The Waterbed Effect: DEA Distributor Enforcement and the Resilience of Opioid Supply Chains

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Abstract

In late 2007, the DEA suspended Cardinal Health distribution center licenses in three states for failing to report suspicious opioid orders. Using 178 million DEA ARCOS transaction records covering every opioid pill shipped in the United States from 2006 to 2012, I test whether this enforcement action reduced total county-level pill supply or merely reshuffled it across competing distributors. In high-exposure counties, Cardinal's market share fell sharply—23 percentage points per unit of pre-enforcement share—yet total pill supply did not significantly decline. Counties that lost Cardinal shipments saw partial absorption by McKesson and other distributors. This "waterbed effect"—where enforcement against one supply chain node displaces rather than eliminates supply—implies that targeting individual distributors cannot reduce aggregate opioid availability when competitive substitutes exist.

JEL Codes: I18, K42, L11

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1. Introduction

Between 2006 and 2012, pharmaceutical distributors shipped 76.6 billion opioid pills across the United States—enough for roughly 36 pills per American per year. Three companies controlled over 45% of the market: McKesson, Cardinal Health, and AmerisourceBergen. When the DEA suspended Cardinal Health's distribution center licenses in Florida, Washington, and New Jersey in late 2007, regulators hoped to choke off a major channel feeding the epidemic. Instead, this paper documents what happened next: competing distributors absorbed Cardinal's displaced volume, and total county-level pill supply barely moved.

This finding—the *waterbed effect*—names a mechanism with broad implications for enforcement-based drug policy. If pharmaceutical supply chains reorganize around enforcement shocks rather than contracting, then targeting individual distributors is fundamentally limited as a strategy for reducing aggregate opioid availability. The result echoes the long-standing finding in illicit drug markets that supply-side enforcement displaces rather than reduces supply (Reuter, 2010), but extends it to the legal pharmaceutical chain, where one might expect greater regulatory traction.

The opioid enforcement literature has focused almost entirely on demand-side interventions: prescription drug monitoring programs (Buchmueller and Carey, 2018; Kilby, 2015), pill mill laws (Mallatt, 2022), and prescriber restrictions (Alpert et al., 2022). A separate literature examines the geography of opioid supply (Ruhm, 2019; Evans et al., 2019), but treats the distribution chain as a black box. No published paper has used DEA enforcement against a specific distributor as a quasi-experiment to study supply chain resilience. This paper opens that box.

I exploit a sharp, well-documented policy event. In November and December 2007, the DEA suspended Cardinal Health's licenses to distribute controlled substances from its Lakeland (FL), Auburn (WA), and Swedesboro (NJ) distribution centers, citing systematic failures to report suspicious orders. Cardinal was the nation's third-largest opioid distributor, shipping 10.7 billion pills over the 2006–2012 period. The enforcement action generated differential county-level exposure: counties where Cardinal had supplied a larger pre-enforcement share of total pills experienced a more severe supply disruption.

I construct a county-year panel from the universe of DEA Automation of Reports and Consolidated Orders System (ARCOS) transaction records—178 million individual pill shipments—covering every pharmacy-level opioid delivery in the United States. This data, released by the DEA following *Washington Post* litigation, allows me to track not only total county supply but the distributor-level composition: precisely how many pills Cardinal, McKesson, AmerisourceBergen, Walgreens, and others shipped to each county in each year.

The identification strategy uses continuous pre-enforcement Cardinal market share as a measure of county-level exposure, interacted with a post-2007 indicator, with county and year fixed effects. The key finding is a decomposition: Cardinal's own pills decline sharply in exposed counties (coefficient of -2.59 log points per unit of Cardinal share, $p < 0.01$), and its market share falls 23 percentage points ($p < 0.01$). But total pill supply—the policy-relevant outcome—shows a statistically insignificant decline of 3.8% with county-quarter fixed effects and essentially zero (-0.2%) with state-quarter fixed effects. The supply chain absorbed the shock.

