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The impact of budget cuts on individual patient health: Causal evidence from hospital closures

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ABSTRACT

Public finance constraints following the 2008 financial crisis in Europe often affected the hospital sector. This paper investigates i) the causal health impacts of reduced hospital supply, and ii) possible mechanisms to explain these. Using a staggered difference-in-differences framework, we study the effects of hospital closures on outcomes of all heart attack patients admitted to an Italian hospital between 2008 and 2015. Results show that closures increased in-hospital mortality by 10 % and length-of-stay by 0.3 days, but had no impact on readmissions. We explore potential mechanisms using different estimation approaches, and show that increased travel time following closures explains most of the mortality effect.

1. Introduction

Increasing expenditures in the healthcare sector are a key policy challenge given that healthcare accounts for a large share of the gross domestic product (GDP) and public budgets among most modern welfare states (WHO, 2022). Cost containment strategies have been particularly targeted towards the reorganization of the cost-intense hospital market (Aiken et al., 2001; Schwierz, 2016). Such restructuring policies mainly involve hospital (ward) closures or merging of several hospitals into larger entities potentially leading to the closure of one or more hospital sites[1] (Arcà et al., 2020; Burkey et al., 2017; Dranove and Lindrooth, 2003; Friedman et al., 2016; Wenzl et al., 2017). In Europe, there has been an increasing trend of hospital closures and downsizing of hospital capacity since the 1960s, which has been revived following the financial crises of 2008 (Aiken et al., 2001; Schwierz, 2016).

Conceptually, the consequences of hospital closures are not immediately clear. On the one hand, it has been pointed out that apart from cutbacks in public spending, there could be other external (market) or internal (hospital) factors motivating hospital or emergency department (ED) closures (Kaufman et al., 2016). Therefore, targeted hospitals or hospital wards are often characterized by low volume and reduced efficiency, leading to improved patient outcomes after a site-closure due to economies of scale and scope (Gujral and Basu, 2019). On the other hand, the closure of hospitals, especially remote ones, may worsen patients' spatial access to medical services (Alexander and Richards, 2023; Burkey et al., 2017), which is crucial for patients who require timely provision of care (Avdic et al., 2018; Avdic, 2016; Bentham, 1986). Making use of individual patient-level data, we address this conceptual inconclusiveness and study the effects of hospital closures on patient outcomes after an acute myocardial infarction

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[1] Hospital mergers may or may not lead to hospital closures depending on whether they share a common physical facility (Dranove and Lindrooth, 2003).

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(AMI) in a difference-in-differences framework. Additionally, we investigate potential channels, and employ a set of approaches to disentangle and quantify the indirect effect of hospital closures via travel time.

Existing empirical studies on the effects of hospital closures on health outcomes find mixed results. For example, Avdic (2016), using Swedish patient level data recorded between 1990 and 2010, exploited the variation in closure induced changes in geographical distance to find increased mortality among patients who experienced an AMI. Similarly, Avdic et al. (2018) reported adverse child-birth outcomes following a series of hospital mergers in Sweden. On the contrary, the same authors found that closure of cancer surgical wards in Sweden was associated with improvements in patient outcomes (Avdic et al., 2019). The study exploits regional variation in closures of cancer clinics, and identified learning-by-doing effects emerging from an increase in surgery volumes to be a potential mechanism underlying the relationship. Some other literature concludes that there is no effect of hospital closures on patient outcomes. Using a matching approach to construct a control group, Gaynor et al. (2012) investigate hospital mergers in England between 1997 and 2006 and report no changes in patient outcomes. Similarly, Grytten et al. (2014) analyse local hospital closures in Norway using a propensity score matching method to find no effects on neonatal and infant mortality. Inconclusive findings were also reported in studies conducted in the US. Buchmueller et al. (2006) study hospital closures in the Los Angeles county between 1997 and 2003 and find an increase in mortality from heart attacks and unintentional injuries among patients who experienced an increase in distance to the closest hospital. More recent studies including (Gujral and Basu, 2019; Song and Saghafian, 2019; Carroll, 2019) also report hospital closures in several US states to negatively impact patient welfare. On the other hand, a number of studies show closure of inefficient hospitals to increase efficiency via an increase in inpatient volume which in turn reduces the cost per admission (Capps et al., 2010; Lindrooth et al., 2003). Further studies in the US context find no significant differences in patient outcomes, including mortality and hospitalization rates (Hsia et al., 2012; Joynt et al., 2015; Rosenbach and Dayhoff, 1995). A more recent study by Fischer et al. (2024), on obstetric unit closures in the US between 1989 and 2019 found slightly beneficial effects on Caesarean-section rates and maternal morbidity, while other maternal and child outcomes are not significantly affected. The authors conclude that these effects are most likely driven by a reallocation of births to higher quality hospitals (Fischer et al., 2024). In a similar study, Durrance et al. (2024) find worse perinatal outcomes in moderately rural counties, but improved outcomes for residents of more rural areas (Durrance et al., 2024). While closures and mergers in the US are mostly due to financial pressures on individual healthcare facilities (Lindrooth et al., 2018; Kaufman et al., 2016), the closures we investigate here were not marked-driven. Our study is set against the background of a tax-based National Health Service, where closing down hospitals was orchestrated by the national government in an effort to increase efficiency during the financial crises in the early 2000s. While increased efficiency through market forces is also at the heart of most closures in privately dominated systems, policy makers in public systems have to take into account equity and accessibility considerations to ensure the well-being of their electorate. Further, in single-payer systems, resources and funds from closed hospitals will be reallocated to still existing facilities. Therefore, through closing small hospitals in rural areas in Italy, the general expectation was that patient outcomes would not deteriorate, or even improve, while public healthcare spending could be reduced.

We add to the literature on the causal effect of hospital closures on patient outcomes using emergency AMI admissions recorded between 2008 and 2015 in Italy. In a first step, we employ a staggered difference-in-differences approach to estimate the average treatment effect, and second, we elicit the indirect effect of a hospital closure via the travel time channel. For the causal interpretation of a difference-in-differences model, the identifying assumption, that the supply shock is conditionally exogenous to the individual patient's outcome and individual selection into or out of treatment can be ruled out (quasi-randomization), needs to hold. This assumption might be violated if hospitals are specifically selected for closure due to an expected change in patient composition, or if spatial sorting or attrition occurs after the closure. The former concern is conceptually unfounded in the context of this study as hospitals were closed based on regional economic rather than health indicators. Due to the latter violation, we focus on an acute condition that mostly affects the elderly and, hence, less mobile parts of the population (contrary to e.g. maternity care). If spatial sorting due to a reduction in available health infrastructure were to occur, this would lead to lower-bound estimates for AMI patients, as those with pre-existing conditions might choose to relocate following a closure, while the low-risk patients remain and are observed in the treatment group after the closure. We verify these conceptual justifications by analysing pre-closure trends in our outcomes and show that the parallel trends assumption holds. Further, there seem to be no anticipation effects with respect to the number of AMI patients reaching a hospital. The results of our main analysis show that hospital closures during the study period significantly increased the probability of in-hospital mortality by 10% and length-of-stay after admission by 0.3 days. The effects on in-hospital mortality are persistent across the post-closure years indicating that there is no short- to medium-term adaptation. The effects we find on 30-day cardiac/circulatory and AMI compatible readmissions are not significantly different from zero at conventional levels. We demonstrate the robustness of our results in a set of sensitivity analyses, including the approach proposed by Callaway and Sant'Anna (2021), as well as ruling out effects from spill-overs between treatment and control group. In a set of heterogeneity analyses, we find that the robust effects on in-hospital mortality are mainly driven by the subsamples of elderly and female patients. We also observe and discuss some geographic effect heterogeneity between the Southern and Northern regions of Italy.

Our analysis of the potential mechanisms is based on the notion that a detrimental effect of hospital closures on patient outcomes for time-sensitive conditions such as AMI, stroke, accidents or child-birth mainly owes to a delayed treatment due to an increase in average travel time (Gujral and Basu, 2019). In the case of an AMI event, blood clots are formed in the coronary arteries leading to oxygen deficiency in the heart, disrupting the blood supply to all parts of the body. Five minutes after the event, the body begins to experience damage and death is practically unavoidable if some form of treatment is not ensured within 15 min (Antman, 2008; Avdic, 2016; Ryan et al., 1996). To elicit whether time to treatment is the main or even the only mechanism through which hospital closures may affect patient outcomes, we employ two different econometric approaches. In particular, we disentangle the indirect effect through travel time to the hospital from the direct effects (which might comprise other channels such as bed congestion due to spill-overs) using a never-taker analysis, and by blocking the travel time channel. We are able to show that for in-hospital mortality the travel time channel is the most relevant one, while for length of stay and readmissions some other (latent) channels seem to drive the effects.

