Department of
Geography and
Environment

Papers in Economic Geography and Spatial Economics

The local impact of closing undersized schools

Marco Di Cataldo and Giulia Romani

Paper No. 43
Geography and Environment Discussion Paper Series

June 2023

Published by
Department of Geography and Environment
London School of Economics and Political Science
Houghton Street
London
WC2A 2AE

geog.comms@lse.ac.uk
www.lse.ac.uk/Geography-and-Environment

Rational cuts?
The local impact of closing undersized schools

Marco Di Cataldo[1, 2] and Giulia Romani[1]

[1]*Department of Economics, Ca' Foscari University of Venice*
[2]*Department of Geography and Environment, London School of Economics*

Abstract

The availability of public education services can influence residential choices. Hence, policies aiming to 'rationalise' service provision by reducing the number of undersized nodes in the public school network can lead to population decline. This paper examines the demographic and income effects of primary school closures by exploiting an Italian education reform that resulted in a significant contraction of the school network. We assess whether school closures impact households' residential choices, on top and beyond preexisting negative population trends that motivate school closures. To address endogeneity, we combine a Two-Way Fixed Effects model with an instrumental variable approach, constructing the IVs based on institutional thresholds for school sizing adopted by some Italian regions. Our findings suggest that municipalities affected by school closures experience significant reductions in population and income. The effect is driven by peripheral municipalities located far from economic centres and distant from the next available primary school. This evidence indicates that school 'rationalisation policies', by fostering depopulation of peripheral areas, have an influence on the spatial distribution of households and income, thus affecting territorial disparities.

Keywords: school closures, residential choices, education policy, core-periphery patterns, Italy
JEL: H40; H52; R23

Acknowledgements: we are grateful to Gabriel Ahlfeldt, Guglielmo Barone, Agar Brugiavini, Guido de Blasio, Henry Overman, Giacomo Pasini, Jorge Pérez Pérez, Enrico Rettore, Olmo Silva, Daniel Sturm, all participants to the Ca' Foscari-Groningen Economics seminar, the 2020 AISRe conference, the 2021 and 2022 ERSA conferences, the 2021 UEA European and North-American Meetings, the 2021 UEA Summer school, the 2022 EEA/ESEM Conference for useful comments and suggestions. The publication of the paper in the series "LSE Papers in Economic Geography and Spatial Economics" has been endorsed by Riccardo Crescenzi. We are grateful to LSE for its hospitality. All errors are our own.
Marco Di Cataldo: marco.dicataldo@unive.it; Giulia Romani: giulia.romani@unive.it

1 Introduction

Access to publicly provided services plays a key role in influencing residential choices. People decide where to live taking into account not just job opportunities and idiosyncratic preferences, but also the availability of nearby and good-quality public services. In particular, a crucial factor affecting households' location decisions is the availability of public education and schooling (Black, 1999; Hoxby, 2000).

In turn, the organisation and territorial distribution of education services are directly dependent on government policies. A key aspect often considered by policymakers in the design of policies influencing public services is the reduction of fixed costs (Alesina et al., 2004; Urquiola, 2005). This is the case for so-called 'rationalisation policies', i.e. public interventions aimed at removing undersized service centres to reduce public expenditures. However, these policies may also shape the location decision of households and, by providing unequal incentives for relocation depending on income levels, they can affect income differentials across space (OECD, 2021). This article investigates whether people 'vote with their feet'[1] in favour of school access, in a context where rationalisation policies have cut undersized nodes of the school network.

Our focus is on Italy, exploiting an education reform that took place in 2008 in the country. The Italian context represents an interesting and unique analytical setting for our purpose. On the one hand, despite the traditionally low mobility of the Italian population, there is evidence of significant internal migrations, mainly directed towards big urban centres. This especially concerns young adults with children, representing the highest fraction of all internal migrants.[2] Italy displays territorial disparities in terms of population, services, and economic opportunities, not only across but also within regions.[3] On the other hand, austerity measures implemented in the last decade have led to a deep rationalisation of key services,

[1]The idea of people moving across jurisdictions to access public services stems from Tiebout's (1956) proposition of people 'voting with their feet', i.e. the idea that people relocate in space to find the jurisdictional unit maximising their public goods' preferences. Empirical studies testing the validity of Tiebout's hypothesis have investigated the role of public schools for residential choices by focusing on school quality differentials, either by looking at inter-district choice programmes (Brunner et al., 2012), or indirectly by estimating changes in house prices (Black, 1999; Fack and Grenet, 2010; Gibbons and Machin, 2006; Gibbons et al., 2013).

[2]This is confirmed by recent reports on migrations of the Italian Institute of Statistics (ISTAT, 2019). Appendix Figure A1 shows the distribution of internal migrants by age.

[3]The link between population dynamics and access to services lies at the core of the National Strategy for Inner Areas SNAI (2014), which constitutes one of the largest ongoing National policy efforts to address Italian disparities within regions.

a process that has touched the public education system as well. In this respect, the so-called 'Gelmini reform' of 2008 represents the most decisive and effective push towards the contraction of the Italian school network. The objective of the reform was to cut public spending by eliminating undersized centres of service provision. Per-student public expenditures were considered excessively high, a feature attributed by the reform to the geographically dispersed configuration of the Italian schooling system. This has implied a significant reduction of educational infrastructure and the closure of several schools across the country.[4]

Such a reduction in schooling services may have affected population dynamics. This is especially true for the most basic education infrastructure services, such as the availability of primary schools. Particularly in small and peripheral areas with comparatively fewer schooling options, the closure of primary schools may condition residential choices. Primary schools are mandatory, they last five years and primary school-age children still depend on their parents for daily commuting. The lack of available primary school services may therefore represent a valid reason for a family to change residence.

To the best of our knowledge, no study has ever performed a systematic assessment of the population dynamics induced by schooling rationalisation policies. In this article, we aim to fill this gap and investigate whether service cuts to undersized school services foster population decline. In addition, we look at the consequences such population decline may have on the income composition of local communities, and study the spatial heterogeneity of the estimated impact. A related contribution to ours - yet focusing on a different type of negative shock to public services - is the work of Gibbons et al. (2018), assessing the relocation effect of transport infrastructure cuts of non-profitable rail lines and finding that they induce the depopulation of local areas experiencing the largest cuts.

We employ geo-located data on the universe of Italian public and private schools to assess the population and income dynamics of municipalities experiencing the closure of their only primary school during the 2010-2019 period, as a result of the 'Gelmini' rationalisation reform. Our analysis faces a fundamental empirical challenge, in that 'treated' municipalities experiencing primary school closures are often characterised by negative population pre-trends. We address this empirical issue through a Two-Way-Fixed-Effects (TWFE) model in combination with an in-

[4]Table 1 shows the absolute and relative numbers of primary school closures in municipalities endowed with a single primary school in 2010. Closures amount to 271, corresponding to 6.34% of the total.

strumental variable approach. We construct instruments exploiting institutional rules governing primary school sizing, enforced by some Italian regions during the period of analysis, interacting them with pre-threshold school characteristics. We also consider the margin of deviation from the school sizing threshold.

Our findings suggest that school cuts negatively affect population dynamics on top and beyond preexisting trends. The effect is sizeable for children in mandatory school age and young adults, which represent the most direct recipients of school services and hence are the most affected by primary school closures. Conversely, no effect is found on the elder population, less likely to be affected by educational infrastructure cuts. We also find a reduction in total income of municipalities where education services are reduced, while per-capita income seems to remain unaffected. The overall impact of school closures on municipal depopulation is mainly driven by peripheral municipalities, i.e. those distant from economic centres and from alternative school options.

The remainder of the paper is structured as follows. Section 2 reviews the related literature; section 3 describes the institutional context of the Italian schooling system and the 2008 reform; section 4 presents the dataset; section 5 outlines our main empirical strategy; section 6 presents the results; section 7 explores the territorial heterogeneity of the estimated effect; section 8 concludes.

2 Literature review and contribution

There exists a large body of literature studying how residential choices respond to the provision of public services. The seminal contribution of Tiebout (1956) postulates that, in a context of decentralised provision of tied-to-residence public goods, households would relocate in order to match their preferences. This hypothesis has recently undergone several empirical tests, with contributions focusing on different kinds of local services or amenities, such as local environmental quality (Banzhaf and Walsh, 2008; Gamper-Rabindran and Timmins, 2011) or rail transit lines (Kahn, 2007). These studies tend to confirm that households are willing to move to places offering them desirable amenities and public services.

Other tests of Tiebout's argument focus on school services. Schools are especially relevant for residential choices, since households with children have a daily need for schooling. Households evaluate school alternatives, whose quality depends on per-student expenditure and peer-average performance. Hoxby (2000) shows that higher choice among jurisdictions improves public school productivity, Baum-Snow and Lutz (2011) study the residential and school choice response to the desegregation of public school districts, Brunner et al. (2012) demonstrate that inter-district schooling choice programmes have an effect on residential location decisions. Taken together, these works indicate that different institutional contexts related to the local public education system affect households' location decisions. Other studies perform indirect tests of Tiebout's hypothesis by looking at house prices, finding that public school performance is capitalised into house prices and parents are willing to bear higher housing costs to access better quality schools (Black, 1999; Fack and Grenet, 2010; Gibbons and Machin, 2006; Gibbons et al., 2013). Private schools mitigate the effect, offering an outside option (Fack and Grenet, 2010). These works largely confirm the predictions of models suggesting that increased school choice reduces district disparities in terms of income and housing values (Nechyba, 2000, 2003; Ferreyra, 2007; Epple and Romano, 2003).

In this literature, the focus has mainly been on school quality differentials and related dynamics of households sorting by socio-economic status. Little attention, instead, has been devoted to the possible role of fixed costs in schooling provision and the public policies implemented in order to reduce them. Exceptions in this respect are Urquiola (2005) and Alesina et al. (2004), arguing that school fixed costs make average cost decrease in district size. These works, however, are mainly concerned with the formation of jurisdictions (school districts) in response to the trade-off between scale economies and the costs of community heterogeneity, overlooking the consequences of public interventions intended to minimise fixed costs - infrastructure maintenance and teachers' expenses - which may induce the closure of undersized schools.[5]

Another aspect largely overlooked by the literature is that of transport costs to access schools. These can play a relevant role in household location decisions and are strongly connected to the organisation of the school network. Many undersized schools are located in peripheral areas, so in these places school cuts are likely to increase transport costs to access schools considerably. In turn, this can induce households to reconsider their residential choices. The interaction between scale economies and transport costs is at the centre of the New Economic Geography

[5]While our focus is on school closures, it is worth mentioning that a related but different literature exists on the effect of school creation or school improvements. As an example, studies have focused on how school *construction* projects can impact home values and educational attainments (Cellini et al., 2010; Neilson and Zimmerman, 2014).

(NEG) tradition (Krugman, 1991). This literature strand[6] focuses on firm location choices and the key idea is that industries with increasing returns concentrate where they can gain larger market access, while serving peripheral areas thanks to decreasing transport costs. Under factor mobility and preferences for variety, households will relocate close to industrial centres, giving rise to a process of 'cumulative causation' that leads to a core-periphery pattern, whereby residence and industry are increasingly concentrated. In this literature, the public sector mainly enters through the provision of infrastructure to firms (Ottaviano, 2008). Residential choices are either confined to responses to wage differentials or neglected, assuming immobile workers.[7] However, core-periphery patterns may also be reinforced as a result of policies affecting the provision of public services (Ehrlich and Overman, 2020; Fretz et al., 2022). Accessibility to services - i.e. the availability of activities such as work, education, and health care, or the ease of reaching the location where such activities occur - can induce population movements and thus affect spatial inequalities (Kelobonye et al., 2020; OECD, 2021). Hence, government policies cutting undersized schools in places characterised by fewer schooling alternatives may induce households to relocate closer to other schools, fostering the concentration of people and services in more central areas to the detriment of more peripheral locations.

To the best of our knowledge, rationalisation policies have not been subject to any systematic evaluation in terms of household location choices. This paper aims to fill this gap by studying how household residential choices are affected by changes in the provision of public school services.

3 Institutional context

3.1 The Italian schooling system

Despite recent trends towards decentralisation, the Italian schooling system still displays a considerably centralised and unitary configuration.[8]. The national gov-

[6]For a theoretical review see Fujita and Thisse (2002), while for empirical works see Redding (2010).

[7]In this case, agglomeration derives from relocations of intermediate input firms as in Krugman and Venables (1995).

[8]For a historical perspective on Italian school design and achievements, see Checchi et al. (2007) In more recent years, in line with the trend towards 'regionalisation' of the whole public system, some jurisdictional powers have been transferred from the central government to local authorities.

ernment has authority over the general norms in the field of education, including the definition of school programmes, quality standards and their evaluation (Di Giacomo and Pennisi, 2012). Moreover, it regulates and directly manages the recruitment and payment of the schooling personnel, which constitutes the largest component of the expenditure for education.[9]

The first educational cycle includes preschool (*scuola dell'infanzia*), primary school (*scuola elementare*) and lower secondary school (*scuola secondaria di primo grado*). Primary school and lower secondary school are mandatory, whereas preschool is not. The vast majority of pupils of the relative schooling ages attend public schools[10]. These are mainly managed by the central government, with the exception of some residual municipal preschools and schools of any order in the autonomous regions of Trentino-Alto Adige and Valle d'Aosta.

The Italian system allows for school choice. Parents can enrol children in their preferred school, even in municipalities different from the one they reside in.[11] In making primary school choices for their children, parents have to combine work and family needs. Primary school is mandatory, it lasts five years, and children attending it largely depend on their parents for daily commuting. As a consequence, house-school commuting times become particularly relevant in orienting residential choices. Conversely, school *quality* appears less of a determinant for selecting primary schools. This is because in the Italian context there is no school tracking in educational offer over the first educational cycle[12], so that in principle school

Since the 1990s, the establishment of school autonomy and the 2001 reform of the Italian Constitution have contributed to such a process.

[9]In all OECD countries, school expenditure accounts for 90% of current expenditures. Four-fifths of that amount consist of personnel's wages. Compared to other OECD countries, in Italy the unbalanced expenditure distribution in favour of school personnel is even more marked (MIUR, 2007).

[10]More than 70% of pupils enrolled in preschools attend public schools. The percentage rises to over 90% for primary and lower secondary education (ISTAT data available at dati.istat.it).

[11]If the chosen school happens to be oversubscribed, the priority is given to pupils residing in the school's catchment area. Each school institution has to declare its admission criteria in case of over-subscription. On admission rules, see Ministry of Education document 22994 for school year 2020-21: miur.gov.it/web/guest/-/iscrizioni-alle-scuole-dell-infanzia-e-alle-scuole-di-ogni-ordine-e-grado-anno-scolastico-2020-2021.

[12]Over the first educational cycle (i.e., pre-schools, primary and lower secondary schools) educational offer is rather uniform across schools. Conversely, higher secondary school displays relevant school tracking, with multiple educational programmes offered to students. Note that, for our purposes, school quality differentials are relevant only in case they influence the decision of closing schools. Neither official documents nor informal interviews with school directors mention students' performance as a criterion orienting the decision of closing schools. Building conditions or the presence of additional school services, such as gyms and canteens, can be thought of as features influencing closure decisions. By implementing a fixed effect model, we already account for stable differences in school quality across municipalities. Therefore, only *variations* in school quality differential might represent a confounding factor. We further discuss this point in section 5.

quality of primary schools is approximately equalised, at least within provinces. Indeed, the strongest evidence of sorting across schools on the basis of school quality is visible at the level of higher secondary school (*scuola secondaria di secondo grado*), whereas it does not seem particularly relevant for the first educational cycle (Bertola and Checchi, 2004; Brunello and Checchi, 2007).[13] In conclusion, at least for the first educational cycle, residence and school choice are not completely independent. It seems plausible that households take into account distance to school when evaluating residential decisions.

The distribution of schooling services across the country depends on laws regulating two fundamental aspects: the criteria for class formation and the guidelines for the organisation of the school network. Concerning the former, since 2009 class formation is regulated nationally by the Ministry of Education (MIUR) through decree 81/2009, part of the 'Gelmini reform'. The guidelines for the organisation of school networks are provided by each Italian region, independently for its own territory, and they contain directives on activation, suppression and merger of school complexes. According to such guidelines, the annual school sizing regional plan (*Piano di dimensionamento scolastico regionale*) is agreed upon by the regional government based on inputs received from each province composing the region.

