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Keywords
School spending, test scores, fiscal rules, regression discontinuity design

JEL Codes
I22, I24, H52, H75

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Do Fiscal Restraints Harm Test Scores? Evidence from Italy*

Caterina Pavese† Enrico Rubolino‡

This version: December 2020

Abstract

Most countries discipline their public budget through a set of fiscal rules aiming at limiting public debt accumulation. Yet, apart from the direct effect on public finance outcomes, there is limited evidence on whether these policies affect broader socio-economic outcomes. This paper provides regression discontinuity estimates of fiscal rules-induced school spending drops on test scores of Italian students. We show that school spending per-pupil is around 102 euros lower in municipalities subject to fiscal restraints. Using longitudinal data on pupils' attainment in national test at the beginning and the end of primary school, we find that spending differences lead to a gap in standardized test score gains of nearly 12 percent of a standard deviation. The impact is particularly strong for lower socio-economic groups. We find that both the lack of several basic instructional tools and limited investments in school facilities explain most of the observed achievement gap. Our results reveal how fiscal restraints can create "unintended" consequences for younger generations and exacerbate cross-generation inequalities when governments need to reduce public spending.

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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 expenditures.[1] For instance, school expenditure per-pupil has declined by around 15 percent in Italy and Spain, and by roughly 7 percent in the US over the recent years (OECD Education Database).

Would education budget cuts harm students' cognitive abilities? Whether money affect educational performance is disputed. The returns of resource-based education policies have been seriously debated by economists since the Coleman et al. (1966) report. As a consequence, policy makers started to move away from traditional "input directed" policies to those providing performance incentives (Burtless 1996; Hanushek 2006).[2] However, most of the existing literature at the time had failed to credibly deal with endogeneity issues. Supported by the dramatic expansions in school-level data, the recent literature has proposed more methodologically convincing analysis. Although the existing evidence on the impact of school resources is still mixed (see Jackson 2018 for a recent extensive review of this literature), a series of recent papers has provided suggestive evidence on positive economic returns from increasing school spending (e.g., Jackson et al. 2016; Lafortune et al. 2018; Jackson et al. 2020), thus challenging the consensus view and calling for additional spending to raise long-term labor market outcomes (Chetty et al. 2011; Hyman 2017).

In this paper, we study the impact of school resources on standardized test scores of Italian primary schools' students. We believe that Italy provides an interesting laboratory for two key reasons. First, there is very limited evidence on how schools resources affect students' attainments in Italy.[3] This is surprising, given that Italy shows one of the lowest level of school spending among OECD countries and Italian students have comparatively lower scores in Programme for International Student Assessment (PISA) tests. Most of the existing literature has focused on the US, where the educational system differs in many aspects from that of other both developed and developing countries. As most schools around the world, the Italian schooling system is

[1] The depressing effect of the last economic recession on school spending, when states and local districts short of cash curtailed education spending for the first time in decades, has been extensively covered in the public debate (see, for instance, New York Times, *The Numbers That Explain Why Teachers Are in Revolt, June 2018*).

[2] This view was strongly influenced by the work of Eric Hanushek (see Hanushek 1986, Hanushek 1997, Hanushek 2003, and Hanushek 2006). For instance, Hanushek (1997), p.153, argues that "simply providing more funding or a different distribution of funding is unlikely to improve student achievement."

[3] 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 a specific input, such as class size (Brunello and Checchi 2005; Angrist et al. 2017; Ballatore et al. 2018).

mostly *publicly* financed.

Second, although the lion share of education spending in Italy is provided by the national government, we observe striking heterogeneity in the amount of school spending financed by municipalities. This source of geographical heterogeneity is not a second-order issue, given that municipalities are in charge of financing school infrastructure, technology and other material resources that are crucial for an effective and quality education.[4] Figure 1 illustrates the degree of cross-municipality heterogeneity in school spending per-pupil. While the average per-pupil municipal school spending is 741 euros, municipalities in the top quintile of the school spending distribution spend more than ten times more than municipalities in the bottom quintile (1,596 vs 151 per-pupil euros). These differences in spending across municipalities are the largest component of inequality in resources available to students in primary and secondary schools. Yet, this geographical dispersion is correlated with several socio-economic and labor market characteristics, making hard to establish any causal link between school spending and test scores.

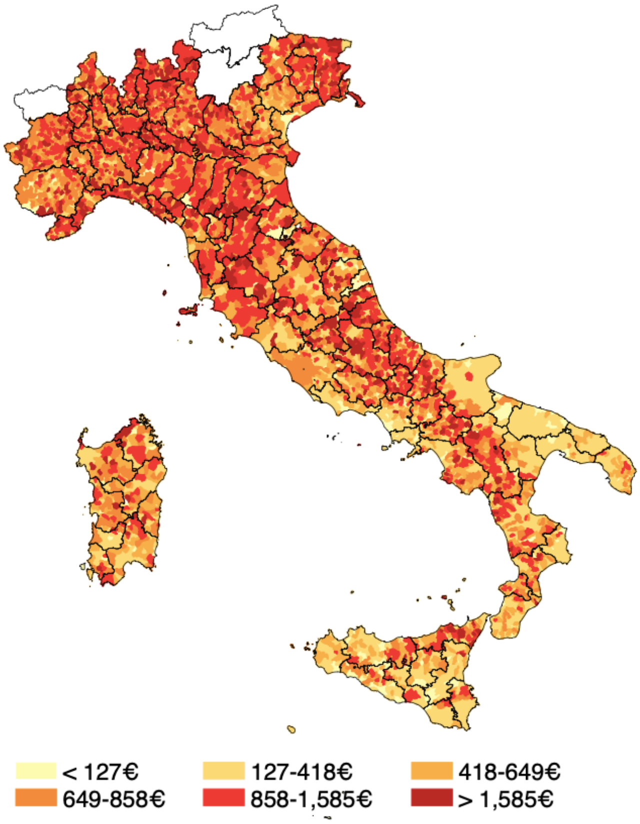
To overcome this challenge, we take advantage of the Domestic Stability Pact (DSP), which targets municipalities with population exceeding a discrete threshold to be eligible for fiscal restraints. Namely, the DSP keeps municipalities accountable through a set of fiscal rules that limits their possibility to run deficit and accumulate debt. As municipalities have limited autonomy in setting taxes, they are compelled to reduce public expenditures in order to achieve the public finance targets set by the DSP. We find that fiscal restraints cause municipalities just above the population threshold to spend around 102 euros per-pupil less to finance schools, allowing us to estimate the causal impact of school spending using a fuzzy regression discontinuity design.

We combine this empirical approach with administrative data provided by the Institute for the Evaluation of the Educational System (INVALSI) on standardized test scores for the universe of Italian students. The tests have been uniform across schools and over time, facilitating comparisons. We construct a longitudinal version of the dataset containing information on test scores at the beginning and the end of primary school, along with several demographic and socio-economic information. Our primary outcome variable is the standardized *test score gain* in math and language, computed by subtracting each student's fifth-grade score from her second-grade score. Then, we relate test score gains with cross-municipality fiscal rules-induced differences in school spending per-pupil, which we measure by using administrative data on the balance sheets of Italian municipalities.

Our results show that fiscal rules generate a gap on test score gains by around 13.9 (12.7) percent of a standard deviation unit in math (language). We find that differences

[4]Specifically, municipalities are in charge of constructing, renovating and maintenance of buildings of pre-primary, primary and lower secondary schools. They also provide a number of basic goods and services, such as school meals, school transportation, textbooks, teaching tools, educational materials needed to set up laboratories, libraries and gyms, and are charged for water, heating, internet, electricity and cleaning costs.

Figure 1: Municipal school spending per-pupil



Note: The figure depicts municipal school spending per-pupil (2015 euros) averaged over the 2001-2015 period. Yellow (red) area depicts municipalities with a lower (larger) spending per-pupil. The black line refers to provincial boundaries. Break points are quintile intervals in municipal school spending per-pupil. Missing values are imputed with the regional average. Data for Aosta Valley and Trentino Alto-Adige region (the white areas in the North-West and North-East) are not available.

in test scores are substantially driven by students from lower socio-economic parental background. By contrast, students' gender and nationality appear to have a trivial impact. We find remarkably similar results when we leverage within-municipality school spending variation across consecutive cohorts, using time elapsed from the inception of fiscal rules as an instrument for school spending variation.

We validate our results by showing that baseline characteristics of students, including *baseline* test scores (as measured at the beginning of primary school), as well as family characteristics are not systematically correlated with eligibility for fiscal rules. Moreover, we do not find any systematic difference in other salient characteristics, such as house prices and taxable income, that would otherwise indicate sorting. We argue that in our institutional setting municipal school expenditure was a very weak signal about school quality and was not a factor that parents consider in their residential choice. We also rule out any crowding out effect in school spending from parents or other funding sources.

In the last part of the paper, we explore the mechanisms through which fiscal rules affect school resources. In this analysis, we make use of rich survey data on a nationally representative sample of teachers that gathers information on the availability of Information and Communication Technologies (ICT) equipment at school. Moreover, we collect data from the Italian Ministry of Interior and the Ministry of Education and Research (MIUR) on school infrastructures directly financed by municipalities. Our findings emphasize that fiscal rules eligibility dramatically reduces investments in ICT educational tools as well as in school facilities. For instance, we find that the probability of *not* having a computer at school is around one-fourth larger in municipalities subject to fiscal restraints. The picture looks similar when we focus on measures of technological innovation and investments in physical infrastructures: the probability of having automatic heating, sound-proofed classes and facilities for disabled students is 4.2, 4.6, and 3.6 percent lower, respectively, in municipalities eligible for fiscal restraints. These results suggest that disparities in school equipment and in physical environment might explain a non trivial portion of the observed educational gap.

This paper contributes to a wide literature estimating the impacts of school resources on students' achievements. According to some studies, there is little or no systematic relationship between school spending and students' educational achievements. In a recent influential study, Cellini et al. (2010) study the impact of school capital investments in California by comparing school districts where referenda on bond issues passed and failed by a narrow margin. While the evidence on house prices shows large and positive impact of school facilities investment, findings on students' test outcomes are quite imprecise and marginally significant.[5] Similar results are also found in Martorell et al. (2016).

On the other hand, some recent relevant studies have provided evidence that money matter. Jackson et al. (2016) explore the long-run effects of school resources by linking school spending and school finance reform data to nationally representative data on children born between 1955 and 1985 and followed through 2011. By leveraging cross-cohort differences in exposure to school finance reforms, they find that school spending has a significant and large impact on a number of socio-economic and labor market outcomes, including years of education, wages and the probability of being poor. Using an event study design leveraging the impact of a massive school construction project in the US, Lafortune and Schonholzer (2019) find large and positive effects on students' cognitive and non-cognitive outcomes. Likewise, Neilson and Zimmerman (2014) exploit the staggered implementation of a large school investment in poor urban areas of New Haven, Connecticut, and find a gradual positive impact on reading scores for elementary and middle school students similar effects. Closely related to our findings is the recent paper by Jackson et al. (2020), which study the impact of public

[5]Yet, leveraging a similar empirical approach in the context of school districts in Wisconsin, Baron (2019) finds that increases in *operational* spending have substantial positive effects on test scores, dropout rates, and post-secondary enrollment, while additional *capital* expenditures have little impact.

school spending cuts in the aftermath of the great recession in the US, when per-pupil spending fell by roughly 7 percent. Consistent with our findings, they show that the impact of such large and sustained education funding cuts had significant effects on test scores and college-going rates.

We speak to this literature by presenting the first nationwide study on the impact of school resources on test scores in Italy. The existing evidence is limited to some particular areas, such as schools hit by the 2012 earthquake (Belmonte et al. 2020) or on specific inputs, such as effects of class size on education attainment (Brunello and Checchi 2005) and test scores manipulation (Angrist et al. 2017), and the impacts of increasing the number of immigrants in a classroom on natives' test scores (Ballatore et al. 2018).[6] Crucially, we estimate the student-level effect of school spending on directly treated students, while most of prior studies examine effects on district-level or school-level average outcomes. The latter might explain why they fail to find precise and conclusive evidence on the impact of capital school spending. Moreover, by combining a rich set of administrative data with survey data, we are able to shed light on a vast array of heterogeneity and mechanisms. Most of the existing studies are unable to show and detail mediating mechanisms due to the lack of granular data.

Additionally, our paper contributes to the growing literature exploring the effect of fiscal rules on various outcomes, including spending and taxes (Chiades and Mengotto 2015; Grembi et al. 2016; Chiades et al. 2019; Alpino et al. 2020), politicians' selection (Gamalerio 2017), political budget cycle (Bonfatti and Forni 2019) and local administrators' compliance in enforcing anti-tax evasion policies (Rubolino 2020a).[7] We provide compelling evidence that fiscal restraints have significant negative effect on students' performance through their effects on the local governments' abilities to provide public educational services. To the best of our knowledge, we are the first to provide evidence on the effect of fiscal rules on educational outcomes. Our results reveal how fiscal rules can create "unintended" consequences for younger generations and exacerbate cross-generation inequalities when governments need to reduce public spending.

The rest of the paper proceeds as follows. We present background information on local public finance and the Italian school system in section 2. Section 3 illustrates the data employed in this paper. In section 4, we describe a conceptual framework to help contextualizing our empirical strategy, presented in section 5. Section 6 and 7 present our baseline results and shed light on the underlying mechanisms behind them. Finally, we offer some discussion and our concluding remarks in section 8.

[6]Our findings are consistent with the results by Belmonte et al. (2020), which studies the relationship between capital spending and achievements. Yet, their paper focus exclusively on physical infrastructure, while we explore a broader range of school inputs. Furthermore, we overcome the issues related to using test scores' data aggregated at school-level by relating granular variation in school spending with *student-level* variation in test score *gains*.

[7]Beetsma and Debrun (2003) and Beetsma and Debrun (2007) provide a review on the literature focusing on both national and sub-national fiscal rules.

2 Institutional framework

In this section, we describe the relevant background information on local public finance and the educational system in Italy.

2.1 Local public finance and fiscal rules

Italy is composed of three different sub-national tiers of government: there are 20 regions (*Regioni*), 107 provinces (*Province*), and 7,918 municipalities (*Comuni*). The focus of our analysis is on municipalities, which are responsible for financing pre-school, primary and lower secondary schools. In general, municipalities manage around 10 percent of total public expenditure and are responsible for providing a large array of public goods and services to citizens, including public transportation, local police, town planning, and manage public utilities. To finance these services, they set taxes on properties and a surtax on personal income, which raise nearly 15 percent of total revenue. Yet, municipalities have limited autonomy in setting local tax rates: both the municipal surtax rate on personal income and the property tax can vary within a specific range set by the national government (see Rubolino 2020b for the income tax and Bordignon et al. 2003 for the property tax). Moreover, both the national and regional governments transfer resources to municipalities to cover ordinary running costs. Transfers are determined by law on the basis of a municipality's population, density, surface, age composition and previous expenses (see Decreto Legislativo n. 504/1992).[8]

The municipal government is composed of a mayor and an executive committee. Any change in fiscal policy, such as local tax rates and public goods provision, is proposed by the mayor and the executive committee. An elected municipal council endorses the annual budget proposed by the mayor.

In order to achieve the public finance targets set by the European Union (see 1997 Stability and Growth Pact), the national government has introduced the Domestic Stability Pact (DSP). The DSP keeps municipalities accountable through a set of subnational fiscal rules, which constrained municipalities by limiting their possibility to accumulate debt and fiscal deficit (see *Legge Finanziaria* 23 December 1998, no. 448, Article 28). To enforce these rules, the national government reduces interest payments for municipalities that complied and cut transfers for those who did not.[9] At the outset, all municipalities, with the exception to those belonging to regions with special autonomy (*Regioni a statuto speciale*), were bound to achieve the set targets. The 2001 tax reform

[8] It is worth mentioning that resources allocated to municipalities (as well as teachers' salary) are independent from students' performance in standardized test scores.

[9] Noncompliers are subject to the following penalties: i. a 5 percent cut in the government transfers; ii. a ban on hires; iii. a 30 percent cut on reimbursement and non-absenteeism bonuses for employees of the municipal administration. By contrast, municipalities complying with fiscal rules benefit from interest rate cuts for loans from the national government. Patrizii et al. (2006) provide evidence of a very large compliance rate in meeting the Domestic Stability Pact requirements.

exempted municipalities with population below 5,000 inhabitants.[10] The rationale behind this exemption traced back to the determination of lightening the burdensome requirements for small administrative structures. In 2013, the exemption threshold was lowered to municipalities with a population below 1,000 residents. The 2015 reform eventually replaced the DSP with a balance budget rule for all municipalities.

The fact that municipalities have limited autonomy in setting local tax rates implies that the fiscal consolidation targets might not be achieved exclusively by raising taxes, but they would need to reduce expenditures. The goal of this paper is to study whether any fiscal rules-induced drop in municipal spending to finance schools has an impact on educational achievement of Italian students in primary schools. To provide context, we illustrate the relevant institutional features of the Italian school system in the next subsection.

