THE ECONOMIC JOURNAL

The Economic Journal, **134** (*April*), 1199–1227 https://doi.org/10.1093/ej/uead092
Advance Access Publication Date: 23 October 2023

AUSTERITY HARMED STUDENT ACHIEVEMENT*

Caterina Pavese and Enrico Rubolino

This paper shows that austerity spending cuts harmed student performance in standardised national tests. To identify this relationship, we use cross-municipality variation in the timing of eligibility for the Italian Domestic Stability Pact as an exogenous shifter of local public spending. We then compare test scores for students that were from the same municipality, but who were exposed to different levels of austerity cuts based on their birth year. Combining administrative data on public spending and test scores with an instrumental variable model, we show that the test score impact from austerity spending cuts is around 5.1% of a standard deviation in math and 4.6% in reading. These effects are more pronounced for children with limited resources at home. We provide suggestive evidence that school budget cuts account for most of the observed test score impact.

Education is often considered a pivotal instrument to curb inequality and promote social mobility. The demand for high-quality education, which can translate into higher costs per student, needs to contend with the public sector budget constraint. This tension has been particularly salient for many countries in the aftermath of the financial crisis, when the explosion of public debt and the adoption of austerity policies put hurdles on the ability of governments to finance public services (Fetzer, 2019). For instance, the European Commission, Directorate-General for Education, Youth, Sport and Culture (2016) argued that education in Mediterranean Europe was 'strongly affected by very low and decreasing public spending, due to strict fiscal consolidation'.

This paper studies the impact of austerity spending cuts on student achievement. The ultimate effect of austerity policies has sparked extensive debate by both policymakers and economists (see, for instance, VoxEU, 2012). The potential outcomes of austerity policies are not straightforward. On the one hand, spending cuts could have a limited effect on student outcomes if austerity encouraged policymakers to eliminate inefficiencies and wasteful spending in public services provision (Bandiera *et al.*, 2009). Conversely, suppose that the quality of public services is already poor, and that policymakers lack the capacity to cut expenditures efficiently. In that case, austerity may lead to reductions in essential programs and services contributing to child

* Corresponding author: Enrico Rubolino, Department of Business and Economics, University of Lausanne, Quartier de Chambronne, Internef 261, 1015 Lausanne, Switzerland. Email: enrico.rubolino@unil.ch

This paper was received on 20 May 2022 and accepted on 17 October 2023. The Editor was Marco Manacorda.

The authors were granted an exemption to publish their data because access to the data is restricted. However, the authors provided the Journal with temporary access to the data, which allowed the Journal to run their codes. The codes are available on the Journal repository. The data and codes were checked for their ability to reproduce the results presented in the paper. The replication package for this paper is available at the following address: https://zenodo.org/records/10013359.

We are very grateful to the editor, Marco Manacorda and three anonymous referees for their insightful comments and suggestions. We would like to thank Ainoa Aparicio Fenoll, Zareh Asatryan, Erich Battistin, Marius Brülhart, Christian Dustmann, Margherita Fort, Edoardo Grillo, Kirabo Jackson, Rafael Lalive, Salvatore Lattanzio, Elena Meschi, Sauro Mocetti, Giulia Romani, Alessia Russo, Camille Terrier and seminar participants at Ifo Institute, University of Essex, University of Padua, University of Turin, Statistics Norway, the International Workshop on Applied Economics of Education (IWAEE), SIEP, SIE, AIEL, EALE and at the INVALSI Workshop for helpful comments. We thank Pasquale Recano and Carmine La Vita from the Italian Ministry of Interior for providing municipal financial data and Patrizia Falzetti and Michele Cardone for providing INVALSI data. We are responsible for any error.

Rubolino gratefully acknowledges financial support from the Swiss National Science Foundation (grant number 192546).

development. Therefore, the impact of austerity on student achievement remains uncertain. Yet, to our knowledge, there is a dearth of empirical research exploring the relationship between austerity spending cuts and student outcomes.

We aim to fill this gap by focusing on the Italian Domestic Stability Pact (DSP): an austerity policy implemented in 1999 to achieve the public finance targets set by the European Union Stability and Growth Pact. This policy sets constraints on local government budgets through sub-national fiscal rules, aiming to prevent budget deficits and excessive public debt burdens. As a result, the DSP has significantly curtailed the ability of local governments to raise public spending and invest in public programs. The policy was gradually implemented across municipalities based on population size, offering useful variation in exposure to austerity across municipalities and over time.[1]

Several empirical studies, recently surveyed in Jackson and Mackevicius (2023), have sought to understand whether (and how) public spending affects student outcomes. Our focus on the impact of austerity spending cuts on the performance of Italian students offers several advantages. First, our design accounts for adjustments of declining local public spending on teacher characteristics, who are paid and allocated across schools by the national government. Furthermore, since local taxes in Italy can vary only within a specific range set by the national government, local politicians have limited scope for counteracting spending cuts through higher tax rates. These features of the Italian context are noteworthy, as some previous studies find that local governments (or schools) tend to offset spending changes through property tax adjustments or by changing teachers' salaries and hiring (see, e.g., Lafortune *et al.*, 2018 and Brunner *et al.*, 2020). These types of responses are de facto limited in our setup. Therefore, austerity spending cuts mainly operate through reductions in public services and facilities provided by municipalities, such as school building renovation and construction, a range of essential school goods and services (e.g., school meals, textbooks, teaching tools, laboratories, libraries), early childcare programs, public transport and other social and recreational activities that promote child development.

Another advantage of our study is that we can combine nationwide student data with granular information on public spending. We access rich balance sheet panel data on municipal spending, broken down into several budget chapters. We then merge this dataset with administrative data on standardised test scores for Italian primary school students, provided by the Institute for the Evaluation of the Educational System (INVALSI). Our final dataset covers children straddling the period of DSP inception, spanning cohorts born between 2002 and 2008.

To identify the causal effect of austerity spending cuts on student outcomes, we use the timing of DSP inception and the pre-existing budget composition as exogenous shifters of spending. Namely, we predict the austerity-induced spending cut that a municipality would experience based on years elapsed from DSP inception and its interaction with the ex ante share of rigid spending. The ex ante extent of budget rigidity is meant to capture imperfect compliance with the DSP (Grembi *et al.*, 2016), and the possibility that the scope for reallocating existing expenditures and cutting inefficiencies might be limited in municipalities with a more rigid budget (Corte dei Conti, 2012). We then compare test scores for students from the same municipality, but exposed to different levels of DSP-induced spending drops. Given that some children were not yet born at the time of DSP inception while others were already in their final stage of primary school, we

[1] Similar programs have been implemented in both the developed and developing world: according to the International Monetary Fund, 96 countries have imposed national or local fiscal rules (Lledò *et al.*, 2017). The DSP set of rules, ranging from a balanced budget rule to expenditure ceilings, has been changed over the years. As opposed to Grembi *et al.* (2016), we focus on the introduction of expenditure ceilings.

can examine whether cohorts less exposed to austerity policies had better test scores relative to more exposed cohorts, within a given municipality (and school).

We show that austerity exposure and its interaction with the ex ante share of rigid spending are highly predictive of reductions in per-pupil spending. We find that an additional year of austerity exposure reduces per-pupil spending by 632 euros, representing around 1% of the pre-existing municipal spending. Using an instrumental variable framework, we find that the impact of an additional year of austerity is 0.9% (1%) of an SD in the reading (math) test score per 1,000-euro per-pupil reduction in spending. These are relevant effects: a back-of-the-envelope calculation suggests that the overall austerity spending cut reduced test scores by 5.1% in math and 4.6% in reading. We conduct a suite of robustness checks to validate our findings, such as using alternative measures of austerity exposure, checking the sensitivity to different empirical specifications, alternative control variables and changes in the sample definition.

Our study provides new insights into the effects of austerity policies. At the same time, our results raise several questions, such as the following. Is austerity particularly harmful to children from a lower socio-economic background? What are the mechanisms through which spending cuts affect test scores? Can skilled school principals mitigate the adverse test score impacts of declining public spending?

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

To understand which budget cuts are mainly responsible for the negative test score impact, we propose two analyses. First, we conduct a conventional mediation analysis. We find that educational spending cuts represent the most relevant expenditure chapter, accounting for around half of the total test score impact. Second, we focus on students enrolled at private schools. We exploit the fact that the municipal government does not control private schools to conduct a 'placebo' exercise. If we detect any significant test score impact on students at private schools, our results would then suggest that test score impacts of austerity cuts do not operate through educational spending cuts. We find that austerity cuts do not impact the test scores of private school students. This result also holds when we control for several student-specific characteristics and school fixed effects, which account, at least in part, for the fact that students in private and public schools might differ in many aspects. This finding suggests that austerity spending cuts have minor effects on student achievement when the cut operates through non-school-related expenditures.

We then explore the detailed impact of austerity cuts on educational expenditure. We gather information on a wide array of services funded by municipalities, including the availability of several primary school facilities and teaching tools. We provide two results. First, we show that austerity significantly reduced funding to laboratories, libraries and gyms at school. Second, we provide suggestive evidence that spending cuts' marginal test score effects are mitigated in schools led by more competent school principals (proxied by their educational attainment). One interpretation of this result is that more competent school principals might be able to offset declining spending by favouring the conditions and climate in which teaching and learning take place (Bloom *et al.*, 2015; Di Liberto *et al.*, 2015).

To the best of our knowledge, we provide the first empirical evidence that austerity spending cuts negatively impact student outcomes. Our paper relates to the debate on the effect of austerity policies. While the agenda of austerity reforms is to make the welfare state leaner and more efficient, we show that austerity could also undermine governments' ability to pursue social and redistributive policies. Fetzer (2019) provided clear evidence that exposure to austerity welfare cuts raised support for the UK Independence Party and, in turn, to vote for Leave in the 2016 UK ('Brexit') referendum on European Union membership. Other recent studies focusing on the cost of austerity policies are Facchetti (2021) and Bray *et al.* (2022), who found significant effects on hate and violent crimes.

Our results also contribute to the literature studying the impact of public spending on student outcomes. The existing empirical literature has primarily focused on specific public budget items, typically school spending, or state-specific policies (see, e.g., Cellini *et al.*, 2010; Hyman, 2017; Lafortune *et al.*, 2018).[2] Although they provide notable contributions, they have weaker generalisability owing to their more localised focus. As emphasised above, our setting and data allow us to tackle many plausible identification concerns that arise when focusing on a specific input. The richness of our data allows us to provide suggestive evidence on what kind of budget item matters for student outcomes, encompassing both school and non-school-related expenditure chapters. Although there could still be other channels that we cannot fully measure, our data allow us to study the test score impact of several key inputs of child development that are influenced by austerity cuts.

The impacts of austerity cuts are likely to persist into adulthood. Both economic theory and empirical evidence have established a robust association between test scores and many important outcomes in life, such as earnings, college attendance, home ownership, retirement savings and health outcomes (see, e.g., Cunha *et al.*, 2010; Chetty *et al.*, 2011; Heckman *et al.*, 2013; Almond *et al.*, 2018). The relevance of austerity cuts on children's cognitive ability calls for a better understanding of whether their impact will persist later in life, with important implications for the persistence of inequality and social immobility.

The remainder of the paper proceeds as follows. Section 1 presents the institutional background and the data. We describe our research design in Section 2. Our main results and mechanisms are presented in Sections 3 and 4. Section 5 concludes.