In defining these plans, regional authorities are constrained by the number of public school workers assigned to each region by the central government. The binding constraint to class and school activation is represented by the scarcity of teachers and janitors, which are the more valuable and costly resource of the schooling system.[14] In this framework, each individual school has little control over its own activation and/or suppression. School workforce is assigned on the basis of student enrolments (*organico di diritto*) and then adjusted to cover particular and transitory needs, determining the effective personnel for the school year (*organico di fatto*). Therefore, despite the formal decentralisation of power on these matters to regional authorities, the central government's reforms crucially affect the organisation of the school network.

[13]The possibility of choosing to attend a primary school outside the municipality pupils reside in would constitute a downward bias for our estimates on the impact of school closures, as pupils would be unaffected by the closures of schools in their residing municipalities. A more extensive discussion on this is in section 4.

[14]Those resources are financed by the national government, whereas local authorities - for the first educational cycle, municipalities - are in charge of school buildings and finance their maintenance.

3.2 School rationalisation policy: the 'Gelmini Reform'

The Italian school system has been historically characterised by a high degree of territorial dispersion, following the polycentric distribution of the Italian population. However, since the 1950s Italian demography has considerably changed, increasing the population of already larger cities to the detriment of more peripheral areas. In addition, since the 1990s, policies of rationalisation started to be implemented in the field of public services, including public education. In this regard, the last noticeable turn occurred after the 2008 crisis with the 'Gelmini reform' (from the name of the then Minister of Education), which led to a relevant contraction of the school network, both in terms of the number of school complexes (i.e. single or multi-school structures) and classes activated (MIUR, 2010). Indeed, by 2008 rationalisation policies had mainly intervened to reduce autonomous school institutions[15], but they had not strongly affected the distribution of school complexes. The territorial fragmentation of school complexes and the limited class size were identified as the main reasons for the high per-pupil expenditure compared to OECD countries (Fontana, 2008; MIUR, 2007).

The reform process started with law 133 of August 2008, which established the increase by one percentage point of the pupils-teacher ratio and the elaboration of a strategic plan (*piano programmatico*) to achieve a "more rational use of human and material resources" in the schooling system, from which public savings for 8 billion euros by 2012 were expected.

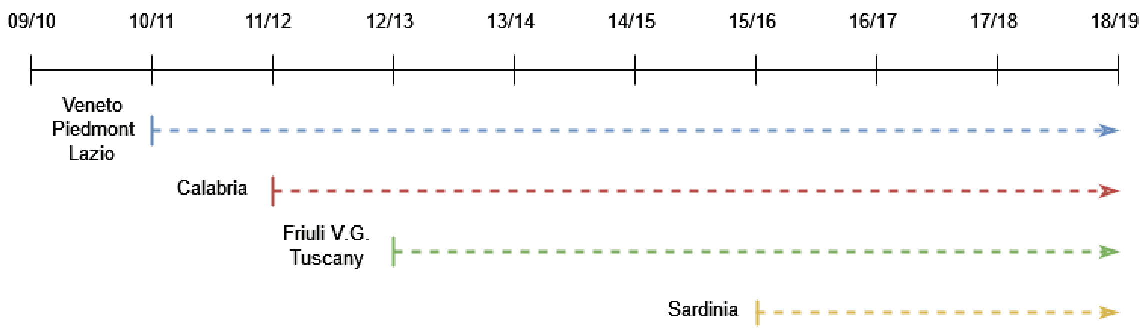
The Ministry declared the need to eliminate undersized school complexes. For that purpose, regions were allowed to establish numerical criteria for the activation or suppression of school complexes.[16] Some regions formulated general norms for the organisation of the school network, including directives towards a more rational distribution of school complexes, to be achieved through the suppression of the undersized ones. Other regions introduced proper numerical criteria to determine

[15] Autonomous school institutions are legal entities which comprehend multiple school complexes. They are managed by a single school director, who has - in principle - some autonomy in the organisation of the member schools. School autonomy was introduced in the Italian system by law 21/1997.

[16] "The institution, suppression, or merger of schools is under the jurisdiction of regions [...] on the basis of sizing criteria defined by the Ministry of Education" (*Schema di Piano Programmatico del Ministero dell'Istruzione, dell'Università e della Ricerca di concerto col Ministero dell'Economia e delle Finanze.*). This is a quote from decree 81/2009, revising the numerical limits to form 1st-year classes, determining the increase in pupils/class ratio, and allowing for exceptions only in case of growing schooling population (*Norme per la riorganizzazione della rete scolastica e il razionale ed efficace utilizzo delle risorse umane nella scuola.*). It still constitutes the normative reference for class formation in all regional guidelines for the elaboration of sizing plans.

whether a school should be suppressed. This kind of school sizing threshold has been introduced by seven Italian regions over the period considered: Veneto, Piedmont, Lazio, Calabria, Friuli Venezia-Giulia, Tuscany, and Sardinia. The timeline of regional interventions varies, and it is displayed in Figure 1. These criteria consist of thresholds on the minimum number of required students in order to keep a school active.[17] In addition, some regions specify that a full cycle of five years has to be in place for the school to remain active and/or that the formation of multi-grade classes is not allowed. In primary schools the cutoff is mostly fixed at 50 students, the only exceptions being Piedmont and, since 2018, Tuscany, which set up a threshold of 35 students.[18]

Figure 1: Timeline for the introduction of regional thresholds



The graph reports the school year in which different regions introduced numerical thresholds for school closure over the period considered.

4 Data and sample

The dataset for the analysis has been obtained from a variety of sources. To begin with, data on active schools have been provided to us by the Italian Ministry of Education (MIUR - *Ufficio Gestione Patrimonio Informativo e Statistica*) for the 2009-2019 period, and they refer to the activity of preschools, primary and lower secondary schools. They cover the entire population of public and private Italian schools at fine geographical details (street address). MIUR represents the most reliable source of information about the Italian schooling system. We exclude from

[17] These rules generally apply to the whole region but there are some minor exceptions, allowing for smaller number of students in mountain or island schools.

[18] Apulia had numerical thresholds in its sizing plans until 2011. Since our analysis starts in 2010, we exclude that region when focusing on the sub-sample of those adopting thresholds. More details and guidelines for regional sizing plans can be found on the regions' websites or requested to competent regional offices.

our analysis the regions of Trentino-Alto Adige and Valle d'Aosta because school policy in those two regions is regulated by the jurisdiction of their autonomous provinces.

We look at the impact of the closure of *primary* schools and use municipalities as units of analysis.[19] Therefore, for each municipality in the sample, primary school closure represents the treatment.

To identify school closures, we exploit the information about the location of each school and the universal coverage of our data. Data are available annually from school year 2009/2010 to school year 2018/2019. School sizing plans for a given school year are approved by December of the previous year, meaning that if, for instance, the school complex is not activated for school year 2010-2011, the decision about the closure is taken in December 2009 and the announcement is made at the beginning of 2010. The school closes in June 2010 and students have to find a new school for school year 2010-2011, starting in September 2010.

Our goal is to examine the effect of school closures on population dynamics. As for the outcome variable, we have collected data on residential population at the municipal level from the Italian Institute of Statistics (ISTAT).[20] These are administrative data reporting yearly statistics on residents in each municipality on the 1st of January of each year, sub-divided by age class.

We focus on two age groups in particular. The first is the residential population in mandatory school age (5 to 14 years old)[21], which we assume is directly affected by primary school closures. The second is the group including the pupils' potential parents, which we identify as individuals between 35 and 49 years old, who possibly became parents between 25 and 44 years old.

We also explore income-related outcomes, namely total and per-capita municipal income. For that, we have extracted information on taxable income at municipal level from the Italian Ministry of Economy and Finance for the period 2010-2019.[22] This information comes from households' tax records and it is then aggregated at

[19]In Italy, municipalities are the smallest local authorities. In the period considered, Italian municipalities are around 8,000. 4,004 of them have a single primary school in 2010, with an average size of 29 square kilometres. Further summary statistics for this set of municipalities are reported in Table A1.

[20]Historical data on municipal demography is available at demo.istat.it/archivio.html.

[21]In fact, mandatory school age ends at 16. Our choice of focusing on the population between 5 and 14 years old is due to the fact that we are constrained by the age groups definitions provided by ISTAT and we want to include only mandatory-school-age pupils.

[22]Data are publicly available at www1.finanze.gov.it.

the municipal level. We compute per-capita income by dividing overall municipal income by the number of taxpayers.

From ISTAT we also collect data on the Local Labour Market (LLM, *Sistema Locale del Lavoro*) each municipality belongs to, in order to control for labour market conditions.[23].

We complete the dataset with information on municipal public expenditures for primary schools, available from the Italian Ministry of Interior's *Certificati Consuntivi*[24], yearly, until the year 2015. Italian municipalities' balance sheets are subdivided into two different categories, current and capital expenditures. The dataset is further disaggregated into different functions and sub-functions. The one we are interested is 'Primary School', a sub-function of total spending for 'Education'.

Crucially, to define the sample of municipalities for the analysis, we focus exclusively on municipalities that have *only one* primary school within their borders at the beginning of the sample period, i.e. school year 2009/2010. We exclude the few municipalities that have undergone processes of administrative reorganisation - i.e. merging over the period considered - so we can easily trace the municipal unit over the entire period considered. In order to capture the effect of school closures where it is expected to be stronger, namely in localities where there are no other public options locally available, we also exclude municipalities increasing their primary school endowment over the period considered. In this way, we make sure we compare municipalities that keep their single primary school for the entire span with municipalities where the only school closes and does not re-open over the observed period.

School sizing plans for a given school year are approved by December of the year preceding the closure, meaning that if, for instance, the school complex is not activated for school year 2010-2011, the decision about the closure is taken and announced in December 2009. For a school that closes at the end of school year 2009/2010, if residents decide to relocate *after* the closure, they will do so starting from the second half of the year 2010, because school years end in June. Given that we observe the number of residents at the beginning of each year, to associate population trends and closures correctly, in our municipality-year dataset we con-

[23]Data can be found at istat.it/it/informazioni-territoriali-e-cartografiche/sistemi-locali-del-lavoro LLM boundaries are re-defined every census. Given the period of analysis, we refer to the 2011 LLM definition.

[24]The *Certificati Consuntivi* dataset has been widely employed in the literature. Please refer to Di Cataldo and Mastrorocco (2022) for a detailed description of the data.

sider the municipality with the school closing in June 2010 as having a primary school until 2010 (included) and lacking any school from the start of 2011.

In municipalities endowed with a single primary school, residents in key age classes are the likely recipients of given school services and arguably they represent the population that would be most affected by school closure. In this respect, the possibility of school choice - i.e. the fact that individuals can decide to attend primary schools outside the municipality they reside in - would constitute a downward bias for our estimates. If some primary school-age residents are attending school in a municipality in which they do not reside, they will not be affected by school closure in their residing municipality, hence biasing downward the magnitude of the estimated effect of school closure on municipal residents.

To provide visual representations of the Italian school network, Figure A2 in the Appendix plots the geographical distribution of primary schools by municipality in the first school year considered, i.e. 2009/2010. Most Italian municipalities are endowed with at most one primary school (light yellow areas). They make up 57% of all coloured municipalities in the Figure. The set of single-school municipalities is shown in Panel a) of Figure 2. In this Figure, red municipalities are those experiencing school closures during the time span considered (treated units), whereas the green ones are those that do not (control units). Panel b) of Figure 2 restricts the sample to single-school municipalities from regions adopting numerical thresholds for school sizing over the period considered. As can be seen from the map, they are fairly evenly distributed across the whole Italian territory, as regions from the north, centre, and south of the country are all represented. In 2010, 20% of the Italian population was living in single-primary-school municipalities (Panel a); 7% when focusing only on regions adopting school thresholds (Panel b). As visible in Tables A1 and A2, reporting key summary statistics for the variables in our sample, the characteristics of single-primary-school municipalities in regions adopting thresholds seem largely comparable to those in the full sample.[25]

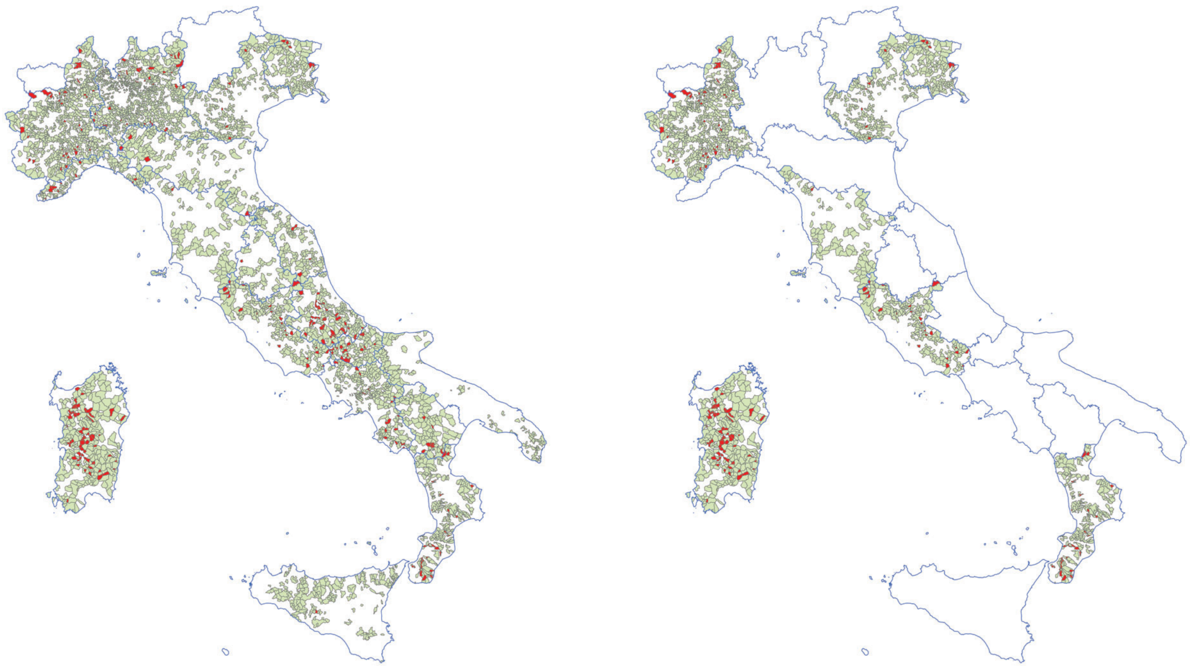
The choice of focusing on the restricted group of municipalities with only one primary school clearly reduces the number of observations, as compared to a sample considering multiple-schools municipalities. However, we expect any effects of closures to be visible particularly in municipalities lacking easy alternatives to the closed schools. By restricting the analysis to municipalities with a single primary school in 2009/2010, we are left with a total of 4,004 municipalities, of which

[25] For a description of all the employed variables and their relative sources see Table A3 in the Appendix.

Figure 2: Single-primary-school municipalities - closures

a. all sample

b. regions with thresholds



The map in Panel a shows all single-primary-school municipalities, reporting in colour red those experiencing school closures over the period considered (2010-2019), and in colour green those that do not. The map in Panel b reports the same exact information, only displaying the single-primary school municipalities of regions which introduced numerical thresholds for school sizing over the span considered: Veneto, Piedmont, Lazio, Calabria, Friuli Venezia-Giulia, Tuscany, and Sardinia..

271 experienced primary school closures during the period of analysis. They are distributed across regions as shown in Table 1, reporting in bold the regions introducing specific numerical criteria for school closures.

