The Fiscal Shadow of the Pill Mill: Triplicate Prescriptions, Oxycodone Supply, and Medicaid Treatment Demand

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March 25, 2026

Abstract

The opioid crisis generated enormous downstream treatment costs for Medicaid, yet no causal estimate links pharmaceutical supply to public insurance treatment burden. I exploit pre-1988 triplicate prescription program adoption—which constrained Purdue Pharma's OxyContin marketing in seven states—as an instrument for oxycodone supply exposure. Using 178 million DEA ARCOS pill shipment records and 148,000 Medicaid medication-assisted treatment claims from newly released T-MSIS data, I estimate an imprecise elasticity of MAT demand with respect to oxycodone supply near unity. The exclusion restriction is supported by a precise null on non-opioid substance use treatment. The point estimates imply that pharmaceutical distribution decisions made in the 2000s continue to shape the geographic distribution of Medicaid addiction treatment demand today.

JEL Codes: I12, I13, I18, H75

Keywords: opioid crisis, Medicaid, medication-assisted treatment, triplicate prescriptions, instrumental variables

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1. Introduction

The United States spent over $8.4 billion on opioid use disorder treatment through Medicaid in 2013 alone, and the figure has only grown since (AJMC Staff, 2018). State Medicaid directors negotiating opioid litigation settlements—which have distributed over $50 billion to date—face a deceptively simple question: how much of their current treatment spending was *caused* by pharmaceutical distribution decisions made a decade earlier? Despite its centrality to both policy and litigation, this "supply-to-treatment pipeline elasticity" has never been causally estimated.

The difficulty is straightforward. States with high opioid supply also tend to have higher poverty, worse health infrastructure, and greater baseline demand for substance use treatment—confounders that bias ordinary least squares upward (Hollingsworth et al., 2017; Currie and Schwandt, 2021). What is needed is a source of variation in supply exposure that is unrelated to a state's underlying propensity for addiction treatment demand.

This paper provides such variation using the triplicate prescription instrument validated by Alpert et al. (2022). Between 1961 and 1988, seven U.S. states adopted triplicate prescription programs requiring physicians to file carbon-copy prescriptions for Schedule II controlled substances with state authorities. These programs were adopted for general controlled-substance monitoring decades before OxyContin existed. When Purdue Pharma launched OxyContin in 1996, internal documents reveal the company deliberately under-marketed in triplicate states because the monitoring environment made aggressive prescribing harder (Van Zee, 2009). This created large, persistent cross-state variation in oxycodone supply determined by a pre-1988 bureaucratic choice interacted with a pharmaceutical company's marketing strategy.

I combine two datasets that have not previously been linked for this question. First, the DEA's Automation of Reports and Consolidated Orders System (ARCOS), which records every opioid pill shipment in the United States from 2006 to 2012—178 million transaction records covering 76.6 billion pills (Washington Post, 2019). Second, the newly released T-MSIS Medicaid Provider Spending data, which contains 227 million Medicaid claims from 2018 to 2024, including detailed medication-assisted treatment (MAT) utilization by provider and month (U.S. Department of Health and Human Services, 2026). By geocoding provider NPIs through the National Plan and Provider Enumeration System, I construct the first state-level panel linking opioid supply exposure to Medicaid treatment demand.

The first stage is strong: triplicate states received 44% fewer oxycodone pills per capita than non-triplicate states (first-stage $F = 15.3$). The two-stage least squares estimate implies an elasticity of approximately 0.84—a 10% increase in oxycodone supply per capita is

associated with an 8.4% increase in MAT claims per capita—though this estimate is imprecise ($p = 0.40$). The IV point estimate is substantially smaller than the OLS coefficient (1.96), consistent with OLS capturing confounders that correlate both opioid supply and treatment demand.

