Efficiency cuts: the local impact of closing undersized schools

Marco Di Cataldo and Giulia Romani*

February, 2022

Abstract

The availability of local public goods can influence residential choices. Schools are especially relevant in this respect, since households with children have a daily need for school services. Hence, rationalisation policies aiming to increase efficiency by cutting on undersized nodes of the school network can induce population decline. This paper investigates the demographic and income effects of primary school closures by focusing on the Italian context (2010-2019), exploiting a 2008 education reform that produced a significant contraction of the school network. We assess whether school closures have an impact on citizens' residential choices, on top and beyond preexisting negative population trends which motivate school closures. We address endogeneity by combining a two-way-fixed-effects estimation with an instrumental variable approach, constructing the IV on the basis of institutional thresholds for school sizing adopted by some Italian Regions. Our findings suggest that municipalities affected by school closures experience significant reduction in population and income. The effect is driven by municipalities far away from economic centres, and distant to further schools. This evidence indicates that schooling rationalisation policies, by inducing depopulation of peripheral areas, has an influence on the spatial distribution of the labour force and of income, thus affecting intra-regional disparities.

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

Acknowledgements: we are especially thankful to Olmo Silva, Jorge Pérez Pérez, Daniel Sturm, and Enrico Rettore for their helpful advice. We also thank all participants to the Ca' Foscari-Groningen Economics seminar, the 2020 AISRe conference, the 2021 ERSA conference, the 2021 UEA European and North-American Meetings, and the 2021 UEA Summer school for useful comments and suggestions. All errors are our own.

*Ca' Foscari University of Venice. Corresponding author: giulia.romani@unive.it

1 Introduction

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

The idea of people moving across jurisdictions to access public services has a long tradition, stemming from Tiebout's (1956) proposition of people 'voting with their feet', i.e. the idea that people relocate in space in order to find the jurisdictional unit maximising their public goods' preferences. Empirical studies testing the validity of Tiebout's hypothesis have investigated the role of public schools for residential choices by focusing on school quality differentials, either by looking at inter-district choice programmes (Brunner et al., 2012), or indirectly by estimating changes in house prices (Black, 1999; Fack and Grenet, 2010; Gibbons and Machin, 2006; Gibbons et al., 2013). In this article we take a different perspective and assess the impact that public policies impacting on the distribution of school services can have on residential choices.

In implementing policies that influence school services, one of the fundamental goals for the legislator is the development of scale economies (Urquiola, 2005; Alesina et al., 2004). The whole idea of 'rationalisation policies' - i.e. policies removing undersized service centres in order to increase economic efficiency - is based on the fact that public interventions should trigger the untapped potential for scale economies. However, this kind of policies may also shape the location decision of households and, by providing unequal incentives for relocation depending on income levels, they may affect income differentials across space. 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 to exploit economies of scale.

Our focus is on Italy, and particularly an education reform that took place in 2008 in the country. The Italian context represents an interesting and unique analytical setting for our purpose. On the one hand, despite the traditionally low mobility of the Italian population, there is evidence of significant internal migrations, mainly directed towards big urban centres. This especially concerns young adults with children, representing the highest fraction of all internal migrants[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 rationalisation of the Italian school network. The objective of the reform was to cut on public spending by eliminating undersized centres of service provision. Per-student public expenditures were considered excessively high, a feature attributed by the reform to the too geographically dispersed configuration of the Italian schooling system. This has implied a significant reduction of educational infrastructure and the closure of several schools across the country.

Such a drastic rationalisation of schooling services may have affected population dynamics[2]. 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 for changing residence.

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

[2]The link between population dynamics and access to services lies at the core of the National Strategy for Inner Areas SNAI (2014), which constitutes one of the largest ongoing National policy efforts to address the Italian regional disparities. This cohesion programme is based on the verification that Italy has progressively evolved from a polycentric configuration towards a core-periphery structure, where people, opportunities and services are always more concentrated in few attractive centres, to the detriment of the rest of the country. For an historical perspective on Italy's territorial issues, see Borghi (2017).

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

Our analysis faces a fundamental empirical challenge, in that primary school closures mainly concern municipalities experiencing negative population trends. We address this empirical issue through a Two-Way-Fixed-Effects (TWFE) model in combination with an instrumental variable approach. Our instrument exploits institutional rules governing primary school sizing, enforced by some Italian Regions during the period of analysis. We construct the instrument as a dichotomous variable taking value one if the school was below the threshold before its introduction. As a robustness check, we exploit the margin of deviation from the threshold as a predictor of school closure.

Our findings suggest that school cuts negatively affect population dynamics on top and beyond preexisting trends. The effect is sizeable for children in mandatory school age and young adults, which represent the most direct recipients of school services and hence are the most negatively affected by primary school closures. Conversely, no effect is found on the elder population, less likely be affected in any way by educational infrastructure cuts. We also find a reduction in total income of municipalities where education services are reduced, while per-capita income increases after schools are closed. This is attributable to the concentration of young adults among low-income taxpayers. The overall impact of school closures on municipal depopulation is mainly driven by peripheral municipalities, i.e. those above the median distance from economic centres. Finally, the depopulation effect is stronger the further away the next available school is located.

The remainder of the paper is structured as follows. Section 2 reviews the related literature and locate our contribution; section 3 describes the institutional context of the Italian schooling system and the key elements of the recent reform process; section 4 presents the dataset and some descriptive statistics; sections 5 and 6 outline our main empirical strategy and the related results; section 7 explores the heterogeneity of the effect by municipality location; section 8 describes the robustness strategy and its results; section 9 concludes.

2 Literature review and contribution

There exists a large body of literature studying the way in which residential choices respond to the provision of public services. The seminal contribution of Tiebout (1956) postulates that, in a context of decentralised provision of tied-to-residence public goods, citizens would relocate in order to match their preferences. Such a hypothesis has recently undergone several empirical tests, with contributions focusing on different kinds of local services or amenities. For example, Banzhaf and Walsh (2008) and Gamper-Rabindran and Timmins (2011) study the population and income effects of variations in local environmental quality. Kahn (2007), instead, deals with the gentrification impact of new rail transit lines, looking at house prices and local percentages of college graduates. All in all, these studies tend to confirm that citizens are willing to move to places offering them desirable amenities and public services.

Focusing on schools, Hoxby (2000) analyses the relation between Tiebout choice and school productivity, while Baum-Snow and Lutz (2011) investigate the residential and school choice response to the desegregation of public school districts. In the context of schooling, the first direct empirical evidence of Tiebout choice is provided by Brunner et al. (2012), who assess the impact of inter-district choice programmes on residential location decisions. As for school service, it is also worth mentioning a number of papers performing indirect tests of Tiebout's hypothesis by looking at house prices. A seminal contribution is provided by Black (1999). More recent ones are Fack and Grenet (2010), Gibbons and Machin (2006) and Gibbons et al. (2013). These works agree in that public school performance is capitalised into house prices and parents are willing to bear higher housing costs to access better quality schools. Moreover, Fack and Grenet (2010) show that private schools mitigate the effect, offering an advantageous outside option. These empirical works largely confirm the predictions of theoretical models of school choice, suggesting that increased choice reduces district disparities in terms of income and housing values. In this regard, relevant contributions are those by Nechyba (2000), Nechyba (2003), and Ferreyra (2007), who study the role of private school voucher programmes, and the work of Epple and Romano (2003), who compare neighbourhood and open enrollment policy regimes. In these models, households evaluate school alternatives, whose quality depends on per-student expenditure and peer-average performance.

It is no coincidence that many of these works have placed their attention on school services. First of all, schools are especially relevant for residential choices, since households with children have a daily need for schooling. Secondly, school systems display a variety of policy regimes that can be exploited to investigate the dynamics of interest. In particular, school finance can be managed at local or central level and enrollment rules can allow for school choice outside the district of residence. 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 scale economies in schooling provisions. Exceptions in this respect are Urquiola (2005) and Alesina et al. (2004), who recognise the existence of school fixed costs that make average cost decreasing in district size. These works, however, are more concerned with the formation of jurisdictions (here, school districts) in response to the trade-off between scale economies and the costs of community heterogeneity. In practice, school fixed costs represent a significant issue for the 'survival' of undersized complexes. They mainly consist of infrastructure maintenance and teachers per class and can result in high per-student expenditure for small-sized schools. The implementation of 'rationalisation policies' on schools represents in itself a proof of existing scale economies in education services, in that these policy measures consist of eliminating undersized centres in order to increase economic efficiency.

Another aspect largely overlooked by the literature is that of transport costs to access schools[3]. These can play a relevant role in households location decisions and are strongly connected to the organisation of school network. Specifically, undersized schools are mostly located in peripheral areas, so that school cuts are likely to increase considerably transport costs to access school. In turn, this can induce households to reconsider their residential choices.

The interaction between scale economies and transport costs is also at the centre of the New Economic Geography (NEG) tradition (Krugman, 1991). This literature strand[4] 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'. Once activated, this self-sustaining dynamic produces a core-periphery pattern, whereby residence and industry are increasingly concentrated. In this literature, the public sector mainly enters through the provision of infrastructure to firms (Ottaviano, 2008). Residential choices are either confined to wage differentials or neglected, assuming immobile workers.

[3]Exceptions in this regard are Epple and Romano (2003), Urquiola (2005) and Alesina et al. (2004).

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

The intuition behind this paper is that similar core-periphery patterns could arise from changes in the provision of public services characterised by relevant scale economies. Specifically, government cutting undersized schools to exploit economies of scale may induce households to relocate closer to other schools. This kind of dynamics could foster the concentration of people and services in denser areas, to the detriment of more peripheral locations. Therefore, rationalisation policies could have the effect of widening regional disparities.

