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# Rational cuts? The local impact of closing undersized schools

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ABSTRACT

The availability of public education services can influence residential choices. Therefore, policies aimed at 'rationalising' service provision by reducing the number of undersized nodes in the public school network can lead to population decline, especially in spatially isolated areas lacking valid alternatives to the removed services. This paper examines the demographic and income effects of primary school closures by exploiting an Italian education reform that resulted in the contraction of the school network. We assess whether school closures impact households' residential choices, over and above preexisting negative population trends that motivate school closures. Our findings indicate that municipalities affected by school closures experience significant reductions in population and income. The effect is primarily driven by peripheral municipalities located far away 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.

## 1. Introduction

Access to publicly provided services plays a key role in influencing residential choices. People decide where to live taking into account not only 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; Epple and Romano, 2003).

In turn, the organisation and territorial distribution of education services are directly dependent on government policies. A key aspect often considered by policy-makers in the design of policies concerning 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. These measures are generally implemented in places where local demand is decreasing, thus unevenly affecting territories depending on their pre-existing trends.

These policies may also shape the location decision of households and, by providing unequal incentives for relocation based on income levels, they can affect income differentials across space (OECD, 2021). This article investigates whether people 'vote with their feet' 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 large urban centres. This especially concerns young adults with children, representing the highest fraction of all internal migrants.[1] On the other hand, austerity measures implemented in the last decade have led to a deep rationalisation of key services, 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. Public expenditures per student were considered excessively high, a feature attributed by the reform to the geographically dispersed configuration of the Italian schooling system. The reform involved a significant reduction in educational infrastructure and the closure of several schools across the country.

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

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Similar reductions in schooling services can affect 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.

The way in which changes in public goods provision interact with local demand has been widely investigated. Variations in public goods such as schools (Baum-Snow and Lutz, 2011; Brunner et al., 2012; Fack and Grenet, 2010), but also transport (Kahn, 2007; Billings, 2011; Gibbons et al., 2018; Gupta et al., 2022) or environmental quality (Banzhaf and Walsh, 2008; Gamper-Rabindran and Timmins, 2011; Currie et al., 2015) can affect the decisions of households and the value of housing. While other works have examined the impact of school closures (Engberg et al., 2012; Brummet, 2014; Steinberg et al., 2019; Taghizadeh, 2020; Damm et al., 2022), no study has ever performed a systematic assessment of the population dynamics induced by schooling rationalisation policies. We take a novel perspective on the study of the relationship between public goods and residential choices, by looking at the impact of education service cuts determined by declining demand. Our focus is on reductions to local public schools and the way in which this type of shock hits differently more and less peripheral areas.

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 2008–19 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 instrumental 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.

Our findings indicate 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 experiencing school cuts, while per-capita income seems to remain unaffected. The overall impact of school closures on municipal depopulation is mainly driven by more peripheral municipalities, i.e. those distant from economic centres and from alternative school options. In such contexts, nearby schools absorb a portion of pupils left without a school after closure, but only in the short-term. At a more aggregate scale, we find that more peripheral Local Labour Markets are negatively impacted by school closures, while those including sizeable urban areas are virtually unaffected. This hints at migrations towards core areas after the closure of schools.

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 local public goods. 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 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; Currie et al., 2015) or rail transit lines (Kahn, 2007; Billings, 2011; Gupta et al., 2022). These studies investigate the impact of changes in local public good provision on residential choices, either looking at population responses or housing values. Overall, the evidence tends to confirm that households are willing to move to places offering them desirable amenities and public services.

In this literature studying the interaction of public goods with local demand and location choices, many works have focused on the role of education services, particularly public schools. Early theoretical contributions in this field have studied how the interplay between peer effects and housing prices impacts residential choices and possibly leads to segregation (Benabou, 1993, 1996; Epple and Sieg, 1999; Epple and Romano, 2003). More recent empirical works have tested the related predictions. Among others, 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.

These works indicate that the institutional design of education systems affects 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). This evidence largely confirms 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 (e.g. infrastructure maintenance and teachers’ expenses) 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 demographic consequences of public interventions intended to minimise fixed costs which may induce the closure of undersized schools.

The impact of school closures has already been investigated in the literature. Previous works have mainly focused on the consequences of closures for the educational attainments of students displaced from their original school, finding that school displacement has strong detrimental effects on academic achievements and individual wellbeing (Engberg et al., 2012; Brummet, 2014; Taghizadeh, 2020; Damm et al., 2022).[2] Different from these contributions, our goal is to look at the demographic and economic trajectories of the local territories experiencing the school cut.

Another aspect largely overlooked is that of transport costs to access schools.[3] These can play a relevant role in household location

[2] A related literature has also studied the effect of school creation or school improvements. As an example, some works have focused on how school *construction* projects can impact house values and educational attainments (Cellini et al., 2010; Neilson and Zimmerman, 2014; Horn, 2015).

[3] Dinerstein and Smith (2021) highlights the role of fixed costs and distance between schools in the decision of private schools to enter or exit the market.

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 (NEG) tradition (Krugman, 1991). This literature strand 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 (Fujita and Thisse, 2002). 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.[4] 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, such as education and health care, 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.[5] The national government 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.[6]

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.[7] 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.[8] 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.[9] Conversely, school quality does not generally orient primary school choices. This is because in the Italian context there is no school tracking in educational offer over the first educational cycle.[10] 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*), while it does not seem particularly relevant for the first educational cycle (Bertola and Checchi, 2004; Brunello and Checchi, 2007).[11]

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 schools. 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

In their setting, closure/exit decisions are made at school level and based on private business criteria. Different from this contribution, we deal with a setting in which central authorities decide the organisation of the school network balancing economic efficiency and service access.

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

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

[6] 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).

[7] 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).

[8] 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.

[9] School buses are managed at the local level, which makes bus service provision not uniform across space. While we cannot exclude the possible presence of school buses, we believe that parents with children attending primary school positively value proximity to school in their residential choices.

[10] 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 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.

[11] 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.

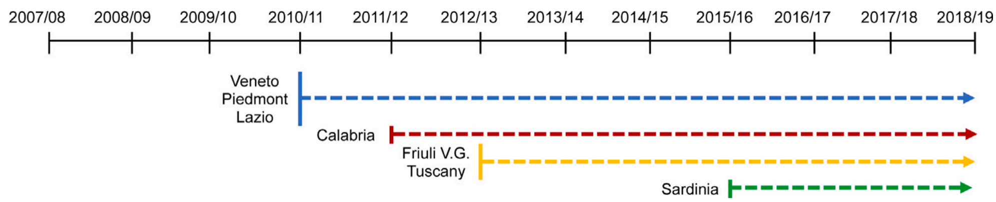


**Fig. 1.** Timeline for the introduction of regional thresholds.
The graph reports the school year in which different regions introduced numerical thresholds for school closure over the period considered.

more valuable and costly resource of the schooling system.[12] 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.