1. Background and Data

1.1. *Italian Municipalities and the Domestic Stability Pact*

This paper studies the effect of austerity spending cuts in the context of the Italian municipalities, which are the lowest administrative division in Italy. Similar to other developed countries, Italy has a relatively large local public sector. Municipalities manage around 10% of total public expenditures and are responsible for providing many public goods and services to their residents. This includes primary and lower-secondary schools, public transportation, early childcare and town planning. The 7,904 Italian municipalities are small open economies: the median population size is 2,532, and around two-thirds of municipalities have less than 5,000 residents. The local

[2] Litschig and Morrison (2013) is an exception offering empirical evidence on the impact of local (overall) public spending. They found that municipalities in Brazil receiving extra financing from the central government benefited in terms of educational outcomes.

government is composed of a mayor and an executive committee. An elected municipal council approves the annual budget proposed by the mayor and the executive committee.

Since 1999, the DSP (*Patto di Stabilità Interno*) has regulated fiscal relations between the national and local governments. Introduced to achieve the public finance targets set by the European Union (1997 Stability and Growth Pact), the DSP aimed at holding Italian municipalities accountable through a set of sub-national fiscal rules. These rules place annual constraints on the budget balance and/or local government's expenditures. To enforce fiscal rules, the national government introduced sanction schemes, which encompass a cut in government transfers, a ban on municipal hires, a salary cut for mayors and other councillors, and restrictions on borrowing for investment (law 133/2008).[3] Compliant municipalities benefit from a lower interest rate on loans from the central government. We provide details on the Domestic Stability Pact in Online Appendix E.

Eligibility for the DSP is based on the municipal population. Following some changes implemented over the first years, the DSP covered only municipalities with more than 5,000 inhabitants until 2012. Starting in 2013, the exemption threshold was lowered for municipalities with more than 1,000 residents. The 2015 reform eventually extended the DSP to all municipalities. These reforms modified, not only the eligibility conditions, but also the set of local public finance requirements that municipalities need to satisfy (see Online Appendix Table A1 for a summary of the main features of the DSP). Since its inception and up to 2004, the DSP consisted of a balanced budget rule that merely limited the growth rate of the fiscal gap. Since most sources of revenue and expenditure (including capital spending) were excluded from the target, the DSP had a negligible impact on municipal spending during this period. The DSP began to severely constrain municipal spending only from 2005, when the national government introduced expenditure ceilings and included a broader set of municipal expenditures, including investments, as subject to the DSP rules. In fact, studies focusing on the period before 2005, such as Grembi *et al.* (2016), find a limited impact of the DSP on local public expenditures. For this reason, we model exposure to austerity policies based on whether the DSP includes rules constraining local public spending.

In theory, municipalities have two main strategies to meet the DSP requirements while retaining local public services: increasing revenue or reallocating existing expenditures. On the revenue side, however, municipalities' autonomy is limited: both the municipal surtax rate on personal income and the property tax can vary only within a specific range set by the national government (see Rubolino, 2023).[4] Other sources of revenue, such as transfers from the national or regional government, are solely determined by the law based on a municipality's population, density, surface and age composition (see *Decreto Legislativo* n. 504/1992).

The DSP can encourage local policymakers to remove inefficient expenditures. According to Bandiera *et al.* (2009), passive waste (defined as public spending that weakly, if any, increases the utility of decision-makers and citizens) accounts for a large share of municipal spending in Italy. However, budget rigidity can substantially limit the scope for reallocating existing expenditures and cutting inefficiencies. According to Corte dei Conti (2012) and Grembi *et al.* (2016), about two-thirds of municipal expenditures are classified as rigid. The extent of structural budget

[3] Except for the cut in government transfers, note that the sanctions mainly include penalties that do not directly affect the municipal budget. The rationale is that the government aims at blaming (and punishing) local politicians for non-compliance, rather than residents through expenditure cuts.

[4] Moreover, the national government introduced a local tax freeze for several years (see law 296/2006 and law 208/2015), hindering the municipality's capacity to finance spending through tax hikes.

rigidity severely curtails a municipality's financial autonomy. Because payroll expenses are the primary source of budget rigidity, firing frictions prevent municipalities from passing on austerity cuts to public sector employees.

Budget rigidity also hurdles municipalities' ability to comply with the DSP. As shown in Online Appendix Figure A1, we find a strong positive correlation between the pre-DSP share of rigid expenditure and the DSP non-compliance probability. This relationship implies that the share of rigid expenditure tends to dampen the austerity impact through imperfect compliance. The sanctions applied to non-compliant municipalities do not compensate for the failure to reduce spending. As discussed above, the sanctions mainly include penalties that do not directly affect the municipal budget (except for the cut in government transfers). Therefore, a higher share of rigid expenditure would lead to a *lower* spending cut following austerity, compared to the spending cut experienced by compliant municipalities.[5] In our empirical approach, we account for the fact that the pre-DSP share of budget rigidity tends to dampen the expenditure fall due to austerity.

1.2. *The Italian School System*

Compulsory education in Italy starts at the age of six and lasts for ten years. This education period is organised in two cycles: primary education, which lasts five years, and secondary education for successive years. In primary schools, the focus of our analysis, students are assigned to a class at the beginning of grade 1 and share the same peers and teachers until the end of primary school.

The national government is responsible for the general organisation of the education system (e.g., minimum education standards, school staff and quality assurance).[6] Class formation criteria are established by the national government, and each school needs to ensure that classes are equally distributed by ability, gender and socio-economic background. The enrolment process characteristics of the Italian primary school system substantially mitigate endogeneity concerns relative to students' selection across schools. The most relevant criteria to admit a student is the distance between the student's residence and the school. Therefore, students from other municipalities can be accepted only under the exceptional circumstance that a school has spare capacity (law 81/2009). The national government is also responsible for hiring and paying teachers, whose salary is set by a national collective agreement. Schools are therefore unable to select, pay or fire their teachers. The allocation of teachers and school principals occurs through a process mainly based on seniority (law 59/1998).

Since the late 1990s, the Italian education system has experienced an intense period of reform and radical changes, aiming to foster school autonomy and decentralise the supply of school resources. As a result, municipalities started to invest a large portion of their budgets to finance schools. In particular, municipalities are in charge of constructing and renovating buildings of pre-primary, primary and lower-secondary schools, and providing several essential goods and

[5] Moreover, the extent of budget rigidity might also dampen the austerity impact because payroll expenses, the primary source of 'rigid expenditure', were not included in the DSP requirements for the years 2006 and 2007 (see Online Appendix E for details). This exclusion implies that the 'stringency' of austerity depended on the share of payroll expenses. Notably, compliant municipalities do not receive government grant increases that might offset the DSP-induced spending cut. See Ministero degli Interni (2021b) for data on sanctions.

[6] The Italian schooling system can be regarded as a good representation of the OECD education system regarding the distribution of school responsibilities between the national, regional and local (including schools) governments. According to OECD (see OECD, 2018), around 30% of educational decisions in Italy are taken at local or school levels, which is fairly similar to the OECD average (35%).

services, such as school meals, transportation, textbooks, teaching tools and other educational materials needed to set up laboratories, libraries and gyms (see article 139 of law 112/1998). Municipalities are also charged for water, heating, internet, electricity and cleaning costs.

There is ample anecdotal evidence that the supply of municipal school resources and infrastructure has not kept pace with the increase in demand over the last decades in Italy (see, e.g., OECD, 2020). According to the Ministry of Education and Research (MIUR), school buildings are fairly old and obsolete: more than 60% of the school building stock was built over the 1960–80 period. The sub-standard quality of school buildings has been referred to as a 'national emergency' (see, e.g., Rei, 2012; Tripodi, 2017). According to a report presented by *Cittadinanzattiva* (see Cittadinanzattiva, 2022), 45 school buildings collapsed between September 2021 and August 2022.[7]

1.3. *How Does Austerity Affect Child Development?*

Since municipalities are responsible for financing the provision of several critical inputs of child development, spending cuts may affect student outcomes in various ways. Austerity cuts can prevent municipalities from funding public infrastructures. Public facilities, including schools, may then fall into disrepair, resulting in students attending schools in poor and unsafe conditions. The availability of a well-equipped schooling environment can influence various aspects of child development. Several papers, recently surveyed in Jackson and Mackevicius (2023), have shown that school investments improve student outcomes. For instance, the lack (or underfunding) of school goods and services, such as ICT materials (Comi *et al.*, 2017) or air conditioning (Park *et al.*, 2020), might significantly impair student achievement through lower attendance or chronic distractions. Budget constraints can also reduce funding to early childcare programs, which are directly managed and funded by municipalities.

The lack of a secure and engaging atmosphere beyond educational institutions could also hinder child development. Austerity can hamper the municipalities' ability to promote child-friendly environments (e.g., playgrounds) and fund recreational and cultural events that would enrich child development.[8] The availability of environments suitable for outdoor activities can also foster child development by enhancing social interactions and reducing antisocial behaviour.

Several other mechanisms, which we might not be fully able to capture in our data, could affect student outcomes through austerity spending cuts. For instance, as austerity cuts might lead to persistent public spending drops, they can depress incomes through a lower level of economic activity and employment opportunities. Lower parental incomes can then restrict families' ability to provide essential educational resources, such as books, computers and extracurricular activities. These resources, which play a crucial role in enhancing learning and school performance (see, e.g., Holden, 2016), might then become out of reach for financially disadvantaged students.

[7] The Italian government has corroborated these figures. In a 2013 report (see Camera dei Deputati, 2013), the Italian government contended that more than 15,000 school buildings need urgent maintenance and are unsafe. This corresponds to around 37.5% of the total stock of school buildings. Moreover, the government declared that almost one-quarter of school buildings should be demolished due to very poor conditions.

[8] Dadvand *et al.* (2015) showed that exposure to green spaces positively impacts child outcomes. The absence of public spaces for performing physical activities and exposure to noise and/or pollution are also channels that can significantly affect child development (see, e.g., Klatte *et al.*, 2013). Child outcomes can also benefit from visits to museums and exhibitions that encourage children's informal learning (see, e.g., Tan *et al.*, 2021).

1.4. *Data*

1.4.1. *Student and school outcomes*

We use administrative records on student test scores from the *Istituto nazionale per la valutazione del sistema educativo di istruzione e di formazione* (INVALSI): a standardised achievement test administered to the entire population of Italian students (INVALSI, 2021). We collect data on math and reading test scores at the end of primary school (fifth grade) over the 2010/2011–2016/2017 school years. To ensure comparability, we normalise test scores by school year and subject. We also adjust test scores by a cheating factor provided by INVALSI to account for potential cheating behaviours.[9] These data provide a comparable and objective measure of performance across cohorts and schools. INVALSI also provides standard demographic and socio-economic information, such as gender, birth date, preschool attendance, immigrant status and parental background (i.e., educational attainment and working status).

Moreover, from INVALSI, we retrieve student-level survey data on home resources, such as books and a Wi-Fi connection.[10] We complement these data with survey information provided by school principals, selected by INVALSI to be a representative sample (we validate the randomness of this sample in Online Appendix Table A4). From the survey, we collect school-level information on the availability (and proper functioning if available) of several school facilities and resources, such as laboratories, gyms, libraries, multimedia devices and teaching tools.

