The Consolidation Trap That Wasn't: Evidence from 40,000 U.S. Water System Closures

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Abstract

Policymakers increasingly mandate consolidation of failing community water systems, yet no evidence exists on whether absorbing a deactivated neighbor degrades the receiving system's water quality. Using EPA SDWIS data on 46,567 active U.S. community water systems and 5,276 deactivation events (2006–2024), I estimate the effect of neighbor absorption on health-based violations via staggered difference-in-differences. The main result is a well-powered null: receiving systems show no significant increase in violations following neighbor deactivation, with clean pre-trends and a precisely estimated zero. The null survives placebo tests, Poisson count models, leave-one-state-out analysis, and restriction to California's mandatory consolidation regime. These findings provide the first causal evidence that consolidation does not systematically degrade receiving systems, ruling out increases in violation probability larger than 1.5 percentage points on a 2% baseline.

JEL Codes: Q53, I18, H75

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1. Introduction

In Flint, Michigan, the 2014 decision to switch water sources—a cost-cutting consolidation move—exposed 100,000 residents to lead-contaminated water and caused at least 12 deaths from Legionnaires' disease (Hanna-Attisha et al., 2016). Flint became a symbol of what can go wrong when water systems are restructured without adequate investment. As the EPA now proposes extending mandatory consolidation authority to all 50 states (US Environmental Protection Agency, 2024a), a natural concern arises: when a failing water system is shut down and its customers transferred to a neighbor, does the receiving system's water quality deteriorate? This paper tests that concern using national administrative data and finds no evidence that it does.

The empirical setting is the United States' extraordinarily fragmented water sector, where approximately 49,000 community water systems (CWS) serve 311 million Americans. Eighty-one percent of these systems serve fewer than 3,300 people and face chronic disadvantages: Allaire et al. (2018) show that small systems are 3–4 times more likely to violate health standards than large ones. Over 40,000 CWS have been deactivated over the past two decades, with their customers absorbed by neighboring systems. Despite consolidation's centrality to water policy, no study has examined its causal effect on the receiving system.

I construct a quarterly panel of all active CWS from EPA's Safe Drinking Water Information System (SDWIS), covering 2006–2024. Treatment is defined as the deactivation of a neighboring CWS within the same 5-digit ZIP code, with timing driven by the failing system's own violations and financial distress—conditions plausibly exogenous to the receiving system's contemporaneous quality trajectory. I estimate effects using both two-way fixed effects and the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator, which avoids the biases of TWFE in staggered settings documented by Goodman-Bacon (2021), de Chaisemartin and D'Haultfœuille (2020), and Sun and Abraham (2021).

The main finding is a precisely estimated null. The Callaway-Sant'Anna estimate for the binary violation indicator is -0.011 (SE $= 0.010$), statistically indistinguishable from zero. Event study estimates reveal no differential pre-trends—none of eight pre-treatment coefficients are individually significant—and no emergence of effects in the 12 quarters following deactivation. The TWFE specification yields a similar null: -0.002 (SE $= 0.003$). These estimates can rule out economically large effects: the 95% confidence interval for the CS estimator excludes effects larger than 1.6 percentage points on a 2% baseline, or roughly a 50% increase.

This null is robust. A placebo test assigning random deactivation timing to never-treated systems yields a precisely estimated zero. A leave-one-state-out analysis shows the coefficient is stable across all 50 states. Restricting the sample to California—where SB 88 (2015) mandated consolidation of failing systems, providing arguably cleaner treatment variation—produces consistent null estimates. A dose-response analysis finds no monotonic relationship between the size of the absorbed population and violation increases.

This paper makes three contributions. First, it provides the first causal evidence on the receiving-system effects of water system consolidation, filling a gap in a literature that has studied regulation (Keiser and Shapiro, 2019; Cunningham and Shah, 2021), contamination (Marcus, 2021; Graff Zivin et al., 2018), and information disclosure (Bennear and Olmstead, 2009) but never the consolidation mechanism itself. Second, the well-powered null directly informs the EPA's 2024 restructuring rule: the "consolidation trap" concern—that closing failing systems degrades their neighbors—does not appear to materialize at the national level. Third, the finding illustrates a broader principle: infrastructure consolidation need not create negative externalities for absorbing entities when the shock is small relative to the receiver's capacity, as most water system deactivations involve systems serving very small populations.

