PAPER

Title: Fiscal rules and budget forecast errors of Italian municipalities

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Abstract: *(minimum1500 words)*

We study the impact of the domestic stability pact on the budget forecast errors of Italian municipalities. The identification of the causal effect exploits a quasi-natural experiment generated by the removal in 2001 of the fiscal restraints on budget decisions for municipalities with less than 5,000 inhabitants and by stricter budgetary restrictions and severe penalties for noncompliers in 2002. We find that relaxing fiscal rules had a sizeable impact on budget forecast errors, especially in 2002. Revenue (expenditure) forecast errors for municipalities below 5,000 inhabitants became indeed 26% (22%) larger than in the past.

Keywords: budget forecast errors, sub-central fiscal rules, Italian municipalities, quasi-natural experiment, difference-in-discontinuities design
JEL codes: E62, H68, H72

1 Introduction

Over the past two decades, many decentralized countries have used sub-central fiscal rules to enforce local fiscal discipline, thus avoiding excessive spending and excessive debt of local governments. It has been indeed recognized that high levels of sub-central deficit could turn into higher levels of central government spending and debt (Fornasari et al., 2000), undermining the long-term sustainability of national public finances. In some European Union (EU) countries, e.g. Austria, Belgium, Spain, Italy, the adoption of sub-central fiscal rules was the result of keeping faith with budget agreements taken at the supranational level through the Stability and Growth Pact (SGP).[1]

Fiscal rules have been often evoked as a useful tool to curb fiscal indiscipline, especially of local authorities, and to prevent biased budget estimates that are responsible for excessive deficit (Von Hagen and Harden, 1994; Alesina and Perotti, 1996; Frankel, 2011; Chatagny and Soguel, 2012; Frankel and Schreger, 2013). Systematic biased formulation of fiscal variables has indeed important drawbacks in the creation of structural deficit and public debt accumulation to the detriment of local welfare (Boukari and Veiga, 2018) and long-term national fiscal sustainability. However, fiscal rules have been accused of forcing discretionary pro-cyclical fiscal policy (Marinheiro, 2008) and creating "window dressing" measures (Milesi-Ferretti, 2004; Balduzzi and Grembi, 2011) and excessive optimism in official budget forecasts (Frankel, 2011; Frankel and Schreger, 2013), especially in the run-up to elections (Brück and Stephan, 2006; Pina and Venes, 2011). There is a rich empirical literature which has found controversial results using cross-country data. Empirical studies on the impact of sub-central fiscal rules at the local level are instead much scarcer. Luechinger and Schaltegger (2013) and Chatagny (2015), who exploited the variation in fiscal rules across the Swiss cantons, found that sub-central fiscal rules are effective in improving budgetary forecasting.

In 1999 and 2000, all Italian municipalities were subject to the Domestic Stability Pact (DSP), sub-central fiscal rules restraining the budgetary liberty of local governments. The DSP embraced a "carrot and stick" approach, to encourage virtuous behaviour and punish noncompliers. However, since 2001 the municipalities below 5,000 inhabitants

[1] Sub-central fiscal rules are generally the result of formal negotiations between the central and sub-national governments (especially in federal countries) or obligations imposed by the national government to contain overspending and excessive indebtedness of local governments.

have been exempted from the DSP.[2] In 2002, more stringent budgetary restrictions and severe penalties for the violation of the pact were introduced to discourage further not-complying municipal budgeting decisions. The DSP has been at the centre of numerous political debates, fuelled by the protests of the mayors, who have required the relaxation of its constraints, especially those on investment spending, if not even the elimination of the pact. Grembi et al. (2016) showed that the Italian municipalities affected by the DSP have significantly increased their taxes and decreased their deficit, suggesting that the DSP was effective in favouring the sustainability of public finances. However, the misrepresentation of budgetary decisions in the Italian panorama is a further focal issue, which was not investigated by Grembi et al. (2016). It is indeed well-documented in Ceparulo et al. (2014) and Anessi-Pessina and Sicilia (2015) for national and local context, respectively.

In this study, we analyze the effects of the DSP on budgetary projections of Italian municipalities. The analysis of the Italian context is very interesting. There are indeed high levels of compliance among the municipalities (Brugnano and Rapallini, 2009; Balduzzi and Grembi, 2011), which could be a measure of the DSP effectiveness. However, this may reflect creative accounting to circumvent the fiscal restraints (Balduzzi and Grembi, 2011). Furthermore, the removal in 2001 of fiscal restrictions on budgetary decisions for municipalities with less than 5,000 inhabitants leads to a quasi-natural experiment to credibly identify the causal effect of the sub-central budget rule. Our contribution to the literature is threefold. First, we shed more light on the relation between local fiscal rules and budgetary behaviour of local governments by focusing on the impact on budgetary forecast accuracy, a dimension of the budget that Grembi et al. (2016) overlooked. It is indeed very important how and to what extent the DSP affects municipal budget forecasting because repeated forecasting errors in local government fiscal variables could frustrate the efforts made by the central government to consolidate national public finances and worsen local welfare. Second, we provide robust estimates of the causal effect on budget forecast errors by exploiting: i) the quasi-natural experiment generated by the exemption in 2001 from the DSP of the municipalities below 5,000 inhabitants, following the identification strategy in Grembi et al. (2016); ii) stricter budgetary restrictions and severe penalties for noncompliers in 2002. Third, we study whether the effect was heterogeneous

[2] In more recent years, the population threshold has been lowered by re-including municipalities with a population between 1,000 and 5,000 inhabitants.

across municipalities with different characteristics, so as to enrich further the scenario and speculate on the mechanisms behind the change in budgetary forecast behaviour of local administrators.

We find that the 2001 removal of the fiscal restraints for small municipalities and the introduction of incentives for compliers did not affect the forecast errors of either revenues or expenditures. However, once in 2002 the ceiling on current expenditure growth was introduced together with more severe penalties for noncompliers, the difference between municipalities just below and above the population threshold became sizeable and significant. We find indeed that revenue (expenditure) forecast errors of municipalities just below 5,000 inhabitants became 26% (22%) larger than those of municipalities just above the cutoff in 2002. The results for the revenue forecast deviation are due to increases in the forecast errors in taxes and, especially, in fees and tariffs. Larger errors in revenue projections might be due to an excessive exuberance in budget forecasting and/or in smaller ability to collect taxes and fees resulting in a lower amount of realized revenues, as shown in Grembi et al. (2016). The results for the expenditure forecast errors are instead driven by changes in the forecast errors of the capital outlays. Considering different dimensions of municipality heterogeneity, we find that the effects for the revenue forecast errors are driven by municipalities in the North-West, with larger territories, and with a higher fraction of youth. The results of the expenditure forecast errors are ascribed to municipalities in the North-West, but also to those with a high share of immigrants, youth, and inhabitants with tertiary education.

The set-up of our paper is as follows. Section 2 summarises the literature on theoretical and empirical studies on the effects of fiscal rules on fiscal outcomes, including budgetary projections. Section 3 focuses on the DSP in Italy. Section 4 describes the econometric model, the identification assumptions, and the sample used in the econometric analysis. Section 5 reports the empirical findings. Section 6 concludes.

2 Literature review

Several reasons justify the use of fiscal rules among local governments. They can be used to restrain spending appetites of local authorities financed by a "common pool" of national resources (Rodden, 2002). Indeed, intergovernmental transfers alter local politicians' and residents' perception of the amount of sustainable expenditure, since they perceive that

the costs of local public services can be shifted to non-residents. The transfer of the costs to non-residents turns out into larger local public expenditure, which could be restricted by expense ceilings. Fiscal estrictions can be imposed on local borrowing autonomy to avoid the excessive use of bank loans or other forms of lending, when intergovernmental transfers do not match the financial capacities of local jurisdictions to provide centralized standards of local public goods and services. Sub-central budget rules can be used by the central government to avoid the provision of special *ad-hoc* transfers to insolvent local jurisdictions and prevent a possible fiscal crisis due to their fiscal profligacy (Prud'homme, 1995; Tanzi, 1996; Ter-Minassian, 2007).

There is no wide consensus in the literature on the desirability and the effectiveness of sub-central fiscal rules to restrain fiscal profligacy of local governments. Ter-Minassian (2007) claims that sub-central fiscal rules can be used only when market discipline and cooperative arrangements across levels of governments fail to enhance fiscal responsibility at the local level. The disciplinary role of the market is effective only if the commitment by the central government to bail out the sub-national insolvent governments is not credible. Moreover, privileged access to credit to local governments weakens the market discipline as well as the lack of information of market participants about the financial soundness of local governments. Milesi-Ferretti (2004) shows that fiscal rules can create good or bad outcomes, including "ugly" outcomes such as "creative accounting" to meet the budget rules. He emphasizes the role played by transparent budgetary procedures for limiting accounting creativity and for adopting less stringent fiscal rules. Other scholars share this view by considering budget transparency a powerful means for guaranteeing fiscal discipline among local administrations (Alesina and Perotti, 1996; Alesina et al., 1999). Moreover, greater sub-national fiscal autonomy has been suggested as a strong disciplinary device to contain fiscal profligacy of local governments (Argimón and Hernández de Cos, 2012).

