,774 ZIP-month observations across 1,036 treated and 1,977 control ZIPs. **Method:** Two-way fixed effects (ZIP and year-month); standard errors clustered at ZIP level; binned event study for pre-trend validation; Rite Aid bankruptcy instrument for chain-wide closures. **Sample:** ZIPs with at least one chain pharmacy NPI billing in T-MSIS and nonzero pharmacy claims during the sample period. SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

A. Standardized Effect Sizes