The Unintended Composition Effect of the Subnational Government Fiscal Rules: The Case of Italian Municipalities

Fiorenza Venturini∗

Abstract

Although numerical fiscal rules may be introduced to achieve several objectives, to date the maintenance of fiscal sustainability is their predominant goal. This is particularly true at subnational level; maintaining fiscal discipline in a decentralized setting is challenging and subnational government fiscal rules are considered one of the most valid solutions to the problem. While theoretical and empirical literature has mainly focused on their effectiveness in containing subnational deficit and/or debt, little attention has been paid to the possible trade-offs and side effects of the rules on the composition of subnational expenditure. The aim of this paper is to fill this gap by exploiting the case of Italian municipalities, which have been subject for fifteen years (1999–2015) to a set of rules called Domestic Stability Pact. The Italian DSP framework – imposing rules only on municipalities above a population threshold (5,000 inhabitants) – allows us to implement a quasi-experimental technique to investigate the unintended composition effects of the rules. A difference-in-discontinuities design permits to find rigorous empirical evidence that the switching in 2007 to rules which are more binding in terms of fiscal discipline leads to a recomposition of municipal expenditure against investment spending. The analysis is then integrated by evaluating the impact of the rules on six categories of investment expenditure. Investment in human capital and infrastructure seems to be the most affected.

(Keywords: fiscal rules, local expenditure, local spending composition, investment spending; JEL E62, H72, H77, R53)

1 Introduction

Most of the recent empirical literature on Subnational Government numerical fiscal Rules (SGR) has focused on which typologies, strictness and coverage features of the rules are the most suitable to constrain the subnational deficit and/or debt. It aims at verifying whether the rules are truly effective in pursuing this objective.

∗Sapienza University of Rome — fiorenza.venturini@uniroma1.it. I would like to thank Massimo Bordignon, Antonio Di Majo, Paolo Liberati, Maria Grazia Pazienza, Alessandro Santoro, Michele Trimarchi, Alberto Zanardi and the participants of the XXIX Siep Conference (Catania, September 2017) for precious comments and suggestions. Special thanks are due to Francesca Gastaldi and Simone Tedeschi. The usual disclaimers apply.
Here we propose to investigate whether those rules that are supposed to have a stronger positive (intended) effect on fiscal sustainability may also have (indirect and unintended) effects on the expenditure composition of the SubNational Governments (SNG).

The issue is relevant, especially as the trends towards fiscal decentralization have been strengthening over time in many countries; from the 1990s onwards they have actually intensified in many EU Member States, even if almost exclusively on the expenditure side. OECD (2017) provides data on spending decentralization in EU countries updated to 2015. It is worth pointing out that decentralization has involved not only constitutionally federal states (Austria, Germany) but also unitary countries (Italy, Nordic Countries) as well as those unitary and traditionally centralized (France). In 2015, the subnational government spending to total general government expenditure ratio is above 60% in Denmark and 30–40% in Austria, Belgium, Finland, Germany, Italy and Spain. Breaking down subnational expenditure according to the ECOG reveals that investment accounts for more than 50% in most countries, with peaks close to or above 70% in Belgium and Germany (Allain-Dupré, 2017). As far as expenditure functions are concerned, according to the European Commission (2012) environment and housing are typically assigned to the SNG – about 60–80% of the general government expenditure in these areas is carried out at the subnational level – followed by education, culture and recreational services (more than 40%).

Although the SNG have been undoubtedly gaining importance in public finance, the assignment of functions to subcentral authorities is not unproblematic. Several conflicts may arise, which have their roots in the different suitability of each level of government to perform the three key functions – allocation, redistribution and stabilization (Musgrave, 1959) – of the public sector. The highlighted decentralization trends are explained by the importance the public sector attaches to the optimal resources allocation (Pedone, 2008). The allocative function is the activity against which the SNG seem to have the main advantage over the central government (Tiebout, 1956; Oates, 1972) without conflicting with its objectives, as is likely to happen with respect to the other two functions. The very fact that the SNG mainly deal with environment, education, culture and recreational services is consistent with the traditional theory of fiscal federalism (European Commission, 2012). The spending items included in these functions concern services which are more efficiently organized on a fairly small scale, as the heterogeneity of preferences is likely to be more pronounced. This focus on service differentiation according to the local needs has been one of the driving forces of the decentralization trends.¹

Fiscal federalism, however, brings about two problems. On the one hand, it usually leads to lower uniformity in the provided local public goods/services; economic disparities and differences in taxing capacity among the SNG often result in an unbalanced provision. On the other hand, a risk for opportunistic behaviours

¹It is worth noting that, while EU–level decentralization trends are usually explained (e.g. European Commission, 2012) by the traditional theory of fiscal federalism, the aspects pointed out by the second–generation theories of fiscal federalism may have also had relevance in the decentralization process (Boetti et al., 2012).
does emerge, as the single entities are usually not able to assess the impact of their decisions at the general government level. This second point is crucial; it is at the root of the introduction of SGR mainly aiming at fiscal discipline. Inducing subcentral authorities to share the responsibility of fiscal sustainability and fiscal discipline with the central government has indeed become overriding. To a certain extent, it represents a strong push towards (re)centralization. At the EU level and especially after the last financial crisis, the SNG have been actively involved in public finance consolidation. In Italy, for example, the municipal sector has made a significant contribution, although its debt only represents 2% of the general government debt between 2010 and 2015 (IFEL, 2016).

The fact that the SNG play a key role in public spending, however, would require special attention to the setting of the rules. This is a non–secondary aspect. The OECD (Sutherland et al., 2005; Fredriksen, 2013) has indeed recently built (and updated) a summary indicator of the quality of various countries’ SGR. One of the criteria for the index construction is the effect of the rules on spending, that is their suitability to support spending allocative efficiency. Actually, allocative efficiency has the same weight of fiscal sustainability among the relevant criteria to evaluate the overall quality of the SGR. A comparison across countries of the SGR currently in force clearly reveals that these objectives might be at odds. For instance, according to Fredriksen (2013) Danish SGR score high as regards sustainability, while performing poorly as for allocative efficiency. The opposite happens at the Belgian state level. The OECD indicator is precious in this regard. The conflict that emerges between the different objectives of the rules points to the basic conflict in the allocation of functions to the SNG.

However, obtaining a proper balancing of the various demands is not an easy task. On the one hand, greater autonomy may lead to a shift of costs to the general government, potentially resulting in sustainability issues. On the other hand, an excessive emphasis on fiscal consolidation may lead to distortions in spending, by reducing "the supply of infrastructure and public services in the economically most vital part of the country without reducing, or perhaps even accentuating, the shortcomings of the most disadvantaged parts" (Pedone, 2008, p. 210). In other words, a conflict of priorities worthy of deeper consideration does appear.

Our aim here is to show that when the SNG are subject to binding fiscal rules such a conflict does emerge. We thus intend to fill the gap in literature related to the trade–offs and side–effects of the SGR. Our case study are the Italian municipalities, which constitute an attractive setting for our purposes. They have in fact always played a key role in public expenditure – as similarly happens in other EU countries – while at the same time being subject for several years to imposed fiscal rules primarily aiming at fiscal discipline; a Domestic Stability Pact is indeed in force between 1999 and 2015.

The way such rules are devised gives rise to an ideal setting for a quasi–experimental design. The DSP has indeed envisaged for several years a threshold (5,000 inhabitants) such that only the municipalities above have actually been subject to the rules. Unfortunately, the presence of a confounding policy at the same threshold – the higher wage of the mayor (and of the members of the municipal executive body) in the municipalities above 5,000 inhabitants – prevents
us from adopting a Regression Discontinuity Design (RDD). Nevertheless, we can still remove the confounding policy by exploiting the fact that we have years before and after the introduction of the rules. In other words, we can exploit both the discontinuity at the threshold and the time variation (before/after estimation) by using the difference–in–discontinuities estimator implemented by Grembi et al. (2016).

We mainly focus on the effects of the rules on composition of subnational expenditure classified according to the ECOG. In particular, we find rigorous empirical evidence that switching to rules which are stricter in terms of fiscal discipline leads to a recomposition of municipal expenditure against investment spending – an issue that has always been mentioned in literature without being systematically investigated. In fact, according to our preferred specification, while per capita total expenditure – accrual basis of accounting – experiences a decline between 7 and 11%, per capita investment spending is between 32 and 38% lower. Clearly, a recomposition of the expenditure arises. It similarly happens with regard to the cash basis of accounting. Per capita total expenditure experiences a 6–8% decline, while per capita investment spending is 24–25% lower.

The previous analysis can be usefully integrated by evaluating the impact of the rules on investment spending categorized according to a COFOG–like classification. As we shall see, the sharpest reductions regard per capita investment spending in human capital, infrastructure and pure public goods – respectively -43/-31%, -50/-33% and -40/-32% as for accrual basis of accounting.

In the final part we perform a series of checks to test the robustness of our results. In particular, we test the two fundamental assumptions behind the implementation of the diff–in–disc design. As we shall see, one of the hypotheses can be directly tested thanks to the specific Italian framework; as regards the other, a series of indirect tests still permit an evaluation of its plausibility.

The remainder of the paper is structured as follows. Sections 2 briefly reviews the relevant literature. Section 3 defines the institutional framework behind our analysis. Sections 4 and 5 present the identification strategy and the dataset. Sections 6 and 7 present and discuss the estimation results and related robustness checks. Section 8 deals with some possible concerns and section 9 concludes.

