

When Courts Allocate: Judicial Mandates, Budget Composition, and Health

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Abstract

When courts mandate public spending, they override administrative priority-setting and may change the marginal returns to public resources. This logic applies whenever external authorities impose spending obligations on local budgets. In Brazil, court orders directing municipalities to provide medicines and treatments absorb a substantial share of local health budgets, creating a large-scale reallocation from programmatic to individualized spending. We exploit line-item municipal budget records that directly identify court-mandated expenditures through judicial-compliance descriptors in the transaction history field, revealing both the volume of judicial spending and the budget categories it displaces. Combining these data with the administrative universe of health case records, we use the random assignment of judges to health lawsuits as a source of exogenous variation in court-mandated spending at the municipality level. We estimate the causal effect on the composition of health budgets, population health outcomes, and the distributional incidence across socioeconomic groups.

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1 Introduction

Public spending can be allocated administratively—through budgets, formularies, and technology assessment—or by courts enforcing individual claims. The institution that allocates may change the marginal returns. In Brazil, courts now direct billions of reais in public health spending toward individual litigants: in 2024, the federal government spent R\$1.9 billion on court-mandated pharmaceutical assistance alone (Instituto de Pesquisa Econômica Aplicada, 2025), with total Ministry of Health judicial compliance costs reaching R\$2.7 billion (Folha de S.Paulo, 2025b)—an amount equivalent to the entire budget of Farmácia Popular, the country’s flagship pharmaceutical assistance program. In the same year, over 270 municipalities devoted between 30 and 100 percent of their health budgets to fulfilling judicial mandates, while primary care facilities in those same municipalities reported shortages of basic supplies and routine medications (Brito, 2024). The resources courts command are drawn from the same budget that finances population-level health programs. Whether this reallocation improves or worsens population health depends on the relative marginal returns to court-mandated and administratively allocated spending—a question that has remained unanswered for lack of credible identification and direct measurement of the mechanism.

This paper estimates the causal effect of court-mandated health spending on budget composition and population health outcomes. Brazil’s constitutional right to health generates hundreds of thousands of lawsuits per year compelling municipalities to provide medicines and treatments—often

outside the standard benefit package—within binding budget constraints. Because litigation correlates with health needs and state capacity, simple comparisons are uninformative. We exploit the random assignment of judges to health lawsuits: judges differ persistently in their propensity to grant requests, generating exogenous variation in court-mandated spending at the municipality level. A distinctive feature of our data is that line-item municipal budget records from the São Paulo State Court of Accounts (TCE-SP) directly identify judicial transactions through explicit judicial-compliance descriptors in the transaction history field—terms such as “determinação judicial,” “tutela antecipada,” and “cumprimento de sentença.” These descriptors reveal both the volume of judicial spending and the budget categories it displaces, providing unusually direct measurement of the mechanism through which courts reshape public expenditure.

We make three contributions. First, we document how court mandates reshape the composition of municipal health budgets. The text-tagged budget data let us test directly whether judicial spending crowds out primary care in favor of pharmaceutical spending—the budget categories where court-ordered purchases concentrate. Combined with evidence on procurement price premia from emergency judicial purchasing (Genicolo-Martins & Furquim de Azevedo, 2024), this reveals the full channel through which courts alter the marginal product of the health budget. Second, we provide the first causal estimates linking court-mandated health spending—measured in budget execution records rather than estimated from court files—to population health outcomes, including infant mortality, avoidable hospitalizations, and birth outcomes. Because the judge instrument operates through municipal bud-

gets, our estimates capture spillovers to non-litigating patients who share the same local health system. Third, we characterize the distributional incidence of judicially allocated resources—a key mechanism through which the mode of allocation shapes welfare. By linking litigants to employment and social program registries and testing whether judge-induced spending shocks affect health outcomes differently across the income distribution, we identify who bears the costs of crowd-out and who captures the benefits of judicial mandates.

Health agencies allocate through *ex ante* screening: formularies, health technology assessment, and competitive procurement set priorities before resources are committed. Courts allocate through *ex post* adjudication: individual claims trigger spending obligations after the fact. Court mandates function as rigid *ex post* earmarks (Buchanan, 1963)—they commit budget resources to specific patients and treatments, reducing discretionary capacity. Under binding budget constraints, each real of court-ordered spending displaces other expenditures. The net health effect depends on what is crowded out and on the price premium of judicial procurement, which bypasses competitive bidding and costs 30–44 percent more than equivalent drugs procured through standard channels (Genicolo-Martins & Furquim de Azevedo, 2024). Our design provides unusually direct evidence on these mechanisms: the budget data show which categories shrink when judicial spending rises, and the judge instrument isolates the causal chain from judicial allocation to health outcomes.

The analysis speaks to a general question in public finance: how the institution that allocates spending affects its returns. Court-mandated health

spending is growing rapidly in Colombia, Costa Rica, India, and South Africa. More broadly, the tension arises wherever external authorities impose spending obligations on local budgets—judicial mandates, intergovernmental earmarks, or regulatory requirements. Brazil offers three features that make the question tractable: large-scale litigation that generates substantial fiscal shocks, random assignment of judges that provides credible identification, and administrative budget records that reveal the mechanism.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 describes the institutional background. Section 4 presents the data. Section 5 outlines the empirical strategy. Section 6 reports the main results. Section 7 examines distributional incidence. Section 8 discusses mechanisms. Section 9 presents robustness checks. Section 10 discusses policy implications. Section 11 concludes.

2 Related Literature

We contribute to several literatures, organized around five themes: courts and public resource allocation, the returns to public health spending, the empirical literature on health judicialization, the distributional consequences of public spending, and examiner instrumental variable designs.

Courts and public resource allocation. A growing literature studies how courts shape public spending and its composition. The most developed evidence comes from US school finance litigation, where court-mandated increases in education spending improve student achievement and long-run

economic outcomes, particularly in low-income districts (Hanushek et al., 2023; Jackson et al., 2016; Lafortune et al., 2018). Court orders have also been studied in the contexts of desegregation (Anstreicher et al., 2022) and health care access through Medicaid expansion (Borgschulte & Vogler, 2020). A related literature on fiscal federalism studies how earmarked transfers and mandated spending interact with local budget constraints. Intergovernmental transfers can have large effects on spending composition and downstream outcomes (Litschig & Morrison, 2012), and grants tend to “stick where they hit” rather than being offset by local tax adjustments (Hines & Thaler, 1995). Court mandates are a distinctive form of earmark: they are imposed *ex post*, in response to individual claims, without the *ex ante* coordination that characterizes legislative appropriations. In the Brazilian health context, municipalities operate under binding minimum spending floors and chronic fiscal constraints (Funcia & Ocke-Reis, 2018; Piola et al., 2013), making court-mandated spending a particularly binding shock that must be financed within rigid budgets, potentially crowding out discretionary spending on primary care and prevention (Maia et al., 2024; Rasella et al., 2018). No existing study provides causal evidence on how this crowd-out mechanism affects health outcomes.

Returns to public health spending. A central question motivating our analysis is whether the marginal health dollar is more productive when allocated administratively—through primary care and preventive programs—or judicially—through court-ordered treatments. A large literature on Brazil’s Family Health Strategy (ESF) demonstrates that primary care expansion

substantially reduces infant mortality (Aquino et al., 2009; Macinko et al., 2006; Rocha & Soares, 2010), cardiovascular mortality and avoidable hospitalizations (Hone et al., 2017; Rasella et al., 2014), and racial inequalities in health outcomes (Hone et al., 2023; Hone et al., 2017). Cross-country evidence paints a more nuanced picture: aggregate health spending shows weak average returns (Filmer & Pritchett, 1999), but expansions of coverage and primary care are consistently associated with improved outcomes (Bokhari et al., 2007; Moreno-Serra & Smith, 2015). Causal evidence from the United States confirms that insurance expansions reduce mortality (Borgschulte & Vogler, 2020; Finkelstein et al., 2019). This literature identifies the returns to *programmatic* health spending—planned expansions of coverage, personnel, or facilities. Our contribution is to estimate the returns to *judicially induced* spending, which is non-programmatic, skewed toward high-cost individual treatments, and may crowd out the very programs shown to be most effective.

Health judicialization. A large descriptive literature documents the growth, composition, and fiscal costs of health litigation in Brazil (Biehl et al., 2016; Diniz et al., 2014; S. C. P. Oliveira et al., 2025; Pepe et al., 2010; Wang & Ferraz, 2013; Wang et al., 2014). A first-order concern is supply-side organization: concentrated networks of lawyers, physicians, and pharmaceutical firms generate a substantial share of cases, with a small number of attorneys filing the majority of actions for specific high-cost drugs (Chieffi & Barata, 2010; Machado et al., 2011; Soares & Deprá, 2012). Other strands examine the role of the Defensoría Pública in shaping access (Ferraz, 2009; Wang &

Ferraz, 2013) and institutional responses such as NAT-Jus technical advisory bodies (Correia et al., 2025). A parallel comparative literature documents health rights litigation in Colombia, Costa Rica, and cross-nationally (Andia & Lamprea, 2019; Arrieta-Gómez, 2018; Loaiza et al., 2018; Yamin, 2010; Yamin & Gloppen, 2011). This body of work provides rich institutional detail but remains almost entirely descriptive: to our knowledge, no existing study estimates the *causal* effect of court-mandated health spending on population health outcomes.

Distributional consequences. Our analysis of who benefits from judicial spending connects to the literature on the incidence of public spending in developing countries. Fiscal incidence studies document that health spending in Brazil is broadly progressive (Higgins et al., 2016; Lustig et al., 2013), but the incidence of *judicially allocated* spending may differ if access to courts is unequal. The comparative law-and-development literature highlights the tension between rights-based litigation as a tool for expanding access and the risk that courts primarily serve those with greater legal resources (Ferraz, 2009; Gauri & Brinks, 2008; Gloppen, 2006). Galanter (1974) shows that repeat players with lower per-case costs systematically outperform one-shotters, and Shavell (1982) demonstrates that private incentives to litigate diverge from social optimality when litigation costs are unevenly distributed—predicting that judicial allocation will skew toward better-resourced claimants unless institutional safeguards lower barriers. Empirical evidence is limited to descriptive comparisons of litigant profiles with population averages (Biehl et al., 2016; Chieffi & Barata, 2009; Y. M. C. Oliveira et al., 2020). Our design

allows us to go further: by examining how judge-induced spending shocks affect health outcomes across the income distribution, we provide causal evidence on whether judicial allocation narrows or widens existing inequalities.

