The Marginal Benefit of Inpatient Hospital Treatment: Evidence from Hospital Entries and Exits

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Abstract

The marginal benefit of health care determines the extent to which policies that change health care consumption affect health. I use variation in access to hospitals caused by nearly 1,300 hospital entries and exits to estimate the marginal benefit of inpatient care. I show that hospital entries and exits cause sharp changes in the quantity of inpatient care, but there is no evidence of an effect on average mortality with tight confidence intervals. I find suggestive evidence of an effect on mortality in rural areas and for the over-65 population with magnitudes that imply the marginal benefit of inpatient care is significantly higher for these populations than for the average patient.

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1 Introduction

Estimates of the marginal benefit of inpatient care are crucial for evaluating policies that affect the over $900 billion spent annually at US hospitals (NCHS 2015). Some have argued that utilization of low marginal benefit care is common (Berwick and Hackbarth 2012, Cutler 2014). Policies that reduce the quantity of inpatient care for populations or at margins where the incremental benefit of care is low would decrease spending without affecting patient health. However, obtaining estimates of the incremental benefit of care is difficult because without exogenous variation in the quantity of care, the unobserved health of the population will bias the estimate toward zero.

Changes in the quantity of health care caused by hospital entries and exits are a potential source of exogenous variation in inpatient care. Hospital exits have also been the focus of significant policy debate. In 1997, the possibility that rural hospital exits would limit access to inpatient care prompted legislation that required Medicare to pay Critical Access Hospitals enough to cover their costs.1 This subsidy costs about $2 billion dollars a year, and Medicare programs collectively provide $3.6 billion in subsidies to rural hospitals (MedPAC 2012). Concerns about health care access have also motivated a number of protests over hospital exits.2 For example, in the summer of 2014, the mayor of Belhaven, NC protested a hospital exit that he blamed for the death of one of his constituents by walking to Washington DC (Mackey 2015).

In this paper, I estimate the marginal benefit of inpatient care using variation in access to hospitals caused by hospital entry and exit. I estimate the benefit for the population as a whole, by patient age, and for rural areas. These are the relevant margins for evaluating policies that seek to prevent hospital exit, and these estimates also contribute to answering the broader question of whether the incremental benefit of hospital care is high or low – particularly for patients on the extensive margin.

I construct a hospital-level panel using American Hospital Association data from 1982-2010 that includes measures of the quantity of health care and indicators for hospital entry and exit. I combine this file with county-level measures of health, including mortality rates from 1982-2010 and self-reported health from 2002-2010, and detailed health care utilization and mortality data for all Medicare fee-for-service enrollees from 1999-2011.

My empirical strategy is based on the observation that hospital entry and exit causes an immediate, persistent change in the amount of inpatient care in a market. I quantify the marginal benefit of care using the ratio of the change in health outcomes from a hospital entry/exit to the change in the amount of inpatient care caused by an entry/exit. My data have nearly 1,300 general hospital entries and exits during the 1982-2010 period and 510 entries and exits that affect the 1999-2011

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Medicare sample time period. The large number of entries and exits allows for precise estimates of the effect of inpatient care on mortality rates.

After pooling entries and exits, I find that an entry/exit causes the number of admissions in an average market to change by 2,012 and the number of days spent in the hospital to change by 13,402. These estimates are about 1.3 percent of the admissions and inpatient days in an average market. The admissions estimates imply substantial effects on the quantity of inpatient care at the extensive margin. These estimates are robust to a number of alternative specifications, although some specifications suggest the effect on the quantity of care is up to 35 percent smaller.

Despite the significant effects on the quantity of care, I find hospital entry and exit has no immediate effect on self-reported health and no effect on mortality rates for at least 10 years after the event. The confidence intervals on the mortality results are narrow and allow us to reject an increase of more than 0.4 deaths per 100,000 from an exit. These results imply an average hospital exit does not substantially increase mortality for an average person, despite the effects on the quantity of care and the time required to travel to a hospital in an emergency.

Although I find no evidence of an effect on mortality in the sample as a whole, rural areas and the over-65 population are subsamples where effects on mortality are more likely. The effect of an exit on access to care in rural areas should be larger than in urban areas, and the over-65 population is in poorer health than the sample as a whole, which could result in a larger benefit of inpatient care. I find hospital entry/exit affects the quantity of care in rural areas, and there is suggestive evidence that those changes in inpatient care have an effect on mortality, particularly for the over-65 population. However, the effect is not detectable until a year after the event and is not evident in some specifications.

To investigate the value of inpatient care for the over-65 population, I turn to the Medicare data set. I find that hospital entry/exit significantly affects measures of the quantity of inpatient care and total Medicare spending. Marginal increases in spending reduce mortality by 15 deaths per 100,000. Although this effect is statistically significant in my main specification, it is not robust to some alternative specifications.

Distance and the severity of illness affect the estimates from the Medicare data in the expected ways. Effects are bigger for entries/exits near a Medicare beneficiary’s residence and that result in a relatively large change in distance to the nearest hospital. Ordering diagnoses by a measure of deferrability (share admitted on weekends) shows little evidence of an effect on diagnoses that are not very deferrable (e.g., hip fractures) and some evidence of an effect on more deferrable conditions (Card et al. 2009). I find evidence of an effect on admissions for some serious conditions associated with poor health (e.g., congestive heart failure, cardiac dysrhythmia, and heart disease). There is little evidence of an effect on births in the AHA data, which I would not expect to be affected by entry/exit.
The lack of an effect on non-deferrable care such as births and hip fractures suggests my estimates are not biased by shocks that coincide with hospital entry/exit. In addition, plots of the leads of hospital entry/exit show little evidence of pre-trends in the outcome variables that would suggest my results are biased.

The substantial effect on quantity paired with the null effect on measures of health in the aggregate data indicates that the marginal benefit of inpatient care in the population as a whole is low. My estimates suggest an increase in inpatient costs does not reduce mortality with a confidence interval that allows us to reject 0.7 lives saved per million dollars. In rural areas, the significant effect on the quantity of care and the marginally significant effect on mortality implies about 1.2 lives saved per million dollar increase in inpatient costs. Estimates using the Medicare data imply about 11.9 lives saved per million dollar increase in inpatient costs.

The total change in Medicare spending associated with an entry/exit is approximately double the change in acute inpatient spending. This finding suggests the number of lives saved per million dollars in total medical spending in both the aggregate and Medicare data could be lower than the number of lives saved per million dollars of inpatient spending. In fact, 3.6 lives are saved per million dollars in total Medicare spending. Although these results imply relatively little additional spending is needed to save a life, I cannot reject the null of no effect on mortality in a number of specifications for rural areas and using the Medicare data. These alternative specifications suggest the amount of spending per life saved could be much higher. The marginal benefit of inpatient care is also overestimated if distance to the hospital has a direct effect on mortality.

I can reject that the cost per life saved in the population as a whole equals the cost per life saved of the Medicare population, which implies the marginal benefit of inpatient care is higher for the Medicare population than the non-Medicare population. I can also reject that the cost per life saved for the population as a whole equals the cost per life saved in rural areas. My estimates suggest the marginal benefit of inpatient care for the population as whole, on the dimensions of health I can measure, is low compared to some estimates in the literature (e.g., Doyle 2005, Doyle et al. 2015a, and Sommers et al. 2012). This result weakens the case for policies that increase access to inpatient care across the board, but suggests programs that target high marginal benefit populations could be a cost-effective approach to saving lives.

My paper contributes to a literature that estimates the marginal benefit of health care along a number of margins. Many studies have shown that receiving health insurance increases the quantity of medical treatment, with mixed evidence of health benefits (Baicker et al. 2013, Card et al. 2008, Card et al. 2009, Doyle 2005, Finkelstein et al. 2012, Finkelstein and McKnight 2008, Sommers et al. 2012, Sommers et al. 2014). Work that uses cross-sectional variation in the quantity of health care finds substantial geographic dispersion in health spending that is typically uncorrelated with measures of health care quality and mortality rates (e.g., Chandra et al. 2010, Fisher et al.
2003, Newhouse et al. 2013, and Yasaitis et al. 2009). However, some but not all studies that have attempted to address the endogeneity of the quantity of care found that higher levels of spending, conditional on entering the hospital, improves survival rates (Almond et al. 2010, Almond et al. 2011, Almond and Doyle 2011, Doyle 2011, and Doyle et al. 2015a, Doyle et al. 2015b, and Jena et al. 2014).

This work also contributes to a literature that tries to identify the causes of the geographic variation in medical spending. Skinner (2011) notes that places with more bed capacity per person provide more hospital treatment (Feldstein 1965, Fisher et al. 1994, Wennberg and Gittelsohn 1973) but have similar mortality rates (Fisher et al. 1994). Unlike early work in this literature, I exploit panel variation in hospital capacity to measure the effect of capacity on the quantity of inpatient care and mortality rates.

My paper is closely related to concurrent work by Joynt et al. (2015), which finds that 195 hospital exits from 2003-2011 did not significantly affect the amount of inpatient care Medicare beneficiaries’ received or the probability they died during the two years following an exit. However, the magnitudes of the effects on the quantity of care they estimate (in comparably sized markets) are larger than my estimates for the Medicare population, and would be statistically significant if they appeared in my sample. My longer sample period using the aggregate data allows me to show there was no effect on aggregate mortality several years after the exit, to test for evidence of bias in my estimates due to differential trends between the treatment and control markets, and estimate effects for rural areas during a time period when many rural hospital exits occurred, although I do not use as extensive a set of controls as Joynt et al. (2015).

In earlier work on hospital exit, Rosenbach and Dayoff (1995) found that 11 rural hospital exits in 1986-1987 significantly decreased the quantity of inpatient care received by Medicare patients and found a large but statistically insignificant increase in the mortality rate. Related work found that a similar number of hospital exits affected survey measures of outpatient health care utilization and access, and resulted in higher levels of mortality from AMI (heart attacks) and accidents at home in LA County (Buchmueller et al. 2006), and that a large number of emergency department closures increased inpatient AMI mortality (Shen and Hsia 2012) and overall in-hospital mortality rates (Liu et al. 2014). Two recent working papers measured effects on the quantity of care using the effect of hospital construction subsidies on admissions from 1948-1975 (Chung et al. 2013) and the effect of hospital exits on market-wide hospital revenue in the 1980s-2000s (Garthwaite, Gross, and Notowidigdo 2015). When converted to comparable units, my quantity estimates are similar in magnitude to Rosenbach and Dayoff (1995) and Chung et al. (2013), and are larger than Garthwaite, Gross, and Notowidigdo (2015).³

³A related literature suggests hospital exits result in a more efficient allocation of patients in a market by estimating changes in admissions per hospital and cost per admission (Lindrooth et al. 2003), revenue per admission (Wu 2008),
2 Data

2.1 Medicare Data

The Medicare data set is based on all Medicare claims for all fee-for-service Medicare beneficiaries from 1999-2011. My primary outcome and control variables are taken from the Master Beneficiary Summary File and the National Death Index. The former indicates when a beneficiary dies and includes the beneficiary’s zip code of residence, demographic characteristics, and Medicare enrollment information. I use these data for sample selection and to control for beneficiary sex, age fixed effects, and race fixed effects. I supplement this mortality data with National Death Index data from 1999-2008 that provides the cause of death, which I aggregate into 30 causes using a standard set of categories for ICD-10 codes. The overall mortality rate and mortality by cause of death are key outcome variables.