Our dataset covers children straddling the period of DSP inception, including cohorts born between 2002 and 2008. We cover all students attending a public school for whom we can retrieve information on test scores, municipality of school attendance and demographic characteristics.[11] We restrict the sample to students attending a school in municipalities with a population under 30,000 residents, where just a single primary school is present in most cases (in Online Appendix Figure A6, we show that our estimates are not sensitive to this sample selection choice).[12] We also omit first-generation immigrants (since the arrival date is poorly measured) and students attending schools located in regions with special autonomy, for whom the DSP had different requirements and rules. Our final dataset covers 962,897 students, enrolled in 2,684 primary schools that are located in 2,276 municipalities. Online Appendix Table A2 displays summary statistics of the variables used in the empirical analysis.

1.4.2. *Public spending measurement and municipality outcomes*

We collect data on public expenditures from the balance sheets of Italian municipalities, which are annual reports provided by the Italian Ministry of Interior (Ministero degli Interni, 2021a). The accounting models are homogeneous across municipalities and over time, adopting a functional

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

[10] In the student-level survey, the sample size shrinks for two reasons. First, we lack information on the 2002 birth cohort (not surveyed by INVALSI). Second, the response rate is large (more than 85%), but not perfect.

[11] One limitation of our data is that we can observe the municipality where a school is located, but not the student's residence. This implies that we could have a measurement error in assigning public spending across students. Whether this measurement error leads to attenuation bias is not a priori obvious since it depends on whether it is correlated with spending changes across municipalities. This issue, however, should be limited in practice: as described in Section 1.2, the enrolment process characteristics of the Italian primary school system substantially mitigate endogeneity concerns relative to students' selection across schools. We provide supportive evidence in Section 3.4.

[12] This restriction aims at reducing errors that might arise when assigning school spending across students. Since school spending represents a substantial portion of total municipal spending and we do not observe whether municipalities allocate spending differently across schools, the size of this measurement error is correlated with the share of municipalities with multiple schools.

classification structure consistent with the United Nations' Classifications of the Functions of Government (COFOG). We have accessed municipality-level data since 2002 on both current and capital expenditures, divided into several budget categories.[13] Using the consumer price index from the ISTAT (2021), we adjust all the nominal amounts to 2020 euros.

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

A second challenge relates to the time horizon for measuring public spending. A naïve approach for calculating the returns to spending would be to assume that spending effects are immediate and not cumulative, such that the spending effects in a given year map to the test score effects in that same year. However, achievements are likely to be the product of years of cognitive and behavioural development: early interventions (including school investments) could also impact many years later. Test score effects might thus take time to materialise fully.

With these concerns in mind, we compute a cohort-specific measure of public spending by averaging annual spending from birth year up to primary school final year (fifth grade), when we observe test scores. Linking our multi-year measure of spending with test scores allows us to account for cumulative learning and for the fact that investment processes require time to take effect. To account for any timing difference between when capital spending occurs and when it actually materialises, we use the annualised accounting value of the one-time increase in capital spending. Following Jackson and Mackevicius (2023), we assume annual depreciation of 7% for capital spending. Finally, we scale spending by the total (cohort-specific average) 0–10 population of each municipality, using ISTAT data (ISTAT, 2021).[14] On average, per-pupil municipal spending is 67,253 euros, which is composed of 47,933 euros of current expenditures and 19,320 euros of capital expenditures (see Online Appendix Table A3 for summary statistics). Figure 1 offers a graphical representation of per-pupil spending (panel (a)), showing wide differences across municipalities.

At the student level, we measure *exposure* to austerity as the difference between the year when a child attended fifth grade (when we observe test scores) and the DSP inception year (depicted in Figure 1(b)). Our exposure variable ranges from 0 (for those who completed primary school before DSP introduction) up to 13. Given that, as discussed in Section 1.1, the incidence of the spending cut is likely to vary across municipalities depending on the extent of budget rigidity, we follow Corte dei Conti (2012) and Grembi *et al.* (2016) to compute an estimate of *budget rigidity intensity* as the share of payroll expenses and debt service before DSP inception (regarded mostly as administrative expenditures in the municipal balance sheet, and averaged over the whole pre-DSP period available in our data). We find that, on average, around 56.8% of

[13] Municipal balance sheet data are also available over the 1998–2001 period, but they adopted a different methodology to classify public expenditures. To classify expenditures, we follow the three-digit classification adopted in the balance sheet (see Online Appendix B for details).

[14] We use per-pupil spending following the approach of Jackson *et al.* (2016). In Section 3.4, we show that our estimates are substantially similar when using spending per capita.

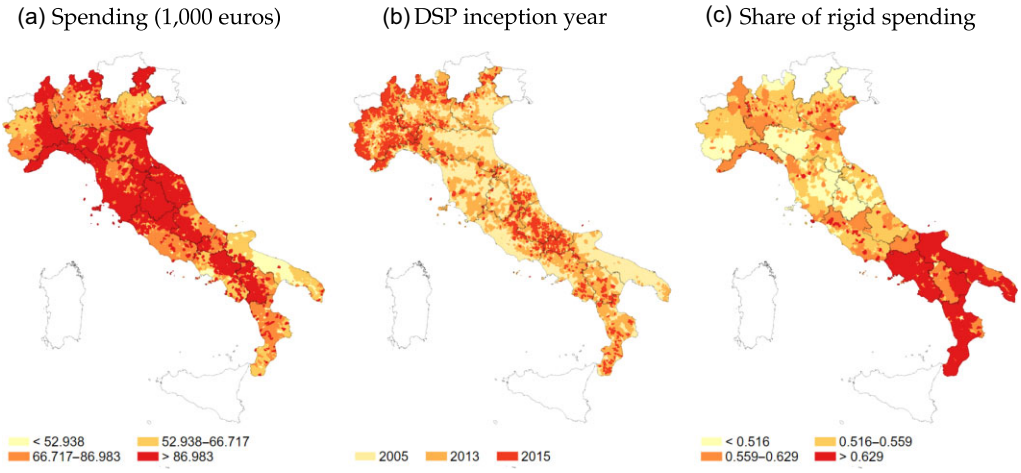

Fig. 1. *A Map of Public Spending and Exposure to Austerity Policies.*
Notes: Panel (a) depicts municipal spending per pupil (in thousands of 2020 euros). Panel (b) shows the inception year of the DSP. Panel (c) presents the pre-DSP share of municipal spending that was rigid. Break points in panels (a) and (c) are quartile intervals in municipal spending per pupil. The black line refers to regional boundaries. White areas refer to regions with special autonomy that the DSP does not cover.

the spending is rigid, with higher values in municipalities located in Southern Italy. Figure 1(c) presents the dispersion in the budget rigidity index, ranging from 23.4% to 88.7% of spending. Online Appendix Figure A2 plots the distribution of the budget rigidity index.

To account for the confounding effect of other policies overlapping with our period of interest, we also collect several time-varying municipality-level variables. From the Ministry of Interior, we retrieve data on the socio-demographic characteristics (age, gender and educational attainment) of mayors and other town council components. We also compute taxable income per capita using data from the Ministry of Economy and Finance. Finally, we calculate house selling prices using data from the Italian Internal Revenue Agency. Summary statistics are presented in Online Appendix Table A3.

2. Empirical Strategy

Our empirical work aims to understand whether public spending cuts harm student achievement. The main empirical obstacle is that spending is not randomly allocated across municipalities. A naïve OLS regression of student outcomes on municipal spending could result in a biased estimate if other variables concurrently affect both test scores and public spending. For instance, an economic downturn could negatively impact both public spending and test scores through mechanisms unrelated to expenditure, such as parental income. This would result in a spurious *positive* correlation between spending and test scores. By contrast, an increase in low-income students could lead to additional government funding while simultaneously depressing student outcomes. This would lead to a spurious *negative* relationship between spending and test scores. To account for these issues, we leverage the differential variation in spending cuts across municipalities and cohorts that can be attributed to the inception of the DSP.

2.1. *Two-Stage Least-Squares Model*

The gradual implementation of the DSP gives rise to two sources of identifying variation. First, we can leverage cross-municipality variation in austerity exposure for a given birth cohort. Depending on the number of years elapsed from DSP inception, we can exploit variation within birth cohorts between 'earlier' versus 'later' DSP eligible municipalities. The fact that DSP eligibility does not depend on local circumstances, but followed agreements between the European Union and Italy (with specific eligibility criteria based on population size) substantially alleviates endogeneity concerns. A second source of variation emerges across birth cohorts in a given municipality. Based on the years that a birth cohort has spent under the DSP, we can take advantage of variation within a municipality across 'more exposed' versus 'less exposed' cohorts depending on their birth year.

We implement a two-stage least-squares (2SLS) model that builds on these sources of variation to identify the test score effect of austerity spending cuts. This approach allows us to account for both different treatment effects across cohorts within municipalities and different treatment intensities across municipalities for a given cohort. Because DSP eligibility is likely to affect municipalities differently based on the pre-existing composition of expenditures, we use the ex ante share of rigid spending as a predictor of austerity spending cuts. As a shorthand, we denote the austerity-induced change in spending based on the pre-existing extent of budget rigidity as *dosage*.[15] We then predict within-municipality cross-cohort spending changes using austerity exposure and its interaction with our measure of dosage.

Our 2SLS model compares the difference in test scores between birth cohorts from the same municipality exposed to different amounts of time (variation in exposure) across municipalities with different austerity-induced changes in per-pupil spending based on the pre-existing extent of budget rigidity (variation in dosage).[16] Specifically, for each child i in municipality m belonging to birth cohort c, we run systems of equations of the following form:

$$S_{i,m,c} = \alpha_1 \times Exp_{m,c} + \alpha_2 \times (Exp_{m,c} \times Dosage_m) + \eta_m + \theta_c + \rho \times X_{i,m,c} + v_{i,m,c} \qquad (1)$$

$$y_{i,m,c} = \beta \times S_{i,m,c} + \gamma_m + \delta_c + \pi \times X_{i,m,c} + u_{i,m,c}.$$

Our measure of student performance is the standardised fifth-grade test score in math and reading, $y_{i,m,c}$. The treatment variable of interest, $S_{i,m,c}$, is per-pupil public spending, as described in Section 1.4.2. We present estimates using both the natural log of spending, which allows for non-linearity in the relationship between student performance and spending, and the level of spending.

Our measure of exposure, $Exp_{m,c}$, is the number of years that a birth cohort c in municipality m has spent under austerity. To account for variation in dosage conditional on exposure, we interact exposure with $Dosage_m$, which measures the pre-existing extent of budget rigidity. For the sake of simplicity, we use a discrete measure of dosage: it is equal to 1 for municipalities whose budget rigidity index is in the bottom quintile of the budget rigidity index distribution; 0

[15] In principle, we could predict public spending changes solely by austerity exposure. However, this would violate the monotonicity condition for a valid instrument: as discussed above, DSP eligibility affects municipalities differently based on their pre-existing share of rigid expenditure. Furthermore, conditioning austerity exposure on the extent of budget rigidity rules out significant differences across municipalities in the compliance rate.