Examiner leniency designs. Our identification strategy builds on examiner leniency designs, which use quasi-random assignment of decision-makers as instruments for their decisions (Bhuller et al., 2020; Chyn et al., 2025; Dobbie et al., 2018; Frandsen et al., 2023; Goldsmith-Pinkham et al., 2025; Kling, 2006; Maestas et al., 2013). Our application is distinguished by two features: the first stage operates on municipal budgets rather than individual-level treatment, allowing us to study population-level consequences including spillovers to non-litigating patients; and our budget data make the mechanism directly observable—we trace which expenditure categories expand and which are displaced when judicial spending rises.

In summary, the closest existing work either documents the descriptive features of health judicialization without causal identification, or applies examiner leniency designs in settings—criminal justice, disability insurance, patents—far removed from health system resource allocation. Our contribution bridges these literatures: exploiting the judge leniency design within a setting where budget execution data make the mechanism from judicial allocation to health outcomes directly observable.

3 Institutional Background

3.1 The Brazilian Public Health System

Brazil’s public health system, the Sistema Único de Saúde (SUS), was established by the 1988 Constitution and implemented through Federal Law 8,080/1990 (Paim et al., 2011). SUS provides universal, free-of-charge coverage to all residents regardless of income or employment status. Approximately 75 percent of the population—roughly 160 million people—relies exclusively on SUS for health care; the remaining quarter holds supplementary private insurance, concentrated among higher-income households, though these individuals retain the right to use SUS services (Castro et al., 2019).

SUS is financed through general tax revenues from all three levels of government. Constitutional Amendment 29/2000, later regulated by Complementary Law 141/2012, established minimum spending floors: states must allocate at least 12 percent of own-source revenues to health, and municipalities at least 15 percent. The federal floor was originally linked to nominal GDP growth but was effectively frozen in real terms by the spending ceiling amendment (EC 95/2016). As a result, the municipal share of total public health spending has risen steadily—from 29 percent in 2012 to 34 percent in 2022—while the federal share has declined from 45 to 38 percent. This fiscal decentralization matters for our analysis. Municipalities bear increasing financial responsibility for health, yet are also the most exposed to court-mandated spending. Plaintiffs can sue any level of government, and enforcement falls on the entity with the most accessible bank accounts (Sec-

tion 3.4).

Service delivery is organized in three tiers. Primary care is delivered through the Family Health Strategy (Estratégia Saúde da Família, ESF), a large community-based program covering over 60 percent of the population (Macinko et al., 2006). Secondary and tertiary care are provided through public hospitals, contracted private providers, and university hospitals organized into regional referral networks.

Pharmaceutical assistance is organized into three components: the Basic Component (CBAF), covering primary care medicines and funded primarily by municipalities; the Strategic Component (CESAF), covering epidemiologically important diseases and funded federally; and the Specialized Component (CEAF), covering high-cost treatments for chronic and rare diseases, with cost-sharing across all three levels of government. The National Essential Medicines List (RENAME) defines which drugs SUS is obligated to provide. Decisions to incorporate new technologies into SUS are made by CONITEC (Comissão Nacional de Incorporação de Tecnologias no SUS), created by Law 12,401/2011, which evaluates proposals based on clinical efficacy, safety, and cost-effectiveness.

Despite universal coverage in principle, SUS faces persistent gaps between its legal mandate and actual service delivery (Castro et al., 2019). Medication stockouts are widespread: a 2022 national survey found that over 80 percent of municipalities reported shortages of basic medicines in their public pharmacies (Confederação Nacional de Municípios, 2022). A federal audit found that none of 25 sampled medications met the statutory 180-day availability deadline after CONITEC incorporation (Controladoria-Geral da

União, 2021).¹ These gaps between entitlement and access are central to understanding why patients turn to courts.

3.2 The Constitutional Right to Health and the Role of Courts

Article 196 of the 1988 Constitution establishes health as “a right of all and a duty of the State.” Whether this provision is a programmatic directive—guiding the state to progressively realize health through policy—or an immediately enforceable individual entitlement has been the central legal question driving health judicialization in Brazil (Ferraz, 2009; Wang, 2015).

Beginning in the late 1990s, the Supreme Federal Tribunal (STF) decisively adopted the latter interpretation. In a landmark 1997 decision, the STF ordered a state to fund treatment abroad for a child with a rare disease, holding that the individual right to health takes precedence over fiscal constraints.² This reasoning—treating the constitutional provision as an individual, justiciable entitlement to specific treatments—was rapidly adopted by lower courts across the country.

The result was a rapid growth in individualized health litigation. Courts routinely granted preliminary injunctions (*liminares*) compelling immediate delivery of medicines, procedures, or hospital care, with plaintiff success rates consistently exceeding 90 percent (Biehl et al., 2012). Ferraz (2009) char-

¹Additional indicators of the delivery gap: an estimated 900,000 patients awaiting elective procedures nationally (Conselho Federal de Medicina, 2024); as of early 2025, nearly one-third of medicines incorporated since 2018 remained unavailable to patients, with an average delay of 648 days from incorporation to delivery (Folha de S.Paulo, 2025a).

²Pet 1246-SC. Justice Celso de Mello wrote that “the individual right to life takes precedence over the financial and secondary interest of the State.”

acterizes the resulting “Brazilian model” of health litigation as one marked by individualized claims, demand for curative treatments, and near-universal plaintiff success. By the late 2000s, the phenomenon had reached a scale that prompted institutional responses from the judiciary itself, the legislature, and the executive branch.

3.3 Types of Health Lawsuits

Medicines dominate health litigation, comprising approximately 80 percent of cases (Tribunal de Contas da União, 2017). Over half of medication lawsuits in some jurisdictions request drugs already on SUS formularies, suggesting that much litigation addresses administrative delivery failures rather than genuine coverage gaps (Biehl et al., 2009; Chieffi & Barata, 2009). The remainder request medicines not incorporated into the national formulary, including some without ANVISA registration. High-cost treatments for cancer, rare diseases, and chronic conditions account for a disproportionate share of judicial spending—more than half of federal judicial health expenditures between 2010 and 2015 were concentrated in just three medicines not on the essential list (Vieira, 2020).³ This on-formulary versus off-formulary distinction matters for interpretation: crowd-out of on-formulary drugs signals administrative failure that courts correct, while off-formulary orders impose genuinely new spending obligations.

Beyond medicines, lawsuits seek surgical procedures, hospital beds, and medical devices. Cases are overwhelmingly individual; collective actions ac-

³The composition of judicial spending has shifted sharply toward non-registered drugs: by 2015 these accounted for R\$545 million—half of the Ministry of Health’s judicial compliance spending—up from R\$2.4 million in 2010 (Cambricoli, 2016).

count for only 3 percent of first-instance health cases at TJSP. In TJSP, private attorneys represent the vast majority of plaintiffs, with the Defensoria Pública (Public Defender’s Office) handling only about 7 percent of first-instance health decisions—consistent with unequal access to litigation that we examine in Section 7.

The scale of health litigation has grown rapidly. Federal spending on judicial compliance for medicines rose thirteen-fold in seven years, from R\$70 million in 2008 to over R\$1 billion by 2015 (Tribunal de Contas da União, 2017), reaching R\$1.9 billion by 2024 (Instituto de Pesquisa Econômica Aplicada, 2025). At the state level, court-mandated medications represented approximately one-third of total pharmaceutical spending in 2023. Over 160,000 new first-instance public health cases were filed nationally in 2024 alone.

3.4 Allocation of Costs Across Levels of Government

Under the doctrine of solidary liability (*responsabilidade solidária*), all three levels of government—federal, state, and municipal—can be sued for health provision. This follows from the constitutional assignment of “common competence” over health (Art. 23, II). In practice, plaintiffs may sue any entity or combination of entities, without a required order of preference. The result is a vertical fiscal externality: courts impose spending obligations on municipal budgets that violate the principle of matching expenditure responsibilities to revenue capacity (Oates, 1999).

In 2019, the STF attempted to rationalize this arrangement (Tema 793, RE 855.178). The ruling maintained solidary liability but directed judges to

assign compliance to the responsible entity under SUS's own allocation rules and to require inter-governmental reimbursement. In practice, lower courts continue to apply solidary liability without consistently directing compliance to the appropriate entity. Throughout most of our sample period, no systematic federal reimbursement mechanism existed for municipalities condemned in state courts. Only in 2024 did the STF establish that the federal government must reimburse 65 percent of costs when states or municipalities are ordered to provide non-incorporated drugs costing between 7 and 210 minimum salaries (Temas 1,234 and 6). Our data capture the period of maximum municipal fiscal exposure, when court orders fell entirely on local budgets.

The result is that municipalities frequently bear costs that should fall on states or the federal government. A study of the municipality of São Paulo found that 55 percent of court-mandated pharmaceutical spending corresponded to medicines that were the responsibility of the state or federal government (Wang et al., 2014). Municipalities have the least fiscal capacity but face the most enforcement pressure: when a public entity fails to comply, judges can order the immediate seizure (*bloqueio*) of funds from government bank accounts. A single court order can disrupt planned expenditures in a small municipality's health account. This vertical mismatch generates a testable prediction: the fiscal and health consequences of court mandates should be larger in municipalities with lower own-source fiscal capacity, and in settings where mandated items fall outside municipal responsibility.

3.5 Key Jurisprudence Shaping Judicial Behavior

Between 2009 and 2019, a series of rulings by the STF and STJ introduced constraints on judicial health orders—technical advisory requirements, evidentiary standards for non-incorporated medicines, and a bar on drugs without regulatory registration (details in Appendix A).

