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Abstract

We explore whether investment in school infrastructure affects students' achievement. We use data on extra funding to undamaged state high schools after the 2012 Northern Italy earthquake and apply a quasi-experimental design and an instrumental variable strategy. We find that spending on school infrastructure increases standardized test scores in Mathematics and Italian language, and the effect is stronger for lower-achieving students and in Mathematics. These results provide evidence in favor of a positive impact of capital spending in improving the learning environment and performances of high school students.

Keywords: education; infrastructure spending; high school; Italian school system.
JEL Classification: I22, I24, H75.

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1 Introduction

Whether or not school spending has an impact on student outcomes is a highly debated issue in economics (Card & Krueger, 1996). The contemporary literature has been pioneered by Coleman (1966) in a prominent report published by the US Government in 1966, whose main conclusion is that school funding does not play a central role in determining students' achievement. A wealth of studies follow in the footsteps of Coleman (1966) and explore the relation between resources and educational outcomes (see e.g., Neilson & Zimmerman, 2014; Jackson *et al.* , 2016). Whereas in a meta-analysis Greenwald *et al.* (1996, p.384) conclude that "school resources are systematically related to student achievement and that these relations are large enough to be educationally important," subsequent studies find little or no effect (see e.g., Hanushek, 1996; Card & Krueger, 1996). More recently, however, Hyman (2017) shows that school funding can boost students' college enrollment and the likelihood to earn a postsecondary degree. And in a recent study, which is closely related to ours, Lafortune *et al.* (2018) use an event study framework and convincingly demonstrate that school finance reforms, by increasing spending in low-income school districts, caused an important increase in students' achievements.[1]

Most of these studies face severe difficulties in attempting to unravel a causal relationship between school spending and educational outcomes. Counterfactual outcomes are sensitive to the choice of the estimator and the identification strategy to address the endogeneity of school resources. Although previous studies have made a good deal of progress in dealing with the joint determination of educational inputs and outputs, modest estimated effects of school spending could be a consequence of unresolved endogeneity biases (see Jackson *et al.* , 2016).

Against this background, we explore whether spending on physical infrastructure affects student outcomes by focusing on test scores in Mathematics and Italian language using data on Italian state high schools and contribute to this debate in two ways. To handle endogeneity concerns, the majority of previous studies exploit bond elections, and compare school districts where bond referenda narrowly pass to those that narrowly fail (e.g., Cellini *et al.* , 2010; Martorell *et al.* , 2016). Yet, they suffer from relative scarcity of observations around the cutoff and it is unclear how their estimates generalize to an average school district distant from the cutoff, particularly when districts that approve bonds are wealthier (Goncalves, 2015). We employ a quasi-experimental design and make use of information on exogenous extra funds that a specific group of schools received in the aftermath of the 2012 Northern Italy earthquake that generates geographical differences in the damage and in the allocation of fundings. In fact, in May 2012, the seismic events in Northern Italy caused considerable damages to state buildings and prompted specific interventions for the mitigation of the seismic risk, even in locations not affected by the earthquake but close enough to its epicenter, i.e., where the earthquake originated. As a result, a large number of undamaged schools received large extra funds to modernize and improve the quality of their buildings as well as to mitigate their vulnerability to future — and potentially

[1]Interestingly, they also find no evidence of an effect of reforms on achievement gaps between high- and low-income students and between white students and those belonging to minority groups.

more intense — earthquakes. We compare the relative change of students' test score in the post-earthquake period (relative to the pre-earthquake period) between undamaged schools in the earthquake-affected area, that were awarded special funding, (i.e., the treatment group) and a control group of schools in neighboring municipalities that did not receive any extra-funds. The schools in the control group are in areas sufficiently far from the earthquake epicenter and are therefore both undamaged as well as unfunded.

The issue of school capital funding features prominently in the public debate and in many countries the lack of investment remains a pressing priority for state schools, where many governors believe that schools are not "fit for purpose" (Guardian, 27/01/2015).[2] Italy is a particularly interesting case, as school principals have long lamented that poorly maintained school facilities and a lack of funding to conduct essential repairs prevent schools from delivering their curriculum (Corriere della Sera, 18/07/2017). This squares with the theoretical arguments put forward by educational researchers, social psychologists and sociologists on the importance of the physical environment of schools and the condition of their facilities in explaining variation in students' learning across schools (Earthman, 2002; Mendell & Heath, 2004; Bakó-Biró *et al.* , 2012; Haverinen-Shaughnessy *et al.* , 2015). Yet, previous studies often explore very heterogeneous inputs of the educational production process that are also driven by different regional and institutional characteristics. Jones & Zimmer (2001) note that most of the literature focuses on school-specific local inputs, local school organization inputs (e.g., class size), local environmental characteristics and socioeconomic (family) characteristics but neglects capital inputs such as school infrastructure. Moreover, and perhaps more crucially, whereas there are only a handful of studies on the school infrastructure-students' learning relationship, they focus preponderantly on the US School System (Aaronson & Mazumder, 2011; Neilson & Zimmerman, 2014; Cellini *et al.* , 2010; Goncalves, 2015; Martorell *et al.* , 2016; Conlin & Thompson, 2017).[3]

In terms of the specific mechanism mapping school infrastructure onto students' learning, this literature has stressed the role of social norms, conformity and social signaling in the school environment (Branham, 2004). On the one hand, a safe and

[2]For example, in 2017, the Australian government aimed at bringing forward $200 million in capital investment to fast track state school infrastructure throughout Queensland (`https://goo.gl/GGe1Pf`). In 2015-16, the UK Department for Education spent GBP 4.5 billion in capital funding and the National Audit Office has predicted that it will take a further GBP 6.7 billion investment to bring all schools up to scratch (`https://goo.gl/SQzHDE`). In Germany, Martin Schulz, leader of the Social Democrats, vowed to pour billions into crumbling schools infrastructure in campaigning for 2017 September's election (see FT, 17/07/2017).

[3]Aaronson & Mazumder (2011) investigate the impact of the "Rosenwald initiative" in the US between 1914 and 1931 and find that substantial improvements to school quality and access in relatively deprived environments are followed by large productivity gains. Neilson & Zimmerman (2014) find that school construction programs led to sustained gains in reading scores for elementary and middle school students. Conlin & Thompson (2017) investigates the effect of a capital subsidy program for Ohio school districts and find that test scores improve once construction on the new and renovated buildings is completed. Yet, Cellini *et al.* (2010) and Martorell *et al.* (2016) find little evidence that school facility investments improves student achievement. Similarly, Goncalves (2015) explores the effect of a large-scale construction program in Ohio and finds no evidence of positive returns to students.

clean school environment provides important signals to students that the school is well managed, that teachers enforce discipline in the classroom and that antisocial behaviors are not tolerated. On the other hand, unhealthy and unsafe buildings, with e.g., broken windows, graffiti, nonfunctioning toilets, poor lighting, inoperative heating and cooling systems, leaking roofs, signal a lack of attention and respect for the students, who either put less efforts or distract colleagues and disrupt the learning environment, as they perceive lower costs and risks of detection. Furthermore, older buildings have usually worse air quality, poor lighting and are less likely to handle state-of-art education technologies (see e.g., Lemasters, 1997; Goncalves, 2015; Conlin & Thompson, 2017). This so-called "*broken windows theory*" (Wilson & Kelling, 1982) is based on the premise that the school environment "communicates" to students and that "good signals" correlate with a more efficient learning process. Students in well-maintained schools are therefore more likely to focus on academic challenges than those who are distracted or depressed by poorly maintained facilities. By the same token, physical conditions also affect teachers' feelings of effectiveness and sense of personal safety in the classrooms. Lawrence (2003) reviews a number of studies showing how the condition of the school facility affects the health and morale of staff. This interpretation identifies a potential pathway to explain the direction of educational outcome's change in response to infrastructure spending.

We find that being a recipient of funding increases students' achievement. As the amount of funds received by each school might be driven by potential unobservable characteristics, we implement an instrumental variables (IV) strategy. In particular, we use seismic hazard maps and exploit exogenous values of peak ground acceleration (henceforth PGA), which explain substantial variation in the amount of funds received. Our 2SLS estimates are positively signed and statistically significant and predict that transferring an extra 100 euro per student to a school increases, on average, test score by about 4.6% in the Mathematics test and 1.2% in the Italian test. This corresponds to about 10% and 2% increase relative to the sample mean in Mathematics and Italian, respectively. As such, these effects are not only statistically significant but also meaningful and support a number of psychological studies that document how a renewed, well-maintained school may explain a more efficient learning process in mathematics, relative to reading.[4]

Furthermore, and perhaps more importantly, we find that this effect is more pronounced among the students more in need of academic support. Specifically, our results suggest that allocating 100 euro more per student increases, on average, the test score of the students belonging to the 5th percentile of the school distribution (i.e., the low-achieving students) by 12.6% in Mathematics and by 2.3% in the Italian test. The effect is substantially smaller for the high-achieving students, those in the 95th percentile of the school test score distribution: their test scores in Mathematics and Italian language increase only by 1.5% and 1%, respectively, when an additional 100 euros are spent in their schools.

[4]Using data on Italian high school students, Primi *et al.* (2014) document how students' anxiety in mathematics is substantially higher than in reading. Other studies have pointed to the role of a more orderly learning environment in explaining more efficient learning processes in mathematics, relative to reading (Jehng *et al.* , 1993).

We provide a number of extensions to demonstrate the robustness of our main conclusions to changes in model assumptions. In particular, we check the robustness of our empirical results in six directions. First, we show that our estimates are not driven by the migration, reassignment and change in composition of students. Second, we explore variations across municipalities and show that e.g., our results are not sensitive to the exclusion of schools in particular locations, such as large municipalities or small villages or that other characteristics at the municipality-level, such as population density, height, and number of schools, do not drive our results. Third, we document that our findings are not driven by specific schools that received an unusual high amount of funds per students, we show that funding are not picking up non-linearities in school quality and we show that results are robust to the exclusion of relevant covariates. Fourth, show that our results are robust to alternative definitions of our control group. Fifth, we demonstrate that cheating is not a confounding mechanism and, finally, we take into account the renovation of private buildings, and show that this does not change our results. Taken together, our evidence suggests that improving the quality of school buildings has a positive effect on students' achievements. Moreover, students more in need of support are those that benefit the most from improved physical infrastructure.

2 Data

2.1 The 2012 Northern Italy earthquake and school funding

Deciphering the impact of school resources on achievements is complicated by the fact that students' performance and the selection of funded schools, or the spending levels, are potentially simultaneously determined. We address this issue by using data on school funding provided after a natural disaster. On May 20, 2012 an earthquake of magnitude 6.1, followed by a second one on May 29, hit a territory of 3.5 thousands squared kilometers in the Northern part of Emilia-Romagna, an Italian region near the borders with Veneto and Lombardia. Before the 2012 seismic events, this area was generally not considered at risk of seismic activities.[5]

In the aftermath of the earthquake, the Italian government made available more than 24.4 millions of euros to several state buildings in the affected municipalities, including 276 high schools, with the aim of reconstructing damaged buildings, renewing and maintaining all school buildings as well as keeping undamaged buildings safe from future seismic threats. In fact, this extra funding was given to both damaged schools as well as to schools considered at risk for earthquakes in the future. We use several legislative acts to assemble data on the amount of extra funding to state schools in the region.[6] As the earthquake could have had a direct effect on the learning environment

[5]With the exception of the seismic sequence of Ferrara in 1570, Argenta in 1624 and Bologna in 1929 (Vannoli *et al.* , 2015), few other small intensity earthquakes had had an impact on its inhabitants' collective memory. As a result, the perception of a seismic risk was comparably very small relative to the rest of Italy. In fact, PGA values in this area are, on average, only 20% of those characterizing the nearby Apennine mountain chain. See `http://zonesismiche.mi.ingv.it/`

[6]Starting from June 2012, the deputy commissioner enacted a series of legislative acts with specific guidelines for securing school buildings as well as the criteria for assigning available funds. See

and on students' performances, we use information on the volume of damaged buildings in each municipality, estimated by the INGV (National Institute of Geophysics and Volcanology) in the aftermath of the seism using a macroseismic survey. For our empirical analysis, we only select municipalities where the level of damage of their buildings was assessed by the INGV as "negligible" (D1) or lower.[7]

In more details, we collect data for a total of 236 municipalities, as shown on the map in Figure 1. Out of 236, 69 are discarded as they had a level of damage greater than D1 (see grey shaded areas in Figure 1). Out of the 167 remaining municipalities, only 43 have at least one high school, for a total of 173 schools (white dots in Figure 1). The treated schools are those located in treated municipalities (shaded areas in Figure 1) and make up a good portion of the total number of schools, 39% (68). Although these schools reported no damage, they received about 3.6 millions of euros to improve the quality of their buildings. Our control group is made up of 105 schools that received no extra-funding and were not affected by the earthquake, but they are located in neighboring municipalities, proximate to the treated areas. We consider the second-order contiguity definition for neighbors: two municipalities are neighbors if they directly share a border or if they have a common neighbor with which they share a border (see dashed areas in Figure 1).[8]

The map also contains information on the peak ground acceleration (PGA) values, gathered from the INGV database. The color bar shows the gradient of PGA for each municipality, from low to high. PGA is the maximum ground acceleration during earthquakes and it is commonly used as an index for seismic hazard intensity, i.e., the higher the PGA the larger will be the intensity of a possible earthquake in a specific geographic area. Therefore, areas with higher PGA have a higher probability to suffer a damage on physical infrastructures and buildings whenever an earthquake occurs. In our sample, the PGA varies between 0.09 and 0.21, with an average intensity of 0.16. As we explain in Section B, in Appendix, where we provide information on the reconstruction, the damage assessment and the eligibility to funding, the amount of extra funding per student in the treated areas was driven by the necessity to safeguard school buildings from future seismic threats and minimize potential damages to school infrastructure, on the basis of the reported damage assessment;[9] hence, this is a function, among other things, of PGA levels.

