Does Uninsurance Affect the Health Outcomes of the Insured? Evidence from Heart Attack Patients in California*

N. Meltem Daysal†

Department of Economics
University of Maryland, College Park

Job Market Paper

This Version: November 23, 2008

Abstract

One of the major challenges confronting policy makers today is the persistently high rate of U.S. residents who do not have health insurance. Any health care reform that aims to tackle this issue requires a solid understanding of the effects of uninsurance on society. In this paper, I examine the impact of uninsured patients on the health outcomes of the insured. I focus on one measure of health outcome, the in-hospital mortality rate of insured heart attack patients, and implement panel data models using patient discharge data from California hospitals for the period 1999–2006. Overall, my results indicate that uninsured patients have an economically significant effect on the health outcomes of insured heart attack patients. I show that these results are not driven by unobserved characteristics of insured heart attack patients or hospitals and that they are robust to a host of specification checks. My results indicate that eliminating uninsurance would reduce the annual number of insured heart attack deaths by 125–200, roughly corresponding to a 3–5% reduction in the total number of deaths. My calculations place the marginal cost of a statistical life year saved from reducing uninsurance between $38,093 and $63,569, implying that reducing uninsurance may be a cost effective way of improving the health outcomes of heart attack patients.

JEL Classifications: I11, I12, I18.

*I would like to thank the Department of Economics at the University of Maryland and the Maryland Population Research Center for financial support in obtaining the data. I am particularly indebted to my advisors Mark Duggan, Darrell Gaskin and especially Judith Hellerstein for their guidance and support throughout this project. I am also grateful for useful comments provided by John Ham, Jeanne Lafortune, Mircea Trandafir and seminar participants at the University of Maryland, the Johns Hopkins School of Public Health and the Southern Economic Association Meetings. I thank the California Office of Statewide Health Planning and Development for making available the data and for attending my queries while performing this study. All remaining errors are my own.

†Contact Information: 3105 Tydings Hall, Department of Economics, University of Maryland, College Park, MD 20742. Email: daysal@econ.umd.edu

1 Introduction

The United States is the only developed country that does not feature universal health insurance coverage for its citizens. Although politicians at different ends of the spectrum advocate different types of policies to reform the health care system, they all agree that uninsurance is one of the most important challenges policy makers face today. With 17 percent of the nonelderly population or 45 million individuals lacking health coverage in 2007 (Frostin, 2007), the problem of the uninsured is likely to keep its place at the center stage of policy debates in the coming years. Any health care reform that aims to tackle this issue of persistently high uninsurance requires a solid understanding of its effects on society.

Most research to date has focused on those without health insurance coverage and examined how lack of insurance affects their own health outcomes, health care access and utilization.[1] The overall effect of uninsurance, however, also depends on how the uninsured affect the provision of care to the insured people in their communities.

There are various mechanisms through which the uninsured may affect the care provided to those with insurance. Uninsured people tend to come from low-income families with nearly two thirds of them living in families with incomes less than twice the poverty line (Gruber, 2008) and they have lower health care utilization rates (Institute of Medicine, 2002). If the uninsured reduce the overall demand for hospital services, hospitals may lower their capacity, thereby affecting the availability of services for the whole community. Similarly, if the uninsured demand lower quality services to reduce their out-of-pocket payments, the overall quality of care may be reduced.

Despite their relatively low utilization rates, the uninsured have considerable health care costs because they tend to forgo care until it becomes an emergency (Institute of Medicine, 2003). A significant portion of these costs are born by third parties in the form of uncompensated care. In 2005, such costs were close to \$30 billion (Gruber and Rodriguez, 2007), accounting for nearly two percent of the total health care spending in the US.[2] Furthermore, the burden of uncompensated care is not evenly distributed across health care providers. Cunningham and Tu (1997) and Mann et al. (1997) report that urban public hospitals bear a disproportionate share of these costs.[3] In an era of increased managed care and price competition where cost shifting is no longer an option for health care providers (Morrisey, 1996), hospitals may turn to alternative strategies to recover the uncompensated care costs of the uninsured such as reducing the provision of certain types of

[1] For a review of the literature, see Institute of Medicine (2002).

[2] In 2005, total health care spending in the US was \$1,987.7 billion (Centers for Medicare and Medicaid Services).

[3] In the case of physicians, Gruber and Rodriguez (2007) find that physicians, on the net, do not provide any uncompensated care.

services or reducing the quality of care.

There is a small but growing literature on the community effects of uninsurance.[4] Virtually all of this literature has focused on how the uninsured affect insured individuals' access to health care. An equally important but unexplored question, however, is how uninsurance affects the health outcomes of the insured. The aim of this paper is to increase our understanding of the community effects of uninsurance by being the first to investigate its impact on insured health outcomes. In particular, I examine the effects of the uninsured on the health outcomes of insured heart attack patients using patient discharge data from California hospitals (1999–2006) merged with spatial geocode information.

California is an appropriate setting to study this question for several reasons. It is the most populous state in the US, accounting for 12 percent of the total population in 2000. It is also a high uninsurance state: between 2004 and 2006, over 20 percent of its residents were uninsured compared to a national uninsurance rate of 17 percent. Because of its size, it has the largest number of uninsured, close to 7 million during 2004–2006.[5] In addition, it is a state where uninsurance was identified as a major policy issue. In recent years, policy makers in California made deliberate attempts to reform the health care system and bring the state to universal coverage. Finally, the patient discharge dataset collected by the Office of Statewide Health Planning and Development is one of the richest US health care datasets available to researchers: in addition to basic demographic information, it provides very detailed clinical information on every discharge from non-federal hospitals.

I focus my attention on heart attack patients for a number of reasons. First of all, it is an important health outcome, representing the leading cause of death for both men and women in the US. It is also a health event for which the cost of treatment has increased substantially over time. Second, it is a health condition for which evidence suggests that high mortality may be associated with insufficient quality of care. Finally and above all, focusing on heart attack patients ameliorates the potential endogeneity bias arising from patients' selecting into hospitals based on health status or other unobservable characteristics.

In the empirical analysis, I employ panel data models to address the potentially endogenous choice and location of hospitals. My baseline empirical specification estimates the impact of the fraction of uninsured patients in a hospital's health care market on the in-hospital mortality rate of insured heart attack patients, controlling for hospital and year effects. My preferred measure of uninsurance is calculated at the health care market level because the choice of a health care market

[4]See Institute of Medicine (2003); Pagán and Pauly (2006); Pauly and Pagán (2007); Pagán et al. (2007).

[5]All the statistics are provided by California Healthcare Foundation (2007).

is presumably exogenous, compared to the choice of a hospital (Doyle, 2008).[6] I define health care markets in several ways to check the sensitivity of the results. Under the geopolitical boundaries approach, the market is defined as the county in which the hospital is located. Under the distance based approach, the market is defined as a fixed mile radius area around each hospital.

Overall, my results indicate that uninsured patients have statistically and economically significant effects on the health outcomes of insured heart attack patients. I find that eliminating uninsurance would reduce the number of insured heart attack deaths, in the average year from 1999 to 2006, by 125–200 depending on the market definition. These numbers roughly correspond to a 3–5% reduction in the total number of insured AMI deaths in the average year. My back-of-the-envelope estimates place the marginal cost of a statistical life year saved from reducing uninsurance between \$38,093 and \$63,569.

The use of fixed effects methods alleviates concerns about time invariant factors biasing my estimates, but it does not eliminate the possibility of a bias arising from time varying unobservable characteristics of hospitals and insured heart attack patients. While I cannot eliminate all sources of endogeneity, I provide evidence suggesting that the results are unlikely to be biased under a broad set of scenarios where the violation of the fixed effects assumptions might be of concern.

First, I examine whether temporarily low quality hospitals become magnets to uninsured patients over time using the method developed by Duggan (2000). Although there is some evidence of reallocation of uninsured patients across health care markets, the evolution of the market uninsurance rates of hospitals classified as low and medium quality in the baseline period is not statistically different than that of high quality ones. If anything, the evidence suggests that the uninsured leave low quality hospitals for high quality ones.

Second, I analyze whether time-varying unobserved characteristics of insured heart attack patients could be biasing the results. I check whether insured patients reallocate across hospitals over the sample period as we may be concerned about healthy insured patients leaving high uninsurance areas. I do not find evidence of such reallocation for either insured heart attack patients or insured patients in general. Next, I examine whether the observed health status of insured heart attack patients in hospitals located in high and medium uninsurance areas in the base year deteriorates relative to hospitals located in low uninsurance areas, and I do not find any evidence in support of such deterioration. Finally, I estimate a sorting model *à la* Murphy and Topel (1990) to check whether patients who are more likely to die in the event of a heart attack sort into high uninsurance markets and I do not find evidence for that.

Third, I examine whether the results may be driven by local shocks that affect both heart attack

[6] However, I also provide results using the fraction of uninsured patients in the hospital.

survival risk and the market uninsurance rate. The results are robust to adding county specific time trends to the baseline empirical specification. A specific example of a local shock is the addition of sicker but still insured individuals to the market, due to events such as the development of low income housing around a high quality hospital. To address this concern, I check how the observable health characteristics of insured heart attack patients change over the sample period separately for hospitals in different terciles of the distribution of heart attack mortality in the base year. The results do not suggest any differences between hospitals located at different points in the distribution of heart attack mortality.

Fourth, I analyze whether the results may be biased due to sample selection. If hospitals located in low uninsurance areas discharge patients faster over the period of analysis, my results will be upward biased. I check the evolution of average length of stay in hospitals located in high and medium uninsurance areas in 1999 relative to hospitals in low uninsurance areas in 1999 and the results do not show a systematic correlation with market uninsurance.

Finally, using the informal framework suggested by Altonji et al. (2005), I estimate the extent of selection based on unobservables relative to selection based on observables needed to wipe out the effects of uninsurance. Although the estimated ratios are relatively small in absolute value, the estimated bias is negative for spatially narrow market definitions indicating that the fixed effect results are, if anything, downward biased.

I supplement these analyses with further robustness checks. While none of these tests are individually sufficient to claim that the main results are not driven by the unobserved characteristics of hospitals or insured heart attack patients in high uninsurance markets, taken together they provide compelling evidence that such a bias is unlikely.

The remainder of the paper is organized as follows. Section 2 presents a background on the financing and delivery of care to uninsured people in California and reviews the previous literature on the community effects of uninsurance. Section 3 outlines the empirical framework, while section 4 introduces the data and provides descriptive statistics. The results are presented in section 5 along with a discussion on robustness checks and heterogeneous effects. Section 6 provides simple calculations of the marginal cost of a statistical life-year saved and discusses cost effectiveness. Section 7 provides directions for future research and concludes.

2 Background

2.1 Institutional Background

According to the California Welfare and Institutions Code (1933), counties have a statutory obligation to provide care to the medically indigent.[7] The current organization of the county medically indigent programs emerged in the early 1980s when the state legislature eliminated medically indigent individuals from California's Medicaid program, Medi-Cal, and transferred responsibility for them to counties.[8] Since then the 24 largest counties manage their own distinct Medically Indigent Service Programs (MISP) and the 34 smaller rural counties participate in the centralized County Medical Services Program (CMSP) (see Table 1). Although there is a lot of variation in the funding levels, eligibility requirements and benefit levels of the MISP (see Table 2), these programs can be broadly classified into three categories: provider counties that have their own hospitals and clinics; payer counties that contract with private hospitals and clinics; and hybrid counties that contract with private hospitals for inpatient care but operate their own clinics. CMSP, on the other hand, is administered centrally by an independent governing board composed of representatives from the member counties.[9] Like Medi-Cal, it is organized as a traditional fee-for-service program but has fewer benefits.

Despite their historical role as provider of last resort of health care services, medically indigent programs cover only a third of the uninsured population in California (Insure the Uninsured Project, 1998). During the period 1999–2006, county indigent patients also represent around 30 percent of the uninsured discharges in California. The lack of reliable data on the funding and expenditures[10] of the county indigent programs makes it difficult to analyze how they fare relative to the cost of the care provided to these individuals. However, health care providers frequently ar-

[7] "Every county and every city and county shall relieve and support all incompetent, poor, indigent persons, and those incapacitated by age, disease, or accident, lawfully resident therein, when such persons are not supported and relieved by their relatives or friends, by their own means, or by state hospitals or other state or private institutions." (California Welfare and Institutions Code, Division 9, Part 5, Chapter 1, Section 17000)

[8] For more detail on the institutional background, see Kelch (2004, 2005); Insure the Uninsured Project (2007).

[9] When it was founded in 1983, CMSP was under the administration of the State Department of Health. It was transferred to the Governing Board in 1995.

[10] Counties receive funds from two major sources to finance the medically indigent programs: (1) state health and welfare realignment program funds which are composed of dedicated sales and motor vehicle license fees (2) county general revenues, the majority of which comes from property taxes. In addition to these two major sources, counties are eligible to receive funds from the Tobacco Tax and Health Protection Act (Proposition 99). However, in return for receiving these additional funds, they are required to submit data on their indigent care programs and provide follow-up treatment as indicated by the Child Health and Disability Prevention Program. Most of the available data on urban indigent program claims and costs come from the Medically Indigent Care Reporting System (MIRS) as all urban counties chose to participate in Proposition 99 funds. Data on CMSP counties, on the other hand, are available from the CMSP Governing Board as only a few of these counties receive Proposition 99 funds.

gue that reimbursement for indigent care is far less than its cost. During the 2002–2003 fiscal year, MISP counties reported reimbursing hospitals around $703 million for 70,040 inpatient discharges.[11] During the 2003 calendar year, urban California hospitals which reported financial data had 63,366 county indigent discharges with an estimated cost of over $661 million.[12] Similarly, according to the CMSP governing board, the rural indigent programs spent close to $117 million on 11,907 inpatient discharges during the 2004–2005 fiscal year.[13] Rural California hospitals with available financial data, on the other hand, reported providing care to 6,443 county indigent discharges in 2005 with a total cost of over $74 million. Simple calculations for both urban and rural areas suggest that the cost of indigent patients outweighs reimbursements provided by the county indigent programs.

The responsibility of the remaining two thirds of the uninsured mainly falls on the hospitals. The Emergency Medical Treatment and Active Labor Act (EMTALA, 1986) requires hospitals participating in Medicare to provide medical screening to all patients seeking emergency care. If a patient is determined to have an emergency condition,[14] the hospital is then expected to stabilize the person, regardless of income or insurance status. Some hospitals have additional mandates to provide care to uninsured patients. These include Hill-Burton Act (1946) participants that received loans and grants for construction projects in return for their agreement to provide care to those who are unable to pay and not-for-profit hospitals that must provide community benefits in exchange for their tax-exempt status.

The costs of care to these individuals are born mutually by the patients as out-of-pocket payments and the hospitals as uncompensated care. Hospitals receive partial reimbursement for their uncompensated care costs through various federal and state supplemental payments. In California, the majority of the funds come from the Medicaid Disproportionate Share Hospital Payment Program (SB 855). A program jointly administered by the federal and state governments, SB 855 aims

[11] MIRS data available at http://www.dhs.ca.gov/ochs/micrs/county_data.htm, accessed on March 2, 2008.

[12] California inpatient discharge data provides the total charges corresponding to every discharge. I multiply these charges with the hospital's cost-to-charge ratio using data from the California Annual Hospital Financial Disclosures. Some hospitals do not report financial information. Hence, the calculated figure reflects only those hospitals that disclosed financial data. The total number of urban county indigent discharges in the inpatient data during 2003 was 65,044.

[13] Available at http://www.cmspcounties.org/data/county_specific.html, accessed on March 2, 2008.

[14] EMTALA describes an emergency medical condition as "(A) one that manifests itself by acute symptoms of sufficient severity (including severe pain) such that the absence of immediate medical attention could reasonably be expected to result in (1) placing the health of the individual (or, with respect to a pregnant woman, the health of the woman or her unborn child) in serious jeopardy, (2) serious impairment to bodily functions, or (3) serious dysfunction of any bodily organ or part; (B) with respect to a pregnant woman who is having contractions (1) that there is inadequate time to effect a safe transfer to another hospital before delivery, or (2) that transfer may pose a threat to the health or safety of the woman or the unborn child." (United States Code, Title 42, Chapter 7, Subchapter 18, Part E, Section 1395)

to help hospitals that serve a disproportionately high number of Medi-Cal and uninsured patients.[15] Public entities are required to transfer funds to the state, which are then matched by the federal government at California's federal Medicaid matching rate. The state keeps part of the combined fund as an administrative fee and redistributes the remaining part as supplemental payments to both private and public hospitals based on the indigent care they provided in the previous year. However, because SB 855 has a structure that makes Medicaid patients financially more attractive than the uninsured[16], some believe that the "DSH mechanism places the need for appropriate and efficient care to Medi-Cal recipients at odds with the desire to supplement the cost of care for the indigent" (Huen, 1999, p. 8) and that only a small fraction of these supplemental payments are actually used to cover the uncompensated care costs to uninsured patients.[17]

Hospitals and their ability to serve the uninsured are also affected by legislative and economic changes that are not directly related to the provision of care to the uninsured. For example, reductions in Medicaid and Medicare payments passed by the Balanced Budget Act (1997), the mandates relating to nurse-to-patient ratios (AB 394) and hospital seismic regulations (SB 1953) are all believed to contribute to the existing financial difficulties of California hospitals, making provision of uncompensated care even more difficult (Price Waterhouse Coopers, 2007).

2.2 Previous Literature

This paper is at the intersection of several strands of economic literature. It is related to a line of research that links economic incentives and hospital behavior. It is also linked with, and in part motivated by, studies on cost-shifting. Finally, and most relevantly, it adds to a small but growing literature on how market level uninsurance rate affects the availability and quality of health care services.

There is a substantial body of literature that examines how hospitals respond to changes in their economic environment. The findings of this literature suggest that hospitals act strategically to increase their profits or recover their costs, and that hospital strategic response takes on many forms including changes to patient composition (e.g. (Duggan, 2000)), staff levels (e.g. (Lindrooth et al., 2006)), provision of uncompensated care (e.g. (Gruber, 1994; Currie and Fahr, 2004; Bazzoli

[15] A hospital qualifies for SB 855 if (1) the number of its Medicaid inpatient days is at least one standard deviation above the statewide mean, or (2) its revenues from low-income utilization exceed 25 percent of its total revenues.

