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A Matter of Life and Death? Hospital Distance and Quality of Care

Evidence from Emergency Hospital Closures and Myocardial Infarctions

Imprint

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Abstract

The article analyzes to which extent residential proximity from an emergency hospital affects the probability of surviving an acute myocardial infarction (AMI). The critical time aspect in AMI treatment provides an ideal application for evaluating this proximity outcome hypothesis. Previous studies have encountered empirical difficulties relating to potential endogenous health-based spatial sorting of involved agents and data limitations on out-of-hospital mortality. Using policy-induced variation in hospital distance arising from plausibly exogenous emergency hospital closures in the highly regulated Swedish health care sector, and data on all AMI deaths in Sweden over two decades, estimation results show a clear, robust and gradually declining probability of surviving an AMI of about two percentage points (three percent) per additional ten kilometers distance from a hospital. Results further show that spatial sorting and sample selection from out-of-hospital mortality are likely to significantly attenuate the distance effect unless accounted for.

JEL Classification: C23, I14, I18, R41.

Keywords: myocardial infarction, geographical access, hospital closures, health policy, spatial sorting, self-selection, out-of-hospital mortality, causal effect.

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1 Introduction

Cardiovascular disease (CVD) is the primary global cause of death, causing 17.3 million deaths each year (expected to increase to more than 23 million per year in 2030), and the single most important factor contributing to that non-communicable diseases have recently overtaken communicable diseases as the world's greatest burden of disease. Of these deaths, atherosclerotic diseases, especially ischemic heart disease (e.g., myocardial infarction (MI)) and cerebrovascular disease (e.g., stroke), accounted for approximately 80 percent, with the former category responsible for a slightly larger overall share (WHO, 2011).

Although most CVD deaths occur in low- and middle-income countries, also in Sweden, a high-income country with approximately nine million inhabitants, more than half a million persons suffered from an acute myocardial infarction (AMI) over the last two decades. Most of these individuals are deceased today with AMI as either the underlying or a contributing cause of death. Total AMI incidence in Sweden during the same period exceeded 800,000 cases, making the AMI one of the most frequent causes for hospitalization and the leading cause of death in the country, with about one-seventh of all deaths (National Board of Health and Welfare, 2009, 2014). In other words, around 12 percent of the Swedish population are expected to have an AMI at some point in their life and one-third of these will not survive its aftermath (CPR, 2011). Far from unique in this respect, Sweden shares these morbidity and mortality patterns with most OECD countries. For example, almost half a million, or one in six, deaths in the U.S. per year are the result of an MI (Go *et al.*, 2013).

The relatively high mortality rates for AMI arise primarily from two specific characteristics of the disease: the lack of indication signals, or the unexpectedness, of the disease (more than two-thirds of Swedish AMIs occur at home) and the crucial importance of time for a successful treatment.[1] In the event of a cardiac arrest, a common manifesta-

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[1]Indeed, for cardiovascular diseases "time is muscle" and "door-to-balloon time" are common phrases, referring, in particular, to the critical importance in reducing the ischemic time from first MI symptoms to reperfusion treatment (see e.g., Antman, 2008).

tion of the infarction, the brain suffers irreversible damage after only five minutes due to the lack of oxygen. After 15 minutes, death is essentially unavoidable regardless of any resuscitation attempts made (see e.g., Pell *et al.*, 2001; GUSTO Investigators, 1993). These two characteristics together imply that professional medical assistance may be unavailable when the life-threatening condition sets in and, hence, many AMI patients die before they reach an emergency room.[2,3]

Given the fundamental importance of time in AMI treatment, it is interesting to note the emerging trends of health care consolidation in many countries. In countries with more unregulated health care markets, such as the U.S., these trends have been primarily driven by increased competition in the health care sector where hospitals have either merged into giant multi-hospital units, or been ousted due to competition from more efficiently managed hospitals (see e.g., Dranove *et al.*, 1996; Evans-Cuellar and Gertler, 2003). In countries with more regulated and, mainly, public provision of health care, such as Sweden, rapidly increasing health care costs and public budget deficits, along with general technological progress, innovations and a need to attract physicians and specialist nurses, have been a driving factor behind the structural changes (FFCC, 2002; SALAR, 2008). Thus, regardless of the institutional context, the long-run trend in the organization of inpatient health care has been a considerable increase in the concentration of resources. One noteworthy feature of these recent trends have been the tendency of an increase in the number of rural hospital closures and a corresponding growth in size of urban hospitals. Hence, while potentially leading to efficiency gains, these consolidation trends are also likely to have entailed adverse effects on health care equity in terms of a general deterioration in geographical access to care.[4]

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[2]More than 60 percent of all AMI deaths in the U.S. occur in the home (Go *et al.*, 2013). The corresponding figure in Sweden is around two-thirds. Other common locations are: streets and squares (8%), in an ambulance (5%) and in nursing homes (2%) (cf. CPR, 2011).

[3]Researchers from the European Resuscitation Council (ERC) estimated that each year about half a million people in Europe suffer a sudden cardiac arrest, out of which 100,000 could be saved if bystander CPR rates are increased according to the new 2010 ERC guidelines (Nolana *et al.*, 2010). Furthermore, in a literature review authors estimated that 29,000 lives are saved in Europe every year among individuals affected by a out-of-hospital cardiac arrest (Atwood *et al.*, 2005).

[4]In this context it is interesting to note that Swedish health care authorities justified the health care consolidation policies with the argument that emergency hospitals, while traditionally important

The focus of this article is to empirically assess the impact of geographical access to health care on AMI survival for individuals who suffered an AMI in Sweden between 1990 and 2010. Previous research on this topic have typically found that ambulance response time increases the chance of surviving an out-of-hospital AMI (see e.g., Bachmann *et al.*, 1986; Piette and Moos, 1996; Norris, 1998; Pell *et al.*, 2001). However, this conclusion mainly stems from evidence based on case studies, i.e., studies using data on single hospitals and/or data culled at one particular point in time, and results inferred from these studies may potentially suffer appreciably from limitations associated with both the external and internal validity of any estimated parameters. Furthermore, location data on both patients and hospitals are likely to be subject to dynamic spatial sorting where agents' choice of residence is based on factors related to AMI survival probabilities such as individual health and the quality of nearby hospitals. In particular, individuals in poor health would, *ceteris paribus*, choose to reside closer to a hospital, compared to individuals in good health.[5] A few economic studies have taken the analysis a step further by exploiting hospital closures and openings using U.S. data (see e.g., McClellan *et al.*, 1992; Buchmueller *et al.*, 2006; Cutler, 2007; Herr, 2009).[6] However, one potential drawback with this approach is that hospitals in more market-oriented health care systems are likely to be strategically located with respect to underlying patient characteristics and regional competition. For example, profit-maximizing hospitals are unlikely to be located in impoverished areas where the patient population has poor general health (cf. Dranove *et al.*, 1996; Succi *et al.*, 1997). For this reason, there is a risk that hospital closures used to estimate health effects of geographical access to health care may partly be the result of selection.

for health care equity policies, are less important today due to recent innovations in emergency medical treatment (SALAR, 2004). For example, over the last few decades some therapeutic progress has been made, including the introduction of specific MI wards, mobile defibrillators, more effective treatment of cardiac arrest and the introduction of drugs such as beta blockers, thrombolytic agents, aspirin, ACE inhibitors and lipid-lowering drugs (cf. Julian, 1961; Dellborg *et al.*, 1994; Herlitz, 2000).

[5]See also e.g., Currie and Reagan (2003); Pons *et al.* (2005); Attar *et al.* (2006); Grzybowski *et al.* (2011); Wilde (2013); Etzioni *et al.* (2013); Chou *et al.* (2014).

[6]There is also an extensive literature on the effect of hospital closures *per se* on patient health outcomes; see e.g., Bindman *et al.* (1990); Rosenbach and Dayhoff (1995); Succi *et al.* (1997); Liu *et al.* (2001); Lindrooth *et al.* (2003); Kansagra *et al.* (2004); Capps *et al.* (2010).

A second problem hampering the assessment of the impact of distance to hospital on health in previous studies is the lack of out-of-hospital data. Using only inpatient data when attempting to quantify the effect of distance implies that patients who die before reaching a hospital will be censored. If geographical access to health care has an impact on survival probability, omitting patients that dies en route to hospital will underestimate any true distance effect, since admitted patients living farther away from, rather than closer to, the hospital will, on average, be in a better health state (cf. Gillum, 1990; O'Neill, 2003).

The main contribution of this paper is to extend the, comparatively scarce, literature on the effects of geographical access to health care on health outcomes, by utilizing detailed individual-level Swedish administrative data on all AMI occurrences for the whole population over a twenty-year period. The rich data make it possible to account for both cross-sectional and time variation in AMI survival rates and to control for observed individual heterogeneity. In addition, out-of-hospital AMI mortality sample selection is accounted for by supplementing the Swedish National Patient Registry with the Swedish Causes of Death Registry, containing detailed information on all deaths occurring in Sweden. Furthermore, to account for unobserved heterogeneity from spatial selection by residential hospital distance, the paper exploits a number of Swedish emergency hospital closures over the studied time period. In the beginning of the 1990s, Sweden had an extensive geographical spread of emergency hospitals across the country. However, the economic crisis of the 1990s resulted in large public deficits and, as a reaction to this, aggregate health care spending was cut by more than 10 percent. A large portion of these cost savings were derived from an increased concentration of specialist care; in particular the closure of a number of emergency hospitals. These closures, plausibly unrelated to individual AMI survival probabilities due to the public nature of health care provision, entailed an implicit change in the distance to an emergency hospital for patients residing in the catchment areas of a closed emergency hospital. Utilizing variation in individual distances to hospitals generated from the policy-induced closures circumvents endogeneity

problems arising from self-selection by estimating AMI survival probability as a function of the current geographical distance to an emergency hospital, while conditioning on pre-closure distance. Finally, the richness of the data also allows for detailed analyzes of the mechanisms and channels through which the distance to hospital affects health outcomes, as well as evaluating the stability of inferences through extensive sensitivity analyses.

Results from estimation show that an increase in distances significantly predicts a lower AMI survival probability for patients residing in the catchment area of a closed emergency hospital. Specifically, the estimates suggest that increasing geographical distance to an emergency hospital from within 10 kilometer to more than 50 kilometers would result in a decrease in expected AMI survival probability of almost 13 percentage points, corresponding to a 15 percent reduction at sample mean survival rates. This effect is primarily driven by an increased risk of out-of-hospital mortality among affected patients. Much smaller effects are found when estimating the impact of distance to hospital based on actual distances, indicating that selective residential sorting is likely to greatly dilute the effect. Moreover, the effect is greatest in the first year after the closures and diminishes gradually thereafter, indicating no long-run elevated AMI mortality from the closures. Some evidence is provided that this attenuation is partly caused by post-closure migration from the previous catchment areas of the closed hospitals. A causal interpretation is also supported by results from relaxing the linear restriction and, furthermore, the finding of a symmetric effect; that is, that patients who experienced a *decreased* hospital distance from the closures *increased* their probability to survive an AMI. Finally, there is no evidence of selective closures with respect to catchment area case-mix or the quality of closed hospitals; a conclusion strengthened by the results from a placebo test, carried out by estimating separate effects for ST-elevated and non ST-elevated infarctions.

The results from this study may be contrasted with the volume-outcome literature, in which resource consolidation may increase health care quality due to, for example, scale effects and learning-by-doing (see e.g., Maerki *et al.*, 1986; Luft *et al.*, 1987; Hamilton and Ho, 1998). According to this literature, consolidation increases health care quality

and is hence considered desirable. However, the disease context is likely to be crucial as to which of these effects is likely to dominate. Specifically, while Thiemann *et al.* (1999) find a positive association between hospital volume and survival of AMI patients, it is likely that any positive quality effects from health care concentration in this context should be more counteracted by the negative effects on survival, arising from a decrease in geographical access to health care, than for planned surgery where the situation is less acute (e.g., organ transplants and cancer surgery). In particular in more market-based systems, where geographical coverage of hospitals to a large extent should be determined by outcomes from competition and selective location of profit-maximizing hospitals, long distances to hospitals may exacerbate already existing health inequalities prevalent in many communities. In such cases, the results from the present study should rather be regarded as conservative estimates of the effect of geographical access to health care.

The remainder of the article begins with a brief summary of the Swedish health care system and general information on treatment of AMI in Section 2. Section 3 contains a presentation of the data and the sampling methodology while Section 4 offers a careful review of the empirical approach, in particular with respect to the various inferential problems encountered. Section 5 presents the results from estimations and Section 6 contains a short summary along with some concluding remarks.

2 Background and institutional context

This section is intended as a conceptual background for the studied context. It first provides a brief summary of the pathology and treatment of AMI and subsequently outlines the organization of the Swedish health care system in which the analysis is conducted.

2.1 Pathology and treatment of AMI

AMI, or ischemic heart disease, is a circulatory disruption in the coronary arteries, usually caused by one or more blood clots occluding the blood supply to the heart. The blood clots causes hypoxia in the heart muscle, *myocardium*, which damages the parts of the heart affected by the oxygen deficiency. The longer it takes before the blood clot is dissolved, the more severe the damage, or *infarction*, will be. Common symptoms of AMI include sudden and radiating chest pain, palpitations, nausea, dizziness and shortness of breath, but the condition is in general quite hard to diagnose. Common risk factors are lifestyle factors such as obesity and smoking, hypertension, previous history of heart disease, diabetes, pneumonia and chronic obstructive pulmonary disease and some neoplasms.

