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DISCUSSION PAPER SERIES

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ABSTRACT

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

We examine the link between public spending in health and health outcomes by leveraging differential exposure to a health spending reform prompted by Brazil's 29th Constitutional Amendment, which mandated municipalities to spend at least 15% of their budget on health. We map dynamic effects on health care spending, inputs, access, outputs and outcomes. For municipalities initially spending below the 15% threshold, we find (a) large increases in health spending specifically, driven by administrative spending, infrastructure investment, and human resources; (b) a resulting greater supply of personnel, primary care coverage, and municipal hospitals; and (c) reductions in infant mortality rates, in particular for deaths during the neonatal period. While we find substantial cost increases and lower mortality elasticities compared with previous correlational parameters, benefits still exceed costs provided any VSL greater than US$764 thousand. Our results contribute to the literature by providing one of the first well-identified causal parameters of the relationship between public spending in health and health outcomes, by documenting the links in the chain connecting government health expenditure to health outcomes, and by considering spillovers across space and sectors.

JEL Classification: I1, I3, O5
Keywords: health spending, public spending, health care provision, health outcomes

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1 Introduction

Global spending on health 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 estimated to reach over 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 not higher than 3% in OECD countries, and now ranges between 7% and 10% in most cases (OECD Stat, 2022).[1] Yet, there is surprisingly scarce causal evidence on whether government health expenditure is effective in improving health outcomes. Evidence is particularly sparse in developing countries, where life expectancy is generally lower and unmet social demands are greater (Mills, 2014). While within- or cross-country studies exist, these are generally based on country or region fixed effect specifications, or simply cross-space and cross-time settings. Moreover, the existing research has overlooked the links in the chain connecting government health expenditure to 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. Specifically, we assess how municipalities allocate resources when increasing health spending, and how expenditures translate into health inputs and outputs – such as health infrastructure, human resources and primary care services – and improved health outcomes, with a focus (though not exclusively) on infant health. We additionally examine whether public spending reform spills over into the private health system and changes patient mobility across regions.

We combine many sources of administrative microdata and leverage the variation in municipal health spending generated by a constitutional amendment enacted in 2000, which brought about a sudden and sharp increase in health spending across municipalities. In September of 2000, the Brazilian Congress enacted the 29th Constitutional Amendment (EC/29). It established the minimum share of resources that the federal, state and municipal governments need to spend on the provision of health care services. This reform was responsible for increasing government health

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[1] Government health expenditure generally covers direct spending on provision or subsidized insurance. On average, in both high- and low- and middle-income countries, direct spending has been growing, now corresponding to more than half total government health expenditure (see Figure A.1).

spending and for raising the direct participation of municipalities in the financing of health care. In particular, 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 a difference-in-differences style approach that relies on the distance to the minimum spending threshold when EC/29 is enacted, conditional on municipality and state-year fixed effects as well as on additional controls and specific time trends. 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 state-by-time invariant unobservables. In Brazil, the public health system is decentralized and municipalities are autonomous in choosing how to allocate their own funds. We map out changes over time, documenting the dynamic impacts of spending across multiple dimensions. 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 constitutional reform has promoted substantial increases in health spending for municipalities below the target at baseline. Increases in spending took place mainly through administrative spending, investment, and human resources, which in turn were translated into greater supply of personnel, primary care coverage, and municipal hospitals. The shift in health inputs and outputs led to reductions in infant mortality rates, in particular for deaths during the neonatal period and deaths caused by perinatal conditions. Yet, we find elasticities ranging from close to 0 in the immediate aftermath of the spending reform, to -0.2 ten years following the reform. Even 10 years out, upper ends of confidence intervals can rule out estimates greater than -0.5. These figures are therefore lower than estimates from previous studies, and vary considerably over time. Importantly, we show that the constitutional reform also induced a contraction in spending in municipalities that were above the target at baseline, but this came without adverse consequences for health outcomes. These municipalities managed to reduce spending, at the cost of reducing inputs, however without substantially affecting access to health nor production outputs.

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 any changes in private insurance coverage. As production outputs remained stable in these municipalities, the stability in infant 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 observe 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 screening and 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. On one hand, there is dense research documenting 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 arbitrary birth weight cut-offs, while Cutler et al. (1998) and Cutler (2007) document highly cost-effective investments in cardiovascular care when considering later-life survival. Doyle et al. (2015) find that spending at the hospital level can have substantial impacts on health outcomes. On the other hand, higher spending may not necessarily nor efficiently translate into better health outcomes if access to health care and service utilization are not well targeted. Influential results from the RAND Health Insurance Experiment (HIE) suggest that while reductions in individual co-pays boost total health spending, on average no significant improvements in health were observed (Manning et al., 1987). 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.[2]

Understanding the production function of health care is challenging, as it involves, for instance, 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 micro-level health-seeking behaviour (Lleras-Muney, 2005), physician and provider incentives (Clemens and Gottlieb, 2014; Batty and Ippolito, 2017), and political economy factors (Mobarak et al., 2011; Bhalotra et al., 2023). What these factors have in common is that at least in theory they are amenable to be modified by spending.[3] Yet, information failures and the interactive nature of health care production functions may constrain health spending returns.[4]

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. Although research isolating the specific connections between government health expenditures and health outcomes is scant, evidence from fiscal windfalls based on oil shocks 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, increasing corruption given that politicians can

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[2] Also, Finkelstein et al. (2012) find positive impacts on mental health and self-reported physical health when individuals were randomly assigned access to Medicaid during the first year of an experimental expansion, but a two-year follow up from Baicker et al. (2013) with clinical measures of health finds much weaker impacts.

[3] 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).

[4] 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).

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 total health spending, usually estimate cross-country relationships and cannot account for unobserved heterogeneity. Results are in general sensitive to robustness checks (Nakamura et al., 2020).[5] Some of the identification issues faced by this literature 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, a number of studies considering large differentials in Medicaid spending across US regions suggests 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 and increases in life expectancy. Bhalotra (2007), working on a panel at the individual level in India and exploring cross state variation, does not find any contemporaneous effects of state health expenditures on infant mortality, but observes 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.[7] While these studies move towards capturing a number of time

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[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).

[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).

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.

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 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. Our unique empirical context and the richness of the microdata allow us to uncover a comprehensive chain of causation triggered by a spending reform and propagated through local health systems, covering decisions on public spending (and potential responses by the private health system), health inputs, health production outputs and health outcomes.

The remainder of this article is organized as follows. In Section 2 we describe the institutional background and Brazil's 29th Constitutional Amendment. In Section 3 we detail the data used in this paper. In Section 4 we lay out our empirical strategy and identifying assumptions. The main results are presented in Section 5. In Section 6 we discuss mechanisms, while Section 7 provides additional robustness checks. Section 8 provides discussion and conclusions.

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 in 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 (Emenda Constitutional 29, 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 amount of resources that each government level needed to spend on the provision of health care. 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 and intergovernmental transfers. States and municipalities spending less than the thresholds established by the EC/29 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, the EC/29 did not explicitly regulate how governments should spend the resources, thus providing autonomy for government entities to allocate their funds.

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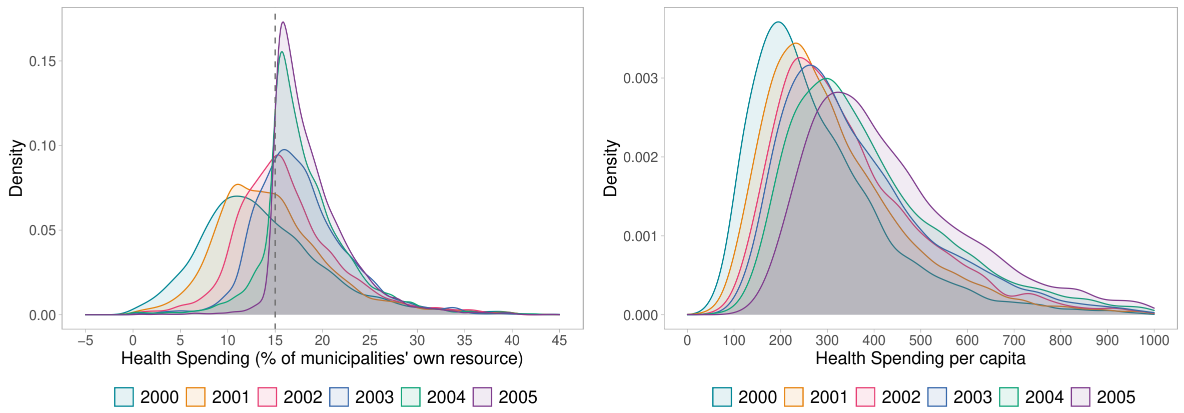
[8] While health expenditures as a share of GDP has been relatively higher in Brazil in comparison to upper-middle-income countries, the share of public spending in total health expenditure is relatively lower. Private spending has remained above 55% of total health expenditure, while around 25% of the 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 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.

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.

Figure 1: Spending Density Plots



(a) Health Spending (% of own resources)

(b) Health Spending per capita (2010 R$)

Notes: Density plots calculated using SIOPS data (see Section 3 for more details). Dotted line in Figure 1a marks the EC/29 target (see Section 2 for more details).

Figure 2 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. Figure 2a shows that 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, as shown in Figures 2b and 2c, expenditures funded by own resources explain almost the entire difference in spending increase between the bottom and the top quartiles.[10]

As expected, the baseline share of own revenues spent in health care is predictive of the change in municipal health spending per capita. Figure 3a 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 3c does the same, but looks at the change in spending per capita. Consistent

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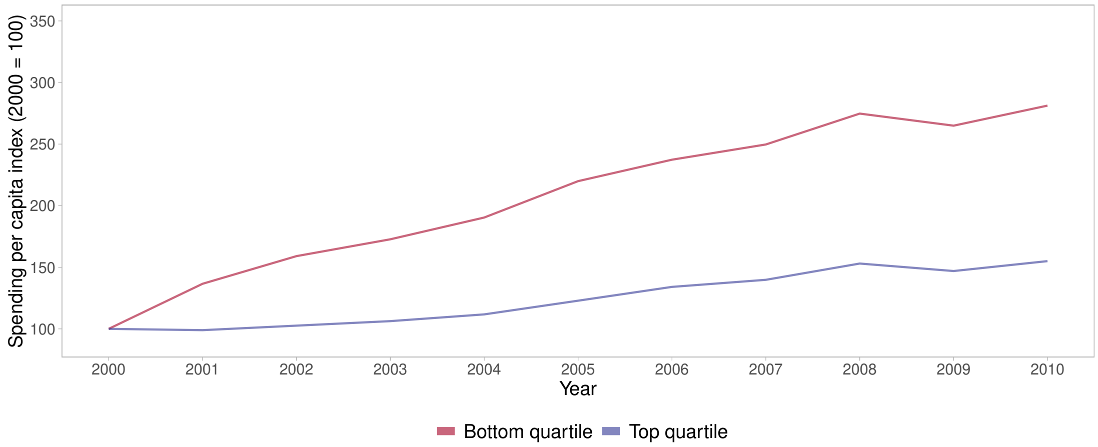
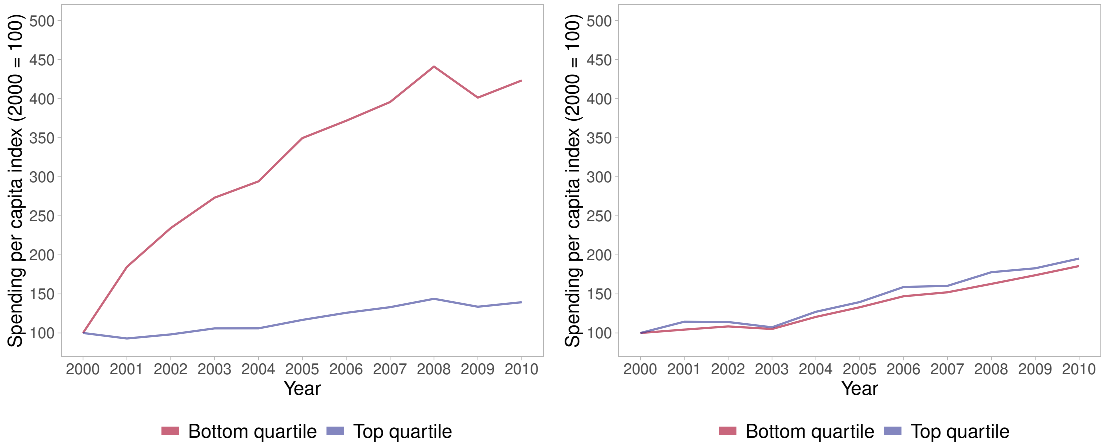
[10] Appendix Figure A.2 plots trends for health spending per capita by source of funding. Own revenues have always been the main source of municipal funding of health care, but the trends suggest that it gained more importance after the EC/29 (Figure A.2a). In 2000, health spending per capita in the bottom quartile was half of the top quartile. Figures A.2b and A.2c indicate that all these differences come from trends in spending funded by own resources.

Figure 2: Health Spending Trends

(a) Total Health Spending





(b) Health Spending from Own Resources

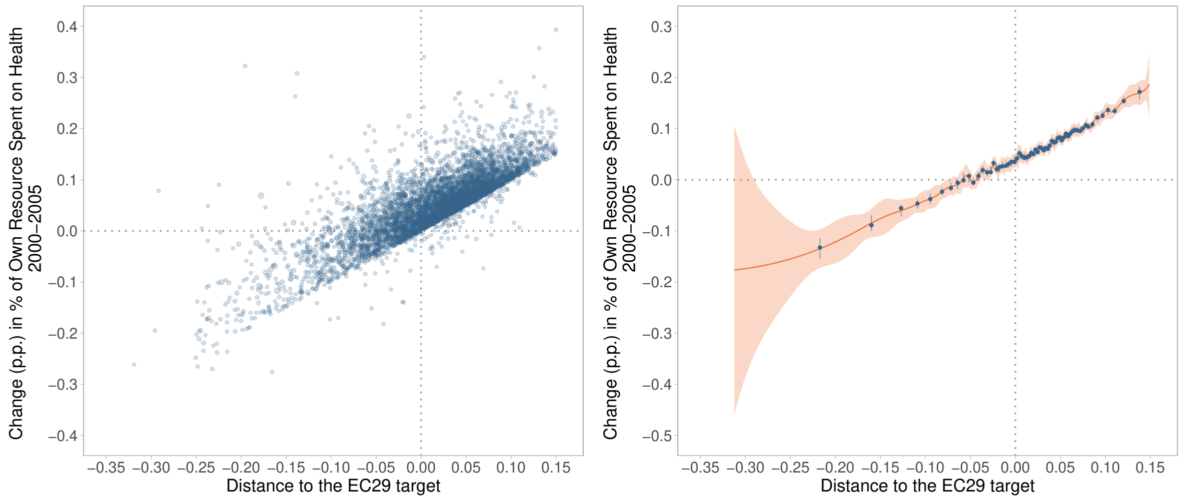
(c) Health Spending from Transfers

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

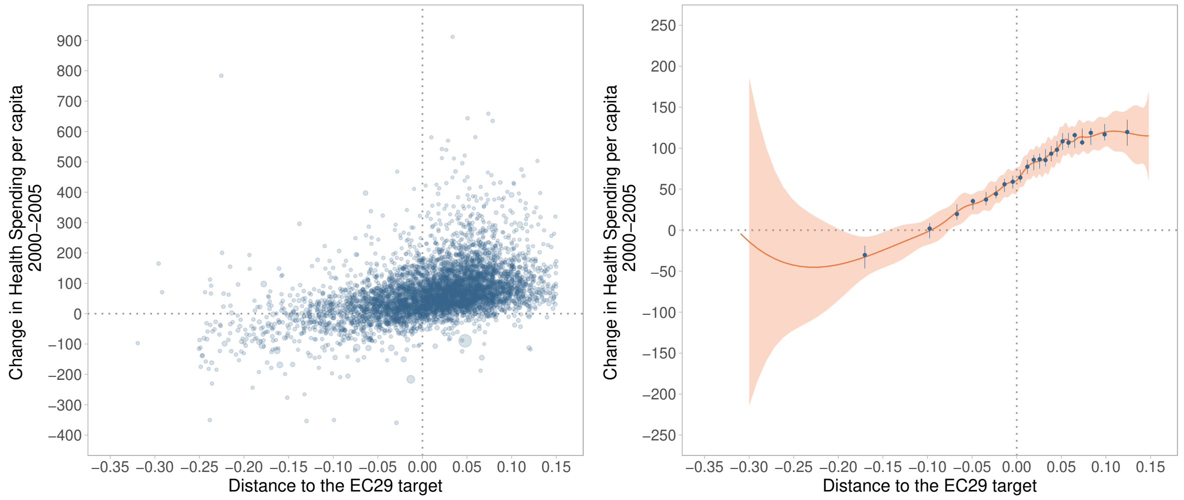
with Figure 2, we observe that increases in health spending were greater in places with initially low levels of spending. Figure A.3 indicates substantial spatial variation both across and within states in the share of own resources spent in health in the baseline year. Also importantly, Figure A.4 documents no clear relationship in correlations between baseline spending and pre-reform evolution in a range of municipal characteristics such as poverty, access to water and sewage, economic development, inequality and so forth. 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 more resources into the public provision of health services across the country, and these changes were overwhelmingly conducted in a way consistent with the thresholds determined by the Constitution.

Figure 3: Changes in Health Spending (2000-2005)

(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)



Notes: Distance to the EC/29 target is calculated from SIOPS data. 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 equals 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).

3 Data

We construct a municipality-by-year panel of data, covering 5,507 Brazilian municipalities over the period of 1998-2010.[11] 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.[12] 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, this monitoring 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 additionally consider measures from FINBRA to observe pre-reform figures.

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[11] Brazil has 5,570 municipalities, we excluded from our sample just the few that did not have fiscal records in the National System of the Public Health Budget.

[12] All spending values are presented in 2010 R$. We used the General Price Index (IGP-M/FGV) to adjust nominal values.

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 (within the first day, first month and first year of life), as well as by cause of death (infectious, respiratory, perinatal, congenital, and so forth). 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 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). Time series plots of infant mortality suggest a steady decline in rates over the years under study, with no clear indication of sharp changes consistent with improvements in reporting (see Figure A.5). We nevertheless consider sensitivity to the inclusion of controls for potential changes in data quality, discussed in Section 4.

3.3 Health Inputs and Service Production

In considering the way which municipalities may alter health investments and provision following the reform, 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 hospital 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).

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. This data is used to create variables on total ambulatory production, primary care ambulatory production, and production by procedure complexity.[13]

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 this 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 hospitalization 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 cause of hospitalization. We once again use the classification from Alfradique et al. (2009) to split hospitalizations into admissions for causes that are amenable and not amenable to primary care services.

Given the range of variables available which capture underlying measures of inputs or access to health, 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).[14] While we work with these two principal indices when considering mechanisms, we additionally further break these down into two sub-indices of 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 Table A.2.

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[13] We also use this 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 that 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.

[14] Specifically, these indexes are constructed by consistently re-scaling variables so that more positive values imply 'better' results, and then aggregating outcomes into a single standardized summary index, where each measure is weighted by the inverse of the variance-covariance matrix among all variables in the index. The indexes are all standardized such that parameter estimates can be cast in terms of standard deviations.

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 coverage of private insurance, compiled from the National Agency of Supplementary Health (ANS), as well as the coverage of non-municipally financed hospitals (private or federal and state hospitals) measured in the AMS census 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 the National System of Information on Hospitalizations discussed previously. Finally, we also consider adult mortality, calculated analogously to infant mortality. Unlike infant mortality, which is measured per live birth, adult mortality is measured per population. We use yearly data on population by age and sex from Datasus to calculate mortality rates.

3.5 Controls

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* transfers per capita, from the Ministry of Social Development.[15] 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. As we discuss below, we consistently consider results both with and without controls.

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[15] The *Bolsa Família* is the main conditional cash transfer program in Brazil.

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:

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

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 required 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 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 j years pre- or 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. 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. State-year fixed effects have the added benefit of also capturing other state-specific policies that might coincidentally affect outcomes in all municipalities within a state, and capture the fact that some health policies and institutions are decentralized to state governments in Brazil. This implies that threats to identification must come specifically from events that differentially affect municipalities within a particular state, and that are more or less affected by the reform due to their baseline share of health spending.

We consider a range of 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 in particular 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}). The time-varying fiscal controls include compliance with the Fiscal Responsibility Law (LRF) (Brasil, 2000) and average health spending per capita in the neighboring municipalities.[16] The LRF determines that municipalities must spend less than 60% of its revenue in personnel. Municipalities not complying or close to the 60% cap might have different incentives when increasing spending relative the municipalities complying with the LRF.[17] We document results without any time-varying controls, and discuss the stability of estimates to the progressive inclusion of controls. 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. Specification (1) thus serves a dual purpose. The first of these is via the examination of coefficients β_{pre} to consider whether – even prior to the reform – municipalities were following different trends, which would cast doubt on our identifying assumptions. And the second is via the coefficients β_{post} to understand how any reform impacts may emerge over time given potential delays in converting health spending changes into health outcome changes.

Yet, a key component of the EC/29 reform is that it may imply differential responses by municipal-

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[16] Castro et al. (2021) show that public spending may have spatial spillovers in Brazil.

[17] This control is potentially endogenous to the EC29, and will be used only in auxiliary specifications.

ities spending more or less at baseline. Municipalities spending less than 15% of their budget on health 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 $Above_{m,pre} = \mathbb{1}\{Spending_{m,pre} \geq Target\}$. Using this binary split, we allow for the response to spending targets to differ for above and below spending-target municipalities by estimating:

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

This replicates equation (1), however for ease of presentation, we take the absolute value of the distance to the target. This transformation ensures that coefficients can be interpreted as capturing the bite of the reform for each municipality, and more clearly visualize differential results above and below target.[18] 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 set of J pre-event leads and K post-event lags are included, where we consistently omit an indicator 1 year prior to reform implementation as a baseline reference period. This is thus an interacted event-study following equation (1).

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[18] 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.

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 reform coefficient, namely:

$$Y_{mts} = \alpha + \beta\,(Dist_{m,pre} \times Post_t) + \delta_{st} + \mu_m + \theta(Z_{m,pre} \times \lambda_t) + \gamma\,X_{mts} + \varepsilon_{mts} \qquad (3)$$

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, Figure A.4 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 (panels (a) to (k)), and in per capita health spending before the EC/29 reform by using the available FINBRA data (panel (l)). We do not observe any systematic associations between changes in relevant determinants of population health 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.[19]

We formally define these assumptions, and their implications in our setting, at more length in Appendix C. In practice, we argue that the strong parallel trends assumption is likely reasonable here. As is salient in Figures 3a-3b, the EC/29 spending reform was approximately binding.[20] 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.[21] 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 C.

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[19] 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.

[20] 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.

[21] 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. What's more, 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.

5 Results

In this section we first present the estimates of the impact of EC/29 on fiscal outcomes, seeking to understand shifts in spending both 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 7). 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 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 threefold greater in comparison to revenues, though coefficients are not statistically significant. 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

Table 1: Fiscal Reactions, in natural logarithm of Reais per capita

	(1)	**(2)**	**(3)**	**(4)**
Panel A: FINBRA				
Total Revenues	-0.124	-0.01	0.029	0.057
	(0.139)	(0.117)	(0.112)	(0.112)
Total Spending	-0.048	0.056	0.093	0.082
	(0.137)	(0.115)	(0.112)	(0.111)
Health Spending	1.075***	1.199***	1.245***	1.232***
	(0.25)	(0.225)	(0.224)	(0.224)
Non-Health Spending	-0.242*	-0.152	-0.115	-0.128
	(0.131)	(0.111)	(0.107)	(0.106)
Non-Health Social Spending	-0.118	-0.069	-0.041	-0.053
	(0.164)	(0.136)	(0.134)	(0.133)
Non-Social Spending	-0.296*	-0.163	-0.119	-0.131
	(0.174)	(0.148)	(0.141)	(0.141)
Panel B: SIOPS				
Total Health Spending	2.258***	2.352***	2.375***	2.378***
	(0.258)	(0.209)	(0.198)	(0.198)
From Own Resources	5.519***	5.565***	5.593***	5.595***
	(0.278)	(0.267)	(0.255)	(0.255)
From Other Resources	1.533	1.481	1.479	1.48
	(1.503)	(1.252)	(1.24)	(1.235)
Personnel	2.512***	2.539***	2.559***	2.538***
	(0.418)	(0.357)	(0.355)	(0.353)
Investment	5.576***	5.231***	5.235***	5.256***
	(1.037)	(0.736)	(0.729)	(0.73)
Outsourced (3rd party services)	0.792	1.127*	1.155*	1.181*
	(0.73)	(0.648)	(0.628)	(0.622)
Admin, Management and Others	4.475***	4.35***	4.368***	4.379***
	(1.059)	(0.953)	(0.948)	(0.939)
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 is 63,280 for FINBRA variables and 55,232 for SIOPS variables. 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. * $p<0.10$; ** $p < 0.05$; *** $p < 0.01$.

spending and debt.[22]

The remaining results from Panel A indicate that, across all classes, the EC/29 reform drives large increases in health spending, with no other 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 the results. In column 3 of Table 1, the point estimate of 1.245 indicate an increase of 12.45% 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 23.75% 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 (56% 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 (52%) and in administrative expenses (43%) are particularly large, followed by spending in personnel (25%) and outsourcing (11%).[23]

We consider dynamic effects of the reform on health spending in Figure 4. 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 4a plots the dynamic effects on spending per capita on health and sanitation. We observe no significant pre-trends in spending and a clear 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.