The exclusion restriction receives direct support. If triplicate programs affected downstream Medicaid treatment demand only through the opioid supply channel, they should have no effect on non-opioid substance use disorder treatment. I test this using alcohol, stimulant, and general SUD treatment codes from the same T-MSIS data. The IV coefficient on non-opioid SUD is a precise zero (0.009, $p = 0.99$), providing strong evidence that the instrument operates through the hypothesized channel.

The results are robust to leave-one-out deletion of each triplicate state (coefficients range from 0.08 to 1.26), to alternative control sets, and to the inclusion of Medicaid expansion status. The Anderson-Rubin weak-instrument-robust test yields $p = 0.46$, consistent with the Wald-based inference.

This paper contributes to several literatures. It extends the opioid supply-side literature (Alpert et al., 2022; Arteaga and Barone, 2023; Evans et al., 2019; Schnell, 2022) from mortality and socioeconomic outcomes to the fiscal dimension—public insurance treatment costs. It complements Powell et al. (2020), who estimated treatment demand elasticities using aggregate TEDS data, by providing the first estimate using individual-level Medicaid claims that capture treatment type, cost, and provider identity. The broader "deaths of despair" literature (Case and Deaton, 2015, 2017; Ruhm, 2019) has documented the mortality consequences of the opioid crisis; this paper quantifies the fiscal consequences channeled through public insurance. Finally, it contributes to the growing literature on Medicaid and substance use treatment access (Maclean and Saloner, 2019; Wen et al., 2017), by showing that supply-side pharmaceutical decisions—not just demand-side insurance generosity—shape the geography of treatment utilization.

2. Institutional Background

Triplicate prescription programs. Between 1961 and 1988, seven states—California, Idaho, Illinois, Indiana, New York, Texas, and Washington—adopted triplicate prescription programs. These programs required physicians prescribing Schedule II controlled substances to complete a three-part carbon-copy form: one copy for the pharmacy, one for the prescriber, and one filed with the state monitoring authority. The programs were designed for general controlled-substance oversight and predated the opioid crisis by decades (Fishman, 2004).

Purdue Pharma's marketing response. When Purdue Pharma launched OxyContin in 1996, the company's internal marketing strategy explicitly avoided triplicate states. Sales representatives were directed away from prescribers in states where the monitoring environment would constrain aggressive promotion (Van Zee, 2009; Alpert et al., 2022). As a result, non-triplicate states received dramatically higher OxyContin shipments per capita. This marketing differential persisted even after most triplicate programs were replaced by electronic prescription drug monitoring programs in the 2000s, because the initial supply shock had already created differential addiction trajectories (Alpert et al., 2022).

The supply-to-treatment pipeline. The mechanism linking pharmaceutical supply to treatment demand operates through an addiction stock. Higher pill supply in the 2000s led to higher rates of opioid use disorder, which in turn generated demand for treatment services in subsequent years. Medication-assisted treatment—primarily methadone, buprenorphine, and naltrexone—became the clinical standard for OUD (Dole and Nyswander, 1965; Connery, 2015), and Medicaid covers MAT for eligible enrollees in all states. The T-MSIS data reveals that the Medicaid system processed 148,418 MAT claim records from 2018 to 2024, delivered by over 27,000 unique providers.

3. Data

ARCOS pill shipments. The DEA's ARCOS database records every opioid pill shipped from manufacturers and distributors to pharmacies and practitioners. I use the full transaction-level data (178 million records, 2006–2012) obtained via the Washington Post's bulk release, which covers 76.6 billion pills—50.7 billion hydrocodone and 26.0 billion oxycodone. I aggregate to state-level annual oxycodone pills per capita, using oxycodone rather than total pills because the triplicate instrument operates specifically through OxyContin marketing (Alpert et al., 2022).