Most heart disease fatalities results from a sudden *cardiac arrest*, where the heart has suffered from ventricular fibrillation, or "electrical chaos", which means that it ceases to actively pump blood to the rest of the body. Cardiac arrest is in the majority of cases (between 70-80 percent) caused by an AMI (other causes include asthma, overdoses, suffocation and drowning). Therefore, a cardiac arrest has similar symptoms as an AMI but is a much more serious condition: After only five minutes, the body, particularly the brain, begins to take serious damage due to the lack of blood supply and, after fifteen minutes, death is virtually inevitable, as most vital organs have suffered irreparable damage by then. As with AMI, the symptoms of a cardiac arrest are often quite vague, which makes it difficult to identify in time.

AMI is generally treated by restoring circulation to the heart, called *reperfusion theraphy*, either through pharmacological means (thrombolysis), through percutaneous coronary intervention (PCI) or, surgically, through coronary artery bypass surgery (CABG). Thrombolytic drugs stimulate fibrinolysis, which counteracts coagulation of the blood, and hence works to break down the blood clots in the coronary arteries, giving rise to the condition. PCI is typically performed by feeding a deflated balloon through the skin and transporting it, through the blood vessels, to the heart, where it is inflated in order to restore blood supply by crushing the atherosclerotic plaque against the artery wall.

A PCI is usually done in combination with the implanting of small mesh tubes, called "stents", at the blocked regions in order to prevent subsequent obstructions of the blood flow. CABG, on the other hand, is a method to restore circulation to the heart by surgically grafting veins from other parts of the body onto the coronary artery in order to bypass the atherosclerotic narrowings causing the infarction. Which procedure is chosen depends primarily on the severity and type of the infarction, the number of obstructed arteries and the prevalence of co-morbidities, such as, for example, diabetes.

The primary medical treatment for cardiac arrest is cardiopulmonary resuscitation (CPR) and defibrillation. CPR is a manual method to maintain blood circulation when the heart has stopped. It is implemented through powerful chest compressions (to generate circulation) and careful breathing through the victim's mouth (to oxygenate the blood). The prevailing rule, according to contemporary international guidelines, is to first carry out thirty compressions, immediately followed by two valves compounds; the so-called 30:2 method. Defibrillation is performed using an electrical defibrillator which delivers an electrical shock to the heart, hopefully ending the fibrillation and facilitating return to its normal rhythm. Innovations in both these treatments have been significant over the last thirty years: It is estimated that, presently, about half of the adult population in Sweden has undergone basic CPR training, and mobile, fully automatic, defibrillators, requiring no education to use, are now available in many workplaces.

2.2 The Swedish health care system

In contrast to more market-based systems, such as, for example, the U.S., health care in Sweden is highly regulated and, almost exclusively, government funded. The public sector in Sweden comprises three tiers; the national, the regional and the local level, and the responsibility for health care generally takes place on the regional level, as regulated by the Swedish Health Services Act (1982:763). The health care system is organized and financed by 21 independent regions (counties), Stockholm being the largest (with

about 2 million inhabitants) and Gotland the smallest (with about 60,000 inhabitants).[7] Health care is the single most important responsibility for the regional administration; for instance, in 2012 on average 82 percent of the county budgets were spent on health care. The regional administrations are governed by political councils chosen in national elections every four years. Besides following a few general guidelines set by the national government (e.g.„ that health care should be provided to all Swedish citizens) the regional authorities have high levels of discretion in organizing health care. In practice, this institutional setting implies that political representatives of the county councils and local bureaucrats, rather than competition among health care providers, largely determine the number, size, location and coverage of hospitals in each region.

Another feature of the highly regulated Swedish health care sector is that patients have little choice of which hospital to visit when they require medical care. As health care in Sweden is funded essentially by direct taxes, there exist no individual contracts between patients and hospitals.[8] Instead, depending on where a patient lives, he or she will be directed to a specified nearby hospital when in need of health care. This institutional setting ensures that each patient has a designated "home hospital" each year, which can be identified by using aggregated historical admission data for each municipality and linking this information to the patients' registered home.

The time period studied in this article, i.e., 1990–2010, was a period of strong con-solidation of the Swedish health care sector. These measures were deemed necessary by

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[7]Two major structural changes in the Swedish regional administration occurred during the time period studied in this article. Both occurred in 1999 and reduced the total number of regions from 25 to 21. First, three of the counties around the Gothenburg metropolitan area, Bohus, Skaraborg and Älvsborg counties, merged with the fourth, creating the Västra Götaland region. Second, the two counties in the Malmö area, Malmöhus and Kristianstad counties, merged into the new Skåne region. These changes pose no problem for the empirical analysis as it is mostly carried out on the individual and hospital catchment area level.

[8]When seeking health care in Sweden a small fee is normally paid up front by the patient. In Stockholm county (there are regional variations) this fee currently (2013) ranges from 100 SEK ($\approx$10 EUR) when, for example, visiting a physiotherapist to 400 SEK ($\approx$40 EUR) when visiting an emergency ward. However, when a patient has paid a total of 1,100 SEK ($\approx$110 EUR) in health care fees in one year, he or she receives a "free card" and health care is free for the remainder of the year. A similar payment system exists for pharmaceuticals in which the patient's share of the drug cost decreases with the total amount spent. In 2013, the maximum amount paid by the patient was 2,200 SEK ($\approx$220 EUR). See, for example, http://www.vardguiden.se/Sa-funkar-det for more information.

regional authorities in order to increase efficiency and to cover public deficits caused by the economic turbulence in Sweden in the beginning of the 1990s.[9] In total, government spending on health care decreased by more than 10 percent, from 8.8 to 7.7 percent of GDP, between 1990 and 2000. A significant share of these savings were derived from structural changes in health care organization within counties; in particular the closure of a number of emergency hospitals across the country (FFCC, 2002).

Importantly, due to the institutional features of the Swedish health care sector and the economic turbulence in the 1990s, the observed hospital closures are likely to be arbitrarily related to the health characteristics of the underlying population in the hospitals' catchment areas. This is in contrast to more market-based systems, where mainly poor and less efficiently driven hospitals are closed (see e.g., Lindrooth *et al.*, 2003). As each individual patient's designated hospital is observed at each point in time, these policy-induced closures can be used to compute the shift in geographical distance to the new home hospital among patients whose emergency hospitals were closed, thereby generating plausibly exogenous variation in hospital distance. However, even if the hospital closures in the Swedish context should not be driven by competition in the health care market, one might still worry about politicians' selective choices of which hospitals to close. In order to evaluate this potential problem and to validate the empirical approach, a number of supplementary sensitivity analyses are therefore also carried out in addition to the main analysis.

3   Data and sampling

The data used in this study is primarily based on administrative registers from the Swedish National Board of Health and Welfare, covering all Swedish citizens during the

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[9]The Swedish 1990s economic crisis took place between 1990–1994 and was a combined banking, financial and housing market crisis which is said to have been primarily caused by an unfortunate deregulation of the Swedish credit markets in 1985 (Wohlin, 1998). The financial deregulation led to currency and housing speculation bubbles which deflated in 1991 and resulted in a severe credit crunch and widespread bank insolvency. The cause and development of the Swedish 1990s crisis had much in common with the U.S. sub-prime mortgage crisis of 2007–2008.

years of study. The registers include the Swedish National Patient Register (NPR), consisting of detailed information on all recorded hospitalizations in Sweden, and the National Causes of Death Register (NCDR), consisting of all recorded deaths of individuals with a permanent residence in the country.[10,11] Specifically, the NPR includes individual-level data, for each hospital, on date of admission and discharge, whether the patient was admitted from home or from another clinic, a set of patient characteristics, discharge diagnoses classified according to the ICD standard,[12] and any surgical procedure(s) undertaken during the hospital stay. In addition, the NCDR includes the date, place and underlying and contributing causes for each death.

The population of interest consists of all Swedish residents who suffered an AMI between 1990 and 2010. Because the available data include linked information on both AMI hospitalizations and AMI mortality, it is possible to generate the full AMI population each year. In contrast, if information on out-of-hospital mortality was not available, then a sample selection problem would arise if a distance effect did exist, since individuals living further away from a hospital would have a lower probability of being admitted. Therefore, the analysis sample includes all hospitalization and deaths records caused by ischemic heart diseases with a primary ICD-10 diagnosis code of I21 or I22, corresponding to an acute myocardial infarction or re-infarction.[13] Additional information from each hospitalization is also collected, such as patient age, gender, residence, and hospital and clinic of visit, as well as medical histories on previous admissions and AMI risk factors for each patient dating back to 1987. The date of death is added to this data from the

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[10]The population consists of all deaths that were reported to the Swedish Tax Agency, including all individuals registered as Swedish residents at the time of death. Hence, registered citizens who died outside Sweden (e.g.,, vacationers) are included while unregistered citizens who died in Sweden are not.

[11]The number of deaths recorded in the NCDR is in practice equivalent to all deaths that occurred in the relevant population. For example, the number of unrecorded deaths in the NCDR in 2007 amounted to 0.84% (773) of all deaths.

[12]The diagnoses are made by physicians and classified according to the World Health Organization's International Statistical Classification of Diseases and Related Health Problems (ICD-10). ICD-10 is a seven digit coding of diseases and signs, symptoms, abnormal findings, complaints, and external causes of injury or diseases. See, for example, http://www.who.int/classifications/icd/en.

[13]The re-infarctions (less than one percent of all infarctions) are sampled for completeness but, as individuals with previous infarctions may be different than other patients, has also been removed from the empirical analysis in an unreported robustness check. The results remain robust to this exclusion and are available from the author upon request.

NCDR (if the individual died at some point).[14]

As the data contains individual identifiers it is possible to link the AMI sample to other population registers from Statistics Sweden to add further patient characteristics. One such crucial characteristic is detailed geographical coordinates for each individual's registered place of residence, measured according to the RT-90 standard.[15] These coordinates are subsequently used to compute the geographical distance from the registered place of residence of each AMI patient included to his or her designated home hospital for each year in the analysis.[16]

Hospital distance $D(\cdot,\cdot)$ is in the empirical analysis of this study primarily measured by the Pythagorean distance formula in two-dimensional Euclidean space:

$$D(\mathbf{p},\mathbf{q}) = \sqrt{(q_1 - p_1)^2 + (q_2 - p_2)^2},$$

where $\mathbf{p} = (p_1, p_2)$ and $\mathbf{q} = (q_1, q_2)$ are the Cartesian coordinates for the individual's registered place of residence and the location of the individual's hospital, respectively. This formula gives the minimal distance ("as the crow flies") between the points as a proxy

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[14]As the main outcome of the empirical analysis is the probability of surviving an AMI, the following population breakdown is important; *i)* patients who survived until they were admitted to a hospital, survived the AMI and were discharged, *ii)* patients who survived until they were admitted to a hospital but died while in hospital and *iii)* individuals who died before reaching a hospital and hence were not admitted. It is assumed that all AMI patients need inpatient care and hence that there are no patients who survived the AMI but were not admitted. This should not be a crucial assumption as infarctions that do not require an admission are not likely to be very dangerous. As the goal of the empirical analysis is to investigate the effects of the distance to hospital on AMI mortality, excluding out-of-hospital AMI deaths will entail an endogenous sample selection under the alternative hypothesis of the existence of an effect of distance. Therefore, the inclusion of all three categories, using data from both the NPR and the NCDR, is essential to establish inference to the population of interest.

[15]Coordinates in "Rikets koordinatsystem" (RT-90) are computed using the Gauss conformal projection or the Transverse Mercator map projection. In contrast to the Standard Mercator projection, the transverse projection takes into account that the world is shaped as an ellipsoid and uses complicated calculations and so-called geodetic datums in order to deliver improved accuracy positioning measurements. According to the Swedish Ordnance Survey, the RT-90 measurements cover approximately 3800 triangular points over the country with a relative distance accuracy of 1-2 ppm (mm/km).

[16]In practice, the coordinates used in this paper are based on the Small Areas for Market Statistics (SAMS) classification, created by Statistics Sweden in January 1994 and last revised in 2003. The classification is based on registered property names (NYKO) in the larger municipalities and on electoral districts in the smaller. The total number of SAMS districts in Sweden are about 9,200. The SAMS division has remained largely intact over the years and any revisions are minor adjustments have been made to adjust the boundaries of updated municipal borders.

for the true travel distance for an AMI patient. In order to investigate the robustness of this distance measure, information on actual driving distance and driving time has also been collected using geocoding software from STATA® and Google® and reported in subsequent analyses.

3.1 illustrates the total number of recorded AMIs between 1990–2010, broken down into relevant categories. As can be seen, out of approximately 817,000 AMIs, about 75 percent (626,000) show up in the NPR as inpatient care records while the remaining quarter (191,000) consists of individuals who died before arriving at a hospital, and hence only show up in the NCDR. In total, about 65 percent (535,000) of the AMI population survive the AMI while about 35 percent (281,000) die, either before (68 percent) or after (32 percent) being admitted. Clearly, ignoring out-of-hospital mortality will substantially underestimate total AMI mortality in Sweden during this period.

