Austerity Harmed Student Achievement*

Caterina Pavese† Enrico Rubolino‡

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Abstract

This paper shows that austerity-induced spending cuts harmed student performance in standardized national tests. To identify this relationship, we use cross-municipality variation in the timing of eligibility for the Italian Domestic Stability Pact as an exogenous shifter of local public spending. We then compare test scores for students that were from the same municipality but were exposed to different levels of austerity spending cuts, based on their birth year. Combining administrative data on public spending and test scores with an instrumental variable model, we show that the implied test score impact of austerity is between 2.1-2.4 (1.7-1.9) percent of a standard deviation per 1,000 euros per-pupil reduction in current (capital) spending. The effects are more pronounced for children with limited resources at home. By contrast, effects are substantially dampened in municipalities with high-skill politicians and school principals, which are more likely to allocate the marginal spending cut toward less productive spending categories.

Keywords: austerity; public spending; test scores.
JEL Classification: I22; I24; H52; H75

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†University of Padua. Email: caterina.pavese@unipd.it
‡University of Lausanne. Email: enrico.rubolino@unil.ch

1 Introduction

Education is often considered as the key instrument to curb inequality and promote social mobility. The demand for high-quality education, which can translate into higher costs per student, needs to align with sustainable public debt requirements and tax revenue. This tension has been particularly important for many countries in the aftermath of the financial crisis, where the explosion of public debt and the adoption of austerity policies put hurdles on the ability of governments to finance public services (Fetzer 2019). For instance, the European Commission Education and Training Monitor (2016) argued that education in Mediterrenean Europe was "strongly affected by very low and decreasing public spending, due to strict fiscal consolidation."

Understanding if and how much austerity spending cuts harm student outcomes is of key societal importance. While a functioning welfare state can compensate disadvantaged children starting with lower parental investments, welfare cuts may do the opposite, exacerbating existing inequalities. The theoretical effect of austerity policies on student outcomes is contentious, making it an important empirical question. Leading policy makers and economists have taken opposite positions on the implications of austerity.[1] Although austerity policies vary in design and magnitude, it is often extremely difficult to estimate their impacts. A key challenge is the inability to construct a meaningful counterfactual scenario: What would have happened to student outcomes in the absence of austerity policies?

We address this question in the context of the Italian Domestic Stability Pact (DSP): an austerity policy that set budget constraints on local governments. Introduced in 1999 with the aim of achieving the public finance targets set by the European Union Stability and Growth Pact, the DSP aimed at keeping municipalities accountable through a set of sub-national fiscal rules, which significantly tightened local public expenditures.[2] The program was gradually implemented across municipalities based on their population size, offering us useful variation in exposure to austerity across municipalities and over time.

Because Italian municipalities are responsible for financing the provision of several important inputs for child development, austerity budget cuts might affect student outcomes in a multifaceted way. In particular, municipalities fund early childcare, primary schools, local transportation, culture and social activities. To account for all these inputs, we access rich balance sheets data on public expenditures financed by municipalities. Data are available for both current and capital expenditures, broken down into several categories, such as administration, education, culture, sport, and

[1] See, e.g., VoxEU (2012), collecting views from many influential economists. The political and academic debate builds on findings from Giavazzi and Pagano (1990), Alesina and Perotti (1995), Alesina and Ardagna (1998), Alesina and Ardagna (2010), Reinhart and Rogoff (2010). Blyth (2013) and Krugman (2015) heavily criticized austerity polices.

[2] Similar programs have been implemented in both the developed and developing world: according to the International Monetary Fund, 96 countries have imposed national or local fiscal rules (Lledò et al. 2017).

transportation. Since a relatively large share of the municipal budget is rigid and budget rigidities could hinder municipalities' capacity to reallocate existing expenditures (Grembi et al. 2016), we construct a measure of budget rigidity to predict the spending change that a municipality would experience after the DSP introduction. We then merge this information with administrative data on standardized test scores for Italian primary school students, provided by the Institute for the Evaluation of the Educational System (INVALSI). Our final dataset covers children straddling the period of DSP inception, spanning cohorts born between 2002 and 2008.

To identify the causal effect of austerity spending cuts on student outcomes, we use the timing of DSP inception and the pre-existing budget composition as exogenous shifters of spending. Namely, we predict the austerity-induced spending cut that a municipality would experience based on the years elapsed from DSP inception (variation in *exposure*) and its interaction with the ex ante share of spending that was rigid (variation in *dosage*).[3] We then compare test scores for students who were from the same municipality, but who were exposed to different levels of DSP-induced spending drops. Because some children were not born yet at the time of DSP inception while others were already in their final stage of primary school, we can examine whether cohorts that were less exposed to austerity policies had better test scores relative to more exposed cohorts, within a given municipality (and school). We present empirical tests that validate our approach. In particular, we find neither evidence of a significant effect of austerity exposure on residential mobility, nor we uncover relevant impacts on parents' labor market outcomes.

We show that austerity exposure and its interaction with the ex ante share of rigid spending are highly predictive of reductions in public spending per-pupil. We find that an additional year of austerity exposure reduces per-pupil current (capital) spending by 300 (339) euros, representing around 0.9 (4.1) percent of the pre-existing municipal spending. We document a monotonic negative relationship between per-pupil public spending and both austerity exposure and the ex ante share of rigid spending. This trend of deteriorating public spending following austerity is mirrored in test scores.

Using these patterns in an instrumental variables framework, we find that the implied impact of austerity is between 2.1 (2.4) percent of a standard deviation in reading (math) test score per 1,000 euros per-pupil reduction in *current* spending. Test score effects from *capital* spending cuts are relatively similar. These are relevant effects: a back-of-the-envelope calculation suggests that the the overall spending reductions induced by austerity policies reduced test scores by 10.2 percent in math and 9 percent in reading. Given existing estimates of the relationship between test scores and future earnings (Chetty et al. 2011), our results imply that a 1 per-pupil euro austerity spending cut will decrease students' eventual earnings by around 0.852 per-pupil euros in present value.[4]

[3]Similar approaches have been proposed in Jackson et al. (2016) and Mian and Sufi (2012).
[4]Chetty et al. (2011) find that a 1 percent of a standard deviation increase in test scores increases earnings

Our study provides new insights into the (unintended) effects of austerity policies. At the same time, our results raise a number of questions, such as: Is austerity particularly harmful for children from a lower socio-economic background? What are the mechanisms through which spending cuts affect test scores? Are skilled policy makers capable of mitigating adverse test score impacts of declining public spending by reallocating existing resources?

We take several steps to shed light on these questions. First, we examine whether marginal test score impacts from austerity cuts are more intense for students with limited resources at home. To this end, we match our dataset with survey data on the availability of several resources at home, including computers, books, and a study room. We find that math test score impacts are relatively larger for students with limited home resources, suggesting that austerity spending cuts might exacerbate pre-existing inequalities.

Next, we study the composition of austerity spending cuts. In a seminal work on the effect of fiscal consolidation policies, Alesina and Perotti (1995) convey a clear message: the composition of fiscal adjustment is of key importance in determining its ultimate impact on the economy. Do spending cuts rely primarily on cutting waste (e.g., some administrative expenditures) or productive spending items, such as school spending? We find that public budget rigidities lead to important asymmetries in the allocation of spending cuts. Municipalities disproportionally reduce school investments and current spending on cultural activities, services and public transportation, while they cut relatively less spending categories that are rigid, such as payroll expenses.

Delving into the impact on pre-school and primary school expenditures, we match our dataset with information on early childcare services funded by municipalities and with survey data filled out by school principals. We are able to observe a wide array of outcomes, including early childcare attendance and fees, the availability of several primary school facilities, and teaching tools. We present two main results. First, while austerity cuts do not significantly affect early childcare attendance rates, we find that some of the austerity costs are passed on to parents via higher fees. Second, austerity significantly reduced funding to laboratories, libraries, gyms and technological items at school. All these findings are robust to false discovery rates due to multiple testing hypotheses.

Finally, we focus on the role of local politicians and school principals. How local politicians and school principals allocate the austerity-induced spending cut can be an important determinant of students' outcomes. For instance, more skilled politicians might be more likely to reduce current administrative expenditures, characterized by large passive waste in Italy (Bandiera et al. 2009), instead of capital expenditures. By the same token, more skilled school principals can mitigate the negative test score im-

in adulthood by 535 dollars. Assuming an exchange rate of 0.9 and given our preferred estimate of test score impacts from austerity cuts (average coefficients displayed in column (5) of Table 3), we find that a 1,000 per-pupil euro austerity cut would decrease earnings by around 852 per-pupil euros.

pacts of austerity by reallocating existing resources or by promoting organizational innovations that enable teachers to work more effectively (Bloom et al. 2015; Di Liberto et al. 2015). We show that skilled policy makers and school principals are more able to allocate the austerity-induced marginal spending cut toward less productive spending items. Accordingly, we uncover almost zero marginal effects from spending cuts operated in municipalities (schools) governed by skilled politicians (school principals).

Our paper contributes to the debate on the effect of austerity policies. While the agenda of austerity reforms is to make the welfare state leaner and more efficient, austerity might also undermine the ability of governments to pursue social and redistributive policies.[5] We provide the first empirical evidence that austerity spending cuts have negative impacts on students' performance in standardized national tests. These impacts from austerity cuts are likely to persist in adulthood. Both economic theory and empirical evidence have established a robust association between test scores and many important outcomes in life, such as earnings, college attendance, home ownership, retirement savings, and health outcomes (see, e.g., Cunha et al. 2010; Chetty et al. 2011; Heckman et al. 2013; Golsteyn et al. 2014; Almond et al. 2018). The relevance of austerity cuts on children cognitive ability at this early stage calls for a better understanding on whether their impacts will persist later on in life, with important implications for the persistence of inequality and social immobility.

The (unintended) impacts of austerity policies have been also examined by Fetzer (2019), who provides clear evidence that exposure to austerity welfare cuts raised support for the UK Independence Party and, in turn, to vote for Leave in the 2016 UK ("Brexit") referendum on European Union membership. Other recent studies focusing on the cost of austerity policies are Facchetti (2021) and Bray et al. (2022), which find significant effects on hate and violent crimes. These findings suggest that the potential public finance benefits from austerity should be evaluated vis-à-vis with their economic and social costs.

By showing that austerity-induced spending cuts hurt student performance in national tests, our paper also relates to a large literature on whether "money matters" in education (see the recent surveys by Jackson 2018 and Jackson and Mackevicius 2021). Our paper offers three main insights on this literature. First, our setting and data allow us to tackle many plausible identification concerns that arise when focusing on a specific input. Because municipalities face trade-offs when allocating finite resources, any increase in a specific education input might be offset by reductions in other potentially productive spending items. We overcome this issue by focusing on *total* municipal public spending. Second, we show that marginal *capital* spending cuts are similar to those of *non-capital* spending reductions. This result ties with the findings in Jackson and Mackevicius (2021), but it contrasts with other studies that do not account for

[5] A large literature considers the (direct) effects of fiscal consolidation policies on public finance outcomes (see Beetsma and Debrun (2007) for a review on both national and sub-national fiscal rules, and Asatryan et al. (2018) for cross-country historical evidence).

the fact that test score impacts from capital spending cuts might take time to materialize (see, e.g., Goncalves 2015; Martorell et al. 2016). Third, in contrast to most of the existing studies, our analysis is based on plausibly exogenous per-pupil spending *cuts*, rather than spending *increases*. Our paper is close to Jackson et al. (2021), who exploit cross-state variation in exposure to the Great Recession to isolate per-pupil school spending reductions in the US.[6]

The remainder of the paper proceeds as follows. Section 2 presents the institutional background and the data. We describe our research design in section 3. Our main results and mechanisms are presented in sections 4 and 5. Section 6 concludes.

2 Background and Data

2.1 The Domestic Stability Pact

This paper studies the effect of austerity spending cuts in the context of Italian municipalities, which are the lowest level of administrative division in Italy. Similarly to other developed countries, Italy has a relatively large local public sector. Municipalities manage around 10 percent of total public expenditures and are responsible for providing a large array of public goods and services to citizens, including primary and lower secondary schools, public transportation, early childcare, and town planning. The almost 8,000 Italian municipalities are very small open economies: the median population size is 2,532 and around two-thirds of municipalities have less than 5,000 residents. The local government is composed of a mayor and an executive committee. An elected municipal council endorses the annual budget proposed by the mayor and the executive committee.

Table 1: The Italian Domestic Stability Pact

Period	Population above	Deficit target?	Expenditure cap?	Borrowing ceiling?
1999-2000	0	Yes	No	Yes
2001-2004	5,000	Yes	No	Yes
2005-2012	5,000	Yes	Yes	Yes
2013-2015	1,000	Yes	Yes	Yes
2015-	0	Yes	Yes	Yes

Note: This table summarizes the features of the Italian Domestic Stability Pact (DSP) since its introduction in 1999. The sources are Italian budget laws (*Legge Finanziaria*) in every year since 1999 (see online appendix B for details).

Since 1999, fiscal relations between the national government and local governments have been regulated through the Domestic Stability Pact (DSP, *Patto di Stabilità Interno*). Introduced with the aim of achieving the public finance targets set by the European

[6]Furthermore, this paper allows to put Italy in international perspective for evaluating the effect of public spending on test scores. The existing evidence is limited to some particular areas, such as schools hit by the 2012 earthquake (Belmonte et al. 2020) or on the effects of specific inputs, such as class size (Brunello and Checchi 2005), test scores manipulation (Angrist et al. 2017), or the impacts of increasing the number of immigrants in a classroom (Ballatore et al. 2018).

Union (1997 Stability and Growth Pact), the DSP aimed at keeping Italian municipalities accountable through a set of sub-national fiscal rules, setting annual constraints on the budget balance and/or local governments' expenditures. To enforce these rules, the national government introduced sanctions schemes. We provide sources and details on the Domestic Stability Pact in the online appendix B.

Table 1 summarizes the main features of the DSP, including eligibility criteria and types of rules. Two main observations emerge from this table. First, DSP eligibility rests on municipal population. After some changes implemented over the first years, the DSP covered only municipalities with more than 5,000 inhabitants until 2012. The rationale behind this exemption traced back to the determination of lightening the burdensome requirements for small administrative structures. Starting from 2013, the exemption threshold was lowered to municipalities with population of more than 1,000 residents. The 2015 reform eventually extended the DSP to all municipalities.

A second relevant fact emerging from Table 1 is that DSP rules have changed over time. Since its inception and up to 2004, the DSP consisted in a balanced budget rule that merely limited the growth rate of the fiscal gap. Because most sources of revenues and expenditures (including capital spending) were excluded from the target, the DSP had a negligible impact on public spending provisions by municipalities during this period (Chiades and Mengotto 2015; Monacelli et al. 2016; Venturini 2020). The DSP began to significantly tighten municipal spending only from 2005, where the national government introduced expenditure ceilings and extended the source of spending subject to the DSP requirements.[7] For this reason, we will model exposure to austerity policies based on whether the DSP includes rules constraining local public spending.

In theory, municipalities have two smoothing strategies to meet the DSP requirements while keeping local public services unchanged: increasing revenue or reallocating existing expenditures. On the revenue side, however, municipalities' autonomy is limited: both the municipal surtax rate on personal income and the property tax can vary only within a specific range set by the national government (see Rubolino (2020) for the income tax and Bordignon et al. (2003) for the property tax).[8] Likewise, additional grants and transfers from the national and regional government, which mostly cover ordinary running costs, cannot be requested in order to comply with DSP rules.[9]

[7]Some previous studies have shown that municipal public expenditures started to drop in 2005. For instance, Chiades and Mengotto (2015) show that per-capita municipal capital expenditures halved (from 413 to 205 euros) in 2010 compared to 2004. Similar patterns are shown in Monacelli et al. (2016). Building on data from the Italian Institute of Statistics, Figure A1 shows that the middle-2000s also appear as a turning point from a historical perspective.

[8]The national government introduced a local tax freeze for several years (see law 27 December 2006, n. 296, article 1; law 28 December 2015, n. 208), hindering the municipality capacity to finance spending through tax hikes. Furthermore, most municipalities set the maximum top marginal tax rate on both income and property taxes allowed by the national government. Specifically, 65.3 (45.6) percent of municipalities set the top statutory marginal tax rate on income (property tax rate on main residence) in 2015.

[9]Transfers from higher government tiers are determined by law on the basis of a municipality's population, density, surface and age composition (see *Decreto Legislativo* n. 504/1992).

Expenditure reallocation could be a viable road: municipalities can finance additional public expenditures by reducing passive waste. For instance, Bandiera et al. (2009) show that this accounts for a large share of municipal spending in Italy. Austerity policies could lead to a more efficient allocation of spending by increasing politicians' accountability: local politicians may be more willing to reduce inefficient expenditures because alternative policy choices, such as raising local taxes or reducing service provision, might threaten their re-election chances (Asatryan et al. 2018; Daniele and Giommoni 2021). Moreover, the incidence of austerity spending cuts crucially depends on the pre-existing share of rigid spending. Grembi et al. (2016) find that about two-thirds of current and capital expenditures are classified as rigid (i.e., composed of payroll expenses or debt service).

2.2 How Does Austerity Affect Child Development?

Since municipalities are responsible for financing the provision of several important inputs for child development, spending cuts can affect child outcomes in various ways. Most importantly, municipalities fund early childcare and both primary and lower secondary schools. Specifically, municipalities are in charge of school buildings' construction, renovation and maintenance. They are also responsible for providing several basic goods and services, such as transportation, textbooks, teaching tools, meals, and materials needed to set up laboratories, libraries and gyms (see article 139 of law 112/1998 for details). Municipalities are also responsible for water, heating, internet, electricity and cleaning costs for educational venues. The national government is responsible for the general organization of the education system (e.g., minimum standards of education, school staff, quality assurance), and to allocate teachers and principals across schools. Italian schools are thus not able to select, pay or fire their teachers, which are paid directly by the national government.

Austerity budget cuts can prevent municipalities from funding public infrastructures. Public facilities, including schools, may then fall into disrepair, leading students to attend schools in poor conditions. The availability of a well-equipped schooling environment can influence various aspects of child development. Several papers, recently surveyed in Jackson (2018) and Jackson and Mackevicius (2021), have shown that school facility investments improve student outcomes. For instance, outdated buildings can lead to poor indoor air quality that can impair student learning through a lower attendance rate and chronic distractions (Mendell and Heath 2005). The lack (or underfunding) of school goods and services, such as textbooks (Holden 2016), air conditioning (Park et al. 2020) and ICT materials (Comi et al. 2017), can also significantly affect student achievement.

Budget constraints can also reduce funding to early childcare programs. Research in social sciences has generated a much deeper appreciation of the importance of early interventions for establishing the building blocks of subsequent human capital forma-

tion (see, e.g., Institute of Medicine and National Research Council 2000; Heckman 2013). Funding of early childcare is under the spotlight in Italy, where childcare provisions are among the lowest in Europe, and the number of applications largely exceeds the number of places in almost all municipalities (Dipartimento Per Le Politiche Della Famiglia 2020).

Municipalities can also foster child development by providing a safe and stimulating environment outside schools. Child development can be seriously compromised by the absence of a stimulating social environment. Austerity can put hurdles on the ability of municipalities to promote child-friendly environments (e.g., playgrounds) and to fund recreational and cultural events that would enrich child development.[10] Availability of environments suitable for outdoor activities can also foster child non-cognitive outcomes by enhancing social interactions and reducing antisocial behaviors (Kaplan and Kaplan 1994; Moore 1996). Positive benefits on child cognitive outcomes also emerge from visits to museums and exhibitions that encourage children informal learning (Tan et al. 2021).

2.3 Data

2.3.1 Student and School Outcomes

We use administrative records on student test scores from the *Istituto nazionale per la valutazione del sistema educativo di istruzione e di formazione* (INVALSI): a standardized achievement test administered to the entire population of Italian students. We collect data on math and reading test scores at the end of primary school (fifth grade) over the 2010/2011-2016/2017 school years. To ensure comparability, we normalize test scores by school year and subject. We also adjust test scores by a cheating factor provided by INVALSI to account for potential cheating behaviors (Angrist et al. 2017; Bertoni et al. 2013).[11] These data provide a comparable and objective measure of performance across cohorts and schools. INVALSI also provides standard student demographic and socio-economic information, such as gender, birth date, preschool attendance, immigrant status, and parental background (i.e., years of schooling and working status). Using the classroom identifier, we also compute class size. Moreover, from INVALSI, we retrieve student-level information on home resources, such as books, Wi-Fi connection and a study room.[12] We complement these data with survey information provided by school principals and teachers, selected by INVALSI to be a nationally representative

[10] Dadvand et al. (2015) show that exposure to green spaces, such as parks, has a positive impact on children's cognitive outcomes. The absence of public spaces for performing physical activities and exposure to noise and/or pollution are also channels that can significantly affect child development (Fedewa and Ahn 2011; Klatte et al. 2013; Sunyer et al. 2015).

[11] Since the cheating factor is not available for the school year 2010/11, we impute the missing information with the school-level average, computed over the other available school years.

[12] In the survey data, the sample size shrinks for two reasons. First, we miss information on the 2002 birth cohort (that was not surveyed by INVALSI). Second, the response rate is large (more than 85 percent) but not perfect.

sample. We collect school-level information on availability (and proper functioning if available) of several school facilities and resources, such as laboratories, gym, library, multimedia devices, and teaching tools.

Our dataset covers children straddling the period of DSP inception, spanning cohorts born between 2002 and 2008. We include all students attending a public school for whom we can recover information on test scores, municipality of school attendance and demographic characteristics. We restrict the sample to students attending a school in municipalities with a population under 30,000 residents, where just a single primary school is present in most cases. We also drop first-generation immigrants and students attending schools located in regions with special autonomy, for whom the DSP had different requirements and rules. Our final dataset covers 962,897 students, enrolled in 2,681 primary schools that are located in 2,274 municipalities. Appendix Table A1 displays summary statistics of the variables used in the empirical analysis.

