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June 30, 2020

**Abstract**

We study the impact of the domestic stability pact on the accuracy of budget forecasts of Italian municipalities. 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 fewer 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. In fact, revenue (expenditure) forecast errors for municipalities with fewer than 5,000 inhabitants became 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 classification codes:** E62, H68, H72

*We would like to thank the Editor, Jan-Egbert Sturm, and the two anonymous reviewers for their valuable comments. We are grateful to Ugo Fratesi and the participants in the XL AISRe Conference (L'Aquila, Italy), XXXI SIEP Conference (Turin, Italy) and International Conference on Regional Science (Castellò de la Plana, Spain) for their useful suggestions. We are also grateful to Daniele Ripanti for his help with the data collection. The usual disclaimer applies.

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# 1 Introduction

In 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 may 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, and Italy, the adoption of sub-central fiscal rules has been the result of abiding with budget agreements taken at the supranational level through the Stability and Growth Pact (SGP).[1]

Fiscal rules have been often seen as useful means to curb fiscal indiscipline, especially by local authorities, and to prevent biased budget estimates that are responsible for excessive deficits (Von Hagen and Harden, 1994; Alesina and Perotti, 1996; Frankel, 2011; Chatagny and Soguel, 2012; Frankel and Schreger, 2013). The systematic biased formulation of fiscal variables has indeed major 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 large body of 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 subjected to the Domestic Stability Pact (DSP), a sub-central fiscal rule which restrained the budgetary liberty of local governments. The DSP adopted a "carrot and stick" approach to encourage virtuous behavior and punish noncompliers. However, since 2001 municipalities with fewer than 5,000 inhabitants have been exempted from the DSP.[2] In 2002, more stringent budgetary

[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 curb overspending and excessive indebtedness of local governments.

[2] In 2012, the population threshold was lowered and re-included municipalities with populations be-

restrictions and severe penalties for the violation of the pact were introduced to discourage further non-compliant municipal budgeting decisions. The DSP has been at the center of numerous political debates, fuelled by the protests of the mayors, who have demanded relaxation of its constraints, especially those on investment spending, or even elimination of the pact. Grembi et al. (2016) showed that the Italian municipalities affected by the DSP had significantly increased their taxes and decreased their deficits, suggesting that the DSP was effective in favoring the sustainability of public finances. However, the misrepresentation of budgetary decisions in Italy is a further key issue, which was not investigated by Grembi et al. (2016). It is indeed well-documented in Cepparulo et al. (2014) and Anessi-Pessina and Sicilia (2015) for national and local contexts, respectively.

In this study, we analyze the effects of the DSP on the accuracy of 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 fewer than 5,000 inhabitants generates a quasi-natural experiment to 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 behavior 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 to determine how and to what extent the DSP affects municipal budget forecasting, because repeated forecasting errors in local government fiscal variables may frustrate the efforts made by the central government to consolidate national public finances, and they may 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 with fewer than 5,000 inhabitants, following the identification strategy described in Grembi et al. (2016); ii) stricter budgetary restrictions and severe penalties for noncompliers in 2002. Third, we study whether the effect was heterogeneous across municipalities with different characteristics, the purpose being to enrich the scenario further and speculate on the mechanisms inducing change in the budgetary forecast behavior of local administrators.

We find that the 2001 removal of the fiscal restraints for small municipalities and the

tween 1,000 and 5,000 inhabitants (Law 228/2012).

introduction of incentives for compliers did not affect forecast errors as regards either revenues or expenditures. However, after the ceiling on current expenditure growth was introduced in 2002 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 with just under 5,000 inhabitants became 26% (22%) larger than those of municipalities just above the cutoff in 2002. The results for revenues are due to increases in the forecast errors in taxes and, especially, in fees and tariffs. Larger errors in revenue projections may be due to excessive exuberance in budget forecasting and/or to a lesser 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 forecast errors concerning capital outlays. Considering different dimensions of municipality heterogeneity, we find that the effects on the revenue forecast errors are driven by municipalities in the North-West, with larger territories, and with a higher proportion of young people. The results of the expenditure forecast errors are ascribed to municipalities in the North-West, but also to those with high shares of immigrants, young people, and inhabitants with tertiary educations.

Our paper is organized as follows. Section 2 summarizes the literature on theoretical and empirical analysis of the effects of fiscal rules on fiscal outcomes, including budgetary projections. Section 3 focuses on the DSP in Italy. Section 4 describes the sample used in the empirical analysis, the outcome variables, the econometric model, and the identification assumptions. Section 5 reports the empirical findings. Section 6 concludes.

## 2 Literature review

Several studies have investigated the reasons for errors in fiscal forecasts. They have done so on the basis of political-institutional considerations by comparing distortions in revenue, expenditure, and deficit projections in different countries. Budget forecasts have been theorized and found to be especially sensitive to political factors,[3] the form of fiscal governance, and fiscal rules.

First, political considerations often come into play with the electoral cycle jointly

[3]More in detail, countries that face elections, those that are ruled by left-wing coalitions, and those that have fragmented governments appear to make over-optimistic budget plans (Strauch et al., 2004; Brück and Stephan, 2006; Boylan, 2008; Pina and Venes, 2011; Jochimsen and Lehmann, 2017).

with overly optimistic assumptions about the business cycle. Some empirical studies have shown that the overestimation of GDP growth rates is a significant determinant of upward biases in revenue forecasts in many countries (Strauch et al., 2004; Pina and Venes, 2011). Indeed, finance ministers and the government, in search of a broader electoral consensus, tend to formulate overly optimistic economic growth projections that enable them to prepare rosy estimates of both revenue and expenditure. In this way, they avoid unpopular tax increases and spending cuts that would penalize them in the elections (Jonung and Larch, 2006).

Second, the nature of the fiscal governance, like the nature of the negotiations among the members of the executive branch of the government and/or the degree of political commitment to fiscal programs, is a further influencer of the budget forecast biases (Annett, 2006; Von Hagen, 2010). For example, in the *delegation approach*, the finance minister is responsible for the budget as a whole and can use his/her agenda-setting power over the other ministers to control their spending bids. Because there is less need to bargain over the spending of each ministry, the incentive for strategically predicting with bias the real GDP growth is smaller than in the *contract approach* where the finance minister negotiates with the other ministers the numerical targets of the budget at the start of the annual budget process. In line with this speculation and using data for the EU-15 countries from 1998 to 2004, Von Hagen (2010) found that projection errors of both real GDP and revenues are biased upwards (downwards) in countries operating under the delegation (contract) approach.