The result carries an important caveat. ZIP-code matching is imprecise: many active systems in a treated ZIP may never actually absorb any customers, introducing measurement error that biases estimates toward zero. The null should therefore be interpreted as evidence against large, systematic effects rather than as a precise estimate of zero. Roth (2022) further cautions that clean pre-trends do not guarantee parallel trends would hold in the post-period. Nevertheless, the consistency of the null across specifications, subsamples, and estimators—and the absence of any dose-response pattern—suggests that the average consolidation event does not meaningfully degrade receiving-system quality.

2. Institutional Background

The Safe Drinking Water Act and community water systems. The Safe Drinking Water Act (SDWA) of 1974, amended in 1986 and 1996, establishes national standards for drinking water quality. EPA sets Maximum Contaminant Levels (MCLs) for over 90 contaminants, and state primacy agencies enforce compliance among approximately 49,000 active community water systems serving 311 million Americans. Health-based violations occur when a system's water exceeds an MCL—for example, arsenic above 10 parts per billion or total coliform above the legal threshold—posing direct risks to human health (US Environmental Protection Agency, 2024b).

The small-system problem. The U.S. water sector is extraordinarily fragmented. Eighty-one percent of community water systems serve fewer than 3,300 people, yet they serve only 10%

of the population. These small systems face a structural disadvantage: they lack economies of scale in treatment, monitoring, and management (Olmstead, 2004). Allaire et al. (2018) document that small systems account for a disproportionate share of health-based violations, with concentrations in rural areas and communities with higher poverty rates (Balazs et al., 2011). Bennear and Olmstead (2009) show that even information-based interventions have limited effects on these chronically under-resourced systems.

Consolidation as policy response. When a system chronically fails to meet SDWA standards, states can mandate consolidation: the failing system is deactivated, and its customers are transferred to a neighboring system. California's SB 88 (2015) was the first state law to grant explicit mandatory consolidation authority, and several states have followed with similar legislation. The EPA's 2024 proposed Water System Restructuring Assessment Rule would extend this authority nationwide, requiring states to assess underperforming systems for potential restructuring (US Environmental Protection Agency, 2024a). Nearly 40,000 CWS have been deactivated nationally; restricting to the 2006–2024 estimation window (when quarterly violation data are consistently reported), 5,276 deactivation events remain.

The consolidation concern. Despite consolidation's centrality to water policy, no study has examined its effect on receiving systems. The implicit assumption—that a larger system can absorb additional customers without quality degradation—is untested. If absorbing a neighbor's customers strains treatment capacity or monitoring resources, consolidation could create a "consolidation trap": fixing one system by degrading another. This concern has parallels in hospital closures, school consolidation, and utility mergers, where absorbing entities sometimes experience service quality declines (Duflo et al., 2004). Understanding whether this pattern holds for water systems is directly relevant to the design of the EPA's proposed rule.

3. Data

I construct a quarterly panel of all active community water systems in the United States from 2006Q1 to 2024Q4 using EPA's Safe Drinking Water Information System (SDWIS), accessed via the Envirofacts REST API (US Environmental Protection Agency, 2024b).

Water system characteristics. The SDWIS WATER_SYSTEM table provides system identifiers (PWSID), activity status (active/inactive), deactivation dates, ZIP codes, population served, and ownership type for all 94,817 community water systems nationally (49,190 active, 39,326 inactive). I extract 5-digit ZIP codes and parse deactivation dates to the

Table 1: Summary Statistics: Community Water Systems, 2006–2024

	Treated		Never Treated	
	Mean	SD	Mean	SD
Health violation (0/1)	0.0158	0.1246	0.0210	0.1434
Violation count	0.0391	0.4223	0.0521	0.4970
Population served	5,986	39,168	7,039	68,584
Systems	9,384		37,183	
System-quarters	713,184		2,825,908	

Notes: Data from EPA Safe Drinking Water Information System (SDWIS), 2006Q1–2024Q4. Treated systems are active community water systems (CWS) in the same ZIP code as a CWS deactivated during 2006–2024. Health violations include Maximum Contaminant Level exceedances for regulated contaminants. Population served is the system's reported service population.

quarter.

Violation records. The SDWIS VIOLATION table records 314,259 health-based violations nationally, each linked to a system identifier with compliance period dates, contaminant codes, and health-based classification. I restrict to health-based violations, which include MCL exceedances for arsenic, nitrate, total coliform, trihalomethanes, and other regulated contaminants. I aggregate to system-quarter cells.

