Privatizing the Pipes: Heterogeneous Effects of Sanitation Reform on Waterborne Disease in Brazil

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Abstract

Does privatizing water and sanitation reduce disease? Brazil's 2020 Marco Legal do Saneamento triggered the largest sanitation privatization wave in history, transferring 525 municipalities to private operators through three BNDES auction waves (2021–2023). Using a Callaway–Sant'Anna staggered difference-in-differences design with DATASUS hospitalization records for 11 states over 2014–2023, I find no significant aggregate effect: the overall ATT is -0.94 hospitalizations per 100,000 (SE = 7.48, $p = 0.90$). However, this null aggregate masks striking heterogeneity across concession waves. Alagoas—with Brazil's worst baseline sanitation—shows a significant 38-point reduction, while CEDAE/Rio de Janeiro shows a significant 13-point *increase*. Pre-treatment trends are not parallel ($p = 0.00$), complicating causal interpretation. The results suggest that privatization effects are fundamentally context-dependent, challenging both pro- and anti-privatization narratives that treat ownership reform as uniformly beneficial or harmful.

JEL Codes: L33, I18, H42, O18

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1. Introduction

Nearly two billion people worldwide lack safely managed drinking water, and inadequate sanitation kills more than 800,000 people each year—most of them children under five (World Health Organization and UNICEF, 2023). In the developing world, the dominant model for water provision has been the public utility, yet decades of underinvestment have left infrastructure crumbling, coverage stagnant, and waterborne disease endemic. Whether private operators can do better is among the most contested questions in development economics, touching on fundamental tensions between market efficiency and equitable access to essential services (Megginson and Netter, 2001; Estache et al., 2001).

The canonical evidence comes from Argentina. Galiani et al. (2005) showed that water privatization in the 1990s reduced child mortality by 5–7 percent in the poorest municipalities, establishing that ownership reform can yield measurable health dividends. Yet the Argentine experience unfolded in a specific institutional context—bilateral negotiations between provincial governments and multinational concessionaires—that limits generalizability. Subsequent work has painted a more ambiguous picture. Motta and Moreira (2006) found quality improvements but limited coverage gains from early Brazilian privatizations; Davis (2008) documented failures in Bolivia and elsewhere; and a large empirical literature has debated whether efficiency gains from private ownership translate into welfare improvements for the poor (La Porta and Lopez-de Silanes, 1999; Megginson, 2005; Estache and Rossi, 2005).

This paper exploits the largest sanitation privatization wave in history to provide new evidence on this question. Brazil's 2020 Marco Legal do Saneamento (Law 14,026) mandated competitive bidding for all water and sanitation concessions, replacing decades of direct municipal contracting with state-owned companies. The law catalyzed three major auction waves organized by Brazil's national development bank (BNDES): Alagoas Block A in 2021 (13 municipalities), the CEDAE/Rio de Janeiro concession in 2022 (29 municipalities), and the Corsan/Rio Grande do Sul concession in 2023 (483 municipalities). In total, 525 municipalities—spanning the arid Northeast, the urban Southeast, and the temperate South—transitioned to private operators within three years.

I estimate the effect of this privatization on waterborne disease hospitalizations using a Callaway–Sant'Anna staggered difference-in-differences design (Callaway and Sant'Anna, 2021). The outcome is the municipality-level hospitalization rate for intestinal infectious diseases (ICD-10 codes A00–A09), constructed from DATASUS Hospital Information System (SIH) records linked to IBGE population estimates for 11 Brazilian states over 2014–2023. The staggered rollout of BNDES auctions provides three treatment cohorts (2021, 2022,

2023) with five to seven pre-treatment years, and I use not-yet-treated municipalities as the comparison group with doubly robust estimation.

The principal finding is a null aggregate effect that conceals dramatic heterogeneity. The preferred Callaway–Sant'Anna specification yields an overall ATT of -0.94 hospitalizations per 100,000 population (SE = 7.48, $p = 0.90$)—an estimate indistinguishable from zero and economically negligible relative to the pre-treatment mean of 89.0. The under-5 ATT is similarly small at -2.44 per 100,000 (SE = 3.76). But decomposing by concession wave reveals a striking pattern: Alagoas, where baseline sanitation deficits were most severe, shows a large and significant reduction of -38.43 per 100,000 (SE = 17.83); CEDAE/Rio de Janeiro, operating in a wealthier urban context, shows a significant *increase* of $+12.69$ per 100,000 (SE = 4.41); and Corsan/Rio Grande do Sul shows a positive but insignificant effect of $+11.09$ (SE = 8.23). These opposing effects wash out in the aggregate.

I characterize this pattern as what I call the *context dependence hypothesis*: privatization's health effects are not a fixed property of ownership reform but depend critically on the baseline deficit being addressed. Where public systems have catastrophically failed (Alagoas), private operators with contractual investment obligations can produce rapid improvements. Where systems are already functioning tolerably (Rio de Janeiro), the disruption of transition may temporarily worsen outcomes, or efficiency gains may not translate into health improvements on the margin. This framing reconciles the conflicting findings in the prior literature and provides a testable framework for future evaluations.

However, the causal interpretation of all estimates must be qualified by the failure of the parallel trends assumption. The event study reveals significant pre-treatment coefficients at $t - 5$ (-32.35, SE = 7.43) and $t - 3$ (-16.15, SE = 4.23), and the formal test of parallel trends rejects with $p = 0.00$. While the $t - 1$ coefficient is close to zero (1.21, SE = 2.94), suggesting trends may have converged immediately before treatment, the earlier divergences prevent a clean causal reading of the estimates. The TWFE estimates are also sensitive to specification: the baseline TWFE yields $+3.75$ (SE = 4.51), while adding region-by-year fixed effects flips the sign to -6.60 (SE = 6.88). Neither is significant.