Besides the high policy relevance of the relationship between austerity measures in the public sector and population well-being (Depalo, 2019; Karanikolos et al., 2013; Quaglio et al., 2013; Reinhard et al., 2018; Stuckler et al., 2017), our paper contributes to the existing research on hospital closures in at least four ways. First, exploiting multiple conditionally exogenous supply shocks in the hospital market, we identify the causal effect of hospital closures on outcomes of a common and often deadly medical condition. Second, we apply a set of econometric approaches to disentangle direct and indirect effects of hospital closures and identify one of the main channels: travel time. Third, we collated, for the first time, a list of public and private hospital (ward) closures in the decentralized Italian healthcare system from 2008 to 2015 based on observed hospital admissions. Finally, despite the increasing number of hospital closures and mergers, rather few empirical studies have provided credible evidence on their effects in the European context. This is especially true for Southern European countries that have been hit hardest by the financial crises of the early 2000s such as Portugal, Spain, Italy and Greece. Italy represents an interesting case study, since it adopted tough fiscal consolidation policies that targeted the public healthcare system (Arcà et al., 2020), and hence provides an opportunity to evaluate the health consequences of austerity-induced cost containment strategies. Our findings are crucial for planning efficient geographic allocation of services while at the same time optimizing coverage and accessibility.

2. Background and institutional context

In Italy, healthcare is a constitutional right. The Italian government established the National Health Service (NHS) in 1978 based on the Beveridge model where healthcare is financed by general taxation (Brenna, 2011). Since then, the system has been through some substantial reforms which led to the transfer of fiscal, financial and managerial responsibilities to the regional level (Mauro et al., 2017). According to the OECD, in 2017 the per capita health expenditure in Italy was about 15% below the EU average (OECD, 2023). General health spending in 2020 was 9.6% of GDP, more than one percentage point below the EU average of 10.9%. Although the system went through many reforms in the nineties, the tightest containment policies were experienced during the second decade of the 2000s. According to Eurostat data, for example, from 2012 to 2017 Italy was among the bottom three countries in Europe in terms of health expenditure growth. Cutback management strategies were implemented at all levels. For example, according to the Italian Congressional Budget Office the average number of tenured personnel in the Italian NHS has reduced by 6.2% between 2008 and 2017 (Ufficio Parlamentare di Bilancio, 2019). This cutback was not achieved by firing doctors and nurses, who are employees of the Public Administration. Rather, despite the high number of retiring employees (baby boom generation), Italy has implemented a strict policy of limited hiring that started before the financial crisis and continued throughout the entire second decade of the 2000s. Importantly, since the budget is allocated and in part decided at the regional level, budget tightening was applied more directly at sub-national level. Following the fiscal federalism reforms, the Government fixed the spending targets which were then adjusted according to the regional healthcare needs and goals. Soon after, ten regions (Abruzzo, Molise, Apulia, Campania, Calabria, Sicily, Lazio, Piedmont, Sardinia and Liguria) ran into budget deficits due to shortfalls in management skills and service levels (Arcà et al., 2020). As a consequence, a set of budget recovery plans (Piani di Rientro, PdR) were introduced in 2006 (and ongoing) in order to restore the financial and economic balance in the budget deficit regions. This region-specific recovery schemes included active involvement by both the Ministry of Economics and Finance and the Ministry of Health in setting spending targets, designing and evaluation of healthcare services delivery via a system of ex-ante and ex-post monitoring, and tightening of overall autonomy in healthcare decision making at the regional level (Aimone Gigio et al., 2018; Depalo, 2019). Although the reforms were implemented in a staggered manner with varying intensities across the PdR regions, these financial recovery plans resulted in an overall reduction in the number of hospital beds, workforce, number of hospitals and hospitalization rates (Mauro et al., 2017). Regarding their consequences, the scientific literature has not yet reached a consensus. While Arcà et al. (2020), Depalo (2019) and di Bobini et al. (2019) showed that the PdR scheme was successful in reducing the healthcare expenditure, but simultaneously resulted in a worsening of health outcomes, Bordignon et al. (2019) conclude that budget containment policies increased efficiency by reducing expenditure with no relevant health effects on the affected population. As these cost containment measures were rolled out, Italy was also struck by the 2008 financial crisis which escalated the public debt and drastically reduced the healthcare expenditure growth (6% in 2000–2007 to 2.3% in 2008–2010) (de Falco, 2019). In addition to an increase in out of pocket payments and increased waiting times, there was a significant reduction in both hospitals and hospital beds during the crisis (de Falco, 2019). According to the OECD statistics on healthcare resources, Italy witnessed around 144 hospital closures between 2008 and 2015 (although an official closure dataset does not exists in Italy), and total acute care beds per 1000 inhabitants went down from 3.2 in 2008 to 2.6 in 2015 (OECD, 2023).

In Italy, like in many European countries, a hospital is considered a public good, whether it is publicly or privately owned. Ultimately the number and the distribution of hospital beds across a territory is a political and policy decision made by public authorities. More specifically, given that the Italian NHS is strongly decentralized, hospital closures are decisions that are taken by regional and local authorities, hence, they do not follow a centralized rule, with the exception of the regions being subject to budget recovery plans (and therefore, de facto, exauthorated of their decision power).

Given its strong local public dimension, closing a hospital is a political hazardous decision. Local communities are often opposing such decisions and the political costs could be high. Hence, it is a measure that must be justified clearly. The most common argument has been the increased efficiency, according to which closing small clinics or wards shifts patients to the large hospitals, which are generally safer and more efficient. Hence, we would expect that the closed hospitals should be either the ones that perform badly or the ones to which easier cases were allocated. We indeed find evidence of the latter also in our data, confirming that severe cases were treated by bigger hospitals even before the closures.

Another important feature of the Italian emergency healthcare system is that the first response is coordinated by a centralized (private or public) unit that takes all emergency calls and organizes the first aid response. These independent organizations ensure that ambulance services are spatially distributed to minimize the response time at any given location within Italy. Hence, the closure of a hospital (ward) does not imply longer ambulance waiting times.

Provided that hospital and ward closures were among the cutback management strategies implemented, their impact has never been directly investigated. In the present paper we focus on AMI hospitalizations. The choice of this specific condition is motivated by the emergency nature of the intervention. Hospital admissions for AMI typically cannot be anticipated or postponed. The lack of selection associated to the timing of intervention makes AMI an ideal condition for investigating the human costs associated to policy measures directed to reduce accessibility to points of care. In addition, heart attacks and strokes still represent a major source of mortality and impairment in most high income countries. In Italy, cardiovascular diseases remain the first cause of death (34.8% of total mortality) despite the downward trend in both prevalence and mortality registered since the early nineties (Cortesi et al., 2021).

3. Data and empirical strategy

3.1. Data

We use data from various sources to build our final admission-level analytic file. We start with the hospital discharge data from the Ministry of Health for the years 2008 to 2015, with information on all patients admitted for AMI during this time period in Italy. This dataset consisting of patient characteristics is merged with several datasets that contain hospital and municipality information. The hospital-level data includes characteristics such as the name of the hospital, geographical and administrative identifiers of the hospital and type of hospital (public/private). This is then linked to a dataset on beds made available by the Italian Ministry of Health and a dataset on municipality-to-municipality minimum travel time which was constructed for this study. Finally, we add data on population statistics at municipality level available on the website of the Italian statistical office (ISTAT).

The outcome variables of our econometric analyses include in-hospital mortality, length of stay and cardiac/circulatory related and AMI compatible 30-day readmission rates. For the latter variables we defined one or more index admissions for each patient and excluded planned readmissions. Further, we only considered an observation a readmission if the reason for admission was a cardiac, circulatory or AMI compatible diagnosis (see table A1). The length of stay is simply the difference between date of admission and date of discharge, hence, a continuous variable, and inpatient mortality is a binary variable that equals 1 if the reason for discharge was death and 0 otherwise.

3.2. Identification of home hospitals and hospital closures

Our main identification strategy is to compare the outcomes of AMI patients that were affected by a hospital closure with those that were not affected over time. As each patient is usually observed only once over the study period, it is necessary to define a hypothetical 'home hospital' for each patient even prior to their observed AMI admission. Following Avdic (2016), we define the home hospital for each patient as the modal hospital to which most residents of their municipality were admitted due to an AMI in a given year. When there are ties between two hospitals that admitted the same share of patients from one municipality, we choose the hospital that is the closest (i.e. minimum travel time by car between the municipalities' centroids). If two hospitals still compete to be a home hospital for a municipality, we choose one of them at random. This allows us to assign each patient, depending on their place of residence, to one specific home hospital every year. We refrain from using a simpler identification of home hospital, such as the hospital in the municipality of the patient, because a municipality might have zero or more than one hospital. In both cases a somewhat arbitrary decision would have to be made on whether a patient is potentially affected by a closure or not. The approach suggested by Avdic (2016) avoids a value judgement on this important issue.