2.2 Italian school system and funding

Compulsory education in Italy starts at the age of 6 and lasts for ten years. This education period is organized in two cycles: primary education, which lasts five years, and secondary education for the successive years. In primary schools, which are the focus of our analysis, students are assigned to a class at the beginning of grade 1 and share the same peers until the end of primary school. Students are not required to pass any national examination to enter lower secondary education.

The organization and proper functioning of the schooling system is a prerogative of the national government. In particular, subjects, instruction hours and teaching programs are set at the central level and common across schools throughout the country. The school's principal is in charge of allocating children to classes. Formation criteria are established by the national government and each school needs to ensure that classes are equally distributed by ability, gender, socio-economic background and disability (we will show that this allocation is *de facto* valid in our sample).[11] The national government is also responsible for the hiring and payment of teachers, whose salary is set by a national collective agreement. The lack of any involvement from individual schools in their staff recruitment process implies that schools have no interest in cutting personnel costs or enhancing the quality of their teachers. Teachers' allocation across schools is settled by seniority and follow the same students throughout the primary school (Barbieri et al. 2008).[12]

[10]Over this period, the requirements has been modified, being function of different requirements ranging from imposing direct limits to the fiscal balance to constraints to the growth of the fiscal gap (see Grembi et al. 2016 for details)

[11]School principals are required to check the consistency of classes' characteristics with the criteria set by the national government (see article 15 of decree 331/1998). In primary schools, class size varies between 15 and 26 pupils. Classes with a minimum of 10 pupils can be created in municipalities located in mountains, small islands and geographical areas inhabited by linguistic minorities."Setting practices" (i.e., a selected group of pupils sharing comparable ability or attainments in a specific subject) are not allowed in Italian schools.

[12]We will show that teachers observable characteristics, such as age, gender and education, are well

Since the late 1990s, the Italian education system has lived an intense period of reforms and radical changes, aiming at fostering school autonomy and decentralizing the supply of school resources.[13] Despite the national government has kept the exclusive legislative power on the general organization of the education system (e.g., minimum standards of education, school staff, quality assurance), school spending differences have started to emerge across places (as we previously showed in Figure 1). As a result, municipalities have started to invest a large portion of their budgets to finance schools. In particular, municipalities are in charge of constructing, renovating and maintenance of buildings of pre-primary, primary and lower secondary schools,[14] and to provide a number of basic goods and services, such as school meals, school transportation, textbooks, teaching tools and other educational materials needed to set up laboratories, libraries and gyms (see article 139 of law 112/1998 for details). Municipalities are also charged for water, heating, internet, electricity and cleaning costs.

There is ample descriptive and anecdotal evidence that the supply of municipal school resources and infrastructure 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 60 percent of the school buildings' stock was build over the 1960-1980 period (see Ministry of Education and Research (MIUR), *Anagrafica Edilizia Scolastica website*). School principals and teachers have long lamented that poorly maintained school facilities and a lack of funding to conduct essential repairs prevent schools from delivering their curriculum (see, e.g., *Corriere della Sera*, July 2017).

In the empirical analysis, we will test whether this anecdotal evidence is confirmed in practice. We expect fiscal rules to affect students' performance both through reductions in running costs and missed investments in infrastructure (e.g., renovation and construction of buildings, laboratories and gyms) and adoption of instructive tools, such as whiteboards, computers, projectors or textbooks. As suggested by the existing literature, most of these spending items financed by municipalities can significantly impact test scores.[15]

3 Data and descriptive evidence

The empirical analysis combines three data sources. First, we use administrative records drawn from the Institute for the Evaluation of the Educational System (INVALSI) and

balanced across treated and control schools in our empirical analysis.

[13]For an overview on educational policy-making in Italy during the post-war period, see Grimaldi and Serpieri (2012) and Turati et al. (2017).

[14]Provinces supply these services for upper secondary schools (see law 23/1996).

[15]For instance, Park et al. (2020) shows the effect of heating on test scores in the US; Holden (2016) for textbooks. In the Italian context, Comi et al. (2017) provide evidence on the effectiveness of information and communications technology's adoption on students' achievement.

covering the entire Italian population of students. Second, we exploit rich questionnaires filled out by teachers and gathering several school-level information on resources available. Finally, we collect administrative data on the balance sheets of Italian municipalities, allowing us to retrieve information on current and capital expenditures to finance schools. In Appendix Table A1, we report additional information on the variables employed and their sources.

3.1 Test scores

Our student-level test scores data come from INVALSI: a standardized achievement test administered to all students in second, fifth, eighth and tenth grade in Italy. At every grade, students are tested in literacy and numeracy by means of multiple choice and open-ended questions. Literacy test aims at evaluating pupils' reading comprehension and mastering of vocabulary and grammatical skills. Numeracy test is designed to assess logical and mathematical reasoning in problem solving and interpretation of quantitative phenomena. Student's final attainment in each subject corresponds to the fraction of correct answers spanning the range 0 - 100. In our analysis, test scores are standardized by subject and cohort to have mean 0 and standard deviation equal to 1. To take into account for potential cheating behaviors, test scores are adjusted by a cheating factor provided by INVALSI.[16] The main advantage of using these data is that they provide a comparable and objective measures of performance. Test scores are standardized and nationwide: students are asked to answer the same battery of questions in the same time window and correction is made externally, following a predetermined marking scheme.

For the purpose of our analysis, we focus on the two cohorts enrolled in school year 2011/12 and 2012/13, covering around 500,000 students for each cohort. For these cohorts, INVALSI has recently allowed for the possibility of building a longitudinal version of the data, so that we can observe test scores of the same student in two different moments: second and fifth grade.[17] Our primary outcome variable is the student-level standardized *test score gain* in math and language, computed by subtracting each student's fifth-grade score from her second-grade score. Moreover, we access to detailed demographic and socio-economic information, including gender, age, date of birth, attendance to preschool, immigrant status, and parents' educational attainments and working status.

3.2 Teachers' questionnaires

Our second source of data are questionnaires filled out by primary schools' teachers. INVALSI allows us to match these questionnaires with the students' administrative

[16]To assess the reliability of the test, every year INVALSI randomly selects a number of classes where an external examiner supervises the test administrations and inspects whether the test implementation is conform with the required testing standards (Bertoni et al. 2013).

[17]See Appendix Figure A3 for a graphical representation of the timing of test scores' measurement.

data using a unique (class-level) identifier.[18] They collect detailed information on teachers' demographic information (gender, age, educational attainments), job position (field of study, experience), teaching practices implemented, and school's facilities and environment.

The survey is designed to gather information on teachers' qualification and experience, pedagogical practices, the teachers-school and teachers-students relationship. Crucially for our purposes, the questionnaire contains a set of questions on school and class equipment of educational tools and multimedia devices, such as availability of interactive white boards, tablets, computers and functioning WIFI, among the others.[19]

3.3 School spending

Finally, we collect data on school expenditures from the balance sheets of Italian municipalities, which are annual reports provided by the Italian Ministry of Interior (*Ministero degli Interni*) over the 2001-2015 period. Municipal balance sheets have been introduced with the aim to better monitor local public spending in the frame of the Internal Stability Pact.[20] The current accounting models are homogeneous both across municipalities and over the period of interest.

Our main independent variable in the analysis is municipal school expenditure per-pupil, summing up both current and capital spending. We construct this variable by incorporating the whole history of school investments over the period covered in our dataset, that is the 2001-2015 period. As a matter of fact, the impact of some specific school investments (e.g., investments in facilities or laboratories) needs time to realize and employing contemporaneous measures would not account for them. In alternative specification, we also construct a cohort-specific measure of school expenditure computed over the 5-year when a cohort was enrolled at primary school. Data on the number of pupils come from the Ministry of Education and Research (*Ministero dell'Università e della Ricerca, MIUR*).

Municipal balance sheets include detailed information on the composition of spending across several budget items. We illustrate the composition of spending in the appendix Figure A1, separately for current and capital expenditures. The figure shows that the almost half of municipal current expenditures are invested to finance goods and services (corresponding to about 215 euros per-pupil), while capital spending amounts to nearly 70 euros per-pupil.

[18]The questionnaires have a high response rate: about 80% of teachers filling up the form. This data source is unique, particularly in the Italian context where there are no publicly available data containing valuable information on teachers.

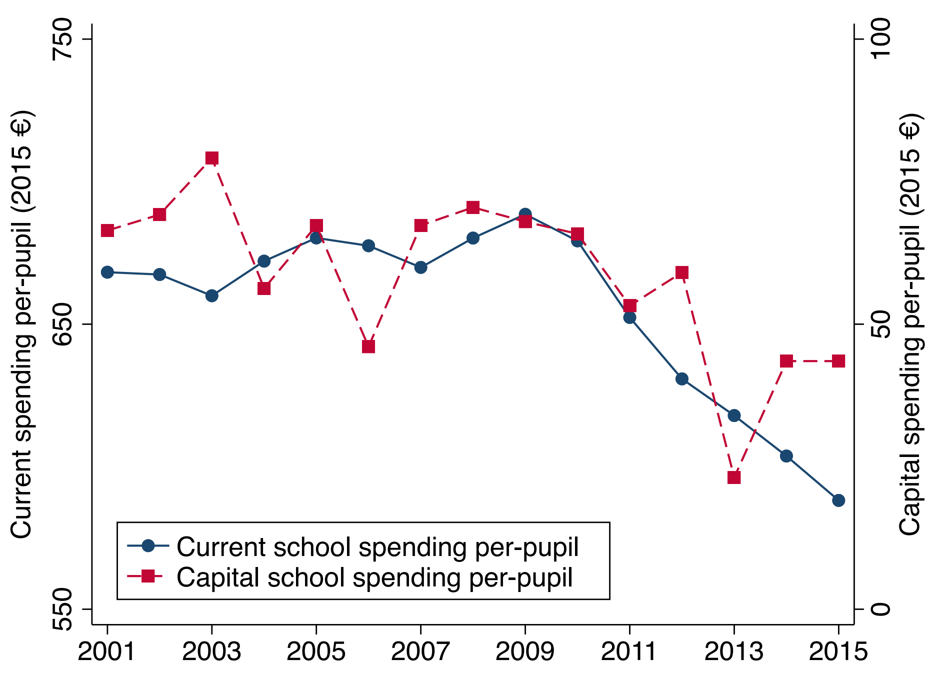
[19]Specifically, teachers' answers are reported in a 4 point scale indicating how much do they use each item: "regolar use", "occasional use", "no usage", "not available at school".

[20]They are approved by the town council by the 30th of April of the following year. The Ministry of Economy and Finance regulates the accounting models through the T.U.E.L. (*Testo Unico Enti Locali).*

3.4 Descriptive evidence

To provide context, we start by documenting the evolution in municipal school expenditure per-pupil over the 2001-2015 period in Figure 2. In the early 2000s, municipalities spend on average around 670 euros per-pupil in current spending, which sum up to nearly 4 billions of euros in total. Capital spending amounted to nearly 60 euros per-pupil in the same period. Both current and capital spending slightly increased during the pre-2009 period, before starting to gradually fall in response to the debt crisis that hit Italy in the early 2010s. In 2015, municipalities spent, on average, around 588 euro per-pupil to finance current spending and 44 euros for capital spending.

Figure 2: Trend in municipal school spending per-pupil

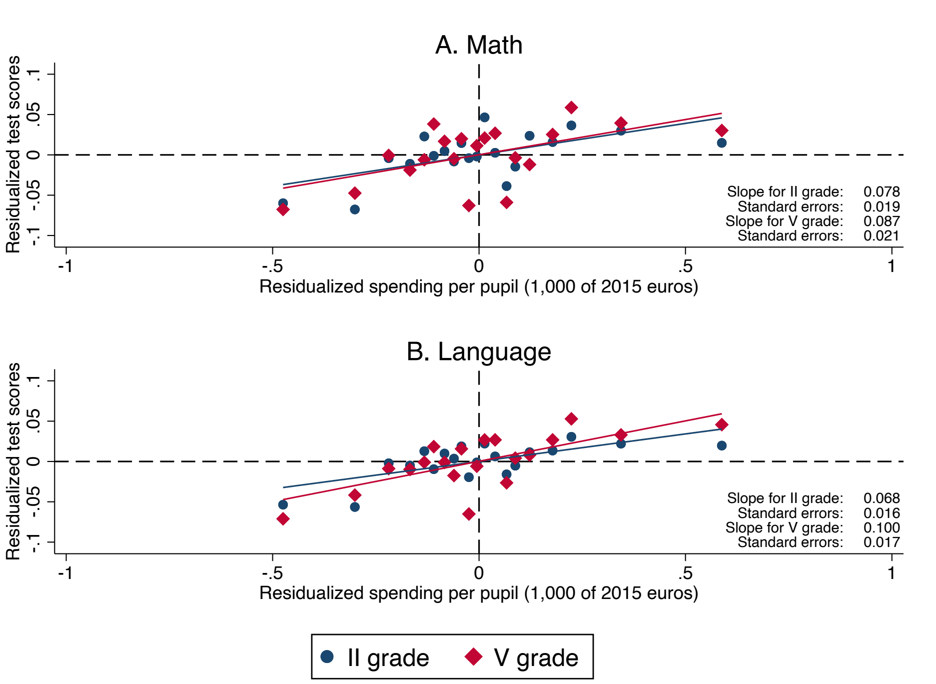


Note: The figure shows the trend in municipal school spending per-pupil (in 2015 euros) over the 2001-2015 period. The blue solid line displays current spending per-pupil (values in left-hand side axis); the red dashed line shows capital school spending per-pupil (values in right-hand side axis). Data from municipal balance sheets.

In appendix Figure A2, we show spending trends separately by macro-areas and according to municipal population size. We find a substantial geographical gradient in spending: municipalities in the richer Northern regions spend, on average, around 250 euro per-pupil more than municipalities located in the poorer Southern regions. In particular, this spending heterogeneity has not converged over time: the difference in spending per-pupil across Northern and Southern municipalities has increased from 170 to 300 euros over the 2001-2015 period. Unsurprisingly, we also find that spending per-pupil is a negative function of municipal population size, reflecting the possibility of exploiting economies of scale for more populous municipalities.

Does this spending differential translate in systematic heterogeneity in student performance? Although it is well known that Northern and Southern Italy differ substantially in several socio-economic and labor market outcomes, there is limited evidence on whether cross-municipality school spending differences have any impact on performance of Italian students.[21] Figure 3, we scatter equal-sized bins of standardized test scores (vertical axis) and municipal school spending (horizontal axis), residualized by provincial dummies. We depict this correlation separately for standardized test score in math (top graph) and language (bottom graph) at in second (black dots) and fifth grade (red dots). This descriptive evidence rests on within-province cross-municipality variation in school spending per-pupil and test scores. The positive slope suggests that school spending has a positive impact on test score. On average, we estimate that a 1,000 euro increase in school spending per-pupil raises standardized math (language) test scores by .078 (.068) of a standard deviation in second grade and by .087 (.1) in fifth grade.

Figure 3: Correlation between school spending and standardized test scores



Note: This figure shows the correlation between standardized test scores in math (top graph) and language (bottom graph) and municipal school spending per-pupil (in thousand of 2015 euros), net of province fixed effects. Blue (red) plotted points are means of observed values and refer to standardized test scores in second (fifth grade). The positive slope suggests that, on average, within-province cross-municipality variations in school spending are associated with better test scores. We report coefficient estimates and standard errors clustered at municipality-level. The sample includes 749,591 students observed in 7,709 municipalities.

Although suggestive, this raw comparison across the universe of Italian municipalities might be misleading and plagued by endogeneity issues, as richer municipalities are much more likely to spend more on schools. Regression-based estimates of the effect of school expenditures on test scores that adjust for municipality and year fixed

[21] Bratti et al. (2007) provide evidence on regional disparities in performance of Italian students using PISA data.

effects as well as observable student background characteristics cannot fully address this problem, as any unobserved municipality-year shock might independently affect school spending and student's performance and would be confounded with omitted variable bias (Blackburn and Neumark 1993; Blackburn and Neumark 1995). In the next sections, we will present our strategy to get rid of this issue in estimating the causal effect of school spending on test scores.

4 Conceptual framework

Our paper aims to estimate the causal impact of fiscal rules-induced changes in school resources on test scores. To help contextualize our research question in the educational literature, we present our empirical analysis in the context of a standard education production function.

Following Todd and Wolpin (2003), we adopt the so called "value-added" specification, which assumes that students' cognitive outcomes at any age are determined by a cumulative function of current and past family and school inputs. We can thus relate students' test score *gains* from age $g-1$ to age g with family and school inputs:

$$\Delta y_{i,m}^{s} = y_{i,g,m}^{s} - y_{i,g-1,m}^{s} = \beta_o + \beta_1 S_m + \beta_2 X_{i,m} + u_{i,m}^{s}, \quad (1)$$

where the outcome variable, $\Delta y_{i,m}^{s}$, is the test score gains, computed subtracting test score in grade $g-1$ from that achieved in grade g, of student i in subject s and enrolled at a school located in municipality m. Test scores gains are determined by a matrix of parents' investments, $X_{i,m}$, and school resources, S_m. Finally, $u_{i,m}^{s}$ is an idiosyncratic error term.