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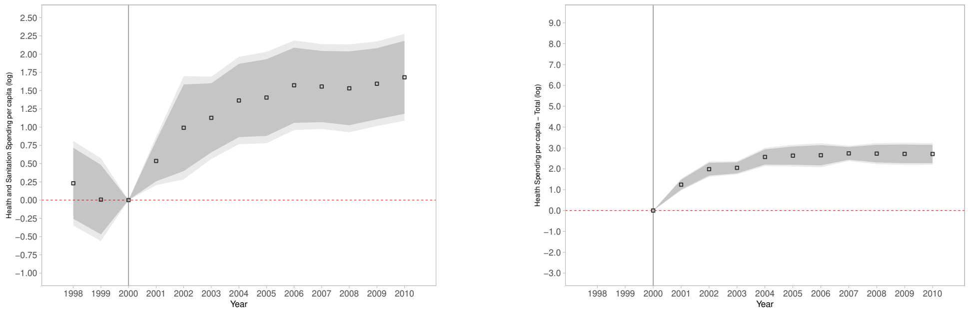
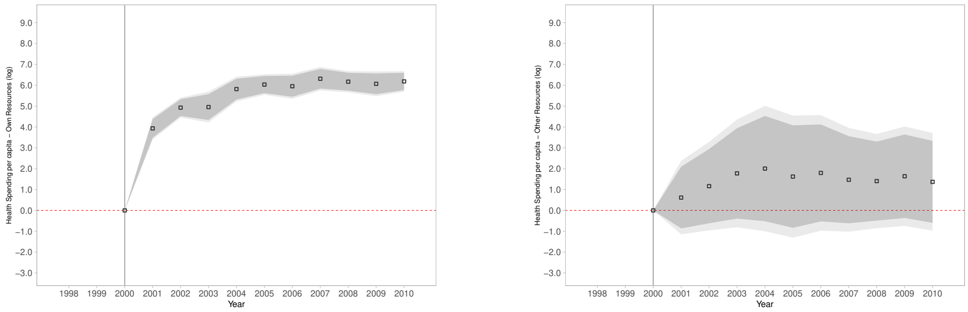
[22] The Fiscal Responsibility Law establishes that municipal spending can exceed revenues by no more than 20%, with municipalities having 15 years to meet the balance (Brasil, 2000).

[23] Note that baseline statistics show little resources allocated in investments within total municipality health spending, the great majority of resources were allocated in human resources and in administrative expenses.

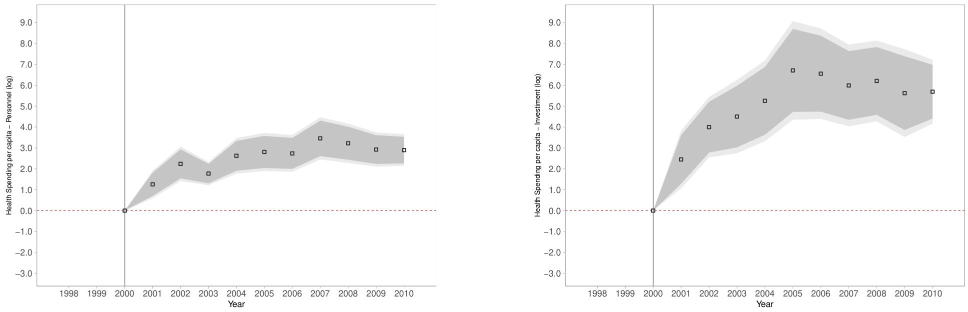
Figure 4: Dynamic Effects on Health Spending, by Spending Classes and Source of Funding



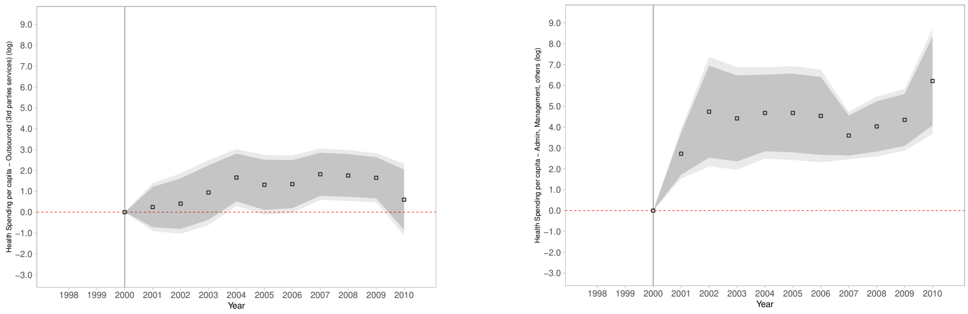
(a) Total Health Spending (FINBRA) (b) Total Health Spending (SIOPS)



(c) From Own Resources (d) From Other Resources



(e) Human Resources (f) Investment



(g) Outsourced (3rd party services) (h) Admin, Management and Others

Notes: 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 D.1.

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 4a, is similar to that observed for total health spending recorded from SIOPS (Figure 4b), and as noted above, is largely driven by spending of own resources. Panels (e)-(h) of Figure 4 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 3 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, documented 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 5 further breaks down the impacts of spending reforms on municipalities' fiscal responses. This figure is analogous to Figure 4, 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 3, we observe in Figure 5 that municipalities below the target systematically increased 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 relatively smaller. 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. 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.[24] We also note that point estimates for those below the target are often greater than the average effects shown in Figure 4. For example, if considering total health spending as measured by SIOPS, from around 2004 onwards point estimates converge on around 3.6-3.9, indicating that a 10 p.p. distance below the spending target is associated with increases in health spending by around 36-39%. This is larger than the value of 23.8% reported in

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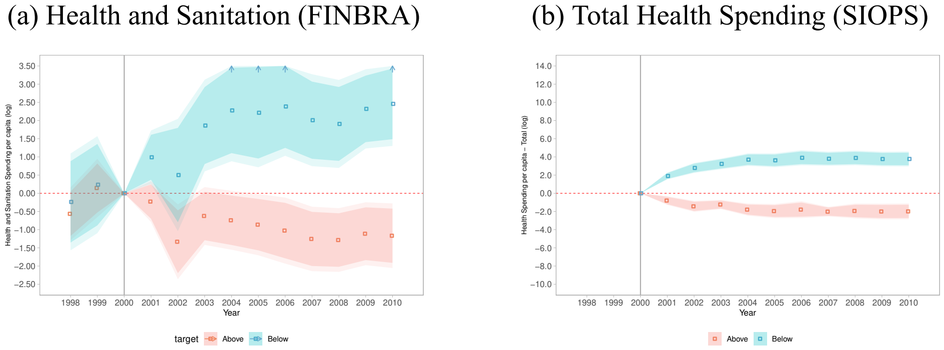
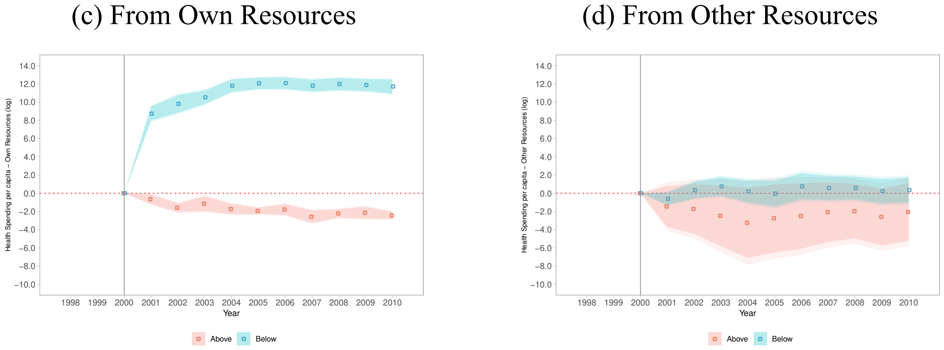
[24] Note also that point estimates for those below the target are positive and slightly larger in comparison to the average effects shown in Figure B.1, at least until 2004, but the data are noisier and confidence intervals are larger as well.

Figure 5: Effects on Health Spending per capita (Distributional Effects)



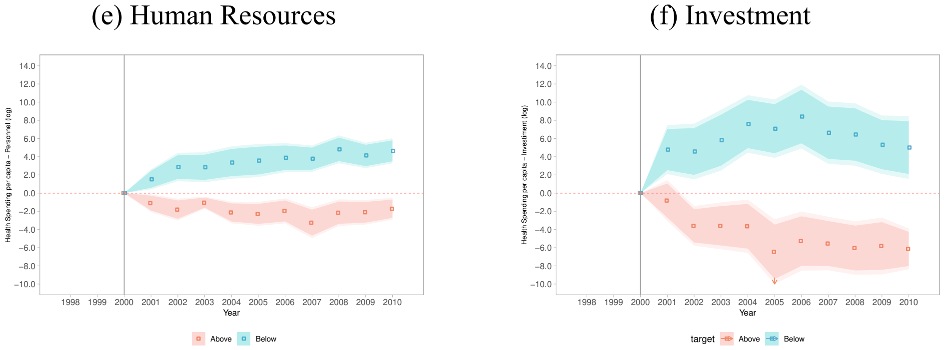
(a) Health and Sanitation (FINBRA)

(b) Total Health Spending (SIOPS)



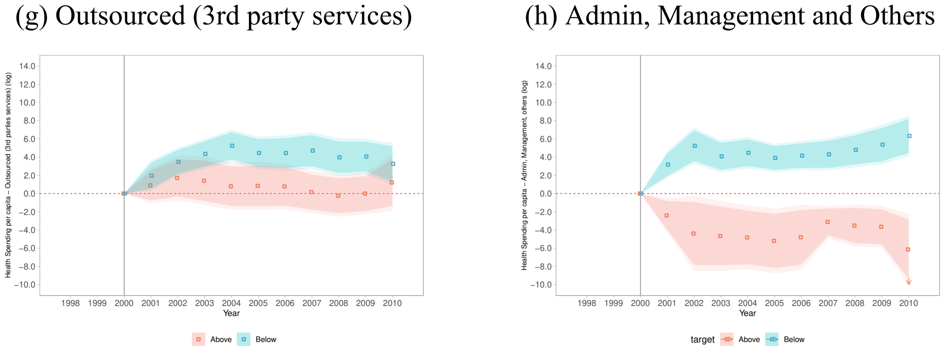
(c) From Own Resources

(d) From Other Resources



(e) Human Resources

(f) Investment



(g) Outsourced (3rd party services)

(h) Admin, Management and Others

Notes: 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 D.2.

Table 1, confirming greater spending increases for these municipalities, holding fixed changes that occurred in the group above the target.

5.2 Infant Mortality Rates

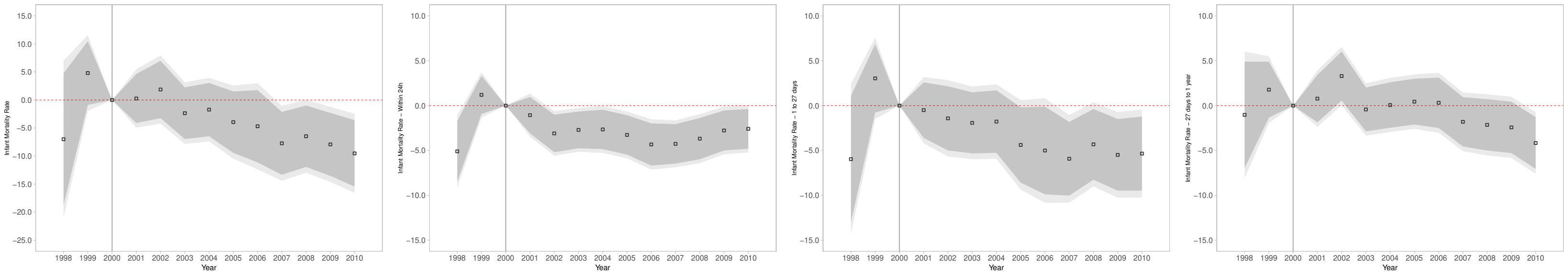
5.2.1 Main Results

We now assess whether the shifts documented on spending translate into effects on health outcomes. Figure 6 presents dynamic effects for all-cause infant mortality, and infant mortality by time of death. The top row of this figure, Panels (a)-(d), 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. 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 we additionally document how these effects line up with the timing of potential mechanism variables.

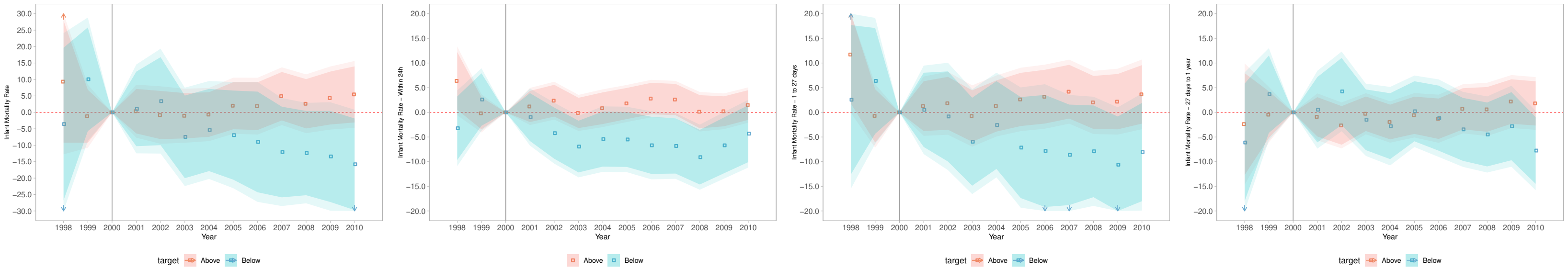
In considering infant mortality by the time of death, we observe broadly similar results for deaths occurring in the neonatal period, which refers to the first 28 days of life. In panel (b) we observe rapid declines in deaths within the first 24 hours of life, while in panel (c) we find a similar pattern 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 -7.7 for total mortality, -4.2 for deaths within the first 24 hours and -5.9 for the remaining neonatal period. Taking a 10 p.p. increase in health spending, these represent, respectively, reductions of 0.77 (corresponding to 3.3% of the baseline average of this measure), 0.42 (7.6%) and 0.59 (4.3%).

Figure 6: Effects on Infant Mortality Rates, Total and By Timing of Death

(a) Total (b) Deaths within 24 hours (c) Deaths between 1-27 days (d) Deaths from 27 days to 1 year



(e) Total (f) Deaths within 24 hours (g) Deaths between 1-27 days (h) Deaths from 27 days-1 year



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 D.3 (panels (a)-(d)) and D.4 (panels (e)-(h)).

Municipalities below the 15% threshold increased spending, whereas those exceeding the target reduced spending. While Panels (a)-(d) of Figure 6 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.

In Panels (e)-(h) of Figure 6 we 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 0.67 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 broadly flat trends in municipalities which cut health spending 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.

5.2.2 Effects on IMR by Cause of Death

Figures B.3 and B.4 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, specially 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 spending increases in below-target municipalities, rather than by shifts owing to spending declines in above target municipalities (Figure B.4).

5.2.3 Implied Elasticities

In general, papers estimating the causal relationship between health spending and mortality run log-log regressions and present estimates for the elasticity of mortality with respect to health spending.[25] We explicitly choose not to apply transformations to our health outcomes variables due to the number of observations with values equal to 0, notably those related to mortality.[26] 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. 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:

$$\begin{aligned}
\text{Elasticity}_j &\equiv \frac{\left(\frac{\partial \text{IMR}_{mts}}{\partial Dist_{m,pre} \times EC29_{t+j}}\right) / \text{IMR}_{pre}}{\left(\frac{\partial \text{Health Spending}_{mts}}{\partial Dist_{m,pre} \times EC29_{t+j}}\right) / \text{Health Spending}_{pre}} \\
&= \frac{\left(\partial \text{IMR}_{mts} / \text{IMR}_{pre}\right) \big|_{t=j}}{\left(\partial \text{Health Spending}_{mts} / \text{Health Spending}_{pre}\right) \big|_{t=j}} \qquad (4)
\end{aligned}$$

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

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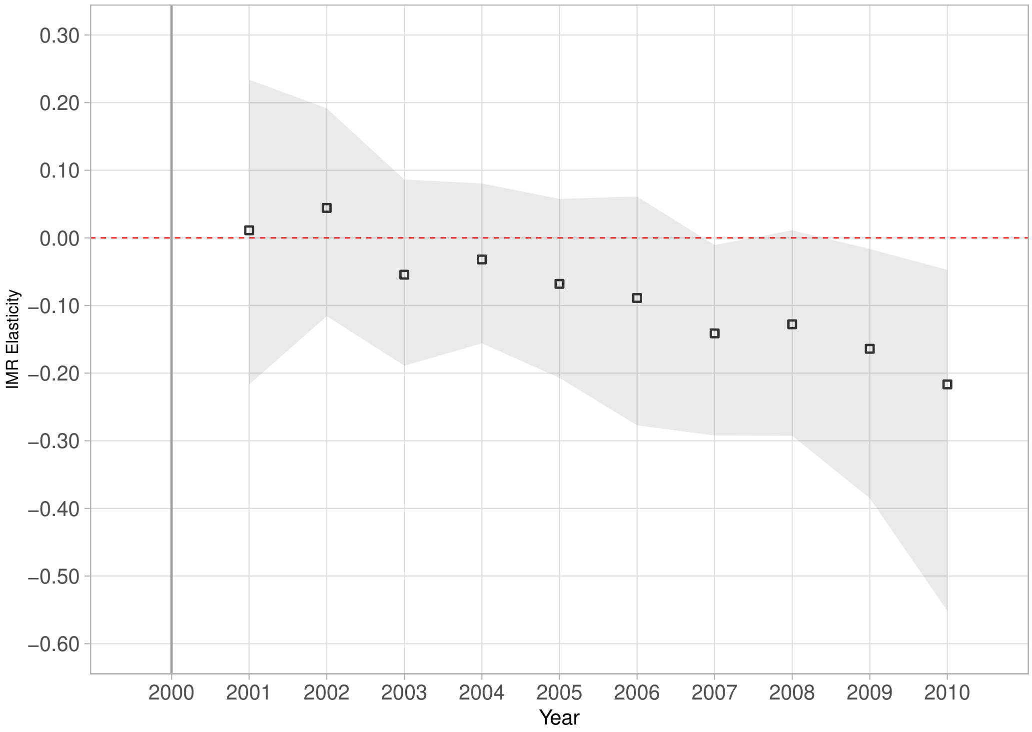
[25] See, for example, Filmer and Pritchett (1999) and Crémieux et al. (1999b), among others.

[26] 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).

to enter improved paths for health outcomes. The quantities in parentheses in the numerator and denominator in equation (4) 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 (4), we present confidence intervals on these estimates.[27]

Figure 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 (4), 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.

Figure 7 and 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 elas-

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[27] These confidence intervals are estimated by block bootstrap where municipalities are resampled, the numerator and denominator of equation (4) 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 from the empirical quantiles 2.5 and 97.5 of bootstrap resamples.

ticity 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 -0.2 ten years following the reform. Our results suggest that even 10 years out upper ends of confidence intervals can rule out estimates of 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 and spending increases, we observe that mortality is more sensitive to increases in spending than declines in spending (Figure B.5), 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 -0.126 (column 7) for infant mortality, and greater proportional changes in mortality in certain classes such as mortality within the first day (-0.289) or for specific causes such as infectious, respiratory, nutritional, and perinatal mortality.

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 access to health services, health production outputs, and health inputs at the municipal level. Figure 8 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, 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.[28]

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, while spending increased. 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 8 we observe that increases in access and production as well as in health inputs owe to municipalities which were below the target. Indices 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 indices of access and production and of health inputs. As benchmark, 2005 point estimates are 0.925 and 1.054, respectively (see Appendix Table D.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 indices are considered to avoid excessive multiple testing, however we can further separate them in sub-indices. Specifically, the access and production index can be 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 8 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,

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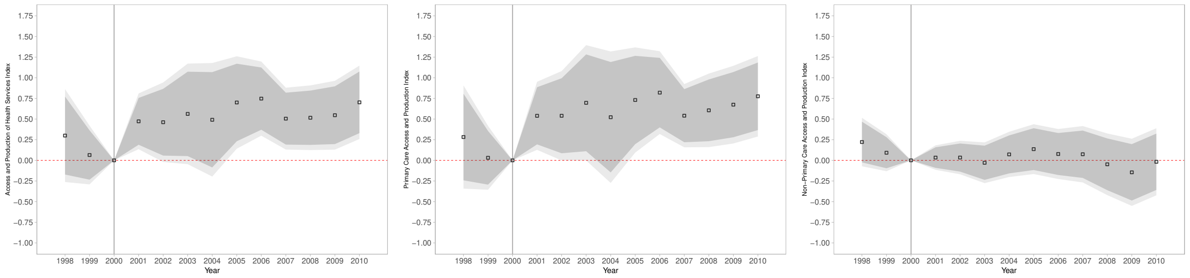
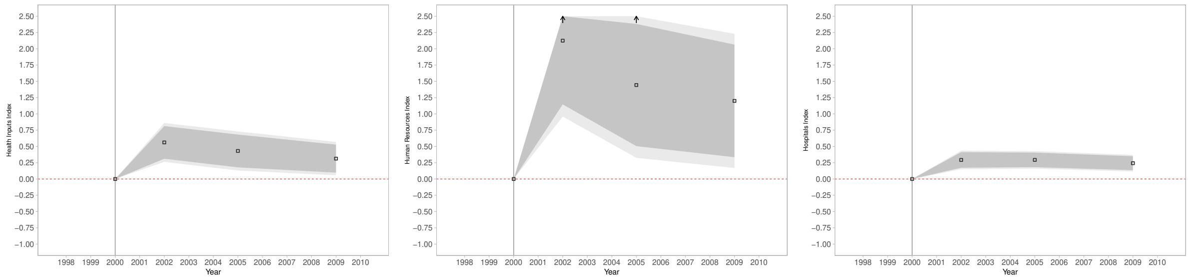
[28] The complete list of index components is available in Appendix Table A.2.

Figure 8: Effects on Access to Services, Production and Health Inputs

(a) Access and Production of Health Service (b) Primary Care Access and Production (c) Non-Primary Care Access and Production

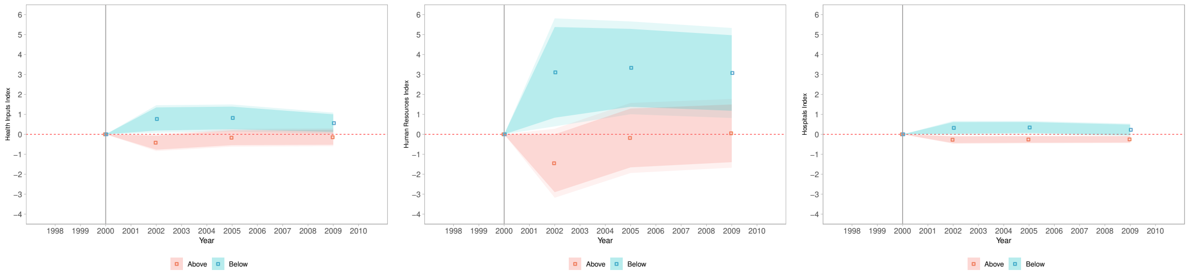


(d) Health Inputs (e) Human resources (f) Hospitals



(g) Access and Production (Distributional) (h) Primary Access/Prod. (Distributional) (i) Non-Primary Access/Prod. (Distributional)

(j) Health Inputs (Distributional) (k) Human resources (Distributional) (l) Hospitals (Distributional)



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 D.5 (panels (a)-(f)) and D.6 (panels (g)-(l)).

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 16% 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 4, panel (c)). 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 4, panel (d)). 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 an imprecisely estimated 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

In this section we examine whether the spending reform affected other potential pathways connecting variation in spending and in health outcomes. 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.

6.2.1 Effects on the Private Sector and Other Public Providers

Figure 9 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 Figure 9 present aggregate estimates, while bottom panels consider distributional effects. Figures 9a-9c 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. Figure 9a, in line with increased infrastructure spending, shows clear evidence of increases in availability of hospitals administered by municipalities.[29] 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.

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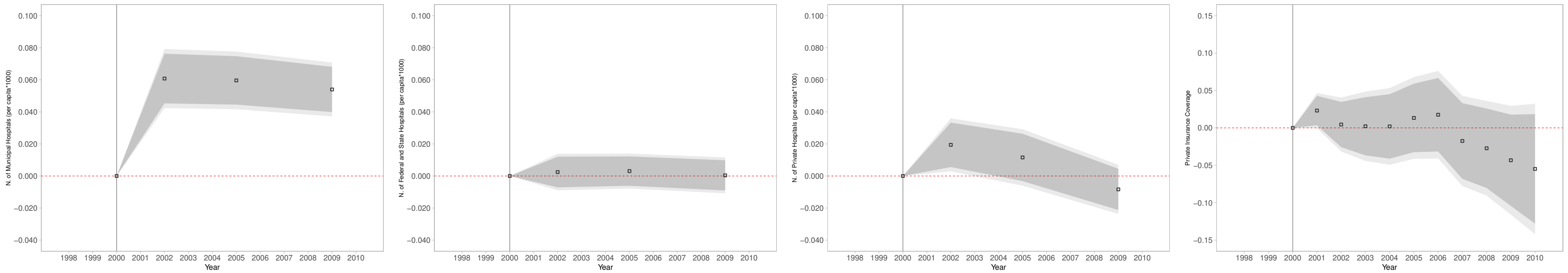
[29] It is important to note that municipal hospitals are typically small-scale facilities, providing inpatient services but often having on average around 50 or fewer hospital beds (Carpanez and Malik, 2021).