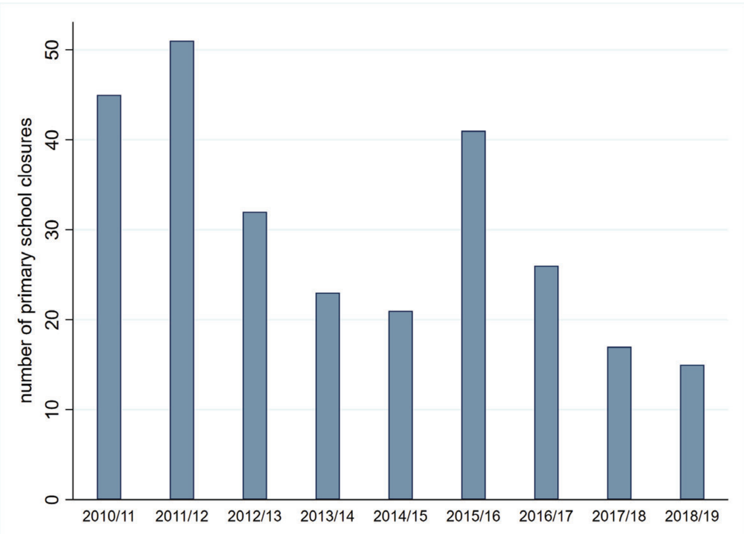
The timing of school closures is also relevant. Figure 3 shows the number of closures by year in the sample of municipalities with a single primary school in 2009/2010. We can notice a concentration of cases of closure in the first three years. The period 2010-2012 coincides with the time horizon indicated by the 'Gelmini reform' for collecting 8 billion euros in public savings through the policy of rationalisation.

Table 1: Single-primary-school municipalities - closures by region (2010-2019)

Region	No closure	Closure	Total	% closures over total
Abruzzi	159	29	188	15.43
Apulia	108	1	109	0.92
Basilicata	88	8	96	8.33
Calabria	190	22	212	10.38
Campania	276	19	295	6.44
Emilia Romagna	140	3	143	2.10
Friuli V.G.	128	5	133	3.76
Lazio	205	22	227	9.69
Liguria	126	9	135	6.67
Lombardy	922	35	957	3.66
Marche	136	4	140	2.86
Molise	82	16	98	16.33
Piedmont	649	35	684	5.12
Sardinia	214	53	269	19.85
Sicilia	174	1	175	0.57
Tuscany	96	1	97	1.03
Umbria	49	1	50	2.00
Veneto	262	7	269	2.60
Total	4,004	271	4,277	6.34

The Table reports, for each region, the number of municipalities endowed with a single primary school in 2009/2010, which experienced or not school closures over the period considered (2010-2019) and related percentage over total municipalities. Highlighted in bold are the regions introducing numerical thresholds for school sizing over the observed time span.

Figure 3: Single-primary-school municipalities, closures by school year



The figure shows the number of primary school closures in single-primary-school municipalities over the period considered.

5 Empirical strategy

5.1 Two-Way-Fixed-Effects model

Our sample consists of municipalities with only one primary school experiencing the closure of that school - an event which can take place at any moment during the 2010-2019 sample period - and municipalities with one school that does not close during the period of analysis. As such, the setting lends itself to a difference-in-differences (DID) type of strategy, with staggered treatment adoption (Goodman-Bacon, 2021).

Formally, we estimate:

$$y_{icrt} = \alpha + \beta\, Closure_{icrt} + \gamma_i + \delta_{ct} + \eta\, X_{icrt} + \epsilon_{icrt} \qquad (1)$$

where i is the municipality identifier, t is the year, c is the LLM, and r the region to which the municipality belongs. Equation 1 refers to our starting model, where we regress our outcomes of interest (population in key age classes and municipal income) on a treatment dummy for school closure ($Closure_{icrt}$), municipal (γ_t) fixed effects, year-local labour markets (LLM) interacted fixed effects (δ_{ct}), and a set of controls (X_{icrt}). The inclusion of both municipality and year fixed effects entails that the specification takes the form of a Two-Way-Fixed-Effects model.[26] The treatment variable $Closure_{icrt}$ takes value 1 from the school year in which the only primary school in the municipality has closed until the end of the period, and 0 before that.[27] The model controls for complementary and substitute school services X_{icrt}: the endowments of public pre-schools, public lower secondary schools, and private schools of any order (primary schools included).[28] Year-LLM interacted

[26]Working with a municipality-year panel, we employ the terminology 'Two-Way-Fixed-Effects' with reference to municipality and year fixed effects. On top of these, we also add LLM dummies interacted with year dummies, in order to control for time-varying LLM characteristics. We also estimate a more traditional TWFE model with only municipality and year fixed effects included (Table A5 in the Appendix).

[27]The treatment dummy is constructed to make sure that population dynamics and closures are associated correctly in our annual dataset. As per ISTAT measurement, municipal residents each year correspond to the total residents in a given municipality on January 1^{st}. Closures occur in June. If a school is absent from the dataset starting from school year 2010/2011 (it closed in June 2010), the dummy $Closure_{ict}$ takes value 1 from 2011. The total residents of 2011 are therefore observed 6 months after the closure of that school.

[28]Private schools can represent a substitute service for public ones, and they may even endogenously respond to the closure of public schools. In Italy, however, private school enrolment is very residual at primary school level. Over 90% of primary-school pupils are enrolled in public schools (ISTAT data). Moreover, in our preferred sample of single-primary-school municipalities in regions

fixed effects δ_{ct} are included in the model to restrict comparisons of treated and control units to municipalities exposed to the same labour market conditions and control for any time-varying factors within local labour market.[29] Standard errors are clustered at the municipality level.

Note that possible stable differences in school quality are accounted for by municipality fixed effects. Moreover, the inclusion of year-LLM interacted fixed effects controls for variations in school quality that concern the entire LLM, such as natural events that may damage school buildings. Therefore, the only residual concern about the possible confounding role of school quality may lie in idiosyncratic variations at municipal level influencing both the decision of closing schools and residential choices. Such residual concerns are addressed by the instrumental variables strategy (see sub-section 5.3).

The key identifying assumptions underlying TWFE models is the absence of anticipation effects and the parallel trend in the evolution of treated and control outcomes prior to treatment adoption. The plausibility of those assumptions is generally inspected by looking at pre-treatment coefficients of an event study of the following form:

$$y_{icrt} = \alpha + \Sigma_{m=-G}^{M} \beta_m \, z_{icr(t-m)} + \gamma_i + \delta_{ct} + \eta \, X_{icrt} + \epsilon_{icrt}, \tag{2}$$

where the term $\Sigma_{m=-G}^{M} z_{icr(t-m)}$ refers to a set of leads and lags dummy variables before and after the treatment event (school closure), capturing the possible dynamic effects of the treatment. Specifically, the outcome at time t can only be directly affected by the value of the policy at most $M \geq 0$ periods before t and at most $G \geq 0$ periods after t (Freyaldenhoven et al., 2021). All the pre-treatment coefficients should be non-significant for the parallel trends assumption to hold. Indeed, the estimated $\{\beta_m\}_{m=-G}^{M}$ can be interpreted as the cumulative effect of the policy up to period $(t-m)$. The significance of pre-treatment coefficients would highlight pre-trends in the outcome.

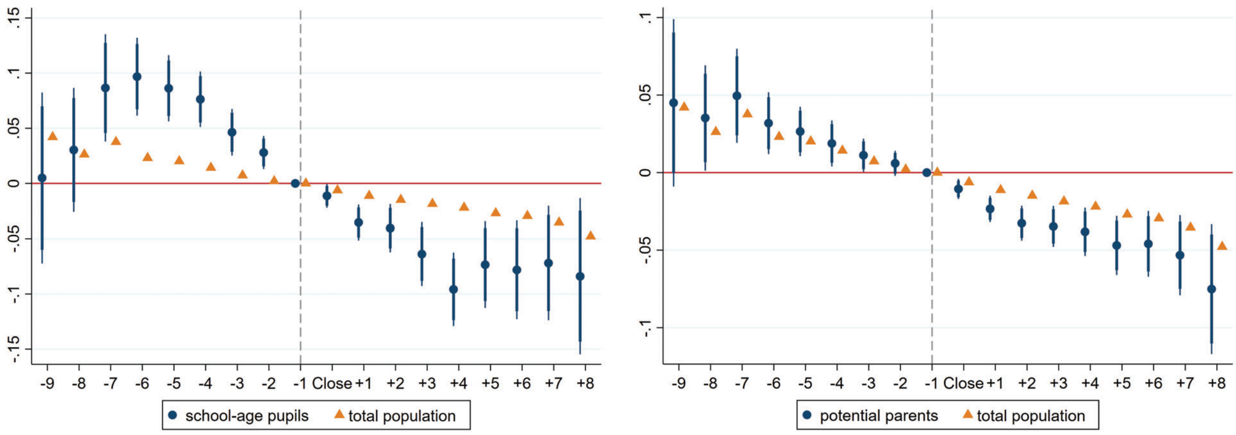
adopting thresholds, we have at most one private primary or lower secondary school, and four private preschools (see Table A2). In that sample, municipalities experiencing primary school closures do not have any private primary or lower secondary school, and have at most one private preschool.

[29] One possible concern is that the inclusion of LLM fixed effects generates problems of double counting in case changes of residence mainly involve adjacent municipalities. We verify that our estimates are not inflated by double counting by taking out LLM dummies from the baseline specification, therefore allowing for comparisons of treated and control units across different LLMs. The related results are reported in Table A5 in the Appendix.

We report the event study plots estimating equation 2, providing a visual intuition of the plausibility of the identifying assumptions, in Figure 4. The $\{\beta_m\}_{m=-G}^{M}$ coefficients are estimated with three different dependent variables: the population of school-age children, total residents, and the population of potential parents.

Figure 4: Population by age classes around school closure

a. school-age pupils b. potential parents



The Figure shows event study plots corresponding to equation 2, using as dependent variable (log) total and school age population (Panel a) or (log) total population and potential parents, i.e. residents between 35 and 49 years old (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

As can be seen from the plots, all outcomes show pre-trends, which can be due either to anticipatory responses or to pre-existing depopulation trends in single-school municipalities experiencing school closures. Both explanations are plausible in our context. Indeed, school cuts may be discussed for some time before being actually put in place and young adults are likely to adapt their fertility and/or residence choices according to the expected change. Moreover, by definition school rationalisation policies affect municipalities in population decline, and this constitutes the greatest challenge for the parallel trend assumption to be met. School cuts take place precisely where the demand for school services is shrinking, making its provision inefficient. The pre-trends displayed in Figure 4 confirm this.

There is a growing literature discussing identification issues due to treatment effect dynamics in setting with staggered adoption (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). These contributions highlight that heterogeneity in treatment effects across cohorts may represent a bias in such context, as event study estimates of pre-treatment periods can be contaminated by post-treatment effects, invalidating the common procedure of testing for pre-trends by looking at pre-treatment coefficients (Sun and Abraham, 2021). We follow this literature strand and adopt the estimator proposed by Sun and Abraham (2021), allowing to compute event studies as weighted averages of cohort-specific $ATTs$, with weights corresponding to the shares of treatment cohorts. The corresponding event study plots are displayed in Appendix Figure A4. As visible, these estimates confirm the presence of significant pre-trends, indicating that identification concerns are not resolved by accounting for treatment heterogeneity. Instead, pre-trends are likely to derive from a combination of anticipatory behaviour and pre-determined municipal demographic conditions.

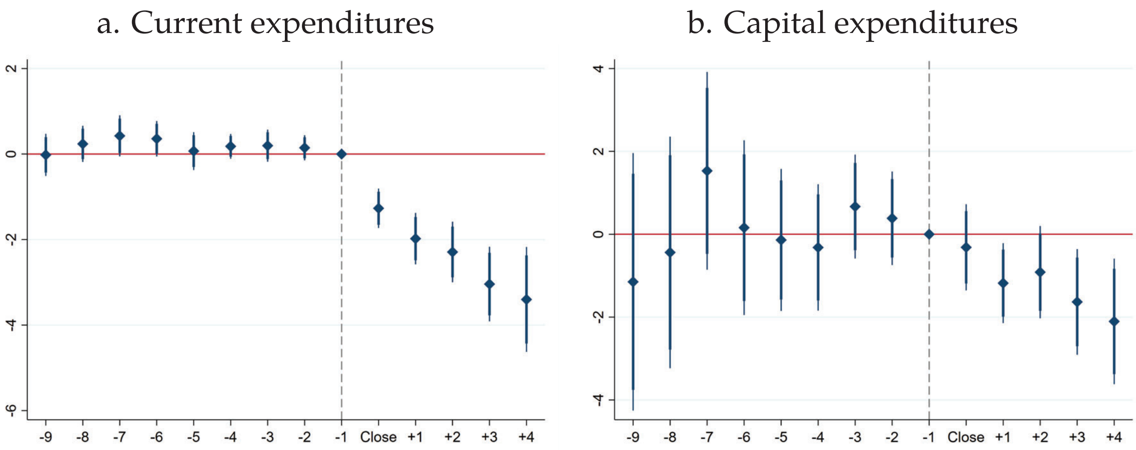
5.2 Fixed costs of primary schools

Using the same kind of event study model we can also visualise the fixed costs of primary schools, whose reduction was the purpose of the Gelmini reform. Exploiting data on municipal public accounts, available until the year 2015, we can look specifically at primary school spending at the municipal level and observe its variation year-by-year before and after school closure. Hence, we re-estimate model 2 using (log) current and capital expenditures for primary school per-inhabitant as outcomes, for our sample of single-primary-school municipalities.[30] The estimates are reported in Panels a and b of Figure 5. The corresponding event studies using Sun and Abraham (2021) estimator are in Figure A5 in the Appendix.

No coefficient of dummy variables referring to the pre-closure period emerges as statistically significant, suggesting that spending patterns of treated and control municipalities are very similar prior to school closures. Primary school budgets of municipalities mainly concern school infrastructure maintenance and utility bills, while school personnel is financed by the central government. As infrastructure maintenance and utilities represent fixed costs independent of school size, it comes as no surprise that pre-closure spending appears to be evolving similarly across treatment and control units. Even if school population is decreasing in the years preceding closure, these expenditures are constant as long as the only primary school in the municipality is active. As expected, we observe a very sharp reduction of expenditures for primary schools following the closures, both in the current and the capital expenditures of the treated municipalities.

[30]Current expenditures refer to spending for ordinary management (e.g. public employees' salaries, maintenance, rents for public buildings) and it is generally low-changing or constant, while capital expenditures refer to public investments (e.g. public procurement tenders, building acquisition) and it is more likely to be fluctuating and characterised by peaks and lows.

Figure 5: Municipal expenditures for primary schools



The Figure shows event study plots corresponding to equation 2, using as dependent variable (log) current expenditures for primary schools per inhabitant (Panel a); (log) capital expenditures for primary schools per inhabitant (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

5.3 Instrumental Variable models

To address possible anticipation effects and reverse causality, we combine the TWFE estimation presented above with Instrumental Variable (IV) strategies.[31] For our IV models, we exploit the institutional rules on school sizing adopted by seven Italian regions over the period considered. Therefore, we restrict the analysis to the sample of regions adopting school sizing thresholds, illustrated in Panel b of Figure 2.

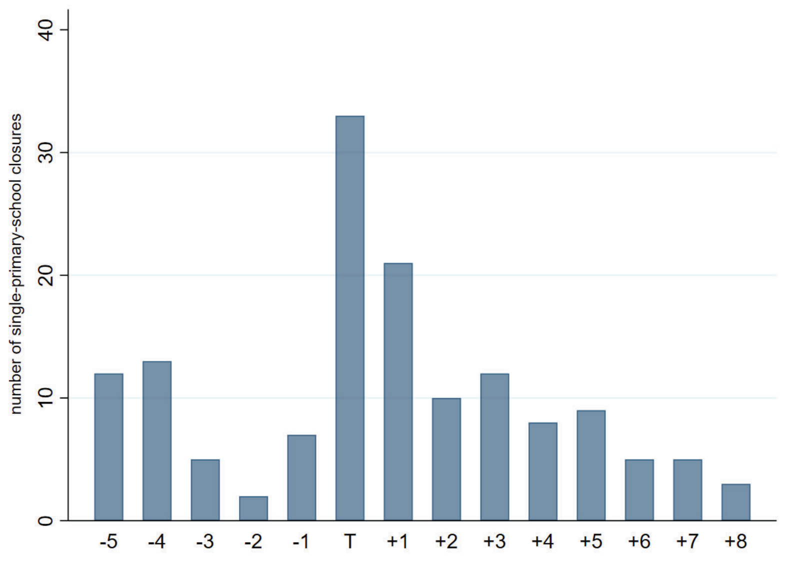
School sizing thresholds were adopted in different years by the various regions and, once activated, applied to all schools within the region. Figure 6 shows the number of single-primary-school closures by relative year before or after the introduction of the threshold. It can be noticed that, in the very first school year since their implementation, these thresholds produced a marked increase in school closures.[32]

We leverage this setting and implement two complementary IV models. Firstly, we

[31] The combination of TWFE and IV strategies is proposed and discussed by Freyaldenhoven et al. (2021). Examples of its applications are Besley and Case (2000) and Jackson et al. (2016).