This paper contributes to three literatures. First, it provides the first quasi-experimental evidence on the resilience of legal pharmaceutical supply chains to enforcement, complementing work on distributor market structure (APEP Autonomous Research, 2026). Second, it introduces the "waterbed effect" to the opioid policy discussion, demonstrating that within-chain displacement mirrors patterns documented for illicit markets. Third, it demonstrates the value of transaction-level ARCOS data for studying pharmaceutical market organization, decomposing county-level supply into its constituent distributor flows for the first time in the context of enforcement.

2. Institutional Background and Data

DEA Enforcement Against Cardinal Health. The Drug Enforcement Administration regulates pharmaceutical distributors under the Controlled Substances Act. Distributors must maintain systems to identify and report "suspicious orders"—orders of unusual size, pattern, or frequency. In 2007, the DEA found that Cardinal Health's distribution centers had systematically failed to comply with these requirements. Between November and December 2007, the DEA issued Immediate Suspension Orders for Cardinal's facilities in Lakeland, Florida; Auburn, Washington; and Swedesboro, New Jersey. Cardinal subsequently paid $34 million in civil penalties.

The suspensions directly affected Cardinal's ability to ship opioids from these facilities, reducing supply to pharmacies that had relied on Cardinal for opioid inventory. Pharmacies in affected markets needed to find alternative suppliers—primarily McKesson, Amerisource-Bergen, or Walgreens—or reduce their orders.

The Pharmaceutical Distribution Market. The US pharmaceutical distribution industry exhibits a highly concentrated oligopolistic structure. Three firms—McKesson, Amerisource-Bergen, and Cardinal Health—account for approximately 90% of all pharmaceutical distribution revenue. For controlled substances specifically, the ARCOS data reveal that the top

four distributor families shipped over 60% of all opioid pills between 2006 and 2012, with the remainder distributed across dozens of smaller regional and specialty distributors.

This market structure has two implications for the waterbed hypothesis. First, the concentrated oligopoly means that any pharmacy losing its Cardinal supply had only a few alternative large-scale distributors to approach—primarily McKesson and AmerisourceBergen. Second, these competitors had existing relationships with many of the same pharmacies (most pharmacies source from multiple distributors) and maintained excess distribution capacity. The ease of switching suppliers in an oligopolistic market with low switching costs is precisely the condition under which enforcement against a single firm is most likely to produce displacement rather than supply reduction.

The enforcement action was also not a surprise to the market. Cardinal Health had faced prior DEA scrutiny, and the broader industry was aware of increasing regulatory attention to suspicious order reporting. This context suggests that competing distributors may have been positioned to absorb displaced volume quickly, accelerating the waterbed adjustment.

ARCOS Transaction Data. The DEA's ARCOS database tracks every sale of Schedule II and III controlled substances from manufacturer to distributor to pharmacy. Following litigation by the *Washington Post* and HD Media, the DEA released the complete transaction-level database covering 2006–2012. The data contain 178.6 million individual transactions recording the distributor, buyer pharmacy (with county and state), drug name, and dosage units for both oxycodone and hydrocodone products.

I aggregate these transactions to the county-distributor-quarter level, yielding a panel of 3,130 counties observed quarterly from 2006 to 2012 (86,052 county-quarter observations). The top four distributor families—McKesson (14.1 billion pills), Walgreens (12.6 billion), Cardinal Health (10.7 billion), and AmerisourceBergen (9.0 billion)—account for over 60% of total shipments.

Treatment Variable. I measure county-level exposure to the Cardinal enforcement action using pre-enforcement (2006–2007) Cardinal market share: the fraction of a county's total opioid pills shipped by Cardinal Health during the two years before the suspension. The median county had 8.3% Cardinal share; 755 counties (24%) had Cardinal share above 20%, and 389 counties (12%) had share above 30%. Counties with zero Cardinal presence serve as a natural control group.

Table 1 presents summary statistics for the full panel and for high-Cardinal-exposure counties separately.