Our coefficient of interest is β_1, which measures the impact of municipality school spending on students' test score. The advantage of using this specification is that it rules out a number of time-invariant (potentially unobservables) individual characteristics and explicitly controls for different starting conditions. In comparison to the contemporaneous specification, the inclusion of lagged test score reduces dramatically the potential for omitted variable bias.

Yet, the causal estimation of β_1 has two main challenges. First, parents might sort across schools. For instance, if parents of high-ability students choose to enroll them into more equipped schools, we may confound school sorting with the impact of better school resources. Second, municipalities might differ along several dimensions, so that we might confound school spending variation with other factors potentially determining students' cognitive outcomes. For instance, municipalities of larger dimensions might provide more opportunities and resources to families and their children, thus affecting students' educational outcomes through mechanisms unrelated to school spending.

The literature has dealt with these issues by using different empirical approaches.

Some studies focusing on the US have implemented a regression discontinuity design exploiting the features of school district-level capital campaigns (Cellini et al. 2010; Martorell et al. 2016; Baron 2019), while others have leveraged (state-level) school finance reforms in an event study or instrumental variable empirical setting (see, among the others, Card and Payne 2002; Guryan 2003; Papke 2005; Jackson et al. 2016; Hyman 2017; Lafortune et al. 2018). A handful of studies have investigated the impact of school spending in England using a geographical boundary discontinuity design (Gibbons et al. 2018) and within-pupil across-subjects variation (Nicoletti and Rabe 2018). The only existing study for Italy in this context, Belmonte et al. (2020), has investigated the impact of school spending on physical infrastructure by exploiting exogenous extra funds that a specific group of schools received in the aftermath of the 2012 Northern Italy earthquake using both a difference-in-difference strategy and an instrumental variable approach. In line with this literature, we estimate the impact of school resources on test scores by exploiting quasi-experimental variation in school resources.

5 Empirical strategy

This section sets out the empirical approach implemented to identify the effects of school spending on student performance.

5.1 Fuzzy regression discontinuity design

Our goal is to relate the gain in test scores for students educated in different schools to the per-pupil baseline spending observed in their municipality of residence during the time they attended primary school. For this purpose, we exploit a discontinuity in municipal spending to finance schools. As previously described, eligibility for fiscal restraints is a function of the municipal population: municipalities with population size below 5,000 are not eligible for fiscal restraints (henceforth, control group), while municipalities above 5,000 are subject to fiscal restraints (treated group). Therefore, we can attribute any observed discontinuous difference in school spending across treated and control group to the impact of fiscal rules.

Although eligibility for fiscal rules solely depends on the cutoff rule, not all the eligible municipalities might necessarily decide to vary school spending. The fact that municipalities might change school spending for non-fiscal rules-related reasons suggests that treatment (i.e., school spending) is not solely defined by fiscal rules. In addition, imperfect compliance in meeting fiscal rules might also generate imperfect treatment take-up. Therefore, as long as these rules gave rise to systematic differences in expenditure across municipalities, school spending will present a *fuzzy* discontinuity at the population threshold (as opposed to a sharp change). This quasi-experimental variation allows to estimate the effect of school spending on student performance by using a fuzzy regression discontinuity (RD) design (Hahn et al. 2001; Lee and Lemieux 2010).

Following the recommendations of Imbens and Lemieux (2008) and Gelman and Imbens (2019), our main specification uses local linear regressions within a given bandwidth of the treatment threshold, and control for the running variable (municipality population) on either side of the threshold.[22] We run the following two-stage instrumental variable regression:

$$\begin{aligned} S_m = \alpha_o + \alpha_1 \cdot 1(Pop_m \geq T) + \alpha_2 \cdot (Pop_m - T) \\ + \alpha_3 \cdot (Pop_m - T) \cdot 1(Pop_m \geq T) + v_m; \end{aligned} \tag{2}$$

$$\begin{aligned} \Delta y_{i,m}^s = \beta_o + \beta_1 \hat{S}_m + \beta_2 \cdot (Pop_m - T) \\ + \beta_3 \cdot (Pop_m - T) \cdot 1(Pop_m \geq T) + u_{i,m}^s, \end{aligned} \tag{3}$$

where the outcome of interest, $\Delta y_{i,m}^s$, is the standardized test score gain in subject s, computed subtracting second grade to fifth grade test scores, of student i enrolled in a school located in municipality m (in alternative specifications, we will also use second grade or fifth grade test scores as the outcome variable). Test scores are standardized by subject, year and grade. S_m measures school spending per-pupil (in 2015 euros) in municipality m as the mean value observed over the whole period. Pop_m is municipal population and T is the threshold defining eligibility for fiscal rules, so that the dummy variable $1(Pop_m \geq T)$ defines treatment and control municipalities. In some specifications, we will add student- and municipality-specific controls, cohort fixed effects and geographical fixed effects. These controls are not necessary for identification, but improve the efficiency of the estimation by reducing the sampling variability. We will present results both with and without these set of controls. Standard errors are clustered at municipality-cohort level.[23]

The fuzzy RD estimator, β_1, calculates the local average treatment effect (LATE) of school spending per-pupil on standardized test scores. This estimate can be interpreted as LATE for "compliers" municipalities, i.e., those who comply with fiscal rules. As in an instrumental variable setting, the treatment effect can be recovered by scaling "intent-to-treat" (ITT) effects of fiscal rules on test scores (the "reduced form") by the first-stage estimate. As previously discussed, ITT effect might be clouded by non-

[22]In our baseline model, we run local linear regression with a triangular kernel optimal bandwidth, computed using the algorithm proposed by Calonico et al. (2014). The only difference between a triangular and a rectangular kernel is that the latter gives larger weights to observations near the threshold. The choice of other kernels functions (e.g., uniform, Epanechnikov, etc.) has little impact in practice.

[23]To keep a common sample across specifications, we compute the optimal bandwidth on the student-specific standardized test score gains averaged at the combined math and language score. Following the suggestion of Imbens and Lemieux (2008), we estimate equations (2) and (3) using the same bandwidth. Although using a wider bandwidth for the treatment equation might improve efficiency, it complicates the computation of standard errors since the outcome and treatment samples used for the estimation would not longer be the same. Results are not sensitive to alternate bandwidth.

compliance with fiscal rules or by the fact that not all eligible municipalities needed to adjust their school expenditure. The two-stage least squares account for this by allowing for the sampling variation introduced in the first stage.

In interpreting the coefficient estimate, we need to stress how our LATE school spending effects might significantly differ from those estimated using alternative identification strategy or samples. Setting apart cross-country institutional differences, our school spending effect relies on variation from municipalities that are relatively small. This does not undermined the external validity relative to Italy, where around 69 percent of municipalities have less than 5,000 inhabitants, but it should be considered by the international reader when putting our results in perspective. As long as the benefit of additional school funding is a negative function of the population (due, e.g., to economies of scale), we would estimate a larger coefficient compared to that estimated leveraging variations from large size municipalities or focusing on aggregated units, such as school districts or states. The main advantage of this approach is that the vast majority of municipalities in our sample has only one school complex. This reduces our analysis to a quasi school-level type of analysis.

5.2 Validity of fuzzy RD design

5.2.1 Absence of manipulation

The key assumption of our fuzzy regression discontinuity design is that assignment around the population threshold that determines eligibility for fiscal rules is locally random. This condition would be violated if municipalities can actively manipulate their population size. Figure A4 shows that the density of the municipality population (as reported in Population Census) is smooth around the threshold for fiscal rules eligibility, as would be expected in a valid RD design (Lee 2008; McCrary 2008); the McCrary discontinuity estimate is -.195 (.263).

5.2.2 Exclusion restriction

We can interpret the fuzzy RD estimate as causal under the assumption that crossing the threshold determining eligibility for fiscal restraints affects the outcome variable only through differences in school spending. Yet, it is possible that being eligible for fiscal restraints could have a direct impact on test score gains through other mechanisms, such as sorting of parents and/or teacher or if fiscal rules distorts local labor market demand and supply.

To assess the plausibility of this assumption, we examine the distribution of a wide range of student- and municipality-level characteristics around the population cutoff. We present this test in Table 1, where we report mean values (columns 1-3), the p-value from the difference in means between values below and above the threshold (column 4), fuzzy RD estimates obtained by running equations (2) and (3) (column 5), and the p-value on the RD estimate (column 6).

We present the following results. First, we find a remarkable balanced distribution of demographics and socio-economic characteristics of students, such as gender, nationality and parental background (through dummies for father or mother employed and dummies for the parents' educational background). Second, we confirm that class-level characteristics, such as school size, the proportion of repeaters and pre-school service attendance rate, are equally balanced across treated and control groups. Finally, we show that the distribution of taxable income and housing price is balanced, thus suggesting that parents did not systematically sort across municipalities (and, thus, schools).

Overall, this test suggests that municipalities below the eligibility threshold are an appropriate counterfactual for those located just above it. Absence of sorting is not surprising, since in our institutional setting and period of interest, these spending differences were hard to observe in the process of choosing schools or residence, and municipal expenditure provides a weak signal about school quality. In addition to these baseline characteristics, next section will show that *baseline* test scores, as measured at the beginning of primary school, do not show any discontinuity, thus suggesting that the distribution of students' cognitive ability is smoothly distributed around the cutoff.

Table 1: Summary statistics and balance

	Full sample (1)	Below threshold (2)	Above threshold (3)	p-value on difference (4)	RD estimate (5)	p-value on RD estimate (6)
Female (0/1)	0.496	0.506	0.494	0.293	-0.013	0.757
Father employed (0/1)	0.938	0.931	0.938	0.560	0.021	0.683
Mother employed (0/1)	0.589	0.543	0.541	0.960	0.013	0.675
Father high education (0/1)	0.121	0.088	0.094	0.461	-0.012	0.748
Mother high education (0/1)	0.173	0.139	0.139	0.986	0.001	0.968
Immigrant (0/1)	0.093	0.079	0.096	0.253	-0.056	0.713
Repeater (0/1)	0.010	0.010	0.011	0.605	0.004	0.789
Pre-school (0/1)	0.919	0.960	0.956	0.879	-0.080	0.702
Females in class (%)	0.491	0.498	0.493	0.638	-0.021	0.692
Repeaters in class (%)	0.021	0.018	0.022	0.372	-0.002	0.868
Immigrants in class (%)	0.105	0.090	0.109	0.227	-0.054	0.722
Low background in class (%)	0.459	0.520	0.506	0.645	-0.123	0.682
School size (# of students)	85.937	60.544	70.035	0.087	2.064	0.898
Altimetry zone (1/5)	238.478	384.896	303.590	0.144	-14.306	0.928
Seaside (0/1)	0.125	0.104	0.103	0.981	-0.121	0.707
Surface (mq)	50.548	49.250	53.592	0.703	-16.410	0.772
Taxable income pc (€)	17,719	17,353	17,589	0.782	464	0.661
Housing price (€/mq)	1,108	946	1,355	0.052	68	0.689

Note: Columns (1)–(3) show the unconditional means for all municipalities, municipalities below the treatment threshold, and municipalities above the treatment threshold, respectively. Column (4) shows the p-value for the difference of means between values reported in column (2) and (3). Column (5) shows the fuzzy RD estimate, following the main estimating equation, of the effect of being above the threshold defining eligibility for fiscal rules on the baseline variable. Column (6) is the p-value for the fuzzy RD estimate.

This result is consistent with the graphical evidence presented in Figure A5, where we run reduced form regressions of fiscal rules on several variables. All the panels depict a smooth evolution around the cutoff determining eligibility for fiscal rules, thus confirming that students' background characteristics and classroom's characteristics are very similar.

5.2.3 Differential attrition and parents' grants

To be included in our longitudinal sample, a student must be enrolled in primary school at the beginning of the second grade and remain in the same school through the end of the fifth grade when we measure again her test score. A threat to our design could arise if students enrolled at a school located in treated municipalities are more (or less) likely to leave our sample (for whatever reason, including school drop out and mobility both towards control and out-of-sample municipalities) than those in control municipalities.[24] We address this concern in Appendix Figure A6, which shows the relationship between population and the probability of not being included in our final fifth grade sample. We find no evidence of discontinuities in the likelihood of missing a student, thus suggesting that differential attrition is not a concern in our empirical framework.

Another potential issue is represented by the possibility that parents in municipalities subject to fiscal restraints might offset lower spending by providing grants to the school attended by their children. We investigate this option by using aggregate information from municipal balance sheets on the amount of grants received from private donors. Figure A9 suggests that this is not the case: the amount of grants received by municipalities is smoothly distributed around the cutoff.

5.2.4 Dynamic effect and 2013 reform

There is a remaining threat to identification that concerns us: the 2013 reform, which moved the fiscal rules' eligibility cutoff from 5,000 to 1,000 inhabitants. As long as these newly treated municipalities immediately reacted by cutting school expenditures, attenuation bias would affect our first stage coefficient α_1. We explore the sensitivity of our specification to this issue in Figure 4, where we depict the dynamic effect of fiscal rules eligibility on the differential in school spending per-pupil between treated and control municipalities (based on pre-2013 fiscal rules' eligibility). This figure points to three conclusions. First, at the onset of fiscal rules in 2001, school spending did not significantly vary across treated and control municipalities. This is a reassuring evidence, suggesting that school spending differences followed fiscal rules' inception and not viceversa. Second, school spending has then started to gradually diverge across the two groups of municipalities. Indeed, from 2006 onward, the differential in spending was broadly stable. Third, the 2013 reform did not have any immediate impact on the school spending differential. Based on the evidence presented above, we believe that

[24] This restriction is similar to the monotonicity assumption in an instrumental variable setting (i.e., being eligible for fiscal rules cannot cause some students to change behavior as a result of the fiscal rules elibility).

the 2013 reform did not represent a threat to our identification approach.[25]

Figure 4: Dynamic effect of fiscal restraints on school spending

Note: The figure shows the dynamic "first-stage" relationship between school spending and eligibility for fiscal restraints. The vertical axis is the annual average differential in school spending per-pupil between treated and control municipalities. Each point is the year-specific regression discontinuity estimate on the effect of fiscal rules eligibility on school spending. Lateral lines represent the 95 percent confidence interval.

6 Results

In this section, we present our main results on the effect of school spending on standardized test scores. We start by providing graphical evidence, then we present our main fuzzy RD estimates. Finally, we analyze heterogeneous responses and we test the robustness of our main findings.

6.1 Baseline results

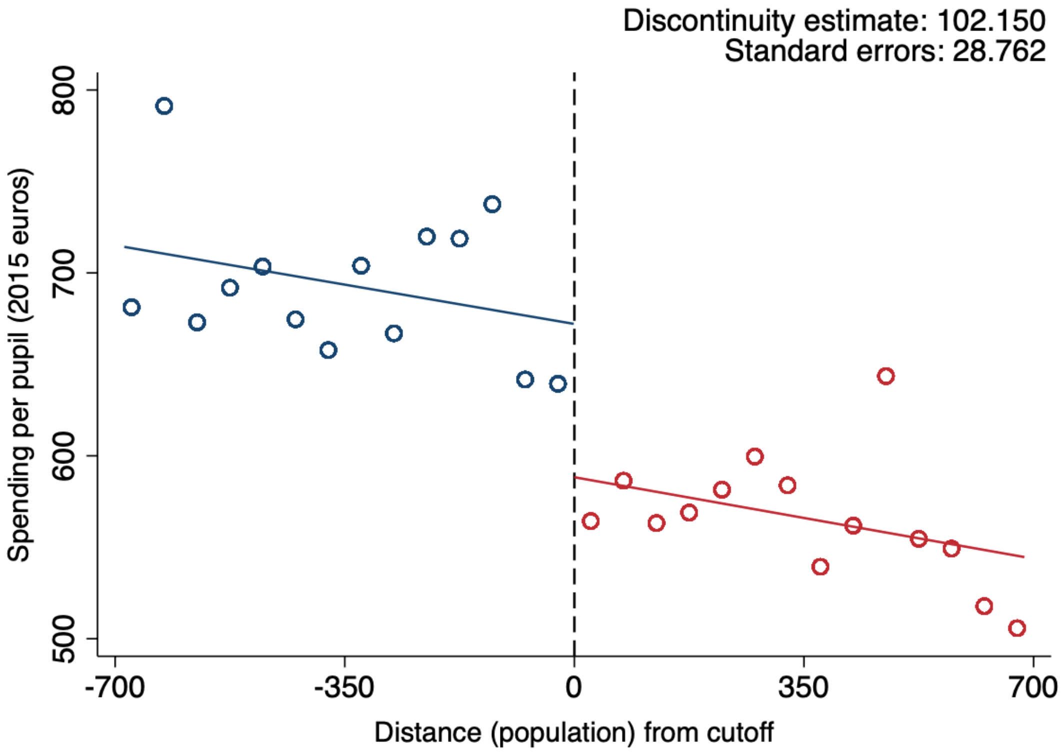
6.1.1 Graphical evidence

We begin by presenting the effect of eligibility for fiscal rules on school spending. Figure 5 plots the "first-stage" relationship between municipality's population size and school spending per-pupil. The graph shows a fuzzy discontinuity in this relationship, thus providing compelling evidence that municipalities eligible for fiscal rules spend significantly less to finance schools. The downward-sloping pattern in this graph is driven by economies of scale, as more populated municipalities purchase inputs at a lower cost per-unit than smaller municipalities.

