

# Do Fiscal Rules Matter? A Difference-in-Discontinuities Design\*

Veronica Grembi  
Catholic University of Milan

Tommaso Nannicini  
Bocconi University, IGER & IZA

Ugo Troiano  
Harvard University

First version, February 2011

This version, July 2011

## Abstract

We evaluate the effect of relaxing fiscal rules on budget outcomes in a quasi-experimental setup. In 1999, the Italian central government introduced fiscal rules—also known as the Domestic Stability Pact—aimed at imposing fiscal discipline on municipal governments, and in 2001 the Pact was relaxed for municipalities below 5,000 inhabitants. This institutional change allows us to implement a “difference-in-discontinuities” design by combining the before/after with the discontinuous policy variation. Our estimates show that relaxing fiscal rules triggers a substantial deficit bias, captured by a shift from zero deficit to a deficit that amounts to 2% of total budget. The adjustment mostly involves revenues as unconstrained municipalities cut their real estate and income taxation. The impact is larger when mayors can run for reelection and the number of political parties is high. A falsification test in 1999 shows that our findings are not driven by the fact that cities just below and above 5,000 are differentially affected by fiscal rules.

**JEL codes:** C21, C23, H72, H77.

**Keywords:** fiscal rules, fiscal federalism, difference-in-discontinuities.

---

\*We thank Larry Katz for initial encouragement and detailed comments. We also thank Daron Acemoglu, Alberto Alesina, Robert Barro, Roland Benabou, Marianne Bertrand, Raj Chetty, David Cutler, Emmanuel Farhi, Ed Glaeser, Sandy Jencks, David Laibson, Roberto Perotti, Jim Poterba, Guido Tabellini, and seminar participants at Bank of Italy, Bologna, Boston University, CIFAR meeting, CSEF Naples, Erasmus University Rotterdam, Harvard Labor and Macro Lunch, and IGER for their insightful comments. Errors are ours and follow a random walk. Financial support is gratefully acknowledged from Catholic University of Milan for Veronica Grembi; the ERC (Grant No. 230088) and Bocconi University for Tommaso Nannicini; Harvard Department of Economics, Harvard Multidisciplinary Program in Inequality & Social Policy, and Bank of Italy for Ugo Troiano. *Corresponding author:* Ugo Troiano, Harvard University, Department of Economics, Littauer Center, 1805 Cambridge Street, Cambridge, MA 02138; e-mail: troiano@fas.harvard.edu.

# 1 Introduction

The persistent budget deficits and rising public debt levels in OECD countries have inspired a large amount of research in the past decades. More recently, the need for fiscal adjustment in the aftermath of the 2008–09 financial crisis, as well as the mounting expenditure challenges faced by subnational governments in federal states, have revived the policy interest for fiscal rules aimed at disciplining the discretionary power of budget policymakers both at the national and subnational level. Yet, in the economic literature, a number of questions on fiscal rules remain unsettled.<sup>1</sup> From a normative perspective, it is not obvious whether tight rules such as a balanced budget requirement are optimal or not. On the one hand, the optimal tax smoothing theory (see Barro, 1979; Lucas and Stokey, 1983) would suggest that deficits increase welfare by equalizing the distortionary cost of taxation across booms and recessions. On the other hand, deficit bias might be the suboptimal result of the interplay between rational politicians, voters, and interest groups (e.g., see Alesina and Tabellini, 1990; Persson and Svensson, 1989; Aghion and Bolton, 1990).<sup>2</sup>

Even if the adoption of fiscal rules were deemed necessary, their impact would still face serious commitment problems, in the form of future overhaul, soft budget constraints, and lack of enforcement. This is the reason why a number of empirical studies have tried to evaluate whether fiscal rules have a causal impact on budget outcomes or not. The conventional wisdom in this literature is that fiscal rules do indeed result in lower budget imbalances and faster initiative to reduce unexpected deficits. The evidence comes either from cross-country comparisons in specific regions, such as the European Union (Hallerberg and Von Hagen, 1999) or Latin America (Alesina, Hausmann, Hommes and Stein, 1996), or from comparisons between local governments in a federal state such as the U.S. (e.g., see Poterba 1994, 1996).<sup>3</sup> Yet, as the authors in this literature have repeatedly acknowledged—see, for exam-

---

<sup>1</sup>For a survey of the economics of fiscal rules, see Poterba and Von Hagen (1999) and Alesina and Perotti (1996, 1999) at the national level, and Rodden, Eskeland, and Litvack (2003) at the subnational level.

<sup>2</sup>See Alesina and Perotti (1995) for a critical survey of the political economy of fiscal rules. Other political economy models on deficit determination include Tabellini and Alesina (1990), Lizzeri (1999), Battaglini and Coate (2008), Azzimonti, Battaglini, and Coate (2008), and Yared (2010). The Public Choice approach provides alternative explanations of politically motivated deficits, mostly based on voters' fiscal illusion and politicians' opportunistic behavior (e.g., see Buchanan and Wagner, 1977).

<sup>3</sup>Other studies exploiting the within-U.S. variation in fiscal rules include Von Hagen (1991), Alt and Lowry (1994), Bayoumi and Eichengreen (1995), Bohn and Inman (1996), Alesina and Bayoumi (1996), Auerbach (2006), and Fatas and Mihov (2006). Feld and Kirchgassner (2006) and Krongstrup and Walti

ple, the discussion in Poterba (1996) or Alesina and Perotti (1996)—a definitive conclusion is hampered by serious identification issues, namely in the form of omitted bias and reverse causation. If voters’ taste for fiscal restraint caused both the adoption of tighter fiscal rules and the election of fiscally conservative politicians, the correlation between rules and discipline (if any) would be completely spurious. And many other omitted factors—especially at the cross-country level—might lead more disciplined constituencies to introduce tighter rules.<sup>4</sup> Furthermore, fiscal results in the past might trigger the approval of certain rules, introducing a problem of reverse causation in the presence of serially correlated budget outcomes.<sup>5</sup>

This paper contributes to the above literature in two steps, both empirical in nature. First, we estimate the casual impact of fiscal rules on fiscal discipline in a quasi-experimental setup. Our testing ground is Italy, where the central government set a balanced budget requirement for all municipal governments in 1999—the so called “Domestic Stability Pact,” DSP henceforth—and relaxed it for municipalities below 5,000 inhabitants in 2001. This allows us to combine two sources of variation, before/after 2001 and just below/above 5,000, and implement what we call a “difference-in-discontinuities” design (or “diff-in-disc”). Under milder conditions than cross-sectional regression discontinuity and diff-in-diff, our econometric strategy identifies the effect of relaxing fiscal rules on budget outcomes while controlling for observable and unobservable factors and, at the same time, for pre-existing policy discontinuities at 5,000.<sup>6</sup> Second, we shed light on the political economy of budget deficits, as we show how an exogenous (albeit local) variation in the tightness of fiscal rules affects deficit bias according to the underlying features of voters and politicians.

The main rule established by the Italian DSP imposed a gradual reduction of the “fiscal gap,” defined as the budget deficit net of transfers and debt service. Municipalities with less than 5,000 inhabitants were exempted from complying with this rule only in 2001. The rationale for the exemption was to avoid burdening very small entities with onerous requirements, as they may be disadvantaged by economies of scale in managing the municipal government.

---

(2007) exploit the variation within Switzerland.

<sup>4</sup>On the endogenous determination of laws, see—among others—Aghion, Alesina, and Trebbi (2004) and Givati and Troiano (2011).

<sup>5</sup>In addition to endogeneity problems, most of the existing empirical studies suffer from small sample size (as they use countries worldwide, or states within a federation) and measurement issues (as they often involve the comparison of tight vs. loose fiscal rules, whose definition is somehow discretionary).

<sup>6</sup>See Section 3 for a discussion of identification, estimation, and diagnostics in the diff-in-disc setup.

Penalties for not complying with the DSP were indeed quite severe: (i) 5 percent cut in the yearly transfers from the central government; (ii) ban on new hires; (iii) 30 percent cut on reimbursement and non-absenteeism bonuses for the employees of the municipality. These sanctions notwithstanding, the approval of the DSP were accompanied by widespread skepticism about its effectiveness, as Italy usually ranks last among OECD countries in law enforcement and government effectiveness (e.g., see Kaufmann, Kraay, and Mastruzzi, 2010). This reinforces the interest in our test and improves external validity, as the lessons we draw on the effectiveness of fiscal rules may extend to regulatory environments where the fiscal authority setting the rules faces critical ex-ante commitment problems.

Our empirical findings show that relaxing fiscal rules translates into a larger fiscal gap by about 40%-60% (depending on the specification). This large effect on the main target of the DSP has real consequences on the budget, as unconstrained municipalities shift from a situation of balanced budget to a deficit that amounts to about 20 Euros per capita (2% of total expenditure). The adjustment mainly takes place on the side of revenues; municipalities for which fiscal rules are relaxed decrease their taxes, but leave expenditure almost unchanged. Lower taxes are the result of cuts in the main tax rates decided by municipal governments: a real estate tax rate on home property (*Imposta Comunale sugli Immobili*, ICI), which provides almost 50% of municipal tax revenues, and a surcharge on the personal income tax (*Imposta sul Reddito delle Persone Fisiche*, IRPEF), which amounts to about 10% of municipal tax revenues. Cities for which fiscal rules are relaxed have both a lower real estate tax rate (by about 14%) and a lower income tax surcharge (by 30%) after the policy change. Furthermore, we use the introduction of the DSP for all municipalities in 1999 as a falsification test to show that our findings are not driven by the fact that cities just below and above 5,000 respond differently to the same set of fiscal rules.

Finally, we look for potential heterogeneity in the treatment effect with the aim to shed light on the political economy of deficit bias. As (i) Italian mayors face a two-term limit, (ii) municipalities do not vote at the same time, and (iii) the introduction of the DSP is independent of local politics because it followed agreements between the European Union and its member countries in 1999, whether the relaxation of fiscal rules is managed by a mayor with or without a binding term limit is completely random. We therefore remain within a quasi-experimental framework and look at the differential impact of relaxing fiscal rules for

mayors who can or cannot be reelected, finding that the increase in deficit bias arises only for the former. Given that mayors without term limit face both stronger reelection concerns and a higher expected probability that they (or their party) will remain in power, the above result provides more support for models linking deficit to reelection incentives (e.g., see Aghion and Bolton, 1990) or to politicians' pandering to voters (e.g., see Maskin and Tirole, 2004) than models viewing deficit as a way to tie the hands of future governments with different political preferences (e.g., see Alesina and Tabellini, 1990; Persson and Svensson, 1989).

A second heterogeneity test involves the comparison of municipalities where only two political parties are represented in the city council (about half of the sample) versus municipalities with more parties. Our results show that relaxing fiscal rules increases deficit only where many parties seat in the council, which must approve the budget proposed by the mayor. This finding is consistent with models that explain deficit as the result of political fragmentation and of dynamic common pool (e.g., see Persson and Tabellini, 2000), and also with the cross-country evidence that coalition governments are associated with higher deficits (see Roubini and Sachs, 1989; Kontopoulos and Perotti, 1999).

The paper proceeds as follows. Section 2 describes the Italian institutional framework. Section 3 lays out our identification and estimation strategy. Section 4 describes the data. Section 5 discusses the empirical results and validity tests. We conclude with Section 6.

## 2 Institutional framework

The Italian municipal government (*Comune*) is composed by a mayor (*Sindaco*), an executive committee (*Giunta*) appointed by the mayor, and an elected city council (*Consiglio Comunale*) that must endorse the annual budget proposed by the mayor. Since 1993, mayors are directly elected (single round below 15,000 inhabitants and runoff above) and face a two-term limit. Municipalities manage about 10% of total public expenditure and are in charge of a vast set of services, from water supply to waste management, from municipal police to infrastructures, from welfare to housing. In terms of revenues, they largely depend on central transfers as local taxes amount to only 20% of municipal revenues.