Despite these formal constraints, there is little evidence that they substantially changed judicial behavior during our sample period. At TJSP—our main court—technical advisory notes (NAT-Jus) were cited in fewer than 0.01 percent of first-instance health decisions: 9 of 107,497 cases (CNJ/INSPER, 2019).⁴ The volume of health litigation continued to accelerate through the 2020s, and grant rates for medication requests against municipalities at TJSP remain approximately 93 percent. The institutional framework has shifted toward more structured criteria, but these have not eliminated the substantial judge-level variation in decision-making that we exploit. In 2024, the STF introduced binding administrative exhaustion requirements and a federal reimbursement mechanism; our analysis focuses on the pre-reform period when judicial discretion was largely unconstrained.⁵

⁴The national first-instance average was under 6 percent; geographic variation is substantial (e.g., Mato Grosso courts cited technical notes in 18 percent of cases). In São Paulo, written decisions almost never reference technical advisory input, consistent with a regime in which technical screening was not systematically integrated into adjudication during our sample period.

⁵Early evidence on the 2024 reforms (Súmulas Vinculantes 60 and 61; Temas 1,234 and 6) suggests modest effects: health lawsuits declined approximately 7 percent in the first half of 2025, but grant rates for preliminary injunctions remain above 73 percent (Conselho Nacional de Justiça & Programa das Nações Unidas para o Desenvolvimento, 2025).

3.6 Why Courts Affect Local Health Budgets

Several features of health litigation make court orders consequential for municipal budgets. First, compliance is mandatory and swiftly enforced. The typical preliminary injunction at TJSP is granted within 10 days of filing and requires compliance within 24 hours to 15 days. Non-compliance triggers direct seizure of funds from municipal bank accounts, daily fines, and potential personal criminal liability for public officials.⁶ Second, treatments are considered irreversible once consumed, so the fiscal impact is locked in at the preliminary stage. Even if the decision is later reversed on appeal, the municipality almost never recovers its expenditure. Third, many orders mandate continuous provision of chronic disease medications, creating recurring budget commitments that accumulate over time. The cumulative stock of active orders—not just the flow of new cases—determines the fiscal burden, motivating the dynamic analysis in Section 6.

The fiscal burden is amplified by procurement inefficiency. Court orders require urgent compliance, so municipalities must purchase drugs through emergency procurement (*dispensa de licitação*), bypassing competitive bidding. Genicolo-Martins and Furquim de Azevedo (2024) estimate that court-mandated drug purchases cost 30–44 percent more than equivalent drugs procured through standard SUS channels.⁷ The premium reflects retail-quantity purchases, brand-name mandates, and strategic pricing by pharmaceutical

⁶Fund seizure (*bloqueio*) is sanctioned by STJ Tema 84; daily fines (*astreintes*) by STJ Tema 98.

⁷In some municipalities, judicial health spending absorbs a large share of the pharmaceutical budget—16 percent in Campinas in 2009, and over half in some smaller municipalities (Diniz et al., 2014).

firms that delay SUS incorporation to profit from the higher-priced judicial channel. Each real of judicial spending thus displaces more than one real of planned health expenditure. Because court orders create non-discretionary spending within a constitutionally mandated health budget floor, they crowd out planned allocations to other programs—primarily primary care, prevention, and population-level interventions (Ferraz, 2009).

The next sections describe the data and empirical strategy we use to estimate these returns.

4 Data

4.1 Health Lawsuits

Our litigation data cover São Paulo, the state with the highest volume of health cases and the setting where budget and case records can be linked at the transaction level. We draw on two courts: the Tribunal de Justiça de São Paulo (TJSP) for cases against municipalities and the state, and the Tribunal Regional Federal da 3^a Região (TRF3) for cases against the federal government. We identify health cases in the DataJud national repository (Conselho Nacional de Justiça, 2020), which provides structured metadata (case number, court, filing date, subject codes, parties, judge) for all cases from 2020 onward. We supplement DataJud with the official electronic gazette (available from 2007 for TJSP) and full-text decisions from the Consulta de Julgados portal (Tribunal de Justiça do Estado de São Paulo, 2024), avail-

able from 2014 for TJSP.⁸ From full-text decisions we extract the treatment requested, diagnosis, grant/deny outcome, and deciding judge—the building blocks for the judge stringency instrument and the municipality-level judicial spending measure.

Cases at both TJSP and TRF3 are assigned to judges through an automated sequential distribution system within each court; we describe the assignment mechanism and address exceptions (related-case routing [*prevenção*], entity-dimension forum shopping) in Section 5.

4.2 Health Spending and Budgets

Our primary source for municipal health spending is the line-item expenditure data published by the Tribunal de Contas do Estado de São Paulo (TCE-SP) through its transparency portal (Tribunal de Contas do Estado de São Paulo, 2024). These data originate from the Audesp (Auditoria Eletrônica de Órgãos Públicos) system, which requires all 644 municipalities under TCE-SP jurisdiction to report every budget execution event—commitment (*empenho*), verification (*liquidação*), and payment (*pagamento*)—in a standardized electronic format. The resulting dataset provides transaction-level records of municipal health expenditures from 2008 to 2024, classified by the functional-programmatic budget structure (Portaria 42/1999): function (*função*), subfunction (*sub função*), program (*programa*), and expenditure type (*natureza da despesa*). Revenue source (*fonte de recurso*) distinguishes federal SUS transfers from own-source municipal revenues.

⁸TPU code curation, cross-source validation, and case-variable extraction are described in Appendix B.

A key feature of the TCE-SP data is the transaction history field (*histórico*)—a free-text description accompanying each expenditure entry that records the legal and administrative basis for the transaction. We identify court-mandated spending by searching this field for explicit judicial-compliance descriptors such as “determinação judicial,” “tutela antecipada,” and “cumprimento de sentença” (the full list of descriptors and validation procedures are in Appendix B). This identification captures actual expenditures incurred to comply with court orders, providing a direct measure of the fiscal burden of health litigation at the municipality-month level—without requiring us to estimate costs from court records.

To examine how court orders affect the composition of health spending, we exploit the subfunction classification within the Health function. The six health subfunctions—Primary Care, Hospital and Outpatient Care, Pharmaceutical and Therapeutic Support, Health Surveillance, Epidemiological Surveillance, and Nutrition—have remained stable since 1999, ensuring consistent definitions across our sample period.⁹ This allows us to test directly whether judicial spending crowds out Primary Care in favor of Pharmaceutical and Therapeutic Support, the category where court-ordered purchases concentrate. We supplement the TCE-SP data with SIOPS (Sistema de Informações sobre Orçamentos Públicos em Saúde; Ministério da Saúde, 2024d) for cross-validation and for comparison with municipalities outside São Paulo.

⁹The subfunctions and their codes under the functional-programmatic structure (Portaria 42/1999) are: Primary Care (Atenção Básica, 301), Hospital and Outpatient Care (Assistência Hospitalar e Ambulatorial, 302), Pharmaceutical and Therapeutic Support (Suporte Profilático e Terapêutico, 303), Health Surveillance (Vigilância Sanitária, 304), Epidemiological Surveillance (Vigilância Epidemiológica, 305), and Nutrition (Alimentação e Nutrição, 306).

TCE-SP covers all 644 municipalities in the state of São Paulo except the capital, which falls under the separate jurisdiction of the Tribunal de Contas do Município de São Paulo (TCM-SP). Health litigation at TJSP is concentrated in the interior: approximately 77 percent of health cases are filed in the 321 judicial districts (*comarcas*) outside the capital, handled by over 3,000 judges. The capital accounts for the remaining 23 percent. Our spending data therefore cover the jurisdictions where most health litigation occurs. We discuss the implications of excluding the capital in Section 9.

4.3 Health Outcomes

We measure health outcomes using individual-level microdata from three DATASUS registries: hospital admissions (SIH/SUS) (Ministério da Saúde, 2024a), mortality (SIM) (Ministério da Saúde, 2024b), and live births (SINASC) (Ministério da Saúde, 2024c). The SIH records every hospitalization funded by SUS, including patient municipality, admission and discharge dates, primary and secondary diagnoses (ICD-10), procedure codes, age, sex, and cost. SIM covers the universe of deaths with cause (ICD-10), municipality, date, and demographic characteristics. Both registries are available from approximately 2000 onward, covering our full sample period. SINASC records all live births with birth weight, gestational age, prenatal visit count, and maternal characteristics.

Individual-level microdata enable three capabilities beyond municipality-year aggregates from DATASUS tabulations. First, we construct cause-specific and age-stratified outcomes: avoidable hospitalizations (ICSAP cri-

teria; Alfradique et al., 2009) decomposed by condition family (cardiovascular, respiratory, infectious), amenable mortality by age group, and birth outcomes. Second, we aggregate outcomes at the municipality-month or municipality-quarter level rather than municipality-year, exploiting the higher-frequency variation in judicial spending available from TCE-SP budget records. Third, by linking litigant names to SIH and SIM records (Section 4.5), we can partition outcomes into litigant and non-litigant components, enabling separate estimation of direct benefits and population spillovers.

For intermediate (mechanism) outcomes we draw on ESF coverage (number of active Family Health Strategy teams per municipality, from the Ministry of Health’s e-Gestor platform) and vaccination coverage (from the National Immunization Program, PNI). For placebo tests, we use educational attainment from the School Census (INEP), formal employment from RAIS, and nighttime light intensity from VIIRS satellite data.

4.4 Socioeconomic Characteristics

We draw on two individual-level administrative registries. RAIS (Relação Anual de Informações Sociais) is the annual census of formal-sector employment, covering the universe of formal employment relationships with earnings, occupation, employer, and employment duration (Ministério do Trabalho e Emprego, 2024). We use RAIS both to profile litigants (earnings level, sector, employment trajectory) and as an individual-level outcome source: for linked litigants, we construct pre- and post-filing earnings and employment indicators to estimate the direct labor-market effects of win-

ning a health lawsuit. CadÚnico (Cadastro Único) is the registry for social programs targeting low-income families (Ministério do Desenvolvimento e Assistência Social, 2024), which identifies individuals at the bottom of the income distribution and captures participation in Bolsa Família and other transfer programs.

We link individuals across litigation records, SIH, SIM, RAIS, and CadÚnico using a complete Brazilian name registry that covers all formally registered names. Approximately 50 percent of full names in the registry are nationally unique, enabling deterministic matching without probabilistic record linkage for those individuals. Our baseline linkage restricts to verified unique names—individuals whose full name appears exactly once in both the litigation records and the target registry within the relevant state and year. This conservative approach sacrifices coverage for precision; we assess robustness to alternative linkage criteria in Section 9 and Appendix B.4. At the municipality level, we complement individual-level linkage with aggregate socioeconomic controls from the Brazilian Census and PNAD.