Another segment of the Master Beneficiary Summary File contains health care utilization by beneficiary and year broken down by the setting and type of medical service. I use these data to construct outcome variables including total Medicare expenditures, total acute care hospital inpatient expenditures, total skilled nursing facility expenditures, acute care inpatient hospital stays, covered acute care inpatient hospital days, the average length of hospital stay, emergency room (ER) visits, and the number of non-institutional physician office services. The expenditures include spending by both Medicare and the beneficiary, but exclude Medicare part D, and are inflated to 2012 dollars using the CPI for all urban consumers.

I use more detailed inpatient claims data to construct measures of the number of admissions by diagnosis, the average severity of diagnosis, and the distance traveled to the hospital. I treat each claim as a separate admission because less than 2 percent of claims appear to be from an ongoing hospital stay. I measure the severity of an illness for admitted patients with the Charlson Co-Morbidity Index (using all 10 available diagnosis codes), and with the average in-hospital mortality rate for each primary diagnosis across all observations in the sample. The analysis by diagnosis uses CCS (Clinical Classification Software) diagnosis codes to combine the ICD-9 diagnoses into clinically relevant sets of diagnoses. Distance to the hospital is measured as the spherical distance between the beneficiary and hospital zip codes. All non-binary Medicare outcomes are winsorized to the 1st and 99th percentiles.

In each year, I exclude all beneficiaries from the analysis who are living outside the 50 states and Washington DC, who are enrolled in the Medicare HMO, who are not enrolled in Medicare part A or part B, who have zip codes or county codes that cannot be merged with the list of hospital service areas or health service areas, and individuals in the years after they turn 100 and the years

or using estimated cost and demand functions to show hospital exits increase welfare (Capps et al. 2010).
before they turn 66. The final data set includes 47 million beneficiaries and 318 million beneficiary-years. Because of the large size of the data set, I analyze the data at the level of the beneficiary birth cohort, race, sex, zip code, county, and year. The aggregated data set has 5 million bins and 36 million bin-years.

2.2 Geographic, Aggregate Demographic, and Aggregate Employment Data

For most analysis that does not involve the Medicare data, I aggregate the data to geographic areas called health service areas (HSAs). HSAs are groups of counties that are “relatively self-contained with respect to the provision of routine hospital care” (Makuc et al. 1991). My HSA-county crosswalk is from the National Cancer Institutes SEER program. I define HSAs as rural during the entire sample period using data from the Area Resource File if in 1995 they had urban populations of under 20,000 in each county and they did not border metropolitan areas.

In the HSA-level analysis, I control for changes in demand for health care using annual data from the decennial census, intercensal population estimates, and county business patterns. I use the US Census Bureau data to construct 36 population variables defined by gender, race (white or non-white), and age (10-year age groups from 0 to 80 and 80+). The county business patterns combined with the census data provide employment, payroll, establishment counts, payroll per capita, and the employment to population ratio. I use logged versions of these variables when the outcome is in logs and per capita versions when the outcome is deaths per 100,000.4

2.3 American Hospital Association Survey

The American Hospital Association (AHA) annual survey provides measures of the quantity of care and the hospital location from 1980-2011 for member hospitals and non-members identified using a number of sources. A document accompanying the survey from 1983-2011 called the Summary of Hospitals lists all hospitals added or removed from the survey and the reason for the addition or removal. The unit of observation in the survey is an independently reporting hospital, which is typically smaller than a hospital system but may be larger than an individual hospital. I focus on short-term, general hospitals and conduct robustness checks where the outcome variables use all hospitals in the AHA survey.

Two of my main dependent variables are the change in the number of admissions and inpatient days in the HSA (excluding long-term care units). A third dependent variable is the change in the average length of stay (ALOS), which is the change in the number of inpatient days divided by the

4Some of the variables based on county business patterns are already in per capita terms. When the outcome is deaths per 100,000, I include both the level and per capita versions of each control derived from the county business patterns.
number of admissions. I also test how the entry and exit of hospitals affects hospital capacity using
the total number of beds in each HSA (excluding long-term care units), and other services offered
by hospitals including the number of ER visits, births, and inpatient surgeries.

The independent variable I focus on is the change in the number of hospitals in the HSA due
to entry or exit. It is equal to the number of entries in a year minus the number of exits. I use
the Summary of Hospitals to separate entries and exits from mergers and demergers and only
use changes in number of hospitals due to entry and exit.\textsuperscript{5} I treat conversions from and to non-
hospital medical facilities, for example, nursing homes, as entries and exits. Once a hospital exits,
it typically no longer appears in the data and reporting a partial year of data for the last months it
is in the market appears to be uncommon.\textsuperscript{6} The lack of data for those months will tend to make
the effect of an exit on measures of the amount of care in the HSA larger in the year of the exit than in
the following year.

I audit the entry and exit data by comparing the AHA exits to lists of exits published by other
organizations and by merging the indicators for entry and exit onto hospital-level Medicare claims
data during the 1999-2011 period when both are available. The audit suggests the AHA rarely
codes a hospital as exiting when it did not, and a majority of exits happen in the year the AHA
indicates. In the remaining cases, the exit date is typically off by exactly one year. This feature of
the data is perhaps in part caused by the AHA survey reporting data for fiscal rather than calendar
years. I show that my main analysis is robust to this type of measurement error in the timing of the
exit.

Despite some error in the timing of the exits relative to calendar years, regressions of the
number of Medicare inpatient days at a hospital on leads and lags of entry or exit of that hospital
(from the AHA data) show a sharp change in inpatient days for both entries and exits (online
appendix figure 1). The result for entries appears to be attenuated more than the result for exits.
The timing of entry in the AHA data and Medicare data match closely for entrants that report they
were open less than a year or report less than a year of data. The effect of entries on inpatient days
at the hospital is much larger in this subsample (online appendix figure 1). In the main analysis
that covers the 1982-2010 period, I limit the sample of entries to the 109 (out of 494) that meet one
of these two criteria, and use all 1,185 exits.

In the analysis that uses Medicare data as an outcome (and covers only the 1999-2011 period),
I adjust the timing of entries and exits reported in the AHA data so they match exactly the year
when a hospital starts and stops receiving patients in the Medicare claims data. Among the set of

\textsuperscript{5}I used optical character recognition software along with manual data entry and data cleaning to make machine-
readable versions of the Summary of Hospitals for 1983-1999 and 2009. All other years are available in a machine-
readable format.

\textsuperscript{6}Less than 5 percent of exits report data for less than 365 days during the final year they are in the data (although
the number of days the data covers is sometimes missing).
hospitals that the AHA indicates entered or exited, I assume it enters in the first year it receives Medicare patients and exits in the final year it treated patients.\footnote{For entries before 2000 and exits in 1998 and 2011, I cannot fully check the accuracy of the timing against the Medicare data. In the analysis of the Medicare data, I assume the timing of those entries is correct if they report they were open less than a year or report less than a year of data. AHA exits in 1998 are used if the hospital does not have patients in 1999, and AHA exits in 2011 are used if there is at least a 10 percent decrease in inpatient days over the previous year. Hospitals that enter or exit multiple times are coded as never entering or exiting.} After this recoding, I have 200 entries and 310 exits.

I use the address information in the AHA Survey and Google Maps to obtain the latitude and longitude of each hospital, assuming no hospital physically moves.\footnote{I manually check and correct all cases in which the hospital address is more than 10 miles from the hospital city and cases where the only address available for the hospital appears to be of low quality. I then select the highest quality address available for each hospital using the following procedure (I refer to non-low-quality addresses as high-quality): (1) all hospitals for which only one address is available, (2) the most recent manually corrected location, (3) a high-quality address if only one is available, (4) the most recent high-quality address found by Google with “rooftop” precision, (5) the most recent high-quality address (with non-rooftop precision), (6) the most recent address flagged as low-quality because it is an intersection, suite, or highway with rooftop precision, (7) the most recent rooftop address, and (8) the most recent address.} I compute the spherical distance between all pairs of hospitals in states near each other and use these distances to construct alternative geographic markets around an entering or exiting hospital.

The AHA asks hospitals to report data from their most recent fiscal year by September. I assume the AHA data in the survey correspond to the previous calendar year, although the reported fiscal years commonly include data from two years prior. The response rate of short-term, general hospitals to survey questions about admissions and inpatient days is 88 percent on average and at least 81 percent in all years during the 1983-2010 period. When a hospital does not report data in a particular year, the AHA imputes it. I use the imputed data but show my estimated effects on quantity are robust to alternative assumptions about the missing data.

### 2.4 State Inpatient Databases

The State Inpatient Databases (SID) are a set of databases constructed by the Agency for Healthcare Research and Quality’s Healthcare Cost and Utilization Project (HCUP), in partnership with state governments that report discharge-level hospital data. In the early 1990s few states participated, but it has since expanded to include 48 states. I use data for NY (1995-2011), NJ (1995-2010), AZ (1995-2010), MD (1995-2010), FL (1997-2011), MA (1999-2011), WA (1999-2009), CA (2003-2009), and CO (2003-2007). The completeness of the data varies from state to state, but they currently include “97 percent of all U.S. community hospital discharges” and in some cases include discharges from specialist or institutional facilities (HCUP 2015).

The unit of observation is a discharge, and for each discharge the data include the length of stay and the total charges. I aggregate the data to the level of the hospital’s HSA. I merge my main
independent variable, the change in hospitals from entry and exit, dropping entries and exits in the hospital data that are not evident in the SID. The main dependent variables are the number of discharges, inpatient days, total charges, and charges per patient. Hospital charges are inflated to real 2012 dollars using the CPI for all urban consumers.