After the European Union adopted its Stability and Growth Pact in 1997, some European countries—including Italy—adopted subnational fiscal rules to keep local governments

accountable. In December 1998, the Italian yearly national budget law (*Legge Finanziaria*) for 1999 introduced a set of rules that constrained all municipalities in terms of fiscal discipline, the aforementioned Domestic Stability Pact or DSP (*Patto di Stabilità Interno*).<sup>7</sup> Municipal governments were constrained to keep the growth of their fiscal gap under tight control, where “fiscal gap” was defined by the DSP itself as the budget deficit (total expenditure minus total revenues) net of debt service and transfers. The rationale for the exclusion of debt service and transfers in the definition of the DSP target is twofold. First, mayors are not accountable for expenses on interests (which depend on previously contracted loans) and for revenues from transfers (which are not raised by the municipality). Second, these two budget items are the tools that the central government uses to enforce fiscal rules, reducing interest payments for compliers and cutting transfers for non-compliers. In fact, the punishment established for not complying with the DSP included the following penalties: (i) 5 percent cut in the yearly transfers from the central government; (ii) ban on new hires; (iii) 30 percent cut on reimbursement and non-absenteeism bonuses for the employees of the municipal administration. Cities complying with the DSP, instead, benefited from a reduction of the expenses on interests for loans contracted with the central government.

The exact DSP rule constraining fiscal gap changed from one year to another, but over our sample period it mainly consisted in imposing a cap of 3 percent on the growth rate of the gap. In evaluating the impact of the DSP on fiscal discipline, we therefore focus on the pattern of both fiscal gap and budget deficit. Constrained and unconstrained municipalities can borrow from the central government, but if they run into a permanent debt they need to go through a special procedure of budget consolidation (*Piano di Risanamento*).

After 2001, all municipalities below 5,000 inhabitants were exempted by the DSP.<sup>8</sup> The motivation for this exemption was not made explicit by the central government, but it is probably linked to the goal of providing some relief to small municipalities in the presence of economies of scale in managing the municipal government. Fiscal rules, however, are not the only policy varying with population size at 5,000. In particular, at this cutoff, there is a sharp 33% increase in the wage received by the mayor and by the other members of the executive committee, based on a remuneration policy that has been in place since the

---

<sup>7</sup>See Law 23 December 1998, no. 448, article 28.

<sup>8</sup>See Law 23 December 2000, no. 388, article 53.

early 1960s.<sup>9</sup> Table 1 summarizes all the Italian policies on municipal governments relying on population thresholds. Population size determines the size of the city council; the size of the executive committee; the electoral rule; and whether a municipality can have additional elective bodies at the neighborhood level. But only the DSP (after 2001) and the salary of local politicians display a discontinuity at the 5,000 threshold.

In 2002, regions with special autonomy (*Regioni a Statuto Speciale*) were allowed to set their own fiscal rules for municipal governments, and this is why we do not consider these regions in our study. Furthermore, since 2005 fiscal rules have been frequently changing from one year to another, shifting the population cutoff from 5,000 to 3,000 and back, and replacing the balanced budget requirement with expenditure caps in some years. This is the reason why we focus our empirical evaluation on the period from 1997 to 2004.

Table 2 summarizes the evolution of the DSP coverage over the sample period. In the next section, we explain how we exploit these peculiar Italian institutions to identify the effect of fiscal rules on fiscal discipline.

### 3 The diff-in-disc design

#### 3.1 Identification

Define  $Y_{it}(1)$  as the potential budget outcome of municipality  $i$  at time  $t$  in the case of treatment, and  $Y_{it}(0)$  as the potential budget outcome of the same municipality at the same time in the case of no treatment. Because of the institutions described in the previous section, our treatment of interest coincides with “relaxing fiscal rules,” so that fiscal rules are relaxed when  $D_{it} = 1$  and binding when  $D_{it} = 0$ . The treatment year is  $T_0$ , that is, if  $t \geq T_0$ , only municipalities below a certain population cutoff  $P_c$  are treated; the running variable  $P_i$  is set at the Census level and therefore time-invariant. The fiscal rules relaxed for small cities at  $T_0$  were introduced everywhere at  $T_{-1}$ . As a result, treatment assignment is given by:

$$D_{it} = \begin{cases} 1 & \text{if } P_i \leq P_c, t \geq T_0 \\ 0 & \text{if } P_i > P_c, t \geq T_0 \\ 0 & \text{if } T_{-1} \leq t < T_0 \\ 1 & \text{if } t < T_{-1} \end{cases} \quad (1)$$

---

<sup>9</sup>Gagliarducci and Nannicini (2011) show that the wage increase at 5,000 attracts more educated individuals into politics and improves their performance once elected.

It would be tempting to implement a regression discontinuity (RD) design and estimate the discontinuity in the observed outcome  $Y_{it}$  at  $P_c$  after  $T_0$ . Unfortunately, as discussed above, Italian institutions show a confounding policy discontinuity at  $P_c$ , because local politicians just above this threshold receive a higher wage. This means that the standard RD assumption of continuity of potential outcomes (see Hahn, Todd, and Van der Klaauw, 2001) is not verified in a cross-sectional dimension. Figure 1 exemplifies this argument by showing how potential outcomes may be discontinuous at  $P_c$ , that is:  $\lim_{P_i \uparrow P_c} E[Y_{it}(k)|P_i, t \geq T_0] - \lim_{P_i \downarrow P_c} E[Y_{it}(k)|P_i, t \geq T_0] = \gamma_k \neq 0$ , with  $k \in \{0, 1\}$ . The discontinuity in the observed outcome captured by the cross-sectional RD estimator (the solid line in Figure 1) would therefore identify the (local) treatment effect at  $P_c$ —that is,  $\tau \equiv E[Y_{it}(1) - Y_{it}(0)|P_i = P_c]$ —plus a mixture of the confounding discontinuities  $\gamma_0$  and  $\gamma_1$ .

This is why, in order to identify the treatment effect of relaxing fiscal rules, we implement an estimator that exploits both the (sharp) discontinuous variation at  $P_c$  and the time variation at  $T_0$ : the difference-in-discontinuities (or diff-in-disc) design. The intuition is simple and illustrated in Figure 2. As the confounding policy at  $P_c$  is time-invariant around  $T_0$ , we can estimate the discontinuity in  $Y_{it}(0)$  before  $T_0$ —equal to  $\lim_{P_i \uparrow P_c} E[Y_{it}|P_i, T_{-1} \leq t < T_0] - \lim_{P_i \downarrow P_c} E[Y_{it}|P_i, T_{-1} \leq t < T_0] = \gamma'_0$ —and then remove it from the observed discontinuity after  $T_0$  to identify the treatment effect  $\tau$  (see again Figure 1), as long as the confounding discontinuity is constant, that is,  $\gamma'_0 = \gamma_0$ .<sup>10</sup>

Formally, the diff-in-disc design relies on the following identifying conditions, which are milder than those required by both cross-sectional RD and diff-in-diff.

**Assumption 1**  $E[Y_{it}(0)|P_i, t \geq T_0] - E[Y_{it}(0)|P_i, t < T_0]$  is continuous in  $P_i$  at  $P_c$ .

**Assumption 2**  $E[Y_{it}(1) - Y_{it}(0)|P_i, t \geq T_0]$  is continuous in  $P_i$  at  $P_c$ .

---

<sup>10</sup>There exist alternative approaches that exploit the longitudinal nature of the data in a regression discontinuity setting, such as the fixed-effect RD estimator used by Pettersson-Lidbom (2008), the first-difference RD estimator used by Lemieux and Milligan (2008), or the dynamic RD design in the presence of repeated spells of treatment assignment by Cellini, Ferreira, and Rothstein (2010). In all of these cases, however, treatment assignment changes over time because the running variable is not constant, and not because the policy threshold has changed as in our diff-in-disc design. Our econometric strategy also relates to those evaluation designs that exploit the difference or comparison between different discontinuities across space, such as in different U.S. states (see Dickert-Conlin and Elder, 2010) or for politicians facing different term limits (see Gagliarducci and Nannicini, 2011). The time variation embedded in the diff-in-disc setup, however, has specific implications for the identifying assumptions and for the diagnostics tools.



Assumption 1 states that the confounding discontinuity must be time-invariant and, in a sense, is equivalent to the RD condition on the continuity of potential outcomes, as it requires that the *difference* in  $Y_{it}(0)$  before and after  $T_0$  be continuous at the cutoff. This assumption is more than plausible in our institutional setting, but it can nevertheless be verified by indirect testing procedures as it is the case for the cross-sectional RD continuity assumption (see more below). Assumption 2 states that there must be no interaction between the treatment and the confounding discontinuity and, in a sense, is equivalent to the *additivity* condition in diff-in-diff. In our institutional setting, this assumption would be violated if mayors just below and above  $P_c$  reacted differently to fiscal rules. Fortunately, under the maintaining hypothesis that Assumption 1 holds, this second assumption is testable, because of the introduction of fiscal rules everywhere at  $T_{-1}$ . For Assumption 2 to hold, the observed jump in  $Y_{it}$  before  $T_{-1}$ , which identifies the discontinuity in  $Y_{it}(1)$ , should be equal to the jump in  $Y_{it}$  between  $T_{-1}$  and  $T_0$ , which identifies the discontinuity in  $Y_{it}(0)$ .<sup>11</sup>

Under Assumption 1 and Assumption 2, the following diff-in-disc estimator  $\hat{\tau}$  identifies  $\tau$ , that is, the (local) treatment effect of relaxing fiscal rules at  $P_c$ :

$$\begin{aligned} \hat{\tau} \equiv & \left( \lim_{P_i \uparrow P_c} E[Y_{it}|P_i, t \geq T_0] - \lim_{P_i \downarrow P_c} E[Y_{it}|P_i, t \geq T_0] \right) \\ & - \left( \lim_{P_i \uparrow P_c} E[Y_{it}|P_i, T_{-1} \leq t < T_0] - \lim_{P_i \downarrow P_c} E[Y_{it}|P_i, T_{-1} \leq t < T_0] \right). \end{aligned} \quad (2)$$

## 3.2 Estimation and diagnostics

The estimator defined in equation (2) takes the difference between two discontinuities in the observed outcome—one after  $T_0$  and one before  $T_0$ —and can be implemented by estimating the boundary points of four regression functions of  $Y_{it}$  on  $P_i$ : two on both sides of  $P_c$ , before and after  $T_0$ . We borrow two different estimation methods from the RD literature for this purpose: local linear regression and spline polynomial approximation.