To the best of our knowledge, rationalisation policies have not been subject to any systematic evaluation in terms of households location choices. The only exception we are aware of is Gibbons et al. (2018). In that contribution, the authors assess the population effect of British rail cuts directed towards non-profitable rail lines over the period 1951-1981, finding that transport infrastructure service cuts substantially affect population dynamics. This paper aims to fill this gap by studying the role played by scale economies in the provision of public schooling and, consequently, shed light on the related dynamics of households residential choice.

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

[5]For an 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 '90s, the establishment of school autonomy and the 2001 reform of the Italian Constitution have contributed to such process.

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[8].

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[9]. In making primary school choices for their children, parents have to combine work and family needs. As a consequence, house-school commuting times become particularly relevant in orienting these choices. Conversely, school *quality* appears less of a determinant for selecting primary schools. This is due to the fact that in the Italian context there is basically no school tracking in educational offer over the first educational cycle[10], so that in principle school quality of primary schools is approximately equalised, at least

[6]In all OECD countries, school expenditure accounts for 90% of current expenditures. Four fifths of that amount consists of personnel' 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: http://dati.istat.it/).

[8]From our elaborations on data from the Ministry of Education, excluding Trentino-Alto Adige and Valle d'Aosta, public preschools managed by the national government range from 75% to almost 100% across Italian regions. In Trentino-Alto Adige and Valle d'Aosta schools are under the jurisdiction of the autonomous Provinces.

[9]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 n. 22994 for school year 2020-21 at the link https://www.miur.gov.it/web/guest/-/iscrizioni-alle-scuole-dell-infanzia-e-alle-scuole-di-ogni-ordine-e-grado-anno-scolastico-2020-2021. At least since 2008, these rules have been the same.

[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.

within provinces. Indeed, the strongest evidence of sorting across schools on the basis of school quality is visible at the level of higher secondary school (*scuola secondaria di secondo grado*), whereas it does not seem particularly relevant for the first educational cycle[11]. This is one of the reasons that induced us to focus on primary schools for our analysis. In conclusion, at least for the first educational cycle, residence and school choice are not completely independent. In particular, it seems plausible that households take into account distance to school when evaluating residence decisions.

The distribution of schooling services across the country depends on laws regulating two fundamental aspects: the criteria for class formation and the guidelines for the organisation of the school network. Concerning the former, since 2009 class formation is regulated nationally by the Ministry of Education (MIUR) through decree 81/2009, part of the 'Gelmini reform', which substituted decree 331/1998. More details on this are reported in the following subsection. For what concerns the latter, the guidelines for the organisation of school networks are provided by each Italian Region, independently for its own territory, and they contain directives on activation, suppression and merger of school complexes[12]. According to such guidelines, each Italian Province elaborates sizing plans, agreeing any variations in the school network with school institutions and municipal authorities. If consistent with Regional guidelines, provincial plans are approved. Taken together, the plans of all Provinces composing each Region constitute the annual school sizing Regional plan (*Piano di dimensionamento scolastico regionale*).

In defining these plans, it is important to note that Regional authorities are constrained by the number of public school workers assigned to each Region by the central government. The truly binding constraint to class and school activation is

[11]For the second educational cycle, the differentiation between vocational (*istituti tecnici-professionali*) and high schools (*licei*) leads to self-selection of students according to their family background. On school tracking and social segregation, see Bertola and Checchi (2004) and Brunello and Checchi (2007).

[12]The normative references in this regard are law 112/1998 and the reformed fifth title of the Italian Constitution.

represented by the scarcity of teachers and janitors, which are the more valuable and costly resource of the schooling system[13]. In this framework, each single school has little control over its own activation and/or suppression. Indeed, school workforce is assigned on the basis of student enrolments (*organico di diritto*). This is then adjusted to cover particular and transitory needs, giving the effective personnel for the school year (*organico di fatto*). Therefore, despite the formal decentralisation of power on these matters to Regions and Provinces, central government's reforms crucially affect the organisation of school network. For this reason, for the purpose of our analysis we focus our attention on the most recent and decisive effort of school network's rationalisation produced by the central State, the national reform realised by the Minister for Education Gelmini in 2008.

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 the Italian demography has considerably changed, increasing the population of already larger cities to the detriment of more peripheral areas, which have been shrinking. This phenomenon was mainly due to changes in the economic structure of the country, inducing residential mobility towards cities providing better job opportunities. In addition, since the 1990s policies of rationalisation started to be implemented in the field of public services. This kind of policies also concerned public education, which in recent decades experienced continuous expenditure cuts. 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 number of school complexes (i.e. single or multi-school structures) and classes activated (MIUR, 2010).

[13] 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.

Indeed, by 2008 rationalisation policies had mainly intervened to reduce autonomous school institutions (therefore, cutting on school directors)[14], but they had not strongly affected the distribution of school complexes. The territorial fragmentation of school complexes and the limited class size were identified as the main reasons for the high per-pupil expenditure compared to OECD countries[15].

The reform process started with law 133 of August 2008, which established the increase of 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 should derive public savings for 8 billions euros by 2012. The resulting strategy plan of the Ministry of Education states that "by almost one decade the school network has been substantially unchanged in its school complexes (*plessi*) and institutions (*istituzioni scolastiche*); this, despite the demographic dynamics which have emptied or excessively filled school classrooms, and made difficult or superfluous the management and coordination of schools".

In the same document, the Ministry declares the need to eliminate undersized school complexes, clarifying that "the institution, suppression or merge of schools is under the jurisdiction of regions and local authorities according to law 112/1998 and to the reformed fifth title of the Constitution, on the basis of sizing criteria defined by the Ministry of Education with the specific regulation planned by art. 64"[16]. This is the already mentioned decree 81/2009, which revised the numerical limits to form initial classes, ordered the increase in pupils/class ratio and allowed for exceptions only in

[14]Autonomous school institutions are legal entities which comprehend multiple school complexes. They are managed by a single school director, who has - in principle - some autonomy in the organisation of the member schools. School autonomy was introduced in the Italian system by law n.21 of 1997, and reformed by presidential decree (D.P.R.) n. 275 of 1999.

[15]These arguments are maintained by Fontana (2008) and supported by the analysis of MIUR (2007). This report states that the 2007 school network was historically inherited, since the consolidation process - by that time - had not reduced the number of school complexes.

[16]*Schema di Piano Programmatico del Ministero dell'Istruzione, dell'Università e della Ricerca di concerto col Ministero dell'Economia e delle Finanze.*

case of effective increase in schooling population[17]. It still constitutes the normative reference for class formation in all Regional guidelines for the elaboration of sizing plans.

In addition, some Regions may establish numerical criteria for the activation or suppression of school complexes. Instead, proper numerical criteria have been introduced by seven Italian Regions over the period considered. The Regions setting up this type of criteria are Veneto, Piedmont (Piemonte), Lazio, Calabria, Friuli Venezia-Giulia, Tuscany (Toscana), and Sardinia (Sardegna). The timeline of Regional reforms varies, and it is displayed in Figure 1. These criteria consist of thresholds on the minimum number of required students in order to keep a school active[18]. 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, from 2018, Tuscany, which set up a threshold of 35 students. It is also worth clarifying that Apulia had numerical thresholds in its sizing plans until 2011. Since our analysis starts in 2010, we exclude that Region when focusing on the sub-sample of those adopting thresholds[19].

The Regions that do no have specific school-closing thresholds simply formulate general norms for the organisation of the school network. These include directives towards a more rational distribution of school complexes, to be achieved through the suppression of the undersized ones. Particularly in the case of mountain areas or small islands, these norms generally recommend to consider the distance to further schools.

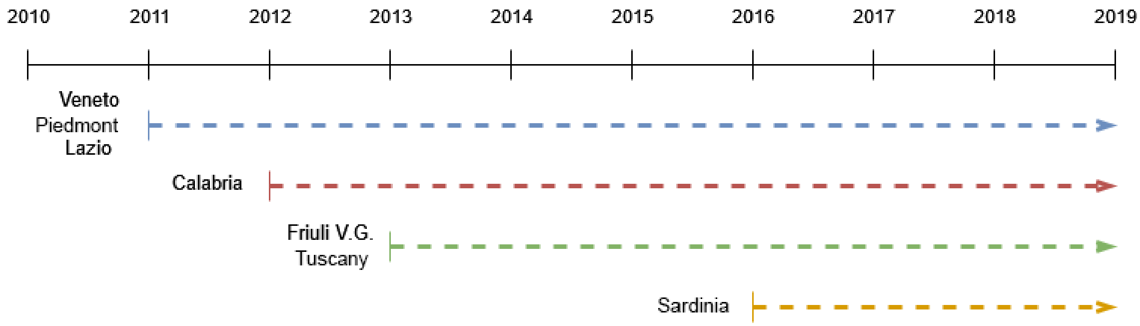
---

[17] *Norme per la riorganizzazione della rete scolastica e il razionale ed efficace utilizzo delle risorse umane nella scuola, ai sensi art. 64, comma 4, del decreto legge 25 giugno 2008 n.112, convertito, con modificazioni, dalla legge 6 agosto 2008, n.133.*

[18] 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

[19] More details, guidelines for Regional sizing plans can be found on the Regions' websites or requested to competent Regional offices.

Figure 1: Timeline for the introduction of Regional thresholds



Note: The graph reports the year in which different Regions introduced numerical thresholds for school closure over the period we consider.

4 Data and Descriptives

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

We investigate the effects of school rationalisation policies on *public* schools, employing the information about private schools to control for substitute services. We exclude from our analysis the Regions of Trentino-Alto Adige and Valle d'Aosta because school policy and school funding in those two Regions are regulated by the jurisdiction of the autonomous Provinces, therefore following different dynamics compared to the rest of the country.