### *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 schools and classes activated (MIUR, 2010). Indeed, by 2008 rationalisation policies had mainly intervened to reduce autonomous school institutions,[13] but they had not strongly affected the geographical distribution of schools. The territorial fragmentation of schools and the limited class size were identified as the main reasons for the high per-pupil expenditure compared to other OECD countries (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 schools. For that purpose, regions were allowed to establish numerical criteria for the activation or suppression of schools.[14] Some regions formulated general norms for the organisation of the school network, including directives towards a more rational distribution of schools, to be achieved through the suppression of the undersized ones. Other regions introduced proper numerical criteria to determine 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 Fig. 1. These criteria consist of thresholds on the minimum number of required students in order to keep a school active.[15] 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.[16]

This process of service rationalisation led to the closure of 1200 primary schools over the period we consider. When a school closes, students are suggested to enrol in the nearest school but keep the possibility of choosing any school, provided this is not oversubscribed. Our prior is that, in places with fewer school options, the closure of a school might considerably worsen service access and therefore induce households to reconsider their residential choices.

## 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 2007/08-2018/19 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 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.[17] 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 2007/08 to school year 2018/19.

[12] 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.

[13] Autonomous school institutions are legal entities which comprehend multiple schools. 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.

[14] "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.

[15] 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.

[16] Apulia had numerical thresholds in its sizing plans until 2011. Since our analysis spans over the period 2008–2019, we exclude Apulia when focusing on the sub-sample of regions adopting thresholds. More details and guidelines for regional school-sizing plans can be found on the regions' websites or requested to competent regional offices.

[17] In Italy, municipalities are the smallest local authorities. In the period considered, Italian municipalities are around 8000. 4234 of them had a single primary school in 2008, with an average size of 29 square kilometres. Further summary statistics for this set of municipalities are reported in online Appendix Table A1.

Our goal is to examine the effect of school closures on population and income dynamics. As for the outcome variables, we have collected data on residential population at the municipal level from the Italian Institute of Statistics (ISTAT).[18] 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),[19] 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 of primary-school-age children between 25 and 44 years old.[20]

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 2008–19.[21] This information comes from households' tax records and it is aggregated at 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.[22]

We complete the dataset with information on municipal public expenditures for primary schools, available from the Italian Ministry of Interior's *Certificati Consuntivi*, yearly, until the year 2015. Italian municipalities' balance sheets are sub-divided into two different categories, current and capital expenditures.[23] The dataset is further disaggregated into different functions and sub-functions. The one we are interested in is 'Primary School', a sub-function of total spending for 'Education'.

Crucially, to define the main sample of municipalities for the analysis, we mostly focus on municipalities that have *only one* primary school within their borders at the beginning of the sample period, i.e. school year 2007/08. 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 is not activated for school year 2008/09, the decision about the closure is taken and announced in December 2007. For a school that closes at the end of school year 2007/08, if residents decide to relocate *after* the closure, they will do so starting from the second half of the year 2008, 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 consider the municipality with the school closing in June 2008 as having a primary school until 2008 (included) and lacking any school from the start of 2009.

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 online Appendix plots the geographical distribution of primary schools by municipality in the first school year considered, i.e. 2007/08. 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 Fig. 2. In this Figure, red municipalities are those experiencing school closures during the time span considered (treated units), whereas green ones are those that do not (control units). Panel b of Fig. 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 represented. In 2008, 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). Tables A1 and A2 in the online Appendix, reporting key summary statistics for the variables in our sample, show that the characteristics of single-primary-school municipalities in regions adopting thresholds are largely comparable to those in the full sample of all single-primary-school municipalities.[24]

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 2007/08, we are left with a total of 4236 municipalities, of which 296 experienced primary school closures during the period of analysis. They are distributed across regions as shown in Table 1, reporting in italics the regions introducing specific numerical criteria for school closures.

The timing of school closures is also relevant. Fig. 3 shows the number of closures by year in the sample of municipalities with a single primary school in 2007/08. We can notice a concentration of cases of closure in 2010/11 and 2011/12. The period 2010-12 coincides with the time horizon indicated by the 'Gelmini reform' for collecting 8 billion euros in public savings through the policy of rationalisation.

18 Historical data on municipal demography is available at https://demo.istat.it/#sezione2.

19 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.

20 In Italy, primary school age ranges from 5 to 10 years old. If we consider the 25–44 age range for childbearing, we obtain an age window of 35–49 years old for adults with children between the ages of 5 and 10. In 2021, the average age at which women have their first child in Italy was 32.1 years old, the highest in Europe. This ranges from a minimum of 31.4 years old in Sicily to a maximum of 32.8 years old in the Basilicata region. Only 8% of women had children at 24 years old or less (Eurostat).

21 Data are publicly available at www1.finanze.gov.it.

22 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, which includes a total of 611 LLMs. In our restricted sample of regions adopting thresholds, we have 232 LLMs. These geographical units contain, on average, 12 municipalities. Municipalities are partitions of LLMs, and there are no cases of municipalities crossing LLM boundaries.

23 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.

24 For a description of all the employed variables and their relative sources see Table A3 in the online Appendix.

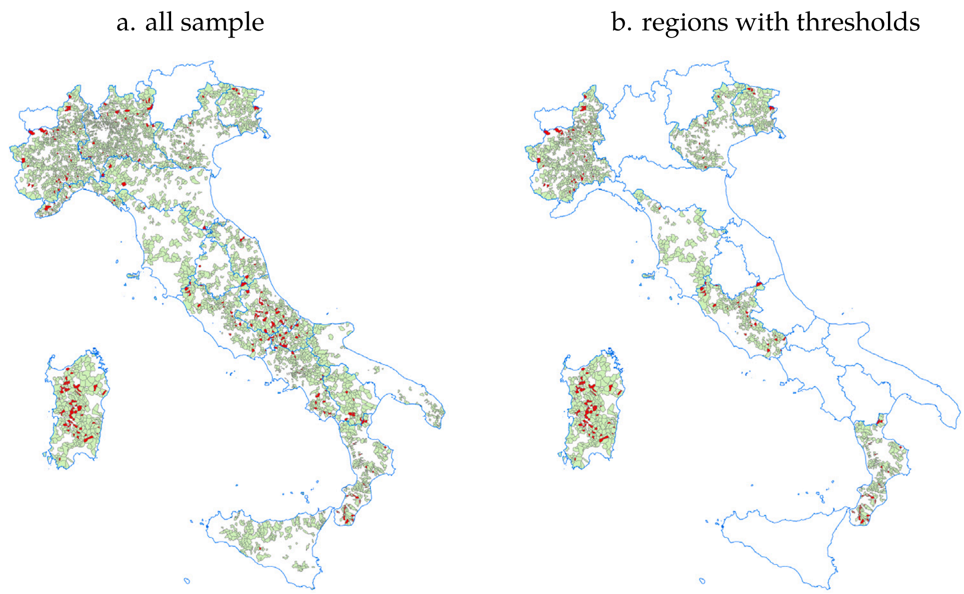


**Fig. 2.** Single-primary-school municipalities — closures.
The map in Panel a shows all single-primary-school municipalities, reporting in colour red those experiencing school closures over the period considered (2008–19), and in colour green those that do not. The map in Panel b reports the same information, 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. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

