Università
Ca'Foscari
Venezia

Corso di Dottorato di ricerca

In Economics

ciclo 34°

Tesi di Ricerca

Essays in Public and Urban Economics

SSD: SECS-P/01, SECS-P/03

Coordinatore del Dottorato
ch. prof. Pietro Dino Enrico Dindo

Supervisori
ch. prof. Agar Brugiavini
ch. prof. Marco Di Cataldo

Dottoranda
Giulia Romani
Matricola 956425

Essays in Public and Urban Economics

Giulia Romani

Acknowledgements

This dissertation would not have been possible without the help and generous support of many people, inside and outside the academia. Few lines of acknowledgements cannot give credit to all the exchanges and insights that motivated me along this path. In one sentence, I am grateful to all colleagues and scholars who shared with me their passion for research, but also their attitude and thoughts on how to approach the academic profession, to make it compatible with life! Similarly, I thank all friends and people met in these years outside the University, which inspired my investigation activity and continuously reminded me that the greatest research questions do not lie in a small gap in the literature.

I also wish to personally thank few people who have been essential for this dissertation. First of all, my sincerest gratitude goes to my supervisors Doctor Marco Di Cataldo, Professor Agar Brugiavini, and Professor Olmo Silva. What I have learnt so far and the scholar I have become are - to a large extent - due to you. Then, I feel glad and honoured to have received comments from my examiners Professor Giacomo Pasini, Professor Henry Overman, and Doctor Guido De Blasio. Your insightful comments and suggestions greatly helped improve this dissertation. Last but not least, I thank my dearest cohort mates, who have been a constant support team along this journey.

Introduction

Socio-economic inequalities among local territories have long been recognised as a crucial issue by academics, policy-makers, and members of civic society (OECD, 2019). How to deal with them is nowadays a hotly-discussed topic in the scholarly debate, and many policy efforts have been made to attempt mitigating territorial disparities in socio-economic conditions. As an example, the European Union devotes one third of its overall budget to Cohesion Policy, which consists of place-based interventions aimed at reducing sub-national inequalities among EU Member States.

The issue of spatial disparities is not confined to Europe but rather has a global scale, and it is often associated with structural changes in the economy. Specifically, the emergence of the so-called 'knowledge economy', which shows a remarkable tendency to cluster in space, has been regarded as a determinant of growing inequalities within countries (Rosés and Wolf, 2018, Moretti, 2012).

Another key aspect associated with spatial disparities among local territories is access to essential services. This has become especially relevant in light of the recent financial crisis and the austerity measures implemented by several countries as a response to that. Spatial disparities have been broadened by the financial crisis and more public investment would be needed if the goal is to contain this trend towards divergence. Moreover, this reshaping geography and its consequent demographic trajectories will pose serious challenges to efficient service delivery, as fewer places will have the necessary critical mass to sustain the provision of fundamental local services (OECD, 2019).

The answer of policy-makers to these issues has often been place-based interventions, which however have not always produced the desired outcomes (e.g. Barone et al., 2016, De Angelis et al., 2020). Place-based policies may be subject to institutional distortions that prevent public funds to be allocated efficiently (Persson and Tabellini, 2002, Golden and Min, 2013). If we wish to tackle spatial disparities in an efficient way, we need to understand which institutional design is more appropriate in each given context.

This dissertation aims to shed light on some aspects of these fundamental issues. First of all, it analyses the role of access to key public services - in particular, primary schools - for demographic and income dynamics at the local level. Next, it investigates how the emergence of the knowledge economy and its induced internal migration affects local labour market conditions. Finally, it studies how institutional shifts in the design of regional development programmes can give rise to political distortions in the management of public funds. All these topics are empirically analysed exploiting the Italian context as a testing ground, focusing on different periods of time and employing administrative data from various sources.

Some key stylised facts make Italy a very interesting case for the issues at hand. First, austerity measures implemented in the last decade have led to a deep contraction of key public services, a process that has touched the education system as well, leading to the closure of several public schools across the country. Second, 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. In addition, migrations seem to primarily involve qualified workers which move towards the few 'knowledge hubs' of the country (ISTAT, 2019a). In this regard, Italy seems to be catching up with European standards along various knowledge-related dimensions, such as investment in R&D or employment in research and creative activities (ISTAT, 2018b), yet this improvement appears to be spatially concentrated in some regions, provinces, and local areas, leaving a large part of the country lagging behind. In summary, Italy seems characterised by a significant polarisation of population, services and opportunities. This has led some policy-makers to attempt to deal with this *'Italian territorial issue'* (Borghi, 2017) through e.g. the National Strategy for Inner Areas (MUVAL, 2014), aiming to address spatial inequalities *within-regions*. Italy is also the country of a long-standing *'Southern issue'*, referring to the North-South divide which dates back to the Italian unification in 1861. In order to tackle this regional gap, after the Second World War the newborn Italian Republic's ruling class implemented a massive programme for the development of Southern Italy, the *Cassa per il Mezzogiorno*, which lasted 40 years and was second in size only to the US Tennessee Valley Authority. The institutional design of the *Cassa per il Mezzogiorno* was radically modified in the 1970s, moving from a centralised to a regionalised management of local public funding. This historical experience represents a perfect setting where to study how institutional design affects the efficacy of regional development programmes.

More in detail, the first chapter of the thesis deals with the local impact of rationalisation policies (i.e. public spending cuts) in the field of school services. It combines a staggered difference-in-differences (DID) model with an instrumental variable strategy (IV) that exploits institutional thresholds for school closures, demonstrating how drastic cuts in public school services influence demographic and income dynamics at the local level. Here the fundamental empirical challenge is that schools close precisely where population is shrinking, creating a problem of reverse causality. This latter, together with likely anticipation effects, generates pre-trends in outcome evolution, that we address through the IV strategy. The second chapter adopts administrative employer-employee data from the Italian Social Security Institute (INPS). It shows that the uneven growth of knowledge-intensive sectors produces multiplicative effects on the employment of local workers, decreases their probability of migration, and increases house prices more than nominal wages, resulting in a reduction of real wages. We employ a two-steps estimation á la Combes et al. (2008) in combination with a shift-share IV, which serve - respectively - to account for workers sorting into areas based on unobservables, and to control for possible local idiosyncratic shocks. Finally, the third chapter uses unique historical data that we personally collected from the Italian Ministry of Interior to look at the extent to which institutional design has influenced the efficacy of the *Cassa del Mezzogiorno*. Specifically, it demonstrates that the devolution of authority over public investments in institutionally-fragile settings can generate distributive politics dynamics, creating a political bias in the allocation of public funds. We implement a DID model and exploit the exogeneous variation provided by a governance reform which transferred the power over funds allocation from a central committee of technicians to the newborn regional governments.

Far from being an exhaustive exploration of the issue of spatial inequalities, in Italy and in general, the results reported in this thesis still entail some relevant implications. For example, pursuing economic efficiency of public spending through the closure of undersized schools appears to have the effect of fostering demographic and economic decline of peripheral areas. Furthermore, the thesis shows that the spatially uneven growth of the knowledge economy produces multiplicative employment effects at the local level, therefore contributing to broaden spatial disparities among local labour markets. Finally, in institutionally-fragile settings, the devolution of authority over public funds for regional development can trigger dynamics of tactical redistribution, reducing the overall efficacy of place-based interventions. The dissertation takes a positive (i.e. non-normative) perspective on these topics, yet it highlights some important side effects and unexpected consequences of public policy and labour market changes, which should be well-acknowledged by policy-makers dealing with the related phenomena. More generally, the findings reported here contribute to a deeper understanding of the interconnections between spatial disparities and access to local public services, labour market changes, and the governance of regional development programmes.

Contents

List of Figures

List of Tables

Chapter 1

Rational cuts? The local impact of closing undersized schools

Authors: Marco Di Cataldo, Giulia Romani

Abstract

The availability of public education services can influence residential choices. Hence, policies aiming to 'rationalise' service provision by cutting on undersized nodes of the public school network can induce population decline. This paper exploits an Italian education reform inducing a significant contraction of the school network to investigate the demographic and income effects of primary school closures. We assess whether school closures have an impact on households' residential choices, on top and beyond preexisting negative population trends which motivate school closures. We address endogeneity by combining a Two-Way-Fixed-Effects model with an instrumental variable approach, constructing the IVs 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 peripheral municipalities located far away from economic centres, and distant from the next available primary school. This evidence indicates that school 'rationalisation policies', by fostering depopulation of peripheral areas, have an influence on the spatial distribution of households and income, thus affecting territorial disparities.[1]

[1] This chapter has been published in the Working Papers series of the Economics Department, Ca' Foscari University of Venice.

1.1 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).

In turn, the organisation and territorial distribution of education services is directly dependent on government policies. A key aspect considered by policy-makers in the design of policies influencing public services is the reduction of fixed costs (Alesina et al., 2004; Urquiola, 2005). This is the case for so-called 'rationalisation policies', i.e. public interventions aimed at removing undersized service centres in order to reduce public expenditures. 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' (Tiebout, 1956) in favour of school access, in a context where rationalisation policies have cut undersized nodes of the school network.[2]

Our focus is on Italy, exploiting an education reform that took place in 2008 in the country. The Italian context represents an interesting and unique analytical setting for our purpose. On the one hand, despite the traditionally low mobility of the Italian population, there is evidence of significant internal migrations, mainly directed towards big urban centres. This especially concerns young adults with children, representing the highest fraction of

[2] The idea of people moving across jurisdictions to access public services stems from Tiebout's (1956) proposition of people 'voting with their feet', i.e. the idea that people relocate in space 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).

all internal migrants.[3] Italy displays territorial disparities in terms of population, services, and economic opportunities, not only across but also within regions.[4] On the other hand, austerity measures implemented in the last decade have led to a deep rationalisation of key services, a process that has touched the public education system as well. In this respect, the so-called 'Gelmini reform' of 2008 represents the most decisive and effective push towards the contraction of the Italian school network. The objective of the reform was to cut 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 the closure of several schools across the country.[5]

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

To the best of our knowledge, no study has ever performed a systematic assessment of the population dynamics induced by schooling rationalisation policies. In this article we aim to fill this gap and investigate whether service cuts to undersized school services foster population decline. In addition, we look at the consequences such population decline may have on the income composition of local communities, and study the spatial heterogeneity

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

[4] 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 ongoing National policy efforts to address Italian territorial disparities. However, to the best of our knowledge, the causal relation between depopulation and service access has not been properly investigated yet.

[5] Table 1.1 shows the absolute and relative numbers of primary school closures in municipalities endowed with a single primary school in 2010. Closures amount to 271, corresponding to 6.34% of the total.

of the estimated impact. A related contribution to ours - yet focusing on a different type of negative shock to public services - is the work of Gibbons et al. (2018), assessing the relocation effect of transport infrastructure cuts of non-profitable rail lines and finding that they induce the de-population of local areas experiencing the largest cuts.

We employ geo-located data on the universe of Italian public and private schools to assess the population and income dynamics of municipalities experiencing the closure of their only primary school during the 2010-2019 period, as a result of the 'Gelmini' rationalisation reform. Our analysis faces a fundamental empirical challenge, in that 'treated' municipalities experiencing primary school closures are often characterised by negative population pre-trends. We address this empirical issue through a Two-Way-Fixed-Effects (TWFE) model in combination with an Instrumental Variable approach (IV). Municipalities fixed effects are needed to account for any time-invariant difference among treated and control units. Moreover, we also include in our specification year-local labour markets (LLM) fixed effects which control for any time-varying factor at LLM level. These fixed effects models, however, do not satisfy the parallel-trends condition required to causally interpret the corresponding OLS estimates. This is plausibly due to residual issues of reverse causality and anticipation, which we address through the Instrumental Variable models. We construct instruments exploiting institutional rules governing primary school sizing, enforced by some Italian regions during the period of analysis, interacting them with pre-threshold school characteristics. We also consider the margin of deviation from the school sizing threshold for an alternative IV model.

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

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

1.2 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, households would relocate in order to match their preferences. This hypothesis has recently undergone several empirical tests, with contributions focusing on different kinds of local services or amenities, such as local environmental quality (Banzhaf and Walsh, 2008; Gamper-Rabindran and Timmins, 2011) or rail transit lines (Kahn, 2007). These studies tend to confirm that households are willing to move to places offering them desirable amenities and public services.

Other tests of Tiebout's argument focus on school services. Schools are especially relevant for residential choices, since households with children have a daily need for schooling. Households evaluate school alternatives, whose quality depends on per-student expenditure and peer-average performance. Hoxby (2000) shows that higher choice among jurisdictions improves public school productivity, Baum-Snow and Lutz (2011) investigate the residential and school choice response to the desegregation of public school districts, Brunner et al.

(2012) demonstrate that inter-district schooling choice programmes have an effect on residential location decisions. Other works perform indirect tests of Tiebout's hypothesis by looking at house prices, finding that public school performance is capitalised into house prices and parents are willing to bear higher housing costs to access better quality schools (Black, 1999; Fack and Grenet, 2010; Gibbons and Machin, 2006; Gibbons et al., 2013). Private schools mitigate the effect, offering an outside option (Fack and Grenet, 2010). These works largely confirm the predictions of models suggesting that increased school choice reduces district disparities in terms of income and housing values (Nechyba, 2000, 2003; Ferreyra, 2007; Epple and Romano, 2003).

In this literature, the focus has mainly been on school quality differentials and related dynamics of households sorting by socio-economic status. Little attention, instead, has been devoted to the possible role of fixed costs in schooling provision and the public policies implemented in order to reduce them. Exceptions in this respect are Urquiola (2005) and Alesina et al. (2004), arguing that school fixed costs make average cost decrease in district size. These works, however, are mainly concerned with the formation of jurisdictions (school districts) in response to the trade-off between scale economies and the costs of community heterogeneity, overlooking the consequences of public interventions intended to minimise fixed costs - infrastructure maintenance and teachers expenses - which may induce the closure of undersized schools.[6]

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

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

The interaction between scale economies and transport costs is at the centre of the New Economic Geography (NEG) tradition (Krugman, 1991). This literature strand focuses on firm location choices and the key idea is that industries with increasing returns concentrate where they can gain larger market access, while serving peripheral areas thanks to decreasing transport costs.[7] Under factor mobility and preferences for variety, households will relocate close to industrial centres, giving rise to a process of 'cumulative causation' that leads to a core-periphery pattern, whereby residence and industry are increasingly concentrated. In this literature, the public sector mainly enters through the provision of infrastructure to firms (Ottaviano, 2008). Residential choices are either confined to responses to wage differentials or neglected, assuming immobile workers.[8] However, core-periphery patterns may also be reinforced as a result of changes in the provision of public services. Government cutting undersized schools in places characterised by fewer schooling alternatives may induce households to relocate closer to other schools, fostering the concentration of people and services in more central areas to the detriment of more peripheral locations.

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

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

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

1.3 Institutional context

1.3.1 The Italian schooling system

Despite recent trends towards decentralisation, the Italian schooling system still displays a considerably centralised and unitary configuration.[9]. The national government has authority over the general norms in the field of education, including the definition of school programmes, quality standards and their evaluation (Di Giacomo and Pennisi, 2012). Moreover, it regulates and directly manages the recruitment and payment of the schooling personnel, which constitutes the largest component of the expenditure for education.[10]

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.[11] These are mainly managed by the central government, with the exception of some residual municipal preschools and schools of any order in the autonomous regions of Trentino-Alto Adige and Valle d'Aosta.

The Italian system allows for school choice. Parents can enrol children in their preferred school, even in municipalities different from the one they reside in.[12] In making primary school choices for their children, parents have to combine work and family needs. Primary school is mandatory, it lasts five years, and children attending it largely depend on their

[9] For a historical perspective on Italian school design and achievements, see Checchi et al. (2007) In more recent years, in line with the trend towards 'regionalisation' of the whole public system, some jurisdictional powers have been transferred from the central government to local authorities. Since the 1990s, the establishment of school autonomy and the 2001 reform of the Italian Constitution have contributed to such a process.

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

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

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

parents for daily commuting. As a consequence, house-school commuting times become particularly relevant in orienting residential choices. Conversely, school *quality* appears less of a determinant for choosing among neighbouring 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, so that in principle school quality of primary schools is approximately equalised, at least within provinces.[13] Indeed, the strongest evidence of sorting across schools on the basis of school quality is visible at the level of higher secondary school (*scuola secondaria di secondo grado*), whereas it does not seem particularly relevant for the first educational cycle (Bertola and Checchi, 2004; Brunello and Checchi, 2007).[14] In conclusion, at least for the first educational cycle, residence and school choice are not completely independent. It seems plausible that households take into account distance to school when evaluating residential decisions.

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

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

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

plan (*Piano di dimensionamento scolastico regionale*) is agreed by the regional government on the basis of inputs received from each province composing the region.

In defining these plans, regional authorities are constrained by the number of public school workers assigned to each region by the central government. The binding constraint to class and school activation is represented by the scarcity of teachers and janitors, which are the more valuable and costly resource of the schooling system.[15] In this framework, each single school has little control over its own activation and/or suppression. School workforce is assigned on the basis of student enrolments (*organico di diritto*) and then adjusted to cover particular and transitory needs, determining the effective personnel for the school year (*organico di fatto*). Therefore, despite the formal decentralisation of power on these matters to regional authorities, the central government's reforms crucially affect the organisation of the school network.

1.3.2 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. In addition, since the 1990s, policies of rationalisation started to be implemented in the field of public services, including public education. In this regard, the last noticeable turn occurred after the 2008 crisis with the 'Gelmini reform' (from the name of the then Minister of Education), which led to a relevant contraction of the school network, both in terms of number of school complexes (i.e. single or multi-school structures) and classes activated (MIUR, 2010). Indeed, by 2008 rationalisation policies had mainly intervened to reduce autonomous school institutions,

[15] 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.

but they had not strongly affected the distribution of school complexes.[16] The territorial fragmentation of school complexes and the limited class size were identified as the main reasons for the high per-pupil expenditure compared to OECD countries (Fontana, 2008; MIUR, 2007).

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

The Ministry declared the need to eliminate undersized school complexes. For that purpose, regions were allowed to establish numerical criteria for the activation or suppression of school complexes.[17] Some regions formulated general norms for the organisation of the school network, including directives towards a more rational distribution of school complexes, to be achieved through the suppression of the undersized ones. Other regions introduced proper numerical criteria to determine whether a school should be suppressed. This kind of school sizing threshold has been introduced by seven Italian regions over the period considered: Veneto, Piedmont, Lazio, Calabria, Friuli Venezia-Giulia, Tuscany, and Sardinia. The timeline of regional interventions varies, and it is displayed in Figure 1.1. 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

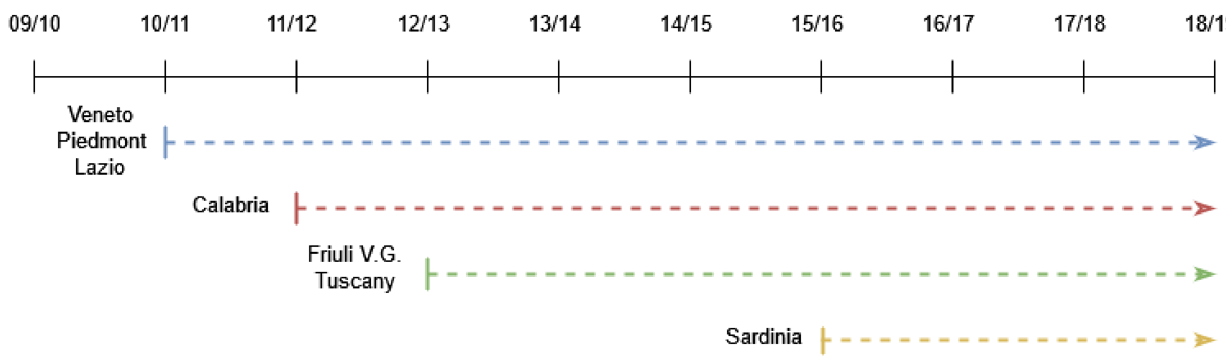
[16] Autonomous school institutions are legal entities which comprehend multiple school complexes. They are managed by a single school director, who has - in principle - some autonomy in the organisation of the member schools. School autonomy was introduced in the Italian system by law 21/1997.

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

[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.

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.[19]

Figure 1.1: Timeline for the introduction of regional thresholds



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

1.4 Data and sample

We look at the impact of the closure of *primary* schools and use municipalities as units of analysis.[20] We exclude municipalities that have undergone processes of administrative reorganisation - i.e. merging over the period considered - so we can easily trace the municipal unit over the entire period considered. For each municipality in the sample, primary school closure represents the treatment. The choice of focusing on primary schools is motivated by the fact that primary school attendance is mandatory, primary schools are extremely diffused and dispersed across Italy, and primary school children still depend on their parents for daily commuting.

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

[20] In Italy, municipalities are the smallest local authority, not taking into account sub-municipal districts, which have some relevance only in biggest cities. In the period considered, Italian municipalities are around 8,000 (some mergers and suppression took place in those years). Looking at municipalities with a single primary school in 2010 - i.e. the sample we will focus on -, they amount to 4,004 and their average size is 29 square kms. Further summary statistics are reported in Table A1.1.

To identify school closures, we exploit the information about the location of each school and the universal coverage of our data. Annual data on active schools from school year 2009/2010 to school year 2018/2019 have been provided to us by the Italian Ministry of Education (MIUR - *Ufficio Gestione Patrimonio Informativo e Statistica*), 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).[21] MIUR represents the most reliable source of information about the Italian schooling system. We exclude from our analysis the regions of Trentino-Alto Adige and Valle d'Aosta because school policy in those two regions is regulated by the jurisdiction of their autonomous provinces.

Our goal is to examine the effect of school closures on population dynamics. As for the outcome variable, we have collected data on residential population at the municipal level from the Italian Institute of Statistics (ISTAT).[22] These are administrative data reporting yearly statistics on residents in each municipality on the 1st of January of each year, subdivided by age class.

We focus on two age groups in particular. The first is the residential population in mandatory school age (5 to 14 years old), which we assume is directly affected by primary school closures.[23] The second is the group including the pupils' potential parents, which we identify as individuals between 35 and 49 years old, who possibly became parents between 25 and 44 years old.

We also explore income-related outcomes, namely total and per-capita municipal income. For that, we have extracted information on taxable income at municipal level from the Italian

[21] We focus on *public* schools, which represent the target of the reform process, and employ the information about private schools to control for substitute services. Moreover, in Italy the vast majority of pupils attend public schools. 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/).

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

[23] 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.

Ministry of Economy and Finance for the period 2010-2019.[24] This information comes from households' tax records and it is then aggregated at the municipal level. We compute per-capita income by dividing overall municipal income by the number of taxpayers.

From ISTAT we also collect data on the Local Labour Market (LLM, *Sistema Locale del Lavoro*) each municipality belongs to, in order to control for labour market conditions.[25].

We complete the dataset with information on municipal public expenditures for primary schools, available from the Italian Ministry of Interior's *Certificati Consuntivi*, yearly, until the year 2015.[26] Italian municipalities' balance sheets are sub-divided into two different categories, current and capital expenditures. The dataset is further disaggregated into different functions and sub-functions. The one we are interested is 'Primary School', a sub-function of total spending for 'Education'.

Crucially, to define the sample of municipalities for the analysis, we focus exclusively on municipalities that have *only one* primary school within their borders at the beginning of the sample period, i.e. school year 2009/2010.[27] The aim is to capture the effect of school closures where it is expected to be stronger; namely, in localities where there are no other public options locally available. In sub-section 1.6.2, as verification test, we check whether an effect can be detected in municipalities with two schools experiencing the closure of one of them.

School sizing plans for a given school year are approved by December of the previous year, meaning that if, for instance, the school complex is not activated for school year 2010-2011, the decision about the closure is taken and announced in December 2009. The school closes in June 2010 and students have to find a new school for the school year starting in

[24] Data are publicly available at finanze.gov.it/finanze/analisistat

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

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

[27] We discard municipalities that increase the number of primary schools over the period considered. In this way, we compare municipalities that keep their single primary school for the entire span and municipalities where the school closes and does not re-open over the period observed.

September 2010.[28] Given that we observe the number of residents at the beginning of each year, to associate population trends and closures correctly, in our municipality-year dataset we consider the municipality with the school closing in June 2010 as having a primary school until 2010 (included) and lacking any school from the start of 2011.

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

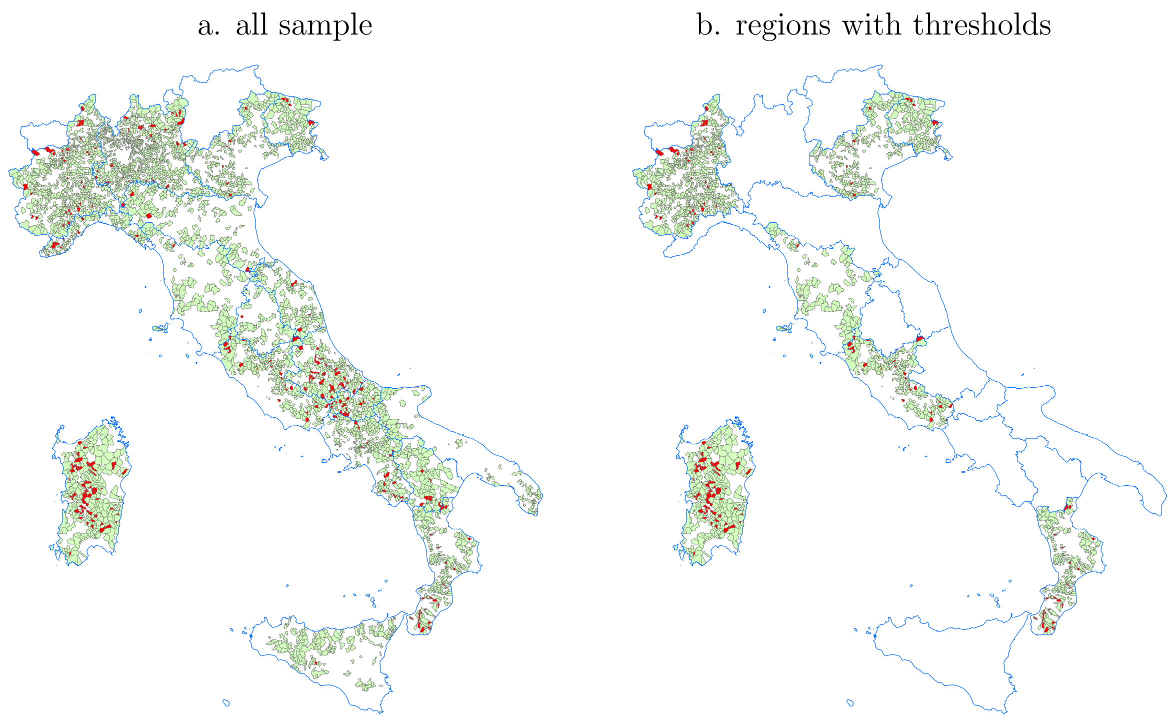
To provide visual representations of the Italian school network, Figure A1.2 in the Appendix plots the geographical distribution of primary schools by municipality in the first school year considered, i.e. 2009/2010. Most of Italian municipalities are endowed with at most one primary school (light yellow areas). They make 57% of all coloured municipalities in the Figure.

The set of single-school municipalities is shown in Panel a) of Figure 1.2. 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 1.2 restricts the sample to single-school municipalities from regions adopting numerical thresholds for school sizing over the period considered. As can be seen from the map, they are fairly evenly distributed across the whole Italian territory, as regions from north, centre, and south of the country are represented. In 2010, 20% of Italian population was living in single-primary-school municipalities (Panel a); 7% when focusing only on regions

[28] In Italy, school years begin in September and end in June of the following year.

adopting school thresholds (Panel b). As visible in Tables A1.1 and A1.2, reporting key summary statistics for the variables in our sample, the characteristics of single-primary-school municipalities in regions adopting thresholds seem largely comparable to those in the full sample.[29]

Figure 1.2: Single-primary-school municipalities - closures



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

By restricting the analysis to municipalities with a single primary school in 2010, we are left with a total of 4,004 municipalities, of which 271 experienced primary school closures during the period of analysis. They are distributed across regions as shown in Table 1.1, reporting in bold the regions introducing specific numerical criteria for school closures.

[29] For a description of all the employed variables and their relative sources see Table A1.3 in the Appendix.

Table 1.1: Single-primary-school municipalities - closures by region (2010-2019)

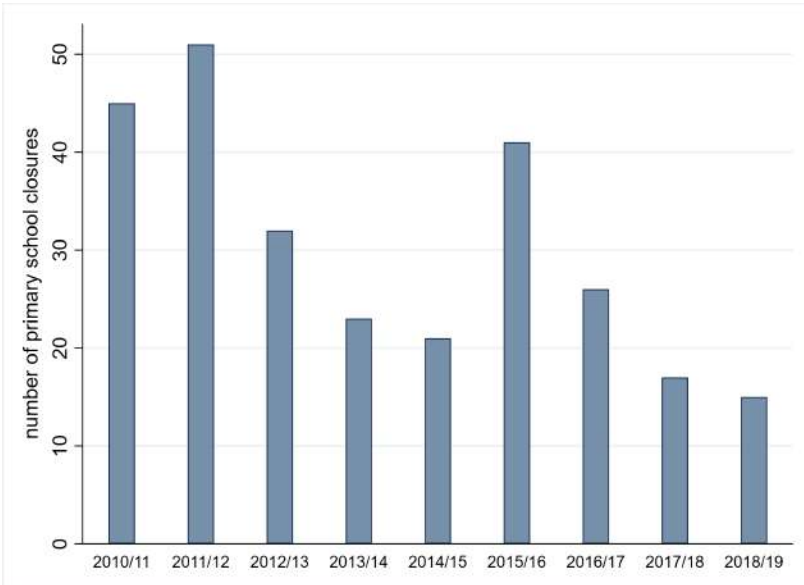
Region	No closure	Closure	Total	%
Abruzzi	159	29	188	15.43
Apulia	108	1	109	0.92
Basilicata	88	8	96	8.33
Calabria	190	22	212	10.38
Campania	276	19	295	6.44
Emilia Romagna	140	3	143	2.10
Friuli V.G.	128	5	133	3.76
Lazio	205	22	227	9.69
Liguria	126	9	135	6.67
Lombardy	922	35	957	3.66
Marche	136	4	140	2.86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

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

The timing of school closures is also relevant. Figure 1.3 shows the number of closures by year in the sample of municipalities with a single primary school in 2009/2010. We can notice a concentration of cases of closure in the first three years. The period 2010-2012 coincides with the time horizon indicated by the 'Gelmini reform' for collecting 8 billion euros in public savings through the policy of rationalisation.[30]

[30] School year 2015/2016 also stands out for the high number of closures. Our intuition is that the peak is due to the introduction of school sizing thresholds in Sardinia, which resulted in 17 closures right in that year.

Figure 1.3: Single-primary-school municipalities, closures by school year



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

1.5 Empirical strategy

Our empirical strategy combines Two-Way-Fixed-Effects and Instrumental Variables models. Municipality fixed effects are needed to account for any time-invariant difference among treated and control municipalities. We also include in the specification year-local labour markets (LLM) fixed effects, which control for any time-varying factor at LLM level. These fixed effects models, however, do not satisfy the parallel-trends condition required to causally interpret the corresponding OLS estimates. This is plausibly due to residual issues of reverse causality and anticipation, which we address through Instrumental Variable models. In the following sub-sections, we explain in details each step of the empirical strategy.

1.5.1 Two-Way-Fixed-Effects model

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

Formally, we estimate:

$$y_{ict} = \alpha + \beta\, Closure_{ict} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{1.1}$$

where i is the municipality identifier, t is the year index, and c indicates the LLM to which the municipality belongs. Equation 1.1 refers to our starting model, a Two-Way-Fixed-Effects model where we regress our outcomes of interest (population in key age classes and municipal income) on a treatment dummy for school closure ($Closure_{ict}$), including time (δ_t) and municipal (γ_t) fixed effects, year-LLM interacted fixed effects (θ_{ct}), and a set of controls (X_{ict}).[31] The treatment variable $Closure_{ict}$ takes value 1 from the school year in which the only primary school in the municipality has closed until the end of the period, and 0 before that.[32] The model controls for complementary and substitute school services X_{ict}: the endowments of public pre-schools, public lower secondary schools, and

[31] Working with a municipality-year panel, we employ the terminology 'Two-Way-Fixed-Effects' with reference to municipality and year fixed effects. On top of these, we also add LLM dummies - interacted with year dummies - in order to control for time varying characteristics within the LLM. Results for the more traditional TWFE model with only municipality and year fixed effects included are reported in Table A1.9 of the Appendix. Formally, we estimate:

$$y_{ict} = \alpha + \beta\, Closure_{ict} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_t + \epsilon_{ict} \tag{1.2}$$

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

private schools of any order (primary schools included).[33] While municipality fixed effects account for whatever time invariant difference among localities; year-LLM interacted fixed effects θ_{ct} restrict comparisons of treated and control units to municipalities exposed to the same labour market conditions and control for any time-varying factors within local labour market.[34] Standard errors are clustered at the municipality level.

Note that possible stable differences in school quality are accounted for by municipality fixed effects. Moreover, the inclusion of year-LLM interacted fixed effects controls for variations in school quality that concern the entire LLM.[35] Therefore, the only residual concern about the possible confounding role of school quality lies in idiosyncratic variations at municipal level which influence both the decision of closing schools and residential choices. Such residual concern should be mitigated by the use of instrumental variables that exploit regional institutional thresholds for identification (see sub-section 1.5.2).

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

$$y_{ict} = \alpha + \Sigma_{m=-G}^{M} \beta_m \, z_{ic(t-m)} + \gamma_i + \delta_t + \eta \, X_{ict} + \theta_{ct} + \epsilon_{ict}, \tag{1.3}$$

[33] Private schools can represent a substitute service for public ones, and they may even endogenously respond to the closure of public schools. In Italy, however, private school enrolment is very residual at primary school level. Over 90% of primary-school pupils are enrolled in public schools (ISTAT data, available at: http://dati.istat.it/). Moreover, in our preferred sample of single-primary-school municipalities in regions adopting thresholds, we have at most one private primary school. In that sample, municipalities experiencing primary school closures do not have any private primary school, suggesting that endogenous supply of private schools is not a relevant phenomenon in this setting.

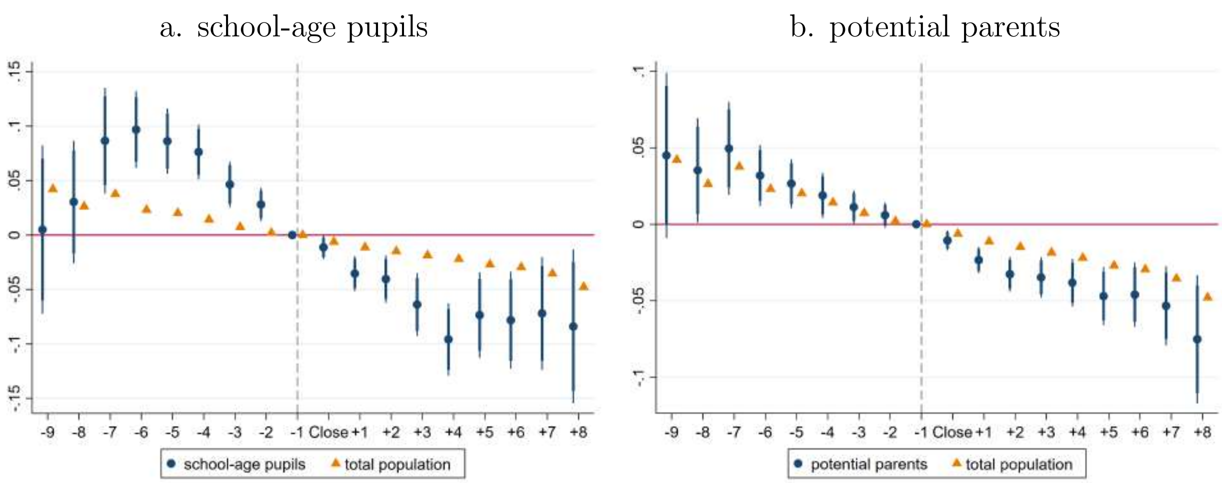
[34] One possible concern is that the inclusion of LLM fixed effects generates problems of double counting in case changes of residence mainly involve adjacent municipalities. We verify that our estimates are not inflated by double counting taking out LLM dummies from the specification, therefore allowing for comparisons of treated and control units across different LLM. The related results are reported in Table A1.9 of the Appendix.

[35] For example, natural events that may damage school buildings. In fact, school quality can be proxied either by students performance or by measures concerning buildings conditions, additional school services (gyms, canteens,...) School sizing plans and informal interviews with school directors suggest that the latter elements may be more relevant in the decision of closing schools compared to students performance.

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

We report the event study plots estimating equation 1.3, which provides a visual intuition of the plausibility of the identifying assumptions (Figure 1.4). The $\{\beta_m\}_{m=-G}^{M}$ coefficients are estimated with three different dependent variables: the population of school-age children, total residents, and the population of potential parents.

Figure 1.4: Population by age classes around school closure



The Figure shows event study plots corresponding to equation 1.3, using as dependent variable (log) total and school age population (Panel a) or (log) total population and potential parents, i.e. residents between 35 and 49 years old (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

As can be seen from the plots, all outcomes show pre-trends, which can be due either to anticipatory responses or to pre-existing depopulation trends in single-school municipalities experiencing school closures (i.e. reverse causality). Both explanations are plausible in our context. Indeed, school cuts may be discussed for some time before being actually put in place and young adults are likely to adapt their fertility and/or residence choices according to the expected change. Moreover, by definition school rationalisation policies affect municipalities in population decline, and this constitutes the greatest challenge for the parallel trend assumption to be met. School cuts take place precisely where the demand for school services is shrinking, making its provision inefficient. The pre-trends displayed in Figure 1.4 confirm this. They are especially marked for the population of potential recipients of that service: school-age children (Figure 1.4 a) and potential parents (Figure 1.4 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 bias in such context, as event study estimates of pre-treatment periods can be contaminated by post-treatment effects, invalidating the common procedure of testing for pre-trends by looking at pre-treatment coefficients (Sun and Abraham, 2021). We follow this literature strand and adopt the estimator proposed by Sun and Abraham (2021), allowing to compute event studies as weighted averages of cohort-specific $ATTs$, with weights corresponding to the shares of treatment cohorts. The corresponding event study plots are displayed n Appendix Figure A1.4. As visible, these estimates confirm the presence of significant pre-trends, indicating that identification concerns are not resolved by accounting for treatment heterogeneity. Instead, pre-trends are likely to derive from a combination of anticipatory behaviour and pre-determined municipal demographic conditions.

1.5.2 Instrumental Variable models

To address possible anticipation effects and issues of reverse causality, we combine the TWFE estimation presented above with Instrumental Variable (IV) strategies.[36] For our IV models, we exploit the institutional rules on school sizing adopted by seven Italian regions over the period considered. Therefore, we restrict the analysis to the sample of regions adopting school sizing thresholds, illustrated in Panel b of Figure 1.2.

As explained in section 1.3.2, following the rationalisation push of the 'Gelmini' reform, seven Italian regions introduced in their school sizing plans thresholds in the number of students required to keep a school active. Such thresholds were adopted in different years by the various regions and, once activated, they applied to all schools within the region. The introduction of these thresholds significantly increase the number of school closures by making more binding the size requirements to keep a school active. Figure 1.5 shows the number of single-primary-school closures by relative year from the introduction of the regional threshold. It can be noticed that in the very first years since their implementation, these thresholds produced a marked increase in school closures.[37]

We leverage this setting and implement two complementary IV models. Firstly, we construct the following instrument:

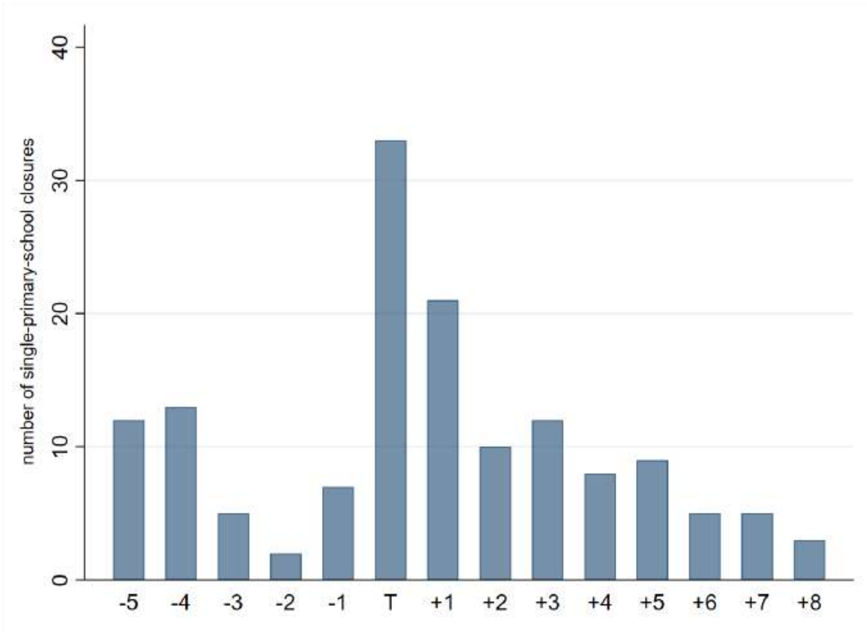
$$Dummy\, IV_{irt} = S_{ir,2010} \cdot T_{rt} \tag{1.4}$$

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 school year, 2009/2010, and T_{rt}

[36] 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).

[37] In the years preceding threshold introduction, we can still notice some closures, in particular 4 and 5 years before. It is worth clarifying that those values correspond to Sardinia in school years 2010-2011, and 2011-2012. In those years, the rationalisation effect of the 'Gelmini' reform was the strongest, as it can be observed looking at the overall number of school closures in Figure 1.3.

Figure 1.5: Number of school closures since threshold introduction



The graph reports the number of school closures by relative year from the introduction of school sizing thresholds at regional level. The sample consists of single-primary-school municipalities in the regions adopting thresholds.

is a dummy taking value 1 from the school year in which a threshold for school closure has been introduced in region r until the end of the period.[38] While all regional thresholds have been introduced years after 2010 (see the timeline in Figure 1.1), we still refer to the school conditions in 2010 to construct the IV. Therefore, the instrument is constructed as a dummy variable taking value 1 from the moment of the introduction of the regional threshold if the school was below that threshold in the pre-threshold year 2010, and 0 before. Figure A1.3 in the Appendix displays the number of municipalities above/below the threshold according to 2010 school characteristics.

The choice of employing school characteristics in 2010 instead of contemporaneous ones is expected to make the IV more exogenous. Indeed, the exclusion restriction for the validity of

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

our instrument is that being below the threshold does not directly affect municipal population and income. Parents may react even to the *risk* of school closure induced by the presence of the threshold by sending their children to other schools, making contemporaneous school characteristics endogenous. However, by 2010 none of the sample regions had introduced numerical criteria for school closure yet. Therefore, taking school characteristics prior to the introduction of thresholds mitigates the concerns of endogenous household response. In this way, we can take the (initial) school position with respect to the threshold as exogenous ($S_{ir,2010}$). At the same time, the timing for the introduction of the threshold at regional level (T_{rt}) is plausibly exogenous with respect to municipal conditions.

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

$$y_{ict} = \alpha + \beta\, Clo\hat{s}ure_{ict} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{1.5}$$

where $Clo\hat{s}ure_{ict}$ is predicted from the first stage equation

$$Closure_{ict} = \alpha + \beta\, Dummy\, IV_{irt} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_{ct} + \epsilon_{ict} \tag{1.6}$$

We run the above specification for the full sample of single-primary-school municipalities in regions adopting thresholds. Moreover, we restrict the estimation to schools closer to the regional threshold, in order to focus on a more homogeneous group of schools and municipalities. We exploit the symmetric window of ±50 students around the threshold shown in Figure 1.7.[39] In the main analysis, we select a bandwidth of 50 students above and below the threshold, while Appendix Table A1.8 reports the estimates for windows of ±45 and ±40 students, to check the sensitivity of our results to alternative bandwidth choices. Esti-

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

mations on the restricted sample around the threshold have greater internal validity, since we compare schools with a similar number of students. Conversely, full sample estimations entail greater external validity, since bigger schools are included in the control group. We conduct the analysis comparing treated and control municipalities within the same region, which mitigates possible concerns related to the different number of sample units in the various regions.

To provide supporting evidence for our claims of instrument exogeneity, we perform event studies of reduced form estimates for a model mirroring equation 1.3, where instead of computing leads and lags referring to each year before/after school closure, we look at periods before/after the introduction of the threshold. These estimates allow to observe the evolution of the outcome variables around the threshold introduction event. We would expect to see no pre-trends as a sign of no difference between municipalities whose school was below a school-sizing threshold, before its introduction, and municipalities whose school was above it.

In this reduced-form setting, the verification of the parallel trend assumption can be interpreted as a test for instrument exogeneity. It should be noted that, due to the way in which the instrument is constructed, we do not have staggered IV adoption within regions. For all municipalities of a given region whose school is below the future threshold in 2009/2010, the instrument takes value one from the moment a threshold is introduced until the end of the period. Our analysis is performed within-region, since we impose LLM-year fixed effects and LLMs are partitions of regions.[40] Therefore, for these reduced-form regressions, we should not face treatment heterogeneity issues potentially associated with TWFE models with staggered adoption and we employ the traditional event study estimator.

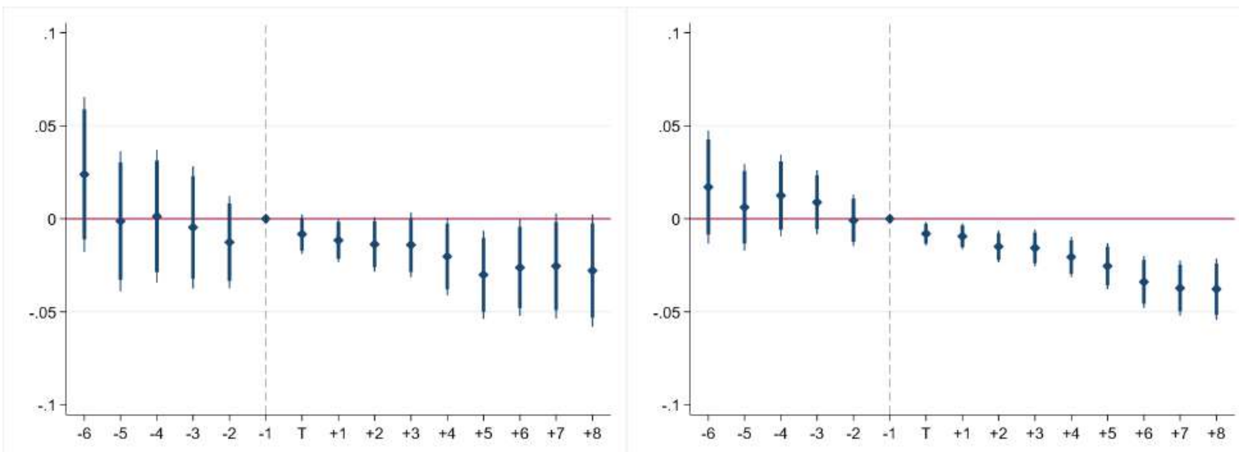
Figure 1.6 present the results of these estimates in the form of event study plots, using the restricted sample of schools/municipalities around the threshold and population out-

[40] 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.

come variables - school-age and potential parents' population. Figure A1.7 in the Appendix reports analogous plots for total and per-capita income. Overall, we find no significant pre-threshold differences in terms of demographic or income for municipalities below the threshold, suggesting that the instrument is exogenous.