2.3.2 Public Spending Measurement and Municipality Outcomes

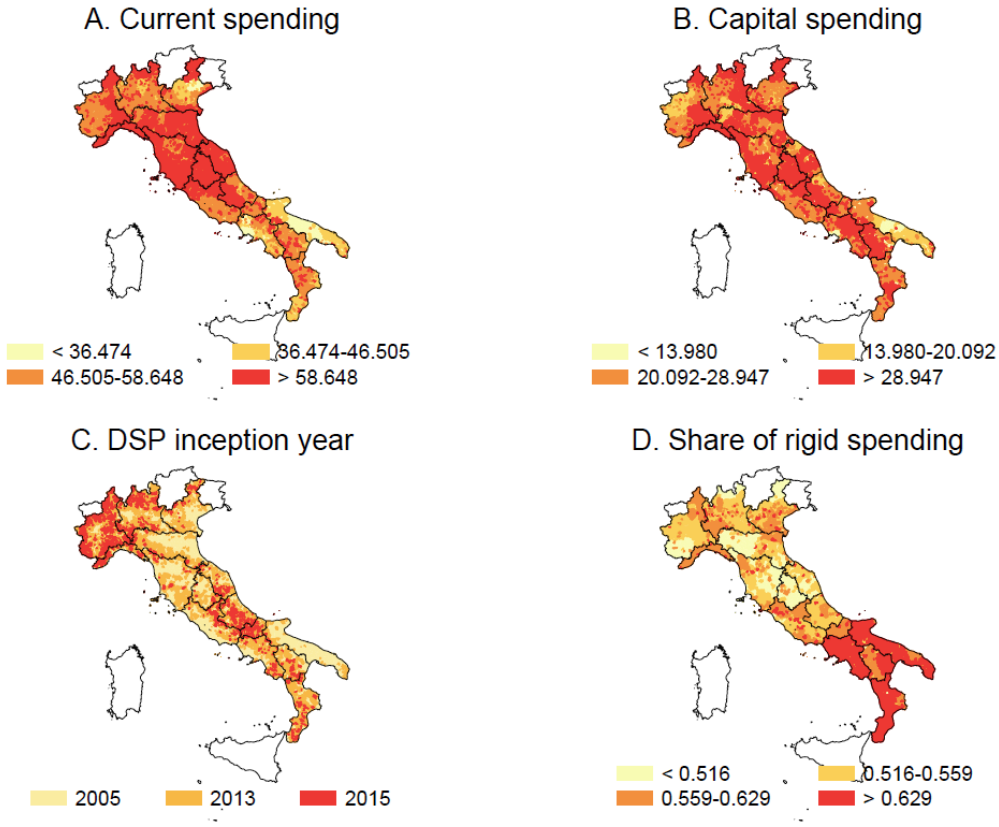
We collect data on public expenditures from the balance sheets of Italian municipalities, which are annual reports provided by the Italian Ministry of Interior (*Ministero degli Interni*). The accounting models are homogeneous both across municipalities and over time, adopting a functional classification structure for revenues and expenditures that is consistent with the United Nations Classifications of the Functions of Government (COFOG). We access municipality-level data since 2002 on both current and capital expenditures, broken down into several budget categories. Using the consumer price index from the Italian Institute of Statistics (ISTAT), we adjust all the nominal amounts to 2020 euros.

To measure public spending, we need to tackle two key challenges. Our first challenge is the selection of the relevant expenditures that can lead to a meaningful impact on student outcomes. While school spending would be the most obvious budget category, we recognize the fact that austerity can influence many potentially productive public expenditures financed by municipalities. Because municipalities face trade-offs when allocating finite resources, reductions in a given public budget category could be offset by increases in other potentially productive budget items. We overcome the biases inherent in focusing on a given budget category by using a single comprehensive measure of municipal public spending, computed as the sum of all expenditures.

A second challenge is related to the time horizon where public spending is measured. A naive approach for calculating the returns to spending would be to assume that spending impacts are immediate and not cumulative, such that the spending effects in a given year map to the test score effects in that same year. However, achievement is likely to be the product of years of cognitive and behavioral development: early interventions (including school investments) could also have impacts many years later.

Test score effects might thus take time to fully materialize.[13]

Figure 1: A Map of Public Spending and Exposure to Austerity Policies

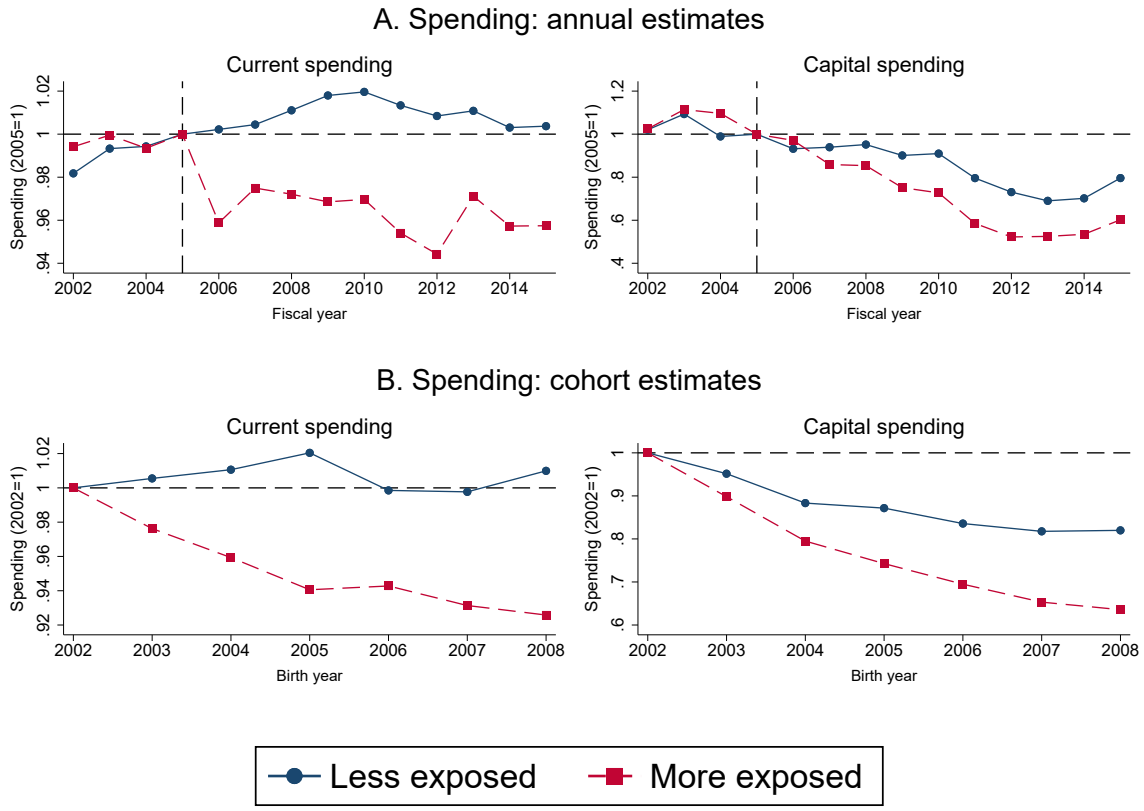


Note: Panel A and B depict municipality-level current and capital spending per-pupil (in thousands of 2020 euros). Panel C shows the inception year of the Domestic Stability Pact (DSP). Panel D presents the pre-DSP share of municipal spending that was rigid. Break points in panels A, B and D are quartile intervals in municipal spending per-pupil. The black line refers to regional boundaries. White areas refer to regions with special autonomy that are not covered by the DSP.

With these concerns in mind, we compute a cohort-specific measure of public spending by averaging annual spending since birth year up to primary school final year (fifth grade), when we observe test scores. Linking our multi-year measure of spending with test scores allows us to account for cumulative learning and for the fact that investment processes take time to set in. To account for any timing difference between when capital spending occurs and when it actually materializes, we use the annualized accounting value of the one-time increase in capital spending. Following Jackson and Mackevicius (2021), we assume annual depreciation of 7 percent. Finally, we scale spending by the total (cohort-specific average) 0-10 population of each municipality, using ISTAT data.

[13]This kind of gradual response is much more likely to take place when spending changes are mostly composed of changes in capital expenditures. For example, some previous studies (see, e.g., Jackson et al. 2016, Lafortune et al. 2018, and Jackson and Mackevicius 2021) show that achievement gains only set in gradually in response to immediate, yet persistent, spending changes. While some capital expenditures could have an almost immediate impact on student learning (e.g., purchasing air conditioners (Park et al. 2020)), construction and other durable assets are used for years after the initial financial outlay (see, e.g., Cellini et al. 2010; Hyman 2017; Lafortune and Schonholzer 2019).

Figure 2: Trends in Municipal Spending Per-Pupil



Note: The figure presents trends in municipal current and capital spending per-pupil, separately for municipalities with a population greater than ("more exposed") and less than ("less exposed") 5,000 residents. The top graphs depict annual spending; the bottom graphs show average birth cohort-specific spending since birth year up to primary school final year (fifth grade). For capital spending, we assume annual depreciation of 7 percent.

On average, per-pupil municipal current (capital) spending is 47,933 (19,369) euros (see Appendix Table A2 for summary statistics). Figure 1 offers a graphical representation of per-pupil current (panel A) and capital (panel B) spending; it shows wide differences in public spending across municipalities. Figure 2 shows trends in per-pupil current and capital spending for municipalities with a population greater than and less than 5,000 residents (labelled as "more exposed" and "less exposed", respectively). The top graphs depict the evolution in yearly expenditures, while the bottom graphs present the cohort-specific spending measure. The figures show a significant decline in both current and capital expenditures in "more exposed", compared to "less exposed", municipalities. Both current and capital per-pupil spending shows a monotonic decline across birth cohorts in more exposed municipalities. These patterns offer prima facie evidence on the negative effect of the DSP on public spending.

We measure exposure to austerity as the difference between the year when a child attended fifth-grade (when we observe test scores) and the DSP inception year (depicted in panel C of Figure 1). Our exposure variable goes from 0 (for those who completed primary school before DSP introduction) up to 13 (for those born 3 years after DSP

inception). Since the incidence of the spending cut is likely to vary across municipalities depending on the extent of budget rigidity, we follow Corte dei Conti (2012) and Grembi et al. (2016) to compute an estimate of budget rigidity intensity as the share of rigid spending in a municipality before the DSP introduction.[14] We find that 56.88 percent of spending is rigid, on average. Panel D of Figure 1 presents the dispersion in the budget rigidity index, ranging from 23.37 to 88.74 percent of spending. The budget rigidity index takes higher values in municipalities located in the Southern Italy.

To account for the confounding effect of other policies that overlap our period of interest, we also collect time-varying municipality-level information for several variables. First, we retrieve data from the Ministry of Interior on socio-demographics characteristics (age, gender and years of schooling) of mayors and other town council's components. Second, we compute taxable income-per capita using data from the Ministry of Economy and Finance. Finally, we calculate housing selling prices using data from *Fondazione IFEL*. Summary statistics are presented in Appendix Table A2.

3 Empirical Strategy

3.1 Sources of Identifying Variation

The gradual implementation of the DSP across municipalities gives rise to two sources of identifying variation. First, we can leverage cross-municipality variation in austerity exposure for a given birth cohort. Consider children born in the following two towns: a. Maratea (2004 population = 5,283), that has been subject to the DSP since 2005; b. Positano (2004 population = 3,914), eligible from 2013. A child born in 2002 would have spent 6 of her 10 years under austerity if born in Maratea, but she would have never been exposed to austerity if born in Positano. Therefore, we can exploit variation within birth cohorts between "more exposed" versus "less exposed" municipalities depending on the number of years elapsed from DSP inception.

A second source of variation arises across birth cohorts in a given municipality. Consider our first (2002) and last observed birth cohort (2008). Students born in Maratea in 2008 have spent their whole life under austerity, while they would have spent three-fifths of their life under austerity if born in 2002. Such a cross-cohort difference in exposure further varies by municipality. Students born in Positano in 2002 have never been exposed to austerity, while those born in 2008 have spent half of their life under austerity. We can thus take advantage of variation within a municipality across "more exposed" versus "less exposed" cohorts depending on the year of birth.

Our research design builds on these two sources of variation to identify the test score effect of austerity spending cuts. A comparison in the change in test scores between

[14] In 2009 (see law 112/2008, article 77-bis, commi 23-26), the Ministry of Interior introduced some basic indicators to evaluate the effectiveness of the Domestic Stability Pact. These indicators measure the extent of structural budget rigidity and the degree of the financial autonomy of each municipality. Both these indicators are developed by "*Corte dei Conti*".

"more exposed" versus "less exposed" birth cohorts in a given municipality and between "more exposed" versus "less exposed" municipalities in a given cohort allows us to identify the effect of interest by accounting for any underlying difference and unobservable heterogeneity across both birth cohorts and municipalities.

3.2 Event Study Approach Based on 2005 Reform

To motivate our regression models, we start by presenting an event study approach exploiting variation in spending across municipalities that have been differently exposed to the DSP. Since the DSP started to constrain public expenditures in 2005 in municipalities with more than 5,000 inhabitants, but only from 2013 in municipalities below this cutoff, we can test the impact of the DSP on public spending by comparing these two groups of municipalities, before and after 2005. This exercise is similar in spirit to the graphical evidence presented in Figure 2, but a formal event study approach is valuable because it allows us to account for both municipality unobserved heterogeneity and any common shocks or policies. We run regressions as the following:

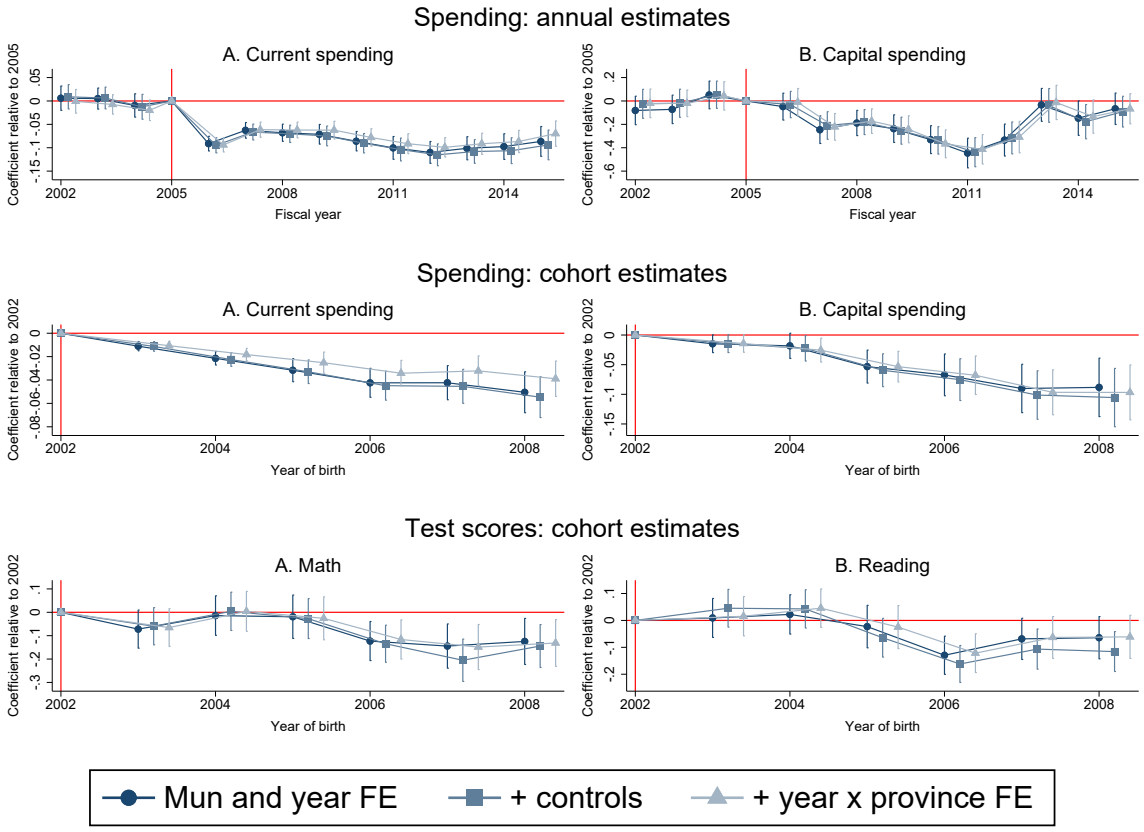
$$y_{m,t} = \beta_j \cdot \sum_{j \neq 2005} 1(pop_{m,t-1} > 5,000) \cdot (j = t) + \gamma_m + \delta_t + u_{m,t}, \tag{1}$$

where the outcome variable, $y_{m,t}$, is the log of current or capital spending per-pupil in municipality m at year t. The interaction between the indicator for "more exposed" municipalities and years, $1(pop_{m,t-1} > 5,000) \cdot (j = t)$, omits the year of DSP inception (2005), which represents our reference year. Municipal population refers to the number of inhabitants registered in the previous year. We include municipality and year fixed effects, γ_m and δ_t, and we cluster the standard errors at municipality level. The β_j estimates illustrate the average spending difference between the two groups of municipalities, relative to the reference year.

The top panel in Figure 3 plots the β_j estimates and 95 percent confidence intervals. The figure provides two key findings. First, both current and capital spending per-pupil did not evolve differently across "more exposed" and "less exposed" municipalities: the β_j estimates are not significantly different from zero over the years leading to the reform. Second, the figure shows a significant reduction in public spending per-pupil for municipalities eligible for the DSP over the post-2005 period. The decline in current spending was sudden and then remained substantially flat. For capital spending, the decline was roughly linear in time between 2005 and 2013, when eligibility was extended to municipalities with less than 5,000 inhabitants. We also test the sensitivity of our estimates to the inclusion of municipality-specific time-varying factors (age, education, and gender of mayor and average values for town council's components), and province-year fixed effects. Our estimates are hardly affected.[15]

[15] In Appendix Figure A2, we present event study estimates obtained from the estimator developed by Borusyak et al. (2021). Their estimator is designed to provide robust estimates in setting with staggered treatment adoption. We estimate the impact of DSP eligibility on public spending by comparing the

Figure 3: The Impact of Austerity on Public Spending and Test Scores



Note: The top panel presents the impact of DSP introduction on the log of current and capital spending per-pupil. The figure plots the β_j coefficients estimated from equation (1) and the 95 percent confidence intervals. The middle panel plots β_j coefficients estimated from equation (2) and the 95 percent confidence intervals. Relative to the baseline 2002 birth cohort, each coefficient reports the difference in per-pupil spending between municipalities that have been more exposed to the DSP (i.e., those with more than 5,000 inhabitants) and less exposed municipalities (less than 5,000 inhabitants). The bottom panel presents estimates obtained from the same model as in the middle panel, but using fifth-grade test scores as outcome variables. Each specification includes municipality and cohort fixed effects. The alternative specifications include municipality-level time-varying controls (age, education, and gender of mayor and average values for town council members), and province-year fixed effects. Standard errors are clustered at municipality level.

Next, we translate the year-specific measure of spending per-pupil in our (baseline) cohort-specific measure, computed as described in section 2.3.2. For each child i in municipality m and cohort c, we estimate the following model:

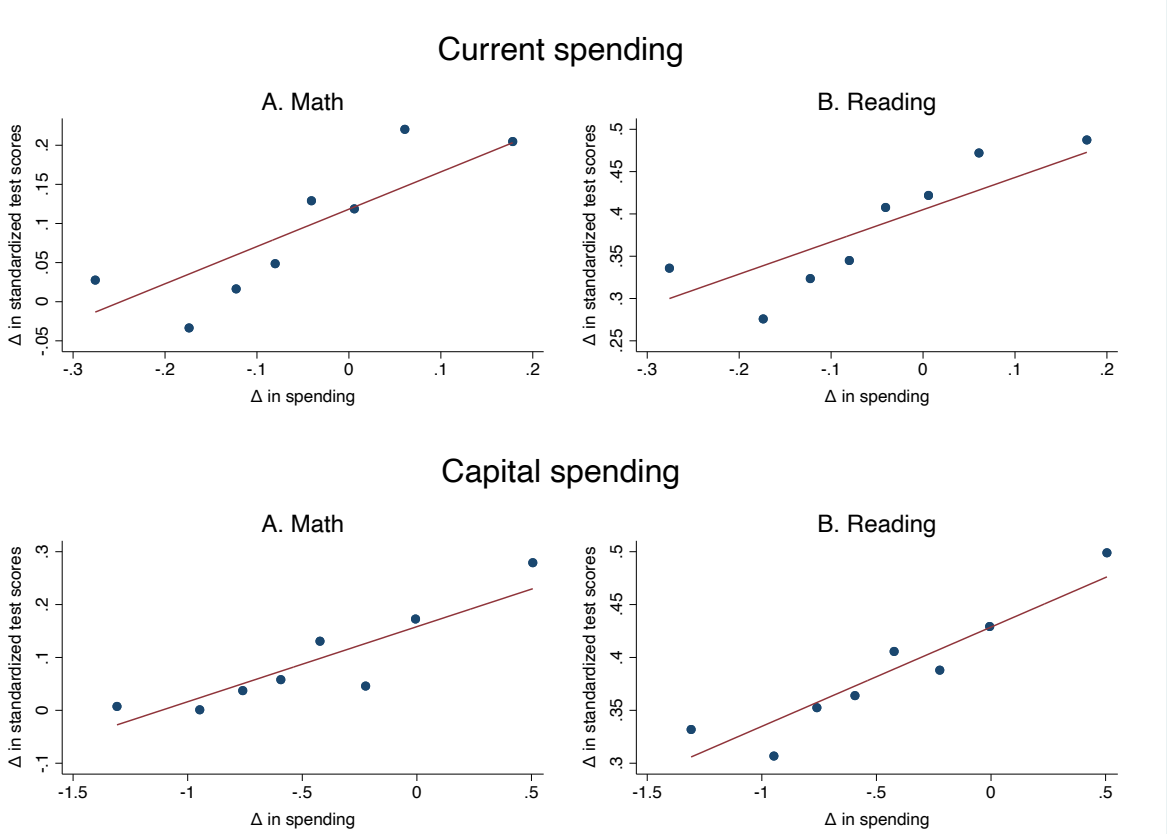
$$y_{i,m,c} = \beta_j \cdot \sum_{j \neq 2002} 1(pop_{m,\bar{c}} > 5,000) \cdot (j = c) + \gamma_m + \delta_c + u_{i,m,c}. \quad (2)$$

This model compares outcomes across cohorts relative to the first (least exposed) cohort (2002), and with respect to changes for the same cohort in less exposed municipalities. Municipal population is computed as the cohort-specific average. The middle

outcome by event time (years from DSP inception). This approach exploits the staggered implementation of the DSP across municipalities to construct treated and control groups. We find substantially similar results.

panel in Figure 3 shows sizable reductions in both current and capital spending in more exposed birth cohorts. The most exposed birth cohort experienced, on average, a 6.5 (12.3) percent reduction in per-pupil current (capital) spending relative to the least exposed birth cohort.