—— Figure 1 here ——

Summary statistics, reported in Table 1, show that these schools received on average an extra 389 euros per student, about 200% of the annual amount in capita expenditure in Italy in 2013 (OECD, 2016). According to the OECD report, the average total spending per student in Italy in 2013 was 9,174 euros; but only 2% (i.e., 184

https://goo.gl/Lqm8Uk.

[7]In Section C of the Appendix we provide a through description of the macro-seismic survey and the classification of the damages.

[8]In Section 6 we propose several other definitions of the control group and check whether our results are sensitive to it.

[9]See the first decrees enacted by the deputy commissioner, i.e., ODC #2 (16 June 2012) and the ODC #4 (3 July 2012). For a more extensive explanation, see Appendix B.3

euros) was devoted to school capital. As such, capital spending tripled in the treated schools.[10]

—— Table 1 here ——

2.2 Test scores and control variables

Information on test scores is taken from the Italian National Institute for the Evaluation of the Educational System (INVALSI). Since the academic year 2010/2011, tenth graders in Italian high schools take standardized assessments on the same day (May 9). In these tests, questions capture the same dimensions over time and across schools, making possible to compare the relative performance of schools across academic years. The participation of all state schools is compulsory and the assessment encompasses only Mathematics and Italian language skills in the tenth grade.[11] Our dependent variable is the percentage of correct answers for each high school. From the same database, we also take information on cohort size (i.e., the number of 10th grade students in each school) and on the shares of male and native students in each school. We refer the interest reader to the Invalsi online user's guide for a thorough description of the test and a more comprehensive overview than we can possibly give here.[12]

Table A2 provides balance tables to check for systematic differences across treated and control schools. In particular, Panel D shows that there are virtually no differences in average observable characteristics (percentage of males, natives and cohort size) and we have a very good covariate balance, indicated by the *p-values* across t tests on the difference of means. Furthermore, Table A6 shows that treatment did not have any significant effect on these school-specific characteristics. We return to this issue in the following sections to show that the earthquake did not displaced students in areas hit by the event in significant ways. For each school, we also compute average

[10] This amount is very small if one compares it with the funding transferred to schools in other European countries of the same size: capital expenditure in Germany, for example, was about 1,300 euros, and about 1,200 euros in France in the same year.

[11] Only the tenth grade is assessed in the secondary school. Our sample is an unbalanced panel of 692 observations (i.e., school $\times$ year) for the Mathematics test and 696 for the Italian language test. In Table A1 we illustrate the distribution of schools per year in our sample. The distribution shows a general slow increase in their number over time, although no discontinuity at any date. Regarding the differences in the number of schools between the Italian language tests sample and the Mathematics test sample, INVALSI (2016) says that no results are provided when less than 50% of eligible students take the test. In principle, students could, in fact, after having completed the Italian language test, decide not to take the Mathematics test and leave the room for serious reasons.

[12] Available at `https://invalsi-dati.cineca.it/2017/docs/Tutorial_Invalsi/guida_invalsi.html`. See also Battistin & Meroni (2016) and Angrist *et al.* (2017) for additional discussions. Battistin & Meroni (2016) in particular offer a novel study on instruction time and students' performance in Italy, using the same data. Finally, Brunello & Checchi (2005) offer a comprehensive study of school quality and educational attainment in Italy using historical data. Note that, to the best of our knowledge, there are no other available school-level characteristics that can be included. Yet, on the one hand, note that we use school and time fixed effects, which absorbs school-specific or slow-moving characteristics; on the other hand, in Section 6 we include a battery of robustness checks where we exploit important differences across municipalities to increase confidence in our results

test scores for low-achieving and high-achieving students, the fraction of students in the 5th/10th percentile of the score distribution and in the 90th/95th percentile, respectively. We assemble school-level annual data over six academic years, from 2010/2011 to 2015/2016.

3 Empirical strategy

3.1 Extensive margin

To get a handle on the direction of causation in the infrastructure spending – students' achievement relationship, we begin by exploiting the quasi-experimental setting induced by the 2012 Northern Italy earthquake. To this aim we use two sources of variation: i) the *timing* of the earthquake and the subsequent intervention (see Figure 2); and ii) the allocation of additional funding across *space* (see map in Figure 1).

—— Figure 2 here ——

Taken together, they allow us to measure the impact of receiving additional resources on test scores by comparing the difference in test scores in years following the allocation of funding to years when there was no funding available, in municipalities that are recipient of funding relative to municipalities that are not eligible to receive extra-funds. We start with a simple empirical research design, a difference-in-difference estimation strategy, which takes the following form:

$$log\, y_{it} = \alpha_1(D_i \times P_{t-1}) + X'_{it}\alpha_2 + \mu_i + \eta_p \times P_t + \theta Trend + \varepsilon_{it}, \tag{1}$$

where the outcome variable y_{it} denotes the average test score in either Mathematics or Italian language in school i in year t;[13] D_i is a dummy that takes value one if the school has received extra-funds; P_t is a dummy that takes value one if the observation is in the post-treatment period (i.e., post 2012); we lag the treatment by one year to allow time for the funding to be invested. X_{it} is a vector of school covariates which includes the cohort size, the share of males as well as the share of native students in each school; μ_i is the school fixed effect, which absorbs school-specific constant (or slow-moving) features; as provinces could have implemented local interventions after the earthquake, we interact province fixed effect η_p with P_t to control for province-specific policies after 2012;[14] θ is the coefficient of a school-specific time trend variable and ε_{it} is an error or disturbance term. $D_i \times P_{t-1}$ is the interaction between the treatment schools D_i and P_{t-1}, the dummy variable equal to one in the post-treatment period; therefore, α_1 is our parameter of interest, the difference-in-difference estimates of the impact of receiving funding on students' achievement. For small values of the coefficient, $100\times\alpha_1$ can be interpreted as the percentage increase in the test score when schools receive extra funding.

[13] We use a logarithmic transformation to scale down the variance and lower the impact of potential outliers as well as to facilitate the interpretation of our estimates.

[14] A province is an administrative division between a municipality and a region, and constitute the third NUTS administrative level. Provinces have, among other functions, the local planning and the coordination of schools activities. In our sample, we have a total of 10 provinces.

3.2 Intensive margin

Our main analysis — the one we mostly rely on to draw conclusions — exploits variation in the amount of funds allocated to estimate the elasticity of test scores with respect to spending per capita. Yet, as noted above, idiosyncratic changes in school spending are likely endogenous as the amount of funding allocated to each school can be correlated with unobservable school-level characteristics. To quantify this relation, we estimate 2SLS models where we instrument for school spending with the values of peak ground acceleration (PGA), the maximum ground acceleration during the earthquakes. Recall that funding was allocated to schools to reduce the vulnerability of their buildings to earthquakes and more funding per capita was granted to schools in municipalities with higher earthquake risks. The proposed instrument is thus expected to be strongly correlated with school funding.

We capture this process by modeling, in the first stage, funding per capita as a function of PGA values, as follows:

$$FUND_{it-1} = \pi_1(PGA_i \times P_{t-1}) + X'_{it}\pi_2 + \mu_i + \eta_p * P_t + \theta Trend + \varepsilon_{it}, \tag{2}$$

where $FUND_{it}$ is the amount of funding per student received by school i after 2012.

In the second stage of the IV estimation we use $\widehat{FUND}_{it}$, the exogenous variation in extra-funds per student, predicted by Equation (2), to explain changes in students' test scores after funding was allocated to their schools:

$$log\, y_{it} = \beta_1 \widehat{FUND}_{it-1} + X'_{it}\beta_2 + \mu_i + \eta_p * P_t + \theta Trend + \varepsilon_{it}, \tag{3}$$

where the outcome variable y_{it}, the vector of controls at the school level, the trend variables and the fixed effects are the same as in Equation (1). Given the log-linearity of the model, the interpretation of β_1 is that of a proportional change in the test score given a unit change in funding, holding all else constant.

Our exclusion restriction holds if variation in the PGA affects students' test score only through the funding enacted after the 2012 earthquake. It could be, however, that differences in PGA levels underline economic differences across municipalities (such as local income, environment, or level of urbanization) that, in turn, may affect students' outcomes. We argue that this is unlikely. Our analysis holds these effects fixed by solely exploiting within-school variation across years. Indeed, we compare same schools, before and after the earthquake, and test whether variation in funds received (explained by differences in PGA) moves students' test score, keeping fixed any municipal characteristics. Nonetheless, it could be that these underlined differences (correlates of PGA) may in turn explain variation in the time-variant school characteristics. To rule out these potential concerns we proceed in two ways. First, we show that schools in areas below and above PGA median values have very similar observable features. In Table 2, we regress each control (% males, % native students, and the cohort size) on a dummy variable equals to 1 if located in an area with a PGA level above the median. We find no statistical difference regardless of the characteristics employed in the left hand side. Second, in Table 3 we show that in the post-earthquake period (relative to the pre-period) schools located in an area with a higher PGA level do not record statistically significant changes in the percentage of males (column a), percentage of native students (column b), and in the cohort size (column c).

—— Table 2 and 3 here ——

4 Results

In Table 4 we present the relation between funding and student scores in Mathematics, whereas in Table 5 we focus on Italian language. We start with column (a), where we present our difference-in-difference estimates (i.e., the extensive margin). Here, we uncover a positive effect of receiving extra funding on test scores in Mathematics, which will increase by 10% if a school is a recipient of funding. However, the relation is not significantly different from zero for Italian language.[15] Yet, recall that the assumption needed for our identification strategy to work is that, in the absence of the extra-funding, scores in all classes would have presented parallel trends. To assess whether this assumption holds and thus to evaluate the validity of the design, in Figure 3 we check whether the coefficients of interest in Equation 1 are statistically different from zero in the pre-treatment period, relative to the baseline category year 2011.[16] Two clear patterns emerge. First, the plot suggests that the parallel trend assumption holds when we look at the Mathematics test scores, as the coefficients in the pre-treatment period are never statistically different from zero. The treatment i.e., being a recipient of extra-funding, induces an immediate deviation from the common trend for Mathematics in the academic year 2013/14. This is encouraging as a sudden increase in test scores after the treatment makes us more confident that this change is indeed the effect of extra-funding rather the result of unobservables. The effect is quite short-lived and, though positive, its magnitude decreases after one year. Second, when we focus on Italian language, the treatment seems to have an effect before it actually occurs, thus the key identifying assumption that test scores in Italian language would be the same in both groups in the absence of treatment is violated. As such, results in column (a) of Table 3 should be treated with caution.[17]

—— Figure 3 here ——

Turning to the elasticity of student outcomes with respect to the amount of resources devoted to school infrastructure (i.e., the intensive margin), we move to our

[15]Note that all models include the share of males, of native students and the total number of students in the tenth grade in each school as well as school fixed effects, time trends and interactions between province fixed effects and post-treatment period dummy. Using linear trends, quadratic trends, cubic polynomial in time (i.e., t, t^2, and t^3) or year dummies produce similar results.

[16]Formally, Figure 3 plots the estimated coefficients α_{1t} of the following flexible estimation of Equation 1:

$$log\, y_{it} = \sum_{t=2012}^{2016} \alpha_{1t}(D_i \times \mu_t) + X'_{it}\alpha_2 + \mu_i + \eta_p \times P_t + \theta Trend + \varepsilon_{it},$$

where μ_t is a dummy equals to 1 for the year t.

