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The Effect of Public Policy on Health Service Providers

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Abstract

Chapter 1

Low-income, publicly insured admissions historically cost more to treat than the average patient. To ensure that hospitals are reimbursed an adequate amount for care of indigent populations, Medicare reimburses hospitals an additional percentage amount according to federally set financial schedule. The reimbursement cutoff is discrete: at fifteen percent of a disproportionate patient percentage, a hospital is reimbursed an extra 2.5 percent of the standard prospective payment rate. I extend a simple model of hospital quality as a function of insurance reimbursement increases to determine that under certain circumstances there exists a positive relationship between quality and reimbursement. I use Hospital Consumer Assessment of Healthcare Providers and Systems data to analyze hospital ratings around the fifteen percent disproportionate patient percentage cutoff and find that on average, hospital ratings increase by six percentage points. When a subsample of non-profit hospitals is analyzed, hospital ratings increase by an average of 6.5 percentage points, primarily driven by patient facility cleanliness and medical provider communication ratings.

Chapter 2

The Center for Medicare and Medicaid Services (CMS) created the Hospital Compare Program in 2003 to increase transparency between health care providers and consumers. Implemented in 2005, this transparency consists of hospitals' collecting and making publicly available a set of hospital quality score measures. The CMS induced participation by financially penalizing hospitals that did not publicly report a specific subset of these measures (called "starter" measures). Three years into the program, the penalty for non-reporting both the starter measures

and other ("non-starter") measures was increased. I use a difference-in-differences methodology to analyze the effect of the increased CMS penalty on the likelihood that a hospital publicly reported its starter and non-starter measure scores. I find that the penalty had an economically and statistically insignificant effect on the probability that a hospital publicly reported its starter scores but a statistically significant eight percent effect ($p < 0.01$) on whether it reported its non-starter scores. These findings are robust to a series of alternative empirical specifications.

Chapter 3

In 2006, Massachusetts passed a health care reform which required individuals to purchase health insurance and provided subsidized health insurance to the poor. The reform greatly increased the proportion of the state population that was insured. In this study we use a large data set of private health insurance claims to analyze the effect of the increase in the number of insured on physician reimbursement. We find that reimbursement for well-infant visits rose by approximately 4 percent during the reform implementation period, but the increase did not persist. Reimbursement for well-adult visits and appendectomies remained unchanged. Triple difference estimates using appendectomies (for which demand is extremely inelastic) as an additional control group show a 2 percent rise in well-infant visit reimbursement during the implementation period and no effect afterwards or on well-adult visit reimbursement. Estimates imply a temporary increase in the cost of health services with relatively elastic demand following a large scale insurance mandate, such as the Affordable Care Act.

The Effect of Public Policy on Health Service Providers

by

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Chapter 1: Where Does the Money Go? Analyzing the Patient Experience in Safety Net Hospitals

Introduction

For the same illness, low-income patients are more costly to treat than those who are not indigent. To compensate hospitals for the difference in the cost of care between patients, Medicare reimburses hospitals with greater than 15 percent low-income patient admissions an additional percentage of the prospective payment rate. I analyze patient-reported hospital ratings to determine whether funds that should be allocated toward patient care are being used for this purpose.

Twenty-six percent of a hospital's admissions are, on average, low-income patients. This percentage is called a hospital's "disproportionate share," and a hospital's Medicare reimbursement rate directly depends on this percentage. Hospitals that qualify for the disproportionate share reimbursement can expect, on average, to receive an additional 2-3 million dollars yearly from Medicare. Federal Medicare disproportionate share spending reached 9.1 billion dollars in 2009[1]: more than 75 percent of acute-care hospitals in the United States qualified for these funds. Of debate in the economics literature is whether the money is used for patient care: most disproportionate share research examines the impact of additional reimbursement on hospital mortality rates. Using patient satisfaction scores instead of mortality rates, I am able to determine whether patients treated at hospitals that qualify for Medicare disproportionate share payments (DSH) receive different care than those who do not.

Hospital quality and effective use of funds are typically measured using patient outcome data. Until recently, this measure has been the best available data for hospital quality research, despite the fact that patients who are severely ill may choose different hospitals than the less ill (Cutler et al., 2004). Using patient mortality and hospital financial data, Duggan (2000) finds that

[1] http://www.naph.org/Archived-Advocacy/Industry-DSH-letter.aspx?FT=.pdf

not-for-profit and for-profit hospitals in California that qualified for Medicaid disproportionate share payments saw no drop in infant mortality rates, but instead increased their financial holdings dollar for dollar. Baicker and Staiger (2006) find that public hospitals that receive Medicaid DSH funds see a slight decrease in infant and heart attack mortality rates. Lindrooth et al. (2006) uses staffing decisions instead of patient outcomes in a study of the effects of the Balanced Budget Act on safety-net hospitals[2]. When hospital revenues were adversely affected by a change in reimbursement rates, non-safety-net (non-DSH) hospitals reduce nursed staffing by approximately 6 percent and no significant effect was found for DSH hospitals. My study differs from previous work in two fundamental ways: hospital patient experience data are used instead of hospital mortality ratings or staffing ratios and only the Medicare DSH program is evaluated instead of jointly with a state's Medicaid DSH program.

The data used in my study are not new to the medical and health services literature. Countless health services research articles use data from the recent United States Center for Medicaid and Medicare Services (CMS) Physician, Nursing Home, and Hospital Compare programs. Lehrman et al. (2009) provides qualitative analysis that describes the correlations between hospital characteristics and hospital performance on clinical process scores and patient experience measures. They find that small and large hospitals (fewer than 100 beds or greater than 200 beds), non-profit hospitals, and northeastern and mid-western hospitals perform in the top quartile of both patient experience and clinical process measures. The closest research to this study, Werner et al. (2008) use the first three years of the Hospital Compare clinical process data to determine how disproportionate share hospital payment affect hospital performance on clinical process quality measures. The authors separate hospitals with high and low (40% and 5%,

[2] A safety net hospital is one that treats a large volume of Medicaid or Medicare/SSI patients and in most cases is eligible to receive both Medicare and Medicaid disproportionate share funds.

respectively) Medicaid patient percentages, and simulate the effect of a change in reimbursement on the hospital process quality measures. They find that from 2004 to 2006, safety-net hospitals show a smaller performance increase than non-safety-net hospitals. In the health economics literature, Werner et al. (2012), implements a difference in differences strategy using the Nursing Home Compare Program data to find that there is a small, causal, and positive relationship between nursing home "report cards" and the market share of nursing homes. Dafny and Dranove (2008) use Medicare enrollee HMO plan assessments to determine the effect of health management organization (HMO) patient experience scores and Healthcare Effectiveness Data and Information Set (HEDIS) on future changes of plan enrollees.

The first part of the analysis uses hospital ratings from the Consumer Assessment of Healthcare Providers and Systems (CAHPS). These patient-provided data rate aspects of the hospital experience. I restrict the analysis to all and non-profit hospitals just above and below the fifteen percent disproportionate patient percentage (DPP) cutoff, and then regress hospital ratings on DSH status. I include hospital-level fixed effects to control for unobserved time-invariant hospital heterogeneity. The effect of DSH hospital status is identified by hospitals switching over the fifteen percent boundary.

A hospital's level of Medicare DSH reimbursement increases with the number of Medicare patients treated. In my secondary analysis, I perform quantile regression analysis to determine the effect on hospital quality of Medicare admissions in hospitals that qualify for DSH reimbursement. Quantile regression allows for the identification of the effect of increased levels of DSH reimbursement on a hospital's quality score at given points in the quality distribution. I expect that high performing hospitals to be less affected by increased levels of DSH reimbursement as a result of high Medicare patient populations, whereas low-performing hospitals will be positively affected by increased levels of DSH reimbursement. I conduct the quantile analysis for the full sample of hospitals, and then for the subsample of non-profit hospitals.

I find that the impact of the DSH reimbursement increases hospital ratings by eight percent (six percentage points) for all owners, and modestly increases to a ten percent (6.5 percentage points) for non-profits hospitals. DSH status for non-profit hospitals increases ratings in all individual categories, but the effects are significant (approximately a ten percentage point increase) in the hospital cleanliness and medical staff communication categories. This finding is in line with a recent opinion article published by Drs. Herbert Pardes and Edward Miller who argue against a proposed cut in Medicare graduate medical education expenditures.[3] The article states that a cut in graduate medical education funding would affect all services offered by hospitals; it is reasonable to expect that Medicare funding cuts through the DSH reimbursement would similarly affect all hospital services.

The quantile regression esimation yields interesting results. On average, DSH hospital status increases hospital ratings by six percent, but a ten percent increase in a hospital's Medicare admissions increases the median quality score by approximately one percentage point. The effect of Medicare patient admissions, a statistically significant increase of approximately one percentage point, is largest for hospitals in the 25th and 50th percentiles of the score distribution but is small and statistically insignificant for hospitals in the 75th percentile of the quality distribution. These results are indicative that additional reimbursement may make a large difference for hospitals that struggle to meet pay-for-performance benchmarks.

[3] Pardes, Herbert, and Edward D. Miller. "We Can't Afford to Train Fewer Doctors; the Savings from Government Funding Cuts to Graduate Medical Education Aren't Worth the Negative Effect on Patients." Wall Street Journal (Online) (2011).

Disproportionate Share Hospitals

The disproportionate share reimbursement was established in 1985 through the Consolidated Omnibus Reconciliation Act (COBRA). COBRA actualized a switch from cost-based reimbursement to a prospective payment scheme. Lawmakers knew during the creation of the act that indigent patients cost more to treat than those who are not. To compensate hospitals for treating low-income patients, Congress created an upward adjustment to traditional Medicare reimbursement for hospitals that treat a higher share of the needy.[4] Without additional financial incentive, the shift from a cost-based reimbursement scheme (pre-1986) would not necessarily ensure that those who most need intensive care would receive it.[5]

The additional reimbursement is federally funded through the Medicare program. Hospitals submit cost reports to both the federal and state governments at the end of each fiscal year and then are appropriately reimbursed for their "disproportionate patient percentage." The Medicare Disproportionate Share Hospital (DSH) payment adjustments are provider-specific, not patient-specific. Medicare reimbursement and the DSH adjustment are separate programs but can function together. For example, if a patient covered by Medicare enters a hospital that qualifies for a DSH adjustment, then whatever Medicare payments are owed to the hospital for the cost of the patient’s care are multiplied by a DSH adjustment. If the same patient sees a doctor in a private office setting, then the doctor does not receive a DSH adjustment. Medicare reimburses hospitals an additional percentage of the prospective payment rate for treating large percentages (15 percent and over) of indigent patients. This percentage is attached to all Medicare patients treated at the hospital, not only those who are considered indigent.

Federal minimum qualifications determine whether a hospital is considered a DSH hospital. The federal requirements for hospital DSH reimbursement are based on the hospital

[4] COBRA act of 1986
[5] CMS website, disproportionate share hospital definition

type, number of beds in the hospital, and the disproportionate patient percentage that the hospital treats. The current minimum qualifications for DSH status are detailed in Table 1. Most important for qualification as a DSH hospital is the number of low-income patients admitted into a hospital: additional reimbursement is a function of this number. Most hospital types face reimbursement caps of 12 percent additional reimbursement. Currently, the minimum "disproportionate patient percentage"(DPP) necessary for qualification as a DSH is fifteen percent[6], while historically, the minimum percentage has been as high as twenty-five percent.[7] The calculation of the disproportionate share percentage is as follows:

$$DPP = \frac{Medicare\ SSI\ Days}{Total\ Medicare\ Days} + \frac{Medicaid, Non{-}Medicare\ Days}{Total\ Patient\ Days} \tag{1}$$

Equation 1 adds the hospital percentage of dually eligible Medicare and supplemental security income patient-days to the hospital percentage of Medicaid (and non-Medicare) patient-days. Mathematically, the DSH adjustment for hospitals with a specific DPP can be expressed as:

$$DSH\ Adjustment = \left[\begin{array}{ll} 0 & if\ DPP < 0.15 \\ 0.025 + [.65 * (\mathrm{DPP} - .15)] & if\ 0.15 \leq DPP \leq 0.202 \\ 0.0588 + [.825 * (\mathrm{DPP} - .202)] & if\ 0.202 < DPP \end{array}\right. \tag{2}$$

The discontinuity in hospital DSH status adjustment provides an exceptional opportunity to study hospital quality as a function of a change in service price: while hospitals qualify for DSH status when 15 percent of admissions are low-income and publicly insured, there should be no significant difference between observables other than quality in hospitals just above and below

[6] 42 CFR 412.106

[7] Social Security Act

the cutoff. [8]

Differences by Ownership

A non-profit hospital is fundamentally different than a proprietary hospital in that any net profits cannot be redistributed to the owners of a non-profit hospital. A public hospital is owned and operated by the state or Federal government: any net profit belongs to the public.

The hospital ownership literature presents three reasons for why the quality of hospital services may differ due to ownership:

1) A soft budget constraint: This exists when firms can operate and provide services at a cost greater than their revenue. Public hospitals are operated by the government: funds may be (and empirically are) transferred both in and out of the hospital's budget to subsidize other public activities. Numerous studies, most notably Duggan (2000) and Baicker and Staiger (2005) find that government ownership and an increased budget constraint, which intuitively and fundamentally should provide a higher quality of service to those most in need, does not guarantee that a change in treatment is offered. Examining revenue changes before and after the California Medicaid DSH program went into effect, Duggan (2000) finds that every Medicaid DSH dollar received by public hospitals was reclaimed by the state. As a result, Duggan (2000) finds no effect of Medicaid DSH hospital status on infant mortality rates (his measure of quality). Baicker and Staiger (2005) report similar, though more optimistic findings: while a significant portion of DSH program dollars are reclaimed by the state, not all states reclaim DSH funds. As a result of net Medicaid DSH program dollars (DSH money minus an intergovernmental transfer), every hundred dollars spent resulted in a 6.2 percentage

[8] A reimbursement kink exists when a hospital reaches the 20.2 percent DPP; I do not analyze this kink as it does not provide enough variation for me to identify an effect of reimbursement on patient satisfaction.

point reduction in infant mortality and a 1.2 percentage point reduction in post-heart attack mortality.

2) Altruism: Non-profit firms (or the managers and contributors to the non-profit firms) may be considered more altruistic than for-profit firms. Rose-Ackerman (1996) discusses that the utility functions of those who manage non-profits may result in an allocation of resources to activities or services which may not be provided in a for-profit environment. Donors to (and managers of) non-profits may be incentivized by both a "warm glow" (the feeling of well-being when one contributes to a charitable cause), or prestige (others know that one has contributed to a charitable cause). Either motivation yields the same result: services or resources provided to an institution that does not redistribute the funds back to owners (Harbaugh, 1998). Duggan (2000) tests the "altruism" theory by measuring the change in costs of care after a change in DSH status of hospitals with different ownership. If a non-profit hospital is more altruistic, then one would expect to see an increase in the cost of operating a hospital – DSH funds may be allocated to purchasing new equipment, hiring more expensive (better) doctors, and so forth. Instead of an increase in the cost of operation, Duggan (2000) finds that the Medicaid DSH funds are directed to hospital financial holdings and assets.

3) Ease of access to profits: As previously discussed, non-profit hospitals are legally barred from accessing directly any net profit that the non-profit hospital may acquire. Instead, the non-profit or public firm may spend the extra resources on quality of care Hansmann, 1980). From the previous paragraph, Duggan (2000) empirically finds that this is not the case for non-profit hospitals.

Theoretical Model

To theoretically ascertain the effect of additional reimbursement on hospital quality, I rely on previous work by Lindrooth, Bazzoli, and Clement (2006), Hodgkin and McGuire (1994) and Meltzer, Chung, and Basu (2002) for the presented theory. I consider a utility maximizing hospital where utility is a function of both profit and quality:

$$U(\pi, Q) \tag{3}$$

$$\pi = M(Q) * DSH + O(Q) - c(S) - c(Q) \tag{4}$$

$$Q \geq 0 \tag{5}$$

$$\pi \geq \underline{\pi} \tag{6}$$

Equation (3) is the hospital's utility function, which can vary by hospital. Equation (4), the profit function, is a function of hospital quality (Q) and patient illness severity (S): M(Q) is the revenue from publicly insured patients (through Medicare or Medicaid) and O(Q) is the revenue from "other" insurance types. I directly include quality in the hospital utility function because non-profit hospitals may derive additional utility from providing high quality services. I keep separate the costs of patient severity and care quality: the cost of treatment of a severely ill patient is fundamentally separate from the cost of basic customer service. If one were to consider two nurses, identical in skill of patient care, but one with a more pleasing bedside manner than the other, the difference in wages between the two nurses could be argued to reflect the difference in the nurses' "people skills." Hospital quality must be positive or zero, and profits are constrained by a floor condition, with the assumption being that if profits are below the floor, the hospital either closes or merges with another institution.

I can substitute (4) into (3) and solve for first order conditions:

$$P'(Q) + M'(Q) * DSH - C'(Q) + \frac{U_Q}{U_\pi} = 0 \qquad (7)$$

One can totally differentiate the first order conditions to find the change in quality of care when DSH payments increase, under the basic assumptions that the revenue functions from publicly and privately insured patients are concave with respect to quality and that the cost function is convex with respect to quality ($M',\ P',\ C'\ > 0;\ \ M'', P'' < 0\ ;\ \ C'' > 0$):

$$\frac{dQ}{dDSH} = \frac{-M'(Q)}{P''(Q) + M''(Q) * DSH - C''(Q)} > 0 \qquad (8)$$

As discussed in Lindrooth et. al, (2006) and Hodgkin and McGuire (1994), when the budget constraint is binding, the hospital will choose to offer zero quality. That is, when a hospital has no excess profit, no money will be allocated toward increasing hospital quality. An explicit assumption in order to arrive at Equation 8 is that the ratio of utilities with respect to quality and profit in Equation 7 must be constant.

The model indicates that when price or Medicare percentage increase, a hospital's average quality offered will increase as a result. The model cannot address whether a different quality of care is offered to different patients.

Private insurance reimbursement rates may increase as a result of a rise in Medicare reimbursement rates. The relative attractiveness to hospitals of Medicare admissions, as compared to Medicaid patients or the privately insured, rises when Medicare reimbursement rates rise. In order to ensure that patients in their networks maintain access to the same health care provider networks, insurance companies will raise their reimbursement rates. It is possible that the hospital obtainment of DSH status affects the financial relationship between hospitals and insurance companies.

Data

The Centers for Medicare and Medicaid Services (CMS) makes publicly available on its website the impact files for each fiscal year.[9] The impact files contain hospital-aggregated data for each individual hospital fiscal year. The data includes information needed for Medicare reimbursement adjustment, as well as demographic information that I use for control variables. The disjoint timing of each hospital's fiscal year with the release of the impact files makes any interpretation more difficult than if each hospital, state, and the federal government kept the same timeline.[10] I use the hospital impact files from the fiscal year before each patient survey update. If a hospital is considered a DSH hospital at the end of the fiscal year 2006, I estimate the impact of this designation on the patients in the fiscal year 2007.