We look at the impact of the closure of *primary* schools and we use municipalities as units of analysis. Therefore, for each municipality in sample, primary school closure represents the treatment. The choice of focusing on primary schools is motivated by the fact that primary school attendance lasts five years and it is mandatory, primary schools are extremely diffused and dispersed across Italy, and primary school children still depend on their parents for daily commuting. Conversely, preschools and lower secondary schools lack some of these features and therefore seem less suitable for our analysis.

To identify school closures, we exploit the information about the exact location of each school and their state of activity, i.e. whether they are active or they are closed. Data is available for every school year from 2009/2010 to 2018/2019. We associate the school year 2009/2010 to outcome variables referring to 2010, and similarly for subsequent years. Moreover, if a given school is absent from the dataset for school year 2010/2011, in our municipality-year dataset we consider a municipality 'treated' - i.e. experiencing primary school closure - from 2010. The reason behind this timing choice is that school sizing plans for a given year are approved by December of the previous year. This means that if, for instance, the school complex is not activated for school year 2010-2011, the decision about the closure is taken in December 2009 and the announcement is made at the beginning of 2010. In light of this, we consider 2010 as the first year in which the school is closed.

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

By exploiting the different age groups available, we can focus on two of them in particular. The first is the residential population in mandatory school age (5 to 14 years old)[21], which we assume is the most directly affected by primary school closures.

[20] This information is available from ISTAT warehouse http://dati.istat.it/ only from 2019, whereas historical data on municipal demography is available at http://demo.istat.it/archivio.html.

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

The second is the group including the pupils' potential parents, which we identify as at individuals between 35 and 49 years old, who possibly became parents between 25 and 44 years old.

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

From ISTAT[23] we collect other municipal-level variables: land area in square km, municipal centre's elevation, and an indicator of whether a municipality is an island.We also collect data on the Local Labour Market (LLM, *Sistema Locale del Lavoro*) each municipaly belongs to, to control for labour market conditions[24].

To define the sample of municipalities for the analysis, we select only municipalities that have not undergone processes of administrative reorganisation at municipal level[25], so that we can easily trace the municipal unit over the entire period considered. In addition, we focus exclusively on municipalities that have *only one* primary school within their borders at the beginning of the sample period (i.e. school year 2009/2010).

We perform the analysis by exploiting single-school municipalities because for them catchment areas essentially coincide with municipal boundaries. Therefore, almost all residents (in key age classes) of those municipalities are the recipients of given school

[22]Data are publicly available at https://www1.finanze.gov.it/finanze3/analisistat

[23]Data are available at https://www.istat.it/it/archivio/156224.

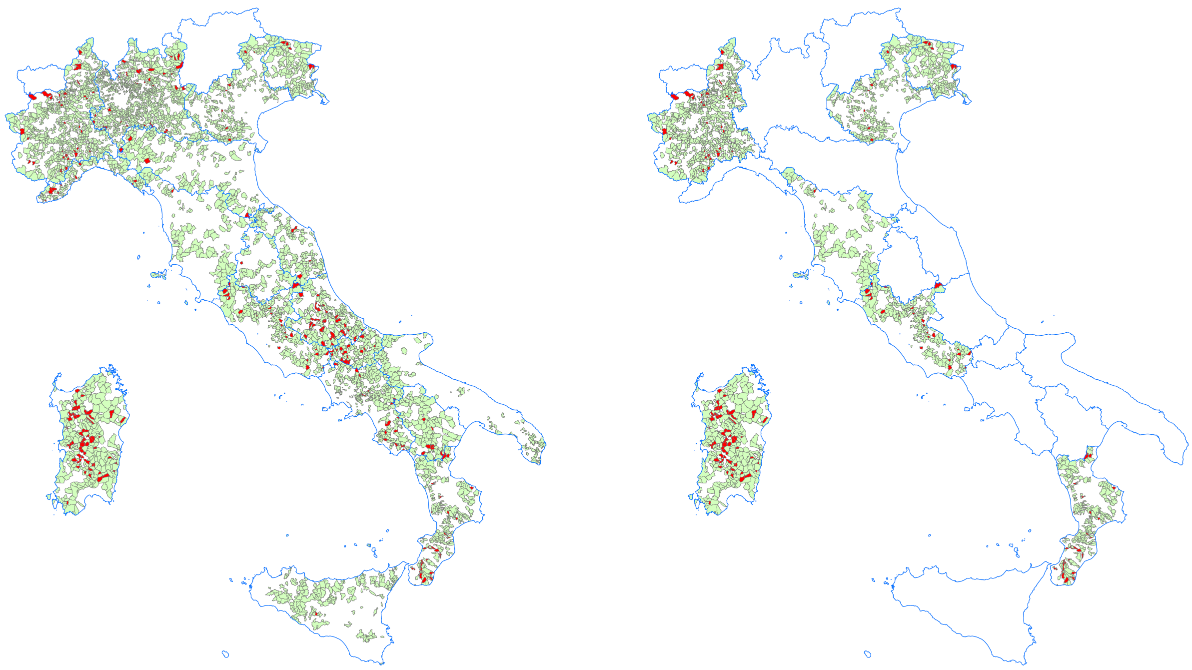
[24]Data can be found at https://www.istat.it/it/informazioni-territoriali-e-cartografiche/sistemi-locali-del-lavoro. LLM boundaries are re-defined every census. Given the period of analysis, we refer to the 2011 LLM definition.

[25]We still include municipalities which have changed Province or Region over the period considered. More details and data about administrative reorganisations in Italy can be found at https://www.istat.it/it/archivio/6789 and https://www.tuttitalia.it/variazioni-amministrative/.

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 should not be affected by school closure in their residence municipality, hence biasing downward the magnitude of the estimated effect of school closure on municipal residents.

Figure 2: Single-primary-school municipalities: closures

a) All sample b) Regions with thresholds



Note: The map in Panel a) shows all single-primary-school municipalities, reporting in colour red those which experienced school closures over the period considered (2010-2019), and in colour green those which did not. The map in Panel b), instead, focuses only on Regions which introduced numerical thresholds for school sizing over the span considered.

To provide visual representations of our sample, Figure A2 in the Appendix plots the geographical distribution of primary schools by municipality in the first school year considered, i.e. 2009/2010. Light yellow municipalities, with one or no schools, represent our sample. They make 57% of all coloured municipalities in the Figure.

The set of single-school municipalities is shown in Panel a) of Figure 2. In this Figure, red municipalities are those experiencing school closures during the time span considered (treated units), whereas the green ones are those that do not (control units). Panel b) of Figure 2 restricts the sample to single-school municipalities from Regions adopting numerical thresholds for school sizing over the period considered[26]. As can be seen from the map, they are fairly evenly distributed across the whole Italian territory and they include areas with different morphological characteristics.

The choice of focusing on this restricted group of municipalities clearly reduces the amount of 'treated' observations, as compared to a sample considering multiple-schools municipalities. However, we prefer to adopt a conservative approach and to look for an effect where we expect it can be more clearly identified. By restricting the analysis to municipalities with a single primary school in 2010, we are left with 271 municipalities with primary school closures over a total of 4,006, which distribute across Regions as shown in Table 1. We report in bold the Regions which introduced specific numerical criteria for school closures during the period observed.

[26]See the Institutional context section for more details about those criteria for school sizing.

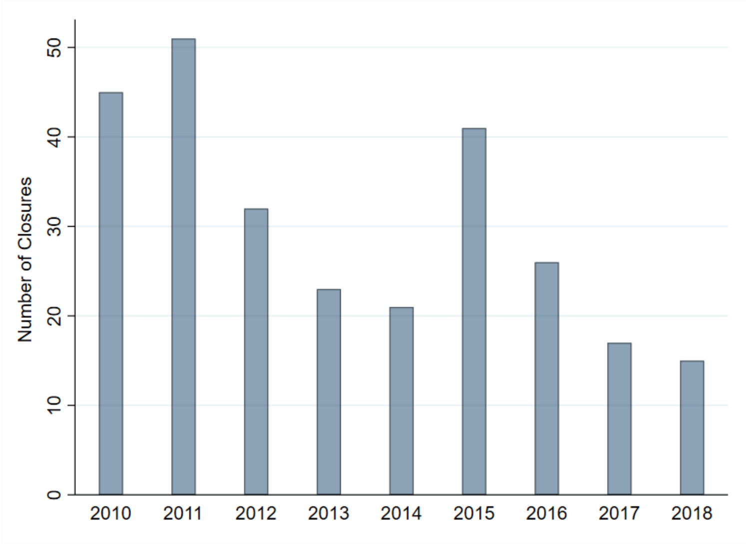
Table 1: Single-Primary-School Municipalities: closures by Region (2010-2019)

Region	No Closure	Closure	Total
Abruzzi	159	29	188
Apulia	108	1	109
Basilicata	88	8	96
Calabria	190	22	212
Campania	276	19	295
Emilia Romagna	140	3	143
Friuli V.G.	128	5	133
Lazio	205	22	227
Liguria	126	9	135
Lombardy	922	35	957
Marche	136	4	140
Molise	82	16	98
Piedmont	649	35	684
Sardinia	216	53	269
Sicilia	174	1	175
Tuscany	96	1	97
Umbria	49	1	50
Veneto	262	7	269
Total	4,006	271	4,277

Note: The Table reports, for each Region, the number of municipalities endowed with a single primary school in 2009/2010, which experienced or not school closures over the period considered (2010-2019). We highlight in bold the Regions which introduced numerical thresholds for school sizing over the span observed.

The timing of school closures is also relevant. Focusing again on the sample of municipalities with a single primary school in 2009/2010, we can notice a concentration of closure cases in the first three years observed (Figure 3). Indeed, that period 2010-2012 coincides with the horizon indicated by the 'Gelmini reform' for collecting 8 billions euros in public savings through the rationalisation policies induced by the reform. As discussed, the 'Gelmini reform' mostly modified criteria for class formation, while the definition of the school network is under the Regions' jurisdiction. However, those reformed criteria entered the guidelines for school sizing of all Regions and constituted a more stringent constraint to the activation of school complexes.