Considering private hospitals, there is some relatively weak evidence in favour of complementarities, at least in the short term. Distributional results in Figures 9e-9g 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 5). 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 Figure 9d, 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 9h similarly point to largely flat patterns at least in the 7 years following the passage of the EC/29 amendment.

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 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.

Figure 9: Spending Reform and Health System Spillovers



(a) Municipal Hospitals

(b) State and Federal Hospitals

(c) Private Hospitals

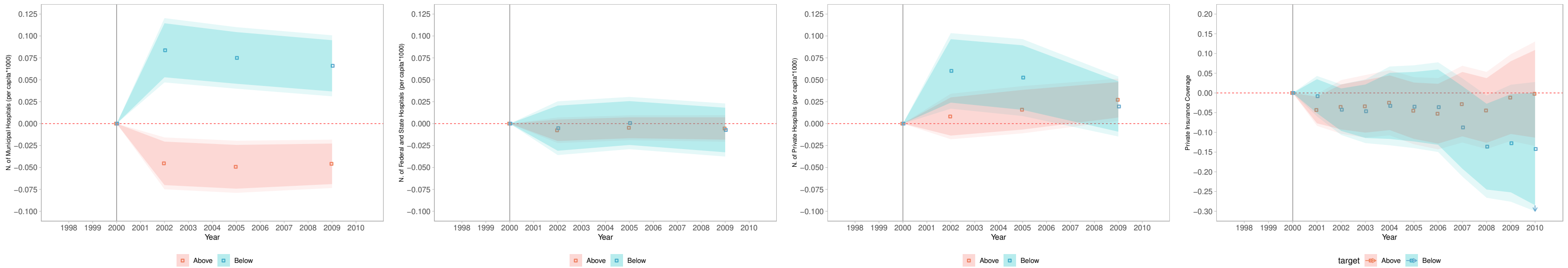
(d) Private Insurance

(e) Municipal Hospitals (Distributional)

(f) State and Federal Hospitals (Distributional)

(g) Private Hospitals (Distributional)

(h) Private Insurance (Distributional)



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. 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 D.7 (panels (a)-(d)) and D.8 (panels (e)-(h)).

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.

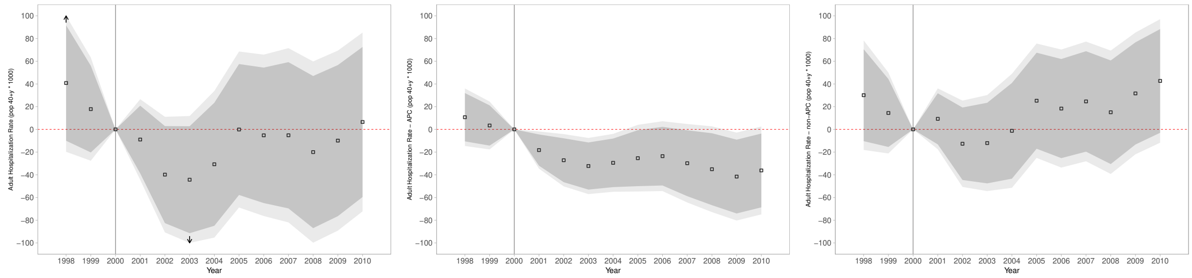
In Figure 10 we present results where we consider adult hospitalisation rates (top row) and adult mortality rates (bottom row). Outcomes consider all adults aged 40 years and above, and are standardized as rates per 1,000 individuals. We do not observe evidence consistent with crowding out of health 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 only weak evidence suggestive of potentially mortality declines at older ages too.

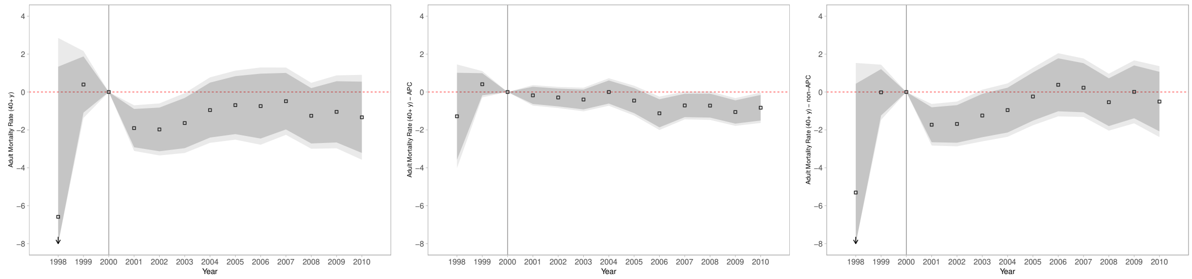
6.2.3 Geographical Spillovers

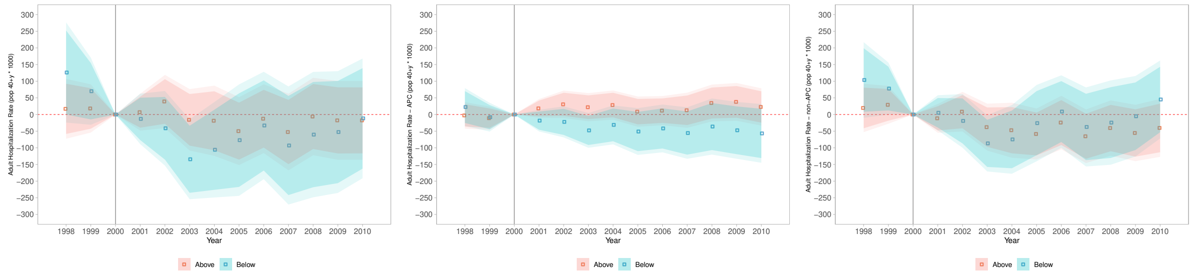
An alternative consideration is whether greater spending in a given municipality may reduce the rate of individuals seeking treatment in other municipalities, or attract residents from other municipalities to receive treatment. Both such phenomena could lead to greater congestion, by its turn limiting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

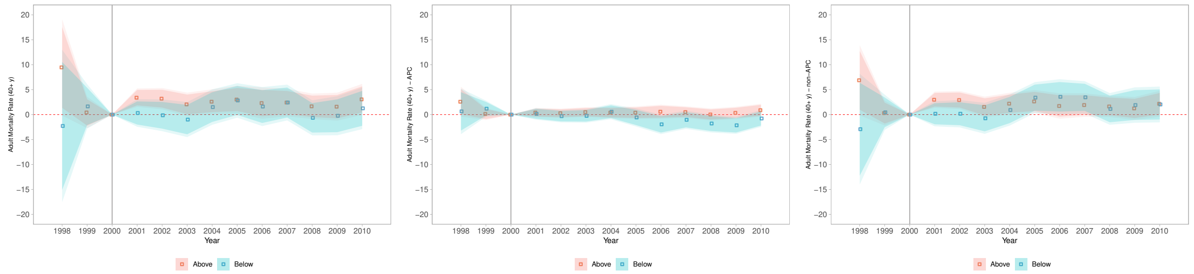
Geographical spillovers are not expected in primary care services, as access is restricted to catchment areas defined within the municipality of residence. We therefore focus on relatively higher complex services by taking advantage of the information contained in the hospitalization microdata, which 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 mu-

Figure 10: Spending Reform Impacts on Adult Hospitalization and Mortality rates


(a) Adult Hospitalization (b) Adult Hospitalizations – APC (c) Adult Hospitalization – Non-APC


(d) Adult Mortality (e) Mortality – APC (f) Mortality – Non-APC


(g) Adult Hospitalization (Distributional) (h) Hospitalization – APC (Distributional) (i) Hospitalization – Non-APC (Distributional)


(j) Adult Mortality (Distributional) (k) Mortality – APC (Distributional) (l) Mortality – Non-APC (Distributional)

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. 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. Tabular output including observation numbers is available as Appendix Tables D.9 (panels (a)-(f)) and D.10 (panels (g)-(l)).

nicipalities receiving treatment in a given municipality (hospitalization inflows). In Figure 11 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 that. 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.

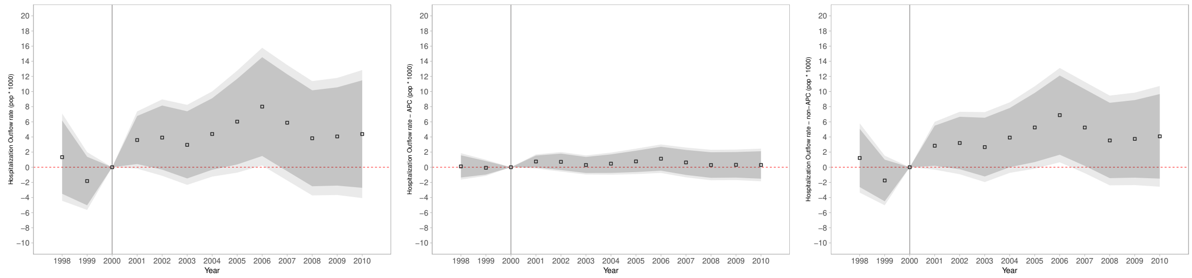
7 Additional Robustness Checks

We consider a number of robustness checks, which in the interests of space are presented in Appendix D. 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.

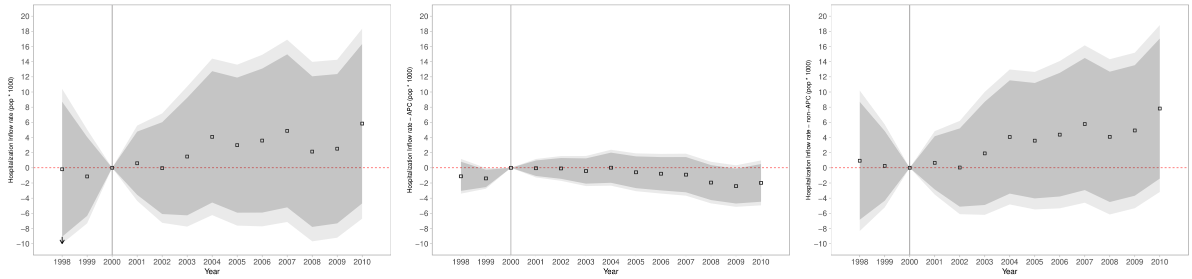
In principal models displayed in the paper we control for baseline municipality characteristics measured from the census and a data quality proxy interacted with a linear time trend, as well as time-varying municipal controls. In Appendix Figures we document the stability of principal dynamic estimates 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,

Figure 11: Patient Mobility and Geographical Spillovers

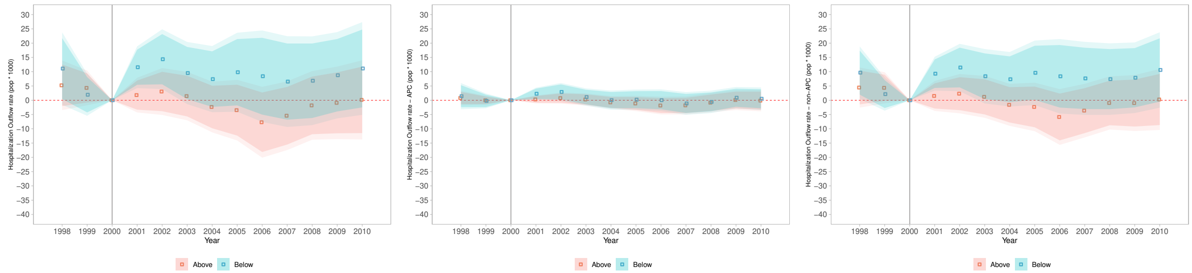
(a) Total Hospitalization Outflow (b) Outflow: Amenable to Primary Care (c) Outflow: Non Amenable to Primary Care



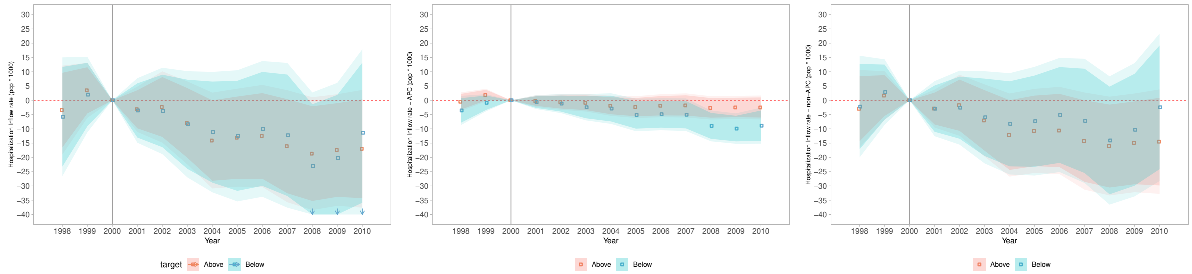
(d) Total Hospitalization Inflow (e) Inflow: Amenable to Primary Care (f) Inflow: Non Amenable to Primary Care



(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)



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. 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. Tabular output including observation numbers is available as Appendix Tables D.11 (panels (a)-(f)) and D.12 (panels (g)-(l)).

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 D1 (spending measures), D3 (infant mortality), D5 (input and health service measure), D7 (health system measures), D9 (adult health outcomes), and D11 (geographic spillovers). 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 D2 (spending measures), D4 (infant mortality), D6 (input and health service measure), D8 (health system measures), D10 (adult health outcomes), and D12 (geographic spillovers). 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 reweights 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 C). These results are presented as dashed lines in Appendix Figures D1, D3, D5, D7, D9, and D11. 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 C1).[30]

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 provides two contributions to the understanding of how government health spending shapes health outcomes. Firstly, it provides evidence from a quasi-experimental setting. This allowed us to understand the nature of a large health resource shock using an empirical design based on arbitrary fiscal threshold. Secondly, beyond simply isolating the effects of spending shock on downstream health outcomes, we examine the implications of this shock as it flows through the health production function.

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 observe that for municipalities spending below the target at baseline, health spending sharply increased, resulting in an expansion of inputs including hospitals and human resources for health. Access to health care services increased, ultimately 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.

It is illustrative to consider what the implications of these results are in terms of health care costs and benefits. Because we estimate the reform's impact on health care spending and its impact on mortality declines, we can ask whether the reform pays for itself in terms of lives saved. Combining our results 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

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[30] The only exception to this is the outcome of private hospital supply (Appendix Figure D7, panel (c)), where we observe negative effects, suggesting this outcome in particular should be viewed as sensitive to weighting considerations.

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 at an aggregate level the reform increased spending by around R$10 billion in 2001 to R$60 billion in later years (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 700 infant deaths 3 years post-reform, up to 2,500 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$4.8 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$764,000.

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. Like that of Brazil, for instance, constitutions of South Africa, Thailand, Kenya, Rwanda, Colombia, Ghana, The Philippines, Tanzania and Zambia were adopted or amended in the past 2 or 3 decades, and include formal provisions for access to health. Secondly, 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. The results from this paper also 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, the results suggest that large increases in health care spending since the turn of the century can lead, at least in some cases, to improvements in health outcomes, and can pay for themselves multiple times over.

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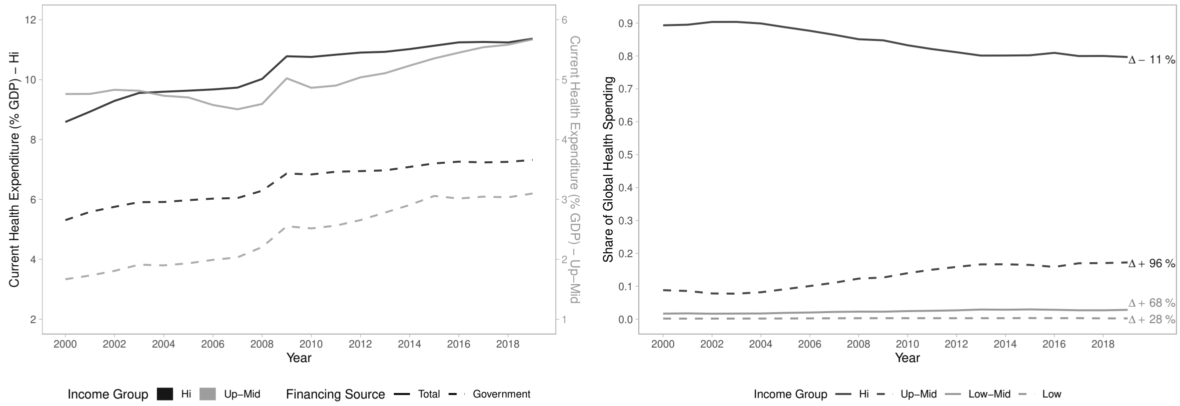
ONLINE APPENDIX

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

Michel Szklo *Damian Clarke* *Rudi Rocha*

A Descriptive Figures and Summary Statistics

Figure A.1: Descriptive Trends of Global Health Spending

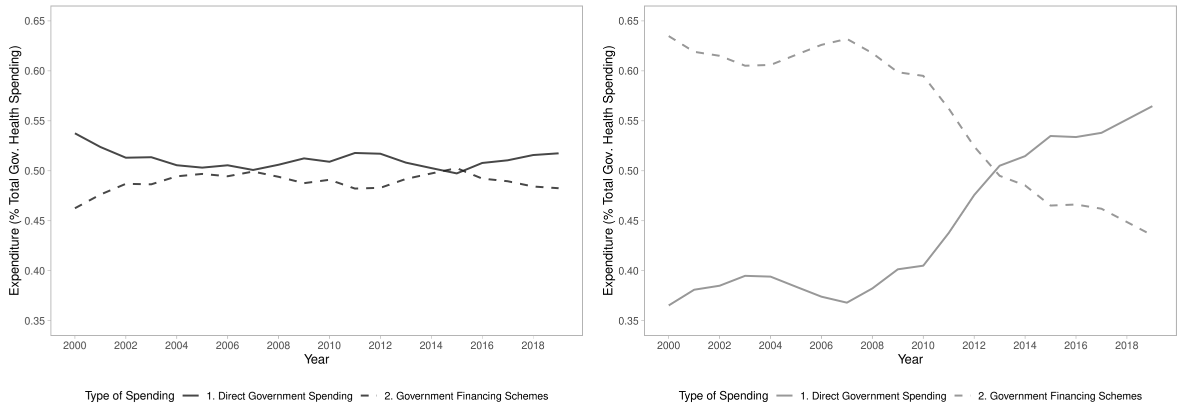


(a) Total Health Spending

(b) Spending Proportions by Income Group

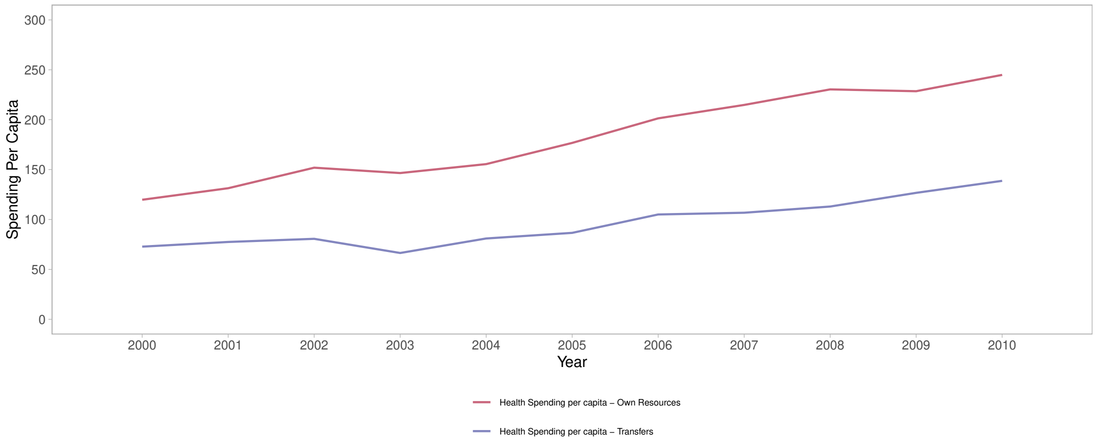
(c) Government Spending: High Income

(d) Government Spending: Upper-Middle Income

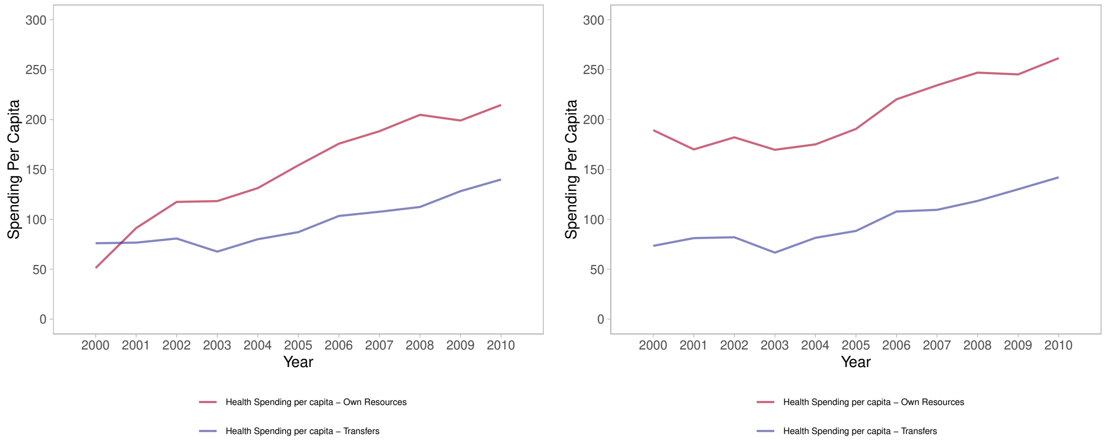


Notes: Descriptive trends in total health spending are presented. Data is drawn from the World Health Organization's Global Health Expenditure Database. Panel (a) presents total health spending as a percent of GDP in high and upper middle income countries. Panel (b) presents the makeup of global health spending by country income group. Panels (c) and (d) present the composition of government health spending (direct versus government financed schemes) in high and upper middle income countries respectively.

Figure A.2: Health Spending Trends



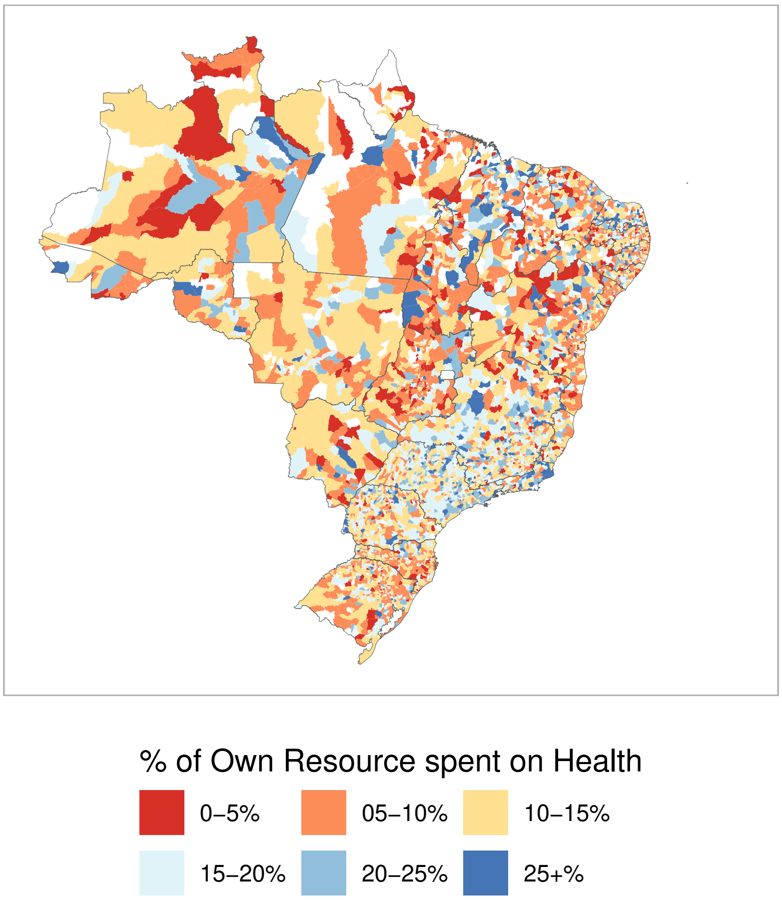
(a) Health Spending by Source - Full Sample



(b) Health Spending by Source - Bottom Quartile (c) Health Spending by Source - Top Quartile)

Notes: Trends in health spending per capita are calculated using SIOPS spending data (refer to Section 3 for more details). All amounts are plotted in R$ as at 2010. Red lines located at the top of each plot measure spending financed by each municipal's own resources, while blue lines (located below in each plot) reflect spending financed from transfers by State or Federal governments.

