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Does Increasing Public Spending in Health Improve Health? Lessons from a Constitutional Reform in Brazil

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There is surprisingly scarce evidence regarding the extent to which and how government health expenditure affects health outcomes. Exploiting variation generated by Brazil's 29th Constitutional Amendment, which mandated minimum thresholds for municipal spending on health, we examine the chain connecting government health spending to health inputs, production and outcomes, with a focus on infant mortality. We find relatively low average elasticities, but relevant heterogeneity in spending returns. Reductions in infant mortality are greater where baseline spending was lower, pointing to concave returns; where investments in infrastructure and personnel were complementary; and particularly where strong institutional and public management capabilities exist.

KEYWORDS

Health spending; healthcare provision; health outcomes; state capacity

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¿Aumentar el Gasto Público en Salud Mejora la Salud? Lecciones de una Reforma Constitucional en Brasil

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Existe sorprendentemente poca evidencia sobre cómo y en qué medida el gasto gubernamental en salud impacta los resultados en salud. Utilizando la variación generada por la Enmienda Constitucional 29 de Brasil que impuso umbrales mínimos de gasto municipal en salud, este estudio examina la cadena que conecta el gasto público en salud con los insumos, la producción y los resultados de salud, enfocándose en la mortalidad infantil. Los resultados muestran elasticidades de gasto relativamente bajas en promedio, pero con heterogeneidad en los rendimientos del gasto. La reducción de la mortalidad infantil es mayor en lugares donde el gasto inicial era bajo, donde las inversiones en infraestructura y personal fueron complementarias, y donde existen capacidades institucionales y de gestión pública sólidas.

KEYWORDS

Decentralización, gasto en salud, gasto público, gobernanza local, provisión de servicios de salud

1 | INTRODUCTION

Global spending on health has more than doubled in real terms since the turn of the century, reaching US$ 8.5 trillion in 2019, or 9.8% of global GDP, and is projected to exceed US$24 trillion by 2040 (WHO, 2021; Dieleman et al., 2017). Most of this growth has been funded by public sources. Half a century ago, government health expenditure as a share of GDP was under 3% in OECD countries, and now ranges between 7% and 10% in most cases (OECD Stat, 2022). Government health expenditure as a share of total health spending is currently estimated to be 60% worldwide, ranging from around 42% in lower-middle and upper-middle income countries, to nearly 70% in high-income countries (WHO, 2022). Despite these figures, there is surprisingly scarce causal evidence regarding the extent to which and how government health expenditure can effectively improve health outcomes.

In this paper, we assess whether and how a public spending reform in Brazil which resulted in sharp increases in health spending in certain municipalities translates into micro-level improvements in health. To do so, we examine several factors along the chain connecting government health spending to health outcomes, across municipalities and over time. We assess how municipalities allocate resources when increasing health spending, and how expenditures translate into health inputs (such as health infrastructure and human resources), outputs (such as production of primary care services) and improved health outcomes, with a focus (though not exclusively) on infant health. Importantly, our empirical setting allows us to examine non-linearities and input complementarities within local health production functions, particularly assessing whether government capacity and other institutional factors affect the returns on spending.

We combine many sources of administrative microdata and leverage the variation in municipal health spending generated by Brazil's 29th Constitutional Amendment (EC/29). Enacted by the Federal Congress in September 2000, the EC/29 mandated that municipalities spend at least 15% of their own revenues on health care. This induced an increase in public spending for most municipalities in the years which followed. We use this variation to identify spending effects in an event study design that relies on the distance to the minimum spending threshold when EC/29 is enacted, conditional on municipality and state-year fixed effects, robust to a range of controls. In Brazil, the public health system is decentralized and municipalities are autonomous in choosing how to allocate their own funds. Our natural experiment then can be thought of as comparing changes in health spending, inputs, outputs, and outcomes across municipalities along the baseline distribution of the distance to the arbitrary spending target, while flexibly capturing all municipal and state-by-time invariant unobservables. A series of robustness checks shows that pre-trends in observables are uncorrelated with the distance to the target, and that changes in public spending are specifically related to changes in health care spending.

We show that the reform promoted substantial increases in health spending for municipalities below the target at baseline. Increases in spending took place mainly through administrative spending, human resources, and investment in physical resources, which in turn were translated into greater supply of personnel, health infrastructure, and expansions in primary care services. The shift in health inputs and outputs led to reductions in infant mortality rates, in particular for deaths during the neonatal period. Yet, average elasticities range from close to 0 in the immediate aftermath of the reform, to -0.2 ten years following the reform. These figures are lower than existing estimates. Importantly, the reform also induced a contraction in spending in municipalities that were above the target at baseline, but this came without adverse measurable consequences for studied health outcomes. These municipalities managed to reduce spending, at the cost of reducing inputs, however without substantially affecting access to health nor production outputs.

Despite the relatively low average elasticities, we find relevant heterogeneity in spending returns. We observe concave returns to spending, and complementarities in health production factors, with greater reductions in infant mortality where investments in infrastructure and personnel complemented each other, particularly when spending prioritized factors with low baseline coverage. This "low-hanging fruit" phenomenon aligns with correlational patterns seen in Preston curves (Preston, 1975), suggesting that concave movements along the curve can be in part attributed to concave spending returns as income per capita and spending increase. Additionally, the most significant gradients are found in political and management capacity measures. Provided similar resource allocations, effects are eroded in high-corruption areas but substantially enhanced in regions with strong management and institutional capabilities. This supports concerns about corruption (Ferraz and Finan, 2011) and highlights the importance of management practices at the health system level, and beyond the hospital level (Bloom et al., 2015; Hollingsworth et al., 2024; Muñoz and Otero, 2023).

We observe that the expansion of municipal services in municipalities below the target at baseline was complemented by an expansion in private services during the initial increase of pubic spending, which is consistent with an increase in contracting out of services in the profit and not-for-profit sectors. We also observe an expansion in private services in municipalities above the target at baseline, where spending was reduced and the supply of municipal hospitals decreased. Yet, that expansion is not significant in the first years after the reform, and we do not observe changes in private insurance coverage. As production outputs remained stable in these municipalities, the stability in mortality rates after spending cuts may have been partially sustained by efficiency gains in the public sector.

Finally, improvements in infant mortality are not achieved at the cost of other population groups. While we do observe declines in adult hospitalization rates driven by causes amenable to primary care where spending increased, we find weak evidence suggestive of mortality declines at older ages. Spending expansions appear not to generate congestion effects, but rather, appear to slightly increase rates of individuals referred for hospitalization in other municipalities for causes which are not amenable to primary care, suggestive of improved referral to higher complexity care providers.

Empirical evidence on the relationship between health spending and health outcomes is generally unsettled and depends on the links analyzed within the chain connecting variation in spending to changes in access to health care, service utilization and health outcomes. A range of prior studies shows that increases in spending which lead to greater utilization of certain types of care have substantive returns at the patient level. For example, Almond et al. (2010) document declines in infant mortality owing to increases in spending and treatment around birth weight cut-offs, while Cutler et al. (1998) and Cutler (2007) document cost-effective investments in cardiovascular care. Doyle et al. (2015) find that in the US spending at the hospital level can have substantial impacts on health outcomes, while Gruber et al. (2014) document that increased funding to hospitals in Thailand improved access to healthcare among the poor and their health outcomes. However, higher spending may not necessarily translate into better health outcomes if access to health care and service utilization are not well targeted. For example, while evidence from developing countries indicates improved health outcomes among the poor following expansion in specific health insurance schemes (e.g. Miller et al., 2013; Camacho and Conover, 2013; Bernal et al., 2017; Conti and Ginja, 2023), influential results from the RAND Health Insurance Experiment suggest that reductions in individual co-pays boost health spending without significant improvements in health on average (Manning et al., 1987).[1]

Understanding the production function of health care is challenging, as it involves, *inter alia*, the hiring and retention of health workers (Custer et al., 1990; Okeke, 2023), the procurement and dispensation of drugs (Américo and Rocha, 2020), the construction and maintenance of infrastructure (Auster et al., 1969; Mora-García et al., 2023), management of hospitals and health systems (Bloom et al., 2014), as well as navigating interactions with health-seeking behaviour (Lleras-Muney, 2005), physician and provider incentives (Clemens and Gottlieb, 2014; Batty and Ippolito, 2017), and political economy factors (Bhalotra et al., 2023). What these factors have in common is that at least in theory they are amenable to be modified by spending.[2] Yet, information failures and the interactive nature of health care production functions may constrain health spending returns.[3]

Specifically related to government expenditure, precedents in political economy suggest that following the effects through the links connecting public spending to health outcomes may be even more challenging. While government health expenditure can take multiple routes, depending on the health care system model adopted, this question is particularly relevant for countries where the state either owns or controls the factors of health production, and where government failures exist.[4] Although research isolating the specific connections between government health expenditures and health outcomes is scant, evidence from oil shocks and fiscal windfalls in Brazil, for instance, suggests that large shocks in available resources led to small or null impacts on social spending, with considerable waste owing to patronage and embezzlement (Caselli and Michaels, 2013; Monteiro and Ferraz, 2010). Particularly worrying are cases where such transfers can lead to deterioration in the quality of political leaders and corruption once politicians can extract political rents in manners which are not transparent to voters (Brollo et al., 2013). More generally, if spending increases are diverted due to corruption, no change in inputs may even be observed to impact health outputs (Gupta et al., 2001). Government health expenditure may therefore impact final health outcomes, but should any individual step from changes in health spending to health inputs to health outputs break down, spending will not necessarily lead to improvements in health.

Previous research has documented the direct relationship between health spending (at the baseline) and population health outcomes (at the end line), but most studies focus on

[1] Results from the Oregon HIE which consider expansions in Medicare again point to large increases in medical care usage (Taubman et al., 2014), but relatively weak impacts on health outcomes. Finkelstein et al. (2012) find positive impacts on mental health and self-reported physical health, but a two-year follow up from Baicker et al. (2013) with clinical measures of health finds much weaker impacts.

[2] For instance, higher salaries may attract medical workers and boost primary care coverage, often at relatively low cost (Banke-Thomas et al., 2020), costly information campaigns can shape health-seeking behaviour and health outcomes (e.g. Hinde et al., 2015), increased competition owing to more health facilities can improve management practices in health care (Bloom et al., 2015) with knock-on effects to health outcomes (Gaynor et al., 2013).

[3] Such structures are well-known in microeconomic theory as represented by Stone-Geary style production functions, where inputs at certain margins may lead to no change in outputs given required minimum thresholds. For example, greater spending on technology or infrastructure will have no impact on outputs if trained healthcare personnel are not available to operate or staff newly acquired inputs, and systems will generally perform poorly if absenteeism is high (Banerjee et al., 2008). Similarly, increases in hospital budgets may have minimal returns if hospitals are poorly managed, or spend inefficiently (Baicker and Chandra, 2011; Baicker et al., 2012; Chandra and Staiger, 2016).

[4] Government health expenditure generally covers direct spending on provision or subsidized insurance. On average, direct spending has corresponded to more than half total government health expenditure in both high- and upper-middle income countries (WHO, 2022).

total health spending, usually estimate cross-country relationships and cannot account for unobserved heterogeneity.[5] Some of the identification issues faced by earlier studies have been partially addressed by the use of fixed effects in micro-level studies. On the evidence from government subsidized insurance care models, for instance, studies considering large differentials in Medicaid spending across US regions suggest that wide variation in spending at this level is not associated with improvements in population health outcomes (Fisher et al., 2003a; Skinner et al., 2008; Baicker and Chandra, 2004; Cutler et al., 2019), despite the fact that higher-spending regions provide substantially more healthcare inputs (Fisher et al., 2003b).[6] On the evidence from direct government spending, and closer to the case of Brazil, Crémieux et al. (1999a) use a panel of Canadian provinces and find that increases in government health expenditure are associated with decreases in infant mortality. Bhalotra (2007) explores cross state variation in health spending in India, and does not find any contemporaneous effects on infant mortality, but small long-term impacts for rural residents. Castro et al. (2019) find that greater receipt of federal health transfers correlate with improvements in infant health in a panel of municipalities in Brazil. While these studies move towards capturing a number of time and area-invariant unobservables, local governments can endogenously choose whether and when to adopt any spending policies or adjust spending to respond to poor health outcomes. Moreover, the causal chain linking spending to health outcomes has been overlooked.[7]

A common thread in the existing literature is therefore that health spending may be sufficient to impact health outcomes at certain margins. However, this is certainly not a foretold conclusion and identification concerns remain. This points to the importance of collecting new empirical evidence. We take this forward here. The first main contribution of this paper lies not only in providing one of the first well-identified causal parameters on the relationship between government health spending and health outcomes, but also in assessing the links from spending to outcomes through a comprehensive chain of causation propagated within local health systems, covering decisions on public spending and potential responses by the private health system, health inputs, production outputs and health outcomes. Our second main contribution lies in uncovering non-linearities and input complementarities within the production function of public healthcare, thus allowing us to map margins of spending effectiveness, its potential mediators and constraints. This exercise goes beyond the assessment of the role of typical supply-side health production factors, such as physical and human resources. We empirically document significant gradients in spending returns due to institutional and management capacity measures. Overall, our results suggest that improving government management capacity and local governance, alongside granting local discretion in fund allocation, appears to be an effective strategy for decentralized public service delivery.

Finally, while there is dense literature on whether and how health outcomes are affected by specific medical treatments, health inputs, insurance schemes and policies, which by design may involve additional funding, these interventions are often tightly tailored and designed to impact specific diseases or the coverage of specific services and population

[5] See, for instance, cross-country studies in Filmer and Pritchett (1999), Gupta et al. (2002), Nixon and Ulmann (2006), and Bokhari et al. (2007). Results are in general sensitive to robustness checks (Nakamura et al., 2020).

[6] There are, however, suggestions that this may owe to endogeneity. In an analysis of individuals who have an emergency when visiting areas away from their home, health outcomes are observed to be better when this event occurs in higher spending areas (Doyle, 2011).

[7] A related stream of research examines returns to healthcare spending at different spending levels, with elasticities generally estimated using fixed effect models or 2SLS models based on demanding exclusion restrictions and imperfect IVs, such as endogenous socioeconomic characteristics or general macroeconomic shocks (e.g. Claxton et al., 2015; Vallejo-Torres et al., 2018; Edoka and Stacey, 2020; Moler-Zapata et al., 2022).

groups. We exploit a unique setting where a top-down social choice, enacted in the Federal Constitution, mandated thousands of autonomous local health systems to increase spending in health. We track local choices to outcomes at an unprecedented level.

The remainder of this article is organized as follows. Section 2 describes the institutional background. Section 3 describes the data, while in Section 4 we lay out our empirical strategy. Main results are presented in Section 5. In Section 6 we discuss mechanisms and additional robustness checks. In Section 7 we examine heterogeneity in spending returns. Section 8 concludes.

2 | BACKGROUND

The Brazilian Federal Constitution, enacted in 1988, established universal and egalitarian access to health care as a constitutional right, and the Unified Health System (Sistema Único de Saúde, or SUS) was created to provide health care to all citizens, free at the point of use and funded out of general taxation. SUS therefore comes closer to a national health service model, where the provision of health care services is administered by the state, which either directly owns or contracts out the factors of production and delivery to the private and philanthropic sectors. As also established in the Federal Constitution, Brazil follows a federalist political system organized in three administrative levels—the federal government, states and municipalities. The funding, the delivery of services and the implementation of health policies within SUS are decentralized, with states and municipalities playing a relevant role in the financing and in the provision of health care. Municipalities in particular cover nearly a third of total government spending in health, with a substantial level of autonomy in the allocation of resources.

Although Brazil established a national health service to cover the entire population, the fiscal space to meet the constitutional rights remained limited, and SUS remained chronically underfunded (Piola et al., 2013).[8] The 29th Constitutional Amendment (hereafter EC/29) was therefore enacted to secure resources for SUS. The proposal was approved by the Lower House in November of 1999, and sent to the Upper House, where it was approved in September of 2000.

2.1 | The 29th Constitutional Amendment

The EC/29 established a minimum spending floor for the provision of health care that each government level was required to meet. According to the amendment, in 2000 the Federal Government should increase spending by 5% above the amount spent in 1999, and then this value should increase at the rate of the GDP growth from 2000 to 2004. States should spend at least 12% of their tax income net of transfers to municipal governments, and municipalities should spend at least 15% of their own resources, which include municipal tax income. States and municipalities spending less than the thresholds would have to gradually increase expenditure in health, reducing the distance to the target by at least one fifth per year, and spending annually at least 7% of their tax income.[9] Importantly,

[8] While health expenditures as a share of GDP has been relatively higher in Brazil in comparison to upper-middle-income countries, the share of public spending in total health expenditure is relatively lower. Private spending has remained above 55% of total health expenditure, while around 25% of the Brazilian population have private insurance plans (Rocha et al., 2021).

[9] The EC/29 established the shares of resources that governments needed to spend throughout the following years until 2004, and that a Complementary Law should be designed and approved to regulate thresholds

the EC/29 did not explicitly regulate how governments should spend the resources, thus providing autonomy for government entities to allocate their funds.

2.2 | The EC/29 and Changes in Municipal Health Expenditures

Figure 1a shows the distribution of municipalities according to their share of own resources spent in health care. While in 2000, our baseline year, most municipalities spent less than 15% of their revenues in health care, in 2005 nearly all complied with that minimum threshold. Figure 1b shows that the distribution of the municipal health spending per capita (in 2010 R$) also moved accordingly.

(a) Health Spending (% of own resources) (b) Health Spending per capita (2010 R$)

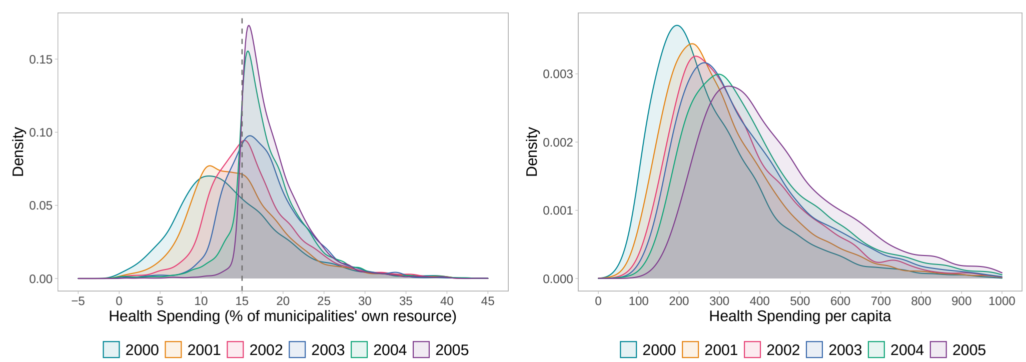


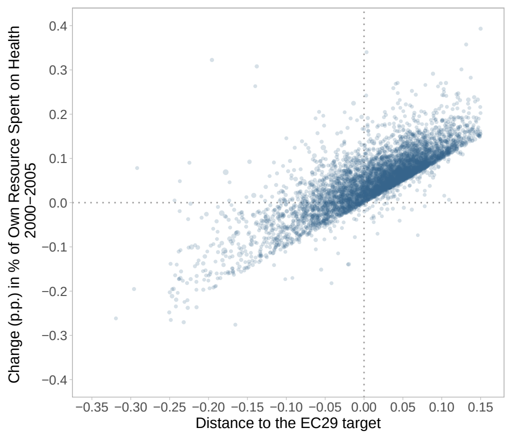
FIGURE 1 Spending Density Plots. *Notes*: Density plots calculated using SIOPS data (see Section 3 for more details). Dotted line in Figure 1a marks the EC/29 target.

Appendix Figure A.1 presents trends in health spending at the municipality level converted into indices set equal to 100 in 2000, for the bottom and top quartiles of the distribution of the share of own resources spent on health care. Municipalities in the bottom of the distribution experienced a much greater increase in health spending relative to the municipalities on the top of the distribution. Moreover, expenditures funded by own resources explain almost the entire difference in spending increase between the bottom and the top quartiles.

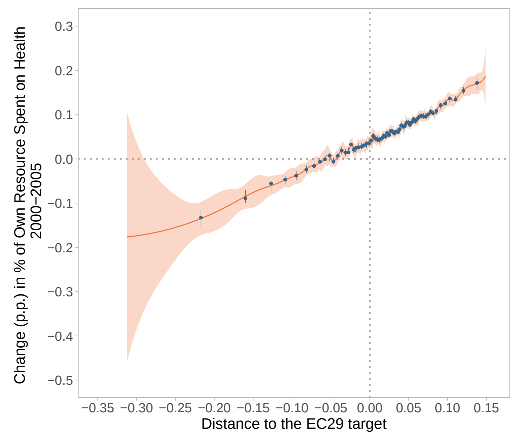
As expected, the baseline share of own revenues spent in health care is predictive of the change in municipal health spending per capita. Figure 2a plots, for all municipalities, the distance in percentage points to the EC/29 target versus the change in the share of own revenues spent in health between 2000 and 2005. Figure 2c does the same, but looks at the change in spending per capita. Consistent with Appendix Figure A.1, we observe that increases in health spending were greater in places with initially low levels of spending. Appendix Figure A.2 indicates substantial spatial variation both across and within states in the share of own resources spent on health in the baseline year. Also importantly, Appendix Figure A.3 documents no clear relationship in correlations between baseline spending and pre-reform evolution in a range of municipal socioeconomic characteristics. In Section 5 we provide further details on the fiscal response of municipalities to the EC/29 in terms of revenue collection and spending by type of expenditure and government sector. In general, the descriptive evidence indicates that the EC/29 was responsible for bringing

from 2005 onwards. In the a absence of a Complementary Law, the share of resources defined by EC/29 would apply. The Complementary Law was only approved in 2012, but it made no changes to the thresholds.

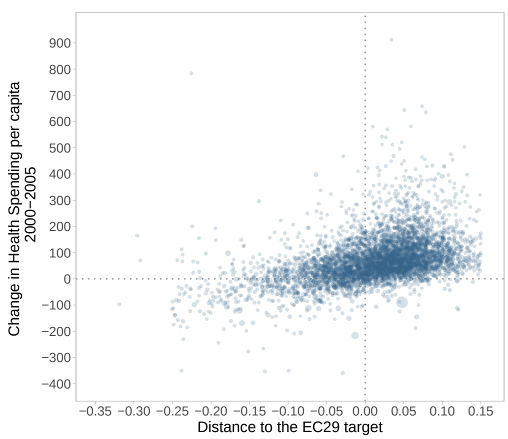
(a) Shifts in % of Own Resource Spent on Health



(b) Shifts in % of Own Resource Spent (Binscatter)



(c) Shifts in Health Spending per capita



(d) Shifts in Health Spending per capita (Binscatter)

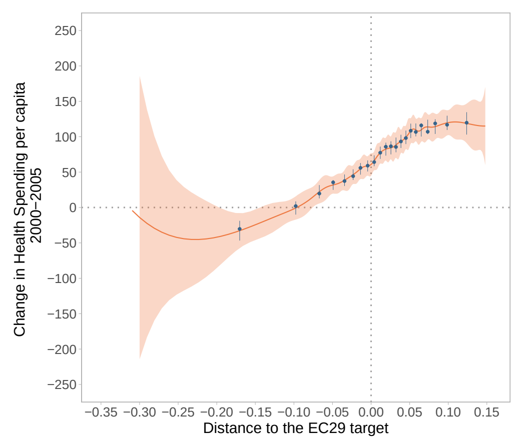


FIGURE 2 Changes in Health Spending (2000-2005). *Notes*: Distance to the EC/29 target is calculated from SIOPS data as target spending (15%) minus actual spending in 2000. Changes in Health Spending per capita calculated using Health and Sanitation spending per capita from FINBRA (see Section 3 for more details of all measures). Dot sizes in panels (a) and (c) are proportional to municipal population; correlations in (a) and (c) are equal to 0.81 and 0.45, respectively. In panels (b) and (d) curves, confidence bands, dots and confidence intervals are estimated following Cattaneo et al. (2019).

more resources into the public provision of health services across the country, and these changes were conducted in a way consistent with the thresholds determined by the Federal Constitution.

3 | DATA

We construct a municipality-by-year panel of data, covering 5,224 Brazilian municipalities over the period of 1998-2010.[10] Appendix Table A.1 describes the main data and their sources, and also presents summary statistics at the baseline year for all variables used in the analysis. We provide details below.

3.1 | EC/29 and Fiscal Data

We combine data on public spending from the Brazilian Finance System (FINBRA), which covers the 1998-2010 period, with data from the Brazilian National System of the Public Health Budget (Datasus/SIOPS), available from 2000 onward.[11] FINBRA provides data on total public spending, and spending by a number aggregated categories, such as Health and Sanitation, Education and Culture, as well as data on public revenues. SIOPS provides more detailed information on public spending in health care, and allows us to observe how municipalities allocate resources within the health sector. The system gathers data on total health spending and spending by source of funding (from own resources or intergovernmental transfers), and by type of spending (on human resources, investments, services from third parties, and others, which mainly include administrative spending). Moreover, SIOPS calculates for each municipality the share of own resources spent on the provision of health care, which is used to define our variable of interest. While SIOPS has richer data and is our preferred measure of health spending given that it separates health spending from sanitation, the system was created in the immediate aftermath of the EC/29 reform precisely to monitor revenues and expenditure in the provision of health care at the state and municipal levels, and to monitor compliance with the EC/29. Given this, we consider measures from FINBRA to observe pre-reform figures. In analyses of reform impacts on health spending, we remove a small number of municipalities which are outliers in terms of per capita health spending, defined as spending more than 5 standard deviation above the mean in per-capita terms. This results in the removal of 40 municipalities from these analyses (<1%), though we note that results are not sensitive to this choice.

3.2 | Infant Mortality and Birth Outcomes

We use microdata from the Brazilian National System of Mortality Records (Datasus/SIM) and from the from Brazilian National System of Birth Records (Datasus/SINASC) to construct infant mortality rates (IMR). Infant mortality is measured as deaths per 1,000 live births, and microdata from SIM additionally allows us to generate measures of infant mortality by timing and cause of death. We use the classification from Alfradique et al. (2009) to classify deaths as amenable to primary care and non-amenable to primary care, and calculate IMR for each case.

Infant mortality data from Brazil are generally recognized as being of high quality. For

[10] Brazil has 5,570 municipalities. Our sample excludes the relatively small number of municipalities which did not have fiscal records in the National System of the Public Health Budget and those for which information on health spending at baseline was not recorded, as for these municipalities we are unable to determine their exposure to the EC/29 reform given its dependence on baseline health spending.

[11] All spending values are presented in 2010 R$. We used the General Price Index (IGP-M/FGV) to adjust nominal values. For interests of comparison, in early 2000 1 USD bought approximately 2 BRL. In the period under study, the exchange rate between USD/BRL varied from anywhere between 1.1 to 3.8 BRL/USD.

example, Mikkelsen et al. (2015) classify Brazilian vital statistics registers as "high quality" for the entire period under study. Lima and Queiroz (2014) and França et al. (2020) suggest that more than 95% of deaths are captured in administrative data. Nevertheless there are concerns that infant mortality may be under-reported early in our study period and, in particular, that the quality of the classification by cause of death may have increased over time (França et al., 2020). Further discussion of measurement and data quality can be found in Appendix B.2. We consider sensitivity to the inclusion of controls for potential changes in data quality (discussed in Section 4), and to the inclusion of additional years before our period of analysis for further inspection of data quality and pre-trends.

3.3 | Health Inputs and Service Production

We combine data from several sources to build a data set on health inputs and service production. First, we collect data on primary care coverage and production of services from the Brazilian National System of Information on Primary Care (Datasus/SIAB). Data on human resources and infrastructure come from the 1999, 2002, 2005 and 2009 Medical-Sanitary Assistance Survey (AMS), a census of the health sector conducted by the Brazilian Institute of Geography and Statistics (IBGE). Health infrastructure refers to the number of hospitals – municipal, state, federal and private hospitals – as this information can be harmonized since 1999. Municipal hospitals are often small-scale facilities, with less than 50 beds, typically resembling medical polyclinics that also provide inpatient services (Carpanez and Malik, 2021).

The Brazilian National System of Information on Ambulatory Care (Datasus/SIA) covers every ambulatory procedure funded by the SUS, with information on the type and complexity of the procedure, the health professional who delivered it, and the corresponding health facility identification number. These data are used to create variables on total ambulatory production, primary care ambulatory production, and production by procedure complexity.[12]

To measure access to health services, we use data from the from Brazilian National System of Birth Records (Datasus/SINASC), which records every birth in Brazil and provides detailed information on birth outcomes. From these data we calculate the share of live births from resident mothers that did not have any prenatal visits, or had 1-6 or more than 7 prenatal visits during the gestational period. Lastly, we collect data on hospitalizations from the National System of Information on Hospitalizations (Datasus/SIH), which provides administrative records of all hospital admissions funded by SUS with detailed information on the cause of hospitalization. We once again split hospitalizations into admissions for causes that are amenable and not amenable to primary care services.

Given the range of variables available to measure health care inputs and access, we construct indices to broadly measure (a) access and production of health services; and (b) health inputs. The use of these indices avoids concerns related to inflated type-I error rates owing to multiple hypotheses testing (see e.g. Romano et al., 2010), and are generated following Anderson (2008).[13] While we work with these two principal indices when considering mechanisms, we additionally further break these down into two sub-indices of

[12] We also use these data to indirectly create variables that measure the supply of ambulatory facilities. This is done by computing the number of facilities within a municipality that recorded a given procedure, by type of procedure and professional who delivered it. We are able to construct these variables only for the period of 1998 to 2007, as changes in the SIA classification of ambulatory procedures changed in 2008.

[13] Specifically, these indices are constructed by consistently re-scaling variables so that more positive values imply "better" results from health policy point of view, and then aggregating outcomes into a single stan-

primary care access and production and non-primary care access and production (when considering access and production), and human resources and number of hospitals (when considering health inputs). The precise definition of the variables which make up each index are provided in Appendix Table A.2.

3.4 | Other Outcome Measures

In considering potential reform spillovers and broader reform effects we draw on a number of other measures. This includes the population coverage of private insurance, compiled from the National Agency of Supplementary Health (ANS), as well as the number of non-municipally financed hospitals measured in the AMS census and described above. To capture potential spillovers across municipal borders, we calculate hospital inflows and outflows as rates of individuals who are hospitalized in a given municipality, but reside in a different municipality, and rates of individuals who live in a municipality, but seek hospitalization in another municipality. These measures are drawn from Datasus/SIH. Finally, we use yearly data on mortality from Datasus/SIM, and on population by age and sex also from Datasus to calculate adult mortality rates.

3.5 | Controls and Municipal Baseline Measures

Our control variables can be classified into three different categories: baseline socioeconomic controls, time-varying socioeconomic controls, and time-varying fiscal controls. The first comes from the 2000 Population Census (IBGE) and will be used to construct municipality time trends. Our time-varying socioeconomic controls include GDP per capita, also from IBGE, and *Bolsa Família* cash transfers per capita, from the Ministry of Social Development. The last set of controls comes from FINBRA. We use as fiscal controls the average health spending per capita in the bordering municipalities and, in additional specifications, the share of total current public revenue spent on personnel. The precise set of controls in each of these groups is documented under 'Controls' in Appendix Table A.1. As we discuss below, we consistently consider results both with and without controls.