In a next step we identify which of the home hospitals closed during our study period. Since hospital closures have never been studied in the context of Italy, a centralized list of hospital closures does not exist. We rely on previous literature on hospital closures in the context of other countries for our definition of closures (Avdic, 2016; Gujral and Basu, 2019; Song and Saghafian, 2019). Using patient discharge data from the Italian Ministry of Health, we consider hospitals to be closed for AMI patients if the volume of emergency AMI admissions in a hospital reduces by 90% or more between two years and stays the same in the subsequent years in the study sample. Any remaining observations in the closed hospitals are dropped from the sample. Note that as a consequence of this definition we also capture partial (e.g. ward) closures as long as the respective hospital appears to be closed for emergency AMI admissions. Similar to Gujral and Basu (2019), we define the closure year as the year prior to the year the AMI admissions dropped below 10%. The reason being that hospitals might not close at the beginning of the calender year, and therefore appear to be open because admissions do not decrease enough in the year of the actual closure but only one year later. The list of closed hospitals was then reconfirmed using several external sources including Italian newspapers, online articles on the list of hospitals under risk of closure and other related news or documents on the official website of Italian Ministry of Health.[2] We also went through the websites of closed hospitals (if available) to check for any news on renovation, mergers or closures. A total of 47 hospitals from 2008 to 2015 were identified to have had a sustained drop in AMI admissions to 10% or less of the previous admissions and therefore identified as closures. The closed hospitals treated significantly less AMI patients in the initial year of our study period (61.6 versus 126.4 patients in 2008) and were more likely to be publicly run hospitals (see Table A2 in Appendix B).

Out of all closed hospitals, 37 were identified as a home hospital for at least one municipality based on the definition outlined above. For example, eight hospitals closed both in 2012 and 2013, but in 2013 only seven of these were considered a home hospital. This is likely due to the fact that some hospitals were never really relevant for acute treatment of heart attacks, but occasionally still treated some patients. Our definition of closure, which is based on a year-to-year comparison of AMI admissions, should overcome any discrepancies arising from a simple collation of hospitals that were reported to be closed from various sources. Similarly, the definition of closure year as the year prior to closure captures the earliest interruptions in any services provided. In the year prior to closure, not all services are necessarily cut off and this, coupled with the definition of closures (90% reduction in AMI), indicates that our main estimates reported in the paper are conservative (Gujral and Basu, 2019; Troske and Davis, 2019). Fig. 1 highlights the spatial distribution of the municipalities affected by a home hospital closure between 2008 and 2015.

3.3. Empirical analysis of hospital closures

Given the variation in timing of hospital closures between 2008 and 2015, we use a 'staggered' difference-in-differences (DID) design with municipality and year fixed effects to examine changes in patient outcomes before and after a hospital closure event. This approach allows to control for observed and unobserved heterogeneity between the treatment and control group that is constant over time. If the parallel trend assumption is met, a DID analysis allows for a causal interpretation. The treatment is defined at the municipality level, and the population of a municipality is considered treated if at least one home hospital closes (as defined in Section 3.2). The model is estimated at the individual (admission) level based on Eq. (1):

$$y_{ihjt} = \alpha_0 + \alpha_1(TreatPost_{jt}) + \mathbf{X}\beta'_{ihjt} + \mathbf{H}\gamma'_{ht} + \mathbf{M}\mu'_{jt} + \delta_j + \lambda_t + \epsilon_{ihjt} \tag{1}$$

where y_{ihjt} is a binary variable indicating (a) whether an admission i to hospital h from municipality j resulted in death at time t, (b) a continuous variable indicating the length of stay, or (c) whether admission i to hospital h from municipality j lead to a readmission within 30 days from the initial index admission. $TreatPost_{jt}$ is an interaction term of the treatment dummy with a dummy that equals 1 if an admission occurred in the post-closure period and 0 otherwise. Hence, α_1 is the parameter of interest in our policy analysis. X_{ihjt}, H_{ht} and M_{jt} are vectors with admission–, hospital– and municipality-level covariates that might affect health outcomes. Admission-level controls include age, gender, marital status, education level and Elixhauser comorbidity index (Elixhauser et al., 1998). The comorbidity index is constructed as the weighted sum of the presence of 16 secondary diagnoses.[3] The index is then transformed into a categorical variable (a sum of 0, 1, or 2 and more, respectively). Hospital of admission level controls include type of hospital and volume of AMI hospitalizations per hospital-year. Municipality level controls include resident population and the share of resident population above 65. Municipality and year fixed effects are used in all regressions, whereas macro-region year fixed effects are only included in some individual level specifications.

Municipality and year fixed effects are captured by terms δ_j and λ_t, respectively. The estimates for in-hospital death and 30-day readmission are obtained through linear probability models whereas the estimate for average length of stay is obtained through linear regression models. Robust standard errors are clustered at the municipality level.

Sample and variables for analysis

The analytical sample for our analysis consists of all acute patients treated for heart attack in an Italian hospital (private and public) between 2008 and 2015. Table 1 reports the descriptive sample statistics for a set of variables, both, overall and by treatment group (untreated, not-yet treated, treated after closure). Over the 8-year study period, we observe a total of 818,098 patients, of which 6.5% died in the hospital, 5.1% had an unplanned readmission within 30 days of discharge, and the average length of stay was 7.3 days. Comparing the untreated with the treated sample (roughly 2.7% of patients), it seems that in-hospital mortality and length of stay is lower for patients affected by home hospital closures compared to the control group, while 30-day readmissions are higher for the treated group. This is true for observations before and after the actual closure. It is worth stressing that these statistics are not informative of the effect of hospital closures or any pre-closure trends as they reflect unconditional averages. The mean age of the patients was 70.2 years, 35.0% were female and 45.8% reported that they are married. Overall, AMI patients in the treatment group are slightly younger, more likely to be male, married, and to suffer from comorbidities (Table 1, panel A). Interestingly, the share of patients with the lowest educational attainment is highest in the pre-closure group and lowest in the post-closure group.

[2] Given the system of coding hospitals at the national level, the same hospitals are sometimes assigned different codes in different years, or two different hospitals are assigned the same code in different years. Mergers between hospitals at the same location are also assigned different codes, indicating a false closure. To deal with this, we manually identify hospitals over the years by matching them on hospital names, address, and postcodes (and telephone numbers). Cases that had inputting errors or inconsistencies in entries after the first stage were double checked using external sources. Finally, we used the trend of the admission volume to double check the final list of hospitals over time.

[3] The 16 secondary diagnoses include congestive heart failure, peripheral vascular disease, cerebrovascular disease, dementia, chronic pulmonary disease, rheumatologic disease, peptic ulcer disease, mild liver disease, diabetes without and with chronic complications, hemiplegia or paraplegia, renal disease, cancer, moderate or severe liver disease, metastatic carcinoma and AIDS/HIV.

Fig. 1. Spatial distribution of municipalities affected by home hospital closures, 2008–2015.
Notes: The municipalities highlighted in red are those that were affected by a hospital closure between 2008 and 2015. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

This likely reflects a general cohort effect in education and/or changes in coding practices over time — which are not picked up in the untreated group as this reflects the average over the entire study period.[4]

The descriptive statistics provide some important insights regarding the rationale of the closures. In table A2 in the appendix we note that average mortality rates are lower in closed hospitals even stratifying for the size of the hospitals, with the only exception of very low volume hospitals. This is consistent with rational policy-making: shutting-down or merging wards with low-severity cases minimizes the risk of adverse (health and political) impacts, as severe cases were admitted to bigger, high-quality hospitals even before the closure.

Regarding the potential channels, as expected, travel time in minutes is highest for treated patients after the closure, and lowest for untreated patients (24.0 versus 14.5 min). The travel time measure reflects the travel time by car between the centroid of the patient's municipality of residence and the hospital's municipality. It will thus be most accurate for municipalities with a smaller surface area[5] and a settlement structure concentrated around the geographical centre. The average bed utilization rate,[6] which is

[4] We confirm this change in coding patterns by providing visual evidence of this variable by year differentiating between the untreated and treated sample. To rule out biased estimates we run our main analysis without controlling for education and find that results are robust to this change in specification (see table A8).

[5] According to the Italian Statistical Office (ISTAT), the average (median) municipality surface area is 37.33 km^2 (21.89 km^2) or 14.41 mi^2 (8.45 mi^2).

[6] Note that the data base for the bed utilization rate is only available for the years 2010 to 2015.

Table 1
Descriptive statistics of main variables for 2008–2015.