[32] In the years preceding threshold introduction, we can still notice some closures, in particular 4 and 5 years before. Those values correspond to Sardinia in school years 2010-2011, and 2011-2012. In those years, the rationalisation effect of the 'Gelmini' reform was the strongest, as can be observed by looking at the overall number of school closures in Figure 3.

Figure 6: Number of school closures before/after threshold introduction



The figure shows the number of school closures by relative school year before/after the introduction of school sizing thresholds. Sample of single-primary-school municipalities in regions adopting thresholds. T = school year of threshold introduction.

construct the following instrument:

$$Dummy\ IV_{icrt} = S_{icr,2010} \cdot T_{rt} \tag{3}$$

where $S_{icr,2010}$ is a dummy variable taking value one if school *i* in local labour market *c* and region *r* was below the regionally-set threshold on school size in the first observed school year, 2009/2010, and T_{rt} is a dummy taking value 1 from the school year in which a threshold for school closure has been introduced in region *r* until the end of the period.[33] While all regional thresholds have been introduced years after 2010 (see the timeline in Figure 1), we still refer to the school conditions in 2010 to construct the IV. Therefore, the instrument is constructed as a dummy variable taking value 1 from the moment of the introduction of the regional threshold if the school was below that threshold in the pre-threshold year 2010, and 0 before. Figure A3 in the Appendix displays the municipalities above/below the threshold according to 2010 school characteristics.

The choice of employing school characteristics in 2010 instead of contemporane-

[33]We need to associate correctly the timing of threshold introduction, closures, and population measurement. If a threshold is introduced from the school year 2010/2011, in our annual dataset T_{rt} will take value 1 in 2011, where we observe population at the beginning of the year 2011. Similarly, if a school is closed from school year 2010/2011, $Closure_{icrt}$ will take value 1 from 2011.

ous ones is expected to make the IV more exogenous. Parents may react even to the *risk* of school closure induced by the presence of the threshold by sending their children to other schools, making contemporaneous school characteristics endogenous. However, by 2010 none of the sample regions had introduced numerical criteria for school closure yet. Therefore, taking school characteristics prior to the introduction of thresholds mitigates the concerns of endogenous household response.

We then estimate a TSLS model, where the treatment variable $Closure_{icrt}$ is instrumented by the $Dummy\, IV_{icrt}$. Specifically, we estimate:

$$y_{icrt} = \alpha + \beta\, \hat{Closure}_{icrt} + \gamma_i + \delta_{ct} + \eta\, X_{ict} + \epsilon_{icrt} \tag{4}$$

where $\hat{Closure}_{ict}$ is predicted from the first stage equation

$$Closure_{icrt} = \mu + \nu\, Dummy\, IV_{icrt} + \rho_i + \tau_{ct} + \varphi\, X_{icrt} + \upsilon_{icrt} \tag{5}$$

We run the above specification for the full sample of single-primary-school municipalities in regions adopting thresholds. Moreover, we restrict the estimation to schools closer to the regional threshold, in order to focus on a more homogeneous group of schools and municipalities. We exploit the symmetric window of ± 50 students around the threshold.[34] In the main analysis, we select a bandwidth of 50 students above and below the threshold, while Appendix Table A8 reports the estimates for windows of ± 45 and ± 40 students, to check the sensitivity of our results to alternative bandwidth choices. Estimations on the restricted sample around the threshold have greater internal validity, since we compare schools with a similar number of students. Conversely, full sample estimations entail greater external validity, since bigger schools are included in the control group. We conduct the analysis comparing treated and control municipalities within the same region, which mitigates possible concerns related to the different number of sample units in the various regions.

To provide evidence on the validity of the IV, we perform event studies of reduced form estimates for a model mirroring equation 2, where instead of computing leads and lags referring to each year before/after school closure, we look at periods before/after the introduction of the threshold. These estimates allow to observe the

[34]The 50-students bandwidth is selected because regional thresholds are mostly fixed at 50 students. In fact, selecting the ± 50 only entails excluding the largest schools, as there is no school with less than 50 students below the regional threshold.

evolution of the outcome variables around the threshold introduction event. We would expect to see no pre-trends as a sign of no difference between municipalities whose school was below a school-sizing threshold, before its introduction, and municipalities whose school was above it.

In this reduced-form setting, the verification of the parallel trend assumption can be interpreted as a test for instrument exogeneity. It should be noted that, due to the way in which the instrument is constructed, we do not have staggered IV adoption within regions. For all municipalities of a given region whose school is below the future threshold in 2009/2010, the instrument takes value one from the moment a threshold is introduced until the end of the period. Our analysis is performed within-region, since we impose LLM-year fixed effects and LLMs are partitions of regions.[35] Therefore, for these reduced-form regressions, we should not face treatment heterogeneity issues potentially associated with TWFE models with staggered adoption and we employ the traditional event study estimator.

Figure 7 present the results of these estimates in the form of event study plots, using the restricted sample of schools/municipalities around the threshold and population outcome variables - school-age and potential parents' population. Figure A6 in the Appendix reports analogous plots for total and per-capita income. Overall, we find no significant pre-threshold differences in terms of demographic or income for municipalities below the threshold, suggesting that the instrument is exogenous.

As a form of placebo test, we estimate the event study of the reduced form model using the population between 55 and 65 years old as dependent variable. We expect such age class to be little or no affected by the introduction of school-sizing thresholds, since these individuals are too old to be parents of primary school children. Most people in that age group are still in the labour market. Therefore, if our estimates were driven by labour market dynamics affecting residential choices, we should find an impact also on that population sub-group.[36] As shown in Figure A7 in the Appendix, all coefficients of post-threshold dummies are insignificant, indicating no effect of the introduction of school thresholds on this age group.

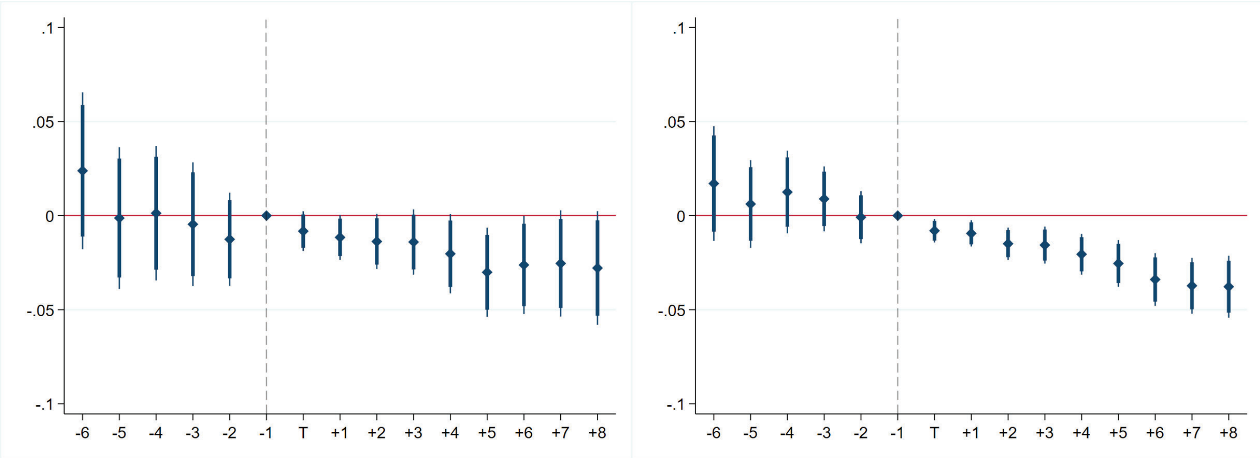
One residual concern could be the presence of differential trends in outcome evo-

[35] There exist some LLMs which spread across regional borders. However, in our restricted sample we just have four of these cases and we exclude them from sample.

[36] An alternative placebo age class is that of residents between 20 and 35 years old, presumably too young to be parents of primary school-age children. People in this age group may still value the presence of a school if they plan to have children, but they are not immediately affected by school closure. At the same time, they are generally more mobile than elderly people ISTAT (2019). Estimates using the 20-35 years old age group as placebo are reported in Table A7 in the Appendix.

Figure 7: Event study plots of the reduced-form estimation: population

a. School-age population b. Potential parents

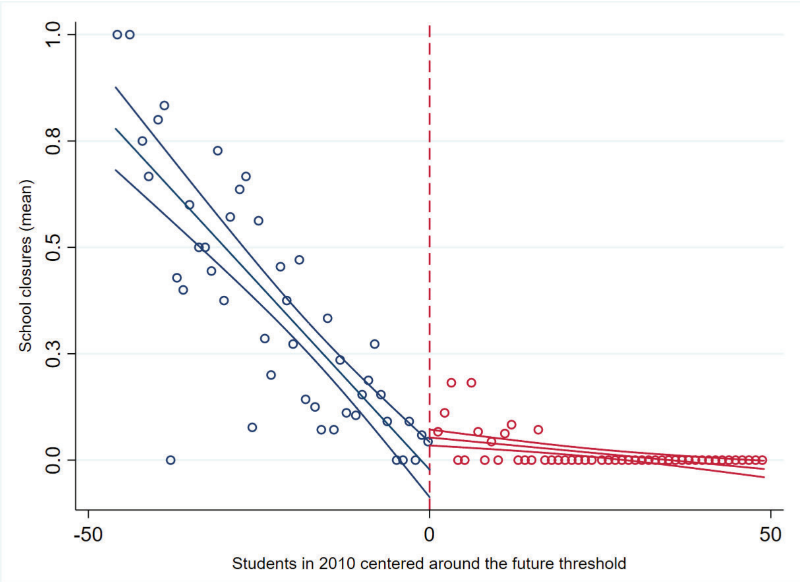


The Figure shows the event study plots corresponding to the reduced form of equation 2, where dependent variable is (log) population in school age (Panel a) or (log) population of potential parents, i.e. residents between 35 and 49 years old (Panel b). Those outcome variables are regressed on leads and lags of the instrument. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

lution depending on *how far* below the threshold the school was in 2010. If the margin of deviation from the threshold correlates with the predictive capacity of our main instrument (equation 3), this would create an omitted variable problem.

Using the information on the number of students in primary schools, we can test for a significant difference in the probability of closure around the school-sizing regional threshold. We centre the number of students around the threshold and show schools with up to 50 students above/below the threshold. The number of students refers to the first year in sample, school year 2009/2010. Figure 8 plots the probability of experiencing school closures over the time span considered. It shows no evidence of a significant difference in treatment probability at the regional school sizing threshold's cutoff. However, we do observe a significant difference in derivatives at the cutoff. The likelihood of closure increases with the distance from the threshold on the left-hand side of the graph, i.e. for schools with fewer students than the threshold. We also note that there are schools below the threshold which do not close, and schools above the threshold that experience school closure. This is mainly due to the fact that we are taking school characteristics in 2010. Most closures above the regional threshold refer to schools that lose students after 2010 and were below the threshold when they close. Overall, this evidence is consistent with the existence of some margins of negotiation and discretion at the regional level in the choice of closing schools.[37]

Figure 8: Probability of closure by deviation from the regional threshold



The graph reports the mean of school closure for different levels of deviation from the regional school-sizing threshold. The deviation is measured as the number of students enrolled in the school in 2009/2010 minus the value the region will adopt for school-sizing threshold.

To account for differential trends in outcome evolution depending on the initial distance from the threshold, as a robustness strategy we construct an alternative instrument. The second TSLS model incorporates the deviation from the threshold for the construction of the IV, by multiplying our previous dichotomous instrument by the number of students in school year 2009/2010. Formally:

$$Kink\ IV_{icrt} = (Students - c)_{icr,2010} \cdot S_{icr,2010} \cdot T_{rt} \tag{6}$$

where $(Students - c)_{icr,2010}$ is the number of students in 2009/2010, in deviation from the future regional threshold; $S_{icr,2010}$ is a dummy variable taking value one if school i in local labour market c region r was below the regional threshold, according to school characteristics in 2009/2010; and T_{rt} is a dummy for the introduction of a threshold for school closure in region r, year t. In practice, this $Kink\ IV_{icrt}$ is a continuous variable resulting from the interaction between $(Students - c)_{icr,2010}$

[37]While school directors and local authorities do not have much room to attract students and therefore manipulate their position with respect to the regional school sizing threshold, they can negotiate with regional decision-makers to keep undersized schools open. In this sense, their main limitation is the total school personnel the National Government has assigned to that region. It seems plausible that the more undersized a school is, the lower the probability that it can be kept open in derogation from institutional rules.

and the $Dummy\ IV_{icrt}$.

We label it 'kink' because it exploits the kink in treatment probability at the cutoff shown in Figure 8. This strategy draws insights from the kink RDD, a recent advancement of the RDD approach in which identification is based on discontinuity in derivatives - rather than levels - of treatment probability at the cutoff.[38] Here we exploit the slope change in closure probability at the threshold to construct the IV.

We perform the estimation of the impact of school closure, instrumenting it with the kink IV, using the full sample of single-primary-school municipalities in regions adopting thresholds. For the exclusion restriction to hold, we need to control for the number of students, as this plausibly correlates with our outcomes and it is included in the kink instrument. Therefore, not accounting for it would cause the instrument to directly predict our dependent variables. We augment the specification of equation 1 with the interaction between the number of students in 2009/2010 and the dummy for the regional threshold being active. In a context with municipality fixed effects, this time-varying interaction term can be interpreted as a running variable capturing the underlying relationship between the number of students and the outcome at the policy change. Formally:

$$y_{icrt} = \alpha + \beta\, \hat{Closure}_{icrt} + \gamma_i + \delta_{ct} + \eta\, X_{icrt} + (Students - c)_{icr,2010} \cdot T_{rt} + \epsilon_{icrt} \quad (7)$$

where $\hat{Closure}_{ict}$ is obtained from the following first stage regression:

$$\begin{aligned} Closure_{icrt} = \mu + \nu\, (Students - c)_{icr,2010} \cdot S_{icr,2010} \cdot T_{rt} + \rho_i + \tau_{ct} \\ + \varphi\, X_{icrt} + (Students - c)_{icr,2010} \cdot T_{rt} + \upsilon_{icrt} \quad (8) \end{aligned}$$

[38]Among the proponents of this design are Dong (2018) and Dong and Lewbel (2015), who build on the existing knowledge on RDD to get identification even in absence of a jump, and to derive conclusions about the effect of interest away from the cutoff. Different applications of the kink RDD estimation strategy exploit continuous rather than binary treatments (Nielsen et al., 2010; Card et al., 2015).

6 Main results

6.1 Instrumental variable estimates

In this section, we present the main results of the paper. All estimates are performed on a set of dependent variables measured at the municipality level: the school-age population, potential parents, total and per-capita income, and elder population. We always include municipality fixed effects, LLM-year dummies, school endowment controls, and we exclude cross-regional LLMs.

We report the OLS estimates of the TWFE model presented in equation 1 in Table A4 of the Appendix, both for the full sample of all regions (Panel a) and the restricted sample of all single-primary-schools in regions with thresholds (Panel b). The results, remarkably similar across samples, display negative coefficients linking school closure with school-age population, potential parents, and total income, while no relationship with per-capita income and elder population. We cannot interpret these coefficients causally due to the pre-trends visible in Figure 4.