**Table 1**
Single-primary-school municipalities — closures by region (2008–19).

| Region | No closure | Closure | Total | % closures over total |
|---|---|---|---|---|
| Abruzzi | 157 | 30 | 187 | 16.04 |
| Apulia | 108 | 2 | 110 | 1.82 |
| Basilicata | 79 | 8 | 87 | 9.20 |
| *Calabria* | 181 | 20 | 201 | 9.95 |
| Campania | 270 | 22 | 292 | 7.53 |
| Emilia Romagna | 140 | 3 | 143 | 2.10 |
| *Friuli V.G.* | 128 | 5 | 133 | 3.76 |
| *Lazio* | 201 | 23 | 224 | 10.27 |
| Liguria | 126 | 9 | 135 | 6.67 |
| Lombardy | 914 | 38 | 952 | 3.99 |
| Marche | 132 | 8 | 140 | 5.71 |
| Molise | 82 | 19 | 101 | 18.81 |
| *Piedmont* | 647 | 44 | 691 | 6.37 |
| *Sardinia* | 210 | 55 | 265 | 20.75 |
| Sicilia | 170 | 1 | 171 | 0.58 |
| *Tuscany* | 93 | 1 | 94 | 1.06 |
| Umbria | 48 | 1 | 49 | 2.04 |
| *Veneto* | 254 | 7 | 261 | 2.68 |
| Total | 3,940 | 296 | 4,236 | 6.99 |

Number of municipalities endowed with a single primary school in 2007/08 by region, experiencing or not school closures over the period considered (2008–19). Regions introducing numerical thresholds for school sizing are in italics.

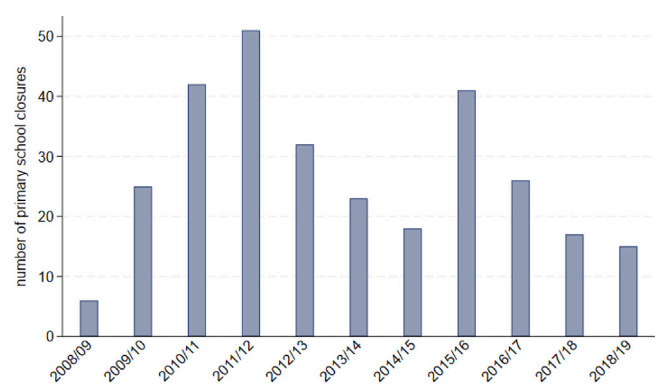


**Fig. 3.** Single-primary-school municipalities, closures by school year.
Number of primary school closures in single-primary-school municipalities over the period considered.

## 5. Empirical strategy

### 5.1. Two-way-fixed-effects model

Our main 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 2008–19 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:

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

where $i$ is the municipality identifier, $c$ is the LLM and $r$ the region to which the municipality belongs, and $t$ is the year. Eq. (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 fixed effects ($\gamma_t$), year-local labour markets (LLM) interacted fixed effects ($\delta_{ct}$), 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.

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.[25] The model controls for

[25] The treatment dummy is constructed to make sure that population dynamics and closures are associated correctly in our annual dataset. As for ISTAT measurement, municipal residents each year correspond to the total residents in a given municipality on January 1st. Closures occur in June. If a school is absent from the dataset starting from school year 2008/09 (it closed in June 2008), the dummy $Closure_{ict}$ takes value 1 from 2009. The total residents of 2009 are therefore observed 6 months after the closure of that school.

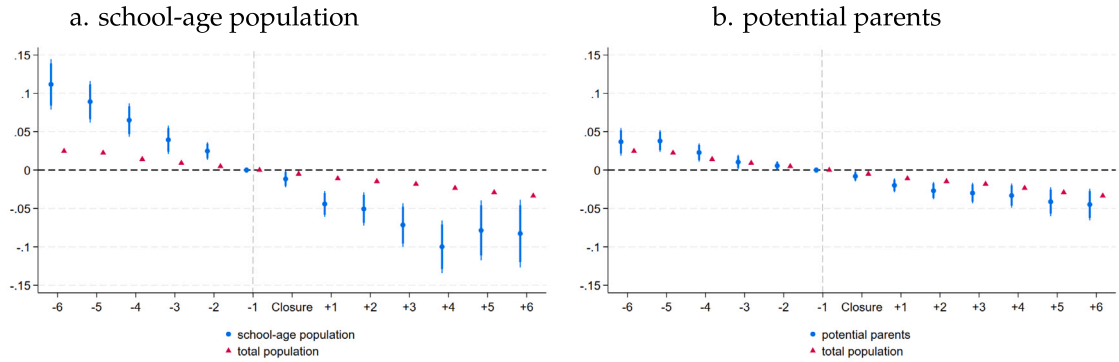


**Fig. 4.** Population by age classes around school closure.
The Figure shows event study plots corresponding to Eq. (2), using as dependent variable (log) total and school-age population (Panel a) or (log) total population and potential parents, i.e. residents between 35 and 49 years old (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

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).[26] Year-LLM interacted fixed effects $\delta_{ct}$ 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.[27,28] Standard errors are clustered at the municipality level.

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:

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

where the term $\sum_{m=-G}^{M} z_{icr(t-m)}$ 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. The estimated $\{\beta_m\}_{m=-G}^{M}$ can be interpreted as the cumulative effect of the policy up to period $(t-m)$.

We report the event study plots estimating Eq. (2), providing a visual intuition of the plausibility of the identifying assumptions, in Fig. 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.

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 (i.e. reverse causality). 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. The pre-trends displayed in Fig. 4 confirm that school cuts take place precisely where the demand for school services is shrinking, making its provision inefficient.[29]

As visible from Fig. 4, Panel b, before school closure the total population and that of potential parents showed equal trends, while after closure the latter appears to decrease at a faster rate, with all point estimates (blue circles) lying below those referring to total population (red triangles). This confirms that the population of young adults is particularly affected by the service cut. The pre/post-closure difference in trends between total and school-age population is less visible, since in the years preceding closure the decline of school-age children was already quite sharp and steeper compared to total population (Fig. 4, Panel a). This is likely due to a combination of anticipation effects and reverse causality, particularly evident when looking at school-age population since this group represents the main target for the decision of closing schools.

School rationalisation policies were intended to reduce the number of undersized primary schools, and in doing so cut public expenses. Using the same kind of event study model of Eq. (2), it is also possible to visualise the significant reduction in fixed costs the closure of schools has entailed. Municipalities experiencing closures have experienced a sharp decline in current and capital expenditures for primary school maintenance (online Appendix Figure B1).