Figure A.3: 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.	Min	Max	Obs.	Source of Data
EC 29 Variables						
Own Resources Spent on Health	0.138	0.068	0	0.802	5224	Datasus/SIOPS
Distance to the EC29 Target	0.012	0.068	-0.652	0.15	5224	Datasus/SIOPS
Public Revenue						
Total Revenue per capita	1254.5	725.8	132.5	12338.6	5271	Finbra
Public Spending						
Total Spending per capita	1238.9	712.3	129.7	12950.8	5271	Finbra
Spending by Category - per capita						
Health and Sanitation	212.11	136.09	0.04	1992.31	5254	Finbra
Non-Health Spending per capita	1027.457	614.252	89.086	10958.511	5271	Finbra
Non-Health Social Spending per capita	577.598	336.93	0	5110.228	5271	Finbra
Non-Social Spending per capita	449.859	323.26	32.002	5848.283	5271	Finbra
Public Health Spending						
Total Health Spending per capita	192.33	109.21	24.63	1397.58	5185	Datasus/SIOPS
Health Spending by Source (p.c.)						
Own Resources spending	119.54	95.64	0	1232.88	5185	Datasus/SIOPS
Transfers Spending	72.80	49.95	0	1099.10	5185	Datasus/SIOPS
Health Spending by Type (p.c.)						
Human Resources Spending	71.31	61.31	0	1118.76	5185	Datasus/SIOPS
Investment Spending	14.57	26.69	0	361.97	5185	Datasus/SIOPS
3rd parties services Spending	33.05	43.06	0	1041.14	5185	Datasus/SIOPS
Other Spending	73.40	52.62	0	602.70	5185	Datasus/SIOPS

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 IBGE/AMS data (“FINBRA”) are measured at year 1999 and statistics for all remaining variables (“SIOPS”) refer to the baseline year of 2000.

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

	Mean	Std. Dev.	Min	Max	Obs.	Source of Data
Primary Care Coverage						
Extensive Margin (share)						
Population covered by ACS	0.635	0.409	0	1	5507	Datasus/SIAB
Population covered by PSF	0.311	0.383	0	1	5507	Datasus/SIAB
Intensive Margin (per capita)						
N. of People Visited by PCA	0.271	0.285	0	2.798	5507	Datasus/SIAB
N. of People Visited by ACS	0.121	0.18	0	1.518	5507	Datasus/SIAB
N. of People Visited by PSF	0.15	0.252	0	1.834	5507	Datasus/SIAB
N. of Household Visits & Appointments	1.876	2.541	0	88.85	5507	Datasus/SIAB
N. of Household Visits, Appointments by ACS	1.072	2.156	0	85.989	5507	Datasus/SIAB
N. of Household Visits, Appointments by PSF	0.8	1.505	0	43.389	5507	Datasus/SIAB
Health Human Resources (per capita × 1,000)						
N. of Health Professionals	5.104	4.825	0	187.904	5507	IBGE/AMS
N. of Doctors	1.529	2.385	0	95.132	5507	IBGE/AMS
N. of Nurses	1.159	1.636	0	95.097	5507	IBGE/AMS
N. of Nursing Assistants	1.26	1.456	0	22.009	5507	IBGE/AMS
N. of Administrative Professionals	1.155	1.251	0	36.599	5507	IBGE/AMS
Primary Care Related Infrastructure & HR						
N. of Health Facilities (per capita × 1,000) with						
Ambulatory Service and ACS Teams	0.14	0.197	0	2.41	5493	Datasus/SIA
Ambulatory Service and Community Doctors	0.082	0.154	0	1.957	5493	Datasus/SIA
Ambulatory Service and ACS Nurses	0.072	0.156	0	2.41	5493	Datasus/SIA
Ambulatory Service and PSF Teams	0.083	0.159	0	2.41	5493	Datasus/SIA
Ambulatory Service and PSF Doctors	0.077	0.149	0	1.957	5493	Datasus/SIA
Ambulatory Service and PSF Nurses	0.075	0.149	0	2.41	5493	Datasus/SIA
Ambulatory Production (per capita × 1,000)						
N. Outpatient Procedures	8.8	4.55	0	48.258	5507	Datasus/SIA
N. Primary Care Outpatient Procedures	7.415	3.974	0	39.367	5507	Datasus/SIA
N. Low & Mid Complex. Outpatient Procedures	9.467	5.801	0	171.126	5493	Datasus/SIA
N. High Complexity Outpatient Procedures	0.005	0.052	0	2.58	5493	Datasus/SIA
Access to Health Services (share)						
Prenatal Vists: Unknown	0.044	0.094	0	1	5460	Datasus/SINASC
Prenatal Visits: None	0.053	0.077	0	0.921	5437	Datasus/SINASC
Prenatal Visits: 1-6	0.53	0.216	0	1	5507	Datasus/SINASC
Prenatal Visits: 7+	0.375	0.235	0	1	5507	Datasus/SINASC
Hospitalization (per capita × 1,000)						
Maternal Hospitalization Rate	50.979	36.041	0	2194.472	5507	Datasus/SIH
Infant Hospitalization Rate - APC	207.185	252.586	0	10000	5507	Datasus/SIH
Infant Hospitalization Rate - non-APC	73.308	119.599	0	4410.256	5507	Datasus/SIH

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.

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

	Mean	Std. Dev.	Min	Max	Obs.	Source of Data
Health Infrastructure (per capita × 1,000)						
N. of Municipal Hospitals	0.06	0.138	0	1.357	5507	IBGE/AMS
N. of Federal and State Hospitals	0.015	0.084	0	1.892	5507	IBGE/AMS
N. of Private Hospitals	0.03	0.058	0	0.609	5507	IBGE/AMS
N. of Health Facilities with Ambulatory Service	0.517	0.355	0	3.628	5493	Datasus/SIA
Adult Hospitalization (per capita × 1,000)						
Adult Hospitalization	357.192	221.88	0	12116.608	5466	Datasus/SIH
Adult Hospitalization - APC	130.952	90.109	0	2613.074	5466	Datasus/SIH
Adult Hospitalization - non-APC	226.24	157.109	0.	9503.534	5466	Datasus/SIH
Adult Mortality Rates (per capita × 1,000)						
Adult Mortality	14.474	5.411	0	45.946	5466	Datasus/SIM
Adult Mortality - APC	3.858	2.434	0	17.57	5466	Datasus/SIM
Adult Mortality - non-APC	10.617	4.176	0	35.135	5466	Datasus/SIM
Hospitalization Flows (per capita × 1,000)						
Total Hospitalization Outflow	39.865	54.344	0.170	3434.08	5466	Datasus/SIH
Outflow Amenable to Primary Care	9.461	13.976	0	673.039	5466	Datasus/SIH
Outflow Non Amenable to Primary Care	30.404	42.217	0.170	2761.042	5466	Datasus/SIH
Total Hospitalization Inflow	10.297	25.618	0	673.481	5466	Datasus/SIH
Inflow Amenable to Primary Care	2.975	8.433	0	194.67	5466	Datasus/SIH
Inflow Non Amenable to Primary Care	7.323	19.778	0	613.946	5466	Datasus/SIH

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.

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

	Mean	Std. Dev.	Min	Max	Obs.	Source of Data
Infant Mortality Rate (per 1,000)						
All Cause IMR	23.07	26.16	0	1000	5507	Datasus/SIM
Amenable to Primary Care (APC)	2.10	7.10	0	333.33	5507	Datasus/SIM
non-APC	20.97	22.29	0	666.67	5507	Datasus/SIM
Fetal	0.003	0.08	0	3.57	5507	Datasus/SIM
Within 24h	5.55	10.15	0	333.333	5507	Datasus/SIM
1 to 27 days	13.73	15.89	0	333.33	5507	Datasus/SIM
27 days to 1 year	9.34	16.34	0	666.67	5507	Datasus/SIM
Infectious	1.99	7.03	0	333.33	5507	Datasus/SIM
Respiratory	1.52	4.45	0	142.86	5507	Datasus/SIM
Perinatal	11.04	16.32	0	666.67	5507	Datasus/SIM
Congenital	2.13	5.01	0	93.02	5507	Datasus/SIM
External	0.37	1.91	0	43.48	5507	Datasus/SIM
Nutritional	0.60	3.22	0	166.67	5507	Datasus/SIM
Other	0.87	3.59	0	142.86	5507	Datasus/SIM
Ill-Defined	4.55	10.68	0	142.86	5507	Datasus/SIM

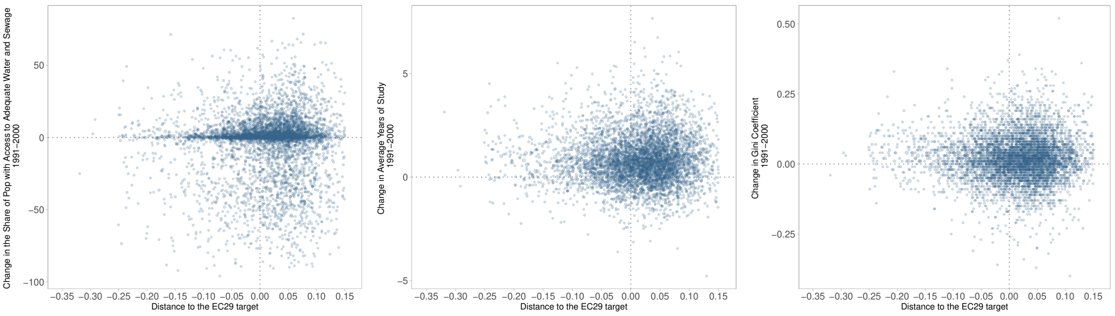
Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here refer to deaths per 1,000 live births. Baseline periods refer to years 1998-1999.

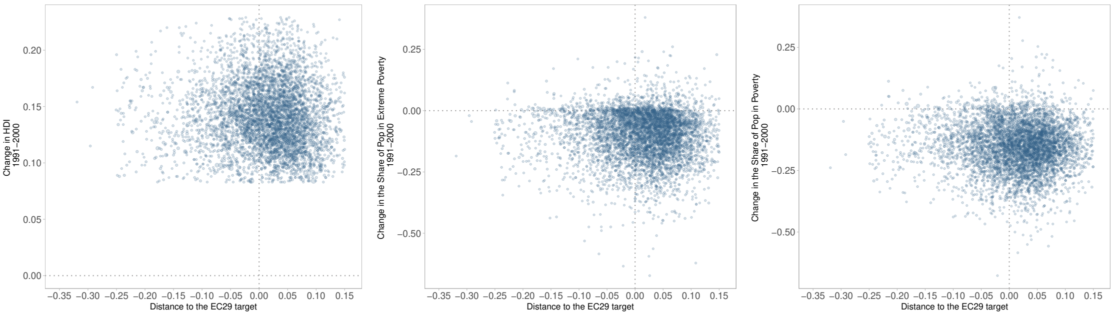
Table A.1: Descriptive Statistics (at the baseline year) – *Cont.*

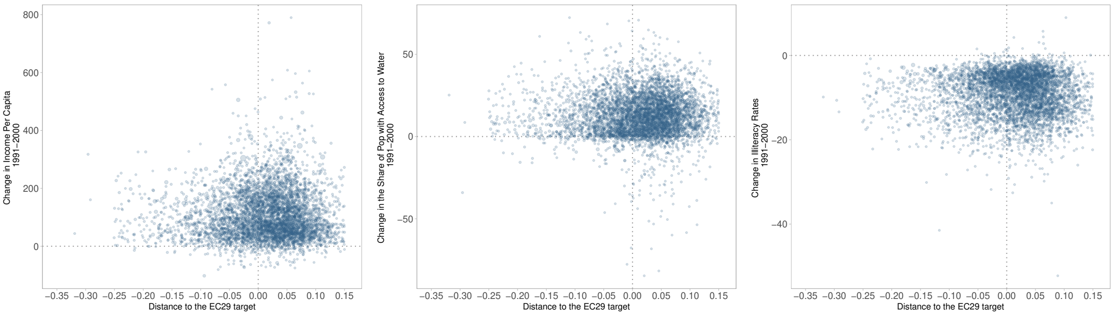
	Mean	Std. Dev.	Min	Max	Obs.	Source of Data
Controls						
Population (1,000)	29.77	178.83	0.71	9968.49	5507	IBGE/Census
GDP per capita (2010 R$)	9.53	11.23	1.37	271.78	5507	IBGE/Census
'Bolsa Familia' transfers per capita	0	0	0	0	5507	IBGE/Census
Life Expectancy	68.39	3.96	57.46	77.24	5507	IBGE/Census
Expected Years of Study	8.34	1.79	2.29	13.02	5507	IBGE/Census
Illiteracy Rate (above 18y old)	23.63	13.52	1	63.01	5507	IBGE/Census
Income per capita	338.35	192.81	62.65	1759.76	5507	IBGE/Census
Share of Population Below Poverty Line	0.411	0.23	0.007	0.91	5507	IBGE/Census
Gini Coefficient	0.55	0.07	0.30	0.87	5507	IBGE/Census
Access to Sewage Network	0.25	0.30	0	0.99	5507	IBGE/Census
Access to Garbage Collection Service	0.54	0.27	0	1	5507	IBGE/Census
Access to Water Network	0.59	0.24	0	1	5507	IBGE/Census
Access to Electricity	0.87	0.17	0.08	1	5507	IBGE/Census
Urbanization Rate	0.60	0.23	0	1	5507	IBGE/Census
Average Neighbour Health Spending p.c.	206.39	125.04	1.74	3298.40	5504	Finbra
Human Resource Spending (/Revenue)	0.415	0.109	0	1.242	5304	Finbra

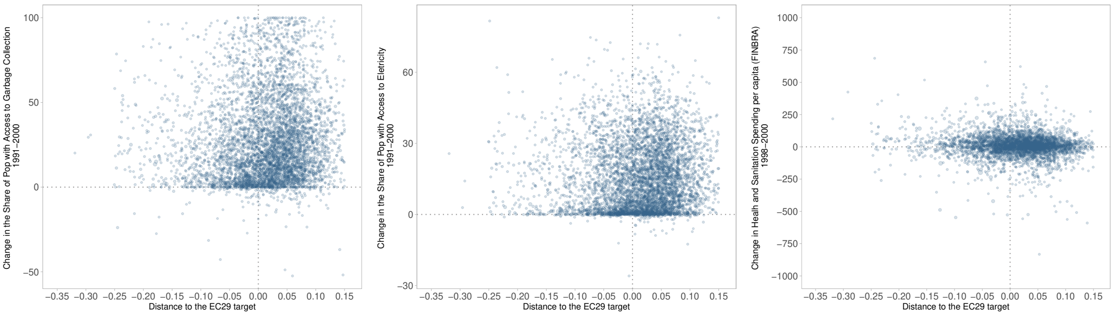
Notes: Summary statistics are presented at baseline with observations referring to the number of municipalities with observed data. All measures presented here are controls included in certain models. Baseline for variables drawn from the census are based on the 2000 census, while statistics from FINBRA are measured at 1999. Spending is measured in 2010 R$ unless otherwise indicated. Human resource spending refers to the proportion of total municipal revenue dedicate to human resources.

Figure A.4: Distance to Health Spending at Baseline and Municipal Characteristics


(a) Access to Water & Sewage (b) Years of Education (c) Gini coefficient

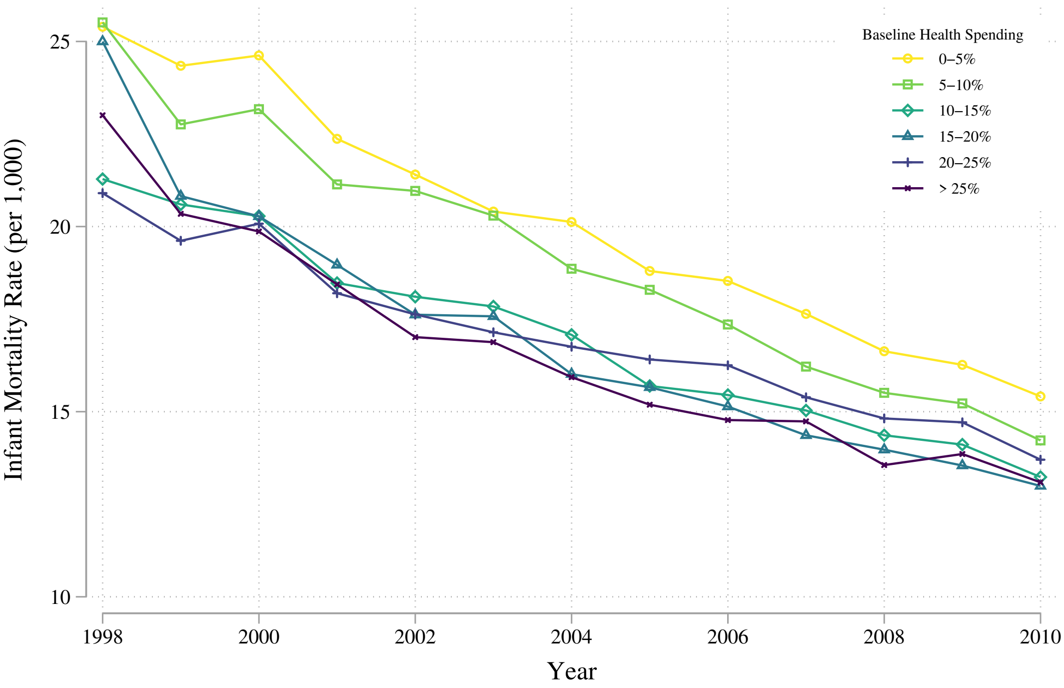

(d) Human Development Index (e) Extreme Poverty (f) Poverty


(g) Income Per Capita (h) Access to Water (i) Illiteracy


(j) Garbage Collection (k) Electricity (l) Health Spending per Capita

Notes: Each plot presents correlates of each municipality's distance to the EC/29 spending target at baseline, and municipal level characteristics measured from the 2000 and 1991 census. 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. All municipal level values of characteristics are calculated from full census microdata.

Figure A.5: Infant Mortality Rates by Baseline Health Spending



Notes: Trends present mean infant mortality rates by municipalities classified by baseline spending (health spending as a share of total municipal budget). All rates are presented as deaths per 1,000 live births, and weight by municipal population. Groupings are open at the right side, ie [0%,5%), [5%,10%), and so forth.

Table A.2: Definitions of Indexes

Index	Sub-Index	Variables
1. **Access & Production of Health Services 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 Nurses (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)

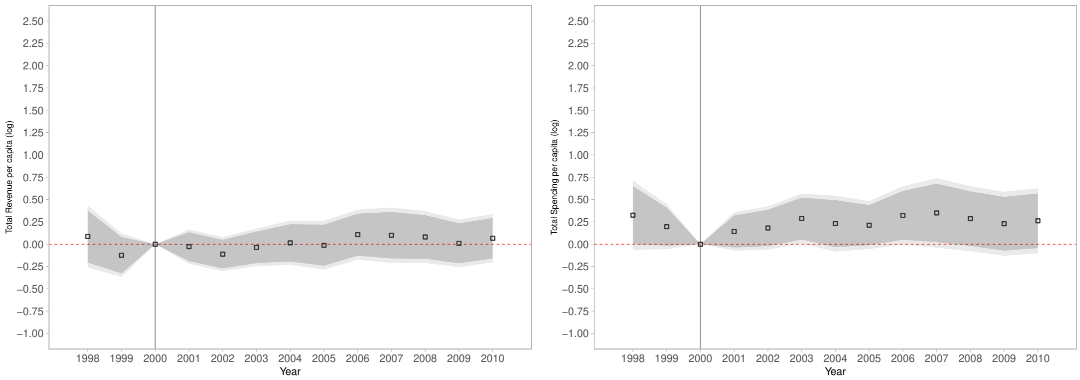
Notes: Main indexes and sub-indexes consist of the variables listed here, in each case following Anderson (2008) in the construction of indices. The abbreviation pc refers to per-capita. Each variable is included in one and only one index, and one and only one sub-index. ‡ Variable has been multiplied by minus 1 such that higher values refer to 'better' outcomes.

B Additional Results – Single-Coefficient and Dynamic Estimates

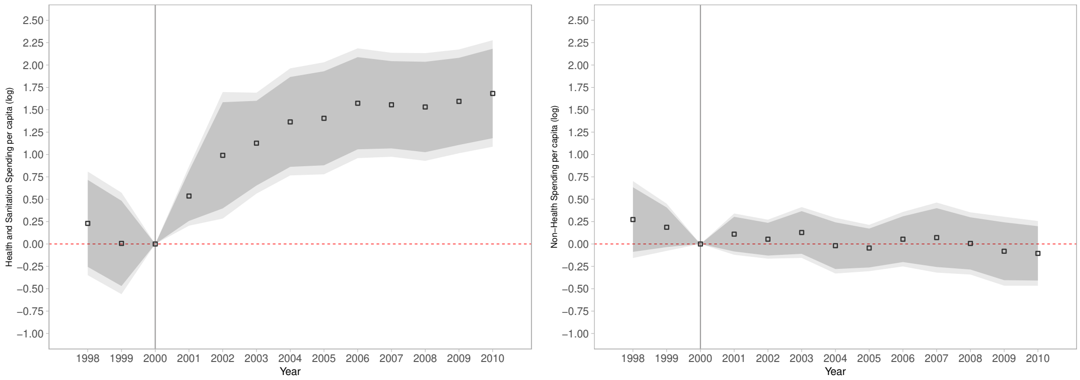
B.1 Fiscal Reactions

Figure B.1: Dynamic Effects on Revenues and Spending by Aggregate Classes (FINBRA)

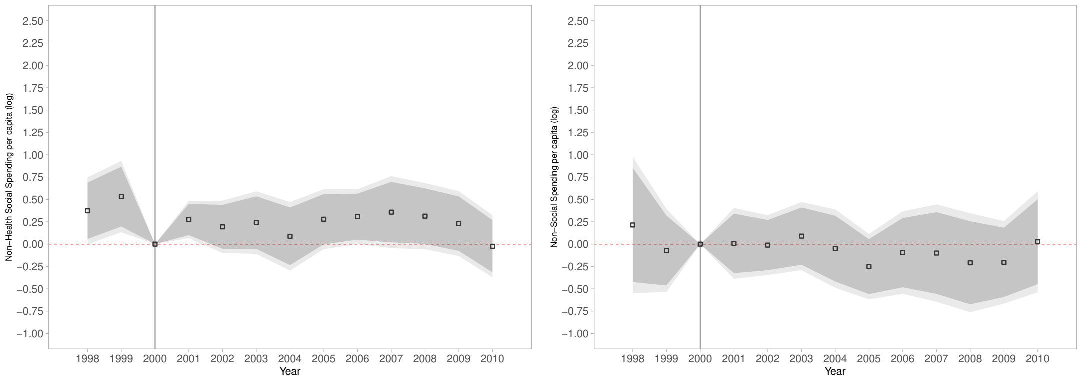
(a) Total Revenue (b) Total Public Spending



(c) Health and Sanitation (d) Non-Health

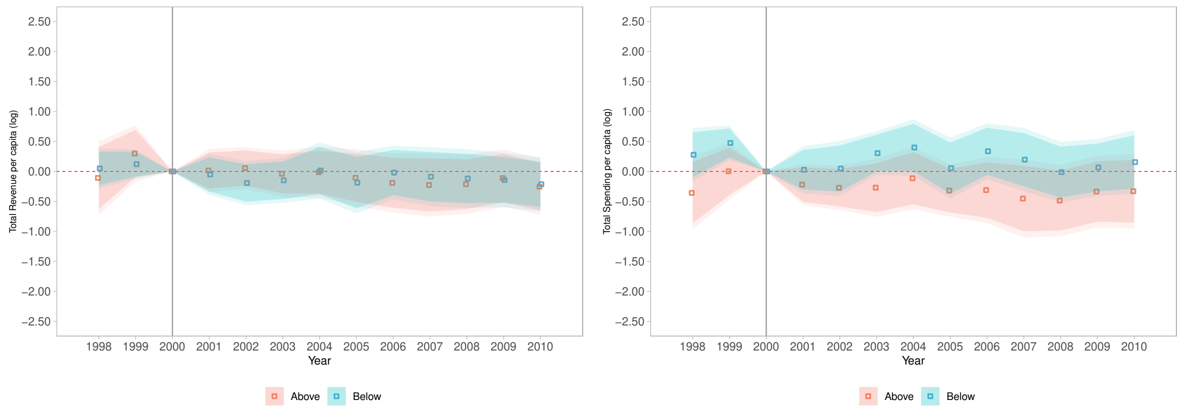


(e) Non-Health Social (f) Non-Social



Notes: Refer to Notes to Figure 4. Identical models are estimated, however here considering total revenue and spending per capita. Estimation is based on 64,481 municipality by year cells and FINBRA spending and revenue data.

Figure B.2: Dynamic Effects on Revenues and Spending (Distributional Effects)

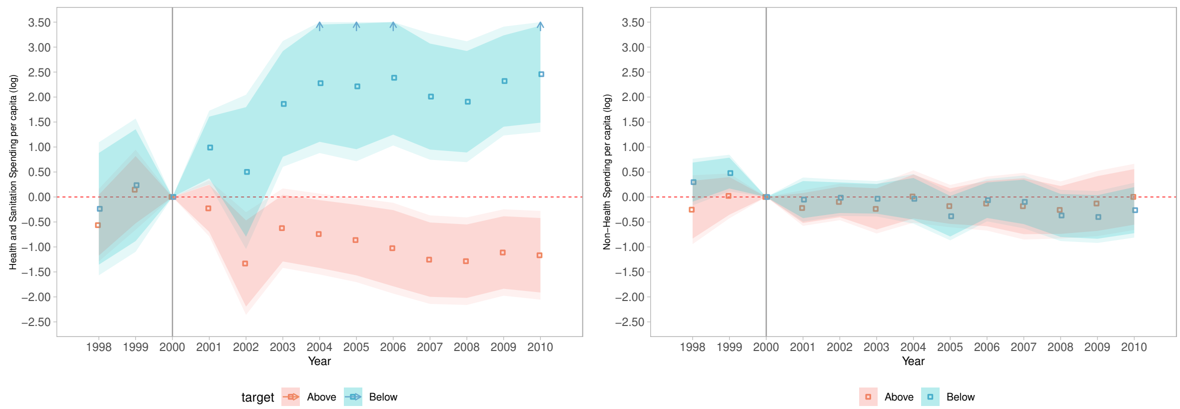


(a) Total Revenue

(b) Total Public Spending

(c) Health and Sanitation

(d) Non-Health



(e) Non-Health Social

(f) Non-Social

Notes: Refer to Notes to Figure 5. Identical models are estimated, however here considering total revenue and spending per capita. Estimation is based on 64,481 municipality by year cells and FINBRA spending and revenue data.