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



Note: The graph plots the number of single-primary-school closures by year over the period considered.

5 Empirical strategy: TWFE + IV

Our sample consists of municipalities with only one primary school experiencing the closure of that school - an event which can take place at any moment during the 2010-2018 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:

$$y_{ict} = \alpha + \beta \, Closure_{ict} + \gamma_i + \delta_t + \eta \, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{1}$$

where i is the municipality identifier, t is the year index, and c indicates the LLM to which the municipality belongs. Equation 1 refers to our starting model, a two way fixed effects (TWFE) model, where we regress our outcomes of interest (population in key age classes and municipal income) against time (δ_t) and municipal (γ_t) fixed effects and a treatment dummy for school closure ($Closure_{ict}$). The treatment variable takes value 1 from the year of closure of the only primary school in a given municipality until the end of the period, and 0 before that. The model includes controls for complementary and substitute school services (X_{ict}): the endowments of public pre-schools, public lower secondary schools, and private schools of any order (primary schools included). Moreover, we also add year-local labour markets (LLM) interacted fixed effects (θ_{ct}), to compare municipalities exposed to the same labour market conditions and control for any time-varying factors within local labour market.

The key identifying assumptions underlying the empirical strategy 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 corresponding to Equation 1:

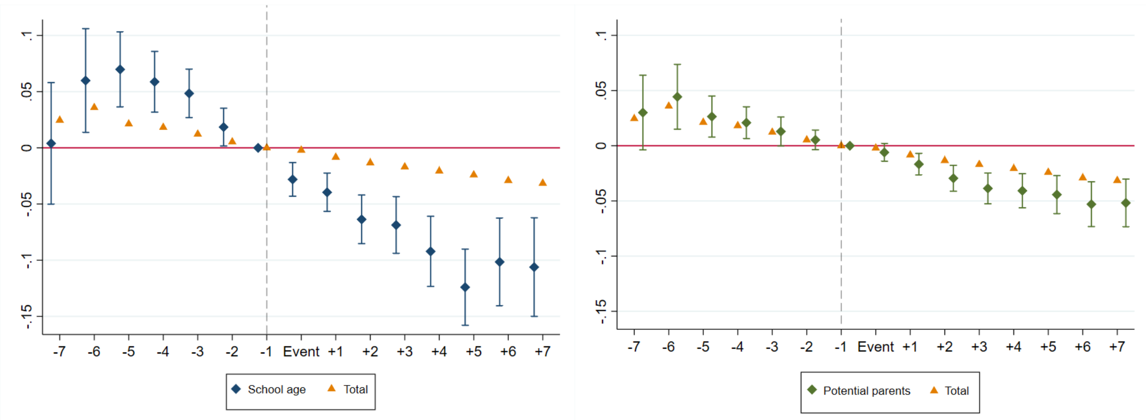
$$y_{ict} = \alpha + \Sigma_{m=-G}^{M} \beta_m \, z_{ic(t-m)} + \gamma_i + \delta_t + \eta \, X_{ict} + \theta_{ct} + \epsilon_{ict}, \tag{2}$$

where the term $\Sigma_{m=-G}^{M} \beta_m \, z_{ic(t-m)}$ refers to a set of leads and lags dummy variables before and after the treatment event, capturing the possible dynamic effects of the treatment. Specifically, the outcome at time t can only be directly affected by the value of the policy at most M ≥ 0 periods before t and at most G ≥ 0 periods after t (Freyaldenhoven et al., 2021). In other words, by choosing the values for M and G, we allow the policy effects to last at most M periods after treatment and to begin no more than G period before. Under the assumption of no anticipation, G will be equal to zero, and all the pre-treatment coefficients should be non-significant for the parallel trends assumption to hold. Indeed, the estimated $\{\beta_m\}_{m=-G}^{M}$ can be interpreted as the cumulative effect of the policy up to period $(t-m)$. Therefore, the significance of pre-treatment coefficients highlights pre-existing trends ('pre-trends') in the outcome, casting doubts on the identification of treatment effects.

It is common in this kind of applications to visualise $\{\beta_m\}_{m=-G}^{M}$ estimates through an event study plot, which provides a clear visual intuition of the plausibility of the identifying assumptions. We conform to that practice and report in Figure 4 the estimated $\{\beta_m\}_{m=-G}^{M}$ for Equation 2, as estimated with three different dependent variables: the population of school-age children, total residents, and population of potential parents.

Figure 4: Population by age classes around school closure

a. b.



Note: The Figure shows the event study plots corresponding to Equation 2, where dependent variable is (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.

As it can be seen from the plot, all outcomes show pre-trends, which can be due either to anticipatory responses or to pre-existing negative trends in single-school municipalities experiencing school closures. Both explanations are plausible in our context. Indeed, it is common that school cuts are discussed for few years before being actually put in place[27] and young adults are likely to adapt their fertility and/or residence choices according to the expected change. By definition school rationalisation policies affect municipalities in population decline, and this constitutes the greatest challenge for the parallel trend assumption to be met. School cuts take

[27]The school sizing plans specify that recent demographic trends may be a condition leading to the decision of closing the school. Moreover, informal interviews with school directors confirm that school cuts usually occur after few years in which the first grade has not been activated due to insufficient demand. Therefore, the decision of school closure is likely to be anticipated by few years by local residents.

place precisely where the demand for school services is shrinking, making its provision inefficient. The pre-trends displayed in Figure 4 confirm this. They are especially marked for the population of potential recipients of that service: school-age children (Figure 4 a.) and potential parents (Figure 4 b.).

There is a growing literature discussing identification issues due to treatment effect dynamics in setting with staggered adoption (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). These contributions highlight that heterogeneity in treatment effects across cohorts may represent a severe bias in such context, as event study estimates of pre-treatment periods can be contaminated by post-treatment effects, invalidating the common procedure of testing for pre-trends by looking at pre-treatment coefficients (Sun and Abraham, 2021). We follow this literature strand and adopt the estimator proposed by Sun and Abraham (2021), allowing to compute event studies as weighted averages of cohort-specific ATT_s, with weights corresponding to the shares of treatment cohorts. We report the corresponding event study plots in Appendix Figure A3. As clearly visible in the Figure, these estimates still display significant pre-trends, indicating that identification concerns are not resolved by such correction. Instead, pre-trends are likely to derive from a combination of anticipatory behaviour and pre-determined municipal demographic conditions.

To address that identification issue, we combine the TWFE estimation presented above with an Instrumental Variable (IV) strategy[28]. We construct our instrument exploiting the institutional rules on school sizing adopted by some Italian Regions over the period considered. For our IV model we restrict the analysis to the sample of Regions adopting school sizing thresholds, illustrated in Panel b. of Figure 2[29] and

[28] 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).

[29] These are Friuli V.G., Veneto, Piedmont, Tuscany, Lazio, Calabria, Sardinia.

construct the following instrument:

$$IV_{irt} = S_{ir,2010} \cdot T_{rt} \tag{3}$$

where $S_{ir,2010}$ is a dummy variable taking value one if school i in Region r was below the Regionally-set threshold on school size in the first observed year (i.e. school year 2009/2010), and T_{rt} is a dummy for the introduction of a threshold for school closure in Region r, year t. While all Regional thresholds have been introduced years after 2010 (see the timeline in Figure 1), we still refer to the school conditions in 2010 to construct the IV. Therefore, the instrument is constructed as an absorbing state, as a dummy taking value 1 from the moment of the introduction of the Regional threshold if the school was below that threshold in 2010, and 0 before.

The choice of employing school characteristics in 2010 - instead of contemporaneous ones - is done to achieve instrument exogeneity. By 2010 none of the sample Regions had introduced numerical criteria for school closure yet. Therefore, taking school characteristics prior to the introduction of the threshold mitigates the concerns of endogenous household response. Parents could 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. For this reason, we have selected pre-threshold school characteristics to construct our IV.

To further support our claim of instrument exogeneity, in the next section we report event study plots of reduced form estimates for a model mirroring Equation 2, where instead of computing leads and lags in the term $\Sigma_{m=-G}^{M} \beta_m \, z_{ic(t-m)}$ referring to each year from school closure, we look at periods before/after the introduction of the threshold. More specifically, we take as event time the first year in which the instrument in Equation 3 takes value one, and look at the outcome dynamics around that event. Therefore, relative period dummies take value one for those municipalities whose school was below the threshold in 2010. In those plots, the absence of pre-trends can be interpreted as evidence of instrument exogeneity, since it suggests no difference in outcome evolution among municipalities above and below the threshold prior to its introduction.

It should be noted that, for the way in which the instrument is constructed, we do not have staggered IV adoption within Regions. For all municipalities in the Region below the threshold according to their 2010 characteristics, 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[30]. Therefore, for these reduced-form regressions, we do not face treatment heterogeneity issues potentially associated with TWFE models with staggered adoption. Hence, we employ the traditional event study estimator.

After providing evidence in support of instrument exogeneity, we estimate a 2SLS model for Equation 1, where the treatment variable $Closure_{ict}$ is instrumented by the measure defined in Equation 3. Specifically, we estimate the following structural model:

$$y_{ict} = \alpha + \beta \, Clo\hat{s}ure_{ict} + \gamma_i + \delta_t + \eta \, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{4}$$

where $Clo\hat{s}ure_{ict}$ is predicted from a first stage estimation regressing school closure on the instrument in Equation 3. Formally,

$$Closure_{ict} = \alpha + \beta \, S_{ir,2010} \cdot T_{rt} + \gamma_i + \delta_t + \eta \, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{5}$$

[30]In fact, there exist some LLMs which spread across Regional borders. However, in our restricted sample, we just have four of these cases and we exclude them from sample.