The first method fits linear regression functions to the observations distributed within a distance  $h$  on either side of  $P_c$ , both at  $T_{-1} \leq t < T_0$  and at  $t \geq T_0$ . Formally, we restrict the

---

<sup>11</sup>Note that Assumption 2 is sufficient but not necessary for the identification of a treatment effect. Without this assumption, however, the estimand would be more difficult to interpret, as it would result in a weighted average of the treatment effect for cities just above and below  $P_c$ . As in our institutional framework Assumption 2 can be tested, we prefer to impose it and identify  $\tau$  as the (local) treatment effect at  $P_c$ .

sample to cities in the interval  $P_i \in [P_c - h, P_c + h]$  and estimate the model:

$$Y_{it} = \delta_0 + \delta_1 P_i^* + J_i(\gamma_0 + \gamma_1 P_i^*) + T_t[\alpha_0 + \alpha_1 P_i^* + J_i(\beta_0 + \beta_1 P_i^*)] + \xi_{it}, \quad (3)$$

where  $J_i$  is a dummy for cities below 5,000,  $T_t$  an indicator for the post-treatment period, and  $P_i^* = P_i - P_c$  the normalized population size. Standard errors are clustered at the city level. The coefficient  $\beta_0$  is the diff-in-disc estimator and identifies the treatment effect of relaxing fiscal rules, as  $D_{it} = J_i \cdot T_t$ . We present the robustness of our results to multiple bandwidths, namely  $h = 500$ ,  $h/2$ , and  $2h$ .<sup>12</sup>

The second method uses all observations and chooses a flexible functional form to fit the relationship between  $Y_{it}$  and  $P_i$  on either side of  $P_c$ , both at  $T_{-1} \leq t < T_0$  and at  $t \geq T_0$ :

$$Y_{it} = \sum_{k=0}^p (\delta_k P_i^{*k}) + J_i \sum_{k=0}^p (\gamma_k P_i^{*k}) + T_t \left[ \sum_{k=0}^p (\alpha_k P_i^{*k}) + J_i \sum_{k=0}^p (\beta_k P_i^{*k}) \right] + \xi_{it}. \quad (4)$$

Again, standard errors are clustered at the city level, and the coefficient  $\beta_0$  is the diff-in-disc estimator identifying the treatment effect of relaxing fiscal rules. We present the robustness of our results to multiple orders of the polynomial approximation, namely  $p \in \{2, 3, 4\}$ .

A nice feature of our diff-in-disc design is that it can combine the diagnostics tools of RD and diff-in-diff, resulting in a vast array of tests that should validate the underlying identification strategy. First, the generalized introduction of fiscal rules at  $T_{-1}$  allows us to test for the lack of interactive effects between the treatment and the pre-existing policy discontinuities, in the spirit of the falsification test discussed above. Second, it is important to check not whether the running variable is manipulated around the threshold, but whether manipulation changes (or arises) over time. In other words, the cross-sectional test of continuity of the density at  $P_c$  (see McCrary, 2008) should be extended to test for the continuity of the *difference* in the densities before and after  $T_0$ , as long as the running variable can be altered before the policy change is announced. Third, diff-in-disc estimations with time-invariant characteristics as outcomes should be implemented to indirectly test for changes in the pattern of manipulative sorting, because the mean difference of these characteristics just below and above  $P_c$  is not supposed to vary after  $T_0$  in the absence of manipulation. Fourth, as a further check in this direction, time-invariant characteristics and year fixed effects should be

---

<sup>12</sup>Results are also robust to the use of  $h = 250$  or  $h = 1,000$ , and then  $h/2$  and  $2h$  (available upon request).

included as covariates in the baseline diff-in-disc estimations; in the absence of manipulation, point estimates are expected to remain similar and accuracy to increase.

In the spirit of the test of common trend before the treatment year in the diff-in-diff setup, as a fifth diagnostics tool, *yearly* diff-in-disc estimations over the pre-treatment period should be implemented, that is, each pre-treatment year should be used as the (fake) treatment period and the preceding year as the (fake) control period. In particular, the yearly diff-in-disc estimation for the year immediately before  $T_0$  would test for anticipation effects, which are supposed to be zero. Sixth, placebo diff-in-disc estimations should be implemented at fake (or time-invariant) cutoffs, where the estimates are expected to be zero.

## 4 The data

We use data from the Italian Ministry of Internal Affairs (*Ministero dell'Interno*) containing information at the municipality level on budget items, municipal tax rates, electoral outcomes, and individual characteristics of the mayor. Based on the local nature of our diff-in-disc design, we restrict the sample to Italian municipalities between 3,500 and 7,000 inhabitants.<sup>13</sup> For the reason discussed in Section 2, we drop municipalities in regions with special autonomy. This leaves us with a final sample of 1,050 municipalities for a total of 6,300 observations from 1999 to 2004. Among them, 555 municipalities are treated after 2001 (because they are below 5,000 inhabitants) and 495 are in the control group. The population size that decides treatment status is the 2001 Census. Because the relaxation of the DSP was decided in December 2000, it is very unlikely that municipalities had the time to influence their population and sort below the 5,000 threshold, and—on top of this—it is also unlikely that elected officials wanted to do that at the price of cutting their wage by 33%. This notwithstanding, in Section 5.2 we will test for manipulative sorting below 5,000 before/after 2001 by comparing population size in the 1991 and 2001 Census. Our final sample contains about 13% of all Italian municipalities and about 8% of the national population.

Our main variables of interest are the budget quantities. To measure fiscal discipline, we evaluate the fiscal gap (total expenditure minus total revenues, net of transfers and debt

---

<sup>13</sup>We restrict the sample to the interval 3,500–7,000 to stay relatively far from the 3,000 threshold, where other policies change (see Table 1), and to balance the sample size on either side of the 5,000 threshold. All the results are robust to this interval choice, i.e., they are virtually unchanged for alternative choices, such as 3,250–6,750; 3,000–7,000; 3,500–6,500; 4,000–6,000; and 3,500–7,500 (available upon request).

service), which is the target of the DSP, and the budget deficit (total expenditure minus total revenues). We divide expenditure into current outlays (including personnel expenditure) and capital outlays (mostly investments); and we divide revenues into municipal taxes, fees and tariffs, transfers from the central government, transfers from the regional government, and other revenues. The main tax instruments decided by municipal governments are the real estate tax rate on home property (*Imposta Comunale sugli Immobili*, ICI), providing about 50% of their tax revenues, and the municipal surcharge on the personal income tax (*Imposta sul Reddito delle Persone Fisiche*, IRPEF), amounting to about 10% of tax revenues.

One possible concern in evaluating the reaction of budget variables and tax instruments to fiscal rules might be that mayors have very little autonomy in adjusting local revenues or expenditure, but this is not the case for Italian municipalities. On the revenues side, over our sample period, mayors could vary ICI within a bracket from 0.4 to 0.7 percent of the legal home value, and the IRPEF surcharge within a bracket from 0 to 0.5 percent of taxable income.<sup>14</sup> And they were also free to set other local taxes (such as those on building rights or the occupation of public areas), or fees and tariffs for the services they provided (such as waste management or child care). On the expenditure side, Bandiera, Prat, and Valletti (2009) show how similar Italian municipalities can pay very differently for similar goods, and they interpret this as evidence of passive waste or inefficiency at the local level. This implies that, even if all current outlays were rigid (and this is not certainly the case), mayors would still have the ability to reduce passive waste so as to improve the fiscal gap.

The dataset also contains *time-invariant* information on each municipality (geographic location, area size in km<sup>2</sup>, sea level in meters), as well as *time-varying* information on the elected mayor (gender, years of schooling, tenure in office, term limit, margin of victory), the economic environment (taxable income of resident inhabitants), and the political environment (number of political parties seating in the city council).

Table 3 provides descriptive statistics on the main outcome variables (budget items and tax instruments) for cities below and above 5,000 inhabitants. All budget variables are per capita and expressed in real terms (with 2009 as base year); tax rates are in percentage points. Municipalities below (above) 5,000 manage an yearly budget equal to almost 1,041 (943) Euros per capita in terms of expenditure, and the budget deficit amounts to about

---

<sup>14</sup>Bordignon, Nannicini, and Tabellini (2010) use ICI as the main policy tool of Italian municipalities.

15 (11) Euros. Taxes are only slightly lower than 20% of total revenues and higher in municipalities above 5,000. The main tax rates on ICI and the IRPEF surcharge, however, are fairly similar for municipalities in the two groups.

Table 4 summarizes the yearly pattern of fiscal discipline for cities below and above 5,000 over our sample period. Both the fiscal gap and the budget deficit are substantially larger in cities below 5,000 after 2001. Yet, also before 2001, the two groups seem on different pattern of fiscal discipline at least from a quantitative point of view, as the reduction in fiscal gap and the increase in budget deficit are more pronounced in cities below 5,000. This evidence casts doubts on the plausibility of the common trend assumption, and this is why a standard diff-in-diff strategy might lead to biased estimates. As a descriptive benchmark, however, it is useful to note that the diff-in-diff estimates deliver the expected result: relaxing fiscal rules increases the fiscal gap by 48.260 Euros and the deficit by 6.273 Euros in a naive specification without municipality fixed effects, and by 16.653 and 5.276 with fixed effects, where both estimates on fiscal gap are statistically significant at a 1% level and those on deficit at a 5% level. In the next section, we discuss the results of our diff-in-disc design, which provides more robust evidence on the impact of relaxing fiscal rules on fiscal discipline.

## 5 Empirical results

### 5.1 The effect of relaxing fiscal rules on budget policy

Table 5 contains the main (diff-in-disc) estimation results. For each outcome variable, we show the robustness of the results to six estimation methods: local linear regression as in equation (3) with three different bandwidths (i.e., 250, 500, and 750); spline polynomial approximation as in equation (4) with three different orders of the polynomial (i.e., 2, 3, and 4). The main outcomes of interest are our two measures of fiscal discipline: fiscal gap and budget deficit (see panel A of the table). While the former is the main target of the DSP, we believe that the latter should be the real variable of policy interest.

The impact of relaxing rules on the fiscal gap is positive and significant both in statistical and in economic terms. The DSP relaxation translates into a higher gap by about 40% to 60%, with respect to the average value of the control group and depending on the specification. This effect passes through to budget deficit, which increases by about 20 Euros per capita

with respect to a baseline situation of balanced budget. The effect on deficit is statistically significant in all specifications and the point estimates are somehow more stable than those on fiscal gap. The deficit bias created by the relaxation of the DSP is also substantial from an economic point of view, as it amounts to about 2% of the total expenditure.

The estimation results on fiscal discipline are consistent with the descriptive graphs shown in Figure 3 (fiscal gap) and Figure 4 (deficit), where we draw scatters and (3<sup>rd</sup>-order) polynomial fits of the differences between each post-2001 outcome value and each pre-2001 value. These graphs allow us to see whether those differences exhibit a discontinuity at the 5,000 threshold. We see that both variables measuring fiscal discipline exhibit a sharp jump when moving from the left to the right of the threshold in the whole sample (top left graph in both figures). Furthermore, in the other graphs in Figure 3 and Figure 4, we shed some light on the timing of the effect; we redraw the scatters and polynomial fits considering in the post-treatment period only observations after 2002 (top right), 2003 (bottom left), and 2004 (bottom right). The evidence is consistent for both the fiscal gap and the deficit, as the adjustment accelerates in 2002 and 2003 but slows down in 2004 (pattern that can be consistent with municipalities eventually realizing that the punishment is less harsh than expected). The observed discontinuities, however, remain statistically significant for all years.<sup>15</sup>

In panels B and C of Table 5, we assess whether the fiscal (de)stabilization takes place on the side of revenues or expenditure. The estimates clearly show that the adjustment involves revenues, as current and capital outlays remain almost unchanged, while tax revenues are lower by 20% to 45% in unconstrained municipalities, with respect to the average value of the control group and depending on the specification. Lower tax revenues are the result of cuts in the principal tax rates decided by the municipal government (see panel D of Table 5); cities for which fiscal rules are relaxed reduce their real estate tax rate (ICI) by 14% and the income tax surcharge (IRPEF) by 30%. Figure 5 provides graphical evidence on the diff-in-disc jumps at 5,000 in the budget components and tax instruments; the graphs refer to the entire post-treatment period and are constructed like the top left graphs in Figure 3 and Figure 4. Consistently with the estimation results, tax revenues, ICI, and IRPEF show significant and negative jumps moving from just above to just below the 5,000 threshold.

---

<sup>15</sup>The yearly diff-in-disc estimates for 2001, 2002, 2003, and 2004 confirm the graphical evidence on the timing of the effect of relaxing fiscal rules on fiscal discipline (available upon request).