Figure 6 illustrates ITT effect of eligibility for fiscal rules on standardized test score in math (top panel) and language (bottom panel) on second and fifth grade. Right-

[25]Furthermore, our baseline results are not sensitive to the exclusion of the cohorts enrolled in 2013.

Figure 5: First-stage relationship for school spending and fiscal restraints



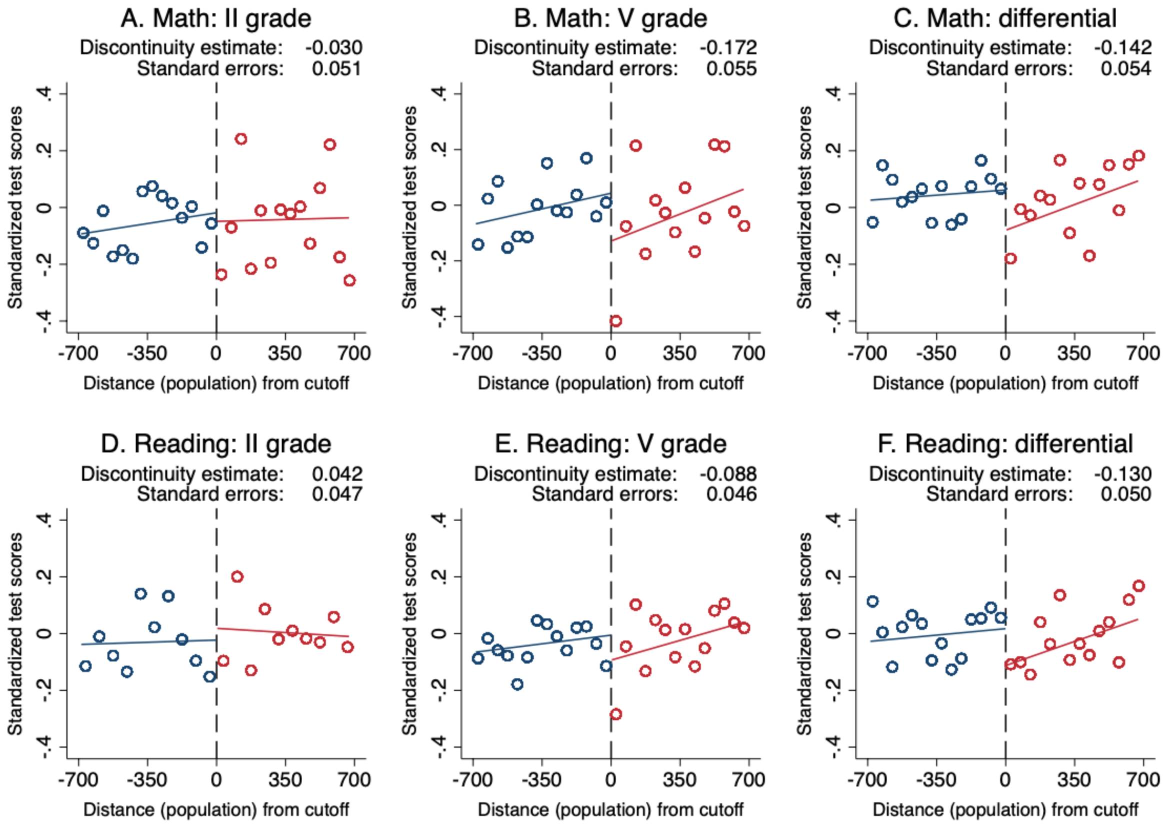
Note: The figure shows the "first-stage" relationship between school spending and eligibility for fiscal restraints. The vertical axis is municipal school spending per-pupil (in 2015 euros). The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 50 population size bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014).

hand side graphs show the test score gains, computed by subtracting each student's fifth-grade score from her second-grade score. In both cases, the graphs show a clear jump at the cutoff determining eligibility for fiscal rules, indicating the positive effect of school spending on students' performance.

6.1.2 Fuzzy RD estimate

Our baseline results are illustrated in Table 2 and Table 3, which present the estimated discontinuities in standardized test score for math (Table 2) and language (Table 3), as well as reduced form and first-stage effect. We run regressions on three outcomes: standardized test score gains (top panel), second grade test score (middle panel) and fifth grade test score (bottom panel). Each outcome variable has mean of 0 and a standard deviation of 1, while school spending per-pupil are hundreds of 2015 euros. The first column presents the ITT effect, while the second column reports the first-stage relationship between eligibility for fiscal rules and municipal school expenditure per-pupil. Columns (3)-(8) display the fuzzy RD estimate of school spending on standardized test score. We start from a baseline model without control, and then we add student-specific controls, class-specific controls, municipality characteristics, cohort fixed effects, and a dummy for each region where the school is located.

Figure 6: Intent to treat effect on standardized test scores



Note: The figure shows reduced form evidence on the effect of fiscal restraints on standardized test scores. The vertical axis is the difference in standardized test score in between fifth and second grade. Scatter points are sample average over intervals of 50 population size bins. Test scores are standardized by subject, year and grade. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014). The sample includes 26,906 students.

The reduced form equations regress test scores on the binary indicator for being above the threshold and linear trends on both sides of the threshold. It computes the ITT impact of eligibility for fiscal rules on test scores. In line with the graphical evidence presented before, the first column shows a large negative effect of fiscal rules eligibility on spending per-pupil. On average, math (language) test score gains are around 14.2 (13) percent larger in municipalities not subject to fiscal rules.

Interpretation of these ITT impacts are clouded by noncompliance with fiscal rules assignment or heterogeneous changes in school spending. Therefore, to estimate the effect of school spending on standardized test scores, the ITT estimates must be scaled up by the probability that fiscal rules trigger changes in school spending. As discussed above, two-stage least squares (2SLS) does this while accounting for the sampling variation introduced in the first stage. Columns (3)-(8) report instrumental variable (IV) estimates, where the ITT estimates are scaled by the first stage estimates.

First stage effect, reported in column (2), shows that eligible municipalities spend, on average, 102 euros per-pupil less than municipalities not subject to fiscal rules, a differential of almost one-fifth of the average school spending per-pupil measured in the

Table 2: School spending and standardized math test score

	ITT	First stage	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. *Outcome*: Math test score gains						
$1(Pop_m \geq T)$	-0.142***	-1.021***					
	(0.054)	(0.288)					
Spending per-pupil			0.139**	0.134**	0.168*	0.167*	0.169*
			(0.066)	(0.065)	(0.088)	(0.088)	(0.091)
	B. *Outcome*: Math test score at second grade						
$1(Pop_m \geq T)$	-0.030	-1.021***					
	(0.051)	(0.288)					
Spending per-pupil			0.030	0.023	0.041	0.041	0.050
			(0.051)	(0.050)	(0.064)	(0.064)	(0.065)
	C. *Outcome*: Math test score at fifth grade						
$1(Pop_m \geq T)$	-0.172***	-1.021***					
	(0.055)	(0.288)					
Spending per-pupil			0.169**	0.156**	0.209**	0.208**	0.219**
			(0.071)	(0.069)	(0.097)	(0.097)	(0.103)
# of students	26,906	26,906	26,906	26,906	26,906	26,906	26,906
# of municipalities	279	279	279	279	279	279	279
Student-level controls	No	No	No	Yes	Yes	Yes	Yes
Municipality-level controls	No	No	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	No	Yes	Yes
Region FE	No	No	No	No	No	No	Yes

Note: Column (1) presents intent-to-treat effect of eligibility for fiscal rules on standardized test scores in math. Column (2) shows the first stage regression estimate of fiscal rules on municipal school spending per-pupil (hundreds of 2015 euros). Column (3)-(8) display the fuzzy RD coefficients. Test scores are standardized by subject, year ad grade. Standard errors in parenthesis clustered at municipality-cohort level.

sample estimate (mean value of 520 euro per-pupil). This differential in school spending engendered by fiscal rules causes a significant differential in math and language skills. Columns (3)-(8) show that school spending has a significant positive effect on test score gains in both math and language.

Column (3) in top panels shows that a 100 euro increase in school expenditure per-pupil raises test score gains by 13.9 (12.7) percent of a standard deviation in math (language). As expected, estimates are not sensitive to the inclusion of the baseline covariates: point estimates are substantially similar across specifications.

Crucially, we do not find any significant difference in test score at second grade, thus suggesting that baseline test scores did not present any discontinuity when measured at the beginning of primary school. This result implies that all the determinants of school performance were smoothly distributed around the cutoff determining eligibility for fiscal rules, while differences emerge once exposed to different expenditures.

Overall, these tables point to three main conclusions. First, municipalities not eli-

Table 3: School spending and standardized language test score

	ITT	First stage	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	A. *Outcome*: Language test score gains						
$1(Pop_m \geq T)$	-0.130***	-1.021***					
	(0.050)	(0.288)					
Spending per-pupil			0.127**	0.121**	0.134*	0.135*	0.146*
			(0.059)	(0.058)	(0.073)	(0.073)	(0.078)
	B. *Outcome*: Language test score at second grade						
$1(Pop_m \geq T)$	0.042	-1.021***					
	(0.047)	(0.288)					
Spending per-pupil			-0.041	-0.050	-0.046	-0.046	-0.054
			(0.048)	(0.049)	(0.061)	(0.061)	(0.062)
	C. *Outcome*: Language test score at fifth grade						
$1(Pop_m \geq T)$	-0.088*	-1.021***					
	(0.046)	(0.288)					
Spending per-pupil			0.086*	0.072	0.089	0.089	0.092
			(0.049)	(0.046)	(0.061)	(0.061)	(0.062)
# of students	26,906	26,906	26,906	26,906	26,906	26,906	26,906
# of municipalities	279	279	279	279	279	279	279
Student-level controls	No	No	No	Yes	Yes	Yes	Yes
Municipality-level controls	No	No	No	No	Yes	Yes	Yes
Cohort FE	No	No	No	No	No	Yes	Yes
Region FE	No	No	No	No	No	No	Yes

Note: Column (1) presents intent-to-treat effect of eligibility for fiscal rules on standardized test scores in language. Column (2) shows the first stage regression estimate of fiscal rules on municipal school spending per-pupil (hundreds of 2015 euros). Column (3)-(8) display the fuzzy RD coefficients. Test scores are standardized by subject, year ad grade. Standard errors in parenthesis clustered at municipality-cohort level.

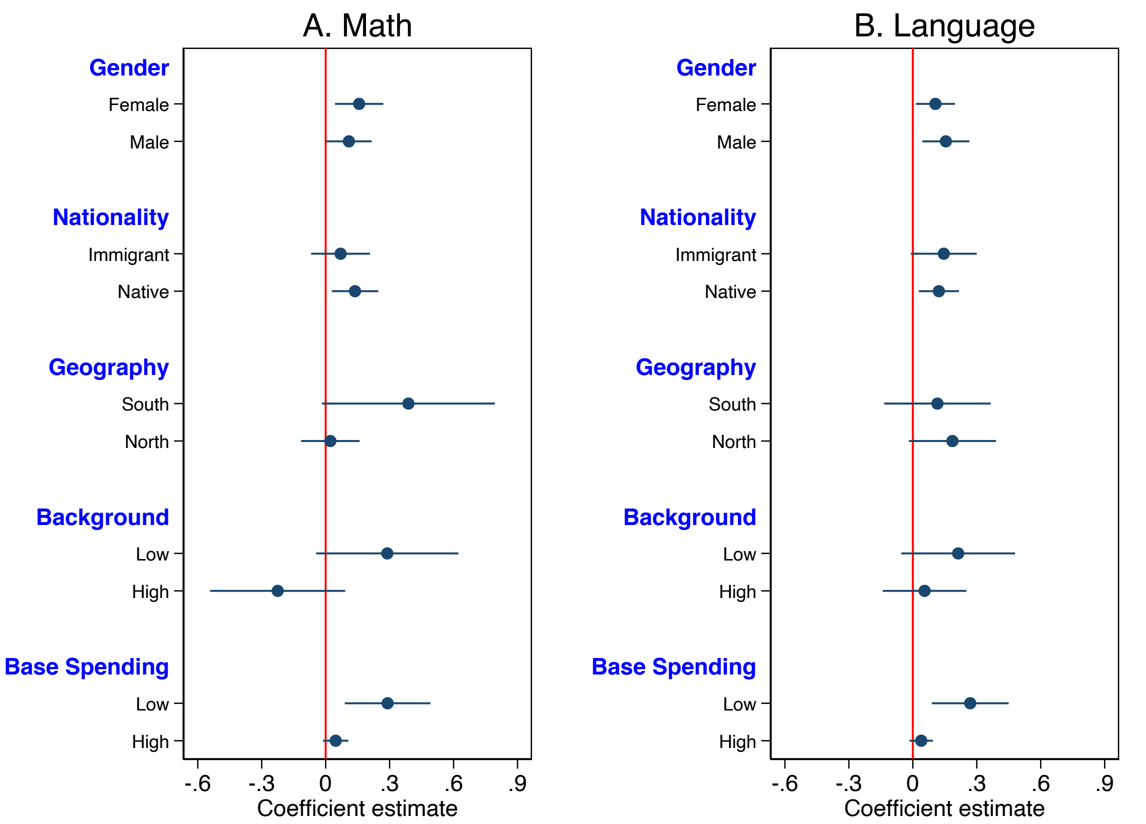
gible for fiscal rules enjoyed larger spending to finance schools. Second, standardized test scores did not present any discontinuity at the beginning of primary school. Third, larger spending lead to performance improvements at the end of the primary school.

6.2 Heterogeneity analysis and distributional impact

We now look at heterogeneous effects of school spending on test score gains. In Figure 7, we report fuzzy RD coefficient estimate and 95 percent confidence intervals from specifications as in column (3) in Table 2 and Table 3. We focus on five sources of heterogeneity. First, we estimate the impact of school spending by student gender. While the point estimate is somewhat larger for female (male) students in math (language), differences are small and not statistically significant.

We do not find any heterogeneous impact also when we look at the effect by nationality. This finding is in line with Lafortune et al. (2018), which do not find any

Figure 7: Heterogeneity analysis



Note: This figure tests heterogeneous effects of school spending on means of standardized test score gains. We report fuzzy RD coefficient estimate and 95 percent confidence intervals obtained from subsamples including only female vs male students; municipalities located in South vs North of Italy; students with low vs high background (based on parents' occupation); municipalities with low vs high baseline spending. Standard errors clustered at municipality-cohort level.

heterogeneous effect of school finance reforms on achievement gaps between white students and those belonging to minority groups.

Third, we study geographical heterogeneity by running regressions separately for municipalities located in Southern and Northern regions. We find that school spending has a significantly larger impact on math test scores of students located in the Italian *Mezzogiorno*.

Fourth, we investigate the role of parental background. Most of the previous studies focusing on the impact of school spending find that low income-students are the most affected group (see, e.g., Jackson et al. 2016; Lafortune et al. 2018; Jackson et al. 2020). However, Hyman (2017) finds the opposite result in Michigan, where districts targeted the marginal dollar toward schools serving students' of wealthier families within the district. It follows that the extent to which school spending cuts may disproportionately harm disadvantaged students remains an open question. To shed light on this issue, we classify students' background using the Economic, Social and Cultural Status (ESCS) index, which is constructed by INVALSI applying principal component analysis taking advantage of the information provided by students and schools on families' cultural resources, and parents' educational levels and working status.[26] Specifically, socio-economically disadvantaged students are defined as those in the bottom quartile of the ESCS index; advantaged students as those in the top quartile of the ESCS index. Figure 7 shows that school spending cuts harm significantly more low-background students, particularly in math. A simple explanation would be that disadvantage families are unable to compensate the lack of school resources by providing additional resources or instruction at home.

Does spending differences contribute to widening the gap in cognitive abilities? In Figure 8, we compare the test score gains' distribution in high-spending vs low-spending municipalities (as those in bottom vs top decile of the school spending per-pupil, respectively). Although descriptive, this figure provides clear evidence that school spending differences translate in larger dispersion in test scores. As long as these inequalities have the potential to persist over the long run, spending differences might also contribute, at least in part, to labor market and other socio-economic outcomes, as illustrated in the work of Chetty et al. (2011) and Jackson et al. (2016). In other words, this evidence suggests that investing in school facilities and educational tools has the potential for increasing equity and reduce the influence of socio-economic background on students' educational achievements.