[17]Recall however that we also rely on an instrumental variable strategy, where we use an exogenous source of variation in funding, which is immune from this issue.

main research design, the IV strategy. In Tables 4 and 5, column (b), we show a naive OLS estimation, which reveals a positive correlation between funding per pupil and test scores. If for purely illustrative purposes one interprets the OLS estimates as causal, then, according to the estimates, a one-unit increase in school infrastructure spending per student (that is, 100 euros) is associated with an estimated increase in test scores in Mathematics of 0.3%, holding all else constant. The relation is insignificant at conventional levels when we replace test scores in Mathematics with those in Italian language (column (b), Table 5).

Yet, recall that in column (b) our main coefficients of interest are most certainly contaminated by endogeneity from uncontrolled confounding variables. Therefore in column (c) we turn to the estimated coefficient of school funding in the second stage of our 2SLS. We use the PGA, an index of seismic hazard, as exogenous instrument. As we can see, the coefficients are now substantially larger than those of the naive regressions in column (b) and they are all statistically different from zero. Distributing an extra 100 euros per pupil to schools will produce an estimated test score gain of 4.6% in Mathematics and 1.2% in Italian language.[18]

To better appreciate the magnitude of our 2SLS estimation, it is worth noting that our $\hat{\beta}_1$ captures a local average treatment effect (LATE), which is not informative about the effect of extra-funds on *always-takers* — that is, on schools that have received funds even if they are located in low risk areas. This has important implications for our 2SLS estimates that, unlike the OLS ones, are likely to be upward biased. For example, renovation works, after the earthquake, to secure school buildings in areas at relatively high seismic risk (i.e., the *compliers*) were likely to provide a relatively stronger signal to students — by e.g., lowering students' anxiety — compared to interventions in low-risk areas (i.e., the *always-takers*).

Finally, in columns (d) and (e) we show the first stage estimates and the reduced form, respectively. As expected, we find that an increase in the PGA level has a sizable impact on students' scores. At the same time, the first stage reveals that the PGA level leads to a higher amount of infrastructure funding received by the school, as one would expect. We report the Kleibergen-Paap F-Statistic, which is similar to the conventional F-statistic, but takes into account the clustering of the standard errors. The values are all above conventional levels characterizing weak instruments.

—— Tables 4 and 5 here ——

There are two important concerns that can potentially undermine our identification strategy. First, it could be that the first stage coefficient, $\hat{\pi}_1$ in Equation 2, is by a large extent explained by the discontinuity between unfunded and funded schools and not by a genuine variation in the amount of funds received. To mitigate this concern we reestimate the difference-in-differences model in the Appendix, Section D, by solely examining funded schools. Our results do not change substantially. Remarkably, the first stage estimation, reported in column (c) of Table D1, is three folds larger than what we estimate when employing the full sample (i.e., Table 4).[19]

[18]These results are not driven by the upper tail of funds and are robust to the exclusion of the schools that received more than 2,000 or 5,000 euros per student. See Section 6.

[19]Also the Kleibergen-Paap F Statistics is above the relevant threshold.

Second, the amount of funding allocated to the schools can potentially depend on the efforts that mayors exerted when the allocation of funds was determined. We discuss this potential issue in Section E of the Appendix. There, we document that mayors are not statistically different along relevant observable characteristics, that is gender, education, age, number of years in office, affiliation to apolitical coalitions or left wing coalitions, and a dummy for re-election after the mandate. This mitigates concerns about the possibility that specific "political machine" mechanisms are driving our results. At the same time, in Section E, we also complement our previous IV analyses using exogenous variation in the PGA levels to instrument the effect of receiving funding on educational outcomes (in lieu of the sheer amount of funding). Our results are robust to this alternative specification.

5 Heterogenous effects

Our previous models show that more capital spending can lead to high class achievement, but our estimates might conceal a degree of heterogeneity in students' responses to available funding. We could expect that students at various points of the test score spectrum do not display a uniform response to increasing levels of funding. Are low-achieving students more or less likely to benefit from extra-funding?

In Table 6 we represent the relation between funding and student scores in Mathematics, whereas in Table 7 we focus on Italian language. In column (a) of each table we replicate the models presented in Tables 4 and 5, which use the average score for all students as a dependent variable. In columns (b) and (c) the dependent variables are the test scores for students in the 5th and 10th percentile of the score distribution (i.e., low-achieving students), and in columns (d) and (e) the test scores for students in the 90th and 95th percentile (i.e. high-achieving students). For each table, Panel A presents the difference-in-difference estimates, panel B the naive OLS, panel C the IV estimations and Panel D the corresponding reduced-form.[20]

Table 6 explores various estimation techniques and specifications and consistently find that the marginal return to investment in school infrastructure is always greater the lower the score of the students. The estimated magnitudes of the relationship between funding and students' achievement in Mathematics are statistically significant and economically meaningful. On the one hand, distributing an extra 100 euros per pupil to schools will produce an estimated test score gain in Mathematics among low-achieving students in the range of 11% to almost 13% (see Panel C, columns b and c). On the other hand, a school that is recipient of extra funding will see an average increase in Mathematics test scores of high-achieving students between 1.5% and 1.9% (see Panel C, columns d and e).

When we turn to test scores in Italian language (see Table 7), we obtain similar results although of smaller magnitude. These results also confirm that students with high class percentile ranks (high-achievers within classes) benefit less from an increase in funding than their lower-achieving classmates. Yet, this last round of results are

[20]Note that we do not report in Tables 6 and 7 the first stage estimations of the effect of the PGA values on the extra-funds per students as they are the same as the ones reported in column (d) of Tables 4 and 5.

only statistically significant for the IV estimation (second stage and reduced from), which is not surprising as they mirror results in Table 5 (which are also reported in column (a) of Table 7).

—— Tables 6 and 7 here ——

To dig deeper into the relationship between school funding and students' standardized test scores, we divide the sample by quantiles of baseline (pre-earthquake) test scores and run the specification in Equation (1). Figure 4 shows the relation between the estimated coefficient α_1 and the full set of quantiles of the distribution of the test scores. Note that while Figure 4a presents the test score in Mathematics, Figure 4b shows the test score in the Italian language test. As the two figures clearly reveal, gains are larger among students initially in the lowest quartiles of the test score distribution. Whereas in Italian language the pattern is less clear-cut, in Mathematics the estimated effect decreases monotonically as we move from the 10th to the 90th percentile of the standardized test score distribution. Results are overall similar when we look at the relation between the estimated coefficient β_1 in Equation (3) and the quantiles of the distribution of test scores in Figure 5; the effect is again substantially larger for low-achieving students. The effect of school funding on students' achievement is overall quantitatively large, statistically significant and robust, in particular in Mathematics and for low-achieving students.

—— Figures 4 and 5 here ——

6 Robustness checks

Overall, our results thus far show that an increase in capital spending improves the performance of high school students. Our findings are subject to several potential alternative mechanisms and concerns. In this section, we present several robustness checks grouped into six macro categories. Due to the length of this section we report regression tables and figures in the online Appendix and only for the test scores in Mathematics.

6.1 Displacement, commuting and composition of students

We explore a number of issue related to the composition and potential displacement of the students. First, we ask whether the earthquake has displaced students in some areas hit by the event and forced them to switch to schools located in safer areas (see e.g., Sacerdote, 2012; Tincani, 2017). Anecdotal evidence and qualitative background material suggest that this is unlikely as, immediately after the earthquake, the government hastily provided temporary building to students before the new academic year started, particularly in areas near to the epicenter, while waiting for temporary structures to be completed.[21] Yet, it could still be the case that parents of children

[21] The first decree that the deputy commissioner enacted was indeed on the organization of temporary schools around the epicenter. See decree #1 (20 July 2012).

who previously attended schools in damaged areas managed to get their children assigned to schools in undamaged areas even in absence of tangible risks and despite the provision of temporary buildings. This would be especially problematic for our study if these parents were also those who cared relatively more about the education of their offspring and if the students who moved were also more motivated than the average. If this was the case, then, we should find a positive and significant effect of the treatment on the cohort size in the schools in the treated group. Tables A3 shows that this is not the case by presenting a model similar to the one in Equation (1) but with the cohort size on the left-hand side. The difference between column (a) and (b) is that the former does not include time-varying controls (i.e., share of males and native over the total number of students), whereas the latter does include these additional controls. In both cases the difference-in-difference coefficient is not statistically significant. In addition, in Figure A1 we plot the flexible coefficient over time (relative to 2011), and we further show that there is no difference in school population between treated and control regions both before and after the treatment takes place.

Second, we note that students' commuting in high schools is quite common, particularly from rural areas. A potential threat to our analysis is that a share of students in our sample of schools come from the damaged area and may have experienced a negative direct effect of the earthquake at home. Building on the prediction of gravity models for commuting flows (e.g., Simini *et al.* , 2012), in this section we test whether our results are robust to the exclusion of the municipalities closest and best connected to rural areas. In Table A4 we exclude schools in municipalities that share one border with the damaged zone. Column (c) replicates the 2SLS estimation, while column (d) replicates the difference-in-difference estimation using the restricted sample. In Table A5 we replicate our estimations by excluding schools in the most important towns in our sample (i.e. the head of the Provinces): column (b) excludes schools from Bologna, column (c) from Ferrara, column (d) from Ravenna, column (e) from Parma, column (f) from Modena, column (g) from Reggio Emilia. Column (h) excludes all the schools located in the provincial capitals. Overall, our results are unchanged and students' commuting from rural, affected areas is unlikely to affect our results.

Third, it might be possible that student composition changed while cohort size remained nearly the same. To rule out this alternative confounding mechanism, we first replicate the difference-in-difference estimation but use the share of males or the share of natives as the dependent variable. Table A6 shows that there is no difference in school population in terms of student mobility as well as composition. We then check whether sorting effects may have occurred within the same municipality across school with substantially different state of maintenance of their buildings. In column 2 of Table A7, we restrict our main analysis (reported in column 1, DiD in Panel A and 2SLS in Panel B, for easiness of comparison) to schools located in municipalities where no more than one school is located. In columns 3, we drop municipalities where no more than one typology of school present (i.e., liceum, technical studies or professional), as sorting may have occurred among schools of the same type. The results confirm that sorting across schools in the same municipalities is unlikely to drive our results.

6.2 Variations across municipalities

Our sample includes a variety of small, medium and large municipalities, and results could be heterogenous across them. In Table A8 we thus group together municipalities according to their size and probe the robustness of our results to the omission of each group. Column (a) shows the baseline model; column (b) uses weighted least squares (WLS) estimations with weights given by the population of the municipality where the school is located; column (c) excludes the first decile; column (d) excludes the second decile; column (e) excludes the first and the second top deciles; and column (f) excludes the first top decile. Panel A of Table 9 displays that our baseline difference-in-difference estimation for test scores in Mathematics (column (a) of Table 4) is not sensitive to variations in the municipalities' population. Similarly, Panel B shows the robustness of our two-stages least squares estimation (column (c) of Table 4). In both cases, the coefficients are always statistically significant across the columns and the effect seems to increase slightly when we exclude the biggest municipality from the sample.

Recall that in our analysis we control for a baseline set of school characteristics, in particular the fraction of males, of native students, the number of students in the tenth cohort as well as linear trend and province dummies interacted with the treatment dummy. Nonetheless, alternative mechanisms working at the municipal level could partially drive our results. For example, in the aftermath of the earthquake smaller communities could have mobilized more vigorously, which, in turn, could have boosted the motivations of both teachers and students. Our estimates could therefore pick up this mechanism rather than the effect of school funding, per se. In columns (b) and (c) of Tables A9 and A10 we show that there are no differences in test scores between schools in small and large municipalities, and between schools in more and less densely populated areas, relative to the pre-earthquake period. Similarly, municipalities in the Po Valley could have been more effective in recovering than those in the mountains. Yet, we find no differences in test scores between schools in mountainous regions and schools in the plains in the post-treatment period (see column (d) of Tables A9 and A10). Additionally, the local and national administration could have devoted relatively more attention and time to students in municipalities with fewer schools after the earthquake than to students in municipalities with a higher number of schools. In column (e) of Tables A9 and A10 we show that our mechanism is robust to this alternative explanation. Finally, in column (f) we show that our estimations are not sensitive to the simultaneous inclusion of all these alternative mechanisms.

6.3 High vs small funding, non-linearities and relevant covariates

Here, we show that our finding are not driven by schools that received unusual amount of funds per students, we test whether funding might be picking up non-linearities in school quality and we exclude relevant covariates. In Table A11, columns (b) and (c), we exclude schools that received limited resources from the central government, less than 100 euro and less than 200 euro, respectively. Both the difference-in-difference estimation and the 2SLS estimation are not sensitive to their exclusion for both types of test. In a similar vein, in columns (d) and (e) we exclude schools that received a disproportionately large amount of resources from the state. Recall that the average amount transferred is 389 euro per student. Therefore, in column (d) we exclude schools that received more than 2,000 euro, while in column (e) we drop those that received more than 5,000 euro. As we can see, there are virtually no changes in the size and statistical significance of our coefficients. We also compute the 5th, 10th, 90th, and 95th percentile of the distribution of schools according to the pre-treatment test score and check whether excluding schools at the top or bottom of the distribution affects our results. The evidence, presented in Table A12, does not support this alternative explanation and the effect of funding on mathematics test score is similar across specifications. Finally, in Tables A13, we ask whether our main results are sensitive to the inclusion/exclusion of school controls and the interaction between year fixed effects and the province dummy. As we can see, neither the province-year fixed effects nor the battery of control variables seem to significantly affect our estimates. In fact, the magnitude of the coefficients is virtually the same or larger (compared to the baseline presented in Tables 4).