The fact that the adjustment is driven by revenues could receive a twofold explanation. On the one hand, politicians might have a hard time convincing interest groups to cut expenditure, while taxpayers are more prone to a problem of internal free-riding and do not self-organize (see Olson, 1965). On the other hand, tax increases might be less salient than expenditure cuts for individuals (see Chetty, Looney, and Kroft, 2009), and therefore they might be more easy to adopt (and revert) for politicians.

## 5.2 Validity tests

The results presented above rest on Assumption 1 and Assumption 2—derived in our evaluation framework—for identification. As discussed in Section 3, the first condition can be indirectly assessed by means of testing procedures aimed at detecting changes in manipulative sorting before/after 2001, while the second condition can be directly tested using pre-treatment data and keeping Assumption 1 as the maintaining hypothesis.

Figure 6 tests the null hypothesis of continuity of the *difference* in the density at 5,000 between the 1991 and the 2001 Census (top graph), by drawing both scatters and (3<sup>rd</sup>-order) polynomial fits. If mayors were able to manipulate population size and sort below the threshold to avoid fiscal rules, our estimates would still suffer from the selection bias that was common in the previous empirical literature. However, in principle, there is very little room for changing manipulation between the two Censuses, because (i) the DSP is only implemented in December 2000, (ii) the Census is run independently by the National Statistical Office, so that false reporting should be ruled out, and (iii) mayors willing to sort below 5,000 to enjoy a relaxation of fiscal rules would pay the price of cutting their wage by 33%. Nevertheless, it might still be the case that some municipalities under financial stress tried to sort below 5,000 moving from the 1991 to the 2001 Census, by forcing some residents to leave or (more plausibly) not counter-reacting to population drops. Yet, the top graph in Figure 6 is reassuring about the absence of manipulation, as there is no jump in the difference between the two densities. The point estimate from the spline polynomial approximation is equal to -0.078 (s.e., 0.114), and therefore it is not statistically different from zero. For the sake of completeness, we also report the cross-sectional density tests for 1991 (bottom left) and 2001 (bottom right). Also there, there is no evidence of manipulation.<sup>16</sup>

---

<sup>16</sup>The 1991 point estimate is 0.0678 (s.e., 0.082); the 2001 point estimate is -0.010 (s.e., 0.076).

Table 6 further evaluates the absence of manipulative sorting. There, we perform two types of exercises. First, in the top panel, we implement diff-in-disc estimations with time-invariant characteristics (geographic location, area size, and sea level) as outcome variables, but we use changing population numbers: the 1991 Census before the treatment year, and the 2001 Census afterwards.<sup>17</sup> This is meant to assess whether the fraction of cities with certain fixed characteristics just below or above 5,000 varies from 1991 to 2001. No time-invariant characteristics display a statistically significant jump.<sup>18</sup> We think that geographical location is a particularly interesting dimension to control for, because Italian geography is very correlated with economic development, crime rates, labor market shirking, or political accountability, and it could thus be associated also with opportunistic manipulation.<sup>19</sup>

Second, in the bottom panel of Table 6, we implement diff-in-disc estimations with some (time-varying) economic or political characteristics of the municipality as outcome variables, using the 2001 Census population as the running variable exactly as in the baseline specifications for the main budget outcomes. The time-varying characteristics we control for are the taxable income at the municipality level; the mayor’s gender, years of schooling, and previous tenure in office (in years); whether the term limit is binding or not; and the number of political parties seating in the city council. These variables are potentially endogenous to the policy change, but detecting significant effects would disclose unexpected channels of adjustment through income or political selection. This does not seem to be the case, as also potentially endogenous variables are balanced around 5,000 before/after 2001.

Based on this large amount of supporting evidence on Assumption 1, in Table 7, we directly test for Assumption 2. In other words, we check whether our results are driven by different responses to fiscal rules by cities just below or just above the 5,000 threshold. We use the introduction of the DSP in 1999 for all municipalities as an experiment to test for the absence of any differential response around 5,000. Specifically, we implement the diff-in-disc estimations for the interval 1998–2001, using the period after 1999 as the post-treatment

---

<sup>17</sup>As for geographic location, we use the National Statistical Office classification: North-West, North-East, Center, and South. Islands are missing because Sicily and Sardinia are regions with special autonomy and therefore they do not belong to our sample.

<sup>18</sup>We also check for the balancing of time-invariant characteristics by including them as covariates (together with year fixed effects) in the baseline diff-in-disc estimations; point estimates remain almost unchanged and accuracy increases (results available upon request).

<sup>19</sup>See—among others—Ichino and Maggi (2000) and Nannicini, Stella, Tabellini, and Troiano (2010).



period and the year 1998 as the pre-treatment period. All outcome variables are perfectly balanced around the threshold before/after 1999, confirming the assumption that there is no interaction between the DSP and the confounding wage discontinuity.<sup>20</sup>

Finally, we perform a set of placebo tests to evaluate the possibility that our results arise from random chance rather than a causal relationship. In Table 8, we implement the baseline diff-in-disc estimations at fake population thresholds (median on the left and median on the right of the 5,000 threshold). The results are never statistically significant, except for three variables in only one specification out of six and at the 10% level. This further confirms the robustness of our results.

### 5.3 The political economy of budget deficits

In this section, we exploit our quasi-experimental setup to shed light on the political economy of budget deficit. Evaluating the differential response of different politicians or voters to an exogenous (albeit local) variation in fiscal rules can identify important determinants of politically motivated deficits. In particular, we look at three political factors. First, we consider whether mayors face a binding term limit or not, because mayors in their second term have no reelection incentives and no personal stake in the city’s budget for the following years.<sup>21</sup> Second, we consider whether there are many or just a few parties in the city council—which must approve the budget proposed by the mayor—to capture political fragmentation and potential common pool problems. Third, we look at the degree of political competition, which is again related to reelection incentives and may play as a disciplining device.<sup>22</sup>

Results are reported in Table 9, where we implement the baseline diff-in-disc estimations in split samples: (i) binding vs. non-binding term limit; (ii) two parties in the city council (the median value) vs. more than two parties; (iii) mayor’s margin of victory above the median value vs. below. For every heterogeneity exercise, the Wald test p-value indicates whether

---

<sup>20</sup>In the same spirit of the falsification test, we repeat yearly diff-in-disc estimations considering each pre-treatment year (1999 and 2000) as the treatment year, and the preceding year as the control year. The estimates in 2000 are meant to test for the absence of any anticipation effect; the estimates in 1999 test for the absence of any differential trend in the discontinuity at 5,000. As expected, none of these tests detects a statistically significant effect (available upon request).

<sup>21</sup>On the effect of term limit on political accountability and in-office performance, see—among others—Besley and Case (1995) and List and Sturm (2006).

<sup>22</sup>Besley, Persson, and Sturm (2010) and Galasso and Nannicini (2011) show the positive effect of political competition on economic growth and political selection, respectively.

the estimates in the two separate subsample are statistically different from each other. The estimation results show that only mayors that are in their first term and municipalities with high political fragmentation react to the DSP. There is also some evidence that a high degree of political competition is associated with a larger impact of the DSP, but this result is less robust in statistical terms.

Given that mayors without term limit face both stronger reelection concerns and a higher expected probability that they (or their party) will remain in power, the above result provides more support for models linking deficit to reelection incentives (e.g., see Aghion and Bolton, 1990) or to politicians' pandering to voters (e.g., see Maskin and Tirole, 2004) than models viewing deficit as a way to tie the hands of future governments with different political preferences (e.g., see Alesina and Tabellini, 1990; Persson and Svensson, 1989; Tabellini and Alesina, 1990). The result on the number of parties, instead, is consistent with models that explain deficit in terms of political fragmentation or dynamic common pool (e.g., see Persson and Tabellini, 2000), and also with the cross-country evidence that coalition governments lead to higher deficits (see Roubini and Sachs, 1989; Kontopoulos and Perotti, 1999).

Regarding identification, we think that the heterogeneity exercise on term limit is particularly instructive. Indeed, because of the combined fact that (i) Italian mayors face a two-term limit, (ii) municipalities do not vote at the same time, and (iii) the introduction of the DSP is independent of local politics because it followed agreements between the European Union and its member countries in 1999, whether the relaxation of fiscal rules is managed by a mayor with or without a binding term limit is completely random. We therefore remain within a quasi-experimental framework when we look at the differential impact of relaxing fiscal rules for mayors who can or cannot be reelected. We are aware, instead, that the other heterogeneity dimensions (that is, political fragmentation and competition) might be associated with other observable and unobservable city characteristics driving our heterogeneity results. It is reassuring, however, that we detect no other relevant heterogeneity with respect to observable characteristics such as geographic location or income, so that our (political) heterogeneity exercises on the number of parties and on the margin of victory are at least robust to these potential confounding factors.

## 6 Conclusion

Limiting the rise of public debt is a key policy issue in most economies. Fiscal rules are usually mentioned as one of the possible solutions to reduce public debt growth. In this paper, relying on quasi-experimental identification, we show that fiscal rules can be effective in reducing deficit, and hence the accumulation of debt. Our findings also show that fiscal adjustment to the relaxation of fiscal rules is more likely to take place on the side of revenues rather than expenditure. Indeed, in the case of Italian (small) local governments, relaxing fiscal rules substantially increases the budget deficit, and cities for which the rules have been relaxed have lower tax rates and revenues. Additional heterogeneity results on the effectiveness of fiscal rules show that political factors—namely, reelection incentives and bargaining between political parties in the legislative body—are a crucial determinant of deficit bias, which only arises when mayors can run for reelection and many parties seat in the city council.

The scope of this paper is positive in nature, as we estimate the causal impact of relaxing fiscal rules on the budget. We do not have normative suggestions on the determination of optimal fiscal rules. Yet, our findings show that (i) political motivations are likely to be a crucial determinant of budget deficits, and (ii) fiscal rules can be effective also in regulatory environments characterized by serious enforcement and commitment issues. Hence, fiscal rules might be useful in far more cases than those suggested by the conventional wisdom.

We are aware that the enhanced internal validity of our evaluation design comes at the price of lower external validity, as it is always the case in (local) econometric strategies based on policy discontinuities. Our empirical results, however, are completely independent of geography, that is, they are the same in different macro-regions (North-West, North-East, Center, and South). This finding is interesting per se, given the wide range of heterogeneity of Italian cities, which differ not only in terms of economic development, but also in terms of history colonization (South: Spanish domination; North: Austrian-German domination), crime rates, shirking, passive waste, and political accountability. And it is also reassuring about the external validity of the results.

## References

- Aghion, Philippe, Alberto Alesina and Francesco Trebbi (2004). “Endogenous Political Institutions.” *Quarterly Journal of Economics* 119: 565–612.
- Aghion, P. and P. Bolton (1990): “Government Domestic Debt and the Risk of Default: a Political–Economic model of the Strategic Role of Debt,” in R. Dornbusch and M. Draghi (eds), *Public Debt Management: Theory and History*, Cambridge, UK: Cambridge University Press.
- Alesina, A. and T. Bayoumi (1996): “The Costs and Benefits of Fiscal Rules: Evidence from U.S. States,” NBER Working Paper 5614.
- Alesina, A., and R. Perotti (1995): “The Political Economy of Budget Deficits,” *IMF Staff Papers*, 42, 1–31.
- Alesina, A., and R. Perotti (1996): “Fiscal Discipline and the Budget Process,” *American Economic Review Papers and Proceedings*, 86, 401–407.
- Alesina, A., and R. Perotti (1999): “Budget Deficits and Budget Institutions,” in J. Poterba and J. Von Hagen (eds), *Fiscal Institutions and Fiscal Performance*, Chicago, IL: University of Chicago Press.
- Alesina, A., and G. Tabellini (1990): “A Positive Theory of Fiscal Deficits and Government Debt,” *Review of Economic Studies*, 57, 403–414.
- Alesina, A., R. Hausmann, R. Hommes, and E. Stein (1999): “Budget Institutions and Fiscal Performance in Latin America,” *Journal of Development Economics*, 59, 253–273.
- Alt, J., and R. Lowry (1994): “Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States,” *American Political Science Review*, 88, 811–828.
- Auerbach, A. (2006): “Budget Windows, Sunsets, and Fiscal Control,” *Journal of Public Economics*, 90, 87–100.
- Azzimonti, M., M. Battaglini, and S. Coate (2008): “Analyzing the Case for a Balanced Budget Amendment to the U.S. Constitution,” mimeo.
- Bandiera, O., A. Prat, and T. Valletti (2009): “Active and Passive Waste in Government Spending: Evidence from a Policy Experiment,” *American Economic Review*, 99, 1278–1308.
- Barro, R. (1979): “On the Determination of the Public Debt,” *Journal of Political Economy*, 87, 940–71.