Finally, we test heterogeneous responses with respect to the initial level of per-pupil spending. Recent evidence on the impact of large-scale educational investments suggests that these investments are particularly likely to improve students' outcomes in context where existing resources were obsolete or non-existing (Duflo 2001; Aaronson and Mazumder 2011; Neilson and Zimmerman 2014). Consistently, we find that the effect of school spending cuts were concentrated in places with lower baseline spending. We will come back to this point in section 7, where we will shed light on the specific spending categories and tools that were particularly underfunded.

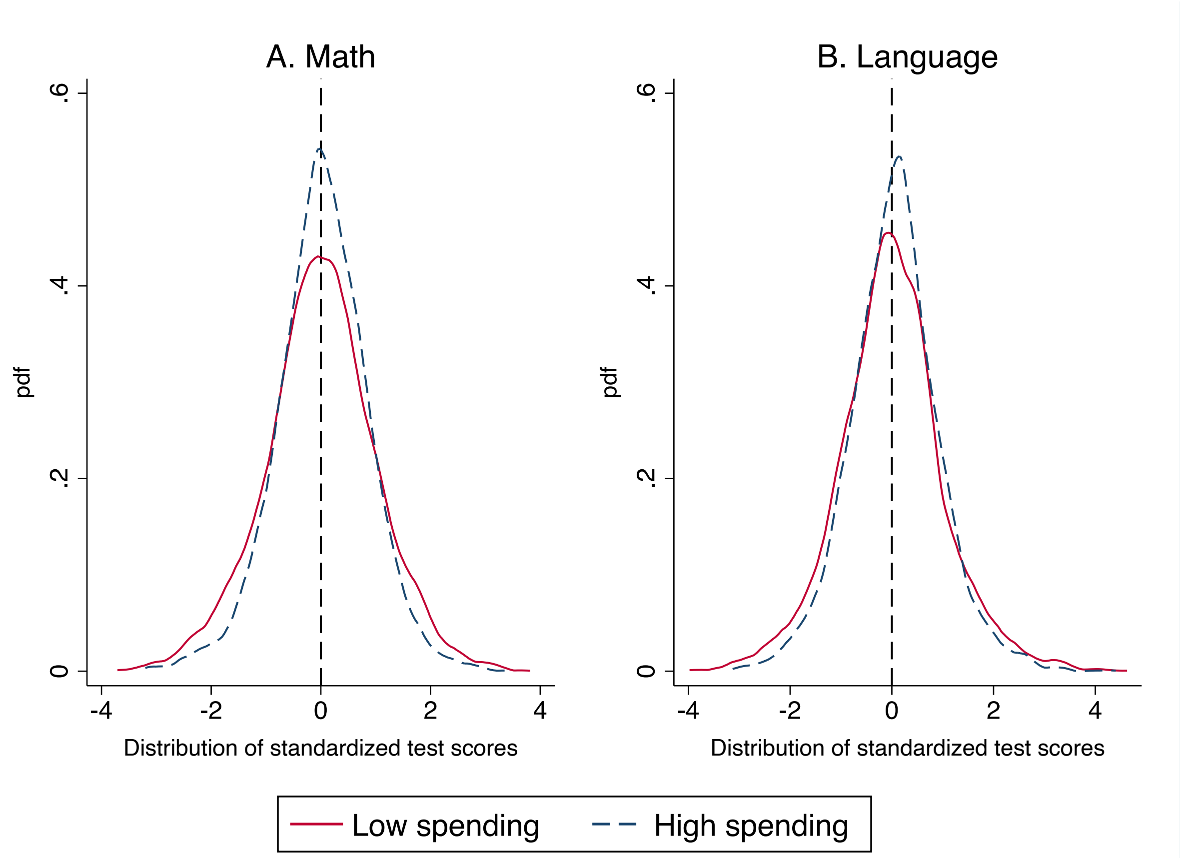
6.3 Robustness checks

6.3.1 Other municipal budget categories

Our fuzzy RD estimate relies on the assumption that the relationship between eligibility for fiscal rules and test scores is a school spending effect. To corroborate this causal relationship, we estimate our first stage model presented in equation (2) on several other municipal spending categories. Then, we regress standardized test score gains on each instrumented spending category using the fuzzy RD model presented in equation (3). We show these results in Appendix Table A2, which presents coefficient estimates and standard errors for seven different municipal budget items. This table consistently shows that eligibility for fiscal restraints led municipalities to significantly

[26] By construction, the ESCS index has null mean and unit standard deviation. It implies that a student with a strictly positive individual value on the ESCS index has a socio-economic-cultural background more favorable than the Italian average.

Figure 8: Distributional impact of school spending



Note: This figure depicts the distribution of standardized test score gains in math and language in municipalities with spending in the bottom decile (solid line) and top decile (dashed line) of school spending per-pupil distribution.

change the composition public expenditures. However, none of the first-stage effects turn out to have a statistically significant effect on test scores. This suggests that the impact of fiscal rules on test scores is exclusively through school spending cuts.

6.3.2 Other policies changing at the same cutoff

A different threat to our identification could come from other policies changing discontinuously at the same population threshold. Our fuzzy RD coefficient estimate provides an unbiased estimate of the effect of school spending cuts on test scores under the assumption that there is no interaction between the fiscal rules-induced school spending cut and other policies or factors that changed discontinuously at the 5,000 cutoff. This assumption might be violated if local administrators just below and just above the cutoff, who are paid differently, are systematically different in their preferences for school spending and/or their ability to provide a more efficient allocation of public resources.[27] We test this assumption by estimating jumps in per-pupil school

[27] Higher mayor's wage can positively select local administrators and increase their performance (Gagliarducci and Nannicini 2013). If this is the case, then our estimates might be *downward* biased, since they would not account for the fact that municipalities eligible for fiscal restraints select local

spending at population thresholds where eligibility for fiscal rules does not change, but mayor's salary does. As in the treatment effect literature (Imbens 2004), we test for a zero effect at these "fake" population cutoff, where we do not expect any discontinuity in per-pupil school spending. Figure A7 shows that school spending does not present any significant jump in these cases, thus indirectly suggesting that the change in mayor's salary would not confound the effect that we assign to eligibility for fiscal restraints.

6.3.3 Teacher sorting

One concern is that teachers might eventually sort from less equipped toward more equipped schools and, thus, from control to treated municipalities.[28] To dispel doubts on this point, we take advantage of the detailed information on teachers demographics collected in the *teachers' questionnaires*. Reassuringly, Figure A10 shows that several teachers' observable characteristics, such as age, gender, education and experience, are well balanced across treated and control schools in our empirical analysis, reporting no significant differences. This is not surprising given that teachers' allocation is decided by the national government and based on teachers' seniority. As we discussed above, the fact that teachers cannot apply to a target school and schools cannot select or retain teachers strongly validates our assumption of the absence of teachers' sorting.

6.3.4 Sensitivity to bandwidth choice

Finally, we test whether our results are sensitive to bandwidth choice. Appendix Figure A11 reports coefficient estimates and 95 percent confidence intervals obtained by estimating our baseline model using different bandwidths around the threshold. The figure shows that our coefficient estimate is qualitative similar when using a reasonable range of bandwidths.

6.4 Cross-cohort analysis

A remaining concern is that there might be other municipality-specific characteristics or policies that are not captured by our set of controls and might confound the effect we ascribe to school spending. We attempt to account for this issue by exploiting within-municipality cross-cohort variation in exposure to fiscal rules. This approach allows us to leverage the differential (exogenous) exposure to fiscal rules across cohorts to identify school spending effects. It relies on the fact that school spending differences across municipalities began to gradually widen after the onset of the Domestic Stability Pact.[29]

administrators that are more able to provide a more efficient allocation of public resources.

[28]For instance, Barbieri et al. (2011) show that teachers tend to move away from disadvantaged areas and mostly go back to their hometowns at the middle or end of their career.

[29]This approach is similar in spirit to the one proposed by Jackson et al. (2016), which study the effect of school finance reform-induced changes in public school spending on long-run adult outcomes by

Namely, we focus on municipalities that eventually became eligible for fiscal rules and we instrument per-pupil school spending with the interaction between per-pupil *baseline* spending, $BaseSpending_m$ (i.e., per-pupil spending observed in the oldest cohort),[30] and the time elapsed from the introduction of the Domestic Stability Pact, $(Year_c - introDSP_m)$.[31] We run the following regressions:

$$S_{m,c} = \alpha_o + \alpha_1[BaseSpending_m \cdot (Year_c - introDSP_m)] + \gamma_m + \delta_c + v_{m,c}; \tag{4}$$

$$y^g_{i,m,c} = \beta_o + \beta_1 \hat{S}_{m,c} + \gamma_m + \delta_c + u^g_{i,m,c}, \tag{5}$$

where $S_{m,c}$ is a municipality-by-cohort specific measure of per-pupil school spending, computed as the 5-year average that a cohort c has spent in primary school in municipality m. We include municipality fixed effects, γ_m, and cohort fixed effects, δ_c, to exploit within-municipality variation across consecutive cohorts to identify the effect of school spending on test scores. These fixed effects allow us to control for any unobserved time-invariant municipality characteristics or policies as well as any nationwide policy change. Differently from our baseline model presented in equations (2) and (3), here our outcome variable is the student-specific test score in fifth grade, since we miss second grade test scores for earlier cohorts.

Our prediction is that later cohorts will be more exposed to the depressing effect of the Domestic Stability Pact than earlier cohorts. Exploiting this pattern, we document a robust negative correlation between per-pupil baseline spending and the annual decline in per-pupil school spending following the inception of the Domestic Stability Pact: a_1 coefficient estimate is -.023 (.004). This coefficient estimate implies that an additional year of exposure to fiscal rules reduces per-pupil school spending by 2.3 percent (about 12.3 euros) relative to baseline spending.

Table 4 presents the results β_1 coefficients estimate obtained by running equations (4) and (5). We find that a 100 euros increase in per-pupil spending raises standardized test scores by 18.6 percent in math, and by 15.4 percent in language. These coefficients are remarkably similar to those estimated from our baseline specifications presented in Table 2 and Table 3. This evidence reassures us: fiscal rules-induced school spending effects appear fairly similar both if computed by exploiting cross-cohort within-municipality variation or by using cross-municipality variation.

In the rest of the table, we test the robustness of our results. The empirical approach presented in equation (4) and (5) requires that test scores in municipalities with different per-pupil baseline school spending were not differentially affected by the intro-

leveraging the differential cross-cohort exposure to school finance reforms depending on place and year of birth.

[30] In this sample, the oldest cohort is composed of students enrolled in 2003.

[31] In addition to cross-cohort within-municipality variation, additional variation within a given cohort comes from municipality that became eligible for fiscal rules in different years. This is due to eligibility cutoff changes and when municipality crossed the cutoff due to population growth.

Table 4: Cross-cohort analysis

	(1)	(2)	(3)	(4)	(5)
	A. *Outcome*: Math test score at fifth grade				
$Spending_{m,c}$	0.186***	0.189***	0.188***	0.162***	0.102**
	(0.054)	(0.054)	(0.054)	(0.050)	(0.044)
	B. *Outcome*: Language test score at fifth grade				
$Spending_{m,c}$	0.154***	0.164***	0.167***	0.140***	0.102***
	(0.043)	(0.044)	(0.044)	(0.040)	(0.036)
# of students	177,544	177,544	177,544	177,544	177,544
Municipality FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
Student controls	No	Yes	Yes	Yes	Yes
Student controls x Cohort FE	No	No	Yes	Yes	Yes
Municipality time-varying controls	No	No	No	Yes	Yes
Province-Cohort FE	No	No	No	No	Yes
First stage F-stat	40.706	40.702	40.793	42.085	41.213

Note: This table reports coefficient estimates and standard errors clustered at municipality-cohort level obtained regressing equations (4) and (5).

duction of the Domestic Stability Pact for reasons other than through school spending. We account for changing demographics across cohorts (columns 3) by interacting students' characteristics with cohort fixed effects. Our coefficient estimate remains highly similar.

Another confounding factor might be represented by any municipality-level time-varying shocks. We attempt to account for these factors by including two proxies in column (4): taxable income per-capita and house price index. Although not perfect, these variables aim to capture any change in amenities or the local business cycle that might be otherwise omitted from our model. Our coefficient estimate reduces somewhat, but it remains statistically significant at usual confidence intervals. Finally, in column (5), we incorporate province-by-cohort fixed effects that allow us to account for any local business cycle or province- and region-level policies. The coefficient estimates remain fairly similar across specifications.

7 Mechanisms

In this section, we use our fuzzy RD specification to study whether the impact of school spending on standardized test scores can be explained by characteristics of classrooms, lack of investments, and under-provision of school facilities. Exploiting the granularity and richness of our data, we explore which are the spending categories mostly affected by the fiscal-rules induced drop in school spending. This exercise would shed light on the main mechanisms driving the observed effects on test scores. The existing empirical literature offers little guidance in this sense. Most of the previous studies employ district-level variation in school spending and cannot detail which specific school features or educational tools were introduced or renovated. Additionally, they do not measure effects on directly treated students (see, e.g., Cellini et al. 2010 and Martorell et al. 2016).

7.1 Spending categories

In our main analysis, we show that municipalities eligible for fiscal rules spend less to finance schools, and lower school spending is causally associated to smaller test scores. A natural question is then: in what do municipalities eligible for fiscal rules spend less? Exploiting information from municipal balance sheets, we can retrieve specific information on spending in the following budget items: "Good and service provision"; "Non-teaching personnel costs"; "School assistance, transportation, school meals and other services"; "Infrastructure"; "Interest payable on loans"; "Other expenses".[32] Figure 9 plots the "first-stage" relationship between eligibility for fiscal restraints and the level of spending by type of expenditure (in per-pupil units). In almost each budget item, we find large and significant spending differences. Namely, we find that municipalities eligible for fiscal rules spend around 33 euros per-pupil less in capital spending, nearly 30 euros per-pupil less for providing goods and services, and slightly more than 20 euros per-pupil less for hiring non-teaching staff and to provide transportation and school meals. "Interest payable on loans" is the only item where we observe a positive sign.[33]

Overall, this exercise leads to two main conclusions. First, our findings suggest that the effect in the main analysis are mediated by cuts in all key school spending categories. Second, we detail a number of direct and indirect unexplored channels that might contribute explaining our results (e.g., spillover effects deriving by a more efficient school transportation service or the hiring of better non-teaching staff).

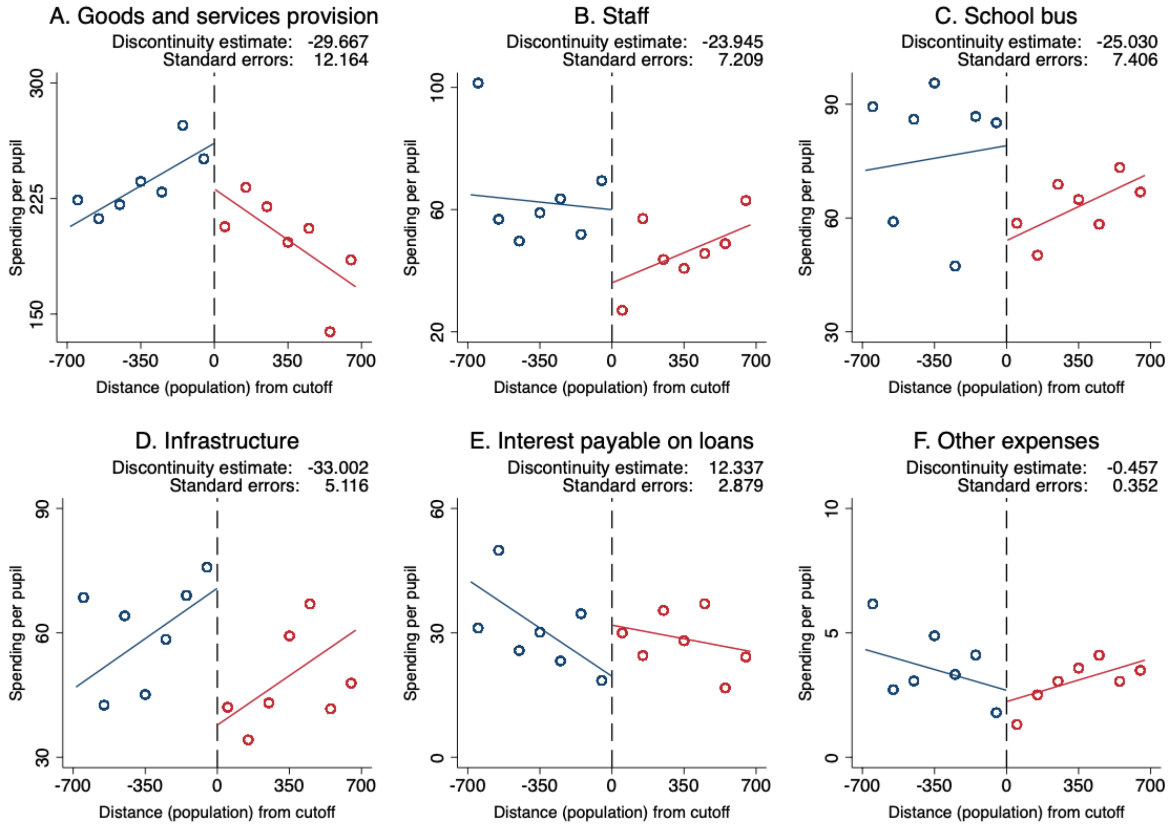
7.2 Teachers' tools

After showing that the effect of fiscal rules operates through reductions in different school spending categories, we now focus on the provision of instructive materials. For this end, we use the teachers' questionnaires data to gather school-level information on the availability of ICT tools. Specifically, we look at whether a school is equipped with interactive white boards (IWB), tablets, computers and access to the internet. Well-equipped schools might motivate teachers and as well as making the instruction time more effective by benefiting children's learning. By the same token, fully furnished

[32]Non-teaching personnel costs include the salary of janitors and special education teachers. As explained in section 2.2, the national government pays teachers' salary. In the very last category, we include some small budget items, such as "yearly depreciation cost" and "usage cost for third-party properties."