2 A brief overview of the relevant literature

The empirical analyses on the unintended effects of numerical fiscal rules on subnational spending composition are rather scant. In order to understand the reason for this fact, a brief overview of the use of the SGR may prove very insightful. We first note that, according to the literature (Sutherland et al., 2005), SGR are effective tools for achieving three key objectives: ensuring public finance sustainability, limiting the size of the public sector and promoting allocative efficiency. However, fiscal sustainability has over time gained an almost exclusive significance. In fact, maintaining fiscal discipline in a decentralized setting is challenging; the SNG tend to overspend, undertax and overborrow, thus threatening the macroeconomic stability of the general government. The common pool problem is at the
root of this behaviour (Pisauro, 2001), causing a strong need for control mechanisms. Among the possible solutions, fiscal rules are regarded as a particularly suitable tool (Ter-Minassian and Craig, 1997).

The need to hinder subnational fiscal indiscipline seems therefore to be the main reason behind the adoption of SGR, which are in fact widespread in the majority of OECD countries (Fredriksen, 2013) – especially in the form of Balanced Budget Rules (BBRs) and borrowing constraints.

Among the countries adopting SGR, the EU Member States are of special interest. The common pool problem is indeed particularly strong at the EU level: the increasing European integration has over time required ever greater budgetary coordination mechanisms between the countries, resulting in the adoption of supranational fiscal rules. The need to respect these constraints (e.g., the Stability and Growth Pact) has encouraged the adoption of fiscal rules at all government levels in various Member States. In fact, although in each State – regardless of the decentralization degree – the sole central government is responsible for meeting the EU requirements, those rules bind the general government as a whole. As a result, the more are the functions carried out locally, the greater is the contribution of the subcentral authorities in ensuring compliance with the constraints. Yet, each entity has an incentive not to comply with the rules while at the same time exploiting the benefits arising from the disciplined behaviour of the others. The EU penalties for Member States that do not respect the supranational rules, however, apply to the sole central government, so that the latter might be forced to compensate for the opportunistic behaviour of non–cooperating SNG (Balassone and Franco, 1999). As a result, many EU countries (e.g., Austria, France, Germany, Spain) have adopted sets of SGR (usually called Domestic Stability Pact – DSP) since the early 1990s.\footnote{These DSPs have similar features to the Italian pact (Ambrosanio and Bordignon, 2007; Gastaldi and Giuriato, 2009); this strengthens the relevance of our analysis within the EU context.}

In response to the almost exclusive focus on the fiscal sustainability objective, recent empirical contributions aim to assess the effects of these rules on subnational budget outcomes – deficit and/or debt (e.g. Foremny, 2011; Argimón and Hernández de Cos, 2012; Grembi et al., 2016; Galli and Grembi, 2013; Luechinger and Schaltegger, 2013; von Hagen and Foremny, 2013). The effectiveness in hindering SNG\textsuperscript{'} fiscal indiscipline is confirmed by virtually all the empirical analyses.

Conversely, little attention has been paid to the trade–offs and side–effects of the SGR on subnational spending choices, especially on spending composition. The issue has not been completely neglected: a number of recent papers (see Borge et al., 2001; Vallés and Zárate, 2007; Chiades and Mengotto, 2013; Galli and Grembi, 2013; Monacelli et al., 2014), all related to EU countries, warn against the potential distorting effects of the SGR. This literature is, however, less systematic. It raises challenging questions, by highlighting the risk that SGR properly designed in order to ensure a disciplined fiscal behaviour on behalf of the SNG may induce a reduction in the most flexible local spending items (typically, investment); it seems, however, to call for a deeper analysis rather than arriving at widely accepted results. Our paper fits in this context.
3 The Italian institutional framework

The Italian municipalities represent the third level, after regions and provinces, of the Italian SNG. They have always played a prominent role in national expenditure, especially in carrying out investment spending (Chiades and Mengotto, 2013). In particular, the municipalities have always had a key role with regard to all those infrastructural and social expenditures that guarantee the welfare of the community.

Consequently, the central government deemed it unavoidable to constrain their fiscal policy through the so-called *Patto di Stabilità Interno*, which is in fact a DSP. The Italian DSP was introduced by the 1999 annual budget law (*l. n. 448/1998*) and remained in force until 2015. It imposed specific rules on each subnational government level and the municipal level was particularly affected. It is worth noting that the Italian DSP changed over time. That is, the typology as well as the strictness and coverage features of the rule were repeatedly changed between 1999 and 2015.

We do not dwell here on all the changes. Rather, we try as far as possible to identify the key step that marked the passage from *less to more effective* rules in ensuring fiscal sustainability. In order to evaluate the effectiveness on fiscal sustainability we refer to the literature on fiscal rules. We account for both the typology of the rule and its strictness and coverage features (see European Commission, 2006; IMF, 2009). The BBRs are very effective in ensuring fiscal sustainability, especially if they prescribe a balanced budget or a surplus. It indeed targets precisely the aggregate of interest (unlike, e.g., the expenditure ceilings). Moreover, the wider the coverage of the expenditure items is, the greater the effectiveness of the rule. Finally, more severe supervision and penalties together with a double constraint on both the budget plan and the final balance are certainly aspects contributing to the effectiveness of the rule.

Initially, the Italian DSP binds all the municipalities (see Table 1), while from 2001 to 2011 only the municipalities above 5,000 inhabitants are subject to the rules. The sole exception is 2005, when the DSP is extended to all the municipalities above 3,000 inhabitants. From 2012 onwards all the municipalities above 1,000 inhabitants are required to respect the DSP.

From its introduction to 2004 the DSP rule is basically a BBR. In 2005 an expenditure ceiling on overall expenditure substitutes the BBR. In 2006 two distinct ceilings for current and capital expenditure replace the overall cap. The BBR is reintroduced from 2007 onwards.

Initially the DSP rules are not particularly binding, especially between 2001 and 2004. In fact, up to 2004 the BBR merely limits the growth rate of the fiscal gap (defined as deficit net of transferred-from-above resources on the one hand and debt service on the other) and only current expenditure is accounted for in the target. Moreover, several expenditure items (i.e., exceptional expenditure, expenses related to the exercise of delegated functions) are expressly excluded from the target. Even though the specifications of the rule change from year to

\[\text{In this respect see Ambrosanio and Bordignon (2007) and Gastaldi and Giuriato (2009).}\]
year, there is some consistency in limiting the growth of the fiscal gap. With the introduction of the ceilings, final expenditure – i.e. both current and capital spending, initially together then separately – becomes the object of the rules. This extension, however, which is supposed to reinforce the effectiveness of the rules, is once again offset by the exclusion of numerous items from the target.\footnote{Furthermore, the expenditure ceilings have no direct effects on fiscal sustainability – as is the case for the BBRs – for they do not directly target the balance.}

A significant tightening of the rules takes place in 2007. The BBR is reintroduced, the constraint on final expenditure is confirmed and virtually no item is excluded from the target. Furthermore, each municipality is expected to reach a specific, individual target balance calculated according to an automatic (and rather contorted) mechanism.\footnote{It refers to the average expenditure carried out in the last three years. Appropriate coefficients are applied to this aggregate in order to identify the required budget balance.} As a result, it is not uncommon that the individual contribution is a budget surplus. Finally, not only the final balance but also the budget plan must respect the constraints. The rule is similar in the subsequent years.\footnote{We do not study here the extension of the DSP, starting from 2012, to all the municipalities above 1,000 inhabitants. In our opinion the results would be less interesting due to the very small size of the involved authorities. Furthermore, from 2011 onwards, the regulatory framework of reference for the municipalities is much more fragmented (see footnote 17).}

The exclusion and reinclusion of some municipalities in the DSP rules according to the number of inhabitants seemingly allows us to implement a sharp regression discontinuity design (RDD) with a population threshold at 5,000 inhabitants. However, this quasi–experimental technique cannot be used here, as a second policy sharply changes at the same threshold. This leads to a biased RDD estimator. A slightly different methodological approach is therefore to be followed.

### 4 Identification strategy

#### 4.1 Population–threshold RDD with more than one policy change

It is not infrequent, when it seems appropriate to use a population–threshold RDD, that the threshold determining whether the policy of interest is applied or not also determines whether other policies are applied. In other words, the policy change object of study may occur alongside one or more confounding treatments. In this case it is no longer possible to assume with confidence that the RDD estimator identifies the effect of one specific policy rather than another.\footnote{Several empirical analyses have recently addressed the problem (e.g. Campa, 2011; Asatryan et al., 2016). The research designs exploiting population thresholds are appealing (Eggers et al., 2016), but may present pitfalls complicating their use. It is interesting to note that the papers trying to address these issues – albeit with different research aims – study the municipal context in EU countries (Spain and Germany in the cases above). This is a proof of the complexity and relevance of the municipal dimension in the EU countries.}

This is exactly what happens in our setting, where at the relevant threshold – 5,000 inhabitants – deciding the inclusion/exclusion of the municipalities in/from the DSP, a second policy sharply changes. The salary paid to the mayor of the
Table 1: The Italian Domestic Stability Pact

<table>
<thead>
<tr>
<th>Year</th>
<th>Subj. Municipalities</th>
<th>Rule</th>
<th>Year</th>
<th>Subj. Municipalities</th>
<th>Rule</th>
</tr>
</thead>
<tbody>
<tr>
<td>1999</td>
<td>All municipalities</td>
<td>BBR (cash basis)</td>
<td>2007</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cash and accrual basis)</td>
</tr>
<tr>
<td>2000</td>
<td>All municipalities</td>
<td>BBR (cash basis)</td>
<td>2008</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2001</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cash basis)</td>
<td>2009</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2002</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cash basis)</td>
<td>2010</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2003</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cash and accrual basis)</td>
<td>2011</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2004</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cash and accrual basis)</td>
<td>2012</td>
<td>&gt; 1,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2005</td>
<td>&gt; 3,000 inhabitants</td>
<td>expenditure ceiling on overall spending on two distinct expenditure ceilings on current and capital spending</td>
<td>2013</td>
<td>&gt; 1,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
<tr>
<td>2006</td>
<td>&gt; 5,000 inhabitants</td>
<td>expenditure ceiling on overall spending on two distinct expenditure ceilings on current and capital spending</td>
<td>2007</td>
<td>&gt; 5,000 inhabitants</td>
<td>BBR (cm)</td>
</tr>
</tbody>
</table>

Notes: Up to 2002 the BBR is applied to the cash–basis–of–accounting target. From 2003 to 2007, both cash and accrual basis are subject to the rule. In 2008 the criterion of competenza mista (mixed competence – cm) is introduced, according to which current expenditure is taken under accrual basis of accounting and capital expenditure under cash basis of accounting.