Figure 4: Comparing Per-Pupil Spending Changes with Test Scores Changes



Note: The figure compares the difference in standardized fifth-grade test scores in math and reading with the difference in (log of) current (top panel) and capital (bottom panel) per-pupil spending (horizontal axis). These outcomes are computed as the within-municipality difference between the most versus least austerity exposed birth cohort. We plot these outcomes in equal sized bins and show the line of best fit.

In the bottom panel, we plot the coefficient estimates for math and reading test scores. Consistent with the pattern of declining spending, test scores of cohorts that were more exposed to austerity-induced spending cuts are relatively lower than test scores of less exposed cohorts. Differently from the spending response, test score impacts were not instantaneous, but they materialize after a few years of exposure. This gradual response is consistent with some previous studies, such as Lafortune et al. (2018), who show that test score impacts only set in gradually in response to immediate, yet persistent, spending increases induced by School Finance Reforms in the US. The delay in response is also in line with estimates focusing on capital spending changes: in a review of the existing estimates, Jackson and Mackevicius (2021) show that test score impacts take between 4 and 6 years to materialize.

Although suggestive, test scores estimates do not account for treatment intensity

(i.e., different cross-municipality spending reductions for a given exposure effect). A simple way to test whether within municipalities differences in spending across cohorts are related to similar changes in test scores is to compare within municipalities variation in spending and test scores between the most exposed and the least exposed cohort. We present this relationship in Figure 4, which plots the difference in math and reading test scores against changes in (log of) current and capital spending per-pupil. We then plot these differences in equal sized bins with the line of best fit. The positive slope suggests that the difference in test scores between the most and least exposed cohort from the same municipality tends to be larger, on average, for municipalities that experienced larger drops in per-pupil spending.

3.3 2SLS Model

To leverage these sources of identifying variation, we implement a two-stage least-squares (2SLS) model. This approach allows us to account for both different treatment effects across cohorts within municipalities and different treatment intensity across municipalities for a given cohort. Because DSP eligibility is likely to differently affect municipalities based on the pre-existing composition of expenditures, we use the ex ante share of rigid spending as predictor of austerity spending cuts. As a shorthand, we denote the austerity-induced change in spending based on the pre-existing extent of budget rigidity as *dosage*.[16] We then predict within-municipality cross-cohort spending changes using austerity exposure and its interaction with our measure of dosage.

Our 2SLS model compares the difference in test scores between birth cohorts from the same municipality exposed to different amounts of time (variation in *exposure*) across municipalities with larger or smaller austerity-induced changes in per-pupil spending based on the pre-existing extent of budget rigidity (variation in *dosage*).[17] Specifically, for each child i in municipality m belonging to birth cohort c, we run systems of equations of the following form:

$$S_{i,m,c} = \alpha_1 \cdot Exp_{m,c} + \alpha_2 \cdot (Exp_{m,c} \cdot Dosage_m) + \eta_m + \theta_c + \rho \cdot X_{i,m,c} + v_{i,m,c}; \quad (3)$$

$$y_{i,m,c} = \beta \cdot \widehat{S_{i,m,c}} + \gamma_m + \delta_c + \pi \cdot X_{i,m,c} + u_{i,m,c}. \quad (4)$$

Our measure of student performance is the standardized fifth-grade test score in math and reading, $y_{i,m,c}$. The treatment variable of interest, $S_{i,m,c}$, is per-pupil public

[16] In principle, we could predict public spending changes solely by austerity exposure. However, this would violate the monotonicity condition for a valid instrument: DSP eligibility differently affects municipalities based on their pre-existing composition of public spending. Furthermore, because some previous studies have shown imperfect compliance with DSP requirements (Patrizii et al. 2006; Grembi et al. 2016), conditioning austerity exposure on the extent of budget rigidity rules out large differences across municipalities in the compliance rate.

[17] Jackson et al. (2016) adopt a similar empirical approach to study the effect of the US School Finance Reforms, while Mian and Sufi (2012) study the effect of fiscal stimulus in the US.

spending (in 2020 thousand euros), as described in section 2.3.2. Our baseline measure is the natural log of spending, which allows for non-linearity in the relationship between student performance and spending, such as diminishing marginal product, but we will also report estimates using the level of spending. We will present separate estimates relative to current and capital per-pupil spending.

Our measure of exposure, $Exp_{m,c}$, is the number of years that a birth cohort c in municipality m has spent under austerity. To account for variation in dosage conditional on exposure, we interact exposure with $Dosage_m$, which measures the pre-existing extent of budget rigidity. For the sake of simplicity, we use a discrete measure of dosage: it is equal to 1 for municipalities whose budget rigidity index is in the bottom quintile of the budget rigidity index distribution; 0 otherwise. The inclusion of municipality fixed effects, η_m and γ_m, allows us to rely on variation across birth cohorts within municipalities. Cohort fixed effects, θ_c and δ_c, account for general underlying differences across birth cohorts, irrespective of exposure. Because there are multiple schools in large size municipalities, we will also add school fixed effects to account for any time-invariant unobserved heterogeneity across schools within municipalities, such as (permanent) differences in school facilities, and teachers or school principal quality.[18]

To ensure that we isolate changes due to austerity, we include several controls in $X_{i,m,c}$. Specifically, we account for gender, nationality, quarter of birth, father's and mother's education, and a dummy for grade repeaters. We also control for a wide range of time-varying municipality-specific characteristics, including the tax base, house prices, and socio-demographics characteristics (age, gender and education) of the mayor and other town council members. Furthermore, given the staggered timing of elections across municipalities, we also add election year-cohort fixed effects to account for the fact that electoral incentives for pursuing policies aiming to capture voters, such as larger public spending, are stronger when elections approach (Bonfatti and Forni 2019). Finally, $v_{i,m,c}$ and $u_{i,m,c}$ are random error terms. Because the effect of austerity is likely to be correlated within a municipality, we account for any dependence between observations within a municipality by clustering all regression results at the municipality level.

3.4 Identifying Assumptions

The 2SLS estimator, β, calculates the local average treatment effect of spending on test scores. A positive β estimate would suggest that students that experienced larger austerity spending cuts tend to perform relatively worse in national tests. β yields the causal effect of austerity spending cuts on test scores under three main assumptions: i) *relevance:* austerity exposure and its interaction with our measure of dosage signif-

[18]To the best of our knowledge, municipalities do not allocate spending across schools according to specific (national or sub-national) laws or by ranking schools according to student ability or parental socio-economic background. Informal discussions and mail exchanges with Italian mayors also give credence to this fact.

icantly predict public spending cuts; ii) *monotonicity*: each municipality's probability of reducing spending increases in austerity exposure and dosage; iii) *exclusion restriction*: austerity affects test scores only through its effects on spending. We discuss these conditions below and we will assuage additional threats to identification in section 4.3.

3.4.1 First-Stage and Reduced-Form Estimates

We validate the relevance condition of our instruments by examining the "first-stage" relationship presented in equation (3). Panels A-D of Table 2 reports the α_1 and α_2 coefficient estimates relative to current and capital spending per-pupil, both in levels and in logs. In each panel, the first row presents the α_1 estimates: the effect of austerity exposure for municipalities with lower predicted dosage (i.e., those whose budget rigidity index is above the bottom quintile of the distribution). The second row shows the α_2 estimates: the coefficient estimate from the interaction between austerity exposure and higher predicted dosage.

We start from a basic model with municipality and cohort fixed effects (column 1). Our estimates show that an additional year of austerity exposure led to a nearly 0.6 percent reduction, on average, in current spending and a 2.5 percent reduction in capital spending in municipalities with lower predicted dosage. This implies that a cohort subject to an additional year of austerity exposure would experience a reduction in per-pupil current (capital) spending by 208 (219) euros. Consistent with the notion that budget rigidities predict spending cuts, we find that an extra year of austerity exposure had significantly larger negative effects on municipalities with higher predicted dosage. On average, an additional year of austerity exposure reduced current (capital) expenditures by a further 0.9 (4.1) percent in municipalities with higher predicted dosage. This maps into an additional reduction of per-pupil current (capital) spending of 667 (817) euros.

In column (2)-(5), we add school fixed effects (column 2), municipality-level controls (column 3), student-level controls (column 4), and the average tax base and house prices (column 5). The inclusion of these controls does not significantly affect the coefficient estimates. With the exception of using capital spending in level (panel C), these results imply a strong first-stage relationship, with first-stage F-statistics that are fairly above the conventional thresholds for weak instruments (Stock and Yogo 2005).

Based on the estimates from our preferred model (column 5) and on the average austerity exposure in our sample (8.485 years), the implied spending impact of austerity is the following. A child studying in a municipality with lower predicted dosage would experience 1,324 (1,935) per-pupil euros of lower current (capital) spending, compared to a child located in the same municipality who has never been exposed to austerity. The effects are much stronger for children studying in municipalities with higher predicted dosage: the spending cut will be equal to 4,989 euros of per-pupil current spending, and 6,228 euros of per-pupil capital spending. Weighting these estimates by the average student population in these two groups of municipalities, this result

suggests that the implied per-pupil cost of austerity is of nearly 2,057 euros of current spending and 2,794 euros of capital spending.

Before presenting our 2SLS estimates, it is helpful to first understand the direct effect of austerity exposure to test scores (the "reduced-form" effect). Panels E and F show the α_1 and α_2 coefficient estimates obtained by regressing equation (3) on math and reading test scores. The coefficients are negative, suggesting that austerity harmed students' performance in national tests. The coefficient estimates further drop in municipalities with higher predicted dosage, which is consistent with the fact that these effects reflect lower austerity spending cuts.

3.4.2 Monotonicity Condition

The interpretation of our β estimate as a weighted average of compliers treatment effects rests on the monotonicity assumption: each municipality's probability of reducing public spending increases in austerity exposure *and* dosage. In the presence of multiple instruments, the monotonicity condition needs to be satisfied for each instrument separately. This assumption is key for ensuring that our instrumental variable (IV) strategy aggregates treatment effects across complier municipalities using the weighting strategy proposed by Imbens and Angrist (1994).

Recent IV works have shown that linear IV still yields a convex combination of treatment effects under a *partial* (weaker) version of the monotonicity condition. Mogstad et al. (2021) show that multiple instruments are consistent with the weighting strategy proposed by Imbens and Angrist (1994) if the monotonicity condition is valid for each instrument separately (keeping the other instrument fixed). In our context, a sufficient condition for partial monotonicity is that all municipalities are at least as likely to cut spending if they are more exposed to austerity policies or face lower budget rigidities. Partial monotonicity allows for some municipalities to respond more to exposure than to the extent of budget rigidity, and for others to respond more to budget rigidity than to exposure.

To check whether the partial monotonicity condition is satisfied, we examine whether the unconditional correlation between per-pupil spending and each instrument has the expected sign. We test this condition graphically in Figure A3 and Figure A4, which plot each variable of interest in equal sized bins with the line of best fit. Figure A3 displays the relationship between spending changes (comparing pre- versus post-DSP inception) by budget rigidity intensity; it shows that, on average, spending decreased relatively more in places with lower budget rigidity. Figure A4 shows that austerity exposure is negatively associated with per-pupil spending. In both cases, the scatter plots show a clear linear relationship, suggesting that spending cuts were significantly stronger in places with higher predicted dosage and higher exposure. This result suggests that the partial monotonicity condition is valid in our setup.

Table 2: First-Stage and Reduced-Form Estimates

	(1)	(2)	(3)	(4)	(5)
	A. *Outcome*: current spending (1,000€)				
$Exp_{m,c}$	-0.208***	-0.186***	-0.184***	-0.184***	-0.156**
	(0.060)	(0.062)	(0.062)	(0.062)	(0.061)
$Exp_{m,c} \cdot Dosage_m$	-0.667***	-0.675***	-0.635***	-0.635***	-0.588***
	(0.125)	(0.126)	(0.131)	(0.131)	(0.129)
F-statistics	19.675	18.303	15.787	15.781	13.224
	B. *Outcome*: log of current spending				
$Exp_{m,c}$	-0.006***	-0.006***	-0.006***	-0.006***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$Exp_{m,c} \cdot Dosage_m$	-0.009***	-0.009***	-0.008***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
F-statistics	27.548	21.720	20.056	20.050	17.459
	C. *Outcome*: capital spending (1,000€)				
$Exp_{m,c}$	-0.219	-0.224	-0.228	-0.228	-0.228
	(0.159)	(0.158)	(0.162)	(0.162)	(0.161)
$Exp_{m,c} \cdot Dosage_m$	-0.817***	-0.840***	-0.761***	-0.760***	-0.734***
	(0.273)	(0.278)	(0.279)	(0.279)	(0.277)
F-statistics	5.360	5.491	4.691	4.687	4.542
	D. *Outcome*: log of capital spending				
$Exp_{m,c}$	-0.025***	-0.026***	-0.026***	-0.026***	-0.023***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
$Exp_{m,c} \cdot Dosage_m$	-0.041***	-0.042***	-0.037***	-0.037***	-0.034***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
F-statistics	17.356	18.705	15.804	15.796	13.266
	E. *Outcome*: standardized V grade math test score				
$Exp_{m,c}$	-0.011**	-0.011**	-0.013***	-0.012**	-0.011**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Exp_{m,c} \cdot Dosage_m$	-0.019***	-0.017***	-0.014**	-0.013**	-0.011*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
F-statistics	8.486	7.449	6.297	5.602	4.495
	F. *Outcome*: standardized V grade reading test score				
$Exp_{m,c}$	-0.010**	-0.010**	-0.011**	-0.011**	-0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Exp_{m,c} \cdot Dosage_m$	-0.016***	-0.016***	-0.012**	-0.011**	-0.010*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
F-statistics	7.587	7.295	5.583	4.948	4.029
# of students			962,897		
# of schools			2,681		
# of municipalities			2,274		
Municipality FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes	Yes
Individual controls	No	No	No	Yes	Yes
Other controls	No	No	No	No	Yes

Note: This table reports the "first-stage" (panels A-D) and "reduced-form" (panels E and F) estimates. We report the effect of austerity exposure ($Exp_{m,c}$) and its interaction with ex ante budget rigidity index ($Exp_{m,c} \cdot Dosage_m$) on per-pupil current and capital spending (in levels and logs), and standardized fifth-grade test scores in math and reading. Individual-level controls include gender, nationality, quarter of birth, father education, mother education, and a dummy for grade repeaters. Municipality-level controls include age, gender and education of the mayor and average values for the other town council members, and election year-cohort fixed effects. Other controls include the municipal average tax base and house price index. Each panel also reports F-statistics. Standard errors clustered at municipality level reported in parentheses.

3.4.3 Multi-Dimensionality of Austerity

The exclusion restriction requires that austerity exposure and its interaction with dosage affect test scores only through spending cuts. One could be concerned that the DSP trigger other changes, such as higher tax rates and more general labor market dynamics. If more exposed cohorts also systematically differ in other aspects, and these other aspects influence test scores, then the exclusion restriction would be threatened.

In principle, it is possible that channels different from spending cuts could affect student outcomes. A simple way for evaluating this possibility is to test whether we witness significant austerity-induced effects on test scores driven by changes in alternative channels. We estimate a variant of our main specification that instruments for the following variables: local tax rates on income and on property, the average municipal tax base as reported in tax returns data, and parental employment. We present the results in Appendix Table A3. We find no significant effect of austerity-induced changes in any of these variables. The result of this test suggests that our instruments operate through their negative impact on spending.

We also test whether instrumented per-pupil spending predicts economic conditions or other labor market outcomes that might be associated with test scores. We again use local tax rates, municipal average tax base and parental employment as potential outcomes. We then estimate our 2SLS on each of these variables. We do not find significant effects on any of these economic and labor market outcomes (see Appendix Table A4). This result suggests that there is no significant relationship between austerity-induced spending changes and these economic and labor market predictors of students' test scores.

In theory, municipalities could offset austerity spending cuts by attracting greater resources from other governments. To this aim, we use balance sheets data to check whether austerity exposure predicts changes in the total amount of European Union funds and transfers from the national and regional governments that a municipality received. Appendix Table A3 and Table A4 show no evidence of a significant relationship. As discussed in section 2, this is not surprising because government grants are assigned according to rules that are orthogonal to DSP exposure (or public spending choices operated by municipalities).

An additional threat to our identification approach is the possibility that our estimates are biased by residential mobility and Tiebout sorting. A simple way to evaluate this possibility is to test whether austerity-induced spending changes predict changes in students' parental background across cohorts within municipalities. For instance, as long as stricter austerity exposure leads wealthy families to systematically move towards less exposed (and, thus, higher spending) municipalities, we should observe a negative correlation between austerity-induced spending drops and the parental background of a cohort in a given municipality. Using our baseline 2SLS model (but dropping parental background as control variable), we test the effects on various measures of parental education, separately for fathers and mothers. Appendix Table A5 presents

the results of this test: we do not detect any systematic correlation between our instruments and parental education. Overall, these tests suggest that endogenous mobility in response to austerity spending cuts is not an empirically meaningful source of bias in our estimated effects.

4 The Impact of Austerity Spending Cuts on Test Scores

This section presents and discusses our findings on the test score impacts of austerity spending cuts. We will also examine heterogeneity responses and assuage other potential threats to identification.

4.1 Baseline Results

Table 3 presents our 2SLS estimates, showing the impact of austerity-induced spending changes on standardized test scores for math (panel A) and reading (panel B). Each outcome variable has a mean of 0 and a standard deviation of 1, while per-pupil spending is expressed both in levels (thousand of 2020 euros) and in log, separately for current and capital spending.

We find significant effects on test scores from austerity cuts on both current and capital spending. Our baseline estimate (column 5) suggests that a 1,000 euros austerity-induced drop in per-pupil spending reduced math test scores by about 2.4 percent of a standard deviation. The impact of reading test scores is similar, amounting to 2.1 percent of a standard deviation. The logged measure of spending implies that a nearly 1 percent reduction in current spending reduces test scores by nearly 1.761 (1.571) percent of a standard deviation in math (reading) test scores. Cuts on capital expenditures have fairly similar impacts: a 1,000 euros austerity-induced drop in per-pupil capital spending reduces math (reading) test scores by 1.9 (1.7) percent of a standard deviation.[19]

Our evidence that marginal capital spending cuts are similar to those of non-capital spending reductions ties in with the findings in Jackson and Mackevicius (2021), but it contrasts with other studies that do not account for the fact that test score impacts from capital spending cuts might take time to materialize (see, e.g., Goncalves 2015; Martorell et al. 2016). We show that once we measure the test score impacts from the present discounted "flow" value of capital spending, the marginal impacts of capital spending changes are fairly similar to those of non-capital spending changes. A naive comparison of contemporaneous capital spending to contemporaneous outcomes is instead inappropriate and would significantly understate the test score impacts of capital spending.

[19]Differences between current and capital expenditures in test score impacts from the *logged* measure of spending should not mislead the reader. The different point estimate reflects the fact that a 1 log point change in current expenditures corresponds to a much lower change in capital expenditures (see Table A2 for summary statistics).

Table 3: Baseline Results

	(1)	(2)	(3)	(4)	(5)
	A. *Outcome*: standardized fifth-grade math test score				
Current spending (1,000€)	0.032***	0.029***	0.027***	0.025***	0.024**
	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)
Current spending (log)	1.991***	1.938***	1.897***	1.778***	1.761***
	(0.577)	(0.591)	(0.616)	(0.610)	(0.670)
Capital spending (1,000€)	0.026**	0.023**	0.023**	0.021**	0.019*
	(0.011)	(0.010)	(0.011)	(0.010)	(0.010)
Capital spending (log)	0.463***	0.423***	0.427***	0.400***	0.389***
	(0.138)	(0.129)	(0.139)	(0.137)	(0.148)
	B. *Outcome*: standardized fifth-grade reading test score				
Current spending (1,000€)	0.027***	0.027***	0.024***	0.022***	0.021**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)
Current spending (log)	1.725***	1.751***	1.680***	1.570***	1.571**
	(0.535)	(0.550)	(0.574)	(0.569)	(0.628)
Capital spending (1,000€)	0.022**	0.021**	0.020**	0.018**	0.017**
	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)
Capital spending (log)	0.390***	0.371***	0.366***	0.341***	0.334**
	(0.128)	(0.122)	(0.134)	(0.132)	(0.143)
# of students			962,897		
# of schools			2,681		
# of municipalities			2,274		
Municipality FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
School FE	No	Yes	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes	Yes
Individual controls	No	No	No	Yes	Yes
Other controls	No	No	No	No	Yes

Note: This table reports 2SLS estimates on the effect of austerity-induced per-pupil spending changes on standardized test scores on math (top panel) and reading (bottom panel). Individual-level controls include gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students. Municipality-level controls include age, gender and education of the mayor and average values for the other town council members, and election year-cohort fixed effects. Other controls include the municipal average tax base and an house price index. Standard errors clustered at municipality level reported in parentheses.

The implied test score effects from austerity spending cuts are relatively large. A back-of-the-envelope calculation suggests that the implied test score effect of austerity *current* spending cuts is around 4.9 (4.3) percent of a standard deviation in math (reading) test scores.[20] Cuts in *capital* expenditures imply a reduction in test scores of 5.3 (4.7) percent of a standard deviation in math (reading). The aggregate (current plus capital) spending reduction induced by austerity reduced test scores by 10.2 percent in math and 9 percent in reading.