Many studies in the last decade have documented the effectiveness of fiscal rules in curbing sub-national fiscal outcomes (Krogstrup and Wälti, 2008; Tapp, 2013; Grembi et al., 2016; Iskandar, 2016; Burret and Feld, 2018; Heinemann et al., 2018; Asatryan et al., 2018), especially in countries with unitary political system (Foremny, 2014) and a high degree of fiscal vertical imbalance (Rodden, 2002). A growing interest has been also devoted to the study of the effects of fiscal rules on budgetary projections from both a theoretical and an empirical perspective. Fiscal rules might be a tool to prevent excessive

exuberance in the estimation of budget balance and tax revenue that is responsible for excessive deficit (Von Hagen and Harden, 1994; Alesina and Perotti, 1996; Frankel, 2011; Chatagny and Soguel, 2012; Frankel and Schreger, 2013). In particular, Baldi (2016) shows the ability of fiscal rules to reduce the deficit. His model predicts that ex-post rules on the realized deficit are more effective at restraining fiscal deficit than ex-ante rules placed on the forecasted deficit. The effects of ex-post rules on both the forecasted and the actual fiscal deficit are also reinforced if they are accompanied by a high degree of political stability and greater government size. His model also suggests that pressures on the financial market can act as a discipline device for governments, making both ex-ante and ex-post rules less effective on forecasted and actual deficits. Fiscal rules are often evoked by ministers of finance to deny excessive spending requests from ministers or legislators, rather than underestimate budgetary projections (Luechinger and Schaltegger, 2013). They can also encourage over-optimistic fiscal projections to postpone unpopular decisions, such as tax increases and/or spending cuts (Alesina and Perotti, 1996), and/or "creative accounting" through overly pessimistic or overly optimistic fiscal projections.

Empirical studies have found controversial results about the impact of fiscal constraints on budgetary forecasts accuracy. On the one hand, several studies showed that fiscal rules, such as the SGP, created over-optimism in official budget forecasts, especially in the run-up to election (Brück and Stephan, 2006; Frankel, 2011; Pina and Venes, 2011). Frankel (2011) showed that the SGP creates over-optimistic bias in budget balance forecasts of a sample of 33 countries. Frankel and Schreger (2013) found over-optimistic forecasts when countries are most in danger of breaking the limit of 3% imposed by the SGP. The forecast bias is reduced when countries adopt own national budget balance rules or have independent fiscal institutions that provide their own independent forecasts. Von Hagen (2010) used data from the annual Stability and Growth Programs and Convergence for the EU-15 countries from 1998 to 2004. He found that governments operating under a *contract* approach and strong fiscal rules submit too large revenue projections on average. Heinemann (2006) found that the surveillance procedures of the Maastricht Treaty and SGP have made medium-term budgetary planning less realistic and fiscal projections overly optimistic in Germany. On the other hand, Annett (2006) showed that EU countries make smaller forecast errors in fiscal balance under the SGP. Using data from the excessive deficit procedure notifications and national drafts for the EU-15 countries, Pina and Venes (2011) found that budget balance forecast errors are more prudent when national

numerical rules on public expenditure come into force, especially in the post-SGP period. Luechinger and Schaltegger (2013) found that in Swiss cantons fiscal rules reduced on average the probability of projected and realized deficits by about 28% and 15%, respectively. Finally, Chatagny (2015) found that an increase in the degree of stringency of fiscal rules in Swiss cantons attenuated the positive effects of finance ministers' political ideology on tax revenue projections errors.

3 Domestic Stability Pact for Italian municipalities

In 1999 Italy introduced a sub-central fiscal rule (Article 28, Law No. 448/1998) to fulfill its long-term commitment to fiscal sustainability accepted at European level with the Stability and Growth Pact (SGP). The rule, called *Patto di Stabilità Interno* (Domestic Stability Pact, DSP), was imposed on all municipalities and upper-tier levels of local government (regions and provinces) to progressively reduce the expenditures financed with the deficit and the share of debt on the gross domestic product. The DSP was initially conceived as a set of prescriptions shared by the central government and the local administrators to respect the fiscal criteria of the SGP. Its primary goal was to make local administrators more fiscally disciplined and co-responsible with the central government in complying with the European fiscal obligations (Giarda and Goretti, 2001). Substantial amendments were made annually to the DSP by the Italian Parliament through the national budget law (*legge finanziaria*), making harder for municipalities to plan in advance their activities (Balduzzi and Grembi, 2011). The amendments mainly concerned the definition of the programmatic objectives (based on the deficit and/or expense growth targets), the balance sheet items, and the basis of accounting (expressed in cash and/or accruals) on which these objectives were defined. Furthermore, both the number of municipalities involved and the penalty system have also been modified over the years (Patrizii et al., 2006).

In the first year of its introduction, the DSP established a reduction of the deficit of municipalities by 1% of the gross domestic product (GDP). This goal was achieved through the implementation of various actions, such as increasing productivity in the provision of public services, reducing the growth rate of current expenditure, and/or strengthening tax collection for increasing the local tax base. The not-complying municipalities were exposed to sanctions only if Italy was fined by the European Union for the excessive

deficit. While the goal of reducing local deficit by 1% of the GDP was reconfirmed in 2000 (Art. 30, Law No. 488/1999), some changes were introduced on the side of deficit calculation, with additional categories of revenues (e.g. transfers from the EU and occasional revenues) and expenditures (e.g. mandatory and occasional expenses) excluded from it. Moreover, the penalty system was replaced by a reward system consisting of a lower interest rate on borrowings for complying municipalities.

The subsequent pact changes in 2001 favoured the municipalities that in 1999 did not comply, in whole or in part, with the local budget rule (Bertocchi, 2009). Indeed, for that year, the DSP required municipalities to maintain a deficit no greater than the 1999 deficit (net of expenses for passive interests and health care) increased by 3%. The virtuous municipalities continued to benefit from a lower interest rate on borrowings. A further relevant change introduced in 2001 was the exemption of municipalities below 5,000 inhabitants from the fiscal restraints (Art. 53, law No. 388/2000).[3] Their exclusion was decided for preventing them from being subject to onerous budget requests, as they are disadvantaged by economies of scale in the provision of local public services (Pignatti, 2009; Grembi et al., 2016). Other motivations concerned the difficulty of monitoring their activities because they represent more than 70% of municipal administrations (Pazienza and Rapallini, 2008). Finally, they were also excluded because they have little impact on the containment of Italian public spending for fiscal consolidation purpose (Pignatti, 2009).

A strong crackdown on fiscal constraints was made in 2002 since the annual growth rate limits were imposed to both current expenditure and deficit at 6% and 2.5%, respectively (Art. 24, law No. 448/2001). Severe penalties to the violation of the DSP were also included by blocking the municipal permanent staff recruitment (Art. 19, law No. 448/2001) and cutting current transfers (Art. 9, law No. 448/2001) to the municipalities that did not comply with the pact. Although severe, some sanctions, like the reduction of central government transfers, encountered legal problems as they were considered unconstitutional, therefore, difficult to be implemented (Bartolini and Santolini, 2012). Most likely, this was one of the reasons why in 2003 the penalty system was modified by imposing on noncompliers a reduction of at least 10% of their expenses on local public

[3]The criterion for defining the population threshold has been established by Art. 156 (*comma* 2) of the Legislative Decree No. 267/2000 known as TUEL (*Testo Unico degli Enti Locali*). Accordingly, the population is calculated at the end of the penultimate previous year according to data provided by the National Institute of Statistic (Istat), i.e. for the year 2003, inhabitants of 31 December 2001.

goods and services and the prohibition to hire public employees and to get into debt to finance public investments.

In 2003 fiscal constraints on municipal deficit remained in force, whereas the ceiling on current expenditure growth was eliminated. A novelty was introduced on the side of the compilation of the projected balance sheet: municipalities subject to the DSP had to draw up budget projections on the programmatic objectives in line with the annual fiscal target (Art. 29, *comma* 17, law No. 289/2002). This obligation was also confirmed in some of the subsequent years,[4] leading municipalities constrained by the DSP to draw up more precise budget projections, with the consequence of fewer budget forecast errors respect to municipalities that were not subject to fiscal constraints.

No substantial changes were made to the DSP in 2004. Although initially spending ceilings were imposed to municipalities above 3,000 inhabitants (Art. 21, law No. 311/2004), this population threshold has never been applied. Indeed, shortly after law No. 88/2005 (Art. 1-*ter*) re-established the original threshold of 5,000 inhabitants.[5]

Two relevant new features were introduced in 2005. A cap on the total public expenditure growth was set at 2%. Furthermore, it was introduced the distinction between virtuous and non-virtuous municipalities.[6] Virtuous municipalities can benefit from a greater increase in the growth rate of expenditure than non-virtuous municipalities. This distinction was confirmed in 2006, whereas the cap on the total expenditure was replaced with ceilings on the growth of current and investment spending at -6.5% and +8.1%, respectively. Since the spending ceilings generated the paradox that municipalities refused state transfers, because in using them the risk of violating the DSP spending ceilings would have been higher, since 2007 onwards, the government reintroduced restrictions on the side of the municipal deficit growth. This choice also aimed at making the DSP more adherent to the European Union financial requirements and at offering greater autonomy to local governments on what measures to adopt, between reducing spending and/or increasing revenues, to contain the deficit growth (Valerio, 2009).

The DSP adoption has made Italian municipalities more fiscally accountable by in-

[4]See Art. 31, law No. 311/2004; Art. 1, *comma* 684, law No. 296/2006; Art. 1, *comma* 379, *letter g)*, law No. 244/2007.

[5]The original threshold of 5,000 inhabitants was continuously adopted from 2001 until 2012 (Art. 31, law No. 183/2011).

[6]The municipality is virtuous if the average per-capita current expenditure, calculated over the period 2001-2003, is lower than that of its demographic class.

creasing their own revenues and reducing their own debt (Grembi et al., 2016; Monacelli et al., 2016). However, their investments were strongly penalized, especially among compliers (Chiades and Mengotto, 2015; Monacelli et al., 2016). The ability of the municipalities subject to the DSP to achieve medium-long term objectives was undermined by the excessive stringency of fiscal constraints and the frequent changes in the rule definition, which created greater uncertainty in the management of their activities.