2.5 Aggregate Health and Vital Statistics Mortality Data

County-level mortality data are from the National Vital Statistics System via the publicly available CDC Wonder databases. The data cover 1982-2010, but starting in 1989 the number of deaths for cells with less than 10 deaths are suppressed. Aggregate mortality rates are available for virtually all counties during the entire 1982-2010 time period, and data for subgroups are available in counties that have at least 10 deaths in a particular year for that subgroup. Using these data, I construct the mortality rate per 100,000 individuals by county and HSA for all deaths, as well as by age group (0-64 and over 65), and from AMI. For deaths by age and from AMI, I only use counties for which the number of deaths is available in all years.

Self-reported health data are from the Behavioral Risk Factor Surveillance System (BRFSS), a large nationwide survey. BRFSS data with county codes are available from 2002-2010 through the BRFSS Selected Metropolitan/Micropolitan Area Risk Trends for counties that meet certain sample size criteria. The data contain a total of 383 counties and 2,028 county-years. I construct six measures of self-reported health: the share of adults who report being in good or better health; the number of days out of 30 in which physical health was not good; the number of days out 30 in which mental health was not good; whether their activities were limited due to physical, mental, or emotional problems; whether they have health problems that require use of special equipment; and whether they indicate they are unable to work. I recoded these variables so higher values represented poorer health and rescale them in standard deviation units.

2.6 Additional Sample Restrictions

The county and HSA-level analyses are limited to the set of counties that ever had a hospital in my data. I exclude Virginia and Alaska from the county-level analysis because of inconsistencies in how their counties are coded over time. I include the District of Columbia and treat it as a single-county state. I exclude Alaska prior to 1996 from the HSA-level analysis because of county boundary changes, and include Virginia in all years of the HSA-level analysis. Logged outcomes and control variables are all computed as one plus the value of the variable so as not to lose observations when the variable equals zero.
3 Background

3.1 Summary of Hospital Data

Figure 1 shows the aggregate time series for hospitals, beds, admissions, and inpatient days. The number of hospitals decreased from 5,994 in 1980 to 4,735 in 2010, and an even greater percentage decrease in the number of beds occurred. Most of the decrease in hospitals occurred in the 1980s and 1990s with entries and exits driving about half of the aggregate decrease in the number of hospitals. The decrease in capacity was accompanied by a large decrease in inpatient days that Cutler (2014) argues was caused by changes in insurance reimbursement policy and perhaps advances in medical treatment.

Changes in the number of hospitals in a geographic market due to entry and exit are the source of variation in the data used to estimate the parameters of interest. The bottom panel of figure 1 shows the number of HSAs with net entry and net exit between 1983 and 2011. It excludes entrants that report being open for a full year in their year of entry because they are not used in the main sample. The ratio of exits to entries is quite high the first 20 years of the data, and falls starting around 2000. Because the analysis includes less than one fourth of entries, the ratio of exits to entries would be lower in all periods, and particularly after 2000 when a fairly large number of entries occur. The main estimation sample includes 81 HSA-years with net entry and 992 HSA-years with net exit.

The identification strategy I use relies on the fact that hospital entries and exits sharply change the capacity in a market. Table 1 shows that the year a hospital enters it has 62 beds on average and the year prior to a hospital exiting it has 79 beds on average.

3.2 Modeling Entry and Exit of For-profit and Non-profit Hospitals

A large fraction of hospitals are non-profits or public hospitals that may choose to enter or exit based on expected profitability or to achieve a number of other objectives (Chang and Jacobson 2012). However, if non-profits and for-profits behave similarly or the marginal firm is for-profit, we can focus the empirical model on the decisions of profit-maximizing firms. Theory and empirical evidence suggest assuming hospitals maximize profits is reasonable in this context, although

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9 In 2013, about 79 percent of hospitals were either non-profit or public (http://kff.org/other/state-indicator/hospitals-by-ownership/)
10 Lakdawalla and Philipson (2006) show that if a donor who values the non-profits production can convince it to maximize the donor’s utility function, a non-profit will behave like a for-profit that is willing to take losses. Their results also suggest the marginal firms will tend to be for-profit.
11 The correlation in the time series of non-profit entry/exit and for-profit entry/exit is quite high, which is consistent with both types of firms responding to national shocks. The number of for-profit entrants was greater than the number of non-profit entrants, and the fraction of exiters that were for-profits was much higher than the fraction that were...
non-profit and for-profit hospitals do not always make similar choices (Chang and Jacobson 2012).

4 Empirical Strategy

4.1 Empirical Model

In the empirical model the dependent variable, $y_{ct}$, is a linear function of the number of hospitals and one lag of the number of hospitals, $h_{ct}$ and $h_{ct-1}$; other observables including a vector of demographic characteristics and employment data, $x_{ct}$; year fixed effects, $\gamma_t$; market effects, $\rho_c$; market time trends, $\rho_c t$; and an error term assumed to be orthogonal to the regressors, $\varepsilon_{ct}$. In some specifications I replace $\gamma_t$ with state-year fixed effects. The dependent variables include the number of hospital beds, admissions, inpatient days, and the mortality rate in year $t$ and in a geographic hospital market $c$ (typically an HSA). I include the lag of the number of hospitals because even if the effect of the entry or exit is immediate, hospitals opening and closing part way through the year will cause part of the effect to spill over into the following year of data.

Written in first differences the model is

$$\Delta y_{ct} = \beta_0 \Delta h_{ct} + \beta_1 \Delta h_{ct-1} + \delta \Delta x_{ct} + \gamma_t + \rho_c + \varepsilon_{ct}$$

(1)

When the outcome is a mortality rate, in my main specifications I weight the regression by population to increase efficiency.

I analyze the more detailed Medicare data at the level of birth cohort-race-sex-zip code-county bins that I index by $b$. I replace the demographic and employment controls with a vector of age, race, and sex fixed effects, and replace the market fixed effects with zip code fixed effects. This model is

$$\Delta y_{bct} = \beta_0 \Delta h_{ct} + \beta_1 \Delta h_{ct-1} + \Theta_{bct} + \gamma_t + \rho_{zip} + \varepsilon_{bct}$$

(2)

In this specification the dependent variables are the average value within the bin, and when I estimate the model, I weight by the number of individuals within the bin, so the estimates can be interpreted as the effect of an entry or exit on the average Medicare patient in the sample.

The first differences model allows me to remove geographic-area trends by including geographic-area fixed effects. This approach has substantial computational advantages over a similar model estimated in levels, particularly in the Medicare data set where I remove zip code trends in a data set with over 30 million observations.

non-profits (Chakravarty et al. 2006).

12 I draw on Gentzkow, Shapiro, and Sinkinson’s (2011) model and discussion of the bias of the estimates of the effect of entry and exit of firms on an outcome using a difference-in-differences estimator.
The primary challenge in estimating $\beta_0$ and $\beta_1$ is that hospital entry and exit, $\Delta h_{ct}$, will be in part driven by changes in unobserved profits. I expect the change in profits should be positively related to the quantity outcomes and negatively related to mortality. Thus, the estimated effects on the quantity of care will tend to be biased away from zero, and the estimated effects on mortality will tend to be biased toward zero.

A second potential problem is incorrectly specifying the relationship between the outcome of interest and the change in number of hospitals. This specification imposes that the effect of a hospital entry/exit occurs immediately following the entry/exit. If the effect of entry/exit on the outcome of interest occurs with a longer lag or results in a change in slope rather than a change in intercept, the estimated effect will be biased toward zero. I will provide evidence that each equation captures the dynamics of the effect of hospital entry and exit on the dependent variables.

### 4.2 Identification

The key assumption of this model is that absent the entry/exit, the trends in the outcome in the treated and control markets would be the same. I look for evidence that the parallel trends assumption is violated and that the function form I use does not fit the data by estimating pre-and post-trends using the following specification for the aggregate data:

$$
\Delta y_{ct} = \sum_{j=-4}^{4} \alpha_j \Delta h_{c(t-j)} + \delta \Delta x_{ct} + \gamma_t + \rho_c + \epsilon_{ct}
$$  

(3)

The Medicare data are again analyzed at the bin level:

$$
\Delta y_{bct} = \sum_{j=-2}^{2} \alpha_j \Delta h_{c(t-j)} + \Theta_{bct} + \gamma_t + \rho_{zip} + \epsilon_{bct}
$$  

(4)

I will sometimes use versions of these two specifications where the outcome is in levels, not first differences, and in these specifications I will always normalize $\alpha_{-1}$ to zero. In the aggregate levels specifications I will replace $\Delta x_{ct}$ with $x_{ct}$, and add market time trends, $\rho_c t$. In the Medicare specification I will include bin fixed effects (and drop $\rho_{zip}$), replace $\Theta_{bct}$ with fixed effects for age (race and gender do not vary within a bin), and for computational reasons include race, gender, and birth cohort time trends rather than zip code time trends.

Statistically significant lags (except for the first lag) would suggest the models I estimate are not good fits for the dynamics of hospital entry/exit. Statistically significant coefficients on the leads of hospital entry/exit would indicate the parallel trends assumption is violated. If no evidence of pre-trends exists, the most likely source of bias is a shock that happens in the year of the entry/exit but is not preceded by demand shocks that are detectable in the leads of entry/exit.
To the extent those types of shocks occur, they are unlikely to immediately cause an entry because new hospitals may need to receive regulatory approval to open and hospitals require a substantial amount of time to build. Some states also have regulations that slow hospital exits by requiring advance notice or development of plans to manage the exit, although even in these states we see examples of hospitals closing quickly.\textsuperscript{13}

In addition, the shock that pushes a hospital to exit need not be large relative to the size of the effect of the exit itself on the market. If a hospital is close to the threshold where it will choose to exit if its earnings decrease further, small shocks could cause the exit.\textsuperscript{14} If slowly evolving forces like the population of a market primarily drive demand for health care, we would have more reason to expect profitability shocks in the year of the entry/exit to be small and thus for the bias in the estimate of $\beta_0$ and $\beta_1$ to be small. The population of a market explains much of the variation in demand for hospital care. A regression of the number of admissions in an HSA on the population has an $R^2$ of 0.95. Entry and exit is also tied to population growth. Online appendix figure 2 shows a positive relationship between population growth and net entry in the years leading up to an entry/exit.

I construct placebo tests to directly check for evidence of bias in the year of the entry/exit due to unobservables. I find no evidence of an effect of entry/exit on births in the AHA data or on admissions for diagnoses such as hip fractures in the Medicare data. These types of conditions could be affected by unobserved shocks, but are unlikely to be directly affected by entry/exit. In addition, I show my aggregate quantity results are robust to including state-year fixed effects that will remove state-level policy changes and other state-level shocks that may cause a hospital to exit.