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

	With Data Quality Controls				Without Data Quality Controls
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Panel A: FINBRA	**Ln (per capita)**				
Total Revenues	-0.124	-0.01	0.029	0.057	0.03
	(0.139)	(0.117)	(0.112)	(0.112)	(0.112)
Total Spending	-0.048	0.056	0.093	0.082	0.093
	(0.137)	(0.115)	(0.112)	(0.111)	(0.112)
Health Spending	1.075***	1.199***	1.245***	1.232***	1.244***
	(0.25)	(0.225)	(0.224)	(0.224)	(0.224)
Non-Health Spending	-0.242*	-0.152	-0.115	-0.128	-0.115
	(0.131)	(0.111)	(0.107)	(0.106)	(0.107)
Non-Health Social Spending	-0.118	-0.069	-0.041	-0.053	-0.041
	(0.164)	(0.136)	(0.134)	(0.133)	(0.134)
Non-Social Spending	-0.296*	-0.163	-0.119	-0.131	-0.118
	(0.174)	(0.148)	(0.141)	(0.141)	(0.141)
Panel B: SIOPS					
Total Health Spending	2.258***	2.352***	2.375***	2.378***	2.375***
	(0.258)	(0.209)	(0.198)	(0.198)	(0.198)
From Own Resources	5.519***	5.565***	5.593***	5.595***	5.595***
	(0.278)	(0.267)	(0.255)	(0.255)	(0.255)
From Other Resources	1.533	1.481	1.479	1.48	1.479
	(1.503)	(1.252)	(1.24)	(1.235)	(1.241)
Personnel	2.512***	2.539***	2.559***	2.538***	2.561***
	(0.418)	(0.357)	(0.355)	(0.353)	(0.355)
Investment	5.576***	5.231***	5.235***	5.256***	5.237***
	(1.037)	(0.736)	(0.729)	(0.73)	(0.73)
Outsourced (3rd party services)	0.792	1.127*	1.155*	1.181*	1.157*
	(0.73)	(0.648)	(0.628)	(0.622)	(0.628)
Admin, Management and Others	4.475***	4.35***	4.368***	4.379***	4.368***
	(1.059)	(0.953)	(0.948)	(0.939)	(0.948)
Mun & Time-State FE	Y	Y	Y	Y	Y
Data Quality Controls	Y	Y	Y	Y	N
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. The number of observations is 63,280 for FINBRA variables and 55,232 for SIOPS variables. All other details follow those described in Table 1. * p<0.10; ** p < 0.05; *** p <0.01.

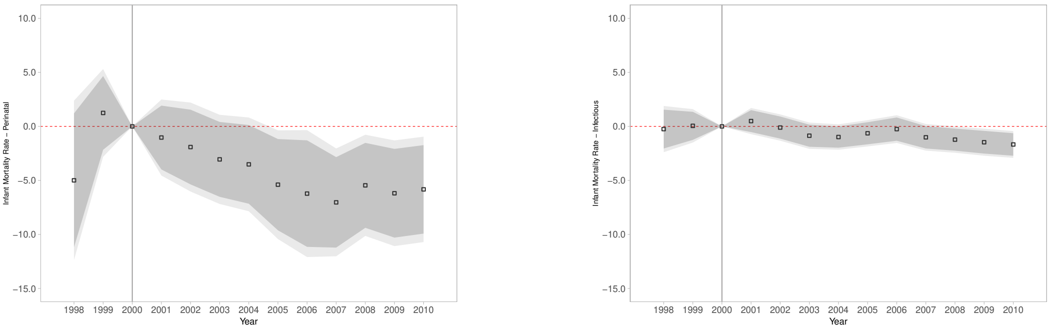
B.2 Infant Mortality

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	-0.101	-0.072	-0.071	-0.072	-0.206	-0.143	-0.126	-0.135
Amenable to Primary Care	-0.053	-0.164	-0.164	-0.165	-0.108	-0.323	-0.289	-0.307
Non-Amenable to Primary Care	-0.106	-0.063	-0.062	-0.063	-0.216	-0.125	-0.110	-0.117
By timing								
Fetal	-1.174	-1.067	-1.075	-1.089	-2.386	-2.099	-1.901	-2.028
Within 24h	-0.185	-0.169	-0.164	-0.164	-0.377	-0.332	-0.289	-0.305
1 to 27 days	-0.143	-0.096	-0.094	-0.095	-0.290	-0.189	-0.167	-0.176
27 days to 1 year	-0.040	-0.038	-0.038	-0.040	-0.081	-0.075	-0.067	-0.074
By Cause of Death								
Infectious	-0.071	-0.171	-0.167	-0.168	-0.145	-0.336	-0.295	-0.313
Respiratory	-0.168	-0.166	-0.162	-0.163	-0.342	-0.326	-0.286	-0.304
Perinatal	-0.226	-0.150	-0.148	-0.148	-0.459	-0.295	-0.261	-0.276
Congenital	-0.054	-0.042	-0.040	-0.040	-0.109	-0.083	-0.070	-0.075
External	-0.037	-0.127	-0.106	-0.103	-0.074	-0.250	-0.187	-0.191
Nutritional	-0.107	-0.209	-0.202	-0.210	-0.218	-0.411	-0.357	-0.391
Other	-0.077	-0.040	-0.044	-0.046	-0.157	-0.078	-0.077	-0.086
Ill-Defined	0.179	0.192	0.186	0.185	0.365	0.378	0.328	0.344

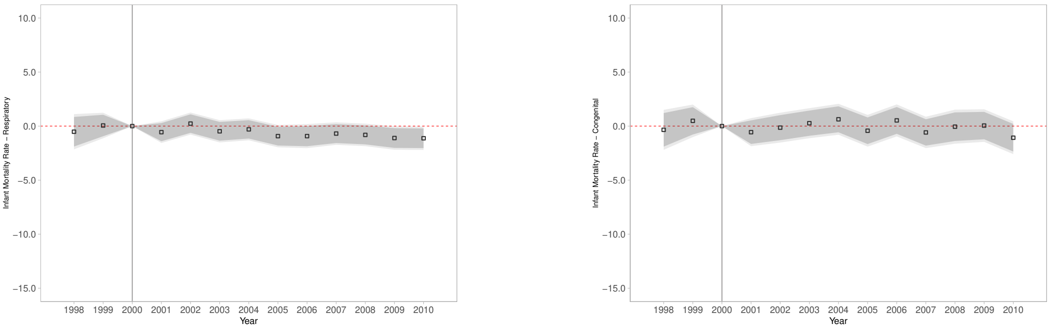
Notes: Elasticity of Infant Mortality Rates is estimated following (4), based on aggregate single coefficient estimates of EC/29 impacts on infant mortality and health spending following (3). Alternative columns correspond to control sets indicated in Table 1, and measures of health spending calculated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

Figure B.3: Infant Mortality and Public Health Spending (By cause)



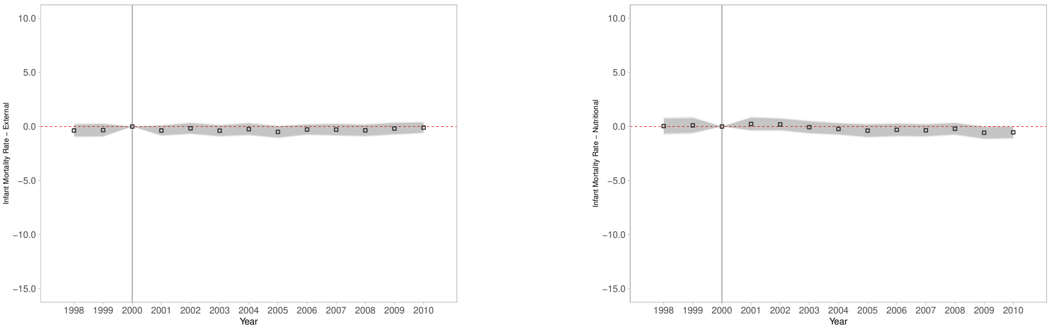
(a) Perinatal

(b) Infectious causes



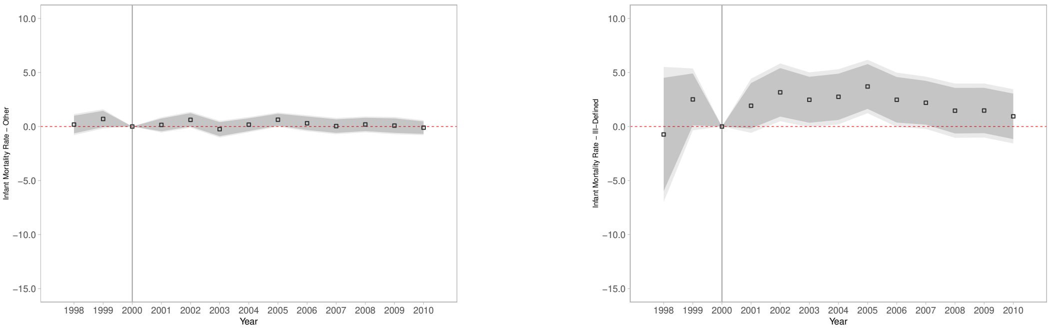
(c) Respiratory Causes

(d) Congenital anomalies



(e) External causes

(f) Nutritional

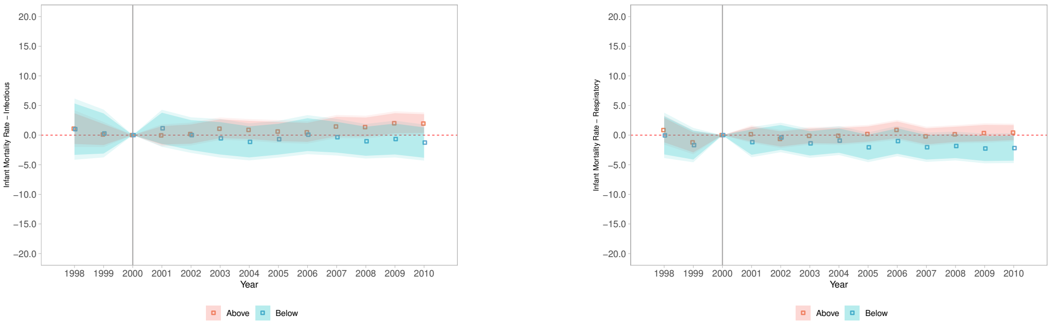


(g) Other

(h) Ill-defined

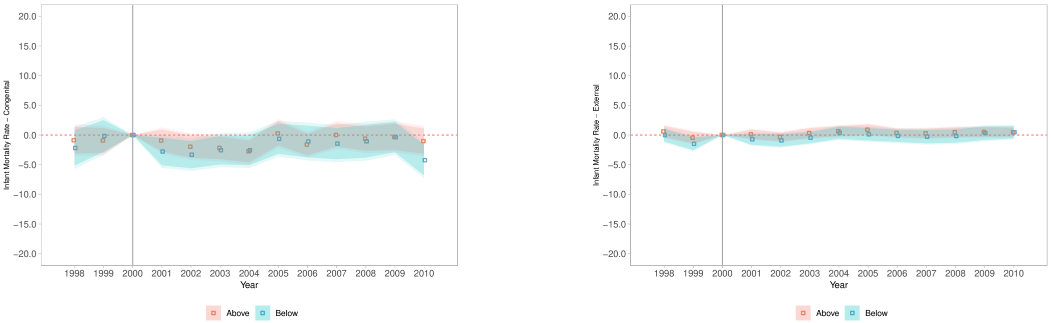
Notes: Refer to Notes to Figure 6. Identical models are estimated, however here examining rates of mortality by specific (mutually exclusive) mortality classes. Point estimates are presented as black squares, and 90% and 95% confidence intervals are presented as dark and light grey shaded areas respectively. Estimation is based on 64,481 municipality by year cells.

Figure B.4: Infant Mortality and Public Health Spending (By cause): Above and Below Threshold Effects



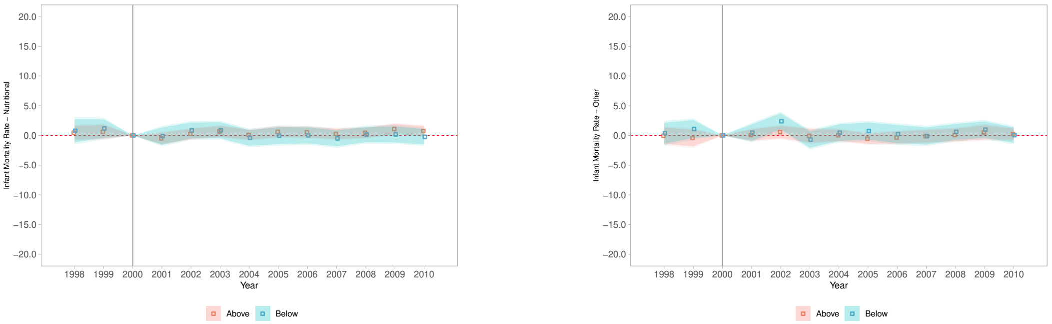
(a) Infectious causes

(b) Respiratory Causes



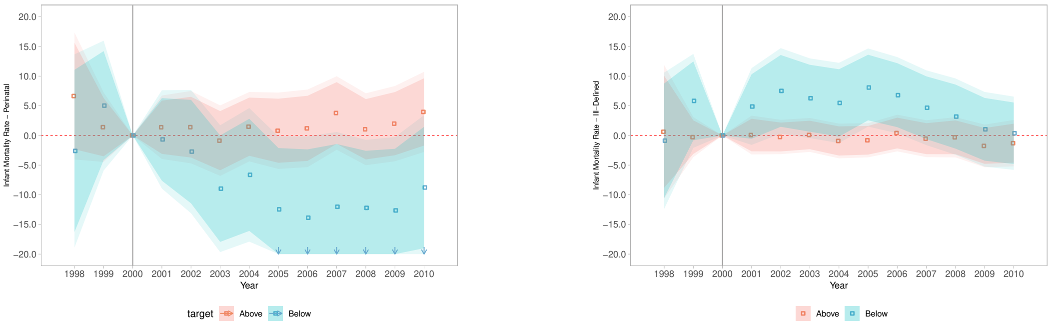
(c) Congenital anomalies

(d) External causes



(e) Nutritional

(f) Other

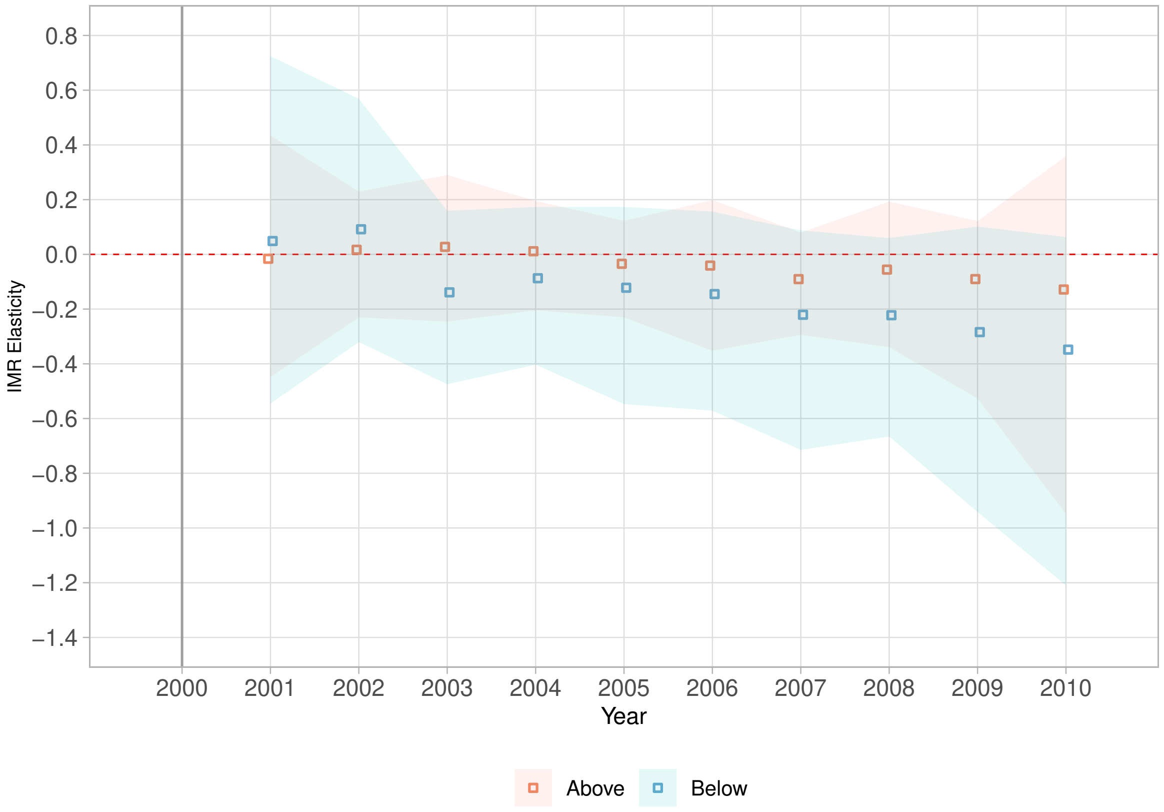


(g) Perinatal

(h) Ill-defined

Notes: Refer to Notes to Figure 6. Identical models are estimated, however here examining rates of mortality by specific (mutually exclusive) mortality classes. Estimation is based on 64,481 municipality by year cells.

Figure B.5: 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 (4), 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 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 (see Figure 3a), these values vary between around 15 and -35. 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 $d \in \mathcal{D}$:

$$E[Y_t(0) - Y_{t-1}(0)|D = d] = E[Y_t(0) - Y_{t-1}(0)|D = 0]. \tag{C1}$$

In words, this is that observed trends in outcomes for un-treated 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 (C1), the two-way fixed effect estimate captures the following:

$$\beta^{twfe} = \int_{dL}^{dU} w_1(l) \left[ACRT(l|l) + \left. \frac{\partial ATT(l|h)}{\partial h} \right|_{h=l} \right] dl + w_0 \frac{ATT(d_L|d_L)}{dL} \tag{C2}$$

where:

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

and:

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

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 to 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 (C2). This first term suggests that under standard parallel trends assumptions as in (C1), we will thus not necessarily capture a weighted average of average causal response functions, given the existence of the second term: $\partial ATT(l|h)/\delta h|_{h=l}$. 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 $d \in \mathcal{D}$:

$$E[Y_t(d) - Y_{t-1}(0)] = E[Y_t(d) - Y_{t-1}(0)|D = d] \tag{C3}$$

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 (C3) 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

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[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.

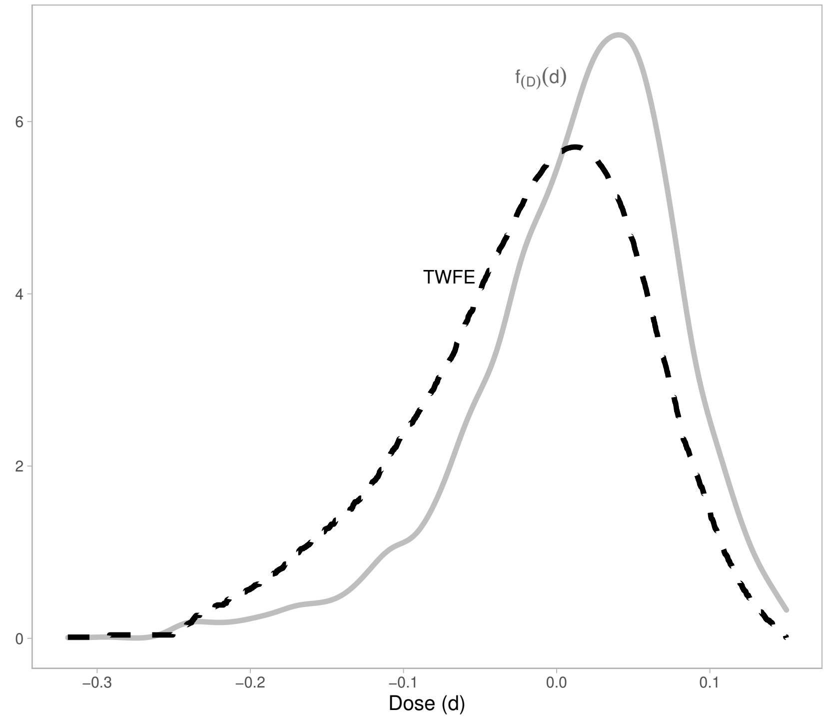


Figure C1: Weights implicit in Two-way FE models and the Empirical Distribution of Spending Target Distances

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. What's more, 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 different ways in the pre-reform period (Appendix Figure A.4). 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:

$$\beta^{twfe} = \int_{dL}^{dU} w_1(l) ACR(l) dl + w_0 \frac{ATT(d_L)}{dL}, \tag{C4}$$

where:

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

Thus, in this case, we can 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 7 of the paper we show an additional test where we reweight 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 C1). 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 C1.

D Robustness Checks and Additional Results

Table D.1: Full Tabular Output – Spending Event Studies

	Health (FINBRA)	Health (SIPOS)	Own Resources	Other Resources	Human Resources	Investment	Outsourced 3rd Party	Admin Management
Year = 1998	0.227	–	–	–	–	–	–	–
	(0.296)	–	–	–	–	–	–	–
Year = 1999	0.003	–	–	–	–	–	–	–
	(0.29)	–	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.535***	1.243***	3.938***	0.618	1.264***	2.452***	0.247	2.726***
	(0.169)	(0.144)	(0.272)	(0.9)	(0.338)	(0.708)	(0.589)	(0.614)
Year = 2002	0.991***	1.984***	4.931***	1.165	2.241***	4.002***	0.411	4.74***
	(0.361)	(0.187)	(0.245)	(1.082)	(0.422)	(0.739)	(0.738)	(1.34)
Year = 2003	1.127***	2.05***	4.959***	1.78	1.778***	4.505***	0.948	4.423***
	(0.288)	(0.161)	(0.377)	(1.318)	(0.285)	(0.897)	(0.795)	(1.254)
Year = 2004	1.368***	2.572***	5.82***	2.013	2.629***	5.256***	1.666**	4.684***
	(0.306)	(0.222)	(0.304)	(1.534)	(0.435)	(0.988)	(0.7)	(1.118)
Year = 2005	1.403***	2.633***	6.036***	1.629	2.814***	6.709***	1.314*	4.681***
	(0.32)	(0.266)	(0.25)	(1.494)	(0.467)	(1.207)	(0.732)	(1.148)
Year = 2006	1.576***	2.652***	5.955***	1.804	2.743***	6.555***	1.349*	4.545***
	(0.313)	(0.295)	(0.308)	(1.414)	(0.45)	(1.106)	(0.701)	(1.136)
Year = 2007	1.557***	2.742***	6.318***	1.475	3.467***	5.998***	1.822***	3.601***
	(0.297)	(0.187)	(0.286)	(1.27)	(0.518)	(1.001)	(0.631)	(0.583)
Year = 2008	1.533***	2.731***	6.175***	1.408	3.229***	6.206***	1.762***	4.038***
	(0.307)	(0.258)	(0.26)	(1.152)	(0.481)	(0.988)	(0.623)	(0.734)
Year = 2009	1.596***	2.717***	6.072***	1.644	2.928***	5.624***	1.647***	4.354***
	(0.296)	(0.271)	(0.303)	(1.217)	(0.419)	(1.075)	(0.602)	(0.757)
Year = 2010	1.683***	2.711***	6.188***	1.371	2.901***	5.687***	0.595	6.217***
	(0.304)	(0.266)	(0.251)	(1.195)	(0.387)	(0.783)	(0.881)	(1.293)
Observations	62,889	55,389	55,469	55,320	55,379	53,931	55,231	55,361

Notes: Output corresponds to graphical event studies displayed in Figure 4. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table D.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	-0.56	-0.239	–	–	–	–	–	–
	(0.371)	(0.679)	–	–	–	–	–	–
Year = 1999	0.148	0.233	–	–	–	–	–	–
	(0.41)	(0.681)	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	-0.223	0.993***	-0.799***	1.905***	-0.656**	8.746***	-1.461	-0.606
	(0.286)	(0.376)	(0.247)	(0.193)	(0.329)	(0.466)	(1.371)	(0.41)
Year = 2002	-1.331**	0.504	-1.436***	2.792***	-1.597***	9.811***	-1.734	0.327
	(0.524)	(0.789)	(0.309)	(0.283)	(0.289)	(0.585)	(1.658)	(0.544)
Year = 2003	-0.622	1.863***	-1.237***	3.233***	-1.144**	10.53***	-2.491	0.74
	(0.406)	(0.644)	(0.259)	(0.301)	(0.488)	(0.45)	(2.035)	(0.609)
Year = 2004	-0.747*	2.277***	-1.803***	3.698***	-1.738***	11.803***	-3.261	0.19
	(0.413)	(0.714)	(0.341)	(0.351)	(0.344)	(0.419)	(2.346)	(0.731)
Year = 2005	-0.864**	2.206***	-1.959***	3.624***	-1.941***	12.062***	-2.768	-0.047
	(0.43)	(0.764)	(0.396)	(0.388)	(0.216)	(0.375)	(2.273)	(0.856)
Year = 2006	-1.028**	2.388***	-1.796***	3.909***	-1.778***	12.092***	-2.521	0.752
	(0.464)	(0.69)	(0.473)	(0.422)	(0.359)	(0.389)	(2.185)	(0.86)
Year = 2007	-1.257***	2.009***	-2.024***	3.799***	-2.586***	11.81***	-2.091	0.57
	(0.452)	(0.645)	(0.27)	(0.398)	(0.411)	(0.394)	(1.992)	(0.804)
Year = 2008	-1.287***	1.907***	-1.945***	3.888***	-2.223***	11.992***	-1.981	0.566
	(0.447)	(0.617)	(0.418)	(0.412)	(0.281)	(0.4)	(1.822)	(0.766)
Year = 2009	-1.116**	2.318***	-2.007***	3.765***	-2.14***	11.869***	-2.619	0.215
	(0.441)	(0.555)	(0.441)	(0.411)	(0.394)	(0.395)	(1.892)	(0.81)
Year = 2010	-1.169***	2.455***	-1.993***	3.773***	-2.444***	11.718***	-2.069	0.346
	(0.453)	(0.589)	(0.445)	(0.42)	(0.243)	(0.473)	(1.92)	(0.827)
Observations	62,889	62,889	55,389	55,389	55,469	55,469	55,320	55,320

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 continued overleaf.

