



Consolidation of Local Health Authorities and mortality in the Italian NHS[☆]

Silvia Balia^{a,c}, Rinaldo Brau^{a,c} , Sara Pau^{b,c}

^a Department of Economics and Business, University of Cagliari, Italy

^b Department of Economics and Business, University of Sassari, Italy

^c CRENoS, Italy

ARTICLE INFO

Dataset link: Istat Contact Centre, doi: [10.17632/g67jfv6mb2.2](https://doi.org/10.17632/g67jfv6mb2.2)

JEL classification:

I11
I18
L38
G34

Keywords:

Healthcare Consolidation
Administrative Centralization
NHS
Mortality
Staggered roll-out design

ABSTRACT

This paper studies the health consequences of administrative centralization in healthcare, focusing on the consolidation of Local Health Authorities (LHAs) within the Italian National Health Service. This type of reform fits within a broader set of cost-containment efforts aimed at reducing administrative expenditures and exploiting economies of scale. In the Italian case, the reform centralized governance and expanded the scale of health administration, creating authorities with substantially larger catchment areas and shifting decision-making to more heterogeneous jurisdictions. Such changes may affect the responsiveness of healthcare provision to local needs, the coordination of service delivery, and access to care. Using an event-study Difference-in-Differences design, we estimate the policy's impact on municipal mortality rates, accounting for heterogeneous treatment effects and co-occurring fiscal adjustment policies. We find a significant increase in mortality starting four years after implementation, with an average treatment effect on the treated of 2.1%. The effects are concentrated in municipalities belonging to absorbed LHAs and in those exposed to mergers involving relatively large pre-reform LHAs and substantial post-merger expansions in catchment areas; they are also unevenly distributed, creating new vulnerable areas. Welfare estimates based on life-years lost indicate that the monetary costs of increased mortality exceed plausible administrative savings by roughly an order of magnitude.

1. Introduction

Over the past decades, many countries have implemented public sector reforms that restructured local governance by consolidating administrative entities into larger units, a process usually referred to as amalgamations, mergers, or consolidations. These reforms, typically motivated by the need to reduce public spending and justified on efficiency grounds, have been implemented even

[☆] In memory of Sara Pau, who passed away after a long disease at the young age of 38 before seeing the completion of this work to which she significantly contributed.

Acknowledgments: We gratefully acknowledge funding from the EU Horizon Project ESSPIN “Economic Social and Spatial Inequalities in Europe in the Era of Global Mega-Trends” (project n. 101061104). We are also very grateful to Istat, in particular to Antonella Ciccicarese, for providing the death data. We thank the Editor, Maria Polyakova, and three anonymous reviewers for their valuable comments and suggestions. We also thank Friedrich Breyer, Adriana Di Liberto, Marco Giovanni Nieddu, Cristian Usala and Eugenio Zucchelli for their comments on earlier stages of this study. We also thank participants at the 9th ICEEE Conference, 59th SIE Conference, 23rd AIES Conference, 2022 LAGV Conference, 2022 EUHEA Conference, 34th SIEP Conference, 63rd ERS Conference.

* Correspondence to: Department of Economics and Business, Università di Cagliari, Viale S. Ignazio, 17 - 09123 Cagliari, Italy.

E-mail address: brau@unica.it (R. Brau).

<https://doi.org/10.1016/j.jhealeco.2026.103174>

Received 14 February 2025; Received in revised form 21 July 2026; Accepted 22 July 2026

Available online 3 August 2026

0167-6296/© 2026 The Authors. Published by Elsevier B.V. This is an open access article under the CC BY license (<http://creativecommons.org/licenses/by/4.0/>).

in highly decentralized settings. While the potential for cost-saving is often emphasized, the broader implications of such reforms remain unclear.

Amalgamations, in fact, may weaken responsiveness to local needs, increase the organizational complexity of service delivery, and raise the effective distance between citizens and essential services. Numerous studies on municipal and county amalgamations, as well as on school district consolidations, have examined the effects of these reforms on welfare, economic growth, and other outcomes related to the institutional mission of the consolidating entity. Some studies document public expenditure savings without compromising service levels (e.g., Reingewertz, 2012; Ferraresi et al., 2018), increases in subjective well-being (Wilner, 2023), higher levels of prosperity (Andini et al., 2017), and enhanced student achievements (De Haan et al., 2016; Sandsør et al., 2022). Other contributions find evidence of adverse effects, such as exacerbating urban–rural economic divides (Hall et al., 2020), lowering returns to education and reducing educational attainment (Berry and West, 2010), or increasing political costs, for instance through reduced voter turnout (e.g., Roesel, 2017).

In this paper, we examine a specific and relatively underexplored form of public service amalgamation: the administrative consolidation of local health jurisdictions. These entities, often labeled authorities, departments, or districts, organize and coordinate health services at the local level in many countries. Our analysis focuses on a large-scale reform affecting Italian Local Health Authorities (LHAs) over the past two decades. Similar processes have occurred in other countries, such as Canada and Australia, albeit with different institutional trajectories.¹ We study whether, and in which direction, moving from smaller to larger authorities has affected the core mission of Italian LHAs: ensuring adequate healthcare for residents in the municipalities they serve. To assess this, we focus on municipal-level mortality rates, a broad welfare-relevant health outcome, and a robust indicator for evaluating health policy intervention over time.²

Conceptually, the expected effects of consolidating local health jurisdictions point in opposite directions. On the one hand, consolidations may reduce fixed administrative costs and free up resources that can, at least in principle, be reallocated to patient care. Larger catchment areas and higher patient volumes could enhance service quality through learning-by-doing, selective referral, and greater specialization among healthcare professionals. On the other hand, consolidations may increase organizational complexity and make LHAs harder to manage. As decision-making becomes more centralized and service delivery more standardized, primary and outpatient care may become less responsive to the heterogeneous epidemiological and socio-economic profiles of local communities. In addition, a single governance structure may fail to effectively manage a substantially enlarged scale due to limitations in coordination and monitoring mechanisms, ultimately undermining the performance of even routine activities. Possible reconfigurations in the spatial distribution of services may further raise average travel times and access costs for citizens, particularly in non-urban and already disadvantaged areas.

LHAs represent the local tier of the Italian National Health Service (NHS), which operates through a regionally decentralized governance structure with national, regional, and local levels. Each Authority is responsible for planning and delivering a broad spectrum of services – including primary care, outpatient specialist care, hospital care, public health programs, and health-related social services – to residents in a defined catchment area. The population assigned to each LHA corresponds to a group of municipalities designated by the regional government. The number and configuration of LHAs are determined independently by each of the 21 Italian autonomous regions. Since the 1990s, national policies have sought to contain rising healthcare expenditures by implementing cost-containment reforms. As part of this effort, the number of LHAs was progressively reduced: from 228 in 1996 to 99 in 2021, resulting in an average catchment population of about 611,000 in a country of roughly 60 million inhabitants. This represents a relatively large-scale “experiment” in the centralization of local health-care governance.

The LHAs’ consolidation reform within the Italian NHS was implemented at different times, with some regions opting not to merge their LHAs. Exploiting detailed administrative records and official documentation from the Ministry of Health, we reconstructed the whole consolidation process over the period 2002–2018, including the potential reassignment of municipalities from one LHA to another. We combine this information with demographic and mortality data from the Italian National Statistical Institute (Istat), which allows us to construct municipal-level mortality rates.

The staggered implementation of the reform generates substantial variation in exposure at the municipal level: some regions never merged their LHAs, others merged only a subset of them, and the length of exposure differs across adopting regions. We estimate the causal effect on mortality rates by leveraging variation across municipalities, over time, and in treatment exposure duration, and exploiting the presence of never-treated municipalities. We specifically address the presence of contemporaneous national cost-containment policies for some regions – notably the Debt Rescheduling Plans (DRPs) implemented in fiscally distressed regional health services – by explicitly accounting for DRP exposure into the analysis and exploring the robustness of our findings across municipalities differentially affected by these plans.³ Our empirical strategy is based on an event-study Difference-in-Differences design, primarily using the estimator proposed by Sun and Abraham (2021), which addresses heterogeneous treatment effects across

¹ In Canada, the number of public local authorities decreased from about 130 in 2005 to 75 in 2023 (Statistics-Canada, 2024, 2006). In Australia, a notable episode of mergers occurred in 2005 in New South Wales, where 17 Area Health services were amalgamated into 8 larger entities (Johar and Savage, 2014). However, a legislative shift in 2011 went in the opposite direction, leading to the creation of 15 Local Health Districts with smaller catchment areas. More recently, in 2021, four services in Victoria merged into a single entity.

² For example, Bailey and Goodman-Bacon (2015) measure the health effects of establishing the Community Health Centers on mortality rates in US counties. Miller et al. (2021), Clayton (2019), and Wherry and Meyer (2016) examine the effects of Medicaid expansion on mortality across different legislative contexts. Mora-García et al. (2024) evaluate the long-term effects of implementing primary healthcare intervention in Costa Rica.

³ Recent literature has documented remarkable adverse health effects associated with the DRPs (see, e.g., Depalo, 2019; Arcà et al., 2020). Under the DRPs, the central government imposed stringent limits on public health spending, reorganized the hospital system, and introduced co-payments for essential services in regions with substantial budget deficits in health.

cohorts and captures the full dynamics of the treatment effects. This is important, as the reorganization of LHAs is expected to affect population health gradually and cumulatively over time. The baseline model includes municipality and DRP cohort-by-year-fixed effects and time-varying controls, thereby identifying the effects under a conditional parallel trends assumption.

Our findings indicate that, prior to consolidation, mortality evolves along similar trends in treated and never-treated municipalities, and that the policy does not exhibit immediate effects. Significant and persistent adverse health effects emerge four years after consolidation. Once these effects materialize, we estimate an average treatment effect on the treated (ATT) of 2.1%, while the ATT computed over the entire post-treatment period is 1.3%, reflecting the absence of effects in the years immediately following consolidation. These causal effects are supported by a placebo analysis in which treatment status is randomly reassigned by altering the policy year, the regions involved, and the LHAs included within those regions. The policy's detrimental health effects remain consistent across various control group specifications, particularly when the analysis is restricted to treated and never-treated municipalities that are spatially proximate, defined as being within a 30-, 20- or 15-minute travel distance or adjacent to one another. The overall effect on total mortality is only partially attributable to the oldest age group. Significant post-merger variations in mortality are observed within the working-age population (ages 25–49 and 50–64). For individuals aged 0–74, we document a statistically significant increase in mortality attributable to treatable causes, which largely depend on timely diagnosis and effective treatment, as well as in mortality for “less preventable and less treatable” causes. Furthermore, our analysis indicates that the policy's adverse impact on the all-ages mortality rate is primarily driven by a substantial rise in deaths related to circulatory system diseases and cancers.

To gain insights into the factors underlying these negative outcomes, we exploit the observed differences in municipality types and explore potential patterns of heterogeneity in the overall impact. We find that municipalities whose LHAs were absorbed by other LHAs experience the most significant negative effects. Further analysis reveals that the reform is particularly detrimental when consolidation involves large LHAs and when it leads to large increases in their catchment areas. Differentiating between urban and rural municipalities, based on Istat classification of inner areas, we find that Outlying municipalities, which surround Core urban centers that provide all essential services, are the most adversely affected by the policy.

While consolidations primarily target the local administrative and governance layer of the health system, they may also reshape the hospital networks and other services within the newly formed LHAs, which now serve larger catchment areas. Hospital reorganizations have been widely examined in the literature, highlighting both the potential pitfalls and benefits for patients' health outcomes (see, e.g., Buchmueller et al., 2006; Gaynor et al., 2012). A central question in our analysis is therefore whether the effect of consolidation on mortality reflects more the consequences of administrative centralization than those of downsizing or facility closures. We find that hospital closures are more frequent in consolidated areas and that the consolidation process leads to centralization in the spatial allocation of hospital services: despite a national increase in the number of hospital physicians, municipalities exposed to LHA mergers experience a relative decline in the availability of hospital physicians within reasonable travel-time distances, especially in public hospitals. Nevertheless, limiting the analysis to municipalities unaffected by hospital closures within relevant travel-time distances the estimated increase in mortality remain consistent both in size and in the years during which it emerges.

Overall, we believe that our paper offers three main contributions. First, we add to the broader literature on local government amalgamation and consolidation of public services by showing that a large-scale reform, primarily aimed at achieving administrative economies of scale in healthcare, was implemented at the cost of substantial and unevenly distributed health consequences. Second, by investigating the mechanisms underlying the reform's effects, our study speaks to the literature on hospital mergers and closures. We pay particular attention to the role of hospital closures as one possible channel through which administrative centralization may affect health outcomes, and show that the centralization of governance and planning can affect health outcomes even in the absence of widespread hospital closures. Third, we inform the ongoing policy debate on the optimal scale of local health administration in decentralized health systems. While this debate has largely focused on the relationship between catchment-area size and expenditure savings, our findings emphasize that cost-efficiency objectives must be balanced against potential losses in responsiveness and access, particularly for populations in already fragile areas.⁴ By expressing the estimated mortality increase in monetary terms, using the value of life-years lost, and comparing it to potential administrative savings, we show that the consolidation of LHAs in Italy is unlikely to be welfare-improving.

The remainder of the paper is organized as follows. Section 2 presents the institutional background and details the consolidation process of Italian LHAs. Section 3 describes the data sources used to construct our dataset and provides descriptive evidence. Section 4 outlines the empirical strategy. Section 5 presents the econometric results for total mortality, age-specific mortality, and mortality by cause, along with robustness and heterogeneity analyses. Section 6 discusses the role of hospital services as a potential channel through which the policy may have affected mortality outcomes. The final section concludes by providing a simplified welfare analysis and discussing the implications of our findings for the design of consolidation policies in decentralized health systems.

⁴ See, in particular, Di Novi et al. (2018) for the Italian case; Santerre (2009) and Bernet and Singh (2015) for the US.

2. The Italian LHAs consolidation process

The Italian NHS, established in 1978, is a regionally decentralized system funded through regional corporate taxation, regional top-ups to the national income tax, and VAT revenue allocations. This funding structure ensures the provision of legally defined essential levels of care. The primary objective of the NHS is to provide uniform and comprehensive health care nationwide.

The current institutional setting has arisen from a series of reforms initiated in 1992 (Amato - De Lorenzo reform) and implemented through legislative decrees in 1992 and 1993, progressively introducing universal free-patient choice and establishing 21 autonomous Regional Health Services (RHSs). The decentralization of the NHS became fully effective with a constitutional reform approved in 2001, which granted regions greater autonomy in organizing healthcare services, including technological investments, the hospital network, and the diversification and specialization of local supply. This increased autonomy, naturally, came with added responsibilities in terms of controlling healthcare spending and ensuring efficiency in service provision. The national government maintains a general authority over healthcare policy, including defining the essential levels of care and setting overall spending limits.

The RHSs deliver public health care through sub-regional health authorities. In addition to directly managing a large share of all public hospital providers, the LHAs' role extends to broader responsibilities: from planning and managing the delivery of essential healthcare services, including primary care, outpatient clinic care, preventive health programs, social care-related health care for residents in the included municipalities, and public health initiatives, to contracting with general practitioners and pediatricians to manage budget allocation for healthcare services. The origin of the current role of LHAs dates back to 1995, with the creation of 228 LHAs and the elimination of previous, much more numerous Local Health Units (USLs), which were managed by individual municipalities or groups of municipalities. This change shifted governance from a system in which municipal councils assigned management roles, reflecting local political dynamics, to one in which LHAs were endowed with legal personality and entrepreneurial autonomy and managed by a general manager appointed by the regional government.⁵ While most regions adopted the reform in 1995, Sardinia and Sicily, as special statute regions, implemented it one year later. The "regionalization" of local health entities was finalized in 1997, when the Veneto region made a minor adjustment to its structure by merging two LHAs. In 1998, the overall number of LHAs was further reduced to 197 following an autonomous initiative by the Lombardia region to restructure its RHS into a quasi-market. This reorganization introduced a clear distinction between funding and purchasing entities (LHAs) and service providers (hospital entities).⁶

In the years after the constitutional reform of 2001, as regions faced financial pressures from population aging and national austerity constraints, several opted to merge smaller LHAs into larger entities. The principal motivation for this consolidation was the prospect of efficiency gains through economies of scale, expected to result from expanding catchment areas for indivisible services and improving organizational integration. Estimates provided by Di Novi et al. (2018) suggest that these gains were indeed realized for a specific subset of production costs – namely, administrative expenses; the cost of purchasing health-related goods (such as drugs, medical devices, dietary products, and chemicals) and non-health goods (e.g., foodstuffs and cleaning products); and non-healthcare-related services (e.g., laundry, cleaning, and catering).