Source: Own elaboration on Ministry of Economic and Finance’s circulars.

municipality (and to the members of the giunta, which is the municipal executive body) increases at that threshold. As noted by Grembi et al. (2016) precisely in relation to the Italian institutional context, this confounding policy leads to a biased RDD estimator. Its effect must therefore be removed in order to correctly identify the effect of the relevant policy. This result can be obtained by exploiting not only the discontinuity at the threshold but also the time variation before/after the introduction of the relevant policy. In other words, it must be used a technique combining the RDD with the difference–in–differences – a difference–in–discontinuities technique.

We recall the basic features and assumptions behind this approach following Grembi et al. (2016). As our setting is rather complex, in this section we identify the policy change of interest with the imposition of fiscal rules on the municipalities above 5,000 inhabitants from the year \( T_0 \) onwards (as we shall see in section 4.2 this is a simplification of what happens in our context). There are two

8In Italy, these are not the sole policies that change according to the number of inhabitants of the municipality. The population is used as a benchmark also for the number of the council and the giunta members, and the electoral rule. However, only the salary of the mayor (and that of the members of the giunta) change at the DSP threshold. The electoral rule, for example, switches from single round to runoff at 15,000 inhabitants.
relevant policies:

1. \( DSP_{it} \) equal to one in the municipalities above the threshold \( (P > P_c, \text{ where } P_c = 5,000) \) and from year \( T_0 \) onwards, and zero otherwise;

2. \( W_{it} \) equal to one in the municipalities above the threshold, where the mayor’s wage is higher, and zero otherwise.

The potential outcome \( S_{it} \) has a more complex expression compared to the case of a single policy, as all the possible combinations of the two policies are taken into account.

\[
S_{it} = W_{it}DSP_{it}S_{(W_1DSP_1),it} + W_{it}(1 - DSP_{it})S_{(W_1DSP_0),it}
+ (1 - W_{it})DSP_{it}S_{(W_0DSP_1),it} + (1 - W_{it})(1 - DSP_{it})S_{(W_0DSP_0),it}.
\]  

We then define the average potential outcomes for the municipalities at the threshold when they implement the two policies, in any of the four possible combinations, by taking the limit either from above or from below:

\[
S^+ \equiv \lim_{p \to P_c^+} E[S_{it}|P_{it} = p, t \geq T_0]
\]  

\[
S^- \equiv \lim_{p \to P_c^-} E[S_{it}|P_{it} = p, t \geq T_0].
\]

Let us suppose now that all the assumptions of the regression discontinuity design hold, so that \( \hat{\tau}_{rdd} = S^+ - S^- \) identifies the average treatment effect (ATE) of imposing the rules. As in this case the rules are not the sole policy changing at the threshold, however, this estimator is biased. In fact, even if the expected values of all the potential outcomes are continuous functions of the forcing variable at the threshold, when we add and subtract \( S_{W_1DSP_0} \) in the expression for \( \hat{\tau}_{rdd} \) we have:

\[
\hat{\tau}_{rdd} = S^+ - S^- = S^+_{(W_1DSP_1),it} - S^-_{(W_0DSP_0),it}
+ E[S_{(W_1DSP_1),t} - S_{(W_1DSP_0),t}|P_{it} = P_c, t \geq T_0]
+ E[S_{(W_1DSP_0),t} - S_{(W_0DSP_0),t}|P_{it} = P_c, t \geq T_0].
\]  

The first term of the last equality is the ATE of the relevant policy (\( DSP_1 \) with respect to \( DSP_0 \)) in the municipalities with \( W = W_1 \) (i.e., where the mayor receives higher salary). The second term represents the bias (the ATE of increasing the mayor’s wage in the towns where the rules are not binding).

However, we have additional information allowing us to remove the bias. We must recall that our treatment is equal to one when \( P > P_c \) and besides \( t \geq T_0 \). That is, in order for a municipality to be treated it is not sufficient that its population exceeds a certain threshold, but it is also necessary to be in a year
in which the rules are actually in force. If we want to remove the bias we need to exploit a moment where the other policy ($W_{it}$ in our case) occurs in isolation. This condition is crucial, as it allows us to subtract the effect of the confounding policy from the effect of both policies. Before the treatment we have:

$$S_{pre}^+ = \lim_{p \to P_c} E[S_{it}|P_{it} = p, t < T_0]$$ \hspace{1cm} (5)$$

$$S_{pre}^- = \lim_{p \to P_c} E[S_{it}|P_{it} = p, t < T_0].$$ \hspace{1cm} (6)$$

By exploiting the difference before/after the relevant policy we have:

$$\hat{\tau}_{didisc} \equiv (S^+ - S^-) - (S_{pre}^+ - S_{pre}^-),$$ \hspace{1cm} (7)$$

that, under specific assumptions, identifies the effect of interest.

The idea of combining the RDD with the difference–in–differences approach has been repeatedly proposed in literature (e.g. Campa, 2011). However, Grembi et al. (2016) formally specify the fundamental hypotheses behind the estimation of the effect through this technique:

1. continuity of all the potential outcomes;
2. local parallel trends assumption;
3. independence of the effect of the relevant policy from the confounding policy.

The first one is the basic assumption of the RDD. It allows us to assimilate the design to a local randomized experiment (the independence assumption applies locally). The second one requires that the effect of $W_{it} = 1$ with respect to $W_{it} = 0$, holding $DSP_{it}$ fixed, is always the same, at any time it is introduced (that is, when it is introduced either alone or in combination with the other policy). In our case we need to be sure that the effect of the higher salary at the threshold is constant over time. If it holds, we can estimate the discontinuity before $T_0$ and then subtract it from the observed discontinuity after. It is analogous to the parallel trends assumption in the difference–in–differences, but local in nature, as it must hold only in a neighbourhood of the threshold. From equation 7 we have:

$$\hat{\tau}_{didisc} \equiv S_{(W1DSP1)}^+ - S_{(W0DSP0)}^- - S_{pre(1)}^+ + S_{pre(0)}^-.$$ \hspace{1cm} (8)$$

If the assumption of local parallel trends does hold, the second and fourth terms of equation 8 are equal and the estimator identifies $E[S_{(W1DSP1),t} - S_{(W0DSP0),t}|P_{it} = P_{c}]$. Finally, the last assumption (independence between the effects of the two policies) implies that it is totally irrelevant that the $DSP_{it}$ switches from zero to one either when $W_{it} = 1$ or when $W_{it} = 0$. As a result, we can identify the effect not only immediately above, but in a neighborhood of the threshold. Provided that these assumptions hold we can estimate the following equation:
\[ S_{it} = \beta_0 + \beta_1 \bar{P}_{it} + \beta_2 D_t + \beta_3 D_t \bar{P}_{it} + \beta_4 T_t + \beta_5 T_t \bar{P}_{it} + \beta_6 T_t D_t \bar{P}_{it} + \epsilon_i, \] (9)

where \( S_{it} \) is our outcome variable, \( \bar{P}_{it} \) is the forcing variable rescaled so that the threshold is at zero, \( D_t \) is the population–related dummy and \( T_t \) is the time–related dummy (introduction of the relevant policy). The coefficient representing our estimator is \( \beta_6 \); we account for the discontinuity both at the threshold and at the – we might say – time threshold.

4.2 Rules and thresholds changing over time

As seen in section 3, the Italian DSP–related legislative framework has been subject to several changes over time. The lack of consistency, albeit slightly confusing, allows us to investigate a variety of situations.

Step (1) is the passage from 2000 to 2001, when the rules are relaxed for municipalities below 5,000 inhabitants. Actually, we would be interested in studying the effect of the introduction of those rules, which as we discussed in section 3 are not particularly binding. Therefore, similarly to Grembi et al. (2016), we assume that our conclusions are symmetrically extensible. In (1) the municipalities with \( P_{it} < 5,000 \) are the treated units, the remaining are untreated.

The second step – (2) – can be usefully divided into four passages. In 2005, simultaneously, an expenditure ceiling replaces the BBR and the threshold is shifted from 5,000 to 3,000 inhabitants. That is, on the one hand municipalities that until then had been subjected to a BBR switch to an expenditure ceiling; on the other hand, towns that until then had been excluded from the DSP see the introduction of an expenditure ceiling. In 2006, however, two separate expenditure ceilings replace the cap on overall spending and the threshold is moved back to 5,000. That is, municipalities above 5,000 switches from one to the other typology, while towns between 3,000 and 5,000 inhabitants see the introduction of an expenditure ceiling. The municipalities below 3,000 inhabitants are never subject to the rules.

Before proceeding with our identification strategy, we point out that in (2) we expect a limited impact of the rules on the composition of municipal expenditure. The first reason is quite intuitive; it is unlikely that a rule in force for a single year has a remarkable effect. This is particularly true in our case, as we analyze the composition of expenditure. It is implausible that a single year of an overall expenditure cap is able to induce the municipalities between 3,000 and 5,000 inhabitants to alter the mix investment/current expenditure. The second reason has its roots in the assessment, on the grounds of the relevant literature, of the features of the rules; as seen in section 3 the expenditure ceilings introduced in 2005 and 2006 cannot be considered as significantly binding.