During the year, a random sample of patients is contacted after their hospital visits to answer surveys about their stays. The patients are asked ten questions regarding their hospital stays; each hospital collects the data and reports it to the Centers for Medicare and Medicaid Services. The aggregated data are made publicly available by hospital which allows me to construct an average overall rating for the hospital and to analyze the hospital rating by specific measure. The questions asked are located in Table 3: I ultimately use for analysis the percentage of patients who responded that the hospital scored "high" or "very good" in a category. For brevity, I call these percentages "hospital ratings." An easy example is room cleanliness: a patient has a choice of three options when asked about how often her room was clean during her stay. The choices are: "always," "sometimes," or "never" clean. The Hospital Compare data reports the percentages of patients who answer affirmatively in each category: if a hospital

[9] https://www.cms.gov, last accessed 3/30/2012

[10] Each hospital is legally obligated to submit its cost report once every year, but the hospital can decide when to report. The federal fiscal year begins on October 1, and the impact files are released during the late summer and before the new fiscal year. The impact files include the information collected since the last Medicare/Medicaid update during the fiscal year.

treated three patients, and all answer in different ways to the “room cleanliness” question (one says “never”, another says “sometimes”, the last says “always”), then the hospital rating is calculated to be 33 percent. If instead two patients answer that their room was “always” clean, while the third says that the room was “never clean,” then the calculated rating jumps to 66 percent.

Demographic Variables

Disproportionate Patient Percentage:

A hospital is designated as a “disproportionate share hospital” if the percentage of low-reimbursement patients (Medicaid patients and Medicare patients who quality for supplemental security income) exceeds fifteen percent. At the end of the year a hospital receives additional funds from Medicare for patient care if the hospital exceeds the fifteen percent threshold.

Teaching Status:

Hospitals with teaching programs receive additional Medicare adjustments to the base prospective payment rate set by Medicare. The adjustments are meant to compensate for the “learning curve” of residents – diagnostics and equipment may be used at a greater rate than would be expected due to the learning environment of the hospital. Despite this, teaching hospitals are considered to be the forefront of the medical field and may perform better on the measure scores than would otherwise be expected. A dummy variable that captures the teaching status of the hospital accounts for fundamental environmental differences in the hospital. Hospitals do not change teaching hospital status during the time frame that I analyze, but may change the number of residents in the program. To account for this in later analysis, I separate teaching hospitals from non-teaching to account for any systematic differentiation in quality scores.

Medicare patient days to total days:

Ultimately, a hospital's reimbursement rate from Medicare rests on the number of number of Medicare patients admitted and whether it admits any Medicare patients at all. If a hospital does not admit Medicare patients, a difference in the reimbursement rate would make no difference to the reimbursement that a hospital would receive if it were above or below the cutoff.

Operating and capital costs to Medicare covered charges ratio:

This number is an indication of how costly the operations (capital and labor) of the hospital are – A number less than one indicates that the standard hospital operating costs for care are greater than the amount reimbursed by Medicare for care provided, while a number greater than one would indicate that the hospital is making a profit from operations. These variables act as proxies for different hospital operating environments; it is reasonable to assume that these variables will capture differences in hospital equipment and competence of staff.

Average daily census and total number of cases:

These variables capture the difference in aptitude of taking care of a great deal of patients; size and number of patients seen may increase the proficiency with which hospitals treat patients. Conversely, it is possible that an overcrowding of patients may decrease the approval rating of the hospital. The average daily census is the average number of patients seen per day in the hospital, while the total number of cases is transfer adjusted – only the patients who stay in the hospital are counted. Neither measure depends on patient insurer.

Estimation

Identification in a regression-discontinuity design rests on the assumption that, barring complete agent manipulation, the likelihood of an observation falling on either side of the cutoff is random. In the case of hospital qualification for DSH funding, hospitals are able to target potential patients but are unable to completely manipulate their disproportionate patient percentage (Duggan, 2000). I include in the robustness checks a test for hospital selection into the Medicare DSH program.

I explicitly assume that when hospitals are within a certain percentage of the disproportionate share hospital qualification cutoff, the allocation of the DSH funds can be treated as exogenous because these hospitals are "close" in their disproportionate patient percentages and are unable to exactly manipulate this variable. Later, I show the effects of DSH status as the percentage from the DSH cutoff changes.

The regression discontinuity design is estimated in a similar fashion as the difference-in-differences setup but executes a different strategy for identifying a control group. The hospitals that fall short of the DSH cutoff are considered to be the counterfactual of the hospitals directly to the right of the cutoff; the assumption that the differences between the hospitals above and below the cutoff are either observable and that unobservable characteristics are time-invariant allows me to identify the impact of the DSH funds.

I use hospital ratings as a dependent variable to determine whether hospitals that receive a DSH reimbursement are using extra resources in a significantly different way that hospitals that are just below the cutoff. The bandwidth is determined using non-parametric cross validation methods as described in Lee and Lemieux (2010) and Imbens and Lemieux (2008). Non-parametrically, I determine the ideal "distance" away from the cutoff of fifteen percent by selecting from a range of bandwidths and fitting an estimated curve to the data both above and below the cutoff. The bandwidth that yielded the lowest mean squared errors within a restricted sample of five percentage points above and below the cutoff was chosen as the “optimal” bandwidth.

I repeat the procedure for the full sample and then for the subsample of non-profit hospitals. Bandwidths of two and three percentage points are chosen for both samples. All of the following tables report estimates using the three percent bandwidth selection, unless explicitly stated otherwise. I do not repeat the analysis for for-profit and public hospitals due to small sample sizes.

I primarily focus on the effect of DSH hospital payment incentives on non-profit hospitals. Non-profit acute care hospitals compose the vast majority of hospital types: approximately two-thirds of the hospitals reported by the Centers for Medicare and Medicaid Services in 2009 are non-profit organizations. For-profit, acute care hospitals are a distant second in sheer numbers: 20 percent of the 2,452 hospitals in the data are for-profit. The remaining 328 hospitals (approximately 13 percent of the data) are publicly owned at the federal, state, or local levels. After constraining my sample to hospitals that have a disproportionate share percentage just around the cutoff, I am left with approximately 500 hospitals (Table 2).

To address concerns about unobserved hospital characteristics such as initial reputation, span of medical services, presence and size of teaching programs, etc., I include hospital-specific fixed effects. Concerns about hospital adaptation to scores and cross-subsidization across hospital services (David et al., 2011) may exist if a several-year panel of data were analyzed. For this reason, I specifically limit the data to the span of five quarters: I make the assumption that if a hospital’s resource allocations changes in response to the HCACPS, the hospital responses are gradual and lagged after the public reports of the scores are released. However, to control for wide variation on both sides of the cutoff, I include provider time trends. I include time dummies as additional controls.

The initial estimating equation is:

$$\begin{aligned} Approval\ Rating_{it} &= \alpha_0 + \beta_1(DSH\ status_{it-1}) + \beta_2(DSH\ status_{it-1} * \%\ DSH\ Patients_{it-1}) \\ &+ \beta_3(\%\ DSH\ Patients_{it-1}) + \lambda_t + \alpha_i + Provider\ Trend_{it} + \epsilon_{it} \end{aligned}$$

Here, I estimate the effect of hospital i*'s* DSH status in the previous year, DSH_{it-1} on hospital approval ratings in the next year, $Approval\ Rating_{it}$. To allow for hospital ratings to have different slopes on either side of the DSH cutoff, I include disproportionate patient percentage (% DSH Patients) in time *t-1*, and the interaction between DSH status and the disproportionate patient percentage. This interaction captures any potential difference in patient satisfaction trends between hospitals above and below the cutoff. The regressions include whether a hospital qualified for DSH adjustments in the previous year to avoid upward simultaneity bias issues with staffing and quality.

I average all ratings to determine whether hospitals just above or below the DSH cutoff have higher ratings *on average* than those below the cutoff. The ratings can also be separated by individual question. I repeat the same analysis for the overall ratings and for the category ratings. Additional analysis includes the covariates previously discussed:

$$\begin{aligned} Approval\ Rating_{it} &= \alpha_0 + \beta_1(DSH\ status_{it-1}) + \beta_2(DSH\ status_{it-1} * \%\ DSH\ Patients_{it-1}) \\ &+ \beta_3(\%\ DSH\ Patients_{it-1}) + \boldsymbol{X}'_{\boldsymbol{it}}\boldsymbol{\beta} + \lambda_t + \alpha_i + Provider\ Trend_{it} + \epsilon_{it} \end{aligned}$$

A testable implication provided by the model is that hospitals with a large Medicare population, or "bite," will have a greater increase in quality than those with a low Medicare population, all else equal. Empirical evidence supporting this hypothesis is mixed: Kaestner and Guardardo (2008) use hospital geographic reclassification[11] as exogenous variation, finding that nursing-intensive patient outcomes, such as the presence of hospital acquired urinary tract infections and pressure ulcers, are unaffected by changes in Medicare reimbursement of up to 10 percent. Contrary to this finding, Lindrooth et al. (2006) and Bazzoli et al. (2004) create financial pressure indices that account for the Medicare reimbursement reductions as a result of the 1997 Balanced Budget Act (BBA). These results indicate that nurse-to-patient ratios and patient lengths of stay decrease, while outpatient visits increase as a result of financial pressure. Wu and Shen (2011) estimate the long-term effects of Medicare reimbursement cuts on hospitals by instrumenting reimbursements with a "BBA bite" index and a Medicare patient volume. With these two instruments, the authors are able to determine whether changing Medicare patient volume or hospital "BBA bites" affected adjusted acute myocardial infarction (AMI) patient mortality rates. The authors find that all discharged patient mortality rates significantly increased by 0.7 to 1.6 percentage points as a result of a hospital's position in the top decile of BBA reimbursement cuts. The authors find that nurse staffing ratios decreased significantly in the long term as a result of declines in hospital reimbursement.

I conduct a quantile regression analysis to determine the effect of large Medicare populations and Medicare DSH status on average hospital quality. The purpose of this quantile regression estimation is to determine how the effects of a hospital’s Medicare population differ across parts of the score distribution. Understandably, if a hospital has a high score, which corresponds to a high percentile in the distribution, the effects of additional reimbursement due to the hospital’s Medicare population will be close to zero. Alternatively, for hospitals with low ratings, the additional reimbursement from a high Medicare population may have a larger impact

[11] A hospital’s geographic classification directly affects the hospital’s Medicare reimbursement percentage.

on hospital quality scores than the reimbursement for hospitals with a smaller Medicare population. I estimate the following equation:

$$
\begin{aligned}
&Approval\ Rating_{it} \\
&\quad = \alpha_0 + \beta_1(DSH\ status_{it-1}) + \beta_2(DSH\ status_{it-1} * \%\ DSH\ Patients_{it-1}) \\
&\quad + \beta_3(\%\ DSH\ Patients_{it-1}) \\
&\quad + \beta_4(DSH\ status_{it-1} * Medicare\ Patient\ Percentage) + \boldsymbol{X'_{it}\beta} + \lambda_t + \alpha_i \\
&\quad + Provider\ Trend_{it} + \epsilon_{it}
\end{aligned}
$$

The coefficient of interest, β_4, is the effect on quality of Medicare DSH reimbursement due to different hospital Medicare populations. I expect a nonnegative coefficient estimate, and interpret the coefficient as the effect on quality of a larger level of Medicare DSH reimbursement.

I do not include hospital fixed effects in the quantile regressions, but do include basic hospital characteristics: urban, teaching, and ownership dummies. By estimating a quantile regression, I test whether a large Medicare population affects hospital quality and also whether different quantiles of the score distribution are dissimilarly affected.

Results

All Owners versus Non-Profit

I find that DSH hospital status on average increases the hospital's overall rating in the full sample by six percentage points (Table 4). When I stratify by non-profit hospitals in columns 3 and 4, this effect jumps to a little under seven percentage points - which translates to roughly a ten percent increase in average hospital rating. After the inclusion of covariates, this effect remains statistically significant and stable. For the non-profit hospital subsample, the coefficient on the interaction term is strongly negative and significant both before and after the addition of covariates. The cost of an increase of low-income patients may not be fully compensated by the DSH payments, and as a result stretching further the hospital's resources. The positive coefficient on the DPP variable captures a hospital's increasing ability to provide care as the number of challenging cases increases.[12]

In the categorical regressions for the full sample (Table 5), DSH status raises the doctor communication ratings measure by eight percentage points. The effect on the other categories is generally positive, but statistically insignificant. Hospitals may choose to distribute their reimbursements differently, which increases the noise of my estimates but leads to an overall positive effect of DSH status on hospital quality. DSH hospital status has a positive impact on all categories when non-profit hospitals are analyzed (Table 6). The largest and statistically significant difference that DSH status has on non-profit hospitals is in the hospital cleanliness, doctor communication, and nurse communication categories, at just under fourteen, nine, and eight percentage points, respectively.

Quantile Regression Results

Table 7 reports the quantile regression estimates. I find that the average quality effects of Medicare DSH reimbursement are primarily driven by the increases in the lower quantiles of the score distribution. Columns 1 and 2 report the estimates of DSH status and increased Medicare admissions on the 25th and 50th percentile of the score distribution for the full sample of hospitals. An increase in DSH reimbursement insignificantly increases a hospital's average score

[12] The point estimates of the effect of hospital attainment of DSH status on hospital quality after inclusion of quadratic and cubic estimates increase, but are statistically insignificant. For robustness, I show that the point estimates of DSH status on average quality scores estimated using local linear regressions are robust to bandwidth selection. However, the regression results with the inclusion of quadratic and cubic terms are available upon request.

by ten percentage points, whereas a ten percentage point increase in Medicare admissions increases a hospital's average quality score by approximately one percentage point. As expected, the effect of the increased Medicare admissions disappears in the 75th percentile of the distribution (Column 3).

Columns 4 through 6 report quantile regression results for the subsample of non-profit hospitals. I find that hospitals in the 25th and 50th percentiles are similarly affected by an increase in Medicare reimbursement through DSH status and also through large numbers of Medicare patient admissions. The effects for non-profit hospitals are similar in magnitude to the estimates for all hospital owners. A ten percentage point increase in Medicare admissions increases average hospital quality in hospitals that qualify for DSH status by approximately one percentage point.

Robustness

Hospital Manipulation and Robustness to Bandwidth Selection

Imbens (2008) and Lemieux (2010) discuss that care should be taken when implementing a study that uses a regression discontinuity design. One of the explicit assumptions with the design is that the running variable cannot be fully manipulated, but partial manipulation does not invalidate the experiment. Full manipulation of the running variable renders the experiment invalid, due to selection. The classic example, and the first use, of regression discontinuity is that of a financial award based on student test scores: students are aware of the score cutoff for financial aid and may adjust study behavior accordingly, but are unable to fully control the outcome of the test. The same principle holds for DSH hospital status: hospitals are aware of the cutoff and may target Medicaid and SSI Medicare patients for admission, but ultimately cannot fully control how many patients are admitted to the hospital.

In Figure 1, I include a standardized frequency histogram of the running variable, hospital disproportionate patient percentage. One can see that there exists an upward slope to the DSH percentage but no jump in the smoothed frequencies at the DSH status cutoff. I use McCrary's test to test for hospital manipulation of the disproportionate patient percentage. McCrary's test of manipulation of the running variable formalizes the rejection of hospital manipulation of the disproportionate share percentage. I run the tests at a bandwidth slightly above and at the optimal bandwidth of the non-profit subsample for reassurance that selection does not occur in the immediate vicinity of the optimal bandwidth.

I implement a placebo discontinuity at 12 percent of DPP and find the estimated effect of the placebo cutoff on patient satisfaction surveys and quality scores by running the same local linear regressions with the same bandwidths (Table 8). On average, I find that the placebo DSH status increases average hospital ratings by a negative 1.2 to a positive 1.7 percentage points, and nothing is statistically significant.[13]

To ensure that my findings are robust to the choice of bandwidth, I include graphs that illustrate the average effect of DSH status as bandwidth increases (Figures 2-3). The effect of DSH status remains greater than zero until approximately 3 percent away from the cutoff. This attenuation could be due to imperfect hospital selection of patients or differences in hospitals due to unobservable variables.

Cross Section versus Fixed Effects

I estimate the effect of DSH status in cross sectional regressions that contain a lagged outcome variable. The coefficient of DSH status in the linear cross-sectional regressions is

[13] The procedure is not included for a placebo at 18% or above because hospital eligibility increases for Medicaid DSH reimbursement and additional Medicare DSH reimbursement. The increased hospital eligibility for the reimbursement programs disqualifies any group above 18% as a suitable placebo group.

similar in magnitude (within a percentage point) to the linear fixed-effects regressions and is statistically significant. Inclusion of quadratic and cubic expressions increases both the magnitude and significance of the estimates, but provides implausibly large estimates of the effects of DSH status. This is likely due to the small bandwidth of 3%.

I am comfortable interpreting the fixed-effect regression estimates as unbiased estimates of the true effect of a hospital's DSH status attainment.

Cost Effectiveness

Most medical research compares patient satisfaction in hospitals in a cross-section but cannot provide any causal inference about hospital characteristics and hospital ratings. Despite this weakness, Jha et al. (2008) provide useful numbers from which I can draw a comparison: after including hospital characteristics, a move from the lowest quartile of nurse to patient-day ratios to the highest quartile is associated with a 5 percentage point change in overall hospital ratings. A move from teaching to non-teaching status is associated with a 0.5 percentage point increase in hospital ratings.

Similar correlations occur in the categorical ratings. A move from hospitals in the lowest quartile of nurse to patient-days ratios (those with the fewest nurses to patients) to hospitals in the highest quartile of nurse to patient-days ratios is correlated with a maximum of a seven percentage point increase in the categorical rankings, with the most improvement in the metrics that quantify satisfaction with nurse communication and hospital recommendation to others. Kutney-Lee et al. (2009) find that improvements in nurse working environments, as measured by nurse leadership, nurse standards for quality care, and nurse-physician relationships, are associated with a maximum of a four percentage point increase in categorical hospital ratings.

My findings, relative to the previous published work discussed above, are large in magnitude. For comparison, I can roughly calculate the amount of money that a six percentage point increase in hospital satisfaction would cost if the money were dedicated solely to an increase in nursing staff. Kutney-Lee et al. (2009) report an average of 5.3 patients per nurse in hospitals with a poor work environment and an average of 4.6 patients per nurse in hospitals that have productive (as determined by a “productivity index”) work environments. If I make the (admittedly unrealistic) assumption that the only difference between these hospitals is the nurse to patient ratio, and assume that nurses make approximately 70,000 dollars per year, then a six percentage point increase in hospital ratings would correspond to a change from approximately a 5:1 patient-to-nurse ratio to a 1:1 patient-to-nurse ratio, distributional assumptions aside. If the hospital operates at a capacity of 200 patients per day, year round, then the costs of maintaining a 1:1 patient to nurse ratio would be fourteen million dollars per year.

If I assume that a hospital is reimbursed 5,000 dollars per patient and discharges 6,000 patients per year, then the hospital receives approximately 750,000 dollars in DSH money (the DSH reimbursement is an additional 2.5 percent of the prospective payment rate). The DSH program is relatively more cost-effective in improving patient satisfaction than a program that focuses solely on nurse staffing.

Policy Implications

The disproportionate share adjustment was designed to reimburse hospitals for a higher cost of care for large numbers of the indigent population. However, it appears that a large number of hospitals that receive these funds have higher hospital ratings than those that do not. This paper reveals that hospitals that are DSH eligible tend to have cleaner patient facilities and better doctor and nurse communication than those who do not receive the funds: evidence presented here suggests that this effect is primarily driven by the quality increase in care provided by lower performing hospitals after receiving Medicare DSH funds. It is unclear whether hospitals that fall short of the DSH qualifications do not have the resources to maintain proper medical or maintenance staff or whether those hospitals that receive the DSH adjustment are expanding current programs because they are no longer constrained by the cost of current care. Lindrooth et al. (2006) suggests the former. However, one could consider the improvement of hospital service as a response to the attainment of DSH status to be a legitimate hospital response to meet the needs of a higher resource-intensive patient population.