Over the years, as illustrated in Figure A1 in the Appendix, many regions redefined the configuration of LHAs, resulting in an overall reduction of 96 authorities. A distinctive aspect of this process is that regions acted independently and at different times, as detailed in Table A3 in the Appendix, with the first mergers occurring in 2004 and the latest ones in 2017, across 12 distinct years in which mergers took place.⁷ Fig. 1 documents the consequent expansion of the catchment areas of the LHAs involved in the merger process.⁸

In the same period, the Italian NHS was also affected by the "Debt Rescheduling Plans" (DRPs), a national cost-containment policy that must be considered when assessing the impact of the LHAs consolidation reform on health outcomes. Introduced by the central government in ten regions, the DRPs aimed to restore financial stability by addressing persistent healthcare deficits through a combination of expenditure controls and structural interventions. These plans targeted regions with persistent deficits and involved a combination of measures aimed at achieving both fiscal consolidation and efficiency gains. Such measures included strict expenditure ceilings, cuts to public healthcare funding, and structural reorganization of hospital networks.⁹ The potential public health impact of the DRPs is substantial. Recent empirical evidence confirms that the target regions succeeded in containing expenditures (Bordignon et al., 2020), but also experienced adverse effects, including increased mortality, lower hospitalization rates, and a rise in south-to-north patient mobility (Depalo, 2019; Arcà et al., 2020; Cirulli and Marini, 2023; Guccio et al., 2024). The DRPs partially overlapped with the implementation of the LHA consolidation policy. Specifically, six regions – Abruzzo, Calabria, Campania, Lazio, Molise, and Sicily – were placed under a DRP in 2007, followed by Puglia in 2010. These regions remained under the regime for the entire analysis period (2002–2018) while Liguria and Sardinia were under a DRP for a shorter duration (2007–2009), and Piedmont between 2010 and 2015.¹⁰

⁵ This change did not necessarily diminish political influence; instead, it reallocated it from the local to the regional level.

⁶ This restructuring not only decreased the number of LHAs in Lombardy from 44 to 15 but also fundamentally transformed the 1995 LHAs by establishing autonomous hospital trusts and Scientific Institutes for Research, Hospitalization, and Healthcare. These changes partially relieved LHAs of direct management responsibilities, refocusing their role on preventive care, health promotion, and healthcare planning.

⁷ Only two regions, Emilia-Romagna and Calabria, experienced two waves of mergers involving different LHAs: 2004 and 2014, and 2008 and 2011, respectively.

⁸ It is worth noting that, while the transformative reorganization of the RHS in Lombardy in 1998 anticipated the consolidations implemented by other regions after the 2001 reform, the changes from 2004 onward were intended solely to redistribute and expand the LHAs' catchment areas without altering their core mission.

⁹ See Arcà et al. (2020) for a thorough description of the DRPs, therein labeled "Recovery Plans".

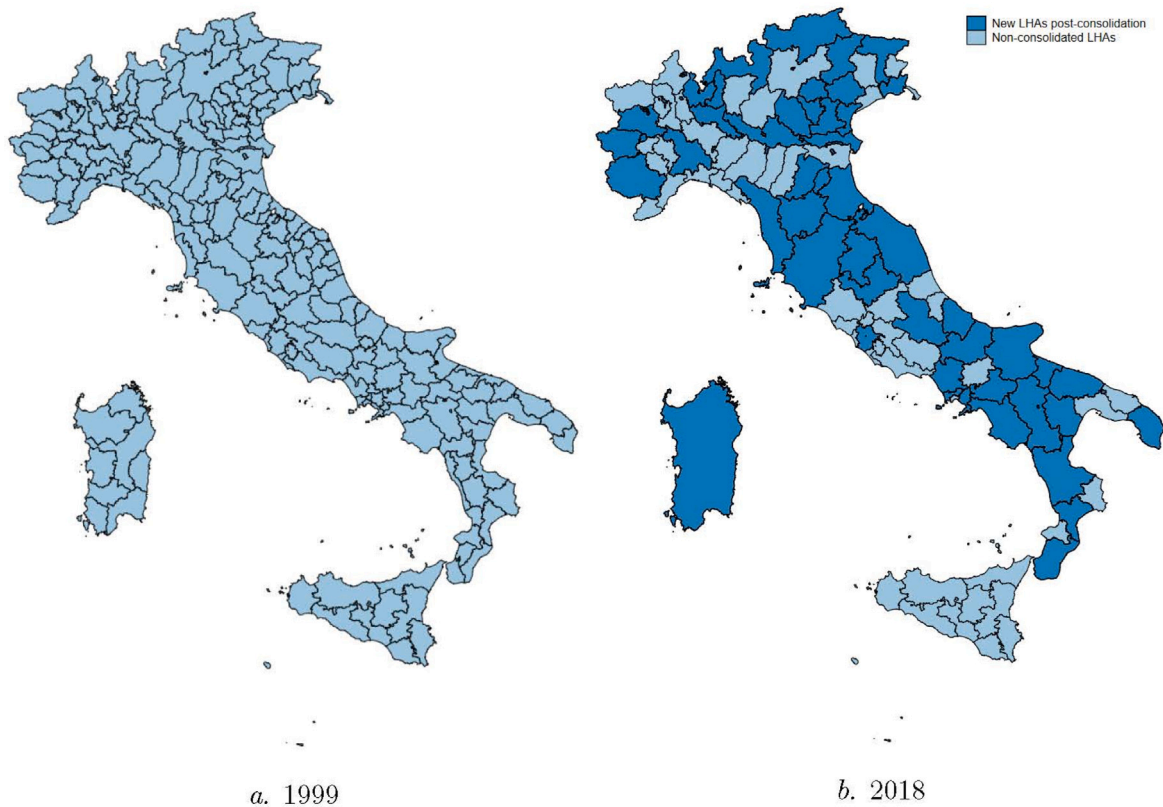


Fig. 1. Borders of Italian local health authorities in 1999 and 2018.

Notes: Elaborations on Ministry of Health Data. Intra-municipal LHAs borders for Turin, Milan and Naples are not drawn.

3. Data

Our empirical analysis relies on a comprehensive dataset covering Italian municipalities for the period 2002–2018. The Italian NHS transformation described in Section 2 suggests that 2002 is the appropriate year to begin studying the effects of regional consolidation policies introduced by the 2001 constitutional reform, which granted regions greater autonomy in healthcare governance.¹¹ The effect window we have chosen ends in 2018 to prevent potential confounding effects arising from the COVID-19 pandemic.

Our dataset includes annual inter-census data on resident populations, as well as yearly municipal-level death counts disaggregated by (5-year) age group, gender, and cause of death (according to the ICD10 classification), as provided by Istat.¹² Each municipality is linked to average taxable income data released by the Italian Ministry of Economy and Finance and is classified according to Istat's definition of inner areas (Core, Outlying, Intermediate, Peripheral, and Very Peripheral municipalities).¹³

To evaluate the impact of the consolidation policy, we assign each municipality to the LHA to which it belongs in each year of the period of interest. This process relies on data from the Italian Ministry of Health (MH). Specifically, we identified the municipalities involved in the consolidation using the administrative dataset “Asl-comuni-popolazione” released by the MH in 2023, which reports the population enrolled in each LHA by municipality of residence for the years 1999, 2011, and for the period 2013–2018. To account for the years not covered by this dataset, we conducted a documentary analysis based on official records released in 2018 by the Ministry of Health and known as “Variazioni ambiti territoriali e ricodifica delle ASL” (Changes in catchment areas and recoding of LHAs). These records detail the LHA mergers that took place between 2006 and 2017. When this information proved insufficient, we

¹⁰ As required, regions under DRPs could exit the regime once financial equilibrium was re-established and maintained.

¹¹ This choice also ensures at least two pre-policy periods for all municipalities involved in the LHAs' mergers.

¹² Istat provided us granular data on causes of death and on age at death in two separate datasets that cannot be linked due to privacy-related restrictions. This prevents any analysis of age-specific mortality within causes of death, or vice versa.

¹³ This classification is identified by The National Strategies for the Inner Areas (SNAI) and is based on municipalities' accessibility to essential public services (local public transport, education, and healthcare services), and it is described at <https://www.istat.it/it/archivio/273176>.

consulted the official NHS hospitals' address book, "Indirizzi delle strutture di ricovero pubbliche e private", released by the MH in 2007. This source allowed us to track the reassignment of hospital locations across LHA territories after consolidation, providing a reliable indicator of municipal involvement in the consolidation process. By systematically monitoring these changes, we were able to reconstruct all changes in LHA catchment areas over the period 2002–2018. In particular, for 2002, we were able to confirm that the LHA configuration was consistent with the 1999 official data, as indicated by the 2002 address book listing the headquarters of 197 LHAs.¹⁴

As outlined in Section 2, the LHA restructuring process primarily involved merging two or more smaller LHAs into a larger entity. Municipalities within these newly formed LHAs were directly affected by the restructuring and are therefore considered treated.¹⁵ A few LHAs experienced jurisdictional changes because of the reassignment of one or more municipalities between neighboring territories. In these cases, we classified the municipalities that changed LHA as treated if they were incorporated into a larger LHA or an LHA of similar size to the original one. Because the reduction in LHA's size could impact local health services provision, we excluded from the analysis all municipalities within LHAs whose population catchment area was reduced by more than 5% due to these reassignments.¹⁶ Additionally, we excluded from the sample any municipalities for which treatment status could not be definitively determined.¹⁷

3.1. Mortality patterns and summary statistics

To evaluate the medium and long-term health impacts of LHA mergers, we focus on (municipal-level) mortality rates, a widely recognized indicator of population health. These rates capture the impact of organizational changes across the entire healthcare system, as they reflect not only hospital care quality but also the effectiveness of preventive, primary, and secondary healthcare services managed by LHAs and provided outside hospital settings (see Section 2).

Unlike clinical quality metrics commonly used in research on hospital closures and mergers – which typically focus on mortality rates linked to specific conditions or procedures – population mortality rates provide a more comprehensive indicator of healthcare performance. A key limitation of clinical quality metrics is their susceptibility to distortions arising from patient mobility. Specifically, if patients perceive a decline in service quality – such as increased travel distances or reduced availability of specialized inpatient care – they may seek treatment outside their LHA, where they expect higher standards of care. This selective outflow can lead hospitals that attract more complex or severe cases from other LHAs to report worse average clinical outcomes, even if their quality of care remains unchanged. Moreover, hospital-based quality indicators may misleadingly reflect conditions in areas different from those directly affected by LHA consolidation. In this setting, because patients can cross administrative boundaries to seek care, hospital-level data may not accurately represent the healthcare experiences of residents in municipalities exposed to the merger.¹⁸

Specifically, in our analysis, we calculate the total mortality rate (all ages, all causes) per 100,000 residents and specific mortality rates for different groups.¹⁹ The latter include age-specific mortality rates for the broad categories of individuals aged 0–14, 15–49, and 50 and older individuals, along with a more granular classification of the adult population: 15–24, 25–49, 50–64, 65–74, and 75 and older.

Moreover, we calculate mortality rates for the leading causes of death and for avoidable mortality – that is, preventable and treatable deaths – as well as for a third category capturing deaths from causes that are "less preventable and less treatable", all restricted to the 0–74 age group. Following the classification developed by OECD/Eurostat (2022), avoidable mortality is subdivided into preventable mortality, referring to deaths that can be mainly avoided through public health and primary prevention, and treatable mortality, referring to deaths that can be mainly avoided through timely and effective healthcare. This classification builds on earlier work by Nolte and McKee (2004, 2011) and is currently used for cross-country monitoring of healthcare system performance. Causes of death not included in either group are classified as less preventable and less treatable, as they are comparatively less responsive to either public health interventions or clinical care.²⁰

¹⁴ To validate our methodology, we used the hospital address book to recode the LHAs and verify the correspondence with mergers that occurred between 2006 and 2017, for which we possess official data. Ultimately, we encountered only 10 cases nationwide (all in Apulia) where we could not determine the exact year of change in LHA affiliation. Consequently, these municipalities were excluded from the analysis.

¹⁵ An exception is Lombardia's LHA No. 303, where the catchment area was split into two parts, subsequently merging with two separate LHAs (322 and 323).

¹⁶ This exclusion specifically applies to LHA No. 103, 106, and 107 in Sardinia, LHA No. 311 in Lombardy, LHA No. 104 in Friuli-Venezia Giulia, LHA No. 114 in Veneto, and LHA No. 105 in Calabria.

¹⁷ This includes Rome in Lazio, where mergers involved only part of the city; Turin in Piedmont, which initially consisted of four LHAs, then reduced to two, and finally to one, leading to a distinct treatment regime characterized by a double merger; and fifty-five municipalities within a specific LHA in Calabria (coded as LHA 205), due to a double consolidation within the observed period. Altogether, the exclusion of these cases and the aforementioned reassignments led to the removal of 516 municipalities from our analysis.

¹⁸ These issues are particularly relevant in the Italian NHS, where, as in other universal healthcare systems, patients have the right to seek specialized and hospital care anywhere in the country.

¹⁹ A detailed inspection of the total mortality rate reveals several abnormal values. While some anomalies can be attributed to extraordinary events, such as three major earthquakes and a few floods during the period under analysis, others are due to administrative errors in the death registry, particularly in smaller municipalities. To eliminate these outliers, we exclude from the analysis any municipalities that, for at least one year, report a total mortality rate exceeding the threshold of mean + 3.5 σ . This additionally reduces our sample by 428 municipalities, corresponding to an additional loss of population of only 0.37%.

²⁰ Our measure of avoidable mortality in the age group 0–74 considers Infectious diseases with ICD10 codes A00–A09, A36, A35, A37, A80, B05 as preventable, and those with codes A40–A41 as treatable, while Tuberculosis (A15–A19, B90) is classified as 50% preventable and 50% treatable; Neoplasms (cancers) (C18–C21, C44, C50, C54–C55, C62, C81) as treatable, with cervix uteri cancer (C53) considered 50% preventable and 50% treatable; Endocrine, nutritional and metabolic

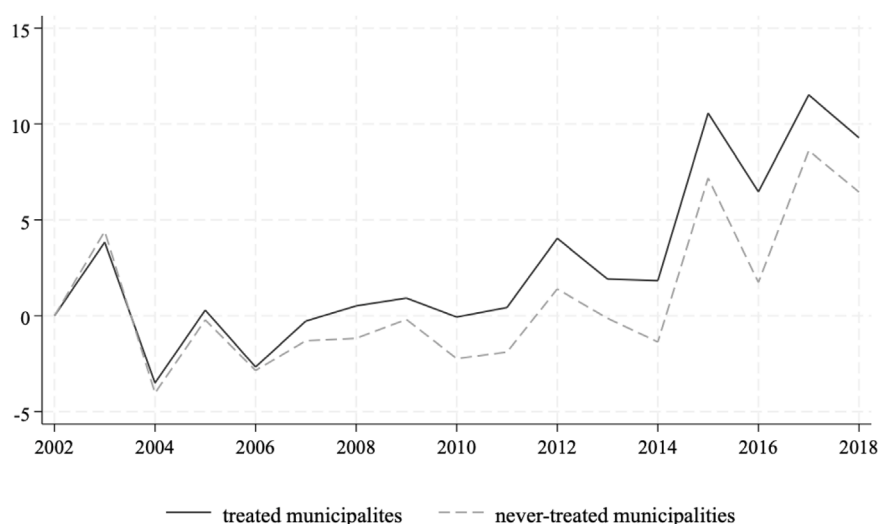


Fig. 2. Annual percentage changes in total mortality rate relative to 2002 by treatment status.

We begin by examining raw mortality trends, both in the aggregate and disaggregated by leading causes of death. These trends are not adjusted for time-varying municipal characteristics or treatment timing heterogeneity, as they are intended to provide descriptive context and highlight potential differences between treated and never-treated municipalities. Fig. 2 shows the annual percentage change in total mortality rates relative to the 2002 baseline, separately for treated and never-treated municipalities. Both series are normalized to zero in 2002. While a general upward trend is observable for both groups, likely reflecting demographic factors such as population aging, the increase is more pronounced among treated municipalities, particularly after 2007, when LHA consolidation accelerates (with a 9% reduction in the total number of LHAs). This pattern may suggest an emerging effect of the reform. However, some divergence appears already before the onset of treatment, which likely reflects differences in cohort composition (i.e., municipalities treated earlier may have had distinct pre-reform mortality dynamics).

