

Bitter Pills to Swallow: The Enforcement Costs of Health Litigation*

Darcio Genicolo-Martins^{a,*}, Paulo Furquim de Azevedo^a

^a*Inspere Institute of Education and Research, Rua Quatá 300, São Paulo, SP 04546-042, Brazil*

Abstract

Judicial enforcement is essential for the rule of law, yet the *form* of enforcement can generate economic inefficiency. We study this tension using bid-level data on court-mandated pharmaceutical procurement in São Paulo, Brazil (2009–2019). With item, time, and buyer fixed effects, court orders raise prices by 5.4% and reduce competition by 5.4%. Exploiting an institutional distinction between urgent purchases that share planning constraints but differ in penalty exposure, we isolate the “under the gun” effect: judicial sanctions alone raise costs by 23–30%. This premium operates entirely through demand fragmentation—sanctions prevent order aggregation—not through officials accepting higher unit prices. The finding speaks to a general asymmetry in accountability design: when non-completion is punished more severely than overpayment, compliance crowds out cost-efficiency.

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*Corresponding author.

Email addresses: darciogm1@insper.edu.br (Darcio Genicolo-Martins),
paulofa1@insper.edu.br (Paulo Furquim de Azevedo)

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1. Introduction

A judge orders the government to buy a cancer drug within 72 hours. The procurement officer has no time to aggregate demand, no time to research market prices, and faces personal fines if the purchase fails. The drug arrives—but at a 30% total cost premium over what the same agency pays when no court is watching.

This paper estimates the fiscal costs of such judicial enforcement. Courts and regulatory agencies are alternative instruments of social control ([Glaeser and Shleifer, 2003](#)), but court intervention in procurement compresses planning timelines and distorts incentives in ways that raise prices and reduce competition ([Coviello et al., 2018](#)). In health care, the stakes are high: judicial mandates directly determine how—and at what price—governments acquire essential medicines ([Wang, 2015](#); [Ferraz, 2009](#)). Brazil’s 1988 Constitution enshrines health as a “right of all and a duty of the state,” and courts have granted virtually every medication request—96,000 first-instance claims nationally in 2017, a 913% increase in São Paulo since 2008 ([CNJ/INSPER, 2019](#)).

We use bid-level data covering all pharmaceutical procurement by the São Paulo State Department of Health (SES/SP) from 2009 through 2019:

over 479,000 purchase-offer-item observations conducted through the state’s electronic procurement platform (BEC). Court orders and administrative requests partition purchases into *ordinary* (standard timeline) and *urgent* (compressed timeline). Within urgent purchases, a further institutional distinction proves useful: *administrative* requests carry no penalty for procurement failure, while *litigated* purchases expose officials to fines, personal liability, and fund seizure. Comparing these two types—which share planning constraints but differ in penalty exposure—isolates what we call the “under the gun” effect. We estimate high-dimensional fixed effects regressions with item, year (or year-month), and buyer (PBU) fixed effects, clustering at the PBU level.

Urgent purchases carry negotiated prices 5.4% higher (3.0% net of quantity), reference prices 2.7% higher, and attract 5.4% fewer bidders—yet succeed *more often* (2.1 pp), consistent with officials accepting worse terms to avoid failure. The “under the gun” comparison is sharper: litigated tenders cost 23–30% more than administrative ones that face identical planning constraints but no sanctions. This premium operates entirely through demand fragmentation, not direct price inflation. A placebo on never-litigated items confirms the premium is absent where courts play no role, and supplier fixed effects reveal the cost splits equally between officials accepting worse matches and firms exploiting judicial urgency.

Three contributions follow. First, we provide the first estimates of the fiscal cost of judicial enforcement in public procurement, contributing to the lit-

erature on government spending efficiency (Bandiera et al., 2009; Best et al., 2023; Decarolis et al., 2020). Second, the “under the gun” comparison—exploiting within-urgent variation in penalty exposure—offers direct evidence that accountability mechanisms can generate unintended costs (Rasul and Rogger, 2018; Dufflo et al., 2012). Third, we show that judicial mandates distort the planning phase of procurement, adding to the evidence on ex ante design in auction performance (Coviello et al., 2018; Baltrunaite et al., 2021). Enforcement costs concentrate where competitive pressure is weakest (Online Appendix).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 presents results. Section 6 concludes.

Related Literature

The paper speaks to three literatures. On passive waste in public spending (Bandiera et al., 2009): prior work has studied bureaucratic capacity (Best et al., 2023), management practices (Rasul and Rogger, 2018), and procurement design (Lewis-Faupel et al., 2016; Coviello et al., 2018; Decarolis et al., 2020; Baltrunaite et al., 2021; Szücs, 2024). We identify a new channel—judicial compression of planning time. Unlike Coviello et al. (2018), where courts *extend* procurement timelines, ours is a setting where courts *compress* them. Second, on unintended costs of accountability: our “under the gun” effect shows that the threat of judicial punishment—intended to

ensure compliance—distorts procurement incentives in ways that raise costs (Duflo et al., 2012; Williams, 2021). Third, we add fiscal evidence to the legal and public health literatures on health litigation in Latin America (Wang, 2015; Ferraz, 2009; Biehl et al., 2009), which have focused on legal and health dimensions with limited attention to procurement costs.

2. Institutional Background

2.1. Health as a Constitutional Right and the Rise of Litigation

Brazil’s 1988 Constitution created the Unified Health System (SUS), which maintains a formulary of approved medications subject to cost-effectiveness criteria (Castro et al., 2019; Soares, 2019). Courts, however, have adopted a strict literal interpretation of Article 196 (“health is a right of all and a duty of the state”), under which any individual can obtain virtually any medication through litigation—from common analgesics to rare-disease treatments costing over US\$400,000 per year. Access is inexpensive (a prescription and a public defender suffice), and approximately 85% of first-instance claims are granted (CNJ/INSPER, 2019).

The result: 96,000 health court orders nationally in 2017, a 913% increase in São Paulo since 2008 (Online Appendix, Figure A.1). Litigated health spending alone amounts to approximately US\$300 million annually—nearly 5% of São Paulo’s public health budget.

2.2. Public Procurement under Judicial Pressure

The São Paulo State Department of Health (SES/SP) manages health policy through 99 decentralized public buyer units (PBUs) distributed across the state (Online Appendix, Figure A.2).

Federal Law 8,666/1993 requires all government purchases to follow a formal tendering process in three phases: (i) an **internal phase** where PBUs identify needs, specify items, set quantities and reference prices, choose the auction format, and publish a public notice; (ii) an **external phase** where firms compete through sealed-bid (*convite*) or multi-round descending auction (*pregão*); and (iii) a **delivery phase**.

For ordinary purchases, the internal phase typically spans 30 to 180 days. Court orders fundamentally disrupt this process. Almost all court decisions (99.94%) are enforced as preliminary injunctions with delivery deadlines of 1 to 10 days, compressing the internal phase to roughly one-third of the ordinary timeline. This compression affects procurement in several ways. First, PBUs lose the ability to aggregate demand across units and over time, reducing their bargaining leverage from bulk purchasing. Second, reference prices must be set quickly with limited market research, often resulting in higher reservation prices. Third, approximately 75% of litigated items are not on the SUS formulary, meaning PBUs have less experience procuring them. Fourth, roughly 65% of court orders involve “packages” combining SUS and non-SUS items, further complicating procurement planning.

2.3. *The Sanction Channel: Administrative vs. Litigated Purchases*

A critical institutional feature for our identification strategy is the distinction between *litigated* and *administrative* urgent purchases. Both types face identical planning constraints—compressed timelines, small quantities, and diverted budget resources. However, they differ sharply in the consequences of procurement failure.

For litigated purchases, failure to comply with a court order exposes public officials to severe sanctions: substantial fines (sometimes disproportionate to the purchase value), administrative and criminal liability, and seizure or blocking of public funds. These penalties are public information, as the court order must be referenced in the tender notice.

Since 2009, the SES/SP has attempted to mitigate these costs through administrative requests, a mechanism allowing the government to negotiate supply directly with individuals before litigation occurs. Administrative requests undergo evaluation by a scientific committee using evidence-based medicine criteria. Crucially, failure to complete an administrative purchase carries no penalties for public officials. Between 2009 and 2019, administrative requests generated approximately 9,700 purchases, or about 6.5% of the total from court orders.

This institutional distinction provides a natural comparison group for isolating the “under the gun” effect, since administrative and litigated purchases share all planning and execution constraints but differ only in the penalty regime. Figure 1 illustrates the three purchase types. The Online

Appendix presents complementary geographic evidence on the distribution of administrative demand.

Table 1: Purchase Types: Institutional Characteristics

	Ordinary	Administrative	Litigated
Source of funds	Dedicated budget	Diverted budget	Diverted budget
Quantity	Large	Small	Small
Delivery time	Long (30–180 d)	Short (1–10 d)	Short (1–10 d)
Threat of punishment	None	None	Fines, liability, fund seizure

Notes: Administrative and litigated purchases share planning constraints (short timelines, small quantities, diverted budgets) and differ only in the penalty regime. The “under the gun” comparison exploits this variation.

2.4. The Bidding Process

All purchases—ordinary and urgent—are conducted through the *Bolsa Eletrônica de Compras* (BEC), São Paulo’s electronic procurement platform, mandatory for common goods since 2007. Two competitive procedures are used: sealed-bid tendering (*convite*) and multi-round descending auctions (*pregão*) with a reverse auction phase. The key implication is that all purchase types use the same platform and procedures; the variation we exploit arises exclusively from differences in planning conditions and penalty regimes.