6.4 Alternative definition of the control group

Recall that the schools in the control group are located in municipalities close enough to the treated areas, and we use the second-order contiguity definition for neighbors, i.e., the municipalities in the control group either share borders with the treated areas or there is no more than one municipality between them and the treated areas (neighbors of neighbors). In other words, the neighborhood is defined in relation to the two closest units. Therefore, we check the robustness of our estimates to the use of alternative orders of contiguity using Equations 1 and 3. In column (b) of Table A14 we use a more restricted definition of neighborhood, and we only take the schools located in municipalities that share borders with the treated areas (first-order contiguity). In column (c), we relax the definition of neighborhood and use for our control group municipalities that are at maximum third-order contiguous. Remarkably, the substantive impact of receiving additional funding, or the elasticity of 100 euro, on students' performances in Mathematics is virtually the same across columns and it does not depend on the choice of the control group.

6.5 Cheating

Cheating can also be a confounding mechanism if, after receiving the funding, teachers may want to show that such resources effectively map into better educational achievements. We test this by using data on the cheating score, provided by INVALSI, that ranges theoretically between 0, that indicating no cheating in the classroom, and 1, when the entire test in the class is falsified (See, INVALSI, National Report, 2016).[22] These data are available at the class-level, and we transform them at the school-level by taking the average. Cheating is not a frequent phenomenon in our sample—the

[22]See also Bertoni *et al.* (2013) on how external monitoring in the class on the test day affects cheating using similar data.

score is 0.026 on average but it is higher than 0.10 in the highest fifth percentile. In Table A15 we test whether our results pick up increasing cheating attitudes after 2012. We also add an interaction term between the cheating score and the post-2012 dummy, in both our difference-in-difference model (column b) and in our instrumental strategy (column d). As we can see from Table A15, our main results are unaffected. Taken together, they show that the increase in students' achievements in recipient schools (or in schools that have received more funding) is not explained by an increase in the propensity to cheat during the examination.

6.6 Renovation of private building

In the aftermath of the earthquake the government financed private buildings renovation. Although these renovations were not very frequent in the area under scrutiny, it is still possible that they have (negatively) affected the learning process of the students. In this section, we test the validity of our 2SLS estimation, i.e. whether, conditional on the amount of resources spent on private buildings, variation in PGA levels explain variation in students' outcomes, through the quantity of funding allocated to school buildings. To do so we gather additional available data on public resources that the government has allocated to private citizens to modernize and improve the quality of their buildings and thus mitigate their vulnerability to future earthquakes.[23] On average 50000 euro have been allocated to private buildings in a municipality, but relevant variation exists across them (the standard deviation is about 92705 euro). As we can see in Table A16, neither our difference-in-difference results (column b), nor our OLS and 2SLS estimates of the elasticity of funds on students' outcomes (column d and f) change when we control for the allocation of funding to private buildings.

7 Conclusion

In this article, we explore the impact of school infrastructure investments on students' achievement. We use data on school funding provided after a natural disaster, a magnitude 6.1 earthquake that hit the Northern part of Emilia Romagna region in May 2012, affecting an area of 3.5 thousands squared kilometers. We exploit plausibly exogenous variation in the allocation process (whether schools received funding or not) and in the amount of funding that each school received (which is a function of predetermined seismic risks). This allows us to consider the responsiveness of students' achievements to school infrastructure investments, along both the extensive and intensive margin. We implement two intertwined yet different identification strategies, a difference-in-difference approach and an instrumental variable strategy, so as to give our regression estimates a causal interpretation. Given the size of the treatment and the relatively low average capital spending in Italian state schools, our data allow us to

[23] This information is available at `https://openricostruzione.regione.emilia-romagna.it/open-data` for the municipalities of the Region Emilia Romagna. It covers 83% of the total number of schools.

ask the following question: how much high-school students would gain in terms of test scores if spending on school infrastructure tripled? Our empirical results suggest that tripling school infrastructure spending corresponds to important increases in students' test score, particularly in Mathematics and for low-achieving students. Our results are robust to a variety of model specifications and do not depend on specific decisions in the research design.

A set of facts, peculiar to the Italian school system, may help us reconciling our findings with recent contributions that specifically use US data. Few resources are spent in school capital in Italy, about 184 euros per student in 2013, which places Italy near the bottom of school infrastructure spending among OECD countries, including the US (OECD, 2016). Whereas the average condition of school infrastructure is quite poor (more than 39% of school buildings need urgent maintenance, see e.g., Antonini *et al.* , 2015) interventions on school facilities are likely to affect the health, safety and morale of students and teachers and in turn their ability to learn and teach. Thus, high class achievement is often thought to indicate better teaching or a more efficient distribution of students.

We hope that this research provides important insights into the role of physical capital spending in improving the learning environment of high schools. An important avenue for further research might emerge from our work, particularly with regard to the underlying transmission mechanism. In fact, we are largely agnostic about which specific channel explains how better infrastructures improves student outcomes. Education infrastructure of high quality means that students have adequate temperature, lighting, and functional furniture that are likely to improve the quality of their learning experience. Certainly, basic safety and health standards are necessary for effective learning environments. At the same time, improving the technical conditions of a school can increase the student satisfaction while the characteristics of the learning space can increase teachers' motivation and retention, and provide important signals to the quality and commitment of the institution. Identifying potential pathways is therefore important in light of offering potential policy prescriptions for investing in school infrastructure.

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Table 1: Summary Statistics

	mean	sd	min	max	count
Panel A – Treatment and IV					
Spending dummy	0.39	0.49	0.00	1.00	173
Funds per capita (× 100)	3.89	10.29	0.00	80.65	173
Seismic hazard (PGA)	0.16	0.03	0.09	0.21	173
Panel B – Mathematics					
log Score (mean)	3.80	0.33	2.79	4.46	692
log Score (p5)	3.09	0.62	0.00	4.30	692
log Score (p10)	3.28	0.50	0.00	4.32	692
log Score (p90)	4.15	0.28	3.11	4.59	692
log Score (p95)	4.22	0.26	3.11	4.59	692
Panel C – Italian Language					
log Score (mean)	4.10	0.27	1.59	4.50	696
log Score (p5)	3.57	0.60	0.00	4.39	696
log Score (p10)	3.73	0.47	0.00	4.44	696
log Score (p90)	4.34	0.20	1.59	4.58	696
log Score (p95)	4.38	0.18	1.59	4.59	696
Panel D – Controls					
% Male	0.56	0.26	0.00	1.00	692
% Native	0.82	0.14	0.21	1.00	692
Cohort Size	88.57	77.39	3.00	372.00	692

Notes: Funds per capita are expressed in 100 euros. Test scores in Panel A and B are transformed in logarithm. Cohort size is the number of tenth grader. The unit of observation is school in Panel A, and school × year in Panel B, C, and D.

Table 2: Means of school characteristics below and above PGA median value

	Below (a) mean	Above (b) mean	(c) Diff.	(d) Std. Error	(e) Obs.
% Male	0.55	0.54	-0.02	0.04	173
% Native	0.82	0.85	0.03	0.02	173
Cohort Size	81.17	89.92	8.75	11.34	173

Notes: Column (a) refers to the observations below the median PGA value, while column (b) the ones above. Column (c) represents the difference between column (a) and column (b) and column (d) the standard error of the estimate of the difference. PGA values measure the seismic hazard. Cohort size is the number of tenth graders. The unit of observation is school.

Table 3: Balance test

	Dependent variable is:		
	% Male	% Native	Cohort Size
	(a)	(b)	(c)
	OLS	OLS	OLS
Seismic hazard $\times post_{2012}$ (lag)	0.059	-0.101	-22.745
	(0.082)	(0.070)	(19.590)
Observations	692	692	692
R^2	0.015	0.064	0.071

School Fixed-effect models. Each column includes the school-level controls. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Secondary School, Mathematics

	(a)	(b)	(c)	(d)	(e)
	OLS	OLS	2SLS	first stage	reduced-form
Spending dummy $\times post_{2012}$ (lag)	0.099***				
	(0.024)				
Funding per capita (00) $\times post_{2012}$ (lag)		0.003***	0.046***		
		(0.001)	(0.011)		
Seismic hazard $\times post_{2012}$ (lag)				26.128***	1.204***
				(6.070)	(0.101)
Kleibergen-Paap F Statistics				18.522	
Observations	692	692	692	692	692
R^2	0.147	0.132	0.091	0.239	0.153

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Secondary School, Italian

	(a)	(b)	(c)	(d)	(e)
	OLS	OLS	2SLS	first stage	reduced-form
Spending dummy $\times post_{2012}$ (lag)	0.020				
	(0.022)				
Funding per capita (00) $\times post_{2012}$ (lag)		0.001	0.012^{**}		
		(0.001)	(0.005)		
Seismic hazard $\times post_{2012}$ (lag)				27.413^{***}	0.328^{***}
				(6.887)	(0.113)
Kleibergen-Paap F Statistics				15.841	
Observations	696	696	696	696	696
R^2	0.394	0.393	0.175	0.399	0.123

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Italian. Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Secondary School, Mathematics: funding and students scores

	(a) Score (mean)	(b) Score (p5)	(c) Score (p10)	(d) Score (p90)	(e) Score (p95)
Panel A OLS Estimation.					
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.329***	0.262***	0.043**	0.040*
	(0.024)	(0.094)	(0.063)	(0.021)	(0.021)
Panel B OLS Estimation.					
Funding per capita (00) $\times post_{2012}$ (lag)	0.003***	0.014**	0.010**	0.002**	0.003***
	(0.001)	(0.005)	(0.004)	(0.001)	(0.001)
Panel C IV Estimation – Second Stage.					
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.126***	0.107***	0.019***	0.015***
	(0.011)	(0.028)	(0.023)	(0.006)	(0.005)
Panel D Reduced Form Estimation.					
Seismic hazard $\times post_{2012}$ (lag)	1.204***	3.284***	2.803***	0.495***	0.381***
	(0.101)	(0.441)	(0.291)	(0.093)	(0.099)
Observations	692	692	692	692	692

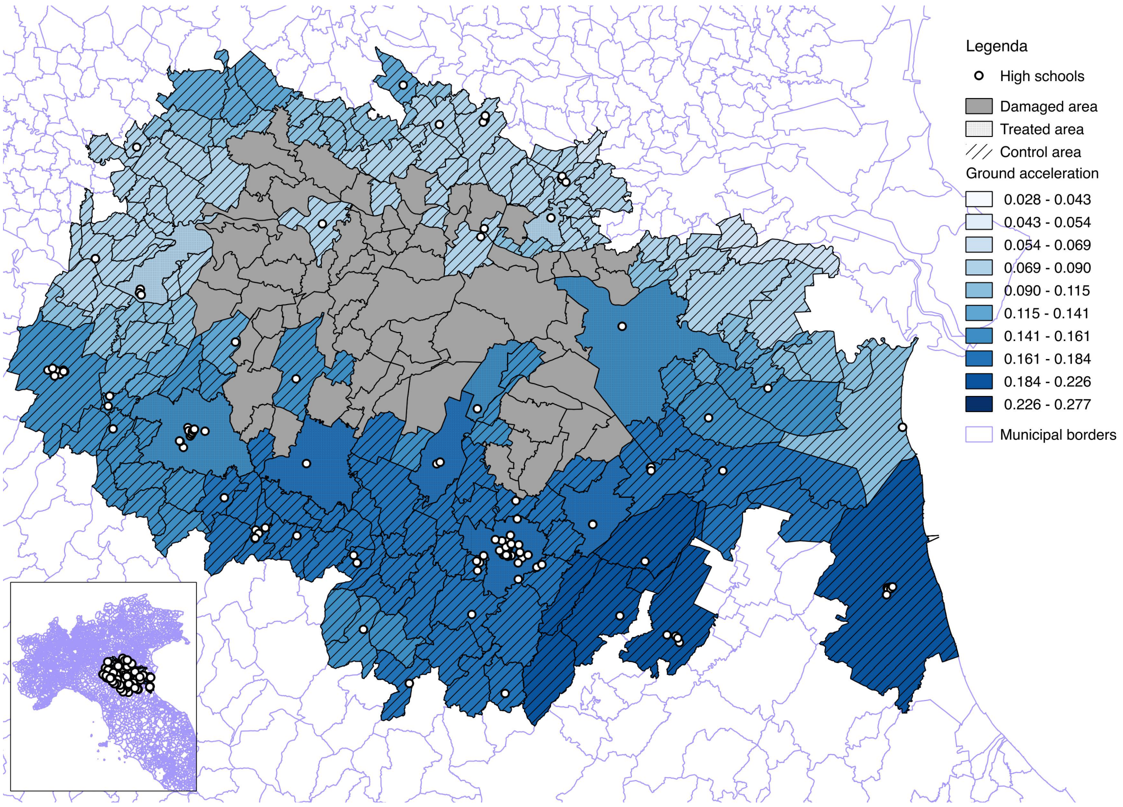
Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics in column (a), the logarithm of the 5th percentile of the test score in Mathematics in column (b), the logarithm of the 10th percentile of the test score in Mathematics in column (c), the logarithm of the 90th percentile of the test score in Mathematics in column (d), the logarithm of the 95th percentile of the test score in Mathematics in column (e). Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Secondary School, Italian: funding and students scores