4 Method

4.1 Data and sample

Our main data source is the database on local public finance realized by the Italian Department of Territorial and Internal Affairs.[7] This dataset contains detailed information on public finance (revenues and expenditures) and public individual-demand services for all the Italian municipalities, among which end-of-year realizations of revenues and expenditures and their forecasts at the start of each year. A secondary data source, still at the municipality level, is the 1991 census gathered by the *Atlante Statistico dei Comuni* of the Italian National Institute of Statistics (Istat).[8] We extracted a set of demographic and economic variables, like the employment rate, the fraction of young/elderly people, women, immigrants, and high/old educated people.[9]

The empirical analysis focuses on a sample of Italian municipalities over the period 1999-2004. We start from 1999 because data on budgetary forecasts are not available before this year at the municipal level. We do not use data after 2004 because many features of the pact changed in 2005 and later years, making it difficult to isolate the mechanisms behind the change in budgetary forecast behaviour of local administrators. Hence, by considering only the initial years after the introduction of the pact, we have a period of almost homogeneous norms.

Municipalities are the lowest level of local government in Italy. Our sample includes only municipalities belonging to the 15 regions with "ordinary regime". The remaining five regions[10] are indeed subject to a "special regime", defining a different relationship

[7] See Finanza Locale website on https://finanzalocale.interno.gov.it/banchedati.html.
[8] See https://www.istat.it/it/archivio/113712.
[9] We will exploit this information in a validation test of the identifying assumptions of the causal effect.
[10] Friuli-Venezia Giulia, Sardegna, Sicilia, Trentino Alto-Adige, Valle d'Aosta.

with the regional government and implying that: i) they have more legislative and fiscal power than the other regions, thus affecting fiscal policy decisions of their municipalities; ii) in 2002 they stated their own municipal budget rules, not allowing a comparison with the municipalities in the rest of the country (Grembi et al., 2016). The sample size shrank from about 8,000 municipalities per year to almost 6,700. Since the identification strategy will be based on local random assignment of the treatment at the cutoff of 5,000 inhabitants, we limited our sample to municipalities which were close to this cutoff and kept only those between 3,500 and 7,000 inhabitants. This left us with about 1,180 municipalities per each year. Finally, in order to get rid of potential outliers, we eliminated municipalities reporting a value of the revenue or expenditure forecast equal to 0 and cut the first and last percentiles of the distribution of the expenditure or revenue forecast errors. The final sample was a panel across 6 years for a total of 6,767 (6,765) observations when the dependent variable was the revenue (expenditure) forecast error.

Table 1 shows the absolute frequencies of the municipalities by year across our sample selection criteria. The figures reported in column (iv) refer to the municipalities used to study the impact of relaxing fiscal restraints on revenue forecast errors. The ones in column (v) are instead those used for the analysis of expenditure forecast errors.

Table 1: Sample selection criteria and the absolute frequencies of municipalities

Year	(i) Original dataset	(ii) After removing municipalities in regions with special autonomy	(iii) After keeping municipalities with 3,500-7,000 inhabitants	(iv) After removing municipalities in the 1st or last percentile of revenue error distr.	(v) After removing municipalities in the 1st or last percentile of the expenditure error distr.
1999	8,084	6,692	1,192	1,153	1,159
2000	8,084	6,695	1,185	1,152	1,158
2001	8,084	6,694	1,184	1,118	1,112
2002	8,084	6,688	1,186	1,063	1,063
2003	8,084	6,695	1,182	1,129	1,127
2004	8,084	6,691	1,179	1,150	1,146
Total	48,504	40,155	7,108	6,767	6,765

The outcome variables of interest are the revenue forecast error (rfe_{it}) and the expenditure forecast error (efe_{it}) of municipality i at time t. We define them as the ratio between the realized total revenues and total expenditures in municipality i at the end of year t and their forecasts at the beginning of period t minus 1:

$$rfe_{it} = \frac{\text{actual total revenues}_{it}}{\text{forecast total revenues}_{it}} - 1; \quad (1)$$

$$efe_{it} = \frac{\text{actual total expenditures}_{it}}{\text{forecast total expenditures}_{it}} - 1. \quad (2)$$

If multiplied by 100, they are, in other words, the percentage deviation of the realized revenues and expenditures at the end of year t from their forecasts at the beginning of year t. Figure 1 draws the density distribution of the revenue and expenditure forecast errors. After the removal of the lowest and highest percentiles, they are both always positive and smaller than 1. This means that in our sample: i) all the municipalities under-estimated both the actual total revenues and the actual total expenditures; ii) the realized revenues and expenditures never doubled their forecasts.

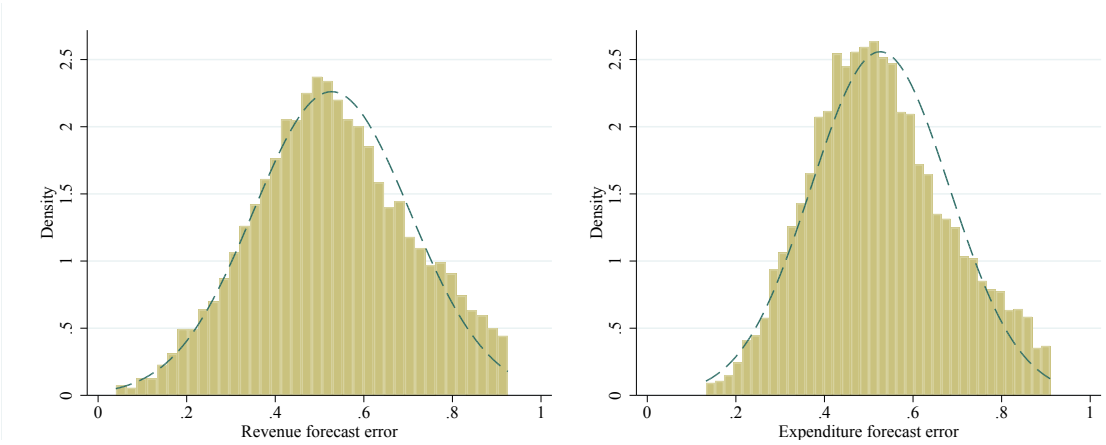
Table 2 reports summary statistics of aggregate forecast errors, as well as the forecast errors by time, municipality size, and by subcategories of revenues (taxes, fees and tariffs, and a residual category) and expenditures (current outlays, capital outlays, and other expenditures).[11] By looking at the before-after averages, it emerges an important change in the budget forecast errors. After 2001 the revenue forecast error was 50.7%, against 56.6% before 2001. The reduction in the revenue forecast error was especially driven by the reduction in the forecast deviation of taxes. At the level of expenditures, no variation over time in forecast error is observed. By splitting these statistics above and below the 5,000 inhabitants cutoff, no particular difference is observed in the budget forecast errors.

We will use an estimator based on the sharp regression discontinuity design. As assignment variable, we use the number of resident inhabitants at December 31 of 2 years before as reported by Istat. This is the official source used by the central government to

[11] Among "taxes", we included revenues from taxes on property rights, income, waste disposal, advertising, and for the occupation of public areas (*Titolo I - Entrate tributarie*). In "fees and tariffs", we included the revenues due to the payment for services, like childcare services and swimming pool, but also, for example, those related to the management of the territory and city planning (*Titolo III - Entrate extra-tributarie*). "Current outlays" (*Titolo I - Spesa corrente*) include current expenditure on personal, purchase of consumer goods and/or raw materials, services, expenses for current transfers, expenses for passive interest and other financial charges. "Capital outlays" (*Titolo II - Spesa in conto capitale*) include expenses for the purchase of real estate and movable assets, the purchase of machinery and technical-scientific equipment, the assignment of external professional collaborations, capital transfers among the other capital expenditure items.

distinguish the municipalities subject to the DSP from those which were not.[12] Our assignment variable differs from the one in Gagliarducci and Nannicini (2013) and Grembi et al. (2016), who instead used the population in the last available census (1991 or 2001). Using the last available census, instead of the official measure used by the central and local administrations, generates a risk of incurring in biases related to measurement error in the running variable (Davezies and Le Barbanchon, 2017), which we avoid. We denote the assignment variable as $x_{it} \equiv pop_{it-2} - 5,000$, where pop_{it-2} is the population of municipality i on the last day of year $t-2$, so that the cutoff is normalized to 0. Hence, starting from 2001, the municipalities were split into treated units if $x_{it} < 0$ and untreated units if $x_{it} \geq 0$.

Figure 1: Distribution of revenue and expenditure forecast errors, 1999-2004



Notes: The dashed lines are normal densities.

Figure 2 graphically illustrates the change over time of the discontinuity in the budget forecast error before and after 2001. In 1999-2000, all the municipalities were subject to the DSP. In 2001, the fiscal rules imposed by the DSP were removed for the municipalities below 5,000 inhabitants and, for larger municipalities were introduced incentives to comply with the DSP.[13] Finally, in 2002, the ceiling on current expenditure growth and severe penalties for municipalities larger than 5,000 not complying with the DSP were introduced. From graphs a) and d) of Figure 2, it clearly emerges that before 2001 there was

[12] See art. 156 of Legislative Decree No. 267/2000.

[13] Local governments satisfying the DSP were rewarded with a 0.5-1 percentage points cut in the interest rate on debts started before 1998 (Bertocchi, 2009).