3. Data and Sample

3.1. Data Sources

We combine three administrative datasets covering January 2009 through December 2019: (i) bid-level records from the BEC electronic procurement platform for all SES/SP purchases of medical supplies (BEC Group 65), where each observation is a purchase-offer-item (POI) recording reference prices, bid prices, number of bidding firms, quantities, and tender outcomes; (ii) purchase type classification, where we assign each POI as ordinary, administrative, or litigated using a regular expression algorithm applied to tender notices, which by law must reference any underlying court order; and (iii) health litigation records from S-CODES, the SES/SP database of health-related lawsuits, from which we derive SUS formulary status and injunction enforcement data. All continuous variables are winsorized at the 1st and 99th percentiles; robustness checks with alternative winsorization are in the Appendix.

3.2. Sample Construction

The raw dataset comprises 479,330 POI observations across 32,532 distinct items, 94,182 purchase orders, and 100 PBUs (descriptive statistics in Online Appendix, Table A.1).

The analysis sample is restricted to items for which at least one ordinary and at least one litigated purchase is observed, ensuring within-item comparability. This yields approximately 226,000 observations covering 3,856

items.¹ For regressions using bid prices, we further restrict to winning bids (approximately 197,000 observations).

Several features of the data are noteworthy. In levels, ordinary purchases have substantially higher mean reference prices (R\$909) compared to administrative (R\$226) and litigated (R\$370) purchases, reflecting the mix of items across procurement types. However, the composition effect is absorbed by our item fixed effects. In logs—which form the basis of our regressions—litigated purchases show higher mean prices (1.56 for reference price, 1.02 for negotiated price) than ordinary purchases (0.93 and 0.32), consistent with a price premium conditional on the item. Quantities are markedly lower for litigated purchases (mean of 9,646 units vs. 31,487 for ordinary), reflecting the inability to aggregate demand under compressed timelines. The number of bidding firms is also lower for urgent purchases (2.2 for litigated vs. 3.2 for ordinary), suggesting reduced competition in the external phase.

The Online Appendix presents complementary geographic evidence on the spatial distribution of administrative demand, which mirrors the patterns documented for litigation in Section 2.

For the “under the gun” analysis, we construct a subsample of urgent purchases (administrative and litigated) for items with both types present, yielding approximately 57,000 observations (balance statistics in Online Appendix, Table A.2).

¹Sample sizes vary slightly across tables (196,883–226,305) due to missing values in specific dependent variables and the restriction to winners for price regressions.

4. Empirical Strategy

The analysis has two stages: we first estimate the overall cost of urgency by comparing ordinary and urgent purchases, then isolate the sanction channel by comparing litigated and administrative urgent purchases.

4.1. Identification

Identification requires that, conditional on fixed effects, urgency is uncorrelated with unobserved determinants of procurement outcomes. Three institutional features support this. First, court orders originate from patients' health needs and legal claims—not from the procurement process. Brazilian public administration operates under a principle of impersonality: tender outcomes depend on item specifications and market conditions, not on who requested the purchase. Second, court orders are unpredictable from the buyer's perspective. Only 4% of claims arise from stock shortages or service failures, so litigation is poorly correlated with PBU-level mismanagement. Third, the urgency regime is determined externally—by judges or by the SES/SP's scientific committee—not by the purchasing unit. While PBUs could in principle misclassify purchases, the legal requirement to reference court orders in tender notices, combined with our REGEX-based classification of official texts, limits this concern. An event study around each item's first court order (Online Appendix) shows prices *declining* in the years before litigation and rising sharply afterward—ruling out the reverse-causality story in which high prices attract lawsuits.

4.2. Enforcement Costs: Ordinary vs. Urgent

We estimate the following baseline specification:

$$y_{i,g,t} = \beta \cdot \text{Urgent}_{i,g,t} + \gamma_g + \lambda_t + \delta_b + \varepsilon_{i,g,t}, \quad (1)$$

where $y_{i,g,t}$ is the outcome for purchase order i of item g at time t ; γ_g are item fixed effects; λ_t are time fixed effects; and δ_b are PBU fixed effects. Standard errors are clustered at the PBU level. We report four specifications with progressively richer fixed effects, from item FE only through item + year-month + PBU FE. Our preferred specification (item + year + PBU FE) absorbs time-invariant item characteristics, common annual trends, and buyer-specific heterogeneity, while retaining sufficient within-cell variation for precise estimation.

For negotiated prices and number of firms, we additionally estimate a direct effect specification controlling for log quantity:

$$y_{i,g,t} = \beta \cdot \text{Urgent}_{i,g,t} + \alpha \cdot \ln Q_{i,g,t} + \gamma_g + \lambda_t + \delta_b + \varepsilon_{i,g,t}, \quad (2)$$

This isolates the direct effect of urgency net of the quantity channel, since urgent purchases involve smaller orders that forgo bulk discounts.

4.3. The “Under the Gun” Effect: Litigated vs. Administrative

To isolate the effect of judicial sanctions, we restrict the sample to urgent purchases only and estimate:

$$\ln(\text{Price}_{i,g,t}) = \beta \cdot \text{Admin}_{i,g,t} + \gamma_g + \lambda_t + \delta_b + \varepsilon_{i,g,t}, \quad (3)$$

where Admin equals 1 for administrative purchases and 0 for litigated. Since both types face identical planning constraints, β isolates the cost attributable to the sanction regime.

5. Results

5.1. Enforcement Costs: Prices, Competition, and Tender Success

Urgency distorts every stage of the procurement process. Reference prices—set during the planning phase—are 2.7% higher in the preferred specification ($p < 0.10$; Online Appendix, Table A.3). The raw premium is 17.8% with item FE alone but attenuates sharply with PBU FE, indicating that buyer-level heterogeneity accounts for much of the unconditional gap.

Negotiated prices are the core outcome (Table 2). Panel A reports the total effect; Panel B controls for log quantity.

Total effect (Panel A). Negotiated prices are significantly higher for urgent purchases across all specifications. The total effect ranges from 17.2% ($e^{0.159} - 1$) with item FE only to 5.4–5.8% with our preferred specifications

Table 2: Negotiated Prices

	(1)	(2)	(3)	(4)
<i>Panel A: Total Effect</i>				
Urgent Purchase	0.159*** (0.042)	0.151*** (0.038)	0.053*** (0.016)	0.056*** (0.016)
<i>Panel B: Direct Effect</i>				
Urgent Purchase	0.075 (0.045)	0.060 (0.042)	0.030* (0.017)	0.030* (0.017)
Log Quantity	-0.321*** (0.026)	-0.325*** (0.026)	-0.392*** (0.029)	-0.393*** (0.029)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	196,886	196,886	196,883	196,883
Within R ² (A)	0.003	0.003	0.000	0.000
Within R ² (B)	0.315	0.327	0.358	0.359

Notes: Dependent variable: log negotiated price. Sample: winners only. Panel A reports the total effect; Panel B controls for log quantity. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

(columns 3–4). The attenuation pattern mirrors that of reference prices, confirming the importance of buyer-level heterogeneity.

Direct effect (Panel B). Controlling for log quantity reduces the coefficient on urgency by roughly half in the most saturated specifications: from 0.053 to 0.030 (column 3). The direct effect of 3.0% ($e^{0.030} - 1$, $p < 0.10$) represents the price premium attributable to urgency net of the quantity channel. The coefficient on log quantity is large and negative (-0.392), confirming the importance of bulk discounts: a one standard deviation reduction in log

quantity is associated with substantially higher per-unit prices.

5.2. *Competition and Tender Success*

Urgent purchases attract 5.4% fewer bidding firms (-0.056 , $p < 0.01$; Online Appendix, Table A.4), consistent with reduced competition under compressed timelines (Bulow and Klemperer, 1996). Quantities also fall, though imprecisely (-0.058 , not significant; Online Appendix, Table A.5). Yet urgent tenders are 2.1 pp *more* likely to succeed ($p < 0.01$; Online Appendix, Table A.6)—precisely what the “under the gun” mechanism predicts: officials accept worse terms to ensure the purchase goes through. This also rules out sample selection: urgent tenders succeed *despite* less favorable conditions.

5.3. *The “Under the Gun” Effect*

The sharpest test compares litigated and administrative urgent purchases—identical planning constraints, different penalty exposure (Table 3).

Total effect (Panel A).. Administrative purchases have significantly lower negotiated prices than litigated ones across all specifications. In our preferred specification (column 3), the coefficient is -0.262 ($p < 0.05$), implying that litigated purchases are 30.0% ($e^{0.262} - 1$) more expensive than comparable administrative purchases. The magnitude is even larger with year-month FE (column 4): 30.3%.