Variables	Total (all years)	Untreated (all years)	Treated (pre-closure)	Treated (post-closure)
Panel A: individual-level				
Main outcomes				
In-hospital mortality in %	6.5	6.5	4.3	4.3
30-day readmission in %	5.1	5.0	8.8	7.8
Length of stay (SD)	7.3 (5.3)	7.3 (5.3)	6.7 (4.8)	6.7 (4.9)
Comorbidities per patient in %				
0	56.9	57.0	51.2	53.0
1	25.8	25.8	28.9	27.7
2 or more	17.3	17.2	19.9	19.3
Socio-demographic characteristics				
Age (SD)	70.2 (13.5)	70.2 (13.5)	68.6 (13.6)	68.1 (13.4)
Female in %	35.0	35.0	34.5	32.2
Married in %	45.8	45.6	50.2	50.6
Level of education in %				
Elementary/no qualification	76.4	76.6	91.1	64.1
Junior high school diploma	14.0	13.9	5.5	23.3
High school diploma	7.6	7.5	3.1	10.3
University degree/short degree	0.4	0.4	0.1	0.7
Graduate degree	1.5	1.5	0.2	1.7
Channels				
Travel time in minutes (SD)	14.7 (18.0)	14.5 (17.9)	21.4 (20.1)	24.0 (17.9)
Bed utilization rate[a] (SD)	44.9 (22.5)	45.0 (22.4)	33.4 (19.6)	44.4 (26.1)
Observations	818,098	795,464	10,027	12,607
Panel B: municipality-level				
Municipality characteristics				
Resident population in 1000	8.6 (44.5)	8.7 (44.96)	5.6 (8.0)	6.1 (8.2)
Resident population $\geq$ 65 in %	22.1 (5.3)	22.1 (5.3)	22.3 (5.7)	22.2 (5.6)
Observations	55,350	53,143	1030	1177

Notes: Panel A reports proportions or means (standard deviations) for variables at the individual (AMI patient) level. Panel B reports means (standard deviations) for variables at the municipality of residence level. In both panels, descriptive statistics are reported for the treatment and control group separately.
[a] Data available for years 2010–2015 only (total observations: 617,797).

the number of AMI admissions divided by the official number of acute cardiac beds, on the other hand, is lower in the treatment group before closure (33.4), but increases to almost the rate in the untreated (44.4 and 45.0) in the years after closure. We observe patients from 7992 different municipalities over the 8-year time frame, which amounts to 55,350 municipality-year combinations.[7] The average size of the observed municipalities is around 8600 inhabitants, of which 22.1 are over the age of 65 years, with municipalities affected by a closure being generally smaller.

Robustness and heterogeneity checks

To elicit the validity of our policy analysis, we extend our staggered DID model by including leads and lags of the treatment effect. This is not only an indirect check of the parallel trends assumption, but also allows us to elicit if the effects of hospital closures were stable in the long run. That is, we interact the treatment dummy with dummies capturing years from hospital closure. As we do not know the precise timing of a hospital closure within a year, we use the year prior to closure as baseline. The dynamic model specification is given in Eq. (2) below.

$$y_{ihjt} = \alpha + \sum_{t=s}^{-2} \theta_t T_{jt} + \sum_{t=0}^{k} \phi_t K_{jt} + \mathbf{X}\beta'_{ihjt} + \mathbf{H}\gamma'_{ht} + \mathbf{M}\mu'_{jt} + \delta_j + \lambda_t + \epsilon_{ihjt} \tag{2}$$

where $t = (s, \dots, -2, -1, 0, 1, 2, \dots, k)$ represents time from home hospital closure, T_{jt} are interactions of the treatment indicator (which equals 1 if municipality j experienced a home hospital closure) and time dummies for all periods before $t = -1$ (i.e. year prior to 90% drop in admissions). Likewise, K_{jt} are interactions of the treatment indicator and time dummies for all periods from time 0 (i.e. year of closure) onwards. Lack of statistical significance on coefficients θ_t provides indirect evidence in support of the parallel-trends assumption. We would expect either the long-term benefits of hospital closure in terms of improved efficiency to offset the short-term adverse effects or no such benefits in the long run.

There are several other threats to the validity and robustness of our results. First, we check for anticipation effects and whether closures lead to decreased admissions for AMI due to patients dying on the way to the hospital. We do so by estimating Eq. (2) but

[7] There are some municipalities with no patients in some years which creates small variations in the number of municipalities over the years (e.g. in 2008 patients came from 6915, while in 2015 they came from 6937 distinct municipalities.).

using the number of total AMI admissions on the municipality level as the outcome variable. Significant coefficients on the leads will indicate anticipation effects, while significant coefficients on the lags would suggest attrition due to reduced access. The former would violate our identification strategy while the latter could introduce a downwards (upwards) bias to our point estimate for in-hospital mortality (length of stay and readmissions).

Second, as we observe some patients multiple times due to a transfer and not due to a readmission, we create unique episodes of care (EoC). We collapse the data so that there is only one observation per patient per AMI incident and re-run our main model detailed in Eq. (1). We identify transfers in our dataset as those AMI patients that were discharged on a specific day and admitted to another hospital on the same or the next calender day. We then recalculate our outcome variables to reflect whether a patient died during the whole EoC, what the total length of stay was, and whether they were readmitted after the EoC.

Third, as the observed hospital closures occur at different points in time during our study period, we also repeat our analysis using the Callaway and Sant'Anna (CS) estimator for DID with multiple periods (Callaway and Sant'Anna, 2021). The CS estimator accounts for the fact that treatment effects may vary with time and length of exposure, hence, using earlier treated units as control group might bias the results due to a violation of the parallel trends assumption. Therefore, to obtain consistent results, only never-treated or not-yet treated units are used as controls. The CS approach also allows an event study type estimation similar to Eq. (2), as well as varying point estimates for different treatment cohorts. Related to this, there is some scepticism in the econometric field towards including time varying control variables in a TWFE-DID model (Caetano et al., 2024). As a robustness check, we therefore estimate an "unadjusted" model without any covariates to compare with our main specification.

Fourth, there are two covariates in our main specification that might lead to biased results. For one, we exclude the level of education of the patient in a sensitivity check as anecdotal evidence suggests that this was often coded incorrectly or not at all, especially in the earlier years of our study period.[8] The second variable that we change for robustness is the volume of AMI hospitalizations per hospital-year because there might be spill-overs in this variable between our treatment and control group. In particular, a hospital that is located nearby a closed hospital is likely to experience higher rates of AMI hospitalizations due to the closure. We therefore replace the AMI volume per year and hospital of admission with the baseline AMI volume, i.e. the number of patients in the first year. We also check the threat to our identification strategy due to spill-overs more generally by excluding patients from the control group if they were admitted to a hospital with a share of patients from treated municipalities above 10, 30 and 50% at any given year.

The final two sensitivity checks concern the definition and level of treatment. First, in our main analysis we define a hospital as closed if the AMI admissions decrease by 90% from one year to another and stay low in the following years (see Section 3.2). To see if our results are robust to a more restrictive definition of closure, we repeat our analysis with hospitals that reduced their emergency AMI admissions to zero. Second, while in our main specification we cluster the standard errors at the municipality level as this is the level of treatment, this might be to narrow given that a hospital closure potentially affects multiple municipalities. We therefore calculate robust standard errors clustered at the level of the home hospital.

Finally, we also perform a set of heterogeneity analyses. To identify particularly vulnerable groups that might drive our results, we split the sample into patients above and below 80 years of age at the time of hospitalization, as well as into male and female patients. Further, as the geographic gradient in Italy between the North and the South, especially in terms of health but also in terms of financial resources, is still persistent, we split our sample into patients treated in Southern and Northern regions (the former being Molise, Campania, Puglia, Basilicata, Calabria, Sicilia, Sardegna). We repeat the staggered DID (see Eq. (1)) for the respective subsamples.

3.4. Exploration of potential mechanisms

We are not only interested in the average effect of hospital closures on patient outcomes, but also in the underlying mechanisms. Most recent publications on hospital closures focus on the increased travel time as a channel through which closures can impact the outcomes of acute and time-sensitive conditions such as AMI (Avdic et al., 2018; Buchmueller et al., 2006; Bentham, 1986; Burkey et al., 2017). We therefore propose different approaches to disentangle the indirect effect of hospitals closures on AMI outcomes via travel from the direct effect. It should be noted that the direct effect is a compound of all other possible mechanisms that are unknown, unobservable, or unmeasurable.