6 Main results

6.1 First stage and reduced form estimates

We start by presenting first stage results, to provide evidence of the relevance and strength of our instrument. We regress the treatment variable on the instrument, including as controls other school endowments, both public and private. Moreover, we add municipality fixed effects, LLM-time dummies, and we exclude cross-Regional LLMs. By doing this, our results can be interpreted as within-Region and LLM estimates, cleaned by any contemporaneous change in LLM conditions.

Table 2: IV estimation: first stage results

	School closure
Instrument	0.113***
	(0.0126)
Other school endowments	✓
Municipality fe	✓
LLM-time fe	✓
R-squared	0.793
F test	81.33
N	18,340

Note: Standard errors in parentheses clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports first stage estimates corresponding to Equation 5, where we regress school closure on our instrument, together with controls for other school endowments - public and private -, municipality and LLM-time fixed effects. The instrument is a dummy taking value one if the school was below the Regional threshold in 2010, from the year of its introduction.

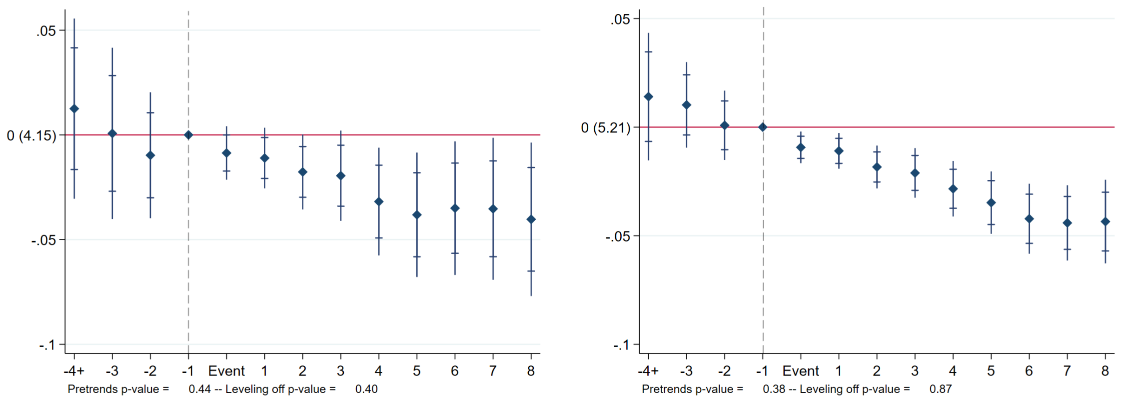
As it can be seen in Table 2, the instrument is a good predictor of the probability of treatment. For a single-school municipality, being below the threshold for school closure in 2010 increases by 11.3% the probability of experiencing school closure years after. The F-test is well above the conventional value of 10, meaning that we can safely exclude weak instrument concerns.

With regard to instrument exogeneity, instead, in the previous section we argue that constructing the IV on the basis of school characteristics in 2010 considerably reduces the risk of endogenous response by households. To further support this claim, we perform event study regressions of reduced-form models. For each dependent variable, we estimate specifications including leads and lags dummy variables of the instrument, also adding the set of controls related to other school endowments. More formally, we are estimating Equation 2 where the term $\Sigma_{m=-G}^{M} \beta_m \, z_{ic(t-m)}$ refer to lags and leads with respect to threshold introduction, instead of school closure. In this sense, we can interpret those regressions as reduced form estimations, since dependent variables are directly regressed on leads and lags of the instrument. These estimates allow to observe the evolution of the outcome variables around the event of threshold introduction. We would expect to see the absence of pre-trends as a sign of no difference in outcome dynamics among municipalities whose school was above or below a school-sizing threshold, before the introduction of the threshold. In this reduced-form setting, the verification of the parallel trend assumption can be interpreted as a test for instrument exogeneity.

Figures 5 and 6 present the results of these estimates in the form of event study plots, obtained for our main dependent variables: school-age population, potential parents' population, total income, and per capita income. Following Freyaldenhoven et al. (2021), we include in the plots the p-values of the Wald tests for pre-trends and the levelling-off test. The null hypotheses of those tests are, respectively, the absence of pre-trends and the dynamics levelling-off in the time window considered. Moreover, we augment the 95 pointwise confidence intervals with sup-t confidence bands for multiple hypotheses testing: these uniform confidence bands are designed to contain the true value of a set of coefficients at least 95 percent of the time. Finally, we also add the average value of the outcome in the year preceding threshold introduction, to give an order of magnitude for the interpretation of coefficients[31]. It is worth clarifying that we include in the plot all the post-event estimated coefficients; whereas, we summarise estimates referring to the fourth, fifth and sixth year prior to the event in the coefficient labelled '-4+'. That choice is motivated by the limited number of municipalities for which we observe up to four, five or six year before threshold introduction (see Table A1 in the Appendix for more details).

Figure 5: Event study plots of the reduced-form estimation: population

a. School-age population | b. Potential parents



Note: The Figure shows the event study plots corresponding to the reduced form of Equation 2, where dependent variable is (log) population in school age (Panel a) or (log) population of potential parents, i.e. residents between 35 and 49 years old (Panel b). Those outcome variables are regressed on leads and lags of the instrument. Note that the coefficient labelled '-4+' summarises the effect of periods from 4 to 6 years prior to threshold introduction.

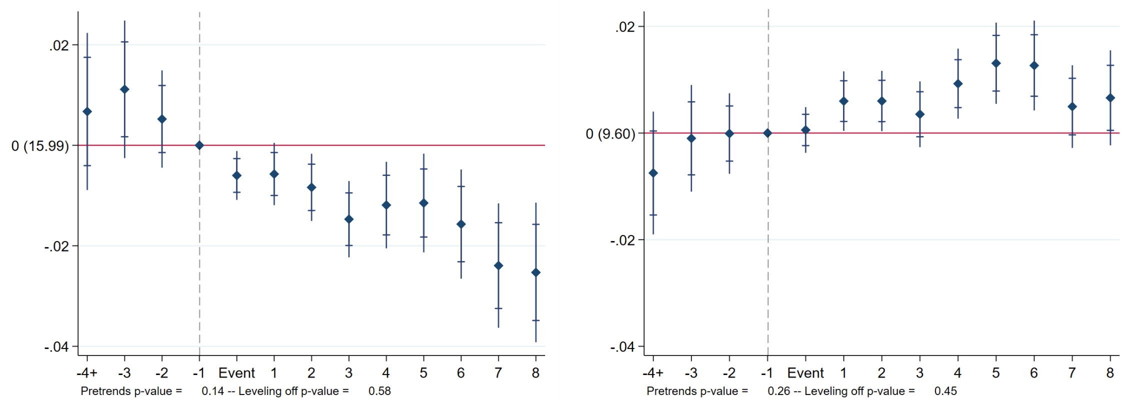
As visible in Figures 5 and 6, in no case do we detect any significant effect of the thresholds on the demographic or income dynamics of municipalities, up to four years before the thresholds' introduction. Both the hypotheses of no pre-trends and dynamics levelling-off are not rejected at conventional levels. This set of findings supports our claim of instrument exogeneity.

As a form of placebo test, we estimate the event study of the reduced form model using the municipal population between 55 and 65 years old as dependent variable. We expect such age class to be little or not affected by school closure, since these

[31] All variables are expressed in logarithm. Therefore, a value of 4.15 corresponds to an average school-age population of 69.84 children in the year preceding threshold introduction.

Figure 6: Event study plots of the reduced-form estimation: income

a. Total income b. Income per-capita



Note: The Figure shows the event study plots corresponding to the reduced form of Equation 2, where dependent variable is (log) total (Panel a) or (log) per-capita income (Panel b). Those outcome variables are regressed on leads and lags of the instrument. Note that the coefficient labelled '-4+' summarises the effect of periods from 4 to 6 years prior to the event.

individuals are too old to be parents of primary-school-age children. The majority of 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. As shown in Figure A4 in the Appendix, visually reporting the results of this exercise, we find no effect on this age group from the introduction of school thresholds. All coefficients of post-threshold dummy variables appear as insignificant. This finding can be seen as further confirmation that the results in Figures 5 and 6 are truly due to changes in residential choices resulting from variations in local school services.

6.2 Second stage estimates

In this subsection we present the main results of the paper, i.e. the second stage results of the 2SLS model. Table 3 replicates the same 2SLS estimates for a set of dependent variables measured at the municipality level: the school-age population, potential parents, total and per-capita income, and elder population (placebo). Those estimates correspond to the β of Equation 4. The coefficients represent the average yearly percentage variation over the post treatment period in municipalities experiencing school closures, relative to the pre-closure period and to municipalities not experiencing school closures.

Table 3: IV estimation: second stage results

	School-age population	Potential parents	Total income	Per-capita income	Elder population
School closure	-0.198***	-0.232***	-0.127***	0.064***	-0.036
	(0.0663)	(0.0443)	(0.0257)	(0.0169)	(0.0487)
Other school endowments	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓
LLM-time fe	✓	✓	✓	✓	✓
N	18,340	18,340	18,324	18,324	18,340

Note: Standard errors clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the second stage results from the 2SLS estimation of Equation 4, where we regress different dependent variables (school-age population, potential parents, total income, per-capita income and elder population) on school closure, instrumented with an indicator for the school being below the Regional threshold in 2010, from the year of its introduction. We also include controls for other school endowments - public and private -, municipality and LLM-time fixed effects.

The estimates in Table 3, first and second columns show a significant yearly reduction of around 20% in school-age population and potential parents. This is consistent with the idea of 'people voting with feet' for public services. The size of coefficients is rather large. However, we have to bear in mind that the sample is composed of small municipalities, with an average population of around 70 and 200 residents in school-age and potential parents, respectively. Therefore, even small reductions in absolute population appear considerable in relative terms.