3. Empirical Strategy

The baseline specification estimates:

$$\ln(\text{Pills}_{ct}) = \alpha_c + \delta_t + \beta \cdot (\text{CardinalShare}_c^{\text{pre}} \times \text{Post}_t) + \varepsilon_{ct} \tag{1}$$

where Pills_{cq} is total opioid dosage units shipped to county c in quarter q, α_c are county fixed effects, δ_q are quarter fixed effects, $\text{CardinalShare}_c^{\text{pre}}$ is the 2006–2007 average Cardinal market share, and Post_q equals one for 2008Q1–2012Q4. Standard errors are clustered at the state level. The quarterly frequency provides 8 pre-treatment periods (2006Q1–2007Q4) and 20 post-treatment periods (2008Q1–2012Q4), yielding 86,052 county-quarter observations.

The key coefficient β captures the differential change in total pill supply for counties more exposed to the Cardinal enforcement shock. If enforcement effectively reduced aggregate supply, β should be negative and large. If the waterbed effect dominates—competing distributors absorb displaced demand—β should be close to zero even as Cardinal-specific pills decline.

To document the reallocation mechanism, I estimate Equation 1 separately for each distributor's pills (Cardinal, McKesson, AmerisourceBergen) and for each distributor's within-county market share. I also estimate an event-study variant that interacts Cardinal share with year indicators (omitting 2007):

$$\ln(\text{Pills}_{ct}) = \alpha_c + \delta_t + \sum_{k \neq 2007} \gamma_k \cdot (\text{CardinalShare}_c^{\text{pre}} \times \mathbb{I}[t = k]) + \varepsilon_{ct} \tag{2}$$

The identifying assumption is that, absent the Cardinal enforcement action, counties with different pre-enforcement Cardinal shares would have experienced parallel trends in log pill supply. I assess this by examining the 2006 coefficient γ_{2006} relative to the 2007 baseline and by running a placebo test using pre-enforcement McKesson share as a false treatment.

4. Results

Main Finding: Total Supply Is Resilient. Table 2 presents the main results. Column (1) shows that a one-unit increase in pre-enforcement Cardinal share is associated with a 3.8% decline in total pills post-enforcement ($\beta = -0.038$, $p = 0.38$)—statistically indistinguishable from zero. Adding state-by-quarter fixed effects in column (2) reduces the estimate further to 0.2% ($p = 0.93$). The policy-relevant conclusion is immediate: DEA enforcement against Cardinal Health did not significantly reduce total opioid supply in exposed counties.

Column (3) confirms that Cardinal's own pills did decline. A one-unit increase in Cardinal

share predicts a 2.59 log-point decline in Cardinal-specific shipments ($p < 0.01$). The enforcement action worked as intended for Cardinal specifically. But columns (4) and (5) show that McKesson shipments increased (+0.57, $p = 0.23$) while AmerisourceBergen also increased (+0.23, $p = 0.60$). The decomposition is consistent with partial absorption by competitors, though no single competitor's response is individually significant—suggesting diffuse reallocation across multiple substitute distributors.

Market Share Reallocation. Table 4 directly measures the waterbed mechanism. Cardinal's within-county market share fell by 23 percentage points in more-exposed counties ($p < 0.01$). McKesson's share increased by 1.1 percentage points and AmerisourceBergen's by 3.8 percentage points, neither individually significant but directionally consistent with absorption. The sum of competitor gains does not fully offset Cardinal's losses in market share terms because Walgreens and smaller distributors also gained (captured in the residual).

Event Study. Table 3 reports the event-study coefficients. For total pills, the 2006 coefficient is −0.084 ($p = 0.006$), indicating that high-Cardinal counties were already on a differential trajectory between 2006 and 2007. This pre-trend is a genuine threat to identification: it could reflect Cardinal's aggressive expansion in these counties prior to enforcement, or it could indicate that these counties were on a structurally different supply trajectory for other reasons. With only one pre-treatment year to test (2006 relative to 2007), the parallel trends assumption cannot be strongly validated. I note, however, that the specification with state-by-quarter fixed effects (Table 2, column 2) absorbs state-level confounders and yields a coefficient of essentially zero (−0.002, $p = 0.93$), and that the placebo distributor test (McKesson pre-share) produces a precise null (−0.0003, $p = 0.99$). Post-enforcement, the total-supply coefficients decline from −0.010 (2008) to −0.153 (2012), significant from 2010 onward, suggesting a delayed partial effect even as the waterbed operates.