[26] 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). Moreover, in our preferred sample of single-primary-school-municipalities in regions adopting thresholds, we have at most one private primary or lower secondary school, and four private preschools. 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.

[27] The inclusion of year-LLM fixed effects also controls for variations in school quality that concern the entire LLM, such as natural events that may damage school buildings. The only residual concern about the possible confounding role of school quality may lie in idiosyncratic variations at the municipal level influencing both the decision of closing schools and residential choices. We address that with the instrumental variables strategy (sub- Section 5.2).

[28] LLM fixed effects may generate problems of double counting, in case changes of residence after school closures mainly involve adjacent municipalities. We verify this is not an issue by estimating a specification without including LLM dummies (more on this in Section 6). We also observe the demographic evolution of municipalities within the same LLM experiencing closure or not, before and after the only school closure in the LLM (online Appendix Figure A5). While average population declines in treated municipalities after school closure, it is steady in control ones, suggesting that most households do not relocate to nearby municipalities after closures. Additional evidence on this is in Section 7.2.

[29] 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 treatment effects heterogeneity may represent a bias in this kind of models. 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 in online Appendix Figure A4 confirm the presence of significant pre-trends, which are not corrected by accounting for treatment effect heterogeneity.

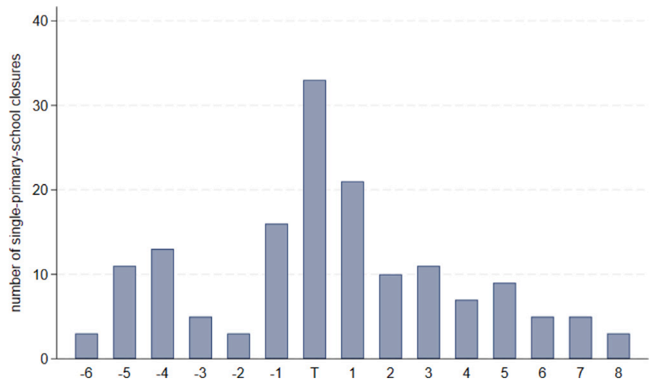


**Fig. 5.** Number of school closures before/after threshold introduction.
The Figure shows the number of school closures by relative school year before/after the introduction of school sizing thresholds. Sample of single-primary-school municipalities in regions adopting thresholds. $T$ = school year of threshold introduction.

### 5.2. Instrumental variable models

The pre-trends in population evolution shown in Fig. 4 confirm the rationalisation purpose of school closures. At the same time, they also represent the greatest empirical challenge in estimating the demographic impact of school closures. To address these concerns, we combine the TWFE estimation presented above with Instrumental Variable (IV) strategies.[30] 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 Fig. 2.

School sizing thresholds were adopted in different years by the various regions and, once activated, applied to all schools within the region. Fig. 5 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.[31]

We leverage this setting and implement two complementary IV models, estimating the impact of closure for municipalities complying with school-sizing rules. Firstly, we construct the following instrument:

$$Dummy\,IV_{icrt} = S_{icr,2008-10} \cdot T_{rt} \tag{3}$$

where $S_{icr,2008-10}$ is a dummy variable taking value one if school $i$ in local labour market $c$ and region $r$ has ever been below the regionally-set threshold on school size in any of the first observed school years, i.e. from 2007/08 to 2009/10, 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.[32] To construct the IV, we refer to the school conditions during a period in which no thresholds had been set yet. The instrument is constructed as a dummy variable taking value 1 from the moment of the introduction of the regional threshold if the school has ever been below that threshold in the pre-threshold years 2008–10, and 0 before. Figure A3 in the online Appendix displays the municipalities above/below the threshold according to initial school characteristics.

The choice of employing school characteristics in 2008–10 instead of contemporaneous ones is expected to make the IV more exogenous. The identifying assumption of our instrument is that being below the threshold does not directly affect municipal population and income. 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. Taking school characteristics in 2008–2010, prior to the introduction of any threshold, should mitigate the bias derived by this kind of endogenous household response.

In this way, we can take the initial school position with respect to the threshold ($S_{icr,2008-10}$) as exogenous. At the same time, the timing for the introduction of the threshold at regional level ($T_{rt}$) is plausibly exogenous with respect to municipal conditions. We then estimate a TSLS model, where the treatment variable $Closure_{icrt}$ is instrumented by the $Dummy\,IV_{icrt}$:

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

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.[33] In the main analysis, we employ the bandwidth of 50 students above and below the threshold, while online Appendix Table C5 reports the estimates for windows going from ±55 to ±35 students around the thresholds, 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.

To provide evidence on the validity of the IV, we perform event studies of reduced form estimates for a model mirroring Eq. (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 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 was below the future threshold in 2008–10, 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.[34] Therefore, for these reduced-form regressions, we should not face treatment effect heterogeneity issues

[30] 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).

[31] In the years preceding threshold introduction, we can still notice some closures, in particular in the year preceding threshold introduction. Those closures refer to a large extent to school year 2009/10, the first year of implementation of the 'Gelmini' reform.

[32] We need to associate correctly the timing of threshold introduction, closures, and population measurement. If a threshold is introduced from the school year 2010/11, in our annual dataset $T_{rt}$ will take value 1 in 2011, since we observe population at the beginning of the year 2011. Similarly, if a school is closed from school year 2010/11, $Closure_{icrt}$ will take value 1 from 2011.

[33] 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.

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

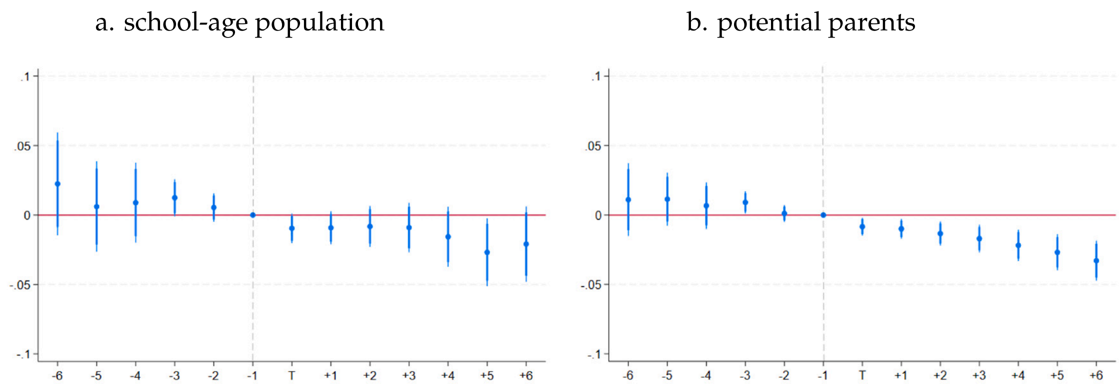


**Fig. 6.** Event study plots of the reduced-form estimation: population.
The Figure shows the event study plots corresponding to the reduced form of Eq. (2), where dependent variable is (log) population in school age (Panel a) or (log) population of potential parents, i.e. residents between 35 and 49 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. 2008–10. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

potentially associated with TWFE models with staggered adoption and we employ the traditional event study estimator.