As for the effect of school closures on income, the estimates in the third column of Table 3 indicates that total income decreases by almost 13% in municipalities experiencing the closure of their only primary school, after the closure and relative to pre-closure and untreated municipalities. Per-capita income, instead, increases in these municipalities by 6%. These findings can be interpreted as an indication that re-locations mainly concern low income households. This seems at odds with the standard assumption that wealthier households display a higher willingness to pay for services and, as a consequence, they may be more reactive to changes in local public goods[32].

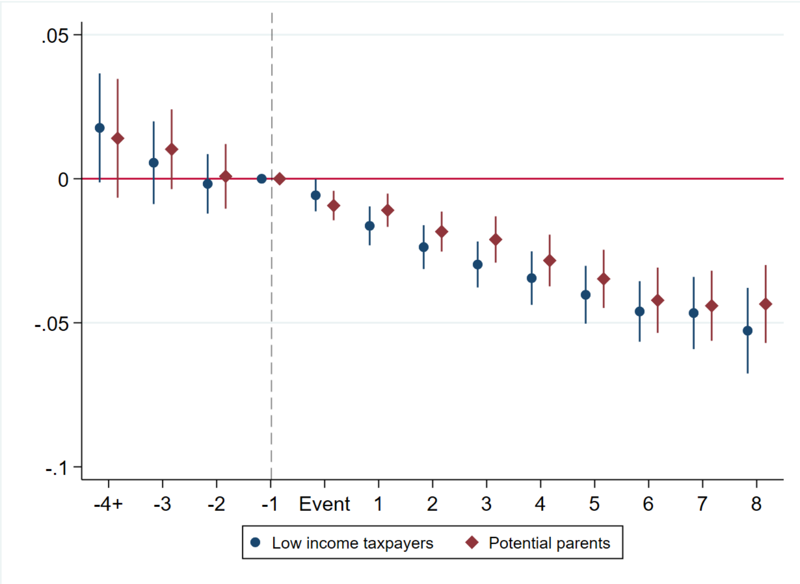
To find more evidence on this, we have replicated event study estimates using the number of taxpayers in lowest income class[33] and the number of potential parents as outcomes. If we look at the event study plot, displayed in Figure 7 we can note that the trajectories for these two groups overlap almost perfectly[34]. We interpret this as evidence of the fact that young adults concentrate in the lowest income class. This finding is consistent with the pattern of income distribution by age class at the national level. Indeed, from the national release of the survey EU-SILC (European Union Statistics on Income and Living Conditions), it can be observe that households whose head is below 45 years holds have lower annual income compared to those with a household's head of 45-64 years old (see Figure A5 in the Appendix). To make age classes comparable with EU-SILC aggregate data, we report in Figure A6 of the Appendix an event study plot analogous to that in Figure 7, but restricting the 'potential parents' to population between 35 and 44 years old. There, the overlap between outcome dynamics is even clearer. This evidence supports the view that school closures mostly affect young adults, who are highly concentrated in low income classes. Therefore, the positive coefficient on per-capita income may be ultimately due to the demographic effect detected on potential parents.

[32] In the local public goods literature, that assumption translates into the single-crossing property of indifference curves of high/low income households in the choice between public goods and house prices. For a discussion, see Brunner et al. (2012) and Fernandez (2003).

[33] The Italian Ministry of Economy and Finance (MEF) classifies taxpayers into seven income classes (euros): 0-10,000; 10,000-15,000; 15,000-26,000; 26,000-55,000; 55,000-75,000; 75,000-120,000; more than 120,000. Those figures refer to annual taxable income. By 'low income class' we mean households with an annual income between 0 and 10,000 euros.

[34] Both graphs refer to reduced-form estimates, obtained from models where outcome variables are regressed on leads and lags of the instrument.

Figure 7: Event study plot of the reduced-form estimation: low income taxpayers and potential parents



Note: The Figure shows the event study plot corresponding to the reduced form of Equation 2, where dependent variable is (log) population of potential parents (i.e. residents between 35 and 49 years old) or (log) of low income taxpayers (below 10,000 euros per year). Those outcome variables are regressed on leads and lags of the instrument defined in Equation 3.

Finally, the coefficient 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, which can be viewed as some sort of a placebo test, supports our claim that the demographic impact found for the relevant age classes is indeed due to service cuts.

7 Who loses the most?

So far our estimates have uncovered a clear effect of primary schools' 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 vary depending on the pre-determined conditions of treated municipalities. In particular, more peripheral places located further away from economic centres and with less access to alternative school services may be most affected by the closures of their only primary school. However, economic centres may not only act as substitutes for local services, but also as attractive poles, draining resources from more peripheral areas.

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 nearest centre of the Local Labour Market, and distance to the next available public primary school[35]. Next, for both these indicators, we sub-divide our full sample of municipalities in sub-groups on the basis of their median value, to determine local areas located close to (above median), or far from (below median) LLM centres or alternative primary schools[36]. Those two criteria do not largely overlap, as municipalities far from LLM centres are not necessarily also far from a further primary school, and *viceversa* (see Table A2 in the Appendix). Moreover, municipalities far from LLM centres are, on average, slightly smaller and more elevated compared to close ones. They were also less populated at the beginning of the period observed (i.e. 2010). Similarly, municipalities far from next available schools are more elevated than those close to further schools. However, they display larger land area[37].

By looking at distance from LLM centres, we aim to capture differences in access to job opportunities and, more generally, in economic centrality of locations. The

[35]Note that distances to next available primary school is measured at the beginning of the period considered, i.e. school year 2009/2010.

[36]The median distance to LLM centres is 7,123 meters, while the median distance to the next primary school is 3,014 meters. As a robustness check, we also subdivide the sample using the 25th or 75th percentile as cutoff. The pattern of results is stable across those alternative choices.

[37]This might simply result from how we compute distances. Indeed, we measure distances from LLM centres in meters from municipal boundaries; whereas, for distances to next available school, we consider meters between schools' exact locations, which plausibly increase in land area for single-school municipalities.

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 high 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 is the next school when the only available primary school closes, the higher would be the incentive of residents to relocate.

Table 4 reports the results sub-dividing the full sample along these dimensions. 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, while total income is the dependent variable in the fifth and sixth columns. The result of Panel a) seems to suggest that the whole result of school closures on residential dynamics and local income is driven by municipalities located far away from the centres of Local Labour Markets. This finding support the view that households value the proximity to economic centres. This presumably offers a valid and relatively accessible school alternative, which induces residents of nearby municipalities not to relocate when the school closes. On the contrary, the same cannot be said for municipalities too far from urban areas, where commuting is not much of an option. The estimates of Panel b), instead, confirm our prior that the incentive to relocate after a school cut is stronger where the next primary school is located further away.

In summary, school cuts appear to especially harm locations which already had limited access to school services and job opportunities. Concerning school access, this is clearly further reduced by school closures. In addition, the relocation of young adults determines a decrease in the number of taxpayers and total income at the municipal level. This may produce long-run depressive effects on the local economy, in terms of reduced demand for local services, entrepreneurial capacity, and thus job creation[38].

The evidence emerging from Table 4 suggests that school closures mostly affect peripheral locations. We have shown that these service cuts foster population decline and consequently reduce local income. All this is in line with the idea that rationalisation policies in key public services affect territorial disparities. These policies seem to widen the existing infra-regional gaps in terms of population and income conditions.

Table 4: School closure effect by municipality location

	School-age population		Potential parents		Total income	
	far	*close*	*far*	*close*	*far*	*close*
a) *LLM centres*						
School closure	-0.291***	-0.027	-0.301***	-0.116	-0.152***	-0.009
	(0.1082)	(0.1175)	(0.0692)	(0.0797)	(0.0403)	(0.0397)
N	8,860	9,070	8,860	9,070	8,850	9,064
b) *Next public school*						
School closure	-0.360***	-0.062	-0.299***	-0.174**	-0.198***	-0.078*
	(0.1283)	(0.1088)	(0.0826)	(0.0715)	(0.0480)	(0.0418)
N	7,670	7,960	7,670	7,960	7,662	7,948
Other school endowments	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
LLM-time fe	✓	✓	✓	✓	✓	✓

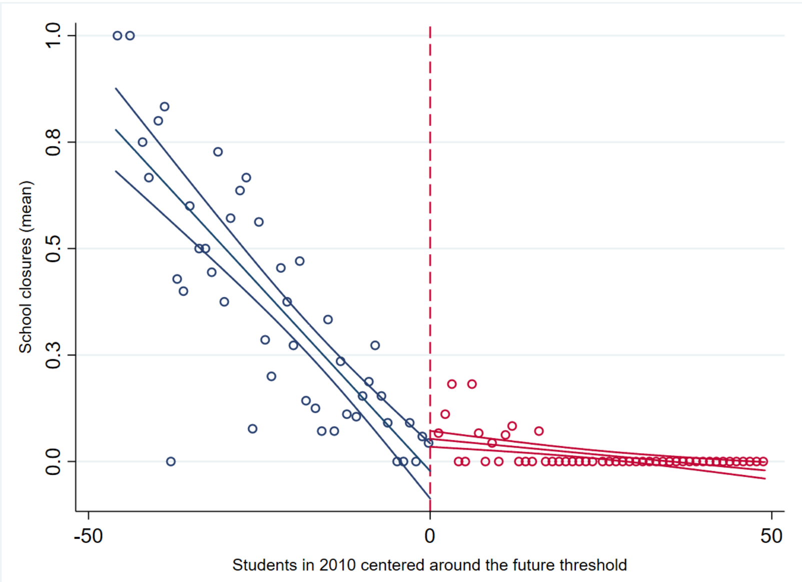
Note: Standard errors clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports second stage results from Equation 4, where dependent variables are school-age population, potential parents and total income. In Panel a), we subdivide our sample by distance to LLM centres and separately estimate Equation 4 for municipalities above (far) or below (close) the median distance to LLM centres. In Panel b), we follow an analogous procedure, considering instead distances to further public primary school.