A simple way to test this is to examine yearly hospital cost report data: differences in what hospitals are spending on patients could potentially be found in cost-center level data.

It is also possible that hospitals use DSH reimbursement to fund structural improvement. The Federal government offers a substantial subsidization for the installment of hospital electronic medical records (EMR) systems. However, the EMR installment is not costless for health services providers. One mechanism through which the DSH payments could improve hospital performance on patient satisfaction measures is to reduce the cost of communication between physicians and nurses, and between health care providers and patients.

Implications for the Affordable Care Act

The current passage of the Affordable Care Act (ACA) will lead to many more individuals insured through either Medicaid or private insurance. As a direct result of the increase in the number of insured, the federal government plans to reduce, and ultimately, eliminate the Medicare disproportionate share adjustment. The implications for the elimination of the program are unclear: the previous analysis shows that the disproportionate share payments are used, at least in part, by hospitals to increase staff quality. This increase in staff quality could be considered necessary (i.e. hospitals were operating at low staff levels because of an inability to pay salaries) or could be viewed as excessive, in that the DSH payments cover the cost of indigent care and additionally subsidize an expansion of hospital operation.

Conclusion

The historical test of hospital quality has been to examine hospital mortality rates for various conditions. However, the recent availability of consumer satisfaction surveys has allowed for a different, precise, estimation of hospital quality. This research finds that disproportionate share status increases hospital ratings by six percentage points across all owners, jumping to over six percentage points when non-profits are isolated. Extensive robustness checks of these results do not invalidate my findings.

Future research involves hospital cost-center level comparisons of hospitals with DSH status.

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Table 1.1: Reimbursement Rules for Disproportionate Share Hospitals

Hospital Type:	Beds	DPP Threshold	Adjustment	Note:
Urban	0-99	>= .15, < = .202	.025+ [.65*(DPP- .15)]	Can't exceed .12
		> .202	.0588+ [.825*(DPP- .202)]	Can't exceed .12
Urban	>= 100	>= .15, < = .202	.025+ [.65*(DPP- .15)]	No cap
		> .202	.0588+ [.825*(DPP- .202)]	No cap
Rural Referral Center	All	>= .15, < = .202	.025+ [.65*(DPP- .15)]	No cap
		> .202	.0588+ [.825*(DPP- .202)]	No cap
Medicare Dependent Hospital	All	>= .15, < = .202	.025+ [.65*(DPP- .15)]	No cap
		> .202	.0588+ [.825*(DPP- .202)]	No cap
Other rural	0-499	>= .15, < = .202	.025+ [.65*(DPP- .15)]	Can't exceed .12
		> .202	.0588+ [.825*(DPP- .202)]	Can't exceed .12
	>=500	>= .15, < = .202	.025+ [.65*(DPP- .15)]	No cap
		> .202	.0588+ [.825*(DPP- .202)]	No cap

Table 1.2: Control Variables, Above and Below the 15% DPP Cutoff

	All Owners		Non-Profit	
	DSH < .15	DSH >= .15	DSH < .15	DSH >= .15
Disproportionate Patient Percentage	0.13 (0.01)	0.17 (0.01)	0.13 (0.01)	0.17 (0.01)
Operating Cost to Medicare Reimbursement	0.38 (0.15)	0.36 (0.15)	0.40 (0.15)	0.38 (0.14)
Capital Cost to Medicare Reimbursement	0.03 (0.01)	0.03 (0.02)	0.03 (0.01)	0.03 (0.01)
Percent Patients Medicare	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)	0.33 (0.47)
Number of transfer-adjusted cases	4371 (3210)	4600 (3352)	4681 (3351)	4810 (3467)
Average Daily Census	130 (0108)	138 (0113)	140 (0116)	146 (0118)
Teaching	0.37 (0.48)	0.40 (0.49)	0.45 (0.50)	0.45 (0.50)
Number of Providers	408	522	315	390

Standard Deviations in Parentheses.
Note: The number of providers on either side of the DSH cutoff is not stationary through time.

Table 1.3: Consumer Assessment Questions and Hospital Rating Answers

Question:	Patient Response:	Mean	Std. Dev.
How often was the area around patient's rooms kept quiet at night?	Always quiet at night	0.528	0.1022
How often did the nurses communicate well with patients?	Nurses always communicated well	0.716	0.0693
How often was the patient's pain well controlled?	Pain was always well controlled	0.665	0.0609
How often were the patient's rooms and bathrooms kept clean?	Room was always clean	0.660	0.0757
How often did patients receive help quickly from hospital staff?	Patients always received help as soon as they wanted	0.583	0.0851
How often did staff explain about medicines before giving them to patients?	Staff always explained	0.565	0.0676
How do the patients rate the hospital overall?	Patients who gave a rating of 9 or 10 (high)	0.627	0.0959
Would the patients recommend the hospital to friends and family?	Yes, patients would definitely recommend the hospital	0.675	0.1045
Were patients given information about what to do during their recovery at home?	Yes, staff did give patients this information	0.791	0.0543
How often did doctors communicate well with patients?	Doctors always communicated well	0.778	0.0549

Table 1.4: Overall Satisfaction Regression

	All Owners		Non-Profit	
	(1)	(2)	(3)	(4)
VARIABLES	Average Quality	Average Quality	Average Quality	Average Quality
Medicare DSH Hospital	0.0569*	0.0592*	0.0616	0.0664*
	(0.0341)	(0.0347)	(0.0383)	(0.0390)
DSH Hospital X DPP	-0.420*	-0.415*	-0.478*	-0.454*
	(0.225)	(0.234)	(0.251)	(0.258)
DPP	0.398**	0.345**	0.480***	0.415**
	(0.155)	(0.165)	(0.174)	(0.186)
Medicare Percentage X DSH	0.00748	0.00118	0.0134	0.00448
	(0.0194)	(0.0193)	(0.0211)	(0.0226)
Medicare Percentage	0.00992	0.0141	0.00277	0.0100
	(0.0401)	(0.0401)	(0.0445)	(0.0451)
Constant	0.595***	0.632***	0.590***	0.618***
	(0.0307)	(0.0390)	(0.0340)	(0.0467)
Observations	1,533	1,521	1,171	1,159
R-squared	0.744	0.751	0.755	0.763
Number of providern	518	513	393	388
Covariates	No	Yes	No	Yes
State Effects	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
Quadratic	No	No	No	No
Cubic	No	No	No	No

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Categorical Results for Full Sample (With Covariates)

VARIABLES	(1) Patient considers excellent hospital	(2) Pain was always well managed	(3) Rooms / Bathrooms were always clean	(4) Doctors always communicated well	(5) Nurses always communicated well
Medicare DSH Hospital	0.0540	0.0471	0.0874	0.0840**	0.0656
	(0.0537)	(0.0535)	(0.0654)	(0.0378)	(0.0440)
DSH X DPP	-0.400	-0.292	-0.723	-0.609**	-0.449
	(0.357)	(0.344)	(0.461)	(0.267)	(0.297)
Disproportionate Patient Percentage	0.306	0.404	0.779**	0.528***	0.500**
	(0.293)	(0.265)	(0.311)	(0.199)	(0.213)
Constant	0.717***	0.577***	0.552***	0.651***	0.706***
	(0.0813)	(0.0700)	(0.0746)	(0.0507)	(0.0529)
Observations	1,521	1,521	1,521	1,521	1,521
R-squared	0.733	0.666	0.676	0.690	0.660
Number of Providers	513	513	513	513	513
Covariates	Yes	Yes	Yes	Yes	Yes
Provider Effects	Yes	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(6) Staff always provided information	(7) Would definitely recommend to friends	(8) Area outside room was always quiet	(9) Always received help when needed	(10) Discharge information was given
Medicare DSH Hospital	0.000548	0.130*	0.0966	0.0672	-0.0280
	(0.0599)	(0.0712)	(0.0713)	(0.0603)	(0.0371)
DSH X DPP	-0.167	-0.924**	-0.670	-0.480	0.162
	(0.391)	(0.436)	(0.517)	(0.392)	(0.238)
Disproportionate Patient Percentage	0.412	0.532**	0.129	0.267	-0.0230
	(0.293)	(0.240)	(0.329)	(0.313)	(0.170)
Constant	0.465***	0.714***	0.576***	0.582***	0.842***
	(0.0675)	(0.0820)	(0.0718)	(0.0774)	(0.0448)
Observations	1,521	1,521	1,521	1,521	1,521
R-squared	0.687	0.624	0.732	0.705	0.740
Number of Providers	513	513	513	513	513
Covariates	Yes	Yes	Yes	Yes	Yes
Provider Effects	Yes	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Categorical Results for Non-Profit Subsample (With Covariates)

VARIABLES	(1) Patient considers excellent hospital	(2) Pain was always well managed	(3) Rooms / Bathrooms were always clean	(4) Doctors always communicated well	(5) Nurses always communicated well
Medicare DSH Hospital	0.0836	0.0623	0.133**	0.0927**	0.0761*
	(0.0642)	(0.0672)	(0.0669)	(0.0410)	(0.0460)
DSH X DPP	-0.541	-0.419	-0.990**	-0.682**	-0.478
	(0.423)	(0.451)	(0.433)	(0.274)	(0.305)
Disproportionate Patient Percentage	0.402	0.469	1.030***	0.622***	0.526**
	(0.351)	(0.352)	(0.312)	(0.209)	(0.242)
Constant	0.718***	0.561***	0.453***	0.646***	0.662***
	(0.0902)	(0.0732)	(0.0790)	(0.0563)	(0.0600)
Observations	1,159	1,159	1,159	1,159	1,159
R-squared	0.764	0.678	0.749	0.728	0.701
Number of providers	388	388	388	388	388
Covariates	Yes	Yes	Yes	Yes	Yes
Provider Effects	Yes	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

VARIABLES	(6) Staff always provided information	(7) Would definitely recommend to friends	(8) Area outside room was always quiet	(9) Always received help when needed	(10) Discharge information was given
Medicare DSH Hospital	-0.0107	0.152	0.0110	0.0798	-0.0397
	(0.0590)	(0.0958)	(0.0725)	(0.0692)	(0.0456)
DSH X DPP	-0.0981	-0.911	0.138	-0.649	0.270
	(0.383)	(0.594)	(0.489)	(0.469)	(0.299)
Disproportionate Patient Percentage	0.167	0.535*	-0.240	0.458	-0.00508
	(0.301)	(0.277)	(0.356)	(0.375)	(0.204)
Constant	0.517***	0.736***	0.615***	0.547***	0.858***
	(0.0612)	(0.0968)	(0.0815)	(0.0883)	(0.0522)
Observations	1,159	1,159	1,159	1,159	1,159
R-squared	0.724	0.624	0.768	0.739	0.753
Number of providers	388	388	388	388	388
Covariates	Yes	Yes	Yes	Yes	Yes
Provider Effects	Yes	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Quantile Regression Results, Medicare Percentage and DSH Status

	All Owners			Non-Profits		
	(1)	(2)	(3)	(4)	(5)	(6)
	Average Quality	Average Quality	Average Quality	Average Quality	Average Quality	Average Quality
VARIABLES	q25	q50	q75	q25	q50	q75
Medicare DSH Hospital	0.107	0.0966	0.0286	0.253	0.109	0.0345
	(0.0797)	(0.111)	(0.105)	(0.163)	(0.134)	(0.112)
DSH Hospital X DPP	-0.936*	-0.932	-0.346	-1.984**	-1.241	-0.576
	(0.530)	(0.675)	(0.682)	(0.957)	(0.866)	(0.693)
DPP	0.322	0.580	0.252	0.910*	0.841	0.584
	(0.543)	(0.504)	(0.548)	(0.495)	(0.648)	(0.584)
Medicare Percentage X DSH Hospital	0.0743**	0.0738*	0.0308	0.108**	0.141***	0.0765
	(0.0325)	(0.0431)	(0.0497)	(0.0430)	(0.0414)	(0.0504)
Medicare Percentage	0.00436	0.0167	-0.0289	-0.0136	-0.0178	-0.0460
	(0.0568)	(0.0602)	(0.0508)	(0.0691)	(0.0674)	(0.0577)
Constant	0.691***	0.681***	0.732***	0.538***	0.586***	0.637***
	(0.0951)	(0.114)	(0.109)	(0.0894)	(0.0952)	(0.0895)
Observations	980	980	980	725	725	725
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	Yes	Yes	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes	Yes	Yes
Quadratic	No	No	No	No	No	No
Cubic	No	No	No	No	No	No

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Regressions conducted using a linear bandwidth of 2%.

Figure 1.1: McCrary Plot for Manipulation of DPP

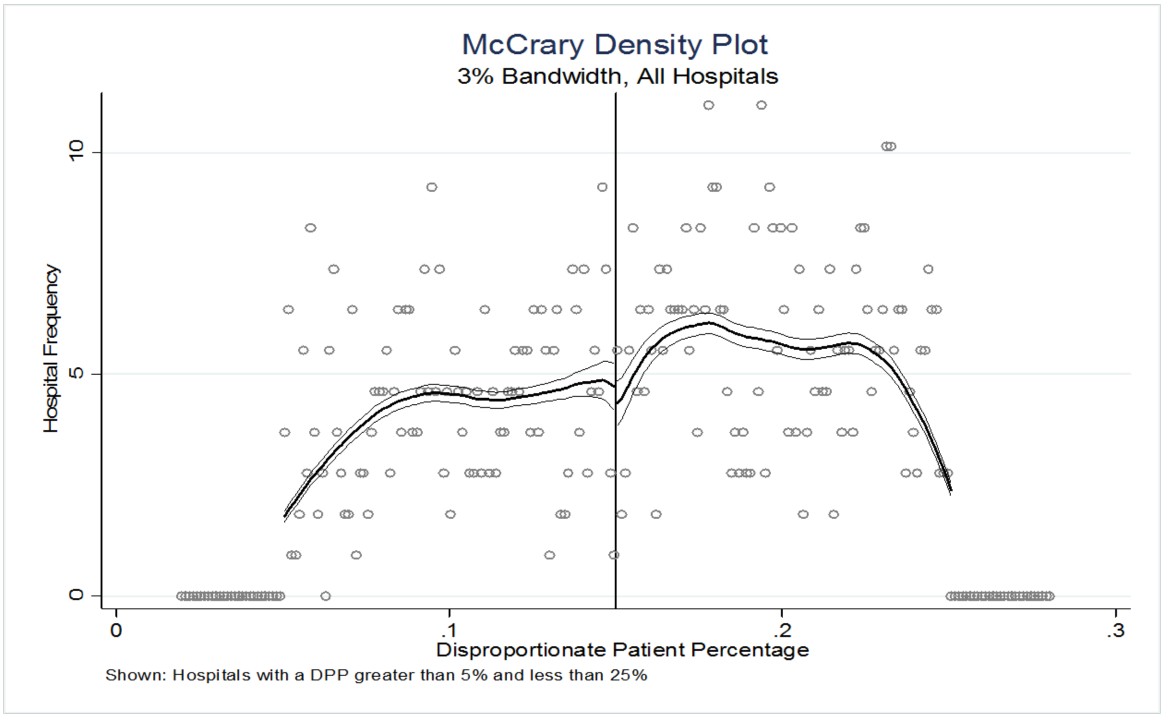

Figure 1.2: Robustness of Bandwidth Choice, All Hospitals

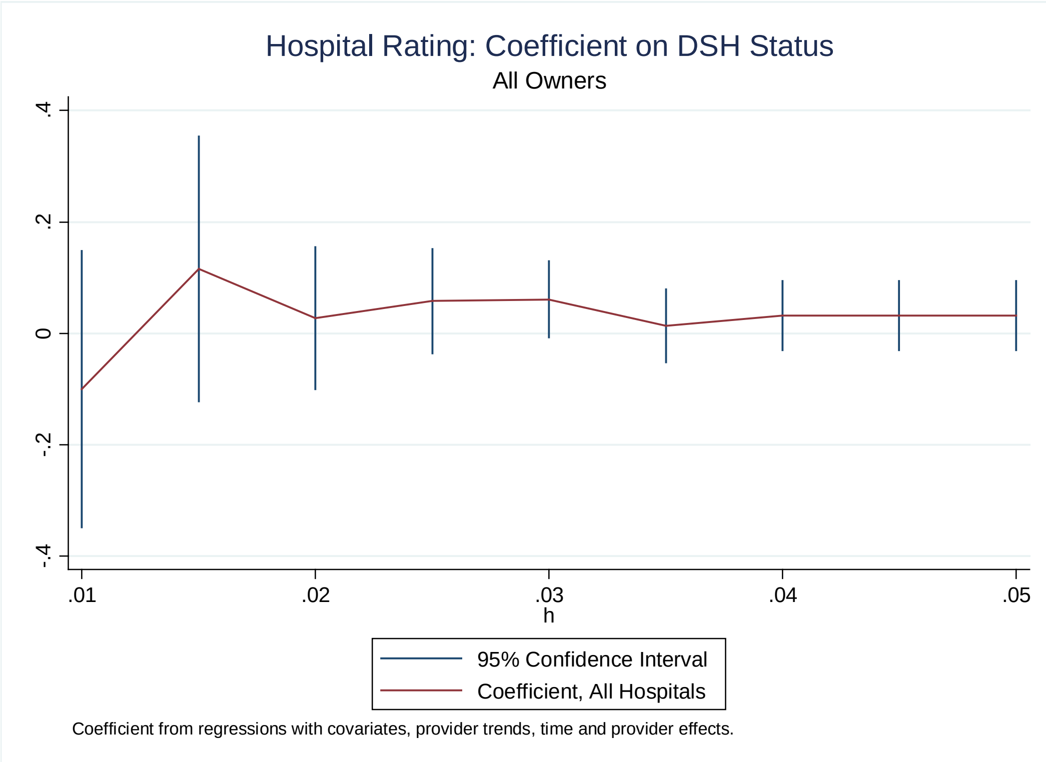

Figure 1.3: Robustness of Bandwidth Choice, Non-Profit Hospitals

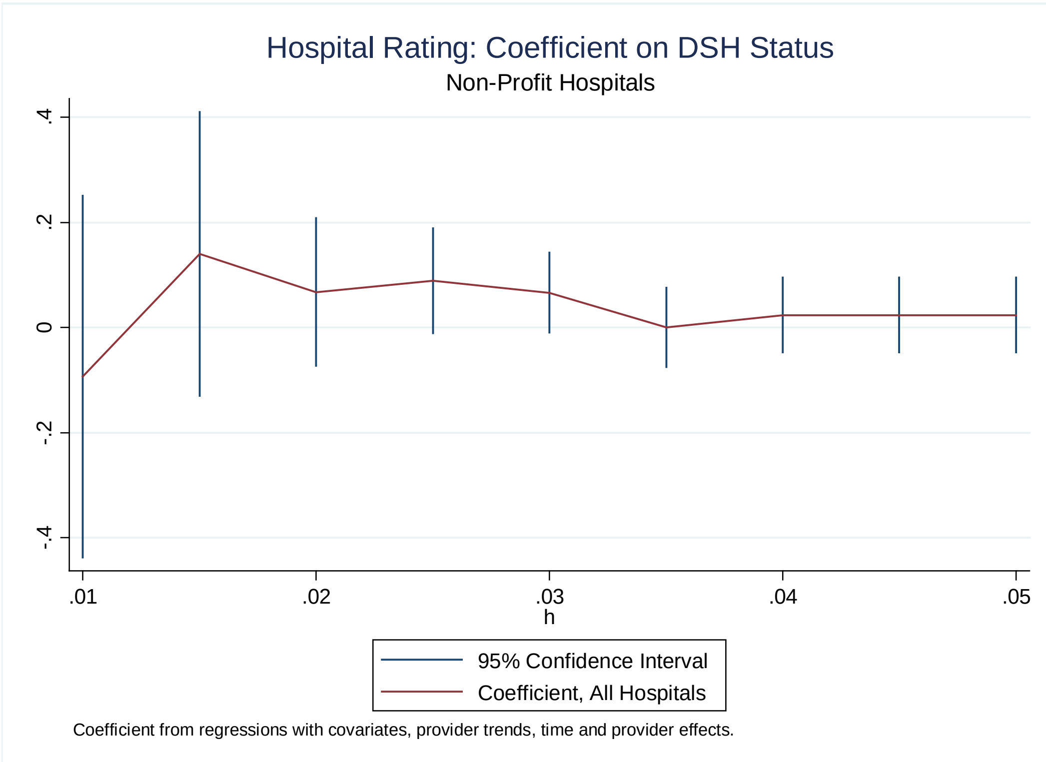

Table 1.8: Placebo Discontinuity Regressions

DPP = 12%

	All Owners		Non-Profit	
	(1)	(2)	(3)	(4)
VARIABLES	Average Quality	Average Quality	Average Quality	Average Quality
Medicare DSH Hospital	-0.0120	-0.00495	0.00493	0.0175
	(0.0285)	(0.0297)	(0.0293)	(0.0318)
DSH Hospital X DPP	0.102	0.0588	-0.0491	-0.162
	(0.238)	(0.236)	(0.243)	(0.240)
DPP	-0.0526	-0.0221	0.118	0.189
	(0.177)	(0.175)	(0.183)	(0.175)
Constant	0.662***	0.706***	0.646***	0.702***
	(0.0189)	(0.0434)	(0.0209)	(0.0431)
Observations	1,321	1,306	1,007	1,000
R-squared	0.751	0.754	0.762	0.769
Number of Providers	435	424	324	318
Covariates	No	Yes	No	Yes
Time Effects	Yes	Yes	Yes	Yes
Provider Effects	Yes	Yes	Yes	Yes
Provider Trends	Yes	Yes	Yes	Yes
Quadratic	No	No	No	No
Cubic	No	No	No	No