We also use a range of measures when considering gradients in the impact of EC/29. We use baseline municipal characteristics (income per capita, the proportion of individuals living in poverty, living in urban areas, the Gini index, and population density), measured from the 2000 census. We also include political measures, namely the mayor's margin of victory in the last election prior to the passage of EC/29 (2000), measures of the mayor and municipal council members' education level, and an indicator for whether municipalities have a formal government planning project. Data on elections is published by the Brazil Superior Electoral Court, and data on mayor and Councillors' education, as well as government planning are collected from a nationwide municipal survey conducted during the period 2002-2003 by IBGE (Munic 2003).

Finally, we collect information on municipal-level management capacity and measures of corruption. Management capacity is measured using a custom-designed instrument applied by the Ministry of Planning and Budget (the Municipal Institutional Quality Indicator, or IQIM) with the stated aim of capturing local management capacity and institutional quality (see e.g. Pereira et al., 2011; Brassiolo et al., 2024). Our measure of corruption is drawn

dardized summary index, where each measure is weighted by the inverse of the variance-covariance matrix among all variables in the index. The indices are all standardized such that parameter estimates can be cast in terms of standard deviations.

from randomly assigned audits occurring in municipalities across Brazil. These audits are conducted by the *Corregedoria Geral da União* (CGU), resulting in reports indicating any municipal irregularities in the use of federal resources, and have been used in a broader literature on corruption (e.g. Brollo et al., 2013; Ferraz and Finan, 2011). Unlike other variables which are recorded at baseline, given the timing of corruption audits, this measure is recorded only after the passage of EC/29. It covers indication of corruption related to the use of federal funds up to the audit year, and can therefore include the period before the passage of the reform. We interpret this measure as detecting latent presence of corruption in the locality. Descriptive statistics for each measure are provided in Table A.1.

4 | EMPIRICAL APPROACH

4.1 | Main Empirical Specifications

We estimate the effects of the EC/29 using an event study design with a continuous treatment measure, exploiting variation in exposure to the reform owing to baseline municipal spending proportions, interacted with the time-specific adoption of the EC/29 approval. More specifically, we estimate effects based on two empirical models. The first specification follows the equation below:

$$Y_{mts} = \alpha + \sum_{i=2}^{I} \beta_{pre,i}\, Dist_{m,pre} \times EC29_{t-i} + \sum_{j=0}^{J} \beta_{post,j}\, Dist_{m,pre} \times EC29_{t+j} + \delta_{st} + \mu_m + \theta\, Z_{m,pre} \times \lambda_t + \gamma\, X_{mts} + \varepsilon_{mts} \quad (1)$$

Here Y_{mts} is an outcome of interest in municipality m, state s, year t. $Dist_{m,pre}$ is the baseline proportional distance to EC/29 target in municipality m, defined as $Dist_{m,pre} = Target - Spending_{m,pre}$. Thus, if municipalities were spending below the target at baseline, $Dist_{m,pre} > 0$ indicates increase in resources required to meet the target, whereas if municipalities were spending above the target at baseline, then $Dist_{m,pre} < 0$, indicating the decrease in resources possible to still meet the threshold. Both $Target$ and $Spending_{m,pre}$ are recorded as budget proportions, i.e., the target value is recorded as 0.15. Across the support of $Dist_{m,pre}$, higher values therefore imply larger expected changes in health spending. This measure is interacted with indicators capturing time to the passage of EC/29, the terms $EC29_{t+j}$, which are dummies that equal one if the observation year is i years pre- or j years post-reform passage. Fixed effects δ_{st} and μ_m are included to flexibly capture state-year variation in outcomes and time-invariant municipality level factors. The inclusion of state-year fixed effects are particularly relevant, given that the EC/29 also targeted state health expenditure, and that some health policies are decentralized to state governments in Brazil. Here, our models isolate municipal-specific variation in exposure to the reform, identifying effects which owe to changes in municipal spending brought about by EC/29 within states.

In the most saturated specifications we also include time-varying controls. The vector $Z_{m,pre} \times \lambda_t$ includes a measure of data quality, given concerns related to measurement error of health outcomes particularly in earlier periods. This consists of the share of infant deaths classified as "ill-defined" in each municipality at baseline (pre-2000 average) interacted with time, and is included for all outcomes to ensure consistency across models. We also consider an interaction between socioeconomic baseline controls and time (the remainder of the vector $Z_{m,pre} \times \lambda_t$), and time-varying socioeconomic and fiscal controls (the vector X_{mts}), as listed in the previous section.[14] We document results without any time-varying controls, and discuss the stability of estimates to the progressive inclusion of controls. Population weights are consistently used in all estimation. Finally, ε_{mts} is a stochastic error component. Standard errors are clustered at the municipality level.

Our interest in this specification is to inspect dynamic impacts of the reform. A series of parameters indicated $\beta_{pre,i}$ captures evolution between areas with higher and lower reform exposure prior to the reform, while corresponding estimates $\beta_{post,j}$ capture evolution between these areas in the post-reform period. The former allows us to inspect pre-trends in the outcome variable when comparing between municipalities which are further and closer to the spending target, while the latter allows us to evaluate any dynamic impacts through the years following EC/29.

Yet, a key component of the EC/29 reform is that it may imply differential responses by municipalities spending below versus above the 15% threshold at baseline. Municipalities spending less than 15% are obligated to increase spending to meet the target. However, in municipalities spending greater than 15%, health spending as well as other outcomes may have increased, decreased, or remained fixed after the reform. Given the continuous nature of dose treatments in equation (1), if municipalities above the target at baseline also respond to the EC/29 reform, the parameters β_{post} from equation (1) may thus reflect dynamic changes in the group of municipalities below the target relative to the group above the target after the reform. Thus, while estimates from equation (1) are informative of relative responses to EC/29 reform passage, they may obscure potential differential response patterns within each group.

In our second specification, we therefore test for such differential policy responses by stratifying equation (1) by above versus below target municipalities. Specifically, we define $Below_{m,pre} = \mathbb{1}\{Spending_{m,pre} < Target\}$ and $Above_{m,pre} = \mathbb{1}\{Spending_{m,pre} \geqslant Target\}$. Using this binary split, we allow for the response to spending targets to differ for above and below spending-target municipalities by estimating:

$$
\begin{aligned}
Y_{mts} = \alpha + \sum_{j=-J}^{K} \beta_j (|Dist_{m,pre}| \times EC29_{t+j} \times Above_{m,pre}) + \\
\sum_{j=-J}^{K} \gamma_j (|Dist_{m,pre}| \times EC29_{t+j} \times Below_{m,pre}) + \\
\delta_{st} + \mu_m + \theta\, Z_{m,pre} \times \lambda_t + \gamma\, X_{mts} + \varepsilon_{mts} \quad (2)
\end{aligned}
$$

This replicates equation (1), however, for ease of presentation, we take the absolute value of the distance to the target. This transformation allows us to more clearly visualize differential results above and below target.[15] Importantly, parameters are now estimated specifically from variation along the support of baseline spending within each group, irrespective of changes that occur in the other group. All other details in equation (2) follow corresponding definitions in equation (1). In each group of municipalities (above and below target), a full

[14]This includes the rate of spending on personnel, which was capped by the Fiscal Responsibility Law (LRF), as well as average health spending per capita in the neighboring municipalities (see for e.g. Castro et al., 2021). These controls are potentially endogenous to the EC29, and will be used only in auxiliary specifications.

[15]For example, if municipalities which are below the target increase spending, and municipalities which are above the target decrease spending, coefficients will capture this mirrored behavior as a positive value for β and negative value for γ.

set of J pre-event leads and K post-event lags are included, where we consistently omit an indicator one year prior to reform implementation as a baseline reference period. This is thus an interacted event-study following equation (1).

Parameter estimates from equations (1) and (2) along with 95% confidence intervals will be presented graphically. In some cases we will also present tabular estimates based on the single-coefficient version of equation (1) to generate a single summary estimate for reform impacts, namely:

$$Y_{mts} = \alpha + \beta\,(\mathrm{Dist}_{m,pre} \times \mathrm{Post}_t) + \delta_{st} + \mu_m + \theta(Z_{m,pre} \times \lambda_t) + \gamma X_{mts} + \varepsilon_{mts} \quad (3)$$

All details follow those laid out in equation (1), with the exception of the single interaction term based on Post_t, a dummy that equals one if the year is 2001 or later. Such single-coefficient models will complement event-study graphs in auxiliary results where summary estimates are needed.

4.2 | Identification and Validity of the Research Design

Identification relies on the assumption that outcomes would have followed parallel trends across municipalities in the absence of the reform. The first main threat to identification refers to potential non-observable pre-trends that correlate with baseline spending in health, and which would have persisted in the absence of the reform. For instance, while fixed effects should absorb the influence of differences in spending levels as well as of slow-moving determinants of health, other sources of convergence in health spending and population outcomes might still exist, even within states. To examine the relevance of this concern, Appendix Figure A.3 presents a series of plots which correlate the baseline distance to the EC/29 target with changes in municipality socioeconomic characteristics over the 1991-2000 intercensal period, i.e., before the EC/29 reform. These characteristics are typically considered relevant socioeconomic determinants of population health, both at the individual and the family level – such as education, income level and household characteristics. We do not observe any systematic associations between changes in these measures and the baseline distance to the target, which lends support to the parallel trends assumption if pre-reform trends are informative of post-reform trends.

The second main threat to identification has been identified in recent advances in econometric theory, which point to drawbacks in the two way fixed effects regressions frequently used in empirical research based on difference-in-differences designs. Callaway et al. (2024) highlight that difference-in-differences models based on continuous treatment require stronger parallel trends assumptions, as comparisons between different intensities of treatment can also be confounded by selection bias. Unlike standard (binary) models, this bias comes from the heterogeneity in treatment effects. If groups of units have different responses to a certain dosage of treatment, estimates will be contaminated by the differences in expected returns for these different dosage groups. Moreover, this bias persists even under traditional parallel trends assumption. For the estimator to be unbiased, we thus require a stronger parallel trends assumption which in practice implies that treatment effects across different dosage groups would be homogeneous had they received the same specific treatment dosage.[16]

[16]Note however that the typical concerns related to heterogeneity in treatment effects in staggered designs – as discussed by Goodman-Bacon (2021), de Chaisemartin and D'Haultfoeuille (2020), and Callaway and Sant'Anna (2021) – are not an issue in our case as the passage of EC/29 was fixed in time.

We formally define these assumptions, and their implications in our setting, at more length in Appendix D. In practice, we argue that the strong parallel trends assumption is likely reasonable here. As is salient in Figures 2a-2b, the EC/29 spending reform was approximately binding.[17] Thus, if a municipality which was some distance d away from the spending target were actually $d + h$ units away from the spending target, it seems likely that their spending change would have followed that of municipalities which were $d + h$ units away from the spending target, and as such, counterfactuals from these municipalities are reasonable. This is precisely the logic of the strong parallel trends assumption.[18] Callaway et al. (2024) additionally note that the aggregation of unit specific effects in regression models potentially underweights certain units and overweights others based upon the distribution of treatment exposures. For this reason, in robustness checks we consider a re-weighting approach as discussed in Callaway et al. (2024). Additional details are provided in Appendix D.

5 | RESULTS

In this section we first present the estimates of the impact of EC/29 on fiscal outcomes, seeking to understand shifts in municipal spending patterns more broadly, as well as within classes of health spending. We then assess impacts on health outcomes. While we consistently report average causal responses, we also cast effects in terms of a benchmark spending shift. This benchmark considers a variation of 10 percentage points (p.p.) in spending as a result of the EC/29 reform. This is equivalent to the distance to the target for municipalities in the bottom quartile of the distribution of the share of own resources spent in health, which is the group of municipalities that experienced the greatest increase in health spending after the EC/29 was enacted.

5.1 | Municipalities' Fiscal Response to the EC/29

We start by presenting in Table 1 summary single-coefficient estimates of the impact of the spending reform on total public revenue and spending, public spending by category, and public spending on health, by source and type. All outcomes are measured as the natural logarithm of Reais (BRL) per capita. In column 1 we present our baseline estimates from a specification with municipality and state-year fixed effects, and for consistency with later models, a data quality control (we document robustness to control sequences in Section 6.2.4). Column 2 further adds baseline controls interacted with a linear time trend. Column 3 adds socioeconomic time-varying controls, and column 4 adds time-varying fiscal controls discussed in Section 4. The final specification is the most saturated, still, in the context of our analysis fiscal controls may be considered endogenous. For that reason, our preferred

[17] According to the Ministry of Health Financial Management Manual (Minitério da Saúde 2003), non-compliance with the minimum amount of resources that should be spent in the provision of healthcare can lead to sanctions such as retention of resources from the Municipalities' Participation Fund and States' Participation Fund, suspension of a term of office, and even Federal intervention.

[18] A particular concern one may have related to this assumption is that municipalities may have shifted spending in the pre-treatment period as a response to the spending reform. Given the relatively quick passage of the reform this seems unlikely. Moreover, given that the process of approval of the EC/29 involved several political stages and actors, it was arguably quite difficult to predict when the proposals would become an amendment, what exactly this amendment would entail, and how it would affect municipalities' public health spending decisions. As the reform refers to total spending, municipalities would gain nothing from shifting spending away from health in the pre-reform period.

specification is that presented in column 3.

In Panel A, column 3, we observe that the EC/29 spending reform is positively associated with total spending and total revenue collected by municipalities, with a point estimate for spending nearly threefold greater in comparison to revenues, though coefficients are not statistically significant. Appendix Figure B.1 presents dynamic effects and suggests that impacts on revenues are flat around zero, while point estimates on spending show an insignificant downward trend in spending before EC/29, followed by marginally positive effects of around 0.25 after the reform. This is consistent with municipalities beginning to spend slightly more on average, while still complying with legal restrictions on spending and debt. The Fiscal Responsibility Law establishes that municipal spending can exceed revenues by no more than 20%, with municipalities having until 2016 to comply with the 20% target. Excess above this target must be reduced by at least 6.6% per year, while according to Federal Senate Resolutions non-compliance with debt ceilings implies that municipalities can no longer receive public transfers, get access to federal loans and bank credit (Brasil, 2000; Rocha, 2007).[19]

The remaining results from Panel A indicate that, across all classes, the EC/29 reform drives large increases in health spending, with no such effects in other classes. Note that in column 3 point estimates for other spending classes are generally negative, although much smaller in magnitude and statistically insignificant. On average, these results point to municipalities re-optimising in order to increase the fiscal space for health, smoothing across other spending classes such that drastic cuts are avoided. Dynamic effects shown in Figure B.1 reinforce these results. In column 3 of Table 1, the point estimate indicates an increase of 12.8% in health spending per capita for a representative municipality in which the reform led to an increase in spending of 10 p.p.

SIOPS data considered in Panel B provides a richer break-down of impacts on health spending, having both a dedicated measure of health spending, as well as measures of spending by classes within health. Estimates are stable across columns. In column 3, we find a 23.3% increase in total health spending per capita relative to baseline for our representative municipality. This effect is almost twice as large as that on health and sanitation spending reported in Panel A, given that it focuses exclusively on health spending. Additionally, this effect comes almost entirely from increases in spending from own resources (55% increase relative to baseline). When considering sub-classes of spending, all types of health spending were observed to move as a result of the EC/29 reform, but increases in investments (54%) and in administrative expenses (43%) are particularly large, followed by spending in personnel (26%) and outsourcing (12%).[20]

We consider dynamic effects of the reform on health spending in Figure 3. Even though SIOPS is a more complete source of data on health spending, the system is only available after the year 2000. Therefore, we will use FINBRA data to evaluate the presence of pre-trends in health spending and then move on to further assess health spending and resource allocation with SIOPS data. Figure 3a plots the dynamic effects on spending per capita on health and sanitation. We observe no significant pre-trends in spending and a clear

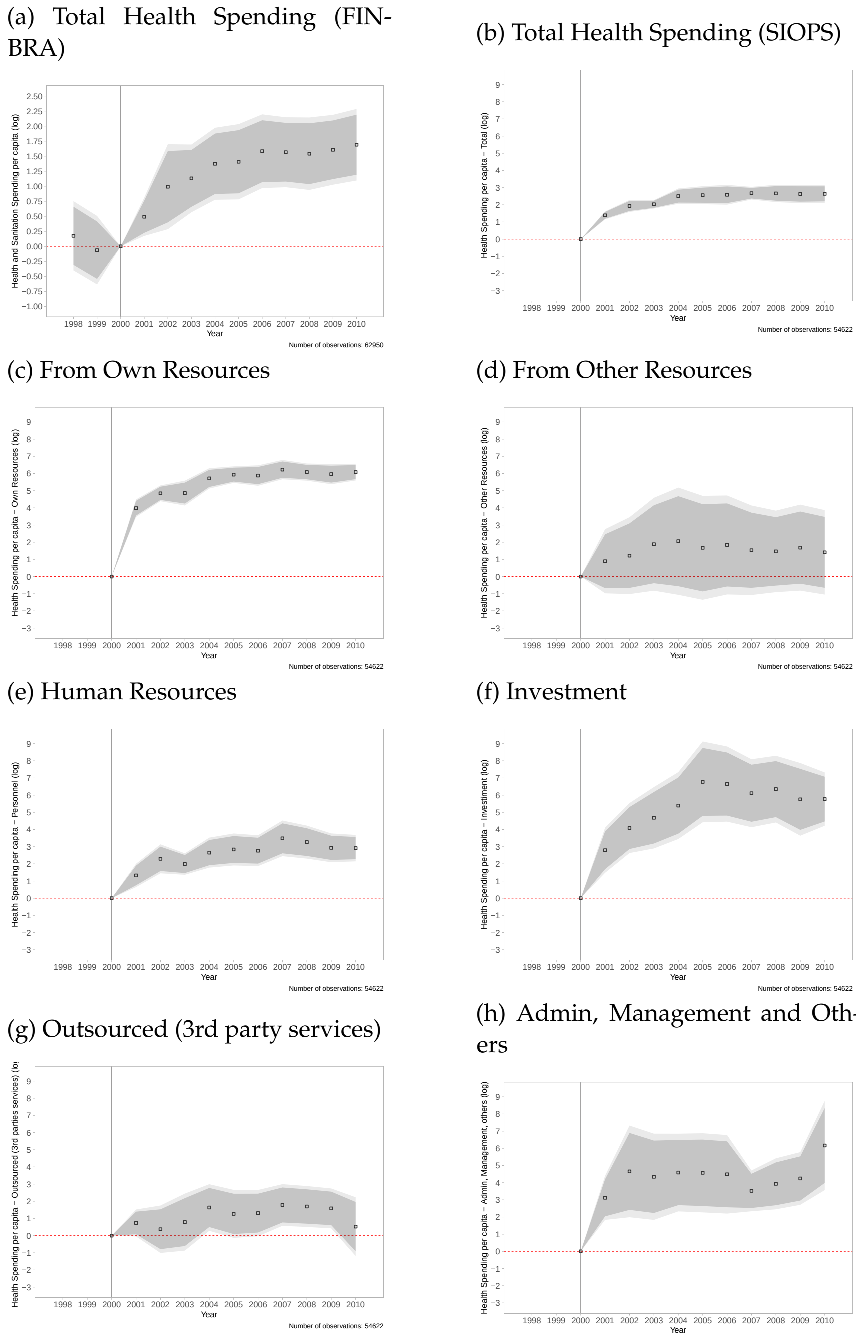
[19] Descriptive evidence also suggests that municipalities often face difficulties in executing primary expenditure across the different government sectors, which may typically lead to unspent budgetary funds (IFI, 2018). For instance, just after the EC/29 passage, and until 2005, average figures related to unspent funds ranged around 4.3% to 7.4% of government budgets, potentially providing municipalities with budget flexibility to meet EC/29 requirements.

[20] Note that baseline statistics in Appendix Table A.1 show relatively low shares of resources allocated to investments within total municipality health spending, with the great majority of resources being allocated to human resources and administrative expenses.

TABLE 1 Fiscal Reactions, in natural logarithm of Reais per capita

	ln(Spending)			
	(1)	(2)	(3)	(4)
Panel A: FINBRA				
Total Revenues	-0.118	0.001	0.040	0.063
	(0.139)	(0.117)	(0.112)	(0.112)
Total Spending	-0.039	0.071	0.110	0.092
	(0.137)	(0.115)	(0.111)	(0.111)
Health Spending	1.109***	1.236***	1.282***	1.232***
	(0.250)	(0.227)	(0.225)	(0.224)
Non-Health Spending	-0.234*	-0.134	-0.097	-0.111
	(0.130)	(0.109)	(0.105)	(0.105)
Non-Health Social Spending	-0.112	-0.058	-0.030	-0.049
	(0.163)	(0.136)	(0.134)	(0.133)
Non-Social Spending	-0.300*	-0.170	-0.124	-0.135
	(0.174)	(0.147)	(0.141)	(0.141)
Observations (Each cell)	62950	62950	62950	62886
Panel B: SIOPS				
Total Health Spending	2.200***	2.303***	2.328***	2.316***
	(0.248)	(0.195)	(0.185)	(0.208)
From Own Resources	5.430***	5.473***	5.501***	5.487***
	(0.271)	(0.260)	(0.248)	(0.261)
From Other Resources	1.594	1.558	1.558	1.590
	(1.561)	(1.309)	(1.298)	(1.316)
Personnel	2.544***	2.581***	2.600***	2.564***
	(0.428)	(0.365)	(0.364)	(0.370)
Investment	5.691***	5.353***	5.358***	5.304***
	(1.044)	(0.744)	(0.738)	(0.752)
Outsourced (3^{rd} party services)	0.771	1.117*	1.150**	1.123*
	(0.695)	(0.606)	(0.580)	(0.640)
Admin, Management and Others	4.418***	4.308***	4.332***	4.355***
	(1.081)	(0.975)	(0.972)	(0.990)
Observations (Each cell)	54622	54622	54622	53685
Mun FE, Time-State FE, Data Quality Control	Y	Y	Y	Y
Baseline Socioeconomic Controls × Time	N	Y	Y	Y
Time-Varying Controls	N	N	Y	Y
Fiscal Controls	N	N	N	Y

Notes: Each cell represents a separate regression of spending or revenue on exposure to the EC/29 reform, following (3). The number of observations in each cell is indicated at the foot of each panel (all spending outcomes are observed for each observation) within FINBRA (panel A) and SIOPS (panel B) measures. Column 1 presents the baseline model with municipality and state-year fixed effects, plus data quality controls. Column 2 adds baseline socioeconomic controls from the Census interacted with time. Column 3 adds controls for GDP per capita and *Bolsa Familia* transfers per capita. Column 4 adds fiscal controls; namely neighbouring municipality spending and exposure to the LRF. Covariates are omitted for ease of presentation. Standard errors presented in parentheses are clustered at the municipality level. * $p<0.10$; ** $p < 0.05$; *** $p < 0.01$.



(a) Total Health Spending (FINBRA)

(b) Total Health Spending (SIOPS)

(c) From Own Resources

(d) From Other Resources

(e) Human Resources

(f) Investment

(g) Outsourced (3rd party services)

(h) Admin, Management and Others

FIGURE 3 Dynamic Effects on Health Spending, by Spending Classes and Source of Funding. *Notes*: All outcomes refer to health spending, measured using SIOPS data on health spending unless otherwise indicated. Estimated leads and lags to EC/29 reform are presented following equation (1) controlling for baseline socioeconomic controls from the Census interacted with time, plus data quality controls. Point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality. Tabular output including observation numbers is available as Appendix Table E.1.

and significant pattern of increase in spending, with each of the first years after the EC/29 presenting larger effects, that stabilize around 2004 onwards. This is in-line with the nature of the reform, which allows municipalities a period to achieve the mandated spending target. The dynamics of the increase in health and sanitation spending, depicted in Figure 3a, is similar to that observed for total health spending recorded from SIOPS (Figure 3b), and as noted above, is largely driven by spending of own resources. Panels (e)-(h) of Figure 3 show that spending on human resources continuously increases until at least 2004, while administrative expenses and investments sharply increase from 2000, stabilizing in 2002 and 2005, respectively.

Results documented to this point are based on all spending variation induced by the EC/29 constitutional reform. However, this potentially masks heterogeneity in the nature of spending shifts. Figure 2 showed that spending changes appear in municipalities which were below the 15% cut-off, but also in those which were above the cut-off, acting to drive down spending in these municipalities. As discussed in Section 4, results could thus be driven by a number of shifts in outcomes as well as by the dynamic changes in patterns in one group relative to another after the reform. Figure 4 further breaks down the impacts of spending reforms on municipalities' fiscal responses. This figure is analogous to Figure 3, however here we follow equation (2) and separately consider municipalities which were above the spending target at baseline (red points and CIs), and those which were below the spending target at baseline (blue points and CIs).

Consistent with Figure 2, we observe in Figure 4 that municipalities below the target systematically increase health spending, specifically out of own resources and across all spending classes. The opposite is documented for those municipalities above the target, although point estimates (in absolute terms) are smaller in magnitude. These municipalities may have used the target as a focal point around which health spending should be set, potentially resulting in a reduction in total spending towards reform compliance. Appendix Figure B.2 shows dynamic effects for those above and below the target based on FINBRA data. Results are similar in qualitative terms, and suggest a tendency of total spending to decrease for those above the target. We also note that point estimates for those below the target are often greater than the average effects shown in Figure 3.[21]

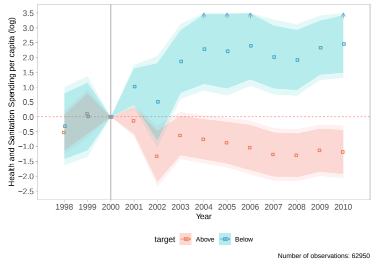
5.2 | Infant Mortality Rates

5.2.1 | Main Results

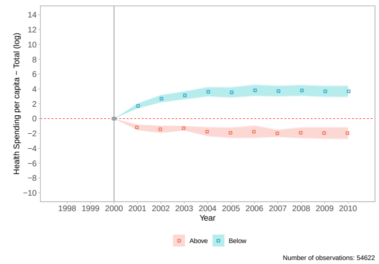
We now assess whether the shifts documented on spending translate into effects on health outcomes. Figure 5 presents dynamic effects for all-cause infant mortality, and infant mortality by time of death. The top row of this figure presents results from estimates across all municipalities following equation (1). Coefficients for the period before the year 2000 point to noisy, but statistically insignificant pre-reform effects. Findings remain similar in Appendix B.2, where we further assess pre-trends and discuss data quality by extending the period of analysis before the reform. Following reform implementation, we observe a decline in infant mortality, which in the case of all-cause infant mortality occurs gradually, resulting in statistically significant effects from around 2007 onward. The timing of this decline lines up in patterns in health spending which are scaled up over time. In Section 6

[21] For example, if considering total health spending as measured by SIOPS, from around 2004 onwards point estimates indicate that a 10 p.p. distance below the spending target is associated with increases in health spending by around 35-38%. This is larger than the value of 23.3% in Table 1, confirming greater spending increases for these municipalities, holding fixed changes that occurred in the group above the target.

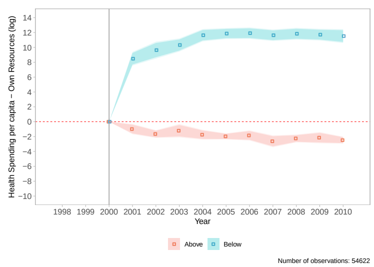
(a) Health and Sanitation (FINBRA)



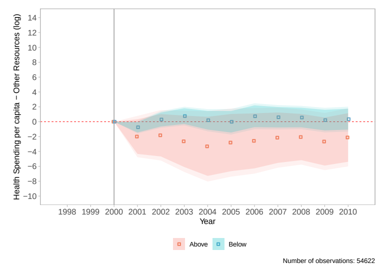
(b) Total Health Spending (SIOPS)



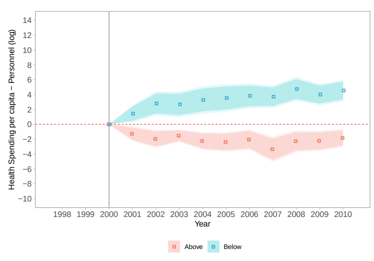
(c) From Own Resources



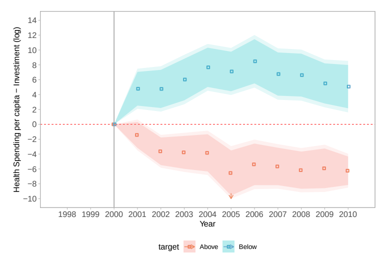
(d) From Other Resources



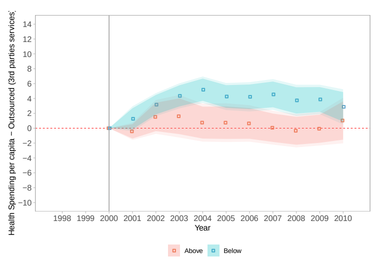
(e) Human Resources



(f) Investment



(g) Outsourced (3rd party services)



(h) Admin, Management and Others

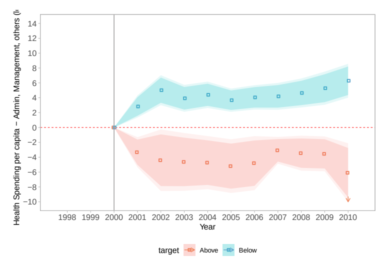


FIGURE 4 Effects on Health Spending per capita (Distributional Effects. *Notes*: All outcomes refer to health spending, measured using SIOPS data on health spending unless otherwise indicated. Estimated leads and lags to EC/29 reform are presented following equation (2) controlling for baseline socioeconomic controls from the Census interacted with time, plus data quality controls. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case, 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality. Tabular output including observation numbers is available as Appendix Table E.2.

we additionally document how these effects line up with the timing of potential mechanism variables.

In considering infant mortality by the time of death, we observe broadly similar results for deaths occurring in the neonatal period, which refers to the first 28 days of life. In panel (b) we observe rapid declines in deaths within the first 24 hours of life, while in panel (c) we find a similar pattern observed for total mortality within the remaining neonatal period. Finally, in panel (d) there is little evidence pointing to broader declines in infant mortality after the first month of life. If we consider 2007, the first year when effects become statistically significant for total mortality, we observe a point estimate of -8.7 for total mortality, -4.1 for deaths within the first 24 hours and -6.3 for the remaining neonatal period. Taking a 10 p.p. increase in health spending, these represent, respectively, reductions of 0.87 (corresponding to 3.8% of the baseline average of this measure), 0.41 (7.4%) and 0.63 (4.6%).

Municipalities below the 15% threshold increased spending, whereas those exceeding the target reduced spending. While panels (a)-(d) of Figure 5 focus on average effects across all municipalities, in principle these estimates could owe to the aggregation of a number of different effects. It could be that municipalities which increased spending experienced IMR declines, or it could be that IMR increased in areas where spending was cut, or it could be a combination of both.

Panels (e)-(h) of Figure 5 present results analogous to panels (a)-(d), but now separating by effects driven by above- and below-target municipalities as in equation (2). We observe clear mortality declines occurring in below-target municipalities, and the pattern is similar to those portrayed in panels (a)-(d). For example, in considering deaths within 24 hours, by 2006 point estimates suggest that a 10 p.p. increase in spending would result in 0.66 fewer deaths per 1,000 live births, or around a 12% decline when compared with baseline rates of mortality. On the other hand, we do not observe any statistically significant changes in mortality where spending was contracted, with point estimates generally being at most one third of the magnitude of those in municipalities which increased spending. These trends are important as they indicate that municipalities reduced spending without measurable adverse consequences for health outcomes, at least in terms of extreme outcomes such as infant mortality.