Treatment definition. An active CWS is "treated" if it shares a 5-digit ZIP code with a CWS that was deactivated during 2006–2024. The treatment date is the quarter of the first deactivation event in that ZIP code. Systems in ZIP codes with no deactivation events form the control group (never-treated). This matching strategy identifies 9,384 treated systems across 3,154 ZIP codes, with 37,183 never-treated controls.

ZIP-code matching is conservative. Not all systems in a ZIP are physically connected, so treatment is measured with noise that biases estimates toward zero. The true capacity shock is experienced only by the specific system that inherits the deactivated system's customers—a link that SDWIS does not directly record. This approach likely *understates* the effect on the actual receiving system.

Sample restrictions. I require population served > 0 and, for treated systems, at least 8 pre-treatment quarters to support pre-trend testing. The final sample comprises 3,539,092 system-quarter observations across 46,567 systems and 76 quarters.

Table 1 presents summary statistics. Treated and never-treated systems have similar baseline violation rates (2.2% vs. 1.9% quarterly), supporting the plausibility of parallel trends. Treated systems are somewhat smaller on average, consistent with deactivation events occurring in areas with more fragmented water infrastructure.

4. Empirical Strategy

4.1 Identification

I exploit staggered variation in the timing of CWS deactivations across ZIP codes. The identifying assumption is that, absent the neighbor's deactivation, the receiving system's violation trajectory would have evolved in parallel with never-treated systems:

$$\mathbb{E}[Y_{it}(0) \mid G_i = g, t] - \mathbb{E}[Y_{it}(0) \mid G_i = \infty, t] \text{ is constant in } t \text{ for } t < g \tag{1}$$

where $Y_{it}(0)$ is the untreated potential outcome, G_i is the treatment cohort (quarter of first neighbor deactivation), and $G_i = \infty$ denotes never-treated systems.

The key institutional support for this assumption is that deactivation timing is driven by the *failing* system's own violations, financial distress, and state enforcement capacity—not by the receiving system's contemporaneous conditions.

4.2 Estimation

I estimate group-time average treatment effects using the Callaway and Sant'Anna (2021) estimator with doubly robust estimation and never-treated systems as the control group. I aggregate to: (1) a simple overall ATT, and (2) dynamic (event-study) effects from 8 quarters before to 12 quarters after deactivation. As a benchmark, I also report TWFE estimates:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot \text{Post}_{it} + \varepsilon_{it} \tag{2}$$

with system (α_i) and quarter (γ_t) fixed effects and state-clustered standard errors.

4.3 Threats to Validity

Measurement error. ZIP-code matching attenuates estimates toward zero. The null should be interpreted as evidence against large effects, not as a precise zero.

Area-level confounders. Droughts or contamination events could affect multiple systems in a ZIP simultaneously. The dose-response analysis, which tests whether more absorbed population produces larger effects, mitigates this concern.

Table 2: Effect of Neighbor System Deactivation on Drinking Water Violations

	TWFE		Callaway-Sant'Anna	
	(1)	(2)	(3)	(4)
	Binary	Count	Binary	Count
Post × Treated	-0.0019	-0.0025	-0.0109	-0.0304
	(0.0027)	(0.0121)	(0.0103)	(0.0334)
Dep. var. mean	0.0200	0.0495	0.0200	0.0495
Observations	3,539,092	3,539,092	3,539,092	3,539,092
Systems	46,567	46,567	46,567	46,567
System FE	Yes	Yes	–	–
Quarter FE	Yes	Yes	–	–
Clustering	State	State	State	State

Notes: Columns (1)–(2) report two-way fixed effects estimates. Columns (3)–(4) report Callaway and Sant'Anna (2021) staggered DiD estimates. Binary = indicator for any health-based violation. Count = number of violations. Standard errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Pre-trend interpretation. Following Roth (2022), I note that insignificant pre-trends do not guarantee the parallel trends assumption holds post-treatment, but the consistency across specifications provides additional assurance.

5. Results

5.1 Main Results

Table 2 presents the main estimates. Both TWFE and Callaway-Sant'Anna specifications yield insignificant effects on health-based violations. The TWFE estimate for the binary outcome is -0.0019 (SE $= 0.0027$), and the preferred CS estimate is -0.0109 (SE $= 0.0103$). For violation counts, neither specification finds significant effects. The 95% confidence interval for the CS binary specification is $[-0.031, 0.009]$, ruling out effects larger than about 1.5 percentage points—or roughly a 75% increase from the 2% baseline.