	(a) Score (mean)	(b) Score (p5)	(c) Score (p10)	(d) Score (p90)	(e) Score (p95)
Panel A OLS Estimation.					
Spending dummy $\times post_{2012}$ (lag)	0.020	0.073	0.051	0.008	0.005
	(0.022)	(0.064)	(0.042)	(0.019)	(0.019)
Panel B OLS Estimation.					
Funding per capita (00) $\times post_{2012}$ (lag)	0.001	0.005	0.002	0.000	0.000
	(0.001)	(0.006)	(0.003)	(0.001)	(0.001)
Panel C IV Estimation – Second Stage.					
Funding per capita (00) $\times post_{2012}$ (lag)	0.012**	0.023*	0.017**	0.008*	0.010**
	(0.005)	(0.012)	(0.007)	(0.004)	(0.005)
Panel D Reduced Form Estimation.					
Seismic hazard $\times post_{2012}$ (lag)	0.328***	0.623*	0.462**	0.218**	0.267**
	(0.113)	(0.345)	(0.197)	(0.106)	(0.109)
Observations	696	696	696	696	696

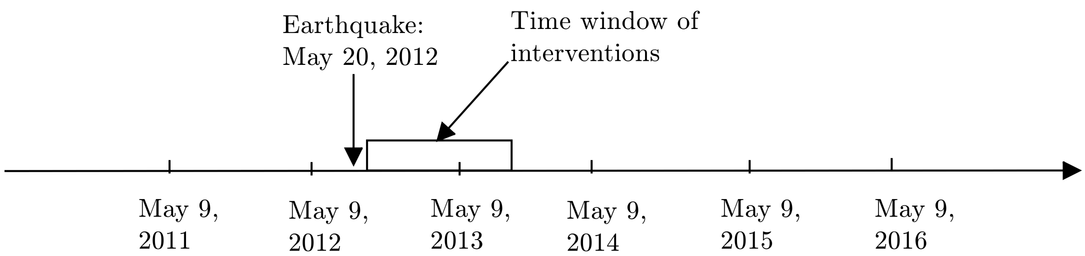
Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Italian in column (a), the logarithm of the 5th percentile of the test score in Italian in column (b), the logarithm of the 10th percentile of the test score in Italian in column (c), the logarithm of the 90th percentile of the test score in Italian in column (d), the logarithm of the 95th percentile of the test score in Italian in column (e). Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Treated and control areas



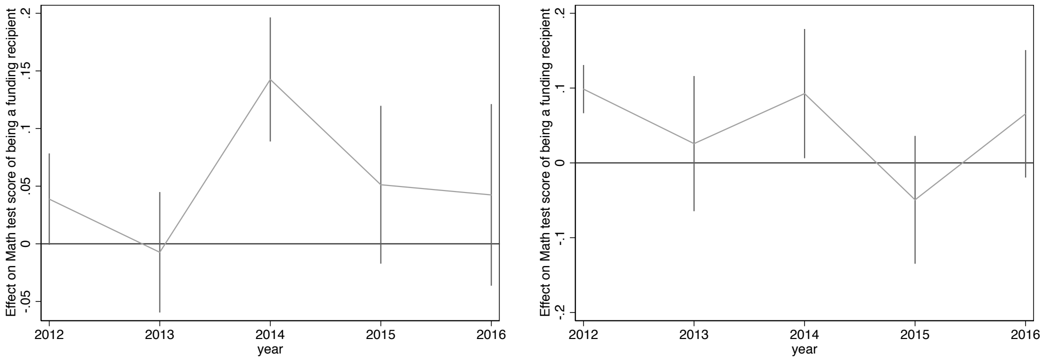
Notes: The figure illustrates the spatial distribution of the maximum ground acceleration during earthquakes (i.e., the PGA value) across municipalities. The color bar (in the top-right corner) shows the gradient of PGA for each municipality, from low (light blue) to high (dark blue). Each white dot indicates the geo-location of a state high school in the area. Schools that received extra-funds are located in shaded municipalities (i.e., our treated group). Schools that have not received extra-funds are located in neighboring municipalities (the dashed area), proximate to the treated areas up to a second order of contiguity. Municipalities that experienced considerable damages, namely a level of damage greater than D1, represent the 'damaged area' and are depicted in grey (dropped from our analysis).

Figure 2: Timeline



Notes: The figure shows the timeline of events of our quasi-natural experiment. Every 9th of May all the students in the tenth grade undertake the standardized test in Italian language and Mathematics. On May 20, 2012, the earthquake hit the area and extra-funds were given immediately after. Interventions were conducted with the beginning of academic year 2012/13.

Figure 3: Flexible estimates of the relationship between test scores and funding

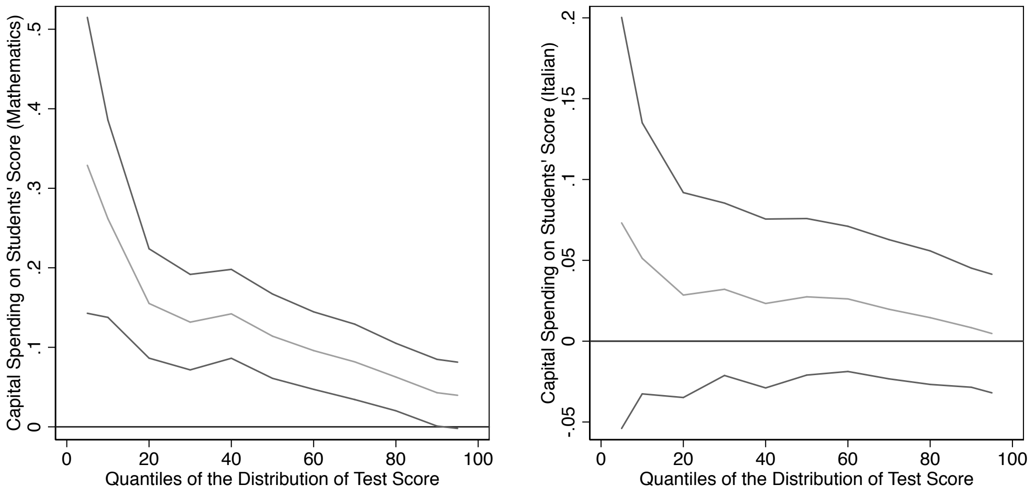


Mathematics

Italian

Notes: The two Panels illustrate the effect of being a recipient of extra-funding in the test score observed in year t relative to the baseline category, the year 2011. Vertical bars signify 95% confidence intervals.

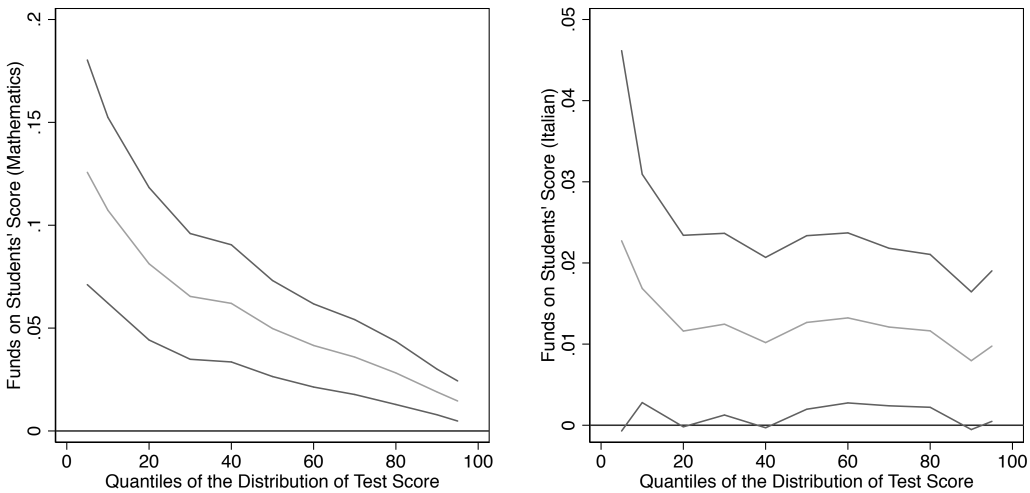
Figure 4: Estimated impact of receiving extra-funding on test scores by quantiles of the distribution of test scores: Extensive margin (Equation 1)



(a) Mathematics (b) Italian language

Notes: The two Panels shows the effect of being a recipient of extra-funding (i.e., the estimated coefficient α_1 in Equation 1) when different quantiles of the distribution of the test scores are employed in the right hand side. Dark lines delineate 95% confidence intervals.

Figure 5: Estimated impact of receiving extra-funding on test scores by quantiles of the distribution of test scores: Intensive margine (Equation 3)



(a) Mathematics (b) Italian language

Notes: The two Panels shows the effect of receiving 100 euros more (i.e., the estimated coefficient β_1 in Equation 3) when different quantiles of the distribution of the test scores is employed in the right hand side. Dark lines delineate 95% confidence intervals.

Online Appendix

School Infrastructure Spending and Educational Outcomes: Evidence from the 2012 Earthquake in Northern Italy

By Alessandro Belmonte, Vincenzo Bove, Giovanna D'Inverno, and Marco Modica

A Additional Tables and Figures

Table A1: Distribution of schools per year

	2012	2013	2014	2015	2016	total
# Schools	127	132	135	139	159	692
% Schools	18.35	19.07	19.50	20.08	22.97	100.00

Table A2: Balance test between treated and control groups

	Mean				
	(a)	(b)	(c)	(d)	(e)
	Treated	Control	Diff.	Std. Error	Count
Panel A – Treatment and IV					
Spending dummy	1.00	0.00	1.00	0.00	173
Funds per capita (× 100)	9.91	0.00	9.91***	1.42	173
Seismic hazard (PGA)	0.16	0.15	0.01*	0.00	173
Panel B – Mathematics					
log Score (mean)	3.80	3.82	-0.02	0.03	692
log Score (p5)	3.13	3.01	0.12**	0.05	692
log Score (p10)	3.31	3.24	0.07	0.04	692
log Score (p90)	4.13	4.18	-0.05**	0.02	692
log Score (p95)	4.21	4.25	-0.04*	0.02	692
Panel C – Italian Language					
log Score (mean)	4.11	4.08	0.03	0.02	696
log Score (p5)	3.60	3.50	0.10**	0.05	696
log Score (p10)	3.76	3.68	0.08**	0.04	696
log Score (p90)	4.34	4.34	0.00	0.02	696
log Score (p95)	4.39	4.38	0.01	0.01	696
Panel D – Controls					
% Male	0.56	0.56	0.00	0.02	692
% Native	0.82	0.83	-0.00	0.01	692
Cohort Size	85.38	95.42	-10.04	6.31	692

Notes: Funds per capita are expressed in 100 euros. Test scores in Panel B and C are transformed in logarithm. Cohort size is the number of tenth graders. The unit of observation is school in Panel A, and school × year in Panel B, C, and D. The number of observations are reported in column (e). Column (c) represents the difference between column (a) and column (b), and column (d) the standard error of the estimate of the difference.