Table 3: Under the Gun: Administrative vs Litigated

	(1)	(2)	(3)	(4)
<i>Panel A: Total Effect</i>				
Administrative (vs Litigated)	-0.209*** (0.074)	-0.205** (0.074)	-0.262** (0.096)	-0.265*** (0.094)
<i>Panel B: Direct Effect</i>				
Administrative (vs Litigated)	-0.006 (0.056)	0.016 (0.058)	0.117 (0.122)	0.115 (0.115)
Log Quantity	-0.284*** (0.036)	-0.292*** (0.038)	-0.341*** (0.042)	-0.344*** (0.040)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	56,803	56,803	56,803	56,802
Within R ² (A)	0.011	0.010	0.007	0.007
Within R ² (B)	0.287	0.292	0.307	0.310

Notes: Dependent variable: log negotiated price. Sample: urgent purchases only (administrative and litigated), winners, items with both types present. Panel A reports the total effect; Panel B controls for log quantity. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Direct effect (Panel B). Controlling for log quantity, the coefficient falls to 0.117 and becomes statistically insignificant (SE = 0.122, column 3). The 23–30% total cost premium operates entirely through demand fragmentation: judicial sanctions prevent quantity aggregation, yielding smaller orders that forgo bulk discounts. The direct price effect—what officials pay per unit for items of identical quantity—is indistinguishable from zero. A back-of-envelope calculation confirms the channel: the log-quantity gap between administrative and litigated purchases (1.34) times the bulk-discount elas-

ticity (-0.34) implies a 58% price difference from quantity fragmentation alone—more than enough to account for the 30% total premium. The fiscal cost of sanctions is not that officials accept worse prices under pressure, but that they cannot aggregate demand across patients and over time. Framework agreements for commonly litigated items would address this channel directly.

With item \times year-month fixed effects—comparing administrative and litigated purchases of the *same item in the same month*—the coefficient is virtually unchanged (-0.243 , $p < 0.05$, $N = 27,122$), ruling out within-item time-varying confounders as a driver.² Adding log quantity to this saturated specification drives the coefficient to zero (-0.010 , $SE = 0.081$), confirming that the premium operates entirely through demand fragmentation even under the tightest possible comparison. Crucially, regressing log quantity on the administrative indicator with item \times year-month and PBU fixed effects yields a coefficient of 0.787 ($p < 0.10$): for the same item in the same month, administrative orders are 120% larger than litigated ones. Sanctions do not merely correlate with smaller orders—they prevent demand aggregation.

A dose-response analysis reinforces the pattern: the urgency premium rises with the number of court orders per item-year (1.8% at 1–2 orders, 4.8% at 3–5, 6.1% at 6–10) but flattens for chronically litigated items (11+),

²Item \times year-month FE absorb 30,029 singleton observations, leaving 27,122 with within-cell variation. Of 40,966 item \times month cells, 4,402 (11%) contain both purchase types. The identifying variation, while limited, produces estimates indistinguishable from the baseline.

suggesting that the system adapts when litigation becomes routine.

These results demonstrate that judicial sanctions are a quantitatively important driver of procurement costs. The 23–30% total premium represents a substantial and previously unquantified cost of judicial enforcement in health procurement, operating entirely through the quantity channel.

5.4. Falsification: Items Never Litigated

If the urgency premium reflects the causal effect of judicial pressure rather than unobserved item characteristics, it should be absent for items that are never subject to court orders. We test this by running the preferred specification on items with zero litigated purchases across the entire 2009–2019 period, restricting to those with both ordinary and administrative purchases for within-item comparability.

	bid_price_log		bid_price_ref_log	
	(1)	(2)	(3)	(4)
Urgent Purchase	-0.0204 (0.0316)	0.0513*** (0.0153)	-0.0306 (0.0447)	0.0305** (0.0141)
Observations	39,283	196,883	46,874	226,278
R ²	0.83364	0.86796	0.81121	0.85183
Within R ²	7.03×10^{-6}	0.00022	1.73×10^{-5}	7.43×10^{-5}
item_id fixed effects	✓	✓	✓	✓
year_n fixed effects	✓	✓	✓	✓
pbu_id fixed effects	✓	✓	✓	✓

Placebo sample: items with zero litigated purchases across 2009-2019. Main sample: items with at least one litigated and one ordinary purchase. DV: log price. Item + Year + PBU FE. SE clustered at PBU level.

The placebo coefficient is -0.020 (SE = 0.032) for negotiated prices and -0.031 (SE = 0.045) for reference prices—both economically small and statistically insignificant. The urgency premium exists only for items that are targets of litigation, ruling out the possibility that the main estimates reflect a generic correlation between urgency and prices.

5.5. Demand vs. Supply Side

Adding supplier fixed effects (2,202 unique winners; Online Appendix, Table A.7) attenuates the urgency coefficient from 0.051 to 0.025—a 51% reduction. Since 92% of winning firms serve both ordinary and urgent tenders, this reflects within-firm pricing: the same firms charge more when the purchase is court-mandated. The remaining half reflects officials selecting into worse matches under time pressure. With both firm FE and quantity controls, the residual coefficient falls to 0.011 (insignificant)—the premium operates through supplier exploitation and demand fragmentation, each with distinct policy implications (Section 6).

5.6. Heterogeneity: Market Competition

Table 4 splits the sample by competition intensity. The urgency premium is three times larger in competitive markets (0.143 vs. 0.046 log points), with a significant interaction (0.226, $p < 0.01$). This pattern is consistent with the mechanism: in concentrated markets, fewer bidders leave less room for urgency to erode bargaining power; in competitive markets, the loss of planning time and bidder participation has the largest marginal impact on prices.

Table 4: Heterogeneity: Market Competition

	Split Sample		Interaction
	Low Comp. (1)	High Comp. (2)	Full Sample (3)
Urgent Purchase	0.046*** (0.016)	0.143*** (0.034)	0.026 (0.018)
Urgent \times High Competition			0.226*** (0.044)
FE: Item + Year + PBU	Yes	Yes	Yes
Observations	125,950	61,313	187,264
Within R ²	0.000	0.001	0.001

Notes: Preferred specification: Item + Year + PBU FE; PBU-clustered SEs in parentheses. Columns (1)–(2) are split-sample estimates. Column (3) reports the interaction. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6. Conclusion

Courts that compel governments to buy medications save individual lives. They also cost the public budget 5.4% more per purchase, attract 5.4% fewer bidders, and—when officials face personal sanctions for failure—generate a 23–30% total cost premium driven entirely by demand fragmentation.

The “under the gun” comparison is the paper’s sharpest result. Litigated and administrative urgent purchases face identical planning constraints; they differ only in whether the official faces fines and fund seizure if the tender fails. That this penalty exposure alone accounts for 23–30% in excess costs—operating through smaller orders that forgo bulk discounts, not through officials accepting worse unit prices—reveals a specific and addressable channel. Officials under judicial pressure prioritize compliance over cost-efficiency, and the resulting demand fragmentation is costly ([Rasul and Rogger, 2018](#); [Duflo](#)

[et al., 2012](#)).

Where does the money go? Supplier fixed effects split the premium roughly in half: officials accept worse matches under pressure (demand side), and firms charge more when they know the government has no choice (supply side). This points to two complementary reforms. On the demand side, expanding São Paulo’s administrative request mechanism—which since 2009 has allowed procurement without judicial sanctions, lowering costs by 23–30% relative to litigated purchases—to other states would eliminate the “under the gun” premium where institutional capacity permits. On the supply side, removing the court-order reference from tender notices (which currently serves as a public signal of the government’s desperation) or establishing framework agreements for commonly litigated items would curtail supplier exploitation of judicial urgency.

The estimated price premiums, applied to the approximately US\$300 million spent annually on litigated health items in São Paulo alone, imply approximately US\$16–18 million in annual excess costs. These findings highlight a tension at the heart of right-to-health enforcement: judicial mandates intended to guarantee individual access impose substantial costs on the public budget, potentially reducing resources available for the broader health system. Institutional designs that balance individual rights with procurement efficiency—rather than treating them as competing objectives—could improve welfare for both litigants and the broader population. Brazil’s recent procurement reform (Lei 14.133/2021), which expands electronic auc-

tions and introduces framework agreements, could mitigate the quantity-fragmentation channel we document—a prediction future work can test as the reform takes effect.

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Online Appendix

This appendix provides tables and figures from the main text, robustness checks, and supplementary evidence.

A.0 Tables and Figures from Main Text

Table A.1: Descriptive Statistics by Purchase Type

	Ordinary		Administrative		Litigated		Diff	Diff
	Mean	(SD)	Mean	(SD)	Mean	(SD)	(O-L)	(A-L)
<i>Panel A: Levels</i>								
Reference Price	909.17	(5,959.41)	226.38	(2,257.35)	370.39	(2,540.20)	538.78*** [0.000]	-144.01*** [0.000]
Negotiated Price	477.51	(2,915.88)	148.03	(1,177.52)	260.25	(1,561.81)	217.26*** [0.000]	-112.22*** [0.000]
Quantity	31,487.15	(163,507.29)	117,146.00	(356,962.37)	9,646.19	(82,683.10)	21,840.96*** [0.000]	107,499.81*** [0.000]
N. Bidding Firms	3.17	(1.93)	2.43	(1.35)	2.20	(1.19)	0.97*** [0.000]	0.23*** [0.000]
<i>Panel B: Log Transformations</i>								
Log Reference Price	0.926	(2.723)	0.909	(2.541)	1.557	(2.446)	-0.632*** [0.000]	-0.648*** [0.000]
Log Negotiated Price	0.319	(2.862)	0.480	(2.604)	1.024	(2.568)	-0.705*** [0.000]	-0.544*** [0.000]
Log Quantity	6.947	(2.678)	7.472	(3.160)	6.087	(2.083)	0.860*** [0.000]	1.385*** [0.000]
Log N. Firms	0.984	(0.590)	0.747	(0.529)	0.665	(0.493)	0.319*** [0.000]	0.081*** [0.000]
<i>Panel C: Tender Characteristics</i>								
Successful Tender (%)	0.859	(0.348)	0.869	(0.338)	0.908	(0.288)	-0.050*** [0.000]	-0.040*** [0.000]
Observations	136,250		15,371		45,351			

Notes: Sample restricted to items with at least one ordinary and one litigated purchase. Winners only for price and quantity variables. Variables winsorized at 1%/99%. Stars on differences from Welch *t*-tests; *p*-values in brackets. *** *p*<0.01, ** *p*<0.05, * *p*<0.1.