[16] Jackson *et al.* (2016) adopted a similar empirical approach to study the effect of the US School Finance Reforms. Our methodology differs from Grembi *et al.* (2016) because it allows us to predict austerity spending changes within a given municipality (and school) by exploiting multiple policy changes, rather than the 1999 reform solely. This is relevant because the enforcement and rules of the DSP have changed over the years.

otherwise (in the robustness section below, we show that our estimates are not sensitive to this choice). The inclusion of municipality fixed effects, η_m and γ_m, allows us to rely on variation across birth cohorts within municipalities. Cohort fixed effects, θ_c and δ_c, account for general underlying differences across birth cohorts, irrespective of exposure. Note also that the impact of our instruments on the outcome variable is controlled for by the fixed effect structure (e.g., the fact that the share of rigid expenditure is systematically higher in the South is filtered out by the municipality fixed effects). Because there are multiple schools in large size municipalities, we also add school fixed effects to account for any time-invariant unobserved heterogeneity across schools within municipalities, such as (permanent) differences in school facilities, and teachers or school principal quality.[17]

To ensure that we isolate changes due to austerity, we include several controls in $X_{i,m,c}$. Specifically, we account for gender, nationality, quarter of birth, father's and mother's education and a dummy for grade repeaters. We also control for several time-varying municipality-specific characteristics, including the tax base, house prices and socio-demographic characteristics (age, gender and education) of the mayor and other town council members.[18] Furthermore, given the staggered timing of elections across municipalities, we add election year-cohort fixed effects to account for the fact that electoral incentives for pursuing policies that aim to capture voters, such as larger public spending, are stronger when elections approach.[19] Finally, $v_{i,m,c}$ and $u_{i,m,c}$ are random error terms. Because the effect of austerity is likely to be correlated within a municipality, we account for any dependence between observations within a municipality by clustering all regression results at the municipality level.

2.2. *Identifying Assumptions*

The 2SLS estimator, β, calculates the local average treatment effect of spending on test scores. A positive β estimate would suggest that students who experienced larger austerity spending cuts tend to perform relatively worse in national tests. The β estimate yields the causal effect of austerity spending cuts on test scores under three main assumptions. First, austerity exposure and its interaction with dosage significantly predict public spending cuts. We validate the relevance condition of our instruments by examining the 'first-stage' relationship presented in (1).

Second, the interpretation of our β estimate as a weighted average of complier treatment effects is based on the monotonicity assumption: each municipality's probability of reducing public spending increases with austerity exposure *and* dosage. In the presence of multiple instruments, the monotonicity condition must be satisfied separately for each instrument. This assumption is critical for ensuring that our instrumental variable (IV) strategy aggregates treatment effects across complier municipalities using the weighting strategy proposed by Imbens and Angrist (1994). Recent IV works have shown that linear IV estimation still yields a convex combination of treatment effects under a *partial* (weaker) version of the monotonicity condition. Mogstad *et al.* (2021) showed that multiple instruments are consistent with the weighting strategy proposed by

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

[18] See Agenzia delle Entrate (2021), Ministero degli Interni (2021c) and Ministero Economia e Finanza (2021) for data on the municipal tax base, house prices and socio-demographic characteristics of local politicians.

[19] Election year-cohort fixed effects can be included in the model because local elections usually occur every five years in Italy. The inclusion of these fixed effects implies that we exploit variation across cohorts within each different local government that has been elected within a given municipality.

Imbens and Angrist (1994) if the monotonicity condition is valid for each instrument separately (keeping the other instrument fixed). In our context, a sufficient condition for partial monotonicity is that all municipalities are at least as likely to cut spending if they are more exposed to austerity policies or face lower budget rigidity. Partial monotonicity allows some municipalities to respond more to exposure than to the extent of budget rigidity, and others to respond more to budget rigidity than to exposure.

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

Finally, the exclusion restriction requires that austerity exposure and its interaction with dosage affect test scores only through spending cuts. The plausibility of this assumption is substantiated by several placebo and robustness checks that we present below. In particular, we provide evidence of no significant relationship between austerity spending cuts and several test score predictors.

3. The Impact of Austerity Spending Cuts on Test Scores

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

3.1. *First-Stage and Reduced-Form Estimates*

We start by presenting the first-stage estimates, obtained by regressing (1). Panel A of Table 1 reports the α_1 and α_2 coefficient estimates relative to spending per pupil (in 1,000 euros), while panel B shows estimates relative to the logged measure of spending. In each panel, the first row presents the α_1 estimates: the effect of austerity exposure for municipalities with lower predicted dosage (i.e., those whose budget rigidity index is above the bottom quintile of the distribution). The second row shows the α_2 estimates: the coefficient estimates from the interaction between austerity exposure and higher predicted dosage. We also report SEs clustered at the municipality level in parentheses.

We first present estimates obtained from a basic model with municipality and cohort fixed effects (column (1)). Our estimates show that an additional year of austerity exposure led to nearly a 1% reduction, on average, in spending in municipalities with lower predicted dosage. This implies that a cohort subject to an additional year of austerity exposure would experience a reduction in per-pupil spending by around 418 euros. In line with the notion that budget rigidity predicts spending cuts, we find that an extra year of austerity exposure has significantly larger negative effects on municipalities with higher predicted dosage. On average, an additional year of austerity exposure reduces expenditures by a further 1.5% in municipalities with higher predicted dosage. This maps into an additional reduction in per-pupil spending of 1,488 euros. Weighting

Table 1. *First-Stage and Reduced-Form Estimates.*

	(1)	(2)	(3)	(4)	(5)
Panel A. Outcome: spending (1,000 euros)					
$Exp_{m,c}$	−0.418***	−0.400**	−0.402**	−0.402**	−0.348**
	(0.160)	(0.159)	(0.159)	(0.159)	(0.153)
$Exp_{m,c} \times Dosage_m$	−1.488***	−1.519***	−1.399***	−1.399***	−1.283***
	(0.333)	(0.338)	(0.342)	(0.342)	(0.336)
F-statistics	13.254	13.100	11.535	11.529	9.905
Panel B. Outcome: log of spending					
$Exp_{m,c}$	−0.010***	−0.010***	−0.010***	−0.010***	−0.009***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
$Exp_{m,c} \times Dosage_m$	−0.015***	−0.015***	−0.013***	−0.013***	−0.012***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
F-statistics	22.453	22.337	19.040	19.034	15.853
Panel C. Outcome: standardised V grade math test score					
$Exp_{m,c}$	−0.011**	−0.011**	−0.013**	−0.012**	−0.011**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Exp_{m,c} \times Dosage_m$	−0.019***	−0.017***	−0.014**	−0.013**	−0.010*
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
F-statistics	8.486	7.449	6.289	5.595	4.062
Panel D. Outcome: standardised V grade reading test score					
$Exp_{m,c}$	−0.010**	−0.010**	−0.011**	−0.011**	−0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
$Exp_{m,c} \times Dosage_m$	−0.016***	−0.016***	−0.012**	−0.011**	−0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
F-statistics	7.587	7.295	5.586	4.951	3.774
# of students	962,898	962,897	962,897	962,897	962,897
# of schools	2,684	2,684	2,684	2,684	2,684
# of municipalities	2,276	2,276	2,276	2,276	2,276
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes
School FEs	No	Yes	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes	Yes
Individual controls	No	No	No	Yes	Yes
Other controls	No	No	No	No	Yes

Notes: This table reports the 'first-stage' (panels A and B) and 'reduced-form' (panels C and D) estimates. We report the effect of austerity exposure ($Exp_{m,c}$) and its interaction with the ex ante budget rigidity index ($Exp_{m,c} \times Dosage_m$) on per-pupil spending (in levels and logs), and standardised fifth-grade test scores in math and reading. Individual-level controls include gender, nationality, quarter of birth, father's education, mother's education and a dummy for grade repeaters. Municipality-level controls include the age, gender and education of the mayor and average values for the other town council members, and election year-cohort fixed effects. Other controls include the municipal average tax base and house price index. Each panel also reports *F*-statistics to test for weak instruments. SEs clustered at the municipality level are reported in parentheses. Significance level: *** 1 percent, ** 5 percent, * 10 percent.

these estimates by the average student population in these two groups of municipalities, our results suggest that the implied per-pupil cost of an additional year of austerity exposure is nearly 632 euros of spending.[20]

In columns (2)–(5), we add school fixed effects (column (2)), municipality-level controls (column (3)), student-level controls (column (4)) and the average tax base and house prices (column (5)). The inclusion of these controls does not significantly affect the coefficient estimates.

[20] This estimate is computed as the weighted average of the implied effect for students with low predicted dosage (experiencing a spending drop of 418 euros and representing around 80% of the population) and the estimated impact for students with high predicted dosage (experiencing a drop of 1,488 euros and representing the remaining 20% of the population).

These results imply a strong first-stage relationship, with first-stage F-statistics that are fairly above the conventional thresholds for weak instruments.

Based on estimates from our preferred model (column (5)) and the average austerity exposure in our sample (8.485 yr), the implied spending impact of austerity is as follows. A child studying in a municipality with a lower predicted dosage would experience $8.485 \times 0.348 = 2{,}953$ per-pupil euros of lower spending, compared to a child located in the same municipality who has never been exposed to austerity. The effects are much stronger for children studying in municipalities with higher predicted dosage, where the spending cut would be more than three times larger ($8.485 \times (1.283 + 0.348) = 13{,}839$ euros of lower per-pupil spending). Weighting these estimates by the average student population in these two groups of municipalities, this result suggests that the implied per-pupil cost of austerity is nearly 5,130 euros of spending.

In Online Appendix C.1, we offer a more thorough evaluation of the first-stage relationship between austerity and public spending by examining how different spending categories are reduced in response to austerity. We find that austerity spending cuts are allocated differently than the average spending. On average, we find that municipalities cut disproportionally less on current administrative expenditures, and substantially more on school investments and current spending on public services and cultural activities. For instance, we find that municipalities allocated almost 36% of the total cut for school capital expenditures, despite municipalities allocating around 19% of total capital spending to this budget item.

Before presenting our 2SLS estimates, it is helpful to first understand the direct effect of austerity exposure on test scores (the 'reduced-form' effect). Panels C and D show the α_1 and α_2 coefficient estimates obtained by regressing (1) on math and reading test scores. The negative coefficients suggest that austerity harmed students' performance on national tests. On average, we find that an additional year of austerity exposure reduces test scores by around 1% of an SD in the sample of municipalities with lower predicted dosage. The table also shows that the test score impact from austerity exposure is consistently larger for municipalities with higher predicted dosage.

To offer graphical evidence, we also present OLS estimates on the relationship between austerity spending cuts, as measured by the change between the least and most exposed cohorts within a municipality, and test score changes. Figure 2 shows a binned scatterplot of the change in math and reading test scores (y axis) and the average change in (the log of) spending following austerity (x axis). We plot these differences in equal-sized bins with the line of best fit. The positive slope suggests that the difference in test scores between the most and least exposed cohorts from the same municipality tends to be larger, on average, for municipalities that experienced larger drops in per-pupil spending. This figure provides graphical support that students more exposed to austerity spending cuts experience larger test score losses.