Following Gregori (2014), we proceed step by step and check what happens to our dependent variables in each step. The analysis is based each time on the result of the preceding step. This allows us to formally verify what stated above. In particular:
• in (2a), passage from 2004 to 2005, the treated units are the municipalities with \( 3,000 < P_{it} \leq 5,000 \), the untreated units those with \( P_{it} \leq 3,000 \). The treatment is the introduction of the rules (expenditure ceiling);

• in (2b), from 2005 to 2006, the treated units are the municipalities with \( 3,000 < P_{it} \leq 5,000 \), the untreated units those with \( P_{it} \leq 3,000 \). The treatment is the exclusion from the DSP;

• in (2c), from 2004 to 2005, the treated units are the municipalities with \( P_{it} > 5,000 \), the untreated units those with \( 3,000 < P_{it} \leq 5,000 \). The treatment is the switch from the BBR to the expenditure ceiling;

• In (2d), from 2005 to 2006, the treated units are the municipalities with \( P_{it} > 5,000 \), the untreated units those with \( 3,000 < P_{it} \leq 5,000 \). The treatment is the introduction of two separate ceilings instead of the unique cap.

In other words, both between 2004 and 2005 and between 2005 and 2006 there are two simultaneous treatments; one on the municipalities between 3,000 and 5,000 and the other on those above 5,000 inhabitants. Clearly, this may lead to biased results. Hence, we first need to verify that there are no effects in (2a) and (2b). In this way, we freeze the treatment on the towns between 3,000 and 5,000. Then, strengthened by these results, we investigate the effect of the treatment on the municipalities above 5,000 inhabitants. As we shall see, we have no effects for all outcomes of interest in all four cases (2).

The last step — (3) — sees the replacement, in 2007, of the expenditure ceiling with a new and more stringent BBR. We start from the analysis conducted in (2) and extend it to a period forward to study what happens to the municipalities above 5,000 inhabitants. As a counterfactual we can use all the remaining observations, since none of the previous treatments has statistically significant effects.

\section{Data}

We use municipal–level data based on the certificates of the balance sheet account (\textit{Certificati di Conto Consuntivo} – CCC) that each Italian municipality annually transmits to the Ministry of the Interior. Our full dataset includes observations related to 5,388 municipalities over a period of sixteen years (1999–2013).

We consider, with few exceptions, the sole units for which we have observations over the entire period. Furthermore, the municipalities of the five regions enjoying a special statute are excluded. On the one hand, three of these regions (Friuli Venezia Giulia, Trentino Alto Adige and Valle d’Aosta) have had the possibility to negotiate measures different from those of the DSP with the central government.

\footnote{Data are updated to 2016. Note that the CCC must be carefully reviewed before being exploited, as data are directly provided by each municipality and tend to be subject to inaccuracies. Actually, as a further check, we verify that in the aggregate they are consistent with those provided by the ISTAT in the context of the public administration statistics.}
since 2003. On the other hand, the other two regions (Sicilia and Sardegna), which still continued to apply the DSP, have features and privileges that suggest excluding their respective municipalities. We also exclude the città metropolitane, which are urban and densely populated areas with additional functions with respect to a municipality.

Among the towns thus selected we use those between 1,500 and 8,500 inhabitants (see section 6). We altogether have, depending on the year, between 2,551 and 2,477 cross-section observations. This sample accounts for more than 30% of all Italian municipalities and for 17–18% of the Italian population.

Our variables of interest are municipal spending aggregates in per capita terms and in 2010 euros. First, we classify them into current and capital expenditure, according to the ECOG. Actually, adhering to the literature (e.g. Turrini, 2004), we distinguish between capital expenditure in a broader sense and investment spending and we focus on the latter. Capital spending includes in fact financial items which cannot be considered investment. Actually, the two aggregates follow entirely comparable patterns, but the distinction is still relevant. We define investment spending of the Italian municipalities as capital expenditure net of financial items (Chiades and Mengotto, 2013).

In particular, our outcome variables are log per capita total and investment expenditure (the former being the sum of current and investment spending). Conclusions on the effect of the rules on expenditure recomposition are drawn by comparing the two aggregates. We separately examine accrual and cash basis of accounting. As a second step, we attempt to study the effects of the rules on expenditure categorized according to a COFOG–like classification. This analysis is very interesting though more complex. First, a greater segmentation of expenditure is unavoidable. It is therefore necessary to identify the appropriate aggregation criteria of the spending items. Second, it should never be neglected that the CCC are directly provided by the municipalities; the distinction of expenditure according to the COFOG might be ambiguous and the municipal officials might be misled. In fact, mistakes and missing data are numerous. Our outcome variables are once again log per capita spending aggregates.

Accrual–basis data are the most reliable and are thus our main focus. However, considering both the accrual and cash basis of accounting is unavoidable. First, they usually present discrepancies (reasons are numerous: the features of the national accounting system, the inability of the administrators to translate spending programs into actual works, external constraints). Second, the rules are likely to have different effects on the two aggregates. For example, a rule binding the budget plan forces to pay greater attention to the expenditure planning, thus it is likely to significantly affect the accrual–basis side.

Although a qualitative analysis of all the municipalities’ budget data goes beyond our purposes, it is useful to highlight some key points. As regards accrual basis of accounting, we note that while current expenditure remains fairly stable between 1999 and 2013, there is a dramatic collapse in capital spending (see Figure A.1a). The latter increases up to 2003–2004 and then starts to diminish in both absolute and per capita terms. Such a reduction is paralleled by that of investment spending (per capita investment drops from 460 to 157 euros between 2004 and 2013). As a result, we observe a significant recomposition of municipal expenses: while in 2003–2004 capital expenditure accounts for more than a half of current expenditure, in 2013 the ratio shrinks to one fifth. Let us now compare the superior municipalities (above 5,000 inhabitants
Our dataset also includes data on the municipal population, provided by the Italian Statistical Office – ISTAT. The same data have been the benchmark for the application of the DSP throughout the years. The ISTAT also provides information related to territorial features (area size in km², height above sea level, geography of the area) of each municipality.

6 Estimation results

We present here the estimates of the effect of the rules on current and investment expenditure and then on spending by functional categories. As far as the latter is concerned, as just highlighted, it seems promising to combine the ECOG and the COFOG and study the impact of the rules on capital – or rather investment – expenditure by functions. See Table 2 for summary statistics.

6.1 Current and investment expenditure

We recall that we exploit the municipalities between 1,500 and 8,500 inhabitants. This choice is driven by the need to remain around the 5,000-inhabitant threshold, while at the same time having a sufficiently large number of observations. Basically, the problem is that the municipalities above 5,000 inhabitants are far less numerous than those below. Moreover, there are more observations close to the threshold when we approach it from below. Clearly, the most populous municipalities – considerably fewer – also include medium-to-large-sized cities and metropolitan areas. These are far away from the threshold and cannot be usefully exploited for our purposes. The window we indicate not only fits the case of the Italian municipalities (see Chiades and Mengotto, 2013) but – which is crucial – it also seems appropriate regardless of the peculiarities of our context (see Casas-Arce and Saiz, 2015). In any case we shall see below that the results are robust to changes in this window.12

We apply a local linear regression. The choice of the linear case is generally suggested by literature (Imbens and Lemieux, 2007; Gelman and Imbens, 2014). The use of high-order polynomials is not recommended as inference is poor (the obtained confidence intervals may be misleading) and the results are sensitive to the order chosen.

– always subject to the DSP) with the inferior ones (below 5,000 inhabitants). The trend of total expenditure is virtually the same for both groups; it rises until 2003–2004 and then starts decreasing, with a slight recovery in 2013. The drop in capital spending between 2004 and 2013 is however more dramatic in the superior municipalities (-69%), and is paralleled by an analogous reduction in investment. The comparison in per capita terms is particularly indicative: in the superior municipalities per capita investment decreases significantly in virtually all territorial macro-areas (North-west: Piemonte, Lombardia, Liguria; North-east: Veneto, Emilia Romagna; Centre: Toscana, Umbria, Marche, Lazio; South: Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria). Conversely, in the inferior municipalities the reduction is moderate; between 2006 and 2010 we even notice slight increases, especially in the North-east and South (compare Figures A.1c and A.1e). Similar considerations apply as regards cash basis of accounting.

12Note that when we narrow the window from below the number of the municipalities on the two sides of the threshold is more balanced.
To compute the estimation bandwidth we use the algorithm introduced by Calonico et al. (2014b) and the cross-validation procedure (hereafter CV) proposed by Ludwig and Miller (2007). The cross-validation procedure, together with the plug-in procedure (Imbens and Kalyanaraman, 2012), is very popular. However, as the plug-in procedure, it presents some limitations.\(^{13}\) Calonico et al. (2014b) thus propose an alternative approach, not dissimilar from the plug-in, but with key changes.\(^{14}\) They aim to solve a fundamental problem of the traditional methods, which in their opinion generate bandwidth choices leading to a "non-negligible bias in the distributional approximation of the estimator" (Calonico et al., 2014b, p. 2296) and consequently to a bias in the associated confidence intervals. This can induce an over-rejection of the null hypothesis. In other words, the traditional approaches to bandwidth selection makes it more likely for the treatment’s coefficient to be significant.

Therefore, we deem it convenient to present the results obtained through both the CV and the method developed by Calonico et al. (2014b) – hereafter CCT.\(^{15}\)

Table 3 presents the estimation results for case (1). The relaxation of the rules for the municipalities below 5,000 inhabitants has no effect on either investment expenditure or total expenditure (accrual and cash basis of accounting). No remarkable significance arises, meaning that the exclusion from the rules does

\(^{13}\)We recall that both methods are based on the concept of mean square error (MSE), which measures the trade-off between precision and bias (as is known, the wider the bandwidth the more precise the estimates, though a larger bandwidth implies a larger potential for bias). The CV identifies the bandwidth that minimizes the cross-validation criterion and produces the smallest MSE. The plug-in procedure uses a formula providing a closed form analytic solution for the bandwidth that minimizes a particular function of bias and precision.