Figure 1.6: Event study plots of the reduced-form estimation: population

a. School-age population b. Potential parents



The Figure shows the event study plots corresponding to the reduced form of equation 1.3, where dependent variable is (log) population in school age (Panel a) or (log) population of potential parents, i.e. residents between 35 and 49 years old (Panel b). Those outcome variables are regressed on leads and lags of the instrument. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

As a form of placebo test, we also estimate the event study of the reduced form model using the population between 55 and 65 years old as dependent variable. We expect such age class to be little or no affected by the introduction of school-sizing thresholds, since these individuals are too old to be parents of primary school children. Most people in that age group are still in the labour market. Therefore, if our estimates were driven by labour market dynamics affecting residential choices, we should find an impact also on that population sub-group.[41] As shown in Figure A1.8 in the Appendix, all coefficients of post-threshold dummies are insignificant, indicating no effect of the introduction of school thresholds on this age group.

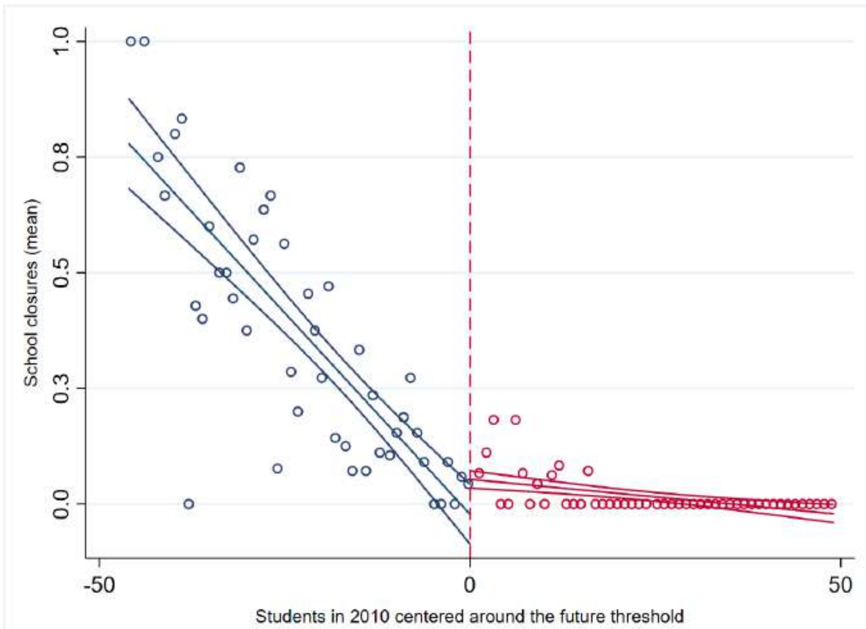
One residual concern could be the presence of differential trends in outcome evolution depending on *how far* below the threshold the school was in 2010. If the margin of deviation from the threshold correlates with the predictive capacity of our main instrument (equation 1.4), this would create an omitted variable problem.

As a first check, using the information on the number of students in primary schools, we test whether we can detect a correlation between the deviation from the threshold and the probability of closure. We centre the number of students around the threshold and show schools with up to 50 students above/below the threshold, since most regions fix the threshold for school closure at 50. The number of students refers to the first year in sample, school year 2009/2010. Figure 1.7 plots the probability of experiencing school closures over the time span considered. It shows a significant difference in derivatives at the cutoff. The likelihood of closure increases with the distance from the threshold on the left-hand side of the graph, i.e. for schools with fewer students than the threshold. We also note that there are schools below the threshold which do not close, and schools above the threshold that experience school closure. This is mainly due to the fact that we are taking school characteristics in

[41] An alternative placebo age class could be that of residents between 20 and 35 years old, which are presumably too young to be parents of primary school age children. People in this age group might still value the presence of a school if they plan to have children, but they are not immediately affected by school closure. At the same time, they are generally more mobile than elderly people ISTAT (2019). For that reason, we also report placebo estimates for that age group (see Table A1.7 in the Appendix).

2010. Most closures above the regional threshold refer to schools that lose students after 2010 and were below the threshold when they close.[42] 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.[43]

Figure 1.7: Probability of closure by deviation from the regional threshold



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

Therefore, we construct a second instrumental variable which incorporates the deviation from the threshold in the construction of the IV, by multiplying our previous dichotomous

[42] To provide some descriptive evidence on schools complying or not with the institutional threshold, Figure A1.6 shows the percentage of compliers within each Region adopting school sizing thresholds. Moreover, Table A1.4 reports few summary statistics for complier *versus* non-complier schools, showing that - on average - the former are in smaller and less elevated municipalities, closer to economic centres and to the next available primary school. By 'economic centres', we refer to municipalities that represent the core of the corresponding LLM. See sub-section 1.7.1 for more details on how those distances are computed.

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

instrument by the number of students in 2009/2010. Formally:

$$Kink\ IV_{irt} = (Students - c)_{ir,2010} \cdot S_{ir,2010} \cdot T_{rt} \tag{1.7}$$

where $(Students - c)_{ir,2010}$ is the number of students in 2009/2010, 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 2009/2010; and T_{rt} is a dummy for the introduction of a threshold for school closure in region r, year t. In practice, this $Kink\ IV_{irt}$ is a continuous variable resulting from the interaction between $(Students - c)_{ir,2010}$ and the $Dummy\ IV_{irt}$.

We label it 'kink' because it exploits the kink in treatment probability at the cutoff shown in Figure 1.7. This strategy draws insights from the kink RDD, a recent advancement of the RDD approach in which identification is based on discontinuity in derivatives - rather than levels - of treatment probability at the cutoff.[44] Here we exploit the slope change in closure probability at the threshold to construct the IV.

We perform the estimation of the impact of school closure, instrumenting it with the kink IV, using the full sample of single-primary-school municipalities in regions adopting thresholds. For the exclusion restriction to hold, we need to control for the number of students, as this plausibly correlates with our outcomes and it is included in the kink instrument. Therefore, not accounting for it would cause the instrument to directly predict our dependent variables. We augment the specification of equation 1.1 with the interaction between the number of students in 2009/2010 and the dummy for the regional threshold being active. In a context with municipality fixed effects, this time-varying interaction term can be interpreted as a running variable capturing the underlying relationship between the number of students and

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

the outcome at the policy change. Formally, we estimate:

$$y_{ict} = \alpha + \beta\, \hat{Closure}_{ict} + \gamma_i + \delta_t + \eta\, X_{ict} + \theta_{ct} + (Students - c)_{ic,2010} \cdot T_{rt} + \epsilon_{ict} \qquad (1.8)$$

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

$$\begin{aligned} Closure_{ict} = \alpha + \beta\, (Students - c)_{ir,2010} \cdot S_{ir,2010} \cdot T_{rt} + \gamma_i + \delta_t \\ + \eta\, X_{ict} + \theta_{ct} + (Students - c)_{ic,2010} \cdot T_{rt} + \epsilon_{ict} \qquad (1.9) \end{aligned}$$

1.6 Main results

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

We report the OLS estimates of the TWFE model presented in equation 1.1 in Table A1.5 of the Appendix, both for the full sample of all regions (Panel a) and the restricted sample of all single-primary-schools in regions with thresholds (Panel b). The results, remarkably similar across samples, display negative coefficients linking school closure with school-age population, potential parents, and total income, while no relationship with per-capita income and elder population.[45] We cannot interpret these coefficients causally due to the pre-trends visible in Figure 1.4.

We address the endogeneity induced by pre-trends with IV estimates. First, in Table 1.2 we present first stage results from equations 1.6 and 1.9, to provide evidence of the relevance and

[45] In Table A1.7 we report analogous estimation for the alternative placebo outcome of resident population between 20 and 35 yeard old. Also for that age class we obtain a non-significant effect of school closure.

strength of our instruments $Dummy\,IV_{irt}$ and $Kink\,IV_{irt}$. Column 1 refers to the sample of all single-primary-school municipalities in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of school year 2009/2010. For both samples, the instrument is a good predictor of the probability of treatment. The F-test is well above the conventional value of 10, meaning that we can safely exclude weak instrument concerns. For single primary schools, being below the threshold in 2009/2010 increases the probability of experiencing school closure by 15%. The relatively small size is determined by the fact that there is significant non-compliance below the threshold - some undersized schools are kept active in derogation from regional rules. In addition, there is some non-compliance above the threshold, i.e. schools closing while being above the threshold in 2009/2010. This is mostly due to the way in which the instrument is constructed. We mark as 'above thresholds' schools that were so in 2009/2010, but then decline in enrolments, and - once below the threshold - close (Figure 1.7). Column 3 of Table 1.2 reports the first stage results of equation 1.9. The negative sign of the estimated coefficient of the kink instrument in the Table relates to the negative slope of the left-side plot of Figure 1.7. Once the threshold is active, the lower the number of students below the cutoff, the greater the probability of closure.

Table 1.2: First stage results

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

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Columns 1 and 2 report first stage estimates corresponding to equation 1.6, regressing school closure on the instrument dummy variable, taking value one if the school was below the regional threshold in 2010, from the year of its introduction. Column 1 refers to the sample of all single-primary-schools in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Column 3 reports first stage estimates corresponding to equation 1.9, regressing school closure on the kink instrument: the interaction between the deviation from the regional threshold in 2010 and the dummy instrument. All specifications include controls for other school endowments, municipality and LLM-year fixed effects; column 3 includes distance from threshold in 2010 interacted with threshold introduction (labeled running variable).

In Table 1.3 we report second stage estimates corresponding to equation 1.5. The coefficients represent the average percentage variation over the post-treatment period in municipalities experiencing school closures, relative to the pre-closure period and to municipalities not experiencing school closures. Panel a refers to the sample of all single-primary-school municipalities in regions adopting thresholds, while Panel b restricts the sample to schools with up to 50 students above or below the regional threshold as of school year 2009/2010. In Table A1.8 of the Appendix, we report results from analogous estimations using bandwidths of 45 or 40 students above/below the threshold.[46] Moreover, in Table A1.9 of the Appendix we also report results of analogous estimations which do not include LLM fixed effects, so to verify that our estimates are not affected by double counting due to possible changes of residence within the same LLM. In fact, allowing for controls outside the LLM increases coefficients' magnitude, suggesting that possible double counting is at least less relevant than accounting for local labour market trends. Alternatively, not including LLM fixed effects is likely to result in less comparable treated and control units, and thus inflate coefficients.

[46] Results for those alternative bandwidth choices largely confirm the estimated coefficients of Panel b in Table 1.3. The estimate for school-age population and a bandwidth of 45 students is marginally insignificant, with a p-value of 0.106, while the coefficient's size confirms the main estimate of Panel b, Table 1.3. The estimate for the 40-students bandwidth is significant and similarly sized. All other coefficients are comparable to those of Panel b - Table 1.3 in significance, sign and size.

Table 1.3: IV estimation, second stage results

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

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 1.5, regressing school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with a dummy indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a refers to the sample of all single-primary-schools in regions adopting thresholds; Panel b, instead, to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010.

The estimates in the first and second columns of panel a show a significant reduction of around 15-18% in school-age population and potential parents. When looking at the restricted sample of schools with up to 50 students above/below the threshold (Panel b), coefficients appear slightly smaller in size. We obtain a 10% reduction in school-age population and an 14% decrease in the population of potential parents.[47] To interpret the size of coefficients, we have to bear in mind that the sample is composed of small municipalities,

[47] Table A1.6 in the Appendix reports the IV estimates including coefficients of the control variables, i.e. the time-varying endowments of public pre-schools and lower secondary schools, and private schools of any order.

with an average population of around 150 potential parents in the year preceding school closure. 18% of 150 corresponds to 27 residents, which could be parents of school-age children. We are dealing with approximately 10-13 couples.[48] Therefore, even small reductions in absolute population appear considerable in relative terms. In practice, such reductions are likely to be highly relevant for these small municipalities that suffer from population decline.

Coefficients are equally signed but larger in (absolute) size compared to the OLS TWFE estimates of Table A1.5 in the Appendix. This is consistent with the correction of the downward-sloping pre-trends we achieve through the IV strategy. Alternatively, possible anticipation effects and reverse causality issues created a downward bias in the OLS TWFE estimates, that we have now corrected through the IV.

As for the effect of school closures on income, the estimates in the third column of Panels a and b (Table 1.3) indicate that total income decreases by almost 13% in municipalities experiencing the closure of their only primary school, after the closure and relative to pre-closure and untreated municipalities. Per-capita income, instead, increases in these municipalities by 5% (Table 1.3, panel a, fourth column). This finding may result from the fact that re-locations mainly concern low-income households. School closures mostly affect young adults, who are highly concentrated in low-income classes and the positive coefficient on per-capita income may be ultimately due to the demographic effect detected on potential parents.[49] However, when we look at the restricted sample of schools with up to 50 students

[48] It is plausible that among relocating residents there are also previous school workers. However, note that we focus on the specific outcome group of residents between 35 and 49 years old. School personnel is not necessarily restricted to that age group nor undoubtedly residing in the municipality. Moreover, I deal with very small schools, with an average of 19 students in their last year of activity. It is quite unlikely that such schools employed 10-13 workers. For these reasons, it seems implausible that all the effect is driven by school personnel.

[49] To find more evidence on this, we have replicated event study estimates using the number of taxpayers in the lowest income class and the number of potential parents as outcomes. By 'low-income class' we mean households in the lowest category by annual taxable income as defined by the Italian Ministry of Economy and Finance (MEF), i.e individuals with an annual income between 0 and 10,000 euros. If we look at the resulting event study plot, displayed in Appendix Figure A1.10 we can note that the trajectories for these two groups overlap almost perfectly.

above/below the threshold (panel b), the coefficient on income per-capita loses significance.

Finally, the coefficient describing the impact of school closure using elder population as dependent variable (Table 1.3, last column) is statistically insignificant, confirming our prior that residents between 55 and 65 years old are not affected by school closures. This evidence supports our claim that the observed demographic dynamics are indeed due to school service cuts.

Table 1.4: Kink instrument: second stage results

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

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Second stage results from the TSLS estimation of equation 1.8, where we regress different dependent variables (potential parents, total income, per-capita income and elder population) on school closure, instrumented with the interaction between the margin of deviation from the threshold and an indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. The sample includes all single-primary-schools in regions adopting thresholds.

Next, in Table 1.4 we report the second stage results of the TSLS model instrumenting school closures with the 'kink' IV (equation 1.8). Since we adopt the number of students to construct the instrument, we cannot employ school-age population as dependent variable - we would have almost the same variable on both sides of the equation - and only use the population of potential parents and income. The model is estimated for the full sample of single-primary-school municipalities in regions adopting thresholds. The results in Table 1.4 confirm that school closures do not affect elder population, consistent with our prior expectation, but they affect the residential choices of young adults (i.e. potential parents) inducing their relocation, which in turn reduces the overall income of municipalities experiencing closures. The effect on per-capita income is insignificant, as in panel b, Table 1.3.

1.6.1 Fixed costs of primary schools

Our main outcomes of interest are population and income dynamics. In addition, we also investigate the impact of school closure on municipal public finances. Specifically, we are interested in checking the effect on the fixed costs of primary schools, whose reduction was the purpose of the Gelmini reform.

Exploiting data on municipal public accounts, available until the year 2015, we can look specifically at primary school spending at the municipal level and observe its variation year-by-year before and after school closure. Hence, we re-estimate event study models from equation 1.3 using (log) current and capital expenditures for primary school per-inhabitant as outcomes, for our sample of single-primary-school municipalities.[50] The estimates are reported in Panels a and b of Figure 1.8. The corresponding event studies using Sun and Abraham (2021) estimator are in Figure A1.5 in the Appendix.

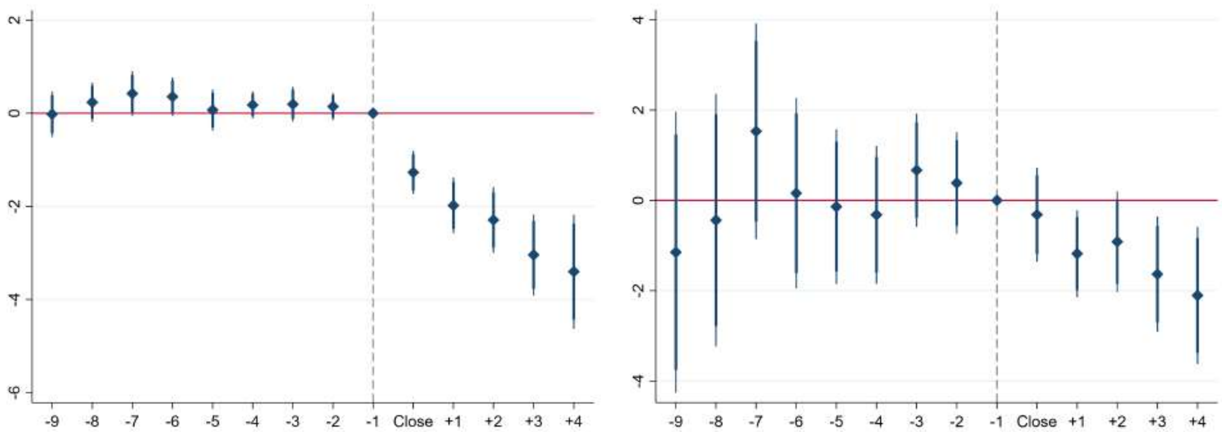
No coefficient of dummy variables referring to the pre-closure period emerges as statistically significant, suggesting that spending patterns of treated and control municipalities are very similar prior to school closures. Primary school budgets of municipalities mainly concern school infrastructure maintenance and utility bills, while school personnel is financed by the central government. As infrastructure maintenance and utilities represent fixed costs independent of school size (namely, of the number of students enrolled), it comes as no surprise that pre-closure spending appears to be evolving similarly across treatment and

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

control units. Even if school population is decreasing in the years preceding closure, these expenditures are constant as long as the only primary school in the municipality is active. As expected, we observe a very sharp reduction of expenditures for primary schools following the closures, both in the current and the capital expenditures of the treated municipalities. Such an evidence further confirms the 'rationalisation purpose' of closing undersized schools.

Figure 1.8: Municipal expenditures for primary schools

a. Current expenditures b. Capital expenditures



The Figure shows event study plots corresponding to equation 1.3, using as dependent variable (log) current expenditures for primary schools per inhabitant (Panel a); (log) capital expenditures for primary schools per inhabitant (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

1.6.2 Placebo closures

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

We select municipalities endowed with one or two primary schools in school year 2009/2010. We employ single-primary-school municipalities only as controls, thus discarding those experiencing school closures.[51] Further controls are municipalities with two schools that do not close over the period. Instead, we consider as treated municipalities with two schools which loose one of them in the span observed. For that sample, we re-estimate the TSLS model of equations 1.6 and 1.5. Here, the dummy instrument of equation 1.4 takes value one since the year of threshold introduction if at least one school was below the regional threshold according to 2010 school characteristics. As for the main analysis, we further restrict the estimation to municipalities with at most 50 students above or below the regional threshold, so to exclude large schools and thus make treated and control units more similar.[52]

Table A1.10 reports the related second stage results. None of the estimated coefficients appears significant. These findings support the claim that what matters for residential choices is the access to school services. If there exist other public options locally available, individuals do not relocate after school closures. Up to this point, we have defined 'local availability' taking as reference municipal boundaries. In the next section, we expand on this and investigate how the effect varies depending on the distance to further public primary schools.

1.7 Who loses the most?

1.7.1 Core and peripheral municipalities

Our estimates have uncovered a clear effect of primary school closures on residential dynamics. Parents of school-age children and pupils appear to respond to unexpected school cuts by moving away from their place of residence. While this result has been obtained with a varied

[51] We include single-primary-school municipalities to preserve sample size. Indeed, municipalities with two schools in 2009/2010 are just 11% of Italian municipalities, against 28% of single-primary-school municipalities

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

sample of single-school municipalities distributed across the whole Italian territory, it may differ depending on the pre-determined conditions of treated municipalities. In particular, more peripheral places located further away from economic centres and with less access to alternative school services may be most affected by the closures of their only primary school. Economic centres may not only act as substitutes for local services, but also as attractive poles, draining resources from more peripheral areas.

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.[53]

In order to capture municipal peripherality, we consider two different dimensions. We compute municipal distance in metres to the nearest centre of the Local Labour Market, and distance to the next available public primary school measured at the beginning of the period considered, school year 2009/2010. Distance to economic centres is computed as the distance in metres between the borders of the municipality representing the centre of the LLM and the borders of a given single-primary-school municipality. Distance to the closest school is measured by exploiting the exact geo-location of schools, computing the distance in metres between the closing school and the next one available.[54] By 'centre of LLM' we mean the municipality constituting the core of the corresponding LLM as identified by the Italian Institute of Statistics.

Next, for both these indicators, we divide our full sample of municipalities in sub-groups on the basis of their median value, to identify areas located close to (below median), or far from (above median) LLM centres or alternative primary schools.[55] Those two criteria do not

[53] In Table A1.13 of the Appendix, we repeat the heterogeneity analysis by interacting the school closure dummy with an indicator for the municipality being above the median distance from economic centres or alternative schools. Results are qualitatively equal to those obtained with the sample-split method.

[54] The median distance to LLM centres is 7.1 kilometres, while the median distance to the next primary school is 3.1 kilometres.

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

overlap, as municipalities far from LLM centres are not necessarily also far from the closest available primary school, and *viceversa* (see Table A1.11 in the Appendix).[56]

By looking at the distance from LLM centres, we aim to capture the degree of centrality of the municipality and the differences in access to job opportunities. The predictions are not straightforward. On the one hand, being close to economic centres can entail better market access and reduced commuting time, which would mitigate the negative effect of school cuts. On the other hand, economic centres can exert a highly attractive force on nearby locations, while municipalities located far away from them might suffer less from congestion and provide better amenities, such as environmental quality. Distance to the nearest primary school, instead, can be seen as reflecting differentials in treatment intensity among municipalities. Our hypothesis is that the further away the next school is when the only available primary school closes, the higher would be the incentive for residents to relocate.[57]

Table 1.5 reports the results sub-dividing the full sample along these dimensions.[58] School-age population is the dependent variable in the first two columns, the population of potential parents is the dependent variable in the third and fourth columns, and total income is the dependent variable in the fifth and sixth columns. Reduced form event study plots showing the evolution of municipalities with schools below regional threshold in 2010 around the threshold introduction, for the two samples of municipalities far from SLL and far from the next available school, are displayed in Figures A1.11 and A1.12 in the Appendix.

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

[57] It is worthy to note here that regional sizing plans often suggest to provide schoolbus to facilitate access to further available school in case of school closures. Unfortunately, we do not have information on this point for the period observed. However, the presence of such transport services would represent a downward bias in our estimates, since it would - at most - mitigate the effect of school closure.

[58] The estimates refer to the sample of schools less than 50 students above/below regional thresholds in 2010. Estimates with all single-primary-school municipalities are in Appendix table A1.12, while comparable estimates using interaction terms rather than sample splits are in Appendix table A1.13

The result of panel a, Table 1.5 seems to suggest that the whole result of school closures on residential dynamics and local income is driven by municipalities located far away from the centres of Local Labour Markets. This finding supports the view that households value proximity to economic centres. This presumably offers more and relatively accessible service and labour opportunities, which induces residents of nearby municipalities not to relocate when the school closes. On the contrary, the same cannot be said for municipalities too far from urban areas, where commuting is not much of an option. The estimates reported in panel b, instead, confirm our prior that the incentive to relocate after a school cut is stronger when the next primary school is located further away.

In summary, the evidence emerging from Table 1.5 suggests that school closures foster population decline and consequently reduce local income especially in peripheral locations. Hence, school cuts appear to harm locations which already had limited access to school services and job opportunities. The reduction in population and total income may in turn produce additional depressive effects on the municipality, in terms of reduced demand for local services, entrepreneurial capacity, and thus job creation. All this is in line with the idea that rationalisation policies in key public services affect territorial disparities, by widening the existing intra-regional gaps in terms of population growth and income.

Table 1.5: School closure effect by municipality location

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

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from equation 1.5, where dependent variables are school-age population, potential parents and total income. Sample of schools with up to $\pm$ 50 students from threshold in 2010. In Panel a we subdivide our sample by distance to LLM centres and separately estimate equation 1.5 for municipalities above (far) or below (close) the median distance to LLM centres. In Panel b we follow an analogous procedure, considering instead distances to the closest public primary school.

1.7.2 Core and peripheral labour markets

It would be interesting to know whether school cuts only produce a re-distribution of population and income across municipalities or whether they also generate losses (or gains) on a more aggregate scale. While a complete welfare analysis is beyond the scope of this paper, we can give an initial indication of whether school closures have a population or income impact beyond municipal boundaries. Specifically, we investigate possible effects at the LLM level. In doing so, we define treatment in a cumulative way, summing up single-primary-school closures as they occur within the same LLM. For that second definition of treatment, we cannot instrument closure with our proposed measure on the school being below the threshold in

2009/2010 (equation 1.4). Therefore, the related results are not soundly causal, and must be interpret just as suggestive evidence. However, going for a simple OLS estimation allows us to enlarge our sample to all LLMs, independently from the number of single-school closures and from whether the corresponding region adopts any threshold over the period considered. We then estimate a TWFE model where we regress LLM-level population or income on the treatment measure defined above, control for school endowments at LLM-level, LLM fixed effects, region-year fixed effects, and cluster standard errors at LLM level. Formally,

$$y_{ct} = \alpha + \beta\, Closure_{ct} + \gamma_c + \delta_t + \eta\, X_{ct} + \theta_{rt} + \epsilon_{ct} \tag{1.10}$$

where c refers to LLM and r to region.

The results are illustrated in Table 1.6. When focusing on the sample of LLM with no provincial city (panel a), we find a negative relationship between school closures and population and income, which could signal a general decline of this type of labour market areas. Interestingly, however, the significant coefficient disappears when we focus only on LLMs containing a provincial city.[59] This evidence seems to support the view that only the most peripheral LLMs are negatively affected by school closures within their boundaries. Conversely, LLMs with provincial cities, which are generally sizeable urban areas, do not suffer negative consequences from the closure of primary schools in single-primary-school municipalities.

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

Table 1.6: Cumulative effect of school closures at Local Labour Market level

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

Clustered standard errors at LLM level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Results from the OLS estimation of equation 1.10, regressing school-age population, potential parents, total and per-capita income - aggregated at LLM level - on the (cumulative) number of single-primary-school closures occurred in that LLM at any given year over the period considered (2010-2019). All specifications include controls for other school endowments - public and private -, LLM and Region-year fixed effects. The sample for this estimation includes all Italian LLMs, with the exceptions of those of Trentino-Alto-Adige and Valle d'Aosta and cross-region LLMs.

1.8 Concluding remarks

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

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

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

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

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

Bibliography

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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.

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

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

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

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

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

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

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

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.

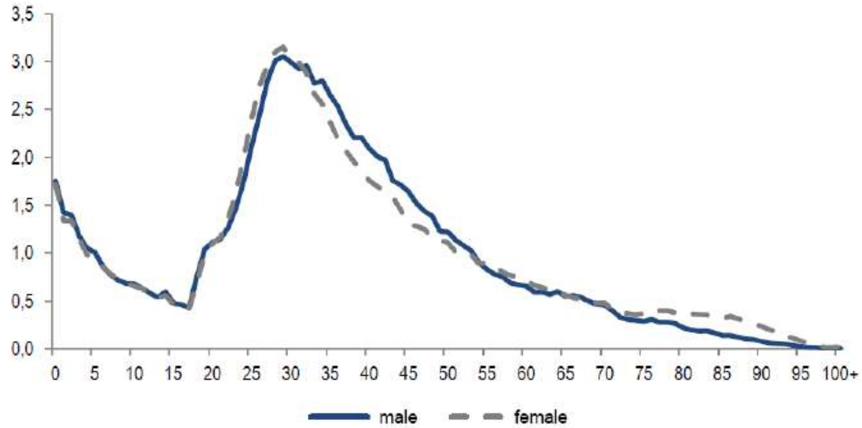
Sun, L. and Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199.

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.

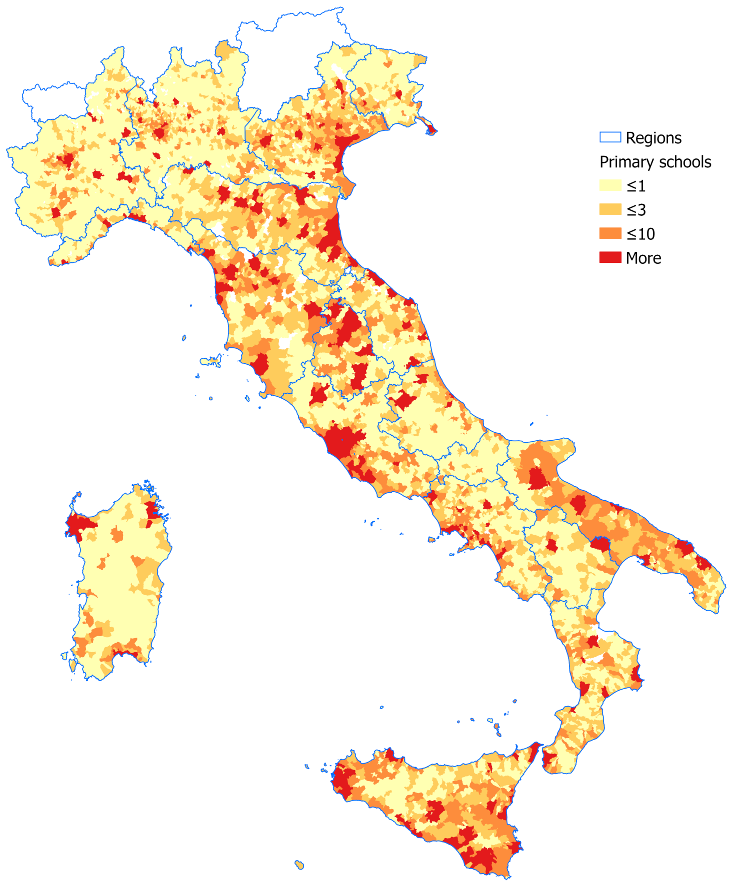
Appendix

Figure A1.1: Percentage of internal migrants by age (2017)



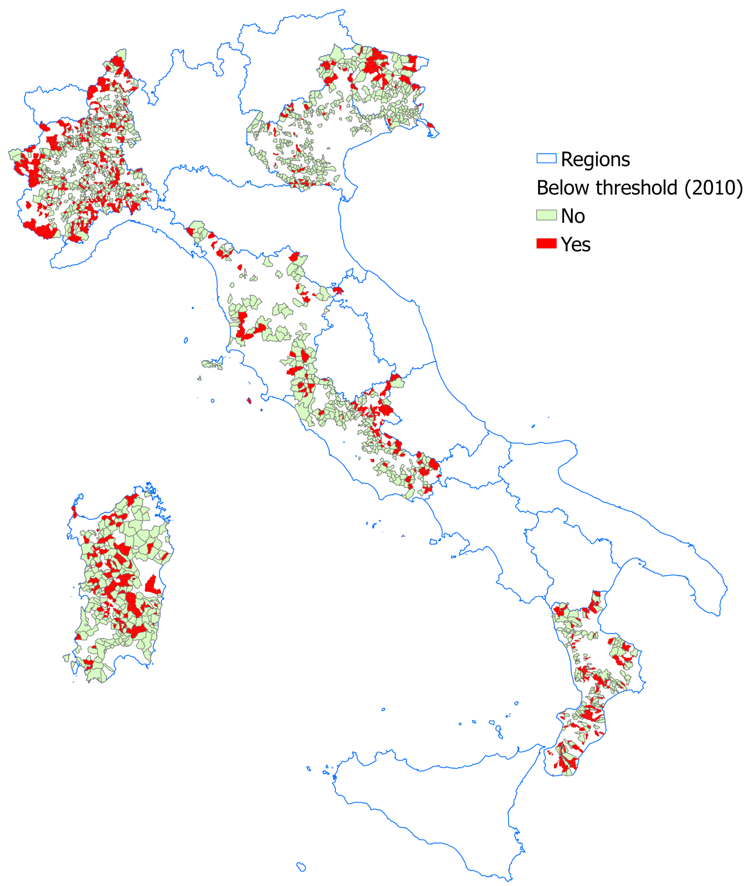
The Figure shows the distribution of internal migrations (i.e. changes of residence across Italian Provinces) in percentage values by age class (horizontal axis). Data refer to 2017. Source: ISTAT (2018).

Figure A1.2: Primary school endowments by municipality in school year 2009/2010



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

Figure A1.3: Single-primary-school municipalities above/below the regional school-sizing threshold, in regions adopting a threshold



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

Table A1.1: Summary statistics: all single-primary-school municipalities

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

Table A1.2: Summary statistics: single-primary-school municipalities in regions with thresholds

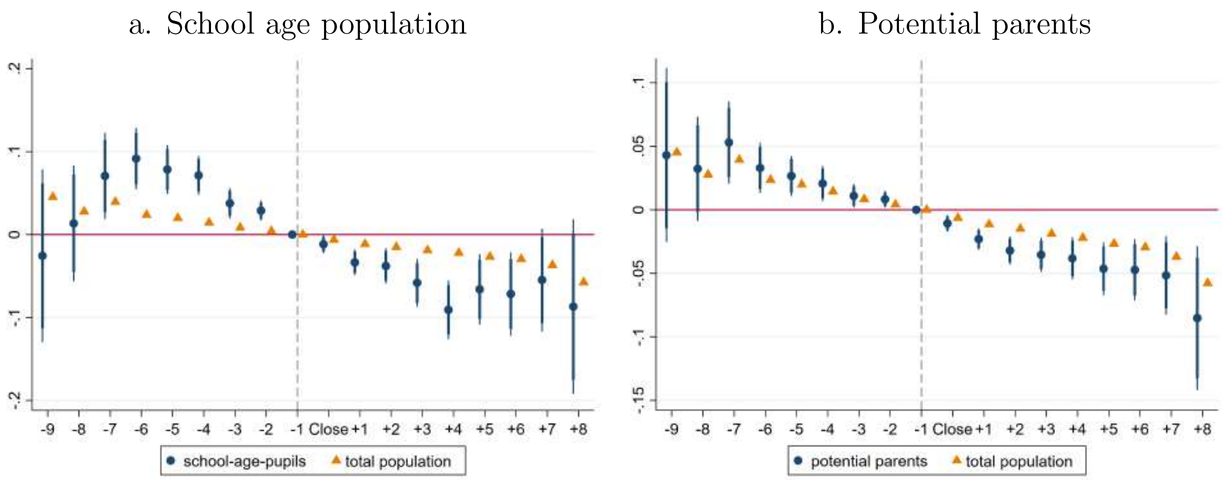
Variable	Obs	Mean	Std. Dev.
School-age population	18,900	197.69	180.56
Population of potential parents	18,900	509.755	454.3136
Elder population	18,900	304.07	245.78
Total population	18,900	2255.1	1879.8
Total income	18,884	28544.4	26797.7
Numb. of taxpayers	18,884	1572.4	1305.6
Per-capita income	18,884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

Table A1.3: Summary statistics: variables description and source

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

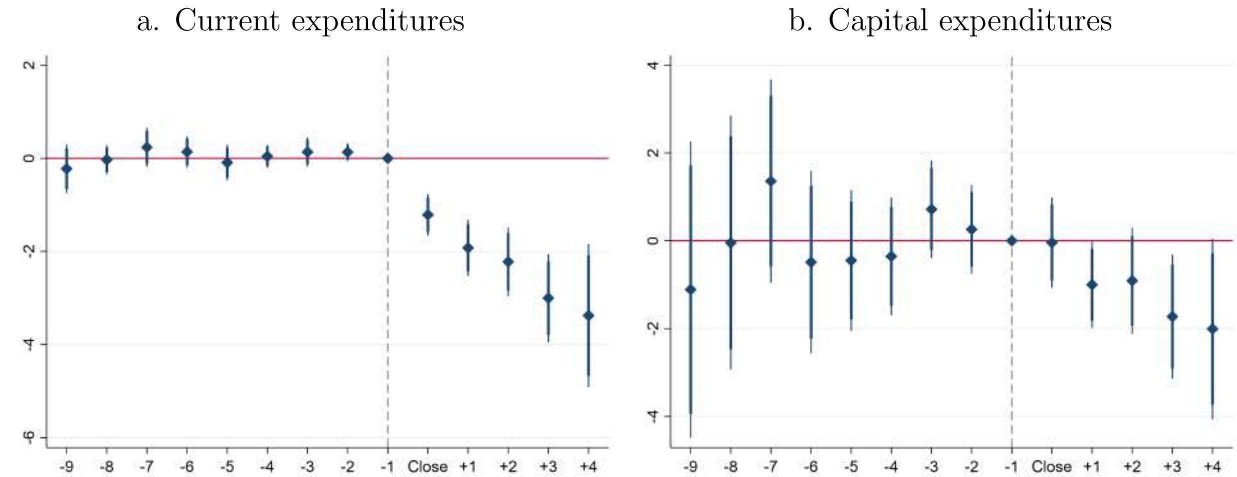
ISTAT: Italian Institute for Statistics; MIUR: Italian Ministry of Education; MEF: Italian Ministry of Economy and Finance

Figure A1.4: Population by age classes around school closure - Sun and Abraham (2021) estimator



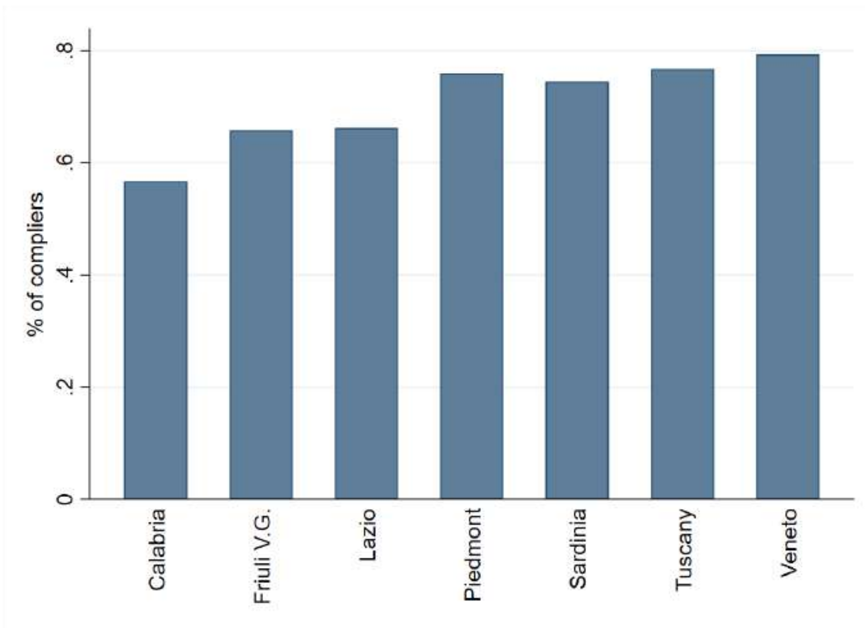
The Figure shows event study plots employing the estimator proposed by Sun and Abraham (2021), which corrects for possible heterogeneous treatment effects across cohorts. Plotted coefficients relate to equation 1.3, where dependent variable are total and school-age population (Panel a) or total population and potential parents, i.e. residents between 35 and 49 years old (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A1.5: Municipal expenditures for primary schools - Sun and Abraham (2021) estimator



The Figure shows event study plots employing the estimator proposed by Sun and Abraham (2021), which corrects for possible heterogeneous treatment effects across cohorts. Plotted coefficients relate to equation 1.3, using as dependent variables: log current expenditures for primary schools per inhabitant (Panel a), log capital expenditures for primary schools per inhabitant (Panel b). Event time corresponds to the year of primary school closure. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A1.6: Percentage of schools complying with Regional thresholds



The Figure shows the percentage of single-primary-schools complying with the school sizing threshold by Region. The sample refers to single-primary-schools in Regions adopting thresholds and with at most 50 students below/above the threshold as of 2010.

Table A1.4: Summary statistics: complier and non-complier schools

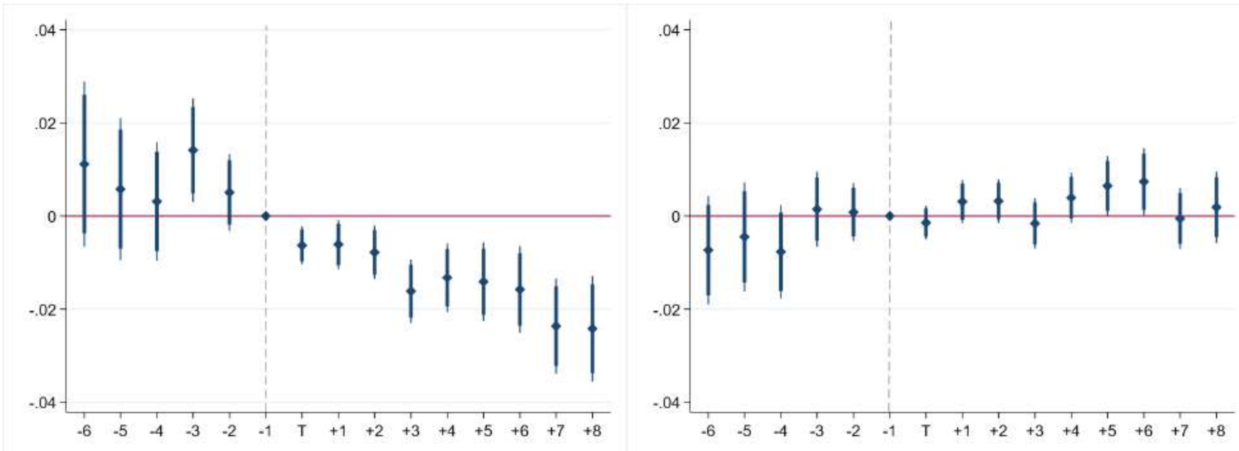
Variable	Obs	Mean	Std. Dev.	Min	Max
		Non-complying single-primary-schools			
Municipal centre's elevation	3,360	515.1815	294.1522	5	1760
Municipal land area	3,360	31.28259	27.34783	1.7454	160.7339
Distance to next school (2010)	3,070	3729.348	2950.523	183.2421	36925.44
Distance to LLM centre (2010)	3,360	9666.012	6802.212	165.2856	38875.25
		Complying single-primary-schools			
Municipal centre's elevation	8,450	372.8568	263.6295	2	2035
Municipal land area	8,450	28.96765	27.51871	2.1726	183.172
Distance to next school (2010)	7,300	3475.359	2041.092	453.5333	19279.45
Distance to LLM centre (2010)	8,450	8663.398	6036.995	0	38058.83

The Table reports some summary statistics about schools complying or not with the Regional threshold. The sample refers to single-primary-schools in Regions adopting thresholds and with at most 50 students below/above the threshold as of 2010. We select few variables related to municipal characteristics (centre's elevation and land surface) and to the geographical location of the municipality (distance to next school and LLM centre). For a more detailed description of these variables see Table A1.3.

Figure A1.7: Event study plots of the reduced-form estimation: income

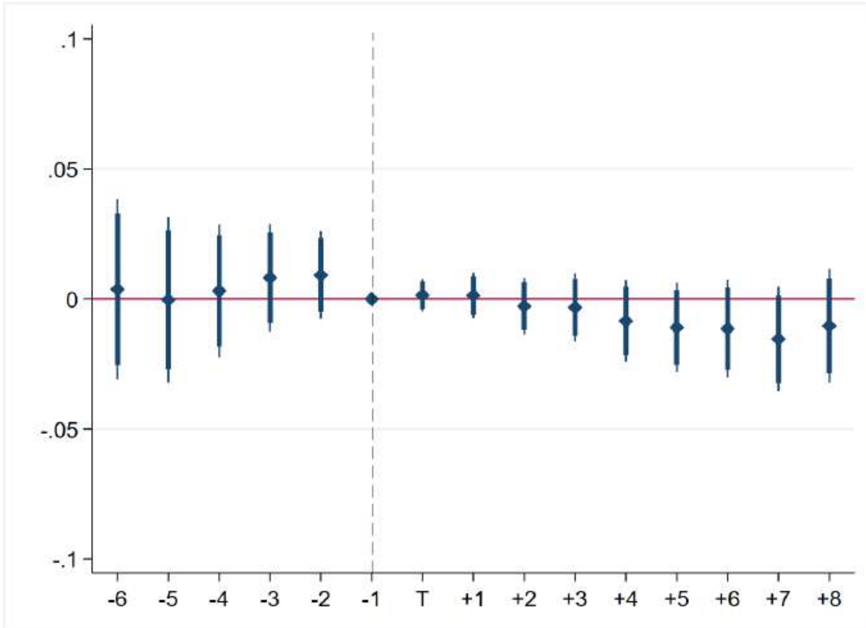
a. Total income

b. Income per-capita



The Figure shows the event study plots corresponding to the reduced form of equation 1.3, where dependent variable is (log) total (Panel a) or (log) per-capita income (Panel b). Those outcome variables are regressed on leads and lags of the instrument. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A1.8: Event study plot of the reduced-form estimation: elder population



The Figure shows the event study plot corresponding to the reduced form of equation 1.3, where dependent variable is (log) population between 55 and 64 years old. The outcome variables is regressed on leads and lags of the instrument. We interpret the plot as a sort of placebo, since we do not expect residents in that age class to be affected by school closures, while they are plausibly still in the labour market. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Table A1.5: OLS estimates (TWFE model)

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

Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Results of the OLS estimation of equation 1.1 on the sample of single-primary-school municipalities in regions adopting thresholds for school sizing. We regress school-age population, potential parents, total income, per-capita income and elder population on school closure. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a: full sample of all single-primary-school municipalities; panel b: sample of all single-primary-schools in regions with thresholds.

Table A1.6: IV estimation, second stage results showing controls

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

Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 1.5, where we regress different school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with a dummy variable referring to the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Sample of all single-primary-schools in regions with thresholds.

Table A1.7: IV estimation, second stage results for alternative placebo outcome

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

Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. Columns 1 and 2 report second stage estimates corresponding to equation 1.5 for the alternative placebo outcome corresponding to resident population between 20 and 35 years old. Column 1 refers to the sample of all single-primary-schools in regions adopting thresholds; column 2 refers to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. All specifications include controls for other school endowments, municipality and LLM-year fixed effects.