First, we follow the literature on total treatment effects within specific sub-populations (see e.g. Deuchert et al., 2019) to disentangle the direct effects of a hospital closure from the indirect effects of travel time changes due to hospital closures. The intuition behind the approach proposed by Deuchert et al. (2019) is that patients who are affected by a closure, but are so called 'never-takers', i.e. travel time to the home hospital did not increase, should only show adverse health outcomes compared to the control group (i.e. those unaffected by a closure) if there is another channel besides travel time. We therefore estimate the original DID (see Eq. (1)) but restrict our sample to those patients that had no change in travel time to their home hospital. As we usually observe each patient only once (unless they are readmitted), we evaluate this change on the municipal level by comparing the travel time to the designated home hospital in the first and last year the municipality is observed (usually 2008 and 2015, respectively). A municipality's patients will hence be never-takers if there is another hospital nearby, or in the same municipality, that can absorb the patients without a change in travel time after closure of their home hospital.

[8] See also figure A3 in appendix C for a comparison of the educational attainment of untreated and treated patients over the years.

The average treatment effect on the never-takers is identified in a DID framework under certain assumptions (see Deuchert et al., 2019 for technical details). First, conditional on observed covariates, there are no confounders that affect both, the treatment (i.e. the hospital closure) and the mediator or the treatment and the outcome. Since, given certain district and hospital level covariates, the hospital closures are randomly assigned to individual patients (no individual selection into treatment), the first assumption holds in our case. The second identifying assumption is that of weak monotonicity of the mediator in the treatment, hence, the ruling out of defiance. In our application, this means that patients affected by a closure are more likely to face increased travel times than those unaffected by a home hospital closure. Third, there must not be any anticipation effects of the mediator on the treatment. Hence, in the case of travel time, patients do not voluntarily bypass their home hospital because they anticipate its closure. In the case of a heart attack this kind of anticipation behaviour is very unlikely as usually the closest hospital is chosen by the ambulance service to minimize time to treatment. Further, due to our definition of a hospital closure (i.e. a maintained reduction of AMI treatments of 90%), a pre-emptive avoidance of closing hospitals will result in an earlier observed closing year. We still check this assumption in a sensitivity analysis described in Section 3.3. Finally, the common trend assumption, which is standard in DID, needs to be valid conditionally on baseline controls. We check the parallel trends indirectly by adding lag and lead variables of our treatment to the staggered DID model for the restricted sample of never-takers using Eq. (2).

We extend the never-takers analysis suggested by Deuchert et al. (2019) by comparing it to the effects of a closure on patients that did experience a change in travel time to their home hospital. To do so, we create subsamples of patients with a change of 1 to 10 min and of over 10 min. As above for the never-takers, we compare those that had a change in travel time to their home hospital due to a hospital closure (treatment group) to those that had a change in travel time to their home hospital e.g. due to changes in the distribution of AMI patients amongst close-by hospitals (control group). While the latter is arguably a rather small and potentially selective control group,[9] this additional analysis can provide suggestive evidence on potential non-linearities in the travel time channel.

Another possible approach to elicit whether travel time is the only channel through which hospital closures affect AMI outcomes is to block this channel. We do so by restricting our sample to patients with a centroid-to-centroid travel time by car of less than 10 and 15 min. These thresholds are based on findings from the medical literature indicating that AMI treatment is most effective if first medical contact and diagnosis occur as soon as possible after the incident (Ibanez et al., 2018). We then run our main staggered DID analysis (see Eq. (1)) on this subsample. Conceivably, if there is still a direct effect of the treatment, it is very unlikely caused by longer travel times.

The two described approaches to identify the direct effect of a hospital closure (besides that channelled by travel time) could potentially be repeated for other channels. We refrain from doing so, as there is no clear medical indication of which thresholds to use, e.g., for bed utilization rates. However, it is possible to check if there is effect heterogeneity with respect to bed utilization in the overall treatment effect (estimated using Eq. (1)). We therefore compare the estimated effect sizes based on patients treated in hospitals in the highest versus the lowest quintile of bed utilization rates across our study period.

4. Results

4.1. Main analysis

As outlined in Section 3.3, we estimate the effect of a home hospital closure on patients from different municipalities using a two-way fixed effects staggered difference-in-differences approach.

Table 2 reports the results on the impact of home hospital closure on in-hospital mortality, length of stay, and 30-day readmission after an AMI. When accounting for municipality and year fixed effects as well as individual, hospital and municipality characteristics, we find a home hospital closure to increase the probability of in-hospital death following an AMI by about 1.1 percentage points (Table 2, column (1)). When including region-year fixed effects to account for yearly region-specific shocks, the effect decreases in magnitude to 0.7 percentage points (column (2)), but remains statistically significant at a conventional 5%-level. With respect to length of stay, we find that home hospital closure increases the duration of in-hospital stays between 0.27 and 0.42 days depending on the inclusion of controls and fixed effects (columns (3) and (4) in Table 2). We do not find any statistically significant effect of home hospital closure on the probability of a readmission 30 days after the index admission (columns (5) and (6) in Table 2).

As the validity of the DID estimates rests on the assumption of pre-treatment parallel trends, we formally test this by running a series of regressions including leads and lags of the treatment effect as specified in the methods section. Incidentally, this approach allows also to inspect the effect of a home hospital closure on the outcomes of interest over time. Results for each outcome are presented in table A3 in appendix C, and are generally supportive of the parallel trends assumption, especially for mortality (see Fig. 2).

Indeed, the coefficients for the periods prior to the year before closure are non-significant at a 5%-level, indicating a parallel trend before the closure. Only when applying 90% confidence intervals, length of stay and readmission rates (columns (2) and (3) in table A3) differ between treatment and control group in some years before the closure occurs. To further alleviate concerns regarding a violation of the parallel trends assumption, we provide visual evidence in figure A1 in the appendix where we plot, for each outcome variable, the average of the not-yet treated versus the never treated patient population by region for the years 2008 to 2013. The graphs show parallel linear fits with overlapping confidence intervals of the not-yet treated and untreated municipalities

[9] Less than 8% of patients unaffected by a closure had a change in travel time to their home hospital of more than 10 min over the observation period.

Table 2
Effect of hospital closures on in-hospital mortality, length of stay and 30-day readmissions.

	(1) Dead	(2) Dead	(3) LoS	(4) LoS	(5) 30-day	(6) 30-day
Post X Treated	0.011***	0.007**	0.269**	0.416***	0.003	−0.010*
	(0.003)	(0.003)	(0.123)	(0.103)	(0.006)	(0.006)
Constant	−0.170***	−0.133***	0.645	1.362**	0.150***	0.145***
	(0.021)	(0.020)	(0.599)	(0.531)	(0.031)	(0.031)
Mean dep.var[a]	0.043	0.043	6.673	6.673	0.082	0.082
Patient controls	Yes	Yes	Yes	Yes	Yes	Yes
Hospital controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Region x Year FE	No	Yes	No	Yes	No	Yes
Municipalities	7992	7992	7992	7992	7992	7992
Observations	817,724	817,724	817,724	817,724	751,701	751,701

Notes: All models are linear probability models with two-way fixed effects (FE). Robust standard errors clustered at municipality level in parentheses. Dead is a dummy measuring whether hospitalization ends with death. LoS is a continuous measure of the days between admission and discharge of the patient. 30-day is a dummy measuring whether an AMI patient was readmitted 30 days after initial discharge. Controls include patient age, gender, marital status, education, Elixhauser comorbidity index, type of hospital, AMI admission volume by hospital, size of municipality population and municipality population aged 65+ (%).
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.
[a] Mean of dependent variable for treated population before closure.

for all outcomes.

The coefficients from the post-closure periods reveal that the effect of hospital closure on the probability of in-hospital mortality is rather persistent (until $t + 4$), while for length of stay the effect is not significantly different from zero from $t + 2$ onwards. For the readmission rates, only the coefficient on the $t + 1$ treatment period is significantly negative. Hence, probability of readmission actually decreases immediately after a closure. In contrast with findings from prior work (Avdic, 2016), our results suggest that adaptation to negative supply shocks may take relatively long.

To elicit the robustness of our results, we perform a series of sensitivity checks.[10] First, we check for the presence of anticipation effects and patient attrition by checking trends in emergency AMI hospitalizations in treated municipalities (see table A4 in appendix C). We find no evidence of diverging trends prior to the hospital closure in terms of AMI emergency admissions per 100 inhabitants. However, one and four years after closure (at $t + 1$ and $t + 4$) a slightly lower rate of AMI patients from municipalities initially served by closing hospitals reached a hospital. Assuming that the incidence rate of heart attacks was not affected by closures, this finding can be explained by increased out-of-hospital AMI mortality either before or on the way to the hospital. This attrition could introduce a downward bias to our point estimate, leading to a conservative estimation of the effect on total mortality. As these effects are small and do not persist over time, it is likely that organizational measures were implemented to minimize patient and emergency system delays.

Second, we apply an episode-of-care perspective by joining observations that can be assumed to refer to the same health shock (i.e. heart attack), but appear as multiple admissions due to transfers within the hospital system. Results are shown in table A5 in appendix C, and reveal no significant impact of these transfers on our main results.