Fig. 6 presents 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 C1 in the online Appendix reports analogous plots for total and per-capita income.

Overall, we find no significant pre-threshold differences in terms of demographics for municipalities below the threshold, suggesting that the instrument is exogenous. This event study exercise is replicated in Section 7.1, by specifically focusing on more peripheral municipalities.

This type of reduced form event study estimates can be performed with other demographic groups as well. First, as a form of placebo test, we estimate the 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.[35] As shown in Figure C2 in the online Appendix, all coefficients of post-threshold dummies are insignificant, indicating no effect of the introduction of school thresholds on this age group.

Second, we compute reduced form event study estimates using the newborn, i.e. the population 0 to 5 years old, as dependent variable. A possible omitted variable in our model may be the fertility rate, as local authorities may decide to close a primary school on the basis of demographic trends concerning newborn residents who will need a primary school in the near future. Reduced-form estimates using 0–5 years old as outcome allow us to rule out this possible confounder, verifying that our IV strategy corrects for possible differential trends in fertility rate. As shown in Figure C3 in the online Appendix, the population of newborns displays a similar evolution in municipalities below or above the threshold.

One residual concern could be the presence of differential trends in outcome evolution depending on *how far* below the threshold the school was in 2008–10. If the margin of deviation from the threshold correlates with the predictive capacity of our dummy instrument (Eq. (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 average across the first years in sample, i.e. 2008–10. Fig. 7 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, 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 thresholds, 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. For these reasons, school-sizing rules do not appear to be sharply binding at the cutoff, while the more a school deviates from the sizing threshold, the higher the probability of experiencing closure. The implication of this is that the predictive power of the IV for municipalities closer to the thresholds is lower.

To account for differential trends in outcome evolution depending on the initial distance from the threshold, 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 average number of students in school years 2008–10. Formally:

$$Kink\ IV_{icrt} = (Students - c)_{icr,2008-10} \cdot S_{icr,2008-10} \cdot T_{rt} \tag{6}$$

where $(Students - c)_{icr,2008-10}$ is the average number of students over 2008–10, in deviation from the future regional threshold; $S_{icr,2008-10}$ is a dummy variable taking value one if school $i$ in local labour market $c$ region $r$ has ever been below the regional threshold, according to school characteristics in 2008–10; and $T_{rt}$ is a dummy for the introduction of a threshold for school closure in region $r$, year $t$. In practice,

[35] 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 C4 in the online Appendix.

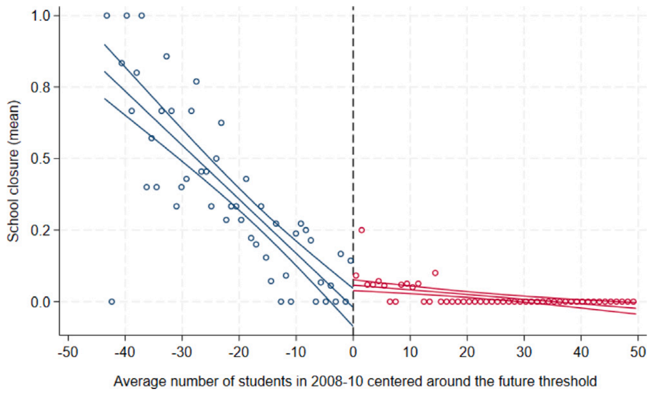


**Fig. 7.** Probability of closure by deviation from the regional threshold.
The Figure reports the mean of school closure for different levels of deviation from the regional school-sizing threshold. The deviation is measured as the average number of students enrolled in the school in 2008–10 minus the value the region will adopt for school-sizing threshold.

this $Kink\ IV_{icrt}$ is a continuous variable resulting from the interaction between $(Students - c)_{icr,2008-10}$ and the $Dummy\ IV_{icrt}$.

We label it 'kink' because it exploits the kink in treatment probability shown in Fig. 7.[36] Here we exploit the slope change in closure probability around the threshold to construct the IV. In fact, the scarce compliance with school-sizing thresholds in schools with enrolment just below the cutoff, visible in Fig. 7, implies a high degree of fuzziness.[37]

We estimate the impact of school closure, instrumenting it with the $Kink\ IV_{irt}$, using the full sample of single-primary-school municipalities in regions adopting thresholds. The exclusion restriction in this IV strategy is based on the assumption that a school's (average) deviation from the threshold in 2008–10 does not directly affect municipal population and income, conditional on controls. For this condition to hold, we need to control for the average number of students in 2008–10, as this plausibly correlates with our outcomes and 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 Eq. (1) with the interaction between the average number of students in 2008–10 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:

$$y_{icrt} = \alpha + \beta\, \hat{Closure}_{icrt} + \gamma_i + \delta_{ct} + \eta\, X_{icrt} + (Students - c)_{icr,2008-10} \cdot T_{rt} + \epsilon_{icrt} \quad (7)$$

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

$$Closure_{icrt} = \mu + \nu\,(Students - c)_{icr,2008-10} \cdot S_{icr,2008-10} \cdot T_{rt} + \rho_i + \tau_{ct} + \varphi\, X_{icrt} + (Students - c)_{icr,2008-10} \cdot T_{rt} + \upsilon_{icrt}. \quad (8)$$

[36] 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. Among the proponents of this design are Dong (2018) and Dong and Lewbel (2015). Different applications of the kink RDD estimation strategy exploit continuous rather than binary treatments (Nielsen et al., 2010; Card et al., 2015).

[37] Performing standard RDD checks such as testing density of observations across the cutoff or continuity tests, we find no evidence of significant jumps at the cutoff in terms of density or in terms of our outcomes before the introduction of the threshold.

## 6. Main results

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, the population of potential parents, total and per-capita income, and the 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 Eq. (1) in Table C1 of the online Appendix, both for the full sample of single-primary-schools in all regions (Panel a) and the restricted sample of 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, positive coefficients for per-capita income and insignificant estimates for elder population. We cannot interpret these coefficients causally due to the pre-trends visible in Fig. 4.

We address the endogeneity induced by the presence of pre-trends with the IV strategy outlined in Section 5.2. First, in Table 2 we present first stage results from Eqs. (5) and (8), to provide evidence of the relevance and strength of our instruments $Dummy\ IV_{irt}$ and $Kink\ IV_{irt}$. Column 1 and 3 refer 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 years 2008–10. 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 at any time between 2008 and 2010 ($Dummy\ IV_{irt}$ - columns 1 and 2) increases the probability of experiencing school closure by 13 percentage points. The relatively small size of the coefficient is determined by the fact that there is significant non-compliance below the threshold, because some undersized schools are kept active in derogation from regional rules. This can be done within the limits of the overall school personnel assigned to the region, and thus at the cost of having overcrowded schools in other regional municipalities.

