

# ECONOMIC AND POLITICAL EFFECTS OF FISCAL RULES: EVIDENCE FROM A NATURAL EXPERIMENT IN COLOMBIA <sup>\*</sup>

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## Abstract

Fiscal rules are a promising policy tool to address fiscal indiscipline, but their effectiveness and political feasibility remain unclear, particularly in weakly institutionalized settings. To answer this question, we leverage exogenous variation across Colombian municipalities in exposure to a subnational fiscal rule that set a cap on operating expenses as a share of current revenues. We show that the fiscal rule was highly effective at reducing operating costs and the probability of a current deficit, with no changes in capital spending (i.e., no creative accounting). This large reduction in operating expenses came at no meaningful cost in terms of local public goods and living standards, as proxied by education and health indicators, nighttime lights, and sanctions for corruption. Adoption of the rule increased electoral support for the party of the incumbent mayor and weakly reduced protests against the municipal government. This suggests that fiscal rules can improve political agency in settings, like Colombia, with weak political parties and limited career concerns for local politicians.

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# 1 Introduction

Government overspending is a perennial source of concern among academics and policymakers, as it could reflect an inefficient resource allocation and may lead to a costly fiscal adjustment (Yared, 2019). In Latin America and the Caribbean, public spending has been growing for several decades and a recent IDB report estimated that waste in procurement, civil service and targeted transfers amounted to 4.4% of GDP, an amount comparable to what countries in the region spend in each of education or health (Izquierdo et al., 2018). Fiscal rules that set a numerical target for government spending offer a potential solution to fiscal indiscipline and are currently in use in multiple settings across the developed and the developing world (Wyplosz, 2013; Grembi and Manoel, 2012). However, their effectiveness, political feasibility and broader desirability remain unclear (Alesina and Perotti, 1996). There are three key open questions. First, are fiscal rules effective, or are they undermined by weak enforcement and creative accounting? Second, are fiscal rules politically feasible, or do they lead to political backlash, making policymakers unwilling to introduce or enforce them? Third, do fiscal rules improve welfare, or do they negatively affect public good provision and prevent the government from adequately responding to unanticipated fiscal needs?

There is a large literature on the economic impact of fiscal rules, but it mostly consists of cross-country studies. Establishing causality has proven difficult, given that multiple economic and political factors plausibly underlie the decision to introduce a fiscal rule (Heinemann et al., 2018). Similar issues affect previous work on the political consequences of fiscal rules. While conventional wisdom links austerity measures to political backlash, the available evidence is mixed and mostly correlational, leaving this debate largely unresolved (see Arias and Stasavage (2019) for a recent summary). Previous research has largely focused on high-income countries and very little is known about the functioning of fiscal rules in the developing world. Fiscal rules could be particularly useful in the latter, given that governments in developing countries tend to be financially vulnerable and prone to deficits, especially at the subnational level (Rodden et al., 2003; Gadenne and Singhal, 2014). However, limitations in enforcement and the threat of political backlash could be particularly detrimental to the effectiveness or sustainability of fiscal rules in weakly-institutionalized settings.

In this paper, we leverage exogenous variation in exposure to a fiscal rule across Colombian municipalities to study its causal effect on fiscal, economic and political outcomes. Four features of our study are noteworthy. First, we focus on a comparable set of municipalities (almost 90% of the country's total) that face *de jure* the same fiscal rule and share a com-

mon institutional structure. These are the least developed municipalities (i.e., excluding large cities), which increases the external validity of our findings to other settings in the developing world. Second, we exploit differential *de facto* exposure across municipalities to the fiscal rule, which set a limit to the operating costs of municipal governments. This type of fiscal rule, often referred to as a *golden rule*, is not unique to our setting but has seldom been studied before (Poterba, 1996; Bassetto and Sargent, 2006; Glaeser, 2013). Our setting does provide a unique opportunity to learn about the possibility of reducing the size of government without directly sacrificing on public investment. Third, we provide a comprehensive assessment of the effectiveness, political feasibility and overall desirability of the fiscal rule by analyzing its impact on fiscal outcomes, various indicators of local public goods and living standards, and local political outcomes, including election results and protests. Finally, our sample period covers almost two decades after the introduction of the rule, which allows us to assess its economic and political sustainability over a long time horizon.

The fiscal rule at the center of our study addressed the fiscal imbalance affecting a large number of municipalities following an ambitious decentralization reform in the early 1990s. The rule aimed to curb the rapid growth in the size of municipal governments by setting a cap to operating costs, expressed as a share of the municipality's disposable current revenue. The latter corresponds to local tax and non-tax revenue (i.e., self-generated revenue) net of a small set of earmarked revenues, plus a small share of intergovernmental transfers. Operating costs include the payroll of the municipal government and the pensions of retired staff (excluding employees in the education and health sectors), procurement of goods and services, rent and maintenance of government properties, and payments from legal disputes. The country's fiscal watchdog (Contraloría General de la República, CGR) was charged with verifying compliance. Municipalities that fail to comply lose access to financial support from the central government and their top officials (i.e., mayor) may be subject to disciplinary sanctions. The fiscal rule set different caps based on municipal population and the level of disposable current revenues. To maximize sample size and abstract away from other aspects of the reform affecting larger cities, we focus our attention on municipalities in the lowest category, which comprises almost 90% of all municipalities in the country. These are small municipalities (median population of 11,000 inhabitants) with the lowest levels of disposable current revenue and a common institutional structure.

For our analysis, we construct a binary measure of exposure to the fiscal rule based on each municipality's average ratio of operating expenses to disposable current revenue (i.e., the targeted fiscal indicator, henceforth referred to as overspending indicator) over the five-year period before the rule came into effect in 2001. Our empirical strategy exploits the fact that the fiscal rule was binding *de facto* only for those municipalities whose operating costs

exceeded the legal limit when the reform came into effect, even though all municipalities were exposed to it *de jure*. We use this measure of predetermined exposure to the reform to implement a difference-in-differences (DiD) research design, including municipality and department-year fixed effects. The identifying assumption is that our outcomes of interest should not differentially vary based on our measure of exposure in the absence of the actual reform. We use event studies to provide indirect evidence in support of the *parallel trends* assumption. To further address potential bias arising from imbalance in predetermined covariates, we verify that our results are robust to the inclusion of additional controls or to the use of propensity-score weighting (Hirano and Imbens, 2001; Hirano et al., 2003).

Our analysis proceeds in three stages. First, we examine the effectiveness of the reform using administrative data on municipal public finance between 1996 and 2018. We find that municipalities exposed to the reform experience an average decrease of 32 percentage points (pp) in the overspending indicator, equivalent to 30% of the sample mean. Accordingly, the probability of a current deficit decreases by 32 pp, which corresponds to 49% of the sample mean. This sizable improvement in the health of municipal finances is mostly driven by a reduction in operating expenses (21% decrease), though we also find a 7% increase in disposable current revenue. In turn, the decrease in operating expenses is mostly driven by general expenses (i.e., procurement), while the main contributor to the increase in revenue is the non-tax component. These are plausibly the margins that can be adjusted at lower cost, both economically and politically. Importantly, we find no meaningful change in capital expenses (i.e., investment) and we observe an 11 pp decrease in the probability of a total deficit. This shows that affected municipalities did not respond by strategically shifting operating expenses into the capital account through creative accounting practices.

We then move to study the effects of the reform on local political outcomes. Our analysis of elections focuses on support for the party of the incumbent mayor, who faces a one-term limit, and shows that voters reward the incumbent party differentially in municipalities exposed to the reform. We find that the vote share for the incumbent party in mayoral elections increases by 6 pp in municipalities exposed to the reform, which is equivalent to 13% of the sample mean. This is associated with a somewhat noisy increase of 5 pp in the probability of re-election for the incumbent party. These findings suggest that fiscal rules ameliorate problems of political agency in a setting like ours, characterized by weak political parties and limited career concerns for individual politicians (i.e., no re-election). As a complementary measure of political behavior, we study the incidence of protests against the municipal government. We find that the probability of protests weakly decreases (if anything), especially for those motivated by labor disputes and breach of agreements, in line with anecdotal evidence showing that large fiscal deficits before the introduction of the

fiscal rule were causing substantial delays in the payment of salaries and other obligations. The absence of a political backlash that we document is in line with recent cross-country evidence (Arias and Stasavage, 2019), and with survey evidence showing that voters support austerity when it does not affect social spending (Ardanaz et al., 2020; Bansak et al., 2021).

In line with the absence of a political backlash, we show in the final part of the analysis that the fiscal rule was not associated with negative consequences for local public goods and living standards. In particular, we find no change in several indicators in the areas of education or health, or in nighttime luminosity, which we use as a proxy for local economic activity (Henderson et al., 2012). We also investigate whether the reform had broader impacts on the quality of governance by studying its effect on the extent of corruption across municipalities using administrative data on the universe of sanctions by CGR since 1990. We do not see any change in the probability of sanctions involving the municipality. These findings suggest that fiscal rules can be effective at reducing oversized governments without compromising the quality of local public services or local governance more broadly.

Our paper contributes to the large literature on fiscal rules. Early theoretical work in macroeconomics analyzed rules in the context of the optimal management of public debt (Barro, 1979; Lucas and Stokey, 1983), while the Public Choice school argued that rules could be necessary to rein in *Leviathanic* governments (Brennan and Buchanan, 1980). More recent theoretical work has increasingly focused on the impact of political factors on the desirability and the effectiveness of fiscal rules (e.g., Besley and Smart, 2007; Battaglini and Coate, 2008; Halac and Yared, 2018). The empirical literature is also voluminous. A recent meta-analysis by Heinemann et al. (2018) finds that rules are seemingly effective at constraining spending, but acknowledges that the existing literature is mostly cross-country and has struggled to establish causality. Recent work by Grembi et al. (2016) and Daniele and Giommoni (2020) uses a difference in discontinuities design to show that fiscal rules help to reduce deficits and corruption in Italian municipalities in the short run.<sup>1</sup> To the best of our knowledge, there is no specific evidence on *golden* fiscal rules that target current spending.

Whether fiscal rules can work in weakly institutionalized settings remains an open question. One source of concern is the combination of weak political parties and term limits, which may provide little incentive for incumbents to comply (Klašnja and Titiunik, 2017). Limited oversight can also facilitate artificial compliance through creative accounting (Milesi-Ferretti, 2004). We fill this gap in the literature by showing that fiscal rules can be highly effective at restoring fiscal discipline among subnational governments in the developing world.<sup>2</sup>

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<sup>1</sup>Gamalerio and Trombetta (2021) study the impact of fiscal rules on political selection in Italy.

<sup>2</sup>Restrepo and Alvarez (2005) use a similar methodology to ours to show a positive impact of the Colombian fiscal rule on operating costs in the short run (2001-2003). Relative to that study, our sample includes three times as many municipalities and covers more years both before and after the reform. Moreover, we

Unlike most of the previous literature, we further consider the possibility that fiscal rules could have negative unintended effects on public good provision and living standards. In this regard, we provide novel evidence showing that golden fiscal rules can lead to a meaningful reduction in the size of government without negatively affecting local public goods.

Our paper also speaks to a large literature on the political consequences of fiscal consolidation. Most work in this area has focused on the electoral effects of large fiscal contractions (i.e., austerity), with many studies finding no evidence of political backlash (Alesina et al., 1998, 2013; Arias and Stasavage, 2019; Bansak et al., 2021). However, several other papers have documented negative electoral effects of austerity (Fetzer, 2019; Ardanaz et al., 2020; Hübscher et al., 2020; Bojar et al., 2021). Other studies further show that austerity leads to an increase in social unrest, including riots and protests (Passarelli and Tabellini, 2017; Ponticelli and Voth, 2020; Vegh and Vuletin, 2014; Genovese et al., 2016). Yet another strand of literature finds that voters are best described as *fiscal conservatives* (Peltzman, 1992; Brender and Drazen, 2008; Drazen and Eslava, 2010). The existing literature is mostly correlational and it is heavily concentrated in high-income countries. Findings from the developed world may not easily extrapolate to developing countries due to differences in political behavior and institutions.

We contribute to this literature by providing within-country evidence of the causal effect of fiscal consolidation on elections and protests in a developing-country setting. In contrast to most previous work, we focus on fiscal rules instead of discretionary austerity policies, which likely affects voters' perceptions and the willingness of politicians to change course near elections or when faced with a negative shock. Moreover, while austerity often involves large cuts to welfare spending, which are inherently unpopular, our paper sheds light on the political effects of a *golden* fiscal rule that only targets governments' operating expenses. Finally, while most previous research has focused on the immediate political impact of fiscal reforms, we provide evidence of a persistent effect of fiscal rules on local politics. Our findings suggest that fiscal rules can help to align fiscal policy with the preferences of voters in settings with weak parties and limited individual incentives for politicians.

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consider potential effects on creative accounting, local public goods, and political outcomes. Sánchez and Zenteno (2011) show that Colombian municipalities that comply with the fiscal rule have better fiscal outcomes. Their empirical strategy uses the lagged share of minor taxes (i.e., excluding property and industry tax) in total tax revenue as an excluded instrument, which may fail to satisfy the exclusion restriction.

## 2 Institutional Background

### 2.1 Basic Information

Colombia is administratively divided into 32 departments and 1,122 municipalities. The mayor is the top municipal authority and is elected every four years using plurality rule.<sup>3</sup> Mayors face a one-term limit, but can be re-elected after one term out of office. Political parties are weak in Colombia, as in other countries in Latin America (Mainwaring, 2018). This is reflected in constant changes in party affiliation by politicians, as well as in the existence of an incumbency *disadvantage* in mayoral elections (Klašnja and Titunik, 2017).

Municipalities vary in their institutional complexity, based on a seven-tier categorization that depends on population and disposable current revenues. Categories range from 1 to 6, with larger numbers corresponding to smaller municipalities with less revenue, plus an additional special category for the largest cities. Municipalities in the upper categories (i.e., larger and richer) have their own Comptroller (contraloría municipal) to oversee local public finances, while those in lower categories are overseen by the Comptroller of the corresponding department (contraloría departamental). Municipalities in the upper categories also have elected neighborhood councils (Juntas Administradoras Locales, JAL) that provide additional oversight on the municipal government. All municipalities have a *personero*, who is appointed by the municipal council and acts as a local representative of the Inspector General's office (Procuraduría General de la Nación, PGN). The municipal category determines the salary of the mayor, which also serves as a cap on the remuneration of all other local public officials, including members of the municipal council and the *personero*.

### 2.2 Municipal Public Finance

Municipalities rely on three main sources of revenue. These are tax revenue, local non-tax revenue (i.e., fines and fees), and transfers from the central government.<sup>4</sup> The main local taxes are the property tax, a tax on gross business receipts and a surcharge on the price of gasoline.<sup>5</sup> Municipalities can issue fines for traffic violations or for the infringement of public ordinances, and can charge fees for public services such as energy or street cleaning, as well as for the use of public spaces such as slaughterhouses or market squares. Transfers from the central government are determined using a formula and mostly provide funding for expenses in the areas of education, health, water and sanitation. The system of transfers,

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<sup>3</sup>Term length increased from two to three years in 1994, and to four years in 2003.

<sup>4</sup>Municipalities also earn royalties from the extraction of natural resources in their territory. Royalties are formula-determined and earmarked for investments in education, health and water (Martinez, 2020).

<sup>5</sup>Subnational taxes can only be created by national laws, which also regulate tax bases and rates.

called Sistema General de Participaciones (SGP), is highly regulated and funds must be kept in a separate account from other sources of municipal revenue. In contrast, municipal governments enjoy almost complete discretion over the use of their own tax and non-tax revenue, with the exception of certain earmarks. For instance, a share of property tax revenue must be transferred to the corresponding regional environmental agency.<sup>6</sup> The sum of tax and non-tax revenue, net of these earmarks, together with a small share of SGP transfers specifically designated for this purpose, constitutes disposable current revenue.

Spending by municipal governments can be disaggregated into current spending and investment. Current spending is the sum of operating costs and debt interest payments. There are three subcategories of operating costs. The first is called *personnel expenses* and corresponds to the payroll of permanent and temporary employees of the municipal government, the city council and the offices of the personero and the comptroller (if it exists), including all elected officials. This component includes the staff of the mayor's office and subsidiary departments (e.g., secretary of finance), but it does not include staff in the education or health sectors, as these are paid using SGP transfers. The second subcategory is called *general expenses* and includes purchases of goods and services (i.e., procurement), insurance premiums, and publications, as well as rent, maintenance and utility payments for municipal property. This component also includes travel and training expenses for local public employees. The final subcategory is called *paid transfers* and includes pension payments for qualifying former municipal employees and payments mandated by legal sentences.

## 2.3 Fiscal Reform

The subnational government structure just described was created through a series of decentralization reforms enacted in the early 1990s. These reforms in fact began in 1988 with the introduction of local elections for mayors. Pivotal in this effort was the drafting of a new constitution in 1991, which was soon complemented by several laws that further developed the decentralized institutional framework. In particular, Law 60 of 1993 established responsibilities across the different levels of government for the public provision of education and health and created the system of intergovernmental transfers that would become SGP.

Endowed with substantial new powers and resources, spending by subnational governments grew dramatically throughout the 1990s. The rapid increase in the size and complexity of local public administrations meant that operating costs soon started to exceed current revenue in many municipalities. Moreover, the combination of high economic growth, a large

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<sup>6</sup>Other earmarks include a share of the gasoline surcharge to be spent on road repairs and transportation projects, and a share of traffic fines used to finance a national information system. There are also several earmarked stamp taxes charged on all public contracts (e.g., elderly, culture).



inflow of transfers, and easy access to credit provided little incentive for the development of local taxation. By the end of the decade, the fiscal outlook for most subnational governments was dire, with a total subnational deficit (municipalities and departments) equal to 0.6% of GDP in 1999, three times larger than in 1990 (MHCP, 2009). Larger cities were highly indebted and were hit hard by rising interest rates and reduced access to credit amid a large recession in 1999-2000. In smaller municipalities the deficit was reflected in long delays in the payment of salaries, pensions, and other obligations, which led to a rising number of lawsuits and legal rulings against the municipal governments (El Tiempo, 1998, 1999).

To address the growing fiscal imbalance affecting subnational governments, the national government introduced several additional reforms. The focus of our analysis is Law 617 of 2000, which specified that the operating costs of municipal governments can only be financed with disposable current revenue (i.e., not SGP transfers or natural resource royalties) and set a cap on operating expenses as a share of disposable current revenue.<sup>7</sup> This cap ranges from 50% to 80% depending on the municipal category, with municipalities in the upper categories (i.e., larger/richer) facing a more stringent one. To facilitate compliance, the law (i) eliminated the requirement set by previous legislation for the existence of certain dependencies within municipal governments (e.g., environmental protection office), (ii) eliminated the office of the municipal comptroller in smaller municipalities, (iii) set limits on the operating costs of the municipal council and the offices of the personero and the comptroller (if it exists), and (iv) set more stringent requirements for the creation of new municipalities. Municipalities were also granted a four-year transition period (2001-2004) for compliance, with the cap on operating costs becoming more stringent over time.<sup>8</sup>

The Comptroller General (Contraloría General de la República, CGR), the country's fiscal watchdog, was charged with producing yearly reports on compliance with the fiscal rule. Municipalities found to be non-compliant face several consequences. First, the municipality is barred from conducting any credit operations and loses access to financial support from the national government, including co-financing for investment projects and guarantees on credit operations, unless it enters a financial restructuring program. Second, non-compliance is considered a serious disciplinary offence, which can be punished by the Inspector General's office (PGN). Finally, non-compliant municipalities are reclassified one category down (if possible), which negatively affects the remuneration of all local public officials.

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<sup>7</sup>Law 358 of 1997 regulated credit operations by subnational governments and established conditions under which approval from the central government is required. Law 550 of 1999 facilitated the restructuring of liabilities for municipalities that declare bankruptcy. Law 715 of 2001 modified the formula used to allocate transfers and reassigned responsibilities across levels of government in the areas of education and health. As part of our robustness checks below, we consider potential confounding effects of these reforms.

<sup>8</sup>The cap was set at 0.95 in 2001, 0.90 in 2002, 0.85 in 2003 for municipalities in category six.

## 3 Empirical Strategy

### 3.1 Data

The National Department for Planning (DNP) publishes yearly balance sheets for all municipal governments. This administrative dataset is available for 1993-2018 and includes information on revenue and spending, each disaggregated into current and capital accounts. Revenue sub-accounts include tax revenue (property, gross receipts, other), non-tax revenue, transfers (current and capital), natural resource royalties, and co-financing. Current spending includes operating costs and debt interest payments, with the former being disaggregated into personnel expenses, general expenses and paid transfers, as discussed above. The balance sheets include measures of current and total surplus or deficit, as well as information on credit inflows and outflows (principal repayments) and changes in wealth. DNP also provides disaggregate data on SGP transfers since 1994. We use the data from DNP to construct our measure of exposure to the fiscal rule (which we discuss below) and our fiscal outcomes of interest. We express all monetary values in 2010 Colombian Pesos (COP).

The office of the Comptroller General (*Contraloría General de la República, CGR*) provides information on the category of each municipality since 2002. We use this information to implement our sample inclusion criteria. CGR also provides information on compliance with the fiscal rule for the years 2010-2018. This includes disaggregate information on disposable current revenues and operating costs by municipality. We use this information to validate our measure of exposure to the fiscal rule and to provide granular evidence on compliance. Since 1995, CGR also publishes a quarterly bulletin with the list of individuals and organizations facing sanctions related to the misuse of public funds, including the municipality of occurrence. We use this information to construct measures of corruption.

The *Centro de Estudios sobre Desarrollo Económico (CEDE)*, a research center at Universidad de los Andes, provides data on local electoral outcomes, based on records from the National Civil Registry. Our sample period includes elections for mayor in 1990, 1992, 1994, 1997, 2000, 2003, 2007, 2011, and 2015. Data on the mayoral elections until 1994 only includes the name and the party of the winning candidate, but after that year we observe the respective vote shares of all candidates. CEDE also provides information on local public goods, including educational enrollment and number of teachers.

Additional information on public goods and development outcomes comes from various sources. The Ministry of Health provides data on coverage of poor population with subsidized health insurance. We use microdata from the vital statistics made available by the National Department of Statistics to construct several health outcomes, including the average number of pre-natal checks and the share of newborn with low birth weight. The

National Geographical Institute (IGAC) provides information on cadastral updates. As a broad proxy for economic activity, we construct a measure of night-time luminosity based on data from the US Air Force’s Defense Meteorological Satellite Program (DMSP).

Finally, we use proprietary data on social mobilizations provided by *Centro de Investigación y Educación Popular (CINEP)*. This is an event-based dataset on protests, strikes and riots that covers the period 1995-2015. For each event, the data includes the municipality of occurrence, the main actors, the motivation for the protest, and the type of event.

Online Appendix A provides information on variable definitions, sample availability and sources for all variables in the paper. Appendix Table A1 provides summary statistics.

### 3.2 Research Design

To study the economic and political effects of the subnational fiscal rule in Colombia, we use a difference-in-differences research design. Our design compares the change in our outcomes of interest before and after the introduction of the fiscal rule in 2000, between municipalities with varying exposure to it. We base our measure of exposure on the average value of the overspending indicator (i.e., operating costs/disposable current revenue) in the years before the reform. Intuitively, we leverage the fact that while the fiscal rule applies *de jure* to all municipalities, *de facto* it represents a sudden shock only for those that were spending above the limit in the pre-reform period, as only these municipalities had to adjust their finances after 2000 in order to comply with the rule.