Figure 3.1: Acute Myocardial Infarctions in Sweden,1990–2010
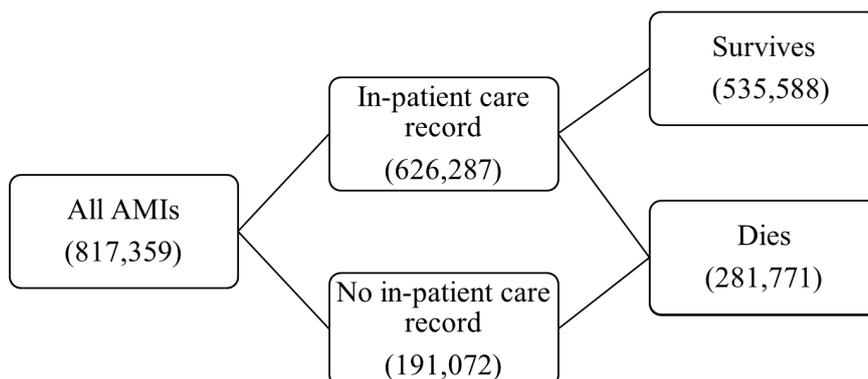
3.1 Home hospitals, emergency room closures and referral hospitals

In order to compute an individual's distance to a hospital, a "home hospital" is defined for each individual and calendar year based on place of residence. This hospital is selected using historical data on AMI hospitalizations and municipality of residence from the NPR for each municipality and year. In particular, the hospital to which most of the inhabitants of a given municipality are admitted (i.e., the modal hospital) is defined as

the home hospital for all individuals residing in this municipal.[17] For most municipalities this procedure is straightforward. However, a few municipalities do not have a clearly defined home hospital for all the years concerned and, for this reason, patients residing in these municipalities are removed from the analysis.[18] Rather than using the actual hospital a patient visits to compute hospital distance, the distance to the designated home hospital is used. In most cases, but not always, these are the same.[19]

The home hospital definition is used to compute distance changes due to hospital closures in two steps: First, in order to identify individuals who were affected by an emergency hospital closure and, subsequently, to compute the new distance to hospital for these individuals by defining a new home hospital (the referral hospital) and the new geographical distance to this hospital.[20] The distance to the new home hospital after closure is subsequently used in the empirical application to estimate the parameters of interest. Emergency hospital closures are defined by the change in the number of AMI admissions they receive across two consecutive years.[21] A total of sixteen closures are observed between 1990–2010.[22] The closures identified in the data are also validated from

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[17]In a subsequent robustness check, the closest hospital is also used to define an individual's home hospital. As these hospital are typically the same it is not surprising that the results are qualitatively and quantitatively the same irrespective of the definition.

[18]The dropped municipalities are: Salem, Håbo, Boxholm, Ödeshög, Vaggeryd, Hultsfred, Mönsterås, Aneby, Osby, Kungsbacka, Tanum, Färgelanda, Herrljunga, Örkelljunga, Svedala, Falkenberg, Lerum, Grästorp, Vansbro, Leksand and Jokkmokk. These municipalities constitute about seven percent of the total number of Swedish municipalities and much less of the total AMI population. Moreover, none of these municipalities are located in regions where an emergency hospital closure occurred.

[19]This classification is used for several reasons: First, a counter-factual hospital needs to be assigned for AMI patients who died before reaching a hospital. Second, patients observed to be treated at other hospitals than their designated home hospital are likely to be unrepresentative with regard to the distance they actually traveled (e.g., because they were in another region when the AMI occurred). Third, the Swedish institutional setting makes the home hospital definition very reliable; between 80-90 percent of all admissions in the sample occur at the home hospital.

[20]The same strategy is used to define the referral home hospitals as the other home hospitals, i.e., from historical admissions in the NPR it is inferred which hospital patients living in closure-affected municipalities are referred to after a closure.

[21]Specifically, a hospital is classified as closed if the number of AMI admissions between two years decreases by more than 90 percent. Remaining admissions in closed hospitals (less than one hundred) are dropped from the analysis.

[22]The closed emergency hospitals are Löwenströmska, Nacka, Finspång, Simrishamn, Landskrona, Strömstad, Falköping, Kristinehamn, Säffle, Sala, Fagersta, Sandviken, Söderhamn, Härnösand, Boden and Luleå hospitals. Only one hospital opening was observed during the time period: Sunderby hospital in 1999, which replaced Boden and Luleå hospitals.

other sources such as official documents, local media coverage and previous research.[23]

3.2 (and A.1–A.6 in the Appendix) present the monthly number of visits for each closed hospital and the corresponding referral hospital over the period of study. The names of the hospitals are kept undisclosed for protection. The panel on the left of each closure plot depicts the unadjusted raw number of admissions while the panel on the right displays a six-month moving average of admission frequency. The figures clearly show that the referral hospitals essentially absorb the full reduction in admissions from the closed hospitals.[24]

Figure 3.2: Number of visits at closing hospitals and their referral hospitals over time
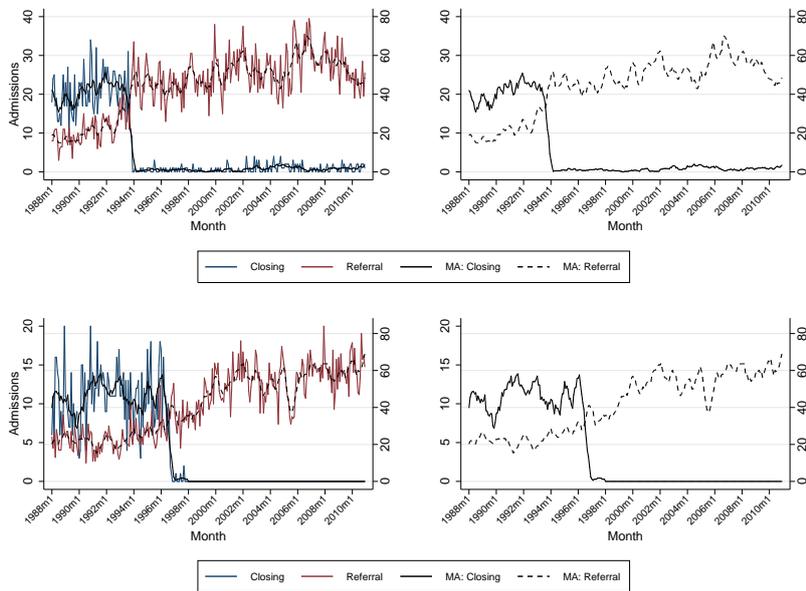
NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

The left panel in 3.3 shows the distribution of distance from a home hospital in the data aggregated over all years of study. Approximately 95 percent of the population lives within a sixty-kilometer radius of their home hospital with a median distance of nine kilometers.[25] As the distance distribution is highly right skewed, the upper five percentiles

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[23]For example, Lindbom (2013) investigates protests movements in relation to the hospital closures over the same time period and FFCC (2002) discusses Swedish emergency hospital closures between 1992 and 2000.

[24]Note that the hospitals are plotted on different axes.

[25]The metric system is adopted as length measurement in this article. One English mile is approxi-

of the distribution are trimmed away in order to have a more homogeneous sample and to avoid estimation problems due to extreme outliers (the maximum hospital distance in the sample is 142 kilometers). This restriction mainly affects individuals living in the rural parts of northern Sweden. The panel on the right in 3.3 shows the corresponding distribution of the changes in distance generated from the emergency hospital closures. These changes in distance have reasonably good coverage over the support of the baseline distance distribution in the panel on the left.

Figure 3.3: Distribution of distance and changes in distance to home hospital
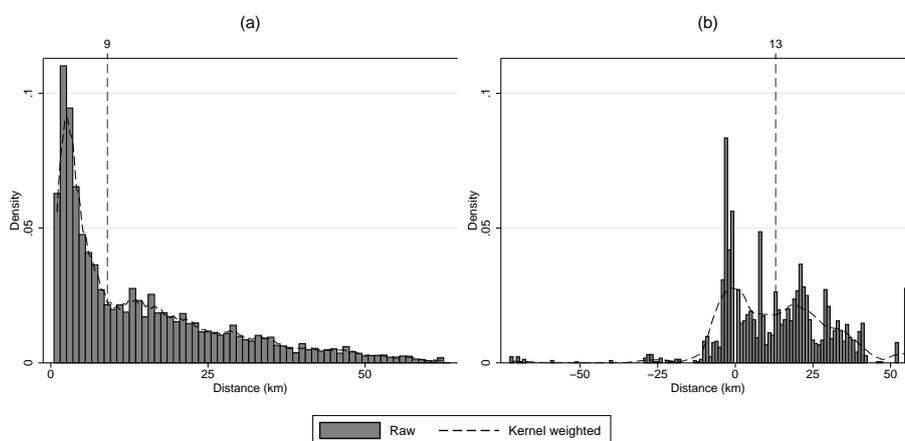
NOTE.— The left panel depicts the home hospital distance distribution for the analysis sample. The right panel shows the distribution of changes in distance generated by the emergency hospital closures. The dashed vertical lines indicate the median of the distributions and the dashed smoothed lines are kernel density estimates.

3.2   Sample restrictions and descriptive statistics

A few sample restrictions had to be imposed due to data limitations. First, the previously mentioned municipalities which were dropped due to the lack of a clearly defined home hospital account for about 20,000 observations (2.5 % of the sample). None of these municipalities are located in catchment areas where a hospital was closed and hence their exclusion should be innocuous. More importantly, as the population registry used to obtain the geocodes only covers individuals in working age, it is only possible to compute hospital distances for individuals below 74 years of age. This excludes around 290,000

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mately 1.61 kilometers.

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observations (35 %) from the original sample. Hence, the subsequent empirical analysis should be mainly seen as generating internally valid inference for the working age population. However, as older individuals are typically more fragile, it is not unreasonable to argue that the estimates would also provide a lower bound for the effect of the full AMI population. Furthermore, trimming the upper five percent of the sample distance distribution drops another 20,000 cases from the analysis. As individuals living further away from a hospital should not have an increased probability to survive an AMI, this restriction would, if anything, generate a conservative bias of the effect of distance. Finally, as some individuals are admitted from other places than their homes (such as, for example, another hospital clinic for those who were already admitted) it is important to be able to exclude such hospitalizations. Fortunately, the NPR contains information on admission origin which can be used for this purpose. Excluding individuals with other admission origins than home (other hospital clinics and long-term care homes) excludes another 40,000 observations (5 %) from the original sample.

After the above mentioned restrictions we are left with around half of the original sample, or 374,000 AMI cases. 3.1 reports sample statistics separated into individuals who did and did not experience a distance change due to a hospital closure, and the mean difference between the groups, respectively. Included variables are health information on previous hospital visits and the prevalence of known AMI risk factors in the medical history, such as diabetes, coronary heart disease (CAD) and chronic obstructive pulmonary disease (COPD), according to Smith *et al.* (2004, 2012). Socioeconomic characteristics include gender, age, schooling, income, being a commuter and marital status. The groups do not differ significantly in most of the factors but, to the extent that they do, individuals who experienced a distance change seem to be in somewhat better health. Furthermore, the change in distance due to the hospital closure increases the average distance to a hospital with around 12 kilometers for the group affected by the closures; a highly significant change. Finally, with respect to sample selection of individuals who die before they reach a hospital, it is interesting to examine the observed AMI mortality patterns across the

groups. In particular, the probability of out-of-hospital mortality (OHM) from an AMI in our sample is significantly higher for the individuals who experienced a hospital closure, while, in contrast, in-hospital mortality (IHM) is lower (albeit not significantly so) in the same group, compared with individuals unaffected by the closures. This further stresses the importance of sampling all AMIs for consistent estimation of distance effects.

Table 3.1: Descriptive sample statistics

Variable	Group averages		Group difference	
	No distance change	Distance change	Mean difference	$P(\lvert Z \rvert \leq z)$
Health characteristics				
Number of heart surgeries	0.304	0.287	-0.017	0.346
	(0.823)	(0.763)	(0.018)	
Years since last hospital visit	5.632	5.991	0.360	<0.001***
	(2.973)	(3.084)	(0.066)	
Years since last heart surgery	8.236	9.012	0.776	<0.001***
	(2.660)	(2.070)	(0.059)	
Days in hospital	6.735	6.393	-0.342	0.043**
	(6.640)	(5.137)	(0.169)	
Hypertension	0.025	0.028	0.004	0.303
Diabetes	0.057	0.073	0.016	0.002***
Obesity	0.002	0.005	0.002	0.046**
Substance dependence	0.024	0.027	0.003	0.395
Pneumonia	0.049	0.048	-0.001	0.898
CAD	0.263	0.261	-0.002	0.865
COPD	0.033	0.034	0.002	0.685
Neoplasm	0.033	0.026	-0.006	0.108
Socioeconomic characteristics				
Female	0.310	0.295	-0.015	0.159
Age	71.70	71.19	-0.510	0.040**
	(1.115)	(1.114)	(0.025)	
Unknown education	0.149	0.144	-0.006	0.478
Compulsory schooling	0.453	0.440	-0.013	0.239
Secondary schooling	0.295	0.310	0.015	0.140
Post-secondary schooling	0.102	0.106	0.004	0.585
Unknown marital status	0.255	0.231	-0.024	0.012**
Married	0.439	0.474	0.035	0.002***
Unmarried	0.100	0.098	-0.002	0.790
Divorced	0.136	0.135	-0.002	0.842
Widow	0.069	0.063	-0.007	0.243
Commuter	0.058	0.091	0.034	<0.001***
Personal income	116,477	120,119	3,642	0.255
	(133,492)	(126,412)	(3,203)	
Household income	261,035	260,067	-967	0.901
	(301,113)	(184,512)	(7,718)	
Distance and AMI characteristics				
Hospital distance in j	14.228	14.169	-0.059	0.846
	(13.557)	(11.117)	(0.305)	
Hospital distance in $j+1$	14.228	26.219	11.991	<0.001***
	(13.557)	(12.673)	(0.305)	
AMI Survival	0.744	0.729	-0.015	0.122
AMI OHM	0.203	0.227	0.024	0.008***
AMI IHM	0.054	0.045	-0.009	0.083*
Observations	372,334	1,982	374,316	-

NOTE.— The table reports means and (standard deviations) of included covariates for the analysis sample, separated into groups affected by a change in distance due to an emergency hospital closure or not, respectively. See the text for variable information. The last two columns show the difference in group means and the result from a standard z-test of equality of means between the groups. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

4 Empirical approach

Let D be the geographical distance from an AMI patient's home to his or her designated home hospital. Furthermore, let y be a binary variable indicating whether an AMI patient survived the infarction or not. Specifically, y is coded as one if the individual survives a certain follow-up period from the time the AMI occurred and as zero otherwise. The focus of this study is to empirically evaluate the impact of D on y.