Table A1.8: IV estimation, second stage results with alternative bandwidth choices

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

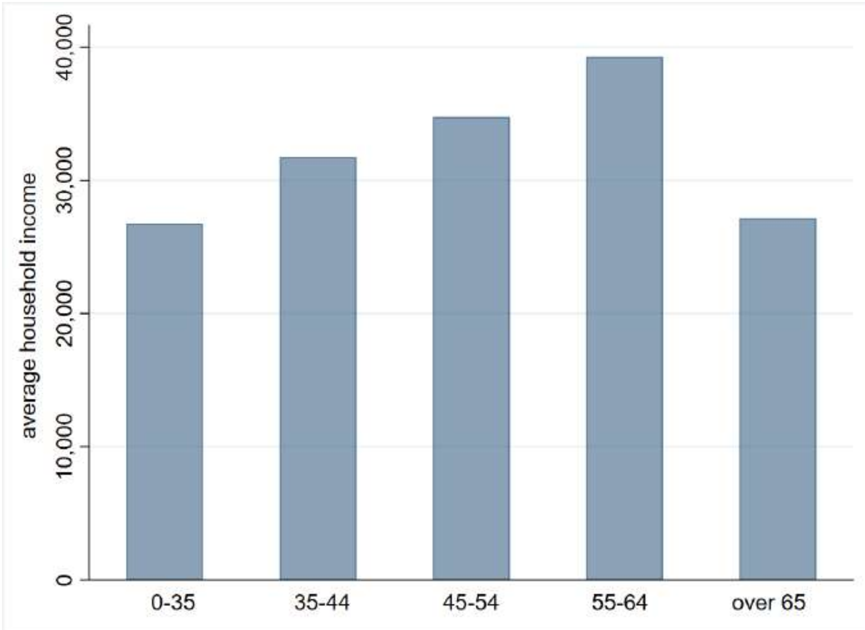
Standard errors clustered at municipal level in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 1.5, regressing school-age population, potential parents, total income, per-capita income and elder population on school closure, instrumented with an indicator for the school being below the regional threshold in 2010, from the year of its introduction. All specifications include controls for other school endowments, municipality and LLM-year fixed effects. Panel a and b refer, respectively, to the sample of schools with at most 45 and 40 students above/below the regional threshold in 2010.

Table A1.9: OLS and IV estimation without including LLM fixed effects

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

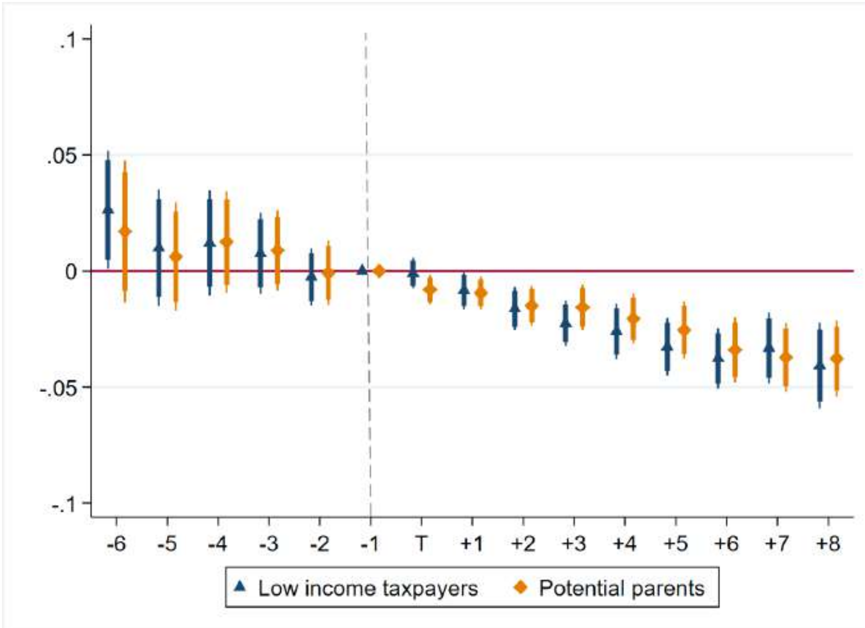
Clustered standard errors at municipal level in parentheses; * p<0.10, ** p<0.05, *** p<0.01. The Table reports OLS (Panel a) and IV - second stage (Panel b) results, respectively from equations 1.1 and 1.5, but taking out from these specification LLM dummies. The estimation sample is that of all single-primary-schools in regions adopting thresholds.

Figure A1.9: Household income distribution by age class of family's head



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

Figure A1.10: Event study plot of the reduced-form estimation: low income taxpayers and potential parents



The Figure shows the event study plot corresponding to the reduced form of equation 1.3, 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 1.4. The sample is restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Table A1.10: Placebo closures: IV estimation, second stage results

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

Clustered standard errors at municipal level in parentheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Second stage results from the TSLS estimation of equation 1.5. Panel a refers to the sample of all municipalities with one or two primary schools in regions adopting thresholds; Panel b, instead, to the restricted sample of schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. 'School closure' (i.e. treatment) refers to the closure of one of the two schools in the municipality; while as controls we employ municipalities with one or two schools not experiencing closures. The instrument is a dummy variable (equation 1.4) taking value one from the year of threshold introduction if at least one school was below the regional threshold according to 2010 characteristics. All specifications include controls for other school endowments, municipality and LLM-year fixed effects.

Table A1.11: Municipalities by distance to LLM centres and next available school

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

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

Table A1.12: School closure effect by municipality location (all single-primary-school municipalities in regions with threshold)

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

Standard errors clustered at municipal level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Second stage results from equation 1.5, where dependent variables are school-age population, potential parents and total income. School closure instrumented with dummy IV. In Panel a we subdivide our sample by distance to LLM centres and separately estimate equation 1.5 for municipalities above (far) or below (close) the median distance from LLM centres. In Panel b, we follow an analogous procedure, considering instead distance from the closest public primary school. Sample of all municipalities with one primary school in regions with threshold.

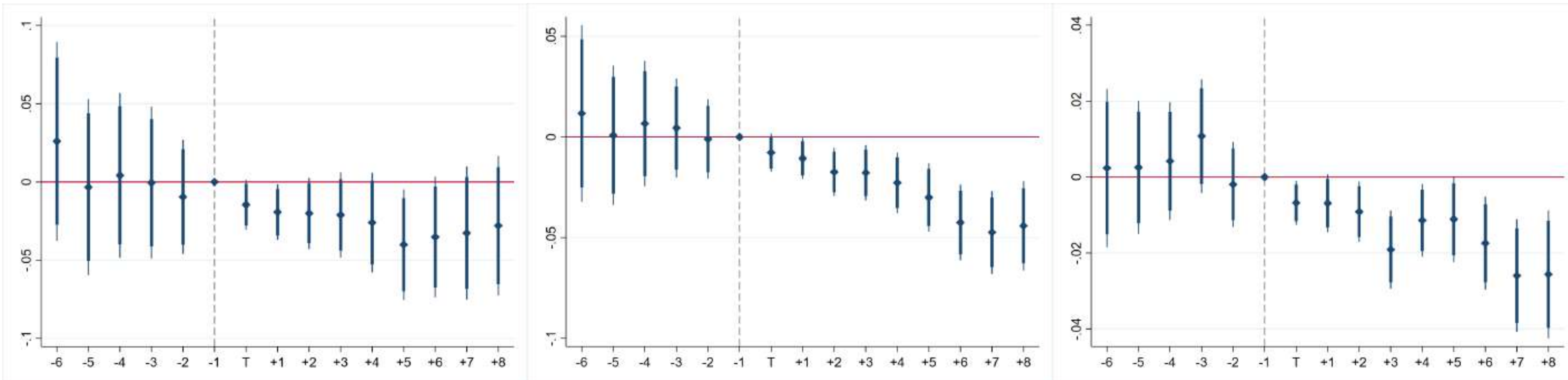
Table A1.13: School closure effect by municipality location, interaction term

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

Standard errors clustered at municipal level in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Second stage results from equation 1.5, where dependent variables are school-age population, potential parents and total income. In Panel a we add to the specification of equation 1.5 the interaction between school closure and an indicator taking value one if the municipality is above the median distance from LLM centre. In Panel b we follow an analogous procedure, considering instead distance from the closest public primary school. School closure instrumented with dummy IV; interaction term instrumented with dummy IV × *far*. Full sample of single-primary schools in regions with threshold (Tfs) in columns 1, 3, 5; sample of schools ± 50 students from regional thresholds (±50) in columns 2, 4, 6.

Figure A1.11: Event study plots of the reduced-form estimation: municipalities far from LLM

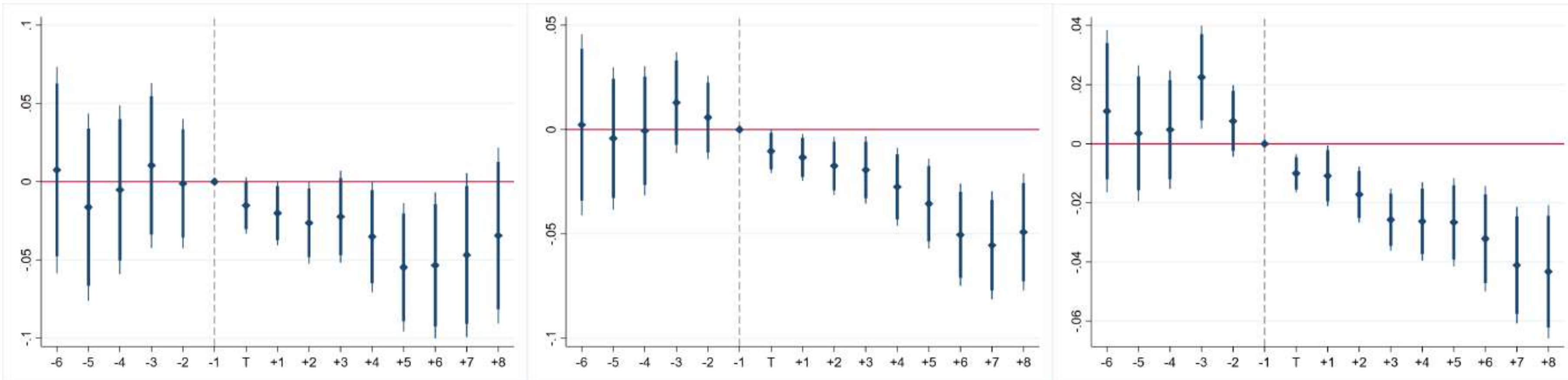
a. School age population b. Potential parents c. Total income



The Figure shows the event study plots corresponding to the reduced form of equation 1.3. Dependent variables: (log) school-age population (panel a), (log) potential parents population (panel b), total income (panel c). Those outcome variables are regressed on leads and lags of the Dummy IV. The sample is composed of single-primary-school municipalities of regions with school-sizing thresholds above the median distance from Local Labour Market centres. Sample further restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Figure A1.12: Event study plots of the reduced-form estimation: municipalities far from next primary school

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



The Figure shows the event study plots corresponding to the reduced form of equation 1.3. Dependent variables: (log) school-age population (panel a), (log) potential parents population (panel b), total income (panel c). Those outcome variables are regressed on leads and lags of the Dummy IV. The sample is composed of single-primary-school municipalities of regions with school-sizing thresholds above the median distance from the next available primary school. Sample further restricted to schools with up to 50 students above or below the regional threshold as of s.y. 2009/2010. Thicker confidence intervals refer to 90% level, thinner ones to 95%.

Chapter 2

Knowledge economy, internal migration, and the effect on local labour markets

Authors: Agar Brugiavini, Marco Di Cataldo, Giulia Romani

Abstract

Knowledge-intensive activities may generate significant multiplicative effects at the local level. In particular, inflows of workers in knowledge-related sectors may contribute to make local labour markets more attractive for other kind of workers as well. This paper assesses how the employment growth and inflow of workers in knowledge-intensive sectors affect wage, employment, and probability of outmigration of local workers in other sectors. We focus on Italy during the 2005-2019 period, taking advantage of matched employer-employee social-security data, which allows to track workers' histories across jobs and locations. To address the identification concerns of sorting and idiosyncratic shocks, we implement a two-step procedure combined with a shift-share IV strategy. We separately identify the contribution of sorting and spillovers to labour market outcomes. Our results suggest that the employment growth and inflow of workers in knowledge-intensive sectors have multiplicative effects on employment, increasing the number of days worked by local workers, and they also seem to reduce the probability of outmigration. Nominal wages of local workers seem unaffected, while house prices increase producing a negative effect on local real wages.

2.1 Introduction

The emergence of the so-called 'knowledge economy' has led to a territorial concentration of human capital-intensive activities. This tendency of knowledge-intensive sectors to cluster in space can generate significant multiplicative effects at the local level (Moretti, 2010b), as highly-innovative industries require more intermediate services, pay higher wages, and generate larger productivity spillovers (Moretti, 2012; Peri, 2002; De La Roca and Puga, 2017; Duranton and Puga, 2001). In turn, these concentration patterns may significantly affect local labour market conditions and drive the location decision of workers. Because of these processes of human capital concentration, the rise of the knowledge economy has been regarded as a determinant of spatial inequalities within countries (Rosés and Wolf, 2018, Moretti, 2012, OECD, 2019).

This paper investigates how the progressive increase (or decline) in knowledge-intensive activities in some areas can impact the local conditions of workers in those territories. We evaluate the effect of labour demand shocks in the form of a growth of workers operating in knowledge-related sectors (henceforth labelled as 'knowledge workers') on the rest of the local economy. We also disentangle the effect's component due to the internal migrations of knowledge workers, as these labour flows represent endogenous supply shocks which may produce their own effects on local labour markets (Card, 2009; Ottaviano and Peri, 2010; Anelli et al., 2020). We observe the local response to these changes in terms of wage and employment, and outmigration probability of local workers.

We focus on Italy during the period 2005-2019, and resort to an extremely rich administrative individual-level dataset containing matched employer-employee information on the working histories of all social-security-paying Italian workers. In addition, since 2005, we have information about the workplace, which allows us to track workers across localities and occupations.

Our work speaks to different literature streams. First, it relates to the studies looking at the impact of labour demand shocks, and in particular their local multiplier effects (Bartik, 1991; Blanchard and Katz, 1992; Moretti, 2010a,b). This literature has focused on shocks in specific sectors, such as biotech and energy (Allcott and Keniston, 2017, Marchand, 2012, Moretti and Wilson, 2014), or it has devoted attention to the local impact of public spending (Acconcia et al., 2014, Faggio and Overman, 2014, Suárez Serrato and Wingender, 2016) and trade shocks (Dix-Carneiro and Kovak, 2019). However, no one has so far investigated the local impact of employment growth in knowledge-related activities. Second, by investigating the effect of employment growth in knowledge-intensive sectors, we also build on the literature on the social returns to education, demonstrating the existence of productivity spillovers accruing from educated workers to the advantage of other local workers and firms (Moretti, 2004a and 2004b, Peri et al., 2015). Lastly, this work also connects to the literature on the labour market impacts of migrations, which has traditionally focused on cross-countries labour flows (Card, 2001 and 2009, Ottaviano and Peri, 2010, Anelli et al., 2020) while devoting relatively less attention to the effects of *internal* migrations, the focus of our paper.[1]

No study has systematically assessed the local impact of the rise of the knowledge economy in the way we do in this paper. Most studies in the local multipliers literature focus on manufacture (Moretti, 2010b, Moretti and Thulin, 2013) or macro-sector shocks (Allcott and Keniston, 2017, Marchand, 2012, Acconcia et al., 2014). Here we take a different perspective, selecting the industries of interest based on knowledge-intensity, *across* macro-sectors. Moreover, most works have thus far focused on the US context. Analysing a different setting can help to better understand the role played by institutional factors in labour market dynamics. Indeed, the local impact of labour demand changes crucially depends on specific institutional features, such as wage-setting mechanisms, unemployment rates, and labour mobility (Moretti and Thulin, 2013; Ottaviano and Peri, 2010; Faggio and Overman, 2014). One close contribution to ours is the work by Serafinelli (2019), who focuses on the Italian

[1] Relevant exceptions in this respect are the contributions by Bound and Holzer (2000) and Cadena and Kovak (2016), framing migrations as supply-side responses to labour demand shocks as we do in the present work.

region of Veneto and addresses the local spillovers due to workers job mobility within LLMs. Instead, we consider the whole country and look at cross-sector spillovers. Moreover, we also disentangle the contribution to local spillovers of cross-LLM migrations of knowledge workers. This can highlight a competitive dynamics among LLMs, where those with a high concentration of knowledge-intensive activities drain qualified human resources from others, imposing them a '*negative* local multiplier'. Last but not least, the richness of our data allows us to develop an estimation strategy mitigating some empirical concerns generally encountered by previous studies. In particular, we can employ fixed effects specifications, which enable us to separately identify sorting and spillover effects.

In this work we face two major identification issues. First, workers may sort across local labour markets according to some unobservable characteristics (e.g. ability). As a consequence, it is possible that local workers display better labour outcomes in areas with greater increases in knowledge jobs because they are inherently better workers. Second, idiosyncratic shocks at the local level can correlate with our treatment, thus generating a downward or upward bias, depending on whether they are labour supply or demand shocks. To address those concerns, we combine a two-step procedure *á la* Combes et al. (2008) with a shift-share instrumental variable strategy (Bartik, 1991). In the 1-st step we estimate wage, employment, and outmigration probability of local workers, accounting for observable time-varying worker and firm-level characteristics as well as time-invariant individual unobservables, while in the 2nd-step we regress the predicted local labour market area-year characteristics on our treatment variables - the stock and inflow of knowledge workers - instrumenting them to account for idiosyncratic local labour market shocks. In doing so, we capture the wage, employment, and outmigration probability premium derived from an increase in the stock or the inflow of knowledge workers.

Our results suggest that knowledge-workers are highly geographically concentrated and display relevant multiplicative effects at the local level. Specifically, looking at the employment in local sectors, we find evidence of a multiplier effect, arising both from the employment growth and inflow of workers in knowledge-intensive sectors. Moreover, we observe a significant reduction in outmigration probability, signalling an increased attractiveness of the local economy, again determined both by the share variation in knowledge workers and by their net migration rate. Conversely, we do not find any significant effect on wages, which highlights their limited responsiveness to local labour market conditions in the Italian context. Nominal wages appear to be positively affected only when we do not account for workers sorting. This suggests that the rise of the knowledge economy fosters the self-selection of more productive workers into 'knowledge-intensive' areas, thus only *indirectly* affecting nominal wages. In fact, local prices seem more reactive than nominal wages, resulting in a negative impact on real ones. Sorting plays a role also for the outmigration probability of local workers. The rise of the knowledge economy seems to increase the self-selection of more mobile workers into knowledge-intensive areas, resulting in an insignificant effect of stock and inflow of workers in knowledge-intensive sectors on the outmigration probability of local workers when one does not account for sorting.

The effects on the local economy produced by an inflow of knowledge workers appear smaller in size than those of an increase in their stock, consistent with our prior that migration response is just a component of the overall adjustment process.

The paper is structured as follows. Sections 2 and 3 respectively review the relevant literature and motivate the setting choice; sections 4 and 5 present the data employed and some descriptive statistics; section 6 and 7 explains the empirical strategy adopted and reports the related results; section 8 investigates the role of sorting (versus spillovers); section 9 compares the effects on nominal and real wages; section 10 concludes.

2.2 Related literature and predictions

2.2.1 Literature review

This work mainly relates to three streams of the literature. First of all, it speaks to the contributions concerning local multipliers and, more generally, the local impacts of sector-specific labour demand shocks. The notion of local multipliers has been popularised by Moretti (2010a,b), even if the analysis of the local impacts of productivity shocks has a longer history, stemming from the contributions of Bartik (1991) and Blanchard and Katz (1992) which have recently been resumed for methodological purposes. Thereafter, a rich literature has developed on the local price adjustments and employment effects of local labour demand shocks, focusing mainly on specific sectors.

Among others, Marchand (2012) and Allcott and Keniston (2017) investigate the local impact of booms and busts in the energy sector, testing the hypothesis of positive spillovers to other local industries against that of manufacturing crowding out (the 'Dutch disease'); while Walker (2013) analyses the transitional dynamics produced by regulatory shocks, both on the workers of the interested sector and on other workers within the same LLM. Moretti and Wilson (2014) address the effects of state subsidies to the biotech sector, looking both at labour outcomes within biotech and at employment responses in other local sectors. Lee and Clarke (2019) and Lee (2014) respectively investigate the labour market impact of employment growth in high-tech or creative industries in UK, focusing on wage and employment responses of workers in other local sectors. Looking at trade shocks, Dix-Carneiro and Kovak (2019) investigate the impact of trade liberalisation in Brazil on earnings, employment, and migration responses of both tradable and non-tradable workers.

The studies on local multiplier effects have also devoted attention to the local impact of public spending. Acconcia et al. (2014) estimate the static and dynamic multiplier of sharp fiscal contractions, exploiting national rules to contrast Mafia in Italy, while Suárez Serrato and Wingender (2016) propose an analogous exercise and causally identify the impact of local multipliers through variations in accounting methods at censuses. Faggio and Overman (2014), instead, investigate the impact of public sector employment growth on private sector employment and other labour market indicators.

This literature investigates different margins of adjustments to labour demand shocks. However, the common feature of all the above references is that they address the *indirect* effect of labour demand shocks in a given sector, namely the consequences for the rest of the local economy. Institutional factors play a key role in determining the sign and size of the multiplier. Specifically, any cultural or regulatory/institutional aspect influencing the elasticity of labour supply affects the magnitude the impact and can make the difference between a multiplier or crowding out effect (Faggio and Overman, 2014; Moretti and Thulin, 2013). As shown by Moretti and Thulin (2013), estimating the local multiplier of tradables in Sweden and comparing it with the US benchmark, the size of local multipliers may be context-dependent.[2] This *per se* motivates any study focusing on novel institutional settings.

The second strand of the literature this work relates to is the set of works on social returns to education. By investigating the effect of employment growth in knowledge-intensive sectors, we build on previous contributions examining productivity spillovers accruing from educated workers to the advantage of other local workers and firms. Moretti (2004a) looks at workers' wages and disentangles the effect of productivity spillovers versus imperfect substitution of working in cities with different percentages of graduates, while Moretti (2004b) focuses on productivity of manufacturing plants and investigates the human capital externalities of higher graduate presence in the local economy outside the firm. Similarly, the literature on

[2] Furthermore, the What Works Centre for Local Economic Growth in its toolkit on multiplier effects compares the magnitude of estimated multipliers in various OECD countries (available at: https://whatworksgrowth.org/resource-library/toolkit-local-multipliers/). Accounting for differences due to estimation methods, it seems evident that institutional factors play an important role in determining such variability in estimates. In that report, Auricchio (2015) focusing on Italy, finds evidence of a multiplier for high-tech industries which is smaller compared to other OECD countries, while no significant effect for generic tradable-industries, confiriming previous findings by De Blasio and Menon (2011). In sum, in Italy multiplicative effects on employment seem smaller than in other countries.

learning externalities in cities explains agglomeration processes in terms of better learning opportunities in large urban areas, due to knowledge spillovers. These opportunities are especially attractive for young workers investing in their human capital (Peri, 2002; De La Roca and Puga, 2017) and for newly-created innovative firms (Duranton and Puga, 2001), which are indeed over-represented in cities.

Lastly, as our aim is to study the impact of knowledge workers brought by internal migrations, this work also links with the literature on the labour market impacts of migrations. This has traditionally focused on cross-country labour flows, mainly conceived as labour *supply* shocks. Moreover, most papers dealing with developed countries have focused on *in*-migrations (e.g. Card, 2001, 2009).[3] Considering *out*-migrations, instead, a study by Anelli et al. (2020) focuses on Italy and finds that outflows decrease place of origin's labour demand more than labour supply, due to the positive selection of outmigrants among highly innovative potential people. This result questions the traditional approach of modelling outmigration as a reduction of labour supply and highlights the existence of a demand channel driven by *brain drain* outflows. Despite international outmigration being an important part of the brain drain picture in countries like Italy, brain drain can also occur *within* national boundaries, generating a divergent geography of human capital resources. We focus on this aspect in our analysis. Among the few studies that analyse internal migrations, it is worth mentioning Bound and Holzer (2000), Cadena and Kovak (2016) and Dix-Carneiro and Kovak (2019), who frame migrations as mobility responses to labour *demand* shocks.[4] In a similar fashion, our work aims to investigate the labour market impact of migration responses to labour demand shocks in the knowledge sector.

[3] For an overview of the literature on migrations, see Lewis and Peri (2015).

[4] Bound and Holzer (2000) look at a variety of workers demographic groups and uncover large wage effects of relative supply response to exogenous demand shocks. Cadena and Kovak (2016) find that the high mobility response of Mexican-born workers to demand shocks has a smoothing effect on labour market outcomes of US natives. Dix-Carneiro and Kovak (2019) investigate how negative demand shocks in the tradable sector transmit to non-tradable workers through reduced consumer demand for local services and workers competition for jobs in both sectors.

2.2.2 Theoretical predictions

From those streams of the literature, we draw some theoretical predictions on how wage and employment of workers in non-tradable sectors respond to the employment growth or inflows of workers in knowledge-intensive tradable sectors. In addition, we are also interested in observing the response in terms of probability of outmigration of workers in non-tradables.

The effects of increased exposition to qualified workers on local *wages* can materialise through two channels: (1) the imperfect substitutability in the production process of different types of workers, or (2) productivity spillovers (Moretti, 2004a). Here, we identify knowledge workers based on sector employment. Therefore, possible complementarities should act across sectors rather than across skill levels. At the same time, we select industries based on a knowledge-intensity criterion, so it is reasonable to expect knowledge workers to produce externalities enhancing productivity and wages of other sectors of the local economy. Hence, this framework predicts positive wage effects on other local workers, either deriving from complementarities between knowledge sector and other local industries, or from education externalities due to high average skill level in the knowledge sector.

Yet, this applies only to flexible wages, negotiated in an independent and decentralised manner and reacting to local labour market shocks, while wages determined through collective bargaining at the industry level should not be affected by changes in local labour demand. This is the case for Italy, where most employees are subject to national collective labour agreements (Belloc et al., 2019), implying that wages should not be particularly reactive to local labour conditions in our setting.[5]. In addition, local prices can respond to changes in labour market conditions. If local prices are more reactive than *nominal* wages, we could

[5] In the employer-employee INPS dataset we use there is also information on independent contractors and standard self-employed, which we plan to use in future work to compare differential responses depending on the wage-setting scheme. Using INPS data, Belloc et al. (2019) estimate the urban wage premia for employees vs self-employed and independent contractors, and find different results for the two categories. Collective bargaining imposes a downward constraint to wages, while leaving the possibility to the employer to raise wages above the level nationally negotiated (*in melius* clause). Therefore, in principle, there is no institutionally-binding upward constraint to wages and this makes it possible to observe wage increases in local labour markets experiencing higher growth in knowledge workers.

also have a negative impact on *real* ones.

In case of rigid wages, the literature predicts that any upward wage push will be transferred to *employment*. Ottaviano and Peri (2010), analysing the labour market impact of immigration in Germany, address the role of labour market institutions and, specifically, of wage rigidities. The authors show that, in the context of rigid wages, employment represents an important margin of adjustment to shocks.

However, the net employment effect may go in different directions. On the one hand, positive labour demand shocks can have a multiplier effect acting through increased demand for local services (Moretti, 2010b). On the other hand, if labour supply elasticity is very low, positive sector-specific shocks can displace workers from other local industries, therefore causing a decrease in employment in the rest of the local economy. As an example of the latter dynamic, Faggio and Overman (2014) estimate a crowding out effect of public sector growth on other tradable industries, attributing this to labour market rigidities (e.g. generous benefit system) and strict housing regulation that prevent labour supply to respond to positive local demand shocks.

While Italy is characterised by low labour mobility - which may suggest these crowding out effect may materialise in such context - in presence of involuntary unemployment not all the employment adjustment has to come from immigrants, but it can also derive from incumbent residents previously unemployed (Moretti, 2010a). Specifically, the less mobile the workers are, the more incumbent residents will benefit from positive local labour demand shocks (Bartik, 1991). Since unemployment is sizeable in Italy over the period observed, a possibility is that the employment response of incumbent residents outweighs the little inflow of immigrants, making the overall employment effect positive.

In sum, without knowing the size of labour supply elasticity, we cannot predict the sign of the employment response to an increase in the level of knowledge workers. With regards to the effect produced by an inflow of knowledge migrants, instead, we should not observe any displacement of non-tradable workers and therefore expect a positive employment impact for locals.

The above considerations also relate to our third outcome of interest, namely the probability of *outmigration* of local workers. According to the literature, negative demand shocks in the tradable sector transmit to non-tradable workers through reduced consumer demand for local services and increased workers' competition for jobs in both sectors (Dix-Carneiro and Kovak, 2019). Dealing with *positive* demand shocks, we expect both increased demand for local services and possibly reduced labour supply in case of relevant displacement effects of non-tradable workers. Therefore, we would predict the outmigration probability of local workers to decrease in response to positive demand shocks in the knowledge sector.

2.3 Knowledge economy and internal migration in Italy

Some key stylised facts make Italy a very interesting case for the issue at hand. Italy displays significant internal migrations (on top of international outflows), which mainly concern young qualified adults, directed towards big urban areas of the country (ISTAT, 2019). Indicative, in this regard, are the increased commuting flows around big urban centres of the North and the reduction in the number of LLMs between the last two censuses (ISTAT, 2010). From the 2000s, all Italian regions have experienced an increased international outflow of qualified workers ('brain drain'). However, if we consider both internal as well as international migration, some Italian regions come to display positive net inflows of young qualified population, to the detriment of the rest of the country. In other words, young qualified people, when not moving abroad, migrate towards the most dynamic and productive centres of the country, leaving territories of origin without valuable resources for local development.

That loss is a key aspect of emerging spatial disparities, which are not limited to the traditional North-South divide, but rather are visible at a more refined geographical scale, arising across the whole national territory. Nowadays, Italy seems characterised by a significant polarisation of population, opportunities, services and investments. That evidence has made policy-makers speak of an Italian 'territorial issue' (Borghi, 2017) and motivated policy efforts to reduce territorial disparities, such as the National Strategy for Inner Areas (MUVAL, 2014).

The issue of growing territorial inequalities within countries is actually a global trend, which is often associated with structural changes in the economy and, specifically, to the consolidation of the knowledge economy, which shows a remarkable tendency to cluster in space.[6] In this regard, the Italian institute of statistics (ISTAT) provides evidence of a catching up with European standards by some Italian regions and provinces, regarding - for example - R&D investments, brands registration, industrial design, employment in research and cultural activities (ISTAT, 2018). However, that improved performance in knowledge-related dimensions appears to be spatially concentrated, with a large part of the country lagging behind. In summary, Italy seems to display a specific geography of knowledge, with migrations and qualified opportunities deeply interconnected. Our hypothesis is that this channel can represent an important determinant of the emerging territorial disparities.

2.4 Data

The data used for the analysis are drawn from matched employer-employee datasets collected by the Italian National Institute of Social Security (INPS). We gained access to these data through the VisitINPS Scholars programme, which allows selected scholars to employ social-security data for research purposes.[7] These data contain information on the universe of social-security-paying Italian workers, employed in the private sector.[8] For those workers,

[6] In the European context, Rosés and Wolf (2018) offer a historical perspective on the evolution of territorial inequalities, showing a new rise from the '80s and mainly relating it to technological change. Similarly, for the US, Moretti (2012) speaks of 'great divergence' among areas of the country, driven precisely by the concentration of high-tech firms and qualified workers.

[7] For more information about the programme, visit https://www.inps.it/dati-ricerche-e-bilanci/attivita-di-ricerca/programma-visitinps-scholars.

[8] Note that self-employed workers and public servants are not included in the datasets we work on.

INPS data report the whole working history, tracking them across different occupations, employers and working locations up until the 1970s. However, geographical information on the employment municipality of each worker is available only from 2005. For this reason, our study focuses on the period 2005-2019.

We restrict our sample to workers aged between 15 and 64, not retired, for which we have information on employment sector and location. In addition, we drop from the sample individuals working less than 30 days per year and outliers in the two 1% tails of the wage distribution, after having computed the full-time equivalent wage for part-time workers. This is done to focus on 'average workers', discarding extreme and marginal working situations. For analogous considerations, we choose to select a yearly-dominant contract for each worker, identified as the employment providing the highest annual income and displaying the highest number of days worked. After that sample selection, we are left with over 100 million observations.

In addition to INPS data, we collect information on local house prices from the Italian Revenue Agency.[9] These data provide minimum, maximum, and average prices at sub-municipal level, that we then aggregate at LLM area. We employ this information as a proxy for local living costs, used to compute average real wages in the area.

[9] For more details, see https://www.agenziaentrate.gov.it/portale/schede/fabbricatiterreni/omi/banche-dati/quotazioni-immobiliari.

2.4.1 Knowledge workers

We identify 'knowledge workers' on the basis of their employment sector.[10] This choice is in line with the literature on local multipliers, which deals with the labour market consequences of *sector*-specific shocks (Moretti, 2010b; Marchand, 2012; Allcott and Keniston, 2017). Most importantly, it is consistent with the aim of estimating the labour market consequences of the knowledge *sector* growth.

To identify workers employed in knowledge-intensive sectors, we adopt the classification provided by EUROSTAT, which establishes a threshold of 33% of graduates workers on whole sector employment to qualify an activity as 'knowledge-intensive'.[11] That listing of sectors is based on the average number of employed people between 15 and 64 years old at aggregated EU-27 level in 2008 and 2009, according to the NACE Rev.2 at 2-digit, using the EU Labour Force Survey data.

To construct our knowledge workers variables in accordance with local multipliers literature, we focus on knowledge workers within *tradable* sectors. Following Moretti (2012), our aim is to investigate the impact of 'cause jobs' created in a given labour market on all other local workers.[12] In order to select tradable sectors, we follow Faggio and Overman (2014)

[10] An alternative choice would have been to select workers by looking at their occupation and/or education. Unfortunately, those variables report relevant percentages of missing information in INPS data. More importantly, the sample for which we have such information appears to be a non-random selection with respect to key worker characteristics. Also for that reason, we opt for an identification based on sectors. To provide some information on the skill composition inside and outside the knowledge sector, Table A2.1 in the Appendix shows the distribution of (non)knowledge-sector workers by education group and job position. Within the knowledge sector, the percentage of college-educated workers is twice as much as outside it, while high-schools dropouts are half of non-knowledge sector workers. Similarly, almost 70% of knowledge sector workers are white collars or managers compared to the 46% outside the knowledge sector. Moreover, Figure A2.1 plots the wage distributions of knowledge-sector and non-tradables workers. The former distribution is shifted to the right, with the right tail displaying considerably more weight. This suggests that, among knowledge-sector workers, a larger mass of individuals earns more than the average.

[11] For further details, see https://ec.europa.eu/eurostat/statistics-explained/index.php?title=Glossary:Knowledge_Intensive_Activity_(KIA) and related annex ec.europa.eu/eurostat/cache/metadata/Annexes/htec_esms_an8.pdf.

[12] The notion of 'cause jobs' refers to the fact that tradable industries derive a relevant part of their revenues from outside-LLM demand. In this sense, those job opportunities are generated by external factors and represent, in turn, possible causes for other jobs in the non-tradable industries at the local level ('consequence jobs').

and adopt the Jensen and Kletzer (2006) classification of tradable service sectors, together with its extension to industry activities provided by Hlatshwayo and Spence (2004). Jensen and Kletzer (2006) classify service activities according to their degree of tradability based on a locational Gini index. The assumption underlying such a criterion lies in the fact that sectors which serve a more widespread demand - therefore, tradable ones - happen to be more geographically concentrated.[13] Thus, they use spatial clustering as an indicator of that service being potentially traded nationally and internationally. Hlatshwayo and Spence (2004) build on that criterion to classify industrial sector by degree of tradability. Both contributions refer to US data (thus, to NAICS sector codes) and make the assumption that sector tradability stays constant over a few decades.[14] To adopt such classification with Italian data, we follow Faggio and Overman (2014) and map the 2-digit NAICS codes and industry description into our 4-digit ATECO codes, assuming that US sector technology applies to the Italian economy as well.[15]

Combining knowledge and tradability criteria, we identify 94 4-digit ATECO codes relating to tradable, knowledge-intensive sectors.[16] Hereafter, we simply refer to those sectors and related workers respectively as knowledge sectors and knowledge workers. These workers will form part of our treatment variables; namely, the percentage and inflow of knowledge workers in a given area.[17]

[13] Besides supported by empirical evidence, that assumption is theoretically demonstrated by the works of Helpman and Krugman (1985) and Krugman (1991).

[14] Such an assumption could be somehow restrictive for sectors benefiting from ICT revolution; however, most of these latter are already classified as tradable (Hlatshwayo and Spence, 2004).

[15] Specifically, we assume that sectors are as spatially concentrated in the US as in Italy, an assumption made also by Faggio and Overman (2014) for the UK.

[16] See a summarising table in Figure A2.2 and the full list of industries included in Table A1 of the Appendix.

[17] Note that if a tradable worker switches from a non- to a knowledge-intensive sector, he will contribute to treatment variables only for the years in which he is employed in knowledge-intensive sectors.

2.4.2 Locals and migrants

With the term 'locals' we refer to the workers employed in *non-tradable* sectors. These constitute the population which we expect to be affected by variations in knowledge employment or net inflows. By definition, non-tradable sectors mostly produce for a *local* demand, which makes them especially sensitive to local labour market (LLM) shocks. Some tradable activities can also be affected by sector-specific shocks, but part of the effect is likely to be transferred to other LLMs, so that the impact is expected to be milder (Moretti, 2010b). Local workers do not need to be permanently observed in the same LLM over 2005-2019, but they can migrate to other LLMs. We assume they can be affected by knowledge workers when they are based in an LLM experiencing an increase in stock or an inflow of knowledge workers.

We identify as knowledge migrants those workers who move into (or out of) a LLM from one year to another and are employed in the knowledge sector in the destination area. It is worth clarifying that we only refer to internal migrants, i.e. within-Italy migrants, since we do not observe re-locations across country borders. To locate workers, we assign them to the LLM where they work the highest number of days in a year (hereafter, 'dominant LLM'). In this way, we can compute net migration into an LLM by simply subtracting in and out-flows of workers over the period of interest.[18]

Given the period under observation, as LLM units we employ the 2011 definition of ISTAT's *Sistemi Locali del Lavoro*. That partition is elaborated from effective commuting flows at each Census year and represents areas where most people live and work. Therefore, they constitute the most accurate definition of local labour markets, characterised by homogeneous labour

[18] As explained, INPS data contain information on employment municipality, which can be easily associated to a given LLM. However, during the period considered, some Italian municipalities experienced mergers, which sometimes concern more LLMs. In this respect, we attribute the LLM of the new municipality to those merged before 2011, while for those merged after 2011 we assign to the new municipality the LLM of merged ones only if they were all part of the same LLM.

market conditions inside them.[19]

2.5 Descriptive analysis

In the 2005-2019 time span, the analysed period, knowledge workers represent around 10% of the Italian workforce, slightly increasing over the observation period (see Figure A2.3).[20] Looking at internal migrations (Figure A2.4), 8% of the whole working population migrates each year across local labour markets. A considerable fraction of them is below 40 years old ('young'). Instead, just one fourth is employed in sectors classified as knowledge intensive.[21] Interestingly, a great portion of knowledge migrants is employed in tradable sectors, which provides supportive evidence to the claim that geographical concentration mainly concerns tradable industries. In addition, the vast majority of knowledge migrants is young, which confirms the stylised facts presented in section 2.3.

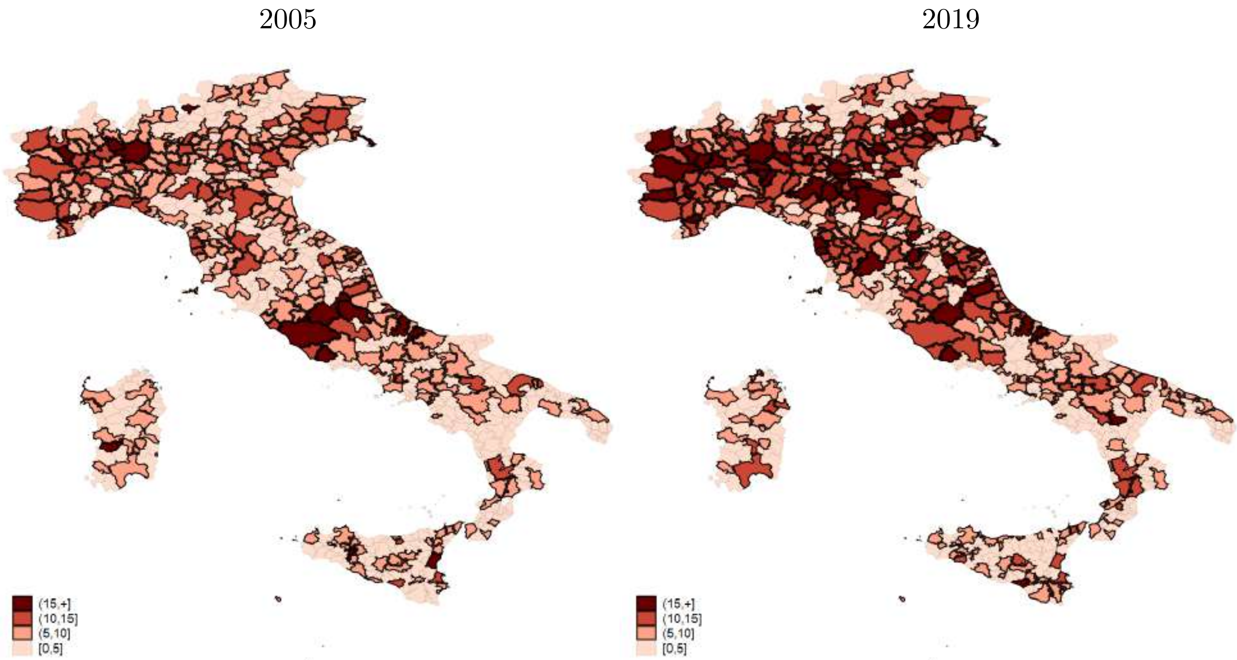
If we observe the spatial distribution of knowledge workers (Figure 2.1), we notice an overall increase over the period considered, with many LLMs reaching 15% or more of knowledge employment over the total. However, they seem to concentrate in space, mainly in the centre-North. Moreover, considerable variability exists within regions, with neighbouring LLMs displaying very different percentages.

[19] For further details on the construction of *Sistemi Locali del Lavoro*, see https://www.istat.it/en/labour-market-areas. We employ the 2011 definition, since that seems the most representative description of local labour markets in our period of analysis (2005-2019).

[20] As explained in the previous section, these are workers active in tradable and knowledge-intensive sectors. Workers employed in tradable sectors are around 40% of overall employment, on a decreasing trend due to the decline of manufacturing, while those in knowledge-intensive sectors are around 20% and show a slight upward trend in the last years of sample.

[21] While this may seem a relatively low percentage, a potential explanation is that we are not looking at individual education, but rather at employment sectors. Thus, a poorly educated workforce in the knowledge sector can partially motivate this finding, as well as the possibility for highly educated migrants to find qualified occupations in non-knowledge-intensive sectors.

Figure 2.1: %jobs in the knowledge sector

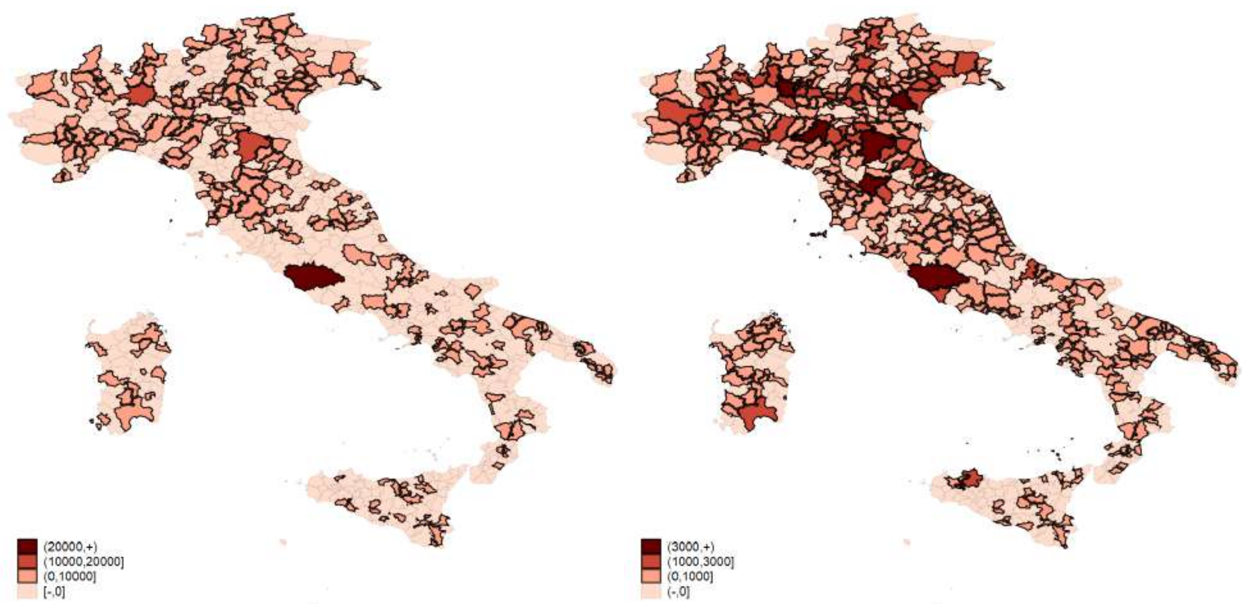


The maps show the percentages of workers employed in the knowledge sector by LLM, at the beginning (2005 - Panel a) and at the end (2019 - Panel b) of the period considered.

Similarly, looking at net migrations (Figure 2.2), we note that most of the country displays outflows of workers over the whole period observed. If we then focus on knowledge workers, more LLMs display positive net migrations, but a large part of them received less than 1000 knowledge migrants over 15 years. Here, some highly dynamic LLMs stand out for a high number of incoming workers, such as Rome, Bologna, Florence, and Padua. Moreover, in the islands, LLMs with regional administrative centres display rather high inflows, confirming that the dynamics are largely intra-regional.

Figure 2.2: 2005-2019 net migrations

(a) all (b) knowledge workers

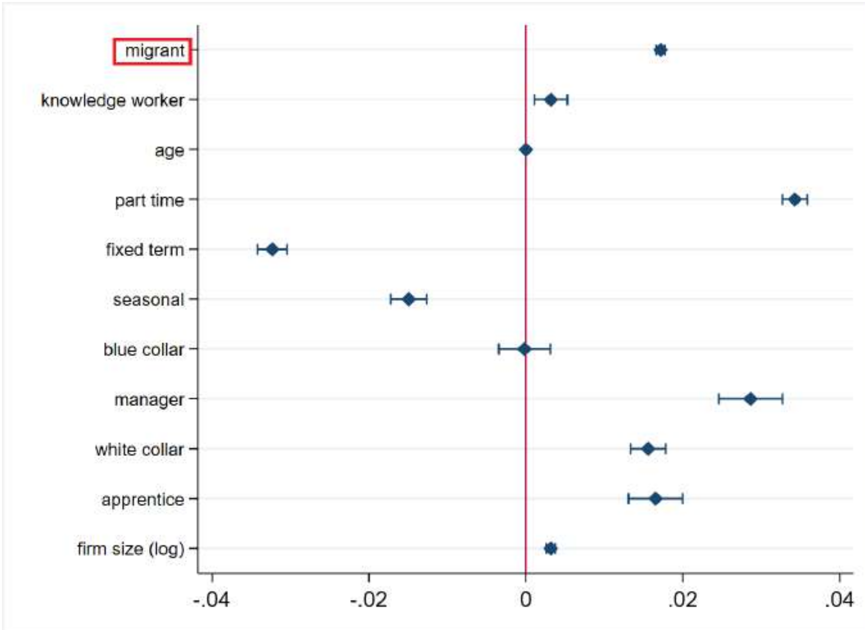


The maps plot the number of migrants received by each LLM over the span 2005-2019. Panel a refers to all workers, while Panel b focuses on workers of the knowledge sector.