Third, fiscal rules and their strictness may affect budget forecast errors. In the past two decades, many countries have introduced national fiscal rules to contain government expenditure and debt accumulation so as to ensure the long-term sustainability of public finances and greater equity across generations (IMF, 2009). Countries in currency unions, especially in the eurozone, have additionally adopted supranational fiscal rules to improve the coordination of monetary-fiscal policies among their country members and to internalize the regional costs of fiscal indiscipline (Annett, 2006; IMF, 2009). The 2008 financial crisis induced many countries to adopt fiscal austerity policies through drastic budget cuts and stricter budget rules to prevent excessive exuberance in estimating the budget balance and the tax revenues, which are responsible for the excessive deficit and debt accumulation (Von Hagen and Harden, 1994; Alesina and Perotti, 1996; Frankel, 2011; Chatagny and Soguel, 2012; Frankel and Schreger, 2013). Expenditure rules proved to be particularly effective in achieving these goals (IMF, 2009). Indeed, using data from the

excessive deficit procedure notifications and national drafts for the EU-15 countries, Pina and Venes (2011) found that countries formulated more prudent budget forecasts when national-level numerical rules on public expenditure came into force, especially in the post-SGP period. They did not find any statistically significant effect of balance budget rules on deficit forecasts. Using a sample of 33 countries, Frankel (2011) found instead that deficit rules, in the form of the SGP, create over-optimistic bias in budget balance forecasts. From a theoretical viewpoint, Baldi (2016) showed that the adoption of ex-ante rules on the planned deficit could be effective in containing the budget deficit, although less effective than ex-post rules on the realized deficit. The effects of ex-post rules on both the planned 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 not without drawbacks, however. They can encourage measures of "creative accounting" (Milesi-Ferretti, 2004) through the formulation of biased budget projections aimed at overcoming the rule prescriptions or postponing unpopular decisions such as tax increases and spending cuts (Alesina and Perotti, 1996). Several empirical studies have found significant evidence of strategic manipulation of budget forecasts, especially in the eurozone. In particular, fiscal rules created over-optimism in official budget forecasts, especially in the run-up to election (Brück and Stephan, 2006; Frankel, 2011; Pina and Venes, 2011; Merola and Pérez, 2013). 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. Frankel and Schreger (2013) found over-optimistic forecasts when countries are most in danger of breaching the limit of 3% imposed by the SGP. The forecast bias is smaller when countries have adopted their own national budget balance rules or have independent fiscal institutions providing independent forecasts. As suggested by Merola and Pérez (2013), delegating the preparation of budget projections to independent fiscal institutions could avoid distortions in the fiscal projections provided by national governments and international organizations (e.g. the European Commission, the Organization for Economic Co-operation and Development).

Many countries have adopted fiscal rules also at the local level of government, as the result of the bargaining process between central and local governments or unilateral decision by the central government. Several reasons can be adduced to justify the use

of sub-central fiscal rules. They can be used to restrain the 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 realize that the costs of local public services can be shifted to non-residents. The transfer of the costs to non-residents results in larger local public expenditure, which could be restricted by spending ceilings. Fiscal restrictions can be imposed on local borrowing autonomy to prevent the excessive use of bank loans or other forms of lending when intergovernmental transfers do not match the financial capacities of local authorities to provide centralized standards of 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 broad consensus in the literature on the desirability and the effectiveness of sub-central fiscal rules to restrain fiscal profligacy by 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 bailing out the sub-national insolvent governments is not credible. Moreover, privileged access to credit by local governments weakens the market discipline; so too does 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" ones such as "creative accounting", to meet the budget rules. He emphasizes the role played by transparent budgetary procedures in limiting accounting creativity and adopting less stringent fiscal rules. Other scholars share this view by considering budget transparency a powerful means to 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 with which to contain fiscal profligacy of local governments (Argimón and Hernández de Cos, 2012).

Many studies in the past two decades 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 systems (Foremny, 2014) and a high degree of fiscal vertical imbalance (Rodden, 2002). Increasing interest has also

been shown in the effects of sub-central fiscal rules on budgetary projections of local governments.[4] 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. 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. More recently, Rullán and Villalonga (2018) pointed out that budget deficit targets had most likely distorted the budget forecasts of the Spanish Autonomous Communities, with consequences on the regional fiscal forecast errors. Moreover, Delgado-Téllez et al. (2017) found that Spanish Autonomous Regions tend not to comply with the annual fiscal deficit target set by the central government. They also showed that the intensity of regional fiscal non-compliance increases when fiscal targets are tightened, growth forecast errors become larger, and elections are imminent.

# 3 Domestic Stability Pact for Italian municipalities

In 1999 Italy introduced a sub-central fiscal rule (Article 28, Law No. 448/1998) to fulfil 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) with the purpose of progressively reducing 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 it harder for municipalities to plan their activities in advance (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

[4]Several studies also found that political factors create significant biases in budget forecasts at local government level (see, among others, Lago-Peñas and Lago-Peñas, 2008; Benito et al., 2015; Boukari and Veiga, 2018; Rullán and Villalonga, 2018).

number of municipalities involved and the penalty system have 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 in order to increase the local tax base. The non-complying municipalities were subjected 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 reaffirmed 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 by complying municipalities.

The subsequent pact changes in 2001 favored the municipalities that in 1999 had not complied, 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 significant change introduced in 2001 was the exemption of municipalities with fewer than 5,000 inhabitants from the fiscal restraints (Art. 53, law No. 388/2000).[5] Their exclusion was decided in order to prevent 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 purposes (Pignatti, 2009).