We address the endogeneity induced by pre-trends with IV estimates. First, in Table 2 we present first stage results from equations 5 and 8, to provide evidence of the relevance and strength of our instruments $Dummy\, IV_{irt}$ and $Kink\, IV_{irt}$. Column 1 refers to the sample of all single-primary-school municipalities in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of school year 2009/2010. For both samples, the instrument is a good predictor of the probability of treatment. The F-test is well above the conventional value of 10, meaning that we can safely exclude weak instrument concerns. For single primary schools, being below the threshold in 2009/2010 increases the probability of experiencing school closure by 15%. The relatively small size is determined by the fact that there is significant non-compliance below the threshold - some undersized schools are kept active in derogation from regional rules. In addition, there is some non-compliance above the threshold, i.e. schools closing while being above the threshold in 2009/2010. This is mostly due to the way in which the instrument is constructed. We mark as 'above thresholds' schools that were so in 2009/2010, but then decline in enrolments, and - once below the threshold - close. Column 3 of Table 2 reports the first stage results of equation 8. The negative sign of the estimated coefficient of the kink instrument in the Table relates to the negative slope of the left-side plot of Figure 8. Once the threshold is active, the lower the number of students below the cutoff, the greater the probability of closure.

Table 2: First stage results

	School closure		
Dummy instrument	0.146*** (0.0139)	0.149*** (0.0173)	
Kink instrument			-0.010*** (0.00087)
Other school endowments	✓	✓	✓
Municipality fe	✓	✓	✓
LLM-year fe	✓	✓	✓
Running variable			✓
F-test on instrument	109.02	73.48	123.36
N	18,330	11,290	18,330

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Columns 1 and 2 report first stage estimates corresponding to equation 5, regressing school closure on the instrument dummy variable, taking value one if the school was below the regional threshold in 2010, from the year of its introduction. Column 1 refers to the sample of all single-primary-schools in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Column 3 reports first stage estimates corresponding to equation 8, regressing school closure on the kink instrument: the interaction between the deviation from the regional threshold in 2010 and the dummy instrument. All specifications include controls for other school endowments, municipality and LLM-year fixed effects; column 3 includes distance from threshold in 2010 interacted with threshold introduction (labeled running variable).

In Table 3 we report second-stage estimates corresponding to equation 4. The coefficients represent the average percentage variation over the post-treatment period in municipalities experiencing school closures, relative to the pre-closure period and to municipalities not experiencing school closures. Panel a refers to the sample of all single-primary-school municipalities in regions adopting thresholds, while Panel b restricts the sample to schools with up to 50 students above or below the regional threshold as of school year 2009/2010. In Table A8 of the Appendix, we report results from analogous estimations using bandwidths of 45 or 40 students above/below the threshold.[39]

[39]Results for those alternative bandwidth choices largely confirm the estimated coefficients of Panel b in Table 3. The estimate for school-age population and a bandwidth of 45 students is marginally insignificant, with a p-value of 0.106, while the coefficient's size confirms the main estimate of Panel b, Table 3. The estimate for the 40-students bandwidth is significant and similarly sized. All other coefficients are comparable to those of Panel b - Table 3 in significance, sign and size.

Table 3: IV estimation, second stage results

	School-age population	Potential parents	Total income	Per-capita income	Elder population
Panel a: All single-primary-school municipalities in regions with thresholds					
School closure	-0.154***	-0.180***	-0.099***	0.050***	-0.030
	(0.0515)	(0.0330)	(0.0193)	(0.0129)	(0.0379)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	18,330	18,330	18,314	18,314	18,330
Panel b: Schools with up to 50 students above/below threshold					
School closure	-0.105*	-0.139***	-0.102***	0.018	-0.052
	(0.0602)	(0.0382)	(0.0237)	(0.0144)	(0.0467)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	11,290	11,290	11,284	11,284	11,290

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 4, regressing school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with a dummy indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a refers to the sample of all single-primary-schools in regions adopting thresholds; Panel b, instead, to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010.

The estimates in the first and second columns of panel a show a significant reduction of around 15-18% in school-age population and potential parents. When looking at the restricted sample of schools with up to 50 students above/below the threshold (Panel b), coefficients appear slightly smaller in size. We obtain a 10% reduction in school-age population and an 14% decrease in the population of potential parents.[40] To interpret the size of coefficients, we have to bear in mind that the sample is composed of small municipalities, with an average population of around 150 potential parents in the year preceding school closure. 18% of 150

[40] Table A6 in the Appendix reports the IV estimates including coefficients of the control variables, i.e. the time-varying endowments of public pre-schools and lower secondary schools, and private schools of any order.

corresponds to 27 residents, which could be parents of school-age children. We are dealing with approximately 10-13 couples. Therefore, even small reductions in absolute population appear considerable in relative terms. In practice, such reductions are likely to be highly relevant for these small municipalities that suffer from population decline.

Coefficients are equally signed but larger in (absolute) size compared to the OLS TWFE estimates of Table A4 in the Appendix. This is consistent with the correction of the downward-sloping pre-trends we achieve through the IV strategy.

As for the effect of school closures on income, the estimates in the third column of Panels a and b (Table 3) indicate that total income decreases by almost 13% in municipalities experiencing the closure of their only primary school, after the closure and relative to pre-closure and untreated municipalities. Per-capita income, instead, increases in these municipalities by 5% (Table 3, panel a, fourth column). This finding may result from the fact that re-locations mainly concern low-income households. School closures mostly affect young adults, who are highly concentrated in low-income classes and the positive coefficient on per-capita income may be ultimately due to the demographic effect detected on potential parents.[41] However, when we look at the restricted sample of schools with up to 50 students above/below the threshold (panel b), the coefficient on income per-capita loses significance.

Finally, the coefficient describing the impact of school closure using elder population as dependent variable (Table 3, last column) is statistically insignificant, confirming our prior that residents between 55 and 65 years old are not affected by school closures. This evidence supports our claim that the observed demographic dynamics are indeed due to school service cuts.

Next, in Table 4 we report the second stage results of the TSLS model instrumenting school closures with the 'kink' IV (equation 7). Since we adopt the number of students to construct the instrument, we cannot employ school-age population as dependent variable - we would have almost the same variable on both sides of the equation - and only use the population of potential parents and income. The model is estimated for the full sample of single-primary-school municipalities in

[41] To find more evidence on this, we have replicated event study estimates using the number of taxpayers in the lowest income class and the number of potential parents as outcomes. By 'low-income class' we mean households in the lowest category by annual taxable income as defined by the Italian Ministry of Economy and Finance (MEF), i.e individuals with an annual income between 0 and 10,000 euros. If we look at the resulting event study plot, displayed in Appendix Figure A9 we can note that the trajectories for these two groups overlap almost perfectly.

regions adopting thresholds. The results in Table 4 confirm that school closures do not affect elder population, consistent with our prior expectation, but they affect the residential choices of young adults (i.e. potential parents) inducing their relocation, which in turn reduces the overall income of municipalities experiencing closures. The effect on per-capita income is insignificant, as in panel b, Table 3.

Table 4: Kink instrument: second stage results

	Potential parents	Total income	Per-capita income	Elder population
School closure	-0.108***	-0.077***	0.003	-0.015
	(0.0309)	(0.0166)	(0.0103)	(0.0363)
Running variable	✓	✓	✓	✓
Other school endowments	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓
N	18,330	18,314	18,314	18,330

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Second stage results from the TSLS estimation of equation 7, where we regress different dependent variables (potential parents, total income, per-capita income and elder population) on school closure, instrumented with the interaction between the margin of deviation from the threshold and an indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. The sample includes all single-primary-schools in regions adopting thresholds.

6.2 Placebo closures

The main analysis focuses on the sample of single-primary-school municipalities. Our prior behind that choice is that school closures are particularly harmful in localities where no other local school options are available. An interesting verification test for such an hypothesis is to look at the effect of school closures in municipalities with more schools at the beginning of the period, losing just one of them and yet maintaining *some* local school services. If what matters for residential choices is the local availability of school services, we should not detect a negative impact of school closure when using this alternative sample.

We select municipalities endowed with one or two primary schools in school year 2009/2010 and remove from sample single-primary-school municipalities experiencing school closures.[42] Never-treated units are therefore municipalities with one or with two schools experiencing no closures over the period, while treated municipalities are those with two schools losing one of them during the observed time span. We re-estimate the TSLS model of equations 5 and 4, computing the dummy instrument as taking value one since the year of threshold introduction if at least one of the two schools was below the regional threshold according to 2009/2010 school characteristics. As for the main analysis, we further restrict the estimation to municipalities with at most 50 students above or below the regional threshold, in order to exclude large schools and thus make treated and control units more similar.[43]

Table A9 reports the related second stage results. None of the estimated coefficients appears significant. These findings support the claim that what matters for residential choices is access to school services. If there exist other school options locally available, individuals do not relocate after school closures. Up to this point, we have defined 'local availability' on the basis of municipal boundaries. In the next section, we expand on this and investigate how the effect varies depending on the distance to further public primary schools.

7 Who loses the most?

7.1 More peripheral and less peripheral municipalities

Our estimates have uncovered a clear effect of primary school closures on residential dynamics. Parents of school-age children and pupils appear to respond to unexpected school cuts by moving away from their place of residence. While this result has been obtained with a varied sample of single-school municipalities distributed across the whole Italian territory, it may differ depending on the pre-determined conditions of treated municipalities. In particular, more peripheral places located further away from economic centres and with less access to alternative school services may be most affected by the closures of their only primary school. Economic centres may not only act as substitutes for local services, but also as attractive poles, draining resources from more peripheral areas.

[42] We include single-primary-school municipalities without school closure to preserve sample size. Municipalities with two schools in 2009/2010 are just 11% of Italian municipalities, whereas single-primary-school municipalities are 28% of the total.

[43] To be conservative, for municipalities with two schools, we take the maximum value of deviation from the threshold.

In this section, we explore the heterogeneity of our general result with respect to the spatial conditions of treated municipalities, estimating the effect of school closures by sub-groups of municipalities, depending on their location.[44]

In order to capture municipal peripherality, we consider two different dimensions. We compute municipal distance in metres to the nearest centre of the Local Labour Market, and distance to the next available public primary school measured at the beginning of the period considered, school year 2009/2010. Distance to economic centres is computed as the distance in metres between the borders of the municipality representing the centre of the LLM and the borders of a given single-primary-school municipality. Distance to the closest school is measured by exploiting the exact geo-location of schools, computing the distance in metres between the closing school and the next one available. The median distance to LLM centres is 7.1 kilometres, while the median distance to the next primary school is 3.1 kilometres. By 'centre of LLM' we mean the municipality constituting the core of the corresponding LLM as identified by the Italian Institute of Statistics.

Next, for both these indicators, we divide our full sample of municipalities in sub-groups on the basis of their median value, to identify areas located close to (below median), or far from (above median) LLM centres or alternative primary schools.[45] Those two criteria do not overlap, as municipalities far from LLM centres are not necessarily also far from the closest available primary school, and vice versa (see Table A10 in the Appendix).[46]

By looking at the distance from LLM centres, we aim to capture the degree of centrality of the municipality and the differences in access to job opportunities. The predictions are not straightforward. On the one hand, being close to economic centres can entail better market access and reduced commuting time, which would mitigate the negative effect of school cuts. On the other hand, economic centres can exert a highly attractive force on nearby locations, while municipalities located far away from them might suffer less from congestion and provide better amenities, such as environmental quality. Distance to the nearest primary school, instead,

[44] In Table A12 of the Appendix, we repeat the heterogeneity analysis by interacting the school closure dummy with an indicator for the municipality being above the median distance from economic centres or alternative schools. Results are qualitatively equal to those obtained with the sample-split method.

[45] As a robustness check, we also subdivide the sample using the 25th or 75th percentile cutoffs. The results (available upon request) are stable across these alternative choices.

[46] Municipalities far from LLM centres are, on average, slightly smaller in size and more elevated - i.e. more often located in mountain areas - compared to close ones. They are also less populated at the beginning of the observed period. Municipalities far from the next available schools are on average more elevated than those close to the next schools, and larger.

can be seen as reflecting differentials in treatment intensity among municipalities. Our hypothesis is that the further away the next school is when the only available primary school closes, the higher would be the incentive for residents to relocate.

Table 5: School closure effect by municipality location

	School-age population		Potential parents		Total income	
	far	*close*	*far*	*close*	*far*	*close*
Panel a: LLM centres						
School closure	-0.166*	0.075	-0.160***	-0.045	-0.098***	-0.028
	(0.0950)	(0.1185)	(0.0541)	(0.0795)	(0.0328)	(0.0401)
N	5,900	4,980	5,900	4,980	5,900	4,978
Panel b: Next public school						
School closure	-0.180*	0.033	-0.160**	-0.075	-0.160***	-0.053
	(0.1039)	(0.1133)	(0.0623)	(0.0689)	(0.0399)	(0.0427)
N	4,650	4,630	4,650	4,630	4,646	4,630
Other school endowments	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓	✓

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from equation 4, where dependent variables are school-age population, potential parents and total income. Sample of schools with up to $\pm$ 50 students from threshold in 2010. In Panel a we subdivide our sample by distance to LLM centres and separately estimate equation 4 for municipalities above (far) or below (close) the median distance to LLM centres. In Panel b we follow an analogous procedure, considering instead distances to the closest public primary school.

Table 5 reports the results sub-dividing the full sample along these dimensions.[47] School-age population is the dependent variable in the first two columns, the population of potential parents is the dependent variable in the third and fourth columns, and total income is the dependent variable in the fifth and sixth columns. Reduced form event study plots showing the evolution of municipalities with schools below regional threshold in 2010 around the threshold introduction, for the two samples of municipalities far from SLL and far from the next available school, are displayed in Figures A10 and A11 in the Appendix.

[47] The estimates refer to the sample of schools less than 50 students above/below regional thresholds in 2010. Estimates with all single-primary-school municipalities are in Appendix table A11, while comparable estimates using interaction terms rather than sample splits are in Appendix table A12

The result of panel a, Table 5 seems to suggest that the whole result of school closures on residential dynamics and local income is driven by municipalities located far away from the centres of Local Labour Markets. This finding supports the view that households value proximity to economic centres. This presumably offers more and relatively accessible service and labour opportunities, which induces residents of nearby municipalities not to relocate when the school closes. On the contrary, the same cannot be said for municipalities too far from urban areas, where commuting is not much of an option. The estimates reported in panel b, instead, confirm our prior that the incentive to relocate after a school cut is stronger when the next primary school is located further away.

In summary, the evidence emerging from Table 5 suggests that school closures foster population decline and consequently reduce local income especially in peripheral locations. Hence, school cuts appear to harm locations which already had limited access to school services and job opportunities. The reduction in population and total income may in turn produce additional depressive effects on the municipality, in terms of reduced demand for local services, entrepreneurial capacity, and thus job creation. All this is in line with the idea that rationalisation policies in key public services affect territorial disparities, by widening the existing intra-regional gaps in terms of population growth and income.

7.2 Core and peripheral labour markets

It would be interesting to know whether school cuts only produce a re-distribution of population and income across municipalities or whether they also generate losses (or gains) on a more aggregate scale. While a complete welfare analysis is beyond the scope of this paper, we can give an initial indication of whether school closures have a population or income impact beyond municipal boundaries. Specifically, we investigate possible effects at the LLM level. In doing so, we define treatment in a cumulative way, summing up single-primary-school closures as they occur within the same LLM. For this definition of treatment, we cannot instrument closure with our proposed measure on the school being below the threshold in 2009/2010 (equation 3). Therefore, the related results are not soundly causal, and must be interpreted just as suggestive evidence. However, going for a simple OLS estimation allows us to enlarge our sample to all LLMs, independently from the number of single-school closures and from whether the corresponding region adopts any threshold over the period considered. Using the full sample of 530 LLMs from all

Italian regions (excluding Valle d'Aosta and Trentino Alto-Adige), we then estimate a TWFE model where we regress LLM-level population or income on the treatment measure defined above, control for school endowments at LLM-level, LLM fixed effects, region-year fixed effects, and cluster standard errors at LLM level. Formally,

$$y_{ct} = \alpha + \beta\, Closure_{ct} + \gamma_c + \delta_t + \eta\, X_{ct} + \theta_{rt} + \epsilon_{ct} \tag{9}$$

where c refers to LLM and r to region.