Table A.2: Balance Table: Administrative vs Litigated (Urgent Purchases)

	Administrative		Litigated		Diff (A-L)	p-value
	Mean	(SD)	Mean	(SD)		
<i>Panel A: Procurement Outcomes</i>						
Reference Price	226.38	(2,257.35)	232.56	(1,432.18)	-6.18	[0.752]
Negotiated Price	148.03	(1,177.52)	174.79	(1,052.56)	-26.75**	[0.013]
Quantity	117,146.00	(356,962.37)	9,306.87	(80,252.25)	107,839.13***	[0.000]
Log Reference Price	0.91	(2.54)	1.43	(2.32)	-0.52***	[0.000]
Log Negotiated Price	0.48	(2.60)	0.89	(2.44)	-0.41***	[0.000]
Log Quantity	7.47	(3.16)	6.18	(1.99)	1.29***	[0.000]
<i>Panel B: Market Structure</i>						
N. Bidding Firms	2.427	(1.351)	2.155	(1.120)	0.272***	[0.000]
Log N. Firms	0.747	(0.529)	0.650	(0.483)	0.097***	[0.000]
Successful Tender (%)	0.869	(0.338)	0.914	(0.280)	-0.045***	[0.000]
<i>Panel C: Purchase Characteristics</i>						
Electronic Auction	0.985	(0.123)	0.941	(0.236)	0.044***	[0.000]
Observations	15,371		41,491			

Notes: Sample restricted to urgent purchases (administrative and litigated) for items with both types present. Winners only for price/quantity. Variables winsorized at 1%/99%. *p*-values from Welch *t*-tests in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

	bid_price_log		
	(1)	(2)	(3)
Urgent Purchase	0.0513***	0.0245	0.0109
	(0.0153)	(0.0159)	(0.0179)
Log Quantity			-0.3931***
			(0.0325)
Observations	196,883	196,642	196,642
R ²	0.86796	0.88083	0.92157
Within R ²	0.00022	5.41×10^{-5}	0.34189
	27		
item_id fixed effects	✓	✓	✓
year_n fixed effects	✓	✓	✓
pbu_id fixed effects	✓	✓	✓

Table A.3: Reference Prices

	(1)	(2)	(3)	(4)
Urgent Purchase	0.164*** (0.056)	0.156*** (0.050)	0.027* (0.014)	0.027* (0.014)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	196,899	196,899	196,896	196,896
Within R ²	0.003	0.003	0.000	0.000

Notes: Dependent variable: log reference price. Sample: winners only, items with both ordinary and litigated purchases. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

DV: log negotiated price. Sample: winning bids for items with both ordinary and litigated purchases. Column (1): item + year + PBU FE. Column (2) adds firm FE. Column (3) adds log quantity. SE clustered at PBU level.

Section A.1 examines the sensitivity of the main estimates to the choice of winsorization level. Section A.2 assesses the robustness of the “under the gun” estimates to progressive inclusion of control variables. Section A.3 presents distributional evidence on key outcome variables and additional descriptive figures. Section A.4 presents geographic descriptive evidence on administrative demand, complementing the litigation maps shown in Section 2. Section A.5 presents an event study around the first court order for each item. Section A.6 examines heterogeneity in the urgency premium along several dimensions.

Table A.4: Quantities

	(1)	(2)	(3)	(4)
Urgent Purchase	-0.264 (0.183)	-0.279 (0.181)	-0.058 (0.056)	-0.065 (0.058)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	196,899	196,899	196,896	196,896
Within R ²	0.003	0.003	0.000	0.000

Notes: Dependent variable: log quantity. Sample: winners only. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

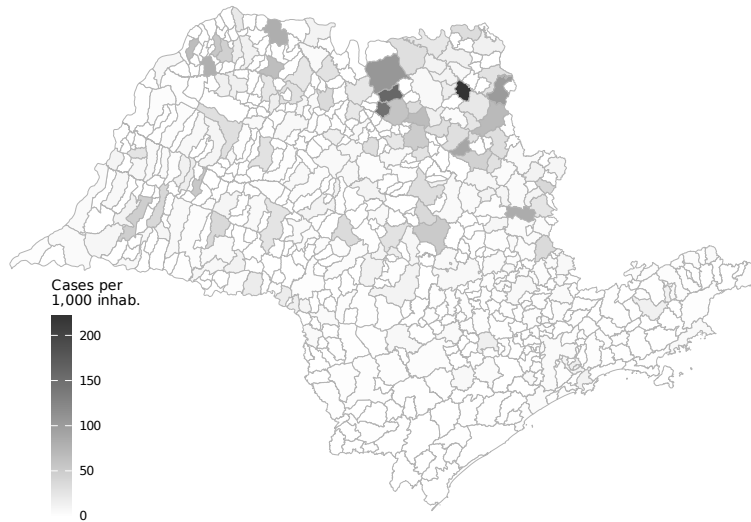


Figure A.1: Health Litigation Cases per 1,000 Inhabitants Across Municipalities in São Paulo (2009–2019)

Source: Authors' elaboration based on CNJ/INSPER (2019) litigation data and IBGE population estimates.

Table A.5: Participant Firms

	(1)	(2)	(3)	(4)
<i>Panel A: Total Effect</i>				
Urgent Purchase	-0.101*** (0.023)	-0.113*** (0.022)	-0.056*** (0.014)	-0.054*** (0.015)
<i>Panel B: Direct Effect</i>				
Urgent Purchase	-0.091*** (0.014)	-0.102*** (0.011)	-0.042*** (0.012)	-0.040*** (0.013)
Log Quantity	0.063*** (0.004)	0.064*** (0.004)	0.065*** (0.003)	0.065*** (0.003)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Obs. (Panel A)	81,805	81,805	81,801	81,801
Obs. (Panel B)	81,793	81,793	81,789	81,789
Within R ² (A)	0.004	0.006	0.001	0.001
Within R ² (B)	0.063	0.069	0.043	0.044

Notes: Dependent variable: log number of bidding firms. Sample: winners only. Panel A reports the total effect; Panel B controls for log quantity. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.6: Tender Success (LPM)

	(1)	(2)	(3)	(4)
<i>Panel A: Total Effect</i>				
Urgent Purchase	0.023** (0.009)	0.023** (0.009)	0.021*** (0.006)	0.021*** (0.006)
<i>Panel B: Direct Effect</i>				
Urgent Purchase	0.023** (0.009)	0.023** (0.010)	0.021*** (0.006)	0.022*** (0.006)
Log Quantity	0.003 (0.004)	0.003 (0.004)	0.012*** (0.002)	0.012*** (0.002)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Obs. (Panel A)	226,305	226,305	226,301	226,301
Obs. (Panel B)	226,282	226,282	226,278	226,278
Within R ² (A)	0.001	0.001	0.000	0.000
Within R ² (B)	0.001	0.001	0.004	0.004

Notes: Dependent variable: successful tender (LPM). Sample: all observations. Panel A reports the total effect; Panel B controls for log quantity. Winsorized at 1%/99%. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.



Figure A.2: Public Buyer Units (SES/SP)

Source: Authors' elaboration based on SES/SP administrative records.

A.1 Winsorization Sensitivity

Our baseline estimates use 1%/99% winsorization to limit the influence of extreme outliers while preserving meaningful variation in the data. To verify that our results are not driven by this specific choice, Tables [A.7–A.10](#) replicate the main regression tables under three alternative winsorization treatments: no winsorization (Panel A), the 1%/99% baseline (Panel B), and a more aggressive 5%/95% winsorization (Panel C).

Table [A.7](#) reports the sensitivity of the reference price estimates. The positive coefficient on urgent purchases is stable across all three winsorization levels: the preferred specification (column 3) yields estimates of 0.029 (no winsorization), 0.027 (baseline), and a similar magnitude with aggressive winsorization. The qualitative pattern—a large raw premium that attenuates substantially with PBU fixed effects—is preserved throughout, indicating that the reference price result is not driven by outliers.

Table [A.8](#) presents the corresponding sensitivity analysis for negotiated prices. The total effect of urgency on negotiated prices is positive and statistically significant across all winsorization levels. The coefficient magnitudes are slightly larger without winsorization (reflecting the influence of extreme values) and slightly smaller with aggressive winsorization, but the economic message remains unchanged: urgent purchases are associated with higher negotiated prices, with the preferred estimate ranging from approximately 3% to 7%.

Table [A.9](#) examines the robustness of the bidder participation results.