Third, instead of modelling a standard two-way fixed effects DID, we apply the Callaway and Sant'Anna (2021) approach (CSA-DID) for heterogeneous cohort and time effects. Table A6 in Appendix C displays the average treatment effects on the treated population. The results in column (1) show that the effect on in-hospital mortality is significantly positive and similar in size to the point estimate of our base model in Table 2. The event study plot for in-hospital mortality in figure A2 confirms the insignificant pre-trend test. Contrary to this, the CSA-DID does not yield a significant effect of hospital closure on the length of stay and the readmission rates. It should also be noted that, following the results of the pre-trend test, we cannot reject the Null-hypothesis that there is a significant trend (at 5%) in the length of stay before the closure happens. Following recent literature on DID with staggered treatments, we exclude all time-varying covariates from our main model (Eq. (1)) and report the unadjusted point estimates in table A7 in Appendix C. The coefficients are qualitatively similar, albeit slightly larger, compared to the full model.

Fourth, we investigate the robustness of our results with respect to various sources of endogeneity. Due to potential coding errors in the patient-specific level of education in the earlier observation periods, we exclude this variable in a sensitivity check. Further, we replace the hospital-specific volume of AMI admissions per year with the number of admissions in the first year as the former might be driven by the closures due to spill-overs between treatment and control group. Both analyses confirm the robustness of our main specification (see tables A8 and A9 in appendix C). We also check if our results might be driven by spill-overs between treatment and control group more directly by restricting the control group to patients treated in hospitals that treated less than

[10] For an overview of all robustness checks for the three main outcomes see the coefficient plots in figure A4 in appendix C.

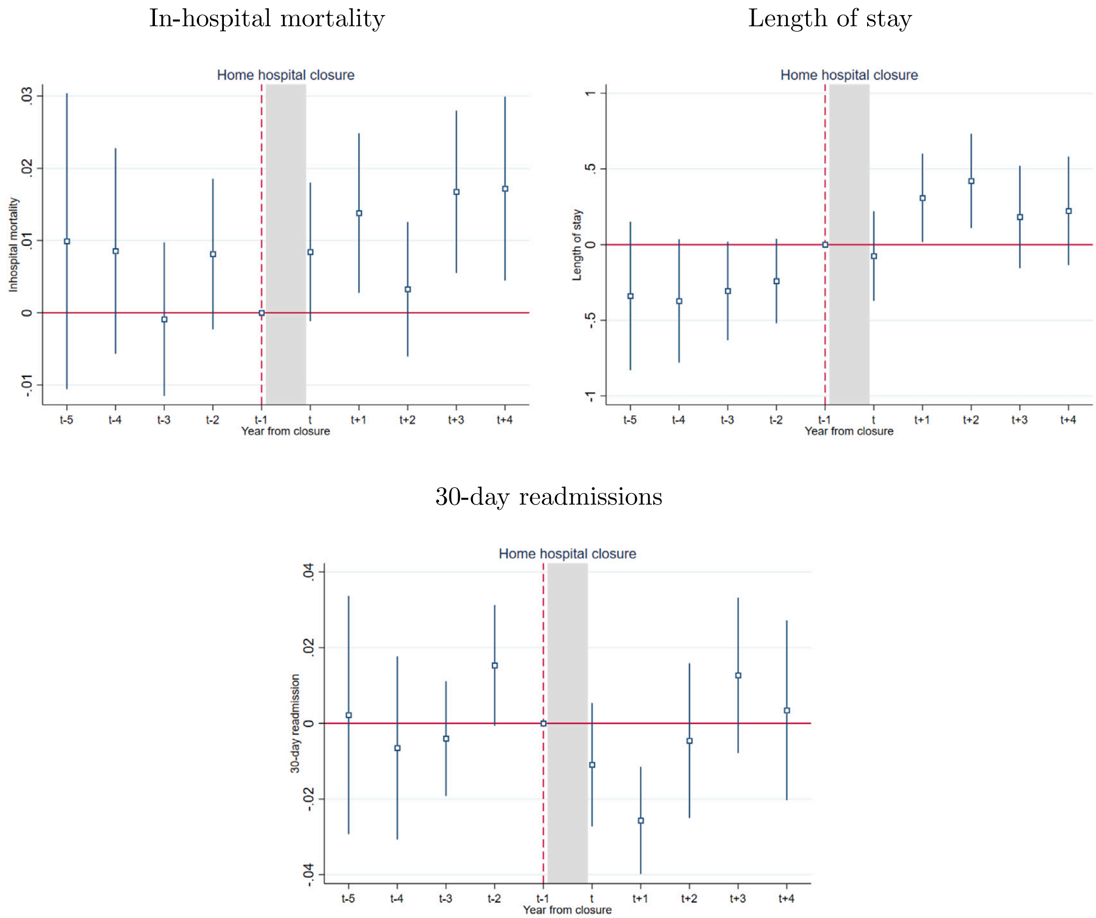

Fig. 2. Time trends in the effects of hospital closures.
Notes: These figures report the point estimates θ and 95-% confidence intervals from Eq. (2) four years before and after closure. Note that the scales on the vertical axes differ across the three panels. The red dashed line marks the beginning of the hospital closure (year prior to drop in admissions by 90%), and the grey area represents the time period during which the closure happened.

10, 30 and 50% of patients whose home hospital closed. As shown in tables A10 and A11 in appendix C, the results are virtually unchanged compared to that in our main analysis.

Fifth, we apply a stricter definition of hospital closure (reduction to zero instead of 10% emergency AMI patients compared to the previous year) and find results very similar to that of our main DID analysis see tables A12 in appendix C. Finally, we also check if our estimation results hold when clustering the standard errors at the home hospital instead of the municipality level, and find that they are also robust to this specification change (see table A13 in appendix C).

4.2. Heterogeneity analysis

As described at the end of Section 3.3, we also perform various heterogeneity analyses using the same identification strategy as outlined in Eq. (1). Table A14 displays the results on the subsamples of male and female patients, patients above and below 80 years old, and patients from the South/North of Italy (see Fig. 3).

It becomes clear that the effects of hospital closures on in-hospital mortality are mostly driven by women, the elderly and patients treated in the South, although the point estimates are significant at a 5% level only for the latter two sub-populations. Further, increased length of stay after a closure is driven mostly by patients treated in Northern hospitals. Similarly, significantly lower readmission rates following hospital closures are observed in patients admitted to hospitals in the North but not in the South of Italy. This pattern is in line with the general perception that overall treatment quality is higher in Northern regions, where more resources are available. This might increase patients' probability of surviving, even if their home hospital was closed and travel time is now longer, while the closure might still increase their length of stay as they are more severely impacted when arriving at the hospital. The decreased readmission rates might be due to better in-hospital care for severely affected patients, availability of high-quality rehabilitation facilities and better outpatient care in the North. Additionally to the differences in quality, we also observe that travel time to the hospital on average increased significantly more for patients from Southern municipalities compared

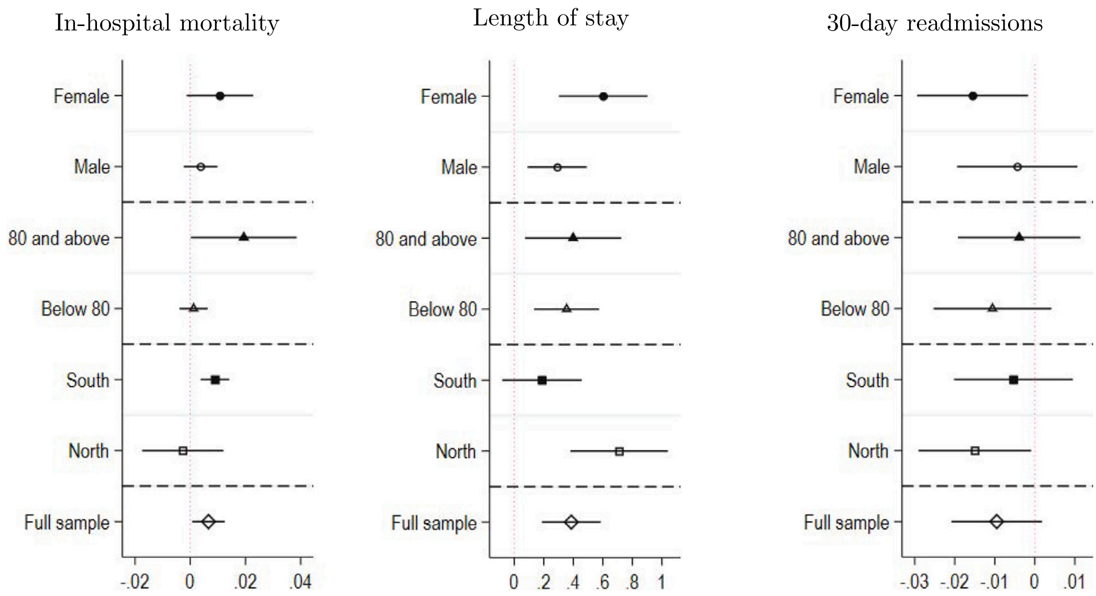

Fig. 3. Heterogeneity analyses for the three main outcomes.
Notes: These figures report the point estimates α_1 and 95-% confidence intervals for all heterogeneity checks described in Section 3.3. Note that the scales differ across the three panels.

to those from the North if they experienced a closure (3.62 versus 1.37 min). This differential increase might interact with hospital quality, and further drive the heterogeneous results between the South and the North described above.