\(^{14}\)See Calonico et al. (2014b) and Calonico et al. (2014a).

\(^{15}\)As the CCT is a sort of plug-in rule of order two (Calonico et al., 2014a) it seems more interesting to compare it with the cross-validation rather than the plug-in procedure.
Table 3: Effects of (1)

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp.</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.039</td>
<td>-0.024</td>
<td>-0.129</td>
<td>0.008</td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.102)</td>
<td>(0.040)</td>
<td>(0.083)</td>
<td>(0.032)</td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>-0.045</td>
<td>0.019</td>
<td>0.001</td>
<td>0.033</td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.060)</td>
<td>(0.023)</td>
<td>(0.051)</td>
<td>(0.017)*</td>
<td></td>
</tr>
</tbody>
</table>

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting.

not seem to induce any recomposition of the expenditure in the unconstrained towns. Actually, this is not exactly the effect we intend to estimate. We would rather verify what happens to the introduction of loose rules. However, it is reasonable to extend the obtained result, by assuming it is symmetrically valid. As we suggested, rules that are not particularly binding do not have side-effects on composition and in fact the estimates are not significant with either of the bandwidth–selection methods. Graphical analysis (Figure 1) confirms our findings.

Table 4 presents the effects for the four cases (2). Once again, nothing relevant happens to the outcome variables. The few significant results do not raise particular concern. First, significance is never confirmed by both the bandwidth–selection methods. Second, here our framework requires us to use only one pre and one post–treatment year: estimates may thus be sensitive – especially with the CV – to very strong but occasional variations in spending from one year to the other in some municipalities – actually, the results seem decidedly driven by the fluctuations in investment spending in few small towns (e.g., Rapolla). Nonetheless, in order to remove any doubt, we check what happens when dropping the outliers as well as using the log differences (rates of growth) of the various spending aggregates as outcome variables; as expected, significance disappears. Therefore, the results seem to confirm our hypothesis. The expenditure ceilings are not particularly binding and above all change from year to year. The lack of both stringency and consistency makes it unlikely that the municipalities change their behaviour so as to determine a recomposition of their expenditure.

Table 5 presents the estimation results for the most interesting case. Here the estimates are obtained with two different time–bandwidths. In our opinion, a challenging aspect of the difference–in–discontinuities design is the very choice of the time frame to be used for the estimates. In case (1) we follow Grembi et al. (2016); we use 1999–2000 as years before and 2001–2004 as years after the treatment. With regard to case (2), we use only one pre–treatment and one post–treatment year (Gregori, 2014), due to the variability of the framework. In case

16This means to demonstrate symmetrically the absence of an effect, which seems quite plausible.
Table 4: Effects of (2)

<table>
<thead>
<tr>
<th>Case (2a)</th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. (cb)</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>0.237</td>
<td>0.132</td>
<td>0.011</td>
<td>-0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.150)</td>
<td>(0.053)**</td>
<td>(0.098)</td>
<td>(0.032)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>0.094</td>
<td>0.044</td>
<td>0.094</td>
<td>0.030</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.100)</td>
<td>(0.036)</td>
<td>(0.060)</td>
<td>(0.020)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Case (2b)</th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. (cb)</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>0.093</td>
<td>0.029</td>
<td>0.140</td>
<td>0.029</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.151)</td>
<td>(0.054)</td>
<td>(0.095)</td>
<td>(0.027)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>-0.038</td>
<td>-0.021</td>
<td>0.014</td>
<td>0.001</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.098)</td>
<td>(0.034)</td>
<td>(0.057)</td>
<td>(0.018)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Case (2c)</th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. (cb)</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.047</td>
<td>0.009</td>
<td>-1.00</td>
<td>-0.021</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.282)</td>
<td>(0.093)</td>
<td>(0.172)</td>
<td>(0.048)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>-0.074</td>
<td>-0.002</td>
<td>0.057</td>
<td>0.038</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.137)</td>
<td>(0.049)</td>
<td>(0.076)</td>
<td>(0.026)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Case (2d)</th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. (cb)</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>0.285</td>
<td>0.024</td>
<td>0.077</td>
<td>0.013</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.279)</td>
<td>(0.100)</td>
<td>(0.165)</td>
<td>(0.061)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>0.351</td>
<td>0.068</td>
<td>0.146</td>
<td>0.021</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.137)**</td>
<td>(0.048)</td>
<td>(0.077)**</td>
<td>(0.027)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting.
Figure 1: Difference–in–discontinuities (1)

Notes: Horizontal axis: normalized population. Each triangle represents a bin of 150 inhabitants. Spline cubic fit and 95 percent confidence intervals are included.

(3) we opt for two different solutions. First we consider 2006 as pre and 2007–2011 as post–treatment years (time–bandwidth 1).\footnote{The choice is driven by the fact that in 2012 there is a relevant DSP change – the threshold being shifted to 1,000 inhabitants – and that since then the legislative framework of reference for the municipalities is much more fragmented, due to the numerous laws adopted after the 2011 budgetary crisis. It should not be neglected, however, that already in 2011 part of the Fondo Sperimentale di Riequilibrio – a fund with horizontal equalization purposes – is allocated using the same threshold at 5,000 inhabitants (\textit{D.M. 21 Giugno 2011 n. 178}). Moreover, the \textit{d.l. n. 78/2010} cuts part of the 2011 central government transfers to the municipalities above 5,000. Transfer cuts may actually impact on spending (Marattin et al., 2017). Anyway, the results (available upon request) excluding 2011 are virtually equal to the baseline, both in the size and in the significance of the coefficients. The robustness of the estimates and the fact that the year concerned is at the end of the period seems to confirm that the 2006–2011 time–bandwidth captures the effect of the sole relevant policy.}

Nevertheless, the strong asymmetry of the 2006–2011 window induces us to check what happens also with a \textit{centred} time–bandwidth. We consider two years before and after \(T_0\), (time–bandwidth 2). This centred case should exclude the possibility that an unbalanced time frame alters the results. As we can see, the results are robust.\footnote{As a further check the estimates can be repeated considering 2005 and 2006 as pre and 2007, 2008, 2009, 2010 and 2011 as post–treatment years. The magnitude of the coefficients is virtually the same and the statistical significance is high. For example, as regards Investment (cb) we have \(\beta_6 = -0.197 (0.091)^*\) with the CCT and \(\beta_6 = -0.221 (0.051)^{***}\) with the CV, while as regards Total Expenditure (cb) \(\beta_6 = -0.065 (0.026)^*\) with the CCT and \(\beta_6 = -0.061 (0.024)^{**}\) with the CV. Moreover, we subject results to an additional test. In order to ensure that they are
Table 5: Effects of (3)

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp.</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Time–bw 1</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CCT</td>
<td>-0.476</td>
<td>-0.112</td>
<td>-0.283</td>
<td>-0.079</td>
<td>(Calonico et al., 2014b)</td>
</tr>
<tr>
<td>CV</td>
<td>-0.383</td>
<td>-0.068</td>
<td>-0.278</td>
<td>-0.067</td>
<td>(Ludwig and Miller, 2007)</td>
</tr>
<tr>
<td></td>
<td>(0.177)***</td>
<td>(0.047)***</td>
<td>(0.094)***</td>
<td>(0.028)***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.080)***</td>
<td>(0.035)*</td>
<td>(0.054)***</td>
<td>(0.026)**</td>
<td></td>
</tr>
<tr>
<td>Time–bw 2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>CCT</td>
<td>-0.296</td>
<td>-0.079</td>
<td>-0.115</td>
<td>-0.049</td>
<td>(Calonico et al., 2014b)</td>
</tr>
<tr>
<td>CV</td>
<td>-0.305</td>
<td>-0.069</td>
<td>-0.149</td>
<td>-0.036</td>
<td>(Ludwig and Miller, 2007)</td>
</tr>
<tr>
<td></td>
<td>(0.115)**</td>
<td>(0.036)**</td>
<td>(0.092)</td>
<td>(0.026)*</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.066)***</td>
<td>(0.023)***</td>
<td>(0.049)***</td>
<td>(0.018)*</td>
<td></td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; time–bw 1 = 2006–2011, time–bw 2 = centred.

Switching to a – as we discussed – more stringent rule has a statistically and economically significant effect on expenditure composition. Let us take as a benchmark the estimates achieved by the CCT and time–bandwidth 1 and let us observe first what happens to the accrual basis of accounting. As a result of the treatment, the municipalities above the threshold experience about 11% decline in per capita total expenditure (current plus investment spending).\(^{19}\) On the other hand, per capita investment spending is about 38% lower. Although we cannot conclude on current spending, we observe a clear expenditure recomposition against investment. As regards the cash basis side, we observe about 8% decline in total expenditure, while investment is 25% lower. We can draw similar conclusions. The results can be appreciated also graphically; Figure 2 clearly shows the discontinuity at the threshold.

The magnitude of the coefficients is virtually the same when using different methods to compute the bandwidth around the threshold. According to Grembi et al. (2016), smaller bandwidths remove better the bias and thus estimate the magnitude of the coefficients more correctly, albeit with less precision. Hence, the magnitude of the effect seems to be better identified through the CCT. In any case, the core findings do not change. Conversely, different time–bandwidths lead to slightly different results; nevertheless, the estimates are still very consistent.

These results are interesting. First, the sharp reduction in per capita investment is significant in itself, given the central role of the Italian municipalities within the public administration in carrying out this type of expenditure. In fact, not driven by the introduction of the *competenza mista* in 2008 (see Table 1), we estimate the effect using 2006 as pre and the sole 2007 as post–treatment year. The coefficients’ magnitude is similar (results available upon request).

\(^{19}\)For a precise coefficients’ interpretation of dummy variables in log models see Giles (2011).
the rules end up negatively affecting the public sector’s contribution to capital accumulation and aggregate demand.