The fiscal constraints were further tightened in 2002 since annual growth rate limits were imposed on both current expenditure and deficit at 6% and 2.5%, respectively (Art. 24, law No. 448/2001). Severe penalties for breach of the DSP were also included by

[5]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 Statistics (Istat), i.e. for the year 2003, inhabitants of 31 December 2001.

blocking 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. They were therefore difficult to implement (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 expenditure on local public goods and services and the prohibition on hiring public employees and getting 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,[6] inducing municipalities constrained by the DSP to draw up more precise budget projections, with the consequence of fewer budget forecast errors with 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 on municipalitieswith more than 3,000 inhabitants (Art. 21, law No. 311/2004), this population threshold has never been applied. Indeed, shortly afterwards, law No. 88/2005 (Art. 1-*ter*) re-established the original threshold of 5,000 inhabitants.[7]

Two significant new features were introduced in 2005. A cap on the total public expenditure growth was set at 2%. Furthermore, a distinction was drawn between virtuous and non-virtuous municipalities.[8] 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 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,

[6]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.

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

[8]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.

because in using them the risk of violating the DSP spending ceilings would have been higher, from 2007 onwards, the government reintroduced restrictions on the side of the municipal deficit growth. This choice also had the purpose of making the DSP more compliant with the European Union financial requirements and of 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 made Italian municipalities more fiscally accountable by increasing their revenues and reducing their debts (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. To be noted is that the attempt to increase the fiscal responsibility of municipalities started even earlier than the DSP, at the beginning of the 1990s. While progressively reducing the role of the central government in the direct financing of municipalities, the local authorities were receiving greater autonomy in collecting taxes. In 1993 municipalities became entitled to set a property tax (Legislative Decree 421/1992) with the possibility to choose the tax rate within a predefined range. In 1997, a tax on production activities was introduced at regional level by Legislative Decree 446/1997 and a share of it was attributed to the municipalities. Finally, since 1999 municipalities have been empowered by Legislative Decree 360/1998 to set a personal income tax surcharge, with some autonomy in deciding the tax rate. After these major steps, in the following years the process continued. Table 1 shows the evolution of the revenue composition of Italian municipalities during the time window studied in our empirical analysis. From 1999 until 2004, the weight of local taxes increased by almost 6 percentage points whereas the importance of intergovernmental transfers decreased by about 7 percentage points. This reduction was essentially due to the decline of those from the central government. It is also evident that the figures changed most between 2001 and 2003.

Table 1: Revenue composition of Italian municipalities from 1999 until 2004 (%)

| Revenues | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 |
|---|---|---|---|---|---|---|
| Taxes | 25.4 | 23.9 | 20.7 | 26.7 | 30.4 | 30.9 |
| Transfers | 22.4 | 24.4 | 27.1 | 22.7 | 17.4 | 15.7 |
| *State level* | *16.6* | *18.1* | *20.3* | *15.7* | *10.8* | *9.9* |
| *Regional level* | *4.4* | *5.1* | *5.5* | *6.7* | *6.2* | *5.4* |
| *Other* | *1.4* | *1.3* | *1.3* | *0.4* | *0.4* | *0.4* |
| Other revenues | 52.2 | 51.6 | 52.2 | 50.6 | 52.2 | 53.4 |

*Source:* Our elaboration on Istat data, "Finanza locale: entrate e spese dei bilanci consuntivi (comuni, province e regioni)", 1999–2004.
*Note:* We calculated the above figures summing up the different types of revenues across municipalities and then computing the fraction of each item.

# 4 Method

## 4.1 Data and sample

The main data source for our analysis was the database on local public finance compiled by the Italian Department of Territorial and Internal Affairs.[9] Italian municipalities are obliged to communicate their balance sheets as approved by the local council to the Ministry of the Interior, on paper until 2001 and on electronic support since 2001.[10] The collected data are checked and re-organized by the staff of the Ministry of the Interior to form the final database that is now available on the website of the Italian Department of Territorial and Internal Affairs. The dataset therefore contains the figures as reported in both the official budget forecasts, which municipalities must approve by the beginning of the year, and the official final balance sheets, which municipalities must approve by the end of the year. However, if accounting errors are detected by the staff of the Ministry of the Interior, the local government is asked to fix the problems in the amounts that have been budgeted. The figures in the final database are then accordingly corrected. Similar adjustments have been made as a consequence of an audit by the Italian Court of Auditors. The database contains detailed administrative 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 official forecasts at the start of each year.

Our secondary data source, still at the municipality level, was the 1991 census con-

[9] See Finanza Locale website on https://finanzalocale.interno.gov.it/banchedati.html.
[10] See Art. 44 of Legislative Decree 1992/504 and Art. 161 of Legislative Decree 2000/267.

ducted by the *Atlante Statistico dei Comuni* of the Italian National Institute of Statistics (Istat).[11] We extracted a set of demographic and economic variables: the employment rate, the fraction of young/elderly people, women, immigrants, and high/old educated people.[12]

Our empirical analysis focused on a sample of Italian municipalities over the 1999-2004 period. We started from 1999 because data on budgetary forecasts were not available before that year at the municipal level. We did not use data after 2004 because many features of the domestic stability pact changed in 2005 and later years, making it difficult to isolate the mechanisms behind the change in the budgetary forecast behavior of local administrators. Hence, by considering only the initial years after the introduction of the pact, we had a period of almost homogeneous norms.

Municipalities are the lowest level of local government in Italy. Our sample comprised only municipalities belonging to the 15 regions with an "ordinary regime". The remaining five regions[13] are subject to a "special regime", defining a different relationship with the regional government and implying that: i) they have more legislative and fiscal power than the other regions, thus affecting the fiscal policy decisions of their municipalities; ii) in 2002 they enacted their own municipal budget rules, preventing a comparison with the municipalities in the rest of the country (Grembi et al., 2016). Because we removed these five regions, the sample size shrank from about 8,000 municipalities per year to almost 6,700. Since the identification strategy would be based on local random assignment of the treatment at the cutoff of 5,000 inhabitants, we limited our sample to municipalities close to this cutoff and kept only those with 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 2 shows the absolute frequencies of the municipalities by year across our sample selection criteria. The figures reported in column (iv.a) refer to the municipalities used to study the impact of relaxing fiscal restraints on revenue forecast errors. The ones in

[11] See https://www.istat.it/it/archivio/113712.

[12] We will exploit this information in a validation test of the identifying assumptions of the causal effect.

[13] Friuli-Venezia Giulia, Sardegna, Sicilia, Trentino Alto-Adige, Valle d'Aosta.

column (iv.b) are instead those used for the analysis of expenditure forecast errors.