3.2. *2SLS Estimates*

Table 2 presents our 2SLS estimates, showing the impact of austerity-induced spending changes on standardised test scores for math (panel A) and reading (panel B). Each outcome variable has a mean of 0 and an SD of 1, while per-pupil spending is expressed in levels (in thousands of 2020 euros) and logs. We find significant effects on test scores from austerity cuts. Our baseline estimate (column (5)) suggests that a 1,000-euro austerity-induced drop in per-pupil spending reduces math test scores by about 1% of an SD. The impact of reading test scores is similar, amounting to 0.9% of an SD. The logged measure implies that nearly a 1% drop in spending

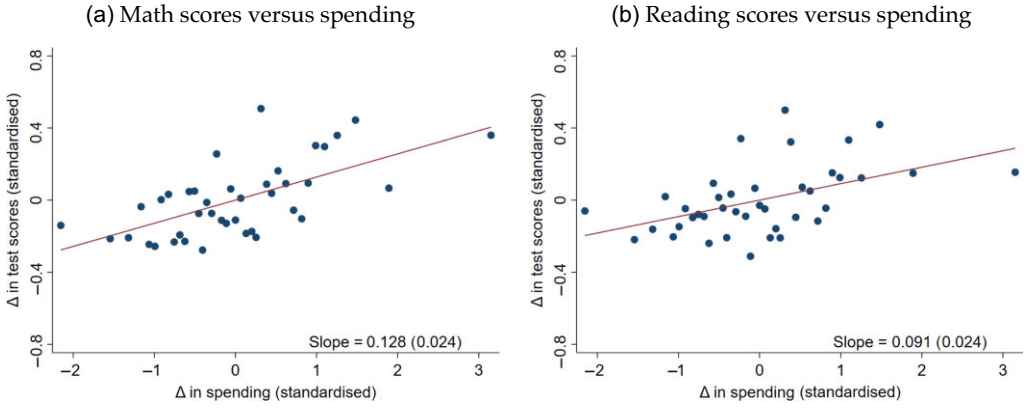

Fig. 2. *Comparing per-Pupil Spending Changes with Test Score Changes.*
Notes: The figure compares the difference in standardised fifth-grade test scores in math (Panel a) and reading (Panel b) on vertical axis, with the standardised difference in (the log of) per-pupil spending on the horizontal axis. These outcomes are computed as the within-municipality difference between the most and least austerity-exposed birth cohorts. We plot these outcomes in 40 equal-sized bins, and show the line of best fit. Each graph also reports the estimated slope.

reduces test scores by nearly 1.024% (0.933%) of an SD in math (reading).[21] The implied test score effects from austerity spending cuts are relatively large. A back-of-the-envelope calculation suggests that the implied test score effect of austerity spending cuts is around 5.1% (4.6%) of an SD for math (reading) test scores.[22]

Point estimates are fundamentally similar across specifications and lie in the range of existing estimates on the marginal impact of spending changes on student outcomes. In Online Appendix Figure A5, we put our estimates in an international perspective. Following Jackson and Mackevicius (2023), we select studies investigating the causal impact of (mostly school) spending on standardised test scores. Each coefficient reported in the figure shows the effect of a 1,000-dollar per-pupil school spending change on standardised test scores, assuming an exposure effect of four years. To ensure comparability, we apply three adjustments to our baseline estimates. First, we average our coefficient estimate by subject. Second, we convert the estimate to 2,020 dollars. Third, we re-weight our estimated effect, assuming an exposure effect of four years. It turns out that an austerity spending cut of 1,000 dollars reduced test scores by 0.62% of an SD. This estimate is close to the median value of the effect distribution, where most precise estimates are located. However, one caveat of this comparison is that our estimates refer to *total* public spending changes, while most of the literature has focused only on *school* spending changes.

In Online Appendix Table A6, we compare OLS and 2SLS estimates. The OLS estimates show a small, although statistically significant, positive correlation between spending and test scores.

[21] The table also reports the *p*-value from the Sargan-Hansen test of overidentifying restrictions, where the joint null hypothesis is that the instruments are valid (i.e., uncorrelated with the error term). The test provides reassuring evidence that the null hypothesis is not rejected in each specification.

[22] This is computed by multiplying the implied average per-pupil cost of austerity (5,130 euros, as described in Section 3.1) by the marginal test score impact of a 1,000-euro spending change (the coefficient displayed in column (5) of Table 2). Hence, the implied test score effect of austerity cuts is $5.130 \times 0.010 = 5.1\%$ for math; $5.130 \times 0.009 = 4.6\%$ for reading.

Table 2. *Baseline Results.*

	(1)	(2)	(3)	(4)	(5)
Panel A. Outcome: standardised V grade math test score					
Spending (1,000 euros)	0.014***	0.013***	0.012***	0.011**	0.010**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
F-statistics	13.254	13.100	11.535	11.529	9.905
Sargan-Hansen test (*p*-value)	0.308	0.227	0.114	0.121	0.129
Spending (log)	1.188***	1.126***	1.132***	1.062***	1.024**
	(0.344)	(0.337)	(0.361)	(0.356)	(0.399)
F-statistics	22.453	22.337	19.040	19.034	15.853
Sargan-Hansen test (*p*-value)	0.777	0.993	0.727	0.697	0.678
Panel B. Outcome: standardised V grade reading test score					
Spending (1,000 euros)	0.012***	0.012***	0.011***	0.010**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
F-statistics	13.254	13.100	11.535	11.529	9.905
Sargan-Hansen test (*p*-value)	0.390	0.328	0.198	0.172	0.179
Spending (log)	1.030***	1.016***	1.002***	0.939***	0.933**
	(0.325)	(0.321)	(0.350)	(0.346)	(0.392)
F-statistics	22.453	22.337	19.040	19.034	15.853
Sargan-Hansen test (*p*-value)	0.799	0.925	0.801	0.695	0.692
# of students	962,898	962,897	962,897	962,897	962,897
# of schools	2,684	2,684	2,684	2,684	2,684
# of municipalities	2,276	2,276	2,276	2,276	2,276
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Cohort FEs	Yes	Yes	Yes	Yes	Yes
School FEs	No	Yes	Yes	Yes	Yes
Municipality controls	No	No	Yes	Yes	Yes
Individual controls	No	No	No	Yes	Yes
Other controls	No	No	No	No	Yes

Notes: This table reports 2SLS estimates on the effect of austerity-induced per-pupil spending changes on standardised test scores in math (panel B) and reading (panel B). Individual-level controls include gender, nationality, quarter of birth, father's education, mother's education and dummies for grade repeaters. Municipality-level controls include the age, gender and education of the mayor and average values for the other town council members, and election year-cohort fixed effects. Other controls include the municipal average tax base and a house price index. Each panel also reports the *F*-statistic to test for weak instruments and the *p*-value from the overidentification test of the instruments (Sargan-Hansen test). SEs clustered at the municipality level are reported in parentheses. Significance level: *** 1 percent, ** 5 percent.

This is consistent with the fact that there is a negative bias when using actual spending changes across cohorts within municipalities to predict student outcomes, leading the OLS estimates to be underestimated.

To summarise, our results suggest that austerity cuts had a significant negative effect on test scores. Our estimates imply that austerity spending cuts reduced math test scores by 5.1% of an SD, and reading test scores by 4.6%.

3.3. *Heterogeneous Responses*

We present the heterogeneous test score impacts from austerity cuts in Figure 3, which depicts coefficient estimates and 90% confidence intervals from our 2SLS baseline model. Each point estimate is computed by estimating the 2SLS model on different sub-samples of the original population. The test score is averaged between math and reading, and spending is expressed in 1,000 euros (we find similar results using the logged measure of spending). In each graph, we present estimates obtained from specifications that include municipality and cohort fixed effects,

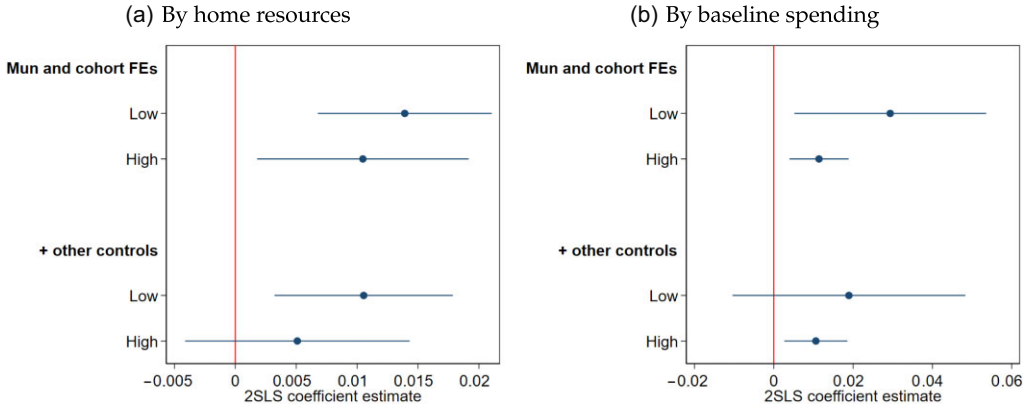

Fig. 3. *Heterogeneous Responses.*

Notes: The figure reports 2SLS coefficient estimates and 90% confidence intervals on the test score impact (average between math and reading) of austerity spending cuts (1,000 euros). We divide our sample according to home resources (Panel a), where the median value is computed within each municipality-cohort cell, and baseline spending per pupil (Panel b). For each specification, we divide the original sample according to whether a municipality is below (denoted as 'Low') or above (denoted as 'High') the median value in a given category. In each panel, we present both the 2SLS estimate with municipality and cohort fixed effects, and those that also include school fixed effects, student-level controls (gender, citizenship, quarter of birth, father's education, mother's education and dummies for grade repeaters) and municipality-level controls (age, gender and education of the mayor and average values for other town council members, election year-cohort fixed effects, taxable income and house prices).

and estimates that also account for school fixed effects, student-level controls and municipality-level controls.

We start by testing whether the test score impact is more intense among students with limited resources at home (left-hand side graph). We construct a student-specific index of home resources by using survey information on the availability of the following resources at home: computer, desk, encyclopedia, internet connection, study room and books. We compute this index as the average probability of owning these resources. We then split students according to whether they are below or above the (municipality-cohort-specific) median value. The scatter plot shows that the marginal test score impact from austerity spending cuts is relatively larger for students with limited home resources. Our baseline model shows that, on average, a 1,000-euro austerity cut in per-pupil spending reduces test scores by around 1% of an SD in the sample of students with limited home resources (t-statistics = 2.37), while the impact is around half (0.5%) and not statistically significant for students with more resources at home (t-statistics = 0.91).

We then examine whether the marginal test score impact of austerity cuts depends on the baseline spending level (right-hand side graph). As shown in Figure 1, baseline spending widely varied across municipalities. In principle, austerity cuts could have smaller effects in municipalities starting with higher levels of spending. Our estimates reflect a pattern of diminishing marginal impacts for municipalities with larger pre-existing spending: the marginal test score impacts of a 1,000-euro austerity spending cut is around 1.9% of an SD in municipalities with lower baseline spending, but only 1.1% in municipalities with higher baseline spending. However, this

heterogeneity should be taken cautiously, since our test score effects relative to low-spending municipalities are imprecisely estimated.

3.4. *Robustness Checks and Alternative Specifications*

This section presents several falsification tests and robustness checks. For the sake of space, we report more details about the following analyses in Online Appendix C.

3.4.1. *Current versus capital spending cuts*

Our test score impact of austerity incorporates spending cuts from both current and capital expenditure drops. If the type of spending cut matters for test scores then our results would mask heterogeneity depending on whether municipalities mostly cut current or capital expenditure in response to austerity. In Online Appendix Table C2, we separately investigate the test score impact for current versus capital austerity spending cuts. We find reasonably similar effects on test scores from the two types of spending. Our baseline estimate suggests that a 1,000-euro austerity-induced drop in current (capital) spending reduces math test scores by about 2.3% (1.8%) of an SD. We also find a similar impact on reading test scores. Our results thus suggest that marginal capital spending cuts are similar to non-capital spending reductions.