Second, they are entirely consistent with the concerns raised in the literature as regards the potential distortive effects of fiscal rules on expenditure composition. This issue is already pointed out in relation to the fiscal rules imposed on the centre (Poterba, 1995; Dahan and Strawczynski, 2013), but it is particularly relevant as regards the subcentral levels of government (Sutherland et al., 2005). In particular, according to Sutherland et al. (2005) in the presence of constraints – that are, we add, adequately binding to ensure fiscal discipline – a recomposition at the expense of investment might emerge. The distortion generates a bias against those expenditure items that are flexible, typically easier to change in the short–term (Oxley and Martin, 1991; Lane, 2002). In the presence of the rules, decisions on the spending priorities tend to be taken in the light of short–term political considerations. This is especially true at local level, where it is usually more difficult to raise adequate funds, as the decisions on the revenue side are frequently taken centrally.

This means that allocative efficiency, which usually is the main incentive towards decentralization, may be compromised. Furthermore, we said that fiscal federalism is a source of tension between several objectives. One of these is the principle of uniformity, which implies the need for an equalizing activity. A recomposition of expenditure against investment spending particularly damages the less economically developed areas, which on the contrary would need a boost towards
development.

We can now refer to OECD (2017) and reconsider the score of the synthetic index assigned to the Italian SGR (at the provincial and municipal level). A careful consideration of the high score obtained – 7.9 out of 10 – in the light of the above analysis, cautions us about the possibility that the distortions induced by the DSP on municipal spending have not been adequately accounted for.

6.2 Capital spending by functions

In this second part we investigate what happens to expenditure functions. Once the negative impact of the rules on capital spending is established, it is in fact interesting to examine how the rules affect the various capital spending functions (Poterba, 1995). Actually, greater consistency with our analysis requires us to study the effect of the rules on investment expenditure by functions.\(^{20}\) Therefore, we decompose investment spending into six functional categories:\(^{21}\)

- **Human capital**: Education (nursery schools, primary and secondary education, related services), culture (libraries, museums, galleries), recreational services;
- **Social protection**: Childcare centres, services for the children, residential structures for the elderly, services to the person, housing;
- **Infrastructure**: Transports and viability, city planning and development policies
- **Pure public goods**: Administration, justice, local police;
- **Productive activities support**: Tourism, economic development (e.g., support to agriculture, crafts, industry and commerce, organization of markets and fairs);
- **Productive services**: Network services (electric energy, gas supply, water supply, waste disposal).

This analysis adds information on the reduction in investment spending and in this sense it complements the previous one. Net of some sporadic significance with one or the other bandwidth–selection method, no effects arise in cases (1) and (2).\(^{22}\) Conversely, in case (3) relevant effects do emerge (Table 6). As in the

\(^{20}\)This requires to use the CCC to a highly–detailed level that might not provide guarantees. For this reason, it is better to rely on accrual–basis data (see note 10). Moreover, we compare our results – exploiting investment data – with those using capital spending data (available upon request): as they are entirely consistent, we can present the former with confidence.

\(^{21}\)These categories are based on the twelve functions – linked to the COFOG – distinguished within the Italian municipal budget, and are defined following the literature that study the effects of decentralization on subnational spending composition (e.g. Kappeler and Valli, 2008; Grisorio and Prota, 2015).

\(^{22}\)We find only marginal significances and/or estimated coefficients reassuringly non–consistent between the two bandwidth–selection methods (results available upon request).
### Table 6: Effects of (3) – functions (ab)

<table>
<thead>
<tr>
<th>Time–bw 1</th>
<th>Human Capital</th>
<th>Infrastructure</th>
<th>Pure Public Goods</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT (Calonico et al., 2014b)</td>
<td>-0.569</td>
<td>-0.699</td>
<td>-0.514</td>
</tr>
<tr>
<td>CV (Ludwig and Miller, 2007)</td>
<td>-0.374</td>
<td>-0.395</td>
<td>-0.390</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Time–bw 2</th>
<th>Human Capital</th>
<th>Infrastructure</th>
<th>Pure Public Goods</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT (Calonico et al., 2014b)</td>
<td>-0.527</td>
<td>-0.639</td>
<td>-0.512</td>
</tr>
<tr>
<td>CV (Ludwig and Miller, 2007)</td>
<td>-0.364</td>
<td>-0.341</td>
<td>-0.350</td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting; time–bw 1 = 2006–2011, time–bw 2 = centred.

Previous analysis, the coefficients’ magnitude tends to differ according to whether the CCT or the CV is used; as mentioned above, the former should be preferred. Investment in human capital and infrastructure is deeply affected. With the CCT and the time–bandwidth 1 as a reference, we observe a 43% reduction for the former and almost 50% for the latter; pure public goods diminish by about 40% (see Figure 3). On the other hand, investment in welfare, productive activities support and productive services, which also represent non–negligible shares of municipal investment (Figure A.2), is not affected.  

6.3 Composition, rules and political choices

The composition effects of the SGR has sometimes been linked with the literature studying the determinants of subnational expenditure, and in particular that related to the political budget cycle. A branch of this literature suggests that the cycle tends to manifest itself on the composition rather than on the spending level. We propose here to re–examine these considerations.

According to Rogoff (1990), in proximity of the elections the incumbent politician is induced to signal his competence to voters by biasing pre–election fiscal policy towards more visible expenditures. That is, the information asym-

---

23 Although, as mentioned, cash–basis data are not particularly reliable at this level of detail, it is still interesting to repeat the estimates by exploiting them. While cash–basis investment in human capital faces reductions similar to the accrual–basis case, the decline in investment commitments for pure public goods seems not to be paralleled by a reduction on the cash–basis side. Finally, the effect on cash–basis infrastructure is dubious (we shall try to explain these results in section 6.3).

24 This is a signaling–competence model, see Rogoff and Sibert (1988); Rogoff (1990).
Figure 3: Difference-in-discontinuities (3) – functions (ab)

Notes: Horizontal axis: normalized population. Each triangle represents a bin of 150 inhabitants. Spline cubic fit and 95 percent confidence intervals are included.
metry on the incumbent’s level of competence and the features of the production function (investment undertaken in the election period only becomes visible after the elections, whereas current spending is immediately observable) induces the incumbent to bias expenditure to his advantage. In other words, the visibility becomes the driving force of resource allocation.

In Drazen and Eslava (2010) politicians have different preferences as regards different types of expenditure, but these preferences are not observed by voters. If the incumbent succeeds in shifting spending towards the items mostly preferred by voters, it may induce them to believe that he has exactly their preferences. The incumbent has then a strong incentive to manipulate the composition towards those items which are targetable in favour of certain groups. Again, the composition is affected. According to Drazen and Eslava (2010), the most targetable expenditure aggregate is investment spending. However, as argued by Vergne (2009), some current expenditure items are also highly targetable.

Against this background, the idea that the political cycle may occur on the composition rather than on the levels is clearer. The mechanism the literature proposes is the following: in proximity of the elections the incumbent reduces the most visible part of taxation, while at the same time restricting the budget impact of the fall in revenues by decreasing public investment (Veiga and Veiga, 2007; Katsimi and Sarantides, 2012). The literature (see Drazen and Eslava, 2010) also highlights that this is more likely to happen in developed countries, where the voters are supposed to be well–informed and deficit–adverse. It is worth noting that a further conclusion is drawn from these considerations. According to Katsimi and Sarantides (2012), the presence of a composition cycle implies that the existing balanced budget rules or expenditure ceilings cannot hinder cycle–induced distortions, as they are not capable of eliminating the composition cycle.

Actually, this claim opens a different perspective. We deem that it is not voters’ aversion to deficit spending to induce a composition rather than a level cycle (which the rules in turn are unable to curb). Otherwise, we argue, it would be hard to justify the very existence of numerical fiscal rules. We recall that the literature widely agrees in justifying the rules with their ability to contain the deficit bias (Kydland and Prescott, 1977).25 Rather, we could affirm that the very presence of external constraints – such as the rules – may induce the cycle to manifest itself on the composition rather than on the level of expenditure.

This literature sheds light on the above results. It is sensible to argue that the rules lead municipal administrations to behave differently with respect to spending priorities. As said above and outlined by the European Commission (2003) and IFEL (2010), it is quite plausible that in the presence of constraints the less visible spending – namely, investment spending – is sacrificed for current spending. Inasmuch less visible, it is more flexible.26

25In fact, the rules seem to be quite effective in imposing a constraint on the discretionary margin of the incumbents and in limiting pre–electoral overspending at the municipal level (see Cioffi et al. (2012) for the Italian municipal context).

26In order to plausibly draw these conclusions we deem it advisable to replicate the estimates excluding from current expenditure its most rigid component, namely personnel expenditure. Table A.5 presents similar results.
A similar argument is useful for the less immediate interpretation of what happens to the expenditure functions. More–long–term and less–direct–in–character investment tends to be the most adversely affected spending items (Grisorio and Prota, 2015). The reduction in investment on human capital and infrastructure found above may be due precisely to their limited visibility or, similarly, to the fact that these items ultimately have long–run effects. In particular, human capital as defined above is the less (and less immediately) visible component. Its sharp decline finds then a potential explanation – see the comparable results of Poterba (1995).

The other investment components have different characteristics. The lack of effects on social protection stems probably from the effort the municipalities made to preserve the more markedly redistributive component of expenditure, especially at a time of economic crisis and thus of difficulties for households. Actually, these needs are typically satisfied through current expenditure. Nevertheless current spending inevitably drags capital expenditure (e.g., the renovation and extraordinary maintenance of the structures necessary for service delivery). On the other hand, the lack of effects on productive activities support and productive services can be seen in the light of the literature on competition between SNG (Keen and Marchand, 1997). In particular, Grisorio and Prota (2015) investigate the effects of competition on various types of subnational capital expenditure. It emerges that the SNG are less prone to contract spending in those sectors where the horizontal competition is sharper. Investment directly benefiting local businesses, for instance, tends to be less affected.\(^\text{27}\)

### 7 Robustness checks

Our estimates survive a series of robustness checks.\(^\text{28}\) First, we check what happens when we consider alternative population windows. As already said, we exploit the municipalities between 1,500 and 8,500 inhabitants. However, using alternative windows – 8,500–2,000 / 8,000–2,000 / 8,500–2,500 / 8,500–3,000 / 8,000–3,000 inhabitants – returns consistent values for the coefficients.