Robust standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Chapter 2: Do Hospitals React to Penalties? The Impact of Financial Penalties on Hospital Score Reporting Behavior

Introduction

In 2005, the Centers for Medicaid and Medicare Services (CMS) implemented Hospital Compare, a program dedicated to publicly reporting hospital quality information. Participation in Hospital Compare is voluntary, but the CMS provides a financial incentive for hospitals to submit quality scores to the program. The financial penalty for non-reporting changed for federal fiscal year 2007: I measure the impact that the changed CMS incentive had on hospital rates of participation in Hospital Compare.

The CMS created Hospital Compare in early 2003, and by 2009, over 3,200 hospitals participated in the program by submitting scores on a number of specific, clinically-evaluated, measures that improve specific patient population outcomes. Public knowledge of Hospital Compare is low: in 2008, only 6% of Americans were aware of the existence of the program despite previous efforts in 2007 to publicize both the program and local hospital quality. Despite little common knowledge of Hospital Compare, hospital quality scores are used by state governments, health care researchers, and anecdotally, hospitals themselves, to track progress in the quality of health care services. Wide industry use of the data indicates that the scores hold some value for Hospital Compare participants and that non-participation in the program in itself sends a signal about the quality of a hospital. It is also likely that the 20% of Americans in 2008 who reported familiarity with hospital quality information were viewing Hospital Compare information from an alternate source, instead of the original Hospital Compare website (Kaiser, 2008).

Initially, Hospital Compare publicly posted hospital "scores" for clinically appropriate measures that span three conditions: heart attacks (acute myocardial infarction), heart failure, and pneumonia. In essence, these measures comprise a standardized medical treatment checklist for hospitals to apply to each patient admitted with one of the three conditions. Hospital Compare reports the percent of the time that a hospital adheres to the medical checklist.

Complete non-participation in Hospital Compare came with a reimbursement penalty. Acute care hospitals were informed that they must participate in the program by submitting patient-level quality scores for 10 “starter measures” in order to receive the full insurance reimbursement for treatment of Medicare patients. Hospital non-reporting of the remaining 9 measures was penalty-free, in that a hospital was not financially compelled by the government to collect or publicly report performance data for the rest of the measures. I refer to these measures as "non-starter" measures.

During the first two years of the program, the reimbursement penalty for non-reporting of the starter measures was 0.4 percent; after 2007, this penalty rose to a punitive 2.0 percent and expanded to require reporting of 21 total measures. Hospitals were informed approximately six months in advance of the expansion of the penalty to the non-starter measures. Data collection began in 2004, and the data were published on the Hospital Compare website in 2005.

Use of hospital quality data in economics research is rare: Jung, Feldman, and Scanlon (JHE, 2011) use the early hospital compare data to examine the effect of hospital quality and patient preferences on patient hospital choice, and recent unpublished work by Huang (2011) explores the impact of hospital quality on inpatient reimbursement rates. Neither of these works discusses, in depth, the voluntary nature of Hospital Compare.

Critical access hospitals are exempt from the Hospital Compare program. These specific hospitals are small, geographically remote, hospitals that serve the purpose of providing access to hospital services in rural areas. Medicare reimbursement to critical access hospitals is cost-based, thus rendering them exempt from the Hospital Compare reimbursement penalties. Despite the absence of a financial incentive to report, critical access hospitals report scores from 2005 through 2009. I estimate the impact of the change in the CMS penalty on Hospital Compare starter measure participation rates of acute-care hospitals using a difference-in-differences strategy. Critical access hospital starter measure reporting rates are the first control group for the starter measure analysis.

Critical access hospitals may not be the most appropriate control group for the starter measure analysis because of the remote nature of the hospitals, potentially different patient populations, and different reimbursement schemes. I address the inadequacy of using critical access hospital reporting rates as a control group by including hospital time trends in my empirical estimation as well as additionally using acute-care starter measure reporting rates as a control group to estimate the change in penalty on non-starter measures. I am able to conclude that the estimates from using acute care starter measures are an underestimate of the true effect because starter measures are also subject to a financial penalty for non-reporting.

Another way to estimate the effect of a change in penalty rates is to directly include the penalty percentage for the measures in the regressions. I estimate the effect of a percentage change in reimbursement penalty for both starter and non-starter measures, and compute the marginal effects of a change from 0.4 percent to 2 percent for starter measures and similarly no penalty to 2 percent for non-starter measures.

The primary estimates for the effect of a change in reimbursement from 0.4 percent to 2 percent on the reporting rates of the starter measures are close to zero in magnitude. The largest estimate that I am able to provide for the change in reimbursement penalty on the reporting rate of the starter measures is an effect of 2 percentage points, translated into just over a 2 percent change in reporting rates. Most of my estimates of the effect of a change in penalty rates on starting measure reporting rates are statistically insignificant and extremely close to zero.

The effect of the change in penalty for non-starter measures, from 0 to 2 percent, on acute care reporting rates is much larger in magnitude than the effect for starter measures. For all estimates, I find a maximum penalty effect of 7.5 percentage points (about an 8.5 percent increase), and a minimum of about 4.5 percentage points in regressions that likely may produce estimates with a downward bias. The estimates most consistently point to a 6 to 7 percentage point increase in reporting rates of non-starter measures. The direct policy implications of my findings are unsurprising: hospitals responsiveness to a change in financial penalties depends on the level of the existing penalty and the magnitude of the change. In this case, a penalty of 0.4 percent for the non-starter measures would likely have been sufficient to induce hospitals to publicly report their non-starter measure scores.

Hospital Compare and Medicare Reimbursement

The Hospital Compare quality initiative program began as joint venture between the Centers for Medicare and Medicaid Services (CMS) and the Hospital Quality Alliance (HQA) as a result of the Medicare Prescription Drug, Improvement, and Modernization Act (MMA) of 2003.[14]

The Hospital Compare Program publishes condition-specific hospital quality scores from 2005 onward[15], and starting in 2008, patient evaluations of hospital cleanliness, promptness of service, and experience of stay. The data for the process score, a measure for whether a hospital

[14] The HQA is a national collaboration formed in 2002 between providers, consumers, and oversight agencies. The purpose of the HQA is to work towards providing information about health care providers to consumers. The CMS is a subsidiary of the Department of Health and Human Services and is the governmental agency responsible for the implementation of Medicare, Medicaid, the Children's Health Insurance Program (CHIP) as well as other health-based services.

[15] The calculations of these scores are discussed more extensively in the data section.

provided condition and patient appropriate care for its admissions, and outcome score, a risk-adjusted mortality rate for each condition and hospital, are collected published on the Hospital Compare website after a year of individual hospital data collection.

Score reporting is optional for all hospitals in the sense that participation in the program is not legally required. However, the MMA established that hospitals that do not participate by reporting 10 “starter” quality measures would be financially penalized through their Medicare reimbursements. The 10 starter measures were a subset of the initial 19 available quality measures that measured hospital performance for treatment of heart attacks, pneumonia, and heart failure.[16]

The financial penalty for non-reporting of the starter measures consisted of a reduction of 0.4 percent of the hospital’s total Medicare reimbursement during the years 2005 through 2007. The Deficit Reduction Act (DRA) of 2005 increased both the penalty for non-reporting and the number of measures that a hospital was obligated to report in order to quality for full reimbursement. The number of process measures expanded to the full 21 measures found in Table 1, while the penalty for non-participation increased to 2.0 percent of the Medicare reimbursement rate.

Medicare reimbursement rates are set by the CMS in advance of each procedure; procedures and conditions treated are assigned a reimbursement rate that is a function of resource use intensity, geographic location, and probability of malpractice. The patient’s reimbursement rate is dependent on the Diagnosis Related Group of the patient: upon admission, each patient

[16] The Surgical Care Improvement Project measures are not included in the analysis: these measures were simultaneously introduced and penalized for non-reporting in late 2007. The concurrent introduction of the penalty and the addition of the measures would not allow me to identify an effect of the change in penalty in 2007 on the probability of reporting these measures.

receives a diagnosis – reimbursement for the treatment of a patient's diagnosis is subject to a specific, pre-set reimbursement rate set by Medicare.

Additionally, a hospital receives a higher reimbursement rate if the government determines that it is:

a) A "disproportionate share hospital," which is a hospital that accepts a high share of low-income (and presumably high cost to the hospital) patients.

b) A teaching hospital. The costs of educating future and young physicians include both indirect and direct expenses.

c) An "outlier" case. If a patient receives a high amount of medically necessary treatment that exceeds a certain cost threshold, Medicare will reimburse the provider more than the standard amount.

Ultimately, hospital reimbursement for Medicare patient, *i*, with a diagnosis of *d*, takes the following form if a hospital, *h*, chooses to report its starter scores:

$$Reimbursement_{ih} = (\,Condition\ rate(d)_i) * (adjustment_h).$$

If the hospital declines to report the starter scores in the years 2005 through 2007, then:

$$Reimbursement_{ih} = (Condition\ rate(d)_i) * (adjustment_h) * (0.996)$$

Reimbursement after 2007, with the suppression of either starter scores or non-starter scores:

$$Reimbursement_{ih} = (Condition\ rate(d)_i) * (adjustment_h) * (0.98)$$

The decrease in reimbursement takes place for all hospitals except for Veteran's Administration and critical access hospitals. Critical access hospitals are privy to their own reimbursement agreements with Medicare: critical access hospitals are reimbursed 101 percent of the cost of patient care instead of facing a fixed reimbursement rate per diagnosis. This paper focuses on critical access hospital score reporting as a control group because of the lack of a change in financial penalty, but acknowledges that the different reimbursement scheme and attributes of critical access hospitals may insert doubt as to the suitability of the control group designation.

Public quality scores are effective only if patients and physicians are aware of and believe the scores – an unknown scoring system that produces incredible scores is a waste of resources. To avoid the pitfall of obscurity, the CMS took steps to advertise the hospital compare scores in 2007 by posting local hospital names and quality scores in area newspapers around the United States.

Theoretical

A hospital's decision to participate in Hospital Compare affects hospital revenue in three distinct ways. The first and most direct way is through an immediate drop in the hospital's Medicare reimbursement percentage if the hospital chooses to suppress its score. The second is through a hospital's future quantity of patients: if a hospital reveals a high (or particularly low) quality score, it is likely that the future flow of patients may change as a result of the score (Werner, 2012). The final way that hospital revenue is affected by the hospital's participation in Hospital Compare is that revelation of the score may affect the average patient illness severity (Jung, 2010).

Basic economic theory hypothesizes that hospitals will reveal their scores if the net benefits exceed the costs of reporting. A change in the magnitude of the Medicare reimbursement percentage penalty of non-participation in Hospital Compare directly affects the cost of a non-participating hospital's decision, while leaving the benefits of revelation unchanged. An increase in the cost of non-participation in Hospital Compare may thus compel a score-withholding hospital to participate in the program. The Appendix contains thorough treatment of the estimated model.

I use logistic and ordinary least squares regressions to estimate the effect of a change in the CMS penalty on hospital reporting behavior, including some specifications with lagged hospital demographic controls: patient severity and number, the hospital's overall percentage of Medicare patients, teaching status, wage and capital information, public or private ownership, and the number of nearby hospitals.

Data

Quarterly score data are publicly available on the Hospital Compare website; I am able to determine for each measure and hospital whether a score is available. I consider that a hospital has reported all starter measures when all starter measures for the hospital are reported in the data.[17] In later years, when the non-starter measure penalty is in effect, I require that a hospital report all 19 measures consistently available since 2005. The unit of analysis is hospital-quarter, using the years 2006 through 2009 (quarters 4 through 18 of the program).

Hospital Compare scores are composed of quality scores by condition: acute myocardial infarction (heart attack), heart disease, pneumonia, and surgical infection prevention measures. These condition scores are, in turn, composed of measure scores and patient outcomes. Patient outcomes (risk adjusted mortality rates) receive 10 percent of the total quality score weight, while the remaining 90 percent of the score is calculated by averaging the condition measure

[17] A hospital's measure score may not be reported if the hospital treats fewer than ten patients for the measure. These cases are documented in the data and I do not treat the measure for the hospital as non-reported when this occurs. I treat the scores as suppressed if measure scores are missing without further documentation.

scores. The specific measures vary by condition, but hospital performance on each measure receives equal weight when calculating the condition score.

A quality score for a specific measure is calculated by determining the average hospital percentage of patients who receive appropriate care for the measure. A patient who is admitted with heart failure, for example, should be given an aspirin within 30 minutes of admission. If 70 percent of the patients admitted to the hospital with heart disease receive aspirin when admitted, the hospital receives a quality score of 70 percent. Of course, patients who are physically ineligible for treatment do not count towards the score: if the patient with heart disease is allergic to aspirin, a hospital that withholds the medication would not be punished for recognizing appropriate care for the patient.

National Provider Identifiers are standardized provider identification numbers used amongst private and public health insurance companies; the CMS created this system shortly before the Health Insurance Portability and Accountability Act (HIPAA) of 1996 mandated a standard system of identifying providers. In the case of a hospital merger, the “surviving” institution retains its NPI, and in the case of a hospital closure, the NPI is deactivated. If a hospital restarts after a closure, the NPI is reactivated. I use the NPI in the CMS data to determine whether a hospital merges with another or closes during the period of interest. I determine the latitude and longitude of each hospital address in the year 2005 and calculate through 2009 the number of hospital neighbors.

I am unable to determine the market share (not to mention the local market area) of a hospital, and thus am unable to compute a Herfindahl Index. Instead, I use the number of hospitals within fifteen miles of each individual hospital. The advantage to using distance instead of a Herfindahl index is that distance may proxy for a hospital’s local market area but does not require definition of a total market area. That is, the Herfindahl index must take all hospital market shares into account (a fraction of patients out of the total patient population in the total market area).

A total hospital market area is notoriously difficult to define; different market areas and patient populations may exist for different procedures, and it is entirely possible that hospitals may straddle market areas rather than exist completely within one market. This issue is discussed in greater detail in Chapter 20 of Handbook of Health Economics: The Industrial Organization of Health Care Markets (Dranove and Satterthwaite, 2000).

While using the number of hospitals within a certain distance may seem arbitrary, the method avoids the problem of defining a hospital market area or population. The Dartmouth Atlas Project expands the idea of a Hospital Service Area by determining patient origin (United States Postal Service zip-code) from Medicare Inpatient files – each zip-code faces a plurality rule to which hospital it is assigned. The Dartmouth Atlas reports that the propensity of a local population to patronize a local hospital is high – more than 50% of the country lives in an area where 70% of the patient population relies on the local hospital.

Additional Control variables:

Percentage of disproportionate share patients:

A hospital is designated as a "disproportionate share hospital" if the percentage of low-reimbursement patients (Medicaid patients and Medicare patients who quality for supplemental security income) exceeds fifteen percent. When a hospital exceeds this fifteen percent threshold, it receives additional funds from Medicare for care provided its patients.

Medicare patient days to total days:

Medicare patient days are defined as the percentage of total patient days in the hospital spent by Medicare patients. Ultimately, a hospital's reimbursement rate from Medicare rests on whether it admits any Medicare patients. If a hospital does not admit Medicare patients, then a difference in the reimbursement rate should not affect the hospital's reporting behavior.

Operating and capital costs to Medicare covered charges ratio:

This number is an indicator of how costly the operations (capital) of the hospital are – A number less than one indicates that the standard hospital operating costs for care are greater than the amount reimbursed by Medicare for care provided, while a number greater than one would indicate that the hospital is making a profit from operations. These variables act as proxies for different hospital operating environments; it is reasonable to assume that these variables will capture differences in hospital equipment and competence of staff.

Hospital Ownership:

There is discussion in the literature that the appropriate model for non-profit hospital operation is a utility function, rather than a profit function that is appropriate for proprietary hospitals. Research examining the difference between the two finds differences in resource allocation between hospitals of different ownership but similar behavior in terms of overall treatment of patients (Duggan, 2000). Nonetheless, I include a variable that indicates the hospital's yearly ownership status, including those that switch from profit to non-profit during 2005-2008.

Average daily census, number of beds, and total number of cases:

These variables capture the difference in aptitude of taking care of a great deal of patients; size and number of patients seen may increase the proficiency with which hospitals treat patients. Conversely, it is possible that an overcrowding of patients may decrease the quality score of the hospital.

Resident to bed ratio:

As previously discussed, hospitals with teaching programs receive additional Medicare adjustments to the base prospective payment rate set by Medicare. The adjustments are meant to compensate for the “learning curve” of residents – diagnostics and equipment may be used at a greater rate than would be expected due to the learning environment of the hospital. Despite this, teaching hospitals are considered to be the forefront of the medical field and may perform better on the measure scores than would otherwise be expected. A control variable that captures the teaching status of the hospital accounts for fundamental environmental differences in the hospital.

Table 2 presents summary statistics for acute-care hospitals. Table 3 presents the federally mandated requirements to be classified as a critical-access hospital.