Table 2: 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.a) After removing municipalities in the 1st or last percentile of revenue error distr. | (iv.b) 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 |

## 4.2 Measuring the accuracy of budget forecast errors

We measure the accuracy of the budget forecasts through a formula of the budget forecast error expressed in terms of absolute value (de Deus and de Mendonça, 2017; Boukari and Veiga, 2018):

$$fe = \left| \frac{a_{it} - f_{it}}{f_{it}} \right|, \tag{1}$$

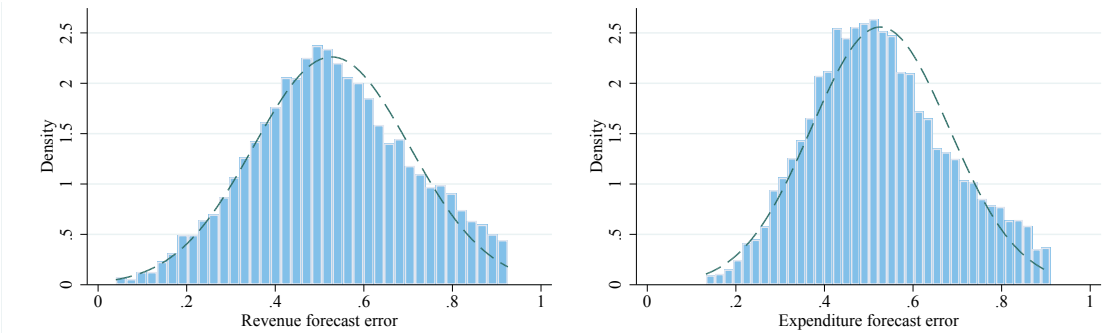
where $a$ is the realized total accrual revenues (expenditures) in municipality $i$ at the end of year $t$ and $f$ is forecasts of revenues (expenditures) at the beginning of period $t$ minus 1. If equal to 0, the revenues (expenditures) realized at the end of year $t$ correspond to their forecasts at the beginning of year $t$; the larger $fe$, the lower the accuracy, i.e. the higher the forecast errors, either due to overestimation or underestimation of the realized outcome.

The argument of the absolute value in Equation (1) multiplied by 100 is the percentage deviation of the realized outcome from its forecast. By taking the absolute value of the relative deviation of the realized outcome from its forecast, we define the forecast error as the Euclidean distance between the realized outcome and its forecast, weighted by the latter. This is consistent with the aim of our study, which is to estimate the impact of fiscal constraints on the accuracy of municipal budget forecasting (de Deus and de Mendonça, 2017; Boukari and Veiga, 2018). If instead our analysis were aimed at unveiling the impact of fiscal restraints on overoptimistic budget forecast, we could have picked the relative deviation as dependent variable, i.e. the argument of the absolute value

in Equation (1), which can take both positive and negative values. However, the relative deviation of both the realized total revenues and the realized total expenditures from their forecasts are negative for all the observations in our sample. This implies that: i) in our sample all the municipalities overestimated both the actual total revenues and the actual total expenditures, with the realized variables on average about one half of their forecasts; ii) by taking the absolute value there is not a loss of generality; iii) our findings can be interpreted as the impact of fiscal restraints both on the accuracy of the budget forecasts and on the overoptimism in budgetary projections.[14,15]

Figure 1 displays the distribution of the revenue and expenditure forecast errors as defined in Equation (1). The overestimation of the actual total revenues and the actual total expenditures is large and systematic in all the municipalities. Moreover, the revenue and the expenditure forecast errors are strongly correlated.[16] There may be different explanations for these empirical facts.

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




*Notes*: The dashed lines are normal densities.

First, the evidence that all the municipalities overestimated expenditures may be related to the fact that overestimating expenditures gives flexibility dealing with unforeseen costs, without a sudden increase in taxes (Boukari and Veiga, 2018), and conveys the

[14]In other words, if the dependent variable were defined as in Equation (1) but without the absolute value, we would have an equivalent finding interpretation.

[15]In the empirical analysis, we will also study the impact of fiscal restraints on the forecast errors for subcategories of revenues and expenses. Not for all of them the relative deviation of the realized outcome from its forecast is negative across all the observations. When it is not, the interpretation of our findings as the impact on both budget accuracy and the overoptimism in budgetary projections is lost and only the former is valid. On the revenue side, this happens for taxes, fees and tariffs, and the residual category. On the expenditure side, this happens for the residual category, but not for current outlays and capital outlays.

[16]The Pearson production-moment correlation coefficient is 0.860.

impression that by the end of the year the local government has been able to well and prudently manage the available resources (Mayper et al., 1991). This increased flexibility in spending in the coming year may be especially useful in election years. There is indeed empirical evidence that in election years opportunistic behavior in overestimating expenditures gives the incumbent a better chance of being re-elected (Aidt et al., 2011).[17] In line with what we had in our sample, Anessi-Pessina and Sicilia (2015), on studying the revenue misrepresentation for Italian municipalities larger than 15,000 inhabitants, found that revenue overestimation was frequent and seemingly aimed at creating room for greater spending in the budget year.

Second, if we calculate the forecast error for subcategories of revenues and expenses, we note that the large mean value of the forecast error of total expenditure is mainly due to the forecast error of capital outlays (average value of 0.885). In Italy, public investments were characterized by high degree of uncertainty in terms of duration and costs of construction (D'Alpaos et al., 2013; Decarolis and Palumbo, 2015). This turned out into highly distorted capital expenditure forecasts. We also find that the large mean value of the forecast error of total revenues is mainly due to the forecast error in "other revenues" (the mean of forecast error is 0.593). In this residual category of revenues, we include, among others revenue items, the revenues from the contraction of new loans (the average forecast error is 0.929) and the sale of assets (the average forecast error is 0.649). These categories of revenue are very sensitive to economic fluctuations which made them more volatile and uncertain than other categories of municipal revenues such as local tax, especially on immovable property (Presbitero et al., 2014).[18]

Third, in our data the average expenditure overestimation is large. This was already noticed by Repetto (2018), who explained it as being "due to the fact that, while there is no penalisation in forecasting a high amount and then lower estimates, in case expenditures exceed those planned in the budget the council approval is required". Moreover, although Legislative Decree 77/1995 stated that the budget forecasts should be in compliance with the principles of truthfulness and reliability, Caperchione (2003) noted that it did not exactly define these principles, resulting in wide discretionary margins for the officials and a low quality of the economic information for the decisions taken by local governments.