There is also some non-compliance above the threshold, i.e. schools closing while always being above the threshold in 2008–10. This is mostly due to the way in which the instrument is constructed, as there are schools above thresholds in 2008–10 that decline in enrolments in the following years, and – once below the threshold – close. Column

**Table 2**
First stage results.

| | School closure | | |
|---|---|---|---|
| Dummy instrument | 0.133***<br>(0.0118) | 0.131***<br>(0.0132) | |
| Kink instrument | | | −0.011***<br>(0.0009) |
| *Other school endowments* | ✓ | ✓ | ✓ |
| *Municipality fe* | ✓ | ✓ | ✓ |
| *LLM-year fe* | ✓ | ✓ | ✓ |
| $(Students - c)_{icr,2008-10} \cdot T_{rt}$ | | | ✓ |
| F-test on instrument | 127.88 | 97.44 | 145.12 |
| N | 21,552 | 13,374 | 21,552 |

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 Eq. (5), regressing school closure on the dummy instrument. Column 1 refers to the sample of all single-primary-schools in regions adopting thresholds, column 2 to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2008–10. Column 3 reports first stage estimates corresponding to Eq. (8), regressing school closure on the kink instrument. All specifications include controls for other school endowments, municipality and LLM-year fixed effects; column 3 includes the average distance from threshold in 2008–10 interacted with threshold introduction.

**Table 3**
Second stage results.

| | School-age population | Potential parents | Total income | Per-capita income | Elder population |
|---|---|---|---|---|---|
| *Panel a: Dummy instrument* | | | | | |
| School closure | −0.159** | −0.184*** | −0.120*** | 0.038** | −0.066 |
| | (0.0729) | (0.0439) | (0.0273) | (0.0171) | (0.0543) |
| N | 13,374 | 13,374 | 13,370 | 13,370 | 13,374 |
| *Other school endowments* | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Municipality fe* | ✓ | ✓ | ✓ | ✓ | ✓ |
| *LLM-year fe* | ✓ | ✓ | ✓ | ✓ | ✓ |
| *Panel b: Kink instrument* | | | | | |
| School closure | −0.082* | −0.106*** | −0.088*** | 0.011 | −0.027 |
| | (0.0498) | (0.0277) | (0.0166) | (0.0101) | (0.0368) |
| N | 21,552 | 21,552 | 21,538 | 21,538 | 21,552 |
| $(Students - c)_{icr,2008-10} \cdot T_{rt}$ | ✓ | ✓ | ✓ | ✓ | ✓ |
| *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 the TSLS estimation of Eqs. (4) (Panel a) and (7) (Panel b), regressing school-age population, potential parents, total income, per-capita income and elder population on school closure. Panel a: school closure instrumented with dummy instrument; Panel b: closure instrumented with kink IV. All specifications include controls for other school endowments, municipality, LLM-year fixed effects. Sample in panel a: single-school municipalities with up to 50 students above or below the regional threshold as of s.y. 2008–10; sample in Panel b: all single-school municipalities in regions adopting thresholds.

3 of Table 2 reports the first stage results of Eq. (8) ($Kink\,IV_{irt}$). The negative sign of the estimated coefficient of the kink instrument relates to the negative slope of the left-side plot of Fig. 7. Once the threshold is active, the lower the number of students below the cutoff, the greater the probability of closure. Specifically, for every 10 additional students below the threshold, the probability of closure rises by 11 percentage points.

In Table 3 we report second-stage estimates corresponding to Eq. (4) and (7). The coefficients represent the average percentage variation in the outcomes 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 of Table 3 displays estimates from the TSLS models instrumenting school closures with the $Dummy\,IV_{irt}$ (Eq. (4)), restricting the sample to schools with – on average – up to 50 students above or below the regional threshold as of school year 2008–10. The results for the full sample and $Dummy\,IV_{irt}$ are shown in online Appendix Table C3, while in online Appendix Table C5 we report results from analogous estimations using bandwidths from 55 to 35 students above/below the threshold.[38] Panel b, Table 3 reports the second stage results instrumenting school closures with the $Kink\,IV_{irt}$ (Eq. (7)), adopting the full sample of single-primary-school municipalities in regions with thresholds.

The estimates in panels a and b show a significant reduction of around 8 to 16% in school-age population and 11 to 18% in potential parents. 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 141 potential parents in the year preceding school closure. 18% of 141 corresponds to 25 residents, while 11% corresponds to 15 residents. These could be parents of school-age children. We are dealing with approximately 7–12 couples. This calculation is consistent with the average size of schools experiencing closures, which have 17 students in their last year of activity. Alternatively, those 7–12 couples of potential parents could be the actual parents of the 17 students left without school and forced to move. However, it is also possible that many of these students are siblings or that some parents are single mothers or fathers. In that case, some of those 15–25 residents could be young adults without children enrolled in the closing school which react indirectly to the school cut. This would point to a negative multiplier effect where the exodus of directly affected families triggers additional population loss. A further option could be that some of these young adults are teachers and janitors employed in the closing school which decide to change residence when reallocated to another school. All of these are plausible dynamics that we cannot disentangle in a precise way. However, the key finding remains that these small reductions in absolute population are considerable in relative terms. In practice, such reductions are likely to be highly relevant for these small municipalities that already suffer from population decline.

The coefficients are equally signed but generally larger in (absolute) size compared to the OLS TWFE estimates (online Appendix Table C1). This is consistent with the correction of the downward-sloping pre-trends we achieve through the IV strategy. Moreover, the size of coefficients is smaller in panel b (instrumenting with $Kink\,IV_{irt}$) than in panel a, consistent with a more demanding specification in which we control for the initial number of students in deviation from the regional threshold. In these specifications, we account for the underlying demographic dynamics more explicitly. Therefore, these estimates can be interpreted as lower bound effects of school closure.

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 around 9%–12% 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,

[38] The size and significance of coefficients in Table C5 are considerably robust across the different bandwidth choices. Estimates look relatively stable around 16%–18%–12%, respectively for school age population, potential parents, and total income. It is worth noting that moving closer to the threshold significantly reduces compliance with school-sizing cutoffs – both below and above the thresholds (Fig. 7) – and therefore the predictive capacity of our instrument. For this reason we do not restrict the window around the thresholds more than ± 35 students.

increases in these municipalities by 4% (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 seems to be ultimately due to the demographic effect detected on potential parents.[39] However, when we look at the estimates using the $Kink\,IV_{irt}$ (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.

We also replicate these estimates without including LLM fixed effects (Table C2 in the online Appendix). In case changes of residence after school closures mainly involve within-LLM municipalities, LLM fixed effects may generate problems of double counting. Estimates are larger than our baseline estimated coefficients, suggesting that double counting – if present – is at least a less relevant concern than controlling for different LLM characteristics.

## 7. Who loses the most?

### 7.1. More 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 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.

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.