The lack of fully disaggregate data on municipal public finance for our period of interest complicates slightly the implementation of this research design. In particular, the data on current revenue that is available does not allow us to distinguish between earmarked revenues (e.g., share of property tax earmarked for environmental agency) and the remaining disposable current revenues that should go into the calculation of the overspending indicator. Similarly, the data on operating costs does not distinguish between these earmarked expenditures (i.e., the resources transferred to the environmental agency) and the remaining expenses, only the latter of which should count for the overspending indicator. Fortunately, the measurement error in the numerator and the denominator of the overspending indicator calculated with the available data go in the same direction and should roughly cancel out.

Hence, we construct the overspending indicator by dividing (total) operating costs by (total, not disposable) current revenue, based on the data in the municipal balance sheets provided by DNP.<sup>9</sup> As mentioned above, data on the actual values for net operating costs

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<sup>9</sup>Operating costs are equal to the sum of personnel expenses, general expenses and paid transfers. Current revenue is equal to tax revenue, non-tax revenue (fines and fees) and current transfers. Our preferred measure of transfers replaces this last line item in the DNP balance sheets with the figure for SGP transfers listed

and disposable current revenue used by CGR to determine compliance with the fiscal rule is available for the period 2010-2018. We use this information to validate our proxy for the overspending indicator by comparing the actual and estimated values for the years for which both measures are available. We find that our proxy for the overspending indicator aligns well with the estimate by CGR. The average value of our estimated indicator is 0.63, while the average of the indicator produced by CGR is 0.59, meaning that on average we overestimate operating expenses as a share of disposable current revenue by 4 percentage points (the median discrepancy is 4.6 pp). Appendix Figure B1 shows the distribution of the overspending indicators from both sources for each year, which largely overlap.

Our preferred measure of exposure to the fiscal rule is an indicator equal to one if the average value of the overspending indicator between 1996 and 2000 (i.e., last five years before the start of the transition period) takes a value of one or higher. Even though the steady-state cap for the overspending indicator for the municipalities in our sample was 0.8, we opt for a larger baseline cut-off for several reasons. First, while less than 12% of municipalities in the sample met the cap of 0.8 in 2000, about 43% met the cap of 1 at that time, which allows us to have a more balanced composition of the exposed and non-exposed groups. Second, the cap at the start of the transition period in 2001 was 0.95, which is closer to the value we use and may have served as a benchmark for the initial fiscal adjustment. Third, the comparison in the previous paragraph between our estimate of the overspending indicator and the one produced by CGR suggests that we should adjust our measure of compliance with the fiscal rule upwards. We show below that our results are robust to different thresholds for the exposure measure, as well as to the use of the continuous overspending indicator. We also verify that our results are robust to using different (shorter) time periods to construct the exposure measure (all pre-reform), but we prefer the five-year average because it reduces the impact of volatility in the overspending indicator in any one year. In particular, a longer average reduces the potential impact of the recession that hit Colombia in 1999.<sup>10</sup>

The map in Figure B2 shows the geographic distribution of the municipalities that we deem as exposed and not exposed to the fiscal rule at the time of the reform in 2000. There is substantial spatial variation in our exposure measure. Municipalities left blank in the map are those not included in our sample. To ensure that our sample remains as large as possible, but only includes a comparable set of municipalities (i.e., not combine very large cities with

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as free disposal (*libre destinación*). Results are robust to only using data from the DNP balance sheets. To ensure that our results are not driven by extreme outliers, which arguably correspond to reporting errors, we winsorize our measures of operating costs, disposable current revenue and the overspending indicator at the 1% and 99% levels (the latter after calculating with the unadjusted data). We verify below that our results are robust to omitting this winsorization.

<sup>10</sup>We show below that our results are also robust to excluding the years 1999 and 2000 from the construction of the exposure measure or to excluding these years from the sample altogether.

very small towns), we restrict our sample to municipalities in category six, which is the lowest category and encompasses almost 90% of municipalities in the country. This sample inclusion criterion ensures that we only compare municipalities with a common institutional framework, as discussed in section 2.1. In practice, we deal with temporary fluctuations in the categorization by requiring that municipalities in the sample be classified in category six for at least 14 years in the 16-year period between 2003 and 2018. Our final estimation sample includes 844 municipalities (75% of the total). We show below that our results are robust to the exclusion of municipalities that are not in category six at any point in time.

Naturally, those municipalities that we deem as exposed to the fiscal rule are likely to differ from those non-exposed in several other dimensions. For instance, the variation in fiscal discipline before the reform may reflect differences in economic structure, political competition, or state capacity. Table B1 shows results from cross-sectional regressions comparing multiple predetermined characteristics (at the time of the reform) in exposed and non-exposed municipalities. Focusing on the results with department fixed effects in columns 3-4, we find that municipalities exposed to the fiscal rule are located at higher altitude and are farther away from Bogotá. They were more likely to have a school or a branch of the Agricultural Bank in 1996, and were more likely to have presence of paramilitary groups between 1996 and 2000. Mayoral elections in 1997 and 2000 in these municipalities had higher votes shares for the Liberal Party and a higher Herfindahl–Hirschman Index (HHI), indicating greater concentration of votes among a reduced number of candidates.

We address the potential confounding effect of these differences in observable characteristics, as well as unobservable time-invariant differences, by including municipality fixed effects in all our regressions. We also include department by year fixed effects in all regressions, which means that the counterfactual for municipalities exposed to the fiscal rule is always provided by non-exposed municipalities located within the same department. The department-year fixed effects capture the impact of macroeconomic shocks, such as the recession in 1999, and allows it to vary across departments. They also capture the effect of the other fiscal reforms from the late 1990s and early 2000s, which likely differ across departments. These reforms include the reassignment of responsibilities for the provision of education and health across levels of government in the context of the reform to the transfers system in 2001 and the introduction of the respective fiscal rules at the department level in 2000. We provide additional robustness checks below showing that our results are not driven by differential exposure to these additional reforms across municipalities.

Our main econometric specification is as follows:

$$y_{mt} = \alpha_m + \delta_{d(m)t} + \beta(\text{Affected}_m \times \mathbb{1}[t > 2000]_t) + \sum_{\tau \neq 2000} \gamma_\tau (\mathbb{1}[t = \tau]_t' \times X_m) + \varepsilon_{mt} \quad (1)$$

where  $y_{mt}$  is an outcome of interest in municipality  $m$  in year  $t$ , while  $\alpha_m$  and  $\delta_{d(m)t}$  are the municipality and department-year fixed effects. We define  $\text{Affected}_m$  as an indicator taking value one if the average of the overspending indicator in municipality  $m$  during the pre-reform period (1996-2000) was above one. We interact this measure of exposure with an indicator equal to one for all years on or after 2001 ( $\mathbb{1}[t > 2000]_t$ ), which is the year when the fiscal rule came into effect. The coefficient of interest,  $\beta$ , captures the average difference in the outcome between affected and non-affected municipalities after the reform, relative to the difference in the pre-reform period.  $\varepsilon_{mt}$  is an error term that we cluster two-way by municipality and department-year, following Cameron et al. (2011). This clustering structure allows for autocorrelation of the error term within each municipality and for spatial correlation within the same department and year.

Despite the inclusion of municipality and department-year fixed effects in our regression specification, our estimate of  $\beta$  could still be biased due to the presence of time-varying effects of cross-sectional differences across municipalities with varying exposure to the fiscal rule, including those documented in Table B1. In our preferred specification,  $X_m$  is a vector of predetermined characteristics that we interact with year fixed effects and include as additional controls to further address imbalance in covariates. For this purpose, we focus on those covariates that show significant differences in Table B1.<sup>11</sup> As a complementary strategy, we also estimate propensity-score weighted regressions, following Hirano and Imbens (2001).<sup>12</sup>

The identifying assumption for  $\beta$  in equation 1 is that the difference in our outcomes of interest between municipalities deemed as exposed and non-exposed to the fiscal rule would not have changed after 2000 in the absence of the reform, conditional on the set of fixed effects and controls. We provide indirect evidence in support of the parallel trends assumption by estimating the following event study version of the baseline specification, which flexibly tracks the difference in the outcome  $y_{mt}$  at all points in time, relative to the year before the reform came into effect (i.e., the omitted category):

$$y_{mt} = \alpha_m + \delta_{d(m)t} + \sum_{\tau \neq 2000} \beta_{\tau} (\text{Affected}_m \times \mathbb{1}[t = \tau]_t) + \sum_{\tau \neq 2000} \gamma_{\tau} (\mathbb{1}[t = \tau]_t' \times X_m) + \varepsilon_{mt} \quad (2)$$

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<sup>11</sup>We include all covariates for which we find a significant difference between affected and non-affected municipalities in columns 3-4 of Table B1. These include: Altitude, Distance to Bogotá, an indicator for school presence in 1996, an indicator for presence of branch of Agricultural Bank in 1996, an indicator for any paramilitary presence between 1996 and 2000, average vote share for the Liberal party in 1997-2000 mayoral elections, and the HHI for these same elections.

<sup>12</sup>We first estimate a Probit regression of our measure of exposure to the fiscal rule on all available municipal characteristics shown in Table B1. We then re-estimate equation 1 (i) restricting the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and (ii) weighting the control observations by a non-parametric function of the propensity score (Hirano et al., 2003).

If the coefficients  $\beta_\tau$  corresponding to pre-reform years are close to zero, this suggests that the parallel trends assumption is satisfied and that we are not picking up the effect of other changes. Such a finding would also suggest the absence of anticipation effects of the reform, which we expect given the high level of uncertainty regarding the approval of any piece of legislation by Congress. The  $\beta_\tau$  coefficients for the post-reform period also allow us to track the effect of the reform over different time horizons, ranging from the initial transition period in 2001-2004 to the longer term, ten or more years after the fiscal rule came into effect.

## 4 Results: Public Finance

In this section, we provide evidence on the effect of the fiscal rule on the main municipal fiscal outcomes. We focus our attention on the overspending indicator (i.e., the targeted variable) and an indicator for current deficit. We then use disaggregate data on the sub-components of the overspending indicator and other fiscal outcomes to shed light on mechanisms.

### 4.1 Raw Data

Figures 1 and 2 provide preliminary visual evidence on compliance with the fiscal rule. This evidence suggests that the results that follow are not an artifact of the additional structure imposed by the more formal econometric analysis presented below.

Figure 1 plots the distribution of the overspending indicator in various years before the reform, during the transition period, and in the decade that followed (Appendix Figure B3 provides results for other years). Panels (a) and (b) show that 77% of municipalities had operating expenses that exceeded 80% of disposable current revenue (i.e., overspending indicators above the cap of 0.8) before the introduction of the fiscal rule. By 2002, shortly after the reform, panel (c) shows that the distribution starts to compress and shifts to the left, with 63% of municipalities exceeding the cap. Panel (d) shows that compliance rapidly increased after the end of the transition period, with only 26% of municipalities breaking the fiscal rule in 2005. Panels (e)-(h) show that compliance further increased and remained high for the rest of the sample period. For instance, in 2017 only 5% of municipalities had overspending indicators that exceeded the legal cap of 0.8.

Figure 2 plots the average value of the overspending indicator among affected and non-affected municipalities for each year between 1996 and 2018. Before the fiscal rule came into effect in 2001, municipalities exposed to the fiscal rule were spending approximately 130% of yearly disposable current revenue in operating expenses, while non-exposed municipalities were averaging 85%, only slightly above the cap of 80%. This suggests that local governments

in affected municipalities were excessively large and could not be paid for with disposable current revenue. After the fiscal rule is introduced, the average of the overspending indicator declines dramatically for the affected group and by the end of the transition period in 2004 it has largely converged to the average for the non-affected group. The average municipality in both groups seemingly complies with the fiscal rule in all years between 2004 and 2012, with the overspending indicator declining to a common average of approximately 60% in 2018.

Appendix Figure B4 plots the distribution of the overspending indicator based on the granular CGR estimate, pooling data for the period 2010-2018 (Figure B5 provides disaggregate results for each year). The graph shows a clear discontinuity in the distribution at the 80% legal threshold, which we confirm using the test by Cattaneo et al. (2020). This pattern indicates that municipalities actively work to comply with the fiscal rule.<sup>13</sup>

## 4.2 Estimation Results

Figure 3 shows point estimates and 95% confidence intervals for  $\beta_\tau$  in equation 2, using the overspending indicator as dependent variable in panel (a) and an indicator for current deficit in panel (b). Panel (a) shows that the indicator increases slightly in exposed municipalities in the years immediately before the reform, but decreases sharply shortly after the introduction of the fiscal rule, in line with the evidence in Figure 2. The graph suggests a stable decrease in operating expenses as a share of disposable current revenues of as many as 40 percentage points as a result of the fiscal rule. Panel (b) shows that the probability that affected municipalities had a current deficit remained stable in the years before the reform, but decreased sharply in the years after. The plot suggests that the fiscal rule led to a long-run reduction in the probability of a current deficit that is also approximately equal to 40 pp.

Table 1 provides estimates of equation 1 for these outcomes and presents additional robustness checks. The dependent variable in columns 1-2 is the overspending indicator, while in columns 3-4 it is the dummy for current deficit. Columns 1 and 3 correspond to the baseline specification with municipality and department-year fixed effects. Columns 2 and 4 include the predetermined municipal characteristics interacted with year fixed effects as additional controls to address imbalance in covariates. Column 1 shows that the fiscal rule led to an average reduction of 32 pp in the overspending indicator. This effect is very precisely estimated (statistically significant at the 1% level) and is equivalent to 30% of the sample average. The results hardly change in column 2 with the additional controls.

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<sup>13</sup>This pattern is unlikely to reflect manipulation of the reported data because the municipal governments in our setting can wilfully reduce spending near the threshold to achieve compliance (i.e., unlike Fisman and Wang, 2017). In particular, municipalities are required by the law to adjust their operating expenses during the year if actual revenue falls below expectations. This would lead local governments to forgo expenses late in the year until they bring the overspending indicator down to the legal limit.



Column 3 shows that the probability of a current deficit decreased by 33 pp on average after the introduction of the fiscal rule. This effect is also very precisely estimated and is quite sizable, equivalent to 50% of the sample mean. It is also robust to the inclusion of additional controls in column 4. Appendix Table C2 shows that the results for both outcomes are likewise unaffected if we run our regressions with propensity-score weighting.

We provide a large battery of additional robustness tests. The results of these tests are available in the online Appendix. Regarding our discrete measure of exposure to the fiscal rule, Figure B6 shows that the results remain of a similar magnitude and precision for any threshold value for the overspending indicator between 0.8 and 1.1. Table B2 shows that the results are also unaffected if we change the set of pre-reform years used to construct our exposure measure or if we use the continuous measure instead (i.e., 1996-2000 average of overspending indicator). Table B3 further shows that the results look very similar if we normalize by population (i.e., per capita values), while Table B4 verifies that the results are unaffected if we do not winsorize our main fiscal variables of interest. Regarding the composition of the sample, Table B5 shows that the results look very similar if we exclude municipalities that are not classified in category six at any point between 2003 and 2018. To verify that our results are not driven by events in one specific region, Figure B7 shows that the results hardly change if we exclude any one department. Table B6 shows that the results are unaffected if we exclude the years 1999 and 2000 from the sample, when a sharp recession hit the country.

Table B7 verifies the robustness of the results to potential effects of other reforms taking place near to the introduction of the fiscal rule. Regarding the 2001 reform to the system of intergovernmental transfers, we verify that our results are robust to controlling for the yearly amount of SGP transfers or for municipalities that become *certified* to directly manage their education share of SGP. Regarding the 1999 reform of bankruptcy law for subnational governments, we show that our results are unaffected if we control for those municipalities that signed a financial restructuring agreement with the Ministry of Finance.<sup>14</sup> Finally, we verify that our results are not driven by the more stringent requirements for the creation of new municipalities also put in place by Law 617/2000. In this regard, our results are unchanged if we exclude from the sample all the new municipalities created since 1986.

### 4.3 Components of Fiscal Adjustment

The previous results suggest that the fiscal rule was highly effective at improving fiscal discipline by municipal governments in Colombia. We turn now to the sub-components of

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<sup>14</sup>Table B8 shows that the fiscal rule had a larger impact on the overspending indicator in municipalities that subscribe a financial restructuring agreement, driven by a larger reduction in operating expenses.

the overspending indicator to shed light on the mechanisms driving the fiscal adjustment. Table 2 shows the results. The dependent variables in columns 1-4 are the logarithm of operating expenses and its sub-components: personnel expenses, general expenses, and paid transfers. In columns 5-8, the dependent variables are disposable current revenue and its respective sub-components: tax revenue, non-tax revenue and the disposable share of SGP transfers. Figure 4 shows the corresponding event-study plots. For this analysis, we focus on our preferred specification with additional controls.

Column 1 shows that the fiscal rule led to an average decrease of approximately 21% in the operating costs of affected municipal governments. Columns 2-4 show that all sub-components contributed to this reduction in operating expenses. The largest fall is observed in general expenses, which decreased 25% on average, while both payroll and paid transfers fell by 16%.<sup>15</sup> The general expenses component includes procurement of goods and services, training, travel, rent, maintenance and utilities. Arguably, this component is more flexible and more easily adjusted than the salaries of public employees, the pensions of former public servants or payments dictated by legal sentences (the latter two are the main components of paid transfers in column 4). The event study graphs in panels (a)-(d) of Figure 4 provide evidence of a reduction in operating expenses and all its sub-components after the introduction of the fiscal rule in 2000, particularly personnel and general expenses.

Column 5 shows that disposable current revenue increased by roughly 7% on average in affected municipalities after the introduction of the fiscal rule. This increase in revenue is mostly driven by non-tax revenue (i.e., fines and fees), which increase by 29%. Local tax revenue and disposable transfers from the central government increase at much lower rates, 12% and 8% respectively.<sup>16</sup> Panel (g) in Figure 4 shows a clear increase in non-tax revenue after the reform, while the evidence for tax revenue and transfers in panels (f) and (h) is less conclusive. In particular, tax revenue is on a downward trajectory in the years before the reform, arguably due to weak incentives for the generation of own revenue and the 1999 economic crisis, but recovers to its level from the mid-1990s after the reform.

The previous results indicate that the effect of the fiscal rule on operating expenses (the numerator in the overspending indicator), was three times as large as that on dispos-

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<sup>15</sup>Unfortunately, data on the number of municipal employees is largely unavailable during our sample period. Using information for 2010-11 (i.e., one decade after the reform), Appendix Figure B8 shows that the average total number of employees in both affected and non-affected municipalities is close to 13. The average number of employees with a college degree is approximately 8, while roughly 5 employees serve at the discretion of the mayor. There are no statistically significant differences in these averages across the two groups, suggesting that the fiscal rule led to a convergence in the size of municipal governments.

<sup>16</sup>Appendix Table B9 and Figure B9 show that the increase in tax revenue is not driven by the property or gross receipts taxes (i.e., the main local taxes). Other tax revenue includes the gasoline surcharge, vehicle registration fees, and taxes for street lighting, animal slaughter, construction licences, obstruction of roads, public spectacles, or billboards. It also includes tax arrears (irrespective of tax).

able current revenue (the denominator). This result stands in contrast to the findings by Grembi et al. (2016), who show that a fiscal rule in Italy affects municipal public finance exclusively via taxation, with no change in public spending. One possible explanation for this discrepancy is that voters in Latin America prefer spending-based fiscal adjustments to taxation-based ones, as shown by Ardanaz et al. (2020). The larger observed increase in non-tax revenue than in tax revenue in our setting lends support to this interpretation. A complementary explanation is that weak state capacity hinders efforts by local governments in developing countries to raise additional revenue (Besley and Persson, 2011).

A frequent concern regarding fiscal rules is the possibility that governments artificially comply using creative accounting practices (Alesina and Perotti, 1996; Milesi-Ferretti, 2004). In our setting, local governments could strategically classify some of their operating expenses as investment in order to bring down the overspending indicator. We look into this possibility in Table 3, with the corresponding event study graphs in Figure 5. The dependent variable in column 1 is log capital revenue, which includes SGP transfers, co-financing, and natural resource royalties. As expected, the estimated  $\beta$  is very small (1% increase in affected municipalities after the reform) and not statistically significant.<sup>17</sup> The estimate for log capital expenses (i.e., investment) in column 2 is also small (roughly 4% increase) and statistically insignificant. Hence, it is not the case that the large documented reduction in operating expenses is offset by an increase in capital expenses. Panels (a) and (b) in Figure 5 confirm the absence of an effect on capital revenue or spending.

The dependent variable in column 3 is an indicator for a total deficit (current plus capital accounts), the likelihood of which decreases by 11 pp in affected municipalities after the reform. Panel (c) in Figure 5 provides clear visual evidence of a reduction in the probability of a total deficit after 2000. This is a large effect, equivalent to 20% of the sample mean, and constitutes further proof of a real impact of the fiscal rule, as the total deficit is immune to the reshuffling of expenses across accounting categories.<sup>18</sup> Columns 4-6 show that affected municipalities use the resulting fiscal surplus to accumulate wealth rather than repay debt. In particular, columns 4-5 show that the fiscal rule has a negligible impact on the probability of positive net credit inflows (or outflows), or in log interest payments. Column 6 shows that affected municipalities experience a 12 pp increase in the probability of a positive change in wealth (equivalent to 25% of the sample mean). Panel (f) in Figure 5 confirms this result.

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<sup>17</sup>Appendix Table B10 further shows that SGP transfers do not change differentially in affected municipalities.

<sup>18</sup>The long post-reform period in our sample allows us to further rule out that creative accounting is taking place through the intertemporal reallocation of expenditure. The relatively simple institutional structure of the municipalities in our sample also limits governments' ability to shift liabilities to off-budget entities.

## 5 Results: Local Politics

The previous results indicate that the introduction of the fiscal rule led to a sizable reduction in operating expenses and a moderate increase in disposable current revenue among affected municipalities. This shows that fiscal rules can be effective at curbing overspending by local governments in developing countries. In this section, we investigate the effects of the fiscal rule on local political outcomes. As mentioned above, the previous literature has mostly focused on whether austerity measures cause an immediate political backlash and has provided very mixed findings. In our setting, the fiscal adjustment may have reduced support for local incumbents if public good provision was negatively affected, or if operating expenses were partially used to benefit powerful local interest groups. Alternatively, support for the incumbent may have increased if the improved health of municipal public finance allowed the local government to pay salaries, pensions and other obligations in a more timely fashion, or if the electorate rewarded a cut in what it perceived as wasteful spending.

Our sample period covers four subnational elections after the reform, which allows us to go beyond the immediate political impact of the fiscal rule and study potentially more persistent effects on local political outcomes over a long time horizon. In particular, we would like to know whether the fiscal rule helps to alleviate problems of political agency affecting the local government. These issues are highly likely in our setting, given that individual politicians such as the mayor have very weak incentives for good governance, insofar as parties are weak and fail to provide adequate oversight (Mainwaring, 2018). Moreover, the one-term limit for mayors reduces the disciplining effect of elections (Ashworth, 2012).

In Table 4, we provide estimates of equation 1 for two measures of electoral support for the incumbent party in the following election. Odd-numbered columns correspond to our basic specification with municipality and department-year fixed effects (in this case, election years), while even-numbered columns correspond to our preferred specification with additional controls for predetermined characteristics interacted with year fixed effects. The dependent variable in columns 1-2 is the vote share for the incumbent party in the mayoral election. Unfortunately, data on vote shares for all parties competing in the mayoral election is only available since 1997, which leaves us with a short sample period before the reform (1997 and 2000 elections). Still, we find that incumbent parties experience a 6 pp increase in the vote share for their candidate for mayor, which is equivalent to 13% of the sample mean. This suggests that local voters become increasingly satisfied with the performance of the incumbent government after the introduction of the fiscal rule. The event-study plot in panel (a) of Figure 6 shows a persistent increase in the incumbent's vote share in all elections after the introduction of the fiscal rule.