[17] See also Boukari and Veiga (2018) who found that Portuguese municipalities considerably overestimate both revenues and expenditures in election years and especially in the year before and Benito et al. (2015) who found systematic revenue overestimation by the incumbent in the election year.

[18] In Italian municipalities, they were the most important source of municipal tax revenues in the period under examination (e.g. 47% of total tax revenues in 2004).

This enables the use of possible "tricks", as pointed out by Alesina and Perotti (1996), for example creative and strategic use of what is kept on and what is off the budget, optimistic predictions on key macroeconomic variables, and strategic forecast of the effects on the budget of new policies.

Fourth, the strong correlation between the expenditure and the revenue overestimations is explained by the fact that Italian municipalities have been forced to approve budget projections with no deficit and to certify the financial coverage of the spending commitments.[19] Since the forecast of the expenditures should be covered by revenue forecasts of the same amount, every expenditure overestimation translated into a symmetric revenue overestimation to comply with the legal requirements imposed on the budgeting phase.[20]

Table 3 reports summary statistics of aggregate forecast errors as defined in Equation (1), 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).[21] Inspection of the before-after averages, it reveals an important change in the budget forecast errors. After 2001 the revenue forecast error was 0.507, against 0.566 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

[19] See art. 4 of Legislative Decree 77/1995 and art. 162 of Legislative Decree 267/2000. As a matter of fact, in our sample only 1.6% of the observations had a budget forecast in deficit, 97.5% displayed budget balance, and 0.9% a surplus.

[20] The systematic and large overestimation of expenditures and revenues was also a feature of regional budgets. From the Osservatorio Finanziario Regionale (www.issirfa.cnr.it), we gathered budget data for the 15 Italian regions with "ordinary regime" for the period 1999–2004 and computed revenue and expenditure forecast errors. We found that the actual revenues (expenditures) were on average 71.5% (64.5%) of their forecasts.

[21] 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, but also, for example, those related to territory and city planning (*Titolo III - Entrate extra-tributarie*). "Current outlays" (*Titolo I - Spesa corrente*) include current personnel expenditure, 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 consultancies, capital transfers, and other capital expenditures.

before as reported by Istat. This is the official source used by the central government to distinguish the municipalities subject to the DSP from those which were not.[22] 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 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 $x_{it} \geq 0$.

Table 3: 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 |

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 with fewer than 5,000 inhabitants, and, for larger municipalities, incentives to comply with the DSP were introduced.[23] 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 a large discontinuity in both revenue and expenditure forecast error,

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

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

with municipalities above the cutoff forecasting more poorly both revenues and expenditures.[24] Although all the municipalities were subject to the same fiscal rules before 2001, a further discontinuity was present at the same cutoff and could explain why the revenues and expenditures were badly forecasted in larger municipalities: the wages of the mayor and of the executive mayors appointed by the mayor are higher in municipalities with more than 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 this impacts on the budget.

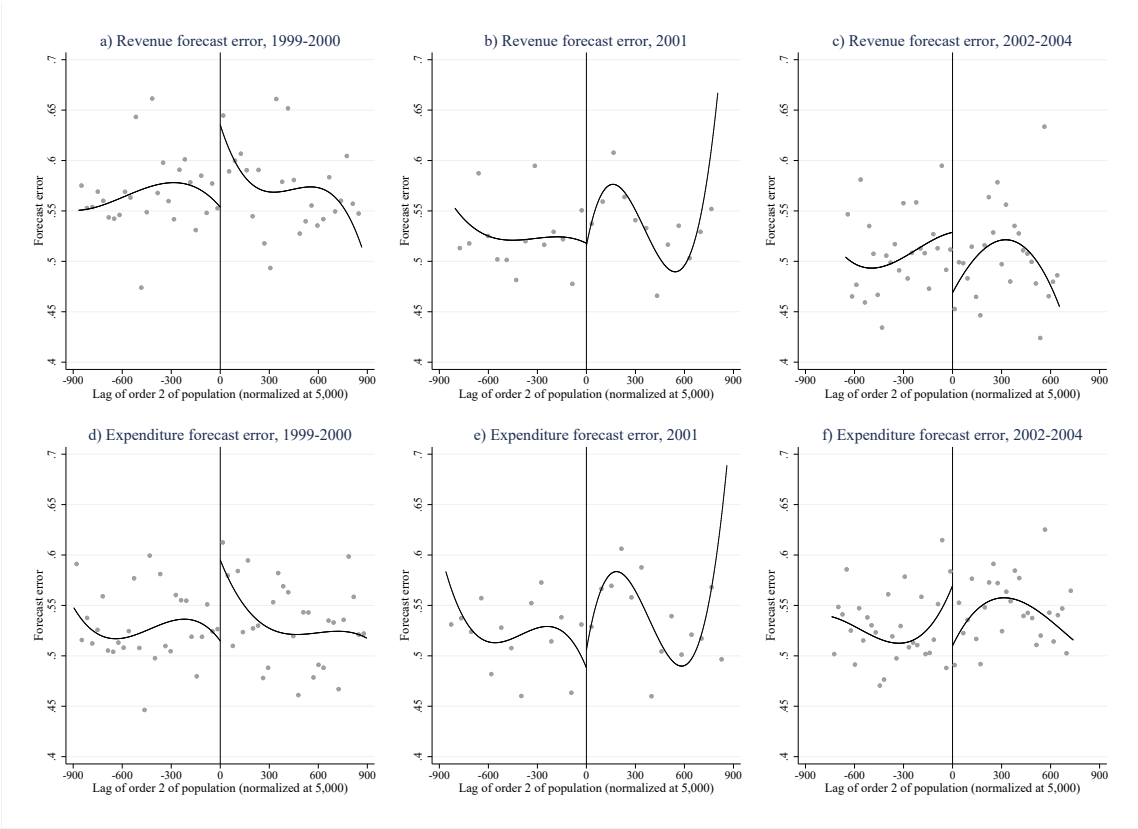
Graphs b) and e) of Figure 2 show that in 2001, with respect to the previous 2 years, two features are worth mentioning. First, although small municipalities were exempted in 2001 from complying with the DSP no change over time in the budget forecast is detectable, suggesting that the budgetary forecast behavior 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 diminished compared to both the 1999-2000 level and that of the municipalities below the cutoff. This suggests that more stringent budgetary restrictions, accompanied by the "stick" (severe sanctions) and the explicit requirement on budgetary projections, could may been effective in changing the budgetary behavior of local administrators.