Fig. 3 disaggregates these trends by major causes of death. Mortality from circulatory diseases remains below the 2002 baseline throughout the period, but shows a steeper relative increase among treated municipalities from 2007 onward. Cancer mortality begins to rise gradually in treated municipalities as early as 2003, before the first consolidations occur. This early divergence should not be interpreted as an anticipatory effect of the reform. Rather, it reflects underlying differences in mortality trends that are not accounted for in the raw data. Respiratory and other-cause mortality exceeded the 2002 baseline from 2008 onward in both groups, but other-cause mortality shows a more pronounced increase among treated municipalities after 2007. Overall, the descriptive health trends and time paths shown in the two figures reinforce the importance of controlling for treatment timing and underlying health trends and observable differences in the causal analysis.

To conclude this section, we compare summary statistics for mortality outcomes and the main observable characteristics used in the analysis. Table A1 in the Appendix reports statistics for treated municipalities, grouped by consolidation year (i.e., by cohort). In all cohorts, the total mortality rate is higher in the post-policy period than in the pre-policy period. Yet, when considering age-specific mortality rates, we observe a general decline across cohorts between the pre- and post-treatment periods. Table A2 in the Appendix, in contrast, compares treated and never-treated municipalities over the full 2002–2018 period. Mortality rates are consistently higher in never-treated municipalities, both for total mortality and by age group. While the difference in total mortality may partly reflect a slightly older population in treated areas, the persistence of the difference across age-specific rates suggests that baseline mortality disparities are not entirely explained by demographic composition. These patterns support the use of log-transformed mortality rates in the econometric analysis, as this adjustment helps address initial differences in mortality levels and mitigates potential convergence effects.²¹ The table also shows that, on average, the never-treated and treated groups are very similar in terms of per capita income, whereas larger differences emerge at the cohort level. A similar pattern holds for population age structure. These findings motivate the inclusion of per capita income and age composition as time-varying covariates to help satisfy the conditional parallel trends assumption underlying our identification strategy.

diseases (E00–E07) as treatable, while Diabetes (E10–E14) is classified as 50% preventable and 50% treatable; Diseases of the nervous system (G40–G41), as treatable; Diseases of the circulatory system (I10–I13, I15, I20–I25, I60–I69) as 50% preventable and 50% treatable, while Chronic rheumatic heart disease (I05–I09) is classified as treatable only; Diseases of the respiratory system with codes J10–J11 as preventable, while those with codes J12–J18 as treatable; Diseases of the digestive system (K25–K27, K35–K38, K40–K46, K80–K81) as treatable; Diseases of the genito-urinary system (N00–N07, N17–N19, N25–N27, N40), as treatable; Perinatal mortality (O00–O99, P00–P96, A33, A34, Q20–Q28), as treatable; and External causes (Y60–Y69, Y83–Y84) as treatable.

²¹ Improving mortality rates may be easier when starting from a higher baseline than from a lower one.

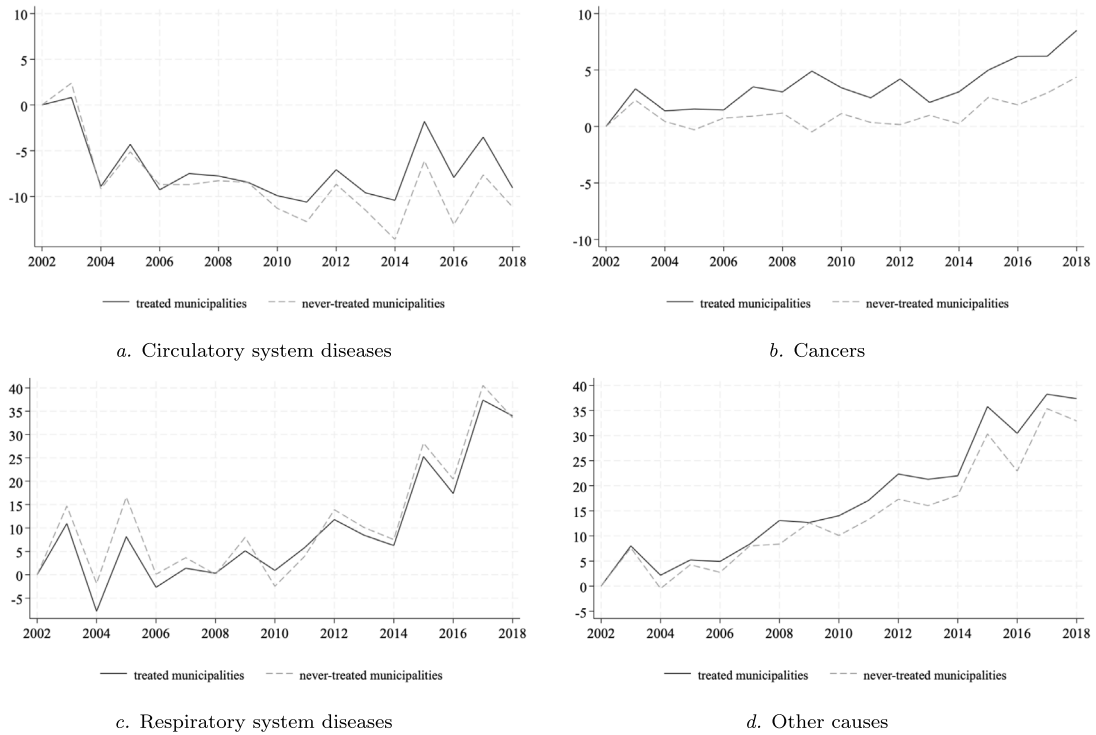


Fig. 3. Annual percentage changes in mortality rates for causes of death relative to 2002 by treatment status.

4. Empirical strategy

We identify the impact of the LHA consolidation process on mortality by leveraging variation across and within Italian regions. Our unit of observation is the municipality-year. A municipality is defined as treated since the first year in which the LHA serving it is consolidated into a larger administrative unit. Treatment is absorbing thereafter. While some regions implemented the consolidation policy, others did not. Among the regions that adopted the policy, the timing of amalgamations varied. Moreover, not all LHAs within adopting regions were involved in the mergers, resulting in differential exposure to the policy at the municipal level. This staggered rollout quasi-experimental design allows us to group municipalities by the year of treatment, creating twelve distinct treatment cohorts over the period from 2002 to 2018.

We employ a Difference-in-Differences (DiD) approach implemented through an event-study specification in which treatment effects are indexed by event time, defined relative to the first year of LHA consolidation. We estimate dynamic treatment effects using the year immediately preceding consolidation as the reference period. This allows us to assess whether mortality rates followed similar trends in treated and never-treated municipalities before treatment and distinguish immediate responses from effects that emerge gradually after reorganization. The estimating equation is:

$$y_{it} = \alpha_i + \lambda_{gt} + \sum_{l=-K}^L \beta_l D_{it}^l + \delta X_{it} + \epsilon_{it} \quad (1)$$

where y_{it} represents the crude mortality rate per 100,000 residents for municipality i in year t . The terms D_{it}^l are binary indicators equal to one when municipality i is l time periods before or after first exposure to consolidation, with $l = 0$ denoting the consolidation year. The coefficients β_l capture the leads (pre-treatment) and lag (post-treatment) effects relative to the omitted year. Never-treated municipalities serve as the control group; as a robustness check, we also re-estimate the model using the last-treated cohort as an alternative control group.

Identification relies on a conditional parallel trends assumption: absent treatment, mortality patterns in treated and never-treated municipalities would have evolved similarly after accounting for municipality fixed effects α_i and the municipal-level time-varying covariates X_{it} defined in Section 4.1. As discussed in Section 4.2, our preferred specification further accounts for the co-occurring DRP policy by replacing common year fixed effects with DRP cohort-by-year fixed effects, λ_{gt} , which absorb mortality dynamics common to municipalities sharing the same timing of exposure to this co-occurring policy and to those not exposed to DRP. Here g denotes the DRP cohort: municipalities located in regions that first entered a DRP in the same year are assigned to the same cohort, while municipalities in regions never exposed to that policy form a separate group.

In staggered adoption settings with heterogeneous treatment effects across cohorts, the conventional two-way fixed effects (TWFE) estimator is typically affected by negative-weighting issues, which yield uninterpretable and misleading estimates (see Goodman-Bacon, 2021). We therefore adopt the Interaction Weighted (IW) estimator of Sun and Abraham (2021), which estimates cohort-specific treatment effects at each relative time and aggregates them into overall event-study coefficients, weighted by the relative share of treated units in each cohort.²²

4.1. Baseline specification

In the baseline specification, we control for municipal-level time-varying characteristics, X_{it} : the population shares of age groups 50–64, 65–74, 75–84, and 85 and above, as well as average income.²³ Controlling for the evolution of age composition is essential when working with crude mortality rates, especially because age-adjusted mortality rates cannot be computed for several outcomes of interest.²⁴ Including taxable income captures the differences in socioeconomic status that may influence mortality dynamics before and after consolidation. Figure A2 in the Appendix supports the conditional parallel trends assumption by documenting substantial overlap in the distributions of these covariates across treated and never-treated municipalities. This overlap indicates that exposure to treatment is not systematically related to these characteristics and that never-treated municipalities provide a credible control group for the treated units. In our application, observations are weighted by municipal population in 2002, thereby reflecting differences in the relative exposure to the policy, and standard errors are clustered at the pre-reform LHA level, reflecting the level at which treatment is assigned.

When the outcome is the total crude mortality rate, we adopt a logarithmic transformation. This specification accommodates the substantial heterogeneity in baseline mortality levels across municipalities, reduces the skewness of the mortality distribution, and allows the estimated event-time coefficients to be interpreted as proportional changes in the mortality rate. We estimate models for different population segments using age-specific mortality rates as outcomes, and provide a decomposition of the policy effect by examining the distribution of deaths across causes. For age-specific mortality or mortality by causes, however, municipal-level death counts often display a mass point at zero. For these outcomes, we estimate Poisson pseudo-maximum likelihood (PPML) models with two-way fixed effects using death counts as the dependent variable and the relevant population counts as exposure variables (see Chen and Roth, 2024), so that the estimated coefficients can be interpreted as approximations of proportional changes in age-specific mortality rates and in mortality rates by causes.²⁵ This approach, however, shares with linear TWFE the limitation that it does not account for treatment-effect heterogeneity across cohorts and over time.

4.2. Potential confounding policies

A critical challenge for identification in our empirical setting arises from the partial overlap between the LHA consolidation policy and the DRPs during the same period. This overlap was not the result of coordinated policy design but poses a threat to identification, as DRP exposure may independently influence mortality outcomes. Between 2002 and 2018, 2246 municipalities (33.8%) were exposed only to the LHA consolidation, 1622 (24.4%) only to the DRP, 1748 (26.3%) to both, and 1030 (15.5%) to neither. These figures highlight the importance of accounting for DRPs when identifying the effects of LHA consolidation.

To address this issue, we explicitly condition on DRP exposure through flexible, non-parametric trend adjustments. This strategy follows the logic of de Chaisemartin and D'Haultfoeuille (2023), who show that, when multiple policies affect the same outcome, estimates of the effect of one policy may be contaminated by the effects of the others unless the identifying variation is restricted or conditioned so as to hold the other treatments fixed. Specifically, we consider two types of adjustments: (i) DRP exposure-by-year fixed effects; (ii) DRP cohort-by-year fixed effects, which capture the timing of DRP entry at the regional level. The second specification offers a finer adjustment, as it accommodates the staggered rollout of DRPs and identifies the effect of LHA consolidation net of mortality dynamics associated with DRP exposure. To reinforce the credibility of our findings, we also estimate separate event-study models for municipalities in regions with and without DRP exposure (Section 5.2).

²² A large set of alternative estimation approaches has been developed in recent years. We can broadly distinguish between approaches based on weighting up cohort-specific ATT (e.g., Sun and Abraham, 2021; Callaway and Sant'Anna, 2021), focusing on 'first-time switchers' (de Chaisemartin and d'Haultfoeuille, 2020), using "imputation" estimators of counterfactual outcomes for treated units (Gardner, 2022; Borusyak et al., 2024; Wooldridge, 2025), and stacking data through balancing in relative event time (Cengiz et al., 2019).

²³ In the excluded age group (below age 50), deaths account for about 3.8% of total mortality between 2002 and 2018; the corresponding shares are 8.8% for ages 50–64, 15.2% for ages 65–74, 32.2% for ages 75–84, and roughly 40% for ages above 85.

²⁴ Adjusting for age structure is necessary when covariates cannot be expressed in an age-standardized form (Bauernschuster et al., 2020; Rosenbaum and Rubin, 1984).

²⁵ Municipalities with zero deaths in all years are excluded because, under statistical separation, maximum-likelihood estimates in Poisson models do not exist. This exclusion primarily occurs when we study age-specific mortality for the 0–14 and 15–24 age groups, resulting in a non-negligible reduction in sample size.

4.3. Alternative control groups

We assess the robustness of our baseline specification by restricting the sample to treated and never-treated municipalities that are spatially proximate. Focusing on geographically close municipalities, these model specifications strengthen internal validity by limiting the influence of unobservable regional heterogeneity in health systems and other potential confounders.

To further address potential threats to identification, we implement additional sample restrictions aimed at mitigating two distinct sources of bias. First, to limit the influence of cross-regional differences in healthcare systems, we estimate the model on a restricted sample composed of regions that exhibit internal variation in LHA consolidation and regions that never adopted the policy, excluding those regions where all LHAs were subject to consolidation.

Second, we consider the potential for geographic spillovers across LHA boundaries. Since patients may access care outside their LHA of residence, consolidation in one area could indirectly affect healthcare in neighboring municipalities. To account for this, we exclude from the control group all never-treated municipalities that are proximate to any treated municipality.

5. Results

5.1. Effects on total mortality

This section presents annual event-study estimates from Eq. (1), and analyzes the dynamic effects of LHA mergers on total municipal mortality rates (all causes, all ages). The event-study window ranges from six years before to nine years after consolidation. To avoid producing long-run estimates based on a small number of treated municipalities, the data are binned at $t = -7$ and $t = 10$. This prevents sparsely populated tails in event time from influencing the results.²⁶

We compare three specifications that differ in how they control for DRP exposure. The baseline includes only municipality and year fixed effects. The second replaces the year fixed effects with non-parametric DRP trends, implemented by interacting a DRP indicator with year dummies. The third, which we adopt as our preferred specification, further refines this approach by interacting DRP cohort indicators with year dummies, thereby accounting for heterogeneity in DRP timing across regions. This more granular approach mitigates the risk of over-adjustment – where a coarse binary trend might absorb variation attributable to LHA consolidation. Results are reported in Table A4 in [Appendix](#). The estimates maintain similar levels of precision.²⁷ We interpret this as evidence that the preferred specification better isolates the identifying variation without introducing excess noise or instability. The similarity of results across specifications reflects the fact that the DRP adjustments affect the underlying two-by-two comparisons in opposite directions, so their net effect on the aggregated ATT is small.²⁸

[Fig. 4](#) compares the event-study estimates obtained using IW (column 3 of Table A4) and the TWFE estimators. Both lines in the plot indicate the absence of differential trends in the years preceding the LHA mergers, and a positive and statistically significant increase in mortality starting four years after the merger. The TWFE estimates are smaller, suggesting an underestimation of average treatment effects at each event time. The IW estimator produces evidence of a proportional (percentage) increase in the mortality rate in the post-treatment period, ranging from 1.6% (at $t + 4$) to 2.7% (at $t + 7$). These changes should be interpreted relative to a baseline average of 993.2 deaths per 100,000 inhabitants in the year preceding the reform. As reported in Table A5 in the [Appendix](#), the ATT in the entire post-treatment period indicates an increase of 1.3% in the death rate. This rises to 2.1% when restricting the effect window between $t + 4$ and $t + 9$, during which the policy has a statistically significant and persistent impact on mortality.²⁹

As a diagnostic check, we estimate the event-study model using the logarithm of municipal per capita taxable income as the outcome. Following [Pei et al. \(2019\)](#), this exercise allows us to assess whether treated and control municipalities exhibit differential pre-treatment income trends. We find no evidence of differential pre-trends and a statistically significant increase in income starting from the second year after the reform ([Figure A4](#) in the [Appendix](#)). This post-treatment increase is not straightforward to interpret and may reflect changes in the composition of the local tax base. Hence, if anything, mortality increased despite the observed income improvements, suggesting that the adverse health effects are unlikely to be driven by deteriorating local economic conditions. This is also reassuring for our baseline specification, where income enters among the time-varying controls. We do not apply the same test to population age shares, as they play a mechanical role in adjusting for demographic structure when using crude mortality rates.