Estimation

The reimbursement penalty changed from 0.4 percent to 2.0 percent in late 2007 for the ten starter measures in acute care hospitals. To determine the effect of this specific change in reimbursement penalty on the probability of reporting the ten starter measures, I conduct a difference-in-differences estimation using the starter measures that critical access hospitals report as a control group. Conditioning on acute care hospitals, I estimate:

1) $\mathrm{Pr(Report\ starter\ measures|Acute\ Care\ Hospital)_{it}} = \alpha + \mathrm{Reimbursement\ Change_t} * \beta + \mathrm{X_{it-1}}\,\mathrm{B} + \gamma_t + \lambda_i + \epsilon_{it}$

Equation 1 estimates the change in probability of acute care hospital participation in Hospital Compare as a function of the Medicare reimbursement penalty change, lagged hospital demographics, time-invariant hospital effects, and time effects.

Critical access hospitals are not subject to a change in reimbursement and have mandated stationary characteristics to remain classified as a CAH, so for this specific type of hospital the estimating equation becomes:

2) $\mathrm{Pr(Report\ starter\ measures|Critical\ Access\ Hospital)_{it}} = \alpha + \gamma_t + \lambda_i + \epsilon_{it}$

Equation 2 estimates the change in the probability of critical access hospital participation in Hospital Compare as a function of time-invariant hospital effects and time effects.

When the two equations are combined, the final difference-in-differences estimating equation becomes:

3) $\mathrm{Pr(Report\ starter\ measure)} = \alpha + \mathrm{Acute\ Care_i} \times \mathrm{Reimbursement\ Change_t} * \beta_1 + \mathrm{Rembursement\ Change_t} * \beta_2 + \mathrm{Acute\ Care_i} * \beta_3 + \mathrm{X_{it-1}B} + \gamma_t + \lambda_i + \epsilon_{it}$

Equation 3 provides the estimate of interest, β_1, the effect of the reimbursement change on acute-care hospitals. Theoretically, $\beta_1$is anticipated to be greater than or equal to zero: either the penalty has a positive effect on hospital reporting or it has zero effect.

I am able to estimate the effect of a change in reimbursement on hospital reporting using both logistic and ordinary least squares regression techniques. Logistic regressions ensure that the predicted probabilities of reporting are between zero and one, and the ordinary least squares

(OLS) regressions include hospital level fixed effects.[18] Logistic regressions assume a binomial error distribution, while the OLS errors are assumed to be normally distributed. All logistic marginal effects are manually calculated and standard errors are clustered at the hospital level.

A more interesting question is the effect on reporting of the reimbursement penalty that changes from zero percent to 2 percent in 2007 for the non-starter measures. I anticipate that since most hospitals respond to the initial incentive of zero to 0.4 percent for the starter measures, a larger effect on reporting of the reimbursement penalty increase will be found for the non-starter measures. I can estimate the effect of the change in reimbursement for the non-starter measures by using as a control group the initial starter measures in acute care hospitals. By using the starter measure reporting rates as a control group, I avoid the concern that simple differencing may not control for unobservable time-varying hospital characteristics.

I estimate the effect of a change in the penalty on non-starter measures by using as a control group the starter reporting rates of acute care hospitals. I estimate, similar to Equation 3:

4) $$\Pr(\text{Report}) = \alpha + (\text{non} - \text{starter penalty enacted} \times \text{non} - \text{starter measure}) * \beta_1 + \text{non} - \text{starter measure} * \beta_2 + \text{starter penalty enacted} * \beta_3 + \text{starter trend} + X_{it-1}B + \gamma_t + \lambda_i + \epsilon_{it}$$

The coefficient of interest is β_1, the effect of the interaction between the non-starter penalty enactment and the non-starter measures. β_1 should be positive: a financial penalty enacted on non-starter penalties will have a positive effect on the reporting rate of non-starter measures. I include indicators for the different measures, a penalty indicator, and a reporting time trend for the different kind of penalties as well as lagged hospital demographic variables.

[18] Horrace and Oaxaca (2006) state that OLS estimates are unbiased and consistent if all predicted probabilities are bounded within the unit interval. Post-estimation, I verify that this condition holds,

This technique eliminates the concern that unobserved hospital-level characteristics may bias the estimates, but introduces a possible downward bias to the estimates of the change in reimbursement. The starter measures are also subject to a change in reimbursement, but from 0.4 percent to 2 percent, instead of zero to 2 percent. If regression estimates reveal that there is a change in reporting behavior as a result of the change in the non-starter penalty using the starter measures as a control, I may be subtracting a change in reporting behavior due to an increase in penalty for the starter measures:

5) $\mathrm{Pr}(\mathrm{Report} \mid \mathrm{non-starter\ measures}) = \alpha + \mathrm{non-starter\ penalty\ enacted}_t * \beta_1 + X_{it}B + \gamma_t + \lambda_i + \epsilon_{it}$

6) $\mathrm{Pr}(\mathrm{Report} \mid \mathrm{starter\ measures}) = \alpha + \mathrm{starter\ penalty\ change\ enacted}_t * \beta_2 + X_{it}B + \gamma_t + \lambda_i + \epsilon_{it}$

If I assume that the effect of the starter reimbursement penalty change, β_2, is equal to 0 (evidence supporting this is reported in Table 4), then the estimate of the non-starter reimbursement change is unbiased. However, if the starter reimbursement change has a positive effect on starter reporting, then:

7) $\mathrm{Pr}(\mathrm{Report} \mid \mathrm{non-starter}) - \mathrm{Pr}(\mathrm{Report} \mid \mathrm{starter}) = \mathrm{Non-starter\ penalty\ enacted}_t * \beta_1 - \mathrm{Starter\ penalty\ change}_t * \beta_2 < \mathrm{Non-starter\ penalty\ enacted}_t * \beta_1$

Thus the estimates from the second difference-in-differences regression results are a lower bound estimate of the effect of penalty change in non-starter reimbursement.

Results

Effect of Penalty Change on Starter Measures

Presented in Table 4 are the estimates of the change in the reimbursement penalty on starter measures. I find that, in most cases, there is an extremely small, insignificant, and negative effect of the reimbursement penalty change on starter measure reporting.

The logistic regression estimate in the first row of Column 1 in Table 4 shows a statistically insignificant and positive effect of the penalty increase on starter measure reporting rates of about 1.8 percentage points. Adding hospital covariates decreases the estimate to approximately 1.7 percentage points. The addition of hospital fixed effects in the ordinary least squares regressions in column 3 reduces the magnitude of the effect of the penalty change on starter measures to approximately a 0.2 percentage point effect. When I add covariates to the linear model with fixed effects, the coefficient rises to about 0.5 percentage points and remains insignificant. I conclude from these estimates that the effect of the change in penalty on starter measures is not statistically different from zero.[19]

Effect of Penalty Change on Non-Starter Measures

Table 5 reports the non-starter measure results from using starter measures as a control group. The marginal effect of the change in the penalty on non-starter measures is reported in the first row. The logistic regressions (Columns 1 and 2) report an increase in reporting of about 8.2 percentage points, and the fixed effects regressions (Columns 3 and 4) report estimates of a change in reporting rates of 4.5 percentage points.

The OLS estimates, which add hospital-level fixed effects to the estimation, are approximately the same magnitude as the previous estimates. Without demographics, the OLS estimates that the effect of the penalty enactment is approximately a 4.5 percentage point change.

[19] Analysis is also conducted using a conditional logistic function: in this case, the marginal effect of the penalty on the starter measures is found to be a statistically insignificant and negative; the model predicts no marginal effects in the treatment group because the predicted probability of reporting is high, with slight movement in the critical access starter measure control group.

The addition of demographics does not affect the estimate of the penalty change. With a baseline in these estimates of about an 85 percent reporting rate, the magnitudes of the estimates in Columns 3 and 4 yield approximately 5 percent changes in the probability of reporting. The estimated effect on reporting of a change in penalty of the non-starter measures is much larger in magnitude than the estimates of the effect on reporting of a change in penalty effect for the starter measures.

Robustness

Pre-trends:

I also test for pre-policy trends in both the starter difference-in-differences equations and the two non-starter difference-in-differences equations. To test for different time trends in the analysis, I only use the pre-treatment data, (all quarters available during the year 2006 and most of the year 2007), and implement a placebo penalty on the treated variables. The placebo penalty analysis assumes that the penalty began in the beginning of 2006.

Results for the pre-trend analysis are reported in Table 6: I find insignificant and mixed-sign results for the starter measures that use critical-access hospitals as a control group. Figure 1 further illustrates the absence of a pre-trend by in the starter measure difference-in-differences equations by plotting the starter measure reporting rates for both types of hospitals across time. The logistic regressions that do not assume an individual provider trend through time yield negative results, while the inclusion of fixed effects yields a significant result of about 1.5 percentage points. The insignificant but positive results possibly point to a different trend that could lead to an underestimate of the true effect of the change in policy.

Table 7 reports pre-trend estimates that indicate significantly different positive reporting trends exist for the non-starter measures. Columns 1 and 2, which report logistic regression marginal effects, show a large difference in the reporting trends between types of measures. After the addition of fixed effects in Columns 3 and 4 of Table 7, the placebo policy shows that reporting rates for non-starter measures increase by 2 percentage points in response to the "policy." It is likely that since most hospitals already fully reported the starter measures, the lower baseline for the non-starter measures allowed for an upward trend. These results reveal what was already suspected: non-starter measures may be subject to different trends than the starter measures; thus, the original estimated effect of the penalty change on reporting could be an underestimate of the true effect. To illustrate these trends, I provide a graph of the reporting trends for non-starter and starter measures in Figure 2.

Marginal Effects of a Reimbursement Penalty Increase:

I discussed in previous sections the fact that the estimate of the change in the penalty on the non-starter measures may an underestimate of the change in reporting. Another way of determining the marginal effect of a change in the penalty rates is to directly estimate the effect of penalty percentage rates.

I estimate the following equation:

$$8)\quad \mathrm{Pr(Report)} = \alpha + \mathrm{Reimbursement\ Penalty} * \beta_1 + (\mathrm{Reimbursement\ Penalty})^2 * \beta_2 + (\mathrm{non-starter}) * \beta_3 + (\mathrm{non-starter} \times t) * \beta_4 + X_{it}B + \gamma_t + \lambda_i + \epsilon_{it}$$

The penalty rates are included in the equation as a quadratic function to capture the non-linearity of reporting behavior as a function of the penalty change. I use the same hospital demographic estimates included previously in the regressions and as well as time and provider fixed effects.

The effect of a percent change in penalty rate is found in Table 8. My estimates of the effect of a penalty increase from 0 to 1 percent imply an increase in the probability of reporting by approximately 11 percentage points (Columns 1 and 2), which becomes 8 percentage points with the inclusion of fixed effects (Columns 3 and 4). Using estimates in Column 4, I calculate that the penalty increase on the voluntary measures has approximately a 6.5 percentage point effect on the probability of reporting, and the penalty increase (from 0.4 percent to 2 percent) affects hospital reporting by approximately 2 percentage points. From a baseline of 75 percent reporting rates, the change in penalty from 0 percent to 2 percent increased the reporting rates of non-starter measures by approximately 8.5 percent, and a penalty increase from 0.4 percent to 2 percent raised reporting rates of starter measures by 2.5 percent.[20]

To determine whether these results are robust to the inclusion of critical-access starter measures, which do not encounter a reimbursement penalty for non-reporting, I estimate:

9) $$\mathrm{Pr(Report)} = \alpha + \mathrm{Reimbursement\ Penalty} * \beta_1 + (\mathrm{Reimbursement\ Penalty})^2 * \beta_2 + (\mathrm{non-starter}) * \beta_3 + (\mathrm{non-starter} \times \mathrm{t}) * \beta_4 + \mathrm{Acute\ Care} * \beta_5 + \mathrm{X}_{it}\,\mathrm{B} + \gamma_t + \lambda_i + \epsilon_{it}$$

Table 9 reports that the addition of the critical-access starter measures are similar to the original regression that includes only the acute care starter and non-starter measure reporting rates. An increase in penalty from 0 percent to 1 percent increases reporting rates by approximately 13 percent in the logistic regressions, while the OLS fixed effect regressions report a reporting increase of about 10 percentage points when the penalty increases from 0 to 1 percent. I calculate using Column 4 that an increase in the penalty from 0 to 2 percent raises the probability of reporting by 7.5 percentage points, an increase of about 8 percent, while the estimate of the increase from 0.4 percent to 2 percent remained at 2 percentage points (an increase of slightly larger than 2 percent).

[20] The marginal change in reporting rates is manually calculated: i.e. a change in reporting rates when the penalty moves from 0.4 percent to 2 percent is calculated (from Column 4 in Table 8): (.132*2-.0495*4)-(.132*.4-.0495*.16) = .021.

Discussion

Current research indicates that an increase in Medicare reimbursement leads to an increase in private insurance reimbursement, as private insurers must compensate hospitals for the change in relative financial attractiveness of private enrollees (Nicholson, American Society of Health Economists Conference presentation, 2012). However, in my analysis, if a hospital chooses not to report a measure, this is a hospital-level decision in response to federal reimbursement, rather than an exogenous change in federal reimbursement. I have not found any research or evidence suggesting that private insurers made reimbursement changes based on hospital quality scores or hospital reporting decisions: this is an avenue for future research. As this paper stands, the theoretical model relies on the assumption that the reimbursement rates that a hospital receives for treating privately insured individuals remain unaffected by the change in the CMS penalty.

It is possible that the number of nearby hospitals is endogenous to a hospital's quality. However, the analysis is robust to the exclusion of the competition measure in the regressions. The robustness of the estimates raises the natural question of whether the Hospital Compare quality measures accurately capture a hospital's actual quality, or instead, provide a metric on which performance may be easily manipulated by a hospital to appear of a higher quality than it is. The latter situation seems most probable, as the measures generally tend to include low-cost interventions, both in terms of the hospital's cost of care for the patient, and insurance payouts (public and private). An area with research potential is that which analyzes the content of Hospital Compare scores and their impacts on consumer, hospital, and insurer behavior. The inclusion of post-2008 measures may help to address this question in later work.

The importance of this paper lies in the implications for the implementation of the Patient Protection and Affordable Care Act (PPACA) of 2010. Public quality score schemes for full Medicare reimbursement are currently mandated for hospitals and nursing homes. Starting in 2014, physician reimbursement rates will also be affected by both the presence and content of quality scores. Without the reimbursement penalty, providers would likely not participate fully in their quality programs and instead would report the metrics that shed the most favorable light on the provider. This research shows that these financial incentives play a crucial role in providing relevant information to both patients and providers.

Conclusion

The Hospital Compare data is potentially an important tool for the assessment of hospital performance on standardized, clinically appropriate, measures of hospital quality. However, before researchers can attempt to analyze the data, the issue of hospital selection into the program must first be addressed.

This paper specifically determines the effect of a changed financial penalty on hospital selection into Hospital Compare. The financial penalty varies by measure, which is an important source of variation for the analysis. For the ten "starter" measures, I find that the change in 2007 in the penalty for non-reporting had little effect: my top estimate of an increase of 2 percent in reporting rates is small compared to my estimates of 7.5 percentage point increases in reporting rates due to the penalty change from 0 to 2 percent. Non-starter measures were more responsive to the change in penalty, revealing that hospitals likely respond non-linearly to changes in penalties for reporting: my lowest estimate for a change in reporting is about a 5 percent change, with a top estimate of approximately 8 percent.

My results suggest that the presence of any penalty for non-reporting is more important than an increase in a penalty for non-reporting. However, the increase in the penalty provides more federal revenue from the non-reporters. A primary assumption of my model is that the cost of collecting and reporting data is low; if this assumption is incorrect, it may be possible that non-reporting hospitals are already in financial distress and are unable to organize their resources in a way that would allow participation in Hospital Compare. More research is needed to investigate other reasons for Hospital Compare participation.

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Table 2.1: Hospital Compare Quality Measures

Condition	Measure	Starter Measure
Heart Attack (Acute Myocardial Infarction)	• Aspirin at arrival	Yes
	• Aspirin prescribed at discharge	Yes
	• ACE inhibitor (ACE–I) or Angiotensin Receptor	Yes
	• Beta blocker at arrival	Yes
	• Beta blocker prescribed at discharge	Yes
	• Thrombolytic agent received within 30 minutes	
	• Percutaneous Coronary Intervention (PCI)	
	• Adult smoking cessation advice/counseling	
Heart Failure (HF)	• Left ventricular function assessment	Yes
	• ACE inhibitor (ACE–I) or Angiotensin Receptor	Yes
	• Discharge instructions	
	• Adult smoking cessation advice/counseling	
Pneumonia (PNE)	• Initial antibiotic received within 4 hours of	Yes
	• Oxygenation assessment	Yes
	• Pneumococcal vaccination status	Yes
	• Blood culture performed before first antibiotic	
	• Adult smoking cessation advice/counseling	
	• Appropriate initial antibiotic selection	
	• Influenza vaccination status	
Surgical Care Improvement Project (Omitted from	• Prophylactic antibiotic received within 1 hour	
	• Prophylactic antibiotics discontinued within 24	

Table 2.2: Summary Statistics for Acute Care Hospitals and Critical Access Hospitals

Summary Statistics, Acute Care Hospitals

	Before Penalty Change		After Penalty Change	
Variable	Mean	Std. Dev.	Mean	Std. Dev.
Report All Starter Measures	0.93	0.25	0.92	0.27
Report All Non-Starter Measures	0.85	0.36	0.93	0.26
Operating Cost to Charge Ratio	0.39	0.17	0.37	0.16
Capital Cost to Charge Ratio	0.03	0.02	0.02	0.02
Resident to Bed Ratio	0.06	0.15	0.06	0.15
Percentage Medicare Patients	0.47	0.17	0.48	0.17
Number of Cases	3256	3151	3208	3129
Number of Beds	178	165	181	168
Average Daily Census	111	126	113	129
Case Mix Index	1.33	0.33	1.35	0.33
Disproportionate Patient Percentage	0.26	0.18	0.26	0.18
Number of Neighbors within 15 miles	3.54	2.13	3.57	2.12

Reporting Means, Critical Access Hospitals

	Before Penalty Change		After Penalty Change	
Variable	Mean	Std. Dev.	Mean	Std. Dev.