## 4.3 Difference-in-discontinuities design

Let $r_{it} \equiv \mathbb{1}(x_{it} < 0)\mathbb{1}(t \geq 2001)$ denote the treatment indicator, where $\mathbb{1}(\cdot)$ 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

[24] Before 2001, the discontinuity in the revenue and expenditure forecast error amounted to 8.2 and 8 points, respectively ($p$-values equal to 0.050 and 0.044, respectively).

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 on 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. The 0 equality tests for the jump at the cutoff returned the following $p$-values from graph (a) to graph (f): (a) 0.050; (b) 0.801; (c) 0.284; (d) 0.044; (e) 0.969; (f) 0.138.

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 $t \geq 2001$,

$$\delta_t \equiv \lim_{x\to 0^-} \mathrm{E}\left[y_{it}|x_{it}=x\right] - \lim_{x\to 0^+} \mathrm{E}\left[y_{it}|x_{it}=x\right] = y_t^- - y_t^+. \tag{2}$$

However, at the same cutoff, also another treatment is assigned to Italian municipalities: mayors and the members of the executive committee are entitled to higher wages if the municipality has more than 5,000 inhabitants. Let $w_{it} \equiv \mathbb{1}(x_{it} < 0)$ 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 could 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).[25] They also detailed the assumptions for identifying the pure effect of relaxing the fiscal restraints and proposed diagnostic tools with which 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

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

$$
\begin{aligned}
\delta_{DD} \equiv & \lim_{x \to 0^-} \mathrm{E}\left[y_{it} | x_{it} = x, t \geq 2001\right] - \lim_{x \to 0^+} \mathrm{E}\left[y_{it} | x_{it} = x, t \geq 2001\right] \\
& - \left( \lim_{x \to 0^-} \mathrm{E}\left[y_{it} | x_{it} = x, t < 2001\right] - \lim_{x \to 0^+} \mathrm{E}\left[y_{it} | x_{it} = x, t < 2001\right] \right) \qquad (3) \\
= & \; y_t^- - y_t^+ - (\tilde{y}_t^- - \tilde{y}_t^+). \qquad (4)
\end{aligned}
$$

As proved by Grembi et al. (2016), $\delta_{DD}$ 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 essential 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 effect 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 neighborhood of the cutoff. Grembi et al. (2016) showed that Assumption 3 is not necessary to prove that $\delta_{DD}$ identifies the local average treatment effect of relaxing fiscal restraints for municipalities below the cutoff. However, without Assumption 3, $\delta_{DD}$ cannot be extended to municipalities without the confounding treatment at the cutoff.

In Section 5.3 we report tests conducted 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 for 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 would react in similar ways, independently of 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, have returned a nil effect if Assumption 3 held. We could not run this test because our dependent variable could not be computed before 1999: before that year, in fact, information on budget forecasts is not available. Given that Grembi et al. (2016) did not find any evidence against Assumption 3 in terms of revenues and expenditures, it is likely that it also holds when referred to revenue and expenditure forecast errors. Finally, although our identification strategy was based on local randomness in the neighbourhood of the population cutoff, one might wonder whether omitted variable biases could still challenge the causal interpretation of our estimates. This might be the case if, for example, transfers to municipalities from higher levels of governments – which are linked, among others, to cyclical conditions, to changes induced by the adherence to the SGP, and to modifications in the budget biases of regional and central governments – varied with different intensities right below and right above the cutoff, leading to the failure of Assumption 1. However, to the best of our knowledge, there was no institutional feature which could have led to such a differential change in transfers from higher level governments right below and right above the cutoff. This is empirically confirmed by one of the results in Grembi et al. (2016). They indeed found no jump at the 5,000 inhabitants cutoff in transfers from higher level governments.[26]

## 4.4 Estimation

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

[26] See panel B of Table 4 in Grembi et al. (2016).

$$
\begin{aligned}
y_{it} = {} & \alpha_0 + \alpha_1 x_{it} + \alpha_2 x_{it}^2 + \mathbb{1}(x_{it} \geq 0) \cdot (\gamma_0 + \gamma_1 x_{it} + \gamma_2 x_{it}^2) \\
& + \mathbb{1}(t \geq 2001) \cdot \left[\beta_0 + \beta_1 x_{it} + \beta_2 x_{it}^2 + \mathbb{1}(x_{it} \geq 0) \cdot (\delta_{DD} + \delta_1 x_{it} + \delta_2 x_{it}^2)\right] \\
& + \quad u_{it}, \qquad \text{with} \quad t = 1999, \ldots, 2004, \quad \text{and} \quad x_{it} \in [-h, +h],
\end{aligned} \tag{5}
$$

where $u_{it}$ is the error term and $h$ is the bandwidth restricting observations near the cutoff. We chose the bandwidth following the mean-squared error optimal criterion in Calonico et al. (2014).[27] We fitted the model in Equation (5) using weighted least squares, using the triangular kernel function to weight observations. As pointed out by Cattaneo et al. (2020), 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 gave more importance to observations that were closer to the cutoff. More in detail, the triangular kernel function was maximized (and equal to 1) at the cutoff, it was zero for municipalities with $x_{it} \notin [-h, +h]$, and it decreased linearly and symmetrically when the assignment variable moved away from the cutoff. In drawing inferences, we clustered standard errors at the municipal level.

# 5 Results

## 5.1 Baseline effects

Table 4 reports the estimation results of the baseline model in Equation (5). 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 revenue (expenditure) forecast errors larger 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

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

Table 4: 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 parentheses and are robust to heteroskedasticity and within-municipality correlation. The optimal bandwidth was 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.