To assess the robustness of our findings to a different source of identifying variation, we estimate the model using the municipalities treated in 2017 – the last cohort to undergo consolidation – as the control group, rather than the never-treated. This strategy restricts the comparison to treated units that are likely to share unobserved characteristics associated with fiscal stress and cost-containment pressures. This specification excludes never-treated units entirely and omits the two most recent years (2017

²⁶ This choice does not affect the coefficients within the window from $t - 6$ to $t + 9$, as shown in [Figure A3](#) in the [Appendix](#), which compares the binned specification with the unbinned one.

²⁷ Standard errors are clustered at the LHA level; results remain stable and robust when clustering at the municipal level.

²⁸ On one side, such adjustments remove a component that would otherwise inflate the raw mortality gap between municipalities exposed to both policies (26.3%) and those never exposed to either; on the other, they remove a component that would otherwise compress the raw gap between municipalities exposed only to LHA consolidation and those exposed only to DRPs (24.4%).

²⁹ Table A5 also reports the estimates obtained using the [Gardner \(2022\)](#) “two-stage DiD” estimator, which is well suited to incorporating time-varying controls. The estimator first residualizes the outcome by regressing it on unit and time fixed effects, and then regresses the residuals on the treatment indicators for each cohort and event time. The results, shown in the third column of the table, are largely consistent, although the ATTs are slightly smaller.

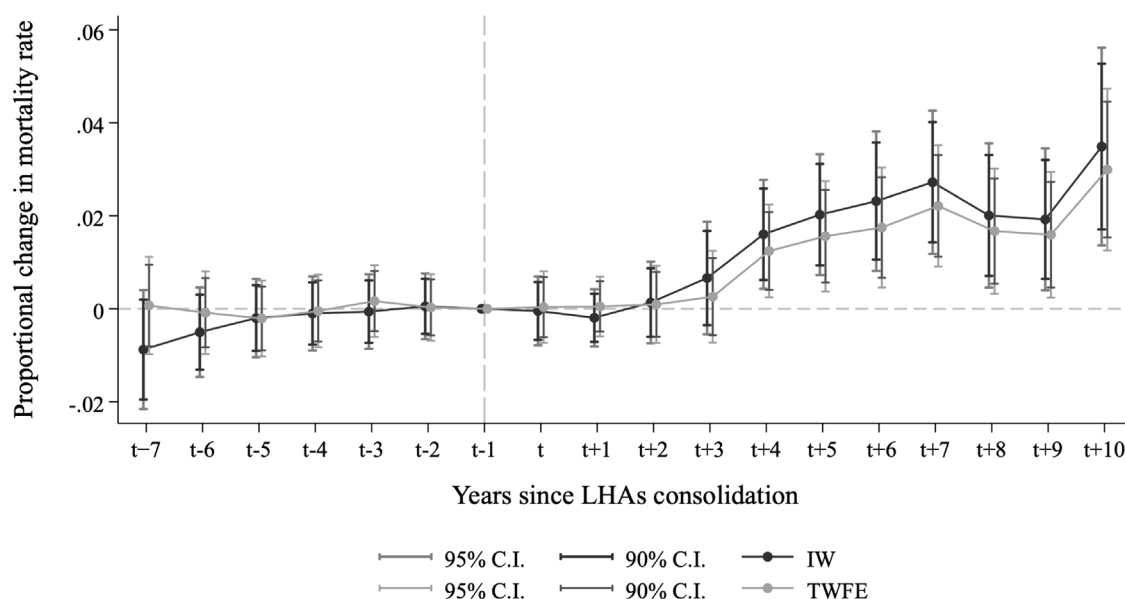


Fig. 4. Effects of LHAs consolidation on total mortality rate.

Notes: Dots indicate the estimated coefficients for each year before and after the consolidation reform using data for the period 2002–2018 (see Eq. (1)) for the TWFE estimator and the IW estimator by Sun and Abraham (2021). The TWFE and interacted regression are estimated by OLS using the population in 2002 as weights, including municipality fixed-effects and Debt Rescheduling Plan cohort-by-year fixed effects. The relative time before consolidation, $t = -1$, is excluded and indicated by the vertical line. The left endpoint is binned at $t = -7$, and the right endpoint is binned at $t = 10$. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. The models include time-varying controls: the shares of individuals aged 50–64, 65–74, 75–84, and 85 and older, and per capita income. Standard errors are clustered at the pre-reform LHA level. Confidence intervals are indicated at the 90 and 95 per cent level.

and 2018), for which the cohort-specific treatment effects are unavailable. Results are presented in Figure A5 in the Appendix, which compares event-study estimates based on never-treated controls and last-treated controls. While both series indicate an increase in mortality following consolidation, estimates based on the last-treated sample are systematically larger, with the effect emerging from $t + 1$ and reaching approximately 3.5% above the baseline mortality level (993.2 deaths per 100,000 residents) within five to seven years. However, this specification may be subject to imbalanced confounding, as the last-treated group differs systematically from earlier-treated municipalities. In particular, only 14% of the municipalities in the last-treated cohort were exposed to the DRP reform (and only in 2007), whereas DRP exposure is more widespread and heterogeneous among earlier-treated units. This limits the ability of the DRP cohort-by-year fixed effects to fully capture policy-driven regional heterogeneity. The resulting imbalance likely contributes to the appearance of significant pre-treatment estimates (notably at $t - 3$ and $t - 2$), which may reflect residual confounding rather than genuine violations of the parallel trends assumption. Taken together, the estimates from this alternative comparison likely represent an upper bound of the effect, and their interpretation should be viewed in light of these limitations.

The direction and intensity of the consolidation's impact on mortality are somewhat unexpected. In view of that, we are particularly concerned that the rejection of the null hypothesis of no effects may be driven by unobserved trends – such as population aging not adequately accounted for through population shares – that are not captured by time-fixed effects or unobserved heterogeneity. To address these concerns and assess the robustness of our findings, we conduct a placebo analysis by simulating alternative patterns to the observed policy. Specifically, we generate 1000 “fake merger policies” that replicate the nature of the actual policy while maintaining its essential structure: the policies affected only groups of municipalities (LHAs) and never individual municipalities. Treatment status is randomly assigned to municipalities by varying the policy year, the regions involved, and the LHAs within those regions. This ensures that all municipalities within an LHA are either treated – if their LHA is merged with another – or untreated, otherwise.³⁰ We estimate our main model for each of the 1000 fake policies and examine whether these simulated mergers detect a statistically significant change in mortality rate. Fig. 5 illustrates the results: the effects of the actual consolidation policy (the red connected circles), the point placebo estimates (the small gray dots), and the average of the placebo estimates (the black connected squares). As the figure shows, the fake amalgamations neither significantly affect mortality nor exhibit significant pre-trends. This confirms the specificity of the effects estimated for the actual amalgamation policy.

³⁰ Given that fake treatment times and fake treatment units are both considered, our exercise represents a “mixed placebo” test.

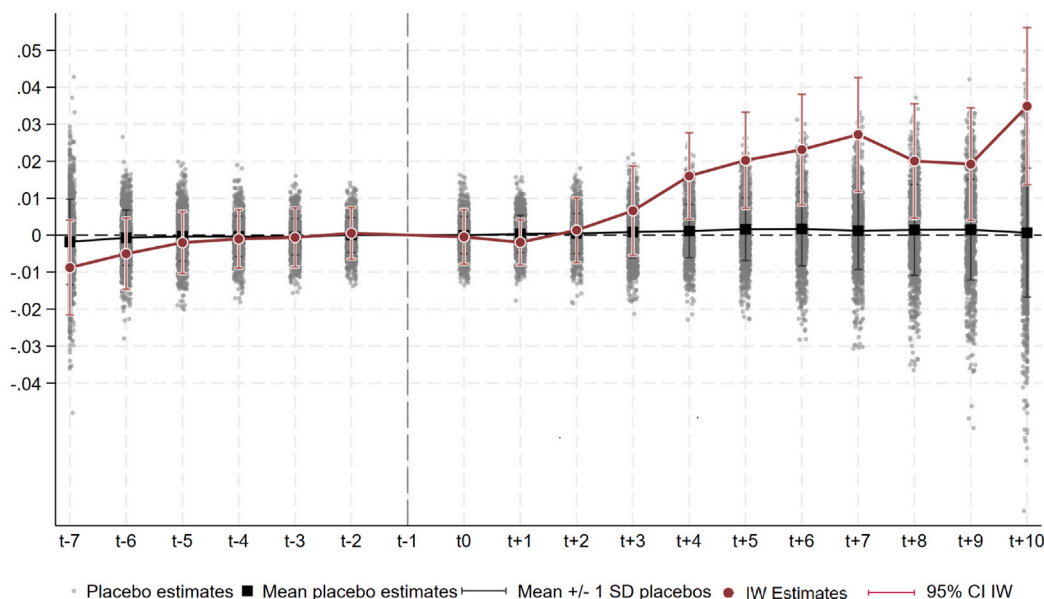


Fig. 5. Placebo estimates of LHAs consolidation on total mortality rate.

Notes: Small dots represent point estimates for 1000 “fake merger policies” randomly generated. The black connected squares indicate the mean of the placebo estimates; the red connected circles represent the point estimates obtained for the actual consolidation policy. The IW estimator by Sun and Abraham (2021) is used for all models. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. Confidence intervals are indicated at the 95 per cent level. See the note of Fig. 4 for details. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

5.2. Consolidation effects net of debt rescheduling plans

Fig. 6, panel *a*, reports event-study estimates of the DRP’s impact on mortality using the same empirical framework adopted for LHA consolidation.³¹ This analysis serves two purposes. First, it confirms that DRPs have a statistically significant and persistent impact on mortality within our sample, consistent with existing literature. Second, it reinforces the need for accounting for DRP exposure in the main analysis. The plot compares the full sample, which includes all municipalities, with the subsample of those exposed to LHAs consolidation at some point, which could potentially confound the causal effect of DRPs. Estimates indicate that DRP adoption leads to an average increase in mortality rate of approximately 1.7%, rising to 2.8% from four to nine years after implementation. When restricting the sample to municipalities never exposed to LHA mergers, the effect is slightly larger, suggesting that the estimated DRP impact is not driven by overlap with the consolidation reform. Panel *b* of Fig. 6 compares the event-study estimates for LHA consolidation separately for municipalities not exposed to DRPs and for municipalities exposed to DRPs.³² The results indicate that among municipalities exposed to DRPs, LHA consolidation has no additional detectable effect on mortality. By contrast, where DRPs are absent, LHA mergers exert an independent effect on mortality: the trend steepens significantly from $t + 5$ onward.³³ Overall, these results suggest that the adverse impact of consolidation is unlikely to be attributable to co-occurring policy shocks.

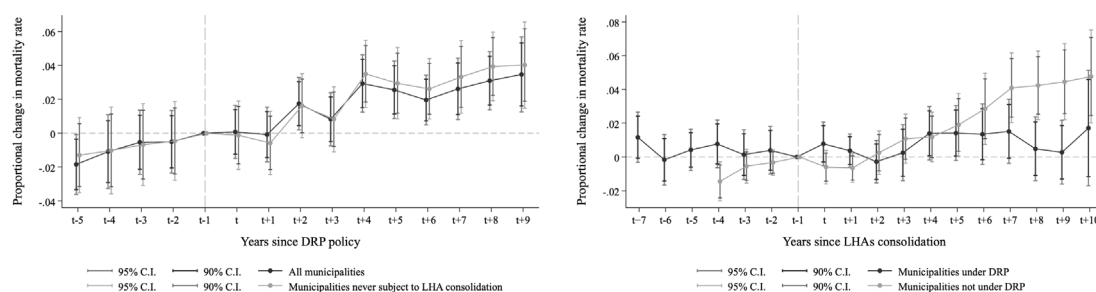
5.3. Robustness to spatial and regional heterogeneity

We discuss the robustness of our findings to different sources of variation in identification. First, we explore the robustness of our estimates to alternative spatial sampling restrictions. We restrict the sample to include only treated and never-treated municipalities

³¹ The event-study uses the $t - 5$ to $t + 9$ window to guarantee a sufficiently long post-treatment horizon with the support available across DRP cohorts, while minimizing pre-treatment imbalance. The model includes LHA cohort-by-year fixed effects to adjust for potential confounding from the co-occurring consolidation policy. Standard errors are clustered at the region-year level. Results are robust to clustering at the regional level and are available upon request.

³² The specification bins the leads at $t - 4$ due to limited support in earlier periods. In the subsample of municipalities not exposed to DRPs, treatment timing is concentrated in the later adoption waves (2016–2017), which reduces comparability in the distant pre-treatment years. Left-binning improves balance and avoids placing excessive weight on sparsely populated event-time cells. The coefficient at $t - 4$ is slightly positive, suggesting that the evidence on pre-treatment dynamics in this subsample should be interpreted with some caution.

³³ This pattern is consistent with the supplementary evidence reported in Figure A6, which compares the main event-study estimates reported in Fig. 4 with those obtained from a specification restricted to municipalities never exposed to DRPs.



a. DRP policy

b. LHAs consolidation

Fig. 6. Effects of LHAs Consolidation and Debt Rescheduling Plan policies on total mortality rate.

Notes: In each figure, dots indicate the estimated coefficients for each year before and after the reform, Debt Rescheduling Plan (Panel *a*) and LHAs consolidation (Panel *b*), using data for the period 2002–2018 (see Eq. (1)). The IW estimator by Sun and Abraham (2021) is used. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. All models include time-varying controls: the shares of individuals aged 50–64, 65–74, 75–84, and 85 and older, as well as per capita income. The models estimating the effect of the DRP policy in municipalities never exposed to LHA consolidation, and the effect of LHA consolidation in municipalities not subject to a DRP, include year fixed effects rather than non-parametric trends for the co-occurring policy. Confidence intervals are indicated at the 90 and 95 per cent level. For details on the specification, refer to the note in Fig. 4.

that are spatially proximate, using three distinct definitions of proximity. Specifically, by using inter-municipal travel times released by Istat (“Matrici di contiguità, distanza e pendolarismo”), we consider municipalities within a 30-minute travel distance, within a 20-minute travel distance, within a 15-minute travel distance, and municipalities sharing a border (adjacent). These restrictions aim to improve comparability by limiting the analysis to treated and control municipalities that are more likely to share observable and unobservable characteristics relevant for mortality trends, such as geography, weather, pollution exposure, demographics, and other factors that may affect mortality rates and trends. These specifications also strengthen internal validity by reducing the risk of bias from regional heterogeneity in health systems and unobserved confounders. However, these spatial restrictions come at a cost in terms of statistical power and coverage.³⁴

Fig. 7 presents event-study estimates for the total mortality model, comparing the main sample to the four sub-samples defined above. The findings confirm the policy’s detrimental effects, with estimates being notably large and statistically significant, especially in the samples of municipalities within 30- and 20-minute travel distances (Panels *a* and *b*). In contrast, estimates from the most restrictive sample – adjacent municipalities (Panel *d*) – should be interpreted with caution. The smaller sample size results in wider confidence intervals and lower estimation precision. While a few pre-treatment coefficients are statistically significant, they do not point to a clear systematic pre-trend. If anything, mortality appears to decline from $t - 3$ up to the onset of the policy, which would be consistent with consolidation reversing a previously favorable trend. However, we do not over-interpret these coefficients, as these fluctuations likely reflect sampling variability rather than genuine violations of the parallel trends assumption.