As a descriptive investigation, we test whether these internal migrations are related to better labour opportunities in the destination area, that is if workers 'move to opportunities'. We regress individual (annual) wage growth on a set of worker characteristics and fixed effects (see Figure 2.3).[22] These estimates suggest that migrants experience a significantly higher increase in wages compared to stayers, which supports the claim that internal migrations are likely due to the search for better job opportunities.

[22] In Figure A2.5 of the Appendix, we do a similar exercise using as dependent variable the dummy for migration. In this way, we check which individual characteristics are associated with a higher probability of migrating. Working in the knowledge sector positively correlates with migration, even if the point estimate is sightly non significant. Temporal jobs are clearly associated with a higher likelihood of moving.

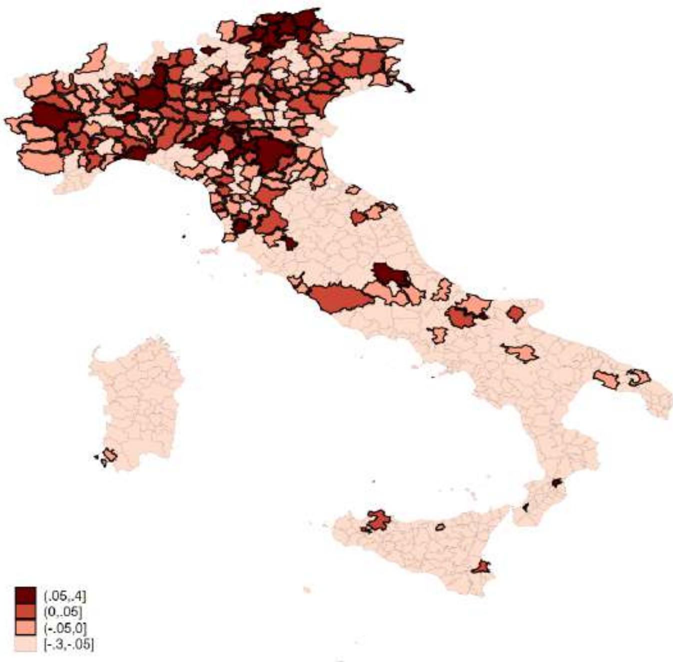
Figure 2.3: Expected wage growth by worker characteristics



The graph reports the estimated coefficients from a regression of annual wage growth on a set of individual characteristics (2005-2019). We also include in the specification individual and LLM-year fixed effects, and 2-digits sector fixed effects.

Focusing on the knowledge sector, we further check how the wage premium to work in those industries varies across space. The map in Figure 2.4 displays the local wage premia as of 2005, estimated from a regression of (log) individual wages on time fixed effects and the interactions between area and knowledge-sector dummies. The map reports estimates of these interaction terms, which can be interpreted as the wage premium to work in the knowledge sector in a given area in 2005. Most of the country displays negative wage premia, up to -30 log points. Conversely, positive premia are concentrated around Rome and, mostly, in the North, with the LLMs of Bologna, Milan, Turin, and the Bolzano province standing out for the highest wage premia (up to 40 log points).

Figure 2.4: Wage premium to work in the knowledge sector (2005)



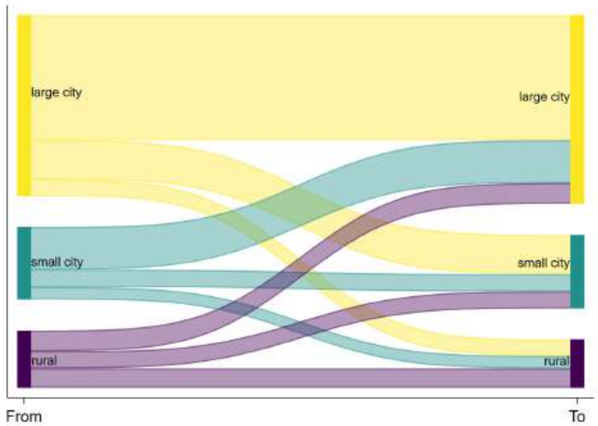
The map plots the estimated coefficient - for 2005 - from a regression of (log) wage on time fixed effects and the interaction between area and knowledge sector dummies. The plotted coefficient refers to that interaction term, which we interpret as the wage premium to work in the knowledge sector in a given area in 2005.

For an overview of mobility patterns, we also classify LLMs by initial population density and compute average origin-destination flows over 2005-2019. We do so either looking at overall migrations (Panel a of Figure 2.5) or focusing on migrants in the knowledge sectors (Panel b).[23] In both panels, the greater flows concerning large cities are likely due to a size effect, since these are the most densely populated areas. The interesting fact is that most migrants from those areas move to other large cities, so as the larger fraction of movers from small cities. Flows from large to small cities or rural areas are much more limited. This pattern is even more marked when looking at knowledge migrants (Panel b). For workers in the knowledge sector, large cities represent the most common destination of internal migration, while rural areas are mostly places of outmigration.

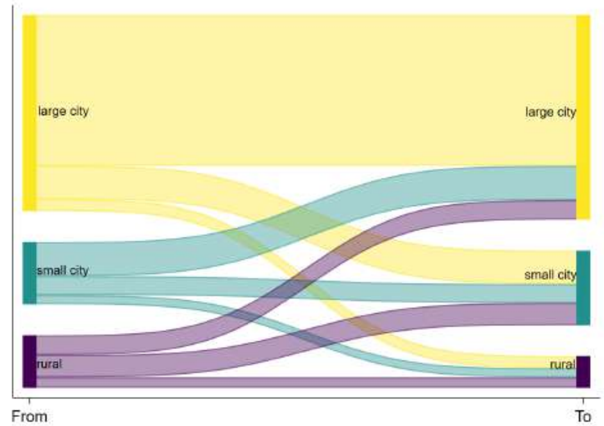
[23] Specifically, areas are classified as 'large city' if they belong to the 4th quartile of the population density distribution in 2006, 'small city' if to the 3th quartile, 'rural' if to the 1st or 2nd quartile.

Figure 2.5: Mobility patterns (average flows over 2005-2019)

(a) all migrants



(b) knowledge-sector migrants

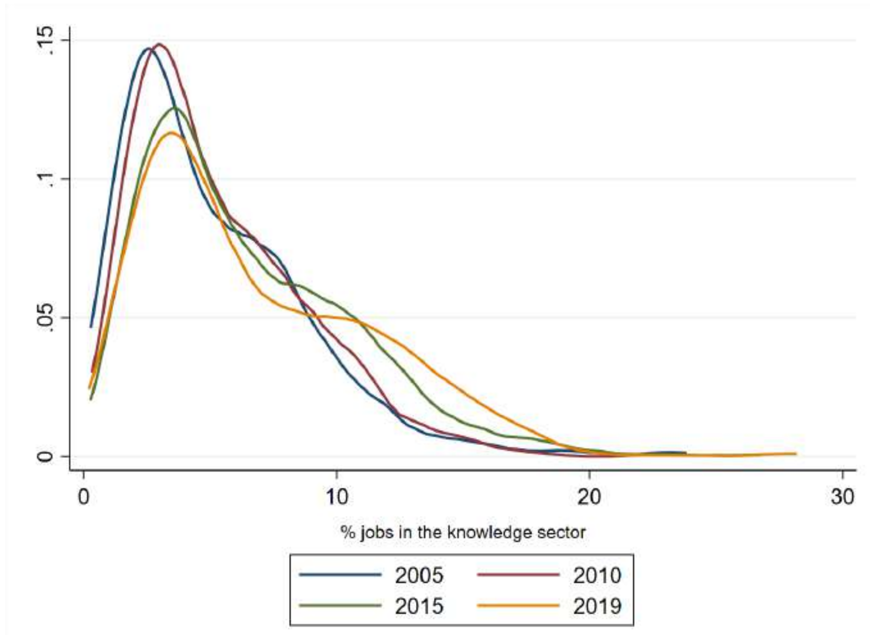


The graphs report average migration flows by pair of origin-destination LLMs, over the span 2005-2019. We distinguish LLMs by initial population density and classify them as 'large city', 'small city', and 'rural'. Panel a refers to all migrants, while Panel b focus on migrants within the knowledge sector.

To investigate the spatial dispersion of knowledge workers over time, in Figure 2.6 we plot kernel distributions of percentages of knowledge workers across LLMs in 2005, 2010, 2015, 2019. The distribution slightly shifts to the right, due to the overall increase in knowledge sector employment. Moreover, it becomes less peaked around the mean, with increased weight on the right tail of the distribution. This feature can be interpreted as some LLMs pulling ahead of the country average, which is consistent with our hypothesis of few LLMs benefiting from the rise of knowledge economy.

We also look at the kernel distributions of two of our key outcome variables, namely wage and employment of local workers. The plots in Figure 2.7 refer to the distribution of average (log) daily wage and days worked per year in local sectors across LLMs. We clearly see an overall increase in wages, up until 2015. As in the previous graph, since the 2010 the distribution flattens, showing increased variance in average wage across LLMs. Regarding days worked, there progressively emerge two peaks, around 170 and 230 days per year. Here the interpretation is less straightforward, but it could suggest a polarisation of LLMs between low and high work intensive.[24]

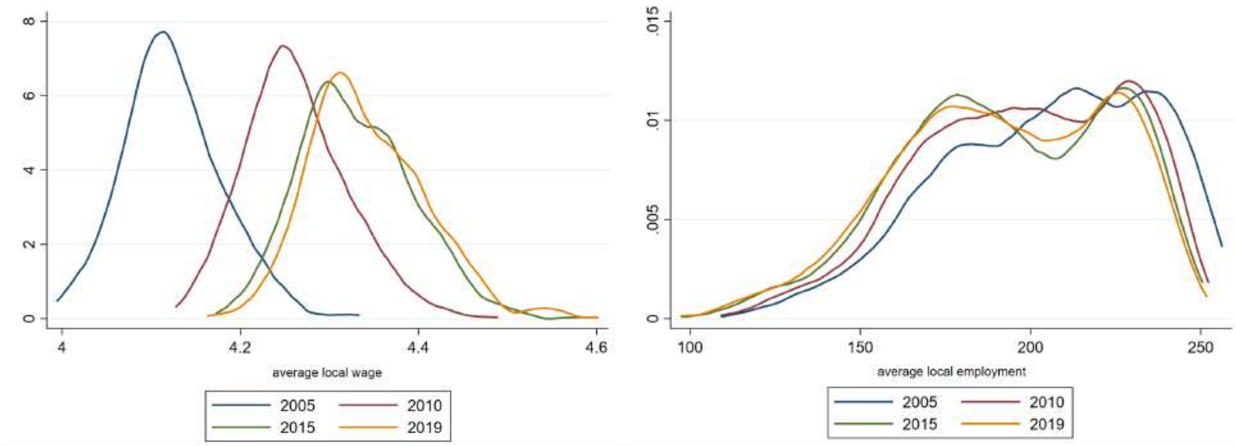
Figure 2.6: Dispersion of knowledge workers across LLMs



The graph reports the kernel distributions of the percentages of workers in the knowledge sector across LLMs in 2005, 2010, 2015, 2019.

[24] The two peaks observed could be driven by different working patterns across North and South Italy. We checked the geographical distribution of LLMs below or above 200 days worked in year 2019. Such number of days worked corresponds to the bottom between the two peaks observed. Among low intensive working areas (below 200), the largest amount is concentrated in the South (75%); while among high intensive working LLM (above 200), geographical distribution is more balanced (55% in the Centre-North and 45% in the South). Therefore, those peaks only partially reflect the traditional Italian North-South divide. Among low work intensive areas, there exists some geographical variation. Concerning high work intensive LLMs, they almost evenly distribute across North and South Italy.

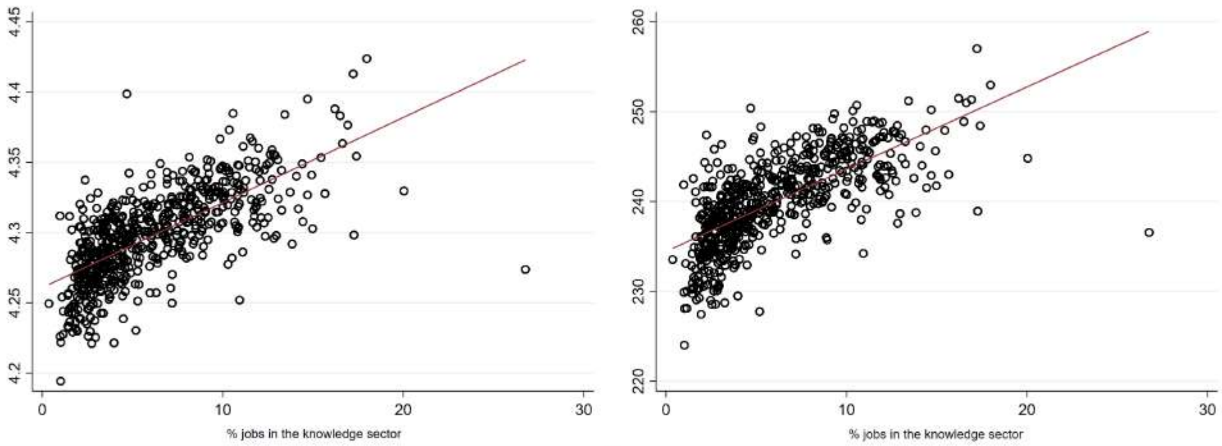
Figure 2.7: Dispersion of average wage and employment of local workers across LLMs



The graphs report the kernel distributions of average (log) daily wage (Panel a) and days worked (Panel b) across LLMs in 2005, 2010, 2015, 2019.

Finally, we provide some basic correlation analysis to start investigating the relation between percentages of knowledge workers and labour outcomes of local employees. To that aim, in Figure 2.8 we plot average daily wage and days worked at LLM level against percentages of knowledge workers. The average labour outcomes are computed from individual adjusted wages and days worked, predicted through a regression including sex, age, year of entrance in the labour market and a set of occupational dummies. In Figure 2.8, variables are expressed in levels and observations refer to a given LLM-year combination. Correlations are clearly positive, which provides preliminary evidence of a positive relationship between employment growth in the knowledge sector and labour outcomes.

Figure 2.8: Correlation between adjusted local wage/employment and % jobs in the knowledge sector



The graphs report the correlation between local adjusted daily wage (Panel a) and days worked (Panel b) at LLM level, and the percentages of knowledge workers. The adjusted labour outcomes are obtained through a regression including sex, age, year of entrance in the labour market and a set of occupational dummies.

2.6 Empirical strategy

Our aim is to identify the labour market impact of relative employment growth in the knowledge sector, either as a whole or deriving from net migrations of knowledge workers from other LLMs.

In estimating such relationships, we face two main identification issues. First, local workers may sort according to some unobservable characteristics (e.g. ability) in a way that is correlated with the presence of knowledge workers. In other words, it can be the case that local workers inherently display better labour outcomes in LLMs with increased percentages of knowledge workers. Second, unobserved idiosyncratic shocks to labour outcomes correlated with the local shares of knowledge workers may bias our estimates. Specifically, in case of unobserved local demand (supply) shocks, our estimates would be upward (downward) biased. To address these concerns, we combine a two-step estimation (Combes et al., 2008) with a shift-share instrument. The two-step estimation allows us to account for individual sorting; while the shift-share instrument mitigates possible concerns about unobserved idiosyncratic shocks at LLM level. More generally, the instrumental variable strategy should address any problem of time varying unobservables which influence local labour outcomes and correlate with the increased presence of knowledge workers.

2.6.1 Two-step model

First of all, we run an individual level estimation in which we regress (1) individual log daily wage, (2) log employment - proxied by the number of days worked per year - and (3) a dummy defining the (out)migrant status of a worker in the following year, on a set of worker and firm characteristics: indicators for whether the worker has a part-time, fixed-term, seasonal job, occupational dummies (white/blue collar, manager, apprentice), log firm size. We also include employment sector dummies (2-digit ateco), worker and LLM-year fixed effects.

Formally, in the 1st-step we estimate:

$$y_{it} = \alpha + \beta_1 X_{it} + \beta_2 Y_{j(it)t} + \gamma_i + \delta_{c(it)t} + \epsilon_{it}, \tag{2.1}$$

where y_{it} is the individual outcome of local worker i in year t; X_{it} and $Y_{j(it)t}$ are, respectively, time-varying worker and firm characteristics, with j relating to the firm where worker i is employed at time t; γ_i are worker fixed effects and $\delta_{c(it)t}$ are LLM-time fixed effects, with c referring to the LLM where individual i works in year t.

The fitted values of the term $\delta_{c(it)t}$ can be interpreted as the labour outcome premium to work in LLM c in year t (Combes et al., 2008, 2010). These become the dependent variable of the 2nd-step estimation, a LLM-level regression. In this equation, the main explanatory variable is one of our two $KW_{ct} \in \{KW1_{ct}, KW2_{ct}\}$ treatments of interest: (1) the percentage of workers within the knowledge sector in a given LLM, or (2) the net inflow of knowledge workers. We also add year and LLM fixed effects, and include analytical weights for the number of observations contributing to the 1st-step estimation, in order to control for the different precision in area-year estimates. We apply that correction to deal with the sampling errors possibly contained in the area-year estimates employed as dependent variable in the 2nd-step (Combes et al., 2008). "This weighted two-step procedure gives rise to estimates that are consistent although asymptotically less efficient than optimally weighted two-step estimates, which are numerically equal to one-step estimates. The two-step procedure yields standard errors that account for the grouped structure of the data" (Moretti, 2004a). Moreover, both in the 1st and in the 2nd-step estimation (equations 2.1 and 2.2), we cluster standard errors at the local level, i.e. LLM.

Our model is estimated in long-differences form, where variations refer to the whole 2005-2019 period, using observations for 2005 and 2019:

Formally, the 2nd-step equation is:

$$\hat{\delta}_{ct} = \zeta + \eta\, KW_{ct} + \theta_t + \lambda_c + \phi_{ct} \qquad t = 05, 19. \tag{2.2}$$

This two-step estimation allows us to control for a wide range of individual characteristics which can influence labour outcomes and, more importantly, to clean out unobserved individual heterogeneity through worker fixed effects. In this way, we avoid that our coefficient of interest η is biased by ability sorting. Compared to a one-step estimation, this specification allows to include - in the 1-st step - LLM-time fixed effects and, therefore, separately identify the effects of individual versus area time-varying characteristics. These area-year effects, indeed, are our outcomes of interest when it comes to estimate treatment effects at

LLM level.[25]

In the 2nd-step, we include year fixed effects to control for any possible business cycle dynamics influencing labour market outcomes. Moreover, since our aim is to estimate the effects of knowledge-employment relative *growth*, we add LLM fixed effects, in order to first differentiate regression variables. By doing so, η only captures the labour market impact of *variations* in our treatments, which are specified either as the percentage of knowledge workers over the total workforce in that LLM-year or as the net (cumulative) inflows of knowledge migrants from to 2005 up to year t, discounted by the 2005 number of knowledge workers in that LLM.[26] More formally, our treatments are defined as follows:

$$KW1_{ct} = \frac{K_{ct}}{N_{ct}} \cdot 100 \tag{2.4}$$

$$KW2_{ct} = \frac{\Sigma_{05}^{t}\, m_{ct}}{K_{c,05}} \cdot 100 \tag{2.5}$$

where K_{ct} and N_{ct} are, respectively, the total number of knowledge and overall workers in

[25] The two-step approach is discussed in details by Combes et al. (2008) and Combes et al. (2010). Other applications are also Mion and Naticchioni (2009) and Belloc et al. (2019). Working with large samples, this procedure improves computational tractability, compared to a one-step individual estimation with LLM fe. Moreover, it allows to include *time-varying* area effects and therefore avoids to estimate the LLM fixed effects only from movers, which represent a highly selected sample of the population. Finally, working at LLM level in the 2nd-step, we avoid the 'shock bias' (Combes et al., 2010) deriving from non-zero covariance between the treatment and individual error term. As a robustness check, we also estimate the above specification in one step. Formally:

$$y_{it} = \alpha + \beta_0\, KW_{ct} + \beta_1 X_{it} + \beta_2 Y_{j(it)t} + \gamma_i + \delta_{c(it)} + \eta_t + \epsilon_{it}. \tag{2.3}$$

One-step estimates - available upon request - substantially confirm our main results. Wage and employment coefficients are equally signed and significant compared to two-step estimates. As for outmigration, point estimates are almost identical to two-step coefficients, but standard errors are larger, providing insignificant estimates. This is likely due to the considerable reduction in sample size resulting from the inclusion of LLM fixed effects (in the one-step estimation, we just rely on movers). More importantly, in the two-step estimation we compute standard errors of *means*, i.e. area-year estimates; while here we deal with the original individual outcome, which displays larger variance.

[26] We borrow that specification for net inflows from Anelli et al. (2020). Discounting for the initial number of knowledge workers in the LLM serves to account for the different sizes of the sector among LLMs at the beginning of the period.

LLM c, year t; m_{ct} is the net inflow of knowledge migrants to LLM c in year t and $K_{c,2005}$ is the total number of knowledge workers in LLM c in 2005. Note that for LLMs with greater outflows than inflows, $KW2_{ct}$ will have negative sign.

Therefore, when employing treatment $KW1_{ct}$, we can interpret η as the percentage variation in the outcome - daily wage, days worked, or outmigration probability premium - due to a 1% increase in employment in the knowledge sector. When, instead, we use $KW2_{ct}$, η can be read as the percentage outcome variation deriving from a 1% increase in incoming knowledge workers with respect to their initial presence.

2.6.2 Shift-share instrumental variable strategy

Equation 2.2 does not account for the possibility that labour market shocks at local level could bias our coefficients. In other words, there could be time varying unobservables that influence local labour outcomes and correlate with the treatment, creating a problem of omitted variables. To correct for this, we employ a shift-share (Bartik, 1991) instrumental variable strategy, following the implementation in first-differences adopted by Moretti (2004a). The purpose of this IV strategy is to isolate the exogenous shift in the demand for labour in the knowledge sector. Essentially, we construct our instrument interacting historical local shares of each 4-digit sector code in our knowledge classification with the overall percentage of that specific sector at national level in the period considered. Formally,

$$Instrument_{ct} = \Sigma_s \, w^s_{c,95} \cdot \%S_t, \tag{2.6}$$

where

$$w^s_{c,95} = \frac{S_{c,95}}{N_{c,95}} \cdot 100 \tag{2.7}$$

are the historical shares of LLM c employment in industry s, and

$$\%S_t = \frac{S_t}{N_t} \cdot 100 \tag{2.8}$$

is the overall share of employment in sector s at national level, over the period observed. Therefore, we can interpret the instrument as a LLM-specific weighted average of national changes in the employment shares of knowledge industries. We take historical shares at 1995, ten years before the beginning of our period of analysis.[27]. This is done to mitigate the potential concerns about shares exogeneity: shares are themselves LM equilibrium outcomes and therefore can correlate with fundamentals directly related to subsequent LLM outcomes (Jaeger et al., 2018, Goldsmith-Pinkham et al., 2020).

In fact, the literature has developed two alternative approaches to the validity of shift-share instruments, either based on the exogeneity of the shares or shift component of the instrument. According to the 'shares approach' (Goldsmith-Pinkham et al., 2020), Bartik-type instruments mainly derive identification from differing initial industry composition across LLMs, which result in differential exposures to common shocks. Alternatively, the instrument isolates the shift in local labour demand only coming from national changes, provided that neither past industrial composition nor unobservables correlated with it directly predict the outcome of interest conditional on controls (Baum-Snow and Ferreira, 2015). Instead, following the 'shift approach' (Borusyak et al., 2018), shares exogeneity is not a necessary condition for the identification of causal effects. It is sufficient that shares are not correlated with the differential changes associated with the national shock itself. This approach perfectly applies to settings characterised by quasi-experimental exogenous shocks (e.g. Autor et al., 2013, Peri et al., 2015). However, it can still be appropriate when the researcher can "conceive an underlying set of shocks that, if observed, would be a useful instrument" (Bartik, 1991; Blanchard and Katz, 1992). While we do not exploit quasi-experimental shocks,

[27] Note that, to construct the historical shares, we had to refer to the municipality where the employer was located, since before 2005 we do not have data on individuals' workplace location.

we can still imagine exogenous variation in industry-specific productivity within the knowledge sector, deriving from the global technological change. Since we cannot directly observe those aggregate demand changes, we have to estimate them from national employment variations, introducing mechanical bias. Borusyak et al. (2018) asymptotic result is that "if one is willing to assume quasi-random assignment of the underlying industry demand shocks and that the regional supply shocks are spatially-uncorrelated, one can interpret the uncorrected [shift-share] estimator as leveraging demand variation in large samples".[28]

We believe that - in our setting - the orthogonality condition required by the 'shares approach' seems more plausible. Indeed, we employ initial shares specific to knowledge industries as instrument for equally specific treatment (i.e. only concerning the knowledge sector), which makes quite unlikely that unobservable industry shocks enter the error term in a way that is correlated with past shares.[29] Moreover, as already said, we take initial shares ten years before the measurement of any other variable in the estimation. If, instead, we took the 'shift perspective', for the asymptotic validity result to apply we would need to assume that regional supply shocks are spatially uncorrelated or, alternatively, to employ split sample methods, as those estimating shocks from distant regions. Since this is not straightforward in our context, we stick to the 'shares approach' and related identifying assumption, which appears reasonably plausible in our setting.

We use the instrument in equation 2.6 for both our treatments. $KW1$ is the employment-growth treatment commonly used in the local multipliers literature. $KW2$ can be viewed as the component of $KW1$ due to internal migrations. In other words, $KW2$ is itself a margin of adjustment to the demand shock underlying $KW1$, which can thus produce its own labour

[28] With the term 'uncorrected', Borusyak et al. (2018) refer to the Leave-Out-Option (LOO) correction. In that contribution, the authors show that in large samples that correction is irrelevant, since the risk that a single unit is driving national employment changes is negligible. Here, since we have a high number of geographical units in our sample, i.e. 611 LLM_s, we do not apply any LOO correction.

[29] In the words of Borusyak et al. (2018), "share exogeneity may be a more plausible approach in the case when the exposure shares are 'tailored' to the specific economic question, and to the particular endogenous variable included in the model. In this case, the scenario considered [..] that there are unobserved shocks which enter [the error term] through the shares may be less of a concern."

market effects. Therefore, in principle the same exclusion restriction should hold in both cases.[30]

Shift-share diagnostics

Our instrument combines the cross-sectional variation in past industry shares as of 1995 ('the share' component) with the industry employment growth at the national level over 2005-2019 ('the shift' component). If one takes the 'share' approach to instrument exogeneity, the validity of the IV relies on the assumption "that neither past industrial composition nor unobservables correlated with it directly predict the outcome of interest conditional on controls" (Goldsmith-Pinkham et al., 2020). To test that assumption, Goldsmith-Pinkham et al. (2020) propose some diagnostic tests. Firstly, they suggest to compute the weights that the instrument attributes to each share (the so-called Rotemberg weights): higher weight means greater importance in the identifying variation. Then, for the five industries receiving highest weight, they advise to check the correlation between past shares and pre-treatment characteristics of the local labour market.

We follow these suggestions and run the above tests for our treatment $KW1$, i.e. the variation in the percentage of knowledge workers. Table A2.3 report few diagnostics on Rotemberg weights.[31] Panel A shows the shares of Rotemberg weights (α_s) that are positive and negative. Almost all of them are positive meaning that individual shares positively correlates with the IV. This suggests that our instrument is a convex combination of the industry-specific estimated β coefficients and does not show signs of mis-specification. Panel B reports correlations among the components of the IV ($\%S$ and $w_{s,95}$ - see equation 2.6), the Rotemberg weights (α_s), the power of the IV (F_s) and the estimated coefficients of equation 2.2 with - respectively - wage (Panel B.1), employment (Panel B.2), and outmigration probability

[30] In Bound and Holzer (2000), the authors also apply the same Bartik instrument both to the overall demand shock and to the induced supply shift, of which they investigate the wage effect.

[31] To obtain these results, we employ the stata command bartik_weight by Goldsmith-Pinkham. See https://github.com/paulgp/bartik-weight.

(Panel B.3) as dependent variable. From Panel B, we can verify that we are mostly leveraging variation from the share component of the instrument. Indeed, Rotemberg weights (α_s) show a higher correlation with the shares ($w_{s,95}$) rather than with the shifts ($\%S$): 0.376 versus 0.215. A larger correlation implies a higher relevance of that IV component in generating the identifying variation. Finally, Panel C reports the five industries receiving highest weight with related industry-specific estimates of equation 2.2, respectively using wage (Panel C.1), employment (Panel C.2), or outmigration probability (Panel C.3) as dependent variable. Firstly, notice that none of the top-5 Rotemberg weight industries generates more than 26.3% (Reinsurance) of the total instrument variation. This is reassuring, since it implies that we are not relying only on few industry-specific variations for our estimation. Moreover, the related β_s estimates are mostly equally signed, similar in size and largely comparable to the estimated coefficients of the main analysis.[32]

We have verified that - in our setting - identifying variation mostly derives from the share component of the IV. Therefore, it is important to check that past industry shares are not correlated with LLM characteristics prior to the treatment period. Significant correlations would cast doubts on the exogeneity of past industry shares. Unfortunately, we cannot observe the evolution of our outcomes before 2005, since we do not have information on individual workplace prior to that date. For this reason, we cannot check for parallel trends before 2005, nor investigate the correlation of past industry shares with LLM outcomes over pre-treatment periods. However, we can observe whether past industry shares (as for 1995) correlates with changes in LLM characteristics over the years preceding our period of analysis. We consider past industry shares of the top 5 Rotemberg weight industries. Then, we regress those shares on the 2001-2004 variation in the number of local firms, the births of new firms, and the growth rate in local employees. Table A2.4 reports the related results. Essentially no coefficient appears significant, providing some confidence about shares

[32] These just-identified coefficients must be considered with some caution, since it was not possible to define the weak instrument robust confidence intervals using the method from Chernozhukov and Hansen (2008).

exogeneity. Specifically, 1995 industry shares seem not to predict the local changes in labour market conditions over the three years preceding the period of interest.[33]

2.6.3 Spatial clustering

Our units of analysis are the Italian *Sistemi Locali del Lavoro*, which represent the most accurate measure of LLMs. Their definition is based on actual commuting flows, which mostly occur within their boundaries.[34] However, some residual commuting takes place also across LLM borders: in 2011, an average 6% of the LLM population used to commute across LLM borders for work.[35] Despite cross-LLM commuting concerns only a small fraction of the population, we could be overestimating internal migrations, since we count as migrants individuals that start working in neighbouring LLMs without changing residence. Moreover, cross-LLM commuters (changing job or not) are likely to spend their income in the LLM of residence more than in the LLM they work in. Therefore, they mostly contribute to labour outcomes of the LLM of residence. In addition, the rise of the knowledge economy in one LLM can spill-over neighbouring areas, in the form of multiplier or displacement effects. All this can generate spatial correlation in the standard errors of neighbouring LLMs.

To check that spatial correlation is not driving our results, as a robustness we cluster standard errors using the method proposed by Conley (1999). This procedure does not restrict the choice about the level of clustering to administrative boundaries; conversely, it allows to define buffers of different radius around a given point, and use them for spatial clustering. As a reference point, we take the centroid of the LLM, and we re-estimate our long-difference IV specification (equation 2.2 with KW_{ct} instrumented with the shift share in 2.6) clustering

[33] The number of observations is reduced to 604 because we are excluding the 7 LLMs that in 1995 had no knowledge worker, as suggested in the replication files for the bartik-weight command. In the main estimation, for those LLMs the shift-share instrument takes value zero. However, our main estimates are not affected by the exclusion of those LLMs.

[34] For more details on the definition of *Sistemi Locali del Lavoro*, see the data section.

[35] We compute this statistics using data on cross-LLM commuting flows in 2011 provided by ISTAT through the application BTFlussi (https://gisportal.istat.it/bt.flussi/).

standard errors at buffer-level. We tried with buffers of radius 10, 20, and 30 kilometres.[36] We chose these distances considering the median land area of LLMs, which is slightly below 400 square kilometres.[37]

2.7 Results

2.7.1 OLS estimates

We start by reporting the results of the individual-level estimations (equation 2.1), where our outcomes are regressed on a set of worker and firm characteristics. Among regressors, we include: indicators for whether the worker has a part-time, fixed-term, seasonal job; occupational dummies (blue/white collar, manager, apprentice); and log firm size. Moreover, we add fixed effects for employment sector (2-digit ateco), together with worker and LLM-year fixed effects. Table 2.1 shows the correlation of these characteristics with log daily wage, log days worked, and outmigration probability of local workers.

[36] This is done with the STATA command acreg, which allows to specify the geographical coordinates of a given point and set the distance cutoff in kilometres beyond which the correlation between error term of two observations is assumed to be zero.

[37] Assuming a circle surface, the corresponding radius is around 11 kilometres. Thus, a cutoff distance of 10 kilometres is almost equivalent to cluster at LLM level, for LLMs of median size; while with 30 kilometres, we are including in the clustering the entire neighbouring LLMs (assuming again median size LLMs).

Table 2.1: Individual level estimation

	Wage	Employment	Outmigration
Part time job	0.040***	-0.384***	-0.005***
	(0.0027)	(0.0029)	(0.0006)
Fixed term job	-0.043***	-0.369***	0.028***
	(0.0030)	(0.0030)	(0.0017)
Seasonal job	-0.011***	-0.409***	0.038***
	(0.0029)	(0.0070)	(0.0013)
Blue collar	-0.019**	-0.027***	0.004
	(0.0063)	(0.0041)	(0.0033)
White collar	0.020***	0.028***	-0.005*
	(0.0034)	(0.0040)	(0.0030)
Manager	0.202***	0.007**	-0.013***
	(0.0082)	(0.0043)	(0.0028)
Apprentice	-0.178***	-0.074***	-0.007*
	(0.0062)	(0.0049)	(0.0032)
Firm's size	0.011***	0.034***	-0.0003*
	(0.0002)	(0.0017)	(0.0002)
2-digit sector fe	✓	✓	✓
individual fe	✓	✓	✓
LLM-year fe	✓	✓	✓
N	103,350,759	103,350,759	103,350,759

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports estimated coefficients from the 1st-step regression at the individual level (equation 2.1), where we regress (log) wage, (log) employment and outmigration probability on a set of time-varying individual characteristics and fixed effects. The sample includes all local workers.

Then, we move on to the results from LLM-level estimations, employing as dependent variables the LLM-year fixed effects estimated from equation 2.1.

We begin by presenting the OLS results of equation 2.2, where KW_{ct} corresponds either to the share of knowledge workers in the LLM or to net migrations of knowledge workers (see Table 2.2). We estimate this equation in long-differences, employing observations in the initial (2005) and final (2019) period and including LLMs fixed effects to first-differentiate regression variables.

Table 2.2 reports - for both our treatments - positive wage effects, while no significant impact is detected on employment and outmigration probability. However, these results may be biased, since they do not account for the likely idiosyncratic shocks to labour outcomes correlated with the treatment variables. To address this empirical concern, we refer to the IV estimation, where both treatments are instrumented by the shift-share measure of equation 2.6.

Table 2.2: OLS estimation: long-difference results (2005 - 2019)

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	0.003***	-0.002	-0.0001			
	(0.0013)	(0.0010)	(0.0003)			
% knowledge migrants				0.00003**	-0.00001	-0.000001
				(0.00001)	(0.00001)	(0.00001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the estimated coefficients from the OLS regression of the 2nd-step specification (equation 2.2). The outcome variables are the area-year effects predicted from equation 2.1. We regress these area-year estimates for 2005 and 2019 on our treatment variables (equation 2.4) or 2.5), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

2.7.2 IV estimates

In Table 2.3 we report estimates for the first stage regressions, where we regress both treatments on the shift-share instrument. The F-statistics is above the conventional level of 10 for both estimations, showing the instrument's relevance. To provide a visual intuition of the predictive power of the instrument, in the Appendix (Figures A2.6 and A2.7), we report the maps for actual and predicted percentages of knowledge workers by LLM, respectively in 2005 and 2019.

Table 2.3: IV estimation: first stage results (long-differences)

	% knowledge workers	% knowledge migrants
Bartik instrument	2.090***	24.125***
	(0.5938)	(4.7083)
year fe	✓	✓
LLM fe	✓	✓
F test	12.38	26.26
N	1,222	1,222

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the coefficients for the first stage estimations in which we separately regress our treatment variables (equation 2.4 or 2.5) in 2005 and 2019 on the shift-share measure in equation 2.6, together with area and year fixed effects.

In Table 2.4, we report the second stage results of the instrumented versions of equation 2.2. Both Tables 2.3 and 2.4 refer to long-differences estimations, over the entire period 2005-2019. In Tables from A2.5 to A2.7 of the Appendix, we repeat the estimation clustering standard errors as suggested by Conley (1999). In this way, we verify that spatial correlation among neighbouring LLMs is not driving our results. Our main estimates are robust to different levels of spatial clustering, with buffers around the LLM's centroid of radius from 10 to 30 kilometres.

Comparing OLS to IV estimates, we can notice an increase in the employment coefficient and a decrease in the outmigration one, both becoming significant. We interpret these shifts in coefficients as evidence of a labour supply bias in the OLS estimates, which can be explained in terms of better amenities in areas with higher presence and/or inflow of knowledge workers. It is plausible that places growing in knowledge employment also improve on life quality, in the form of cultural initiatives and better services; a dynamics consistent with the theory of endogenous amenities described by Diamond (2016). Conversely, estimates of the impact of knowledge workers on wage premia return insignificant coefficients, differently from the positive ones of OLS estimates. This change is likely to derive from the definition of the outcome variable. Daily wage is the ratio between yearly labour income and days worked, which is also our proxy for employment. Therefore, these findings suggest that the positive effect on wages in the OLS estimation was only due to a downward bias in our measure for employment.

In sum, increases in the stock and inflow of knowledge workers determine a multiplicative effects on local employment, and a decrease in outmigration probability, whereas wages do not respond to these changes. Looking at the coefficients' size, an increase of 10 in the share of knowledge workers in the area rises local employment by 6% and reduces outmigration probability by 2%. The effect of knowledge migrations is smaller in magnitude, but significant and equally signed. These findings confirm our prior that migration responses are only one component of the overall effect of the rise in knowledge employment. Moreover, our results are consistent with the theoretical prediction of decreased outmigration, and support the claim of a local multiplier effect of knowledge employment, which - in our context - seems to dominate possible displacement effects. The insignificant effect on wages, instead, is quite interesting, considering that there is no institutional upward constraint to wages.

Looking for international comparisons, the What Works Centre for Local Economic Growth has published a toolkit on multiplier effects where they summarise empirical results obtained for various OECD countries.[38] The toolkit confirms that larger multiplier effects are observed for tradable industries with higher technological content (1.88 multiplier in high-tech *versus* 0.9 in generic tradable industries). Moreover, the report quotes Auricchio (2015) focusing on Italy, who finds a 0.7 increase in non-tradable jobs for a unit increase in high-tech tradable industries. Such figure, largely comparable to our findings, is considerably lower that the 1.88

[38] The toolkit is publicly available at https://whatworksgrowth.org/resource-library/toolkit-local-multipliers/

average.[39] Furthermore, Auricchio (2015) does not find any significant effect for employment growth in generic tradable-industries, which confirms previous findings by De Blasio and Menon (2011). In other words, in Italy multiplicative effects on employment are smaller than in other countries. This plausibly relates to institutional factors, such as labour mobility and wage setting mechanisms, and thus to the dynamics on sorting and nominal *versus* real wages that we explore more deeply in the following sections.

Table 2.4: IV estimation: second stage results (long-differences)

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	-0.009	0.006**	-0.002***			
	(0.0069)	(0.0031)	(0.0008)			
% knowledge migrants				-0.001	0.001**	-0.0002**
				(0.0005)	(0.0003)	(0.0001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the coefficients for the first stage estimations in which we separately regress our treatment variables (equation 2.4 or 2.5) in 2005 and 2019 on the shift-share measure in equation 2.6, together with area and year fixed effects.Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are the area-year effects predicted from equation 2.1. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

2.7.3 The role of sorting

So far, we have treated sorting as an identification issue for our analysis. Not accounting for individual unobserved heterogeneity can bias the results if more productive workers self-

[39] Our analysis is in relative terms. We find a 6% increase in non-tradable employment for a 10 points increase in the percentage of knowledge workers in the area. This result is on the intensive margin of employment, and the classifications of high-tech/knowledge workers may not perfectly overlap; still the magnitude of the effect seems very similar.

select into areas with higher presence or inflow of knowledge workers (Combes et al., 2008). However, it is worthy to investigate the relative contribution of sorting and cross-sector spillovers to the whole effect on labour outcomes. It could be the case that local workers increasingly sort in areas where the knowledge sector is growing more. A similar patter would be part of the overall dynamics we aim to describe.

Therefore, we re-estimate the 1st-step regressions (equation 2.1) without individual fixed effects, and run the instrumented 2nd-step estimation employing as dependent variables the newly-computed area-year effects. Table 2.5 reports the second stage results of the IV estimation not accounting for sorting. The coefficient on employment is comparable to the one obtained with the inclusion of individual fixed effects. However, here we find a positive effect on wage which was absent in our main results (see Table 2.4). Moreover, the outmigration estimate appears insignificant when we do not account for individual sorting. These findings point to a self-selection of more productive and more mobile local workers into areas characterised by an increased presence of knowledge workers. Alternatively, workers who are intrinsically more likely to migrate and to earn higher wages increasingly concentrate into 'knowledge-intensive' areas. This is consistent with an overall positive dynamics induced by the knowledge sector growth: it generates new labour opportunities, making the labour market more prosperous and dynamic, and thus more appealing to workers with higher expected wages and propensity to move.

Table 2.5: IV estimation: not accounting for sorting (long-differences)

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	0.008***	0.008***	-0.0010			
	(0.0015)	(0.0019)	(0.0009)			
% knowledge migrants				0.001***	0.001***	-0.0001
				(0.0002)	(0.0002)	(0.0001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered at LLM level * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are the area-year effects predicted from equation 2.1,*without* individual fixed effects. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

2.7.4 Nominal vs real wages

In the main analysis we focus on nominal wages. In this section, we aim to observe the impact on real living conditions in the area, and thus we investigate also the effect on real wages. If the employment growth in the knowledge sector increases the local cost of living, we could even observe a negative effect on real wages. This is particularly likely in our setting since *nominal* wages seem not to respond to the positive shock once we account for individual sorting.

To proxy for the cost of living at local level, we employ average house prices in the LLM.[40] Firstly, we employ house prices as a further dependent variable, to check the impact on the cost of living of employment growth in the knowledge sector. Secondly, we use them as a

[40] Original data on house prices are provided at sub-municipal level. We aggregate them at LLM level, taking the average of minimum, maximum, and average prices in the area. Within a given LLM, there can be significant variation in house prices, mostly due to amenity differentials. However, an individual working in the area can choose where to reside inside the LLM depending on her willingness to pay for amenities. Therefore, more or less variance in house prices within the LLM makes little difference for real wage analysis, and we can rely on average prices.

discounting factor for the area-year effects estimated in the 1st-step regression (equation 2.1) for wages. Since both the area-year effects and house prices are expressed in logs, we take the difference between those variables. We interpret that difference as real wage premium to work in the area.[41] We run all these estimations at the LLM level and instrument our treatments with the usual shift-share instrument. Table 2.6 reports IV estimates for regressions using as dependent variable minimum, maximum, and average (log) house prices in the LLM. For all these outcomes, we see a positive effect of employment growth in the knowledge sector. Alternatively, areas attracting more knowledge workers become more expensive. This is consistent with an increased demand to reside in these areas, due to more and better labour opportunities locally available. Then, we consider real wages as the difference between area-year estimates for (log) nominal wages and the (log) average house prices, and employ such difference as dependent variable. Table 2.7 displays the related results. We find a negative impact on real wages of employment growth in the knowledge sector: the cost of living increases, while nominal wages do not adjust accordingly.

[41] For the properties of logarithms,

$$ln(\text{nominal wage}_{ct}) - ln(\text{house price}_{ct}) = ln\left(\frac{\text{nominal wage}_{ct}}{\text{house price}_{ct}}\right). \tag{2.9}$$

Table 2.6: IV estimation: impact on house prices (long-differences)

	Local housing prices					
	Minimum	Maximum	Average	Minimum	Maximum	Average
% knowledge workers	0.026**	0.016*	0.020**			
	(0.0106)	(0.0085)	(0.0088)			
% knowledge migrants				0.002**	0.001	0.002**
				(0.0010)	(0.0008)	(0.0008)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,132	1,132	1,132	1,132	1,132	1,132

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are minimum, maximum, and average house prices at the local level. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects.

This pattern is consistent with previous evidence on the responsiveness of nominal and real wages in the Italian context. Belloc et al. (2019), for example, estimate the urban wage premium for Italy using INPS data. In nominal terms, they do not find any significant premium; while, in real terms, the premium is actually negative.[42] These findings suggest that in the Italian context local prices are more reactive than nominal wages to local shocks. Therefore, real wage variations are negatively correlated with positive local demand shocks.

To provide some international comparisons, Moretti (2004a) and Peri et al. (2015) focus on the US context and find a positive wage effect of an increased supply of, respectively, college educated and STEM workers at local level. Looking at sector-specific shocks, Dix-Carneiro and Kovak (2019) highlight a negative wage impact of trade liberalisation on non-tradable

[42] Belloc et al. (2019) employ a Consumer Price Index (CPI) which accounts for housing and non-housing living costs. However, house prices are among the main drivers of the spatial variation in the local cost of living. Moreover, according to the theoretical framework proposed by Rosen-Roback, in equilibrium any shock to the local demand or supply of labour is fully capitalised into house prices (Roback, 1982). Therefore, we just focus on housing price indexes to compute real wages.

Table 2.7: IV estimation: impact on real wage (long-differences)

	Real wage	
% knowledge workers	-0.029**	
	(0.0107)	
% knowledge migrants		-0.003**
		(0.0009)
year fe	✓	✓
LLM fe	✓	✓
N	1,132	1,132

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variable is the area-year effect predicted from equation 2.1 referring to wage, discounted by local housing prices. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

workers in Brazil. Again, Marchand (2012) find a positive wage impact of energy booms for non-energy workers in Canada. Looking at the effect of immigration on natives wages in Germany, Ottaviano and Peri (2010) highlight that labour market rigidities influence the extent to which shocks translate into wage or employment effects. Not all these works account for the role of sorting, possibly overestimating wage responsiveness to local shocks. Still, it seems that in other institutional settings wages are more reactive to local conditions than in Italy. National-level wage bargaining could partially explain the non-significant effect on wages. However, the Italian wage-setting mechanism does not impose any upward limit to wage determination. In fact, when we do not account for unobserved individual heterogeneity, we find a positive impact on nominal wages. Such an evidence suggests that nominal wages have some upward flexibility, but this goes to attracting inherently better workers to the local labour market, and does not translate into a proper wage effect. In other words, the positive labour demand shock creates better labour opportunities which are filled by more productive workers, so that we do not observe an increase in nominal wages when accounting for individual fixed effects.