Point estimates are substantially similar across specifications and lie in the range of the existing estimates on the marginal impact of spending changes on student outcomes (mostly focusing on *school* spending changes). In Appendix Figure A10, we put our estimates in international perspective. Following Jackson and Mackevicius (2021), we select studies investigating the causal impact of spending on standardized test scores. Each coefficient reported in the figure shows the effect of a 1,000 dollars per-pupil school spending change on standardized test scores, assuming an exposure effect of 4 years. To ensure comparability, we apply three adjustments to our baseline estimates. First, we average our coefficient estimate by subject and spending. Second, we convert the estimate in 2020 dollars. Third, we re-weight our estimated effect assuming an exposure effect of 4 years. It turns out that an austerity spending cut of 1,000 dollars reduced test scores by 1.25 percent of a standard deviation. This estimate is near the median value of the effect distribution, where most precise estimates locate.

To summarize, our analysis yields two main results. First, austerity spending cuts have a significant negative effect on test scores, with comparable magnitudes on math and reading test scores. Second, the test score impacts from current expenditure cuts are similar to those induced by capital expenditure cuts.

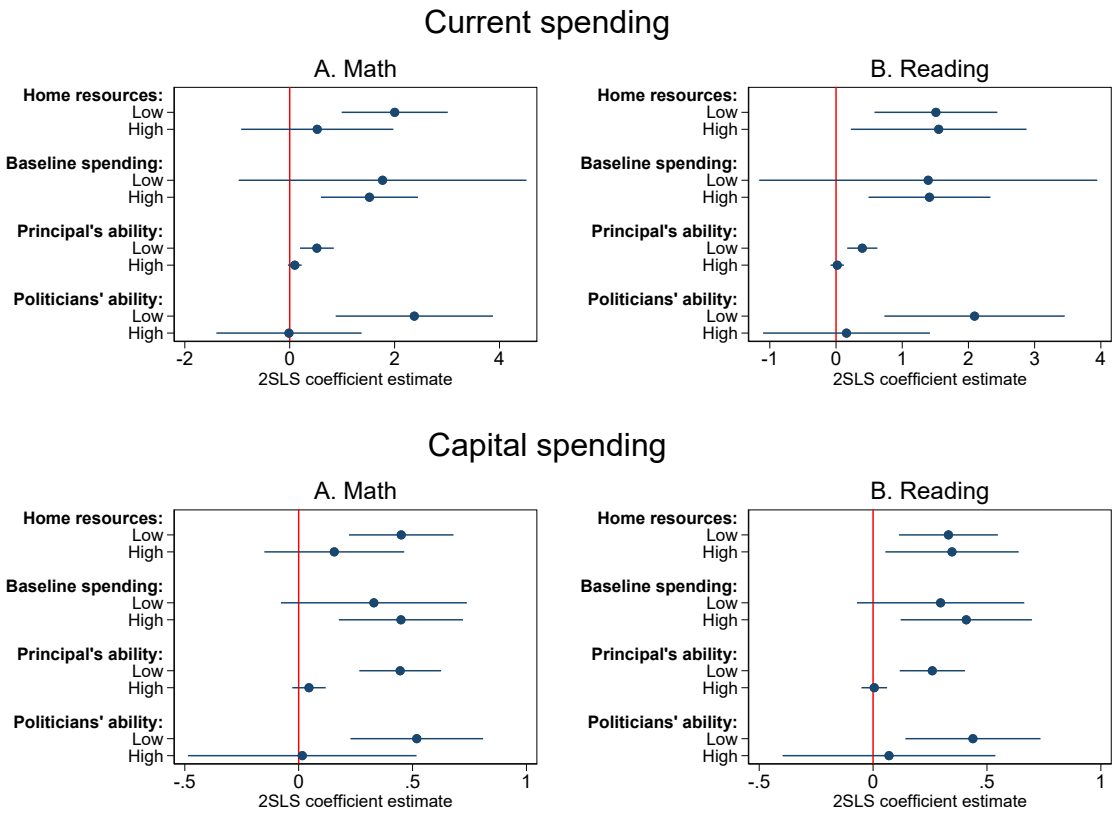
4.2 Heterogeneous Responses

We present heterogeneous test score impacts from austerity cuts in Figure 5, which depicts coefficient estimate and 95 percent confidence intervals from our 2SLS baseline model. Each point estimate is computed by estimating equation (4) on four subsamples of the original population.

We start by testing whether test score impacts of austerity spending drops are more intense for students with limited resources at home. We construct a student-specific index of home resources by using survey information on the availability at home of the following resources: computer, desk, encyclopaedia, internet connection, study room, and books. We compute this index as the average probability of owning these resources. We then split students according to whether they are below or above the (municipality-cohort specific) median value. The scatter-plots show that the marginal *math* test score impact from austerity spending cuts is larger for students with limited

[20] This is computed by multiplying the average exposure effect (8.485 years) by the average per-pupil spending change (2,057 euros for current spending; 2,794 euros for capital spending) and the marginal test score impact of a 1,000 euros spending change (the coefficient displayed in column (5) of Table 3).

Figure 5: Heterogeneity Analysis



Note: The figure reports 2SLS coefficient estimate and 95 percent confidence intervals on the test score impact of austerity spending cuts from different sub-samples of the original population. For each specification, we divide the original sample according to whether a municipality is below or above the median value in a given category. We split our sample according to home resources (where the median value is computed within each municipality-cohort cell), baseline spending per-pupil, school principal ability, and local politicians ability. Apart from the specification on school principal ability that use province fixed effects (rather than municipality fixed effects), each specification includes municipality fixed effects, cohort fixed effects, student-level controls (gender, nationality, quarter of birth, father's education, mother's education, and dummies grade repeaters), and municipality-level controls (age, gender and education of the mayor and average values for other town council's members, and election year-cohort fixed effects).

home resources. This result holds both for current and capital expenditures. By contrast, we find that the *reading* test score impact differs relatively less across students with different private resources.

We then examine whether the marginal test score impacts of austerity cuts depend on the baseline spending level. As shown in Figure 1, baseline current and capital spending widely varied across municipalities. In principle, it is possible that austerity cuts would have smaller effects in municipalities starting with a higher level of spending. Our estimates do not provide any pattern of diminishing marginal impacts for municipalities with larger pre-existing spending: the marginal test score impacts of austerity spending cuts are not affected by baseline spending level.[21] One explanation

[21]This result is in line with existing evidence from the U.S. Although there is wide geographical variation in per-pupil (school) spending, Jackson and Mackevicius (2021) show that marginal impacts of

for this result is that spending is not allocated to the most productive inputs at the margin.

Even though baseline spending might not have been allocated to the most productive budget category, it is possible that more skilled local politicians are more able to allocate the *marginal spending cut* toward less productive budget categories. If this were the case, then the marginal test score impacts of austerity cuts should be larger in municipalities managed by less skilled local politicians, ceteris paribus. To study this heterogeneity, we first compute a municipality-specific index of local politicians ability as the average experience level of town council's components. We then divide municipalities according to whether the index is below or above the median. We uncover relatively larger test score impacts in municipalities governed by less skilled politicians, implying that skilled politicians are more likely to earmark austerity cuts towards less productive spending categories.

By the same token, more skilled school principals could be better at managing declining spending by reallocating resources from low to high productive items. Their contribution to mitigate the negative test score impacts of austerity cuts includes the conditions and climate in which teaching and learning occur. As we argued in section 2, the Italian context is interesting because the process of assigning school principals to schools greatly reduces endogeneity concerns: school principals are assigned through a process mostly based on seniority. To evaluate the role of school principals in attenuating the test score costs of austerity, we first classify high(low)-skill school principals as those with at least (less than) a master degree. We then run separate regressions according to school principals' skill level. Since our principal-level data is a repeated cross-section and we have just a few cases where a municipality appears multiple times in the data, we use province, instead of municipality, fixed effects (the other baseline controls remain the same as in the other specifications). Our results show that marginal test score impacts of austerity cuts are much larger in schools managed by less skilled principals. Motivated by these heterogeneous marginal test score impacts, we will investigate the role played by local politicians and school principals in section 5.1.

4.3 Robustness and Threats to Identification

4.3.1 Manipulation of Municipal Population

One concern is that the population threshold that determines austerity exposure could be manipulated by municipalities. To check for this possibility, we perform a McCrary test for discontinuity in population size at the population threshold determining eligibility for the DSP. Figure A5 shows that the density of the municipality population is smooth around the threshold; the McCrary discontinuity estimate is 0.032 (0.190). We also perform this exercise in a dynamic way by running the McCrary test for each birth cohort separately (see Figure A6). We find no evidence of a significant difference in the

spending on student achievement are not correlated with baseline spending level.

population distribution around the cutoff.

4.3.2 Alternative Measures of Austerity Exposure and Dosage

Our exposure definition implicitly imposes a linearity assumption: the spending change generated by 2 years of exposure will be almost half of the spending change induced by 4 years of exposure. To assuage concerns related to non-linear effects, we test the sensitivity of our estimates to alternative measures of austerity exposure in Appendix Table A6. First, we allow for non-linearity in exposure effects by adding the square of exposure and its interaction with dosage (column 2). Second, we use a binary measure of exposure (computed according to whether a municipality's value is above or below the median value) rather than a continuous measure (column 3). Third, we interact exposure with dummies for each quintile of budget rigidity intensity (column 4). Finally, we interact exposure directly with the budget rigidity intensity variable (column 5). While the interpretation of the first-stage effect differs across these specifications, we find that regardless of how we measure exposure, austerity had a negative effect on public spending, and the resulting effects on test scores are fairly similar to our baseline estimates.

4.3.3 Sensitivity to Sample Selection

Our baseline estimates refer to the population of primary school students that are located in municipalities with less than 30,000 inhabitants. In Appendix Figure A7 and Figure A8, we test whether our results are sensitive to population bandwidth selection by estimating our baseline model on different upper and lower population limits. The figures show that our coefficient estimates are remarkably similar when using a reasonable range of bandwidths. Furthermore, because large-size municipalities have the same weight as small-size municipalities, we wonder if our results are driven by very small-size municipalities that would not be representative of the Italian population as a whole. To this aim, we also estimate our baseline specification using population weights, where population refers to the average municipal population observed over the period covered in our analysis. Appendix Table A7 shows that coefficients obtained from weighted 2SLS specifications are similar to our baseline results, although less precisely estimated in some specifications.

4.3.4 Measurement Error in Determining Municipality of Residence

One limitation of our data is that we are able to observe the municipality where a school is located, but not pupil residence. This implies that we could have a measurement error in assigning public spending across students. Whether this measurement error leads to attenuation bias is not a priori obvious, since it depends on whether it is correlated with spending changes across municipalities. This issue, however, should be limited to just a small portion of our sample. In Italy, students are assigned to pri-

mary schools according to municipality of residence. Parents enroll children in schools, while school principals are responsible for verifying that students are residents of the same municipality where the school is located (or in the same zip code in the case of multiple schools within a municipality). Therefore, student municipality of residence mostly maps into school municipality of residence.

However, there could be cases, especially in small-size municipalities, where there is no primary school and children need to attend schools located in neighboring municipalities. Since Italy has a relatively large concentration of small-size municipalities, we take this issue to heart. To test the sensitivity of our estimates to this issue, we proceed in the following way. First, we identify municipalities where there is no primary school. Second, we flag all the municipalities sharing a border with that municipality, which could be affected by an inflow of students whose spending is measured with errors. Finally, we estimate our 2SLS model from a sample without these flagged municipalities. We report coefficient estimates in Appendix Table A8. The table shows that the measurement error in determining municipality of residence is not a significant source of bias: estimates obtained from the original sample are fairly similar to those computed from the sample that excluded municipalities that might have been exposed to an inflow of students.

4.3.5 Spatial Correlation in the Error Term

Following the suggestions by Angrist and Pischke (2009) to "pass the buck up one level", we account for the possibility of spatial correlation in the error term by clustering the standard errors on a higher level of aggregation, which in our case is the province. Figure A9 shows that our estimates remain statistically significant at usual confidence intervals when we use standard errors that are clustered at school, municipality (our baseline), or province level. An additional concern is that the error term could be correlated not only over time within the panel dimension, but also across cohorts taking the test in the same year. To account for this issue, we also present standard errors that allow for two-way clustering by cohort and school or municipality (using the estimator proposed by Cameron et al. 2011). Despite coefficient estimates being less precise when we implement the two-way clustering strategy, they remain statistically significant at conventional confidence intervals.

5 Mechanisms

5.1 Spending Categories

In a seminal work on the effect of fiscal consolidation policies, Alesina and Perotti (1995) convey a clear message: the composition of the fiscal adjustment is of fundamental importance in determining its impact. Disaggregating the austerity spending cut on various expenditures is thus key for evaluating the mechanisms behind the

documented test score impacts. The impact of austerity on test scores could either be mitigated or exacerbated depending on whether municipalities targeted the marginal spending cut toward productive spending items.

We examine how different spending categories are reduced in response to austerity-induced expenditure cuts. We regress the level of per-pupil spending in each budget category on total (instrumented) per-pupil spending, separately for current and capital expenditures.[22] This analysis offers a more thorough evaluation of the first-stage relationship between austerity and public spending.

Table 4 shows the results using our preferred 2SLS model (including school FE, cohort FE and both student-level and municipality-level controls). Each coefficient should be interpreted as the marginal propensity to spend in each budget category: how much spending is reduced in a budget category for each euro in austerity-induced per-pupil spending cuts.[23] Using this specification, we can formally test whether marginal and average spending propensity are similar. For each budget item, the table also reports the spending share, the mean value (in thousand euros), and the p-value testing whether the marginal and average propensity to spend in each budget category are equal. We also report sharpened false discovery rate q-values to account for false discovery rates due to multiple testing hypotheses.[24]

We find that austerity spending cuts are allocated differently than the average spending on both current and capital expenditures. Focusing on *current* spending, our results show that municipalities cut disproportionally less spending on administration. While current administrative spending represents the lion's share of municipal current expenditures (57 percent of total current spending), municipalities decrease administrative spending by around 0.23 euros for each euro in austerity-induced spending cuts. This is consistent with the notion that administration is a rigid source of spending, being mostly composed of labor payoffs. By contrast, we find that austerity leads to disproportionally cut current spending on culture, development, services, and public transportation. Specifically, the spending cut on culture, which comprises funding to public libraries, museums, art galleries and theatres, accounts for around 7.7 percent of the total spending cut, despite municipalities allocate around 4.8 percent of total spending to this budget item (p-value from a statistical test of difference between marginal and average spending cut is 0.070). Cuts in services bloated after austerity: we find that municipalities decrease expenditure on public services by nearly 0.342 euros for each euro in austerity-induced spending cuts, despite this budget category representing a small portion of the total budget (1.5 percent). This budget category includes expenditures on public lighting, heating and other services produced and

[22] Similar approaches have been proposed in Hyman (2017) and Jackson et al. (2021).

[23] For the sake of a clearer interpretation of marginal propensity to spend, we report the results using the *level* of spending. We find similar results using the log of spending.

[24] The false discovery rate q-values are the expected proportion of rejections that are type I errors (false rejections). We compute them by following the procedure described in Anderson (2008).

Table 4: The Impact of Austerity by Budget Category

	Budget category:							
	Adm	Edu	Cul	Spo	Tou	Dev	Ser	Roa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Current Sp.	0.226***	0.088**	0.077***	0.014	0.017**	0.043***	0.342***	0.192***
	(0.049)	(0.035)	(0.019)	(0.010)	(0.009)	(0.014)	(0.063)	(0.033)
Mean share	0.570	0.182	0.045	0.030	0.010	0.013	0.015	0.136
Mean dependent	26.840	8.478	2.213	1.474	0.655	0.640	0.942	6.691
p-value	0.000	0.008	0.098	0.125	0.390	0.031	0.000	0.083
FDR q-value	0.001	0.022	0.131	0.143	0.390	0.062	0.001	0.131
Capital Sp.	0.362***	0.396***	0.113**	-0.072	0.021	0.041**	-0.000	0.140
	(0.174)	(0.120)	(0.054)	(0.122)	(0.020)	(0.020)	(0.029)	(0.100)
Mean share	0.247	0.186	0.048	0.074	0.013	0.068	0.020	0.344
Mean dependent	4.864	3.221	0.969	1.432	0.383	1.577	0.506	6.466
p-value	0.187	0.074	0.227	0.231	0.696	0.185	0.476	0.041
FDR q-value	0.308	0.096	0.308	0.308	0.696	0.308	0.544	0.076
# of students	962,897							
# of schools	2,681							
# of municipalities	2,274							
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table examines how different municipal budget categories are reduced in response to austerity-induced spending cuts. It reports 2SLS estimates on the effect of austerity-induced total public spending changes on changes in each budget category. Each coefficient should be interpreted as the marginal propensity to spend in each budget category. Each specification includes school fixed effects, cohort fixed effects, student-level controls, and municipality-level controls. For each budget category, we report its share of total spending, the mean value (in per-pupil 1,000 euros), the p-value testing whether the marginal and average propensity to spend in each budget category are equal, and the sharpened false discovery rate q-values to account for false discovery rates due to multiple testing hypotheses. Standard errors clustered at municipality level reported in parentheses.

sold directly by a municipality.[25] Finally, although we find a statistically significant decrease in education spending (around 8.8 percent of the total spending cut), the marginal spending cut on current educational spending is relatively lower than the average spending.

Focusing on *capital* expenditures, we find that austerity leads to a disproportional cut in school investments. Our results show that municipalities allotted almost 40 percent of the total cut for school funding, compared to 18 percent of the average spending (p-value from a statistical test of difference between marginal and average spending cut is 0.074). This disproportionate reduction in capital school projects is consistent with evidence from school-level data that we will provide below. Austerity also forced municipalities to disproportionally cut investments in culture (by 11 percent against an average spending share of around 5 percent) and administration (by nearly 36 percent against an average spending of nearly one-fourth of total capital spending). By contrast, austerity is relatively less harmful for local public transportation.

Taken together, one view of these results is that austerity leads municipalities to allocate spending cuts toward less rigid budget items, where politicians have discretion to decide how much of public spending to allocate. How local policy makers allocate austerity spending cuts can be an important determinant of student outcomes. In theory, more skilled politicians could be better at making the government machinery more efficient. For instance, more skilled politicians might be more likely to reduce current administrative expenditures, characterized by large passive waste in Italy (Bandiera et al. 2009), instead of capital expenditures.[26] Does the allocation of the austerity-induced spending cut vary according to local politician ability?

To shed light on this question, Table 5 presents effects separately according to local politician skill level. We first compute a municipality-specific index of local politician skill level by calculating the average years of schooling of town council components. We then classify a municipality as managed by high (low)-skill politicians if local politician average years of schooling are larger (lower or equal) than the median value.

Our results show that the marginal spending cut on administration is more than two times larger in municipalities led by high-skill politicians (30.1 percent against 14.3 percent of the cut). Because administrative spending is likely to reflect many sources of unproductive spending, this result would suggest that high-skill politicians are more able to allocate the marginal spending cut toward less productive spending categories. Accordingly, low-skill politicians present a relatively larger propensity to earmark school capital expenditures and current expenditures on culture and ser-

[25] It is possible that the large austerity-induced reduction in this budget category reflects outsourcing: in order to spur efficiency and reduce public debt, municipalities contracted out the distribution of some public services to private companies.

[26] For example, Gagliarducci and Nannicini (2013) find that politicians with more years of schooling are more likely to reduce the amount of personnel and other current expenditures (by about 11 and 22 percent, respectively). They also show that more skilled mayors make town bureaucratic organizations more efficient.

Table 5: Austerity-Induced Budget Change By Politicians' Ability

	Budget category:							
	Adm	Edu	Cul	Spo	Tou	Dev	Ser	Roa
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	A. Low-skill politicians:							
Current Sp.	0.143*	0.075**	0.084***	0.030**	0.017	0.037*	0.411***	0.203***
	(0.080)	(0.037)	(0.029)	(0.015)	(0.013)	(0.020)	(0.091)	(0.051)
Mean share	0.573	0.187	0.041	0.030	0.008	0.011	0.014	0.136
Mean dep.	26.875	8.684	1.983	1.453	0.620	0.561	0.880	6.719
p-value	0.000	0.003	0.094	0.793	0.302	0.175	0.000	0.188
FDR q-value	0.001	0.008	0.188	0.793	0.346	0.251	0.001	0.251
Capital Sp.	0.131	0.625***	0.086	0.096	-0.011	0.026	0.046	0.082
	(0.202)	(0.205)	(0.058)	(0.111)	(0.045)	(0.024)	(0.041)	(0.124)
Mean share	0.235	0.198	0.042	0.076	0.013	0.066	0.020	0.350
Mean dependent	4.754	3.506	0.870	1.477	0.413	1.547	0.474	6.740
p-value	0.607	0.074	0.447	0.860	0.592	0.103	0.517	0.031
FDR q-value	0.694	0.175	0.694	0.860	0.694	0.275	0.694	0.148
# of students	480,186							
# of schools	1,406							
# of municipalities	1,157							
	B. High-skill politicians:							
Current Sp.	0.301***	0.135**	0.052	0.006	0.013	0.064***	0.236**	0.193***
	(0.081)	(0.058)	(0.035)	(0.016)	(0.014)	(0.024)	(0.102)	(0.054)
Mean share	0.567	0.176	0.049	0.031	0.011	0.014	0.016	0.137
Mean dependent	26.766	8.242	2.446	1.490	0.689	0.719	0.981	6.655
p-value	0.001	0.485	0.928	0.124	0.888	0.040	0.031	0.302
FDR q-value	0.008	0.647	0.928	0.248	0.928	0.107	0.107	0.484
Capital Sp.	0.427*	0.294	0.157	-0.327	0.064	0.082	-0.064	0.265
	(0.221)	(0.227)	(0.133)	(0.538)	(0.060)	(0.065)	(0.117)	(0.229)
Mean share	0.258	0.174	0.053	0.073	0.014	0.071	0.020	0.338
Mean dependent	4.973	2.938	1.067	1.387	0.354	1.606	0.538	6.193
p-value	0.635	0.347	0.434	0.457	0.410	0.865	0.475	0.749
FDR q-value	0.847	0.760	0.744	0.754	0.723	0.865	0.712	0.856
# of students	482,711							
# of schools	1,275							
# of municipalities	1,117							

Note: This table examines how different municipal budget categories are reduced in response to austerity-induced spending cuts for municipalities with high- versus low-skill politicians. We first compute a municipality-specific index of politicians' skill level by calculating the average years of schooling of town council members. We then classify a municipality as managed by high (low)-skill politicians if local politicians' average years of schooling are larger (lower or equal) than the median value. The table reports 2SLS estimates on the effect of austerity spending changes on variation in each budget category. Each coefficient should be interpreted as the marginal propensity to spend in each budget category. Each specification includes school fixed effects, cohort fixed effects, student-level controls, and municipality-level controls. For each budget category, we report its share of total spending, the mean value (in per-pupil 1,000 euros), the p-value testing whether the marginal and average propensity to spend in each budget category are equal, and the sharpened false discovery rate q-values. Standard errors clustered at municipality level reported in parentheses.

vices. Specifically, we find that more than 60 percent of the austerity capital spending cut is allocated to school spending in municipalities governed by low-skill politicians, while this fraction almost halves when high-skill politicians allocate spending. Such discrepancies in the allocation of the austerity spending cut based on the skill level of local policy makers are consistent with the heterogeneity shown in Figure 5. Because, ceteris paribus, high-skill politicians are more able to allocate the austerity spending cut toward less productive spending categories, test score impacts are then relatively lower for students located in municipalities governed by high-skill politicians.