- Battaglini, M., and S. Coate (2008): “A Dynamic Theory of Public Spending, Taxation, and Debt,” *American Economic Review*, 98, 201–236.
- Bayoumi, T., and B. Eichengreen (1995): “Restraining Yourself: The Implications of Fiscal Rules for Economic Stabilization,” *IMF Staff Papers*, 42, 32–48.
- Besley, T., and A. Case (1995): “Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits,” *Quarterly Journal of Economics*, 110, 769–798.
- Besley, T., T. Persson, and D. M. Sturm (2010): “Political Competition, Policy and Growth: Theory and Evidence from the United States,” *Review of Economic Studies*, 77, 1329–1352.
- Bohn, H., and R. Inman (1996): “Balanced-Budget Rules and Public Deficits: Evidence from the U.S. States,” *Carnegie-Rochester Conference Series on Public Policy*, 45, 13–76.
- Bordignon, M., T. Nannicini, and G. Tabellini (2010): “Moderating Political Extremism: Single Round vs. Runoff Elections under Plurality Rule,” mimeo.
- Buchanan, J.M., and R.E. Wagner (1977): *Democracy in Deficit*. London: Academic Press.
- Cellini, S.R., F. Ferreira, and J. Rothstein (2011): “The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design,” *Quarterly Journal of Economics*, 125, 215–261.
- Chetty, R., A. Looney, and K. Kroft (2009): “Salience and Taxation: Theory and Evidence,” *American Economic Review*, 99, 1145–77.
- Dickert-Conlin, S., and T. Elder (2010): “Suburban Legend: School Cutoff Dates and the Timing of Births,” *Economics of Education Review*, 29, 826–841.
- Fatás, A., and I. Mihov (2006): “The Macroeconomic Effects of Fiscal Rules in the US States,” *Journal of Public Economics*, 90, 101–117.
- Feld, L., and G. Kirchgassner (2006): “On the Effectiveness of Debt Brakes: The Swiss Experience,” CREMA Working Paper 2006/21.
- Gagliarducci, S., and T. Nannicini (2011): “Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection,” *Journal of the European Economic Association*, forthcoming.
- Galasso, V., and T. Nannicini (2011): “Competing on Good Politicians,” *American Political Science Review*, 105, 79–99.

- Givati, Y., and U. Troiano (2011): “Law, Economics and Culture: Theory and Evidence from Maternity Leave Laws,” Harvard Law and Economics Working Paper.
- Hahn, J., P. Todd, and W. Van der Klaauw (2001): “Identification and Estimation of Treatment Effects with Regression Discontinuity Design,” *Econometrica*, 69, 201–209.
- Hallerberg, M., and J. Von Hagen (1999): “Electoral Institutions, Cabinet Negotiations, and Budget Deficits in the EU,” in J. Poterba and J. Von Hagen (eds), *Fiscal Institutions and Fiscal Performance*, Chicago, IL: University of Chicago Press.
- Ichino, A., and G. Maggi (2000): “Work Environment and Individual Background: Explaining Regional Shirking Differentials in a Large Italian Firm,” *Quarterly Journal of Economics*, 115, 1057–90.
- Kaufmann, D., A. Kraay, and M. Mastruzzi (2010): “The Worldwide Governance Indicators: Methodology and Analytical Issues,” World Bank Policy Research Working Paper 5430.
- Kontopoulos, Y., and R. Perotti (1999): “Government Fragmentation and Fiscal Policy Outcomes: Evidence from OECD Countries,” in J. Poterba and J. Von Hagen (eds), *Fiscal Institutions and Fiscal Performance*, Chicago, IL: University of Chicago Press.
- Krogstrup, S., and S. Wälti (2007): “Do Fiscal Rules Cause Budgetary Outcomes?” TEP Working Paper 0607.
- Lemieux, T., and K. Milligan (2008): “Incentive Effects of Social Assistance: A Regression Discontinuity Approach,” *Journal of Econometrics*, 142, 807–828.
- List, J.A., and D.M. Sturm (2006): “How Elections Matter: Theory and Evidence from Environmental Policy,” *Quarterly Journal of Economics*, 121, 1249–1281.
- Lizzeri, A. (1999): “Budget Deficits and Redistributive Politics,” *Review of Economic Studies*, 66, 909–928.
- Lucas, R., and N. Stokey (1983): “Optimal Fiscal and Monetary Policy in an Economy without Capital,” *Journal of Monetary Economics*, 12, 55–94.
- Maskin, E., and J. Tirole (2004): “The Politician and the Judge: Accountability in Government,” *American Economic Review*, 94, 1034–1054.
- McCrary, J. (2008): “Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test,” *Journal of Econometrics*, 142, 698–714.
- Nannicini, T., A. Stella, G. Tabellini, and U. Troiano (2010): “Social Capital and Political Accountability,” CEPR Discussion Paper 7782.

- Olson, M. (1965): *The Logic of Collective Action*. Cambridge, MA: Harvard University Press.
- Persson, T., and G. Tabellini (2000): *Political Economics*. Cambridge, MA: MIT Press.
- Pettersson-Lidbom, P. (2001): “An Empirical Investigation of the Strategic Use of Debt,” *Journal of Political Economy*, 109, 570–584.
- Pettersson-Lidbom, P. (2008): “Does the Size of the Legislature Affect the Size of Government: Evidence from Two Natural Experiments,” mimeo.
- Poterba, J. (1994): “State Responses to Fiscal Crises: The Effects of Budgetary Institutions and Politics,” *Journal of Political Economy*, 102, 799–821.
- Poterba, J. (1996): “Budget Institutions and Fiscal Policy in the U.S. States,” *American Economic Review Papers and Proceedings*, 86, 395–400.
- Poterba, J., and J. Von Hagen (eds) (1999): *Fiscal Institutions and Fiscal Performance*. Chicago, IL: University of Chicago Press.
- Rodden, J.A., G.S. Eskeland, and J. Litvack (eds) (2003): *Fiscal Decentralization and the Challenge of Hard Budget Constraints*. Cambridge, MA: MIT Press.
- Roubini, N., and J. Sachs (1989): “Political and Economic Determinants of Budget Deficits in the Industrial Democracies,” *European Economic Review*, 33, 903–938.
- Tabellini, G., and A. Alesina (1990): “Voting on the Budget Deficit,” *American Economic Review*, 80, 37–49.
- Von Hagen, J. (1991): “A Note on the Empirical Effectiveness of Formal Fiscal Restraints,” *Journal of Public Economics*, 44, 199–210.
- Weingast, B.R., K.A. Shepsle, and C. Johnsen (1981): “The Political Economy of Benefits and costs: A Neoclassical Approach to Distributive Politics,” *Journal of Political Economy*, 89, 642–664.
- Yared, P. (2010): “Politicians, Taxes and Debt,” *The Review of Economic Studies*, 77, 806–40.

## Tables and figures

**Table 1: Legislative thresholds for Italian municipalities in 1997–2004**

Population	Wage Mayor	Wage Ex. Com.	Ex. Com. Size	Council Size	Electoral Rule	Domestic Stability Pact
Below 1,000	1,291	15%	4	12	single	until 2001
1,000-3,000	1,446	20%	4	12	single	until 2001
3,000-5,000	2,169	20%	4	16	single	until 2001
5,000-10,000	2,789	50%	4	16	single	yes
10,000-15,000	3,099	55%	6	20	single	yes
15,000-30,000	3,099	55%	6	20	runoff	yes
30,000-50,000	3,460	55%	6	30	runoff	yes
50,000-100,000	4,132	75%	6	30	runoff	yes
100,000-250,000	5,010	75%	10	40	runoff	yes
250,000-500,000	5,784	75%	12	46	runoff	yes
Above 500,000	7,798	75%	14-16	50-60	runoff	yes

Notes. Policies varying at different legislative thresholds in the period 1999–2004. *Population* is the number of resident inhabitants as measured by the last available Census. *Wage Mayor* and *Wage Ex. Com.* refer to the monthly gross wage of the mayor and the members of the executive committee, respectively; the latter is expressed as a percentage of the former, which refers to 2000 and is measured in euros. The wage thresholds at 1,000 and 10,000 were introduced in 2000; all of the others date back to 1960. *Ex. Com. Size* is the maximum allowed number of executives appointed by the mayor. *Council Size* is the number of seats in the City Council. All of the size thresholds were set in 1960. Since 1993, *Electoral Rule* can be either single round (with 60% premium) or runoff (with 66% premium) plurality voting. *Domestic Stability Pact* is a set of fiscal rules imposed by the central government to discipline the fiscal management of local governments.

**Table 2: The rules of the Domestic Stability Pact**

Year	Target	Covered municipalities
1997	None	All
1998	None	All
1999	Balanced budget (cash)	All
2000	Balanced budget (cash)	All
2001	Balanced budget (cash)	Above 5,000
2002	Balanced budget (cash)	Above 5,000
2003	Balanced budget (cash/accrual)	Above 5,000
2004	Balanced budget (cash/accrual)	Above 5,000

Notes. The *Domestic Stability Pact* is a set of fiscal rules imposed by the central government to discipline the fiscal management of local governments. Legislative sources: yearly national budget law (*Legge Finanziaria*) from 1999 to 2004.



**Table 3: Outcome variables, descriptive statistics**

	Above 5,000	Below 5,000
<b>A. Fiscal discipline</b>		
Fiscal gap	170.673	208.560
Deficit	11.078	15.452
<b>B. Expenditures</b>		
Current outlays	503.981	532.128
Capital outlays	439.025	508.634
<b>C. Revenues</b>		
Taxes	194.887	175.825
Fees & tariffs	56.601	58.938
Central transfers	102.052	128.337
Regional transfers	86.674	94.870
Other revenues	491.714	567.340
<b>D. Tax instruments</b>		
Real estate tax rate	0.587	0.576
Income tax surcharge	0.309	0.309
Obs.	2,970	3,330

Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1999 and 2004. The average values of per-capita budget variables are in 2009 Euros. The real estate tax rate and the income tax surcharge are in percentage points; the former can vary from 0.4 to 0.7 percent; the latter can vary from 0 to 0.5 percent.

**Table 4: Fiscal discipline, time pattern**

Year	Above 5,000	Below 5,000
<b>Fiscal gap</b>		
1999	152.168	178.035
2000	149.960	177.663
2001	176.932	209.151
2002	172.686	218.899
2003	192.234	240.121
2004	180.058	227.491
<b>Deficit</b>		
1999	11.546	10.777
2000	13.197	15.680
2001	9.766	13.588
2002	7.483	15.780
2003	12.932	20.067
2004	11.541	16.821

Notes. Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1999 and 2004. The average values of per-capita budget variables are in 2009 Euros.