Report All Starter Measures	0.69	0.46	0.66	0.47
Report All Non-Starter Measures	0.59	0.49	0.59	0.49

Table 2.3: Critical Access Hospital Mandated Characteristics

Federally Mandated Critical Access Hospital Eligibility Requirements	
Beds:	No more than 25 Beds
Length of Stay:	No more than 96 hours (4 days) average length of stay.
Medicare:	Must be a participant
Owner:	Not-for-profit
Location:	At least 35 miles from another hospital, 15 for mountainous areas.
24 Hour Services:	Must make available
Hospital System:	Must participate in a rural health network, with at least one acute-care hospital
Credentials:	Must review staff and quality within the network

Figure 2.1: Starter Measure Reporting Rates

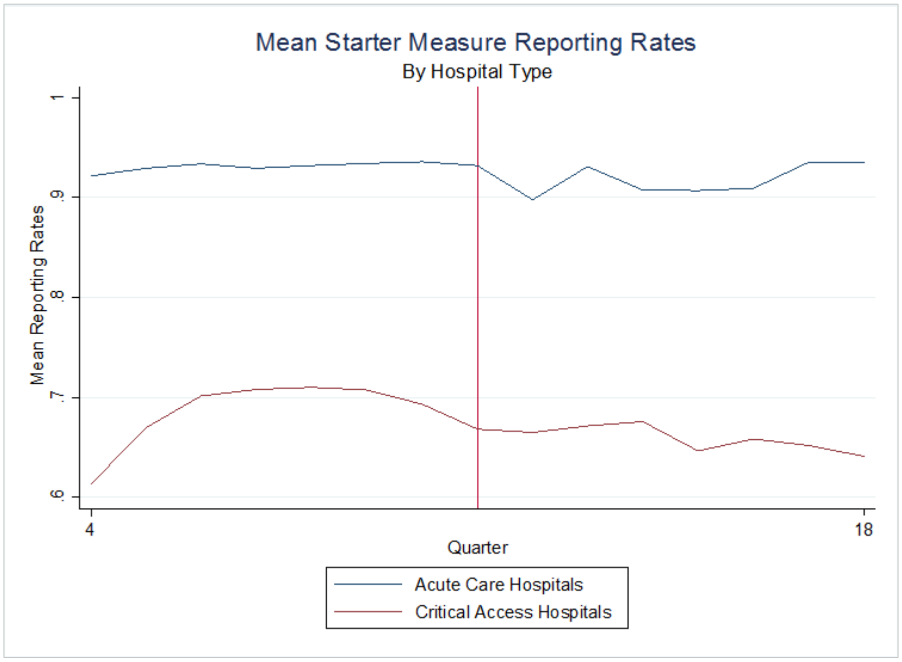

Figure 2.2: Mean Reporting Rates for Starter and Non-Starter Measures, Acute Care Hospitals

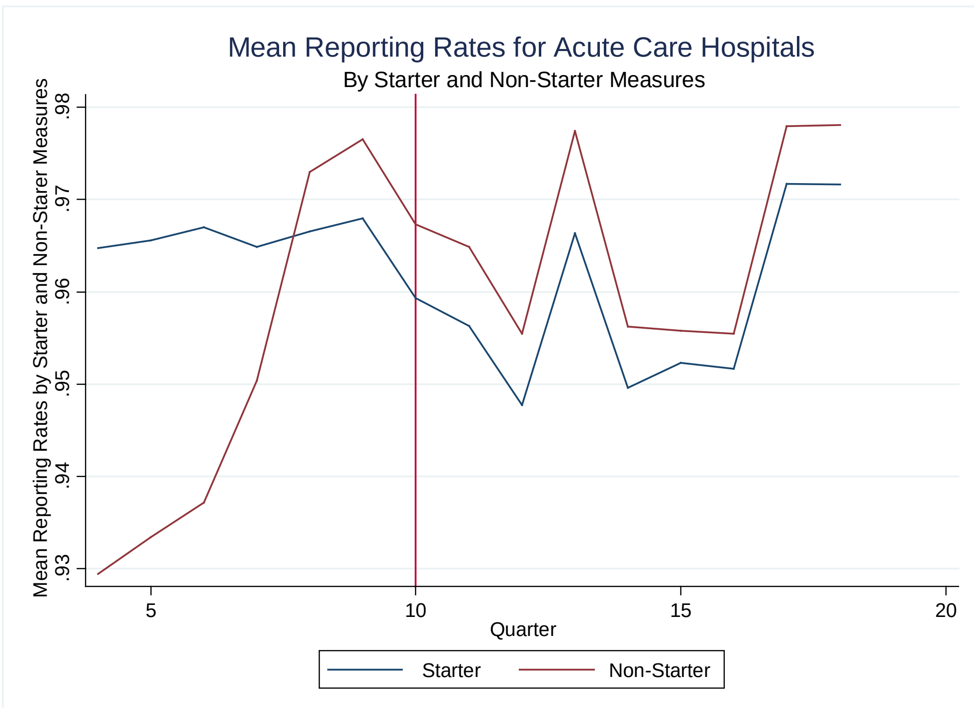

Table 2.4: Difference-in-Differences Estimates of the Reimbursement Penalty Change on Starter Measures

	Logistic Regressions		Linear Fixed Effect Regressions	
VARIABLES	(1)	(2)	(3)	(4)
	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Acute Care X Penalty Increase	0.0181	0.0169	0.00242	0.00479
	(0.0268)	(0.0355)	(0.0081)	(0.0081)
Penalty Increase	-0.0213	-0.0232	0.0206**	0.0215**
	(0.0071)	(0.0103)	(0.0099)	(0.0100)
Acute Care Hospital	0.2416***	0.2426***	-	-
	(0.0518)	(0.0471)		
Acute Care Hospital X Time	-	-	-5.26E-05	-0.000715
			(0.0010)	(0.0011)
Constant	-	-	0.860***	0.740***
			(0.0027)	(0.0369)
Observations	63,109	63,109	63,109	63,109
R-squared			0.822	0.822
Demographics	No	Yes	No	Yes
Provider Effects	No	No	Yes	Yes
Quarter Effects	Yes	Yes	Yes	Yes

Columns 1-2: Marginal effects reported, bootstrapped method standard errors in parentheses

Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.5: Difference-in-Differences Estimates of the Effect of Non-Starter Penalty Enactment Using Acute Care Starter Measures as Control Group

	Logistic Regressions		Linear Fixed Effect Regressions	
	(1)	(2)	(3)	(4)
VARIABLES	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Non-Starter Measure X Penalty Increase	0.0815***	0.0835***	0.0447***	0.0447***
	(0.0095)	(0.0114)	(0.0029)	(0.0029)
Non-Starter Measure	-0.0219***	-0.0219***	-0.125***	-0.125***
	(0.0035)	(0.0041)	(0.0035)	(0.0035)
Penalty Increase	0.0267***	0.0275***	0.0187***	0.0206***
	(0.0064)	(0.0071)	(0.0030)	(0.0036)
Non-Starter Measure X Time	-	-	0.00620***	0.00620***
			(0.0004)	(0.0004)
Constant	-	-	0.900***	0.815***
			(0.0026)	(0.0234)
Observations	102,144	102,144	102,144	102,144
R-squared			0.729	0.73
Demographics	No	Yes	No	Yes
Provider Effects	No	No	Yes	Yes
Quarter Effects	Yes	Yes	Yes	Yes

Columns 1-2: Bootstrapped method standard errors in parentheses

Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.6: Starter Measure Pre-Trend Robustness Checks

	Logistic Regressions		Linear Fixed Effect Regressions	
	(1)	(2)	(3)	(4)
VARIABLES	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Acute Care X Placebo Penalty Increase	-0.0425	-0.0444	0.0175	0.0159
	(0.0546)	(0.0543)	(0.0127)	(0.0128)
Placebo Penalty Increase	-	-	0.0813***	0.0809***
			(0.0122)	(0.0122)
Acute Care Hospital	-	-	-	-
Acute Care Hospital X Time	-	-	-0.0164***	-0.0164***
			(0.0041)	(0.0041)
Constant	-	-	0.924***	0.846***
			(0.0129)	(0.0367)
Observations	24,726	24,726	24,726	24,726
R-squared			0.894	0.894
Demographics	No	Yes	No	Yes
Provider Effects	No	No	Yes	Yes
Quarter Effects	Yes	Yes	Yes	Yes

Columns 1-2: Bootstrapped method standard errors in parentheses
Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.7: Non-Starter Pre-Trend Robustness Checks

	Logistic Regressions		Linear Fixed Effect Regressions	
	(1)	(2)	(3)	(4)
VARIABLES	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Non-Starter Measure X Placebo Penalty Increase	0.0916***	0.0909***	0.0207***	0.0207***
	(0.0126)	(0.0175)	(0.0077)	(0.0077)
Placebo Penalty Increase	-	-	-0.239***	-0.239***
			(0.0118)	(0.0118)
Non-Starter Measure	-	-	0.0129***	0.00939***
			(0.0033)	(0.0034)
Non-Starter Measure X Time	-	-	0.0222***	0.0222***
			(0.0022)	(0.0022)
Constant	-	-	0.927***	0.832***
			(0.0024)	(0.0520)
Observations	41,172	41,172	41,172	41,172
R-squared			0.69	0.69
Demographics	No	Yes	No	Yes
Provider Effects	No	No	Yes	Yes
Quarter Effects	Yes	Yes	Yes	Yes

Columns 1-2: Bootstrapped method standard errors in parentheses

Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Calculating the marginal effects of penalty increases, Acute Care Hospitals

	Logistic Regressions		Linear Fixed Effect Regressions	
	(1)	(2)	(3)	(4)
VARIABLES	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Reimbursement Penalty	0.1123***	0.1102***	0.133***	0.132***
	(0.0076)	(0.0074)	(0.0078)	(0.0086)
$(\text{Reimbursement Penalty})^2$	-	-	-0.0520***	-0.0495***
			(0.0034)	(0.0038)
Non-Starter Measure	-	-	-0.0798***	-0.0798***
			(0.0048)	(0.0051)
Non-Starter Measure X Time	-	-	0.00620***	0.00620***
			(0.0004)	(0.0004)
Constant	-	-	0.860***	0.749***
			-0.00532	-0.0154
Quadratic?	No	No	Yes	Yes
Demographics	No	Yes	No	Yes
Provider Fixed Effects	No	No	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
Observations	102,144	102,144	102,144	102,144
R-squared	0.0255	0.3756	0.018	0.73

Columns 1-2: Delta method standard errors in parentheses

Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 2.9: Calculating the marginal effects of penalty increases, Acute Care and Critical Access Hospitals

	Logistic Regressions		Linear Fixed Effect Regressions	
	(1)	(2)	(3)	(4)
VARIABLES	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting	Probability of Full Reporting
Reimbursement Penalty	0.2416***	0.1344***	0.167***	0.165***
	(0.0107)	(0.0113)	(0.0092)	(0.0088)
(Reimbursement Penalty)^2	-	-	-0.0664***	-0.0637***
			(0.0040)	(0.0038)
Acute Care Hospital	-	-	0.260***	-
			(0.0158)	
Non-Starter Measure	-	-	-0.0584***	-0.0592***
			(0.0052)	(0.0051)
Non-Starter Measure X Time	-	-	0.00467***	0.00473***
			(0.0004)	(0.0004)
Constant	-	-	0.592***	1.046***
			(0.0151)	(0.1260)
Quadratic	No	No	Yes	Yes
Demographics	No	Yes	No	Yes
Provider Effects	No	No	Yes	Yes
Time Effects	Yes	Yes	Yes	Yes
Observations	106,284	106,284	106,284	106,284
R-squared			0.056	0.736

Columns 1-2: Delta method standard errors in parentheses

Columns 3-4: Clustered Standard Errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Appendix 2.1

I develop a simple model to determine how a change in reimbursement rates will effect a hospital's reporting decision. I use a hospital profit function to express an individual hospital's profit at time *t*:

1) $$\Pi_t(R) = Revenue_t\,(S_t, N_t, R_{t-1}, Q_{t-1}) - Cost_t(S_t, N_t, X_t)$$

Revenue is affected directly by the CMS penalty for non-reporting and the quality score that it reports, if the hospital chooses to report at all. The financial penalty is expressed as a percentage of the standard Medicare reimbursement and only affects the hospital's Medicare reimbursements.

The severity of patient illness during time t, S_t, affects the amount of reimbursement from insurance companies but also increases hospital cost of care. N_t is the hospital's total number of admissions in time t. Total hospital costs are a function of the number and illness severity of patients, and include the operating costs of the hospital (wages and capital). R_t is a dummy for public score reporting. I assume that in small spans of time, quality changes are zero: $\frac{\delta Q}{\delta t} = 0$. With this assumption, I can fix the hospital quality Q_t to Q^*, at least locally. From this, we can say that $Q_{t-1} = Q_t = Q^*$.

Further, hospital patient case mix index and aggregate number of patients are affected by the hospital's quality and whether the hospital reports this quality. Both the decision to report and the actual score reported sends a signal to patients – patients may interpret a missing score as a signal of poor hospital quality, and may seek admission to a different hospital for procedures.

2) $$S_t = S_t(Q_{t-1}, R_{t-1})$$

3) $$N_t = N_t(Q_{t-1}, R_{t-1})$$

From above, one can make the assumption that the changes in patient severity and number in a short period of time rest on the hospital's fixed quality and whether the hospital chooses to report its scores.

2a) $$S_t = S_t(Q_{t-1}, R_{t-1}) = S_t(Q_t, R_{t-1}) = S_t(Q^*, R_{t-1})$$

3a) $$N_t = N(Q_{t-1}, R_{t-1}) = N_t(Q_t, R_{t-1}) = N_t(Q^*, R_{t-1})$$

Under the assumption that hospitals are profit-maximizers (and thus rational), hospitals choose to publicly report their scores at time t if:

4) $$E(\Pi_{t+1} | R_t = 1) \geq E(\Pi_{t+1} | R_t = 0).$$

I remove expectations in the model and make the explicit assumption that $E_t[\pi_{t+1}(R_t)] = \pi_{t+1}(R_t)$, and embed Q^* into the profit function, leaving only the dummy report variable, R. To save space, I change the notation so that $Revenue_{t+1}\big(S_{t+1}(R_t = 1)\big) = Revenue_{t+1}\big(S(1)\big)$. Using this shortened notation, the hospital decision to report becomes:

5)

$$\Pi_{t+1}(1) = Revenue_{t+1}(S(1), N(1), 1) - Cost_{t+1}\big(S(1), N(1), X(1)\big) \geq$$
$$Revenue_{t+1}(S(0), N(0), 0) - Cost_{t+1}\big(CMI(0), N(0), X(0)\big) = \Pi_{t+1}(0)$$

Further simplifying, I obtain the following inequality:

5a)

$$Revenue_{t+1}(S_t(1), N_t(1), 1) - Revenue_{t+1}(S_t(0), N_t(0), 0) \geq$$
$$Cost_{t+1}\big(S_t(1), N_t(1), X_t(1)\big) - Cost_{t+1}\big(S_t(0), N_t(0), X_t(0)\big)$$

The above inequality shows the familiar condition that the hospital will publicly reveal its score if the increased revenue from doing so offsets its cost of care. The change cost of care is a function of the changes in patient illness severity and the number of patients from the reporting decision.

In this model, reporting information publicly can affect hospital profit in three ways: through a change in illness severity, through a change in hospital volume of patients, and through the Medicare reimbursement penalty.

1) Through a change in illness severity. Empirically, patients and referring physicians tend to select different health care providers when hospital quality is revealed. Recent literature has explored the implications of hospital choice when quality is known: Jung et al. (2011) use a conditional logit model to explore the hospital choices of non-hospitalized and recently hospitalized employees of a firm with the same health plan. Hospital quality in this framework plays a small role in where patients choose to receive health care. In contrast, Dranove et al. (2003) examine the effect of the mandatory hospital report cards for coronary artery bypass graft surgery (CABG) implemented in the early 1990s in New York State and Pennsylvania on the number of admissions and average patient severity in each hospital. Examining both provider-level and patient-level data, they find that patients of greater illness severity and in the most need of CABG surgery select into hospitals of greater quality (as measured by the CABG scores and teaching status), while healthier patients receive more surgery, presumably to boost the hospital's score. While Dranove et al. (2003) do not conclude that report cards are harmful, in *general*, but rather the authors encourage reporting measures that minimize provider incentive for selection. Perhaps not coincidentally, the CMS passed the Hospital Compare program in 2003, and was set into action in 2005.

2) Through a change in the number of patients. Previous empirical research dating from the 1990s suggests that not only does average severity of patient illness change after quality

is revealed by a hospital but also that sicker patients are reluctant to patronize hospitals that may have a lower quality. Mukamel and Mushlin (1998) analyze the change in market share due to the public release in New York State of the CABG mortality rate report cards. Using OLS, they find that both surgeons and hospitals with an increase in mortality rates experience a subsequent decrease in market share (percentage of surgeries in the state). Cutler et. al. (2004), using the provider as the level of observation, find that not only do sicker patients tend to select into different hospitals based on the whether a hospital is flagged as “high mortality” but that the number of overall admissions is driven by the selection of the severely ill into different hospitals. More recently, Pope (2011) measures the impact of the US News and World Reports hospital rankings on the number of non-emergency admissions and specialty admissions of hospitals ranked in the magazine. He finds that a change in ranking, while controlling for the quality score on a scale of 1-100 is associated with a 1% increase in hospital revenue and estimates that more than 750 million dollars have changed hands due to the USNWR rankings.

3) The most obvious way that a hospital is affected by public reporting is the immediate drop in Medicare reimbursement for the year if the hospital chooses not to report. As previously discussed, the CMS makes public a prospective payment schedule for the reimbursement of each illness treated in the beginning of each fiscal year: if the hospital chooses not to report its score, this reimbursement for patient treatment is dropped by a small percent. If a hospital chooses not to report its score in 2005, Medicare patient revenues are deducted by 0.4 percent (the annual inflation adjustment). Put another way: if a hospital treated a patient in 2005 and Medicare normally reimbursed a dollar for that

treatment, the hospital would receive 99.6 percent of the standard reimbursement rate if it had chosen not to report.

Under the assumptions of the model, an increase in the CMS penalty does not directly affect the flow of patients. It increases instead the price that the hospital must pay to the CMS to keep the hospital's quality information private, while expected patient changes as a result of reporting remain the same. It is likely that hospitals on the margin of reporting before the CMS penalty increase goes into effect are provided enough financial incentive to publicly report their scores.

A plausible reason for a hospital to consider that its patient flow may be altered as a result of score reporting is the local presence of alternative health care providers. Empirical research suggests that patients choose their health care provider not only based on the observed and unobserved characteristics of care, but also on the distance or time spent traveling to receive the care. A change in the number of providers within a certain distance or traveling time from each hospital would affect a hospital's expected patient revenue, all else equal.

Chapter 3: The Effect of Mandated Health Insurance on Physician Reimbursement: Evidence from the Massachusetts Health Reform

Introduction

Massachusetts passed a health care reform in 2006 which was designed to expand insurance coverage to those without employer-provided health insurance and to offer catastrophic health insurance coverage for the entirety of its citizenry. The state required that insurance companies adopt a community-rating pricing schedule to guarantee that individuals were not priced out of affordable care, and a mandate required individuals to purchase health insurance to curb adverse selection and in doing so keep down costs. The state of Massachusetts also subsidized, to varying extents, the purchase of health insurance for individuals with incomes lower than 300 percent of the poverty line. This study analyzes the effects of the reform on the cost of health care by examining changes in physician reimbursement for three types of health care services for which health insurance coverage was mandated.

Estimates of the expansion of health coverage as a result of the reform range from approximately a 5 percentage point to almost a 10 percentage point increase in the number of covered individuals (Kolstad and Kowalski, 2010; Health Connector, 2012). Massachusetts initially underestimated the number of uninsured that would be affected by the program: 600,000 consumers enrolled in health insurance when the mandate took effect instead of the projected 400,000. It is likely that the surge of newly insured individuals into the health insurance market had an effect on fees for health services, as well as quality and access to care.[21]

[21] Masi and Long (2009) document an increase in access to care in Massachusetts during the years 2006 through 2008 using Current Population Survey data. They find an increase of approximately 7 percentage points, from 86.4 percent, in individuals reporting that they have a usual source of care, and about a 4 percentage point decrease, from 25.4 percent, in the number of individuals who reported that they did not receive care in the past year. However, it was also much more likely that physician’s offices were not accepting new patients, a finding that matches reports from the Massachusetts Medical Society (2009).

In Massachusetts, the price of health care services may have changed as a result of the influx of newly insured. Consumers become less sensitive to the price of a service when they are not responsible for its full payment, as in the case of health insurance (Manning et. al 1987). This price insensitivity can lead to overconsumption, increasing the equilibrium price of common procedures. It is also possible that the newly insured in the market are less likely to get sick and have little to no history of health complications, implying that the most ill patients selected into health insurance pre-reform. In this case of pre-reform adverse selection, the influx of healthier patients could lower the average cost of care and thus lower the equilibrium price of common procedures. These countervailing forces leave us without a clear prediction of the direction of the equilibrium price of care as a result of the health insurance mandate.