2.8 Concluding remarks

In this paper we investigate the effect of employment growth in knowledge-intensive sectors on the labour outcomes of local workers. Specifically, we look at wage, employment and outmigration probability of other workers in non-tradable sectors. We separately investigate the impact due to the change in the percentage of knowledge-sector employment, and to inflows of workers in this sector. In this way, we disentangle the effect's component attributable to internal migrations of knowledge-sector workers. Moreover, thanks to the richness of our panel data, we are able to distinguish the role played by individual sorting from that of local spillovers. We study the Italian context, between 2005 and 2019. Our analysis provides a number of results.

First of all, we find no effect on *nominal* wage induced by the increase in employment in knowledge-intensive industries. This is consistent with the expectation of wages being not particularly reactive to local labour conditions, in a context characterised by industry-level national bargaining. Instead, living costs (proxied by house prices) positively respond to the employment growth in the knowledge sector, resulting in a negative impact on local *real* wages. Secondly, we find evidence of multiplicative employment effects, which implies that displacement of non-tradable workers from their sectors is not a dominant force in this context. Conversely, our results are consistent with the hypothesis of a local multiplier effect, which acts through an increase in the demand for local services. Thirdly, the evidence of decreased outmigration probability is perfectly in line with the theoretical predictions of an increased labour demand in non-tradable sectors induced by the growth in knowledge workers, thus reducing incentives to migrate. In other words, the positive demand shock brought by the rise of the knowledge economy increases the attractiveness of the LLM, and fosters agglomeration processes. The reduction in real wages may partly counterbalance that overall positive dynamics. Specifically, if wages were more flexible – and thus adjusted to rising living costs -, the impact in terms of internal migrations would probably be even larger.

Those findings hold for both our treatments, the percentage variation in knowledge workers and their net migration rate. The effects are smaller in size in the case of knowledge workers migrations, consistent with our prior of migration being only a component of the overall adjustment process to labour demand shocks.

The above results are cleaned by the confounding dynamics of workers sorting based on unobservables. However, this latter mechanism is part of the overall process that we aim to describe. Comparing estimates that do or do not account for unobserved individual heterogeneity, we can identify the role of sorting in labour market changes. We detect a self-selection of more productive and more mobile workers in areas with an increased presence of knowledge workers. This is consistent with an overall positive dynamics induced by the knowledge sector growth: it generates new labour opportunities, making the labour market more prosperous and dynamic, and thus more appealing to workers with higher expected wages and propensity to move.

If we combine those results with the evidence of a rise in the knowledge economy which is not uniform across space, we get a picture that resembles 'the great divergence' process described by Moretti (2012). *Some* LLMs benefit from the technological change, attract qualified workers and experience positive multiplicative effects in other local sectors. The rest of the country, instead, lags behind, losing human capital and suffering from negative circular dynamics at the local level. Therefore, our findings seem to support the claim that the uneven growth of the knowledge economy with its related internal migrations contributes to spatial inequalities. We do not investigate whether these dynamics generate aggregate gains at National level or they are simply a zero sum game among local labour markets. In either case, enlarging spatial inequalities can represent a relevant policy issue. For example, some workers may face mobility constraints preventing them from relocating closer to economic opportunities. Alternatively, some people may have strong idiosyncratic preferences for living in areas 'left behind' by the economic change, and be forced to move by the lack of qualified labour demand in those places. This internal 'brain drain' contributes to the decline of these areas, leaving untapped their economic potential. Finally, it is not obvious that more dynamic areas are prepared to host workers re-locating from places with few job opportunities. These internal migrations - if not properly addressed by policy makers - can lead to congestion and worsened living conditions in the destination areas. Assuming that decision makers care about territorial disparities, our results entail relevant policy implications. If we wish to contain regional divergence, public intervention should be directed to mitigate the economic disadvantages of left-behind places and their inhabitants, who cannot or will not move. This can imply different and complementary policies, ranging from the promotion of alternative sector-specialisation to facilitating the spreading of the benefits of the knowledge economy to less attractive areas.

Bibliography

Acconcia, A., Corsetti, G., and Simonelli, S. (2014). Mafia and public spending: Evidence on the fiscal multiplier from a quasi-experiment. *American Economic Review*, 104(7):2185–2209.

Allcott, H. and Keniston, D. (2017). Dutch Disease or Agglomeration? The Local Economic Effects of Natural Resource Booms in Modern America. *The Review of Economic Studies*, 85(2):695–731.

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

Auricchio, M. (2015). Local Manufacturing Multiplier and Human Capital in Italian Local Labor Markets? *PhD thesis, Libera Universita Internazionale degli Studi Sociali Guido Carli.*

Autor, D. H., Dorn, D., and Hanson, G. H. (2013). The china syndrome: Local labor market effects of import competition in the united states. *American Economic Review*, 103(6):2121–68.

Bartik, T. J. (1991). Who benefits from state and local economic development policies? *W. E. Upjohn Institute for Employment Research.*

Baum-Snow, N. and Ferreira, F. (2015). Chapter 1 - causal inference in urban and regional economics. In Duranton, G., Henderson, J. V., and Strange, W. C., editors, *Handbook of*

Regional and Urban Economics, volume 5 of *Handbook of Regional and Urban Economics*, pages 3–68. Elsevier.

Belloc, M., Naticchioni, P., Naticchioni, P., and Vittori, C. (2019). Urban wage premia, cost of living, and collective bargaining. *CESifo Working Paper No. 7253, Available at SSRN: https://ssrn.com/abstract=3275427*. Spatial Dimensions of Labor Markets.

Blanchard, O. J. and Katz, L. F. (1992). Regional evolutions. *Brookings Papers on Economic Activity*, pages l–75.

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

Borusyak, K., Hull, P., and Jaravel, X. (2018). Quasi-experimental shift-share research designs. Working Paper 24997, National Bureau of Economic Research.

Bound, J. and Holzer, H. J. (2000). Demand shifts, population adjustments, and labor market outcomes during the 1980s. *Journal of Labor Economics*, 18(1):20–54.

Cadena, B. C. and Kovak, B. K. (2016). Immigrants equilibrate local labor markets: Evidence from the great recession. *American Economic Journal: Applied Economics*, 8(1):257–90.

Card, D. (2001). Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics*, 19(1):22–64.

Card, D. (2009). Immigration and inequality. *American Economic Review.*

Chernozhukov, V. and Hansen, C. (2008). The reduced form: A simple approach to inference with weak instruments. *Economics Letters*, 100(1).

Combes, P.-P., Duranton, G., and Gobillon, L. (2008). Spatial wage disparities: Sorting matters! *Journal of Urban Economics*, 63(2):723–742.

Combes, P.-P., Duranton, G., and Gobillon, L. (2010). The identification of agglomeration economies. *Journal of Economic Geography*, 11(2):253–266.

Conley, T. G. (1999). Gmm estimation with cross sectional dependence. *Journal of econometrics*, 92(1):1–45.

De Blasio, G. and Menon, C. (2011). Local effects of manufacturing employment growth in italy. *Giornale degli Economisti e Annali di Economia*, pages 101–112.

De La Roca, J. and Puga, D. (2017). Learning by working in big cities. *The Review of Economic Studies*, 84(1):106–142.

Diamond, R. (2016). The determinants and welfare implications of us workers' diverging location choices by skill: 1980-2000. *American Economic Review*, 106(3):479–524.

Dix-Carneiro, R. and Kovak, B. K. (2019). Margins of labor market adjustment to trade. *Journal of International Economics*, 117:125–142.

Duranton, G. and Puga, D. (2001). Nursery cities: Urban diversity, process innovation, and the life cycle of products. *American Economic Review*, 91(5):1454–1477.

Faggio, G. and Overman, H. (2014). The effect of public sector employment on local labour markets. *Journal of Urban Economics*, 79:91–107.

Goldsmith-Pinkham, P., Sorkin, I., and Swift, H. (2020). Bartik instruments: What, when, why, and how. *American Economic Review*, 110(8):2586–2624.

Helpman, E. and Krugman, P. R. (1985). *Market Structure and Foreign Trade: Increasing Returns, Imperfect Competition, and the International Economy.* Cambridge, MA: MIT Press.

Hlatshwayo, S. and Spence, M. (2004). Demand and defective growth patterns: The role of the tradable and non-tradable sectors in an open economy. *American Economic Review*, 104(5):272–77.

ISTAT (2010). Nota metodologica sistemi locali del lavoro. *Statistiche report.*

ISTAT (2018). Rapporto sulla conoscenza 2018. *Economia e Società.*

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

Jaeger, D. A., Ruist, J., and Stuhler, J. (2018). Shift-share instruments and the impact of immigration. Working Paper 24285, National Bureau of Economic Research.

Jensen, J. B. and Kletzer, L. G. (2006). *Undestanding the scope and impact of services outsourcing*, pages 75–134. Brookings Institution, Washington DC.

Krugman, P. (1991). *Geography and Trade.* Cambridge, MA: MIT Press.

Lee, N. (2014). The creative industries and urban economic growth in the uk. *Environment and Planning A: Economy and Space*, 46(2).

Lee, N. and Clarke, S. (2019). Do low-skilled workers gain from high-tech employment growth? high-technology multipliers, employment and wages in britain. *Research Policy*, 48(9).

Lewis, E. and Peri, G. (2015). Chapter 10 - immigration and the economy of cities and regions. In Duranton, G., Henderson, J. V., and Strange, W. C., editors, *Handbook of Regional and Urban Economics*, volume 5 of *Handbook of Regional and Urban Economics*, pages 625–685. Elsevier.

Marchand, J. (2012). Local labor market impacts of energy boom-bust-boom in western canada. *Journal of Urban Economics*, 71(1):165–174.

Mion, G. and Naticchioni, P. (2009). The spatial sorting and matching of skills and firms. *Canadian Journal of Economics/Revue canadienne d'économique*, 42(1):28–55.

Moretti, E. (2004a). Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data. *Journal of Econometrics*, 121(1):175–212.

Moretti, E. (2004b). Workers' education, spillovers, and productivity: Evidence from plant-level production functions. *American Economic Review*, 94(3):656–690.

Moretti, E. (2010a). Local labour markets. (15947).

Moretti, E. (2010b). Local multipliers. *American Economic Review*, 100(2):373–77.

Moretti, E. (2012). *The New Geography of Jobs*. Houghton Miffin Harcourt.

Moretti, E. and Thulin, P. (2013). Local multipliers and human capital in the United States and Sweden. *Industrial and Corporate Change*, 22(1):339–362.

Moretti, E. and Wilson, D. J. (2014). State incentives for innovation, star scientists and jobs: Evidence from biotech. *Journal of Urban Economics*, 79:20–38. Spatial Dimensions of Labor Markets.

MUVAL (2014). Strategia nazionale per le aree interne: definizione, obiettivi, strumenti e governance. *Materiali UVAL*, 31.

OECD (2019). *OECD Regional Outlook 2019*.

Ottaviano, G. I. P. and Peri, G. (2010). The labor market impact of immigration in western germany in the 1990s. *European Economic Review*, 54(4):550–570.

Peri, G. (2002). Young workers, learning, and agglomerations. *Journal of Urban Economics*, 52(3):582–607.

Peri, G., Shih, K., and Sparber, C. (2015). Stem workers, h-1b visas, and productivity in us cities. *Journal of Labor Economics*, 33(S1):S225–S255.

Roback, J. (1982). Wages, rents, and the quality of life. *Journal of political Economy*, 90(6).

Rosés, J. and Wolf, N. (2018). *The Economic Development of Europe's Regions: A quantitative History since 1900*. Routledge.

Serafinelli, M. (2019). "good" firms, worker flows, and local productivity. *Journal of Labor Economics*, 37(3):747–792.

Suárez Serrato, J. C. and Wingender, P. (2016). Estimating local fiscal multipliers. Working Paper 22425, National Bureau of Economic Research.

Walker, W. R. (2013). The Transitional Costs of Sectoral Reallocation: Evidence From the Clean Air Act and the Workforce*. *The Quarterly Journal of Economics*, 128(4):1787–1835.

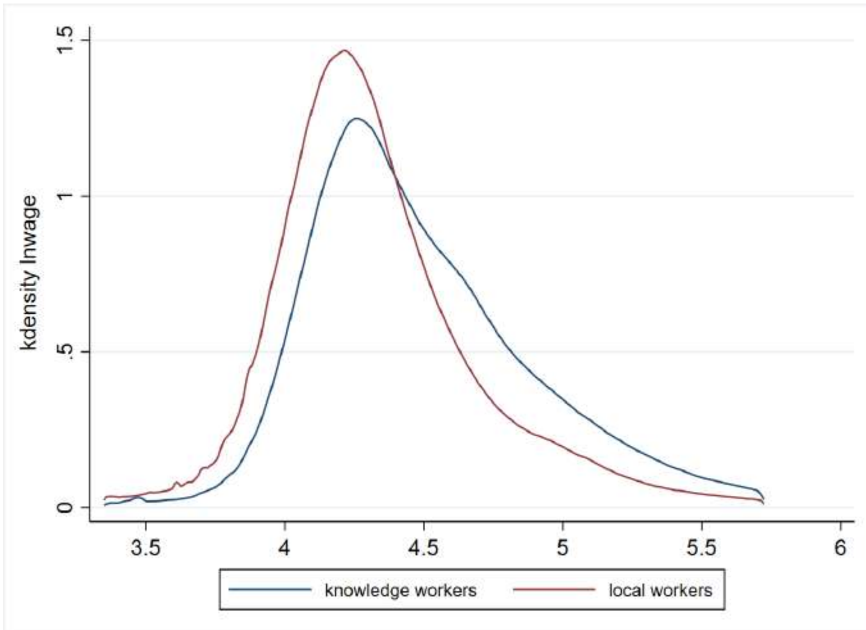
Appendix

Table A2.1: Distribution of (non-)knowledge-sector workers by education group and job position

	Knowledge sector	Non-Knowledge sector
Panel a. Education group		
% High-school dropouts	24.07	41.72
% High-school diploma	47.21	44.09
% College degree	25.51	12.14
% Master or PhD	3.21	2.05
Total	100	100
Panel b. Job position		
% Blue collar workers	27.98	57.33
% White collar workers	61.42	33.56
% Managers	6.17	3.17
% Apprentices	3.43	5.64
Total	100	100

The Table reports the percentages of workers in each education group and job position inside and outside the knowledge sector. Panel a. focus on the years 2017-2019, for which the percentage of missing information is reduced to a 22%. Panel b., instead, refers to the full sample over the 2005-2019 period.

Figure A2.1: (log) wage distribution of knowledge-sector and non-tradables workers



The graph visualises the (log) wage kernel distribution of knowledge-sector and non-tradable workers.

Figure A2.2: The knowledge sector

		Knowledge-intensity	
		High	Low
Tradability	High	Knowledge workers	
	Low	Local workers	

The graph visualises the definition of knowledge and local workers: respectively, workers of tradable and knowledge-intensive sectors, and workers of non-tradable industries.

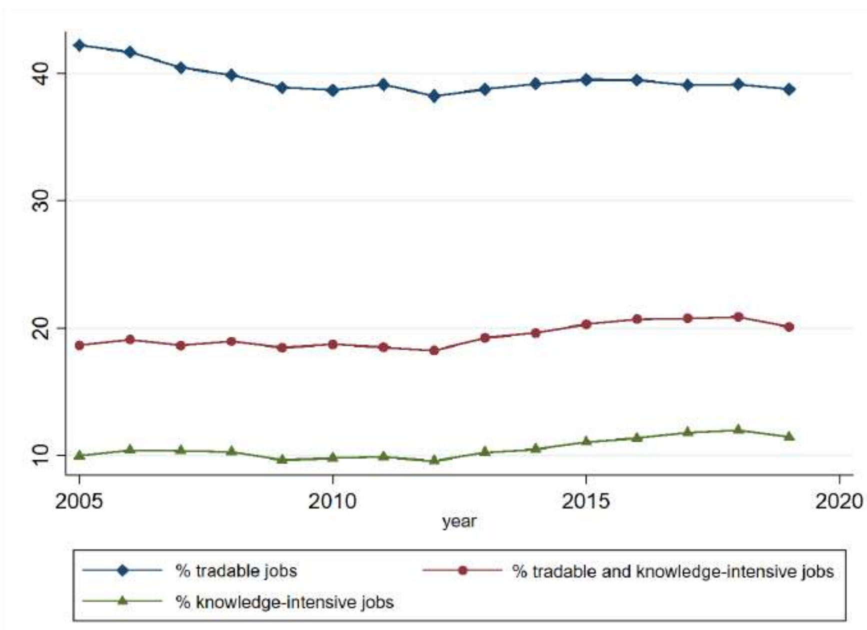
Table A2.2: Sector codes in the knowledge sector (4-digit)

Code	Description
910	Support activities for oil and natural gas extraction
990	Support activities for the extraction
1910	Manufacture of pitch and pitch coke
1920	Oil refineries and manufacture of refined petroleum products
2110	Manufacture of basic pharmaceutical products
2120	Manufacture of medicinal products and other pharmaceutical preparations
2612	Manufacture of assembled electronic boards
2620	Manufacture of computers and peripheral equipment
2630	Manufacture of other electrical and electronic telecommunications equipment, radio and television transmitter, anti-theft and fire protection systems
2640	Manufacture of sound and image reproducing and recording apparatus, video game consoles (excluding electronic games)
2651	Manufacture of instruments for navigation, hydrology, geophysics and meteorology, flame and combustion detectors, mine, motion detectors, pulse generators and metal detectors, other measuring and regulating apparatus, drawing instruments, meters for electricity, gas, water and other liquids
2652	Manufacture of watches
2660	Manufacture of irradiation equipment for food and milk and other irradiation instruments and other electrotherapeutic equipment
2670	Other irradiation instruments and other electrotherapeutic equipment, photographic and cinematographic equipment, optical measuring and control equipment
2680	Manufacture of magnetic and optical media
5110	Scheduled passenger air transport, non-scheduled passenger air transport; charter flights
5121	Air cargo transport
5122	Space transport
5811	Publishing of books
5812	Publication of lists
5814	Publishing of journals and periodicals
5819	Other publishing activities
5821	Edition of computer games
5829	Edition of other package software (excluding computer games)
5911	Motion picture, video and television production activities
5912	Film, video and television post-production activities
5913	Motion picture, video and television programme distribution activities
5914	Activities of film projection
5920	Printed music edition and sound recording studios and edition
6110	Fixed telecommunications
6120	Mobile telecommunications
6130	Satellite telecommunications
6201	Production of software not related to the edition
6202	Consultancy in the field of information technology
6203	Management of hardware IT facilities and equipment - housing (excluding repair)

6311	Electronic accounting data processing (excluding Tax Assistance Centres - Caf), database management, other electronic processing of data
6312	Web portals
6391	Activities of news agencies
6399	Other information services activities
6420	Activities of holding companies (holding companies)
6430	Mutual funds (open and closed, real estate, securities market), Sicav (Variable Capital Investment Company)
6491	Financial leasing
6492	Activities of credit guarantee consortia and other credit activities
6499	Brokering activities, merchant bank, factoring
6511	Life insurances
6512	Other insurance activities
6520	Reinsurance activities
6530	Pension funds
6611	Administration of financial markets
6612	Trading of securities and commodities contracts
6619	Credit card payment processing activities, money transfer, financial promoters, activities of Administrative Trustees
6621	Activities of independent insurance valuers and liquidators
6622	Insurance agents, brokers and other insurance intermediaries
6629	Central supervisory authorities for insurance and pension funds and auxiliary activities
6630	Management of mutual funds and pension funds
6910	Activities of notarial and law studios
7010	Activities of holding companies engaged in management activities
7021	Public relations and communication
7111	Activities of architectural firms
7112	Activities of engineering studies, cartography and aerophotogrammetry activities, technical activities carried out by surveyors
7211	Research and experimental development in biotechnology
7219	Research and experimental development in other natural sciences and engineering
7220	Research and experimental development in the social sciences and humanities
7311	Conducting marketing campaigns and other advertising services
7312	Activities of concessionaires and other advertising agents
7320	Market research and opinion polls
7410	Fashion design and industrial design activities, graphical and web design
7430	Translation and interpretation
7490	Consulting on safety, agriculture and other technical issues; weather forecasts; entertainment and sports agencies and agents or prosecutors; technical activities carried out by industrial experts
7810	Search, selection, placement and support services for personnel relocation
7820	Activities of temporary work agencies (temporary agency work)
7830	Other human resources supply and management activities (staff leasing)
7911	Activities of travel agencies
7912	Tour operators
8411	General planning activities and general statistical services; activities of central and local legislative and executive bodies; financial administration; regional, provincial and municipal administrations
8421	Foreign affairs
8422	National defense
8423	Justice and judicial activities

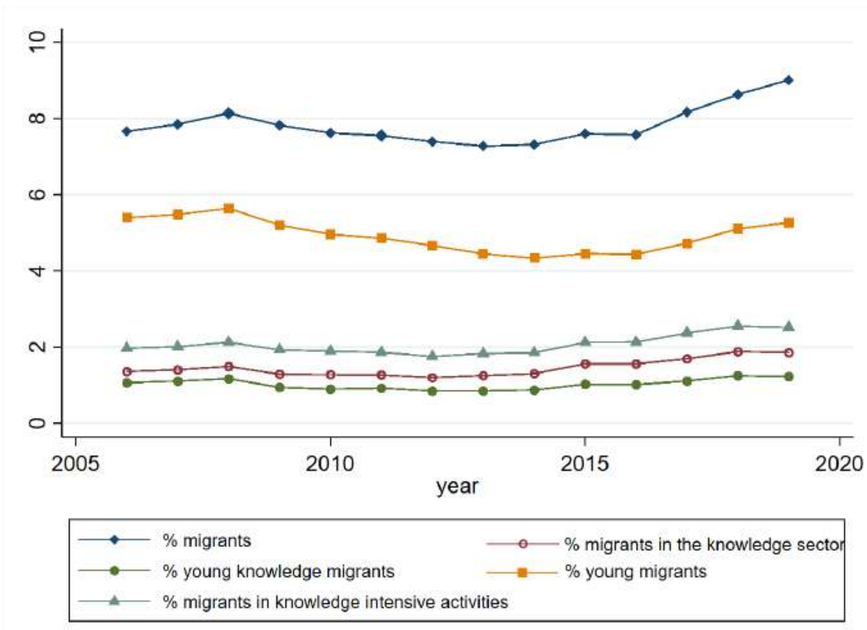
8424	Public order and national security
8559	Language courses, training and retraining courses, other educational services
8560	School counselling and guidance services
9001	Activities in the field of acting and other artistic representations
9002	Rental with operator of structures and equipment for events and shows, other support activities for artistic performances, directing
9003	Activities of independent journalists, conservation and restoration of works of art, other artistic and literary creations
9004	Management of theatres, concert halls and other artistic structures
9102	Museum activities
9103	Management of historical sites and monuments and similar attractions
9104	Activities of botanical gardens, zoos and nature reserves
9411	Activities of employers' organizations, industrial federations, commerce, craft industries and services, associations, unions, federations between institutions
9412	Activities of professional federations and councils and colleges
9420	Activities of trade unions
9492	Activities of political parties and associations
9499	Activities of organisations for international cooperation and solidarity, filanthropy, cultural organisations, organisations for human and animals rights
9900	Extraterritorial organisations and bodies

Figure A2.3: 2005- 2019 evolution of the knowledge sector



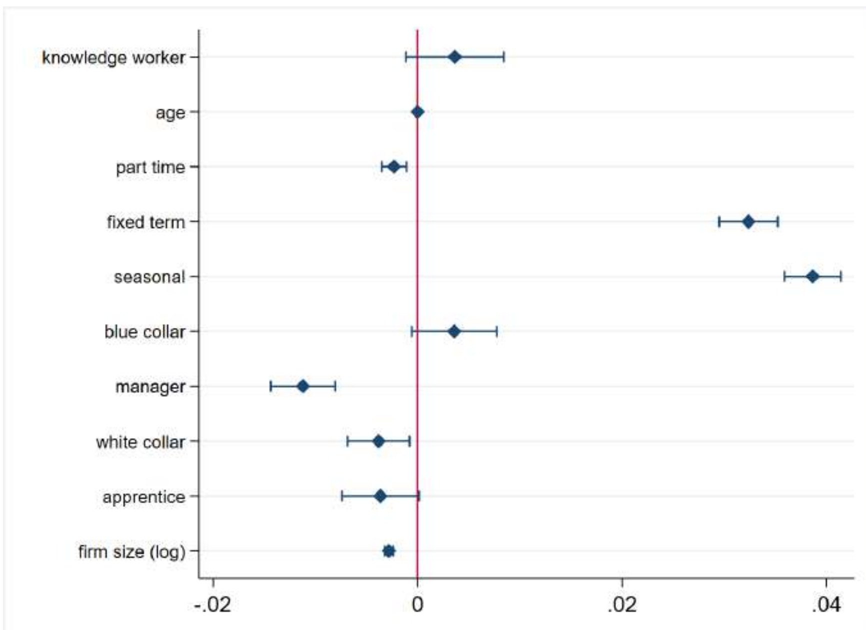
The graph shows the percentages of workers employed in tradable (blue line) and knowledge-intensive (red line) sectors, and in both of them (green line), over the period 2005-2019.

Figure A2.4: 2005- 2019 evolution of internal migrations



The graph shows the percentages of workers migrating across LLMs (blue line), over the period 2005-2019. It then distinguishes migrants according to age (below or above 40 yo) and employment sector (in knowledge-intensive or knowledge-intensive *and* tradable industries).

Figure A2.5: Likelihood of moving by worker characteristics



The graph reports the estimated coefficients from a regression of outmigration probability on a set of individual characteristics (2005-2019). We also include in the specification individual and LLM-year fixed effects, and 2-digits sector fixed effects.

Table A2.3: Shift-share diagnostics

Panel A: Negative and positive weights

	Sum	Mean	Share
Negative	-0.052	-0.003	0.047
Positive	1.054	0.016	0.953

Panel B.1: Correlations of Industry Aggregates _ Wage

	α_s	$\%S$	β_k	F_s	$\text{Var}(w_{s,95})$
α_k	1				
$\%S$	0.215	1			
β_s	0.048	-0.206	1		
F_s	-0.112	-0.042	0.003	1	
$\text{Var}(w_{s,95})$	0.376	-0.029	0.162	-0.123	1

Panel B.2: Correlations of Industry Aggregates _ Employment

	α_s	$\%S$	β_s	F_s	$\text{Var}(w_{s,95})$
α_s	1				
$\%S$	0.215	1			
β_s	0.048	-0.181	1		
F_s	-0.112	-0.042	0.021	1	
$\text{Var}(w_{s,95})$	0.376	-0.029	0.190	-0.123	1

Panel B.3: Correlations of Industry Aggregates _ Outmigration

	α_s	$\%S$	β_s	F_s	$\text{Var}(w_{s,95})$
α_s	1				
$\%S$	0.215	1			
β_s	0.089	-0.128	1		
F_s	-0.112	-0.042	-0.056	1	
$\text{Var}(w_{s,95})$	0.376	-0.029	0.014	-0.123	1

Panel C.1: Top 5 Rotemberg weight industries _ Wage

	$\hat{\alpha}_s$	$\%S$	$\hat{\beta}_s$	Ind Share
Law firms	0.107	0.236	0.003	43.710
Reinsurance	0.263	0.364	0.003	76.190
Staff leasing	0.076	4.855	0.003	0.947
Organisations for citizens' rights	0.069	0.393	0.010	20.122
Judicial activities	0.076	0.219	0.006	35.890

Panel C.2: Top 5 Rotemberg weight industries _ Employment

	$\hat{\alpha}_s$	$\%S$	$\hat{\beta}_s$	Ind Share
Law firms	0.107	0.236	0.004	43.710
Reinsurance	0.263	0.364	0.004	76.190
Staff leasing	0.076	4.855	0.004	0.947
Organisations for citizens' rights	0.069	0.393	0.005	20.122
Judicial activities	0.076	0.219	0.005	35.890

Panel C.3: Top 5 Rotemberg weight industries _ Outmigration

	$\hat{\alpha}_s$	$\%S$	$\hat{\beta}_s$	Ind Share
Law firms	0.107	0.236	-0.00003	43.710
Reinsurance	0.263	0.364	0.00003	76.190
Staff leasing	0.076	4.855	-0.0003	0.947
Organisations for citizens' rights	0.069	0.393	-0.0004	20.122
Judicial activities	0.076	0.219	-0.0002	35.890

The table reports the IV diagnostics as suggested by Goldsmith-Pinkham et al. (2020). Panel A reports the sum, the mean and the share of negative and positive Rotemberg weights α_s. Panel B reports correlations between the weights (α_s), the 2005-2019 industry employment shares within the knowledge sector at national level ($\%S$), the just-identified coefficients (β_s), the related first stage F-statistics (F_s) and the variance in past industry shares ($\text{Var}(w_{s,95})$). Panel C reports the top five industries according to the Rotemberg weights. The coefficients $\hat{\beta}_s$ are based on the regression of Table 2.4, where we regress our outcomes of interest (wage, employment and outmigration) on treatment ($KW1$), LLM and year fixed effects, and weight by the number of observations employed in the 1st-step estimation (2.1). We computed the Rotemberg decomposition using the bartik_weight Stata package.

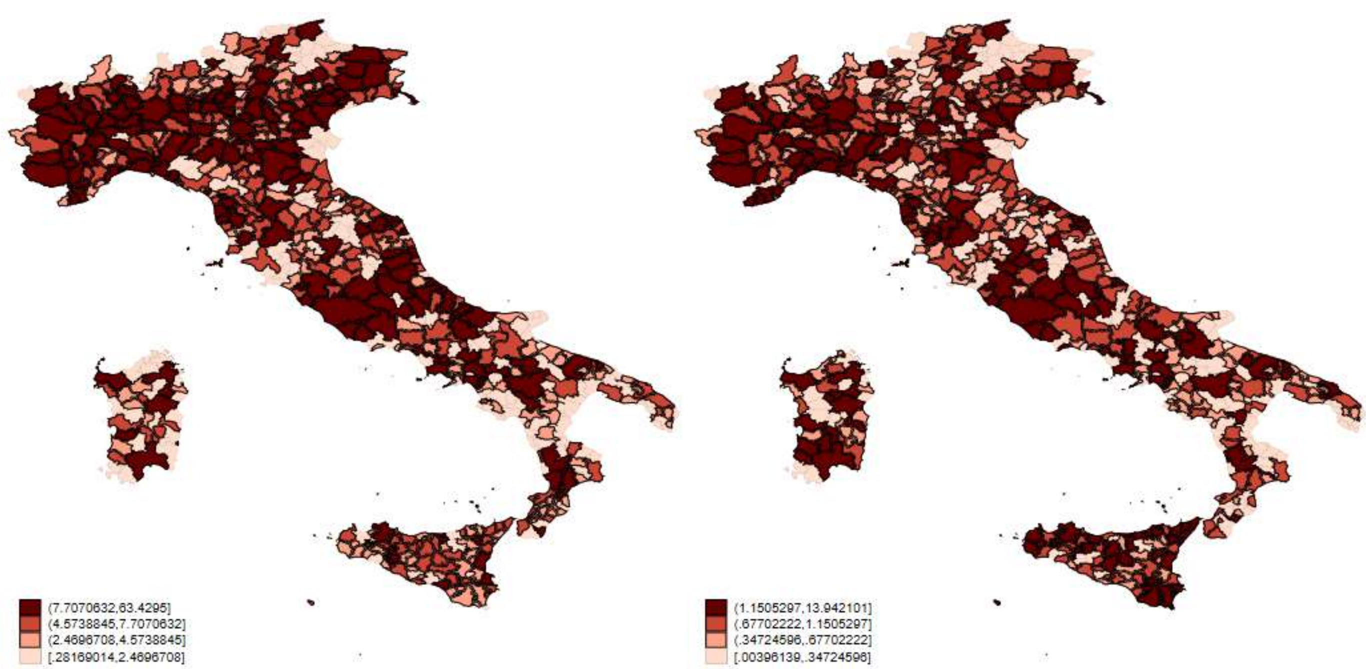
Table A2.4: Correlation between Top 5 Rotemberg weight industry shares and LLM characteristics

	Law firms	Reinsurance	Staff leasing	Organisations for citizens' rights	Judicial activities
Δ numb. of firms	0.00018	-0.00036	-0.00001	0.00006	0.00055
	(0.0001)	(0.0004)	(0.0001)	(0.0001)	(0.0004)
Numb. of firm births	0.00001	0.00025*	0.00001	0.00002	-0.00025
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Employees growth rate	0.11872	-0.69713	0.01353	-0.05517	-4.77037
	(0.1386)	(0.4327)	(0.0153)	(0.1078)	(5.5580)
N	604	604	604	604	604

Standard errors clustered at LLM level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the estimated correlation between past shares (1995) of the top 5 Rotemberg weight industries and LLM characteristics over the period 2001-2004. Regressors of the OLS estimation include the variation in the number of local firms, the number of newly created firms (births), and the growth rate of employees of local firms. Observations are reduced to 604 since we exclude the 7 LLMs that had no knowledge workers in 1995.

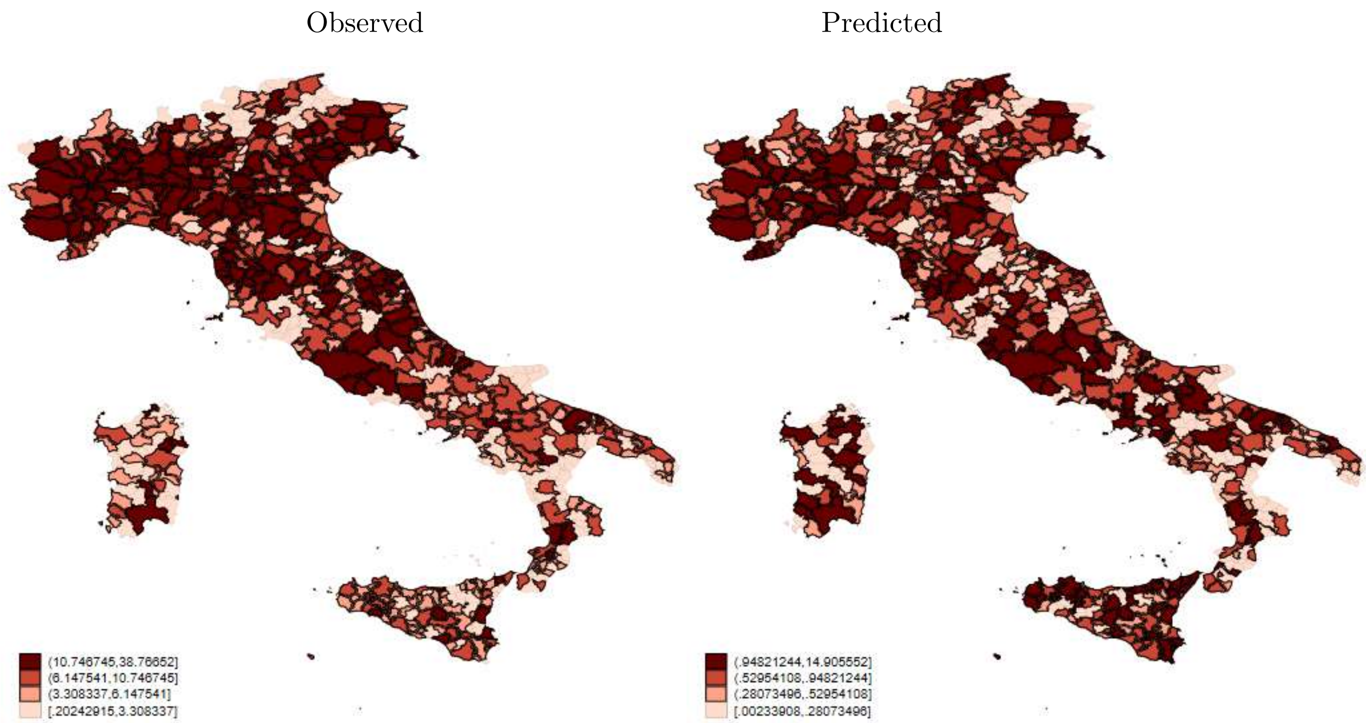
Figure A2.6: 2005 % knowledge workers

Observed Predicted



The maps report the percentages of knowledge workers by LLM in 2005, observed (Panel a) and predicted by the instrument (Panel b).

Figure A2.7: 2019 % knowledge workers



The maps report the percentages of knowledge workers by LLM in 2019, observed (Panel a) and predicted by the instrument (Panel b).

Table A2.5: IV estimation: second stage results (long-differences), Conley-standard errors using buffers of 10km radius

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	-0.009	0.006**	-0.002***			
	(0.0069)	(0.0031)	(0.0008)			
% knowledge migrants				-0.001	0.001*	-0.0002**
				(0.0005)	(0.0003)	(0.0001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered using the method by Conley (1999) * p<0.10, ** p<0.05, *** p<0.01. Here we employ buffers of 10km around the LLM's centroid. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are the area-year effects predicted from equation 2.1. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

Table A2.6: IV estimation: second stage results (long-differences), Conley-standard errors using buffers of 20km radius

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	-0.009	0.006*	-0.002**			
	(0.0069)	(0.0032)	(0.0008)			
% knowledge migrants				-0.001	0.001*	-0.0002**
				(0.0005)	(0.0003)	(0.0001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered using the method by Conley (1999) * p<0.10, ** p<0.05, *** p<0.01. Here we employ buffers of 20km around the LLM's centroid. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are the area-year effects predicted from equation 2.1. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

Table A2.7: IV estimation: second stage results (long-differences), Conley-standard errors using buffers of 30km radius

	Wage	Employment	Outmigration	Wage	Employment	Outmigration
% knowledge workers	-0.009	0.006*	-0.002***			
	(0.0070)	(0.0034)	(0.0008)			
% knowledge migrants				-0.001	0.001*	-0.0002**
				(0.0005)	(0.0003)	(0.0001)
year fe	✓	✓	✓	✓	✓	✓
LLM fe	✓	✓	✓	✓	✓	✓
N	1,222	1,222	1,222	1,222	1,222	1,222

Standard errors clustered using the method by Conley (1999) * p<0.10, ** p<0.05, *** p<0.01. Here we employ buffers of 30km around the LLM's centroid. The Table reports the estimated coefficients from the second stage regression corresponding to equation 2.2, where the treatment variable is instrumented by the shift share measure of equation 2.6. The outcome variables are the area-year effects predicted from equation 2.1. Variables refer to 2005 and 2019, to estimate the model in long-differences. Regressors are our treatment variables (instrumented), area and year fixed effects. We also include as weights the number of local workers in the LLM, to account for different precision in 1st-step estimates.

Chapter 3

The Political Economy of Regional Development: Evidence from the *Cassa per il Mezzogiorno*

Authors: Tancredi Buscemi and Giulia Romani

Abstract

Institutional design can influence the efficacy of public investment programmes. Specifically, devolution of authority may trigger tactical redistribution between different tiers of government and facilitate patronage dynamics at the local level. We test this hypothesis in the context of the *Cassa per il Mezzogiorno* (CasMez). By 1971, the authority over funds allocation was transferred from a centralised and technical committee to the newborn regional governments. We investigate how the reform affected CasMez's distributive politics. We focus on the period 1960-1984 and study whether municipalities aligned with the regional government (i.e controlled by the same party) received more funds compared to unaligned ones. We combine unique historical data on local administrators with detailed information on project approval and financing, and implement a Two-Way-Fixed-Effects strategy. Our results suggest that aligned municipalities were assigned a higher number of projects and received larger per-capita amounts, without producing any positive impact on long-run economic outcomes. The effect is driven by subsidies to local firms. This evidence supports our claim that the decentralisation process of 1971 distorted funds allocation, and possibly paved the way for rent-seeking pressures by local lobbies and patronage dynamics.[1]

[1] This chapter has been published in the Working Papers series of the Economics Department, Ca' Foscari University of Venice.

3.1 Introduction

The institutional design of development programmes can largely affect their overall efficacy. Specifically, the allocation of authority over funds distribution can give rise to agency problems (Bandiera et al., 2009 and 2021; Decarolis et al., 2020). These are more likely to emerge when agents are poorly equipped with governance capacity, are subject to incentives different from the common goal (e.g. electoral returns), and have wide discretionary power (Best et al., 2017; Bucciol et al., 2013; Bosio and Shleifer, 2020). In such circumstances, devolution processes that assign decision power to lower tiers of government can trigger tactical redistribution, distorting funds allocation (Golden and Min, 2013; Persson and Tabellini, 2002).

In this paper, we investigate whether the devolution of authority over public investments generates dynamics of distributive politics, in the form of partisan alignment effects. We exploit the quasi-natural experiment offered by the 1971 institutional reform of the *Cassa per il Mezzogiorno* (CasMez): a massive public investment programme for the economic development of Southern Italy, implemented between 1950 and 1984. The reform radically modified the CasMez governance, moving the authority over funds allocation from a central committee of technicians to regional governments, just created in 1970. These were assigned a highly discretionary power in deciding which projects to finance within their jurisdictions, in a context of fragile institutions and characterised by rent-seeking pressures by local lobbies. Our hypothesis is that such a reform created a moral hazard incentive for regional governments to distribute CasMez funds to achieve electoral consensus and strengthen their political power at the local level. Specifically, we ask whether - after the 1971 reform - municipalities ruled by the same political party in power at the regional level received a higher number and amount of CasMez funding.

Our paper aims to contribute to two main streams of the literature. Firstly, to the growing field of studies on decision-making delegation in public policies; secondly, to the work on distributive politics and, specifically, to studies investigating partisan alignment effects. The first analyses the trade-off between efficiency and corruption in institutional settings characterised by different levels of discretion(vs rules) and decentralisation(vs centralisation), and has mainly focused on public procurement (Bandiera et al., 2009; Decarolis et al., 2020; Bandiera et al., 2021). The second deals with the political economy of funds allocation, investigating how electoral objectives affect the distribution of resources (Golden and Min, 2013; Persson and Tabellini, 2002). In particular, some studies in this field analyse how upper tiers of government tend to favour lower tiers of government ruled by the same political party, namely the 'partisan alignment effect' (Solé-Ollé and Sorribas-Navarro, 2008; Bracco et al., 2015; Dotti, 2016). We contribute to the literature on delegation by focusing on a key area of government activity – namely, public investments -, and highlighting the political distortions that can arise from devolution processes. Moreover, we also contribute to the literature on distributive politics by investigating whether and how partisan alignment effects depend on the broader institutional setting and, specifically, on the degree of centralisation.

To conduct our analysis, we have collected and digitalised unique historical data on Italian local administrators for the period 1960-1984 from the Italian Ministry of Interior. We decided to focus on the 374 municipalities having a population of at least 10,000 inhabitants by 1971, within the regions part of the CasMez programme. We combine that information on local governments with data on CasMez projects. These derive from the Archives of Territorial Economic Development (ASET), containing project-level information about all CasMez investments (1950-1984), distinguishing among three main types of funds: public works, non-refundable firm grants, and concessional financing. We aggregate that information at the municipal level and focus on the time span 1960-1984. In this way, we observe a time window of twelve years before and after the reform and focus on the period when CasMez investments have been larger.

We implement a Two-Way-Fixed-Effects (TWFE) strategy, taking the CasMez reform of

1971 as the event time for treatment. We define treated and control municipalities according to partisan alignment as for 1970, i.e. when Italian regions were created. If a municipality in 1970 was (not) ruled by the same political party that won the first regional elections, we consider it treated (control) after the 1971 reform. We perform event study estimations and a placebo test on the pre-treatment period to verify that treated and control units were evolving in a comparable way prior to the reform and did not anticipate its effects.

Our findings suggest that the devolution process brought about by the 1971 CasMez reform triggered significant tactical redistribution. Specifically, the shift of authority from the central technical committee to the newborn regional governments made relevant the partisan alignment between the local and regional tier of government: aligned municipalities (i.e. controlled by the same political party) were assigned a higher number of projects and received larger per-capita funds. That effect is driven by subsidies to local firms, a finding that points to possible patronage and pork-barrel politics. Compared to public works, firm subsidies can be parcelled out more flexibly and assigned in a less visible way, which makes them a more suitable tool to acquire the consensus of local lobbies.

These results are robust to a number of tests, including alternative treatment definition and sample selection choices. Moreover, pre-processing data through a combination of matching algorithms prior to parametric estimation provides similar results. Similarly, including a spatial lag to capture possible cross-border spillovers does not alter our estimates.

Finally, we explore the effects on long-run economic outcomes through a TSLS cross-section analysis, focusing on the period after the reform. we regress the long-term change in local economic outcomes between 1971 and 1991 on the predicted CasMez funds received by the municipality over the span 1972-1984. These predicted values are estimated from a first-stage equation whose main explanatory variable is alignment status in the aftermath of the 1971 reform. In this way, we indirectly test a competing interpretation of our main findings: namely, that regional governments favour aligned municipalities because of some information or coordination advantage. If so, there would be an economic rationale behind the tactical redistribution that we have labelled as politically biased. We do not detect any significant effect. Therefore, the devolution process did not produce any economic benefit; most likely, the larger amounts of firm subsidies granted to aligned municipalities served to strengthen the local connections between elected politicians and entrepreneurs, feeding patronage and pork-barrel politics. We repeat an analogous exercise on the pre-reform period (decade 1961-1971), without instrumenting CasMez funds. Such correlation analysis highlights a positive relation between CasMez funding and local economic outcomes prior to the 1971 reform. These findings, far from being causal, do not exclude the possibility of positive economic effects of CasMez funds prior to the governance reform of 1971.

The paper is organised as follows: section 2 reviews the related literature; section 3 presents the institutional context and history of the CasMez programme; section 4 describes the data we collected and shows some descriptive evidence; section 5 and 6, respectively, explains our main identification strategy and presents the related results; section 7 reports the robustness checks and related results; section 8 investigates the long-run economic effects; section 9 concludes.

3.2 Related literature

Our work speaks to different streams of literature. First of all, a growing literature is focusing on the choices of rules versus discretion, and decentralisation versus centralisation in the management of public resources. These contributions investigate the trade-off between efficiency and corruption in models of delegation applied to public procurement. More specifically, this body of research addresses the economic consequences of delegating authority to lower-level tiers of government in the purchase of public goods. These works often assume the presence of some 'ex-ante constraints', such as central guidelines in the employment of resources, which reduce the discretionary power of delegated agents (e.g. Huber and Shipan, 2002 and Bendor et al., 2001). For example, Decarolis et al. (2020) focus on Italy and show how delegation and discretion in procurement auctions boost efficiency more than they foster corruption: discretion leads to greater potential efficiency and to more opportunities for extractive behaviours; however, discretionary procedures are used less in administrations suspected of corruption. Therefore, it seems plausible that a central monitor can manage the underlying trade-off by limiting discretion where the risk of corruption is higher. Bandiera et al. (2021) find similar results studying the shift of authority for public good purchases from monitors to officers in Pakistan. In that contribution, the authors underline that the overall impact depends on the monitor's type: "giving autonomy to the agent is desirable when it means taking it away from an extractive monitor" (i.e. 'bad type'), since it eliminates the 'competing bandits problem' (Shleifer and Vishny, 1993); while it has no positive effect when the monitor is 'good' (i.e. aligned with the common goal). Again on Italy, Bandiera et al. (2009) highlight that most waste of resources in decentralised public procurement is due to inefficiency ('passive waste') rather than to corruption ('active waste'). Passive waste can arise when delegated officials lack of the necessary skills or incentives to minimise costs, or when the regulatory burden is too heavy. Therefore, to the extent that increased autonomy reduces inefficiency without excessively rising corruption, it would decrease overall public waste. Along this line, other works highlight that limiting decentralisation is convenient when the skills of the delegated public officers are not adequate (Best et al., 2017; Bucciol et al., 2013; Bosio and Shleifer, 2020). Specifically, Bosio and Shleifer (2020) conduct a cross-country analysis and get to the conclusion that reducing discretion in public procurement produces substantial benefits only in countries where public sector capacity is low.