5 Main Quantity Results

The quantity estimates have two main purposes. The first is to determine if hospital entry and exit has a meaningful effect on access to health care. If hospital exit did not affect the quantity of care, the case for policies to increase access to care, such as subsidizing rural hospitals, would be weakened. The second is to estimate what is essentially the first stage in the marginal benefit of inpatient care calculation.

\textsuperscript{13}See Huron Consulting’s list of steps for closing a hospital (Gideon et al. 2015) and evidence of such regulations in IL (Farr 2013), MA (Ronan 2014), MI (Bureau of Health Care Services 2014), NJ (NJHA 2008), and RI (Rules and Regulations for Licensing Hospitals 2012).

\textsuperscript{14}See Gentzkow, Shapiro, and Sinkinson (2011) for a more formal version of this argument.
5.1 Main Quantity Estimates

I expect the effect of an entry/exit will be detectable in data for both the year of the event and in the following year because many hospitals will not exit at the beginning of the year. I find clear evidence of effects in both years when I regress Medicare inpatient days at a hospital on leads and lags of entry or exit of that hospital (online appendix figure 1). However, in the AHA data, only a small percentage of hospitals appear to report less than a full year of data in their final year in the survey. This observation suggests that the survey typically does not include the final months a hospital was in the market. As a result, in the analysis that uses AHA data, I expect to see sharp changes in quantity in the year of the event followed by a smaller change in the opposite direction in the following year due to competing hospitals treating some of the exiting hospital’s patients.

Each panel in figure 2 shows plots of coefficients from two separate regressions. The estimates plotted with circles are from a regression of changes in hospital capacity or quantity of care within an HSA on leads and lags of the change in the number of hospitals within that HSA, controlling for the variables specified in equation 3 plus leads and lags of the change in the number of hospitals in all neighboring HSAs. The estimates plotted with open diamonds are from a regression with the same right-hand-side variables but that use the sum of the bed capacity or quantity in all markets that border each HSA as the outcome variable. The latter specification tests if the effect of an entry/exit spills over into neighboring markets. Substantial spillovers would suggest HSAs are too small to use in this analysis because individuals travel between markets to receive care.

The first panel of figure 2 shows that when a hospital enters, the number of beds within the market increases with no statistically significant pre-trend or post-trend. The effect of entry/exit appears to be entirely on-impact, which indicates equation 1 will do a good job of measuring the effect of hospital entry and exit. The lack of a large and statistically significant pre-trend suggests the omitted variable bias from my identification strategy is small. Table 2 column (1) shows that hospital entry and exit change hospital capacity in an HSA by 86 beds. This increase in beds is computed as the sum of the effect in the year of the event plus the following year because the effect of entries/exits that occur partway through the year should spill over into the next year.

The results for admissions and inpatient days in figure 2 are similar to the results for beds. For both dependent variables, there is a positive on-impact effect, and almost all the coefficients on the leads and lags of the change in number of hospitals are not statistically different from zero at the five percent level. The figure indicates hospital exit decreases the amount of inpatient hospital treatment that individuals receive. Consistent with this result, table 2 columns (2) and (3) show that a change in the number of hospitals changes admissions by 2,012 and inpatient days by 13,402 or about 1.3 percent of the admissions and inpatient days in the market. Column (4) shows an increase in average length of stay that is significant at the 5 percent level. The effect on admissions is consistent with a change in quantity along the extensive margin.
Figure 2 also shows hospital entries and exits do not affect admissions or inpatient days in neighboring markets in the year of the event. There is a marginally significant decrease in inpatient days of about 4,641 and a statistically significant decrease in beds of 22 one year after the event. For each outcome, the point estimates of the within-HSA effects are well outside the confidence intervals of the cross-HSA effects. These results indicate HSAs are large enough that the within-HSA quantity estimates are not driven by patients traveling between markets, but provide some evidence that the effect of entry/exit on the quantity of care may be smaller than the within-HSA estimates. These results are also consistent with entry/exit affecting quantity both in the year of the event and in the subsequent year.

A significant advantage of the Medicare data over the AHA data is that it allows me to define markets using a Medicare beneficiary’s residence rather than the location of the hospital. Effects of entry/exit on markets defined using the patient’s residence are immune to the concern that the estimates are inflated because patients travel between markets to receive care. The Medicare data also include measures of the quantity of outpatient care, which allows us to see the effect of entry/exit on a larger portion of the medical care an individual consumes.

Figure 3 plots the coefficients on the leads of hospital entry/exit in the Medicare data and shows little evidence of a pre-trend. Figure 3 shows that hospital entry/exit in a Medicare beneficiary’s HSA of residence affects total expenditures in the year of the event and the subsequent two years. There is evidence of an effect on acute inpatient expenditures, acute inpatient stays, and inpatient days in the year of the event and at least one additional year. The individual coefficient estimates are often only marginally significant, and in some cases, are not statistically significant. The effects in the year of the event and following year are both smaller than the on-impact effect in the AHA data, but the sum of the effects is fairly similar in magnitude.

Table 3 confirms that the sum of the increase in quantity in the year of the event and the following year is statistically significant for each of these outcomes except acute inpatient expenditures for which it is marginally significant. Inpatient stays increase by 0.0015 per person, which is quite similar to the aggregate data in which hospital entry and exit change admissions as the share of the population by 0.16 percent, but it is much smaller as a percentage of admissions because Medicare patients have higher hospital admission rates than the general population. This finding suggests hospital entry/exit has similar effects on the probability that Medicare beneficiaries and younger individuals go to the hospital. Online appendix figures 3 and 4 show similar results if the data are analyzed in levels, and show sharper effects concentrated in the year after the event for smaller markets called hospital service areas.

The results using both the AHA data and the Medicare data show that hospital entry and exit affect the quantity of inpatient care. However, these changes in care may not affect patient health if inpatient care and outpatient care are substitutes. Table 3 shows the effect on total Medicare
expenditures is larger than the effect on acute inpatient Medicare expenditures, which is consistent with little substitution between inpatient and outpatient care. I find no evidence of substitution for particular outpatient and post-acute care services including ER visits, skilled nursing home care, and physician office services (online appendix figures 5 and 6). Figure 4 shows that hospital exit significantly decreases ER visits in the AHA data, which is consistent with the Taubman et al. (2014) result from the Oregon Health Insurance Experiment.

My estimates are consistent with other work on the effect of hospital entry and exit on the quantity of inpatient care. Rosenbach and Dayhoff (1995) estimate a 6.3 percent decrease in discharges and a 8.1 percent decrease in inpatient days using 11 rural hospital exits. I find effects on admissions and days in rural areas of about 4 percent (online appendix table 1). Joynt et al. (2015) estimate exits cause a statistically insignificant 0.0084 decrease in admissions per Medicare beneficiary, which is larger in magnitude than the significant effect I estimate using the same hospital market definition (online appendix table 9). In online appendix table 2 I show that entry and exit change the number of admissions per bed by 22. This result is similar to the Chung et al. (2013) finding that hospital construction subsidies caused an increase in admissions per bed of about 19 after three years. However, the effects on quantity I estimate are much smaller than the effect of receiving health insurance. Receiving Medicaid in Oregon raised the frequency of ever being admitted to a hospital by 2.1 percentage points (Finkelstein et al. 2012), and gaining access to Medicare increased the probably of admission by 1.2 percentage points (Card et al. 2008). I find that hospital entry and exit change admissions as a share of the population by 0.2 percentage points in the full sample and by 0.5 percentage points of the population in rural areas.

5.2 Quantity Robustness Checks

The AHA quantity results are robust to a number of changes to the specification including estimating the model in logs, in per-bed units, adding state-year fixed effects, and using quantity measures from all hospitals rather than only short-term, general hospitals (online appendix tables 2 and 3 and online appendix figures 7 and 8). The bias due to using imputed data appears to be small (online appendix figures 9 and 10). I find significant (but smaller) effects of entry and exit on discharges and inpatient days in an alternative data source, the State Inpatient Database (online appendix figure 11), although the results with this data set are not robust to including market level trends. In the AHA data, I cannot reject the null hypothesis that the effect of net entry equals net exit on the quantity of care, but the effects of net entry are much weaker and never statistically

15 I compute the per-bed numbers using Chung et al. (2013) tables 9 and 15.
16 Only the closest hospital appears to be affected by an entering/exiting hospital and excluding imputed observations increases the effect by a small fraction of the total effect of entry/exit on the market (online appendix figure 10). Aggregating across competitors by distance from the entering/exiting hospital, as in Lindrooth et al. (2003), shows effects out to the 10-20 mile range (online appendix figure 16).
significant (online appendix table 5 and online appendix figure 12). In the Medicare data, both entries and exits affect the quantity of care in the expected direction, but the evidence for the effect of exits is stronger (online appendix figure 13).

Errors in the timing of entry/exit in the AHA data will bias up the estimate because it may result in the first differences model missing part of the effect of the entry/exit on competing hospitals. Estimating the lead-lag figures in levels should substantially reduce this problem for entries/exits with typical discrepancies in the timing of the event of plus or minus one year. There is no evidence from the levels specifications that the results are biased due to measurement error in the timing of entry/exit (online appendix figure 14 and online appendix table 6). The trends in the levels specification are different from in the first differences specification, but they also do not suggest the estimates are biased. Estimating the levels model with log outcomes substantially reduces the trends but suggests (unlike other specifications) the effect on quantity fades out after several years (online appendix figure 15).

The results may also be biased up if HSAs are small enough that many people treated at an entering hospital were previously treated at hospitals outside the market, and many people previously treated at an exiting hospital are subsequently treated at hospitals outside the market. I show in figure 5 that even for very large markets, there is evidence of an effect on admissions.\textsuperscript{17} For markets with a radius of 150 miles, the on-impact effect on admissions only falls to 1,844 admissions. After including the spillover into the next period, the total effect is a statistically insignificant, imprecisely estimated 1,298 admissions, or about 35 percent smaller than the HSA-level estimate.