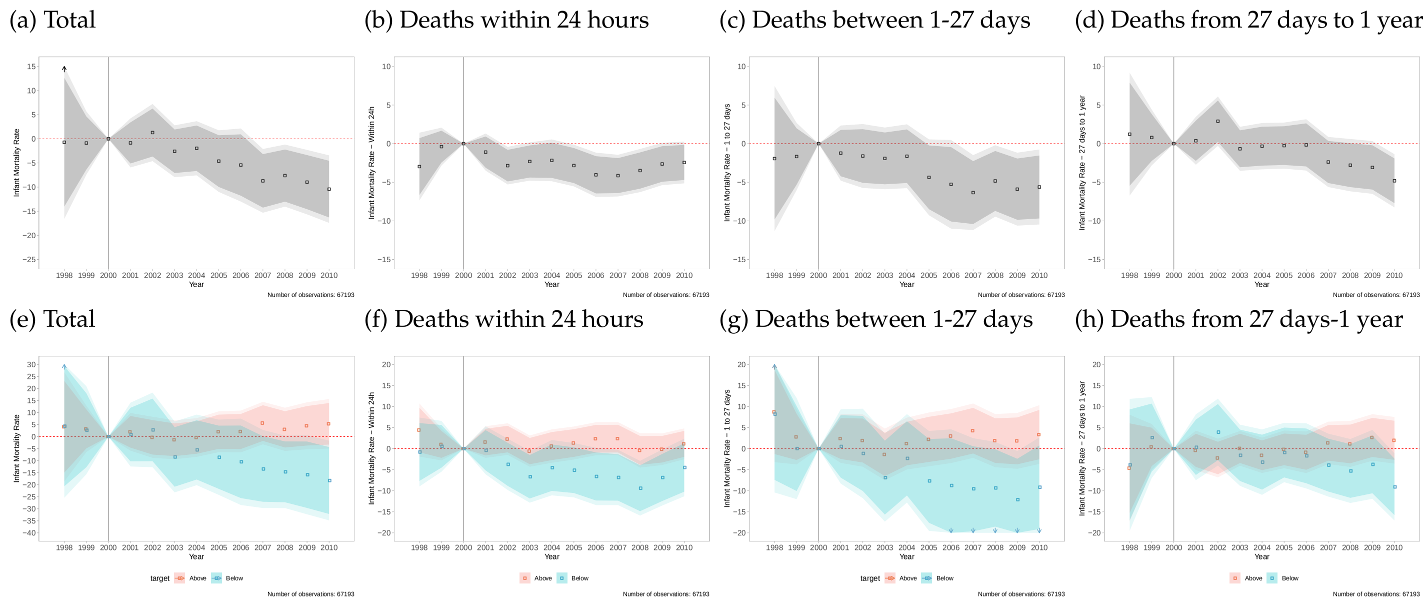


FIGURE 5 Effects on Infant Mortality Rates, Total and By Timing of Death. *Notes*: Panels (a) to (d) present estimates from equation (1), and panel (e) to (h) present estimates from equation (2). In each specification lags and leads to the EC/29 passage are presented, controlling for data quality, and baseline socioeconomic controls from the Census interacted with time. Panels (a) to (d) present global estimates from spending shifts, where point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Panels (e) to (h) present spending impacts separating by municipalities located below and above the spending threshold (15%) at baseline. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality. Tabular output including observation numbers is available as Appendix Tables E.3 (panels (a)-(d)) and E.4 (panels (e)-(h)).

5.2.2 | Effects on IMR by Cause of Death

Figures B.5-B.6 present estimates of impacts on infant mortality by cause of death for aggregate and distributional models, respectively. While such models imply challenges in terms of power given the lower counts of cause-specific deaths, we observe that mortality declines are primarily concentrated on perinatal conditions. We also find suggestive evidence pointing to smaller effects on IMR for infectious, respiratory and nutritional causes. Perinatal mortality refers to death in late pregnancy and very early in life. It is often related to maternal conditions, and is potentially modifiable with interventions provided to women in the pre-natal and intra-partum period (Allanson et al., 2016). On the other hand, causes that are unlikely to respond to health investments such as external causes are observed to be flat in the pre and post-reform period. Finally, in panel (h) we observe a transitory increase in ill-defined mortality, specifically in the first years following the reform, reverting to zero from 2006 onward. This may reflect that deaths which had not been detected started being recorded, although records still faced quality issues in the first years after the reform. Across outcomes, effects are observed to be entirely driven by below-target municipalities, rather than by shifts in above-target municipalities (Figure B.6).

5.2.3 | Implied Elasticities

In general, papers estimating the causal relationship between health spending and mortality often run log-log regressions and present estimates for the elasticity of mortality with respect to health spending.[22] We explicitly choose not to apply transformations to our health outcomes variables due to the number of observations with values equal to zero, notably those related to mortality.[23] Nonetheless, to relate our results to the literature, we back out elasticities for IMR using the estimates from our regressions. To calculate these elasticities we scale reform-mediated effects on health outcomes by reform-mediated effects on spending (refer to Appendix B.3.2 for formal definitions).

Appendix Figure B.7 and Appendix Table B.2 present the elasticity estimates for each year after the reform. As a benchmark, the elasticities presented in the literature vary greatly. Within cross-country studies, while Filmer and Pritchett (1999) find a very small elasticity of −0.08, Gupta et al. (2002) find an elasticity of −0.31, and Bokhari et al. (2007) estimate elasticities ranging between −0.4 and −0.5. In the micro studies, Crémieux et al. (1999b) find large elasticities between −0.8 and −1.1, Bhalotra (2007) finds an elasticity of −0.24 for rural populations, and Castro et al. (2021)'s elasticities range between −0.5 and −0.9. Our results suggest that even within a single setting elasticities can vary considerably depending on the spending horizon studied, but point to smaller elasticities than many of those estimated from standard two-way fixed effect models. Using SIOPS as the measure of health spending, we find IMR elasticities ranging from close to 0 in the immediate aftermath of the reform, to around −0.2 ten years following the reform. Our results suggest that even 10 years out upper ends of confidence intervals can rule out estimates greater than −0.5. In early years estimates larger in magnitude than around −0.2 to −0.3 can be ruled out. In general, if combining estimates from equation (2) to consider elasticities in areas with spending cuts

[22] See, for example, Filmer and Pritchett (1999) and Crémieux et al. (1999b), among others.

[23] Our data comprises all the Brazilian municipalities with available data for the period of analysis, some with population sizes as small as 700 inhabitants, and it is common to find null infant mortality rates. Running log transformations would therefore discard relevant information for several outcomes. The consistent use of rates also avoids problems inherent in log transformations with zero outcomes described by Chen and Roth (2022).

and spending increases, we observe that mortality is more sensitive to increases in spending than declines in spending (Figure B.8), though effects are more noisily estimated. Finally, if we wish to consider global elasticities across the entire time-horizon studied, we can repeat this procedure with single coefficient estimates following equation (3). These results are presented in Table B.2 suggesting average values of -0.15 (column 7) for infant mortality, and greater proportional changes in mortality in certain classes such as mortality within the first day of life (-0.26) where EC/29 spending was found to be particularly effective.

All elasticities documented to this point refer to mean responses of the EC/29 spending reform. We further explore heterogeneity in returns to spending in Section 7, in particular considering how such returns depend upon health spending patterns as well as on institutional factors.

6 | MECHANISMS

6.1 | Effects on Health Inputs, Production Outputs, and Access to Services

How do spending changes map into changes in health outcomes? We start by considering how public spending shifts affect intermediate outcomes in the public sector. This includes measures of health inputs, access to health services, and health production outputs at the municipal level. Figure 6 presents in panel (a) reform impacts on an index constructed to measure access to health and the production of health services, while panel (d) presents impacts on an index of health inputs. Access to services and production outputs refer to factors such as the number of family visits per capita by health teams, the coverage of prenatal care, and so forth. Health inputs include factors such as the number of doctors per capita and the number of public hospitals per capita.[24]

We see immediate and large increases in access to services and production outputs as well as in health inputs. In the case of access and production outputs, we observe flat trends in the pre-EC/29 period, and then a sharp increase in the year following reform implementation, which is then maintained thereafter. In panel (d) we observe a single pre-reform period, but estimates suggest that the EC/29 reform led to substantial increases in health inputs.

Infant mortality declined in municipalities which at baseline were below the spending target, in line with spending increases in these areas. Those above the target, on the other hand, cut health spending but did not experience any clear adverse consequences on health outcomes. Consistent with these patterns, in panels (g) and (j) of Figure 6 we observe that increases in access and production as well as in health inputs owe to increases in municipalities which were below the target. Indexes are all expressed in terms of standard deviations, and so estimates are comparable across plots. We observe that a representative municipality 10 p.p. below the target experienced a similar increase of approximately 10% of a standard deviation in both indexes of access and production and of health inputs. As benchmark, 2005 point estimates are 0.798 and 0.804, respectively (see Table E.6). On the other hand, for those municipalities above the target we observe that while the health input index experiences a small and imprecisely estimated reduction, the access and production output index remains relatively stable around zero during the entire period.

Aggregate indexes are considered to avoid excessive multiple testing, however we can further separate them in sub-indices. Specifically, the access and production index can be

[24] The complete list of index components is available in Table A.2.

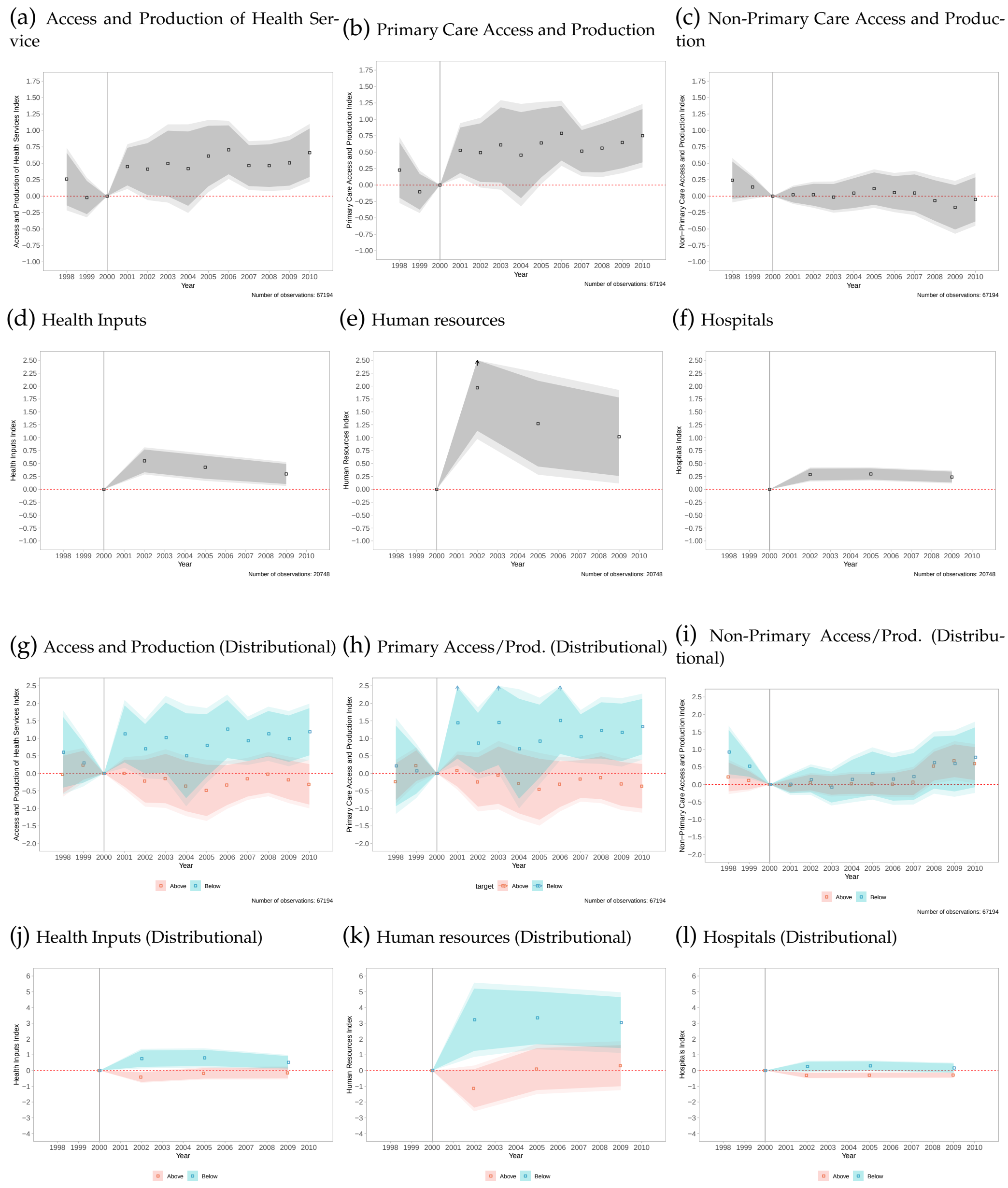


FIGURE 6 Effects on Access to Services, Production and Health Inputs. *Notes*: Panels (a) to (f) present estimates from (1), and panel (g) to (l) present estimates from (2). In each specification lags and leads to the EC/29 passage are presented, controlling for data quality, and baseline socioeconomic controls from the Census interacted with time. Panels (a) to (f) present global estimates from spending shifts, where point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Panels (g) to (l) present spending impacts separating by municipalities located below and above the spending threshold (15%) at baseline. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality. Tabular output including observation numbers is available as Appendix Tables E.5 (panels (a)-(f)) and E.6 (panels (g)-(l)).

separated into elements related to primary care access and production (e.g. ambulatory care, household visits, and outpatient primary care), and non-primary care (e.g. high complexity procedures). The health inputs index can similarly be separated into factors related to human resources (e.g. doctors, nurses, and administrative professionals per capita), and infrastructure (e.g. hospital availability).

Effects on Access to Services and Production Outputs. Panels (b) and (c) of Figure 6 break down effects of reform exposure by primary care and non-primary care related measures. We observe that results are driven by increasing access to primary care and related production outputs, which is suggestive of reform-driven changes occurring at the point of entry to the system, and in increasing access to low complexity outpatient procedures. This is also consistent with municipalities being the main providers of primary health services in Brazil (Mrejen et al., 2021). In panel (h) we observe that increases in primary care owe to the group of municipalities below the target. A representative municipality 10 p.p. below the target would be expected to see increases in primary care access and health production outputs greater than 14% of a standard deviation right in the first year after the reform. In the case of non-primary care access, effects remain around zero, with little evidence suggestive of a shift in more highly complex procedures. In both cases, we do not observe evidence to suggest significant differential pre-trends, though note that confidence intervals are wide, particularly 2 years prior to reform implementation.

Effects on Health Inputs. Turning to health inputs, panels (e) and (f) disaggregate inputs as those owing to human resources and those owing to physical infrastructure, namely the availability of public hospitals. In panel (e) we observe large and immediate effects on human resources, in line with the spending changes discussed in Section 5.1 (Figure 3, panel e). We also observe smaller, though still large, impacts on hospital infrastructure. The impact on physical inputs is relatively smaller in magnitude than changes on spending, despite the fact that the increase in spending on infrastructure substantially surpassed the increase in spending on human resources (Figure 3, panel f). This reflects the fact that human resource spending is largely a flow, and so baseline resources reflect the yearly cost, while infrastructure is a stock, requiring upfront and large investment per unit, and so any increases in infrastructure inputs will require large increases in spending.

As previously shown, for municipalities above the target, we do not observe any clear variation in access to services nor in production outputs. We nevertheless observe an imprecisely estimated but large decline in human resources in the first years after the reform, reverting to zero afterwards. We also observe small decline in the availability of public hospitals. In the next section we further break the availability of public hospitals into municipal *versus* state and federal facilities, and detect for municipalities above the target a clearer and persistent reduction in municipal hospitals specifically. Results therefore suggest that these municipalities managed to reduce spending, at the cost of reducing inputs, however without substantially affecting access to health nor production outputs, at least over the time-frame considered.

6.2 | Discussion on Other Pathways and Additional Robustness Checks

In this section we examine whether the spending reform affected other potential pathways connecting variation in spending and in health outcomes, and provide additional robustness checks in order to test for pathways that could still threaten identification. We first examine impacts on private provision and insurance coverage as well as on the provision of state and federal hospitals. We then assess effects on adult outcomes, to further test for crowding out effects within health care services, and geographical spillovers across municipalities, to check for changes in service referral and patient mobility across local health systems. We close this section with robustness exercises.

6.2.1 | Effects on the Private Sector and Other Public Providers

Appendix Figure C.1 sheds light on whether changes in municipal spending affected private health care demand or supply, and whether other public providers responded to the spending reform. Crowding out of private services could be observed as long as municipal health services improve and start absorbing demand. This could be particularly the case of individuals covered by private insurance, who may start substituting private services with municipal health services. Moreover, the expansion of municipal services may have induced a contraction of services provided by states and the federal government. On the other hand, the public sector often outsources to private services provided by profit and not-for-profit providers, thus potentially inducing private supply. We now examine whether there is evidence of such shifts based in the expansion of public spending flowing from the EC/29 reform, and discuss implications for health outcomes.

We focus on private insurance coverage and availability of hospitals given the availability of comparable and systematically measured data. Top panels of Appendix Figure C.1 present aggregate estimates, while bottom panels consider distributional effects. Panels C.1a-C.1c present estimates of impacts on the supply of hospitals. All variables are measured as hospitals per 1,000 residents, and are presented on a common scale. Panel C.1a, in line with increased infrastructure spending, shows clear evidence of increases in availability of hospitals administered by municipalities.[25] In the case of federal and state hospitals, which are not directly affected by municipal spending shares, we see no evidence of crowding out, with flat and approximately zero effects.

Considering private hospitals, there is some relatively weak evidence in favour of complementarities in the short term. Distributional results in panels C.1e-C.1g suggest that such complementarities between public and private hospital expansions are driven by municipalities below the target, and are consistent with increases in spending in outsourcing among these municipalities (see Figure 4). These results are also consistent, at least in the short term, with effects observed in other settings, where private investment has been noted to be complementary to public investment (Corbi et al., 2018). On the other hand, there is a weak but upward trend in the availability of private hospitals in municipalities above the target, where the supply of municipal hospitals was contracted. As we do not observe any changes in outsourcing spending among these municipalities, results suggest a potential role for substitution effects. Finally, in panel C.1d, we observe relatively little evidence to suggest that the EC/29 resulted in changes in individual coverage by private insurance providers. Estimates are broadly flat and insignificant. Distributional effects in Figure C.1h similarly point to largely flat patterns at least in the 7 years following the passage of the EC/29 amendment — except for a downward trend after 2007 for municipalities below the target, though imprecisely estimated.

Based on the available data, evidence therefore suggests an expansion of municipal services specifically, complemented by weak evidence of an expansion in private services during the initial increase of municipal spending in municipalities below the target at baseline. This is where we observe the reduction in infant mortality rates. On the other

[25] It is important to remind that municipal hospitals are typically small-scale facilities, providing inpatient services but often having on average around 50 or fewer hospital beds.

hand, there is some evidence pointing to an expansion in private services in municipalities above the target, where spending was contracted and the supply of municipal hospitals decreased. Yet, that expansion is not significant in the first years after the reform, and we do not observe any changes in private insurance coverage. Moreover, access to public services and production outputs remained stable in these municipalities after the reform. This suggests that the stability in infant mortality rates after spending cuts may have been partially sustained by efficiency gains in the public sector.

6.2.2 | Effects on Adult Health Outcomes

In Section 5.2 we focused primarily on infant mortality as this outcome is well characterized in terms of timing and health service needs. Yet, we can extend the analysis to examine adult hospitalization and mortality outcomes. In particular, this allows us to consider the concern that spending changes may improve certain outcomes which are amenable to being targeted by resources, such as prenatal care, at the cost of other outcomes, such as chronic conditions among adults, which require continuous support and inputs. In that case, for instance, reform impacts in municipalities below the target may lead to improvement in infant mortality, but could potentially lead to deterioration in other outcomes. Alternatively, sharp improvements in adult outcomes could suggest that spending changes did target services more related to adult rather than infant health, eventually limiting greater improvements in birth outcomes.

In Figure C.2 we present results where we consider adult hospitalisation rates (top row) and adult mortality rates (bottom row). Outcomes consider all adults aged 40 years and above, and are standardized as rates per 1,000 individuals. We do not observe evidence consistent with crowding out of health outcomes. If anything, and in particular among municipalities below the target at baseline, while we see declines in rates of hospitalisation driven by causes amenable to primary care, there is (weak) evidence suggestive of mortality declines at older ages too.

6.2.3 | Geographical Spillovers

An alternative consideration is whether greater spending in a given municipality may reduce the rate of individuals seeking treatment in other municipalities, or attract residents from other municipalities to receive treatment. Both such phenomena could lead to greater congestion, as a result limiting any positive effects on health outcomes, despite changes in spending.

Geographical spillovers are not expected in primary care services, as access is restricted to catchment areas defined within the municipality of residence. We therefore focus on relatively higher complex services by taking advantage of the information contained in the hospitalization microdata, which allows us to track patient flows across municipalities. We examine patient outflows and inflows as measured based on the rate of individuals from a given municipality treated in hospitals in other municipalities (hospitalization outflows), as well as the rate of individuals from other municipalities receiving treatment in a given municipality (hospitalization inflows). In Appendix Figure C.3 we observe positive changes only in outflows, mainly driven by residents in municipalities that were below the target at baseline, and receiving care outside of their municipality for conditions that are not amenable to primary care services. The expansion of primary care coverage allows for greater detection and timely treatment of health problems, which should lead to demand-driven declines in hospitalizations for causes that are amenable to primary care. However, such a pattern would not be reflected in causes which are not amenable to primary care and that require more complex treatment, and we may even expect hospitalisation rates to increase through better referral if primary care coverage and quality improves (Bhalotra et al., 2019). The increase in outflow rates for conditions not amenable to primary care may thus reflect this. Although imprecisely estimated, we observe negative changes for patient inflow rates for both groups of municipalities, below and above the target. Among municipalities where spending increased, in particular, this pattern may reflect an improved municipal capacity to organize patient flows within the health system and to increase the referral of primary care services for local residents. Moreover, unlike outflows which occur relatively uniformly in all municipalities in the country, inflows are skewed, with certain areas with greater capacity of absorbing high complexity cases concentrating patient inflows. Overall, if anything, results point against the conjecture that spending increases bring about an increase in congestion via inflows.

6.2.4 | Additional Robustness Checks

We consider a number of robustness checks in Appendix E. These checks consist of examining the sensitivity of estimates to alternative time-varying controls, including specifications with no time-varying controls, and considering re-weighting methods given concerns related to the estimation of treatment effects based on a dose response design.

In principal models displayed in the paper we control for baseline municipality characteristics measured from the census and a data quality proxy interacted with a linear time trend, as well as time-varying municipal controls. In Appendix Figures we document the stability of principal dynamic estimates to alternative control variables as laid out in Table 1. This includes models where we include no time-varying controls, and versions progressively controlling for data quality measures, census characteristics interacted with time trends, time-varying measures of municipal development and fiscal spending controls such as neighboring municipalities' health spending.

Across outcomes, we observe that results are not particularly sensitive to control sequences, and, fundamentally, even if one prefers to consider models with no time-varying controls, dynamic results are qualitatively similar to models which we report as our principal specification which do include data quality measures. We present these models in Appendix Figures E.1 (spending measures), E.3 (infant mortality), and E.5 (input and health service measure). In the interests of space, robustness checks are presented only for outcomes included in principal analyses, but stability is also observed for all results presented in Appendix C. For example, when considering spending, across all outcomes the inclusion of controls virtually does not affect coefficients or confidence intervals at any time frame. For infant mortality, the inclusion of controls makes the largest difference for deaths in the first month, with our preferred control specification being the most conservative, at most attenuating results by around 20% by year 10 post-reform. Across all outcomes considered, we do not observe cases where models with and without covariates lead to changes in the rejection of null hypotheses. A similar robustness to control specifications is observed for distributional models. Estimates are presented varying covariates in distributional models in Appendix Figures E.2 (spending measures), E.4 (infant mortality), and E.6 (input and health service measure). Again, across outcomes, estimates are observed to be relatively stable across control sequences.

In Section 4.2 we stressed that the validity of our research design relies on a strong parallel trends assumption. While we generally present pre-reform coefficients based on the same continuous spending measures, we additionally consider an alternative specification which re-weights to avoid potentially non-representative weighting given the particular distribution of treatment doses. Specifically, and in line with the discussion in Callaway et al. (2024), we present models re-weighting such that the estimand is matched to the true treatment effect distribution rather than the weights implicit in fixed effects models (refer to Appendix D). These results are presented as dashed lines in Appendix Figures E.1, E.3, and E.5. In nearly all cases, re-weighted estimates are similar, if not slightly larger in magnitude than standard population weighted counterparts. This is perhaps not surprising given that implicit two-way FE weights place slightly less weight on municipalities spending below the target where effects are observed to be larger (Appendix Figure D.1).

Finally, we may be concerned that we have data on relatively few pre-EC/29 periods, particularly for infant mortality rates, and pre-event coefficients are estimated with considerable noise. Quality of data on infant mortality in Brazil prior to 1998 can be considered substantially lower, as we discuss at more length in Appendix B.2. Nevertheless, we were able to collect two additional years of data by accessing raw vital statistics included in the SINASC and SIM systems, subject to the caveat of data measurement issues. In Appendix Figure B.4 we document event studies analogous to that presented in Figure 5, however now extending the sample as far back as feasible to overcome measurement issues. These results suggest similar patterns, especially in the post EC/29 period and years immediately prior to the reform, minimising concerns that results on infant mortality are simply capturing pre-existing differential trends in this outcome between areas more and less exposed to the reform.

7 | HETEROGENEITY IN SPENDING RETURNS

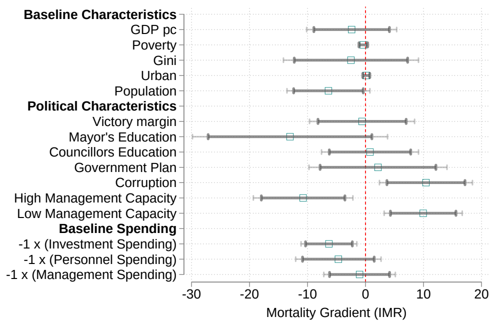
Up to this point, we have focused on documenting the average impacts of health spending and their corresponding elasticities. We now turn to examining heterogeneity in spending returns. More generally, this analysis enables us to enhance the understanding of public health production functions by identifying input complementarities, relevant non-linearities, and constraints in health production. This analysis is also important as it allows us to shed light on potential margins of spending effectiveness and its potential mediators, thus informing policy design.

7.1 | Assessing Observable Mediators and Constraints to Spending Returns

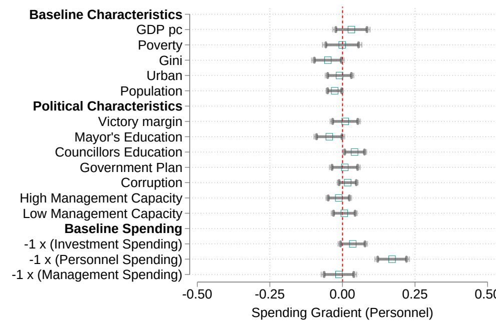
We start by asking whether municipalities with certain characteristics are systematically better poised to take advantage of spending shifts. We test for differential responses to spending shocks by estimating models following equation (3), but allowing for gradients in impacts of health spending by specific municipal characteristics. Namely, we estimate:

$$\begin{aligned} \mathsf{IMR}_{mt} = \alpha + \beta_0(\mathsf{Dist}_{m,pre} \times \mathsf{Post}_t) + \beta_1(\mathsf{Dist}_{m,pre} \times \mathsf{EC29}_t \times \mathsf{Characteristic}_{m,pre}) \\ + X'_{mt}\Gamma + \delta_{st} + \phi_m + \varepsilon_{mt} \quad (4) \end{aligned}$$

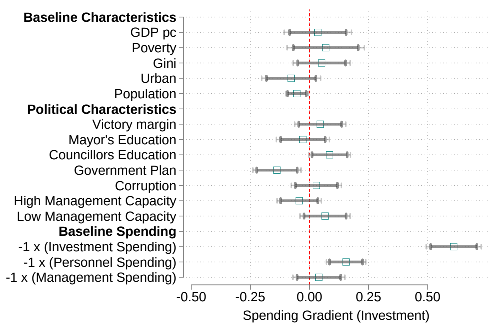
where $\mathsf{Characteristic}_{m,pre}$ refers to municipal characteristics, which consider factors such as state capacity, electoral competition and politicians' characteristics, spending levels at baseline, and socioeconomic determinants of health. These factors will be systematically scaled as Z-scores for comparability, and all other details are identical to those in equation (3).[26] We will also examine gradients in spending themselves, by replacing the outcome IMR_{mt} in equation (4) with the log of health spending on human and physical resources. Any gradient in responses to spending shocks will be captured by β_1. Note that these coefficients all refer to differential effects by the characteristic of interest, and thus positive values need not imply increases in infant mortality, but rather just smaller declines. In Section 7.2 we discuss marginal effects themselves.



(a) IMR Gradients



(b) Spending Gradients (Personnel)



(c) Spending Gradients (Investment)

FIGURE 7 Gradients in Mortality by Municipal Characteristics. *Notes:* Green squares reflect estimates of $\widehat{\beta}_1$ from equation (4) along with 95% confidence intervals. The characteristic for which spending "gradients" are displayed is indicated on the vertical axes. All characteristics are scaled as Z-scores for comparability, and thus (given their mean 0) coefficients can be viewed as indicating differential impacts beyond mean EC/29 effect reported in previous sections. Point estimates are presented as hollow squares, while 90 and 95% confidence intervals based on cluster-robust standard errors are presented as darker and lighter error bars respectively.

Results are presented in Figure 7, documenting estimates for $\widehat{\beta}_1$ across available municipal-level measures. Panel (a) presents results for infant mortality. When considering municipal characteristics such as development levels, poverty, inequality and rurality, we observe relatively little evidence to suggest differential impacts. We do observe some weak evidence suggestive of larger infant mortality declines in more populated areas, potentially indicative of returns to scale.

The most relevant gradients are observed in measures of political or state capacity. Though imprecisely estimated, we observe large declines as the mayor's education increases, while municipalities with greater management capacity are observed to bring about the largest declines in infant mortality. These effects are large, and are observed as both larger infant mortality declines in high-capacity areas (top quartile based on management quality) and smaller declines in low-capacity areas (bottom quartile). For example, a municipality whose exposure to the EC/29 amendment was 10 p.p. larger *and* which had a

[26] In these models we consistently control for the same baseline variables interacted with linear time trends as we did previously, but then include one $\text{Characteristic}_{m,pre}$ interacted with EC29_t at a time. Results are generally stable to omitting controls (results upon request).

high management capacity would bring about a decline of about 10 more infant deaths per 1,000 than in all other areas, while a similar distance in a low capacity area would essentially erase the entire decline observed. Importantly, we also observe that municipalities found to have committed corruption, as recorded in randomised audits, are those where observed declines are most eroded, with a 1 standardised increase in this variable effectively erasing all estimated IMR declines. This coheres with evidence that reductions in corruption improve local service provision (Funk and Owen, 2020), and justifies well-documented electoral penalisation of mayors in corrupt municipalities (Ferraz and Finan, 2011). Finally, we observe evidence that areas with the largest mortality declines were those which were spending less on investments and personnel at baseline, in line with larger increases in spending in these areas. We discuss the role that management capacity plays, and how this interacts with resources and health production functions, at more length in the next section.