Table 6: Cumulative effect of school closures at Local Labour Market level

	School-age population	Potential parents	Total income	Per-capita income
Panel a: LLMs without provincial city				
Number of school closures	-0.0110***	-0.0060**	-0.0052***	0.0006
	(0.0030)	(0.0024)	(0.0018)	(0.0014)
Other school endowments	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓
Region-year fe	✓	✓	✓	✓
N	4,400	4,400	4,400	4,400
Panel b: LLMs with provincial city				
Number of school closures	-0.0036	-0.0007	0.0012	0.0013
	(0.0034)	(0.0030)	(0.0021)	(0.0017)
Other school endowments	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓
Region-year fe	✓	✓	✓	✓
N	870	870	870	870

Clustered standard errors at LLM level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Results from the OLS estimation of equation 9, regressing school-age population, potential parents, total and per-capita income - aggregated at LLM level - on the (cumulative) number of single-primary-school closures occurred in that LLM at any given year over the period considered (2010-2019). All specifications include controls for other school endowments - public and private -, LLM and Region-year fixed effects. The sample for this estimation includes all Italian LLMs, with the exceptions of those of Trentino-Alto-Adige and Valle d'Aosta and cross-region LLMs.

The results are illustrated in Table 6. When focusing on the sample of LLM with no provincial city (panel a), we find a negative relationship between school closures and population and income, which could signal a general decline of this type of labour market areas. Interestingly, however, the significant coefficient disappears when we focus only on LLMs containing a provincial city.[48] This evidence seems to support the view that only the most peripheral LLMs are negatively affected by school closures within their boundaries. Conversely, LLMs with provincial cities, which are generally sizeable urban areas, do not suffer negative consequences from the closure of primary schools in single-primary-school municipalities.

8 Concluding remarks

This paper has studied the local impact of spending cuts on public education services determining the closure of undersized schools. This kind of 'rationalisation policy' is designed to act precisely where demand for service is shrinking. As a consequence, its demographic and economic impact should not be uniform across space and be visible mainly in areas already lagging behind. If households relocate in response to service variations, this policy can lead to widening territorial disparities.

The analysis has provided some interesting insights in this regard. First of all, it has verified that school closures have occurred particularly in municipalities displaying negative pre-trends in the population of school service recipients, and that primary schools entail fixed costs for municipalities that are independent of their size. Second, it has demonstrated that school cuts affect population dynamics on top and beyond preexisting trends. In municipalities with only one primary school, the closure of that school translates into a 10-15% reduction in the population of children of mandatory school-age and in the population of potential parents, i.e. residents between 35 and 49 years old. Conversely, no significant effect is detected on the population plausibly still in the labour market but too aged to be parents of school-age children, in line with the hypothesis that post-closures demographic dynamic observed is indeed due to school closures and not to concurring economic changes. Third, the population decrease determines approximately a 10% reduction in taxable income in these municipalities.

The estimated effect of school closures on residential choices and income appears to be driven by peripheral municipalities, i.e. those located at a distance from the cen-

[48] In the period considered, Italy had 107 Provinces. Since we exclude the regions of Trentino-Alto Adige and Valle d'Aosta, we are left with 87 Provincial cities in our largest sample.

tre of local labour markets, or those with less access to alternative primary schools. When looking at a more aggregate scale, Local Labour Markets without urban centres acting as potential catalysers seem to be those losing out the most as a result of school closures. Hence, school service cuts appear to impact especially on locations which already had limited availability of school services and job opportunities. This loss of young adults and income may trigger a depressive effect on the local economy, further increasing the peripherality of already marginal territories.

The analysis has a number of limitations, including the fact that the sample used is made of single-primary-school municipalities only. As such, the results refer specifically to the impact of school closures on this type of local areas, while the effect of closing schools in larger municipalities with plenty of school alternatives may be different. It should be noted, however, that single-primary-school municipalities represent half of the total in Italy, hosting approximately 20% of the Italian population. In addition, as we are unable to follow individuals over time we cannot provide an accurate account of where they relocate as a result of school closures. We reserve the investigation of this aspect for the future.

Having acknowledged these issues, these results still have relevant policy implications. We have demonstrated that, while the closure of undersized schools is made with the intent of increasing aggregate efficiency at the national level, it can also affect population dynamics and the spatial distribution of income at the local level. This analysis does not aim to take a normative perspective by claiming that rationalisation policies are detrimental to people and places on an aggregate scale - this may well not be the case. Rather, our aim is to highlight possibly problematic side effects of these policies. The population sub-group most affected is that of young adults with children. These households are induced to relocate, draining valuable labour resources from peripheral areas and further depressing local demand. It might be the case that they enjoy better learning and working opportunities in larger urban areas, so that the aggregate gains of school service cuts outweigh the negative local impacts. Nevertheless, it is still worth highlighting the role of these policies for peripheral areas, as their decline may be problematic for a number of reasons. For example, not all their inhabitants may be equally equipped to respond to public service cuts - some households may face mobility constraints preventing them from relocating closer to services and economic opportunities. Alternatively, some people may have strong idiosyncratic preferences for living in those places, and be forced to move by the closure of key services. Finally, it is not obvious that bigger cities are prepared to host households re-locating from more peripheral areas due to the lack of local opportunities. These internal migrations - if not properly addressed by policy makers - can lead to congestion and worsened living conditions in larger cities. In conclusion, the local impacts of rationalisation policies are *per se* worthy of attention, both from an academic and a policy perspective. We leave a more thorough analysis of the overall costs and benefits of this kind of policy to future investigations.

References

Alesina, A., Baqir, R., and Hoxby, C. (2004). Political jurisdictions in heterogeneous communities. *Journal of Political Economy*, 112(2):348–396.

Banzhaf, H. S. and Walsh, R. P. (2008). Do people vote with their feet? an empirical test of tiebout. *American Economic Review*, 98(3):843–63.

Baum-Snow, N. and Lutz, B. F. (2011). School desegregation, school choice, and changes in residential location patterns by race. *American Economic Review*, 101(7):3019–46.

Bertola, G. and Checchi, D. (2004). Sorting and private education in italy. In *Education, training and labour market outcomes in Europe*, pages 69–108. Springer.

Besley, T. and Case, A. (2000). Unnatural experiments? estimating the incidence of endogenous policies. *The Economic Journal*, 110(467):672–694.

Black, S. E. (1999). Do better schools matter? parental valuation of elementary education. *The Quarterly Journal of Economics*, 114(2):577–599.

Brunello, G. and Checchi, D. (2007). Does school tracking affect equality of opportunity? new international evidence. *Economic Policy*, 22(52):782–861.

Brunner, E. J., Cho, S.-W., and Reback, R. (2012). Mobility, housing markets, and schools: Estimating the effects of inter-district choice programs. *Journal of Public Economics*, 96(7-8):604–614.

Callaway, B. and Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230.

Card, D., Lee, D. S., Pei, Z., and Weber, A. (2015). Inference on causal effects in a generalized regression kink design. *Econometrica*, 83(6):2453–2483.

Cellini, S. R., Ferreira, F., and Rothstein, J. (2010). The value of school facility investments: Evidence from a dynamic regression discontinuity design. *The Quarterly Journal of Economics*, 125(1):215–261.

Checchi, D., Fiorio, C., and Leonardi, M. (2007). *Sessanta anni di istruzione scolastica in Italia*. SIPI.

Di Cataldo, M. and Mastrorocco, N. (2022). Organized crime, captured politicians, and the allocation of public resources. *The Journal of Law, Economics, and Organization*, 38(3):774–839.

Di Giacomo, G. and Pennisi, A. (2012). Analisi dell'efficienza delle scuole italiane rispetto agli apprendimenti degli studenti. differenze territoriali e possibili determinanti. *Efficiency analysis of Italian schools: geographical differences and determinants", Studi e Pubblicazioni, MEF-RGS: Rome (Italy)*.

Dong, Y. (2018). Jump or kink? regression probability jump and kink design for treatment effect evaluation. unpublished manuscript.

Dong, Y. and Lewbel, A. (2015). Identifying the effect of changing the policy threshold in regression discontinuity models. *Review of Economics and Statistics*, 97(5):1081–1092.

Ehrlich, M. v. and Overman, H. G. (2020). Place-based policies and spatial disparities across european cities. *Journal of Economic Perspectives*, 34(3):128–49.

Epple, D. N. and Romano, R. (2003). Neighborhood schools, choice, and the distribution of educational benefits. In *The economics of school choice*, pages 227–286. University of Chicago Press.

Fack, G. and Grenet, J. (2010). When do better schools raise housing prices? evidence from paris public and private schools. *Journal of Public Economics*, 94(1-2):59–77.

Ferreyra, M. M. (2007). Estimating the effects of private school vouchers in multidistrict economies. *American Economic Review*, 97(3):789–817.

Fontana, A. (2008). La rete scolastica: un modello per il numero di studenti per classe nella scuola primaria e secondaria di i grado. In *Paper presentato alla XX Conferenza della Società italiana di economia pubblica. Pavia, settembre. y IP address*, volume 66.

Fretz, S., Parchet, R., and Robert-Nicoud, F. (2022). Highways, market access and spatial sorting. *The Economic Journal*, 132(643):1011–1036.

Freyaldenhoven, S., Hansen, C., Pérez, J. P., and Shapiro, J. M. (2021). Visualization, identification, and estimation in the linear panel event-study design. Technical report, National Bureau of Economic Research.

Fujita, M. and Thisse, J. (2002). *Economics of Agglomeration: Cities, Industrial Location and Regional Growth*. Cambridge University Press, Cambridge.

Gamper-Rabindran, S. and Timmins, C. (2011). Hazardous waste cleanup, neighborhood gentrification, and environmental justice: Evidence from restricted access census block data. *American Economic Review*, 101(3):620–24.

Gibbons, S., Heblich, S., and Pinchbeck, T. (2018). The spatial impacts of a massive rail disinvestment program: the beeching axe. *Working paper*.

Gibbons, S. and Machin, S. (2006). Paying for primary schools: admission constraints, school popularity or congestion? *The Economic Journal*, 116(510):C77–C92.

Gibbons, S., Machin, S., and Silva, O. (2013). Valuing school quality using boundary discontinuities. *Journal of Urban Economics*, 75:15–28.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*.

Hoxby, C. M. (2000). Does competition among public schools benefit students and taxpayers? *American Economic Review*, 90(5):1209–1238.

ISTAT (2018). Mobilità interna e migrazioni internazionali della popolazione residente. *ISTAT Statistiche report*.

ISTAT (2019). Iscrizioni e cancellazioni anagrafiche della popolazione residente anno 2018. *ISTAT Statistiche report*.

Jackson, C. K., Johnson, R. C., and Persico, C. (2016). The effects of school spending on educational and economic outcomes: Evidence from school finance reforms. *The Quarterly Journal of Economics*, 131(1):157–218.

Kahn, M. E. (2007). Gentrification trends in new transit-oriented communities: Evidence from 14 cities that expanded and built rail transit systems. *Real Estate Economics*, 35(2):155–182.

Kelobonye, K., Zhou, H., McCarney, G., and Xia, J. C. (2020). Measuring the accessibility and spatial equity of urban services under competition using the cumulative opportunities measure. *Journal of Transport Geography*, 85:102706.

Krugman, P. (1991). Increasing returns and economic geography. *Journal of Political Economy*, 99(3):483–499.

Krugman, P. and Venables, A. J. (1995). Globalization and the inequality of nations. *The Quarterly Journal of Economics*, 110(4):857–880.

MIUR (2007). *Quaderno bianco sulla scuola*.

MIUR (2010). *La scuola statale: sintesi dei dati a.s. 2009/2010*.

Nechyba, T. (2003). School finance, spatial income segregation, and the nature of communities. *Journal of Urban Economics*, 54(1):61–88.

Nechyba, T. J. (2000). Mobility, targeting, and private-school vouchers. *American Economic Review*, 90(1):130–146.

Neilson, C. A. and Zimmerman, S. D. (2014). The effect of school construction on test scores, school enrollment, and home prices. *Journal of Public Economics*, 120:18–31.

Nielsen, H. S., Sørensen, T., and Taber, C. (2010). Estimating the effect of student aid on college enrollment: Evidence from a government grant policy reform. *American Economic Journal: Economic Policy*, 2(2):185–215.

OECD (2021). *Access and Cost of Education and Health Services*, volume OECD Rural Studies.

Ottaviano, G. I. (2008). Infrastructure and economic geography: An overview of theory and evidence. *EIB papers*, 13(2):8–35.

Redding, S. J. (2010). The empirics of new economic geography. *Journal of Regional Science*, 50(1):297–311.

SNAI (2014). Strategia nazionale per le aree interne: definizione, obiettivi, strumenti e governance.

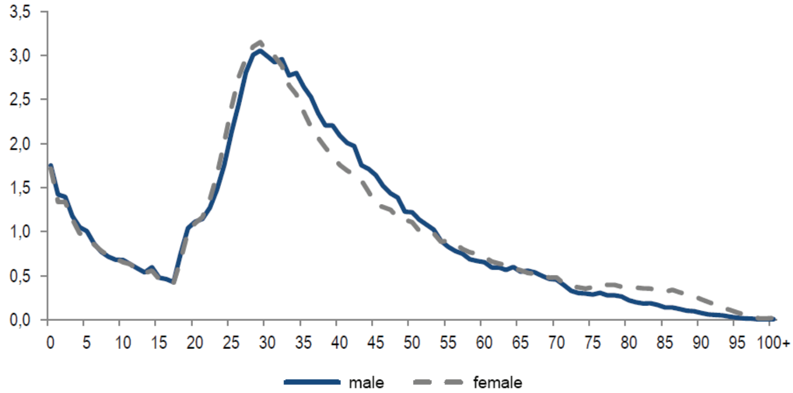
Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

Tiebout, C. M. (1956). A pure theory of local expenditures. *Journal of Political Economy*, 64(5):416–424.

Urquiola, M. (2005). Does school choice lead to sorting? evidence from tiebout variation. *American Economic Review*, 95(4):1310–1326.

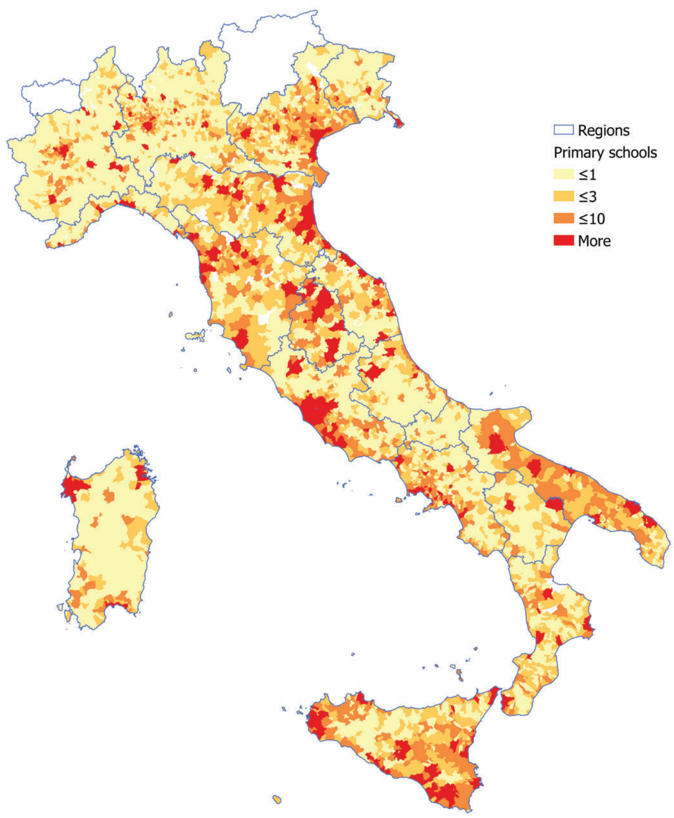
Appendix

Figure A1: Percentage of internal migrants by age in Italy



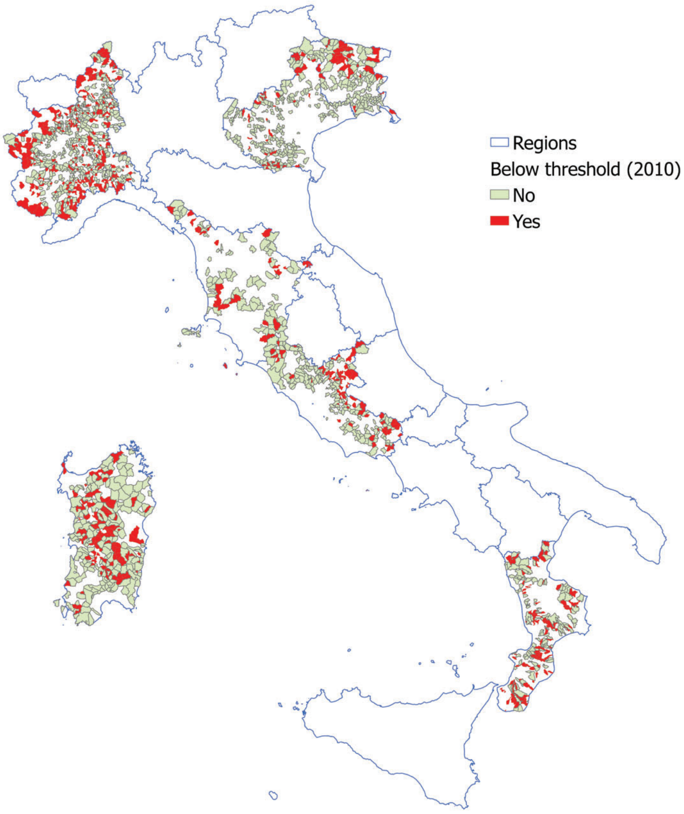
The Figure shows the distribution of internal migrations (i.e. changes of residence across Italian Provinces) in percentage values by age class (horizontal axis). Data refer to 2017. Source: ISTAT (2018).