Moreover, because regional heterogeneity may influence mortality dynamics, we first check whether the estimated medium-run effect is driven by a specific region. We repeatedly re-estimate our preferred specification, excluding one region at a time from the sample, and compute the corresponding average post-treatment effect. Figure A7 in the Appendix plots these estimates, along with the 95% confidence intervals, and reports the benchmark $ATT_{t+4:t+9}$ obtained from estimation on the full sample. The results indicate that the medium-run effect remains positive in all specifications and that the point estimates cluster tightly around the full-sample benchmark. No single region exclusion can materially shift the effect or eliminate it.³⁵ Among the regions whose exclusion generates a relatively larger reduction in the effect, we find both northern (e.g., Piemonte) and southern (e.g., Campania) regions where LHA mergers occurred. Symmetrically, the regions whose exclusion leads to a mild amplification of the ATT include both northern (e.g., Liguria) and southern (e.g., Sicily) areas. Overall, these patterns confirm that the aggregate effect does not reflect an isolated regional episode but instead arises across diverse institutional and demographic contexts.

Time-varying mortality dynamics at the regional level could threaten identification if reforming regions were already on different mortality trajectories prior to consolidation. To assess this concern, we estimate two complementary specifications. The first replaces the DRP cohort-by-year fixed effects with calendar-year fixed effects interacted with an indicator for whether the municipality

³⁴ As proximity criteria become more stringent, the number of usable units declines significantly: relative to the full sample, the number of treated municipalities decreases by approximately 55%, 73%, 84%, and 86% under the 30-minute, 20-minute, 15-minute, and adjacency thresholds, respectively; the corresponding reductions among never-treated municipalities are 40%, 64%, 78%, and 80%. This asymmetric reduction alters the treated-to-control ratio and limits the variation available for estimating treatment effect heterogeneity across subgroups (e.g., absorbed vs. absorbing LHAs, LHA size increase, urban vs. rural municipalities). As a result, these specifications are best interpreted as robustness checks on the internal validity of the identification strategy, rather than as alternative baseline designs.

³⁵ This includes Lombardia, whose institutional features and population size make it a natural candidate for a leverage check. Excluding all municipalities of region Lombardia leaves the medium-run $ATT_{(t+4:t+9)}$ unchanged (0.021), with respect to the full-sample estimate.

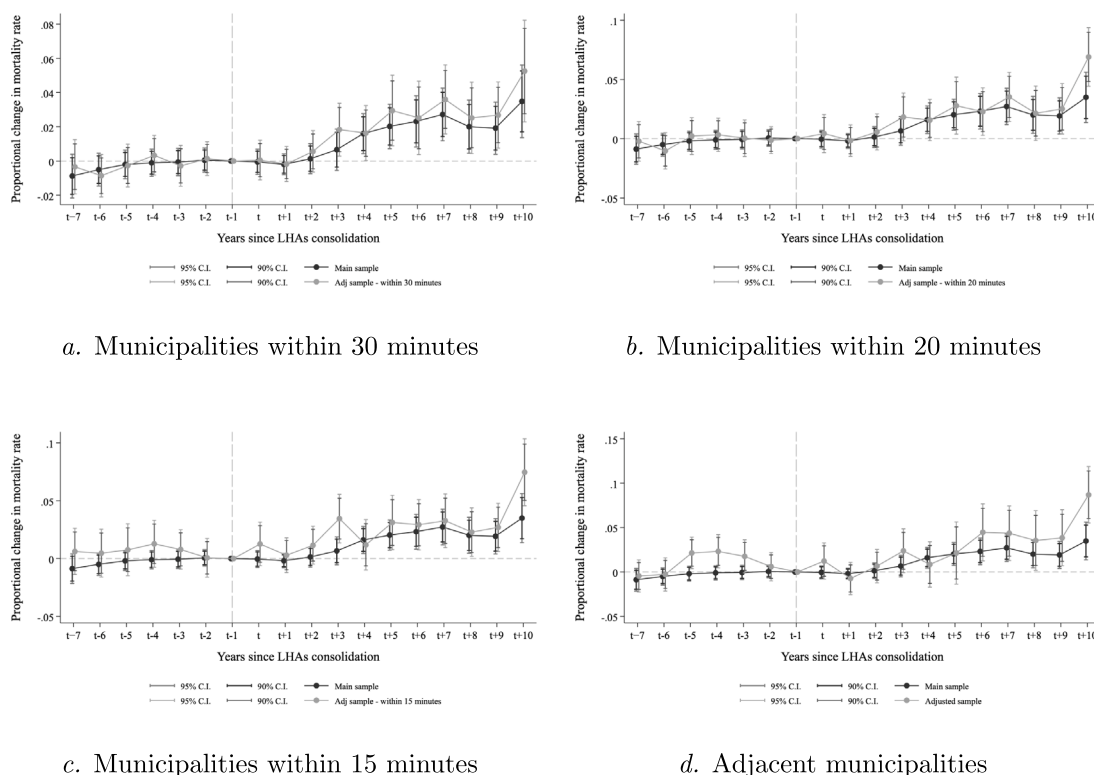


Fig. 7. Effects of LHAs consolidation on total mortality rate using sample restrictions based on distance.

Notes: Dots indicate the estimated effect for each year before and after the consolidation reform, using the IW estimator by [Sun and Abraham \(2021\)](#), (see Eq. (1)), on data from 2002 to 2018. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. Each estimation considers alternatively treated and never-treated municipalities within a 30-, 20-, and 15-minute travel distance, and municipalities sharing a border (adjacent). For estimation details, refer to the note in [Fig. 4](#).

belongs to a region that ever implemented LHA consolidation. This model is less saturated than region-by-year fixed effects, but it absorbs year-specific shocks common to reforming regions and tests whether the estimates are driven by differential mortality dynamics in regions that chose to consolidate their LHAs.³⁶ The second re-estimates the baseline model on a restricted sample composed of partially treated regions, which display internal variation in LHA consolidation, and regions that never adopted the policy. This exercise excludes fully treated regions, which provide no untreated municipalities within the same regional health system, thereby giving greater relative weight to comparisons within regions that display internal treatment variation.³⁷ Figure A8 in the [Appendix](#) compares these specifications with the baseline model. Both alternatives produce dynamic patterns close to the baseline estimates. The specification with ever-consolidating-region-status-by-year fixed effects shows no evidence of differential pre-treatment trends and yields positive, statistically significant effects from the medium run onward. The restricted-sample estimates are also similar, although slightly less precise at longer event times, as expected given the exclusion of some early-adopting regions. Overall, these exercises suggest that the estimated mortality increase is not driven by pre-existing mortality dynamics common to reforming regions, nor does it rely exclusively on the across-region component of the identifying variation.

Finally, we consider the possibility of geographic spillovers across LHA boundaries. Given that patients in Italy can access services outside their LHA of residence, consolidation in one LHA may influence neighboring LHAs through changes in resource allocation, access to care, or patient flows. To test for such spillovers, we re-estimate the model excluding from the control group all municipalities that were never treated and are either adjacent to or within a 30-minute travel distance of any treated municipality. This restriction ensures that control units are unlikely to be indirectly affected by consolidation occurring nearby. The resulting sample includes all treated municipalities and 2138 never-treated municipalities (approximately 65% of the original control group).

³⁶ In principle, region-by-year fixed effects would provide a more saturated way to control for region-specific shocks. In our setting, however, they are not feasible in the preferred specification because they are not jointly identified with the DRP cohort-by-year fixed effects. These cohort-based effects represent a more economically and institutionally meaningful dimension of heterogeneity than purely geographic groupings. Moreover, since LHA consolidation was decided at the regional level and implemented with staggered timing, a fully saturated set of region-by-year fixed effects would absorb most of the identifying variation.

³⁷ The control group in this specification includes both untreated municipalities located in partially treated regions and municipalities located in never-consolidating regions.

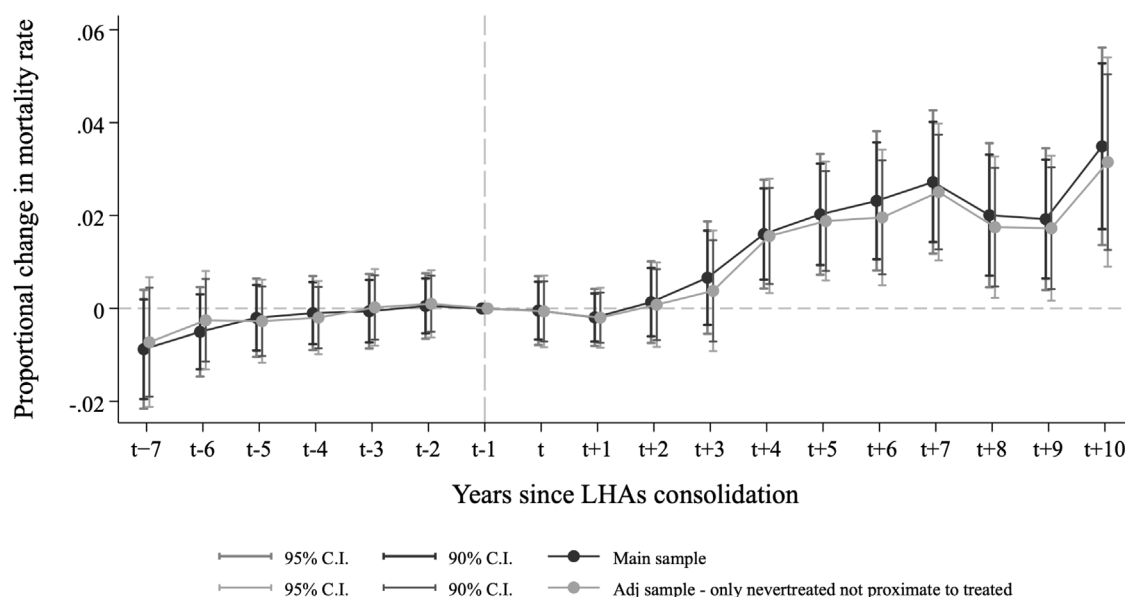


Fig. 8. Effects of LHAs consolidation on total mortality rate controlling for spillover effects.

Notes: Dots indicate the estimated effect at a given event time relative to the year of LHA consolidation, using the IW estimator by Sun and Abraham (2021) (see Eq. (1)), on data from 2002 to 2018. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. Estimates are presented for the main sample and for a specification that excludes from the control group all that never-treated municipalities that are located either adjacent to or within a 30-minute travel distance of any treated municipality. See Fig. 4 for estimation details.

Fig. 8 compares event-study estimates from the main sample and the restricted sample. The two sets of estimates are nearly identical, both in magnitude and in the pattern of dynamic effects. This supports the interpretation that, if any, spillover effects are limited and that the main results are not driven by contamination of the control group.

5.4. Effects on age-specific mortality

We further investigate the effects of the consolidation policy across specific age groups: the youngest population (ages 0–14), the working-age population (divided into young adults aged 15–24, adults aged 25–49 and older adults aged 50–64), the retirement-age population (ages 65–74), and the oldest old population (ages 75 and above). This granular age categorization uncovers important heterogeneity in the policy's impact across demographic segments. Due to the presence of a mass point at zero in municipal-level age-specific death counts, we estimate all models using the PPML – TWFE estimator as described in Section 4.

Table 1 presents the ATTs, while the full set of event-study estimated coefficients is shown in Figure A9 in Appendix. The results indicate that the percentage increase in the mortality rate is particularly pronounced among the working-age population (ages 25–49 and 50–64), while for the oldest old (75 and older) the increase is somewhat smaller, though still statistically significant. No significant effect is detected for the youngest sub-populations (ages 0–14 and 15–24) and adults of retirement age (65–74). In particular, for the 25–49 age group, as shown in Figure A9, mortality rates begin to rise noticeably from year $t+5$ onward, stabilizing from $t+7$ onward. Among older working-age adults (50–64), the effect also becomes statistically significant at $t+5$ and reaches its peak at $t+7$. Between $t+4$ and $t+9$, the ATTs are 4.4% for the 25–49 age group and 3.2% for the 50–64 age group. Given baseline mortality rates of 83 and 431 deaths per 100,000 individuals in the respective age groups, these estimates imply average increases of approximately 4 and 14 deaths per 100,000 individuals per year. The detrimental effect of consolidation is also evident among the oldest segment of the population (75 and older). As shown in Figure A9, the increase in mortality rate peaks at around 1.5% seven years after the policy, with an ATT in the effect window ($t+4$; $t+9$) of 1.1%. Given a baseline mortality rate of 7110 per 100,000, this corresponds to approximately 78 additional deaths per 100,000 individuals per year.

With the currently available data, we cannot provide a definitive account of the underlying mechanisms. However, several plausible channels may explain the observed age-specific patterns. These include disruptions in service provision, increased organizational complexity, delayed access to care, and potential under-responsiveness to the differentiated healthcare needs of the working-age and elderly populations in the aftermath of consolidation.

5.5. Effects decomposition by causes of death

To unfold the possible underlying drivers of the policy's impact on total mortality, we estimate Eq. (1) using mortality rates calculated for specific types of deaths. The total mortality rate is disaggregated by the major causes of death (those accounting for

Table 1
Effects of LHAs consolidation on age-specific mortality rates.

	(1) 0–14	(2) 15–24	(3) 25–49	(4) 50–64	(5) 65–74	(6) 75 and older
ATT	0.031 (0.048)	−0.037 (0.052)	0.026 (0.016)	0.023** (0.009)	−0.001 (0.009)	0.006 (0.005)
ATT _(t+4;t+9)	0.027 (0.052)	−0.013 (0.056)	0.044** (0.019)	0.032*** (0.011)	−0.004 (0.011)	0.011* (0.006)
Baseline average	27.889	31.739	83.079	431.090	1382.872	7110.305
pseudo-R ²	0.400	0.344	0.667	0.795	0.852	0.950
Observations	84 915	83 623	112 132	112 982	112 982	112 982

Notes: Average treatment effects on the treated (ATTs) are estimated using the PPML-TWFE estimator, on data from 2002 to 2018. The dependent variable in each model is the number of deaths in a specific age group; the corresponding population is included as exposure variable. All models control for per capita income, municipality fixed effects and DRP cohorts-by-year fixed effects. Standard errors are clustered at the pre-reform LHA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2
Effects of LHAs consolidation on mortality rate by causes of death.

	(1) Circulatory system diseases	(2) Cancer	(3) Respiratory system diseases	(4) All other causes
ATT	0.020*** (0.007)	0.012* (0.006)	0.021 (0.014)	−0.008 (0.008)
ATT _(t+4;t+9)	0.030*** (0.009)	0.018** (0.008)	0.022 (0.017)	−0.005 (0.010)
Baseline average	376.665	283.448	71.043	262.034
pseudo-R ²	0.917	0.905	0.764	0.893
Observations	112 982	112 982	112 846	112 982

Notes: Average treatment effects on the treated (ATTs) are estimated using the PPML-TWFE estimator in an event-study model, on data from 2002 to 2018. The dependent variable in each model is the number of deaths by cause group; population is included as exposure variable. All models control for per capita income, municipality fixed effects and DRP cohorts-by-year fixed effects. Standard errors are clustered at the pre-reform LHA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

more than 5% of total deaths) and also according to the OECD/Eurostat classification of preventable and treatable deaths, as defined in Section 3. Estimates are based on the PPML-TWFE, due to the presence of zero deaths at the municipal level in these specific outcomes.

The analysis in Table 2 and Figure A10 in the Appendix highlights that the adverse impact on total mortality is primarily driven by increased deaths caused by diseases of the circulatory system and cancers. In particular, for deaths related to circulatory system diseases (Panel a), the effect is persistent over time, becoming statistically significant five years after consolidation and peaking in year $t + 7$. By contrast, the effect on cancer-related deaths reaches its maximum at $t + 6$, while barely significant at the 10% level in most of the other periods between $t + 4$ and $t + 9$. As shown in columns 3 and 4 of Table 2, no effects are detected for mortality related to respiratory system diseases and for the residual category.

Effective health policies are expected to reduce the number of avoidable deaths. For this reason, using deaths due to preventable and treatable causes as outcomes is particularly relevant. These causes range from chronic diseases requiring continuous management (e.g., hypertensive disease, thyroid disorders) to acute conditions requiring urgent intervention (e.g., sepsis, acute myocardial infarction, stroke). For such conditions, structured care pathways, such as screening, follow-up, and emergency response protocols, are often in place, making them more likely to receive systematic monitoring and clinical attention. By contrast, conditions outside the avoidable mortality list, namely those referred to as “less preventable and less treatable” mortality, are by definition less responsive to preventive or clinical action.