Table 5: The effect of relaxing fiscal rules, diff-in-disc estimates

	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$	Spline $3^{rd}$	Spline $2^{nd}$	Spline $4^{th}$
<b>A. Fiscal discipline</b>						
Fiscal gap	154.835*** (48.485)	61.736* (32.576)	57.827** (25.010)	102.180*** (38.452)	75.797** (31.956)	108.107** (48.357)
Deficit	22.081* (13.244)	17.355** (8.375)	19.231*** (6.255)	21.445** (9.483)	24.295*** (8.118)	25.106** (12.753)
<b>B. Expenditures</b>						
Current outlays	-85.016 (88.498)	-54.410 (59.915)	-5.789 (37.954)	-35.660 (59.494)	-15.061 (44.630)	-62.584 (83.810)
Capital outlays	59.681 (114.666)	42.316 (87.277)	44.475 (63.507)	92.268 (103.118)	3.534 (92.556)	202.323 (139.989)
<b>C. Revenues</b>						
Taxes	-94.839** (37.642)	-45.248* (25.980)	-36.779* (19.185)	-57.028** (27.193)	-40.759* (21.681)	-85.077** (35.162)
Fees & tariffs	-9.510 (13.834)	-3.359 (10.214)	0.100 (7.416)	1.173 (10.601)	-2.180 (8.864)	-4.051 (13.910)
Central transfers	75.663*** (27.678)	18.640 (19.222)	20.483 (15.545)	37.138* (21.902)	25.617 (18.945)	37.915 (26.950)
Regional transfers	49.097 (31.332)	23.890 (19.344)	16.523 (15.014)	41.261* (24.352)	25.037 (20.303)	42.712 (31.169)
Other revenues	-67.828 (149.493)	-23.372 (108.236)	19.128 (72.810)	12.619 (118.429)	-43.539 (102.188)	123.134 (165.612)
<b>D. Tax instruments</b>						
Real estate tax rate	-0.083** (0.033)	-0.040* (0.024)	-0.028 (0.018)	-0.056** (0.026)	-0.045** (0.021)	-0.060* (0.033)
Income tax surcharge	-0.093* (0.054)	-0.036 (0.036)	-0.057** (0.029)	-0.058 (0.041)	-0.043 (0.035)	-0.111** (0.051)
Obs.	1,012	2,080	3,068	6,300	6,300	6,300

Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1999 and 2004. Diff-in-disc estimates of the impact of relaxing fiscal rules on budget variables and tax instruments below 5,000 after 2001. All budget variables are per capita and in 2009 Euros. Tax instruments are in percentage points; the real estate tax rate can vary from 0.4 to 0.7 percent; the income tax surcharge can vary from 0 to 0.5 percent. Estimation methods: local linear regression as in equation (3), with bandwidth  $h = 250$ ,  $h = 500$ , and  $h = 750$ ; spline polynomial approximation as in equation (4), with  $3^{rd}$ ,  $2^{nd}$ , and  $4^{th}$ -order polynomial. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

**Table 6: Balance tests of municipal characteristics, diff-in-disc estimates**

	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$	Spline $3^{rd}$	Spline $2^{nd}$	Spline $4^{th}$
<b>Time-invariant characteristics</b>						
North-West	-0.012 (0.168)	0.012 (0.109)	0.057 (0.088)	0.043 (0.122)	0.081 (0.109)	0.167 (0.152)
North-East	0.056 (0.127)	0.072 (0.088)	0.039 (0.071)	0.042 (0.095)	0.031 (0.085)	-0.039 (0.115)
Center	-0.153 (0.139)	-0.043 (0.096)	-0.032 (0.079)	-0.071 (0.106)	-0.051 (0.094)	-0.181 (0.133)
South	0.110 (0.110)	-0.041 (0.075)	-0.064 (0.064)	-0.013 (0.088)	-0.061 (0.082)	0.053 (0.110)
Area size	-9.080 (11.898)	-2.863 (9.041)	-0.751 (7.374)	-0.063 (10.195)	5.728 (9.030)	-7.803 (11.729)
Sea level	-20.185 (63.217)	-10.800 (44.481)	-9.053 (34.398)	-17.736 (46.537)	3.585 (37.502)	9.258 (59.757)
<b>Potentially endogenous characteristics</b>						
Taxable income	-0.133 (0.154)	0.116 (0.113)	0.012 (0.088)	0.042 (0.120)	0.021 (0.104)	0.115 (0.151)
Female mayor	-0.068 (0.101)	-0.078 (0.072)	-0.083 (0.064)	-0.112 (0.079)	-0.079 (0.066)	-0.068 (0.095)
Mayor's age	-1.701 (4.118)	0.457 (2.846)	-1.361 (2.166)	-0.770 (3.114)	-2.087 (2.558)	-0.907 (3.930)
Mayor's schooling	2.131* (1.174)	0.627 (0.839)	0.682 (0.645)	0.796 (0.929)	0.221 (0.752)	1.165 (1.155)
Mayor's tenure	2.009 (1.979)	0.551 (1.378)	-0.081 (1.110)	0.444 (1.497)	0.526 (1.254)	1.287 (1.848)
Term limit	-0.172 (0.192)	-0.097 (0.136)	-0.075 (0.107)	-0.136 (0.150)	-0.176 (0.122)	-0.123 (0.184)
No. of parties	-0.146 (0.642)	-0.394 (0.440)	-0.200 (0.346)	-0.498 (0.485)	-0.244 (0.381)	-0.958 (0.621)
Obs.	1,012	2,080	3,068	6,300	6,300	6,300

Notes. Municipalities between 3,500 and 7,000 inhabitants. For time-invariant characteristics: diff-in-disc estimates with changing population levels (1991 Census before 2001 and 2001 Census after 2001). For potentially endogenous characteristics: baseline diff-in-disc estimates. All time-invariant characteristics are dummies except area size (in km<sup>2</sup>) and sea level (in meters). Potentially endogenous characteristics: taxable income at the municipal level is per capita and in 2009 Euros; mayor's age, schooling, and tenure are expressed in years; female mayor and term limit are dummies; number of parties refer to political parties seating in the city council. Estimation methods: local linear regression with bandwidth  $h = 250$ ,  $h = 500$ , and  $h = 750$ ; spline polynomial approximation with  $3^{rd}$ ,  $2^{nd}$ , and  $4^{th}$ -order polynomial. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

**Table 7: Falsification test in 1999, diff-in-disc estimates**

	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$	Spline $3^{rd}$	Spline $2^{nd}$	Spline $4^{th}$
<b>A. Fiscal discipline</b>						
Fiscal gap	1.480 (34.129)	-0.018 (25.906)	-492.980 (499.176)	-788.490 (801.826)	-126.210 (132.911)	-201.128 (208.106)
Deficit	8.447 (14.010)	5.064 (9.558)	-493.178 (498.697)	-785.705 (801.214)	-126.551 (131.305)	-189.388 (205.811)
<b>B. Expenditures</b>						
Current outlays	3.232 (42.220)	-2.533 (31.310)	-5.676 (27.991)	-6.832 (37.338)	5.846 (30.668)	-6.318 (45.161)
Capital outlays	59.681 (114.666)	42.316 (87.277)	44.475 (63.507)	92.268 (103.118)	3.534 (92.556)	202.323 (139.989)
<b>C. Revenues</b>						
Taxes	-0.242 (19.364)	-3.181 (14.356)	-1.159 (12.881)	-4.460 (15.247)	-3.855 (13.531)	0.072 (18.740)
Fees & tariffs	-3.698 (7.747)	-0.694 (5.249)	-1.435 (5.022)	-4.812 (5.693)	-3.962 (5.081)	-3.434 (6.665)
Central transfers	-1.303 (19.578)	-1.727 (14.489)	1.666 (11.549)	0.018 (15.490)	3.745 (12.184)	-4.164 (19.020)
Regional transfers	-4.226 (18.451)	-2.690 (13.644)	-1.449 (11.200)	-2.683 (14.470)	-3.721 (11.779)	-6.408 (17.686)
Other revenues	-71.952 (112.409)	8.229 (76.557)	443.683 (504.645)	792.353 (808.340)	157.121 (154.850)	133.753 (237.930)
<b>D. Tax instruments</b>						
Real estate tax rate	-0.121 (0.276)	0.016 (0.201)	0.068 (0.162)	0.021 (0.214)	-0.018 (0.172)	-0.037 (0.267)
Obs.	534	1,056	1,554	3,495	3,495	3,495

Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1998 and 2001. Diff-in-disc estimates of the impact of the introduction of fiscal rules on budget variables below 5,000 after 1999 (when no discontinuity was introduced by the DSP). All budget variables are per capita and in 2009 Euros. The real estate tax rate is in percentage points; and it can vary from 0.4 to 0.7 percent. No information available on the income tax surcharge before 1999. Estimation methods: local linear regression as in equation (3), with bandwidth  $h = 250$ ,  $h = 500$ , and  $h = 750$ ; spline polynomial approximation as in equation (4), with  $3^{rd}$ ,  $2^{nd}$ , and  $4^{th}$ -order polynomial. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

**Table 8: Placebo tests at fake thresholds, diff-in-disc estimates**

	Median on the left			Median on the right		
	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$
<b>A. Fiscal discipline</b>						
Fiscal gap	32.575 (42.099)	9.219 (30.354)	11.767 (26.934)	40.736 (36.646)	-26.837 (29.151)	-2.493 (54.334)
Deficit	18.082 (15.405)	15.314 (12.031)	26.722 (20.169)	9.420 (8.737)	9.085 (7.048)	6.305 (6.069)
<b>B. Expenditures</b>						
Current outlays	106.663 (85.200)	35.141 (53.843)	19.579 (43.619)	-15.554 (71.760)	27.708 (50.351)	2.269 (43.680)
Capital outlays	4.470 (233.899)	30.414 (115.309)	81.520 (105.887)	-105.258 (172.050)	-212.917 (181.016)	4.716 (80.232)
<b>C. Revenues</b>						
Taxes	11.301 (41.101)	-10.045 (25.961)	-5.929 (21.806)	1.808 (37.357)	44.943* (26.030)	10.908 (22.850)
Fees & tariffs	26.775* (13.848)	7.635 (9.039)	7.810 (7.569)	-10.306 (12.429)	6.122 (8.862)	4.523 (7.711)
Central transfers	-12.461 (26.008)	-25.300 (18.266)	-24.410 (15.156)	20.153 (31.673)	-18.634 (21.769)	-6.813 (18.191)
Regional transfers	32.002 (27.681)	18.712 (18.894)	23.960 (17.175)	10.785 (26.595)	-20.175 (22.469)	-5.751 (16.357)
Other revenues	32.358 (255.813)	60.337 (122.318)	88.554 (110.050)	-152.671 (184.616)	-206.549 (185.994)	-2.187 (91.027)
<b>D. Tax instruments</b>						
Real estate tax rate	0.024 (0.028)	-0.003 (0.018)	-0.013 (0.016)	0.025 (0.034)	0.028 (0.024)	0.005 (0.020)
Income tax surcharge	0.063 (0.040)	0.023 (0.030)	0.019 (0.026)	-0.058 (0.054)	-0.066* (0.035)	-0.045 (0.031)
Obs.	1,290	2,488	3,436	786	1,600	2,390