3.4.2. *Alternative measures of austerity exposure and dosage*

Our exposure definition implicitly imposes a linearity assumption: the spending change generated by two years of exposure will be almost half of the spending change induced by four years of exposure. To mitigate concerns related to non-linear effects, we test the sensitivity of our estimates to alternative measures of austerity exposure in Online Appendix Table C3. First, we allow for non-linearity in exposure effects by adding the square of exposure and its interaction with dosage (column 2). Second, we use a binary measure of exposure (computed according to whether a municipality's value is above or below the median value) rather than a continuous measure (column 3). Third, we interact exposure with dummies for each quintile of budget rigidity intensity (column 4). Finally, we interact exposure directly with the budget rigidity intensity variable (column 5). While the interpretation of the first-stage effect differs across these specifications, we find that regardless of how we measure exposure, austerity had a negative effect on public spending. The effects on test scores are relatively similar to our baseline estimates. To assuage concerns related to the fact that dosage might be correlated with other unobservable municipality characteristics, Online Appendix Table C4 also shows that our estimates are fairly similar, although less precise, when we remove dosage (and its interaction with exposure) as an instrument.

3.4.3. *Residential mobility*

As previously discussed, one limitation of our data is that we can observe the municipality where a school is located, but not the student residence. This implies that we could have a measurement error in assigning public spending across students. Although the enrolment process characteristics of the Italian primary school system substantially mitigate concerns related to student selection across schools, there could be cases, especially in small-size municipalities, where there is no primary school, and children need to attend schools in neighbouring municipalities. To test the sensitivity of our estimates to this issue, we proceed in the following way. First, we identify municipalities where there is no primary school (Ministero dell'Istruzione, 2021). Second, we

flag all the municipalities that share a border with a municipality lacking a primary school, which could be affected by an inflow of students whose spending is measured with errors. Finally, we estimate our 2SLS model from a sample without these flagged municipalities. We report the 2SLS coefficient estimates in Online Appendix Table C5. The table shows that the measurement error in determining the municipality of residence is not a significant source of bias: estimates obtained from the original sample are fairly similar to those computed from the sample that excluded municipalities that may have been exposed to an inflow of students.[23]

3.4.4. *The impact of austerity on local taxes and parental labour market outcomes*

One could be concerned that the DSP triggers other changes, such as higher tax rates and more general labour market dynamics. If more exposed cohorts systematically differ in other aspects, and these other aspects influence test scores, then the exclusion restriction would be jeopardised. A simple way to evaluate this possibility is to test whether instrumented per-pupil spending predicts economic conditions or other labour market outcomes that might be associated with test scores. We thus estimate our 2SLS model on the following outcomes: local tax rates,[24] the municipal average tax base and parental employment. Online Appendix Table C6 shows no significant effects on these outcomes.

3.4.5. *Spatial correlation in the error term*

We account for the possibility of spatial correlation in the error term by clustering the SEs on a higher level of aggregation, which in our case is the province. Online Appendix Figure C1 shows that our estimates remain statistically significant at usual confidence intervals when we use SEs clustered at the school, municipality (our baseline) or province level. An additional concern is that the error term could be correlated, not only over time within the panel dimension, but also across cohorts taking the test in the same year. To account for this issue, we also present SEs that allow for two-way clustering by cohort and school or cohort and municipality. Although coefficient estimates are less precise when implementing the two-way clustering strategy, they remain statistically significant at conventional confidence intervals.

3.4.6. *Alternative specifications and other placebo tests*

In Online Appendix C, we propose additional tests and checks to inspect the robustness of our results. First, Online Appendix Figures C2 and C3 show that our baseline estimates are robust to a richer set of student-level controls, and for additional proxies for the local business cycle.[25] Second, we show that our estimates are robust to the following alternative definition of spending: (*a*) per-capita, instead of per-pupil, spending (Online Appendix Table C7); (*b*) assuming different annual devaluation rates (Online Appendix Figure C4); (*c*) when we account for discrepancies between approved and disbursed investments (Online Appendix Table C8). Third, we estimate the test score impact of austerity spending cuts by year of birth, including pre-birth and post-primary school years. This test allows us to check whether past or future expenditures impact test scores measured at the end of primary school. Online Appendix Figure C5 shows that public expenditure

[23] An additional interpretation of this result is that austerity cuts in school resources, rather than in other expenditures, are the key driver of student performance. A different municipality of residence, which might translate into a different exposure to non-school-related expenditure cuts, has no meaningful impact on test scores among students exposed to the same school environment.

[24] See Fondazione IFEL (2021) for data on local tax rates.

[25] While our estimates are robust to including region-cohort fixed effects, using province-cohort fixed effects leads to estimates that are not statistically significant at usual confidence intervals.

before birth is irrelevant for test scores, while effects from future expenditure are imprecisely estimated. Fourth, we show that our estimates remain substantially similar when we do not adjust test scores for cheating (Online Appendix Table C9). Finally, we propose a placebo test based on geographical differences in DSP eligibility. If our instruments were correlated with unobserved confounders, we should observe a similar test score impact for students who attend schools in places not impacted by the DSP. We perform this placebo test on students attending a school located in regions with special autonomy (Aosta Valley, Friuli Venezia-Giulia, Sardinia, Sicily and Trentino Alto-Adige) that are not subject to the DSP (or at least not to the same extent as in the other regions). Online Appendix Table C10 shows that the first-stage and reduced-form effects are not statistically significant. Taken together, these placebo tests provide reassuring evidence that our estimated effects are due to austerity policies, rather than to some municipality-level or student-level unobserved confounders.

4. Mechanisms

This section investigates the mechanisms behind the test score impact of austerity cuts. First, Section 4.1 provides two exercises to distinguish what types of public spending cuts drive the test score impact. We conduct a conventional mediation analysis and a test based on comparing private and public schools. In Section 4.2, we explore the impact of austerity on several school inputs and facilities funded by municipalities. Finally, we provide suggestive evidence on the role of school principals.

4.1. *What Type of Spending Cut Drives the Test Score Impacts?*

4.1.1. *Mediation analysis*

As a first step, we conduct a conventional mediation analysis to study the role of several public budget items.[26] Figure 4 reports the results of this analysis, where we break down the overall treatment effect into shares attributed to each budget item. We find that educational spending represents the most relevant spending chapter, which explains almost half of the total test score impact. Our results also show that administrative spending has a significant effect, explaining around 20% of the impact of the test score. Since administrative spending represents the lion's share of municipal spending and is possibly composed of passive waste (Bandiera *et al.*, 2009), this result could reflect the negative correlation between test scores and the amount of public expenditures 'wasted' in administrative expenses (which primarily include rigid sources of spending, such as payroll expenses and debt services). We also find a non-negligible impact from tourism (around 8% of the effect) and sport (about 10%). All other expenditure chapters have a negligible effect. Given that we do not have an instrument per each spending chapter comprising the municipal balance, one issue with this conventional mediation analysis is that choosing how to allocate the austerity spending cut is endogenous. We thus interpret this evidence as merely suggestive.

4.1.2. *Private versus public schools*

Was the austerity-driven school spending cut mainly responsible for the negative test score impact, or are other austerity-driven cuts in public expenditures driving the effect on test scores?

[26] See Online Appendix B for details on how these budget items are calculated from balance sheet data and Online Appendix D for more information on the mediation analysis.

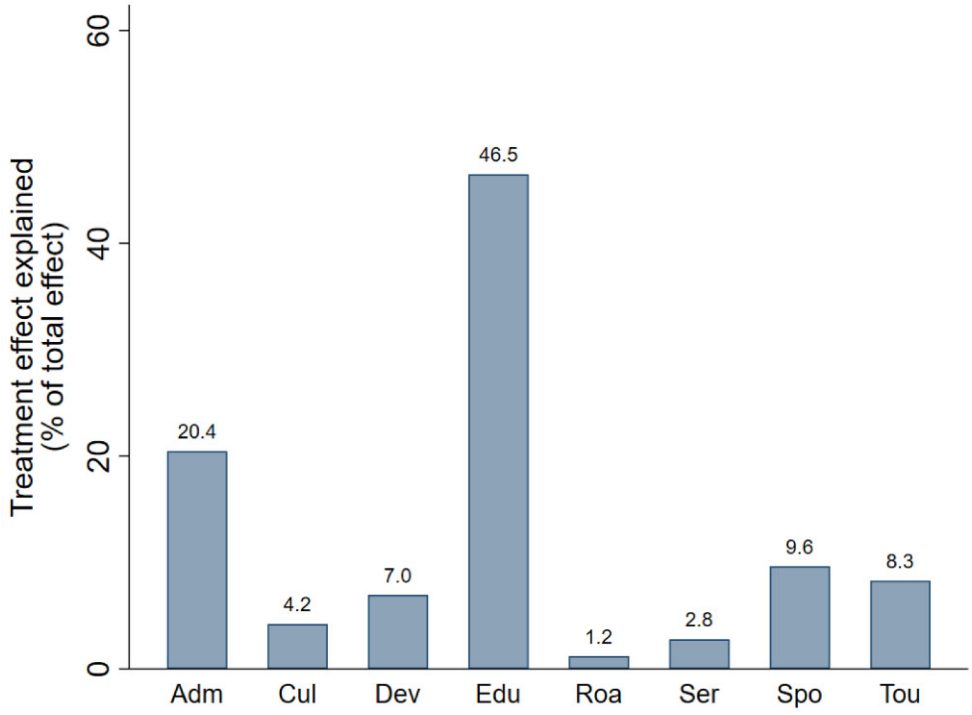

Fig. 4. *Mediation Analysis.*
Notes: The figure shows the results of mediation analysis, depicting the share of the effect of austerity on student test scores attributable to each spending item (education, administration, tourism, public transportation, service, development, culture and sport), our mediators. To compute these estimates, we control for school fixed effects, cohort fixed effects, individual-level controls (gender, nationality, quarter of birth, father's education, mother's education and a dummy for grade repeaters), municipality-level controls (age, gender and education of the mayor and average values for the other town council members, election year-cohort fixed effects, the municipal average tax base and house price index) and all the main (excluded) capital and current spending chapters comprising the municipal budget: administration ('Adm'), culture ('Cul'), development ('Dev'), education ('Edu'), roads and public transportation ('Roa'), services ('Ser'), sport ('Spo') and tourism ('Tou'). Spending items are expressed in logs and are aggregated between current and capital expenditures.

To better understand this, we focus on students enrolled at private schools. We exploit the fact that the municipal government does not control private schools to conduct a 'placebo' exercise. If we uncovered any significant test score impact on students at private schools, our results would then suggest that austerity spending cuts operate through other (non-educational spending-related) channels.