All in all, these studies seem to conclude that delegation is a more efficient agency model as long as agents are well-equipped with governance capacity and relatively aligned with the common goal.

That view is supported by a long standing argument in the economic literature on federalism, which claims that decentralising public choices is a driver of efficiency, since it moves decision making closer to the needs of the local communities (Oates, 1993, 2005). On the other hand, a large body of research takes the opposite view, arguing that decentralisation can negatively affect economic efficiency. Persson and Tabellini (1994) maintain that decentralisation could obstacle economic growth by making more difficult redistribution among regions. Prud'Homme (1995) and Tanzi (1996) highlight that decentralised settings are more exposed to the risk of corruption, since local governments are more susceptible to the pressures of local interest groups. In the context of place-based policies, D'Amico (2021) develops and tests a theoretical model showing that investment decisions of regions tend to favour the dominant skill-group of workers; while centralised management seems more independent of the local workforce composition. Moreover, devolution seems to produce detrimental consequences in developing countries, because it exacerbates the weak accountability capacity of institutions (Bardhan and Mookherjee, 2006). Nevertheless, the inefficacy of devolution has recently been highlighted also for developed countries. For example, Rodríguez-Pose and Ezcurra (2010) find a negative effect of decentralisation on economic growth for OECD countries, a result recently confirmed by Gemmell et al. (2013).

Our paper also relates to the literature on distributive politics. Contributions in this field have largely explored different distorting mechanisms that politics can induce in the allocation of public resources under democratic systems.[2]. Within the several branches of this literature, the one most closely related to our work focuses on the distributive effects of partisan alignment between different tiers of government. This alignment emerges when an upper layer of government is more likely to assign public resources to lower-level districts that are ruled by the same political party, despite other socio-economic considerations. For example, Solé-Ollé and Sorribas-Navarro (2008) show that intergovernmental transfers in Spain favour local governments ruled by the same political party that is in power at the

[2] For a comprehensive overview of the literature on distributive politics, see Golden and Min (2013) and Persson and Tabellini (2002) Many works in this literature focus on Italy: relevant references are Sapienza (2004); Golden and Picci (2008); Bracco et al. (2015); Carozzi and Repetto (2016).

National level. Bracco et al. (2015) go further on this issue, and elaborate an agency model in which the central government assigns more grants to aligned municipalities as a fake signal of the expertise of the corresponding mayors. Finally, Dotti (2016) shows that structural funds are more likely directed to regions aligned with the central government throughout Europe. Despite the widespread evidence of alignment effects, the link between these latter and the underlying institutional design has been so far overlooked.

Our article fills a gap at the intersection of these fields of the literature. Indeed, we investigate whether devolution of authority can give rise to partisan alignment effects in the allocation of public investments. Therefore, we contribute to the discussion on decentralisation versus centralisation in the management of public resources by focusing on a key area of government activity – namely, public investments for regional development -, and highlighting the possible political distortions that can arise from devolution processes. This relates to the debate about the economic consequences of delegation, tackling the issue from a development policy perspective. In our setting, we hypothesise that the discretionary power attributed to the newborn regional governments created a moral hazard incentive to extract political rents from the distribution of CasMez funds. Moreover, we also contribute to the literature on distributive politics by investigating whether and how partisan alignment effects depend on the broader institutional setting and, specifically, on the degree of centralisation. This provides a different perspective on distributive politics, showing that - in a given analytical context - tactical redistribution can emerge as a consequence of institutional shifts in the allocation of authority.[3]

[3] This aspect has not been investigated yet by the economic literature. Searching into other fields, few contributions suggest that decentralised systems are more exposed to partisan alignment effects. See, for example, Nunes (2013) and Carlitz (2017) in the literature of political and development studies.

3.3 The institutional context of the *Cassa per il Mezzogiorno*

After World War II, the newborn Republic's ruling class saw the Southern Italy development as a priority to reduce the wide regional divide between the North and South of Italy. Alternative development strategies were debated among Italian economists and politicians (Costabile, 2021). This led to the foundation of a *Cassa* for extraordinary works of public interest in Southern Italy, instituted by the Italian parliament with law 646 approved in 1950.[4] The law provided legal autonomy to the newborn CasMez, with the task of implementing top-down extraordinary interventions for all Southern regions: Abruzzo, Apulia, Molise, Campania, Basilicata, Calabria, Sicily, Sardinia, and few provinces of Lazio and Marche. The agency's innovation was the board's autonomy: the Italian Prime Minister appointed technicians (e.g., economists and engineers) with a 4-year charge, and external authorities could not remove the board's members, which granted complete autonomy in policy making. The CasMez activity focused on mid-term goals to foster modernisation, with the sole criterion of economic development (Lepore, 2013; Felice and Lepore, 2017). These institutional features mitigated the risk of misleading exploitation of CasMez's resources for political purposes.

Until 1957, the agency's activity mainly focused on the development of agricultural techniques and infrastructures. The aim of that strategy was to create the initial conditions to induce a take-off in the modernisation and industrialisation processes. Therefore, most funding was devoted to increase agricultural productivity, and to build road and railway networks. A first relevant change occurred with law 634 approved in 1957, which extended the CasMez's lifespan until 1965, opening the 'second half' of the extraordinary intervention, in which policy efforts were directed towards bolstering industrialisation and infrastructures development. The intervention of those years consisted of sustaining the Southern economy's

[4] The complete timeline of the CasMez experience is reported in Figure A3.1.

supply side and fostering capital accumulation, in line with a policy approach defined 'Keynesianism of supply' (Saraceno, 1986). The CasMez's programme strengthened its efforts in capital-intensive sectors, such as the chemical, iron and steel industries. Many Southern localities were identified as potential growth poles, where to establish new industrial plants for those sectors, and related satellite activities. Then, law 717 approved in 1965 further extended the extraordinary intervention until 1980, linked it to the national planning programme and created a Ministry of extraordinary intervention (*Ministero per gli interventi straordinari del Mezzogiorno*).[5]

With law 281 approved in 1970, Italian regions were created.[6] These represent an intermediate level of governance between central government and municipalities. This institutional reform triggered a decentralisation process in several development and social policies. The CasMez made no exception: law 853 of 1971 put an end to the centralised and autonomous configuration of the CasMez's governance. This institutional revolution is comprehensively outlined in article 4 of the 1971 law, which assigned full control over all projects to regional governments, leaving only an advisory role to the CasMez technicians.

Southern economists saw the reform of 1971 as motivated by purely political reasons, and totally disconnected from the extraordinary intervention principles. They considered the governance shift as a transformation of the agency's activity into an *ordinary* intervention (Cafiero, 1996), and thus a failure to fulfil the ultimate scope of its creation (Saraceno, 1976). The combination of political involvement and power delegation led to a radical change in the CasMez's history. Trigilia (1992) highlights that local elites' involvement spoiled the experience of the CasMez, opening the way to patronage dynamics and rent-seeking behaviours. Organised crime, which is firmly rooted in the economic system of

[5] The national planning programme was an attempt by the Italian government to create an inter-ministerial committee to plan the national development of the country. However, this national programme was never effectively implemented (Lavista, 2010).

[6] More precisely, the special status regions of Sicily and Sardinia had regional governments already since the '50s. However, before 1971, they had no authority over the allocation of CasMez funds. For completeness, we also report estimated results excluding those two regions.

Southern Italy (Barone and Narciso, 2015; Pinotti, 2015) used the newly created political connections at the local level to strengthen its influence over local businesses. Several authors highlight that decentralisation did not improve the economic performance of the CasMez; on the contrary, it triggered dynamics of tactical redistribution of CasMez funds and reduced the programme's overall efficacy (Trigilia, 1992; Cafiero, 1996; Sbrescia, 2014 and Felice and Lepore, 2017). Cafiero (2000) defined the fourteen years from the 1971 reform to the end of the programme (1984) as the 'long agony of extraordinary intervention', characterised by a considerable waste of opportunities and resources. As Saraceno warned in SVIMEZ (1981) and 1982, these high levels of expenditure did not favour the expansion of Southern economy into new sectors and the creation of a new workforce; on the contrary, they enforced positions of power and rent-seeking attitudes at the local level. Therefore, the creation of Italian regions and the 1971 reform represent a turning point in the CasMez experience, which radically transformed the economic intervention of the following years and worsened its performance since then.

By 1971, the CasMez sought to take the next step towards modernisation; namely, the creation of a permanent industrial structure in Southern Italy. Such an objective was never achieved and Southern Italy started to lose the recovery reached between 1950 and 1970. Buscemi (2022) shows the macroeconomic consequence of the CasMez reform, and argues that the devolution process of the '70s brought a persistent regional divergence with the rest of the country.[7] Currently, the North-South gap is the same of 1950 (Svimez, 2019). Moreover, D'Adda and De Blasio (2016) find that - after the '70s - the combination of low levels of social capital and reduced government quality negatively affected the outcomes of the CasMez programme. The authors maintain that the historical legacy of social capital resurged with the decentralisation process of the '70s, undermining the efficacy of the CasMez programme.

[7] We report in Figure A3.2 the graph by Buscemi (2022), where he shows the evolution of Southern GDP and the difference in growth rates between Southern Italy and the rest of the country.

Since 1984, the end year of the CasMez, several types of decentralised cohesion policies have been implemented in Southern Italy, and plenty of studies highlight their limited efficacy (e.g. Barone et al., 2016; De Angelis et al., 2020). The common ground of this literature is that none of the subsequent policies managed to reduce the wide Italian regional divide, which remerged after the '70s.

Recently, the CasMez programme has attracted new attention in the economic literature. For example, Albanese et al. (2021) look at the long-run political outcomes of having received CasMez funds; while Colussi et al. (2022) study how the exposure to CasMez funding increased support for the majority party, even long after the end of the programme. Our work also contributes to this new evidence on the CasMez, which is *per-se* worthy of interest. This programme has been one of the most important public interventions to promote regional development, second only to the US Tennessee Valley Authority. Therefore, it seems crucial to understand the strengths and weaknesses of its institutional design. In this paper, we exploit the quasi-experiment offered by the 1971 reform to investigate the effect of that institutional shift on the political economy of the programme.

3.4 Data and Descriptives

Our dataset combines information from a variety of sources. First of all, we collected unique historical data on local administrators for 374 Italian municipalities of Southern Italy, covering the period 1960-1984. This information derive from the Register of Local Administrators (*Anagrafe degli Amministratori Locali*) of the Italian Ministry of Interior, and include name, occupation, education level, political affiliation and position of each member of the municipal council.[8] We decided to focus on municipalities having a population of at least 10,000 inhab-

[8] Specifically, data were kindly provided by the Microfilm Office of the Central Directorate of Electoral Services (*Ufficio Microfilm della Direzione Centrale dei Servizi Elettorali, Dipartimento Affari Interni e Territoriali*).

itants by 1971, within the regions interested by the CasMez programme.[9] This selection is motivated, firstly, by the willingness to focus on municipalities that represent important electoral constituencies for political competition. Moreover, these denser municipalities received - on average - more funding than out-of-sample ones (see Figure A3.4 in the Appendix). In-sample municipalities attracted 69% of CasMez funds granted over the pre-reform period. This is consistent with the industrial composition of in- and out-of-sample municipalities: those with less than 10,000 inhabitants (i.e. out-of-sample) display higher percentages of agricultural employment (see Figure A3.5), suggesting that they are mostly rural places and thus not the main target of CasMez industrial investments. Finally, that sample choice is constrained by the big effort of collecting archival evidence, which consists of 8,986 observations retrieved from more than 5,600 archival files.[10] As for the time span, we decided to focus on the period 1960-1984 to observe a time window of at least ten years before and after the governance reform of 1971. In addition, this is also the time span where CasMez investments have been higher (see Figure A3.6 in the Appendix).

Secondly, we drew data on CasMez funds from the Archives of Territorial Economic Development (ASET).[11] Those archives contain historical sources and datasets on the extraordinary interventions for the development of Southern Italy; namely, the CasMez and the subsequent Agency for the Promotion and Development of Southern Italy (*Agenzia per la promozione e lo sviluppo del Mezzogiorno - AgenSud*).[12] In the ASET dataset we have project-level information on the timing, location, amount, type and purpose of each fund granted by the CasMez over the period 1950-1984. We consider three types of fund: public works (*opere pubbliche*), non-refundable firm grants (*fondo perduto*), and concessional financing (*finanzi-*

[9] These are Abruzzi, Apulia, Basilicata, Calabria, Campania, Molise, Sardinia, Sicily, the province of Ascoli Piceno (Marche) and two provinces of Lazio, namely, Frosinone and Latina. As for Molise and Marche, there was no municipality with at least 10,000 inhabitants; therefore, we excluded those regions from our analysis.

[10] Figure A3.3 in the Appendix provides an example of archival file.

[11] See the online portal at https://aset.acs.beniculturali.it/aset-web/.

[12] AgenSud was created in 1986, in substitution of the suppressed CasMez. The aim of the programme was to finance projects, support agreements with local authorities, and manage the completion of previously approved works. In the present paper, we do not deal with the AgenSud programme.

amenti agevolati). Table 3.1 displays a brief description of these types of funds, and the related time spans. Over the period considered, these were the key instruments of the CasMez activity. More precisely, concessional financing started to be distributed in 1978. Since this tool was intended as a further government aid to firms, when we distinguish by type of fund we sum non-refundable firm grants and concessional financing, and name them 'firm subsidies'.

Table 3.1: The CasMez types of funds: description and time span

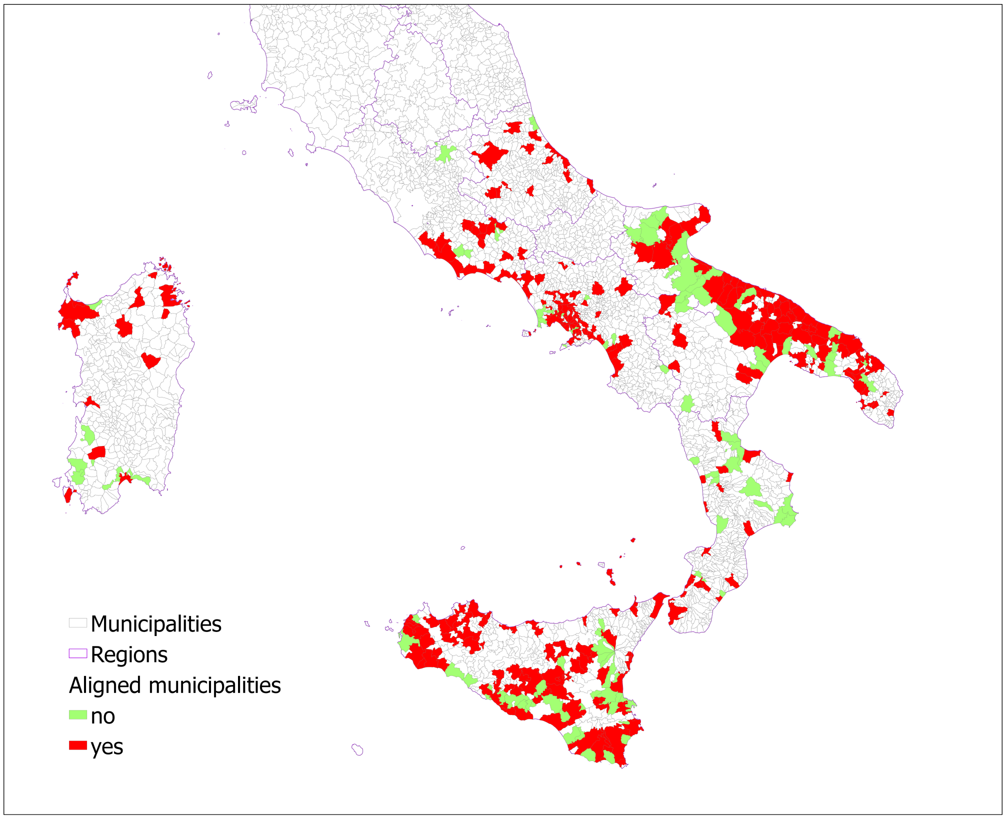
Type of fund	**Description**	**Time span**
Public works	Infrastructure investments	1950-1984
Firm grants	Non-refundable contributions for firms' investments	1950-1984
Concessional financing	Loans with interests below the market rate for firms' investments	1978-1984

Finally, we collected information on key municipal characteristics from the 1971 Italian Census.[13] Specifically, we obtained data on population, industrial composition and geological features, which we employ in the robustness strategy and in the analysis of the long-run economic effects. The combination of those information sources provides us with a unique and detailed dataset to study the issue at hand.

We now present some descriptive evidence of the phenomenon under study. First of all, we provide a graphical visualisation of the 374 municipalities in our sample. Figure 3.1 displays in red the municipalities that were aligned with the newly born regional government (i.e controlled by the same party) as of 1970, in green those that were not. The aligned ones account for 72% of the sample (271 in total), while the residual 28% of unaligned municipalities amounts to 104 in total.

[13] That information is publicly available at the Statistical Atlas of Italian Municipalities (http://asc.istat.it/ASC/).

Figure 3.1: Sample municipalities: aligned vs unaligned ones (1970)

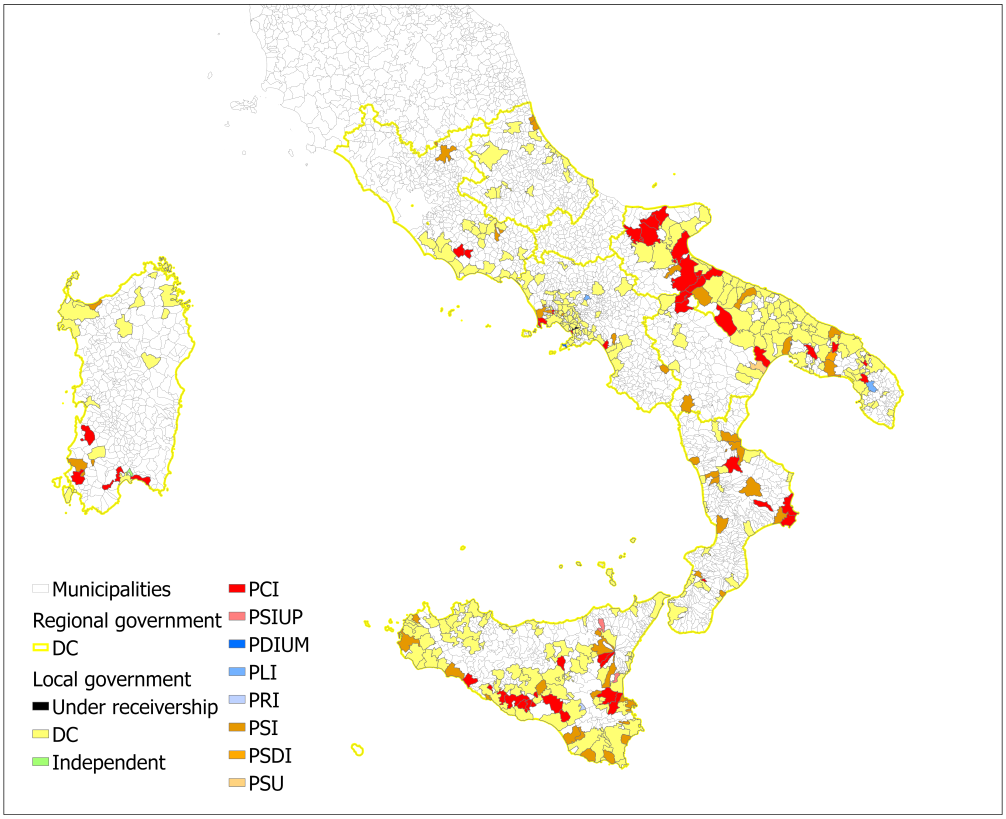


More in detail, Figure 3.2 shows the political party in power by 1970, i.e. at the creation of Italian regions. The Christian Democracy (DC) won the first electoral turn in all the regions we consider.[14] Most municipalities in our sample were also ruled by the DC in 1970, and thus classified as 'aligned' in Figure 3.1.[15] The rest of sample municipalities were largely controlled by the Communist Party (PCI) or by political forces in the 'Socialist area' (i.e. PSI, PSDI, PSU), with few exceptions governed by right-wing parties (i.e. PDIUM, PLI, PRI) or by independent mayors.[16] Not surprisingly, the Christian Democracy was the dominant political force in Southern Italy in 1970, both at the local and regional level. The DC maintained that dominant position in regional governments until 1984, with just a few

[14] The yellow borders indicate that the DC was in power at regional level in 1970.
[15] Note that alignment status can change over time, due to variations in the local or regional party government. See the following section for more details on the definition of alignment status.
[16] See Table A3.1 in the Appendix for the full list of Parties' acronyms and names.

regions going to the Socialist Party (PSI).[17] However, over the whole period observed, we have considerable variability in local government parties.

Figure 3.2: Political parties ruling sample municipalities and regions by 1970



Local administrations distribute across parties and alignment status in 1970 as shown in Table 3.2. The Table reports, for each party, the number of municipalities-years where it was in power at the local level, distinguishing between municipalities aligned/unaligned in 1970. For example, 440 municipalities-years were controlled by the DC *before* 1970, but were not so in that year, and thus they are not classified as aligned. Conversely, among those municipalities aligned in 1970 (i.e. ruled by the DC in that year), for 90 observations we have independent mayors governing the municipality, for 95 the PSI, and for 75 the PCI. Therefore, despite the large dominance of the DC, we have significant variability across

[17] Specifically, Lazio passed to the PSI by 1975, while Calabria by 1980.

municipalities and over time in the party governing the local councils.[18]

Table 3.2: Distribution of local administrations across parties and 1970 alignment status (1960-1984)

Political party	Aligned in 1970 no	Aligned in 1970 yes	Total
Under receivership	10	0	10
DC	440	5,535	5,975
Independent mayor	30	90	120
MSI	0	15	15
PCI	795	75	870
PDIUM	15	0	15
PLI	30	25	55
PRI	45	10	55
PSDI	55	15	70
PSI	675	95	770
PSIUP	15	0	15
PSU	15	0	15
USCS	0	10	10
Total	2,125	5,870	7,995

The Table reports, for each party, the number of (in-sample)municipalities-years where it was in power at the local level. The Table distinguishes between municipalities aligned/unaligned in 1970. Note that we are not including in the computation those municipalities-years for which alignment is missing; that is, those municipalities that change alignment status over the post-reform period, for the years after the change.

We then plot the time evolution of CasMez investments over the period considered. Figure 3.3 reports in Panel a) and b), respectively, the number of projects and the per-capita amount of funds assigned on average to the municipalities in our sample, distinguishing among funds attributed for public works, non-refundable firm subsidies, and concessional financing. It is worth clarifying that per-capita amounts are computed using resident population in 1971.[19]. Moreover, we express the amount of funds in (log) thousands of liras and adjust for inflation

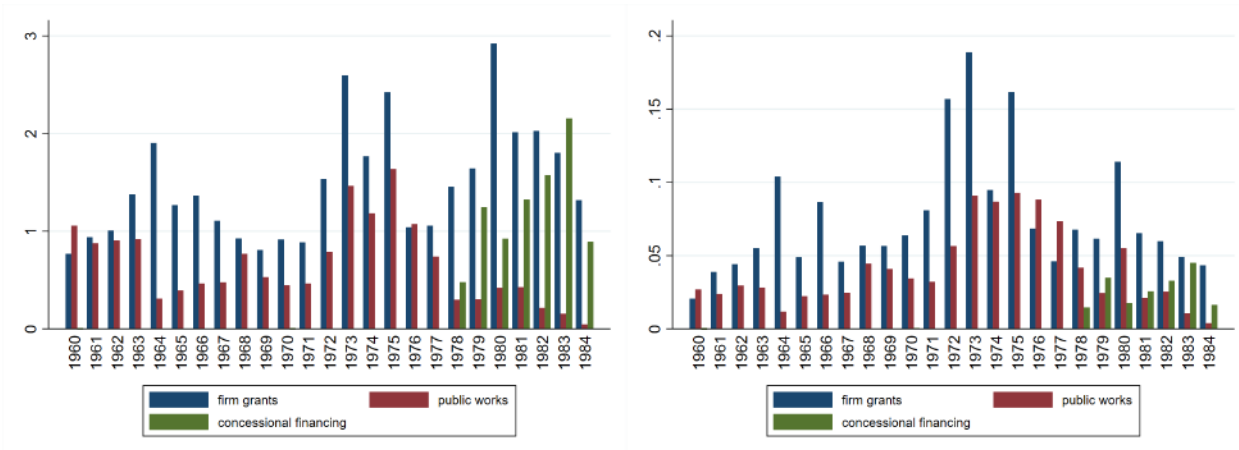
[18] The 55% of in-sample municipalities changes the party in power at the local level at least once over the period considered. Specifically, 23% change the ruling party twice and 13% three or four times. Focusing on the DC, 7% is never governed by this party, 17% is ruled by the DC for at most 10 years, and 33% has it in power for 15-20 years (over the 25 years we consider).

[19] This choice is constrained by data availability. In addition, we choose to employ the information at 1971 (i.e. prior to the institutional change) not to introduce endogeneity in subsequent estimations.

at 2011 prices.[20]

Figure 3.3: Time evolution of investments

a) Number of project approvals b) Per-capita funding (log)



The Figures report the average number of project approvals and per-capita funding by year, across all municipalities in our sample. In Panel a, the unit of measure is the number of projects approvals. In Panel b, instead, the per-capita amount of funds received is expressed in log millions of liras, adjusted for inflation (at 2011 prices).

Looking at the number of projects (Panel a), it can be noticed that non-refundable firm grants and - in the last seven years - concessional financing progressively increased their relative importance compared to public works. This finding is consistent with a development strategy which proceeds by first building infrastructures and then supporting local entrepreneurs through direct financing. However, from 1971 onward, an overall higher number of projects has been approved. That boost is even clearer when we look at per-capita amounts (Panel b). Here, we notice significant spikes between 1972-1975, mostly concerning non-refundable firm grants. Afterwards, the amount of funds gradually shrinks as we approach the end of the programme (i.e. 1984), suggesting a progressive fragmentation of investments.

Finally, we report the time evolution of investments comparing aligned and unaligned municipalities. We compute the average number of project approvals and per-capita funding

[20] To do so, we use the coefficients for currency value provided by ISTAT at https://seriestoriche.istat.it.

by year, across aligned and unaligned municipalities. Figure A3.7 reports the corresponding graph. Before the 1971 reform, the two groups of municipalities follow similar trends. Afterwards, we can notice a generalised increase in investments, with aligned municipalities receiving systematically more funds than unaligned ones. We now move to the empirical strategy to causally estimate the effect of interest.

3.5 Empirical strategy

Our aim is to estimate how the institutional shift in the CasMez governance affected the distributive politics of the programme. In particular, we want to investigate whether the devolution process determined the emergence of an alignment effect between the local and regional tier of government. To this aim, we exploit the longitudinal nature of our data and the governance reform of 1971, and implement a Two-Way-Fixed-Effects (TWFE) estimation strategy. Our unit of analysis is the municipality, over the period 1960-1984. The main outcomes of interest are the number and amount of funding received by the municipality in a given year. Specifically, we want to estimate whether and how, after the reform, funds allocation is affected by partisan alignment. To this purpose, the use of a TWFE strategy enable us to control for any time invariant characteristic of the municipality and for business cycle dynamics that influence all sample units. Moreover, we exploit the sharp governance reform intervened right after the creation of Italian regions to achieve treatment exogeneity.

Partisan alignment constitutes our treatment variable. It takes the form of a dummy variable which can take value one, starting from 1972, if the municipality in 1970 was ruled by the same political party as the newborn regional government. Before 1972, treatment is zero for all municipalities in our sample. Recall that regions were created in 1970, while the governance reform of the CasMez was made in October 1971.[21] Therefore, assignment

[21] More precisely, the special status regions of Sicily and Sardinia had regional governments already since the '50s. However, before 1971, they had no authority over the allocation of CasMez funds. In the following, we also report estimation results excluding Sicily and Sardinia and focusing only on those two regions.

to treatment is based on political conditions prior to the CasMez governance reform, which mitigates possible concerns about selection bias. To make the quasi-experiment as clear as possible, in the main estimation we restrict the post-treatment period to the first legislature after the CasMez reform, namely to 1974. In this restricted span, no municipality could adjust to the institutional change through local elections. In this way, we avoid the possibility that endogenous re-election probability confounds our results. Indeed, if the chance of remaining (un)aligned is influenced by the CasMez funds received in the previous legislature, we would have an issue of reverse causality when considering the entire post-reform period[22].

We also include in our model region-time fixed effects, so to clean our estimates from any contemporaneous change at the regional level. Note that these changes can also include electoral shifts in the regional government. Thus, in the within-region analysis, we leverage treatment variation only deriving from municipal governments. Moreover, comparing aligned and unaligned municipalities within the same region mitigates possible concerns due to the different number of in-sample municipalities we have for the various regions.

Formally, our main specification is:

$$y_{irt} = \alpha + \beta\, Alignment_{irt} + \gamma_i + \delta_{rt} + \epsilon_{irt} \tag{3.1}$$

where i, t, and r refer, respectively, to municipality, year, and region.[23] $Alignment_{it}$ is the treatment dummy just described, and y_{it} stands for number of project approvals or (log) per-capita amount of funds received. For these outcome variables, we estimate the overall

[22] In the following, we run an auxiliary exercise to provide some evidence on the risk of having a reverse causality issue in our setting. We focus on the period 1972-1975 and estimate a cross-section model that regresses re-alignement probability in 1975 on funds received over the span 1972-1975. However, the focus of our analysis remains the estimation of the effect going from alignment to funds allocation.

[23] Note that, according to our definition of treatment, there is no staggered adoption in this setting. Consequently, we should not be concerned about possible bias due to treatment effect heterogeneity across cohorts (Goodman-Bacon, 2021). Therefore, we stick to the traditional Two-Way-Fixed-Effects (TWFE) estimator

effect and also distinguish by type of funding: namely, public works and firms subsidies.[24]

We cluster standard errors at province level.[25] This choice is motivated by the likely spillovers among neighbouring municipalities, which can induce spatial correlation in the error term (Bertrand et al., 2004). This concern is especially relevant for public works, whose benefits possibly regard wider areas than the assigned municipality.[26] Moreover, spatial correlation across neighbouring municipalities can also derive from geographical concentration in voting patterns and funds distribution. Province-level clustering of standard errors should clean our estimates from any source of spatial correlation within province.[27]

In most of our analysis, we also include an indicator for the municipality being ruled by the Christian Democracy (DC). That party was dominant in Southern Italy over the period considered, both at the local and at the regional level. More importantly, the DC ruled the National government continuously over the decades observed. Therefore, controlling for the DC being in power at municipal level also accounts for possible alignment effects between the local and National government. Note that the DC dummy is a time-varying indicator, which can take value one or zero both before and after the 1971 reform[28]. Formally, we estimate the following equation:

[24] Recall that firm subsidies include both firm grants and concessional financing. The first are non-refundable contributions to firms, while the second are firm loans at a favourable interest rate. However, concessional financing has been introduced only in 1978; therefore, they do not enter our dependent variable in the specification with restricted post-treatment period.

[25] Italian provinces are an intermediate level of government between municipalities and regions, which tend to be politically and economically homogeneous units.

[26] We repeated the estimations for public works excluding interventions on road infrastructures, and results are not affected by such correction. Estimates are available upon request. However, most public works - roads, but also aqueducts, land decontamination and recovery - are likely to spill-over neighbouring municipalities, so that we opt for province-level clustering.

[27] For completeness, in Table A3.3 of the Appendix we also report estimates with standard errors clustered at municipal level.

[28] Conversely, alignment can take value one only since 1972 based on alignment status defined in 1970. The inclusion or exclusion of the DC dummy variable does not significantly alter our results, which mitigates possible concerns about multicollinearity. Its addition is aimed to control for a DC-specific effect, since this was the dominant party in Southern Italy and it also ruled the National government in the period observed. Apparently, over the whole span considered, having the DC in power at local level did non granted a specific advantage in the allocation of CasMez's funds.

$$y_{it} = \alpha + \beta\, Alignment_{it} + DC_{it} + \gamma_i + \delta_{rt} + \epsilon_{it} \tag{3.2}$$

The specification with restricted post-treatment period maximises internal validity. However, we are also interested in observing whether alignment effects are present over the entire post-reform period, up until the end of the CasMez (i.e. 1984). To gain some insights on the possible role for endogenous re-election probability, we check if the funds received in the years following the reform influence the chances of remaining (un)aligned in the first electoral turn, namely in 1975. We sum the number and amount of funds over the years 1972-1974, and construct a dummy for 're-alignment', taking value one if the municipality remains aligned with the regional government in 1975. Then, we conduct a cross-section analysis regressing the re-alignment dummy separately on the (cumulative) number and amount of funds received over the span 1972-1974. We also include in the specification region fixed effects. Table A3.2 in the Appendix reports the related results. Both the estimated coefficients appear non-significant, suggesting that - at least in the first years after the reform - endogeneous re-election probability should not represent a major concern. Therefore, we relax the above requirements and check if our results hold for the whole period 1960-1984.[29]

In the full-period specification, $Alignment_{it}$ is defined as before, with the difference that we follow the municipality as long as it does not change its treatment status with respect to the situation in 1970. If a municipality modifies its alignment status, for that municipality we exclude all years after the change in alignment status. Thus, in this specification we have post-treatment periods of different length in our sample, depending on the duration of the (un)alignment between local and regional governments. By doing so, we avoid forcing our treatment to be an absorbing status. This would imply - for example - considering aligned municipalities even when they experience changes of local or regional running party. As a robustness, we employ the estimation method proposed by De Chaisemartin

[29] We further estimate cross-region specifications, so to check which source of variation we leverage. We compare and discuss cross- versus within-region estimates in the results section.

and D'Haultfoeuille (2022), which allows for treatment switching on and off. In this way, we avoid discarding observations when municipalities change alignment status. More details on this alternative definition of treatment are reported in the robustness section.

The key assumption underlying TWFE estimations is the existence of a parallel trend in outcome evolution between treated and control units prior to the treatment event. This would suggest that no confounding factor is inducing selection into treatment, and that no anticipation effect is present.[30] To check the validity of these assumptions in our setting, we perform two complementary exercises. First of all, we conform to the practice of estimating event studies to inspect the pre-event coefficients of the related event study plots, and verify the absence of pre-trends. Indeed, the non-significance of pre-treatment coefficients can be interpreted as evidence of no systematic difference in outcome evolution between treated and controls prior to treatment. Alternatively, it can be read as a confirmation of validity of the parallel trend assumption. Therefore, we estimate the following event study regression:

$$y_{it} = \alpha + \Sigma_{m=-G}^{M} \beta_m \, z_{i(t-m)} + DC_{it} + \gamma_i + \delta_{rt} + \epsilon_{it}, \tag{3.3}$$

where the term $\Sigma_{m=-G}^{M} z_{i(t-m)}$ refers to the set of dummy variables indicating leads and lags with respect to the event of treatment. Recall that, in our setting, the 'event' coincides with 1972, when the governance reform of the CasMez becomes effective. According to equation 3.3, the reform can affect the outcome up until M periods after and G periods before (if one can date known anticipation effects). In our specification of equation 3.3, we include all the available pre/post-reform periods from 1960 to 1984, i.e. twelve years before and after the reform.[31] Given our treatment definition, we do not expect any anticipation effect, and therefore assume $G = 0$. Secondly, we also implement a placebo estimation, focusing on

[30] For a discussion of these assumptions and related verification strategies, see as a reference Freyaldenhoven et al. (2021).

[31] However, due to the reduced number of post-treatment observations, we group time periods 1980-1984 and - symmetrically - 1960-1964. Therefore, our event study plots display time windows of 8 years before and after the reform. See the next section for more details.

the period before 1971 and artificially anticipate treatment status to the span 1965-1970. That placebo treatment takes value one if the municipality is effectively treated starting from 1972. If our main estimates are truly capturing the effect of the reform, we should not detect any significance of that placebo treatment before the reform was implemented.

3.6 Main results

We start by presenting results from equation 3.1 estimated on the restricted time span 1960-1974. We regress the outcomes of interest against the alignment dummy which constitutes our treatment, together with municipality and region-year fixed effects (i.e. TWFE estimation), and we cluster standard errors at province-level.[32] Table 3.3 reports the estimated coefficients.

We detect significantly positive effects for the total number of projects approved and the (log) per-capita amount of funding received. The effect is driven by firm subsidies rather than public works. Indeed, if we distinguish by type of funding, the coefficients on firm subsidies are significant and comparable in size to the overall effect, while those on public works are not. Looking at coefficients' magnitude, partisan alignment increases the number of projects approved by 0.64 and the per-capita funding received by 5.3%.[33] These findings suggest

[32] In Table A3.3 of the Appendix, we report results from an analogous estimation with standard errors clustered at municipal level. Moreover, Table A3.4 presents results from an analogous estimation excluding Sicily and Sardinia. These two special status regions had regional governments already since the '50s; therefore, they could have experienced local-regional alignment dynamics before the reform. However, prior to 1971, regional governments had no formal authority over the allocation of CasMez funds. Symmetrically, we also report in Table A3.5 results for the same estimation, focusing only on Sicily and Sardinia. Indeed, on the one side, one could argue that newborn jurisdictions are more susceptible to the pressures by local lobbies; on the other side, patronage ties could need time to consolidate, and thus tactical redistribution may be stronger among older jurisdictions. Table A3.5 shows that the effect on the number of projects approved is confirmed; whereas, per-capita funding becomes non-significant. Our interpretation is that the non-significant result on per-capita funding is due to the large amounts of firm subsidies granted in the pre-reform period to few municipalities in Sicily and Sardinia. Indeed, in the '60s Porto Torres and Assemini in Sardinia, and Gela and Melilli in Sicily attracted considerable amounts of firm subsidies. These were all places chosen for the development of the petrochemical sector, which was heavily promoted by the CasMez in the '60s.

[33] Over the pre-reform period, the average number of project approvals by municipality-year is 2. Therefore, a 0.64 increase amounts to 32% growth.

Table 3.3: TWFE estimation (1960-1974)

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignement	0.639***	0.462***	0.177	0.053**	0.039**	0.015
	(0.1742)	(0.1255)	(0.1354)	(0.0229)	(0.0180)	(0.0139)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.660	0.601	0.441	0.317	0.304	0.185
N	5610	5610	5610	5610	5610	5610

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for municipality and region-year fixed effects. Here we restrict the post-treatment period to the first legislature after the CasMez reform, up until 1974.

that the devolution of authority brought about by the 1971 reform fostered dynamics of tactical redistribution, which favoured municipalities ruled by the party in power at regional level. The effect is mostly due to firm subsidies, rather than public works. This result points to possible patronage and pork-barrel politics, triggered by the institutional shift. Indeed, compared to public works, firm subsidies can be more easily distributed to acquire the consensus of local lobbies. Moreover, they can also be parcelled out more flexibly and assigned in a less visible way.

We then repeat the estimation adding the indicator for the Christian Democracy (DC) ruling the local government. Recall that this is a time-varying dummy taking value one in the years when the DC was in power at local level, before and after the CasMez reform. Note also that the DC_{it} indicator controls for possible alignment effects between the local and National tier of government, since in the whole period observed the National government was ruled by the Christian Democracy. Table 3.4 reports the estimated coefficients.

Table 3.4: TWFE estimation (1960-1974): controlling for Christian Democracy ruling the municipality

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.631***	0.442***	0.189	0.050**	0.034*	0.018
	(0.1761)	(0.1185)	(0.1461)	(0.0247)	(0.0202)	(0.0134)
DC	0.019	0.044	-0.025	0.006	0.010	-0.008
	(0.0785)	(0.0694)	(0.0550)	(0.0117)	(0.0114)	(0.0069)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.660	0.601	0.441	0.317	0.304	0.185
N	5610	5610	5610	5610	5610	5610

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and region-year fixed effects.

Results are not substantially affected by the addition of that control. Notice also that the coefficient for the DC_{it} indicator is never significant. Despite its dominant position, being governed by the DC seems not to play a prominent role in funds distribution once we control for partisan alignment. In other words, it was not the party *per se* to affect funds allocation, but rather its alignment with the upper-tier of government. Moreover, this result is also informative of the fact that being aligned with the National government did not grant significant advantages over the span 1960-1974. In other words, looking at the CasMez experience before and after the 1971 reform, it seems that the National tier of government did not engage in significant tactical redistribution[34].

Next, we investigate whether our results hold over the entire span 1960-1984, up to the end of the CasMez programme. Recall that in this full-period specification, we follow the municipality as long as it does not change its alignment status with respect to the situation of 1970. If a municipality experiences a change in alignment status, for that municipality we exclude all years after the change. Table 3.5 reports the related coefficients. Results are largely comparable to those obtained for the restricted time period. The effect on total amount of per-capita funding becomes slightly non-significant. However, our key finding that tactical redistribution is driven by firms subsidies is confirmed.

Table 3.5: TWFE estimation (1960-1984)

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.519***	0.451**	0.068	0.027	0.020*	0.010
	(0.1838)	(0.1790)	(0.0838)	(0.0163)	(0.0120)	(0.0085)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.656	0.622	0.416	0.313	0.308	0.165
N	8005	8005	8005	8005	8005	8005

Standard errors in parentheses clustered at province level * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and region-year fixed effects.

[34] We also explore possible alignments effects - prior to the reform - between local and National governments. Essentially, we regress number and amount of CasMez funds on the DC_{it} dummy over the period 1960-1971 (i.e. pre-reform). We include municipality and region-year fixed effects, and cluster standard errors at province level. Table A3.6 reports the related results. Only estimates on the number of project approvals - total and firm subsidies - appear significant. Looking at coefficients' size, these are one-third/half compared to our main estimates in Table 3.4. These findings suggest that some alignment effects were already present before 1971; however, the devolution process brought about by the governance reform exacerbated moral hazard incentives and thus tactical redistribution.

Finally, in Table A3.7 of the Appendix, we report estimates for the corresponding cross-region analysis. In that specification, we exploit treatment variation coming both from the local and regional tier of government, whereas in our main estimations regional electoral shifts are cleaned out by region-year fixed effects. Estimated coefficients are stable across the two specifications (i.e. cross- versus within-region). This pattern of results suggests that we mostly leverage within-region variation in alignment status, consistently with the little variability we observe in the parties ruling regional governments over the period considered.

3.6.1 Event studies and Placebo

To check the validity of the assumptions underlying our estimation, we run event study regressions (equation 3.3) for all our outcomes, in order to inspect the possible presence of pre-trends. Moreover, we also focus on the period 1960-1971 (i.e. pre-reform), and assign to later-treated municipalities a 'placebo alignment' status. That anticipated treatment takes value one from 1965 to 1971 for municipalities that - at the creation of regions in 1970 - were effectively aligned with the newborn regional government.

We report in the Appendix (Figures from A3.8 to A3.11) the event study plots corresponding to separate estimations of equation 3.3. Recall that the event time coincides with 1972, when the governance reform becomes effective. We report a symmetric time window of eight periods before and after 1972. We group coefficients corresponding to years 1960-1964 and 1980-1984, because - due to our definition of treatment - we just have a 1.79% of treated observations contributing to 1980-1984 estimates (see Table A3.8 in the Appendix). If we inspect the estimated coefficients in the pre-reform period, they are almost never significant at 90% confidence levels. More generally, no relevant pre-trend can be observed in the years preceding the reform. As for the post-reform period, it can be noticed a clear rise in the number of projects approved (Figures A3.8 and A3.9), mostly due to firm subsidies and concentrated in the first years following the reform. Looking at per-capita funding (Figures A3.10 and A3.11), we detect considerable variance in our estimates, which enlarges confidence intervals. That broad variance is due to the high variability in the amount of subsidies granted to firms, consistently with the flexibility of the instrument. However, post-reform coefficients appear generally larger than pre-reform ones, with a marked increase in the very first year from its implementation (i.e. 1972). These findings apply to the total amount of per-capita funding and firms subsidies, while almost no dynamics is detected for public works.

In Table 3.6, instead, we show the estimated coefficients for the placebo exercise we perform on the pre-reform period (1960-1971).

Table 3.6: TWFE estimation (1960-1971): Placebo alignment

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Placebo alignment	-0.108	0.048	-0.157**	-0.003	-0.000	-0.001
	(0.1198)	(0.1126)	(0.0591)	(0.0132)	(0.0119)	(0.0066)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.654	0.562	0.455	0.333	0.329	0.198
N	3740	3740	3740	3740	3740	3740

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of *placebo* alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Placebo alignment takes value one from 1965 to 1971 if the municipality was aligned with the regional government by 1970. We also control for the DC being in power at the local level and for municipality and region-year fixed effects.

The placebo treatment appears to be non-significant for all our outcomes, with the exception of number of public works approvals. This evidence provides some confidence in that our main estimation is indeed capturing the effect produced by the governance reform of 1971 and not some pre-existing dynamics, including anticipation effects. Concerning the number of public works approvals, the negative coefficient may suggest - if anything - a possible downward bias in our main estimates for this outcome.

3.7 Robustness

In this section we do a number of robustness tests. First of all, we challenge our definition of alignment employing the estimation method by De Chaisemartin and D'Haultfoeuille (2022), which allows treatment to switch on and off. Secondly, we further restrict our sample to municipalities with at least 12,000 residents in 1971 to check the sensitivity of our results to alternative threshold choices. Thirdly, we pre-process data through a combination of matching algorithms, so to run parametric estimations on a more balanced sample. Finally, we investigate the possible role for cross-border spillovers by adding a spatial lag to our specification.

3.7.1 Alternative alignment definition

We take advantage of the estimation method suggested by De Chaisemartin and D'Haultfoeuille (2022) and re-estimate event study specifications using their proposed STATA command *did_multiplegt*. This estimation strategy not only controls for possible treatment effect heterogeneity in settings with staggered adoption, but it also allows treatment to switch on and off. Therefore, we can employ a raw measure of alignment, which takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data.

We re-estimate the specification of equation 3.3 employing the method of De Chaisemartin and D'Haultfoeuille (2022) and obtain the event study plots reported in Figures from A3.12 to A3.15.[35]. These Figures plot the effect of first treatment change (i.e. becoming aligned

[35] To be precise, in this estimation we include region-specific non-parametric trends and not region-year fixed effects, which dramatically slow down the computation. The interpretation of coefficients should be equivalent.

for the first time) after t period.[36] On the horizontal axis, it is reported the relative time to the year when treatment first changes (i.e. $t = 0$).

The result of the event studies validate our research design. As for the number of project approvals, it is clearly visible an increase associated to the switch to alignment status. When distinguishing by type of funds, we get the confirmation that the increase is mostly due to firm subsidies, while the event study plot for public works is rather flat. Concerning, instead, per-capita funding, estimates are highly fuzzy, due to the high variability in the amount of firm subsidies. Nevertheless, we can still detect a rise in firm subsidies in coincidence with the switch into alignment, while we do not observe much variation for public works. In all these event study plots, the possible presence of pre-trends seems soundly ruled out.[37]

It is worth clarifying that the estimated coefficients have to be interpreted as 'intention-to-treat' effects of having received a weakly higher amount of treatment for t periods. However, they do not account for the number of switches into (out of) alignment that occur after the first one. To get an easier-to-interpret parameter, De Chaisemartin and D'Haultfoeuille (2022) propose to average these intention-to-treat estimates and divide them by the average of the corresponding first-stage estimates, obtained from an analogous regression where the outcome is replaced with the treatment itself. This first-stage regression reports the fraction of aligned municipalities when a group becomes aligned for the first time and in the following years.[38] That ratio can be interpreted as the average total effect per unit of treatment, where 'total' refers to the sum of instantaneous and dynamic effects. Alternatively, it gives us the difference between municipalities actual outcomes (i.e. funds received) and those they would have obtained if they had remained unaligned throughout 10 years after the first switch.