5.2 Zooming in on Educational Spending

There is ample descriptive and anecdotal evidence that the supply of municipal school resources has not kept up with the increase in the demand over the last decade in Italy (see, e.g., OECD, *Education at a Glance 2020*). According to Antonini et al. (2015), more than one-third of school buildings needs urgent maintenance. Moreover, school buildings are quite old and obsolete: more than one-third of the school buildings' stock was built before the 1960s (see Ministry of Education and Research (MIUR), *Anagrafica Edilizia Scolastica website*).

In this section, we combine various sources of data to explore the effect of austerity spending cuts on the availability of several school resources and inputs. The analysis proceeds in two steps. First, using municipality-level balance sheets data, we explore whether austerity affects early childcare funding and attendance. Second, we study the effect of austerity spending cuts on several school inputs and resources by using school-level administrative data on school facilities and survey information on a stratified sample of teachers and school principals.

5.2.1 Early Childcare Funding and Attendance

The test score impacts that we measure at the end of primary school are the product of years of cognitive and behavioral development. It is possible that achievement losses due to austerity are in part the product of limited interventions at an early (pre-school) stage.[27] This channel might be particularly important in Italy, where childcare provisions are among the lowest in Europe and the number of applications for accessing childcare services largely exceeds the number of places in almost all municipalities (Dipartimento Per Le Politiche Della Famiglia 2020). Since municipalities are responsible for childcare policies, budget constraints can influence municipality decisions about

[27] Whether nursery school attendance is beneficial to children in the middle and long run is not a priori obvious. Previous studies focusing on Italy have provided mixed evidence. Fort et al. (2020) provide clear evidence that additional nursery school attendance at age 0–2 significantly reduces test scores at age 8-14. Carta and Rizzica (2018) do not find any significant test score impact from early access to pre-primary school by exploiting an Italian reform in mid-2000s. Brilli et al. (2016) find that childcare availability has a positive effect on students' cognitive outcomes. Corazzini et al. (2021) find heterogeneous effect by immigrant status. While immigrant students gain from early childcare attendance, native students cognitive outcomes are negatively affected.

fees and the number of available slots.

Table 6: The Impact of Austerity on Early Childcare Funding and Attendance

	Outcome variable:					
	Fees per-pupil (1,000€)	Staff pupil ratio	Instructional staff	Non-instructional staff	Coverage (0/1)	Attendance (0/1)
	(1)	(2)	(3)	(4)	(5)	(6)
Spending (1,000€)	-0.126***	0.018	0.018	0.000	0.002	-0.001
	(0.039)	(0.013)	(0.013)	(0.000)	(0.002)	(0.001)
# of students	962,897	962,897	962,897	962,897	962,897	818,064
# of schools	2,681	2,681	2,681	2,681	2,681	2,681
# of municipalities	2,274	2,274	2,274	2,274	2,274	2,274
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent	2.726	0.154	0.126	0.028	0.404	0.263
FDR q-value	0.006	0.314	0.314	0.479	0.399	0.503

Note: This table reports 2SLS estimates on the effect of austerity-induced public spending changes on the following outcome variables: yearly fee charged for attending early childcare (column 1); count of per-pupil workers employed in early-childcare services (column 2); count of per-pupil instructional workers employed in early childcare services (column 3); count of per-pupil non-instructional workers employed in early childcare services (column 4); share of accepted applications (column 5); early childcare attendance, as reported in student-level survey data from INVALSI (column 6). Each specification includes school fixed effects, cohort fixed effects, student-level controls and municipality-level controls. Standard errors clustered at municipality level reported in parentheses.

We study the effect of austerity on the Italian *asilo nido*: an early childcare service for 0-3 age children directly financed by municipalities. From balance sheets data, we first collect municipality-level cohort-varying information on yearly fees,[28] staff-student ratio (further divided in instructional and non-instructional staff), and the share of accepted applications.

We regress each outcome variable on total (instrumented) per-pupil spending. To simplify coefficient interpretations, we focus on total spending levels aggregating both current and capital spending. Table 6 shows the results using our preferred 2SLS model (including school FE, cohort FE and both student-level and municipality-level controls). We also report test statistics corrected for multiple testing hypotheses. Our key result is that austerity leads municipalities to increase early childcare fees. Specifically, the coefficient estimate shown in column (1) implies that a 1 euro decrease in per-pupil spending raises childcare fees by 0.126 euros. This maps into a fairly large effect: since the average yearly fee is 2,726 euros, our estimate suggests that an additional year of exposure to austerity (which implies, on average, a spending reduction of 412 per-pupil euros) would raise fees by around 2 percent, which is equal to charging 52 euros more for each additional year of austerity exposure.

Columns 2-4 show that austerity spending cuts do not significantly affect hiring of both instructional and non-instructional staff. This result is in line with the evidence presented above, suggesting that austerity cuts do not harm rigid source of spending, such as personnel costs.

Do austerity spending cuts (and the resulting price increase in early childcare pro-

[28]Childcare fees are modulated according to household characteristics (e.g., family composition, parents' working status). In balance sheets data, we observe the average per-pupil yearly fee.

visions) affect early childcare attendance? According to a survey conducted by ISTAT (2018), household budget constraints are a non-negligible factor determining early childcare attendance: more than 12 percent of parents do not utilize childcare services because they cannot bear the cost. Using municipality-level data on the share of accepted applications, we uncover a small positive effect on coverage, suggesting that a 1,000 per-pupil euros decrease in spending would reduce the share of accepted applications by 0.2 percentage points (column 5). However, this effect is not statistically significant at usual confidence intervals. To further investigate this channel, we also measure the early childcare attendance rate from pupil-level survey responses (provided by INVALSI). Although the response rate is not perfect (we are able to cover around 85 percent of students in our sample), the result confirms the municipality-level evidence: the probability of attending early childcare is not significantly associated with austerity-induced spending changes (column 6).

Overall, we conclude that some of the austerity costs are passed on to parents via higher early childcare costs. Although the price increase does not seem to significantly affect early childcare attendance, it is possible that test score impacts are influenced by other price increase-related channels. For instance, a raise in prices might reflect outsourcing of early childcare service provisions to private businesses, which usually set higher prices.[29]

5.2.2 Primary School Resources and Facilities

The results presented in Table 4 show that austerity spending cuts disproportionally hit school capital spending. Recent studies have shown that school facility investments improve student outcomes (see, e.g., Duflo 2001; Cellini et al. 2010; Aaronson and Mazumder 2011; Neilson and Zimmerman 2014; Lafortune et al. 2018; Lafortune and Schonholzer 2019; Jackson and Mackevicius 2021), especially in contexts where school facilities were in very poor conditions. To study the impact of austerity cuts on school inputs and facilities, we gather information on various school inputs, resources and facilities. We then regress each outcome variable on total (instrumented) per-pupil spending. To facilitate interpretations, we aggregate current and capital spending and we present the results for spending level.

We report 2SLS coefficient estimates from our baseline specification in Table 7. We focus on three main categories of school inputs. First, since austerity cuts could have prevented municipalities from financing renovation, repairs and expansions of school buildings, we examine whether austerity leads to overcrowded classrooms. We find that austerity spending cuts leads to a statistically significant, but economically small, increase in class size. On average, we find that a 1,000 euros per-pupil reduction in spending raises class sizes by nearly 0.048 students. This estimate suggests that the

[29]Municipalities have outsourced over the recent years to meet DSP requirements and reduce public expenditures. According to ISTAT (2018), spending for early childcare managed by non-public agents covers around 10 percent of total spending.

Table 7: The Impact of Austerity on Primary School Resources

	Outcome variable:					
	Class size (#) (1)	Schedule (0/1) (2)	Lab (factor) (3)	Library & gym (factor) (4)	Teaching items (factor) (5)	Tech items (factor) (6)
Spending (1,000€)	-0.048**	-0.002	0.008***	0.005**	0.000	0.002
	(0.024)	(0.003)	(0.003)	(0.002)	(0.002)	(0.001)
# of students	962,897	962,897	128,998	128,998	128,998	128,998
# of schools	2,681	2,681	2,681	1,344	1,344	1,344
# of municipalities	2,274	2,274	1,221	1,221	1,221	1,221
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent	15.782	0.318	-0.000	-0.000	0.000	-0.000
FDR q-value	0.098	0.521	0.012	0.042	0.970	0.282

Note: This table reports 2SLS estimates on the effect of austerity-induced per-pupil spending changes on the following outcome variables: class size (column 1); whether the school provides a longer schedule (column 2); a factor variable capturing whether the school is equipped with functioning computer, language, scientific, musical, technical or art laboratories (column 3); a factor variable capturing whether the school has a functioning library, book loan services and a gym (column 4); a factor variable capturing availability of teaching tools, such as teaching items, books, audiovisual tools and interactive blackboards (column 5); a factor variable capturing availability of technological items, such as software, tablets, a functioning connection to the internet and computers (column 6). The factor variables are computed by applying a principal component analysis and using survey responses from school principals. Specifications in (1) and (2) include school fixed effects, cohort fixed effects, student-level controls, and municipality-level controls. Specifications in (3)-(6) include province fixed effects (instead of school fixed effects). Standard errors clustered at municipality level reported in parentheses.

implied class size effects from austerity spending cuts is a 1.1 percent (0.168 students) class size increase.[30] Given existing estimates on the relationship between test scores and class size (Chetty et al. 2011), the austerity-induced class size effects would map into a 0.00114 standard deviations decrease in test scores.[31] This effect accounts for nearly 5 percent of the total test score impact of austerity. We thus conclude that the impact of austerity cuts through larger class sizes has a negligible effect on test scores.

Second, because the reduction in current school spending could have been due to the hiring of fewer non-instructional staff (e.g., laboratory technicians, janitors), we look at whether instrumented per-pupil spending predicts the availability of out-of-hours reception of pupils before or after school timetable. In the INVALSI dataset, we observe whether a school allows students to opt for a school schedule that is longer than the standard 27-hour timetable. We find negative, but imprecisely estimated, effects, suggesting that austerity spending cuts had no clear effects on the possibility for schools to provide a longer schedule.

Third, we use survey data from school principals to elicit information on availability (and proper functioning if available) of library, gym, laboratories (including musical, art and linguistic), teaching tools (e.g., interactive blackboard, audiovisual tools) and technological items (computers, internet connection and tablets). The presence

[30] This is computed by multiplying the average exposure (8.485 years) by the average spending reduction (summing current and capital expenditures) from an additional year of austerity (0.412 thousand euros) and by the class size impacts of austerity cuts (0.048 students).

[31] Leveraging experimental variation from Project STAR, Chetty et al. (2011) find that students randomly assigned to a small class size (15 students) have 4.76 percent of a standard deviation higher test scores (column (1) of Table 5) compared to students assigned to a large class size (22 students).

of art and musical laboratories can encourage student initiative, creativity, and interdisciplinarity, perhaps spurring both cognitive and non-cognitive abilities. It can also support students who wish to develop their own ideas and require a practical learning space. Since, as we showed before, austerity was particularly harmful for students with limited resources at home and severely reduced funding to cultural activities, the availability at school of this kind of inputs might help to mitigate the test score effects of austerity cuts.

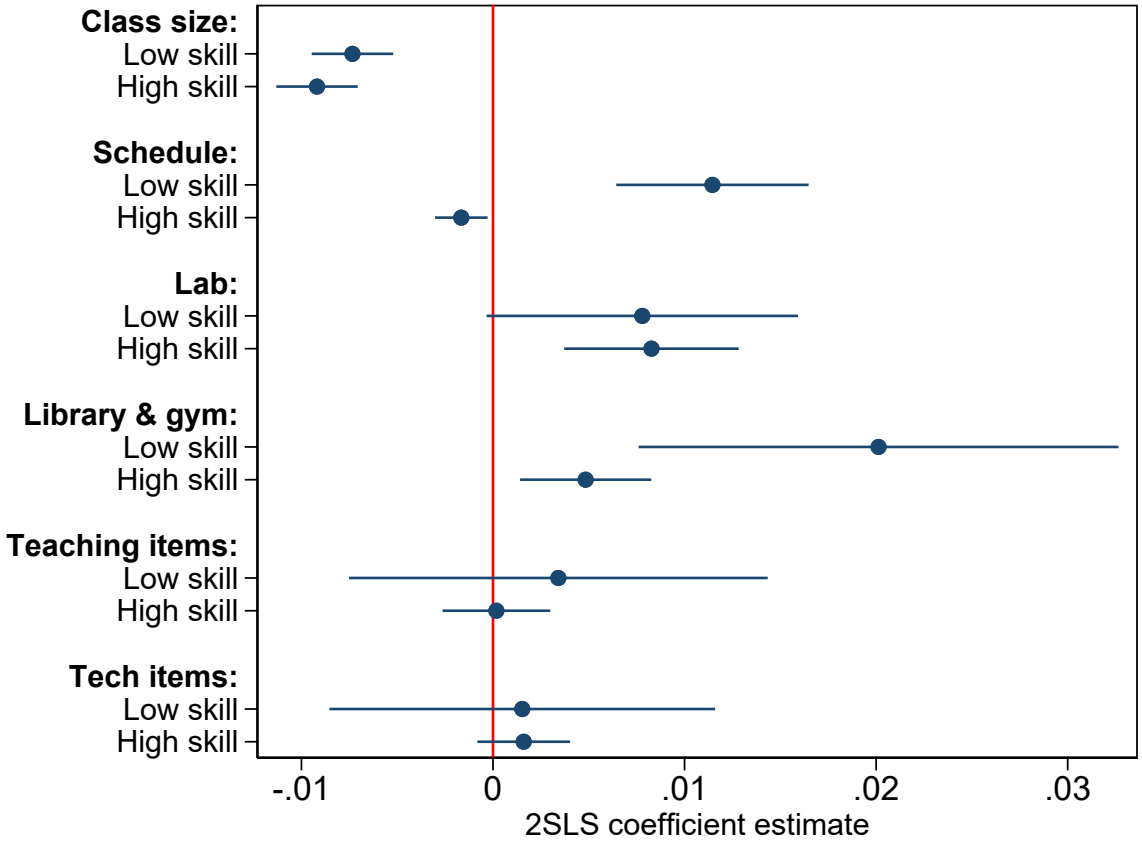
In this analysis, we make use of province fixed effects, rather than municipality fixed effects, because the panel dimension is severely limited (just a few schools are interviewed for two different years). Although representative by construction, our final sample is relatively smaller than our main sample, being composed of 128,998 students and 1,221 municipalities. We summarize all the information gathered from survey data in four indexes: i. laboratories; ii. library and gym; iii. teaching items; iv. technological items. Each factor variable has mean zero and standard deviation equal to one.

We find that austerity spending cuts had a negative effect on availability (and proper functioning) of laboratories, library, gym and technological items, while we do not find any significant effect on teaching tools. On average, the probability of utilizing laboratories (libraries and gym) would decrease by 2.8 (1.8) percent of a standard deviation in cohorts with average austerity exposure, compared to never exposed cohorts. For technological items (e.g., internet connections, computer, tablets available at school), the effect is lower (around 0.7 percent of a standard deviation) and not statistically significant at usual confidence intervals.

Overall, these results provide a clear conclusion: austerity spending cuts significantly affect school resources and facilities. As we showed in section 4.2, the marginal test score impacts from austerity spending cuts are relatively lower in school managed by skilled school principals. One explanation for this heterogeneity is that skilled school principals are more able to mitigate impacts from declining spending by reallocating resources from low to high productive items. To shed light on this channel, we first classify high(low)-skill school principals as those with at least (less than) a master degree. We then run separate regressions according to school principals' skill level.

Figure 6 displays coefficient estimates and 95 percent confidence intervals on the impact of austerity spending cuts on school resources by school principal ability. In schools managed by low-skill school principals, we find that austerity spending cuts affect the provision of a longer school schedule and further exacerbate the effects on the availability of libraries and gyms. The effect on other school resources does not significantly vary by school principal ability. We interpret this result as complementary with the heterogenous responses by local politician ability that we showed above. Taken together, these heterogeneous marginal impacts from austerity cuts suggest that school principal and politician ability are relevant. With the caveats discussed above in mind, our suggested explanation is that skilled politicians and school principals are

Figure 6: The Impact of Austerity on Primary School Resources By Principal Ability



Note: This figure depicts 2SLS coefficient estimates and 95 percent confidence intervals on the test score impact of austerity spending cuts by school principal ability. Low-(high-)skill school principals are those with less than (at least) a master degree. Each specification includes province fixed effects, cohort fixed effects, student-level controls (gender, nationality, quarter of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of the mayor and average values for the other town council members and election year-cohort fixed effects). For graphical purposes, the estimates for class size are divided by 10.

more able to allocate the marginal spending cut towards less productive spending categories, mitigating the adverse test score effects of austerity.

6 Conclusions

The welfare state has come under attack from austerity policies in the aftermath of the financial crisis. Calls to roll back public spending have been especially forceful in Europe, where the welfare state has traditionally played a major role. While the agenda of austerity reforms is to make the welfare state leaner and more efficient, austerity might also undermine the ability of governments to finance public expenditures. Understanding if and how much austerity cuts can hurt socio-economic outcomes is of key societal importance for evaluating the overall effects of austerity policies.

In this paper, we study whether austerity spending cuts harmed student cognitive abilities, as measured in fifth-grade standardized test scores. Our context is Italy, which

offers data and quasi-experimental variation that allow us to estimate the test score impacts of austerity spending cuts. We find that the overall impact of austerity spending cuts reduced test scores by 10.2 percent in math and 9 percent in reading. The effects are more pronounced for children with limited resources at home, suggesting that austerity loosens substitutability between private and public resources.

We want to caution against an overinterpretation of our findings for evaluating austerity policies. The ultimate impact of austerity policies depends on the *form* of policy changes. In this paper, we are able to evaluate a specific form of austerity policies: reductions in productive spending items. It is conceivable that there may be alternative forms of austerity policies that could have positive or zero impacts on student outcomes. Our heterogeneity analysis suggests that austerity policies are likely to be substantially less detrimental in contexts with skilled politicians and school principals, who are more able to allocate the marginal spending cut towards less productive spending categories. This result implies that the composition of a fiscal adjustment is of fundamental importance in determining its impacts: an implication that ties with the earlier results emphasized by Alesina and Perotti (1995).

The ultimate impact of austerity policies has significant implications for our society. Our findings suggest that the potential public finance benefits from austerity policies need to be evaluated vis-à-vis with their economic and social costs. The relevance of austerity cuts on child cognitive ability at this early stage calls for a better understanding of whether impacts will persist later on in life, with important implications for the persistence of inequality and social immobility.