Table A.7: Robustness: Reference Prices

	(1)	(2)	(3)	(4)
<i>Panel A: No Winsorization</i>				
Urgent Purchase	0.167*** (0.056)	0.159*** (0.051)	0.026* (0.014)	0.026* (0.014)
<i>Panel B: Winsorized 1%/99%</i>				
Urgent Purchase	0.164*** (0.056)	0.156*** (0.050)	0.027* (0.014)	0.027* (0.014)
<i>Panel C: Winsorized 5%/95%</i>				
Urgent Purchase	0.156*** (0.053)	0.150*** (0.048)	0.027* (0.014)	0.028** (0.014)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	196,899	196,899	196,896	196,896

Notes: Dependent variable: log reference price. Sample: winners only (success uses all obs). Each panel uses a different winsorization level. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The negative effect of urgency on the number of bidding firms is remarkably stable across winsorization levels, with preferred estimates in the range of -0.05 to -0.06 log points. This stability is expected, since the number of firms is a count variable with limited extreme values, making it less sensitive to winsorization.

Table A.10 replicates the tender success analysis. The positive coefficient on urgency—indicating that urgent tenders are more likely to succeed—is robust across all winsorization levels. The magnitude is consistent at approximately 2 percentage points in the preferred specification, reinforcing

Table A.8: Robustness: Negotiated Prices

	(1)	(2)	(3)	(4)
<i>Panel A: No Winsorization</i>				
Urgent Purchase	0.160*** (0.043)	0.152*** (0.038)	0.051*** (0.015)	0.054*** (0.016)
<i>Panel B: Winsorized 1%/99%</i>				
Urgent Purchase	0.159*** (0.042)	0.151*** (0.038)	0.053*** (0.016)	0.056*** (0.016)
<i>Panel C: Winsorized 5%/95%</i>				
Urgent Purchase	0.162*** (0.041)	0.153*** (0.037)	0.058*** (0.017)	0.061*** (0.017)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	196,886	196,886	196,883	196,883

Notes: Dependent variable: log negotiated price. Sample: winners only (success uses all obs). Each panel uses a different winsorization level. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the interpretation that procurement officials under pressure accept less favorable terms to ensure compliance.

Table A.9: Robustness: Participant Firms

	(1)	(2)	(3)	(4)
<i>Panel A: No Winsorization</i>				
Urgent Purchase	-0.101*** (0.023)	-0.113*** (0.022)	-0.056*** (0.015)	-0.054*** (0.015)
<i>Panel B: Winsorized 1%/99%</i>				
Urgent Purchase	-0.101*** (0.023)	-0.113*** (0.022)	-0.056*** (0.014)	-0.054*** (0.015)
<i>Panel C: Winsorized 5%/95%</i>				
Urgent Purchase	-0.100*** (0.022)	-0.111*** (0.021)	-0.055*** (0.014)	-0.053*** (0.015)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Obs. (A)	81,377	81,377	81,373	81,373
Obs. (B)	81,805	81,805	81,801	81,801
Obs. (C)	81,805	81,805	81,801	81,801

Notes: Dependent variable: log number of bidding firms. Sample: winners only (success uses all obs). Each panel uses a different winsorization level. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.10: Robustness: Tender Success (LPM)

	(1)	(2)	(3)	(4)
<i>Panel A: No Winsorization</i>				
Urgent Purchase	0.023** (0.009)	0.023** (0.009)	0.021*** (0.006)	0.021*** (0.006)
<i>Panel B: Winsorized 1%/99%</i>				
Urgent Purchase	0.023** (0.009)	0.023** (0.009)	0.021*** (0.006)	0.021*** (0.006)
<i>Panel C: Winsorized 5%/95%</i>				
Urgent Purchase	0.023** (0.009)	0.023** (0.009)	0.021*** (0.006)	0.021*** (0.006)
Item FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No
Year-Month FE	No	No	No	Yes
PBU FE	No	No	Yes	Yes
Observations	226,305	226,305	226,301	226,301

Notes: Dependent variable: successful tender (LPM). Sample: winners only (success uses all obs). Each panel uses a different winsorization level. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.2 Under the Gun: Progressive Controls

Table [A.11](#) examines the sensitivity of the “under the gun” estimate to the progressive inclusion of control variables and to the choice of winsorization level. Starting from the baseline specification with item + year + PBU fixed effects (column 1), we sequentially add log quantity (column 2), log reference price (column 3), log number of firms (column 4), and finally replace year FE with year-month FE (column 5). Each row within a panel shows how the coefficient on the administrative indicator responds to additional controls.

The key finding is that the administrative indicator remains negative across all specifications and winsorization levels, indicating that litigated purchases are consistently more expensive than administrative ones. The coefficient is largest and most precisely estimated in the baseline specification without additional controls (column 1), where it captures the total effect including indirect channels through quantity and competition. As controls are added, the coefficient attenuates—consistent with some of the price premium operating through the quantity and competition channels—but the sign is preserved throughout. This pattern confirms that the sanction channel has an economically meaningful direct effect on prices beyond its indirect effects through procurement conditions.

Table A.11: Robustness: Under the Gun with Progressive Controls

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: No Winsorization</i>					
Administrative (vs Litigated)	-0.259** (0.095)	0.138 (0.130)	0.115** (0.054)	0.201** (0.090)	0.195** (0.087)
<i>Panel B: Winsorized 1%/99%</i>					
Administrative (vs Litigated)	-0.262** (0.096)	0.117 (0.122)	0.110* (0.055)	0.199** (0.093)	0.194** (0.090)
<i>Panel C: Winsorized 5%/95%</i>					
Administrative (vs Litigated)	-0.261** (0.096)	0.060 (0.093)	0.089* (0.049)	0.169* (0.084)	0.165* (0.080)
Log Quantity	No	Yes	Yes	Yes	Yes
Log Ref. Price	No	No	Yes	Yes	Yes
Log N. Firms	No	No	No	Yes	Yes
Item FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
Year-Month FE	No	No	No	No	Yes
PBU FE	Yes	Yes	Yes	Yes	Yes
Obs. (A)	56,803	56,803	56,803	14,913	14,913
Obs. (B)	56,803	56,803	56,803	15,052	15,052
Obs. (C)	56,803	56,803	56,803	15,052	15,052

Notes: Dependent variable: log negotiated price. UTG sample (urgent, winners, items with both administrative and litigated). Progressive controls added left to right. Column (5) replaces Year with Year-Month FE. Standard errors clustered at the PBU level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

A.3 Distributional Evidence

The regression estimates in the main text capture average treatment effects. The figures below complement these estimates by showing how the *distributions* of key outcome variables differ across purchase types, providing visual evidence of the systematic shifts that underlie our regression results.

Figure A.3 plots kernel density estimates of log reference prices separately

for ordinary, administrative, and litigated purchases. The rightward shift of the litigated distribution relative to ordinary purchases is clearly visible, consistent with the positive coefficient on urgency in Table A.3. The administrative distribution lies between the other two, reflecting the intermediate planning conditions of this purchase type.

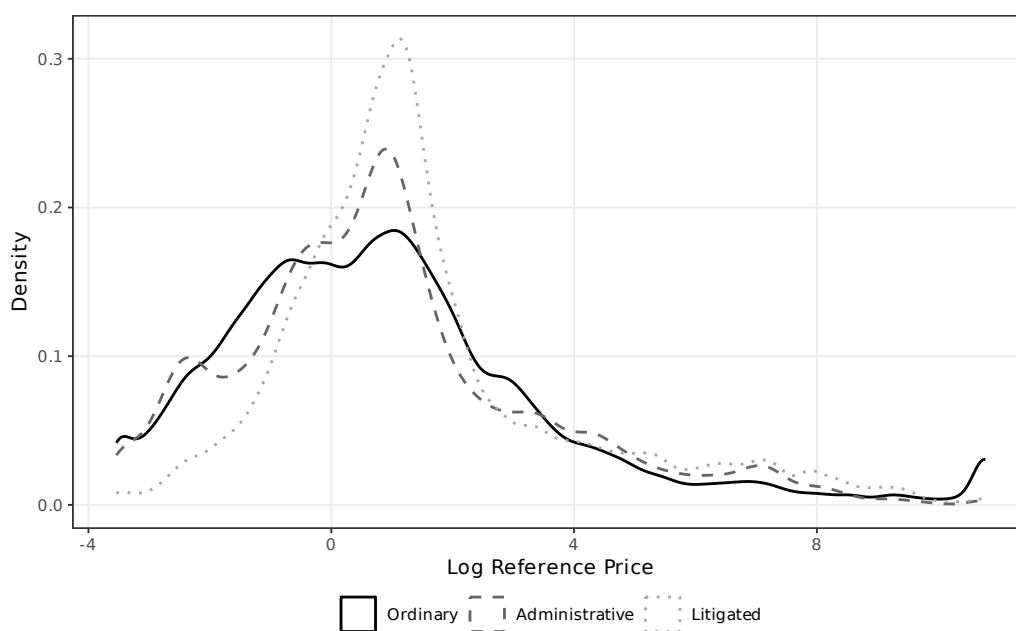


Figure A.3: Distribution of Log Reference Price by Purchase Type

Figure A.4 presents the analogous comparison for log negotiated prices. The distributional shift is qualitatively similar to reference prices, with litigated purchases showing higher negotiated prices on average. Notably, the litigated distribution also exhibits a somewhat thicker right tail, suggesting that a subset of litigated purchases involves particularly large price premiums.