This paper makes three contributions. First, it provides the first evidence on the health effects of Brazil's landmark 2020 sanitation reform, exploiting staggered adoption across three distinct auction waves and a rich administrative panel. The scale dwarfs prior studies: 525 treated municipalities versus the 30–40 privatizations studied by Galiani et al. (2005). Second, the null aggregate result paired with dramatic heterogeneity advances the privatization-and-health literature by demonstrating that the question "does privatization work?" may be fundamentally misspecified—the answer depends on where, when, and under what baseline conditions the reform is implemented. Third, the paper identifies a specific pattern—that effects are largest where baseline deficits are most severe—that has direct implications for targeting future concession auctions and for reconciling conflicting findings across the prior literature on water privatization (Galiani et al., 2005; Davis, 2008; Estache and Rossi, 2005).

The paper proceeds as follows. Section 2 describes the institutional background and policy setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results, including heterogeneity and robustness. Section 6 discusses implications and limitations. Section 7 concludes.

2. Institutional Background

Brazil's sanitation crisis. Brazil entered the 2020s with severe sanitation deficits. Despite being an upper-middle-income country, roughly 100 million Brazilians—nearly half the population—lacked access to sewage collection, and 35 million lacked treated water (Ministério do Desenvolvimento Regional, 2021). Coverage was deeply unequal: while the Southeast (São Paulo, Minas Gerais) approached universal access, the Northeast and North lagged far behind. State-owned water companies (CESBs—Companhias Estaduais de Saneamento Básico), created during the military-era PLANASA program of the 1970s, dominated provision. These entities operated under "program contracts" with municipalities—agreements that were renewed automatically, without competition, for decades (Turolla, 2002).

The Marco Legal do Saneamento. On July 15, 2020, President Bolsonaro signed Law 14,026—the New Legal Framework for Sanitation (Marco Legal do Saneamento). The law's central provision required that all sanitation contracts be awarded through competitive bidding, effectively ending the CESBs' monopoly over service provision. It established universal coverage targets (99% water, 90% sewage by 2033), created the National Water and Sanitation Agency (ANA) as the federal regulator, and empowered BNDES to structure and auction regional concession blocks (República Federativa do Brasil, 2020). The reform was explicitly designed to attract private capital: Brazil's sanitation infrastructure deficit was estimated at R$600–700 billion, far exceeding public fiscal capacity (ABCON/SINDCON, 2021).

The BNDES auction waves. BNDES organized concession blocks by aggregating municipalities served by the same state utility, then auctioning these blocks to private bidders. This block structure is central to identification: municipalities did not individually choose to privatize; rather, they were included in blocks defined by their existing utility relationship. Three major waves occurred during the study period:

Wave 1: Alagoas Block A (September 2021). The first auction under the new framework transferred water and sewage services for 13 municipalities in the northeastern state of Alagoas from the state company CASAL to private operator BRK Ambiental. This small initial wave served as a proof of concept for the new regulatory model.

Wave 2: CEDAE/Rio de Janeiro (April 2022). BNDES auctioned the concession for water distribution and sewage services in metropolitan Rio de Janeiro, transferring 29 municipalities from the state company CEDAE (Águas do Rio de Janeiro) to private operators. This was the highest-profile auction, involving Brazil's second-largest metropolitan area and attracting international attention.

Wave 3: Corsan/Rio Grande do Sul (December 2022, effective 2023). The largest single privatization event in Brazilian sanitation history transferred 483 municipalities in Rio Grande do Sul from the state company Corsan to the Água Mineral Investimentos consortium. This wave alone accounted for over 90% of all treated municipalities in the sample.

What changes with privatization. Upon assuming operations, private concessionaires were contractually obligated to meet phased investment targets, expand coverage to underserved areas, and comply with water quality standards monitored by ANA. The investment commitments were substantial: the Corsan concession alone carried R$10 billion in contractual investment over 35 years. Crucially, the mechanism runs through infrastructure: new water treatment plants, expansion of the sewage collection network, repair of distribution pipelines that were losing 30–40% of treated water to leaks, and installation of water quality monitoring systems (BNDES, 2022). These investments directly affect the bacteriological quality of drinking water and the extent to which untreated sewage contaminates water sources—the proximate causes of waterborne disease.

3. Data

3.1 Data Sources

The analysis combines three administrative datasets, all accessed via Google BigQuery.

Hospitalization records. I use the DATASUS Hospital Information System (SIH/SUS), which records all hospitalizations financed by Brazil's public health system (Sistema Único de Saúde) (Ministério da Saúde, 2024). I extract all hospital admissions with a primary diagnosis of intestinal infectious diseases (ICD-10 codes A00–A09), which include cholera (A00), typhoid and paratyphoid (A01), other salmonella infections (A02), shigellosis (A03), other bacterial intestinal infections (A04), other bacterial foodborne intoxications (A05), amoebiasis (A06), other protozoal intestinal diseases (A07), viral and other specified intestinal infections (A08), and diarrhea and gastroenteritis of presumed infectious origin (A09). These codes capture the full spectrum of waterborne enteric diseases. For each admission, I observe the patient's municipality of residence, age, and hospitalization cost. I aggregate to municipality-year counts of total waterborne hospitalizations and under-5 waterborne hospitalizations for the period 2014–2023.

Population estimates. Municipal population estimates come from IBGE (Instituto Brasileiro de Geografia e Estatística), Brazil's national statistics office. These are intercensal projections based on the 2010 Census, updated annually. I use these to construct per-capita hospitalization rates.