Elections for mayor usually involve more than two candidates (average of 4.1) and the winner is determined using plurality rule. Hence, a higher vote share does not necessarily translate into a greater probability of the incumbent party remaining in office. To examine this possibility, we use as dependent variable in columns 3-4 an indicator equal to one if the party of the incumbent mayor wins the following mayoral race (i.e., re-election at the party level). The results show that the probability of re-election increases by 5 pp in affected municipalities after the introduction of the fiscal rule. This effect is economically meaningful (equivalent to 9% of the sample mean), but it is imprecise and only statistically significant at the 10% level in our preferred specification with additional controls. However, it still suggests a slight increase in local political support for the incumbent party and certainly rules out the possibility that the fiscal adjustment caused a political backlash. Panel (b) in Figure 6 shows that the relative re-election rate in affected municipalities is stable and close to zero in the four elections before the reform, but becomes positive in all subsequent elections, particularly the first one in 2003. If anything, this suggests that the incumbent party at the time of the introduction of the fiscal rule in 2001 benefited disproportionately.

We subject the previous results to several robustness checks. Appendix Figures B6 and B7 shows that the results hardly change if we modify the threshold used to define exposure to the fiscal rule or if we exclude any one department from the sample. Appendix Table C2 further shows that the estimates are very similar if we use propensity-score weighting to address imbalance in covariates, though the effect of the fiscal rule on the probability of re-election for the incumbent party loses precision and becomes statistically insignificant.

We study other characteristics of mayoral elections in Appendix Table B11 and find no evidence of change after the introduction of the fiscal rule. These characteristics include the number of parties competing in the election and several measures of competitiveness. Appendix Table B12 then looks at party vote shares, focusing on the two main parties (Liberal and Conservative) that we are able to consistently observe throughout the sample period. We find that the Conservative vote share increases 3 pp in affected municipalities after the reform (11% over sample mean), which could reflect voters rewarding the incumbent party at the national level at the time of the reform for its introduction. However, we show in the same table that our main political results on incumbent parties are robust to including party fixed effects as additional controls, thereby ensuring that we are not picking up changes in party affiliation.

As a complementary measure of political behavior, we consider the incidence of protests against the municipal government in Table 5. In our setting, protests may reveal patterns of political opposition that are confounded in the electoral data due to countervailing political strategies such as clientelism or vote-buying. Our analysis of protests relies on a granular

event-based dataset provided by a Colombian think-tank called CINEP. We focus on protests against the municipal government and aggregate the data to the municipality-year level. The possibility of disaggregating protests by cause further allows us to shed light on the aspects of local governance that are potentially changing due to the fiscal rule.

The dependent variable in columns 1-2 is an indicator equal to one if local residents stage any protest against the municipal government on that year. Column 1 shows results from the basic specification, while column 2 includes the additional controls. Results show no effect of the fiscal rule on the incidence of protests against the municipal government. If anything, we see a negative but noisy point estimate in both columns. Panel (a) of Figure 7 shows the event study plot for this result. In columns 3-5, we analyze protests disaggregated into three main causes: local public services (column 3), labor disputes and breach of agreements by the municipal government (column 4), and other causes (e.g., human rights violations, column 5). We find a statistically significant decrease of 0.7 pp in the probability of protests related to labor disputes and breach of agreements (equivalent to 140% of the sample mean), but no change in the probability of protests related to local public services. These results are in line with anecdotal evidence suggesting that affected municipalities often incurred in long delays in the payment of salaries and other obligations before the reform (El Tiempo, 1998, 1999). Relatedly, when we disaggregate by type of event in columns 6-8, we find a similar decrease in the probability of strikes against the municipal government (column 7).<sup>19</sup>

Taken together, the previous results suggest that voters and municipal employees became more satisfied with the local government after the introduction of the fiscal rule. While contrary to the conventional wisdom on the political costs of austerity, these results are in line with recent cross-country evidence showing no electoral penalty of austerity (Arias and Stasavage, 2019) and with recent survey evidence showing that voters are less averse to austerity than typically assumed (Bansak et al., 2021). Arguably contributing to our political results is our previous finding that municipalities disproportionately respond to the fiscal rule by reducing operating expenses rather than by raising more revenue, which is the type of austerity package that voters express support for in surveys, especially if reductions in expenditures imply cuts in public sector wages (Ardanaz et al., 2020; Bansak et al., 2021).

Importantly, these political effects were not limited to the administration or the party in power at the time of the reform, but persisted through several electoral cycles. This suggests that the fiscal rule helped to align the policies of the municipal governments with the preferences of their constituents, thereby alleviating problems of political agency. A misalignment between incumbents' policies and voters preferences is very likely in our setting: mayors face a one-term limit in Colombian municipalities, which prevents them from internalizing the

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<sup>19</sup>Appendix Figure B10 provides event studies for these other protest outcomes.

costs that their policies impose on the next mayoral candidate from the same party (Klašnja and Titiunik, 2017).<sup>20</sup> If mayors receive a private benefit from public overspending (while voters don't), spending under a one-term limit will exceed the amount preferred by voters (and by the incumbent party that does care about winning the next election). Therefore, in a setting with weak parties, fiscal rules can act as a means for party discipline, aligning incumbents with voters and with their own party.

To illustrate the functioning of this mechanism, we introduce a simple formal model in Appendix D. This is a two-period model with an election in-between. In the model, individual politicians face a one-term limit, but parties can be re-elected. Each period, the mayor observes available revenue and makes a decision on public spending. At the end of the first period, a representative voter observes the combination of public revenue and spending and decides whether to re-elect the incumbent party. There are two types of politicians, congruent and dissonant, and we assume that different parties have varying shares of the two types. These shares are unknown to the voter, but she can update her beliefs by observing the performance of the incumbent. Dissonant politicians prefer high spending irrespective of available revenue, while congruent politicians prefer high spending only when revenue is high, as does the voter. In the absence of the fiscal rule, the dissonant politician incurs in a fiscal deficit, which reveals his type and leads the voter to update negatively on the incumbent party, potentially voting for the other one. The introduction of the fiscal rule leads to a pooling equilibrium in which both types of candidate spend in accordance with available resources. This prevents learning, but also eliminates the agency problem (i.e., no undesired fiscal deficit), thereby reducing the incentive to vote the incumbent out of office.

## 6 Results: Public Goods

The previous results indicate that voters in Colombia rewarded incumbent parties following a sizable reduction in the operating costs of the municipal government. This suggests that local public services were not affected, despite the large cuts to public spending. In this section, we explore this mechanism and provide evidence on the potential impact of the fiscal rule on public good provision and living standards. This analysis also allows us to shed light on the broader welfare effects of fiscal rules. Our previous findings showing that capital revenue and spending were not affected suggest a null effect on local public goods. However, the reduction in operating expenses may have affected the *quality* of public spending (e.g.,

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<sup>20</sup>Appendix Figure B12 shows the share of former mayors that run for office (mayor or council) again and the share that are elected. On average, 17% of former mayors run again in the first election that they are eligible, but only 7% win. These numbers become smaller in subsequent cycles. Appendix Figure B13 provides disaggregate results by exposure to the fiscal rule, with no discernible difference.

weaker oversight) or other aspects of governance not captured by the fiscal outcomes.

Table 6 provides estimates of  $\beta$  in equation 1 for various relevant outcomes. The corresponding event studies are available in Appendix Figure B11. Since subnational governments are responsible for the provision of services in the areas of education and health, we start by analyzing several indicators in these areas. In the case of health, we focus on coverage of the poor population with subsidized health insurance and indicators of maternal-child health. In the case of education, we examine potential changes in the number of schools, teachers and students. Even though most related expenses in these areas are funded with SGP transfers, and these are managed by the department-level secretaries of education and health, municipal governments do have some discretion over non-pecuniary aspects of policy and can also use their own resources for complementary expenses.

The dependent variable in column 1 is the number of people enrolled in the government's subsidized health insurance (regimen subsidiado), expressed as a share of the number of people with Unmet Basic Needs (UBN) in the 1993 census. Local management of this program (using SGP transfers) is the municipal government's main responsibility concerning health, since healthcare provision is mostly managed at the department level. The results in column 1 suggest a 1 pp decrease in enrollment in affected municipalities after the introduction of the fiscal rule (equivalent to 1.2% of sample mean), which is not statistically different from zero. In columns 2-3, we use information from the vital statistics to construct two measures of maternal-child health. This is an important component of public health, which is the other major responsibility of municipalities in the area of health. The dependent variable in column 2 is the share of newborn with low birth-weight, which is an important predictor of cognitive and labor market outcomes (Black et al., 2007). In column 3, the dependent variable is the number of prenatal visits reported by the mother. We find no significant effects of the fiscal rule on these outcomes either. Moreover, both estimates are economically negligible, corresponding to 4% and 0.5% of the respective sample means.

We turn to measures of education in columns 4-6. The dependent variable in column 4 is the number of public schools in the municipality per 10,000 inhabitants. SGP transfers provide funding for most current expenses in education, but municipalities can use their own resources for investments in educational infrastructure. However, column 4 shows that the reduction in operating expenses caused by the fiscal rule had no impact on the number of schools. Municipal governments can also reallocate teachers and students across schools (though hiring and promotions are the responsibility of the department) and are responsible for managing SGP transfers for materials, school maintenance, food and transportation. The results in columns 5 and 6 show no changes in the log number of students (primary and early secondary) or in the teacher-pupil ratio, in public schools. All the point estimates in columns



4-6 represent less than a 1% change relative to the sample mean and they are all statistically insignificant.

Having found no evidence of impact on public services in education and health, we turn to other policies and complementary measures of governance. The dependent variable in column 7 is an indicator equal to one if the municipality had a cadastral update on that year. These updates are performed by the National Geography Institute (IGAC) and involve a reassessment of the value of all properties in the municipality (urban, rural or both depending on the scope). The update decision is jointly determined by IGAC and the municipality, and depends on factors such as the number of years since the previous update, and the availability of funding and geographical inputs (Martinez, 2020). This is an interesting policy to study in our setting because the cadastral value is the base for the property tax. Hence, municipalities affected by the fiscal rule may have become more willing to update the municipal cadastre as a way to increase their disposable current revenue. However, cadastral updates are highly unpopular and could cause a political backlash, so the effect of the fiscal rule is theoretically ambiguous. The estimates in column 7 ultimately show a negligible change in the probability of having an update in affected municipalities after the introduction of the fiscal rule. This is consistent with the findings above showing that the reduction in the overspending indicator is mostly driven by cuts to operating expenses and that the increase in current revenue is mostly driven by non-tax revenue.

Columns 8 and 9 provide evidence on broader measures of the quality of governance and living conditions. The dependent variable in column 8 is an indicator equal to one if the mayor has been sanctioned for mishandling of public funds. We construct this variable by matching the names of the mayors in the sample with those of all individuals sanctioned by CGR since 1990.<sup>21</sup> The smaller sample size is due to the fact that the unit of observation is municipality-mayoral term. The effect of the fiscal rule on corruption is theoretically unclear. On the one hand, the reduction in operating expenses may lead to a decrease in corruption if the forgone expenses were not well justified. For instance, if part of the expenditures cut by the reform would have been used for patronage or nepotism. On the other hand, the reduction in operating costs may lead to more corruption if the spending cuts weakened oversight of public service delivery or public contracting. If anything, the results in column 8 point to a slight decrease in the probability of having a sanctioned mayor, though the effect

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<sup>21</sup>The data on corruption comes from quarterly bulletins published by CGR with the list of offenders. We merge this data with the list of mayors by name (i.e., fuzzy merge) and set a cut-off of 0.9 for the precision of the match. We verify in Appendix Table B13 that the results are robust to different thresholds. In our baseline analysis we ignore the time variation in the corruption data and focus on whether the mayor ever appears in the CGR bulletins, but in Table B14 we show that the results are similar for sanctions occurring before or after the mayor's term in office (i.e., adverse selection vs moral hazard).

is small and not statistically significant.<sup>22</sup> Overall, the evidence suggests that the incidence of corruption in affected municipalities did not change after the introduction of the fiscal rule. This result stands in contrast to the findings by Daniele and Giommoni (2020) for Italy. The difference arguably stems from the fact that the fiscal rule in Italy predominantly affected public investment, which is more prone to corruption than operating expenses.

Finally, in column 9 we study a broad measure of economic activity, with the aim of detecting potential effects of the fiscal rule on the functioning of the local economy that are not picked up by the previous outcomes on public good provision. Unfortunately, data on GDP is not available at the municipality level in Colombia. However, nighttime lights (NTL) provide a useful proxy for local economic activity at a granular level throughout our sample period (e.g., Henderson et al., 2012). The dependent variable in column 9 is the logarithm of NTL, which we calculate using data from the US Air Force’s Defense Meteorological Satellite Program (DMSP). The estimate for  $\beta$  is very close to zero and not statistically significant, suggesting that the introduction of the fiscal rule and the resulting decrease in public spending did not affect local economic activity.

Overall, the results in this section strongly suggest that the large reduction in operating expenses following the introduction of the fiscal rule did not lead to a deterioration of local public goods provision or to a worsening of living standards in the affected municipalities. The null effect on local public goods is the likely explanation for the divergence between our political results and those from the only other within-country study on the electoral costs of austerity that we are aware of: Fetzer (2019) documents an increase in political discontent and more support for the opposition party UKIP in areas more exposed to a series of austerity measures introduced in the UK after 2010. The key difference between the large fiscal adjustment in Fetzer (2019) and the Colombian reform studied here is that the UK austerity policy translated into substantial welfare cuts averaging around 24% lower spending per person, and reaching highs of 64% lower welfare spending, while we do not document any cut in welfare nor lower public goods provision.

## 7 Conclusion

In this paper, we study the introduction of a subnational fiscal rule in Colombia in 2000. This *golden* rule set a cap on the operating expenses of municipal governments as a share of disposable current revenue. We study the effects of the fiscal rule on fiscal, political, and economic outcomes by comparing municipalities with varying exposure to the rule at

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<sup>22</sup>Appendix Table B14 provides similar null results for corruption sanctions involving the municipality more generally (i.e., place of occurrence), the party of the incumbent mayor, or any mayoral candidate.

the time of the reform over a long time horizon. Our difference-in-differences analysis yields three main findings. First, the fiscal rule was highly effective at reducing operating costs and the probability of a current deficit. Importantly, we find little evidence of a strategic response through creative accounting practices. Second, the introduction of the fiscal rule led to an increase in electoral support for the party of the incumbent mayor over multiple cycles, as well as to a reduction in the incidence of protests against the municipal government. Third, despite the reduction in public spending and in line with the absence of a political backlash, we find no evidence of change in local public goods or living standards, as proxied by several indicators in the areas of education and health, nighttime lights or sanctions for corruption. These results suggest that the fiscal rule helped to alleviate problems of political agency affecting local governments in a setting with weak parties and limited career concerns for local politicians.

Our findings provide valuable policy lessons for other settings in the developing world. Colombia was not alone in embracing decentralization in the 1990s as a way to improve local governance, nor in struggling to balance subnational public finances in the following years (Gadenne and Singhal, 2014). In this regard, a report in 2018 by the IDB found that many countries in Latin America and the Caribbean feature growing shares of current spending over the past several decades, with the compensation of public employees at the subnational level being an important contributor (Izquierdo et al., 2018). Our findings shed light on the challenges for fiscal and political decentralization in the presence of weak incentives for the generation of own revenues and low levels of political accountability. In particular, our results suggest that institutional arrangements like the golden fiscal rule that we study can help to improve the health of subnational public finances without sacrificing on the quality of local public services. Additionally, these institutional arrangements can help to overcome issues of political agency at the local level, rather than causing a political backlash.

When thinking of extrapolating our findings to other settings, a natural question arises concerning the factors that contributed to the success of Colombia's subnational fiscal rule. We conjecture that the timely and transparent reporting on compliance, together with the existence of credible enforcement mechanisms, were key factors. Colombia's fiscal rule stands out because compliance can be easily verified using information that the municipal governments routinely produce. It also stands out because of its multidimensional approach to enforcement, involving the fiscal and disciplinary watchdog agencies (CGR and PGN), as well as the central government through the Ministry of Finance. The lack of political leverage of the affected subnational governments relative to the national agencies overseeing compliance was arguably crucial. In contrast to the Colombian experience, Rodden et al. (2003) attribute substantial responsibility for the failure of policies aimed at curbing overspending

by state governments in Brazil to the large influence of regional political elites over the national legislative assembly. Similarly, the unpunished violation of the EU's deficit ceiling by Germany and France in 2003 was plausibly the result of the large political clout held by these countries (Yared, 2019).

While our findings suggest that the specific fiscal rule imposed on municipal governments in Colombia was highly successful, we cannot speak to the broader question about the optimal design of a fiscal rule. This question has received attention in the theoretical literature, mostly from a macroeconomic perspective (e.g., Azzimonti et al., 2016; Halac and Yared, 2018). Future empirical work can hopefully shed light on this subject, either by comparing the effects of different rules or by studying more complex rules with varying features.

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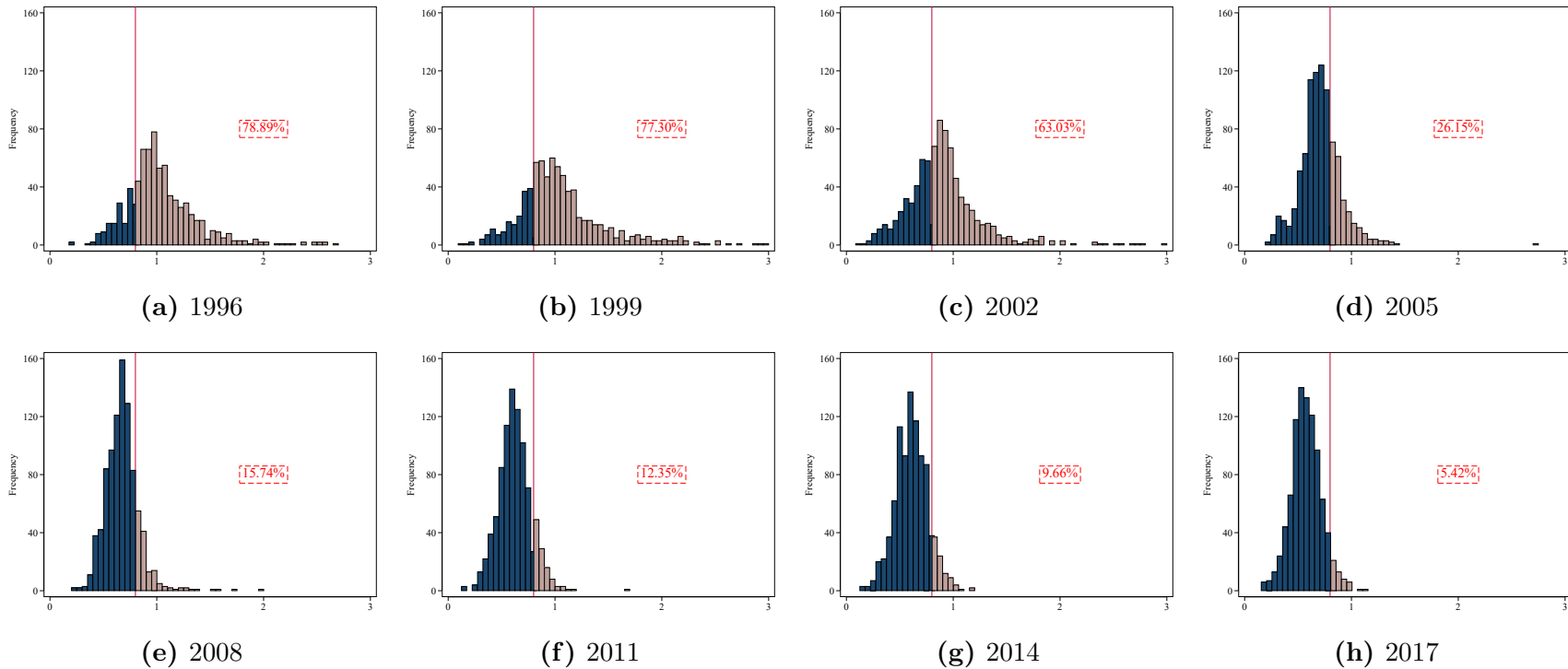
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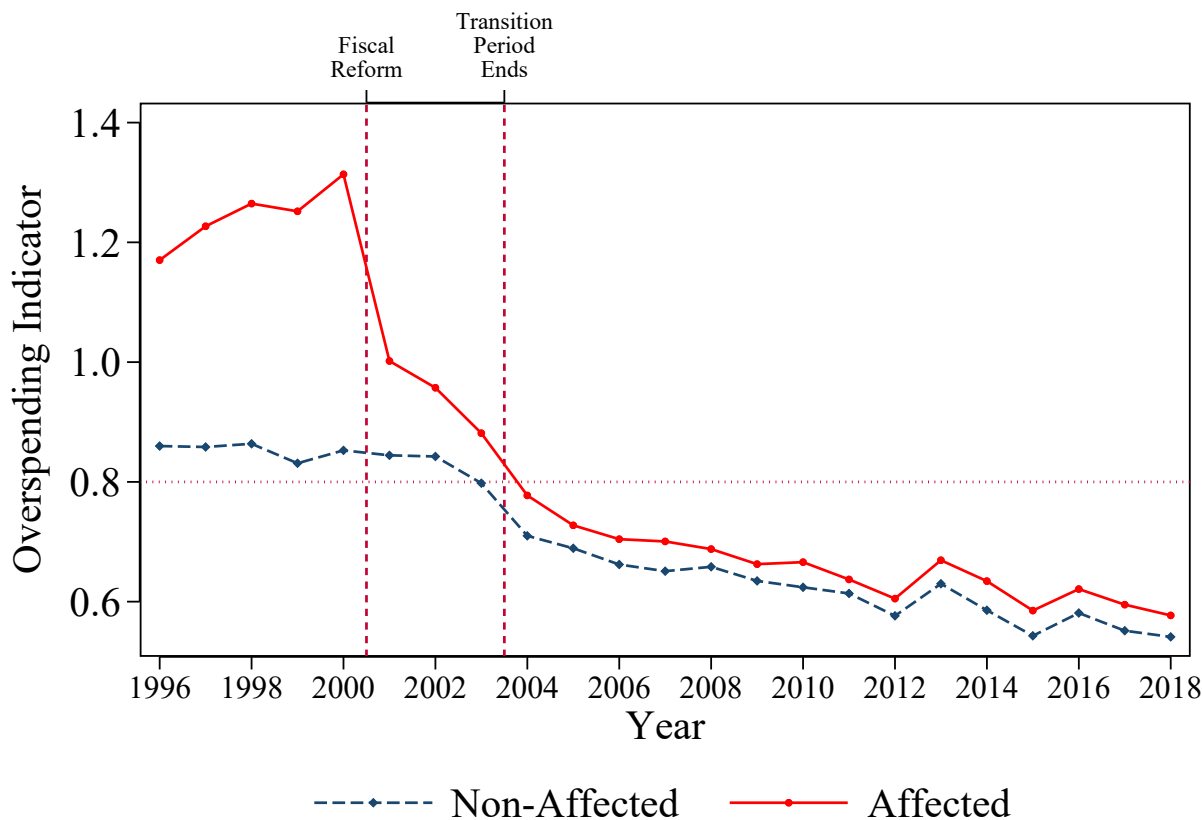


**Figure 1:** Aggregate Compliance with the Fiscal Rule



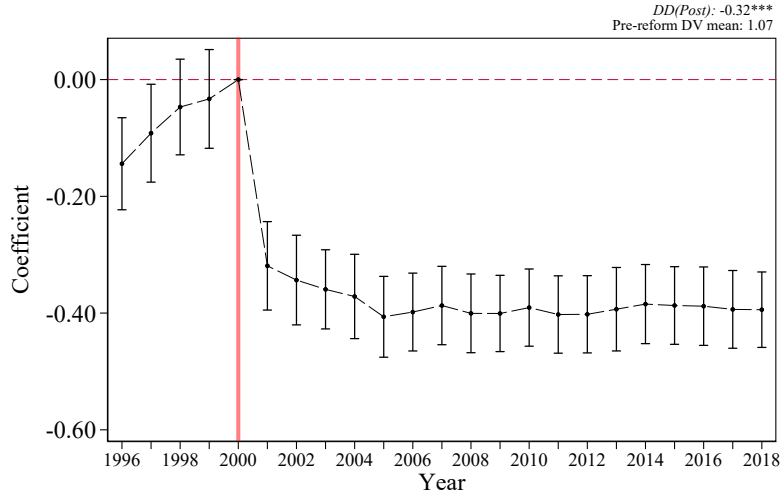
*Notes:* Each panel shows the distribution of the overspending indicator in the year in the caption. This indicator is defined as operating expenses divided by disposable current revenue and is estimated using data from the municipal balance sheets published by DNP. The red vertical line denotes the 80% cap on the overspending indicator set by the fiscal rule, which became binding in 2004 (transition period: 2001-2003). The number in the box indicates the percentage of municipalities that exceed the legal cap. These are shaded in red in the graph.