Table A3: Sensitivity check: moving of the students

	(a) OLS	(b) OLS
Spending dummy $\times post_{2012}$ (lag)	-2.889	-2.689
	(4.701)	(4.630)
Controls	No	Yes
Observations	697	692
R^2	0.059	0.070

Notes: School Fixed-effect models. The dependent variable is the number of tenth graders. Column (b) includes the percentage of males, and the number of native students as controls. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Secondary School, Mathematics: Excluding schools near the damaged, rural area

	(a)	(b)
Panel A OLS Estimation.		
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.057*
	(0.024)	(0.033)
Panel B 2SLS Estimation.		
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.050***
	(0.011)	(0.013)
Observations	692	522

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Column (a) shows the baseline model; column (b) excludes schools near the damaged, rural area. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Secondary School, Mathematics: Excluding schools located in the provincial capitals

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
Panel A OLS Estimation.								
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.133***	0.094***	0.103***	0.104***	0.099***	0.084***	0.117***
	(0.024)	(0.028)	(0.024)	(0.024)	(0.024)	(0.024)	(0.027)	(0.037)
Panel B 2SLS Estimation.								
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.110*	0.053***	0.044***	0.042***	0.046***	0.042***	0.180*
	(0.011)	(0.057)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.107)
Observations	692	557	684	669	622	690	629	391

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Column (a) shows the baseline model; column (b) excludes schools from Bologna, column (c) from Ferrara, column (d) from Ravenna, column (e) from Parma, column (f) from Modena, column (g) from Reggio Emilia. Column (h) excludes all the schools located in the provincial capitals. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Sensitivity check — changes in the students' characteristics in the post-earthquake period relative to the pre-period

	Cohort Size		% Male		% Native	
	(a)	(b)	(c)	(d)	(e)	(f)
	OLS	OLS	OLS	OLS	OLS	OLS
Spending dummy $\times$ $post_{2012}$ (lag)	-2.889	-2.689	0.016	0.016	-0.001	-0.001
	(4.701)	(4.630)	(0.015)	(0.015)	(0.011)	(0.010)
Controls	No	Yes	No	Yes	No	Yes
Observations	697	692	693	692	696	692
R^2	0.059	0.070	0.013	0.016	0.042	0.060

Notes: School Fixed-effect models. Column (b) includes the percentage of males and the percentage of native students as controls. Column (d) includes the number of tenth graders and the percentage of native students as controls. Column (f) includes the number of tenth graders and the percentage of males as controls. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Secondary School, Mathematics: Within municipality sorting effects

	(1)	(2)	(3)
Panel A OLS Estimation.			
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.499***	0.165***
	(0.024)	(0.138)	(0.052)
Panel B 2SLS Estimation.			
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.076	0.070*
	(0.011)	(0.085)	(0.043)
Observations	692	46	194

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Column (1) shows the baseline model; column (2) restricts the analysis to municipalities with at most one school located. Column (3) drops municipalities where more than one typology of school is offered (i.e., liceum, technical studies or professional). Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Mathematics: Sensitivity on Municipalities' population

	(a) baseline	(b) weighted	(c) $\geq$ 10th pctile	(d) $\geq$ 20th pctile	(e) $\leq$ 80th pctile	(f) $\leq$ 90th pctile
Panel A OLS Estimation.						
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.105***	0.095***	0.101***	0.120***	0.133***
	(0.024)	(0.027)	(0.024)	(0.029)	(0.038)	(0.028)
Panel B 2SLS Estimation.						
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.019***	0.042***	0.027***	0.157*	0.110*
	(0.011)	(0.004)	(0.010)	(0.007)	(0.095)	(0.057)
Observations	692	692	612	333	359	557

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Column (a) shows the baseline model; column (b) uses weighted least squares (WLS) estimations with weights given by the population of the municipality where the school is located; column (c) excludes the first decile; column (d) excludes the second decile; column (e) excludes the first and the second top deciles; and column (f) excludes the first top decile. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Mathematics: Municipalities' characteristics

	(a) OLS	(b) OLS	(c) OLS	(d) OLS	(e) OLS	(f) OLS
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.110***	0.106***	0.098***	0.109***	0.109***
	(0.024)	(0.022)	(0.022)	(0.024)	(0.022)	(0.022)
Population $\times post_{2012}$ (lag)		-0.000				-0.000
		(0.000)				(0.000)
Pop. Density $\times post_{2012}$ (lag)			-0.000			0.000
			(0.000)			(0.000)
Height $\times post_{2012}$ (lag)				-0.000***		-0.000***
				(0.000)		(0.000)
# Schools $\times post_{2012}$ (lag)					-0.002	0.002
					(0.002)	(0.007)
Observations	692	692	692	692	692	692
R^2	0.147	0.149	0.148	0.150	0.148	0.152

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Mathematics: Municipalities' characteristics

	(a) 2SLS	(b) 2SLS	(c) 2SLS	(d) 2SLS	(e) 2SLS	(f) 2SLS
Funding per capita (00) $\times post_{2012}$ (*lag*)	0.046***	0.046***	0.046***	0.046***	0.046***	0.047***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Population $\times post_{2012}$ (*lag*)		-0.000***				-0.000***
		(0.000)				(0.000)
Pop. Density $\times post_{2012}$ (*lag*)			-0.000***			0.000
			(0.000)			(0.000)
Height $\times post_{2012}$ (*lag*)				-0.000***		-0.000
				(0.000)		(0.000)
# Schools $\times post_{2012}$ (*lag*)					-0.010***	0.107**
					(0.003)	(0.045)
Observations	692	692	692	692	692	692
R^2	0.132	0.132	0.132	0.137	0.132	0.138

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Mathematics: sensitivity on funding

	(a) baseline	(b) $\geq$ 100 Euro	(c) $\geq$ 200 Euro	(d) $\leq$ 2,000 Euro	(e) $\leq$ 5,000 Euro
Panel A OLS Estimation.					
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.117***	0.127***	0.090***	0.092***
	(0.024)	(0.025)	(0.028)	(0.024)	(0.023)
Panel B IV Estimation.					
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.048***	0.048***	0.101***	0.056***
	(0.011)	(0.011)	(0.012)	(0.022)	(0.013)
Observations	692	623	585	664	685

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Funding per capita is expressed in 100 euros. In columns (b) and (c) we exclude schools that received limited resources from the central government, less than 100 euro and less than 200 euro, respectively. In columns (d) and (e) we exclude schools that received more than 2,000 Euros and more than 5,000 Euros, respectively. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Secondary School, Mathematics: Non-linearities in school quality

	(a) baseline	(b) $\geq$ 5th perc.	(c) $\geq$ 10th perc.	(d) $\leq$ 90th perc.	(e) $\leq$ 95th perc.
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.040***	0.038***	0.043***	0.044***
	(0.011)	(0.009)	(0.009)	(0.010)	(0.011)
Observations	666	526	508	591	618

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Secondary School, Mathematics — Alternative specifications

	(a)	(b)	(c)	(d)	(e)	(f)
	OLS	OLS	OLS	2SLS	2SLS	2SLS
Spending dummy $\times post_{2012}$ (lag)	0.097***	0.096***	0.099***			
	(0.024)	(0.024)	(0.024)			
Funding per capita (00) $\times post_{2012}$ (lag)				0.047***	0.048***	0.046***
				(0.011)	(0.011)	(0.011)
School controls	No	Yes	Yes	No	Yes	Yes
Province $\times$ Year FE	No	No	Yes	No	No	Yes
Observations	697	692	692	697	692	692
R^2	0.130	0.135	0.147	0.113	0.118	0.132

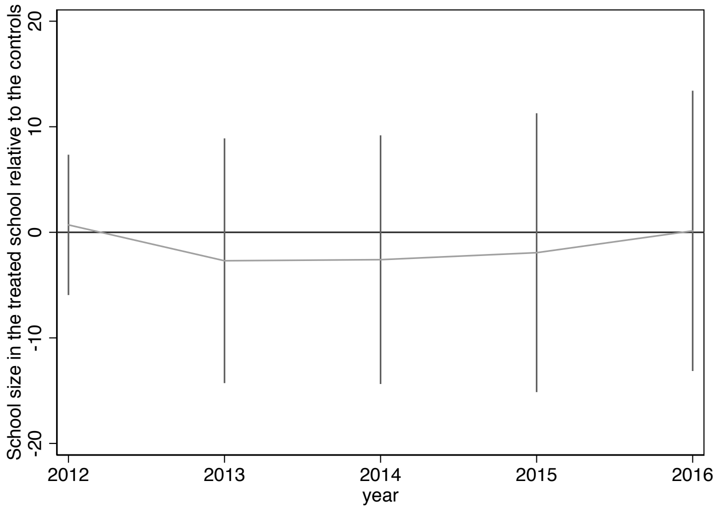
Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Funding per capita is expressed in 100 euros. Each column includes a linear trend. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Sensitivity check, Mathematics: treatment dummy

	(a) 2nd order contiguity (baseline)	(b) 1st order contiguity	(c) 3rd order contiguity
Panel A OLS Estimation.			
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.118***	0.100***
	(0.024)	(0.026)	(0.023)
Panel B 2SLS Estimation.			
Funding per capita (00) $\times post_{2012}$ (lag)	0.046***	0.030***	0.084**
	(0.011)	(0.007)	(0.034)
Observations	692	439	774

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A1: Difference in school population between treated and control areas



Notes: The plot illustrates the effect of being a recipient of extra-funding on the cohort size of tenth graders observed in year t relative to the baseline category, the year 2011. Vertical bars signify 95% confidence intervals.

Table A15: Secondary School, Mathematics: funding, cheating, and students scores

	(a)	(b)	(c)	(d)
	OLS	OLS	2SLS	2SLS
Spending dummy $\times post_{2012}$ (lag)	0.099***	0.098***		
	(0.024)	(0.022)		
Funding per capita (00) $\times post_{2012}$ (lag)			0.046***	0.046***
			(0.011)	(0.011)
Cheating score $\times post_{2012}$ (lag)		0.722***		0.172
		(0.176)		(0.382)
Observations	692	692	692	692
R^2	0.147	0.181	0.091	0.094

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Secondary School, Mathematics: Private houses reconstruction and students scores

	(a)	(b)	(c)	(d)	(e)	(f)
	OLS	OLS	OLS	OLS	2SLS	2SLS
Spending dummy $\times\, post_{2012}$ (*lag*)	0.096***	0.099***				
	(0.026)	(0.027)				
Funding per capita (00) $\times\, post_{2012}$ (*lag*)			0.003**	0.003**	0.049***	0.049***
			(0.001)	(0.001)	(0.012)	(0.012)
Funding for private renovation $\times\, post_{2012}$ (*lag*)		-0.000		0.000		-0.000
		(0.000)		(0.000)		(0.000)
Observations	570	570	570	570	570	570
R^2	0.132	0.133	0.118	0.119	0.081	0.089

School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B The Northern Italy earthquake overview

To investigate the effect of school capital spending on student outcome, we use information on the extra funding that a group of schools received after the 2012 Northern Italy earthquake. In the following, we give a brief sketch of the seismic-related events and the post-quake interventions.

B.1 The May 2012 earthquake

On May 20, 2012, an earthquake of magnitude 6.1 hit a wide portion of the Po Valley in the Northern part of Italy. The epicenter was located near the town of Finale Emilia (MO), about 30 km west of the city of Ferrara, and the earthquake involved exclusively an area of about 3.5 thousand square kilometers across the three regions of Emilia Romagna, Veneto and Lombardy. The provinces affected by the earthquake were those of Ferrara, Modena, Mantova, Bologna, Reggio Emilia, and Rovigo, as officially stated in the law # 122/2012.

Modena, Bologna, and Ferrara, all in the Region of Emilia Romagna, were the most affected provinces. In the first province nearly thousand square kilometers were damaged by the earthquake (about 36% of its territory). In the province of Bologna the area involved was 930 square kilometers (the 25% of the total). Finally, 31% of the province of Ferrara reported damages, in a territory of 818 square kilometers. The other three provinces were marginally impacted, with a total hit territory amounting to a thousand square kilometers.

B.2 Perception of the risk and reconstruction

The region was not considered a highly exposed seismic zone until 2012. With the exception of the seismic sequence of Ferrara in 1570, Argenta in 1624 and Bologna in 1929 (Vannoli *et al.* , 2015), few other small intensity earthquakes have had an impact on the collective memory of their inhabitants. As a result, the perception of a seismic risk was really small in this area compared with the rest of Italy. In fact, PGA values in this area are, on average, only 20% of those characterizing the nearby Apennine mountain chain.[24] Moreover, the INGV estimated the zone's seismic hazard to be about 0.05 and 0.15 in terms of maximum horizontal ground acceleration rate, up to five times smaller than the one estimated in the Appenini zone in the rest of the Italian peninsula.[25] Accordingly, in this area housing construction was not subject to any specific anti-seismic measure compared with the rest of the country.

In the aftermath of the earthquake the perception of the risk dramatically changed and money has been sent to finance the reconstruction as well as to secure the whole area.[26]

The intervention was implemented in two stages.

[24] See http://zonesismiche.mi.ingv.it/

[25] Source: INGV http://www.mi.ingv.it/pericolosita-sismica.

[26] On the whole, 11 million euros have been disposed in the province of Mantua, 122 millions in the provinces of Bologna, Ferrara, Modena, and Reggio Emilia, and 8.8 in the province of Rovigo.

A first phase, implemented in the very next days that followed the end of the seismic sequence, concerned the urgent operations required to provide first aid, to refurbish buildings and equipments, especially those related to water, electric and drainage system.