Treatment assignment. Treatment timing is identified from BNDES auction records and news sources documenting the operational transfer dates for each concession wave. The treatment indicator equals one for municipality i in year t if t is at or after the municipality's first full year under private operation. Municipalities in the Alagoas Block A are treated from 2021, CEDAE/Rio municipalities from 2022, and Corsan/RS municipalities from 2023.

3.2 Sample Construction

The analysis panel covers 11 Brazilian states: three with treated municipalities (Alagoas, Rio de Janeiro, Rio Grande do Sul) and eight neighboring control states (Sergipe, Pernambuco, Bahia, São Paulo, Minas Gerais, Espírito Santo, Santa Catarina, Paraná). The control states are chosen to provide geographic and socioeconomic comparability—each treated state is paired with neighbors in the same macroregion (Northeast, Southeast, South). The panel spans 2014–2023 at the municipality-year level, yielding 10,389 observations across 2,462 municipalities over 9 years (2014, 2016–2023). I exclude municipalities with missing population data and compute hospitalization rates per 100,000 population. The primary outcome is winsorized at the 1st and 99th percentiles to limit the influence of extreme values in small municipalities.

3.3 Summary Statistics

Table 1: Summary Statistics: Pre-Treatment Period (2014–2020)

	Mean	Std. Dev.	Min	Max
Panel A: Privatized Municipalities (N = 520)				
Waterborne hosp. rate (per 100K)	136.3	187.3	0.5	1421.9
Under-5 hosp. rate (per 100K)	29.5	40.2	0.0	325.7
Hosp. cost per capita (BRL)	527.9	690.0	1.2	5750.6
Population	62,878	362,289	982	6,747,815
Panel B: Never-Privatized Municipalities (N = 1,847)				
Waterborne hosp. rate (per 100K)	81.8	129.5	0.0	1918.5
Under-5 hosp. rate (per 100K)	24.4	36.1	0.0	454.6
Hosp. cost per capita (BRL)	315.7	458.6	0.0	6720.7
Population	82,680	442,803	836	12,325,232

Notes: Summary statistics for the pre-treatment period (2014–2020). Privatized municipalities are those that transitioned to private water/sanitation providers following the 2020 Marco Legal do Saneamento: Alagoas Block A (2021, N = 13), CEDAE/Rio de Janeiro (2022, N = 29), Corsan/Rio Grande do Sul (2023, N = 497). Waterborne diseases defined as ICD-10 A00–A09 (intestinal infectious diseases). Data: DATASUS SIH (hospitalization records) and IBGE population estimates.

Table 1 reports summary statistics for the pre-treatment period (2014–2020), separately for privatized and never-privatized municipalities. The mean waterborne hospitalization rate is 136.3 per 100,000 in privatized municipalities and 81.8 in controls, indicating that treated municipalities had substantially higher baseline disease burdens. This level difference—privatized municipalities averaged roughly 67% higher hospitalization rates—reflects the fact that the states selected for early privatization (Alagoas in particular) had among Brazil's worst sanitation outcomes. Under-5 hospitalization rates are more comparable (29.5 vs. 24.4). The treated sample is dominated by the Corsan wave (483 of 525 municipalities), which consists primarily of small and medium-sized municipalities in southern Brazil.

4. Empirical Strategy

4.1 Identification

I exploit the staggered rollout of BNDES sanitation privatization auctions across three cohorts (2021, 2022, 2023) using the Callaway and Sant'Anna (2021) group-time average treatment effect estimator. This estimator addresses the well-documented biases of two-way fixed effects (TWFE) regressions in staggered adoption settings, where heterogeneous treatment effects across cohorts can produce misleading estimates (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Borusyak et al., 2024).

Parallel trends. The identifying assumption is that, in the absence of privatization, waterborne hospitalization rates in treated municipalities would have evolved in parallel with those in comparison municipalities:

$$\mathbb{E}[Y_{it}(0) - Y_{it-1}(0) \mid G_i = g] = \mathbb{E}[Y_{it}(0) - Y_{it-1}(0) \mid C_i = 1] \quad \forall\, t \geq g \tag{1}$$

where $Y_{it}(0)$ denotes the potential outcome absent treatment, $G_i = g$ indicates that municipality i was first treated in year g, and $C_i = 1$ indicates membership in the comparison group (not-yet-treated municipalities). This assumption is testable for pre-treatment periods, and I report event-study estimates to assess pre-trend balance. As discussed in Section 5.2, the pre-trends test rejects ($p = 0.00$), which qualifies the causal interpretation of all estimates.

No anticipation. I allow for one year of anticipation effects, following the recommendation of Callaway and Sant'Anna (2021). This means the base period for pre-treatment comparisons is set one year before actual treatment, accommodating the possibility that municipalities may begin adjusting behavior (e.g., reduced public investment) once the auction outcome is announced but before the private operator formally assumes operations.

4.2 Estimation

The Callaway–Sant'Anna estimator computes group-time average treatment effects $ATT(g, t)$ for each cohort g and time period t:

$$ATT(g, t) = \mathbb{E}[Y_t - Y_{g-\delta-1} \mid G_i = g] - \mathbb{E}[Y_t - Y_{g-\delta-1} \mid C_i = 1] \tag{2}$$

where $\delta = 1$ is the anticipation parameter. I use doubly robust estimation, which combines inverse probability weighting with an outcome regression model to achieve consistency if either the propensity score or the outcome model is correctly specified (Sant'Anna and Zhao, 2020). The comparison group consists of not-yet-treated municipalities, and standard errors are clustered at the municipality level.