**Figure 2:** Overspending Indicator: Yearly Average by Group

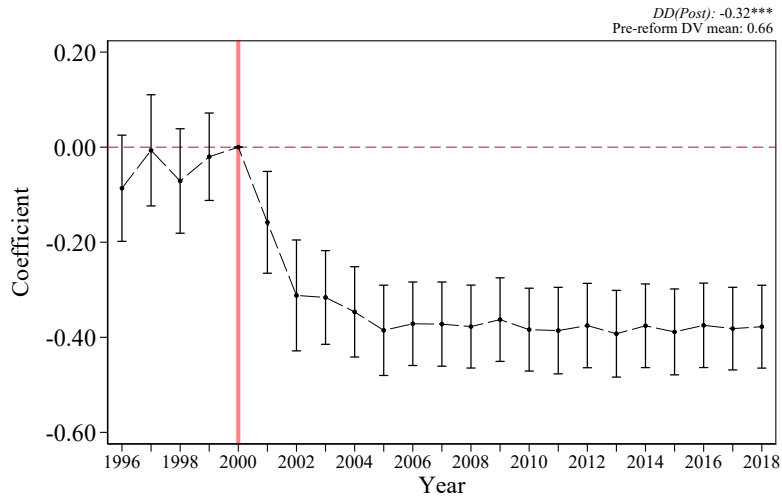


*Notes:* The graph shows the average value of the overspending indicator in the affected and non-affected groups by year. The overspending indicator is defined as operating costs divided by disposable current revenue. The exposed group corresponds to those municipalities that had an average value of the overspending indicator between 1996 and 2000 larger than one. The dashed vertical lines correspond to the start and the end of the phase-in period in 2001 and 2003. The cap on the overspending indicator was set by the fiscal rule at 0.95 in 2001, 0.9 in 2002, 0.85 in 2003 and 0.8 from 2004 onward, as indicated by the dotted horizontal line.

**Figure 3: Main Fiscal Outcomes: Event Studies**



**(a) Overspending Indicator**



**(b) Current Deficit (indicator)**

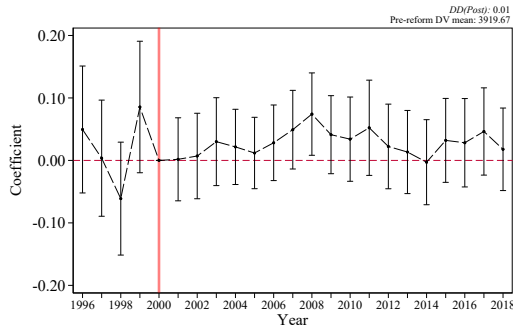
*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is the overspending indicator, defined as operating costs over disposable current revenue, while in panel (b) it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

**Figure 4: Sub-components of the Overspending Indicator: Event Studies**

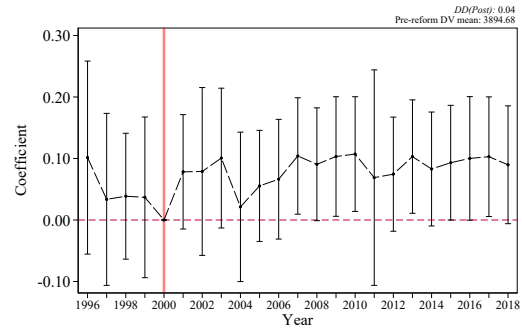


*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is operating expenses, while in panel (e) it is disposable current revenue. Panels (b)-(d) correspond to the sub-components of operating expenses, while panels (f)-(h) correspond to the sub-components of disposable current revenue. All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

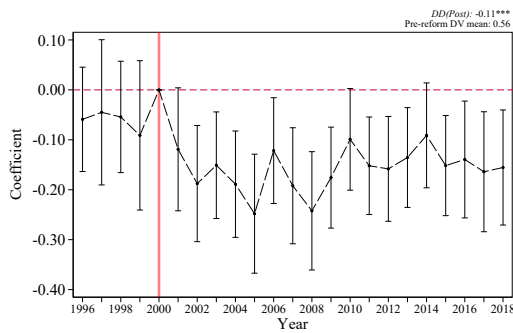
**Figure 5: Other Fiscal Outcomes: Event Studies**



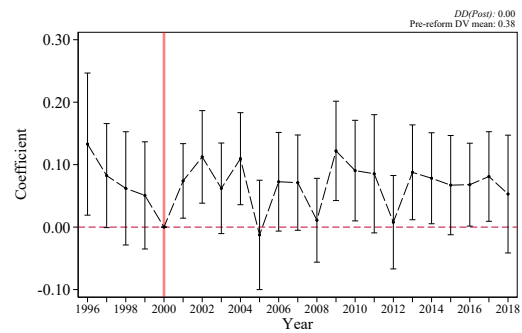
**(a) Capital Revenue**



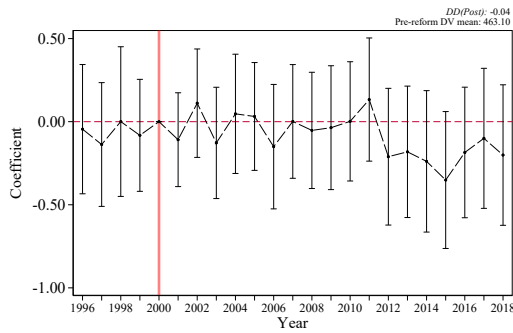
**(b) Capital Expenses**



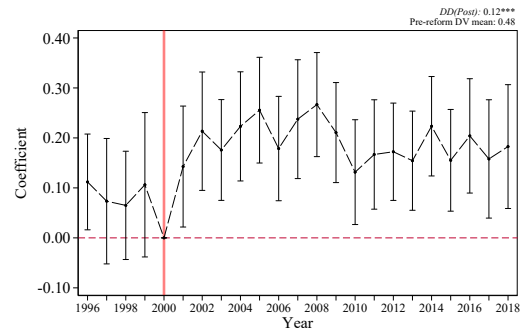
**(c) Total Deficit (=1)**



**(d) Net Credit Inflows (=1)**



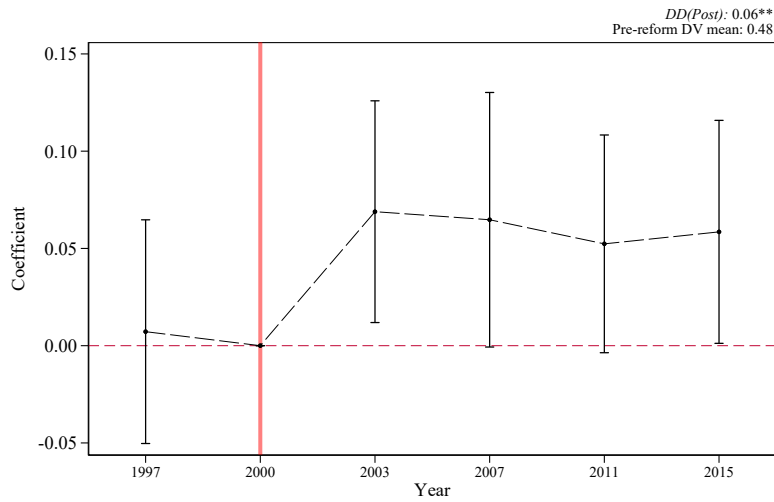
**(e) Interest Payments**



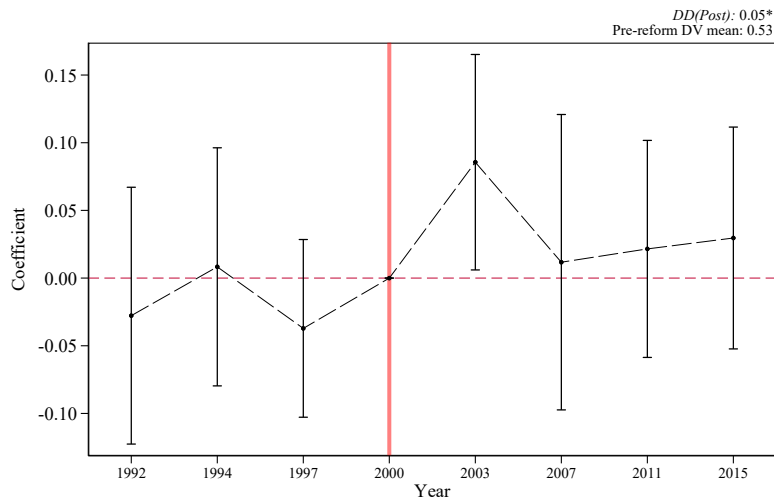
**(f) Positive Balance (=1)**

*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is capital revenue, in panel (b) it is capital expenses, in panel (c) it is an indicator equal to one if the municipal government experiences a total deficit, in panel (d) it is an indicator equal to one if the municipal government experiences positive net credit inflows, in panel (e) it is interest payments, and in panel (f) it is an indicator equal to one if the municipal government experiences a net increase in wealth. All monetary outcomes correspond to the natural logarithm of the value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

**Figure 6: Main Political Outcomes: Event Studies**



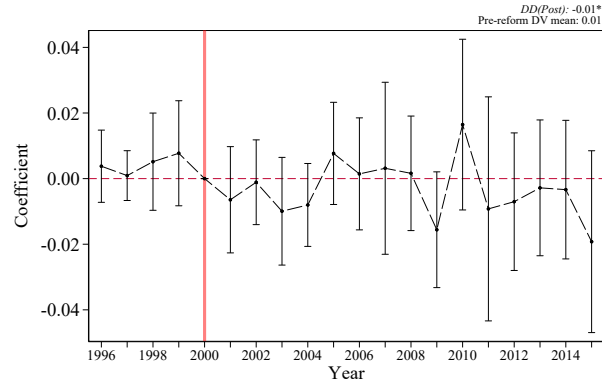
**(a) Vote Share for the Incumbent Party**



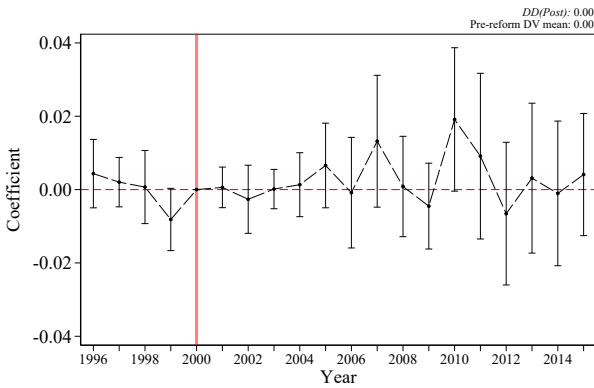
**(b) Incumbent Party Wins (=1)**

*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is the share of votes for the incumbent party in the mayoral election, while in panel (b) it is an indicator equal to one if the incumbent party wins the subsequent election. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year. Data on vote shares for all parties competing in the mayoral election is only available since 1997, which leaves us with a shorter sample period before the reform (only 1997 and 2000 elections) in panel (a).

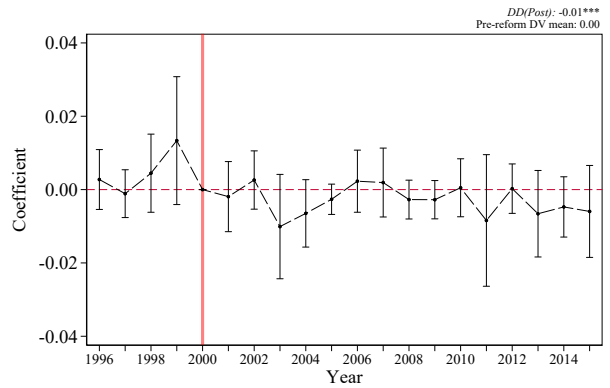
**Figure 7: Protests Outcomes: Event Studies**



**(a) Protests: Any (=1)**



**(b) Protests: Public Services (=1)**



**(c) Protests: Labor Disputes (=1)**

*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is an indicator taking value one for any protest against the municipal government. The dependent variable in panel (b) is an indicator taking value one for any protest against the municipal government motivated by public services. The dependent variable in panel (c) it is an indicator taking value one for any protest against the municipal government motivated by labor policies or breach of agreements. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

**Table 1: Main Fiscal Outcomes**

	Overspending Indicator		Current Deficit (=1)	
	(1)	(2)	(3)	(4)
Affected $\times 1[t > 2000]$	-0.32*** (0.015)	-0.32*** (0.015)	-0.33*** (0.019)	-0.32*** (0.019)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls		✓		✓
Observations	18,569	18,569	18,569	18,569
DV Mean	1.07	1.07	0.66	0.66
DV Std. Dev.	0.38	0.38	0.47	0.47

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in columns 1-2 is the overspending indicator, defined as operating costs over disposable current revenue, while in columns 3-4 it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects. In columns 2 and 4 we also include year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .



**Table 2:** Sub-components of the Overspending Indicator

	Operating Expenses				Disposable Current Revenue			
	Total	Personnel	General	Paid Transfers	Total	Tax Revenue	Non-Tax Revenue	Disposable Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected $\times \mathbb{1}[t > 2000]$	-0.21*** (0.018)	-0.16*** (0.021)	-0.25*** (0.031)	-0.16*** (0.058)	0.07*** (0.017)	0.12*** (0.037)	0.29*** (0.057)	0.08*** (0.029)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,569	18,569	18,569	18,569	18,569	18,569	18,569	18,569
DV Mean	1395.47	714.90	479.22	310.65	1387.27	465.40	281.48	641.61
DV Std. Dev.	1038.36	3094.41	1158.44	538.54	1097.34	690.04	482.99	355.46

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is operating expenses, while in column 5 it is disposable current revenue. Columns 2-4 correspond to the sub-components of operating expenses: personnel expenses, general expenses (i.e., procurement), and paid transfers (mostly pensions and payments from legal rulings). Columns 6-8 correspond to the sub-components of disposable current revenue: Tax revenue, Non-tax revenue (i.e., fees and fines), and disposable SGP transfers from the central government. All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table 3: Other Fiscal Outcomes**

	Capital Revenue	Capital Expenses	Total Deficit (=1)	Net Credit Inflows (=1)	Interest Payments	Positive Balance (=1)
	(1)	(2)	(3)	(4)	(5)	(6)
Affected $\times \mathbb{1}[t > 2000]$	0.01 (0.023)	0.04 (0.027)	-0.11*** (0.024)	0.00 (0.019)	-0.04 (0.108)	0.12*** (0.024)
Municipality FE	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	18,569	18,569	18,569	18,569	18,569	18,569
DV Mean	3919.67	3894.68	0.56	0.38	463.10	0.48
DV Std. Dev.	3545.80	3243.00	0.50	0.48	13578.32	0.50

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is capital revenue, in column 2 it is capital expenses, in column 3 it is an indicator equal to one if the municipal government experiences a total deficit, in column 4 it is an indicator equal to one if the municipal government experiences positive net credit inflows, in column 5 it is interest payments, and in column 6 it is an indicator equal to one if the municipal government experiences a net increase in wealth. All monetary outcomes correspond to the natural logarithm of the value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table 4:** Performance of Incumbent Mayor’s Party in Next Election

	Vote Share		Wins (=1)	
	(1)	(2)	(3)	(4)
Affected $\times \mathbb{1}[t > 2000]$	0.07*** (0.025)	0.06** (0.023)	0.06** (0.028)	0.05* (0.028)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls		✓		✓
Observations	4,754	4,754	6,366	6,366
DV Mean	0.48	0.48	0.53	0.53
DV Std. Dev.	0.39	0.39	0.50	0.50

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in columns 1-2 is the share of votes for the incumbent party in the mayoral election, while in columns 3-4 it is an indicator equal to one if the incumbent party wins the election. Regressions include municipality and department-year fixed effects. Columns 2 and 4 also include year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. Data on vote shares for all parties competing in the mayoral election is only available since 1997, which leaves us with a shorter sample period before the reform (only 1997 and 2000 elections) in columns 1-2. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table 5:** Protests Against the Municipal Government

	Any Protest (=1)		Cause (=1)			Type (=1)		
			Public Services	Labor Disputes	Other	Mass Mobilization	Strike	Roadblock / Occupation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected $\times \mathbb{1}[t > 2000]$	-0.004 (0.0038)	-0.007* (0.0038)	0.003 (0.0027)	-0.007*** (0.0026)	-0.003* (0.0019)	-0.002 (0.0019)	-0.007** (0.0027)	0.001 (0.0024)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls		✓	✓	✓	✓	✓	✓	✓
Observations	16,880	16,880	16,880	16,880	16,880	16,880	16,880	16,880
DV Mean	0.010	0.010	0.004	0.005	0.001	0.002	0.006	0.003
DV Std. Dev.	0.098	0.098	0.065	0.070	0.031	0.041	0.078	0.051

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in all columns is an indicator taking the value of one if protests against the municipal government took place in the municipality-year. In columns 1-2, any protest against the municipal government. In columns 3-5, protests related to a specific cause: local public services, labor disputes or breach of agreements, other (e.g., human rights violations). In columns 6-8, specific types of protest: mass mobilizations, strikes, or roadblocks or occupation of buildings. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table 6:** Public Goods and Living Standards

	Health Outcomes			Education Outcomes			Other Outcomes		
	Subsidized Health Insurance	Low Birth-Weight	Average Prenatal Visits	Schools per 10,000 inh.	Teacher-Pupil Ratio	Student Enrollment	Cadastral Update (=1)	Corruption Sanctions (=1)	Night-time Lights
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Affected $\times \mathbb{1}[t > 2000]$	-0.01 (0.008)	2.66 (1.990)	0.02 (0.037)	-0.02 (0.404)	0.00 (0.000)	-0.01 (0.022)	-0.01 (0.011)	-0.01 (0.023)	0.00 (0.010)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,908	11,815	11,813	15,144	15,147	15,147	14,348	6,499	15,192
DV Mean	0.80	60.89	4.08	30.89	0.05	7.69	0.13	0.15	0.94
DV Std. Dev.	0.42	36.41	0.96	15.54	0.01	1.13	0.34	0.36	0.75
Sample first year	1998	1998	1998	1996	1996	1996	1996	1990	1996
Sample final year	2004	2011	2011	2013	2013	2013	2012	2011	2013

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is the share of poor population receiving subsidized health insurance. In column 2, it is the share of newborn (per 1,000) with low birth weight (<2,500 grams), while in column 3 it is the average number of prenatal visits. The dependent variable in column 4 is the number of public schools in the municipality per 10,000 inhabitants, in column 5 it is the teacher-pupil ratio in the public sector and in column 6 it is the logarithm of the number of students in public education (primary and early secondary). The dependent variable in column 7 is an indicator equal to 1 if the municipality had a cadastral update on that year. In column 8, it is an indicator equal to 1 if the mayor has been sanctioned for corruption by CGR. The dependent variable in column 9 is the natural logarithm of the area-weighted average night-time lights Digital Number (DN). Unit of observation is municipality-year in all columns except column 8, where it is municipality-mayoral term. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

# Appendix (for online publication)

## ECONOMIC AND POLITICAL EFFECTS OF FISCAL RULES: EVIDENCE FROM A NATURAL EXPERIMENT IN COLOMBIA

Authors: Maria Carreri and Luis R. Martínez

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## A Data Appendix

This appendix provides detailed information on data sources, sample availability and other details for the different variables used in the paper.

### Fiscal variables

- *Overspending Indicator*: Defined as the yearly ratio of operating expenses over disposable current revenue. We construct this variable for all years between 1996 and 2018 by dividing (total) operating expenses by (total, not disposable) current revenue, based on the data in the municipal balance sheets provided yearly by DNP.
- *Current Deficit (=1)*: Indicator that equals one if the municipality experiences a current deficit (i.e., current revenue below current expenses). Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Operating Expenses*: Measured in millions of 2010 COP. This variable measures each municipality-year total operating expenses. It is equal to the sum of personnel expenses, general expenses and paid transfers. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Personnel Expenses*: Measured in millions of 2010 COP. This variable measures each municipality-year personnel expenses (i.e., payroll of permanent and temporary employees of the municipal government). It is a sub-component of operating expenses. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *General Expenses*: Measured in millions of 2010 COP. This variable measures general expenses in each municipality-year (i.e., procurement, insurance premiums, publications, rent, maintenance and utility payments for municipal property). It is a sub-component of operating expenses. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Paid Transfers*: Measured in millions of 2010 COP. This variable measures each municipality-year paid transfers (i.e., pension payments for qualifying former municipal employees and payments mandated by legal sentences). It is a sub-component of operating expenses. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Disposable Current Revenue*: Measured in millions of 2010 COP. This variable measures each municipality-year disposable current revenue: the sum of tax and non-tax revenue, and SGP transfers specifically designated for this purpose (*libre destinación*). Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets and SGP transfers provided yearly by DNP.

- *Tax Revenue*: Measured in millions of 2010 COP. This variable measures tax revenue in each municipality-year (property tax, a tax on gross business receipts, surcharge on the price of gasoline, other taxes). It is a sub-component of disposable current revenue. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Non-Tax Revenue*: Measured in millions of 2010 COP. This variable measures each municipality-year non-tax revenue (i.e., fines and fees issued by the municipality). It is a sub-component of disposable current revenue. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Disposable Transfers*: Measured in millions of 2010 COP. This variable measures each municipality-year disposable transfers from the central government. It is a sub-component of disposable current revenue. Information is available for all municipalities between 1996 and 2018, based on the data on SGP transfers provided yearly by DNP.
- *Capital Revenue*: Measured in millions of 2010 COP. This variable measures each municipality-year capital revenue, which includes SGP transfers, co-financing, and natural resource royalties. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Capital Expenses*: Measured in millions of 2010 COP. This variable measures each municipality-year capital expenses (i.e., investment). Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Total Deficit (=1)*: Indicator that equals one if total spending in the municipality-year (current plus capital) exceeds total revenue. By construction, total deficit is also equal to net credit inflows plus change in balance. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Net Credit Inflows (=1)*: Indicator that equals one if new inflows of credit exceed outflows (i.e., payment of principal) in the municipality-year. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Interest Payments*: Measured in millions of 2010 COP. This variable measures each municipality-year interest payments. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.
- *Positive balance (=1)*: Dummy that equals one if the municipality experiences a positive change in wealth. Information is available for all municipalities between 1996 and 2018, based on the data in the municipal balance sheets provided yearly by DNP.