Table 3 reports the ATTs for total avoidable mortality (Column 1) and its two components, preventable and treatable mortality (Columns 2 and 3), as well as for less avoidable mortality (last column), in the population aged 0–74.³⁸ Event-study estimates are presented in the Appendix (Figure A11). The effect on total avoidable mortality is modest. However, its decomposition reveals that the overall estimate is primarily driven by the treatable subcategory. This pattern is consistent with the nature of the two categories: treatable causes are more directly affected by healthcare system performance, including timely diagnosis and effective treatment, whereas preventable causes depend more on broader public health and behavioral factors that may be less immediately sensitive to changes in LHAs’ service delivery. Interestingly, the policy also appears to increase mortality in the “less preventable and less treatable” category. A plausible interpretation is that consolidation may have generated indirect effects (such as reduced continuity of care and organizational disruption) on less monitored domains, where performance incentives are weaker or less defined and where clinical or managerial attention may have declined.

³⁸ In the 0–74 age group, less avoidable causes account for approximately 21% of all deaths but represent 58.3% of deaths within this age range. Therefore, focusing on less avoidable mortality provides a more accurate basis for assessing the impact of healthcare interventions on this population segment.

Table 3

Effects of LHAs consolidation on mortality rate for avoidable and not avoidable causes.

	(1) Preventable and treatable	(2) Preventable	(3) Treatable	(4) Less preventable and less treatable
ATT	0.015* (0.009)	0.010 (0.011)	0.018* (0.010)	0.014* (0.007)
ATT _(t+4;t+9)	0.021* (0.011)	0.012 (0.013)	0.025** (0.012)	0.020** (0.009)
Baseline average	85.767	24.447	61.320	210.824
pseudo-R ²	0.787	0.645	0.755	0.874
Observations	112 965	112 829	112 965	112 982

Notes: Average treatment effects on the treated (ATTs) are estimated using the PPML-TWFE estimator in an event-study model, on data from 2002 to 2018. The dependent variable in each model is the number of deaths by cause group in the population of age 0–74; population is included as exposure variable. All models control for per capita income, municipality fixed effects and DRP cohorts-by-year fixed effects. Standard errors are clustered at the pre-reform LHA level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.6. Heterogeneity analysis

In this section, we examine heterogeneity in the mortality effects of LHA consolidation. First, we distinguish between treated municipalities in “merging” and “merged” LHAs. Second, we differentiate between treated municipalities located in LHAs that consolidated exclusively with LHAs of the same pre-merger size category (small or large) and those involved in consolidations between small and large LHAs. We then assess whether the reform’s impact varies with municipal size, LHA size, and the magnitude of the scale change induced by the merger. Finally, we examine whether the policy’s effects display heterogeneous spatial patterns, particularly with respect to remoteness, in light of the persistent health disparities between urban and rural areas (see, e.g., [Cosby et al., 2019](#)).

The distinction between municipalities in merging LHAs (which retained the original administrative headquarters after consolidation) and merged LHAs (those incorporated into an LHA with a different headquarters) aims to capture differences in administrative continuity and potential disruptions in local governance and service coordination following the reform.³⁹ [Fig. 9](#) shows no clear or persistent effect on municipalities within merging LHAs. While there is a 0.9% decline in mortality in the year immediately following the merger, this effect is short-lived. The only subsequent increase, around 1.6%, appears seven years after consolidation and is statistically weak. In contrast, for municipalities in merged LHAs, the reform has a detrimental impact on mortality, with an ATT of 2%, rising to 2.9% when focusing on the restricted post-merger window ($t+4$; $t+9$) as reported in [Table A6](#) in the [Appendix](#).

We further group treated municipalities according to the pre-merger size of their LHA. LHAs are classified as small or large depending on whether the population they served one year before the reform was below or above the median among treated LHAs. Accordingly, mergers may involve only small LHAs (Small–Small), only large LHAs (Large–Large), both small and large LHAs (Small–Large). [Table 4](#) and [Figure A12](#) in the [Appendix](#) show that the reform is particularly detrimental for municipalities exposed to Large–Large mergers, which account for 28% of treated units, with ATTs of 2.1% and 3.3% in the entire post-treatment period and in the restricted effect window ($t+4$; $t+9$), respectively.⁴⁰ Lower effects, but closer to those estimated using all treated units together, are reported for the Small–Large mergers (1.4% and 2.1%). No effect is detected for the Small–Small mergers, which involved 30% of treated municipalities.

The contrast between the null effects for Small–Small mergers and the stronger effects observed when at least one large LHA is involved naturally raises a related question: whether the mortality response is associated with post-merger LHA size *per se*, or rather with the increase in administrative scale generated by the merger. [Table 5](#) helps disentangle these two dimensions. In Columns (1) and (2), we report separate estimates for municipalities belonging to LHAs below or above the median of the post-merger LHA size distribution (measured in 2018), by which time all treated municipalities in our sample had been exposed to the reform. In each specification, both treated and control municipalities are restricted to the same LHA size category. Conditional on similar post-merger LHA size, treatment effect heterogeneity is limited and the event-study coefficients are generally not statistically significant (see also the left panel of [Figure A13](#) in the [Appendix](#)). By contrast, Columns (3) and (4) split treated municipalities according to whether the increase in LHA catchment-area population is below or above the median, and compare them with a common cohort of never-treated municipalities (see also the right panel of [Figure A13](#)). The picture is starker in this case: the reform has no detectable effect when the expansion in LHA size is below the median, whereas mortality rises among municipalities whose LHAs experienced above-median growth in catchment population. In this group, the ATT reaches 1.6%, increasing to 2.7% in the restricted post-treatment window. Overall, these findings suggest that the adverse health effects are more closely related to the increase in administrative scale induced by the merger than to post-merger LHA size *per se*.

³⁹ Proximity to the administrative headquarters has been emphasized in the literature on decentralization and development as a key determinant of service quality, as it shapes the flow of information and the responsiveness to local needs; see [Dahis and Szerman \(2026\)](#).

⁴⁰ Recent evidence on primary care mergers similarly shows that mergers between larger entities are associated with more negative post-merger outcomes than those involving smaller entities ([Lyu and Zhang, 2025](#)).

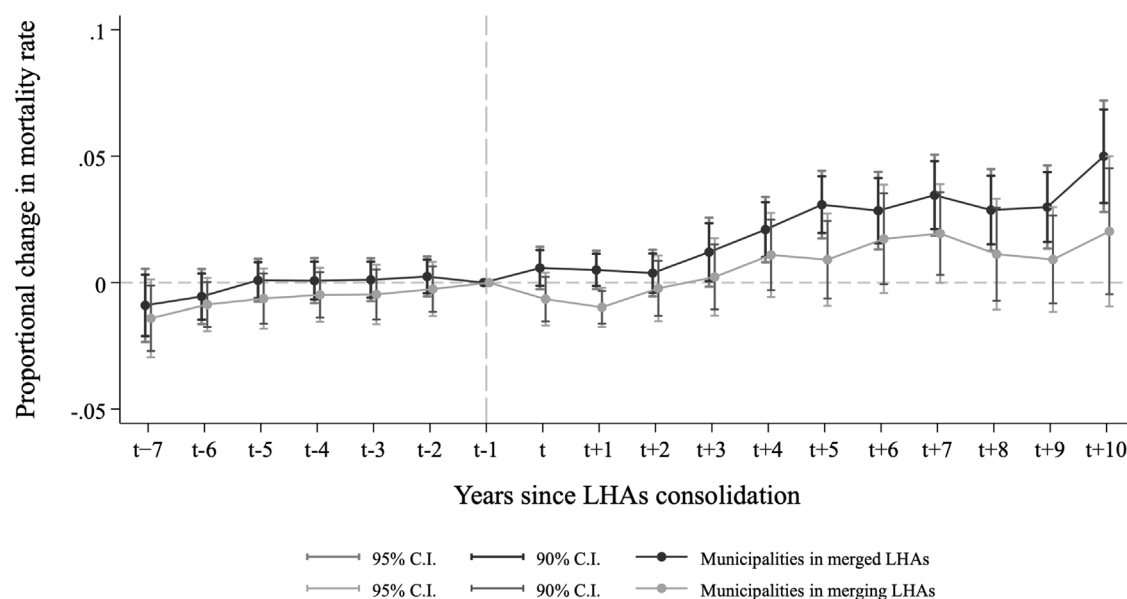


Fig. 9. Effects of LHAs consolidation on mortality rate by merger status.

Notes: Dots indicate the estimated coefficients for each year before and after the consolidation reform using data for the period 2002–2018, (see Eq. (1)), for the IW estimator by Sun and Abraham (2021). The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. All never-treated municipalities are used as controls. Confidence intervals are indicated at the 90 and 95 per cent level. See the note of Fig. 4 for details.

Table 4

Effects of LHAs consolidation on mortality rate by LHAs pre-consolidation size.

	(1) Small–Small LHA	(2) Small–Large LHA	(3) Large–Large LHA
ATT	–0.001 (0.007)	0.014** (0.007)	0.021*** (0.007)
ATT _(t+4;t+9)	0.002 (0.009)	0.021** (0.009)	0.033*** (0.009)
Baseline average	1061.897	1018.700	929.464
R ²	0.773	0.790	0.805
Observations	65 212	73 576	64 362

Notes: Average treatment effects on the treated (ATTs) are estimated using the IW estimator by Sun and Abraham (2021), on data from 2002 to 2018, separately for municipalities in LHAs that merged only with LHAs of the same size category (Small–Small and Large–Large), and those that merged with LHAs of a different size category (Small–Large). The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. All never-treated municipalities are used as controls. See the note of Fig. 4 for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Finally, to assess the role of remoteness in mitigating or amplifying the effects of the consolidation process, we estimate our main model separately for Core, Outlying, Intermediate, Peripheral, and Very Peripheral municipalities. According to the official classification, Core municipalities, representing about 3% of all Italian municipalities, are hubs or inter-municipal hubs that provide all essential public services (local public transport, education, and healthcare services). In the healthcare domain, a Core municipality hosts at least one hospital with an emergency department. Outlying municipalities, which account for roughly 49% of all municipalities, are those with travel times to the nearest Core municipality below the median of actual travel times. Intermediate municipalities (about 24%) have travel times between the median and the third quartile. Peripheral municipalities (about 19%) fall between the third quartile and the 95th percentile, whereas Very Peripheral ones (about 4.8%) are those with travel times exceeding the 95th percentile. Because Very Peripheral municipalities are few in number in our sample, we group them together with the Peripheral category in the analysis. Table 6 reports the estimated ATTs. For Core municipalities, the results are inconclusive due to the small sample size. We find a significant effect of the reform in the group of Outlying municipalities, which manifests four years after consolidation (as shown in Figure A14 in the Appendix), with an ATT of 1.8% in the entire post-treatment period, rising to 2.8% in the restricted window. No effect is found for Intermediate municipalities. For Peripheral and Very Peripheral municipalities, which are traditionally those receiving limited public health investment and are more exposed to access barriers, the effect manifests one year later and is less persistent. The ATT is significant, but only at a lower significance level and within

Table 5

Effects of LHAs consolidation on mortality rate by LHA size and LHA size increase.

	(1) Small LHA	(2) Large LHA	(3) Small LHA size increase	(4) Large LHA size increase
ATT	0.003 (0.006)	0.006 (0.005)	0.010 (0.006)	0.016*** (0.006)
ATT _(t+4;t+9)	0.005 (0.008)	0.012* (0.007)	0.013 (0.008)	0.027*** (0.008)
Baseline average	1048.651	975.278	1024.048	1021.388
R ²	0.733	0.820	0.768	0.810
Observations	56 763	56 219	79 101	78 965

Notes: Average treatment effects on the treated (ATTs) are estimated using the IW estimator by Sun and Abraham (2021), on data from 2002 to 2018, separately for municipalities in LHAs below and above the median of the 2018 LHA size distribution (columns 1 and 2), and for those located in LHAs whose catchment-area population increase is below or above the median (columns 3 and 4). The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. See the note of Fig. 4 for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6

Effects of LHAs consolidation on mortality rate by municipal type.

	(1) Core	(2) Outlying	(3) Intermediate	(4) Peripheral and very peripheral
ATT	0.007 (0.007)	0.018** (0.007)	0.013 (0.009)	0.015 (0.010)
ATT _(t+4;t+9)	0.010 (0.009)	0.028*** (0.009)	0.019 (0.012)	0.026** (0.012)
Baseline average	1070.730	910.712	1046.027	1075.563
R ²	0.947	0.764	0.730	0.702
Observations	3638	57 409	27 251	24 684

Notes: Average treatment effects on the treated (ATTs) are estimated using the IW estimator by Sun and Abraham (2021), on data from 2002 to 2018, separately by municipality type according to the Istat classification of inner areas. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. See the note of Fig. 4 for details. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

the restricted effect window. The point estimates for Peripheral and Very Peripheral municipalities, however, may lack precision, likely because of the different composition in terms of treated and never-treated units, and the wider confidence intervals resulting from the reduced amount of informative variation available to the staggered DiD design. ATTs should therefore be interpreted with caution. Overall, because the most precisely estimated effects arise for Outlying municipalities, the consolidation process appears to have created a new vulnerable area, potentially due to an inadequate allocation of physical and human resources within the newly expanded administrative entities.

Since Outlying municipalities provide the most precise estimates, we restrict the analysis to this group to assess whether the findings documented for the full sample also hold within this subsample. Within Outlying municipalities, we distinguish between those belonging to merged LHAs and those belonging to merging LHAs. We further separate municipalities according to whether their post-merger LHA experienced the lowest or the highest growth in its catchment-area population. Figure A15 in the Appendix confirms that, also within the Outlying group, the mortality increase mainly concerns municipalities in LHA that experienced the largest population growth following the merger, as well as those in merged LHA.

Overall, the heterogeneity analysis indicates that economies of scale, often cited as a potential benefit, do not translate into improved health outcomes when LHAs expand to the large sizes observed in the Italian NHS. Moreover, the resulting mortality effects are spatially uneven and associated with the emergence of new vulnerable areas.

6. Hospital services and potential mechanisms

The heterogeneity analysis presented so far provides important indications on how and why consolidations led to negative health outcomes. However, such analysis does not by itself isolate the specific mechanisms that may have driven the medium-run increase in mortality caused by the LHAs merger. In many regions, it is plausible that consolidation reforms were accompanied by structural adjustments in the hospital sector, either through organizational restructuring aimed at achieving economies of scale, or the extension of the hub-and-spoke configuration – typically applied within hospital networks – to the larger catchment areas of the newly amalgamated LHAs. In this case, as originally remarked by Buchmueller et al. (2006) and more recently confirmed, among others, by Avdic (2016) and Avdic et al. (2024), what matters is not only the potentially higher clinical quality offered by larger

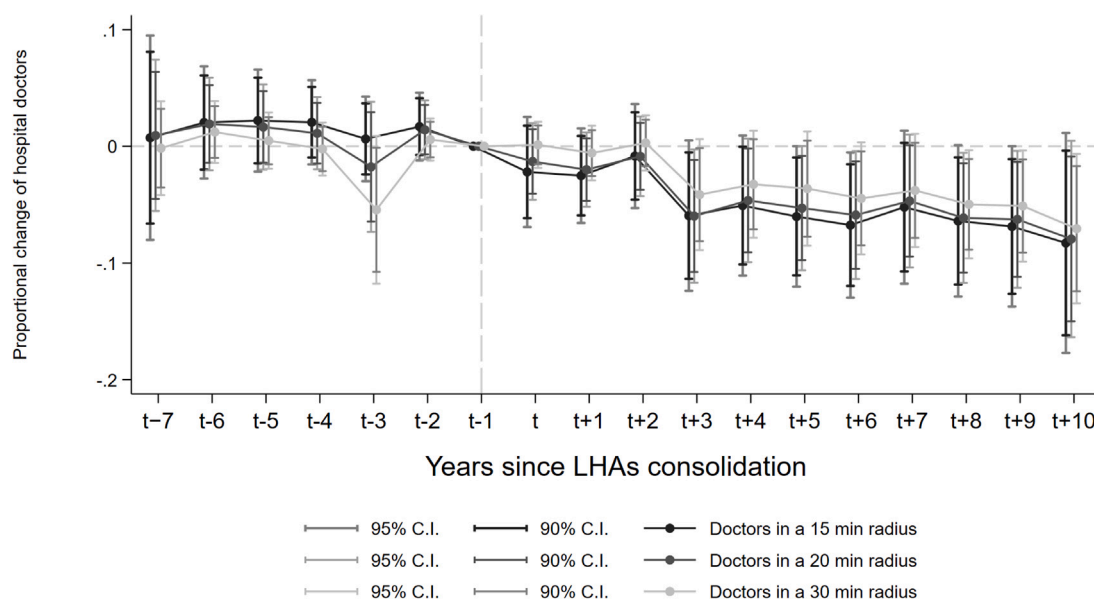


Fig. 10. Effect of LHAs consolidation on local hospital physician availability.