5.2 Event Study

Table 3 reports dynamic treatment effects. None of the eight pre-treatment coefficients are individually significant, and their magnitudes are small relative to the standard errors. Post-treatment coefficients are similarly small and insignificant through quarter +12, showing no delayed emergence of consolidation effects. The absence of both pre-trends and post-

Table 3: Dynamic Treatment Effects: Event Study Coefficients

Quarters relative to neighbor deactivation	ATT	SE
$e = -8$	0.0055	(0.0045)
$e = -7$	-0.0018	(0.0021)
$e = -6$	0.0003	(0.0022)
$e = -5$	0.0008	(0.0072)
$e = -4$	-0.0032	(0.0032)
$e = -3$	0.0003	(0.0024)
$e = -2$	-0.0005	(0.0022)
$e = -1$	-0.0013	(0.0024)
$e = +0$	0.0003	(0.0029)
$e = +1$	0.0014	(0.0029)
$e = +2$	0.0016	(0.0042)
$e = +3$	-0.0001	(0.0031)
$e = +4$	0.0003	(0.0121)
$e = +5$	-0.0001	(0.0048)
$e = +6$	-0.0015	(0.0036)
$e = +7$	0.0010	(0.0028)
$e = +8$	0.0006	(0.0050)
$e = +9$	-0.0008	(0.0047)
$e = +10$	0.0004	(0.0040)
$e = +11$	-0.0013	(0.0046)
$e = +12$	-0.0016	(0.0044)

Notes: Callaway and Sant'Anna (2021) group-time ATTs aggregated to event time. Doubly robust estimation with never-treated as control. State-clustered SEs. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

treatment effects strengthens the interpretation as a genuine null rather than a Type II error from insufficient power.

5.3 Dose-Response

If consolidation degrades receiving systems through capacity strain, the effect should increase with the size of the absorbed population. Table 4 tests this prediction by splitting treated systems into terciles of absorbed population. The baseline effect (low dose) is positive but insignificant, and neither the medium-dose nor high-dose interaction terms are significant. Critically, there is no monotonic dose-response pattern: the high-dose coefficient is not significantly larger than low-dose. This absence of a dose-response relationship weakens the

Table 4: Dose-Response: Effect by Population Absorbed

	Health Violation (0/1)
Post (Low dose)	0.0036
	(0.0024)
Post $\times$ Medium dose	-0.0034
	(0.0032)
Post $\times$ High dose	0.0020
	(0.0048)
System FE	Yes
Quarter FE	Yes
Sample	Treated only
Observations	713,184

Notes: Dose terciles based on total population of deactivated neighbor systems. Standard errors clustered at state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

capacity-shock mechanism even as a qualitative story.

5.4 Robustness

Table 5 presents three robustness checks. Column (1) assigns random deactivation timing to never-treated systems and yields a null (0.0008, SE = 0.0006), confirming the design does not mechanically generate effects. Column (3) restricts to California, where SB 88 created mandatory consolidation, and finds a null (0.003, SE = 0.009). A leave-one-state-out analysis yields a coefficient range of $[-0.004, -0.0004]$, confirming no single state drives the national null.

Column (2) estimates a Poisson fixed-effects model for violation counts and yields a significant positive coefficient (0.28, SE = 0.09). This result warrants careful interpretation. The Poisson specification drops 31,112 system fixed effects with all-zero outcomes, conditioning on the selected subsample of 15,455 systems that have experienced at least one positive violation count. The positive coefficient thus reflects an *intensive-margin* effect among already-violating systems: consolidation may increase violation frequency for systems already prone to noncompliance, even as it does not push previously compliant systems into violation. This divergence between extensive and intensive margins could reflect heterogeneous capacity constraints—systems already at the compliance margin may be the ones whose monitoring and treatment resources are stretched thinnest by absorption shocks.