We use a large data set of health insurance claims to private insurers to estimate the impact of the Massachusetts Health Insurance Reform on physician reimbursement for well-infant, well-adult, and appendectomy visits.[22] Estimates are obtained using a difference-in-differences strategy which compares Massachusetts to similar nearby states. We focus on well-infant, well-adult, and appendectomy visits for their representativeness of newly covered services under the mandate which have different price elasticities of demand.

Well-infant visits are the most price-elastic of the services: parents may balk at the cost of a medical checkup for infants who seem healthy.[23] Empirically, uninsured children receive

[22] Data Source: FAIR Health, Inc., an independent, New York nonprofit corporation.

[23] A simple internet search for "Are infant well-care visits necessary?" leads to numerous parent forums debating the necessity of post-infant immunization well-infant visits, anecdotally indicating a high price elasticity of demand for infant well-care visits. The American Academy of Pediatrics recommends 6 well-infant visits during the first year of life: the average insurance-negotiated price of one of these visits is approximately 100 dollars. Most children receive some of the recommended visits, but on average, do not receive all six exams.

fewer than half of the recommended number of well-care visits in their first year,[24] and economics research indicates that health insurance expansions to low income individuals increases the utilization of child preventative care (Currie and Gruber, 1996; De La Mata, 2012). Appendectomies are an emergency procedure with a low price elasticity of demand: failure to get an appendectomy when it is needed can result in severe infection and possibly death if the appendix bursts. Well adult visits are somewhere in between, neither as inelastic as appendectomies nor as elastic as well-child visits.

A more intuitive way to think of the impact of the Massachusetts Health Insurance Reform on prices is to think of the newly insured as shifting the demand curve for health services to the right (outward), thus driving the equilibrium price upward. Different procedures will experience different degrees of increased demand due to an increase in the newly insured. The amount of increased demand is a result of the post-insurance consumer price (both financial and physical) of each procedure.

Of the three services that we choose, well-child visits will experience the greatest increase in demand because the net benefit of receiving the procedure is the highest, as well-child visits become low-cost both financially and physically after insurance. Well-adult physicals will experience a lesser increase in demand, primarily because the most ill patients already receive adult check-ups. The less-ill, newly-insured adult patients will drive an increase in demand for well-adult visits. Appendectomies are one-time emergency procedures necessitated by the emergence of acute appendicitis. Because these procedures are conducted in the case of a medical emergency, the demand for appendectomies will not be affected by patient changes in

[24] Estimates from the National Survey of Early Childhood Health show that of the American Academy of Pediatrics recommended 6 well-care visits in the first year, privately insured children receive over 4 visits in their first year of life, while uninsured children receive fewer than 3. The National Survey of Early Childhood Health data is made available by the Centers for Disease Control (www.cdc.gov).

insurance status. Estimates show that infant well-care reimbursement rises 4 percent while the reform is being implemented, but the increases are not persistent. Reimbursements for adult well-care and appendectomy are unaffected by the mandate. The estimates can be further refined by adding in a third difference between well visits (which likely had a large post-reform patient influx) and appendectomies (which likely did not). The impact of the mandate on reimbursements for well-infant visits during the implementation period is smaller in magnitude.

Passed in 2010, the Patient Protection and Affordable Care Act (PPACA) is primarily based on the Massachusetts Health Insurance reform model, including an individual insurance mandate. The PPACA is designed to simultaneously expand health insurance coverage while controlling government spending on health services. To our knowledge, this is the first study to analyze the price effects of a health insurance mandate, which may drastically impact current and future public policy cost-benefit analyses.

The Massachusetts Health Insurance Reform

In 2006, the state of Massachusetts passed "An Act Providing Access to Affordable, Quality, Accountable Health Care," which mandated individual health insurance coverage by mid-2007. The reform was designed to create a larger pool of insured though an insurance mandate to reduce adverse selection. The law requires individuals to purchase health insurance or alternatively to pay a fine for lack of coverage. Most employer-provided health insurance coverage was unaffected by the reform. By eliminating selection into health insurance, the intention was to reduce total state spending on health care as well as to ensure near-universal health insurance coverage (Gruber, 2008).

Under the law, individuals are required to purchase insurance that qualifies for minimum creditable coverage or face a fine for non-compliance. Minimum creditable coverage includes basic preventative, diagnostic, and emergency care. Appendix 1 details the specific coverage areas required to qualify as insured in Massachusetts after 2006. Partial or full subsidies for low-income adults and children were provided in January 2007 to ensure that the most financially needy individuals would be in compliance.[25]

Since 2007, over 400,000 people have entered the Massachusetts insurance pool. Over 98% of the state population is now insured (Courtemanche and Zapata, 2012). This mass enrollment in health insurance applied a shock to the health care market: a sizeable portion of the patient population gained insurance coverage and became far more price inelastic with the coverage.[26] In addition, recent work by Kolstad, Hackmann, and Kowalski (2012) indicates that the majority of the newly insured required less care and were likely healthier than those who purchased insurance before the mandate, as indicated by lower average hospital costs per capita after the mandate. The average patient seeking care in Massachusetts became healthier as new individuals entered the market, which drove the average cost of health services downward. The mandate's impact on the price elasticity of demand for health care (which exerted upward pressure on prices) was counteracted by the downward pressure on price for health services from the expansion of the insurance pool.

Theoretical Considerations

A simple model follows to illustrate how the increase in the insured population would influence the equilibrium price of health care. Consider a health care economy with two types of potential patients: healthy patients who have a low propensity to need medical services and sick

[25] Families who qualified for MassHealth, the state's Medicaid program continued to receive creditable coverage; individuals who earned up to 300% of the poverty line but who did not qualify for MassHealth received heavily state-subsidized health insurance. The mandate also required individuals to purchase insurance if it is "affordable;" those who could not afford health insurance were not penalized by the fine that ranges from $0 to approximately $1200 per year.

[26] The newly insured tend to buy health insurance with the lowest monthly premium – specifically plans featuring the highest possible deductible and copay rates allowed by law (Marzilli Ericson and Starc, 2012).

patients who have a high propensity to need medical services. Each patient has a demand for health care:

1) $q_i = q_i(P, x, h), i \in (healthy, sick)$,

where q_i is the quantity of health care demanded by patient type *i* at a price level *P* given patient demographics *x*, and the type of insurance, h, that the patient has (if any).

$N_{healthy}$ is the number of healthy patients in the market and N_{sick} is the number of sick patients. The market demand, Q_i, for a particular patient type is the summation of all of the demands of individuals of that patient type and can be expressed as:

2) $Q_i = \sum_{N_i} q_i$.

Total market demand can be expressed as:

3) $Q = Q_{healthy} + Q_{sick}$.

We assume that sick patients are more likely to purchase health insurance than healthy patients, and when purchasing health insurance, enroll in more generous plans when the price of health insurance is the same for both types of patients. We also assume that health insurance makes demand less price elastic; this assumption is in line with the findings of the Rand Health Insurance Study (Manning et al., 1988). The market demand functions for healthy patients ($Q_{healthy}$), sick patients (Q_{sick}), and the entire market (Q) are shown in Figures 1, 2, and 3, respectively. Q_{sick} is more inelastic than $Q_{healthy}$ because a larger proportion of sick patients are insured, pre-reform.

The marginal cost, MC, of producing a unit of health care is

4) $$MC = MC(Q_{healthy}, Q_{sick}, g)$$

where g is the available technology for producing health care. We assume that on the margin, production of a unit of health care is more expensive if the unit is produced for a sick patient than if the health care is produced for a healthy patient. However, health care providers cannot price discriminate and charge sick patients a higher price than healthy patients.

We now consider a health insurance mandate where all patients are now required to hold a health insurance policy of at least a minimum level of coverage. We assume that all the previously uninsured patients purchase a plan with the minimum acceptable level of coverage; this assumption follows the findings of Ericson and Stark (2012). We also assume that all patients that held insurance before the insurance mandate held an insurance policy that is acceptable under the mandate and choose not to change plans.

The mandate will have two major effects on the market. The first is that aggregate health care demand will become more inelastic as more patients purchase insurance. The decrease in the magnitude of the elasticity (increase in steepness of the demand curve), will be of greater magnitude for healthy individuals because less of them held insurance before the mandate was enacted. The change in the elasticity of demand will happen very quickly, as soon as individuals have insurance the demand curve will change. The second effect is that healthy patients (who had the larger increase in insurance enrollment) will now demand more health care relative to sick patients than they did before the reform. The increase in healthier patient demand will lower the marginal cost function for health services as it is less expensive on the margin to treat a healthy patient. The effect of healthier patients in the patient pool on expected costs will be slower, as physicians will need to experience treating healthier patients before the expect to see more of them in the future. Both of these effects are shown in Figure 1.[27]

Data

Price Data

Our analysis uses physician reimbursement data from the Medical/Surgical Module of the FAIR Health Database between 2005 and 2009. FAIR Health collects claims data from private health insurance companies and uses them to produce tables for calculating out of network reimbursement. The complete Medical/Surgical module accounts for roughly 28 percent of the total number of private insurer claims in the United States in a given year.[28] Within a claim, individual procedure types can be identified by line item via the American Medical Association's Current Procedure Terminology codes. Each claim's date is known and is designated with the three digit zip code in which the service was provided.

For each line item we are able to observe how much the provider charges to the insurance company and how much the insurance company reimburses the provider. The amount charged to the insurance company is an amount that is influenced by a number of factors that change when the mandate comes into play. For example, it is the fee that would be charged to uninsured individuals – who become a rarity after the reform. This introduces variation other than the effect of the reform through the channels discussed above into the amount providers charge insurance companies, and as such we focus on the amount that the insurance company

[27] The above analysis ignores the presence of deductibles or copays in the insurance policies, which are present in the Massachusetts health care reform. This omission is for simplicity's sake, deductibles and copays can be included with the same result as long as the size of deductibles and copays are small compared to the overall cost of the procedures used.

[28] Kleiner, et. al (2012) discuss the representativeness of the data to the entirety of insurance claims in the United States in 2008. They find that the distribution of prices for well-baby (including well-infant) services in the Fair Health data and a nationally representative dataset (MarketScan) are extremely similar, leading them to infer that the price analysis would be representative of all well-infant health insurance claims in the United States.

reimburses the provider, henceforth the allowed amount. The allowed amount is the final price paid for all medical services observed in the data.

We focus on three groups of line items as identified by their CPT codes. The first group is comprised of appendectomies, and includes CPT codes for appendectomies, laparoscopic appendectomies and appendectomies performed on an already burst appendix. The demand for appendectomies is extremely price inelastic, as failure to undergo surgery when an appendectomy is recommended by a physician can lead to death. We do not expect the demand for appendectomies to change as a result of the health reform.

The second group of line items examined is for well-infant visits. This group includes CPT codes for visits for patients under one year old (one code for new patients and one code for returning patients). Well-infant visits are likely an elastic set of procedures pre-reform, as uninsured children receive fewer than half of the recommended six well-infant visits in their first year of life. Of the procedure groups that we examine, demand for well-infant visits is the most price elastic pre-reform. An increase in the number of insured should greatly decrease the magnitude of the price elasticity of demand for well-infant visits.

The final group of line items is for well-adult visits and includes codes for 18 to 44 year old visits (one code for new patients and one code for returning patients) and for 44 to 65 year old visits (one code for new patients and one code for returning patients). Price elasticity of demand for well-adult visits is likely somewhere between that of appendectomies, which are very inelastic, and well-infant visits, which are relatively elastic. Table 1 presents summary statistics for the allowed amounts used in the analysis. Mean allowed prices for well-adult visits are approximately 110 dollars, and mean well-infant visits are approximately 85 dollars. Mean appendectomy allowed prices are much higher at 1000 dollars. An appendectomy is an inpatient visit: the charge for the individual appendectomy CPT code is a large fraction of the total price of the procedure (we do not observe anesthesia and other hospital allowed amounts), thus the magnitude of the mean allowed amount of appendectomies can be misleadingly small.[29]

Demographic Information

Demographic information was collected from a commercial database purchased from Zip-codes.com and the publicly available American Community Survey. Zip-codes.com consolidates demographic, economic, and geographic information about each postal zip-code in the United States using raw data from existing sources including the United States Postal Service and the United States Census Bureau. Table 2 includes a list of the covariates in the analysis along with their means and standard deviations. Specifically, we control for measures of population density (population, housing units per zip code and persons per housing unit) and measures of the price level (median household income and average price of a home) to control for population differences that could influence the price of health care. Zip-codes.com data is aggregated to the state level to avoid dummy oversaturation in the regression equation.

The American Community Survey (ACS) provides individual level demographic data that we population weight and aggregate to the state-by-year level. The variables include age, marital status, number of children per household, the percent of the population that is Black, the percent of the population with Hispanic origin, employment status, family income, sex, and educational attainment.

[29] Appendix 2 includes a brief discussion of allowed price trends in Massachusetts.

Methods

A difference-in-differences estimation strategy is used to identify the effect of the Massachusetts health insurance reform on reimbursement. The outcome of interest is the allowed amount for a specific health service type: well-adult visits, well-infant visits, or appendectomies. Each service corresponds to a group of CPT codes. A particular CPT code indicates the specifics of the service that took place (for example, a separate CPT code is used if the appendectomy was performed after the appendix burst). To allow for differences in the conditions under which a procedure occurred, we estimate all models with CPT-specific fixed effects.

The allowed amounts for procedures performed in other northeastern states are used as a control group. These states include Maine, Vermont, New Hampshire, Connecticut, Rhode Island, New York, Pennsylvania, New Jersey, Maryland, Delaware, and Washington D.C. Washington D.C. is omitted from the appendectomy analysis due to the small number performed in the D.C. area.

Models of the following specification are estimated using ordinary least squares:

$$5)\quad Price_{pst} = \alpha_0 + \beta_1 * MA_s + \beta_2 * PostReform_t + \beta_3 * MA_s \times Post\ Reform_t + \beta_4 * Implementation_t + \beta_5 * MA_s \times Implementation_t + X'_{st}\beta + \omega_s + \lambda_t + \psi_p + \epsilon_{pst}.$$

The subscript *p* denotes CPT codes, *s* denotes states and *t* denotes years. $Price_{pst}$ is the allowed amount for the procedure grouping in question. MA_s is a dummy variable which takes on the value of 1 if the procedure was performed in Massachusetts and takes on a value of 0 if the procedure was performed in a different northeastern state. $Post\ Reform_t$ is a dummy variable that takes on the value 1 for the years after the reform (2008 and 2009) and takes on the value 0 for years prior to the reform (2005 and 2006), A separate dummy for 2007, $Implementation_t$, is used to capture the effect of the reform immediately after implementation, before all actors in the market have time to adjust. This dummy is also interacted with the post period. The differences-in-differences estimators, $\beta_3\ and\ \beta_5$, are the coefficients on the interaction between the two post period indicators and the dummy for Massachusetts. β_5 provides the estimated effect of the health care reform on the allowed price of the procedures before the market has time to totally adjust and β_3 provides the estimated effect of the health care reform on the allowed price of the procedures after the market has time to totally adjust. $\mathrm{X}'_{\mathrm{st}}$ is a matrix of control variables from the Zipcode.com data. Additionally, ω_s is a set of state fixed effects, λ_t is a set of year fixed effects, $\psi_p$is a set of CPT code fixed effects, and $\epsilon_{pst}$is a set of robust standard errors clustered at the state-CPT-year level. Additional specifications include state by CPT, year by CPT fixed effects and state specific time trends.

The difference-in-differences approach compares changes in the price of procedures in Massachusetts to changes in the price of procedures in similar states that did not pass a reform. The first difference across time removes any time-invariant state level characteristics that could influence prices. The second difference across states removes an approximation of the baseline effect that would have occurred had Massachusetts not passed the reform.[30]

Results

Appendectomy

Estimates show that the Massachusetts Health Insurance reform did not differentially affect the prices of appendectomies in post-reform Massachusetts. There was no effect in the implementation period as well as no effect once the implementation period was over. Table 3A reports regression results: the first row reports the difference-in-differences estimate for the post-

[30] This approximation is valid to the extent that the control states are similar to Massachusetts.

implementation period. The fourth row reports the estimate for during the implementation period. Each column shows a regression with a different set of controls and fixed effects which are noted in the bottom portion of the table. All coefficient estimates other than the difference-in-differences estimate are suppressed.[31] The largest effect found on reimbursements for appendectomy price is approximately 3 percent, however, none of the estimates are statistically significant at conventional levels, and in many cases the estimate has a lower magnitude than its standard error, which makes a strong case for the average effect of the estimates being effectively zero given the number of observations. These results are in line with the demand for appendectomies not changing substantially as a result of the reform.

Well-Infant Visits

The results for well-infant visit reimbursements are reported in Table 3B. The layout of Table 3B is identical to that of Table 3A, the only difference being the dependent variable in the regressions. Reimbursement for well-infant visits did exhibit a statistically significant increase during the implementation period; all of the estimates were very close to a 4 percent increase in reimbursement. This result is robust to the inclusion of several different groups of fixed effects, and hardly changes at all once demographic controls are added. The increase in reimbursement is consistent with an increase in the number of parents taking their children for well-infant visits, during the implementation period. Once the implementation period is over, we find a very small negative effect (8 tenths of a percent) of the reform on reimbursement once all controls and fixed effects are included.

Well-Adult Visits

The regression results for well-adult visits are reported in Table 3C, which shares the same layout as Tables 3A and B. We find no effect of the reform on reimbursement at

[31] Full regression results which include covariates as control variables are available upon request.

conventional levels during the implementation period, and also no effect of the reform at conventional levels after the implementation period once all controls and fixed effects are included in the estimation.

Threats to Identification

The above estimates may be biased by omitted variables that are correlated with reimbursement for health services and that occur at the same time as the Massachusetts health reform went into effect. Many of the likely candidates for such variables are controlled for directly. For example, we include state populations, education levels and incomes, which do not change dramatically from 2003 through 2007, but show enough variation to warrant inclusion in the regressions (Table 2). However, the Massachusetts reform had effects other than increasing the number of insured which makes identifying the effect on reimbursement of changes in demand elasticity difficult to identify.

The pool of providers may have increased as a result of the reform. In 2008, the state and private insurers established a loan forgiveness program for medical professionals who committed to working in underserved areas for a minimum of two years (MassResources.org). The physician population is an example of an omitted variable that would have likely changed around the same time as the reform and that would be correlated with the price of care.

There are other such variables. Despite efforts to draw more health care professionals to Massachusetts, the Massachusetts Medical Society (2011) reported that patients were facing longer wait times and that more than half of primary care physicians were not taking new patients as an unintended consequence of the reform. The Physicians Workforce Study in Massachusetts (MMS, 2011) found that physicians who practiced family and internal medicine were in severely short supply for a sixth consecutive year in 2011. Clearly, wait times would have changed with the reform and this could have also influenced willingness to pay and thus the price of services. The same could be true for quality of care.[32]

To illustrate this point we conduct a simple analysis of the impact of the reform on the supply of physicians. We estimate an equation similar to those used for the reimbursement analysis on the state supply of physicians and pediatricians:

$$6)\quad \ln(Number\ of\ Physcians_{st}) = \alpha_0 + \beta_1 * MA + \Sigma_{t=2003}^{2008}\beta_2 * MA\ \times \lambda_t + \mathrm{X}'_{\mathrm{st}}\beta + \omega_s + \lambda_t + \epsilon_{st}.$$

The left hand side variable $\ln(Number\ of\ Physcians_{st})$ is the natural log of the number of physicians in a given state in a given year. We estimate equation 6 for all physicians and for just pediatricians. The above estimating equation does not specifically capture the effect of the Massachusetts health reform. We instead report Massachusetts by year interaction effects, which show percent changes in the number of doctors choosing to practice in Massachusetts over time.