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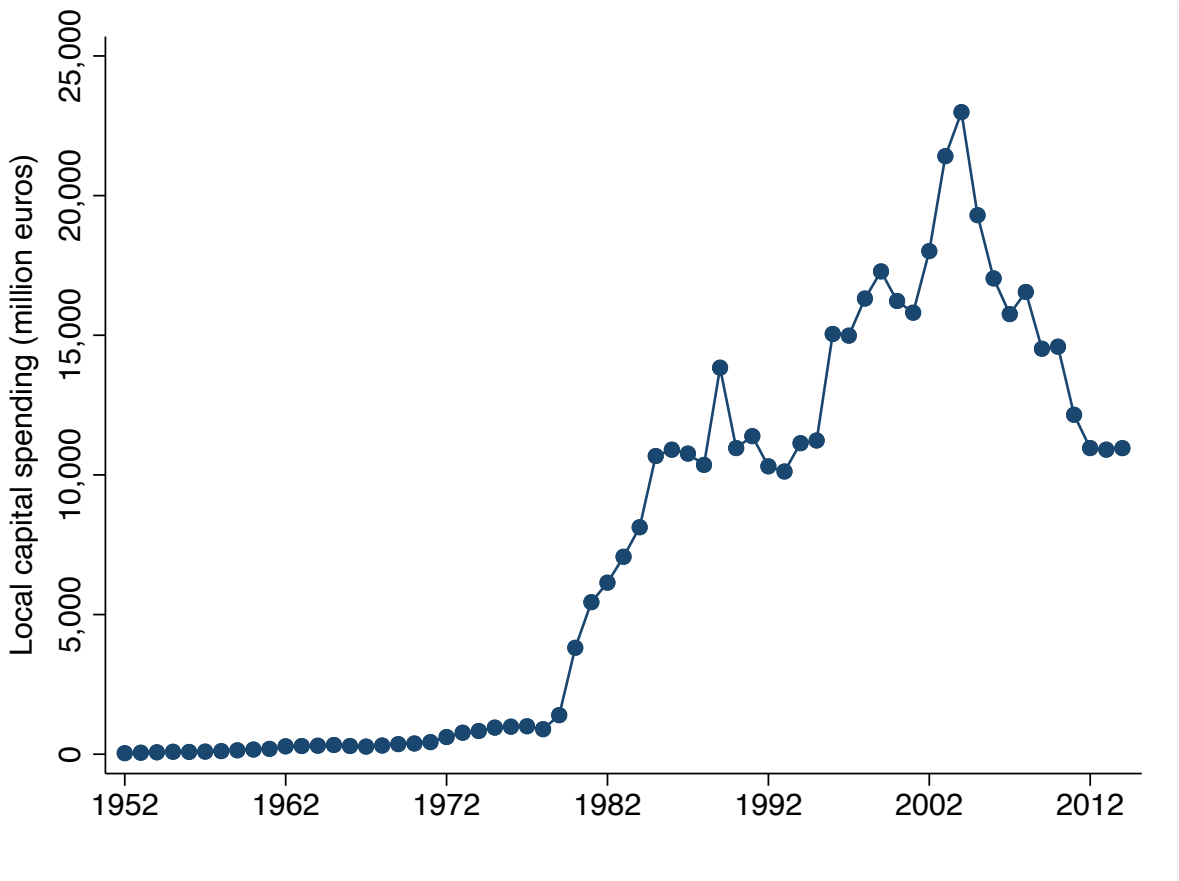
Appendices

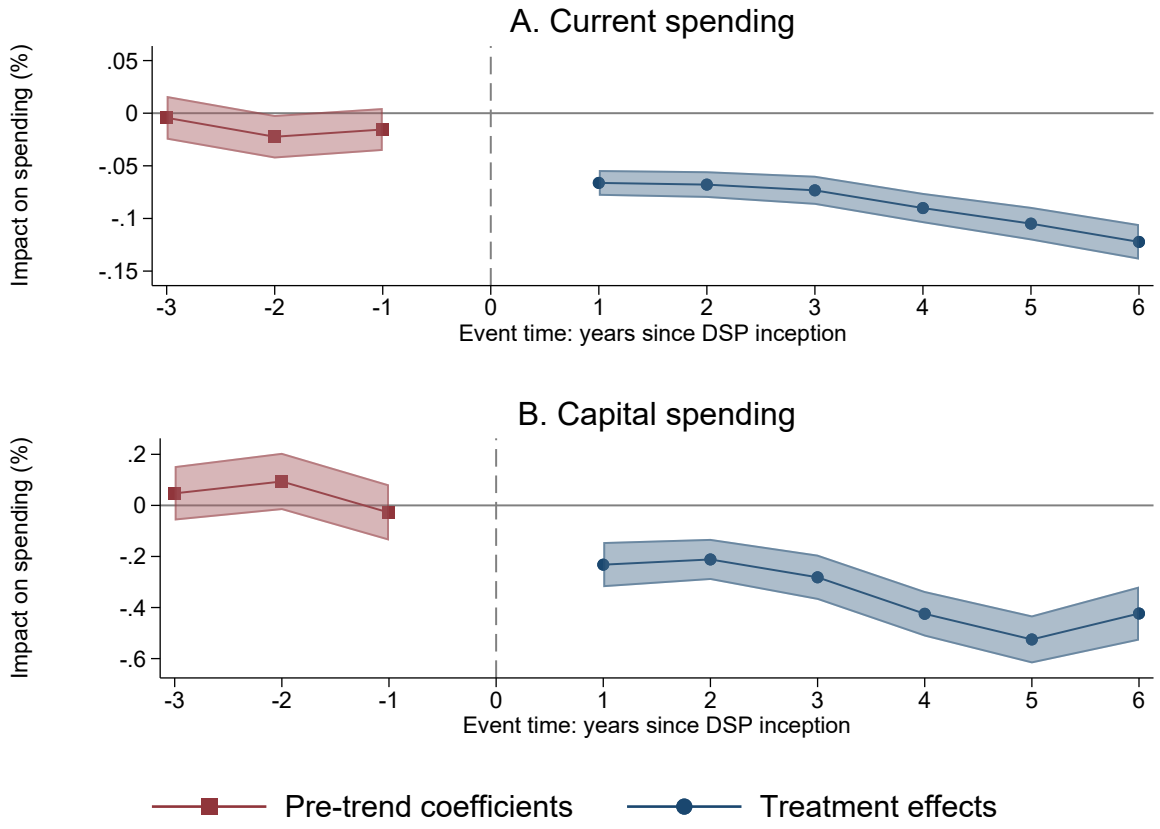
A Additional Tables and Figures

Figure A1: Historical Evolution of Local Capital Spending



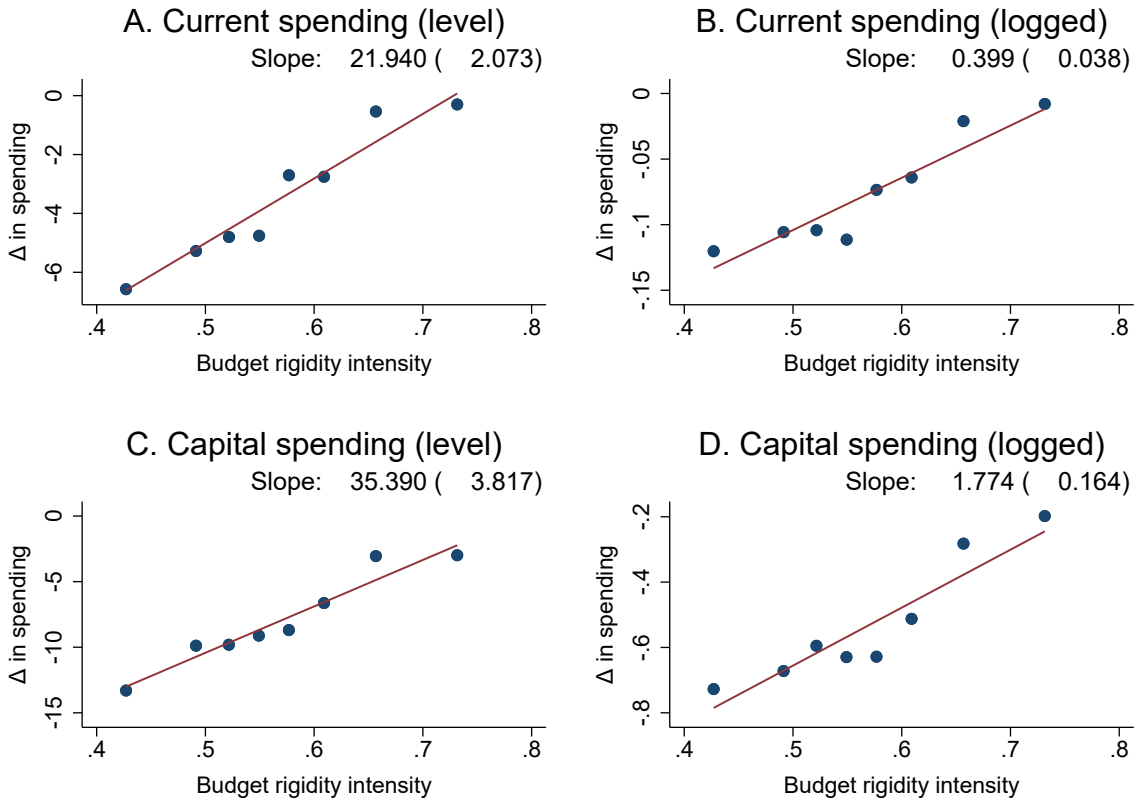
Note: The figure displays the evolution in the total capital expenditures (in 2014 million euros) financed by municipalities over the 1952-2014 period. The figure shows that municipalities spent around 5,000 million euros during the early post-war period. Starting from the 1980s, local capital expenditures spectacularly increased and surged in the early 2000s. Then, starting exactly from 2005, capital expenditures began to dramatically decline. In 2014 (the final data point in this dataset), capital expenditures returned to early 1980s levels, dissipating more than 30 years of local investments. Data from the Italian Institute of Statistics.

Figure A2: Event study with staggered treatment adoption



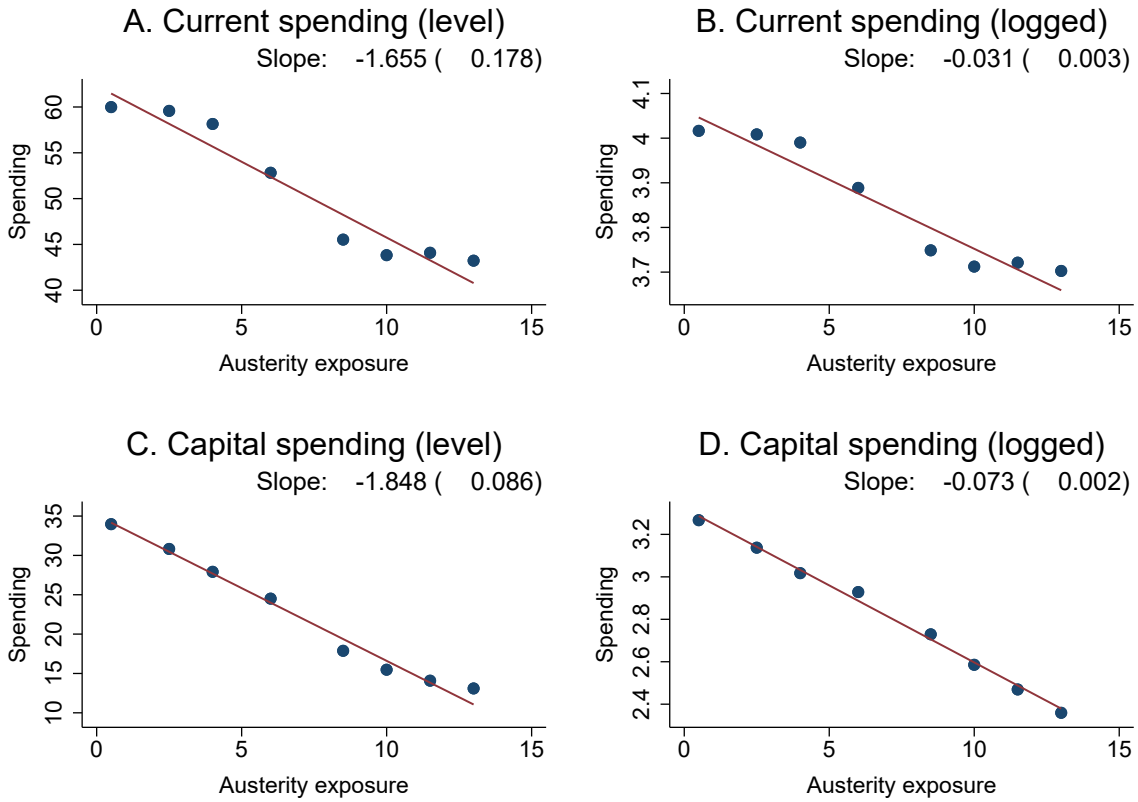
Note: The figure shows coefficient estimate and 95 percent confidence intervals obtained by applying the Borusyak et al. (2021) methodology. The top panel present the effect of the DSP on the log of current spending per-capita; the bottom panel on the log of capital spending per-capital. The impact of DSP inception on public spending is computed by exploiting the staggered implementation of the DSP across municipalities based on their population. Each specification includes municipality fixed effects, cohort fixed effects, and municipality-specific time-varying controls. Standard errors clustered at municipality level.

Figure A3: Austerity-Induced Spending Change and Budget Rigidity



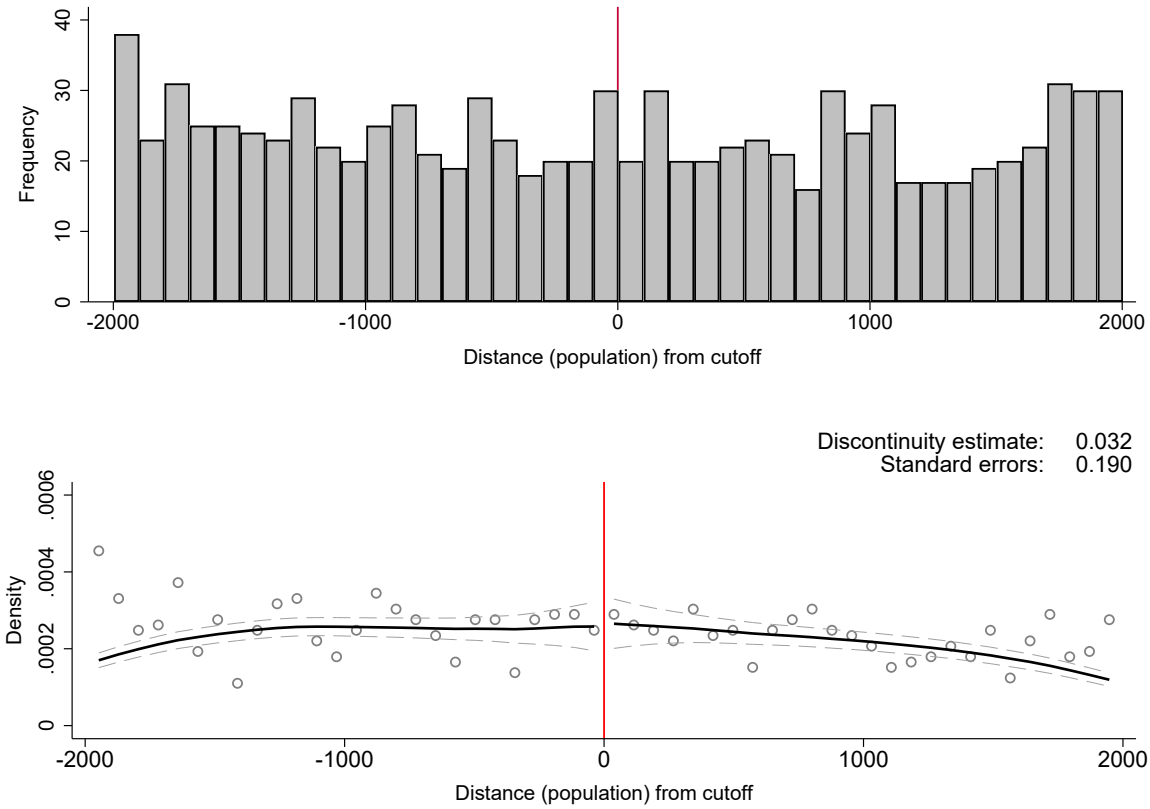
Note: The figure compares austerity-induced changes in current and capital spending pre-pupil (vertical axis) with budget rigidity intensity (horizontal axis). Austerity-induced spending changes are calculated as the within-municipality spending change between the most and least exposed birth cohorts. We plot these outcomes in equal sized bins and show the line of best fit.

Figure A4: Per-Pupil Spending and Austerity Exposure



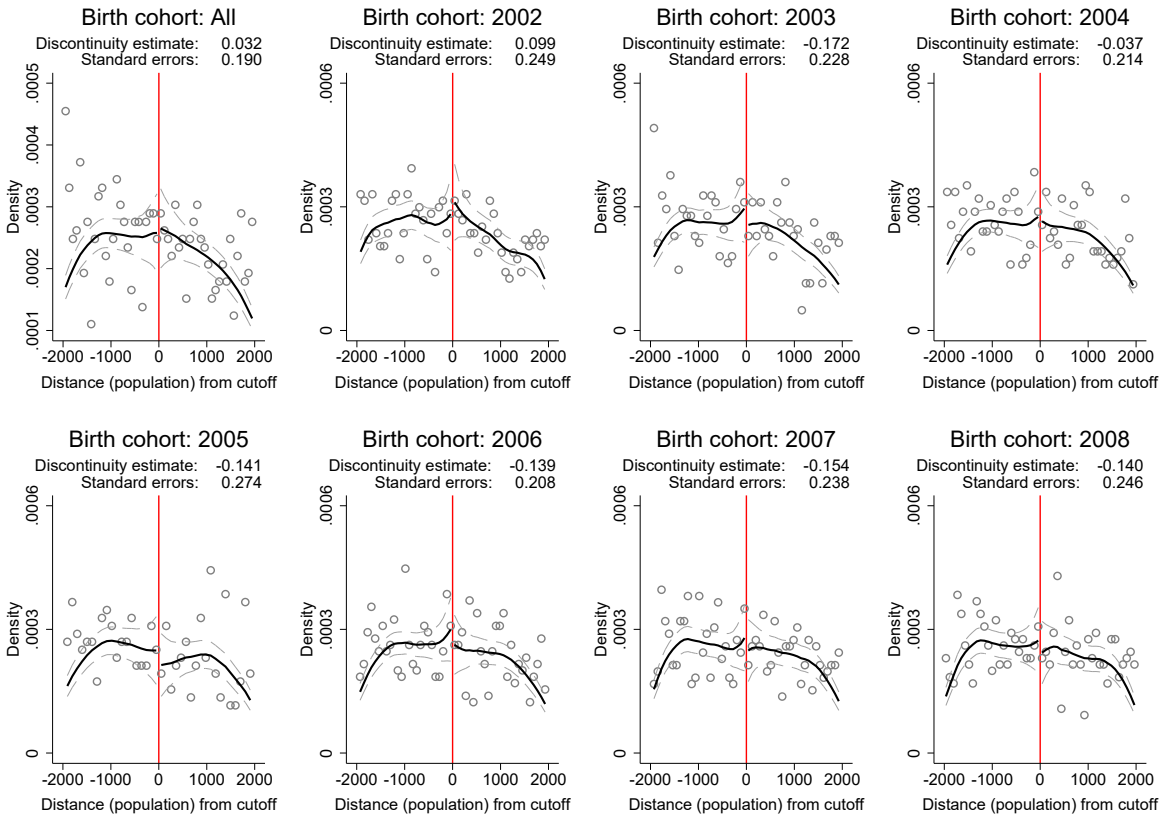
Note: The figure compares current and capital spending per-pupil (vertical axis) with austerity exposure (horizontal axis), computed as the number of years elapsed from DSP inception. We plot these outcomes in equal sized bins and show the line of best fit.

Figure A5: McCrary Test



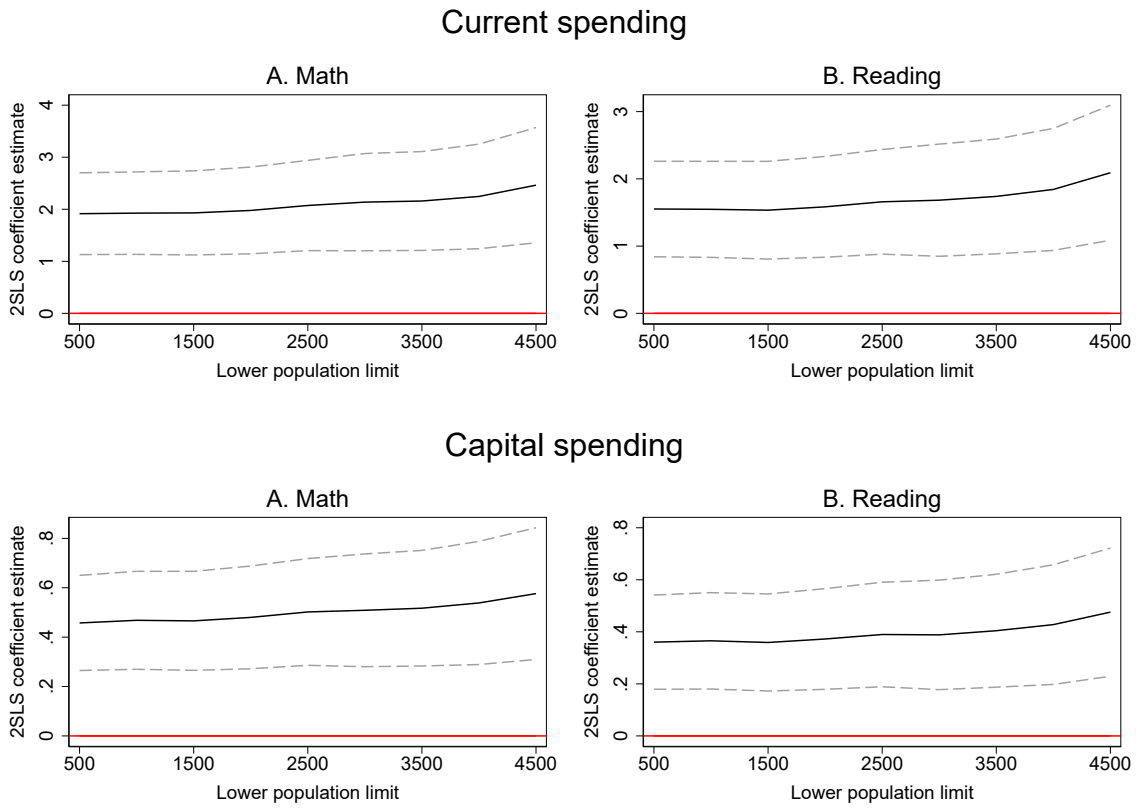
Note: The figure shows the distribution of the municipal population around the eligibility threshold for fiscal rules (red vertical line) in municipalities with population between 3,000 and 7,000. Circles represent the difference between the municipal population and the 5,000 threshold. Circles are average observed values. The central solid line is a kernel estimate; the lateral lines represent the 95 percent confidence intervals. Discontinuity estimate (standard errors) is 0.032 (0.190).