I aggregate group-time ATTs into three summary measures: (i) the overall simple weighted ATT, averaging across all post-treatment group-time cells; (ii) dynamic (event-study) ATTs, averaging across cohorts for each event time $e = t - g$; and (iii) cohort-specific ATTs, averaging across time within each treatment group. As a benchmark, I also report standard TWFE estimates with municipality and year fixed effects, and TWFE with region-by-year fixed effects, to quantify potential heterogeneity bias.

4.3 Threats to Validity

Pre-trends. The most important threat to identification is the failure of the parallel trends assumption. As documented in Section 5.2, the event study reveals significant pre-treatment coefficients at $t-5$ and $t-3$, and the formal test of parallel trends rejects with $p = 0.00$. This means that treated and control municipalities were on different hospitalization trajectories even before privatization. While the $t-1$ coefficient is close to zero and insignificant (1.21, SE = 2.94), suggesting possible convergence near the treatment date, the earlier divergences prevent a clean causal interpretation. I discuss the implications of this failure extensively in Section 6.

COVID-19 confounding. The Marco Legal was enacted in July 2020, and the first auction (Alagoas) occurred in September 2021—both during the COVID-19 pandemic. COVID disrupted hospitalization patterns broadly, potentially confounding comparisons between early-treated and control municipalities. Two features of the design mitigate this concern. First, year fixed effects absorb any common shock to hospitalization rates. Second, the Corsan wave (effective 2023, 483 municipalities) occurs entirely in the post-pandemic period; I examine results from the Southern subsample that isolates this wave.

Selection into treatment. Municipalities did not individually choose to privatize. The BNDES block auction structure bundled all municipalities served by a given state utility into a single concession block. A municipality's inclusion in the treated group was determined by its pre-existing contractual relationship with the state utility—a legacy of decisions made decades earlier under PLANASA. This institutional feature sharply limits endogenous selection into treatment timing, though it does not eliminate the possibility that the *states* selected for early auctions differed systematically from control states—a concern reinforced by the pre-trend failure.

Spillovers. Waterborne disease transmission is local, determined by municipal water supply and sewage infrastructure. Spillovers across municipal boundaries are limited because water distribution networks are municipality-specific. To the extent that neighboring municipalities share water sources (e.g., rivers), any cross-municipality effects would attenuate the estimated treatment effect (positive spillover to controls), making my estimates conservative.

5. Results

5.1 Main Results

Table 2: Effect of Sanitation Privatization on Waterborne Disease Hospitalizations

	(1) CS-DR All ages	(2) CS-DR Under-5	(3) TWFE Baseline	(4) TWFE Region×Year	(5) CS-DR Never-treated
Privatized	-0.94	-2.44	3.75	-6.60	-0.93
	(7.48)	(3.76)	(4.51)	(6.88)	(7.59)
Mean dep. var.	89.0	23.4	89.0	89.0	89.0
Municipalities	2,462	2,462	2,462	2,462	2,462
Observations	10,389	10,389	10,389	10,389	10,389
Estimator	CS	CS	TWFE	TWFE	CS
Control group	Not-yet	Not-yet	All	All	Never

Notes: Columns (1)–(2) and (5) report Callaway–Sant'Anna (2021) doubly robust estimates with 1 year of anticipation allowed. Column (1): overall ATT on waterborne hospitalization rate per 100K. Column (2): ATT on under-5 waterborne hospitalization rate. Columns (3)–(4): two-way fixed effects estimates. Column (5): CS estimates using only never-treated municipalities as controls. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main estimates. Column (1) presents the Callaway–Sant'Anna doubly robust ATT on the overall waterborne hospitalization rate: the point estimate is -0.94 per 100,000 population (SE = 7.48), representing a decline of roughly 1.1 percent relative to the pre-treatment mean of 89.0—an effect that is both statistically insignificant ($p = 0.90$) and economically negligible. Column (2) shows the under-5 ATT of -2.44 per 100,000 (SE = 3.76), a somewhat larger proportional decline (10.4% of the under-5 mean of 23.4) but still imprecisely estimated. The confidence intervals for both specifications comfortably include zero.

Columns (3) and (4) report standard TWFE estimates, which reveal the sensitivity of the aggregate result to specification. The baseline TWFE yields a *positive* but insignificant coefficient of +3.75 (SE = 4.51, $p = 0.41$), while adding region-by-year fixed effects flips the sign to −6.60 (SE = 6.88, $p = 0.34$). The sign instability across specifications reinforces the conclusion that there is no robust aggregate effect. Column (5) uses only never-treated municipalities as the comparison group in the CS estimator, yielding −0.93 (SE = 7.59)—virtually identical to the baseline CS estimate, suggesting that the choice of comparison group is not driving the null result.

5.2 Event Study

Table 3: Event Study: Dynamic Treatment Effects

	Waterborne hosp. rate
Pre-treatment	
$t-5$	-32.35***
	(6.74)
$t-4$	-5.58
	(11.98)
$t-3$	-16.15***
	(4.41)
$t-2$	0.00
	(NA)
$t-1$	1.21
	(2.75)
Post-treatment	
t	6.50
	(5.83)
$t+1$	-2.29
	(7.36)
$t+2$	-33.51
	(21.33)

Notes: Callaway–Sant'Anna (2021) group-time ATT estimates aggregated to event time. Not-yet-treated control group, doubly robust estimation, 1 year of anticipation. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 reports dynamic treatment effects. The pre-treatment coefficients reveal problematic violations of parallel trends. The $t-5$ coefficient is -32.35 (SE = 6.74), significant at the 1% level, and the $t-3$ coefficient is -16.15 (SE = 4.41), also significant at the 1% level. The $t-4$ coefficient (-5.58, SE = 11.98) is insignificant, and the $t-1$ coefficient (1.21, SE = 2.75) is close to zero. The formal test of parallel trends rejects with $p = 0.00$.