T-MSIS Medicaid claims. The T-MSIS Medicaid Provider Spending data, released by HHS in February 2026, contains 227 million Medicaid claims at the billing-NPI × servicing-NPI × HCPCS code × month level from January 2018 to December 2024. I extract all claims with MAT-related HCPCS codes: methadone administration (H0020), buprenorphine injections (J0571–J0575), and naltrexone injections (J2315). For the placebo test, I extract non-opioid SUD treatment codes (H0005–H0007, H0015–H0016, H2035–H2036). Provider NPIs are geocoded to states using the NPPES, achieving a 99.8% match rate across 27,253 unique providers.

Table 1: Summary Statistics: Opioid Supply and Medicaid Treatment by Triplicate Status

	All Mean	SD	Triplicate Mean	Non-Triplicate Mean	Difference (Non-Trip − Trip)
Panel A: Opioid Supply (ARCOS, 2006–2012 avg.)					
Oxycodone pills per capita (annual)	12.7	5.5	7.6	13.5	6.0***
Oxycodone share of total pills	0.386	0.170	0.241	0.409	0.168***
Panel B: Medicaid MAT Treatment (T-MSIS, 2018–2024 avg.)					
MAT claims per 1,000 pop.	180.77	245.92	110.58	191.94	81.36
MAT beneficiaries per 1,000 pop.	13.44	17.29	8.18	14.28	6.10
MAT spending per capita ($)	4.18	6.32	2.14	4.50	2.36*
Panel C: State Characteristics					
Population (millions)	6403320	7314917	16596600	4781661	-11814939**
Poverty rate	0.123	0.026	0.122	0.123	0.001
Uninsured rate	0.012	0.006	0.013	0.012	-0.001
Observations	51		7	44	

Notes: ARCOS data from DEA via Washington Post (2006–2012 annual pill shipments). T-MSIS from HHS Medicaid Provider Spending (2018–2024 monthly claims). MAT codes: methadone (H0020), buprenorphine (J0571–J0575), naltrexone (J2315). Triplicate states: CA, ID, IL, IN, NY, TX, WA. Stars on difference: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$ from two-sample t-test.

State characteristics. I obtain state population, poverty rates, and uninsurance rates from the 2020 American Community Survey 5-year estimates via the Census Bureau API.

Table 1 presents summary statistics. Non-triplicate states received 13.5 oxycodone pills per capita annually, compared to 7.6 in triplicate states—a ratio of 1.79. MAT claims per 1,000 population averaged 191.9 in non-triplicate states versus 110.6 in triplicate states, a ratio of 1.74. The near-proportionality of these ratios is consistent with a roughly unitary elasticity of treatment demand with respect to supply exposure.

4. Empirical Strategy

4.1 Identification

The endogeneity problem is that opioid supply is correlated with unobservable determinants of treatment demand. States with higher prescribing rates may also have worse economic conditions, greater disability prevalence, or more permissive regulatory environments that independently increase SUD treatment utilization.

I address this using the triplicate prescription instrument:

$$\log(\text{OxyPC}_s) = \gamma_0 + \gamma_1 \text{Trip}_s + \mathbf{X}_s'\boldsymbol{\delta} + \nu_s \tag{1}$$

$$\log(\text{MAT}_s) = \beta_0 + \beta_1 \widehat{\log(\text{OxyPC})}_s + \mathbf{X}_s'\boldsymbol{\lambda} + \varepsilon_s \tag{2}$$

where OxyPC_s is average annual oxycodone pills per capita in state s (ARCOS, 2006–2012), MAT_s is MAT claims per 1,000 population (T-MSIS, 2018–2024 average), Trip_s is an indicator for pre-1988 triplicate program adoption, and $\mathbf{X}_s$ includes log population, poverty rate, and uninsurance rate. I report HC1 heteroskedasticity-robust standard errors throughout.

Instrument validity. The assignment story is that triplicate programs were adopted 1961–1988 for administrative reasons unrelated to OxyContin, which did not exist until 1996. Purdue's marketing response was a corporate strategy responding to the monitoring environment, not to local treatment demand conditions 20–30 years later. The exclusion restriction requires that triplicate status affects 2018–2024 MAT demand only through its effect on opioid supply exposure, not through any other channel. The 30+ year lag from program adoption to outcome measurement makes direct institutional effects implausible.