[36] Over the span considered, switchers are the 70% of the entire sample.

[37] Note that we are not normalising period -1 to zero. Post-treatment coefficients are still estimated in long-differences with respect to -1; while pre-treatment coefficients compare outcome evolution over pairs of consecutive periods, t periods before switchers switch. Such first-difference placebos are useful to investigate more closely the presence of pre-trends and possible anticipation effects.

[38] The corresponding graph is shown in Figure A3.16 of the Appendix. As it can be noticed, 20% of municipalities turns unaligned three years after the first alignment and, apparently, re-switching into treatment is not substantial in this setting. That switching off can possibly explain the drop in estimates observed in Figure A3.12 and in Panel a of Figure A3.13.

These average total effects are 0.870, for the overall number of project approvals, and 0.031, for the total amount of per-capita funding, which seem largely comparable to our main estimates of Table 3.4.[39]

3.7.2 Sample selection

In the main analysis, we focus on the 374 municipalities - among those interested by the CasMez - that in 1971 had at least 10,000 inhabitants. The choice is motivated, firstly, by the willingness to focus on electoral districts of a certain relevance, and by the fact that these larger municipalities received 69% of CasMez funding in the pre-reform period. This is consistent with the industrial composition of in- versus out-of-sample municipalities: those with less than 10,000 inhabitants display higher percentages of agricultural employment (see Figure A3.5). Out-of-sample municipalities were most likely rural places and thus represent a less suitable target for CasMez investments. Secondly, the considerable effort of collecting and digitalising a large amount of historical data (see an example of archival file in Figure A3.3) forced us to restrict the sample of analysis.

In the period we focus on, municipalities with less than 10,000 residents followed a majoritarian rule for mayor's election; while those with more than 10,000 residents elected - through proportional representation - the municipal council, which then expressed the mayor.[40] Our dataset consists only of municipalities in this second group, so that no differential electoral rule applies within the sample. For this group of municipalities, the municipal council effectively reflects parties vote shares, and therefore their actual electoral consensus at local level. Nevertheless, to verify that our results are robust to alternative population thresholds, we repeat our most complete specification (equation 3.2) focusing only on municipalities with more than 12,000 or 15,000 inhabitants in 1971.

[39] These estimates are automatically reported by the STATA command *did_multiplegt*. Distinguishing by type of funds, firm subsidies estimates are 0.688 - number of project approvals - and 0.038 - per-capita funding, whereas for public works, respectively, 0.182 and -0.007. This pattern of coefficients largely resembles the one of the main analysis.

[40] These differential electoral rules were established by the Presidential Decree of May 16 1960, n. 570.

We report in Table A3.9 the estimated coefficients for the sample of municipalities with at least 12,000 residents in 1971; those referring to the threshold of 15,000 inhabitants display an analogous pattern and are available upon request. If anything, coefficients are larger in size compared to our main estimates. More importantly, in this restricted sample, we find a significant and positive effect also on public works. We may interpret these findings as evidence of more relevant dynamics of distributive politics in more populated municipalities. This interpretation would not contradict our main results, but simply highlight heterogeneous effects by municipality size. However, we should also consider the possibility of a residual selection bias concern behind this pattern of results. It is possible that municipalities more populated in 1971 displayed a greater economic potential and were also more politically attractive than smaller ones. Therefore, they could have higher chances of attracting CasMez funds and, at the same time, higher probability of being aligned with the regional government. This would create an omitted variable bias which is not fully accounted for by municipality fixed effects nor by the verification of parallel trends assumption in the pre-reform period.

To rule out this possibility, we develop a robustness estimation procedure, which consists of matching aligned and unaligned municipalities based on a set of 1971 characteristics, and then run TWFE regressions on the matched sample. The next paragraph describes in more details this estimation strategy and reports the related results.

3.7.3 Matching + TWFE

As a robustness strategy, we pre-process data prior to parametric estimation through a combination of matching algorithms.[41] This is done to improve the balancing between treated and control municipalities with respect to some key covariates, mainly regarding population and industrial composition. Indeed, a possible residual concern from our main estimation

[41] According to Ho et al. (2007), pre-processing data can help to solve issues of model dependence, while extending matching to parametric estimation makes the identification more precise in case covariates balancing were not perfect. Therefore, the combination of non-parametric and parametric methods reduces the concerns arising from the separate application of each specific technique.

is that - after the reform - political parties could be more interested in controlling municipalities that were growing faster in terms of population and economic performance. If this were the case, we would have a selection bias that is not fully accounted for by municipal fixed effects. While the verification of parallel trends prior to the reform should exclude the possibility of selection bias before the institutional change, similar dynamics could also emerge in the post-reform period, influencing the probability of re-election. More specifically, our estimates would be *upward* biased if aligned municipalities were also those with highest industrial potential, and thus more likely to receive funds from the CasMez for development purposes. In addition, population growth may also positively affect both political attractiveness and funds allocation. Conversely, we may have a *downward* bias if most of the aligned municipalities were already dynamic centres and/or urban areas largely specialised in the services sector. These were not the targeted areas of the CasMez programme, whose main goal was to foster industrial growth in less developed territories.[42]

Matching treated and control units on pre-reform covariates can help to mitigate those concerns. For this reason, we select a set of variables from the 1971 Italian census, providing information on population and industrial composition. Our maintained assumption is that matching treated and control municipalities on 1971 characteristics can also account for possible differential trends in those covariates. In other words, matching on the selected set of variables should control for possible differences in the economic potential and population growth of treated and control municipalities. To implement our matching strategy, we proceed as follows. First of all, we divide our sample by 1971 population range (25th, 50th, and 75th percentiles), and impose exact matching on population class.[43] Then, within each population group, we compute the propensity score according to a list of key covariates, all measured in 1971. These are: population and firm density; share of employment in industry, construction and mining, and services; percentage of population with a high school

[42] For more details on the objectives and strategies of the CasMez, see section 3.3.
[43] This matching method is also known as Coarsened Exact Matching (CEM). As a reference, see Iacus et al. (2012).

degree; and an indicator for coastal municipality.[44] We run a Propensity Score Matching (PSM) with replacement, imposing a caliper of 0.035 standard deviations.[45] In this way, we match each treated municipality to one control unit in its same population class and with a propensity score which differs by no more than 0.035 standard deviations. Alternatively, we perform a nearest-neighbour-matching with replacement and caliper. By doing so, our sample is substantially restricted: we are left with 271 municipalities, among which only 66 controls.[46]

To check for the quality of the match, we perform a balancing test (*pstest*) on covariates. The matching procedure corrects for any significant misalignment in means. Moreover, looking at overall measures of covariates imbalance, both Rubins' B and Rubins' R are within the ranges prescribed.[47] Figure 3.4 reports the related graph for a visual intuition:

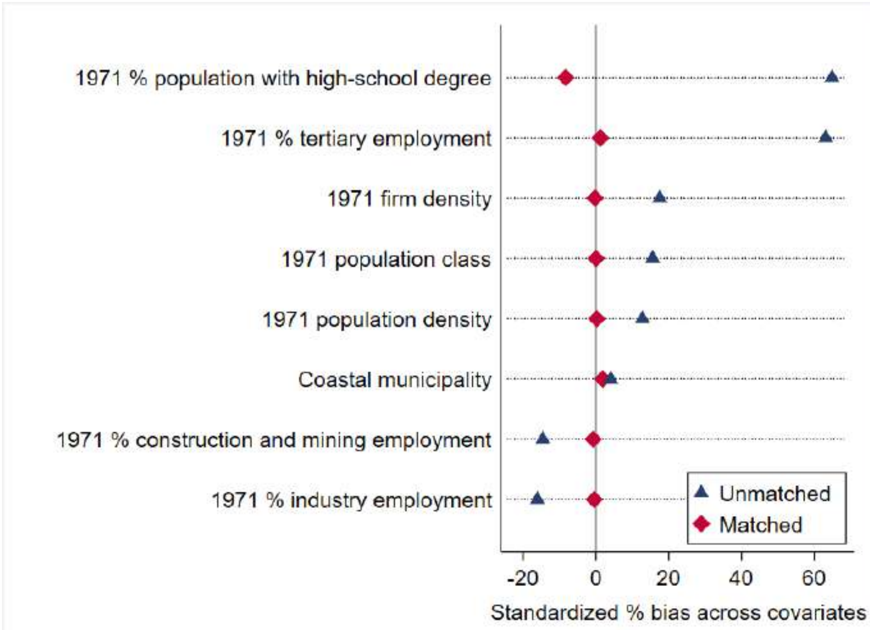
[44] We include the percentage of population with a high school decree as a proxy for the local endowments of skilled labour force. Moreover, the indicator for coastal municipality serves to account for the possible presence of the petrochemical sector, which constitutes an important component of CasMez investments.

[45] In order to achieve a good match, Dehejia and Wahba (1999) suggest to allow for replacement whenever the researcher has few control units in the sample, as in our case. At the same time, we also impose a caliper, so to fix a maximum difference in the propensity scores of matched units. By doing so, we employ the same municipality as a control for different treated units at most 18 times (see Table A3.10 in the Appendix). This combined choice is motivated by the need to achieve a good balance without excessively restricting the sample, which would decrease estimates efficiency (Ho et al., 2007).

[46] For further details on sample restriction, see in the Appendix Table A3.10 and the psgraph of Figure A3.17.

[47] Those measures correspond, respectively, to the absolute standardised difference of the means of the linear index of the propensity score in the treated and (matched) non-treated group, and to the ratio of treated to (matched) non-treated variances of the propensity score index. For further details, see Rubin (2001). See Table A3.11 in the Appendix.

Figure 3.4: Nearest-neighbour-matching: *pstest* graph



The Figure shows the degree of mean misalignment (standard % bias) in the distributions of key variables for treated and control municipalities, before and after the matching procedure.

For all covariates, we obtain a significant reduction in the standardised percentage bias. It is also worth underlining that - before matching - there existed a positive bias for the share of tertiary employment and population with high school degree; while a negative one for the employment share in industry, construction and mining. Connecting to the discussion above, this evidence suggests a possible downward bias in our main estimates. Indeed, according to the CasMez goals, funding should not be directed to areas already specialised in services and well equipped with educated labour force. At the same time, we can also observe a positive bias in population and population density. Following the above discussion, this could result in an upward bias for our previous estimates.

Therefore, we turn to parametric estimation on matched data. Table 3.7 reports the results for the TWFE estimation on the balanced sample, which includes matching weights in addition to the full list of regressors of our complete specification (equation 3.2).[48] These results are largely comparable with those presented in Table 3.5, suggesting that our findings are robust to the selection of a more balanced sample.

In conclusion, our robustness estimation confirm that being politically aligned with the regional government increases the probability of obtaining funds and the per-capita amount received. The effect is driven by firm subsidies, which points to possible patronage and pork-barrel dynamics. This further evidence provides additional support to our claim that the devolution of power to regional governments made relevant the political alignment between the local and regional government, fostering dynamics of tactical redistribution of CasMez investments.

Table 3.7: Robustness: TWFE estimation on matched data (1960-1984)

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.448**	0.420**	0.028	0.038	0.032*	0.007
	(0.1907)	(0.1821)	(0.0930)	(0.0227)	(0.0178)	(0.0087)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
Matching weights	✓	✓	✓	✓	✓	✓
R-squared	0.574	0.583	0.384	0.316	0.299	0.211
N	6170	6170	6170	6170	6170	6170

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and region-year fixed effects. The sample is restricted to those municipalities selected by the matching procedure. We further include matching weights to account for replacement.

[48] All treated municipalities in the balanced sample have weight equal to one. Control units, instead, can have weight from one to 18, depending on how many times they have been selected as controls by the matching algorithm (see Table A3.10 in the Appendix).

3.7.4 Spatial correlation

In all our analysis we cluster standard errors at province level, so to control for possible serial correlation in the error term induced by cross-border spillovers or by possible geographical concentration in voting patterns and funds allocation.

As an alternative check, we also include in the most complete specification (equation 3.2) a spatial lag for treatment. Essentially, we consider all the adjacent municipalities we have in our sample and sum their alignment indicators. In this way, the spatial lag captures the number of aligned municipalities surrounding a given unit in a given year.[49] Note that for the 39 municipalities for which we do not have neighbours in our sample, the spatial lag is always zero. Table A3.12 reports the related results. Estimates are robust to the inclusion of the spatial lag, while this latter is mostly non-significant. We interpret these findings as evidence of negligible cross-border effects. This check also helps to mitigate possible concerns related to sample selection, since it indirectly accounts for the spatial distribution of in-sample municipalities.

3.8 Long-run Economic Outcomes

We are also interested in observing whether the amount of CasMez funds granted in the aftermath of the 1971 reform produced any positive impact on local economic outcomes in the long run. In fact, the alignment effect we find may be compatible with the existence of some information or coordination advantage between tiers of government ruled by the same party. In other words, it is possible that regional governments favour politically aligned municipalities because they are more willing to provide useful information on local economic conditions or to collaborate with regional authorities in the realisation of projects. If so, there would be an economic rationale behind the mechanism of funds allocation that we

[49] We also tried with a dichotomous spatial lag, taking value one whenever at least one neighbour is aligned. Results are substantially the same and are available upon request.

label 'politically biased'.

To indirectly test this alternative explanation, we look at the impact on long-run economic outcomes of funds granted after 1971, as they are predicted by the alignment status of the municipality. Specifically, we focus on the period following the reform and collapse our dataset to a cross-section. Then, we estimate a TSLS model, where - in the first stage - we regress the funds received by a given municipality over the span 1972-1984 on a dummy taking value one if the local council has ever been aligned with the regional government over that period.[50] Moreover, we add to the specification municipal controls and region fixed effects[51]. In the second stage, we employ the predicted funds from the first stage as the main explanatory variable and investigate their effect on the change in local economic outcomes between 1971 and 1991. Formally, the first stage equation is:

$$\sum_{72-84} Funds_i = \alpha + \beta Ever\,aligned_i + \gamma Municipal\,controls_i + \delta_r + \epsilon_i \tag{3.4}$$

while the second stage:

$$y_{i,91} - y_{i,71} = \zeta + \eta \sum_{72-84} \hat{Funds_i} + \theta Municipal\,controls_i + \phi_r + \psi_i \tag{3.5}$$

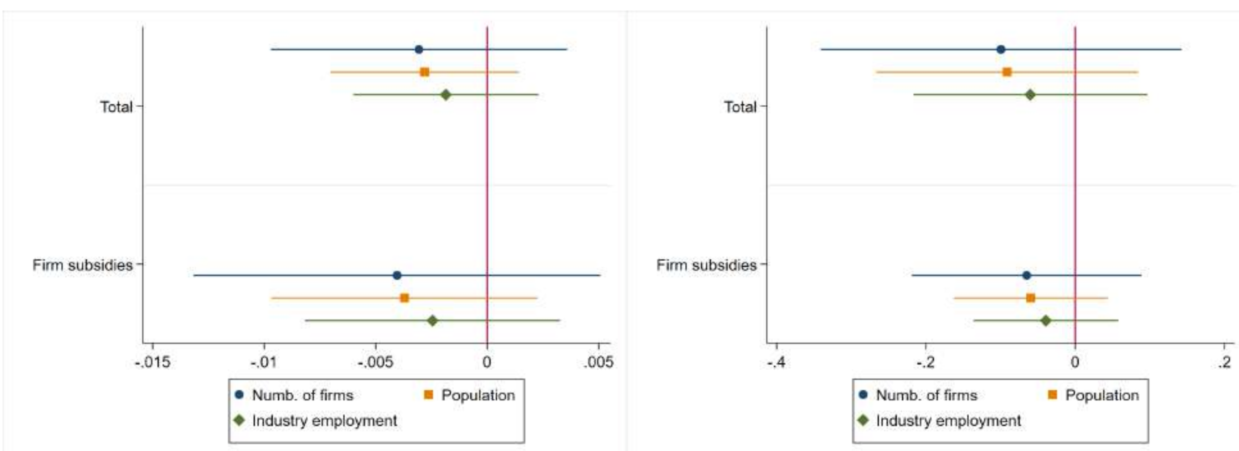
The variable $Funds_i$ refers either to the overall number of project approvals or to the total amount of per-capita funding (log), cumulated over the years 1972-1984. As local economic outcomes, we select a number of variables from the Italian censuses of 1971 and 1991. In the following, we report results for the following economic outcomes: growth rate of industrial employment, number of local firms, and resident population. We argue that industrial employment and the number of local firms are the most natural performance indicators, given the industrial development purpose of the programme. Moreover, resident population proxies for municipality's attractiveness as a whole. Figure 3.5 plots the estimated coefficients

[50] These represent 70% of our entire sample.
[51] Among municipal controls, we include land area, elevation, mountain land area, and two indicators for whether the municipality is a coastal or island one.

from the second stage equation 3.5. In Panel a, we use as measure of $Funds_i$ the number of project approvals, while in Panel b the amount of per-capita funding (log). In both panels, we report the estimated effect looking at the overall number of projects or per-capita funding (i.e. 'Total') and the one focusing specifically on firm subsidies. In no case we detect any positive effect. For this results, we do not make any claim of causality. However, we interpret these findings are suggestive of purely political reasons motivating the tactical redistribution of funds we observe in the second-half of the CasMez programme. Alternatively, the devolution process brought about by the 1971 reform triggered dynamics of distributive politics which did not produce any economic benefit. Most likely, the larger amounts of firm subsidies granted to aligned municipalities served to strengthen the local connections between elected politicians and entrepreneurs, feeding patronage and pork-barrel politics.

Figure 3.5: Long-run economic effects of distorted funds allocation

a. Number of project approvals b. Per-capita funding (log)



The Figure shows the second stage results of the TSLS estimates corresponding to equation 3.5. It reports the long-run effect - over the period 1971-1991 - of predicted CasMez funds on the growth rate of industrial employment, number of local firms, and resident population. CasMez funds are estimated from the first stage regression, and refer either to the number of project approvals (Panel a) or to the (log) amounts of per-capita funding (Panel b).

We cannot repeat an analogous exercise for the pre-reform period, since we do not have an equivalent instrument for that time span. However, we can inspect the correlation between the number and amount of funds received prior to the reform and local economic outcomes.

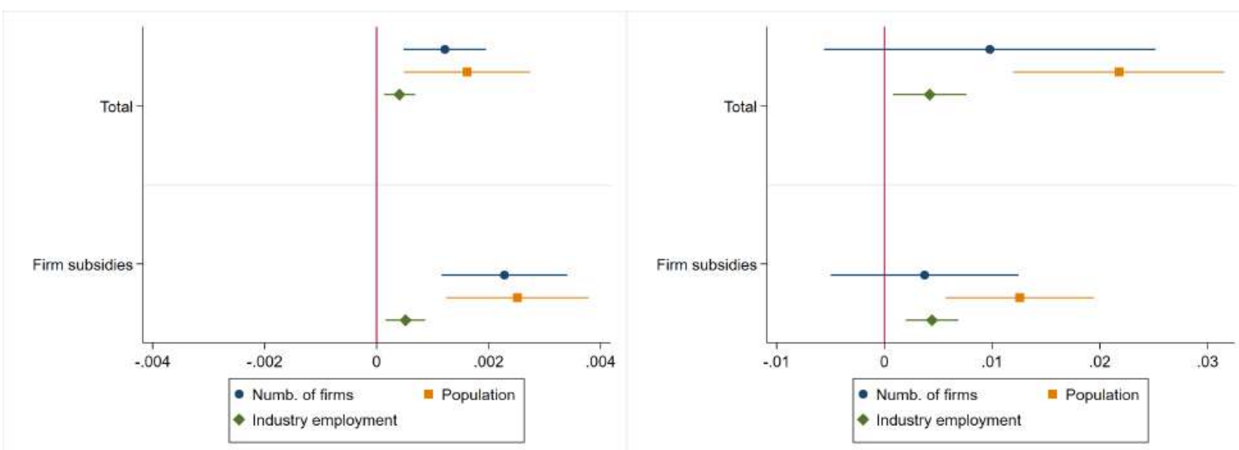
This is just a correlation analysis, but it may be helpful for a comparison with the results in Figure 3.5. We estimate the following regression:

$$y_{i,71} - y_{i,61} = \zeta + \eta \sum_{70-69} Funds_i + \theta Municipal\, controls_i + \phi_r + \psi_i \tag{3.6}$$

As for local economic outcomes, we are constrained to census years; therefore, we look at the growth rate over the decade 1961-1971. Regarding CasMez funds, we start summing from census year 1961 and stop in 1969, i.e. the year before the creation of Italian regions. Figure 3.6 plots the related coefficients, employing as dependent variable the growth rate in number of local firms, industrial employment, and resident population. Compared to zero estimates of Figure 3.5, here we find positive correlations between CasMez funds and local economic outcomes. There findings cannot be causally interpreted, but they do not exclude positive economic impacts of CasMez funding prior to the governance reform of 1971.

Figure 3.6: Long-run economic effects of funds allocation prior to 1971 reform

a. Number of project approvals b. Per-capita funding (log)



The Figure shows estimates from equation 3.6. It reports the long-run effect - over the period 1961-1971 - of CasMez funds on the growth rate of industrial employment, number of local firms, and resident population. CasMez funds refer either to the number of project approvals (Panel a) or to the (log) amounts of per-capita funding (Panel b).

3.9 Concluding remarks

In this paper we investigate whether devolution of authority over public investment can generate distributive politics dynamics, in the form of partisan alignment effects between different tiers of government. We focus on the Italian *Cassa per il Mezzogiorno*, a massive regional development programme implemented between 1950 and 1984, and exploit the quasi-experiment offered by the governance reform of 1971. We implement a Two-Way-Fixed-Effects estimation, employing as main explanatory variable an indicator taking value one since 1972 if the municipality was ruled by the same political party as the new born regional government.

First of all, we find that CasMez expenditure rose in the 1970s, both in terms of number of projects approved and amount of funding. Secondly, our main results support the hypothesis of tactical redistribution emerging from the devolution process brought about by the 1971 reform. After that institutional shift, municipalities ruled by the political party in power at the regional level obtained a higher number and amount of funds, compared to unaligned ones. The effect is driven by firm subsidies, rather than public works. This finding points to possible patronage and pork-barrel politics, triggered by the institutional shift. Indeed, compared to public works, firm subsidies can be more easily distributed to acquire the consensus of local lobbies.

The robustness analysis confirms that our estimates are not substantially affected by alternative treatment definition or sample selection choices. Moreover, pre-processing data through a combination of matching algorithms provides similar results. Similarly, the inclusion of a spatial lag to capture possible cross-border spillovers does not alter our estimates.

Finally, we explore the long-run economic effects of distorted funds allocation. We do not find any positive effect on the growth rate of industrial, number of local firms, and resident population. We interpret these findings as suggestive that the tactical redistribution of funds observed in the 1972-1984 period of the CasMez programme is driven by purely political, not economic, reasons. That biased allocation of funds did not produce any economic benefit, ruling out competing interpretations based on some economic rationale of political favouritism. Most likely, the larger amounts of firm subsidies granted to aligned municipalities served to strengthen the local connections between elected politicians and entrepreneurs, feeding patronage and pork-barrel politics. Conversely, in the decade prior to the 1971 reform, we find positive correlations between CasMez funds and local economic outcomes. These findings, far from being causal, do not exclude the possibility of positive economic impacts of CasMez interventions before the governance reform of 1971.

One note of caution should be made in interpreting these findings. We label 'partisan alignment' the political favouritism we highlight in the aftermath of the 1971 reform. Indeed, the devolution process created an incentive for regional government to allocate more resources to municipalities ruled by the same political party, i.e. aligned municipalities. However, in our sample we have little variability in the parties in power at the regional level, and we can not exclude that the tactical redistribution observed is actually due to the Christian Democracy governing the municipality after the reform. Even in that case, the bottom line message of the paper remains the same. In institutionally fragile settings, the devolution of authority can induce agency problems in the allocation of public investments. Specifically, intermediate tiers of governments can have the incentive to distribute public funds to achieve electoral consensus and consolidate their political power at the local level.

This evidence contributes to the literature on the trade-off between efficiency and corruption in delegation. Looking at public investments, we verify that intermediate tiers of government can be more exposed to the rent-seeking pressures of local lobbies. Thus, if assigned discretionary power over funds allocation, they can be induced to distribute government money to acquire electoral consensus. These political distortions may divert public resources from the declared goal of economic development, worsening programme's efficacy. Our findings speak also to the literature on distributive politics, showing that - in a given context - institutional design largely affects the political economy of public investment programmes. Specifically, our results suggest that tactical redistribution can be fostered by processes of devolution.

These considerations entail relevant policy implications for the design of regional development programmes and, more generally, of public investment projects. In institutional contexts characterised by weak local authorities and significant pressures by local lobbies, centralised management of public funds seems less exposed to the risk of resources misallocation, and thus it better safeguards the scope of the programme. Alternatively, rules should be preferred over discretion, so to mitigate the incentives of intermediate tiers of government to allocate funds for their electoral returns, diverting them from the programme's goals.

Bibliography

Albanese, G., DeBlasio, G., Incoronato, L., et al. (2021). Hooked on a subsidy: transfers and preferences for state intervention. Technical report, Gran Sasso Science Institute, Social Sciences.

Bandiera, O., Best, M. C., Khan, A. Q., and Prat, A. (2021). The allocation of authority in organizations: A field experiment with bureaucrats. *The Quarterly Journal of Economics*, 136(4):2195–2242.

Bandiera, O., Prat, A., and Valletti, T. (2009). Active and passive waste in government spending: evidence from a policy experiment. *American Economic Review*, 99(4):1278–1308.

Bardhan, P. and Mookherjee, D. (2006). Decentralization, corruption and government accountability. *International handbook on the economics of corruption*, 6(1):161–188.

Barone, G., David, F., and De Blasio, G. (2016). Boulevard of broken dreams. the end of eu funding (1997: Abruzzi, italy). *Regional Science and Urban Economics*, 60:31–38.

Barone, G. and Narciso, G. (2015). Organized crime and business subsidies: Where does the money go? *Journal of Urban Economics*, 86:98–110.

Bendor, J., Glazer, A., and Hammond, T. (2001). Theories of delegation. *Annual review of political science*, 4(1):235–269.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, 119(1):249–275.

Best, M. C., Hjort, J., and Szakonyi, D. (2017). Individuals and organizations as sources of state effectiveness. Technical report, National Bureau of Economic Research.

Bosio, E, S. D. E. G. and Shleifer, A. (2020). Public procurement in law and practice. *NBER Working Paper*, (27188).

Bracco, E., Lockwood, B., Porcelli, F., and Redoano, M. (2015). Intergovernmental grants as signals and the alignment effect: Theory and evidence. *Journal of Public Economics*, 123:78–91.

Bucciol, A., Chillemi, O., and Palazzi, G. (2013). Cost overrun and auction format in small size public works. *European Journal of Political Economy*, 30:35–42.

Buscemi, T. (2022). No revenge from places that don't matter? evidence from italy's cassa per il mezzogiorno. *Working Paper*.

Cafiero, S. (1996). *Questione meridionale e unità nazionale: 1861-1995*, volume 268. Carocci.

Cafiero, S. (2000). *Storia dell'intervento straordinario nel Mezzogiorno, 1950-1993*. Lacaita.

Carlitz, R. D. (2017). Money flows, water trickles: Understanding patterns of decentralized water provision in tanzania. *World Development*, 93:16–30.

Carozzi, F. and Repetto, L. (2016). Sending the pork home: Birth town bias in transfers to italian municipalities. *Journal of Public Economics*, 134:42–52.

Colussi, T., Lecce, G., Marco, M., and Onorato, M. G. (2022). The politics and economics of government aid: Evidence from the italian cassa per il mezzogiorno. *Working paper*.

Costabile, L. (2021). Alle origini della cassa per il mezzogiorno. il punto di vista degli economisti. *Rivista giuridica del Mezzogiorno*, 35(2-3):447–458.

D'Adda, G. and De Blasio, G. (2016). Historical legacy and policy effectiveness: the long-term influence of preunification borders in italy. *Journal of Regional Science*, 57(2):319–341.

D'Amico, L. (2021). Place based policies with local voting: Lessons from the eu cohesion policy. *Available at SSRN 3866098*.

De Angelis, I., de Blasio, G., and Rizzica, L. (2020). Lost in corruption. evidence from eu funding to southern italy. *Italian Economic Journal*, 6(2):355–377.

De Chaisemartin, C. and D'Haultfoeuille, X. (2022). Difference-in-differences estimators of intertemporal treatment effects. Technical report, National Bureau of Economic Research.

Decarolis, F., Fisman, R., Pinotti, P., and Vannutelli, S. (2020). Rules, discretion, and corruption in procurement: Evidence from italian government contracting. Technical report, National Bureau of Economic Research.

Dehejia, R. H. and Wahba, S. (1999). Causal effects in nonexperimental studies: Reevaluating the evaluation of training programs. *Journal of the American Statistical Association*, 94(448):1053–1062.

Dotti, N. F. (2016). Unwritten factors affecting structural funds: The influence of regional political behaviours on the implementation of eu cohesion policy. *European Planning Studies*, 24(3):530–550.

Felice, E. and Lepore, A. (2017). State intervention and economic growth in southern italy: The rise and fall of the 'cassa per il mezzogiorno'(1950–1986). *Business History*, 59(3):319–341.

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.

Gemmell, N., Kneller, R., and Sanz, I. (2013). Fiscal decentralization and economic growth: spending versus revenue decentralization. *Economic Inquiry*, 51(4):1915–1931.

Golden, M. and Min, B. (2013). Distributive politics around the world. *Annual Review of Political Science*, 16:73–99.

Golden, M. A. and Picci, L. (2008). Pork-barrel politics in postwar italy, 1953–94. *American Journal of Political Science*, 52(2):268–289.

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

Ho, D. E., Imai, K., King, G., and Stuart, E. A. (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political analysis*, 15(3):199–236.

Huber, J. D. and Shipan, C. R. (2002). *Deliberate discretion?: The institutional foundations of bureaucratic autonomy*. Cambridge University Press.

Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, 20(1):1–24.

Lavista, F. (2010). *La stagione della programmazione: grandi imprese e Stato dal dopoguerra agli anni Settanta*. il Mulino.

Lepore, A. (2013). *La Cassa per il Mezzogiorno e la Banca Mondiale: un modello per lo sviluppo economico italiano*. Rubbettino.

Nunes, F. (2013). Core voters or local allies? presidential discretionary spending in centralized and decentralized systems in latin america. *Presidential Discretionary Spending in Centralized and Decentralized Systems in Latin America (August 26, 2013)*.

Oates, W. E. (1993). Fiscal decentralization and economic development. *National tax journal*, 46(2):237–243.

Oates, W. E. (2005). Toward a second-generation theory of fiscal federalism. *International tax and public finance*, 12(4):349–373.

Persson, T. and Tabellini, G. (1994). Does centralization increase the size of government? *European Economic Review*, 38(3-4):765–773.

Persson, T. and Tabellini, G. (2002). *Political economics: explaining economic policy*. MIT press.

Pinotti, P. (2015). The economic costs of organised crime: Evidence from southern italy. *The Economic Journal*, 125(586):F203–F232.

Prud'Homme, R. (1995). The dangers of decentralization. *The world bank research observer*, 10(2):201–220.

Rodríguez-Pose, A. and Ezcurra, R. (2010). Is fiscal decentralization harmful for economic growth? evidence from the oecd countries. *Journal of Economic Geography*, 11(4):619–643.

Rubin, D. (2001). sing propensity scores to help design observational studies: Application to the tobacco litigation. *Health Services Outcomes Research Methodology*, 2:169–188.

Sapienza, P. (2004). The effects of government ownership on bank lending. *Journal of Financial Economics*, 72(2):357–384.

Saraceno, P. (1976). *Le radici della crisi economica*. Il Mulino.

Saraceno, P. (1986). *Il nuovo meridionalismo (Vol. 6).* Istituto italiano per gli studi filosofici.

Sbrescia, V. M. (2014). La parabola della cassa per il mezzogiorno a trent'anni dalla liquidazione (dpr 6 agosto 1984). *Rivista giuridica del Mezzogiorno*, 28(4):745–760.

Shleifer, A. and Vishny, R. W. (1993). Corruption. *The Quarterly Journal of Economics*, 108(3):599–617.

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

SVIMEZ (1981). *Rapporto 1980 sull'economia del Mezzogiorno.*

SVIMEZ (1982). *Rapporto 1981 sull'economia del Mezzogiorno.*

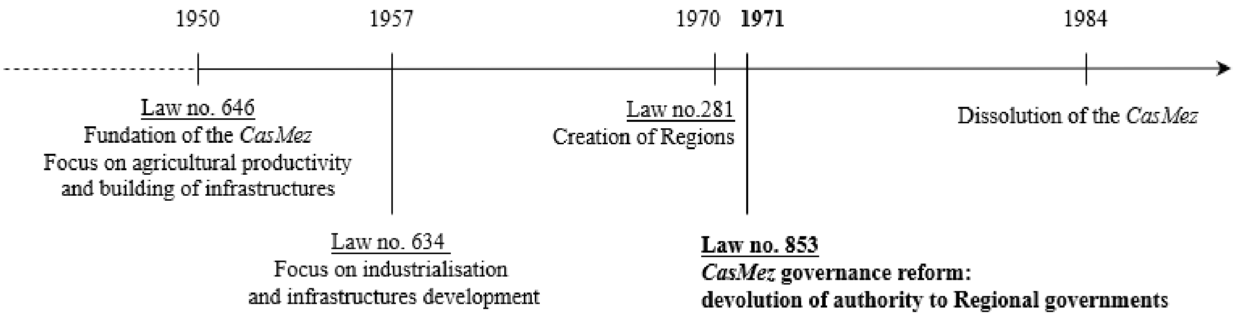
Svimez, R. (2019). L'economia e la società del mezzogiorno.

Tanzi, V. (1996). Review of some efficiency and macroeconomic aspects. In *Annual World Bank Conference on Development Economics 1995*, page 317. World Bank Publications.

Trigilia, C. (1992). *Sviluppo senza autonomia: effetti perversi delle politiche nel Mezzogiorno*, volume 54. il Mulino.

Appendix

Figure A3.1: Timeline of the CasMez programme

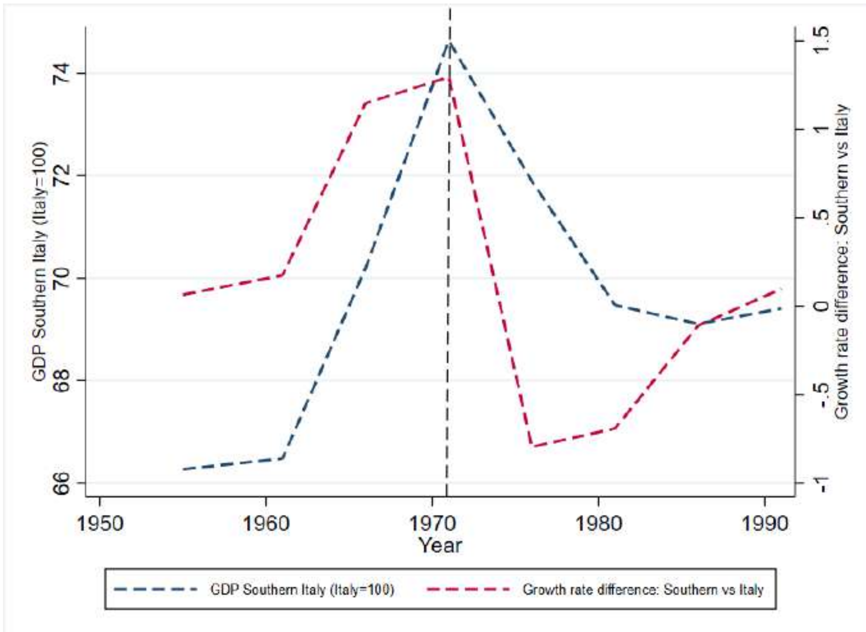


The Figure reports the key events and reforms of the CasMez programme.

Table A3.1: Italian parties acronyms and full names

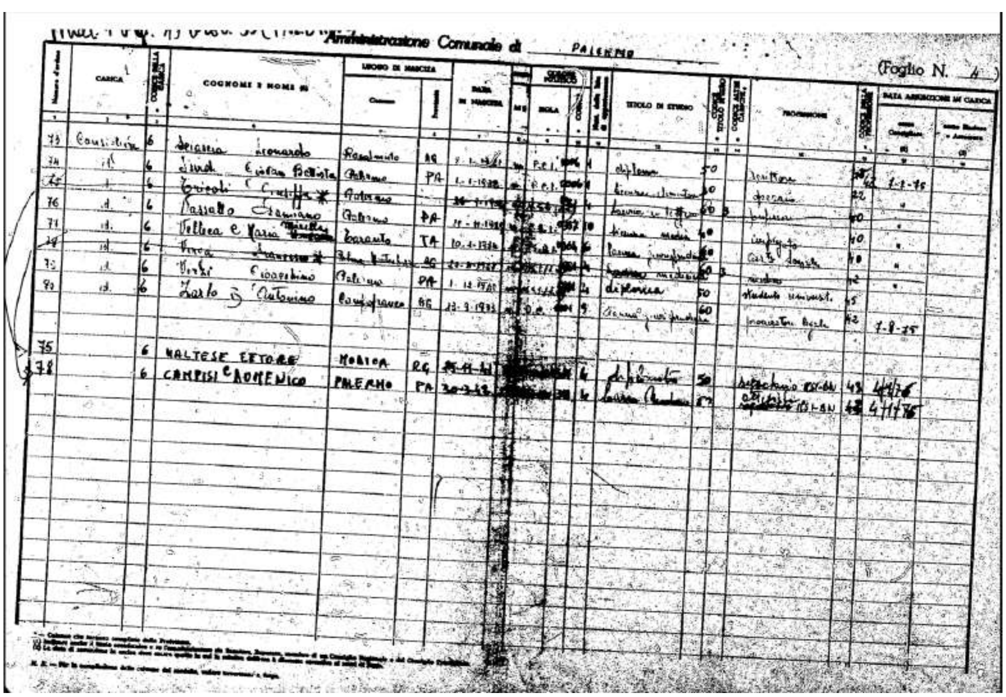
Acronym	Full Name
DC	Christian Democracy
MSI	Social Italian Movement
PCI	Italian Communist Party
PDIUM	Italian Democratic Party of Monarchical Unity
PLI	Liberal Italian Party
PRI	Republican Italian Party
PSDI	Italian Democratic Socialist Party
PSI	Italian Socialist Party
PSIUP	Italian Socialist Party of Proletarian Unity
PSU	Socialist Unitarian Party
USCS	Sicilian Christian Social Union

Figure A3.2: The evolution of North-South divide in Italy (1950-1990)



Source Buscemi (2022). The figure shows the evolution of Southern GDP and the difference in growth rates of Southern Italy and the rest of the country.

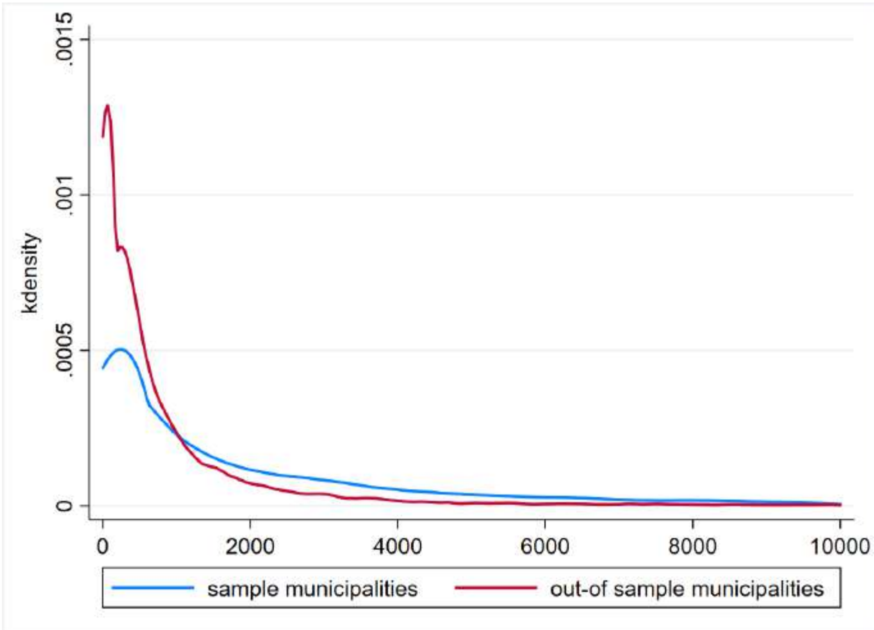
Figure A3.3: Example of archival file


Amministrazione Comunale di PALERMO

(Foglio N. 4)

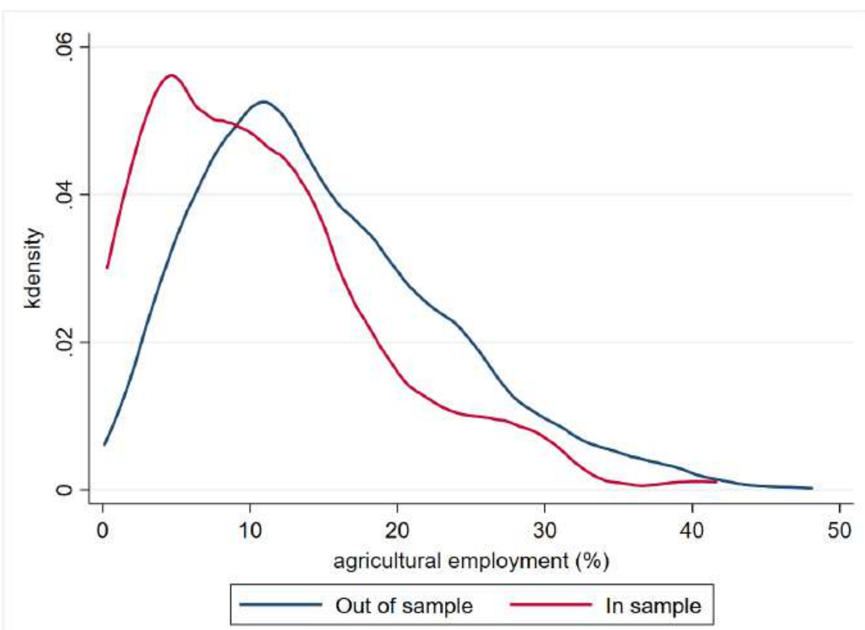
The Figure reports an example of original file with information on local administrators (municipality of Palermo) that we collected and digitalised.

Figure A3.4: Distribution of funds across in/out-of-sample municipalities



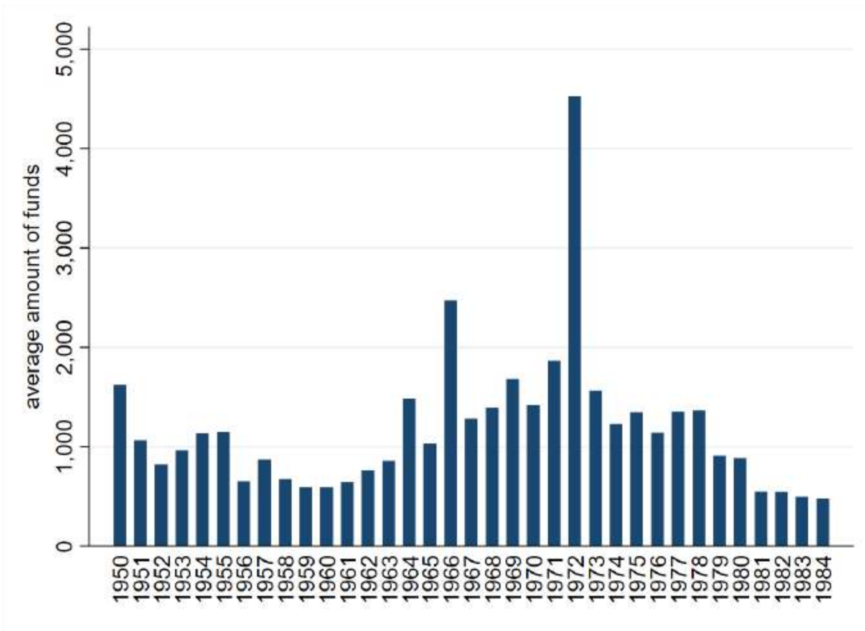
The horizontal axis reports the total amount of funds received by municipalities. That amount is expressed in thousands of liras and adjusted for inflation (at 2011 prices). We include in the computation funds for public works, non-refundable firm grants and concessional financing.

Figure A3.5: Agricultural employment (%) across in/out-of-sample municipalities



The Figure reports the distribution of agricultural employment (%) as of 1971 across in- and out-of-sample municipalities.

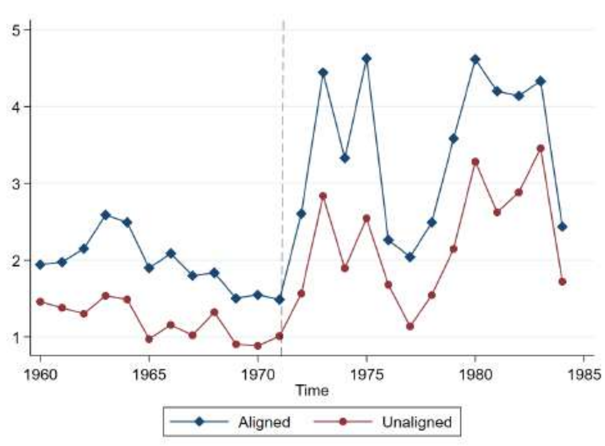
Figure A3.6: Time distribution of funds (1950-1984)



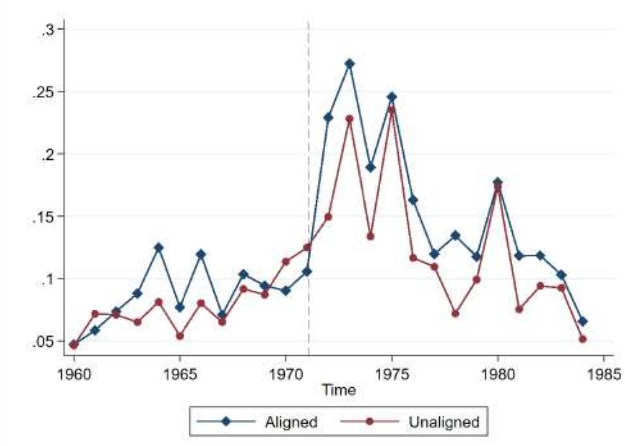
The (average) amount of funds is expressed in thousands of liras, adjusted for inflation (at 2011 prices). We include in the computation funds for public works, non-refundable grants and concessional financing.