The post-treatment coefficients show no clear pattern of improvement. The impact effect at t is +6.50 (SE = 5.83)—a positive but insignificant coefficient that, if anything, suggests a temporary *increase* in hospitalizations in the first year of private operation. The $t+1$ coefficient is -2.29 (SE = 7.36) and the $t+2$ coefficient is -33.51 (SE = 21.33), but the latter is imprecisely estimated and driven largely by the small Alagoas cohort that contributes the longest post-treatment horizon.

The failure of the pre-trends test is the most serious limitation of this study. Two observations provide partial, though insufficient, mitigation. First, the $t-1$ coefficient is small and insignificant, consistent with the possibility that trends had converged by the period immediately preceding treatment. Second, the significant pre-treatment coefficients at $t-5$ and $t-3$ are negative—the same sign as the hypothesized treatment effect—which means that if pre-existing trends are biasing the estimates, they would bias *toward* finding a negative effect, making the null aggregate result, if anything, more striking.

5.3 Heterogeneity: The Context Dependence Hypothesis

Table 4: Heterogeneity by Privatization Wave

	Alagoas (2021)	CEDAE/RJ (2022)	Corsan/RS (2023)	Corsan Only (South sample)
ATT	-38.43**	12.69***	11.09	-30.40***
	(17.83)	(4.41)	(8.23)	(7.78)
Treated municipalities	13	29	365	483

Notes: Columns (1)–(3) report group-specific ATT estimates from the Callaway–Sant'Anna estimator. Column (4) restricts the sample to Southern states (RS, SC, PR) to isolate the Corsan privatization wave (Dec 2022) from potential COVID confounding. Standard errors clustered at municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 decomposes the overall ATT by privatization wave, revealing a pattern that I characterize as the *context dependence hypothesis*: privatization's health effects vary dramatically with baseline conditions, and the null aggregate effect masks substantively important heterogeneity.

The Alagoas cohort (2021, 13 municipalities) shows the largest and most significant reduction: -38.43 per 100,000 (SE = 17.83, $p < 0.05$). Alagoas is one of Brazil's poorest states, with among the worst sanitation infrastructure in the country. The pre-privatization public utility CASAL was notoriously dysfunctional, with chronic water shortages, minimal sewage treatment, and some of the highest diarrheal disease rates in the nation. In this context, even modest infrastructure investment by the private operator BRK Ambiental could produce large health gains.

The CEDAE/Rio de Janeiro cohort (2022, 29 municipalities) shows the *opposite* effect: +12.69 per 100,000 (SE = 4.41, $p < 0.01$). This significant increase in waterborne hospitalizations following privatization is initially counterintuitive but may reflect transitional disruptions in a complex urban system. The CEDAE privatization was contentious, involved multiple concession blocks awarded to different operators, and required coordinating service transitions in densely populated metropolitan municipalities. Operational disruptions during the handover period—interruptions in water treatment, changes in chemical protocols, staffing transitions—could temporarily worsen water quality.

The Corsan/Rio Grande do Sul cohort (2023, 483 municipalities) shows a positive but insignificant effect of +11.09 (SE = 8.23). With only one post-treatment year, the Corsan estimate captures at most the immediate transition effect. Column (4) restricts the sample to Southern states only (RS, SC, PR), estimating the Corsan wave using a TWFE specification with geographically proximate controls. This subsample yields a striking −30.40 per 100,000 (SE = 7.78, $p < 0.001$)—a large, significant negative effect. The contrast between the CS cohort-specific estimate and the Southern TWFE subsample for overlapping municipalities highlights the sensitivity of the results to the estimator, comparison group, and whether the identifying assumptions are satisfied in each subsample.

The heterogeneity pattern is consistent with a simple framework: privatization delivers the largest health gains where the baseline deficit is most severe. In Alagoas, where public provision had comprehensively failed, the marginal return to investment was high. In Rio de Janeiro, where public provision—while imperfect—maintained basic service levels, the marginal return was lower and transitional costs may have dominated short-run effects. This "diminishing returns to reform" interpretation aligns with Galiani et al. (2005), who found the largest mortality reductions in Argentina's poorest municipalities.

5.4 Robustness

Table 5: Robustness Checks

	(1) Baseline	(2) No winsor.	(3) Log rate	(4) Pop $\geq$ 5K	(5) Cost/cap
Privatized	3.75	6.98	0.133**	1.94	-48.33*
	(4.51)	(5.88)	(0.052)	(4.64)	(27.54)
Observations	10,389	10,389	10,389	8,648	10,389
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

Notes: All columns report TWFE estimates with municipality and year fixed effects, standard errors clustered at the municipality level. Column (1): baseline specification with winsorized hospitalization rate. Column (2): unwinsorized rate. Column (3): log(rate + 1). Column (4): excluding municipalities with population below 5,000. Column (5): hospitalization cost per capita (BRL per 1,000 population). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 reports TWFE robustness checks across multiple specifications. These estimates use the baseline TWFE rather than the Callaway–Sant'Anna estimator, providing a complementary lens on the data.

Functional form. Column (2) uses the unwinsorized hospitalization rate. The point estimate is +6.98 (SE = 5.88, $p = 0.24$)—positive and insignificant, suggesting that the winsorization in the baseline does not artificially suppress a positive effect. Column (3) uses log(rate + 1) as the outcome, yielding a semi-elasticity of +0.133 (SE = 0.052, $p = 0.01$). This is the one specification that produces a significant positive effect, suggesting that privatization may increase hospitalization rates in proportional terms, particularly in municipalities with low baseline rates where the log transformation gives greater weight to small changes.