In order to capture municipal peripherality, we consider two different dimensions. We compute municipal distance in metres to the centre of the Local Labour Market, and distance to the next available public primary school measured at the beginning of the period considered. By 'centre of LLM' we mean the municipality constituting the core of the corresponding LLM as identified by the Italian Institute of Statistics.[40]

Next, for both these indicators, we divide our full sample of municipalities in sub-groups on the basis of their mean value, to identify areas located close to (below mean distance), or far from (above mean distance) LLM centres or alternative primary schools.[41] The mean distance to LLM centres is 8.4 km, while the mean distance to the next primary school is 3.2 km. Those two criteria do not necessarily overlap (see Table A4 in the online Appendix).[42]

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, 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 4 reports the results sub-dividing the full sample along these dimensions and instrumenting school closure either with the $Dummy\,IV_{irt}$ of Eq. (3) (Panels a.1 and b.1) or with the $Kink\,IV_{irt}$ of Eq. (6) (Panels a.2 and b.2).[43] 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 2008–10 around the threshold introduction, for the two samples of municipalities far from SLL and far from the next available school, are displayed in Figures C4 and C5 in the online Appendix.

Panel a of Table 4 indicates that the effect of school closures on residential dynamics and local income is stronger in municipalities located far away from the centres of Local Labour Markets. This finding supports the view that households value proximity to economic centres. These presumably offer relatively more services and labour opportunities, motivating residents of nearby municipalities to maintain their residences when the school closes. On the contrary, the same cannot be said for municipalities too far from economic centres, 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 4 reveals that school cuts are particularly harmful to more peripheral 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 these municipalities, in terms of reduced demand for local services, entrepreneurial capacity, and thus job creation. This suggests that rationalisation policies can affect territorial disparities, by widening the existing intra-regional gaps in terms of population and income growth.

[39] 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. The resulting event study plot, displayed in online Appendix Figure A6, shows that the trajectories for these two groups are almost overlapping.

[40] 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.

[41] As a robustness check, we also subdivide the sample using the median, 25th, or 75th percentile cutoffs. The results (available upon request) are stable across these alternative choices. In Tables C7 and C8 of the online Appendix, we repeat the heterogeneity analysis by interacting the school closure dummy with an indicator for the municipality being above the mean distance from economic centres or alternative schools. We then instrument school closure with either of our instruments - $Dummy\,IV_{irt}$ (online Appendix Table C7) or $Kink\,IV_{irt}$ (online Appendix Table C8). Results are qualitatively equal to those obtained with the sample-split method.

[42] 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.

[43] The DummyIV estimates refer to the sample of schools less than 50 students above/below regional thresholds in 2008–10. Estimates with all single-primary-school municipalities are in online Appendix Table C6, while comparable estimates using interaction terms rather than sample splits are in online Appendix Tables C7 and C8.

**Table 4**
School closure effect by municipality location.

| | School-age population | | Potential parents | | Total income | |
|---|---|---|---|---|---|---|
| | *far* | *close* | *far* | *close* | *far* | *close* |
| *Panel a:* ***distance from LLM centres*** | | | | | | |
| a.1: *Dummy instrument* | | | | | | |
| School closure | −0.157 | −0.064 | −0.199*** | −0.082 | −0.136*** | −0.055 |
| | (0.1397) | (0.1007) | (0.0743) | (0.0618) | (0.0459) | (0.0364) |
| F-test | 34.78 | 46.92 | 34.78 | 46.92 | 34.78 | 46.92 |
| N | 5,518 | 7,406 | 5,518 | 7,406 | 5,518 | 7,402 |
| a.2: *Kink instrument* | | | | | | |
| School closure | −0.161** | 0.035 | −0.126*** | −0.052 | −0.084*** | −0.062*** |
| | (0.0772) | (0.0709) | (0.0402) | (0.0409) | (0.0252) | (0.0230) |
| F-test | 51.42 | 80.66 | 51.42 | 80.66 | 51.42 | 80.66 |
| N | 8,244 | 12,802 | 8,244 | 12,802 | 8,240 | 12,792 |
| *Panel b:* ***distance from the next public school*** | | | | | | |
| b.1: *Dummy instrument* | | | | | | |
| School closure | −0.431*** | 0.070 | −0.313*** | −0.107* | −0.207*** | −0.051 |
| | (0.1552) | (0.1070) | (0.0877) | (0.0625) | (0.0561) | (0.0373) |
| F-test | 27.56 | 48.55 | 27.56 | 48.55 | 27.56 | 48.55 |
| N | 4,804 | 7,704 | 4,804 | 7,704 | 4,804 | 7,702 |
| b.2: *Kink instrument* | | | | | | |
| School closure | −0.161* | −0.050 | −0.172*** | −0.081** | −0.157*** | −0.058*** |
| | (0.0866) | (0.0570) | (0.0483) | (0.0341) | (0.0305) | (0.0219) |
| F-test | 52.14 | 83.20 | 52.14 | 83.20 | 52.14 | 83.20 |
| N | 7,900 | 12,608 | 7,900 | 12,608 | 7,896 | 12,598 |

Clustered standard errors at municipal level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Second stage results from Eqs. (4) and (7), where dependent variables are school-age population, potential parents, total income. Panel a: sample subdivided by above/below (far/close) mean distance to LLM centres. Panel b: sample subdivided by above/below (far/close) mean distance to the closest public primary school. Panel a.1, b.1: sample of schools with up to ± 50 students from threshold in 2008–10; Panel a.2, b.2: full sample of single-primary-schools in regions with thresholds. All specifications control for municipality fixed effects, LLM-year fixed effects, other school endowments. Models using the kink instrument control for $(Students - c)_{icr,2008-10} \cdot T_{rt}$.

### 7.2. Neighbouring municipalities

The data we employ do not allow to closely track internal migrations, so we cannot explore precise household re-locations after school closures. To gain some insights in this regard, we inspect whether school closure influences primary school enrolments of neighbouring municipalities.

It is plausible that at least a portion of pupils left without a school after closure enrol in neighbouring municipalities' schools. To investigate whether this is the case, we adopt the sample of municipalities that did *not* experience school closures and estimate student enrolment in the primary school of each municipality neighbouring those experiencing school closure, before and after the closure takes place in its neighbour.[44] We distinguish by municipality location, splitting the sample along the mean distance from LLM centre and mean distance from the next available primary school.

Demographic conditions in neighbouring municipalities are more likely to be orthogonal to school closure decision as compared to demographic conditions in the municipality itself. In line with this hypothesis, we do not detect any pre-trends in the event study plots of online Appendix Figure C6, illustrating enrolment in municipalities neighbouring those experiencing closure, before and after closures. By inspecting these event study plots, we can have an indication of the dynamic effect of school closure on the population of neighbouring municipalities.