There are several difficulties associated with isolating the effect of hospital distance on AMI survival probability. First, an individual's choice of where to live in relation to the proximity of a hospital might depend on the individual's own health status. In particular, any estimated effect of distance based on cross-sectional residential information would be biased downwards if individuals in poorer health are more likely to take health care access into consideration when choosing where to live.[26] In addition to identification problems arising from the optimizing behavior of individuals, there are also other empirical issues related to the organization of health care and the population case-mix in the catchment areas, causing AMI survival rates to vary both over time and across hospitals.[27]

It is possible to control for heterogeneity and common trends across hospitals by including fixed-effects for these factors in a regression model. Moreover, as the data contains a number of individual health and socioeconomic characteristics, it is also possible to adjust for individual-level heterogeneity of the patient population within catchment areas. Specifically, for an individual i experiencing an AMI at calendar time t with home

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[26]An upward bias could also occur if individuals who choose to live further away from a hospital in general are less caring about about their health relative to people living closer to a hospital due to, for example, heterogeneous health preferences. This possibility is not ruled out in the estimations but are considered less likely from a theoretical point of view.

[27]Hospitals located in rural areas admit patients with on average both longer distances to the hospital and poorer health characteristics (e.g., older and with a lower level of education). In addition, the preparedness levels for emergency situations may vary between hospitals (e.g., the number of turnkey ambulances) as a consequence of the geographical size of the catchment area. Finally, innovations in AMI treatment and general urbanization trends may create spurious correlations between average hospital distance and survival probabilities.

hospital h, the effect of distance on survival could be estimated with the following model:

$$y_{iht} = \alpha + D_{iht}\beta_1 + X_{it}\gamma + \lambda_h + \lambda_t + \nu_{iht}, \tag{4.1}$$

where λ_h and λ_t are hospital and time fixed effects. The distance effect, captured by β_1, would be identified in this model if the individual error term ν_{iht} is uncorrelated with the distance measure, D_{iht}. As the health status of AMI patients is, most likely, partly unobservable, residential sorting within hospital catchment areas may exist also after including observable characteristics, X_{it}, hence invalidating the independence assumption.[28]

To address the problem of residential sorting, this paper exploits individual variation in distance to a hospital generated from closures of emergency hospitals. Using this variation, it is possible to estimate the impact of hospital distance on AMI survival among patients who lived in a closed hospital's catchment area.[29]

Specifically, assume that the variation in hospital distance generated from the closures can be implemented by estimating the following model (OLS)

$$y_{iht} = \alpha + D_{iht}\beta_1 + D_{iht-j}\beta_2 + X_{it}\gamma + \lambda_h + \lambda_t + \eta_{iht}, \tag{4.2}$$

where D_{iht-j} is the distance for an AMI patient j years before the AMI occurred. Here, $\Delta_{D_{iht-j}} = D_{iht} - D_{iht-j}$ is the change in distance to the home hospital between the years t and $t-j$. For all patients living in the catchment area of a non-closing hospital,

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[28]A simple example may be illustrative. Assume that ($y_{it} =$ AMI survival, $\kappa_i =$ health status)

$$Pr(y_{it} = 1 | D_{it}) = 0 \quad \text{if} \quad I(\kappa_i < \kappa_i' | D_{it}), \tag{i}$$

and that $\frac{\partial \kappa_i'}{\partial D_i} < 0$ so that patients in poor health have incentives to reside nearer to a hospital. Furthermore, consider the following relation determining distance

$$D_{it} = \alpha + \kappa_i^* \xi + \tau_{it},$$

where health is measured with error, i.e.,

$$\kappa_i^* = \kappa_i + \pi_{it}.$$

Then distance will be endogenously related to health if (i) applies, i.e., if $Cov(\pi_{it}, D_{it}) < 0$.

[29]With respect to the previous footnote the hospital closures would correspond to random shocks to τ_{it} in the distance equation.

these distances are the same, i.e., $D_{iht} = D_{iht-j}$. The latter do not contribute to the identification of the effect of distance but are still included as they increase the statistical precision of the estimated parameters.[30]

Inference drawn from estimation of model (4.2) will vary depending on the underlying residential sorting process active in the data. In particular, if the relation between individual health, κ, and the distance changes, generated from the closures, is additively separable, that is, $\Delta_{D_{it}} \perp\!\!\!\perp \kappa_i$. If this condition holds, then the difference $\hat{\beta}_D = \hat{\beta}_2 - \hat{\beta}_1$ can be interpreted as the estimated impact from experiencing a change in hospital distance of Δ_D. However, if, for example, individuals living further away from a hospital at baseline are affected by greater distance changes, then $\Delta_{D_{it}} \perp\!\!\!\perp \kappa_i | D_{it-j}$ has to be assumed. Hence, $\hat{\beta}_2$ would then be interpreted as the distance sorting factor while $\hat{\beta}_1$ is the causal distance effect for patients who experienced a change in hospital distance, conditional on their pre-closure distance, D_{it-j}. The results will be primarily discussed under the latter, less restrictive, assumption of conditional (mean) independence between health and the changes in hospital distance.

The primary justification for the identification strategy is that individuals in affected catchment areas cannot immediately adapt to the changing health care environment caused by the decision to close a hospital. The total number of data observations experiencing a change in distance will vary depending on the length of the time window between the closure and the AMI. However, extending the time window to increase the number of patients that are affected also increases the risk of endogenous reactions to the closures, such as selective migration, and may hence bias the estimation results. Thus, the credibility of the maintained assumption of no endogenous response decreases with

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[30]Variation in residential distance to a hospital may hypothetically emerge from two different sources; hospital closures and individual migration. Specifically, consider the following distance-generating functions for time periods t and $t-j$, $D_t(coord_{it}, coord_{ht})$, $D_{t-j}(coord_{it-j}, coord_{ht-j})$, where the first argument in the functions is the patient's residential coordinates and the second argument is the coordinates of the patient's home hospital. Now, given that a patient in the year of the closure $(t-j)$ does not migrate between the two time periods (i.e., $coord_{it} = coord_{it-j}$) only a switch of home hospital may result in a distance change. Hence, under the assumption that individuals do not selectively migrate between the two time periods, the change in distance should be unrelated to individual AMI survival probabilities, conditional on the pre-closure distance.

the lag, j.

In model (4.2) the distance variables are included linearly. To increase the flexibility of the regression model, the linearity restriction is subsequently relaxed by instead including a set of indicator variables for each ten-kilometer distance.[31] Specifically,

$$y_{iht} = \alpha + \sum_{m=1}^{M} I_{iht}^m \beta_1^m + \sum_{m=1}^{M} I_{iht-j}^m \beta_2^m + X_{it}\gamma + \lambda_h + \lambda_t + \eta_{iht}, \qquad (4.3)$$

where

$$I_{iht-j}^m = 1[((m-1) \times 10) < D_{iht-j} \le (m \times 10)].$$

Finally, as the emergency hospital closures also generated distance *cuts* for some patients to their home hospitals, it is possible to evaluate the existence of effect symmetry by regressing the effect of a positive change and a negative change separately and statistically test whether the coefficients differ. Specifically, consider the following model

$$y_{iht} = \alpha + (\Delta_{D_{it}} \times I_\Delta^-)\delta_1 + (\Delta_{D_{it}} \times I_\Delta^+)\delta_2 + X_{it}\gamma + \lambda_h + \lambda_t + \eta_{iht}, \qquad (4.4)$$

where $I_\Delta^+ = 1(\Delta_{D_{it}} > 0)$ and $I_\Delta^- = 1(\Delta_{D_{it}} < 0)$. To test the symmetry of the distance effect, a simple Wald test of equality of δ_1 and δ_2 is performed. In addition, this model also serves as a specification check by including the distance changes directly into the model rather than, as in model (4.2), using both distance variables separately.

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[31]Furthermore, since the dependent variable in the models is dichotomous, the linear probability models are an approximation of an unknown data generating process. To test the validity of the model approximation, all the results below were also estimated using non-linear (logit) regression models. The results remain qualitatively unchanged by this particular change in specification. The linear estimates are presented throughout the paper to facilitate coefficient interpretation.

5 Results

This section begins with a descriptive analysis of the observed distance-survival relationship in the data. 5.1 plots the observed correlation of distance to home hospital and AMI survival rates under different parametric assumptions. In particular, the dots indicate the average survival rate for each kilometer to hospital while the dotted, dashed and solid lines illustrate the relationship under a linear regression model, a locally smoothed and a kernel weighted parameterization, respectively. The figure suggests a negative, albeit weak, relationship between distance and AMI survival with slightly higher survival rates for individuals living closer to their home hospital. The estimate from the linear model, reported below the plot, suggests a decreased survival probability of 0.07 percentage points for each additional kilometer a patient resides from his or her home hospital. With a mean sample survival rate of about 0.78, this is a relatively small change of less than one percent decrease in survival per 10 kilometer increased hospital distance. However, the upward sloping survival trend at the lower end of the distance distribution raises some doubt about whether the plotted relationship can be interpreted causally. For example, A.7–A.8 in the Appendix show substantial heterogeneity in survival rates both across hospitals and over time. If these factors are correlated with hospital distance, any estimated effect of distance will be confounded unless they are accounted for.

Figure 5.1: Correlations of distance to home hospital and survival probability from an AMI under various parametric assumptions

Linear coeff: −.0682
t–stat: −5.98
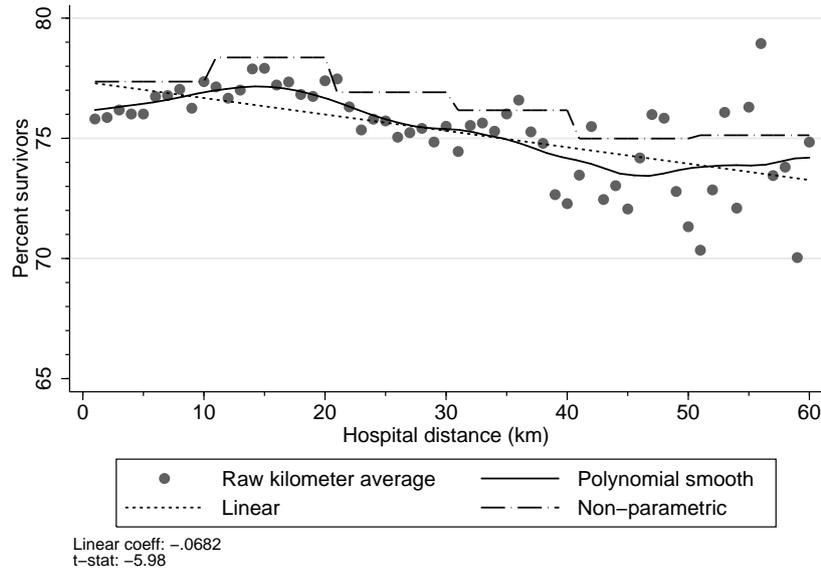
Note.— The figure depicts the observed correlation between distance to home hospital and survival probability for the analysis sample under different parametric assumptions. The dots indicate the raw kilometer average and the lines show the relationship for different models: the dotted line indicates the linear relationship, the dashed line the non-parametric relationship with a dummy indicator for each ten kilometers and the solid line shows a kernel density estimator using a standard Epanechnikov kernel with a bandwidth of 3.9.

5.1   Main results

5.1 presents the main results from the estimation of the effect of distance for different models using the full analysis sample. In order to facilitate interpretation, the coefficients are scaled with a factor of one thousand in order to report the effect in percentage points per ten kilometers distance. The first and second columns include only the observed *current* distance to the home hospital, i.e., the observed distance in the year the AMI occurred. In particular, the first column reports the individual-level linear estimate of the bivariate distance-survival correlation from 5.1. The second column reports the results from estimating model (4.1), including the full set of health-related and socioeconomic characteristics reported in 3.1 along with hospital and calendar time fixed effects, respectively. The estimated distance coefficient decreases with about twenty percent when controls are included, implying that some residual confounding is being accounted for.

The third column of 5.1 additionally includes the one year *lagged* distance for patients

in the year before they had an AMI, corresponding to equation (4.2) with $j = 1$. The coefficient on current distance now increases in magnitude by a factor of four while the lagged distance coefficient is slightly smaller in magnitude and with opposite sign as would be expected if significant residential sorting were prevalent in the data. Comparing over specifications, note that netting out the predicted effect for individuals with the same distance in both periods reproduces, as expected, the distance coefficient displayed in the second column. The current distance coefficient is now interpreted as the marginal effect of increasing a patient's distance to his or her home hospital by ten additional kilometers. Hence, the estimated difference in AMI survival probability is about 12 percentage points (15 percent at the sample mean survival rate) for individuals at the lower and upper support of the distance distribution, or about two percentage points (three percent) per ten kilometers.