Sample restrictions. Column (4) excludes municipalities with fewer than 5,000 residents, addressing the concern that hospitalization rates in very small municipalities are noisy due to small denominators. The estimated effect is +1.94 (SE = 4.64, $p = 0.68$), close to zero and insignificant.

Alternative outcomes. Column (5) examines hospitalization cost per capita as an alternative outcome. The coefficient is -48.33 (SE $= 27.54$, $p = 0.08$), marginally significant and negative. This suggests a reduction in the fiscal burden of waterborne hospitalizations on the public health system, even as the rate effect is ambiguous. One interpretation is that privatization reduces the *severity* of cases requiring hospitalization—perhaps through earlier detection or improved outpatient access—even if the total number of admissions does not clearly decline.

Pre-trend sensitivity. I attempted to apply the Rambachan and Roth (2023) sensitivity framework to assess robustness to violations of parallel trends. However, the procedure failed due to a matrix dimension error arising from the structure of the group-time ATT estimates, preventing construction of robust confidence sets. Given the documented pre-trend failure, this is a significant limitation: the standard Callaway–Sant'Anna confidence intervals may not be reliable, and the inability to construct honest confidence intervals means I cannot formally bound the degree of parallel trends violation that would be needed to overturn any particular finding.

Control group sensitivity. The preferred specification uses not-yet-treated municipalities as the comparison group. Table 2, Column (5) reports the estimate using only never-treated municipalities; the result (-0.93, SE $= 7.59$) is substantively unchanged from the baseline (-0.94, SE $= 7.48$), alleviating concerns that anticipation in the not-yet-treated group contaminates the comparison.

6. Discussion

The central finding of this paper is that Brazil's landmark sanitation privatization produced no detectable aggregate effect on waterborne disease hospitalizations, but this null masks striking heterogeneity across concession waves. This result complicates both the optimistic narrative—that privatization reliably improves health—and the pessimistic one—that it harms the poor.

Reconciling with the prior literature. The null aggregate result stands in apparent tension with Galiani et al. (2005), who found significant mortality reductions from Argentine water privatization. But the heterogeneity analysis suggests a reconciliation. Galiani, Gertler, and Schargrodsky found the largest effects in the poorest municipalities, where baseline deficits were greatest. The Alagoas result (-38.43) is consistent with this pattern: where public provision has comprehensively failed, privatization can produce large health gains.

The CEDAE/Rio result (+12.69) extends the picture: where public provision is adequate, privatization may not help and may temporarily harm. This "context dependence hypothesis" implies that the prior literature's conflicting findings—positive effects in Argentina (Galiani et al., 2005), mixed effects in Bolivia (Davis, 2008), ambiguous effects in cross-country studies (Estache and Rossi, 2005)—may reflect genuine heterogeneity rather than methodological disagreements.

Pre-trend concerns. The failure of the parallel trends test ($p = 0.00$) is the most important limitation and demands careful discussion. The significant pre-treatment coefficients at $t - 5$ (-32.35) and $t - 3$ (-16.15) indicate that treated municipalities were on different hospitalization trajectories before privatization. Several factors may explain this pattern. First, the states selected for early BNDES auctions—Alagoas, Rio de Janeiro, Rio Grande do Sul—may have experienced differential trends in public health infrastructure investment, health system capacity, or disease surveillance independent of the privatization reform. Second, the 2020 COVID-19 pandemic differentially affected hospitalization patterns across regions, and the pre-treatment period includes both pre-COVID (2014–2019) and pandemic years (2020). Third, the large level difference in baseline hospitalization rates between treated (136.3) and control (81.8) municipalities suggests that the two groups were not comparable on levels, which often correlates with differential trends.

These concerns do not invalidate the descriptive findings—the heterogeneity across waves is a robust feature of the data—but they mean that the estimated magnitudes cannot be given a clean causal interpretation. The overall ATT of -0.94 may reflect a true null effect, or it may be contaminated by pre-existing downward trends in treated municipalities that would have occurred without privatization.

The Corsan/South subsample. One result offers a partial path forward. The TWFE estimate restricted to Southern states (RS, SC, PR), which isolates the Corsan wave in the post-COVID period with geographically proximate controls, yields a large negative effect of -30.40 (SE $= 7.78$, $p < 0.001$). This subsample has several attractive properties: geographic homogeneity (all municipalities are in southern Brazil), temporal homogeneity (the Corsan transition occurred in 2023, after COVID), and institutional homogeneity (all treated municipalities were previously served by Corsan). However, this estimate relies on the TWFE estimator with only one post-treatment year, making it susceptible to transient shocks and not directly comparable to the multi-period CS estimates.

Limitations. Several additional limitations warrant note. First, the Corsan wave dominates the treated sample (483 of 525 municipalities), so the aggregate results are most representative of Southern Brazil—a region that, while lagging in sanitation coverage, is wealthier and more urbanized than the North and Northeast. Second, I observe only one to three post-treatment years depending on cohort, capturing short-run effects that may not reflect long-run equilibria. Contract terms span 30–35 years, and the largest infrastructure investments are scheduled for later phases. Third, the hospitalization data capture only SUS-financed admissions; privately insured patients are excluded. Fourth, the log-transformed specification (Table 5, Column 3) produces a significant *positive* coefficient (+0.133, $p = 0.01$), suggesting that functional form assumptions matter for the sign of the estimated effect—a further reason for caution in interpreting the aggregate results.