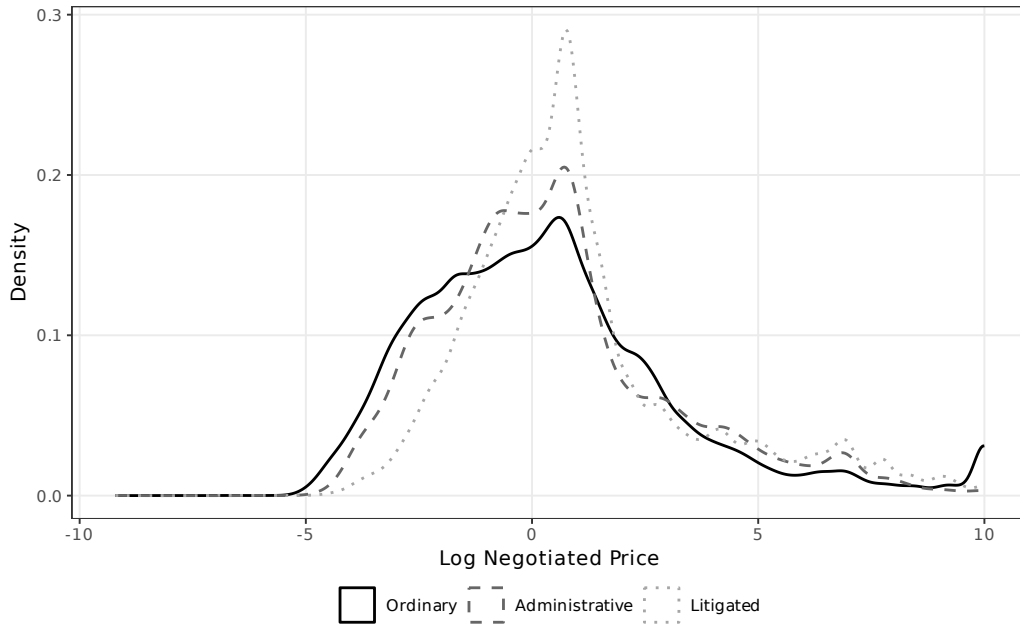


Figure A.4: Distribution of Log Negotiated Price by Purchase Type

Figure A.5 shows the distribution of log quantities. In contrast to prices, here the litigated distribution is shifted *leftward*, reflecting the smaller quantities demanded in urgent purchases. The ordinary distribution has a heavier right tail, consistent with the ability to aggregate demand under standard planning timelines. The administrative distribution is bimodal, spanning both small (individual prescription) and larger (stock replenishment) quantities.

Figure A.6 displays the distribution of log number of bidding firms. The litigated distribution is clearly shifted leftward relative to ordinary purchases, with a larger mass at low participation levels. This visual pattern confirms the regression finding that urgent tenders attract fewer bidding firms, reduc-

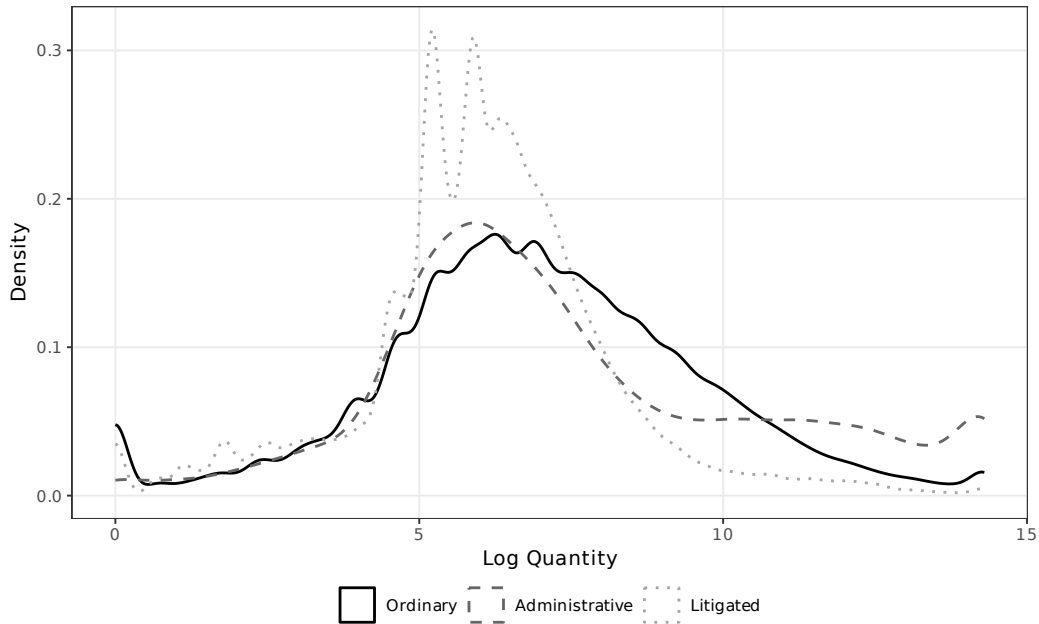


Figure A.5: Distribution of Log Quantity by Purchase Type

ing the competitive pressure that normally restrains prices.

Figure A.7 focuses on the “under the gun” subsample, comparing the distributions of log negotiated prices for administrative and litigated urgent purchases only. The rightward shift of the litigated distribution relative to administrative purchases provides direct visual evidence of the sanction channel: holding constant the urgency of the purchase, judicial pressure shifts the entire price distribution to the right.

Figure A.8 compares tender success rates across purchase types. Consistent with the LPM estimates, litigated purchases have the highest success rate, followed by administrative and ordinary purchases. This ordering is consistent with the “under the gun” mechanism: the greater the penalty for

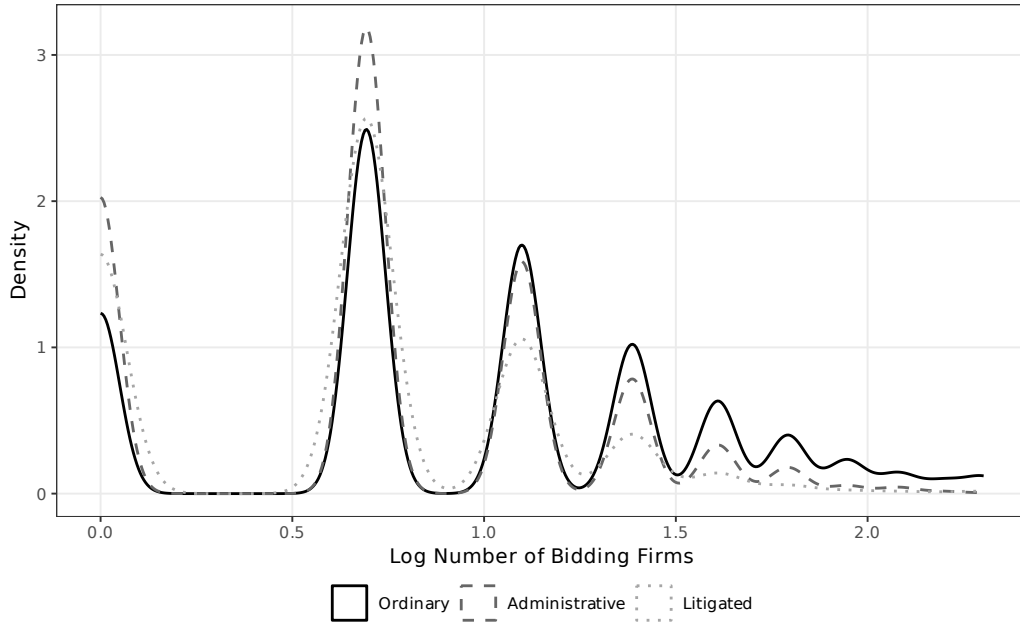


Figure A.6: Distribution of Log Number of Bidding Firms by Purchase Type

procurement failure, the higher the observed success rate, as officials accept less favorable terms to avoid sanctions.

Figure A.9 plots mean log negotiated prices over time for ordinary and urgent purchases separately. The figure reveals two patterns. First, the price gap between urgent and ordinary purchases is persistent throughout the sample period, ruling out the possibility that the regression estimates are driven by a specific subperiod. Second, both series exhibit common trends, supporting the identification assumption that time-varying confounders do not differentially affect the two purchase types.

Figure A.10 summarizes the main coefficient estimates from the preferred specification (item + year + PBU FE) with 95% confidence intervals. The

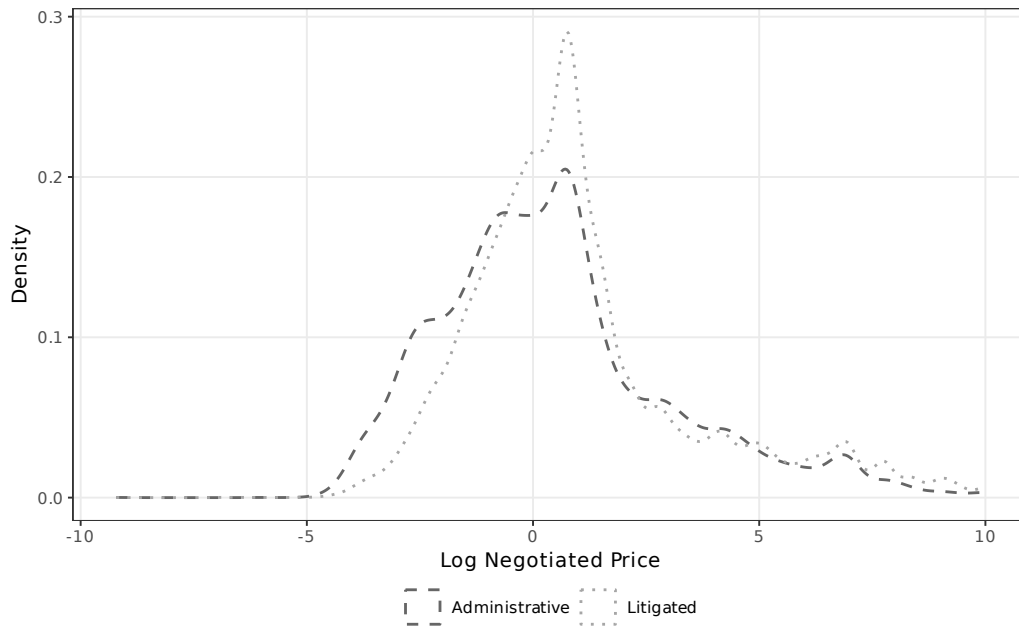


Figure A.7: Distribution of Log Negotiated Price: Administrative vs. Litigated (Urgent Only)

figure provides a compact visual overview of the paper’s key results: positive effects on reference prices and negotiated prices, a negative effect on bidder participation, a positive effect on tender success, and a large negative coefficient on the administrative indicator in the “under the gun” analysis (indicating higher prices for litigated relative to administrative purchases).