[16] Qualified hospitals receive a per diem rate for all of their *Medi-Cal* patient days. Although uninsured patients can affect the per diem rate the hospital receives, they do not contribute to the number of days used in the calculation of the SB 855 reimbursement.

[17] Using data on the Medicaid Disproportionate Share program, Baicker and Staiger (2005) also found that some states divert these funds and public hospitals located in these states do not receive any DSH payments.

et al., 2006)) and classification of diagnosis codes (e.g. (Dafny, 2005)). Hospitals could respond in similar ways to recover the uncompensated care costs generated by uninsured patients.

One particular mechanism through which hospitals could try to recover these costs is by shifting them to insured patients.[18] Although the findings of the cost-shifting literature for the early 1980s are mixed, more recent evidence from the mid 1990's suggest that cost-shifting is no longer an issue: "[c]ost shifting is dead, apparently killed off by price competition, sensitive employers, aggressive insurers, and excess capacity in the hospital industry." (Morrisey, 1996, pp. 3-4) This result motivates examining the effects of the uninsured on other margins such as the availability and quality of health care services.

To the best of my knowledge, the first study to attempt to examine the effects of community uninsurance is Institute of Medicine (2003). In addition to qualitative discussions on a broad range of health care issues, this project uses GLS strategies to bring some quantitative evidence on the effects of uninsured populations in urban (Gaskin and Needleman, 2003) and rural areas (Needleman and Gaskin, 2003) on hospital finances and services.[19] In both cases, the authors merge information from American Hospital Association annual surveys and Medicare Cost Reports to measure outcomes but they use different data sources to measure the uninsurance rate and they have slightly different analysis periods. Gaskin and Needleman (2003) focus on the 85 largest SMSAs during a 4 non-consecutive year period that spans the 1990s. Using March CPS to measure uninsurance, they find that SMSA level uninsurance is negatively related to the availability of most hospital services but it has no relation to hospital margins. Needleman and Gaskin (2003) concentrate on the experiences of 168 rural counties in seven states during 3 years from the early 1990s. They use hospital discharge data to calculate the uninsurance rate and conclude that county level uninsurance reduces hospital margins but has no effect on the provision of hospital services.

More recently, a series of studies employ the household and physician surveys from the Community Tracking Study to examine the effects of community uninsurance rates on self-reported health care access and quality measures (Pagán and Pauly, 2006; Pauly and Pagán, 2007; Pagán et al., 2007). Using multilevel logistic regression and data from a single year, they find that insured people residing in high uninsurance areas are more likely to have unmet medical needs and less likely to

[18]For a review of the cost-shifting literature, see Clement (1997-1998); Morrisey (1993) and Morrisey (1996).

[19]Financial status: hospital margins. Capacity: total beds, medical/surgical beds, psychiatric beds, ICU beds, beds for patients diagnosed with alcoholism, drug abuse and chemical dependency. Services to vulnerable populations: psychiatric outpatient and ER services, outpatient and rehab services for patients diagnosed with alcoholism, drug abuse and chemical dependency, services for HIV patients. Community services: community outreach centers, transportation services, meals on wheels. High-tech services that require bed, equipment and personnel: trauma center, neonatal ICU, transplant services, burn units. High-tech services that only require equipment and personnel: MRI, radiation therapy, angioplasty, SPECT, ESWL.

be satisfied with the quality of health care than those residing in low uninsurance areas. Similarly, they find a negative relationship between community uninsurance and the ability of primary care physicians to get referrals for their patients.

3 Empirical Strategy

A social experiment aimed at estimating the causal effect of uninsurance on insured health outcomes would randomly assign uninsured patients to hospitals. However, in reality patients often choose which hospital to go to. Finding an exogenous source of variation that affects the uninsurance rate but not the health outcomes of insured patients is also difficult. Macroeconomic conditions that affect uninsurance rates likely have direct effects on the health outcomes of the overall population, including insured individuals. Similarly, characteristics of hospitals located in high uninsurance areas, such as list prices and managerial policies are likely to be related to the quality of care.

In order to account for potential endogeneity arising from choice and location of hospitals, I identify the effects of uninsurance on insured patients using panel data models. The key equation of interest can be described as:

$$Y_{jt} = \alpha + \beta UNINS_{jt} + \delta X_{jt} + c_j + \nu_t + \epsilon_{jt} \tag{1}$$

where the unit of observation is hospital j in year t. Y is the health outcome of interest aggregated over the sample of insured patients in the hospital, $UNINS$ is a measure of uninsurance, X includes demographic and clinical characteristics of the insured patient population studied as well as observable hospital characteristics, c is a hospital fixed effect that corrects for selection into hospitals based on permanent unobserved characteristics, ν is a set of year dummies that controls for overall changes in the health outcome over time and ϵ is the idiosyncratic error.

In the baseline model, I assume that:

$$E(Y_{jt}|Z_{j1}, Z_{j2}, ..., Z_{jT}, c_j) = E(Y_{jt}|Z_{jt}, cj) \tag{2a}$$

$$E(\epsilon_{jt}|Z_{j1}, Z_{j2}, ..., Z_{jT}, c_j) = 0, \quad t = 1, 2, ...T \tag{2b}$$

where $Z_{jt} = [UNINS_{jt}, X_{jt}]$. Under this strict exogeneity assumption, β represents the causal effect of uninsurance on the insured health outcome. This assumption, while commonly made in panel data applications, is non-trivial as it states that the explanatory variables are uncorrelated with the disturbance term in *any* period. There are many potential scenarios under which the strict exogeneity assumption might fail and I investigate them further in the results section.

As discussed in more detail in the data section, the main health outcome of interest is the in-hospital mortality rate of insured heart attack (acute myocardial infarction or AMI) patients. Given that the outcome variable is by nature restricted to the unit interval, one might be concerned that a linear model might be a poor approximation. In order to address this issue, I also estimate non-linear models. First, I estimate a grouped logit model. Under the assumption that the probably of death of an insured AMI patient is described by a logistic distribution, one can show that the log–odds ratio of the insured AMI mortality rate, i.e. $Ln\left(\frac{Y_{jt}}{1-Y_{jt}}\right)$, is linearly related to the explanatory variables.[20] One drawback of the grouped logit model is that it is not applicable when the outcome variable takes on the boundary values. For that reason, I also provide estimates from a fixed-effects fractional logit model. This model assumes that the fraction of insured heart attack patients who die at the hospital, i.e. Y itself, has a logistic distribution.[21] The marginal effects provided by these models are qualitatively similar and quantitatively close to those provided by the fixed-effects specification. They are provided with the baseline regressions results in section 5.1.

4 Data

To assess the effects of uninsurance on insured heart attack patients, I combine information on California hospitals from three different sources for the period 1999–2006. The main source, California Public Inpatient Hospital Discharge Data (PDD), is an annual individual level dataset on every discharge from non-federal CA hospitals (approximately 3.8 million discharges per year). It includes information on basic demographic characteristics of patients including age, sex, race, ethnicity and residential zip code. More importantly, the dataset provides extremely detailed clinical information. For every discharge, I have information on the five-digit ICD-9 codes[22] of the principal and up to 24

[20] To be precise, a grouped logit estimator is given by the weighted least squares regression of the log–odds ratio on the explanatory variables. The weights are given by $\left(n * \widehat{Y} * (1-\widehat{Y})\right)$ where n is the number of insured AMI discharges at the hospital and predicted probabilities are obtained from a first stage unweighted regression of the log–odds ratio of Y on explanatory variables. In this model, β represents the change in the log–odds of an insured AMI patient dying when the uninsurance rate increases by one percentage point and the marginal effect of uninsurance on Y can be calculated as $\left(\beta * \overline{Y} * (1-\overline{Y})\right)$. For more details on grouped logit models, see Maddala (1983) and chapter 21 in Greene (2003).

[21] The panel data application of the fractional logit model is somewhat different than the linear probability model and the grouped logit model. In this case, hospital fixed effects are accounted for by including the averages over time of explanatory variables for each hospital. Hence, the coefficients on level variables capture the deviations from the means. The marginal effects are calculated in the same way as in the case of a binary logit model. For more details, see Papke and Wooldridge (2007).

[22] The International Classification of Diseases (ICD) is published by the World Health Organization and is used to assign codes to diagnoses and procedures associated with inpatient and outpatient utilization.

secondary diagnosis and procedures and the DRG code[23] of the case file, as well as the admission quarter, source of admission and type of discharge location. This allows me to precisely measure the outcomes of heart attack patients while effectively controlling for their underlying health conditions that might predispose them to adverse outcomes. Finally, the PDD data also includes information on the primary expected source of payer which I employ to measure uninsurance.[24]

In addition to the rich information available on individuals, patient discharge data also has the advantage of including elderly Medicare HMO patients and younger patients as opposed to Medicare administrative data (Doyle, 2008). However, this dataset also presents several limitations. Two concerns are worth noting. First, since the data includes only patients who are admitted to the hospital, we may be concerned about a sample selection bias if admission and discharge policies of hospitals change over the sample period in a way correlated with uninsurance. Second, if uninsured patients in outpatient and emergency care settings affect the quality of inpatient care, the fact that the data includes only admitted uninsured patients would produce measurement error in the main independent variable. This measurement error may be non-classical in nature. I will return to these issues later.

The second source of data is the California Annual Hospital Utilization Files. This yearly hospital level dataset provides information on each hospital's license type, location (address, city, zip code), health service area, teaching status and ownership type. The last source of information comes from spatial Geocode data (latitude and longitude) that I constructed using the address information in the Annual Hospital Utilization Files.

In the empirical analysis, I focus on general acute care hospitals operating from 1999 through 2006 that provided care to insured heart attack patients at any time during the sample period. Table 3 summarizes the construction of the analysis sample. First, I restrict the sample to general acute care hospitals using the license type information from the utilization data. Second, I exclude hospitals that exit or enter the sample. Third, I drop individuals with missing or misclassified primary expected payer as I use this variable to define the insurance status of patients. I also drop individuals who were admitted to the hospital more than a year before the year of their discharge. For example, for 1999 data, I drop individuals with an admission year before 1998. This ensures that my results are not driven by a selected sample of very sick patients. The final restriction is that

[23] Diagnosis-related group (DRG) is a system that classifies patients with similar hospital resource use (based on their diagnostic, therapeutic and demographic characteristics) into groups for reimbursement purposes.

[24] To ensure patient confidentiality, some variables in the PDD are masked for certain observations. Masked variables include, in the order of masking: age in years, ethnicity, race, sex, categorical age variables, residential county code, admission quarter and patient zip code. In my empirical analysis, I include indicators for whether the observation had masked information on any of the variables. Note that expected primary payer and clinical variables are never masked.

the hospital provided care to insured heart attack patients. This leaves me with 2,685 hospital-year observations on 352 hospitals that provided care to 426,459 adult insured heart attack patients.

4.1 The Outcome Variable

The outcome variable is the *in-hospital* heart attack (acute myocardial infarction or AMI) mortality rate of insured patients aged 18 and above whose discharge information is not missing.[25] Examining the impact of uninsured patients on the heart attack mortality of insured patients is interesting for several reasons. A heart attack is an acute event that occurs when the heart gets an insufficient supply of blood due to blockage in the coronary arteries. Unless immediate care is provided, part of the heart muscle dies within hours. It is the leading cause of death for both men and women in the US with 668,447 reported hospitalizations in 2006[26] and over 900,000 incidences across the nation every year (Spertus et al., 2003). It is also a health event for which the cost of treatment has increased substantially over time (McClellan et al., 2002). Hence, heart attacks are widely studied in the medical literature and to a smaller extent in economics.[27] However, no study has yet explored the effects of uninsured patients on the health outcomes of insured heart attack patients.

AMI is also a health condition for which evidence suggests that high mortality may be associated with insufficient quality of care. In their examination of the appropriateness of in-hospital mortality rates as quality indicators for ten diagnostic groups, Thomas et al. (1993) found that cardiac diseases have the strongest evidence of validity based on peer reviews. Similarly, McClellan and Staiger (1999) found that heart attack mortality correlates well with other quality indicators. Some researchers also argue that the wide treatment options for heart attack patients make it possible for health care providers to vary the quality of care (Farsi, 2004). In all, while it is not a perfect measure, heart attack mortality is among the best available quality indicators and it has thus far been used by many policy makers (e.g., the Texas Department of State Health Services and the Centers for Medicare and Medicaid Services) and researchers (e.g., Ho and Hamilton (2000); Gowrisankaran and Town (2003); Shen (2003); Farsi (2004); Burgess et al. (2004); Propper et al. (2007)).

[25] I define the outcome variable using all insured heart attack patients in the data, including those who transferred out to another hospital and those who transferred in from another hospital. However, I check the sensitivity of my results to excluding transfers. These additional measures correspond to the inpatient quality indicators (IQI) developed by Agency for Health Research and Quality (2007): IQI15 excludes from the sample those patients who are transferred to another hospital, whereas IQI32 further excludes those who were transferred from another hospital.

[26] Calculated from the HCUP data, available at `http://hcupnet.ahrq.gov/`, accessed on August 16, 2008.

[27] One line of research in economics examines the effects of technological changes on medical outcomes and productivity. Part of this research focused on heart attacks. See, for example, Cutler et al. (1999); Cutler and Berndt (2001); McClellan and Kessler (2002); Murphy and Topel (2003).

A third advantage of heart attack mortality as an outcome variable is that it is superior to other adverse outcomes in a measurement sense. Heart attack, as a diagnosis, is less likely to be affected by coding differences across hospitals. In addition, the timing of death is unambiguous, whereas diagnosis of other adverse outcomes such as infections conveys less information about the timing of the event. For example, the patient could have had the infection before being admitted to the hospital but access to care for some other diagnosis could have made the detection of the infection possible.

Finally, because it is a severe medical condition that requires urgent care, the endogenous selection of heart attack patients into hospitals based on health status or other unobservable characteristics is less of a concern. In addition, in the case of heart attacks, hospitals may not be able to select patients based on their survival risk as the Emergency Medical Treatment and Active Labor Act (1986) requires that they stabilize every patient with an emergency condition. The fact that use of heart attack patients minimize selection bias has led several other researchers to focus on this sample in other contexts.[28]

Several limitations of using in-hospital heart attack mortality as an outcome should be noted, however. To begin with, since heart attack mortality rates are published and made available to patients and insurers, hospitals may avoid reducing the quality of care for these patients but choose to reduce the quality of care of services for which public information is not available. Similarly, hospitals may reduce the quality of services typically used by uninsured patients rather than reducing the overall quality of care. In this case, care to heart attack patients would not be affected as only a very small fraction of uninsured patients have heart attacks and uninsured heart attack patients constitute a small fraction of overall heart attack patients. To the extent that this type of behavior is present, my results, which are positive and statistically significant at conventional levels, should be taken as lower bounds.

Another problem with using annual inpatient AMI mortality is that it is not reliable when hospitals treat relatively few patients. This "small denominators" problem leads to misclassification of the quality of hospitals (McClellan and Staiger, 1999) which amounts to measurement error in the outcome variable, reducing the precision of the estimates. In order to check the noisiness of this measure, I first investigate the persistence in the classification of hospitals. In particular, I divide the hospitals into deciles based on their 1999 insured heart attack mortality rates and check how the ranking of the hospitals in the best decile changes over the sample period. I also check the average difference in the insured AMI mortality between this group of hospitals and the rest

[28] Cutler et al. (2001) examine the effects of managed care on medical productivity, Shen (2002) studies the impact of hospital ownership choices on patient health outcomes, and Hall et al. (2008) examine whether the regulation of nurse wages affects the quality of hospital care in the UK.

of the hospitals in each year. There were 59 hospitals in the best decile of heart attack mortality distribution in 1999. Of these hospitals, 22 and 21 were still ranked in the best decile in 2002 and 2006, respectively. On the other hand, 10 (11) of these hospitals were located in the worst decile of the distribution in 2002 (2006). This suggests that hospitals that were classified as best quality in 1999 were still more likely to be classified like that during the rest of the sample period. Comparing the average insured heart attack mortality in these 59 hospitals to the average of the rest of the hospitals in 2002 and 2006, I find that the average insured AMI mortality is significantly lower among these hospitals throughout the period.[29] Overall, this indicates that the small denominators problem is unlikely to affect my findings. Still, in the results section I will check the robustness of the estimates to various sample selection criteria. I will also estimate long difference models using 3-year weighted averages to alleviate the potential measurement error in the outcome variable.

Finally, using inpatient mortality rates may lead to biased estimates if hospitals change their admission or discharge policies over the sample period in a way correlated with uninsurance. For example, hospitals characterized by high uninsurance may reduce their admission rates, resulting in a sicker patient mix and higher mortality rates over time. Similarly, if the floor-to-door care (care provided between the time of the heart attack and hospital arrival) in high uninsurance areas gets better over time, more patients may survive the trip to the hospital only to die post-admission leading to a spurious correlation between uninsurance and the outcome variable. These issues are common in all studies using heart attack mortality and they are difficult to address directly due to data limitations. However, the richness of the data allows me to bring suggestive evidence on the extent of these problems and I will return to them in section 5.

Regarding the changes in discharge policies, the effects of uninsurance may be upward biased if hospitals classified as low uninsurance discharge patients faster over time. Many studies use the 30-day, 90-day or 1-year window following a coronary attack to assess the effectiveness of medical treatment. As such, they capture post-discharge deaths as well as inpatient deaths. Since such a strategy is not feasible with my dataset, I will rather provide evidence that the average length of stay for an insured AMI patient does not change systematically with the uninsurance rate. In future work, I will check the robustness of my results to using these alternative mortality measures.

4.2 Measures of Uninsurance

The main explanatory variable of interest measures the exposure of insured patients in each hospital to uninsurance. My preferred measure is the fraction of uninsured patients in the health care market of a hospital because the choice of a health care market is presumably exogenous, compared to the

[29]The difference is equal to -0.028^{***} (SE 0.006) in 2002 and -0.018^{***} (SE 0.006) in 2006.

choice of a hospital (Doyle, 2008). However, I will also provide results using the fraction of uninsured patients in the hospital.