Second, we want to be sure that our results are not obtained by chance, but are due to an actual causal relationship. Thus, we verify what happens to the outcome variables at false population thresholds. We refer to our preferred specification, i.e. that using both the CCT and CV and the 2006–2011 as time–bandwidth. If no discontinuity arises at such thresholds, we are allowed to say with greater certainty that our results reflect a truly causal relationship. In particular, we take mock discontinuities at 3,500, 4,000, 4,500, 5,500 and 6,000 inhabitants. The

\(^{27}\)It is also true, though, that spending on certain items such as roads, bridges or underpasses is more visible (and more targetable) locally rather than centrally (Veiga and Veiga, 2007). In this sense, as discussed by Katsimi and Sarantides (2012), the categories of visibility and targetability need to be adapted to the local context. This might explain the less clear-cut impact on infrastructure on the cash–basis side; the municipalities may have tried to limit the related spending cuts.

\(^{28}\)See the appendix for related Tables and Figures.
estimates are mostly non–significant for all variables.\footnote{In some cases significance arises with one or the other bandwidth–selection method (especially with the CV, as we could expect in the light of what is noted in section 6), but the coefficients’ magnitude (and sometimes the very direction of the effect) is non–consistent. Overall the results seem to confirm our analysis.}

A crucial point is the test of the fundamental assumptions of the diff–in–disc design, as identified by Grembi et al. (2016) and reported in section 4. Assumption 2. cannot be tested directly, but we can still assess its plausibility by verifying the absence of any (change of) manipulation of the forcing variable over time. More specifically, we must exclude changes in manipulative sorting before/after the treatment. First, following (Grembi et al., 2016), we check that there is no discontinuity at the threshold in the difference between the density of the reference population (2005 population) for the application of the rules in 2007 and the density of the pre–treatment population. We use 1999 as pre–treatment population.\footnote{It is a sort of McCrary (2008) test for the manipulation of the running variable.} Figure A.3 seems to confirm the absence of discontinuity at the threshold. Actually, manipulation of the forcing variable is unlikely to occur in this context. On the one hand, the reference population is calculated according to ISTAT data (municipalities are not directly in charge of providing such information). On the other hand, assuming the local administrators – e.g. the mayor – are able to influence the choice to or not to locate in the municipality, if the municipal population falls below the threshold with consequent exclusion from the rules, they will suffer a wage reduction (our confounding policy).

As a further check we add time–invariant covariates and year fixed effects to our preferred specification. We return to the partition we made in note 11 and we add four macro–area dummies (North–west, North–east, Centre and South). In addition, we control for the surface of the municipality in \( km^2 \), the altitude level in metres and the geographical features of the territory (i.e., presence of mountains, proximity to the sea). It is comforting to see that the results are virtually the same, as should happen in the absence of manipulation (Tables A.1 and A.2).

Provided that assumption 2. holds, the particular context of the Italian DSP allows us to \textit{directly} test assumption 3. We recall that this hypothesis excludes interactions between the relevant and the confounding policy. In other words, it implies that the mayors react in the same way to the rules regardless of the salary they receive. The introduction of the DSP for all the municipalities in 1999 permits a direct test of this assumption. We can apply the diff–in–disc design with the threshold at 5,000 using (at least) 1998 and 1999 as years, respectively, before and after the treatment and make sure that the estimator returns a zero effect (see Tables A.3 and A.4).

We also verify whether the results are homogeneous throughout the territory. As already mentioned, in Italy significant structural differences between geographical macro–areas (North–west, North–east, Centre, South) have always existed. Thus, it seems advisable to check for heterogeneous treatment effects (Becker et al., 2013) across the municipalities due to the belonging to one or the other macro–area. As can be noted in Table A.6, no remarkable differences in the treatment effect emerge. In other words, the effect is independent from the
geographical location of the municipality.

We finally replicate the estimates, this time excluding the municipalities with compulsory administration due to Mafia infiltration in any year of the considered period. It is reasonable to assume that these municipalities are more likely to be exposed to corruption phenomena, especially as regards certain spending categories (e.g., infrastructure). Due to the association between the efficiency of infrastructure provision and the level of corruption (e.g., Finocchiaro Castro et al., 2014) our results might be biased because of the presence of these towns. Table A.7 and Table A.8 present, however, very similar results.\footnote{These last two checks are particularly relevant in the light of what will emerge in the following section. As we shall see, obtaining consistent results between territories with different situations in terms of efficiency and waste (as well as when the most at-risk observations are excluded) is quite reassuring.}

8 Some further considerations

Our analysis highlights that the composition of municipal expenditure is unquestionably affected by the rules. However, the discussion cannot be confined to what happens to the sole municipal budget data. Our results should be evaluated in the light of two additional issues: the presence of off–balance sheet items and the relationship between municipal expenditure and the actual provision of public goods/services.

The first issue is the possibility that the municipalities exploit accounting tricks to circumvent the rules (so–called fiscal gimmickry). The literature on SGR has always been aware of this risk (Sutherland et al., 2005). In EU countries, for example, the attempts at evading the constraints have recently made use of the Public–Private Partnership (PPP). In fact, the PPP activities’ accounting criteria have not been strictly regulated by the EU. As a result, the PPP makes it possible to register off balance numerous investment items, thus becoming very attractive to local (but also central) governments.

It is undeniable the the Italian DSP rules represent an incentive for the subcentral authorities to externalize public services. The externalization allows indeed the municipalities to spread over time the impact of certain investment projects on the rules–relevant targets (Bellesia, 2013). In other words, outsourcing public services offers an accounting advantage, as it permits a transformation of present higher expenses (e.g., debt–financed infrastructure) into future higher expenses (services purchasing from the private that built the infrastructure) or lower income (chargeable services, sold to the private sector, no longer result in revenues for the public sector).

Italian public services externalization has experienced a remarkable expansion at the subnational level since the 2000s (EPAS, 2014). It has involved network services (transport and energy, gas and water networks), social and environmental services; although it may take different forms, at the municipal level the public–private company (società partecipata) is unquestionably the most common. According to Corte dei Conti (2017), about 86% of the municipalities hold shares
of the capital stock of one or more companies. The largest number of shares is concentrated in the smallest municipalities, though the number of shares per entity increases with the municipal size (MEF, 2016). By asset volume, the most concerned sectors are energy and gas, support to transport, waste management, consultancy, water supply and local public transport; in the South, the partecipate are less numerous and deal mainly with consulting activities and services (Cerved, 2014).

Clearly, if the constrained municipalities are able to control local enterprises the fiscal rules binding the budget figures do not actually prevent local authorities from operating in certain sectors. The lack of unequivocal and certain data on the società partecipate has led us not to try here to directly investigate this possibility. Nonetheless, we can still observe that the productive services sector is not affected by the introduction of the rules in any of the relevant steps – (1), (2) or (3). The result is comforting, as this sector includes numerous functions that have progressively become the object of externalization. It cannot be ignored, however, that also numerous projects belonging to either the human capital or the infrastructure sector have been carried out through public–private companies. Our results are to be read also in the light of these considerations.

As for the second point, we need to delve into the relationship between the spending figures and the municipal public goods/services available to the private sector. In general, there is no exact correspondence between the two concepts. Actually, the relationship is very controversial. Following Eichhorst (2007) we can consider spending as an input. Spending on inputs is different from the output, which is the actual quantity and quality of the produced public goods/services. In turn, the output is enjoyed by the private sector, with different benefits according to its accessibility and effectiveness. These two features depend on a number of context variables (demographical, environmental) which outline a third relevant concept, the outcome. The latter arises from assessing the impact of the public goods/services on citizens and firms and their individual perception as regards those goods/services, and should be evaluated in an intermediate to long–run perspective.

The distinction is relevant. The differences among the SNG do affect the relationship between spending figures and the provided goods/services. For instance, a constant expenditure amount might be paralleled by efficiency gains and thus more or better output; similarly, a spending increase is to be differently assessed depending on whether or not is accompanied by an increase in goods and services. In general, it is reasonable to think that lower spending is at least partially accompanied and compensated by reduced waste and improved efficiency, or by a different accessibility to services. The issue is in any case relevant in the Italian context, traditionally characterized by waste and inefficiencies (Barone and Mocetti, 2011).

The exclusion of the smallest municipalities, usually more inefficient (e.g.

---

32 In 2010 (d.l. n. 78/2010) the municipalities below 30,000 inhabitants were obliged to liquidate or sell their shares in loss–making companies, but the commitment was in fact neglected.

33 It is actually a central dilemma in the estimation of the SNG’ expenditure needs (see Shah, 2012).
Carosi et al., 2014), along with the previous robustness checks seem to support the reliability of our results. It is nonetheless true that our analysis would benefit from being integrated by an efficiency study. Assessing the impact of the rules in terms of efficiency would certainly add stimulating arguments to our discussion. In any case, even if our results turned out to be the upper limit of the true effect, they would certainly still be non-negligible.