Figure A2: Primary school endowments by municipality in school year 2009/2010



The map shows the distribution of public primary schools among municipalities in school year 2009/2010 (i.e. first year in our sample).

Figure A3: Single-primary-school municipalities above/below the regional school-sizing threshold, in regions adopting a threshold



The map shows single-primary-school municipalities in regions adopting thresholds over the period considered. The figure reports in green/red municipalities above/below the threshold according to 2010 school characteristics.

Table A1: Summary statistics: all single-primary-school municipalities

Variable	Obs.	Mean	Std. Dev.
School-age population	42,770	241.49	225.27
Population of potential parents	42,770	600.14	534.44
Elder population	42,770	342.82	275.88
Total population	42,770	2625.6	2190.1
Current expenditures per-capita	23,590	549.84	1221.1
Capital expenditures per-capita	23,590	806.83	5496.5
Total income	42,745	33489.8	32939.2
Numb. of taxpayers	42,745	1810.2	1507.4
Per-capita income	42,745	17233.4	4092.7
Numb. of low income taxpayers	42,745	569.22	439.34
Public pre-school	42,770	0.832	0.568
Public primary schools	42,770	.962	0.188
Public lower secondary schools	42,770	0.650	0.477
Private pre-schools	42,770	0.380	0.619
Private primary schools	42,770	0.0178	0.153
Private lower secondary schools	42,770	0.00846	0.0928
Distance to next school (2010)	38,050	3286.1	2157.9
Distance to LLM centre	42,770	7913.2	5304.7
Total population (2010)	42,770	2647.4	2158.6

Table A2: Summary statistics: single-primary-school municipalities in regions with thresholds

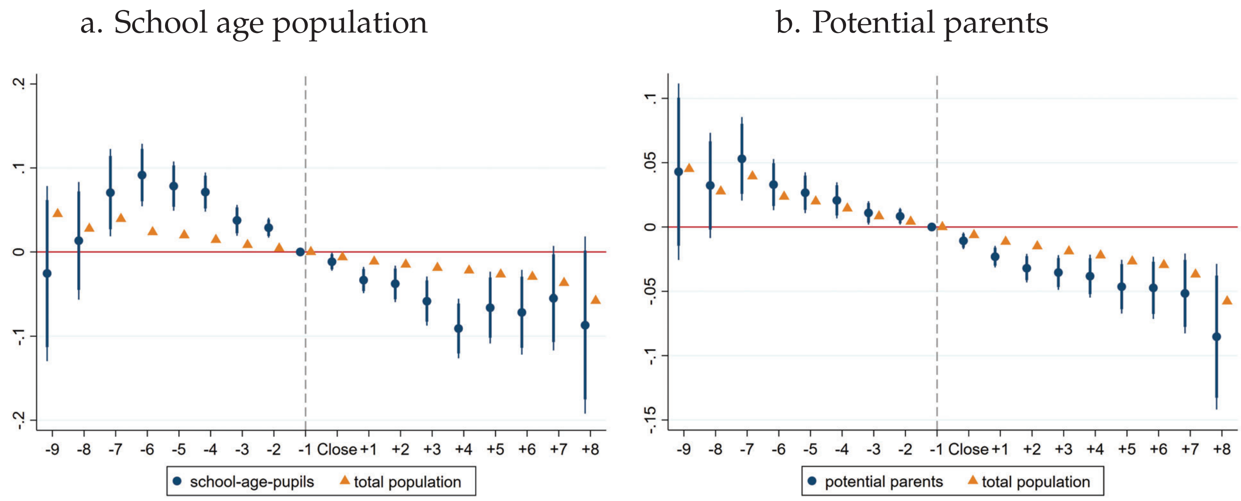
Variable	Obs	Mean	Std. Dev.
School-age population	18,900	197.69	180.56
Population of potential parents	18,900	509.755	454.3136
Elder population	18,900	304.07	245.78
Total population	18,900	2255.1	1879.8
Total income	18,884	28544.4	26797.7
Numb. of taxpayers	18,884	1572.4	1305.6
Per-capita income	18,884	17245.6	3691.6
Numb. of low income taxpayers	18,884	496.21	392.03
Public pre-school	18,900	0.806	.531
Public primary schools	18,900	0.951	0.214
Public lower secondary schools	18,900	0.606	0.489
Private pre-schools	18,900	0.324	0.551
Private primary schools	18,900	0.00862	0.0924
Private lower secondary schools	18,900	0.00671	0.0816
Primary school students (2010)	18,900	96.36	81.57
Primary school classes (2010)	18,900	6.06	5.26
Multi-grade classes (2010)	18,900	0.522	0.796
Distance to next school (2010)	16,800	3462.1	2146.3
Distance to LLM centre	18,900	8437.1	6078.6
Total population (2010)	18,900	2286.3	1862.2

Table A3: Summary statistics: variables description and source

Variable	Description	Source
School-age population	resident population between 5 and 14 years old	ISTAT
Population of potential parents	resident population between 35 and 49 years old	ISTAT
Elder population	resident population between 55 and 65 years old	ISTAT
Total population	total resident population	ISTAT
Total income	total taxable income	MEF
Numb. of taxpayers	number of taxpayers	MEF
Per-capita income	total taxable income/ number of taxpayers	MEF
Numb. of low income taxpayers	number of taxpayers with an annual income below 10,000 euros	MEF
Current expenditures	euros of current expenditures per-capita for primary school	MEF
Capital expenditures	euros of capital expenditures per-capita for primary school	MEF
Public pre-school	numb. of public pre-schools	MIUR
Public primary schools	numb. of public primary schools	MIUR
Public lower secondary schools	numb. of public lower secondary schools	MIUR
Private pre-schools	numb. of private pre-schools	MIUR
Private primary schools	numb. of private primary schools	MIUR
Private lower secondary schools	numb. of private lower secondary schools	MIUR
Primary school students (2010)	numb. of primary school students in school year 2009/2010	MIUR
Primary school classes (2010)	numb. of primary school classes in school year 2009/2010	MIUR
Multi-grade classes (2010)	numb. of primary multi-grade classes in school year 2009/2010	MIUR
Distance to next school (2010)	meter distance to the next available public primary school in 2010	MIUR
Distance to LLM centre	meter distance to the boundary of the closest LLM centre	ISTAT
Total population (2010)	total resident population in 2010	ISTAT

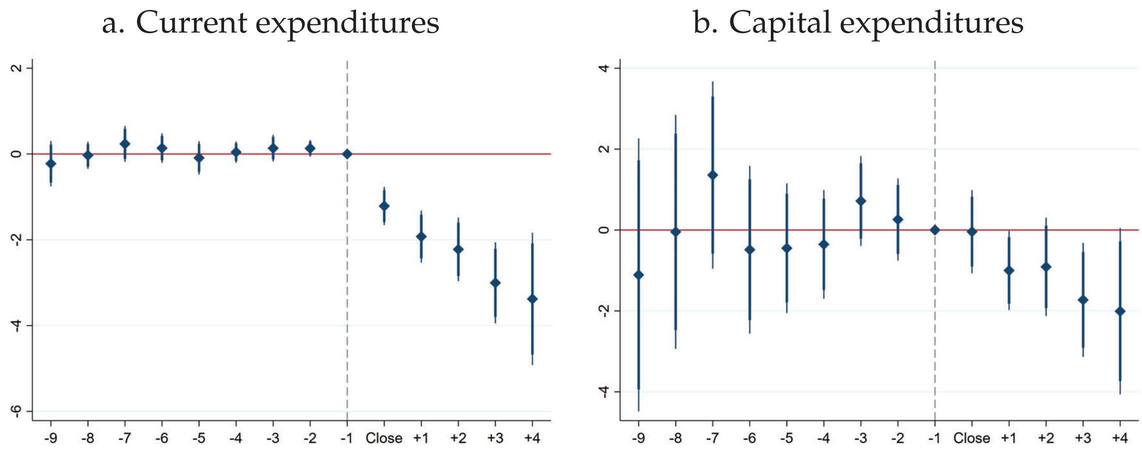
ISTAT: Italian Institute for Statistics; MIUR: Italian Ministry of Education; MEF: Italian Ministry of Economy and Finance

Figure A4: Population by age classes around school closure - Sun and Abraham (2021) estimator



The Figure shows event study plots employing the estimator proposed by Sun and Abraham (2021), which corrects for possible heterogeneous treatment effects across cohorts. Plotted coefficients relate to equation 2, where dependent variable are total and school-age population (Panel a) or total population and potential parents, i.e. residents between 35 and 49 years old (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

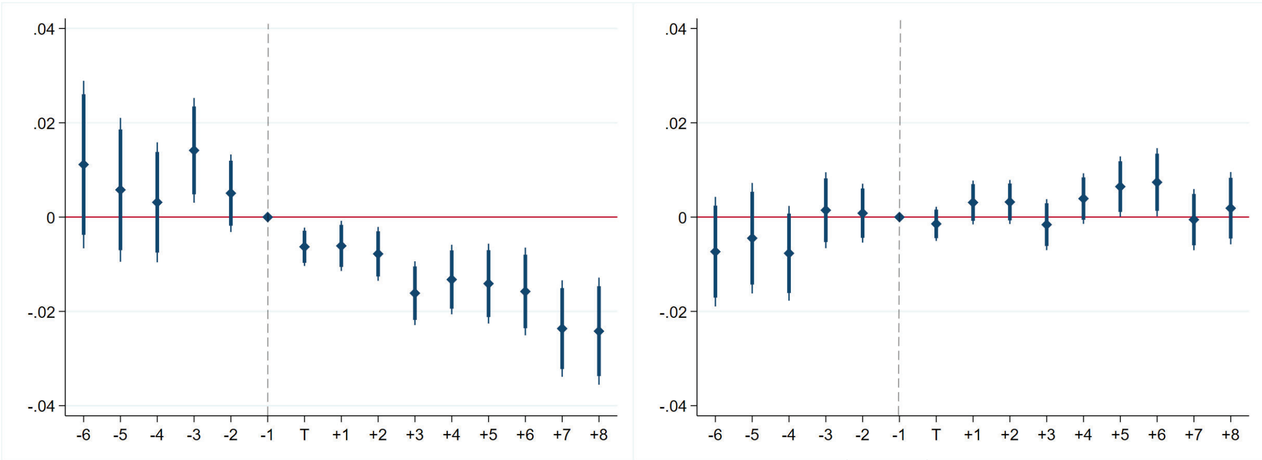
Figure A5: Municipal expenditures for primary schools - Sun and Abraham (2021) estimator



The Figure shows event study plots employing the estimator proposed by Sun and Abraham (2021), which corrects for possible heterogeneous treatment effects across cohorts. Plotted coefficients relate to equation 2, using as dependent variables: log current expenditures for primary schools per inhabitant (Panel a), log capital expenditures for primary schools per inhabitant (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

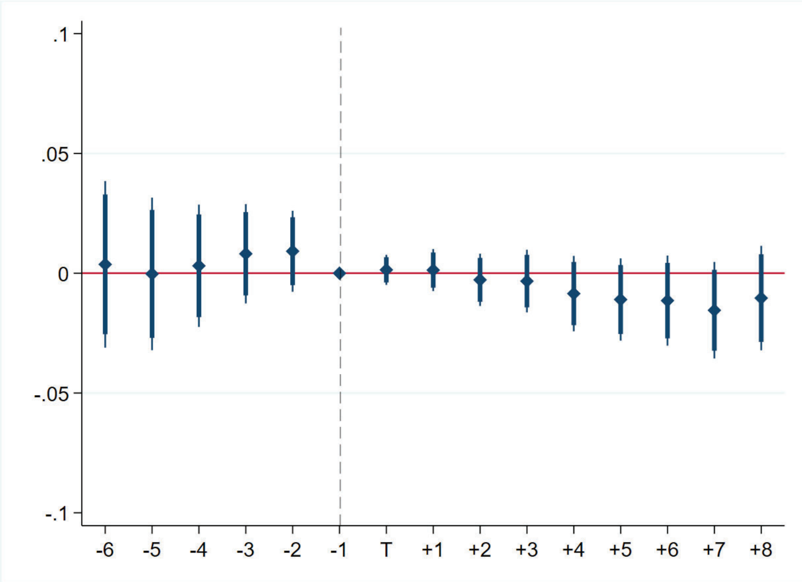
Figure A6: Event study plots of the reduced-form estimation: income

a. Total income b. Income per-capita



The Figure shows the event study plots corresponding to the reduced form of equation 2, where dependent variable is (log) total (Panel a) or (log) per-capita income (Panel b). Those outcome variables are regressed on leads and lags of the instrument. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A7: Event study plot of the reduced-form estimation: elder population



The Figure shows the event study plot corresponding to the reduced form of equation 2, where dependent variable is (log) population between 55 and 64 years old. The outcome variables is regressed on leads and lags of the instrument. We interpret the plot as a sort of placebo, since we do not expect residents in that age class to be affected by school closures, while they are plausibly still in the labour market. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Table A4: OLS estimates (TWFE model)

	School-age population	Potential parents	Total income	Per-capita income	Elder population
Panel a: full sample of all regions					
School closure	-0.080***	-0.038***	-0.016***	0.003	-0.009
	(0.0111)	(0.0057)	(0.0037)	(0.0025)	(0.0075)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	42,030	42,030	42,005	42,005	42,030
Panel b: regions with school-sizing threshold					
School closure	-0.070***	-0.028***	-0.016***	0.003	-0.006
	(0.0144)	(0.0082)	(0.0048)	(0.0032)	(0.0100)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	18,330	18,330	18,314	18,314	18,330

Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Results of the OLS estimation of equation 1 on the sample of single-primary-school municipalities in regions adopting thresholds for school sizing. We regress school-age population, potential parents, total income, per-capita income and elder population on school closure. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a: full sample of all single-primary-school municipalities; panel b: sample of all single-primary-schools in regions with thresholds.

Table A5: OLS and IV estimation without including LLM fixed effects

	School-age population	Potential parents	Total income	Per-capita income	Elder population
Panel a: OLS estimation					
School closure	-0.095***	-0.033***	-0.031***	-0.002	0.003
	(0.0141)	(0.0086)	(0.0053)	(0.0034)	(0.0104)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
year fe	✓	✓	✓	✓	✓
N	18,630	18,630	18,614	18,614	18,630
Panel b: IV estimation					
School closure	-0.325***	-0.229***	-0.239***	0.009	-0.056
	(0.0600)	(0.0391)	(0.0305)	(0.0131)	(0.0416)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
year fe	✓	✓	✓	✓	✓
N	18,630	18,630	18,614	18,614	18630

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The Table reports OLS (Panel a) and IV - second stage (Panel b) results, respectively from equations 1 and 4, but taking out from these specification LLM dummies. The estimation sample is that of all single-primary-schools in regions adopting thresholds.