Hypothetically, the estimated effects on AMI survival should be monotonically decreasing with distance to a hospital. The last two columns of 5.1 evaluate this prediction by relaxing the assumption of linearity of the effect by instead including a set of distance dummy variables for each ten kilometer distance according to model (4.3). The table reports the results from this exercise with and without the full set of controls, respectively, and with the closest distance group (0–10 kilometers distance from a hospital) as the reference group. The result, irrespective of inclusion of controls or not, shows a remarkably clear monotonous pattern on AMI survival probability from experiencing a change in hospital distance. The estimated coefficients are highly significant and the pattern corresponds quite well to a linear specification, except for distances between 11 and 20 and 21 and 30 kilometers where a discontinuous shift in survival probability appears, suggesting a critical distance threshold where the risk of AMI mortality increases dramatically.[32] Thus, the conforming of the results to the theoretical prediction with

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[32]This threshold is plausible since, according to CPR (2011), if medical assistance is not received within 15 minutes after suffering from a cardiac arrest, death is almost certain. Performing a back-of-the-envelope calculation and assuming that an ambulance has an average speed of 100 km/h it will take emergency medical personnel about 15 minutes to travel a distance of 25 kilometers, which is exactly in the middle of the empirical threshold where the distance effect kicks in.

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respect to the pattern of the effect of distance provides some support for the empirical design.

Table 5.1: Estimated effects of distance on AMI survival probability from emergency room closures: Different estimators

	$\hat{\beta}_{OLS}$	$\hat{\beta}_{FE}$	$\hat{\beta}_{D}$	$\hat{\beta}_{NPD}$	$\hat{\beta}_{NPD}$
Current distance	-0.478***	-0.584***	-2.232***		
	(0.055)	(0.059)	(0.524)		
Lagged distance			1.663***		
			(0.525)		
Current Distance Dummies					
11-20 km				2.259	0.716
				(2.019)	(1.983)
21-30 km				-3.921**	-4.732**
				(1.977)	(1.943)
31-40 km				-7.417***	-5.761**
				(2.280)	(2.240)
41-50 km				-7.204**	-8.075***
				(3.151)	(3.091)
51-60 km				-14.256***	-12.868***
				(4.638)	(4.556)
Lagged Distance Dummies					
11-20 km				-1.011	-0.667
				(2.018)	(1.981)
21-30 km				3.518*	3.495*
				(1.979)	(1.944)
31-40 km				6.297***	4.055*
				(2.287)	(2.246)
41-50 km				4.739	5.616*
				(3.163)	(3.103)
51-60 km				11.503**	10.367**
				(4.661)	(4.578)
Patient characteristics		✓	✓		✓
Year fixed effects		✓	✓		✓
Hospital fixed effects		✓	✓		✓
Observations	374,316	374,316	374,316	374,316	374,316

NOTE.— The table reports point estimates (standard error) of the effect of hospital distance on the probability of AMI survival for different estimators as explained in the empirical section using the full analysis sample. The outcome is defined as an indicator function (multiplied by 100) for being alive when discharged from a hospital following the infarction. The last three columns, $\hat{\beta}_{D}$ and $\hat{\beta}_{NPD}$, report the effect of distance using variation in the distance to an individual's home hospital arising from emergency hospital closures. The last two columns replaces the linear specification with a number of distance dummies for each ten kilometers distance. Included covariates are; gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Under the more restrictive assumption of additive separability between hospital distance and health, the difference between the coefficients of 5.1 can be given a causal in-
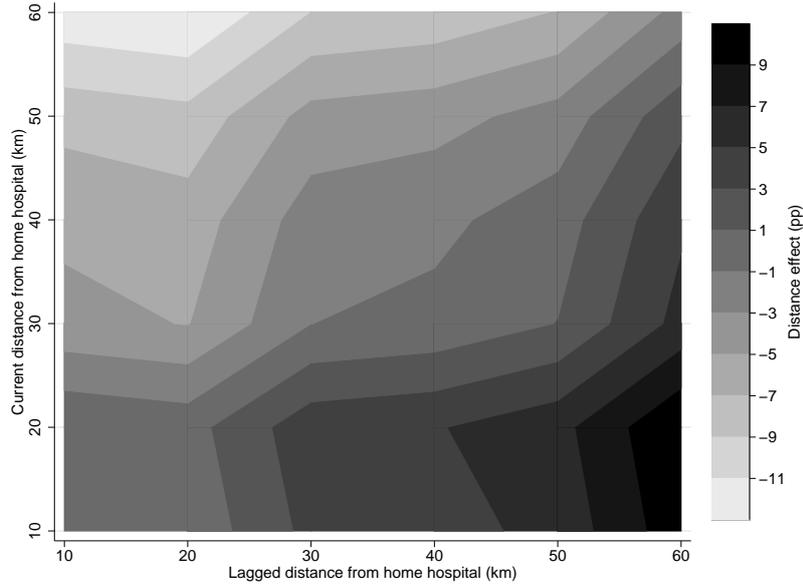
terpretation. 5.2 tabulates all possible combinations of these differences for given lagged and current distances under the additional assumption of homogeneity of the effect of distance across lagged distance. These effects are also presented graphically in a contour plot in 5.2. Specifically, the brighter (darker) areas in the plot show for which combinations of lagged and current distance AMI survival probabilities decrease (increase). Starting from the upper-left corner (illustrating the effect of an *increase* in geographical distance of 50 kilometers) and moving to the lower-right corner (illustrating the effect of a *decrease* in geographical distance of 50 kilometers) the figure shows a clear monotonous and symmetric pattern of the distance effect.

Table 5.2: Estimated effects of distance on AMI survival probability for different pre-closure hospital distances

Current distance (km)	Lagged Distance (km)					
	0-10	11-20	21-30	31-40	41-50	51-60
0-10	0.000	-0.667	3.495	4.055	5.616	10.367
11-20	0.716	0.049	4.212	4.772	6.333	11.083
21-30	-4.732	-5.399	-1.237	-0.677	0.884	5.635
31-40	-5.761	-6.428	-2.266	-1.705	-0.145	4.606
41-50	-8.075	-8.743	-4.580	-4.020	-2.459	2.291
51-60	-12.868	-13.535	-9.373	-8.813	-7.252	-2.501

Note.— The table reports the estimated effect from the last column in 5.1 for all combinations of distance changes. See 5.2 for a graphical illustration of the effect.

Figure 5.2: Contour plot of the estimated distance effect

Finally, some individuals experienced decreased hospital distances after their home
hospital closed as the home hospital was not necessarily the closest hospital. Using this
feature of the data, the changes' model (4.4) was estimated to statistically test the sym-
metry of the distance effect. The result from this exercise is shown in 5.3. The first
column of the table reports the estimated coefficients for the change in distance and an
indicator variable for a negative change interacted with the change in distance. Similarly,
the second column reports results from regressing AMI survival on the *absolute* change in
distance interacted with a dummy variable for a positive and a negative change respec-
tively. Since the hypothesis that the coefficients are the same cannot be rejected for any
conventional statistical significance levels ($p = 0.758$), this suggests that the magnitude
of the effect of distance is similar, regardless of whether an individual experienced an
increase or a decrease in the distance to hospital.

Table 5.3: Estimated effects of distance on AMI survival probability: Symmetry of the effect

Distance Change	(1)	(2)
Δ_D	-1.611***	
	(0.418)	
$I[\Delta_D < 0] \times \Delta_D$	0.474	
	(1.543)	
$I[\Delta_D > 0] \times Abs[\Delta_D] = b_1$		-1.611***
		(0.418)
$I[\Delta_D < 0] \times Abs[\Delta_D] = b_2$		1.136
		(1.484)
Test $b_1 = -b_2$		
χ^2-statistic (1 *df*)		0.09
p-value		0.758
Observations	374,316	374,316

NOTE.— The table reports point estimates (standard error) from estimation of model (4.4) using the full analysis sample. The outcome is defined as an indicator function (multiplied by 100) for being alive when discharged from a hospital following the infarction. The $I[\cdot]$ functions are indicator functions that evaluate to one if the arguments within the brackets are true and zero otherwise. The lower part of the table displays the statistics from a Wald test on parameter equality between the effects of distance from a positive and a negative change in distance, (b_1 and b_2) respectively. Included covariates are; gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

5.2 Extensions

The outcome studied so far has been the probability being discharged alive from a hospital after suffering an AMI. 5.4 presents the results for a number of alternative survival definitions using the same analysis sample and the specification from the last column of 5.1. The first column of the table reproduces the main results while the second column reports the results for the probability of surviving the initial phase before being admitted to a hospital, i.e., the out-of-hospital phase. The four subsequent right-most columns report results when the outcome is defined as a binary indicator for whether the patient was still alive after one day, one month, one hundred days and one year from the AMI, respectively.[33]

The table reveals interesting effect mechanisms; first, comparing the first two columns of the table, it is clear that most of the effect on survival seem to arise from an increased probability of out-of-hospital mortality (OHM).[34] This finding is not unexpected since

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[33]As a complement to this analysis, A.9 in the Appendix plots the distribution of deaths in the sample for the first hundred days after the AMI occurred. Day one is excluded in the figure for scaling reasons as the overwhelming majority of all deaths occur within the first day of the AMI.

[34]The estimated coefficients are much smaller in magnitude and barely statistically significant when

a longer geographical distance to hospital, in general, increases both the time it takes to reach the patient and the time it takes to transport him or her to the hospital. Furthermore, the last four columns of 5.4 evaluate whether the estimated effect is primarily driven by patients in very poor health, in which the additional distance is simply "the straw that broke the camel's back", i.e., a harvesting effect, by comparing results from different survival time horizons after the AMI. Interestingly, the pattern in the last four columns of 5.4 indicate that distance to hospital slightly *decreases* the probability of surviving more than one month, compared with surviving only the first day. This result suggests that the estimated effect is not due to harvesting, in which case we would rather see a substantial effect just after the AMI and thereafter a diminishing and even reversed sign of the effect for the more long-term outcomes.

Table 5.4: Estimated effects of distance on AMI survival probability from emergency room closures: Different survival outcomes

	Survival Outcome					
	Hospitalization		Survival time			
Current Distance	Baseline (AMI=1)	OH Survival (AMI≠ 2)	Survives > 1 day	Survives > 30 days	Survives > 100 days	Survives > 365 days
11-20 km	0.716	-0.075	0.145	0.488	0.751	0.188
	(1.983)	(1.840)	(1.913)	(2.001)	(2.018)	(2.040)
21-30 km	-4.732**	-4.861***	-3.852**	-4.983**	-6.332***	-7.685***
	(1.943)	(1.803)	(1.875)	(1.961)	(1.977)	(1.999)
31-40 km	-5.761**	-6.961***	-6.168***	-6.872***	-7.738***	-8.149***
	(2.240)	(2.079)	(2.161)	(2.261)	(2.279)	(2.305)
41-50 km	-8.075***	-5.703**	-5.676*	-8.446***	-8.129***	-10.003***
	(3.091)	(2.868)	(2.982)	(3.119)	(3.144)	(3.179)
51-60 km	-12.868***	-11.443***	-12.333***	-17.533***	-16.667***	-16.747***
	(4.556)	(4.227)	(4.395)	(4.597)	(4.635)	(4.687)
Observations	374,316	374,316	374,316	374,316	374,316	374,316

NOTE.— The table reports point estimates (standard error) of the effect of distance on survival probability from an AMI as explained in the empirical section using the full analysis sample. Outcomes are defined as indicator functions (multiplied by 100) for being alive when discharged from the hospital following the infarction or surviving until admitted (in the first two columns) and as being alive after a certain time after the AMI occurred (in columns 3-5). Reported coefficients in each column are a set of distance dummies for each ten kilometers. Included covariates are; gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Another interesting extension is to investigate whether the estimated effects of distance vary over the time span between an emergency hospital closure and the AMI. Over

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using the probability of in-hospital mortality as the outcome. See below for details.

time, potential compensatory strategies from both individuals and the health care administrations may arise in order to accommodate any perceived or real distance effects subsequent to the closures. For instance, patients with relatively poor health who experienced reduced access to emergency health care may decide to move closer to the new home hospital. Another possibility is that health care authorities may *ex post* invest more in emergency health care. Both these potential compensatory behaviors would then serve to diminish the distance effect on survival over time from the closure.

5.5 presents estimation results for AMI patients living in a region in year t where an emergency hospital closure occurred j years earlier, with $j = 1, ..., 5$ and where $j = 1$ has been the baseline case studied so far. The sample size is different as the first five years of the sampling period, i.e., 1987–1992, dropped from the analysis. These five years are excluded in all the specifications in the table in order to facilitate comparison of the results. The header of each column indicates the number of years from closure evaluated and the reported results are based solely on variation in distance for AMI patients who experienced a shift in distance to their home hospital for this particular number of years since hospital closure.