9 Conclusions

In this paper we investigate the existence of indirect and unintended effects of SGR aimed at ensuring fiscal discipline on the composition of subnational expenditure. We find rigorous empirical evidence that binding rules aimed at ensuring that the SNG properly contribute to the general government’s fiscal sustainability may lead the municipalities to alter their expenditure composition against investment. In addition, we find that certain categories of investment spending – namely human capital, infrastructure and pure public goods on the accrual–basis side; human capital and to a lesser extent infrastructure on the cash–basis side – are more affected than others. The recomposition might be linked to the presence of a political cycle that tends to manifest itself on the composition rather than on the level of expenditure. Our results refocus attention on all the consequences of the introduction of fiscal rules. The need to ensure that the SNG cooperate in view of the objective of containing the public deficit/debt is clearly unquestionable. Nevertheless, it would be advisable to adapt those rules in order to account for the fact that the subcentral authorities often provide a non-negligible number of sensitive expenditure items. Rules that do not adequately take into account the relevance of subnational expenses to the corresponding economic reality risk to be particularly distortive.

We apply a quasi–experimental design which alleviates many of the validity issues affecting the estimates of the effects of the fiscal rules (e.g. Foremny, 2011). Our design both permits to identify a causal effect and has the advantage of a good internal validity. It is undeniable that this gain in internal validity is partly at the expense of external validity. In particular, we can say nothing about what happens to larger municipalities. This may compromise our intent to investigate the SGR’s side–effects on composition in a comprehensive and convincing manner.

However, this concern is mitigated when recognising the possibility to extend our findings to similar–sized subcentral authorities of similar countries, mostly other EU countries (e.g., Austria, Germany, Spain) with small or tiny municipalities, comparable to those Italian (Eurostat, 2017). This is an attractive extension especially if we recall the importance of fiscal rules as disciplining tools in the presence of supranational rules. (The existence of supranational rules that must be complied with by the general government is primarily an EU countries–related problem.) Clearly, such an extension requires great caution and further considerations, related to the specific features of each country’s SNG especially in terms of spending efficiency. Some of the above robustness checks are comforting in this regard; our results seem not to strongly depend on the specificities of the Italian
context. Nonetheless, the link between binding rules, spending and services provided by the SNG needs further insights. All in all, our analysis has potentially a much broader significance than assessing the effects of the introduction of the Italian DSP.

Besides investigating the additional issues that have been mentioned in the previous section, the analysis may be extended by future research in two directions. On the one hand, it might be useful to examine whether and how the rules’ effects change from one year to another, in order to provide additional information on the dynamics of their impact on the spending aggregates. Secondly, it could be interesting to test empirically for the presence of a political cycle on composition.

References


## Appendix

Table A.1: Effects of (3) with time–invariant covariates and year fixed effects

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. Investment (cb)</th>
<th>Total Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.477</td>
<td>-0.110</td>
<td>-0.278</td>
<td>-0.075</td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.177)**</td>
<td>(0.047)**</td>
<td>(0.095)**</td>
<td>(0.029)**</td>
</tr>
<tr>
<td>CV</td>
<td>-0.386</td>
<td>-0.067</td>
<td>-0.278</td>
<td>-0.064</td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.080)**</td>
<td>(0.035)**</td>
<td>(0.054)**</td>
<td>(0.026)**</td>
</tr>
</tbody>
</table>

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; time–bandwidth = 2006–2011.

Table A.2: Effects of (3) with time–invariant covariates and year fixed effects – functions (ab)

<table>
<thead>
<tr>
<th></th>
<th>Human Capital</th>
<th>Infrastructure</th>
<th>Pure Public Goods</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.573</td>
<td>-0.704</td>
<td>-0.521</td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.292)*</td>
<td>(0.254)**</td>
<td>(0.252)**</td>
</tr>
<tr>
<td>CV</td>
<td>-0.372</td>
<td>-0.398</td>
<td>-0.394</td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.159)**</td>
<td>(0.122)**</td>
<td>(0.130)**</td>
</tr>
</tbody>
</table>

Notes: ***$p<0.01$, **$p<0.05$, *$p<0.1$. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting; time–bandwidth = 2006–2011.
Table A.3: Test for hypothesis 3.

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp.</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp.</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>0.126</td>
<td>0.058</td>
<td>-0.132</td>
<td>0.007</td>
<td>-0.007</td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.151)</td>
<td>(0.049)</td>
<td>(0.108)</td>
<td>(0.032)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>0.052</td>
<td>0.005</td>
<td>-0.009</td>
<td>0.005</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.086)</td>
<td>(0.030)</td>
<td>(0.060)</td>
<td>(0.018)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; 1998 as pre–treatment, 1999 and 2000 as post–treatment years.

Table A.4: Test for hypothesis 3. – functions (ab)

<table>
<thead>
<tr>
<th></th>
<th>Human Capital</th>
<th>Infrastructure</th>
<th>Pure Public Goods</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.103</td>
<td>0.021</td>
<td>-0.280</td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.361)</td>
<td>(0.368)</td>
<td>(0.303)</td>
</tr>
<tr>
<td>CV</td>
<td>-0.263</td>
<td>-0.131</td>
<td>-0.009</td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.192)</td>
<td>(0.270)</td>
<td>(0.174)</td>
</tr>
</tbody>
</table>

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting; 1998 pre–treatment, 1999 and 2000 post–treatment years.
Table A.5: Effects of (3) excluding personnel expenditure from current expenditure

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp.</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.476</td>
<td>-0.136</td>
<td>-0.283</td>
<td>-0.103</td>
<td></td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.177)***</td>
<td>(0.060)**</td>
<td>(0.094)***</td>
<td>(0.036)***</td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>-0.383</td>
<td>-0.084</td>
<td>-0.278</td>
<td>-0.097</td>
<td></td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.080)***</td>
<td>(0.035)**</td>
<td>(0.054)***</td>
<td>(0.032)***</td>
<td></td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; time–bandwidth = 2006–2011.

Table A.6: Heterogeneous treatment effect in (3)

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp.</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>tr. eff.</td>
<td>-0.383</td>
<td>-0.096</td>
<td>-0.231</td>
<td>-0.072</td>
</tr>
<tr>
<td>(Calonico et al., 2014b)</td>
<td>(0.214)*</td>
<td>(0.068)</td>
<td>(0.135)*</td>
<td>(0.057)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. n.w.</td>
<td>0.052</td>
<td>0.016</td>
<td>-0.041</td>
<td>0.006</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.148)</td>
<td>(0.068)</td>
<td>(0.123)</td>
<td>(0.065)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. n.e.</td>
<td>-0.153</td>
<td>-0.047</td>
<td>-0.025</td>
<td>-0.043</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.156)</td>
<td>(0.072)</td>
<td>(0.129)</td>
<td>(0.069)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. south</td>
<td>-0.193</td>
<td>-0.044</td>
<td>-0.091</td>
<td>-0.004</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.169)</td>
<td>(0.071)</td>
<td>(0.131)</td>
<td>(0.066)</td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>tr. eff.</td>
<td>-0.318</td>
<td>-0.027</td>
<td>-0.251</td>
<td>-0.044</td>
</tr>
<tr>
<td>(Ludwig and Miller, 2007)</td>
<td>(0.100)***</td>
<td>(0.055)</td>
<td>(0.083)***</td>
<td>(0.053)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. n.w.</td>
<td>0.035</td>
<td>0.009</td>
<td>0.038</td>
<td>-0.010</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.076)</td>
<td>(0.054)</td>
<td>(0.076)</td>
<td>(0.060)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. n.e.</td>
<td>-0.079</td>
<td>-0.067</td>
<td>-0.026</td>
<td>-0.050</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.081)</td>
<td>(0.057)</td>
<td>(0.079)</td>
<td>(0.064)</td>
<td></td>
</tr>
<tr>
<td>tr. eff. south</td>
<td>-0.172</td>
<td>-0.082</td>
<td>-0.092</td>
<td>-0.030</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.088)</td>
<td>(0.056)</td>
<td>(0.083)</td>
<td>(0.061)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; time–bandwidth = 2006–2011.
Table A.7: Effects of (3) excluding municipalities with compulsory administration due to Mafia infiltration

<table>
<thead>
<tr>
<th></th>
<th>Investment (ab)</th>
<th>Total Exp. (ab)</th>
<th>Exp. (cb)</th>
<th>Investment (cb)</th>
<th>Total Exp. (cb)</th>
<th>Exp. (cb)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.485</td>
<td>-0.110</td>
<td>-0.275</td>
<td>-0.080</td>
<td>(Calonico et al., 2014b)</td>
<td></td>
</tr>
<tr>
<td>CV</td>
<td>-0.380</td>
<td>-0.062</td>
<td>-0.270</td>
<td>-0.065</td>
<td>(Ludwig and Miller, 2007)</td>
<td></td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting, cb = cash basis of accounting; time–bandwidth = 2006–2011.

Table A.8: Effects of (3) excluding municipalities with compulsory administration due to Mafia infiltration – functions (ab)

<table>
<thead>
<tr>
<th></th>
<th>Human Capital</th>
<th>Infrastructure</th>
<th>Pure Public Goods</th>
</tr>
</thead>
<tbody>
<tr>
<td>CCT</td>
<td>-0.611</td>
<td>-0.694</td>
<td>-0.572</td>
</tr>
<tr>
<td>CV</td>
<td>-0.382</td>
<td>-0.393</td>
<td>-0.406</td>
</tr>
</tbody>
</table>

Notes: ***p<0.01, **p<0.05, *p<0.1. Robust standard errors clustered at the municipal level are in parentheses; ab = accrual basis of accounting; time–bandwidth = 2006–2011.
Figure A.1: Municipal expenditure between 1999 and 2013

Notes: ab = accrual basis of accounting, cb = cash basis of accounting.
Sample: all municipalities.
Figure A.2: Municipalities between 1500 and 8500 inhabitants: investment spending composition by functions

Notes: Average 2006–2011 expenditure in percentage terms.

Figure A.3: Test for absence of manipulation of the forcing variable (density difference)

Notes: 1999 population is pre–treatment population; 2005 population is the reference population for DSP application in 2007. Horizontal axis: normalized population. Each triangle represents a bin of 50 inhabitants. Spline fourth–order polynomial fit and 95 percent confidence intervals are included.