We detect an increase in primary school enrolment of neighbouring municipalities in the closure year and the years immediately after that. In less peripheral areas, we find a significant increase in student enrolment at closure, an effect which appears fairly persistent over time (online Appendix Figure C6 b. and d.). Differently, the immediate enrolment effect of closure on more peripheral areas is less strong, gradually reducing in magnitude, and approaching zero in the medium/long-term (online Appendix Figure C6 a. and c.). Therefore, in the medium-term, pupils living in peripheral municipalities do not seem to enrol in neighbouring schools after the closure of their school.

### 7.3. 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.

[44] In order to isolate the effect of each individual school closure on the enrolment of each of its neighbours, we keep municipalities neighbouring multiple closures only until the last year before the second closure, so that we clearly observe enrolment patterns before and after the first (or the only) closure.

To minimise endogeneity we construct an instrumental variable, labelled as $LLM\ IV_{ct}$ and built as the number of single-primary-schools ever below the regional threshold according to student enrolment in 2008–10. We refer to the sample of 221 LLMs in the regions adopting thresholds and include all municipalities in those LLMs-regions, independently from the number of primary schools at the beginning of the sample period. We estimate a TSLS model where we regress LLM-level outcomes on the treatment variable defined above, instrumented by the corresponding IV. We control for school endowments at LLM-level, LLM fixed effects, region-year fixed effects, and cluster standard errors at LLM level. Formally,

$$y_{ct} = \alpha + \beta\, Clo\hat{s}ure_{ct} + \gamma_c + \delta_t + \eta\, X_{ct} + \theta_{rt} + \epsilon_{ct} \quad (9)$$

where $c$ refers to LLM and $r$ to region, and $Clo\hat{s}ure_{ct}$ is obtained from the following first stage regression:

$$Closure_{ct} = \alpha + \beta\, LLM\ IV_{ct} + \gamma_c + \delta_t + \eta\, X_{ct} + \theta_{rt} + \epsilon_{ct} \quad (10)$$

We split our sample by LLMs including or not including the administrative centre of a province (referred to as 'provincial city'). Provinces represent an intermediate level of government between municipalities and regions.[45] The corresponding administrative centres are generally sizeable urban areas, so that LLMs (not) including a provincial city can be thought of as more central (peripheral) areas.

The second stage results are illustrated in Table 5, while the first stage is in online Appendix Table C9. We interpret these estimates as suggestive evidence on the effects of school closures at a larger geographical scale. 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 (or considerably reduces in size) when we focus only on LLMs containing a provincial city. 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 suffer little or no consequences from the closure of primary schools in single-primary-school municipalities.

**Table 5**
Cumulative effect of school closures at Local Labour Market level.

| | School-age population | Potential parents | Total income |
|---|---|---|---|
| *Panel a:* ***LLMs without provincial city*** | | | |
| Number of school closures | −0.030*** | −0.024*** | −0.011* |
| | (0.0102) | (0.0083) | (0.0057) |
| F-test | 50.46 | 50.46 | 50.46 |
| N | 2,160 | 2,160 | 2,160 |
| *Panel b:* ***LLMs with provincial city*** | | | |
| Number of school closures | −0.012* | −0.013 | −0.001 |
| | (0.0067) | (0.0084) | (0.0058) |
| F-test | 10.85 | 10.85 | 10.85 |
| N | 492 | 492 | 492 |
| *Other school endowments* | ✓ | ✓ | ✓ |
| *LLM fe* | ✓ | ✓ | ✓ |
| *Region-year fe* | ✓ | ✓ | ✓ |

Clustered standard errors at LLM level in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Second stage estimation of Eq. (9), regressing LLM-level school-age population, potential parents, and total income on the cumulative number of single-primary-school closures occurred in that LLM at any given year over the 2008–19 period, instrumented by the number of single-primary-schools below the regional threshold. All specifications include controls for other school endowments – public and private –, LLM and region-year fixed effects. Sample of all LLMs in regions adopting thresholds, excluding cross-region LLMs.

[45] In the period considered, Italy had 107 provinces.

## 8. Concluding remarks

This paper has studied the local impact of spending cuts to 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 implementation should not be uniform across space and be visible mainly in already lagging behind areas. 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 costs that are independent of school size. The reduction of these costs is the primary goal of rationalisation policies. 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 results in a reduction in the population of children of mandatory school age by 8 to 16%, and a decrease in the population of potential parents – i.e. residents between 35 and 49 years old – by 11 to 18%. 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 9-to-12% reduction in taxable income in these municipalities.

The estimated effect of school closures on residential choices and income appears to be mainly driven by peripheral municipalities, i.e. those located at a distance from the centre of the corresponding local labour market, or those with less access to alternative primary schools. The schools in neighbouring municipalities manage to absorb only a portion of pupils left without a school after closures, and only in the short-term.

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.

In our analysis, we mostly focus on single-primary-school municipalities. 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.

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. In this paper, our aim is to study the local impact of school closures, while we reserve the investigation of individual-level consequences 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 on an aggregate scale — this may well not be the case. Rather, our aim is to highlight the consequences and possible side effects of these policies. These can be viewed from the perspective of the affected places and from the perspective of the affected people.

Seen from the perspective of places, the study has shown that small-size peripheral municipalities are those most impacted by the population loss. These territories are also shown to be on pre-existing depopulation trends, with school closures determining a substantial acceleration in their ongoing decline. However, the fact that these areas are already losing population does not necessarily imply that they will eventually 'die out'. In the last decade, the Italian State has implemented the so-called Inner Area Strategy, specifically dedicated to prevent the disappearance of peripheral local areas suffering from the lack of public services and employment opportunities (SNAI, 2014). This programme operates in conjunction with EU Cohesion Policy and it has been established in 2014, though the very first disbursements for projects related to this Strategy only occurred in 2018–19. As part of it, almost 2000 dedicated projects have been approved with around €400 million being committed over the next years.[46] No similar programme existed in the past, and it will be interesting to verify whether such an investment, conceptually in sharp contrast with concurrent rationalisation policies, will be able to revitalise local areas depressed by the removal of essential public education services. Our analysis has shown that these communities are losing a substantial share of residents as a result of school closures, some of whom may choose to relocate to larger cities. Yet, it is far from obvious that big urban areas are prepared to host them. These internal migrations – if not properly addressed by policy-makers – can lead to congestion, higher costs of rent, and worsening living conditions in larger cities.

Seen from the perspective of the affected citizens, the analysis has demonstrated that 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 well be the case that relocating households 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 also worth highlighting that not all inhabitants of local peripheral areas 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. Furthermore, some people may have strong idiosyncratic preferences for living in those places, and be forced to move by the closure of key services.

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.

## CRediT authorship contribution statement

**Marco Di Cataldo:** Writing – review & editing, Writing – original draft, Validation, Supervision, Methodology, Formal analysis, Data curation, Conceptualization. **Giulia Romani:** Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

## Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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## Appendix A. Supplementary data

Supplementary material related to this article and including all Appendix figures and tables referred to in this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2024.104057.

## Data availability

Data will be made available on request.

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[46] Source: opencoesione.gov.it/it/dati/strategie/AI/.

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