It is important to note that the clear gradients in management capacity are not reflected in differential spending responses in panels (b) and (c). We also do not observe that areas found to be corrupt spend in significantly different ways across broad spending classes, though this is perhaps not surprising given that corruption audits focus on federal transfers and consider the procedures by which spending processes occur rather than spending patterns themselves, rooting out irregular (and likely highly inefficient) spending (Brollo et al., 2013). Across essentially all baseline characteristics and political characteristics considered, we observe that when subject to similar resource shocks, municipalities act to increase spending in personnel (Figure 7, panel b) and investment (Figure 7, panel c) in similar ways. The one clear pattern in spending observed in these figures is that municipalities act to invest where funds are lowest at baseline, consistent with closing the most acute gap in spending areas—personnel (investment) spending increases most in areas which at baseline have lowest personnel (investment) spending.[27]

7.2 | Assessing the Returns to Health Inputs and Complementarities

The results laid out above suggest a number of relevant patterns: larger effects on infant mortality where investment spending is lowest at baseline, shifts in investment (or personnel) spending to address scarcity in specific areas, and an overarching relevance of management capacity. To understand these results though the lens of municipal behaviour and health care production, we can consider a standard production function in which population health depends flexibly upon inputs (human and physical capital) as well the efficiency with which these resources are deployed: $\texttt{health} = \alpha \cdot f(L, K)$. In the previous section we documented that EC/29 effects scale linearly in these factors, but in fact complementary effects in spending may exist, with (for example) spending on human capital being unlikely to affect health without corresponding investment in physical capital. One particularly unique element of the EC/29 reform is that even across the distribution of total baseline health spending, municipalities had considerably different amounts spent on particular elements. Thus, even certain municipalities which were spending relatively large amounts at baseline were spending relatively little on personnel, or relatively little on physical resources. Indeed, Appendix Figure F.1 makes clear that across the distribution of distance

[27] To understand effect sizes, consider two municipalities whose distance to the spending threshold is 10 p.p., but one of which spends 1 standard deviation less on personnel at baseline. This lower spending municipality would increase spending on personnel by around 20% more than its higher spending counterpart. Similarly, the same is true for investment spending: subject to the same 10 p.p. distance to the EC/29 spending threshold, a municipality spending 1 standard deviation less would increase investment spending by around 60% more than its higher spending counterpart.

to the spending target at baseline, there are municipalities which spend large and small amounts on personnel, and large and small amounts on investments in physical capital. This allows us to consider how reform impacts vary by baseline spending levels.

To consider whether spending complementarities exist, we examine the effects of EC/29 in terms of baseline spending on human and physical resources, as well as their interactions. Specifically, we estimate the following, where for simplicity we have written $\text{Dist}_{m,pre} \times \text{EC29}_t$ (reform exposure) as EC29_{mt}:

$$\begin{aligned} Y_{mt} = \delta + \gamma_0 \text{EC29}_{mt} + \gamma_1 \text{EC29}_{mt} \times \text{Inv}_{m,pre} + \gamma_2 \text{EC29}_{mt} \times \text{Personnel}_{m,pre} \\ + \gamma_3 \text{EC29}_{mt} \times \text{Personnel}_{m,pre} \times \text{Inv}_{m,pre} + X'_{ct}\Gamma + \phi_m + \lambda_{st} + \varepsilon_{mt} \quad (5) \end{aligned}$$

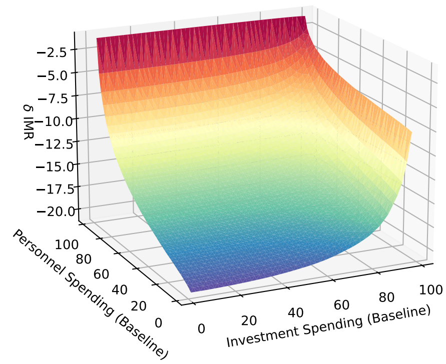
As previously, this specification allows us to estimate reform gradients, however here we can consider gradients in terms of baseline investment spending (γ_1), baseline personnel spending (γ_2) and their interaction (γ_3). If IMR declines are largest in areas with lowest spending at baseline, we would expect γ_1 and γ_2 to be negative, and if spending is complementary across classes such that areas with low spending at baseline on both dimensions had larger IMR declines, we would expect γ_3 to be negative.

In Figure 8, panel (a) we present the marginal effects of EC/29 on infant mortality across the distribution of investment and personnel spending. Because equation (5) allows for non-linear (interactive) effects of the reform by baseline health spending, marginal effects $\partial Y_{mt}/\partial \text{EC29}_{mt}$ depend on both values of personnel spending and investment spending. This figure presents such marginal effects across percentiles of baseline personnel spending (x-axis), and investment spending (y-axis). All effects are scaled in terms of a 10 p.p. distance from the EC/29 spending threshold. We observe substantial non-linearities, indicative that estimated effects are largest when municipalities had the lowest rates of spending on both investment and personnel at baseline. For example, in a municipality spending in the 10^{th} percentile of both personnel and investment in health, a 10 p.p. increase in health spending is estimated to reduce mean IMR by around 20 deaths per 1000. However, for a municipality spending at the 50^{th} percentile in each of these dimensions, this effect is reduced to around 16 deaths per 1000. Appendix Table F.1 provides a tabulation of these results, along with block bootstrap standard errors, at a number of points of baseline spending distributions.

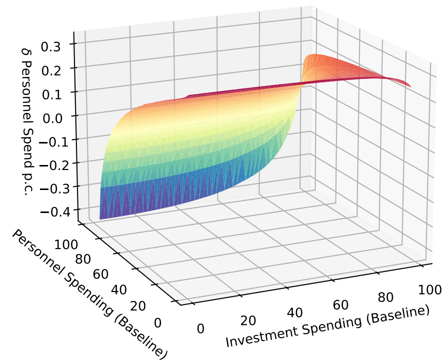
These results also point to substantial, though declining, returns to spending increases if municipalities are spending very little on one input, but considerable amounts on the other. For example, even among municipalities spending in the 90^{th} percentile of investment spending, substantial declines in IMR are observed if they are spending in the 10^{th} percentile on personnel (around 14 fewer deaths per 1,000), with slightly smaller values (9 fewer deaths per 1,000) for municipalities spending in the 90^{th} of personnel, but only the 10^{th} percentile of investment. Where results become effectively null are areas which spend substantially on both dimensions. For municipalities spending above the 80^{th} percentile on both dimensions, effects are observed to be effectively null on infant mortality of increases in spending.[28]

We observe that municipalities moved towards greater balance between inputs. Figure 8 panel (b) shows that municipalities spending little on personnel at baseline increased personnel spending, regardless of their spending on investment. An approximately inverse picture is seen if we consider investment spending in Figure 8 panel (c). We also observe

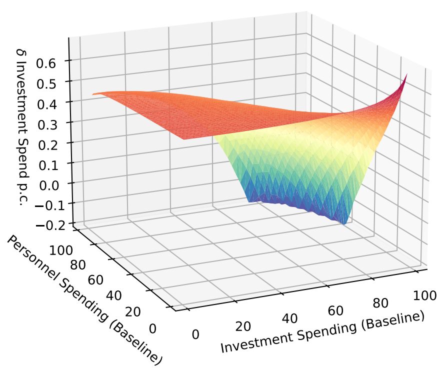
[28] Note that because spending targets refer to proportional amounts, municipalities can be spending above the 80^{th} percentile in both dimensions, and still be below the spending target at baseline. Indeed, across virtually the entire distribution of baseline spending in investment and human capital, estimates are identified off both above and below target municipalities.



(a) Δ Infant Mortality



(b) Δ Personnel Spending



(c) Δ Investment Spending

FIGURE 8 EC/29 Policy Responses and Baseline Spending. *Notes:* Estimated marginal effects are presented of a 10 p.p. increase in health spending on infant mortality (panel (a)) and log spending on personnel and investment (panels (b) and (c)) across the baseline distribution of spending on investment and personnel. Values on horizontal axes refer to baseline percentiles of spending in each dimension (1 is lowest baseline spending percentile, 99 is highest spending percentile), and values on the vertical axis refer to estimated changes in outcomes owing to exposure to EC/29 for municipalities at these values. To avoid large scaling on the vertical axis in panel (c), estimated values are capped at -0.2 for very high levels of personnel and investment spending.

sharp gradients as spending levels at baseline increase. At low levels of baseline investment (personnel) spending, personnel (investment) spending rapidly falls off. This suggests, for instance, municipalities optimally temper spending in personnel when they have substantial needs for investment.[29]

[29] This negative gradient in personnel spending is evident nearly everywhere except for at very high levels of investment spending. In these cases, we observe that when municipalities are spending both large amounts at baseline on investment and personnel, then marginal spending is directed to personnel, though note from panel (a) that this spending appears to have null effects on infant mortality.

TABLE 2 Distributional Responses to Spending Reforms by Management Capacity

Personnel Spending Percentile:	10^{th} percentile			50^{th} percentile			90^{th} percentile		
Investment Spending Percentile:	10^{th}	50^{th}	90^{th}	10^{th}	50^{th}	90^{th}	10^{th}	50^{th}	90^{th}
Panel A: High Management Capacity									
Infant Mortality	-31.82**	-30.68**	-23.42**	-23.61*	-22.88*	-18.20*	-6.92	-7.01	-7.59
	(15.98)	(15.10)	(10.54)	(12.39)	(11.81)	(9.36)	(12.15)	(11.58)	(10.18)
Infant Mortality (24 hours)	-6.36*	-6.17*	-4.99**	-4.61*	-4.52*	-3.93*	-1.05	-1.15	-1.80
	(3.34)	(3.17)	(2.34)	(2.41)	(2.32)	(2.03)	(2.48)	(2.35)	(2.12)
log(Total Health Spending)	0.27***	0.25***	0.18***	0.23***	0.22***	0.15***	0.15***	0.14***	0.07**
	(0.03)	(0.03)	(0.05)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
log(Personnel Spending)	0.29***	0.28***	0.25***	0.16***	0.16***	0.17***	-0.11	-0.09	0.01
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.07)	(0.07)	(0.06)
log(Investment Spending)	0.56***	0.54***	0.40***	0.53***	0.47***	0.09	0.45***	0.32***	-0.56***
	(0.10)	(0.09)	(0.12)	(0.07)	(0.06)	(0.11)	(0.10)	(0.10)	(0.15)
log(Other Health Spending)	0.43***	0.41***	0.28***	0.43***	0.41***	0.25***	0.43***	0.40***	0.20**
	(0.08)	(0.07)	(0.08)	(0.06)	(0.06)	(0.07)	(0.09)	(0.09)	(0.09)
Panel B:Low Management Capacity									
Infant Mortality	4.95	4.84	3.81	1.39	1.48	2.39	-8.22	-7.59	-1.41
	(5.44)	(5.21)	(4.12)	(3.49)	(3.31)	(2.93)	(11.26)	(10.91)	(8.90)
Infant Mortality (24 hours)	2.59	2.46	1.17	1.19	1.16	0.87	-2.58	-2.34	0.06
	(1.83)	(1.76)	(1.20)	(1.10)	(1.06)	(0.75)	(3.39)	(3.30)	(2.60)
log(Total Health Spending)	0.28***	0.28***	0.28***	0.25***	0.25***	0.24***	0.17***	0.17***	0.11***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
log(Personnel Spending)	0.33***	0.32***	0.27***	0.15***	0.15***	0.17***	-0.34***	-0.32***	-0.08
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)	(0.06)
log(Investment Spending)	0.56***	0.55***	0.50***	0.58***	0.55***	0.19**	0.65***	0.53***	-0.67***
	(0.11)	(0.10)	(0.09)	(0.08)	(0.08)	(0.09)	(0.14)	(0.13)	(0.21)
log(Other Health Spending)	0.28***	0.27***	0.19***	0.30***	0.29***	0.21***	0.35***	0.34***	0.27***
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.06)	(0.06)	(0.05)

Notes: Each row consists of a separate regression following (5), with each cell reporting the marginal effect of EC/29 on the outcome indicated in row titles at specific percentiles of the baseline distribution of personnel and investment spending per-capita, with percentiles indicated in table headers. Each estimate refers to the scaled effect of being 10% from the EC/29 spending threshold. Panel A reports results for municipalities above the median based on the nationally collected IQIM management score, while Panel B reports results for below-median municipalities. Standard errors estimated by a municipal level block bootstrap are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

The results presented in Section 7.1 point to the relevance of management. If we consider management as a scale factor, we may expect that quality management lifts up the efficiency with which resources are used, in line with the factor α in the production function discussed above. To test for this, we consider identical models as in equation (5), however allowing for factors of production to act differentially in high and low-management capacity areas. We thus re-estimate equation (5), stratifying by areas above and below the median management score.[30] As above, we consider the marginal impacts of the EC/29 reform across the distribution of baseline spending in investment and personnel. Key elements are summarised in Table 2 (full graphic results in Figure F.4).

Two key patterns emerge. Firstly, across the board municipalities tend to spend more on areas where need is supposedly most acute—where spending on personnel is low, this area is bolstered with relatively less spending on physical capital, and the same is true when spending on physical capital is low. However, the way which this spending maps into infant mortality is remarkably different. In areas with high management capacity large increases in spending are observed to result in large declines in infant mortality, following the patterns observed in Figure 8. In areas with poor management practices, even subject to similar spending, no such decline in infant mortality is observed with all results in Table 2 being close to zero. However, areas which have higher management capacity are much more able to convert this spending into improvements in health outcomes, suggesting substantial returns to improvements in management practices.

These results line up with an emerging literature on management practice in the public sector. In other settings Rasul and Rogger (2017) suggest that autonomy in public sector management improves outcomes, while a number of recent examples suggest that improved management practices in hospitals have substantial effects on mortality rates (Muñoz and Otero, 2023; Hollingsworth et al., 2024). The evidence here suggests that such results extend beyond hospitals to local management more generally.

8 | DISCUSSION AND FINAL REMARKS

In this paper we studied the relationship between public spending in health, health care provision, and population health outcomes. We did this using a constitutionally defined health spending reform in Brazil. We argue that this paper has provided two contributions to the understanding of how government health spending shapes health outcomes. Firstly, we isolated the effects of a spending shock on downstream health outcomes, and examined the implications of this shock as it flows through the health production function. Secondly, we uncovered non-linearities and input complementarities in the production function of public healthcare, which also allowed us to map margins of spending effectiveness, its potential mediators and constraints. In particular, we empirically documented significant gradients in spending returns due to institutional and management capacity measures.

We traced a chain from spending reform to health spending, from health spending to health inputs and access, and from inputs and access to population level health, principally, infant mortality. We observed that for municipalities spending below the target at baseline, health spending sharply increased, resulting in an expansion of inputs including infrastructure and human resources for health. Access to health care services increased, ultimately

[30] We discuss this measure at more length in Appendix F.2. We observe a significant dispersion of management quality nationally, and both across and within states (Appendix Figure F.2). This measure does not simply proxy income, municipal resources, nor does it correlate considerably with baseline health outcomes (Appendix Figure F.3).

leading to improvements in health, measured by infant mortality rates. For municipalities spending above the target at baseline we observe spending reductions in subsequent years, but weaker contractions in inputs and outputs, and correspondingly, no measurable decline in health outcomes.

We can ask whether the reform pays for itself in terms of lives saved. Combining our average elasticities with the most recent estimates for the value of a statistical life (VSL) in Brazil, which is calculated as 1.16 million USD (2010-adjusted) by Lavetti and Schmutte (2018), suggests that the reform pays for itself, and indeed, would still pay even if the VSL were considerably lower. To see this we consider below-threshold municipalities, which increased health spending. In particular, on average these municipalities increased the proportion of their budget dedicated to health by 7.03% in response to the EC/29 reform. We can use year-specific values (given that spending changes evolved over time) to scale estimated effects on spending and mortality from equation (1). The estimated impacts of the distance to the spending threshold on total spending, scaled by reform-induced changes in spending, suggests that the reform increased spending by around R$8 billion in 2001 to R$60 billion in later years at an aggregate level (around US$2 billion to US$12 billion). While this is a substantial cost increase, if we scale estimated infant mortality effects in an analogous way, this suggests declines of approximately 900 infant deaths 3 years post-reform, up to 3,000 fewer deaths 10 years post reform. Taken together, and combined with the value of statistical life, these figures suggest that the mortality benefits of EC/29 exceed total costs by approximately US$6.7 billion aggregated over all post-reform years. This value is substantial, and although sensitive to the value of VSL used, suggests the reform pays for itself provided any VSL greater than US$640,000. Given the heterogeneity in spending returns, benefits may extend far beyond costs in settings where increases in spending moved along balanced combinations of inputs starting from relatively low baseline levels.

While based on a single setting, these results may be informative for other contexts worldwide. These results are germane to a raft of constitutionally defined health care provisions. For instance, like that of Brazil, constitutions of South Africa, Thailand, Kenya, Rwanda, Colombia, Ghana, The Philippines, Tanzania and Zambia include formal provisions for access to health. More generally, and beyond constitutionally defined health care provision, decentralization of health care to local governments has been embraced as a manner to improve access as well as health system responsiveness. To name just a few examples, Mexico, India, Indonesia, and Colombia have decentralized elements of health care provision or health insurance provision. Moreover, the results suggest that evidence from higher income settings, in which a decoupling is observed between health care spending and health care outcomes, need not be seen as informative for lower income settings with low baseline health expenditure. Rather, our results suggest that increases in health care spending can lead to cost-effective improvements in health outcomes, specifically in settings where healthcare is most needed, where mean life expectancy is generally lower and unmet social demands are greater.

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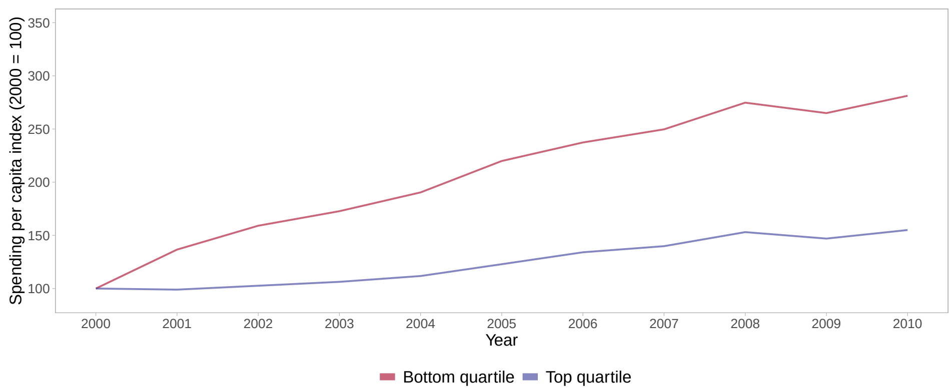
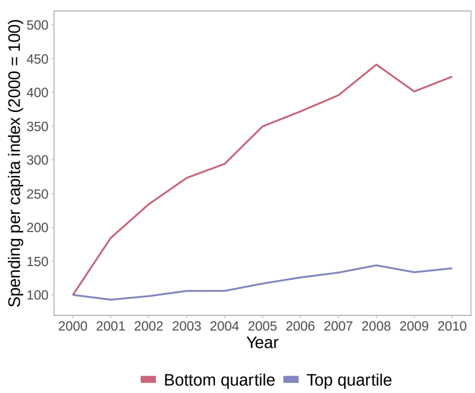
APPENDIX

A | DESCRIPTIVE FIGURES AND SUMMARY STATISTICS

(a) Total Health Spending



(b) Health Spending from Own Resources



(c) Health Spending from Transfers

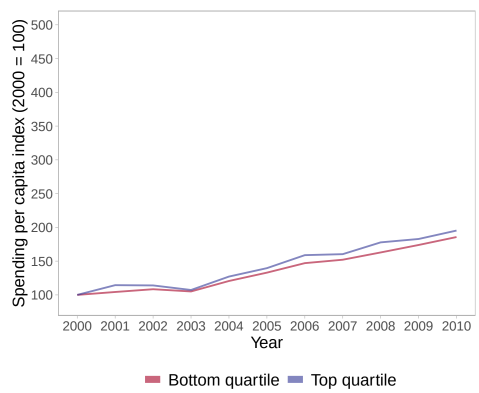


FIGURE A.1 Health Spending Trends. *Notes*: Trends calculated using SIOPS spending data (see Section 3 for more details). In all cases, values in year 2000 are indexed at 100.

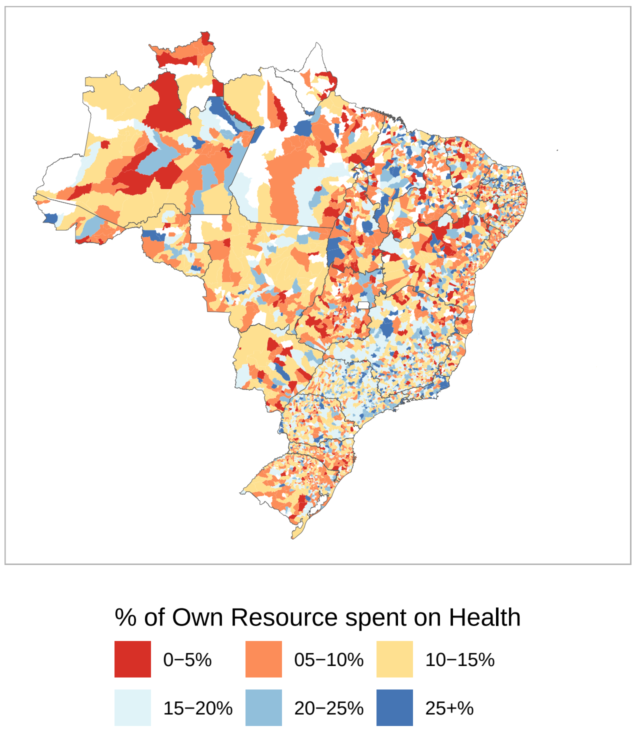


FIGURE A.2 Geographic Variation in Exposure to EC/29 Spending Floors. *Notes*: Baseline health spending as a proportion of total expenditures is plotted at the municipality level. Red, orange and beige colours are municipalities spending below minimum targets imposed by EC/29 ($< 15\%$); blue colours are municipalities spending above minimum targets. Each range indicated in legend labels holds with equality at the lower bracket, and with inequality at the upper end of the bracket. Municipalities are distinguished by shading, and states are distinguished by gray borders.

TABLE A.1 Descriptive Statistics (at baseline)

	Mean	Std. Dev.	Obs.	Source of Data	Coverage
EC 29 Variables					
Own Resource Spent in Health	0.138	0.068	5224	Datasus/SIOPS	1998-2010
Distance to the EC29 Target	0.012	0.068	5224	Datasus/SIOPS	1998-2010
Public Revenue					
Total Revenue per capita	1267.676	711.906	5067	FINBRA	1998-2010
Public Spending					
Total Spending per capita	1252.373	696.065	5067	FINBRA	1998-2010
Spending by Category – per capita					
Health and Sanitation	214.636	134.64	5054	FINBRA	1998-2010
Non-Health Spending	1038.288	600.319	5067	FINBRA	1998-2010
Non-Health Social Spending	584.39	332.338	5067	FINBRA	1998-2010
Non-Social Spending	453.898	313.107	5067	FINBRA	1998-2010
Public Health Spending					
Health Spending per capita	192.138	108.326	5184	Datasus/SIOPS	2000-2010
Health Spending by Source (p.c.)					
Own Resources Spending	119.333	94.518	5184	Datasus/SIOPS	2000-2010
Transfers Spending	72.805	49.949	5184	Datasus/SIOPS	2000-2010
Health Spending by Type (p.c.)					
Personnel Spending	71.291	61.295	5184	Datasus/SIOPS	2000-2010
Investment Spending	14.566	26.687	5184	Datasus/SIOPS	2000-2010
3^{rd} parties services Spending)	32.967	42.602	5184	Datasus/SIOPS	2000-2010
Admin, Management, Other	73.315	52.253	5184	Datasus/SIOPS	2000-2010

Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here capture municipal spending or revenue. Baseline for variables drawn from IBGE/AMS data ("FINBRA") are measured at year 1999 and statistics for all remaining variables ("SIOPS") refer to the baseline year of 2000. Coverage refers to the yearly coverage of each specific data source across our study period (1998–2010).

Table A.1: Descriptive Statistics (at baseline) – *Cont.*

	Mean	Std. Dev.	Obs.	Source of Data	Coverage
Infant Mortality Rate (per 1,000)					
Infant Mortality Rate (all cause)	23.044	26.086	5224	Datasus/SIM	1998-2010
Amenable to Primary Care (APC)	2.09	7.177	5224	Datasus/SIM	1998-2010
non-APC	20.955	22.114	5224	Datasus/SIM	1998-2010
Fetal	0.003	0.08	5224	Datasus/SIM	1998-2010
Within 24h	5.554	10.193	5224	Datasus/SIM	1998-2010
1 to 27 days	13.769	15.865	5224	Datasus/SIM	1998-2010
27 days to 1 year	9.275	16.313	5224	Datasus/SIM	1998-2010
Infectious	2.005	7.149	5224	Datasus/SIM	1998-2010
Respiratory	1.52	4.501	5224	Datasus/SIM	1998-2010
Perinatal	11.107	16.497	5224	Datasus/SIM	1998-2010
Congenital	2.15	5.011	5224	Datasus/SIM	1998-2010
External	0.38	1.959	5224	Datasus/SIM	1998-2010
Nutritional	0.595	3.207	5224	Datasus/SIM	1998-2010
Other	0.882	3.664	5224	Datasus/SIM	1998-2010
Ill-Defined	4.406	10.31	5224	Datasus/SIM	1998-2010

Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here refer to deaths per 1,000 live births. Baseline periods refer to years 1998-1999. Coverage refers to the yearly coverage of each specific data source across our study period (1998–2010).

Table A.1: Descriptive Statistics (at baseline) – *Cont.*

	Mean	Std. Dev.	Obs.	Source of Data	Coverage
Primary Care Coverage					
Extensive Margin (share)					
Population covered by ACS	0.627	0.41	5224	Datasus/SIAB	1998-2010
Population covered by PSF	0.315	0.385	5224	Datasus/SIAB	1998-2010
Intensive Margin (per capita)					
N. of People Visited by PCA	0.273	0.288	5224	Datasus/SIAB	1998-2010
N. of People Visited by ACS	0.119	0.181	5224	Datasus/SIAB	1998-2010
N. of People Visited by PSF	0.153	0.254	5224	Datasus/SIAB	1998-2010
N. of Household Visits and Appointments	1.849	2.571	5224	Datasus/SIAB	1998-2010
N. of Household Visits and Appointments by ACS	1.036	2.171	5224	Datasus/SIAB	1998-2010
N. of Household Visits and Appointments by PSF	0.809	1.524	5224	Datasus/SIAB	1998-2010
Health Human Resources (per capita $\times$ 1,000)					
N. of Health Professionals	5.156	4.889	5224	IBGE/AMS	'99, '02, '05, '09
N. of Doctors	1.567	2.435	5224	IBGE/AMS	'99, '02, '05, '09
N. of Nurses	1.173	1.663	5224	IBGE/AMS	'99, '02, '05, '09
N. of Nursing Assistants	1.25	1.451	5224	IBGE/AMS	'99, '02, '05, '09
N. of Administrative Professionals	1.165	1.267	5224	IBGE/AMS	'99, '02, '05, '09
Primary Care Related Infrastructure & HR					
N. of Health Facilities (per capita$\times$1,000) with:					
Ambulatory Service and ACS Teams	0.14	0.197	5211	Datasus/SIA	1998-2007
Ambulatory Service and Community Doctors	0.083	0.156	5211	Datasus/SIA	1998-2007
Ambulatory Service and PSF Doctors	0.078	0.151	5211	Datasus/SIA	1998-2007
Ambulatory Service and ACS Nurses	0.071	0.155	5211	Datasus/SIA	1998-2007
Ambulatory Service and PSF Nurses	0.076	0.15	5211	Datasus/SIA	1998-2007
Ambulatory Service and PSF Nursing Assistants	0.051	0.124	5211	Datasus/SIA	1998-2007
Ambulatory Production (per capita $\times$ 1000)					
N. Outpatient Procedures	8.824	4.518	5224	Datasus/SIA	1998-2007
N. Primary Care Outpatient Procedures	7.42	3.942	5224	Datasus/SIA	1998-2007
N. Low & Mid Complexity Outpatient Procedures	9.478	5.827	5224	Datasus/SIA	1998-2007
N. High Complexity Outpatient Procedures	0.005	0.052	5224	Datasus/SIA	1998-2007
Access to Health Services (share)					
Prenatal Visits: Unknown	0.043	0.094	5177	Datasus/SINASC	1998-2010
Prenatal Visits: None	0.05	0.073	5155	Datasus/SINASC	1998-2010
Prenatal Visits: 1–6	0.525	0.216	5224	Datasus/SINASC	1998-2010
Prenatal Visits: 7+	0.383	0.235	5224	Datasus/SINASC	1998-2010
Hospitalization (per capita $\times$ 1000)					
Maternal Hospitalization Rate	50.778	36.571	5224	Datasus/SIH	1998-2010
Infant Hospitalization Rate – APC	207.897	256.175	5224	Datasus/SIH	1998-2010
Infant Hospitalization Rate – non-APC	74.183	121.99	5224	Datasus/SIH	1998-2010

Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here are components of indexes measuring health care access, production and inputs. In each case, units for variables are indicated in headings. ACS refers to Community Health Agents. PSF refers to agents in the Programa Saúde da Família. PCA refers to Primary Care Agents. In most cases, per capita figures are reported per all population, with the exception of the maternal hospitalization rate (per female 10-49 year-olds) and infant hospitalization rate (per 0-1 year-olds). APC and non-APC refer to causes amenable to primary care and not amenable to primary care respectively. Coverage refers to the yearly coverage of each specific data source across our study period (1998–2010).

Table A.1: Descriptive Statistics (at baseline) – *Cont.*

	Mean	Std. Dev.	Obs.	Source of Data	Coverage
Health System (per capita $\times$ 1000)					
N. of Municipal Hospitals	0.06	0.139	5224	IBGE/AMS	‘99, ‘02, ‘05, ‘09
N. of Federal and State Hospitals	0.014	0.079	5224	IBGE/AMS	‘99, ‘02, ‘05, ‘09
N. of Private Hospitals	0.03	0.059	5224	IBGE/AMS	‘99, ‘02, ‘05, ‘09
Private Insurance Coverage	0.047	0.088	5129	Datasus/SIOPS	2000-2010
Adult Hospitalization (per capita $\times$ 1000)					
Adult Hospitalization	359.734	223.819	5224	Datasus/SIH	1998-2010
Adult Hospitalization – APC	132.108	90.474	5224	Datasus/SIH	1998-2010
Adult Hospitalization Rate – non-APC	227.626	159.116	5224	Datasus/SIH	1998-2010
Adult Mortality (per capita $\times$ 1000)					
Adult Mortality	14.653	5.367	5224	Datasus/SIM	1998-2010
Adult Mortality – APC	3.951	2.43	5224	Datasus/SIM	1998-2010
Adult Mortality – non-APC	10.702	4.141	5224	Datasus/SIM	1998-2010
Hospitalization Flows (per capita $\times$ 1000)					
Total Hospitalization Inflow	10.359	25.642	5224	Datasus/SIH	1998-2010
Inflow Amenable to Primary Care	2.988	8.483	5224	Datasus/SIH	1998-2010
Inflow Not Amenable to Primary Care	7.371	19.869	5224	Datasus/SIH	1998-2010
Total Hospitalization Outflow	40.329	55.369	5224	Datasus/SIH	1998-2010
Outflow Amenable to Primary Care	9.583	14.228	5224	Datasus/SIH	1998-2010
Outflow Not Amenable to Primary Care	30.745	43.026	5224	Datasus/SIH	1998-2010

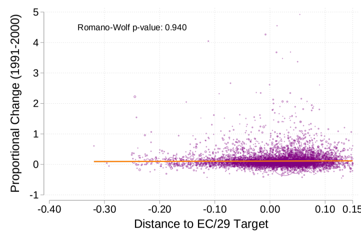
Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here are components of indexes measuring health care access, production and inputs. In each case, units for variables are indicated in headings. In most cases, per capita figures are reported per all population, with the exception of the infant hospitalization rate (per 0-1 year-olds). APC and non-APC refer to causes amenable to primary care and not amenable to primary care respectively. Coverage refers to the yearly coverage of each specific data source across our study period (1998–2010).