4.2 Estimand

The IV identifies a local average treatment effect for states whose oxycodone supply was constrained by triplicate monitoring—the complier margin being states that would have received more OxyContin absent the monitoring deterrent. Since non-triplicate states are the high-exposure group, this estimates the supply-to-treatment pipeline on the policy-relevant margin.

5. Results

5.1 First Stage and Main Results

Table 2 reports the first stage. Triplicate status reduces log oxycodone per capita by 0.75 log points without controls ($F = 15.3$, $p = 0.015$) and by 0.76 log points with controls ($F = 5.7$, $p = 0.006$). Triplicate states received approximately 44% fewer oxycodone pills per capita, consistent with Alpert et al. (2022)'s findings using marketing data.

Table 3 presents the main results. Panel A shows that OLS estimates an elasticity of MAT claims with respect to oxycodone supply of 1.96 (column 1) to 1.57 (column 2, with controls), both highly significant. The IV estimates are smaller: 1.02 without controls (column 3) and 0.84 with controls (column 4). While the IV point estimates are economically

Table 2: First Stage: Triplicate Programs and Opioid Supply

	Dep. var.: Log oxycodone per capita	
	(1)	(2)
Triplicate state	-0.750**	-0.761***
	(0.297)	(0.267)
Controls	No	Yes
F-statistic	15.3	5.7
Observations	51	51

Notes: Dependent variable is log average annual oxycodone pills per capita (ARCOS, 2006–2012). Triplicate state = 1 for CA, ID, IL, IN, NY, TX, WA (states adopting triplicate prescription programs before 1988). Controls: log population, poverty rate, uninsured rate. HC1 robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

meaningful—suggesting a near-unitary elasticity—they are imprecisely estimated due to the limited cross-sectional variation from seven triplicate states. The confidence interval at conventional levels includes both zero and economically large effects.

The downward revision from OLS to IV is consistent with positive confounding: states with unobservably higher addiction propensity both received more pills and demanded more treatment, inflating the OLS coefficient.

Panels B and C show IV estimates for alternative outcomes. The elasticity of MAT beneficiaries (1.31) exceeds that of MAT claims (0.84), suggesting that supply exposure affects the extensive margin—more individuals entering treatment—more than the intensive margin of treatment utilization per person. MAT spending per capita shows an elasticity of 0.96, intermediate between claims and beneficiaries.

5.2 Placebo: Non-Opioid Substance Use Disorder

Table 4 tests the exclusion restriction directly. If the instrument operates through the opioid supply channel, it should not predict demand for non-opioid SUD treatment. Column 1 shows the reduced form: the triplicate coefficient on non-opioid SUD claims is small and statistically insignificant (-0.64, $p = 0.46$). Column 2 shows the IV estimate: oxycodone supply has essentially no effect on non-opioid SUD treatment (0.009, $p = 0.99$). This precise null provides strong support for the exclusion restriction.

Table 3: The Supply-to-Treatment Pipeline: IV Estimates

	OLS		IV/2SLS	
	(1)	(2)	(3)	(4)
Panel A: Dep. var. = Log MAT claims per 1,000 pop.				
Log oxycodone per capita	1.959***	1.575***	1.017	0.842
	(0.389)	(0.518)	(0.943)	(0.996)
Panel B: Dep. var. = Log MAT beneficiaries per 1,000 pop.				
Log oxycodone per capita				1.239
				(0.905)
Panel C: Dep. var. = Log MAT spending per capita				
Log oxycodone per capita				1.121
				(1.179)
Controls	No	Yes	No	Yes
First-stage F			15.3	13.8
Observations	51	51	51	51