## Political variables

- *Vote Share for the Incumbent*: Votes for the party of the incumbent mayor as proportion of the total votes at the municipality-election level. Since we do not observe the number of votes for each candidate previous to 1997, we are only able to construct this measure for elections in 1997, 2000, 2003, 2007, 2011, and 2015. Electoral information was provided by CEDE at Universidad de los Andes and it is based on official electoral records from Colombia's electoral office (*Registraduría Nacional del Estado Civil*).
- *Incumbent Wins (=1)*: Dummy that equals to one if the incumbent party wins the subsequent mayoral election. We are able to construct this dummy for all elections between 1992 and 2015 (i.e., 1992, 1994, 1997, 2000, 2007, 2011, 2015). Electoral information was provided by CEDE at Universidad de los Andes and it is based on official electoral records from Colombia's electoral office (*Registraduría Nacional del Estado Civil*).
- *Share of votes for Liberal Party (mean 1997-2000)*: Average share of votes for the Liberal Party (i.e., votes for the Liberal Party as proportion of the total votes) in 1997 and 2000 mayoral elections at the municipality level. Since we do not observe the number of votes for each party previous to 1997, for elections before the reform, we are only able to construct the vote share for elections in 1997 and 2000. Electoral information was provided by CEDE at Universidad de los Andes and it is based on official electoral records from Colombia's electoral office (*Registraduría Nacional del Estado Civil*).
- *Share of votes for Conservative Party (mean 1997-2000)*: Average share of votes for the Conservative Party (i.e., votes for the Conservative Party as proportion of the total votes) in 1997 and 2000 mayoral elections at the municipality level. Since we do not observe the number of votes for each party previous to 1997, for elections before the reform, we are only able to construct the vote share for elections in 1997 and 2000. Electoral information was provided by CEDE at Universidad de los Andes and it is based on official electoral records from Colombia's electoral office (*Registraduría Nacional del Estado Civil*).
- *Mayoral elections HHI (mean 1997-2000)*: Average Herfindahl–Hirschman Index in 1997 and 2000 mayoral elections at the municipality level. For each election we calculate the normalized HHI as:

$$\text{HHI} = \frac{\sum_{i=1}^N s_i^2 - 1/N}{1 - 1/N}$$

where  $s_i$  is the vote share of party  $i$  in the mayoral election and  $N$  is the number of parties competing. This normalized HHI ranges from 0 to 1, with larger values indicating greater concentration.

- *Any Protest (=1)*: Indicator equal to one if there was a protest against the municipality's local government in a given year, based on a proprietary event-based dataset

provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015.

- *Public Services Protests (=1)*: Indicator equal to one if there was a protest against the municipality's local government related to public services in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by cause.
- *Labor Disputes Protests (=1)*: Indicator equal to one if there was a protest against the municipality's local government related to labor disputes or breach of agreements in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by cause.
- *Other Protests Causes (=1)*: Indicator equal to one if there was a protest against the municipality's local government that is not related to public services or labor disputes in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by cause.
- *Mass Mobilizations Protests (=1)*: Indicator equal to one if we observe a mass mobilization against the municipality's local government in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by type of event. Mass mobilizations include peaceful marches, parades, rallies, sit-ins and demonstrations in general.
- *Strike Protests (=1)*: Indicator equal to one if we observe a strike against the municipality's local government in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by type of event. CINEP directly classifies protests by type of event. Strikes refers to protest with explicit cessation of all activity with a clear chain of command and simultaneity at the beginning and at the end.
- *Road Blocks & Occupations Protests (=1)*: Indicator equal to one if we observe a road-block or building occupation against the municipality's local government in a given year, based on a proprietary event-based dataset provided by CINEP (*Centro de Investigación y Educación Popular*) for the period 1996-2015. CINEP directly classifies protests by type of event. This variable considers protests that involve road blocks and the temporary occupations of public or private entities.

## Public goods

- *Subsidized Health Insurance*: Define as the number of people enrolled in the government's subsidized health insurance (regimen subsidiado) at the municipality-year level,

expressed as a share of the number of people with Unmet Basic Needs (UBN) in the 1993 census. The data is provided by the Ministry of Health and is available between 1998 and 2004. Enrollment for later years is expressed as a share of the number of people classified as poor by Colombia's proxy means testing system (SISBEN) and is not comparable.

- *Low Birth-Weight*: Defined as the number of newborn (per 1,000) with low birth weight (<2,500 grams), expressed as a share of the total number of births at the municipality-year level. Vital statistics are provided by the National Department of Statistics (DANE) and are available between 1998 and 2011.
- *Average Prenatal Visits*: Defined as the average number of prenatal visits for each birth at the municipality-year level. Vital statistics are provided by the National Department of Statistics and are available between 1998 and 2011.
- *Schools per 10,000 inh*: Defined as total number of public schools in the municipality per 10,000 inhabitants. Data was provided by CEDE at Universidad de los Andes and it is based on official records from the Ministry of Education, available between 1996 and 2013.
- *Teacher-Pupil Ratio*: Defined as the number of teachers per student in the public sector. Data was provided by CEDE at Universidad de los Andes and it is based on official records from the Ministry of Education, available between 1996 and 2013.
- *Student Enrollment*: Defined as the number of students in public education (primary and early secondary). Data was provided by CEDE at Universidad de los Andes and it is based on official records from the Ministry of Education, available between 1996 and 2013.
- *Cadastral Update (=1)*: Indicator equal to one if the municipality had a cadastral update in a given year. These updates are performed by IGAC and involve a reassessment of the value of all properties in the municipality (urban, rural or both depending on the scope). Data was provided by the National Geographic Institute (IGAC) and is available between 1996 and 2012.
- *Corruption Sanctions (=1)*: Indicator equal to one if the mayor has been sanctioned for mishandling of public funds. We construct this variable by matching the names of the mayors in the sample with those of all individuals sanctioned by CGR since 1990. This variable is coded at the municipality - mayoral term level.
- *Night-time Lights*: Original data comes from the US Air Force's Defense Meteorological Satellite Program (DMSP), which records night-time lights (NTL) originating from earth using the Operational Linescan System (OLS) sensor. The raw data is cleaned and processed by the National Oceanic and Atmospheric Administration (NOAA). NOAA provides composite images of NTL at the grid-cell level (roughly one squared kilometer at the Equator) for each year between 1992 and 2013. The variable of interest is a NTL Digital Number (DN) that ranges from 0 to 63, with larger values

corresponding to increased luminosity. We combine the DMSP data with a shapefile of Colombian municipalities and calculate an area-weighted average of NTL DN per municipality-year.

## Municipality characteristics

- *Foundation Year*: Year of foundation for each municipality. Information was provided by CEDE at Universidad de los Andes.
- *Area*: Municipality's total area in square kilometers. Information was provided by CEDE at Universidad de los Andes.
- *Altitude*: Municipality's average area-weighted altitude in meters above the sea level. Information was provided by CEDE at Universidad de los Andes.
- *Distance to the Department's Capital*: Shortest geodesic distance to the department's capital in kilometers. Information was provided by CEDE at Universidad de los Andes.
- *Distance to Bogotá*: Shortest geodesic distance to Colombia's capital, Bogotá D.C., measured in kilometers. Information was provided by CEDE at Universidad de los Andes.
- *Share of Rural Population (mean 1995-2000)*: Defined as inhabitants living in rural areas of the municipality as a proportion of the total number of inhabitants. Measured as the average between 1995 and 2000 at the municipality level. Information was provided by CEDE at Universidad de los Andes.
- *Schools in 1996 (=1)*: Indicator equal to one if the municipality had at least one public school in 1996. Information was provided by CEDE at Universidad de los Andes.
- *Unmet Basic Needs index in 1993*: Defined as the average Unmet Basic Needs index across in each municipality in the 1993 General Census. Information was provided by CEDE at Universidad de los Andes.
- *Notary office in 1996 (=1)*: Indicator equal to one if the municipality had at least one notary office in 1996. Information was provided by CEDE at Universidad de los Andes.
- *Agricultural bank office in 1996 (=1)*: Indicator equal to one if the municipality had at least one agricultural bank office (*Banco Agrario de Colombia*) in 1996. Information was provided by CEDE at Universidad de los Andes.
- *Tax collection office in 1996 (=1)*: Indicator equal to one if the municipality had at least one tax collection office in 1996. Information was provided by CEDE at Universidad de los Andes.
- *Health center or hospital in 1996 (=1)*: Indicator equal to one if the municipality had at least one health center or hospital in 1996. Information was provided by CEDE at Universidad de los Andes.

- *FARC demilitarized zone and neighbors (=1)*: Indicator equal to one if the municipality was part of the demilitarized zone awarded to insurgent group FARC between 1999 and 2002, or a neighboring municipality. Information was provided by CEDE at Universidad de los Andes.
- *Guerrilla presence between 1996 and 2000 (=1)*: Indicator equal to one for municipalities with at least one conflict event involving FARC between 1996 and 2000. Information was provided by Universidad del Rosario's Conflict Data Base.
- *Paramilitary presence between 1996 and 2000 (=1)*: Indicator equal to one for municipalities with at least one conflict event involving right-wing paramilitary groups between 1996 and 2000. Information was provided by the Universidad del Rosario's Conflict Data Base.
- *Coca crops between 1999 and 2000 (=1)*: Indicator equal to one for municipalities with at least one hectare of coca crops between 1999 and 2000. Data was provided by CEDE at Universidad de los Andes and is based on official records from the United Nations Office on Drugs and Crime.

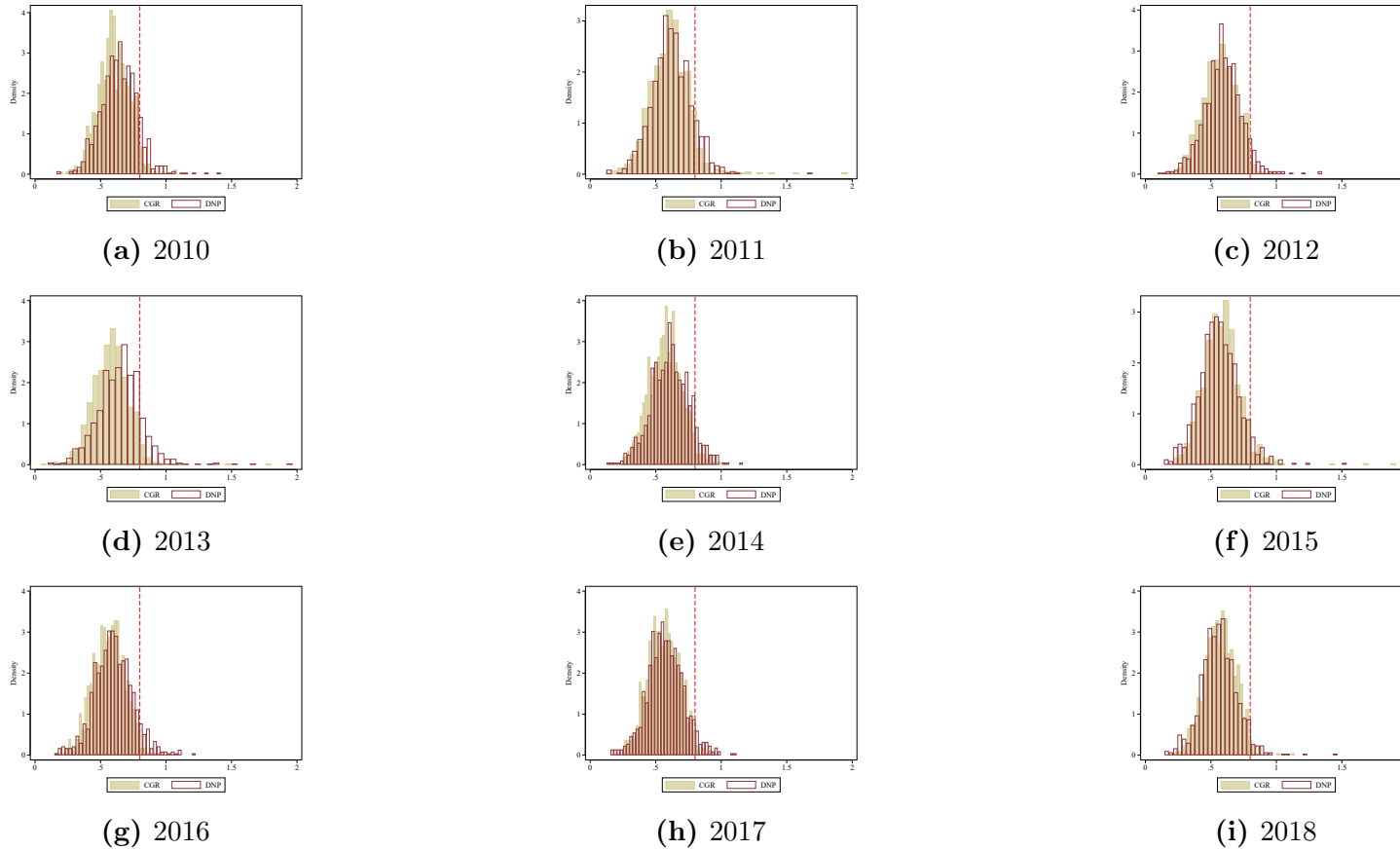
**Table A1: Summary Statistics**

	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Exposure</i>					
Affected (=1)	761	0.6	0.5	0.0	1.0
Mean Overspending Indicator (1996-2000)	761	1.1	0.3	0.4	2.1
<i>Panel B: Covariates</i>					
Mun. foundation year	761	1,866.3	102.7	1,535.0	1,999.0
Area ( $km^2$ )	761	774.7	3,180.4	20.0	65,674.0
Altitude (meters above sea level)	761	1,283.6	1,232.9	2.0	25,221.0
Distance to dep. capital ( $km$ )	761	80.0	51.3	0.0	360.8
Distance to nearest market ( $km$ )	761	118.6	77.6	9.6	662.1
Distance to Bogota ( $km$ )	761	291.9	168.8	12.5	751.2
Share of rural population (mean 1995-2000)	761	0.7	0.2	0.0	1.0
Schools in 1996 (=1)	761	1.0	0.1	0.0	1.0
Unmet Basic Needs index in 1993	761	54.6	17.5	21.4	100.0
Notary office in 1996 (=1)	761	0.4	0.5	0.0	1.0
Agricultural bank office in 1996 (=1)	761	1.0	0.2	0.0	1.0
Tax collection office in 1996 (=1)	761	0.4	0.5	0.0	1.0
Health center or hospital in 1996 (=1)	761	0.8	0.4	0.0	1.0
FARC demilitarized zone and neighbors (=1)	761	0.0	0.1	0.0	1.0
Guerrillas presence between 1996 and 2000 (=1)	761	0.7	0.5	0.0	1.0
Paramilitary presence between 1996 and 2000 (=1)	761	0.4	0.5	0.0	1.0
Coca crops between 1999 and 2000 (=1)	761	0.2	0.4	0.0	1.0
Share of votes for Liberal Party (mean 1997-2000)	761	0.4	0.4	0.0	1.0
Share of votes for Conservative Party (mean 1997-2000)	761	0.3	0.3	0.0	1.0
Mayoral elections HH index (mean 1997-2000)	761	0.3	0.1	0.1	0.8
<i>Panel C: Outcomes</i>					
Overspending Indicator	18,569	0.8	0.3	0.3	2.1
Current Deficit (=1)	18,569	0.2	0.4	0.0	1.0
Operating Expenses: Total	18,569	1,329.3	894.7	326.1	4,993.0
Operating Expenses: Personnel	18,569	754.8	1,525.0	0.0	182,236.4
Operating Expenses: General	18,569	359.8	564.2	0.0	63,543.7
Operating Expenses: Paid Transfers	18,569	246.5	379.1	0.0	19,338.2
Freely Disposable Revenue: Total	18,569	2,007.9	1,591.9	372.7	8,941.6
Freely Disposable Revenue: Tax Revenue	18,569	1,067.1	1,547.7	0.0	36,422.3
Freely Disposable Revenue: Non-Tax Revenue	18,569	258.3	481.0	0.0	24,573.8
Freely Disposable Revenue: Disposable Transfers	18,569	716.7	298.4	0.0	3,366.3
Capital Revenues	18,569	7,424.9	9,183.3	0.0	237,489.3
Capital Expenses	18,569	8,404.9	10,236.0	0.0	218,116.8
Total Deficit (=1)	18,569	0.6	0.5	0.0	1.0
Net Credit Inflows (=1)	18,569	0.2	0.4	0.0	1.0
Interest Payment	18,569	162.7	6,149.0	0.0	735,335.7
Positive Balance (=1)	18,569	0.5	0.5	0.0	1.0
Vote Share Incumbent	4,754	0.3	0.3	0.0	1.0
Incumbent Wins (=1)	6,366	0.4	0.5	0.0	1.0
Subsidized Health Insurance	5,908	0.9	0.4	0.0	3.1
Low Birth-Weight	11,815	69.7	36.8	0.0	1,000.0
Average Prenatal Visits	11,813	5.0	1.1	0.0	8.0
Schools per 10,000 inh.	15,144	29.1	15.9	0.8	131.9
Teacher-Pupil Ratio	15,109	22.0	4.5	1.9	78.0
Student Enrollment	15,147	7.8	1.0	0.0	10.5
Catastral Update (=1)	14,348	0.1	0.3	0.0	1.0
Corruption Sanctions (=1)	6,499	0.2	0.4	0.0	1.0
Night-time Lights	15,192	1.0	0.7	0.0	3.6
Protest: Any Protest (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Public Services (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Labor Disputes (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Other (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Mass Mobilizations (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Strike (=1)	16,880	0.0	0.1	0.0	1.0
Protest: Road Blocks & Occupations (=1)	16,880	0.0	0.1	0.0	1.0

*Notes:* This table shows summary statistics on all relevant variables. Panel A summarizes the main exposure measures, Panel B summarizes the pre-determined covariates consider in our analysis, and Panel C summarizes all outcome variables.

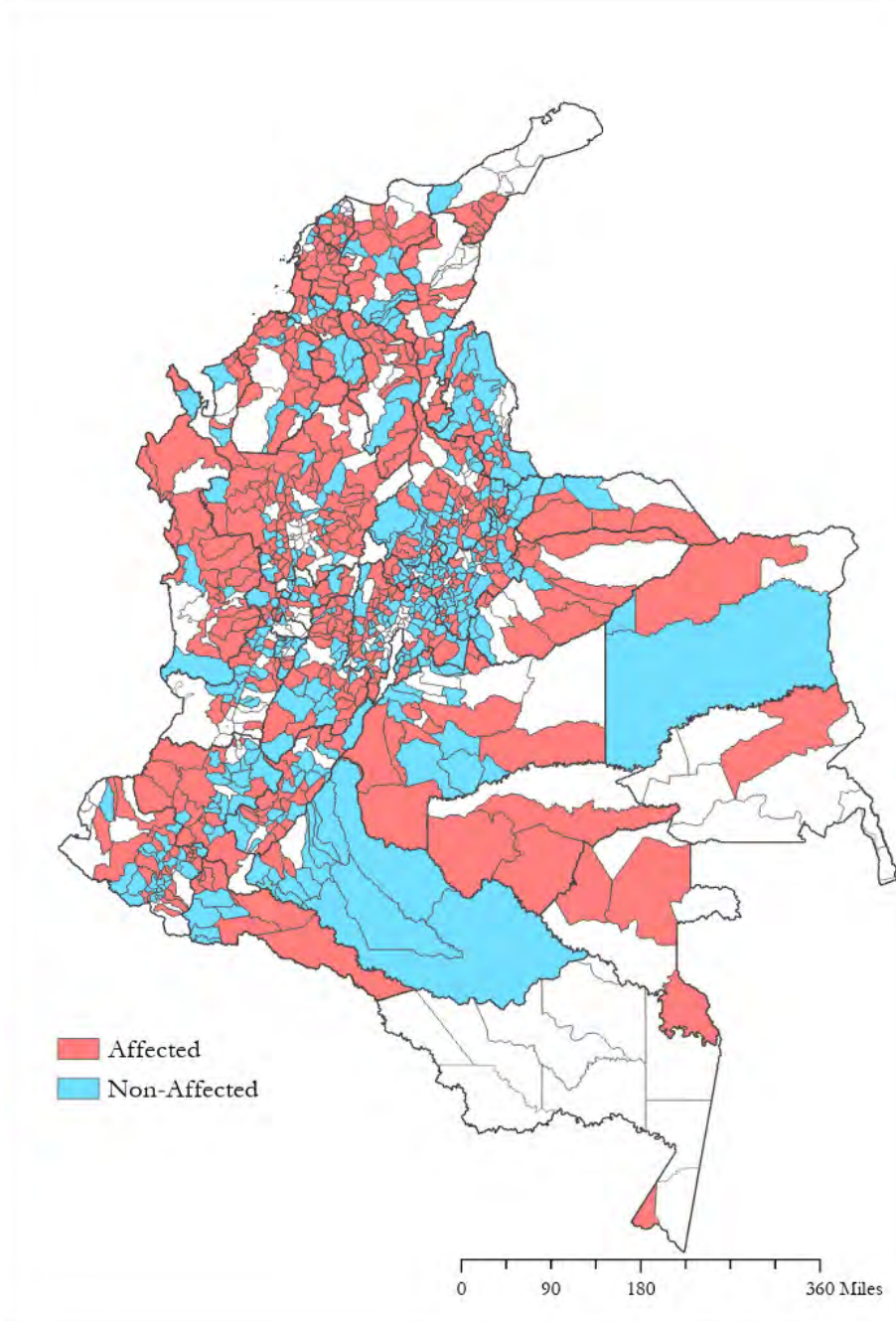
## B Additional Figures and Tables

**Figure B1:** Distribution of the Overspending Indicator: DNP and CGR Estimates



*Notes:* Each panel shows the distribution of the overspending indicator for the year in the caption based on data from DNP (transparent) and CGR (beige). The overspending indicator is defined as operating costs divided by disposable current revenue. The CGR version is based on the actual numbers that municipalities report to the fiscal watchdog for compliance. The DNP version is based on the municipal balance sheets published by this agency. The latter is a proxy, defined as operating expenses divided by current revenue (i.e., without excluding earmarked revenues).

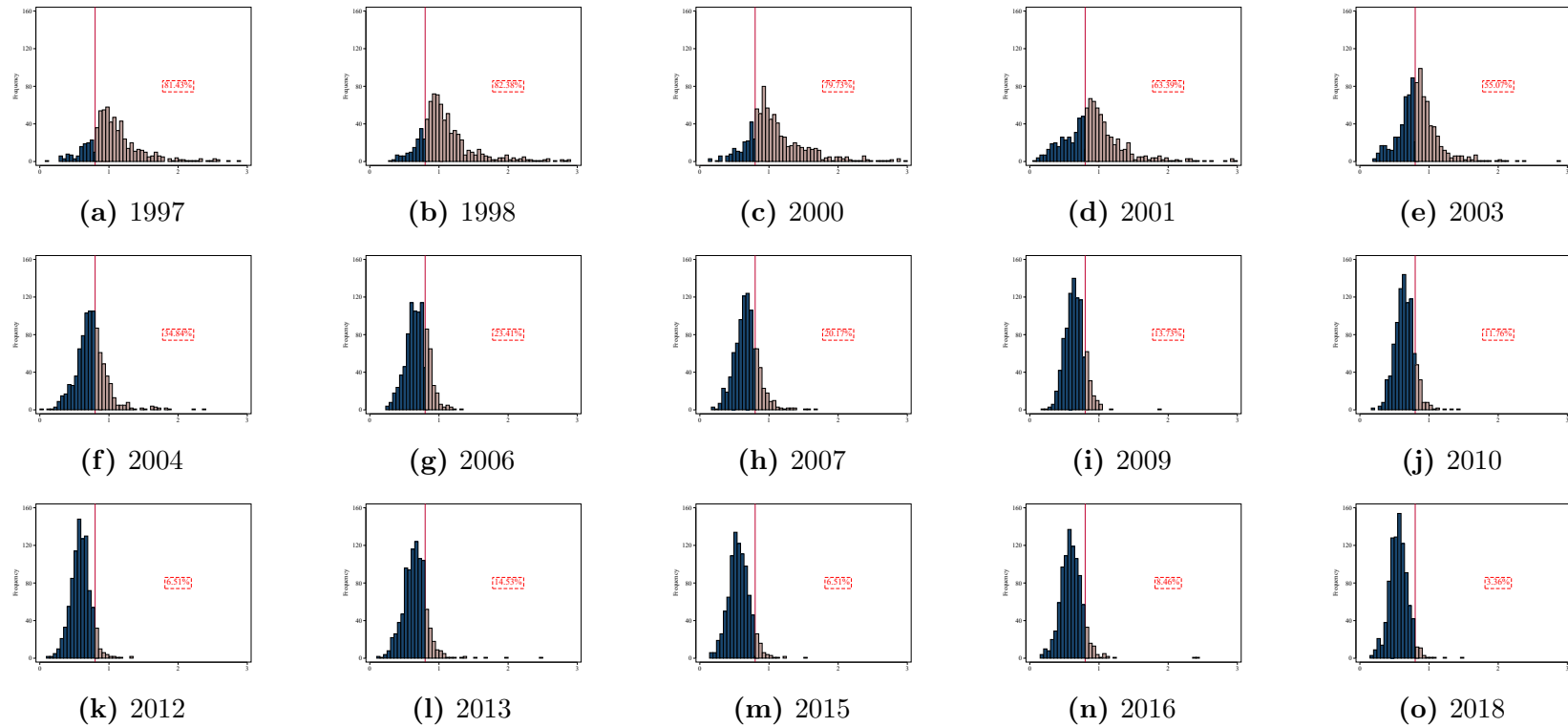
**Figure B2:** Geographic Distribution of Exposure to Fiscal Rule



*Notes:* The map shows the discrete classification that constitutes our baseline measure of exposure to the fiscal rule. Colored in red are those municipalities that we deem as exposed to the rule because the average value of their overspending indicator between 1996 and 2000 was larger than one. Colored in blue are those municipalities whose average overspending indicator was less than one. Municipalities in white are excluded from the sample (i.e., larger cities not in category six). Darker lines correspond to department borders.