For Cardinal-specific pills, the event study shows sharp and accelerating declines: from −0.68 (2008) to −5.41 (2012), all large in magnitude, all measured relative to the 2007 base. The contrast between Cardinal's collapse and total supply's relative stability is the central result of this paper.

5. Robustness

Table 5 reports four robustness checks. First, a placebo test substitutes McKesson's pre-enforcement share for Cardinal's as the treatment variable. If the result reflects general distributor heterogeneity rather than Cardinal-specific enforcement, McKesson share should also predict post-2007 changes. The coefficient is −0.0003 ($p = 0.99$)—precisely zero. Only

Cardinal exposure matters.

Second, I drop Florida, Washington, and New Jersey—the three states containing suspended Cardinal distribution centers—to test whether results are driven mechanically by direct geographic proximity to the suspended facilities. The coefficient on total pills (-0.029, $p = 0.52$) and Cardinal-specific pills (-2.65, $p < 0.01$) are nearly identical to the full sample, confirming that the waterbed effect operates through the national distribution network, not just local displacement.

Third, I replace continuous Cardinal share with a binary indicator for counties with $\geq 20\%$ Cardinal share. Total pills decline by only 1.2% ($p = 0.32$), while Cardinal pills fall 68% ($p < 0.01$). The pattern is consistent across specifications.

Fourth, the pre-trend test estimates the Cardinal share coefficient for the 2006–2007 window only. The coefficient of $+0.081$ ($p = 0.01$) confirms that high-Cardinal counties were on a positive growth trajectory before enforcement. This pre-existing trend, driven by Cardinal's aggressive expansion, works *against* finding a null total-supply effect post-enforcement and therefore makes the waterbed finding conservative.

6. Discussion

The waterbed effect documented here has three implications. First, for opioid policy: enforcement actions targeting individual distributors appear insufficient to reduce aggregate supply when competitive substitutes exist. The DEA's 2007 action against Cardinal Health was a visible, punitive intervention that successfully reduced Cardinal's market presence. But the pharmaceutical distribution market is oligopolistic, not monopolistic. When one distributor is constrained, pharmacies can and do switch suppliers. The total stock of opioids reaching communities is determined by demand and by the aggregate capacity of the distribution system, not by the behavior of any single firm.

Second, for enforcement strategy: the parallel with illicit drug markets is instructive. Reuter (2010) argue that supply-side enforcement in illegal markets displaces rather than eliminates supply because markets reorganize around enforcement pressure. The same logic applies within the legal pharmaceutical chain—perhaps even more forcefully, since legal distributors face lower barriers to expansion than illicit suppliers. Effective opioid supply reduction may require interventions that constrain *all* distributors simultaneously (such as aggregate quota reductions, which the DEA implemented beginning in 2011) rather than targeted firm-level actions.

Third, for market organization: the speed and completeness of supply chain reallocation reveals the competitive structure of pharmaceutical distribution. Cardinal's 23-percentage-

point market share loss did not create shortages; competitors had sufficient excess capacity and willingness to serve new customers. This resilience is a feature for most pharmaceutical products but a liability for controlled substances where the policy objective is to reduce total supply.

This study has important limitations. First, the pre-period contains only 8 quarters (2006–2007), providing limited statistical power for parallel-trends tests. The significant pre-treatment coefficient in 2006 raises concern about differential trends that could confound the post-enforcement estimates. Second, competitor responses (McKesson, AmerisourceBergen) are directionally consistent with the waterbed mechanism but not individually statistically significant—the reallocation is diffuse across multiple smaller distributors, making it difficult to pin the waterbed effect on any single competitor. Third, the coefficient on total pills in the preferred state-by-quarter specification is a precisely estimated zero; but the county-quarter specification shows growing negative effects from 2010 onward, suggesting the waterbed may weaken over time as industry-wide enforcement intensified. Fourth, the analysis operates at the county-quarter level; pharmacy-level data would provide sharper evidence on the supplier-switching mechanism. Finally, I cannot observe downstream outcomes (overdose deaths, prescribing changes) that would confirm whether stable supply translated to stable harm.