Notes: Dots indicate the estimated effect at a given event time relative to the year of LHA consolidation, using the IW estimator by Sun and Abraham (2021) (see Eq. (1)), on data from 2002 to 2018. The PPML-TWFE estimator is used for all models. The dependent variable in each model is the number of physicians employed in hospitals located within 15-, 20-, and 30-minute travel distance from each municipality; population is included as exposure variable. Confidence intervals are indicated at the 90 and 95 per cent level. See the note of Fig. 4 for details.

hospitals, but also patients' effective access to care, that is their ability to reach timely treatment. Furthermore, when closures occur in rural areas, longer travel distances can substantially reduce such access, leading to worse health outcomes (Carroll, 2023).⁴¹

Hence, a central concern in our analysis is whether the observed effects of consolidation primarily reflect “administrative centralization” or, instead, the “closure” of facilities (e.g., hospitals, outpatient clinics). If the main mechanism were simply the shutdown of hospitals, the interpretation of the reform would be closer to a “rationalization of the hospital network” than to a pure change in governance scale, and the welfare trade-off would largely coincide with that studied in the literature on hospital closures and mergers. For this reason, we now examine whether changes in the organization, availability, and spatial distribution of hospital care can cast further light on the channels through which LHA consolidation affects population health. We focus on three dimensions: (i) changes in the local availability of hospital care, (ii) the incidence and geographical pattern of hospital closures, and (iii) the extent to which baseline access to hospital services influences the mortality response to consolidation.

To capture changes in hospital care availability, we collected data on the evolution of hospital services in the period under study. In particular, we obtained from the Ministry of Health the complete record of public and accredited private hospitals in Italy, including information on the number of physicians employed and the municipality in which each hospital is located.⁴² By combining these data with inter-municipal travel times, we compute, for each municipality in our sample, the total number of physicians employed in the hospitals located within 15-, 20-, and 30-minute travel time radii. This measure can be indirectly affected by the consolidation process through two potential channels: hospitals incorporated into larger LHAs may experience staffing adjustments; moreover, accredited private hospitals could, in principle, be affected by changes in contractual arrangements with the public payer, including reimbursement ceilings. Since we do not observe these ceilings directly, we treat this second channel as a possible but untested mechanism.

Fig. 10 shows a clear pattern emerging around three years after consolidation: relative to never-treated municipalities, treated municipalities experience a statistically significant reduction in the number of hospital physicians within each travel-time distance.⁴³ This occurs despite an overall national increase of 9.4% in hospital physicians between 2002 and 2018. Hence, the divergence between treated and untreated areas reflects a process of centralization in the spatial distribution of hospital care rather than a reduction in aggregate capacity. This shift in the spatial allocation of hospital physicians mirrors the delayed increase in mortality rates estimated in our main model.

⁴¹ Rural closures can also generate negative spillovers in neighboring urban areas (Gujral and Basu, 2019).

⁴² The yearly administrative datasets released by the Ministry of Health contain the universe of the Italian NHS hospital care, which is delivered by publicly managed hospitals and accredited private hospitals. As of 2018, public hospitals account for 68% of hospital beds and approximately 42% of all hospital facilities. The datasets also include information on the hospital type, the number of beds available and in use, and the total number of employees.

⁴³ When we break down the analysis by hospital type, distinguishing between public and accredited private hospitals, we find that the overall pattern is entirely driven by differential changes in local hospital physician availability in public facilities.

Table 7

Share of municipalities affected by hospital closures within different travel-time radii by treatment status and hospital type.

	All municipalities	Never-treated	Treated
<i>All hospitals</i>			
Within 15 min	12.5	8.2	15.4
Within 20 min	11.2	6.3	14.5
Within 30 min	4.9	2.0	6.8
<i>Public hospitals</i>			
Within 15 min	13.6	8.6	16.9
Within 20 min	12.8	7.2	16.6
Within 30 min	6.2	2.9	8.4
<i>Private hospitals</i>			
Within 15 min	1.9	1.6	2.2
Within 20 min	2.6	1.7	3.1
Within 30 min	2.0	1.4	2.4

Notes: The table reports the percentages of municipalities experiencing at least one hospital closure within a 15-, 20-, or 30-minute travel distance during the observation window in the years 2002–2018. Municipalities are classified as exposed to hospital closures when the number of employed physicians drops by at least 90% in a given year and does not recover subsequently. Closures are computed using Ministry of Health data on staffing in all public and accredited private hospitals.

Although the first two decades of the twenty-first century were characterized by substantial reductions in the number of hospital beds nationwide, official hospital closures are uncommon events and are not systematically recorded (Ghislandi et al., 2025). We therefore use staffing information to construct a proxy for hospital closures to which municipalities are exposed.⁴⁴ Specifically, we classify municipalities as exposed to hospital closures when the number of physicians employed in hospitals located within a given travel-time radius (15-, 20-, and 30-minute travel time radii) decreases by at least 90% in a given year and does not subsequently recover to at least 50% of its pre-decline level.⁴⁵ Using this approach, we can measure the share of municipalities experiencing a loss of hospital care access within different travel-time radii. As shown in Table 7, the period under study is characterized by a notable reduction in the spatial granularity of hospital services. Moreover, hospital closures or substantial downsizing occurred disproportionately in areas undergoing LHA consolidation, particularly within the two shortest radii and for public hospitals.

Taken together, the evidence from Fig. 10 and Table 7 might suggest that the medium-run increase in mortality following LHA amalgamations is related to reduced proximate hospital access. To assess whether reduced hospital access may account for the overall effect, we re-estimate the event-study model on sub-samples that exclude municipalities experiencing a hospital closure within any of the three travel time distances. Fig. 11 shows that the resulting estimates are very similar to the baseline ones.⁴⁶ Although closures are more frequent in consolidated LHAs, they do not appear to be the primary mechanism driving the mortality increase. Instead, they are best interpreted as one manifestation of the broader organizational changes that accompanied the amalgamation of LHAs.

While hospital closures are not the main mechanism behind our main findings, the degree of pre-existing hospital access significantly shapes the mortality response to consolidation. Fig. 12 shows that municipalities lacking a hospital within a 15-minute radius throughout the observation window experience substantially larger mortality increases compared with municipalities that consistently retain close hospital access. Areas already at a disadvantage due to limited proximity to hospitals are more likely to experience a deterioration in health outcomes when involved in LHA mergers.

Overall, the evidence suggests that LHA mergers were associated with a delayed centralization of hospital resources and a reduction in proximate access to hospital care. However, hospital closures and major downsizing do not fully explain the adverse effect of LHA consolidation on mortality. Although these events were more frequent in amalgamated areas, the estimated effects remain similar when municipalities exposed to nearby hospital closures are excluded. This suggests that hospital reorganization should be interpreted as one component of a broader process of spatial and organizational reallocation within enlarged LHAs, rather than as the sole mechanism behind the mortality increase.

Consistent with this interpretation, we also examined aggregate indicators of territorial care, prevention, and primary care capacity, which are more directly planned or coordinated at the LHA level. LHAs' mergers were accompanied by a sharp reduction in the number of local health districts. These districts are the operational units within each LHA responsible for planning and coordinating territorial health services, including general practitioners, outpatient care, pediatric services, and home care. Because the districts' catchment area is defined by population needs, it should not change automatically when a larger LHA is created; yet their number declined by 49% between 2002 and 2018 in regions where consolidations occurred, while remaining stable or increasing slightly elsewhere. These organizational changes did not translate into improvements in service delivery, as suggested by

⁴⁴ We consider the number of physicians to avoid misclassification of closures that would arise from accounting only for renaming, reclassification, or even reconstruction of facilities in the immediate proximity.

⁴⁵ This is in line with the strategy adopted by Ghislandi et al. (2025), who relies on hospital discharge records.

⁴⁶ This restricted-sample exercise should be interpreted with caution, because the sub-samples are defined using a variable that may itself be affected by the consolidation process. We therefore view it as a robustness check on the main consolidation effect, rather than as a separate causal estimate of the effect of hospital closures.

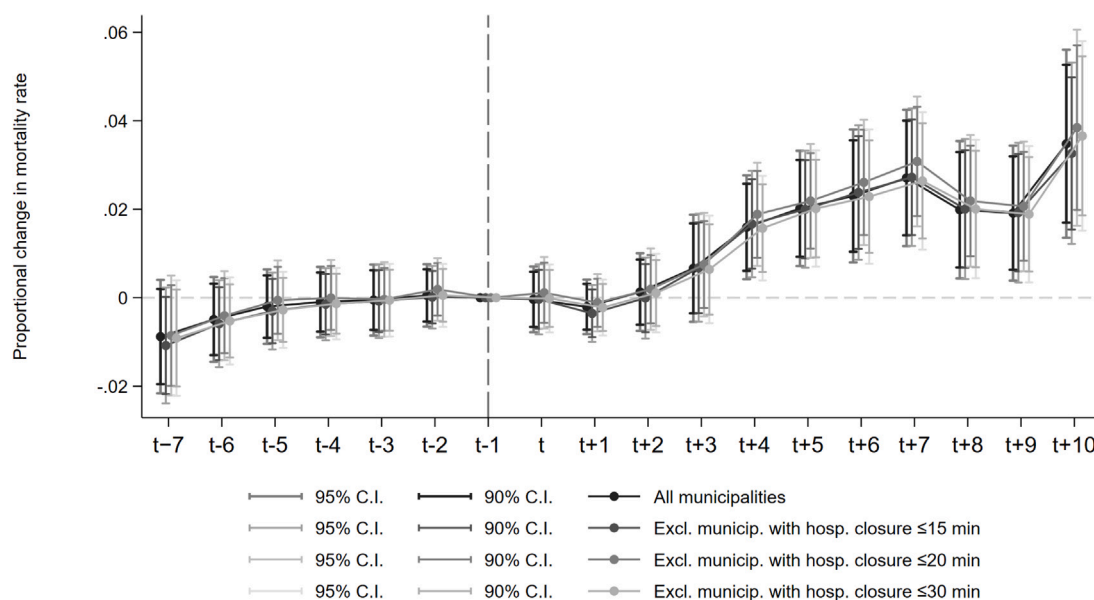


Fig. 11. Effects of LHAs consolidation on mortality rate excluding municipalities exposed to hospital closures.

Notes: Dots indicate the estimated coefficients for each year before and after the consolidation reform using the IW estimator by [Sun and Abraham \(2021\)](#) (see Eq. (1)), on data from 2002 to 2018. The dependent variable is the logarithm of the all ages, all causes death rate per 100,000 residents. The figure reports estimates for the full sample and for sub-samples that exclude municipalities experiencing a hospital closure within 15-, 20-, or 30-minute travel radii. Confidence intervals are indicated at the 90 and 95 per cent level. See the note of [Fig. 4](#) for details.

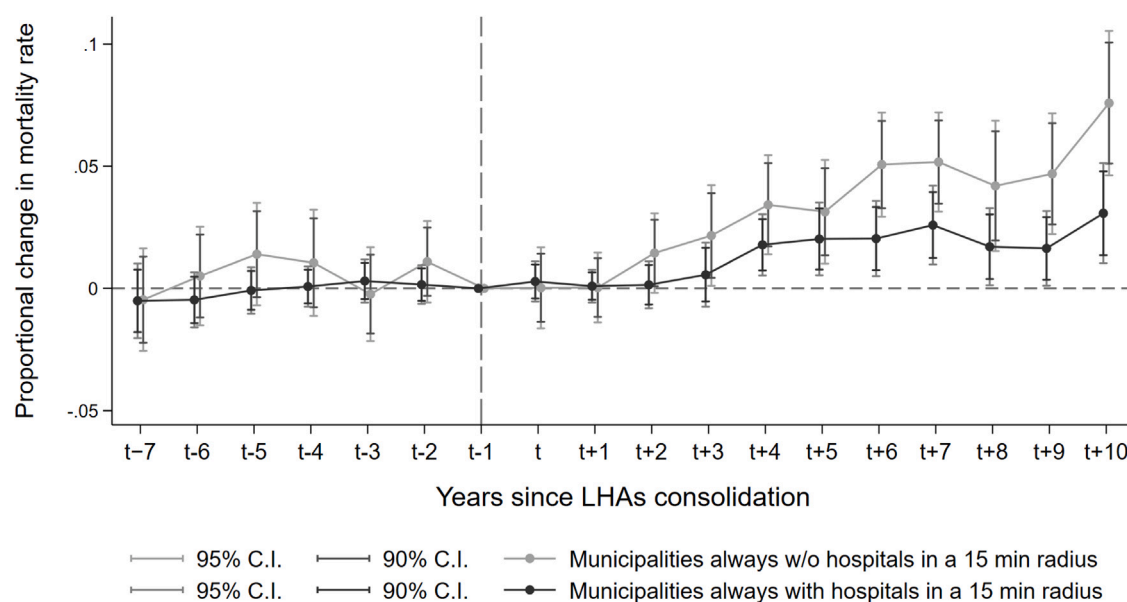


Fig. 12. Effects of LHAs consolidation on mortality rate by access to nearby hospital care.

Notes: Dots represent the estimated event-time coefficients relative to the year of LHA consolidation, obtained using the IW estimator of [Sun and Abraham \(2021\)](#) (see Eq. (1)) on data from 2002 to 2018. The figure compares municipalities that never had a hospital within a 15-minute radius throughout the observation window with municipalities that consistently retained hospital access within the same radius. Confidence intervals are indicated at the 90 and 95 per cent level. See the note of [Fig. 4](#) for details.

a few regional-level indicators. The annual monitoring reports for the implementation of the Essential Levels of Care (LEA) reveal that, although cancer screening coverage improved nationwide, the increase was markedly smaller in regions affected by LHA mergers.⁴⁷ For example, starting from a common baseline coverage of 13.3% in 2002 for cervical cancer screening, the percentage rose to 52.8% in 2019 in regions without LHAs mergers, compared to only 44.5% in regions where mergers occurred. A similar gap is observed for breast cancer screening (51.7% vs. 44.8% in 2019, starting from 15.7% and 16.3% in 2002, respectively). Since these screening programs play a crucial role in reducing avoidable mortality through early detection and timely treatment, such disparities are non-negligible.⁴⁸ Moreover, data from the Istat “Health for All” database show that the share of general practitioners exceeding the recommended maximum limit of 1500 registered patients – a key indicator of uneven workload distribution – increased nationwide, but the rise was significantly steeper in regions where LHAs mergers occurred.

These stylized facts offer suggestive evidence of potential coordination failures and reduced capacity in both screening and primary care services. While these data do not allow us to identify one single mechanism, they point to a broader weakening of local accessibility, coordination, and responsiveness within enlarged administrative units.

7. Discussion and conclusions

Over the past decades, the Italian NHS underwent a gradual consolidation process of its Local Health Authorities, aimed at containing rising healthcare costs and improving efficiency. Similar to districts, departments, or regional units in other high-income countries, LHAs are responsible for delivering healthcare services at the local level. The reform led to the creation of larger entities with expanded catchment areas.

The staggered rollout of the reform across regions created variation in policy exposure over time and space, enabling a quasi-experimental evaluation of its impact on population health. We use administrative data from the Italian Ministry of Health and municipal-level population and mortality records for 2002–2018 to reconstruct the timing and geography of LHAs amalgamation and track municipalities’ transitions. Exploiting this variation, we implement an event-study DiD design to estimate the policy’s causal effect on mortality. Our analysis reveals a significant increase in mortality rates following LHAs amalgamation. The effect typically emerges four years after consolidation and persists over time. The ATT over the entire post-treatment period corresponds to a 1.3% increase in mortality. When restricting the effect window between the fourth and ninth year after consolidation, during which the impact is statistically significant and persistent, the estimated ATT rises to 2.1%. We also show that this adverse impact is not confounded by the co-occurring DRP policy, which exerts its own independent influence on mortality outcomes. Even if the overlap with the DRP does not statistically confound the core estimates, the consolidation occurred within a broader context of fiscal austerity, which may limit the applicability of these findings to settings without similar constraints. The results remain robust across several alternative identification strategies. In particular, spatial sampling restrictions do not materially alter the findings, and the estimates are unaffected by potential cross-regional differences in healthcare systems and policy environments. Furthermore, we verify that the control municipalities are not indirectly affected by consolidations occurring in geographically proximate areas, thereby reinforcing the internal validity of the design.