The main quantity estimates imply effects of 89 percent of admissions at the entering/exiting hospital, whereas estimates using markets with a radius of 150 miles imply effects of about 57 percent of admissions. This level of responsiveness to entry and exit suggests demand is more elastic than the -0.2 elasticity of demand from the RAND health insurance experiment (Aron-Dine, Einav, and Finkelstein 2014; Manning et al. 1987; Keeler and Rolph 1988). If a hospital exit shifts the supply curve and the elasticity of demand is -0.2, price would have to change by 6.5 percent to cause the 1.3 percent change in admissions that I estimate. Estimates of the elasticity of demand from Ho (2006) along with the change in distance to the hospital I estimate, imply changes in quantity at a particular hospital of about 5.1 percent.\textsuperscript{18}

Taken together, the results in this section show the finding that entry/exit affect the amount of inpatient care individuals receive is quite robust. Although I find some evidence that the effects on

\textsuperscript{17}A drawback of this approach is that the employment and demographic controls are defined at the county-level, and as such, I can only approximate the population demographics, employment, and income in these markets. The control variables observed at the county-level are aggregated to the level of each of these areas by summing their values, for example, the employment, in all counties that ever had a hospital in these areas.

\textsuperscript{18}Ho (2006) found a 1 mile increase in distance to a hospital caused a 22 percent decrease in the probability of admission at that hospital, and in HSAs, I find effects on average distance traveled to the hospital of 0.23 (online appendix table 8).
the quantity of care are smaller than implied by HSA-level specification.

6 Main Health Results

6.1 Aggregate data

I interpret mortality rates as a measure of health and test if changes in the quantity of inpatient care caused by hospital entry and exit affects mortality. Figure 6 plots coefficients from unweighted and population-weighted regressions of changes in mortality rates on leads and lags of changes in the number of hospitals and controls. In the unweighted specifications, there is no evidence of a pre-trend in mortality or an effect of entry/exit on the mortality rate. In the weighted specification the estimates are more precise and there are significant positive and negative effects prior to the event, but I find no obvious trend in mortality. There is also a positive and significant increase in mortality in the year of the entry/exit, which is the opposite of the expected sign, and no effect in the next year. In both specifications there is little evidence of an effect on mortality of the expected sign during the 10 years after the entry/exit.

Table 2 column (5) shows population weighted estimates of the effect of hospital entry and exit on mortality. Hospital entry and exit does not have a significant effect on mortality on-impact or in the subsequent year. The confidence intervals allow us to reject an increase in mortality from an exit of over 0.4 deaths per 100,000. I find no effect on mortality using log deaths as the outcome, estimating the effects separately for entries and exits, estimating the effects in per bed units, and using county-level data (online appendix tables 2, 3, 5, and 7 and online appendix figures 12 and 18). The levels specification shows some evidence of a long term increase in mortality (online appendix figure 17).

I expect that hospital entry and exit in rural areas could effect mortality more than in the main sample because distances to the hospital are greater and the effect of entry and exit on inpatient care is larger per capita. Table 4 shows no effect on mortality in rural areas in the year of the event and a marginally significant decrease of 12 deaths per 100,000 in the subsequent year. Although the sum of the two effects is not statistically significant in the HSA specification, it is in the HSA-level per-bed specification (online appendix table 4) and there is also a significant effect in counties a year after the event (online appendix figure 19).

I split this sample into the mortality rate for the under-65 and over-65 age groups to determine if a subsample of the population is driving this effect. Figure 7 shows the effect on mortality is being driven by the over-65 group for whom the decrease in mortality a year after the event is statistically significant. There is also a significant effect on mortality in the opposite direction three years before the event. I find no evidence of an effect on the under-65 group. There is little
evidence of an effect on the over-65 population in counties or in unweighted HSA-level regressions (online appendix figures 19 and 20).

Although these results are by no means conclusive because they are sensitive to weighting and the sum of the effect in the year of the event and following year is only statistically significant in the per-bed specification, they provide some evidence that hospital entry/exit affects mortality in rural areas. In contrast, Joynt et al. (2015) found marginally significant decreases in mortality following a rural hospital exit.

These results also show that the effect of hospital entry/exit on mortality is much larger for the over-65 population than for younger individuals. Although it is not always the case that the confidence interval for the under-65 group rejects the point estimate for the over-65 group, it does for rural HSAs (figure 7).

Hospital entry and exit could have a substantial effect on morbidity even in populations where it does not affect mortality. I use the Behavioral Risk Factor Surveillance System to estimate the effect of entry/exit on several self-reported measures of health. I test if hospital entry and exit affects the share of adults who report being in fair or worse health; the number of days out of 30 in which physical health was not good; the number of days out of 30 in which mental health was not good; whether respondents’ activities were limited due to physical, mental or emotional problems; whether respondents have health problems that require use of special equipment; and if respondents indicate they are unable to work.

The left panel of figure 8 shows that hospital exit does not cause significantly worse health as measured by each outcome in the year of the event. The right panel shows that a year after an exit, individuals are less likely to report being unable to work, which is the opposite of what we expect. Plots of leads and lags of these outcomes show little evidence of a pre-trend for each of these variables, although a similar magnitude increase in being unable to work occurred two years before the event (online appendix figure 21).

### 6.2 Medicare Data

The results using the aggregate data suggest to the extent we see effects of hospital entry/exit on mortality, the traditional Medicare population will drive them. Figure 9 shows estimates of the effect of entry/exit on mortality for a hospital entry/exit in the beneficiary’s HSA. The two lead coefficients are close to zero. Mortality rates fall sharply in the years after the event, but the only effect that is statistically significant is two years after the entry/exit. The estimates in table 3 show that neither the coefficient on the on-impact or the first lag are individually statistically significant, but that summing the two coefficients results in a statistically significant decrease in mortality of 15 deaths per 100,000. This estimate is outside the confidence interval for the over-65 group estimated
using the aggregate data (table 4).

I find suggestive evidence of an effect on mortality from entry/exit in other markets and using other specifications, but the effects are often not statistically significant. For example, table 5 shows decreases in deaths of 17 – 25 per 100,000 in counties, hospital service areas, and within 10 miles of the beneficiary’s zip code of residence. Only the estimate for counties is statistically significant, but the estimates for hospital service areas and hospitals within 10 miles would be significant if they were estimated as precisely as HSAs (see online appendix figure 22 for the corresponding lead lag plots). Plots of leads and lags of hospital entry/exit in levels show some evidence of effects in all four markets, but only significant effects for entry/exit within 10 miles two years after the event (online appendix figure 23). Separating entries and exits, I find some evidence of an increase in mortality in the year prior to the exit in both HSAs and hospital service areas, although the magnitude is relatively small (online appendix figure 24).

The Medicare results suggest the effect of entry/exit on the death rate of the non-Medicare population is small. We can reject the null hypothesis that the effect of entry/exit on the mortality rate in the sample as a whole is the same as the 15 deaths per 100,000 estimated using the Medicare data because the lower bound of the mortality confidence interval in table 2 is only 0.4 deaths per 100,000.

However, the results for the Medicare and aggregate data are not consistent. If we assume entry/exit had no effect on the mortality rate of the under-65 population, then the estimate for the Medicare population times their 12 percent share of the population during the 1982-2010 sample period suggests the effect for the entire population would be 1.8 deaths per 100,000. Alternatively, if we assumed hospital entry/exit had a proportional effect on mortality for the non-Medicare population, the effect of hospital entry/exit in the population as a whole would be 2.5 deaths per 100,000. These effects are both outside the aggregate confidence interval in the weighted specifications, but similar to the lower bound of the confidence interval in the unweighted specification. This check suggests the true effect on the Medicare population may be smaller than 15 deaths per 100,000.

The analysis using the Medicare data differs from the analysis of the aggregate data in that it uses a sample of entries and exits for which the timing of the event is recoded to match when the hospital enters/exits the Medicare data, whereas the aggregate data use an unrecoded sample that will have more error in the timing of the entry/exit and contain fewer entries. Online appendix figure 25 shows no evidence that replacing the entries/exits used in the aggregate data with the ones used to analyze the Medicare data leads to an effect on mortality in the aggregate data.

\[ 2.5 \text{ deaths/100,000} = 0.0028 \times 0.009 \times 100,000 \text{ where } 0.0028 \text{ is the Medicare effect as a share of the mortality rate (0.0028 = 0.00015/0.053) and } 0.009 \text{ is the death rate for the whole population in years before entry/exit.} \]
6.3 Magnitude of Health Effects

The lower bound of the mortality confidence interval in table 2 is $-0.4$ deaths per 100,000. The average population of the treated HSAs is 1.3 million, so I can reject increases in deaths of approximately more than 5 per entry/exit, or 0.05 percent of the deaths in the HSA. The confidence interval implies that it is unlikely more than 0.2 percent of the individuals not admitted because of a hospital exit would have would have lived absent the exit. For comparison, online appendix figure 6 shows that estimating the effect on the number of deaths directly, using a levels specification, results in a lower bound of the confidence interval of about 10 deaths per entry/exit.

The estimated increase in deaths of Medicare beneficiaries following an exit in an HSA is 15 deaths per 100,000 which is about 0.3 percent of deaths among Medicare beneficiaries, but it is about 10 percent of the effect on admissions. The lower bound of the confidence interval on mortality for individuals aged 0-64 is much smaller at 0.5 deaths per 100,000. The effect in rural HSAs is about 12 deaths per 100,000 which is about 1 percent of the deaths in the market and about 2.2 percent of the effect on admissions. The confidence interval for the population as a whole rejects effects of the size we see in the Medicare data and in rural areas.

The lower bound of the confidence interval for the aggregate estimates and point estimate using the Medicare data are well inside Rosenbach and Dayhoff’s (1995) statistically insignificant point estimate of a 9.1 percent increase in the mortality rate of Medicare beneficiaries after an exit. The point estimate of the Joynt et al. (2015) analysis of Medicare mortality does not have the expected sign, but my estimate for the Medicare sample appears to be inside their confidence interval. Buchmueller et al. (2006) show that overall mortality, AMI mortality, and accident mortality all fall after the distance to a hospital increases, but they suggest this decrease in deaths is due to unrelated trends. Conditional on overall mortality, they find that hospital exits result in significant increases in mortality from AMI and accidents at home while finding statistically insignificant effects for several chronic conditions. Their estimates imply at least 5 additional deaths per year after a hospital exit, which is an effect size is close to being detectable in my data.\footnote{I first use their estimates to compute the number of deaths per zip code for each additional mile traveled as $1.05 \times 0.0654 \times 14 + 0.117 \times 1.3 = 2.53 \times 121 \div 59 \times 121$. They report the average increase in distance for the affected zip codes is about 2.4 miles, which implies about 2.53 additional deaths per zip code. They report 121 zipcode-years are affected and 59 post-hospital exit (or entry) years so deaths per hospital entry/exit year are $5.2 \div 59 \times 121 = 2.53$.}

The lower bound of the confidence interval for the aggregate data is small compared to the
effects of certain insurance expansions, but some results in this literature are similar to the effects for rural areas or the Medicare sample. Sommers et al. (2014) found that the Massachusetts health care expansion caused mortality to fall by about 2.9 percent or 8.2 deaths per 100,000. Sommers et al. (2012) found increasing Medicaid enrollment led to a fall in mortality of 6.1 percent or 19.6 per 100,000. The magnitude of both of these effects is well outside the confidence interval for the aggregate data and would be detectable given the precision of my estimates. In contrast, Finkelstein and McKnight (2008) find the introduction of Medicare decreased annual mortality by a statically insignificant 0.15 percent, which is closer in magnitude to the lower bound of the confidence interval for the aggregate data.