Table 2: Summary statistics of the revenue and expenditure forecasting errors

	Total		Before 2001		After 2001		Below 5,000		Above 5,000	
	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
a) Forecasting error in										
Revenues	0.527	0.176	0.566	0.166	0.507	0.178	0.532	0.177	0.521	0.175
Expenditures	0.526	0.156	0.523	0.156	0.527	0.156	0.524	0.159	0.528	0.153
b) Forecasting error by types of revenues										
Taxes	0.358	0.189	0.430	0.175	0.321	0.186	0.370	0.189	0.344	0.189
Fees and tariffs	0.391	0.211	0.413	0.208	0.379	0.212	0.403	0.210	0.376	0.212
Other revenues	0.593	0.204	0.627	0.194	0.576	0.207	0.589	0.208	0.598	0.199
c) Forecasting error by types of expenditures										
Current outlays	0.231	0.075	0.233	0.072	0.229	0.076	0.224	0.074	0.239	0.075
Capital outlays	0.885	0.140	0.911	0.118	0.871	0.149	0.884	0.145	0.887	0.134
Other expenditures	0.431	0.201	0.423	0.202	0.435	0.201	0.424	0.199	0.439	0.204

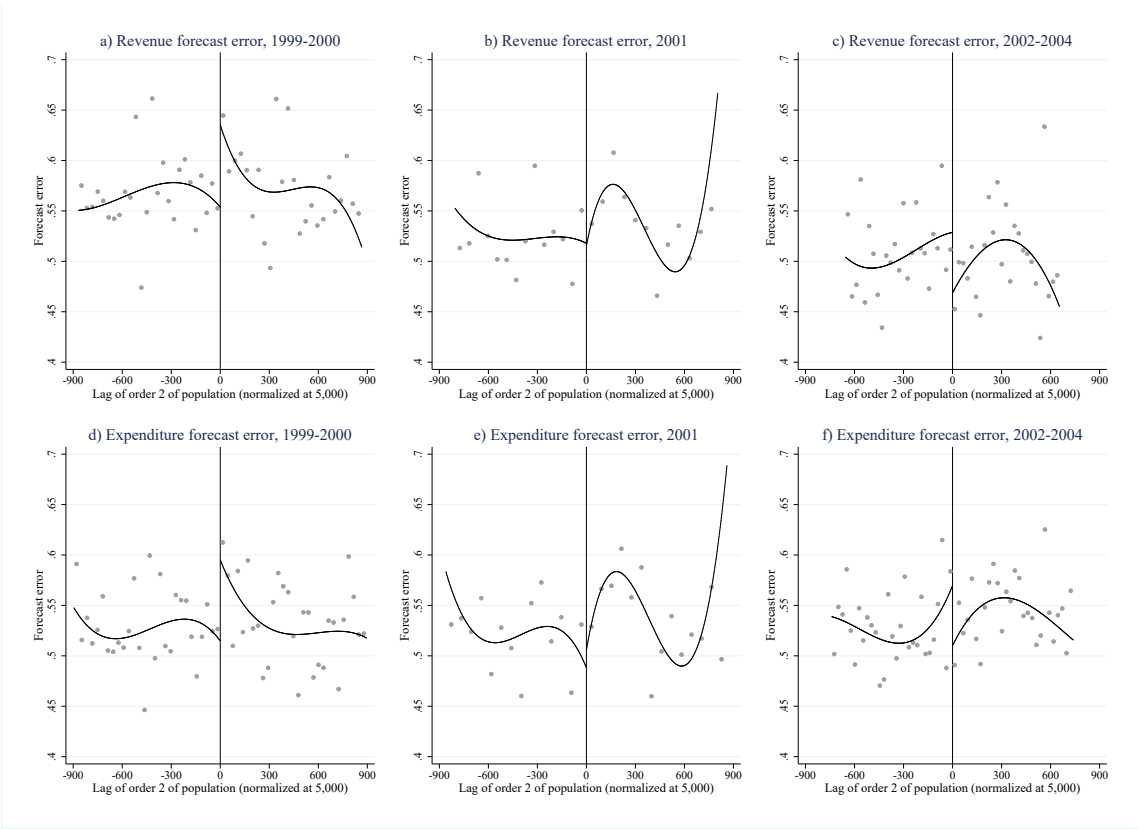
a large discontinuity in both revenue and expenditure forecast error, with municipalities above the cutoff underestimating more severely both revenues and expenditures.[14] Although all the municipalities were subject to the same fiscal rules before 2001, a further discontinuity has been present at the same cutoff and could explain why the revenues and expenditures are underestimated in larger municipalities: the wages of the mayor and of the executive mayors appointed by the mayor are higher in municipalities above 5,000 inhabitants. Gagliarducci and Nannicini (2013) showed that mayors of municipalities right above the cutoff are more-educated and higher-skilled than those of municipalities right below the cutoff and it impacts on the budget.

Graphs b) and e) of Figure 2 show that in 2001, with respect to the previous 2 years, two features are worth mentioning. First, although small municipalities were exempted in 2001 from complying with the DSP no change over time in the budget forecast is detectable, suggesting that the budgetary forecast behaviour of local administrators is not influenced by fiscal rules. Finally, graphs c) and f) of Figure 2 illustrate that with the introduction in 2002 of the ceiling on current expenditure growth and more severe penalties for non-complying municipalities and the inclusion in 2003 of the explicit requirement of drawing up the budget projections in line with the annual fiscal target, the budget forecast errors went down compared to both the 1999-2000 level and the one of the municipalities below the cutoff. This suggests that more stringent budgetary restrictions, accompanied

[14] Before 2001, the discontinuity in the revenue and expenditure forecast error amounted to 8.2 and 8 points, respectively (p-values equal to 0.051 and 0.045, respectively).

by the "stick" (severe sanctions) and the explicit requirement on budgetary projections, could have been effective in changing the budgetary behaviour of local administrators.

Figure 2: Graphical illustration of the discontinuity at the cutoff on revenue and expenditure forecast errors after and before the DSP reform



Notes: The solid lines are obtained by regression functions based a 3rd-order polynomial regression of the outcome variable on the running variable (x_{it}, the lag of order 2 of the population), fitted separately above and below the cutoff. The dots represent local sample means of disjoint bins of the running variable reported in the midpoint of the bin. The number of bins and their lengths are chosen optimally using the mimicking variance integrated mean-squared error criterion.

4.2 Difference-in-discontinuities design

Let $r_{it} \equiv 1(x_{it} < 0)1(t \geq 2001)$ denote the treatment indicator, where $1(\cdot)$ is the indicator function, equal to 1 if its argument is true. When $r_{it} = 1$, municipality i in year t is below the cutoff and, since $t \geq 2001$, its budget is no longer subject to fiscal restraints. Let y_{it} be the outcome variable which, in our application, is either rfe_{it} or efe_{it}. Finally,

following the notation in Hahn et al. (2001), let y_{1it} be the outcome with treatment and y_{0it} the outcome without treatment. If no other treatment is assignment at the cutoff $x_{it} = 0$, we could identify the local effect of the fiscal restraints on revenue (expenditure) forecast error in a canonical sharp regression discontinuity design (RDD) using 2001 and later data. We would have to make the usual assumptions to identify the local average treatment effect (Hahn et al., 2001; Lee and Lemieux, 2010): i) units should not be able to precisely manipulate the value of the assignment variable; ii) $\mathrm{E}\left[y_{it}|x_{it} = x\right]$ must be a continuous function in x at 0 in the absence of the treatment. The conventional sharp RD estimand would be, for $t \geq 2001$,

$$\delta_t \equiv \lim_{x \to 0^-} \mathrm{E}\left[y_{it}|x_{it} = x\right] - \lim_{x \to 0^+} \mathrm{E}\left[y_{it}|x_{it} = x\right] = y_t^- - y_t^+. \tag{3}$$

However, at the same cutoff, also another treatment is assigned to Italian municipalities: mayors and the members of the executive committee are entitled to larger wages if the municipality has more than 5,000 inhabitants. Let $w_{it} \equiv 1(x_{it} < 0)$ denote the treatment indicator for municipality i in year t. When it is equal to 1, the wage of the executive officers is lower. As shown by Gagliarducci and Nannicini (2013), the sharp increase in the wage of mayors at the cutoff attracted higher educated candidates and improved the efficiency of the government machinery. Hence, if we stuck to the discontinuity at the cutoff after 2001 as the only source of identification, we could not disentangle the effect induced by the fiscal restraints from the one related to a different composition of local government officials. However, the wage of the municipal executive officers was determined by the population being below or above the same cutoff both before and after 2001. Hence, we can take advantage of the fact that only one of the two treatment assignments was introduced in 2001 and mix the RDD with a difference-in-differences approach to disentangle the true effect of the removal of the fiscal restraints for smaller municipalities from the one due to lower wages for the municipal executive officers.

This identification strategy was used by Grembi et al. (2016) to analyze the impact of the removal of the fiscal restraints on revenues and expenditures of Italian municipalities. They named this approach difference-in-discontinuities (diff-in-disc).[15] They also detailed the assumptions for identifying the pure effect of relaxing the fiscal restraints

[15] See also Giambona and Ribas (2018), Casas-Arce and Saiz (2015), and Leonardi and Pica (2013) for empirical studies which used the diff-in-disc estimator.

and proposed diagnostic tools to check whether they are supported by the data. In what follows, we closely follow their approach. In the diff-in-disc setup, the estimand is

$$\begin{aligned}
\delta_{DD} \equiv & \lim_{x \to 0^-} \mathrm{E}\left[y_{it} | x_{it} = x, t \geq 2001\right] - \lim_{x \to 0^+} \mathrm{E}\left[y_{it} | x_{it} = x, t \geq 2001\right] \\
& - \left(\lim_{x \to 0^-} \mathrm{E}\left[y_{it} | x_{it} = x, t < 2001\right] - \lim_{x \to 0^+} \mathrm{E}\left[y_{it} | x_{it} = x, t < 2001\right] \right) \qquad (4) \\
= & \; y_t^- - y_t^+ - (\tilde{y}_t^- - \tilde{y}_t^+). \qquad (5)
\end{aligned}$$

As proved by Grembi et al. (2016), δ_{DD} identifies the pure local causal effect of relaxing the fiscal restraints for small municipalities and of the penalties for noncompliers becoming more severe under the following three assumptions.