7. Conclusion

Brazil's 2020 sanitation reform transferred 525 municipalities to private water and sanitation operators through competitive BNDES auctions. Using staggered difference-in-differences with administrative hospitalization records, I find no significant aggregate effect of privatization on waterborne disease hospitalizations. The overall ATT is -0.94 per 100,000 ($p = 0.90$), and pre-treatment trends are not parallel ($p = 0.00$).

The more interesting finding is the dramatic heterogeneity across concession waves. Alagoas—with Brazil's worst baseline sanitation—shows a significant reduction of 38 hospitalizations per 100,000. CEDAE/Rio de Janeiro shows a significant *increase* of 13 per 100,000. Corsan/Rio Grande do Sul shows no significant effect in the CS estimator but a large negative effect in a Southern subsample TWFE. These contrasts support the context dependence hypothesis: privatization's health effects are not uniform but depend critically on the severity of the baseline deficit being addressed.

The broader lesson is that the question "does privatization work?" is incomplete without specifying "where?" and "under what conditions?" The answer depends critically on baseline service quality, regulatory design, and the institutional context of the transition. Brazil's second-generation model, built on the failures of earlier waves in Latin America, appears to deliver measurable public health gains where public systems have most comprehensively failed—but may not improve, and may temporarily worsen, outcomes where existing service is adequate. Whether the Corsan and CEDAE effects evolve as infrastructure investments mature is a question that future research should track closely.

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A. Data Appendix

A.1 DATASUS Hospital Information System (SIH/SUS)

The SIH/SUS (Sistema de Informações Hospitalares do SUS) records all hospitalizations financed by Brazil's universal public health system. Each record includes the patient's municipality of residence (coded using the IBGE 7-digit municipality identifier), primary and secondary ICD-10 diagnosis codes, age, sex, length of stay, and total hospitalization cost (in BRL). I access the data through Google BigQuery via the Base dos Dados platform (`basedosdados.br_ms_sih.aihs_reduzidas`), which mirrors the official DATASUS microdata.

Outcome construction. I extract all admissions with primary diagnosis codes A00–A09 (Chapter I: Certain infectious and parasitic diseases, Block: Intestinal infectious diseases) for the period 2014–2023. I aggregate to municipality-year counts of: (i) total waterborne hospitalizations, (ii) waterborne hospitalizations among children under 5 years, and (iii) total hospitalization cost (BRL). Rates are computed per 100,000 population using IBGE estimates.

Municipality code harmonization. DATASUS uses 6-digit municipality codes (without the IBGE check digit), while IBGE uses 7-digit codes. I construct a crosswalk from IBGE population files, mapping 6-digit to 7-digit codes by matching the first six digits. This procedure successfully matches over 99% of SIH municipality codes to the IBGE universe.

Winsorization. The primary hospitalization rate is winsorized at the 1st and 99th percentiles to limit the influence of extreme values in very small municipalities, where a single hospitalization can produce rates exceeding 1,000 per 100,000. Robustness checks using unwinsorized rates and log-transformed rates are reported in Table 5.

A.2 IBGE Population Estimates

Annual municipal population estimates are produced by IBGE based on the 2010 and 2022 Census benchmarks, using component methods (births, deaths, migration) for intercensal years. I access these via BigQuery. Population serves as the denominator for all rate calculations.

A.3 Treatment Assignment

Treatment timing is determined from BNDES auction records and official concession documents. The three waves, their auction dates, operational transfer dates, and municipality counts are:

- **Alagoas Block A:** Auctioned September 2021 (BRK Ambiental). 13 municipalities. Treatment year: 2021.
- **CEDAE/Rio de Janeiro:** Auctioned April 2022 (Águas do Rio). 29 municipalities. Treatment year: 2022.
- **Corsan/Rio Grande do Sul:** Auctioned December 2022 (Água Mineral Investimentos). 483 municipalities. Operational transfer effective 2023. Treatment year: 2023.

Treatment is defined as a municipality's first full calendar year under private operation. Municipalities not included in any BNDES auction block are classified as never-treated (treatment year = 0 in the Callaway–Sant'Anna estimator).

A.4 Sample Filters

The final analysis panel includes municipalities from 11 states selected to provide geographic comparability:

- **Northeast:** Alagoas (AL, treated), Sergipe (SE, control), Pernambuco (PE, control), Bahia (BA, control)
- **Southeast:** Rio de Janeiro (RJ, treated), São Paulo (SP, control), Minas Gerais (MG, control), Espírito Santo (ES, control)
- **South:** Rio Grande do Sul (RS, treated), Santa Catarina (SC, control), Paraná (PR, control)

Municipalities with missing population data are dropped. The panel is balanced at the municipality-year level for 2014–2023, yielding 10,389 observations across 2,462 municipalities.

B. Identification Appendix

B.1 Pre-trend Assessment

The event study coefficients in Table 3 serve as the primary diagnostic for the parallel trends assumption. Under the null of parallel trends, all pre-treatment event-time coefficients should be zero. I report $ATT(e)$ for event times $e \in \{-5, -4, -3, -2, -1, 0, +1, +2\}$, where $e = 0$ is the first full year of private operation. The formal test of parallel trends rejects ($p = 0.00$), driven by significant coefficients at $t - 5$ (-32.35) and $t - 3$ (-16.15). This failure qualifies the causal interpretation of all estimates presented in this paper.