7 Marginal Benefit of Inpatient Care: Combining the Quantity and Health Estimates

I measure the marginal benefit of inpatient care as the number of lives saved per million dollars in additional inpatient costs. In the aggregate data, the effect on the cost of inpatient care is $27.3 million, which I calculate as 13,402 [inpatient days] × $2,035 [cost per day]. The point estimate of the effect on mortality is positive, which implies that a million dollar increase in spending leads to more deaths. I compute the confidence interval of the marginal benefit estimate by block bootstrapping over HSAs. The confidence interval suggests I can reject an effect of more than 0.70 lives saved per million dollars of inpatient costs.

In the Medicare data, the number of inpatient days increases by 0.0062 per person and the number of deaths decreases by 15 per 100,000, so a similar calculation implies 11.9 lives saved per million dollars in inpatient costs. In rural HSAs the point estimate is 1.2 lives saved per million in inpatient costs. The point estimates for both the Medicare data and for rural areas are outside the aggregate confidence interval, which suggests the marginal benefit of inpatient care is significantly higher for these patients than in the population as a whole.

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21 The cost per day in 2012 is calculated as a weighted average of the average cost per day of state/local hospitals, non-profit hospitals, and for-profit hospitals weighted by the share of hospitals: $2,035 = $1,831 × 0.207 + $2,214 × 0.579 + $1,747 × 0.214. Data are from the Kaiser Family Foundation State Health Facts, see http://kff.org/other/state-indicator/expenses-per-inpatient-day-by-ownership/ and http://kff.org/other/state-indicator/hospitals-by-ownership/.

22 For this calculation, I use an unweighted regression to directly estimate the effect of entry/exit on the change in deaths rather than on the mortality rate to avoid converting the estimated effect on mortality into a number of deaths.

23 11.9 [lives per million dollars] = ($1,000,000 × 0.00015 [lives saved per person]) / (0.0062 [days per person] × $2,035 [cost per day])

24 1.2 [lives per million dollars] = ($1,000,000 × 6.43 [lives]) / (2,687 [days] × $2,035 [cost per day]). For this calculation, I use an unweighted regression to directly estimate the effect of entry/exit on the change in deaths a year after the event rather than on the mortality rate to avoid converting the estimated effect on mortality into a number of deaths.
Each of these estimates only accounts for the effect of the entry/exit on inpatient costs. The Medicare results suggest a hospital entry/exit has a larger effect on total spending than on inpatient spending. An exit reduces total spending by $42 per person, whereas acute inpatient expenditures only fall by $20 dollars (table 3). I am likely underestimating the full cost of saving a patient’s life by only using the costs associated with inpatient care. Recomputing the number of lives saved per million dollars in spending using the point estimate of $42, rather than the number of inpatient days times the cost per day, leads to an estimated 3.6 lives saved per million dollars in Medicare spending.

My measure of the marginal benefit of care is closely related to the cost per life-year saved, which is often approximated as cost of care divided by the number of lives saved over a single year. Typical threshold values used in cost-effectiveness analysis are in the range of $100,000-$200,000 (Doyle et al. 2015a), or about 5 to 10 lives saved per million dollars in spending, although standard estimates of the value of a statistical life (rather than life-year) would imply a much smaller number of lives saved per million (Aldy and Viscusi 2006). My aggregate results can reject effects of the size of the former, but not the latter.

The marginal benefit of inpatient care I estimate for the entire population is low relative to estimates in the literature, whereas the results for Medicare patients are in line with some estimates from the literature. For example, Doyle et al. (2015a) finds a cost per life-year saved of about $80,000 for high intensity care for individuals arriving to the hospital by ambulance, and notes that cardiac catheterization produces values in the same ballpark (Chandra and Staiger 2007, McClellan and Newhouse 1997). Almond et al. (2010, 2011) estimate the effect of high intensity treatment on low-birth-weight babies, Doyle (2005) estimates the effect of insurance on the quantity of care after an accident, and Sommers et al. (2012) estimate the effect of gaining access to Medicaid on mortality. Almond et al. (2010, 2011) estimates a cost per life-year saved of $527,000-$1.3 million, and Doyle (2005) estimates a cost per life-year saved of about $220,000. The Sommers et al. (2012) estimates can be translated into a cost per life-year saved of about $768,000. These estimates imply 0.77 to 4.5 lives saved per million dollars in additional spending, whereas the Doyle et al. (2015a) estimates imply 12.5 lives saved per million dollars in spending. The estimates of the marginal benefit of care in these papers are outside the confidence interval for the aggregate

25Chandra, Gruber, and McKnight (2010) found evidence of substitutability between outpatient care/prescription drugs and inpatient care, but Clemens and Gottlieb (2014) found no evidence of substitution.
26Spending may be a reasonable approximation of the cost of care because average hospital profit margins for Medicare patients are negative (MedPAC 2012).
27The Almond et al. (2010) estimates of the decrease in mortality are much smaller if data points close to the cut off are dropped (Barreca et al. 2011, Almond et al. 2011).
28I convert the Sommers et al. (2012) estimates into these units using their estimates of the effect on the number of enrollees and the number of lives saved along with the cost per enrollee of $4,362 from the 2012 Medicaid Actuarial Report.
data, and would be detectable if they existed in my aggregate data.

A few caveats should be kept in mind when interpreting my marginal benefit estimates and comparing them to other estimates in the literature. First, neither I nor the authors of the cited papers project life-years saved and incremental costs due to the intervention beyond one year to calculate the statistical value of a life-year saved. If one were to calculate the cost per statistical life-year saved using costs and benefits beyond one year, the relative sizes of our estimates could change substantially.

The marginal benefit calculation also assumes hospital entry/exit affects the probability of surviving only by changing the quantity of health care consumed. If distance to the hospital has a direct effect on mortality, for example, because being closer to a hospital may save lives in an emergency, even holding constant the quantity of care, then the marginal benefit estimate is likely to be too large.

A third issue with this calculation is that the cost per day of an inpatient stay I use would be too large if the marginal patients are less costly to treat than the average patients. Online appendix figure 11 indicates entry/exit does not affect average charges per patient in the State Inpatient Databases.

8 Marginal Patients

Identifying the marginal patients is useful for understanding what is driving the effects on health. I focus on how the effects vary with distance to the hospital and the diagnoses of the patient. If the marginal patients include individuals with significant health problems, the effects of hospital entry or exit on health are more believable.

8.1 Distance

Online appendix table 8 shows the average hospital exit in an HSA increases the average distance traveled to the hospital by a statistically significant 0.23 miles, and significantly reduces the number of admissions within 10 and 20 miles of the beneficiary’s zip code of residence.

The effect of hospital entry/exit varies as expected with distance. As the set of entering hospitals increases from within 5 to 50 miles of the beneficiary, the effect on expenditures falls, and changing the distance to the closest hospital affects expenditures and inpatient stays, although there is some evidence of pre-trend in the latter set of results (online appendix figures 26 and 27).

I divide the entries and exits that affect the distance to the nearest hospital into three groups with cutoffs at about 1.6-mile and 4.6-mile changes in the distance. I find a relatively large, statistically significant effect on expenditures and inpatient stay for changes in distance of over 4.6 miles,
no evidence of an effect in the second tercile, and a much smaller but significant decrease in expenditures and inpatient stays for the first tercile (online appendix figure 28).

These results show that distance to the hospital mediates the effect of entry/exit, and that hospital exits that cause relatively small changes in distance to the nearest hospital will have less of an effect than events that cause large changes in distance.

### 8.2 Severity of Diagnosis

I use the Medicare data to test how the effect of entry and exit varies by diagnosis, and then I aggregate the effects by diagnosis using two indexes of their severity.

Figure 10 shows the effect on admissions for the top 40 diagnoses. The diagnoses are ordered by the share of admissions that happen on weekends, which is a measure of patients’ willingness to defer care for a particular diagnosis (Card et al. 2009). The plotted coefficients are the sum of the effect of entry/exit on admissions in the year of the event and the subsequent year minus the sum of the change in admissions in the two years prior to the entry/exit. This approach estimates the effect of the entry/exit as the change in trend. I take this approach because some of the diagnoses, for example, septicemia and COPD, have strong pre-tends, and without this normalization would show large, spurious effects of entry/exit on admissions. (See online appendix figures 29 and 30 for estimates of the effect of leads and lags of hospital entry/exit on admissions for all 40 diagnoses.)

I find a marginally significant increase in admissions for one of the 10 least deferrable diagnoses, urinary tract infection (figure 10). There is also evidence of an effect for some cardiac conditions including congestive heart failure, cardiac dysrhythmias, and heart disease, but not for AMI. I find no effect on admissions from hip fractures, which we would expect to be highly insensitive to changes in access to hospital care. There are effects on some diagnoses that we might expect ex ante would be more responsive to changes in access to hospital care such as fluids/electrolytes and mood disorders, and to overall changes in the quantity of care, such as complications from procedures. These results suggest hospital exit reduces the quantity of care for patients with severe conditions, but not for a condition for which we would expect demand to be very inelastic.

Online appendix figures 31-34 contain dot plots by diagnosis that use variation in access to hospitals caused by entry/exit within the county, within the hospital service area, within 10 miles of the beneficiary, and that affect the distance to the closest hospital. These results are broadly consistent with the results that use entry/exit within the HSA, but not all effects that are significant in figure 10 are significant in these robustness checks, and there are some effects in the robustness checks that are not significant in figure 10.

The AHA survey has limited disaggregated quantity data, but it includes counts of inpatient surgeries and births. Figure 4 shows hospital entry/exit significantly affects inpatient surgeries but
has no effect on births, although individuals traveling between markets appears to at least partially offset the effect on surgeries.