We thus compare our baseline 2SLS coefficient estimates with estimates for 165,283 students enrolled in 1,392 private schools. Figure 5 reports the 2SLS coefficient estimates and 90% confidence intervals on the test score impact of austerity spending cuts from two samples: (*i*) the baseline sample based on public schools; (*ii*) the sample of students enrolled in private schools. We find that austerity spending cuts do not impact the test scores of private school students. We can thus reject the hypothesis that the effect of austerity spending cuts is the same in private

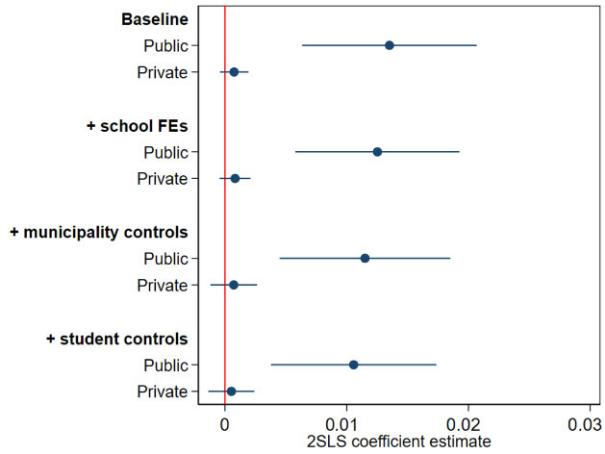

Fig. 5. *Comparing Test Score Impacts in Public and Private Schools.*
Notes: The figure shows the effect of austerity spending cuts on test scores of public versus private schools. The figure reports 2SLS coefficient estimates and 90% confidence intervals on the test score impact of austerity spending cuts from two samples: (*i*) the baseline sample based on public schools; (*ii*) the sample of students enrolled in private schools (165,283 students enrolled in 1,392 different private schools). Test scores are math and reading averages; spending is total (current plus capital) municipal spending per pupil (expressed in 1,000 euros). The baseline specification (first panel) includes cohort fixed effects and municipality fixed effects. We then cumulatively add school fixed effects (second panel), municipality-level controls (age, gender and education of the mayor and average values for other town council members, and election year-cohort fixed effects; third panel) and student-level controls (gender, nationality, quarter of birth, father's education, mother's education and dummies for grade repeaters; fourth panel). SEs are clustered at the municipality level.

and public schools. This finding provides suggestive evidence that there is no adverse effect of austerity spending cuts on student achievement when the austerity cut operates through non-school-related expenditures. It also rules out an effect of austerity on student achievement due to any shocks that are common to the entire municipality, such as municipality-level changes in income.

The figure also shows that this result holds when we control for school fixed effects (second row), which account for the fact that private and public schools might be structurally different for several reasons, including staff and teacher characteristics or quality. We also find that controlling for student-level characteristics (fourth row) has little impact on the coefficient estimates. This implies that austerity spending cuts do not have compositional effects on the distribution of students enrolled in public versus private schools.

4.2. *Zooming in on Educational Spending*

The previous analyses suggest school spending cuts to be the leading explanation for the test score impacts from austerity. To examine this channel in detail, this section combines various data sources to explore the effect of austerity cuts on the availability of several school resources and inputs. The analysis proceeds in two steps. First, we study the impact of austerity spending cuts on several school inputs and resources by using school-level administrative data on

Table 3. *The Impact of Austerity on Primary School Resources.*

	Outcome variable:					
	Class size (#)	Out-of-hours reception (0/1)	Lab (factor)	Library and gym (factor)	Teaching items (factor)	Tech items (factor)
	(1)	(2)	(3)	(4)	(5)	(6)
Spending (1,000 euros)	−0.047*	−0.002	0.008***	0.005**	−0.000	−0.000
	(0.024)	(0.003)	(0.003)	(0.002)	(0.002)	(0.000)
Spending (log)	−3.396*	−0.097	0.675***	0.409**	−0.013	−0.009
	(1.776)	(0.212)	(0.219)	(0.169)	(0.140)	(0.021)
# of students	962,897	962,897	128,998	128,998	128,998	126,705
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent	15.782	0.317	−0.000	−0.000	−0.000	−0.000

Notes: This table reports 2SLS estimates on the effect of austerity-induced per-pupil spending changes on the following outcome variables: class size (column (1)); whether the school provides out-of-hours reception for students before or after the school timetable (column (2)); a factor variable capturing whether the school is equipped with a functioning computer, language, scientific, musical, technical or art laboratories (column (3)); a factor variable capturing whether the school has a functioning library, book loan services and a gym (column (4)); a factor variable capturing availability of teaching tools, such as teaching items, books, audiovisual tools and interactive blackboards (column (5)); a factor variable capturing availability of technological items, such as software, tablets, a functioning connection to the internet and computers (column (6)). The factor variables are computed by applying a principal component analysis and using survey responses from school principals. Specifications in columns (1) and (2) include school fixed effects, cohort fixed effects, student-level controls and municipality-level controls. Specifications in columns (3)−(6) include province fixed effects (instead of school fixed effects). SEs clustered at the municipality level are reported in parentheses. Significance level: *** 1 percent, ** 5 percent, * 10 percent.

school facilities and survey information on a stratified sample of teachers and school principals. Second, we explore the role of school principals in mitigating the adverse test score effect of austerity.

4.2.1. *Primary school resources and facilities*

Several studies have shown that school facility investments improve student outcomes (see reviews in Jackson and Mackevicius, 2023), especially when school facilities are in deplorable conditions. To study whether austerity effects on test scores operate through cuts in school resources and facilities, we gather information on various school inputs from balance sheets and INVALSI data. We then regress several school outcomes on (instrumented) per-pupil spending.

We report the 2SLS coefficient estimates from our baseline specification in Table 3. We focus on three main categories of school inputs. First, since austerity cuts could have prevented municipalities from financing renovation, repairs and expansions of school buildings, we examine whether austerity leads to overcrowded classrooms. We find that austerity spending cuts result in a statistically significant, but economically small, increase in class size. On average, we find that a 1,000-euro per-pupil reduction in spending raises class sizes by nearly 0.047 students. This estimate suggests that the implied class size effects from austerity spending cuts are a 0.25% class size increase.[27] Given existing estimates on the relationship between test scores and class size (Chetty *et al.*, 2011), the austerity-induced class size effects would map into a trivial test score impact. We thus conclude that the test score effect of austerity cuts through larger class sizes was negligible.

[27] This is computed by multiplying the average exposure (8.485 yr) by the average spending cut from an additional year of austerity (632 euros) and by the class size impact of austerity cuts (0.047 students).

Second, because the spending reduction could have been partly due to hiring fewer non-instructional staff (e.g., laboratory technicians, janitors), we look at whether instrumented per-pupil spending predicts the availability of out-of-hours reception for pupils before or after the school timetable. We find negative, but imprecisely estimated, effects, suggesting that austerity spending cuts had no clear impact on the possibility for schools to provide out-of-hours reception.

Third, we use survey data from school principals to elicit information on the availability (and proper functioning if available) of libraries, gyms, laboratories (including musical, art and linguistic laboratories), teaching tools (e.g., interactive blackboard, audiovisual tools) and technological items (computers, internet connection and tablets). The presence of laboratories may encourage student initiative, creativity and interdisciplinarity, perhaps stimulating both cognitive and non-cognitive abilities. Since austerity was particularly harmful to students with limited resources at home, the availability of these types of input at school might help mitigate the test score effects of austerity cuts. To conduct this analysis, we use province fixed effects, rather than municipality fixed effects, since the panel dimension is severely limited (just a few schools are interviewed over two different years). Although representative by construction, our final sample is smaller than our main sample, composed of 128,998 students and 1,221 municipalities. We summarise all the information gathered from the survey data in four indexes: (*i*) laboratories; (*ii*) library and gym; (*iii*) teaching items; (*iv*) technological items. Each factor variable has a mean zero and an SD equal to one.

We find that austerity spending cuts had a negative effect on the availability of laboratories, libraries and gyms. By contrast, we do not see any significant impact on teaching and technological tools. On average, the probability of utilising laboratories (libraries and gyms) would decrease by 2.8% (1.8%) of an SD in cohorts with average austerity exposure, compared to cohorts never exposed.

Taken together, these analyses suggest that austerity limited funding to some school resources and facilities, such as laboratories, libraries and gyms, which are likely to affect child outcomes. By contrast, we find that the test score effect of austerity cuts is unlikely to result from larger class sizes.

4.2.2. *The role of school principals*

This section explores whether more competent school principals could mitigate the adverse test score effect of austerity. Several factors can explain why school principals can matter. For instance, more competent school principals could better manage declining spending by reallocating resources from low- to high-productive items. They can also offset declining spending by favouring the conditions and climate in which teaching and learning occur (see, e.g., Di Liberto *et al.*, 2015).[28]

We classify more competent school principals as those with at least a master's degree.[29] We then run 2SLS regressions of test scores on austerity spending cuts separately by principal competence.

[28] One concern for this analysis is that school principals might sort into more affluent municipalities (or more equipped schools). However, the Italian context greatly reduces endogeneity concerns because principals are assigned to schools by the Regional School Authority, which fills vacant positions through a process mostly based on seniority. Moreover, like teachers, school principals receive neither incentives nor rewards based on student achievement, and their salaries are set centrally, varying only according to experience and seniority.

[29] We recognise that, in practice, competence is a complex mix of skills. It could include intangible leadership skills, like the ability to favour better teaching and learning conditions, and analytical skills, such as detecting inefficient sources of spending in the school budget. Our measure is an (imperfect) proxy for the latter skill.

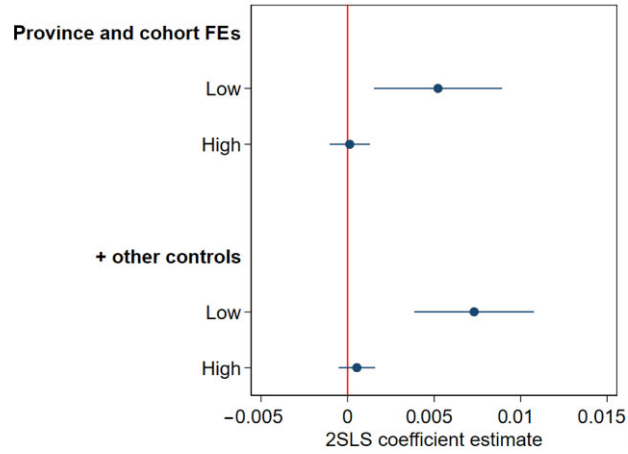

Fig. 6. *Test Score Impact of Austerity Cuts by School Principal Competence.*
Notes: The figure reports 2SLS coefficient estimates and 90% confidence intervals on the test score impact of austerity spending cuts by school principal competence. We define a school principal as 'more competent' when she holds a master's degree (denoted as 'High') and 'less competent' if she does not (denoted as 'Low'). In each panel, we present both the 2SLS estimate with province and cohort fixed effects, and those that also include student-level controls (gender, nationality, quarter of birth, father's education, mother's education and a dummy for grade repeaters) and municipality-level controls (age, gender and education of the mayor and average values for other town council members, election year-cohort fixed effects, taxable income and house prices). SEs clustered at the municipality level.

Since our principal-level data are a repeated cross section and we have just a few cases where a municipality appears multiple times in the data, we use province, instead of municipality, fixed effects (the other baseline controls remain the same as in the other specifications). If principals are relevant, we expect the marginal test score impact of austerity cuts to be systematically lower in schools managed by more competent principals, compared to schools with less competent principals.

Figure 6 presents the results; it displays the coefficient estimates and 90% confidence intervals of austerity spending cuts on test scores by principal competence. We find that marginal test score impacts of austerity cuts are exclusively concentrated among schools led by less competent principals. This result is robust to the inclusion of several municipality-level and student-level controls. More competent school principals thus appear to be able to offset the detrimental effect of austerity cuts.

This finding provides a first indication that austerity cuts can be less detrimental, if any, in settings where more competent school principals head schools. Our data, however, do not allow us to provide a fully fledged analysis of how school principals offset the adverse effects of austerity cuts.