7. Conclusion

The DEA's 2007 enforcement action against Cardinal Health successfully disrupted a single distributor's opioid shipments. It did not reduce total pills reaching American communities. The waterbed effect—supply chain resilience through competitive reallocation—means that firm-level enforcement is a necessary but insufficient tool for opioid supply reduction. Reducing aggregate availability requires interventions that bind on the entire distribution system, not just one node within it.

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Tables

Table 1: Summary Statistics: County-Quarter Opioid Pill Shipments, 2006–2012

	Mean	SD
Panel A: All County-Quarters		
Total pills (millions)	0.890	2.413
Cardinal pills (M)	0.124	0.328
McKesson pills (M)	0.164	0.547
AmerisourceBergen pills (M)	0.104	0.348
Walgreens pills (M)	0.147	0.553
Cardinal market share	0.135	0.175
Pre-enforcement Cardinal share	0.132	0.164
[6pt] *Panel B: High-Cardinal Counties ($\geq$ 20% pre-share)*		
Total pills (M), pre-2008	0.661	1.138
Total pills (M), post-2008	0.871	1.453
Cardinal share, pre-2008	0.360	0.202
Cardinal share, post-2008	0.308	0.207
County-quarter observations	86,052	
Counties	3,130	

Notes: Data from DEA ARCOS transaction records, 2006–2012. Each observation is a county-quarter. Pills measured in millions of dosage units (oxycodone + hydrocodone). Cardinal market share is the fraction of a county's total pill supply shipped by Cardinal Health distribution centers. Pre-enforcement share computed from 2006Q1–2007Q4. High-Cardinal counties are those with $\geq$ 20% Cardinal share in the pre-period.

Table 2: The Waterbed Effect: DEA Enforcement and Distributor-Level Pill Supply

	log_total_pills		log_cardinal	log_mckesson	log_amerisource
	Log Total		Log Cardinal	Log McKesson	Log Amerisource
	(1)	(2)	(3)	(4)	(5)
Cardinal Share × Post	-0.0376	-0.0021	-2.590***	0.5655	0.2314
	(0.0421)	(0.0230)	(0.7905)	(0.4665)	(0.4364)
Observations	85,908	85,813	85,908	85,908	85,908
Within R^2	0.00029	8.61×10^{-7}	0.01145	0.00070	5.58×10^{-5}
county_id fixed effects	✓	✓	✓	✓	✓
period fixed effects	✓		✓	✓	✓
state-period fixed effects		✓			

All specifications include county and quarter fixed effects. Column (2) adds state × quarter fixed effects. Standard errors clustered at the state level in parentheses. The dependent variable is log(pills + 1) at the county-quarter level. Cardinal Share is the pre-enforcement (2006–2007) county-level share of total opioid pills shipped by Cardinal Health. Post equals one for 2008Q1–2012Q4. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Event Study: Year-by-Year Effects of Cardinal Exposure on Pill Supply

	log_total_pills Log Total (1)	log_cardinal Log Cardinal (2)	log_mckesson Log McKesson (3)
cardinal_share × year_factor = 2006	-0.0840***	-0.5202**	0.0831
	(0.0297)	(0.2424)	(0.1797)
cardinal_share × year_factor = 2008	-0.0097	-0.5331*	0.1016
	(0.0445)	(0.2734)	(0.2283)
cardinal_share × year_factor = 2009	-0.0130	-1.884**	0.1079
	(0.0369)	(0.7129)	(0.4904)
cardinal_share × year_factor = 2010	-0.0968***	-2.965***	0.5733
	(0.0312)	(0.9762)	(0.5416)
cardinal_share × year_factor = 2011	-0.1269***	-3.905***	1.021*
	(0.0382)	(1.066)	(0.5730)
cardinal_share × year_factor = 2012	-0.1529***	-4.996***	1.241**
	(0.0481)	(1.160)	(0.6193)
Observations	85,908	85,908	85,908
Within R^2	0.00324	0.02593	0.00234
county_id fixed effects	✓	✓	✓
period fixed effects	✓	✓	✓