The results from the estimation are striking; there is only a clear effect of distance for the first year after a hospital closure. At each subsequent leading year, the effect of distance is diminishing in magnitude and mostly statistically insignificant. This pattern indicates that long-run effects of distance from the closures on AMI survival are unlikely to prevail, perhaps as a consequence of various compensatory strategies among the involved agents. Hence, besides from the initial shock, the hospital closures do not seem to have entailed long-lasting elevated AMI mortality rates.[35]

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[35]A back-of-the-envelope analysis might bring some further insights regarding the cost-benefit trade-off of the closures. In particular, a regression model was estimated of the survival measure including a dummy variable for being affected by a hospital closure on the right hand side (along with the other covariates), which subsequently was related to the average survival rates and AMI incidence in the relevant population. The effect of being affected by a hospital closure reduced the average survival probability with an estimated two percentage points, i.e., from 0.79 to 0.77 at mean survival rates. As the annual average number of AMIs was about 20,000 during the studied time period, this estimate suggests that about 320 extra deaths would have occurred had the closures affected the full AMI patient population. However, as the underlying population of the relevant catchment areas was only about ten percent of the total AMI population in a given year, the closures caused only an estimated 32 additional

Table 5.5: Estimated effects of distance on AMI survival probability from emergency room closures: Short and long-term effects

	Time Horizon (years from closure)				
Current Distance	One	Two	Three	Four	Five
11-20 km	0.716	-1.431	-2.469	-4.141**	2.983
	(1.983)	(1.944)	(1.862)	(1.821)	(1.873)
21-30 km	-4.732**	-3.912**	-2.512	-0.799	-3.422*
	(1.943)	(1.837)	(1.827)	(1.858)	(1.968)
31-40 km	-5.761**	-2.803	-1.909	-1.007	-0.029
	(2.240)	(2.128)	(2.130)	(2.106)	(2.022)
41-50 km	-8.075***	-4.447	-4.008	-4.008	-1.771
	(3.091)	(3.178)	(2.850)	(2.828)	(2.913)
51-60 km	-12.868***	1.125	-8.145*	9.992**	6.684
	(4.556)	(4.456)	(4.744)	(4.304)	(4.762)
Observations	374,316	363,307	351,825	339,320	325,387

NOTE.— The table reports point estimates (standard error) of the effect of distance on survival probability from an AMI as explained in the empirical section using the full analysis sample. The outcome is defined as an indicator function (multiplied by 100) for being alive when discharged from a hospital following the infarction. Each specification pertains to a specific time horizon from an emergency room closure (the number of lagged years). Reported coefficients in each column are a set of distance dummies for each ten kilometers. Included covariates are gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

To analyze whether reactions among the involved agents were responsible for the lack of long-term effects, it is necessary to collect additional data on affected individuals and the health care authorities. In particular, two plausible pathways which would agree with the empirical pattern of a diminishing effect over time are; *i)* individual migration from the catchment areas in which hospital were closed, and; *ii)* investments in ambulatory care by the health care authorities, such as, for example, the opening of ambulance stations in more rural parts of the affected regions. While, unfortunately, information on the time and the place of new ambulance stations have not been possible to procure, data on population size over time exists for all municipals in Sweden for the relevant years. This latter additional information is subsequently utilized in order to investigate whether individuals reacted to the hospital closures by moving out of the closure-affected areas.

5.3 plots the average percentage population change in closure-affected catchment areas by year from the hospital closure, adjusted for year and catchment-specific effects and

deaths, given no long-run effects. Hence, the total of 16 closures in the data meant an additional two deaths per closure. Assuming that the value of a statistical life is about €8m (see e.g., Viscusi, 1993; Viscusi and Aldy, 2003), the closures could thus be deemed "cost-effective" (with respect to AMI) if monetary savings exceeded €16m per closed hospital.

catchment-specific linear yearly trends. In other words, the figure compares the change in population from the base year (six years before the first hospital closure) of areas where hospital closures occurred, relative to unaffected catchment areas within the same region and year and accounting for different calendar year trends. Interestingly, the figure shows a trend break in the relative population growth at the time of the hospital closure between affected and unaffected areas, hence giving a rough indication that individuals might have reacted to the hospital closures by moving out from closure-affected areas.

Figure 5.3: Population growth in catchment areas with closed hospitals in relation to the closure
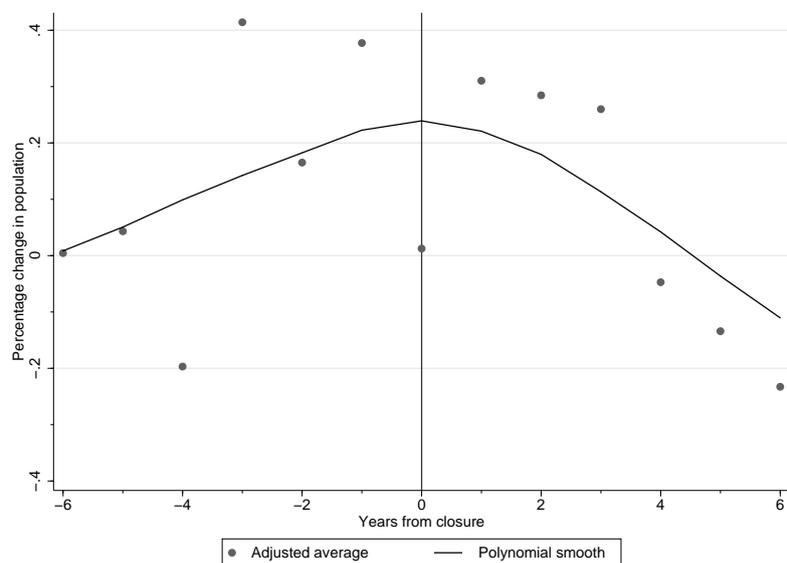
NOTE.— The figure depicts the average percentage change in population growth in catchment areas with closed hospitals in relation to the year of closure (year 0 on the x-axis), adjusted for catchment-specific effects, calendar year effects and a catchment-specific linear time trend. Population growth is measured relative to six years before the closure. The solid line is a fitted local polynomial trend of the adjusted averages.

5.3 Robustness checks

This section contains a number of robustness checks in order to examine the validity and sensitivity of the main results reported so far. First, alternative definitions of hospital distance are explored using supplementary information based on geocoding software from STATA® and Google® and reported in 5.6.[36] Specifically, the first column of the table

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[36]Specifically, the coordinates contained in the data are first converted from RT-90 2,5 gon V to WGS84 format using the CoordTrans® v2.3 software. The user-written STATA® program *traveltime3.ado* was

reports once again the baseline results from using the euclidean distance to the home hospital based on admission history. The second and third columns, in contrast, use the actual driving distance and driving time (in square brackets) to estimate the distance effect, respectively. The driving time categories are defined by the sextiles of the driving time distribution in the data, see A.10 in the Appendix. Finally, the last column reports the results from using the closest hospital, rather than the modal hospital, to define individuals' counter-factual hospital. It is clear that the choice of distance definition used do not change the interpretation of the main results to any important extent.

Table 5.6: Estimated effects of distance on AMI survival probability from emergency room closures: Alternative distance measures

| Current Distance [Time] | Distance Measure | | | |
| | Home Hospital | | | Closest Hospital |
	Euclidean distance	Driving distance	Driving time	Euclidean distance
11-20 km [3-8 min]	0.716	3.559	2.003	-1.800
	(1.983)	(2.342)	(2.221)	(2.005)
21-30 km [9-12 min]	-4.732**	-2.147	-2.968	-3.877*
	(1.943)	(2.152)	(2.222)	(2.087)
31-40 km [13-16 min]	-5.761**	-6.200**	-7.290***	-7.207***
	(2.240)	(2.483)	(2.468)	(2.775)
41-50 km [17-23 min]	-8.075***	-6.639**	-7.440**	-4.914
	(3.091)	(2.920)	(3.170)	(3.156)
51-60 km [24-50 min]	-12.868***	-8.660**	-17.034***	-14.825***
	(4.556)	(3.735)	(4.455)	(4.371)
Observations	374,316	364,359	369,573	373,762

NOTE.— The table reports point estimates (standard error) of the effect of distance on survival probability from an AMI as explained in the empirical section using the full analysis sample. The outcome is defined as an indicator function (multiplied by 100) for being alive when discharged from a hospital following the infarction. Each specification pertains to a specific hospital distance definition: From left to right in the table, euclidean distance to the model hospital based on municipal admission history, driving distance/driving time based on geocoding software from STATA® and Google® and euclidean distance to the closest hospital. Reported coefficients in each column are a set of distance dummies for each ten kilometers and driving time [square brackets]. Included covariates are; gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Even if the hospital closures were not mainly driven by competition or other market factors it is possible that the politicians in charge followed some agenda related to the general health of the population in catchment areas when they decided to close certain hospitals.[37] One way to analyze this potential issue is to construct a placebo test using

subsequently used to compute travel distance and travel time via the Google Distancematrix API V3.

[37]It is important to note that heterogeneity in hospital quality between closed and non-closed hospitals

an outcome related to general health status but where hospital distance should be of less importance. If estimated mortality is different for individuals who live further away from a hospital when using this outcome we would worry that the hospital closures were not unrelated to the health of the affected population. In practice, to keep the analysis as intact as possible, the sample is split into two different types of AMI: the ST Elevated Myocardial Infarction (STEMI) and the Non-ST Elevated Myocardial Infarction (NSTEMI) (see e.g., Graham *et al.* (2007) for detailed information about the differences between the two AMI types). In short, the former type of infarction is more severe, typically requires more aggressive treatment and a is condition where time is a much more crucial factor (see e.g., Bates and Menees, 2012). The latter type of AMI is less severe and usually treated pharmacologically, but clearly also signals poor health of the individual (see e.g., Amsterdam *et al.*, 2014).[38]

The first two columns in 5.7 report the results from estimating the split sample regressions by AMI type. As can be seen, the effect using only the STEMI outcome is even somewhat greater in magnitude than the main estimates from 5.1. More importantly, however, the effects for the NSTEMI outcome are mainly close to zero and do not indicate any effect of distance on survivability, hence supporting the conjecture that health differentials of individuals living in catchment areas where hospital were closed is not very important.

As already noted from 5.4, the estimated effect on survival is mostly driven by out-of-hospital mortality (OHM). However, for completeness, the same models have also been estimated using the probability of both in- and out-of-hospital mortality as separate outcomes, respectively, and are reported in the third and fourth column of 5.7. The reported results confirm the earlier conclusion that the effect of experiencing an increase in hospital distance is exclusively driven by an increased probability of death outside

is less of an issue here, since the empirical analysis compares individuals treated in the same hospital at the same point in time.

[38]In practice, the two different types of AMI are separated in the data by their ICD-10 codes; whereas the STEMI is referred to as a *transmural* infarction with an ICD-10 code of I21.1–I21.3, the NSTEMI is defined as a *subendocardial* infarction with ICD-10 code I21.4 (see e.g., Reznik, 2010).

of a hospital. Hence, given that a patient makes it to the hospital, staff and medical equipment seem to be adequate enough to keep patients alive.

The last four columns in 5.7 report results from various tests of the model and sample specifications. First, while most AMIs occur at home, many infarctions could also happen at the workplace where most working-age persons spend a non-negligible amount of time. In particular for commuters the actual distance to the hospital may hence be measured with substantial error if they work far from home. Even if a large proportion of the sample is already retired, information is nevertheless collected on the location of the still-working individuals' workplaces in order to define commuters as individuals who work in another municipal than in which they live. Re-estimating the model by excluding commuters does not make a difference for the interpretation of the results as is clearly seen in the fifth column of the table.[39] Furthermore, as only individuals whose admission origin were from home (rather than from another clinic or a long-term care home) has been included in the analysis so far, the model is re-estimated including all admission origins. This inclusion unsurprisingly attenuates the distance effect, given that the effect is mainly driven by OHM in the first place and such deaths are impossible when an individual is already admitted.

The last two columns concern the specification of the regression model. The second to last column in the table reports the results from estimating a non-linear, logistic, regression model including the same variables. The results remain robust to this model specification, but, as the linear estimates are more straightforward to interpret, the latter are kept throughout the article.[40] Finally, the last column reports the results from additionally including two covariates for which information for the whole sample were unavailable: taxable personal and household income. A regression model using only the reduced sample with non-missing income data was therefore also estimated. The main results remain robust also to this model adjustment.[41]

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[39]A dummy variable for being a commuter is also included in the set of controls used in the analysis.
[40]All analyses has been estimated and confirmed using the more robust non-linear specification and are available upon request from the author.
[41]Specifically, the income variables are computed as five year averages prior to the AMI occurred.