Births and broken hips and both serve as placebo tests because demand for inpatient care for each of these events is highly inelastic. Virtually all births occur in hospitals (rather than other facilities), and hospital entry and exit is unlikely to affect the decision to have a child. If I had detected a large effect of hospital entry and exit on either of these outcomes, such a finding would suggest my estimates were biased perhaps because of endogeneity problems, poor quality data, or insufficiently large markets. I check if the null effect for births is robust to alternative specifications and find little evidence of an effect on births.29

I aggregate across diagnoses to measure the effect of entry/exit on the average severity of the illness of admitted patients using the Charlson Comorbidity Index and an in-hospital death rate index. The latter index is constructed by assigning each admission the average in-hospital death rate for the patient’s primary diagnosis. Online appendix figures 39 and 40 show little evidence of an effect on the average severity of illness of admitted patients when the data are analyzed in differences, but show some evidence of a decrease in the average severity of admitted patients in levels. These results suggest when a hospital exits, the patients who no longer go to the hospital are less sick than the average patient, although the results are more suggestive than definitive.

The results in this section indicate that although the marginal patient may not be as sick as the average patient, hospital entry and exit affects the amount of care that people with serious medical conditions receive. It does not affect care where we would expect demand to be most inelastic. This pattern of results indicates hospital entry and exit could affect patient health.

8.3 Estimates on Health by Distance and Cause of Death

8.3.1 Distance

Focusing only on entries and exits that affect the distance to the closest hospital, online appendix figure 27 shows that exit of the closest hospital increases the mortality rate, and an additional mile to the closest hospital increases the mortality rate, although neither is statistically significant. I also split the entries/exits into terciles of the change in distance where the terciles are cut at about 1.6 miles and 4.6 miles (online appendix figure 41). I find no effect on mortality rates in the first or third terciles. There is some evidence of an effect in the second tercile where there is a decreasing trend in mortality prior to the change in distance, and then mortality increases sharply

29 After conducting a wide range of robustness checks, I only find significant on-impact effects on birth in the per-bed specification and in some of the markets defined using circles around the entering/exiting hospital (see online appendix figures 35-38). The significant effect in the per-bed specification and the moderate-sized to larger markets do not survive summing the on-impact and one-lag coefficients.
after the event. Although because of the trend, the coefficients in the year of the entry/exit and the following year are not statistically significant.

8.3.2 Cause of Death

I conduct a detailed analysis of the effect of hospital entry/exit by cause of death using the National Death Index data for Medicare patients from 1999-2008. I focus on the effect of hospital entries and exits that change the distance to the Medicare beneficiary’s closest hospital for the top 30 causes of death for patients in my sample. I focus on entries/exits that change the distance to the closest hospital because past work in LA County found effects on some acute conditions following a change in distance to the closest hospital (Buchmueller et al. 2006). I estimate the effect of the entry/exit as the sum of the effect of entry/exit on deaths in the year of the event and the subsequent year minus the sum of the change in deaths in the two years prior to the entry/exit. This approach estimates the effect of the entry/exit as the change in trend in the mortality rate. I take this approach because there is evidence of pre-trends in some of the causes of death. (See online appendix figures 42 and 43 for estimates of the leads and lags of hospital entry/exit for all 30 causes of death.)

Figure 11 shows the effect of hospital entry/exit on mortality by cause of death where the causes of death are sorted from most to least common. The mortality rate decreases significantly for ischemic heart disease, kidney disease, prostate cancer, and hypertension. Ischemic heart disease refers to illness caused by reduced blood flow to the heart and includes AMI. Deaths from non-motor vehicle accidents (“all other accidents and adverse events”) also fall, but the decrease in deaths is not statistically significant. Mortality rates do not significantly increase for any cause.

As a robustness check, I create dot plots by cause of death for hospital entries/exits within the HSA, within the county, within the hospital service area, and within 10 miles of the beneficiary. The estimated effects are less consistent across the alternative specifications than for admission by diagnosis. Deaths from ischemic heart disease fall in all of the other specifications, but the decrease is never statistically significant. I find no causes of death where with consistently negative and significant effects (online appendix figures 44-47).

Taken together, my results do not indicate that reduced access to health care consistently affects the probability of dying from a particular cause. Although, I find no significant effects on deaths from ischemic heart disease in the other specifications, the significant effect in a specification that uses variation in the distance to the closest hospital could be a result of changes in the speed one can receive medical treatment. These results are partially consistent with Buchmueller et al.’s (2006) finding of increased AMI and accident mortality, but contrast with Joynt et al.’s (2015) finding of significant decreases in deaths for people admitted for AMI after a hospital exit in their market.
9 Samples Where Effects Are More Likely

The online appendix includes a series of tables where I separately estimate the effect of entry/exit by hospital size, HSA beds per capita, utilized capacity, and the market share of the entering/exiting hospital (online appendix tables 10-13). The four tables contain 16 population-weighted mortality estimates. Two of the estimates are marginally significant effects and three are statistically significant. When hospitals enter markets with low capacity utilization there is a marginally significant increase in mortality and when they exit such markets there is a marginally significant decrease in mortality. This result is consistent with marginal units of care at hospitals in low capacity utilization markets having negative health effects. A similarly counter-intuitive effect occurs where a hospital entry in markets with few beds per capita leads to an increase in morality, but an entry into a market with the most beds per capita leads to a significant decrease in mortality. When I split the sample by quartiles of the market share of the entering or exiting hospital, I find significant decreases in mortality for the third quartile, but no effect in the fourth quartile, which contains entering and exiting hospitals with the largest market share. For each of these results, there is little evidence of an effect in the unweighted specifications.

10 Conclusion

I show hospital entry/exit significantly affects the quantity of inpatient care along the extensive margin in both the aggregate data and for Medicare beneficiaries. I find no effect on certain diagnoses for which demand for inpatient care should be very inelastic (e.g., births and broken hips), but there are effects on serious medical conditions (e.g., congestive heart failure, cardiac dysrhythmias, and heart disease), and the effects on quantity are larger for entries/exits that result in changes in distance to the hospital of over about 4.6 miles.

These changes in quantity do not lead to changes in the mortality rate or self-reported health in the aggregate data, but there is some evidence of an effect on mortality for Medicare beneficiaries and in rural areas. The confidence interval for the marginal benefit of care in the aggregate data can reject the estimates of the marginal benefit of care found in both of these subsamples and in a number of papers in the literature. These results are consistent with concerns that the marginal benefit of care is low for typical patients, but there is suggestive evidence the marginal benefit of inpatient care is high for Medicare beneficiaries and individuals in rural areas. An important caveat to these results is that the mortality and self-reported health data provide relatively crude measures of health. It is possible that in the full population additional inpatient treatment could lead to better health without reducing the mortality rate by a detectable amount or affecting self-reported health.
11 References


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Manning, Willard G., Joseph P. Newhouse, Naihua Duan, Emmett B. Keeler, and Arleen Lei-


Figure 1: Trends in Short-Term, General Hospitals

Notes: Source is the American Hospital Association Annual Survey and the Summary of Hospitals. The net entrants in the bottom panel excludes the approximately three fourths of entries that report being in operation for a full year in their year of entry because they are not used in the estimation of the main results that cover the 1982-2010 period.
Notes: Data cover the 1982-2010 period. The unit of analysis is the HSA-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit. The within-HSA estimates are from a regression of changes in the outcome within an HSA on leads and lags of the change in the number of hospitals from entry and exit within that HSA and controls including leads and lags of changes in the number of hospitals from entry and exit in all neighboring HSAs, HSA fixed effects, year fixed effects, and demographic and employment variables. The cross-HSA estimates are from a regression with the same right-hand-side variables as the within HSA estimates, but that use the sum of the outcomes in all markets that border each HSA. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 3: Effect of Hospital Entry and Exit on Quantity of Care for Medicare Patients

Notes: Data cover the 1999-2011 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit in the patient’s HSA of residence. The coefficients are from a regression of changes in the outcome on leads and lags of the change in the number of hospitals from entry and exit and controls including an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects. The regressions are weighted by the number of Medicare beneficiaries in each bin and the outcomes are all in per-beneficiary units. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 4: Effect of Hospital Entry and Exit on Other Measures of Hospital Treatment

Notes: Data cover the 1982-2010 period. The unit of analysis is the HSA-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit. The within HSA estimates are from a regression of changes in the outcome within an HSA on leads and lags of the change in the number of hospitals from entry and exit within that HSA and controls including leads and lags of changes in the number of hospitals from entry and exit in all neighboring HSAs, HSA fixed effects, year fixed effects, and demographic and employment variables. The cross-HSA estimates are from a regression with the same right-hand-side variables as the within HSA estimates, but that use the sum of the outcomes in all markets that border each HSA. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 5: Effect of Hospital Entry and Exit on Admissions - By Market Size

Notes: Data cover the 1982-2010 period. The unit of analysis is the hospital-year. Plots are of coefficients on hospital entry/exit variable from regressions of changes in the total number of admissions within a given distance of the hospital on that hospital’s entry/exit variable controlling for the net entry of all other hospitals in that radius, HSA fixed effects, year fixed effects, and demographic and employment controls. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA of the entering/exiting hospital.
Figure 6: Effect of Hospital Entry and Exit on the Mortality Rate in HSAs

Notes: Data cover the 1982-2010 period. The unit of analysis is the HSA-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit from regressions of changes in the mortality rate on leads and lags of the change in the number of hospitals from entry and exit, HSA fixed effects, year fixed effects, and demographic and employment controls. Plots in the top row use unweighted regressions and plots in the bottom row are population weighted. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 7: Effect of Hospital Entry and Exit on the Mortality Rate in Rural HSAs

Notes: Data cover the 1982-2010 period for rural areas. The unit of analysis is the HSA-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit from regressions of changes in the mortality rate on leads and lags of the change in the number of hospitals from entry and exit, HSA fixed effects, year fixed effects, and demographic and employment controls. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.