4.5 Sample Construction and Limitations

Our main analysis sample covers health lawsuits filed at TJSP from 2014 onward, the period for which full-text decisions are available through Consulta de Julgados. We extend the sample back to 2007 using the Diário de Justiça for analyses that require only case-level indicators (filing, grant/deny, judge assignment) rather than detailed treatment information. TRF3 cases from 2011 onward supplement the TJSP data, though for the federal court

we rely solely on the gazette and therefore have less granular information on treatment requests.

The name-linkage procedure is relevant only for the litigant direct-effects analysis (Section 5.4); population-level and non-litigant spillover analyses do not require individual linkage. For litigant analyses, we match plaintiff names from court records to SIH, SIM, and RAIS using the complete name registry. We report a selection table comparing linked and unlinked litigants on observable case characteristics (court, filing year, grant rate, case type, attorney type, municipality of origin). Because name uniqueness may correlate with socioeconomic status and ethnicity, we construct inverse probability weights (IPW) based on a model of linkage probability conditional on observables, and report IPW-reweighted estimates alongside unweighted results. We also stratify results by propensity of name uniqueness to assess sensitivity.

To summarize coverage: litigation records span 2007–present (gazette), 2014–present (full-text decisions), and 2020–present (DataJud structured metadata); TCE-SP budget data cover 2008–2024; SIH and SIM microdata cover approximately 2000–present; RAIS is available annually; SINASC covers all years. Our main analysis uses the intersection of full-text decisions (2014+) and budget data (2008+), yielding a primary estimation sample of 2014–2024.

Three additional scope issues deserve note. Our judicial spending measure captures expenditures that municipalities tag as court-mandated in the transaction history field; we cross-validate against court-records-based spending estimates to assess coverage. Health outcome data from DATASUS cover SUS-funded services only, which is the relevant population for our analysis

since these patients are most affected by municipal budget reallocations. For non-litigant outcome construction (Section 5.3), we exclude linked litigants from SIH and SIM records before aggregating to the municipality-period level; the excluded share is small relative to total hospitalizations and deaths, so the non-litigant outcomes closely approximate population-level measures.

5 Empirical Strategy

5.1 Conceptual Framework

Consider a municipality with a fixed health budget B that an administrator allocates across programs to maximize population health. Courts impose mandatory spending J on specific treatments for individual litigants, reducing the discretionary budget to $B - J$. Because judicial procurement bypasses competitive bidding, each real of court-ordered spending costs a factor $p > 1$ more than equivalent administrative purchases, so the effective displacement is pJ .¹⁰ This setup generates three testable predictions, whose signs depend on which of two polar views of courts is operative at the margin.

Under the *corrective* view, courts remedy genuine failures of administrative delivery—SUS promises treatments it does not provide, and judicial orders compel compliance with existing entitlements. Under the *distortionary* view, courts override administrative priority-setting, channeling resources toward individually litigated treatments with lower marginal returns than

¹⁰Court mandates function as rigid, ex post earmarks in the sense of Buchanan (1963): they commit resources to specific uses after the budget is set, without the ex ante coordination of legislative appropriations. Genicolo-Martins and Furquim de Azevedo (2024) estimate $p \approx 1.3\text{--}1.44$ for court-mandated drug purchases in Brazil.

the programmatic spending they displace. The two views generate identical predictions for budget composition (Prediction 1: both imply displacement of discretionary spending) but diverge sharply on the sign of health effects (Prediction 2) and on where effects are largest. The corrective view predicts health improvements concentrated where administrative capacity is weakest and for treatments already on the formulary; the distortionary view predicts negative net effects, concentrated where litigation is supply-driven and mandates fall outside existing benefit packages. Our heterogeneity analyses (Sections 7 and 8.4) are designed to distinguish these views, focusing on administrative capacity, formulary status, fiscal mismatch, and supply-driven litigation.

Prediction 1: Budget composition. Court mandates shift spending from programmatic categories—primary care, prevention, epidemiological surveillance—toward pharmaceutical and therapeutic spending, the categories where court-ordered purchases concentrate. The displacement is amplified by the procurement premium: a court order for one real of treatment reduces other spending by more than one real. Our budget data, with text-tagged judicial transactions classified by subfunction, allow us to test this prediction directly.

Prediction 2: Population health. The net effect on population health depends on relative marginal returns. If displaced programmatic spending (primary care, vaccination, prenatal services) has higher marginal returns than court-ordered treatments (typically high-cost, individualized, curative), court mandates reduce population health on net.¹¹ The sign is an empiri-

¹¹This is the standard diminishing-returns argument: court-ordered treatments tar-

cal question that our judge IV design can answer. We benchmark the estimated return to court-mandated spending against the return to administrative health expenditure, estimated separately using variation in federal transfers (Section 6).

Prediction 3: Distributional incidence. If access to litigation is positively correlated with socioeconomic status, court mandates transfer health budget resources from the general population—who bear the cost of crowd-out—to a selected subpopulation of litigants.¹² The net distributional effect depends on two margins: who litigates (direct beneficiaries of court orders) and whose care is displaced (those who depend on the crowded-out programs). We test this by linking litigants to income registries and examining whether judge-induced spending shocks affect health outcomes differently across the income distribution.

A further consideration is that the marginal cases induced by lenient judges may be disproportionately supply-driven—generated by concentrated lawyer-physician-pharmaceutical networks rather than by patient need. We probe this in Section 8.4 by splitting cases by the concentration of legal representation.

get patients already receiving attention, while primary care investments operate where marginal returns are highest (Grossman, 1972). Alternatively, if agencies fail to deliver treatments they have approved—medication stockouts are pervasive in Brazilian municipalities (Confederação Nacional de Municípios, 2022)—courts may correct genuine allocation failures, and the sign reverses.

¹²Galanter (1974) shows that repeat players systematically outperform one-shotters in litigation; Shavell (1982) shows that private incentives to litigate diverge from social optimality when costs are unevenly distributed.

5.2 Identification

Judicial health spending is endogenous. Municipalities with sicker populations, weaker public health systems, or more active legal aid organizations are likely to experience both more health litigation and worse health outcomes. Correlations between judicial spending and health outcomes therefore confound the effect of courts with the underlying conditions that drive litigation.

We address this challenge by exploiting quasi-random variation in judge assignments. In the Brazilian first-instance judiciary, cases filed at a given trial court (*vara*) are assigned to judges through an automated, sequential distribution system designed to ensure balanced workloads and prevent judge selection. In judicial districts with multiple public-law trial courts (*varas da fazenda pública*), CNJ Resolution 238/2016 mandates that one court specialize in public health cases. Health cases are therefore randomly assigned among judges within the designated health court, conditional on the district and filing period. Litigants and their attorneys cannot select or influence which judge is assigned to their case.

Judges differ persistently in their propensity to grant health requests, even when facing similar cases within the same court. This variation in judicial behavior—judge “stringency”—generates exogenous shocks to the volume and cost of court-mandated health spending. Because technical screening was rarely binding during our sample period (Section 3), this variation reflects meaningful judicial discretion rather than differential adherence to clinical guidelines.

We measure judge stringency using a leave-one-out (jackknife) estimator

(Bhuller et al., 2020; Kling, 2006; Maestas et al., 2013). For each case i assigned to judge j in court c at time t , we define the instrument as:

$$Z_{ijct} = \frac{1}{n_{jct} - 1} \sum_{k \neq i: j(k)=j} D_k, \quad (1)$$

where D_k is an indicator (or a measure of the cost) of granting the request in case k , and the sum is over all other cases assigned to judge j in the same court-year cell. The leave-one-out construction ensures that the instrument for case i is not mechanically related to the outcome of case i itself.

Because our outcomes of interest—population health measures—are observed at the municipality level, we aggregate the case-level instrument to that unit of analysis. Specifically, we average the instrument across all cases originating in municipality m at time t :

$$\bar{Z}_{mt} = \frac{1}{n_{mt}} \sum_{i \in m,t} Z_{ijct}, \quad (2)$$

where n_{mt} is the number of health cases filed from municipality m in period t . Municipalities whose cases happen to be assigned to more lenient judges receive a positive shock to court-mandated spending; those assigned to stricter judges receive a negative shock.¹³

The same source of exogenous variation—random judge assignment—maps to two tiers of analysis. At the municipality level, judge leniency

¹³The aggregated instrument \bar{Z}_{mt} has a shift-share (Bartik) structure: the “shocks” are judge-level leniency and the “shares” are the case composition linking municipalities to judges. In the framework of Borusyak et al. (2022), identification relies on the exogeneity of shocks—here directly justified by random assignment of judges within courts—rather than on the exogeneity of shares.

aggregated across cases filed from a given municipality generates exogenous shocks to the volume of court-mandated spending, enabling estimation of population-level effects on budgets, health outcomes, and non-litigant spillovers (Section 5.3). At the case level, the leniency of the judge assigned to a specific plaintiff generates exogenous variation in whether the request is granted, enabling estimation of the direct effect on litigant health and labor-market outcomes (Section 5.4). The two tiers share identification assumptions but differ in the unit of analysis, the endogenous variable, and the estimand.

Three setting-specific threats to identification deserve attention.¹⁴ First, related-case routing rules (*prevenção*) direct connected cases to the original judge, creating non-random clustering; we address this by excluding linked cases from the leave-one-out calculation. Second, litigants choose which level of government to sue, but this entity-dimension forum shopping does not affect judge assignment *within* a given court. Third, lenient judges may affect health through non-spending channels: granting interim relief faster, imposing fund seizures (*bloqueio*), setting daily fines (*astreintes*), or creating personal liability threats for officials—all of which could independently disrupt municipal health administration. Our estimand is therefore best interpreted as the effect of “judicial intensity” broadly, not spending alone; we address this by examining whether the instrument predicts non-spending enforcement actions in Section 9. Fourth, the fiscal burden at any point de-

¹⁴The standard examiner-design assumptions are relevance (judge stringency predicts spending; assessed via first-stage F), independence (assignment uncorrelated with municipality characteristics conditional on court-time; tested in Section 9), exclusion (stringency affects health only through spending), and monotonicity (a more lenient judge is weakly more likely to grant all cases; Imbens and Angrist, 1994).

pends on the *stock* of active court orders, while our instrument is constructed from the *flow* of new case assignments. This is not a threat to validity—the flow creates the stock incrementally, and persistent judge assignments mean that flow-based shocks predict stock changes—but it implies that effects may accumulate over time, motivating the dynamic analysis in Section 6.7. We present balance tests and placebo outcomes in Section 9.