[33]This might suggest that municipalities subject to fiscal-rule are more likely to borrow money to finance their school spending budget, so that the increase in interest cost might be larger.

Figure 9: Sources of school spending and fiscal restraints



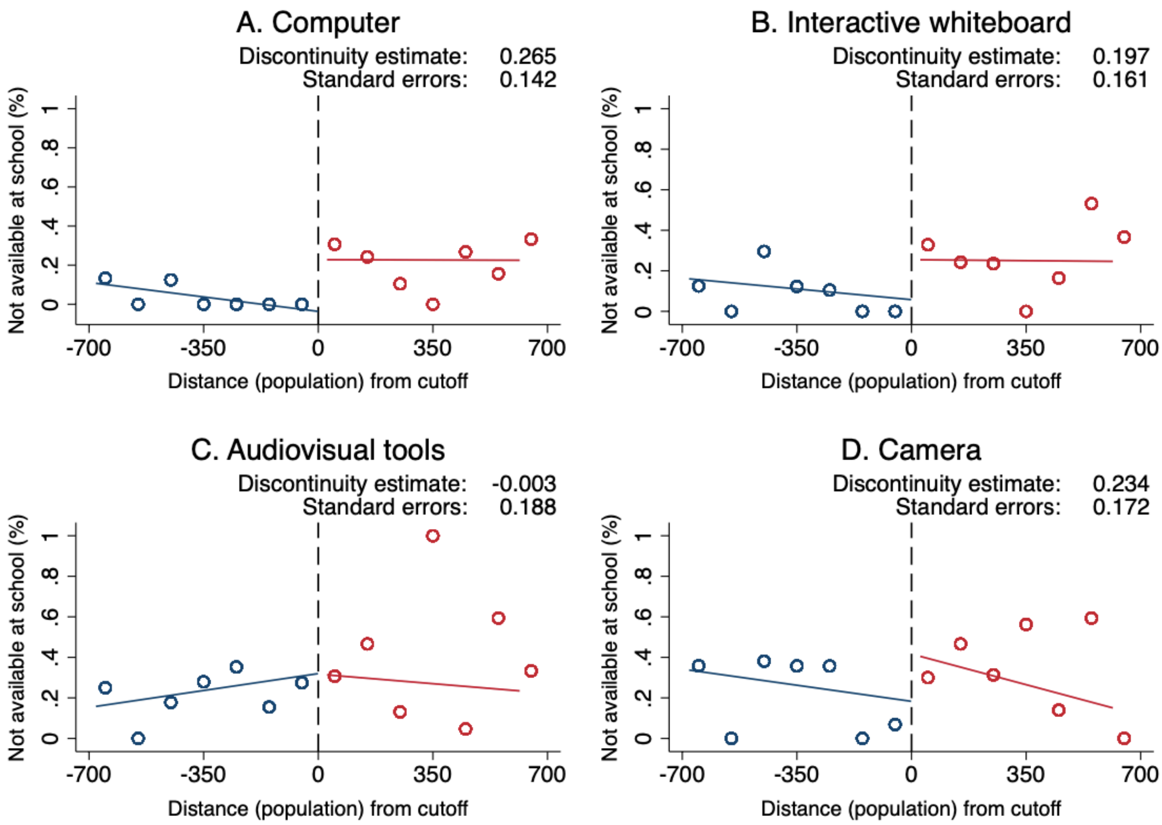
Note: The figure shows the relationship between school spending on each budget item and eligibility for fiscal restraints. The vertical axis is municipal school spending per-pupil in each item (in 2015 euros). The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

schools might act as a signal to students, communicating that the school is properly managed, teachers are actively engaged and that students' has to mainly focus on academic challenges.

We report the results of this analysis in Figure 10. The figure shows the first-stage estimates on the impact of fiscal rules eligibility on the *lack* of computers, interactive whiteboards, audiovisual tools and cameras. We find that the probability that computers are not available at school is one-fourth larger in municipalities eligible for fiscal rules. Similarly, the figure shows a negative relationship between fiscal rules and access to interactive whiteboards and cameras (although the coefficient estimates are not statistically significant at usual levels).

This finding relates to the growing literature investigating the impact of ICT on students' performance. In recent years, a number of studies have investigated whether investments on ICT infrastutures at school promote students' learning providing mixed evidence (Comi et al. 2017, Machin et al. 2007, Leuven et al. 2007). Crucially, most of the existing literature evaluates specific programs or reforms assigning extra funds to introduce or reinforce schools ICT infrastructure. Unlike previous studies, we are able

Figure 10: The impact of fiscal restraints on teacher's tools



Note: The figure shows the impact of eligibility for fiscal restraints on lack of the following tools: computer, interactive whiteboard, audiovisual tools, and camera. The vertical axis is the share of municipalities where these facilities are missing. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

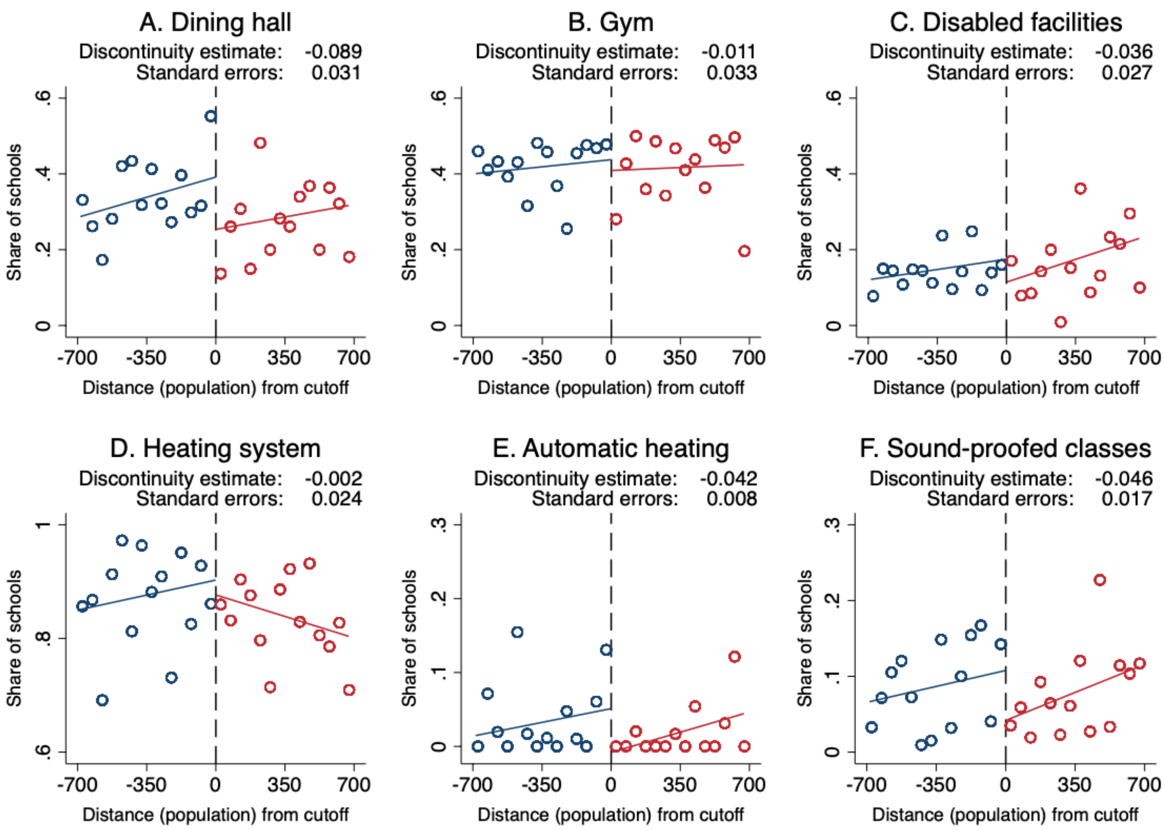
to investigate the unintended consequences of an unprecedented drop in the financing of local authorities.

7.3 School facilities, infrastructure and innovation

Finally, we zoom on the impact of fiscal restraints on school facilities, infrastructure and adoption of innovative technologies. Ameliorating the schooling environment and facilities might benefit students' learning through several channels. For instance, well-maintained facilities can reduce students' discomfort during instruction hours and tests days, minimize distractions, and provide students the proper set of tools for accomplishing learning tasks. Furthermore, well-designed and renovated schools can also reduce missed schooling hours and days. For instance, Park et al. (2020) provide suggestive evidence that heat inhibits learning and that school air-conditioning might mitigate detrimental effects. Both students and teachers might find difficult to work and focus during hotter days and might become less likely to attend schools in extreme temperature cases directly reducing the amount of instructional time. Benefits of investments in educational facilities and services might also realize into a number of non-cognitive outcomes such as enhanced students' safety and effort.

In Figure 11, we report the first-stage estimates on the impact of fiscal rules' eligibility on the availability of the following school-level outcomes: "a dining hall is present and functioning"; "a gym is present and functioning", "facilities for disabled students are available"; "a heating system is present and functioning"; "the school has adopted technologies allowing for automatic heating"; "classes are sound-proofed." The picture emerging from this figure again tells us the usual story: municipalities eligible for fiscal rules are less likely to have a functioning dining hall (although the *availability* of a dining hall in school is not affected), to adopt technologies that allow to make classes sound-proofed, to have automatic heating (even if an heating system is available almost everywhere), and to have invested in facilities for disabled students. Overall, we provided extensive evidence showing that fiscal restraints caused the cut of *productive* educational spending.

Figure 11: The impact of fiscal restraints on school facilities



Note: The figure shows the impact of eligibility for fiscal restraints on each school-level outcomes. The vertical axis is the share of municipalities where these facilities are provided. The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 50 population size bins.

8 Conclusions

Whether school spending has an impact on students' educational achievements is disputed both in policy and academic circles. The existing literature provides mixed evidence on the issue, with some studies finding positive effects and others finding little or no effects (Jackson 2018). In this paper, we study the impact of school resources on standardized test scores. Our laboratory is Italy, which offers rich administrative data and quasi-experimental variation that allow us to offer novel evidence on this long-standing question.

We provide clear evidence that school spending significantly affects the test scores of Italian students. In particular, our paper provides the first empirical evidence on the indirect effect of fiscal restraints on cognitive abilities. Results are remarkably consistent to a number of specification and robustness checks. Additionally, investigating heterogeneous effects, we find that our results are largely driven by students coming from disadvantaged background and from municipalities with a low level of baseline spending. We detail two main channels through which the effects operates. First, fiscal rules-induced cuts in school spending might harm students' educational attainments by indirectly lowering the availability of instructional tools. Second, municipalities eligible for fiscal restraints invest relatively less to finance school infrastructure and facilities, and are less likely to adopt innovative technologies.

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Appendices

Table A1: Definition of variables and sources

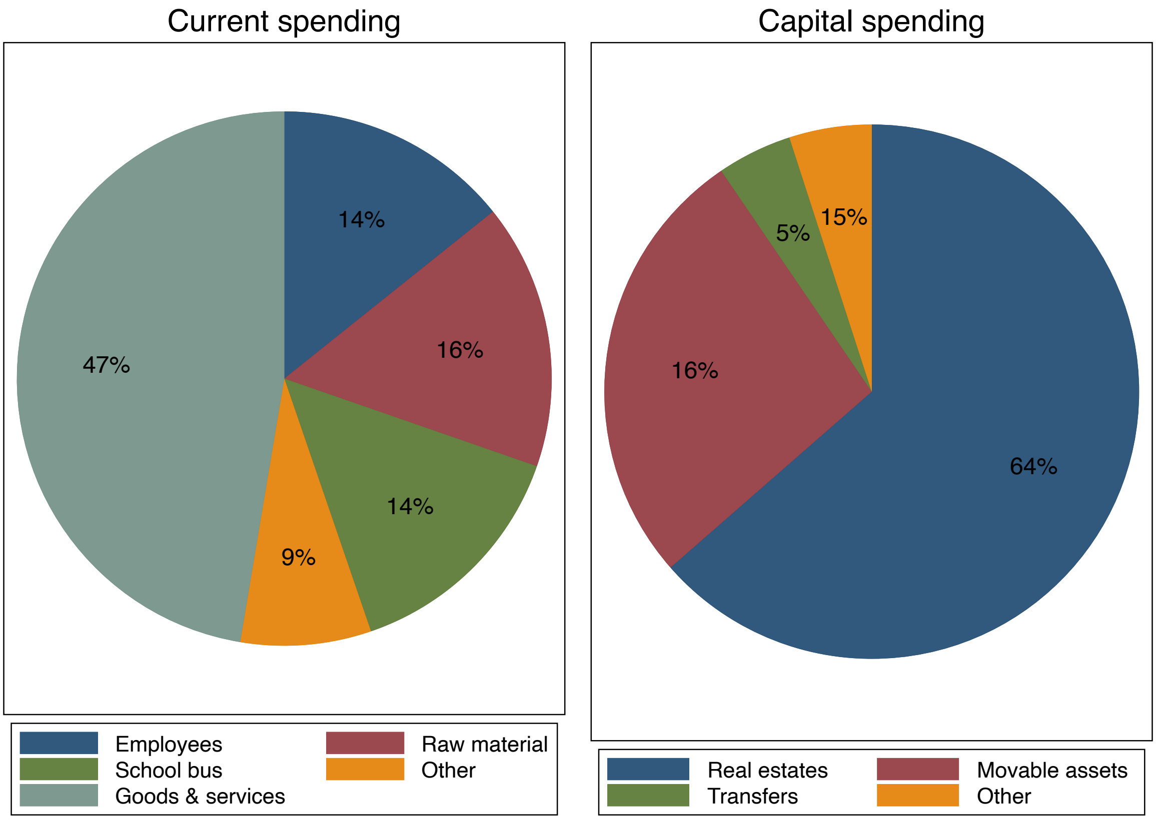
Variable	Description	Source
A. Test scores:		
Math (reading) test score	Correct answers in math (reading) test (%), standardized by cohort; mean = 0 and SD = 1	INVALSI
Math (reading) test score gains	Difference in math (reading) test score between 5th and 2nd grade	INVALSI
B. School spending and resources:		
Per-pupil school spending	Per-pupil municipal school spending (in 2015 euros), averaged over 2001-2015 period	Municipal balance sheets
Dining hall	A dining hall is available and functioning at school	MIUR
Gym	A gym is available and functioning at school	MIUR
Disabled facilities	Disabled facilities are available at school	MIUR
Heating system	Heating system is available and functioning at school	MIUR
Automatic heating system	An automatic heating system is available and functioning at school	MIUR
Sound-proofed classes	Classes are sound-proofed	MIUR
Interactive white boards	Dummy=1 if interactive white boards are available at school	INVALSI
Audiovisual tools	Dummy=1 if audiovisual tools are available at school	INVALSI
Computers	Dummy=1 if computer are available at school	INVALSI
Camera	Dummy =1 if cameras are available at school	INVALSI
C. Students' demographics and socio-economic characteristics:		
Female	Dummy=1 if female	INVALSI
Immigrant	Dummy=1 if child is Immigrant (I or II generation)	INVALSI
Year of birth	Child's year of birth	INVALSI
High level of paternal (maternal) education	Dummy=1 if paternal (maternal) education education is higher than high school	INVALSI
Paternal (maternal) employed	Dummy=1 if father (mother) is employed	INVALSI
Pre-school attendance	Dummy=1 if the child attended pre-school	INVALSI
ESCS index	Standardized socio-economic index; mean = 0 and SD =1	INVALSI
D. Teachers' characteristics:		
Female	Dummy=1 if female	INVALSI
Year of birth	Teacher's year of birth	INVALSI
Experience	Dummy=1 if she has been teaching in the same school for at least 5 years	INVALSI
Education	Dummy=1 if education is higher than lower secondary school	INVALSI
E. Schools' characteristics:		
Females	Share of female in class	INVALSI
Repeaters	Share of retained students in class	INVALSI
Immigrants	Share of immigrants in class	INVALSI
Low background	Share of parents with an educational level lower than lower secondary school	INVALSI
School size	Total number of students in school	INVALSI
F. Municipalities' characteristics:		
Population	Number of inhabitants	CENSUS
Altimetry zone	Index for altimetry zone (1/5)	ISTAT
Seaside	Dummy=1 if coastal municipality	ISTAT
Surface	Surface (expressed in square meters)	ISTAT
Taxable income	Total reported taxable income (in 2015 euros)	MEF
Housing price	Municipal average of biannual selling price of properties (expressed in euros per square meter)	*Osservatorio Mercato Immobiliare*