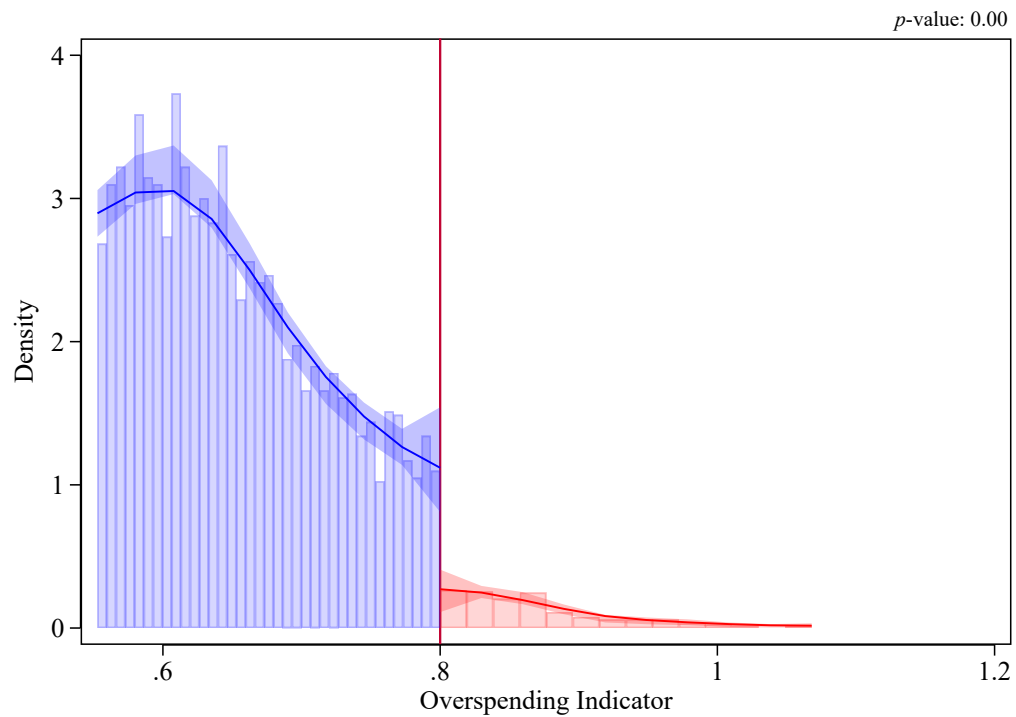


**Figure B3: Aggregate Compliance with the Fiscal Rule: Additional Years**



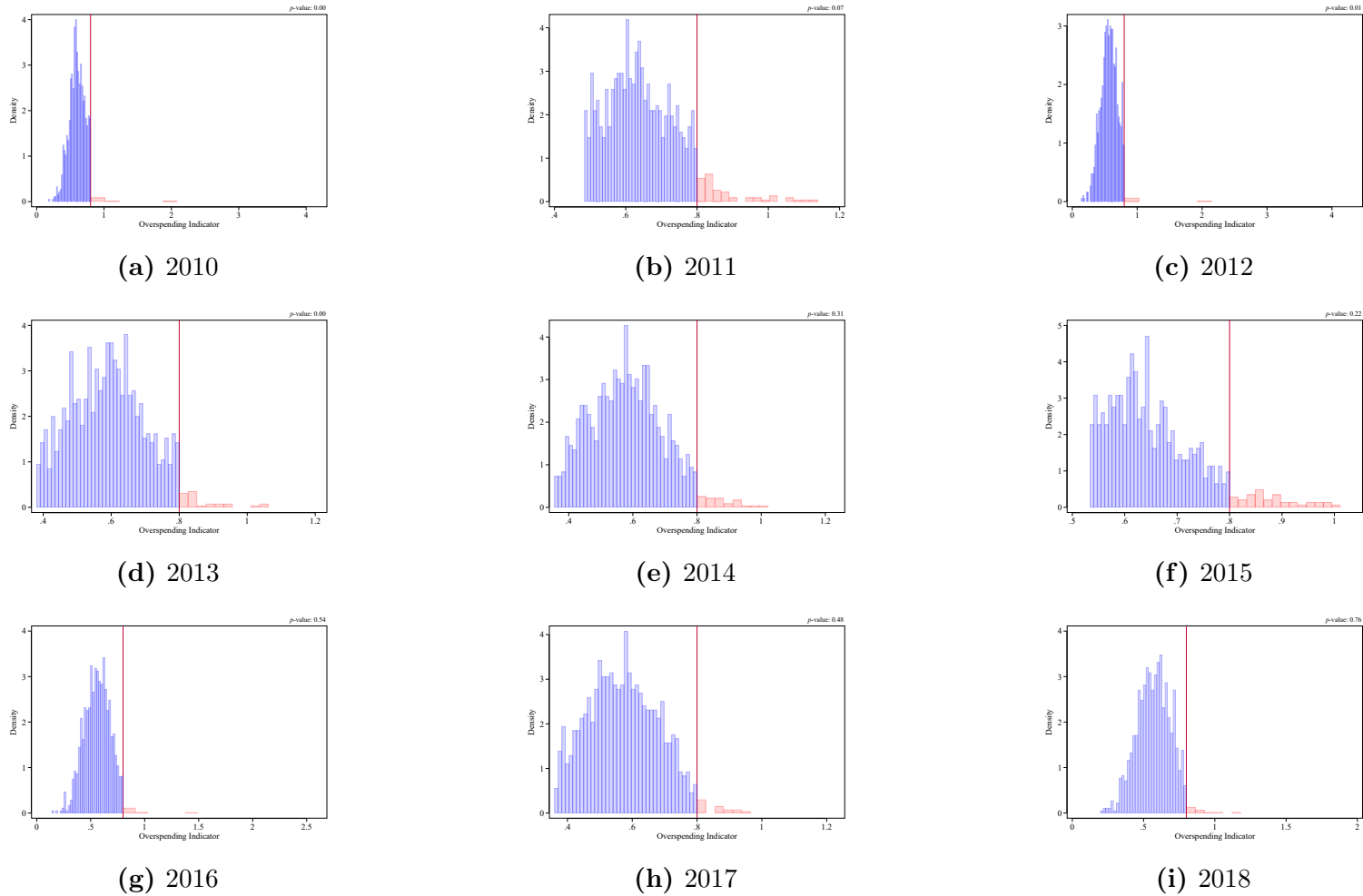
*Notes:* Each panel shows the distribution of the overspending indicator in the year in the caption. This indicator is defined as operating expenses divided by disposable current revenue and is estimated using data from the municipal balance sheets published by DNP. The red vertical line denotes the 80% cap on the overspending indicator set by the fiscal rule, which became binding in 2004 (transition period: 2001-2003). The number in the box indicates the percentage of municipalities that exceed the legal cap. These are shaded in red in the graph.

**Figure B4:** Distribution of Overspending Indicator in CGR Data: 2010-2018



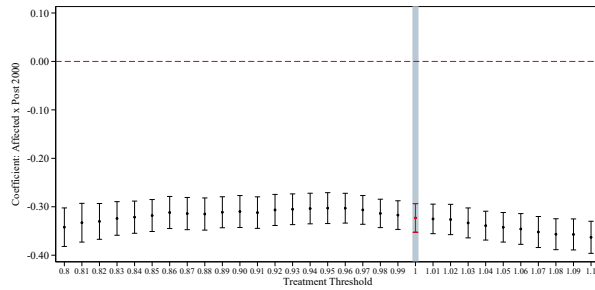
*Notes:* The figure shows the distribution of the overspending indicator based on the CGR figures, pooling data for 2010-2018. This indicator is defined as operating expenses divided by disposable current revenue. The red vertical line denotes the 80% cap on the overspending indicator set by the fiscal rule. We formally test for a discontinuity at the threshold using the test by Cattaneo et al. (2020).

**Figure B5:** Yearly Distribution of Overspending Indicator in CGR Data

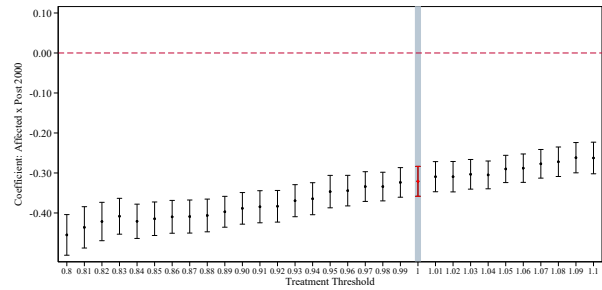


*Notes:* Each panel shows the distribution of the overspending indicator based on the CGR figures for the year in the caption. This indicator is defined as operating expenses divided by disposable current revenue. The red vertical line denotes the 80% cap on the overspending indicator set by the fiscal rule. We formally test for a discontinuity at the threshold using the test by Cattaneo et al. (2020).

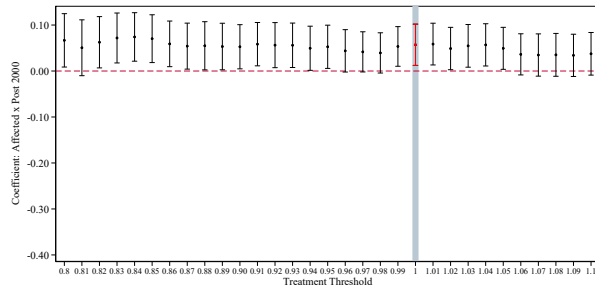
**Figure B6:** Robustness Checks: Different Threshold for Exposure to Reform



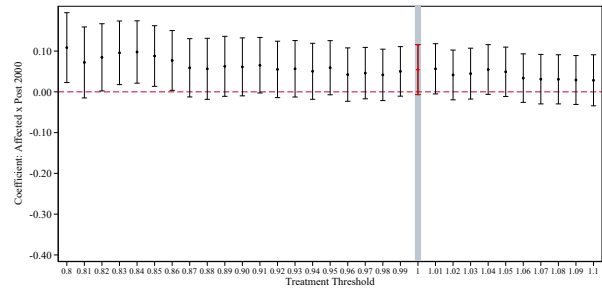
(a) Overspending Indicator



(b) Current Deficit (=1)



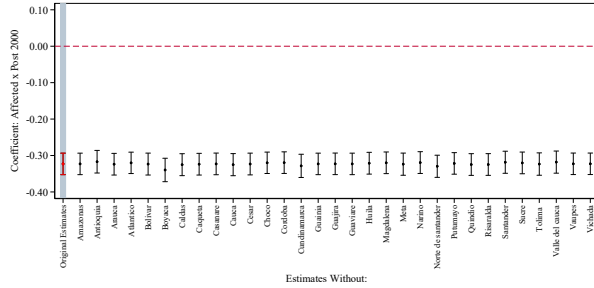
(c) Vote Share Incumbent



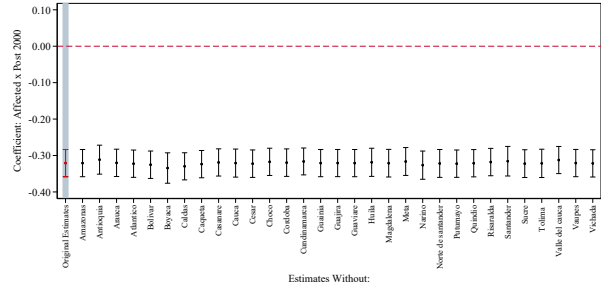
(d) Incumbent Wins (=1)

*Notes:* Each panel shows point estimates and 95% confidence intervals for  $\beta$  in equation 1 for the dependent variable in the caption as we change the threshold value of the overspending indicator used to define exposure to the fiscal rule. This indicator is defined as operating expenses divided by disposable current revenue. Our definition of exposure relies on the average value of the overspending indicator in the pre-reform period (1996-2000). Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

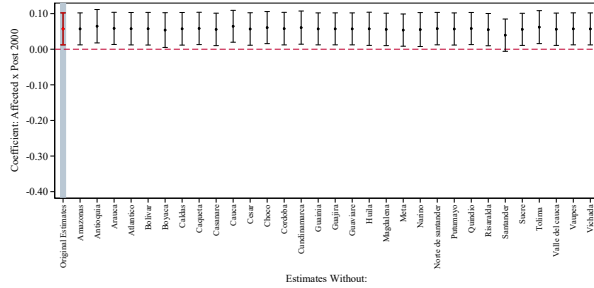
**Figure B7: Robustness Checks: Exclusion of Departments from the Sample**



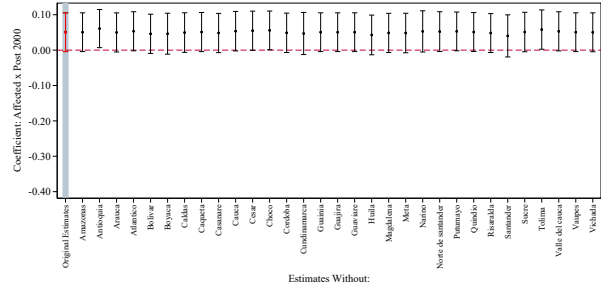
**(a) Overspending Indicator**



**(b) Current Deficit (=1)**



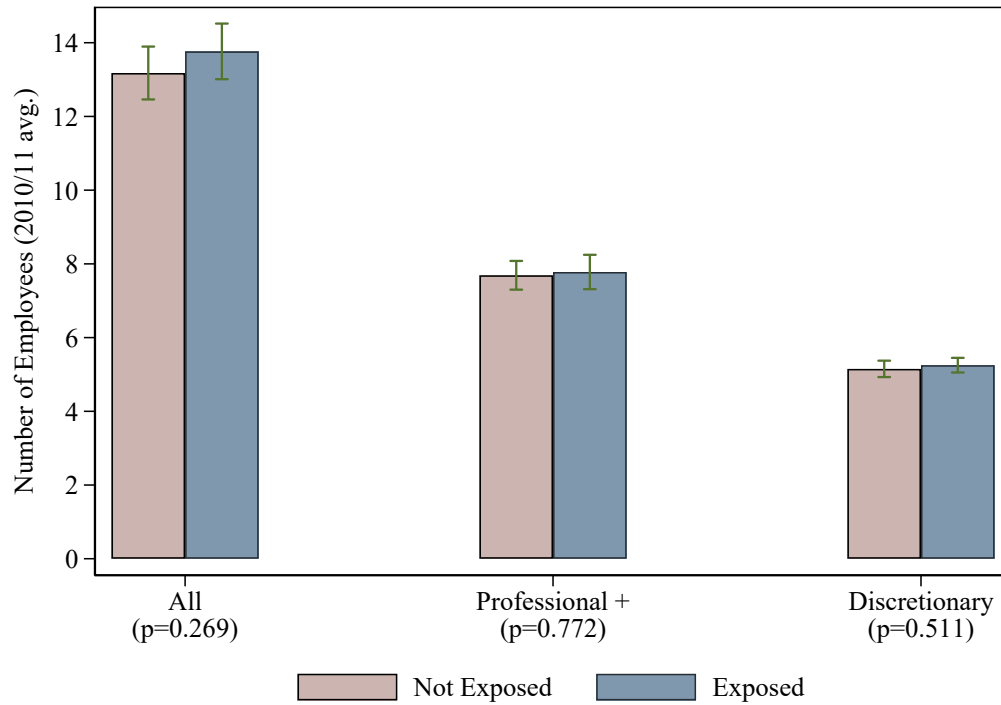
**(c) Vote Share Incumbent**



**(d) Incumbent Wins (=1)**

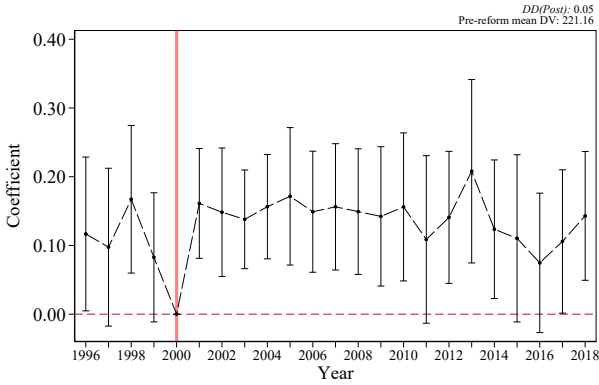
*Notes:* Each panel shows point estimates and 95% confidence intervals for  $\beta$  in equation 1 for the dependent variable in the caption as we exclude the department listed in the x-axis from the sample. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

**Figure B8:** Average Number of Municipal Employees: 2010-2011

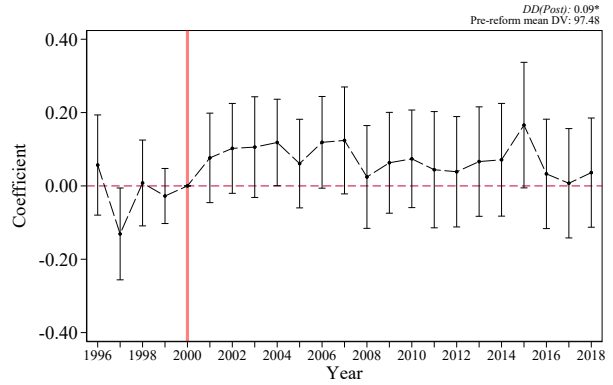


*Notes:* The figure shows the average number of municipal employees, employees with a college a degree, and employees working at the discretion of the mayor (i.e., top officials) in affected and non-affected municipalities, pooling information from 2010 and 2011. These numbers correspond to the mayor’s office and direct dependencies, and the source of information is DNP. Bars indicate the 95% confidence interval for the mean, while the p-values at the bottom correspond to the null hypothesis that the averages are equal across the two groups.

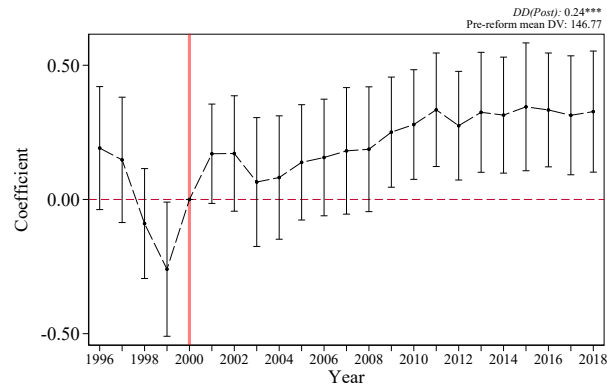
**Figure B9: Disaggregate Tax Revenue: Event Studies**



**(a) Property Tax**



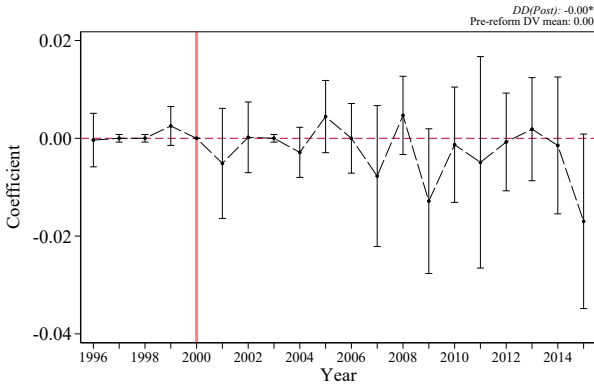
**(b) Gross Business Receipts Tax**



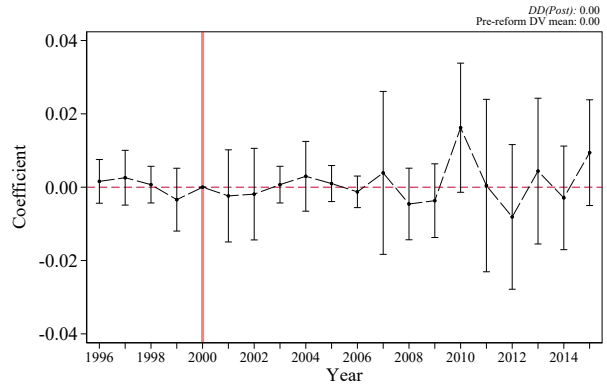
**(c) Other Taxes**

*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_{\tau}$  in equation 2. The dependent variable in panel (a) is property tax revenue, while in panel (b) it is gross business receipts tax revenue (ICA). In panel (c), the dependent variable is revenue from other taxes (vehicles, spectacles, slaughtering, construction licences and tax arrears, among others). All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

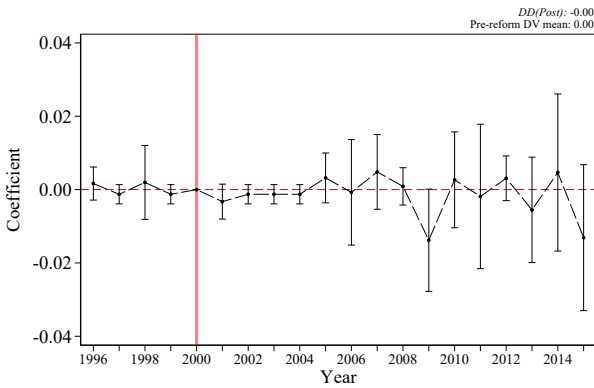
**Figure B10: Protests: Additional Event Studies**



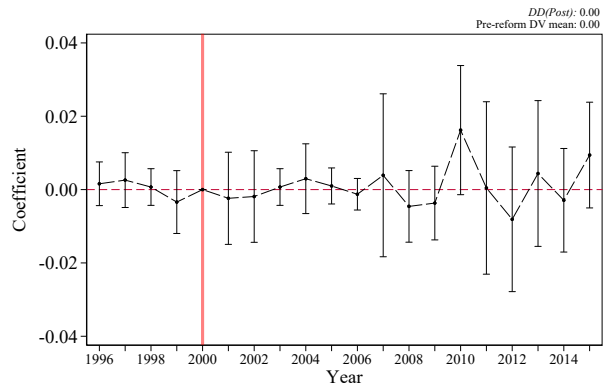
**(a)** Protests: Other Causes (=1)



**(b)** Protests: Roadblocks or Occupations (=1)



**(c)** Protests: Mass Mobilization (=1)

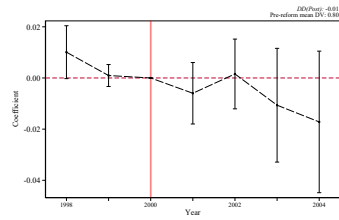


**(d)** Protests: Strike (=1)

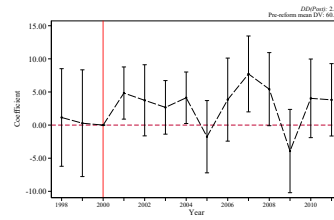
*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_\tau$  in equation 2. The dependent variable in panel (a) is an indicator taking value one for any protest against the municipal government that is not motivated by public services or labor disputes. The dependent variables in panels (b)-(d) are indicators taking value one for protests against the municipal government that take the form of roadblocks or occupations (panel b), mass mobilizations (panel c) or strikes (panel d). Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.