5.3 Population-Level Estimating Equations

Our main specification is a two-stage least squares (2SLS) model estimated at the municipality-period level. The first stage relates judicial spending to the instrument:

$$J_{mt} = \alpha + \delta \bar{Z}_{mt} + X'_{mt}\beta + \gamma_c + \theta_t + \varepsilon_{mt}, \quad (3)$$

where J_{mt} is per capita court-ordered health expenditure in municipality m in period t , \bar{Z}_{mt} is the aggregated leave-one-out judge stringency instrument defined in equation (2), X_{mt} is a vector of time-varying municipality controls (log population, income per capita, Gini coefficient, ESF coverage, and baseline health spending per capita), γ_c are court fixed effects, θ_t are period fixed effects, and ε_{mt} is the error term. The period is municipality-month for spending outcomes (which are observed monthly in TCE-SP) and municipality-quarter or municipality-year for health outcomes depending on cell-size requirements. The coefficient δ captures the first-stage relationship between judge leniency and judicial spending.

The second stage estimates the effect of judicial spending on health out-

comes:

$$Y_{mt} = \alpha + \tau \hat{J}_{mt} + X'_{mt}\beta + \gamma_c + \theta_t + \nu_{mt}, \quad (4)$$

where Y_{mt} is a health outcome constructed from individual-level SIH or SIM microdata—such as the rate of avoidable hospitalizations, cause-specific mortality, or birth outcomes—and \hat{J}_{mt} is the predicted value from the first stage. For population-level outcomes, Y_{mt} includes all individuals residing in municipality m , including litigants. For non-litigant outcomes, we exclude individuals linked to health lawsuits from SIH and SIM records before aggregating; the excluded share is small, so non-litigant outcomes closely approximate population measures. Comparing the two provides a first indication of whether effects operate through direct benefits to litigants or spillovers to the broader population.

To compare the marginal product of judicial and administrative health spending, we extend the specification to a two-endogenous-variable model:

$$Y_{mt} = \alpha + \tau_J J_{mt} + \tau_A A_{mt} + X'_{mt}\beta + \gamma_c + \theta_t + \nu_{mt}, \quad (5)$$

where A_{mt} is per capita non-judicial (administrative) health spending and J_{mt} is judicial spending as before. We instrument J_{mt} with judge leniency \bar{Z}_{mt} and A_{mt} with plausibly exogenous variation in federal SUS transfers—specifically, changes in per capita transfer allocations driven by formula revisions that shift resources across municipalities within the same state and year. The coefficients τ_J and τ_A are partial derivatives—the marginal health return to an additional real of spending through each allocation channel, holding the other constant. This reframes the comparison from “two LATEs from

separate regressions” to “two margins identified by orthogonal shifters in a single equation.” The policy margins differ (judge leniency shifts targeted pharmaceutical spending; transfer variation shifts programmatic spending), so the comparison is informative about relative efficiency but does not yield a clean welfare ranking without additional assumptions about comparability.

We include court fixed effects γ_c throughout, so that identification comes from variation in judge stringency within courts. Period fixed effects θ_t absorb common time trends. Standard errors are clustered at the court (*vara*) level—the unit within which judges are randomly assigned; we assess robustness to alternative clustering (municipality, court-year, two-way) in Section 9.

Our 2SLS identifies the effect of spending changes induced by judicial discretion at the margin—the policy-relevant margin for reforms that tighten or relax granting standards. Because we use a single aggregated instrument rather than individual judge indicators, the specification avoids many-instruments bias (Bhuller et al., 2020). We report the first-stage *F*-statistic throughout and assess robustness to alternative constructions of the instrument.

The estimand τ is a weighted average of marginal effects per real of judicial spending, where the weights reflect how judge assignment moves spending across case types (on-formulary versus off-formulary, drugs versus procedures, high-volume-attorney versus Defensoria cases). It captures the municipality-wide effect, bundling direct benefits to litigants, indirect spillovers through crowd-out, and composition shifts in the health budget. The complier municipalities are those whose judicial spending is responsive to the leniency of assigned judges—plausibly municipalities with many borderline cases and

substantial judge-level variance in stringency. We decompose the first stage by case type to characterize which margins drive spending variation. We report specifications both with and without controlling for the number of health cases filed (n_{mt}); because filing volume may itself respond to perceived judicial leniency, conditioning on n_{mt} risks post-treatment bias, so our preferred specification excludes it.¹⁵

5.4 Litigant-Level Estimating Equations

To estimate the direct effect of winning a health lawsuit on litigant outcomes, we exploit case-level random judge assignment. The unit of analysis is a plaintiff i observed in a panel around the filing date. The first stage relates the grant decision to judge leniency:

$$D_{ijct} = \alpha + \pi Z_{ijct} + W'_{it}\beta + \phi_{ct} + \eta_{ijct}, \quad (6)$$

where D_{ijct} is an indicator (or a measure of the cost) of granting the request for plaintiff i assigned to judge j in court c at time t , Z_{ijct} is the leave-one-out judge leniency measure defined in equation (1), W_{it} are plaintiff-level controls (age, sex, case type), and ϕ_{ct} are court-by-filing-period fixed effects.

The second stage estimates the effect on post-filing outcomes:

$$Y_{i,t+s} = \alpha + \lambda \hat{D}_{ijct} + W'_{it}\beta + \phi_{ct} + \nu_{ijct}, \quad (7)$$

where $Y_{i,t+s}$ is a health or labor-market outcome for plaintiff i measured s

¹⁵We test this directly by estimating the reduced-form effect of \bar{Z}_{mt} on n_{mt} and on case composition measures (off-formulary share, attorney concentration, Defensoria share).

periods after filing—such as hospitalization, mortality, or earnings in RAIS—and \widehat{D}_{ijct} is the predicted grant from the first stage. The parameter λ identifies the causal effect of a favorable judicial decision on litigant outcomes, for the subpopulation of marginal cases (compliers whose grant status is determined by judge leniency). We vary s to trace event-study dynamics, including pre-filing periods as a placebo check.

Standard errors are clustered at the judge level, the unit of random assignment in the case-level design. We present results both unweighted and reweighted by inverse probability of name linkage (IPW) to account for the selection induced by restricting to litigants with nationally unique names (Section 4.5).

5.5 Decomposition

The population-level estimate τ from equation (4) reflects a weighted average of direct effects on litigants and spillovers to non-litigants:

$$\tau \approx \omega \tau^L + (1 - \omega) \tau^{NL}, \quad (8)$$

where τ^L is the per capita effect on litigants (recoverable from the case-level estimates in Section 5.4, scaled by the litigant share of the population), τ^{NL} is the per capita effect on non-litigants (estimated using the non-litigant outcome specification), and ω is the litigant population share. This decomposition connects the two tiers of the analysis. Several sign combinations are informative: $\tau^L > 0$ with $\tau^{NL} < 0$ is consistent with the distortionary view (courts benefit litigants at the expense of crowd-out); $\tau^L > 0$ with $\tau^{NL} \geq 0$

favors the corrective view (courts improve delivery without harming others); and $\tau^L \leq 0$ with $\tau^{NL} < 0$ would suggest that judicial mandates are welfare-reducing even for the targeted population. The decomposition connects to the three predictions in Section 5 and disciplines the interpretation of the aggregate estimate.

6 Effects on Health Outcomes

This section presents the main results in two tiers: population-level effects of judicial spending on health outcomes (Sections 6.1–6.3), and direct effects on litigants (Section 6.4). We then decompose the aggregate estimate (Section 6.5), compare returns across allocation channels (Section 6.6), and examine dynamic effects (Section 6.7). Our primary outcomes are avoidable hospitalizations (ICSAP), infant mortality, and mean birth weight; all other outcomes are treated as secondary or exploratory, and we adjust for multiple testing across the primary set.

6.1 First Stage

Table ?? reports estimates of equation (3). The dependent variable is per capita court-mandated health spending in municipality m in period t . The key regressor is the aggregated leave-one-out judge stringency measure \bar{Z}_{mt} . We report specifications with progressively richer controls: column (1) includes court and period fixed effects only; column (2) adds time-varying municipality characteristics; column (3) adds court-by-period fixed effects. We present first-stage results at both the municipality-year and municipality-

month levels.

Across specifications, municipalities whose health cases are assigned to more lenient judges experience significantly higher judicial health spending. The first-stage F -statistic exceeds conventional thresholds for instrument strength, confirming that judge stringency is a powerful predictor of judicial spending at the municipality level.

6.2 Population Health Effects

Table ?? presents the 2SLS estimates of equation (4). Each column reports the effect of a one-unit increase in per capita judicial health spending on a different health outcome, constructed from individual-level SIH and SIM microdata aggregated to the municipality-period level. Panel A reports results for mortality outcomes (infant mortality rate, under-five mortality, amenable mortality by cause). Panel B reports results for hospitalizations (total hospitalization rate, avoidable hospitalizations decomposed by ICSAP condition family). Panel C reports results for birth outcomes (mean birth weight, incidence of low birth weight, preterm birth rate).

For comparison, we report the OLS estimates alongside the 2SLS estimates. If judicial spending is positively correlated with unobserved health needs—as we would expect if sicker populations generate more litigation—then OLS estimates of τ will be biased toward finding that judicial spending worsens health outcomes (or, equivalently, biased against finding health improvements). A comparison of the OLS and 2SLS estimates thus provides a diagnostic for the direction and magnitude of endogeneity bias.

The reduced-form estimates—regressing health outcomes directly on the instrument \bar{Z}_{mt} —provide a model-free assessment of whether judge stringency affects population health. Because the reduced form does not depend on the first-stage functional form, it offers a transparent check on the main results.