Measuring the fraction of uninsured patients in a health care market requires defining the uninsurance status of a patient as well as the appropriate empirical health care market for a hospital. I classify a patient as uninsured if the primary expected payer is one of the following: County Indigent Program, Other Indigent program, self-pay, or other. I include patients covered by indigent programs in the definition because, as shown in section 2, hospitals incur a net loss for providing care to these individuals.[30] Following Garnick et al. (1987) and Gruber (1994), I define the health care market in two ways: a geopolitical boundaries approach, and a fixed-radius approach.

As the name suggests, the geopolitical boundaries approach defines health care markets using political boundaries (e.g.; Gaskin and Needleman (2003); Needleman and Gaskin (2003)). Under this approach, the main independent variable is given by the fraction of uninsured patients in the county. However, it is likely that hospitals located close to county borders attract patients located in neighboring counties, whereas hospitals located in really large counties such as Los Angeles do not attract all the patients in their county.

Motivated by this idea, the fixed-radius approach defines health care markets as a fixed mile radius area around each hospital. I use a 5-mile, 10-mile and a 15-mile radius to define the health care market of each hospital and calculate the fraction of the uninsured patients among all the hospitals located within that area.

Finally, since appropriate measures of radius for urban and rural hospitals might not be the same, I supplement these market definitions with a third one where I define the market radius depending on hospital location. In particular, I define the market of rural hospitals to lie within a 10 mile longer radius than urban hospitals, using a 10 (15) mile radius for urban hospitals while using 20 (25) miles for rural ones.

4.3 Control Variables

In the empirical analysis, I control for a large set of observable variables. Exclusion of these variables could lead to omitted variable bias as they are likely to be correlated with both AMI mortality and the fraction of uninsured patients in the hospital's health care market.

The first set of variables controls for the observable characteristics of insured heart attack patients. These include basic demographic information such as the distribution of age (3 categories), race and ethnicity (4 categories) and gender. Most importantly, they include a comprehensive set of

[30] This way of defining the uninsurance status is also common in the literature. See, for example, Currie and Fahr (2004).

30 comorbidy measures that the medical literature found to be important predictors of in-hospital mortality (Elixhauser et al., 1998). These comorbidy measures are conditions that are not directly related to the principal reason for hospitalization and provide information about the patient's underlying health status. They are defined using certain exclusion criteria on the secondary diagnosis variables and the DRG code of the case file in the data. To illustrate, a patient is recorded to have uncomplicated diabetes as comorbidity if any of the secondary diagnosis codes include the ICD-9 codes of diabetes (250.00–250.33) and the DRG code of the case is *not* diabetes (294–295). Finally, clinical control variables also include indicators for complications and surgeries the patient had during their stay in the hospital. The indicator for complications is based on a set of diagnosis codes that refer to adverse events resulting from medical treatment, while the surgical indicator is defined using the set of surgical DRG codes (DesHarnais et al., 1990; Elixhauser et al., 1998). One point worth mentioning about these comorbidity variables is that they may reflect the differences in the intensity of screening across hospitals. Since I control for hospital fixed effects, time-invariant differences in screening is not a concern. However, if hospitals change their intensity of screening over time in a way correlated with uninsurance then the FE estimates will be inconsistent. I investigate this issue later.

The second set of variables consists of observable hospital characteristics. Among these, I include the demographic and clinical characteristics of all the patients in the hospital as they might provide information about the quality of care provided.[31] Other variables include type of ownership (3 categories), type of location (rural/urban), teaching status, total discharges, the total number of hospitals in the health care market, for each definition of the market (to control for competition), and the fraction of Medi-Cal patients in the hospital. Naturally, the time-invariant characteristics are dropped once hospital fixed effects are added to the specification. Finally, I also include variables to control for the fraction of unknown/not reported variables as non-reporting might convey information about hospital behavior.

4.4 Descriptive Statistics

Table 4 provides descriptive statistics for insured AMI patients. All values are weighted by the number of insured heart attack patient discharges at the hospital. The mean in-hospital mortality rate is 8.2 percent which is higher than the national average of 7.62 percent for the same period.[32] Not surprisingly, the elderly constitute a big majority of these patients (65.4%) while females repre-

[31]In this case, there are 5 age categories and the clinical variables also include the distribution of major diagnostic categories.

[32]Calculated from HCUP, available at http://hcupnet.ahrq.gov/, accessed on August 18,2006.

sent a much smaller share (36.3%). The largest ethnic group in the sample are non-Hispanic whites (57.9%) and the most common comorbidities include uncomplicated diabetes (26.2%), chronic pulmonary disease (18.4%) and fluid and electrolyte disorders (14.7%). While complications are not uncommon, they are not widespread (4.4%).

As previously stated, the analysis sample consists of hospital-year observations for which the hospitals have positive insured AMI discharges. One might be concerned that the results are not generalizable to hospitals that do not treat heart attack patients. Table 5 thus provides summary statistics separately for hospital-year observations included and excluded from the analysis. Panel A provides information on market level uninsurance rates and basic demographic information on hospitals. These statistics are unweighted. Panel B summarizes observable demographic characteristics of all patients at the hospitals. These values are weighted with the total number of discharges at the hospital.

Columns 1-3 provide descriptive statistics for the analysis sample. The average market-level uninsurance rate is around 6 percent, regardless of the definition of the market, but there is much more variation under the distance based approach than the geopolitical one. Around 82 percent of hospital-year observations belong to an urban hospital. Not-for-profit, for-profit and public hospitals constitute about 57.5, 23.6 and 18.9 percent of observations, respectively. The mean hospital size is over 10,000 discharges and non-reporting (not included in the table) by hospitals does not seem to be a problem. The age distribution among patients is skewed to the right with patients who are above 35 comprising almost 63 percent of all discharges. Female discharges are also represented more (around 60%) and the majority of observations belong to non-Hispanic white individuals. Following them are Hispanic patients with 28 percent of discharges.

Descriptive statistics on hospitals that were in operation from 1999 through 2006 but did not have AMI discharges among insured patients are provided in columns 4-6. Market measures of uninsurance among these hospitals are comparable to those in the analysis sample but the excluded hospitals have significantly less in-hospital uninsurance rates. Although around 83 percent of the observations on these hospitals also come from urban areas, not-for-profit hospitals represent a much smaller fraction of the observations. The mean number of discharges is around 2,600 and the patient profile is also different with patients aged 17 and below representing 63 percent of all discharges. Although the shares of non-Hispanic black patients are similar, the excluded hospitals serve relatively more Hispanic and male patients.

Figure 1 compares the distribution of clinical variables (the 25 major diagnostic categories and the 30 Elixhauser comorbidities) among all hospital patients. The values correspond to sample means weighted by total discharges. The solid line represents the observations included in the analysis while the dashed line represents excluded ones. As seen in figure 1(a), patients in included and excluded hospitals have very similar distributions along major diagnostic categories. One notable difference, as expected, is that excluded hospitals have fewer patients diagnosed with circulatory system problems. In contrast, a relatively higher fraction of their patients are categorized as mentally ill (MDC 19). Figure 1(b) provides a similar analysis for Elixhauser comorbidities. Although the general patterns of the fraction of patients with comorbidities are similar across included and excluded samples, the solid line generally lies above the dashed one indicating that the included observations have relatively higher fractions of patients with comorbidities. This might partly be due to the fact that these conditions are common among heart attack patients but nonetheless it should be kept in mind when considering the generalizability of the key findings to all hospitals.

Figure 2 presents the variation in the outcome variable and the uninsurance rate. In figure 2(a), I plot the evolution of selected percentiles (10th, 25th, 50th, 75th and 90th) of the insured AMI mortality distribution between 1999 and 2006. The figure confirms the well documented cross sectional variation in insured heart attack mortality at any given point in time. For example, in 1999, the mortality rate in the 10th percentile is around 5 percent whereas it is over 12 percent at the 90th percentile. Looking at the evolution of the distribution, the mortality gap between the percentiles roughly stays constant over the sample period but there is a general decline in all deciles consistent with the long-run downward trend in heart attack mortality. Similarly, figures 2(b)-2(d) present the variation in uninsurance for both geopolitical and distance-based market definitions as well as at the hospital level. For presentational clarity, I only provide the variation in the 10/20 mile distance based market definition but other distance based market measures follow a similar pattern. These figures show that the uninsurance rate varies substantially across hospitals. The dispersion is much larger at the hospital level than at the market level. For example, in 1999, the disparity between the 90th percentile and the 10th percentile of the hospital level distribution is close to 13 percentage points where as it is around 7 percentage points for the 10/20 mile market definition and 3.5 percentage points for the county market definition. The evolution of the distribution shows some convergence in uninsurance rates in recent years regardless of the level of aggregation.

Finally, table 6 and figures 3 and 4 compare hospitals in high and low uninsurance areas by splitting the sample using the median uninsurance rate in each year. Columns 1–2 are based on the uninsurance rate in the county while columns 3–4 are disaggregated using the 10/20 miles market uninsurance rate. Comparisons based on other distance based market measures yield qualitatively similar results. Each cell represents the mean of the variable in that sample. The first row presents the mean uninsurance to get a sense of the extent of the differences in the two split samples. The second row shows that insured AMI patients in high uninsurance areas have similar or slightly higher mortality rates, foreshadowing a small positive relationship in the OLS estimates.

The next panel of rows compare the observable characteristics of insured heart attack patients. Both sample splits suggest that while patients in high and low uninsurance areas are similar in terms of gender composition, those in high uninsurance areas are more likely to be non-Hispanic black or Hispanic. Although the elderly constitute the majority of the insured heart attack patients in both high and low uninsurance areas, the sample split based on county uninsurance points to a slightly older population in high uninsurance areas. Figure 3 presents the prevalence of comorbidities among insured AMI patients. The solid line corresponds to high uninsurance areas while the dotted line represents low uninsurance areas. Both splits suggest that the fraction of patients with a given comorbidity are very similar in the two samples.

The last panel of table 6 shows means of observable hospital variables, including the characteristics of the overall patient population. Patients in high uninsurance areas are more likely to be male, non-Hispanic black and Hispanic. High and low uninsurance areas are more similar in terms of the age composition of overall patients when the split is based on county uninsurance. Figure 4 compares the health status of patients in the two samples along dimensions of comorbidities and major diagnostic categories. These figures show that patients characteristics in these areas are very similar regardless of the basis of the split. Returning to hospital characteristics, both splits suggest that hospitals in high uninsurance areas have more neighboring hospitals, are larger in size but have a slightly smaller heart attack patient population. Not-for-profit hospitals comprise the largest share in both high and low uninsurance areas. While both sample splits suggest that high uninsurance areas are more likely to have for-profit hospitals and less likely to have not-for-profit hospitals, the distance based split shows that high uninsurance areas are also more likely to have public hospitals. According to columns 1–2 high uninsurance areas are associated with significantly more urban locations, whereas columns 3–4 suggest a similar urban/rural composition among the two samples.

To sum up, high and low uninsurance areas are similar along many dimensions but the differences in other aspects suggest that patients are not randomly distributed across health care markets. Given the disparities in the samples depending on the basis of the split, the type and degree of selection may also differ among alternative market definitions. I will devote most of the results section to examining whether the results may be driven by selection.

5 Econometric Results

5.1 Baseline Results

Table 7 presents the main results. Each row represents the effect of uninsurance on the in-hospital mortality rate of insured AMI patients using a different aggregation level. In order to gauge the importance of controlling for time-invariant unobserved hospital effects, I first present simple OLS estimates (column 1). Each specification includes demographic and clinical characteristics of insured heart attack patients in the hospital, demographic and clinical characteristics of all the patients in the hospital, basic hospital demographics and year effects. Regressions are weighted by the number of insured heart attack discharges at the hospital and standard errors are clustered at the county level.[33] The coefficient estimate in row 1 of column 1 (0.035, SE=0.016) implies that a one percentage point increase in the fraction of uninsured patients at the hospital is associated with a 0.035 percentage point increase in the inpatient mortality rate of insured heart attack patients. As previously discussed, hospital level uninsurance is potentially more endogenous than uninsurance in the health care market. Thus, the remaining rows provide results using the uninsurance rate in the health care market as the independent variable. The findings in this case consistently point to a statistically insignificant association between uninsurance and in-hospital insured AMI mortality.

Column 2 adds hospital fixed effects to the simple OLS specification in column 1. In contrast to simple OLS results, findings from FE models support the existence of negative spillovers from the uninsured to insured heart attack patients in their health care markets. The effects are much larger (200–500 percent) than those in column 1 and are statistically significant in all cases but the geopolitical market definition. For example, the estimate based on the 10/20 mile market definition implies that a one percentage point increase in the uninsurance rate around a 10(urban)/20(rural) mile radius of a hospital increases the mortality rate of its insured heart attack patients by 0.068 percentage points. In 1999, a move from the worst to the best decile of the uninsurance rate based on the 10/20 mile market definition was about 3.63 percentage points. The same year the insured AMI mortality spread between the worst and the best deciles was about 4.28 percentage points. Thus, the estimated effect of uninsurance (0.25 percentage points) can explain 5.76 percent of this mortality spread.

The next two columns bring evidence from non-linear models. Column 3 reports marginal effects from the grouped logit specification. The effects are in general similar to those generated by the FE model. For example, the estimate in row 4 implies that a one percentage point increase in the uninsurance rate around a 10-mile radius of a hospital is associated with a 0.07 percentage

[33]The results are robust to clustering at the hospital level.

point increase in insured AMI mortality rate, compared to 0.063 percentage points estimated by the FE model. As previously discussed, the samples in grouped logit and FE models are not directly comparable as the grouped logit model is not applicable when the outcome variable takes on the boundary values.

For that reason, in column 4, I present estimates from a fixed-effects fractional logit model. The results are qualitatively similar and quantitatively very close to the FE estimates. For example, the fractional logit estimates suggest that as the uninsurance rate within a 15-mile radius of a hospital goes up by one percentage point, the heart attack mortality rate of its insured patients increases by 0.053 percentage points (row 5 column 4). The marginal effect suggested by the FE model corresponding to a 15-mile market definition is 0.054 percentage points.

Overall, the analysis in this section suggests that results from linear models are reasonably similar to those provided by non-linear models. For the remainder of the paper, I therefore report results from linear models for the sake of brevity. In addition, the qualitative implications of cross-sectional models are found to be very different from those of panel data models. The direction of the bias between these two models imply that uninsured patients are admitted to high quality hospitals. This can be explained by the fact that high quality hospitals tend to be located in central cities, areas characterized by relatively lower incomes and thus higher uninsurance rates. However, we may still be concerned about a potential measurement error bias that gets amplified in the FE model or the possibility of heterogeneous treatment effects where a small group of hospitals drives the FE estimates.

To address this concern, I first conduct a Hausman test of endogeneity to test the orthogonality of the fixed effects and the explanatory variables. This test maintains the assumption of zero correlation between the explanatory variables and ϵ_{jt} under both the null and the alternative hypothesis. It then compares the OLS and FE estimates to test the orthogonality of the fixed effects to regressors. If OLS and FE estimates differ statistically significantly, this suggests that either the orthogonality of fixed effects is violated or the zero correlation between the explanatory variables and ϵ_{jt} is violated. As the p-values reported in column 5 of table 7 shows the null hypothesis is rejected for all distance based market definitions.

Next, I investigate whether the Hausman test is rejected due to measurement error in uninsurance. In column 6 of table 7 I estimate a long difference model using 3 year weighted averages. In particular, I average observations for 1999–2001 and 2004–2006 and then estimate a difference model using these 3 year averages. In the averaging scheme, insured AMI patient variables are weighted with insured AMI discharges, variables pertaining to all patients are weighted using total discharges and hospital level variables are unweighted. The regressions are weighted by the average number of insured discharges seen at the hospital during these six years. Averaging is a common noise reduction method and thus should help with a potential measurement error in both the outcome variable and the measure of uninsurance. The long difference model is similar to FE in that it eliminates the hospital fixed effects but has the advantage of relying on variation between two farther years rather than year-to-year variation which would be more noisy. The results, provided in column 6, are imprecise but the estimated magnitudes are reasonably close to the original FE estimates in column 2.

Finally, I check whether the differences between the OLS and FE estimates may be due to heterogeneous treatment effects. Since the FE model relies only on within hospital variation, if the effects of uninsurance are heterogenous across hospitals with varying levels of within hospital variation, the FE results in column 2 might only represent a local treatment effect. The last two columns of the table present OLS and FE estimates from a sample where observations in the lowest quartile of the distribution of the variance of uninsurance are dropped. The general implications from the previous analysis are confirmed: OLS estimates are small and insignificant and FE estimates are generally larger and statistically significant.

The evidence provided so far does not support the claim that OLS and FE results differ due to a measurement error bias that gets amplified in a within hospital analysis framework or heterogeneous treatment effects. In the next section, I investigate other scenarios under which the strict exogeneity assumption might fail.

5.2 Potential Biases

Hospital Characteristics

One of the key impediments to interpreting the FE results as causal is the possibility of a bias due to time-varying unobservable characteristics of hospitals. If temporarily low quality hospitals have certain unobserved characteristics that make them attractive to uninsured patients over time, they may drive up the market level uninsurance rate and lead to an upward bias in FE estimates. If certain hospitals become magnets to uninsured patients, then we would expect uninsured patients to be reallocated across markets and hospitals. Hence, I start by studying the possibility of reallocation of uninsured patients. Following a strategy similar to Duggan (2000), I estimate the predicted number of uninsured patients in hospital j at year t as:

$$\begin{aligned} PredUninsured_{jt} &= \sum_{z=1}^{Z} \left(\frac{n_{jz99}}{\sum_{j=1}^{J} n_{jz99}} \sum_{j=1}^{J} n_{jzt} \right) \\ &= \sum_{z=1}^{Z} \frac{n_{jz99}}{n_{z99}} n_{zt} \end{aligned}$$

where n_{jzt} is the number of uninsured patients from zip code z that go to hospital j in year t. I use the predicted number of uninsured patients in hospitals to construct the predicted number of uninsured patients in the health care markets. Given that the main explanatory variable of interest is the fraction of uninsured patients, I construct a similar measure to predict the fraction of uninsured patients. I then estimate the relationship between the observed and predicted values. Since reallocation might not be instantaneous, I estimate specifications regressing the actual change from 1999 to 2006 on the predicted change during the same period.[34] In this specification, the coefficient on the predicted change tells us the units of actual change corresponding to a unit predicted change. If there is no reallocation of uninsured patients, then the estimated coefficients should be equal to 1.