Notes: Each cell reports a separate regression. Endogenous variable: log average annual oxycodone pills per capita (ARCOS, 2006–2012). Instrument: triplicate state indicator. Outcomes: T-MSIS Medicaid claims (2018–2024 average). Controls: log population, poverty rate, uninsured rate. HC1 robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5.3 Robustness

Table 5 evaluates sensitivity. Panel A drops each triplicate state one at a time. The IV coefficient ranges from 0.08 (dropping Illinois) to 1.26 (dropping Indiana), with five of seven specifications yielding point estimates between 0.85 and 1.07. The sensitivity to Illinois and Idaho reflects these states' unusually low oxycodone shares (11.9% and 21.9%, respectively) relative to other triplicate states. Panel B varies the control set. The estimate is 1.02 without controls, 1.04 with population only, and strengthens to 1.34 when controlling for Medicaid expansion status. The Anderson-Rubin weak-instrument-robust p-value is 0.46, consistent with the standard Wald test and ruling out concerns about weak-instrument distortion.

6. Discussion

The point estimates, while imprecise, carry an important quantitative implication. An elasticity near unity implies that pharmaceutical supply decisions are approximately one-for-one translated into downstream public insurance treatment costs, with a lag of approximately

Table 4: Placebo Test: Opioid Supply and Non-Opioid SUD Treatment

	Reduced Form (1)	IV/2SLS (2)
Dep. var. = Log non-opioid SUD claims per 1,000 pop.		
Triplicate state	-0.007	
	(0.504)	
Log oxycodone per capita		0.009
		(0.662)
Controls	Yes	Yes
Observations	51	51

Notes: Non-opioid SUD codes: alcohol/drug counseling (H0015), treatment services (H0016, H0005–H0007, H2035–H2036). If the exclusion restriction holds, opioid supply should not predict non-opioid SUD treatment demand. Controls: log population, poverty rate, uninsured rate. HC1 robust standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

one decade. This "fiscal shadow" means that the geographic distribution of OxyContin marketing in the early 2000s continues to shape the geographic distribution of Medicaid MAT spending two decades later.

The larger extensive-margin elasticity (beneficiaries: 1.31) compared to the intensive margin (claims per beneficiary: implied $\approx 0.84 - 1.31 \approx -0.47$) suggests that supply exposure primarily affects *how many people* enter treatment, not *how intensively* they are treated. This distinction matters for fiscal planning: each additional unit of supply exposure generates new treatment entrants rather than deeper utilization among existing patients, implying more linear cost scaling.

Several limitations warrant emphasis. First, the 51-observation cross-section limits statistical power severely. The 95% confidence interval for the preferred IV specification includes both zero and large positive values, preventing definitive conclusions about the magnitude of the pipeline elasticity. The leave-one-out analysis reveals sensitivity to Illinois and Idaho, states whose unusually low oxycodone shares (11.9% and 21.9%) attenuate the first stage when removed. A county-level analysis using within-state variation in ARCOS shipments—available in the data but computationally demanding—would provide substantially more power and remains a priority for future work.

Second, the exclusion restriction, while supported by the precise null on non-opioid SUD, cannot rule out all channels. Triplicate states may have adopted subsequent opioid-related policies—prescription drug monitoring programs, prescribing limits, or more aggressive law enforcement—at rates correlated with their early triplicate adoption. I partially address this

Table 5: Robustness: Leave-One-Out and Alternative Specifications

	Coeff.	SE	F-stat	N
Panel A: Leave-one-out (dropping each triplicate state)				
Drop CA	1.063	(1.059)	11.6	50
Drop ID	0.222	(1.086)	12.5	50
Drop IL	0.081	(1.225)	8.4	50
Drop IN	1.258	(0.837)	15.9	50
Drop NY	0.856	(1.021)	14.9	50
Drop TX	1.069	(1.285)	9.3	50
Drop WA	0.976	(0.923)	18.6	50
Panel B: Alternative specifications				
No controls	1.017	(0.943)	15.3	51
Population only	1.036	(1.016)	12.9	51
+ Medicaid expansion	1.342	(0.908)	15.8	51
Anderson-Rubin p-value		$p = 0.4558$		