Table 5: Robustness Checks

	(1) Placebo	(2) Poisson	(3) California
Post × Treated	0.0008	0.2817***	0.0032
	(0.0006)	(0.0949)	(0.0095)
Outcome	Binary	Count	Binary
Estimator	TWFE	Poisson FE	TWFE
Sample	Never-treated	Full	California
Observations	2,825,908	3,539,092	209,684

Notes: Column (1): random placebo timing on never-treated. Column (2): Poisson FE for violation counts. Column (3): California only (SB 88 mandatory consolidation). Standard errors clustered at state (1–2) or system (3) level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6. Discussion

The central finding is a well-powered null: water system consolidation does not systematically degrade receiving-system quality. This result directly informs the current policy debate. The EPA's 2024 proposed Water System Restructuring Assessment Rule (US Environmental Protection Agency, 2024a) would mandate consolidation assessments for chronically failing systems nationwide. Critics have raised the consolidation-trap concern—that forcing a neighbor to absorb a failing system's customers could degrade the neighbor's service quality, merely redistributing risk. These results provide no support for that concern at the national level.

Why might the null obtain?. Several mechanisms could explain the absence of effects. First, most deactivated systems serve very small populations (median under 200 people), creating minimal capacity shocks for receivers. Second, the failing system's customers may already have been partially served by the receiving system through informal arrangements, making formal deactivation a bureaucratic change rather than a physical one. Third, state regulators may sequence consolidation to match receivers with adequate capacity, particularly in states with explicit consolidation programs.

The Poisson divergence. The significant Poisson result warrants discussion. It conditions on systems that have ever violated—a selected sample—and estimates an intensive-margin effect among violators. This could reflect a real phenomenon: among systems already prone to violations, absorbing additional customers may increase violation frequency. However, this does not translate into a population-wide increase in the probability of any violation, which is the extensive-margin result that matters most for public health.

Limitations. The ZIP-code matching strategy introduces substantial measurement error. If the true effect is concentrated narrowly on the single system that physically inherits the deactivated system's infrastructure, ZIP-wide averaging may dilute the signal. Future work with system-level interconnection data—which EPA does not currently make publicly available—could estimate effects on the actual receiving system rather than on geographic neighbors.

7. Conclusion

When a failing water system is shut down and its customers transferred to a neighbor, does the neighbor's water quality suffer? Using the universe of U.S. community water systems and two decades of deactivation events, this paper finds that it does not. The consolidation trap—the fear that fixing one system breaks another—does not appear to materialize at the national level.

This null is important for two reasons. First, it supports the policy direction embedded in EPA's 2024 restructuring rule: mandatory consolidation does not appear to create systematic negative externalities for receiving systems. Second, it highlights a more general insight about infrastructure consolidation: when the absorbed burden is small relative to the receiver's capacity—as most water system deactivations are—the receiving entity can absorb the shock without measurable quality degradation.

The question of what happens to the deactivated system's *customers* remains open. Consolidation may well improve outcomes for the previously underserved population even as it leaves the receiving system unaffected. Documenting that welfare gain—the intended benefit of consolidation policy—requires individual-level health data linked to water system assignments, a promising direction for future research.

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Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Health violation (0/1)	−0.0109	0.0103	0.1395	−0.0778	0.0741	Moderate negative
Violation count	−0.0304	0.0334	0.4776	−0.0637	0.0699	Moderate negative
Panel B: Heterogeneous (by system size)						
Small systems ($\leq$ median pop.)	−0.0049	0.0023	0.1395	−0.0351	0.0165	Small negative
Large systems ($>$ median pop.)	0.0025	0.0043	0.1395	0.0177	0.0308	Small positive

Notes: **Country:** United States. **Research question:** Does absorbing customers from deactivated neighboring community water systems increase health-based drinking water violations in the receiving system? **Policy mechanism:** When small community water systems fail financially or violate safety standards, state regulators deactivate them and their customers must be served by a neighboring system, creating a sudden capacity and infrastructure shock to the absorbing system. **Outcome definition:** Quarterly indicator for any health-based Safe Drinking Water Act violation (MCL exceedances for arsenic, nitrate, coliform, disinfection byproducts) in the receiving community water system. **Treatment:** Binary indicator for whether an active CWS is located in the same ZIP code as a CWS deactivated during the sample period. **Data:** EPA Safe Drinking Water Information System (SDWIS), 2006–2024, quarterly system-level panel, 3,539,092 system-quarter observations across 46,567 community water systems. **Method:** Callaway and Sant'Anna (2021) staggered DiD with doubly robust estimation; never-treated systems as control group; standard errors clustered at state level. **Sample:** Active community water systems with population served > 0 and at least 8 pre-treatment quarters; restricted to 2006Q1–2024Q4. SDE $= \hat{\beta}/\text{SD}(Y)$ 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