[38]Indeed, young people are generally thought to have greater entrepreneurial skills. In the Italian context, see as reference the contribution by Anelli et al. (2020).

8 Robustness: alternative instrument

As a robustness strategy, we employ the information about institutional rules in an alternative way. We use the number of students - centred around the Regional school sizing threshold - as a sort of running variable, and test for a discontinuity in the probability of closure at the threshold. We take a window of 50 students, since most Regions fix the threshold for school closure at that level[39]. As in the main empirical strategy, the number of students refer to the first year in our sample, namely school year 2009/2010. Figure 8 plots the probability of experiencing school closures over the time span considered, for different levels of deviation from the Regional threshold.

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



Note: The graph reports the mean of school closure for different levels of deviation from the Regional threshold. The deviation is measured as the number of students enrolled in the school by 2009/2010 minus the Regional threshold.

Figure 8 shows no evidence of a clear jump in treatment probability at the Regional school sizing threshold's cutoff. However, we do observe a discontinuity 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. below the cutoff. At the same time, there are

[39]See in the Institutional context section for more details on Regional thresholds.

also schools below the cutoff which do not close in the period observed, and schools above the threshold in 2010 that instead experienced school cuts. This is mainly due to the fact that we are taking school characteristics at 2010. Most of closures above the Regional school sizing threshold refer to schools that lose students after 2010 and were below the cutoff in the year of the closure. Overall, this evidence is consistent with the existence of some margins of negotiation and discretion at the Regional level in the choice of closing schools. While school directors and local authorities do not have much room to attract students and therefore manipulate their position with respect to the Regional school sizing threshold, they can negotiate with Regional decision-makers to keep open undersized schools. 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.

We exploit that evidence and construct an alternative instrumental variable strategy, by multiplying our previous dichotomous instrument by the number of students in 2010. Formally, our new instrument is:

$$Kink\ instrument_{irt} = (Students - c)_{ir,2010} \cdot S_{ir,2010} \cdot T_{rt} \tag{6}$$

where $(Students - c)_{ir,2010}$ is the 2010 number of students in deviation from the future Regional threshold; $S_{ir,2010}$ is a dummy variable taking value one if school i in Region r was below the Regional threshold, according to school characteristics in the first observed year (i.e. school year 2009/2010); and T_{rt} is a dummy for the introduction of a threshold for school closure in Region r, year t. Note that the interaction between the last two terms constitute the instrument employed in the main empirical strategy. We label it 'kink' instrument because it exploit the kink in treatment probability at the cutoff shown in Figure 8.

This alternative instrument has the advantage of employing the additional information on the number of students. This feature allows to obtain a more reliable range for our estimated coefficients.

Therefore, we repeat the TWFE estimation of Equation 1 instrumenting school closures with the measure defined above (Equation 6). For the exclusion restriction to hold, we need to control for the number of students, as this plausibly correlates with our outcomes and it is included in the kink instrument. Therefore, not accounting for it would cause the instrument to directly predict our dependent variables, violating the exclusion restriction. We augment the specification of Equation 1 with the interaction between the number of students in 2010 and the dummy for the Regional threshold being active. In a context with municipality fixed effects, the time-varying interaction term can be interpreted as a running variable capturing the outcome variation due to threshold introduction for those municipalities below the cutoff, while controlling for the underlying relationship between students number and the outcome at the policy change. Formally, we estimate:

$$y_{ict} = \alpha + \beta\, Clo\hat{s}ure_{ict} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_{ct} + (Students - c)_{ic,2010} \cdot T_{rt} + \epsilon_{ict} \quad (7)$$

where $Clo\hat{s}ure_{ict}$ is obtained from the following first stage regression:

$$\begin{aligned} Closure_{ict} = \alpha + \beta\, (Students - c)_{ir,2010} \cdot S_{ir,2010} \cdot T_{rt} + \gamma_i + \delta_t \\ + \eta\, X_{ict} + \theta_{ct} + (Students - c)_{ic,2010} \cdot T_{rt} + \epsilon_{ict} \quad (8) \end{aligned}$$

For this alternative strategy, we only focus on the population of potential parents and income variables as dependent variables. Indeed, since we use the number of students to construct the instrument, we cannot employ school-age population as dependent variable. Due to path-dependence dynamics, we would have almost the same variable on both sides of the equation. Nonetheless, we can still check the robustness of our other outcome and placebo variables.

Table 5 reports the first stage results of this alternative estimation strategy, corresponding to Equation 8.

Table 5: IV estimation (Robustness): first stage results

	School closure
Kink instrument	-0.007***
	(0.0009)
Running variable	✓
Other school endowments	✓
Municipality fe	✓
LLM-time fe	✓
R-squared	0.798
F test	64.96
N	18,340

Note: Standard errors in parentheses clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports first stage estimates corresponding to Equation 8, where we regress school closure on our kink instrument, together with controls for other school endowments - public and private -, municipality and LLM-time fixed effects. The kink instrument is a given by the interaction of the margin of deviation from the Regional threshold and a dummy variable taking value one if the school was below the Regional threshold in 2010, from the year of its introduction.

The negative sign of the estimated coefficient relates to the negative slope of the left-side plot of Figure 8: once the threshold is active, the lower the number of students below the cutoff, the greater the probability of closure. The instrument is highly significant and strong, with an F-test of 64.96.

Then, in Table 6, we report second stage results for our main outcomes and the placebo. This latter is still insignificant, whereas the coefficients for potential parents and total income reduce in size compared to our main estimation (see Table 3), and per-capita income loses significance. We can interpret those estimates as lower bounds for the related coefficients, since here we are filling the regression model with all the available information about institutional rules. In summary, the robustness strategy confirms that school closures do not affect elder population, consistently with our prior. Moreover, it supports our previous findings of a negative impact of school cuts on municipal income and residential choices of young adults (i.e. potential parents). The loss of significance of per-capita income seems not a major concern, since we already highlighted that the positive effect was mainly due to the concentration of young adults among low income households. In other words, it was ultimately a consequence of the demographic effect on potential parents.

Table 6: IV estimation (Robustness): second stage results

	Potential parents	Total income	Per-capita income	Elder population
School closure	-0.147***	-0.107***	0.004	-0.023
	(0.0441)	(0.0243)	(0.0142)	(0.0499)
Running variable	✓	✓	✓	✓
Other school endowments	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓
LLM-time fe	✓	✓	✓	✓
N	18,340	18,324	18,324	18,340

Note: Standard errors clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the second stage results from the 2SLS estimation of Equation 7, where we regress different dependent variables (potential parents, total income, per-capita income and elder population) on school closure, instrumented with the interaction between the margin of deviation from the threshold and an indicator for the school being below the Regional threshold in 2010, from the year of its introduction. We also include controls for other school endowments - public and private -, municipality and LLM-time fixed effects.

9 Concluding remarks

This paper studies the local impact of public spending cuts on education services, , by investigating the effect of closing undersized schools on demographics and income. Following the hypothesis of citizens 'voting with their feet', we expect changes in local public goods that are *not uniform across space* to represent a fundamental driver of the spatial distribution of workers. As rationalisation policies reducing public services aim at increasing efficiency by exploiting economies of scale, they are designed to act precisely where demand for service is shrinking. As a consequence, the related efficiency cuts mostly concern areas already lagging behind. If households relocate in response to service variations, those policies can have the effect of widening territorial disparities.

By definition, rationalisation policies are directed towards areas already experiencing population decline. We deal with this endogeneity concern through a combination of Two-Way-Fixed-Effects (TWFE) estimation and Instrumental Variable (IV) strategy. We build our instruments by exploiting the institutional rules on school sizing adopted by some Italian Regions over the period considered (2010-2019). These consist of thresholds in the number of students required not to close a school.

The analysis has offered some interesting insights on the demographic and economic impacts of rationalisation policies. First of all, we have verified that school closures actually concerned municipalities displaying negative pre-trends in the population of potential recipients. In other words, we have shown that they were in fact rationalisation policies. Secondly, we have demonstrated that school cuts affect population dynamics on top and beyond preexisting trends. In municipalities with only one primary school, the closure of that school translates into a 20% reduction in the population of children in mandatory school-age and potential parents (i.e. residents between 35 and 49 years old). Conversely, we do not detect any significant effect on the population between 55 and 65 years old, plausibly still in the labour market but too aged to include parents of school-age children. This supports our hypothesis that the estimated demographic effect is indeed due to school closures and not to concurring economic changes. Thirdly, we have found a 13% reduction of municipal income in these municipalities after school closures, while per-capita income seems to increase by 6%. The latter finding is driven by the concentration of young adults among low-income taxpayers, and hence it is ultimately due to the demographic effect we observe for potential parents.

Furthermore, we have shown that the effect of school closures on residential choices and income is driven by peripheral municipalities, i.e. those above the median distance from the centre of local labour markets, or those located further away from other primary schools. Hence, school cuts appear to impact especially on locations which already had limited access to school services and job opportunities. Closures further reduce school access. Moreover, if this loss of young adults and income has a depressive effect on the local economy, the observed dynamic may further limit access to job opportunities, increasing the peripherality of already marginal territories.

These results have relevant policy implications. 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. The specific population sub-group most influenced by such intervention is that of young adults with children. These citizens are induced to re-locate towards core territories, leaving peripheral areas already experiencing population decline without valuable labour resources. Policy-makers implementing efficiency cuts of this kind should be aware of how peripheral territories with already limited access to education services and economic opportunities may react to them.