Table D.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	-1.111**	1.519***	-0.825	4.774***	0.906	1.971**	-2.411**	3.173***
	(0.497)	(0.565)	(1.143)	(1.372)	(1.007)	(0.863)	(0.959)	(0.756)
Year = 2002	-1.82***	2.871***	-3.611***	4.577***	1.714	3.489***	-4.384**	5.25***
	(0.611)	(0.795)	(1.111)	(1.564)	(1.239)	(0.786)	(2.106)	(1.023)
Year = 2003	-1.056***	2.838***	-3.604***	5.806***	1.404	4.353***	-4.659**	4.093***
	(0.336)	(0.842)	(1.317)	(1.706)	(1.362)	(0.839)	(1.967)	(0.905)
Year = 2004	-2.135***	3.361***	-3.662**	7.584***	0.78	5.232***	-4.831***	4.474***
	(0.605)	(0.91)	(1.495)	(1.614)	(1.316)	(0.908)	(1.785)	(0.881)
Year = 2005	-2.299***	3.576***	-6.448***	7.082***	0.835	4.461***	-5.199***	3.913***
	(0.667)	(0.929)	(1.819)	(1.639)	(1.355)	(0.912)	(1.811)	(0.789)
Year = 2006	-1.965***	3.892***	-5.268***	8.426***	0.777	4.454***	-4.808***	4.168***
	(0.702)	(0.817)	(1.659)	(1.795)	(1.278)	(0.989)	(1.82)	(0.816)
Year = 2007	-3.265***	3.783***	-5.543***	6.657***	0.156	4.713***	-3.119***	4.308***
	(0.869)	(0.738)	(1.502)	(1.754)	(1.179)	(1.036)	(0.894)	(0.903)
Year = 2008	-2.159***	4.81***	-6.03***	6.454***	-0.254	3.972***	-3.518***	4.801***
	(0.745)	(0.786)	(1.497)	(1.749)	(1.167)	(1.06)	(1.176)	(0.973)
Year = 2009	-2.119***	4.134***	-5.813***	5.328***	0	4.06***	-3.659***	5.379***
	(0.69)	(0.712)	(1.595)	(1.638)	(1.161)	(0.99)	(1.165)	(1.113)
Year = 2010	-1.724***	4.649***	-6.141***	5***	1.219	3.255***	-6.147***	6.328***
	(0.613)	(0.702)	(1.145)	(1.764)	(1.577)	(1.184)	(1.993)	(1.136)
Observations	55,379	55,379	53,931	53,931	55,231	55,231	55,361	55,361

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 continued from previous page.

Table D.3: Full Tabular Output – Infant Mortality Event Studies

	Infant Mortality	IMR 24 hours	IMR 1-27 days	IMR > 27 days
Year = 1998	-7.119	-5.114**	-6.039	-1.08
	(7.176)	(2.074)	(4.285)	(3.614)
Year = 1999	4.834	1.191	3.017	1.818
	(3.458)	(1.291)	(2.322)	(1.898)
Year = 2000	0	0	0	0
	(–)	(–)	(–)	(–)
Year = 2001	0.265	-1.071	-0.537	0.803
	(2.661)	(1.23)	(1.884)	(1.608)
Year = 2002	1.829	-3.094**	-1.45	3.279**
	(3.114)	(1.27)	(2.174)	(1.664)
Year = 2003	-2.361	-2.703**	-1.941	-0.42
	(2.804)	(1.242)	(2.07)	(1.485)
Year = 2004	-1.758	-2.665**	-1.846	0.088
	(2.892)	(1.337)	(2.119)	(1.543)
Year = 2005	-3.977	-3.272**	-4.429*	0.452
	(3.332)	(1.334)	(2.552)	(1.551)
Year = 2006	-4.732	-4.33***	-5.042*	0.31
	(3.919)	(1.431)	(2.969)	(1.71)
Year = 2007	-7.762**	-4.273***	-5.961**	-1.801
	(3.41)	(1.336)	(2.498)	(1.674)
Year = 2008	-6.517*	-3.68***	-4.366*	-2.15
	(3.331)	(1.396)	(2.396)	(1.742)
Year = 2009	-7.973**	-2.778**	-5.536**	-2.437
	(3.421)	(1.359)	(2.431)	(1.748)
Year = 2010	-9.567***	-2.59*	-5.378**	-4.189**
	(3.606)	(1.346)	(2.51)	(1.763)
Observations	64,086	64,085	64,085	64,086

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 D.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	9.521	-3.61	6.404*	-3.239	11.8*	2.464	-2.28	-6.074
	(11.352)	(14.146)	(3.586)	(3.929)	(6.091)	(9.185)	(6.314)	(7.283)
Year = 1999	-1.195	10.1	-0.2	2.59	-0.708	6.354	-0.493	3.741
	(4.895)	(9.588)	(1.972)	(3.237)	(3.286)	(6.528)	(2.873)	(4.767)
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.392	1.1	1.201	-0.966	1.29	0.477	-0.897	0.623
	(4.117)	(6.881)	(1.918)	(2.953)	(3.07)	(4.57)	(2.43)	(4.025)
Year = 2002	-0.749	3.397	2.365	-4.208	1.882	-0.841	-2.631	4.238
	(4.463)	(8.124)	(1.953)	(3.155)	(3.259)	(5.55)	(2.377)	(4.121)
Year = 2003	-0.997	-7.421	-0.125	-6.934**	-0.729	-5.959	-0.268	-1.462
	(4.192)	(7.622)	(1.859)	(3.187)	(3.17)	(5.416)	(2.127)	(3.735)
Year = 2004	-0.582	-5.342	0.841	-5.438	1.365	-2.622	-1.947	-2.72
	(4.129)	(7.593)	(1.95)	(3.394)	(3.156)	(5.413)	(2.144)	(4.053)
Year = 2005	2.048	-6.961	1.797	-5.528	2.636	-7.215	-0.588	0.255
	(4.358)	(8.233)	(1.914)	(3.394)	(3.279)	(6.222)	(2.298)	(3.696)
Year = 2006	1.931	-9.034	2.802	-6.667*	3.196	-7.894	-1.265	-1.14
	(4.411)	(9.29)	(1.936)	(3.52)	(3.314)	(6.897)	(2.493)	(4.007)
Year = 2007	5.013	-12	2.619	-6.808**	4.259	-8.609	0.755	-3.39
	(4.479)	(8.378)	(1.927)	(3.406)	(3.309)	(6.196)	(2.544)	(3.878)
Year = 2008	2.655	-12.443	0.106	-9.104***	2.012	-8.01	0.643	-4.433
	(4.569)	(7.785)	(2.041)	(3.371)	(3.267)	(5.643)	(2.76)	(3.94)
Year = 2009	4.499	-13.341	0.241	-6.66*	2.23	-10.63*	2.269	-2.712
	(4.901)	(8.413)	(1.846)	(3.446)	(3.409)	(5.928)	(2.745)	(4.194)
Year = 2010	5.591	-15.711*	1.491	-4.32	3.689	-8.059	1.903	-7.652*
	(5.197)	(8.468)	(1.825)	(3.512)	(3.607)	(6.058)	(2.732)	(4.104)
Observations	64,086	64,086	64,085	64,085	64,085	64,085	64,086	64,086

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.

Table D.5: Full Tabular Output – Access and Input Event Studies

	Access & Production	Primary Access	Non-Primary Access	Health Inputs	Human Resources	Hospitals
Year = 1998	0.300	0.282	0.222	–	–	–
	(0.287)	(0.319)	(0.15)	–	–	–
Year = 1999	0.065	0.031	0.092	–	–	–
	(0.181)	(0.197)	(0.114)	–	–	–
Year = 2000	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.473***	0.54**	0.034	–	–	–
	(0.172)	(0.21)	(0.076)	–	–	–
Year = 2002	0.462*	0.54*	0.034	0.562***	2.124***	0.293***
	(0.246)	(0.276)	(0.103)	(0.152)	(0.595)	(0.073)
Year = 2003	0.563*	0.697*	-0.029	–	–	–
	(0.311)	(0.356)	(0.126)	–	–	–
Year = 2004	0.491	0.522	0.072	–	–	–
	(0.351)	(0.406)	(0.14)	–	–	–
Year = 2005	0.701**	0.731**	0.136	0.43***	1.443**	0.293***
	(0.286)	(0.325)	(0.154)	(0.153)	(0.571)	(0.068)
Year = 2006	0.748***	0.82***	0.077	–	–	–
	(0.229)	(0.256)	(0.155)	–	–	–
Year = 2007	0.505***	0.541***	0.074	–	–	–
	(0.191)	(0.196)	(0.174)	–	–	–
Year = 2008	0.516***	0.606***	-0.048	–	–	–
	(0.2)	(0.227)	(0.19)	–	–	–
Year = 2009	0.547**	0.675***	-0.145	0.313**	1.199**	0.243***
	(0.213)	(0.24)	(0.208)	(0.131)	(0.526)	(0.065)
Year = 2010	0.703***	0.776***	-0.017	–	–	–
	(0.227)	(0.249)	(0.207)	–	–	–
Observations	64,086	64,086	64,086	19,261	19,261	19,261

Notes: Output corresponds to graphical event studies displayed in Figure 8. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table D.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	-0.101	0.6	-0.261	0.319	0.202	0.854**	–	–	–	–	–	–
	(0.344)	(0.699)	(0.354)	(0.806)	(0.214)	(0.392)	–	–	–	–	–	–
Year = 1999	0.11	0.329	0.055	0.166	0.188	0.512**	–	–	–	–	–	–
	(0.25)	(0.396)	(0.258)	(0.467)	(0.202)	(0.211)	–	–	–	–	–	–
Year = 2000	0	0	0	0	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	0.047	1.24**	0.174	1.591**	-0.059	0.008	–	–	–	–	–	–
	(0.22)	(0.497)	(0.266)	(0.622)	(0.114)	(0.16)	–	–	–	–	–	–
Year = 2002	-0.337	0.65	-0.368	0.8	0.038	0.144	-0.42**	0.769**	-1.451*	3.106**	-0.272***	0.322*
	(0.377)	(0.434)	(0.431)	(0.525)	(0.14)	(0.227)	(0.214)	(0.352)	(0.881)	(1.382)	(0.103)	(0.177)
Year = 2003	-0.239	1.047*	-0.183	1.464**	0.012	-0.046	–	–	–	–	–	–
	(0.428)	(0.611)	(0.496)	(0.741)	(0.17)	(0.27)	–	–	–	–	–	–
Year = 2004	-0.411	0.613	-0.328	0.815	0.003	0.187	–	–	–	–	–	–
	(0.438)	(0.753)	(0.519)	(0.887)	(0.175)	(0.343)	–	–	–	–	–	–
Year = 2005	-0.55	0.925*	-0.515	1.054*	0.005	0.35	-0.17	0.821**	-0.182	3.333***	-0.26***	0.343**
	(0.448)	(0.543)	(0.533)	(0.632)	(0.179)	(0.392)	(0.227)	(0.345)	(0.896)	(1.187)	(0.098)	(0.167)
Year = 2006	-0.376	1.308**	-0.339	1.546***	-0.009	0.184	–	–	–	–	–	–
	(0.345)	(0.509)	(0.395)	(0.586)	(0.191)	(0.38)	–	–	–	–	–	–
Year = 2007	-0.201	0.964***	-0.197	1.061***	0.052	0.268	–	–	–	–	–	–
	(0.304)	(0.357)	(0.324)	(0.388)	(0.226)	(0.414)	–	–	–	–	–	–
Year = 2008	-0.093	1.155***	-0.194	1.228**	0.523**	0.672	–	–	–	–	–	–
	(0.323)	(0.402)	(0.373)	(0.494)	(0.252)	(0.46)	–	–	–	–	–	–
Year = 2009	-0.239	1.013**	-0.348	1.171**	0.664**	0.642	-0.146	0.561**	0.051	3.075***	-0.253***	0.226
	(0.338)	(0.408)	(0.379)	(0.502)	(0.286)	(0.493)	(0.228)	(0.269)	(0.879)	(1.151)	(0.096)	(0.159)
Year = 2010	-0.358	1.225***	-0.403	1.342***	0.583**	0.842	–	–	–	–	–	–
	(0.351)	(0.417)	(0.384)	(0.486)	(0.287)	(0.529)	–	–	–	–	–	–
Observations	64,086	64,086	64,086	64,086	64,086	64,086	19,261	19,261	19,261	19,261	19,261	19,261

Notes: Output corresponds to graphical event studies displayed in Figure 8. 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 D.7: Full Tabular Output – Health Systems Event Studies

	Municipal Hospitals	Other Gov Hospitals	Private Hospitals	Private Insurance
Year = 1998	–	–	–	–
	–	–	–	–
Year = 1999	–	–	–	–
	–	–	–	–
Year = 2000	0	0	0	0
	(–)	(–)	(–)	(–)
Year = 2001	–	–	–	0.023*
	–	–	–	(0.012)
Year = 2002	0.061***	0.003	0.019**	0.004
	(0.009)	(0.006)	(0.008)	(0.018)
Year = 2003	–	–	–	0.002
	–	–	–	(0.024)
Year = 2004	–	–	–	0.002
	–	–	–	(0.026)
Year = 2005	0.06***	0.003	0.011	0.013
	(0.009)	(0.006)	(0.009)	(0.028)
Year = 2006	–	–	–	0.017
	–	–	–	(0.03)
Year = 2007	–	–	–	-0.018
	–	–	–	(0.031)
Year = 2008	–	–	–	-0.028
	–	–	–	(0.032)
Year = 2009	0.054***	0.001	-0.008	-0.044
	(0.009)	(0.006)	(0.008)	(0.037)
Year = 2010	–	–	–	-0.055
	–	–	–	(0.045)
Observations	19,261	19,261	19,261	55,709

Notes: Output corresponds to graphical event studies displayed in Figure 9. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table D.8: Full Tabular Output – Health Systems Distributional Event Studies

	Municipal Hospitals		Other Gov't Hospitals		Private Hospitals		Private Insurance	
	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	–	–	–	–	–	–	–	–
	–	–	–	–	–	–	–	–
Year = 1999	–	–	–	–	-0.027**	-0.019	–	–
	–	–	–	–	(0.012)	(0.017)	–	–
Year = 2000	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	–	–	–	–	–	–	-0.043**	-0.008
	–	–	–	–	–	–	(0.02)	(0.026)
Year = 2002	-0.045***	0.084***	-0.008	-0.005	-0.019*	0.04***	-0.036	-0.043
	(0.015)	(0.019)	(0.007)	(0.016)	(0.01)	(0.015)	(0.035)	(0.033)
Year = 2003	–	–	–	–	–	–	-0.034	-0.047
	–	–	–	–	–	–	(0.041)	(0.041)
Year = 2004	–	–	–	–	–	–	-0.025	-0.034
	–	–	–	–	–	–	(0.042)	(0.051)
Year = 2005	-0.049***	0.076***	-0.005	0.001	-0.011*	0.033**	-0.045	-0.036
	(0.015)	(0.018)	(0.007)	(0.015)	(0.006)	(0.013)	(0.043)	(0.054)
Year = 2006	–	–	–	–	–	–	-0.053	-0.037
	–	–	–	–	–	–	(0.046)	(0.058)
Year = 2007	–	–	–	–	–	–	-0.028	-0.088
	–	–	–	–	–	–	(0.05)	(0.064)
Year = 2008	–	–	–	–	–	–	-0.044	-0.137**
	–	–	–	–	–	–	(0.05)	(0.066)
Year = 2009	-0.046***	0.067***	-0.005	-0.007	–	–	-0.012	-0.128*
	(0.014)	(0.018)	(0.008)	(0.015)	–	–	(0.056)	(0.076)
Year = 2010	–	–	–	–	–	–	-0.002	-0.143*
	–	–	–	–	–	–	(0.068)	(0.087)
Observations	19,261	19,261	19,261	19,261	19,261	19,261	55,709	55,709

Notes: Output corresponds to graphical event studies displayed in Figure 9. Coefficients are displayed with standard errors in 8parentheses. 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 D.9: Full Tabular Output – Adult Hospitalization and Mortality Event Studies

	Hospitalizations			Mortality		
	All	APC	Non-APC	All	APC	Non-APC
Year = 1998	41.173	10.792	30.38	-6.552	-1.274	-5.278
	(30.807)	(12.894)	(24.535)	(4.809)	(1.393)	(3.483)
Year = 1999	17.528	3.272	14.256	0.393	0.408	-0.015
	(23.145)	(10.753)	(18.158)	(0.901)	(0.354)	(0.744)
Year = 2000	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	-8.503	-18.165**	9.661	-1.896***	-0.178	-1.718***
	(18.138)	(8.392)	(13.674)	(0.614)	(0.276)	(0.56)
Year = 2002	-39.397	-27.168**	-12.228	-1.983***	-0.298	-1.684***
	(25.96)	(11.759)	(19.369)	(0.701)	(0.285)	(0.602)
Year = 2003	-43.907	-32.259**	-11.649	-1.645**	-0.398	-1.247*
	(28.578)	(12.635)	(21.513)	(0.81)	(0.319)	(0.699)
Year = 2004	-29.605	-29.151**	-0.454	-0.937	0.006	-0.943
	(32.921)	(13.018)	(25.576)	(0.882)	(0.371)	(0.719)
Year = 2005	0.993	-25.042*	26.034	-0.679	-0.451	-0.228
	(35.026)	(14.974)	(25.663)	(0.929)	(0.402)	(0.774)
Year = 2006	-4.074	-23.155	19.081	-0.719	-1.122**	0.403
	(36.194)	(15.678)	(26.474)	(1.04)	(0.45)	(0.85)
Year = 2007	-4.279	-29.6*	25.32	-0.474	-0.712*	0.237
	(38.966)	(17.609)	(26.717)	(0.909)	(0.375)	(0.789)
Year = 2008	-18.647	-34.626*	15.98	-1.237	-0.719*	-0.518
	(40.542)	(19.274)	(27.528)	(0.887)	(0.384)	(0.766)
Year = 2009	-8.854	-41.307**	32.453	-1.036	-1.055***	0.019
	(40.247)	(19.741)	(27.168)	(0.982)	(0.374)	(0.848)
Year = 2010	7.999	-35.657*	43.656	-1.311	-0.827**	-0.484
	(39.977)	(19.718)	(27.614)	(1.144)	(0.411)	(0.955)
Observations	64,086	64,086	64,086	64,086	64,086	64,086

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

Table D.10: Full Tabular Output – Adult Hospitalization and Mortality Distributional Event Studies

	Adult Hospitalizations		Hospitalizations APC		Hospitalizations Non-APC		Adult Mortality		Mortality APC		Mortality Non-APC	
	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	17.742	127.874*	-2.468	22.985	20.211	104.888*	9.506*	-2.149	2.587*	0.666	6.919*	-2.815
	(45.502)	(76.439)	(20.293)	(28.884)	(36.877)	(57.829)	(4.934)	(7.818)	(1.533)	(2.341)	(3.587)	(5.678)
Year = 1999	18.848	70.021	-10.666	-7.839	29.513	77.86*	0.458	1.684	0.129	1.187	0.329	0.496
	(36.886)	(51.339)	(17.684)	(22.023)	(28.929)	(39.905)	(1.607)	(2.274)	(0.61)	(0.832)	(1.272)	(1.809)
Year = 2000	0	0	0	0	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	7.258	-11.666	18.626	-17.836	-11.368	6.17	3.381***	0.322	0.413	0.153	2.968***	0.169
	(28.191)	(38.623)	(14.312)	(16.601)	(22.032)	(26.875)	(0.941)	(1.41)	(0.471)	(0.598)	(0.829)	(1.288)
Year = 2002	39.127	-40.221	30.72	-21.951	8.408	-18.269	3.213***	-0.139	0.292	-0.313	2.92***	0.174
	(40.547)	(57.274)	(21.074)	(23.834)	(30.225)	(41.15)	(1.088)	(1.629)	(0.473)	(0.637)	(0.918)	(1.401)
Year = 2003	-16.096	-133.404**	22.065	-47.525*	-38.161	-85.879**	2.059*	-1.001	0.51	-0.248	1.549	-0.753
	(46.827)	(60.813)	(21.544)	(26.577)	(35.762)	(42.714)	(1.201)	(1.799)	(0.5)	(0.696)	(1.051)	(1.596)
Year = 2004	-18.675	-102.896	28.439	-30.329	-47.115	-72.567	2.582**	1.536	0.396	0.601	2.185**	0.935
	(53.526)	(72.689)	(22.293)	(29.719)	(41.982)	(52.418)	(1.274)	(1.813)	(0.568)	(0.776)	(1.113)	(1.599)
Year = 2005	-49.413	-73.341	8.823	-49.877	-58.237	-23.464	3.025**	2.878	0.39	-0.557	2.635**	3.435**
	(51.695)	(85.167)	(23.128)	(35.753)	(38.872)	(58.371)	(1.41)	(1.763)	(0.635)	(0.923)	(1.174)	(1.583)
Year = 2006	-12.137	-29.284	11.659	-40.62	-23.796	11.336	2.31	1.674	0.58	-1.952*	1.73	3.627**
	(52.472)	(82.27)	(22.277)	(36.65)	(40.865)	(55.477)	(1.606)	(2)	(0.716)	(0.997)	(1.305)	(1.798)
Year = 2007	-51.721	-89.301	13.29	-54.283	-65.011	-35.018	2.442*	2.481	0.508	-1.036	1.934	3.517**
	(58.169)	(89.633)	(26.842)	(39.205)	(40.624)	(59.985)	(1.42)	(1.832)	(0.589)	(0.927)	(1.207)	(1.65)
Year = 2008	-6.6	-57.784	34.431	-35.191	-41.032	-22.593	1.667	-0.589	0.029	-1.775*	1.638	1.187
	(58.715)	(95.287)	(28.138)	(42.8)	(41.86)	(63.988)	(1.304)	(1.792)	(0.616)	(0.917)	(1.082)	(1.62)
Year = 2009	-18.189	-50.828	37.804	-46.85	-55.993	-3.978	1.602	-0.187	0.357	-2.13**	1.244	1.942
	(59.869)	(92.583)	(28.875)	(43.462)	(42.791)	(61.085)	(1.433)	(1.997)	(0.623)	(0.876)	(1.171)	(1.809)
Year = 2010	-18.93	-9.906	22.427	-55.86	-41.357	45.954	3.049*	1.301	0.884	-0.769	2.165	2.07
	(59.752)	(91.419)	(28.44)	(44.913)	(44.076)	(59.535)	(1.589)	(2.125)	(0.657)	(0.858)	(1.325)	(1.83)
Observations	64,086	64,086	64,086	64,086	64,086	64,086	64,086	64,086	64,086	64,086	64,086	64,086

Notes: Output corresponds to graphical event studies displayed in Figure 10. 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 D.11: Full Tabular Output – Geographical Spillover Event Studies

	Hospital Outflows	Outflows APC	Outflows Non-APC	Hospital Inflows	Inflows APC	Inflows Non-APC
Year = 1998	1.382	0.116	1.266	-0.259	-1.123	0.863
	(2.929)	(0.89)	(2.324)	(5.403)	(1.172)	(4.72)
Year = 1999	-1.859	-0.085	-1.773	-1.202	-1.407**	0.204
	(1.944)	(0.541)	(1.668)	(3.154)	(0.697)	(2.79)
Year = 2000	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	3.621*	0.761	2.86*	0.664	-0.035	0.699
	(1.921)	(0.479)	(1.603)	(2.532)	(0.623)	(2.132)
Year = 2002	3.988	0.729	3.259	0.022	-0.066	0.088
	(2.56)	(0.659)	(2.092)	(3.678)	(0.833)	(3.136)
Year = 2003	3.03	0.316	2.714	1.543	-0.417	1.959
	(2.686)	(0.642)	(2.349)	(4.721)	(1.013)	(4.146)
Year = 2004	4.457	0.492	3.965*	4.167	0.032	4.135
	(2.865)	(0.743)	(2.374)	(5.272)	(1.208)	(4.549)
Year = 2005	6.11*	0.797	5.313*	3.078	-0.565	3.643
	(3.428)	(0.855)	(2.77)	(5.421)	(1.271)	(4.634)
Year = 2006	8.073**	1.153	6.92**	3.671	-0.761	4.432
	(3.96)	(0.957)	(3.175)	(5.779)	(1.334)	(4.964)
Year = 2007	5.961	0.659	5.302*	4.976	-0.88	5.856
	(3.877)	(1.001)	(3.067)	(6.14)	(1.413)	(5.302)
Year = 2008	3.894	0.314	3.58	2.229	-1.924	4.153
	(3.833)	(1.022)	(3.01)	(6.04)	(1.398)	(5.227)
Year = 2009	4.139	0.345	3.794	2.615	-2.387*	5.001
	(3.92)	(1.02)	(3.096)	(5.988)	(1.395)	(5.235)
Year = 2010	4.479	0.33	4.149	5.941	-1.963	7.903
	(4.292)	(1.097)	(3.377)	(6.394)	(1.508)	(5.616)
Observations	64,084	64,084	64,084	64,084	64,084	64,084

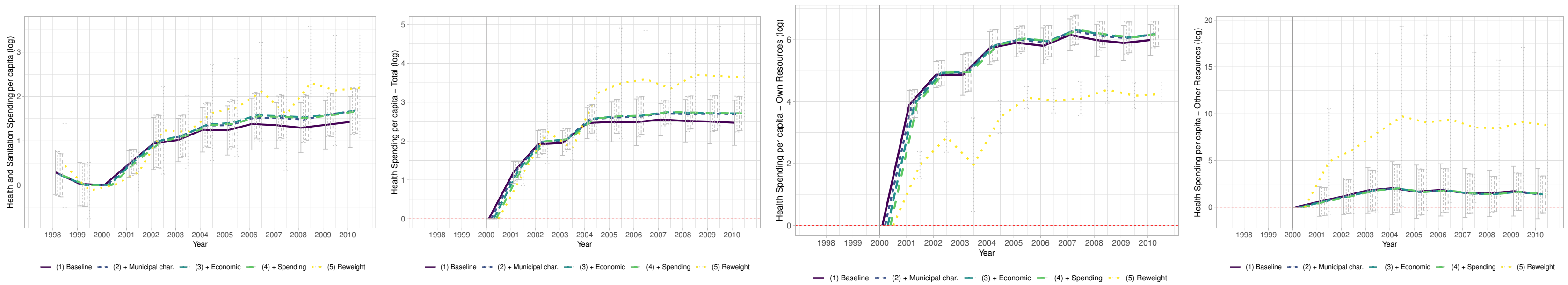
Notes: Output corresponds to graphical event studies displayed in Figure 11. Coefficients are displayed with standard errors in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10%, respectively.