The effect on total mortality is primarily driven by increases in deaths related to diseases of the circulatory system and cancers. Age-specific analysis reveals that, while older populations continue to bear the greatest absolute burden of mortality, the percentage increase in mortality rates is more pronounced among working-age groups (25–49 and 50–64). Moreover, statistically significant increases in mortality are found for both treatable causes (in individuals aged 0–74), which largely depend on timely diagnosis and effective treatment, as well as for “less preventable and less treatable” causes. These patterns suggest that amalgamation may have disrupted care continuity or diverted managerial attention from clinically sensitive areas.

The adverse health effects are not uniformly distributed. The increase in mortality is largely confined to municipalities of the merged LHAs, while those of merging LHAs (the administrative centers) remain unaffected. A plausible interpretation is that post-reform governance tended to preserve service provision patterns inherited from the pre-reform structure. Importantly, negative effects arise almost exclusively from mergers involving large LHAs, with the strongest mortality increases observed when consolidation creates very large administrative entities and entails substantial expansion of catchment areas. As noted in Section 1, the Italian reform resulted in unusually large local entities compared to those in other countries. This scaling-up exposed the newly expanded administrative units, serving heterogeneous populations and territories, to significant management and coordination challenges. This interpretation is reinforced by heterogeneity analyses exploiting the spatial distribution of essential public services. We find that the most affected municipalities are those classified as Outlying, located in the surrounding areas of Core municipalities where services are concentrated. The evidence points to the emergence of new vulnerable areas, plausibly reflecting a suboptimal allocation of human and physical resources in the newly expanded LHAs.

On the supply side, LHA consolidation was associated with a marked centralization of hospital services. Treated municipalities experienced a significant reduction in the availability of public-hospital physicians within short travel-time distances, despite an overall expansion of national hospital capacity, and hospital closures were more frequent in areas affected by consolidation. However, excluding municipalities affected by closures does not materially change the results. This indicates that the observed

⁴⁷ Ministry of Health, “Rapporto nazionale di monitoraggio dei livelli essenziali di assistenza anno 2004”; Ministry of Health, “Monitoraggio dei LEA attraverso la cd. Griglia LEA. Metodologia e Risultati dell’anno 2019”.

⁴⁸ Unfortunately, for the period under review, we could not retrieve any statistics on waiting times, the only health care quality indicator that, to the best of our knowledge, has been examined in relation to amalgamations, namely by Johar and Savage (2014), who report mixed evidence for the adjacent area health services in New South Wales, Australia.

increases in mortality are driven by the broader organizational changes accompanying LHA amalgamation, rather than by hospital closures *per se*. Limited access to hospital services appears to have acted as an amplifying factor, as mortality increased disproportionately among populations residing in areas that already lacked nearby hospital facilities prior to the reform.

Given that LHA mergers were primarily motivated by cost-containment objectives, it is important to assess the net welfare implications of the estimated increase in mortality. To this end, we employ a life-years lost (LYL) approach consistent with standard practices in the literature (see, e.g., Deryugina et al., 2019; Viscusi, 2020; Finkelstein et al., 2025). This method explicitly accounts for heterogeneity in remaining life expectancy across age groups and is particularly appropriate when excess mortality is concentrated among older individuals, as in our case. We monetize LYL using a constant “value of a statistical life year” (VSLY). The VSLY (approximately €125,000) is obtained by dividing the OECD value of a statistical life (VSL) for Italy (€ 4.9 million) by the remaining life expectancy (39.98) of an individual aged 45, the average age in Italy in 2018. By contrast, valuing each excess death using the full VSL would implicitly assign equal monetary value to all deaths, regardless of age or remaining life expectancy, thereby overstating the policy’s welfare cost.

Using the ATT of 1.3% for the entire post-treatment period, we estimate an average of 3731 additional deaths per year. We allocate these deaths across age groups proportionally to the age-specific mortality incidence from the 2018 Italian life tables, and multiply the resulting number of deaths in each group by the corresponding remaining life expectancy. Aggregating across age groups yields an estimated 39,891 LYL annually. Valued at a VSLY of €125,000, this corresponds to an annual welfare loss of approximately €5.0 billion, or 50.1 billion over a ten-year horizon. Focusing instead on the restricted window from year $t+4$ to $t+9$, for which we estimate an ATT of 2.1% and 6028 additional deaths per year, the associated LYL amount to 64,451 annually. This implies welfare losses of about €8.1 billion cumulatively over that period, or €48.6 billion cumulatively. Less conservative calculations based on applying the VSL to excess deaths yield an annual welfare loss of €18.3 billion (€182.8 billion cumulatively) when considering the ATT of 1.3%, and €29.5 billion (€177.2 billion cumulatively) when considering the ATT of 2.1% over the restricted window. To put these magnitudes into perspective, we compare them with available estimates of administrative cost savings associated with LHA consolidation in Italy. Di Novi et al. (2018) provide a suitable benchmark for our setting, as they study the same institutional units, rely on official 2012 LHA income-statement data from the Ministry of Health, and focus on the cost categories most directly affected by administrative consolidation. Their simulated merger scenarios also closely resemble our setting in terms of scale and final number of LHAs. Under their “minimum consolidation policy”, annual savings are estimated at approximately €416 million, primarily driven by reductions in administrative costs and procurement expenditures. Over a ten-year horizon, this corresponds to undiscounted cumulative savings of about €4.2 billion. The welfare losses due to increased mortality are therefore more than twelve times larger than the estimated financial savings.

Although our results should not be interpreted as conclusive, the absence of any positive effect on mortality, especially in the long term and in key population groups, was somewhat unexpected. Overall, our findings cast doubt about the “bigger is better” paradigm in healthcare administration, suggesting that consolidation policies should be pursued only after carefully weighing potential efficiency gains against their effects on population health. Further progress in understanding the mechanisms behind these effects will require access to individual-level data, which would allow a more direct assessment of how large administrative units perform in terms of prevention, timely diagnosis, and equity in access to care.

CRedit authorship contribution statement

Silvia Balia: Writing – review & editing, Writing – original draft, Supervision, Methodology, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Rinaldo Brau:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Sara Pau:** Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of Generative AI and AI-assisted technologies in the writing process

Statement: During the preparation of this work, the authors used AI tools in order to improve the readability and language of the manuscript. After using these tools, the authors reviewed and edited the content as needed and take full responsibility for the content of the manuscript.

Declaration of competing interest

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

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jhealeco.2026.103174>.

Data availability

This paper uses data from several administrative sources. Data from the Italian Ministry of Economics and Finance, and the Ministry of Health are publicly available. Inter-municipal travel times, the SNAI classification, and population data are released by the Italian National Institute of Statistics (Istat) and are publicly available. The death data were provided by Istat after fee-based data processing ([Istat Contact Centre](#)) and were accessed for age groups and, separately, for causes of death under restricted-access conditions and cannot be redistributed by the authors. These datasets cannot be linked. Replication code and more detail on the data are available here: [doi: 10.17632/g67jfv6mb2.2](https://doi.org/10.17632/g67jfv6mb2.2).

References

- Andini, M., Dalmazzo, A., de Blasio, G., 2017. The size of political jurisdictions: A model with some evidence from a fascist consolidation. *J. Comp. Econ.* 45 (4), 889–909.
- Arcà, E., Principe, F., Van Doorslaer, E., 2020. Death by austerity? The impact of cost containment on avoidable mortality in Italy. *Health Econ.* 29 (12), 1500–1516.
- Avdic, D., 2016. Improving efficiency or impairing access? Health care consolidation and quality of care: Evidence from emergency hospital closures in Sweden. *J. Health Econ.* 48, 44–60.
- Avdic, D., Lundborg, P., Vikström, J., 2024. Does Healthcare Consolidation Harm Patients? Evidence from Maternity Ward Closures. *Am. Econ. J.: Econ. Policy* 16 (1), 160–189.
- Bailey, M.J., Goodman-Bacon, A., 2015. The War on Poverty's experiment in public medicine: Community health centers and the mortality of older Americans. *Am. Econ. Rev.* 105 (3), 1067–1104.
- Bauernschuster, S., Driva, A., Hornung, E., 2020. Bismarck's health insurance and the mortality decline. *J. Eur. Econ. Assoc.* 18 (5), 2561–2607.
- Bernet, P.M., Singh, S., 2015. Economies of scale in the production of public health services: An analysis of local health districts in Florida. *Am. J. Public Health* 105 (S2), S260–S267.
- Berry, C.R., West, M.R., 2010. Growing pains: The school consolidation movement and student outcomes. *J. Law, Econ. Organ.* 26 (1), 1–29.
- Bordignon, M., Coretti, S., Piacenza, M., Turati, G., 2020. Hardening subnational budget constraints via administrative subordination: The Italian experience of recovery plans in regional health services. *Health Econ.* 29 (11), 1378–1399.
- Borusyak, K., Jaravel, X., Spiess, J., 2024. Revisiting event-study designs: Robust and efficient estimation. *Rev. Econ. Stud.* 91 (6), 3253–3285.
- Buchmueller, T.C., Jacobson, M., Wold, C., 2006. How far to the hospital? The effect of hospital closures on access to care. *J. Health Econ.* 25 (4), 740–761.
- Callaway, B., Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. *J. Econometrics* 125 (2), 200–230.
- Carroll, C., 2023. Impeding Access or Promoting Efficiency? Effects of Rural Hospital Closure on the Cost and Quality of Care. Working Paper.
- Cengiz, D., Dube, A., Lindner, A., Zipperer, B., 2019. The effect of minimum wages on low-wage jobs. *Q. J. Econ.* 134 (3), 1405–1454.
- Chen, J., Roth, J., 2024. Logs with Zeros? Some Problems and Solutions. *Q. J. Econ.* 139 (2), 891–936.
- Cirulli, V., Marini, G., 2023. Are austerity measures really distressing? Evidence from Italy. *Econ. Hum. Biology* 49, 101217.
- Clayton, D.H., 2019. The effect of prescription drug coverage on mortality: Evidence from Medicaid implementation. *J. Health Econ.* 63, 100–113.
- Cosby, A.G., McDoom-Echibir, M.M., James, W., Khandekar, H., Brown, W., Hanna, H.L., 2019. Growth and persistence of place-based mortality in the United States: The rural mortality penalty. *Am. J. Public Health* 109 (1), 155–162.
- Dahis, R., Sberman, C., 2026. Decentralizing development: The economic impacts of government splits. *Am. Econ. J.: Appl. Econ.* 18 (3), 220–253.
- de Chaisemartin, C., d'Haultfoeuille, X., 2020. Two-way fixed effects estimators with heterogeneous treatment effects. *Am. Econ. Rev.* 110, 2964–2996.
- de Chaisemartin, C., D'Haultfoeuille, X., 2023. Two-way fixed effects and differences-in-differences estimators with several treatments. *J. Econometrics* 236 (2), 105480.
- De Haan, M., Leuven, E., Oosterbeek, H., 2016. School consolidation and student achievement. *J. Law, Econ. Organ.* 32 (4), 816–839.
- Depalo, D., 2019. The side effects on health of a recovery plan in Italy: A nonparametric bounding approach. *Reg. Sci. Urban Econ.* 78, 103–466.
- Deryugina, T., Heutel, G., Miller, N.H., Molitor, D., Reif, J., 2019. The mortality and medical costs of air pollution: Evidence from changes in wind direction. *Am. Econ. Rev.* 109 (12), 4178–4219.
- Di Novi, C., Rizzi, D., Zanette, M., 2018. Scale effects and expected savings from consolidation policies of Italian local healthcare authorities. *Appl. Health Econ. Health Policy* 16 (1), 107–122.
- Ferraresi, M., Migali, G., Rizzo, L., 2018. Does intermunicipal cooperation promote efficiency gains? Evidence from Italian municipal unions. *J. Reg. Sci.* 58 (5), 1017–1044.
- Finkelstein, A., Notowidigdo, M.J., Schilbach, F., Zhang, J., 2025. Lives versus livelihoods: The impact of the great recession on mortality and welfare. *Q. J. Econ.* 140 (3), 2269–2328.
- Gardner, J., 2022. Two-Stage differences in differences. Papers 2207.05943, arXiv.org.
- Gaynor, M., Laudicella, M., Propper, C., 2012. Can governments do it better? Merger mania and hospital outcomes in the english NHS. *J. Health Econ.* 31 (3), 528–543.
- Ghislandi, S., Renner, A.-T., Varghese, N.E., 2025. The impact of budget cuts on individual patient health: Causal evidence from hospital closures. *J. Health Econ.* 101, 102975.
- Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. *J. Econometrics* 225 (2), 254–277.
- Guccio, C., Pignataro, G., Vidoli, F., 2024. It never rains but it pours: Austerity and mortality rate in peripheral areas. *Econ. Hum. Biology* 101408.
- Gujral, K., Basu, A., 2019. Impact of Rural and Urban Hospital Closures on Inpatient Mortality. Technical report, National Bureau of Economic Research.
- Hall, J.C., Matti, J., Zhou, Y., 2020. The economic impact of city-county consolidations: A synthetic control approach. *Public Choice* 184, 43–77.
- Johar, M., Savage, E., 2014. Do mergers benefit patients in underperforming administrations? Lessons from Area Health Service amalgamation. *Econ. Rec.* 90 (291), 526–535.
- Lyu, Y., Zhang, Z., 2025. Impact of primary care market mergers on quality: Evidence from the english NHS. *J. Health Econ.* 103050.
- Miller, S., Johnson, N., Wherry, L.R., 2021. Medicaid and mortality. New evidence from linked survey and administrative data. *Q. J. Econ.* 136 (3), 1783–1829.
- Mora-García, C.A., Pesec, M., Prado, A.M., 2024. The effect of primary healthcare on mortality: Evidence from Costa Rica. *J. Health Econ.* 93, 102833.
- Nolte, E., McKee, M., 2004. Does Health Care Save Lives? Avoidable Mortality Revisited. The Nuffield Trust.
- Nolte, E., McKee, M., 2011. Variations in amenable mortality—trends in 16 high-income nations. *Health Policy* 103 (1), 47–52.
- OECD/Eurostat, 2022. Avoidable mortality: OECD/Eurostat lists of preventable and treatable causes of death.
- Pei, Z., Pischke, J.-S., Schwandt, H., 2019. Poorly measured confounders are more useful on the left than on the right. *J. Bus. Econom. Statist.* 37 (2), 205–216.
- Reingewertz, Y., 2012. Do municipal amalgamations work? Evidence from municipalities in Israel. *J. Urban Econ.* 72 (2–3), 240–251.
- Roesel, F., 2017. Do mergers of large local governments reduce expenditures? Evidence from Germany using the synthetic control method. *Eur. J. Political Econ.* 50, 22–36.

- Rosenbaum, P.R., Rubin, D.B., 1984. Difficulties with regression analyses of age-adjusted rates. *Biometrics* 40 (2), 437–443.
- Sandsør, A., Falch, T., Strøm, B., 2022. Long-run effects of local government mergers on educational attainment and income. *Oxf. Bull. Econ. Stat.* 84 (1), 185–213.
- Santerre, R.E., 2009. Jurisdiction size and local public health spending. *Health Serv. Res.* 44 (6), 2148–2166.
- Statistics-Canada, 2006. Health region in Canada - Reference maps by Province/Territory. (Accessed 25 October 2024).
- Statistics-Canada, 2024. Health regions: Boundaries and correspondence with census geography. (Accessed 25 October 2024).
- Sun, L., Abraham, S., 2021. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *J. Econometrics* 125 (2), 175–199.
- Viscusi, W.K., 2020. Pricing the global health risks of the COVID-19 pandemic. *J. Risk Uncertain.* 61 (2), 101–128.
- Wherry, L.R., Meyer, B.D., 2016. Saving teens: Using a policy discontinuity to estimate the effects of Medicaid eligibility. *J. Hum. Resources* 51, 556–588.
- Wilner, L., 2023. Citizens' attitude towards subnational borders: Evidence from the merger of French regions. *J. Econ. Geogr.* 23 (3), 653–682.
- Wooldridge, J.M., 2025. Two-way fixed effects, the two-way mundlak regression, and difference-in-differences estimators. *Empir. Econ.* 69 (5), 2545–2587.