Assumption 1 (Continuity of the outcome functions): All the outcome functions $\mathrm{E}\left[y_{rit} | x_{it} = x, t \geq 2001\right]$ and $\mathrm{E}\left[y_{rit} | x_{it} = x, t < 2001\right]$, with $r = 0, 1$, are continuous in x at the cutoff.

Assumption 2 (Local parallel trend): The effect at the cutoff of low wages for the municipal executive officers is constant before and after the removal of the fiscal restraints, in the absence of the change in the fiscal restraints.

Assumption 3 (Independence of the treatment effect on the confounding policy): The effect of relaxing fiscal restraints at the cutoff does not depend on the wage of the municipal executive officers.

Assumption 1 is a richer version of the continuity assumption needed in the usual RDD. It states that the continuity at the cutoff must be satisfied both before and after the relaxing of the fiscal restraints in 2001. Assumption 2 is fundamental to remove the confounding component due to lower wages for the municipal executive officers from the discontinuity after the relaxing of the fiscal restraints. The period before the relaxing of the fiscal restraints is used to identify the effect of lower wages only. Under the assumption that this confounding effect is constant over time, we can subtract it from the composite effect after 2001, which is made up of both the one related to lower wages and the one due to relaxing the fiscal restraints. Finally, under Assumption 3, it is possible to identify the local causal effect of relaxing fiscal restraints in the neighbourhood of the cutoff. Grembi et al. (2016) showed that Assumption 3 is not necessary to prove

that δ_{DD} identifies the local average treatment effect of relaxing fiscal restraints for municipality below the cutoff. However, without Assumption 3, δ_{DD} cannot be extended to municipalities without the confounding treatment at the cutoff.

In Section 5.3 we report tests to check whether the data support Assumption 1. Grembi et al. (2016) used 1997 and 1998 data to check whether municipalities around the cutoff reacted differently to the introduction in 1999 of fiscal restraints, as supportive evidence of Assumption 3. If Assumption 3 held, one would indeed expect that when in 1999 the central government introduced the fiscal restraints for all the municipalities, the municipalities around the cutoff reacted in similar ways, independently on the low wages of the municipal executive officers. A diff-in-disc estimate for the introduction of the fiscal restraints in 1999 using 1997-2000 data should, therefore, returns a nil effect if Assumption 3 holds. We cannot run this test because our dependent variable cannot be computed before 1999: before this year information on budget forecasts is indeed not available. Given that Grembi et al. (2016) did not find any evidence against Assumption 3 in terms of revenues and expenditures, it is plausible to think that it holds as well when referred to revenue and expenditure forecast errors.

4.3 Estimation

We estimate δ_{DD} using local polynomial methods. Following the advice in Gelman and Imbens (2019), we stick to low-order polynomials. The baseline model is a local quadratic regression:

$$\begin{aligned} y_{it} = {} & \alpha_0 + \alpha_1 x_{it} + \alpha_2 x_{it}^2 + 1(x_{it} \geq 0) \cdot (\gamma_0 + \gamma_1 x_{it} + \gamma_2 x_{it}^2) \\ & + 1(t \geq 2001) \cdot \left[\beta_0 + \beta_1 x_{it} + \beta_2 x_{it}^2 + 1(x_{it} \geq 0) \cdot (\delta_0 + \delta_1 x_{it} + \delta_2 x_{it}^2)\right] \\ & + \quad u_{it}, \qquad \text{with} \quad t = 1999, \ldots, 2004, \quad \text{and} \quad x_{it} \in [-h, +h], \end{aligned} \tag{6}$$

where u_{it} is the error term and h is the bandwidth restricting observations near the cutoff. We choose the bandwidth following the mean-squared error optimal criterion in Calonico et al. (2014).[16] We fit the model in Equation (6) using weighted least squares, using the triangular kernel function to weight observations. As pointed out by Cattaneo

[16] In Section 5.3, we report sensitivity analyses to check how and to what extent the results are affected by changing the bandwidth and by using local linear regression.

et al. (2018), the point estimator has indeed optimal properties in a mean squared error term, when a mean squared error optimal bandwidth and a triangular kernel function are used. By weighting observations, we give more importance to observations that are closer to the cutoff. More in detail, the triangular kernel function is maximized (and equal to 1) at the cutoff, it is zero for municipalities with $x_{it} \notin [-h, +h]$, and it decreases linearly and symmetrically when the assignment variable moves away from the cutoff. In making inference, we cluster standard errors at the municipal level.

5 Estimation results

5.1 Baseline effects

Table 3 reports the estimation result of the baseline model. For the diff-in-disc approach, in column (1) we used all the years after 2001 (from 2001 until 2004). The estimated impact of the changes in fiscal rules amounted to 13.4 percentage points (pp) for the revenue forecast error, 9.5 pp for the expenditure forecast error. Compared to the pre-reform average of the revenue (expenditure) forecast error, which was 57.8 (53.3) pp, after the removal of the fiscal restraints small municipalities experienced on average larger revenue (expenditure) forecast errors by about 23% (18%).

Columns (2)-(5) report the estimation results when, in the after period, each year is separately and alternatively included. The main finding is that the effect was not homogeneous over time. The results in column (2) show that the removal of the fiscal restraints for small municipalities in 2001 and the introduction of incentives for compliers did not affect the budget forecast errors, neither of revenues nor of expenditures. In 2002, when both spending ceiling and more severe penalties for noncompliers were inserted in the DSP, the difference in forecast errors between treated and untreated municipalities became sizeable and significant. Column (3) shows that the revenue (expenditure) forecast deviation is 15.4 (10.7) pp higher for small municipalities. Compared to the 1999-2000 average, the increase in revenue (expenditure) forecast error amounted to 27% (20%). We find very similar effects if we focus on 2003 and 2004 separately, although the point estimates for the impact on the revenue forecast error are somewhat smaller (see columns (4) and (5)).

Finally, column (6) reports the estimated effects by only excluding observations in

Table 3: Difference-in-discontinuities effect on revenue and expenditure forecast errors

	(1) 1999-2000 vs 2001-2004	(2) 1999-2000 vs 2001	(3) 1999-2000 vs 2002	(4) 1999-2000 vs 2003	(5) 1999-2000 vs 2004	(6) 1999-2000 vs 2002-2004
a) Difference-in-discontinuities effect on revenue forecast error						
	0.134***	0.036	0.154**	0.136*	0.114	0.149**
	(0.049)	(0.055)	(0.071)	(0.074)	(0.073)	(0.062)
Sample mean before[(a)]	0.578	0.574	0.573	0.578	0.577	0.578
Observations	2,103	1,307	1,232	1,058	1,134	1,758
Municipalities	430	480	472	413	454	431
R-squared	0.035	0.023	0.054	0.045	0.034	0.045
Local polynomial order	2	2	2	2	2	2
Bandwidth	534.46	657.10	635.26	529.21	572.93	537.51
b) Difference-in-discontinuities effect on expenditure forecast error						
	0.095**	0.004	0.107*	0.130*	0.097	0.118**
	(0.047)	(0.049)	(0.061)	(0.069)	(0.066)	(0.058)
Sample mean before[(a)]	0.533	0.528	0.529	0.534	0.532	0.534
Observations	2,197	1,621	1,288	1,067	1,174	1,792
Municipalities	453	584	494	414	464	441
R-squared	0.009	0.009	0.009	0.012	0.008	0.010
Local polynomial order	2	2	2	2	2	2
Bandwidth	559.02	791.87	662.53	535.40	596.13	547.39

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are reported in parenthesis and are robust to heteroskedasticity and within-municipality correlation. The optimal bandwidth is chosen by minimizing the mean squared error (Calonico et al., 2014) after imposing local quadratic regression. We used the triangular kernel to weight observations from the cutoff.

[(a)] Mean computed across the 1999 and 2000 observations within the bandwidth.

2001 from the after period. It confirms that once spending ceiling, severe penalties for noncompliers and the explicit requirement of drawing up the budget projections in line with the annual fiscal target were introduced in the DSP, the effect on the forecast errors is large and significantly different from zero at the usual at 5% level. Relatively to the 1999-2000 average, the increase in the revenue (expenditure) forecast error is of about 26% (22%).

In a nutshell, the main findings from the baseline estimates reported in Table 3 are:

1. the municipalities not subject to fiscal rules have larger revenue and expenditure forecast errors;

2. the results limited to 1999-2001 data suggest that the removal of the fiscal restrains for the municipality with less than 5,000 inhabitants and the introduction of incentives for compliers are not be the drivers of the findings;

3. it is rather the introduction in 2002 of stricter budgetary restrictions together with more severe penalties for noncompliers and in 2003 of the explicit requirement of drawing up the budget projections in line with the annual fiscal target which caused the reduction of revenue and expenditure forecast errors in (locally) large municipalities.

Next, we split the revenues and expenditures into three main components and computed for each one the corresponding forecast error. We distinguished the revenues in taxes, fees and tariffs, and a residual category. We divide the expenditures in current outlays, capital outlays, and a residual category. Table 4 reports the estimated impact on forecast error for each of these components of revenues and expenditures.