Table A.1: Descriptive Statistics (at baseline) – *Cont.*

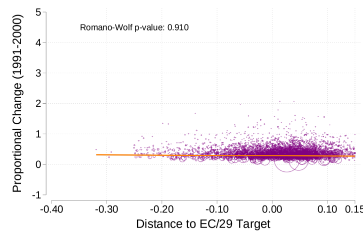
	Mean	Std. Dev.	Obs.	Source of Data	Coverage
Controls					
Baseline Socioeconomic Controls					
Life Expectancy	68.543	3.931	5224	IBGE/Census	1998-2010
Expected Years of Study	8.416	1.772	5224	IBGE/Census	1998-2010
Iliteracy Rate (above 18y old)	23.175	13.439	5224	IBGE/Census	1998-2010
Income per capita	345.061	192.944	5224	IBGE/Census	1998-2010
Share of Population Below Poverty Line	0.401	0.226	5224	IBGE/Census	1998-2010
Gini Coefficient	0.546	0.068	5224	IBGE/Census	1998-2010
Access to Sewage Network	0.26	0.305	5224	IBGE/Census	1998-2010
Access to Garbage Collection Service	0.546	0.267	5224	IBGE/Census	1998-2010
Access to Water Network	0.59	0.239	5224	IBGE/Census	1998-2010
Access to Electricity	0.876	0.161	5224	IBGE/Census	1998-2010
Urbanization Rate	0.606	0.228	5224	IBGE/Census	1998-2010
Time-varying controls					
GDP per capita (2010 R$)	9.754	11.417	5224	IBGE	1998-2010
Bolsa Familia transfers per capita	0	0	5224	Min. D.S.	1998-2010
Fiscal controls					
Average Neighbor Health Spending p.c.	208.465	124.004	5222	FINBRA	1998-2010
Human Resources Spending (/Revenue)	0.415	0.107	5099	FINBRA	1998-2010
Weighting					
Population (1,000s)	29.667	181.178	5224	IBGE/Census	1998-2010
Other Baseline Measures					
Mayor's Victory Margin (2000)	18.50	19.78	5217	TSE	1998-2010
Mayor's Education Level (years)	11.53	4.00	5219	MUNIC 2003	1998-2010
Councillor's Education	0.66	0.26	5160	MUNIC 2003	1998-2010
Government Regulatory Plan	0.45	0.50	5224	MUNIC 2003	1998-2010
Corruption (1=yes, 0=no)	0.34	0.31	1062	CGU	1998-2010
Corruption (1=yes, 0=no/no audit)	0.14	0.35	5224	CGU	1998-2010
Management Index	3.67	0.56	5224	Min. P&B	1998-2010

Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here are controls included in certain models (top panel) or in heterogeneity and policy analysis conducted in Section 7 (top and bottom panel). Baseline for variables drawn from the census are measured with the 2000 census, while statistics from FINBRA are measured at 1999. Election data is at 2000, and the MUNIC survey was released in 2003, but collected principally in 1999-2001. Corruption audits all occur from 2003 and beyond. Spending is measured in 2010 R$ unless otherwise indicated. Human resource spending refers to the proportion of total municipal revenue dedicate to human resources. Min. D.S. refers to the Ministry of Social Development, and Min. P&B refers to the Ministry of Planning and Budget. Coverage refers to the yearly coverage of each specific data source across our study period (1998–2010). As measures are fixed by municipalities over time, coverage is indicated as 1998–2010.

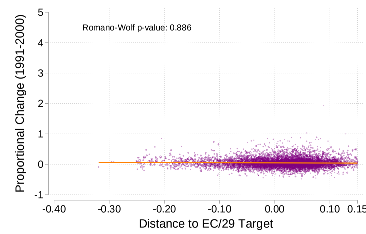
(a) Years of Education



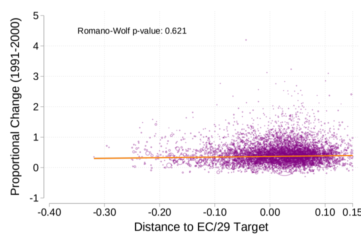
(b) Human Development Index



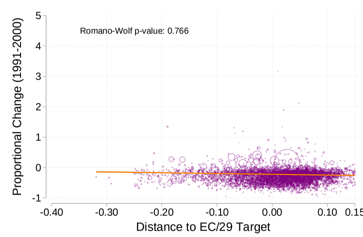
(c) Gini coefficient



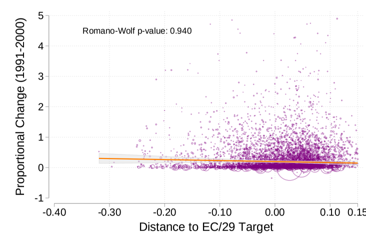
(d) Income Per Capita



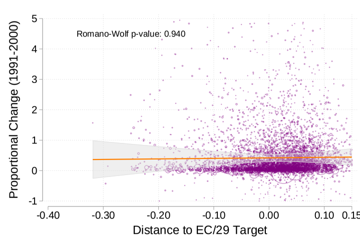
(e) Poverty



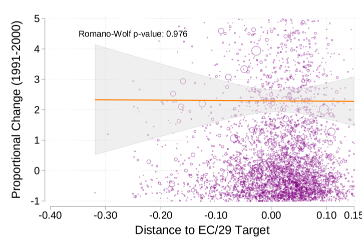
(f) Electricity



(g) Access to Water



(h) Wastewater Network Access



(i) Garbage Collection

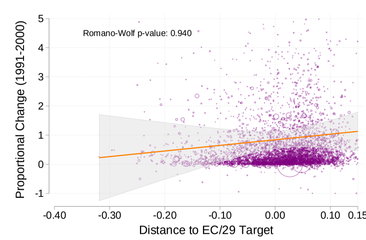


FIGURE A.3 Distance to Spending Target at Baseline and Municipal Characteristics. *Notes:* Each plot presents the raw correlation between each municipality's distance to the EC/29 spending target at baseline, and proportional changes in municipal level characteristics measured from the 2000 and 1991 censuses. On the vertical axis, changes in rates of measures indicated in the plot title between 1991 and 2000 (all pre EC/29 adoption) are presented and on the horizontal analysis, distance to the EC-29 target is presented (positive values imply spending below the target). Each point is a single municipality. Linear fits are presented as orange lines with 95% CIs presented as shaded areas. In the interests of presentation, a very small number of points with proportional changes above 500% are omitted from scatter plots, but are not omitted from formal tests or regression fit lines. Romano-Wolf p-values provide the multiple-hypothesis corrected p-value of the bivariate regression plotted on the graph, and are based on 500 bootstrap replicates.

TABLE A.2 Definitions of Indexes

Index	Sub-Index	Variables
1. **Access & Production of Health Index**	1a. *Primary Care Access & Production Index*	Population covered by Community Health Agents
		Population covered by Family Health Agents
		N. of People Visited by Primary Care Agents (pc)
		N. of People Visited by Community Health Agents (pc)
		N. of People Visited by Family Health Agents (pc)
		N. of Household Visits and Appointments (pc)
		N. of Household Visits and Appointments from Community Health Agents (pc)
		N. of Household Visits and Appointments from Family Health Agents (pc)
		N. of Health Facilities with Ambulatory Service and ACS Teams (pc)
		N. of Health Facilities with Ambulatory Service and Community Doctors (pc)
		N. of Health Facilities with Ambulatory Service and ACS Nurses (pc)
		N. of Health Facilities with Ambulatory Service and PSF Teams (pc)
		N. of Health Facilities with Ambulatory Service and PSF Doctors (pc)
		N. of Health Facilities with Ambulatory Service and PSF Nursing Assistants (pc)
		N. Primary Care Outpatient Procedures (per capita)
		Proportion of births with unknown prenatal care coverage
		Proportion of births with 0 prenatal visits‡
		Proportion of births with 1-6 prenatal visits‡
		Proportion of births with 7+ prenatal visits‡
	1b. *Non-Primary Care Access & Production Index*	N. Non-Primary Care Outpatient Procedures (per capita) (pc)
		Maternal Hospitalization Rate
		Infant Hospitalization Rate - non-APC
2. **Health Inputs Index**	2a. *Human Resources Index*	N. of Doctors (pc)
		N. of Nurses (pc)
		N. of Nursing Assistants (pc)
		N. of Administrative Professionals (pc)
	2b. *Hospitals Index*	N. of Municipal Hospitals (pc)
		N. of Federal and State Hospitals (pc)

Notes: Main indexes and sub-indexes are constructed from variables listed here, in each case following Anderson (2008). The abbreviation pc refers to per capita. Each variable is included in one and only one index, or one and only one sub-index. ‡ Variable has been multiplied by minus 1 such that higher values refer to better outcomes from a public health policy point of view. ‡‡ All maternal and infant hospital admissions computed in these indicators refer to conditions that are not amenable to primary care services, thus indicating improved access and referral to healthcare services (e.g. as discussed in Bhalotra et al., 2019).

B | ADDITIONAL RESULTS – SINGLE-COEFFICIENT AND DYNAMIC ESTIMATES

B.1 | Fiscal Reactions

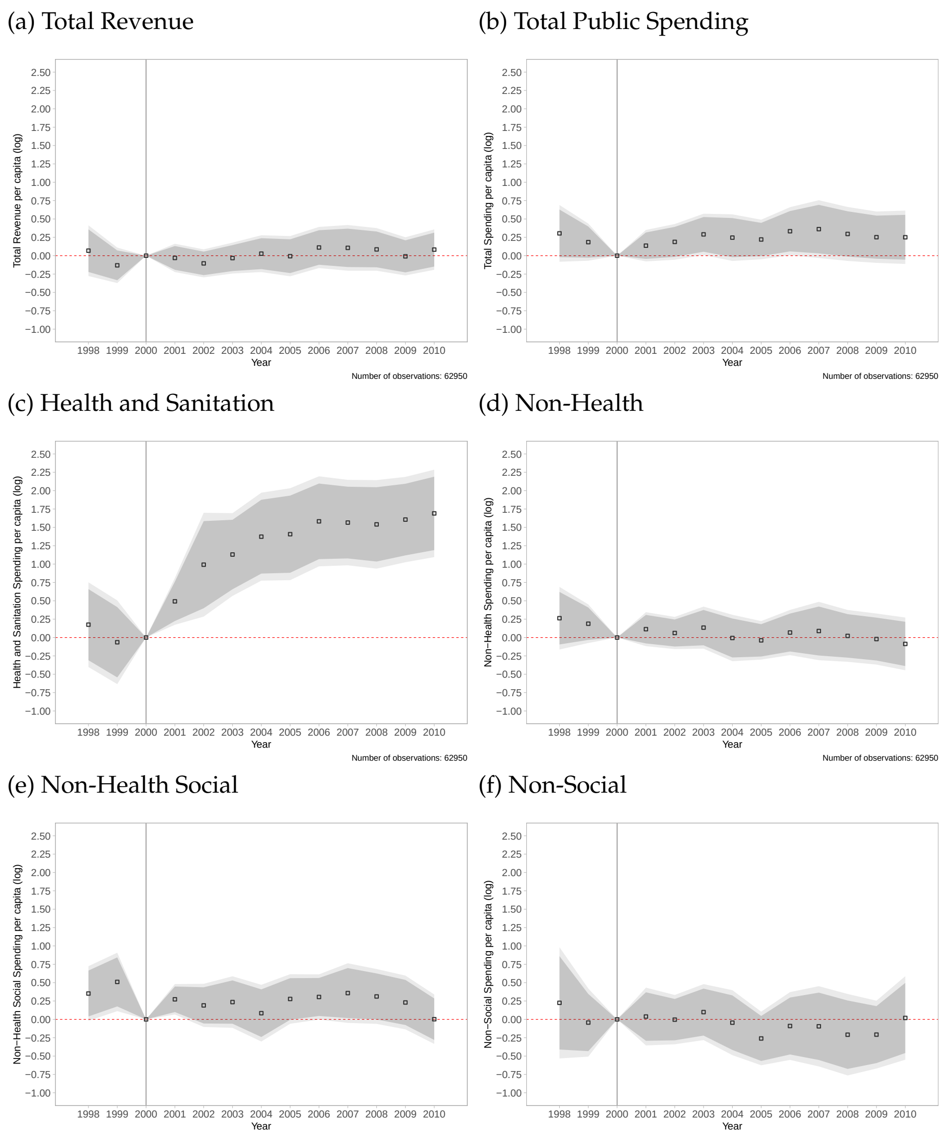


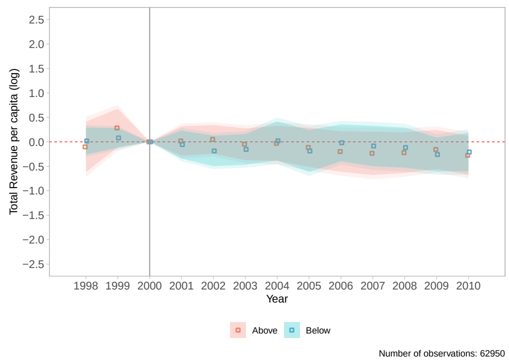
FIGURE B.1 Dynamic Effects on Revenues and Spending by Aggregate Classes (FINBRA). *Notes:* Refer to Notes to Figure 3. Identical models are estimated, however here considering total revenue and spending per capita. Estimation is based FINBRA spending and revenue data.

TABLE B.1 Fiscal Reactions – Robustness to Exclusion of Data Quality Check

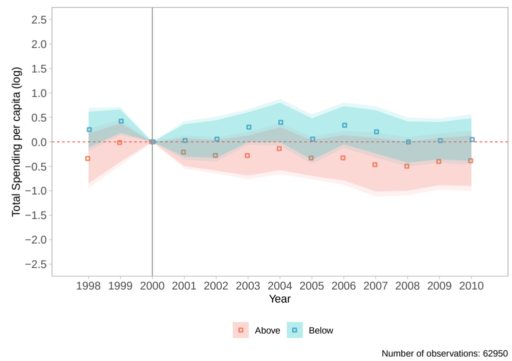
	With Data Quality Control				Without Data Quality Controls
	(1)	(2)	(3)	(4)	(5)
Panel A: FINBRA					
Total Revenues	-0.118	0.001	0.040	0.063	0.040
	(0.139)	(0.117)	(0.112)	(0.112)	(0.112)
Total Spending	-0.039	0.071	0.110	0.092	0.110
	(0.137)	(0.115)	(0.111)	(0.111)	(0.111)
Health Spending	1.109***	1.236***	1.282***	1.232***	1.281***
	(0.250)	(0.227)	(0.225)	(0.224)	(0.225)
Non-Health Spending	-0.234*	-0.134	-0.097	-0.111	-0.097
	(0.130)	(0.109)	(0.105)	(0.105)	(0.105)
Non-Health Social Spending	-0.112	-0.058	-0.030	-0.049	-0.030
	(0.163)	(0.136)	(0.134)	(0.133)	(0.134)
Non-Social Spending	-0.300*	-0.170	-0.124	-0.135	-0.124
	(0.174)	(0.147)	(0.141)	(0.141)	(0.141)
Observations (Each cell)	62950	62950	62950	62886	62950
Panel B: SIOPS					
Total Health Spending	2.200***	2.303***	2.328***	2.316***	2.329***
	(0.248)	(0.195)	(0.185)	(0.208)	(0.186)
From Own Resources	5.430***	5.473***	5.501***	5.487***	5.503***
	(0.271)	(0.260)	(0.248)	(0.261)	(0.248)
From Other Resources	1.594	1.558	1.558	1.590	1.559
	(1.561)	(1.309)	(1.298)	(1.316)	(1.299)
Personnel	2.544***	2.581***	2.600***	2.564***	2.603***
	(0.428)	(0.365)	(0.364)	(0.370)	(0.365)
Investment	5.691***	5.353***	5.358***	5.304***	5.360***
	(1.044)	(0.744)	(0.738)	(0.752)	(0.739)
Outsourced (3rd party services)	0.771	1.117*	1.150**	1.123*	1.152**
	(0.695)	(0.606)	(0.580)	(0.640)	(0.580)
Admin, Management and Others	4.418***	4.308***	4.332***	4.355***	4.331***
	(1.081)	(0.975)	(0.972)	(0.990)	(0.972)
Observations (Each cell)	54622	54622	54622	53685	54622
Data Quality Control	Y	Y	Y	Y	N
Municipal FE & Time-State FE	Y	Y	Y	Y	Y
Baseline Socioeconomic Controls × Time	N	Y	Y	Y	Y
Time-Varying Controls	N	N	Y	Y	Y
Fiscal Controls	N	N	N	Y	N

Notes: Refer to Notes to Table 1. Identical models are presented, with an additional column removing data quality controls. All other details follow those described in Table 1. * p<0.10; ** p < 0.05; *** p <0.01.

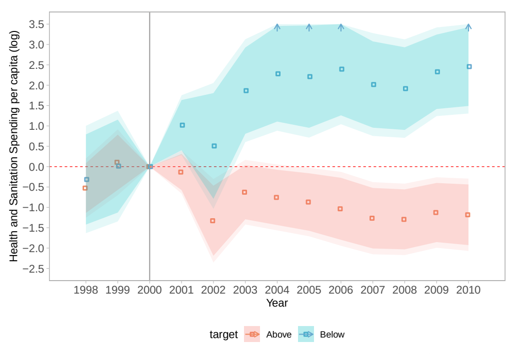
(a) Total Revenue



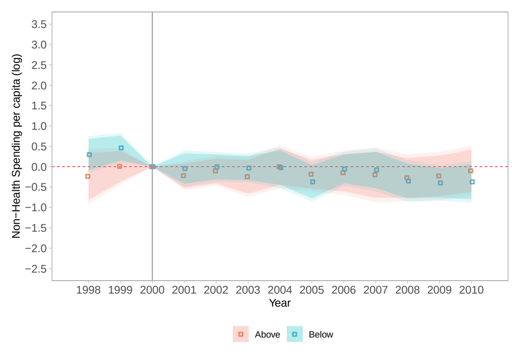
(b) Total Public Spending



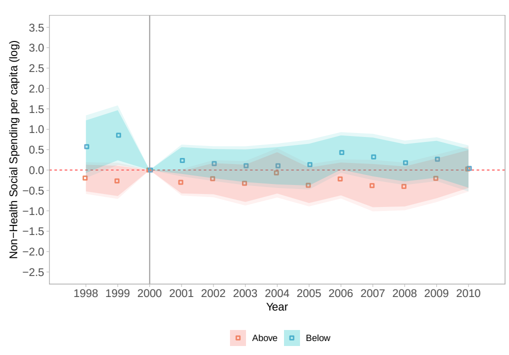
(c) Health and Sanitation



(d) Non-Health



(e) Non-Health Social



(f) Non-Social

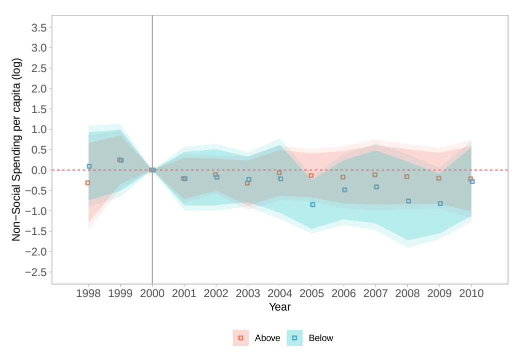


FIGURE B.2 Dynamic Effects on Revenues and Spending (Distributional Effects). *Notes:* Refer to Notes to Figure 4. Identical models are estimated, however here considering total revenue and spending per capita. Estimation is based on FINBRA spending and revenue data.

B.2 | Infant Mortality: Additional Analysis of Data Quality and Pre-Trends

B.2.1 | Measurement of Infant Mortality Prior to 1998

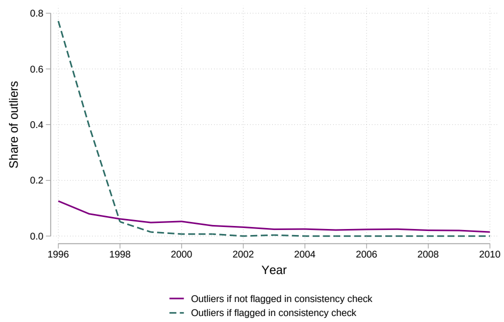
Our analysis considers the 1998-2010 period, allowing for three years for inspection of pre-trends (1998-2000). In this section we extend the analysis until 1996, the year when the National System of Mortality Records (SIM) adopted the ICD-10 classification of cause of deaths. While existing research indicate that the quality of data on vital statistics in Brazil indeed improved from 1996 onward, descriptive evidence, qualitative interviews with experts as well as examination of the raw data indicate concerns related to data quality in initial years, particularly in 1996 and 1997. Szwarcwald et al. (2002), for instance, document that the coverage of death records decreased from 1996 to 1998 in the most developed regions of Brazil, where data should supposedly be of higher quality, thus pointing to concerns related to SIM records in these initial years. Given these concerns, in what follows we examine ways of identifying data quality issues and of improving the measurement of mortality rates as an effort to provide an extended analysis of pre-trends from 1996 onward.

B.2.2 | Identifying Data Quality and Extending the Pre-Trend Analysis

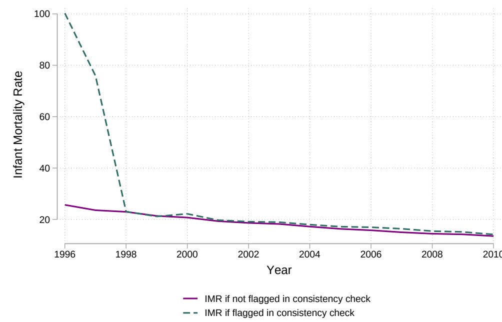
As a first method of examining data quality issues and to remove sample observations with abnormal values we consider the relative variation of IMR over time. We first identified municipalities with abnormally high variations in IMR across years by calculating the standard deviation of IMR *within-municipalities*. While we would expect to observe substantial variation in IMR both across municipalities and over time, we should not observe abnormal variation in IMR from year to year for a given municipality. We thus estimated the within-municipality IMR standard deviation (SD) for the whole sample, and flagged the municipalities above the 95th percentile of the SD distribution.

Figure B.3 panel (a) plots by year the share of municipalities with IMR values greater than 2 standard deviations from the mean (within municipalities), and the same measure for the group of municipalities that were flagged through the procedure described above. We observe that outliers were far more common in 1996 and 1997 (green line) exactly for those municipalities which were flagged as having questionable measures in early years based on the extreme variability of rates over time. For instance, among municipalities with abnormal variation, nearly 80% of those had abnormally high mortality rates exactly in 1996 – with average IMR for this group of municipalities reaching an implausible figure of 1,964 per 1,000 live births in that year, *versus* an average of 28.9 for the rest of the sample. A relatively high proportion is also observed for 1997, then followed by a relatively flat and low trend from 1998 onward.

Second, we also identified municipalities with abnormally high variations in IMR across years by looking directly at year-to-year percentage variation in mortality rates. To avoid extremely high variations from small municipalities, for which we may observe infant mortality rates switching from zero to 1,000, or vice-versa, we dropped from the sample those municipalities that recorded zero birth, IMR equal to zero or equal to 1,000 in at least one year from 1998 onward, when data quality can be considered higher. We then calculated the year-to-year percentage variation in IMR and flagged those municipalities with any extreme variations, i.e., we marked those above the 95th percentile ($\geqslant 1.75$ in $\Delta \ln(IMR_t) - \ln(IMR_{t-1})$) or below the 5th percentile ($\leqslant -1.23$ in $\Delta \ln(IMR_t) - \ln(IMR_{t-1})$) of the distribution of the IMR annual percentage variation. Figure B.3 panel (b) plots by year the IMR for those municipalities with ($n = 1,243$) *versus* without ($n = 1,134$) extreme variations. We observe abnormal values exactly in 1996 and 1997 among those municipalities with extreme variations, then once again followed by a relatively flat and lower trend from 1998 onward.



(a) Outliers Based on IMR Variation

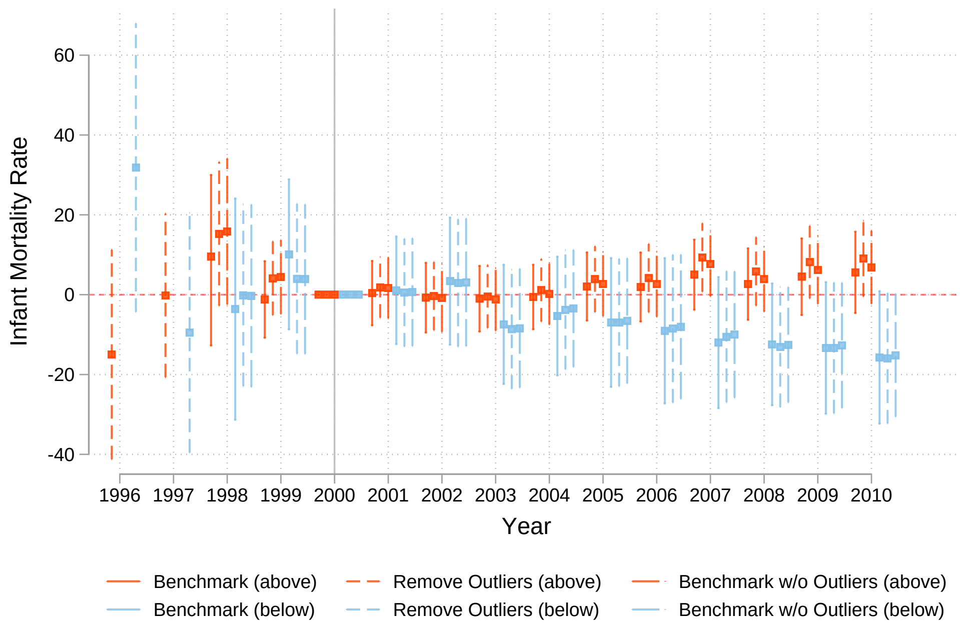


(b) IMR by Outlier Status

FIGURE B.3 Identification of Municipalities with Data Quality Issues. *Notes*: Plots document rates of irregular variation in IMR (left-hand panel), and rates of IMR by municipalities flagged as being highly variable or not highly variable (right hand panel). These correspond to the first and second methods discussed to identify irregular rates of infant mortality respectively.

Next, in Figure B.4 we extend our analysis to also consider the 1996-1997 years. Figure B.4 panel (a) plots the estimates from our benchmark specification for three different samples: (1) our benchmark sample, which starts in 1998 and covers all municipalities; (2) our alternative sample, which starts in 1996 and remove those municipalities with abnormal observations as identified based on the first method laid out above (275 out of 5507 municipalities were removed); (3) our sample that starts in 1998, but that excludes the same municipalities as in (2). In Figure B.4 panel (b) we follow an analogous series of specifications, but now in steps (2) and (3) exclude municipalities with abnormal variation in IMR based on the second procedure described above. Overall, we observe some scattered variation and large standard errors for the 1996-1997 period, even upon the removal of outliers, but without any systematic pattern. This is then followed by estimates around zero and smaller standard deviations just around the pre-reform years, and patterns similar to our benchmark estimates thereafter.

(a) Outlier Check Based on Within-Municipality Variation



(b) Outlier Check Based on Year-to-Year Variation

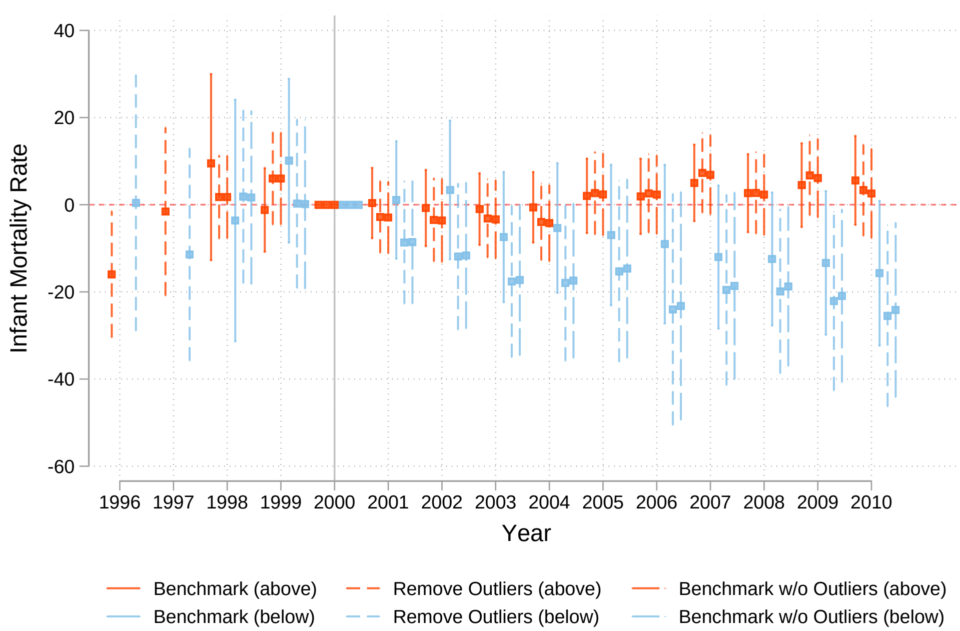


FIGURE B.4 IMR Analysis: Extended Pre-trends. *Notes*: Refer to Notes to Figure 5. Identical models are estimated, however here extending the sample for 1996 and 1997 and conducting data quality checks as laid out above. Point estimates are presented as dots, and 95% confidence intervals are presented as vertical lines. Line types indicate the regression samples.