4.3. Mechanisms

As outlined in Section 3.4 we are interested in the mechanisms or channels behind the effects of hospital closures on patient outcomes.

In order to elicit the direct effect of hospital closures (beyond the travel time channel) we follow Deuchert et al. (2019) and estimate the effect of a closure on so-called never-takers. Those are patients who were affected by a closure but whose travel time to the home hospital did not change from the first year of observation to the last (e.g. because there was another hospital near by). The control group in this DID setting are patients who were not affected by a closure, and had no change in travel time to the home hospital during the study period.

The coefficients on mortality are insignificant (column (1), panel A in table A15 in appendix C), providing evidence that there might not be a direct effect of closure on the probability to die in the hospital. Note that this direct effect includes any channel other than travel time, hence, this indicates that in-hospital mortality is primarily affected by a hospital closure because travel times to emergency care increase. For LoS, too, the direct effect of a closure is not statistically significantly different from zero at conventional confidence levels (column (2), panel A in table A15). For readmission rates, the never-taker analysis yields significantly positive coefficients, (column (3), panel A in table A15) indicating the presence of other channels apart from travel time. As the main DID analysis on the unrestricted sample showed no effect on readmissions, this positive coefficient indicates that the direct and indirect effects work in different directions, cancelling each other out.

We extend this never-takers analysis by comparing it to the effects of a closure on patients that did experience a change in travel time to their home hospital by 1 to 10 min (panel B in table A15 in appendix C) and by over 10 min (panel C in table A15) after the closure. Fig. 4 depicts the DID results for these subsamples. For patients with a moderate increase in travel time (between 1 and 10 min), other channels apart from travel time, such as congestion effects due to spill-overs from the closures, might be driving the increased in-hospital mortality. For those patients that had a substantial increase by more than 10 min the effects are insignificant, suggesting that for this subsample it does not matter whether the increase in travel time was induced by a hospital closure or not. For the other two outcomes, LoS and readmissions, the subsample analysis of changes in travel time to the home hospital of more than 10 min show significant results, albeit the point estimates show significant increases in the former but decreases in the latter outcome. Overall, this suggests that the indirect effect via travel time is less relevant if travel times increase substantially, and that there are potential trade-offs between the three outcomes.

Another approach to disentangle the direct from the indirect effects is to block the travel time channel by restricting the sample to patients with a travel time below 10 and 15 min. Table A16 in appendix D reveals some interesting patterns. For in-hospital mortality, there is no direct effect if we use the stricter travel time threshold (Table A16, column (1)). However, if we relax the restriction, the coefficient becomes significant (column (2)). This indicates that, in line with the medical literature, treatment needs

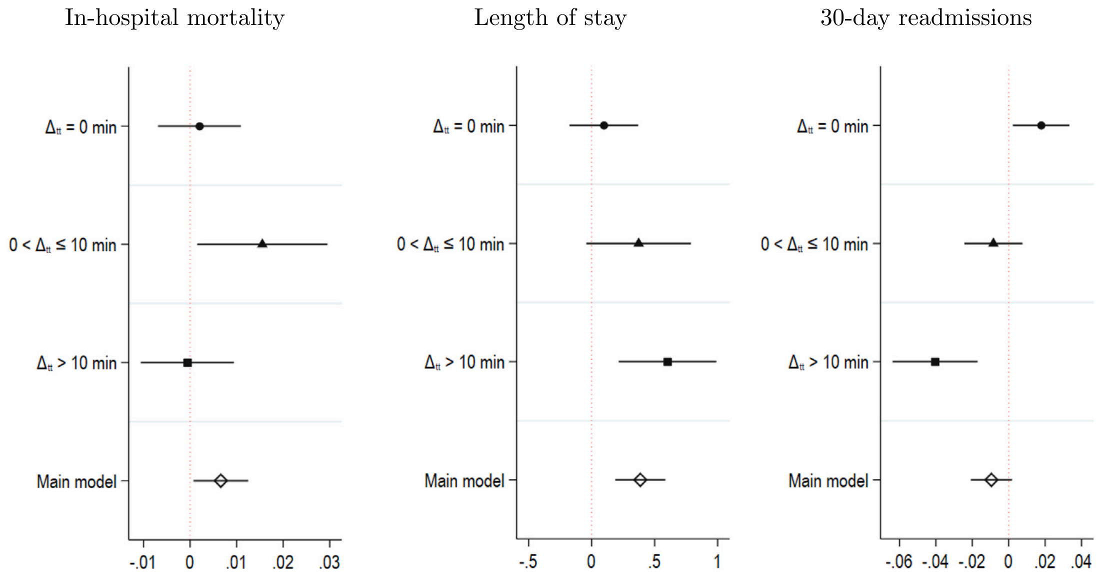

Fig. 4. DID analysis by change in travel time.
Notes: These figures report the point estimates α_1 and 95-% confidence intervals for different cut-offs of changes in travel time (Δ_{tt}) to the home hospital over the study period: 0 min (i.e. never-takers), 1 to 10 min, and more than 10 min. Note that the scales differ across the three panels.

to be provided within a certain time frame after the heart attack occurred. Below the threshold of 10 min, there is no direct effect of closure on mortality, while below 15 min other channels such as quality of care might play a significant role. These results further underpin the findings of the never-takers approach. For length of stay, the coefficients are all not significantly different from zero, too (columns (3) and (4) in Table A16), indicating no direct effect of closure as found in the never-takers analysis.

The analysis of readmission as the outcome shows that when strictly blocking the travel time channel, a closure actually increases the readmission rates after 30 days (see Table A16). This might be due to the fact that, while patients might not die due to reduced quality (e.g. because of bed or staff shortages) if they get to the hospital in time, they still have a higher probability of being readmitted due to a lack of resources after a hospital closure. Again, these results confirm those from the never-takers analysis.

Finally, we also investigate the role of bed utilization in explaining the effect of hospital closure on AMI patient outcomes. As we are interested in whether patients are affected by closures differently if they are admitted to more or less crowded hospitals, we run a heterogeneity analysis comparing the lowest and highest bed utilization quintiles (see table A17 in appendix D). Patients treated in hospitals within the lowest quintile of bed utilization rate in cardiac care units show a positive effect of closure on in-hospital mortality rates, while those patients treated in a high-quintile hospital seem to be unaffected by a closure. This indicates that high bed utilization rates reflect increased quality, possibly due to more experienced health professionals, rather than decreased quality due to congestion effects. However, readmission rates after a closure significantly increased only in hospitals with higher bed utilization rates. So, patients admitted to high-frequency hospitals might be more likely to survive even if they are affected by a closure, but they are also more likely to be readmitted.

5. Discussion

We study the causal impact of hospital closures on AMI outcomes in Italy during a period characterized by high levels of decentralization and cost containment measures. Using a staggered difference-in-differences approach, we compare health outcomes among elderly AMI patients from municipalities that were exposed to home hospital closures with those that were not. With a sample of over 800,000 AMI cases and 37 home hospital (ward) closures identified between 2008 and 2015, our results show a significantly increased probability of experiencing in-hospital mortality following a home hospital closure at the individual level. Specifically, AMI patients affected by home hospital closures display a 0.7 percentage points increase in the likelihood of dying at the hospital which corresponds to an increase of around 10% at the mean mortality rate. Results also show an increased length of stay following a home hospital closure, but no significant effect on cardiac or circulatory related and AMI compatible 30-day readmissions. Overall, our empirical results reveal adverse effects of hospital closures on patient outcomes. These findings are robust to a number of sensitivity checks, and seem to be driven mostly by female patients and those over the age of 80 years. Another sub-group analysis shows interesting geographical patterns, in particular with respect to the increased in-hospital mortality, which is mostly driven by patients treated in hospitals located in Southern regions. Finally, we also investigate the mechanisms driving our results, such as the travel time from the patient's municipality of residence to the hospital's municipality. We employ a novel approach that disentangles the direct effect of a hospital closure from the indirect effect via travel time by identifying what we defined as "never-takers", i.e. municipalities affected by a closure without experiencing a significant increase in travel time to the new home hospital. Results indicate that spatial accessibility is the most important mechanism explaining the increased in-hospital mortality and might also be responsible for longer inpatient stays.