Figure 8: Effect of Hospital Entry and Exit on Self-Reported Health

Notes: Data cover the 2002-2010 period. The unit of analysis is the county-year. Plots are of coefficients of the change in the number of hospitals from entry and exit from regressions of changes in each outcome on the change in the number of hospitals from entry and exit, county fixed effects, year fixed effects, and demographic and employment controls. Error bars show the 95-percent confidence interval. Standard errors are clustered by state.
Figure 9: Effect of Hospital Entry and Exit on Medicare Beneficiary Mortality

Notes: Data cover the 1999-2011 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Plots are of coefficients on leads and lags of the change in the number of hospitals from entry and exit in markets defined using the patient’s residence. The coefficients are from a regression of changes in deaths per 100,000 on leads and lags of the change in the number of hospitals from entry and exit and controls including an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects. The regressions are weighted by the number of Medicare beneficiaries in each bin. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 10: Effect of Hospital Entry and Exit on Discharges by Diagnosis

Notes: Data cover the 1999-2011 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Plots show the top 40 diagnoses during the sample period ordered by the share of admissions that occur on weekends. The plotted coefficients are from a regression of changes in the outcome on two leads and one lag of the change in the number of hospitals from entry and exit within the patients HSA and controls including an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects. The regressions are weighted by the number of Medicare beneficiaries in each bin and the outcomes are all in per-beneficiary units. The plotted coefficients are the linear combination of the sum of the on-impact effect plus the one-year lag minus the sum of the one- and two-year leads. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
Figure 11: Effect of Hospital Entry and Exit that Affects the Distance to the Closest Hospital on Mortality by Cause of Death

Notes: Data cover the 1999-2008 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Plots show the top 30 causes of death during the sample period ordered by the frequency. The plotted coefficients are from a regression of changes in the outcome on two leads and one lag of the change in the number of hospitals from entry and exit that affects the distance to the closest hospital and controls including an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects. The regressions are weighted by the number of Medicare beneficiaries in each bin and the outcomes are all in per-beneficiary units. The plotted coefficients are the linear combination of the sum of the on-impact effect plus the one-year lag minus the sum of the one- and two-year leads. Error bars show the 95-percent confidence interval. Standard errors are clustered by HSA.
## Table 1: Hospital Summary Statistics

<table>
<thead>
<tr>
<th>Characteristic</th>
<th>Never enter or exit</th>
<th>Year of entry</th>
<th>Ever enter</th>
<th>Year before exit</th>
<th>Ever exit</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Beds</strong></td>
<td>153</td>
<td>62</td>
<td>89</td>
<td>79</td>
<td>109</td>
</tr>
<tr>
<td><strong>Admissions</strong></td>
<td>6,033</td>
<td>1,317</td>
<td>4,257</td>
<td>2,255</td>
<td>3,430</td>
</tr>
<tr>
<td><strong>Inpatient Days</strong></td>
<td>34,197</td>
<td>5,174</td>
<td>17,652</td>
<td>14,474</td>
<td>23,100</td>
</tr>
<tr>
<td><strong>Length of Stay</strong></td>
<td>5.5</td>
<td>4.2</td>
<td>4.4</td>
<td>7.3</td>
<td>6.5</td>
</tr>
<tr>
<td><strong>ER Visits</strong></td>
<td>18,061</td>
<td>7,321</td>
<td>17,242</td>
<td>8,031</td>
<td>10,913</td>
</tr>
<tr>
<td><strong>Inpatient Surgeries</strong></td>
<td>1,967</td>
<td>410</td>
<td>1,275</td>
<td>702</td>
<td>1,176</td>
</tr>
<tr>
<td><strong>Births</strong></td>
<td>729</td>
<td>176</td>
<td>634</td>
<td>164</td>
<td>274</td>
</tr>
<tr>
<td><strong># of Observations</strong></td>
<td>15,367</td>
<td>109</td>
<td>1,299</td>
<td>1,185</td>
<td>14,005</td>
</tr>
</tbody>
</table>

Note: Data cover the 1982-2010 period. The unit of observation is hospital-year. Sample statistics are reported in rows for the sample listed in the columns.

## Table 2: Effect of Hospital Entry and Exit on Capacity, Quantity, and Mortality

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Beds (1)</th>
<th>Admissions (2)</th>
<th>Inpatient Days (3)</th>
<th>ALOS (4)</th>
<th>Mortality (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Hospitals</td>
<td>95 (13)</td>
<td>2,356 (382)</td>
<td>17,485 (3,533)</td>
<td>0.04 (0.02)</td>
<td>0.34 (0.25)</td>
</tr>
<tr>
<td>Lag Number of Hospitals</td>
<td>-10 (8)</td>
<td>-344 (299)</td>
<td>-4,083 (2,487)</td>
<td>0.02 (0.02)</td>
<td>0.03 (0.43)</td>
</tr>
<tr>
<td>On Impact + First Lag</td>
<td>86 (15)</td>
<td>2,012 (503)</td>
<td>13,402 (4,351)</td>
<td>0.06 (0.03)</td>
<td>0.37 (0.38)</td>
</tr>
<tr>
<td>HSA and Year FE</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Additional Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

Note: Data cover the 1982-2010 period. The unit of observation is HSA-year. Numbers reported in the Number of Hospitals rows are coefficients and standard errors (in parenthesis, clustered by HSA) from regressions of changes in each of the dependent variables on the change in the number of hospitals from entry and exit and its first lag, HSA fixed effects, year fixed effects, and demographic and employment controls (see section 4.1). The mortality regression is population weighted. The On-Impact + First Lag row is the sum of the number of hospitals coefficient and its first lag.
Table 3: Effect of Hospital Entry and Exit on Medicare Beneficiaries

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Total Expenditures (1)</th>
<th>Acute Hospital Expenditures (2)</th>
<th>Admissions (3)</th>
<th>Days (4)</th>
<th>ALOS (5)</th>
<th>Mortality (6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Hospitals</td>
<td>22</td>
<td>12</td>
<td>0.0010</td>
<td>0.0036</td>
<td>-0.0043</td>
<td>-0.00010</td>
</tr>
<tr>
<td></td>
<td>(11)</td>
<td>(7)</td>
<td>(0.0004)</td>
<td>(0.0014)</td>
<td>(0.0044)</td>
<td>(0.00010)</td>
</tr>
<tr>
<td>Lag Number of Hospitals</td>
<td>20</td>
<td>8</td>
<td>0.0005</td>
<td>0.0026</td>
<td>-0.0021</td>
<td>-0.00005</td>
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<tr>
<td></td>
<td>(11)</td>
<td>(5)</td>
<td>(0.0004)</td>
<td>(0.0028)</td>
<td>(0.0041)</td>
<td>(0.00008)</td>
</tr>
<tr>
<td>On Impact + First Lag</td>
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<td>20</td>
<td>0.0015</td>
<td>0.0062</td>
<td>-0.0063</td>
<td>-0.00015</td>
</tr>
<tr>
<td></td>
<td>(12)</td>
<td>(11)</td>
<td>(0.0006)</td>
<td>(0.0026)</td>
<td>(0.0032)</td>
<td>(0.00006)</td>
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<td>Zip Code and Year FE</td>
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<td>X</td>
<td>X</td>
<td>X</td>
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<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

$R^2$ | 0.0021 | 0.0015 | 0.0013 | 0.0016 | 0.0016 | 0.0308

# HSAs | 805 | 805 | 805 | 805 | 805 | 805

# Observations | 30,870,674 | 30,870,674 | 30,870,674 | 30,870,674 | 13,937,579 | 30,870,674

Note: Data cover the 1999-2011 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Numbers reported in the rows are coefficients and standard errors (in parenthesis, clustered by HSA) from a regression of changes in the dependent variable on the change in the number of hospitals from entry and exit in the beneficiary’s HSA of residence, one lag of that variable, an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects (see section 4.1). The regressions are weighted by the number of Medicare beneficiaries in each bin. The On-Impact + One Lag row displays the linear combination of the two coefficient estimates.
<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Mortality Rate</th>
</tr>
</thead>
<tbody>
<tr>
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<td></td>
<td>(1)</td>
</tr>
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<td>Number of Hospitals</td>
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<tr>
<td></td>
<td>(7.59)</td>
</tr>
<tr>
<td>Lag Number of Hospitals</td>
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<tr>
<td></td>
<td>(6.74)</td>
</tr>
<tr>
<td>On Impact + First Lag</td>
<td>-12.18</td>
</tr>
<tr>
<td></td>
<td>(9.02)</td>
</tr>
<tr>
<td>HSA and Year FE</td>
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</tr>
<tr>
<td>Additional Controls</td>
<td>X</td>
</tr>
<tr>
<td>$R^2$</td>
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</tr>
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<td># Clusters</td>
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<td># Observations</td>
<td>5,057</td>
</tr>
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</table>

Note: Data cover the 1982-2010 period. Unit of analysis is HSA-Year. The sample includes only HSAs where the dependent variable is non-missing in all years. Numbers reported in the Number of Hospitals rows are coefficients and standard errors (in parenthesis, clustered by HSA) from regressions of changes in each of the dependent variables on the change in the number of hospitals from entry and exit and its first lag, HSA fixed effects, year fixed effects, and demographic and employment controls (see section 4.1). Regressions are population weighted. The On-Impact + First Lag row is the sum of the number of hospitals coefficient and its first lag.
Table 5: Effect of Hospital Entry and Exit on Medicare Beneficiary Deaths

<table>
<thead>
<tr>
<th>Deaths from Net Entry in:</th>
<th>HSA (1)</th>
<th>County (2)</th>
<th>Hospital Service Area (3)</th>
<th>Within 10 Miles (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Number of Hospitals</td>
<td>-0.00010</td>
<td>-0.00011</td>
<td>-0.00018</td>
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</tr>
<tr>
<td></td>
<td>(0.00010)</td>
<td>(0.00010)</td>
<td>(0.00023)</td>
<td>(0.00029)</td>
</tr>
<tr>
<td>Lag Number of Hospitals</td>
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<td>-0.00014</td>
<td>0.00001</td>
<td>0.00000</td>
</tr>
<tr>
<td></td>
<td>(0.00008)</td>
<td>(0.00007)</td>
<td>(0.00016)</td>
<td>(0.00016)</td>
</tr>
<tr>
<td>On Impact + First Lag</td>
<td>-0.00015</td>
<td>-0.00025</td>
<td>-0.00017</td>
<td>-0.00025</td>
</tr>
<tr>
<td></td>
<td>(0.00006)</td>
<td>(0.00008)</td>
<td>(0.00016)</td>
<td>(0.00019)</td>
</tr>
<tr>
<td>Zip Code and Year FE</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
<tr>
<td>Additional Controls</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

$R^2$: 0.031 0.031 0.031 0.031

# Clusters: 805 805 3436 805

# Observations: 30,870,674 30,870,674 30,870,674 30,815,798

Note: Data cover the 1999-2011 period. The unit of analysis is the cohort-race-sex-zip code-county-year. Numbers reported in the rows are coefficients and standard errors (in parenthesis, clustered by HSA or hospital service area) from a regression of changes in the dependent variable on the change in the number of hospitals from entry and exit in the beneficiary’s market of residence, one lag of that variable, an indicator for gender, age fixed effects, race fixed effects, year fixed effects, and zip code fixed effects (see section 4.1). The regressions are weighted by the number of Medicare beneficiaries in each bin. The On-Impact + One Lag row displays the linear combination of the two coefficient estimates.