The results are provided in table 8. The first panel includes hospitals in the analysis sample. The outcome variable in the first row is the actual change in the number of uninsured patients, whereas in the second row I focus on the actual change in the fraction of uninsured patients. The columns describe the geographic aggregation and F–statistics from a Wald test that the estimated coefficient is equal to 1 are provided in brackets. The results suggest that there has been some reallocation of uninsured patients during 1999–2006. The allocation seems to take place mostly among the hospitals within a market rather than across markets. For example, the coefficient estimate in row 1 and column 1 indicates that a hospital that was predicted to admit one more uninsured patient actually admitted 1.301 more uninsured patients. The coefficient estimate in row 1 and column 5 suggests that a health care market where the number of uninsured patients was predicted to increase by 1 individual actually had 1.161 more uninsured patients. The story remains the same if the fraction of uninsured patients rather than the levels is used as the outcome variable. However, note that the number of hospitals used in these specifications is less than 352 as the regressions apply to hospitals that provided care to insured AMI patients in both 1999 and 2006. In order to check the robustness of these estimates, in the second panel, I conduct the same analysis using all general acute care hospitals that were in operation from 1999 through 2006. The

[34]Results from specifications using levels rather than changes are similar.

estimates are both qualitatively and quantitatively similar.

The fact that the implied magnitude of reallocation across markets is less than within markets suggests that using an aggregate measure of uninsurance does alleviate the endogeneity problem associated with patients choosing their health care providers and health care providers affecting their patient mix. However, the estimated coefficients of the predicted market level variables are still statistically different than one and thus we cannot rule out that reallocation of patients over time is biasing the FE estimates. The previous analysis suggests that markets where there were predicted increases in uninsurance experienced even larger increases. If markets where uninsurance was predicted to increase also provide low quality health care, then the FE estimates will be upward biased. In order to address this issue, I divide hospitals into terciles based on their in-hospital mortality rates of insured AMI patients in 1999[35] and compare the mean predicted changes in uninsurance in the markets of these hospitals between 1999 and 2006.

Table 9 provides the results. Each row in the first panel presents the mean and standard deviation of the change in the predicted number of patients in the relevant market during 1999–2006. The mean number of uninsured patients in each market in 1999 is provided in brackets. The second panel provides the same statistics for the fraction of uninsured patients separately for each market. Column 1 includes the full sample, while columns 2–4 provide results by the quality of care provided in the hospital. High quality hospitals are those located in the bottom tercile of the insured AMI mortality distribution in 1999, while low quality hospitals are those in the top tercile. The results show that overall the number of uninsured patients in health care markets were predicted to decline over the analysis sample. However, markets where low quality hospitals were located were predicted to experience the largest declines. For example, the figure in row 1 and column 2 indicates that on average low quality hospitals were predicted to have 2,052 fewer uninsured patients (7.1%) admitted to hospitals in their counties. Medium and high quality hospitals on the other hand were predicted to experience smaller declines in the number of uninsured patients in their counties: 968 patients (5.4%) in mid and 723 patients (4.1%) in top quality hospitals. This monotonic decline in the mean predicted changes (both in absolute terms and relative to the baseline levels) across quality terciles holds among all market definitions and the same patterns are observed in the second panel where the variable of interest is the predicted changes in the fraction of uninsured. This suggests that uninsured patients relocate from low quality to higher quality markets, and if anything, the main results are *downward* biased.

[35] I weigh the mortality rates by the number of insured AMI discharges in the hospital. The results using unweighted mortality rates are qualitatively similar.

I supplement the descriptive statistics provided so far with a simple specification

$$UNINS_{jt} = \theta_0 + \theta_1 LowQ99 \cdot t + \theta_2 MidQ99 \cdot t + \theta_3 t + \theta_4 X_{jt} + c_j + u_{jt}$$

where I regress the actual uninsurance rate in the market on a time trend (t), its interaction with low ($LowQuality99 \cdot t$) and medium quality ($MidQuality99 \cdot t$) dummy variables and the control variables included in the baseline specification. The coefficients on the interaction terms capture the average yearly increase in the market uninsurance rates of low/medium quality hospitals relative to those in high quality hospitals. The results, provided in table 10, suggest that low and medium quality hospitals did not experience differential changes in their actual market level uninsurance rates relative to those of high quality hospitals: the coefficients on the interaction terms are all insignificant and very small in magnitude. This supports the previous finding that a bias arising from low quality hospitals attracting uninsured patients over time is unlikely.

Insured AMI Patient Characteristics

Similar to the previous analysis, we may be concerned that insured heart attack patients in high uninsurance markets may have unobservable characteristics that make them more likely to die compared those in low uninsurance markets, leading to a spurious positive correlation between uninsurance and the outcome variable. In addition, if over my sample period healthier insured patients exit high uninsurance areas leaving behind those who are more susceptible to die in the event of a heart attack, then the strict exogeneity assumption fails and the FE estimates will be inconsistent. These concerns are mitigated by the fact that my dataset includes extensive information on the clinical characteristics of the patients and thus, I am able to control for the true underlying health status of each patient. In what follows, I investigate these issues further and provide suggestive evidence that they are not driving the main results.

First, I examine whether hospitals that were located in high uninsurance markets in 1999 experience a deterioration in the health profiles of their insured heart attack patients over time relative to those in low uninsurance markets. If healthy patients exit high uninsurance markets, we would expect the profile of patients to worsen in these areas. Similarly, to the extent that the observable characteristics of patients convey information about their unobservable characteristics, a deterioration in the observed health status of patients would be compatible with the sorting of patients across health care markets based on unobservable clinical characteristics. In order to conduct this analysis, I classify each hospital as high, medium or low uninsurance based on the 1999 level of the uninsurance rate in their market. High uninsurance hospitals are those located in the top tercile of the uninsurance distribution, while low uninsurance hospitals are those in the bottom tercile. For a set of observable comorbidities, I run individual regressions

$$COM^i_{jt} = \theta_0 + \theta_1 TopU99 \cdot t + \theta_2 MidU99 \cdot t + \theta_3 t + \theta_4 X_{jt} + c_j + u_{jt}$$

where the outcome variable, the fraction of insured heart attack patients with a given comorbidity in hospital j at year t, is regressed on a time trend (t) and its interaction with high and medium uninsurance dummy variables ($TopU99 \cdot t$, $MidU99 \cdot t$), controlling for observable characteristics of insured AMI patients and hospitals. In this specification, the coefficients on the interaction terms capture the average yearly increase in the prevalence of a comorbidity in high/medium uninsurance hospitals relative to those in low uninsurance hospitals.

Table 11 provides the results for the 10/20–mile market definition. The results for other market definitions are very similar and available upon request. Each cell of the table represents the coefficient from a separate regression, so a total of 30 regressions are summarized. The results point to an impairment of health status among insured AMI patients over time in every market: the coefficient on the trend is positive and significant for most morbidity measures. However, I do not find any evidence in this specification that the profiles of patients in high and medium uninsurance hospitals got worse than those in low uninsurance markets as the majority of the interaction terms are insignificant and very small in magnitude. In cases where there are statistically significant differential changes, there is evidence of both deterioration and progression of health among insured heart attack patients. Hence, I find no evidence that the observable health status of insured AMI patients in high and mid uninsurance markets got worse over time relative to those in low uninsurance markets.[36]

In order to further probe the possibility of reallocation of insured patients, I estimate the predicted number of insured patients in each hospital and year using the strategy described above. Since the population of interest in this paper is the insured heart attack patients, I construct a similar measure to predict the number of insured AMI patients. I then regress the actual change in the numbers of insured and insured AMI patients on the predicted changes during 1999–2006.

The results are provided in table 12. The first two columns use the analysis sample. The outcome variable in column 1 is actual change in the number of all insured patients, whereas column 2 focuses on the insured heart attack patient admissions. As the F-statistics in the bottom panel of the table show, in both cases, the coefficient estimates of the predicted variable are statistically

[36] As discussed in section 4.3, one concern with the use of comorbidity measures is if hospitals change their intensity of screening over time in a way correlated with uninsurance rate. The results provided in table 11 suggest that changes in screening of comorbidities is unlikely to be systematically related to uninusrance rates.

indistinguishable from 1. This suggests that insured patients remained at the same hospitals during 1999 to 2006. Conducting the same analysis using all general acute care hospitals that were in operation from 1999 through 2006 confirms the initial finding that there is no evidence of reallocation of insured patients among hospitals (columns 3 and 4).

Finally, I investigate the issue of sorting of patients based on unobservable characteristics in more detail by estimating a sorting model *à la* Murphy and Topel (1990):

$$Y_{jt} = \alpha_{MT1} + X_{jt}\delta_{MT1} + c_j + \nu_t + \upsilon_{1jt} \quad (3a)$$

$$\hat{\beta}UNINS_{jt} = \alpha_{MT2} + X_{jt}\delta_{MT2} + c_j + \nu_t + \upsilon_{2jt} \quad (3b)$$

where $\hat{\beta}$ is the FE estimate of the market level uninsurance coefficient from equation (1). In this model, the vector of parameters δ_{MT1} in equation (3a) gives the percentage point change in the insured heart attack mortality rate in the hospital associated with a percentage point change in X_{jt}, all else equal. The outcome variable in equation (3b) can be thought of as the "estimated heart attack mortality premium due to uninsurance". Thus, the coefficient on X_{jt} indicates that when X_{jt} increases by one percentage point, the hospital "moves" to a market with δ_{MT2} percentage points higher insured AMI mortality. It then follows that in the case of positive selection, i.e. when patients who are more likely to die in the event of a heart attack go to hospitals in high uninsurance areas, we would expect $sign(\delta_{MT1}) = sign(\delta_{MT2})$, and in the case of negative selection $sign(\delta_{MT1}) \neq sign(\delta_{MT2})$.

Given the vast number of explanatory variables included in the regressions, instead of providing point estimates, I summarize the findings from the sorting model in table 13. Across the rows of the table, I report the number of all explanatory variables, the number of variables pertaining to insured AMI patients, the number of clinical variables pertaining to insured AMI patients, and the percent of these variables that have the same signs in equations (3a) and (3b). For example, rows 1 and 2 of column 3 indicate that of the 125 explanatory variables included in the model, only 50% had the same sign in the two equations. This is still true among the subset of variables pertaining to the characteristics of insured AMI patients. Furthermore, the relative magnitudes of the coefficient estimates considerably differ between the two equations (results not shown). Overall, the results do not support the hypothesis that patients with worse outcomes significantly sort into high uninsurance markets.

Local Shocks

In this section, I examine whether the results may be driven by local shocks that affect both heart attack survival risk and the market uninsurance rate. For example, some counties might increase their public reach and education programs over time. This could reduce the incidence of hospitalizations among uninsured populations and the likelihood of death among heart attack patients at the same time through increased preventive care and lead to an upward bias in the FE estimates. Similarly, changes in the economic environment, such as the closing of a factory, could lead to violations of strict exogeneity assumptions. In order to address this issue, I add county specific year dummies to the baseline empirical specification and check the sensitivity of the results. The estimates under this specification are slightly smaller in magnitude but the qualitative implications are the same as the original FE estimates with uninsurance leading to statistically significant increases in insured AMI mortality.[37]

One specific example of a local shock that frequently came up in discussions when presenting this work was the possibility of relocation of sick insured patients to areas with high quality hospitals due to development of low income housing around those locations. Since my previous analysis indicates that insured patients tend to stay in the same hospitals, such a scenario is unlikely to bias my estimates. However, in order to further probe this possibility, I divide hospitals into three terciles based on the distribution of the insured AMI mortality in 1999 and check whether the prevalence of comorbidities evolve differentially in low and medium quality hospitals relative to that of high quality hospitals. In particular, for a set of observable comorbidities, I regress the the fraction of insured heart attack patients with a given comorbidity in hospital j at year t, on a time trend (t) and its interaction with low and medium quality dummy variables ($LowQ99 \cdot t$, $MidQ99 \cdot t$), controlling for observable characteristics of insured AMI patients and hospitals.

$$COM^i_{jt} = \theta_0 + \theta_1 LowQ99 \cdot t + \theta_2 MidQ99 \cdot t + \theta_3 t + \theta_4 X_{jt} + c_j + u_{jt}$$

Table 14 provides the results. Each cell of the table represents the coefficient from a separate regression, so a total of 30 regressions are summarized. The results point to an impairment of health status among insured AMI patients over time in every market: the coefficient on the trend is positive and significant for most morbidity measures. However, I do not find any evidence that the profiles of patients in low and medium quality hospitals got better than those in high quality hospitals as the majority of the interaction terms are insignificant and very small in magnitude.

[37] The estimates for each measure of uninsurance are as follows: hospital = 0.054** (SE 0.024); county = 0.003 (SE 0.037); 5 miles = 0.034 (SE 0.024); 10 miles = 0.049** (SE 0.019); 15 miles = 0.042** (SE 0.019); 10/20 miles = 0.053*** (SE 0.020); 15/25 miles = 0.045** (SE 0.019).

Hence, I find no evidence that high quality hospitals got sicker patients over time due to local shocks, such as development of a low income housing around them.

Sample Selection

As I previously discussed, using inpatient mortality rates may lead to biased estimates if hospitals change their admission or discharge policies over the sample period in a way correlated with uninsurance. It is difficult to address changes in admission policies directly due to data limitations on deaths occurring outside the hospitals or on provision of floor-to-door health care services. However, to the extent that these changes lead to the admission of sicker patients over time, we should observe a deterioration in the health status of insured AMI patients in high uninsurance areas. The results provided in table 11 suggest that this is unlikely.

Previous studies that focus on heart attack patients generally use longer term mortality rates to take into account potential changes in discharge policies. Since such a strategy is not feasible with my dataset, I check whether hospitals in low uninsurance areas discharge patients faster over time by examining the changes in average length of stay in these hospitals relative that of high and medium uninsurance hospitals. If low uninsurance hospitals indeed discharge patients faster, then the FE will be upward biased. The estimating equation is as follows:

$$ALOS_{jt} = \theta_0 + \theta_1 TopU99 \cdot t + \theta_2 MidU \cdot t + \theta_3 t + \theta_4 X_{jt} + c_j + u_{jt}$$

where the outcome, the average length of stay of for insured heart attack patients in hospital j in year t, is regressed on a time trend (t) and its interaction with high and medium uninsurance dummy variables ($TopU99 \cdot t$, $MidU99 \cdot t$) defined according the distribution of the uninsurance rate in the base year, and control variables for observable characteristics of insured AMI patients and hospitals. In this specification, the coefficients on the interaction terms capture the average yearly increase in the length of stay in high/medium uninsurance hospitals relative to those in low uninsurance hospitals.

The results, provided in table 15, suggest that over the sample period the average length of stay decreased in all types of hospitals: the coefficient of the time trend is negative and significant for all market definitions. It also provides some evidence of a differential evolution of the average length of stay in high and medium uninsurance areas relative to the low uninsurance ones. However, the results indicate that the duration of stay for insured heart attack patients actually decreased in high and medium uninsurance areas relative to low uninsurance areas. Thus, changes in discharge policies are unlikely to lead to an upward bias in my estimates.

Selection on Observed and Unobserved Variables

In this section, I apply the informal framework suggested by Altonji et al. (2005) to estimate the extent of selection based on unobservables relative to selection based on observables needed to wipe out the effects of uninsurance. The results are provided in table 16. Intuitively, the model starts by assuming that selection on unobservables is the same as selection on observables. Under this assumption if uninsurance has no effect on the health outcomes of insured heart attack patients, one can derive the bias in the estimated coefficient of uninsurance using only information on observables (column 1).[38] In an unrestricted model where uninsurance is allowed to have an effect on the insured, I obtain the coefficients listed in column 2. In order for these estimates to be entirely driven by selection on unobservables, the amount of that selection relative to selection on observables should be equal to the ratio of the coefficient to the estimated bias. This bias factor is reported in column 3.

The first panel of table 16 applies the framework under a simple OLS specification while the second panel presents results when the empirical specification includes hospital fixed effects. There are two things worth mentioning. First, the results in the first panel suggest that a small amount of selection on unobservables relative to selection on observables can account for the estimated effects of uninsurance in the simple OLS framework. Second, although the estimated bias factors in the second panel are relatively small in absolute value, the estimated bias is negative for spatially narrow market definitions indicating that the fixed effect results are, if anything, downward biased.

To summarize, in section 5.2 I examined a broad set of scenarios where violations of fixed effects assumptions may be of concern and provided evidence that these scenarios are unlikely to bias my estimates. While none of these tests are individually sufficient to claim that the main results are not driven by time varying unobserved variables or selection bias, taken together they provide compelling evidence that such a bias is unlikely.

5.3 Robustness Checks

In this section, I perform several checks to confirm the robustness of the main findings. The results are provided in table 17. Each column presents the effects of uninsurance on the in-hospital mortality rate of insured AMI patients using a different market definition. Row 1 reproduces the original FE estimates for reference.

In rows 2–4, I check the sensitivity of the results to various sample selection criteria. Following the guidelines of Agency for Health Research and Quality (2007), row 2 restricts the sample to

[38]For more details see Altonji et al. (2002) and Altonji et al. (2005).

hospitals with at least 30 heart attack patient discharges. Row 3 further restricts the sample to hospitals with at least 150 heart attack patients, while row 4 provides results based on a balanced sample of hospitals which provided care to insured AMI patients in every year.[39] The results are qualitatively the same and quantitatively close to the original FE estimates reported in row 1.

In rows 5–6 I examine the sensitivity of the results to including transfer patients. The problem is two-fold. First, a heart attack patient who gets transferred to another facility is (naturally) recorded as being alive and so hospitals that transfer out a lot of patients mechanically have lower mortality rates. Second, we may be concerned that transfer patients are different than non-transfer patients in terms of unobservable characteristics which make them more or less likely to die. If where patients get transferred is further correlated with the uninsurance rate, then the fixed effects estimates will be biased. In particular, the concern is if patients who are more likely to die get transferred to hospitals in high uninsurance markets, leading to an upward bias in my initial estimates. In row 4, I exclude patients who are transferred to another facility, while in row 5, I further exclude patients who are transferred from another facility.[40] Restricting the sample to non-transfer patients does not change the qualitative results, but the magnitude of the effect is now larger.