Figure A3.7: Average funding by year: aligned and unaligned municipalities

a) Number of project approvals



b) Per-capita funding (log)



The Figure shows the average number of project approvals and per-capita funding by year, across aligned and unaligned municipalities. In Panel a, the unit of measure is the number of projects approvals. In Panel b, instead, the per-capita amount of funds received is expressed in log millions of liras, adjusted for inflation (at 2011 prices).

Table A3.2: Re-alignment probability and funds received: cross-section analysis (1975)

	Re-alignment probability (1975)	
Numb. of project approvals (1972-1974)	-0.001 (0.0025)	
Per-capita funding (log) (1972-1974)		-0.001 (0.0546)
region fe	✓	✓
R-squared	0.135	0.134
N	271	271

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports results from the cross-section analysis of the effect on re-alignment probability of the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Re-alignment refers to the first electoral turn after the CasMez reform, namely to year 1975; and it takes value one if the municipality maintains its alignment status. The number and amount of funds report the sums received by a given municipality over the span 1972-1974. We also include region fixed effects.

Table A3.3: TWFE estimation (1960-1974): municipality-clustered standard errors

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.639*** (0.2211)	0.462*** (0.1733)	0.177 (0.1263)	0.053** (0.0257)	0.039* (0.0212)	0.015 (0.0133)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.660	0.601	0.441	0.317	0.304	0.185
N	5610	5610	5610	5610	5610	5610

Standard errors in parentheses clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for municipality and region-year fixed effects.

Table A3.4: TWFE estimation (1960-1974): excluding Sicily and Sardinia

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.617***	0.375**	0.242	0.079***	0.061***	0.020
	(0.2136)	(0.1514)	(0.1923)	(0.0180)	(0.0163)	(0.0152)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.695	0.645	0.469	0.250	0.259	0.163
N	3840	3840	3840	3840	3840	3840

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for municipality and region-year fixed effects.

Table A3.5: TWFE estimation (1960-1974): Sicily and Sardinia only

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.681**	0.621**	0.060	0.005	-0.001	0.007
	(0.3111)	(0.2151)	(0.1315)	(0.0510)	(0.0397)	(0.0271)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.577	0.503	0.344	0.387	0.347	0.220
N	1770	1770	1770	1770	1770	1770

Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for municipality and region-year fixed effects.

Table A3.6: TWFE estimation (1960-1971): pre-reform alignment between local and National government

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
DC	0.195***	0.172**	0.024	0.015	0.014	-0.001
	(0.0707)	(0.0640)	(0.0552)	(0.0128)	(0.0122)	(0.0068)
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.648	0.573	0.429	0.316	0.300	0.188
N	4488	4488	4488	4488	4488	4488

Standard errors in parentheses clustered at municipal level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of being ruled by a Christian Democracy (DC) local council on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality, over pre-reform years 1960-1971. Since the National government was ruled by DC in that period, the DC coefficient captures alignment effects between local and National government prior to the reform. We also control for municipality and region-year fixed effects.

Table A3.7: TWFE estimation (1960-1984): cross-region analysis

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.457**	0.389**	0.067	0.029*	0.024**	0.010
	(0.1814)	(0.1791)	(0.0823)	(0.0153)	(0.0107)	(0.0085)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.649	0.613	0.404	0.302	0.298	0.165
N	8005	8005	8005	8005	8005	8005

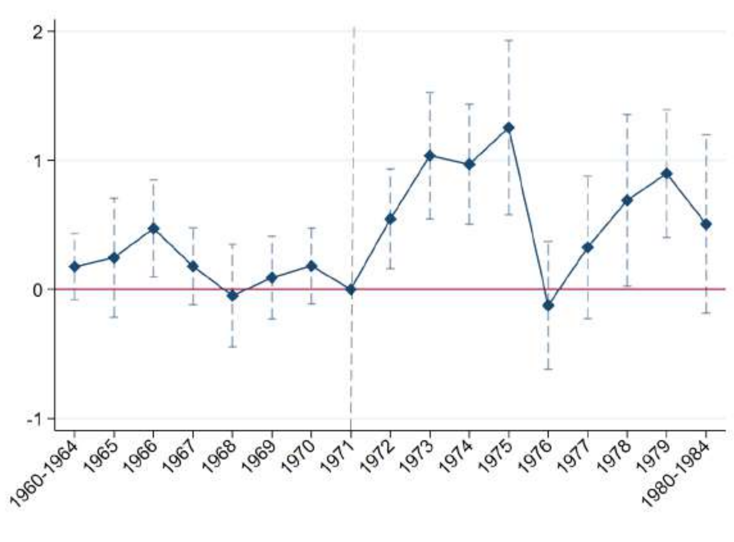
Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and year fixed effects.

Table A3.8: Number of treated observations by period

Relative time period		Year	Numb. of Treated obs.	%	% among non-missing
Valid	-12	1960	271	2.90	4.60
	-11	1961	271	2.90	4.60
	-10	1962	271	2.90	4.60
	-9	1963	271	2.90	4.60
	-8	1964	271	2.90	4.60
	-7	1965	271	2.90	4.60
	-6	1966	271	2.90	4.60
	-5	1967	271	2.90	4.60
	-4	1968	271	2.90	4.60
	-3	1969	271	2.90	4.60
	-2	1970	271	2.90	4.60
	-1	1971	271	2.90	4.60
	0	1972	271	2.90	4.60
	1	1973	271	2.90	4.60
	2	1974	271	2.90	4.60
	3	1975	197	2.11	3.35
	4	1976	197	2.11	3.35
	5	1977	197	2.11	3.35
	6	1978	197	2.11	3.35
	7	1979	197	2.11	3.35
	8	1980	167	1.79	2.84
	9	1981	167	1.79	2.84
	10	1982	167	1.79	2.84
	11	1983	167	1.79	2.84
	12	1984	167	1.79	2.84
	Total		5885	62.94	100.00
Missing	.		3465	37.06	
Total			9350	100.00	

The Table reports, for each year, the number and percentage of treated municipalities employed in the TWFE estimation over the period 1960-1984 (see Table 3.5).

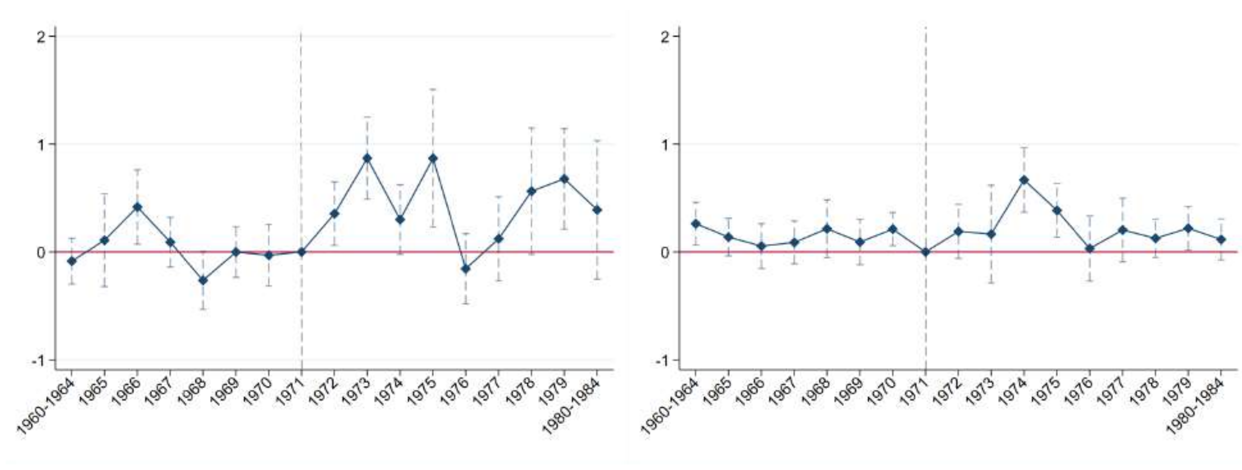
Figure A3.8: Event study plot: number of project approvals (total)



The Figure shows the event study estimates corresponding to equation 3.3. It reports the dynamic effect of partisan alignment as of 1970, provided that the municipality has not changed alignment status afterwards. We take as reference year 1971, when the CasMez reform was implemented. The outcome is the total number of project approvals. Due to the limited number of observations between 1980-1984, we estimate the average effect across those years, and - symmetrically - across 1960-1964. We report 90% level confidence intervals.

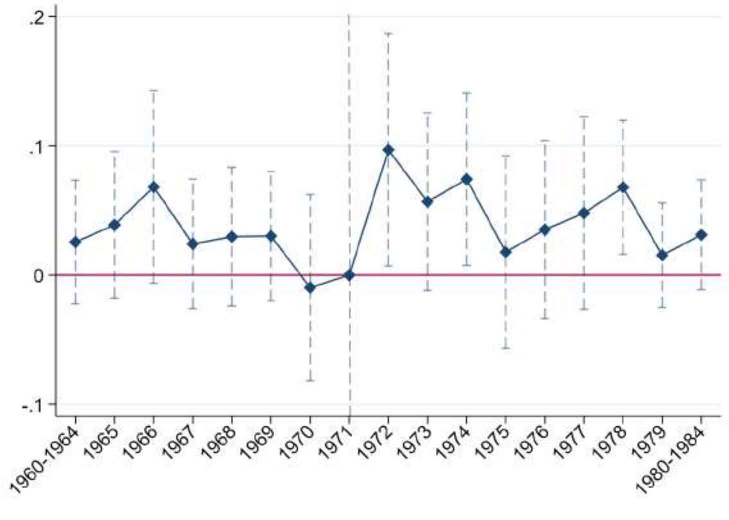
Figure A3.9: Event study plot: number of project approvals

a. Firm subsidies | b. Public works



The Figure shows the event study estimates corresponding to equation 3.3. It reports the dynamic effect of partisan alignment as of 1970, provided that the municipality has not changed alignment status afterwards. We take as reference year 1971, when the CasMez reform was implemented. The outcome is the number of project approvals, respectively focusing on firm subsidies (Panel a) and on public works (Panel b). Due to the limited number of observations between 1980-1984, we estimate the average effect across those years, and - symmetrically - across 1960-1964. We report 90% level confidence intervals.

Figure A3.10: Event study plot: per-capita funding (total)

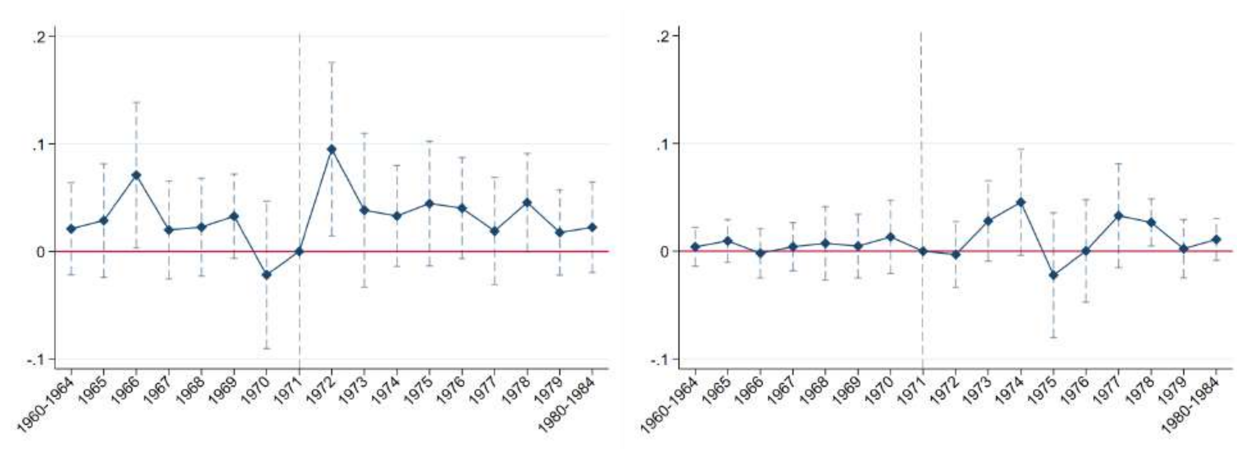


The Figure shows the event study estimates corresponding to equation 3.3. It reports the dynamic effect of partisan alignment as of 1970, provided that the municipality has not changed alignment status afterwards. We take as reference year 1971, when the CasMez reform was implemented. The outcome is the total (log) amounts of per-capita funding received, expressed in millions of liras at 2011 prices. Due to the limited number of observations between 1980-1984, we estimate the average effect across those years, and - symmetrically - across 1960-1964. We report 90% level confidence intervals.

Figure A3.11: Event study plot: per-capita funding

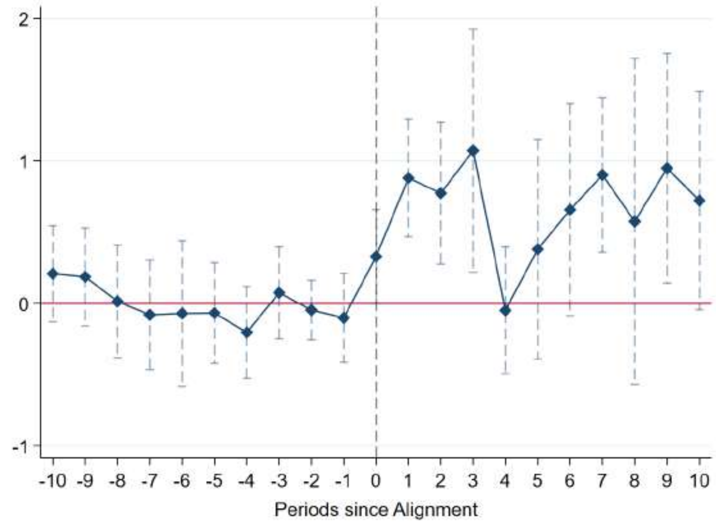
a. Firm subsidies

b. Public works



The Figure shows the event study estimates corresponding to equation 3.3. It reports the dynamic effect of partisan alignment as of 1970, provided that the municipality has not changed alignment status afterwards. We take as reference year 1971, when the CasMez reform was implemented. The outcome is the (log) amounts of per-capita funding received (millions of liras at 2011 prices), respectively focusing on firm subsidies (Panel a) and on public works (Panel b). Due to the limited number of observations between 1980-1984, we estimate the average effect across those years, and - symmetrically - across 1960-1964. We report 90% level confidence intervals.

Figure A3.12: Event study plot: number of project approvals (total)

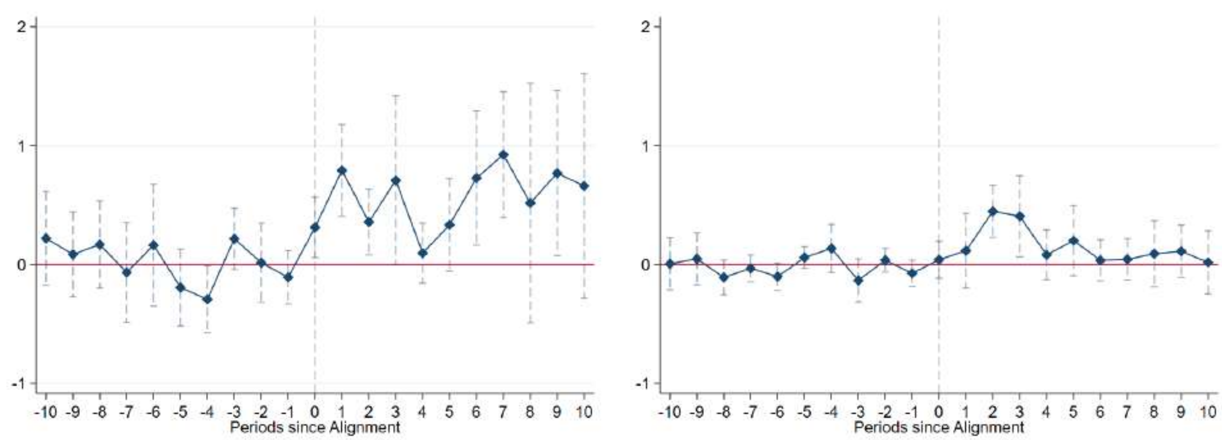


The Figure shows the event study coefficients estimated using the method by De Chaisemartin and D'Haultfoeuille (2022). It reports the dynamic effect of first partisan alignment 10 years before and after its start. Here, alignment takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data. The outcome is the total number of project approvals. We report 90% level confidence intervals.

Figure A3.13: Event study plot: number of project approvals

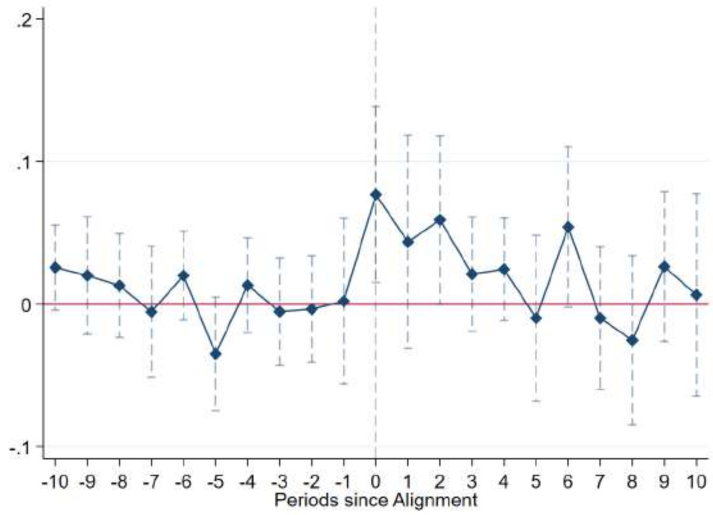
a. Firm subsidies

b. Public works



The Figure shows the event study coefficients estimated using the method by De Chaisemartin and D'Haultfoeuille (2022). It reports the dynamic effect of first partisan alignment 10 years before and after its start. Here, alignment takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data. The outcome is the number of project approvals, respectively focusing on firm subsidies (Panel a) and on public works (Panel b). We report 90% level confidence intervals.

Figure A3.14: Event study plot: per-capita funding (total)

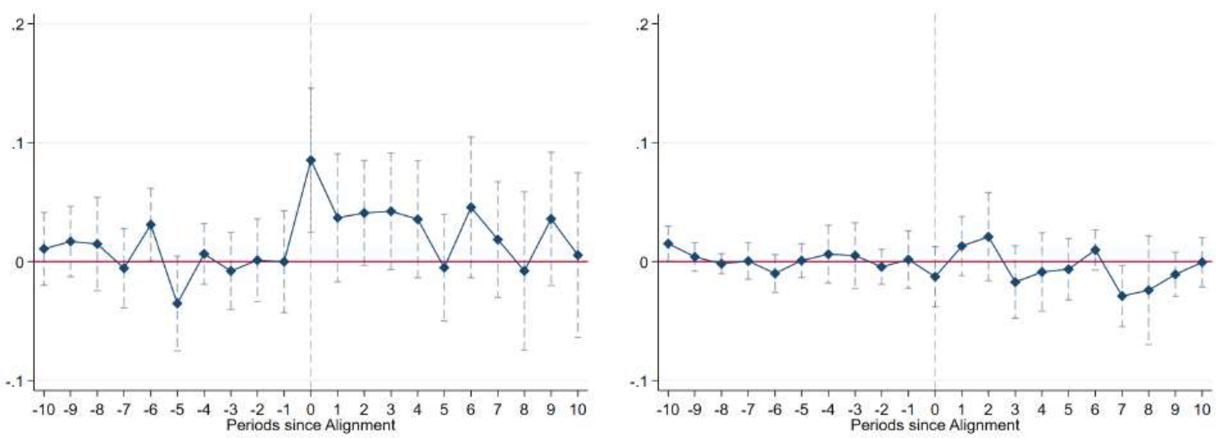


The Figure shows the event study coefficients estimated using the method by De Chaisemartin and D'Haultfoeuille (2022). It reports the dynamic effect of first partisan alignment 10 years before and after its start. Here, alignment takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data. The outcome is the total (log) amounts of per-capita funding received, expressed in millions of liras at 2011 prices. We report 90% level confidence intervals.

Figure A3.15: Event study plot: per-capita funding

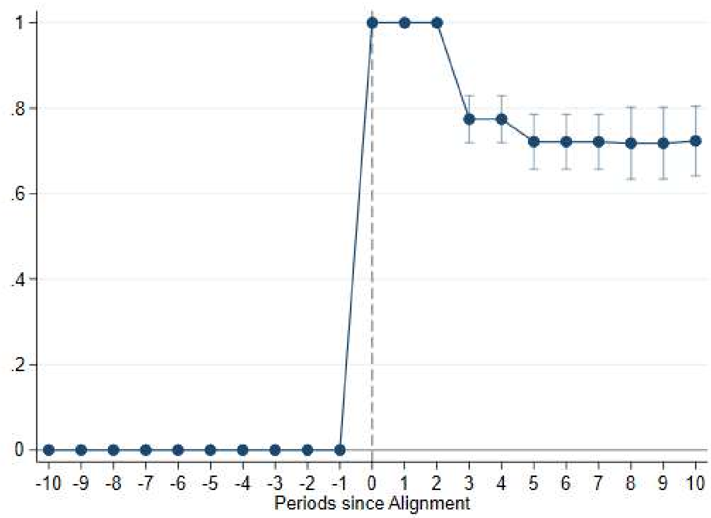
a. Firm subsidies

b. Public works



The Figure shows the event study coefficients estimated using the method by De Chaisemartin and D'Haultfoeuille (2022). It reports the dynamic effect of first partisan alignment 10 years before and after its start. Here, alignment takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data. The outcome is the (log) amounts of per-capita funding received (millions of liras at 2011 prices), respectively focusing on firm subsidies (Panel a) and on public works (Panel b). We report 90% level confidence intervals.

Figure A3.16: Event study plot: alignment status before and after first alignment



The Figure shows the event study coefficients estimated using the method by De Chaisemartin and D'Haultfoeuille (2022) and employing as dependent variable the dummy for alignment (referred to as 'first-stage'). It reports the dynamic effect of first partisan alignment 10 years before and after its start, on the treatment itself. Alternatively, it gives the fraction of municipalities that are aligned t periods before and after first alignment. Here, alignment takes value one - starting from 1972 - whenever local and regional government are ruled by the same party. In this way, we avoid discarding observations when the municipality changes alignment status and exploit all the available information in our data. We report 90% level confidence intervals.

Table A3.9: TWFE estimation: sample of municipalities with at least 12,000 residents in 1971

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.946***	0.763***	0.183*	0.052***	0.033**	0.023**
	(0.2503)	(0.2413)	(0.0911)	(0.0183)	(0.0131)	(0.0104)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.665	0.630	0.432	0.322	0.305	0.177
N	6220	6220	6220	6220	6220	6220

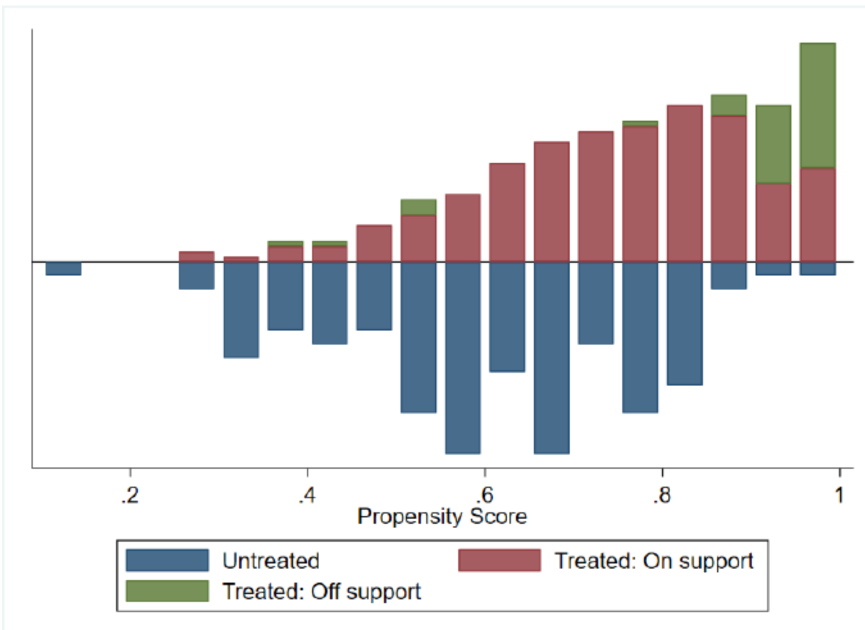
Standard errors in parentheses clustered at province level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and region-year fixed effects. Here we restrict the sample to municipalities with at least 12,000 residents as for 1971.

Table A3.10: Weights of matched controls

Weights of matched controls	Controls	Treated	Total
1	26	222	248
2	7	0	7
3	12	0	12
4	7	0	7
5	4	0	4
6	2	0	2
7	1	0	1
8	3	0	3
12	2	0	2
13	1	0	1
18	1	0	1
Total	66	222	288

The Table reports the weights assigned to control municipalities by the matching procedure. The maximum is 18, meaning that one municipality serves as control for 18 treated units based on nearest-neighbour-matching with a caliper of 0.035 standard deviations.

Figure A3.17: Nearest-neighbour-matching: psgraph



The Figure shows the psgraph corresponding to the matching (with replacement) procedure implemented. In green colour are depicted the treated municipalities for which we could not find an appropriate control (based on the propensity score value), and therefore excluded from the sample.

Table A3.11: Nearest-neighbour-matching: pstest table

Variable	Unmatched / Matched	Treated	Control	% bias	reduct % $\|bias\|$	t-test t	t-test p>$\|t\|$	V(T)/V(C)
1971 population density	U	7.9663	6.0939	12.8		1.05	0.295	1.64*
	M	6.6562	6.6254	0.2	98.4	0.02	0.983	1.05
1971 % industry employment	U	13.999	14.722	-16.1		-1.43	0.153	0.77*
	M	14.251	14.272	-0.5	97.1	-0.05	0.957	1.14
1971 % construction and mining employment	U	14.856	15.532	-14.6		-1.30	0.193	0.77*
	M	15.118	15.153	-0.8	94.8	-0.09	0.931	1.14
1971 % tertiary employment	U	14.378	11.434	63.1		5.10	0.000	1.95*
	M	13.48	13.424	1.2	98.1	0.12	0.902	1.07
1971 % population with high school degree	U	5.4271	4.1369	64.9		5.03	0.000	3.36*
	M	5.0441	5.2098	-8.3	87.2	-0.82	0.411	1.32*
Coastal municipality	U	.39852	.37864	4.1		0.35	0.726	.
	M	.37387	.36486	1.8	54.7	0.20	0.845	.
1971 firm density	U	.25283	.18352	17.5		1.44	0.150	1.61*
	M	.19974	.20082	-0.3	98.4	-0.03	0.975	0.95
1971 population class	U	2.5498	2.3786	15.6		1.32	0.187	1.16
	M	2.491	2.491	0.0	100.0	0.00	1.000	1.00
Sample	**Ps R2**	**LR chi2**	**p>chi2**	**MeanBias**	**MedBias**	**B**	**R**	**%Var**
Unmatched	0.085	37.33	0.000	26.1	15.8	73.5*	2.42*	86
Matched	0.006	3.40	0.907	1.6	0.6	17.5	1.29	14

The Table reports the output of the pstest, before and after the matching procedure. Looking at overall measures of covariates imbalance after matching, both Rubins' B and Rubins' R are within the ranges prescribed, namely B < 25%, R inside [0.5; 2]. Those measures correspond, respectively, to the absolute standardised difference of the means of the linear index of the propensity score in the treated and (matched) non-treated group, and to the ratio of treated to (matched) non-treated variances of the propensity score index. For further details, see Rubin (2001).

Table A3.12: Robustness: TWFE estimation with spatial lag (1960-1984)

	Numb. of project approvals			Per-capita funding (log)		
	Total	Firm subsidies	Public works	Total	Firm subsidies	Public works
Alignment	0.493**	0.428**	0.065	0.027*	0.020*	0.010
	(0.2264)	(0.2106)	(0.0761)	(0.0150)	(0.0118)	(0.0089)
Spatial lag	0.154	0.134*	0.019	-0.002	0.001	-0.001
	(0.1018)	(0.0720)	(0.0506)	(0.0063)	(0.0053)	(0.0028)
DC	✓	✓	✓	✓	✓	✓
Municipality fe	✓	✓	✓	✓	✓	✓
region-Year fe	✓	✓	✓	✓	✓	✓
R-squared	0.657	0.623	0.416	0.313	0.308	0.165
N	8005	8005	8005	8005	8005	8005

Standard errors in parentheses clustered at municipality level * p<0.10, ** p<0.05, *** p<0.01. The Table reports the effect of partisan alignment on the number and (log) per-capita amount (millions of liras at 2011 prices) of CasMez funds received by a given municipality. Partisan alignment is defined as of 1970, and can take value one starting from 1972 (i.e. after the CasMez reform). We also control for the DC being in power at the local level and for municipality and region-year fixed effects. We further include a spatial lag for the number of adjacent aligned municipalities.

Bibliography

Acconcia, A., Corsetti, G., and Simonelli, S. (2014). Mafia and public spending: Evidence on the fiscal multiplier from a quasi-experiment. *American Economic Review*, 104(7):2185–2209.

Albanese, G., DeBlasio, G., Incoronato, L., et al. (2021). Hooked on a subsidy: transfers and preferences for state intervention. Technical report, Gran Sasso Science Institute, Social Sciences.

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

Allcott, H. and Keniston, D. (2017). Dutch Disease or Agglomeration? The Local Economic Effects of Natural Resource Booms in Modern America. *The Review of Economic Studies*, 85(2):695–731.

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

Auricchio, M. (2015). Local Manufacturing Multiplier and Human Capital in Italian Local Labor Markets? *PhD thesis, Libera Universita Internazionale degli Studi Sociali Guido Carli*.

Autor, D. H., Dorn, D., and Hanson, G. H. (2013). The china syndrome: Local labor market effects of import competition in the united states. *American Economic Review*, 103(6):2121–68.

Bandiera, O., Best, M. C., Khan, A. Q., and Prat, A. (2021). The allocation of authority in organizations: A field experiment with bureaucrats. *The Quarterly Journal of Economics*, 136(4):2195–2242.

Bandiera, O., Prat, A., and Valletti, T. (2009). Active and passive waste in government spending: evidence from a policy experiment. *American Economic Review*, 99(4):1278–1308.

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.

Bardhan, P. and Mookherjee, D. (2006). Decentralization, corruption and government accountability. *International handbook on the economics of corruption*, 6(1):161–188.

Barone, G., David, F., and De Blasio, G. (2016). Boulevard of broken dreams. the end of eu funding (1997: Abruzzi, italy). *Regional Science and Urban Economics*, 60:31–38.

Barone, G. and Narciso, G. (2015). Organized crime and business subsidies: Where does the money go? *Journal of Urban Economics*, 86:98–110.

Bartik, T. J. (1991). Who benefits from state and local economic development policies? *W. E. Upjohn Institute for Employment Research.*

Baum-Snow, N. and Ferreira, F. (2015). Chapter 1 - causal inference in urban and regional economics. In Duranton, G., Henderson, J. V., and Strange, W. C., editors, *Handbook of Regional and Urban Economics*, volume 5 of *Handbook of Regional and Urban Economics*, pages 3–68. Elsevier.

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.

Belloc, M., Naticchioni, P., Naticchioni, P., and Vittori, C. (2019). Urban wage premia, cost of living, and collective bargaining. *CESifo Working Paper No. 7253, Available at SSRN: https://ssrn.com/abstract=3275427.* Spatial Dimensions of Labor Markets.

Bendor, J., Glazer, A., and Hammond, T. (2001). Theories of delegation. *Annual review of political science*, 4(1):235–269.

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.

Bertrand, M., Duflo, E., and Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *The Quarterly journal of economics*, 119(1):249–275.

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

Best, M. C., Hjort, J., and Szakonyi, D. (2017). Individuals and organizations as sources of state effectiveness. Technical report, National Bureau of Economic Research.

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

Blanchard, O. J. and Katz, L. F. (1992). Regional evolutions. *Brookings Papers on Economic Activity*, pages l–75.

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

Borusyak, K., Hull, P., and Jaravel, X. (2018). Quasi-experimental shift-share research designs. Working Paper 24997, National Bureau of Economic Research.

Bosio, E, S. D. E. G. and Shleifer, A. (2020). Public procurement in law and practice. *NBER Working Paper*, (27188).

Bound, J. and Holzer, H. J. (2000). Demand shifts, population adjustments, and labor market outcomes during the 1980s. *Journal of Labor Economics*, 18(1):20–54.

Bracco, E., Lockwood, B., Porcelli, F., and Redoano, M. (2015). Intergovernmental grants as signals and the alignment effect: Theory and evidence. *Journal of Public Economics*, 123:78–91.

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.

Bucciol, A., Chillemi, O., and Palazzi, G. (2013). Cost overrun and auction format in small size public works. *European Journal of Political Economy*, 30:35–42.

Buscemi, T. (2022). No revenge from places that don't matter? evidence from italy's cassa per il mezzogiorno. *Working Paper*.

Cadena, B. C. and Kovak, B. K. (2016). Immigrants equilibrate local labor markets: Evidence from the great recession. *American Economic Journal: Applied Economics*, 8(1):257–90.

Cafiero, S. (1996). *Questione meridionale e unità nazionale: 1861-1995*, volume 268. Carocci.

Cafiero, S. (2000). *Storia dell'intervento straordinario nel Mezzogiorno, 1950-1993*. Lacaita.

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

Card, D. (2001). Immigrant inflows, native outflows, and the local labor market impacts of higher immigration. *Journal of Labor Economics*, 19(1):22–64.

Card, D. (2009). Immigration and inequality. *American Economic Review*.

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

Carlitz, R. D. (2017). Money flows, water trickles: Understanding patterns of decentralized water provision in tanzania. *World Development*, 93:16–30.

Carozzi, F. and Repetto, L. (2016). Sending the pork home: Birth town bias in transfers to italian municipalities. *Journal of Public Economics*, 134:42–52.

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

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

Chernozhukov, V. and Hansen, C. (2008). The reduced form: A simple approach to inference with weak instruments. *Economics Letters*, 100(1).

Colussi, T., Lecce, G., Marco, M., and Onorato, M. G. (2022). The politics and economics of government aid: Evidence from the italian cassa per il mezzogiorno. *Working paper*.

Combes, P.-P., Duranton, G., and Gobillon, L. (2008). Spatial wage disparities: Sorting matters! *Journal of Urban Economics*, 63(2):723–742.

Combes, P.-P., Duranton, G., and Gobillon, L. (2010). The identification of agglomeration economies. *Journal of Economic Geography*, 11(2):253–266.

Conley, T. G. (1999). Gmm estimation with cross sectional dependence. *Journal of econometrics*, 92(1):1–45.

Costabile, L. (2021). Alle origini della cassa per il mezzogiorno. il punto di vista degli economisti. *Rivista giuridica del Mezzogiorno*, 35(2-3):447–458.

D'Adda, G. and De Blasio, G. (2016). Historical legacy and policy effectiveness: the long-term influence of preunification borders in italy. *Journal of Regional Science*, 57(2):319–341.

D'Amico, L. (2021). Place based policies with local voting: Lessons from the eu cohesion policy. *Available at SSRN 3866098*.

De Angelis, I., de Blasio, G., and Rizzica, L. (2020). Lost in corruption. evidence from eu funding to southern italy. *Italian Economic Journal*, 6(2):355–377.

De Blasio, G. and Menon, C. (2011). Local effects of manufacturing employment growth in italy. *Giornale degli Economisti e Annali di Economia*, pages 101–112.

De Chaisemartin, C. and D'Haultfoeuille, X. (2022). Difference-in-differences estimators of intertemporal treatment effects. Technical report, National Bureau of Economic Research.

De La Roca, J. and Puga, D. (2017). Learning by working in big cities. *The Review of Economic Studies*, 84(1):106–142.

Decarolis, F., Fisman, R., Pinotti, P., and Vannutelli, S. (2020). Rules, discretion, and corruption in procurement: Evidence from italian government contracting. Technical report, National Bureau of Economic Research.

Dehejia, R. H. and Wahba, S. (1999). Causal effects in nonexperimental studies: Reevaluating the evaluation of training programs. *Journal of the American Statistical Association*, 94(448):1053–1062.

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

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

Diamond, R. (2016). The determinants and welfare implications of us workers' diverging location choices by skill: 1980-2000. *American Economic Review*, 106(3):479–524.

Dix-Carneiro, R. and Kovak, B. K. (2019). Margins of labor market adjustment to trade. *Journal of International Economics*, 117:125–142.

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

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

Dotti, N. F. (2016). Unwritten factors affecting structural funds: The influence of regional political behaviours on the implementation of eu cohesion policy. *European Planning Studies*, 24(3):530–550.

Duranton, G. and Puga, D. (2001). Nursery cities: Urban diversity, process innovation, and the life cycle of products. *American Economic Review*, 91(5):1454–1477.

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.

Faggio, G. and Overman, H. (2014). The effect of public sector employment on local labour markets. *Journal of Urban Economics*, 79:91–107.

Felice, E. and Lepore, A. (2017). State intervention and economic growth in southern italy: The rise and fall of the 'cassa per il mezzogiorno'(1950–1986). *Business History*, 59(3):319–341.

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.

Gemmell, N., Kneller, R., and Sanz, I. (2013). Fiscal decentralization and economic growth: spending versus revenue decentralization. *Economic Inquiry*, 51(4):1915–1931.

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.

Golden, M. and Min, B. (2013). Distributive politics around the world. *Annual Review of Political Science*, 16:73–99.

Golden, M. A. and Picci, L. (2008). Pork-barrel politics in postwar italy, 1953–94. *American Journal of Political Science*, 52(2):268–289.

Goldsmith-Pinkham, P., Sorkin, I., and Swift, H. (2020). Bartik instruments: What, when, why, and how. *American Economic Review*, 110(8):2586–2624.

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

Helpman, E. and Krugman, P. R. (1985). *Market Structure and Foreign Trade: Increasing Returns, Imperfect Competition, and the International Economy.* Cambridge, MA: MIT Press.

Hlatshwayo, S. and Spence, M. (2004). Demand and defective growth patterns: The role of the tradable and non-tradable sectors in an open economy. *American Economic Review*, 104(5):272–77.

Ho, D. E., Imai, K., King, G., and Stuart, E. A. (2007). Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference. *Political analysis*, 15(3):199–236.

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

Huber, J. D. and Shipan, C. R. (2002). *Deliberate discretion?: The institutional foundations of bureaucratic autonomy*. Cambridge University Press.

Iacus, S. M., King, G., and Porro, G. (2012). Causal inference without balance checking: Coarsened exact matching. *Political Analysis*, 20(1):1–24.

ISTAT (2010). Nota metodologica sistemi locali del lavoro. *Statistiche report*.

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

ISTAT (2018b). Rapporto sulla conoscenza 2018. *Economia e Società*.

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

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

Jaeger, D. A., Ruist, J., and Stuhler, J. (2018). Shift-share instruments and the impact of immigration. Working Paper 24285, National Bureau of Economic Research.

Jensen, J. B. and Kletzer, L. G. (2006). *Undestanding the scope and impact of services outsourcing*, pages 75–134. Brookings Institution, Washington DC.

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. (1991a). *Geography and Trade.* Cambridge, MA: MIT Press.

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

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

Lavista, F. (2010). *La stagione della programmazione: grandi imprese e Stato dal dopoguerra agli anni Settanta*. il Mulino.

Lee, N. (2014). The creative industries and urban economic growth in the uk. *Environment and Planning A: Economy and Space*, 46(2).

Lee, N. and Clarke, S. (2019). Do low-skilled workers gain from high-tech employment growth? high-technology multipliers, employment and wages in britain. *Research Policy*, 48(9).

Lepore, A. (2013). *La Cassa per il Mezzogiorno e la Banca Mondiale: un modello per lo sviluppo economico italiano*. Rubbettino.

Lewis, E. and Peri, G. (2015). Chapter 10 - immigration and the economy of cities and regions. In Duranton, G., Henderson, J. V., and Strange, W. C., editors, *Handbook of Regional and Urban Economics*, volume 5 of *Handbook of Regional and Urban Economics*, pages 625–685. Elsevier.

Marchand, J. (2012). Local labor market impacts of energy boom-bust-boom in western canada. *Journal of Urban Economics*, 71(1):165–174.

Mion, G. and Naticchioni, P. (2009). The spatial sorting and matching of skills and firms. *Canadian Journal of Economics/Revue canadienne d'économique*, 42(1):28–55.

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

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

Moretti, E. (2004a). Estimating the social return to higher education: evidence from longitudinal and repeated cross-sectional data. *Journal of Econometrics*, 121(1):175–212.

Moretti, E. (2004b). Workers' education, spillovers, and productivity: Evidence from plant-level production functions. *American Economic Review*, 94(3):656–690.

Moretti, E. (2010a). Local labour markets. (15947).

Moretti, E. (2010b). Local multipliers. *American Economic Review*, 100(2):373–77.

Moretti, E. (2012). *The New Geography of Jobs*. Houghton Miffin Harcourt.

Moretti, E. and Thulin, P. (2013). Local multipliers and human capital in the United States and Sweden. *Industrial and Corporate Change*, 22(1):339–362.

Moretti, E. and Wilson, D. J. (2014). State incentives for innovation, star scientists and jobs: Evidence from biotech. *Journal of Urban Economics*, 79:20–38. Spatial Dimensions of Labor Markets.

MUVAL (2014). Strategia nazionale per le aree interne: definizione, obiettivi, strumenti e governance. *Materiali UVAL*, 31.

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

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

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

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

Nunes, F. (2013). Core voters or local allies? presidential discretionary spending in centralized and decentralized systems in latin america. *Presidential Discretionary Spending in Centralized and Decentralized Systems in Latin America (August 26, 2013)*.

Oates, W. E. (1993). Fiscal decentralization and economic development. *National tax journal*, 46(2):237–243.

Oates, W. E. (2005). Toward a second-generation theory of fiscal federalism. *International tax and public finance*, 12(4):349–373.

OECD (2019). *OECD Regional Outlook 2019*.

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

Ottaviano, G. I. P. and Peri, G. (2010). The labor market impact of immigration in western germany in the 1990s. *European Economic Review*, 54(4):550–570.

Peri, G. (2002). Young workers, learning, and agglomerations. *Journal of Urban Economics*, 52(3):582–607.

Peri, G., Shih, K., and Sparber, C. (2015). Stem workers, h-1b visas, and productivity in us cities. *Journal of Labor Economics*, 33(S1):S225–S255.

Persson, T. and Tabellini, G. (1994). Does centralization increase the size of government? *European Economic Review*, 38(3-4):765–773.

Persson, T. and Tabellini, G. (2002). *Political economics: explaining economic policy*. MIT press.

Pinotti, P. (2015). The economic costs of organised crime: Evidence from southern italy. *The Economic Journal*, 125(586):F203–F232.

Prud'Homme, R. (1995). The dangers of decentralization. *The world bank research observer*, 10(2):201–220.

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

Roback, J. (1982). Wages, rents, and the quality of life. *Journal of political Economy*, 90(6).

Rodríguez-Pose, A. and Ezcurra, R. (2010). Is fiscal decentralization harmful for economic growth? evidence from the oecd countries. *Journal of Economic Geography*, 11(4):619–643.

Rosés, J. and Wolf, N. (2018). *The Economic Development of Europe's Regions: A quantitative History since 1900*. Routledge.

Rubin, D. (2001). sing propensity scores to help design observational studies: Application to the tobacco litigation. *Health Services Outcomes Research Methodology*, 2:169–188.

Sapienza, P. (2004). The effects of government ownership on bank lending. *Journal of Financial Economics*, 72(2):357–384.

Saraceno, P. (1976). *Le radici della crisi economica*. Il Mulino.

Saraceno, P. (1986). *Il nuovo meridionalismo (Vol. 6)*. Istituto italiano per gli studi filosofici.

Sbrescia, V. M. (2014). La parabola della cassa per il mezzogiorno a trent'anni dalla liquidazione (dpr 6 agosto 1984). *Rivista giuridica del Mezzogiorno*, 28(4):745–760.

Serafinelli, M. (2019). "good" firms, worker flows, and local productivity. *Journal of Labor Economics*, 37(3):747–792.

Shleifer, A. and Vishny, R. W. (1993). Corruption. *The Quarterly Journal of Economics*, 108(3):599–617.

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

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

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

Suárez Serrato, J. C. and Wingender, P. (2016). Estimating local fiscal multipliers. Working Paper 22425, National Bureau of Economic Research.

SVIMEZ (1981). *Rapporto 1980 sull'economia del Mezzogiorno*.

SVIMEZ (1982). *Rapporto 1981 sull'economia del Mezzogiorno*.

Svimez, R. (2019). L'economia e la società del mezzogiorno.

Tanzi, V. (1996). Review of some efficiency and macroeconomic aspects. In *Annual World Bank Conference on Development Economics 1995*, page 317. World Bank Publications.

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

Trigilia, C. (1992). *Sviluppo senza autonomia: effetti perversi delle politiche nel Mezzogiorno*, volume 54. il Mulino.

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

Walker, W. R. (2013). The Transitional Costs of Sectoral Reallocation: Evidence From the Clean Air Act and the Workforce*. *The Quarterly Journal of Economics*, 128(4):1787–1835.

Estratto per riassunto della tesi di dottorato

Studente: Giulia Romani matricola: 956425

Dottorato: Economics

Ciclo: 34°

Titolo della tesi : Essays in Public and Urban Economics

Abstract: The thesis focuses on the study of spatial disparities and on their relationship with public policy and labour market shocks. Chapter one deals with the local impact of rationalisation policies in school services. I combine a DID model with an IV strategy that exploits institutional thresholds for school closures, demonstrating how school cuts negatively influence demographic and income dynamics at local level. For chapter two, I gained access to INPS administrative data. Using a two-steps estimation along with a shift-share IV, I show that the uneven growth of knowledge-intensive sectors produces multiplicative effects on the employment of local workers, decreases their probability of migration, and increases house prices more than nominal wages, resulting in a reduction of real wages. Chapter three uses unique historical data to look at the extent to which institutional design influenced the efficacy of a massive regional development programme (Cassa per il Mezzogiorno). Through a DID model I demonstrate that - in institutionally-fragile settings - the devolution of authority over public investments can generate dynamics of distributive politics.

Abstract: La tesi si concentra sullo studio delle disparità spaziali e sulla loro relazione con le politiche pubbliche e gli shock del mercato del lavoro. Il primo capitolo analizza l'impatto locale delle politiche di razionalizzazione nei servizi scolastici. Combino un modello DID con una strategia IV che sfrutta le soglie istituzionali per la chiusure delle scuole, dimostrando come i tagli ai servizi scolastici influenzino negativamente la dinamica demografica e reddituale a livello locale. Per il secondo capitolo, ho avuto accesso ai dati amministrativi INPS. Utilizzando una stima a due stadi insieme a uno strumento shift-share, mostro che la crescita disomogenea dei settori ad alta intensità di conoscenza produce effetti moltiplicativi sull'occupazione dei lavoratori locali, riduce la loro probabilità di migrare e aumenta i prezzi delle case più dei salari nominali, con conseguente riduzione dei salari reali. Il terzo capitolo utilizza dati storici unici per esaminare come il design istituzionale ha influenzato l'efficacia di un massiccio programma di sviluppo regionale (Cassa per il Mezzogiorno). Attraverso un modello DID dimostro che - in contesti istituzionalmente fragili - la delega di autorità sugli investimenti pubblici può generare dinamiche di politica distributiva.

Firma dello studente