6.3 Non-Litigant Effects

To isolate spillovers to the non-litigating population, we re-estimate equation (4) using outcomes that exclude linked litigants from the SIH and SIM microdata before aggregation to the municipality-period level. Because litigants are a small share of total hospitalizations and deaths in any municipality-period, the non-litigant outcomes closely approximate the population-level measures, and any difference between the two indicates the contribution of direct litigant effects to the aggregate estimate.

6.4 Direct Effects on Litigants

Table ?? presents the 2SLS estimates of equation (7), using the case-level judge leniency instrument. The sample is the plaintiff-by-period panel constructed from linked litigants. The endogenous variable is a grant indicator (or the granted cost). We report effects on post-filing hospitalization, mortality, and earnings in RAIS. Event-study figures trace effects from 12 months before filing (as a pre-trends check) through 36 months after.

Court-by-filing-period fixed effects ensure that identification comes from within-court variation in judge leniency. Standard errors are clustered at

the judge level. We present both unweighted estimates and IPW-reweighted estimates that adjust for the selection induced by name-linkage restrictions.

6.5 Decomposition: Direct Benefits versus Crowd-Out

We interpret the population, non-litigant, and litigant estimates jointly using the decomposition in equation (8). The key question is whether the aggregate population effect τ reflects direct benefits to litigants, spillovers (positive or negative) to non-litigants, or both. Several sign combinations are possible. If litigant effects are positive but the population effect is zero or negative, the implied non-litigant effect is negative—consistent with crowd-out. If both litigant and non-litigant effects are positive, courts improve health broadly, consistent with the corrective view. We report the implied decomposition with confidence intervals obtained by bootstrapping jointly across the two tiers.

6.6 Comparing Returns: Courts versus Administrative Allocation

Table ?? reports three numbers for each outcome: (i) the marginal product of judicial spending (from the judge IV), (ii) the marginal product of administrative spending (from the transfer IV in equation (5)), and (iii) the non-litigant-only judge effect, which provides the cleanest “courts versus bureaucracies” comparison by stripping out direct litigant benefits. We translate estimates into cost-effectiveness metrics: deaths averted and ICSAP hospitalizations averted per R\$ million of spending through each channel.

The two instruments identify effects at different policy margins: judge leniency shifts targeted pharmaceutical spending subject to procurement premia, while transfer variation shifts programmatic spending through a fiscal federalism channel. The two-endogenous-variable model (equation 5) addresses this by estimating both partial derivatives simultaneously, but the comparison remains informative about relative efficiency only under the assumption that the two spending channels affect health through distinct and separable mechanisms.

6.7 Dynamic Effects

To examine the timing of effects, we estimate event-study specifications at both tiers. At the municipality level, we allow the effect of judicial spending to vary with the lag between the period of judge assignment and the period of health outcome measurement. This tests whether effects emerge immediately (consistent with direct health gains from court-ordered treatments) or with a delay (consistent with crowding out of preventive services that affects health with a lag, and with the accumulation of ongoing mandates that leave municipal budgets increasingly rigid). At the litigant level, the event-study figures from Section 6.4 trace the trajectory of individual health and earnings relative to filing date.

The event-study estimates also serve as a pre-trends check: effects in periods before the judicial spending shock should be close to zero if the instrument is valid.

7 Who Benefits? Distributional Incidence

7.1 Who Are the Litigants?

Before examining distributional effects, we characterize the population of health litigants. Table ?? compares litigants linked to RAIS and CadÚnico with the general population in their municipality along several dimensions: formal employment status, average earnings (from RAIS), earnings trajectory in the years before filing, participation in Bolsa Família and other social programs, CadÚnico registration, and age. Because RAIS covers the universe of formal employment relationships, we can characterize not just the average litigant but the full earnings distribution—including the shares below the minimum wage, between one and three minimum wages, and above three minimum wages—and compare it to the population distribution in the same municipality-year.

This descriptive analysis speaks to a central question in the literature: whether health litigation is a tool of the relatively advantaged or whether it reaches disadvantaged populations. The answer may differ across court systems (TJSP versus TRF3) and over time, as the Defensoria Pùblica has expanded its role in health litigation.

A notable gap in the existing literature is the absence of racial disaggregation. Despite Brazil’s mandatory race/color reporting in health records and the well-documented racial stratification of SUS dependence—approximately two-thirds of SUS users identify as Black or *pardo* (mixed-race)—no study of health litigation reports the race of litigants. The circumstantial evidence suggests under-representation: litigants are disproportionately drawn from

wealthier, lower-vulnerability neighborhoods, represented by private attorneys, and hold prescriptions from private providers—all characteristics inversely correlated with being Black or *pardo* in Brazil (Chieffi & Barata, 2009; Ferraz, 2009). Our linked administrative data allow us to examine this dimension directly.

7.2 Heterogeneous Effects by Municipality Characteristics

We next examine whether the health effects of judicial spending differ across municipalities with different baseline characteristics. We split the sample along several dimensions: municipality income (above versus below the median), inequality (Gini coefficient), the share of the population enrolled in CadÚnico, and the presence of a Defensoria Pública office. Three additional splits are designed to discriminate between the corrective and distortionary views introduced in Section 5. First, baseline health system capacity (ESF coverage) tests whether courts are most beneficial where administrative delivery is weakest. Second, fiscal capacity (own-source revenue share) tests whether vertical fiscal mismatch amplifies the costs of court mandates. Third, the on-formulary versus off-formulary composition of judicial demand provides a direct discriminator: on-formulary mandates correct administrative delivery failures, while off-formulary mandates impose new obligations relative to the administratively defined benefit package.

The Defensoria split exploits dramatic variation in access to public legal representation: as of 2013, public defenders were absent from 72 percent of

Brazilian comarcas (Instituto de Pesquisa Econômica Aplicada & Associação Nacional dos Defensores Públicos, 2013). Defensoria presence is associated with a seven-fold increase in the probability of health litigation (Biehl et al., 2015), suggesting that legal access—not health need alone—shapes the geography of lawsuits. Municipalities with a Defensoria have a different composition of litigants (more low-income, publicly represented) than those without, allowing us to test whether the health effects of judicial spending depend on who litigates. In the framework of Galanter (1974), the Defensoria functions as a repeat-player institution that lowers per-case costs for disadvantaged litigants, potentially altering both the volume and the health return of judicial spending.

If judicial spending improves health outcomes primarily in municipalities that are already well-resourced, then judicialization reinforces existing inequalities. Conversely, if effects are concentrated in poorer municipalities—perhaps because court orders correct larger deficiencies in public provision—then judicialization may be equalizing.

7.3 Effects on Disadvantaged Populations

We complement the municipality-level heterogeneity analysis with an examination of outcomes that disproportionately affect disadvantaged populations. Individual-level SIH and SIM microdata enable stratification by age group, cause of admission or death, and proxies for SUS dependence. Avoidable hospitalizations (ICSAP) are concentrated among low-income populations who rely on primary care through SUS; we decompose these by condition family

(cardiovascular, respiratory, infectious) and urgency to identify which conditions drive the aggregate effect. Infant mortality and amenable mortality by age provide additional margins where crowd-out of primary care should be most visible. Because SIH records the patient’s municipality, we can construct age- and sex-standardized rates at the municipality-period level, avoiding the ecological fallacy that arises from using only municipality-year aggregates.

8 Mechanisms and Budgetary Trade-offs

The aggregate health effects documented in Section 6 reflect both the direct benefits of court-ordered treatments for litigants and the indirect costs of displacing other health spending. This section investigates the budgetary mechanisms through which judicial spending affects population health.

8.1 Effects on the Composition of Municipal Health Spending

We estimate the effect of judicial spending on the composition of municipal health budgets using subfunction categories as outcomes. Specifically, we replace Y_{mt} in equation (4) with per capita spending on each health subfunction: Primary Care (301), Hospital and Outpatient Care (302), Pharmaceutical and Therapeutic Support (303), Health Surveillance (304), Epidemiological Surveillance (305), and administrative costs.

A key question is whether court-mandated spending increases the total

municipal health budget or merely reallocates within it. If the coefficient on judicial spending is close to one when total health spending is the dependent variable, court orders primarily add to the budget (e.g., through emergency appropriations or deficit spending). If the coefficient is close to zero, court orders are absorbed through reallocation, with other programs shrinking by a corresponding amount.

8.2 Crowding Out of Primary Care

We test directly for crowding out of primary care by examining whether judicial spending affects ESF coverage (the number of active Family Health Strategy teams per capita), vaccination rates, and the quality of prenatal care (measured by the share of births with adequate prenatal visits in SINASC).

These outcomes are important because primary care and preventive services have high average returns to population health (Macinko et al., 2006), and their disruption could generate negative spillovers that offset the direct health gains from court-ordered treatments.

8.3 Effects on Hospital Utilization

Finally, we examine whether judicial spending affects hospital capacity and utilization patterns. Court orders may generate demand for hospitalizations (e.g., court-ordered surgeries or chemotherapy) while simultaneously reducing the resources available for other hospital services. Individual-level SIH microdata enable decomposition by ICD-10 diagnosis group, procedure type, and urgency classification. We decompose the hospitalization effects

along three margins: elective versus emergency admissions, ICSAP condition families (cardiovascular, respiratory, infectious, diabetes-related), and high-cost versus routine procedures. This granularity distinguishes crowd-out of primary-care-sensitive conditions from shifts in hospital case mix driven by court-ordered treatments.

8.4 Supply-Side Drivers: Lawyer Concentration and Pharmaceutical Networks

A growing body of evidence suggests that a substantial share of health litigation is supply-driven: organized networks of lawyers, physicians, and pharmaceutical firms generate cases as a channel for market access, a form of supplier-induced demand (Arrow, 1963; McGuire, 2000). In São Paulo, just 19 lawyers filed 63 percent of 2,927 health cases studied in 2006, with concentration highest for expensive drugs not yet incorporated into SUS (Chieffi & Barata, 2010). Criminal investigations have uncovered fraud networks involving pharmaceutical laboratories, patient associations, and law firms—in one case defrauding R\$63 million from state coffers through fabricated lawsuits (see Soares & Deprá, 2012, and Appendix C for details). Manufacturers also game the health technology assessment process: firms with weak evidence bypass CONITEC evaluation and pursue the judicial channel, where grant rates exceed 80 percent (Tamachiro et al., 2022). If supply-driven cases have lower health returns than demand-driven ones, the marginal cases induced by lenient judges—which may be disproportionately supply-side—will drag down the average return to judicial spending.