Figure A6: McCrary Test By Birth Cohort



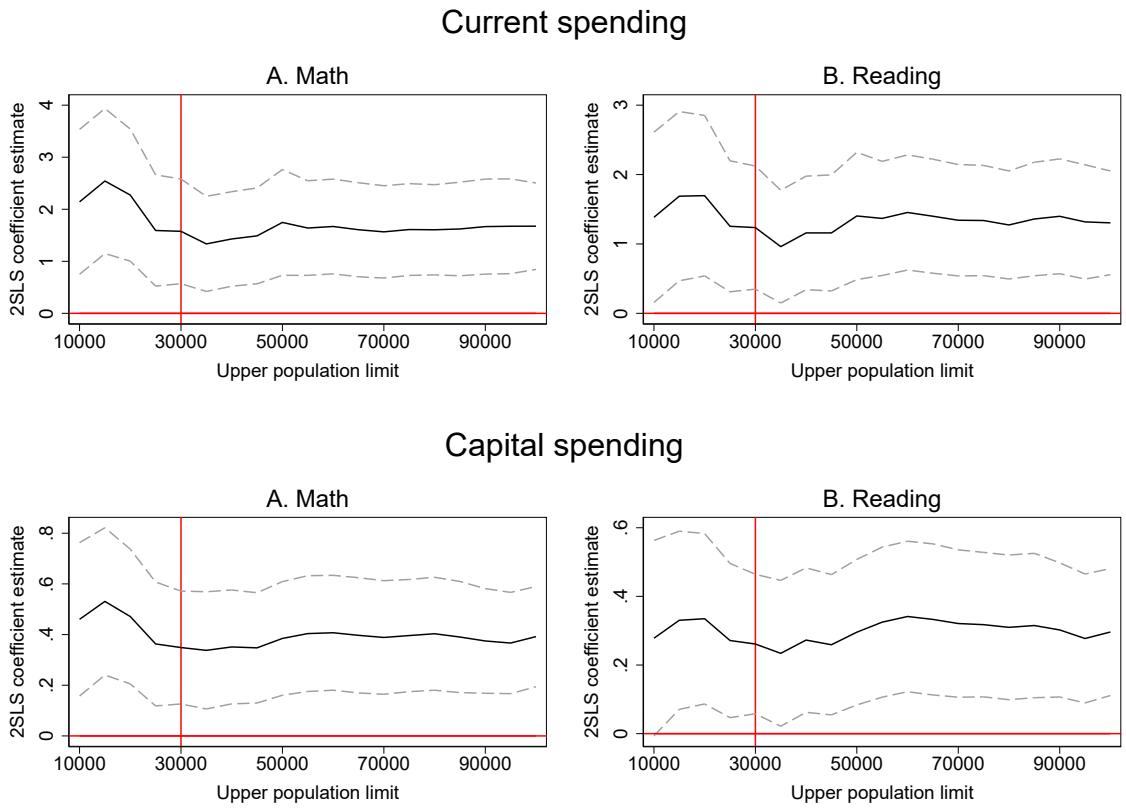
Note: The figure shows the distribution of the municipal population around the eligibility threshold for fiscal rules (red vertical line) in municipalities with population between 3,000 and 7,000. Circles represent the difference between the municipal population and the 5,000 threshold. Circles are average observed values. The central solid line is a kernel estimate; the lateral lines represent the 95 percent confidence intervals.

Figure A7: Sample Selection: Lower Population Limit



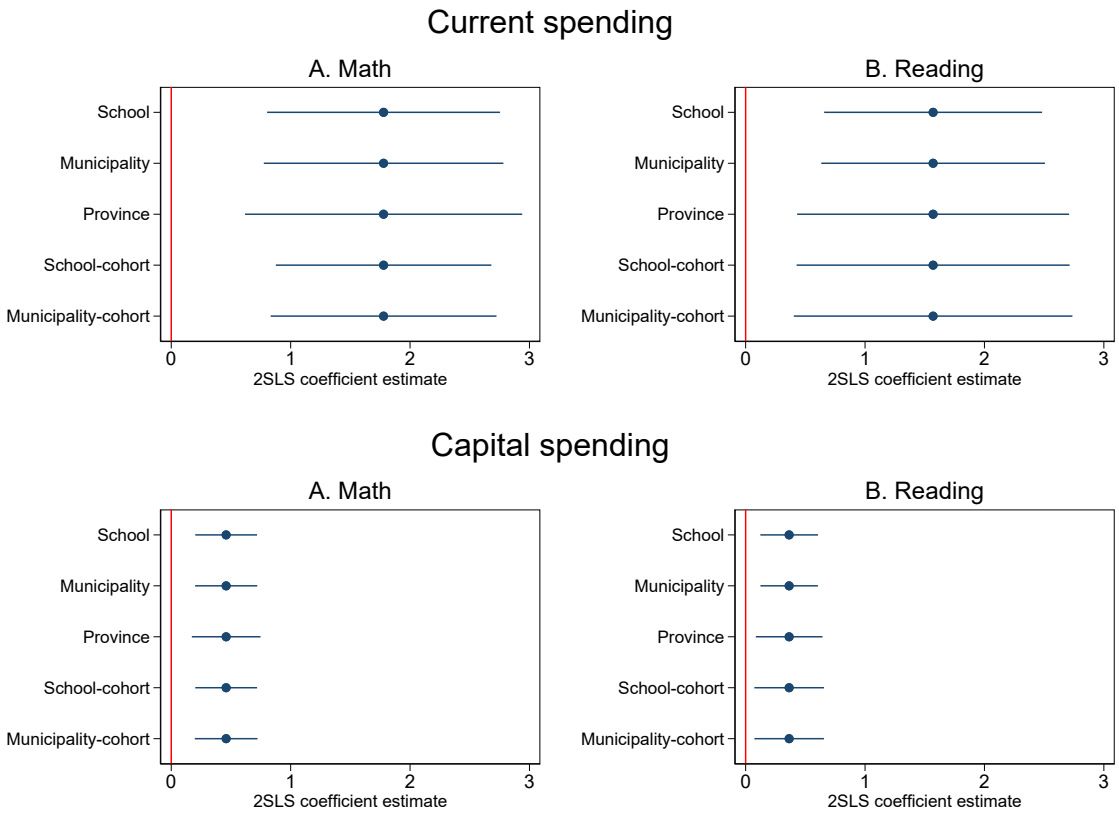
Note: This figure reports β coefficient estimate and 95 percent confidence intervals obtained by running our 2SLS model on the sample of municipalities with population from 30,000 up to a lower limit ranging from 500 to 4,500 residents. We increment the population by 500 residents in each specification. Top (bottom) panel reports test score effects of austerity-induced per-pupil changes in logged current (capital) spending. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members, taxable income per-capita, and house price index). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level.

Figure A8: Sample Selection: Upper Population Limit



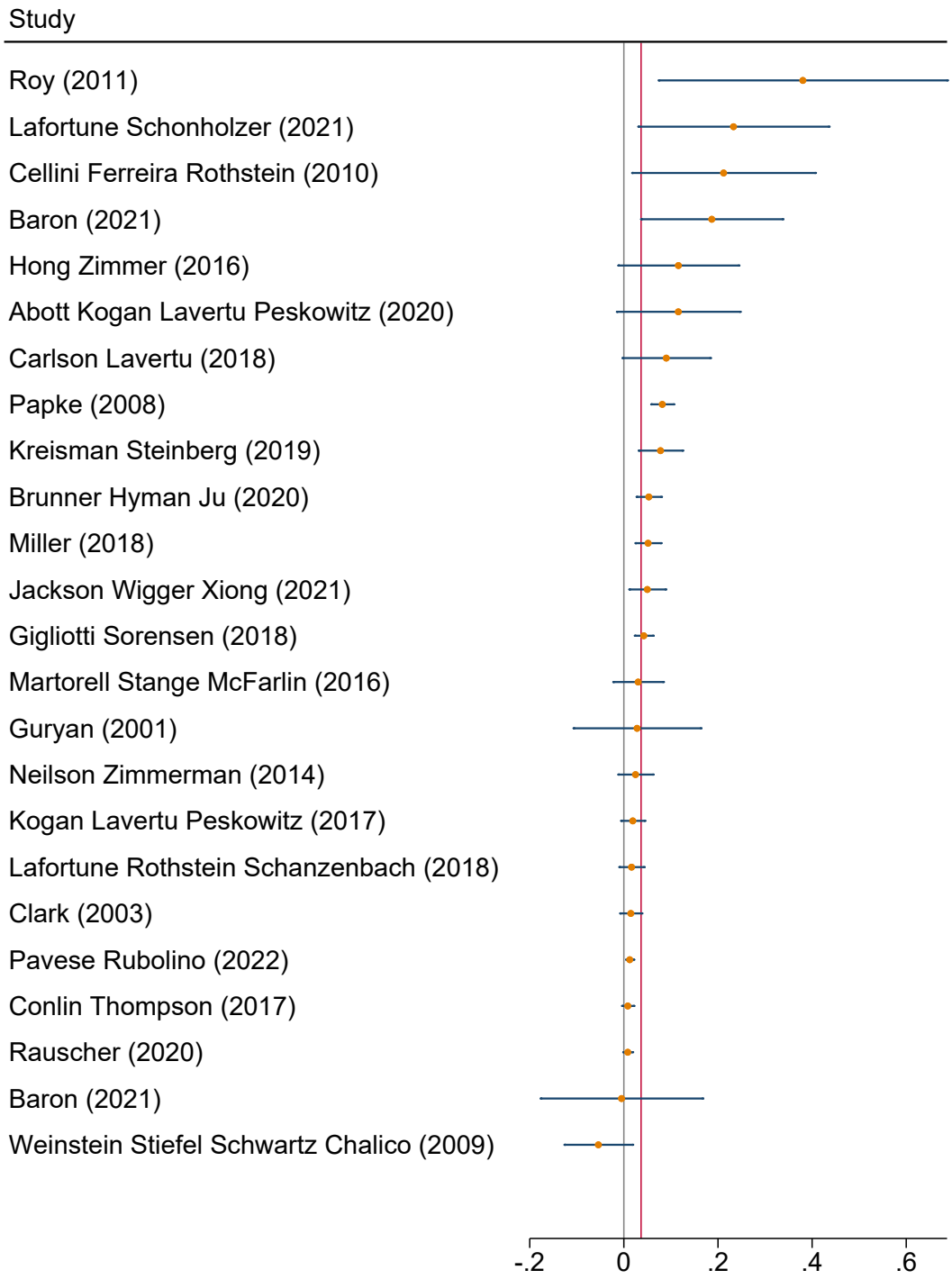
Note: This figure reports β coefficient estimate and 95 percent confidence intervals obtained by running our 2SLS model on the sample of municipalities with population from 0 up to an upper limit ranging from 10,000 to 100,000 residents. We increment the population by 5,000 residents in each specification. Top (bottom) panel reports test score effects of austerity-induced per-pupil changes in logged current (capital) spending. The red solid vertical line refers to our baseline upper population limit (30,000 residents). Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members, taxable income per-capita, and house price index). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level.

Figure A9: Robustness to clustering level



Note: Each figure reports β coefficient estimate from our 2SLS model and 95 percent confidence intervals obtained by clustering the standard errors at the following levels: i. school; ii. municipality (our baseline choice); iii. province; iv. school and cohort; v. municipality and cohort. Top (bottom) panel reports test score effects of austerity-induced per-pupil changes in logged current (capital) spending. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members, taxable income per-capita, and house price index). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact.

Figure A10: Putting Our Results in International Perspective



Note: The forest plot depicts study-specific effects (orange circles) and 95 percent confidence intervals. The red vertical line highlights the study-specific deviations from the overall effect-size and the black vertical line highlight no-effect values. Data are borrowed from the Jackson and Mackevicius (2021) meta-analysis on studies focusing on the causal effect of school spending changes. To allow comparability, each study-specific coefficient is computed as the test score effect of a $1,000 spending change in per-pupil spending (CPI adjusted to 2018), average over four years.

Table A1: Summary Statistics of INVALSI Dataset

	Obs (1)	Mean (2)	SD (3)	Min (4)	Max (5)
	A. Student-level administrative data				
Math test score (V grade)	962,897	-0.000	1.000	-3.943	2.337
Reading test score (V grade)	962,897	0.000	1.000	-4.964	2.473
Female (0/1)	962,897	0.496	0.500	0	1
Father low education (0/1)	962,897	0.441	0.497	0	1
Father middle education (0/1)	962,897	0.450	0.497	0	1
Father high education (0/1)	962,897	0.109	0.312	0	1
Mother low education (0/1)	962,897	0.348	0.477	0	1
Mother middle education (0/1)	962,897	0.504	0.500	0	1
Mother high education (0/1)	962,897	0.147	0.354	0	1
1st quarter of birth (0/1)	962,897	0.318	0.446	0	1
2nd quarter of birth (0/1)	962,897	0.344	0.458	0	1
3rd quarter of birth (0/1)	962,897	0.338	0.455	0	1
Born in Italy (0/1)	962,897	0.936	0.245	0	1
Regular studies (0/1)	962,897	0.978	0.141	0	1
Anticipatory (0/1)	962,897	0.009	0.092	0	1
Repeater (0/1)	962,897	0.012	0.107	0	1
Class size (# of students)	962,897	15.781	3.366	7	29
	B. Student-level survey data				
Room (0/1)	823,580	0.853	0.354	0	1
Computer (0/1)	824,599	0.722	0.448	0	1
Desk (0/1)	823,541	0.863	0.344	0	1
Encyclopedia (0/1)	821,713	0.627	0.484	0	1
WiFi connection (0/1)	824,336	0.867	0.340	0	1
	C. School principal-level survey data				
Gym (0/1)	132,092	0.484	0.500	0	1
Library (0/1)	132,092	0.231	0.421	0	1
Outdoor area (0/1)	132,092	0.607	0.488	0	1
Computer laboratory (0/1)	132,092	0.508	0.500	0	1
Language laboratory (0/1)	132,092	0.082	0.275	0	1
Scientific laboratory (0/1)	132,092	0.141	0.348	0	1
Music laboratory (0/1)	132,092	0.194	0.396	0	1
Technical laboratory (0/1)	132,092	0.049	0.217	0	1
Art laboratory (0/1)	132,092	0.070	0.255	0	1
Heating system (0/1)	132,092	0.960	0.195	0	1
Lighting system (0/1)	132,092	0.968	0.177	0	1
Teaching tools (0/1)	132,092	0.900	0.300	0	1
Stationery items (0/1)	132,092	0.854	0.353	0	1
Textbooks (0/1)	132,092	0.531	0.499	0	1
Textbooks loan service (0/1)	132,092	0.444	0.497	0	1
Audiovisual tools (0/1)	132,092	0.476	0.499	0	1
Interactive whiteboards (0/1)	132,092	0.502	0.500	0	1
Software (0/1)	132,092	0.363	0.481	0	1
Tablet (0/1)	132,092	0.273	0.445	0	1

Note: This table reports summary statistics of the variables collected from the INVALSI dataset.

Table A2: Summary Statistics of Municipality-Level Data

	Obs (1)	Mean (2)	SD (3)	Min (4)	Max (5)
Current spending (1,000 of 2020€)	962,897	47.933	21.789	11.273	332.997
Current spending (log)	962,897	3.793	0.378	2.422	5.808
Capital spending (1,000 of 2020€)	962,897	19.369	19.481	0.772	595.540
Capital spending (log)	962,897	2.708	0.678	-0.026	6.389
Exposure	962,897	8.485	3.926	0	13
Budget rigidity index	962,897	0.569	0.093	0.233	0.887
Mayor female (0/1)	962,897	0.108	0.237	0	1
Mayor high skill (0/1)	962,897	0.533	0.401	0	1
Mayor age	962,897	49.450	7.061	27	76
Share of female in town council	962,897	0.179	0.088	0	0.734
Share of high skill in town council	962,897	0.340	0.123	0	0.823
Average age in town council	962,897	45.110	2.878	34.204	55.276
Taxable income per-capita (1,000€)	962,897	19.507	3.863	8.615	51.961
Housing selling price (€/square meters)	962,897	1,337.381	618.808	205.111	9844.635
Population	962,897	10,840.48	6,998.371	95	29,921

Note: This table reports summary statistics of municipality-level variables. See the main text for information on sources.

Table A3: Robustness to Other Potential Austerity-Induced Changes

Instrumented variable:	*Outcome*: Standardized test scores on Math (1)	Reading (2)
Tax base	0.00079	0.00073
	(0.001)	(0.001)
Income tax (top statutory rate)	-0.04180	-0.03924
	(0.056)	(0.060)
Property tax (main home)	-0.01893	-0.01739
	(0.046)	(0.044)
Property tax (second home)	-0.00005	0.00032
	(0.041)	(0.038)
Transfers and grants (log)	0.03812	0.03643
	(0.301)	(0.291)
Father employed (0/1)	0.88950	-4.44912
	(6.929)	(8.855)
Mother employed (0/1)	1.34443	0.22640
	(3.435)	(2.901)
# of students	962,897	
# of schools	2,681	
# of municipalities	2,274	
Baseline controls	Yes	Yes

Note: This table presents 2SLS estimates of the following instrumented variables: the average municipal tax base (as reported in tax returns data); the municipal top statutory marginal tax rate on personal income; the municipal property tax rate on the main residence; the municipal property tax rate on second homes; (log of) grants and transfers from other governments (including the national and regional government and EU funds), dummy for father employed; dummy for mother employed. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level reported in parenthesis.

Table A4: Do Austerity-Induced Spending Changes Affect Other Outcomes?

	Outcome variable:						
	Tax base	Income tax	Property main	Property secondary	Father employed	Mother employed	Grants and transfers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cur. sp. (1,000€)	0.025	0.002	-0.005	-0.005	-0.003	-0.006	-0.002
	(0.023)	(0.007)	(0.004)	(0.004)	(0.006)	(0.009)	(0.019)
Cur. sp. (log)	1.360	-0.159	-0.179	-0.118	-0.279	-0.185	-0.014
	(1.287)	(0.387)	(0.170)	(0.187)	(0.422)	(0.515)	(0.182)
Cap. sp. (1,000€)	0.031	0.002	-0.006	-0.006	-0.003	-0.005	-0.002
	(0.042)	(0.008)	(0.007)	(0.007)	(0.006)	(0.010)	(0.022)
Cap. sp. (log)	0.263	-0.017	-0.039	-0.031	-0.036	-0.042	-0.007
	(0.224)	(0.067)	(0.032)	(0.034)	(0.052)	(0.087)	(0.207)
# of students				962,897			
# of schools				2,681			
# of municipalities				2,274			
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents 2SLS estimates of austerity-induced public spending changes on the following outcome variables: the average municipal tax base (as reported in tax returns data); the municipal top statutory marginal tax rate on personal income; the municipal property tax rate on the main residence; the municipal property tax rate on second homes; dummy for father employed; dummy for mother employed; (log of) grants and transfers from other governments (including the national and regional government and EU funds). Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level reported in parenthesis.

Table A5: The Effect of Austerity-Induced Spending Changes on Parental Background

	Outcome variable:					
	Father low edu (0/1) (1)	Father mid edu (0/1) (2)	Father high edu (0/1) (3)	Mother low edu (0/1) (4)	Mother mid edu (0/1) (5)	Mother high edu (0/1) (6)
Current spending (1,000€)	-0.002	-0.004	-0.001	0.002	0.002	0.002
	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.004)
Current spending (log)	0.122	-0.227	-0.125	-0.005	0.003	0.232
	(0.325)	(0.298)	(0.333)	(0.304)	(0.185)	(0.202)
Capital spending (1,000€)	-0.001	-0.005	-0.001	0.002	0.002	0.003
	(0.007)	(0.008)	(0.007)	(0.007)	(0.005)	(0.005)
Capital spending (log)	0.013	-0.044	-0.019	0.006	0.007	0.038
	(0.058)	(0.054)	(0.061)	(0.055)	(0.034)	(0.041)
# of students	962,897					
# of schools	2,681					
# of municipalities	2,274					
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table presents 2SLS estimates of austerity-induced public spending changes on parental education. We use separate dummies for low education (i.e., high school graduate or lower), middle education (i.e., college graduate), and high education (master degree or higher), separately for fathers and mothers. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level reported in parenthesis.

Table A6: Robustness to Alternative Measures of Austerity Exposure

	Measure of austerity exposure:				
	Baseline	+ quadratic	Binary	Quintiles	Indicator
	(1)	(2)	(3)	(4)	(5)
	A. *Outcome*: Standardized math test scores				
Current spending (1,000€)	0.025***	0.018**	0.017	0.038***	0.043***
	(0.009)	(0.007)	(0.011)	(0.008)	(0.008)
Current spending (log)	1.778***	0.859**	1.362	2.061***	2.430***
	(0.610)	(0.388)	(0.839)	(0.426)	(0.474)
Capital spending (1,000€)	0.021**	0.021**	0.018	0.025***	0.028***
	(0.010)	(0.010)	(0.012)	(0.006)	(0.007)
Capital spending (log)	0.400***	0.260**	0.335*	0.367***	0.535***
	(0.137)	(0.103)	(0.203)	(0.096)	(0.110)
	B. *Outcome*: Standardized reading test scores				
Current spending (1,000€)	0.022***	0.015**	0.022**	0.025***	0.031***
	(0.009)	(0.007)	(0.010)	(0.008)	(0.009)
Current spending (log)	1.570***	0.700*	1.775**	1.275***	1.762***
	(0.569)	(0.395)	(0.805)	(0.471)	(0.506)
Capital spending (1,000€)	0.018**	0.019**	0.022*	0.014**	0.020***
	(0.009)	(0.009)	(0.012)	(0.006)	(0.006)
Capital spending (log)	0.341***	0.210**	0.427**	0.165	0.384***
	(0.132)	(0.103)	(0.181)	(0.107)	(0.114)
# of students			962,897		
# of schools			2,681		
# of municipalities			2,274		
Baseline controls	Yes	Yes	Yes	Yes	Yes

Note: This table presents 2SLS estimates of austerity-induced public spending changes on standardized test scores on math and reading. The first column reports estimates using our baseline measure of austerity exposure: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. The successive columns report 2SLS estimates using the following measures of austerity exposure: the quadratic form of exposure and its interaction with pre-reform budget intensity (column 2); a binary measure of exposure (0 if cohort's exposure is lower than the municipal average); 1 otherwise) and its interaction with pre-reform budget intensity (column 3); austerity exposure and its interaction with each quintile of the pre-reform budget intensity (column 4); austerity exposure and its interaction with the continuous indicator of pre-reform budget intensity (column 5). Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). Standard errors clustered at municipality level reported in parenthesis.