In rows 7–9, I examine the robustness of the results to using alternative measures of uninsurance. The measure of uninsurance used under the distance based market definitions assigns equal weights to all hospitals in the sample regardless of their relative distance to each other. However, a hospital's behavior may be more responsive to other hospitals that are closer to it even within a given radius. In row 7, I use an alternative measure of distance based uninsurance rate where the hospital itself is assigned a weight of 1 and the neighboring hospitals are assigned weights inversely proportional to their distances from that hospital. The results are robust to using this alternative measure of uninsurance.

In row 8, I examine the effects of uninsured patients who are admitted to the hospital through hospital ER. This is an arguably more exogenous measure of uninsurance since hospitals may have less discretion over who they admit through the ER. Although the precision of the estimates is somewhat reduced, the magnitudes of the estimated effects are much larger and generally statistically significant.

[39] The results are robust to other selection criteria, such restricting the sample to balanced panels with at least 30/150 insured or total AMI discharges every year.

[40] These two measures of AMI mortality, respectively, correspond to Inpatient Quality Indicators #15 and #32, developed by Agency for Health Research and Quality (2007). A discharge is classified as a transfer out of the hospital if the patient's disposition is any of "another acute care hospital, other care or skilled nursing/intermediate care". A patient is classified to be transferred from another facility if the site of his/her source of admission is one of "another acute inpatient hospital care, other inpatient hospital care or skilled nursing/intermediate care" and the licensure of the site is "another hospital".

Another issue regarding the measure of uninsurance is related to the diagnostic characteristics of uninsured patients. When calculating the fraction of uninsured, I include all uninsured patients regardless of their diagnosis. If uninsured heart attack patients crowd out resources available to other cardiac patients, then the estimated effects of uninsurance may be representing resource constraints faced by hospitals rather than true changes in the quality of care provided to insured patients. However, the policy implications under this scenario would be different. In row 9, I provide results from regressions that estimate the impact of uninsured patients who are not diagnosed with AMI and thus do not directly compete for resources with insured AMI patients. The results are qualitatively the same and very close in magnitudes to those using all uninsured patients.[41]

In row 10, I construct measures of market level Medicaid rates and examine its effects on insured AMI mortality. Hospitals often argue that the reimbursement rates for Medi-Cal patients fall short of their costs. Since hospitals presumably receive higher reimbursements for their care to Medicaid patients than to uninsured patients, we may expect Medicaid patients to have similar negative effects but in smaller magnitudes. The effects are estimated imprecisely due to the low time series variation in Medi-Cal rates but the effects are generally positive and as predicted much smaller than those of the uninsured.

In rows 11–12, I check the robustness of the effects across different diagnoses. I consider stroke and congested heart failure (CHF) mortality because they are two common reasons for admissions through ER, and like AMI mortality, they are included in the inpatient quality indicators developed by Agency for Health Research and Quality (2007). Although the results for CHF mortality are generally insignificant, estimated effects are positive for both health outcomes. This suggests that the spillover effects of the uninsured are not particular to heart attack patients.

Finally, in row 13 I check the sensitivity of the results to controlling for the charges of uninsured patients. I argue that one mechanism by which uninsured patients could plausibly affect the quality of the care provided to insured patients is through their uncompensated care costs. Thus, controlling for this variable should lead to a reduction in the estimated effects. Unfortunately, my dataset does not include information on the uncompensated part of the charges billed to patients, so I use the total amount of charges as a proxy for the costs incurred.[42] In accordance with the expectations,

[41] I also ran regressions that separately estimate the effects of uninsured patients with and without AMI. The precision of estimates is reduced but point estimates confirm prior expectations that the effects of uninsured AMI patients would be larger and the effects of uninsured non–AMI patients would be close to the original FE estimates. For example, the coefficient estimates for the 10/20–mile market definition are: uninsured AMI=0.552 (SE 1.217) uninsured non–AMI=0.055 (SE 0.048). Results for other market definitions are qualitatively similar and available upon request.

[42] Another method is to multiply the charges for each patient by the hospital cost-to-charge ratio to convert them into costs. This has two drawbacks. First, the cost-to-charge ratio is only available for the subset of hospitals that provide revenue information. Second, it assumes that the relative cost of care for an uninsured patient is the same

the estimated effects go down. Overall, the analyses in the section show that the effects of the uninsured are robust to a wide range of robustness checks.

5.4 Heterogeneous Effects

The main results of this paper suggest that uninsured patients have significant negative effects on the health outcomes of insured patients. However, these results do not provide information on how the impact differs across different types of health care providers. Although all types of health care providers render care to uninsured patients to some extent, the burden of uncompensated care is not evenly distributed. For example, Cunningham and Tu (1997) and Mann et al. (1997) report that urban public hospitals account for a third of all uncompensated care provided in the US while their market share is only about 15%. Similarly, Norton and Staiger (1994) find that for-profit hospitals locate in low-uninsurance areas to avoid providing charity care. Thus, in this section I estimate the effects of market uninsurance by hospital ownership and location.

The results are provided in table 18. Each row of the table presents the effects of uninsurance on the in-hospital mortality rate of insured AMI patients using a different market definition. Means of uninsurance rates by type of health care provider and market are shown in brackets below coefficient estimates. Column 1 reproduces the results for the full sample for reference. Columns 2–4 show the effects separately by hospital ownership. First, notice that public hospitals are located in areas with the highest uninsurance rates and not-for-profit hospitals are located in markets with the lowest uninsurance rates. The in-hospital mortality of insured heart attack patients are similar among public and for-profit hospitals while it is smaller among not-for-profit hospitals. Yet, the results suggest that market uninsurance affects public and not-for-profit hospitals while for-profit hospitals do not seem to be affected by it. The point estimates are positive and statistically significant in distance-based market measures for both public and not-for-profits hospitals. The magnitudes of the estimates in the not-for-profit sample are close to those in the full sample but the estimates in the public sample are much larger: a percentage point increase in the fraction of uninsured patients around a 10–mile radius of a hospital is found to increase the in-hospital AMI mortality among insured patients by 0.149 percentage points for public hospitals as opposed to 0.064 percentage points for not-for-profit hospitals (see row 43 of columns 2 and 3). The results regarding for-profit hospitals are qualitatively different with negative estimates, though they are less precise and always insignificant.

Columns 5–6 compare the effects of uninsured patients by hospital location. Notice that in this case the 10/20 and 15/25 mile market definitions only apply to rural hospitals as the radius of the

as for an insured one.

market for urban hospitals in these markets are again given by 10 and 15 miles. The results in the urban sample are qualitatively similar to those in the full sample but the estimated effects are somewhat larger. For rural hospitals, the results suggest statistically insignificant and quantitatively small effects in 5, 10 and 15 mile market definitions. The point estimates are much larger when the market is defined to be geographically more spacious but they are imprecise.

6 Interpretation

The estimated effects of uninsurance are in general statistically significant. In this section, I examine whether the effects are economically significant. My results indicate that eliminating uninsurance would reduce the number of insured heart attack deaths, in the average year from 1999 to 2006, by 125–200 depending on the market definition. These numbers roughly correspond to a 3–5% reduction in the total number of insured AMI deaths in the average year.

Another way to interpret the economic significance of uninsurance is to calculate its marginal cost of a statistical life year saved. This requires calculating the total number of life years saved from reducing uninsurance as well as the associated cost of such a reduction. The calculations for different market definitions are provided in table 19. To illustrate, I will focus on the 10/20 mile market definition. A 10 percentage point decline in market uninsurance reduces the in-hospital mortality rate of insured AMI patients by 0.61 percentage points. Since there are 159 insured heart attack patients in the average hospital, the resulting number of averted deaths is roughly 1 ($0.1 \cdot 0.061 \cdot 159$). The average age of a heart attack patient is 68[43] and the life expectancy of a 68–69 year-old male is around 14 years[44]. Similar to Doyle (2008), I will assume that individuals at risk of a heart attack have a lower life expectancy than the average and I will use 7 as the remaining number of years. This means the total number of years saved from reducing uninsurance by 10 percentage points is around 7.5 ($0.1 \cdot 0.061 \cdot 159 \cdot 7$).

On the cost side, a 10 percentage point reduction in uninsurance corresponds to insuring 557 patients in the average hospital.[45] Hadley et al. (2008) simulate the medical spending of an uninsured patient if they were insured and find an upper bound of $5,175 (2008 dollars). Hence, the cost of the reduction in uninsurance amounts to $288,000. The implied marginal cost of a statistical life year saved, then, is $38,093.

[43] My dataset includes intervalled age, so I obtain this number from the HCUP data, available at http://hcupnet.ahrq.gov/.

[44] Available from US Decennial Life Tables provided by http://www.cdc.gov/nchs/products/pubs/pubd/nvsr/nvsr.htm

[45] The average number of uninsured patients in the hospital is calculated as the number of uninsured in the market divided by the total number of hospitals in the market.

Overall, the estimated marginal cost of a statistical life year saved is calculated to be between \$38,093 and \$63,569. Previous estimates of the marginal cost of a statistical life year saved from pharmaceutical interventions and medical technology generally lie between \$50,000–\$100,000 and an additional year of life is valued at around \$100,000.[46] Although these back-of-the-envelope calculations are only approximations based on several assumptions, they suggest that reducing uninsurance may be a cost effective way of improving the health outcomes of heart attack patients.

7 Conclusion

One of the major challenges confronting US policy makers today is the persistently high uninsurance rate. Any attempt to take on the issue of uninsurance requires a solid understanding of its effects on society. While an extensive body of research examines how lack of insurance affects an individual's own health outcomes, health care access and utilization, there is very little evidence on how uninsurance affects the provision of care to the insured. Virtually all of the studies on this topic investigate the impact of the uninsured on the availability of health care services in their communities.

In this paper, I examine the effects of uninsured patients on the health outcomes of the insured. I focus on one measure of health outcome, the in-hospital mortality rate of insured heart attack patients, and implement panel data models using patient discharge data from California hospitals for the period 1999–2006. Overall, my results indicate that uninsured patients have substantial impacts on the health outcomes of insured heart attack patients. I supplement these results with several pieces of evidence suggesting that the estimated effects are not driven by unobserved characteristics of insured heart attack patients or hospitals in high uninsurance markets and that they are robust to a host of specification checks. I find that eliminating uninsurance would reduce the number of insured heart attack deaths, in the average year from 1999 to 2006, by 125–200 depending on the market definition. These numbers roughly correspond to a 3–5% reduction in the total number of insured AMI deaths in the average year. My back-of-the-envelope estimates place the marginal cost of a statistical life year saved from reducing uninsurance between \$38,093 and \$63,569, implying that reducing uninsurance can be a cost effective way of improving the health outcomes of the heart attack patients.

While this study has shown that the uninsured negatively affect the health outcomes of insured heart attack patients, the mechanisms through which this is happening are unclear. Hospitals may try to lower their costs by reducing capacity, services, staff or investments in equipment and

[46] For a review of these studies see Hall et al. (2008) and Doyle (2008).

technology. Future research would be needed to explore this in greater detail. It would also be interesting to investigate whether hospitals respond to the cost of treating uninsured patients with a new form of cost-shifting by overusing treatments with high reimbursement rates. This is left for future research.

References

Agency for Health Research and Quality (2007), "Guide to Inpatient Quality Indicators," Report.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber (2002), "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools," Working Paper.

——— (2005), "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools," *Journal of Political Economy* 113, 151–184.

Baicker, K., and D. Staiger (2005), "Fiscal Shenanigans, Targeted Federal Health Care Funds, and Patient Mortality," *Quarterly Journal of Economics* 120, 345–386.

Bazzoli, G. J., R. C. Lindrooth, R. Kang, and R. Hasnain-Wynia (2006), "The influence of health policy and market factors on the hospital safety net," *Health Services Research* 41, 1159–1180.

Burgess, Simon, Denise Gossage, and Carol Propper (2004), "Competition and Quality: Evidence from the NHS Internal Market 1991-1999," *Journal of Public Economics* 88, 1247–1272.

California Health Care Foundation (2006), "County Programs for the Medically Indigent in California," Facts Sheet.

California Healthcare Foundation (2007), "Snapshot: California's Uninsured 2007," Online brief avalailable at http://www.chcf.org/topics/healthinsurance/index.cfm?subsection=marketoverview.

Clement, Jan P. (1997-1998), "Dynamic Cost-Shifting in Hospitals: Evidence from the 1980s and 1990s," *Inquiry* 34, 340–350.

Cunningham, Peter J., and Ha T. Tu (1997), "A Changing Picture of Uncompensated Care," *Health Affairs* 16, 167–175.

Currie, Janet, and John Fahr (2004), "Hospitals, Managed Care, and the Charity Caseload in California," *Journal of Health Economics* 23, 421–442.

Cutler, David, Mark McClellan, and Joseph Newhouse (1999), *Measuring the Prices of Medical Treatments*, chap. The Costs and Benefits of Intensive Treatment for Cardiovascular Disease, 34–71, Washington, D.C.: The Brookings Institution.

Cutler, David M., and Ernst R. Berndt, eds. (2001), *Medical Care Output and Productivity*, University of Chicago Press.

Cutler, David M., Mark McClellan, and Joseph P. Newhouse (2001), "How Does Managed Care Do It?" *RAND Journal of Economics* 31, 526–548.

Dafny, Leemore (2005), "How Do Hospitals Respond to Price Changes?" *American Economic Review* 95, 1525–1547.

DesHarnais, Susan I., Laurence F. McMahon, Roger T. Wroblewski, and Andrew J. Hogan (1990), "Measuring Hospital Performance: The Development and Validation of Risk-Adjusted Indexes of Mortality, Readmissions, and Complications," *Medical Care* 28, 1127–1141.

Doyle, Joseph J. (2008), "Returns to Local-Area Emergency Health Care Spending: Using Health Shocks to Patients Far From Home," Working Paper.

Duggan, Mark (2000), "Hospital Ownership and Public Medical Spending," *Quarterly Journal of Economics* 115, 1343–1374.

Elixhauser, Anne, Claudia Steiner, D. Robert Harris, and Rosanna M. Coffey (1998), "Comorbidity Measures for Use with Administrative Data," *Medical Care* 36, 8–27.

Farsi, Mehdi (2004), "Changes in Hospital Quality after Conversion in Ownership Status," *International Journal of Health Care Finance and Economics* 4, 211–230.

Frostin, Paul (2007), "Sources of Health Insurance and Characteristics of the Uninsured: Analysis of the March 2007 Current Population Survey," EBRI Issue Brief No.310.

Garnick, Deborah W., Harold S. Luft, James C. Robinson, and Janice Tetrault (1987), "Appropriate Measures of Hospital Market Areas," *Health Services Research* 22, 69–89.

Gaskin, Darrell J., and Jack Needleman (2003), "The Impact of Uninsured Populations on the Availability of Hospital Services and Financial Status of Hospitals in Urban Areas," in *A Shared Destiny: Effects of Uninsurance on Individuals, Families, and Communities*, National Academy of Sciences.

Gowrisankaran, Gautam, and Robert Town (2003), "Competition, Payer and Hospital Quality," *Health Services Research* 38, 1403–1421.

Greene, William H. (2003), *Econometric Analysis*, Prentice Hall.

Gruber, Jonathan (1994), "The Effect of Competitive Pressure on Charity: Hospital Responses to Price Shopping in California," *Journal of Health Economics* 13, 183–211.

——— (2008), "Covering the Uninsured in the United States," *Journal of Economic Literature* 46, 571606.

Gruber, Jonathan, and David Rodriguez (2007), "How Much Uncompensated Care Do Doctors Provide?" *Journal of Health Economics* 26, 1151–1169.

Hadley, Jack, John Holahan, Teresa Coughlin, and Dawn Miller (2008), "Covering The Uninsured In 2008: Current Costs, Sources Of Payment, And Incremental Costs," *Health Affairs* Web Exclusive, w399–w145.

Hall, Emma, Carol Propper, and John Van Reenen (2008), "Can Pay Regulation Kill? Panel Data Evidence On The Effect Of Labor Markets On Hospital Performance," NBER Working Paper No. 13776.

Ho, Vivian, and Barton H. Hamilton (2000), "Hospital Mergers and Acquisitions: Does Market Consolidation Harm Patients?" *Journal of Health Economics* 19, 767–791.

Huen, William (1999), "Californias Disproportionate Share Hospital Program," Medi-Cal Policy Institute Background Paper.

Institute of Medicine (2002), *Care Without Coverage: Too Little, Too Late*, National Academy of Sciences.

——— (2003), *A Shared Destiny: Community Effects of Uninsurance*, National Academy of Sciences.

Insure the Uninsured Project (1998), "California'S Uninsured: Programs, Funding And Policy Options," Background Paper.

——— (2007), "A Summary of Health Care Financing for Low-Income Individuals in California, 1998 to 2007," Background Paper.

Kelch, Deborah Reidy (2004), "The Crucial Role of Counties in the Health of Californians: An Overview," California Health Care Foundation Background Paper.

——— (2005), "Caring for Medically Indigent Adults in California: A History," California Health Care Foundation Background Paper.

Lindrooth, R. C., G. J. Bazzoli, J. Needleman, and R. Hasnain-Wynia (2006), "The effect of changes in hospital reimbursement on nurse staffing decisions at safety net and nonsafety net hospitals," *Health Services Research* 41, 701–720.

Maddala, Gangadharrao Soundalyarao (1983), *Limited Dependent and Qualitative Variables in Econometrics*, Cambridge University Press.

Mann, Joyce M., Glenn A. Melnick, Anil Bamezai, and Jack Zwanziger (1997), "A Profile of Uncompensated Hospital Care, 1983–1995," *Health Affairs* 16, 223–232.

McClellan, Mark, and Douglas Staiger (1999), "The Quality of Health Care Providers," NBER Working Paper No. 7327.

McClellan, Mark B., Nathan Every, Alan M. Garber, Paul Heidenreichand Mark Hlatky, Daniel P. Kessler, Joseph Newhouse, and Olga Saynina (2002), *Technological Change in Health Care*, chap. Technological Change in Heart Attack Care in the United States, 21–54, University of Michigan Press.

McClellan, Mark B., and Daniel P. Kessler (2002), *Technological Change in Health Care: A Global Analysis of Heart Attack*, University of Michigan Press.

Morrisey, Michael A. (1993), "Hospital Pricing: Cost-Shifting and Competition," EBRI Issue Brief No. 137.

——— (1996), "Hospital Cost Shifting, A Continuing Debate," EBRI Issue Brief No. 180.