Table A2: Other municipal budget categories

	Municipal budget item:							
	Adm (1)	Cul (2)	Dev (3)	Env (4)	Soc (5)	Spo (6)	Tra (7)	Tur (8)
	A. *Outcome*: municipal budget category							
$1(Pop_m \geq T)$	-5.067	-6.095***	2.201***	-23.913***	-10.177**	-4.375***	-0.923	-3.753***
	(7.273)	(1.794)	(0.642)	(5.489)	(4.590)	(0.846)	(2.107)	(0.999)
	B. *Outcome*: Math test score gain							
Spending pc	0.026	0.022	-0.060	0.006	0.013	0.030	0.143	0.035
	(0.109)	(0.021)	(0.058)	(0.005)	(0.018)	(0.023)	(0.929)	(0.036)
	C. *Outcome*: Language test score gain							
Spending pc	0.026	0.021	-0.059	0.005	0.013	0.030	0.142	0.035
	(0.109)	(0.021)	(0.058)	(0.005)	(0.017)	(0.023)	(0.917)	(0.037)
# of students	26,906	26,906	26,906	26,906	26,906	26,906	26,906	26,906
# of municipalities	279	279	279	279	279	279	279	279

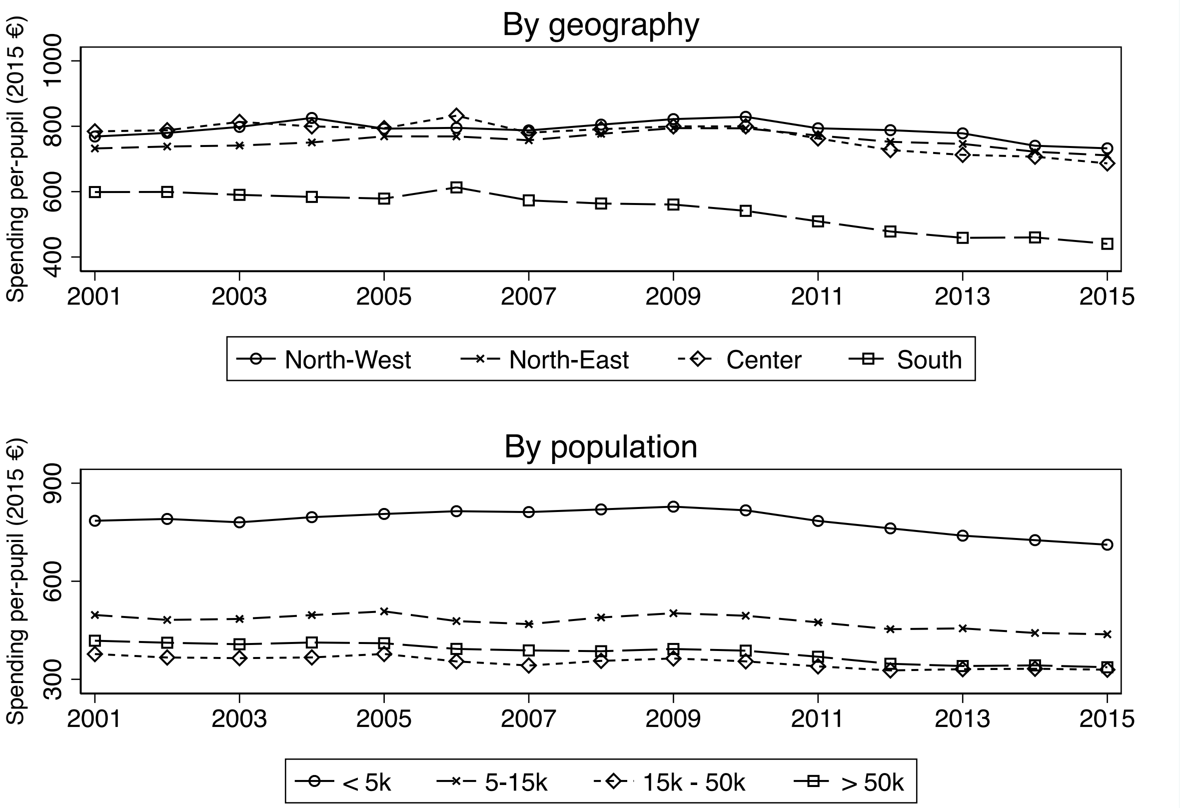
Note: This table displays coefficient estimates and standard errors obtained from equations (2) and (3), using the following municipal budget items as outcome variable in equation (2) and instrumented independent variable in equation (3): administration ("Adm"); cultural activities ("Cul"); development ("Dev"); waste management and environment protection ("Env"); social activities ("Soc"); sport ("Spo"): public transportation and roads ("Tra"). These budget items refer to the following 4-digit code in the municipal balance sheets (2014 format): administration (4190); cultural activities (4090); development (4290 and 4357); environment protection (4125); social activities (4080); sport (4150); public transportation and roads (4110 and 4180); tourism (4290). The first panel shows the "first stage" regression estimate of fiscal rules on per-capita municipal public spending item (2015 euros). The middle (bottom) panel displays the fuzzy RD effect of instrumented spending on standardized test score gains in math (language). Test scores are standardized by subject, year ad grade. Standard errors in parenthesis clustered at municipality-cohort level.

Figure A1: Spending share



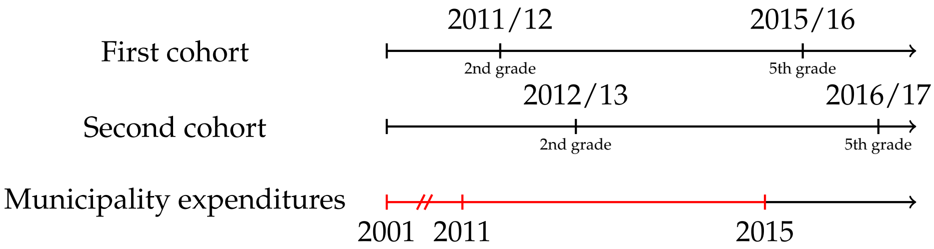
Note: This figure reports shares of municipal spending to finance schools. Left-hand side graph displays current spending; right-hand side graph illustrates capital spending. Data from municipal balance sheets averaged over the 2001-2015 period.

Figure A2: Trend in municipal spending per-pupil, by groups



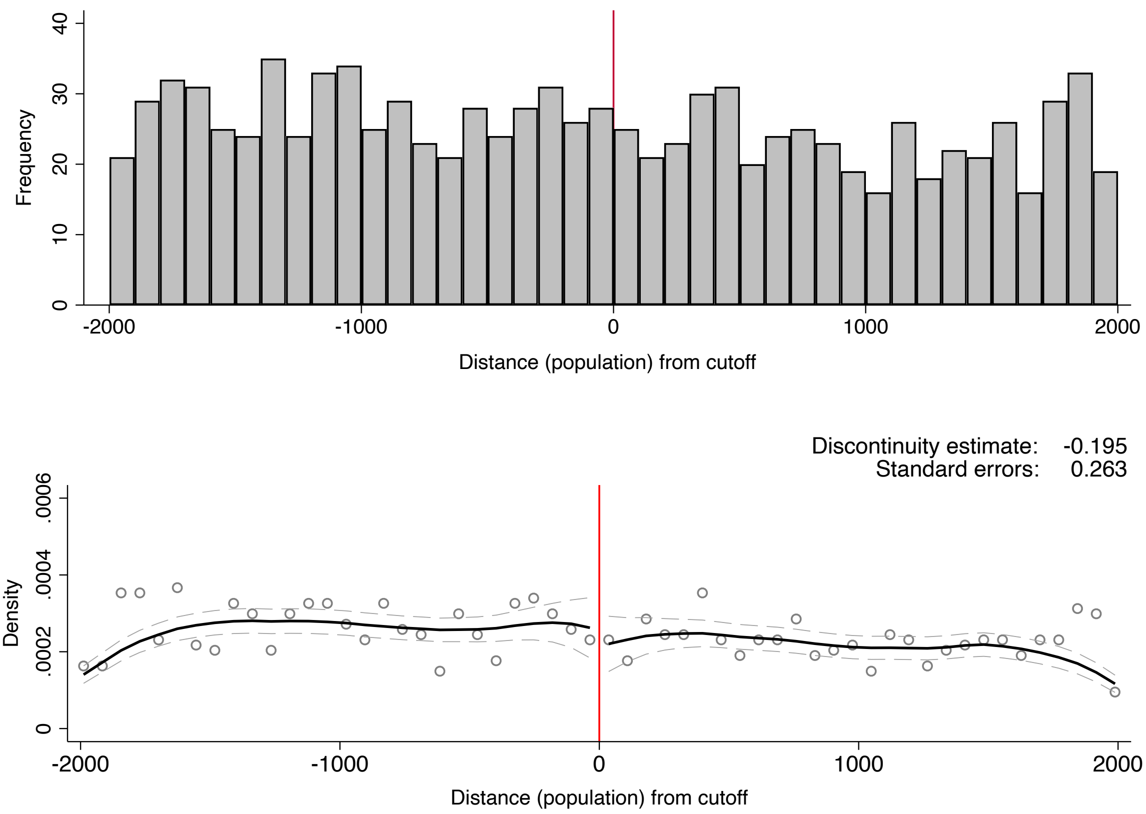
Note: The figure shows the trend in municipal school spending per-pupil (in 2015 euros) over the 2001-2015 period. Top panel shows separate trends for municipalities located in different geographical regions. Bottom panel displays the evolution of spending by municipality population size. Data from municipal balance sheets.

Figure A3: Timing of municipal spending and test scores' measurement



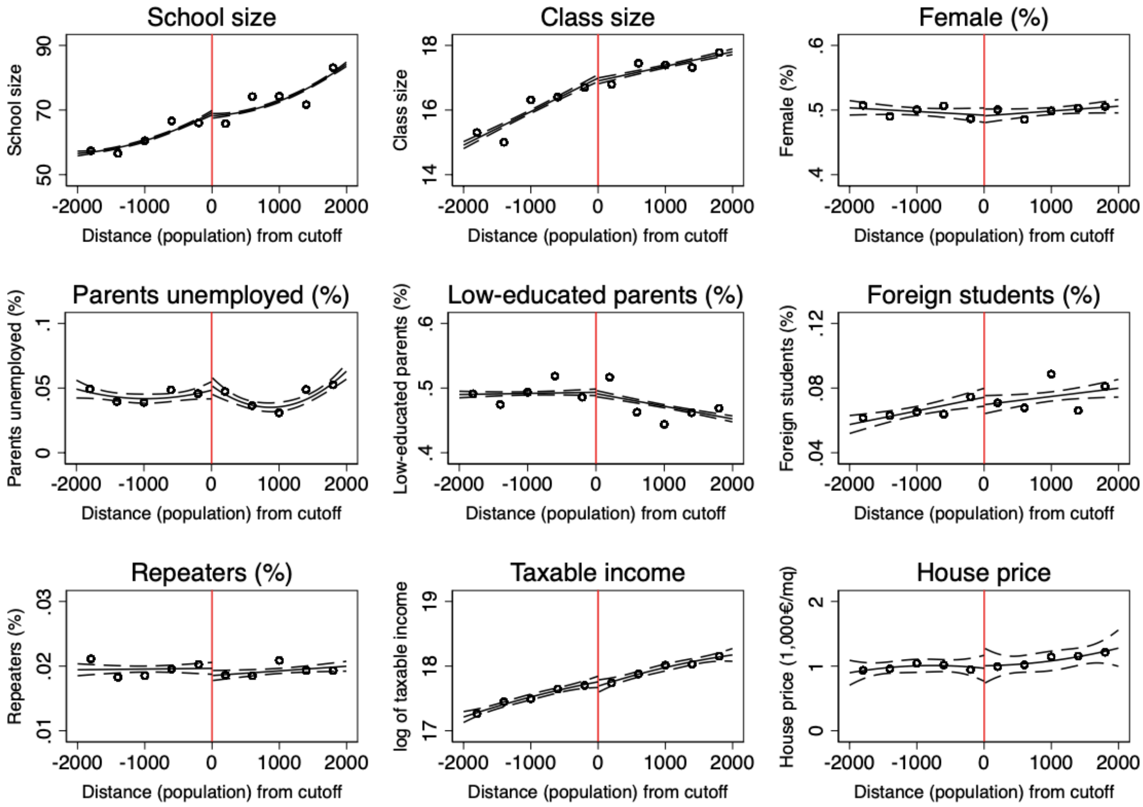
Note: The figure shows the timeline of the data used in the main analysis. Municipality expenditure per pupil is averaged over the 2001-2015 period. INVALSI standardized test scores are administered at the end of second and fifth grade for both first and second cohort. Teachers surveys were administered on the same date of standardized test scores.

Figure A4: Density of running variable



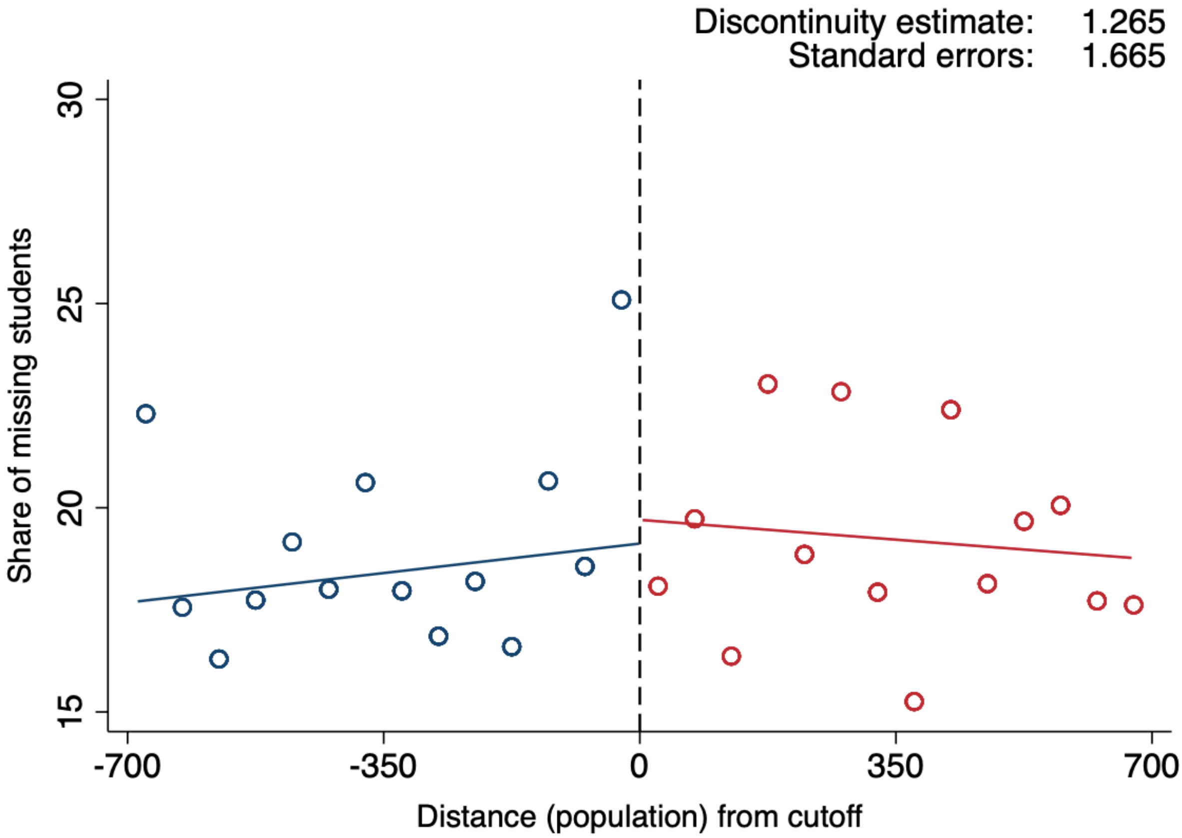
Note: The figure shows the distribution of the municipal population around the eligibility threshold for fiscal rules (red vertical line) in municipalities with population between 3,000 and 7,000 in 2011 Census. Circles represent the difference between the municipal population and the 5,000 threshold. 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 -.195 (.263).