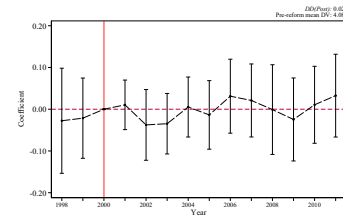
**Figure B11: Public Goods: Event Studies**



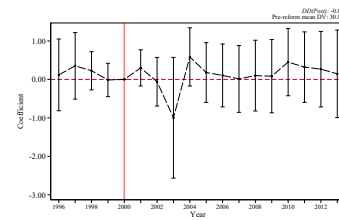
**(a) Subsidized Health Insurance**



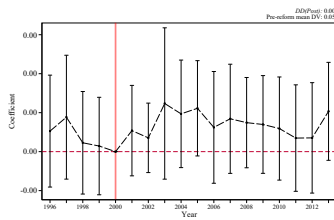
**(b) Low Birth-Weight**



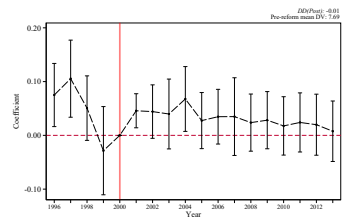
**(c) Avg. Prenatal Visits**



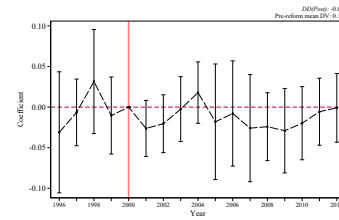
**(d) Schools per 10,000 inh.**



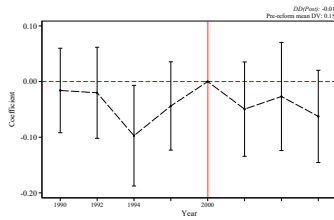
**(e) Teacher-Pupil Ratio**



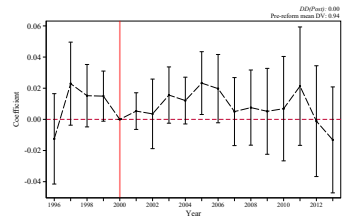
**(f) Student Enrolment**



**(g) Cadastral Update (=1)**



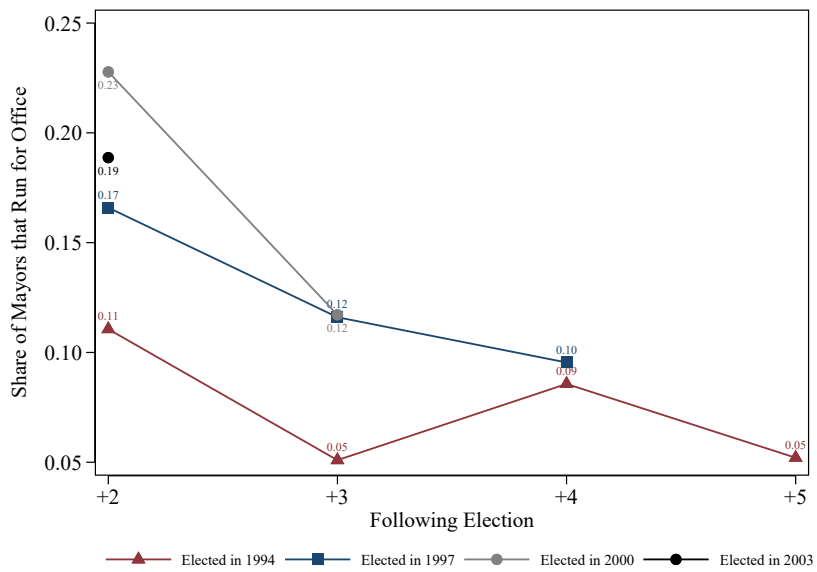
**(h) Corruption Sanctions (=1)**



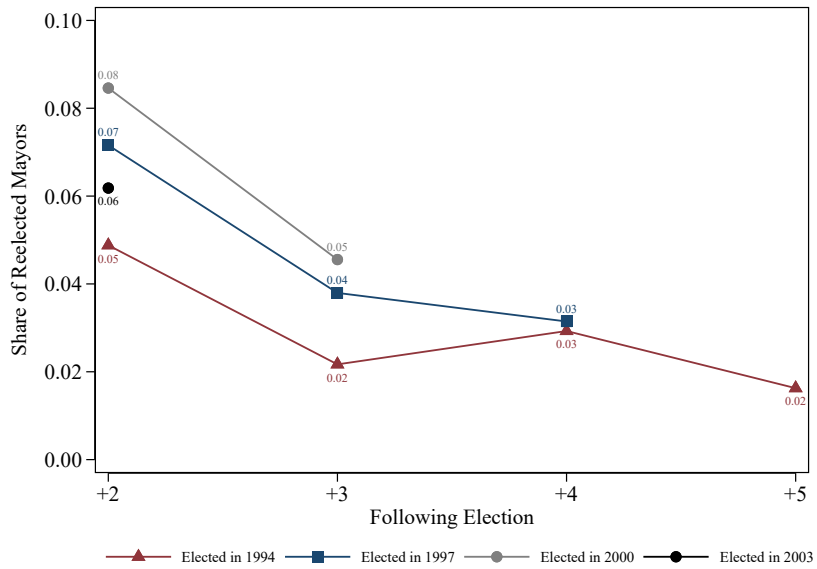
**(i) Night-time Lights**

*Notes:* Figures show point estimates and 95% confidence intervals of  $\beta_{\tau}$  in equation 2. The dependent variable in panel (a) is the share of poor population receiving subsidized health insurance. In panel (b), it is the share of newborn (per 1,000) with low birth-weight (<2,500 grams), while in panel (c) it is the average number of prenatal visits. The dependent variable in panel (d) is the number of public schools in the municipality per 10,000 inhabitants, in panel (e) it is the teacher-pupil ratio in the public sector and in panel (f) it is the logarithm of the number of students in public education (primary and early secondary). The dependent variable in panel (g) is an indicator equal to 1 if the municipality had a cadastral update on that year. In panel (h), it is an indicator equal to 1 if the mayor has been sanctioned for corruption by CGR. The dependent variable in panel (i) is the natural logarithm of the area-weighted average night-time lights Digital Number (DN). Unit of observation is municipality-year in all panels except panel (h), where it is municipality-mayoral term. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year.

**Figure B12: Future Political Outcomes of Former Mayors**



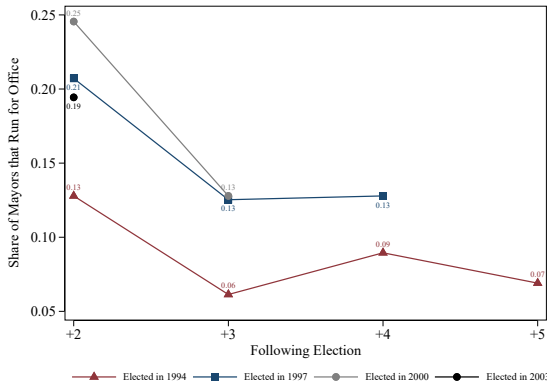
**(a) Running**



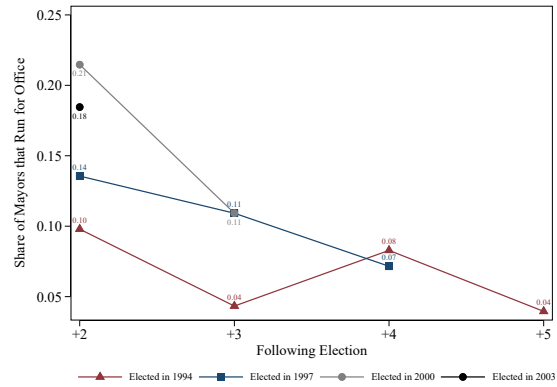
**(b) Winning**

*Notes:* Panel (a) shows the share of mayors that run for office in subsequent electoral cycles (no immediate re-election is possible because of one-term limit), disaggregated by the year in which they were elected, while panel (b) shows the share mayors that are elected in these subsequent election cycles. To construct these shares, we take the list of the elected mayors for each cycle and merge it by name (i.e., fuzzy merge) with the list of candidates for mayor and council from the subsequent electoral cycles up to 2011. We set a precision threshold of 0.9 for these matches.

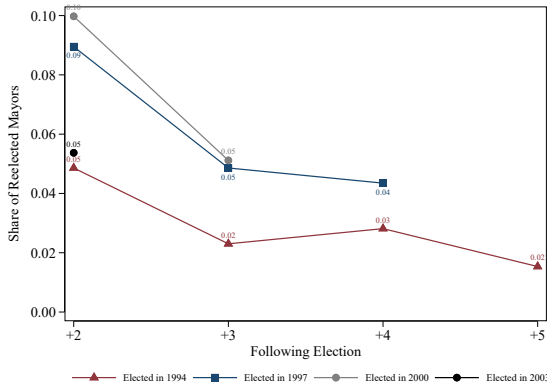
**Figure B13:** Future Political Outcomes of Former Mayors by Exposure to Fiscal Rule



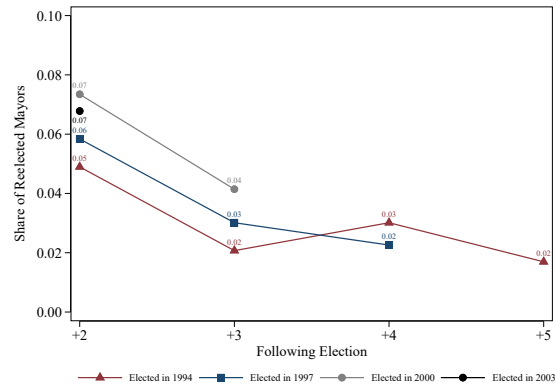
(a) Running for Office - Non-Affected



(b) Running for Office - Affected



(c) Winning - Non-Affected



(d) Winning - Affected

*Notes:* Panels (a) and (b) show the share of mayors that run for office in subsequent electoral cycles (no immediate re-election is possible because of one-term limit), disaggregated by the year in which they were elected, while panels (c) and (d) show the share mayors that are elected in these subsequent election cycles. Panels (a) and (c) correspond to municipalities deemed non-affected by the fiscal rule, while panels (b) and (d) correspond to affected municipalities. To construct these shares, we take the list of the elected mayors for each cycle and merge it by name (i.e., fuzzy merge) with the list of candidates for mayor and council from the subsequent electoral cycles up to 2011. We set a precision threshold of 0.9 for these matches.

**Table B1:** Characteristics of affected and non-affected municipalities

Regress on:	Dependent Variable: Affected (=1)			
	Bivariate Regression		w/ Department FE	
	$\beta$	SE	$\beta$	SE
	(1)	(2)	(3)	(4)
Mun. foundation year	0.001	0.000***	0.000	0.000
Area ( $km^2$ )	0.000	0.000	0.000	0.000
Altitude (10,000 metres above sea level)	-0.068	0.022***	-0.032	0.012***
Distance to dep. capital (1,000 $km$ )	0.275	0.296	-0.209	0.314
Distance to nearest market (1,000 $km$ )	0.310	0.180*	0.046	0.388
Distance to Bogota (1,000 $km$ )	0.427	0.085***	0.555	0.269**
Share of rural population (mean 1995-2000)	-0.108	0.084	0.167	0.102
Schools in 1996 (=1)	0.037	0.084	0.138	0.080*
Unmet Basic Needs index in 1993	0.003	0.001***	0.001	0.001
Notary office in 1996 (=1)	0.000	0.033	-0.013	0.036
Agricultural bank office in 1996 (=1)	0.016	0.063	0.135	0.067**
Tax collection office in 1996 (=1)	0.054	0.033	0.026	0.035
Health center or hospital in 1996 (=1)	0.038	0.037	-0.007	0.042
FARC demilitarized zone and neighbors (=1)	-0.212	0.112*	-0.156	0.120
Guerrillas presence between 1996 and 2000 (=1)	0.070	0.034**	0.045	0.040
Paramilitary presence between 1996 and 2000 (=1)	0.069	0.034**	-0.072	0.041*
Coca crops between 1999 and 2000 (=1)	0.037	0.042	0.057	0.047
Share of votes for Liberal Party (mean 1997-2000)	0.007	0.048	-0.122	0.050**
Share of votes for Conservative Party (mean 1997-2000)	-0.066	0.052	0.064	0.057
Mayoral elections HH index (mean 1997-2000)	0.182	0.167	0.285	0.169*

*Notes:* Columns 1-2 show point estimates and standard errors from univariate cross-sectional regressions of the indicator for exposure to the fiscal rule on each of the variables listed in the table. Columns 3-4 additionally include department fixed effects. All variables considered are measured before the introduction of the fiscal rule in 2001. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B2:** Robustness Checks: Alternative Exposure Measures

	Baseline	Excluding one year from calculation					60% rule	Continuous measure
		1996	1997	1998	1999	2000		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Overspending Indicator</i>								
Affected $\times \mathbb{1}[t > 2000]$	-0.32*** (0.015)	-0.30*** (0.022)	-0.31*** (0.017)	-0.30*** (0.017)	-0.30*** (0.018)	-0.30*** (0.015)	-0.26*** (0.017)	-0.89*** (0.026)
DV Mean	1.07	1.07	1.07	1.07	1.07	1.07	1.07	1.07
DV Std. Dev.	0.38	0.38	0.38	0.38	0.38	0.38	0.38	0.38
<i>Panel B: Current Deficit (=1)</i>								
Affected $\times \mathbb{1}[t > 2000]$	-0.32*** (0.019)	-0.32*** (0.022)	-0.30*** (0.020)	-0.30*** (0.021)	-0.27*** (0.021)	-0.30*** (0.019)	-0.28*** (0.018)	-0.69*** (0.042)
DV Mean	0.66	0.66	0.66	0.66	0.66	0.66	0.66	0.66
DV Std. Dev.	0.47	0.47	0.47	0.47	0.47	0.47	0.47	0.47
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,569	18,404	18,534	18,477	18,516	18,400	18,569	18,569

*Notes:* This table shows estimates of  $\beta$  in equation 1 for different version of our measure of exposure to the fiscal rule. In all cases, our measure is based on the average value of the overspending indicator in the pre-reform period. In column 1 we define exposure as an indicator equal to one if the average value of the overspending indicator between 1996 and 2000 takes a value of one or higher. In columns 2-6, we replicate the analysis excluding the year in the header from the construction of the average. In column 7, we defined municipalities as exposed if the overspending indicator takes a value larger than 1 in at least three of the five pre-reform years (i.e. 60%), while in column 8 we use the continuous measure instead (i.e., 1996-2000 average of overspending indicator). The dependent variable in Panel A is the overspending indicator, defined as operating costs over disposable current revenue, while in Panel B it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B3:** Robustness Checks: Per Capita Fiscal Outcomes

	Operating Expenses				Disposable Current Revenue			
	Total	Personnel	General	Paid Transfers	Total	Tax Revenue	Non-Tax Revenue	Disposable Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected $\times \mathbb{1}[t > 2000]$	-0.18*** (0.020)	-0.13*** (0.023)	-0.22*** (0.031)	-0.14** (0.061)	0.10*** (0.018)	0.16*** (0.037)	0.33*** (0.057)	0.10*** (0.029)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,569	18,569	18,569	18,569	18,569	18,569	18,569	18,569
DV Mean	1185.73	566.84	417.07	242.63	1178.55	339.82	237.62	593.10
DV Std. Dev.	767.23	1507.88	648.16	393.87	744.94	458.93	384.81	312.61

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is operating expenses, while in column 5 it is disposable current revenue. Columns 2-4 correspond to the sub-components of operating expenses: personnel expenses, general expenses (i.e., procurement), and paid transfers (mostly pensions and payments from legal rulings). Columns 6-8 correspond to the sub-components of disposable current revenue: Tax revenue, Non-tax revenue (i.e., fees and fines), and disposable SGP transfers from the central government. All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos per 10,000 inhabitants. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B4:** Robustness Checks: Non-Winsorized Outcomes

	Overspending Indicator	Operating Expenses	Disposable Current Revenue
	(1)	(2)	(3)
Affected $\times \mathbb{1}[t > 2000]$	-0.73*** (0.254)	-0.22*** (0.019)	0.08*** (0.018)
Municipality FE	✓	✓	✓
Department-year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	18,569	18,569	18,569
DV Mean	1.25	1504.77	1388.50
DV Std. Dev.	6.73	4578.19	1124.32

*Notes:* This table shows estimates of  $\beta$  in equation 1 when we do not winsorize the main fiscal indicators. The dependent variable in columns 1 is the overspending indicator, defined as operating costs over disposable current revenue. The dependent variable in column 2 is operating expenses, while in column 3 it is disposable current revenue. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B5:** Robustness Checks: Excluding Non-Category 6 Municipalities

	Overspending Indicator		Current Deficit (=1)	
	(1)	(2)	(3)	(4)
Affected $\times \mathbb{1}[t > 2000]$	-0.31*** (0.017)	-0.31*** (0.017)	-0.34*** (0.023)	-0.33*** (0.024)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls		✓		✓
Observations	9,887	9,887	9,887	9,887
DV Mean	1.07	1.07	0.65	0.65
DV Std. Dev.	0.37	0.37	0.48	0.48

Notes: *Notes:* This table shows estimates of  $\beta$  in equation 1 as we exclude from the sample municipalities that are not classified in category six at any point between 2003 and 2018. The dependent variable in columns 1-2 is the overspending indicator, defined as operating costs over disposable current revenue, while in columns 3-4 it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects. In columns 2 and 4 we also include year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .



**Table B6:** Robustness Checks: Excluding 1999 and 2000

	Overspending Indicator		Current Deficit (=1)	
	(1)	(2)	(3)	(4)
Affected $\times \mathbb{1}[t > 2000]$	-0.34*** (0.019)	-0.34*** (0.019)	-0.34*** (0.022)	-0.33*** (0.022)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls		✓		✓
Observations	16,789	16,789	16,789	16,789
DV Mean	1.06	1.06	0.69	0.69
DV Std. Dev.	0.35	0.35	0.46	0.46

*Notes:* This table shows estimates of  $\beta$  in equation 1 as we exclude years 1999 and 2000 from the sample. The dependent variable in columns 1-2 is the overspending indicator, defined as operating costs over disposable current revenue, while in columns 3-4 it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects. In columns 2 and 4 we also include year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B7:** Robustness Checks: Controls for Other Reforms

	Overspending Indicator					Current Deficit (=1)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Affected $\times \mathbb{1}[t > 2000]$	-0.32*** (0.015)	-0.32*** (0.015)	-0.32*** (0.015)	-0.32*** (0.015)	-0.32*** (0.015)	-0.32*** (0.019)	-0.32*** (0.019)	-0.32*** (0.019)	-0.32*** (0.019)	-0.32*** (0.019)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Control for SGP Transfers		✓					✓			
Control for Certified Municipalities			✓					✓		
Dropped New Municipalities				✓					✓	
Control for Law 550/99					✓					✓
Observations	18,569	18,569	18,569	17,860	18,569	18,569	18,569	18,569	17,860	18,569
DV Mean	1.07	1.07	1.07	1.07	1.07	0.66	0.66	0.66	0.66	0.66
DV Std. Dev.	0.38	0.38	0.38	0.38	0.38	0.47	0.47	0.47	0.47	0.47

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in columns 1-5 is the overspending indicator, defined as operating costs over disposable current revenue, while in columns 6-10 it is an indicator equal to one if the municipal government experiences a current deficit. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Columns 1 and 6 replicate our baseline analysis. In columns 2 and 7, we also control for total SGP transfers per capita. Columns 3 and 8 include as an additional control an indicator equal one if the municipality becomes certified to manage its own SGP transfers for education. In columns 4 and 9 we exclude all municipalities created between 1986 and 2018 from the sample. Columns 5 and 10 include as an additional control an indicator that turns on when a municipality starts a restructuring of liabilities process, in the context of Law 550/1999. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B8:** Heterogeneous Effects: Law 550 of 1999

	Overspending Indicator		Current Deficit (=1)		Disposable Current Revenue		Operating Expenses	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Affected $\times \mathbb{1}[t > 2000]$	-0.32*** (0.015)	-0.32*** (0.015)	-0.32*** (0.019)	-0.32*** (0.019)	0.07*** (0.017)	0.07*** (0.018)	-0.21*** (0.018)	-0.21*** (0.018)
Affected $\times \mathbb{1}[t > 2000] \times$ Law 550 (=1)		-0.07*** (0.027)		-0.02 (0.024)		-0.02 (0.028)		-0.09*** (0.030)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,569	18,569	18,569	18,569	18,569	18,569	18,569	18,569
DV Mean	1.07	1.07	0.66	0.66	1387.27	1387.27	1395.47	1395.47
DV Std. Dev.	0.38	0.38	0.47	0.47	1097.34	1097.34	1038.36	1038.36

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in columns 1-2 is the overspending indicator, defined as operating costs over disposable current revenue, while in columns 3-4 it is an indicator equal to one if the municipal government experiences a current deficit. The dependent variable in column 5-6 is disposable current revenue, while in column 5-8 it is operating expenses. The outcome in columns 5-8 corresponds to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Even-numbered columns allow the effect of the fiscal rule in equation 1 to vary after a municipality subscribes a liability restructuring program, in the context of Law 550/1999. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B9:** Tax Revenues Sub-Components

	Property	Gross Receipts	Other
	(1)	(2)	(3)
Affected $\times \mathbb{1}[t > 2000]$	0.05 (0.031)	0.09* (0.051)	0.24*** (0.062)
Municipality FE	✓	✓	✓
Department-year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	18,569	18,569	18,569
DV Mean	221.16	97.48	146.73
DV Std. Dev.	287.41	331.62	314.39

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is property tax revenue, while in column 2 it is gross business receipts tax revenue, and in column 3 it is revenue from other taxes (e.g., gasoline surcharge). All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B10:** Central Government SGP Transfers and Sub-Components

	Total	Education	Health	General Purpose
	(1)	(2)	(3)	(4)
Affected $\times \mathbb{1}[t > 2000]$	0.01 (0.012)	-0.04 (0.025)	-0.01 (0.020)	0.04* (0.023)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	18,569	18,569	18,569	18,569
DV Mean	3356.10	788.72	657.27	1824.69
DV Std. Dev.	2076.06	519.44	432.87	1052.61

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is the total SGP transfers, while in column 2 it is the education SGP transfers, in column 3 it is the health SGP transfers, and in column 4 it is general purpose SGP transfers. All outcomes correspond to the natural logarithm of the monetary value in constant 2010 Colombian pesos. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B11:** Other Characteristics of Mayoral Elections

	Number of Candidates	Golosov Index	HHI	Margin of Victory	Winner's Vote Share	Total Votes
	(1)	(2)	(3)	(4)	(5)	(6)
Affected $\times \mathbb{1}[t > 2000]$	-0.01 (0.019)	-0.00 (0.010)	0.00 (0.003)	0.00 (0.006)	0.00 (0.004)	-.006*** (0.018)
Municipality FE	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Observations	4,894	4,895	4,894	4,889	7,339	7,343
DV Mean	3.24	2.09	0.28	0.20	0.54	4297.70
DV Std. Dev.	1.44	0.65	0.14	0.20	0.13	3924.33

*Notes:* This table shows estimates of  $\beta$  in equation 1. All outcomes correspond to mayoral elections. The dependent variable in column 1 is the number of candidates. In column 2, it is the Golosov's index of effective parties, while in column 3 it is the Herfindahl–Hirschman concentration index. The dependent variable in column 4 is the margin of victory, and in column 5 it is the vote share for the elected mayor. In column 6 the dependent variable is the log of the total number of votes at the municipality-election level. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B12:** Party Vote Shares

	Vote Share				
	Conservative Party	Liberal Party	Party in Power in 2000	Incumbent Party	Incumbent Wins (=1)
	(1)	(2)	(3)	(4)	(5)
Affected $\times \mathbb{1}[t > 2000]$	0.03** (0.015)	0.01 (0.010)	0.01 (0.015)	0.05*** (0.019)	0.05* (0.025)
Municipality FE	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Party FE				✓	✓
PS Weighting					
Observations	4,895	4,895	4,691	4,706	6,317
DV Mean	0.28	0.39	0.57	0.48	0.53
DV Std. Dev.	0.35	0.38	0.35	0.39	0.50

*Notes:* This table shows estimates of  $\beta$  in equation 1. All outcomes correspond to mayoral elections. The dependent variable in column 1 is vote share for the Conservative Party, while in column 2 it is the vote share for the Liberal Party. In column 3, it is vote share for the party in office in 2000, when the fiscal reform was introduced. The dependent variable in column 4 is the share of votes for the incumbent party in the mayoral election, while in column 5 it is an indicator equal to one if the incumbent party wins the election. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Regressions in column 4 and 5 include incumbent party fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table B13:** Robustness Checks: Different Threshold for Corruption Match Precision

	DV: Corruption Sanctions (=1)			
	Name Matching Scores			
	70/100	80/100	90/100	100/100
	(1)	(2)	(3)	(4)
Affected $\times \mathbb{1}[t > 2000]$	-0.02 (0.017)	-0.01 (0.017)	-0.01 (0.016)	-0.00 (0.015)
Municipality FE	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	6,499	6,499	6,499	6,499
DV Mean	0.23	0.19	0.15	0.10
DV Std. Dev.	0.42	0.39	0.36	0.30

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable is an indicator equal to 1 if the mayor has been sanctioned for corruption by CGR. We construct this outcome by matching the names of mayors with those in the list of sanctioned individuals in the bulletins published by CGR. In each column, we change the threshold value of the precision score used to determine a match. Unit of observation is municipality-mayoral term. Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .



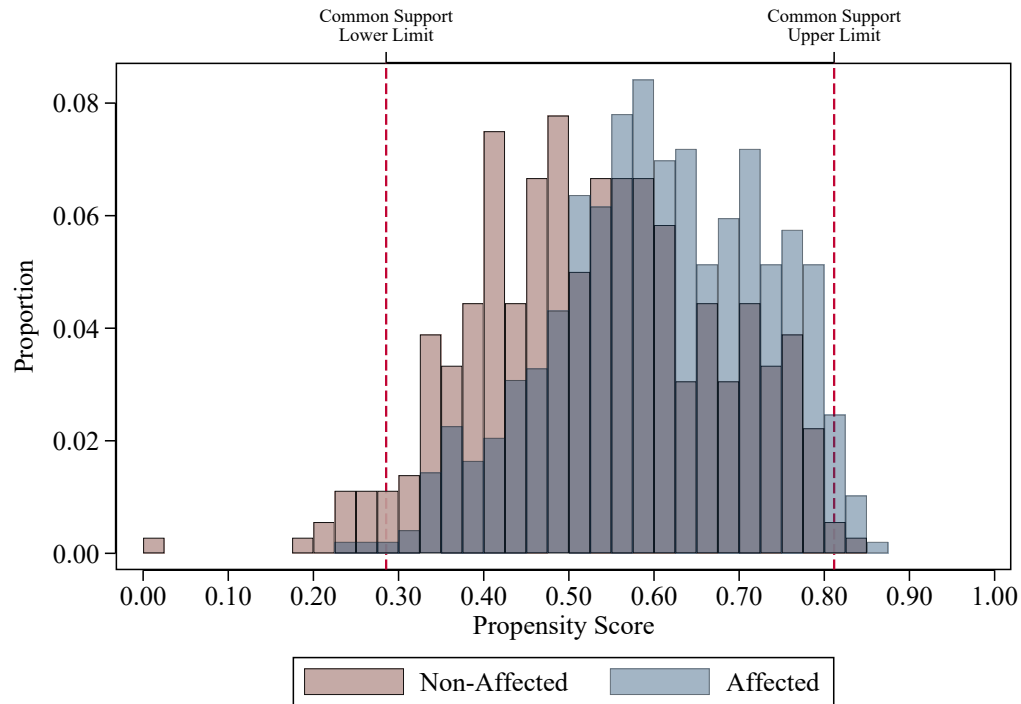
**Table B14: Other Corruption Measures**

	Mayor Sanctions (=1)		Mayor's Party Sanctions (=1)	Municipality Sanctions (=1)	Candidate Sanctions (=1)
	Before Term	After Term			
	(1)	(2)	(3)	(4)	(5)
Affected $\times \mathbb{1}[t > 2000]$	-0.00 (0.015)	-0.00 (0.004)	0.00 (0.017)	0.01 (0.006)	-0.03 (0.021)
Municipality FE	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	6,499	6,499	6,878	21,944	4,050
DV Mean	0.15	0.01	0.43	0.05	0.30
DV Std. Dev.	0.35	0.08	0.49	0.21	0.46

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is an indicator equal to 1 if the mayor was sanctioned for corruption by CGR before his term in office, while in column 2 it is an indicator equal to 1 if the mayor was sanctioned for corruption by CGR after his term. The dependent variable in column 3 is an indicator equal to 1 if the party in office has been implicated in a corruption case in that municipality ever, in column 4 it is an indicator equal to 1 if the mayor's office was sanctioned for corruption by CGR, and in column 5 it is an indicator equal to 1 if the any candidate for mayor was sanctioned for corruption by CGR. Unit of observation is municipality-mayoral term, except in column 4 (municipality-year). Regressions include municipality and department-year fixed effects, as well as year fixed effects interacted with predetermined municipal characteristics: altitude, distance to Bogotá, presence of at least one school in 1996, presence of at least one agricultural bank office in 1996, paramilitary presence between 1996 and 2000, average vote share for the Liberal party between 1997 and 2000, and mayoral elections average HHI during the 1997 and 2000 election cycle. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

## C Propensity Score Weighting

Figure C1: Common Support in the Propensity Score for Exposure to Fiscal Rule



*Notes:* This figure shows the distribution of propensity scores for fiscal rule exposure, disaggregated by actual exposure. Our exposure measure is an indicator equal to 1 for municipalities with an average value of the overspending indicator in 1996-2000 that exceeds 1. The overspending indicator is defined as operating expenses divided by disposable current revenue. The propensity scores are fitted values from a Probit regression of fiscal rule exposure on the 20 pre-determined municipal characteristics in Table C1.

**Table C1:** Characteristics of affected and non-affected municipalities

Regress on:	Dependent Variable: Affected (=1)			
	Bivariate Regression		w/ Department FE	
	$\beta$	SE	$\beta$	SE
	(1)	(2)	(3)	(4)
Mun. foundation year	-0.000	0.000	-0.000	0.000
Area ( $km^2$ )	0.000	0.000	-0.000	0.000
Altitude (10,000 metres above sea level)	0.049	0.222	0.490	0.316
Distance to dep. capital (1,000 $km$ )	-0.189	0.386	-0.618	0.463
Distance to nearest market (1,000 $km$ )	0.032	0.226	-0.079	0.533
Distance to Bogota (1,000 $km$ )	0.011	0.112	0.204	0.391
Share of rural population (mean 1995-2000)	-0.051	0.100	0.125	0.126
Schools in 1996 (=1)	-0.084	0.144	0.124	0.129
Unmet Basic Needs index in 1993	-0.001	0.001	-0.001	0.001
Notary office in 1996 (=1)	0.019	0.039	0.001	0.043
Agricultural bank office in 1996 (=1)	0.006	0.098	0.079	0.098
Tax collection office in 1996 (=1)	0.012	0.039	-0.011	0.040
Health center or hospital in 1996 (=1)	0.008	0.045	-0.029	0.049
FARC demilitarized zone and neighbors (=1)	-0.065	0.153	0.060	0.159
Guerrillas presence between 1996 and 2000 (=1)	-0.030	0.041	-0.039	0.046
Paramilitary presence between 1996 and 2000 (=1)	-0.022	0.040	-0.140	0.046***
Coca crops between 1999 and 2000 (=1)	-0.044	0.050	0.041	0.059
Share of votes for Liberal Party (mean 1997-2000)	0.032	0.055	-0.071	0.056
Share of votes for Conservative Party (mean 1997-2000)	-0.012	0.058	0.031	0.063
Mayoral elections HH index (mean 1997-2000)	-0.158	0.177	0.005	0.176

*Notes:* \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ . All variables are measured before the reform. Coefficient estimates and standard errors shown in the table above are from a regression of the *Affected* indicator on each of the municipal characteristics shown above. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003).

**Table C2:** Main Fiscal Outcomes and Sub-components of the Overspending Indicator

	Main Outcomes		Operating Expenses				Disposable Current Revenue			
	Overspending Indicator	Current Deficit (=1)	Total	Personnel	General	Paid Transfers	Total	Tax Revenue	Non-Tax Revenue	Disposable Transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Affected $\times 1[t > 2000]$	-0.33*** (0.016)	-0.31*** (0.019)	-0.22*** (0.021)	-0.17*** (0.024)	-0.26*** (0.033)	-0.16** (0.063)	0.07*** (0.019)	0.10** (0.041)	0.29*** (0.065)	0.07** (0.033)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Propensity Score Weighting	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,003	18,003	18,003	18,003	18,003	18,003	18,003	18,003	18,003	18,003
DV Mean	1.07	0.66	1395.47	714.90	479.22	310.65	1387.27	465.40	281.48	641.61
DV Std. Dev.	0.38	0.47	1038.36	3094.41	1158.44	538.54	1097.34	690.04	482.99	355.46

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is the overspending indicator, defined as operating costs over disposable current revenue, while in column 2 it is an indicator equal to one if the municipal government experiences a current deficit. The dependent variable in column 3 is operating expenses, while in column 7 it is disposable current revenue. Columns 4-6 correspond to the sub-components of operating expenses: personnel expenses, general expenses (i.e., procurement), and paid transfers (mostly pensions and payments from legal rulings). Columns 8-10 correspond to the sub-components of disposable current revenue: Tax revenue, Non-tax revenue (i.e., fees and fines), and disposable SGP transfers from the central government. All monetary outcomes correspond to the natural logarithm of the value in constant 2010 Colombian pesos. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003). Regressions include municipality and department-year fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table C3:** Other Fiscal Outcomes

	Capital Revenue	Capital Expenses	Total Deficit (=1)	Net Credit Inflows (=1)	Interest Payments	Positive Balance (=1)
	(1)	(2)	(3)	(4)	(5)	(6)
Affected $\times \mathbb{1}[t > 2000]$	0.00 (0.027)	0.05 (0.031)	-0.11*** (0.024)	0.00 (0.019)	-0.05 (0.127)	0.12*** (0.024)
Municipality FE	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓
Propensity Score Weighting	✓	✓	✓	✓	✓	✓
Observations	18,003	18,003	18,003	18,003	18,003	18,003
DV Mean	3919.67	3894.68	0.56	0.38	463.10	0.48
DV Std. Dev.	3545.80	3243.00	0.50	0.48	13578.32	0.50

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is capital revenue, in column 2 it is capital expenses, in column 3 it is an indicator equal to one if the municipal government experiences a total deficit, in column 4 it is an indicator equal to one if the municipal government experiences positive net credit inflows, in column 5 it is interest payments, and in column 6 it is an indicator equal to one if the municipal government experiences a net increase in wealth. All monetary outcomes correspond to the natural logarithm of the value in constant 2010 Colombian pesos. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003). Regressions include municipality and department-year fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table C4:** Performance of Incumbent Mayor’s Party in Next Election

	Vote Share	Wins (=1)
	(1)	(2)
Affected $\times \mathbb{1}[t > 2000]$	0.06** (0.026)	0.04 (0.031)
Municipality FE	✓	✓
Department-year FE	✓	✓
Propensity Score Weighting	✓	✓
Observations	4,604	6,174
DV Mean	0.48	0.53
DV Std. Dev.	0.39	0.50

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is the share of votes for the incumbent party in the mayoral election, while in column 2 it is an indicator equal to one if the incumbent party wins the election. Regressions include municipality and department-year fixed effects. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003). Regressions include municipality and department-year fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table C5: Protests Against the Municipal Government**

	Any Protest (=1)	Cause (=1)			Type (=1)		
		Public Services	Labor Disputes	Other	Mass Mobilization	Strike	Roadblock / Occupation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Affected $\times \mathbb{1}[t > 2000]$	-0.004 (0.0043)	0.006* (0.0034)	-0.008*** (0.0029)	-0.002 (0.0019)	-0.001 (0.0023)	-0.008*** (0.0030)	0.004 (0.0032)
Municipality FE	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓
Propensity Score Weighting	✓	✓	✓	✓	✓	✓	✓
Observations	16,360	16,360	16,360	16,360	16,360	16,360	16,360
DV Mean	0.010	0.004	0.005	0.001	0.002	0.006	0.003
DV Std. Dev.	0.098	0.065	0.070	0.031	0.041	0.078	0.051

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in all columns is an indicator taking the value of one if protests against the municipal government took place in the municipality-year. In column 1, any protest. In columns 2-4, protests related to a specific cause: local public services, labor disputes or breach of agreements, other (e.g., human rights violations). In columns 5-7, specific types of protest: mass mobilizations, strikes, or roadblocks or occupation of buildings. Regressions include municipality and department-year fixed effects. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003). Regressions include municipality and department-year fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .

**Table C6: Public Goods and Living Standards**

	Health Outcomes			Education Outcomes			Other Outcomes		
	Subsidized Health Insurance	Low Birth-Weight	Average Prenatal Visits	Schools per 10,000 inh.	Teacher-Pupil Ratio	Student Enrollment	Cadastral Update (=1)	Corruption Sanctions (=1)	Night-time Lights
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Affected $\times \mathbb{1}[t > 2000]$	-0.01 (0.009)	3.79** (1.665)	-0.01 (0.040)	0.08 (0.455)	-0.00 (0.001)	-0.04 (0.029)	-0.00 (0.012)	-0.01 (0.022)	0.00 (0.010)
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Department-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Propensity Score Weighting	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	5,726	11,451	11,450	14,683	14,681	14,681	13,906	6,303	14,724
DV Mean	0.80	60.89	4.08	30.89	0.05	7.69	0.13	0.15	0.94
DV Std. Dev.	0.42	36.41	0.96	15.54	0.01	1.13	0.34	0.36	0.75
Sample first year	1998	1998	1998	1996	1996	1996	1996	1990	1996
Sample final year	2004	2011	2011	2013	2013	2013	2012	2011	2013

*Notes:* This table shows estimates of  $\beta$  in equation 1. The dependent variable in column 1 is the share of poor population receiving subsidized health insurance. In column 2, it is the share of newborn (per 1,000) with low birth-weight (<2,500 grams), while in column 3 it is the average number of prenatal visits. The dependent variable in column 4 is the number of public schools in the municipality per 10,000 inhabitants, in column 5 it is the teacher-pupil ratio in the public sector and in column 6 it is the logarithm of the number of students in public education (primary and early secondary). The dependent variable in column 7 is an indicator equal to 1 if the municipality had a cadastral update on that year. In column 8, it is an indicator equal to 1 if the mayor has been sanctioned for corruption by CGR. The dependent variable in column 9 is the natural logarithm of the area-weighted average night-time lights Digital Number (DN). Unit of observation is municipality-year in all columns except column 8, where it is municipality-mayoral term. Regressions include municipality and department-year fixed effects. In each regression, we restrict the sample to municipalities in the common support of the propensity score (shown in Appendix Figure C1), and we weight the control observations by a non-parametric function of the propensity score (Hirano et al., 2003). Regressions include municipality and department-year fixed effects. Standard errors clustered two-way by municipality and department-year in brackets. \*  $p \leq 0.1$ , \*\*  $p \leq 0.05$ , \*\*\*  $p \leq 0.01$ .



## D A Model of Political Accountability

**Introduction:** In this section, we present a stylized model of political agency that helps to explain our findings on the political effects of the fiscal rule. In the model, an incumbent politician (i.e., the mayor) observes available revenue and makes a decision on public spending. A representative voter then observes the resulting combination of public revenue and spending and decides whether to re-elect the incumbent party. There are two types of politicians, congruent and dissonant, and we assume that different parties have varying shares of the two types. These shares are unknown to the voter, but she can update her beliefs by observing the performance of the incumbent. In the absence of the fiscal rule, the game has a semi-separating equilibrium that allows the voter to determine the type of the incumbent mayor, learn about party quality and potentially vote for the party that is not in power. The introduction of the fiscal rule leads to a pooling equilibrium. This prevents learning, but also eliminates the agency problem, thereby reducing the incentive to vote the incumbent out of office.

Our model mimics several important features of the Colombian context. First, individual politicians face a one-term limit in office, which drastically reduces the disciplining effect of elections (Ashworth, 2012). Second, and partly as a consequence of the previous point, parties are weak and cannot constrain the behavior of the incumbent (Klašnja and Titunik, 2017). Third, we focus exclusively on fiscal outcomes and abstract away from the provision of public goods, in line with the idea that funding for public goods is earmarked and there is little room for discretion by the incumbent.

**Set-up:** This is a two-period model with an election in-between.<sup>23</sup> For tractability, we assume a very simple policy environment. Each period, public revenue ( $r$ ) takes two possible values,  $\{r_H, r_L\}$ , such that  $r_H > r_L$ . The probability that revenue is high is given by  $q \in (0, 1)$ . Government spending ( $g$ ) also takes two possible values,  $\{g_H, g_L\}$ , such that  $r_H = g_H > g_L = r_L$ . Government spending is chosen each period by the incumbent mayor after observing the available level of revenue. It is possible to spend less than the available revenue (i.e.,  $g_L < r_H$ ) or to spend beyond available resources ( $g_H > r_L$ ) in which case the government incurs in a deficit.

At the end of the first period, the voter observes the outcome dyad (r,g) and decides whether to re-elect the incumbent party or to replace it. Importantly, there is a one-term limit at the individual level. We assume that the representative voter prefers high spending

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<sup>23</sup>Due to its recursive nature (i.e. every period there is a new incumbent that faces a one-term limit), the model can be easily extended to  $T > 1$  periods. The equilibria described below for the cases with and without fiscal rules remain unchanged, as long as we assume that the voter is not dynamically sophisticated and simply chooses the statically optimal strategy.

when revenue is high and low spending when revenue is low:

$$u(g_H|r_H) > u(g_L|r_H)$$

$$u(g_L|r_L) > u(g_H|r_L)$$

These preferences could reflect the fact that while the voter benefits from higher public spending, she internalizes the future cost of the fiscal adjustment necessary to remedy a deficit, which we do not explicitly include in the model.

There are two types of politicians: congruent and dissonant. Congruent politicians share the preferences of the voter. Dissonant politicians always prefer high spending to low spending, irrespective of the level of revenue. The incumbent derives a benefit  $b > 0$  from being in office (e.g., ego rents). The type of each politician is known to him, but is unobservable to the voter. The share of congruent politicians differs across political parties and is given by  $\theta_i \in (0, 1)$ , where  $i$  denotes the party. This variation could reflect differences in the quality of screening across parties or in their ability to monitor or punish misbehavior. For simplicity, we assume that the number of parties is fixed and equal to two, which we denote as A and B. Each period, candidates are drawn i.i.d. from the Bernoulli distribution corresponding to their party, with respective parameters  $\theta_A$  and  $\theta_B$ . These parameters are not known by the voter. We assume that both parties only care about winning elections and are thus willing to implement policy in accordance with the preferences of the voter, but they are weak and unable to control the behavior of elected candidates once in office.

We assume that the voter's prior on  $\theta_i$  follows a Beta distribution with hyperparameters  $\alpha_i > 0$  and  $\beta_i > 0$ , for  $i = A$  and B. As such,  $E[\theta_i] = \frac{\alpha_i}{\alpha_i + \beta_i}$ . This functional form has several advantages. First, it imposes very little structure and captures a wide range of possible beliefs. For instance,  $\alpha_i = \beta_i = 1$  corresponds to a uniform prior, such that  $E[\theta_i] = \frac{1}{2}$ . Second, the Beta-distributed prior implies that the posterior distribution after the acquisition of information based on  $r$  and  $g$  will also follow a Beta distribution with hyperparameters  $\alpha'_i$  and  $\beta'_i$ . In particular, if the realization is a success (i.e. the incumbent mayor revealed as congruent), then  $\alpha'_i = \alpha_i + 1$ , while if the incumbent is revealed as dissonant, then  $\beta'_i = \beta_i + 1$ . If no information is acquired, either because the party was not in power or because the policy outcome is uninformative about the type, then  $\alpha_i = \alpha'_i$  and  $\beta_i = \beta'_i$ .

**Equilibrium without fiscal rule:** The equilibrium concept is Perfect Bayesian Equilibrium. Since the incumbent mayor has no re-election incentives, he chooses his most-preferred policy in both periods. The congruent mayor chooses high spending if revenue is high and low spending if revenue is low, in accordance with the preferences of the voter. The dissonant mayor chooses high spending irrespective of the amount of revenue. As a result, there are

three possible policy outcomes along the equilibrium path:  $(r_H, g_H)$ ,  $(r_L, g_H)$ ,  $(r_L, g_L)$ . If the outcome is  $(r_L, g_H)$ , then the voter knows with certainty that the incumbent is dissonant and updates negatively about  $\theta_I$ , the share of congruent politicians in the incumbent party, as described above. Likewise, if the outcome is  $(r_L, g_L)$ , then the voter knows with certainty that the incumbent is congruent and updates positively on  $\theta_I$ . Both types choose  $g_H$  if revenue is high, so there is no updating in this case. If the voter observes  $(r_H, g_L)$  (which never happens along the equilibrium path), we assume that the voter believes the incumbent to be dissonant. By construction, the voter prefers a congruent politician to a dissonant one. Hence, the voter chooses the party with the highest expected share of congruent politicians based on her posterior beliefs on  $\theta_A$  and  $\theta_B$ :

$$\text{Prob}(\text{vote for incumbent party}) = 1 \text{ if } E[\theta_I|r, g] \geq E[\theta_{-I}|r, g], 0 \text{ otherwise.}$$

If the priors are close enough, the equilibrium probability of re-election will be less than one, as a dissonant incumbent will lead to a switch in the ranking and will cause the incumbent party to lose power. For example, if the voter initially deems both parties to be of equal quality, then she will not re-elect the incumbent party if the mayor is revealed as dissonant.

**Equilibrium with fiscal rule:** Suppose now that a fiscal rule is introduced, such that it is no longer possible to have a deficit (i.e.,  $(r_L, g_H)$  can't happen). The outcome space observed by the voter is now reduced to  $(r_H, g_H)$  and  $(r_L, g_L)$ . For each level of revenue, both types of candidate are forced to choose the corresponding level of spending, so no information is revealed about the incumbent's type. This means that the fiscal rule solves the agency problem, as whichever party is in power always implements the level of spending that the voter prefers. Without any new information being acquired (nor any reason to complain about the performance of the incumbent), the voter is happy to re-elect the incumbent party with probability one in the modified equilibrium.