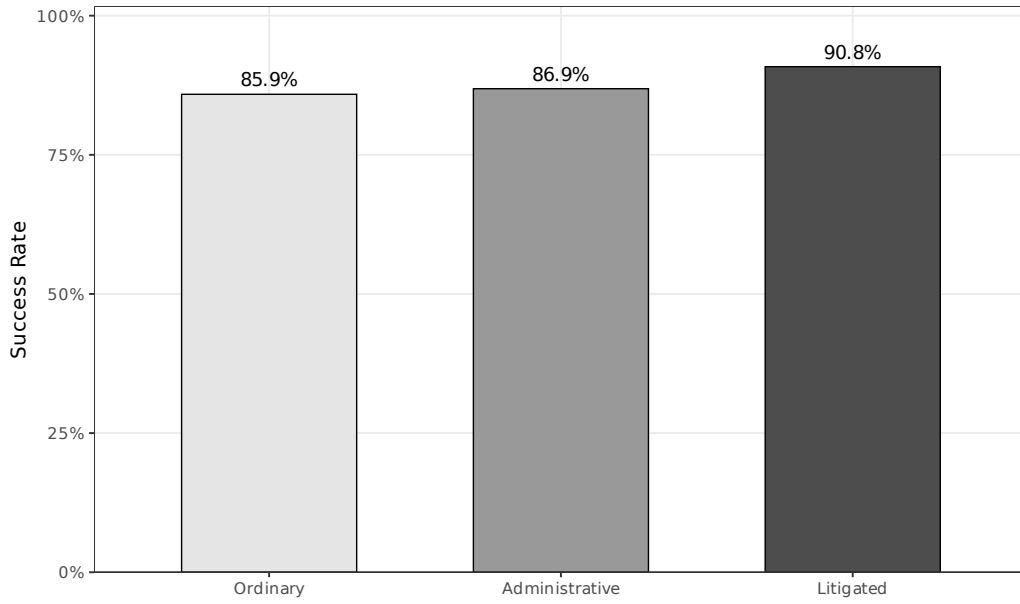


Figure A.8: Tender Success Rate by Purchase Type

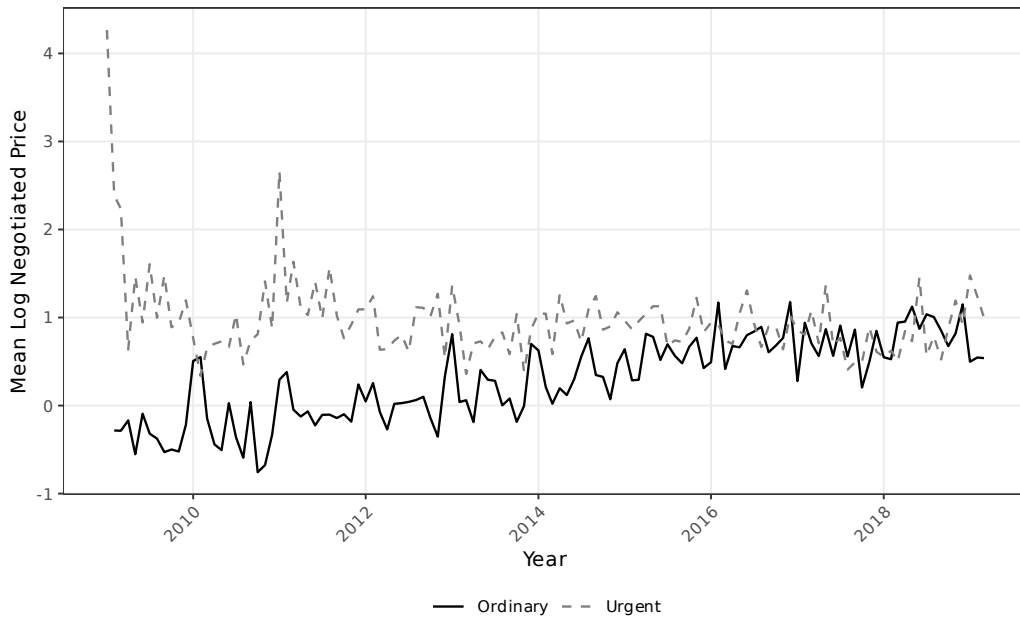


Figure A.9: Mean Log Negotiated Price Over Time: Ordinary vs. Urgent

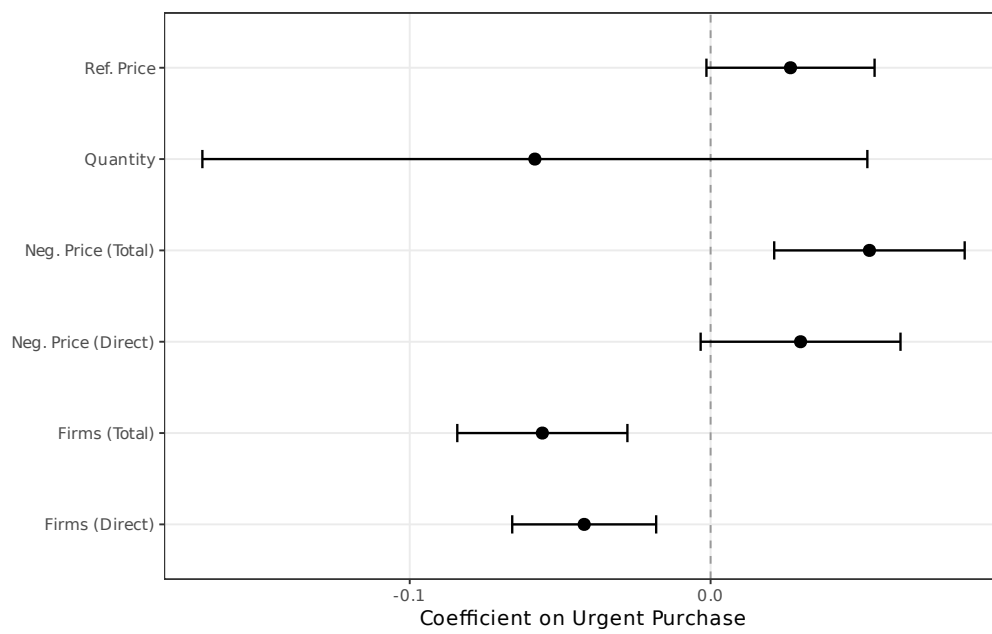


Figure A.10: Coefficient Estimates with 95% Confidence Intervals (Preferred Specification)

A.4 Administrative Demand: Descriptive Evidence

This section presents geographic descriptive evidence on the distribution of administrative demand across municipalities in São Paulo, complementing the litigation maps in Section 2. Administrative purchases—urgent procurements initiated through internal government requests rather than court orders—share the same planning constraints as litigated purchases but carry no sanctions for procurement failure. The spatial patterns documented below confirm that administrative and litigated demand originate from similar geographic areas, supporting the institutional comparability that underlies the “under the gun” identification strategy.

Figure A.11 maps administrative purchases per 1,000 inhabitants across municipalities. The geographic distribution closely mirrors the litigation pattern shown in Figure A.1, with higher per capita rates in municipalities that host PBUs and in the interior of the state. This spatial overlap is consistent with both channels responding to the same underlying health needs.

Figure A.12 shows the location of PBUs that process administrative purchases, with marker size proportional to the volume of transactions. The distribution mirrors the PBU map for litigated purchases (Figure A.2), confirming that the same institutional infrastructure handles both procurement channels.

Figure A.13 provides a visual comparison of administrative and litigated purchases, highlighting that the procurement process is identical across both channels—the only distinguishing feature is the threat of judicial sanctions



Figure A.11: Administrative Purchases per 1,000 Inhabitants Across Municipalities in São Paulo (2009–2019)

Source: Authors’ elaboration based on BEC procurement data and IBGE population estimates.

in litigated cases. This institutional parallel is the foundation for the “under the gun” identification.

Figure A.14 maps the total number of administrative purchases by municipality. As with litigation, administrative demand is concentrated in a small number of urban centers, particularly the São Paulo metropolitan area and regional hubs in the state’s interior.