Murphy, Kevin M., and Robert H. Topel (1990), *Advances in the Theory and Measurement of Unemployment*, chap. Efficiency Wages Reconsidered: Theory and Evidence, 204–240, New York: St. Martin's Press.

Murphy, Kevin M., and Robert H. Topel, eds. (2003), *Measuring the Gains from Medical Research: An Economic Approach*, University of Chicago Press.

Needleman, Jack, and Darrell J. Gaskin (2003), "The Impact of Uninsured Discharges on the Availability of Hospital Services and Hospital Margins in Rural Areas," in *A Shared Destiny: Effects of Uninsurance on Individuals, Families, and Communities*, National Academy of Sciences.

Norton, Edward C., and Douglas O. Staiger (1994), "How hospital ownership affects access to care for the uninsured," *RAND Joumal of Economics* 25, 171–185.

Pagán, José A., and Mark V. Pauly (2006), "Community-Level Uninsurance and the Unmet Medical Needs of Insured and Uninsured Adults," *Health Services Research* 41, 788–803.

Pagán, José A., Mark V. Pauly, Damian Damianovand Lakshmi Sheshadri, and Norma A. Padrón (2007), "Community Uninsurance Spillovers and the Quality of Specialized Health Care," Working Paper.

Papke, Leslie, and Jeffrey M. Wooldridge (2007), "Panel Data Methods for Fractional Response Variables with an Application to Test Pass Rates," Working Paper.

Pauly, Mark V., and José A. Pagán (2007), "Spillovers And Vulnerability: The Case Of Community Uninsuranee," *Health Affairs* 26, 1304–1314.

Price Waterhouse Coopers (2007), "The Financial Health of California Hospitals," California Health Care Foundation Background Paper.

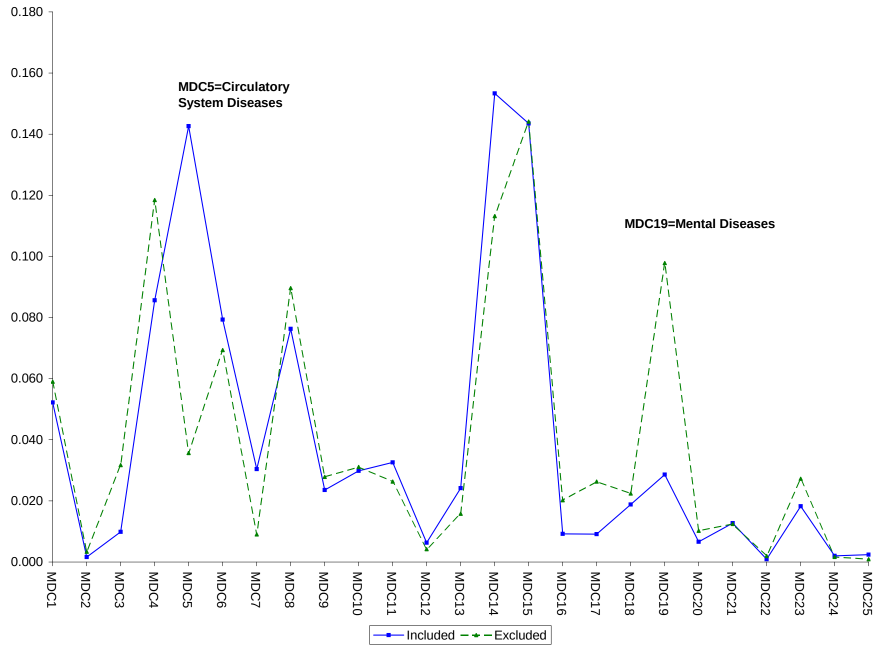
Propper, Carol, Simon Burgess, and Denise Gossage (2007), "Competition And Quality: Evidence From The Nhs Internal Market 199199," *Economic Journal* 118, 138–170.

Shen, Yu-Chu (2002), "The Effect of Hospital Ownership Choice on Patient Outcomes After Treatment for Acute Myocardial Infraction," *Journal of Health Economics* 21, 901–922.

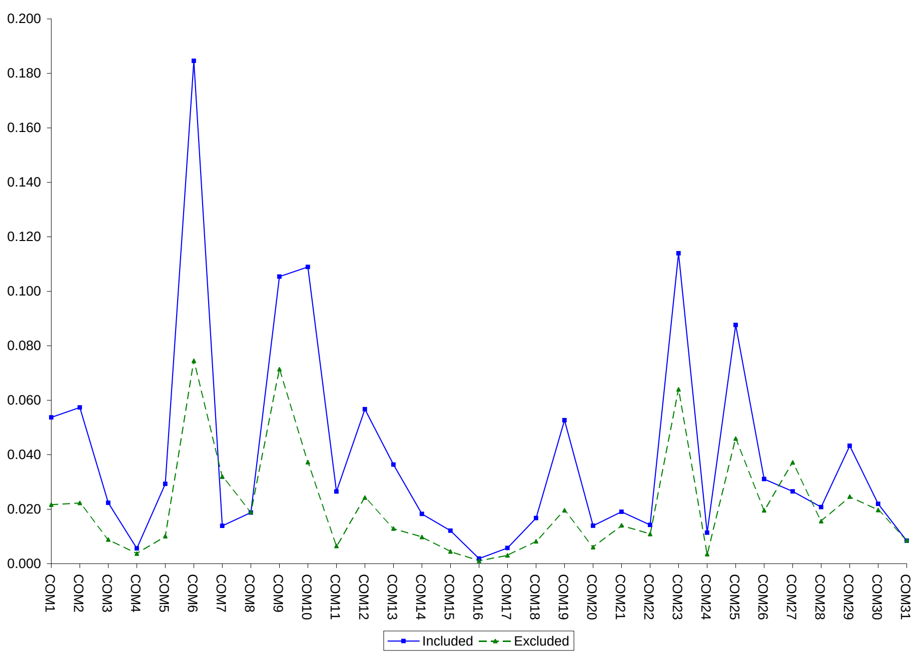
——— (2003), "The Effect of Financial Pressure on the Quality of Care in Hospitals," *Journal of Health Economics* 22, 243–269.

Spertus, John A., Martha J. Radford, Nathan R. Every, Edward F. Ellerbeck, Eric D. Peterson, and Harlan M. Krumholz (2003), "Challenges and Opportunities in Quantifying the Quality of Care for Acute Myocardial Infarction," *Circulation* 107, 1681–1691.

Thomas, J.W., J. J. Holloway, and K. J. Guire (1993), "Validating Risk-Adjusted Mortality as an Indicator for Quality of Care," *Inquiry* 30, 6–22.

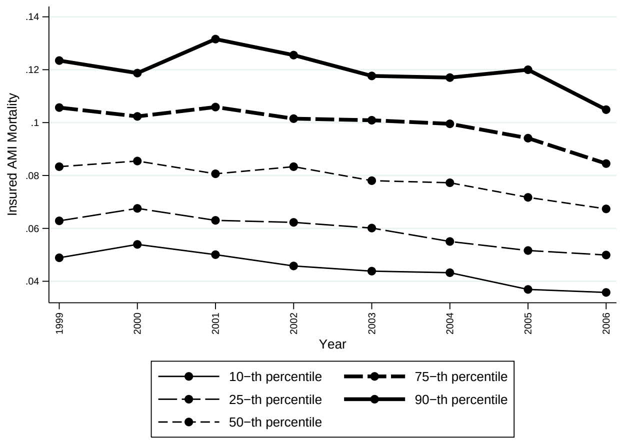


(a) Major Diagnostic Categories

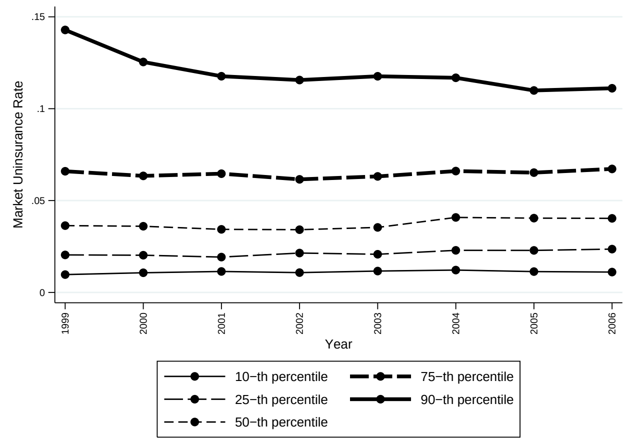


(b) Elixhauser Comorbidities

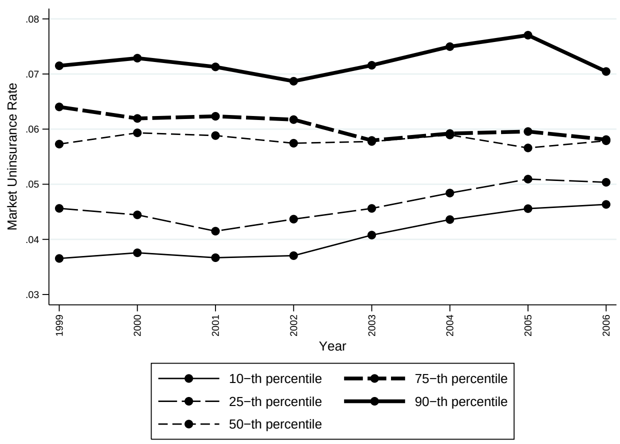
Figure 1: Distribution of Clinical Variables in Included and Excluded Hospitals



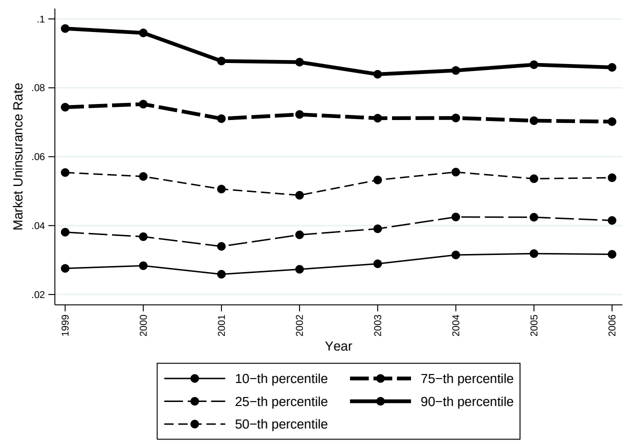
(a) Insured AMI mortality



(b) UNINS: Hospital

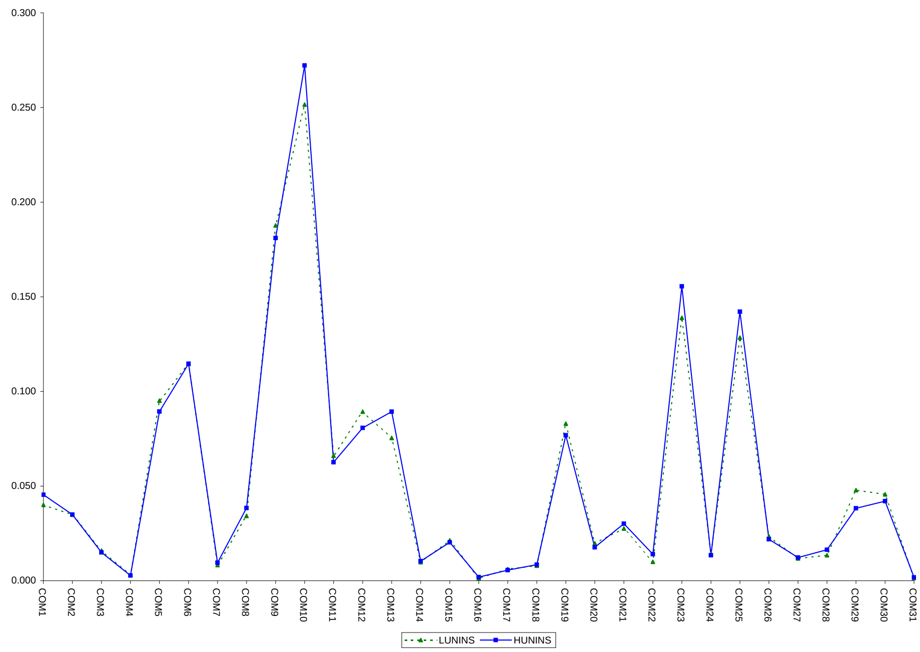


(c) UNINS: County

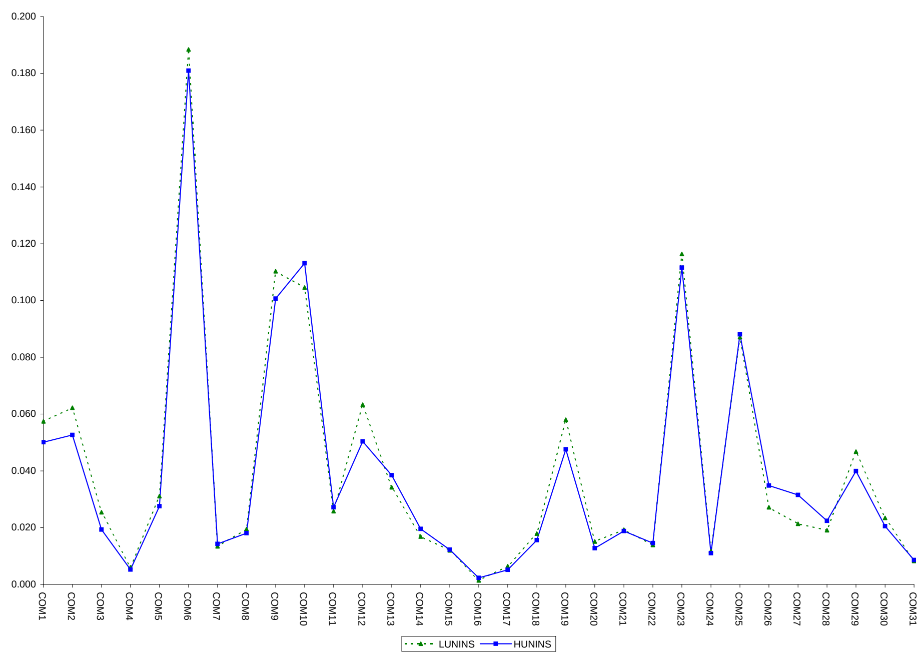


(d) UNINS: 10/20 miles

Figure 2: Variation in Insured AMI Mortality and Uninsurance Rate

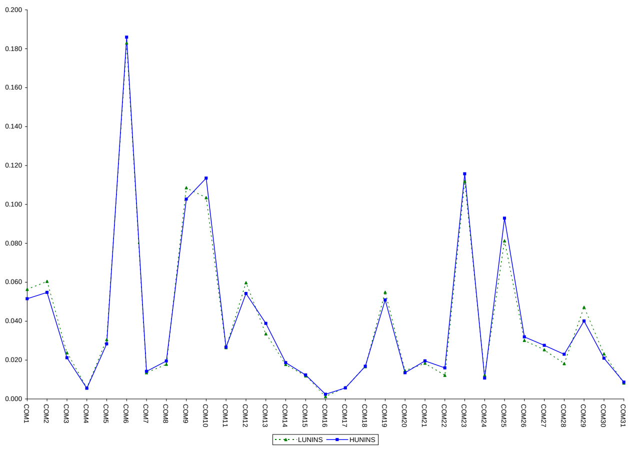


(a) County

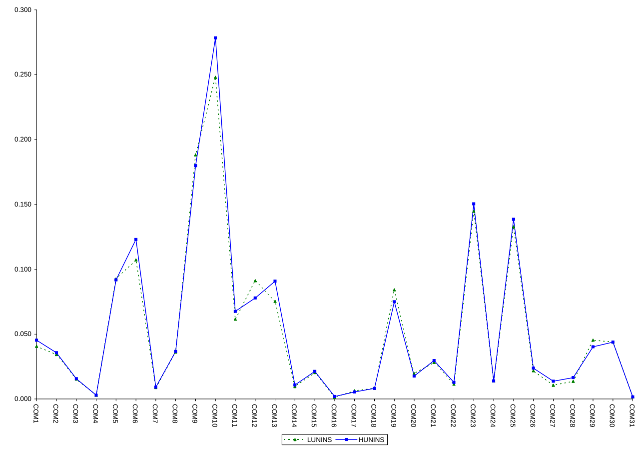


(b) 10/20 miles

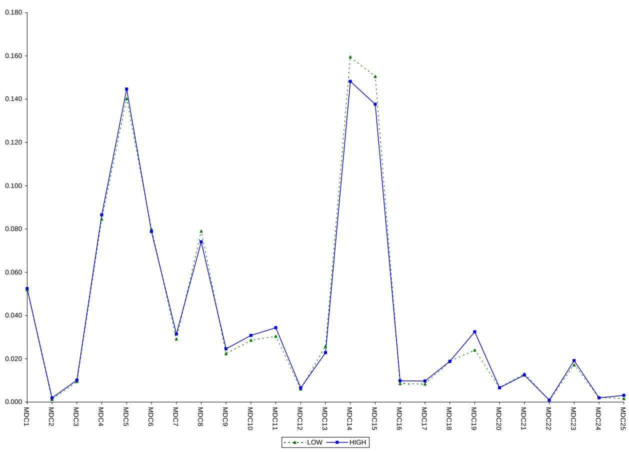
Figure 3: Comorbidities Among Insured AMI Patients in High and Low Uninsurance Areas



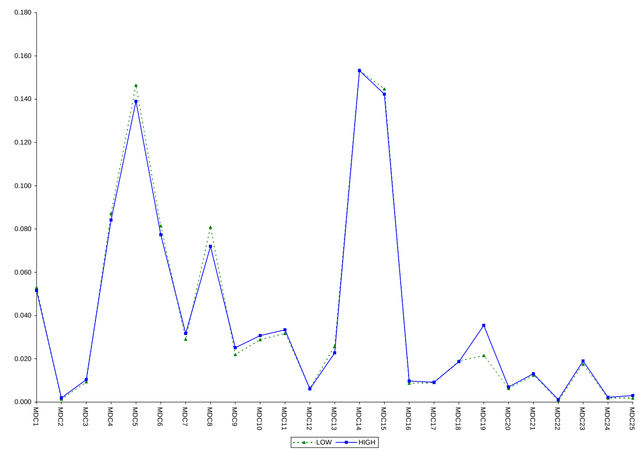
(a) Comorbidities: County



(b) Comorbidities: 10/20 miles



(c) MDCs: County



(d) MDCs: 10/20 miles

Figure 4: Clinical Variables Among All Patients in High and Low Uninsurance Areas

Table 1: California Counties

Rural CMSP Counties		Urban MISP Counties	
Alpine	Mendocino	Alameda	Santa Barbara
Amador	Modoc	Contra Costa	Santa Clara
Butte	Mono	Fresno	Santa Cruz
Calaveras	Napa	Kern	Stanislaus
Colusa	Nevada	Los Angeles	Tulare
Del Norte	Plumas	Merced	Ventura
El Dorado	San Benito	Monterey	Yolo
Glenn	Shasta	Orange	
Humboldt	Sierra	Placer	
Imperial	Siskiyou	Riverside	
Inyo	Solano	Sacramento	
Kings	Sonoma	San Bernardino	
Lake	Sutter	San Diego	
Lassen	Tehama	San Francisco	
Madera	Trinity	San Joaquin	
Marin	Tuolumne	San Luis Obispo	
Mariposa	Yuba	San Mateo	

Table 2: California County Medical Indigent Programs

	CMSP	MISP		CMSP	MISP
Age Requirement			***Acute inpatient hospital care***		
None	0	9	Yes	34	11
21 to 64 years	34	13	Yes (limited)	0	12
18 to 64 years	0	1	No	0	0
Max income threshold			***Emergency room care***		
Below 200% of FPL	0	11	Yes	34	11
Up to 200% of FPL	34	6	Yes (limited)	0	12
Above 200% of FPL	0	6	No	0	0
Can qualify with share of cost			***Emergency ambulance***		
Yes	0	19	Yes	34	7
No	34	4	Yes (limited)	0	10
Residency requirement			No	0	6
County resident (ILL receive ER)	34	2	***Hospital outpatient services***		
County resident (ILL may qualify)	0	10	Yes	34	11
County resident (ILL not covered)	0	11	Yes (limited)	0	12
Medical need required to participate			No	0	0
Yes	0	16			
No	34	7			
Prior authorization for services					
Yes	34	21			
No	0	2			

Notes: CMSP=County Medical Service Program; MISP=Medically Indigent Service Program; FPL=Federal Poverty Line; ILL=Illegal Immigrants. Adapted from California Health Care Foundation (2006). Each entry represents the number of counties that have the corresponding eligibility requirement.