Table 5.7: Estimated effects of distance on AMI survival probability from emergency room closures: Robustness checks

| | Outcome | | | | Model and Sample | | | |
| | AMI Type | | Mortality | | Admission Origin | | Specification | |
Current Distance	STEMI	NSTEMI	IHM	OHM	Exclude Commuter	All Origins	Non-Linear	Extra Controls
11-20 km	1.591	1.335	-0.791	0.075	0.254	1.424	0.045	-0.835
	(2.590)	(1.153)	(1.040)	(1.840)	(2.157)	(1.852)	(0.110)	(2.038)
21-30 km	-5.461**	0.112	-0.129	4.861***	-5.653***	-2.910	-0.255**	-4.851**
	(2.486)	(1.144)	(1.019)	(1.803)	(2.062)	(1.805)	(0.103)	(2.003)
31-40 km	-6.724**	-0.081	-1.200	6.961***	-5.617**	-2.454	-0.299**	-4.992**
	(2.923)	(1.328)	(1.175)	(2.079)	(2.341)	(1.978)	(0.118)	(2.312)
41-50 km	-8.980**	-3.801**	2.372	5.703**	-9.239***	-4.797*	-0.423***	-6.026*
	(3.992)	(1.812)	(1.621)	(2.868)	(3.287)	(2.780)	(0.163)	(3.281)
51-60 km	-18.753***	0.833	1.425	11.443***	-13.339***	-3.201	-0.630***	-14.185***
	(5.524)	(3.145)	(2.389)	(4.227)	(4.765)	(3.698)	(0.230)	(4.779)
Observations	237,999	111,093	374,316	374,316	352,717	414,152	374,316	326,965

NOTE.— The table reports point estimates (standard error) of the effect of distance on survival probability from an AMI as explained in the empirical section using the full analysis sample. The outcome is defined as an indicator function (multiplied by 100) for being alive when discharged from a hospital following the infarction. The first four columns of the table pertains to a separate regression with different outcome definitions and the last four columns pertain to variations of the sample and models specification (see the text for details). Reported coefficients in each column are a set of distance dummies for each ten kilometers. Included covariates are; gender, age, educational level, marital status, occupational sector, an indicator for being a commuter, the number of previous hospitalizations (AMIs), the number of years since the last hospitalization (AMI) and indicators for the prevalence of eight different AMI risk factors based on individual medical histories. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

Since the variation used to identify the effects of distance in the analysis originates on the hospital level, the impact of a hospital closure has also been estimated using an aggregate difference-in-differences empirical design. To this end, the data is collapsed into year-catchment cells of average AMI mortality and the distance effect is estimated using the following model

$$\bar{y}_{ct} = \alpha + \sum_{k=0}^{K} I_{ct}^{k}\eta + X_{ct}\gamma + \lambda_c + \lambda_t + \varepsilon_{rt}, \qquad (5.1)$$

where $\bar{y}_{ct}$ is the share of AMI survivors in catchment area c and year t. Furthermore, I_{ct} is defined according to

$$I_{ct} = \begin{cases} 0 & \text{if } t \leq t' \\ q & \text{if } t > t' \end{cases},$$

where t' is the year of closure and $q = t - t'$ for some pre-specified follow-up period

K.[42] Furthermore, X_{ct} are catchment area characteristics and λ_c and λ_t are fixed effects for catchment area and year, respectively. Since different numbers of observations make up the estimated survival shares across years and catchment areas, each observation in the regression model is also weighted by the inverse of its variance. A major drawback of this model design is that few observations remain after aggregation: only just over 1,800. Despite lacking precision, the first column of 5.8 indicates that AMI survival shares decreased substantially in closure regions the first two years after closure. The estimates imply that average survival decreased by roughly two percent (1.6/74), which is remarkably similar to the results obtained from the micro analysis in 5.3. Furthermore, from the two right-most columns it is once again clear that this effect is driven by an increase in OHM which levels off after the first two post-closure years.

Table 5.8: Estimated effects of distance on AMI survival probability from emergency room closures: Aggregate Difference-in-Differences results

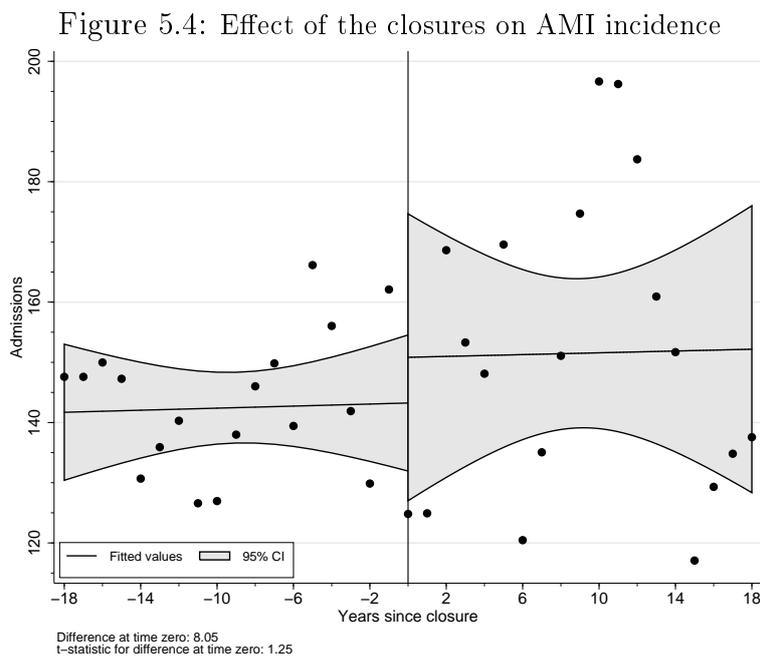| Years Since Closure	Outcome Measure		
	Survival	OHM	IHM
1 year	-1.559	1.934	-0.375
	(1.664)	(1.556)	(0.973)
2 years	-1.769	1.399	0.370
	(1.664)	(1.556)	(0.973)
3 years	0.068	0.452	-0.521
	(1.667)	(1.558)	(0.974)
4 years	1.500	-1.286	-0.214
	(1.668)	(1.559)	(0.975)
5 years	-0.673	0.793	-0.119
	(1.670)	(1.561)	(0.976)
Observations	1,828	1,828	1,828
Mean of outcome	74.52	19.37	6.11

NOTE.— The table reports point estimates (standard error) from estimation of model (5.1) using the full analysis sample aggregated into catchment area-calendar year cells. The outcome is defined as the cell-share (multiplied by 100) of AMI survivors. Each column pertains to a separate regression with a different outcome (total survival, out-of-hospital mortality and in-hospital mortality). Reported coefficients are the separate effect on survival for each year since hospital closure. Fixed effects include hospital and calendar year dummies. Standard errors are estimated using a robust covariance matrix. *, ** and *** denote significance at the 10, 5 and 1 percent levels.

A potential problem caused by restricting the sample to only include individuals who suffered an AMI is that the closures may have endogenously changed the population at risk of having an AMI. This could occur, for example, if admissions for other reasons

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[42]The difference-in-differences model may also be estimated with I_{ct} defined as a simple before-after dummy. However, since the micro-level analysis revealed that only short-run effects are prevailing, it makes more sense to estimate separate effects for each post-closure year.

than AMI changes a patient's general perception of his or her health risks and induces a more proactive behavior. In this respect, the closures may have affected the population at risk for an AMI through the reduced access to health care which, in turn, might have introduced a downward bias to the estimated distance effect.

To evaluate whether the closures affected the population at risk for an AMI we can study AMI incidence rates in closing hospitals' catchment areas over time. 5.4 shows the empirical relationship between AMI admission frequency in municipalities where a closure occurred in years from the time of the closures, after adjusting for calendar year trends in AMI incidence. The dots in the figure indicate yearly averages and the solid line plots the piece-wise linear relationship allowing for a discontinuity in the year of the closure (indicated by the vertical line). The figure reveals a small increase in AMI incidence after, compared to before, the hospital closures. However, the change is not significantly different from zero at any conventional levels of statistical significance.

Figure 5.4: Effect of the closures on AMI incidence

Difference at time zero: 8.05
t–statistic for difference at time zero: 1.25

NOTE.— The figure depicts the relationship between the average number of admissions in a closing hospital's catchment area over time since the closure occurred, adjusting for calendar time trends in AMI incidence. The dots represent average values for each particular time period and the solid line pertains to a piece-wise linear relationship allowing for discontinuity at the time of closure, indicated by the vertical line. The shaded area marks the 95% confidence interval of the linear estimate.

Finally, as previously noted, one potential concern of the empirical design is that

the closed hospitals were selectively shut down with respect to the underlying survival probability of the population case-mix in the respective catchment area. This should be less of a concern in the present setting due to the public nature of health care provision in Sweden. Furthermore, the placebo test using different AMI types did not indicate any health sorting across distance in the closure-affected areas. Nevertheless, this concern is further investigated by comparing aggregate health characteristics in closing and referral hospital's catchments areas. 5.5 shows the average values of a number of aggregate health characteristics for closing and referral hospital catchment areas (left panel) and their difference along with a 95–percent confidence band (right panel) for years prior to the hospital closures. The results are reassuring; both types of regions have, on average, similar health characteristics, indicating that the hospital closures were observationally unrelated to underlying patient population health characteristics.[43]

Figure 5.5: Aggregate health indicators in closing and referral hospital catchment areas
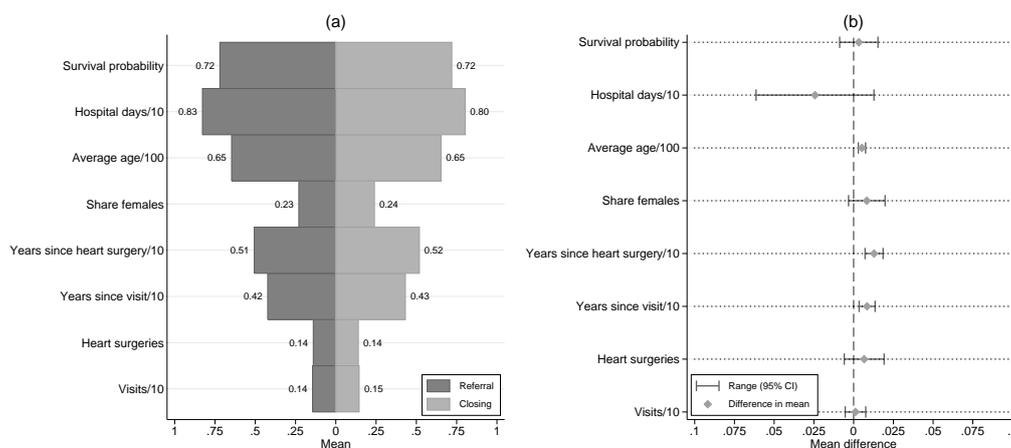
Note.— The left panel depicts averages for a set of aggregate health indicators for each catchment type (closing, referral) and the right panel shows the cross-regional mean difference for each of these indicators (point estimate and 95 percent confidence band). Some variables are scaled to make the plot readable.

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[43]In unreported regressions where aggregate catchment area characteristics were used to predict the hospital closures entailed qualitatively similar results as shown in 5.5.

6   Summary and concluding remarks

Ischemic heart disease, with acute myocardial infarction (AMI) as one of its more serious manifestations, is the most common cause of death in Sweden as well as in most of the Western world. Since infarctions often occur relatively unexpectedly and rapid medical assistance is fundamental for recovery, the probability of surviving an AMI is highly dependent on a well-functioning health care system which can provide quick access to health care in emergency situations. This is particularly important in relatively sparsely populated countries like Sweden, where distances to medical care facilities with emergency room capacities vary greatly between individual residents.

This study evaluates the existence and magnitude of the impact of geographical access to health care on health using administrative data on AMI patients. Both the problem of missing mortality data and the likely residential sorting of individuals are circumvented by: *i)* adding nationwide information on AMI deaths from the Swedish Cause of Death Registry to supplement the National Inpatient Registry, and *ii)* utilizing geographical variation in distance to hospital arising from a number of emergency hospital closures during a period of strong concentration of the publicly administered Swedish health care sector. In Sweden, virtually all inpatient health care is publicly provided and financed, implying that competition effects on the number and location of hospitals in the country should be negligible. Moreover, as individuals are directed to a specific hospital based on their place of residence, variation over time with respect to which hospital patients are directed to can be used to obtain plausibly exogenous shifts in individual distances to hospital. As the full AMI population over a twenty-year period is included in the analysis, i.e., both admitted patients and patients who died before reaching a hospital, the empirical design accounts for both of the presumably most serious identification problems in evaluating the health effects of geographical access to health care.

Using data on more than 300,000 AMI cases and sixteen emergency hospital closures over the period 1990–2010, a monotonously decreasing, economically and statistically

significant, effect of emergency hospital distance on AMI survival probability is found. In particular, patients who experienced an increase in the distance to their home hospital of between 51 and 60 kilometers ran an estimated 15 percent lower risk of surviving the infarction than patients who lived within ten kilometers of their home hospital in both periods. This effect is, as expected, primarily driven by an increased risk of out-of-hospital mortality. Moreover, much smaller effects are found when estimating the effects of distance based on actual distances to hospital, indicating that selective residential sorting is likely to dilute the distance effect. When varying the time window between the closures and AMI occurrence, the effect is shown to be only statistically significant in the first year after the closure. Hence, the closures only seemed to have had a short-run effect, which might later have been counteracted by various types of compensatory behavior among the involved agents. Furthermore, as a number of patients experienced a cut in hospital distance due to the closures, the symmetry of the distance effect is evaluated. The estimated effect is indeed also reversed for patients who experienced a decrease in distance and symmetry cannot be rejected. Finally, the results from a number of robustness checks indicate that inferences are not driven by selective hospital closures, sample selection or model specification.

To conclude, in times when health care expenditure increased in most Western countries, Sweden went in the opposite direction and reduced its health care spending by approximately 11 percent between 1990 and 2000. Most of the cost savings were derived from structural changes in the health care sector: from inpatient to outpatient care and from increased resource consolidation of many care services. These tendencies were perhaps necessary given the public sector budget deficits, a consequence of the economic recession in Sweden at the time, but the question remains whether the reduction in health care expenditure came at the cost of a decrease in access to health care among individuals living in more remote parts of the country. The results in this paper provide some evidence for the notion that geographical access to health care does have an impact, albeit only temporarily, on the survival rates of AMI patients, and hence that concentration of

health care resources may have important side effects that should be taken into account. Perhaps more importantly, this effect of distance to hospital may be more persistent in other countries with more unregulated health care sectors due to the strategical positioning of profit-maximizing hospitals. Specifically, hospitals in these markets may abandon geographical areas in which aggregate incidence rates of costly emergency health care is higher, thus creating a "health care desert" similar to the phenomenon of food deserts recognized in many countries.