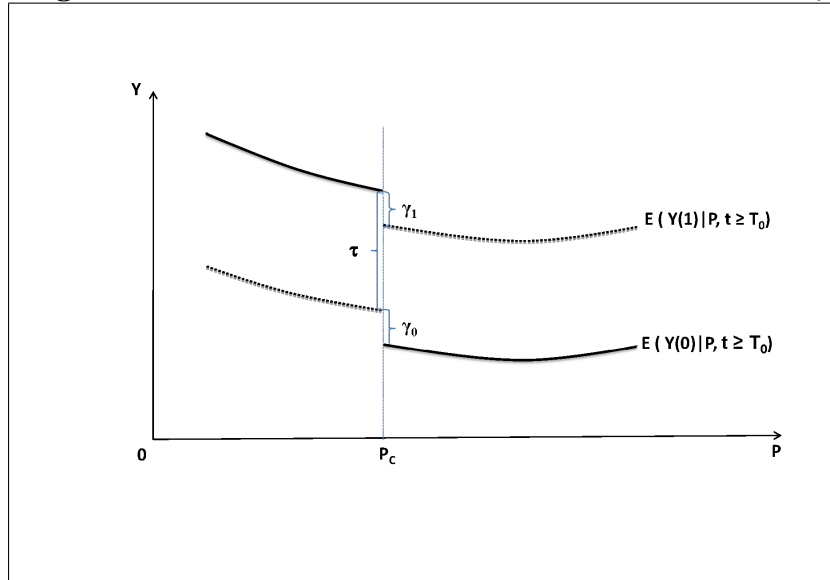
Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1999 and 2004. Diff-in-disc estimates at fake thresholds: median on the left of 5,000 (i.e., 4,244) and median on the right (i.e., 5,793). Estimation methods: local linear regression as in equation (3), with bandwidth  $h = 250$ ,  $h = 500$ , and  $h = 750$ . Robust standard errors clustered at the city level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

**Table 9: Heterogeneity results, budget deficit and political factors**

	LLR $h = 250$	LLR $h = 500$	LLR $h = 750$	Spline $3^{rd}$	Spline $2^{nd}$	Spline $4^{th}$
<b>With binding term limit:</b>						
Deficit	1.652 (12.566)	0.970 (8.462)	7.862 (6.956)	4.020 (9.597)	6.225 (7.590)	5.600 (12.081)
Obs.	447	920	1,375	2,780	2,780	2,780
<b>Without binding term limit:</b>						
Deficit	29.685 (20.939)	29.525** (13.117)	27.182*** (9.426)	33.039** (14.467)	37.052*** (12.670)	36.633* (20.328)
Obs.	565	1,160	1,693	3,520	3,520	3,520
<i>Wald test p-value</i>	0.048	0.037	0.043	0.029	0.012	0.049
<b>With two parties:</b>						
Deficit	2.285 (12.888)	0.721 (9.509)	2.752 (7.648)	3.277 (10.367)	9.230 (8.299)	2.994 (12.376)
Obs.	604	1,187	1,721	3,584	3,584	3,584
<b>With more than two parties:</b>						
Deficit	60.475** (29.936)	44.089*** (15.427)	42.950*** (12.080)	50.859*** (19.442)	49.302*** (18.152)	69.613** (27.407)
Obs.	408	893	1,347	2,716	2,716	2,716
<i>Wald test p-value</i>	0.218	0.057	0.005	0.081	0.079	0.076
<b>With margin of victory above median:</b>						
Deficit	18.539 (16.618)	8.367 (10.439)	11.909 (8.404)	10.008 (11.765)	11.770 (8.641)	19.158 (15.097)
Obs.	535	1,106	1,669	3,317	3,317	3,317
<b>With margin of victory below median:</b>						
Deficit	25.451 (25.007)	27.581** (13.305)	28.898*** (10.971)	36.676** (16.197)	41.069*** (15.034)	35.938 (22.261)
Obs.	477	974	1,399	2,983	2,983	2,983
<i>Wald test p-value</i>	0.466	0.142	0.217	0.052	0.073	0.251

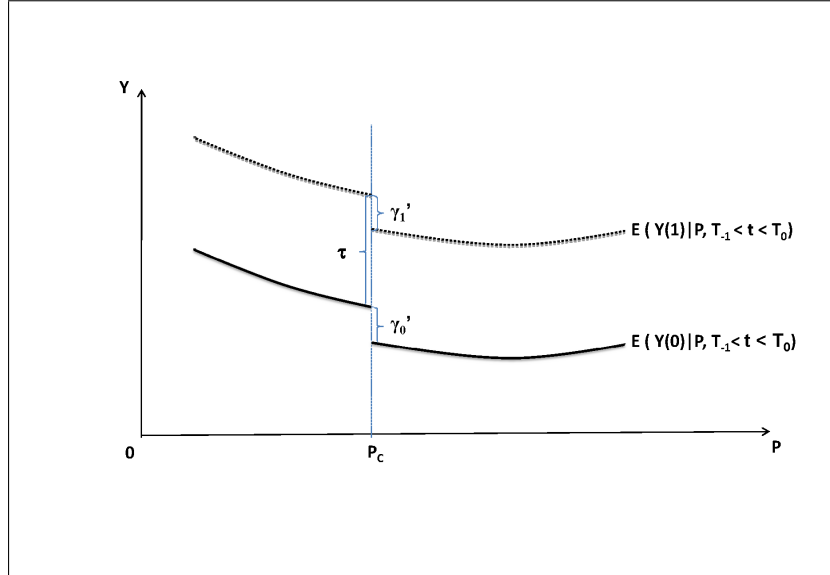
Notes. Municipalities between 3,500 and 7,000 inhabitants; budget years between 1999 and 2004. Diff-in-disc estimates of the impact of relaxing fiscal rules on fiscal discipline below 5,000 after 2001 in different subsamples. The *Wald test p-value* evaluates whether the estimates are statistically different in the two subsamples. All variables are per capita and in 2009 Euros. Estimation methods: local linear regression as in equation (3), with bandwidth  $h = 250$ ,  $h = 500$ , and  $h = 750$ ; spline polynomial approximation as in equation (4), with  $3^{rd}$ ,  $2^{nd}$ , and  $4^{th}$ -order polynomial. Robust standard errors clustered at the municipality level are in parentheses. Significance at the 10% level is represented by \*, at the 5% level by \*\*, and at the 1% level by \*\*\*.

Figure 1: Potential and observed outcomes after  $T_0$



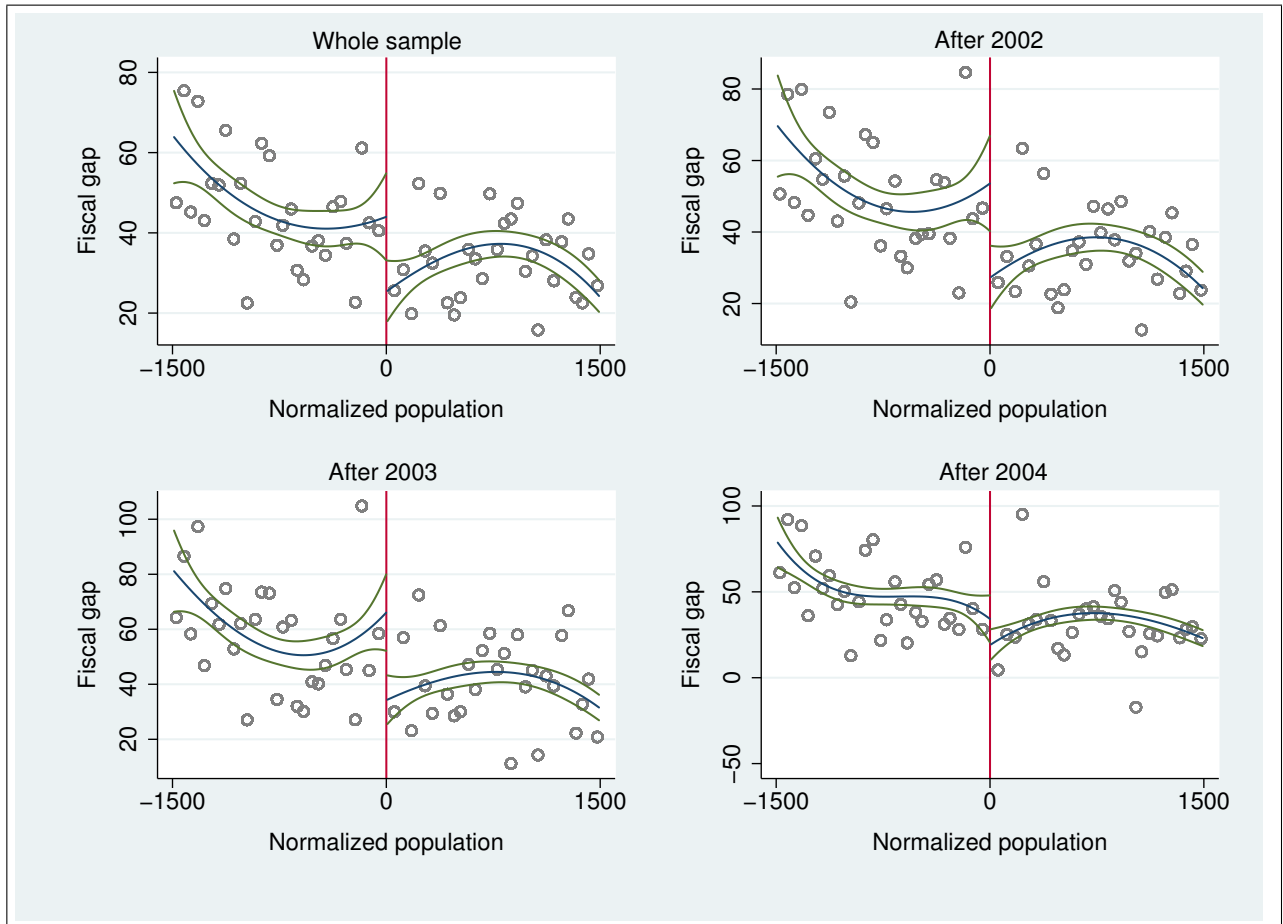
Notes. Theoretical situation after treatment ( $t \geq T_0$ ).  $P$  is the running variable and  $P_c$  the policy threshold. Dashed lines represent potential outcomes in the case of treatment,  $Y(1)$ , and no treatment,  $Y(0)$ . Solid line is the observed outcomes,  $Y$ .

Figure 2: Potential and observed outcomes before  $T_0$



Notes. Theoretical situation before treatment ( $t < T_0$ ).  $P$  is the running variable and  $P_c$  the policy threshold. Dashed lines represent potential outcomes in the case of treatment,  $Y(1)$ , and no treatment,  $Y(0)$ . Solid line is the observed outcomes,  $Y$ .