In conclusion, rationalisation policies intervening on key education services have crucial effects on the sub-national distribution of workers. In this paper we have looked at the demographic impact of such policies and their localised income effect, demonstrating that municipal areas located further away from the core of labour markets are those experiencing the highest loss of demographic and economic resources. This indicates that rationalisation policies are worsening infra-regional income disparities. Yet, we have not estimated the economic consequences of these interventions on an aggregate scale. We reserve to examine the overall economic performance of the broader area experiencing public education service cuts (and consequent internal labour flows) in future works.

References

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

Anelli, M., Basso, G., Ippedico, G., and Peri, G. (2020). Does Emigration Drain Entrepreneurs? *CESifo Working Paper No. 8388.*

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

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

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

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

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

Borghi, E. (2017). *Piccole Italie: le aree interne e la questione territoriale.* Donzelli Editore.

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

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

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

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

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

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

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

Fernandez, R. (2003). Sorting, education, and inequality. In *Dewatripont, M., Hansen, L.P., Turnovsky, S.J. (Ed.), Advances in Economics and Econometrics: Theory and Applications, 8th World Congress*, volume II. Cambridge University Press, Cambridge.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

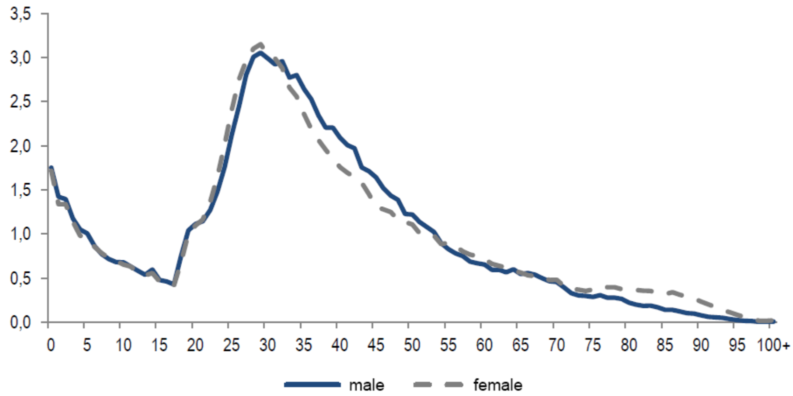
Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

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

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

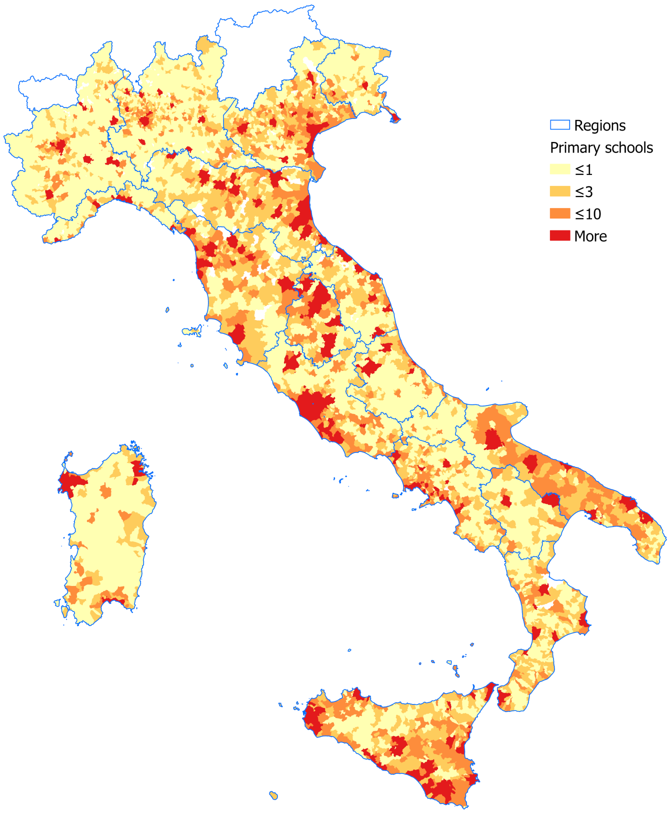
Appendix

Figure A1: Percentage of internal migrants by age (2017)



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

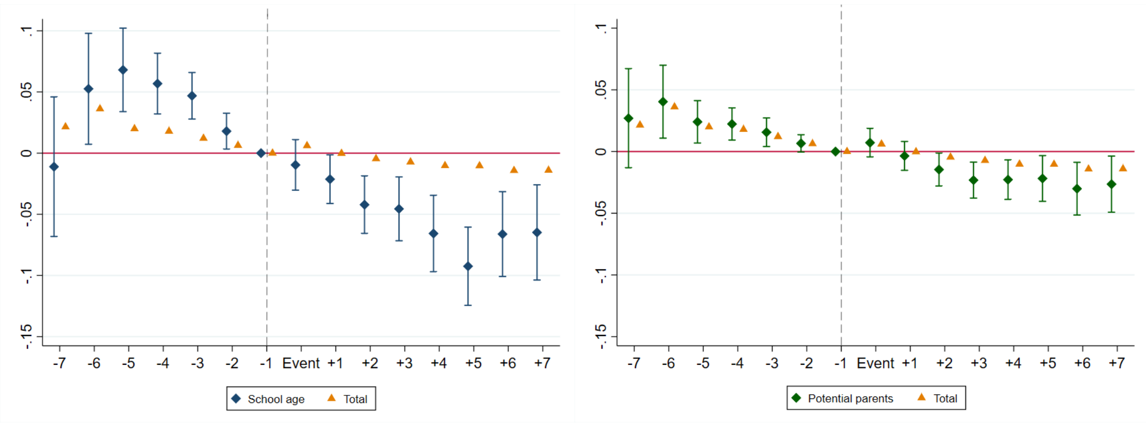
Figure A2: Primary school endowments by municipality in school year 2009/2010



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

Figure A3: Population by age classes around school closure

a. b.



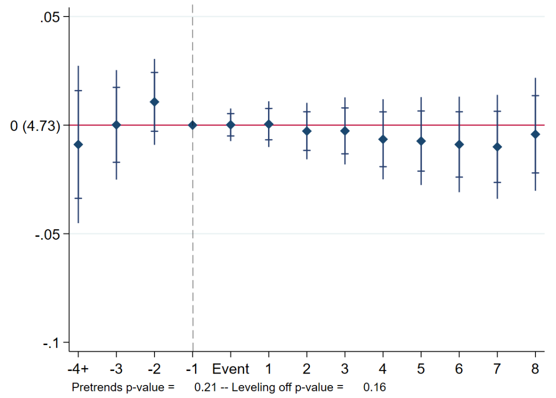
Note: The Figure shows event study plots employing the estimator proposed by Sun and Abraham (2021), which corrects for possible heterogeneous treatment effects across cohorts. Plotted coefficients relate to Equation 2, where dependent variable are total and school-age population (Panel a) or total population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

Table A1: Municipalities by observed time period relative to threshold introduction

Relative time	Treated obs.	Percent
-6	103	1.60
-5	103	1.60
-4	103	1.60
-3	157	2.44
-2	256	3.98
-1	644	10.00
Event	644	10.00
1	644	10.00
2	644	10.00
3	644	10.00
4	541	8.40
5	541	8.40
6	541	8.40
7	487	7.56
8	388	6.02
Total	6,440	100.00

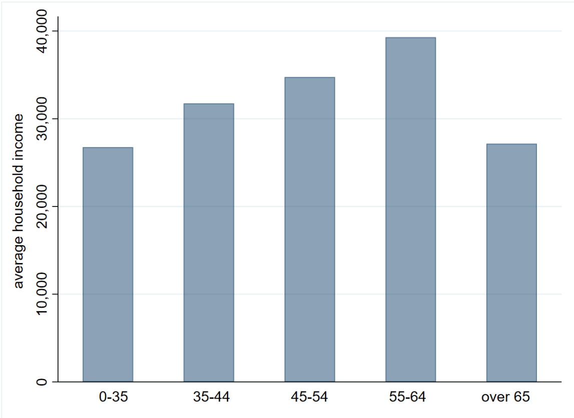
Note: The Table reports the number and percentage of municipalities by observed period relative to the event time. Recall that 'event' corresponds to the year of threshold introduction in the Region to which the municipality belongs. Treated observations correspond to municipalities whose schools were below the threshold according to 2010 characteristics.

Figure A4: Event study plot of the reduced-form estimation: placebo



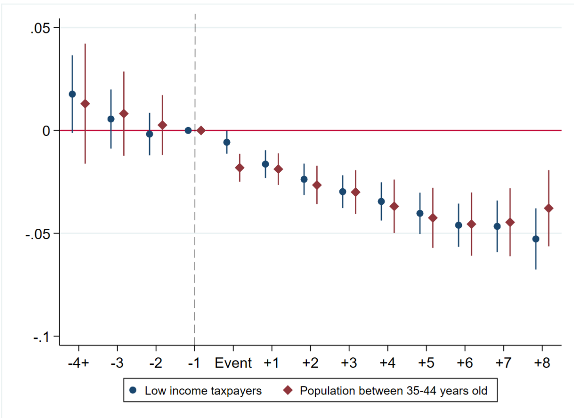
Note: The Figure shows the event study plot corresponding to the reduced form of Equation 2, where dependent variable is (log) population between 55 and 64 years old. The outcome variables is regressed on leads and lags of the instrument. We interpret the plot as a sort of placebo, since we do not expect residents in that age class to be affected by school closures, while they are plausibly still in the labour market.

Figure A5: Household income distribution by age class of family's head



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

Figure A6: Event study plot of the reduced-form estimation: low income taxpayers and young adults



Note: The Figure shows the event study plot corresponding to the reduced form of Equation 2, where dependent variable is (log) population between 35 and 44 years old or (log) of low income taxpayers (below 10,000 euros per year). Those outcome variables are regressed on leads and lags of the instrument defined in Equation 3.

Table A2: Municipalities by distance to LLM centres and next available school

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

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