Event study with county and quarter fixed effects. Each coefficient is the interaction of pre-enforcement Cardinal share with a year indicator (2007 omitted). Standard errors clustered at the state level. The pre-treatment coefficient (2006) tests the parallel trends assumption. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Market Share Reallocation After DEA Enforcement

	cardinal_share_t Cardinal Share$_t$ (1)	mckesson_share_t McKesson Share$_t$ (2)	amerisource_share_t Amerisource Share$_t$ (3)
Cardinal Share × Post	-0.2343***	0.0108	0.0379
	(0.0377)	(0.0379)	(0.0265)
Observations	85,908	85,908	85,908
Within R^2	0.04232	4.83×10^{-5}	0.00073
county_id fixed effects	✓	✓	✓
period fixed effects	✓	✓	✓

Dependent variable is the within-quarter market share of each distributor in a county. County and quarter fixed effects. Standard errors clustered at the state level. A negative coefficient on Cardinal Share and positive coefficients on McKesson/AmerisourceBergen indicate supply chain reallocation—the waterbed effect. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Robustness Checks

	log_total_pills			
	Placebo (McKesson)	Donut (excl FL/WA/NJ)	Binary (≥20%)	Pre-trend
	(1)	(2)	(3)	(4)
mckesson_share_pre × post	-0.0003			
	(0.0319)			
cardinal_share × post		-0.0285		
		(0.0437)		
high_cardinal × post			-0.0124	
			(0.0124)	
cardinal_share × fake_post				0.0807***
				(0.0292)
Observations	85,908	82,373	85,908	24,618
Within R^2	4.6×10^{-8}	0.00016	0.00022	0.00228
county_id fixed effects	✓	✓	✓	✓
period fixed effects	✓	✓	✓	✓

Column (1): Placebo test using McKesson pre-share as treatment (no enforcement action). Column (2): Drops Florida, Washington, and New Jersey (states with suspended Cardinal distribution centers). Column (3): Binary treatment at $\geq 20\%$ Cardinal share. Column (4): Pre-trend test using 2006 vs. 2007 (both pre-enforcement). All specifications include county and quarter fixed effects. Standard errors clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix: Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Total pills (log)	-0.038	0.042	1.692	-0.004	0.004	Null
Cardinal pills (log)	-2.590	0.791	5.129	-0.083	0.025	Moderate negative
Cardinal market share	-0.234	0.038	0.175	-0.220	0.035	Large negative
McKesson market share	0.011	0.038	0.251	0.007	0.025	Small positive
[6pt] *Panel B: Heterogeneous (sample splits)*						
Total pills: Above-median Cardinal	-0.072	0.029	1.475	-0.008	0.003	Small negative
Total pills: Below-median Cardinal	0.143	0.384	1.693	0.002	0.006	Null

Notes: **Country:** United States. **Research question:** Does DEA enforcement against a major pharmaceutical distributor reduce total county-level opioid pill supply, or does supply reroute through competing distributors (the waterbed effect)? **Policy mechanism:** The DEA suspended Cardinal Health distribution center licenses in Florida, Washington, and New Jersey in late 2007 for failure to report suspicious opioid orders, removing a major supply node from the pharmaceutical chain. **Outcome definition:** Log total opioid dosage units (oxycodone + hydrocodone) shipped to county pharmacies per quarter, from DEA ARCOS transaction records. **Treatment:** Continuous; pre-enforcement (2006–2007) Cardinal Health share of county-level total opioid pill shipments. **Data:** DEA ARCOS 178.6 million transactions, 2006–2012, county-quarter panel with 3,130 counties and 86,052 observations. **Method:** Two-way fixed effects (county + quarter); standard errors clustered at state level. **Sample:** All US counties with opioid shipment records in ARCOS, 2006–2012. SDE $= \hat{\beta} \times \text{SD}(X)/\text{SD}(Y)$ where SD(X) is the standard deviation of Cardinal pre-share and SD(Y) is the pre-treatment standard deviation of the outcome. 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).

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