Table D.12: Full Tabular Output – Geographical Spillover Distributional Event Studies

	Hospital Outflows		Outflows APC		Outflows Non-APC		Hospital Inflows		Inflows APC		Inflows Non-APC	
	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below
Year = 1998	5.221	11.196*	0.791	1.452	4.429	9.744**	-3.371	-5.847	-0.481	-3.558	-2.89	-2.289
	(4.402)	(6.463)	(1.287)	(2.347)	(3.558)	(4.669)	(7.94)	(10.58)	(1.663)	(2.616)	(6.918)	(9.047)
Year = 1999	4.367	1.895	-0.006	-0.227	4.373	2.122	3.547	1.909	1.842	-0.864	1.704	2.774
	(3.09)	(3.735)	(0.781)	(1.317)	(2.776)	(2.944)	(4.923)	(6.726)	(1.126)	(1.537)	(4.349)	(5.83)
Year = 2000	0	0	0	0	0	0	0	0	0	0	0	0
	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)	(–)
Year = 2001	1.806	11.632***	0.296	2.292**	1.51	9.34***	-3.164	-3.449	-0.254	-0.598	-2.91	-2.852
	(3.08)	(3.735)	(0.707)	(1.094)	(2.661)	(3.031)	(4.051)	(5.762)	(1.033)	(1.239)	(3.39)	(4.912)
Year = 2002	3.046	14.522***	0.771	2.971*	2.275	11.551***	-2.422	-3.684	-0.634	-1.151	-1.788	-2.533
	(4.151)	(5.357)	(0.991)	(1.604)	(3.478)	(4.24)	(6.368)	(7.704)	(1.388)	(1.703)	(5.451)	(6.602)
Year = 2003	1.374	9.586*	0.268	1.171	1.106	8.415*	-8.045	-8.418	-0.9	-2.475	-7.145	-5.942
	(4.29)	(5.556)	(1.087)	(1.542)	(3.713)	(4.874)	(7.419)	(9.491)	(1.562)	(2.346)	(6.448)	(8.239)
Year = 2004	-2.405	7.504	-0.743	0.1	-1.662	7.404	-14.253*	-11.152	-1.958	-2.908	-12.296*	-8.244
	(4.499)	(5.906)	(1.126)	(1.709)	(3.735)	(4.872)	(8.568)	(10.754)	(1.798)	(2.739)	(7.433)	(9.051)
Year = 2005	-3.499	9.987	-1.128	0.271	-2.371	9.716*	-13.289	-12.515	-2.44	-5.16*	-10.849	-7.355
	(5.376)	(7.067)	(1.358)	(1.798)	(4.3)	(5.777)	(8.758)	(11.716)	(1.943)	(2.921)	(7.515)	(9.707)
Year = 2006	-7.688	8.609	-1.846	0.093	-5.843	8.516	-12.656	-10.059	-1.938	-4.889*	-10.719	-5.17
	(6.297)	(8.215)	(1.581)	(1.933)	(4.953)	(6.692)	(9.122)	(12.12)	(2.059)	(2.862)	(7.805)	(10.185)
Year = 2007	-5.413	6.747	-1.787	-1.053	-3.626	7.801	-16.23	-12.193	-1.83	-5.037*	-14.4*	-7.157
	(6.086)	(8.153)	(1.611)	(2.027)	(4.746)	(6.57)	(10.015)	(12.93)	(2.155)	(2.924)	(8.683)	(11.093)
Year = 2008	-1.823	6.961	-0.855	-0.52	-0.968	7.482	-18.803*	-22.955*	-2.684	-8.94***	-16.119*	-14.016
	(6.084)	(7.963)	(1.723)	(2.005)	(4.699)	(6.425)	(10.079)	(13.129)	(2.083)	(2.761)	(8.825)	(11.443)
Year = 2009	-0.876	8.994	0.086	0.967	-0.962	8.027	-17.563*	-20.192	-2.545	-9.907***	-15.018*	-10.285
	(6.425)	(7.735)	(1.776)	(1.913)	(4.955)	(6.34)	(9.968)	(13.414)	(2.065)	(2.723)	(8.812)	(11.887)
Year = 2010	0.12	11.34	-0.133	0.599	0.253	10.741	-17.176	-11.338	-2.561	-8.889***	-14.615	-2.449
	(7.023)	(8.326)	(1.899)	(2.023)	(5.401)	(6.78)	(10.511)	(14.886)	(2.132)	(3.224)	(9.326)	(13.149)
Observations	64,084	64,084	64,084	64,084	64,084	64,084	64,084	64,084	64,084	64,084	64,084	64,084

Notes: Output corresponds to graphical event studies displayed in Figure 11. 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 D1: Robustness to Control Specification: Spending and Revenue

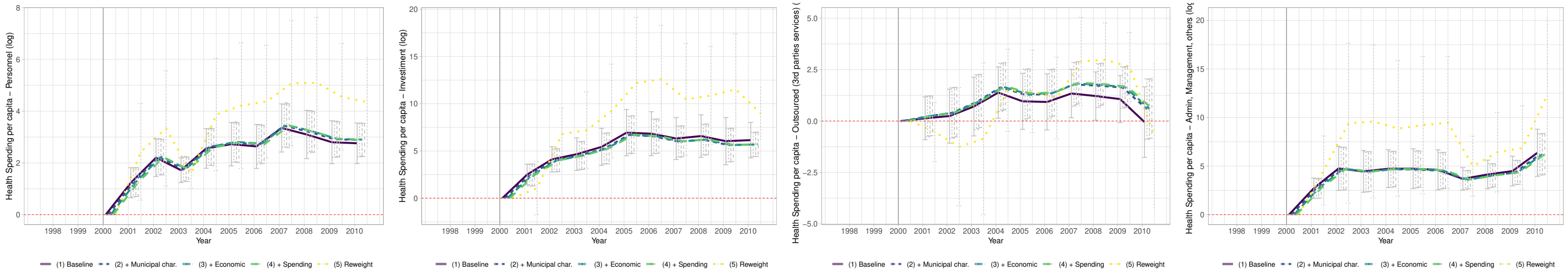


(a) Health and Sanitation (FINBRA)

(b) Total Health Spending (SIOPS)

(c) From Own Resources

(d) From Other Resources



(e) Human Resources

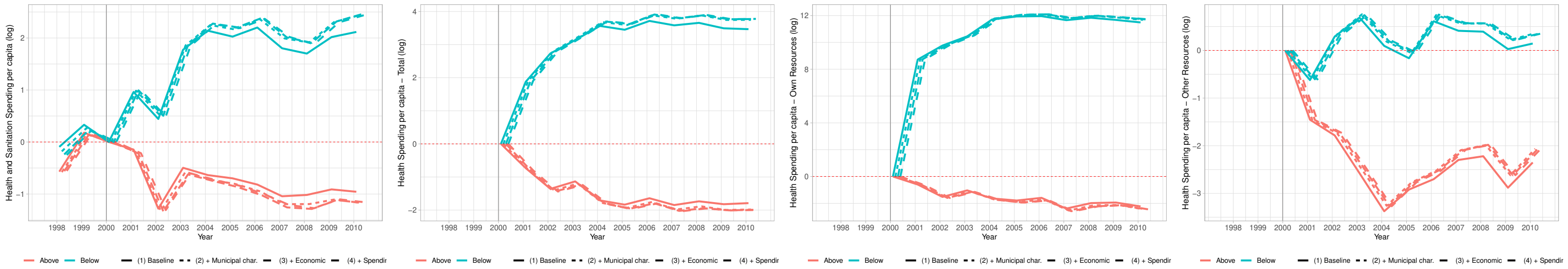
(f) Investment

(g) Outsourced (3rd party services)

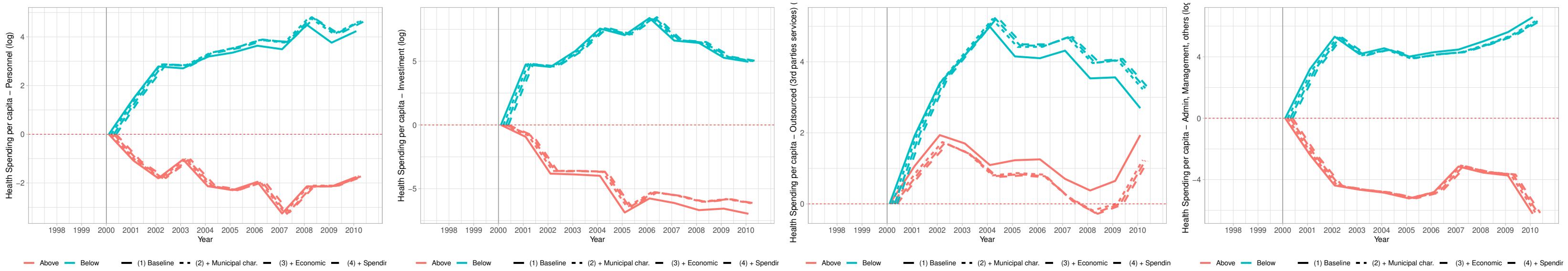
(h) Admin, Management and Others

Notes: Refer to notes to Figure 4. Identical models are estimated, however varying control sets in the manner indicated in legend titles. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 4. Estimation is based on 63,280 municipality by year cells for Figure D1a and D1c to D1f, and 55,321 for Figure D1b.

Figure D2: Robustness to Control Specification for Distributional Effects: Spending and Revenue

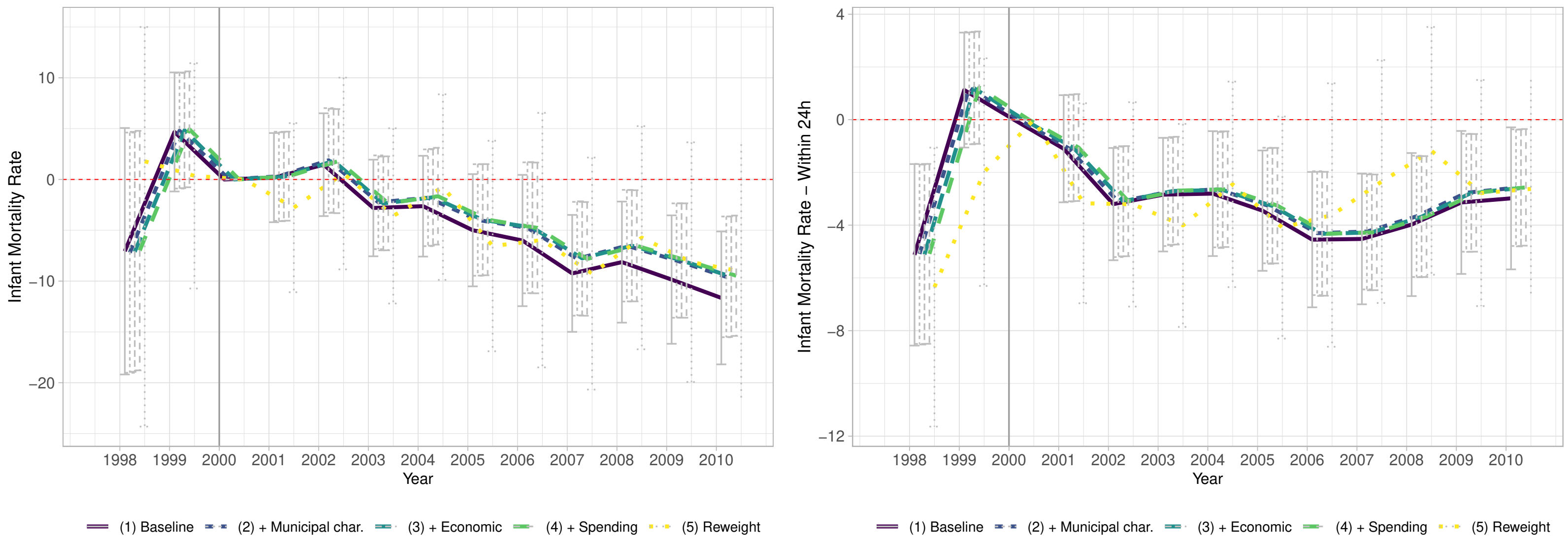

(a) Health and Sanitation (FINBRA)
(b) Total Health Spending (SIOPS)
(c) From Own Resources
(d) From Other Resources

(e) Human Resources
(f) Investment
(g) Outsourced (3rd party services)
(h) Admin, Management and Others

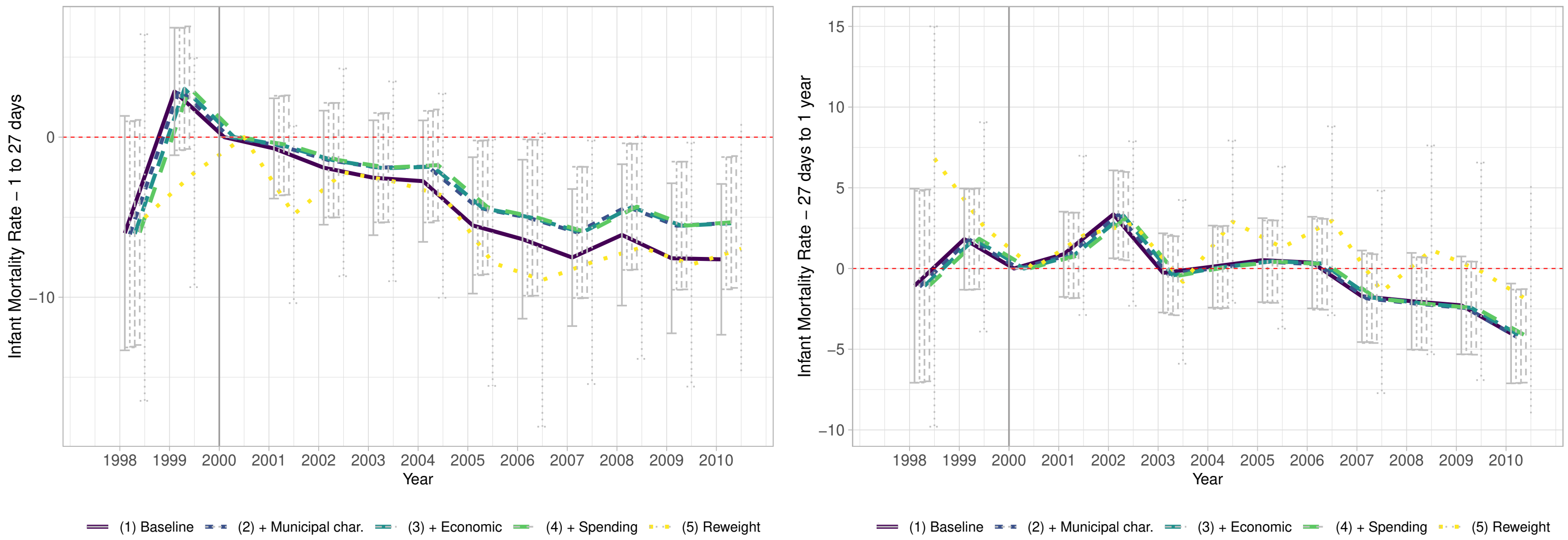


Notes: Refer to notes to Figure 5. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 5. Estimation is based on 63,280 municipality by year cells for Figure D2a and D2c to D2f, and 55,321 for Figure D2b.

Figure D3: Robustness to Control Specification: Infant Mortality


(a) Total

(b) Deaths within 24 hours


(c) Deaths between 1-27 days

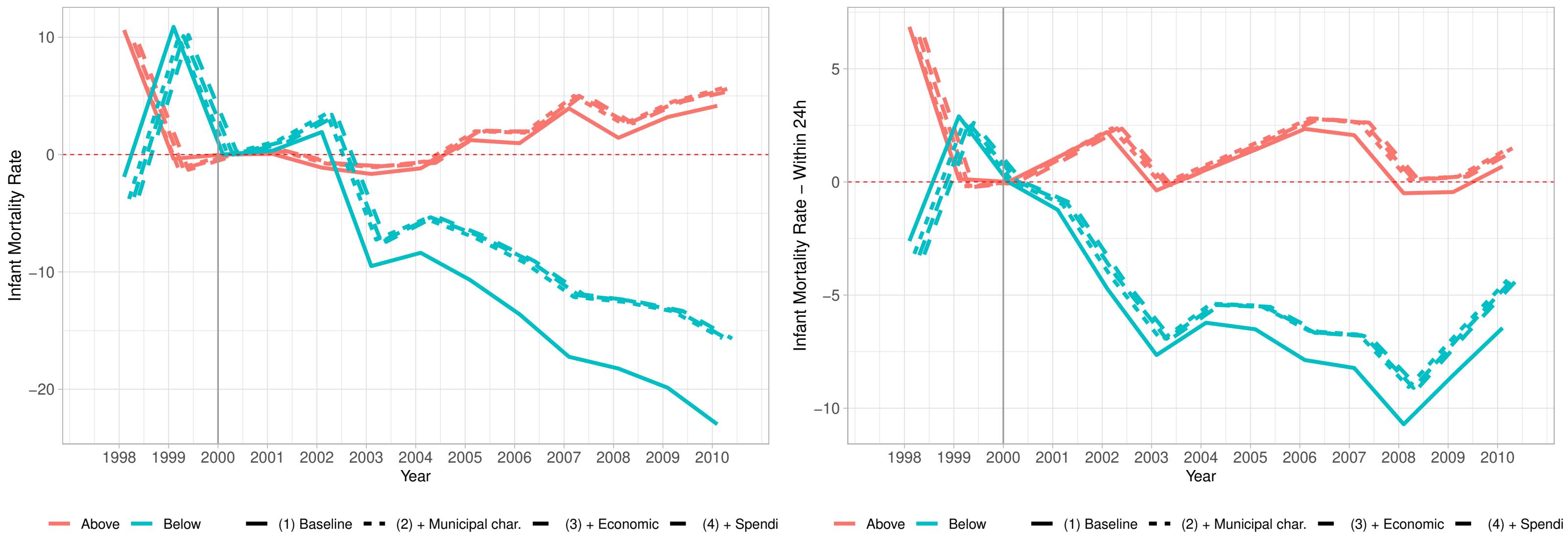
(d) Deaths from 27 days to 1 year

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 90% confidence intervals. All other details follow those described in notes to Figure 6. Estimation is based on 64,481 municipality by year cells.

Figure D4: Robustness to Control Specification for Distributional Effects: Infant Mortality

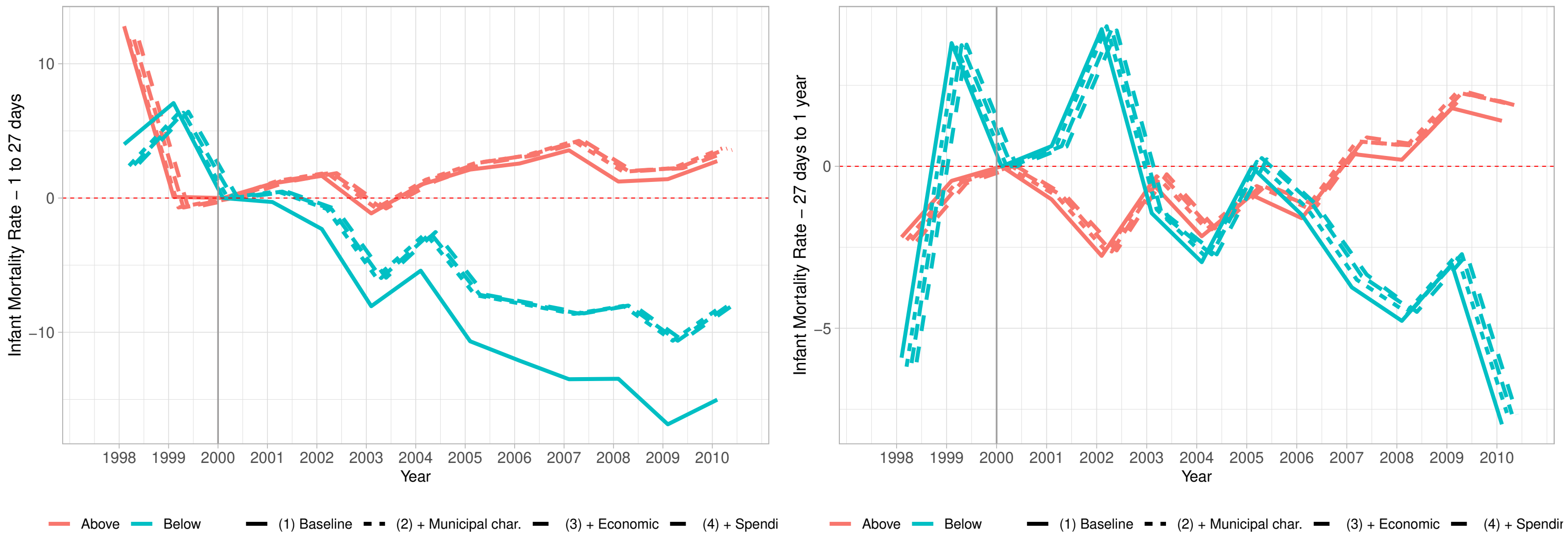
(a) Total

(b) Deaths within 24 hours



(c) Deaths between 1-27 days

(d) Deaths from 27 days to 1 year

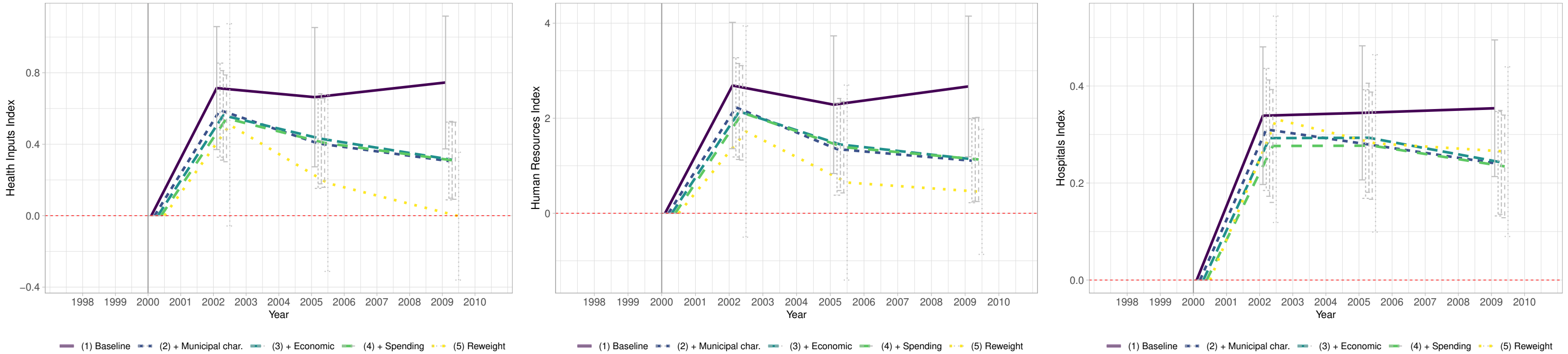


Notes: Refer to notes to Figure 6. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 6.