A second phase aimed at financing a number of projects that were precisely targeted to secure and refurbish buildings in accordance with the new seismic risk. Importantly, fundings were not sent only to finance the reconstruction, but also to secure the whole area and to reinforce the anti-seismic system of the buildings, sometimes also getting the chance to make a better sustainability of the energy consumption so to ameliorate the building system, while increasing the seismic safety and the urban quality.

In our analysis we select only schools with no or slight damage — hence, in the zone we look at none of the emergency interventions have been put into action.[27] In the next section, we detailed the selection procedure of building eligible to be a recipient of funding in low damaged zone.

B.3 The assessment of the damage and eligibility to funding

The main measures enacted to regulate the procedures for the intervention during the reconstruction phase and the actors involved are detailed in the law # 122/2012. According to it, the governors of the affected Regions were appointed to coordinate the reconstruction activities of their respective administrative competencies. However, the assessment of the damage was carried on by the Italian Civil Protection Department (DPC). The DPC deployed more than 3,000 experts to inspect about 40,000 buildings in the affected area in the first two months after the quake (Dolce and Di Bucci, 2014).

Inspections were aimed at assessing the damage and usability of the buildings. In Figures B1 and B2 we illustrate two examples of assessment made in the aftermath of the 2012 earthquake. Buildings were then classified in 7 classes of usability — from usable to fully unusable. Importantly, as remarked by Dolce and Di Bucci (2014, p. 2246), the forms compiled by the DPC experts teams are administrative documents, with legal effects; hence, they were the ground for the allocation of the resources aimed at covering the costs of the damage.

Based on such assessment, technical commissions, chaired by the governors of the Regions involved, evaluate the eligibility to receive funds building by building and approve any program of reconstruction interventions, along with the relative financial plan. Approved funding are then transferred to the involved administrative entities (i.e., municipalities and Provinces) and finally to the eligible subjects. It is worthwhile to remark that high school buildings are managed by Provinces that hence receive, with no intermediation made by the municipalities, direct funds from the commissions.

[27] We remind the reader, interested in the first phase interventions, to a more compelling treatment in Dolce and Di Bucci (2014).

Figure B1: Example of the assessment of the damage in a building in damaged zone. *Source:* Dolce and Di Bucci (2014, p. 2244)

Figure B2: Example of the assessment of the damage in a slightly damaged school building. *Source:* Masi et al. (2016, p. 217)

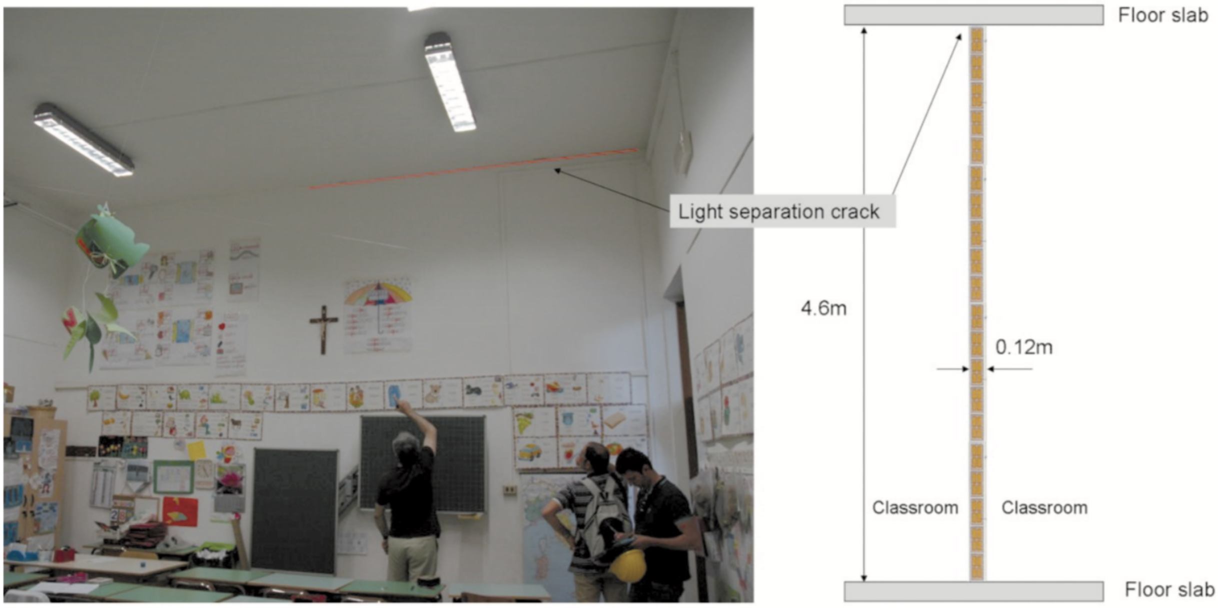
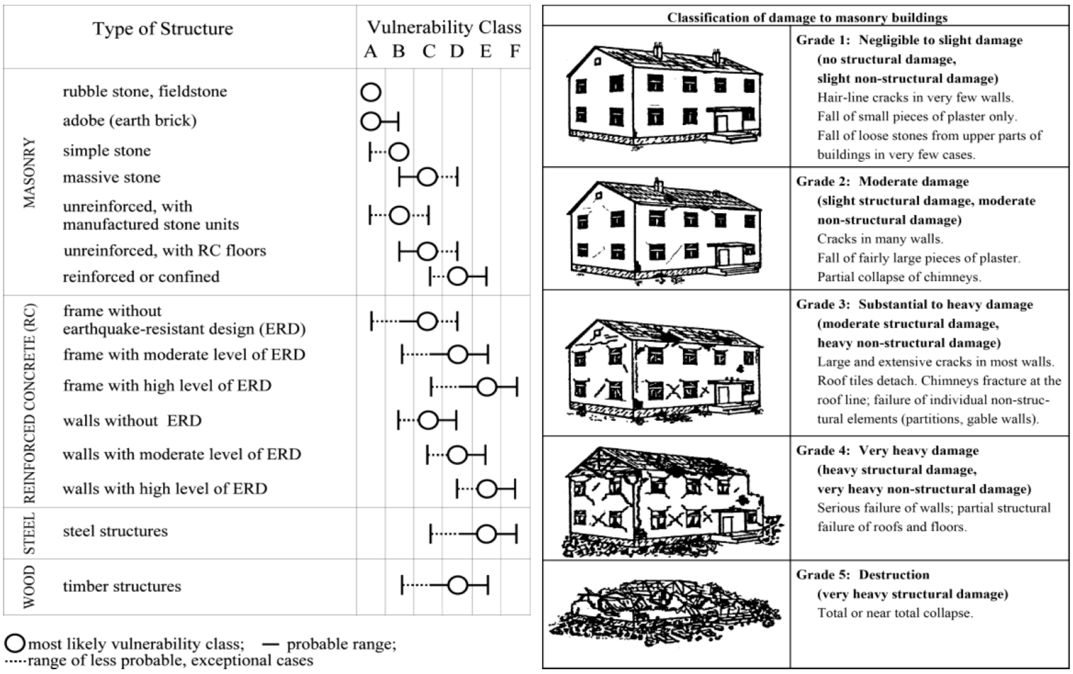


Figure C1: Vulnerability class and classification of damage to buildings. *Source:* Grünthal (1998, pp. 14-15)



(a) (b)

C The INGV macro-seismic survey

We compare the evolution of the test scores in Mathematics and Italian language of students in undamaged schools located in the earthquake-affected area, that were thus awarded special funding, with those of students in schools located in neighboring municipalities, outside the earthquake area, that did not receive any extra-funds. As we discussed in the paper, we select the first group of municipalities using information from the INGV macroseismic survey. In this section, we illustrate how this survey was implemented. For more details we refer to Galli *et al.* (2012).

The INGV macroseismic survey matches information from the macroseismic intensity values, measured through the EMS-98 scale,[28] and the level of vulnerability of the buildings in the municipality, that varies across six classes of vulnerability (A, highest vulnerability, to F, lowest vulnerability) in relation to the structural characteristics of the buildings (e.g. typological and morphological information and age of construction of the buildings). Figure C1(a), from Grünthal (1998), illustrates the likelihood that a building lies in a given vulnerability class based on its structure, whose information are gathered from the 2011 census.

Combining the macroseismic intensity values with the level of vulnerability of the buildings, the INGV macroseismic survey provides estimations of the volume of buildings with a certain level of damage in a given municipality (see Meroni *et al.* (2017)

[28]As explained in Section B.3, the intensity values of the earthquake for the damaged localities are collected by the Italian Civil Protection Department (DPC).

for a technical description). As illustrated in Figure C1(b), the INGV macroseismic survey classifies potential damage in 5 classes. Buildings with a damage of grade 1 (class D1) reported negligible or slight damages that, even in the worst scenario, have not affected the structure of the building. These buildings counted one or two hair-line cracks in the walls or small pieces of plaster broken off the wall. When the cracks in the walls become numerous, or there are large pieces of plaster broken off from the walls, buildings are classified as D2. Although the building does not have yet any structural damage, its use becomes less appropriate for any activity. Buildings with damages of grade higher than D2 feature heavy (and structural) damages. Those of class D3 report moderate structural damage, whereas those of class D4 are seriously damaged. Finally, buildings with a damage of class D5 are destroyed.

To give an example, if a municipality is given a macroseismic intensity value of IX along the EMS-98 scale, it means that many buildings with medium vulnerability level (class B) and few buildings of vulnerability level C are heavily damaged (D4) whereas most buildings with higher vulnerability levels (e.g., class A) are completely collapsed (D5). For each class of damage, the INGV macroseismic survey then provides the percentages of buildings in each class in every municipality.

In our analysis, we keep only schoolhouses located in municipalities where the percentage of buildings is classified at most as D1, as hair-line cracks in the walls or broken off small pieces of plaster do not affect negatively the learning process of the students.

D Additional evidence on the first-stage relationship between funds allocated and seismic hazards

A potential concern is that the results from the 2SLS estimation, presented in Equation 3, are driven by the difference in PGA levels between unfunded schools (i.e., $D_i = 0$), located in areas with relatively low seismic risk, and funded schools (i.e., $D_i = 1$), located in areas with higher levels of seismic risk, and that little additional variation is left within the latter group.

In this section, we show that our results are mostly driven by a genuine variation in the (positive) amount of funds received. We re-estimate the difference-in-difference models using only data on funded schools. This analysis, thus, relies on a restricted panel of 68 schools.[29]

We present these results in Table D1. Specifically, in column 1 we estimate the difference-in-difference model using OLS. In column 2, we employ plausible exogenous variation in the PGA levels using 2SLS. Interestingly, although we only use data on funded schools, 2SLS estimates are 4 times larger than when using the full sample. The difference in magnitude between OLS and 2SLS is now smaller than in Table 4, yet this cannot be ascribed to a weak first stage estimation. In fact, the Kleibergen-Paap F Statistics is larger indicating a strong relationship between the amount of funds and the "fragility" of the territory where the school is located. This first stage relationship is also displayed in Figure D1, where we present the scatterplot of the residuals of the amount of funds received (y-axis) versus the PGA levels of the municipality where the school is located (x-axis).

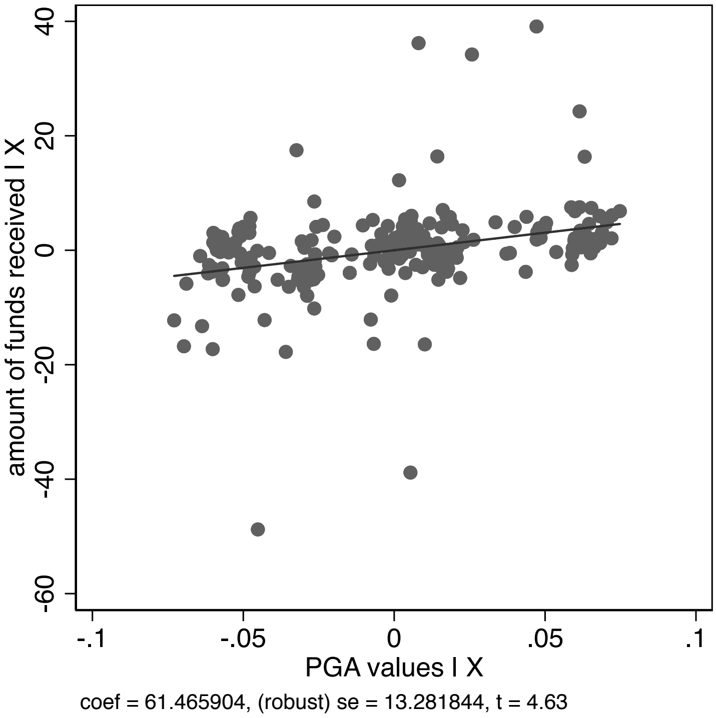
[29]Since we cluster standard errors at school level, our inference is based on 68 clusters.