Figure A.15 shows the share of administrative purchases as a proportion of total purchases (administrative plus litigated) by municipality. The substantial variation across municipalities suggests that the relative importance of administrative versus litigated demand reflects local institutional factors—

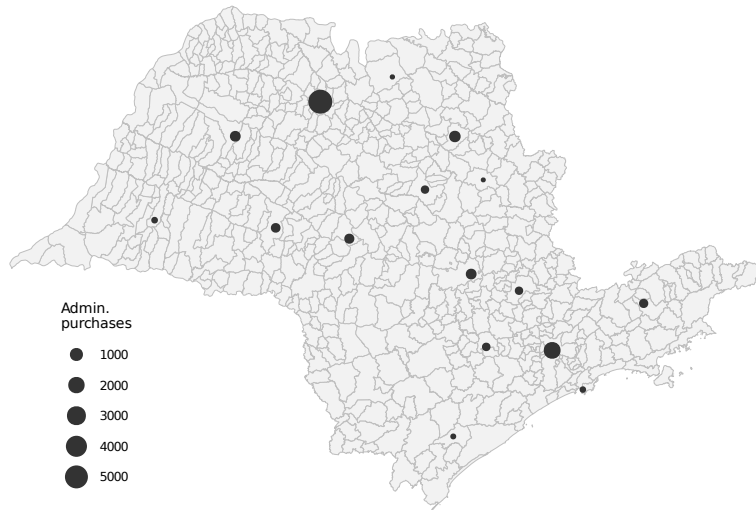


Figure A.12: PBUs Handling Administrative Purchases (Size \propto Volume)

Source: Authors' elaboration based on SES/SP administrative records.

such as the presence of administrative request mechanisms and the propensity of local courts to grant injunctions—rather than a uniform statewide pattern.

	Administrative	Litigated
Origin	SES/SP request	Court order
Source of funds	Diverted budget	Diverted budget
Quantity	Small	Small
Delivery time	Short	Short
Threat of punishment	None	Potential punishment

Figure A.13: Comparison of Administrative and Litigated Purchases
Source: Authors' elaboration.

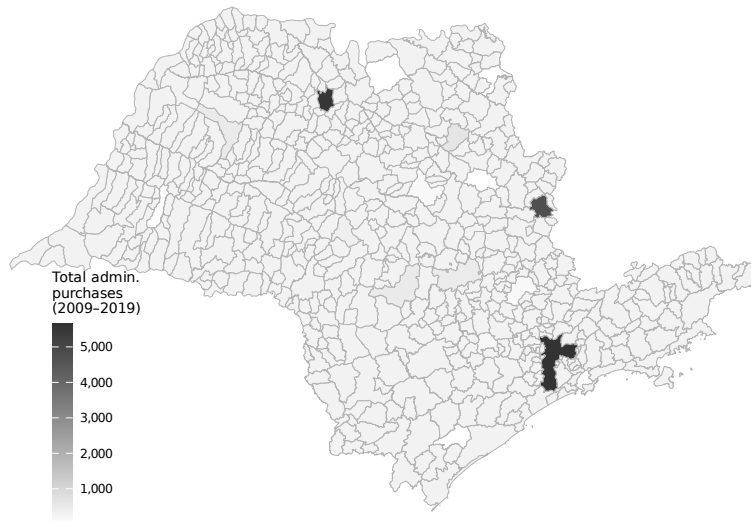


Figure A.14: Total Administrative Purchases by Municipality in São Paulo (2009–2019)
Source: Authors' elaboration based on BEC procurement data.



Figure A.15: Administrative Purchases as Share of Total Urgent Purchases by Municipality
Source: Authors' elaboration based on BEC procurement data.

A.5 Event Study: First Court Order

Figure A.16 plots price dynamics around the first court order for each item (769 items with ≥ 2 pre- and post-treatment years). Prices *decline* monotonically in the years before litigation ($t = -5$: -0.43 ; $t = -2$: -0.03) and rise sharply after ($t = +1$: $+0.12$; $t = +5$: $+0.50$). The negative pre-trends rule out the concern that rising prices attract litigation; if anything, declining prices may generate the unmet demand that triggers court orders.

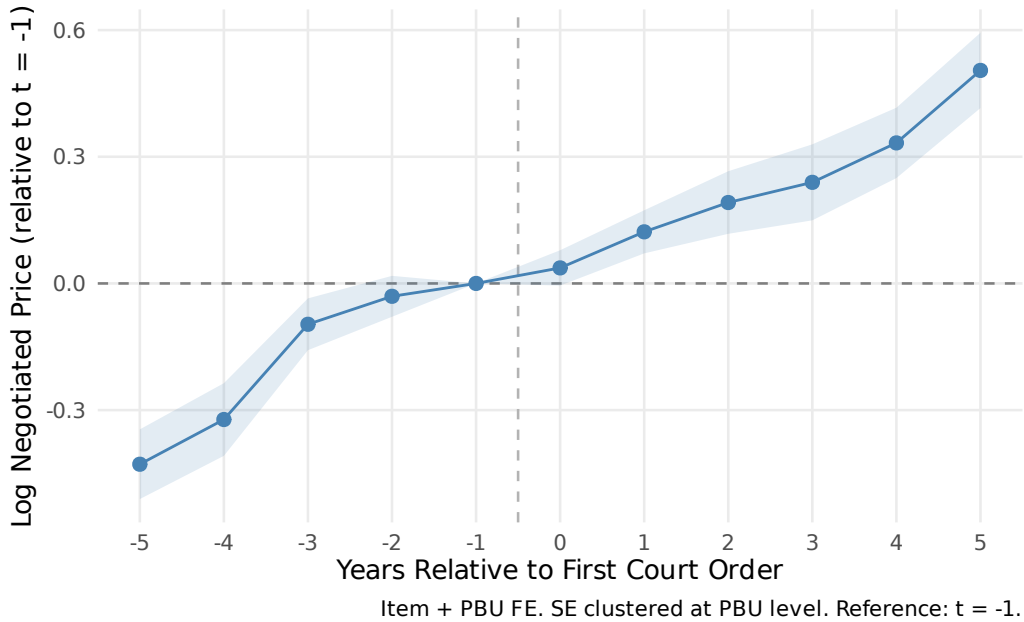


Figure A.16: Log Negotiated Price Relative to First Court Order ($t = -1$ Reference)
Notes: Item + PBU FE. SE clustered at PBU level. 769 items with ≥ 2 pre- and ≥ 2 post-treatment years. 95% CIs shaded.

A.6 Heterogeneity

We examine heterogeneity in the urgency premium along four dimensions. Tables A.12–A.14 report results from split-sample regressions and interaction

models using our preferred specification (item + year + PBU FE).

Table A.12: Heterogeneity: SUS Component

	Split Sample		Interaction
	Specialized (1)	Basic SUS (2)	Full Sample (3)
Urgent Purchase	0.005 (0.046)	0.030* (0.016)	-0.073 (0.097)
Urgent \times Basic SUS			0.159 (0.115)
FE: Item + Year + PBU	Yes	Yes	Yes
Observations	45,048	151,835	196,883
Within R ²	0.000	0.000	0.001

Notes: Preferred specification: Item + Year + PBU FE; PBU-clustered SEs in parentheses. Columns (1)–(2) are split-sample estimates. Column (3) reports the interaction. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

SUS component (Table A.12)... The urgency premium on negotiated prices is concentrated among items in the basic SUS component (common medications): the coefficient is 0.030 ($p < 0.10$) for basic items and an insignificant 0.005 for specialized items, suggesting that procurement disruption is more consequential for items where established supply chains normally deliver better prices.

Time period (Table A.13)... The urgency premium is larger in the later period (2014–2019) than in the earlier period (2009–2013), consistent with the growing volume and complexity of health litigation over time.

Table A.13: Heterogeneity: Time Period

	Split Sample		Interaction
	Early (1)	Late (2014+) (2)	Full Sample (3)
Urgent Purchase	0.074*** (0.025)	0.045** (0.018)	0.259*** (0.035)
Urgent \times Late Period			-0.339*** (0.043)
FE: Item + Year + PBU	Yes	Yes	Yes
Observations	82,658	113,936	196,883
Within R ²	0.000	0.000	0.005

Notes: Preferred specification: Item + Year + PBU FE; PBU-clustered SEs in parentheses. Columns (1)–(2) are split-sample estimates. Column (3) reports the interaction. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Market competition (Table 4). The urgency premium is three times larger in competitive markets (0.143 vs. 0.046), with a significant interaction (0.226, $p < 0.01$). In concentrated markets, fewer bidders leave less room for urgency to erode bargaining power; in competitive markets, the loss of planning time has the largest marginal impact.

PBU size (Table A.14). The urgency premium is slightly larger for purchases made by smaller PBUs (below-median transaction volume), consistent with smaller units having less experience managing urgent procurement.

Table A.14: Heterogeneity: PBU Size

	Split Sample		Interaction
	Small PBU (1)	Large PBU (2)	Full Sample (3)
Urgent Purchase	0.023 (0.026)	0.062*** (0.018)	-0.000 (0.049)
Urgent \times Large PBU			0.070 (0.054)
FE: Item + Year + PBU	Yes	Yes	Yes
Observations	31,902	164,511	196,883
Within R ²	0.000	0.000	0.000

Notes: Preferred specification: Item + Year + PBU FE; PBU-clustered SEs in parentheses. Columns (1)–(2) are split-sample estimates. Column (3) reports the interaction. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.