Table 3: Constructing the Analysis Sample

	1999	2000	2001	2002	2003	2004	2005	2006	Total
HOSPITALS									
Initial	511	502	484	480	477	466	457	453	521
Not general acute care	70	65	57	57	55	53	54	54	65
Exit/enter/gaps	57	53	43	39	38	29	19	15	72
Expected primary payer missing or misclassified	0	0	0	0	0	0	0	0	0
Admission year is more than 1 year prior to discharge year	0	0	0	0	0	0	0	0	0
No insured AMI discharge	46	44	46	48	49	53	50	51	32
Final	338	340	338	336	335	331	334	333	352
OBSERVATIONS									
Initial	3,775,711	3,816,887	3,864,090	3,916,363	3,979,625	3,957,640	3,990,255	3,997,182	31,297,753
Not general acute care	96,330	88,822	87,383	91,569	88,709	91,161	88,902	89,192	722,068
Exit/enter/gaps	188,661	143,725	138,280	124,029	110,527	91,556	65,324	59,995	922,097
Expected primary payer missing or misclassified	6,940	1,785	461	449	701	737	1,284	577	12,934
Admission year is more than 1 year prior to discharge year	843	850	825	909	906	826	870	889	6,918
Not insured AMI	3,426,553	3,527,344	3,582,122	3,644,488	3,723,622	3,721,052	3,783,508	3,798,588	29,207,277
Final	56,384	54,361	55,019	54,919	55,160	52,308	50,367	47,941	426,459

Table 4: Descriptive Statistics of Insured AMI Patients

	OBS	Mean	Sd		OBS	Mean	Sd
AMI mortality	2,685	0.082	0.034				
18–34 years old	2,685	0.006	0.006	Renal failure	2,685	0.082	0.046
35–64 years old	2,685	0.341	0.091	Liver disease	2,685	0.010	0.010
65 years and above	2,685	0.654	0.092	Peptic ulcer disease	2,685	0.021	0.016
Female	2,685	0.363	0.067	AIDS	2,685	0.002	0.004
Non-Hispanic White	2,685	0.579	0.221	Lymphoma	2,685	0.006	0.007
Non-Hispanic Black	2,685	0.041	0.107	Metastatic cancer	2,685	0.008	0.009
Hispanic	2,685	0.089	0.144	Solid tumor	2,685	0.080	0.033
Congestive heart failure	2,685	0.043	0.048	Rheumatoid arthritis	2,685	0.019	0.012
Cardiac arrhythmias	2,685	0.035	0.041	Coagulopathy	2,685	0.029	0.019
Valvular disease	2,685	0.015	0.021	Weight loss	2,685	0.012	0.019
Pulmonary circulation disorders	2,685	0.003	0.005	Fluid & electrolyte disorders	2,685	0.147	0.069
Peripheral vascular disorders	2,685	0.092	0.038	Blood loss anemia	2,685	0.014	0.012
Hypertension	2,685	0.115	0.137	Deficiency anemias	2,685	0.135	0.054
Paralysis	2,685	0.009	0.009	Alcohol abuse	2,685	0.023	0.016
Other neurological disorders	2,685	0.036	0.021	Drug abuse	2,685	0.012	0.015
Chronic pulmonary disease	2,685	0.184	0.056	Psychoses	2,685	0.015	0.014
Diabetes, uncomplicated	2,685	0.262	0.061	Depression	2,685	0.043	0.027
Diabetes, complicated	2,685	0.064	0.039	Complications	2,685	0.044	0.040
Hypothyroidism	2,685	0.085	0.034	Surgery	2,685	0.002	0.003

Notes: All values are weighted by the number of insured heart attack patients at the hospital.

Table 5: Observable Characteristics of Included and Excluded Hospitals

	Included Hospital-Year OBS			Excluded Hospital-Year OBS		
	OBS	Mean	Sd	OBS	Mean	Sd
PANEL A						
Fr(Uninsured): county	2,685	0.058	0.020	387	0.056	0.016
Fr(Uninsured): 5 miles	2,685	0.059	0.055	387	0.052	0.033
Fr(Uninsured): 10 miles	2,685	0.061	0.045	387	0.055	0.025
Fr(Uninsured): 15 miles	2,685	0.059	0.036	387	0.057	0.023
Fr(Uninsured): 10/20 miles	2,685	0.060	0.042	387	0.055	0.024
Fr(Uninsured): 15/25 miles	2,685	0.059	0.035	387	0.057	0.022
Fr(Uninsured): hospital	2,685	0.060	0.069	387	0.042	0.082
Fr(Medi-Cal): hospital	2,685	0.235	0.183	387	0.217	0.202
No. hospitals: county	2,685	34.388	38.853	387	30.646	34.493
No. hospitals: 5 miles	2,685	3.405	3.188	387	4.695	3.797
No. hospitals: 10 miles	2,685	7.896	7.827	387	9.514	8.452
No. hospitals: 15 miles	2,685	14.044	14.670	387	14.969	14.005
No. hospitals: 10/20 miles	2,685	8.167	7.715	387	9.654	8.407
No. hospitals: 15/25 miles	2,685	14.528	14.439	387	15.147	13.903
Public	2,685	0.189	0.391	387	0.178	0.383
Not-for-profit	2,685	0.575	0.494	387	0.411	0.493
For-profit	2,685	0.236	0.425	387	0.411	0.493
Urban	2,685	0.819	0.385	387	0.827	0.379
Rural	2,685	0.181	0.385	387	0.173	0.379
Total discharges	2,685	10656	8271	387	2641	4302
PANEL B						
Less than 1 year	2,685	0.155	0.073	385	0.257	0.174
1–17 years old	2,685	0.038	0.033	385	0.375	0.312
18–34 years old	2,685	0.183	0.061	385	0.129	0.141
35–64 years old	2,685	0.299	0.081	385	0.142	0.201
65 and above	2,685	0.324	0.130	385	0.096	0.235
Female	2,685	0.605	0.052	383	0.539	0.141
Non-Hispanic White	2,685	0.567	0.260	381	0.471	0.243
Non-Hispanic Black	2,685	0.076	0.108	381	0.080	0.110
Hispanic	2,685	0.279	0.225	381	0.357	0.218

Notes: Values in Panel A are unweighted while values in Panel B are weighted by total discharges at the hospital.
* The number of observations vary when hospitals have all of their observations masked.

Table 6: Observable Characteristics of High and Low Uninsurance Hospitals

	County		10/20 miles	
	Low	High	Low	High
	(1)	(2)	(3)	(4)
Uninsurance	0.046	0.069	0.038	0.083
AMI mortality	0.079	0.085	0.082	0.082
Insured AMI Patients				
35–64 years old	0.354	0.328	0.336	0.347
65 and above	0.640	0.666	0.659	0.647
Female	0.360	0.365	0.367	0.357
White	0.624	0.535	0.656	0.490
Black	0.030	0.051	0.030	0.053
Hispanic	0.064	0.112	0.066	0.114
All Patients				
Less than 1	0.162	0.150	0.155	0.156
1–17 years old	0.037	0.039	0.034	0.042
18–34 years old	0.186	0.180	0.176	0.190
35–64 years old	0.295	0.303	0.291	0.308
65 and above	0.320	0.327	0.345	0.305
Female	0.612	0.599	0.611	0.599
White	0.634	0.512	0.671	0.468
Black	0.057	0.091	0.057	0.093
Hispanic	0.224	0.325	0.209	0.346
No. hospitals	11	56	6	10
Fr(Uninsured): hospital	0.050	0.070	0.043	0.077
Public	0.197	0.181	0.167	0.210
Not-for-profit	0.645	0.511	0.639	0.512
For-profit	0.158	0.307	0.194	0.278
Urban	0.761	0.873	0.827	0.811
Total discharges	10118	11150	10466	10845
AMI discharges	171	165	178	158
OBS	1285	1400	1338	1347

Notes: The sample is split into two using the median market uninsurance rate in each year. Columns 1–2 are based on the county market measure while columns 3–4 use the 10/20 mile market uninsurance. Each cell represents the mean of the variable in that sample. AMI mortality and characteristics of insured AMI patients are weighted by insured AMI discharges, characteristics of overall patients are weighted by total discharges and hospital characteristics are unweighted.

Table 7: The Effects of Uninsurance on In-Hospital Mortality Rate of Insured AMI Patients

	Mean Uninsurance	OLS (1)	FE (2)	Grouped Logit (3)	Fractional Logit (4)	Hausman Test (5)	Drop Lowest Quartile OLS (6)	Drop Lowest Quartile FE (7)	Long Differences (8)
UNINS (hospital)	0.060	0.035**	0.061**	0.070***	0.052**	0.225	0.072**	0.035**	0.053*
	(0.069)	(0.016)	(0.026)	(0.292)	(0.308)		(0.028)	(0.017)	(0.028)
UNINS (county)	0.058	0.046	0.042	0.068*	0.048	0.902	0.026	0.048	0.017
	(0.020)	(0.029)	(0.045)	(0.460)	(0.558)		(0.140)	(0.036)	(0.039)
UNSIN (5 miles)	0.059	0.010	0.047*	0.057*	0.041*	0.086	0.046	0.010	0.042
	(0.055)	(0.015)	(0.027)	(0.356)	(0.339)		(0.033)	(0.016)	(0.027)
UNINS (10 miles)	0.061	0.018	0.063**	0.070***	0.057**	0.006	0.057	0.015	0.047*
	(0.045)	(0.019)	(0.025)	(0.289)	(0.314)		(0.036)	(0.019)	(0.027)
UNINS (15 miles)	0.059	0.022	0.054**	0.063**	0.053**	0.026	0.057*	0.015	0.055**
	(0.036)	(0.021)	(0.026)	(0.327)	(0.325)		(0.033)	(0.021)	(0.027)
UNINS (10/20 miles)	0.060	0.013	0.068***	0.074***	0.061***	0.001	0.059	0.013	0.052*
	(0.042)	(0.018)	(0.025)	(0.277)	(0.311)		(0.036)	(0.021)	(0.027)
UNINS (15/25 miles)	0.059	0.028	0.060**	0.070***	0.058**	0.017	0.061*	0.026	0.043
	(0.035)	(0.022)	(0.026)	(0.298)	(0.320)		(0.033)	(0.021)	(0.027)
OBS		2685	2685	2410	2685		337	Various	Various

Notes: Each row represents the effect of uninsurance on the in-hospital mortality rate of insured AMI patients using a different market definition. Columns 1–4 provide marginal effects from different estimation methods. Each regression includes demographic and clinical characteristics of insured heart attack patients in the hospital, demographic and clinical characteristics of all the patients in the hospital, basic hospital demographics and year effects (see section 4.3). Columns 2–4 also include hospital fixed effects. Column 5 provides p-values from a Hausman test of endogeneity. Column 6 estimates a long difference model using 3 year weighted averages (see section 5.1). Columns 7–8 drop observations in the lowest quartile of the distribution of the variance of the uninsurance measure. Robust standard errors clustered at the county level are shown in parenthesis below coefficients. Regressions in columns 1, 2, 4, 7 and 8 are weighted by the number of insured AMI discharges at the hospital. Regressions in grouped logit specifications are weighted by $\left(n \cdot \widehat{Y} \cdot (1-\widehat{Y})\right)$ where n is the number of insured AMI discharges at the hospital, $\widehat{Y}$ is the predicted probability of death obtained from a first stage unweighted regression of the log–odds ratio of Y as described in text (see section 3). Regressions in column 6 are weighted by the average number of insured AMI discharges at the hospital during 1999–2001 and 2004–2006.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Changes in Uninsurance in Hospitals and Health Care Markets: 1999–2006

	Hospital (1)	County (2)	5 miles (3)	10 miles (4)	15 miles (5)	10/20 miles (6)	15/25 miles (7)
Sample Hospitals							
ΔPredUninsured	1.301***	0.922***	1.088***	1.143***	1.161***	1.141***	1.157***
	(0.060)	(0.008)	(0.031)	(0.032)	(0.025)	(0.032)	(0.024)
	[24.79]	[88.55]	[8.121]	[19.968]	[41.829]	[19.846]	[41.032]
ΔPredFr(Uninsured)	1.591***	1.048***	1.291***	1.311***	1.316***	1.310***	1.324***
	(0.066)	(0.020)	(0.044)	(0.034)	(0.025)	(0.033)	(0.024)
	[78.94]	[5.95]	[43.87]	[85.08]	[161.60]	[86.90]	[178.91]
OBS	327	327	327	327	327	327	327
All Hospitals							
ΔPredUninsured	1.300***	0.923***	1.072***	1.137***	1.149***	1.133***	1.146***
	(0.056)	(0.008)	(0.030)	(0.029)	(0.023)	(0.028)	(0.022)
	[28.26]	[99.33]	[5.83]	[22.92]	[43.69]	[22.28]	[43.01]
ΔPredFr(Uninsured)	1.415***	1.042***	1.253***	1.261***	1.265***	1.265***	1.278***
	(0.072)	(0.018)	(0.045)	(0.039)	(0.034)	(0.037)	(0.033)
	[33.38]	[5.67]	[31.45]	[45.75]	[59.48]	[50.96]	[71.47]
OBS	384	384	384	384	384	384	384

Notes: The first panel includes the 327 of 352 hospitals in the analysis sample that provided care to insured AMI patients in both 1999 and 2006. The second panel includes all general acute care hospitals operating from 1999 through 2006. In all specifications the outcome variable represents the actual change between 1999 and 2006 and the predicted change in the outcome during the same period is used to explain the actual change. F–statistics from a Wald test that the estimated coefficient is equal to 1 is provided in brackets.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Changes in Predicted Uninsurance by Quality of Care: 1999–2006

	All (1)	Low Quality (2)	Mid Quality (3)	Top Quality (4)
ΔPredUninsured (county)	−1331.51	−2052.38	−967.85	−723.33
	(3216.06)	(3432.83)	(2962.53)	(2974.13)
	[22332.14]	[28837.00]	[17781.98]	[17842.44]
ΔPredUninsured (5 miles)	−97.40	−132.74	−125.63	−31.35
	(715.84)	(689.16)	(825.64)	(655.09)
	[2103.74]	[2301.37]	[2171.99]	[1804.82]
ΔPredUninsured (10 miles)	−174.97	−262.47	−179.34	−62.96
	(1065.51)	(1117.68)	(1001.43)	(1047.23)
	[4851.03]	[6078.98]	[3728.44]	[4210.99]
ΔPredUninsured (15 miles)	−380.09	−563.17	−309.10	−208.81
	(1561.77)	(1634.51)	(1480.59)	(1521.18)
	[8648.85]	[10853.21]	[7260.42]	[7006.55]
ΔPredUninsured (10/20 miles)	−188.07	−274.14	−176.34	−90.51
	(1085.77)	(1133.26)	(1003.14)	(1089.24)
	[4948.21]	[6162.91]	[3743.54]	[4389.19]
ΔPredUninsured (15/25 miles)	−382.83	−569.94	−304.12	−212.64
	(1581.88)	(1655.26)	(1483.32)	(1553.39)
	[8786.86]	[11004.43]	[7281.13]	[7220.51]
ΔPredFr(Uninsured) (county)	−0.003	−0.004	−0.005	−0.002
	(0.02)	(0.02)	(0.02)	(0.02)
	[0.057]	[0.058]	[0.057]	[0.057]
ΔPredFr(Uninsured) (5 miles)	−0.004	−0.005	−0.004	−0.003
	(0.03)	(0.03)	(0.02)	(0.02)
	[0.060]	[0.061]	[0.064]	[0.057]
ΔPredFr(Uninsured) (10 miles)	−0.004	−0.005	−0.004	−0.002
	(0.02)	(0.03)	(0.02)	(0.02)
	[0.060]	[0.064]	[0.057]	[0.058]
ΔPredFr(Uninsured) (15 miles)	−0.004	−0.004	−0.004	−0.002
	(0.02)	(0.03)	(0.02)	(0.02)
	[0.058]	[0.059]	[0.057]	[0.058]
ΔPredFr(Uninsured) (10/20 miles)	−0.004	−0.006	−0.004	−0.003
	(0.02)	(0.03)	(0.02)	(0.02)
	[0.060]	[0.064]	[0.057]	[0.058]
ΔPredFr(Uninsured) (15/25 miles)	−0.003	−0.005	−0.004	−0.002
	(0.02)	(0.03)	(0.02)	(0.02)
	[0.058]	[0.059]	[0.057]	[0.058]
OBS	327	134	85	108

Notes: Each row in the first panel presents the mean and standard deviation of the change in the predicted number of patients in the market. The mean number of uninsured patients in each market in 1999 is provided in brackets. The second panel provides the same statistics for the change in the fraction of uninsured patients. Columns provide descriptive statistics separately by quality of care provided in the hospital. High quality hospitals are those located in the bottom tercile of the insured AMI mortality distribution in 1999, while low uninsurance hospitals are those in the top tercile.