Table A6: IV estimation, second stage results showing controls

	School-age population	Potential parents	Total income	Per-capita income	Elder population
School closure	-0.154***	-0.180***	-0.099***	0.050***	-0.030
	(0.0515)	(0.0330)	(0.0193)	(0.0129)	(0.0379)
Public pre-schools	0.019*	-0.001	-0.009*	0.003	-0.009
	(0.0116)	(0.0082)	(0.0049)	(0.0028)	(0.0086)
Public lower secondary schools	0.029	0.012	-0.004	0.005	-0.025**
	(0.0208)	(0.0127)	(0.0075)	(0.0050)	(0.0125)
Private pre-schools	0.030***	0.011*	0.007*	0.005*	-0.007
	(0.0095)	(0.0064)	(0.0039)	(0.0026)	(0.0068)
Private primary schools	0.013	0.001	-0.004	-0.013**	-0.014
	(0.0220)	(0.0204)	(0.0103)	(0.0058)	(0.0188)
Private lower secondary schools	0.035	0.010	-0.011	-0.004	0.054**
	(0.0301)	(0.0419)	(0.0133)	(0.0067)	(0.0250)
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	18,330	18,330	18,314	18,314	18,330

Standard errors clustered at municipal level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Second stage results from the TSLS estimation of equation 4, where we regress different school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with a dummy variable referring to the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Sample of all single-primary-schools in regions with thresholds.

Table A7: IV estimation, second stage results for alternative placebo outcome

	Resident population between 20 and 35 years old	
School closure	-0.023 (0.0351)	-0.044 (0.0417)
Other school endowments	✓	✓
Municipality fe	✓	✓
LLM-year fe	✓	✓
N	18,330	11,290

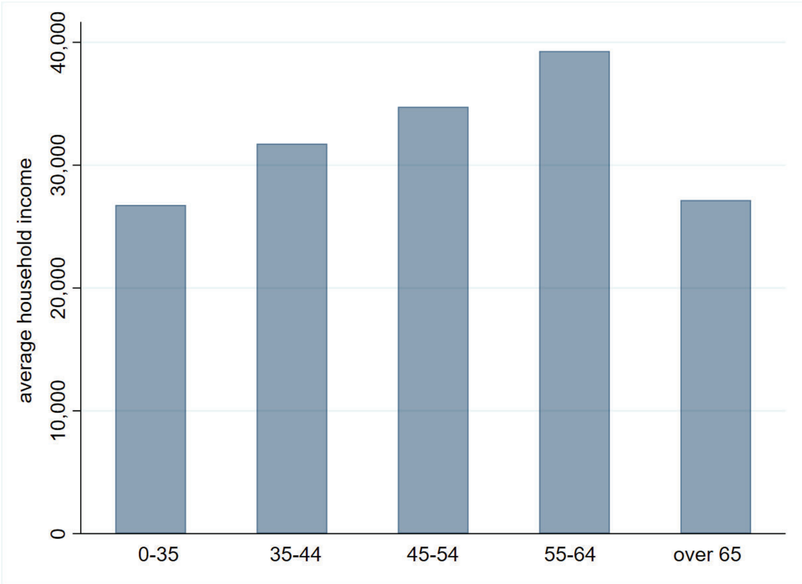
Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Columns 1 and 2 report second stage estimates corresponding to equation 4 for the alternative placebo outcome corresponding to resident population between 20 and 35 years old. Column 1 refers to the sample of all single-primary-schools in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. All specifications include controls for other school endowments, municipality and LLM-year fixed effects.

Table A8: IV estimation, second stage results with alternative bandwidth choices

	School-age population	Potential parents	Total income	Per-capita income	Elder population
Panel a: Schools with up to 45 students above/below threshold					
School closure	-0.102	-0.151***	-0.113***	0.007	-0.051
	(0.0631)	(0.0407)	(0.0250)	(0.0147)	(0.0486)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	10,600	10,600	10,594	10,594	10,600
Panel b: Schools with up to 40 students above/below threshold					
School closure	-0.117*	-0.150***	-0.101***	0.006	-0.048
	(0.0662)	(0.0420)	(0.0253)	(0.0152)	(0.0510)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	9,870	9,870	9,864	9,864	9,870

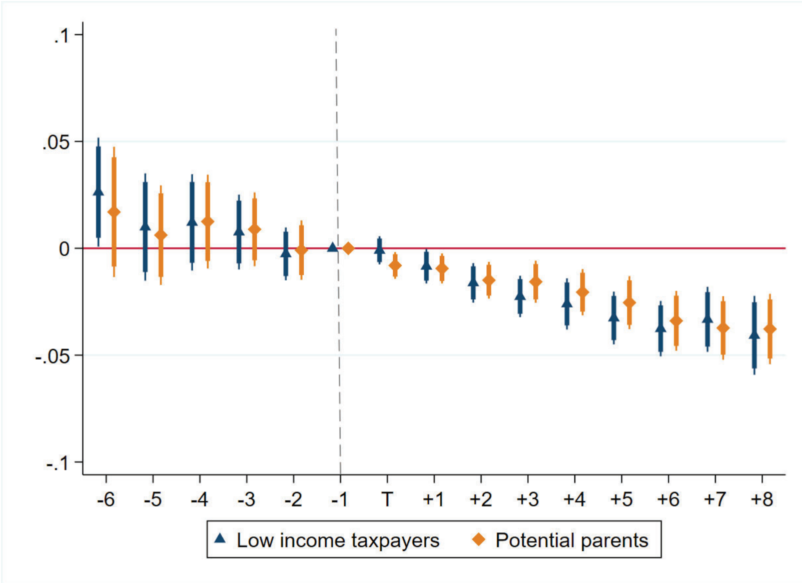
Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 4, regressing school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with an indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a and b refer, respectively, to the sample of schools with at most 45 and 40 students above/below the regional threshold in 2010.

Figure A8: Household income distribution by age class of family's head



The Figure shows the distribution of household annual income (euros) by age class of the family's head. Source: own elaboration on the Italian release of EU-SILC survey data (2018), publicly available at http://dati.istat.it/.

Figure A9: Event study plot of the reduced-form estimation: low income taxpayers and potential parents



The Figure shows the event study plot corresponding to the reduced form of equation 2, where dependent variable is (log) population of potential parents (i.e. residents between 35 and 49 years old) or (log) of low income taxpayers (below 10,000 euros per year). Those outcome variables are regressed on leads and lags of the instrument defined in equation 3. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Table A9: Placebo closures: IV estimation, second stage results

	School-age population	Potential parents	Total income	Per-capita income	Elder population
Panel a: All municipalities with one or two primary schools in regions with thresholds					
School closure	0.057	-0.007	0.009	0.011	0.015
	(0.0399)	(0.0327)	(0.0177)	(0.0098)	(0.0302)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	15,820	15,820	15,802	15,802	15,820
Panel b: Schools with up to 50 students above/below threshold					
School closure	-0.009	-0.069	0.023	0.020	-0.004
	(0.0760)	(0.0519)	(0.0324)	(0.0184)	(0.0513)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓
N	6,010	6,010	6,004	6,004	6,010

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Second stage results from the TSLS estimation of equation 4. Panel a refers to the sample of all municipalities with one or two primary schools in regions adopting thresholds; Panel b, instead, to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. 'School closure' (i.e. treatment) refers to the closure of one of the two schools in the municipality; while as controls we employ municipalities with one or two schools not experiencing closures. The instrument is a dummy variable (equation 3) taking value one from the year of threshold introduction if at least one school was below the regional threshold according to 2010 characteristics. All specifications include controls for other school endowments, municipality and LLM-year fixed effects.

Table A10: Municipalities by distance to LLM centres and next available school

Next school	LLM centre *close*	*far*	Total
close	460	380	840
far	380	460	840
Total	840	840	1,680

The Table reports the number of municipalities respectively below (close) or above (far) the median distance to centres of LLM and next available public primary school.

Table A11: School closure effect by municipality location (all single-primary-school municipalities in regions with threshold)

	School-age population		Potential parents		Total income	
	far	*close*	*far*	*close*	*far*	*close*
Panel a: LLM centres						
School closure	-0.236***	-0.017	-0.244***	-0.073	-0.123***	-0.008
	(0.0872)	(0.0791)	(0.0538)	(0.0524)	(0.0317)	(0.0266)
N	8,860	9,060	8,860	9,060	8,850	9,054
Panel b: Next public school						
School closure	-0.260***	-0.051	-0.216***	-0.143**	-0.147***	-0.064*
	(0.0905)	(0.0897)	(0.0556)	(0.0569)	(0.0327)	(0.0337)
N	7,660	7,960	7,660	7,960	7,652	7,948
Other school endowments	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓	✓

Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from equation 4, where dependent variables are school-age population, potential parents and total income. School closure instrumented with dummy IV. In Panel a we subdivide our sample by distance to LLM centres and separately estimate equation 4 for municipalities above (far) or below (close) the median distance from LLM centres. In Panel b, we follow an analogous procedure, considering instead distance from the closest public primary school. Sample of all municipalities with one primary school in regions with threshold.

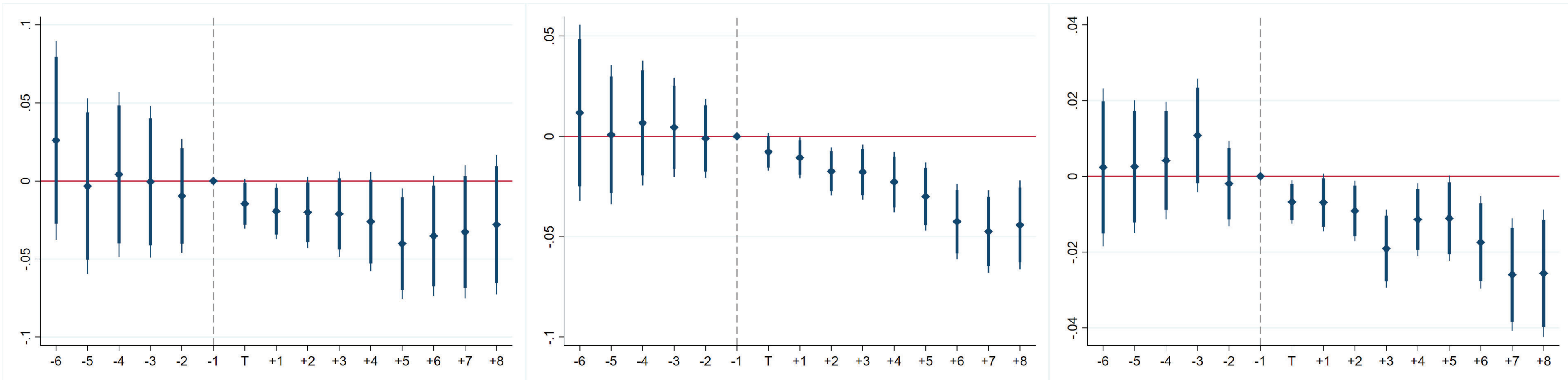
Table A12: School closure effect by municipality location, interaction term

	School-age population		Potential parents		Total income	
	Tfs	±50	Tfs	±50	Tfs	±50
Panel a: LLM centres						
School closure × *far*	-0.254***	-0.251**	-0.181***	-0.189***	-0.137***	-0.143***
	(0.0926)	(0.1042)	(0.0602)	(0.0644)	(0.0339)	(0.0382)
School closure	0.005	0.061	-0.066	-0.015	-0.013	-0.008
	(0.0744)	(0.0932)	(0.0484)	(0.0586)	(0.0258)	(0.0325)
N	18,330	11,290	18,330	11,290	18,314	11,284
Panel b: Next public school						
School closure × *far*	-0.208**	-0.219**	-0.130**	-0.140**	-0.098***	-0.108***
	(0.0875)	(0.0931)	(0.0551)	(0.0558)	(0.0303)	(0.0320)
School closure	-0.023	0.039	-0.098**	-0.047	-0.038	-0.032
	(0.0706)	(0.0833)	(0.0431)	(0.0478)	(0.0233)	(0.0273)
N	18,330	11,290	18,330	11,290	18,314	11,284
Other school endowments	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
LLM-year fe	✓	✓	✓	✓	✓	✓

~~Standard errors clustered at municipal level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Second~~ stage results from equation 4, where dependent variables are school-age population, potential parents and total income. In Panel a we add to the specification of equation 4 the interaction between school closure and an indicator taking value one if the municipality is above the median distance from LLM centre. In Panel b we follow an analogous procedure, considering instead distance from the closest public primary school. School closure instrumented with dummy IV; interaction term instrumented with dummy IV × *far*. Full sample of single-primary schools in regions with threshold (Tfs) in columns 1, 3, 5; sample of schools ± 50 students from regional thresholds (±50) in columns 2, 4, 6.

Figure A10: Event study plots of the reduced-form estimation: municipalities far from LLM

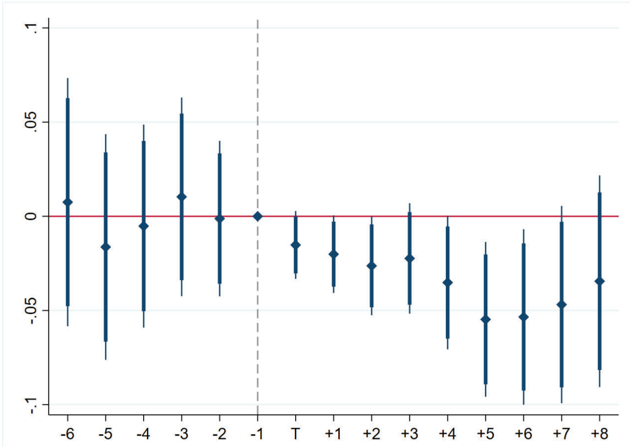
a. School age population | b. Potential parents | c. Total income



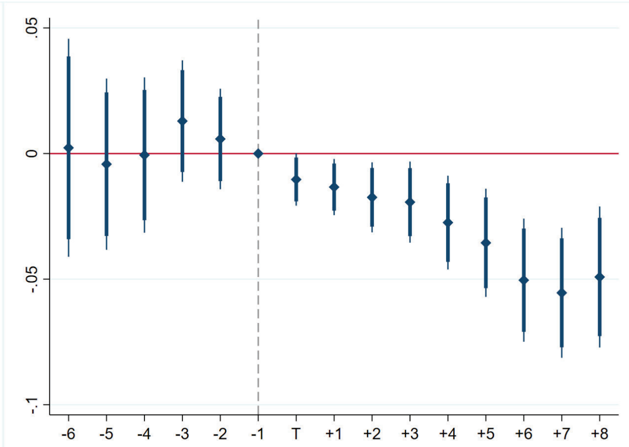
The Figure shows the event study plots corresponding to the reduced form of equation 2. Dependent variables: (log) school-age population (panel a), (log) potential parents population (panel b), total income (panel c). Those outcome variables are regressed on leads and lags of the Dummy IV. The sample is composed of single-primary-school municipalities of regions with school-sizing thresholds above the median distance from Local Labour Market centres. Sample further restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A11: Event study plots of the reduced-form estimation: municipalities far from next primary school

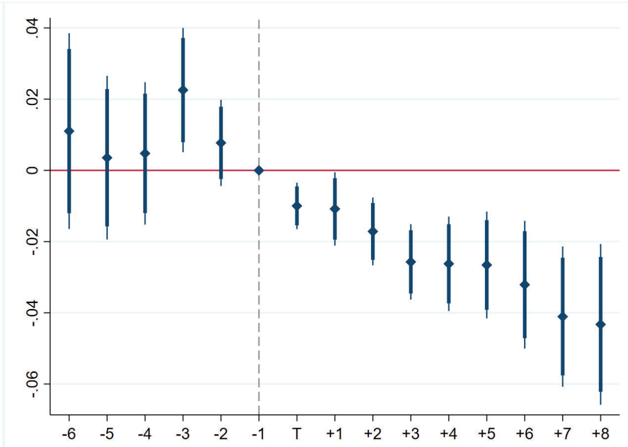
a. School age population



b. Potential parents



c. Total income



The Figure shows the event study plots corresponding to the reduced form of equation 2. Dependent variables: (log) school-age population (panel a), (log) potential parents population (panel b), total income (panel c). Those outcome variables are regressed on leads and lags of the Dummy IV. The sample is composed of single-primary-school municipalities of regions with school-sizing thresholds above the median distance from the next available primary school. Sample further restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.