Table 4 reports the Massachusetts state by year interaction effects, where the year 2005 is the omitted year. The state and year fixed effects soak up much of the variation in the supply of physicians: it is possible that the reported effects are due to near multi-collinearity of the regressors or small sample sizes. Nevertheless, these results are suggestive that the changing supply of physicians played a role in the previously reported effects on reimbursement of the Massachusetts reform. An increase in family practice physicians and pediatricians after 2006 may have counteracted upward pressure on reimbursement that the newly insured exerted. The increase in physician supply changes the interpretation of the effect of health reform on reimbursements. The reform affected both aggregate demand for health services and aggregate

[32] Miller (2012) finds that the ill are more likely to receive care from a physician's office rather than from an emergency room after the reform – clearly an increase in quality for those seeking care.

supply of health service providers. The previous estimates capture the shifts in both supply and demand instead of the previously thought shift in demand for health services.

Refinement – Triple Differences

The results from the difference in differences estimation that we attribute to an increase in the demand for health services may be biased by unobserved factors. Bias in the estimates may occur because the demand for *insurance coverage* of health services increases instead of an increase in the demand for health services.

We further refine our estimates by using a differences-in-differences-in-differences or triple difference estimation strategy. The triple difference takes the difference between the difference-in-differences estimate for well-infant or well-adult exams and the difference-in-differences estimate for appendectomies. Because demand for appendectomies is extremely inelastic (Manning et al., 1987) and we do not expect it to change as a result of the health reform, the third difference removes any effects of the reform that may influence reimbursement that are not tied to changes in demand for services. As long as the effect of new physicians, quality of care, and demand for insurance coverage is the same for well visits as it is for appendectomies, the triple differences strategy will accurately report the effect of changes in demand on price.

The triple difference estimates for well-infant visits are presented in Table 5A. Table 5A follows the same format as tables 3A, B and C. The reported estimates are now from the triple differences estimating equation. Once again, the estimated show an increase in reimbursement during the implementation period and no effect of the reform once the implementation period is over. The estimate of the effect of the implementation period on reimbursement of approximately 2 percent is smaller in magnitude than the difference in differences estimate of approximately 4 percent.

The triple differences estimates for well-adult visits are presented in Table 5B. Table 5B has the same format as Table 5A. Once all controls and fixed effects are included in the estimating equation, we find no effect of the reform in either the implementation period or post period on reimbursement at conventional levels.

Conclusion

The Massachusetts health care reform greatly increased the number of people in Massachusetts covered by health insurance. The above analysis shows that the increase in insurance coverage was accompanied by sizable increases in reimbursement for procedures that gained a larger patient base with the introduction of the insurance mandate. These increases in reimbursement were temporary: the increases were eventually offset by decreases in the cost of provision as healthier individuals entered the market.

Our results have broader implications for the remainder of the United States, as additional portions of the Affordable Care Act come into effect. A nationwide individual mandate would likely have similar effects to the mandate seen in Massachusetts. Specifically, the price of many procedures with relatively elastic demand before the Affordable Care Act should be expected to rise during the period while the individual mandate provisions are coming into effect.

We can get a rough estimate of how large such an impact may be using the Medical Expenditure Panel Survey (MEPS) data. In 2009, the average amount paid by public insurance to physicians per person for an office visit in the United States was 69 dollars. We can approximately calculate the additional amount of money paid to physicians if the average change in price of an office procedure is 2 percent, all else equal.

If we consider the non-Massachusetts, non-Medicare or Medicaid U.S. infant population, roughly 19 million individuals, and assume an average of 3 well care visits per infant during the implementation year, a similar health insurance mandate would create around 200 million dollars in additional office visit costs through increased reimbursement. This does not consider other elastic procedures that may also see increases in prices. Even as a rough estimate, the magnitude of this one year price increase underscores the importance of having a firm grasp of the price effects when considering health insurance mandates such as the Affordable Care Act.

As a final note, it is also important to remember that the full effects of the Massachusetts reforms are still being measured. Our study, along with others in the literature provides estimates that are informative as to the short run effects of the reform. It is unclear how the reform will affect health care prices, access to care, and health service quality in the long run as slow-moving market factors such as the supply of new doctors and hospitals adjust to the program changes.

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Figure 3.1: Market Demand for Health Care

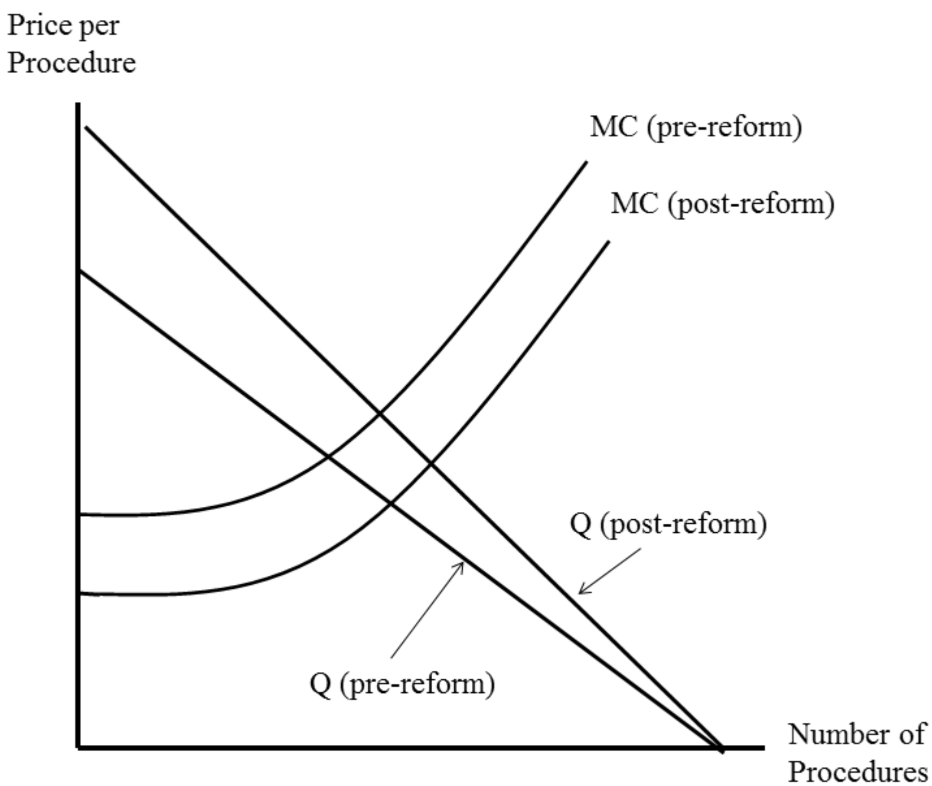


Table 3.1: CPT codes

Procedure	CPT	Description		Allowed Price Mean	Allowed Price Std. Dev.
Well-Adult Exam	99385	Age 18-44	New Patient	123.73	38.27
	99386	Age 45-64	New Patient	142.13	41.53
	99395	Age 18-44	Established Patient	102.13	30.23
	99396	Age 45-64	Established Patient	113.91	32.93
Well - Infant Exam	99381	Age <1 year	New Patient	98.00	31.70
	99391	Age <1 year	Established Patient	76.87	25.08
Appendectomy	44950	Laparoscopic		861.00	1,125.78
	44960	Open		1,096.47	1,577.23
	44970	Open with Perforated Appendicitis		1,104.73	2,157.35

Table 3.2: Demographic Summary Statistics

Variable	All States	Northeastern States	Massachusetts
White	0.80	0.82	0.84
	(0.13)	(0.11)	(0.01)
Black	0.10	0.10	0.06
	(0.11)	(0.08)	(0.00)
Female	0.51	0.52	0.52
	(0.01)	(0.01)	(0.00)
Percent Employed	0.63	0.64	0.64
	(0.04)	(0.03)	(0.01)
Percent Married	0.54	0.53	0.50
	(0.05)	(0.03)	(0.02)
Hispanic Origin	0.08	0.07	0.07
	(0.09)	(0.05)	(0.00)
Percent with High School Degree	0.40	0.39	0.35
	(0.04)	(0.03)	(0.01)
Percent with Some College	0.23	0.21	0.20
	(0.03)	(0.02)	(0.01)
Percent with College Degree	0.25	0.29	0.35
	(0.05)	(0.04)	(0.01)
Age	46.15	46.92	46.54
	(1.22)	(0.69)	(0.13)
ln(Population)	7.94	8.28	8.83
	(0.83)	(0.65)	(0.02)
ln(House Value)	11.38	11.74	12.11
	(0.42)	(0.28)	(0.17)
People per Household	2.2	2.5	2.5
	(0.79)	(0.51)	(0.60)
Number of Children per Household	0.66	0.65	0.65
	(0.08)	(0.05)	(0.03)
Houses per zipcode	3217	3846	4668
	(2302)	(1721)	(841)
Income	33247	44630	49568
	(12691)	(9616)	(8742)
N	255	55	5

Notes: State demographic data was calculated using the publicly available American Community Survey and the proprietary zip-code.com database. Numbers are weighted by state population.

Table 3.3A: Reform Effects on Appendectomy Prices

Appendectomies: ln(Allowed Price)	(1)	(2)	(3)	(4)
MA x Post Health Reform	0.034	0.036	0.035	0.036
Std. Error	0.045	0.08	0.045	0.043
t-statistic	0.75	0.21	0.78	0.84
MA x Implementation Period	-0.01	0.057	0.075	0.069
Std. Error	0.026	0.27	0.154	0.149
t-statistic	0.38	0.21	0.49	0.47
Demographics	No	Yes	Yes	Yes
Procedure FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
State Specific Tends	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Procedure x State FE	No	No	Yes	Yes
Procedure x Year FE	No	No	No	Yes
Observation	49592	48130	48130	48130
R-Squared	0.02	0.02	0.02	0.02

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively.

Table 3.3B: Reform Effects on Well-Infant Visit Prices

Well Infant Visits: ln(Allowed Price)	(1)	(2)	(3)	(4)
MA x Post Health Reform	-0.006	-0.008	-0.007	-0.008*
Std. Error	0.02	0.012	0.007	0.006
t-statistic	0.31	0.65	1.08	1.34
MA x Implementation Period	0.044***	0.040***	0.039***	0.039***
Std. Error	0.009	0.006	0.006	0.003
t-statistic	4.71	7.18	11.72	11.88
Demographics	No	Yes	Yes	Yes
Procedure FE	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Procedure x State FE	No	No	Yes	Yes
Procedure x Year FE	No	No	No	Yes
Observation	2,476,171	1,702,783	1,702,783	1,702,783
R-Squared	0.34	0.36	0.36	0.36

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively.

Table 3.3C: Reform Effects on Well-Adult Visit Prices

Well Adult Visits: ln(Allowed Price)	(1)	(2)	(3)	(4)
MA x Post Health Reform	-0.002	0.007	0.007	0.006
Std. Error	0.017	0.012	0.011	0.011
t-statistic	0.09	0.57	0.62	0.62
MA x Implementation Period	0.037***	-0.009	-0.009	-0.009
Std. Error	0.014	0.012	0.009	0.009
t-statistic	3.61	0.75	0.98	0.98
Demographics	No	Yes	Yes	Yes
Procedure FE	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Procedure x State FE	No	No	Yes	Yes
Procedure x Year FE	No	No	No	Yes
Observation	11,303,645	8,469,844	8,469,844	8,469,844
R-Squared	0.32	0.31	0.31	0.32

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively.

Table 3.4: Physician Supply in Massachusetts, Yearly and DD Estimates

	(1)	(2)	(3)	(4)
ln(number of physicians)	All Fields	All Fields	Pediatricians	Pediatricians
MA x 2006	-0.074		0.182	
	(0.272)		*(0.225)*	
MA x 2007	0.032		0.388*	
	(0.273)		*(0.233)*	
MA x 2008	0.021		0.497**	
	(0.270)		*(0.251)*	
MA x 2009	0.052		0.437**	
	(0.273)		*(0.211)*	
MA x Post Health Reform		0.0726		0.349**
		(0.177)		*(0.142)*
Demographics	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observation	11,872	11,872	474	474
R-Squared	0.30	0.30	0.55	0.55

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively. Robust standard errors in parentheses.

Table 3.5A: Reform Effects on Well-Infant Visit Prices (Triple Difference)

	(1)	(2)	(3)	(4)
MA x Post Health Reform x Well-Infant	-0.003*	0.01	0.005	0.005
Std. Error	0.019	0.016	0.007	0.007
t-statistic	0.14	0.63	0.78	0.78
MA x Implementation Period	0.015***	0.016***	0.023***	0.023***
Std. Error	0.037	0.032	0.013	0.013
t-statistic	0.42	0.49	1.72	1.71
Demographics	No	Yes	Yes	Yes
Procedure FE	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Procedure x State FE	No	No	Yes	Yes
Procedure x Year FE	No	No	No	Yes
Observation	2,524,301	1,750,913	1,750,913	1,750,913
R-Squared	0.60	0.65	0.65	0.65

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively.

We include Post-Reform, Well-Infant, and MA interaction terms in the regressions.

Table 3.5B: Reform Effects on Well-Adult Visit Prices (Triple Difference)

	(1)	(2)	(3)	(4)
MA x Post Health Reform x Well-Adult	0.005	0.037	0.035	0.035
Std. Error	0.058	0.076	0.076	0.076
t-statistic	0.08	0.49	0.46	0.46
MA x Implementation Period	0.052	0.054**	0.044	0.044
Std. Error	0.042	0.041	0.038	0.038
t-statistic	1.25	1.34	1.14	1.14
Demographics	No	Yes	Yes	Yes
Procedure FE	Yes	Yes	Yes	Yes
State Specific Trends	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Procedure x State FE	No	No	Yes	Yes
Procedure x Year FE	No	No	No	Yes
Observation	11,280,363	8,466,662	8,466,662	8,466,662
R-Squared	0.38	0.40	0.40	0.40

Standard errors are clustered by state-year-procedure. Demographics come from Zipcode.com and the ACS. ***, **, and * denote statistical significance and the 1, 5 and 10 percent levels respectively.

Appendix 3.1: Minimum Credible Coverage Requirements

In order to qualify as a credible insurance plan, insurance in Massachusetts must cover at least the following:[33]

- Ambulatory patient services, including outpatient day surgery and related anesthesia
- Diagnostic imaging procedures, including x-rays
- Emergency services
- Hospitalization, including at a minimum, inpatient acute care services which are generally provided by an acute care hospital for covered benefits in accordance with the member's subscriber certificate plan description
- Maternity and newborn care
- Medical/surgical care, including preventative and primary care
- Mental health and substance abuse services
- Prescription drugs
- Radiation therapy and chemotherapy

Appendix 3.2: Prices in Massachusetts and Northeastern States

The assumption that prices for health services always change at a non-negative rate allows us to place an upper bound on our estimates of the effect of the Massachusetts Health Insurance Reform. This assumption takes the mathematical form and allows us to set the second term to zero:

$$Reform\ Effect = \left(P_{MA,post\ reform} - P_{MA,pre\ reform}\right) - \left(P_{NE,post\ reform} - P_{NE,pre\ reform}\right)$$

Empirically, isolating the Massachusetts data and taking a simple first difference of the price data to determine a plausible upper bound for the change in prices, without controlling for demographics. The change in demographics are so slight that they cause near-multicollinearity in regression results and render the post-reform estimates unusable.

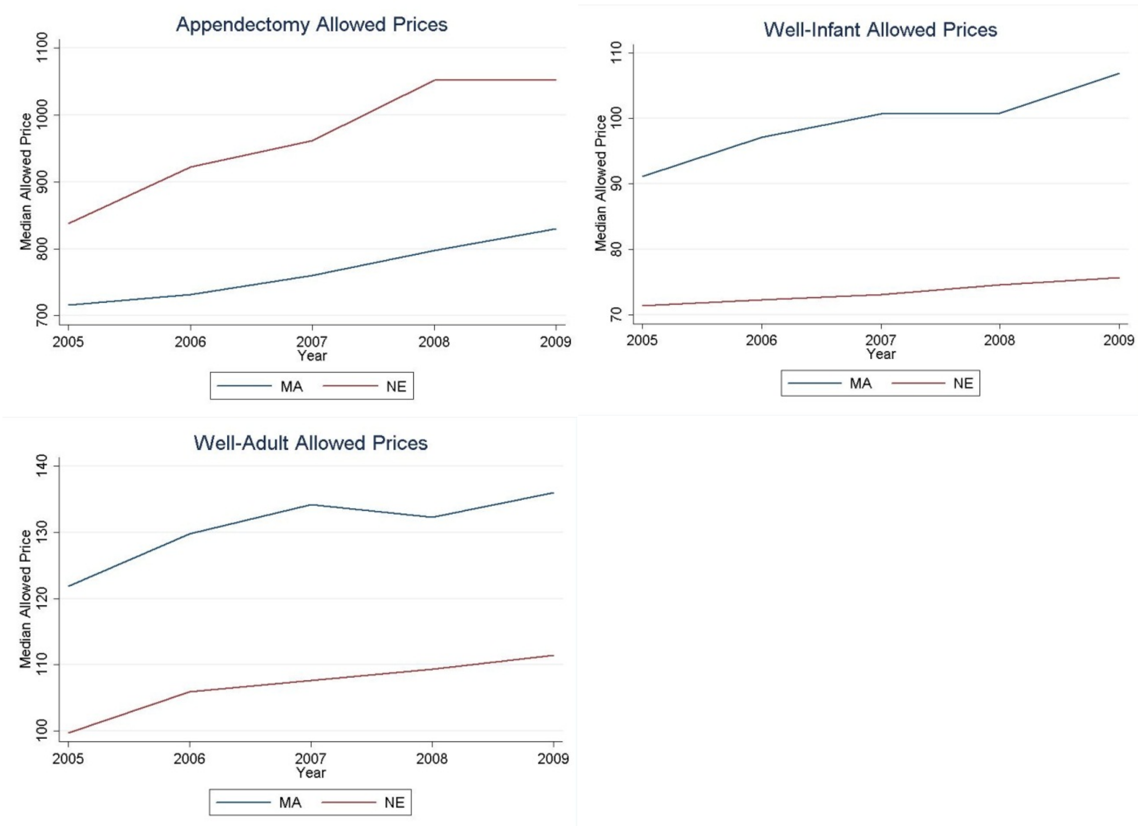
Procedural First Differences in Massachusetts

	Appendectomy: ln(allowed)	Well-Infant: ln(allowed)	Well-Adult: ln(allowed)
Post-Reform	0.066	0.065	0.036

The following graphs provide additional justification for the triple-differences estimation strategy: despite the fact that the demand for appendectomies should remain unchanged, the price of appendectomies increases. This indicates that while demand for the appendectomies remains the same, the demand for *insurance* that covers appendectomies increases.

[33] Requirements takes from the Massachusetts Health Connector website, mahealthconnector.org

Figure 3.2: Price Trends in Health Services



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