Panel a) of Table 4 shows that the baseline findings for the revenue forecast deviation are driven by the increase in the forecast errors in taxes and, especially, fees and tariffs. This result suggests that the low or absent inter-jurisdictional mobility of tax base leads to greater certainty about the amount of tax revenue collected by municipalities, more easily allowing more accurate tax revenue forecasts.

Panel b) of Table 4 points out that the impact on the expenditure forecast error is only due to the change in the forecast error of the capital outlays. Less precise forecasting errors in capital outlays might reflect greater uncertainty in the timing and costs of carrying out medium-long term public investments.

Table 4: Difference-in-discontinuities effect on forecast errors by types of revenues and expenditures

	a) Revenues: diff-in-disc effect on forecast error of:		
	Taxes	Fees and tariffs	Other revenues
	(1)	(2)	(3)
	0.110*	0.154**	0.073
	(0.056)	(0.069)	(0.064)
Sample mean before[(a)]	0.429	0.402	0.638
Observations	2,461	1,955	1,957
Municipalities	499	406	413
R-squared	0.074	0.015	0.022
Local polynomial order	2	2	2
Bandwidth	628.84	495.68	505.48
	b) Expenditures: diff-in-disc effect on forecast error of:		
	Current outlays	Capital outlays	Other expenditures
	(4)	(5)	(6)
	0.003	0.095**	-0.052
	(0.027)	(0.044)	(0.056)
Sample mean before[(a)]	0.236	0.910	0.422
Observations	2,010	2,329	3,457
Municipalities	414	471	681
R-squared	0.029	0.025	0.006
Local polynomial order	2	2	2
Bandwidth	510.98	600.78	850.46

Notes: See footnotes of Table 3.

5.2 Heterogeneity of the effect across municipal characteristics

Municipalities with a different composition of the population, geographical structure, and geographical location could be heterogeneous in terms of composition of local government officials and their political and normative approach to budgeting, of difficulties in forecasting future revenues and expenditures, of different ways in which the electorate reacts to deviations from the promises in terms of revenues and expenditures, especially taxes and services. Hence, in this section, we aim at understanding whether the removal of fiscal constraints had differential effects across some observed dimensions of municipal heterogeneity.

We examine three dimensions of heterogeneity that could capture a different level of social and civic capital of the population and affect therefore the functioning of the institutions (Nannicini et al., 2013). First, we consider the heterogeneity due to geographical location, as correlated with economic development and social capital (Grembi et al., 2016). As such, the needs and forces diverting the local politicians from respecting the fiscal restraints could be different across the Italian regions. Second, we consider the

geographical extension, as there is evidence for Italy that in larger municipalities tax evasion is higher (Casaburi and Troiano, 2016), making it more difficult for local officers to produce a good budget forecast. Finally, the composition of the population in terms of education, age, and immigrants could be an additional source of heterogeneity affecting budget decisions, for example, because highly educated people and/or younger voters might have different preferences towards public debt accumulation and good management of public finances.

Table 5 displays summary statistics of the revenue and expenditure forecast errors across the heterogeneity dimensions under investigations. Information from the composition of the population comes from the 1991 census. The revenue and expenditure underestimation is lower in the North, in small municipalities, when the fraction of immigrants is larger and of youth is smaller.

Table 5: The budget forecast error across different municipal characteristics (1999-2004)

	Forecast error in			
	Revenues		Expenditures	
	Mean	Std. Dev.	Mean	Std. Dev.
North-West[a]	0.460	0.148	0.465	0.128
North-East[b]	0.465	0.140	0.492	0.117
Centre[c]	0.554	0.168	0.547	0.154
South[d]	0.664	0.165	0.630	0.167
High surface area	0.556	0.176	0.548	0.159
Low surface area	0.501	0.173	0.505	0.150
High fraction of people with tertiary degree	0.532	0.176	0.529	0.156
Low fraction of people with tertiary degree	0.523	0.177	0.523	0.156
High fraction of immigrants	0.489	0.165	0.497	0.143
Low fraction of immigrants	0.565	0.180	0.555	0.163
High fraction of young people (0-14 years old)	0.570	0.188	0.561	0.168
Low fraction of young people (0-14 years old)	0.486	0.153	0.491	0.134
High fraction of old people (65+ years old)	0.530	0.172	0.525	0.154
Low fraction of old people (65+ years old)	0.525	0.181	0.527	0.158

Notes: "High" and "Low" refer to being above and below the median of the distribution of the corresponding variable.
[a] The North-West includes municipalities in Liguria, Lombardia, and Piemonte.
[b] The North-East includes municipalities in Emilia-Romagna and Veneto.
[c] The Centre includes municipalities in Lazio, Marche, Toscana, and Umbria.
[d] The North-East includes municipalities in Abruzzo, Basilicata, Calabria, Campania, Molise, and Puglia.

Table 6 reports the estimation results after splitting the sample according to the het-

erogeneity dimensions reported in Table 5. Columns (1)-(4) show the effect heterogeneity across geographical areas. They strongly suggest that the effect at the national level for both the revenue and the expenditure forecast errors are driven by the municipalities in the North-West. It has been recognized that the Italian municipalities in the North are less dependent on intergovernmental transfers and have a greater ability to adjust revenues and expenditures decisions according to citizens' preferences (Balduzzi and Grembi, 2011). Moreover, they are in a more dynamic economic context than the municipalities in the rest of Italy: this might allow them to have more room for manoeuvre on overestimating budgetary projections, especially when fiscal constraints are relaxed.[17]

Columns (5) and (6) of Table 6 focus on the effect heterogeneity by geographical extensions. Casaburi and Troiano (2016), in studying the electoral responses to the introduction of an Italian policy for contrasting the evasion of property taxes, found that tax evasion is higher in geographically larger municipalities. They speculate that in larger municipalities it is easier to hide unregistered buildings, since it is more difficult and more costly for the authority to monitor and enforce building registrations. If so, we might expect geographically large municipalities just below the cutoff to have lower incentives than geographically large municipalities just above the cutoff to program costly activities to collect taxes once released from the DSP. Therefore, their ability to predict the actual revenues could be lower, with consequent larger revenue forecast errors. What we observe in columns (5) and (6) of Table 6 is consistent with our conjecture: the effect on the revenue forecast error at the national level is largely driven by geographically larger municipalities, and we do not observe any difference in terms of impact on the expenditure forecast error.

Columns (7)-(14) of Table 6 report the effect heterogeneity according to different demographic structure of the residents. We find that in municipalities with a younger population, the effect is more relevant both in terms of revenue and expenditure forecasting errors. Moreover, municipalities with a high fraction of highly educated people and of immigrants display a stronger impact of the relaxation of fiscal restraints on expenditure forecast errors. This might be explained by the fact that a greater percentage of graduates, young people, and foreigners might act as a disciplinary device, magnifying the costs of not complying with the DSP. There is indeed evidence that young voters dislike public

[17]The reason commonly invoked for explaining exuberance in budget forecasts is the over-optimism in the official predicting the economic growth rate (Strauch et al., 2004; Frankel, 2011).

Table 6: The heterogeneity of the effect

	(1) North-West	(2) North-East	(3) Centre	(4) South	(5) High surface area	(6) Low surface area	(7) High fraction of tertiary educ.	(8) Low fraction of tertiary educ.	(9) High fraction of immigrants	(10) Low fraction of immigrants	(11) High fraction of youth	(12) Low fraction of youth	(13) High fraction of eldest	(14) Low fraction of eldest
a) Difference-in-discontinuities effect on revenue forecast error														
	0.144**	0.007	0.023	0.053	0.198***	0.060	0.147**	0.140**	0.124*	0.120*	0.165**	0.050	0.085	0.141*
	(0.066)	(0.091)	(0.089)	(0.105)	(0.065)	(0.060)	(0.064)	(0.068)	(0.066)	(0.066)	(0.075)	(0.062)	(0.061)	(0.073)
Sample mean before[(a)]	0.507	0.499	0.619	0.711	0.606	0.548	0.576	0.569	0.541	0.603	0.635	0.513	0.557	0.591
Observations	1,234	764	362	621	1,034	1,294	1,437	1,085	1,426	974	1,013	1,244	1,267	1,118
Municipalities	246	150	72	132	209	268	289	221	287	199	211	249	251	234
R-squared	0.066	0.051	0.057	0.038	0.030	0.049	0.054	0.041	0.064	0.027	0.051	0.041	0.024	0.047
Local polynomial order	2	2	2	2	2	2	2	2	2	2	2	2	2	2
Bandwidth	814.05	853.96	542.67	613.95	538.48	646.01	757.75	497.75	707.48	507.25	520.23	630.90	658.95	556.17
b) Difference-in-discontinuities effect on expenditure forecast error														
	0.133*	0.026	-0.075	0.018	0.095	0.079	0.122*	0.084	0.112*	0.067	0.112	0.034	0.016	0.134*
	(0.078)	(0.080)	(0.093)	(0.101)	(0.62)	(0.512)	(0.067)	(0.062)	(0.067)	(0.060)	(0.072)	(0.068)	(0.052)	(0.073)
Sample mean before[(a)]	0.462	0.483	0.554	0.643	0.552	0.512	0.537	0.523	0.502	0.558	0.584	0.479	0.517	0.547
Observations	932	758	379	668	1,193	1,206	1,223	1,075	1,408	1,013	960	1,160	1,438	1,130
Municipalities	190	150	73	145	243	247	251	221	285	205	206	228	283	237
R-squared	0.024	0.038	0.027	0.014	0.012	0.016	0.030	0.026	0.021	0.011	0.023	0.019	0.0011	0.015
Local polynomial order	2	2	2	2	2	2	2	2	2	2	2	2	2	2
Bandwidth	659.07	847.82	584.63	654.23	628.33	608.92	671.08	493.85	698.44	528.48	493.09	599.18	744.19	563.00

Notes: See footnotes of Table 3.