5. Conclusions

In the aftermath of the financial crisis, the welfare state has come under attack from austerity policies. Calls to roll back public spending have been especially forceful in Europe, where the welfare state has traditionally played a major role. While the agenda of austerity reforms is to

make the welfare state leaner and more efficient, austerity might also undermine the government's ability to finance public expenditures. Understanding if and how much austerity cuts can hurt socio-economic outcomes is of critical societal importance for evaluating the overall effects of austerity policies.

This paper studies whether austerity spending cuts harmed student cognitive abilities, as measured in fifth-grade standardised test scores. Our context is Italy, which offers data and quasi-experimental variation that allows us to estimate the test score impacts of austerity spending cuts. We find that the overall effect of austerity spending cuts reduced test scores by 5.1% in math and 4.6% in reading. The effects are more pronounced for children with limited resources at home, suggesting that austerity loosens substitutability between private and public resources.

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

Ifo Institute and CESifo, Germany
University of Lausanne, Switzerland

Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

References

Agenzia delle Entrate. (2021). 'Italian internal revenue agency—osservatorio del mercato immobiliare: House price data [dataset]', https://www.agenziaentrate.gov.it/portale/web/guest/schede/fabbricatiterreni/omi/forniture-dati-omi-cittadini (last accessed: 1 June 2023).

Almond, D., Currie, J. and Duque, V. (2018). 'Childhood circumstances and adult outcomes: Act II', *Journal of Economic Literature*, vol. 56, pp. 1360–446.

Bandiera, O., Prat, A. and Valletti, T. (2009). 'Active and passive waste in government spending: Evidence from a policy experiment', *American Economic Review*, vol. 99(4), pp. 1278–308.

Bloom, N., Lemos, R., Sadun, R. and Van Reenen, J. (2015). 'Does management matter in schools?', Economic Journal, vol. 125(584), pp. 647–74.

Bray, K., Braakmann, N. and Wildman, J. (2022). 'Austerity, welfare cuts and hate crime: Evidence from the UK's age of austerity', *Journal of Urban Economics*, doi: 10.1016/j.jue.2022.103439.

Brunner, E., Hyman, J. and Ju, A. (2020). 'School finance reforms, teachers' unions, and the allocation of school resources', *Review of Economics and Statistics*, vol. 102(3), pp. 473–89.

Camera dei Deputati. (2013). 'Indagine conoscitiva sulla situazione dell'edilizia scolastica in Italia', Stegenographic reports of cognitive investigations, https://www.camera.it/leg17/1135?id_commissione=&shadow_organo_parlamentare=&sezione=commissioni&tipoDoc=elencoResoconti&idLegislatura=17&tipoElenco=indagini ConoscitiveCronologico&calendario=false&breve=c07_edilizia&scheda=true (last accessed: 1 June 2023).

Cellini, S.R., Ferreira, F. and Rothstein, J. (2010). 'The value of school facility investments: Evidence from a dynamic regression discontinuity design', *Quarterly Journal of Economics*, vol. 125(1), pp. 215–61.

Chetty, R., Friedman, J.N., Hilger, N., Saez, E., Schanzenbach, D.W. and Yagan, D. (2011). 'How does your kindergarten classroom affect your earnings? Evidence from project star', *The Quarterly Journal of Economics*, vol. 126(4), pp. 1593–660.

Cittadinanzattiva. (2022). 'XX Osservatorio nazionale sulla sicurezza delle scuole', 15 September, https://www.cittadinanzattiva.it/notizie/15194-presentato-il-xx-osservatorio-nazionale-sulla-sicurezza-delle-scuole.html (last accessed: 1 June 2023).

Comi, S.L., Argentin, G., Gui, M., Origo, F. and Pagani, L. (2017). 'Is it the way they use it? Teachers, ICT and student achievement', *Economics of Education Review*, vol. 56, pp. 24–39.

Corte dei Conti. (2012). 'Rapporto 2012 sul coordinamento della finanza pubblica', Technical Report.
Cunha, F., Heckman, J.J. and Schennach, S.M. (2010). 'Estimating the technology of cognitive and noncognitive skill formation', *Econometrica*, vol. 78(3), pp. 883–931.
Dadvand, P., Nieuwenhuijsen, M.J., Esnaola, M., Forns, J., Basagaña, X., Alvarez-Pedrerol, M., Rivas, I., López-Vicente, M., De Pascual, M.C., Su, J., Jerrett, M., Querol, X. and Sunyer, J. (2015). 'Green spaces and cognitive development in primary school children', *Proceedings of the National Academy of Sciences of the United States of America*, vol. 112(26), pp. 7937–42.
Di Liberto, A., Schivardi, F. and Sulis, G. (2015). 'Managerial practices and student performance', *Economic Policy*, vol. 30(84), pp. 683–728.
European Commission, Directorate-General for Education, Youth, Sport and Culture. (2016). *Education and Training Monitor 2016 – Country Analysis*, Publications Office, https://data.europa.eu/doi/10.2766/680110 (last accessed: 1 June 2023).
Facchetti, E. (2021). 'Police infrastructure, police performance, and crime: Evidence from austerity cuts', Job Market Paper, Queen Mary University of London.
Fetzer, T. (2019). 'Did austerity cause Brexit?', *American Economic Review*, vol. 109(11), pp. 3849–86.
Fondazione IFEL. (2021). 'Istituto per la finanza e l'economia locale: Local tax rates [dataset]', https://www.fondazioneifel.it/ (last accessed: 1 June 2023).
Grembi, V., Nannicini, T. and Troiano, U. (2016). 'Do fiscal rules matter?', *American Economic Journal: Applied Economics*, vol. 8(3), pp. 1–30.
Heckman, J., Pinto, R. and Savelyev, P. (2013). 'Understanding the mechanisms through which an influential early childhood program boosted adult outcomes', *American Economic Review*, vol. 103(6), pp. 2052–86.
Holden, K.L. (2016). 'Buy the book? Evidence on the effect of textbook funding on school-level achievement', *American Economic Journal: Applied Economics*, vol. 8(4), pp. 100–27.
Hyman, J. (2017). 'Does money matter in the long run? Effects of school spending on educational attainment', *American Economic Journal: Economic Policy*, vol. 9(4), pp. 256–80.
Imbens, G.W. and Angrist, J.D. (1994). 'Identification and estimation of local average treatment effects', *Econometrica*, vol. 62(2), pp. 467–75.
INVALSI. (2021). 'Istituto nazionale per la valutazione del sistema educativo di istruzione e di formazione: Microdata [dataset]', https://serviziostatistico.invalsi.it/ (last accessed: 1 June 2023).
ISTAT. (2021). 'Istituto nazionale statistica: Demographic data [dataset]', https://www.istat.it/ (last accessed: 1 June 2023).
Jackson, C.K., Johnson, R.C. and Persico, C. (2016). 'The effects of school spending on educational and economic outcomes: Evidence from school finance reforms', *The Quarterly Journal of Economics*, vol. 131(1), pp. 157–218.
Jackson, C.K. and Mackevicius, C.L. (2023). 'What impacts can we expect from school spending policy? Evidence from evaluations in the US', *American Economic Journal: Applied Economics*, forthcoming.
Klatte, M., Bergström, K. and Lachmann, T. (2013). 'Does noise affect learning? A short review on noise effects on cognitive performance in children', *Frontiers in Psychology*, vol. 4, pp. 1–6.
Lafortune, J., Rothstein, J. and Schanzenbach, D.W. (2018). 'School finance reform and the distribution of student achievement', *American Economic Journal: Applied Economics*, vol. 10(2), pp. 1–26.
Litschig, S. and Morrison, K.M. (2013). 'The impact of intergovernmental transfers on education outcomes and poverty reduction', *American Economic Journal: Applied Economics*, vol. 5(4), pp. 206–40.
Lledò, V., Yoon, S., Fang, X., Mbaye, S. and Kim, Y. (2017). 'Fiscal rules at a glance', Background Note, International Monetary Fund.
Ministero degli Interni. (2021a). 'Ministry of interior: Municipal balance sheets data [dataset]', https://dait.interno.gov.it/ (last accessed: 1 June 2023).
Ministero degli Interni. (2021b). 'Ministry of interior: Municipal sanctions [dataset]', https://dait.interno.gov.it/ (last accessed: 1 June 2023).
Ministero degli Interni. (2021c). 'Ministry of interior: Registry of local administrators [dataset]', https://dait.interno.gov.it/elezioni/anagrafe-amministratori (last accessed: 1 June 2023).
Ministero dell'Istruzione. (2021). 'Italian ministry of education (MIUR)—ufficio gestione patrimonio informativo e statistica: Active schools [dataset]', https://www.miur.gov.it/ (last accessed: 1 June 2023).
Ministero Economia e Finanza. (2021). 'Ministry of economy and finance: Municipal tax base [dataset]', https://www1.finanze.gov.it/finanze/pagina_dichiarazioni/public/dichiarazioni.php (last accessed: 1 June 2023).
Mogstad, M., Torgovitsky, A. and Walters, C.R. (2021). 'The causal interpretation of two-stage least squares with multiple instrumental variables', *American Economic Review*, vol. 111(11), pp. 3663–98.
OECD. (2018). 'Indicator D6 Who makes key decisions in education systems?', in *Education at a Glance 2018: OECD Indicators*, pp. 408–21, Paris: OECD Publishing, https://doi.org/10.1787/eag-2018-33-en (last accessed: 1 June 2023).
OECD. (2020). *Education at a Glance 2020: OECD Indicators*, Paris: OECD Publishing, https://doi.org/10.1787/69096873-en (last accessed: 1 June 2023).
Park, R.J., Goodman, J., Hurwitz, M. and Smith, J. (2020). 'Heat and learning', *American Economic Journal: Economic Policy*, vol. 12(2), pp. 306–39.

Rei, R. (2012). ‘ “Una scuola su quattro non è in regola”: Cittadinanzattiva denuncia l’emergenza’, *Repubblica*, 9 November, https://inchieste.repubblica.it/it/repubblica/rep-it/2012/11/09/news/sicurezza_qualit_e_comfort_degli_edifici_scolastici_il_rapporto_di_cittadinanzattiva-46283848/ (last accessed: 1 June 2023).

Rubolino, E. (2023). ‘Does weak enforcement deter tax progressivity?’, *Journal of Public Economics*, vol. 219, 104833.

Tan, F., Gong, X. and Tsang, M.C. (2021). ‘The educational effects of children’s museums on cognitive development: Empirical evidence based on two samples from Beijing’, *International Journal of Educational Research*, vol. 106, pp. 1–10.

Tripodi, A. (2017). ‘Scuola: fatiscente un’aula su 3, dal 2013 oltre 150 crolli’, *Il Sole 24 Ore*, 9 October, https://www.ilsole24ore.com/art/scuola-fatiscente-aula-3-2013-oltre-150-crolli–AEn6zYhC?refresh_ce=1 (last accessed: 1 June 2023).

VoxEU. (2012). *Austerity: Too Much of A Good Thing?*, VoxEU.org eCollection of views by leading economists, Centre for Economic Policy Research.

The Economic Journal, **134** (*April*), 1199–1227 https://doi.org/10.1093/ej/uead092 © The Author(s) 2023. Published by Oxford University Press on behalf of Royal Economic Society.
Advance Access Publication Date: 23 October 2023