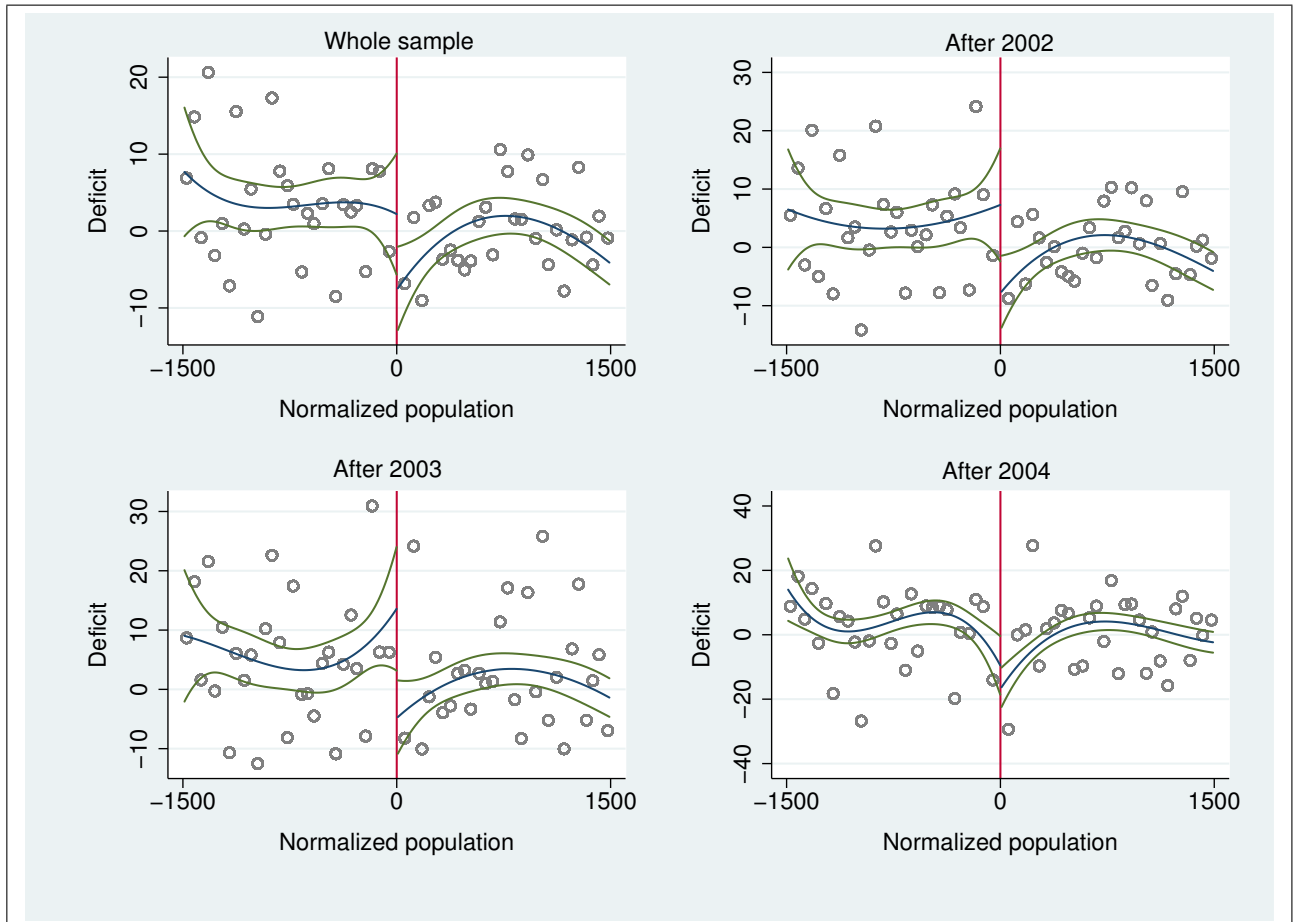
Figure 3: Diff-in-disc discontinuities in fiscal gap



Notes. Vertical axis: difference of each post-2001 outcome value and each pre-2001 outcome value. Horizontal axis: actual population size minus 5,000. The central line is a spline 3<sup>rd</sup>-order polynomial fit; the lateral lines represent the 95% confidence interval. Scatter points are averaged over intervals of 50 inhabitants.

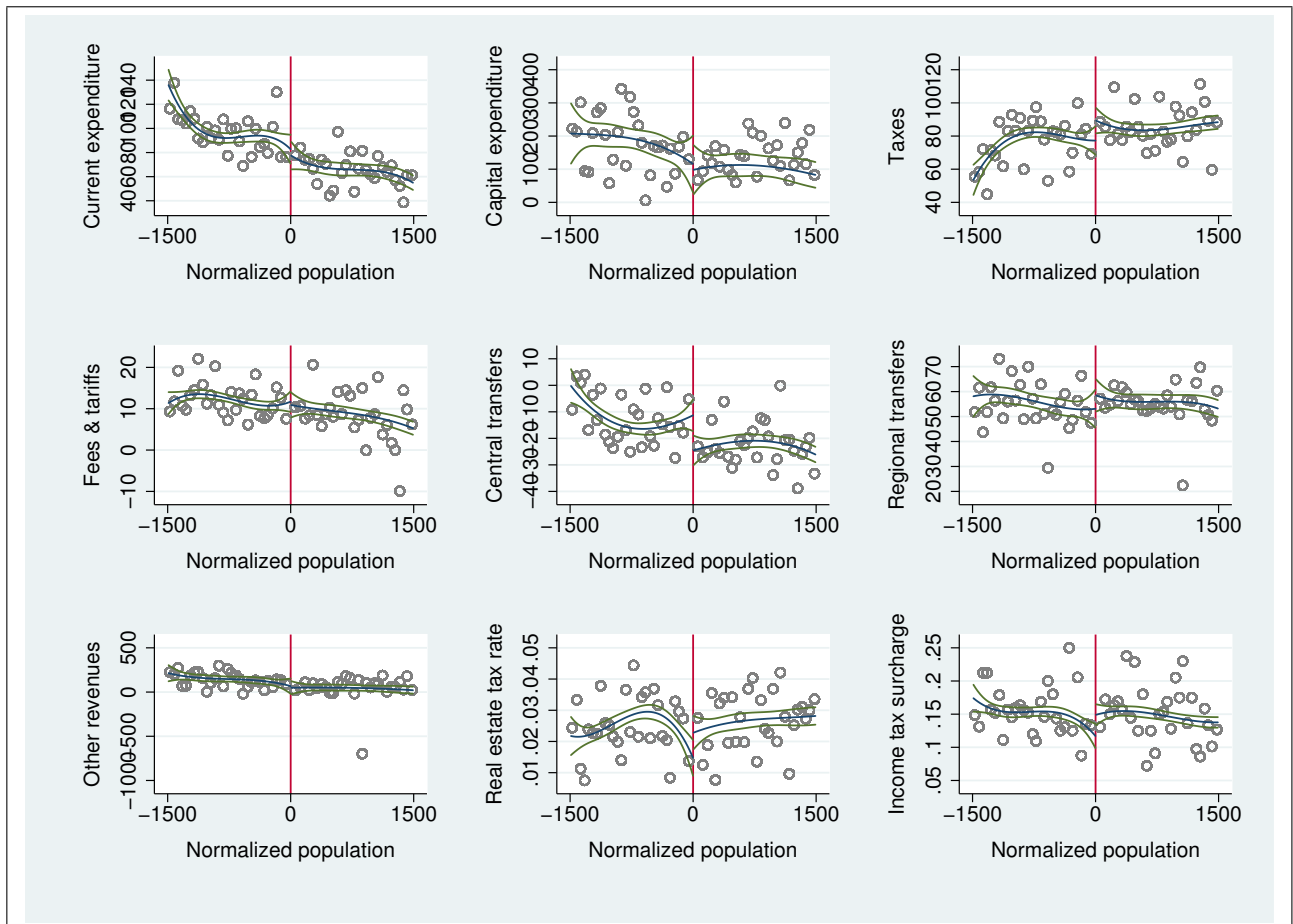


Figure 4: Diff-in-disc discontinuities in deficit



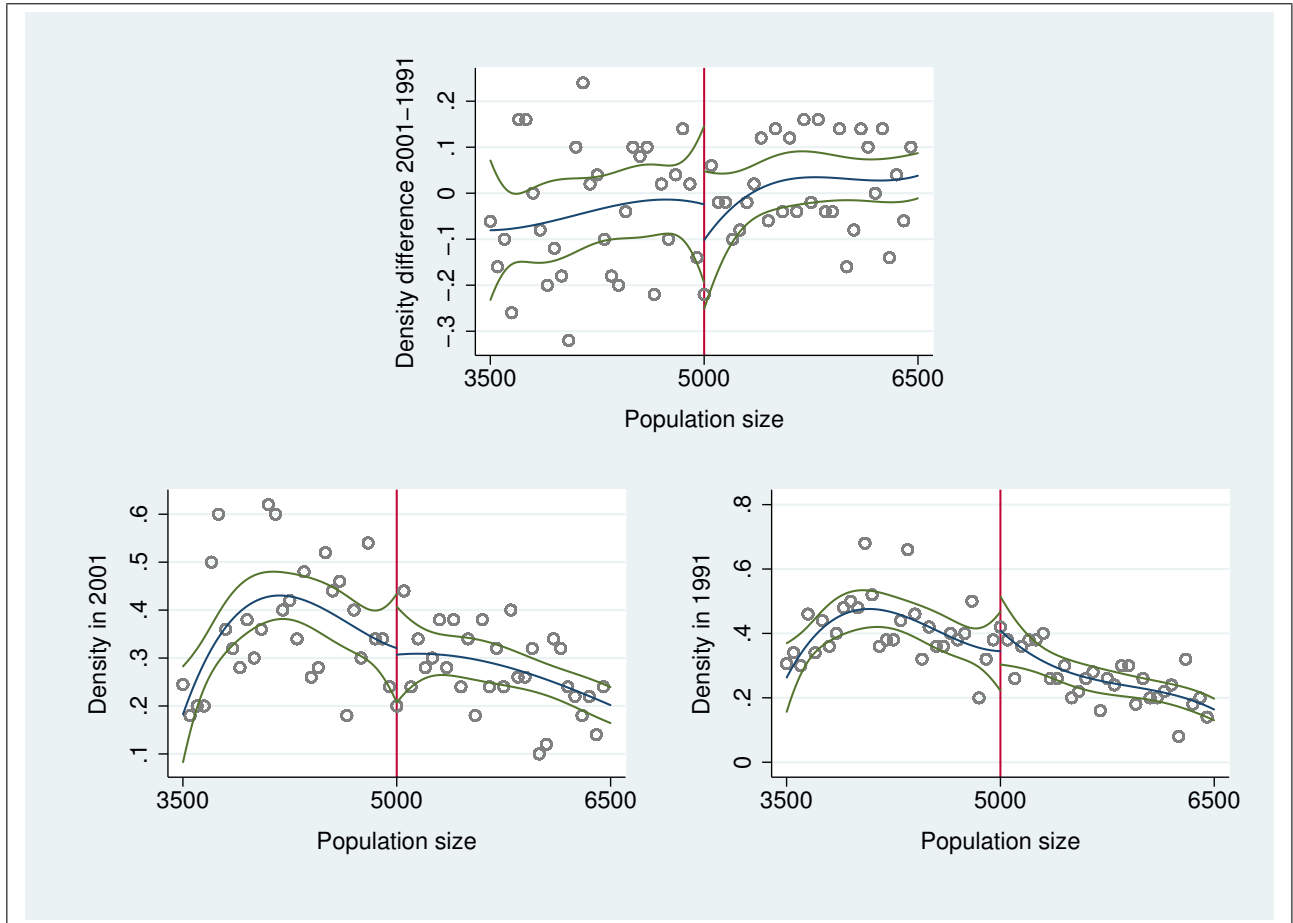
Notes. Vertical axis: difference of each post-2001 outcome value and each pre-2001 outcome value. Horizontal axis: actual population size minus 5,000. The central line is a spline 3<sup>rd</sup>-order polynomial fit; the lateral lines represent the 95% confidence interval. Scatter points are averaged over intervals of 50 inhabitants.

Figure 5: Diff-in-disc discontinuities in budget items and tax instruments



Notes. Vertical axis: difference of each post-2001 outcome value and each pre-2001 outcome value. Horizontal axis: actual population size minus 5,000. The central line is a spline 3<sup>rd</sup>-order polynomial fit; the lateral lines represent the 95% confidence interval. Scatter points are averaged over intervals of 50 inhabitants.

Figure 6: Diff-in-disc density test



Notes. Test of the continuity at 5,000 of: (i) the difference between the density in the 2001 Census and in the 1991 Census (top graph); (ii) the density in the 2001 Census (bottom left graph); and (iii) the density in the 1991 Census (bottom right graph). The central line is a spline  $3^{rd}$ -order polynomial fit in population size; the lateral lines represent the 95% confidence interval. Scatter points are averaged over intervals of 50 inhabitants.