Table 10: Changes in Market Level Uninsurance Rates by Quality of Care

	Hospital (1)	County (2)	5 miles (3)	10 miles (4)	15 miles (5)	10/20 miles (6)	15/25 miles (7)
$LowQuality99 \cdot t$	-0.001	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$MidQuality99 \cdot t$	0.000	-0.000	-0.000	0.000	0.000	0.001	0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
t	0.000	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
OBS	2640	2640	2640	2640	2640	2640	2640

Notes: Each column represents a separate market definition. Specifications regress the fraction of uninsured patients in a market on a time trend, its interaction with low and medium quality dummy variables and observable characteristics of insured AMI patients and hospitals. For details see section 5.2. The number of observations is 2640 as the analysis applies to hospitals that did provide care to insured AMI patients in 1999. Each regression also controls for time-invariant hospital effects. Robust standard errors clustered at the county level are shown in parenthesis below coefficients.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Changes in the Prevalence of Comorbidities Among Insured AMI Patients by Uninsurance Rate (10/20 miles)

	Com1 (1)	Com2 (2)	Com3 (3)	Com4 (4)	Com5 (5)	Com6 (6)	Com7 (7)	Com8 (8)	Com9 (9)	Com10 (10)
$TopU99 \cdot t$	0.000	−0.001	−0.001	−0.000	−0.002	0.000	0.000	−0.000	−0.001	−0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.004)	(0.000)	(0.000)	(0.001)	(0.001)
$MidU99 \cdot t$	0.001	0.000	−0.000	0.000	−0.002	0.003	−0.000	−0.000	−0.000	0.001
	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.005)	(0.000)	(0.000)	(0.001)	(0.001)
t	0.011***	0.010***	0.005***	0.001***	0.006***	0.032***	−0.000	−0.000	0.003***	0.005***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.003)	(0.000)	(0.000)	(0.001)	(0.001)

	Com11 (11)	Com12 (12)	Com13 (13)	Com14 (14)	Com15 (15)	Com16 (16)	Com17 (17)	Com18 (18)	Com19 (19)	Com20 (20)
$TopU99 \cdot t$	0.002*	−0.001*	0.001*	−0.000	0.001	0.000*	−0.000**	0.000	−0.002***	0.001**
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
$MidU99 \cdot t$	0.000	−0.001	0.001	0.000	0.001*	0.000	−0.000	0.000	0.000	−0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
t	0.003***	0.005***	0.008***	0.001***	−0.000	−0.000	0.000***	0.000***	0.004***	0.000**
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)

	Com21 (21)	Com23 (22)	Com24 (23)	Com25 (24)	Com26 (25)	Com27 (26)	Com28 (27)	Com29 (28)	Com30 (29)	Comp. (30)
$TopU99 \cdot t$	−0.001***	−0.000	−0.002	−0.000	−0.002*	0.001**	0.001***	0.000	−0.001	0.000
	(0.000)	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
$MidU99 \cdot t$	−0.001*	−0.000	−0.004***	−0.000	−0.001	0.001**	0.000*	0.000	−0.001	0.000
	(0.000)	(0.000)	(0.002)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
t	0.002***	0.001***	0.010***	0.001***	0.005***	0.001**	0.001***	0.001***	0.005***	−0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)

Notes: Each cell represents the coefficient from a separate regression, so a total of 30 regressions are summarized. Specifications regress the fraction of insured AMI patients with a given comorbidity on a time trend and its interaction with high and medium uninsurance dummy variables controlling for observable characteristics of insured AMI patients and hospitals. For details see section 5.2. The number of observations is 2640. Each regression also controls for time–invariant hospital effects. Robust standard errors clustered at the county level are shown in parenthesis below coefficients. Regressions are weighted by the number of insured AMI discharges at the hospital.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: Changes in Hospitals' Insured and Insured AMI Admissions: 1999–2006

	Sample Hospitals		All Hospitals	
	ΔInsured (1)	ΔAMI insured (2)	ΔInsured (3)	ΔAMI insured (4)
ΔPredicted	0.894***	1.054***	0.913***	1.038***
	(0.107)	(0.108)	(0.097)	(0.096)
OBS	327	327	384	384
p-value (coef=1)	0.321	0.617	0.370	0.695
F-stat (coef=1)	0.987	0.251	0.806	0.154

Notes: The first two specifications include the 327 of 352 hospitals in the analysis sample that provided care to insured AMI patients in both 1999 and 2006. The last two specifications include all general acute care hospitals operating from 1999 through 2006. In all specifications the outcome variable represents the actual change between 1999 and 2006 and the predicted change in the outcome during the same period is used to explain the actual change. The bottom panel includes the p–values and F–statistics from a Wald test that the estimated coefficient is equal to 1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: Evidence on Sorting of Patients: Summary of the Murphy and Topel (1990) Model

	Hospital (1)	County (2)	5–miles (3)	10–miles (4)	15–miles (5)	10/20–miles (6)	15/25–miles (7)
All Variables							
No of vars	124	124	124	125	125	125	125
% same sign	45	62	48	50	50	51	52
AMI Variables							
No of vars	42	42	42	42	42	42	42
% same sign	36	55	43	50	43	50	50
Clinical AMI Variables							
No of vars	31	31	31	31	31	31	31
% same sign	35	52	39	48	39	45	45

Notes: Each column represents a separate market definition. The rows summarize the findings from the Murphy and Topel (1990) sorting model described in section 5.2. They all refer to the same specification but provide information on different subsets of variables. For description of the variables included, see text and section 4.3.

Table 14: Changes in Prevalence of Comorbidities Among Insured AMI Patients by Quality of Care

	Com1 (1)	Com2 (2)	Com3 (3)	Com4 (4)	Com5 (5)	Com6 (6)	Com7 (7)	Com8 (8)	Com9 (9)	Com10 (10)
$LowQuality99 \cdot t$	0.001	−0.001	−0.000	−0.000	0.000	−0.001	−0.000	−0.001***	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.001)	(0.001)
$MidQuality99 \cdot t$	0.002*	0.000	0.001*	0.000	−0.001	0.006*	−0.000*	−0.000	−0.000	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.003)	(0.000)	(0.000)	(0.001)	(0.001)
t	0.007***	0.007***	0.003***	0.001***	0.002***	0.020***	−0.000**	0.001	−0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.003)	(0.000)	(0.001)	(0.001)	(0.001)
	Com11 (11)	Com12 (12)	Com13 (13)	Com14 (14)	Com15 (15)	Com16 (16)	Com17 (17)	Com18 (18)	Com19 (19)	Com20 (20)
$LowQuality99 \cdot t$	0.000	0.000	−0.001	0.000	−0.000	0.000**	0.000	−0.000	0.000	−0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$MidQuality99 \cdot t$	−0.000	−0.000	−0.001***	0.000	0.000	0.000*	0.000**	−0.000	0.000	−0.000
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
t	0.002***	0.001	0.006***	0.001**	0.000	−0.000*	−0.000	0.001**	0.002**	0.000
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)
	Com21 (21)	Com23 (22)	Com24 (23)	Com25 (24)	Com26 (25)	Com27 (26)	Com28 (27)	Com29 (28)	Com30 (29)	Comp. (30)
$LowQuality99 \cdot t$	−0.000	−0.000	−0.001	−0.000	0.001	0.000	0.000*	−0.000	0.000	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$MidQuality99 \cdot t$	−0.000	−0.000	−0.000	−0.000**	0.001	−0.000	0.000**	−0.000	−0.001	−0.001
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
t	0.001**	0.001***	0.001	0.000	−0.001	0.001	0.001**	−0.000	0.002**	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)

Notes: Each cell represents the coefficient from a separate regression, so a total of 30 regressions are summarized. Specifications regress the fraction of insured AMI patients with a given comorbidity on a time trend and its interaction with low and medium quality dummy variables controlling for observable characteristics of insured AMI patients and hospitals. For details see section 5.2. The number of observations is 2640. Each regression also controls for time–invariant hospital effects. Robust standard errors clustered at the county level are shown in parenthesis below coefficients. Regressions are weighted by the number of insured AMI discharges at the hospital.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 15: Changes in Average Length of Stay of Insured AMI Patients by Uninsurance Rate

	Hospital (1)	County (2)	5–miles (3)	10–miles (4)	15–miles (5)	10/20–miles (6)	15/25–miles (7)
$TopU99 \cdot t$	0.024	−0.013	−0.021	−0.020*	−0.016	−0.012	−0.018
	(0.022)	(0.019)	(0.021)	(0.011)	(0.020)	(0.012)	(0.019)
$MidU99 \cdot t$	0.036*	−0.005	−0.012	−0.035**	−0.014	−0.036**	−0.013
	(0.020)	(0.027)	(0.018)	(0.017)	(0.019)	(0.017)	(0.019)
t	−0.092***	−0.069***	−0.068***	−0.058**	−0.062***	−0.060**	−0.062***
	(0.023)	(0.024)	(0.024)	(0.024)	(0.022)	(0.025)	(0.022)
OBS	2640	2640	2640	2640	2640	2640	2640

Notes: Each column represents a separate market definition. Specifications regress the average length of stay of insured AMI patients in a hospital on a time trend, its interaction with high and medium uninsurance rate dummy variables and observable characteristics of insured AMI patients and hospitals. For details see section 5.2. The number of observations is 2640 as the analysis applies to hospitals that did provide care to insured AMI patients in 1999. Each regression also controls for time-invariant hospital effects. Robust standard errors clustered at the county level are shown in parenthesis below coefficients.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Selection on Observed and Unobserved Variables

	Estimated Bias (1)	Coefficient of UNINS (2)	Bias Factor (3)
OLS Model			
Hospital	0.332	0.035	0.107
County	0.188	0.046	0.244
5 miles	0.171	0.009	0.052
10 miles	0.122	0.017	0.14
15 miles	0.087	0.021	0.243
10/20 miles	0.096	0.012	0.129
15/25 miles	0.091	0.027	0.296
FE Model			
Hospital	-0.156	0.061	-0.389
County	0.053	0.042	0.782
5 miles	-0.124	0.047	-0.377
10 miles	-0.096	0.063	-0.654
15 miles	0.107	0.054	0.507
10/20 miles	-0.097	0.068	-0.696
15/25 miles	0.107	0.06	0.563

Notes: The results are based on the framework developed by Altonji et al. (2005) model. The first panel conducts the analysis within the simple OLS framework, while the second panel is based on the FE models. Each row represents a separate market definition. For description of the columns see section 5.2.

Table 17: Robustness Checks

	Hospital (1)	County (2)	5–miles (3)	10–miles (4)	15–miles (5)	10/20–miles (6)	15/25–miles (7)
(1) Baseline Estimates	0.061**	0.042	0.047*	0.063**	0.054**	0.068***	0.060**
	(0.026)	(0.045)	(0.027)	(0.025)	(0.026)	(0.025)	(0.026)
(2) Has 30+ AMI Discharges	0.060***	0.041	0.055**	0.068***	0.060***	0.072***	0.067***
	(0.021)	(0.041)	(0.025)	(0.021)	(0.021)	(0.021)	(0.021)
(3) Has 150+ AMI Discharges	0.052**	0.069**	0.063**	0.088***	0.077***	0.086***	0.079***
	(0.024)	(0.030)	(0.026)	(0.020)	(0.019)	(0.020)	(0.019)
(4) Has Insured AMI Discharges Every Year	0.054**	0.039	0.045	0.061**	0.053**	0.066**	0.059**
	(0.025)	(0.043)	(0.027)	(0.025)	(0.026)	(0.025)	(0.026)
(5) Exclude Out Transfers	0.095**	0.064	0.068*	0.087**	0.071**	0.092**	0.078**
	(0.043)	(0.046)	(0.038)	(0.037)	(0.033)	(0.038)	(0.034)
(6) Exclude All Transfers	0.093**	0.045	0.055	0.081**	0.066**	0.088**	0.076**
	(0.041)	(0.047)	(0.043)	(0.035)	(0.032)	(0.037)	(0.033)
(7) Weighted Uninsurance			0.050**	0.057**	0.055*	0.059**	0.056*
			(0.025)	(0.027)	(0.029)	(0.027)	(0.029)
(8) Uninsurance (ER)	0.119**	0.155	0.078	0.104**	0.079	0.117**	0.099
	(0.056)	(0.123)	(0.056)	(0.051)	(0.064)	(0.048)	(0.060)
(9) Uninsurance (Non-AMI)	0.062**	0.042	0.048*	0.065**	0.056**	0.069**	0.062**
	(0.027)	(0.046)	(0.028)	(0.026)	(0.027)	(0.026)	(0.027)
(10) Effects of Market Medi-Cal Rate	−0.019	−0.007	0.020	0.013	0.011	0.012	0.012
	(0.022)	(0.082)	(0.023)	(0.042)	(0.046)	(0.041)	(0.044)
(11) Effects on Stroke Mortality	0.046	0.026	0.111*	0.160***	0.145**	0.165***	0.148**
	(0.048)	(0.070)	(0.056)	(0.060)	(0.066)	(0.059)	(0.067)
(12) Effects on HF Mortality	0.008*	0.025	0.001	0.004	0.009	0.008	0.014*
	(0.005)	(0.017)	(0.013)	(0.011)	(0.009)	(0.010)	(0.008)
(13) Effects when charges are added		0.044	0.039	0.055*	0.053*	0.059**	0.059**
		(0.044)	(0.030)	(0.028)	(0.027)	(0.028)	(0.027)

Notes: Each column presents the effects of uninsurance on the in-hospital mortality rate of insured AMI patients using a different market definition. Row 1 reproduces the original FE estimates from table 7. Remaining rows conduct various robustness checks as described by their title and in text. Each regression includes demographic and clinical characteristics of insured heart attack patients in the hospital, demographic and clinical characteristics of all the patients in the hospital, basic hospital demographics, year and hospital fixed effects. The number of observations vary across rows. Robust standard errors clustered at the county level are shown in parenthesis below coefficients. Regressions are weighted by the number of insured AMI discharges at the hospital.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 18: Heterogeneous Effects of Uninsurance by Hospital Ownership and Location

		Ownership			Location	
	All	Public	Not–for–Profit	For–Profit	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
UNINS (hospital)	0.061**	0.183***	0.028	0.138***	0.067**	−0.110
	(0.026)	(0.048)	(0.025)	(0.042)	(0.026)	(0.163)
	[0.060]	[0.104]	[0.047]	[0.057]	[0.060]	[0.062]
UNINS (county)	0.042	0.206	0.021	0.020	0.049	0.186
	(0.045)	(0.233)	(0.034)	(0.207)	(0.053)	(0.198)
	[0.058]	[0.061]	[0.056]	[0.059]	[0.058]	[0.057]
UNINS (5 miles)	0.047*	0.155**	0.046*	0.025	0.054*	−0.085
	(0.027)	(0.066)	(0.025)	(0.082)	(0.027)	(0.200)
	[0.059]	[0.089]	[0.051]	[0.057]	[0.059]	[0.061]
UNINS (10 miles)	0.063**	0.149**	0.064***	−0.015	0.072**	−0.068
	(0.025)	(0.064)	(0.023)	(0.100)	(0.026)	(0.196)
	[0.061]	[0.071]	[0.055]	[0.062]	[0.060]	[0.061]
UNINS (15 miles)	0.054**	0.185**	0.055**	−0.045	0.063**	−0.060
	(0.026)	(0.071)	(0.023)	(0.112)	(0.028)	(0.150)
	[0.059]	[0.063]	[0.058]	[0.058]	[0.058]	[0.061]
UNINS (10/20 miles)	0.068***	0.164**	0.068***	−0.011		0.277
	(0.025)	(0.069)	(0.024)	(0.104)		(0.187)
	[0.060]	[0.071]	[0.056]	[0.060]		[0.060]
UNINS (15/25 miles)	0.060**	0.212**	0.060**	−0.044		0.118
	(0.026)	(0.086)	(0.023)	(0.112)		(0.233)
	[0.059]	[0.063]	[0.057]	[0.058]		[0.060]
AMI mortality	0.082	0.091	0.079	0.090	0.082	0.079
OBS	2685	507	1545	633	2200	485

Notes: Each row presents the effects of uninsurance on the in-hospital mortality rate of insured AMI patients using a different market definition. Column 1 provides estimates for the full sample. Columns 2–4 show the effects separately by hospital ownership whereas columns 5–6 show them by location. Each regression includes demographic and clinical characteristics of insured heart attack patients in the hospital, demographic and clinical characteristics of all the patients in the hospital, basic hospital demographics, year and hospital fixed effects. Robust standard errors clustered at the county level are shown in parenthesis below coefficients. Means of uninsurance rates by type of health care provider and market are shown in brackets below coefficient estimates. Regressions are weighted by the number of insured AMI discharges at the hospital.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 19: The Marginal Cost of a Statistical Life Year Saved

	FE Estimate	Life years saved	Av. No. of Uninsured	Total Cost of Uninsured	Marginal Cost
Hospital	0.061	6.78	574	296,842	43,769
County	0.042	4.67	574	296,842	63,569
5 miles	0.047	5.23	554	286,723	54,870
10 miles	0.063	7.00	554	286,692	40,930
15 miles	0.054	6.00	544	281,285	46,851
10/20 miles	0.068	7.56	557	287,999	38,093
15/25 miles	0.060	6.67	546	282,477	42,345

Notes: Each row calculates the marginal cost of statistical life year saved based on a different market definition. For more details see section 6.