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 2001 from the after period. It confirms that, after spending ceilings, severe penalties for noncompliance 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 was large and significantly different from zero at the usual 5% level. Relatively to the 1999-2000 average, the increase in the revenue (expenditure) forecast error was about 26% (22%).

In a nutshell, the main findings from the baseline estimates reported in Table 4 are:

1. the municipalities not fiscally restrained have less accurate revenue and expenditure projections, due to a larger overestimation of the planned revenues and expenditures;
2. the results limited to 1999-2001 data suggest that the removal of fiscal restraints for municipalities with fewer than 5,000 inhabitants and the introduction of incentives for compliers are not 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 the corresponding forecast error for each of them. We distinguished the revenues among taxes, fees and tariffs, and a residual category. We divided the expenditures among current outlays, capital outlays, and a residual category. Table 5 reports the estimated impact on forecast error for each of these components of revenues and expenditures.

Panel a) of Table 5 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, facilitating more accurate tax revenue forecasts.

Table 5: 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 4.

Panel b) of Table 5 shows 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 may reflect greater uncertainty in the timing and costs of carrying out medium-long term public investments.

## 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 therefore affect 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 local politicians from respect for the fiscal restraints could be different across the Italian regions. Second, we consider the geographical extension, since 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 6 displays summary statistics of the revenue and expenditure forecast errors across the heterogeneity dimensions under investigation. Information on the composition of the population comes from the 1991 census. The revenue and expenditure forecast error is lower in the North, in small municipalities, when the fraction of immigrants is larger and that of youth is smaller.

Table 7 reports the estimation results after splitting the sample according to the heterogeneity dimensions reported in Table 6. Columns (1)-(4) show the effect of heterogeneity across geographical areas. They strongly suggest that the effects 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 revenue and expenditure 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 give them more room for manoeuvre in overestimating budgetary projections, especially when fiscal constraints are relaxed.[28]

[28]The reason commonly adduced to explain exuberance in budget forecasts is the over-optimism of the official predicting the economic growth rate (Strauch et al., 2004; Frankel, 2011).

Table 6: The budget forecast error across different municipal characteristics (1999-2004)

| | Forecast error in Revenues | | Forecast error in 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 South includes municipalities in Abruzzo, Basilicata, Calabria, Campania, Molise, and Puglia.

# Table 7: 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 4.

Columns (5) and (6) of Table 7 focus on the effect heterogeneity by geographical extensions. Casaburi and Troiano (2016), in studying the electoral responses to the introduction of an Italian policy to combat the evasion of property taxes, found that tax evasion is higher in geographically larger municipalities. They speculated 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 7 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 7 report the effect heterogeneity according to different demographic structures of the residents. We find that in municipalities with a younger population, the effect is more marked in terms of both 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 acts as a disciplinary device, magnifying the costs of not complying with the DSP. There is indeed evidence that young voters dislike public debt accumulation, which involves higher taxes within their lifetimes and a crowding-out in the provision of the public goods (Song et al., 2012). For similar reasons, people with tertiary education may be able to assess the future costs associated with poor quality management of public finances. Empirical evidence suggests that the fraction of immigrants is larger in the North of Italy (Mocetti and Porello, 2010). Foreigners, in fact, typically move to geographical areas offering more job opportunities. This means that a high share of foreign people is positively correlated with more favorable economic conditions and better economic prospects, 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 prospects.

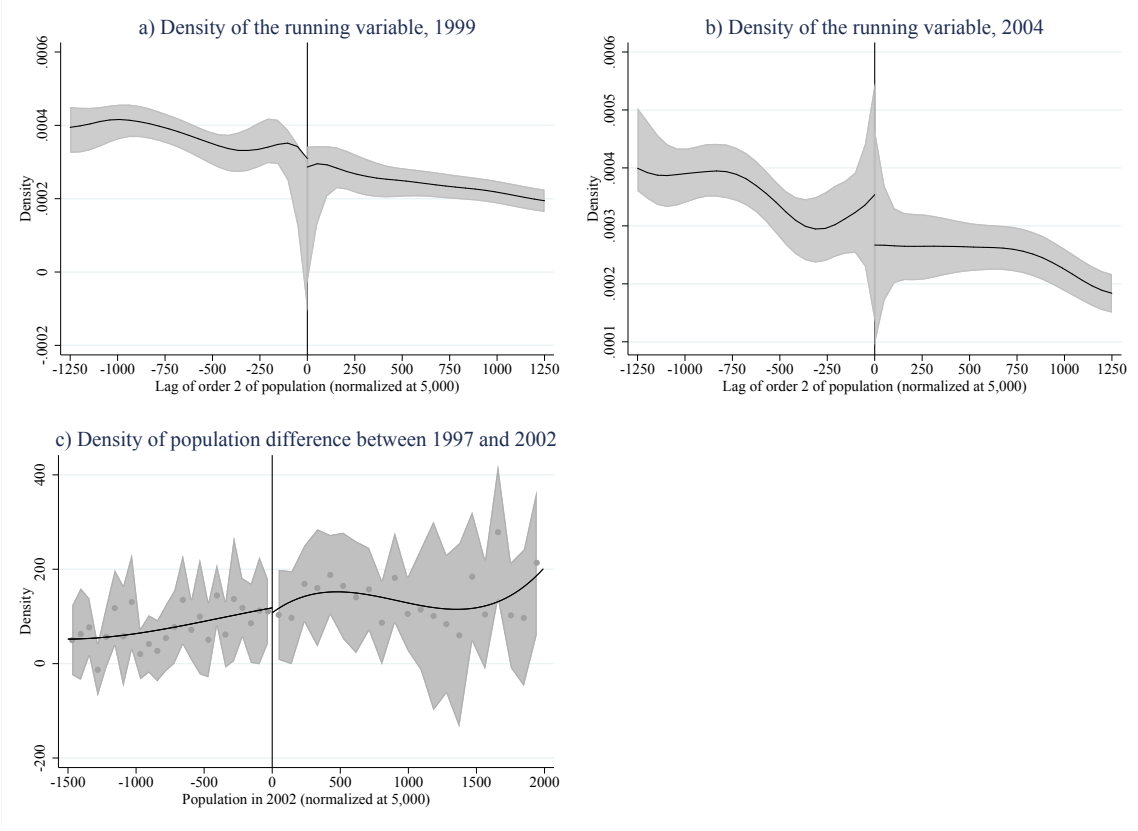
## 5.3 Validity and falsification tests

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 may happen if the municipalities close to the cutoff manipulate the official population records to avoid the fiscal rules. The fiscal rules were changed by national financial law 388/2000 enacted in December 2000. The financial law relaxed the fiscal rules for municipalities with fewer than 5,000 inhabitants, as measured two years earlier. The municipalities eligible in 2002 for 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 implemented in 2002 a set of interventions to affect the population size so as to fall below the cutoff, for example by not counteracting 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, either in 1999 or 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 visualize graphically 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.[29]