B.3 | Infant Mortality Rates: Elasticities and Mortality by Cause of Death

B.3.1 | Mortality by Cause of Death

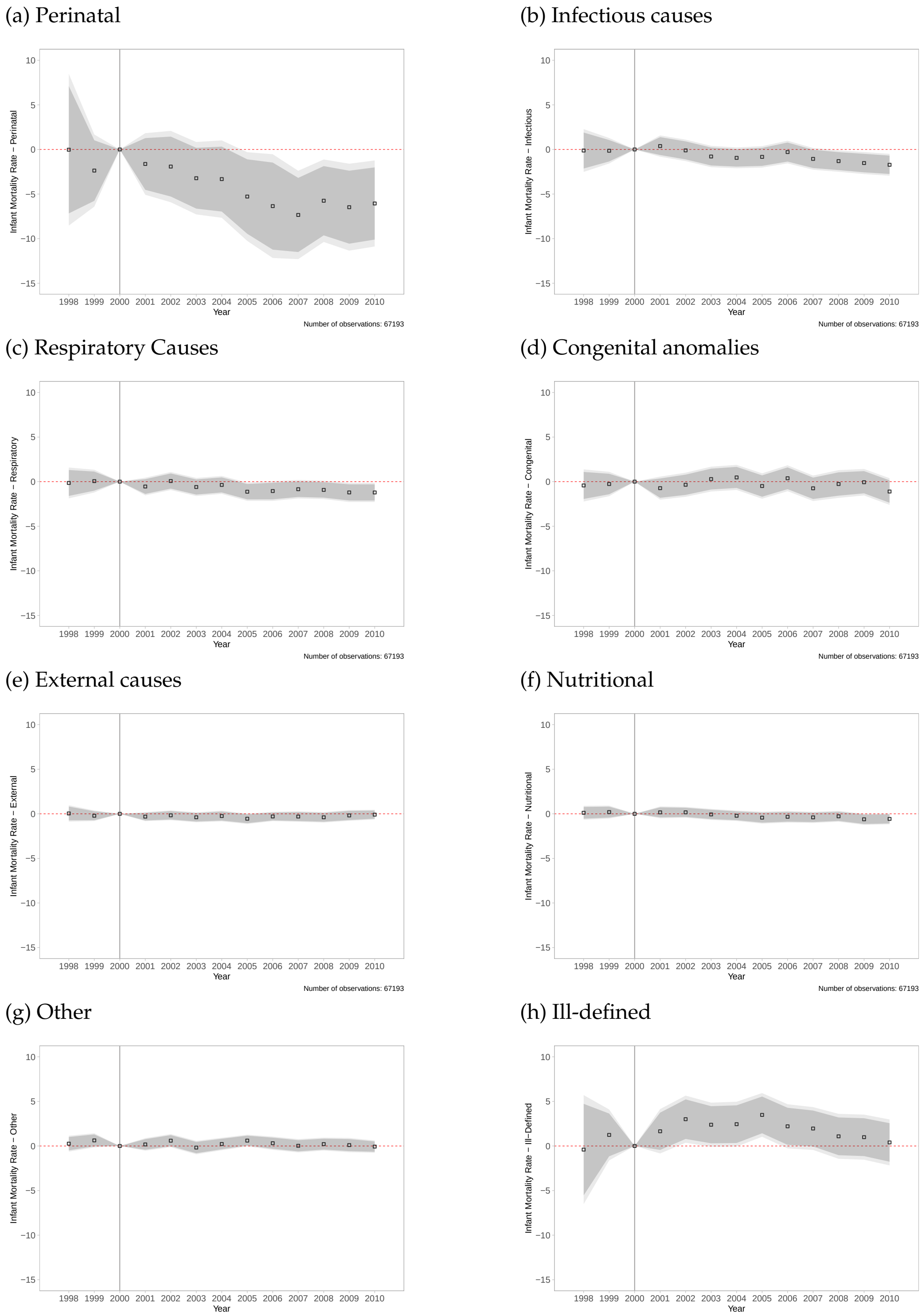
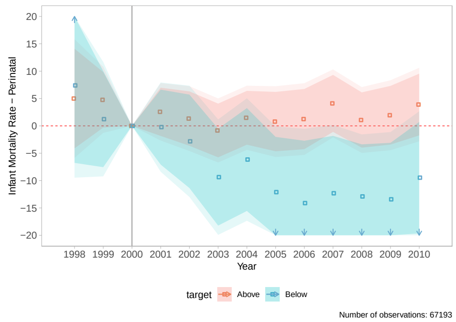
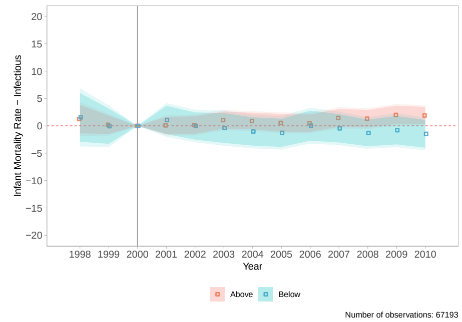


FIGURE B.5 Infant Mortality and Public Health Spending (By cause). *Notes*: Refer to Notes to Figure 5. Identical models are estimated, however here examining rates of mortality by specific (mutually exclusive) mortality classes. Point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively.

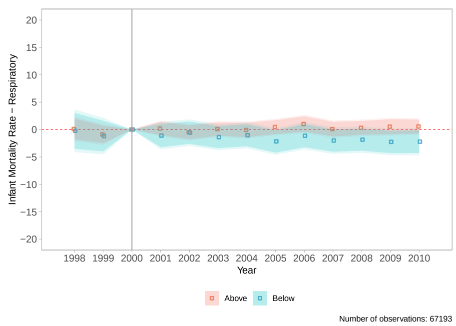
(a) Perinatal



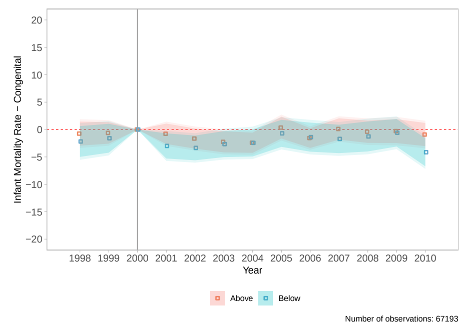
(b) Infectious causes



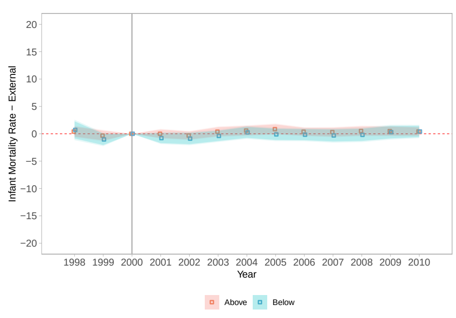
(c) Respiratory Causes



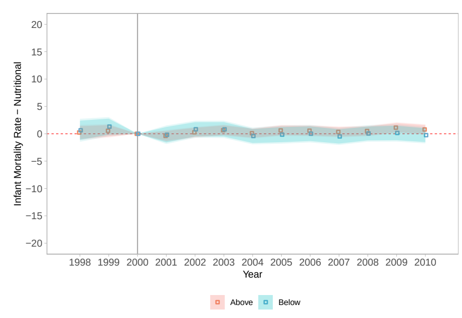
(d) Congenital anomalies



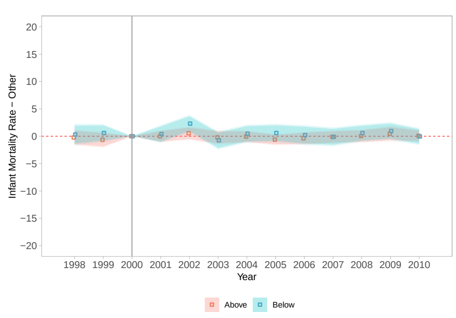
(e) External causes



(f) Nutritional



(g) Other



(h) Ill-defined

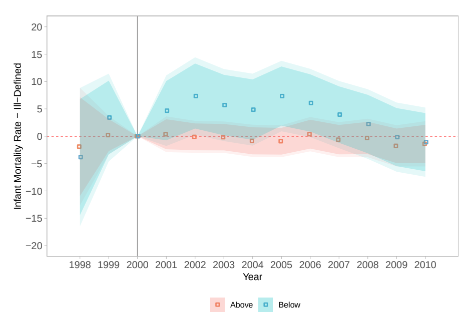


FIGURE B.6 Infant Mortality and Public Health Spending (By cause): Above and Below Threshold Effects. *Notes*: Refer to Notes to Figure 5. Identical models are estimated, however here examining rates of mortality by specific (mutually exclusive) mortality classes.

B.3.2 | Elasticity Calculations

In Section 5.2.3 we report elasticity estimates to measure proportional changes in spending and infant mortality as a result of EC/29. To do so requires estimates of the impact of EC/29 on both spending and infant mortality. Each of these quantities is directly estimated in equation (1) at various post reform years $j = 2001, \ldots, 2010$. By scaling estimated reform effects on health with estimated reform effect on spending, we isolate a time-specific elasticity defined as follows:

$$
\begin{aligned}
\text{Elasticity}_j &\equiv \frac{\left(\frac{\partial \text{IMR}_{mts}}{\partial \text{Dist}_{m,pre} \times \text{EC29}_{t+j}}\right) / \text{IMR}_{pre}}{\left(\frac{\partial \text{Health Spending}_{mts}}{\partial \text{Dist}_{m,pre} \times \text{EC29}_{t+j}}\right) / \text{Health Spending}_{pre}} \\
&= \frac{\left(\partial \text{IMR}_{mts} / \text{IMR}_{pre}\right)\big|_{t=j}}{\left(\partial \text{Health Spending}_{mts} / \text{Health Spending}_{pre}\right)\big|_{t=j}} \qquad (6)
\end{aligned}
$$

Note that this elasticity is explicitly dependent on the reform effect at time j, and needs not be constant across j. Time variation of elasticity estimates may occur given that at different horizons the reform affects spending at different margins, which may have larger or smaller effects on health outcomes like infant mortality. Effects of increases in spending have also been observed to vary by time, potentially reflecting delays between investments in lumpy health inputs such as infrastructure and human capital being complete, and hence reflected in outputs. Similarly, health effects may accumulate over time as past health spending has inter-temporal spillovers, allowing municipalities to enter improved paths for health outcomes. The quantities in parentheses in the numerator and denominator in equation (6) are simply estimated effects of the EC 29 reform estimated from equation (1). These are scaled by baseline values of these measures to estimate a proportional change in infant mortality, and health spending. Elasticities are then estimated by scaling these two proportional changes. Along with point estimates of elasticities estimated following equation (6), we present confidence intervals on these estimates. These confidence intervals are estimated by block bootstrap where municipalities are resampled, the numerator and denominator of equation (6) are re-estimated, along with baseline outcomes for the resampled units, and the elasticity is then re-estimated. The 95% confidence intervals are then constructed as the point estimate $\pm$ 1.96 $\times$ the standard deviation of estimated bootstrap resamples.

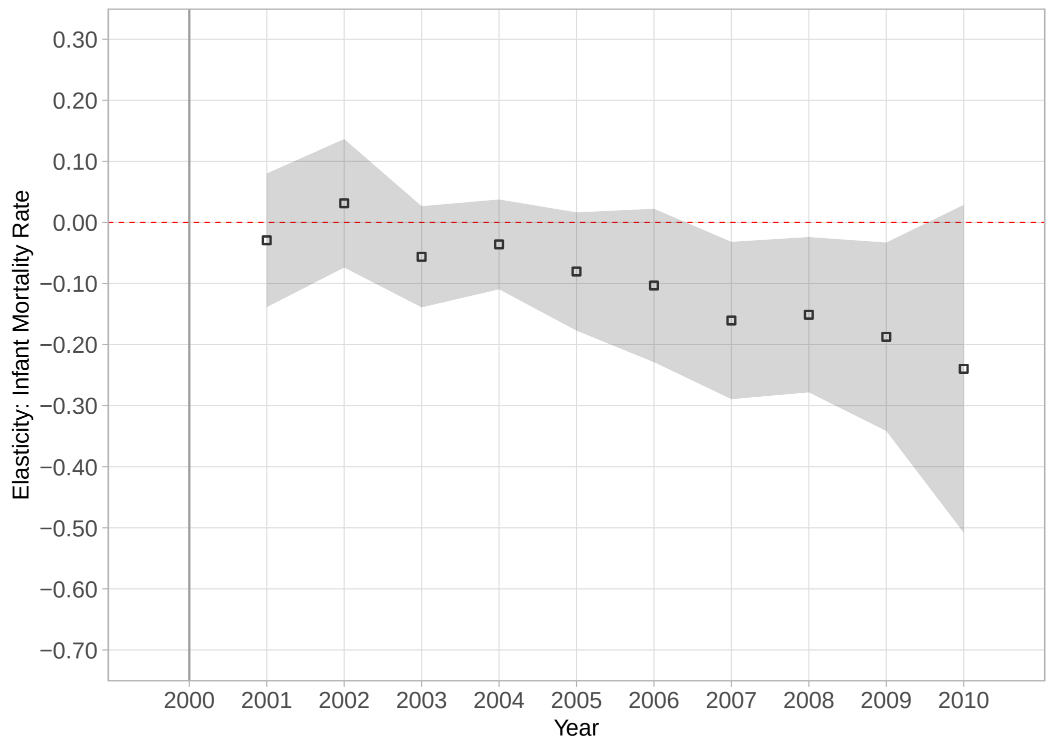


FIGURE B.7 Elasticity Estimates for Infant Mortality. *Notes*: Elasticity estimates are plotted (black squares) along with their 95% CIs (grey shaded area). Elasticities are presented over all post-reform years studied (2001-2010), capturing reform-mediated effects at various horizons. Elasticity estimates are calculated following equation (6), with components estimated following equation (1). Standard errors are calculated by block (clustered) bootstrap resampling accounting for uncertainty in both elements of elasticity, with 500 bootstrap resamples.

TABLE B.2 *Back of the Envelope* Infant Mortality Rates Elasticity

	Health and Sanitation Spending (FINBRA)				Health Spending (SIOPS)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Infant Mortality Rate								
Total	-0.121	-0.100	-0.087	-0.072	-0.249	-0.200	-0.154	-0.134
Amenable to Primary Care	-0.034	-0.158	-0.151	-0.166	-0.069	-0.314	-0.266	-0.309
Non-Amenable to Primary Care	-0.130	-0.095	-0.081	-0.063	-0.267	-0.188	-0.143	-0.116
By Timing								
Fetal	-1.123	-1.022	-1.024	-1.087	-2.305	-2.034	-1.801	-2.021
Within 24 hours	-0.155	-0.148	-0.146	-0.163	-0.317	-0.295	-0.258	-0.302
1 to 27 days	-0.141	-0.102	-0.090	-0.094	-0.290	-0.202	-0.158	-0.175
27 days to 1 year	-0.091	-0.099	-0.084	-0.040	-0.187	-0.196	-0.148	-0.074

Notes: Elasticity of Infant Mortality is estimated following (6), based on aggregate single coefficient estimates of EC/29 impacts on infant mortality and health spending following (3). Alternative columns correspond to control sets indicated in Table 1, and measures of health spending calculated from FINBRA (columns 1-4), and SIOPS (columns 5-8).

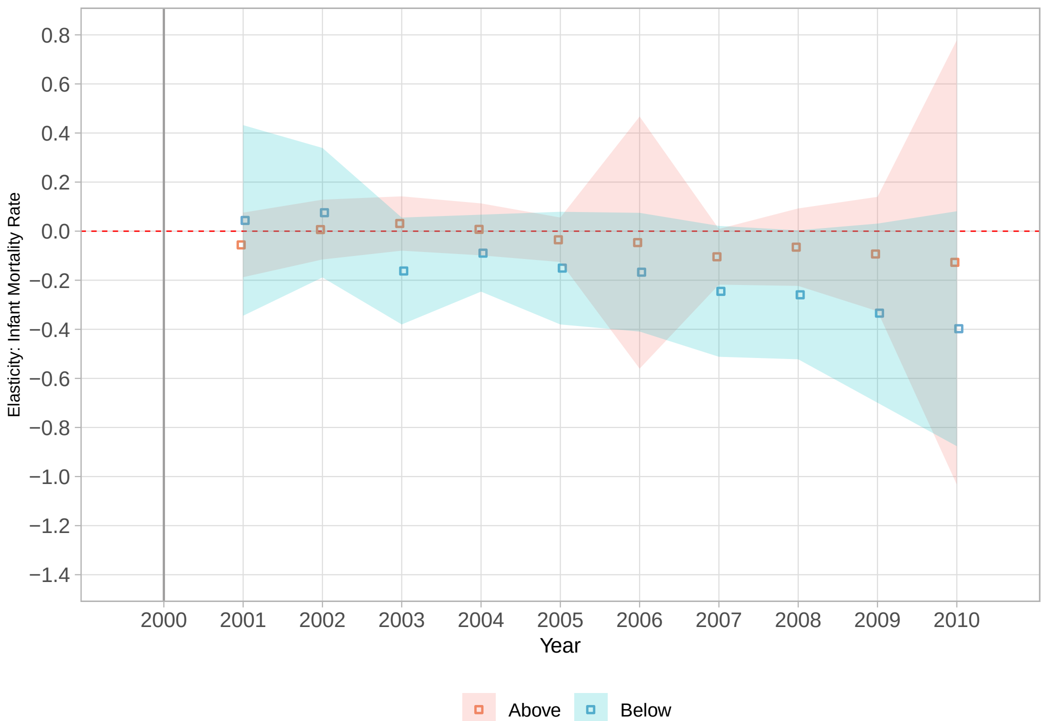
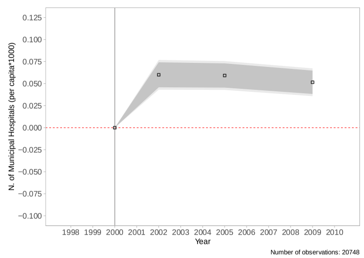


FIGURE B.8 Distributional Elasticity Estimates: Infant Mortality. *Notes*: Back of the envelope elasticity estimates are plotted for above and below spending threshold municipalities along with their 95% CIs (red for above threshold municipalities, and blue for below threshold municipalities). Elasticities are presented over all post-reform years studied (2001-2010), capturing reform-mediated effects at various horizons. Elasticity estimates are calculated following (6), with both spending and infant mortality estimates being group-specific to above and below threshold municipalities, estimated following (2). Standard errors are calculated from block (clustered) bootstrap accounting for uncertainty in both elements of elasticity, with 500 bootstrap resamples.

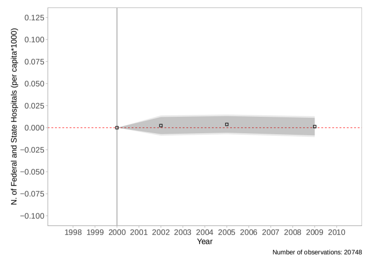
C | DISCUSSION ON OTHER PATHWAYS

Pooled Estimates

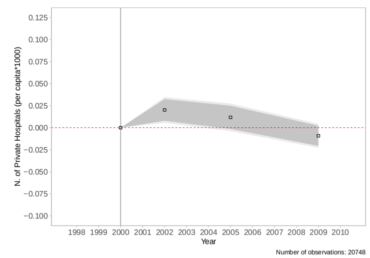
(a) Municipal Hospitals



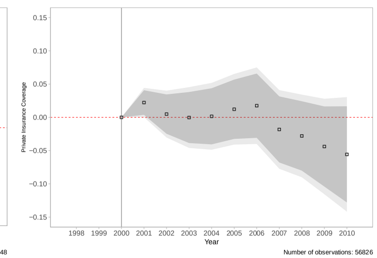
(b) State and Federal Hospitals



(c) Private Hospitals

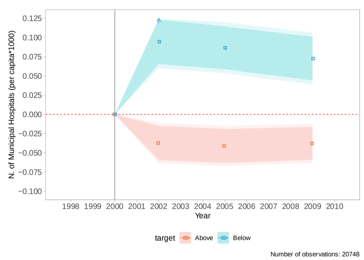


(d) Private Insurance

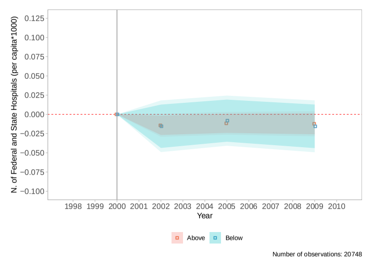


Distributional Estimates

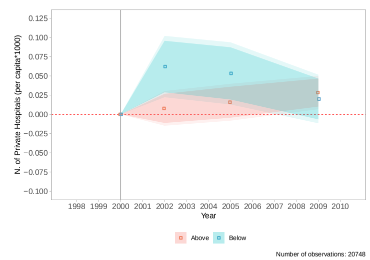
(e) Municipal Hospitals



(f) State and Federal Hospitals



(g) Private Hospitals



(h) Private Insurance

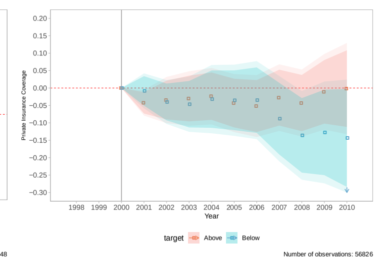


FIGURE C.1 Spending Reform and Health System Spillovers. Pooled Estimates. *Notes*: Panels (a) to (d) present estimates from (1), and panel (e) to (h) present estimates from (2). In each specification lags and leads to the EC/29 passage are presented, controlling for data quality, and baseline socioeconomic controls from the Census interacted with time trends. Panels (a) to (d) present global estimates from spending shifts, where point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Panels (e) to (h) present spending impacts separating by municipalities located below and above the spending threshold (15%) at baseline. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality.

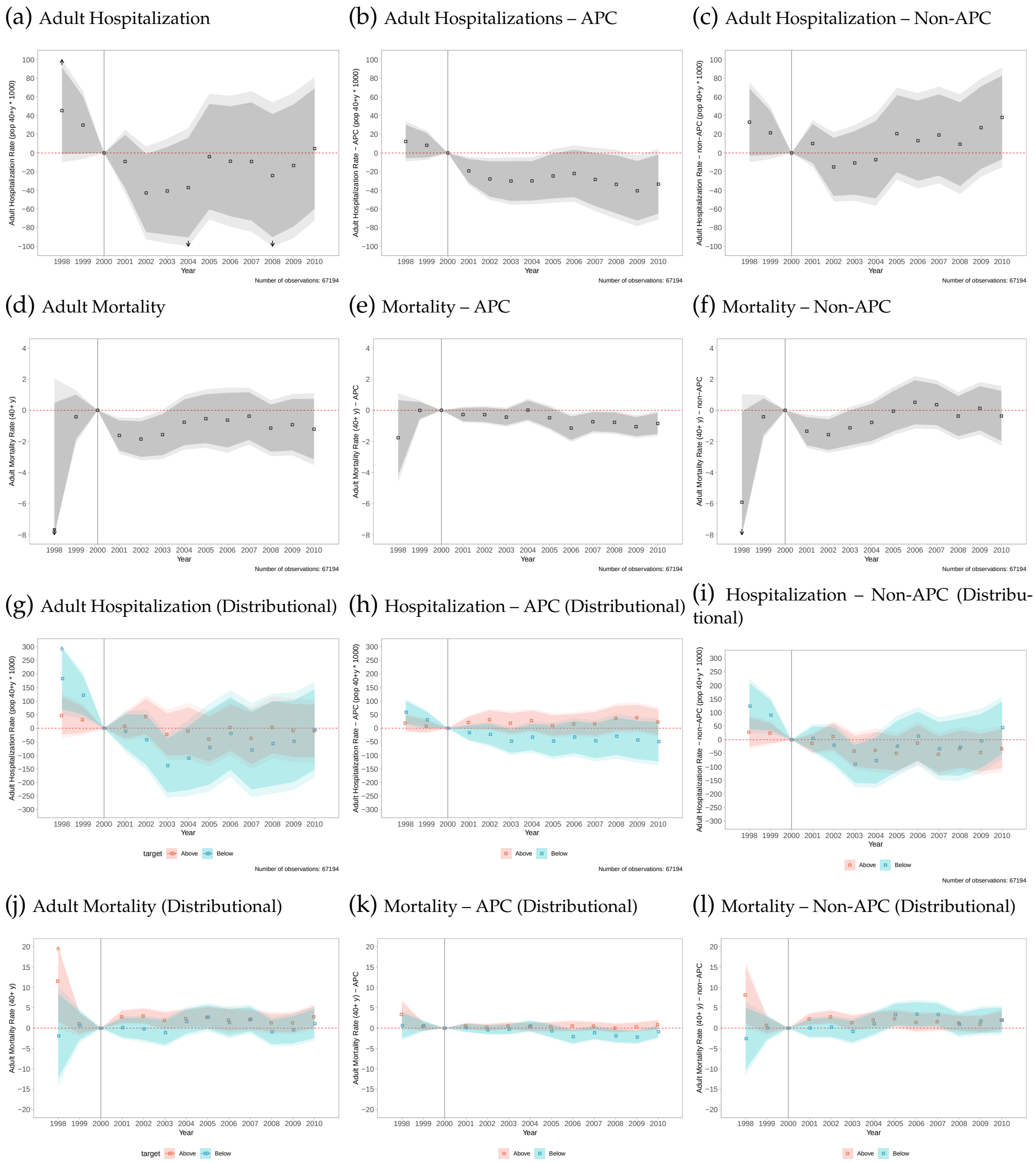
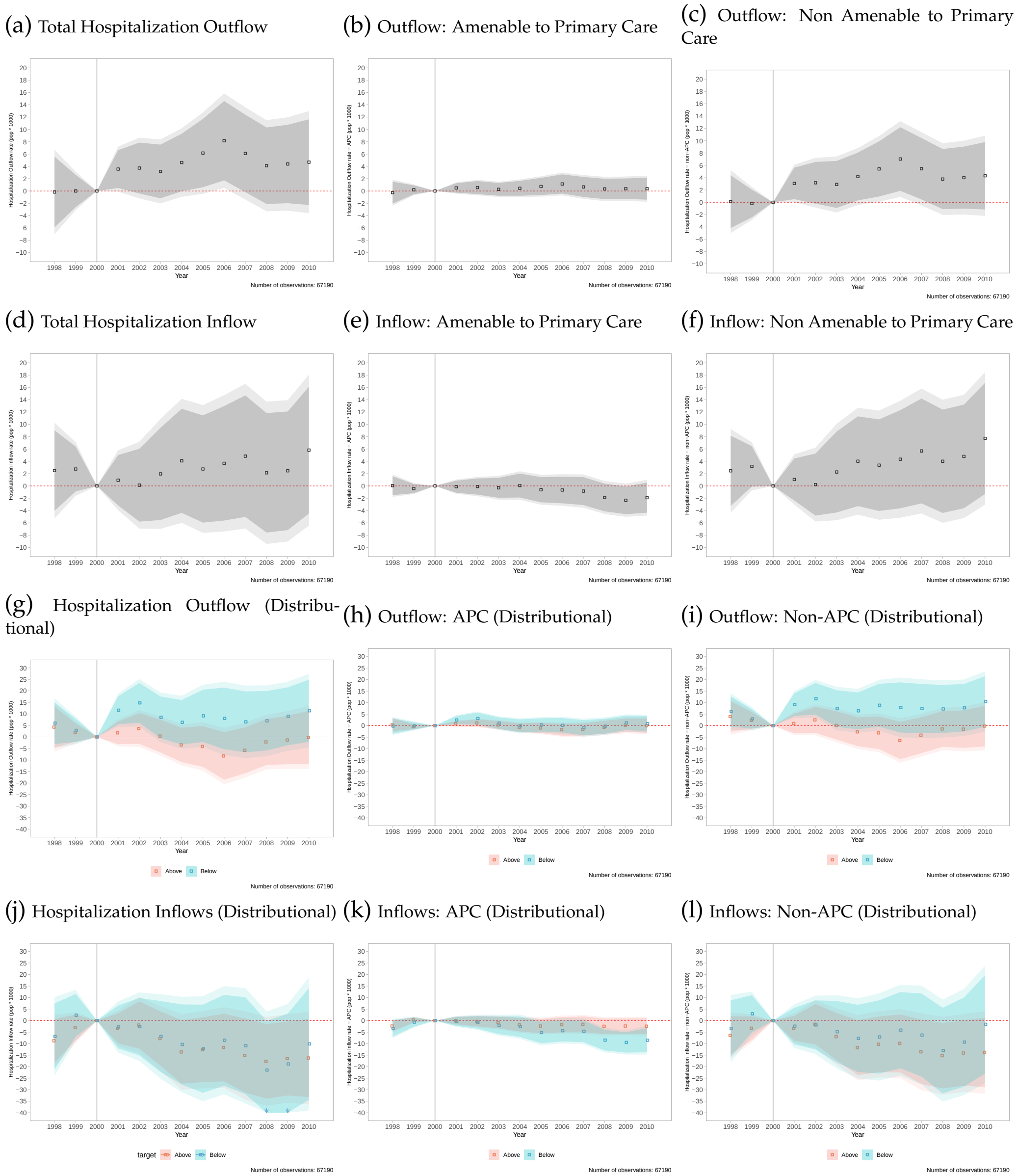


FIGURE C.2 Spending Reform Impacts on Adult Hospitalization and Mortality rates. *Notes*: Panels (a) to (f) present estimates from (1), and panel (g) to (l) present estimates from (2). In each specification lags and leads to the C/29 passage are presented, controlling for data quality, and baseline socioeconomic controls from the Census interacted with time trends. APC refers to Amenable to Primary Care. Panels (a) to (f) present global estimates from spending shifts, where point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Panels (g) to (l) present spending impacts separating by municipalities located below and above the spending threshold (15%) at baseline. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality.



(a) Total Hospitalization Outflow (b) Outflow: Amenable to Primary Care (c) Outflow: Non Amenable to Primary Care

(d) Total Hospitalization Inflow (e) Inflow: Amenable to Primary Care (f) Inflow: Non Amenable to Primary Care

(g) Hospitalization Outflow (Distributional) (h) Outflow: APC (Distributional) (i) Outflow: Non-APC (Distributional)

(j) Hospitalization Inflows (Distributional) (k) Inflows: APC (Distributional) (l) Inflows: Non-APC (Distributional)

FIGURE C.3 Patient Mobility and Geographical Spillovers. *Notes*: Panels (a) to (f) present estimates from (1), and panel (g) to (l) present estimates from (2). In each specification lags and leads to the EC/29 passage are presented, controlling for data quality, and baseline socioeconomic controls from the Census interacted with time trends. APC refers to Amenable to Primary Care. Panels (a) to (f) present global estimates from spending shifts, where point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Panels (g) to (l) present spending impacts separating by municipalities located below and above the spending threshold (15%) at baseline. Point estimates are presented as blue and red squares, with blue referring to the below target baseline while red refers to the above target baseline group. In each case 90% and 95% confidence intervals are presented as darker and lighter shaded areas respectively. Population weights are consistently used, and standard errors are clustered by municipality.

D | FURTHER DETAILS ON IDENTIFYING ASSUMPTIONS

Consider our measure of treatment intensity, which is the distance from the 15% spending target. We refer to this value, which can in theory be as high as 15 (if municipalities were spending 0% of their revenue on health at baseline), or as low as -85 (if municipalities were spending 100% of their revenue on health at baseline). In practice, these values vary between around 15 and -35 (see Figure 2a). Refer to this distance measure for a particular municipality as d, and the set of all distances as $\mathcal{D}$.

Consider pre-spending reform period $t-1$ and post-spending reform period t. The parallel trends assumption in this setting is that for all $d \in \mathcal{D}$:

$$E[Y_t(0) - Y_{t-1}(0)|D = d] = E[Y_t(0) - Y_{t-1}(0)|D = 0]. \tag{D.1}$$

In words, this is that observed trends in outcomes for untreated units (municipalities which were complying with the spending target at baseline) are a good counterfactual for what would have happened to units which were further from the target if there had been no spending reform. This is a standard parallel-trends assumption, where we assume that municipalities close to the spending target are a good counterfactual off of which to estimate outcome trends should other municipalities not have been subject to spending reform changes, with the only difference being that this is assumed to hold $\forall d \in \mathcal{D}$, whereas in a model with binary treatment measures, it would be assumed to hold between these untreated units, and units for whom $D = 1$.