Figure A5: Validity of RD design



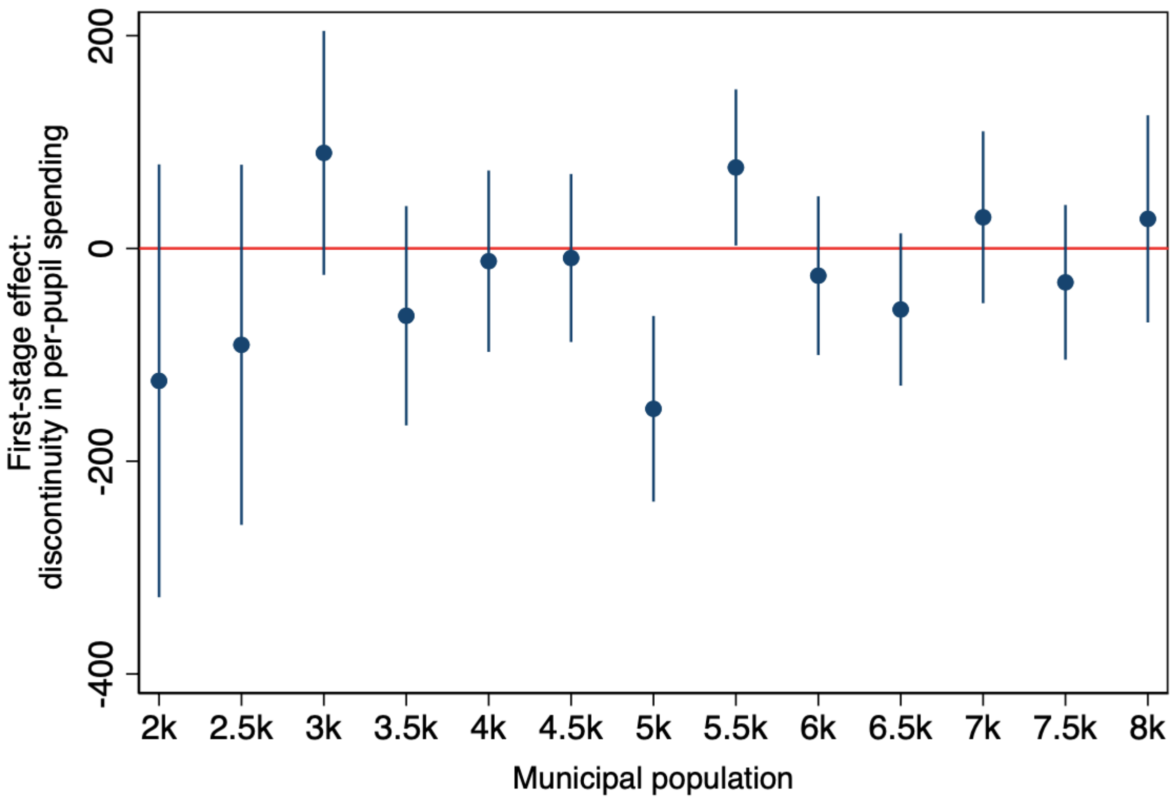
Note: This figure depicts fitted values and 95 percent confidence for the following variables: school size; class size; share of female students; share having both father and mother unemployed; share of parents without college degree; share of foreign students; share of repeater students; log of taxable income (2015 euros); housing selling price (thousand of 2015 euros / square meter). Scatter points are sample average over intervals of 400 population size bins.

Figure A6: Differential attrition



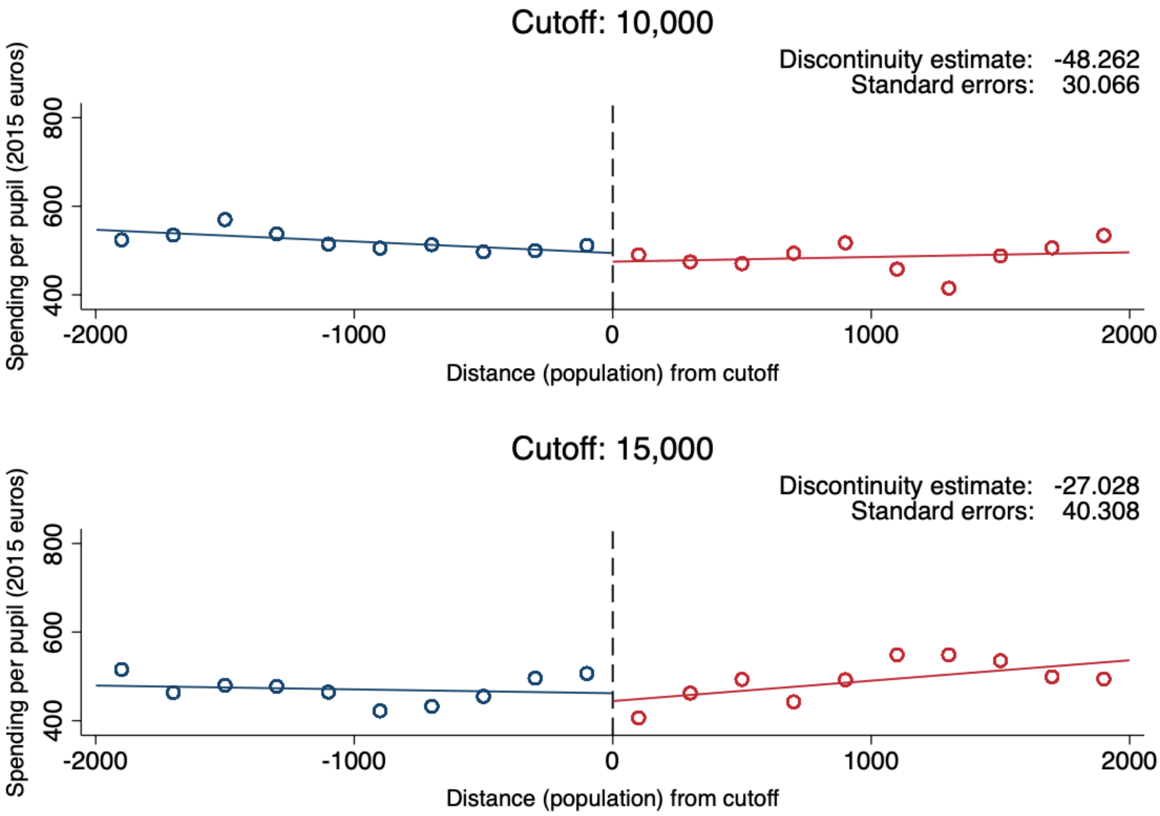
Note: The figure shows the relationship between per-pupil proceeds and eligibility for fiscal restraints. The vertical axis is per-pupil proceeds (in 2015 euros). The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014).

Figure A7: Fake cutoffs



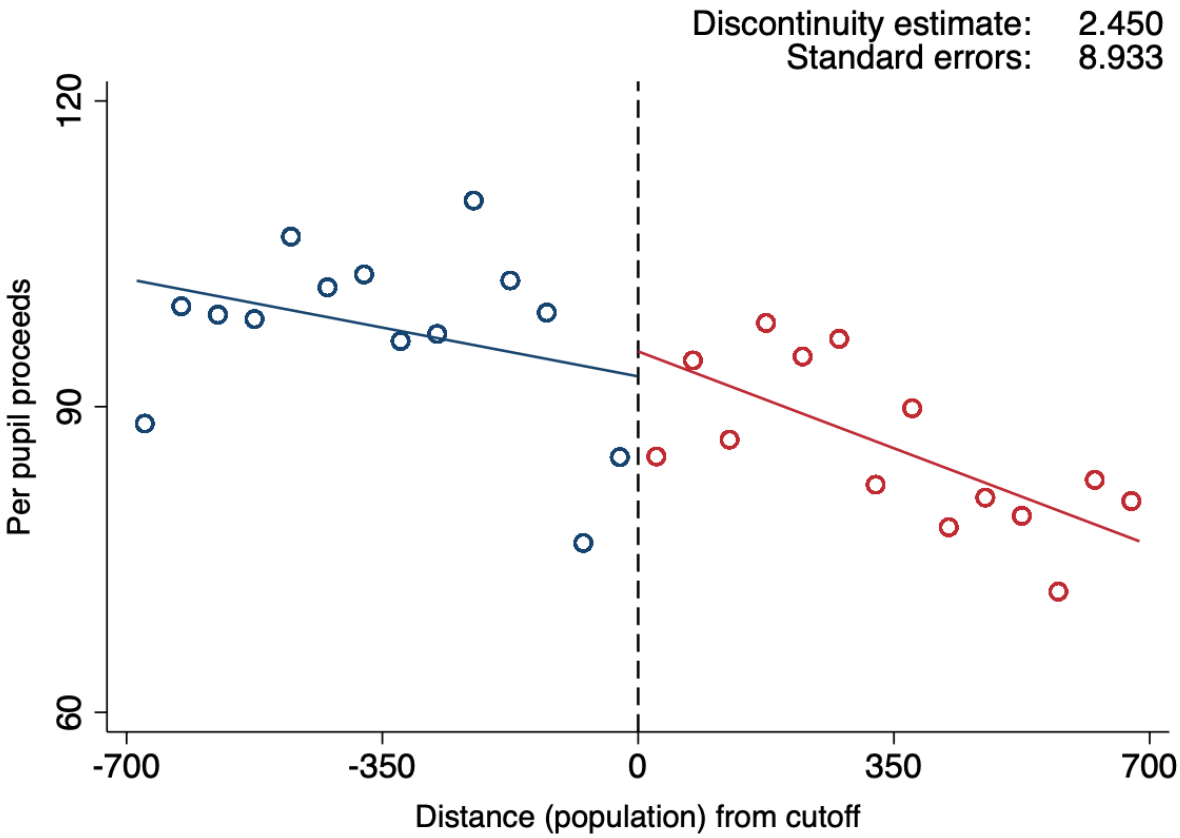
Note: The figure shows the "first-stage" coefficient and 95 percent confidence intervals simulating the effect of school spending on several population cutoffs. The vertical axis is municipal school spending per-pupil (in 2015 euros). The horizontal axis is the population cutoff at which we run equations (2) and (3).

Figure A8: Placebo test on 10,000 and 15,000 inhabitants cutoff



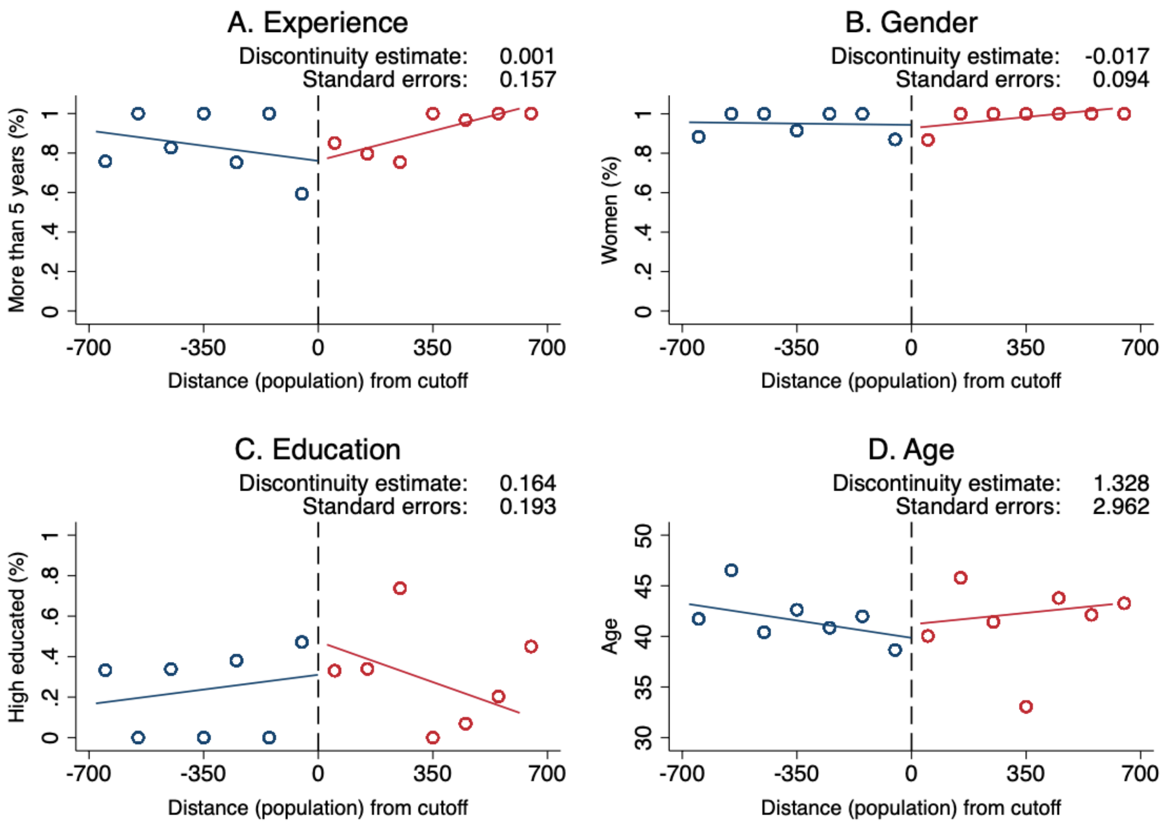
Note: The figure shows the "first-stage" relationship between school spending and eligibility for fiscal restraints. The vertical axis is municipal school spending per-pupil (in 2015 euros). The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins.

Figure A9: Eligibility for fiscal rules and parents' grants



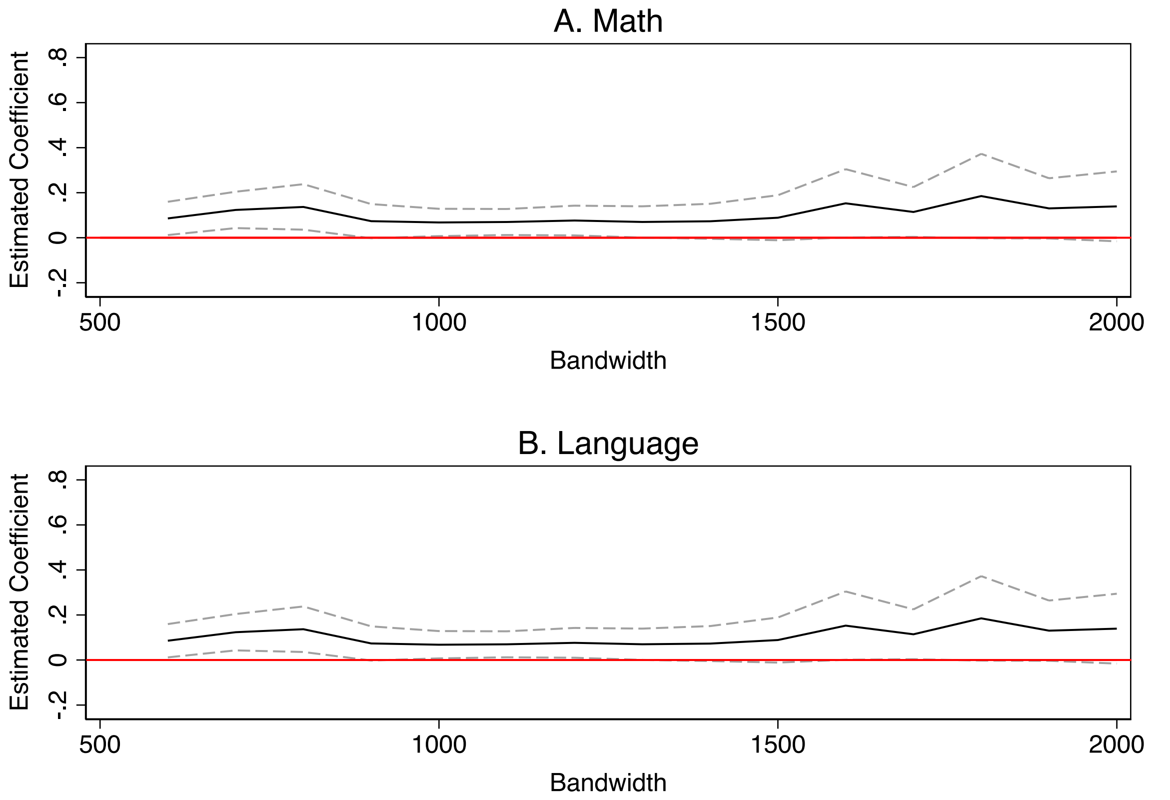
Note: The figure shows the relationship between per-pupil grants to schools and eligibility for fiscal restraints. The vertical axis is per-pupil grants received from private donors (in 2015 euros). The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 50 population size bins.

Figure A10: Eligibility for fiscal rules and teachers' characteristics



Note: The horizontal axis is the actual population size minus 5,000. Scatter points are sample average over intervals of 100 population size bins. Optimal bandwidth is computed following the algorithm developed by Calonico et al. (2014).

Figure A11: Local linear regression with varying bandwidth



Note: This figure reports fuzzy RD estimate and 90 percent confidence intervals from local linear regression with varying bandwidth. Test scores are standardized by subject, year and grade. Standard errors clustered at municipality-cohort level.