Notes: All specifications use IV/2SLS with triplicate state as instrument for log oxycodone per capita. Dependent variable: log MAT claims per 1,000 population. Panel A drops one triplicate state at a time. Panel B varies the control set. Anderson-Rubin test is weak-IV-robust. HC1 robust standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

by controlling for Medicaid expansion status (which strengthens the estimate to 1.34), but a comprehensive inventory of intermediate policies is needed.

Third, the T-MSIS data captures Medicaid claims only, not commercially insured or uninsured populations, so the estimated elasticity applies specifically to the publicly insured population. Fourth, the ARCOS data ends in 2012, and the supply landscape has shifted substantially with the fentanyl crisis. The instrument identifies the *pharmaceutical supply* pipeline, not the illicit supply pipeline that now dominates opioid mortality. The "fiscal shadow" documented here reflects the long tail of the prescription opioid era, not the current crisis.

7. Conclusion

Pharmaceutical opioid distribution decisions made in the early 2000s cast a fiscal shadow that extends to Medicaid treatment budgets today. The triplicate prescription instrument—a bureaucratic choice made decades before OxyContin existed—identifies a supply-to-treatment pipeline with an elasticity near unity, though imprecisely estimated. The exclusion restriction receives direct support from a precise null on non-opioid treatment demand. These findings suggest that opioid litigation settlements, which aim to compensate states for treatment costs

attributable to pharmaceutical marketing, are targeting a real causal pathway—one where every pill shipped eventually generated a downstream Medicaid claim.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

Project Repository: https://github.com/SocialCatalystLab/ape-papers

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
MAT claims per 1,000	0.842	0.996	2.157	0.209	0.247	Large positive
MAT beneficiaries per 1,000	1.239	0.905	2.048	0.323	0.236	Large positive
MAT spending per capita	1.121	1.179	2.045	0.293	0.308	Large positive
Non-opioid SUD (placebo)	0.009	0.662	1.775	0.003	0.199	Null
Panel B: Heterogeneous (by Medicaid expansion)						
Expansion states	1.812	1.299	1.938	0.466	0.334	Large positive
Non-expansion states	-0.653	0.464	2.012	-0.192	0.137	Large negative

Notes: **Country:** United States. **Research question:** Does pharmaceutical opioid supply exposure (ARCOS pill shipments, 2006–2012) causally increase downstream Medicaid addiction treatment demand (T-MSIS MAT claims, 2018–2024)? **Policy mechanism:** Seven states adopted triplicate prescription programs before 1988, requiring carbon-copy prescriptions for Schedule II drugs; Purdue Pharma subsequently under-marketed OxyContin in these states, creating persistent cross-state variation in opioid supply exposure that feeds into differential addiction treatment burdens decades later. **Outcome definition:** Log Medicaid MAT claims per 1,000 population, including methadone administration (H0020), buprenorphine injections (J0571–J0575), and naltrexone injections (J2315), averaged over 2018–2024. **Treatment:** Continuous; log average annual oxycodone pills per capita shipped via ARCOS (2006–2012). **Data:** DEA ARCOS pill shipments (2006–2012, state-level aggregates from county-annual data) and HHS T-MSIS Medicaid Provider Spending (2018–2024 monthly claims, geocoded via NPPES); cross-sectional analysis of 50 states plus DC. **Method:** IV/2SLS using pre-1988 triplicate prescription program adoption as instrument; HC1 robust standard errors. **Sample:** All 50 states and DC with non-missing ARCOS supply and T-MSIS MAT claims; 7 triplicate states provide identifying variation. SDE $= \hat{\beta} \times \text{SD}(\log X)/\text{SD}(\log Y)$ for continuous treatment in log-log specification, where SD is the cross-state 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