Callaway et al. (2024) note that this assumption is sufficient to identify a series of parameters which they refer to as $ATT(d|d)$, the average effect of changing a spending target by d, for municipalities which were effectively d units away from the target at baseline. In the case of the EC/29 spending reform, such an estimand is unlikely to be of interest given that the reform caused all municipalities to vary spending patterns. Instead, for a given unit, we are interested in estimating the impact of spending shocks given higher or lower exposure to the reform. Specifically, we are interested in dose response treatments. Individuals which were further from the spending cutoff at baseline are more exposed to the reform, and we are interested in understanding the impact of marginal spending by leveraging marginal shifts in distance to this spending target.

This is thus an average causal response (ACR), or the change in outcomes given a marginal change in distance to the health spending target. Callaway et al. (2024) note that two-way fixed effect estimates (and corresponding time-dependent quantities presented in dynamic models) are related to average causal response functions. However, they note that without further assumptions, we do not generically estimate $ATE(d)$, and the more simple two-way fixed effect estimate which we implement in specification (3) does not estimate an average of $ATT(d|d)$ parameters. Specifically, under the parallel trends assumption in equation (D.1), the two-way fixed effect estimate captures the following:

$$\beta^{twfe} = \int_{dL}^{dU} w_1(l) \left[ACRT(l|l) + \frac{\partial ATT(l|h)}{\partial h}\bigg|_{h=l} \right] dl + w_0 \frac{ATT(d_L|d_L)}{dL} \tag{D.2}$$

where:

$$w_1(l) = \frac{(E[D|D \geqslant l] - E[D])P(D \geqslant l)}{\sigma_D^2} \qquad w_0 = \frac{(E[D|D \neq 0] - E[D])P(D \neq 0)d_L}{\sigma_D^2},$$

and:

$$\text{ACRT}(d|d) \equiv \left.\frac{\partial E[Y_t(l)|D = d]}{\partial l}\right|_{l=d}.$$

The notation here follows Callaway et al. (2024), however note that we have generalised the formulation such that $\mathcal{D}$ does not have strictly positive support: both positive and negative distances are permitted. This quantity ACRT refers to the average causal response on the treated, which is the change in outcomes given a marginal change in distance to the health spending target. The weights w_0 and $w_1(l)$ integrate to 1, where in this setting, w_0 will be very small given that $E[D|D \neq 0] \approx E[D]$, and so we can focus on the first term in (D.2). This first term suggests that under standard parallel trends assumptions as in (D.1), we will thus not necessarily capture a weighted average of average causal response functions, given the existence of the second term: $\partial \text{ATT}(l|h)/\delta h|_{h=l}$. This term captures any possible selection into treatment effects. For example, if units which have higher values of distance to treatment d generally have larger treatment effects for a specific treatment value, this ATT term will be positive. In the range considered in this setting, it is not clear whether such ATT terms will be non-zero. It is not clear, for example, that a municipality which was 5 points from the target and so increased spending by 5 points would gain more or less from this spending change than if a municipality which was 6 points from the spending target, had increased its spending by 5 points. As this second term refers to changes in ATTs across small changes in spending, it seems likely that this term may be negligible.

More specifically, as laid out in Callaway et al. (2024), if we are willing to make a stronger version of the parallel trends assumption made above, the interpretation of the two-way FE estimator can be simplified considerably. In particular, we require the "strong parallel trends assumption" which states that for all $d \in \mathcal{D}$:

$$E[Y_t(d) - Y_{t-1}(0)] = E[Y_t(d) - Y_{t-1}(0)|D = d] \qquad \text{(D.3)}$$

In our context, this assumption implies that for all distances to spending targets, the average change in outcomes of interest over time across all units if they had instead had a baseline spending differential d equals the the average change in outcomes for all units which *actually* have baseline spending differential d. For example, consider distance $d = 5$, which implies that a municipality was spending 10, rather than 15% of its own resources on health at baseline, and so needed to increase its health spending by 5 percentage points. For this particular value d, equation (D.3) states that what happened to these municipalities in outcomes, between t and $t-1$, is what would have happened to all other municipalities between these periods (those with $d = 15, 14, 13, \ldots, 6, 4, 3, \ldots, -35$) if instead of having their own baseline differential, they had a differential of $d = 5$.[31] This is plausible if we believe that an exogenous shift in health spending of different sizes would have similar impacts if targeted to a municipality which spends relatively less or relatively more of its budget on health care. In our setting, empirical results do point to this being potentially plausible, given that spending targets appear to bind quite tightly across a large range of values, and so it seems plausible that had municipalities been presented with an alternative spending target, their behavior would have adjusted to meet this target. Moreover, we do not observe evidence to suggest that municipalities which spent greater or lower shares of their budget on health have observable measures which are trending in systematically

[31] This strong parallel trends assumption is necessary given that each spending level d is being compared with each other spending level, and so counterfactual mappings are required for each level d. It is thus the natural extension to parallel trends with counterfactual untreated states in a binary treatment setting.

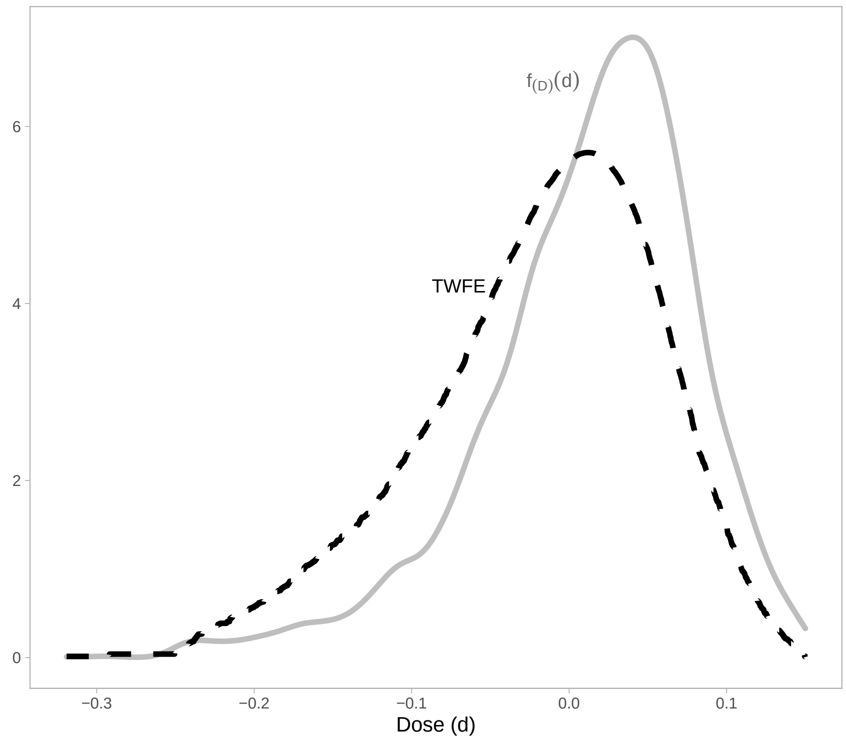


FIGURE D.1 Weights implicit in Two-way FE models and the Empirical Distribution of Spending Target Distances

different ways in the pre-reform period (Appendix Figure A.3). Should this assumption be reasonable, then it can be shown (Callaway et al., 2024, Theorem 3) that the two-way FE estimate in equation provides a weighted average of average causal responses, as laid out in the following:

$$\beta^{twfe} = \int_{dL}^{dU} w_1(l)ACR(l)dl + w_0\frac{ATT(d_L)}{dL}, \tag{D.4}$$

where:

$$ACR(d) = \frac{\partial E[Y_t(d)]}{\partial d}.$$

In this case, we can therefore interpret coefficient estimates as the weighted average of a marginal changes in spending targets on the outcome of interest, where weights are laid out above.

Thus, identification in our setting relists on the strong parallel trends assumption. However a secondary point of note is that the the weights $w_1(l)$ implicit in two-way FE models do not necessarily match those in the empirical distribution of distance to treatment. Indeed, as laid out above, these weights are mechanically related to variance of the treatment variable. We estimate these weights, and document that, in general, two-way FE models tend to put relatively more weight on municipalities which were already spending above the treatment target, and where we observe the health impacts are relatively smaller. Thus, in general, this weighting scheme is likely to be conservative. In robustness figures discussed in Section 6.2.4 we show an additional test where we re-weight two-way FE models such that weights are now based on the empirical distribution of spending targets (i.e. the ratio of the solid curve to the dashed curve in Figure D.1). Specifically, given that we weight models by population, in our reweighted models we use a weighted model where weights consist of $weight_m = population_m \frac{f_{(D)}(d)_m}{TWFE_m}$, with both $f_{(D)}(d)_m$ and $TWFE_m$ referring to municipality- (treatment dose-) specific values plotted in Figure D.1.

E | ROBUSTNESS CHECKS AND ADDITIONAL RESULTS

TABLE E.1 Full Tabular Output – Spending Event Studies

	Health (FINBRA)	Health (SIOPS)	Own Resources	Other Resources	Human Resources	Investment	Outsourced 3rd Party	Admin Management
Year = 1998	0.175	–	–	–	–	–	–	–
	(0.294)	–	–	–	–	–	–	–
Year = 1999	-0.064	–	–	–	–	–	–	–
	(0.291)	–	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.493***	1.387***	3.982***	0.891	1.326***	2.794***	0.736*	3.12***
	(0.163)	(0.119)	(0.261)	(0.953)	(0.347)	(0.67)	(0.401)	(0.657)
Year = 2002	0.992***	1.934***	4.846***	1.218	2.293***	4.085***	0.371	4.656***
	(0.361)	(0.177)	(0.235)	(1.142)	(0.431)	(0.743)	(0.705)	(1.362)
Year = 2003	1.13***	2.03***	4.86***	1.882	1.989***	4.684***	0.787	4.343***
	(0.287)	(0.125)	(0.37)	(1.38)	(0.319)	(0.912)	(0.847)	(1.281)
Year = 2004	1.373***	2.505***	5.715***	2.059	2.654***	5.393***	1.637**	4.59***
	(0.306)	(0.228)	(0.302)	(1.593)	(0.446)	(0.994)	(0.692)	(1.154)
Year = 2005	1.407***	2.56***	5.935***	1.674	2.837***	6.773***	1.265*	4.574***
	(0.319)	(0.267)	(0.246)	(1.544)	(0.475)	(1.201)	(0.713)	(1.176)
Year = 2006	1.582***	2.584***	5.881***	1.84	2.767***	6.647***	1.31*	4.487***
	(0.313)	(0.294)	(0.304)	(1.47)	(0.458)	(1.117)	(0.687)	(1.167)
Year = 2007	1.565***	2.675***	6.227***	1.532	3.486***	6.112***	1.784***	3.522***
	(0.297)	(0.188)	(0.287)	(1.33)	(0.53)	(1.01)	(0.619)	(0.606)
Year = 2008	1.541***	2.662***	6.082***	1.466	3.267***	6.352***	1.695***	3.933***
	(0.308)	(0.258)	(0.255)	(1.211)	(0.491)	(0.991)	(0.608)	(0.758)
Year = 2009	1.606***	2.63***	5.963***	1.685	2.933***	5.755***	1.585***	4.245***
	(0.296)	(0.272)	(0.299)	(1.278)	(0.428)	(1.08)	(0.593)	(0.783)
Year = 2010	1.69***	2.639***	6.082***	1.41	2.916***	5.771***	0.525	6.164***
	(0.304)	(0.265)	(0.249)	(1.255)	(0.393)	(0.792)	(0.873)	(1.326)
Observations	62,950	54,622	54,622	54,622	54,622	54,622	54,622	54,622

Notes: Output corresponds to graphical event studies displayed in Figure 3. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

TABLE E.2 Full Tabular Output – Spending Distributional Event Studies (Part I)

	Health (FINBRA)		Health (SIOPS)		Own Resources		Other Resources	
	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	-0.526	-0.315	–	–	–	–	–	–
	(0.371)	(0.673)	–	–	–	–	–	–
Year = 1999	0.108	0.013	–	–	–	–	–	–
	(0.412)	(0.693)	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	-0.133	1.019***	-1.188***	1.705***	-0.989***	8.474***	-1.997	-0.744*
	(0.27)	(0.375)	(0.209)	(0.189)	(0.358)	(0.471)	(1.418)	(0.427)
Year = 2002	-1.329**	0.508	-1.437***	2.684***	-1.655***	9.63***	-1.826	0.298
	(0.523)	(0.789)	(0.284)	(0.283)	(0.269)	(0.582)	(1.731)	(0.57)
Year = 2003	-0.627	1.866***	-1.294***	3.129***	-1.21***	10.317***	-2.635	0.756
	(0.405)	(0.644)	(0.174)	(0.305)	(0.468)	(0.454)	(2.103)	(0.643)
Year = 2004	-0.756*	2.28***	-1.761***	3.616***	-1.75***	11.635***	-3.315	0.201
	(0.413)	(0.714)	(0.352)	(0.358)	(0.341)	(0.425)	(2.415)	(0.764)
Year = 2005	-0.869**	2.211***	-1.913***	3.53***	-1.973***	11.859***	-2.805	-0.007
	(0.429)	(0.764)	(0.402)	(0.393)	(0.211)	(0.378)	(2.339)	(0.879)
Year = 2006	-1.036**	2.394***	-1.764***	3.807***	-1.845***	11.911***	-2.574	0.736
	(0.463)	(0.69)	(0.474)	(0.431)	(0.354)	(0.395)	(2.249)	(0.892)
Year = 2007	-1.266***	2.016***	-1.99***	3.707***	-2.63***	11.631***	-2.147	0.603
	(0.452)	(0.644)	(0.274)	(0.405)	(0.413)	(0.396)	(2.061)	(0.835)
Year = 2008	-1.295***	1.916***	-1.906***	3.798***	-2.252***	11.832***	-2.061	0.565
	(0.447)	(0.617)	(0.418)	(0.418)	(0.27)	(0.407)	(1.89)	(0.801)
Year = 2009	-1.127**	2.329***	-1.942***	3.668***	-2.145***	11.71***	-2.667	0.218
	(0.441)	(0.555)	(0.445)	(0.419)	(0.391)	(0.401)	(1.963)	(0.843)
Year = 2010	-1.182***	2.457***	-1.954***	3.674***	-2.479***	11.519***	-2.118	0.345
	(0.453)	(0.588)	(0.446)	(0.428)	(0.24)	(0.479)	(1.987)	(0.856)
Observations	62,950	62,950	54,622	54,622	54,622	54,622	54,622	54,622

Notes: Output corresponds to graphical event studies displayed in Figure 4. Coefficients are displayed with standard errors in parentheses. Outcomes are indicated in joint column headers, and coefficients are presented in separate columns for distance to the threshold in above and below-threshold municipalities. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

TABLE E.2 Full Tabular Output – Spending Distributional Event Studies (Part II)

	Human Resources		Investment		Outsourced (3^{rd} Party)		Admin., Management, Others	
	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–
Year = 1999	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	-1.268**	1.437**	-1.429	4.787***	-0.421	1.269	-3.329***	2.824***
	(0.506)	(0.585)	(1.061)	(1.378)	(0.585)	(0.861)	(1.035)	(0.761)
Year = 2002	-1.961***	2.805***	-3.628***	4.764***	1.52	3.171***	-4.407**	5.015***
	(0.622)	(0.815)	(1.128)	(1.559)	(1.161)	(0.793)	(2.115)	(1.03)
Year = 2003	-1.528***	2.686***	-3.771***	6.026***	1.611	4.348***	-4.633**	3.923***
	(0.435)	(0.878)	(1.344)	(1.699)	(1.462)	(0.863)	(1.983)	(0.925)
Year = 2004	-2.24***	3.285***	-3.835**	7.667***	0.754	5.18***	-4.729***	4.394***
	(0.62)	(0.935)	(1.522)	(1.606)	(1.305)	(0.908)	(1.823)	(0.906)
Year = 2005	-2.375***	3.542***	-6.544***	7.112***	0.746	4.261***	-5.201***	3.668***
	(0.678)	(0.944)	(1.844)	(1.616)	(1.329)	(0.93)	(1.844)	(0.798)
Year = 2006	-2.054***	3.835***	-5.383***	8.479***	0.644	4.225***	-4.798***	4.048***
	(0.714)	(0.842)	(1.698)	(1.806)	(1.255)	(1.006)	(1.856)	(0.835)
Year = 2007	-3.353***	3.715***	-5.665***	6.765***	0.063	4.546***	-3.074***	4.186***
	(0.88)	(0.765)	(1.53)	(1.766)	(1.16)	(1.053)	(0.922)	(0.922)
Year = 2008	-2.277***	4.748***	-6.165***	6.622***	-0.34	3.742***	-3.457***	4.64***
	(0.758)	(0.814)	(1.52)	(1.754)	(1.142)	(1.076)	(1.202)	(0.995)
Year = 2009	-2.228***	4.006***	-5.918***	5.504***	-0.067	3.871***	-3.549***	5.282***
	(0.701)	(0.734)	(1.622)	(1.641)	(1.151)	(1.01)	(1.198)	(1.14)
Year = 2010	-1.831***	4.548***	-6.238***	5.073***	1.035	2.877**	-6.094***	6.285***
	(0.621)	(0.73)	(1.162)	(1.77)	(1.565)	(1.206)	(2.029)	(1.164)
Observations	54,622	54,622	54,622	54,622	54,622	54,622	54,622	54,622

Notes: Output corresponds to graphical event studies displayed in Figure 4. Coefficients are displayed with standard errors in parentheses. Outcomes are indicated in joint column headers, and coefficients are presented in separate columns for distance to the threshold in above and below-threshold municipalities. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively. Table continued from previous page.

TABLE E.3 Full Tabular Output – Infant Mortality Event Studies

	Infant Mortality	IMR 24 hours	IMR 1-27 days	IMR > 27 days
Year = 1998	-0.706	-2.969	-1.934	1.227
	(8.114)	(2.24)	(4.799)	(4.06)
Year = 1999	-0.871	-0.385	-1.667	0.796
	(3.345)	(1.253)	(2.222)	(1.847)
Year = 2000	0	0	0	0
	(–)	(–)	(–)	(–)
Year = 2001	-0.85	-1.094	-1.225	0.375
	(2.592)	(1.229)	(1.824)	(1.584)
Year = 2002	1.302	-2.85**	-1.603	2.906*
	(3.023)	(1.243)	(2.113)	(1.632)
Year = 2003	-2.578	-2.311*	-1.911	-0.667
	(2.741)	(1.264)	(2.039)	(1.454)
Year = 2004	-1.957	-2.165	-1.629	-0.327
	(2.865)	(1.382)	(2.11)	(1.531)
Year = 2005	-4.623	-2.836**	-4.366*	-0.257
	(3.259)	(1.379)	(2.509)	(1.525)
Year = 2006	-5.429	-4.038***	-5.267*	-0.162
	(3.866)	(1.466)	(2.93)	(1.696)
Year = 2007	-8.715***	-4.142***	-6.337**	-2.377
	(3.371)	(1.377)	(2.476)	(1.657)
Year = 2008	-7.619**	-3.485**	-4.832**	-2.787
	(3.282)	(1.427)	(2.36)	(1.73)
Year = 2009	-8.975***	-2.631*	-5.895**	-3.08*
	(3.409)	(1.4)	(2.411)	(1.745)
Year = 2010	-10.427***	-2.437*	-5.613**	-4.814***
	(3.571)	(1.381)	(2.479)	(1.759)
Observations	67,193	67,193	67,193	67,193

Notes: Output corresponds to graphical event studies displayed in Figure 5. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

TABLE E.4 Full Tabular Output – Infant Mortality Distributional Event Studies

	Infant Mortality		IMR (24 hours)		IMR (1-27 days)		IMR (27 days-1 year)	
	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	4.092	4.361	4.422	-0.8	8.719	8.213	-4.627	-3.852
	(11.587)	(15.199)	(3.22)	(4.178)	(6.082)	(9.503)	(6.468)	(8.004)
Year = 1999	3.219	2.665	1.013	0.561	2.792	0.031	0.426	2.634
	(4.925)	(9.354)	(1.942)	(3.084)	(3.338)	(6.138)	(2.768)	(4.91)
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	1.998	0.858	1.569	-0.389	2.403	0.53	-0.405	0.328
	(3.999)	(6.772)	(1.921)	(2.911)	(2.952)	(4.508)	(2.391)	(3.996)
Year = 2002	-0.286	2.794	2.277	-3.725	1.935	-1.135	-2.221	3.93
	(4.36)	(7.916)	(1.917)	(3.112)	(3.178)	(5.428)	(2.346)	(4.056)
Year = 2003	-1.303	-8.452	-0.581	-6.675**	-1.37	-6.879	0.068	-1.573
	(4.129)	(7.521)	(1.896)	(3.141)	(3.131)	(5.339)	(2.108)	(3.711)
Year = 2004	-0.363	-5.507	0.616	-4.519	1.214	-2.306	-1.578	-3.201
	(4.109)	(7.487)	(2.024)	(3.354)	(3.142)	(5.363)	(2.147)	(3.965)
Year = 2005	2.041	-8.576	1.339	-5.116	2.21	-7.666	-0.169	-0.91
	(4.325)	(7.99)	(2)	(3.299)	(3.26)	(6.056)	(2.283)	(3.601)
Year = 2006	2.14	-10.46	2.357	-6.602*	2.995	-8.752	-0.856	-1.708
	(4.431)	(9.17)	(2.001)	(3.472)	(3.277)	(6.818)	(2.527)	(3.954)
Year = 2007	5.641	-13.444	2.38	-6.839**	4.269	-9.535	1.372	-3.909
	(4.509)	(8.288)	(1.998)	(3.354)	(3.294)	(6.139)	(2.572)	(3.816)
Year = 2008	3.031	-14.636*	-0.414	-9.396***	1.905	-9.333*	1.126	-5.304
	(4.584)	(7.689)	(2.082)	(3.319)	(3.222)	(5.585)	(2.792)	(3.88)
Year = 2009	4.512	-15.835*	-0.134	-6.854**	1.847	-12.104**	2.665	-3.731
	(4.988)	(8.4)	(1.918)	(3.402)	(3.385)	(5.897)	(2.822)	(4.177)
Year = 2010	5.335	-18.247**	1.138	-4.467	3.349	-9.156	1.986	-9.091**
	(5.253)	(8.455)	(1.872)	(3.495)	(3.563)	(6.035)	(2.824)	(4.076)
Observations	67,193	67,193	67,193	67,193	67,193	67,193	67,193	67,193

Notes: Output corresponds to graphical event studies displayed in Figure 5. Coefficients are displayed with standard errors in parentheses. Outcomes are indicated in joint column headers, and coefficients are presented in separate columns for distance to the threshold in above and below-threshold municipalities. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

TABLE E.5 Full Tabular Output – Access and Input Event Studies

	Access & Production	Primary Access	Non-Primary Access	Health Inputs	Human Resources	Hospitals
Year = 1998	0.261	0.228	0.243	–	–	–
	(0.243)	(0.256)	(0.173)	–	–	–
Year = 1999	-0.021	-0.104	0.139	–	–	–
	(0.154)	(0.165)	(0.09)	–	–	–
Year = 2000	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.45***	0.529**	0.021	–	–	–
	(0.174)	(0.211)	(0.071)	–	–	–
Year = 2002	0.412*	0.493*	0.021	0.551***	1.967***	0.287***
	(0.241)	(0.271)	(0.101)	(0.134)	(0.506)	(0.07)
Year = 2003	0.497	0.611*	-0.014	–	–	–
	(0.304)	(0.348)	(0.121)	–	–	–
Year = 2004	0.419	0.455	0.047	–	–	–
	(0.344)	(0.398)	(0.138)	–	–	–
Year = 2005	0.611**	0.641**	0.115	0.427***	1.273**	0.297***
	(0.28)	(0.319)	(0.15)	(0.135)	(0.505)	(0.066)
Year = 2006	0.705***	0.788***	0.055	–	–	–
	(0.226)	(0.253)	(0.152)	–	–	–
Year = 2007	0.465**	0.516***	0.048	–	–	–
	(0.19)	(0.195)	(0.172)	–	–	–
Year = 2008	0.464**	0.562**	-0.068	–	–	–
	(0.197)	(0.225)	(0.188)	–	–	–
Year = 2009	0.506**	0.648***	-0.17	0.297**	1.019**	0.238***
	(0.21)	(0.238)	(0.205)	(0.118)	(0.462)	(0.064)
Year = 2010	0.661***	0.751***	-0.049	–	–	–
	(0.224)	(0.247)	(0.204)	–	–	–
Observations	67,194	67,194	67,194	20,748	20,748	20,748

Notes: Output corresponds to graphical event studies displayed in Figure 6. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

TABLE E.6 Full Tabular Output – Access and Input Distributional Event Studies

	Access & Production		Primary Access		Non-Primary Access		Health Inputs		Human Resources		Hospitals	
	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	-0.032	0.603	-0.239	0.213	0.215	0.926**	–	–	–	–	–	–
	(0.325)	(0.615)	(0.319)	(0.701)	(0.25)	(0.385)	–	–	–	–	–	–
Year = 1999	0.235	0.298	0.221	0.072	0.117	0.523***	–	–	–	–	–	–
	(0.251)	(0.342)	(0.266)	(0.397)	(0.156)	(0.184)	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.001	1.127**	0.079	1.443**	-0.03	0.007	–	–	–	–	–	–
	(0.234)	(0.495)	(0.279)	(0.62)	(0.108)	(0.156)	–	–	–	–	–	–
Year = 2002	-0.22	0.701	-0.246	0.865*	0.056	0.138	-0.415**	0.755**	-1.135	3.222***	-0.305***	0.259
	(0.373)	(0.431)	(0.429)	(0.522)	(0.137)	(0.223)	(0.185)	(0.318)	(0.742)	(1.202)	(0.098)	(0.186)
Year = 2003	-0.149	1.021*	-0.052	1.452**	-0.029	-0.077	–	–	–	–	–	–
	(0.428)	(0.608)	(0.498)	(0.739)	(0.161)	(0.267)	–	–	–	–	–	–
Year = 2004	-0.364	0.502	-0.292	0.702	0.019	0.147	–	–	–	–	–	–
	(0.437)	(0.737)	(0.518)	(0.869)	(0.173)	(0.337)	–	–	–	–	–	–
Year = 2005	-0.487	0.798	-0.455	0.922	0.016	0.314	-0.178	0.804***	0.095	3.345***	-0.296***	0.298*
	(0.446)	(0.544)	(0.53)	(0.633)	(0.175)	(0.381)	(0.201)	(0.311)	(0.811)	(1.017)	(0.094)	(0.175)
Year = 2006	-0.334	1.264**	-0.309	1.509***	0.01	0.155	–	–	–	–	–	–
	(0.346)	(0.503)	(0.397)	(0.58)	(0.189)	(0.376)	–	–	–	–	–	–
Year = 2007	-0.156	0.932***	-0.163	1.05***	0.068	0.225	–	–	–	–	–	–
	(0.307)	(0.352)	(0.328)	(0.387)	(0.223)	(0.408)	–	–	–	–	–	–
Year = 2008	-0.023	1.13***	-0.123	1.226**	0.525**	0.624	–	–	–	–	–	–
	(0.322)	(0.395)	(0.372)	(0.488)	(0.25)	(0.452)	–	–	–	–	–	–
Year = 2009	-0.186	0.99**	-0.303	1.171**	0.678**	0.598	-0.15	0.522**	0.307	3.043***	-0.287***	0.163
	(0.339)	(0.403)	(0.381)	(0.499)	(0.285)	(0.485)	(0.21)	(0.239)	(0.792)	(0.983)	(0.094)	(0.171)
Year = 2010	-0.313	1.187***	-0.367	1.334***	0.595**	0.777	–	–	–	–	–	–
	(0.351)	(0.41)	(0.386)	(0.481)	(0.286)	(0.519)	–	–	–	–	–	–
Observations	67,194	67,194	67,194	67,194	67,194	67,194	20,748	20,748	20,748	20,748	20,748	20,748

Notes: Output corresponds to graphical event studies displayed in Figure 6. Coefficients are displayed with standard errors in parentheses. Outcomes are indicated in joint column headers, and coefficients are presented in separate columns for distance to the threshold in above and below-threshold municipalities. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

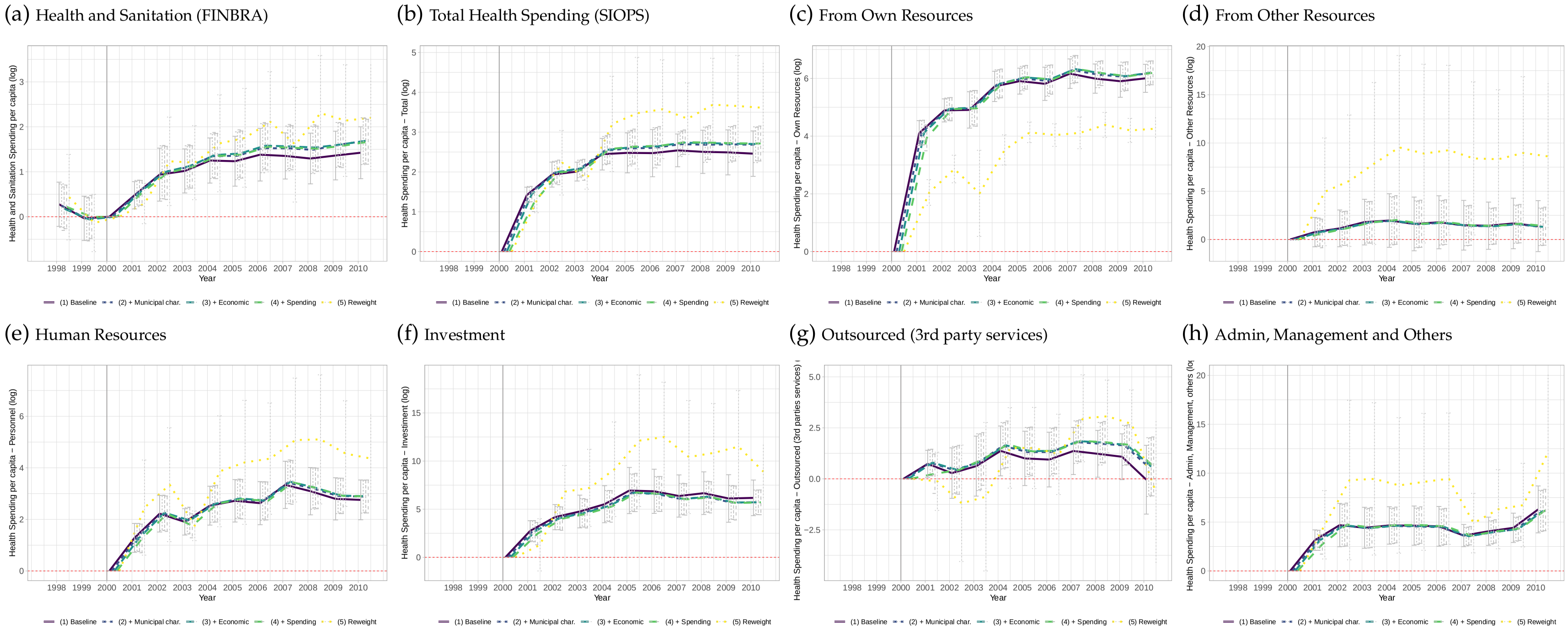


FIGURE E.1 Robustness to Control Specification: Spending and Revenue. *Notes*: Refer to notes to Figure 3. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All other details follow those described in notes to Figure 3.