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Appendix A Tables and figures

Figure A.1: Visits at closing hospitals and their referral hospitals over time
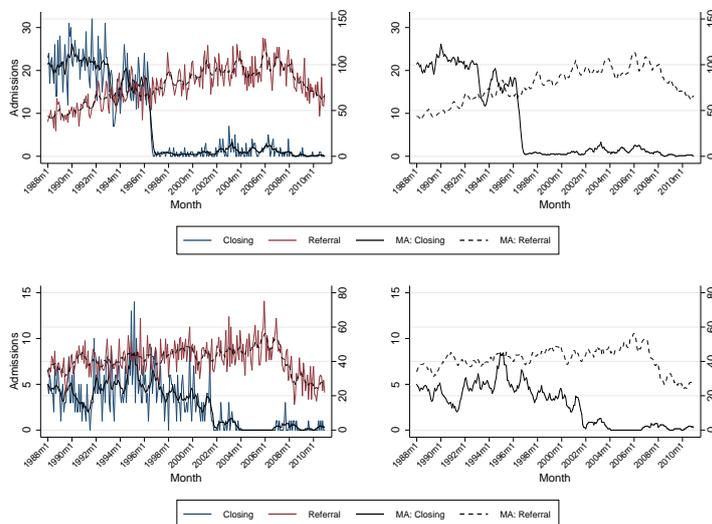
NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

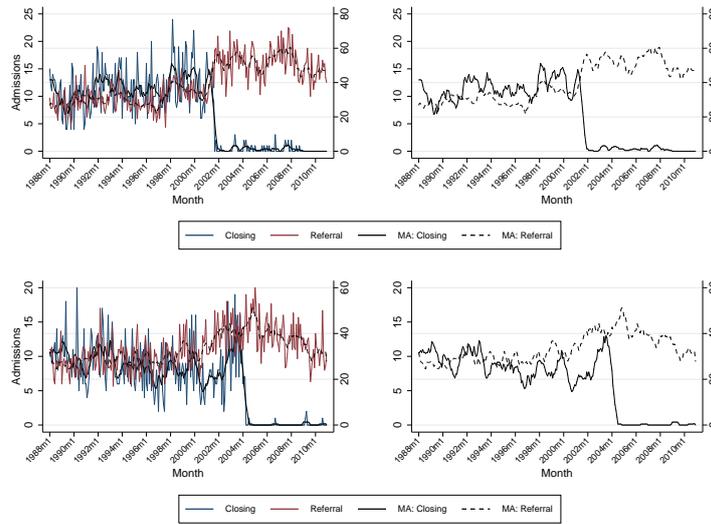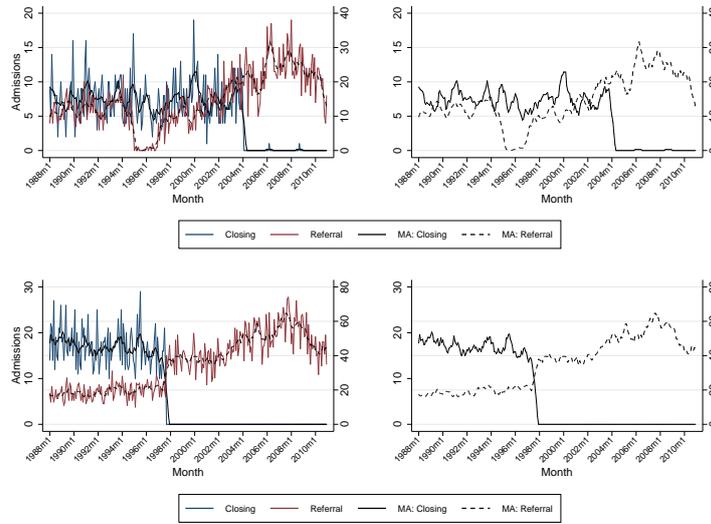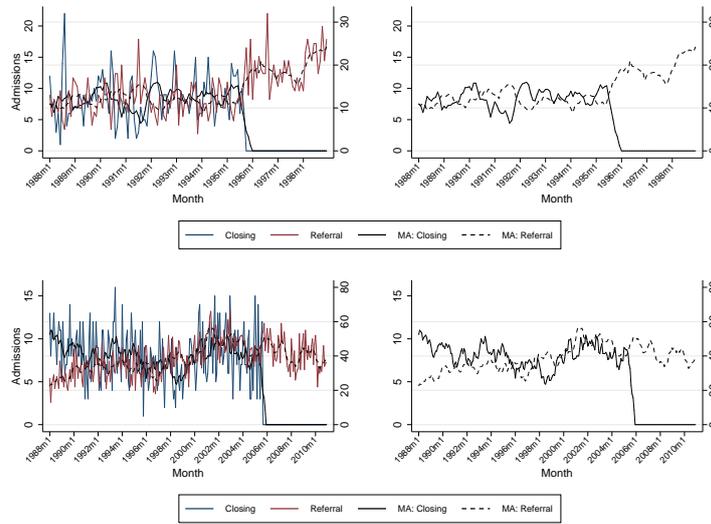Figure A.2: Visits at closing hospitals and their referral hospitals over time
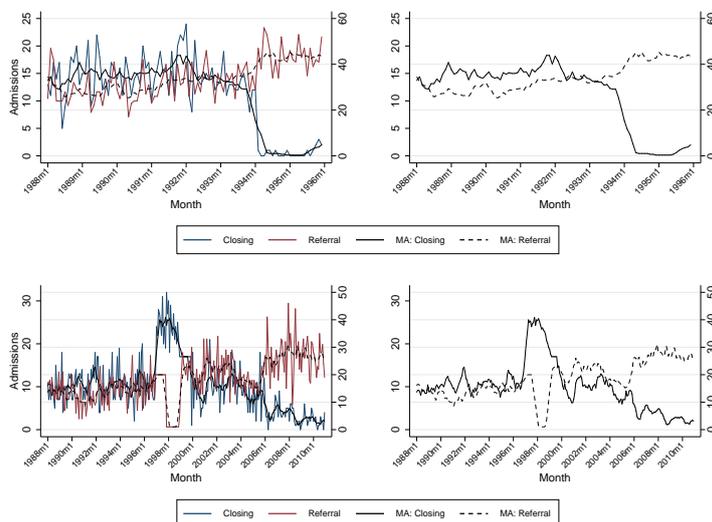
NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

Figure A.3: Visits at closing hospitals and their referral hospitals over time
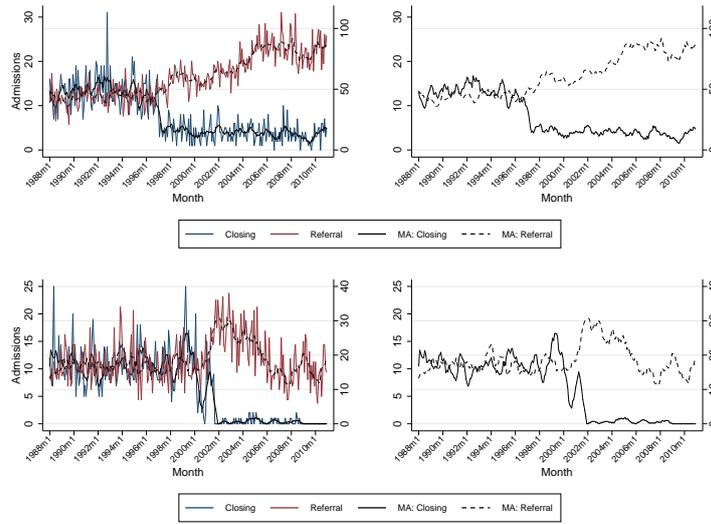

NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

Figure A.4: Visits at closing hospitals and their referral hospitals over time

NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

Figure A.5: Visits at closing hospitals and their referral hospitals over time


NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

Figure A.6: Visits at closing hospitals and their referral hospitals over time

NOTE.— The left panels depict the monthly number of AMI visits at closing hospitals (blue lines and left y-axis) and referral hospitals (red lines and right y-axis). The right plots show corresponding six-month moving averages.

Figure A.7: Survival probability by home hospital
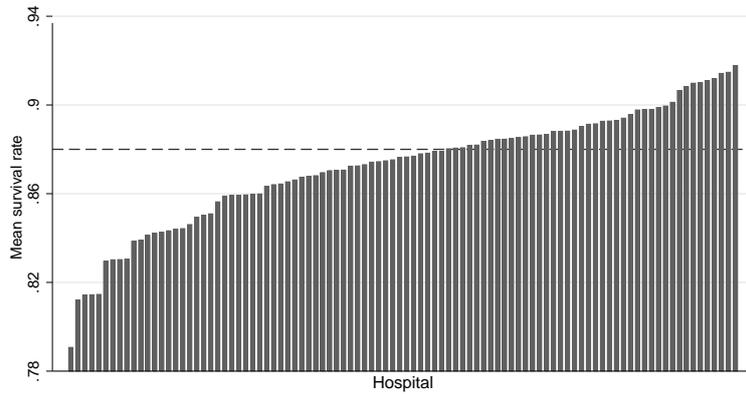
Figure A.8: AMI frequency and average survival rates, 1990–2010
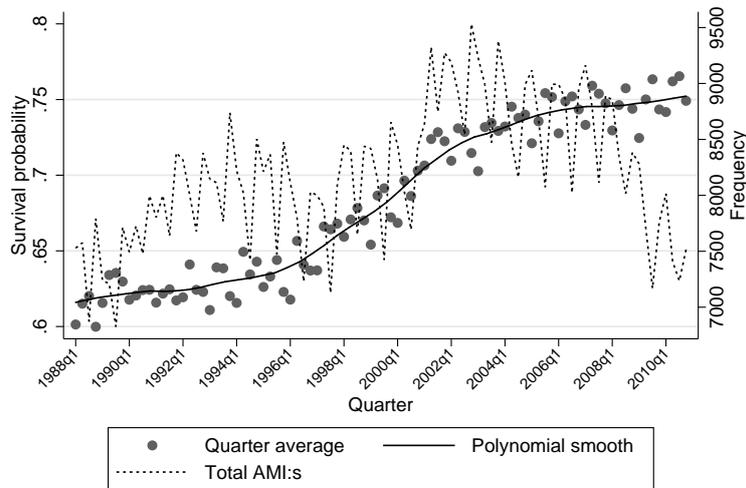
Figure A.9: AMI mortality distribution by days after infarc-
tion
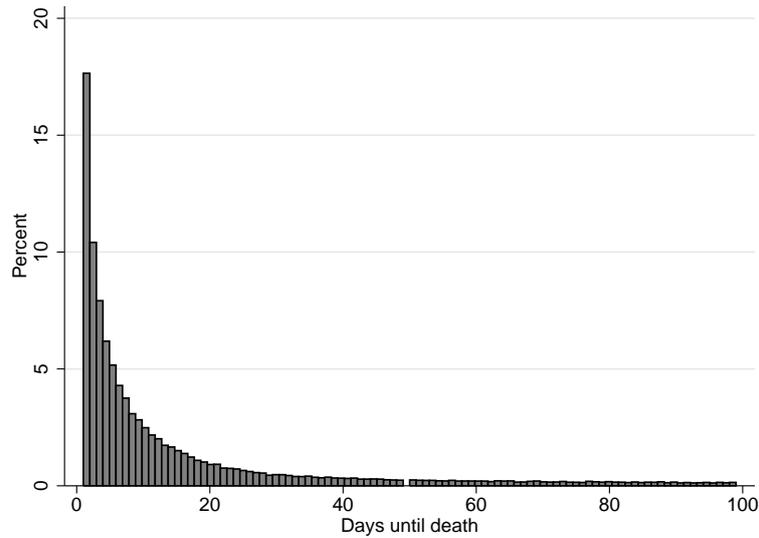
NOTE.— The figure depicts the distribution of AMI deaths in the analysis sample
by days from the AMI. The number of cases expiring on the same day as the
AMI (excluded in the figure) are approximately 191,000 (58 percent).

Figure A.10: Distribution of driving time and changes in driving time to
home hospital
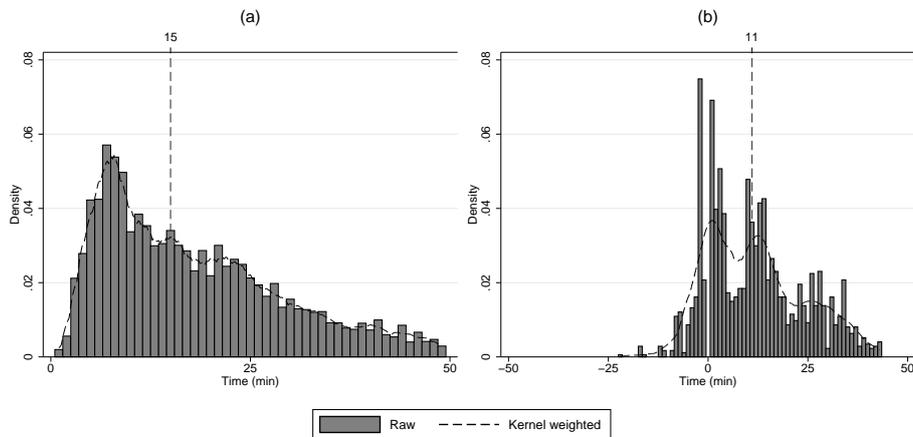
NOTE.— The left panel depicts the home hospital driving time distribution for the analysis sample.
The right panel shows the distribution of changes in driving time generated by the emergency
hospital closures. Driving times are obtained by using geocoding software from STATA® and
Google®. The dashed vertical lines indicate the median of the distributions and the dashed
smoothed lines are kernel density estimates.

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