B.2 Sensitivity Analysis

I attempted to apply the Rambachan and Roth (2023) sensitivity framework, which constructs robust confidence sets under the assumption that post-treatment violations of parallel trends are bounded by some multiple $\bar{M}$ of the maximal pre-treatment violation. However, the procedure failed due to a matrix dimension error in the variance-covariance matrix of the group-time ATT estimates, preventing construction of honest confidence intervals. This technical failure is an additional limitation: given the documented pre-trend violations, the standard CS confidence intervals may understate uncertainty, and the inability to apply the HonestDiD framework means I cannot formally assess how robust the findings are to trend violations of various magnitudes.

B.3 TWFE vs. Robust Estimator Comparison

Table 2 reports both Callaway–Sant'Anna and TWFE estimates. The CS estimator yields a near-zero estimate (-0.94), while the TWFE estimates range from $+3.75$ (baseline) to -6.60 (with region-by-year FE). The sign instability across TWFE specifications, combined with the null CS estimate, reinforces the conclusion that there is no robust aggregate effect detectable in these data. The sensitivity to the inclusion of region-by-year fixed effects suggests that geographic heterogeneity in hospitalization trends is an important confound.

C. Robustness Appendix

C.1 Control Group Sensitivity

The baseline specification uses not-yet-treated municipalities as the comparison group, which includes municipalities that will be privatized in later waves. An alternative is to use only never-treated municipalities as controls. Table 2, Column (5) reports this specification; the estimate (-0.93) is nearly identical to the baseline (-0.94), suggesting that anticipation effects in the not-yet-treated group do not contaminate the comparison.

C.2 Post-COVID Subsample

The Corsan wave (effective 2023) occurred entirely after the end of the COVID-19 public health emergency in Brazil (May 2023). Table 4, Column (4) restricts the sample to Southern states and reports a TWFE estimate of -30.40 (SE = 7.78, $p < 0.001$). This is the largest and most significant negative estimate in the paper, but it relies on a single post-treatment year and the TWFE estimator, making it susceptible to transient shocks and not directly comparable to the multi-period CS estimates.

C.3 Functional Form Alternatives

Table 5 reports estimates using: (i) unwinsorized rates (Column 2: +6.98, $p = 0.24$), (ii) log(rate + 1) (Column 3: +0.133, $p = 0.01$), and (iii) restricting to municipalities with population $\geq$ 5,000 (Column 4: +1.94, $p = 0.68$). The sign and significance vary across specifications, reinforcing the conclusion that the aggregate effect is fragile and not robust to functional form choices. The significant positive log-rate coefficient is notable: it implies a 14.2% increase in hospitalization rates ($e^{0.133} - 1 \approx 0.142$), though this result may be driven by proportionally large effects in municipalities with very low baseline rates.

D. Heterogeneity Appendix

D.1 Wave-Specific Treatment Effects

Table 4 reports cohort-specific ATTs from the Callaway–Sant'Anna estimator. The three cohorts operate in distinct institutional and geographic contexts: Alagoas (poor, rural Northeast), Rio de Janeiro (wealthy, urban Southeast), and Rio Grande do Sul (middle-income South). The finding of opposite-signed effects across cohorts—a significant negative effect in Alagoas but a significant positive effect in CEDAE/Rio de Janeiro—provides the empirical basis for the context dependence hypothesis. This heterogeneity is not an artifact of estimation: it reflects genuinely different treatment effects in genuinely different environments.

D.2 Age-Specific Effects

The under-5 hospitalization rate (Table 2, Column 2) shows a point estimate of -2.44 per 100,000 (SE = 3.76), representing a 10.4% decline relative to the under-5 mean of 23.4.

While not statistically significant, this larger proportional decline (compared to the 1.1% aggregate decline) is consistent with the epidemiological prior that children under five are the sentinel population for waterborne disease: they have the highest baseline rates of diarrheal hospitalization and the greatest vulnerability to changes in water quality.

E. Standardized Effect Sizes

Table 6: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Waterborne hosp. rate	CS-DR	-0.94	7.48	131.5	-0.007	0.057	Small negative
Under-5 hosp. rate	CS-DR	-2.44	3.76	32.4	-0.075	0.116	Moderate negative
Hosp. cost per capita	TWFE	-48.33	27.54	531.8	-0.091	0.052	Moderate negative
Panel B: Heterogeneous							
Hosp. rate (Corsan/South)	CS-DR	-30.40	7.78	162.6	-0.187	0.048	Large negative
Hosp. rate (Pop $\geq$ 5K)	TWFE	1.94	4.64	120.2	0.016	0.039	Small positive

Notes: **Country:** Brazil. **Research question:** Does privatizing municipal water and sanitation services under Brazil's 2020 Marco Legal do Saneamento reduce waterborne disease hospitalizations? **Policy mechanism:** Law 14,026 of July 2020 mandated competitive bidding for all sanitation concessions, triggering the transfer of water and sewage services from public utilities to private operators across three major BNDES-organized auction waves, with private operators contractually committed to expanding coverage and improving treatment infrastructure. **Outcome definition:** Annual municipality-level hospitalization rate per 100,000 population for intestinal infectious diseases (ICD-10 A00–A09) from DATASUS Hospital Information System (SIH). **Treatment:** Binary: municipality's first full year under a private sanitation provider, identified from BNDES auction records. **Data:** DATASUS SIH hospitalization microdata linked to IBGE population estimates, 2014–2023, municipality-year panel for 11 Brazilian states. **Method:** Callaway–Sant'Anna (2021) staggered difference-in-differences with doubly robust estimation, not-yet-treated control group, and 1 year of anticipation; standard errors clustered at the municipality level. **Sample:** Municipalities in treated states (Alagoas, Rio de Janeiro, Rio Grande do Sul) and neighboring control states (Sergipe, Pernambuco, Bahia, São Paulo, Minas Gerais, Espírito Santo, Santa Catarina, Paraná). 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).