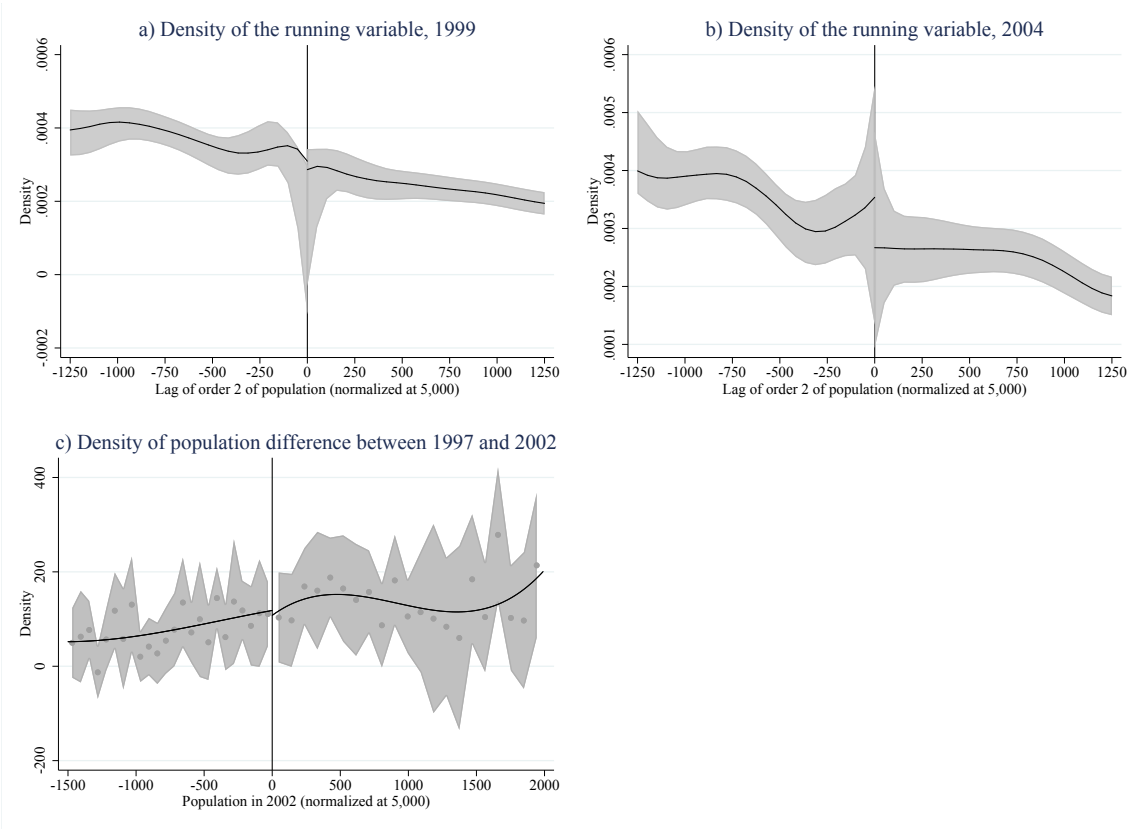
debt accumulation, which involves higher taxes within their lifetime and a crowding-out in the provision of the public goods (Song et al., 2012). For similar reasons, people with tertiary education might be able to assess the future costs associated with poor quality management of public finances. Empirical evidence suggests that the fraction of immigrants are larger in the North of Italy (Mocetti and Porello, 2010). Foreigners indeed typically move to geographical areas offering more job opportunities. This means that the high share of foreign people is positively correlated to more favourable economic conditions and greater economic perspectives, which allow local governments to overestimate more revenues and expenses, especially when they are not constrained fiscally. Hence, it is difficult to pinpoint whether the heterogeneity of the impact across this dimension is related to the presence of immigrants or rather to the economic conditions and perspectives.

5.3 Validity and falsification tests

As suggested by McCrary (2008), a jump in the density of the running variable at the threshold would be direct evidence of the failure of the local randomization assumption and indirectly of Assumption 1. This might happen if the municipalities close to the cutoff manipulated the official population records to avoid the fiscal rules. The fiscal rules were changed by national financial law 388/2000 in force in the last days of 2000. The financial law relaxed the fiscal rules for municipalities smaller than 5,000 inhabitants, as measured two years earlier. The municipalities eligible in 2002 to the removal of the fiscal restraints were defined on the basis of the 2000 population. Thus, the design of the policy intervention makes it very unlikely that mayors around the cutoff were able to manipulate the population size. Although unlikely, it is however possible that some mayors could have anticipated the new institutional set-up and put in practice along 2002 a set of interventions to affect the population size so as to fall below the cutoff, for example by not counter-reacting population drops (Grembi et al., 2016). If this were the case, we might observe a discontinuity in the density of the population size. Graphs a) and b) in Figure 3 report the local polynomial density estimate of the running variable described in Cattaneo et al. (2018). They show that there is no evidence of discontinuity in the population density at the cutoff, both in 1999 and in 2004. The robust bias-corrected test proposed in Cattaneo et al. (2018) cannot reject the null hypothesis of the absence of discontinuity, with a p-value equal to 0.691 in 2004 and 0.483 in 1999. Graph c)

reports, instead, the relation between the difference in the population registered in 2002 and 1999 along with the population in 2002. This is to graphically visualize if there might have been a manipulative sorting changing over time. Indeed, although the densities of the population before and after 2001 do not jump at the cutoff, it might be that after the relaxing of fiscal restraints, some municipalities tried to sort below and some others to sort above the cutoff. The scatter plot and the 3rd order polynomial fit in graph c) suggest that there is no evidence for changes in manipulative sorting before and after 2001.[18]

Figure 3: Graphical density test of the running variable



Graphs a) and b): The solid lines are the the local polynomial density estimate of the running variable described in Cattaneo et al. (2018). The local polynomial is of order 3. The shaded areas are 95% confidence intervals.
Graph c): The solid line is obtained by regression functions based on a 3rd-order polynomial regression of the difference between 2002 and 1997 population on the 2002 population, fitted separately above and below the cutoff. The dots represent local sample means of disjoint bins of the running variable reported in the midpoint of the bin. The number of bins and their lengths are chosen optimally using the mimicking variance integrated mean-squared error criterion. The shaded areas are 95% confidence intervals.

Under the assumption that there is no change over time in the pattern of manipulative

[18]The point estimate of the discontinuity at the cutoff is -10.415 with a standard error equal to 56.951.

sorting around the cutoff, the treatment should not have an effect on the pre-treatment covariates (Grembi et al., 2016). We follow Lee and Lemieux (2010) and test if the differences in the discontinuities are significantly different from zero, by estimating a seemingly unrelated regression (SUR) with one equation for each of the predetermined variables. Each equation is estimated on the observations within its MSE-optimal bandwidth (Calonico et al., 2014) and weighted using a triangular kernel. After the estimation of such a SUR model, we performed joint and individual tests of the significance of the differences in the discontinuities. Table 7 reports these individual and joint test statistics. Only the dummy indicator for municipalities in Puglia displays a significant coefficient with a p-value equal to 0.036. However, the joint test does not reject the null hypothesis that the differences in the discontinuities are significantly different from zero. Since we are testing on many covariates, the joint test suggests that the only significant effect might be so by random chance (Lee and Lemieux, 2010).

A possible concern is that our estimates are not the causal effect of different fiscal treatments of municipality below and above the cutoff, but they are due to omitted variables inducing correlation between population size and the outcome variable, failing therefore the local randomness assumption, or simply to randomness. Akin to the framework for permutation inference tests (Abadie et al., 2010) and as in Grembi et al. (2016), we perform a set of placebo diff-in-disc regressions for revenue and expenditure forecast errors by setting the population cutoff to false thresholds. More in detail, we run 399 diff-in-disc estimates by setting the cutoff from 4,801 to 4,999 and from 5,001 to 5,200. This creates a distribution of 399 placebo effects and allows us to detect the eventual systematic presence of policy effects at the false cutoffs similar to the actual estimates. Figure 4 displays the cumulative distribution function of the 399 placebo effects, along which their 95% confidence interval and the actual estimates of the effect on revenue and expenditure forecast errors. Only 0.5% (0.25%) of the placebo estimates of the discontinuity for the revenue (expenditure) forecast error is larger than the actual estimate, providing strong support for the absence of systematic effects when moving the cutoffs to false thresholds and, therefore, for the robustness of our findings.