We probe this by splitting cases according to the concentration of legal representation: cases filed by high-volume lawyers (attorneys exceeding a threshold number of health cases per year) versus those filed by low-volume or Defensoria Pública attorneys. If the efficiency costs of judicialization are concentrated in the supply-driven segment, the estimated effect of judge leniency on health outcomes should differ across these subsamples, with stronger negative effects in the high-volume-lawyer group.

9 Robustness and Extensions

9.1 Balance Tests

If cases are randomly assigned to judges within courts, the instrument \bar{Z}_{mt} should be uncorrelated with predetermined municipality characteristics after conditioning on court and year fixed effects. Table ?? regresses the instrument on a range of baseline municipality characteristics measured prior to our sample period: population, income per capita, Gini coefficient, ESF coverage, health spending per capita, and baseline health outcomes.

We also test for balance in case characteristics. Conditional on court and year fixed effects, cases assigned to more lenient judges should not differ systematically in diagnosis severity, plaintiff demographics, or the type of treatment requested.

9.2 Alternative Instrument Constructions

We assess the sensitivity of our results to the construction of the instrument. Specifically, we examine: (i) using the judge's grant rate rather than the cost of granted treatments; (ii) varying the leave-out window (leave-one-out versus leave-year-out versus leave-municipality-out); (iii) residualizing the instrument by case characteristics before aggregation; and (iv) restricting the instrument to judges with a minimum caseload to reduce small-sample noise.

9.3 Placebo Tests

We conduct placebo tests using outcomes that should not respond to judicial health spending. These include non-health municipal outcomes such as educational attainment, crime rates, and economic activity (measured by nightlight intensity or formal employment). A significant effect on placebo outcomes would raise concerns about the exclusion restriction.

9.4 Sensitivity Checks

We examine the robustness of the main results to alternative sample definitions and estimation choices: restricting the sample to different time periods; varying the level of clustering (court versus municipality versus court-year); dropping municipalities with very few health cases; and estimating the model separately for TJSP and TRF3 cases.

9.5 Monotonicity

We provide evidence on the monotonicity assumption by testing whether judge stringency is consistent across case types. Specifically, we split health cases by treatment category (medicines, procedures, hospital care) and by case characteristics (on-list versus off-list medicines, cases with Defensoria representation versus private attorneys). If the same judges are consistently lenient or strict across these subgroups, this supports monotonicity.

9.6 Name-Linkage Selection

The litigant-level analysis restricts to plaintiffs with nationally unique names, introducing potential selection. We assess this in three ways. First, we report a selection table comparing linked and unlinked litigants on case characteristics (court, filing year, grant rate, case type, attorney type, municipality of origin). Second, we present IPW-reweighted estimates, where the inverse probability weights are based on a model of linkage probability conditional on observables. Third, we stratify results by quintiles of the predicted name-uniqueness propensity to assess whether treatment effects vary systematically with the likelihood of being linked. If IPW-reweighted and unweighted estimates are similar, selection on observables is unlikely to substantially bias the litigant-level results. We note that name-linkage selection affects external validity (which litigants are represented) but not internal validity (the IV remains valid for the linked subsample).

9.7 Inference Robustness

We assess the sensitivity of inference to alternative clustering and weighting choices. For population-level specifications, we compare standard errors clustered at the court (*vara*) level (baseline), the municipality level, and two-way clustering by court and municipality. For litigant-level specifications, we compare clustering at the judge level (baseline) and the court level. We also report municipality-population-weighted estimates alongside the unweighted baseline, since large municipalities contribute disproportionately to aggregate health outcomes but may respond differently to judicial spending shocks.

10 Discussion and Policy Implications

We use the conceptual framework in Section 5 to interpret the estimates and draw implications for the design of institutions that allocate spending ex post. The discussion is organized around the three predictions developed in Section 5.

10.1 Efficiency

Prediction 1 states that court mandates shift spending from programmatic categories toward pharmaceutical and therapeutic spending, with displacement amplified by the procurement premium. Our 2SLS estimates of τ in equation (4) measure the health return to a marginal real of court-mandated spending. The comparison with administrative spending returns (Section 6) provides an informative benchmark, though the two instruments identify dif-

ferent local average treatment effects—judge leniency shifts targeted pharmaceutical spending, while transfer variation shifts total programmatic spending—so the comparison speaks to relative efficiency only under additional assumptions about the comparability of these margins.

The efficiency interpretation hinges on what gets displaced. If judicial orders primarily crowd out epidemiological surveillance or health administration, even modest returns to judicial spending imply efficiency gains. If instead they displace primary care and prevention, the threshold is correspondingly higher. Our subfunction-level budget data will allow us to distinguish these cases directly.

10.2 Distributional Incidence

Prediction 3 states that if litigation access is positively correlated with income, court mandates transfer resources from the general population to a selected subpopulation of litigants. The distributional analysis in Section 7 tests this by examining two margins: who litigates (direct beneficiaries) and whose care is displaced (indirect costs of crowding out).

Even if litigants are predominantly disadvantaged—as some studies suggest (Biehl et al., 2012)—the net distributional effect could be regressive if crowding out disproportionately harms the poorest populations who depend on primary care. Conversely, if litigants are relatively advantaged (Ferraz, 2009), but judicial spending creates positive spillovers (e.g., by forcing municipalities to increase total health budgets), the net effect could nonetheless be progressive.

10.3 Implications for Health Litigation Institutions

Our estimates translate to specific policy counterfactuals: tightening preliminary injunction standards, mandating NAT-Jus technical review before granting, raising evidentiary thresholds for off-formulary drugs (as in Tema 106), or requiring administrative exhaustion before litigation. If marginal returns to judicial spending are negative, welfare arguments favor raising the evidentiary threshold further, because the marginal case is welfare-reducing. If marginal returns are positive, reforms should prioritize targeting (directing judicial resources toward high-return cases) rather than restriction.

More broadly, the comparison of judicial and administrative returns informs whether demand should be channeled away from courts. If judicial allocation is less efficient, administrative resolution mechanisms—which several municipalities have already developed—may improve population health.¹⁶ If the two are comparable, the equity dimension becomes decisive.

Frontier therapies amplify the information problem. When courts override both the regulatory agency and the health technology assessment body under high clinical uncertainty, the wedge between judicial and administrative allocation is likely largest. A recent gene therapy case illustrates the pattern: despite suspended regulatory registration and a negative HTA assessment, over 200 active lawsuits sought to compel SUS to provide the treatment at a potential cost of R\$3 billion.¹⁷ The efficiency costs of judicial

¹⁶Rio de Janeiro's Câmara de Resolução de Litígios em Saúde (CRLS) resolved over 116,000 health demands without litigation between 2013 and 2023; municipal NAT-Jus units reduced new judicial actions by 37 percent in Joinville (SC).

¹⁷The gene therapy Elevidys (delandistrogene moxeparvovec), priced at R\$14.6 million per patient, had its ANVISA registration suspended after the manufacturer's confirmatory trial did not demonstrate significant functional improvement; CONITEC concluded that

allocation may be most acute where the information disadvantage of courts relative to specialized assessment bodies is greatest.

10.4 Lessons Beyond Brazil

Health judicialization is growing in middle-income countries with constitutionalized social rights, including Colombia, Costa Rica, South Africa, and India. Brazil offers the largest and most mature case, but the underlying question—whether judicial intervention improves or worsens the allocation of scarce public resources—generalizes wherever adjudicators impose case-driven, ex post spending mandates on fixed budgets.

11 Conclusion

This paper estimates the causal effect of court-mandated health spending on budget composition, population health, and distributional incidence in Brazil. Exploiting random assignment of judges to health lawsuits, we use judge stringency as an instrument for court-mandated spending to identify its effects at the municipality level.

The combination of direct spending measurement in budget execution records and a judge-IV design allows us to trace the full chain from judicial mandates through budget composition to population health and distributional incidence—providing direct evidence on how the mode of allocation shapes the returns to public spending.

the therapy's benefits did not exceed standard treatment. As of mid-2025, 213 active lawsuits sought to compel provision (Folha de S.Paulo, 2025b).

The analysis has limitations. Our estimates identify a local average treatment effect for marginal cases affected by judge stringency—cases that may be disproportionately supply-driven and thus yield lower health returns than infra-marginal cases. The focus on São Paulo, while offering a large and well-documented setting, limits external validity to states with different health system capacities and litigation patterns. And our judicial spending measure relies on text-based identification that may understate true judicial spending if municipalities inconsistently tag expenditures.

These limitations notwithstanding, our results speak to a question that extends well beyond Brazil: whether the institution that allocates public spending—courts versus administrative agencies—matters for the outcomes it produces. As courts around the world increasingly adjudicate claims to social services, understanding the efficiency and distributional consequences of this judicial role becomes central to institutional design.

Data Availability Statement

The litigation data are constructed from publicly available records: DataJud (Conselho Nacional de Justiça, 2020), the TJSP electronic gazette, and the Consulta de Julgados portal (Tribunal de Justiça do Estado de São Paulo, 2024). Municipal budget execution data are published by TCE-SP through its transparency portal (Tribunal de Contas do Estado de São Paulo, 2024). Health outcomes are from DATASUS registries (SIH, SIM, SINASC), which are publicly available. Individual-level linkage to RAIS and CadÚnico requires restricted-access authorization from the Brazilian Ministry of Labor

and the Ministry of Social Development, respectively. Replication code and instructions for accessing the public data sources will be made available upon publication.

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A Detailed Jurisprudence

This appendix provides additional detail on the key judicial rulings discussed in Section 3.

STA 175 (2009–2010). The first major institutional response came in 2009–2010, when the STF convened a public hearing on health (Audiência Pública No. 4) and subsequently issued its decision in STA 175. The ruling established parameters for judicial decision-making—distinguishing cases where existing SUS policy is not being implemented from cases requesting treatments outside the standard benefit package, and discouraging orders

for experimental treatments. *[Identification]* If binding, STA 175 would reduce judge discretion for off-formulary requests; the evidence below suggests limited compliance in our period.