Table A7: Weighted regressions

	Outcome: Standardized test scores on			
	Math		Reading	
	Baseline	Weighted	Baseline	Weighted
	(1)	(2)	(3)	(4)
Current spending (1,000€)	0.025***	0.019*	0.022***	0.020**
	(0.009)	(0.011)	(0.009)	(0.010)
Current spending (log)	1.778***	1.441*	1.570***	1.530**
	(0.610)	(0.775)	(0.569)	(0.701)
Capital spending (1,000€)	0.021**	0.013	0.018**	0.013*
	(0.010)	(0.008)	(0.009)	(0.007)
Capital spending (log)	0.400***	0.290*	0.341***	0.303**
	(0.137)	(0.150)	(0.132)	(0.128)
# of students	962,897			
# of schools	2,681			
# of municipalities	2,274			
Baseline controls	Yes	Yes	Yes	Yes

Note: This table compare the baseline 2SLS estimates with 2SLS estimates that use population weights. Population refers to the average municipal population observed over the period covered in our analysis. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level reported in parenthesis.

Table A8: Sensitivity to measurement error in student residence

	Outcome: Standardized test scores on			
	Math		Reading	
	Baseline	Without "flagged"	Baseline	Without "flagged"
	(1)	(2)	(3)	(4)
Current spending (1,000€)	0.025***	0.021**	0.022***	0.018**
	(0.009)	(0.009)	(0.009)	(0.008)
Current spending (log)	1.778***	1.508**	1.570***	1.346**
	(0.610)	(0.626)	(0.569)	(0.588)
Capital spending (1,000€)	0.020*	0.017*	0.018**	0.015*
	(0.010)	(0.010)	(0.009)	(0.009)
Capital spending (log)	0.468***	0.418**	0.413**	0.374**
	(0.164)	(0.179)	(0.160)	(0.176)
# of students	962,897	872,332	962,897	872,332
# of schools	2,681	2,399	2,681	2,399
# of municipalities	2,274	2,024	2,274	2,024
Baseline controls	Yes	Yes	Yes	Yes

Note: This table compare the baseline 2SLS estimates obtained from the original sample with those estimated from a sample that excluded municipalities that share a border with a municipality without a primary school. Each specification includes school fixed effects, cohort fixed effects, pupil-level controls (gender, nationality, quarterly of birth, father education, mother education, and dummies for early or late students), and municipality-level controls (age, gender and education of mayor and other town council's members). The excluded instruments from the second stage are: number of years of exposure to the Domestic Stability Pact, and the interaction between a dummy for municipalities in the bottom quintile of the distribution of pre-reform budget rigidity and the number of years of exposure to the Domestic Stability Pact. Standard errors clustered at municipality level reported in parenthesis.

B The Domestic Stability Pact (for online publication)

From its introduction in 1999, the Domestic Stability Pact (DSP) has changed over time with substantial yearly amendments. Revisions to the Pact related to programmatic objectives (deficit and/or expenses growth targets), balance sheet items, the basis of accounting on which the target was defined, the penalty system, and the municipalities targeted. In this Appendix, we revise the yearly evolution of the DSP along these dimensions. We do not include in our discussion regions with special autonomy that have been subject to a special regime agreed by each region with the central state. Although providing to the reader an organized summary of the main changes occurred and of the expenses and revenues included/excluded in the yearly target, this appendix is by no means exhaustive of the complex and fragment legislative process characterizing the adoption of the DSP in Italy.

The DSP in 1999

- **Legislative Framework**: Law n.448/1999, Circular 12 March 1999, n.11.
- **Targeted municipalities:** The DSP targeted all Italian municipalities in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a cash basis.
- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest).
- **Revenues included in the objectives:** Final revenues collected after deducting transfers from the state, proceeds from the sale of financial assets, debt collection.
- **Control system:** Non-compliant municipalities are subjected to sanctions only if Italy is fined by the European Union for the excessive deficit.

The DSP in 2000

- **Legislative Framework**: Law 448/1999, Circular 4 February 2000, n.4.
- **Targeted municipalities:** The DSP targeted all Italian municipalities in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives were defined on a cash basis.
- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest, exceptional expenses (e.g., expenses for natural disaster)

and expenditures financed by current and capital transfers from the State, the EU and other bodies participating to the DSP.

- **Revenues included in the objectives:** All final revenues collected are targeted after deducting current and capital transfers from the State, the EU and other bodies participating to the DSP. Current and capital revenues from the sale of financial assets, debt collection and exceptional revenues are also exempted.
- **Control system:** Interest rates on borrowings are reduced for complying municipalities.

The DSP in 2001

- **Legislative Framework**: Law n.388/2000, Circular 6 February 2001, n.6.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a cash basis.
- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest, exceptional expenses (e.g. expenses for natural disaster), expenses for which there has been legislative amendments or linked to the exercise of new functions and expenditures financed by current and capital transfers from the State, the EU and other bodies participating to the DSP.
- **Revenues included in the objectives:** All final revenues collected are targeted after deducting revenues for which there has been legislative amendments or linked to the exercise of new functions, current and capital revenues from the sale of financial assets, debt collection, occasional revenues and current and capital transfers from the State, the EU and other bodies participating to the DSP.
- **Control system:** Interest rates on borrowings are reduced for complying municipalities.

The DSP in 2002

- **Legislative Framework**: Law n.488/2001; Circular 26 February 2002, n.11.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit and current expenditures growth.
- **Definition of the target:** The objectives are defined on a cash basis.

- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest, exceptional expenses (e.g. expenses for natural disaster), expenses for which there has been legislative amendments or linked to the exercise of state or regional functions and earmarked expenditures financed by transfers from the State, the EU and other bodies participating to the DSP.
- **Revenues included in the objectives:** All final revenues collected are targeted after deducting proceeds from the sale of financial assets, debt collection, exceptional revenues, revenues for which there has been legislative amendments or linked to the exercise of state or regional functions and transfers from the State, the EU and other bodies participating to the DSP.
- **Control system:** Cuts to current transfers are introduced for non-compliant municipalities. The amount of cuts is distributed among complying municipalities.

The DSP in 2003

- **Legislative Framework**: Law n.289/2002; Circular 26 February 2002, n.11.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives were defined on a cash basis.
- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest, exceptional expenses (e.g. expenses for natural disaster), expenses for which there has been legislative amendments, earmarked expenditures financed by transfers from the State, the EU and other bodies participating to the DSP.
- **Revenues included in the objectives:** All final revenues collected are targeted after deducting proceeds from the sale of financial assets and real estate, debt collection, exceptional revenues, revenues for which there has been legislative amendments and transfers from the State, the EU and other bodies participating to the DSP.
- **Control system:** Non-compliant municipalities are banned from recruiting personnel, recourse to borrowing and must reduce expenditures in local public goods and services of at least 10%.

The DSP in 2004

- **Legislative Framework**: Law n.350/2003; Circular 3 February 2004, n.5.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy. [32]
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a cash basis.
- **Expenditures included in the objectives:** All current expenditures are targeted after deducting interest, exceptional expenses (e.g. expenses for natural disaster), expenses for which there has been legislative amendments, higher specific personnel costs for the years 2002-2004 and earmarked expenditures financed by transfers from the State, the EU and other bodies participating to the DSP.
- **Revenues included in the objectives:** All final revenues collected are targeted after deducting proceeds from the sale of financial assets and real estate, debt collection, exceptional revenues, revenues for which there has been legislative amendments and transfers from the State, the EU and other bodies participating to the DSP.
- **Control system:** Non-compliant municipalities are banned from recruiting personnel, the recourse to borrowing and must reduce expenditures in local public goods and services of at least 10%.

The DSP in 2005

- **Legislative Framework**: Law n. 311/2004; Circular 8 February 2005, n.4.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Expenditure cap.
- **Definition of the target:** The objectives are defined on a cash and accrual basis.
- **Expenditures included in the objectives:** All current and capital expenditures are targeted with the exception of personnel costs subject to specific provisions, expenditures for transfers to public administrations, expenditures for shareholding acquisition and other financial assets, expenses for the granting of credit, expenses connected to actions in favor of minor subject to provisions of the juvenile judicial authority, expenses for natural calamities, capital expenditures stemming from interventions co-financed by the EU, capital expenditures financed by revenues stemming from the dispose of movable and immovable property, capital

[32] In 2004 municipalities below 3000 residents were also exempted by the Domestic Stability Pact (Art. 21, law No. 311/2004), however, this change has never been concretely applied. After few month, law No. 88/2005 (Art. 1-ter) has re-established the 5000 threshold that, de facto, has been continuously in force from 2001 until 2012 (Art. 31, law No. 183/2011).

expenditures financed by the proceeds from the fight to tax evasion and capital expenditures financed by the proceeds stemming from transfers free of charge and donations.

- **Control system:** Non-compliant municipalities are banned from recruiting personnel, recourse to borrowing and must reduce expenditures on local public goods and services of at least 10%. Municipalities are defined as virtuous (average current expenditure per-capita between 2001-2003 lower than the one in the same demographic category) and non-virtuous municipalities. Virtuous municipalities are allowed to a larger growth rate of expenditures than non-virtuous municipalities.

The DSP in 2006

- **Legislative Framework**: Law n. 266/2005; Circular 17 February 2006, n.8.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Expenditure cap.
- **Definition of the target:** The objectives are defined on a cash and accrual basis.
- **Expenditures included in the objectives:** All current expenditures are targeted with the exception of personnel costs subject to specific provisions, expenditures for transfers to public administrations, social expenditure (decree of the President of the Republic 31 January 1996, n.194), interest expense, expenditure for natural calamities, expenditures for costs due to sentences stemming off-balance sheet liabilities, expenses connected to delegated functions from other local authorities. On the side of capital expenditures, the following item are excluded from the target: transfers to public administrations, expenses for natural calamities, expenses for the granting of credit, expenses connected to delegated functions from other local authorities, expenditures stemming from interventions co-financed by the EU, expenditures financed by the proceeds from fighting tax evasion and expenditures financed by the proceeds stemming from transfers free of charge and donations.
- **Control system:** Non-compliant municipalities are banned from recruiting staff, recourse to borrowing and must reduce expenditures on local public goods and services of at least 10%. Municipalities are defined as virtuous (average current expenditure per-capita between 2002-2004 lower than the one in the same demographic category) and non-virtuous municipalities. Virtuous municipalities are allowed to a larger growth rate of expenditures than non-virtuous municipalities.

The DSP in 2007

- **Legislative Framework**: Law n. 296/2006; Circular 22 February 2007, n.12.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a cash and accrual basis .
- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting and collection of credit are excluded by the Pact together with capital expenditures stemming from the creation of new judicial offices (including removal expenses).
- **Revenues included in the objectives:** All current and capital revenues are targeted with the exception of capital revenues collected between 2003-2005 stemming from the disposal of movable and real estate assets earmarked to the early repayments of loans.
- **Control system:** Fiscal automatism, consisting in an increase in the municipal surtax rate on personal income. This is enforced on non-compliant municipalities that do not autonomously adopt measures for adjusting the gap.

The DSP in 2008

- **Legislative Framework**: Law n. 244/2007; Circular 28 February 2008, n.8.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a "mixed accrual basis."
- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting and collection of credits are excluded from the Pact, together with capital expenditures stemming from the creation of new judicial offices (included removal expenses) and wage increases for specific categories.
- **Revenues included in the objectives:** All current and capital revenues. Capital revenues collected between 2003-2005 stemming from the disposal of movable and real estate assets earmarked to the early repayments of loans are excluded from the target, together with revenues originated from credit collection.

- **Control system:** Fiscal automatism consisting in an increase of the municipal surtax rate on personal income. This is enforced on non-compliant municipalities that do not autonomously adopt measures for adjusting the gap.

The DSP in 2009

- **Legislative Framework**: Law n. 133/2008; Circular 27 January 2009, n.2.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a "mixed accrual basis".
- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the Pact, together with current expenditures arising from declarations of state of emergency issued by the Prime Minister.
- **Revenues included in the objectives:** All current and capital revenues are targeted with the exception of revenues from selling equities, receiving dividends, or from selling real estate properties if spent for new structural investments or to reduce the stock of debt. Revenues arising from declarations of state of emergency issued by the Prime Minister are also excluded from the target.
- **Control system:** For non-compliant municipalities for the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are not allowed. Compliant (virtuous) municipalities are rewarded by allowing to reduce the target deficit in the following year.

The DSP in 2010

- **Legislative Framework**: Law n. 133/2008; Law n. 42/2010; Circular 30 March 2010, n.15; Decree-law 25 January 2010, n.2.
- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.
- **Target:** Deficit.
- **Definition of the target:** The objectives are defined on a "mixed accrual basis."
- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the

target together with current and capital expenditures arising from declarations of state of emergency issued by the Prime Minister and current and capital expenses stemming from the organization and holding of major events (only when using transfers from the national government). Expenses for investments to protect public health and temporary and extraordinary social and welfare grants are not subjected to the Pact in the municipalities of the Abruzzo region hit by the earthquake in 2009. Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues from the collection of credit and revenues from selling equities, receiving dividends, or from selling real estate properties are excluded from the target if spent for new structural investments or to reduce the stock of debt. Revenues arising from declarations of state of emergency issued by the Prime Minister are also excluded from the target together with revenues stemming from the organization and holding of major events (only when using transfers from the national government). Direct or indirect transfers from the EU are also excluded from the target.

- **Control system:** For non-compliant municipalities for the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are not allowed. Compliant (virtuous) municipalities are rewarded by allowing to reduce the target deficit in the following year.

The DSP in 2011

- **Legislative Framework**: Law n. 220/2010; Circular 30 March 2010, n.15.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.

- **Target:** Deficit.

- **Definition of the target:** The objectives are defined on a "mixed accrual basis."

- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the Pact together with current and capital expenditures arising from declarations of state of emergency issued by the Prime Minister, current and capital expenses stemming from the organization and holding of major events (only when using transfers from the national government), expenses related to the special commissions for municipalities infiltrated by the mafia, design and execution of the

census, European Food Safety Authority based in Parma, Scuola per l'Europa in Parma, Expo 2015 Milan and State property federalism. Capital expenses for investments are not subjected to the Pact in the municipalities of L'Aquila province hit by the earthquake in 2009. Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues from the collection of credit are excluded from the target together with revenues arising from declarations of state of emergency issued by the Prime Minister, revenues stemming from the organization and holding of major events (only when using transfers from the State) and revenues transferred from ISTAT for the design and execution of the census. Direct or indirect transfers from the EU are also excluded from the target.

- **Control system:** For non-compliant municipalities in the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are also not allowed. Virtuous municipalities contribute less to the target than non-compliant municipalities.

The DSP in 2012

- **Legislative Framework**: Law n. 183/2011; Circular 14 February 2012 n.5.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 5,000 residents in regions with no special autonomy.

- **Target:** Deficit.

- **Definition of the target:** The objectives are defined on a "mixed accrual basis."

- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the Pact, together with current and capital expenditures arising from declarations of state of emergency issued by the Prime Minister, current and capital expenses stemming from the organization and holding of major events (only when using transfers from the State), expenses related to the design and execution of the census, European Food Safety Authority based in Parma, Scuola per l'Europa in Parma, Expo 2015 Milan and State property federalism. Capital expenses for investments are not subjected to the Pact in the municipalities of L'Aquila province hit by the earthquake in 2009. Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues arising from declarations of state of emergency issued by the Prime Minister are also excluded from the target together with revenues stemming from the organization and holding of major events (only when using transfers from the State) and revenues transferred from ISTAT for the design and execution of the census. Direct or indirect transfers from the EU are also excluded from the target.

- **Control system:** For non-compliant municipalities for the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are also not allowed. Compliant (virtuous) municipalities are rewarded by a reduction of the yearly targets agreed with the Ministry of Interior.

The DSP in 2013

- **Legislative Framework**: Law n. 228/2012; Circular 7 February 2013, n.5.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 1,001 residents in regions with no special autonomy.

- **Target:** Deficit.

- **Definition of the target:** The objectives are defined on a "mixed accrual basis."

- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the Pact together with current and capital expenditures arising from declarations of state of emergency issued by the Prime Minister together with current and capital expenses stemming from the organization and holding of major events (only when using transfers from the State), expenses related to the design and execution of the census, the European Food Safety Authority based in Parma, the Scuola per l'Europa in Parma, the Expo 2015 in Milan, the State property federalism, the construction of the Shoah national museum and infrastructural investments using resources stemming from divestment of shares in companies providing economically relevant local public services (except for the water service). Current and capital expenses to finance economic recovery measures and reconstruction interventions in the municipalities belonging to the provinces of Bologna, Modena, Ferrara, Mantova, Reggio Emilia and Rovigo hit by the earthquake in 2012 are not subjected to the Pact (expenses financed with donations from citizens and firms are also exempted). Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues arising from declarations of state of emergency issued by the Prime Minister are excluded from the target together with revenues stemming from the organization and holding of major events (only when using transfers from the State), revenues transferred from ISTAT for the design and execution of the census and revenues related to the economic recovery measures and reconstruction interventions in the municipalities belonging to the provinces of Bologna, Modena, Ferrara, Mantova, Reggio Emilia and Rovigo hit by the earthquake in 2012 (donations from citizens and firms are also exempted). Direct or indirect transfers from the EU are also excluded from the target.

- **Control system:** For non-compliant municipalities for the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are also not allowed. Compliant (virtuous) municipalities have a lower target to achieve than non-compliant municipalities.

The DSP in 2014

- **Legislative Framework**: Law n. 147/2013; Circular 18 February 2014, n.6.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 1,001 residents in regions with no special autonomy.

- **Target:** Deficit.

- **Definition of the target:** The objectives are defined on a "mixed accrual basis."

- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the Pact together with current and capital expenditures arising from declarations of state of emergency issued by the Prime Minister, current and capital expenses stemming from the organization and holding of major events (only when using transfers from the national government), expenses related to the design and execution of the census, the State property federalism, infrastructural investments using resources stemming from divestment of shares in companies providing economically relevant local public services (except for the water service), expenses for requalification interventions of the territory approved by the *CIPE* (Interdepartmental Committee for Economic Planning) with deliberation 57/2011, expenses for the harbor interventions in the Piombino municipality, capital payments of debt (for an amount of 500 million for municipalities, provinces and regions), capital payments (for an amount of 850 million for municipalities) and

current expenditures incurred by the municipality of Campione listed in the decree n. 09804529/15100-525 of 6/10/1998. Current and capital expenses to finance economic recovery measures and reconstruction interventions in the municipalities belonging to the provinces of Bologna, Modena, Ferrara, Mantova, Reggio Emilia and Rovigo hit by the earthquake in 2012 are not subjected to the Pact (expenses financed with donations from citizens and firms are also exempted). Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues arising from declarations of state of emergency issued by the Prime Minister are excluded from the target together with revenues stemming from the organization and holding of major events (only when using transfers from the State), revenues for interventions approved by the *CIPE* (Interdepartmental Committee for Economic Planning) with deliberation 57/2011, revenues transferred from ISTAT for the design and execution of the census, revenues related to the economic recovery measures, revenues stemming from the property tax on buildings owned by the municipality and reconstruction interventions in the municipalities belonging to the provinces of Bologna, Modena, Ferrara, Mantova, Reggio Emilia and Rovigo hit by the earthquake in 2012 (donations from citizens and firms are also exempted). Direct and indirect transfers from the EU are also excluded from the target.

- **Control system:** For non-compliant municipalities for the following year there is a reduction of ordinary transfers from the Ministry of Interior and of benefits to local administrators, current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are also not allowed.

The DSP in 2015

- **Legislative Framework**: Law 190/2014; Decree of the Ministry of Economy and Finance 26 June 2015, n. 52505.

- **Targeted municipalities:** The DSP targeted all Italian municipalities above 1,001 residents in regions with no special autonomy.

- **Target:** Deficit.

- **Definition of the target:** The objectives are defined on a "mixed accrual basis."

- **Expenditures included in the objectives:** All current and capital expenditures are targeted. Expenditures related to the granting of credit are excluded from the

Pact together with provision for doubtful debts allowances on receivables, capital expenditures for safety interventions on school buildings, expenses for exercise of the function of lead institution, expenses for sentences passed in court, current and capital expenditures arising from declarations of state of emergency and protection of territory issued by the Prime Minister, current and capital expenses stemming from the organization and holding of major events (only when using transfers from the State), expenses related to the design and execution of the census, the state property federalism, capital expenses stemming from the partial or total divestment of shareholdings, expenses for interventions approved by the *CIPE* (Interdepartmental Committee for Economic Planning) with deliberation 57/2011 (TAV), expenditures incurred by the municipalities headquarters of the metropolitan city, capital investments related to the implementation of projects for the "Italian capital of culture", capital expenses for the removal of level crossing, expenses incurred by the municipality of Casale Monferrato for the asbestos remediation interventions, capital payments of debts not settled by 31/12/2013, capital payments (for an amount of 850 million for municipalities), current expenditures incurred by the municipality of Campione listed in the decree n. 09804529/15100-525 of 6/10/1998 and expenses financed with municipalities' own resources (donations from citizens and firms) in municipalities hit by the earthquake in 2012. Current and capital expenditure directly or indirectly financed by the EU are also excluded from the target.

- **Revenues included in the objectives:** All current and capital revenues are targeted. Revenues arising from declarations of state of emergency issued by the Prime Minister are excluded from the target together with revenues stemming from the organization and holding of major events (only when using transfers from the State), revenues transferred from ISTAT for the design and execution of the census, revenues related to the economic recovery measures, revenues for interventions approved by the *CIPE* (Interdepartmental Committee for Economic Planning) with deliberation 57/2011 (TAV), regional transfers to the municipality of Casale Monferrato for the asbestos remediation interventions, current transfer of 530 millions (decree-law 78/2015), repayment of mortgage loan installments from the State and revenues from the "*Fondo di rotazione per assicurare la stabilità finanziaria degli enti locali*". Direct or indirect transfers from the EU are also excluded from the target.

- **Control system:** Non-compliant municipalities are subject to a reduction in ordinary transfers from the Ministry of Interior and of benefits to local administrators. Current expenditures are not allowed to increase above the last three-year average, recourse to debt (also to finance investments) and personnel recruitment are also not allowed.