A further check aims at understanding whether the results are sensitive to the local polynomial order and to the bandwidth choice. Table 8 reports the diff-in-disc estimates if we modify the local polynomial order and, instead of using a data-driven optimal bandwidth selector (Calonico et al., 2014), we alternatively and arbitrarily fix the bandwidth at

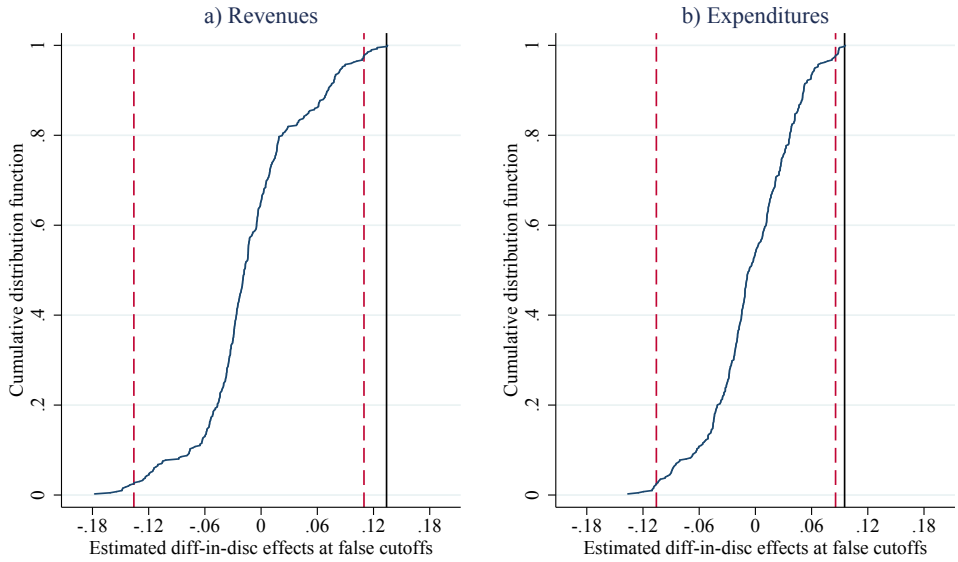
Table 7: Falsification test: treatment effect (difference in discontinuities) on predetermined variables estimated by SUR[(a)]

		Significance test of discontinuity at the cutoff	
		z-stat[(b)]	p-value
Predetermined covariates, 1991 census			
Employment rate		-1.71	0.088
Fraction of people younger than 15		0.81	0.420
Fraction of people older than 64		-0.60	0.547
Fraction of women		1.29	0.197
Fraction of immigrants		0.08	0.936
Fraction of people with higher secondary degree		0.55	0.584
Fraction of people with tertiary degree		1.04	0.298
Number of families per capita		0.94	0.346
Municipality surface		1.19	0.235
Joint significance test of diff-in-disc estimates for predetermined covariates[(b)]	$\chi^2(9) = 12.53$		0.185
Regional time dummies			
Abruzzo/Molise		1.69	0.092
Basilicata		0.29	0.769
Calabria		0.65	0.519
Campania		1.30	0.194
Emilia-Romagna		-1.00	0.317
Lazio		1.19	0.233
Liguria		-1.13	0.260
Lombardia		-0.45	0.652
Marche		-0.66	0.508
Piemonte		-0.50	0.615
Puglia		-2.10	0.036
Toscana		-0.37	0.714
Umbria		-0.98	0.328
Veneto		0.18	0.856
Joint significance test of diff-in-disc estimates for regional dummies[(b)]	$\chi^2(14) = 15.36$		0.354
Joint significance test of diff-in-disc estimates for all covariates[(b)]	$\chi^2(23) = 30.32$		0.140

[(a)] We follow Lee and Lemieux (2010) and test if the differences in the discontinuities are significantly different from zero by estimating a SUR with one equation for each of the predetermined variables. Each equation is estimated using local quadratic regression using the observations within its MSE-optimal bandwidth (Calonico et al., 2014) and weighted using a triangular kernel. The full set of estimation results is not reported for the sake of brevity. They are available from the authors upon request.

[(b)] The test statistics are robust to heteroskedasticity and within-municipality correlation.

Figure 4: Placebo tests for the effect on revenue and expenditure forecast error across false cutoffs



Notes: The solid vertical line is the actual estimate of the difference in the discontinuities. The dashed vertical lines identify the 95% confidence interval of the placebo effects across the false cutoffs. They are obtained by estimating diff-in-disc with 2nd order polynomials across false cutoffs, by fixing each time the threshold from 4,801 to 4,999 and from 5,001 to 5,200.

150, 250, 500, and 1,000. Columns (1)-(4) focus on the estimated effect with the local linear polynomial fit and increasing bandwidth. The remaining columns replicate the same exercise but with local quadratic polynomial fit. Table 8 shows that when we increase the bandwidth but we fix the polynomial order, we gain in precision, but the strict parametric restrictions on the relation between the forcing variable and the outcome variable bias the estimated effect on both revenue and expenditure forecast error towards zero. Grembi et al. (2016) found a similar bias towards zero on the effect on the fiscal gap and deficit when enlarging the bandwidth.

Finally, we report in Table 9 the estimation results if we include in Equation (6) municipality and time fixed-effects. The point estimates are closer to 0 but they are also more precisely estimated. The impact of relaxing fiscal restraints on revenue forecast error is still significant at 1%. The one on expenditure forecast error is now significant only at 10%. However, given the large standard errors, its 95% confidence interval largely includes the previous point estimate. A bootstrapped Hausman statistic to test the difference between the estimated effects cannot reject the null hypothesis that the two estimates are

Table 8: Difference-in-discontinuities effect on revenue and expenditure forecast error using different predetermined bandwidths and local polynomial regression of different orders

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
a) Difference-in-discontinuities effect on revenue forecast error								
	0.153**	0.125**	0.078**	0.031	0.239**	0.155**	0.140***	0.078**
	(0.066)	(0.048)	(0.037)	(0.026)	(0.098)	(0.075)	(0.050)	(0.039)
Observations	552	998	1,970	4,083	552	998	1,970	4,083
Municipalities	177	249	408	773	177	249	408	773
R-squared	0.053	0.042	0.032	0.027	0.061	0.044	0.035	0.028
Local polynomial order	1	1	1	1	2	2	2	2
Bandwidth	150	250	500	1,000	150	250	500	1,000
b) Difference-in-discontinuities effect on expenditure forecast error								
	0.120*	0.083*	0.053	0.009	0.219**	0.116	0.099**	0.048
	(0.065)	(0.048)	(0.035)	(0.024)	(0.098)	(0.072)	(0.050)	(0.037)
Observations	553	993	1,967	4,082	553	993	1,967	4,082
Municipalities	178	250	408	773	178	250	408	773
R-squared	0.017	0.010	0.007	0.004	0.027	0.014	0.010	0.005
Local polynomial order	1	1	1	1	2	2	2	2
Bandwidth	150	250	500	1,000	150	250	500	1,000

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are reported in parenthesis and are robust to heteroskedasticity and within-municipality correlation. We used the triangular kernel to weight observations from the cutoff.

equal to each other.[19]

6 Conclusions

The effectiveness of budget rules to correct distortionary fiscal outcomes is still an open question in the literature. Indeed, they could fail to achieve the main objective, because they could stimulate "creative accounting" measures and/or opportunistic fiscal policy decisions for electoral purposes.

By exploiting the quasi-natural experiment generated in 2001 by the exemption from the DSP of the Italian municipalities below 5,000 inhabitants, the tightening of budgetary constraints, and the introduction of severe sanctions for noncompliers in 2002, we estimated the effect of budget rules on budgetary forecast errors. We found that the DSP was effective in reducing budgetary forecast errors in Italian municipalities. In particular, our

[19]The difference between the ordinary least squares estimate and the municipality and time fixed effects estimate amounts to 0.027. The bootstrapped standard error (1,000 bootstraps), robust to within-municipality correlation, is 0.039 (p-value equal to 0.484).

Table 9: Difference-in-discontinuities effect on revenue and expenditure forecast errors with municipality and time fixed-effects

	(1) 1999-2000 vs 2001-2004	(2) 1999-2000 vs 2001	(3) 1999-2000 vs 2002	(4) 1999-2000 vs 2003	(5) 1999-2000 vs 2004	(6) 1999-2000 vs 2002-2004
a) Difference-in-discontinuities effect on revenue forecast error						
	0.092***	0.042	0.116**	0.097*	0.080	0.090**
	(0.035)	(0.039)	(0.054)	(0.062)	(0.063)	(0.044)
Observations	2,103	1,307	1,232	1,058	1,134	1,758
Municipalities	430	480	472	413	454	431
b) Difference-in-discontinuities effect on expenditure forecast error						
	0.068*	0.004	0.108*	0.081	0.055	0.066
	(0.036)	(0.034)	(0.058)	(0.060)	(0.059)	(0.045)
Observations	2,197	1,621	1,288	1,067	1,174	1,792
Municipalities	453	584	494	414	464	441

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors are reported in parenthesis and are robust to heteroskedasticity and within-municipality correlation. The optimal bandwidth is the same used for the estimation reported in Table 3. We used the rectangular kernel to weight observations from the cutoff.

results point out that municipalities affected by the budget rule had more accurate revenue and expenditure projections, especially for fees and tariffs and capital outlays. Considering geographical and demographic heterogeneity of the effects of budget rule across municipalities, our results show that municipalities in the North-West, which are more economically developed and less dependent on intergovernmental transfers, made more optimistic forecasts in the absence of fiscal constraints. More accurate budgetary projections are also observed in those municipalities where the local fiscal rule is accompanied by a high share of young people and inhabitants with tertiary education.

The DSP was set-up with a "carrot and stick" approach, with incentives for complying municipalities introduced in 2001 and severe penalties for noncompliers in 2002. By splitting the before period year by year, we found evidence suggesting that severe sanctions and stricter fiscal constraints were effective in reducing the budgetary forecast errors of municipalities subject to the DSP relatively to those of small municipalities. The quasi-experimental design of our identification strategy, jointly with the results from several validity and falsification checks, corroborated the internal validity of our findings. Although the policy discontinuity lowers their external validity, it should be considered that in Italy many municipalities are located near the DSP discontinuity cutoff. For example, in 2002 the 50th and 75th percentiles of the population distribution across municipalities

were 2,400 and 5,850 inhabitants, respectively.

There has been much discussion in Italy on the effectiveness of the DSP. The continuous changes in its objectives, criteria, and sanctions have created many uncertainties in its application and have created doubts about its usefulness among Italian mayors. Our empirical analysis shows that the pact, in its "carrot and stick" version, was effective in reducing budgeting bias especially when stringent budgetary restraints are accompanied by severe penalties ("the stick") instead of rewards ("the carrot").

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