Figure D5: Robustness to Control Specification: Services, Production and Inputs

(a) Access and Production of Health Service (b) Primary Care Access and Production (c) Non-Primary Care Access and Production

(d) Health Inputs (e) Human resources (f) Hospitals



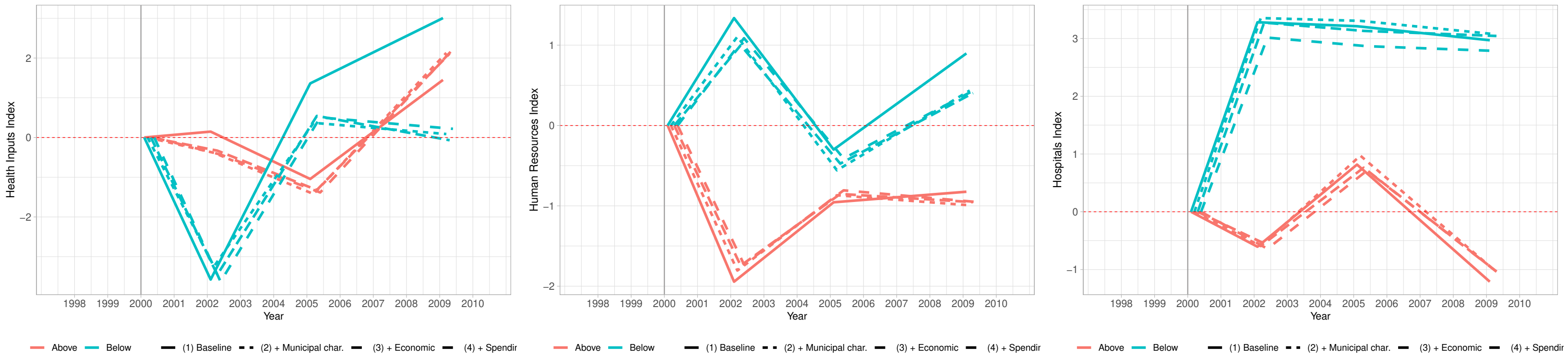
Notes: Refer to notes to Figure 8. Identical models are estimated, however varying control sets in the manner indicated in legend titles. Joined line plots present point estimates, and error bars represent 90% confidence intervals. All other details follow those described in notes to Figure 8. Estimation is based on 64,482 municipality by year cells for D5a, and 19,364 for D5d.

Figure D6: Robustness to Control Specification for Distributional Effects: Services, Production and Inputs

(a) Access and Production of Health Services

(b) Primary Care Access and Production

(c) Non-Primary Care Access and Production



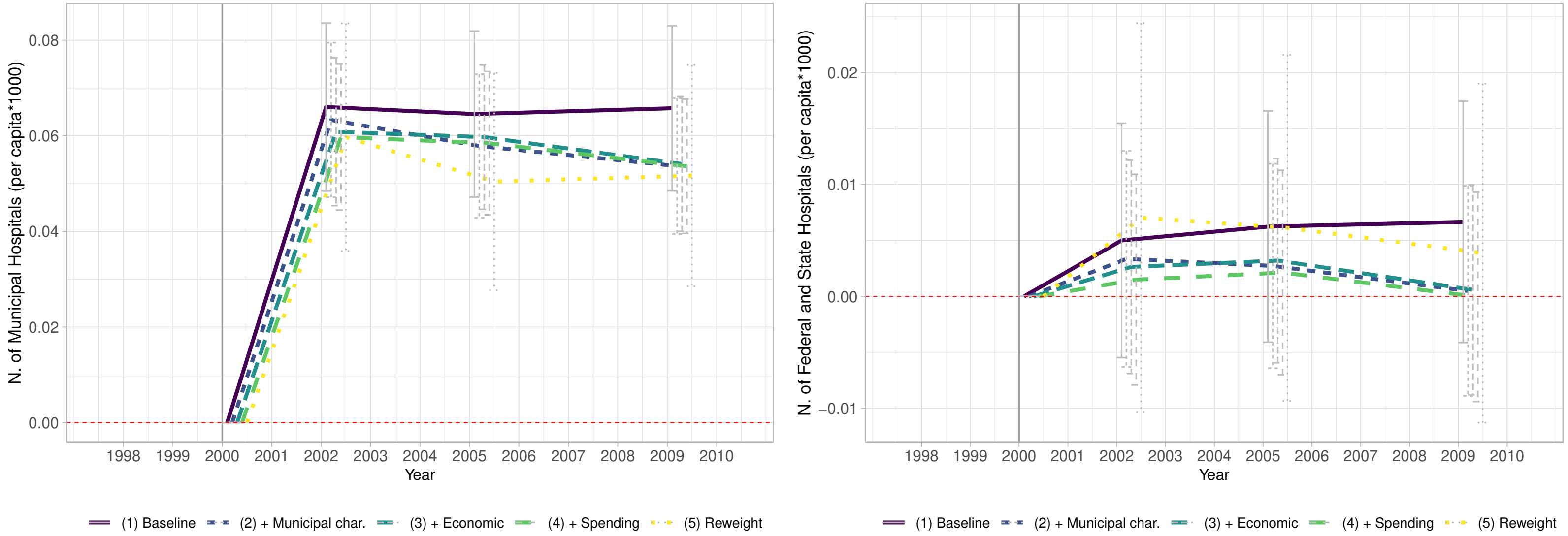
(d) Health Inputs

(e) Human resources

(f) Hospitals

Notes: Refer to notes to Figure 8. 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 8. Estimation is based on 64,482 municipality by year cells for D6a, and 19,364 for D6f.

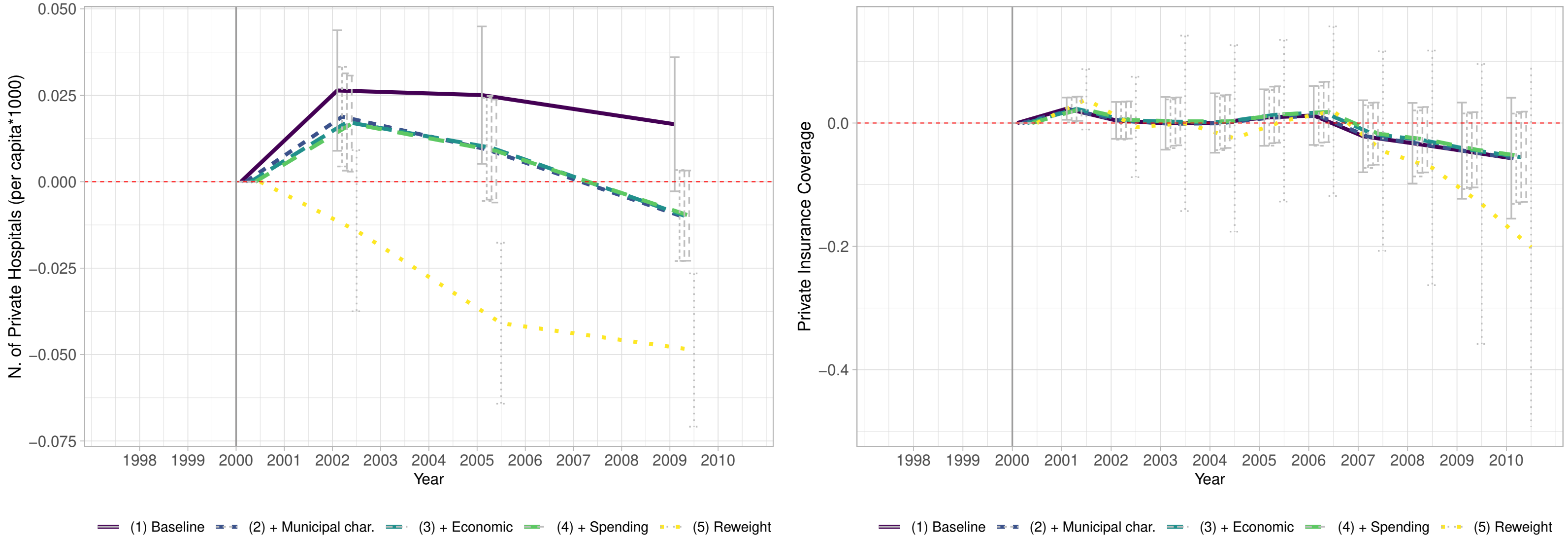
Figure D7: Robustness to Control Specification: Health System Spillovers


(a) Municipal Hospitals

(b) State and Federal Hospitals

(c) Private Hospitals

(d) Private Insurance

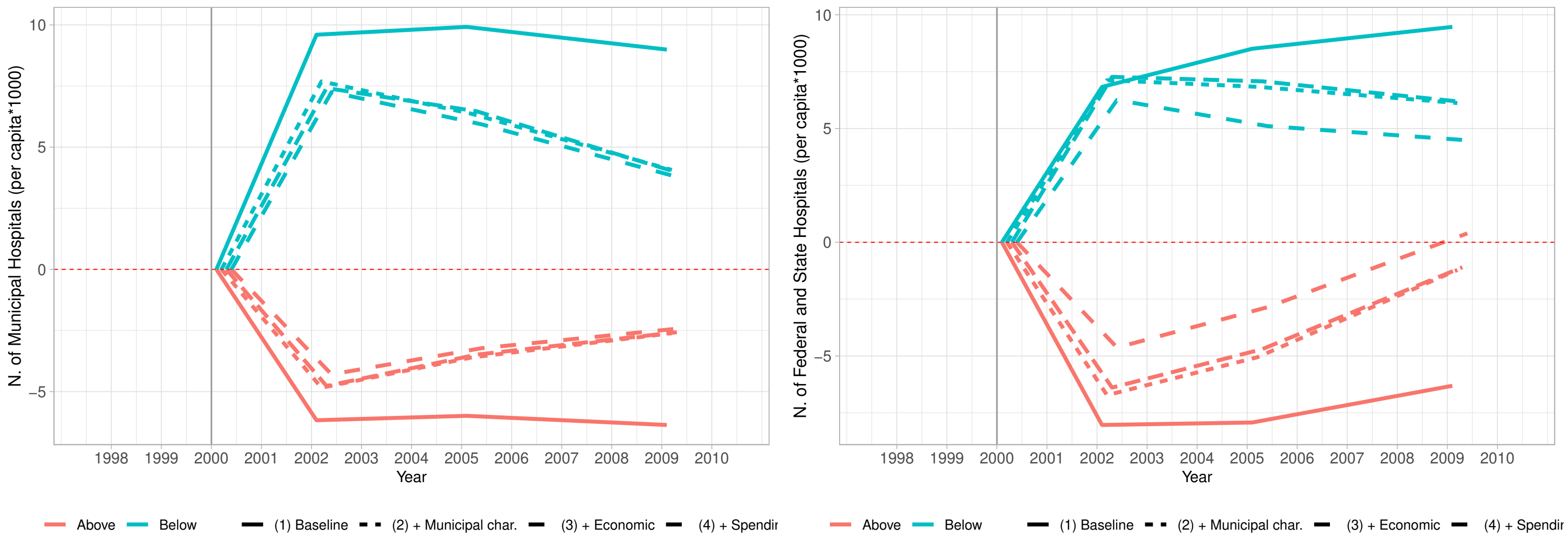


Notes: Refer to notes to Figure 9. Identical models are estimated, however varying control sets in the manner indicated in legend titles. Joined line plots present point estimates, and error bars represent 90% confidence intervals. All other details follow those described in notes to Figure 9. Estimation is based on 64,481 municipality by year cells.

Figure D8: Robustness to Control Specification for Distributional Effects: Health System Spillovers

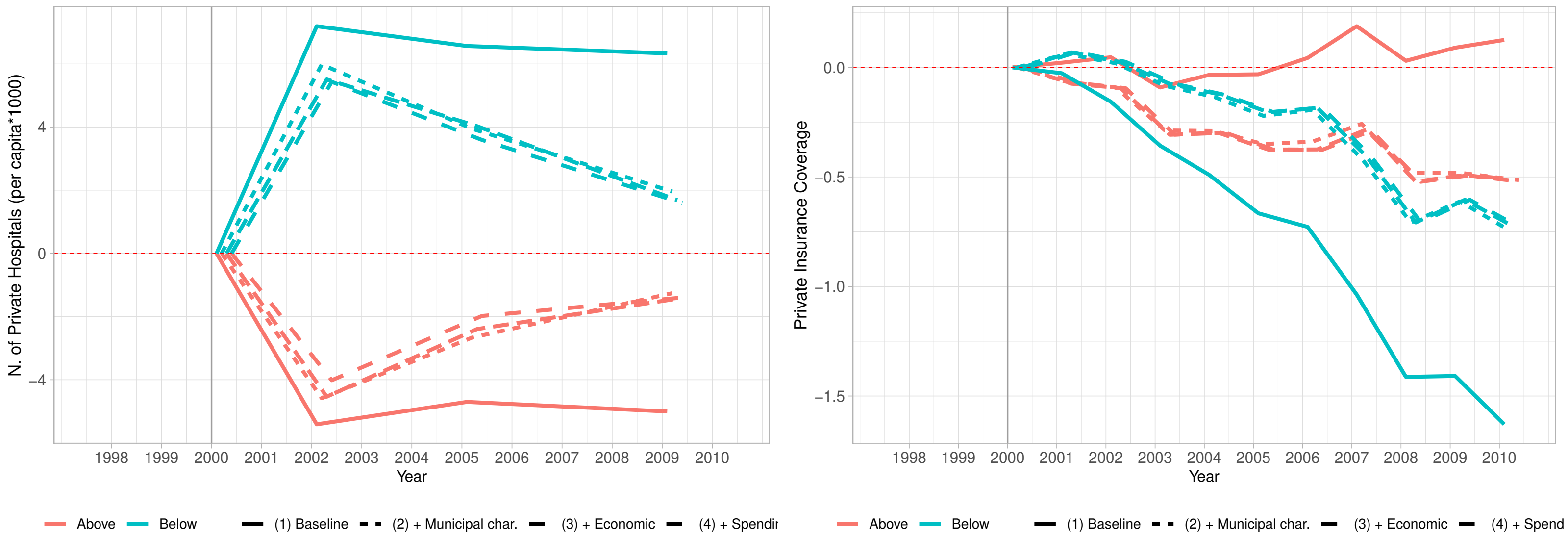
(a) Municipal Hospitals

(b) State and Federal Hospitals



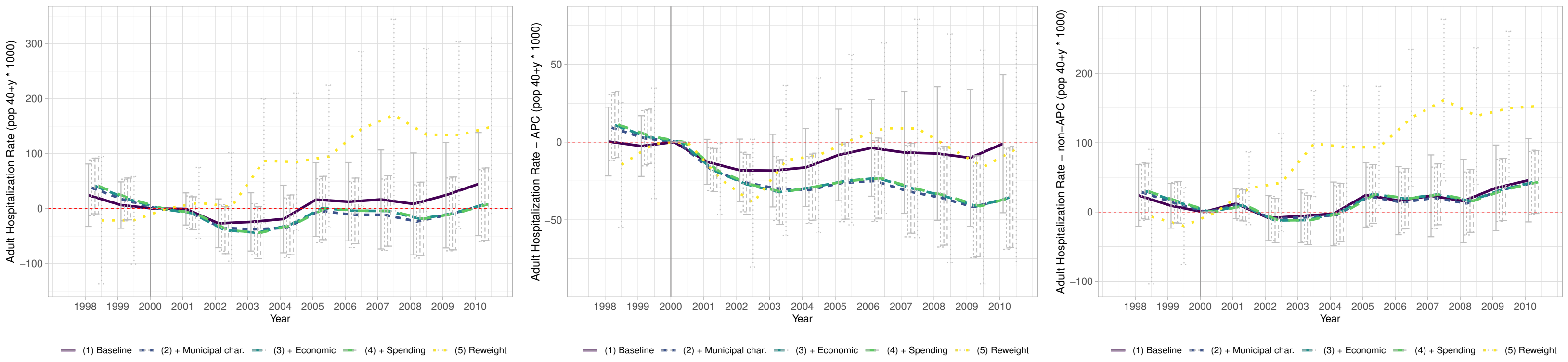
(c) Private Hospitals

(d) Private Insurance Coverage



Notes: Refer to notes to Figure 9. 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. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 9.

Figure D9: Robustness to Control Specification: Adult Hospitalization and Mortality rates


(a) Adult Hospitalization

(b) Adult Hospitalizations – APC

(c) Adult Hospitalization – Non-APC


(d) Adult Mortality

(e) Mortality – APC

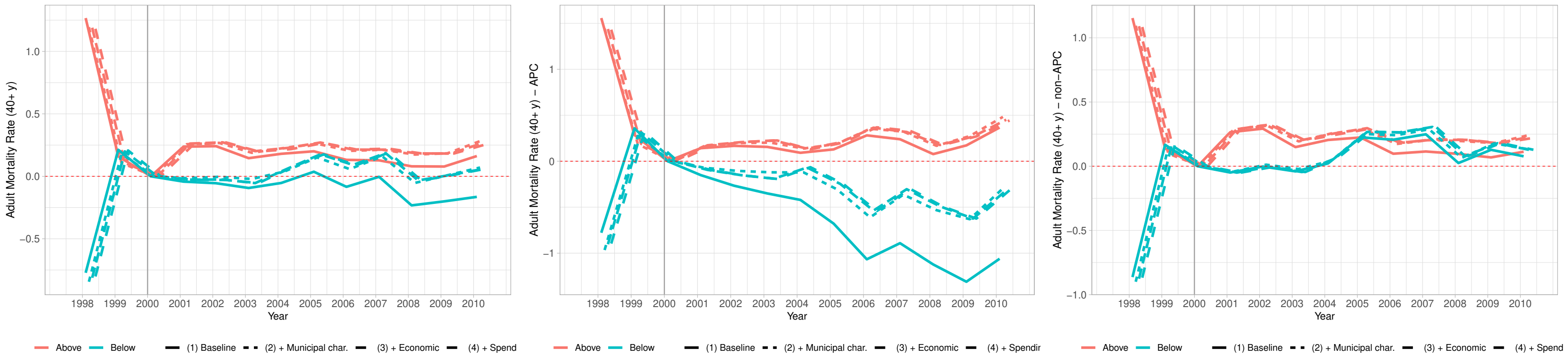
(f) Mortality – Non-APC

Notes: Refer to notes to Figure 10. 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 90% confidence intervals. All other details follow those described in notes to Figure 10. Estimation is based on 64,481 municipality by year cells.

Figure D10: Robustness to Control Specification for Distributional Effects: Adult Hospitalization and Mortality rates

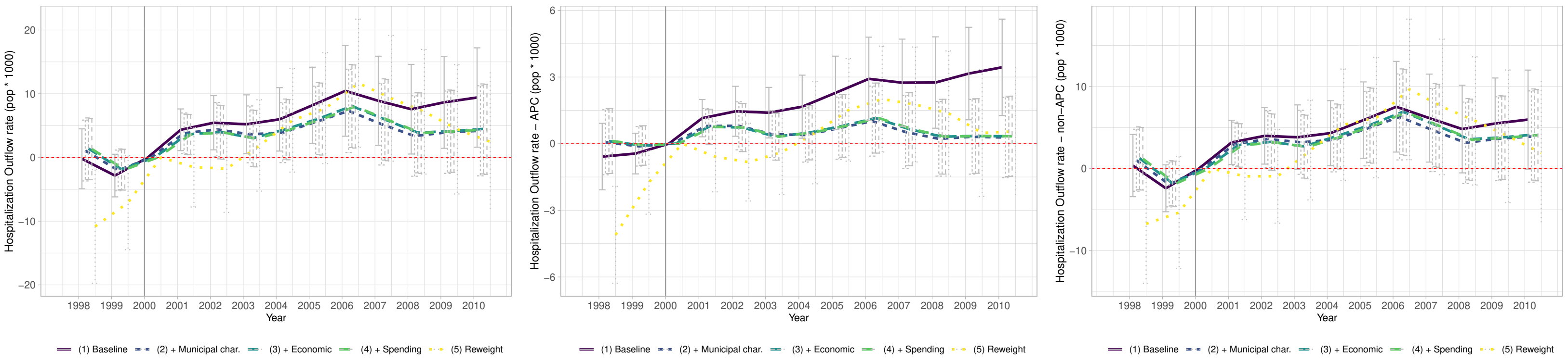
(a) Adult Hospitalization (b) Adult Hospitalizations – APC (c) Adult Hospitalization – Non-APC

(d) Adult Mortality (e) Mortality – APC (f) Mortality – Non-APC



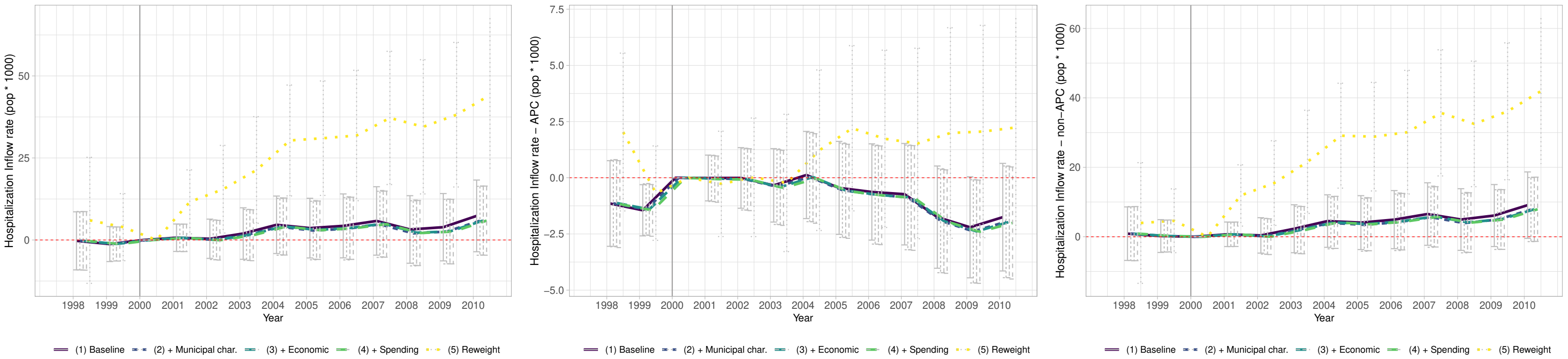
Notes: Refer to notes to Figure 10. Identical models are estimated, however varying control sets in the manner indicated in legend titles. All blue lines refer to above threshold municipalities, and all red lines present identical specifications for below threshold municipalities. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 10.

Figure D11: Robustness to Control Specification: Patient Mobility and Geographical Spillovers


(a) Total Hospitalization Outflow

(b) Outflow: Amenable to Primary Care

(c) Outflow: Non Amenable to Primary Care

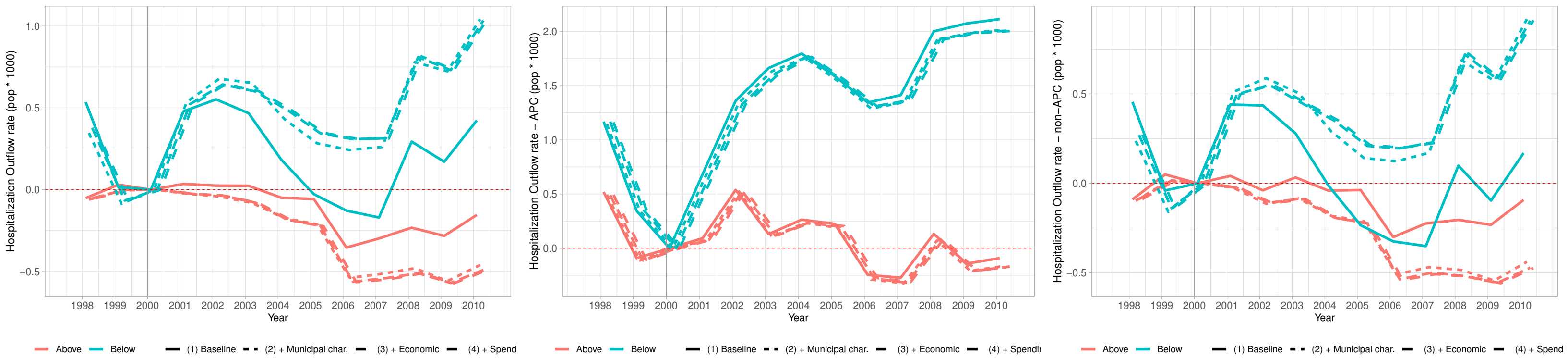

(d) Total Hospitalization Inflow

(e) Inflow: Amenable to Primary Care

(f) Inflow: Non Amenable to Primary Care

Notes: Refer to notes to Figure 11. 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 90% confidence intervals. All other details follow those described in notes to Figure 11. Estimation is based on 64,481 municipality by year cells.

Figure D12: Robustness to Control Specification for Distributional Effects: Patient Mobility and Geographical Spillovers


(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

Notes: Refer to notes to Figure 11. 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. Thick lines refer to the principal specification. All other details follow those described in notes to Figure 11.