NAT-JUS (2016). CNJ Resolution 238 created NAT-JUS (Núcleos de Apoio Técnico do Judiciário), technical advisory bodies composed of physicians and pharmacists that provide evidence-based opinions to assist judges in health cases. The electronic platform e-NatJus, launched in 2017, gives judges access to standardized technical assessments and CONITEC reports, reducing reliance on plaintiff-submitted medical prescriptions alone. *[First stage]* NAT-JUS availability could reduce judge-level variation in granting rates; we test whether the instrument's first stage weakens in districts with NAT-JUS infrastructure.

Tema 106 (2018). The STJ's ruling on Tema 106 (REsp 1,657,156) established three cumulative requirements for ordering medicines not incorporated into SUS: (i) a detailed medical report demonstrating inefficacy of SUS alternatives, (ii) proof of the patient's financial incapacity, and (iii) existing ANVISA registration. *[Composition]* If enforced, Tema 106 raises the bar for off-formulary orders, potentially changing the composition of marginal cases in our sample from 2018 onward.

Tema 500 (2019). The STF's ruling on Tema 500 (RE 657,718) held that drugs without ANVISA registration generally cannot be judicially ordered, with narrow exceptions for unreasonable regulatory delay. *[Composition]* If binding, Tema 500 eliminates the highest-risk judicial orders (unregistered

drugs); we test for compositional shifts in judicial spending around 2019.

Evidence on compliance with judicial standards. Despite these formal constraints, there is little evidence that they substantially changed judicial behavior. A 2019 study commissioned by the CNJ, based on a nationally representative sample of first-instance health decisions, found that fewer than 6 percent explicitly cited CONITEC reports, RENAME lists, or other technical standards when ruling on medication requests (CNJ/INSPER, 2019). Separately, data from state courts with NAT-JUS infrastructure reveal that over 80 percent of cases flagged as “urgent” by plaintiffs do not meet clinical urgency criteria upon technical review, yet the consultation process is frequently bypassed (Conselho Nacional de Justiça & Programa das Nações Unidas para o Desenvolvimento, 2025). The volume of “supply of medicines” cases dipped by only 4 percent between 2017 and 2018, when Tema 106 took effect, and overall health litigation continued to accelerate through the 2020s.

2024 reforms. The STF’s 2024 decisions on Temas 1,234 and 6 consolidated the criteria for non-incorporated drugs, established cost thresholds directing cases above 210 minimum salaries to the federal courts, and introduced a federal reimbursement mechanism. Súmulas Vinculantes 60 and 61, also issued in 2024, imposed a binding administrative exhaustion requirement: patients must first seek the treatment through SUS’s administrative channels before filing suit. Early evidence suggests modest effects: SUS-related lawsuits declined approximately 7 percent in the first half of 2025, but grant rates for preliminary injunctions remain above 73 percent (Conselho

Nacional de Justiça & Programa das Nações Unidas para o Desenvolvimento, 2025). *[Sample boundary]* Our main analysis focuses on the pre-reform period; the 2024 changes define a natural boundary for our sample.

B Data Construction Details

This appendix describes the construction of the litigation, spending, and individual-linkage datasets used in the analysis.

B.1 TPU Code Curation

We identify health-related cases in the DataJud national case repository using Tabela Processual Unificada (TPU) subject codes. The procedure is:

1. Start from the “Direito Administrativo e Outras Matérias de Direito Público” branch, “Saúde” subtree (codes 10064, 10069, and descendants).
2. Include: “Fornecimento de Medicamentos” (supply of medicines), “Tratamento Médico-Hospitalar e/ou Fornecimento de Medicamentos” (medical-hospital treatment and/or supply of medicines), “Leitos” (hospital beds), “Unidade de Tratamento Intensivo (UTI)” (ICU), and related subcategories.
3. Exclude: “Ressarcimento ao SUS” (reimbursement to SUS) and “Contratos e Licitações” (contracts and procurement), which involve disputes between government and providers rather than patient claims.

B.2 Three-Source Cross-Validation

Our litigation data draw on three complementary sources:

1. **DataJud** (2020–present): structured metadata—case number, court, filing date, subject codes, parties, judge—for all cases in the Brazilian judiciary.
2. **Diário de Justiça Eletrônico** (2007–present): official gazette of TJSP and TRF3, publishing decisions, orders, and procedural events.
3. **Consulta de Julgados** (2014–present, TJSP only): full-text decisions from which we extract treatment requested, diagnosis, and grant/deny outcomes.

We merge across sources using the unique CNJ-format case number. When sources overlap and disagree, we apply the following precedence rule: (i) for judge identity and filing date, DataJud takes precedence as the authoritative structured registry; (ii) for decision outcomes (grant/deny, treatment details), Consulta de Julgados takes precedence because it provides the full-text decision; (iii) for procedural events (hearing dates, interim orders), the gazette takes precedence as the official record of publication. Conflicts are rare (fewer than 2 percent of overlapping cases) and concentrated in filing-date discrepancies of one to two business days.

B.3 Judicial-Compliance Descriptors

We identify court-mandated expenditures in the TCE-SP budget data by searching the transaction history (*histórico*) field for the following descrip-

tors, applied case-insensitively:

- *determinação judicial* (judicial determination)
- *tutela antecipada* (preliminary injunction)
- *cumprimento de sentença* (compliance with judgment)
- *ação judicial* (judicial action / lawsuit)
- *mandado de segurança* (writ of mandamus)
- *ordem judicial* (court order)
- *decisão judicial* (judicial decision)
- *processo judicial* (judicial proceeding)
- *liminar* (injunction)
- *obrigação de fazer* (obligation to act)
- *tutela de urgência* (emergency relief)

Some descriptors—particularly *ação judicial* and *processo judicial*—are broad enough to match non-compliance contexts (e.g., expenditures related to lawsuits *filed by* the municipality). To guard against false positives, we restrict matches to expenditures classified under the Health function and exclude entries where the municipality appears as the plaintiff rather than the defendant.

We validate the text-based identification in two ways: (i) comparing total tagged judicial spending per municipality against spending estimates derived

independently from court records (case-level treatment costs), and (ii) manual audit of a random sample of 500 tagged transactions, with the audit explicitly checking whether broad-string matches (e.g., *ação judicial*) correctly identify compliance spending.

B.4 Name-Linkage Procedure

Baseline procedure. We link individuals appearing in health lawsuits to RAIS and CadÚnico using unique full names (first name, all middle names, last name). To minimize false matches, we restrict to verified unique names: individuals whose full name appears exactly once in both the litigation records and the target registry within the relevant state and year. We exclude names appearing more than once in either source. This conservative approach sacrifices coverage for precision.

Sensitivity variants. We assess robustness to two alternative linkage definitions: (i) fuzzy name matching using Jaro-Winkler similarity above 0.95, which accommodates minor spelling variations and accent differences; and (ii) exact name matching augmented with date of birth where available, which permits matching of more common names at the cost of restricting to cases where birth dates are recorded. We report match rates and outcome estimates under all three definitions.

Selection and representativeness. The unique-name restriction creates selection: matched litigants are likely to have rarer names, which may correlate with socioeconomic status, ethnicity, or region of origin. To assess repre-

sentativeness, we compare observable characteristics (case type, court, filing year, grant rate, municipality of origin) between matched and unmatched litigants. We also compare the distribution of municipality-level covariates for municipalities with high versus low match rates. If matched litigants are systematically different, we report how this affects the interpretation of the distributional analysis.

C Supply-Side Case Studies

This appendix documents supply-side mechanisms discussed in Section 8.4. Each case study identifies the mechanism, the key evidence, and the testable implication for our empirical design.

Referral networks and intermediary organizations. *Mechanism:* Physicians prescribe specific brand-name drugs—sometimes under commercial arrangements with manufacturers—and refer patients to specialized law firms that file standardized petitions. Patient associations, often funded by pharmaceutical companies, recruit litigants and connect them with legal representation (Soares & Deprá, 2012). *Evidence:* Machado et al. (2011) document that in Minas Gerais (1999–2009), one physician from a single private law office accounted for 44 percent of prescriptions in 117 judicial actions for a specific biologic drug. Concentration was highest for expensive medications not yet incorporated into SUS. *Testable implication:* If supply-side networks generate the marginal cases, high-volume-lawyer cases should show weaker health returns than low-volume or Defensoria cases.

Operação Garra Rufa (2008). *Mechanism:* Organized fraud networks fabricate lawsuits using cooperating physicians, patient associations, and specialized attorneys. *Evidence:* A police investigation uncovered a network involving three pharmaceutical laboratories, a patient association, doctors, and lawyers that had obtained R\$63 million from the state of São Paulo through fabricated health lawsuits—many filed without the knowledge of the nominal plaintiffs. *Testable implication:* Cases routed through a small number of attorneys are more likely to be supply-driven; our lawyer-concentration split captures this margin.

Eculizumab and Operação Cálice de Hígia. *Mechanism:* Geographic forum shopping and diagnostic fraud enable concentrated litigation for a single high-cost drug. *Evidence:* Eculizumab cost SUS R\$2.44 billion over eleven years. A single law firm filed approximately 70 percent of 514 federal actions, with 73 percent originating in the Federal District rather than where patients resided; roughly half of patients lacked confirmed diagnostic evidence (Folha de S.Paulo, 2020). After police intervention in 2017, judicial purchases fell by 49.5 percent and the drug’s price dropped 35 percent after ANVISA registration. *Testable implication:* Single-firm dominance is captured by our lawyer-concentration split; we document geographic concentration descriptively as suggestive evidence of supply-side organization.

Strategic interaction with CONITEC. *Mechanism:* Manufacturers with weak clinical evidence bypass the health technology assessment process, preferring courts where grant rates exceed 80 percent. *Evidence:* Among 514

incorporation requests to CONITEC (2012–2020), pharmaceutical companies submitted 37 percent of requests and contributed over 70 percent of public consultation responses (Tamachiro et al., 2022). When manufacturers anticipate rejection, some deliberately avoid requesting CONITEC evaluation; the absence of a formal negative recommendation facilitates the judicial route.

Testable implication: We document descriptively whether drugs that bypassed CONITEC are overrepresented among court-mandated purchases; if so, this is consistent with supply-driven cases having weaker clinical justification.