Table D1: Secondary School, Mathematics – Only funded Schools

	(a)	(b)	(c)	(d)
	OLS	2SLS	first stage	reduced-form
Funding per capita (00) $\times post_{2012}$ (lag)	0.004^{**}	0.017^{***}		
	(0.001)	(0.004)		
Seismic hazard $\times post_{2012}$ (lag)			61.466^{***}	1.021^{***}
			(11.420)	(0.175)
Kleibergen-Paap F Statistics			28.954	
Observations	268	268	268	268
R^2	0.119	0.022	0.433	0.174

Notes: School Fixed-effect models. The dependent variable is the logarithm of the average test score in Mathematics. Funding per capita is expressed in 100 euros. Each column includes the cohort size, the percentage of males, and the number of native students. Linear trend and province dummies interacted with $post_{2012}$ are included in all regressions. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure D1: PGA values and amount of funds received (estimation from column c of Table D1). Only funded schools



Notes: Unlike column c of Table D1, this relationship is estimated using school dummies in place of school fixed effects — thus explaining the difference in the size of the estimated standard error.

E Potential endogeneity of funding allocation

In Section B, we have explained how the assessment of the damage and the eligibility to funding followed verifiable criteria made by the DPC and the governors of Regions. This makes likely that our results do not pick up confounding, endogenous mechanisms where mayors have put pressure to include in the list of recipients their municipalities. However, our analysis cannot fully rule out the possibility that this has occurred 'under the table'. To address this potential concern in this section we provide additional evidence against it. First, we show that mayors in municipalities that received funds do not have specific characteristics relative to those ruling unfunded municipalities. Second, we use plausibly exogenous variation in the PGA levels to instrument the effect of receiving funding on educational outcomes.

E.1 Mayors' characteristics

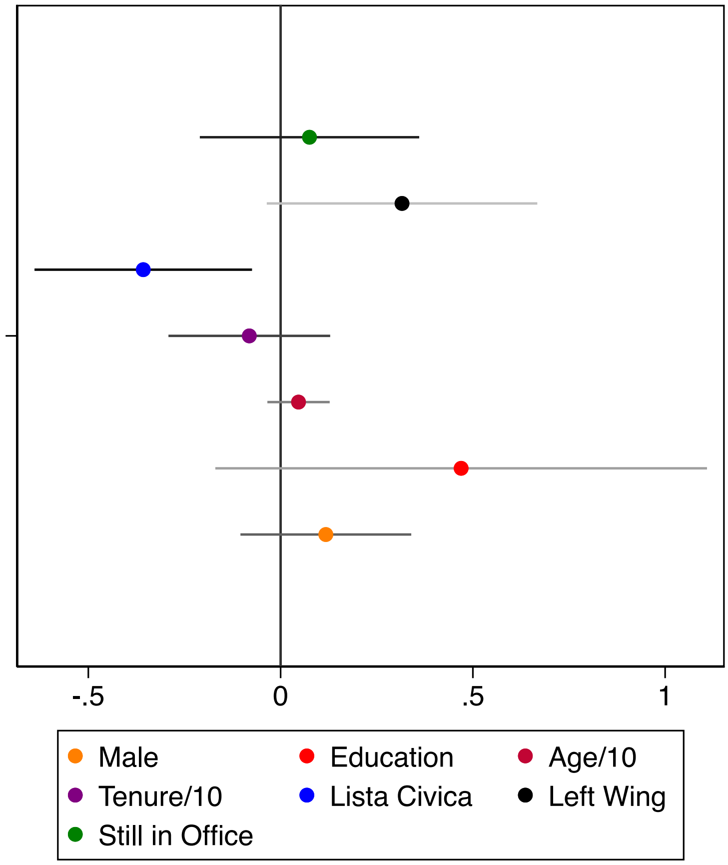
We gather first-hand information on mayors' characteristics in 2012 scraping resumé published in the council hall webpages. We obtained information on mayors' gender (1 if male), education (1 if the mayor holds the elementary degree and 5 if holds the doctoral degree), age, number of years in office in 2012 (2012 minus the year of his or her first appointment), whether he or she competed with an apolitical coalition (*lista civica*), whether he or she is affiliated to a left wing coalition, and whether the mayor gained reelection after his or her mandate in 2012. According to the literature on political selection (e.g., Besley, 2006; Acemoglu, Egorov, and Sonin, 2010), these characteristics may potentially make a politician more 'skilled' in influencing the inclusion of schools in the recipient group.

To curb concerns on mayors having influenced the allocation of funding process, one should ideally obtain that all these characteristics are orthogonal to the allocation of funding. We test this hypothesis running a balance test between municipalities that have received funding after the earthquake and municipalities that did not and illustrate the estimates in Figure E1 (where horizontal bars around the point estimation indicate the 95% confidence interval).[30] Point estimations that lie on the left of the vertical line indicate that those characteristics are relatively more prominent in unfunded municipalities; those stretched rightward indicate mayors' characteristics that are relatively more prominent in funded municipalities.

Setting our confidence level at 95%, we find that mayors are not statistically different in the two samples of municipalities, except for the type of coalition they run for mayorship in the elections. Specifically, we find that unfunded municipalities are relatively more likely to be governed by a major that campaigned with a *lista civica.* This is a systematic feature of the Italian administrative election system and likely to be mechanical with respect to the population. Indeed, once we control for the population of the municipality we cannot reject the null hypothesis.

[30]To improve the graphical presentation of the estimates in Figure E1 we divide both age and tenure by 10.

Figure E1: Balance test between municipalities recipient of funding and municipalities that have not received funds — Mayors' characteristics.



E.2 2SLS estimation

There is a widespread consensus that political alignment between the National and the local government is an element that facilitates top-down transfers of funds (e.g., Solé-Ollé and Sorribas-Navarro, 2008; Brollo and Nannicini, 2012; Arulampalam et al., 2009). However, it is worth remarking that this is unlikely to occur in our analysis as in the period under scrutiny Italy was governed by a government of technicians (*Governo Monti*), that implemented austerity policies. Nonetheless, as we explained in Section B.3, regional governors played a chief role in the selection of the eligible projects and alignment between the Regional and the municipal governments can have still played a role in influencing the decisions regarding the allocation of funding.

Our results illustrated in Figure E1 suggest that this can be a potential issue as funded municipalities seem to be more likely to have mayors from a left-wing platform — the same of the governor of the Region of Emilia Romagna. Yet, this relation is not statistically significant at conventional levels.

To address this potential endogeneity issue we employ a 2SLS estimation using plausibly exogenous variation in the PGA levels and present our 2SLS estimations in Table E1. Second stage estimates are positive and statistically significant (column b) and so is the first stage estimation that captures the effect of a variation in the PGA levels on the probability of being a recipient of funding (column c). Our results are also robust to an alternative clustering strategy that capture intra-class correlation at municipality level (in squared brackets).

Table E1: Secondary School, Mathematics

	(b)	(c)	(d)
	OLS	2SLS	first stage
Spending dummy $\times post_{2012}$ (lag)	0.099	0.507	
	$(0.024)^{***}$	$(0.079)^{***}$	
	$[0.024]^{***}$	$[0.265]^{*}$	
Seismic hazard $\times post_{2012}$ (lag)			2.374
			$(0.297)^{***}$
			$[1.081]^{*}$
Kleibergen-Paap F Statistics			63.820
Observations	692	692	692
R^2	0.147	0.033	0.463

School Fixed-effect models. Linear trend and province dummies interacted with POST are included in all regressions. Standard errors clustered at school level in rounded parentheses and at municipality level in squared parentheses.

Additional references (not cited in the article)

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- Solé-Ollé, A., & Sorribas-Navarro, P. (2008). The effects of partisan alignment on the allocation of intergovernmental transfers. Differences-in-differences estimates for Spain. Journal of Public Economics, 92(12), 2302-2319.

F Data sources and description

School funding: The 2012 seismic events affected six provinces in three regions: Bologna, Modena, Ferrara, Reggio Emilia (Emilia-Romagna), Rovigo (Veneto) and Mantova (Lombardia). The Governors of the involved regions were appointed as deputy commissioners to promote interventions to reconstruct and to secure the affected areas. Accordingly, several legislative acts were enacted providing the guidelines of such interventions and, among others, also those ones related to the school funding. The funds were intended for reconstruction, securing the whole area and reinforcing the anti-seismic system of the buildings, accompanied by an increase of the seismic safety and urban quality in terms of energy consumption sustainability. With particular reference to areas with negligible damages, interventions to the schoolhouses have been not invasive and funds have been mostly used for e.g., painting scratched walls, improving the lightening of rooms, fixing the heating system etc. Money has been used solely to support the school infrastructure and not to buy PCs or other technological devices. Note also that teachers salaries are fixed in Italy and paid by the Ministry of Education: they could not be possibly increased using funds for the earthquake reconstruction.

The following websites list all the enacted laws related to the interventions after the earthquake for each of the three regions:
http://www.regione.emilia-romagna.it/terremoto/gli-atti-per-la-ricostruzione
http://www.sismamantova.regione.lombardia.it
http://www.regione.veneto.it/web/guest/ordinanze-del-commissario-delegato

From the above legislative acts we assemble the information about the amount of funding received by each school after the earthquake. These documents report the name and the location of the school, the amount of funding received, and a short description of the required intervention. Specifically, the following orders of the deputy commissioner (ODC) have been considered:

[-] Emilia-Romagna: "attachment D/1" and "attachment D" in the ODC #111 (27 September 2013) and in the Committee resolution #1388 (30 September 2013);

[-] Lombardia: "attachment A" in the ODC #22 (24 June 2013), "attachment 1" in the ODC #26 (30 July 2013), confirmed in the ODC #69 (5 November 2014);

[-] Veneto: "attachment A" in the ODC #2 (9 August 2012), "attachment A" in the ODC #3 (20 August 2012), confirmed in the ODC #4 (19 November 2012).

We combine this data with the information about the school identification number and their address to geolocate each school in sub-municipal areas using the map provided by the "Italian Revenue Agency" (see the following link: `http://wwwt.agenziaentrate.gov.it/geopoi_omi/index.php`).

Damage evaluation: The INGV macroseismic survey is based on the same territorial classification provided by Meroni et al. (2017). See Section C of the Online Appendix for more information.

Seismic hazard: We collect the data for the variable "*Seismic hazard (PGA)*" to measure the frequency of earthquake occurrence from the following INGV official website http://esse1-gis.mi.ingv.it/s1_en.php. The unit of measure is the gravity acceleration and it refers to the maximum ground acceleration during the earthquakes at municipality level. We match each school with the PGA score using the municipality identification number provided by the Italian National Institute of Statistics (ISTAT).

Test scores: Since 2008, all the Italian students' skills and achievements have been evaluated by an independent public agency, namely the National Institute for the Educational Evaluation of Instruction and Training, known by the Italian acronym INVALSI. From the school year 2010/11, at the end of each year INVALSI administers a standardized test of Mathematics and Italian language skills to state schools students.

The main advantage of such procedure is that those tests are administered at the same time to all students of the same grade and, above all, they are standardized, so that the comparison across students from different schools and academic years is possible. The test results are publicly presented once a year, at the end of the academic year, highly debated by the national media. Note, however, that these tests have no implication at all for the allocation of public funding across schools. The scores collected in those tests do not contribute in any way to the final grade assigned to students either.

For an extensive number of state high schools we have data for both Mathematics and Italian skills over six school years from 2010/11 to 2015/16, that is two years before and four years after the earthquake. Each school has an identification number through which we match the INVALSI test results with the schools of the sample determined as explained above. The test scores range between a minimum of 10 and a maximum of 100, when all the answers are correct. As the test scores are provided at student level, we aggregate them so to construct several variables at the school level, for both Mathematics and Italian test scores. The first variable is "*Score (mean)*": it is the average result of the tenth graders for each school. Then we measure the average test scores of the low-achieving students by considering the fraction of students in the 5th and 10th percentile of the score distribution, so that we have respectively "*Score (p5)*" and "*Score (p10)*." In a similar way, we measure the average test scores of the high-achieving students by considering the fraction of students in the 90th and 95th percentile of the score distribution, so that we have respectively "*Score (p90)*" and "*Score (p95)*."

% Male, % Native and Cohort Size: We take data on the share of male and native students and on the cohort size from INVALSI.

Cheating attitudes: We take data on the cheating scores on each class of students from INVALSI.

Information on mayors: We scraped information on mayors in our sample municipalities around 2012 using resumeón city hall webpages.

Funds on private buildings renovation: We gather additional data on public resources that the government has allocated to private citizens to modernize and improve the quality of their buildings and thus mitigate their vulnerability to future earthquakes, from `https://openricostruzione.regione.emilia-romagna.it/`, for the municipalities of the Region Emilia Romagna. It covers 83% of the total number of schools.