This study adds to the existing body of literature on the effects of hospital (ward) closures on patient outcomes which is mainly based on the US and Northern European welfare states. Regarding in-hospital mortality, our findings are broadly consistent with those reported by previous studies. Using emergency department closures in California between 1999 and 2010, Liu et al. (2014) detected 5% higher odds of inpatient mortality for admissions occurring near a closure compared to farther away among the general patient population. However, this estimate increased to 15% when only patients with an AMI were considered, confirming a higher vulnerability of patients with time sensitive conditions. Similarly, Gujral and Basu (2019) find an increase in AMI mortality of 6.1% following hospital closures in California between 1995 and 2011, and Carroll (2019) finds increased mortality for time-sensitive conditions including AMI of 5% among Medicare enrollees. Our findings are also consistent with existing literature on increases in travel time as a potential mechanism through which closures might affect patient outcomes (Avdic, 2016; Buchmueller et al., 2006).

The result that hospital closures have a positive effect on length of stay (LoS) is also in line with several studies (Gujral and Basu, 2019). Higher LoS may indicate overcrowding, but also that the AMI admissions were of higher severity (and therefore increased complications) following a delay in time to treatment due to a closure. Studies have also found longer LoS to be positively related to hospital readmissions, particularly among heart failure patients (Rachoin et al., 2020; Reynolds et al., 2015). This implies that patients with longer LoS are more complicated cases and given this, they are also more likely to be at the risk of readmission. However, other studies find an inverse relationship between longer length of stay and readmission following an AMI (Eapen et al., 2013). For instance, a lower LoS may indicate a speed-up behaviour to manage the new inflow of patients which might result in an increase in hospital readmissions. Further research is needed to examine how the trade-off between length of stay and readmission affects patient welfare when exposed to shocks in public healthcare supply.

Our study does not find statistically significant effects of home hospital closures on hospital 30-day readmission rates. Prior evidence on the role of hospital closures or mergers on hospital readmissions is mixed. For instance, Ho and Hamilton (2000) use a difference-in-differences methodology to estimate the effects of hospital consolidation in California between 1992 and 1995 to find a positive effect on 90-day readmission following a heart attack. In contrast, several studies have also found statistically non-significant effects of hospital closures (Joynt et al., 2015; Song and Saghafian, 2019) and mergers (Beaulieu et al., 2020) on readmission. This might also be a consequence of the increased mortality following a closure, which mechanically introduces a downward bias to the results on readmission.

The focus of this study is on heart attacks because they represent a major cause of mortality and morbidity in Italy, and at the same time, constitute a typical time-sensitive condition. Not being an elective, AMI is generally seen as less biased by endogeneity caused e.g. by spatial sorting. There are, of course, other acute medical interventions for which variation in access is relevant. Notably, obstetric units are another area of interest in recent literature. Two studies (Fischer et al., 2024; Durrance et al., 2024) find slightly positive results from maternity unit closures. Although interesting, it is difficult to compare outcomes of maternity ward closures to those of emergency cardiac units: giving birth is usually more predictable and less time-sensitive than a heart attack. More generally, hospital closures can be expected to be less harmful, or even beneficial, when considering time-insensitive conditions or elective treatments. This is also why conclusions in the literature on overall patient outcomes are mixed (Beaulieu et al., 2020; Joynt et al., 2015; Song and Saghafian, 2019).

There are some limitations to our study. First, our measure of mortality fails to account for out-of-hospital mortality, such as deaths occurring before hospitalization or after discharge. If a hospital closure decreases the accessibility of emergency services, our results should be considered as a conservative measure of the effect of hospital closure on AMI mortality outcomes. Second, our 'treatment' occurs on the municipality level — so anybody residing in the same municipality in a given year is either treated by a hospital closure or not. This might lead to imprecise estimates if heterogeneity within a municipality in terms of geographic location is high. Third, closures are defined on the basis of a drop in AMI admissions. Therefore, our results reflect the consequences of AMI ward closures and not necessarily complete hospital closures. Adverse effects might be even larger if only closures of entire hospitals are considered. It is also worth noting that the regional governments might potentially take compensating measures in order to counterbalance the impact of a hospital closure. For example, they could decide to invest more in primary care in a specific area, increase availability of outpatient care or even expand ambulance services. With the available data we cannot control for these possibilities, but it is unlikely that our conclusions would change. First of all, although rational, these policies would have been difficult to implement given the tight budget constraints. Indeed, we have no evidence of such counterbalancing mechanisms, neither in the media nor in the scientific literature. Second, even assuming that in some regions compensating measures were actually implemented, our results show that these did not fully counterbalance the negative impact of longer travel distances on the risk of dying after a heart attack. Hence, if anything, the estimated impact of hospital closures on AMI patients would be biased downwards by any implemented compensating measures.

We limit our study to the health effects of the closures without monetizing them. Theoretically, it would be interesting for policy makers to juxtapose the savings (i.e. the public budget cuts that led to the hospital closures) with the human costs of increased in-hospital mortality. We refrain from such a comparison for two reasons. First, estimating the statistical value of life-years lost would require a thorough discussion of the underlying methods and ethical considerations, which is out of the scope of this paper.[11] Second, in a public, single-payer system most expenditures, such as for treatment of patients and for medical staff, will be shifted from the closed hospitals to the open ones. To explore whether the hospital closures led to fewer treatment costs per AMI patient during

[11] For a very crude estimation of the value of a statistical life lost see appendix E.

our study period, we compare the average DRG-related payments in regions that did not have any hospital closures (untreated) with regions that had at least one hospital closure (treated). We find that average payments per patient were generally higher in regions without any closures, but, in regions with a closure, they were lower before the closure than after (with the exception of 2011). In the years 2013 to 2015, when all regions that ever closed a hospital in our observation period had already closed at least one, the difference in average DRG-payments per patient becomes virtually zero (see figure A5 in appendix E). This is in line with the results of our main analysis of significantly increased lengths of stay after a closure.

From a policy perspective, our analysis shows that: (1) decision makers selected small hospital (wards) for closure, consistent with the underlying assumption that efficiency gains could be achieved by shifting patients to bigger facilities; (2) despite the reasonable rationale for these policies, there is no free lunch when it comes to emergency procedures like AMI — i.e. there are hidden costs associated with larger distances. In the present paper we show that these hidden human costs were significant and should be considered when designing future policies. In an NHS context, where planning of services is largely top-down, policy makers need to consider that the optimal spatial distribution of hospital care depends on the area of intervention. Our results show that time-sensitive conditions such as AMI require further attention and, if possible, effective compensating measures in case of a closure. It is important that the political debate also considers the downsides of cutback management strategies. In particular, rational policies in this field require continuous and effective monitoring. Information is key in this process, especially at a micro level. Transparency and timely data access is the only way to provide adequate insights for rational decision making, something that is particularly relevant in the area of health policy and planning. Only when ex-ante specified budgetary and health outcomes data is collected and made available for research, will it be possible to juxtapose the (human) costs and benefits of hospital closures.

6. Conclusions

Using admission-level data from Italy, our study shows that hospital closures severely affect patient outcomes, and that such effects can be persistent over time. This evidence provides the first assessment of such policies under severe public budget constraints. While hospital closures might be justified by efficiency reasons, our results show that effective policies to minimize human and social costs by ensuring continued quality of care are imperative.

At the same time, our findings show that Italian decision makers were generally rational in how they implemented a measure of last resort such as cutting back on acute inpatient care. According to our data, the closed hospital (wards) were used mainly for less severe cases, which are more likely to endure increased travel times. Despite this, we show that the negative externalities were neither adequately internalized in the decision process nor sufficiently compensated by counterbalancing investments. The present study also shows that distance remains the crucial dimension in the definition of optimal policies regarding time-sensitive interventions such as AMI. This is important for policy design and provides evidence in support of increasing attention and investments in emergency medical services and local points of care, especially in rural low-density areas. Finally, it should be noted that results for time-sensitive conditions likely do not apply to non-emergency interventions. In this sense, further research is needed in order to assess the impact of closures in a general economic equilibrium model, including the cost and savings of closure decisions, a wider range of health outcomes and differential condition-specific interventions.

CRediT authorship contribution statement

Simone Ghislandi: Writing – review & editing, Writing – original draft, Supervision, Methodology, Conceptualization. **Anna-Theresa Renner:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Project administration, Methodology, Formal analysis, Data curation, Conceptualization. **Nirosha Elsem Varghese:** Writing – original draft, Validation, Methodology, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

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

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Appendix. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jhealeco.2025.102975.

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