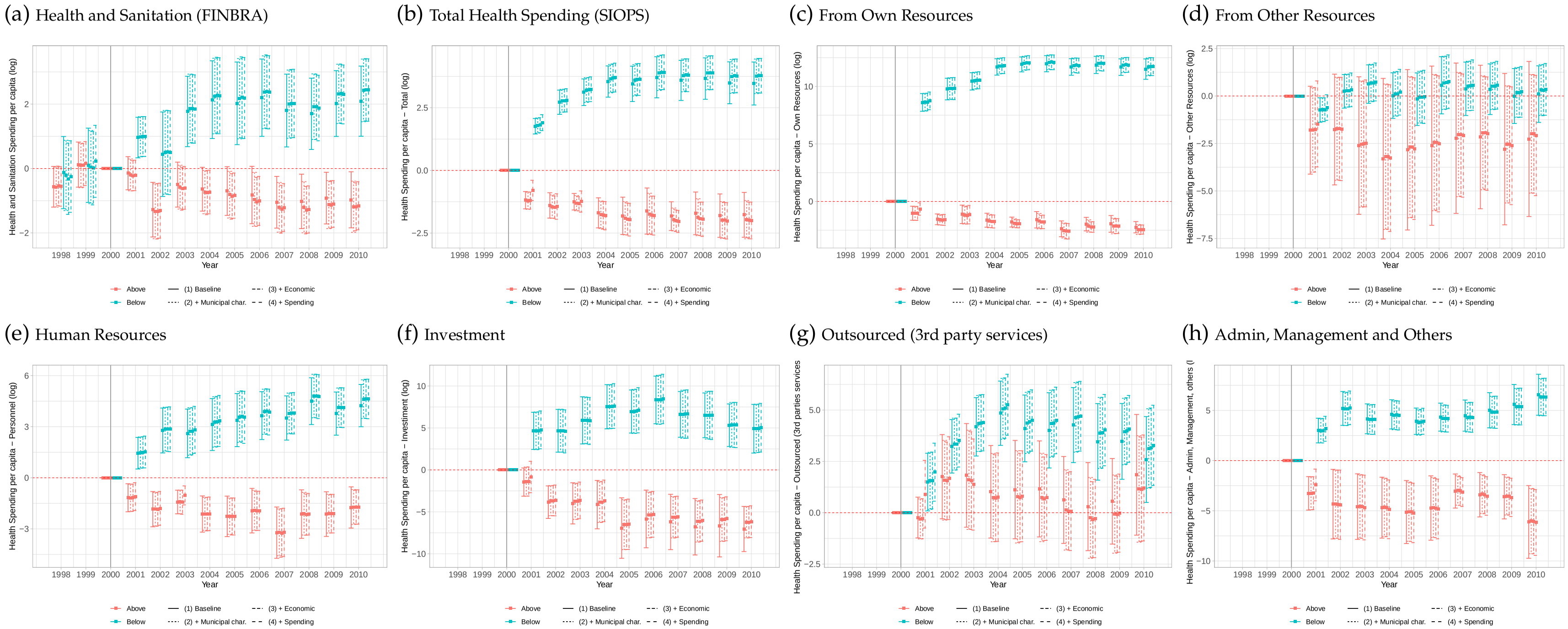
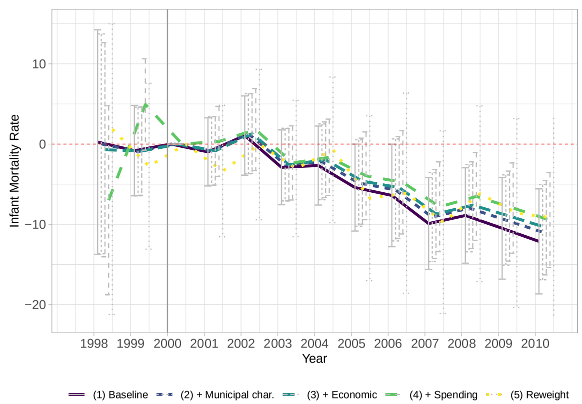


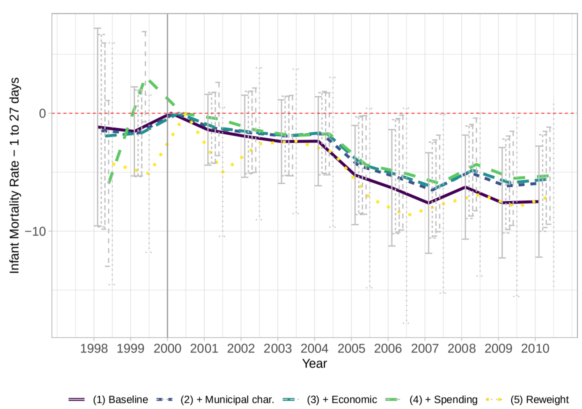
FIGURE E.2 Robustness to Control Specification for Distributional Effects: Spending and Revenue. *Notes*: Refer to notes to Figure 4. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. All other details follow those described in notes to Figure 4.

(a) Total



(b) Deaths within 24 hours

(c) Deaths between 1-27 days



(d) Deaths from 27 days to 1 year

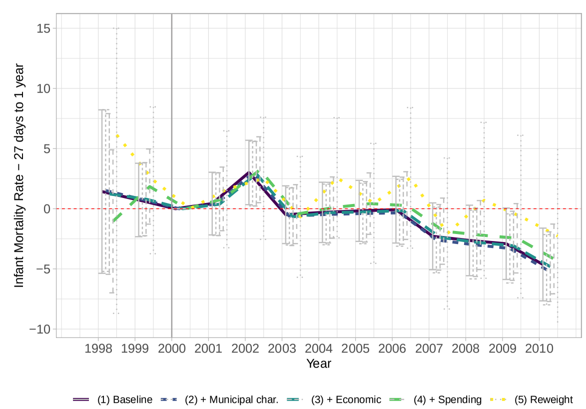


FIGURE E.3 Robustness to Control Specification: Infant Mortality. *Notes*: Refer to notes to Figure 5. Identical models are estimated, however varying control sets in the manner indicated in legend titles. Joined line plots present point estimates, and error bars represent 95% confidence intervals. All other details follow those described in notes to Figure 5.

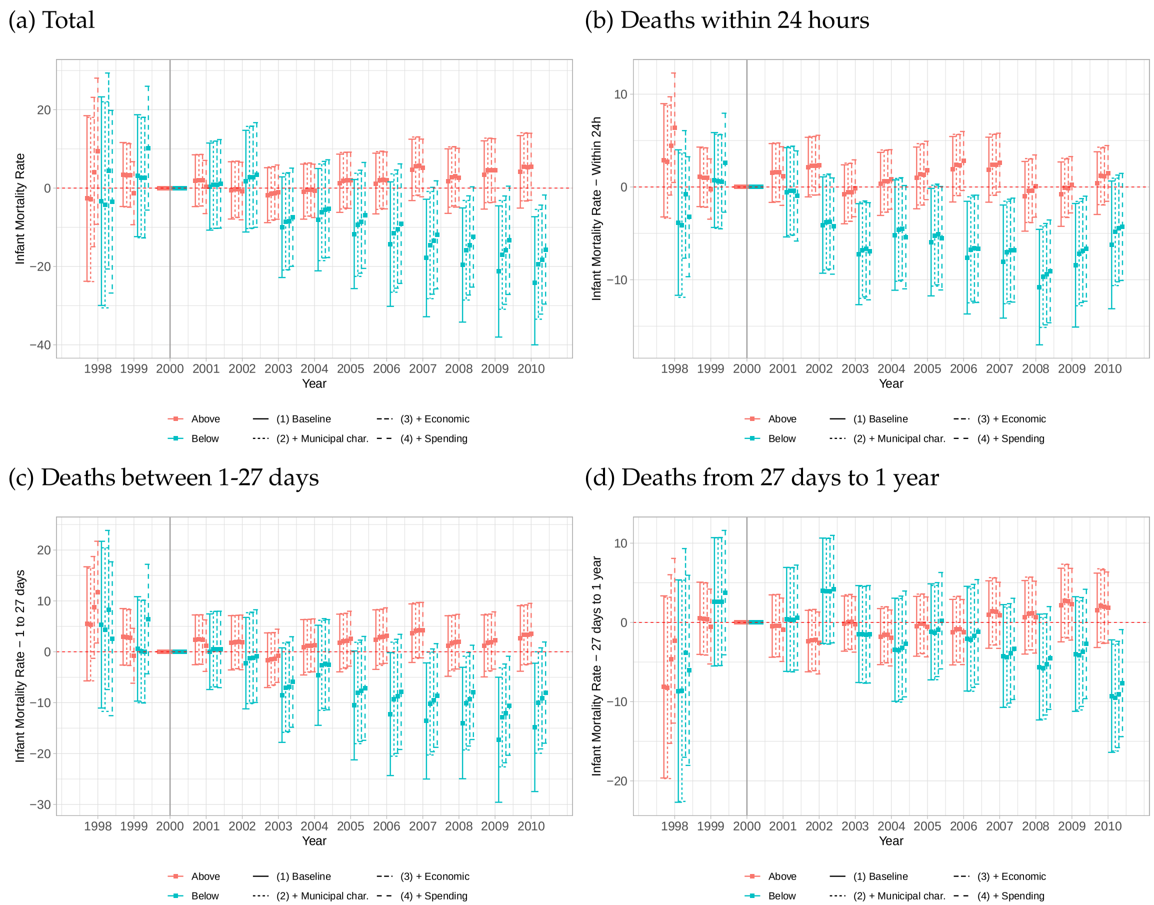


FIGURE E.4 Robustness to Control Specification for Distributional Effects: Infant Mortality. *Notes*: Refer to notes to Figure 5. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. All other details follow those described in notes to Figure 5.

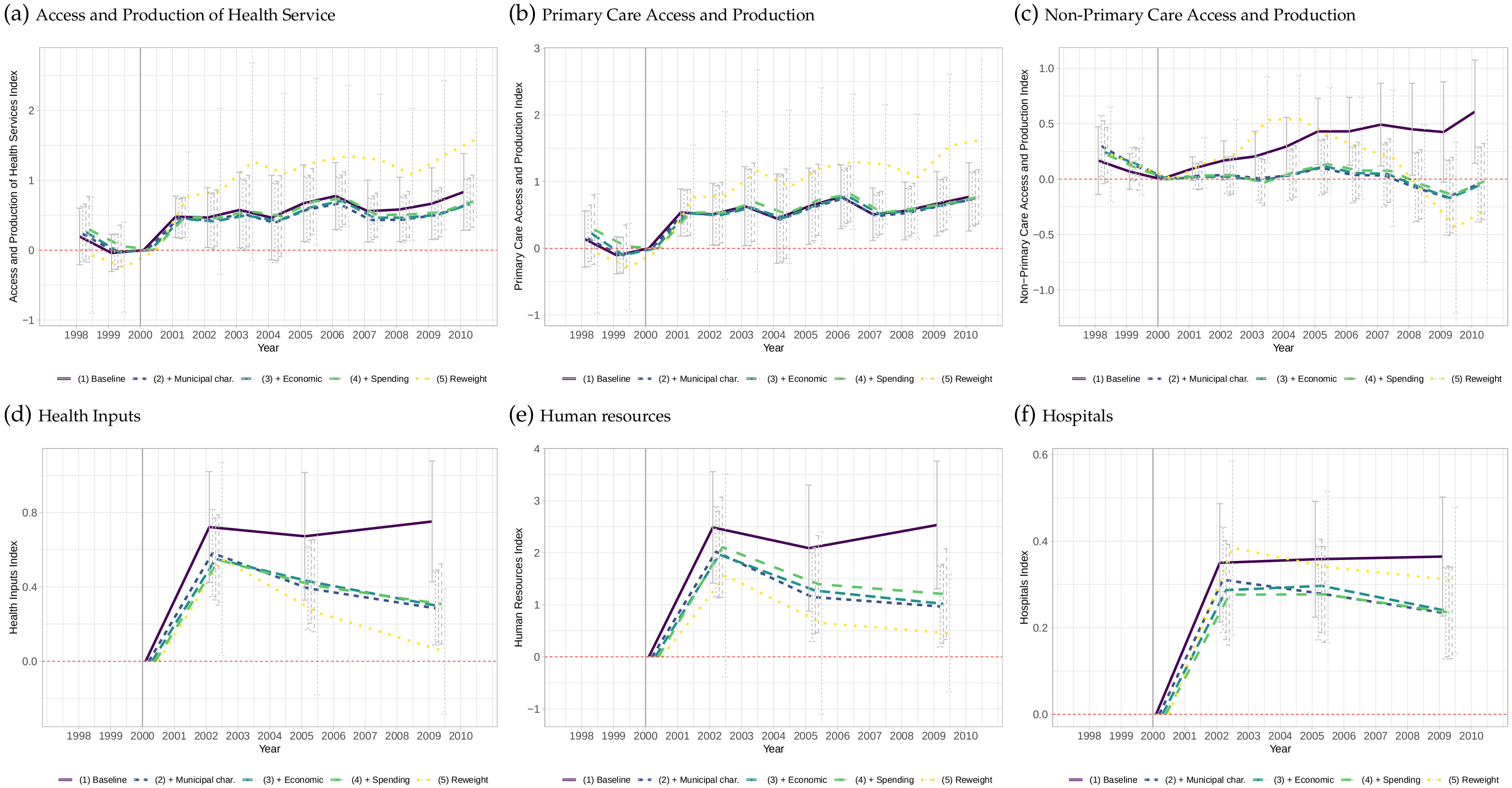


FIGURE E.5 Robustness to Control Specification: Services, Production and Inputs. *Notes*: Refer to notes to Figure 6. Identical models are estimated, however varying control sets in the manner indicated in legend titles. Joined line plots present point estimates, and error bars represent 95% confidence intervals. All other details follow those described in notes to Figure 6.

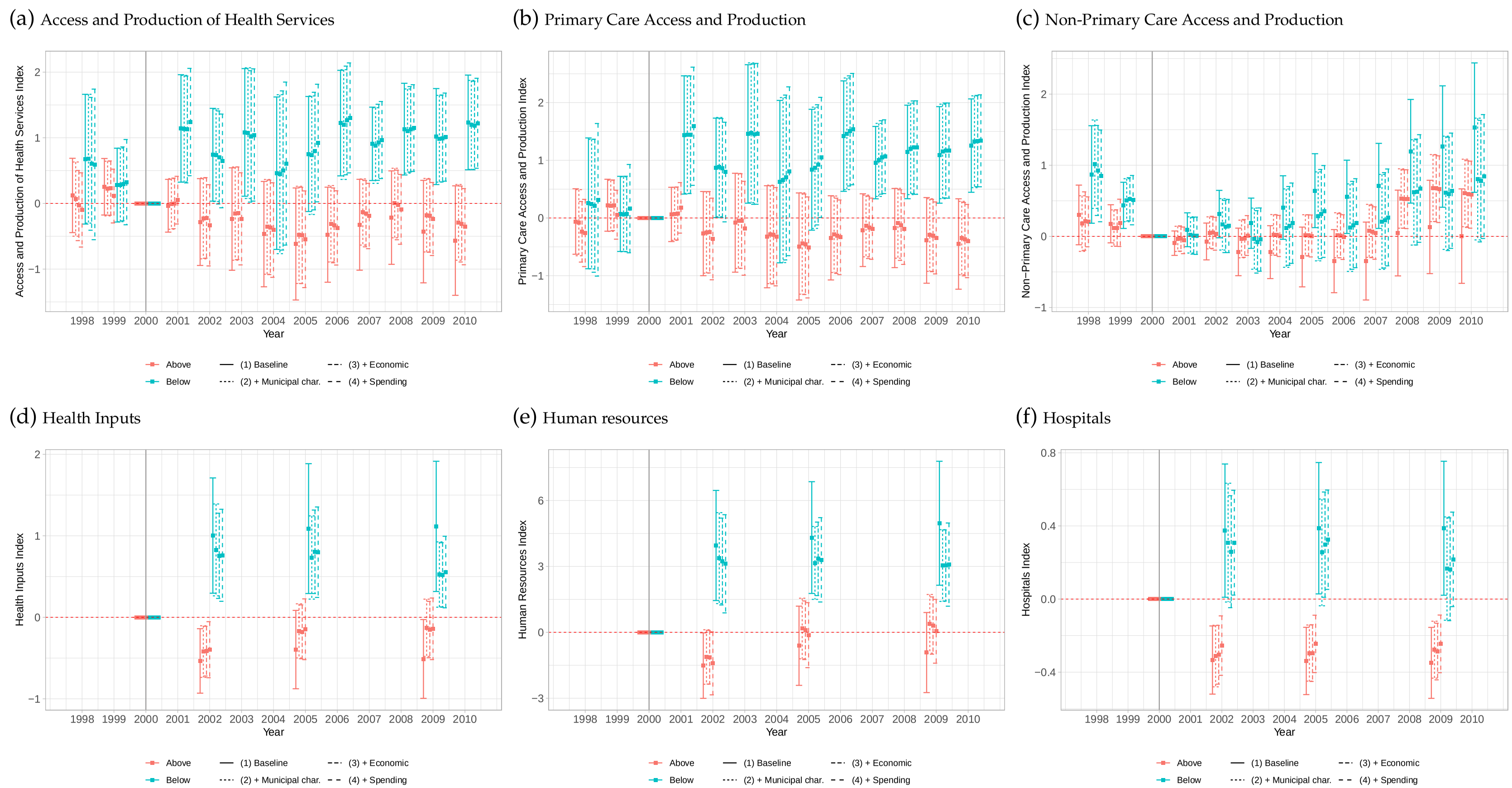
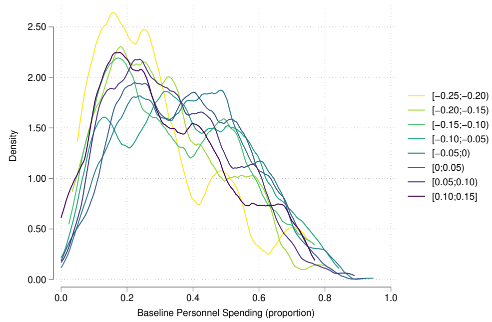


FIGURE E.6 Robustness to Control Specification for Distributional Effects: Services, Production and Inputs. *Notes*: Refer to notes to Figure 6. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. All other details follow those described in notes to Figure 6.

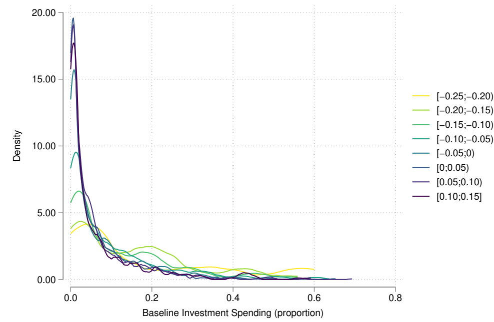
F | MUNICIPAL BEHAVIOUR

F.1 | Descriptive Figures and Marginal Effects with Standard Errors

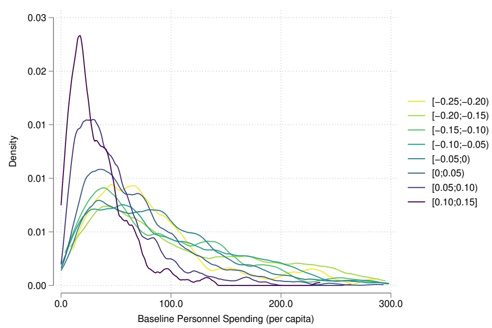
(a) Proportional Personnel Spending at Baseline



(b) Proportional Investment Spending at Baseline



(c) Total Personnel Spending at Baseline



(d) Total Investment Spending at Baseline

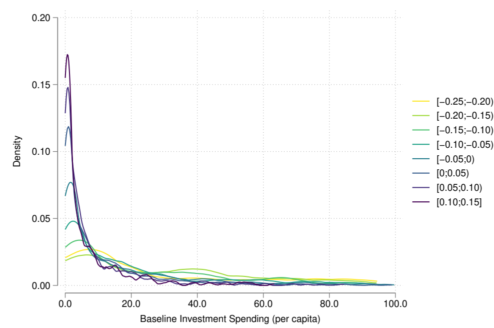


FIGURE F.1 EC/29 Exposure and Health Spending at Baseline. *Notes*: Each figure plots kernel densities of health spending directed to personnel at baseline (left-hand panel) and health spening directed to investment at baseline (right-hand panel), as measured by SIOPS data. The top panel presents spending as a proportion of all health spending, while the bottom panel presents total per-capita spending. Separate densities are presented for municipalities stratified on their distance to the 15% spending threshold at baseline. All values in the bottom row are reported in Reais per capita.

TABLE F.1 Distributional Responses to Spending Reforms

Personnel Spending Percentile:	10^{th} percentile			50^{th} percentile			90^{th} percentile		
Investment Spending Percentile:	10^{th}	50^{th}	90^{th}	10^{th}	50^{th}	90^{th}	10^{th}	50^{th}	90^{th}
Panel A: Mortality									
Infant Mortality	-20.05**	-19.40**	-14.49**	-16.73*	-16.20**	-12.16**	-9.03	-8.77	-6.74
	(10.22)	(9.66)	(5.88)	(8.67)	(8.23)	(5.38)	(9.94)	(9.46)	(7.15)
Infant Mortality (24 hours)	-3.80	-3.71	-3.03**	-3.18	-3.11*	-2.58*	-1.74	-1.72	-1.54
	(2.47)	(2.35)	(1.53)	(1.96)	(1.87)	(1.33)	(1.98)	(1.89)	(1.50)
Panel B: Health Spending									
log(Total Health Spending)	0.27***	0.27***	0.23***	0.24***	0.23***	0.19***	0.15***	0.15***	0.10***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
log(Personnel Spending)	0.30***	0.30***	0.28***	0.17***	0.17***	0.20***	-0.13**	-0.11**	0.01
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.06)	(0.05)	(0.05)
log(Investment Spending)	0.51***	0.51***	0.52***	0.50***	0.47***	0.21**	0.47***	0.36***	-0.49***
	(0.08)	(0.08)	(0.09)	(0.07)	(0.07)	(0.09)	(0.10)	(0.09)	(0.15)
log(Other Health Spending)	0.37***	0.36***	0.25***	0.39***	0.38***	0.24***	0.44***	0.42***	0.22***
	(0.05)	(0.05)	(0.06)	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)	(0.06)

Notes: Each row consists of a separate regression following (5), with each cell reporting the marginal effect of EC/29 on the outcome indicated in row titles at specific percentiles of the baseline distribution of personnel and investment spending per-capita, with percentiles indicated in table headers. Each estimate refers to the scaled effect of being 10% from the EC/29 spending threshold. Standard errors estimated by a municipal-level block bootstrap are reported in parentheses. * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

F.2 | A Measure of Management and Institutional Quality

The Municipal Institutional Quality Indicator (IQIM) was collated by Ministry of Planning and Budget, and has been employed in a range of settings to proxy management capacity. Among others, this has been employed by Pereira et al. (2011); Brassiolo et al. (2024), also see references therein. We provide descriptive figures below capturing its overall and geographic dispersion, as well as correlates between this measure with a range of baseline municipal measures, which make clear that while this measure correlates with factors such as income levels and spending, this is not simply proxying for development. As laid out in Figure F.3 there are municipalities with very high levels of GDP per capita with quite low IQIM scores, and municipalities with quite low levels of GDP per capita, but high IQIM scores. Similar patterns are observed when considering total municipal expenditure, infant mortality rates, and total health expenditures.

(a) Management Density (b) Management Index by Area

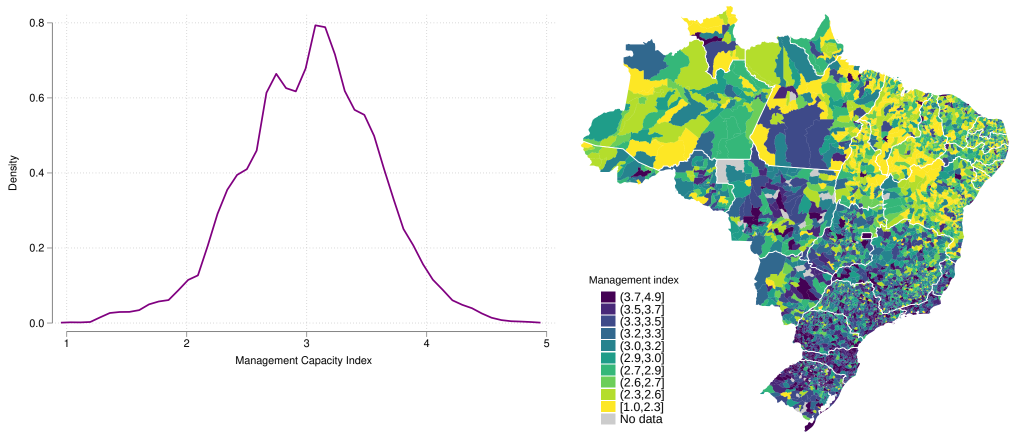
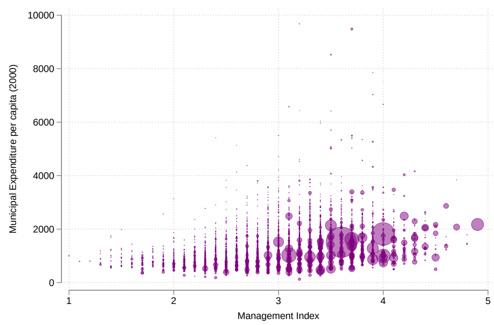


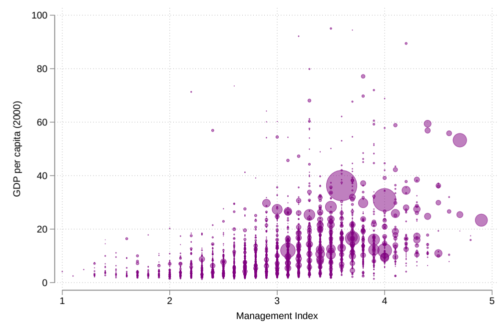
FIGURE F.2 Management and Institutional Capacity Descriptives. *Notes:* Descriptive patterns of the IQIM measure are documented for all municipalities as a simple distribution (panel (a)), and across space (panel (b)). Data is sourced from the Ministry of Planning and Budget. This measures is observed to be stable over time, see Brassiolo et al. (2024).

This variable is reported by the Ministry of Planning and Budget based on an underlying instrument designed to capture a range of factors measuring institutional quality. This includes measures of political participation such as the existence of municipal councils where citizens can air concerns and monitor municipal officials, measures of coordination between municipalities in the provision of public services, and measures of the cost effectiveness of systems to collect taxes as well as the existence of planning and regulatory instruments. A full list of items as well as weighting is provided in Sachsida (2014), and we use this index directly as defined by the Ministry of Planning and Budget.

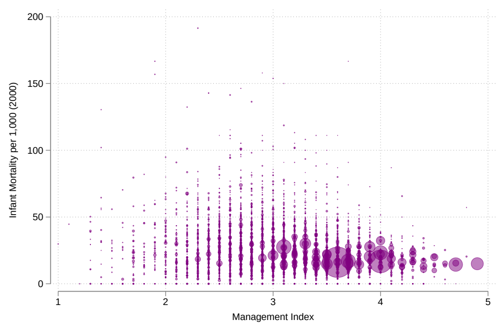
(a) Management Index and Total Expenditure



(b) Management Index and GDP per capita



(c) Management Index and Infant Mortality



(d) Management Index and Health Expenditures

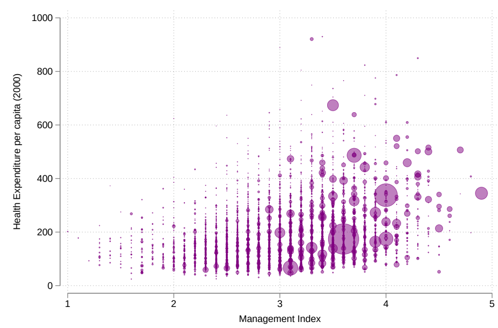
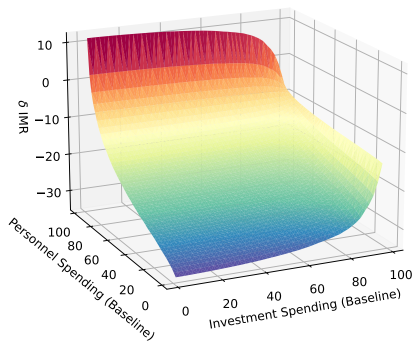
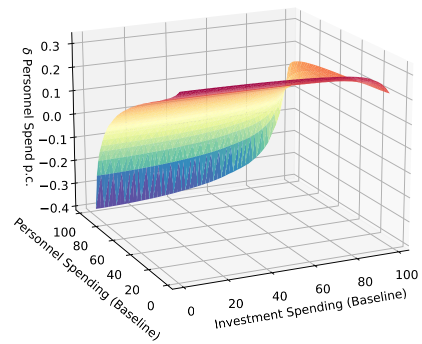


FIGURE F.3 Correlates of Municipal Management and Institutional Capacity. *Notes*: Municipal level values of the IQIM measure calculated by the Ministry of Budget and Planning are plotted against other municipal level variables in year 2000. Each point refers to a single municipality, with point sizes indicative of municipal populations.

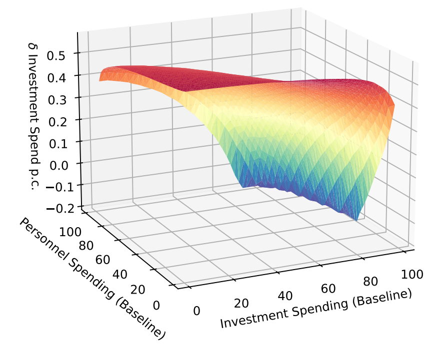
(a) Δ Infant Mortality (High management)



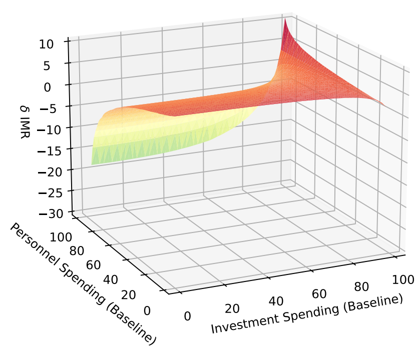
(b) Δ Personnel Spending (High management)



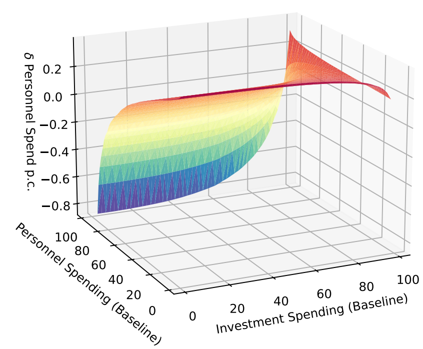
(c) Δ Investment Spending (High management)



(d) Δ Infant Mortality (Low management)



(e) Δ Personnel Spending (Low management)



(f) Δ Investment Spending (Low management)

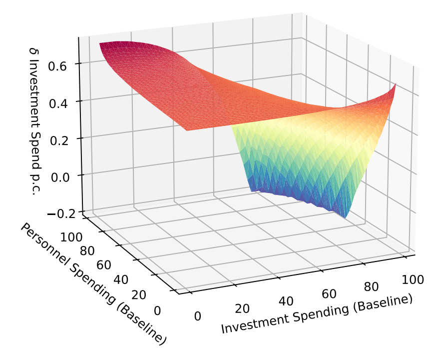


FIGURE F.4 Policy Responses and Baseline Spending by Municipal Management Quality. *Notes:* Refer to Notes to Figure 8. Identical results are presented, however now estimating separately for municipalities with an above-median management practices score (top row), and a below-median management practices score (bottom row).