Under the assumption that there is no change over time in the pattern of manipulative sorting around the cutoff, the treatment should not have an effect on the pre-treatment covariates (Grembi et al., 2016). We followed Lee and Lemieux (2010) and tested if the differences in the discontinuities were significantly different from zero. We did so by estimating a seemingly unrelated regression (SUR) with one equation for each of the pre-

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

Figure 3: Graphical density test of the running variable




*Graphs a) and b)*: The solid lines are 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.

determined variables. Each equation was 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 8 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 many covariates, the joint test suggests that the only significant effect may 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 municipalities below and above the cutoff, but they are instead due to omitted variables inducing correlation between population size and the outcome variable, therefore failing the local randomness assumption. As in permutation inference tests (Abadie et al., 2010) and Grembi et al. (2016), we performed a set of placebo diff-in-disc regressions for revenue and expenditure forecast errors by setting the population cutoff at false thresholds. More in detail, we ran 399 diff-in-disc estimates by setting the cutoff from 4,801 to 4,999 and from 5,001 to 5,200. This created a distribution of 399 placebo effects and enabled 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 with 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 are 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 was conducted to determine whether the results were sensitive to the local polynomial order and to the bandwidth choice. Table 9 reports the diff-in-disc estimates if we modified the local polynomial order and, instead of using a data-driven optimal bandwidth selector (Calonico et al., 2014), we alternatively and arbitrarily fixed the bandwidth at 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 9 shows that when we increased the bandwidth but fixed the polynomial order, we gained in precision, but the strict parametric restrictions on the relation between the forcing variable and the

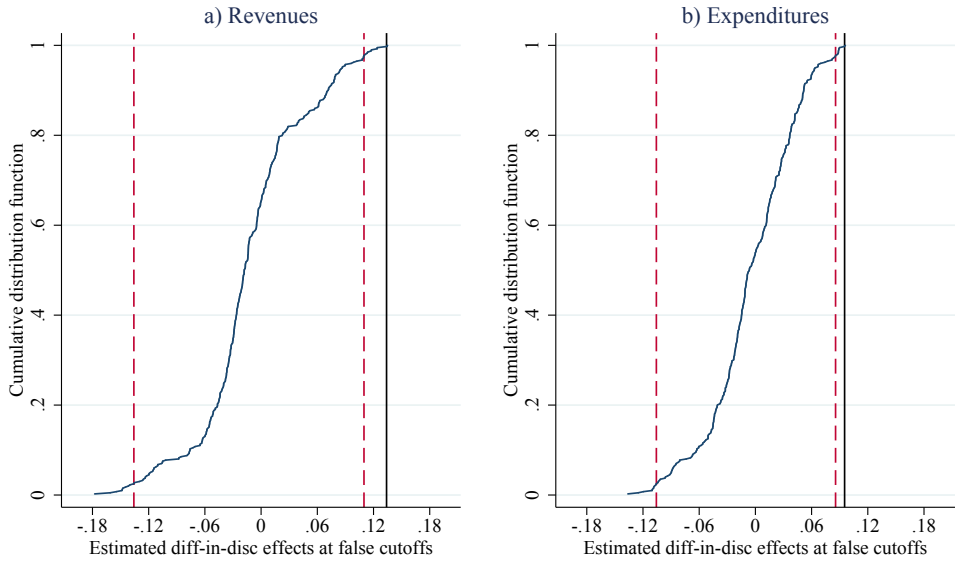
Table 8: Falsification test: treatment effect (difference in discontinuities) on predetermined variables estimated by SUR[(a)]

| | | 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 followed Lee and Lemieux (2010) and tested if the differences in the discontinuities were significantly different from zero by estimating a SUR with one equation for each of the predetermined variables. Each equation was estimated by means of 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 are 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 were 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.

outcome variable biased the estimated effect on both revenue and expenditure forecast error towards zero. Grembi et al. (2016) found a similar bias towards zero of the effect on the fiscal gap and deficit when enlarging the bandwidth.

Finally, we report in Table 10 the estimation results if we included in Equation (5) municipality and time fixed-effects, so as to control for all the municipal predetermined heterogeneity and to capture the common shocks at national level. 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 impact 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 did not reject the null hypothesis that the two estimates were equal to each other.[30]

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

Table 9: 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 parenthesies and are robust to heteroskedasticity and within-municipality correlation. We used the triangular kernel to weight observations from the cutoff.

Table 10: 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 parenthesies and are robust to heteroskedasticity and within-municipality correlation. The optimal bandwidth is the same as used for the estimation reported in Table 4. We used the rectangular kernel to weight observations from the cutoff.

# 6 Conclusions

How effective budget rules in correcting distortionary fiscal outcomes is still an open question in the literature. Indeed, they may fail to achieve the main objective, because they may 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 with fewer than 5,000 inhabitants, the tightening of budgetary constraints, and the introduction of severe sanctions on noncompliers in 2002, we estimated the effect of budget rules on the accuracy of budget forecasts. We found that the DSP was effective in reducing budgetary forecast errors in Italian municipalities. In particular, our results highlight that municipalities affected by the budget rules had more accurate revenue and expenditure projections, especially in regard to fees and tariffs and capital outlays. This is due to the smaller overestimation of the planned revenues and expenditures. Considering the 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 larger budget forecast errors 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 educations.

The DSP was implemented using 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 constant changes in its objectives, criteria, and sanctions have created many uncertainties in its

application and doubts about its usefulness among Italian mayors. Our empirical analysis shows that the pact, with 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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